THREE ESSAYS ON DEVELOPMENT AND LABOUR ECONOMICS

A Thesis Submitted for the Degree of Doctor of Philosophy

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January 2009

PART A: General Declaration [to be inserted at the front of the thesis]

Monash University
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Declaration for thesis based or partially based on conjointly published or unpublished work

General Declaration

In accordance with Monash University Doctorate Regulation 17/ Doctor of Philosophy and Master of Philosophy (MPhil) regulations the following declarations are made:

I hereby declare that this thesis contains no material which has been accepted for the award of any other degree or diploma at any university or equivalent institution and that, to the best of my knowledge and belief, this thesis contains no material previously published or written by another person, except where due reference is made in the text of the thesis.

This thesis includes one original papers published in peer reviewed journals and two unpublished publications. The core theme of the thesis is in empirical development and labour economics The ideas, development and writing up of all the papers in the thesis were the principal responsibility of myself, the candidate, working within the Department of Economics under the supervision of Pushkar Maitra, Dietrich Fausten and mark Harris

[The inclusion of co-authors reflects the fact that the work came from active collaboration between researchers and acknowledges input into team-based research.] *remove for theses with sole-authored work*

In the case of chapter 3 my contribution to the work involved the following:

[If this is a laboratory-based discipline, a paragraph outlining the assistance given during the experiments, the nature of the experiments and an attribution to the contributors could follow.]

Thesis chapter	Publication title	Publication status*	Nature and extent of candidate's contribution
3	Skilled Immigration and Wages in Australia	published	Literature review, empirical estimation and interpretation

[* For example, 'published'/ 'in press'/ 'accepted'/ 'returned for revision']

I have / have not (circle that which applies) renumbered sections of submitted or published papers in order to generate a consistent presentation within the thesis.

Signed: ..[redacted]......................................

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Declaration

I hereby declare that this thesis contains no material that has been accepted for the award of any other degree or diploma in any university or equivalent institution, and that to the best of my knowledge and belief, this thesis contains no material previously published or written by other person, except where due reference is made in the text of this thesis.

The thesis contains a publication of which the candidate was the primary researcher and author. This declaration is followed by a statement signed by the principal co-authors of the publication that the candidate was the primary researcher and author of the publication.

Asadul Islam

To my parents and Rafia

Acknowledgements

I am deeply indebted to my supervisors Pushkar Maitra, Dietrich Fausten, and Mark Harris for their valuable research guidance, encouragement and advice during the last three years. I owe a special thanks to Dietrich Fausten for providing constant support and tireless encouragement since the start of my journey at Monash University.

This work has benefited greatly from the extensive assistance of the faculty and staff in the Department of Economics, in providing a congenial atmosphere in which to live, think, and write. In particular, I am grateful to Russell Smyth, Chongwoo Choe, Madhumita Bhattacharya, Dyuti Banerjee, Jaai Parasnis, Fang-Fah Lam, and Barbara Cramer for their supports. This research would not have been possible without the generous funding from the Monash Graduate Scholarship (MGS) and Endeavour International Postgraduate Research Scholarship (IPRS). I therefore wish to express my profound gratitude to the Monash Graduate School, and the Department of Economics at Monash University for facilitating and providing the financial support that made this study possible.

Some of this research has been submitted for publication to several journals, and I thank the referees and the editors for their valuable comments and suggestions. I am also very much grateful to the valuable comments made by the participants of the various seminars, workshops and conferences where I presented my research. These suggestions have greatly improved each chapter of this thesis. I also thank the Department of Economics and my supervisors for funding my participation at these seminars, workshops and conferences.

My parents, and my brother Rafiq, have stood beside me in hard times, and graciously provided essential moral support to keep me going. They are incomparable.

My wife, Rafia, has been a constant source of inspiration and strength in the completion of this thesis. This research was made possible by her unconditional sacrifice and by her endless love.

Abstract

This thesis is a collection of three self-contained papers in development and labour economics. The first two chapters study the impact of microfinance programs using a new, large and unique cross-section dataset from Bangladesh. Chapter 1 evaluates the impact of microfinance programs on household consumption. The program eligibility requirement and the richness of the data allowed the use of a number of non-experimental impact evaluation techniques, in particular Instrumental Variable (IV) estimation and Propensity Score Matching (PSM). Estimates from both IV and PSM strategies have been interpreted as average causal effects that are valid for various groups of participants in microfinance. The overall results indicate that the effects of micro loans on consumption are not robust across all groups of poor household borrowers. It appears that the poorest of the poor participants are among those who benefit most. The benefits are lower, or sometimes even negative, for households that are marginal to the participation decision. The effects of participation are, in general, stronger for male borrowers.

Chapter 2 uses a similar methodology to examine the impact of these microfinance programs on child welfare, specifically on school attendance and child labour in rural Bangladesh. The empirical results indicate that household participation in a microcredit program may increase child labour and reduce school enrolment. The effects are more pronounced for girls than boys, and they appear to vary inversely with age, with younger children tending to be strongly affected. The estimated effects also vary by income, education and asset holding of households such that the children of poorer and less educated households are affected most adversely.

Finally Chapter 3, employing similar econometric techniques for estimating causal effects, investigates the impact of the changing skill composition of immigrant flows on the structure of Australian wages. Immigrants may self-select to join labour markets in the better performing industrial countries. We address the resulting endogeneity problem using different IV techniques. While existing studies typically use cross-section data, we use macro data to allow for the adjustment of wages and aggregate demand to immigration flows. Our estimation strategies generate results that are consistent with the dominant findings from existing empirical work. We find no robust evidence that a relative increase in skilled immigrants exerts any discernible adverse consequences on the wage structure in Australia.

Table of Contents

Introduction of the Thesis

This thesis is a collection of three self-contained papers in empirical development and labour economics. The first two chapters study the impact of a large-scale microfinance program using a new, large and unique cross-section data set from Bangladesh. The third chapter examines the impact of the relative growth of skilled migration on the structure of Australian wages. A common element of the three chapters is the use of instrumental variable (IV) techniques to deal with the problem of endogeneity in their respective contexts. A successful IV strategy corrects not only for the omitted variable biases and reverse causality, but also for measurement error in the endogenous variable as long as the measurement error has the classical form.[1] IV strategy has been used as the main estimation technique in each chapter of this thesis, both in the context of microfinance and immigration. In addition, a number of alternative econometric techniques have been used to check the robustness of the empirical findings.

Microfinance has become a well-known element of development strategies. Interest in microfinance has been rekindled with the award of the 2006 Nobel Peace Prize to Professor Yunus and the Grameen Bank (GB), the bank Yunus founded more than three decades ago. Microcredit[2] programs expanded rapidly in Bangladesh, generating a wave of enthusiasm in development circles. Their progressive expansion has encouraged many countries to establish microfinance operations as a strategy for poverty reduction. By way of illustration, the microfinance industry has been growing tremendously throughout the developing world. The number of people who received credit from the microfinance institutions (MFIs) rose from 13.5 million in 1997 to 113.3 million in 2005. During the same period the number of MFIs increased from 618 to 3,133 (Daley-Harris 2006).

Microfinance can be thought of as small credit made available to poor people who cannot enter the formal credit market. It requires no collateral and generally focuses on women because of the low-cost peer monitoring, efficient delivery and recovery and borrower trust with positive expectations. Loans are provided through informal groups mobilised as part of a program strategy to reach the poor. The group-based credit program means that contracts effectively make borrowers co-signers to each other's loans, thus providing incentives for peer monitoring and mitigating problems created by informational asymmetries between lenders and borrowers. The program is thus based on joint liability and a self-selective mechanism to generate group collateral. Programs often offer targeted training and information sessions to borrowers with the aim of helping them to best use their loan.

[1] See, for example, Chapter 5 of Wooldridge (2002).

[2] In this study we use the terms *microcredit* and *microfinance* interchangeably. Strictly speaking, microfinance tends to imply something more than just credit because other services are often included, such as insurance and a savings facility.

Empirical work on the impact of microfinance is relatively sparse compared to the scale of operation of this important program worldwide. As pointed out by Armendáriz de Aghion and Morduch (2005) “there have been few serious impact evaluations of microfinance so far, though, so a collection of definitive results is still awaited” (p. 207). Most theoretical literature focuses on joint liability group lending and its implications for reducing information asymmetries (Banerjee, Besley and Guinnane 1994; Besley and Coate 1995; Ghatak and Guinnane 1999; Rai and Sjostrom 2004; Chowdhury 2005). That is, the emphasis of existing theoretical studies is on *why* and *how* microfinance works (Herms and Lensink 2007) but not *whether* or *for whom* it actually works. These are empirical questions which the present study addresses.

Agains the backdrop of popular demand for microfinance, policy makers and practitioners, mainly from developing countries, have renewed the pledge to expand microcredit programs and to increase their outreach to a wider community. The United Nations (UN) declared 2005 to be the International Year of Microcredit. The UN urged multilateral donor agencies and developed countries to support the microfinance movement to achieve its Millennium Development Goal of halving poverty by 2015. Therefore, it is important to know whether households actually benefited from microcredit loans, and if so, who the main beneficiaries of such small loans are. As Morduch (1998) points out: “tens of millions of dollars worth of subsidized resources support these programs, and the question now is whether these benefits are justified by their substantial costs.”

This study examines the impact of large-scale microfinance programs. Chapter 1 evaluates the impact of these programs on household consumption in Bangladesh. Bangladesh makes a good case study for evaluation of microfinance because MFIs are very influential in its economy and are rapidly expanding. In 2004 microcredit loans constituted around 5.3 percent of total private sector credit (CDF 2005).

Improving economic well-being is the main objective of the microfinance program. Household food expenditures and the value of food consumption provide the most common measure of well-being. This study uses two measures of consumption expenditures to evaluate the impact of microfinance: total household consumption and per-capita consumption expenditures. It uses the survey conducted by the Bangladesh Institute of Development Studies (BIDS) for the Palli Karma-Sahayak Foundation (PKSF) (Rural Employment Support Foundation) to monitor and evaluate microfinance programs in Bangladesh.[3] The survey encompasses a wide variety of

[3] The data collection and preliminary analysis was supported by the World Bank. PKSF, established in May 1990, works as an organization for MFIs. The micro-lending community regards it as a regulatory agency and it exercises authority over the MFIs. PKSF mobilises funds from a wide variety of sources (such as the World Bank, the

information at the household, village and organisation level. It covers 3,026 households comprising participants in the program (treatment group) and control groups covering 91 villages spread over 23 thanas (sub-districts). The MFIs studied here are the Association for Social Advancement (ASA), Proshikha and 11 other MFIs which are members of PKSF (see Chapter 1, Table A1 for more details). The dataset comes from the largest survey of microfinance households ever conducted in Bangladesh, and possibly in the world.

One of the most visible recent changes in the lives of the rural women in Bangladesh is the significant increase in their access to credit. This has been made possible by the emergence of many MFIs that have developed large-scale operations by offering a small number of highly standardised products. The Grameen bank (GB) is the flagship of the microfinance movement, and it has been a source of ideas and the model for the many institutions in the field of microcredit that have sprung up around the world (Nobel Committee 2006). The GB, along with two other MFIs, the Bangladesh Rural Advancement Committee (BRAC) and the Bangladesh Rural Development Board (BRDB), is the focus of previous studies on microfinance (Pitt and Khandker 1998; Morduch 1998; Madajewicz 2003). In Bangladesh, the homeland of the GB, hundreds of MFIs replicating the Grameen model have now emerged. Expansion, competition and funding constraints have greatly changed the recent dynamics of microfinance in Bangladesh. For example, ASA, which started its microfinance operations in 1991, has now become a dominant MFI in terms of number of beneficiaries and loan disbursement. Similarly, Proshikha has been able to increase its outreach remarkably during the 1990s, reaching about 2.8 million borrowers by 2001. During that period the number of medium and small MFIs has grown from a very small base to more than a thousand institutions. These are more efficient in terms of credit delivery, services and giving access to poor borrowers (Zohir et al. 2001).

The existing evidence on the impact of the microcredit program in Bangladesh is not unambiguous. Different identification strategies have yielded different conclusions. The best-known impact evaluation study of microfinance is by Pitt and Khandker 1998 (hereafter PK), which was a joint research project of BIDS and the World Bank. PK find that microfinance significantly increases consumption and reduces poverty, and they also find that the marginal impact of microfinance on consumption to be greater for women (18 percent) than for men (11 percent). They use an IV approach and employ the Weighted Exogenous Sampling Maximum Likelihood (WESML) estimator to choice-based sampling. Morduch (1998) applies a Difference-in-Difference (DD) approach to the PK dataset and finds that microcredit has an insignificant, or even negative, effect on the same outcome measures used by PK. Madajewicz

Government of Bangladesh, international donors and lending agencies) and provides these funds to its members for lending as microcredit.

(2003) also uses the IV method (but without WESML estimation) with the PK dataset, and finds results similar to those of Morduch.

Using panel data Khandker (2005) finds a weaker effect of microfinance participation than in his earlier cross-sectional studies. These results cast doubt on the optimistic 5 percent drop in poverty reported by the PK study. There have been a few impact evaluations of the similar microfinance programs in other countries (see, for example, Coleman (1999); Kaboski and Townsend (2005) for Thailand, and Karlan and Zinman (2008) for South Africa). Most of the other studies on impact evaluation of microfinance are descriptive and do not consider the problem of selection bias. This bias, arising from the non-random program placement and self-selection into the program, may compromise the validity of the impact estimates. Armendáriz de Aghion and Morduch (2005) note that "there have been few serious impact evaluations of microfinance so far, though, so a collection of definitive results is still awaited" (p. 207).

This study differs from previous empirical studies in the choice of household sample and microfinance institutions. It also differs in terms of the techniques used to evaluate the impact of microfinance programs. Estimating the causal effect of microcredit is made difficult by the non-random program placement and self-selection of participants. Microcredit programs are available in certain villages, and households self-select into these programs. Participants are likely to differ from non-participants in the distribution of observed characteristics, leading to a "selection-on-observables" bias (Heckman and Robb 1985). There are also problems due to "selection-on-unobservables." In other words, programs may be placed in a non-random sample of villages, and households may self-select into those programs (and subsequently decide how much to borrow). Using non-experimental data we cannot distinguish the bias generated by the non-experimental estimator. We therefore discuss a number of solutions to this problem of selection bias, including parametric and semi-parametric strategies.

In Chapter 1, we mainly use two strategies to address the selection bias in estimates of the effects of participation in microfinance. The first strategy uses an instrumental variable (IV) generated by village-level program placement and by an eligibility rule for receiving microfinance. An instrumental variable is correlated with the endogenous regressor but is uncorrelated, conditional on other covariates, with the error term in the equation of interest. It should also be uncorrelated with the outcomes of interest through any channel other than their effect via the endogenous regressor. As in Imbens and Angrist (1994) (hereafter IA), and Angrist, Imbens and Rubin (1996) (hereafter AIR), estimates from the IV strategy are interpreted as Local Average Treatment Effect (LATE) that are tied to specific intervention. In this case, IV is shown to estimate the effect of microfinance on those households who participated into the credit program solely because they are eligible and live in the program village. The second strategy uses propensity score matching (PSM) of Rosenbaum and Rubin (1983), where participants are compared with matched non-participants (based on propensity score), while controlling for the characteristics used by the MFIs to select the households and other observable household characteristics that are potential determinants of participation in microfinance. Estimates from both strategies are compared. The findings from the alternative evaluation methods demonstrate that IV and matching estimators potentially uncover different parameters, even when the assumptions justifying each approach are valid. This study therefore illustrates and compares a number of techniques for non-experimental evaluation research in models with heterogeneous treatment effects.

The overall results indicate that the effects of micro loans on consumption are not robust across all groups of poor household borrowers. It appears that the poorest of the poor participants are among those who benefit most. The benefits are lower, or sometimes even negative, for households that are marginal to the participation decision. The effects of participation are, in general, stronger for male borrowers. These empirical findings hold across different specifications and methods, and when corrected for various sources of selection bias including possible spillover effects.

Our results in Chapter 1 indicate that microcredit programs can improve poorer household welfare by increasing household consumption. Similar results are obtained by many studies such as Pitt and Khandker (1998); Kaboski and Townsend (2005); Karlan and Zinman (2008). However, there is less evidence on the impact of microcredit on human capital formation, and the limited evidence that exists is far less conclusive than the effect of microcredit on alleviating poverty.

In Chapter 2, we examine the impact of these microfinance programs on child welfare, specifically on schooling and child labour in rural Bangladesh. Poor households in developing countries often keep their children out of school to make an immediate contribution to household earnings. These households face borrowing constraints which raise the marginal cost of attending school (Becker 1993), or they may send their children to work, even in the absence of liquidity constraints, if the return to education is lower. In either case, children’s education is likely to suffer with lower income. This can cause an intergenerational ‘dynastic trap’ problem because less educated children will become poorer adults and, thus, are much more likely to send their own children to work. For the most part, underdevelopment of credit markets, coupled with low household income (Ranjan 1999) or lack of access to credit, are considered major factors responsible for the inadequate education of children in developing countries (Jacoby and Skoufias 1997; Ranjan 2001; Dehejia and Gatti 2005; Edmonds 2006).

One reason why children may be sent to work instead of attending school is the fact that the net return to human capital investment is considered insufficient to cover the opportunity cost of education. This could be the result of large direct and indirect costs of schooling or of a very low return to schooling (close to zero) because the quality of education is very low. Alternatively, credit constraints may impose restrictions on the investment in human capital. Credit-constrained poor households often send their children to work to smooth out consumption (Jacoby and Skoufias 1997) or simply to survive. This is the reason why children are still an economic source for poor parents in developing countries. The availability of credit means that parents need not rely on income that can be raised from child labour. The ability of parents to borrow in order to finance current investment or to maintain income-generating activities plays an important role in the decision whether to invest in education for their children.

There is considerable evidence that poor households in rural areas of developing countries are credit constrained. A major obstacle to obtaining credit from the formal banking sector is the inability of a household to meet collateral requirements. This requirement is not imposed by microcredit organisations. However, the nature of investment for microcredit loans (investment in self-employment and petty activities) and its repayment methods require quick and high returns from the investment. Specifically, in order to service the loan it may be necessary to supplement household income, at least temporarily, with the proceeds from child labour. Therefore, the additional activities made possible by access to microcredit and the factors related to servicing terms of microcredit loan may alter household preferences towards child schooling. Children may be employed directly in the newly created or expanded micro enterprises, or indirectly, as carer for their siblings or in farm and livestock duties and other household chores.

Chapter 2 uses a similar methodology to Chapter 1. The main identification strategy to evaluate the impact is based on an IV method where the instrument is an exogenous variation in treatment intensity among households in different villages. In addition, we estimate the treatment effect by combining regression adjustment with weighting based on propensity score (Rosenbaum and Rubin 1983) – an approach suggested by Robins and Rotnitzky (1995). We also examine the robustness of the estimates by using the alternative control function method, which does not rely completely on the exclusion restriction. The empirical results indicate that household participation in a microcredit program may increase child labour and reduce school enrolment. The effects are more pronounced for girls than boys, and they appear to vary inversely with age, with younger children tending to be strongly affected. The estimated effects also vary by income, education and asset holding of households such that the children of poorer and less educated households are affected most adversely.

Finally, Chapter 3 investigates the impact of the changing skill composition of immigrant flows on the structure of Australian wages. By virtue of its significant influence on Australia's population growth, and the size and composition of its labour force, immigration is an important factor driving the country's economic performance and overall development. However, the determination of the appropriate level of immigration has been a continuing matter of public debate. One prominent issue sustaining the debate is the widespread concern that immigration harms the employment prospects of native-born Australian workers. These questions are at the heart of the debate about immigration in many countries, including most European nations, U.S and Canada (see Bauer, Lofstrom, and Zimmermann 2000). Insights from the Australian context may prove useful in addressing this issue elsewhere.

In recent decades there has been increasing number of skilled worker immigrants in many immigrant-receiving countries such as Australia, Canada and New Zealand. While in the U.S family members continue to dominate the immigrant flows, Australia and Canada's immigration program are increasingly based on skilled worker programs. In Australia, the skill stream accounted for around 65 percent (about 78,000) of the visas granted under the migration program in 2004-05. The skill stream of immigrants consists of particular categories that relate to demand in Australia for particular occupational skills. This stream of visa category is also the main one available to overseas students, and it accounted for 35 percent of the total skilled independent visas granted in 2004-05.

While skilled immigration may not erode the overall employment prospects of the native labour force, it may well affect the relative position of skilled workers. *A priori*, changes in the wages of skilled workers are likely to dominate changes in the wage differential between skilled and unskilled labour. Unskilled wages are relatively unresponsive to market forces (and thus to immigration) by virtue of the minimum wage setting practice in Australia that relies on union-negotiated increases. Skilled wages are not so restricted, and typically respond to the changing labour market situation. Hence, native skilled workers are potentially more exposed to competition from skilled migrants than are native unskilled workers. It follows that skill-targeted immigration policy like Australia's may influence not only domestic wage levels but also the domestic wage structure. Given the relative inertia of unskilled wages, it is plausible to hypothesise that changes in average domestic wages reflect comparable movements in the wages of skilled workers and, hence, in the skilled-unskilled wage differential.

We investigate the impact of migrants' skill enhancement on domestic wages in Australia over the last quarter of a century (1980-2006). Unlike much of the existing literature, we use macro data to allow for the adjustment of wages and aggregate demand to immigration flows. We also use the IV method in this chapter to deal with the potential endogeneity of immigration. Immigrants who come to Australia are probably not a random subset of the source country workforce. Immigrants are, typically, ambitious and entrepreneurial. Immigrants may self-select to join labour markets in the better performing industrial countries. In addition, host countries may base their annual target immigration rates on a predetermined immigration policy or on domestic labour market conditions. We address the resulting endogeneity problem by exploiting the fact that Australia's immigration policy and labour market outcomes in earlier periods may serve as choice criteria for immigrants seeking admission to Australia. The limited time span of macro data raises problems of small sample bias. We address the small sample bias problem by using the Jackknife IV estimation (Angrist, Imbens and Krueger 1999). Various specification and validity tests support the choice of instruments. Our estimation strategies generate results that are consistent with the dominant findings from existing empirical work. We find no robust evidence that a relative increase in skilled immigrants exerts any discernible adverse consequences on the wage structure in Australia.

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Chapter 1

Who Benefits from Microfinance? An Impact Evaluation of Large Scale Programs in Bangladesh

1.1 Introduction

Microfinance has become a prominent element of development strategies. Over the last two decades microcredit programs have expanded rapidly, first in Bangladesh and then around the developing world. Most development practitioners and policy makers believe that microfinance can help the poor to break out of poverty. The academic world has also shown increased interest in microfinance. A great deal has been written on microfinance theory (see, for example, Ghatak and Guinnane 1999; Chowdhury 2005 and the reference therein).[4] Much of this literature has focused on joint liability group lending and its implications for reducing information asymmetries. In spite of the abundance of theoretical literature, empirical work on the impact of microfinance is relatively sparse, especially compared to the worldwide scale of the operation of these programs (Armendáriz de Aghion and Morduch 2005, Herms and Lensink 2007).

This chapter evaluates the impact of microcredit participation on household consumption using a large, nationally representative and unique cross-section data set from Bangladesh. The data comes from a survey conducted by the Bangladesh Institute of Development Studies (BIDS) for the Palli Karma-Sahayak Foundation (PKSF, Rural Employment Support Foundation) specifically for the purposes of evaluating microfinance programs in Bangladesh.[5] The survey encompasses a wide variety of information at the household, village and organisation level. It includes 3,026 households comprising households in the program and control groups, covering 91 villages spread over 23 thanas (sub-districts). This is the largest survey of microfinance households ever conducted in Bangladesh, and possibly in the world.

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[4] The reader can consult the "Group lending" special issues of the *Journal of Development Economics*, 1999, vol. 60(1), pp.1-269, and *The Economic Journal*, 2007, vol. 117(517): F1-F133.

[5] The data collection and preliminary analysis were supported by the World Bank. PKSF, established in May 1990, works as an organization for MFIs. The micro-lending community regards it as a regulatory agency and it exercises its authority over the MFIs.

The existing evidence on the impact of the microcredit program in Bangladesh is ambiguous. Different identification strategies have yielded different conclusions. The best-known impact evaluation study of microfinance by Pitt and Khandker (1998) (hereafter PK) finds that access to microfinance significantly increases consumption and reduces poverty. However, Morduch (1998), using PK's dataset but employing a different estimation methodology, finds that access to microfinance has an insignificant, or even negative, effect on household welfare. More recently Madajewicz (2003), using the same dataset but again a different estimation methodology, finds results similar to those obtained by Morduch (1998).[6]

Using panel data from Bangladesh, Khandker (2005) finds a significantly weaker effect of microfinance participation than he found in his own earlier cross-sectional study with Pitt. These results question the accuracy of the optimistic 5 percent drop in poverty reported by the PK study. There have been a few impact evaluations of the similar microfinance programs in other countries. In the case of Thailand, Coleman (1999) finds that the average program impact is insignificant on physical assets, savings, and expenditure on education and health care. Kaboski and Townsend (2005) find that institutions with good policies can promote asset growth, consumption smoothing and decrease the reliance on moneylenders in Thailand. However, they find no measurable impacts of joint liability or repayment frequency. Karlan and Zinman (2008) examine the impact of expanding access to consumer credit using data gathered from a field experiment in South Africa. Their results indicate significant and positive effects on income, food consumption, and job retention.

We estimate both the effect of participating in microfinance programs (the treatment on the treated effect) and the effect of being offered the chance to participate in a microfinance program (the intention-to-treat effect). The Intention-To-Treat (ITT) effect suggests a smaller positive effect of assignment (eligibility). A good number of eligible households in the treatment village did not participate while some non-eligible (non-encouraged) households participated in the program. In our case, assignment (an eligibility criterion) is merely an encouragement to take treatment and there is non-compliance among those encouraged. As a result, we use two different techniques to address the issue of selection bias, and to link ITT

[6] PK use an instrumental variable (IV) approach considering the choice based sampling, and employ the weighted exogenous sampling maximum likelihood (WESML) estimator of Manski and Lerman (1977). PK's IV approach is parallel to the use of limited information maximum likelihood (LIML) and village fixed effects (FE), and thus called their estimate LIML-WESML-FE. Morduch (1998), on the other hand, applies a simple difference-in-difference (DD) approach. Madajewicz (2003) uses an IV method which is very similar to that of PK's method, but she estimates the impact of lending programs on business profits of borrowers by poverty status. According to Greene (2000), "WESML and choice based sampling method are not the free lunch they may appear to be. In fact, what the biased sampling does, the weighting undoes", p.823. Armendáriz de Aghion and Morduch (2005) argues that despite PK use heavier statistical artillery than other microfinance studies it does not mean that they deliver results that are more reliable or rigorous than others.

effects to treatment effects. We first use an IV approach, where the instruments are generated by village-level program placement and by an eligibility rule for receiving microfinance. We also exploit the variation in the amount of credit borrowed across households of different villages – a feature that has not yet been utilised – based on the exposure to the program. The second approach uses the propensity score matching (PSM) method of Rosenbaum and Rubin (1983). Here treated are compared with matched untreated (based on propensity scores), while controlling for the characteristics used by the MFIs to select the households and other observable household characteristics that are potential determinants of participation in microfinance. When applying matching methods, we find a substantially lower effect on consumption.

Improving economic well-being is the main objective of microfinance programs. Expenditure on food consumption accounts for more than 70 percent of total household expenditure among the rural poor of Bangladesh.[7] Overall, the results suggest that the effects of microfinance loans on food consumption expenditure are not robust across all groups of borrowers. We find evidence against the "common effect" assumption using the analysis on subgroups. The results overwhelmingly suggest that the poorest of the poor benefit the most from participating in microfinance. The impacts are lower, or sometimes even negative, for those households marginal to the participation decision. The effects of participation are, in general, stronger for male borrowers. The empirical findings hold across different specifications and methods, and when corrected for various sources of selection bias, including possible spillover effects.

1.2 The Program, the Data and the Descriptives

1.2.1 The Program and the Context

The microfinance sector of Bangladesh is one of the largest and oldest programs of the world.[8] During the 1990s there was phenomenal growth in the MFI sector, in terms of the number of MFIs as well as total membership, and this growth is continuing. The PKSF was established to monitor the activities of these large numbers of MFIs, and to lend funds to its Partner Organisations (POs) for microcredit. In 1998 PKSF funds made up about 24% of the total microfinance industry in Bangladesh.[9]

Previous studies on microfinance in Bangladesh have primarily focused on the Grameen Bank (hereafter GB) (PK; Morduch 1998, 1999). However, expansion, competition and funding

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[7] Poor households' savings in rural areas of Bangladesh is very negligible. Our hypothesis is that if household income increases significantly to affect their permanent income level, households' consumption expenditure will increase. Moreover, there are well-known difficulties with collecting income data, especially from developing countries.
[8] Around one quarter of the world's micro-credit customers are in Bangladesh with a further quarter in India (State of the microcredit summit campaign report 2006).
[9] See <http://www.bwtp.org>.

constraints have greatly changed the recent dynamics of microfinance in Bangladesh. For example, the Association for Social Advancement (ASA), which started its microfinance operations in 1991, has now become a dominant MFI in terms of number of beneficiaries and loan disbursement. Similarly, Proshikha has been able to increase its outreach remarkably during the 1990s, reaching about 2.8 million borrowers by 2001. During that period the number of medium and small MFIs has grown to more than a thousand institutions. In view of the growing importance of the non-GB MFIs in Bangladesh, in this study, we use data from 13 MFIs of PKSF. (By contrast, both PK and Khandker primarily used GB data, with only two other MFIs.) As a result, the organisations investigated here are different from those studied previously, and include organisations that are very large in terms of loan disbursements and area of coverage, most notably the ASA and Proshikha. ASA provides both credit and savings services on a remarkably large scale. Proshika is the fourth largest microcredit program in Bangladesh. Notable other MFIs which we study here include the Society for Social Services (SSS) and Thengamar Mohila Sabuj Sangha (TMSS). As of December 2004, SSS is the tenth largest MFI in Bangladesh in terms of cumulative disbursements and outstanding borrowers. TMSS is one of the top fifty MFIs in Bangladesh. The other MFIs are relatively small and have similar types of program activities (see Appendix 1, Table A1). All of these MFIs follow the GB-style lending procedure and typically give access to microfinance to households having less than 50 decimals of land. Credit is given mainly to groups of people who are jointly liable for repayment of the loan, and there is no collateral requirement. Loans are primarily advanced for any profitable and socially acceptable income generating activity. The amount of a loan usually lies within the range of US$40 - $150. However, members may take larger loans after repaying their first loan.

1.2.2 The Data and Survey Design

The data was collected initially to monitor and assess the impact of microfinance programs undertaken by MFIs of the PKSF. BIDS was responsible for the collection of data on behalf of PKSF. The survey includes 13 MFIs, each from a different district, covering 91 villages spread over 23 thanas. Following a census of all households in these villages during October 1997, the survey was administered in early 1998. Besides collecting detailed information at the household level, separate modules were administered at the village and institution level.

The survey was conducted to obtain a nationally representative dataset for the evaluation of microfinance programs in Bangladesh where more than 70 percentage of population live in villages. In selecting MFIs for the survey, a stratified random sampling procedure of all MFIs in Bangladesh was followed. Since MFIs were not operating in all thana, a selection of thana within the district of each MFI was made, followed by the selection of villages. The geographic coverage of the survey was spread evenly over Bangladesh, and the selected thanas were similar to the national average (Mahmud 2003). Within a MFI area, the selection of villages involved visiting the local MFI offices and interviewing key informants to prepare a list of all villages in the area and to compile village-specific information. This information included the type of MFI-activities; the number of MFI groups; the number of borrowers; the condition of infrastructure; and the existence of other MFIs. Upon obtaining this information, a sample of villages in each selected MFI was drawn through stratified random sampling. The stratification was based on the presence or absence of microfinance activity. The non-program villages were selected among neighbouring villages. The selection of the MFI within a village was not difficult. During the time of the census, in 1997, it was very rare to have two or more MFIs operating in the same village. However, this issue has become a concern as many villages now have multiple MFIs.

Of the 13 selected MFIs, two (ASA and Proshika) were deliberately chosen from the large category. Secondly, thanas were selected when more than one thana was covered by the MFIs. Then, two control villages and six program villages were chosen from each of the MFI areas. However, since non-program villages could not be found under some of the MFIs, only 11 non-program villages could be included. As a result, six to eight villages from each MFI were selected, depending on the availability of control villages. The households in the program villages, drawn from the census, were grouped according to their eligibility status. A household was regarded to be eligible if it owns 50 decimals (half an acre) or less of cultivable land. Participation was defined in terms of current membership as reported in the census in 1997. From the village census list, 34 households were drawn from each program and non-program village. Because the census found a good number of ineligible households in program villages, the sample was drawn so as to maintain the proportion of eligible and ineligible households at about 12:5. The sample size within program and control villages was determined accordingly.[10] A total of 3,026 households were drawn from program and control villages including 1,740 participants. Of the 1,286 non-participants, 277 were from control villages and 1,009 were from program villages. Because of the absence of appropriate control villages, more non-participants were drawn from program villages.[11] The samples from the control villages include those households whose heads expressed their willingness to participate in MFI programs, if available. Among the total surveyed households 2,051 were eligible, which represented 67.7 percent of all households. The same proportion applied to the program village: 1,835 were eligible out of 2,735 households. Of the total number of 1,740 borrowers, 207 are men.

[10] The sample size and its ratio between participating and non-participating households are different in a few villages because of the absence of the required number of appropriate households in each group.

[11] Khandker (2005) also highlights the limitation of getting the control villages in Bangladesh. He finds that the villages that were controls in 1991-92 in his survey, all became program villages by 1997-98.

1.2.3 Descriptive Statistics

Table 1.1 presents the descriptive statistics for different village-level characteristics. It shows that there are no systematic differences in terms of education and health characteristics. Among transport and communication facilities, there are differences in terms of the presence of *pucca* (brick-built) roads in the village (35 percent in the program villages as opposed to 11 percent in the control villages) and the distance of the village from the nearest thana (the program villages are relatively closer to the thana). Furthermore, the program villages are more likely to have access to electricity than the control villages. There are no statistically significant differences between program and control villages in terms of bazaars, post offices and telephone offices. In terms of irrigation facilities, no statistically significant differences were found, though in all cases program villages have better facilities, as indicated by the higher average proportion of facilities per village. Overall we see that program villages are more developed in terms of infrastructure and other related facilities.

[INSERT TABLE 1.1 HERE]

Table 1.2 provides key descriptive statistics for the household level variables. It shows that the average landholding for the non-treated households is significantly higher than the treated households. For household size, both Kolmogorov-Smirnov (K-S) and t-tests suggest a difference between treatment and comparison households. There are also differences in many household characteristics of treatment and comparison groups as indicated by p-values and K-S tests, but these differences are minimal when we consider only the eligible group of households (households owning less than half an acre). In fact, many of the characteristics are also very similar for samples of households with up to one acre of land.

[INSERT TABLE 1.2 HERE]

[INSERT FIGURE 1.1a AND 1.1b HERE]

Table 1.3 presents summary statistics of food consumption and credit variables. Consumption expenditure data include expenditures of food consumed in the reference period. The information covers a wide range and types (e.g. food purchased, or food produced at home) of food consumption, and is as good as the standard LSMS food consumption module. Table 1.3 suggests that there are no statistically significant differences between treated and non-treated groups of households in terms of food consumption, though non-treated groups have a little more household and per capita consumption levels than the treated group. However, when we consider consumption expenditure by household ownership of land, total household monthly consumption expenditure is higher for treated households having two or less acres (Figure 1.1a). Household consumption expenditures for both groups are a monotonically increasing function of household ownership of land. On a per capita basis, non-treated households have higher consumption expenditure than the treated group (Figure 1.1b). Monthly food consumption expenditures, at the household level, are quite similar between program and control villages (Table 1.3). However, when we consider per capita monthly consumption expenditure, households in control villages have slightly higher consumption than those in program villages. Table 1.3 also shows that villages with male borrowers borrowed more than their female counterparts. Households with male participants also have, on average, a higher number of members in microfinance and have more exposure (length of membership in microfinance) to the program. They also have higher consumption at both the household and per capita levels.[12]

[INSERT TABLE 1.3 HERE]

1.3. Empirical Strategy

There are a number of potential sources of bias that need to be accounted for when examining the effect of participation in microfinance. First, participants are likely to differ from non-participants in the distribution of observed characteristics, leading to a "selection-on-observables" bias. There are also problems due to "selection-on-unobservables," in other words, programs may be placed in a non-random sample of villages, and households may self-select into the program (and subsequently decide how much to borrow). For example, the program village might be poorer than the control village. Microfinance programs are targeted to poor households. A prospective member decides whether or not to participate in the microfinance program. The potential participant also has to be approved by officials of the MFI. Households are therefore self-selected into the program. Thus there are likely to be observable and unobservable differences in characteristics between participants and non-participants.

However, it is likely that the MFIs choose the program village based on some observable characteristics. There are many MFIs working in Bangladesh. Discussions with program officials at the local-office levels indicate that programs are designed by the head office. It also appears that local branch managers and officials of MFIs are generally not from the same area where the program is located. This arrangement is also encouraged by PKSF, the supervising body of the MFIs, to prevent employees making loans to their relatives or acquaintances. There are also specific guidelines from the head office to select the program villages. Given the size of the microfinance program and the number of MFIs working in Bangladesh, it is reasonable to assume that village-level program placement is a problem of "selection-on-observables."[13] We use a wide range of village-level controls to address the selection of villages. We also use MFI-

[12] A treated household consists of either male or female members, but not both, in our sample. Groups are never mixed by genders. A MFI selects the gender of the treatment group, and households do not have a choice of whether males or females will participate.
[13] Gauri and Fruttero (2003) find that NGO programs in Bangladesh are not targeted at the poor villages, and NGOs do not respond to local community needs. Their findings indicate that non-random selection of villages by NGOs (which mainly include MFIs) is not an important issue in Bangladesh.

level fixed effects to deal with the problem of unobserved heterogeneity across different MFIs. These MFI-level fixed effects also partially control for unobserved factors across different geographic areas. With controls for village and fixed effects, we assume that there are no contemporaneous village-level unobservables that are correlated with microfinance program placement in a village and the consumption expenditure of a household.

However, identification also requires controlling for the endogeneity that arises from household self-selection into the program. So, even conditional on a set of observed covariates, X, there could be unobservable factors that may determine a household's decision to participate in a microfinance program. This could be entrepreneurial ability, information advantage, attitudes, traditions, customs or family culture, and so on. In order to understand the difficulties inherent in estimating the treatment effect, the consumption of household i in village j can be described as:

$$Y_{ij} = \pi_1 D_{ij} + \varphi_1 X_{1i} + \varphi_2 X_{2j} + \varepsilon_{ij} \tag{1.1}$$

where X_{1i} is a vector of household-specific variables, and X_{2j} is a vector of village-specific characteristics. D_{ij}=1 if household i is a member of microfinance and D_{ij} = 0 if i is not. (Alternatively, for identifying the effect of credit, D_{ij} is the amount of microcredit borrowed by household i in village j). Selection into microfinance programs on the basis of unobserved characteristics, ε_{ij}, by households may generate a non-zero correlation between ε_{ij} and D_{ij}. Therefore treatment effect estimated using OLS may not reflect the program's causal effect on household consumption.

To solve the problem of endogeneity we consider IV estimation techniques. We utilise the program eligibility criterion set by the MFIs, and use it as an instrument for participation. The eligibility rule is not completely followed so the treatment does not change from zero to one at the threshold of eligibility. If treatment was deterministic with respect to the eligibility rule, we could compare outcomes of households clustered just below the cut-off line to those just above, and directly apply the Regression Discontinuity (RD) design. Figure 1.2a shows that the participation rate falls sharply once households cross the threshold level of half an acre, but it does not fall from one to zero. The eligibility rule could not be applied for many practical considerations, some of which are mentioned below. It therefore raises concern that there could be variables observed by the loan officer but unobserved by the evaluator. As a result, we apply an approach which can be seen as an *indirect* application of (fuzzy) RD designs (see Van der Klaauw 2002). Unlike sharp RD design, selection into microfinance program in fuzzy design is based on both observables and unobservables. We implement our RD approach using an IV approach such as those used by Angrist and Lavy (1999).

[INSERT FIGURE 1.2a HERE]

Figure 1.2a illustrates that eligible households residing in a program village have a higher probability of participating in a microcredit program, and 70 percent of participants are eligible. It therefore seems reasonable to think of eligibility status in a program village as an instrument for program participation. Formally, we define V_j as the presence of a program in a village j and E_i is a variable which takes the value of one if the household is eligible (i.e. if it owns less than half an acre), and zero otherwise. So our instrument is $Z_{ij}=V_j \times E_i$, where Z_{ij} = 1 if the household lives in the program village and is eligible. The eligibility criterion and program placement are exogenous to the household and hence our instrument is 'as good as randomly assigned.'

Therefore our identifying assumption is that household ij's participation, or the amount of credit borrowed, D_{ij}, in microfinance is governed by:

$$D_{ij} = \alpha_1 Z_{ij} + \varphi_3 X_{1i} + \varphi_4 X_{2j} + \omega_{ij} \tag{1.2}$$

where X_1 and X_2 are the same as in Equation (1.1) and ω_{ij} is the household-specific error term embodying the unobserved influences on D_{ij}. We assume that Z and X are exogenous with respect to ε_{ij} and ω_{ij}. We also examine whether there is a differential effect of credit borrowed by male and female borrowers.

1.3.1 Are We Using a Valid Instrument?

Identification requires that land ownership is exogenous conditional on program participation. The exogeneity of land ownership is a plausible assumption. The validity of the land-based eligibility criterion as an instrument is also defended at length by PK, and Pitt (1999) in response to Morduch's (1998) critique. Morduch (1998) argues that the PK dataset shows a great deal of turnover in the land market. However, our data clearly demonstrates a very low turnover in the land market: only 12.8 percent of households purchased land and 9.5 percent of households sold land in the five-year period prior to the survey. This turnover rate does not differ between program and control villages. We can conclude that the land market is not active in our survey area. We do not find evidence that households endogenously sort themselves out in response to the half-acre eligibility rule. Since credit is extended mainly for self-employment activities, households having more land are exogenously ruled out.

However, some participating households own more than half an acre of land. Those households are currently not actively engaged in agriculture or the land is not suitable for cultivation, or sometimes there is mistargeting, as perfect monitoring is not possible. The eligibility rule is set simply to identify the poverty status of the household. Since land price and quality also vary between different regions, a household owning more than half an acre, in some regions, would still be considered to be poor. As a result, the loan officer or branch manager will usually make their own judgement about the poverty status of the households upon their field visit. Note that, in general, richer households get credit at softer terms from formal markets, or through other means. Also there are social norms that bar them from becoming members of a microcredit organisation. Rich people in rural areas still hesitate to become members of MFI, because they consider MFI as an organisation for the poor. Thus the use of program eligibility criterion as an instrument for treatment in microfinance is well justified here.

Moreover, in order to allow Y_{ij} to vary with the level of the landholding status, in our regression specification in Equation (1.1), we also use the amount of land by household as an explanatory variable.[14] So Z is likely to satisfy the exclusion restriction.[15] For Z to be a valid instrument the vector X_2 should include all the village-level characteristics that the MFI may use to decide program placement. We do so by exploiting the rich information collected at the village level and thus vector X_2 includes variables such as education, health, electricity, irrigation, prices, labour market conditions and infrastructure in the village. It may, however, be argued that MFIs base their selections on the unobserved characteristics of the target population in each village, rather than on the entire population of the village. In that case, our estimations would be inconsistent. Therefore, we also experimented with PK's method of using separate fixed effects for target and non-target populations in each village (estimates involve more than 300 fixed effects). Our conclusions do not change with this specification.[16]

[INSERT FIGURE 1.2b HERE]

We check whether the eligibility criterion satisfies the properties of an instrument. First, we need a strong first stage to ensure that we are not using a weak instrument. We estimate a probit model of participation in the first stage using Equation (1.2). There is a strong first stage here though the relationship between participation and eligibility is not deterministic (see Figure 1.2b). The first-stage results show that the instrument is statistically highly significant with a *t*-statistic of 12. The coefficient estimate is positive and also economically significant, which implies that I is significantly related to the demand for credit. The coefficient estimate is positive and also economically significant, which implies that eligibility is significantly related to participation. We also estimate the credit demand equation, using different covariate specifications. Applying a tobit model, we find the instrument is statistically significant at less than 1 percent. We then check whether eligibility affects consumption expenditure only through

[14] We also include a smooth function of landownership as controls. The higher order terms of landownership are not statistically significant, and inclusion of these terms does not affect our impact estimates reported in the next section.

[15] The key identifying assumption that underlies estimation using Z as an instrument is that any effects of eligibility on consumption are adequately controlled by the household land ownership included in X_1 in Equation (1.1) and partialled out of Z by the inclusion of land ownership in X_1 in Equation (1.2).

[16] The results are available upon request.

the credit program participation. We estimate a semi-reduced form equation, in which participation is instrumented but eligibility enters the second-stage regression directly (and naturally in the first-stage regression). The results do not indicate any significant effect of eligibility in any of the specifications. We also estimate a reduced-form regression for consumption expenditure on eligibility status, and we do not find any significant effect. Finally, we consider whether there is a discontinuity in the conditional mean of consumption expenditure at the eligibility cut-off point. Figures 1.1a and 1.1b illustrate no discontinuity. We also check the possible discontinuity in outcomes in treatment villages, but not in control villages, and do not find any. This is expected since the relationship between land ownership and consumption expenditure is not obvious, and microcredit is provided to either landless households or households who are not strongly active in land cultivation.

1.4 Estimation Results

1.4.1 Differences-in-Differences Estimates

In the following, we evaluate the impact of microfinance on household total monthly food consumption expenditure and per capita monthly food consumption expenditure. The dependent variable in the regression is the log of each expenditure measure. Based on household eligibility for the microfinance program, we first specify the following functional form:

$$Y_{ij} = \theta_0 + \delta_1 V_j + \delta_2 E_i + \delta_3 Z_{ij} + \varphi_7 X_{1i} + \varphi_8 X_{2j} + \zeta_{ij} \tag{1.3}$$

where Y_{ij} is the log of consumption expenditure of household i in village j. With this specification, δ_3 is the difference-in-difference (DD) of mean log consumption expenditure. It captures the difference in conditional consumption expenditure between eligible and non-eligible in program villages that is over and above the difference in control villages.

[INSERT TABLE 1.4 HERE]

Reduced-form estimates of Equation (1.3) using OLS are reported in Table 1.4. The covariates included in X_1 and X_2 are presented in the list of variables of Appendix 1. The top panel of Table 1.4 shows the coefficient estimates of the impact on the log of household total consumption expenditure by male and female households, and by land ownership. The estimated coefficient δ_3 is always positive, indicating that the eligible households in the program village are better off due to the presence of the program. The results are similar for the coefficient estimates of the effect on per capita consumption expenditures, as shown in the bottom panel of Table 1.4. The coefficient δ_3 is also known as the ITT effect. The estimates in Table 1.4 indicate that the average ITT effect is between 4 and 8 percent. These results imply that eligible households in program villages are positively impacted by the presence of the program.[17] It also shows that simple difference estimates of only the eligible in program and control villages would understate the effect of eligibility.

The advantage of using the criterion of eligibility, rather than actual participation in microcredit programs, is that we can effectively eliminate the problem of non-compliance. There is no reason to believe that non-compliance would occur in the process of assigning households into the eligible group. The estimated impact on the corresponding participant is, however, likely to be biased downward, since not all of those eligible in the treatment village received the treatment. Thus we cannot interpret the estimate as average effect per participant or TOT. Our DD estimates are thus diluted due to imperfect take-up rates. However, the estimation of the effect of eligibility is one of the most important parameters to estimate, and the estimation of ITT requires less restrictive assumptions than that of TOT. ITT thus likely provides a lower bound of the size of the TOT.

1.4.2 Instrumental Variable Estimates

We estimate the TOT effect using ITT as an instrument for treatment. Indeed, policy makers or practitioners are probably more interested in the TOT parameter. We consider two measures for D_i: (i) an indicator of whether the household is a current member of microfinance (which is a binary treatment indicator); and (ii) the cumulative amount of credit borrowed (which is a continuous treatment measure).

[INSERT TABLE 1.5 HERE]

We first consider a special case of an IV estimate: the Wald estimator, which is the ratio of the two ITTs; in other words, the effect of *Z* on *Y* divided by the effect of *Z* on *D*. Table 1.5 displays the results of the Wald estimates. The first panel reports the estimated treatment effect corresponding to the log of total consumption expenditure. In the first row we present estimates of the program impact using a binary treatment indicator. The coefficient estimates are negative and statistically significant for the whole sample and for the male and female samples individually. All the coefficient estimates are positive when we restrict each group to the eligible sample. The results are similar when we change the participation measure. The second panel of Table 1.5 shows a statistically significant positive treatment effect for the eligible sub-

[17] Most of the coefficients are statistically insignificant, but are sizeable in economic terms. This issue reappears throughout the study. We suspect this result is due to sampling error. However, this problem is common even with using U.S. CPS data. For example, Card (1992) encountered the same problem in his analysis of California's 1988 minimum-wage hike. See also Hamermesh and Trejo (2000) who also encountered similar problem to analyse the effect of overtime penalty on hours work. For more details on this issue, see McCloskey and Ziliak (1996) who suggest looking at economic significance of the results instead of its statistical significance. Note also that there need not be any relationship between weak reduced form and significance for IV estimates. So the statistical significance of the IV estimates of the effects of microfinance is independent of the reduced form estimates presented here.

sample of men and women groups when we consider per capita consumption expenditure. If we look at total consumption expenditure the point estimate is stronger for eligible female borrowers. However, a stronger positive effect is observed for the eligible male sub-sample when we consider the impact on per capita consumption.

The Wald estimator is based on the assumption that nothing other than the differences in the probability of participation is responsible for differences in consumption expenditure. A more efficient estimate would exploit all the available information that both accounts for the households' decision to participate in microfinance and for the outcomes of interest. Below we estimate treatment effects using Equations (1.1) and (1.2) for various sub-samples of households based on their ownership of land.

1.4.2.1 How Participation Impacts Consumption

We present the estimated treatment effect using a binary treatment measure in the first row in each panel of Tables 1.6 and 1.7. In the top panel of each table we consider the samples of both men and women together. The middle panel reports results for female borrowers, and the bottom panel reports results for male borrowers. Consider Panel 1 in Table 1.6 where we present IV estimates of program impact of participation of men and women on (the log of) total household monthly consumption expenditure.[18] The estimated treatment effects are all positive when we limit our samples of households with land ownership of less than or equal to two acres. The results show that participation in microfinance increases household consumption expenditure by about 5 percent for all households who own two or less acres. For women, the treatment effects monotonically increase as the amount of land a household owns decreases. When we consider the full sample, the estimated impact on the log of total monthly consumption expenditure is negative. The corresponding estimates are positive, and are larger in the case of male group households for samples of two acres and one acre of land ownership, but then it gets weaker compared to the female group.

[INSERT TABLE 1.6 HERE]

The mean impacts of participation on the log of monthly per capita consumption expenditure are given in Table 1.7. The results are similar to the effects on total household consumption expenditure. For example, limiting the samples to households owning two or less acres, participation in microfinance increases the log of per capita consumption expenditure by .037.

[18] The sample used here is choice-based: program participants were oversampled relative to the population. So we use weighted IV estimates (Hirano, Imbens and Rider 2003) where each program group member receives a weight of 1, and each comparison group member receives a weight of $p/(1-p)$, where p is the propensity score. The propensity score adjustment does not alter the qualitative conclusion, which holds whether we weight or not. So we report the unweighted results here (the weighted results are available on request).

The overall results indicate that treatment effects are positive when the samples are restricted to two acres of land. But for the male group, the positive impact is observed with ownership of five or less acres of land. Again we observe monotonically increasing treatment effects for women borrowers as their landholdings decrease. The treatment effects vary with land ownership and gender of participant, and they are typically higher for the male group. It should, however, be noted that male borrowers have higher averages of credit borrowed through microfinance. They also have more members, as participants in microfinance per household and the average length of participation in microfinance is also higher. The IV estimates suggest that effects of participation on eligible households are larger than the corresponding reduced-form estimates for all households having two acres or less land. As the sample size shrinks, the estimated coefficients are less precisely estimated.[19]

[INSERT TABLE 1.7 HERE]

1.4.2.2 How Credit Impacts Consumption

A weakness of the binary treatment approach above is that it classifies all treated beneficiaries in the same way, despite the fact that some households have received significantly larger amounts of credit than others. Since the extent of the treatment varies greatly among treated households, we report results using the amount of credit borrowed as the treatment variable. The first stage involves estimating the credit demand equation using a tobit model. The coefficient of the excluded instrument (eligibility) in the first stage is highly significant, both statistically and economically. The second-stage results, using the same specification as above, are reported in the second row of each panel of Tables 1.6 and 1.7**.** The estimates are positive for samples of households owning two or less acres of land, and for males it is positive for ownership of up to five acres of land. The average value of credit borrowed by the households of two or less acres is 3,849.5 taka.[20] So the estimate in the second row of the top panel of Table 1.6 implies an increase in household total monthly consumption expenditure by about 160 taka, or 6.9 percentage points for both gender groups together. Similarly, when the samples are restricted to only eligible group members, participating households enjoy an increase of about 13.3 percent of total consumption expenditures. The estimated effects are higher for male borrowers.

The effects of credit on household per capita monthly food consumption are presented in the second row of each panel of Table 1.7. The coefficients are positive from samples that include households of less than or equal to two acres of land. For male samples, the estimates are all

[19] Combining the regression by adding dummy variables for the sex of the borrowers, or by interacting dummies for different groups of land ownership with treatment status reduce the standard errors slightly, but not significantly. We prefer separate estimation for each group of land ownership and sex of the borrowers, as it allows us to compare IV estimates with those of PSM estimates (see next section).
[20] In 1998, 35 taka =1US$ (approx.)

positive except in Column 1. In terms of magnitude, all eligible participants benefit from an increase in consumption expenditure of 13.6 percent. Using the binary treatment measure, we see that the estimated increase in consumption is 168 taka, or 7.2 percent. The corresponding increase in per capita consumption is 8.2 percent when we consider all households owning two or less acres. We obtain different program effects when we consider men and women groups separately; we see the positive effects on men and women but the size of the effects differs widely. The effects of participation or credit are negative when we consider the entire sample of participants. In general, we find slightly larger coefficient estimates (especially for men) using continuous rather than the binary treatment measures.

1.4.2.3 Treatment Intensity as the Instrument

Households living in different villages borrowed varying amounts. It appears that there is wide variation in the amount of credit borrowed by participants across different villages (Figure 1.3). Thus, the IV method can be improved upon by recognising that MFIs have been available in some villages for far longer.[21] So we can exploit the across-village variation in the intensity of treatment to capture the variation in the loan amounts across households in different villages. Explicitly, this instrument is:

$$Z = V \times E \times \text{treatment intensity}$$

where *treatment intensity* is measured by the number of years an MFI has been in a particular village. We also use interactions with *year of program placement dummies* as the instrument. In particular, we use the following instrument:

$$Z = V \times E \times \sum_t Villyear_t$$

where *Villyear* is the year dummy variable for the introduction of program in the village. Identification here relies on variation in the amount of credit borrowed across households of different villages. We report results on the effect of the log of per capita food consumption expenditure in Table 1.8. The first panel shows the coefficient estimates of the impact of microcredit using a single instrument, which is the years of program placement in a village multiplied by the indicator of eligibility status. We observe the positive program effect in all cases starting from the households owning two or less acres. The impacts typically vary between 8 and 14 percent depending on the gender of participant and samples of different land group. The effects are higher on the male group than the female group. We present the corresponding 2SLS estimates using multiple instruments in the second panel of Table 1.8. The estimates constructed using larger instrument sets differ little from those using a single instrument (in the top panel). We find statistically significant positive effects of microfinance on all borrowers owning one or less acre. In the case of the landless, the coefficients are

[21] 82% of the participants in our sample are members of a MFI for more than a year.

statistically significant for both groups, individually and jointly. All households of one acre (or less) enjoy an increase in food consumption of 13 percent for participating in a microcredit program. If we consider just the landless households, they gain more (25 percent). So our results indicate that over-identified estimates, computed using the multiple instrument set, are more precisely estimated than the just-identified estimates. However, the resulting efficiency gains are not dramatic, and the standard error of estimates falls only slightly with similar coefficient estimates. The p-values of the F-statistics (for both men and women group samples) of the over-identifying restrictions test are shown in square brackets in the bottom panel of Table 1.8. The p-values indicate that over-identifying restrictions cannot be rejected at any reasonable level for any sample of households.

[INSERT FIGURE 1.3 HERE]

[INSERT TABLE 1.8 HERE]

1.4.3 Interpreting the IV Estimates

As mentioned previously, it is not the case that all eligible households in the treatment villages participate in the microfinance program. On the other hand, some ineligible (non-encouraged) households receive the treatment. So we characterise the households affected by the IV approach. The relationship between microfinance program participation (D_i) and its effect on food consumption expenditure (Y_i) can be analysed only for the subpopulation that is affected by the instrument. Imbens and Angrist (1994), and Angrist, Imbens and Rubin (1996) (hereafter AIR), identify this subgroup of units as 'compliers,' and the resulting estimate is called the Local Average Treatment Effect (LATE). In our case, when using the binary treatment indicator, LATE is the average program effect on food consumption expenditure for those households who choose to participate in microfinance only because they are eligible to borrow. Similarly, the IV estimator exploiting more than one instrument is the average of the various single instrument LATE estimators that we obtain using each instrument separately. In this case the weights are proportional to the effect of each instrument on the treatment variable: the bigger the impact of the instrument on the regressor, the more weight it receives in the IV estimation (Angrist and Imbens 1995).[22]

The LATE-IV is based on two assumptions: the Conditional Independence Assumption (CIA) and the monotonocity assumption. The monotonocity assumption implies that anyone in the population who would take microcredit in the absence of eligibility would also take credit if they became eligible. This assumption requires that eligibility can make participation in

[22] The interpretation of LATE also applies in the case of non-binary IVs and non-binary endogenous regressors (see Angrist and Kruger 1999; Frolich 2007). In our case when D is the amount of credit borrowed, the compliance intensity can differ among units. Hence a change in Z induces a variety of different reactions in D, which cannot be disentangled. Only a weighted average of these effects can be identified. For more on this issue see Frolich (2007).

microfinance more likely, not less, and that there is no one in the eligible households who was actually denied credit (i.e., $D_i=1|Z_i=1 \geq D_i=1|Z_i=0$ or $D_i=0|Z_i=1 \leq D_i=0|Z_i=0$ for all i). This assumption means that there are no *defiers,* and that *compliers* exist. Since credit is offered for non-agricultural purposes, there is no reason to think that households choose not to participate in microcredit when they have little land, or that they would participate when they possess more land. The data also indicate that few borrowers use loans for land cultivation: only 18 percent of the respondents mentioned that some portion of the loan was used this purpose. Moreover, we do not find a significant difference between eligible and ineligible groups among those who use the loan for main agricultural activities. The CIA is based on two requirements: (i) comparison between outcomes for the households exposed to different values of Z identifies the causal impact of the instrument; and (ii) the instrument does not directly affect the outcomes. The first requirement is satisfied since eligibility status is assigned by the MFI and thus exogenous to the households. The second requirement is untestable but we have seen previously that our instrument plausibly satisfies the exclusion restriction.[23]

The LATE has very appealing properties in terms of a policy perspective and it is a well-defined economic parameter. Although our estimates capture the treatment effect only for a particular subset of participants, this subset is of great interest from the program perspective. Most of the households in our samples, and the microfinance program in Bangladesh in general, are compliers.[24] Microfinance programs are generally designed for the poor landless (or marginal landholding) households to whom our estimates apply. The IV/2SLS allows causal effects among a very particular subset of household: those affected by the eligibility criterion. Therefore, the results reported above need not generalise to the households of all participants. In a world of heterogeneous program impacts, LATE and TOT are likely to differ and these differences can be a matter of policy purposes.

1.5 Evaluation Using an Alternative Approach

1.5.1 The Propensity Score Matching (PSM) Method

Below we estimate the treatment effect using the PSM method of Rosenbaum and Rubin (1983). PSM estimators permit us to estimate the impact of a treatment on the treated, and to check the consistency of the results under different assumptions about specification and identification. The main purpose of using PSM is to examine whether our basic results hold across different

[23] AIR show that the stronger the first-stage, the less sensitive the IV estimand to the violations of the monotonicity and CIA assumptions. They also show that the smaller the proportions of defiers, the smaller the bias will be from violations of monotonicity assumption. Also if the causal effects of treatment on outcome are identical for defiers and compliers, violations of the monotonicity assumption generate no bias.

[24] Following Angrist and Chen (2007) the proportion of households that are compliers is $P[D_{i1}-D_{i0}|D_i=1]=P[E=1]\{P[D_{i1}-D_{i0}=1/P[D_i=1]\}$, where E=1 whether household is eligible, and P stands for probability. For the whole sample, this calculation is .677*(.506/.575)=.596.

evaluation methods. PSM also provides us a different parameter of interest (TOT) as opposed to IV estimates (LATE).[25]

In order to identify the TOT parameter by the PSM technique, our identifying assumption is that outcomes in the untreated state are independent of D_i conditional on a set of observable village- and household-level characteristics. Rubin (1978) refers to the treatment status that is independent of potential outcomes as an *ignorable* treatment assignment. Although claims for ignorable are usually implausible in a non-experimental setting, it is more plausible in our context that microcredit program status among the program village is ignorable conditional on land holdings and a vector of other covariates. Households in program villages that have less land and non-land assets are likely to participate more. MFI selects households on the basis of eligibility and characteristics that can be observed by a loan officer and a branch manager. It is unlikely that a loan officer, who is unfamiliar with the villagers, could observe an applicant's entrepreneurial ability and drive. However, a potential selection bias may be introduced, because some treated households are not eligible, and because not all eligible households participate in the program. The sources of bias could be the differences in observable variables in terms of household size, sex ratio, schooling, age, family composition, and other household characteristics. The survey contains information on most of the characteristics (including the reasons for participating or not participating in the program) that are potential determinants of household participation in microfinance. Given the richness of data available, we may be willing to assume conditional independence, i.e. selection bias can be eliminated using matching on the covariates.[26] In that case, identification is based on the claim that after conditioning on all observed characteristics that are known to affect participant status, participants and non-participants are comparable.

To alleviate concern regarding selection bias, we take an additional step by using the regression-adjusted matching estimator developed by Heckman, Ichimura, and Todd (1997). The research by Heckman and his co-authors, Hahn (1998) and many others suggest that regression adjustment improves the precision of the matching estimate more than by conditioning on the propensity score alone. In the regression-adjusted version the residual from the regression of Y_{0j} on a vector of exogenous covariates replaces Y_{0j} as the dependent variable in the matching. Formally, we assume a conventional linear model for outcomes in the matched comparison group $Y_o = X\beta_o + U_o$ (the regression is only run on the matched comparison group so it is not

[25] If the selection on observable assumption does not hold the PSM estimates can be interpreted as ITT rather than TOT.

[26] The work by Heckman, his co-authors and others (Dehejia and Wahba 1999, 2002; Michalopoulos, Bloom and Hill 2004) points out that matching estimators perform well when (1) the same questionnaires are used for participants and non-participants; (2) participants and non-participants reside in the same geographic area; and (3) the data contain a rich set of variables relevant to modelling the program-participation decision. Our data meet all these criteria.

contaminated by program participation). Using partial-regression methods applied to the comparison group sample, we estimate the components of $E[Y_0|X,D=0]=X\beta_0+E[U_0|X,D=0]$ imposing any desired exclusion restriction. We first use Local Linear Regression (LLR) weight and use a bi-weight kernel in estimating local linear matching.[27] We then use Nearest Neighbour Matching (NNM). In our empirical work we use the five nearest neighbours. Each of these neighbours receives an equal weight in constructing the counterfactual means. We use matching with replacement where a given non-participant is allowed to match to more than one participant.

Our sample is choice-based, with program participants oversampled relative to their frequency in the population. Therefore, we use matching on the estimate of the odds ratio. We impose the common support restriction based on the method of trimming that was suggested by Heckman, Ichimura, and Todd (1997). In addition, we exclude the 2 percent of the remaining treatment observations that show the lowest odds-ratio of the non-treated observations. We follow Heckman, Ichimura, and Todd (1997), Behrman, Cheng, and Todd (2004), and Rubin and Thomas (2000) for variable selection to estimate the propensity score and regression adjustment. We include all the variables that may affect both program participation and outcomes. We use two different specifications to estimate the propensity score as the estimates produced by matching can be quite sensitive to the choice of covariates used to construct the propensity score (see Heckman, Ichimura, Smith and Todd 1998; Heckman and Navarro-Lozano 2004). First we use a covariate specification similar to the IV specification. Then we use a more generous specification that includes the detailed household demographic and socio-economic variable and village-level characteristics. The list of the full controls is chosen from a set of larger controls, and we chose those that were most often significant in both outcome equation and estimation of the propensity score. The final list of variables included in the matching estimates is reported in Appendix 1. Since the list of observed covariates is rich, and additionally we are using regression adjustment, we may claim that we are able to reach a sufficiently plausible conclusion using a matching technique.

1.5.2 PSM Results

We estimate a standard logit model of participation to estimate propensity scores. The results, which are not reported here for reasons of brevity, indicate that program participants are more likely to be eligible households. The empirical distribution of the estimated odds-ratio of participants and non-participants are shown in Figure 1.4 using the coarser set of covariates. It can be seen that there are very few regions of non-overlapping support. For our estimation we

[27] The LLR is analogous to running a weighted regression for each program household on only a constant term using all the non-participant data. It is a nonparametric regression technique that improves upon kernel matching. It avoids the boundary points bias associated with kernel, and it can also adapt better to different data densities.

exclude non-participants in the non-overlap region, if there is any. Observations with very low or very high logs of odds-ratio values are also eliminated as they may indicate a true value of zero or one. However, as seen in Figure 1.4, we need to discard only a few observations of the treatment group.[28]

[INSERT FIGURE 1.4 HERE]

In Table 1.9 we present estimates of the treatment parameters using two different matching estimators. The dependent variable is the level of consumption expenditure, as opposed to its logarithm, since many of the coefficient estimates are very small in percentage terms. The average difference in food consumption expenditure between treated households and their non-treated counterparts provides the basis for the estimation of the TOT parameter. The first panel of Table 1.9 shows the results of both male and female groups together using two covariate specifications of the propensity score. The left side of the table reports the results based on a coarser set of covariates and the right side presents the results using the same covariate specification used in the IV estimation. The second and third panels represent the corresponding results for female and male groups of borrowers, respectively. All the results are based on the regression-adjusted covariates. Each column of Table 1.9 represents estimates based on the matched sample of households of different groups of land ownership.

[INSERT TABLE 1.9 HERE]

The results for mean impact indicate that the effect of participation on total household consumption is negative for the whole sample. All the results point out that the treatment effect is positive for those households who own less land (one acre or less). Both the LLR and NNM matching estimates give us similar results in both specifications of the propensity score estimates. We observe similar results for the female group of borrowers. However, all TOT coefficients are positive for male groups. The results are also similar to those obtained from two different matching estimators. The impact estimate is higher for male than for female borrowers. Adding controls for estimating propensity score and regression adjustment does not greatly affect the point estimates.

[INSERT TABLE 1.10 HERE]

Table 1.10 provides the coefficients of the estimated mean impact on monthly per capita food consumption expenditure using the same matching estimators. Since the results are not affected by the choice of propensity score estimation we report results based on a broader set of specifications. The results are similar to that of impact on total monthly consumption. All the estimated coefficients are positive, starting from the samples of households with one acre of

[28] It appears that the imposition of common support is not critical in our application using different sets of covariates for estimating propensity scores.

land, and for male groups the treatment effects are always positive. The size of the estimated impact varies with respect to matching estimates and the different groups of land holding households. Overall we find here a stronger coefficient for men than women.

1.5.3 Spillover Effects

Our identification strategy is based on the implicit assumption that there is no spillover effect. Formally we make the Stable Unit Treatment Value Assumption (SUTVA) which assumes that (i) the household's potential outcomes depend on its own participation only and not on the treatment status of other households; and (ii) the microfinance program only affects the outcome of those who participate, and that there is no externality from participant to non-participant. Thus it rules out peer and general equilibrium effects. So, in order to interpret our estimates as a TOT effect, the SUTVA must hold. We examine (ii) by estimating the spillover effects. Accordingly, we check whether the program affects consumption of non-treated households who live in the treatment villages. The difference in the unconditional mean household monthly food consumption expenditure between the non-treated in both program and control villages is less than 1 percent of their household monthly consumption expenditure. The difference, though in favour of households in the program village, is not statistically significant. There is also no difference in unconditional food consumption expenditure between eligible non-participants in program and control villages. To increase the precision of estimates we add a set of conditioning variables and run OLS regressions for all non-treated households where the parameter of interest is an indicator variable of whether the household lives in a treatment village. The estimated coefficient is very small and negative for the full sample of non-participants, while it is also very small but positive for the eligible sub-sample (these results are not reported here). We then use regression-adjusted matching estimates to ensure that we are comparing similar non-participants, and find no support of spillover. We also compare eligible non-treated households who have the same probability of participation, if the program was available, and also find no indication of spillover effects. Thus there is no strong evidence in favour of a positive spillover effect.[29]

1.6.1 IV versus Matching Estimates

We now compare the main results from IV and matching estimates. We see that the results are similar in terms of the sign of the coefficient estimate. Both estimation results suggest that the effects are positive for a subset of borrowers; those having less land. In terms of the magnitude

[29] However, to the extent that the change in behaviour and therefore the resulting program impacts among the treated influence their peers within the group, we are not correct in claiming that there are no spillover effects. In the presence of violation of SUTVA, our estimates are the lower bound to the true effects.

of the coefficients, the matching estimates are substantially smaller than the IV estimates.[30] The estimated standard errors are larger in the case of matching than the IV estimates. Therefore, unlike IV estimates that show modest positive effects on the consumption of eligible or other poorer participants, matching estimates leads to smaller positive and statistically insignificant effects on consumption. The lack of statistical significance in the coefficient estimates is partially the results of the smaller sample size (see also Footnote 14). The divergence between IV and PSM estimates in terms of standard error is probably best explained by differences in the heterogeneity among households. The matching estimator combines propensity score weighting schemes to estimate the TOT. Households most likely to participate get the highest weights in matching estimates. On the other hand, IV produces covariate-specific variance-weighted average effects. The two weighting schemes are likely to cause different estimates (Angrist and Krueger 1999).[31] Moreover, our matching estimates only consider the *extensive* margin (where all treated households are classified in the same way) but not the *intensive* margin (intensity of the treatment). Treating differently treated households as the same, as a binary approach would do, thus seems likely to understate the potential effect of full treatment of microcredit.[32, 33]

The two estimates produce results for two different subgroups of borrowers. The IV estimate applies to a smaller treatment group than the matching estimates. The larger coefficient estimates by IV rather than matching implies that the impacts of microfinance for the 'compliers' are higher than 'always-takers.' This result might be counterintuitive in the sense that the treatment effect for the marginal group (poorer households) should be smaller than the average treatment effect on the treated. However, this need not be the case here because IV estimates the impact of the program for those households that are more credit constrained and/or have greater immediate need to improve their consumption. They are also more likely to participate in a microcredit program. As a result, it is possible that gains from participation are higher for these households.

While IV is a standard technique for non-experimental impact estimates, recent evidence in favour of matching is compelling. However, there is no guarantee that selection on observables will eliminate the total bias, unless they go in the same direction. As a result, matching estimates may still be biased if there are any latent factors correlated with participation decision

[30] Since IV estimates are all in terms of logarithms we multiply IV estimates by household total consumption expenditures in order to compare the IV with the matching estimates.
[31] See Heckman and Navarro-Lozano (2004) for more details for comparisons between matching and IV based on treatment parameters.
[32] PSM techniques are generally confined to binary treatment scenarios. However, some possible extensions have been suggested. For example, Hirano and Imbens (2004) develop a generalized propensity score method when treatment is defined as a continuous variable.
[33] Angrist (1998) finds larger standard error of estimates in covariate matching than the estimates obtained using IV. Zhao (2006) compares the performance of PSM and covariate matching estimators, and finds that PSM estimators have larger standard errors. So our results are consistent with both Angrist and Zhao.

and counterfactual outcomes.[34] The IV method can overcome these problems. Under IV assumptions and the assumed functional form, IV estimation robustly identifies the causal effect to unobserved heterogeneity. However, the IV estimates are valid for the group of compliers and may not be informative for the other group (always-takers).[35]

1.6.2 Summary and Conclusion

Using different non-experimental impact evaluation techniques we find similar results concerning the impact of microfinance. We use different instruments and our results are robust to the use of instruments. Overall, the results indicate that they do not entirely depend on different specification and identifying assumptions. We also estimate the heterogeneous treatment effect by estimating the sub-group specific mean treatment effect where the groups are categorised based on the targeting criterion. The results indicate that there is substantial heterogeneity in the causal impact of participation in microfinance.

The overall results suggest that the effect of microfinance on household consumption expenditure does not seem to be strong. This raises doubt about whether microfinance can be a first-track poverty reduction program. The IV estimates indicate an increase of 6 to 14 percent in the consumption expenditure of the relatively poor participating households. Overall, results signify that, conditional on positive impacts, stronger coefficient estimates are observed for male participants. However, male participants borrow more, so larger treatment impact could be the results of increasing returns in household enterprises. The results for men are based on a very small sample size, and should be interpreted with caution. Note that these results are not directly comparable with PK's study, since we are considering different set of MFIs, and there is no overlap in the households/MFIs evaluated by these two studies.

In general, we find an inverse relationship between household land ownership and the benefits from participation in microcredit program: the less land a household has, the stronger the effect of participation in microfinance. The benefits are lower, or sometimes even negative, for those households marginal to the participation decision. The results indicate that the effects of microfinance loans are not strong across all groups of poor households. Rather, those among the

[34] Blundell, Dearden and Sianesi (2005) argue that the plausibility of the selection on observable assumption of PSM method should always be discussed on a case-by-case basis. In our case, the assumption seems reasonable due to informational richness of the data, and the simple mechanism (land-based eligibility criterion) for participation into the program. However, there are some complexities as well since a number of households from the ineligible group received treatment and some eligibles did not take up the program. So, the divergence between IV and PSM could also be due to unobserved heterogeneity in the selection process.

[35] Our results indicate that the concern regarding selection bias in non-experimental data can be less problematic if the researcher establishes good interactions with borrowers and providers before evaluating the program. In particular, one needs to know how MFI selects villages and households, and why households enter programs. It is, however, to be noted that we do not rely on (regression-adjusted) matching results to conclude our findings since the possibility of selection based on observables could still be questionable. We do not use standard PSM estimate, and we think that the PSM results are at least indicative.

poorest of poor participants are most likely to benefit from participating.[36] The results also imply that microcredit loans may not be effective for land-rich households. Moreover, they are not the focus of the microcredit loans as these groups are not officially eligible. They are also less likely to participate in a microfinance program. The findings indicate that the simple targeting mechanism of microfinance program in Bangladesh, based on household land ownership, is effective. Hence the efficiency of the microfinance program can be enhanced by allocating credit to those for whom the impact is the greatest: the poorest marginal landholding households.

[36] The results may have different interpretations. For example, credit may induce poorer households to increase their consumption while it may not have any sizeable effect on consumption of a relatively less poor because they might invest their money on a long-term project. However, MFI requires that loans to invested and be repaid within a year with payment start about four weeks later upon receiving the loan. It may still be possible that the poorer households use their loan more to augment consumption. My field visits do not support the claim. Also the survey data regarding the use of loans indicate that more than 90 percent of all treated households use loan for productive investment and that there is no difference between poorer and less poor households in this respect. Moreover, MFIs monitor the use of loan, and because of the repayment concern (unlike cash transfer program like Progressa) almost in each week, households cannot sustain their higher level consumption without investing money.

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Table 1.1- Village-Level Descriptive Statistics

Variable	Control village (I)	Program village (II)	Difference III=(II-I)	t-stat
Education Facilities				
Primary school	90.91	86.25	-4.66	0.42
Secondary school	27.27	31.25	3.98	0.26
Maktab/Madrasa (Religious School)	81.82	90.00	8.18	0.80
Health Facilities				
Union health centre	10	17.5	7.5	-0.59
Allopathic doctor	50	42.5	-7.5	0.45
Homeopath doctor	40	38.75	-1.25	0.08
Transport, Communication and Infrastructure				
Electricity connection	17	26	9	3.2
Presence of pucca road	10.6	34.8	24.2	8.4
Distance to nearest Thana (in km)	11.91	7.14	-4.77	-2.07
Presence of grocery market	18.2	22.5	4.3	0.33
Presence or absence of frequent haat (big market)	27.3	32.5	5.2	0.35
Presence of bus stand	9.1	15	5.9	0.59
Presence of post office	18.2	20	1.8	0.14
Presence of telephone office	9.1	6.3	-2.8	-0.3
Presence of Union Parishad (local Government) office	18.2	13.8	-4.4	-0.35
Irrigation Equipment				
Number of low lift pump	0.27	0.44	0.16	0.23
Number of shallow tube-well	11.82	12.13	0.31	0.05
Number of hand tube-well for drinking water	68	78.04	10.04	0.39
Credit-related options				
Percentage of crop received by land owner in sharecropping	49.55	47.53	-2.02	0.96
Number of people who provides advances against crops	2.73	3.85	1.12	0.79
Number of small credit/savings groups	0.91	0.76	-0.15	-0.39

Notes: The first column of this table presents the mean of each variable for the control villages, and the second presents the same for the treatment villages. The third column presents the difference between the two, and the fourth provides the *t*-statistics for the mean difference of participating and non-participating households.

Table 1.2- Selected Descriptive Statistics of Households

Variable	All Sample					Samples of Eligible Households				
	Non-participant (I)	Participant (II)	Difference III=(II-I)	p-value (IV)	K-S	Non-participant(I)	Participant(II)	Difference III=(II-I)	p-value (IV)	K-S
Age of household head	45.14	43.91	-1.224	0.013	0.000	41.18	41.66	0.481	0.393	0.001
Sex of household head	0.93	0.95	0.026	0.003	0.701	0.91	0.94	0.034	0.004	0.609
Marital status of household head	0.90	0.93	0.033	0.001	0.386	0.88	0.91	0.031	0.022	0.735
Whether household head is illiterate	0.35	0.31	-0.049	0.005	0.060	0.44	0.33	-0.103	0.000	0.000
Whether household head can sign only	0.22	0.34	0.115	0.000	0.000	0.23	0.37	0.134	0.000	0.000
Whether household head can read only	0.01	0.01	0.003	0.394	1.000	0.01	0.01	0.001	0.736	1.000
Whether household head can read and write	0.42	0.35	-0.069	0.000	0.002	0.33	0.29	-0.032	0.120	0.682
Highest education achieved by any member	5.72	5.26	-0.453	0.003	0.000	4.28	4.47	0.195	0.262	0.001
Highest education achieved by any male member	5.17	4.59	-0.583	0.000	0.000	3.72	3.78	0.065	0.719	0.052
Highest education achieved by any female member	3.52	3.19	-0.323	0.013	0.001	2.57	2.69	0.125	0.388	0.004
Total arable land owned by household	80.51	58.37	-22.1	0.000	0.000	7.12	7.46	0.340	0.583	1.000
Household size	5.45	5.67	0.220	0.009	0.000	5.03	5.40	0.364	0.000	0.000
Number of children age below 6 years	0.87	0.91	0.035	0.295	0.617	0.93	0.95	0.022	0.590	0.562
Number of children of age 6-15	1.29	1.53	0.237	0.000	0.000	1.21	1.45	0.240	0.000	0.000
Number of old people of age above 60 years	0.29	0.21	-0.077	0.000	0.004	0.22	0.17	-0.051	0.009	0.276
Number of 15-60 years old male member	1.59	1.59	-0.004	0.906	0.672	1.37	1.46	0.088	0.028	0.232
Number of 15-60 years old female member	1.41	1.43	0.029	0.288	0.794	1.30	1.36	0.064	0.031	0.120
Number of male member in the family	2.90	2.96	0.056	0.324	0.057	2.63	2.78	0.152	0.019	0.027
Number of female member in the family	2.55	2.72	0.164	0.001	0.005	2.41	2.62	0.212	0.000	0.004
Lives in a nuclear family (=1) or joint family (=0)	0.67	0.69	0.020	0.236	0.921	0.69	0.72	0.026	0.208	0.896

Notes: The Table gives the summary statistics of the participant and non-participant households. The left-hand side presents the results for the full sample and the right-hand side shows the same results for eligible households. Reported p-values are the two-tailed tests of the null hypothesis that program and control group means are equal. The *K-S* test column is based on the Kolmogorov-Smirnov test of equality of distribution.

Table 1.3- Summary Statistics of Consumption and Credit Variables

Variable	Men (I)	Women (II)	Difference (I-II)	P-value	K-S
Total of amount credit (taka)	4650.9	3799.4	851.6	0.000	0.015
	(3961.7)	(2115.2)	(176.1)		
Total length of membership (in years)	4.1	3.3	0.8	0.000	0.001
	(3.4)	(2.7)	(0.2)		
Number of borrowers per household	1.4	1.1	0.3	0.000	0
	(0.6)	(0.3)	(0.0)		
Household total monthly consumption	2783.6	2365.0	418.5	0.001	0.003
	(2192.3)	(1723.2)	(130.4)		
Household per capita consumption	497.6	436.5	61.1	0.013	0.002
	(394.0)	(325.4)	(24.3)		
Number of observations	213	1565			
	Participant	Non-participant	Difference	P-value	K-S
Household monthly food consumption (taka)	2415.1	2456.6	41.5	0.730	0
	(1801.1)	(1890.9)	(67.7)		
Household monthly per capita food consumption (taka)	443.3	467.7	24.4	0.049	0.022
	(336.1)	(337.4)	(12.4)		
Household monthly food consumption in program village (taka)	2417.8	2461.8	44.1	0.550	0.112
	(1806.9)	(1942.7)	(73.6)		
Per capita monthly food consumption in program village (taka)	444.3	477.8	33.5	0.014	0.028
	(337.0)	(358.3)	(13.7)		

Notes: The top panel represents the descriptive statistics of the selected variables for men and women participants in microfinance. The bottom panel gives the descriptive statistics of the treatment and comparison households. Reported p-values are the two-tailed tests of the null hypothesis that column II and I are equal. (K-S) based on Kolmogorov-Smirnov test of equality of distribution, Standard errors are in parenthesis

Table 1.4- Difference-in-Difference Estimates: The Impacts of Eligibility

Dependent Variable: Household Log of Total Monthly Food Consumption Expenditure						
Estimated Coefficient	Household Land Ownership					
	All sample	Land ≤500	Land ≤200	Land ≤100	Land ≤50	Landless
δ_1	-0.015	-0.021	-0.051	-0.027	0.038	0.051
	(0.046)	(0.047)	(0.054)	(0.077)	(0.034)	(0.040)
δ_2	-0.104	-0.0983	-0.0788	-0.0361		
	(.0520)**	(.0549)+	(.0644)	(.0866)		
δ_3	0.0471	0.0532	0.0826	0.0628		
	(.0523)	(.0531)	(.0588)	(.0799)		
$\delta_1+\delta_3$	0.0321	0.0322	0.0316	0.0358	0.038	0.051
Dependent Variable: Log of Per Capita Monthly Food Consumption Expenditure						
δ_1	-0.023	-0.026	-0.048	-0.025	0.039	0.048
	(0.046)	(0.047)	(0.054)	(0.077)	(0.035)	(0.041)
δ_2	-0.104	-0.092	-0.068	-0.0337		
	(.0522)**	(.0550)+	(.0647)	(.0872)		
δ_3	0.0541	0.057	0.0794	0.0592		
	(.0525)	(.0532)	(.0591)	(.0804)		
$\delta_1+\delta_3$	0.0311	0.031	0.0314	0.0342	0.039	0.048

Notes: Clustered standard errors are reported in parentheses, + significant at 10%; ** significant at 5%. Coefficients are those from estimation of reduced form Equation (1.3). Regressions also include household- and village-level characteristics and MFI fixed effects.

Table 1.5- Wald Estimates of Impacts of Microfinance

Dependent Variable: Household Log of Total Monthly Food Consumption Expenditure						
Treatment Variable	All	All eligible	Women	Eligible Women	Men	Eligible Men
Whether treated or not	-1.345	0.249	-1.313	0.219	-1.434	0.102
	(0.146)*	(0.292)	(0.148)*	(0.295)	(0.146)*	(0.342)
Total amount of credit†	-0.8746	0.1616	-0.8430	0.1381	-0.8864	0.0631
	(0.0605)*	(0.1183)	(0.0620)*	(0.11589)	(0.0896)*	(0.1385)
Dependent Variable: Log of Per Capita Monthly Food Consumption Expenditure						
Participation Variable	All	All eligible	Women	Eligible Women	Men	Eligible Men
Whether treated or not	-0.621	0.312	-0.633	0.273	-0.552	0.386
	(0.121)*	(0.258)	(0.122)*	(0.260)	(0.190)*	(0.264)
Total amount of credit†	-0.4042	0.2024	-0.4080	0.1722	-0.3419	0.2392
	(0.0522)*	(0.1020)**	(0.0535)*	(0.0997)+	(0.0752)*	(0.1173)**

Notes: Each cell in this table corresponds to a separate regression. The first row in each panel represents regression of log of consumption expenditure on a dummy for treatment status using eligibility status as instrument for treatment. The second row of each panel reports the corresponding estimated coefficients using continuous treatment measure (the amount of credit borrowed). Regressions do not include any other covariate. Clustered standard errors are reported in parentheses, + significant at 10%; ** significant at 5%; * significant at 1%. † Coefficient estimates and the corresponding standard errors are multiplied by the average amount of credit borrowed by households of the respective group of land ownership (assuming constant marginal benefit from the credit).

Table 1.6- IV Estimates of Impact of Microfinance on Household Consumption

(Dependent variable: Household Log of Total Monthly Food Consumption Expenditure)

	Household Land Ownership (in decimal)						
Both Men and Women Treatment Variable	All sample	Land ≤500	Land ≤200	Land ≤100	Land ≤50	Landless	Adjusted R^2
Whether treated or not	-0.216	-0.131	0.049	0.128	0.126	0.156	(0.46-0.49)
	(0.102)**	(0.113)	(0.129)	(0.140)	(0.164)	(0.193)	
Amount of credit†	-0.1282	-0.0660	0.0692	0.1173	0.1335	0.1745	(0.46-0.49)
	(0.0660)+	(0.0738)	(0.0885)	(0.0984)	(0.1186)	(0.1374)	
Mean Consumption (taka)	2432.7	2384.3	2319.1	2228.9	2126.3	2082.8	
Observations	3026	2960	2780	2462	2034	1471	
Women							
Whether treated or not	-0.21	-0.157	0.027	0.109	0.149	0.218	(0.46-0.49)
	(0.107)+	(0.116)	(0.132)	(0.144)	(0.167)	(0.197)	
Amount of credit†	-0.1308	-0.0978	0.0357	0.0830	0.1160	0.1886	(0.46-0.49)
	(0.0688)+	(0.0754)	(0.0880)	(0.0976)	(0.1191)	(0.1387)	
Mean consumption (taka)	2406.1	2359.5	2302.8	2216.5	2108.0	2067.3	
Observations	2813	2755	2591	2299	1904	1377	
Men							
Whether treated or not	-0.151	-0.013	0.146	0.21	0.051	0.124	(0.52-0.55)
	(0.154)	(0.168)	(0.190)	(0.210)	(0.248)	(0.299)	
Amount of credit†	-0.0360	0.0849	0.2602	0.2898	0.2299	0.2520	(0.52-0.55)
	(0.122)	(0.1340)	(0.1500)+	(0.1548)+	(0.1753)	(0.2159)	
Mean consumption (taka)	2505.2	2436.8	2350.0	2215.4	2120.8	2085.2	
Observations	1461	1420	1305	1127	922	673	

Notes: Each cell in this table corresponds to a separate regression. The first row in each panel represents a separate regression of log of total household monthly consumption expenditure on a dummy for treatment status, controlling for household- and village-level characteristics and MFI fixed effects, and using eligibility status of household as instrument for treatment indicator. The second row of each panel reports the corresponding estimated coefficients using continuous treatment measure (the amount of credit borrowed). Clustered standard errors are reported in parentheses, + significant at 10%; ** significant at 5%. † Coefficient estimates and the corresponding standard errors are multiplied by the average amount of credit borrowed by households of the respective group of land ownership (assuming constant marginal benefit from the credit).

Table 1.7- IV Estimates of Impact of Microfinance on Per-capita Consumption

(Dependent Variable: Log of Per-capita Monthly Food Consumption Expenditure)

Both Men and Women	Household Land Ownership(in decimal)						Adjusted
Treatment Variable	All sample	Land ≤500	Land ≤200	Land≤100	Land ≤50	Landless	R^2
Whether treated or not	-0.203	-0.119	0.037	0.080	0.072	0.086	(0.26-0.28)
	(0.103)**	(0.113)	(0.129)	(0.141)	(0.165)	(0.197)	
Amount of credit[1]	-0.1071	-0.0437	0.0826	0.1109	0.1361	0.1670	(0.26-0.28)
	(0.0677)	(0.0756)	(0.0885)	(0.0977)	(0.1196)	(0.1415)	
Per capita consumption (taka)	453.7	449.9	444.4	435.2	423.6	415.1	
Observations	3026	2960	2780	2462	2034	1471	
Women							
Whether treated or not	-0.177	-0.14	0.019	0.066	0.104	0.151	(0.25-0.28)
	(0.108)	(0.116)	(0.133)	(0.145)	(0.168)	(0.201)	
Amount of credit[1]	-0.098)	-0.0734	0.0494	0.0784	0.1234	0.1809	(0.25-0.28)
	(0.0690)	(0.0756)	(0.0884)	(0.0983)	(0.1199)	(0.1412)	
Per capita consumption (taka)	450.5	446.8	442.4	433.2	420.1	413.1	
Observations	2813	2755	2591	2299	1904	1377	
Men							
Whether treated or not	-0.143	0.014	0.171	0.188	0.038	0.099	(0.30-0.33)
	(0.156)	(0.170)	(0.192)	(0.213)	(0.252)	(0.309)	
Amount of credit†	-0.0142	0.1256	0.3015	0.2992	0.2443	0.2470	(0.30-0.33)
	(0.1230)	(0.1352)	(0.1515)**	(0.1571)+	(0.1782)	(0.2229)	
Per capita consumption (taka)	472.8	466.1	459.2	444.7	437.5	427.2	
Observations	1461	1420	1305	1127	922	673	

Notes: Each cell in this table corresponds to a separate regression. The first row in each panel represents a separate regression of log of per capita monthly consumption expenditure on a dummy for treatment status, controlling for household- and village-level characteristics and MFI fixed effects, and using eligibility status as instrument for treatment. The second row of each panel reports the corresponding estimated coefficients using continuous treatment measure (the amount of credit borrowed). Clustered standard errors are reported in parentheses, + significant at 10%; ** significant at 5%. † Coefficient estimates and the corresponding standard errors are multiplied by the average amount of credit borrowed by households of the respective group of land ownership (assuming constant marginal benefit from the credit).

Table 1.8- 2SLS Estimates of Impact of Participation in Microfinance

(Dependent Variable: Household Log of Per-Capita monthly Food Consumption Expenditure)

Instrument: $V\times$E $\times$the number of years in microfinance in program village							
	Household Land Ownership (in decimal)						
	All sample	Land ≤500	Land ≤200	Land ≤100	Land ≤50	Landless	Adjusted R^2
All	-0.0645	-0.0080	0.0812	0.1183	0.0579	0.1360	(0.26-0.28)
	(0.0671)	(0.07160	(0.0785)	(0.0838)	(0.0956)	(0.1118)	
Women	-0.0491	-0.0186	0.0674	0.1039	0.0583	0.1440	(0.26-0.28)
	(0.0702)	(0.0741)	(0.0813)	(0.0866)	(0.0988)	(0.1156)	
Men	-0.0481	0.0684	0.2044	0.1883	0.1357	0.3045	(0.31-0.33)
	(0.1234)	(0.1319)	(0.1431)	(0.1447)	(0.1572)	(0.1959)	
Instrument: $V\times$E $\times$ separate dummies for each year in microfinance in program village							
	All sample	Land ≤500	Land ≤200	Land ≤100	Land ≤50	Landless	Adjusted R^2
All	-0.0323	0.0187	0.1040	0.1304	0.1127	0.2467	(0.26-0.28)
	-.0581	-.0625	.0689	(.07.33)+	-.0813	(.0952)*	
F-test	[p= 0.008]	[p= 0.004]	[p= 0.000]	[p= 0.000]	[p= 0.000]	[p= 0.000]	
Women	-0.0241	.0001	0.0813	0.1094	0.0969	0.2446	(0.26-0.28)
	(0.0588)	(0.0624)	(0.0690)	(0.0740)	(0.0823)**	(0.0962)**	
Men	-0.0252	0.07257	0.1834	0.1677	0.1428	0.3014	(0.31-0.33)
	(0.1053)	(0.1129)	(0.1222)	(0.1221)	(0.1279)	(0.158012)+	

Notes: Each cell in this table corresponds to a separate regression of log of per capita monthly consumption expenditure on amount of credit borrowed as treatment variable, controlling for household- and village-level characteristics and MFI fixed effects. Clustered standard errors are reported in parentheses, + significant at 10%; ** significant at 5%; * significant at 1%. All the Coefficient estimates and the corresponding standard errors are multiplied by the average amount of credit borrowed by households of the respective group of land ownership (assuming constant marginal benefit from the credit). The F-test is for whether the coefficients on the excluded instruments are jointly equal to zero, conditional on all other controls.

Table 1.9- Matching Estimates of Impact of Participation in Microfinance

(Dependent Variable: Household Total Monthly Food Consumption Expenditure (in Taka))

Regression Adjusted Estimates	(Estimation based on full set of covariates)						(Estimation based on IV set of covariates)					
	All sample	Land ≤500	Land ≤200	Land ≤100	Land ≤50	Landless	All sample	Land ≤500	Land ≤200	Land ≤100	Land ≤50	Landless
Both Women and Men												
Local Linear	-17.01	-4.47	-16.88	19.47	37.65	50.01	-20.66	-4.85	8.20	40.39	30.50	38.47
	(68.57)	(69.97)	(70.61)	(62.09)	(65.17)	(72.04)	(77.34)	(70.81)	(91.36)	(70.04)	(67.51)	(88.65)
Nearest 5-neighbour	-31.93	32.29	-8.75	37.85	53.33	49.14	-14.97	51.84	-5.66	34.92	23.23	45.41
	(75.24)	(73.67)	(74.03)	(76.54)	(73.47)	(85.16)	(74.37)	(71.56)	(73.20)	(72.45)	(76.41)	(80.60)
Women												
Local Linear	-42.28	-28.20	-7.19	28.51	28.24	32.11	-44.30	-32.45	-0.29	32.17	17.31	4.24
	(93.45)	(83.39)	(95.16)	(64.28)	(65.23)	(77.64)	(73.11)	(72.80)	(82.97)	(70.57)	(73.18)	(81.80)
Nearest 5-neighbour	-33.84	-29.18	74.89	-0.39	18.05	19.72	-30.70	-29.20	22.22	24.34	67.60	20.37
	(80.53)	(74.89)	(76.64)	(75.90)	(71.48)	(84.20)	(76.84)	(74.16)	(76.57)	(77.56)	(72.03)	(77.24)
Men												
Local Linear	90.00	85.98	137.47	156.04	322.13	302.08	162.55	220.00	128.54	160.94	249.43	249.22
	(175.26)	(187.78)	(191.44)	(161.95)	(207.65)	(219.44)	(188.98)	(182.37)	(196.29)	(173.84)	(168.08)	(229.54)
Nearest 5-neighbour	89.40	69.98	83.81	51.61	197.60	301.09	189.42	193.38	208.08	181.48	208.84	274.78
	(161.10)	(165.51)	(169.77)	(175.04)	(211.81)	(269.22)	(138.03)	(155.51)	(146.19)	(157.92)	(209.66)	(221.80)

Notes: Bootstrapped standard errors are shown (in parentheses) for local linear estimator. They are based on 100 replications with 100% sampling. Standard errors for the nearest neighbour estimator are based on Abadie and Imbens (2006). In the estimation of LLR matching the densities were estimated using a bi-weight kernel and a fixed bandwidth of 0.06. IV set of covariates include those variables included in X in the estimation of Equation (1.1). The full set of covariates includes a coarser set of specifications listed in the Appendix 1. All the coefficients estimation is carried out using regression-adjusted version of the corresponding matching estimator.

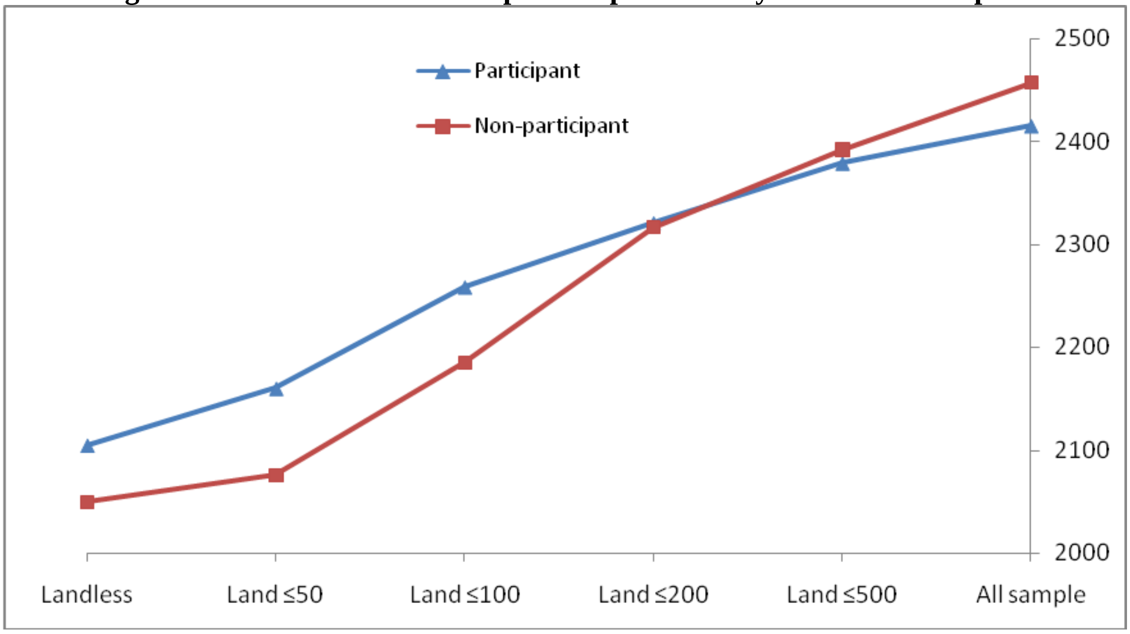
Table 1.10- Matching Estimates of the Impact of Participation in Microfinance

(Dependent Variable: Household Monthly Per-capita Food Consumption Expenditure (in Taka))

Regression Adjusted Estimates of	(Estimation based on full set of covariates)					
	All sample	Land ≤500	Land ≤200	Land ≤100	Land ≤50	Landless
	Both Women and Men					
Local Linear	-1.26	0.70	-1.34	7.10	8.08	5.79
	(14.67)	(14.52)	(15.08)	(15.86)	(16.20)	(19.12)
Nearest 5-neighbour	-10.53	7.29	0.77	7.23	10.66	7.36
	(15.40)	(15.31)	(16.04)	(16.68)	(16.73)	(20.21)
	Women					
Local Linear	-4.56	-1.49	1.38	10.44	8.15	9.39
	(15.23)	(15.03)	(15.72)	(16.54)	(16.65)	(20.35)
Nearest 5-neighbour	-2.66	-1.20	1.76	13.28	6.67	6.52
	(16.40)	(15.96)	(16.38)	(16.72)	(16.27)	(19.79)
	Men					
Local Linear	10.11	14.35	12.43	27.22	59.91	26.58
	33.43	32.83	32.98	42.93	63.56	43.14
Nearest 5-neighbour	15.81	14.60	5.23	7.68	27.75	18.20
	(31.60)	(33.60)	(33.90)	(34.06)	(41.14)	(49.05)

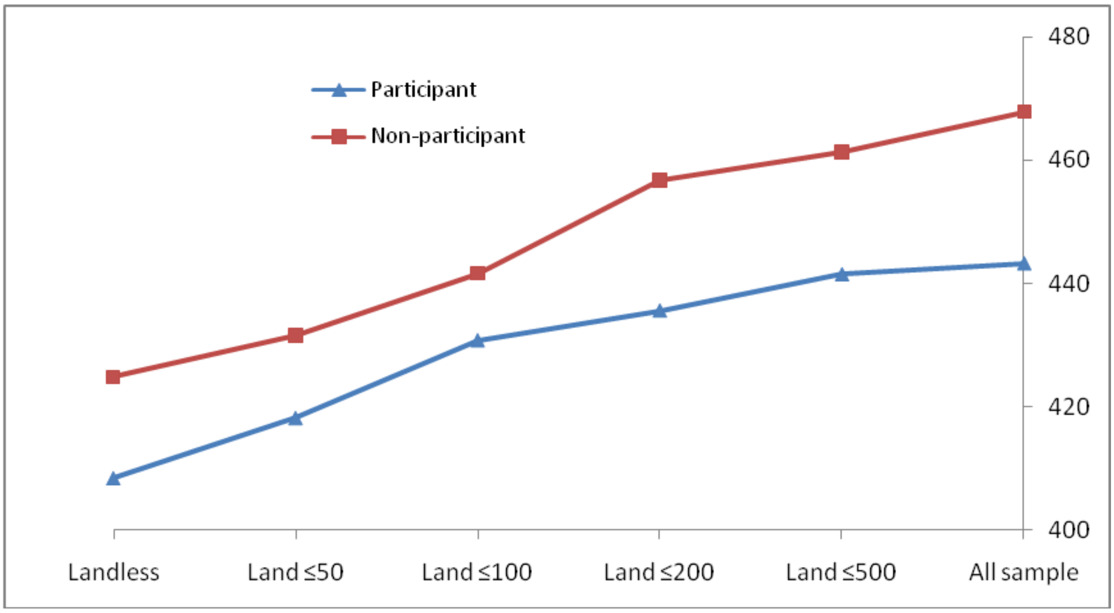
Notes: Bootstrapped standard errors are shown (in parentheses) for local linear estimator. They are based on 100 replications with 100% sampling. Standard errors for the nearest neighbour estimator are based on Abadie and Imbens (2006). In the estimation of LLR matching the densities were estimated using a bi-weight kernel and a fixed bandwidth of 0.06.The full set of covariates includes a coarser set of specifications listed in the Appendix 1. All the coefficients estimation is carried out using regression-adjusted version of the corresponding matching estimator.

Figure 1.1a- Household Consumption Expenditure by Land Ownership



Notes: Figure shows regression un-adjusted household monthly consumption expenditure (in taka) by land ownership. Land ≤ 50 implies household who own less than or equal to 50 decimal (half an acre) land, and so on.

Figure 1.1b- Per-capita Consumption Expenditure by Land Ownership



Notes: Figure shows regression unadjusted per capita consumption expenditure (in taka) by land ownership. Land ≤ 50 implies household who own less than or equal to 50 decimal (half an acre) land, and so on.

Figure 1.2a- Participant Rate by Household Land Holding

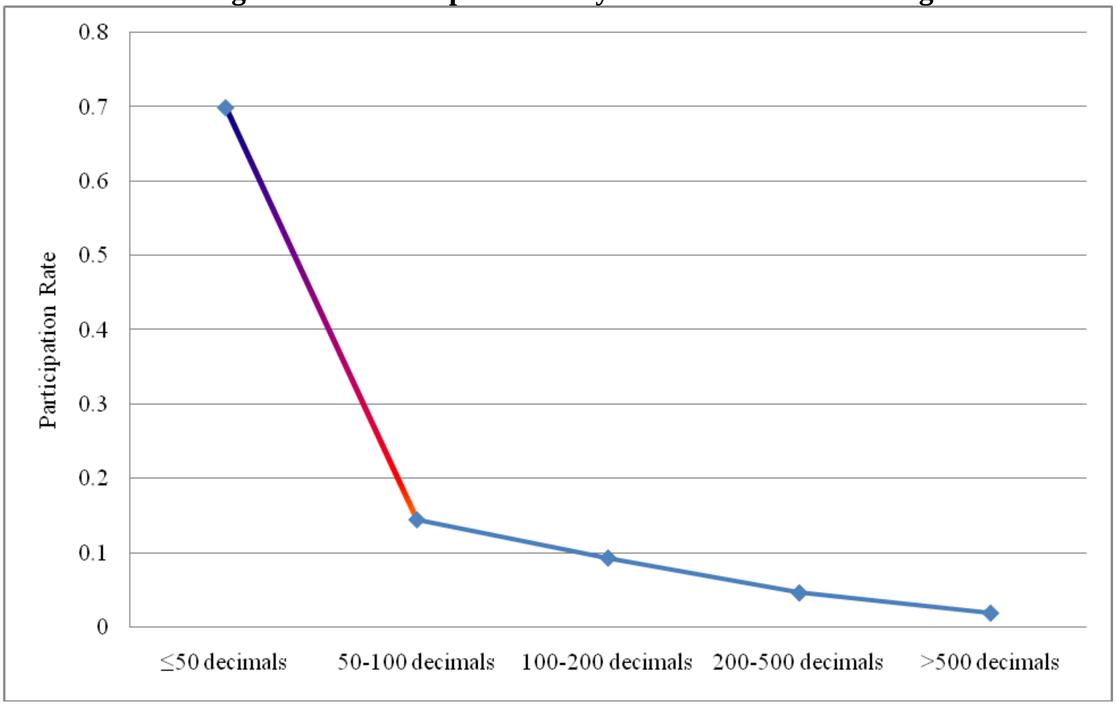
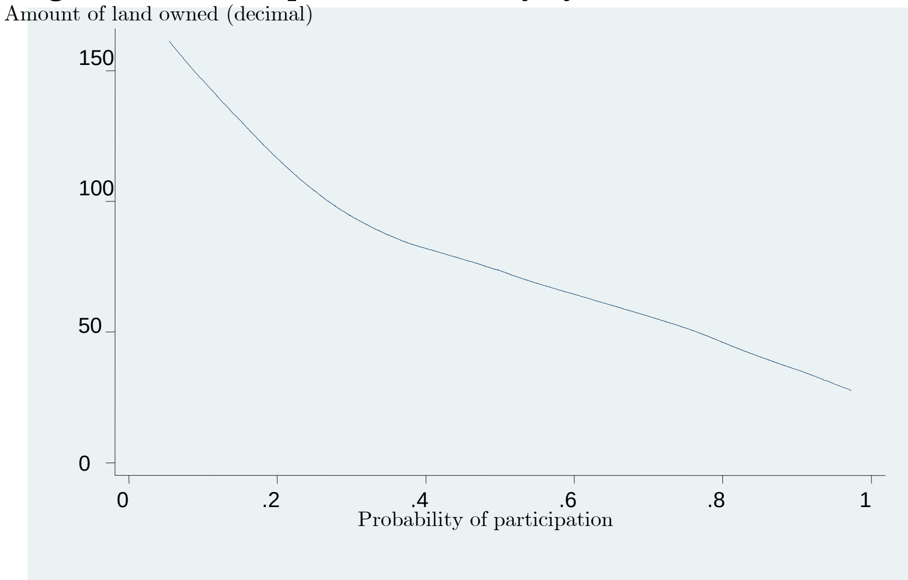
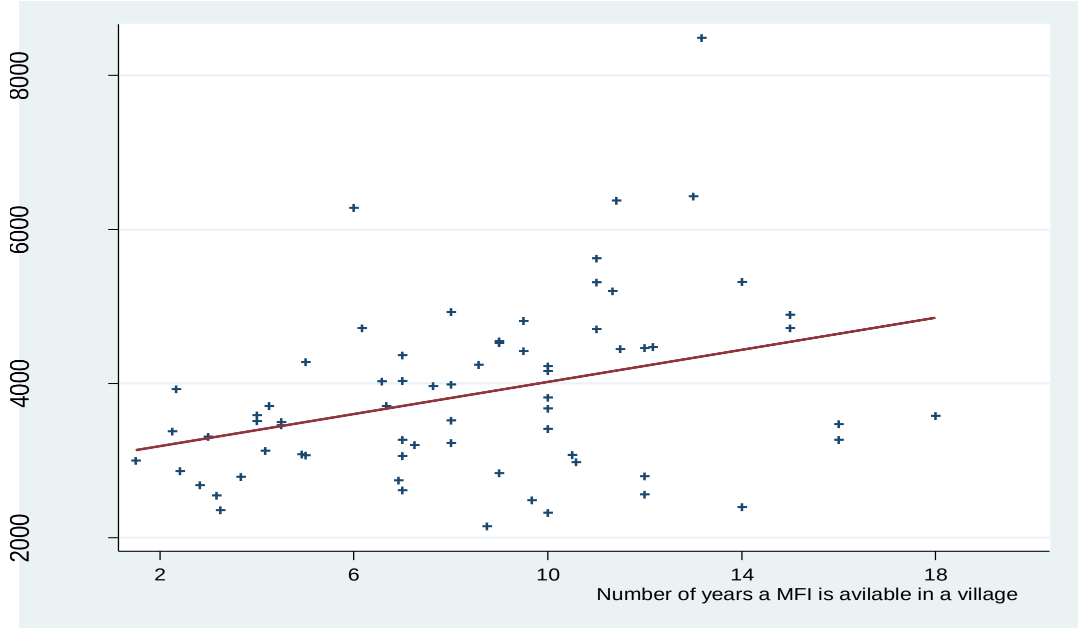


Figure 1.2b- Participation Probability by Household Land Ownership



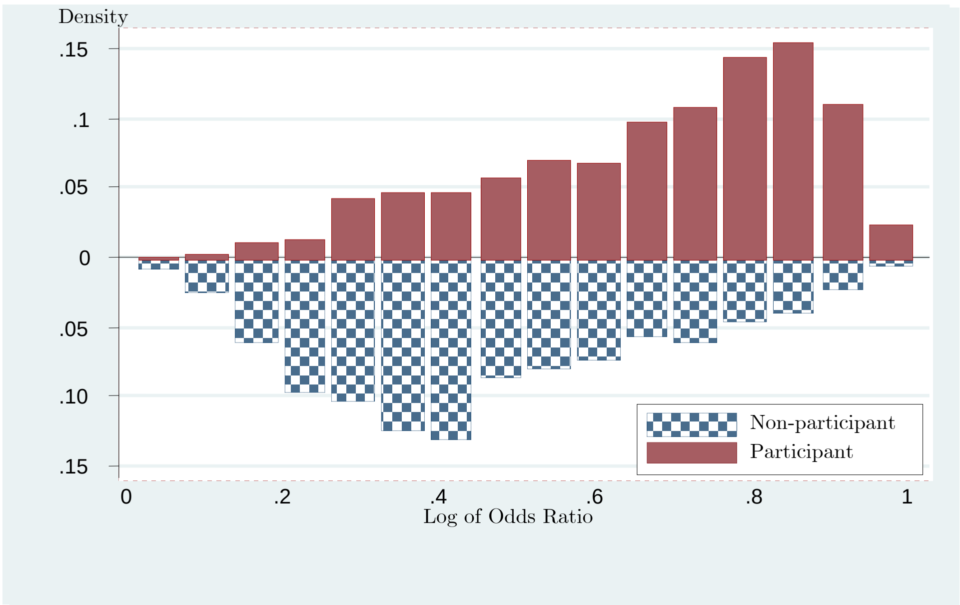
Notes: Figure shows lowess locally weighted regression of the amount of land owned by households on their probability of participation in microfinance (using quartic kernel with bandwidth of 0.8). Regression-adjusted probability of participation is obtained by conditioning on household- and village-level characteristics, MFI fixed effects, and instrument (eligibility status of household in program village)

Figure 1.3- Yeas of Microfinance Program in a Village and the Amount of Credit Borrowed by Households



Notes: Average credit per household in a village is the amount of credit borrwed (in taka) by all households divided by the number of participating households in a program village. Number of years a MFI is available in a village is the period from which microfinance is first avilable in a program village.

Figure 1.4- The Empirical density of the log of odds-ratio



Appendix 1

List of Variables:

Variables used in IV estimation and Program Participation Model

Household-level variables:

Age of household head, Square of the age of household head, Sex of household head, Marital status of household head, Education level of household head and spouse (illiterate, can sign only, can read only, can read and write), Whether household head has spouse, Highest grade achieved by a member in the household, total arable land owned by household, Number of children age below 6 years, age 6-15, Dependency ratio, Number of 15-60 years old male and female member, Type of family (joint family or semi-nuclear, nuclear), Dummies for occupation of the household head (farmer, agricultural labour, non-agricultural labour, self-employed or businessman, professional or salaried job holder, any other job), Electricity connection, Number of living room (beside bathroom/kitchen), If cement or brick used in any of the living room, Whether condition of house is good, liveable, or dirty, Whether household has separate kitchen, toilet facility.

Village-level variables:

Presence or absence of primary school, secondary school or college, health facility, Adult male wage in the village, presence of brick-built road, regular market, post office, local government office, youth organization, Distance to nearest thana, Number of money lenders, large farmers/traders who provides advances against crops in the village, Number of small credit/savings groups in the village, Price of Rice, wheat, oil, potato.

Additional variables used in estimating the Propensity Score:

Additional covariates used in the PSM estimator are household demographic and socio-economic variables decomposed into various categories (e.g., age is divided into different groups), additional household level variable (e.g., number of daughter, son) additional village-level characteristics (e.g., average male, female daily wage). This is a larger set of variables and interactions that are selected to maximize the percentage of observation classified under the model.

Table A1- List of MFIs by Activities in Program District

Name of MFIs	District	Activities
Association for Social Advancement (ASA)	Feni	Credit
Proshika	Barisal	Credit, Forestry/ Nursery, Education, Fisheries.
Thengamar Mohila Sabuj Sangha (TMSS)	Bogra	Credit, sanitation, Crop Diversification, Forestry, Training, Education, Fisheries, Handicrafts, Family Planning, Poultry and Diary, Community Health.
Society for Social services (SSS)	Tangail	Credit, Sanitation, Education, Insurance, Forestry/Nursery, Training, Crops Diversification.
Anyvab	Panchagarh	Credit, Forestry, Nursery, Informal Education.
Solidarity	Kurigram	Credit, Forestry, Education, Sanitation, Family Planning, Training, Legal Aid.
Program for People's Development (PPD)	Sirajganj	Credit, Education, Training, Poultry.
Draidra Bimochan Sangsta (PRP)	Meherpur	Credit
Gano Unnoyan Prochesta (GUP)	Madaripur	Credit, Education, Legal Assistance, Training, health Care, Fisheries and Livestock.
Sabalamby Unnoyan Samity	Netrokona	Credit, Forestry/Nursery, Education, Family Planning, Poultry, Training, Education, Sanitation, Health Care, Income Generating.
Nobaeki ganomukti Somabay Samity Ltd.	Satkhira	Credit, Sanitation, Preliminary Education on Health, Family Planning
OSDER	Munshiganj	Credit, education, Training, Health, Handicrafts, Marketing Support.
Prottayasi	Chittagong	Credit, Mother and Child Health, Forestry, Poultry

Source: Author's compilation based on the annual report of the above MFIs

Chapter 2

Child Labour and Schooling Responses to Access to Microcredit in Rural Bangladesh

2.1 Introduction

Microcredit programs have expanded rapidly in recent decades in the developing world. It has reached more than 20 million borrowers in Bangladesh, or about 60 percent of the country's rural poor households (World Bank 2006). The United Nations (UN) declared 2005 as the International Year of Microcredit, and it urged multilateral donor agencies and developed countries to support the microfinance movement to achieve its Millennium Development Goal of halving poverty by 2015. International donors, lending agencies, national governments are now allocating tens of millions of dollars for the microcredit program each year. There has been a renewed pledge from policy makers and practitioners to expand such programs and to increase their outreach to reduce poverty. One strong testimony to the success of microcredit and its popularity over the past couple of decades may be the fact that today there are more than 7,000 microfinance institutions, serving millions of poor people, and that the microcredit has proved to be an important instrument in helping "large population groups find ways in which to break out of poverty" (The Norwegian Nobel Committee's press release of awarding the 2006 Nobel Peace Prize to the Grameen Bank (GB) and its founder Muhammad Yunus).

In general, it has been implicitly assumed that reduction of poverty would enhance child welfare, especially by investing more in their schooling. Underdevelopment of credit markets coupled with low household income (Ranjan 1999) or lack of access to credit are considered major factors responsible for inadequate education for children in developing countries (Jacoby and Skoufias 1997; Ranjan 2001; Dehejia and Gatti 2005; Edmonds 2006). Credit constraints may impose restrictions on investment in human capital. Credit-constrained poor households often send their children to work to smooth out consumption (Jacoby and Skoufias 1997) or simply to survive, and this is the reason why children are a current economic source for poor parents in developing countries. The availability of credit means that parents need not rely on income that can be raised from child labour. The ability of parents to borrow in order to finance current investment or to maintain income generating activities plays an important role in the decision whether to invest in education for their children.

However, microcredit may also have unintended consequences on children's education. These consequences arise for several reasons: (i) the loan often requires the establishment of a household enterprise, which requires extra labour to work in it, (ii) the loan amount is not large enough to hire external labour, which compels the household to use child labour, (iii) the loan repayment period is short and the interest rate is high. The last two points may make households myopic, and may induce parents to heavily discount the future return on their children's education. The nature of investment for microcredit loans (investment in self-employment and petty activities) and its repayment methods require quick and high returns from the investment.[1] Specifically, in order to service the loan it may be necessary to supplement household income, at least temporarily, with the proceeds from child labour. Therefore, the additional activities made possible by access to microcredit and the factors related to servicing terms of microcredit loan may alter household preferences towards child schooling. Children may be employed directly in the newly created or expanded micro enterprises, or indirectly, as carer for their siblings or in farm and livestock duties and other household chores.

Studies have found that microcredit can improve household welfare by increasing their income and consumption, and reducing poverty (Pitt and Khandker 1998; Kaboski and Townsend 2005; Islam 2008; Karlan and Zinman 2008a). However, there is less evidence on the impact of microcredit on human capital formation, and the limited evidence that exists is far less conclusive than the effect of microcredit on alleviating poverty. Studies find that the effect of microcredit on schooling (child labour) is either negative (positive) or rather ambiguous. For example, Wydick (1999) finds that the relation between access to microcredit and children's education is not unambiguously positive in the case of Guatemala. He finds that a child is more likely to work in a household enterprise when household borrowing is used for capital equipment instead of working capital. A similar conclusion is drawn by Maldonaldo and Gonzalez-Vega (2008) who find that households demand more child labour if they cultivate land and operate labour-intensive microenterprises. Pitt and Khandker (1998) find that schooling of girls increases when women borrow from Grameen, but not when they borrow from other microcredit programs in Bangladesh. Hazarika and Sarangi (2008) detect that, in rural Malawi, children tend to work more in households that have access to microcredit. The above studies on microcredit, except the ones by Pitt and Khandker, are based on a very small

[1] This is important considering the interest rate of above 30 percent on a reducing-balance basis. The effective interest rates are even higher because of commissions and fees charged by microcredit organizations. The frequency of repayments, and the systems adopted to collect repayments also raise the effective interest rates.

sample, and of course it is difficult to compare studies of MOs in different parts of the world, as their operations, regulations and aims are very different.[2] However, these findings indicate that microcredit may have a detrimental effect on human capital formation.

In this chapter, we examine the impact of household participation in microcredit programs on *both* schooling and child labour using a new, large, nationally representative and unique dataset of microcredit programs from Bangladesh. Estimating the causal effect of microcredit is made difficult by the non-random program placement and self-selection of participants into the program. Microcredit programs are available in certain villages, and households self-select into the program. As a result, differences in educational outcomes of children between treatment and control groups may reflect the underlying differences in the characteristics of the two groups rather than the impact of microcredit per se. We address the selection bias using the instrumental variable method. MOs typically select those households who satisfy the eligibility criteria. It also appears that treated households in program villages borrow varying amounts of microcredit, which depends largely on the years a program has been available in different villages. We use both the eligibility criterion and the exogenous variation in treatment intensity among households in different villages as the identifying instrument. We estimate the treatment effect by combining regression adjustment with weighting based on propensity score (Rosenbaum and Rubin 1983), which is an approach suggested by Robins and Rotnitzky (1995). We also examine the robustness of the estimates by using the alternative control function method which does not rely completely on the exclusion restriction.

Overall, the results suggest that microcredit loans adversely affect both child education and the demand for child labour. The results overwhelmingly indicate that girls are more likely to be affected adversely. The effects on boys are ambiguous: there is some evidence that microcredit can lower their involvement in work. These findings are important from a policy perspective because microcredit may reduce the effectiveness of policies to promote gender equality in education in developing countries, which is contrary to what policy makers now believe. The results indicate that younger children, who are more exposed to the program, are more likely to be put to work, and they are also less likely to attend school as their parents borrow microcredit. We also estimate the heterogeneous treatment effects by allowing the treatment effects to vary by gender of participants and by income and asset ownership of households. The results show that income or education of households matter for receiving the benefits or minimising the adverse consequences of microcredit programs. In particular, they suggest that children of poorer households are more likely to be caught in a vicious poverty cycle. The effects do not differ much between male and female credit borrowers. However, there is some evidence that

[2] See World Bank (2006) for characteristics of different types of MOs across different regions of the world.

borrowing by women can reduce the probability that girls will be obliged to work. Male participation in microcredit has a similar effect on boys – boys are more likely to go to school and work less. The empirical findings hold across different specifications and methods, and when corrected for various sources of selection bias.

2.2 Research Context

2.2.1 Access to Microcredit, Schooling and Child Labour

Microcredit, which requires investment in household enterprise, can have both positive and negative effects on child education and labour. It may raise the demand for child labour to fulfill the basic income-generating requirements of households or to facilitate the labour efforts of more productive household members by taking care of younger siblings. If, for example, a household uses microcredit loans to purchase livestock, it will require labour to take care of the animals and increased amounts of raw materials to manufacture craft boxes. Such kinds of investment spending increase the demand for labour, including child labour. Moreover, since microcredit does not offer enough flexibility to hire labour because of the size of the loan, families are often forced to use child labour to enhance its self-employment activities.

The literature has also identified several channels through which access to credit in general may affect human capital formation. First, to the extent that credit affects income of the borrower, the income effect may influence the demand for schooling (Behrman and Knowles 1999). Secondly, the vulnerability of rural households to adverse exogenous shocks may force them to remove their children from school in times of need. Households smooth income by using financial savings, selling assets, taking children out of school, and developing informal insurance and credit arrangements (Jacoby and Skoufias 1997). Loans from MOs can assist consumption smoothing (Pitt and Khandker 1998, Islam 2008). Participation in a microcredit program may, therefore, reduce the probability that children will be withdrawn from school in response to adverse shocks. Several studies have also demonstrated that women show a stronger preference than men for educating their children (see, for example, Pitt and Khandker 1998; Behrman and Rosenzweig 2002). Since women are the dominant group of borrowers from MOs, such borrowing may increase their power to influence household schooling decisions. These preferences toward schooling may be also influenced by the mandatory adult training programs conducted by MOs. Though MOs, in general, do not have any direct declared objective of increasing school attendance, they do educate members about the potential benefits of sending children to school. For example, Grameen Bank members need to memorise sixteen decisions, one of which is "we shall educate our children." As a result, microcredit can conceivably have a positive effect on schooling by increasing the income and knowledge of the borrower.

However, loans from MOs require that a family enterprise be established. This enhances the demand for labour, often from within the family. This constitutes a fundamental difference between microcredit and traditional sources of loans. Typically the size of microcredit loan does not allow a household to hire external labour.[3] This creates a problem for parents: whether to send their children to school or have them work at the domestic 'microenterprise.' Since repayment of microcredit loans is required quite soon after the loan is sanctioned,[4] the decision to keep children in school carries a significant opportunity cost. Thus, the small gestation gap between loan issue and the start of repayment increases the probability that children are employed in the microenterprise. Poor households typically enjoy a high marginal utility from current consumption and heavily discount the future. Hence, the expected return from investing in domestic self-employment activities relative to (heavily discounted) expected returns from schooling tends to increase the probability of putting children to work.

There is a growing literature on the influence of the demand for child labour on schooling outcomes (see Edmonds 2007 for an excellent survey). However, there are relatively few studies on how household participation in microcredit programs affects child labour and education. This chapter contributes to the latter literature by linking two areas of studies. One deals with the effects of credit constraints on schooling, and the other analyses the implications of family farms and enterprises for child labour in developing countries.

Empirical studies tend to conclude that access to credit improves the child labour situation and increases schooling in developing countries.[5] For example, Jacoby (1994) finds that unequal access to credit is an important source of inequality in schooling investment in Peru. Dehejia and Gatti (2005) find a negative association between child labour and access to credit across countries. Jacoby and Skoufias (1997) observe that in India the incidence of child labour increases as access to credit becomes more difficult. Beglee, Dehejia and Gatti (2005) show that access to credit offsets the effect of income shocks on child labour in Tanzania. Edmonds (2006) finds that, in South Africa, the inability of households to borrow against future income forces them to send children to work. On the other hand, Yamauchi (2007) finds that investment in household enterprises may not necessarily eliminate child labour or promote children's education in rural areas of Indonesia.

[3] Loan sizes vary but are typically between US$40 to $150. However, members may take larger loans after repaying their first loan. Loans are made for any profitable and socially acceptable income generating activity, such as: poultry, livestock, sericulture, fisheries, rural trading; rural transport; paddy husking; food processing; small shops and restaurants.

[4] Most of the MOs require that households start repaying the loan after four weeks of getting credit.

[5] See Belly and Lochner (2007) for empirical literature on borrowing constraints and schooling in the context of developed countries.

There is evidence that even increases in income may exacerbate the child labour problem. This may occur if the higher income is associated with expanding economic activity within the household (Edmonds 2007). For example, Bhalotra and Heady (2003) show that farm size has a positive effect on children's hours of work and a negative effect on school attendance, particularly on girls. Ravallion and Woodon (2000) find that an increase in current income in the form of food subsidy as provided by the government "food for education" program in Bangladesh did increase schooling but without any substantial decline in the incidence of child labour. Similarly, in the case of Progressa, de Janvry et al. (2006) find that the conditional transfers helped protect children's school enrolment, but did not prevent parents from increasing child labour in response to shocks.

2.2.2 Conceptual Framework

We consider here a generic Becker (1991) type household decision model. Assume that households derive direct utility from schooling of their children independent of its financial return. Assume that parents take decisions regarding their children, and that they have a utility function defined over a set of commodities of the following form:

$$U = U(x, z, l_c, l_a, s) \tag{2.1}$$

where x is household consumption of market goods; z is the self-produced consumption good; l_c and l_a are leisure of children and adults, respectively; and s denotes the child's school attendance. The utility function is assumed to be continuous, (weakly) monotonic and strictly quasi-concave. z can be produced at the household enterprise using both adult labour, L_a and child labour, L_c:

$$z = z(K, L_a, L_c) \tag{2.2}$$

where K is capital. Assume that the production function is strictly monotonic, strictly concave, bounded from above and all the Inada conditions apply. The household enterprise faces the borrowing constraint that its wage and services bill must not exceed its working capital:

$$wL_h + rK \leq \Omega \tag{2.3}$$

where w is the exogenously given wage rate, L_h is hired labour (in practice L_h=0 in household enterprise); r is the rental rate of capital; and Ω is the amount of working capital which is the maximum amount of money a household can borrow from MO (Ω=0 for those who cannot borrow).[6]

[6] The model makes the simplifying assumption that labour markets are perfectly competitive. This implies that any household member seeking work can find it at the going wage rate.

The child's total time available T_c can be devoted to schooling, s, leisure, l_c, or work L_c: $T_c = s + l_c + L_c$. Similarly the adult's total available time, T_a, can be devoted to leisure, l_a, or wage labour, L_a: $T_a = l_a + L_a$. Assume all children are altruistic in the sense that they return all income receipts to their parents (also a social norm in most developing countries). For simplification assume that child labour and adult labour are perfect substitutes.[7] If a household earns income from two sources - household enterprise production and earnings from outside employment - its budget constraint is given by equation (2.4):

$$Y + P_z z(K, L_a, L_c) = P_x x + (b-c)s + wL_h + rK \tag{2.4}$$

where Y is the exogenously given income of household, P_x and P_z are the prices of x and z, respectively, c is the cost of attendance to school, b is the discounted present value of the returns to schooling so that $(b\text{-}c)=\beta$ measures the discounted net benefit of schooling.

After substituting the borrowing constraint which is binding at the optimum, the budget constraint can be re-written as

$$Y + P_z z(K, L_a, L_c) = P_x x + \beta s + \Omega \tag{2.5}$$

The first-order conditions yield the following system of reduced form demand equations:

$$g = g(P_x, P_z, w, \beta, Y, \Omega) \qquad \text{where } g = x, s, z, l_c, l_f, l_m. \tag{2.6}$$

Comparative static results can be obtained by differentiating (6) with respect to **Ω** to get expressions such as $\delta s/\delta\Omega$. So, we can obtain the relationship between microcredit and schooling. Essentially, the relationship involves a substitution effect and an income effect, and the sign of $\delta s/\delta\Omega$ depends on the relative magnitude and sign of both effects. We can differentiate the time budget constraints to get

$$\frac{\partial s}{\partial \Omega} = \frac{\partial s}{\partial l_a}\frac{\partial l_a}{\partial \Omega} + \frac{\partial s}{\partial l_c}\frac{\partial l_c}{\partial \Omega} + \frac{\partial s}{\partial L_a}\frac{\partial L_a}{\partial \Omega} + \frac{\partial s}{\partial L_c}\frac{\partial L_c}{\partial \Omega} \tag{2.7}$$

Clearly the signs of all terms are ambiguous *a priori*. The effect of capital on child schooling depends on the relative magnitude and sign of the various terms of equation (2.7). Several characterizations here can be made concerning children. If child quality is a normal good, an increase in wage income is expected to contribute positively to child education. An increase in paternal wage income raises the implicit price of leisure which increases child education if the child's education and father's leisure are substitutes. Alternatively, if child and adult work are substitutes, child leisure and education may decline when parent devotes more time to the

[7] Our results do not change if we consider child labour as a fraction of adult labour.

production of consumption goods. An increase in the child's wage or demand for child labour may work through different channels to alter the amount of education. Its impact also depends on the substitutability relationship between leisure and education. If leisure and education are complements then an increase in the cost of leisure will be associated with reduced demand for education. Conversely, if they are substitutes, the demand for education increases with a rise in the wage.

The above household demand framework generates ambiguous effects of microcredit on schooling. Everything depends on the magnitude and signs of the terms associated with the Slutsky decomposition. However, by incorporating household, individual, community and geographic characteristics in a regression framework as suggested by Becker (1993) one can empirically test whether such preferences can be determined by a family's demographics, income and the like.

2.3 The Program, Data and Descriptive Statistics

2.3.1 Background: Education and Child Labour in Bangladesh

Bangladesh has achieved rapid progress in child education in recent years. The gross primary enrolment rate increased from 72 percent in 1990 to 96 percent in 2000**.** This has been made possible due to government's various stipend programs for children in primary and secondary schools in all rural areas. However, the Bangladesh Household Income and Expenditure Survey 2000 indicates a net primary enrolment rate of only 65.4 percent in 2000.[8] At the same time, the primary school completion rate was 66.3 percent. The Bangladesh Child Labour Survey 2002-03 estimates that 6.4 million children aged 5-17 work in rural areas, compared to 1.5 million in urban areas. Most child labour is in agriculture. Nearly 50 percent of primary school students drop out before they complete Grade Five. Among the poorest quintile of households, the share of family income contributed by child labourers reaches nearly 50 percent (Salmon, 2005). Child labourers between the ages 5-14 constitute about 12 percent of the country's labour force (Rahman et al. 1999) of which 73.5 percent are boys and 26.5 percent are girls.

Despite the persistence of child labour, considerable progress has been made in increasing equitable access, reducing dropout rates and implementing quality enhancement measures in primary education. Access to primary education has increased steadily over the past two decades. A compulsory primary education law was adopted in 1990, and the compulsory primary education program was extended nationwide in 1993, although the law is not strictly

[8] Gross primary school enrolment rate is the number of pupils (total) enrolled in primary, regardless of age, expressed as a percentage of the population in the theoretical age group for primary education. Net primary school enrolment rate is the number of pupils (total) in the theoretical age group for primary education enrolled in primary education expressed as a percentage of the total population in that age group.

enforced. Incentives to attend primary school have been introduced with the distribution of textbooks and provision of “food for education,” which was converted to a cash stipend in 2002. Primary education in rural areas consists of government schools, madrasas (Islamic schools) and NGO-run non-formal primary schools.

2.3.2 The Program and Data

The data were collected by the Bangladesh Institute of Development Studies (BIDS) on behalf of the Palli Karma-Sahayak Foundation (PKSF) (Rural Employment Support Foundation) with support from the World Bank.[9] This survey is the largest and the most comprehensive of the existing microcredit programs in Bangladesh. Its geographic coverage is spread evenly across Bangladesh, and the sub-district (thana) level comparisons reveal that selected sub-districts are close to the average (Zohir et al. 2001). The data cover 13 MOs of different sizes in terms of operations and membership. These MOs were selected to constitute a nationally representative dataset for the entire microcredit program in Bangladesh. The most notable MOs studied in this study are ASA and Proshikha, the third and fourth largest MOs, respectively, in Bangladesh. All 13 MOs follow the Grameen Bank-style lending procedure and typically give access to microcredit to households owning less than half an acre of land.

The survey includes 13 districts covering 91 villages spread over 23 sub-districts in Bangladesh. A census of all households in the 91 villages was conducted before the survey was administered in early 1998. The actual targeting of survey households involves two stages: (i) the selection of the villages where MOs operate; (ii) the selection of treated households within the selected villages. The non-participants from the program villages who are observationally similar were also selected as control groups since there were only eleven control villages available. Participation in a credit program was defined in terms of current membership reported during the census. From the village census lists of households 34 were drawn from each program and non-program village. Because the census found a large number of ineligible households in program villages the sample was drawn to maintain the proportion of eligible and ineligible households of about 12:5. The sample size within program and control villages was also determined accordingly.[10]

2.3.3 Descriptive Statistics

The original survey consisted of 3,026 households. In this study, we consider the subset of 2,034 households who had at least one child aged 7-16 at the time of the survey. This represents

[9] The PKSF is the apex organisation for microfinance. The microlending community regards it as a regulatory agency, and it exercises authority over the MOs.
[10] The sample size and its ratio between participating and non-participating households are different in few villages because of the absence of required number of appropriate households in each group.

a total of 4,277 children, of which 2,658 belong to treatment households and the remainder to control groups. Our sample contains both men and women borrowers but the former accounts for only 12 percent of all borrowers (and 133 households) representing 281 children. Boys account for 54.2 percent of these children.

The household level questionnaire includes primary and secondary activity of each child. We define "child labourer" as anyone of age 7-16 who performs any economic activity (i.e., if a parent answers 'employed,' 'household work,' or 'employed but not working'). A child is considered to be in school if he/she is currently enrolled in school and attended school in the last month of the survey period. By this definition 77.4 percent of girls aged 7-16 in the sample were classified as being in school and 10.4 percent in work. The corresponding figures for boys are 71.3 percent and 15.7 percent, respectively. Other children are reported to be neither working nor in school, and possibly many of these are helping parents with household work. So there may well be under-reporting of child labour. The results by participation status are reported in Table 2.1. School enrolment is lower and child labour higher among children of treatment groups. We find a statistically significant difference in school enrolment and child labour between boys of treated and untreated households, but no such difference exists for girls. However, the difference in school enrolment between girls and boys is larger in treatment groups, which implies that school attendance by girls is relatively higher in the treatment sample.

[INSERT TABLE 2.1 HERE]

We plot school enrolment of children of both genders by age in Figure 2.1. High-school age children (12-16 years old) are less likely to be enrolled in school. Among primary school age children, the proportion of enrolled children aged 7 and 8 is lower than children aged 9 to 11. This indicates that there are a considerable proportion of children who start schooling at a later age. The gap between school enrolment of treatment and control groups is higher for boys. 7-11 years old girls have a similar rate of enrolment in both treatment and control groups, but after age 13 girls tend to diverge with treatment groups showing lower participation rates in school. On average, primary-school-age children have a higher school participation rate compared to their older siblings who drop out of school and are more likely to go to work. Overall, a higher proportion of children from treated households are in work (Figure 2.2).

[INSERT FIGURES 1 AND 2 HERE]

Table 2.1 also provides descriptive statistics for child and household demographics and village characteristics. It shows that the average age of children is 11.5 years for both types of households. There is no difference between treatment and control groups in the gender composition of children. The treated group has slightly more members in the household than the non-treated group. There is an average of four children younger than 18 years per household in the survey. Non-treated households tend to be better educated, a little older but smaller in household size. Descriptive statistics not reported in the Table 2.1 shows that a total of about one quarter of mothers did not go to school at all. More than a quarter of our sample has a secondary school in their locality, and primary schools exist in most of the villages. The presence of both primary and secondary schools is slightly lower in program villages compared to control villages. However, program villages have superior health facilities and are located relatively closer to the nearest sub-district. The differences between the program and control villages (in terms of the provision of bus stands, as well as post, telephone and local government offices) do not show statistically significant differences between the two groups.

We also note that the effect of microcredit on child labour or schooling is *a priori* ambiguous. It is also possible that in some cases an interior solution may exist where children both work and go to school. Only 1 percent of children both work and attend school. It is likely that this figure understates the extent of child labour, especially helping parents with household work despite attending school.[11] Because of the shortcomings of data, we do not estimate the results where children both work and go to school. In the next section, we outline our empirical strategy.

2.4. Empirical Methodology

In modelling school attendance or child labour, we follow standard practice (Wydick 1999; Ravallion and Wodon 2000; Edmonds 2006). Let S_i be a binary variable that denotes whether child i (i) works (S_i =1) or not (S_i =0) and (ii) attends school (S_i =1) or not (S_i=0). We estimate the impact of participation in microcredit programs on children's education/work with the following relationship:

$$S_{ijkl} = \beta_{0l} + \beta_1 X_{ijkl} + \beta_2 Z_{kl} + \beta_3 credit_{jkl} + \varepsilon_{ijkl} \tag{2.8}$$

where the subscripts index child (i), households (j), village (k), and district (l). X is a vector of child and household specific covariates, and Z is a vector of village specific covariates. β_{0l} is fixed-effects. 'Credit' is a continuous treatment variable defined by the amount of microcredit borrowed by the household. It is equal to zero if a household did not participate in a microcredit program. The error term ε_{ijkl} is assumed to be i.i.d. Using Equation (2.8) we can estimate employment or school attendance probabilities attributable to credit program participation with the probit model.[12]

[11] It is usual in rural areas of Bangladesh that parents arrange a modest amount of part-time work for their children while still keeping them at school (see, for example, Ravallion and Woodon 2000).
[12] It is possible to use a bivariate probit model to analyse the decision of child labour and schooling simultaneously. However, the number of children who both attend school and work and who do neither is very small in our data. Thus the work versus schooling decision is nearly a dichotomous decision, and so we do not attempt using bivariate probit

Estimating Equation (2.8) using either OLS or probit model is problematic. First, there is the issue of non-random program placement, as programs are placed in specific villages. Selection for placement could be influenced by biases in favour of high-income villages – because they may have higher participation rates – or by official bias in favour of poorer villages. However, given that programs are placed by central decisions and that there are hundreds of MOs, it is reasonable to assume that village-level program placement is a problem of "selection-on-observables." The survey covers a wide range of village-level variables. So we can account for the non-random program placement by a set of control variables at the village level (included in the vector Z). We also use district-level fixed effects to remove any unobserved heterogeneity across different geographic areas. Since we have 13 MOs, each from a different district, this fixed effect also captures the differences between the MOs. Thus, we tackle the potential problem of non-random program placement using both geographic- and MO-level fixed effects and village-level observed covariates.[13] It is to be noted that we adopt an estimation strategy different from that used by Pitt and Khandker (1998). We do not use village fixed effects. Rather we use village-level pre-program characteristics to control non-random program placement. Village fixed effects could give us biased results if the programs are placed based on certain shocks (for example, floods or droughts) at the village level. We control any unobserved heterogeneity using geographic (district level) and MO-level fixed effects.[14]

Second, households self-select into the program but not all of them will be able to obtain microcredit. Generally only the eligible poor households, typically defined by the amount of land-holding, receive microcredit. However, other factors that influence whether a household has access to microcredit could also affect outcomes for children of that household. One such factor could be household income or wealth. For example, MOs may be more willing to provide credit to households that operate non-farm enterprises because the use of credit is less fungible in such households. Microcredit loans often require that family enterprises be established because they provide less opportunity for misuse of the loan. Poor households that operate an

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model. We model school attendance and child labour separately since in many settings a sizeable group of children are neither in school nor reported to be working. Our intention here is to keep the modelling process as simple as possible, and we do not complicate our estimation strategy by employing multinomial logit or probit model. Even if we model idleness of children as one of the utility maximizing decision and apply multinomial choice model, our conclusions do not change.

[13] Probit estimates with fixed effects give rise to inconsistent coefficients of the fixed effects. However, when the number of observations per fixed effect is at least 8, we can consistently estimate the fixed effects (Heckman 1981). We have at least 250 observations per district and so the model is consistently estimated. For the same reason, we do not estimate parental fixed effects which can eliminate unobserved time-invariant household-level variables or permanent heterogeneity. Instead we consider clustering at the household level.

[14] Fixed effects would eliminate village-level omitted characteristics, but differences in initial conditions also matter for program placement. The readers can refer to Keane and Wolpin (2002) for a similar analogy for problems using state-level fixed effects to estimate the welfare impacts in the US, and the resulting bias in the estimates. See also Morduch (1999) for pitfalls using village fixed effects in Pitt and Khandker (1998) study. It is, however, to be noted that our conclusion is not affected even if we use village fixed effects or separate fixed effects for target and non-target populations in each village. Using different fixed effects changes the value of the coefficient estimates but not the sign.

enterprise are also more likely to employ their children in that enterprise, and thus less likely to send them to school. Such negative correlation between credit access and schooling introduces a conservative bias in the coefficients. Hence, we need to consider the endogeneity of microcredit program participation at the household level. The enodogeneity problem implies that selection into treatment is on the basis of unobserved characteristics ε_{ijkl} in Equation (2.8). This implies potential non-zero correlation between ε_{ijkl} and $Credit_{jk}$, that is, $Credit_{jk}$ may be potentially endogenous. Consequently, impact estimates that use a simple probit/linear probability model (LPM) may not reflect the program's causal effect on children's schooling or work.

To account for self-selection into the program, we consider a source of exogenous variation. The MOs set the eligibility criteria for participating in the program. A household is eligible if it does not own more than a half an acre of land. The land ownership criterion is mainly used as a targeting mechanism to identify the poor. Since poverty does not exclusively depend on land ownership, the administrator or local loan officer or branch manager sometimes takes into account the socio-economic conditions of a household. As a result, some ineligible households receive treatment, but these are a distinct minority (70 percent of the treatment group in our sample is eligible). Thus the program eligibility criterion is not strictly followed. But the treatment into the program based on eligibility is probabilistic. So, our approach to estimate the treatment effect is similar to the use of fuzzy regression discontinuity design (see Van der Klaauw 2002) which we implement using IV approach.

It is clear that the likelihood of any household receiving microcredit is enhanced when a microcredit program is already available in a village. Therefore, we consider the use of the following as an instrument for the actual receipt of microcredit: the eligibility status interacted with an indicator of program presence in a given village.[15] However, instead of using this instrument directly, we utilise an unexploited exogenous source of variation in the treatment intensity based on household's exposure to the program in different villages. It appears that treated households in different villages borrow different amounts (Figure 2.3). Intensity of treatment varies widely in different villages, and we focus on explaining differences in treatment intensity across villages. We consider the original introduction of the program across villages in different districts, and note that the earliest program was available in a village in 1980 and the latest program became available in a different village in 1997. We, therefore, exploit the timing of the microcredit program placement in different villages that have largely contributed to the variation in credit demand by treated households. Figure 2.3 shows that

[15] Pitt and Khandker (1998) and Islam (2008) use this instrument for credit program participation in Bangladesh, and discuss the plausibility of using this instrument at length. Morduch (1998) questions the validity of using this instrument, but Pitt (1999) argues at length in his response to Morduch's critique that the eligibility criterion satisfies the conditional exogeneity and exclusion restriction.

significant differences in the amount of loans from microcredit organisation exist across households of different villages. At the household level, the amount of total credit borrowed largely depends on how long the program has been available in the village. So we use the following instrument:

$$I = M_k \times E_j \times \text{number of years microcredit is available in a village}$$

where M_k is a binary variable that equals 1 if a village k has a microcredit program. Similarly, E_j is a binary variable that assumes the value of one if a household j is eligible (i.e., if it owns less than half an acre of land).[16] With controls for village and fixed effects, identification requires that there be no contemporaneous village-level unobservables that are correlated with microcredit program placement and child labour/schooling. The equation of the demand for credit then assumes the form:

$$credit_{jkl} = \alpha_{0l} + \alpha_{1jk}(M_k \times E_j \times N_k) + \alpha_2 X_{jk} + \alpha_2 Z_k + \xi_{jkl}, \tag{2.9}$$

where N_k is the number of years a microcredit program has been in village k.[17] X_j now includes only household-specific covariates since participation in microcredit programs is determined at the household level.

[INSERT FIGURE 2.3 HERE]

The probit estimates are obtained using the two-stage procedure where the second stage regression includes the fitted value of credit obtained from the first stage credit demand Equation (2.9) (using tobit). The use of an estimated variable (instrumented credit) in a non-linear specification may lead to bias but this bias is of the second order and thus very small (Train et al. 1987). We also estimate the second stage using ordinary least squares (OLS) estimations of LPM. Additionally, because of the non-random nature of our sample we use inverse-propensity score weights in the standard fashion for all the estimators (Hirano, Imbens and Rider 2003). This involves attaching an estimated weight to each observation in one sample that corresponds to the probability of observing a similar observation in the other sample. With normalisation, we attach a weight of one to each treated household, and to each comparison group member a weight of $p/(1-p)$, where p is the estimated propensity score.[18]

A potential problem with interpreting these results when using credit as the treatment variable is that the reported amount of credit is subject to misreporting or other types of measurement error

[16] We also use only the interaction between M and E (ignoring the length of time a microcredit program has been present in a village) as instrument and find qualitatively similar results.
[17] In our empirical estimation we also experimented with instruments that include separate dummies for year of microfinance placement in villages. The results turn out to be similar.
[18] The estimated difference in covariate after adjusting propensity scores is lower than the unadjusted difference between treatment and control groups. It is, however, important to point out that our qualitative conclusion remains unchanged with or without weighting.

since households may forget or may not correctly report the amount (see, for example, Karlan and Zinman 2008b, for problems with self-reported credit data). It is possible that households cannot calculate or remember or report properly the amount of microcredit borrowed in the past, which gives rise to measurement error in the credit variable. This measurement error is likely to impart attenuation bias to the credit impact coefficients. It follows that the estimated coefficient of the effect of credit on school enrolment or child labour may be affected by changes in data quality. Since we are using instrumented credit variable as the treatment variable, we can overcome the measurement error problem. However, we also use a binary treatment indicator, *viz.* whether the household is currently a member of a microcredit program or not. This binary variable is unlikely to be measured or reported with error. It can also serve as a robustness check of the earlier estimates using the amount of credit as the treatment variable. However, the use of binary treatment indicator raises another issue as dummy endogenous regressors with limited dependent variables raise some special econometric problems. Angrist (2001) advocates using simple IV estimators as an alternative because they require weaker assumptions and are often sufficient to answer questions of interest in empirical studies. We therefore estimate the treatment effect also by using a LPM in the second stage of an IV regression.[19]

To adjust for clustering at the village level we first use the cluster-correlated Huber-White covariance matrix estimator. Donald and Lang (2007) have pointed out that asymptotic justification of this estimator assumes a large number of aggregate units. Monte Carlo simulations (Bertrand, Duflo and Mullainathan 2004) suggest that when the number of primary sampling units (PSUs) is less than 50 this estimator performs poorly, leading to excessive rejection of the null hypothesis of no effect. Fortunately, with 91 PSUs in our sample we can potentially overcome the problem by using cluster-consistent standard errors. The cluster-adjustment works well for binary outcomes and nonlinear models such as logit and probit models, provided that the number of clusters is large (Angrist and Lavy 2002).[20] Secondly, children of the same household are likely to be similar in a wide variety of characteristics. It follows that there may be large intra-household correlations. Moreover, the data were collected by using households as the survey unit. Thus, we also estimate standard errors clustering at the household level as there is usually more than one school-age child within a household.

[19] When all independent variables are discrete (as is the case with most of our variables) LPM is completely general, and fitted probabilities lie within the interval. In addition to being fairly general in our context, the LPM has also the advantage of allowing straightforward interpretation of the regression coefficients. Moreover, we compute Huber-White standard error to take into account the heteroscedastic error term of LPM.

[20] Alternatives to cluster-adjusted standard errors include the hierarchical linear modelling, two-step procedure by Donald and Lang (2007) and the Bell and McCaffrey's (2002) biased reduced linearization estimator for micro data.

2.4.1 Checking the Validity of the Instrument

We now consider the plausibility of the instrument. We need a strong first-stage to ensure that we are not using a weak instrument. So, we estimate the first-stage regression by estimating a credit demand equation (Equation (2.9)) using a standard tobit model.[21] The first-stage results show that the instrument is statistically highly significant with a *t*-statistic of 8.5. The coefficient estimate is positive and also economically significant, which implies that I is significantly related to the demand for credit.

We also estimate the participation decision equation which regresses a binary indicator for participation on an indicator of interaction of eligibility and program village dummies (plus all controls). The results are stronger with a *t*-statistic of 12. The regression using basic controls and no controls show stronger coefficient estimates. Since we have a single instrument for the endogenous credit variable, we cannot test the exogeneity of the instrument as in overidentified model. The remaining concern is whether the instrument satisfies the exclusion restriction, i.e., whether eligibility affects child labour or school enrolment only through participation in the credit program or the amount of credit borrowed. Unfortunately, the exclusion restriction is not directly testable. However, we investigate this concern in a number of ways. First, we estimate a reduced form regression to examine the effect of loan eligibility on school attendance/child labour. We do not find that any effect of eligibility on school enrolment and child labour. We also estimate an equation in which credit is instrumented but instrument eligibility enters the second-stage regression directly (and naturally in the first stage regression). By definition of IV, the instrument should be uncorrelated with the outcomes of interest through any channels other than their effects via the endogenous regressors. Therefore, once the credit is instrumented, eligibility itself should have no effect on schooling or child labour when both instrumented credit and eligibility status are entered as controls for child labour/school enrolment equation. The results do not indicate any significant effect of eligibility in any of the specifications.[22] It needs to be emphasised here that our identification strategy does not depend exclusively on the eligibility rule since we also exploit the variation in credit demand among households in different villages based on the availability of the program in different villages.

2.5 Empirical Findings

This section reports our empirical findings where the estimated value of credit from the first stage regression (Equation (2.9)) is used as the regressor in the second stage estimation

[21] We also include child characteristics in the first stage estimation. The second stage results do not depend on whether we include or exclude child characteristics. There is a very little reason to incorporate child characteristics in the credit demand equation at the household level. However, there is no harm including them. So we tried with both specifications.
[22] The detailed results of the first-stage regression are available upon request.

(Equation (2.8)). We estimate the separate impacts of credit extended to women and men, respectively, on a child's schooling and work situation.[23] If women command more resources in the household, the overall schooling of their children is likely to be enhanced. For example, Pitt, Khandker and Cartwright (2006) find that women's participation in microcredit programs helps to empower them. If women are empowered through microcredit then it can also increase the relative likelihood of girls being educated through their command over the resources at the household. On the other hand, if parents have differential preferences for the education of their daughters and sons, the market rate of education could be different for boys and girls, and gender pattern of child education could be determined by a production function (Rosenzweig and Schultz 1982). So we estimate the results separately for boys and girls by credit given to both women and men using three sets of control variables: "no controls" (excluding the X and Z variables), "basic controls" (some household and child demographic variables, and village controls), and "full controls" (the full set of X and Z variables). The list of the full controls is chosen from a larger set of controls by selecting those which were most often significant. In identifying the set of control variables we first consider the variables (e.g., household and village characteristics) that the MOs use to select a household and that are likely to determine household demand for credit. We then include a number of regressors to take into account the number of siblings, family composition that can potentially determine the children's schooling or work status. The final set of covariates included in X and Z is listed in Appendix 2.

Table 2.2 presents the estimates of the second stage using the LPM and probit models under different covariate specifications. Columns (1) and (4) represent the treatment effects without any controls. The estimates in column (1) can be considered Wald estimates, representing the difference in the probability of child labour between children of microcredit participants and non-participants divided by the amount of credit borrowed by the participating households. The Wald estimates in Table 2.2 show that credit is associated with higher probabilities of child labour for girls but lower probabilities of child labour for boys, regardless of whether credit is obtained by men or women. However, the coefficients are not statistically significant. Moreover, Wald estimates are likely to suffer from the omitted variable bias since parental decisions on schooling and child labour are likely to be influenced by household demographic and socio-economic characteristics.

To address the above issues, we consider two sets of control variables, basic control and full control variables, as discussed in Section 2.4. In Table 2.2, the results from the LPM model are reported in Columns (2) and (3) and those from the probit model are reported in Columns (5)

[23] Though credit is given to both women and men in different villages, credit groups are never mixed by gender. Households do not have choice over which gender is to participate since MOs select one or the other gender, but not both.

and (6). In Columns (3) and (6), the full set of controls is included, which is our preferred specification. All coefficients are estimated as marginal effects calculated at the mean. The results in Columns (3) and (6) provide a clear picture. Microcredit significantly increases the probability of child labour for girls. For boys, there is some indication that microcredit reduces the probability of child labour, especially when credit is obtained by women. Overall, the impact of microcredit on child labour is positive and significant. The qualitative results are independent of whether credit is obtained by men or women. For example, microcredit increases the probability of child labour for girls by 7.9 percent, according to the probit model, and 13.7 percent according to the LPM model. The probability increases by 8.4 percent and 14.3 percent respectively when women are borrowers. For boys, women's credit has a marginal negative effect on child labour. Table 2.2 also shows that girls are affected more adversely, and boys more favourably, when credit is obtained by men than by women, although these estimates are not statistically significant. A Hausman-like test does not support the difference in treatment effect between men and women borrowers. Finally the magnitude of the estimated coefficients increases as we include more controls. The overall finding is that microcredit clearly increases the likelihood of child labour for girls while the impact on boys is less clear.

[INSERT TABLE 2.2 HERE]

Table 2.3 reports the effect of microcredit on school enrolment. The results overwhelmingly indicate that access to microcredit negatively affects children's school enrolment. This is true across all regression models and regardless of whether credit is obtained by men or women. The negative effect is especially pronounced for girls although, for boys, the negative effect is statistically insignificant. For example, microcredit decreases the probability of school enrolment for girls by 22.6 percent, according to the LPM model, and 19.2 percent according to the probit model. We also find that the negative effect on girls' school enrolment is larger when microcredit is obtained by men than by women: in the probit model, the probability changes from 19.2 percent to 22.8 percent. The negative effect on boys' school enrolment, while statistically insignificant, is almost doubled when women are borrowers. One might surmise that this could be an indication of gender preference by parents. However, Hausman-type tests do not reject the equality of the coefficients between the sexes of the borrower. Once again, the magnitude of the estimated coefficients increases as we move from basic controls to full controls, suggesting that a fuller picture requires the analysis of how a household's socio-economic characteristics affect child labour and school enrolment. We turn to this below.

[INSERT TABLE 2.3 HERE]

Table 2.4 shows how the probabilities of children's school enrolment and child labour are associated with other control variables. The results are mostly consistent with previous studies. For controls at the household level, children's school enrolment is positively associated with education attained by any adult member of the household, the household head's education level, and the male head of the household, while it is negatively associated with the number of younger siblings and the age of the household head. The presence of a mother in the household has a positive but statistically insignificant effect on schooling. For controls at the village level and beyond, children's school enrolment is positively related to presence of secondary school or college, and infrastructure such as a health facility and brick-built roads. Interestingly, the presence of a grocery market and bus stand has a negative effect on children's schooling. A primary school in the village does not have any statistically significant effect on school enrolment or child labour. This may reflect the fact that almost all villages have a primary school. Similarly rice prices do not have any effect on either school enrolment or child labour possibly because the geographical variation in rice prices is very small. The sign of the adult male wage coefficient in the child labour equation is positive but statistically and economically insignificant, suggesting that adult male and child labour are imperfect substitutes.[24]

[INSERT TABLE 2.4 HERE]

The results reported in Tables 2.2 and 2.3 do not change qualitatively if we change the treatment variable. Table 2.5 shows the treatment-on-treated effect using a binary participation indicator as the treatment variable. The estimated effect using two-stage least squares (2SLS) is identical to the indirect least squares estimate obtained from taking the ratio of the reduced-form coefficients, because we are estimating a just-identified equation. The results are qualitatively similar to the previous estimates which used credit as a treatment variable. Girls' education continues to be affected adversely by parental participation in microcredit programs, irrespective of whether credit is obtained by men or women. In probit results, for example, we find that women's microcredit borrowing increases the probability of girls working by 13.7 percent and decreases the probability their school enrolment by 44.4 percent. The magnitude of the impact estimates is similar in case the borrower is a man. The corresponding coefficient estimates for child labour for boys are not statistically significant and have mixed signs. Overall, binary participation measures generate considerably larger coefficient estimates for girls. However, these results are only indicative as they do not take into account the variation of treatment intensity, and treat the program effect to be the same for all children in the treatment group.

[INSERT TABLE 2.5 HERE]

The standard errors reported in the above tables are corrected for clustering at the village level and weighted by the propensity score to take into account the choice-based sampling. The standard errors in square brackets take into account intra-sibling correlations within a household. Both standard errors are typically of similar magnitude. Since they do not differ

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[24] According to Basu and Van (1998), if children and adults are substitutes in production (the "substitution axiom"), the prevalence of child labour depresses adult wages: a condition under which a ban on child labour may be desirable. Our results indirectly suggest that this might not be the case. Moreover, when we regress adult male wages on child labour, we find a positive coefficient (t-ratio=1.53), indicating that the substitution axiom does not hold in our case.

much, we report below the regression results only with the clustered standard error at the village level. We also experimented with the two-step procedure discussed by Donald and Lang (2007). In our case this amounts to estimating village fixed effects (household fixed effects when considering intra-sibling correlation) in an equation like (2.8), and then regressing the estimated fixed effects on instrumented credit and other village covariates (household covariates). Since the estimation results are similar, they are not reported for the sake of brevity. In what follows, we report the results of impact estimates separately by various control variables.

2.5.1 Impact Estimates by Children's School Age

Table 2.6 reports the results of the impact of microcredit on children aged 7-12 (primary school age) and 12-16 (secondary school age: up to Grade Ten). As before, we use the binary treatment status indicator as the participation variable. The results show that the adverse effect of microcredit on children at the primary school age is mostly significant regardless of the gender of borrowers and children. Girls at the primary school age are especially adversely affected compared to boys, and more so when credit is obtained by men. For example, the probability of their school enrolment decreases by 33 percent when credit is obtained by women and by 41 percent when credit is obtained by men. For children of secondary school age, microcredit has a mixed effect. Women's credit has a statistically significant negative impact on girls' schooling while men's credit also has a negative but statistically insignificant effect, possibly due to the smaller sample size of male participants. For boys of secondary school age, microcredit increases their likelihood of school enrolment although coefficient estimates are not statistically significant. Overall, microcredit adversely affects younger children more than their older siblings, and girls more than boys, irrespective of the gender of the borrower.

[INSERT TABLE 2.6 HERE]

2.5.2 Impact Estimates by Household Income

Household income plays an important role in determining child labour and school enrolment (Basu and Van 1998; Edmonds 2005; Bhalotra 2007; Belly and Lochner 2007). Poorer families are more likely to remove their children from school in times of need. Poverty is associated with increased level of parental stress, depression and poor health, and these are all conditions which might adversely affect parents' ability to nurture their children. Impact estimates by household income also allow us to examine the hypothesis implicit in Basu and Van's (1998) 'luxury axiom' that parents send their children to work and keep them from school only if household income falls below a certain (subsistence) level. However, we cannot treat income as exogenous. Income is endogenous because the amount of credit borrowed by the household directly affects household income. If the participation in microcredit programs has a positive effect on household income, then including income as an explanatory variable would underestimate the actual effect of the program. Moreover, children's contribution to household income also makes the income variable endogenous. Since children working on the family farm are not paid a wage, their contribution cannot be deducted from total income. Even if we could measure income from child labour, the endogeneity problem would not be resolved by simply subtracting it from the total household income if the labour supply of different household members is jointly determined. Income is endogenous for another reason: children living in poorer families may have an adverse home environment or be facing other problems. Such omitted variables may continue to affect their schooling or child labour even if family income may increase.

There are two main approaches for dealing with the issue of endogeneity: fixed-effects estimation (Blau 1999) and the instrumental variable technique. While fixed-effects estimation should eliminate any bias from permanent differences in family or children, it may exacerbate bias due to unobserved temporary family shocks (Dahl and Lochner 2005). In the absence of appropriate instruments for income in our context, we use parental education as a proxy for permanent income.[25] If education has a positive return, families with more educated parents are expected to have a higher income. Clearly parental education is not affected by program participation or child labour supply. By using parental education as a proxy for permanent income, we also avoid the problem arising from noisily measured income and, hence, the possible attenuation bias. We use three categories of parental education: *Low* refers to those households where the highest level of education obtained by parents is primary (0-4 years of schooling) or less; *Middle* refers to households where at least one of the parents obtained more than primary but less than a high school degree (5-10 years of schooling), and *High* includes households where one of the parents obtained at least a high school degree (11 or more years of schooling). We adopt the following functional form:

$$Y_{ijkl} = \delta_{0l} + \delta_1 X_{ij} + \delta_2 Z_k + \delta_3(credit_{jk} \times Low_{jk}) + \delta_4(credit_{jk} \times Middle_{jk}) + \delta_5(credit_{jk} \times High_{jk}) + \nu_{ijkl} \tag{2.10}$$

where we incorporate the household's permanent income by interacting the three categories of parental education with the amount of credit borrowed. These interaction terms capture the differences in slope across different levels of education within the treatment group.

Equation (2.10) is unidentified since the number of endogenous regressors exceeds the number of instruments. Therefore, we need additional instruments that are correlated with the interaction

[25] Permanent income does not vary across observations on a given parent in our cross-sectional data, so the parental fixed effects method cannot identify the effects of permanent income. We need differences in family income level across siblings to remove fixed family factors before estimating the impact of income on child outcomes.

terms between credit and different education categories. Since credit is interacted with education dummies all the predicted values will be closely correlated. In the absence of suitable identifying instruments, we use estimated credit from the first-stage and interact the education dummy variables with the estimated credit variable. Our estimating equation thus becomes:

$$Y_{ijkl} = \lambda_{0l} + \lambda_1 X_{ij} + \lambda_2 Z_k + \lambda_3(\hat{D}_{jk} \times Low_{jk}) + \lambda_4(\hat{D}_{jk} \times Middle_{jk}) + \lambda_5(\hat{D}_{jk} \times High_{jk}) + \upsilon_{ijkl} \tag{2.11}$$

where $\hat{D}$ is the credit demand estimated from Equation (2.9).

Figure 2.4 shows how child labour and school enrolment varies as the level of parental education changes. The graphs show that there is a positive relationship between children's school enrolment and parental education and a negative relationship between child labour and parental education. Households in the control group tend to have a higher level of children's school enrolment and a lower incidence of child labour.

[INSERT FIGURE 2.4 HERE]

Table 2.7 reports the impact estimates based on different levels of parental education. A clear picture emerges. Households with the lowest parental education are those with the largest and significant adverse effect of microcredit on children's schooling. For example, the probit estimates imply that the probability of children's school enrolment decreases by 29.3 percent in these households while that of child labour increases by 9.7 percent. For households with medium to high levels of parental education, the impact is largely insignificant, although there is some indication that girls are adversely affected by microcredit in households with a medium level of parental education. Given our interpretation of parental education as a proxy for household income, these results indicate that microcredit to the poorest of the poor households neither alleviates the problem of child labour nor improves children's schooling. These households engage their children more in work in order to generate immediate returns from their microenterprise project. An additional observation is that, while statistically insignificant, the likelihood of children's school enrolment is positive in households with high education. Figure 2.4 also shows that children are more likely to be sent to school as household income proxied by parental education increases. Taken together, these results indirectly support Basu and Van's (1998) 'luxury axiom.'

[INSERT TABLE 2.7 HERE]

2.5.3 Microcredit, Income and Child Schooling

We have demonstrated above that children's schooling is less likely to be adversely impacted if they come from a relatively less poor or more educated family. This means that there is an interaction among schooling of children and income of their parents in presence of microcredit. It has been demonstrated in previous studies that microcredit can increase household income.

Insofar as microcredit enables poor households to graduate out of poverty the present results hold only in the short run. In the long run, participating households are likely to increase their children's schooling as their income increases. Conversely, if escape from poverty proceeds only very slowly then microcredit may intensify the long-term problem of human capital formation. Therefore, we now use the regression coefficients above to examine this issue further, though we do not intend to give a full treatment of this issue. Consider, first, the difference between the coefficients of the low and medium income groups in Table 2.6. It suggests that a 10 percent increase in credit given to medium-income households increases the schooling of children by about 2.6 percent more than if it had been given to the low income group. This can be found by the difference in the probability estimates of schooling between the medium education and low education households reported in Column (1). Similar calculations between low and medium income levels for child labour indicate that children of the medium income group have a relative increase of 0.6 percent of schooling compared to the children of low income households. Since the coefficient estimates measure the difference in the probability of school attendance between children of treated and untreated households in different income groups, the differences in estimated coefficients between the two treated groups can be interpreted as difference-in-difference estimate of the impact of household income. The effects on school attendance vary by child gender, and girls in medium income participating households are less favourably affected than boys. When a household head's education level increases from medium to high, *ceterius paribus*, we observe that the probability of children's employment is reduced by 0.4 percent and that school enrolment is increased by 0.8 percent if there is a 10 percent increase in the amount of microcredit. Given the modest increase in income due to participation in microcredit programs (see, for example, Islam 2008, and the references therein) it seems reasonable to conclude that the child labour problem remains an issue to be solved by the MOs and policy makers, rather than simply hoping that households will eventually graduate out of poverty. In other words, even if microcredit is seen to be successful in increasing income, it still takes a substantial amount of time for rates of child labour to decrease and for enrolment rates and years of schooling to increase.

2.5.4 Impact Estimates by Land Ownership

In many rural areas in developing countries, land is often the most significant asset the household owns. If land can be used as collateral for general-purpose loans, then land ownership may have a positive effect on children's schooling. In this case, more land implies more household wealth and the possible positive relationship between land ownership and children's schooling can be considered a confirmation of the positive relationship between household wealth and children's schooling. However, microcredit is mainly to be used to set up a household enterprise and the purchase of external labour is not often possible. Moreover households in the treated group have microcredit as the main source of loans. Therefore we do not expect a positive relationship between land ownership and children's schooling. Rather we may expect land ownership to have a negative effect since adult labour may need to be shifted from family farms to the household enterprise, increasing the need for child labour in family farms.

To examine this, we divide households into two groups: those with less than a half an acre of land (poorer households) and those with more than a half an acre (less poor households).[26] Although land ownership of less than half an acre is the eligibility criterion for microcredit, there were some households with more land who still obtained microcredit. Table 2.8 reports the impact estimates by land ownership. The results show that microcredit has different effects in the two groups of households. In poorer households, it decreases the likelihood of school enrolment for girls while decreasing the likelihood of child labour for boys. In less poor households, this result is reversed. Although it is not clear why less poor households tend to keep boys at work when they obtain microcredit, we surmise that less poor households engage boys more in agricultural activity, while households with marginal landholding engage girls more in the household enterprise.

[INSERT TABLE 2.8 HERE]

Many of the results are statistically insignificant, possibly because of relatively small sample size. However, overall results also show that our earlier findings were not driven by pre-existing differences in the characteristics of treatment and control groups. It is to be noted that poorer treated households have very similar observed characteristics as their non-treated counterpart.[27] Our results again point out that poor households tend to put girls to work and keep them away from school when they obtain microcredit.

2.6 Additional Robustness Check

2.6.1 Potential Identification Issue: Causal Effect or Selection Bias?

The previous section reported how microcredit affects children's schooling and child labour under the assumption that the differences in schooling and child labour between the treatment and control groups are not due to underlying differences in household characteristics. It could be argued, however, that households from program villages that are less likely to send their children to school are more likely to participate in microcredit programs. If this is the case, then our estimates would identify effects that are attributable to pre-existing differences in the

[26] Household land ownership is less likely to be affected by microcredit. There is not enough evidence in the data that shows a different pattern of buying and selling land after becoming a member of a MO. Since microcredit is mainly provided for non-agricultural purposes, households are not entitled to buy land using the credit. Also, there is no evidence that households sell land to become eligible to get microcredit.

[27] Descriptive statistics for this sub-group is not reported here, but similar results are available in Islam (2008).

characteristics of households of treated and non-treated groups, and not to the participation in a microcredit program. In addressing the issue of possible selection bias, we first note from Table 2.1 that many of the characteristics at the household level are not statistically different between the treatment and control groups. Any remaining differences have been accounted for by using propensity score weights which also significantly reduce the differences between the two groups of households. Selection biases resulting from unobservable variables have been further addressed using the IV strategy. Nonetheless there may still remain some potential confounders that would violate the exclusion restriction. Given our efforts to control for confounders, the risk of such distortions does not seem large.

In order to substantiate the above claim, we check the robustness of the results using alternative approaches. We first use regression-adjusted years of education for older siblings: children who are 16-20 years old. This group of children is less likely to be affected by their parents' microcredit since they would have completed secondary school or dropped out before their parents obtained microcredit. We find no statistically significant difference between the children of treatment and control groups (t-ratio = 0.7). This result is also consistent with our finding that older children are less adversely affected by microcredit because their schooling period is less exposed to microcredit and younger children can work in household enterprise instead of their older siblings. Furthermore, the opportunity cost of using child labour in household enterprise increases with the child's age. If they are at an advanced stage in school, much of the investment in schooling that had already been made would be forfeited or, if they are engaged in market work, their higher wage earnings would need to be sacrificed.

Next, we also control selection bias using an alternative method. We consider corrections for endogeneity using reduced-form residuals that lead to a control function method of accounting for both selection and endogeneity.[28] This is also important if the impact of microcredit varies across households. In that case, IV/2SLS may not estimate the average treatment effect of credit. There are, however, different approaches to estimate control functions, and not all these procedures produce consistent estimates of the treatment effect. We adopt the procedure suggested by Vella (1993) which identifies correctly the treatment effect parameter in our context.[29] We first obtain generalised residuals using either tobit (for credit as the treatment

[28] The control function approach estimates the average treatment effect by controlling directly for the correlation between the error term and the outcome of equations with the treatment variable. It treats the selection bias problem as an omitted variable problem and augments the outcome equation by a term to control for this omission. The traditional example is the Heckman's sample selection model that augments the outcome equation by an estimate of the Mills ratio.

[29] Garen (1984) suggests a linear control function estimator to correct for endogeneity. However, Garen's approach is appropriate when the dependent variable in the first stage can take a value over a continuous range and it should be uncensored. Similarly the two stage conditional maximum likelihood approach of Rivers and Vuong (1988) is not

variable) or probit (for binary treatment indicator) for the reduced-form first-stage equation, and then use the estimated residuals as an additional regressor in the second stage.[30] The results, available upon request from the authors, are similar to those reported before.

2.6.2 Alternative Measures of Children's Educational Attainment

In this sub-section, we consider the impact of microcredit participation on various alternative measures of children's educational attainment. While the previous measure of school enrolment has the advantage of capturing the current status of school age children, it does not measure the achievement of those who are not in school at the time of the survey. Two alternative measures are the number of years of school completed and the 'education gap.' In Bangladesh, children are expected to start school at around the age of six. Therefore, we can construct a variable 'education gap' to measure the achievement in terms of grade completion for a given age. The education gap can be defined as:

Education Gap = *max*{0, Expected education – Actual Education}, where

$$\text{Expected Education} = \begin{cases} 0 \text{ if } \text{age} \le 6 \\ \text{age} - 6 \text{ if } 7 \le \text{age} \le 16 \end{cases}$$

For example, if a child successfully stayed at school as expected, the gap is zero. If a child encountered problems such as late entry, failed grades, or dropping out, then the gap is a positive number. If a child never attended school, then the gap is the level of expected education at that age. Finally, following Patrinos and Psacharopoulos (1997), educational attainment is obtained by defining a grade-for-age dependent variable as 100*[Education Grade/Expected Education], where Education Grade is the number of years a child successfully completed in school.

The control function estimates using the above educational achievement measures are reported in Table 2.9. The results are based on OLS regressions in the second stage for each of these measures.[31] The negative coefficients for grade completion and grade-for-age, and the positive coefficient for education gap all imply that participation in microcredit program adversely affects children's grade achievement. Once more, girls are more adversely affected than boys: coefficient estimates for girls are larger than for boys and statistically significant at the 1 percent level. The effects on boys' school achievement are not statistically significant in general. Once again, the girls are more adversely affected by their parents' participation in microcredit. For

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applicable as the approach also requires that the credit variable be continuous (see Vella 1993, Ravallion and Wodon 2000).

[30] This model is identified even without the exclusion restrictions because of the non-linearity of the residuals.

[31] We use OLS instead of conventional tobit, because in the first stage we estimate credit demand using tobit. Using tobit in the second stage then creates further difficulty in consistently estimating coefficients unless the first stage is exactly correctly specified (see Angrist 2001). This is not the case if we use OLS.

example, women's participation in microcredit reduces girls' education by about three years in schooling while the corresponding decrease for boys' education is about 0.2 years. Male participation has a larger negative impact on girls' grade completion: 3.8 years reduction in schooling compared to the girls from control households. The results from three alternative measures reveal similar pictures which are not particularly surprising since the three measures are likely to be highly correlated. The coefficient of the residual from the first stage provides an exogeneity test. Most of the results reject the exogeneity of credit. Adding square or higher order terms of the control function does not change the sign of the coefficient, and the magnitude of the coefficients becomes stable. The higher order terms are also not statistically significant.

[INSERT TABLE 2.9 HERE]

2.6.3 Are Children Really Working in Household Enterprises?

Our results so far indicate that microcredit adversely affects children's school enrolment and their educational achievement. A possible explanation for this is that microcredit increases demand for labour in household enterprises set up with microcredit, which may cause children's time to be diverted away from school into household enterprises, since the size of loan is not large enough to hire external labour. We examine this issue below. We classify a child's current status into five different categories based on the detailed occupational information collected during the survey. These are (1) self-employment activity (in household enterprise), (2) agricultural activity, (3) day labour, (4) service-related activity, and (5) student (enrolled in school). We run a multinomial logit model where the parameter of interest is the coefficient corresponding to the instrumented credit variable obtained from Equation (2.9). Table 2.10 reports the odds-ratios and corresponding marginal effects of the treatment variable.[32] The results show that, for a child in a treated household, the odds of being in self-employment activities instead of being enrolled in school are more than doubled. The corresponding marginal effect indicates that the probability that children of treated households work in household enterprises is 26.6 percent higher than those of non-treated households. The odds-ratio is higher and negative for agricultural activity. However, the corresponding marginal effect is economically insignificant. All other coefficient estimates (day labour and service-related activities) are not statistically significant. Finally the marginal effect for the student category implies that children of treated households have a 26.6 percent lower chance of being enrolled in school than those of non-treated households. Overall, these results support the explanation that children of treated households are more likely to work for their parents in household enterprises set up with microcredit.

[32] We conducted a Hausman-like test to examine whether the maintained assumption of independence of irrelevant alternatives (IIA) is appropriate in our case. The results do not reject the null hypothesis that IIA holds, hence the multinomial logit model is suitable for the data.

[INSERT TABLE 2.10 HERE]

2.7 Discussion and Conclusion

Our results indicate that household borrowing from microcredit institutions may exacerbate the problem of child labour in Bangladesh. This finding is consistent with the relevant literature. The present findings identify the differential gender-specific impact of microcredit on children. Girls are more likely to be put to work in household enterprises. Since households face constraints in hiring labour for household enterprises, this finding suggests that families adjust to this constraint by allocating resources away from daughters. Evidence in support of this practice is consistent with the return to schooling literature. This also supports the popular perception that parents in developing countries perceive boys' education to yield a higher expected return since they can lay claim to the resources of boys in the future. Girls, on the other hand, get married out to another family and contribute resources to the family of their husband. Consequently, from the perspective of parents, investing in girls' education is not as beneficial. In sum, increasing access to rural microcredit need not improve school enrolment and literacy in a developing country like Bangladesh. Even though the joint-liability lending mechanism reduces many of the informational asymmetry problems, it creates a potentially important moral hazard within the household. Improving the short-term welfare of a household by easing the credit constraint with the provision of microcredit may exacerbate long-term poverty if it reduces the schooling of children.

Our results suggest that children more exposed to the program are more likely to end up working as their parents obtain microcredit finance. We obtain qualitatively similar results when we estimate the impact on student achievement. We check the robustness of the results using an alternative estimation strategy, and obtain similar results. We find that the impact estimates vary with the income and education level of the family: while poorer children are less likely to attend school and more likely to work, those from relatively less poor families are not strongly affected by microcredit borrowing. Poorer households are more vulnerable to keep their girl children in school. Credit-constrained poor households may not send their children to school when credit becomes available. But credit (or aid) tied directly to child schooling may have different implications (Ravallion and Woodon 2000). Accordingly, the present results are not directly comparable with the findings of the educational borrowing constraints literature.

Our findings resoundingly caution that microcredit may not be the ultimate panacea for poverty. The typical lending terms establish incentives for borrowers to sacrifice the education of their children in order to service their loans. In Bangladesh, there is parity in terms of school enrolment between boys and girls. Since, according to the present findings, there is less displacement of boys from schooling compared to that of girls, government policy directed at the gender imbalance in education may turn out to be less effective in the presence of highly active MOs. In general, developing countries that pursue gender equality in education, improving schooling and eliminating child labour may find it increasingly hard to do so in the presence of large microcredit programs. A number of policies can be adopted to mitigate the adverse consequences for child labour and schooling so that microcredit can benefit both current and future generations (Wydick 1999). MOs can extend the gestation period between the actual loan disbursement and the start of repayment. This allows many households to invest in suitable investment projects where they may find a greater balance between employing children at household enterprises and sending them to school.

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Table 2.1- Descriptive Statistics

Variables	Treatment (I)	Control (II)	Difference III=(I-II)
Child Characteristics (7-16 years old)			
Child in work (in percentage)			
Boys	0.169	0.138	**0.030**
Girls	0.110	0.093	0.017
Child in school (in percentage)			
Boys	0.693	0.747	**-0.055**
Girls	0.766	0.789	-0.023
Age of child (in years)	11.497	11.494	0.003
Sex of child (in percentage)	0.557	0.556	0.001
Household Characteristics			
Mother age (in years)	37.66	38.14	-0.48
Mother schooling (years of education)	1.09	1.54	**-0.45**
Father age (in years)	45.85	46.80	**-0.95**
Father schooling (years of education)	2.64	3.20	**-0.56**
Household size	6.56	6.48	-0.08
Number of children			
0-6 years	0.81	0.79	0.02
6-16 years	2.79	2.66	**0.13**
Maximum education by any household member			
Male borrower (years of education)	4.78	5.29	**-0.50**
Female borrower (years of education)	4.17	4.57	**-0.40**
Amount of land (in decimals)	64.7	91.2	**-26.6**
Village Characteristics	Program village (I)	Control village (II)	Difference III=(I-II)
Primary school (%)	86.25	90.91	-4.66
Secondary school (%)	27.27	31.25	-3.98
Union health centre (%)	17.5	10	7.5
Distance to nearest sub-district (km)	7.14	11.91	**-4.77**
Presence of a grocery market (%)	22.5	18.2	4.3
Presence of bus stand (%)	15	9.1	5.9
Presence of post office (%)	20	18.2	1.8
Presence of telephone office (%)	6.3	9.1	-2.8
Presence of UP office (%)	13.8	18.2	-4.4

Notes: The third column presents the difference between columns (1) and (2). Differences that are statistically significant at less than five percent are marked bold.

Table 2.2- Impact Estimates of the Participation in Microcredit Program on Child Labour

	LPM			Probit		
	No Control	Basic Control	Full Control	No Control	Basic Control	Full Control
	(1)	(2)	(3)	(4)	(5)	(6)
Women and Men's credit						
All	-0.0146	0.0252	0.0801	-0.0144	0.0192	0.0541
	(0.0291)	(0.0290)	(0.0441)+	(0.0287)	(0.0219)	(0.0023)+
	[0.0228]	[0.0276]	[0.0418]+	[0.0225]	[0.0203]	[0.0304]+
Boys	-0.0433	-0.0121	0.0034	-0.043	-0.0136	-0.0106
	(0.0357)	(0.0383)	(0.0583)	(0.0354)	(0.0297)	(0.0418
	[0.0320]	[0.0370]	[0.0526]	[0.0316]	[0.0280]	[0.0383
Girls	0.0161	0.0502	0.1367	0.0158	0.0372	0.0794
	(0.0373)	(0.0394)	(0.0594)**	(0.0363)	(0.0241)	(0.0336)**
	[0.0309]	[0.0395]	[0.0609]**	[0.0301]	[0.0239]	[0.0351]**
Women's Credit						
All	-0.0134	0.0335	0.087	-0.0133	0.0276	0.0558
	(0.0285)	(0.0289)	(0.0456)+	(0.0281)	(0.0213)	(0.0302)+
	[0.0231]	[0.0284]	[0.0426]**	[0.0228]	[0.0203]	[0.0302]+
Boys	-0.042	-0.0029	0.0129	-0.0417	-0.0018	-0.0093
	(0.0353)	(0.0369)	(0.0590)	(0.0349)	(0.0261)	(0.0377)
	[0.0325]	[0.0377]	[0.0861]	[0.0320]	[0.0257]	[0.0258]+
Girls	0.0172	0.0611	0.1426	0.0168	0.0442	0.0835
	(0.0370)	(0.0400)	(0.0610)**	(0.0361)	(0.0241)+	(0.0348)**
	[0.0312]	[0.0409]	[0.0626]**	[0.0304]	[0.0245]+	[0.0363]**
Men's Credit						
All	-0.0187	0.0252	0.0774	-0.0184	0.0199	0.064
	(0.0426)	(0.0503)	(0.0746)	(0.0420)	(0.0323)	(0.0378)+
	[0.0363]	[0.0452]	[0.0681]	[0.0358]	[0.0293]	[0.0373]+
Boys	-0.0671	-0.0285	-0.0239	-0.0664	-0.0233	-0.0112
	(0.0550)	(0.0643)	(0.0912)	(0.0542)	(0.0435)	(0.0456)
	[0.0506]	[0.0603]	[0.0841]	[0.0497]	[0.0405]	[0.0426]
Girls	0.0345	0.0685	0.1507	0.0339	0.0416	0.1008
	(0.0570)	(0.0690)	(0.1094)	(0.0556)	(0.0285)	(0.0421)**
	[0.0497]	[0.0654]	[0.1040]	[0.0483]	[0.0273]	[0.0439]**

Notes: All the results are the marginal effects of instrumented credit variable using IV regressions. The regressions include child, household, village characteristics and district fixed effects (except the first and fourth columns). 'Basic control' is the subset of 'full control' and includes some household and child demographic variables. Standard errors presented in parentheses are corrected for clustering at the village level using the formulas in Liang and Zeger (1986), while those in brackets are corrected for clustering at the household level. The coefficients and the standard errors are multiplied by the average credit borrowed by the respective group of households. All the estimates are also weighted propensity scores. Coefficients with + are significant at the 10%, those with ** at the 5%, and those with * at the 1%.

Table 2.3- Impact Estimates of the Participation in Microcredit Program on Children's School Enrolment

	LPM			Probit		
	No Control	Basic Control	Full Control	No Control	Basic Control	Full Control
	(1)	(2)	(3)	(4)	(5)	(6)
Women and Men's Credit						
All	-0.0924	-0.0915	-0.1588	-0.0925	-0.0959	-0.1623
	(0.0468)+	(0.0488)+	(0.0688)**	(0.0470)**	(0.0493)+	(0.0273)**
	[0.0327]*	[0.0410]**	[0.0609]*	[0.0330]*	[0.0416]**	[0.0622]*
Boys	-0.0658	-0.0686	-0.0756	-0.0657	-0.0938	-0.0661
	(0.0513)	(0.0569)	(0.0825)	(0.0516)	(0.0613)	(0.1148)
	[0.0432]	[0.0500]	[0.0768]	[0.0434]	[0.0547]+	[0.1049]
Girls	-0.1208	-0.0938	-0.2261	-0.1211	-0.0765	-0.1918
	(0.0596)**	(0.0600)	(0.0905)**	(0.0600)**	(0.0553)	(0.0850)**
	[0.0432]*	[0.0568]+	[0.0837]*	[0.0437]*	[0.0527]	[0.0782]**
Women's Credit						
All	-0.0908	-0.0969	-0.1717	-0.0908	-0.1032	-0.1733
	(0.0452)**	(0.0475)**	(0.0694)**	(0.0454)**	(0.0476)**	(0.0685)**
	[0.0332]*	[0.0424]**	[0.0620]*	[0.0334]*	[0.0427]**	[0.0632]*
Boys	-0.0655	-0.0752	-0.0965	-0.0654	-0.1043	-0.1237
	(0.0504)	(0.0549)	(0.0846)	(0.0507)	(0.0593)+	(0.0863)
	[0.0441]	[0.0515]	[0.0780]	[0.0443]	[0.0561]+	[0.0839]
Girls	-0.1179	-0.1017	-0.2296	-0.1182	-0.0842	-0.194
	(0.0590)**	(0.0603)+	(0.0921)**	(0.0594)**	(0.0549)	(0.0863)**
	[0.0436]*	[0.0583]+	[0.0853]*	[0.0441]*	[0.0538]	[0.0798]**
Men's Credit						
All	-0.0834	-0.0629	-0.1423	-0.0833	-0.0747	-0.1607
	(0.0664)	(0.0814)	(0.1108)	(0.0665)	(0.0790)	(0.1067)
	[0.0515]	[0.0654]	[0.0984]	[0.0516]	[0.0634]	[0.0954]+
Boys	-0.0295	-0.0153	-0.0194	-0.0293	-0.0482	-0.0781
	(0.0807)	(0.0980)	(0.1372)	(0.0803)	(0.1015)	(0.1356)
	[0.0681]	[0.0803]	[0.1223]	[0.0678]	[0.0838]	[0.1240]
Girls	-0.1422	-0.0966	-0.2635	-0.1434	-0.0867	-0.2278
	(0.0873)	(0.1027)	(0.1496)+	(0.0886)	(0.0900)	(0.1217)+
	[0.0683]**	[0.0900]	[0.1422]+	[0.0692]+	[0.0804]	[0.1161]**

Notes: All the results are the marginal effects of instrumented credit variable using IV regressions. The regressions include child, household, village characteristics and district fixed effects (except the first and fourth columns). 'Basic control' is the subset of 'full control' and includes some household and child demographic variables. Standard errors presented in parentheses are corrected for clustering at the village level using the formulas in Liang and Zeger (1986), while those in brackets are corrected for clustering at the household level. The coefficients and the standard errors are multiplied by the average credit borrowed by the respective group of households. All the estimates are also weighted propensity scores. Coefficients with + are significant at the 10%, those with ** at the 5%, and those with * at the 1%.

Table 2.4- Effects of Control Variables on School Enrolment and Child Labour

	School Enrolment		Child Labour	
	LPM	Probit	LPM	Probit
Control Variables	(1)	(2)	(3)	(4)
Age of child	0.257	0.271	-0.096	-0.005
	(0.023)*	(0.022)*	(0.018)*	(0.014)
Age of child squared	-0.012	-0.013	0.006	0.001
	(0.001)*	(0.001)*	(0.001)*	(0.001)**
Number of younger siblings	-0.015	-0.015	0.023	0.014
	(0.008)+	(0.009)+	(0.006)*	(0.003)*
Number of older sister	0.016	0.018	-0.011	-0.01
	(0.006)*	(0.007)*	(0.004)*	(0.004)*
Sex of household head	0.128	0.119	-0.103	-0.071
	(0.054)**	(0.067)+	(0.032)*	(0.032)**
Whether mother is present in the family	0.039	0.019	-0.051	-0.026
	(0.051)	(0.060)	(0.042)	(0.034)
Highest education of any member	0.038	0.039	-0.02	-0.013
	(0.003)*	(0.004)*	(0.002)*	(0.002)*
Age of household head	-0.004	-0.004	0.004	0.002
	(0.001)*	(0.001)*	(0.001)*	(0.001)*
Education of household head				
(0-4 years of schooling)	0.064	0.033	-0.051	-0.017
	(0.031)**	(0.049)	(0.025)**	(0.030)
(5-9 years of schooling)	0.044	0.004	-0.016	0.008
	(0.024)+	(0.042)	(0.017)	(0.025)
Years of mother’s schooling	-0.002	0.004	0	-0.004
	(0.003)	(0.006)	(0.003)	(0.003)
Village Characteristics:				
Presence of primary school	-0.02	-0.024	0.008	0.007
	(0.032)	(0.029)	(0.025)	(0.014)
Presence of secondary school or college	0.052	0.058	-0.011	-0.011
	(0.019)*	(0.018)*	(0.014)	(0.009)
Presence of religious school	0.026	0.022	-0.008	-0.002
	(0.034)	(0.034)	(0.028)	(0.018)
Presence of health facility	0.041	0.04	-0.017	-0.01
	(0.020)**	(0.023)+	(0.018)	(0.013)
Presence of brick-built road	0.066	0.065	-0.048	-0.031
	(0.027)**	(0.027)**	(0.017)*	(0.011)*
Presence of grocery market	-0.082	-0.09	0.033	0.026
	(0.021)*	(0.025)*	(0.012)*	(0.010)*
Presence of bus stand	-0.072	-0.072	0.058	0.043
	(0.031)**	(0.042)+	(0.020)*	(0.022)+
Distance to nearest sub-district (in km)	-0.002	-0.001	0.001	0.001
	(0.002)	(0.002)	(0.001)	(0.001)
Adult male wage	-0.001	-0.001	0.001	0.001
	(0.001)	(0.001)	(0.001)	(0.001)
Rice price	0	0.001	0.006	0.003
	(0.011)	(0.011)	(0.008)	(0.005)
Number of observations	4277	4277	4277	4277
R-squared	0.22		0.23	

Notes: Regressions also include dummies for birth-order, dummies for land-holding, presence of post-office and instrumented credit variable. All the coefficient estimates are the marginal effects. Standard errors presented in parentheses are corrected for clustering at the village level using the formulas in Liang and Zeger (1986) and using the propensity score weighting scheme. Coefficients with + are significant at the 10%, those with ** at the 5%, and those with * at the 1%.

Table 2.5- Impact Estimates Based on Binary Participation Measure on School Enrolment and Child Labour

	Child is in school		Child is in work	
	LPM	Probit	LPM	Probit
Women's Credit				
Boys	-0.1690	-0.1878	0.0193	-0.0120
	(0.1374)	(0.1463)	(0.0956)	(0.0590)
	[0.1240]	[0.1353]	[0.0879]	[0.0567]
Girls	-0.4782	-0.4438	0.2569	0.1369
	(0.1359)*	(0.1322)*	(0.0946)*	(0.05376)**
	[0.1309]*	[0.1281]*	[0.0944]*	[0.05375]**
Men's Credit				
Boys	-0.1190	-0.1634	0.0097	-0.0325
	(0.1744)	(0.1769)	(0.1175)	(0.0663)
	[0.1636]	[0.1702]	[0.1176]	[0.0679]
Girls	-0.5828	-0.5153	0.3019	0.0842
	(0.1782)*	(0.1521)*	(0.1357)**	(0.04224)**
	[0.1800]*	[0.1571]*	[0.1313]**	[0.03974]**

Notes: All the results are the marginal effects of instrumented binary treatment indicator variable using IV regressions. The regressions include full control using child, household, village characteristics and district fixed effects. Standard errors presented in parentheses are corrected for clustering at the village level using the formulas in Liang and Zeger (1986) and using the propensity score weighting scheme. Coefficients with ** are significant at the 5%, and those with * at the 1%.

Table 2.6- Impact Estimates Based on Children's Age Group

	Child is in school		Child is in work	
	Age 7-12	Age 12-16	Age 7-12	Age 12-16
Women's credit				
Boys	-0.3274	0.1978	0.0491	-0.1784
	(0.1557)**	(0.2952)	(0.0413)	(0.2156)
Girls	-0.3295	-0.5991	0.0785	0.2818
	(0.1507)**	(0.2224)*	(0.0401)+	(0.1416)**
Men's credit				
Boys	-0.2329	0.0491	0.0051	-0.1673
	(0.1612)	(0.3816)	(0.0312)	(0.2666)
Girls	-0.4131	-0.6178	0.0382	0.1077
	(0.1877)**	(0.7428)	(0.0398)	(0.2457)

Notes: All the results are the probit marginal effects of instrumented binary treatment indicator variable using IV regressions. The regressions include full control using child, household, village characteristics and district fixed effects. Standard errors presented in parentheses are corrected for clustering at the village level using the formulas in Liang and Zeger (1986) and using the propensity score weighting scheme. Coefficients with + are significant at the 10%, those with ** at the 5%, and those with * at the 1%.

Table 2.7- Impact Estimates Based on Parental Education

Education of Household Head	Child is in school			Child is in work		
	All children	Boys	Girls	All children	Boys	Girls
Probit						
Low	-0.2931	-0.2235	-0.3477	0.0966	0.0220	0.1373
	(0.0705)*	(0.0839)*	(0.0878)*	(0.0317)*	(0.0410)	(0.0346)*
Middle	-0.0333	0.0415	-0.0920	0.0319	-0.0783	0.1019
	(0.0736)	(0.0911)	(0.1009)	(-0.0338)	(-0.0546)	(0.0367)*
High	0.0472	0.0887	0.0322	-0.0080	-0.1087	0.0649
	(0.1115)	(0.1446)	(0.1220)	(0.0470)	(0.0730)	(0.0472)
LPM						
Low	-0.2756	-0.1938	-0.3479	0.1302	0.0471	0.1972
	(0.0670)*	(0.0797)**	(0.0854)*	(0.0424)*	(0.0526)	(0.0524)*
Middle	-0.0452	0.0420	-0.1191	0.0617	-0.0653	0.1574
	(0.0652)	(0.0797)	(0.0894)	(0.0400)	(0.0585)	(0.0510)*
High	0.0046	0.0668	-0.0454	0.0082	-0.0925	0.1011
	(0.0914)	(0.1223)	(0.0936)	(0.0530)	(0.0769)	(0.0635)

Notes: *Low* refers to households where the highest level of education obtained by parents is primary (0-4 years of schooling) or less; *Middle* refers to households where at least one of the parents obtained more than primary but less than a high school degree (5-10 years of schooling), and *High* refers households where one of the parents obtained at least a high school degree (11 or more years of schooling). All the results are the marginal effects of instrumented credit interacted with education dummies using IV regressions. The regressions include full control using child, household, village characteristics and district fixed effects. Standard errors in parentheses are corrected for clustering at the village level using the formulas in Liang and Zeger (1986) and using propensity score weighting scheme. Coefficients with ** are significant at the 5%, and those with * at the 1%.

Table 2.8- Impact Estimates Based on Land Ownership

	Child is in school		Child is in work	
	LPM	Probit	LPM	Probit
Poorer Households				
Boys	0.136	0.115	-0.085	-0.078
	(0.119)	(0.139)	(0.073)	(0.044)+
Girls	-0.231	-0.238	0.098	0.089
	(0.123)+	(0.128)+	(0.098)	(0.070)
Less Poor Households				
Boys	-0.331	-0.328	0.260	0.451
	(0.844)	(0.848)	(0.696)	(0.476)
Girls	0.017	0.073	-0.007	0.019
	(0.824)	(0.591)	(0.568)	(0.057)

Notes: Poorer household are those who own ≤ ½ acre of land and less poor households own more than ½ acre land. The regressions include full control using child, household, village characteristics and district fixed effects. The coefficients and the standard errors are multiplied by the average credit borrowed by the respective group of households. Standard errors presented in parentheses are corrected for clustering at the village level using the formulas in Liang and Zeger (1986) and using propensity score weighting scheme. Coefficients with + are significant at the 10%.

Table 2.9- Impact of the Microcredit Program on Children's School Achievement

	Boys			Girls		
Female Borrower	Grade Completion	Education Gap	Grade-for-age	Grade Completion	Education Gap	Grade-for-age
Treatment effect	-0.196	-0.092	-21.647	-2.953	2.752	-48.393
	(0.617)	(0.611)	(12.912)+	(0.738)*	(0.696)*	(16.089)*
Control function	0.158	0.143	22.286	2.963	-2.755	49.405
	(0.624)	(0.617)	(13.283)+	(0.745)*	(0.705)*	(16.256)*
Male Borrower						
Treatment effect	-0.423	0.043	-23.727	-3.773	3.39	-72.497
	(0.846)	(0.817)	(17.603)	(0.961)*	(0.923)*	(21.299)*
Control function	0.327	0.047	25.084	3.537	-3.199	69.099
	(0.787)	(0.765)	(16.855)	(0.958)*	(0.921)*	(21.373)*

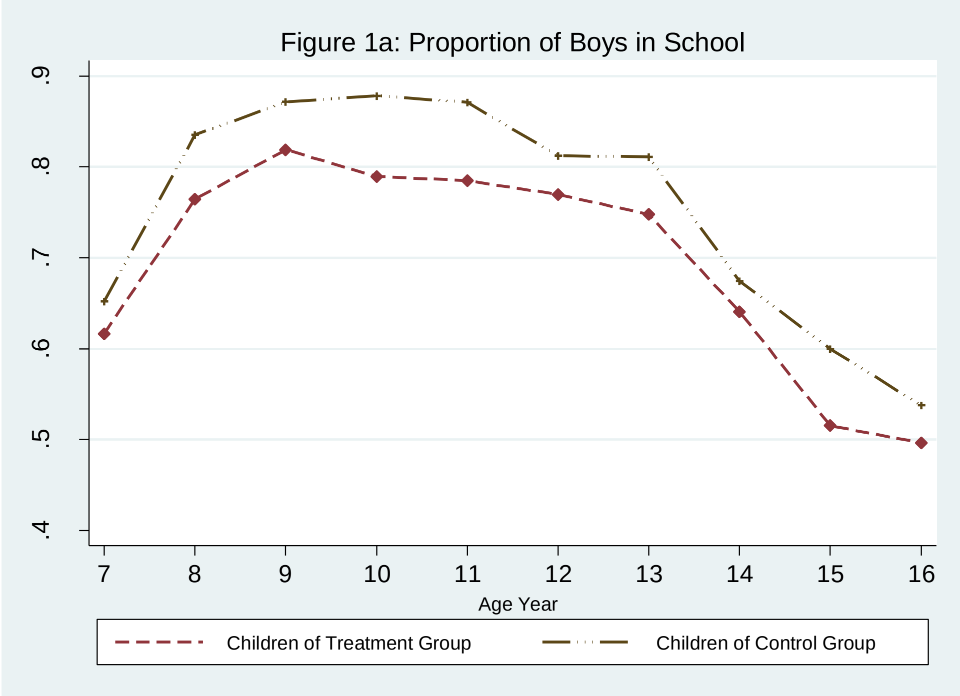
Notes: All the results are estimated using the control function method. The regressions include full control using child, household, village characteristics and district fixed effects. The coefficients and the standard errors of treatment effects are multiplied by the average credit borrowed by male and female borrowers. Standard errors presented in parentheses are corrected for clustering at the village level using the formulas in Liang and Zeger (1986) and using propensity score weighting scheme. Coefficients with + are significant at the 10%, and those with * at the 1%.

Table 2.10- Multinomial Logit Model for Children's Work/School Status

Child Occupation	Coefficient	Marginal Effect
Self-employment activity	2.03	0.266
	(0.71)*	(0.095)*
Agriculture	-6.48	0.000
	(1.73)*	(0.0000)*
Day labourer	1.33	0.0000
	(1.07)	(0.001)
Service-related activity	2.42	0.0000
	(4.50)	(0.0000)
Enrolled in school		-0.266
		(0.096)*

Notes: The regressions include full control using child, household, village characteristics and district fixed effects. Standard errors presented in parentheses are corrected for clustering at the village level using the formulas in Liang and Zeger (1986). Coefficients with * are significant at the 1%.

Figure 2.1- Proportion of Children in School



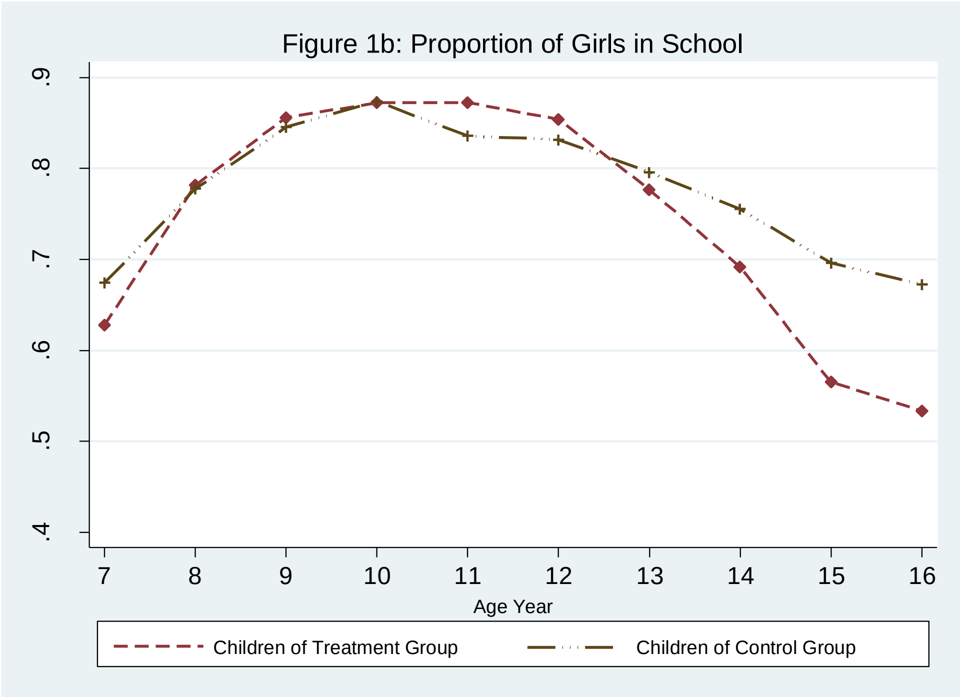
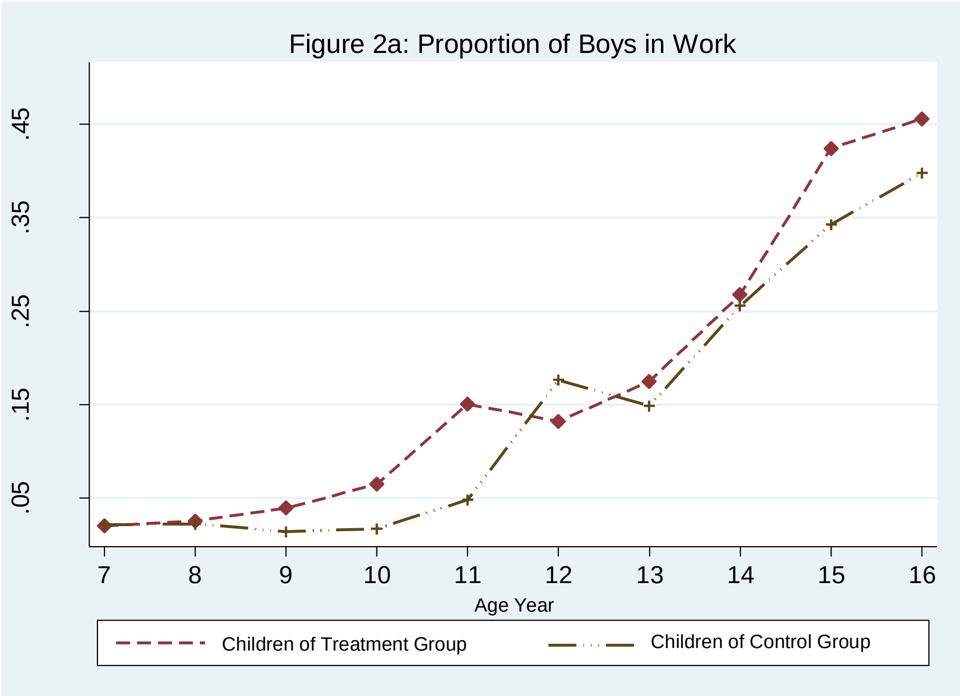


Figure 2.2- Proportion of Children in Work



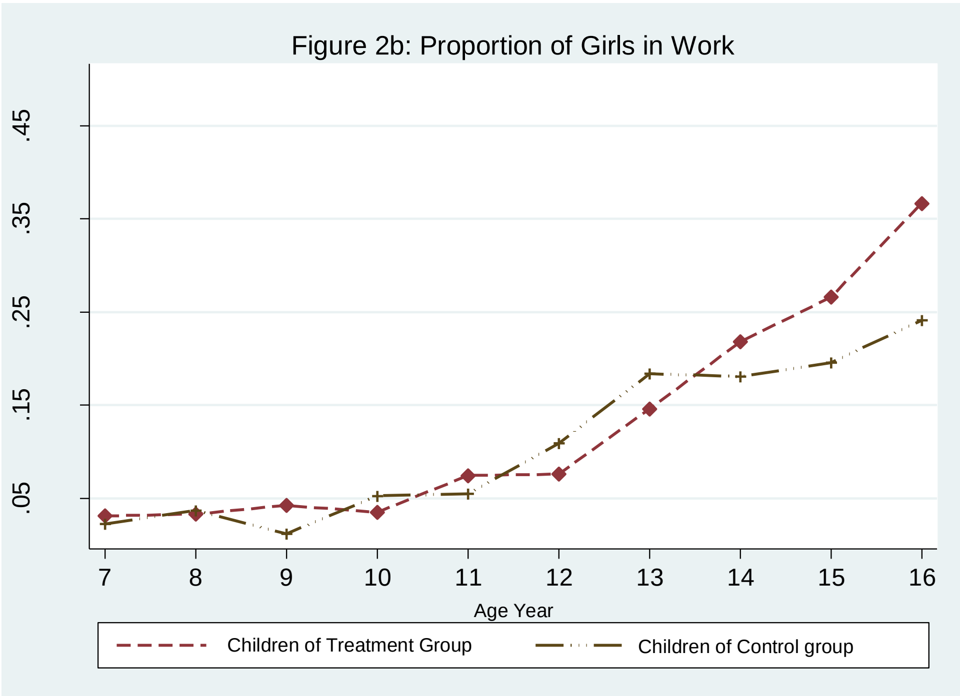
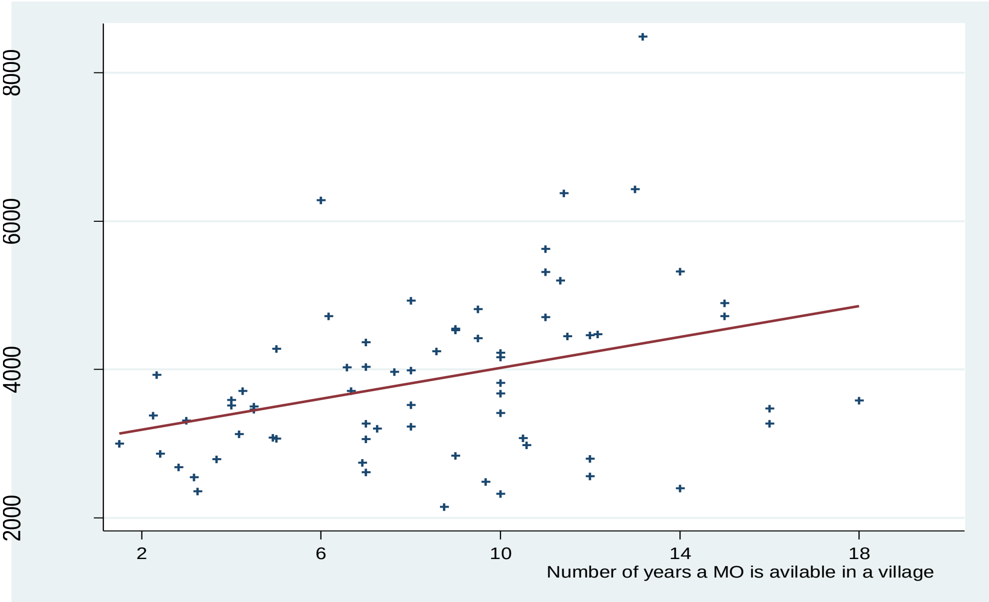
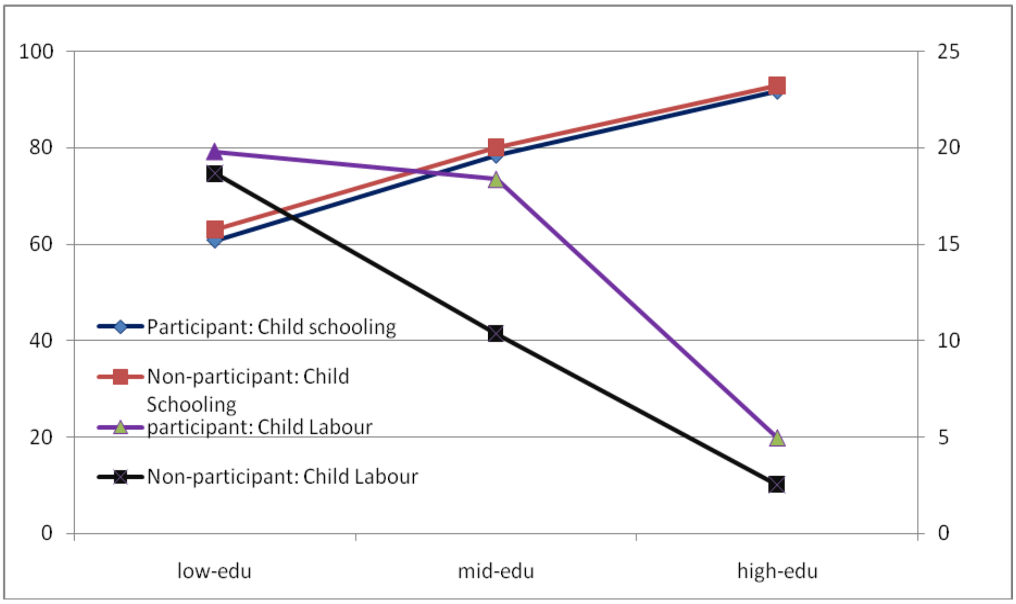


Figure 2.3- Yeas of Microfinance Program in a Village and the Amount of Credit Borrowed by Households



Notes: Average credit per household in a village is the amount of credit borrowed (in taka) by all households divided by the number of participating households in the program village. Number of years a MO is available in a village is the period from which microcredit is first avilable in the program village.

Figure 2.4- School Enrolment and Child Labour at Different Levels of Parental Education



Note: *Low-edu* refers to those households where the highest level of education obtained by parents is primary (0-4 years of schooling) or less; *Mid-edu* refers to households where at least one of the parents obtained more than primary but less than a high school degree (5-10 years of schooling), and *High-edu* includes households where one of the parents obtained at least a high school degree (11 or more years of schooling).

Appendix 2

Variables Included in the Regression:

<u>Basic Control:</u>

Child characteristics: Age, Age square, Sex*

Household characteristics: landholding (less than one acre, one acre to 2 acres, 2 acres to less than 5 acre, more than 5 acres), mother's education, maximum education attained by any member of the household, age of father, sex of household head.

Village characteristics: Presence of primary school, secondary school.

*Sex is included when combined regression is run.

<u>Full Control</u>: Basic Control +

Child characteristics: Sex*Age, first-born, second-born, third-born, fourth-born, fifth or higher born, number of younger siblings, number of elder sisters.

Household characteristics: Number of children 0-6, 7-15, education of father (low (0-4 years), medium (5-10 years), high (11 and above)), age of mother, presence of mother.

Village characteristics: religious school, Distance to nearest school, child wage, adult wage, presence of brick-built road, presence of hospital, post office, grocery market, bus stand, distance to nearest sub-districts, price of rice.

PART B: Suggested Declaration for Thesis Chapter

[This declaration to be completed for each conjointly authored publication and to be placed at the start of the thesis chapter in which the publication appears.]

Monash University

Declaration for Thesis Chapter 3

Declaration by candidate

In the case of Chapter 3, the nature and extent of my contribution to the work was the following:

Nature of contribution	Extent of contribution (%)
Literature review, empirical estimation and interpretation	70

The following co-authors contributed to the work. Co-authors who are students at Monash University must also indicate the extent of their contribution in percentage terms:

Name	Nature of contribution	Extent of contribution (%) for student co-authors only
Dietrich K Fausten	Critique, discussion and revision of ms	

Candidate's Signature	[redacted]	**Date** 29/01/09

Declaration by co-authors

The undersigned hereby certify that:

(1) the above declaration correctly reflects the nature and extent of the candidate's contribution to this work, and the nature of the contribution of each of the co-authors.
(2) they meet the criteria for authorship in that they have participated in the conception, execution, or interpretation, of at least that part of the publication in their field of expertise;
(3) they take public responsibility for their part of the publication, except for the responsible author who accepts overall responsibility for the publication;
(4) there are no other authors of the publication according to these criteria;
(5) potential conflicts of interest have been disclosed to (a) granting bodies, (b) the editor or publisher of journals or other publications, and (c) the head of the responsible academic unit; and
(6) the original data are stored at the following location(s) and will be held for at least five years from the date indicated below:

Location(s)	Department of Economics

[Please note that the location(s) must be institutional in nature, and should be indicated here as a department, centre or institute, with specific campus identification where relevant.]

Signature 1	[redacted]	**Date** 27/02/09

Chapter 3

Skilled Immigration and Wages in Australia

With Dietrich K Fausten

3.1 Introduction

Australia is one of the world's major host nations for immigrants. The country has benefited from the important contributions that immigrants have made to her economic performance and development. However, immigration and its appropriate magnitude continue to be matters of public debate. One prominent issue sustaining the debate is the widespread concern in host countries that immigration harms the labour market prospects of native-born workers. That concern is not restricted to Australia but lies at the heart of the debate about immigration in many countries - including most European nations, the U.S and Canada (Scheve and Slaughter 2001). Accordingly, clarification of the nexus between immigration and domestic wages is called for. Better understanding of the Australian experience, specifically of the potential effect of the skill-mix of immigrants on the domestic wage structure, may prove useful in clarifying the issues in Australia and elsewhere.

Australian immigration policy has become increasingly focused on migrant skill as an entry criterion. Between 2000 and 2006, the total annual immigrant intake by Australia was about 140 thousand. Skilled migration accounted for approximately 65 percent of the migration visas to Australia granted in 2004-05, with approximately one third of these accruing to foreign students (Productivity Commission 2006). While skilled immigration may well improve the overall employment prospects of the native labour force, it may affect adversely the relative position of native skilled workers. A priori, changes in skilled wages are likely to dominate changes in the wage differential between skilled and unskilled labour. Unskilled wages tend to be relatively unresponsive to market forces and, hence, to immigration by virtue of the minimum wage setting practice in Australia that largely relies on union-negotiated wage increases. Skilled wages are not so restrained and typically respond readily to changing labour market conditions.

Hence, in absolute terms, native skilled workers are potentially more exposed to competition from skilled migrants than are native unskilled workers.

Earlier investigations of aggregate employment and labour market outcomes for Australian-born workers dispel the popular notion that immigration reduces the level of domestic real wages. Questions of skill composition have started to receive attention only recently. Addison and Worswick (2002) find no evidence of adverse effects of immigration on either skilled or unskilled workers. Chang (2004) demonstrates that immigration cannot explain the variation in the skilled-unskilled wage differential in Australia during the 1990s. Borjas's (2003) examination of the interaction between skill composition of migrants and wage structure suggests a negative elasticity of approximately 0.35. According to his findings an immigrant influx that increases the size of a particular skill group by ten percent reduces the wages of native workers in that group by about three to four percent. He corroborates this finding in a subsequent examination of US high-skill labour markets (Borjas 2006). Card (2005) reviews the recent evidence on U.S immigration and concludes that immigration-induced changes in the skill composition of the domestic labour force have little effect on average domestic wages.

Empirical estimates using a variety of methodologies and estimation strategies in a variety of settings typically show that the effects of immigration on labour market outcomes are either very small or that they yield conflicting results. This inconclusive state warrants more work. One useful extension of existing empirical work is to employ a database that captures elements of systemic interaction such as the adjustment of wages and aggregate demand to immigration. Many authors including Friedberg and Hunt (1995), Borjas, Freeman and Katz (1996, 1997) and Borjas (2003) suggest that trends in relative wages associated with inflows of migrants should be investigated by time series analysis.[1] In this paper, we use aggregate time-series data to explore the impact of skilled immigration on wages in Australia.

A major problem in studying the impact of immigration is that the choice of host country may be endogenous. Immigrants may self-select to join labour markets in the better performing industrial countries (Friedberg and Hunt 1995). In addition, host countries may base their annual target immigration rates on a predetermined immigration policy or on domestic labour market conditions. We address the resulting endogeneity problem by exploiting the fact that Australia's immigration policy and labour market outcomes in earlier periods may serve as choice criteria for immigrants' decisions to seek admission to Australia. We use quarterly time series data

[1] Existing time series study such as Islam (2007) focuses on the short-run and long-run relationship or causality analysis (Withers and Pope 1985) between immigration and job market prospects.

covering the period 1980-2006. The start of the observation period is fixed by the date at which data for different skill categories of immigrants becomes available for Australia.[2]

We estimate the effects of immigration on Australian wages using various instrumental variable (IV) methods. Since we are using quarterly data over a period of 26 years, we need to address small sample bias problems. We do so by applying Jackknife Instrumental Variable Estimations (JIVE) (Angrist, Imbens and Krueger 1999; Blomquist and Dahlberg 1999) which is particularly suitable in our context. The choice of instruments is independently substantiated by various validity and specification tests. Our fundamental result is that neither skilled nor unskilled immigration exerts discernible adverse effects on wages in the Australian labour market. In fact, immigration may have some positive effects on aggregate wages.

3.2 Empirical Strategy

If firm output is produced by two types of workers, immigrants and native born, then we can present the production function as:[3]

$$W_t = f(I/P)_t \tag{3.1}$$

where I is the stock of skilled (or unskilled) immigrants in the Australian labour market, P is the entire domestic workforce, W_t is the is the average weekly wage of workers at time t and $(I/P)_t$ is the skilled (or unskilled) immigrant share at time t. Equation (3.1) can be interpreted as approximating the first-order condition determining the level of wages, or as a general reduced-form relationship between the domestic wage level and the immigrant share of skilled (or unskilled) workers.[4]

Estimation of equation (3.1) is potentially subject to omitted variable problems that would impart an upward bias to parameter estimates. One obvious omission is a term that represents the state of the labour market. The tightness of the market is typically captured by invoking some variant of the Phillips curve, efficiency wage models or bargaining models of wages. Higher unemployment rates weaken the bargaining position of employees and reduce the rate of wage increase. The Philips curve has been the dominant approach to modelling wage determination as it recognizes the influence of the long-run equilibrium rate of unemployment

[2] Skilled workers entry into Australia is mainly based on the points system which was introduced in the early 1970s.
[3] Equation (1) can be obtained using any standard neoclassical production function (e.g., Cobb-Douglas) and equating wage with marginal product. A similar specification is derived by Borjas (1987a) and Islam (2009) from the generalized Leontief production function. Grossman (1982) and Card (2001) obtain corresponding specifications from translog and CES-type production functions, respectively. See also Altonji and Card (1991), Butcher and Card (1991), Pischke and Velling (1997) for similar specification to examine the effect of immigration flows on aggregate labour market outcomes.
[4] Though we focus on skilled immigration in this study, we also estimate the same regression for unskilled immigrants.

on a fixed growth path. This pins down the equilibrium level of labour utilization in the economy without recourse to other behavioural equations. Including the state of the labour market as well as state dependent determinants of wages outcomes generates the expanded equation (3.2) for estimating the relationship between wages and immigration:

$$W_t = \alpha_0 + \alpha_1 (I/P)_t + \alpha_2 U_t + \alpha_3 X + \xi_t \qquad (3.2)$$

Vector *X* captures the observable time invariant determinants of wages such as state of residence of immigrants and average age of different cohorts of immigrants. U_t is the unemployment rate, ξ_t is the innovation error and α_0 is a fixed effect that captures influences other than those associated directly with the variables in the model. It may include some unobservable policy shift parameters that are not reflected explicitly in the model.

Both theory and empirical evidence suggest a positive association between wages and productivity. In a perfectly competitive market, the wage rate is determined by the productivity of the marginal worker. Given diminishing returns, an increase in the labour force through immigration should influence the wage. We need, therefore, to include a variable that captures the time-varying productivity in the determination of the aggregate trend of wages. Productivity can be defined as output per man-hour at time *t*. We don't know exactly what drives productivity, whether work effort or skill. At the level of the plant or firm, improvements in labour productivity may come from using cooperating inputs of better quality, or they may reflect technological change. Any one of these drivers could cause productivity to improve, and usually more than one factor will be involved. Inclusion of the productivity measure also controls for the capacity of the host country to harness her human and physical resources. We, therefore, model productivity as exogenous in our wage determination system (equation 3.2a)

$$W_t = \alpha_0 + \alpha_1 (I/P)_t + \alpha_2 U_t + \alpha_3 X + \alpha_4 prod_t + \varepsilon_t \qquad (3.2a)$$

where $prod_t$ represents the level of labour productivity at time *t*. After differencing over successive time periods the estimating equation assumes the form

$$\Delta W_t = \beta_1 \Delta(I/P)_t + \beta_2 \Delta U_t + \beta_3 prod_t + \Delta\varepsilon_t \qquad (3.3)$$

where $\Delta prod_t$ is the growth in labour productivity defined as the change in GDP per hour of labour worked and $\Delta(I/P)_t$ is the net immigration rate of skilled workers (or unskilled or both, depending on the population captured in the numerator). The differencing purges the equation of fixed effects and the potential biases they may introduce. It also effectively removes all time invariant variables that could possibly be included in vector *X*.

3.2.1 Endogeneity of Immigration

The estimated value of β_1 in equation (3.3) measures the impact of immigrant inflows on wages growth. It should not reflect any simultaneous causality in the opposite direction. However, immigrants are attracted to countries where their skills are in strong demand. Hence, a potential endogeneity problem arises from the choice of destination country. Furthermore, immigrants who choose to come to Australia are probably not a random subset of the source country workforce. We would expect immigrants to expect higher earnings in Australia than in their country of origin, and vice versa for those who stay (Borjas 1987b). Immigrants are, typically, ambitious, aggressive, and entrepreneurial. They, especially skilled migrants, move across international borders from one place of work and residence to another in order to exploit the economic opportunities that are accessible to them. Another potential source of endogeneity arises from the fact that the Australian immigration policy is based on past immigration rates.[5]

The endogeneity issue has previously been recognised in studies of local labour markets (Altonji and Card 1991; Friedberg 2001) but not in the context of cross-border migration. Those studies typically postulate that immigrants tend to move to cities or occupations where growth in demand for labour can accommodate their supply. Our study is not spatially based, and the endogeneity problems that may arise in the present context are at a higher level of aggregation. In terms of equation (3.3), if the migrant flow is not independent of $\Delta\varepsilon$, then the conditional correlation between wages growth and the (skilled or unskilled or total) immigration rate will confound the two directions of causation, and bias the estimate of β_1. If, for example, immigrants are more skilled, and if they choose high-skilled jobs that have better prospects of high wage growth in Australia, then the estimate of β_1 will be biased upward. Conversely, if immigrants are concentrated in relatively low-paying jobs with little or no prospect of wage growth - possibly due to lack of recognition of foreign qualifications, language barriers or a dip in the earnings just after arrival - then the estimate of β_1 will be biased downward, leading to underestimation of the effect of immigration.

The endogeneity problem can be solved by identifying a source of exogenous variation in immigration flows. In our context, such instruments must be correlated with the inflow of immigrants over time, but must be uncorrelated with the unobserved component of wages growth subsequent to the immigrant's arrival.[6] We follow Altonji and Card (1991), Card (2001), Friedberg (2001) and use the lagged share of immigrants in the labour force as an instrument.

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[5] While immigration policy is generally described as a policy that balances social, economic, humanitarian and environmental objectives, it is ultimately the government that sets the rate – presumably keeping also in mind labour market conditions and other considerations relevant to potential migrants.
[6] The instrumentation is also useful if the error term in equation (3) is correlated over time.

The argument here is that the lagged value of the immigration share acts as information to potential immigrants about Australia's policy towards immigration. Accordingly, we assume that the selection process or the immigrants' decision to enter Australia is governed by the following relationship:

$$\Delta(I/P)_t = \gamma\Delta(I/P)_{t-j} + \mu_t \qquad (3.4a)$$

where j is the lag between the decision to apply to immigrate, or setting the immigration policy at time (t-j), and actual entry at time t.

One problem with our choice of instrument could be that it does not capture the decision of every immigrant and, hence, that it explains only a part of the variation of the proportion of immigrants at time t. It follows that our instrumental variable should be interpreted as reflecting an estimate of a specific group – *viz*., those migrants whose behaviour is influenced by the instrument (Imbens and Angrist 1994). In the present context, that subset of migrants is likely to be dominated by relatively skilled workers if the possession of skill is an indication of a worker's inclination and ability to acquire and process job-relevant information.

The use of time-series data at the national level avoids any (downward) bias that could be attributable to factor price equalization and endogenous regional choice by migrants. However, it introduces a different bias toward zero: Immigrants tend to come to countries when labour market outcomes are favourable. Other potential instruments that can affect the migration decision and that are related to labour market outcomes include the unemployment rate. This indicator could be particularly relevant for those migrants who are desperately looking for jobs. Alternatively, labour market conditions may capture salient aspects of Australia's immigration policy. Australia is a growing and thriving economy with skill shortages in many areas. In order to alleviate the skill shortages the government may select immigrants on the basis of local labour market conditions. In that case, the selection process could be modelled by the following relationship:

$$\Delta(I/P)_t = \gamma\omega U_{t-j} + \upsilon_t \qquad (3.4b)$$

where ωU is the weighted average of antecedent unemployment rates, and ω is the weight. Since the immigration process from the time of the decision to migrate until the time of arrival takes considerable time, we select j=6 in our quarterly data. The weight is taken over the six-quarter period (time t-j is the weighted average of the t- j-1, t- j-2,….t-j-6 period). This specification is similar to Pischke and Velling (1997) and Dustmann, Fabbri and Preston (2005).

It is possible and, indeed, plausible that the pull of family or of the "diaspora" influences the choice of destination country. Immigrants may apply to Australia because relations and friends already live here or because of the presence of individuals with similar cultural and linguistic backgrounds. Therefore, a possible solution to the endogeneity problem is to use measures of historic settlement patterns as instruments for immigration inflows. Our use of the lagged immigrant share as an IV partly addresses this concern. However, family and "diaspora" are not prominent drivers of immigrant flows to Australia (Islam and Fausten 2007). Rather, the overwhelming impression from the available evidence is the variation over time in the pattern of immigration into Australia by source country and region. Moreover, in Australia the potential "diaspora effect" is constrained by the points system. Skilled immigrants who satisfy those criteria typically prefer countries that offer better job prospects or more favourable immigration policies and labour market conditions. Family and cultural ties tend to be of lower orders of importance in selecting their destination country.

These considerations suggest that the decision of skilled foreign workers to migrate to Australia is based on past Australian immigration policy and on the prospective migrants' prospects for success in the Australian labour market. Accordingly, we use the past immigration rate and the past unemployment rate to model exogenous variations in the current immigration rate. Schematically, the decision path is:

> Skilled (or unskilled) foreign worker → decides to leave home country → investigates labour market conditions and/or stance of immigration policy in potential host countries → selects host country (Australia) → applies to host country (Australia) for immigration (→ gets visa → arrives in Australia →looks for job → earns wage).

The exclusion restriction implied by our instrumental variable regression is that, conditional on the controls included in the regression equation (3.3), the six-quarter lagged unemployment and immigration rates have no effect on today's earnings growth other than through their effects on immigration. One concern with the exclusion restriction is that the historical (past) unemployment rate may have a direct effect on the current wage rate which may attract immigrants to Australia. To capture this effect we should include among the explanatory variables a measure of the effect of the past unemployment rate on the wage level received by immigrants. However, we are measuring the growth of wages, as opposed to their level, at time *t*, and the historical unemployment rate is unlikely to exert a prominent influence on current wage increases. The same considerations apply to the policy variable - the past immigration rate. Therefore, the implied exclusion restrictions are plausible. Since we are dealing with aggregate time-series data at the national level we do not need to worry about internal migration by natives in response to immigration inflows and subsequent changes in labour market outcomes. This concern arises when dealing with single cross-section data or local labour market situations (Pischke and Velling 1997; Dustmann, Fabbri and Preston 2005; Hatton and Tani 2005; Borjas 2006).[7]

[INSERT FIGURE 3.1 HERE]

Figure 3.1 shows a strong positive relationship between the past (6-quarter lagged) immigration rate and its current level. The visual impression is confirmed by a statistically significant positive correlation coefficient relating current to past immigration rates. This strong association corroborates our conjecture that the relationship between wages and the immigration rate is influenced by the antecedent immigration policy and the state of the labour market. Without consideration of that endogeneity the relationship between wage growth and the immigration rate might be obscured by changes in the immigration policy.

With two instruments for our single endogenous regressor we estimate equation (3.3) using two-stage least-squares (2SLS).[8] It is expected that the 2SLS estimates improve efficiency relative to OLS and provide better control for earnings growth. We account for possible serial correlation by computing Huber-White standard errors. In the presence of overidentifying restrictions it is sometimes useful to obtain a more efficient estimator when serial correlation may be present by applying the Generalized Method of Moments (GMM) conditions (Hansen, 1982). Since our 2SLS with robust standard errors is *de facto* a GMM estimator we need not conduct separate GMM estimation as this may generate only small additional gains. Moreover, given that GMM is subject to small sample bias it would not seem appropriate to apply this estimator in the present context.[9]

3.3 Data and Descriptive Statistics

Quarterly skill-based immigration data (Overseas Arrivals and Departures 3401.0) for the period 1980-2006 were obtained by special request from the ABS. The net immigration rate is expressed as the total number of immigrants in a given quarter per one thousand adult (15-64 years of age) Australians in that quarter.[10] It represents the arrival of migrants who have been

[7] One assumption we maintain here is that native skilled workers are not emigrating from Australia in response to the arrival of skilled immigrants. It is, however, possible that the overall gain in skilled workers to Australia from international labour movements may be obscuring significant losses amongst highly educated workers.

[8] In this chapter the term IV and 2SLS are not interchangeable. We refer to 2SLS estimates when we use multiple instruments, and to IV estimates in the case of a single instrument.

[9] One problem with our specification (equation 3.3) is that we may be estimating short-run effects as opposed to long-run effects of skilled (or unskilled) immigration and differencing eliminates the long-run effect. Many authors (e.g., Pischke and Veiling 1997; Friedberg 2001) have estimated the same type of equation for examining the labour market impact of immigration. An additional problem with level of wage as opposed to change in wage as the dependent variable is that variables with high persistence over time (such as weekly wage) will have very low correlation between the flow variable (immigration rate) and the level variable (wage). This problem of weak instruments can lead to substantial bias in finite samples.

[10] The "net immigration rate" usually applies to persons born outside Australia but it may also apply to a small number of persons born inside Australia to parents who are foreign nationals. Note that the migration rate used here differs from the 'net migration' rate as the data did not include individuals departing Australia. According to the Productivity Commission report (2006), in recent decades there has been a significant movement of people from

granted the right to live permanently in Australia. Measuring skilled migrant flows is problematic because the Department of Immigration and Citizenship (DIC) records inflows by visa type. Visa categories do not map directly into the general skill classifications. The DIC defines skilled migrant workers as those people who have skills in particular occupations that are *required* in Australia. These occupations are identified in the skilled occupation list. The demand list contains a list of domestic occupations and specialisations for which there is a continuing national shortage. In order to match migrants with the skill classification system, we classify skill in terms of the occupation of immigrants recorded on their landing cards at the time of their first entry into Australia. Since most of the visas granted by Immigration Australia under the skilled category fall under the general skill stream there is substantial agreement between the two definitions. Our practice reflects a preference for defining skill in terms of generic attributes of migrants rather than temporary labour market requirements in the host country. The migrant attributes provide a better guide to the extent of human capital inflow into the host country as well as to subsequent employment relations of immigrants.

The unemployment rate is the percentage of the labour force that actively seeks work but is unable to find work in a particular quarter. Nominal wage data include average weekly compensation paid during the calendar quarter to all employees in Australia, regardless of when the services were performed. Since time-series data for wages of native skilled and unskilled workers are not available for Australia, we use aggregate wages (representing the composite average wage of immigrants and natives) as the dependent variable in our regression. Labour productivity is defined as GDP (at constant prices) per hour worked. The measures of labour productivity are presented as indices and as rates of change.

[INSERT TABLE 3.1 HERE]

Table 3.1 provides descriptive statistics for the key variables of interest. The first two columns report the mean and standard deviation of the full sample. Average weekly wages of all workers have increased significantly over the observation period while unemployment has been declining. The average unemployment rate in recent years (2000-2006) is below the corresponding average over the entire observation period (columns 7-8). The average change in the unemployment rate from its immediately preceding quarter is negative. Productivity is increasing over time. However, the average change in productivity, or productivity growth, of a given quarter compared to its immediately preceding quarter has slowed in 2000-06 compared

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Australia on a long-term basis. But this proportion is relatively small for the Australian-born population. A significant share of emigrants consists of former permanent settlers and overseas visitors returning to their home countries. Moreover, a large number of Australian residents are also returning home every year from extended stays abroad. So, net Australian-born emigration is relatively low. Casual observation suggests that many Australian-born high skilled workers emigrate because of the relatively compressed domestic wage structure. However, to the extent that Australian-born and permanent residents are emigrating in response to the inflow of permanent migrants into Australia, our estimates will provide an upward bound of the true effects of immigration.

to 1990-99. The immigration rate is relatively volatile (Figure 3.2). It declined from a relatively high level of 2.5 in the initial period to 1.8 in 1990-99. However, the number and proportion of new immigrants have increased again in recent years to an average rate of 2.0 though the rate is yet to match its 1980 level (Table 3.1). The proportion of skilled migrants has increased continuously over the observation period. On average, Australia received 2.3 skilled migrants for every unskilled migrant. That ratio has increased almost threefold over the observation period, rising from 1.4 in the 1980s to 3.4 in the 1990s and to 4.1 in the most recent period. Immigrants who were not of working age or did not adequately describe their occupational status at the time of arrival were not classified as either skilled or unskilled but were included in the total immigration rate.

[INSERT FIGURE 3.2 HERE]

Figure 3.3 shows a positive but not very strong relationship between earnings growth in Australia and the immigration rate. A bi-variate regression analysis confirms that this relationship is not statistically significant. In the next section, we examine whether this apparent relationship represents any causal effect of immigration on wages or whether it is merely a statistical association.

[INSERT FIGURE 3.3 HERE]

3.4 Estimation Results

3.4.1 Ordinary Least Squares Regression

The top Panel of Table 3.2 reports the ordinary least-squares (OLS) results from regressing growth of weekly wages on each type of immigration rate, with and without controlling for changes in unemployment and productivity growth (equation 3.3).[11] In Columns 1, 4, and 7 we consider the immigration rate as the sole covariate. Columns 2, 5, and 8 control for the change in the unemployment rate but exclude productivity. Columns 3, 6, and 9 report results using the full set of covariates.

[INSERT TABLE 3.2 HERE]

Overall, the results indicate that the immigration rate has a consistently significant positive effect on wages, irrespective of specification. The first three columns show that total immigration exerts a highly significant (1% level) impact on wages in the Australian labour market. A one unit change in the immigration rate changes the growth of the average weekly wage in a particular quarter by AD$1.55-$1.67. In terms of percentages, a ten percent increase in the immigration rate is associated with a 1.9-2.0 percent rise in the average wage of all

[11] We examine changes in wages as opposed to the log specification because changes in wages between quarters are sometimes zero. In a log specification, the results can be fairly sensitive to how we deal with zero values.

workers.[12] The following two sets of three columns show that this qualitative finding applies to both component groups, skilled and unskilled migrants. The magnitude of the effect is consistently larger (approximately double) for skilled migrants than for total migrants, but the level of significance is lower (columns 4-6). The effect on wage changes is stronger in the case of unskilled migrants than skilled migrants, and the coefficients corresponding to unskilled migrants are statistically significant at the one percent level (columns 7-9). Note also that in all three alternative specifications the magnitude of the coefficient of the immigration rate diminishes as we control for both changes in the unemployment rate and productivity growth. But the coefficients and the sign of the relationship remain stable and significant.

3.4.2 Reduced Form Estimates

Based on our previous specifications, we run the following reduced-form regression:

$$\Delta W_t = \alpha_0 + \alpha_1 \Delta(I/P)_{t-6} + \alpha_2\, \omega U_{t-6} + \alpha_3\, \Delta U_t + \alpha_4 \times \Delta prod_t + \zeta_t \qquad (3.5)$$

The reduced-form results for the total immigration rate presented in Table 3.3[13] produce a fairly strong relationship between the instrument and changes in wages. The first two columns of the Table suggest that the past immigration rate and past unemployment rate each have statistically significant effects on wages growth, and that the instruments are not weak. The signs of the coefficient estimates suggest that the past immigration rate has a positive effect, while the past unemployment rate has the opposite effect on wage growth. These results indirectly support the conjecture about the endogeneity of immigration as immigration policy and labour market outcomes are potentially important determinants of the migration process. When we use both instruments in the reduced-form equation (last column, Table 3.3), the past immigration rate yields a statistically insignificant positive effect on the wage variable (t-ratio=1.44). This implies that controlling for the past unemployment rate reduces the importance of the past immigration rate in determining current wage growth. Though the magnitude of the immigration rate coefficients falls to less than half, they remain at an economically significant level. However, the reduced-form estimates in columns 1 and 2 are each statistically significantly different from zero and, therefore, they support the presumption that the immigration rate without the controls does exert a systematic influence on changes in wages in Australia (Angrist and Krueger 2001)**.** [14]

[INSERT TABLE 3.3 HERE]

[12] Since we are using level as opposed to log of the change in wages as the dependent variable, we need to divide the coefficient estimates by the mean value of the dependent variable to get the results in terms of percentages.

[13] Since the results for the skilled and unskilled immigration rates are similar we do not report both here for brevity. They are available from the authors.

[14] In general, there need not be any relationship between significance of the reduced form and the significance of 2SLS estimates. However, we need a strong first-stage to ensure that we are not using a weak instrument. The standard IV/2SLS estimator, $(z'x)^{-1} z'y$, with dependent variable (regressor x), and instrument z, breaks down when $z'x$ is near singular while it does not when $z'y$ approaches zero.

3.4.3 Instrumental Variable Estimates

Panel B of Table 3.2 reports the results of IV estimations when the past immigration rate – a proxy for the policy variable - is used as an instrument. The first-stage results reported in Table 3.4 suggest that the selected instruments are not weak, and that their use carries no potential bias. In the second stage of IV estimation we run a regression of equation (3.3) where the immigration rate, $\Delta(I/P)_t$, is replaced by its fitted value obtained from the first-stage. The IV estimates display some qualitative similarities with the OLS estimates in panel A. One notable difference is that in the IV estimates the sign of the unemployment rate changes when explaining the effect of skilled migrants. The magnitude of the coefficients of the total immigration rate and unskilled immigration rate is significantly larger in the IV estimations (columns 1-3 and columns 6-9). We reject the hypothesis that the OLS and IV coefficients are the same on the basis of standard Hausman Test results (i.e., the difference in coefficient estimates using OLS and IV are systematic). The coefficients of the skilled migration rate become statistically insignificant, suggesting that the endogeneity bias is more effective in the case of skilled migration but that the bias is quantitatively important in the case of unskilled and total migration rates.

[INSERT TABLE 3.4 HERE]

Table 3.2 shows that the total immigration rate has a positive and statistically significant (at the 5% significance level) impact on current wages growth as does the rate of unskilled immigrants. However, the statistical significance of the IV estimates deteriorates sharply when we estimate the effects of skilled immigration on wages growth. None of the three specifications suggests that the rate of skilled immigrants has a statistically significant effect on domestic wages growth. Overall, the evidence suggests that skilled immigration does not exert a robust influence on wages growth in the Australian labour market. This result also indicates that we need to take the endogeneity of the immigration rate into account.

3.4.4 Two Stage Least Squares Estimates

We now consider both instruments, the past immigration rate and the past unemployment rate, simultaneously. The first stage involves regressing the immigration rate on all predetermined variables. The estimates are presented in panel A of Table 3.5. The exclusion restrictions are that the instruments do not appear in equation (3.3).

[INSERT TABLE 3.5 HERE]

2SLS estimates do not generate any compelling evidence that skilled immigrant flows exert a systematic effect on domestic wages growth. The total immigration rate is statistically significant (at the 10% level) in the first two specifications (columns 1 and 2). But its explanatory power vanishes in the full covariate specification of the wage equation, i.e., when changes in the unemployment rate and productivity are included in the estimation. Estimates with the unskilled immigration rate display a statistically significant effect on wage change while none of the skilled immigration rate specifications generate any statistically significant effects at the conventional level. The coefficient estimates of the unskilled migration rate indicate that a ten percent increase in the immigration rate will result in wage growth of about six percent.

IV and 2SLS estimators are popular and have been used extensively in the literature. Even though they may be consistent, several recent studies (e.g., Bound *et al*.1995; Staiger and Stock 1997) point out that the finite sample properties of both estimators can be very poor, especially when the sample size is very small or the instruments are weak. Often very large samples are needed for the asymptotic properties to yield good approximations. Both IV and 2SLS estimates are biased towards the probability limit of OLS estimates.

3.4.5 Jackknife Instrumental Variable Estimates (JIVE)

The 2SLS estimator can also suffer from bias that is exacerbated when the instruments are only weakly correlated with the endogenous variable and when many instruments are used. In such situations JIVE (see Angrist, Imbens and Krueger 1999) performs better than 2SLS. JIVE estimators eliminate the correlation between the first-stage fitted values and the structural error term that causes the traditional 2SLS estimator to be biased. Angrist *et al*. have shown that under certain forms of misspecification the JIVE estimator may have less bias than limited information maximum likelihood. It is also a useful alternative in applications when there is concern about the number of instruments. So we check the robustness of the earlier results using JIVE.

Panel B of Table 3.5 shows the results obtained with JIVE. The signs of the immigration rate in all three variants are consistent with those obtained from 2SLS estimates. The coefficient estimates for the total and skilled immigration rates are positive and negative, respectively. But they are statistically insignificant. Unskilled workers continue to exert a statistically significant positive effect on wage growth. The magnitude of the coefficients for the unskilled migration rate is similar to those obtained using 2SLS estimate. The point estimates suggest that a ten percent rise in the immigration rate increases Australian wages by about 2.2 to 2.5 percent.[15] With the exception of the initial, and contentious, OLS regressions these findings reveal a persistent lack of robust evidence that skilled immigration affects wages growth in Australia, positively or adversely.

[15] We also experiment with GMM estimates. The results are qualitatively similar to those of JIVE and 2SLS estimates. They are available from the author.

3.4.6 Tests for Validity of Instruments

Weak instruments tend to bias 2SLS estimates toward OLS estimates and may weaken standard tests for endogeneity. The existing econometric literature defines weakness of instrument based on the strength of the first-stage equation (Staiger and Stock 1997; Stock and Yogo 2004). Accordingly, we test the relevance and validity of the instruments. Specifically, we test whether the IVs are correlated with the endogenous regressor and orthogonal to the error process. We test the first condition by examining the fit of the first stage reduced-form regression of the immigration rate on the full set of instruments - both included and excluded instruments - for the 2SLS. We use the F-test of the joint significance of the excluded instruments in the first stage regression. The F-test rejects the null that the instruments are jointly insignificant (Table 3.4). The instruments are both individually significant as is evident from the first stage reduced-form regression coefficient estimates and the corresponding standard errors shown in Table 3.4.

We further check the relevance of instruments using a "partial R^2" measure proposed by Shea (1997) that takes intercorrelations among the instruments into account. We also use a commonly used statistic - the partial R^2 of the regression of endogenous variables on the excluded set of instruments (Bound, Jaeger and Baker, 1995). With a single instrument Shea's partial R^2 and the usual partial R^2 measures should be the same. But with multiple endogenous variables the two statistics should be different (Baum, Schaffer and Stillman 2003). Shea's partial R^2 ranges from 0.25 to 0.31 in our four models. Thus, our instruments pass both the criteria recommended by Bound *et al*. (1995) and Shea (1997)**.** We also test the overidentification problem using the common *J*-statistic of Hansen (1982). Under the null hypothesis of orthogonality we cannot reject it in all cases (Table 3.4). This confirms that the instruments are truly exogenous. This conclusion is corroborated by Sargan's (1958) statistic which is a special case of Hansen's J under the assumption of conditional homoskedasticity. We also adopt the general Hausman (1978) test of endogeneity. Under the null hypothesis that OLS is an appropriate estimation technique, we reject the null and conclude that the immigration rate is indeed endogenous (Table 3.4).

3.5 Discussion and Interpretation of the Results

After correcting for the endogeneity of the immigration rate comparison of the OLS estimates with the 2SLS and JIVE results indicate:

- coefficient estimates of the skilled migration rate using 2SLS and JIVE are absolutely smaller and statistically insignificant compared to OLS ;[16]

[16] The results for the skilled migration rate using a single instrument are different in sign and magnitude from those obtained with multiple instrument 2SLS and JIVE estimates. This is not unusual in the IV literature. For example, Friedberg (2001) finds that the effects of immigration are opposite to those of OLS estimates once the immigration

- for comparable levels of statistical significance the OLS estimates of the unskilled migration rate coefficients are downward biased.

As a robustness check, we also experiment with a specification that includes both skilled and unskilled migration rates as covariates in the same equation using the full set of instruments. The resulting coefficient estimates capture the partial effect of the skill categories. The results, not reported here, are similar. For the skilled migration rate the coefficient estimates become positive (t-ratio= +2/2.8=0.71) but OLS estimates remain upward biased. This supports our conclusion drawn from the 2SLS/JIVE estimates. We conclude that OLS estimates tend to exaggerate the effects of skilled migrants on wages. Once endogeneity is taken into account there is no compelling evidence that skilled immigrants systematically affect wage growth in Australia. The results for the unskilled migration rate do not change either - OLS continues to underestimate the true effect.

Even though our results are consistent with other findings, it is helpful to understand why and how this might be the case. Since OLS estimates are smaller than the estimates that take endogeneity into account, skilled migration is subject to positive selection. If high ability individuals migrate to Australia, then the omitted ability characteristic affects both earnings capacity and the immigration decision. Hence, our results conform to the theoretical prediction. Positive selection implies that skilled immigrants can choose to enter high wage occupations (since skills are classified by occupation). It follows that a positive correlation may exist between earnings potential in Australia, general talent and skilled migration. Shortages of skilled workers in Australia suggest that skilled immigrants can readily find work. Accordingly, immigrants with very high expected returns to skill are likely to migrate to Australia if skills in the source country are correlated with skills valued in Australia. These results could also hold if high-skilled workers get jobs in the skilled labour market while relatively less-skilled migrants switch to unskilled professions or out-migrate from Australia.

The estimation results for unskilled migrants suggest negative selection since OLS estimates are lower than the instrumented migration rate coefficients. The difference between OLS and IV estimates could reflect the fact that a disproportionate share of immigrants enters unskilled occupations if, for instance, the host country is relatively attractive to low earning workers.[17]

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rate is instrumented. The divergence between results using single and multiple instruments for skilled migration is probably a combination of weak instrument and small sample problems. In particular, skilled migrants are probably less likely to be induced to take migration decision by looking at past immigration policy. Rather they look at the labour market characteristics. So we argue that the endogeneity issue, especially for skilled migration, is better dealt with using 2SLS/JIVE. We therefore focus on the OLS, 2SLS and JIVE estimates.

[17] Wu, Harris and Zhao (2007) find that immigrants to Australia, especially those coming from non-western regions, are channelled into inferior jobs post migration. Chiswick and Miller (2007) document that the limited international transferability of human capital results in immigrants being channelled into relatively low status occupations when they first enter the Australian labour market.

For example, moderately skilled immigrants may choose to migrate to Australia as unskilled migrants if they expect above average labour market outcomes or labour market outcomes that are superior to those available in other potential destination countries. This could indicate a considerable earnings premium in Australia for unskilled migrants. Relatively high minimum wages in Australia compared to similar immigrant receiving countries (US and Canada) render this conjecture plausible. By 'subsidizing' low skill, the wage structure in Australia attracts low-skill workers from abroad. In other words, low-skill workers want to migrate to take advantage of the 'insurance' provided by Australia, and by migrating to Australia rather than elsewhere they receive an 'earnings premium'. The possibility of coexistence of earnings premia with negative selection has been noted by Kugler and Sauer (2005) in the context of occupational choice by immigrants to Israel.

Alternative explanations of these results involve measurement errors in OLS estimates attributable, for example, to misclassification of immigrants into the unskilled category. Or skilled migrants work as unskilled workers after migrating to Australia because of onerous labour market requirements for skilled workers or, for that matter, language and other barriers or because the wage structure favours unskilled workers. Alternatively, greater opportunities for outside earnings in the unskilled sector compared to elsewhere could motivate such behaviour. However, measurement errors alone are unlikely to account for the entire difference in the estimates. The divergence between OLS and 2SLS/JIVE estimates is, therefore, likely to involve the endogeneity bias. Our results, however, allow for the possibility that the effect of immigration on wages can vary substantially depending on the type of migrant labour.[18]

The absence of a negative overall impact of immigration on wage growth could be attributable to the configuration of demand and supply elasticities in the Australian labour market (highly inelastic domestic labour supply and highly elastic labour demand). Our findings suggest the possibility of complementarity between Australian-born workers and immigrant workers. This complementarity is stronger in the case of unskilled workers. Our results suggest that skilled migrants are either substitutes or complements to native workers. It is possible, for example, that assignment of unskilled immigrants to relatively basic jobs or supportive roles releases Australians to work on the more productive aspects of the job. By way of illustration, Friedman (2001) and Kugler and Sauer (2005) point out that in Israel Russian doctors - even those with considerable prior experience and earnings - filled positions at the lower end of the job ladder,

[18] One notable limitation of 2SLS estimates is that it uses only a part of the variation in immigration rates that is induced by the instrument(s) (those who decide to migrate to Australia on the basis of her immigration policy or labour market outcome) whereas OLS estimates use all variation (Imbens and Angrist 1994). If the marginal effects of immigration vary between those induced by the instruments and those who are not, then the estimated average effect of immigration will differ. However, as noted above, 2SLS estimates are an improvement over OLS estimates as we take the selection bias into account in the former. Moreover, IV estimates are consistent while OLS estimate may be biased.

pushing Israelis up the ranks into more supervisory, high-paying roles. Anecdotal evidence supports this adjustment in the context of Australia. Informal observation, for example, of retail workers, hospitality service workers, office clerks, and others reveals that many unskilled immigrants often perform lower-level work, with Australian-born workers concentrated in more supervisory roles.

3.6 Conclusion

This chapter empirically examines the implications for labour market outcomes in host countries of the increasing skill intensity of cross-border migration flows. It recognises the heterogeneous nature of the pool of immigrants and the recent thrust of Australian immigration policy to promote skill-intensive immigration patterns. The empirical estimations are based on a wage equation that takes into account macroeconomic aspects of the economy. Potential endogeneity problems due to selection and self-selection of immigrants are addressed by using various instrumental variable approaches that exploit the information content of antecedent immigration policy and labour market outcomes. The contrast between OLS and different versions of the instrumental variable models suggests that the immigration rate is not independent of unobserved determinants of wages. Comparing OLS estimates with the 2SLS and JIVE estimates provides evidence of negative selection for unskilled migrants. We also find evidence of positive selection in the case of skilled migrants reflecting the fact that they can choose to go to relatively high wage occupations. However, other consideration such as measurement errors could help to account for our results, and more research is needed to resolve these issues.

Our main finding is that there is no robust evidence that immigration exerts discernible adverse consequence on wages in the Australian labour market. Our examination of the skill composition of migration flows supports the many prevailing empirical findings that increasingly skill-intensive immigration need not cause labour market outcomes of native workers to deteriorate. In fact, there is some evidence that overall immigration may exert positive effects on wages in Australia.

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Table 3.1- Descriptive Statistics of the Key Variables

	1980-2006		1980-1990		1991-1999		2000-2006	
Variables	Mean	Std Dev	Mean	Std Dev	Mean	Std Dev	Mean	Std Dev
Average weekly wages per quarter	683.4	208.3	457.5	71.2	694.5	68.0	956.4	84.9
Quarterly Unemployment Rate	7.71	1.58	7.84	1.28	8.80	1.23	5.99	0.61
Immigration rate per 1000 Australian	2.1093	0.5166	2.505	0.5822	1.8235	0.343	2.0144	0.2705
Skilled immigrants	0.666	0.162	0.641	0.197	0.611	0.106	0.772	0.101
Unskilled immigrants	0.294	0.162	0.458	0.132	0.179	0.023	0.187	0.040
Productivity	83.87	10.19	73.28	1.69	83.68	5.05	97.70	2.86
Change in productivity	0.342	0.424	0.194	0.451	0.492	0.316	0.316	0.467
Growth of weekly wage of all workers in a quarter	8.21	3.21	7.92	1.89	6.19	2.47	11.48	2.87
change in unemployment rate	-0.038	0.288	-0.032	0.355	-0.026	0.294	-0.061	0.166

Notes: Skilled and unskilled migration rates represent the share of each migration category in total migration. A large number of immigrants did not reveal their occupation at their country of origin during their first entry into Australia, so skilled and unskilled immigration rates do not add up to the total migration rate.

Table 3.2- OLS and IV Estimates of the Effects of Immigration

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Total immigrants			Skilled immigrants			Unskilled immigrants		
Panel A: Ordinary Least squares Estimates (OLS)									
Net immigration rate	1.66	1.67	1.55	3.6	3.55	3.18	4.22	4.27	3.89
	(0.39)*	(0.39)*	(0.40)*	(1.77)**	(1.78)**	(1.75)+	(1.30)*	(1.30)*	(1.38)*
Change in unemployment rate		-0.093	0.011		0.116	0.23		0.572	0.635
		(0.970)	(0.963)		(1.038)	(1.017)		(0.877)	(0.875)
Change in productivity			-0.477			-0.566			-0.537
			(0.297)			(0.297)+			(0.312)+
R-squared	0.1	0.1	0.12	0.04	0.04	0.07	0.06	0.06	0.08
Panel B: Instrumental Variable (IV) Estimates									
Net immigration rate	2.65	2.98	2.91	5.97	6.15	6.24	6.66	6.06	5.81
	(1.08)**	(1.23)**	(1.29)**	(3.66)	(3.89)	(3.78)	(1.97)*	(1.61)*	(1.71)*
Change in unemployment rate		-0.543	-0.476		-0.151	-0.096		0.61	0.668
		(1.134)	(1.147)		(1.193)	(1.191)		(0.860)	(0.856)
Change in productivity			-0.32			-0.48			-0.479
			(0.345)			(0.334)			(0.319)
R-squared	0.1	0.1	0.12	0.04	0.04	0.07	0.06	0.06	0.08

Notes: The dependent variable in each is the growth in weekly wages in a given quarter. Each column in each panel represents a separate regression which also includes a constant term: in (1), (4), (7) the immigration rate is the sole regressor; (2), (5), (8) control for the unemployment rate; (3), (6) and (9) include also productivity growth. The migration rate in a given period is the number of immigrants per one thousand adult (15-64 years of age) Australian population for that period. Skilled and unskilled migration rates are the share of each migration category in total migration. Because a large number of immigrants did not reveal their occupation at their country of origin during their first entry into Australia, the entries for skilled and unskilled immigration rates do not add up to total migration rate. Huber-White standard errors in parentheses, + significant at 10%; ** significant at 5%; * significant at 1%.

Table 3.3- Reduced Form Estimates

	(1)	(2)	(3)
Change in unemployment rate	-0.338	-0.084	-0.484
	(0.921)	(0.848)	(0.838)
Change in productivity	-0.613	-0.631	-0.594
	(0.294)**	(0.287)**	(0.279)**
Six-quarter lag	1.205		0.576
immigration rate	(0.431)*		(0.399)
Six-quarter lag		-0.892	-0.827
unemployment rate		(0.150)*	(0.158)*
R-squared	0.08	0.22	0.22

Notes: The dependent variable in each is the growth in weekly wages in a given quarter. Each column represents a separate regression which also includes a constant term. Past unemployment rate and past immigration rate are six-quarter lags of the respective variable. Huber-White standard errors in parentheses, + significant at 10%; ** significant at 5%; * significant at 1%

Table 3.4- First-Stage Regression: Immigration Decision

	(1)	(2)	(3)	(4)	(5)	(6)
Change in unemployment rate[1]		0.023		0.166	0.17	0.514
		(0.161)		(0.146)	(0.16)	(0.133)*
Change in productivity[1]			-0.376	-0.399	-0.419	-0.48
			(0.097)*	(0.095)*	(0.096)*	(0.114)*
Past immigration rate	0.539	0.534	0.518	0.483	0.389	
	(0.076)*	(0.099)*	(0.071)*	(0.089)*	(0.090)*	
Past unemployment rate[1]	0.116	0.116	0.112	0.113		0.056
	(0.038)*	(0.038)*	(0.035)*	(0.035)*		(0.030)+
Hansen's J-Statistic (Overidentification Test)	[p=0.31]	[p=0.281]	[p=0.260]	[p= 0.33]		
F-test of Joint Significance of Instrument Set	[p=0.00]	[p=0.00]	[p=0.00]	[p=0.00]		
Shea's Partial R^2	0.2983	0.27	0.313	0.259		
Wu-Hausman F test	[p=0.061]	[p=0.076]	[p=0.109]	[p=0.092]		
Durbin-Wu-Hausman chi-sq test	[p=0.059]	[p=0.071]	[p=0.103]	[p=0.085]		
Sargan statistic (overidentification test of all instruments)	[p=0.368]	[p=0.358]	[p= 0.351]	[p=0.434]		
Value of F-statistic (for instruments)	24.3	14.5	25.7	13.9		
R-squared	0.3	0.3	0.39	0.4	0.33	0.21

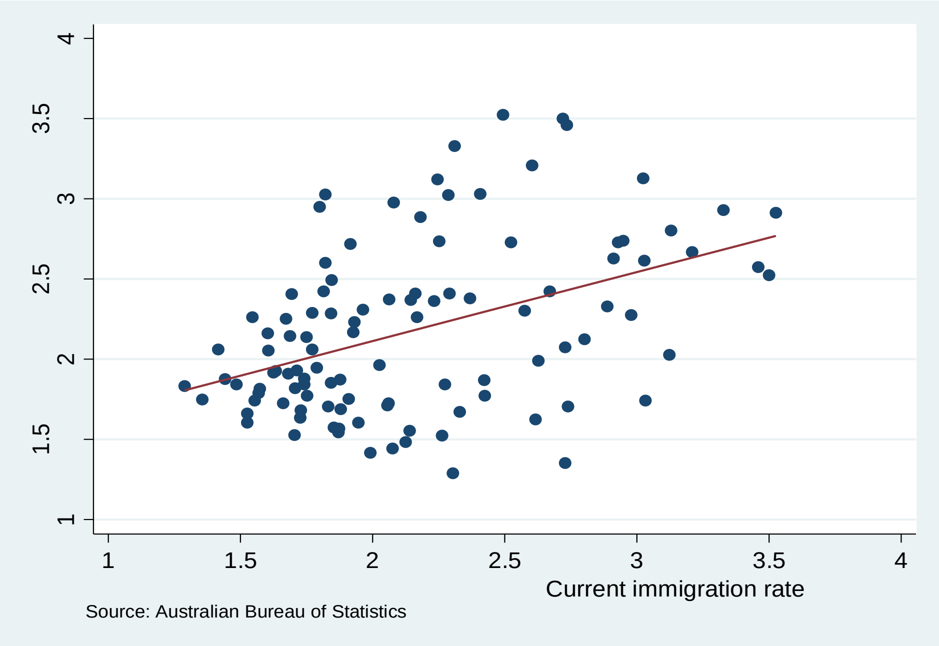
Notes: The dependent variable in each case is the current immigration rate. Each column represents a separate regression which also includes a constant term. Past unemployment rate and past immigration rate are six-quarter lags of the respective variable. Huber-White standard errors in parentheses, + significant at 10%; ** significant at 5%; * significant at 1%.
[1] coefficient estimates are multiplied by 1000

Table 3.5- 2SLS and JIVE Estimates of the Effects of Immigration

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Total immigrants			Skilled immigrants			Unskilled immigrants		
Panel A: Two Stage Least Square (2SLS)									
Net immigration rate	1.82	1.9	1.75	-0.49	-2	-1.94	5.14	5.23	4.95
	(1.00)+	(1.02)+	(1.07)	(3.61)	(3.98)	(3.80)	(1.98)*	(1.62)*	(1.70)*
Change in unemployment rate		-0.171	-0.062		0.688	0.775		0.593	0.653
		(1.005)	(1.007)		(0.934)	(0.919)		(0.856)	(0.848)
Change in productivity			-0.453			-0.711			-0.505
			(0.319)			(0.314)**			(0.311)
R-squared	0.1	0.09	0.11	0.02	0.02	0.03	0.05	0.05	0.07
Panel B: Jackknife Instrumental Variable Estimates (JIVE)									
Net immigration rate	1.86	2.15	2.05	-1.3	-2.97	-3.26	5.2	5.28	5.02
	(1.14)	(1.32)	(1.52)	(4.13)	(4.99)	(5.20)	(2.03)**	(1.65)*	(1.74)*
Change in unemployment rate		-0.258	-0.168		0.787	0.916		0.594	0.654
		(1.066)	(1.102)		(0.966)	(0.969)		(0.856)	(0.848)
Change in productivity			-0.419			-0.748			-0.503
			(0.345)			(0.332)**			(0.311)
Observations	106	106	106	106	106	106	106	106	106
R-squared	0.1	0.09	0.11	0.02	0.02	0.03	0.05	0.05	0.07

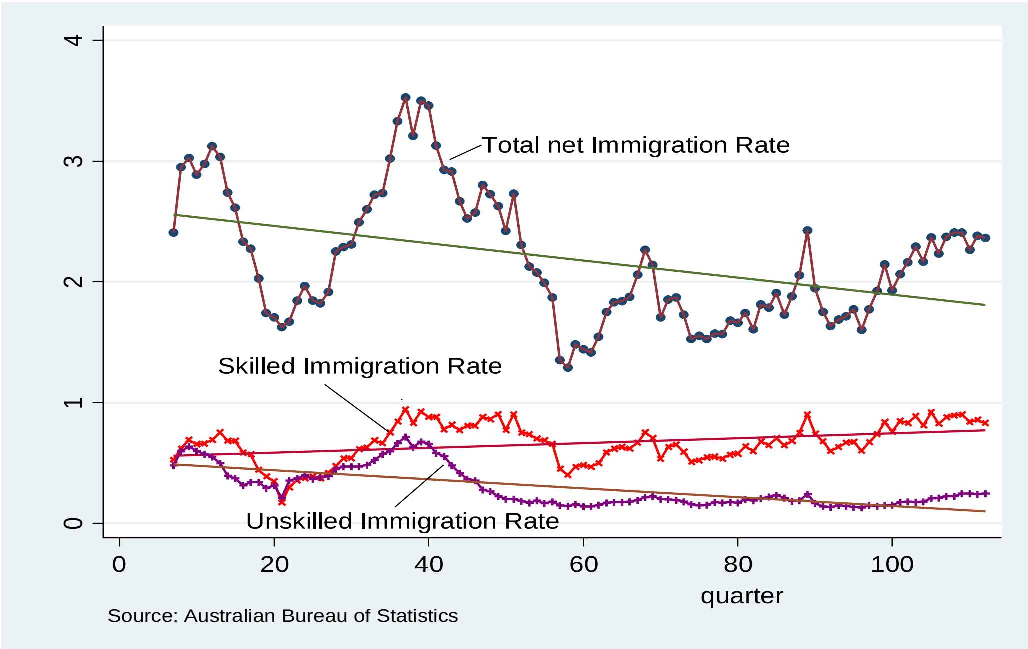
Notes: The dependent variable in each is the growth in weekly wages in a given quarter. Each column in each panel represents a separate regression which also includes a constant term. Migration rate in a given period is the number of immigrations per one thousand adult (15-64 years of age) Australian population for that period. Skilled and unskilled migration rates are the share of each migration category in total migration. A large number of immigrants did not reveal their occupation at their country of origin during their first entry into Australia, so skilled and unskilled immigration rates do not add up to total migration rate. Huber-White standard errors in parentheses, + significant at 10%; ** significant at 5%; * significant at 1%

Figure 3.1- Relationship between Past and Current Immigration Rate, Australia: 1980-2006



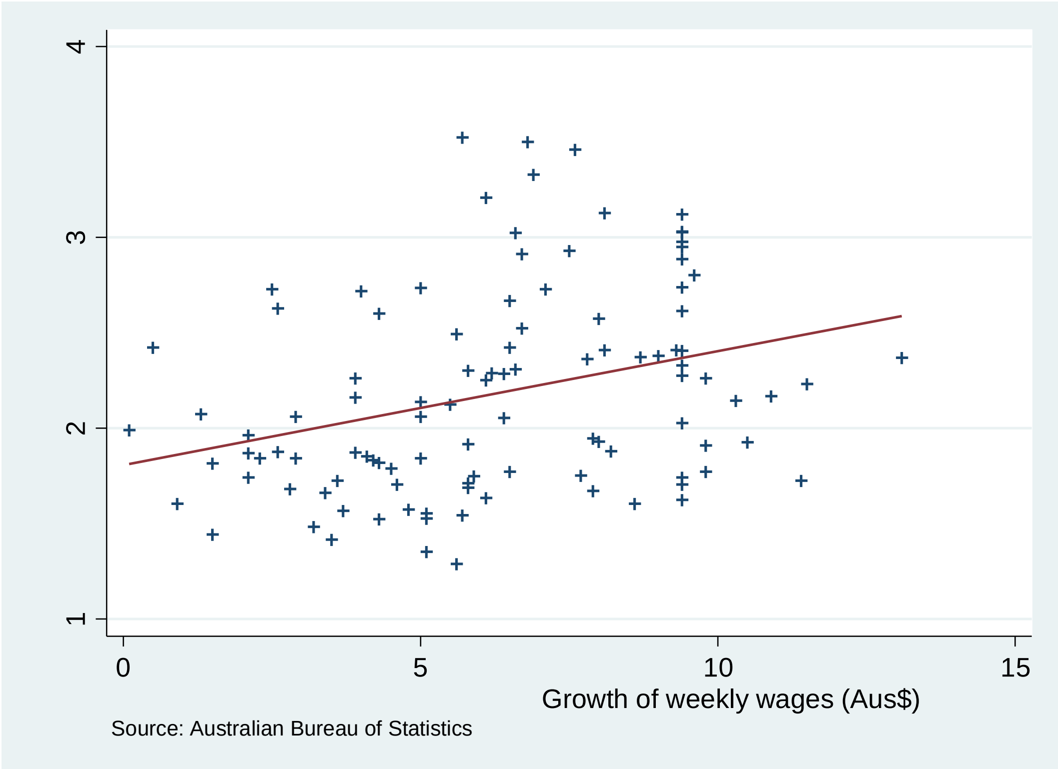
Notes: Current immigration rate is the number of immigrants per one thousand adult (15-64 years of age) Australian population at time t. Past immigration rate is the six-quarter lag of the migration rate.

Figure 3.2- Skilled and Unskilled Migration Rates in Australia



Notes: Migration rate in a given period is the number of immigrants per one thousand adult (15-64 years of age) Australian population for that period. Skilled and unskilled migration rates are the shares of each migration category in total migration. A large number of immigrants did not reveal their occupation at their country of origin during their first entry into Australia, so skilled and unskilled immigration rates do not add up to total migration rate.

Figure 3.3- Relationship between Wage and Immigration Rate, Australia: 1984-2006



Notes: The growth of weekly wages is the change in the average weekly wage between adjacent quarters. The immigration rate is the number of all (skilled and unskilled) immigrants per thousand adult (15-64 years of age) Australian-born population.

Conclusion of the Thesis

The microfinance sector of Bangladesh is the world's largest. The first two chapters of this thesis examine the impact of this large-scale program at the household level. This study also uses a number of non-experimental impact evaluation methods to estimate the causal impact of microfinance. In the first chapter, we find that two widely used non-experimental impact evaluation techniques, IV and PSM, generate similar results concerning the impact of participation in microfinance. We estimate impacts using different instruments and find that the results are consistent, which demonstrates the robustness of the estimates. We also estimate the program impact using the DD approach. We therefore accommodate the approaches taken by both PK (using the IV approach) and Morduch (using the DD approach) and have largely been able to mitigate the controversy regarding the non-experimental impact evaluation of microfinance programs using a new, large and unique dataset.

In addition, we use the PSM methods to estimate the impact of participation in microfinance programs. The choice of PSM is also motivated by the richness and appropriateness of the data. We use more generous (regression-adjusted) matching estimators for estimating the propensity score, and find similar results in all cases. These findings, using both matching and IV methods, illustrate that they can be used as viable impact estimators under certain specific conditions, as described in Chapter 1. Given the paucity of the empirical investigations of the impact of microfinance programs, and the significance of evaluating this large program, we can claim that under the right conditions the evaluation of such (non-experimental) programs can produce credible estimates of program impact.

The use of PSM in the evaluation of microfinance programs is new. The matching estimator permits us to directly analyse the heterogeneous treatment effect simply by sub-grouping the analysis and rematching within the sub-group. We compare the matching estimates with the IV estimates. We also analyse the shortcomings of matching since PSM only eliminates selection-on-observables and thus remains unsolved regarding the selection-on-unobservables. It is worth noting that PSM does not solve all (or even many) of the problems that prevent regression models from generating reliable estimates of causal effects. Even so, we illustrate that the use of the most generalised matching estimator (combining regression with matching), given suitable data, can succeed admirably in estimating the causal effects of microfinance. Overall, our analysis shows that matching can provide reasonable estimates of non-experimental program impacts.

On the whole we find that the effect of microfinance on household consumption expenditure does not seem to be strong. We find that the IV estimates of program impact are larger; in general, they report an increase of 6 to 14 percent in the consumption expenditure of the relatively poor participating households. This impact varies when we consider different samples based on household land ownership. The PSM methods are, however, less precise than the IV methods possibly because the former is essentially a non-parametric estimation strategy. We provide more explanations on the similarities and differences in results between IV and PSM in Chapter 1.

Consumption expenditure accounts for most of the total household expenditure among the rural poor of Bangladesh. Our results indicate that the impacts of microcredit are not strong across all groups of poor households. Instead, we find that the poorest of poor participants are most likely to benefit. These results show that microcredit loans may not be effective for land-rich households. These households require larger amounts of money to invest, and microcredit loans may be too small to generate adequate return.

While participation in microcredit may enhance consumption and other economic well-being, it may increase the probability that some households will keep their children away from school. The impact estimates obtained in Chapter 2 indicate that the child labour problem may be exacerbated due to household borrowing from microcredit institutions. These results are also consistent with the existing literature concerning microcredit and its impact on schooling and child labour. These results also indicate the differential impact of microcredit on gender: girls are more likely to be called upon to work at a younger age. This result raises the possibility that families will allocate resources away from daughters, probably because households face constraints in hiring labour. This result is consistent with the return to schooling literature. It also supports the popular perception that, in developing countries, parents perceive higher expected returns on boys' education as they can claim on future resources of boys. On the other hand, girls get married out to another family and contribute resources to the family of their husbands, and so investing in the education of girls is not perceived to be as beneficial.

The overall results suggest that increasing access to rural microcredit will not necessarily increase school enrolment in a developing country such as Bangladesh. Therefore, though the joint-liability lending mechanism reduces many of the informational asymmetry problems, a more moral hazard potentially exists within the household. This raises the question of whether a household's short-term welfare gains by borrowing microcredit comes at the expense of schooling of children, which is a major cause of long-term poverty. The results also indicate that younger children are more prone to work due to their parents' participation in the microcredit program. The impact estimates also differ by family's income/education level: while children from poorer households are less likely to attend school and more likely to work, those from relatively richer families are not significantly affected. The findings by income/education and asset holdings indicate that poor households may not send their children to school when they have access to credit. But credit (or aid) tied directly to child schooling may have very different results. As a result, we cannot directly compare our results with the findings of educational borrowing constraints literature. However, as demonstrated in previous studies, microcredit can help increase income and consumption expenditure. So, if poorer households can graduate out of poverty by taking credit, then our results could hold only in the short run. In the long run, as the income of households increase, the income effect may offset the substitution effect and could result in an increase of children's education. On the other hand, if there is slow rate of reduction in poverty among participants, then microcredit may create a long-term problem of human capital formation.

Finally, in Chapter 3, we explore the impact of immigration on labour market opportunities, in particular on wages. We take into account the heterogeneous nature of the pool of immigrants and the recent thrust of Australian immigration policy. From the many differentiating characteristics of migrants we emphasise the distinction between skilled and unskilled labour. Not only does this distinction capture the essence of Australia's immigration policy, it also recognises, albeit in a qualitative way, the augmentation of the host country's stock of human capital that is generated from the influx of migrants.

In order to address the potential problem of endogeneity due to selection and self-selection of immigrants we use various IV approaches by exploiting the antecedent immigration policy and labour market outcomes. The basic IV estimates suggest that the immigration rate is endogenous, and that this endogeneity needs to be taken into account. The multiple instrument 2SLS method captures both the Australian government immigration policy and self-selection by immigrants. Given the small size of the sample, the robustness of the results of the 2SLS method is verified by JIVE. The JIVE estimator can simultaneously take care of the small sample bias problems in 2SLS. JIVE is also better suited if weak instruments are used. We have also demonstrated the validity of the instruments on the basis of theoretical considerations and subjected this choice to empirical testing. These tests support the suitability of the instruments and, hence, the analytical soundness of our results.

The core finding is that there is no robust evidence that immigration exerts any discernible adverse consequence on wages in the Australian labour market. This basic finding holds true whether the immigration rate is specified in aggregate form or whether it is decomposed into the two main subsets of skilled and unskilled immigrants. In fact, there is some evidence that overall immigration may exert positive effects on wages in Australia.

One obvious limitation of this chapter is the failure to explicitly allow for international movements of capital. Australia is a small open economy (SOE). Insofar as immigration influences relative factor returns it will elicit a capital account response. Typically, in simple aggregate SOE models, the induced capital flows will tend to re-establish relative factor returns with the net result that both the stock of the domestic labour force and capital has been augmented in response to immigration. The capital flow response becomes significantly more complex as multiple categories of labour and capital are recognised with potential complementarity and substitutability relations between the various components of each group of factors. The consequence of this increasing complexity is that analytical results become more equivocal while the available data for empirical testing ceases to match the analytical constructs. Furthermore, there is little evidence to suggest that any labour market disturbances that may be induced by migrant flows are likely to create relative factor price changes of sufficient magnitude to drive large-scale cross-border capital movements. The analytical limitation of this chapter is, therefore, unlikely to be empirically debilitating.

However, it should also be noted that the relationship between aggregate wages and immigration is difficult to test. Immigration is an endogenous choice and few opportunities exist where a truly exogenous immigration shock can be observed. Furthermore, aggregate wages is a macroeconomic phenomenon that needs to be explained by factors that are not necessarily linked to immigration. Future research could therefore focus on the different composition of immigration flows by age, gender, and its effects on wages and other measures such as employment of specific groups, and using more disaggregated data at the national or state level. To the author's knowledge, however, such data is not yet available in Australia for public use.[87]

[87] See, for example, Borjas, Grogger and Hanson (2008) and Islam (2009) for a comprehensive review of the impact of immigration on wages and employment. Islam (2009) analyses the impact of immigration on wages of Canadian-born workers disaggregated by industry. See also Islam (2007) who examines the relationship between unemployment and immigration in Canada using aggregate time-series data.

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