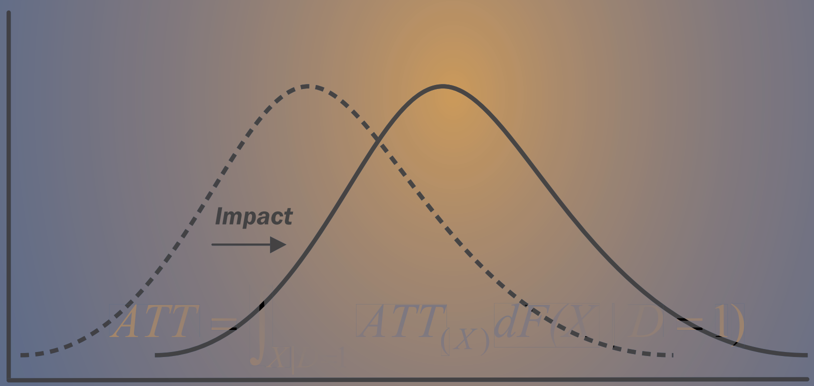
Essays on impact evaluation:
new empirical evidence from Vietnam



$$ATT = \int_{X|D=1} ATT_{(X)}\, dF(X|D=1)$$

$$P_\alpha = \frac{1}{n}\sum_{i=1}^{q}\left[\frac{z-Y_i}{z}\right]^{\alpha}$$

$$G = \frac{n+1}{n-1} - \frac{2}{n(n-1)\bar{Y}}\sum_{i=1}^{n}\rho_i Y_i$$

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Essays on impact evaluation: new empirical evidence from Vietnam

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Abstract

This study estimates the impact of various economic flows including government-subsidized micro-credit, informal credit, public and private transfers, international remittances, and migration on poverty and inequality for Vietnam using Vietnam Household Living Standard Surveys in 2004 and 2006. Impact evaluation methods employed in the study include fixed-effects regression and difference-in-differences with propensity score matching. Poverty is measured by three Foster-Greer-Thorbecke poverty indexes, while inequality is measured by the Gini coefficient, Theil's L and Theil's T indexes. It is found that the impact of the governmental micro-credit, public transfers and international remittances on poverty reduction is very limited. On the contrary, informal credit, domestic (internal) private transfers and migration have positive and statistically significant impacts on poverty reduction. The domestic private transfers have the largest effect on the total poverty of the population due to a high impact on expenditure and a large coverage of the poor. Regarding inequality, both government-subsidized micro-credit and informal credit do not affect inequality significantly. Public transfers and international remittances increase inequality slightly, while domestic private transfers and migration lead to a decrease in inequality.

Keywords

Credit, cash transfers, remittances, migration, poverty, inequality, impact evaluation, Vietnam, Asia

JEL classification

I38, H43, H55, H81, O12, O15

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Chapter 1 Introduction

1.1 Research background

Vietnam is often mentioned as an example of a country successful in poverty reduction. Over the past decade, Vietnam has witnessed a remarkable reduction in poverty. According to the Vietnam Household Living Standard Surveys, the poverty incidence decreased from 58 percent in 1993 to 29 percent in 2002, and continued to decrease to 16 percent in 2006.[1] The incidence of food poverty or ultra poverty decreased from 25 percent to 7 percent during the 1993-2006 period. During this period, the poverty gap index, which measures the poverty depth, was also reduced from 0.185 to 0.038, equivalent to a reduction of nearly 80%.

Both rural and urban Vietnam experienced a fast reduction in poverty. During the 1993-2006 period, the proportion of rural people below the poverty line fell by 46 percentage points from 66 percent to 20 percent. Meanwhile, the poverty incidence decreased by 21 percentage points from 25 percent to 4 percent in the urban areas during the same period. However, rural areas experienced a slower pace of poverty reduction. Between 1993 and 2006, the poverty rate was reduced by 70 percent and 84 percent in the rural and urban areas, respectively. As a result, the share of the rural population in the poor increased from 91 percent in 1993 to 94 percent in 2006, and poverty has become mainly a rural phenomenon in Vietnam.

There is a strong spatial or geographic dimension in Vietnam's poverty (World Bank, 2004a, 2008a). The poverty incidence varies substantially across regions. Although all the regions experienced significant poverty reduction, the speed of poverty reduction is different across regions. There are eight regions in Vietnam, and the North West (mountainous areas) is the poorest region, followed by the North Central Coast and the Central Highlands. The region which has the lowest poverty rate is the South East. The Red River Delta and Mekong Delta have the second and the third lowest poverty rates, respectively. These three regions are also the three largest deltas of the country.

Unlike other developing countries such as China where high economic growth and fast poverty reduction are accompanied by a high increase in inequality, Vietnam has achieved a remarkable decrease in poverty with only a slight increase in inequality. According to the Vietnam Household Living Standard Surveys, the Gini index based on expenditure per capita increased from 0.33 in 1993 to 0.36 in 2006.

Vietnam's success in poverty reduction results from different factors including economic growth and state poverty reduction programs. During the 1991-2008 period, the country achieved high economic growth with an average annual growth rate of around 6 percent in Gross Domestic Product (GDP) per capita. Over the last decade only China and Ireland have experienced higher growth rates (World Bank, 2004a). Broad-based economic growth can have a positive impact on poverty reduction through numerous channels such as increasing household income and consumption, raising private transfers and promoting the credit market. In addition, the extensive public safety net including a large number of poverty alleviation programs maintained by the Vietnamese government may have contributed to

[1] The poverty line is equivalent to the expenditure level that allows for nutritional needs with food consumption securing 2100 calories per day per person and some essential non-food consumption such as clothing and housing. This poverty line is estimated by the General Statistics Office of Vietnam and the World Bank in Vietnam.

poverty reduction. Up to now, a huge amount of funds have been spent on assistance programs targeted at the poor. In the 2006-2010 period, the government plans to spend 44,855 billion thousand VND (approximately 2.8 billion USD) on the poverty alleviation program.[2]

At the micro level, a large number of factors can have direct and positive effects on income promotion and poverty reduction. Important factors may be micro-credit, cash transfers, remittances, and migration. In 2003, the government of Vietnam launched the Vietnam Bank for Social Policies (VBSP), which provides micro-credit for the poor. The poor can borrow from the bank at low interest rates without collateral. In addition to the VBSP, informal credit is an important source of finance for people in Vietnam (McCarty, 2001; Pham and Lensink, 2007; Barslund and Tarp, 2007). Regarding cash transfers, both public and private transfers have increased over time. Public transfers include contribution-based health benefits and social security schemes and non-contributory transfers such as social allowances disbursed to support war invalids and heroes, the elderly, children without guardians, disabled people, and households adversely affected by natural calamities (Government of Vietnam, 1993b, 2003). Private transfers are sent to households by their relatives and friends both from within Vietnam and abroad. More than 80 percent of Vietnamese households currently receive domestic private transfers, and international remittances increased from 26.5 to 57.9 thousand billion VND (in 2001 prices) during the 2001-2007 period. The increase in remittances results from increased migration. Migration can have positive impacts on household wellbeing by increasing not only remittances but also household productivity and income diversification (Stark and Levhari, 1982; Stark and Bloom, 1985, Rosenzweig, 1988; Stark, 1991; Taylor and Martin, 2001; Taylor and Lopez-Feldma, 2007).

1.2 Research objective and questions

The objective of this research is to examine how well economic flows including micro-credit, public and private transfers, international remittances and migration affect the poor, and to measure the extent to which these factors impinge on household welfare, poverty and inequality in Vietnam using Vietnam Household Living Standard Surveys in 2004 and 2006. More specifically, the study aims to answer four empirical research questions:

- How extensive is the access of the poor to governmental micro-credit and informal credit? And what is the impact of these credit sources on consumption expenditure, poverty and inequality?
- How well do public transfers and domestic private transfers reach the poor? And to what extent do public and private transfers affect household consumption expenditure, poverty and inequality?
- How extensive is the access of the poor to international remittances? And what is the impact of international remittances on household consumption expenditure, poverty and inequality?
- What is the pattern of work and non-work migration of the poor? And what is the impact of work and non-work migration on household consumption expenditures, poverty and inequality?

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[2] In January 2008, 1 USD ≈ 16 thousand VND.

Although these research questions are policy relevant for both developed and developing countries, we concentrate on Vietnam. Vietnam is a country which has achieved a remarkable reduction in poverty. Understanding the impact of several important economic factors on poverty can provide some information on the 'story' behind the successful reduction in Vietnam's poverty. Information on the impact evaluation is highly relevant. It can be helpful for policy-makers and researchers in designing and implementing poverty reduction programs.

1.3 Research contribution

It is surely undeniable that poverty and inequality alleviation is what economic development is all about. Many countries throughout the world have made poverty reduction a major goal of development policy and have implemented numerous policies and programs to increase people's income and consumption and reduce poverty.

An important means to increase income and consumption is credit. As is well-known, micro-finance is often seen as a tool for reaching the Millennium Development Goal of halving the proportion of poor people between 1990 and 2015. Micro-credit and other financial services would enable the poor to build assets, increase incomes, and reduce their vulnerability to economic stress. Credit markets are severely rationed, and commercial banks are not interested in poor clients because of information problems and lack of collateral (Hoff and Stiglitz, 1990; Nagarajan, *et al.*, 1995; Kochar, 1997; Bell *et al.*, 1997; Bose, 1998; Boucher *et al.*, 2008). Governments and NGOs have stepped into the gap and have provided credit to the poor, often at highly subsidized interest rates. Although microfinance programs have been set up all over the developing and even the developed world, informal credit remains popular (Nagarajan *et al.*, 1995; Kochar, 1997; Bell *et al.*, 1997; Agénor and Montiel, 1999; Conning and Udry, 2005; Guirkinger, 1998). Micro-credit programs do not require collateral, but they do screen borrowers by other eligibility criteria such as poverty status or repayment capacity. As a result, not all poor households may be able or willing to obtain micro-credit, and some may resort to informal credit. Despite the popular view of moneylenders as usurers, informal loans may help to increase capital and mitigate consumption fluctuations and thus enable the poor to grow out of poverty.

Income transfers are another tool for poverty reduction and living standard improvement. Income transfers to a household can come from public and private sources, which are popular in both developed and less developed countries. The important role of public transfers in improving household welfare can be found in a large number of studies. For example, empirical studies such as Barrientos and DeJong (2006), Maluccio (2005), Behrman and Hoddinott (2000), Skoufias and McClafferty (2001) show that programs providing conditional cash transfers help the recipients reduce child labour, increase child schooling, and improve nutrition and health. Positive effects of social security transfers on income and consumption are also found in Devereux (2002), Hoddinott *et al.* (2000), Sadoulet *et al.* (2001), etc. Regarding the impact on poverty, Morley and Cody (2003) find the Progresa program in Mexico helps the beneficiaries reduce the poverty gap by 36 percent. The effects of private transfers, especially international remittances, on poverty reduction are found in many empirical studies such as Adams (1991), Adams (2004), Lopez (2005), Taylor *et al.* (2005), Adams (2006), and Acosta *et al.* (2007).

Migration is also an important strategy of households for income increases and poverty reduction. Migration can help households increase their income and consumption through not only increased remittances but also increased productivity and income diversification (Stark and Levhari, 1982; Stark and Bloom, 1985, Rosenzweig, 1988; Stark, 1991; Taylor and Martin, 2001; Taylor and Lopez-Feldma, 2007).

Although credit, cash transfers, remittances and migration are important for poverty reduction in general, they are not always a panacea. In certain cases, credit, cash transfers and migration can have limited positive impacts or even harmful impacts on household income and poverty reduction. Empirical research is also inconclusive about the sign and the extent to which credit, cash transfers and migration affect poverty and inequality. For example, micro-credit is found not to have a significant impact on poverty reduction and income in several developing countries (Diagne and Zeller, 2001; Coleman, 1999; Morduch, 1998). Regarding public transfers, poor people may receive less from social security programs than people from middle and high income groups (e.g. Friedman and Friedman, 1979; Howe and Longman, 1992; Castles and Mitchell, 1993). Regarding the impact on inequality, the effect of credit and cash transfers on inequality can be positive and negative depending on whether credit and cash transfers reach the poor more or the non-poor more. As both positive and negative indirect effects are possible, the quantitative effects of credit, cash transfers and migration on poverty and inequality are ambiguous.

This study is expected to contribute new empirical evidence on the impact of different microeconomic factors including credit, cash transfers, remittances and migration on poverty and inequality. This study has several special features. First, we concentrate on the case of Vietnam. Vietnam is interesting to look at since over the past decade Vietnam has achieved a remarkable result in poverty reduction with only a moderate increase in inequality, and there are few studies on quantitative evaluation of credit, cash transfers, remittances and migration on both poverty and inequality in Vietnam. In addition, there are no studies which jointly assess and compare the impact of these factors on poverty and inequality while accounting for potential behavioural responses. Secondly, the study shows not only the ultimate impacts of credit, cash transfers, remittances and migration on poverty and inequality in Vietnam but also some of the underlying mechanisms: the distribution of these factors over the poor and the non-poor, the potential effect of these factors on work effort, income and expenditures. Thirdly, this study is the first study that uses data from the two most recent Vietnam Household and Living Standard Surveys (VHLSS) of 2004 and 2006 to estimate the impact of different economic factors. The use of two years of data allows us to use panel data techniques. This dramatically improves the estimation strategy since biases that arise due to omitted variables, endogeneity and selection, can be addressed by using panel data.

1.4 Data set

The study uses data from the two most recent Vietnam Household Living Standard Surveys (VHLSS), which were conducted by the General Statistics Office of Vietnam (GSO) with technical support from the World Bank (WB) in the years 2004 and 2006. The 2004 and 2006 VHLSS covered 9,188 and 9,189 households, respectively. The samples are representative for the national, rural and urban, and regional levels. The 2004 and 2006 VHLSS set up a panel of 4,216 households, which are representative for the whole country, and for the urban and rural population.

The sample selection of VHLSS 2004 and 2006 follows a method of stratified random cluster sampling. GSO selected households in all rural and urban provinces of Vietnam, i.e. rural and urban areas of all provinces are strata. There were 64 provinces in 2004 and 2006. This means that there were 128 strata. Among each stratum, communes were selected randomly as primary sampling units. The number of communes per stratum is proportionate to the population proportion of the strata over the total population. The number of selected communes in each VHLSS is 3,063. In each commune, around 3 households were selected randomly. It is expected that a large number of communes selected throughout the country will reduce the sampling error of the collected data.

The surveys collected information through household and community level questionnaires. Information on households includes basic demography, employment and labour force participation, education, health, income, expenditure, housing, fixed assets and durable goods, participation of households in poverty alleviation programs, and especially information on credit, international remittances, private transfers, pensions and social allowances that households had received during the 12 months before the interview. In the rare cases that pensions and social allowances are provided in kind, VHLSS reports their equivalent estimated values.

Expenditures and income per capita are collected using very detailed questionnaires in VHLSS. Expenditure includes food and non-food expenditure. Food expenditure includes purchased food and foodstuff and self-produced products of households. Non-food expenditure comprises expenditure on education, healthcare expenditure, expenditure on houses and commodities, and expenditure on power, water supply and garbage. Regarding income, household income can come from any source. Income includes income from agricultural and non-agricultural production, salary, wage, pensions, scholarship, income from loan interest and house rental, remittances and social transfers. Income from agricultural production comprises crop income, livestock income, aquaculture income, and income from other agriculture-related activities.

Information on commune characteristics was collected from 2,181 and 2,280 rural communes in the 2004 and 2006 surveys, respectively. This data can be linked with the household data. Commune data include demography, general economic conditions and aid programs, non-farm employment, agriculture production, local infrastructure and transportation, education, health and health facilities, and social problems.

1.5 Methodology

The main objective of this study is to investigate the poverty targeting and quantitative impacts of several microeconomic flows including credit, cash transfers, remittances and migration on consumption expenditures, poverty and inequality. To assess how an economic flow covers the poor, we can use simple descriptive statistics, which measures the proportion of poor households involved over the total number of poor households. This measure is called the coverage rate. A higher coverage rate means a larger number of the poor covered by the economic flows.

Quantitative evaluation of the impact of a flow is often more complicated. The main objective of impact evaluation is to measure the extent to which this flow has changed outcomes of a group of households. In this study, the impact of an economic flow is measured by the difference between the outcome in the presence of the flow and the counterfactual outcome in the absence of the flow. For example, the effect of the governmental micro-credit program on income of the program's participants is measured by the difference between the participants' observed income and their counterfactual income had they not participated in the micro-credit program.

Therefore, to assess the impact of a flow, we need to estimate the counterfactuals for outcome. This is not straightforward, as obviously there are no data for what would have been the outcome had participants not been affected. Simply comparing participants with a control group does not usually solve the problem. Both groups are likely to be systematically different, unless some randomization of the flow is applied. Randomization is, however, often considered unethical for anti-poverty measures and therefore not applied widely. Moreover, it is impossible to randomize private flows, such as migration and remittances. Using regression or matching techniques, it is relatively easy to correct for between-group and between-household differences that are observed by the researchers. Yet, some relevant variables may go unobserved. For example, people with better production and business skills tend to apply for more micro-credit and at the same time get more earned income from the same resources.

In this study, we rely on two methods to control for selection on observed variables and time-invariant unobserved variables: fixed-effect regression, which is equivalent to first differences regression in the context of two-year panel data, and difference-in-differences with propensity score matching. These are widely-used methods to evaluate the impact of specific programs (Moffitt, 1994; Hoynes, 1997; Heckman *et al.*, 1997, Dehejia and Wahba, 1998; Smith and Todd, 2005; Wagstaff *et al.*, 2009), economic policies (Card, 1992; Card and Krueger, 1994; Currie and Fallick, 1996; Bazen and Skourias, 1997; Bell, 1997; Baker *et al.*, 1999; Stewart, 2004), and other economic factors such as education (Card and Krueger, 1992), migration and remittances (Yang *et al.*, 2005; Acosta *et al.*, 2008), and private transfers (Kang and Lee, 2003).

We used fixed-effect regressions to estimate the impact of credit, cash transfers and remittances on household income and consumption expenditure of households. We applied difference-in-differences with propensity score matching to estimate the impact of migration. Compared to the fixed-effect regressions, this method has the advantage that it does not impose assumptions about the functional form of the relation between flow and outcome. However, this method can only be used when the program variable is binary (dummy), which in this study only holds for migration.

It should be noted that the methods of fixed-effects regression and difference-in-differences only eliminate endogeneity bias caused by unobserved variables that are time-constant between survey rounds. In this study, it is reasonable to assume that the relevant household-level variables, such as business and production skills and ability, or motivation for higher income and expenditure consumption, were time-invariant during the two periods covered. Fixed-effect regression will, however, fail to remove all endogeneity bias if the unobserved variables which affect outcome and flows are not time-invariant. It is expected that the estimation bias resulting from these factors is small relative to the bias eliminated by using fixed-effects regression or difference-in differences. Availability of valid instrumental variables could improve the accuracy of impact estimates. However, finding good instrumental variables is not an easy task. Using invalid instruments can lead to a large bias in the impact estimates. Actually, we tried a large number of instrument-variables regressions, but the estimation results were not robust and reasonable.

We estimate the impact of a flow on expenditure poverty and inequality in several steps. Firstly, we estimate the impact of the flow on expenditure and construct the counterfactual expenditure in the flow. Secondly, a poverty measure or an inequality measure in the state of no flow will be estimated using this counterfactual expenditure. Thirdly, the impact of the flow on the poverty or inequality measure is measured by the difference between the poverty or inequality measure in the presence of the flow and the counterfactual poverty or inequality measure in the absence of the flow.

1.6 Thesis structure

The present thesis is structured in eight chapters as follows:

Chapter 1: Introduction
Chapter 2: An introduction to alternative methods in program impact evaluation
Chapter 3: Impact evaluation of multiple overlapping programs under a conditional independence assumption
Chapter 4: The impact of micro-credit and informal credit on poverty and inequality
Chapter 5: The impact of public and private transfers on poverty and inequality
Chapter 6: The impact of international remittances on poverty and inequality
Chapter 7: The impact of work and non-work migration on poverty and inequality
Chapter 8: Conclusions

Except Chapters 1 and 8 (the chapters on introduction and conclusions, respectively), the main contents of Chapters 2 through 7 are written as separate assays on impact evaluation. Therefore, there can be some overlaps of the contents of these chapters.

Chapter 2 reviews several popular methods of impact evaluation, which are used to address the problem of program selection in impact estimation. This chapter presents an overview of widely-used methods in program impact evaluation. In addition to a randomization-based method, the impact evaluation methods are categorized into methods assuming ‘selection on observables’ and methods assuming ‘selection on unobservables’. Two popular parameters of program impacts discussed in this chapter are the Average Treatment Effect (ATE) and the Average Treatment Effect on Treated (ATT). The chapter discusses how different impact evaluation methods measure ATE and ATT under various identification assumptions. These assumptions are presented in a unified framework of a counterfactual and a two equation model.

Among the impact evaluation methods, the matching method receives special attention and has increasingly been used in recent years. Under the assumption of conditional independence between potential outcomes and program assignment, program impacts measured by ATE and ATT can be identified and estimated using cross-section regression or propensity score matching (PSM). Traditional impact literature often deals with impact evaluation of a single program. In reality, one can participate in several programs simultaneously and these programs may be correlated. For example, the poor people can participate in several poverty alleviation programs at the same time. When measuring the impact of a program, we should note that participants and non-participants might attend other simultaneous programs. If there is a correlation between the selection of the program of interest and the selection of other programs even after observed variables are controlled, neglect of the other programs will lead to biased estimation of the impact of the program. If a correlation between the selection of the program and the selection of the other programs disappears once conditional on the observed variables, we can ignore these other programs. However, Chapter 3 shows that controlling for the participation in the other programs leads to some gain in efficiency in terms of mean-squared-error (MSE) using Monte Carlo simulation. More specifically, Chapter 3 shows that under the PSM method, the impact of a program of interest can be measured as a weighted average of program impacts on groups with different program statuses. In other words, it combines the propensity score matching on the conditioning variables and exact matching on the participation in the other programs. Using the Monte Carlo simulation it is also found that when impacts of the programs are high, this PSM method leads to lower MSE compared with other PSM estimations.

It should be noted that Chapters 2 and 3 do not address the main research questions of the study on the impacts of credit, transfers, remittances and migration on poverty and inequality. These chapters are independent essays which present the literature on program impact evaluation. Chapter 3 contributes to the literature on program impact evaluation by discussing impact evaluation of multiple correlated programs using regression and matching methods. The matching method which is developed in Chapter 3 is not applied in other empirical chapters, since this matching method is developed in the context of a conditional independence assumption and single-cross section data, whereas the other empirical chapters employ fixed-effects regressions and difference-in-differences methods which use panel data and do not rely on a conditional independence assumption. Although the matching method discussed Chapter 3 is not applied in other chapters, Chapter 3 is still included in this study for two reasons. Firstly, Chapter 3 also discusses an impact evaluation method which is somewhat related to the topic of this study. Secondly, this chapter reports an effort of the author to study the impact evaluation literature during his PhD research period.

Chapters 4, 5 and 6 examine the poverty targeting and impacts of the micro-credit from Vietnam Bank for Social Policies (VBSP), informal credit, public and private transfers, and international remittances on household consumption expenditure, poverty and inequality using the fixed-effect regressions. More specifically, Chapter 4 investigates how well the micro-credit from VBSP and informal credit reach the poor and to what extent they affect poverty and inequality in Vietnam. There are several reasons why the findings from this are interesting. First, the microfinance program is the biggest poverty reduction program in Vietnam. Second, nominal interest rates of VBSP are highly subsidized at about half the ‘market’ rates charged by most of the other microfinance programs (World Bank, 2007). The low and even negative real interest rates may have pushed out informal credit suppliers, weakened alternative programs, and/or caused high leakage rates to non-poor households (Burgess and Pande, 2005; Adams *et al.*, 1984). VBSP credit may therefore not only not have reached the poor, but also limit the availability of alternative sources of credit which otherwise would have been available. Third, informal credit has mostly been ignored in both research and policy, while it is presumably a very important source of finance for the poor, given the substantial size of the informal sector and its generally low entrance barriers. If informal credit is indeed important for the poor, the government may shift their focus at least partly away from direct provision of credit to stimulating the linkages between the formal and the informal credit market.

The main objective of Chapter 5 is to estimate and compare the impacts of public and private transfers on poverty and inequality in Vietnam. In addition, this chapter contributes to the existing literature on public and private transfers through a stepwise analysis showing not only the impact of public and domestic private transfers on poverty and inequality in Vietnam but also some of the underlying mechanisms. The first step of the analysis concerns a study of who the recipients of transfers are: do transfers reach the poor and are they equally spread over rural and urban areas? The second step involves analysis of potential interaction effects between transfers: do public transfers affect the level of private transfers? The third and fourth steps are an assessment of the effect of transfers on work effort and the ultimate effect of transfers on both household income and expenditure. The final step of this chapter is to measure the consequences of public and private transfers for poverty and inequality.

Chapter 6 provides empirical evidence on the poverty targeting and the impact of international remittances. International remittances to Vietnam are increasing in size and importance. Similar to the other empirical chapters, we will investigate the distribution of international remittances across the poor and non-poor and estimate the impacts of international remittances on work effort, household income and expenditure, poverty and inequality.

Chapter 7 aims to estimate impacts of work and non-work migration on several welfare indicators including working effort, remittances, income, income diversification, expenditure of the households sending out migrants, and poverty and inequality. In doing so, the chapter is expected to make several empirical contributions to the migration literature. First, it investigates how work and non-work migration affect different aspects of households from working efforts to expenditure and poverty of the migrant-sending households. Second, we estimate the impact of migration on poverty and inequality of the total population to examine the role of migration in reducing total poverty and inequality. Third, we use the panel data of VHLSS 2004 and 2006 to define the migration for the period 2004-2006. As a result, we can apply the difference-in-differences with matching to measure impact of migration. The difference-in-differences with matching is very popular in impact evaluation but as far as we know has never been applied in migration evaluation. Fourth, we compare the impacts between work and non-work migration.

It should be noted that we wrote separate chapters on migration and remittances for several reasons. First, migration does not necessarily lead to remittances, and the effect of migration can differ from the effect of remittances. Secondly, migration which is investigated in Chapter 7 includes both international and internal migration, while remittances analyzed in Chapter 8 refer to international remittances. Because of the features of VHLSS, we cannot distinguish between international migration and internal migration.

Finally, Chapter 8 presents the main empirical findings on the research questions which are posed by this study and proposes policy implications. A discussion on the limitations of the study and an outlook for future research finalizes the study.

Chapter 2 An introduction to alternative methods in program impact evaluation

2.1 Introduction

The main objective of impact evaluation of a program is to assess the extent to which the program has changed outcomes for the subjects. In other words, the impact of the program on the subjects is measured by the change in welfare outcome that is attributed only to the program. The magnitude of a program impact on a subject's outcome depends on many factors, but in general these factors can be grouped into three groups: intervention of the program, time of program implementation to program evaluation, and the characteristics of the subjects.

Obviously, the magnitude of program impact depends on what the program offers to the subject. Any change in the design of an intervention can lead to a change in program impact. For example, a vocational training provides courses in two ways: courses in the morning and course in the evening. For a given person, the impact of participating in morning courses can be higher than the impact of participating in evening courses, since her learning ability is better during the morning. Another example is a program of micro-credit that provides a small amount of credit for a targeted group of people. Eligible people, who meet the conditions for borrowing, can receive two specific amounts of credit, say C_1 and C_2, depending on their demand for credit. It is obvious that the impact of receiving C_1 credit is different from the impact of receiving C_2 credit.

A second factor that affects the measured impact of a program is when the data on outcome are collected. The results from an impact evaluation conducted one year after program implementation can be different from the results of an impact evaluation conducted two years after implementation. It is possible to assume that program impact can be stable after a period of time, i.e. the program can move the outcome level (in the no-program state) to a new level of outcome (in the program state) in the long term.

Thirdly, the impact of a program on a subject depends on her own characteristics. Different people will gain different benefits from a program.

Impact evaluation of a program provides very helpful information for decisions as to whether the program should be terminated or expanded. If a program has no impact on its participants, it needs to be terminated or modified. The impact of a program on a subject is defined as the difference between its outcome with the program and its outcome without the program. However, for participants of the program, we can observe only their outcome in the program state, but not their outcome had they not participated in the program – their counterfactual. Similarly, for non-participants we can observe the outcome in the no-program state, but not the outcome in the program state. This problem is sometimes referred to as a missing data problem, and it complicates impact evaluation.

Although it is virtually impossible to measure program impact for each subject (Heckman *et al.*, 1999), we can estimate the average impact for a group of subjects. The main difficulty is to estimate the average counterfactual outcomes. If there are concurrent factors that affect outcome and we are unable to net out the impact of these factors from program impact, the counterfactual estimates will be biased.

This chapter presents an overview of several widely-used methods in program impact evaluation. In addition to a randomization-based method in which participants are selected randomly, these methods are categorized into: (1) methods assuming 'selection on observables', and (2) methods assuming 'selection on unobservables'. If the impact of the program of interest is correlated with other factors affecting the population, we need to isolate the program impact. 'Selection on observables' methods are based on an assumption that we can observe all these correlated factors. In contrast, if we are not able to observe all correlated factors, we need to resort to 'selection on unobservables' methods. This chapter discusses the identification assumptions and estimation strategy of each method using a unified framework of counterfactuals and a two-equation model.

The chapter is structured into six sections. Section 2 gives an overview of the problems in program impact assessment. Section 3 illustrates how random selection can solve these problems. Next, sections 4 and 5 introduce methods relying on selection of observables and methods relying on selection of unobservables, respectively. Finally, section 6 presents some conclusions.

2.2 Problems in program impact evaluation

2.2.1 Parameters of interest

To make the definition of impact evaluation explicit, suppose that there is a program assigned to some people in a population *P*. For simplicity, let's assume that there is a single program, and denote by *D* the binary variable of participation in the program, i.e. $D=1$ if she/he participates in the program, and $D=0$ otherwise. *D* is also called the variable of treatment status. Further let *Y* denote the observed value of the outcome. This variable can receive two values depending on the participation variable, i.e. $Y=Y_1$ if $D=1$, and $Y=Y_0$ if $D=0$.[3] These outcomes are considered at a point in time or over a period of time after the program is implemented.

The impact of the program on the outcome of person *i* is measured by:

$$\Delta_i = Y_{i1} - Y_{i0}, \tag{2.1}$$

which is the difference in outcome between the program state and the no-program state. The problem is that we cannot observe both terms in Equation (2.1) for the same person. For those who participated in the program, we can observe only Y_1, and for those who did not participate in the program we can observe only Y_0.

It is practically impossible to estimate the program impact for each person (Heckman *et al.*, 1999), because we cannot know the counterfactual outcome exactly. If we constructed an estimator for individual effects, the associated standard error would be very large. Program impact can, however, be estimated for a group of people. In the literature on program impact evaluation, two popular parameters are the Average Treatment Effect (ATE), and the Average Treatment Effect on the Treated (ATT).

ATE is the expected impact of the program on a person who is randomly selected and assigned to the program. It is defined as:

$$ATE = E(\Delta) = E(Y_1 - Y_0) = E(Y_1) - E(Y_0). \tag{2.2}$$

[3] *Y* can be a vector of outcomes, but for simplicity let's consider a single outcome of interest.

This is the traditional average partial effect (APE) in econometrics. To see this, let's write the observed outcome in a switching model (Quandt, 1972):

$$Y = DY_1 + (1-D)Y_0, \tag{2.3}$$

where Y is observed outcome, which is equal to Y_1 and Y_0 for participants and non-participants, respectively.

Then,

$$APE = E(Y \mid D=1) - E(Y \mid D=0) = E(Y_1) - E(Y_0) = ATE. \tag{2.4}$$

Most programs are targeted to certain subjects. The important question is the program impact on those who participated in the program. If the program has a positive impact, policy makers would be interested in expanding the program for similar groups. The expected treatment effect on the participants is equal to:

$$ATT = E(\Delta \mid D=1) = E(Y_1 - Y_0 | D=1) = E(Y_1 | D=1) - E(Y_0 | D=1). \tag{2.5}$$

Except for the case of randomized programs that is discussed in section 3, ATE and ATT are, in general, different from each other, since program participation often depends on the potential outcomes, and as a result $E(Y_1) \neq E(Y_1 \mid D=1)$, and $E(Y_0) \neq E(Y_0 \mid D=1)$. To see this, Equation (2.2) can be rewritten as:

$$\begin{aligned} ATE = E(Y_1) - E(Y_0) &= \left[E(Y_1 \mid D=1)\Pr(D=1) + E(Y_1 \mid D=0)\Pr(D=0)\right] \\ &\quad - \left[E(Y_0 \mid D=1)\Pr(D=1) + E(Y_0 \mid D=0)\Pr(D=0)\right] \\ &= \left\{\left[E(Y_1 \mid D=1) - E(Y_0 \mid D=1)\right]\Pr(D=1)\right\} \\ &\quad + \left\{\left[E(Y_1 \mid D=0) - E(Y_0 \mid D=0)\right]\Pr(D=0)\right\}, \end{aligned} \tag{2.6}$$

where $\Pr(D=1)$ and $\Pr(D=0)$ are the proportions of participants and non-participants of the program, respectively.

Define the average treatment effect on the non-treated (ATNT) as:

$$ANTT = E(Y_1 \mid D=0) - E(Y_0 \mid D=0). \tag{2.7}$$

This parameter can be interpreted as the effect that non-participants would have gained had they participated in the program.

Then, ATE can be written as follows:

$$ATE = ATT\Pr(D=1) + ATNT\Pr(D=0). \tag{2.8}$$

Estimation of ATE and ATT is not straightforward, since there are some components that cannot be observed directly. We can observe the mean outcomes of participants and non-participants. As a result, the terms $E(Y_1 \mid D=1)$ and $E(Y_0 \mid D=0)$ in (2.4) and (2.6) can be estimated directly. However, the counterfactual terms $E(Y_1 \mid D=0)$ and $E(Y_0 \mid D=1)$ are not observed. $E(Y_0 \mid D=1)$ is the expected outcome of the participants had they not participated in the program, while $E(Y_1 \mid D=0)$ is the expected outcome of non-participants had they participated in the program. Thus the estimation of ATE and ATT is not straightforward, and the different methods discussed in this chapter provide estimates under certain assumptions on how the program is assigned to the population and how the outcome is determined.

Note that we can allow program impact to vary across a vector of observed variables, *X*, since we might be interested in the program impact on certain groups that are specified by the characteristics, *X*. The so-called conditional parameters are expressed as follows:

$$ATE_{(X)} = E(\Delta \mid X) = E(Y_1 \mid X) - E(Y_0 \mid X), \tag{2.9}$$

and

$$ATT_{(X)} = E(\Delta \mid X, D=1) = E(Y_1 \mid X, D=1) - E(Y_0 \mid X, D=1). \tag{2.10}$$

If we denote by ATNT$_{(X)}$ the ATNT conditional on *X*:

$$ATNT_{(X)} = E(\Delta \mid X, D=0) = E(Y_1 \mid X, D=0) - E(Y_0 \mid X, D=0), \tag{2.11}$$

then, similar to (2.8):

$$ATE_{(X)} = ATT_{(X)} \Pr(D=1 \mid X) + ATNT_{(X)} \Pr(D=0 \mid X), \tag{2.12}$$

where $\Pr(D=1 \mid X)$ and $\Pr(D=0 \mid X)$ are the proportion of the participants and non-participants given the X variables, respectively.

In the following discussion, we will focus on the conditional parameters − ATE$_{(X)}$ and ATT$_{(X)}$ − since if they are identified, the unconditional parameters − ATE and ATT − can also be identified:

$$ATE = \int_X ATE_{(X)} dF(X), \tag{2.13}$$

$$ATT = \int_{X|D=1} ATT_{(X)} dF(X \mid D=1). \tag{2.14}$$

2.2.2 Econometric framework of program impact evaluation

As mentioned, the selection of a method to estimate ATE and ATT for a program depends crucially on assumptions on how people are selected in the program as well as on how the potential outcomes are affected by the program and other factors. Although the assumptions are often not tested, they need to be stated explicitly so that one can judge whether the results are valid and robust. A popular way to discuss assumptions is to use the model of two outcome equations of Rubin (1974), in which potential outcomes Y_0 and Y_1 are expressed as functions of individual characteristics (conditioning variables), X:[4]

$$Y_0 = \alpha_0 + X\beta_0 + \varepsilon_0, \tag{2.15}$$

$$Y_1 = \alpha_1 + X\beta_1 + \varepsilon_1. \tag{2.16}$$

Y_0 and Y_1 can be any functions of X, not necessarily linearly or parametrically specified, and all the identification strategies presented in this chapter are still valid. However, to illustrate ideas and links with the traditional linear regression framework, we assume linearity.

For simplicity and identification of program impact in some parametric regressions, we require X to be exogenous in the potential outcome equations.

Assumption 2.1. $E(\varepsilon_0 \mid X) = E(\varepsilon_1 \mid X) = 0$ (A.2.1)

In addition, two additional assumptions are needed for the validity of the micro-approach of program impact evaluation. The first assumption is common in the partial equilibrium approach, and required in the literature on program impact evaluation. This assumption is called the stable unit treatment assumption.

Assumption 2.2. $Y_i \perp D_j \quad \forall i, j$ (A.2.2)

i.e. realized (observed) outcome of individual i, Y_i, is independent of the program status of individual j, D_j.

This assumption implies that there is no spill-over effect of the program. In other words, an individual's participation in the program does not affect the outcome of other people. For programs that cover a large proportion of the population, this assumption can be violated. For example, if a large number of farmers receive preferential credit, they can reduce production costs and increase their market share, which can affect the revenue of farmers who do not

[4] For simplicity, subscript i is dropped.

receive similar credit.[5] When the assumption does not hold, one might use general equilibrium analysis.[6]

The second assumption is implicit in the two-equation model. Writing the same X variables in the two Equations (2.15) and (2.16) means that for each person the status of program participation (treatment status) does not affect X. Formally speaking, once conditional on potential outcomes, X are independent of D.

Assumption 2.3.[7] $X \perp D \mid Y_0, Y_1$ (A.2.3)

This assumption does not mean that X is uncorrelated with D, but that X is uncorrelated with D given the potential outcomes. Under this assumption D does not affect X once conditioning on the potential outcomes. Although this assumption is not an indispensable condition to identify program impact, it is maintained for simplicity. If D affects X, it is much more complex to capture the true impact of program. In the following discussions of different methods in impact evaluation, assumptions 2.1, 2.2 and 2.3 are implicitly assumed to hold.

In the two-equation framework, the parameters of interest for impact evaluation are expressed as follows:

$$\begin{aligned} ATE_{(X)} &= E(Y_1 \mid X) - E(Y_0 \mid X) \\ &= E[\alpha_1 + X\beta_1 + \varepsilon_1 \mid X] - E[\alpha_0 + X\beta_0 + \varepsilon_0 \mid X] \\ &= (\alpha_1 - \alpha_0) + X(\beta_1 - \beta_0) \end{aligned} \quad (2.17)$$

and,

$$\begin{aligned} ATT_{(X)} &= E(Y_1 | X, D=1) - E(Y_0 | X, D=1) \\ &= E[\alpha_1 + X\beta_1 + \varepsilon_1 \mid X, D=1] - E[\alpha_0 + X\beta_0 + \varepsilon_0 \mid X, D=1] \\ &= (\alpha_1 - \alpha_0) + X(\beta_1 - \beta_0) + E(\varepsilon_1 - \varepsilon_0 \mid X, D=1). \end{aligned} \quad (2.18)$$

It should be noted that even if coefficients $\alpha_0, \alpha_1, \beta_0, \beta_1$ can be estimated, $ATT_{(X)}$ still includes an unobservable term $E(\varepsilon_1 - \varepsilon_0 \mid X, D=1)$, while $ATE_{(X)}$ does not. To identify $ATT_{(X)}$, in some cases, we need the following additional assumption:

Assumption 2.4. $E(\varepsilon_0 \mid X, D=1) = E(\varepsilon_1 \mid X, D=1)$ (A.2.4)

This assumption states that given X, the expectation of the unobserved variables for the participants is the same regardless of the program so that the unobserved term in (2.18) vanishes. It is worth noting that assumption (A.2.4) does not involve the expectation of the error terms conditional on all the X variables. Instead, this assumption should hold for a subset of the conditional parameters that we are interested in. There might be many explanatory variables X, but we are often only interested in $ATE_{(X)}$ and $ATT_{(X)}$ conditional on a certain number of variables in X, not all X. For example, if we want to estimate impacts of a program on income for different age groups, we need (A.2.4) for age only, i.e. $E(\varepsilon_0 \mid age, D=1) = E(\varepsilon_1 \mid age, D=1)$.

To link the counterfactual data with the observed data, substitute (2.15) and (2.16) into the switching model in (2.3). This results in:

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[5] For other examples on the violation of this assumption, see e.g. Heckman *et al.* (1999) and Rubin (1978).

[6] For a more detailed discussion on general equilibrium approach in impact evaluation, see e.g. Heckman *et al.* (1999) and Heckman *et al.* (1998b).

[7] Another expression for conditional independence $f(X \mid Y_0, Y_1) = f(X \mid D, Y_0, Y_1)$, where *f(.)* is conditional density of X. For discussion on conditional independence, see e.g. Dawid (1979).

$$Y = D(\alpha_1 + X\beta_1 + \varepsilon_1) + (1-D)(\alpha_0 + X\beta_0 + \varepsilon_0)$$
$$= \alpha_0 + X\beta_0 + D[(\alpha_1 - \alpha_0) + X(\beta_1 - \beta_0) + (\varepsilon_1 - \varepsilon_0)] + \varepsilon_0. \quad (2.19)$$

Equation (2.19) is a rather general model of program impact, in which program impact measured by the coefficient of variable *D* varies across subjects. This coefficient depends on both observable and unobservable variables, *X* and ε. It can also be correlated with *D* if *D* is correlated with *X* and ε. This is a random coefficient model in which the coefficient is correlated with observed and unobserved characteristics variables.

The remaining problem is how to estimate $\alpha_0, \alpha_1, \beta_0, \beta_1$ without bias. The error term in (2.19) is required to have conventional property:

$$E[(\varepsilon_1 - \varepsilon_0)D + \varepsilon_0 \mid X, D] = 0. \quad (2.20)$$

To complete this section, a model of program participation is introduced. The participation of a person in the program can depend on the selection criteria of the program and own decisions of the person. The program participation model is often expressed in a latent index framework:

$$D^* = \theta W + v, \quad (2.21)$$
$$D = 1 \ if \ D^* > 0,$$
$$D = 0 \text{ otherwise,}$$

where D^* is the latent index of the program selection that is correlated with observable variables, *W* and unobservable term, *v*. *W* and *v* are all the variables that affect program participation.

2.3 Method based on randomized design

2.3.1 Impact measurement of randomized programs

In the ideal situation for impact evaluation, a program is assigned randomly to subjects, and those who are assigned the program are willing to participate. The non-participants will form the control group, and do not participate in similar programs. In this case, program assignment *D* is said to be independent of the potential outcomes Y_0 and Y_1. We can state this condition as an assumption.

Assumption 3.1. $Y_0, Y_1 \perp D$ (A.3.1)

Proposition 3.1. ATE$_{(X)}$, ATT$_{(X)}$, ATE and ATT are identified under assumption (A.3.1).[8]

Proof: As a result of assumption (A.3.1), conditional on *D* the value of the potential outcomes does not alter:

$$E(Y_1 \mid X) = E(Y_1 \mid X, D=1) = E(Y_1 \mid X, D=0), \quad (3.1)$$
$$E(Y_0 \mid X) = E(Y_0 \mid X, D=1) = E(Y_0 \mid X, D=0). \quad (3.2)$$

Hence:

$$ATE_{(X)} = E(Y_1 \mid X) - E(Y_0 \mid X)$$
$$= E(Y_1 \mid X, D=1) - E(Y_0 \mid X, D=0), \quad (3.3)$$

and

[8] Assumption (A.2.1) is made for all methods in impact evaluation.

$$ATT_{(X)} = E(Y_1 \mid X, D=1) - E(Y_0 \mid X, D=1) \\ = E(Y_1 \mid X, D=1) - E(Y_0 \mid X, D=0). \tag{3.4}$$

Thus, $ATE_{(X)} = ATT_{(X)}$. Since it is possible to observe all terms in $ATE_{(X)}$ and $ATT_{(X)}$, these parameters are identified.

The program impact is estimated simply by comparing the mean outcome between the participants and non-participants. When we have post-program data from a representative sample on participants and non-participants in a randomized program, we can use sample mean of outcomes for treatment and control group to estimate ATE, ATT, and their conditional version $ATE_{(X)}$ and $ATT_{(X)}$.

Another way to estimate the program impact is to use a regression model. In the framework of the two-equation model, assumption (A.3.1) implies:

$$D \perp \varepsilon_0, \varepsilon_1 . \tag{3.5}$$

In order to get unbiased estimators of $ATE_{(X)}$ and $ATT_{(X)}$ using regression, we need the assumption on exogeneity of X, i.e. (A.2.1).

Proposition 3.2. Under assumptions (A.3.1), $ATE_{(X)}$, $ATT_{(X)}$, ATE and ATT can be estimated without bias by OLS regression.

Proof: Under (A.3.1) and (A.2.1), $ATE_{(X)}$ and $ATT_{(X)}$ are the same:

$$ATE_{(X)} = ATT_{(X)} = (\alpha_1 - \alpha_0) + X(\beta_1 - \beta_0) . \tag{3.6}$$

The coefficients can be estimated without bias from the equation:

$$Y = \alpha_0 + X\beta_0 + D[(\alpha_1 - \alpha_0) + X(\beta_1 - \beta_0)] + [D(\varepsilon_1 - \varepsilon_0) + \varepsilon_0], \tag{3.7}$$

since the error term has the following property:

$$E[D(\varepsilon_1 - \varepsilon_0) + \varepsilon_0 \mid X, D] = DE(\varepsilon_1 - \varepsilon_0 \mid X, D) + E(\varepsilon_0 \mid X, D) = E(\varepsilon_0 \mid X) = 0 . \tag{3.8}$$

Thus the estimator of the parameters is:

$$A\hat{T}E_{(X)} = A\hat{T}T_{(X)} = (\hat{\alpha}_1 - \hat{\alpha}_0) + (\hat{\beta}_1 - \hat{\beta}_0)X \tag{3.9}$$

Once the conditional parameters are identified, the unconditional parameters are also identified because of (2.13) and (2.14).

2.3.2 Program impact evaluation under experiment

In reality, we are often interested in the impact of a program that is targeted at specific subjects. For example, poverty reduction programs aim to provide the poor with supports to eliminate poverty. Vocational training programs are targeted at the unemployed. The program is not assigned randomly to people in the population. In this case, experimental designs can be used to evaluate the impact of the targeted program.

A randomization design or experiment is conducted by choosing a group of people who are willing to participate in the experiment. Denote by D^* the variable indicating the experiment participation. $D^* = 1$ for those in the experiment, and $D^* = 0$ otherwise. Among people with $D^* = 1$, we randomly select people for program participation. Denote R as a variable that $R = 1$ for the participants, and $R = 0$ for non-participants in the experiment. The participants are called the treatment group, while the non-participants (among those in the experiment) are called the control group (or comparison group).

The randomization of the program among those in the experiment is stated formally as follows:

Assumption 3.2.[9] $Y_0, Y_1 \perp R \mid D^* = 1$ (A.3.2)

To estimate both ATE$_{(X)}$ and ATT$_{(X)}$, we need an additional assumption:

Assumption 3.3. $E(Y_1 \mid X, D=0) = E(Y_1 \mid X, D=1) = E(Y_1 \mid X, D^*=1)$

$$E(Y_0 \mid X, D=0) = E(Y_0 \mid X, D=1) = E(Y_0 \mid X, D^*=1) \qquad \text{(A.3.3)}$$

That is, once conditional on X, the expected outcome of those in the experiment is the same as the expected outcome of those not participating in the experiment. It is implied that people who participate in the experiment are similar to those in the reality once conditional on X.

Proposition 3.3. ATE$_{(X)}$, ATT$_{(X)}$, ATE and ATT are identified under assumptions (A.3.2) and (A.3.3).

Proof:

Under (A.3.2) and (A.3.3), ATT is identified:

$$\begin{aligned} ATT_{(X)} &= E(Y_1 \mid X, D=1) - E(Y_0 \mid X, D=1) \\ &= E(Y_1 \mid X, D^*=1) - E(Y_0 \mid X, D^*=1) \qquad (3.10) \\ &= E(Y_1 \mid X, D^*=1, R=1) - E(Y_0 \mid X, D^*=1, R=0), \end{aligned}$$

and similarly, the average treatment effect on the non-treated (ATNT) is the same:

$$\begin{aligned} ATNT_{(X)} &= E(Y_1 \mid X, D=0) - E(Y_0 \mid X, D=0) \\ &= E(Y_1 \mid X, D^*=1) - E(Y_0 \mid X, D^*=1) \qquad (3.11) \\ &= E(Y_1 \mid X, D^*=1, R=1) - E(Y_0 \mid X, D^*=1, R=0). \end{aligned}$$

Thus, the ATE$_{(X)}$ is identified and the same as ATT$_{(X)}$ due to (2.12).

As a result, (3.10) is the unbiased estimator of ATT$_{(X)}$ and ATE$_{(X)}$. We simply calculate the difference in the mean outcome between the participants and non-participants of the program among those attending the experiment. Once the conditional parameters are identified, the conditional parameters are also identified because of (2.13) and (2.14).

2.3.3 Advantages and disadvantages of the method based on randomization

There is no dispute that among methods of program impact evaluation, the method that is based on randomization of the program produces the most reliable results. When the randomization of a program is properly conducted, the average impact of the program is identified without any further assumptions. The randomization of programs ensures that there is no systematic difference in both observable and unobservable characteristics between treatment and control groups. As a result, any difference in the average outcome between

[9] Assumption (A.3.2) states that the selection of participants among the experimental people is independent of the potential outcomes. In fact we only need a weaker version to identify ATT:
$E(Y_1 \mid D^*=1, R=1) = E(Y_1 \mid D^*=1, R=1)$ and $E(Y_0 \mid D^*=1) = E(Y_0 \mid D^*=1, R=0)$.
However this assumption is difficult to interpret. Thus we mention the assumption (A.3.2) in discussing the identification of the program impact.

these groups can be attributed to the program effect. Another advantage of the method is the ease in explaining its results to program designers and policy makers, who often do not have much knowledge of statistics and econometrics.

The randomized-program method, however, suffers from several drawbacks. Firstly, it is hard to randomize a program which is targeted at a specific group due to issues of ethics and politics. Randomization of a program means exclusion of some eligible people from the program. It is unfair to deny (or delay) a program that provides supports such as health care or education for some eligible people. Policy makers will be criticized if they cannot explain why some eligible people are not allowed to participate in programs. Nevertheless, the randomization of a program can be conducted if the fund for the program is not sufficient to cover all eligible people. Some people have to participate at a later date, and they can serve as the control group for those who participate at the beginning.

Secondly, the implementation and evaluation of a socio-economic program that is based on randomization is often expensive. Subjects are scattered in the population, which increases the cost of program administration and data collection for impact evaluation.

Thirdly, there can be some factors that bias the estimates from randomization-based evaluation. These factors invalidate the key identification assumption (A.3.1), $D \perp Y_0, Y_1$. Two problems that are widely mentioned are attrition and substitution effects.

Attrition means that some people in the treatment group quit the program during implementation. As a result, their observed outcome is not the potential outcome in the presence of the program, Y_1. If this drop-out is random, there is no concern about this problem since the randomization feature remains preserved. If attrition is not random but correlated with some characteristics of the drop-outs, the remaining subjects in the treatment group who actually take the program will be systematically different from the subjects in the control group. In other words, there is self-selection into the program of the participants, which is dealt with by the alternative methods discussed in the following sections. Estimation of program impact is no longer straightforward: the mean difference in outcome between the treatment and control group is not an estimator of the program impact, but an estimator of 'the mean effect of the offer of treatment' (Heckman *et al.*, 1999). However, if we expect that program impact is negligible for the drop-outs, we can measure the ATE by this mean difference. This is because the drop-outs are not interested in the program, and there would be no impact on them if we force them to follow the whole program.

The substitution effect means that some people in the control group might try to get access to programs that are similar to the program to be evaluated. The substitution programs can contaminate the outcome of the control group. It is implied that if the program had not been implemented, the participants would have taken other similar programs. The mean difference in outcome between the control and treatment groups reflects 'the mean incremental effect of the program relative to the world in which it does not exist' (Heckman *et al.*, 1999). To truly capture the program impact, we need to have information on impacts of the substituted programs, and subtract them from the outcome of the control group to estimate the potential outcome of the treatment group in the absence of the program.

Finally, a randomized program that is used for impact-evaluation purposes is often a pilot program, and the impact of the pilot program can be far from the impact of the program when it is implemented in reality. A pilot program is often smaller and more easily administered. In addition, people involved in a pilot program including the administrators, control and treatment group, may follow the program rules more strictly if they know the program is a pilot.

2.4 Methods assuming selection on observables

2.4.1 Selection bias and conditional independence assumption

As mentioned, most programs are not randomized in reality. When a program is not assigned randomly, the potential outcomes of the participants will be different from those of non-participants. Assumption (A.3.1) no longer holds, and simple comparison of mean outcomes between participants and non-participants does not produce unbiased estimators of the program impact. The bias in these estimators is called the selection bias in the literature.

To see the selection bias in estimating the average treatment effect $ATE_{(X)}$ conditioning on X, rewrite the formula of $ATE_{(X)}$:

$$\begin{aligned} ATE_{(X)} &= (\alpha_1-\alpha_0)+X(\beta_1-\beta_0)+E(\varepsilon_1-\varepsilon_0 \mid X) \\ &= (\alpha_1-\alpha_0)+X(\beta_1-\beta_0) \\ &\quad +\{[\Pr(D=1\mid X)E(\varepsilon_1\mid X,D=1)+\Pr(D=0\mid X)E(\varepsilon_1\mid X,D=0)] \\ &\quad -[\Pr(D=1\mid X)E(\varepsilon_0\mid X,D=1)+\Pr(D=0\mid X)E(\varepsilon_0\mid X,D=0)]\}. \end{aligned} \tag{4.1}$$

When we use the following estimator:

$$\begin{aligned} \hat{ATE}_{(X)} &= E(Y_1\mid X,D=1)-E(Y_0\mid X,D=0) \\ &= \mathrm{E}(\alpha_1+X\beta_1+\varepsilon_1\mid X,D=1)-\mathrm{E}(\alpha_0+X\beta_0+\varepsilon_0\mid X,D=0) \\ &= (\alpha_1-\alpha_0)+X(\beta_1-\beta_0)+E(\varepsilon_1\mid X,D=1)-E(\varepsilon_0\mid X,D=0), \end{aligned} \tag{4.2}$$

the bias is equal to:

$$\begin{aligned} \hat{ATE}_{(X)}-ATE_{(X)} &= [E(\varepsilon_1\mid X,D=1)-E(\varepsilon_0\mid X,D=0)] \\ &\quad -\{[\Pr(D=1\mid X)E(\varepsilon_1\mid X,D=1)+\Pr(D=0\mid X)E(\varepsilon_1\mid X,D=0)] \\ &\quad -[\Pr(D=1\mid X)E(\varepsilon_0\mid X,D=1)+\Pr(D=0\mid X)E(\varepsilon_0\mid X,D=0)]\} \\ &= \Pr(D=0\mid X)[E(\varepsilon_1\mid X,D=1)-E(\varepsilon_1\mid X,D=0)] \\ &\quad +\Pr(D=1\mid X)[E(\varepsilon_0\mid X,D=1)-E(\varepsilon_0\mid X,D=0)]. \end{aligned} \tag{4.3}$$

Even though X are controlled for, selection bias in estimating $ATE_{(X)}$ can arise if the conditional expectation of unobserved variables in potential outcomes, ε_0 and ε_1, is different for the participants and non-participants.

Similarly, if we use the same estimator in (4.2) for $ATT_{(X)}$, the selection bias will be:

$$\hat{ATT}_{(X)}-ATT_{(X)} = E(\varepsilon_0\mid X,D=1)-E(\varepsilon_0\mid X,D=0). \tag{4.4}$$

The selection bias stems from the difference in the conditional expectation of unobserved variables, ε_0, between the participants and non-participants.[10]

One intuitive way to avoid the selection biases, (4.3) and (4.4), in estimating $ATE_{(X)}$ and $ATE_{(X)}$ is to invoke assumptions so that the selection biases are equal to zero. The

[10] If one has data before and after a program, they sometimes use the before and after estimator to estimate the program impact. The bias is equal to $E(Y_{0B}\mid D=1)-E(Y_{0A}\mid D=1)$, where $E(Y_{0B}\mid D=1)$ and $E(Y_{0A}\mid D=1)$ are the expectation of participants' outcome in the state of no program before and after the program, respectively. The assumption is valid if there is no change in the participants' outcome during the program implementation if they had not participated. Intuitively, this assumption seems plausible in the short term, but might be unreasonable in the long term.

assumption on 'selection on observables' assumes that one is able to observe all variables that affect both the program selection and potential outcomes so that once conditioned on these variables, the potential outcomes Y_0 and Y_1 are independent of the program assignment. In Rosenbaum and Rubin (1983), this assumption is called ignorability of treatment or conditional independence. Formally, it is written as:

Assumption 4.1. $Y_0, Y_1 \perp D \mid X$ (A.4.1)

Assumption (A.4.1) can be considered as a conditional version of assumption (A.3.1). Once we have controlled for X, the assignment of the program becomes randomized. Actually, we just need a weaker form of (A.4.1) in order to identify the program impact parameters.

Assumption 4.1'. $E(Y_0 \mid X, D) = E(Y_0 \mid X)$

$E(Y_1 \mid X, D) = E(Y_1 \mid X)$ (A.4.1')

This is called the conditional mean independence assumption. It is weaker than (A.4.1) in the sense that (A.4.1) implies (A.4.1') but the reverse is not correct. Although assumption (A.4.1') is weaker and sufficient to identify the program impacts, it is difficult to imagine that it will hold in reality since it involves the expectation terms. Thus, we will use assumption (A.4.1) in the discussion of program impact evaluation.

A corollary of assumption (A.4.1) is that the error terms in the potential outcomes are also independent of D given X, i.e.:

$$\varepsilon_0, \varepsilon_1 \perp D \mid X . \quad (4.5)$$

Under condition (4.5), we have (Dawid, 1979):

$$E(\varepsilon_0 \mid X, D=0) = E(\varepsilon_0 \mid X, D=1), \quad (4.6)$$

$$E(\varepsilon_1 \mid X, D=0) = E(\varepsilon_1 \mid X, D=1). \quad (4.7)$$

As a result of equation (4.6) and (4.7), the selection biases given in (4.3) and (4.4) are equal to zero. $ATE_{(X)}$ and $ATT_{(X)}$ are identified, and so are ATE and ATT.

In addition, assumption (4.5) results in:

$$E(\varepsilon_1 - \varepsilon_0 \mid X, D=1) = E(\varepsilon_1 - \varepsilon_0 \mid X) = 0 . \quad (4.8)$$

Hence, $ATE_{(X)}$ is equal to $ATT_{(X)}$.

Assumption (A.4.1) is the key assumption for identifying program impacts that several methods rely on. Thus the methods are called methods based on 'selection on observables'. This does not mean that we have to observe all information on the program selection, i.e. D is deterministic, but it implies that all the X variables that correlate D with Y_0 and Y_1 are observed. Put differently, the unobserved variables are required to be uncorrelated with Y_0 and Y_1 given X. Three widely-used sets of methods that use this assumption are presented in this chapter, namely regression methods, matching methods, regression discontinuity. All these methods can be conducted using single cross section data.

2.4.2 Regression methods assuming selection on observables

2.4.2.1 Linear regression

For simplicity we maintain the assumption of linearity in outcome equations for this section. Next we will discuss the case of nonlinear functions of potential outcomes.

Proposition 4.1. Given assumptions (A.4.1), OLS regression produces unbiased estimators of $ATE_{(X)}$, $ATT_{(X)}$, ATE and ATT.

Proof: The observed outcome is as follows:

$$Y = \alpha_0 + X\beta_0 + D[(\alpha_1 - \alpha_0) + X(\beta_1 - \beta_0)] + [D(\varepsilon_1 - \varepsilon_0) + \varepsilon_0] \quad (4.9)$$

The proof is now similar to the proof of Proposition 3.2. The error term has the following property:

$$E[D(\varepsilon_1 - \varepsilon_0) + \varepsilon_0 \mid X, D] = DE(\varepsilon_1 - \varepsilon_0 \mid X, D) + E(\varepsilon_0 \mid X, D) = E(\varepsilon_0 \mid X) = 0 . \quad (4.10)$$

Under assumption (A.4.1), $ATE_{(X)}$ and $ATT_{(X)}$ are the same, and the estimators of these conditional parameters are:

$$A\hat{T}E_{(X)} = A\hat{T}T_{(X)} = (\hat{\alpha}_1 - \hat{\alpha}_0) + (\hat{\beta}_1 - \hat{\beta}_0)X . \quad (4.11)$$

ATE and ATT are identified simply by taking the expectation of $ATE_{(X)}$ and $ATT_{(X)}$ over the distribution of X for the whole population, and the distribution of X for the participant population, respectively.

Instead of running one regression for the whole sample on the participants and non-participants, it is possible to run two separate regressions for the sub-samples of the participants and non-participants, respectively. Under assumption (A.4.1), we can write:

$$E(Y_0 \mid X, D = 0) = E(Y_0 \mid X) = \alpha_0 + \beta_0 X + E(\varepsilon_0 \mid X), \quad (4.12)$$

$$E(Y_1 \mid X, D = 1) = E(Y_1 \mid X) = \alpha_1 + \beta_1 X + E(\varepsilon_1 \mid X) . \quad (4.13)$$

2.4.2.2 Nonlinear regression

In some cases, the assumption about linearity of the potential outcome function is not reasonable. One important case is that the outcome variable is binary, e.g. one can be interested in the impact of a vocational training program on the probability of getting a job. The outcome variable equals 1 if a person is employed, and 0 otherwise. As we know, the widely-used models are logit or probit instead of the linear model.

In general, we write the potential outcome equations as follows:

$$Y_0 = g(X, \beta_0) + \varepsilon_0 , \quad (4.14)$$

$$Y_1 = g(X, \beta_1) + \varepsilon_1 , \quad (4.15)$$

where g(X) is any function of X which can be linear or non-linear in X and parameters β_0 and β_1. Under assumption (A.4.1) we can estimate the two equations separately using the sub-samples of non-participants and participants, respectively. $ATE_{(X)}$ and $ATT_{(X)}$ are identified:

$$ATE_{(X)} = ATT_{(X)} = g(X, \beta_1) - g(X, \beta_0) . \quad (4.16)$$

ATE and ATT are then identified simply by taking the expectation of $ATE_{(X)}$ and $ATT_{(X)}$ over the distribution of X for the whole population, and the distribution of X for the participant population, respectively.

As a matter of estimation, equation (4.16) can suggest the following general estimators for the treatment parameters:

$$A\hat{T}E_{(X)} - A\hat{T}T_{(X)} = g(X,\hat{\beta}_1) - g(X,\hat{\beta}_0), \tag{4.17}$$

$$A\hat{T}E = \frac{1}{n}\sum_{X\in S_X}\left[g(X,\hat{\beta}_1) - g(X,\hat{\beta}_0)\right], \tag{4.18}$$

$$A\hat{T}T = \frac{1}{n_1}\sum_{X\in S_X|D=1}\left[g(X,\hat{\beta}_1) - g(X,\hat{\beta}_0)\right], \tag{4.19}$$

where n is the number of the total observations (including participants and non-participants), n_1 is the number of the participants in sample data, and S_X are the space of the X variables in the data sample.

2.4.2.3 Advantages and disadvantages of the regression methods

The above-described regression methods have the advantage of simple implementation, but also have three main drawbacks. Firstly, they impose a specific functional form on the relation between outcome and conditioning variables and the program participation variable. Secondly, the estimator of the program impact will be inefficient if the parametric regressions are plagued by problems of multicollinearity or heteroscedasticity. Finally, the method relies on the assumption of program selection based on observable variables. This assumption is strong.

2.4.3 Matching methods

2.4.3.1 Identification assumptions

There is a large amount of literature on matching methods of impact evaluation. Important contributions in this area can be found in studies such as Rubin (1977, 1979, 1980), Rosenbaum and Rubin (1983, 1985a), and Heckman *et al.* (1997b). The matching method can be used to estimate the two program impact parameters, ATE and ATT under the conditional independence assumption (A.4.1). The basic idea of the matching method is to find a control group (also called comparison group) that has the same (or at least similar) distribution of X as the treatment group. By doing so, we have controlled for the difference in X between the participants and non-participants. The potential outcomes of the control and treatment group are now independent of the program selection. The difference in outcome of the control group and the treatment group then can be attributed to the program impact.

However for the matching method to be implemented, we must find a control group that is similar to the treatment group but does not participate in the program. This similarity assumption is called common support. If we denote p(X) as the probability of participating in the program for each subject, i.e. $p(X) = P(D=1\,|\,X)$, the assumption can be stated formally as follows:

Assumption 4.2. $0 < p(X) < 1$ (A.4.2)

Proposition 4.2. Under assumptions (A.4.1) and (A.4.2), ATE$_{(X)}$, ATT$_{(X)}$, ATE and ATT are identified by the matching method.

Proof: the proof is straightforward using the conditional independence assumption.

$$ATE_{(X)} = ATT_{(X)} = E(Y_1 \mid X) - E(Y_0 \mid X) = E(Y_1 \mid X, D=1) - E(Y_0 \mid X, D=0)\,. \qquad (4.20)$$

Both terms in (4.20) can be observed. In addition, assumption (A.4.2) ensures that there are some participants and non-participants whose values of X are similar so that we are able to use sample information to estimate (4.24).

ATE and ATT are identified as in (2.13) and (2.14).

2.4.3.2 Alternative matching methods

Construction of a comparison group

To implement the matching method, we need to find a comparison group for which the conditioning variables are comparable to those of the treatment group. The comparison group is constructed by matching each participant i in the treatment group with one or more non-participants j whose variables X_j are closest to X_i of the participant i. The weighted average outcome of non-participants who are matched with an individual participant i will form the counterfactual outcome for the participant i.

For a participant i, denote n_{ic} as the number of non-participants j who are matched with this participant, and $w(i,j)$ the weight attached to the outcome of each non-participant. These weights are defined non-negative and sum up to 1, i.e.:

$$\sum_{j=1}^{n_{ic}} w(i,j) = 1\,. \qquad (4.21)$$

The estimator of the conditional program parameters is then equal to:

$$\hat{ATE}_{(X)} = \hat{ATT}_{(X)} = \frac{1}{\sum\limits_{i\ with\ X_i = X} D_i} \left\{ \sum_{i\ with\ X_i = X} \left[Y_{1i} - \sum_{j=1}^{n_{ic}} w(i,j) Y_{0j} \right] \right\} \qquad (4.22)$$

where Y_{1i} and Y_{0j} are the observed outcomes of participant i and non-participant j. In practice, when there are many variables X, it is difficult to have a large number of observations that have the same variables X in a sample. Estimates of program impact conditional on a large number of X will be associated with a huge standard error. Thus formula (4.22) should be used only to estimate the program impact for several subgroups defined by one or only a few binary or discrete variable X.

ATT is simply the average of differences in outcome between the treatment and comparison group:

$$\hat{ATT} = \frac{1}{n_1} \sum_{i=1}^{n_1} \left[Y_{1i} - \sum_{j=1}^{n_{ic}} w(i,j) Y_{0j} \right] \qquad (4.23)$$

where n_1 is the number of the participants in the data sample.

To estimate the ATE, we use formula (2.8) in which there remains a component $E(Y_1 \mid D = 0)$ that requires an estimator. A similar matching procedure is applied to estimate this term. Each non-participant is matched with one or more participants who have the closest value of X. Put differently, we can estimate the effect of non-treatment on the non-treated:

$$ANTT = E(Y_1 \mid D=0) - E(Y_0 \mid D=0)$$

using an estimator similar to (4.23):

$$A\hat{NTT} = \frac{1}{n_2}\sum_{j=1}^{n_2}\left[Y_{0j} - \sum_{i=1}^{n_{jt}} w(j,i)Y_{1i}\right] \tag{4.24}$$

where n_2 is the number of the non-participants in the sample. n_{jt} is the number of participants matched with a non-participant j, and $w(j,i)$ are weights attached to each participant i in this matching.

Thus using (2.8) the estimator of ATE is expressed as follows:

$$A\hat{T}E = \frac{1}{n_1 + n_2}\left\{\sum_{i=1}^{n_1}\left[Y_{1i} - \sum_{j=1}^{n_{ic}} w(i,j)Y_{0j}\right] + \sum_{i=1}^{n_2}\left[Y_{0j} - \sum_{i=1}^{n_{jt}} w(j,i)Y_{1i}\right]\right\} \tag{4.25}$$

To this end, there are still two essential issues that have not been discussed. The first is how to select non-participants and participants for matching. The second is how to determine weights $w(i,j)$ among these matched people.

Methods to find a matched sample

Clearly, matched non-participants should have X closest to X of participants. There will be no problem if there is a single conditioning variable X. However X is often a vector of variables, and finding 'close' non-participants to match with a participant is not straightforward. In the literature on impact evaluation, there are three widely-used methods to find matched non-participants for a participant (and vice versa matched participants for a non-participant).

The first method is called subclassification of the treatment and control group based on X (see e.g. Cochran and Chambers, 1965; Cochran, 1968). All participants and non-participants are classified into blocks according to the value of X. This means that subjects in a block have the same value of X. Then non-participants will be matched with participants in each block. However the subclassification becomes difficult when there are many variables X or when some variables of X are continuous or discrete with many values.

The second method is called covariate matching and matches participants with non-participants based on their distance of variables defined on some metric (Rubin, 1979, 1980). Since X can be considered as a vector in a space, the closeness between two sets of X can be defined by a distance metric. A non-participant j will be matched with a participant i if the distance from X_j to X_i is smallest as compared with other non-participants. A quickly emerging metric in space is the traditional Euclidean metric:

$$d_E(i,j) = \left\|X_i - X_j\right\|_E = \left(X_i - X_j\right)'\left(X_i - X_j\right) \tag{4.26}$$

However this metric is sensitive to the measure unit of X. To get a unit-free metric distance, a natural way is to standardize the Euclidean metric by multiplying it with the inversed covariance matrix of X to get the Mahalanobis metric (Rubin, 1979, 1980) or the inversed variance matrix of X (Abadie and Imbens, 2002):[11]

$$d_M(i,j) = \left\|X_i - X_j\right\|_M = \left(X_i - X_j\right)' S_X^{-1}\left(X_i - X_j\right) \tag{4.27}$$

$$d_V(i,j) = \left\|X_i - X_j\right\|_V = \left(X_i - X_j\right)' V_X^{-1}\left(X_i - X_j\right) \tag{4.28}$$

where S_X and V_X are the covariance and variance of X in the sample.

The third way to find the matched sample is the propensity score matching. Since a paper by Rosenbaum and Rubin (1983), matching is often conducted based on the probability of being assigned to the program, which is called the propensity score. Rosenbaum and Rubin

[11] The Mahalanobis metric is presented in Mahalanobis (1936).

(1983) show that if the potential outcomes are independent of the program assignment given *X*, then they are also independent of the program assignment given the balance score. The balance score is any function of *X* but finer than p(*X*), which is the probability of participating in the program (the so-called propensity score).

Proposition 4.3 (Rosenbaum and Rubin, 1983). $Y_0, Y_1 \perp D|X \Rightarrow (Y_0, Y_1) \perp D|b(X)$, where $b(X)$ is any function such that $p(X) = f[b(X)]$ and $p(X) = \Pr(D = 1 | X) = E(D | X)$.

Proof: It is sufficient to show that:

$$P[D = 1 | Y_0, Y_1, b(X)] = P[D = 1 | b(X)]. \tag{4.29}$$

Using the law of iterated expectation and noting that $p(X) = f[b(X)]$, we have the following equations:

$$\begin{aligned} P[D = 1 | Y_0, Y_1, b(X)] &= E[D | Y_0, Y_1, b(X)] \\ &= E\{E[D | Y_0, Y_1, X, b(X)] | Y_0, Y_1, b(X)\} \\ &= E\{E[D | Y_0, Y_1, X] | Y_0, Y_1, b(X)\} \\ &= E\{E[D | X] | Y_0, Y_1, b(X)\} \\ &= E\{p(X) | Y_0, Y_1, b(X)\} \\ &= E\{p(X) | b(X)\} \\ &= P[D = 1 | b(X)]. \end{aligned} \tag{4.30}$$

Using the results of Proposition 4.3, the program impacts can be identified as follows:

$$\begin{aligned} ATE_{(X)} = ATT_{(X)} &= E(Y_1 | X, D = 1) - E(Y_0 | X, D = 0) \\ &= E(Y_1 | b(X), D = 1) - E(Y_0 | b(X), D = 0). \end{aligned} \tag{4.31}$$

In fact, the propensity score is often selected as the balance score in estimating the program impacts. The propensity score can estimated parametrically or non-parametrically by running a regression of the treatment variable *D* on the conditioning variables *X*. Since *D* is a binary variable, a logit or probit model is often used. Once the propensity score is obtained for all subjects in the sample, non-participants can be matched with participants based on the closeness of the propensity scores[12].

Researchers can combine the three above methods - subclassification, covariate matching and propensity score matching - to find the matches (Rosenbaum and Rubin, 1984, 1985a). Subclassification can be performed for certain important variables *X* to ensure that participants and matched non-participants have the same value of these variables.

Weighting methods of matched comparisons

Once a metric distance, *D(i,j)*, between a participant *i* and a non-participant *j* is defined, one can select methods to weight their outcomes. If each participant is matched with the one non-participant with the minimum value of *D(i,j)*, the weight *w(i,j)* equals 1 for all pairs of matches. This is called one nearest neighbour matching. When more than one non-participant

[12] The propensity score can also be used instead of *X* in regressions to estimate program impact (see e.g. Wooldridge, 2001; Rosenbaum and Rubin, 1985a).

is matched with each participant (or vice versa), we need some way to define the weights attached to each non-participant.

A number of methods use equal weights for all matches. N-nearest neighbour matching involves matching each participant with *n* non-participants who have the closest distances *D(i,j)*. Each matched non-participant will receive weight $w(i,j)=1/n$. Caliper matching (see, e.g. Dehejia and Wahba, 1998; Smith and Todd, 2005) uses equal weights for matched subjects whose distance *D(i,j)* is smaller than a specific value, say 0.05 or 0.1. This criterion aims to ensure the quality of matching. Stratification (interval) matching divides the range of estimated distances into several strata (blocks) of equal ranges. Within each stratum, a participant is matched with all non-participants with equal weights (see, e.g. Dehejia and Wahba, 1998; Smith and Todd, 2005).

However, it could be reasonable to assign different weights to different non-participants depending on metric distances between their covariates and the covariates of the matched participant. This argument motivates some other matching schemes such as kernel, local linear matching (see, e.g. Heckman *et al.*, 1997b; Smith and Todd, 2005), and matching using weights of inversed propensity score (see, e.g. Hahn, 1998; Hirano *et al.*, 2003).

The kernel matching method matches a participant with one or many non-participants depending on a kernel function *G* and a selected bandwidth *h*. The weight is defined as:

$$w(i,j)=\frac{G\left[\dfrac{d(i,j)}{h}\right]}{\sum_{k=1}^{n_2} G\left[\dfrac{d(i,k)}{h}\right]}. \tag{4.32}$$

Kernel matching can be explained as kernel (non-parametric) estimation of counterfactual $E(Y_0\,|\,X,D=1)$ using sample information on non-participants. However, the kernel function results in biased estimation if the true regression line is linear. Fan (1992) shows that a so-called method of local linear regression is more flexible and robust in the face of different types of outcome function. This method estimates the regression curve by a series of local linear regression lines. Weights estimated from the local linear regression are as follows (Smith and Todd, 2005):

$$w(i,j)=\frac{G_{ij}\sum_{k=1}^{n_2} G_{ik}\left[d(k,i)\right]^2-\left[G_{ij}d(j,i)\right]\left[\sum_{k=1}^{n_2} G_{ik}d(k,i)\right]}{\sum_{j=1}^{n_2} G_{ij}\sum_{k=1}^{n_2} G_{ik}\left[d(k,i)\right]^2-\left[\sum_{k=1}^{n_2} G_{ik}d(k,i)\right]^2}. \tag{4.33}$$

Finally, Hahn (1998) and Hirano *et al.* (2003) use the inversed propensity scores as weights to estimate the potential outcomes as follows:

$$E(Y_1\,|\,X)=\frac{YD}{p(X)}, \tag{4.34}$$

$$E(Y_0\,|\,X)=\frac{Y(1-D)}{1-p(X)}. \tag{4.35}$$

As a result, the conditional program impact is equal to:

$$ATE_{(X)}=ATT_{(X)}=\frac{Y\left[D-p(X)\right]}{p(X)\left[1-p(X)\right]}. \tag{4.36}$$

Thus, these conditional impacts are estimated by:

$$A\hat{T}E_{(X)} = A\hat{T}T_{(X)} = \sum_{i\ with\ X_i=X} \frac{1}{D_i}\left\{\frac{Y_i[D_i - \hat{p}(X_i)]}{\hat{p}(X_i)[1-\hat{p}(X_i)]}\right\}, \tag{4.37}$$

and the unconditional versions:

$$A\hat{T}E = \frac{1}{n}\sum_{i=1}^{n}\frac{Y_i[D_i - \hat{p}(X_i)]}{\hat{p}(X_i)[1-\hat{p}(X_i)]}, \tag{4.38}$$

$$A\hat{T}T = \frac{1}{n_1}\sum_{i=1}^{n}\frac{Y_i[D_i - \hat{p}(X_i)]}{\hat{p}(X_i)[1-\hat{p}(X_i)]}. \tag{4.39}$$

2.4.3.3 Advantages and disadvantages of matching method

The main advantage of the matching method is that it does not rely on a specific functional form of the outcome, thereby avoiding assumptions on functional form, e.g. linearity imposition, and problems such as multicollinearity and heteroscedasticity. Compared with linear regression, the matching method does not require assumption (A.2.1) about the exogeneity of *X*. In addition, the matching method emphasizes the problem of common support, thereby avoiding the bias due to extrapolation to non-data region. Results from the matching method are easy to explain to policy-makers, since the idea of comparison of similar group is quite intuitive.

However, the matching method has several limitations. It relies on the assumption of conditional mean independence. It also requires the assumption of common support. If this assumption does not hold, one can use a method of regression discontinuity, which will be discussed in the next section. Finally, the matching estimators can work very poorly in small samples if the quality of matches is not good, i.e. participants are matched with non-participants who have very different conditioning variables *X*.[13]

2.4.4 Discontinuity design

For the matching method, the assumption about the common support is required to identify the program impacts. When the conditioning variables *X* are different for participants and non-participants, we cannot implement matching methods. In other words, if there are some variables *X* that predict the treatment variable *D* perfectly, the assumption of common support no longer holds. In Van der Klaauw (2002), it means that there is a conditioning variable *S* belonging to *X* such that *D* equals 1 if and only if *S* is larger than a specific value $\overline{S}$.[14] The assignment of the program is called deterministic. To make this assumption consistent with notation in this chapter, we assume that $D = 1$ if and only if $X \geq \widetilde{X}$. Then we have:

$$P(D=1 \mid X \geq \widetilde{X}) = 1, \tag{4.40}$$

$$P(D=1 \mid X < \widetilde{X}) = 0. \tag{4.41}$$

Which means that the common support assumption $0 < P(D=1 \mid X) < 1$ is not valid.

We know that the regression method does not require a common support. As a result it can be applied in this context taking into account some important notes. Under the assumption about conditional mean independence, the conditional and unconditional program impact parameters are the same because of:

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[13] See e.g. Rosenbaum and Rubin (1985b), and Heckman *et al.* (1998a)

[14] Heckman *et al.* (1999) presents the case in which $D = 1$ only if $S < \overline{S}$. These two cases are similar.

$E(Y_0 \mid X, D = 1) = E(Y_0 \mid X, D = 0)$, (4.42)
$E(Y_1 \mid X, D = 1) = E(Y_1 \mid X, D = 0)$, (4.43)
which can be expressed as follows due to (4.40) and (4.41):
$E(Y_0 | X, X \geq \widetilde{X}) = E(Y_0 | X, X < \widetilde{X})$, (4.44)
$E(Y_1 | X, X \geq \widetilde{X}) = E(Y_1 | X, X < \widetilde{X})$. (4.45)

If the potential outcomes are monotonous (as in the case of linear function with first-order variables *X*), (4.44) and (4.45) are obtained only at the point $X = \widetilde{X}$ under a condition that the potential outcome are continuous at this point. Since the potential outcomes are functions of the error terms, we can state this assumption with respect to the error terms.

Assumption 4.3. The conditional means of the error terms $E(\varepsilon_0 \mid X)$, and $E(\varepsilon_1 \mid X)$ are continuous at $\widetilde{X}$. (A.4.3)

Under assumption (A.4.3) the matching method and other non-parametric estimation methods can be used to estimate the program impacts at the mass of $\widetilde{X}$. This is called local treatment effect at $\widetilde{X}$ (see, e.g.Van der Klaauw, 2002; Hahn *et al.*, 2001).

The parametric approach can identify the program impact at the entire range of *X*. Thus the regression method presented in section 4.2 can be used to estimate the program impact parameters. But we need to assume that the parameters, i.e. $\alpha_0, \beta_0, \alpha_1, \beta_1$, in the potential outcomes are the same in the ranges $X \geq \widetilde{X}$ and $X < \widetilde{X}$. By running a regression of the potential outcomes (or observed outcomes), we use data on outcome of participants Y_1 with $X \geq \widetilde{X}$ to extrapolate the value of potential outcome Y_1 for non-participants with $X < \widetilde{X}$. Similarly, data on the outcome of non-participants Y_0 with $X < \widetilde{X}$ are used to extrapolate the value of potential outcome Y_0 for participants with $X \geq \widetilde{X}$. This method might lead to a so-call extrapolation bias since we predict outcome values in regions of no observations.

2.5 Methods assuming selection on unobservables

As discussed, the main assumption that the methods of selection on observables rely on is the conditional independence between the potential outcomes and program assignment (or a weaker version of conditional mean independence). This assumption does not hold if there is an unobserved variable affecting both the potential outcome and program participation. This section presents three methods that are widely used in dealing with the problem of ‘selection on unobservables’. The methods include instrumental variable regression, sample selection models, and panel data models.

2.5.1 Instrumental variables

2.5.1.1 Program impact identification

If there are unobserved variables affecting both the potential outcome and program participation, the program variable is endogenous in the outcome equation and OLS gives biased estimates. A standard solution to this endogeneity problem is to use one or more instrumental variables for the program assignment variable *D*. An instrumental variable has two properties: (1) it is correlated with program assignment; and (2) it is uncorrelated with the error term in the potential outcomes.[15]

To illustrate how the instrumental variables method identifies program impact, recall Equation 2.19:

$$Y = \alpha_0 + X\beta_0 + D[(\alpha_1 - \alpha_0) + X(\beta_1 - \beta_0)] + [D(\varepsilon_1 - \varepsilon_0) + \varepsilon_0]. \tag{5.1}$$

Unless we can assume conditional mean independence between potential outcome and program assignment, D is endogenous, and we need an instrumental variable for D to estimate program impact.

Assumption 5.1. There is at least an instrumental variable Z such that:

$$\begin{aligned} &Cov(D,Z) \neq 0, \\ &E(\varepsilon_0 \mid Z) = E(\varepsilon_0), \\ &E(\varepsilon_1 \mid Z) = E(\varepsilon_1). \end{aligned} \tag{A.5.1}$$

Proposition 5.1. Under assumptions (A.2.4) and (A.5.1), $ATE_{(X)}$, $ATT_{(X)}$, ATE and ATT are identified and estimated by the instrumental variables method.

Proof:

Firstly we show that:

$$Cov([D(\varepsilon_1 - \varepsilon_0) + \varepsilon_0], Z) = 0. \tag{5.2}$$

Note that $E(\varepsilon_1 - \varepsilon_0 \mid D, Z) = E(\varepsilon_1 - \varepsilon_0 \mid D) = 0$ because of (A.2.4) and (A.5.1), hence:

$$\begin{aligned} Cov([D(\varepsilon_1 - \varepsilon_0) + \varepsilon_0], Z) &= Cov([D(\varepsilon_1 - \varepsilon_0)], Z) + Cov(\varepsilon_0, Z) \\ &= E\{D(\varepsilon_1 - \varepsilon_0) - E[D(\varepsilon_1 - \varepsilon_0)]\}\{Z - E(Z)\} \\ &= E[DZ(\varepsilon_1 - \varepsilon_0)] \\ &= 0. \end{aligned}$$

Similarly, we have:

$$Cov([D(\varepsilon_1 - \varepsilon_0) + \varepsilon_0], X) = 0, \tag{5.3}$$

$$Cov([D(\varepsilon_1 - \varepsilon_0) + \varepsilon_0], XZ) = 0. \tag{5.4}$$

Then we have the following covariance equations due to (5.2), (5.3) and (5.4):

$$\begin{aligned} Cov(Y,Z) &= Cov\{\alpha_0 + X\beta_0 + D[(\alpha_1 - \alpha_0) + X(\beta_1 - \beta_0)] + [D(\varepsilon_1 - \varepsilon_0) + \varepsilon_0], Z\} \\ &= Cov(X,Z)\beta_0 + Cov(D,Z)(\alpha_1 - \alpha_0) + Cov(XD,Z)(\beta_1 - \beta_0), \end{aligned} \tag{5.5}$$

$$\begin{aligned} Cov(Y,X) &= Cov\{\alpha_0 + X\beta_0 + D[(\alpha_1 - \alpha_0) + X(\beta_1 - \beta_0)] + [D(\varepsilon_1 - \varepsilon_0) + \varepsilon_0], X\} \\ &= Var(X)\beta_0 + Cov(D,X)(\alpha_1 - \alpha_0) + Cov(XD,X)(\beta_1 - \beta_0), \end{aligned} \tag{5.6}$$

$$\begin{aligned} Cov(Y,XZ) &= Cov\{\alpha_0 + X\beta_0 + D[(\alpha_1 - \alpha_0) + X(\beta_1 - \beta_0)] + [D(\varepsilon_1 - \varepsilon_0) + \varepsilon_0], XZ\} \\ &= Cov(X,XZ)\beta_0 + Cov(D,XZ)(\alpha_1 - \alpha_0) + Cov(XD,XZ)(\beta_1 - \beta_0) \end{aligned} \tag{5.7}$$

It is obvious that the number of unknown parameters is equal to the number of equations. Thus the parameters in (2.19) are estimated without bias, and so are the conditional and unconditional ATE and ATT.

[15] Examples of instrumental variables can be seen in econometrics textbooks such as Wooldridge (2001), Greene (2003) or papers on review of impact evaluation such as Moffitt (1991).

It should be noted that Equation (2.19) includes the interaction between *X* and *D*. Thus it is considered to include endogenous variables *D* and *XD*, and we use instrumental variables *Z* and *XZ* to solve the endogeneity problem.

While Equation (2.19) imposes equal expectation of the error terms between program and no-program state conditional on *X* for participants (A.2.4), it allows for the program impact to be different across subjects. If we are willing to invoke an assumption on homogenous impact given *X*, i.e. $\varepsilon_0 = \varepsilon_1$, which is stronger than assumption (A.5.1), then (5.1) becomes simpler:

$$Y = \alpha_0 + X\beta_0 + D\left[(\alpha_1 - \alpha_0) + X(\beta_1 - \beta_0)\right] + \varepsilon_0 \,. \tag{5.8}$$

There is no component ε_1 in (5.8), thus the condition $Cov(\varepsilon_1, Z) = 0$ in (A.5.1) can be dropped.

The instrumental variable method is presented above for just-identification, i.e. only one instrumental variable. The case of over-identification in which there are more than one instrumental variable for the treatment variable *D* can be solved easily by applying two-stage least square regression (see, e.g. Wooldridge, 2001).[16]

2.5.1.2 Local average treatment effect

The instrumental variable method presented in the above section is standard. It requires assumption (A.2.4) to identify program impact. Imbens and Angrist (1994) propose another method of instrumental variables that does not rely on assumption (A.2.4) in identifying a so-called local average treatment effect (LATE). The LATE parameter measures the effect of the program on those who change program status due to a change in an instrumental variable *Z*. As *Z* is defined as a policy or a set of policies, one would be interested in the impact of a program on those who are included in the program as a result of policy changes.

To formalize the definition, suppose there is an instrumental variable *Z*, whose value changed from $Z = z_0$ to $Z = z_1$. As a result, there are a number of subjects who changed their status from non-participation to participation in the program. Furthermore, let *D*(z,*X*) denote the treatment variable *D* but be conditional on $\mathrm{Z} = \mathrm{z}$ for subjects with *X*. Then LATE is defined:

$$LATE_{(X, z_0, z_1)} = E\left[Y_1 - Y_0 \mid X, D(z_1, X) - D(z_0, X) = 1\right] \tag{5.9}$$

In addition to the condition of instrumental variables (A.5.1), Imbens and Angrist (1994) impose an additional assumption to identify LATE.

Assumption 5.2. For all *z* and *z'* of *Z*, either $D(z, X) \geq D(z', X)$ or $D(z, X) \leq D(z', X)$ for all subjects. (A.5.2)

In other words, if *D* can be expressed in a latent variable context, in which *D* = 1 if D^* is greater than zero, and otherwise, then D^* is required to be monotonous in *Z*. Once conditional on *X*, any subject should prefer to participate (or quit) the program as the instrument *Z* changes its value from z to z'.

Proposition 5.2 (Imbens and Angrist, 1994)**.** Under assumption (A.5.1) and (A.5.2), LATE is identified as follows:

---

[16] For example, in the first stage the propensity score is estimated using instrumental variables. Then in the second stage, the predicted propensity score is used as an instrumental variable in the outcome equation.

$$LATE_{(X,z_0,z_1)} = E[Y_1 - Y_0 \mid X, D(z_1,X) - D(z_0,X) = 1]$$
$$= \frac{E(Y \mid X, Z = z_1) - E(Y \mid X, Z = z_0)}{P(D=1 \mid X, Z = z_1) - P(D=1 \mid X, Z = z_0)}, \tag{5.10}$$

where Y is the observed outcome, and the denominator is different from zero.

Proof: We have

$$E(Y \mid X, Z = z_0) = E\{(Y_1 D(z_0,X) + [(1 - D(z_0,X)Y_0]] \mid X, Z = z_0\}$$
$$= D(z_0,X)E(Y_1 \mid X) + [1 - D(z_0,X)]E(Y_0 \mid X), \tag{5.11}$$

$$E(Y \mid X, Z = z_1) = E\{(Y_1 D(z_1,X) + [(1 - D(z_1,X)Y_0]] \mid X, Z = z_1\}$$
$$= D(z_1,X)E(Y_1 \mid X) + [1 - D(z_1,X)]E(Y_0 \mid X). \tag{5.12}$$

Subtract (5.11) from (5.12), we get

$$E(Y \mid X, Z = z_1) - E(Y \mid X, Z = z_0)$$
$$= [D(z_1,X) - D(z_0,X)]E(Y_1 - Y_0 \mid X)$$
$$= E[Y_1 - Y_0 \mid X, D(z_1,X) - D(z_0,X) = 1]P[D(z_1,X) - D(z_0,X) = 1] \tag{5.13}$$
$$+ E[Y_1 - Y_0 \mid X, D(z_1,X) - D(z_0,X) = -1]P[D(z_1,X) - D(z_0,X) = -1]$$
$$= E[Y_1 - Y_0 \mid X, D(z_1,X) - D(z_0,X) = 1]P[D(z_1,X) - D(z_0,X) = 1].$$

The last line results from assumption (A.5.2) that there is no person who quits the program due to the change in Z from z_0 to z_1.

Hence:

$$E[Y_1 - Y_0 \mid X, D(z_1,X) - D(z_0,X) = 1] = \frac{E(Y \mid X, Z = z_1) - E(Y \mid X, Z = z_0)}{P[D(z_1,X) - D(z_0,X) = 1]}$$
$$= \frac{E(Y \mid X, Z = z_1) - E(Y \mid X, Z = z_0)}{P(D=1 \mid X, Z = z_1) - P(D=1 \mid X, Z = z_0)}. \tag{5.14}$$

The unconditional LATE is identified by taking the expectation of (5.10) over X. The parameters can be estimated non-parametrically since all variables in (5.10) are observed in sample data.

Finally, it should be noted that Z can be a vector of instrumental variables, and LATE is defined as the program impact on those who participate in the program due to a change in a set of program policies.

2.5.1.3 Advantages and disadvantages of instrumental variable methods

The main advantage of the instrumental variable method is that it allows for the program selection based on unobservables. In addition, LATE can be identified by this method under very general conditions. However, the main problem in this method is to find good instrumental variables. A variable that is correlated with the program selection is often correlated with outcomes and error terms in the potential outcome equations. Using an invalid instrumental variable that does not satisfy the instrument conditions will lead to biased and inconsistent estimates of the program impacts. In contrast, a variable that is uncorrelated with the error terms can be very weakly correlated with the program selection. Estimation with weak instruments can result in large standard errors in small samples. In addition, explanation of this method to policy-makers is not straightforward.

2.5.2 Sample selection models

2.5.2.1 Program impact identification

Impacts of a program can be identified using a sample selection model (Heckman, 1978). Recall that we cannot run regression of the potential outcomes using sample data in the presence of the selection bias because of the non-random missing data. For example, in the equation of Y_0 there is no data on the dependent variable for those who participated in the program. This is similar to the case of the censored dependent variable model, in which the dependent variables is censored according to a selection mechanism. Under assumptions about distribution between the error term in the program selection and the error terms in the potential outcome equations, we can estimate coefficients in the potential outcomes without any bias.

Let's write the impact evaluation model again:
The potential outcomes:
$Y_0 = \alpha_0 + X\beta_0 + \varepsilon_0$,
$Y_1 = \alpha_1 + X\beta_1 + \varepsilon_1$,
and the outcome that we observe is:
$Y = DY_1 + (1-D)Y_0$,
where D is determined by the following framework:
$D^* = \theta W + v$,
$D = 1$ if $D^* > 0$,
$D = 0$ otherwise.
As in (2.19), the equation of the observed outcome is:
$Y = \alpha_0 + X\beta_0 + D[(\alpha_1 - \alpha_0) + X(\beta_1 - \beta_0)] + [D(\varepsilon_1 - \varepsilon_0) + \varepsilon_0]$
$ATE_{(X)}$ and $ATT_{(X)}$ can be estimated without bias if we are able to get unbiased estimators of $(\alpha_1 - \alpha_0)$, and $(\beta_1 - \beta_0)$, and the term, $E(\varepsilon_1 - \varepsilon_0 \mid X, D = 1)$.

If we estimate coefficients in (2.19) directly, the term $[(\varepsilon_1 - \varepsilon_0)D + \varepsilon_0]$ that is correlated with X and D will enter the error term. As a result, the coefficient estimators will be biased due to the endogeneity of X and D. To avoid this problem, we need to model the term $[(\varepsilon_1 - \varepsilon_0)D + \varepsilon_0]$ under an assumption about the relation between error terms $v, \varepsilon_0, \varepsilon_1$.

Assumption 5.3. The error term v in the program participation equation and each of the error terms ε_0, ε_1 in the potential outcome equations follows the following bivariate normal distributions:
$(v, \varepsilon_0) \sim N_2(0,0,1,\sigma_{\varepsilon_0}, \rho_0)$
$(v, \varepsilon_1) \sim N_2(0,0,1,\sigma_{\varepsilon_1}, \rho_1)$ (A.5.3)

To get the unbiased estimators of the conditional parameters, we need an assumption on the exogeneity of X in the potential outcome equations, i.e. assumption (A.2.1).

Proposition 5.3. Under assumptions (A.5.3), $ATE_{(X)}$, $ATT_{(X)}$, ATE and ATT are identified.

Proof: We have the conditional expectation of the observed outcome in Equation (2.19):
$$E(Y \mid X, D) = \alpha_0 + X\beta_0 + D[(\alpha_1 - \alpha_0) + X(\beta_1 - \beta_0)] + E\{D[(\varepsilon_1 - \varepsilon_0)] + \varepsilon_0 \mid X, D\}, \quad (5.15)$$
in which:

$$
\begin{aligned}
&E\{D[(\varepsilon_1-\varepsilon_0)]+\varepsilon_0|X,D\}\\
&\qquad = DE(\varepsilon_1-\varepsilon_0 \mid X,D)+E(\varepsilon_0 \mid X,D)\\
&\qquad = E(\varepsilon_1 \mid X,D=1)P(D=1 \mid X)+E(\varepsilon_0 \mid X,D=0)P(D=0 \mid X)\\
&\qquad = E(\varepsilon_1 \mid X,v>-\theta W)P(D=1 \mid X)+E(\varepsilon_0 \mid X,v\le -\theta W)P(D=0 \mid X)\\
&\qquad = \left\{E(\varepsilon_1 \mid X)+\rho_1\sigma_{\varepsilon_1}\frac{\phi(\theta W)}{\Phi(\theta W)}\right\}P(D=1 \mid X)+\left\{E(\varepsilon_0 \mid X)+\rho_0\sigma_{\varepsilon_0}\frac{-\phi(\theta W)}{1-\Phi(\theta W)}\right\}P(D=0 \mid X)\\
&\qquad = \left[\rho_1\sigma_{\varepsilon_1}\frac{\phi(\theta W)}{\Phi(\theta W)}\right]P(D=1 \mid X)-\left[\rho_0\sigma_{\varepsilon_0}\frac{\varphi(\theta W)}{1-\Phi(\theta W)}\right]P(D=0 \mid X),
\end{aligned}
\tag{5.16}
$$

where the fourth lines results from the definition of the truncated distribution (see, e.g. Greene (2003)). $\phi(.)$ and $\Phi(.)$ are the probability density function and the cumulative probability function of the standard normal distribution, respectively.

Hence (5.15) has the form:

$$
\begin{aligned}
Y &= \alpha_0+X\beta_0+D[(\alpha_1-\alpha_0)+X(\beta_1-\beta_0)]\\
&\quad +\left[\rho_1\sigma_{\varepsilon_1}\frac{\phi(\theta W)}{\Phi(\theta W)}\right]P(D=1 \mid X)-\left[\rho_0\sigma_{\varepsilon_0}\frac{\varphi(\theta W)}{1-\Phi(\theta W)}\right][1-P(D=1 \mid X)]+u,
\end{aligned}
\tag{5.17}
$$

where u is an error term. (5.17) can be estimated by OLS or maximum likelihood methods. Estimates of θ are obtained from estimation of the program selection equation, while $P(D=1 \mid X)$ is the propensity score that can be estimated parametrically or non-parametrically.

To identify ATT$_{(X)}$, we need an estimage of the term $E(\varepsilon_1-\varepsilon_0 \mid X,D=1)$, which is equal to:

$$
\begin{aligned}
E(\varepsilon_1-\varepsilon_0 \mid X,D=1) &= E(\varepsilon_1 \mid X,v>-\theta W)-E(\varepsilon_0 \mid X,v>-\theta W)\\
&= \left(\rho_1\sigma_{\varepsilon_1} - \rho_0\sigma_{\varepsilon_0}\right)\frac{\phi(\theta W)}{\Phi(\theta W)},
\end{aligned}
\tag{5.18}
$$

in which $\rho_1\sigma_{\varepsilon_1}$ and $\rho_0\sigma_{\varepsilon_0}$ are estimated from (5.17).

Although there is no strict requirement on exclusion restriction, i.e. at least an instrumental variable included in *W*, such an instrumental variable should be included in *W* to avoid high multicollearity in (5.17). In addition, if we are able to find instrumental variables in *W*, the expectation of the error terms conditional on *X* and *D* can be estimated semi-parametrically or non-parametrically without assumptions about the bivariate normal distribution of the error terms (see, e.g. Heckman, 1990; Powell, 1994).

2.5.2.2 Advantages and disadvantages

Similar to the method of instrumental variables, the main advantage of the sample selection method is that it allows for selection of a program based on unobservable. In addition, it is robust in the face of heterogeneous impacts of the program. However, the main problem in this method is that it requires the assumption about the functional form of the join distribution of the error terms in the selection equation and the potential outcome equations. In addition, a good instrumental variable is often needed to get efficient estimators of the program impact. However, finding a good instrument is rather difficult. It is also difficult to explain the method to policy-makers as well as the program administrators.

2.5.3 Panel data methods

When longitudinal data or panel data on the participants and non-participants in a program before and after the program implementation are available, we can get unbiased estimators of program impacts which allow for 'selection on unobservables'. Methods discussed here are based on the panel data at two points in time, since this type of data is most widely available. Panel data with many repeated observations are rare in reality. In addition, although there are different types of panel models such as pooled OLS regression, random effects, fixed effects, Hausman-Taylor regressions, and panel models combined with instruments, we discuss two popular methods which can be used to deal with the 'selection on unobservables'. The first is called first-differences regression. The second is the difference-in-differences with matching methods. It should be noted that fixed-effects regression is also a widely-used method to deal with the 'selection on unobservables'. Fixed-effect regression and first-differences regression rely on the same identification assumptions. In this section, we present the first-differences regression model, since it is relatively easier to illustrate the estimation strategy. In addition, the fixed-effects method and the first-differences method give the same estimation results in the context of two-period panel data.

2.5.3.1 First-differences method

To illustrate how the method identifies the program impact, let's write the model of the outcome before the program implementation as follows:

$$Y_{0B} = \alpha_{0B} + X_B\beta_{0B} + \varepsilon_{0B} \tag{5.19}$$

where Y, X, and ε are outcome, conditioning variables, and error term, respectively. But they have the subscripts '0' and 'B' that means 'no program' and 'before the program', respectively. Before the program, all people are in status of no program, and the observed outcome is the outcome in the absence of the program.

After the program, the denotation of the potential outcomes is similar to the case of single cross-section data, but has an additional subscript 'A' that means 'after the program':

$$Y_{0A} = \alpha_{0A} + X_A\beta_{0A} + \varepsilon_{0A} \tag{5.20}$$

$$Y_{1A} = \alpha_{1A} + X_A\beta_{1A} + \varepsilon_{1A} \tag{5.21}$$

Then, the conditional parameters of interest are expressed as follows:

$$ATE_{(X)} = (\alpha_{1A} - \alpha_{0A}) + X_A(\beta_{1A} - \beta_{0A}) + E(\varepsilon_{1A} - \varepsilon_{0A} \mid X_A) \tag{5.22}$$

$$ATT_{(X)} = (\alpha_{1A} - \alpha_{0A}) + X_A(\beta_{1A} - \beta_{0A}) + E(\varepsilon_{1A} - \varepsilon_{0A} \mid X_A, D = 1) \tag{5.23}$$

The key assumption in the first-differences method is that the error term includes a time-invariant component and any correlation between D and the error is included in this component. The time-invariant component can be called the fixed and unobserved effect.

Assumption 5.4. Error terms in the potential outcome equations are decomposed into components with the following properties:

$\varepsilon_{0B} = \pi + \eta_{0B}$,

$\varepsilon_{0A} = \pi + \eta_{0A}$,

$\varepsilon_{1A} = \pi + \eta_{1A}$,

where:

$\eta_{0B}, \eta_{0A}, \eta_{1A} \perp D \mid X_B, X_A$ (A.5.4)[17]

For identification of the program impact, we require a weaker assumption, in which the assumption (A.5.4) is stated in terms of expectation of errors.

Assumption 5.4'. Error terms in the potential outcome equations are decomposed to components with the following properties:

$$E(\varepsilon_{0B} \mid X_{BA}, D) = E(\pi \mid X_{BA}, D) + E(\eta_{0B} \mid X_{BA}, D) = E(\pi \mid X_{BA}, D) + E(\eta_{0B} \mid X_{BA}) \quad (5.24)$$

$$E(\varepsilon_{0A} \mid X_{BA}, D) = E(\pi \mid X_{BA}, D) + E(\eta_{0A} \mid X_{BA}, D) = E(\pi \mid X_{BA}, D) + E(\eta_{0A} \mid X_{BA}) \quad (5.25)$$

$$E(\varepsilon_{1A} \mid X_{BA}, D) = E(\pi \mid X_{BA}, D) + E(\eta_{1A} \mid X_{BA}, D) = E(\pi \mid X_{BA}, D) + E(\eta_{1A} \mid X_{BA}) \quad (5.26)$$

where π is a component with the expectation unchanged over time for the state of no program. η is a component that is allowed to change over time, but its expectation is independent of D given the variables, $X_{BA} = \{X_B, X_A\}$. (A.5.4')

This assumption holds if the time-variant component of the error terms is independent of the program selection. However, assumption (A.5.4') requires only the conditional mean independence of this component with respect to the program selection.

In addition, to identify ATE$_{(X)}$ and ATT$_{(X)}$, we need assumptions on exogeneity of X, i.e. an assumption similar to (A.2.1):

Assumption 5.5. $E(\varepsilon_{0B} \mid X_B, X_A) = E(\varepsilon_{0A} \mid X_B, X_A) = E(\varepsilon_{1A} \mid X_B, X_A) = 0$ (A.5.5)

Proposition 5.4. Under assumptions (A.5.4) and (A.5.7), ATE$_{(X)}$, ATT$_{(X)}$, ATE and ATT are identified and can be estimated by OLS regression.

Proof:

Firstly, under assumption (A.5.4) and (A.5.5), ATE$_{(X)}$ and ATT$_{(X)}$ are identified and the same, since:

$$E(\varepsilon_{1A} - \varepsilon_{0A} \mid X_A) = 0,$$

$$\begin{aligned} E(\varepsilon_{1A} - \varepsilon_{0A} \mid X_B, X_A, D=1) &= E(\eta_{1A} - \eta_{0A} \mid X_B, X_A, D=1) \\ &= E(\eta_{1A} - \eta_{0A} \mid X_B, X_A) \\ &= E(\varepsilon_{1A} - \varepsilon_{0A} \mid X_B, X_A) \\ &= 0, \end{aligned}$$

As a result, $E(\varepsilon_{1A} - \varepsilon_{0A} \mid X_A, D=1) = 0$.

The estimator of ATE$_{(X)}$ and ATT$_{(X)}$ is the coefficient of D in the following equation:

$$Y_A = \alpha_{0A} + X_A\beta_{0A} + D[(\alpha_{1A} - \alpha_{0A}) + X_A(\beta_{1A} - \beta_{0A})] + [D(\varepsilon_{1A} - \varepsilon_{0A}) + \varepsilon_{0A}], \quad (5.27)$$

To estimate $(\alpha_{1A} - \alpha_{0A})$ and $(\beta_{1A} - \beta_{0A})$, subtract (5.19) from (5.25) to obtain:

$$\begin{aligned} Y_A - Y_{0B} = {} & (\alpha_{0A} - \alpha_{0B}) + (X_A\beta_{0A} - X_B\beta_{0B}) + D[(\alpha_{1A} - \alpha_{0A}) + X_A(\beta_{1A} - \beta_{0A})] \\ & + [D(\varepsilon_{1A} - \varepsilon_{0A}) + (\varepsilon_{0A} - \varepsilon_{0B})], \end{aligned} \quad (5.28)$$

in which the error term has the traditional property due to the (A.5.4) and (A.5.5):

[17] In some econometrics text, $\eta_{0B}, \eta_{0A}, \eta_{1A} \perp D \mid X_B, X_A$ is called strict exogeneity condition.

$$
\begin{aligned}
&E\{[D(\varepsilon_{1A}-\varepsilon_{0A})+(\varepsilon_{0A}-\varepsilon_{0B})]X_B, X_A, D\}\\
&= DE(\varepsilon_{1A}-\varepsilon_{0A} \mid X_{BA}, D)+E(\varepsilon_{0A}-\varepsilon_{0B} \mid X_{BA}, D)\\
&= DE(\eta_{1A}-\eta_{0A} \mid X_{BA}, D)+E(\eta_{0A}-\eta_{0B} \mid X_{BA}, D)\\
&= DE(\eta_{1A}-\eta_{0A} \mid X_{BA})+E(\eta_{0A}-\eta_{0B} \mid X_{BA})\\
&= 0
\end{aligned}
\tag{5.29}
$$

Thus, we can estimate all coefficients in (5.28) (also in (5.27)) without bias by running regression of the difference in observed outcome before and after the program on X_B and X_A, and the program selection variable D. Then, the estimates of these coefficients will be used to estimate the conditional and unconditional parameters of the program impact.

2.5.3.2 Difference-in-differences with matching method

The method of difference-in-differences with matching can be regarded as a non-parametric version of the first-differences method. It allows the program selection to be based on unobservable variables in the sense that it does not require the conditional independence assumption (A.4.1). It allows for bias in using the conditional expectation of outcome of non-participants to predict the conditional expectation of outcome of participants if they had not participated in the program. However, it requires the bias to be time-invariant. Compared with the first-differences method, it has the advantage of requiring the assumption about the exogeneity of X to identify the program impact parameters.

Proposition 5.4. Under assumptions (A.5.4), $\text{ATE}_{(X)}$, $\text{ATT}_{(X)}$, ATE and ATT are identified and can be estimated non-parametrically by the matching method.

Proof:
From (A.5.4), we get:

$$
\begin{aligned}
E(\varepsilon_{0A}-\varepsilon_{0B} \mid X_B, X_A, D) &= E(\eta_{0A}-\eta_{0B} \mid X_B, X_A, D)\\
&= E(\eta_{0A}-\eta_{0B} \mid X_{BA})\\
&= E(\varepsilon_{0A}-\varepsilon_{0B} \mid X_{BA}),
\end{aligned}
\tag{5.30}
$$

where X_{BA} denote all X_B and X_A. Thus, $E(\varepsilon_{0A}-\varepsilon_{0B})$ is independent of D given X_B and X_A before and after the program. As a result:

$$
E(\varepsilon_{0A}-\varepsilon_{0B} \mid X_{BA}, D=0) = E(\varepsilon_{0A}-\varepsilon_{0B} \mid X_{BA}, D=1) \tag{5.31}
$$

$$
\Leftrightarrow E(\varepsilon_{0A} \mid X_{BA}, D=0) - E(\varepsilon_{0B} \mid X_{BA}, D=0) = E(\varepsilon_{0A} \mid X_{BA}, D=1) - E(\varepsilon_{0B} \mid X_{BA}, D=1)
$$

$$
\Leftrightarrow E(Y_{0A} \mid X_{BA}, D=0) - E(Y_{0B} \mid X_{BA}, D=0) = E(Y_{0A} \mid X_{BA}, D=1) - E(Y_{0B} \mid X_{BA}, D=1) \tag{5.32}
$$

Recall that $\text{ATT}_{(X)}$ is equal to:

$$
ATT_{(X_B,X_A)} = E(Y_{1A} \mid X_{BA}, D=1) - E(Y_{0A} \mid X_{BA}, D=1). \tag{5.33}
$$

Insert (5.32) into (5.33) to obtain:

$$
\begin{aligned}
\text{ATT}_{(X_B,X_A)} = {} & \text{E}(\text{Y}_{1A} \mid \text{X}_{BA}, \text{D}=1) - \text{E}(\text{Y}_{0A} \mid \text{X}_{BA}, \text{D}=1) - \\
& [E(Y_{0A} \mid X_{BA}, D=0) - E(Y_{0B} \mid X_{BA}, D=0)] + [E(Y_{0A} \mid X_{BA}, D=1) - E(Y_{0B} \mid X_{BA}, D=1)]\\
= {} & [\text{E}(\text{Y}_{1A} \mid \text{X}_{BA}, \text{D}=1) - E(Y_{0A} \mid X_{BA}, D=0)] - [E(Y_{0B} \mid X_{BA}, D=1) - E(Y_{0B} \mid X_{BA}, D=0)]
\end{aligned}
\tag{5.34}
$$

Similarly, we can identify the conditional average effect of non-treatment on the non-treated (ANTT):

$$
\begin{aligned}
\mathrm{ANTT}_{(X_B,X_A)} = {} & \mathrm{E}(\mathrm{Y}_{1A} \mid \mathrm{X}_{BA}, \mathrm{D}=0) - \mathrm{E}(\mathrm{Y}_{0A} \mid \mathrm{X}_{BA}, \mathrm{D}=0) - \\
& \left[E(Y_{1A} \mid X_{BA}, D=0) - E(Y_{0B} \mid X_{BA}, D=0)\right] + \left[E(Y_{1A} \mid X_{BA}, D=1) - E(Y_{0B} \mid X_{BA}, D=1)\right] \\
= {} & \left[\mathrm{E}(\mathrm{Y}_{1A} \mid \mathrm{X}_{BA}, \mathrm{D}=1) - E(Y_{0A} \mid X_{BA}, D=0)\right] - \left[E(Y_{0B} \mid X_{BA}, D=1) - E(Y_{0B} \mid X_{BA}, D=0)\right],
\end{aligned} \tag{5.35}
$$

which is the same as $\mathrm{ATT}_{(X)}$. As a result, $\mathrm{ATE}_{(X)}$ is identified, and it is equal to $\mathrm{ATT}_{(X)}$.
The unconditional parameters are also identified due to (2.13) and (2.14).

The method of matching in this context is similar to what is described in section (4.2). However, as (5.35) indicates, a participant is matched with a non-participant based on their conditioning variables before and after the program, X_B and X_A.

The above matching method requires panel data. If only independently pooled cross section data are available, the matching will be performed in a slightly different way. The identification assumption is revised as follows.

Assumption 5.6. The difference in the conditional expectation of outcomes before and after program is the same for the participant and non-participants, i.e.:

$$
\begin{aligned}
&\left[E(Y_{0A} \mid X_A, D=1) - E(Y_{0B} \mid X_B, D=1)\right] = \left[E(Y_{0A} \mid X_A, D=0) - E(Y_{0B} \mid X_B, D=0)\right] \\
&\left[E(Y_{1A} \mid X_A, D=1) - E(Y_{0B} \mid X_B, D=1)\right] = \left[E(Y_{1A} \mid X_A, D=0) - E(Y_{0B} \mid X_B, D=0)\right]
\end{aligned} \tag{A.5.6}
$$

(A.5.6) is different from (A.5.4). For example, in condition (5.32) which results from assumption (A.5.4), all expectation terms include both X_B and X_A, while in the first equation of (A.5.6) the expectation terms include either X_B or X_A.

There is no argument for whether assumption (A.5.6) is stronger than (5.32) or *vice versa*.

Then, under this assumption (A.5.6), the $\mathrm{ATT}_{(X)}$ is equal to:

$$
\begin{aligned}
\mathrm{ATT}_{(X_A)} = {} & \mathrm{E}(\mathrm{Y}_{1A} \mid \mathrm{X}_A, \mathrm{D}=1) - \mathrm{E}(\mathrm{Y}_{0A} \mid \mathrm{X}_A, \mathrm{D}=1) \\
& - \left[E(Y_{0A} \mid X_A, D=0) - E(Y_{0B} \mid X_B, D=0)\right] + \left[E(Y_{0A} \mid X_A, D=1) - E(Y_{0B} \mid X_B, D=1)\right] \\
= {} & \left[\mathrm{E}(\mathrm{Y}_{1A} \mid \mathrm{X}_A, \mathrm{D}=1) - E(Y_{0A} \mid X_A, D=0)\right] - \left[E(Y_{0B} \mid X_B, D=1) - E(Y_{0B} \mid X_B, D=0)\right].
\end{aligned} \tag{5.36}
$$

In implementation, firstly participants are matched to non-participants based on X_B to estimate the difference in their outcome before the program. Secondly, after the program participants are matched to non-participants again but based on X_A to estimate the difference in their outcome. Then, the estimate of the program impact $\mathrm{ATT}_{(X)}$ is equal to the difference in the estimates before and after the program. That is why this method is also called double-matching.

Note that the term $\left[E(Y_{0A} \mid X, D=1) - E(Y_{0A} \mid X, D=0)\right]$ in (A.5.6) is set equal to zero in conditional independence assumption (A.4.1). This is bias when the conditional expectation of outcome of non-participants is used to predict the conditional expectation of outcome of participants if they had not participated in the program. The matching method using single cross-section data assumes this bias equals zero once conditional on X. Thus, the difference-in-difference matching method is more robust than the cross-section matching method in the sense that it allows this bias to differ from zero. However, it requires that this bias be time-invariant.

Similarly, under the second condition of (A.5.6), $\mathrm{ANTT}_{(X)}$ is identified. It is the same as $\mathrm{ATT}_{(X)}$. As a result $\mathrm{ATE}_{(X)}$ is also the same as $\mathrm{ATT}_{(X)}$.

Based on (5.36), we can have the difference-in-differences with matching estimator of ATT as follows:

$$A\hat{T}T = \frac{1}{n_1}\left\{\sum_{i=1}^{n_1}\left[Y_{1Ai} - \sum_{j=1}^{n_{ic}} w(i,j)Y_{0Aj}\right] - \sum_{i=1}^{n_1}\left[Y_{0Bi} - \sum_{j=1}^{n_{ic}} w(i,j)Y_{0Bj}\right]\right\} \tag{5.37}$$

where n_1 is the number of the participants in the data sample. Y_{1Ai} and Y_{0Aj} are the observed outcomes of participant *i* and matched non-participant *j* after the program, respectively. Y_{0i}^{B} and Y_{0j}^{B} are the observed outcomes of participant *i* and matched non-participant *j* before the program, respectively. n_{ic} is the number of non-participants *j* who are matched with this participant, and *w(i,j)* the weight attached to the outcome of each non-participant.[18]

2.5.3.3 Advantages and disadvantages of the panel data methods

The main advantage of the panel data methods is that they allow for the program selection based on some unobservable variables. However, the methods have two disadvantages. The first is the requirement of the data set. Panel data that are collected before and after the program are not widely available as single cross-section data. The second is that the methods require two assumptions to identify $\text{ATE}_{(X)}$ and $\text{ATT}_{(X)}$. The assumptions require that unobservable variables that affect program selection are unchanged over time and unaffected by program status. These assumptions might be violated if the time period between two panel data sets is relatively long. In addition, the unobservable variables can be changed as subjects participate in the program.

2.6 Conclusions

The main issue in impact evaluation is missing data. We cannot observe subjects at the same time in both statuses: participation in a program and non-participation in the program. Unless the program is randomized, the missing data is not random. Subjects are selected in the program based on their decisions and program administrators' decisions. Different methods in impact evaluation rely on different assumptions about the relation between the outcome process and the program selection process to construct the counterfactual so that the program impacts are identified. The chapter discusses alternative methods in terms of identification assumptions and estimation strategies in contexts of the two potential outcome equations and program selection equations with the allowance for heterogeneous program impacts.

The selection of a method to evaluate the impact of a specific program depends mainly on the availability of the budget for impact evaluation and data sources. Ideally, impact evaluation based on randomization produces the most reliable results. However, randomization is often costly and not easily conducted, especially for targeted programs. Non-randomized or non-experimental evaluation methods tend to be less costly, especially when available data can be used (there is no additional data collection). When the program selection is based on 'observables', simple impact evaluation methods such as regression and matching can be used. When the program selection is based on 'unobservables', instrumental variables regression is a reliable method provided that valid instruments are found. However, finding the instruments is not an easy task. If the instruments are lacking and panel data on the participants and non-participants are available, one can use fixed-effects regression or

[18] These weights are defined non-negative and sum up to 1, i.e.: $\sum_{j=1}^{n_{ic}} w(i,j) = 1$.

difference-in-differences methods which are robust in the face of the program selection based on time-invariant unobservables.

Finally, the measurement of program impact is often very complicated. In reality, the treatment variable *D* can be continuous instead of binary. In addition to the program to be assessed, there might be many other programs that can affect the participants and non-participants of the program in question. Unless the program selection of others programs is uncorrelated with the selection program of the program to be assessed, the omission of other contemporaneous programs can lead to serious bias. Furthermore, subjects can participate in a program, e.g. training program or micro-credit program several times. Even if they are allowed to participate in a program one time, they can join the program at different points in time. However, data on subjects' outcomes are often collected at the same point in time. Ignorance of these issues can mean that the results from impact evaluation are misinterpreted. All of these issues require further study to improve the literature on program impact evaluation.

Chapter 3 Impact evaluation of multiple overlapping programs under a conditional independence assumption

3.1 Introduction

The main objective of impact evaluation is to assess the extent to which a program has changed the outcomes of subjects.[19] The average impact of a program on a group of subjects is defined as the difference between their outcome in the status of the program and their outcome in the status of no-program. However, for each subject, we are not able to observe the two potential outcomes at the same time. For example, for a participant in a program, we can observe her outcome in the presence of the program, but we cannot observe her outcome if she had not participated in the program, i.e. the outcome in the absence of the program. This missing data problem can be solved if the assumption of conditional independence of treatment and potential outcomes holds true (Rubin, 1977). Under this assumption, the program impact can be estimated by traditional cross-section regression and matching methods. The idea of the matching method is to compare the outcomes of participants and non-participants who have the similar distribution of conditioning pre-treatment variables.

Matching by conditioning variables becomes difficult when there are a large number of these variables. Rosenbaum and Rubin (1983) prove that the program impact can be identified conditional on the probability of being assigned to the program (the so-called propensity score). Thus, multidimensional matching can be achieved by matching based on the propensity score instead of the conditioning variables.

The literature on program impact evaluation often neglects other programs that simultaneously impact on participants and non-participants of the program in question. Imbens (1999) and Lechner (2001) extend the method of the propensity score matching (PSM) to multiple mutually exclusive programs. Frölich (2002) discusses different impact evaluation methods including those based on the conditional independence assumption (CIA) in a similar context. However, in reality the programs are often overlapping. Some people can join several programs at the same time. For example, for evaluation of a micro-credit program that is provided by a bank, the participants and non-participants in the program can receive credit from other sources such as private lenders, relatives and other credit institutions. Without taking into account the impacts of the other programs, the estimation of the impact of the program of interest can be biased.

This chapter discusses the CIA-based methods consisting of cross-section regression and PSM in this more general context in which people may participate in several programs simultaneously. It is shown that the impact of a particular program can be identified and estimated using the methods of cross-section regression and PSM. Under the matching method, the impact of a program can be measured as a weighted average of impacts of the program on groups with various program statuses. Evidence from Monte Carlo simulation shows that this matching method can lead to lower mean-squared-error (MSE) than matching that simply uses variables of participation in other programs as conditioning variables.

The chapter is organized as follows. The second section discusses the methods of cross-section regression and PSM in impact evaluation of a single program. The third section

[19] In the literature on impact evaluation, a broader term ‘treatment’ instead of program/project is sometimes used to refer to an intervention whose impact is evaluated.

extends the methods to the case of multiple overlapping programs. Simulation results are presented in the fourth section. Finally, the fifth section draws conclusions.

3.2 Impact evaluation of a single program

3.2.1 Problems and parameters of interest

Suppose that some people in population P are assigned to a program, and denote D as a binary variable for participation in the program, i.e. D equals 1 if one participates in the program, and D equals 0 otherwise. Further let Y_0 and Y_1 denote the potential outcomes corresponding to the states of program and no-program.[20]
The impact of the program on the outcome of person i is measured by the following difference:

$$\Delta_i = Y_{1i} - Y_{0i} \tag{2.1}$$

This is the difference between the outcome of the person when she participates in the program and the potential outcome of that person when she does not participate in the program. The problem is that we cannot observe both outcomes in equation (2.1) for one person. The unobservable outcome is called counterfactual.

It is almost impossible to estimate the program impact for each person (Heckman *et al.*, 1999), since we never know the counterfactual outcome. However, an average program impact can be estimated for a group of subjects. Two parameters that are most popular are the Average Treatment Effect (ATE), and the Average Treatment Effect on the Treated (ATT)[21].

ATE is the expected impact of the program on a person who is randomly selected and assigned into the program. It is defined as:[22]

$$ATE = E(\Delta) = E(Y_1 - Y_0) = E(Y_1) - E(Y_0)\,. \tag{2.2}$$

ATT is the expected impact of the program on the actual participants:

$$ATT = E(\Delta \mid D=1) = E(Y_1 - Y_0 | D=1) = E(Y_1 | D=1) - E(Y_0 | D=1)\,. \tag{2.3}$$

More generally, we can allow these parameters to vary across observed variables X, since one might be interested in program impact on certain groups that are specified by the X variables:

$$ATE_{(X)} = E(\Delta \mid X) = E(Y_1 \mid X) - E(Y_0 \mid X)\,, \tag{2.4}$$

and

$$ATT_{(X)} = E(\Delta \mid X, D=1) = E(Y_1 \mid X, D=1) - E(Y_0 \mid X, D=1)\,. \tag{2.5}$$

Estimation of ATE and ATT is not straightforward, since there are some components that we cannot observe directly. Equation (2.2) can be rewritten as:

$$\begin{aligned} ATE = E(Y_1) - E(Y_0) = &\{[E(Y_1 \mid D=1) - E(Y_0 \mid D=1)]\Pr(D=1)\} \\ &+ \{[E(Y_1 \mid D=0) - E(Y_0 \mid D=0)]\Pr(D=0)\}, \end{aligned} \tag{2.6}$$

where Pr($D=1$) and Pr($D=0$) are proportions of participants and non-participants in the program, respectively. The first term in (2.6) is the very parameter ATT multiplied by the proportion of the participants, while the second term is the Average Treatment Effect of the

---

[20] Y_0 and Y_1 can be vectors of outcomes, but for simplicity let's consider a single outcome of interest.

[21] There are other parameters such as local average treatment effect, marginal treatment effect, or even effect of 'treatment on non-treated' which measures what impact the program would have on the non-participants if they had participated in the program, etc.

[22] For simplicity, subscript i is dropped in some formulas.

Non-Treated (ATNT) multiplied by the proportion of the non-participants, which measures the effect that the non-participants would have gained if they had participated in the program:
$ATNT = E(Y_1 \mid D = 0) - E(Y_0 \mid D = 0)$. (2.7)

The problem with measuring ATE and ATT is that the counterfactual terms $E(Y_1 \mid D = 0)$ and $E(Y_0 \mid D = 1)$ are not observed and cannot be estimated directly. Different methods have been devised to estimate ATE and ATT under certain assumptions about how the program is assigned to people in the population and how the outcomes are determined. This chapter will discuss methods of regression and matching which rely on the conditional independence assumption (CIA).

3.2.2 Impact evaluation under the conditional independence assumption

A popular way to discuss program impact evaluation is to use a model of potential outcome equations (Heckman *et al.*, 1999; Heckman, 2005), in which the potential outcomes Y_0 and Y_1 are expressed as functions of conditioning variables, *X*:
$Y_0 = \alpha_0 + X\beta_0 + \varepsilon_0$, (2.8)
$Y_1 = \alpha_1 + X\beta_1 + \varepsilon_1$. (2.9)

In fact, Y_0 and Y_1 can be any functions of *X*, not necessarily linearly or parametrically specified, and all identification strategies presented in this chapter are still valid when Y_0 and Y_1 are non-linear functions of *X*. The assumption about the linear function is made for simplicity of the description of the regression methods. For the matching method, there is no assumption imposed on the functional form of the outcomes.

Substituting (2.8) and (2.9) into (2.4) and (2.5), we get the conditional parameters, $ATE_{(X)}$ and $ATT_{(X)}$:
$ATE_{(X)} = (\alpha_1 - \alpha_0) + X(\beta_1 - \beta_0) + E(\varepsilon_1 - \varepsilon_0 \mid X)$, (2.10)
$ATT_{(X)} = (\alpha_1 - \alpha_0) + X(\beta_1 - \beta_0) + E(\varepsilon_1 - \varepsilon_0 \mid X, D = 1)$. (2.11)
Without additional assumptions, (2.10) and (2.11) cannot be identified since they contain the unobserved terms, $E(\varepsilon_1 - \varepsilon_0 \mid X)$ and $E(\varepsilon_1 - \varepsilon_0 \mid X, D = 1)$. The key assumption to identify the parameters using cross-section regressions or matching methods is the conditional independence assumption.

Assumption 2.1 (CIA). $Y_0, Y_1 \perp D \big| X$ (A.2.1)
The assumption states that once conditioned on the variables *X*, the potential outcomes Y_0 and Y_1 are independent of the program assignment. In Rosenbaum and Rubin (1983), this assumption is called ignorability of treatment or conditional independence.[23]

To estimate the conditional parameters using the regression method, it is required that *X* be exogenous in the potential outcome equations:

[23] The assumption is sometimes stated in a weaker version called the conditional mean independence assumption, i.e. $E(Y_0 \mid X, D) = E(Y_0 \mid X)$ and $E(Y_1 \mid X, D) = E(Y_1 \mid X)$. In addition, if the parameter of interest is ATT, the required assumption is $E(Y_0 \mid X, D) = E(Y_0 \mid X)$ (or we only need $Y_0 \perp D \big| X$ instead of $Y_0, Y_1 \perp D \big| X$). In the paper we often mention the CIA, since the conditional mean independence assumptions which involve the expectation terms are a bit abstract and difficult to interpret.

Assumption 2.2. $E(\varepsilon_0 \mid X) = E(\varepsilon_1 \mid X) = 0$ (A.2.2)
Under assumptions A.2.1 and A.2.2, the unobserved terms in the parameters $ATE_{(X)}$ and $ATT_{(X)}$ vanish:

$$E(\varepsilon_1 - \varepsilon_0 \mid X, D=1) = E(\varepsilon_1 - \varepsilon_0 \mid X) = E(\varepsilon_1 \mid X) - E(\varepsilon_0 \mid X) = 0 . \quad (2.12)$$

The two parameters $ATE_{(X)}$ and $ATT_{(X)}$ are the same, and they can be estimated without bias if coefficients $\alpha_0, \alpha_1, \beta_0, \beta_1$ are estimated. It should be noted that the observed outcome equation can be written using a switching model (Quandt, 1972) as follows:

$$Y = DY_1 + (1-D)Y_0 = \alpha_0 + X\beta_0 + D[(\alpha_1 - \alpha_0) + X(\beta_1 - \beta_0)] + [(\varepsilon_1 - \varepsilon_0)D + \varepsilon_0]. \quad (2.13)$$

The coefficients in (2.13) can be estimated without bias using OLS, since the error term has the conventional property

$$E[D(\varepsilon_1 - \varepsilon_0) + \varepsilon_0 \mid X, D] = DE(\varepsilon_1 - \varepsilon_0 \mid X, D) + E(\varepsilon_0 \mid X, D) = 0 . \quad (2.14)$$

Matching is a non-parametric method to estimate $ATE_{(X)}$ and $ATT_{(X)}$ under the CIA. We have:

$$ATE_{(X)} = E(Y_1 \mid X) - E(Y_0 \mid X) = E(Y_1 \mid X, D=1) - E(Y_0 \mid X, D=0) , \quad (2.15)$$

$$ATT_{(X)} = E(Y_1 \mid X, D=1) - E(Y_0 \mid X, D=1) = E(Y_1 \mid X, D=1) - E(Y_0 \mid X, D=0) . \quad (2.16)$$

As a result, $ATE_{(X)}$ and $ATT_{(X)}$ are the same, and they can be estimated by comparing the outcome of the participants and the outcome of a so-called comparison group, which comprises subjects who do not participate in the program but have variables *X* identical to those of the participants. Thus the matching method assumes the existence of such a comparison group. This assumption is called common support. Let *p(X)* denote the propensity score, the conditional probability of participating in the program, given the pre-treatment variables *X*. Then, the common support assumption can be stated formally as follows:

Assumption 2.3. $0 < p(X) = P(D=1 \mid X) < 1$ (A.2.3)
Compared with the parametric regression, it relaxes assumption on exogeneity of *X*, A.2.2 at the cost of the assumption on common support, A.2.3.

The difficulty in the matching method is to how find matched non-participants for the participants when there are many variables *X*. A popular solution is proposed by Rosenbaum and Rubin (1983) who show that if the potential outcomes are independent of the program assignment given the variables *X*, then they are also independent of the program assignment given the propensity score.[24]

Proposition 2.1 (Rosenbaum and Rubin, 1983).
$Y_0, Y_1 \perp D | X \Rightarrow (Y_0, Y_1) \perp D | p(X)$,
where $p(X) = \Pr(D=1 \mid X) = E(D \mid X)$.

Using this proposition, $ATE_{(X)}$ and $ATT_{(X)}$ are rewritten as:

$$ATE_{(X)} = ATT_{(X)} = E(Y_1 \mid p(X), D=1) - E(Y_0 \mid p(X), D=0) . \quad (2.17)$$

Thus non-participants are matched with the participants based on the propensity score. Once the comparison is constructed, the parameters of program impact can be estimated by comparing the outcome of the comparison and treatment groups.

Finally, the unconditional parameters, *ATE* and *ATT*, can be identified and estimated by simply taking the expectation of the conditional parameters, $ATE_{(X)}$ and $ATT_{(X)}$, since:

---

[24] Other matching methods are subclassification (Cochran and Chambers, 1965) and (Cochran, 1968), and covariate matching (Rubin, 1979, 1980).

$$ATE = E_X\left(ATE_{(X)}\right) = \int_X ATE_{(X)} dF(X), \tag{2.18}$$

$$ATT = E_{X|D=1}\left(ATT_{(X)}\right) = \int_{X|D=1} ATT_{(X)} dF(X \mid D=1), \tag{2.19}$$

where $F()$ and $F(|D=1)$ are the distribution functions of X and $X|D=1$, respectively.

3.3 Impact evaluation in multiple correlated programs under the conditional independence assumption

For an illustration of the ideas, this section discusses impact evaluation in the case of two programs under the CIA. Estimation of program impacts in the case of multiple programs is very similar and presented in Appendix 3.2.

3.3.1 Parameters of interest

Suppose that there are two programs that are assigned to some people in the population. Denote D as a vector variable of program participation for a person. D contains two binary variable elements: d_1 and d_2, i.e.

$$D = \begin{pmatrix} d_1 \\ d_2 \end{pmatrix}.$$

where $d_1 = 1$ if the person receives program 1, and $d_1 = 0$ otherwise; similarly $d_2 = 1$ if the person receives program 2, and $d_2 = 0$ otherwise. As a result, the set of the potential treatments has 4 values:

$$\Omega_D = \left\{ \begin{pmatrix} 1 \\ 1 \end{pmatrix}; \begin{pmatrix} 1 \\ 0 \end{pmatrix}; \begin{pmatrix} 0 \\ 1 \end{pmatrix}; \begin{pmatrix} 0 \\ 0 \end{pmatrix} \right\}. \tag{3.1}$$

Further, let Y denote the observed value of an outcome of interest. This variable equals one of the potential outcomes in $\Omega_{Y_D^P} = \{Y_{11}; Y_{10}; Y_{01}; Y_{00}\}$. These potential outcomes correspond to the values of the participation variable D. The potential outcome Y_D^P can be written as a function of observed variables X and unobserved variables ε:

$$Y_{11} = \alpha_{11} + X\beta_{11} + \varepsilon_{11},$$
$$Y_{10} = \alpha_{10} + X\beta_{10} + \varepsilon_{10},$$
$$Y_{01} = \alpha_{01} + X\beta_{01} + \varepsilon_{01},$$
$$Y_{00} = \alpha_{00} + X\beta_{00} + \varepsilon_{00}.$$[25]

The observed outcome can be written in terms of the potential outcomes as follows:

$$\begin{aligned} Y &= d_1 d_2 Y_{11} + d_1(1-d_2)Y_{10} + (1-d_1)d_2 Y_{01} + (1-d_1)(1-d_2)Y_{00} \\ &= d_1 d_2 (Y_{11} - Y_{10} - Y_{01} + Y_{00}) + d_1(Y_{10} - Y_{00}) + d_2(Y_{01} - Y_{00}) + Y_{00} \\ &= d_1 d_2(\alpha_{12} + X\beta_{12} + \varepsilon_{12}) + d_1(\alpha_1 + X\beta_1 + \varepsilon_1) + d_2(\alpha_2 + X\beta_2 + \varepsilon_2) + \alpha_0 + X\beta_0 + \varepsilon_0 \end{aligned} \tag{3.2}$$

where:

$$\alpha_0 = \alpha_{00},$$
$$\alpha_1 = \alpha_{10} - \alpha_{00},$$
$$\alpha_2 = \alpha_{01} - \alpha_{00},$$
$$\alpha_{12} = \alpha_{11} - \alpha_{10} - \alpha_{01} + \alpha_{00},$$
$$\beta_0 = \beta_{00},$$

[25] In some equations, the superscript 'P' is dropped for simplicity.

$\beta_1 = \beta_{10} - \beta_{00}$,
$\beta_2 = \beta_{01} - \beta_{00}$,
$\beta_{12} = \beta_{11} - \beta_{10} - \beta_{01} + \beta_{00}$,
$\varepsilon_0 = \varepsilon_{00}$,
$\varepsilon_1 = \varepsilon_{10} - \varepsilon_{00}$,
$\varepsilon_2 = \varepsilon_{01} - \varepsilon_{00}$,
$\varepsilon_{12} = \varepsilon_{11} - \varepsilon_{10} - \varepsilon_{01} + \varepsilon_{00}$.

This way of denotation has two advantages. Firstly, it implies the program variables d_1 and d_2 that are interacted with the potential outcomes. For example, α_{12} means that a linear combination of α parameters that is multiplied with $d_1 d_2$, while α_1 means that a linear combination of α parameters that is multiplied with d_1. Secondly, it allows for simple algebra when there are more than two programs (see Appendix 3.2), since there is a relation between the denoted parameters as follows:

$\alpha_1 = \alpha_{10} - \alpha_{00} = \alpha_{10} - \alpha_0$,
$\alpha_2 = \alpha_{01} - \alpha_{00} = \alpha_{01} - \alpha_0$,
$\alpha_{12} = \alpha_{11} - \alpha_{10} - \alpha_{01} + \alpha_{00} = \alpha_{11} - \alpha_1 - \alpha_2 - \alpha_0$.

We will focus on the impact of program d_1. The discussion of program d_2 is the same. Impact of program d_1 on a person is equal to:

$$\Delta_i(d_1) = Y_{d_1=1,d_2,X,\varepsilon} - Y_{d_1=0,d_2,X,\varepsilon} = d_2(\alpha_{12} + X\beta_{12} + \varepsilon_{12}) + (\alpha_1 + X\beta_1 + \varepsilon_1). \tag{3.3}$$

The conditional parameters $ATE_{(X)}$ and $ATT_{(X)}$ for d_1 are expressed as follows:

$$\begin{aligned} ATE1_{(X)} &= E[\Delta_i(d_1) \mid X] \\ &= E\left(Y_{d_1=1,d_2,X,\varepsilon} - Y_{d_1=0,d_2,X,\varepsilon} \middle| X\right) \\ &= [E(d_2 \mid X)(\alpha_{12} + X\beta_{12}) + \alpha_1 + X\beta_1] + [E(d_2\varepsilon_{12} \mid X) + E(\varepsilon_1 \mid X)] \end{aligned} \tag{3.4}$$

$$\begin{aligned} ATT1_{(X)} &= E[\Delta_i(d_1) \mid X, d_1 = 1] \\ &= E\left(Y_{d_1=1,d_2,X,\varepsilon} - Y_{d_1=0,d_2,X,\varepsilon} \middle| X, d_1 = 1\right) \\ &= [E(d_2 \mid X, d_1 = 1)(\alpha_{12} + X\beta_{12}) + \alpha_1 + X\beta_1] + [E(d_2\varepsilon_{12} \mid X, d_1 = 1) + E(\varepsilon_1 \mid X, d_1 = 1)] \end{aligned} \tag{3.5}$$

Similar to the case of a single program, $ATE1_{(X)}$ and $ATT1_{(X)}$ are not identified without additional assumptions, since (3.4) and (3.5) contain unobserved components. It should be noted that there are two possibilities for correlation between d_1 and d_2. In the first case, d_1 is correlated with d_2 but once conditional on *X*, they are independent of each other, i.e.

$$d_1 \perp d_2 \mid X. \tag{3.6}$$

In this case the program impact of d_1 can be estimated similarly to the case of the single program, i.e. the program d_2 can be ignored provided that all variables *X* are controlled for.

In the second case, there is a correlation between d_1 and d_2 even after conditional on *X*. This can be the case if participation in one program affects participation in the other program. For example, people getting the vocational training might be more eager to borrow micro-credit given their characteristics *X*.

To identify the program impacts, the CIA is expressed as follows:

Assumption 3.1. $Y_{11}, Y_{10}, Y_{01}, Y_{00} \perp D | X$, where $D = \begin{pmatrix} d_1 \\ d_2 \end{pmatrix}$. (A.3.1)

We allow the correlation between d_1 and d_2 given *X*, thus the identification assumption that the methods rely on is the assumption A.3.1. Under the assumption A.3.1, the program impacts can be identified parametrically using the OLS regression method, and non-parametrically using the matching method. The unconditional parameters, ATE and ATT, can then be identified and estimated due to (2.18) and (2.19).

3.3.2 Linear regression method

Although regression can be estimated in any form, e.g. linear or non-linear, or even nonparametric, for simplicity this section shows how to estimate the program impacts parametrically using the linear regression method.

As with a single program, we need an assumption on exogeneity of X in the potential outcome equations, i.e.:

Assumption 3.2. $E(\varepsilon_{11} \mid X) = E(\varepsilon_{10} \mid X) = E(\varepsilon_{01} \mid X) = E(\varepsilon_{00} \mid X) = 0$ (A.3.2)

Proposition 3.1. Under the assumptions A.3.1 and A.3.2, the linear regression produces unbiased estimators of all the conditional and unconditional parameters, $ATE1_{(X)}$, $ATT1_{(X)}$, $ATE1$ and $ATT1$.

The proof is very simple as follows. Firstly, the program impact parameters are identified under the assumption A.3.1 and A.3.2, since:

$$\left[E(d_2\varepsilon_{12} \mid X) + E(\varepsilon_1 \mid X)\right] = 0, \tag{3.7}$$

$$\left[E(d_2\varepsilon_{12} \mid X, d_1 = 1) + E(\varepsilon_1 \mid X, d_1 = 1)\right] = 0. \tag{3.8}$$[26]

Secondly, parameters $\alpha_{12}, \beta_{12}, \alpha_1, \beta_1$ are estimated in an unbiased way from equation (3.2). Rewrite (3.2) as:

$$Y = \alpha_0 + X\beta_0 + d_1 d_2(\alpha_{12} + X\beta_{12}) + d_1(\alpha_1 + X\beta_1) + d_2(\alpha_2 + X\beta_2) + (d_1 d_2 \varepsilon_{12} + d_1\varepsilon_1 + d_2\varepsilon_2 + \varepsilon_0). \tag{3.9}$$

In which the error term has the following conventional property:

$$\begin{aligned} E(d_1 d_2\varepsilon_{12} + d_1\varepsilon_1 + d_2\varepsilon_2 + \varepsilon_0 \mid X, d_2, d_1) &= d_1 d_2 E(\varepsilon_{12} \mid X, d_2, d_1) + d_1 E(\varepsilon_1 \mid X, d_2, d_1) \\ &\quad + d_2 E(\varepsilon_2 \mid X, d_2, d_1) + E(\varepsilon_0 \mid X, d_2, d_1) \\ &= 0. \end{aligned}$$

Thus unbiased estimators of $ATE1_{(X)}$ and $ATT1_{(X)}$ are:

$$A\hat{T}E1_{(X)} = \left[(\hat{\alpha}_{12} + X\hat{\beta}_{12})\hat{E}(d_2 \mid X) + \hat{\alpha}_1 + X\hat{\beta}_1\right], \tag{3.10}$$

$$A\hat{T}T1_{(X)} = \left[(\hat{\alpha}_{12} + X\hat{\beta}_{12})\hat{E}(d_2 \mid X, d_1 = 1) + \hat{\alpha}_1 + X\hat{\beta}_1\right], \tag{3.11}$$

where $\hat{E}(d_2 \mid X)$ can be a sample mean of the variable d_2 for given X. The unconditional parameters are estimated using (2.18) and (2.19).

There are two points that should be noted. Firstly, the equation (3.9) allows for the overlap between participation in program d_1 and participation in program d_2. If the two programs are mutually exclusive, $d_1 d_2$ will be equal to zero. Secondly, $ATE1_{(X)}$ is not necessarily equal to $ATT1_{(X)}$ as in the case of a single program. These two parameters are the same if the following equation holds:

$$E(d_2 \mid X) = E(d_2 \mid X, d_1 = 1). \tag{3.12}$$

Equation (3.12) holds if condition (3.6) is satisfied, i.e. d_1 and d_2 are independent conditional on X.

[26] The proofs of (3.7) and (3.8) are presented in Appendix 3.1.

3.3.3 Matching method

Suppose we are interested in the impact of program d_1. The program impact is measured by the parameters $ATE1_{(X)}$ and $ATT1_{(X)}$ as follows:

$$ATE1_{(X)} = E(Y_{d_1=1} - Y_{d_1=0} \mid X), \tag{3.13}$$

$$ATT1_{(X)} = E(Y_{d_1=1} - Y_{d_1=0} \big| X, d_1 = 1). \tag{3.14}$$

To express the two parameters in terms of the four potential outcomes, we rearrange (3.14):

$$\begin{aligned}
ATT1_{(X)} &= E(Y_{d_1=1} \mid X, d_1 = 1) - E(Y_{d_1=0} \mid X, d_1 = 1) \\
&= \left[E(Y_{11} \mid X, d_1 = 1, d_2 = 1)\Pr(d_2 = 1 \mid X, d_1 = 1) + E(Y_{10} \mid X, d_1 = 1, d_2 = 0)\Pr(d_2 = 0 \mid X, d_1 = 1) \right] \\
&\quad - \left[E(Y_{01} \mid X, d_1 = 1, d_2 = 1)\Pr(d_2 = 1 \mid X, d_1 = 1) + E(Y_{00} \mid X, d_1 = 1, d_2 = 0)\Pr(d_2 = 0 \mid X, d_1 = 1) \right] \\
&= \left[E(Y_{11} \mid X, d_1 = 1, d_2 = 1) - E(Y_{01} \mid X, d_1 = 1, d_2 = 1) \right]\Pr(d_2 = 1 \mid X, d_1 = 1) \\
&\quad + \left[E(Y_{10} \mid X, d_1 = 1, d_2 = 0) - E(Y_{00} \mid X, d_1 = 1, d_2 = 0) \right]\Pr(d_2 = 0 \mid X, d_1 = 1).
\end{aligned} \tag{3.15}$$

It is worth noting two points in (3.15). Firstly, (3.15) allows for the overlap between participation in program d_1 and participation in program d_2. If the two programs are mutually exclusive, then $Pr(d_2 = 1 \mid X, d_1 = 1)$ will be equal to 0, and $Pr(d_2 = 0 \mid X, d_1 = 1)$ is equal to 1. In this case the implementation of the matching method is similar to the case of a single binary program, taking into account that the comparison group should exclude those who participate in program d_2.

Secondly, (3.15) allows for correlation between d_1 and d_2 given X. If the two programs are uncorrelated given X (i.e. condition (3.6) holds), then:

$$E(Y_{11} \mid X, d_1 = 1, d_2 = 1) = E(Y_{10} \mid X, d_1 = 1, d_2 = 0),$$

$$E(Y_{01} \mid X, d_1 = 1, d_2 = 1) = E(Y_{00} \mid X, d_1 = 1, d_2 = 0).$$

As a result, the estimation of the program impacts is similar to the case of a single program.

Similarly, the average treatment effect on the non-treated conditional on X is written as follows:

$$\begin{aligned}
ATNT1_{(X)} &= E(Y_{d_1=1} \mid X, d_1 = 0) - E(Y_{d_1=0} \mid X, d_1 = 0) \\
&= \left[E(Y_{11} \mid X, d_1 = 0, d_2 = 1) - E(Y_{01} \mid X, d_1 = 0, d_2 = 1) \right]\Pr(d_2 = 1 \mid X, d_1 = 0) \\
&\quad + \left[E(Y_{10} \mid X, d_1 = 0, d_2 = 0) - E(Y_{00} \mid X, d_1 = 0, d_2 = 0) \right]\Pr(d_2 = 0 \mid X, d_1 = 0).
\end{aligned} \tag{3.16}$$

$ATE1_{(X)}$ can be expressed in terms of the potential outcome $Y_{11}, Y_{10}, Y_{01}, Y_{00}$ using Equation (2.6) and the results from (3.15) and (3.16):

$$ATE1_{(X)} = ATT1_{(X)} Pr(d_1 = 1 \mid X) + ATNT1_{(X)} Pr(d_1 = 0 \mid X). \tag{3.17}$$

In addition to the assumption A.3.1, to estimate $ATE1_{(X)}$ and $ATT1_{(X)}$ for program d_1 the matching method requires that there be remaining people who do not participate in the program d_1 but have identical distribution of the X variables given program d_2. This is the common support assumption:

Assumption 3.3.

$$0 < P(d_1 = 1 \mid X, d_2 = 0) < 1$$

$$0 < P(d_1 = 1 \mid X, d_2 = 1) < 1 \tag{A.3.3}$$

Where $P(d_1 = 1 \mid X, d_2)$ is the conditional probability of being assigned the program d_1 given the X variables and d_2.

This assumption can be written using denotation of the vector variable D:

$0 < P(D = D^* \mid X) < 1$ where $D^* \in \Omega_D = \left\{ \begin{pmatrix}1\\1\end{pmatrix}; \begin{pmatrix}1\\0\end{pmatrix}; \begin{pmatrix}0\\1\end{pmatrix}; \begin{pmatrix}0\\0\end{pmatrix} \right\}$

However, assumption A.3.3 is mentioned to emphasize that the program of interest is d_1.

Proposition 3.2. Under assumptions A.3.1 and A.3.3, the conditional and unconditional parameters, $ATE1_{(X)}$, $ATT1_{(X)}$, $ATE1$ and $ATT1$ are identified by the matching method.

This proposition results from assumptions (A.3.1) and (A.3.3), which allow the unobservable outcomes in (3.15) and (3.16) to equal the observable outcomes:

$$E(Y_{01} \mid X, d_1 = 1, d_2 = 1) = E(Y_{01} \mid X, d_1 = 0, d_2 = 1) \tag{3.18}$$

$$E(Y_{00} \mid X, d_1 = 1, d_2 = 0) = E(Y_{00} \mid X, d_1 = 0, d_2 = 0) \tag{3.19}$$

$$E(Y_{11} \mid X, d_1 = 0, d_2 = 1) = E(Y_{11} \mid X, d_1 = 1, d_2 = 1) \tag{3.20}$$

$$E(Y_{10} \mid X, d_1 = 0, d_2 = 0) = E(Y_{10} \mid X, d_1 = 1, d_2 = 0) \tag{3.21}$$

As a result, $ATT1_{(X)}$ and $ATNT1_{(X)}$ are identified. The parameter $ATE1_{(X)}$ is identified as in (3.17). The unconditional parameters $ATE1$ and $ATT1$ can be identified simply by taking the expectation of the conditional parameters over the range of the *X* variables and d_2 as in Equations (2.18) and (2.19).[27]

To estimate the parameters, the non-participants of program d_1 will be matched to participants of this program based on the closeness of the distance between the pre-treatment variables. The matching is performed for people who have the same program variable d_2, i.e. the participants and matched non-participants have the same participation status in program d_2.

Let n_{ic} denote as the number of non-participants who are matched with the participant *i*, and let $w(i,j)$ be the weight attached to the outcome of each matched non-participant *j*, $j=1,\ldots,n_{ic}$. These weights are non-negative and sum up to 1, i.e.

$$\sum_{j=1}^{n_{ic}} w(i,j) = 1.$$

Weights can be equal weights, e.g. as in n-nearest neighbour matching or different weights e.g. kernel matching.

The estimator of $ATT1_{(X)}$ at a given value *X* of the pre-treatment variables *X* is:

$$\hat{ATT1}_{(X=x)} = \frac{1}{n_{x1} + n_{x2}} \left\{ n_{x1} \sum_{d_2=0, X_i=x} \left[Y_{1i} - \sum_{j=1}^{n_{ic}} w(i,j) Y_{0j} \right] + n_{x2} \sum_{d_2=1, X_i=x} \left[Y_{1i} - \sum_{j=1}^{n_{ic}} w(i,j) Y_{0j} \right] \right\}, \tag{3.22}$$

where

n_{x1} is the number of units who have $d_1 = 1; d_2 = 0; X = x$.

n_{x2} is the number of units who have $d_1 = 1; d_2 = 1; X = x$.

Y_{1i} and Y_{0j} are the observed outcomes of participant *i* and non-participant *j* with *X* = *X*.

To estimate $ATNT1_{(X)}$, each non-participant *j* is matched with n_{jc} participants based on the closeness of variables *X*. The formula of the estimator of $ATNT1_{(X)}$ is similar to (3.22):

[27] If we are interested in the impact of the d_1 program, we only need assumptions which are specified by (3.18) through (3.21) to identify the program impact. Assumption A.3.1 is a general (strong) one which is required to estimate impacts of any change in the program status on any group. For example, one can be interested in joint impact of the two programs on the treated, which is defined as:
$ATT12_{(X)} = E(Y_{11} - Y_{00} | X, d_1 = 1, d_2 = 1)$.
Then assumption A.3.1 guarantees that $E(Y_{00} | X, d_1 = 1, d_2 = 1) = E(Y_{00} | X, d_1 = 0, d_2 = 0)$ so that $ATT12_{(X)}$ can be identified.

$$A\hat{T}NT1_{(X=x)} = \frac{1}{n'_{x1} + n'_{x2}} \left\{ n'_{x1} \sum_{d_2=0, X_j=x} \left[Y_{0j} - \sum_{i=1}^{n_{jc}} w(i,j) Y_{1i} \right] + n'_{x2} \sum_{d_2=1, X_j=x} \left[Y_{0j} - \sum_{i=1}^{n_{jc}} w(i,j) Y_{1i} \right] \right\}, \quad (3.23)$$

where

n'_{x1} is the number of units who have $d_1 = 0; d_2 = 0; X = x$.

n'_{x2} is the number of units who have $d_1 = 0; d_2 = 1; X = x$.

$w(i,j)$ is the weight attached to the outcome of participant i who is matched to non-participant j. The weights are also non-negative and sum up to 1.

Y_{1i} and Y_{0j} are the observed outcomes of participant *i* and non-participant *j* with *X* = *X*.

The estimator of $ATE1_{(X)}$ is the weighted average of the estimators of $ATT1_{(X)}$ and $ATNT1_{(X)}$ according to formula (3.17).

Finally the estimators of the unconditional parameters are:

$$A\hat{T}T1 = \frac{1}{\sum I\{d_1 = 1; x \in S_X\}} \sum_{x \in S_X | d_1 = 1} A\hat{T}T1_{(X=x)},$$

$$A\hat{T}E1 = \frac{1}{\sum I\{x \in S_X\}} \sum_{x \in S_X} A\hat{T}E1_{(X=x)},$$

where *I{}* is an indicator function that equals 1 if the value of {} is true, and 0 otherwise; S_X is the space of the *X* variables in the data sample.

3.3.4 Matching using the propensity score

As mentioned, a popular way to perform matching is to use the propensity score (Rosenbaum and Rubin, 1983). Proposition 2.1 is simply extended to the case of two multiple overlapping programs as follows:

Proposition 3.5. $Y_{11}, Y_{10}, Y_{01}, Y_{00} \perp D | X \;\Rightarrow\; Y_{11}, Y_{10}, Y_{01}, Y_{00} \perp D | P(D | X)$,
where:

$$D = \begin{pmatrix} d_1 \\ d_2 \end{pmatrix},$$

$$P(D | X) = P(D = D^* | X) \text{ with } D^* \in \Omega_D = \left\{ \begin{pmatrix} 1 \\ 1 \end{pmatrix}; \begin{pmatrix} 1 \\ 0 \end{pmatrix}; \begin{pmatrix} 0 \\ 1 \end{pmatrix}; \begin{pmatrix} 0 \\ 0 \end{pmatrix} \right\}.$$

The proof is very similar to the case of one binary program in Rosenbaum and Rubin (1983).

The proposition means that if the CIA holds for the *X* variables, it also holds for the propensity scores. To perform the propensity score matching, a multinomial model can be used to predict the conditional probability of being assigned to each program status (4 statues) given *X*, i.e. $P(d_1 = 1, d_2 = 1 | X)$, $P(d_1 = 1, d_2 = 0 | X)$, $P(d_1 = 0, d_2 = 1 | X)$ and $P(d_1 = 0, d_2 = 0 | X)$. The propensity scores will be selected depending on the program statuses of matched people. For example, if we want to match people having program status $\{d_1 = 1, d_2 = 1\}$ with those having $\{d_1 = 1, d_2 = 0\}$, the probabilities $P(d_1 = 1, d_2 = 1 | X)$ and $P(d_1 = 1, d_2 = 0 | X)$ will be used as the propensity scores. The inconvenience of this matching method is that the matching is performed using two propensity scores.

Since we focus on the impact of a program of interest, (e.g. program d_1), and use the estimators based on (3.22) and (3.23), we perform the matching with exact match on the participation in another program (program d_2 in this discussion). As a result, we do not need to use propensity score estimates from a multinomial model. More specifically, we can use a probit or logit model to predict $P(d_1 = 1 | X)$ using one sample with $d_2 = 1$ and another sample with $d_2 = 0$. The predicted probabilities can be used as propensity scores to match participants with non-participants in the d_1 program who have the same participation status of the d_2 program.

3.4 Results from Monte Carlo simulation

3.4.1 Simulation design

This section presents the simulation results of measuring ATT using the regression and PSM methods when there are two overlapping programs. Suppose that two programs d_1 and d_2 are implemented simultaneously, and we are interested in the impact of program d_1. Corresponding to the values of d_1 and d_2, there are 4 potential outcomes, which are expressed as functions of covariates *X* and error terms *ε*:

$$Y_{00} = 10 + X_1 + X_2 + \varepsilon_{00}, \tag{4.1}$$

$$Y_{10} = 10 + kX_1 + kX_2 + \varepsilon_{10}, \tag{4.2}$$

$$Y_{01} = 10 + kX_1 + kX_2 + \varepsilon_{01}, \tag{4.3}$$

$$Y_{11} = 10 + gX_1 + kX_2 + \varepsilon_{11}. \tag{4.4}$$

Where X_1 and X_2 follow a bivariate normal distribution $N(\mu_1, \mu_2, \sigma_1, \sigma_2, \rho) = N(15, 15, 5, 5, 0.5)$. Each error term follows a normal distribution $N(\mu,\sigma) = N(0,5)$. Impacts of programs d_1 and d_2 are changed by varying the coefficients of X_1 and X_2 from (1, 1) to (*k, g*). That the same coefficient *k* is specified in both (4.2) and (4.3) means if there is no correlation between d_1 and d_2, ATE and ATT of d_1 are the same as those of d_2. The values of *g* and *k* will be changed to examine the sensitivity of the estimates to the magnitude of the program impacts.

The assignment of programs d_1 and d_2 is designed in the two following scenarios:

Scenario 1. d_1 and d_2 are correlated, but once conditional on X_1 they are independent:

$W_1 = X_1 + u_1$,
$d_1 = 1$ if $W_1 < W_1^*$,
$d_1 = 0$ otherwise,
and
$W_2 = X_1 + u_2$,
$d_2 = 1$ if $W_2 < W_2^*$,
$d_2 = 0$ otherwise,
where error terms u_1 and u_2 each follow a normal distribution $N(\mu,\sigma) = N(0,5)$.

Scenario 2. Conditional on X_1, d_1 and d_2 are still correlated. This happens when one participating in d_1 is more promoted to participate in d_2.

$W_1 = X_1 + u_1$
$d_1 = 1$ if $W_1 < W_1^*$
$d_1 = 0$ otherwise,
and
$W_2 = X_1 - 10d_1 + u_2$,
$d_2 = 1$ if $W_2 < W_2^*$,
$d_2 = 0$ otherwise,
where error terms u_1 and u_2 each follow a normal distribution $N(\mu,\sigma) = N(0,5)$.

3.4.2 Simulation results

Table 3.1 and 3.2 present the simulation results of estimation of ATT of program d_1 using different estimators. In each table, there are four panels corresponding to the values of *g* and *k*. There are two OLS regression estimators, one without interactions between *X* and d_1, d_2:
$Y = \beta_0 + \beta_1 X_1 + \beta_2 X_2 + \beta_3 d_1 + \beta_4 d_2 + \beta_5 d_1 d_2 + \varepsilon$,
and one with the interaction:
$Y = \beta_0 + \beta_1 X_1 + \beta_2 X_2 + \beta_3 d_1 + \beta_4 d_2 + \beta_5 d_1 d_2 + \beta_6 X_1 d_1 + \beta_7 X_2 d_1 + \beta_8 X_1 d_2 + \beta_9 X_2 d_2 + \varepsilon$.

There are three methods of matching estimation using the metric of the propensity score. The first is matching using two covariates X_1 and X_2. The second is matching using three covariates X_1, X_2 and d_2. The third uses the matching estimator given in (3.22). For each estimation strategy, there are three matching schemes to select non-participants and weight their outcomes, i.e. nearest-neighbour matching, three-nearest-neighbours matching, and kernel matching with bandwidth of 0.01.

Table 3.1 presents the results in scenario 1. It is shown that in terms of MSE, the regression methods perform best since the models are correctly specified. When the value of the coefficients *g* and *k* are small, matching method 3 gives a slightly smaller MSE compared with matching methods 1 and 2. As the value of *g* and *k* increases, difference in MSE between the three matching methods increases. Method 3 results in the smallest MSE, then method 2, and method 1 gives the largest MSE. The trend happens regardless of sample size and matching scheme. In addition, compared with methods 1 and 2, method 3 works very well when the sample size is small (i.e. n = 250 and n = 500), and the kernel matching estimator is used. This result suggests matching method 3 should be used when the impacts of d_1 and d_2 are expected to be large.

The results of scenario 2 are presented in Table 3.2. Again the regression methods perform better than matching, especially in small samples. Matching method 1 gives substantial magnitude of MSE, since it is a biased estimator. It is evidence that although the participation in program d_2 does not affect the participation on program d_1, failing to control program d_2 will lead to bias in measuring the impact of program d_1 if they are correlated.

Matching method 3 has a slightly smaller MSE compared with matching method 2 when the value of *g* and *k* are small. As the value of *g* and *k* increases, method 3 results in much lower MSE compared to method 2, especially in the case of small sample sizes and kernel matching scheme.

Table 3.1. Mean impact ratio and MSE for two correlated programs: scenario 1.

Measurement		n = 250		n = 500		n = 1000		n = 3000
Model parameter: k = 1.3; g = 1.5								
ATT		4.932		4.800		4.895		4.897
Observed outcome		42.849		42.795		42.846		42.829
Proportion with D_1=1		0.241		0.240		0.240		0.240
Proportion with D_2=1		0.241		0.240		0.240		0.240
Correlation between D_1 and D_2		0.303		0.305		0.305		0.306
	IM	MSE	IM	MSE	IM	MSE	IM	MSE
Regression method								
Without interaction	0.978	0.879	0.983	0.407	0.964	0.242	0.962	0.100
With interaction	0.998	0.906	0.992	0.436	0.974	0.244	0.972	0.089
1 nearest neighbour matching								
Method 1	0.954	3.780	0.973	1.945	0.959	0.926	0.984	0.375
Method 2	0.966	3.393	0.970	1.881	0.955	0.959	0.981	0.340
Method 3	0.909	3.257	0.942	1.707	0.959	0.906	0.978	0.300
3 nearest neighbours matching								
Method 1	0.891	2.912	0.936	1.309	0.945	0.721	0.973	0.257
Method 2	0.888	2.615	0.937	1.258	0.942	0.676	0.973	0.226
Method 3	0.820	2.797	0.904	1.138	0.926	0.664	0.966	0.213
Kernel matching (bandwidth = 0.01)								
Method 1	1.220	3.192	1.163	1.465	1.081	0.574	1.032	0.197
Method 2	1.225	2.939	1.158	1.244	1.076	0.519	1.032	0.163
Method 3	1.129	3.509	1.090	1.167	1.049	0.457	1.018	0.134
Model parameter: k = 1.5; g = 1.8								
ATT		8.066		8.033		8.008		8.053
Observed outcome		44.709		44.692		44.678		44.706
Proportion with D_1=1		0.240		0.240		0.239		0.240
Proportion with D_2=1		0.240		0.241		0.240		0.240
Correlation between D_1 and D_2		0.304		0.304		0.304		0.305
	IM	MSE	IM	MSE	IM	MSE	IM	MSE
Regression method								
Without interaction	0.958	1.002	0.965	0.562	0.967	0.277	0.966	0.161
With interaction	0.973	0.976	0.977	0.526	0.978	0.236	0.976	0.123
1 nearest neighbour matching								
Method 1	0.941	5.953	0.970	3.030	0.982	1.374	0.993	0.467
Method 2	0.937	4.472	0.979	2.393	0.981	1.219	0.995	0.419
Method 3	0.897	4.595	0.953	2.084	0.971	0.959	0.988	0.334
3 nearest neighbours matching								
Method 1	0.919	3.952	0.952	2.073	0.967	0.939	0.985	0.335
Method 2	0.906	3.125	0.951	1.594	0.967	0.759	0.985	0.263
Method 3	0.862	3.365	0.921	1.491	0.951	0.700	0.979	0.228
Kernel matching (bandwidth = 0.01)								
Method 1	1.212	5.699	1.139	2.817	1.085	1.125	1.039	0.335
Method 2	1.206	5.033	1.141	2.375	1.084	0.867	1.039	0.263
Method 3	1.083	3.508	1.065	1.438	1.049	0.595	1.024	0.184
Model parameter: k = 1.8; g = 2.1								
ATT		12.075		12.161		12.040		12.148
Observed outcome		47.345		47.324		47.346		47.354
Proportion with D_1=1		0.239		0.239		0.240		0.240
Proportion with D_2=1		0.240		0.239		0.242		0.240
Correlation between D_1 and D_2		0.304		0.306		0.308		0.305
	IM	MSE	IM	MSE	IM	MSE	IM	MSE
Regression method								
Without interaction	0.945	1.648	0.953	0.909	0.955	0.580	0.957	0.379
With interaction	0.959	1.322	0.963	0.683	0.965	0.423	0.967	0.251

Measurement	n = 250		n = 500		n = 1000		n = 3000	
1 nearest neighbour matching								
Method 1	0.959	8.217	0.984	4.006	0.993	2.229	0.995	0.732
Method 2	0.952	6.067	0.973	3.219	0.987	1.515	0.996	0.582
Method 3	0.926	4.892	0.963	2.410	0.980	1.233	0.991	0.395
3 nearest neighbours matching								
Method 1	0.936	6.168	0.975	2.715	0.985	1.484	0.989	0.526
Method 2	0.934	3.805	0.967	1.913	0.979	0.942	0.989	0.347
Method 3	0.888	4.378	0.939	1.862	0.968	0.812	0.984	0.266
Kernel matching (bandwidth = 0.01)								
Method 1	1.223	12.339	1.156	5.595	1.097	2.522	1.041	0.632
Method 2	1.218	10.153	1.148	4.459	1.091	1.818	1.040	0.452
Method 3	1.072	5.609	1.061	1.908	1.047	0.823	1.022	0.234
Model parameter: k = 2.1; g = 2.5								
ATT		16.557		16.528		16.594		16.618
Observed outcome		50.016		50.087		50.082		50.075
Proportion with D_1=1		0.241		0.240		0.240		0.240
Proportion with D_2=1		0.240		0.240		0.240		0.240
Correlation between D_1 and D_2		0.308		0.305		0.305		0.305
	IM	MSE	IM	MSE	IM	MSE	IM	MSE
Regression method								
Without interaction	0.954	2.121	0.955	1.250	0.954	1.030	0.955	0.676
With interaction	0.966	1.363	0.965	0.809	0.967	0.589	0.966	0.391
1 nearest neighbour matching								
Method 1	0.987	12.143	0.985	6.430	0.988	3.286	0.992	1.155
Method 2	0.974	8.933	0.990	4.593	0.995	2.412	0.994	0.804
Method 3	0.956	5.398	0.969	2.660	0.985	1.452	0.994	0.454
3 nearest neighbours matching								
Method 1	0.968	8.927	0.975	4.688	0.983	2.433	0.991	0.787
Method 2	0.965	5.152	0.971	2.416	0.983	1.496	0.990	0.440
Method 3	0.917	4.631	0.948	2.152	0.973	0.983	0.987	0.308
Kernel matching (bandwidth = 0.01)								
Method 1	1.238	23.626	1.157	10.168	1.091	4.152	1.039	1.136
Method 2	1.231	20.282	1.155	8.904	1.092	3.387	1.039	0.717
Method 3	1.074	7.078	1.062	2.609	1.044	1.104	1.021	0.306

IM: mean ratio of the impact estimate over the true impact.
n is number of observations.
Number of replications: 500.

Table 3.2. Mean impact ratio and MSE for two correlated programs: scenario 2.

Measurement		**n = 250**		**n = 500**		**n = 1000**		**n = 3000**
Model parameter: *k* = 1.3; *g* = 1.5								
ATT		3.284		3.290		3.308		3.280
Observed outcome		42.297		42.303		42.286		42.272
Proportion with D_1=1		0.241		0.240		0.241		0.240
Proportion with D_2=1		0.227		0.226		0.227		0.226
Correlation between D_1 and		0.754		0.755		0.755		0.755
	IM	MSE	IM	MSE	IM	MSE	IM	MSE
Regression method								
Without interaction	0.950	2.554	0.940	0.226	0.932	0.616	0.930	0.246
With interaction	1.030	3.014	0.973	0.198	0.967	0.597	0.967	0.203
1 nearest neighbour matching								
Method 1	2.312	17.374	2.218	16.043	2.193	15.554	2.215	15.865
Method 2	0.900	9.707	1.002	0.686	0.958	2.045	0.992	0.582
Method 3	0.866	6.904	0.990	0.521	0.929	1.566	0.964	0.528
3 nearest neighbours matching								
Method 1	2.253	14.679	2.194	15.341	2.153	14.282	2.202	15.464
Method 2	0.758	7.338	0.973	0.486	0.929	1.344	0.970	0.406
Method 3	0.726	6.863	0.963	0.333	0.897	1.155	0.951	0.334
Kernel matching (bandwidth = 0.01)								
Method 1	2.929	28.542	2.298	18.037	2.399	20.617	2.301	18.027
Method 2	1.444	6.788	1.027	0.414	1.081	1.290	1.025	0.333
Method 3	1.108	6.794	1.002	0.273	1.002	0.869	0.991	0.236
Model parameter: *k* = 1.5; *g* = 1.8								
ATT		5.266		5.260		5.227		5.230
Observed outcome		43.766		43.761		43.740		43.768
Proportion with D_1=1		0.239		0.239		0.239		0.240
Proportion with D_2=1		0.225		0.225		0.225		0.226
Correlation between D_1 and		0.752		0.758		0.754		0.755
	IM	MSE	IM	MSE	IM	MSE	IM	MSE
Regression method								
Without interaction	0.945	2.615	0.911	1.736	0.928	0.857	0.931	0.342
With interaction	0.989	2.876	0.951	1.614	0.962	0.702	0.965	0.233
1 nearest neighbour matching								
Method 1	2.278	44.531	2.257	42.849	2.257	43.195	2.269	44.006
Method 2	0.956	9.136	0.957	4.939	0.955	2.447	0.999	0.801
Method 3	0.898	8.430	0.913	4.423	0.974	2.058	0.991	0.584
3 nearest neighbours matching								
Method 1	2.250	41.065	2.231	40.399	2.252	42.483	2.254	42.872
Method 2	0.884	7.425	0.894	4.036	0.950	1.856	0.979	0.572
Method 3	0.847	6.742	0.876	3.008	0.940	1.336	0.972	0.370
Kernel matching (bandwidth = 0.01)								
Method 1	2.720	75.047	2.559	63.849	2.456	57.008	2.350	49.565
Method 2	1.375	8.810	1.148	3.696	1.064	1.740	1.024	0.491
Method 3	1.055	8.671	0.997	2.644	1.019	1.122	1.002	0.292
Model parameter: *k* = 1.8; *g* = 2.1								
ATT		7.018		7.098		7.051		7.042
Observed outcome		45.703		45.698		45.663		45.693
Proportion with D_1=1		0.239		0.242		0.240		0.240
Proportion with D_2=1		0.225		0.227		0.226		0.226
Correlation between D_1 and		0.755		0.749		0.754		0.755
	IM	MSE	IM	MSE	IM	MSE	IM	MSE
Regression method								
Without interaction	0.892	4.696	0.900	2.311	0.896	1.392	0.900	0.767
With interaction	0.926	4.165	0.932	1.892	0.930	0.974	0.935	0.443

Measurement		n = 250		n = 500		n = 1000		n = 3000
1 nearest neighbour matching								
Method 1	2.576	114.75	2.481	107.54	2.497	110.28	2.504	111.82
Method 2	0.955	14.142	0.964	6.556	0.965	2.869	0.984	0.971
Method 3	0.896	13.929	0.937	5.256	0.949	2.532	0.978	0.785
3 nearest neighbour matching								
Method 1	2.552	109.60	2.467	104.52	2.484	107.71	2.496	110.51
Method 2	0.873	13.271	0.909	4.950	0.950	2.053	0.972	0.691
Method 3	0.832	10.659	0.893	3.338	0.930	1.455	0.969	0.499
Kernel matching (bandwidth = 0.01)								
Method 1	3.047	188.06	2.810	157.47	2.705	141.69	2.595	125.42
Method 2	1.498	21.826	1.200	6.489	1.068	2.173	1.013	0.581
Method 3	1.003	11.925	0.998	2.970	0.990	1.191	0.993	0.331
Model parameter: k = 2.1; g = 2.5								
ATT		9.530		9.439		9.551		9.536
Observed outcome		47.750		47.766		47.788		47.779
Proportion with D_1=1		0.240		0.239		0.239		0.240
Proportion with D_2=1		0.226		0.226		0.225		0.226
Correlation between D_1 and		0.754		0.755		0.755		0.755
	IM	MSE	IM	MSE	IM	MSE	IM	MSE
Regression method								
Without interaction	0.895	5.788	0.894	3.284	0.901	2.032	0.900	1.279
With interaction	0.947	4.572	0.934	2.138	0.938	1.234	0.936	0.643
1 nearest neighbour matching								
Method 1	2.591	218.83	2.568	213.13	2.534	211.74	2.528	211.66
Method 2	0.943	16.232	0.953	7.744	0.978	3.651	0.991	1.136
Method 3	0.930	12.843	0.936	5.958	0.971	2.608	0.982	0.885
3 nearest neighbours matching								
Method 1	2.569	209.68	2.562	210.29	2.530	209.77	2.523	210.13
Method 2	0.888	14.143	0.932	5.849	0.954	2.892	0.982	0.727
Method 3	0.861	11.994	0.910	4.118	0.949	1.853	0.972	0.488
Kernel matching (bandwidth = 0.01)								
Method 1	3.051	353.52	2.899	308.65	2.737	270.06	2.619	237.12
Method 2	1.460	31.838	1.198	8.592	1.078	3.134	1.023	0.647
Method 3	1.054	11.051	1.001	3.159	1.002	1.395	0.996	0.299

IM: mean ratio of the impact estimate over the true impact.
n is number of observations.
Number of replications: 500.

3.5 Conclusions

When measuring the impact of a program, one should be aware that participants and non-participants can attend other simultaneous programs. If correlation between the selection of the program and the selection of the other programs vanishes once conditional on variables *X*, one can ignore those other programs. However, the simulation shows that controlling for the participation in the other programs leads to some gain in efficiency in terms of MSE. If the correlation remains even conditional on the *X* variables, neglect of the other programs will lead to biased estimation of the impact of the interested program. The PSM method can be used to measure the program impact in this case. In this chapter, the matching estimator is written as a weighted average of program impacts on groups with different program statuses. In other words, it combines the propensity score matching on the *X* variables and exact matching on the participation in the other programs. It is shown by the simulation that when impacts of the programs are high, this PSM method leads to lower MSE compared to other PSM estimations. The example of measurement of impacts of international and internal remittances also shows that this PSM method tends to result in lower standard errors, especially for the kernel matching scheme.

Finally, it should be noted that this chapter is written as an independent essay on the matching method. This matching method is not applied in other empirical chapters, since it is developed in the context of a conditional independence assumption and single-cross section data, whereas the other empirical chapters employ fixed-effects regressions and difference-in-differences methods which use panel data and do not rely on a conditional independence assumption. Developing efficient matching methods in a more general context which allows for program selection on unobservables and multiple correlated programs is beyond the scope of the paper, but certainly important for future research.

Appendix 3.1 Proof of equations

The assumption (A.3.1) is equivalent to:

$\varepsilon_{11}, \varepsilon_{10}, \varepsilon_{01}, \varepsilon_{00} \perp D \mid X$

As a result:

$$\left[E(d_2\varepsilon_{12} \mid X) + E(\varepsilon_1 \mid X)\right] = E(d_2\varepsilon_{12} \mid X)$$
$$= E(\varepsilon_{12} \mid X, d_2 = 1)Pr(d_2 = 1 \mid X)$$
$$= \left[E(\varepsilon_{12} \mid X, d_2 = 1, d_1 = 1)Pr(d_1 = 1 \mid X, d_2 = 1) + E(\varepsilon_{12} \mid X, d_2 = 1, d_1 = 0)Pr(d_1 = 0 \mid X, d_2 = 1)\right]Pr(d_2 = 1 \mid X)$$
$$= \left[E(\varepsilon_{12} \mid X)Pr(d_1 = 1 \mid X, d_2 = 1) + E(\varepsilon_{12} \mid X)Pr(d_1 = 0 \mid X, d_2 = 1)\right]Pr(d_2 = 1 \mid X)$$
$$= 0$$

Similarly, we will have

$$\left[E(d_2\varepsilon_{12} \mid X, d_1 = 1) + E(\varepsilon_1 \mid X, d_1 = 1)\right] = 0\,.$$

Appendix 3.2 The case of multiple overlapping binary programs

Parameters of interest

Now suppose that there are m programs that are assigned to subjects in population P. Denote participation in the programs by a vector variable *D*:
$D=(d_1,d_2,...,d_m)$.
where d_k is a variable that equals 1 if she participates in program *k*, and 0 otherwise. Subjects who do not participate in any program will have the value of the vector *D* equal to $D=(0,0,...,0)$. In contrast, subjects who participate in all the programs will have the value of the vector *D* equal to $D=(1,1,...,1)$. The set of the potential treatments has 2^m values:

$$\Omega_D=\left\{\begin{pmatrix}0\\0\\.\\.\\0\end{pmatrix};\begin{pmatrix}1\\0\\.\\.\\0\end{pmatrix};...;\begin{pmatrix}1\\1\\.\\.\\1\end{pmatrix}\right\}.$$

Corresponding to each value of the vector variable *D*, there is a potential outcome, denoted by Y_D^P. Thus for each subject, there are 2^m potential outcomes. However we are able to observe only one outcome of those, depending on the realization of the vector variable *D*. The potential outcomes can be written as functions of the observed variables *X* and unobserved variable *ε*:

$$Y_D^P=\alpha_D+X\beta_D+\varepsilon_D. \tag{1}$$

For example, when there are three programs, i.e. *m*=3, the potential outcomes are as follows:

$$Y_{d_1,d_2,d_3}^P=\alpha_{d_1,d_2,d_3}+X\beta_{d_1,d_2,d_3}+\varepsilon_{d_1,d_2,d_3},$$

which can be more specific as eight equations:

$$Y_{000}^P=\alpha_{000}+X\beta_{000}+\varepsilon_{000},$$
$$Y_{100}^P=\alpha_{100}+X\beta_{100}+\varepsilon_{100},$$
$$Y_{010}^P=\alpha_{010}+X\beta_{010}+\varepsilon_{010},$$
$$Y_{001}^P=\alpha_{001}+X\beta_{001}+\varepsilon_{001},$$
$$Y_{110}^P=\alpha_{110}+X\beta_{110}+\varepsilon_{110},$$
$$Y_{101}^P=\alpha_{101}+X\beta_{101}+\varepsilon_{101},$$
$$Y_{011}^P=\alpha_{011}+X\beta_{011}+\varepsilon_{011},$$
$$Y_{111}^P=\alpha_{111}+X\beta_{111}+\varepsilon_{111}.$$

Similar to (3.2), the observed outcome can be written in terms of 2^m potential outcomes and the program variables, and then the variables *X* and *ε* as follows:

$$\begin{aligned}Y=&\sum_i d_i(\alpha_i+X\beta_i+\varepsilon_i)+\sum_{i\neq j}d_id_j(\alpha_{ij}+X\beta_{ij}+\varepsilon_{ij})+\sum_{i\neq j\neq...\neq h}d_id_jd_h(\alpha_{ijh}+X\beta_{ijh}+\varepsilon_{ijh})+...\\&+(\alpha_{12...m}+X\beta_{12...m}+\varepsilon_{12...m})\prod_i d_i+(\alpha_{D=0}+X\beta_{D=0}+\varepsilon_{D=0}).\end{aligned} \tag{2}$$

where:
$\alpha_i=\alpha_{d_i=1,D\backslash d_i=0}-\alpha_{D=0}$ with $D\backslash d_i$ denotes the vector of the program variables not including d_i. Parameter $\alpha_{d_i=1,D\backslash d_i=0}$ belongs to the equation of potential outcome with the participation in only the program d_i. And:

$$\alpha_{ij} = \alpha_{d_i=1,d_j=1,D\setminus\{d_i,d_j\}=0} - \alpha_i - \alpha_j - \alpha_{D=0},$$
$$\ldots.$$
$$\alpha_{12...m} = \alpha_{D=1} - \sum_{i\neq j\neq k} \alpha_{\underbrace{ij...k}_{m-1}} - \sum_{i\neq j}\alpha_{ij} - \sum_i \alpha_i - \alpha_{D=0},$$

and the denotation is similar for β and ε. It should be noted that in this section, i (i=1,...,m) denotes program i, not observation i.

For example, with three programs (m=3), Equation (2) becomes:

$$\begin{aligned} Y &= d_1(\alpha_1 + X\beta_1 + \varepsilon_1) + d_2(\alpha_2 + X\beta_2 + \varepsilon_2) + d_3(\alpha_3 + X\beta_3 + \varepsilon_3) \\ &+ d_1d_2(\alpha_{12} + X\beta_{12} + \varepsilon_{12}) + d_2d_3(\alpha_{23} + X\beta_{23} + \varepsilon_{23}) + d_1d_3(\alpha_{13} + X\beta_{13} + \varepsilon_{13}) \\ &+ d_1d_2d_3(\alpha_{123} + X\beta_{123} + \varepsilon_{123}) + (\alpha_0 + X\beta_0 + \varepsilon_0). \end{aligned}$$

where:

$$\alpha_0 = \alpha_{000},$$
$$\alpha_1 = \alpha_{100} - \alpha_0 = \alpha_{100} - \alpha_{000},$$
$$\alpha_2 = \alpha_{010} - \alpha_0 = \alpha_{010} - \alpha_{000},$$
$$\alpha_3 = \alpha_{001} - \alpha_0 = \alpha_{001} - \alpha_{000},$$
$$\alpha_{12} = \alpha_{110} - \alpha_1 - \alpha_2 - \alpha_0,$$
$$\alpha_{23} = \alpha_{011} - \alpha_2 - \alpha_3 - \alpha_0,$$
$$\alpha_{13} = \alpha_{101} - \alpha_1 - \alpha_3 - \alpha_0,$$
$$\alpha_{123} = \alpha_{111} - \alpha_{12} - \alpha_{23} - \alpha_{13} - \alpha_1 - \alpha_2 - \alpha_3 - \alpha_0,$$

and the denotation is similar for β and ε.

Suppose that we are interested in the impacts of program k which are measured by the two parameters:

$$\begin{aligned} ATEk_{(X)} &= E(Y^P_{d_k=1} \mid X) - E(Y^P_{d_k=0} \mid X) \\ &= E\left(Y_{d_k=1,X,\overline{D},\varepsilon} - Y_{d_k=0,X,\overline{D},\varepsilon} \middle| X\right) \\ &= \alpha_k + X\beta_k + (\alpha_{ik} + X\beta_{ik})\sum_{i\neq k} E(d_i \mid X) + (\alpha_{ijk} + X\beta_{ijk}) \sum_{i\neq k; i\neq j, j\neq k} E(d_i d_j \mid X) \\ &\quad + \ldots + (\alpha_{12...m} + X\beta_{12...m}) E\left(\prod_{i\neq k} d_i \middle| X\right) \\ &\quad + \left[E(\varepsilon_k \mid X) + \sum_{i\neq k} E(d_i \varepsilon_{ik} \mid X) + \sum_{i\neq k; i\neq j, j\neq k} E(d_i d_j \varepsilon_{ijk} \mid X) + \ldots. + E\left(\varepsilon_{12...m} \prod_{i\neq k} d_i \middle| X\right)\right], \end{aligned} \tag{3}$$

$$\begin{aligned} ATTk_{(X)} &= E(Y^P_{d_k=1} \mid X, d_k = 1) - E(Y^P_{d_k=0} \mid X, d_k = 1) \\ &= E\left(Y_{d_k=1,X,\overline{D},\varepsilon} - Y_{d_k=0,X,\overline{D},\varepsilon} \middle| X, d_k = 1\right) \\ &= \alpha_k + X\beta_k + (\alpha_{ik} + X\beta_{ik})\sum_{i\neq k} E(d_i \mid X, d_k = 1) + (\alpha_{ijk} + X\beta_{ijk}) \sum_{i\neq k; i\neq j, j\neq k} E(d_i d_j \mid X, d_k = 1) \\ &\quad + \ldots + (\alpha_{12...m} + X\beta_{12...m}) E\left(\prod_{i\neq k} d_i \middle| X, d_k = 1\right) + \left[E(\varepsilon_k \mid X, d_k = 1) + \sum_{i\neq k} E(d_i \varepsilon_{ik} \mid X, d_k = 1) \right. \\ &\quad \left. + \sum_{i\neq k; i\neq j, j\neq k} E(d_i d_j \varepsilon_{ijk} \mid X, d_k = 1) + \ldots. + E\left(\varepsilon_{12...m} \prod_{i\neq k} d_i \middle| X, d_k = 1\right)\right]. \end{aligned} \tag{4}$$

where $\overline{D} = D \setminus d_k$, i.e. $\overline{D}$ is the vector of program variables not including the d_k program.

In a more general case, one can estimate the impact of a treatment state $D = D_g$ relative to a treatment state $D = D_h$:

$$ATEgh_{(X)} = E(Y^P_{D=D_g} \mid X) - E(Y^P_{D=D_h} \mid X), \tag{5}$$
$$ATTgh_{(X)} = E(Y^P_{D=D_g} \mid X, D = D_g) - E(Y^P_{D=D_h} \mid X, D = D_g). \tag{6}$$

However, explanation of (5) and (6) is complicated and less practical. For simplicity, we focus on the impact of a particular program, e.g. program d_k.

Regression method

Identification of the impact parameters of the program d_k is not straightforward, since there are unobserved terms in (3) and (4). As with two binary programs, to identify the parameters we need the CIA assumption:[28]

Assumption 1. $Y^P_D \perp D \mid X$. (A.1)
In addition, the assumption about the exogeneity of the *X* variables in all the equations of potential outcomes, i.e.:

Assumption 2. $E(\varepsilon_D \mid X) = 0$. (A.2)

Proposition 1. Under the assumptions A.1 and A.2, the regression method produces unbiased estimators of all the conditional and unconditional parameters $ATEk_{(X)}$, $ATTk_{(X)}$, $ATEk$ and $ATTk$.

The proof is similar to the case of two binary programs. The unobserved terms in (3) and (4) are equal to 0. In addition, the error term in Equation (2) has the conventional property that $E(\varepsilon \mid X, D) = 0$. Thus the conditional parameters $ATEk_{(X)}$ and $ATTk_{(X)}$ are estimated based on (3) and (4) using the coefficient estimates from Equation (2). Once the conditional parameters are estimated, the unconditional ones can also be estimated using Equations (2.18) and (2.19). It should be noted that the observed outcome *Y* can be any function of *X*, and the identification assumptions and estimation strategy are the same as the case of the linear function.

Matching method

In addition to the CIA, the matching method requires the assumption on common support to identify the impact parameters:

Assumption 3. $0 < P(d_k = 1 \mid X, \overline{D}) < 1$. (A.3)
$P(d_k = 1 \mid X, \overline{D})$ is the conditional probability of participating in program d_k given the *X* variables and other program variables. It is required that there be still subjects who do not participate in program d_k but have the same variables *X* and participation statuses of the other programs (not including program d_k) as those of the participants of program d_k.

It should be noted that the common support can be stated in terms of the probability of being assigned the treatment variable *D*, i.e.:
$0 < P(D = D^* \mid X) < 1$,

[28] We can require a weaker assumption 'conditional mean independence' in order to identify the program impact parameters: $E\left(Y^P_{(D)} \mid X, D\right) = E\left(Y^P_{(D)} \mid X\right)$.

where $D^* \in \Omega_D = \left\{ \begin{pmatrix} 0 \\ 0 \\ . \\ . \\ 0 \end{pmatrix}; \begin{pmatrix} 1 \\ 0 \\ . \\ . \\ 0 \end{pmatrix}; \ldots; \begin{pmatrix} 1 \\ 1 \\ . \\ . \\ 1 \end{pmatrix} \right\}$.

Proposition 2. Under the assumptions A.1 and A.3, the conditional and unconditional parameters $ATEk_{(X)}$, $ATTk_{(X)}$, $ATEk$ and $ATTk$ for program d_k are identified by the matching method.

Proof:

Similar to (3.15), the $ATTk_{(X)}$ is written as follows:

$$ATTk_{(X)} = E(Y^P_{d_k=1} \mid X, d_k = 1) - E(Y^P_{d_k=0} \mid X, d_k = 1)$$
$$= \sum_{\overline{D}_g \in \Omega_{\overline{D}}} \left\{ \left[E(Y^P_{\overline{D}=\overline{D}_g, d_k=1} \mid X, \overline{D} = \overline{D}_g, d_k = 1) - E(Y^P_{\overline{D}=\overline{D}_g, d_k=0} \mid X, \overline{D} = \overline{D}_g, d_k = 1) \right] \Pr(\overline{D} = \overline{D}_g \mid X, d_k = 1) \right\}, \tag{7}$$

and $ATNTk_{(X)}$:

$$ATNTk_{(X)} = E(Y^P_{d_k=1} \mid X, d_k = 0) - E(Y^P_{d_k=0} \mid X, d_k = 0)$$
$$= \sum_{\overline{D}_g \in \Omega_{\overline{D}}} \left\{ \left[E(Y^P_{\overline{D}=\overline{D}_g, d_k=1} \mid X, \overline{D} = \overline{D}_g, d_k = 0) - E(Y^P_{\overline{D}=\overline{D}_g, d_k=0} \mid X, \overline{D} = \overline{D}_g, d_k = 0) \right] \Pr(\overline{D} = \overline{D}_g \mid X, d_k = 0) \right\}. \tag{8}$$

where $\Omega_{\overline{D}}$ is the set of potential treatments (programs) $\overline{D} = D \setminus d_k$.

There are unobserved terms in (7) and (8), i.e.

$$E(Y^P_{\overline{D}=\overline{D}_g, d_k=1} \mid X, \overline{D} = \overline{D}_g, d_k = 0)$$

and

$$E(Y^P_{\overline{D}=\overline{D}_g, d_k=0} \mid X, \overline{D} = \overline{D}_g, d_k = 1).$$

However, under assumptions (A.1) and (A.3), we have:

$$E(Y^P_{\overline{D}=\overline{D}_g, d_k=1} \mid X, \overline{D} = \overline{D}_g, d_k = 0) = E(Y^P_{\overline{D}=\overline{D}_g, d_k=1} \mid X, \overline{D} = \overline{D}_g, d_k = 1), \tag{9}$$

$$E(Y^P_{\overline{D}=\overline{D}_g, d_k=0} \mid X, \overline{D} = \overline{D}_g, d_k = 1) = E(Y^P_{\overline{D}=\overline{D}_g, d_k=0} \mid X, \overline{D} = \overline{D}_g, d_k = 0). \tag{10}$$

If we substitute (9) and (10) into the conditional parameters of $ATTk_{(X)}$ and $ATNTk_{(X)}$, we can identify these parameters since all the terms are observed. The parameter $ATEk_{(X)}$ is the weighted average of the $ATTk_{(X)}$ and $ATNTk_{(X)}$. The unconditional parameters are also identified by formulas (2.18) and (2.19).

To estimate the program impacts, the participants of program d_k will be matched to the non-participants based on the closeness of the distance in the *X* variables. In addition, the matching is performed for people who have the same program statuses *D* (except program d_k). The estimator of the $ATTk_{(X)}$ has a similar form as in the case of two programs, i.e. formula (3.22), in which the sample mean outcomes of the participants are estimators of

$E(Y^{P}_{\overline{D}=\overline{D}_g,d_k=1} \mid X, \overline{D}=\overline{D}_g, d_k=0)$, and the sample mean outcomes of the matched non-participants are estimators of $E(Y^{P}_{\overline{D}=\overline{D}_g,d_k=0} \mid X, \overline{D}=\overline{D}_g, d_k=1)$.

The estimator of the $ATNTk_{(X)}$ has the formula similar to (3.23). Finally, the estimator of the $ATEk_{(X)}$ is the weighted average of the estimators of the $ATTk_{(X)}$ and $ATNTk_{(X)}$.

Matching using the propensity score

Proposition 2.1 is extended to the case of multiple overlapping programs as follows:

Proposition 3. $Y^P_D \perp D \mid X \Rightarrow Y^P_D \perp D \mid P(D \mid X)$,
As a result, if the CIA holds for the *X* variables, it also holds for the propensity score. Since we focus on the impact of a program of interest, e.g. program d_k, and use the estimators based on (7) and (8), we will state the proposition in a different way which emphasizes a program of interest.

Proposition 3'. $Y^P_D \perp d_k \mid X, \overline{D} \Rightarrow Y^P_D \perp d_k \mid P(d_k = 1 \mid X, \overline{D})$,
where $\overline{D} = D \setminus d_k$ i.e. $\overline{D}$ does not include d_k,
The proof is very similar to the case of one binary program in Rosenbaum and Rubin (1983).

Chapter 4 The impact of a governmental micro-credit program and informal credit on poverty and inequality[29]

4.1 Introduction

Micro-finance is seen as an important tool for reaching the Millennium Development Goal of halving the proportion of poor people between 1990 and 2015. Micro-credit and other financial services would enable the poor to build assets, increase incomes, and reduce their vulnerability to economic stress. Credit markets are severely rationed for poor households. Commercial banks are not interested in poor clients because of information problems and lack of collateral (Hoff and Stiglitz, 1990; Nagarajan, *et al.*, 1995; Kochar, 1997; Bell *et al.*, 1997; Bose, 1998; Boucher *et al.*, 2008). The poor do borrow from informal sources such as moneylenders, neighbours, relatives and local traders, but their resources are supposedly limited and, if charged, interest rates are mostly very high. Governments and NGOs have stepped into the gap and have provided credit for the poor, often at highly subsidized interest rates.

While there is an intuitive appeal in providing cheap funds to the poor, these subsidies have been severely criticised. Subsidized banks and programs would push out informal credit suppliers on which the poor rely (Adams *et al.*, 1984). They would also break down the rationing mechanism of the interest rate and cause credit to be allocated on the basis of politics or social concerns instead of productivity. Moreover, a steady inflow of money into financial institutions would decrease the incentives to collect savings deposits, leaving poor households with unattractive and inefficient ways to save. Critics of subsidized banks therefore argue that the poor would often have been better off without the subsidies (Armendáriz de Aghion and Morduch, 2005).

Given the wide popularity of microfinance, resulting in a large allocation of development funds, and the controversy about an essential element such as the level of the interest rate charged, it is important to evaluate the impact of ongoing programs. Yet, while there is ample anecdotal evidence consisting of individual success stories and, to a smaller extent, accounts of people who went bankrupt, the number of thorough quantitative evaluations is surprisingly limited. Although inefficient, State Banks in India are shown to have increased income for the poor (Burgess and Pande, 2002; Binswanger and Khandker, 1995). Similar results have been found for, e.g. Bangladesh (Khandker, 1998, 2003; Zaman, 2001), Indonesia (Robinson, 2001), Pakistan (Khandker and Faruqee, 2003), and a number of cases presented in the review paper of Morduch and Haley (2002). Other studies indicate that credit programs are not always effective in improving welfare and reducing poverty. For example, Diagne and Zeller (2001) did not find a statistically significant impact of micro-credit programs on household income in Malawi. Similarly, Coleman (1999) found only negligible effects on household welfare of a micro-credit program in Thailand, and Morduch (1998) showed that most of the potential effects of micro-credit from the Grameen bank in Bangladesh were on vulnerability reduction instead of poverty reduction.

[29] This chapter is written based on the paper Nguyen, V.C., Van den Berg M., and Bigman D. (2008), 'The impact of Micro-Credit and Informal Credit on Poverty and Inequality: The Case of Vietnam', which is currently submitted to a journal for possible publication.

Not only is the evidence mixed, there is no study yet that has achieved wide consensus as to its reliability (Armendáriz de Aghion and Morduch, 2005). Separating out the causal role of microfinance is extremely difficult. Microfinance programs do not lend to random citizens but carefully select areas in which they work and clients to whom they lend. Similarly, not all persons in the target group are equally interested in taking loans. Borrowers are therefore different from non-borrowers. Unfortunately, not all of these differences are easily measured. Borrowers may, for example, have a more entrepreneurial spirit and better business connections than non-borrowers. These unobserved differences, and not getting access to credit, may explain income and investment differences between borrowers and seemingly similar non-borrowers. Failing to account for this problem will lead to biased estimates of program impact, and the bias can be large.

Although microfinance programs have been set up all over the developing and even the developed world, informal credit remains popular (Nagarajan *et al.*, 1995; Kochar, 1997; Bell *et al.*, 1997; Agénor and Montiel, 1999; Conning and Udry, 2005; Guirkinger, 1998). Micro-credit programs do not require collateral, but they do screen borrowers by other eligibility criteria such as poverty status or repayment capacity. Moreover, repayment requirements are usually inflexible. As a result, not all the poor households may be able or willing to obtain micro-credit, and some may resort to informal credit. Despite the popular view of moneylenders as usurers, informal loans may help to increase capital and mitigate consumption fluctuations and thus enable the poor to grow out of poverty. While the existing lending capacity of the informal sector is supposedly limited, carefully designed government policies could help expanding available resources and thus indirectly increase the volume of informal loans. Moneylenders could be linked to banks to enable the use of formal sector money for loans to the poor (Fuentes, 1996, Varghese, 2005). On the other hand, financial policies can limit the terms and availability of informal loans: Subsidized programs may attract the best borrowers and leave the riskier clients with higher enforcement costs to non-subsidized lenders (Morduch, 1999; Hoff and Stiglitz, 1990; Bose, 1998; Jain, 1999). When designing policies to increase credit access for the poor, it is therefore important to consider not only microfinance programs and other formal sources of credit, but also the informal credit market. Yet few countries have explicit policies aimed at strengthening the informal financial sector.

Vietnam has set poverty reduction as a major goal of development policy. The poverty rate decreased remarkably from 29 percent to 16 percent during the period 2002-2006 (according to Vietnam Household Living Standard Surveys (VHLSS) in 2002, 2004, and 2006). The government has maintained an extensive public safety net system to support the poor in all dimensionalities of welfare. One of the most important antipoverty programs is the provision of credit for the poor. In 2003, the Vietnam Bank for Social Policies (VBSP) was established to provide the poor with preferential micro-credit. The poor can borrow from the bank at low interest rates without collateral. In December 2008, the total outstanding loans for the poor households were around VND 27,400 billion (VBSP, 2008). The total number of poor clients was around 8,100 thousand during 2003-2008.

In addition to the VBSP, informal credit is also an important source for people in Vietnam. In the early 1990s, informal credit accounted for more than 70 percent of total credit in the rural areas (McCarty, 2001; Pham and Lensink, 2007). The proportion of informal loans decreased over time because of the growing role of formal credit. Using a data sample of four provinces in Vietnam, Barslund and Tarp (2007) found that the informal loans still accounted for 36 percent of all loans in rural areas in 2003.

This chapter analyzes the impact of VBSP and informal credit on poverty and inequality in Vietnam, which is interesting for at least four reasons. First, the Vietnamese government has spent huge amounts of money on microfinance: in 2003 it established the Vietnam Bank for Social Policies (VBSP) to consolidate the provision of preferential micro-credit to the poor. The VBSP reportedly has received VND 1.515 trillion (US$ 100 million) in charter capital from the state budget and was scheduled to receive another VND 3.5 trillion (US$ 230 million). This funding is complemented by mandatory contributions of 2 percent of total VND deposits by the state-owned commercial banks, which will amount to approximately US$ 200 million, with rates negotiable (World Bank, 2004b). Second, nominal interest rates of VBSP are highly subsidized at about half the ‘market’ rates charged by most of the other microfinance programs (World Bank, 2007). The low and even negative real interest rates may have pushed out informal credit suppliers, weakened alternative programs, and/or caused high leakage rates to non-poor households (Burgess and Pande, 2005; Adams *et al.*, 1984). VBSP credit may therefore not only not have reached the poor, but may have also limited the availability of alternative sources of credit which otherwise would have been available. Third, only a few previous studies have analyzed the quantitative impact of credit in Vietnam, and their findings are not consistent. Quach and Mullineux (2007) used the Vietnam Living Standard Surveys (VLSS) for 1993 and 1998 to measure the impact of total borrowing from both formal and informal sources. They found that credit can help increase household expenditure. Similarly, Nguyen (2008) found that micro-credit from VBSP had positive impacts on income, consumption and poverty reduction of the borrowers in the rural areas using the Vietnam Household Living Standard Surveys (VHLSS) for 2002 and 2004. Using the two most recent VHLSS – 2004 and 2006 – Pham and Lensink (2008) come to a different conclusion. They analyze the effects of micro-credit programs and formal credit on rural self-employment profits and conclude that micro-credit does not affect household self-employment profits, while credit from commercial banks seems to help households increase their self-employment profits. These studies indicate not only that the effect of credit may have changed over time, but also that the impact differs depending on the source of credit. Fourth, informal credit has mostly been ignored in both research and policy, while it is presumably a very important source of finance for the poor, given the substantial size of the informal sector and its generally low entrance barriers. If informal credit is indeed important for the poor, the government may shift their focus at least partly away from direct provision of credit to stimulating the linkages between the formal and the informal credit market.

The present study adds to the limited existing evidence on the impact of credit in general and the contradicting and incomplete evidence of the impact of credit in Vietnam by studying the impact of the VBSP and informal credit on poverty and inequality in Vietnam using the VHLSS of 2004 and 2006. We apply fixed-effects regression using before-after program data from the Vietnam Household Living Standard Surveys (VHLSS) to account for the attribution problem described above. Based on the regressions, we compute the average effects of the VBSP and informal credit household expenditures and compute the impact of the program on poverty and inequality.

The remainder of the chapter is structured into 4 sections. The second section presents background information on the VBSP and informal credit. The third section presents the estimation method. Next, the empirical findings on impact measurement are presented in the fourth section. Finally, the fifth section concludes.

4.2 The VBSP program and informal credit

In this chapter and other empirical chapters, a household is classified as poor if their per capita expenditure is below the poverty line set up by WB and GSO. The poverty line is equivalent to the expenditure level that allows for nutritional needs with food consumption securing 2100 calories per day per person and some essential non-food consumption such as clothing and housing. The poverty lines for the years 2004 and 2006 are 2077 and 2560 thousand VND, respectively.[30]

Figure 4.1 shows that the poverty rate declined continuously over the period 1993-2006. The proportion of poor dropped dramatically from 58 percent in 1993 to 37 percent in 1998, and continued to decrease to 20 and 16 percent in 2004 and 2006, respectively. In rural areas, however, poverty was more prevalent than the country average, with a poverty rate of 20 percent in 2006. The reduction of poverty was associated with a moderate increase in inequality. The Gini index based on expenditure per capita increased from 0.33 in 1993 to 0.36 in 2006.

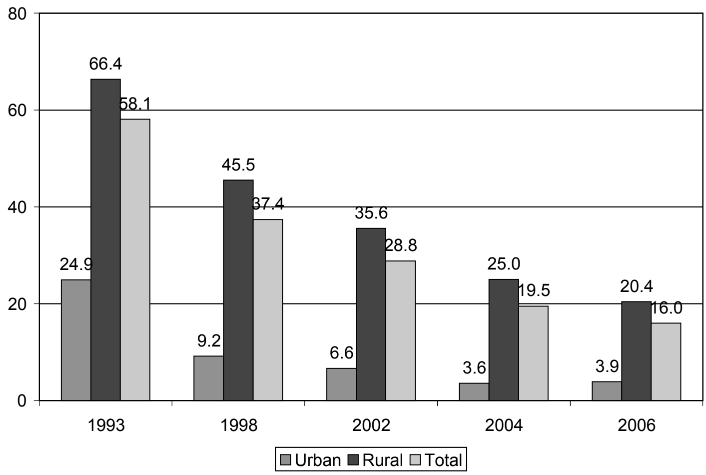

Figure 4.1. Poverty rate over the period 1993-2006 (in percent). Source: Author's calculations using VHLSS in 1993, 1998, 2002, 2004, and 2006.

The VBSP program is designed as a group-based lending scheme with credit disbursed among groups of 5 to 50 members living in a single village. The argument for the group-based design is that monitoring of loan payments by group members would lead to high

[30] 1 USD is approximately equivalent to 15,777 and 16,054 VND in January 2004 and January 2006, respectively.

repayment rates (e.g. Coleman, 1999). This strategy seems to have been successful, as reported default rates are less than 2 percent (VBSP, 2005).

The VBSP was established in 2003 as an independent public institute for the provision of government lending to the poor and other vulnerable groups. The creation of the VBSP meant a consolidation of government-lending for the poor, and since 2003 outreach and outstanding loans have increased continuously (VBSP, 2005). As indicated above, the program is highly subsidized. Average monthly interest rates increased from 0.26 to 0.36 percent during the period 2004-2006, which amounts to about 4 percent on a yearly basis. Given that inflation was 7.7 percent over 2004, this implied that the real interest rate was minus 4 percent. For the sake of comparison, commercial banks used a yearly rate of 12 percent for loans of 6-12 months with collateral.

To apply for credit, a household first sends a formatted letter to their credit group. The credit group will arrange a meeting of all members to consider the relevance of the borrowing. They will determine which household can borrow, and the credit amount for corresponding households. The list of borrowing households will be prepared by the credit group and sent to the People Committee in that commune. Once the list has been approved by the People Committee, it will be sent to a VBSP branch for loan provision. Generally, the VBSP endorses the list sent by the People Committee. Households can then receive their loans at a VBSP branch in their locality or the VBSP staff brings the loans to the households.

There are four criteria that a household should officially meet to become a member of a credit group. First, the household should have a long-term residence permit at the locality in which the group is located. Second, at least one household member should be able to work. Third, the household can receive credit on the condition that the credit is only used for income-generating activities, such as production, business, and services; the repair of a seriously damaged house; or to cover the education costs for primary and secondary school pupils. Finally, the household should be classified as poor by the local authority. The classification procedure is rather complicated. Basically, a village committee prepares a list of the poor based on their own criteria, which may for example include asset levels, food security, type of housing, and school attendance among children. The number and nature of the criteria differ widely between villages. The preliminary list is submitted to a commune-level committee of Hunger Eradication and Poverty Reduction (HEPR), which conducts an income survey for all households on the list. The resulting incomes are compared to the income poverty line of the Ministry of Labour, War Invalids and Social Affairs (MOLISA), which was set relatively low at VND 80-100 thousands per capita in rural areas for the period 2001-2005, the equivalent of about fifteen to twenty kg of rice. Those households with higher per capita income than this poverty line are not entitled to this credit. Finally, the refined list is updated by the village committee and the People's Committee and People's Council in an iterative procedure (MOLISA, 2003).

To examine whether the program reached poor households, we classified households as poor if their per capita expenditure is below the poverty line as defined by GSO and WB and then compared credit use from the VBSP between poor and non-poor households using the data from VHLSS 2004 and 2006. This is not identical to the poverty criteria used as an eligibility criterion for credit group membership. The criteria for the latter classification are partly commune-specific and therefore not consistent throughout the population. Yet the overlap between the two classifications is quite large: more than 70 percent of those classified as poor according to the commune-level classification are also considered poor using the

GSO-WB poverty line. As noted earlier, the reverse cannot be said to be true, as poverty rates are more than twice as high using the GSO-WB classification. This implies that the GSO-WB classification, which we use in the remainder of this chapter, includes most of the poor according to the commune-level classification, a formal requirement for receiving VBSP credit, and many more households.

The coverage rate of the VBSP was low: it includes only 7 percent of all households and twelve percent of the poor borrowed from the program in 2004 (Table 4.1). The share of poor people with VBSP loans has increased slightly to fifteen percent in 2006, with the overall share remaining almost constant. The average loan size was VND 3,576 thousand and 4,414 in 2004 and 2006, respectively, which was about 23 percent of household income or 1.7 times the per capita poverty line.

Table 4.1. Borrowing from VBSP.

	2004			2006		
	Poor	**Non-poor**	**Total**	**Poor**	**Non-poor**	**Total**
% houses borrowing from VBSP	11.8	5.7	6.8	14.5	5.9	7.0
	[0.8]	[0.3]	[0.3]	[1.0]	[0.3]	[0.3]
Loan size per borrowing household (thousand VND)	3,167.0	3,749.7	3,576.2	4,118.4	4,528.4	4,413.7
	[116.5]	[91.1]	[72.9]	[158.5]	[121.8]	[100.1]
Distribution of the borrowing households	29.8	70.2	100	28.0	72.0	100
	[1.9]	[1.9]		[1.8]	[1.8]	
Distribution of loan across borrowing households	26.4	73.6	100	26.1	73.9	100
	[1.9]	[1.9]		[2.0]	[2.0]	
Ratio of loan to expenditure	43.0	24.9	27.6	51.8	26.1	29.3
	[2.0]	[0.8]	[0.8]	[2.3]	[1.0]	[1.0]
Ratio of loan to income	33.4	21.9	23.8	36.9	21.0	23.2
	[1.8]	[0.7]	[0.7]	[1.6]	[0.8]	[0.8]
Monthly interest (%)	0.26	0.26	0.26	0.34	0.36	0.36
	[0.02]	[0.01]	[0.01]	[0.02]	[0.01]	[0.01]
Number of observations	1,769	7,419	9,188	1,427	7,762	9,189

Standard errors in brackets. Standard errors are corrected for sampling weights and cluster correlation.
All money metric variables are in the 2004 price.
Source: Author's estimation from VHLSS 2004 and 2006.

Leakage rates were very high. Only 30 and 28 percent of borrowing households were classified as poor in 2004 and 2006, respectively. Moreover, non-poor households on average obtained larger loans, such that in both years only 26 percent of outstanding credit was allocated to poor households, the official target group of the program. This indicates that eligibility criteria were not always upheld. According to Dufhues *et al.* (2002) credit groups and commune heads were reluctant to include poor households in the list of credit applicants as the non-poor are expected to be more reliable in using credit effectively and repaying loans. Moreover, the negative real interest rates will have added pressure to allocate loans to politically favoured residents, rather than the poor. Finally, the poor may tend to apply for fewer and lower loans than the non-poor, who have higher levels of assets and possibly skills.

There is a tendency towards contraction of informal credit. The percentage of households borrowing from informal credit was reduced from 70 percent in the early 1990s to 20 and 16 in 2004 and 2006, respectively (Table 4.2). However, compared to the VBSP credit, informal credit covered a relatively large share of both poor and non-poor households: 21 and 15 percent in 2006, respectively. The average loan size from informal sources was also higher than that from the VBSP. The non-poor had a much higher average loan size than the poor. In 2006, the informal loan size per borrowing household is VND 3,977 and 6,372 thousand, for poor and non-poor respectively. The average informal interest rate was 0.53 percent, nearly double that of VBSP credit, indicating that informal moneylenders provided an important share of informal credit.

Table 4.2. Borrowing from informal sources.

	2004			2006		
	Poor	Non-poor	Total	Poor	Non-poor	Total
% houses borrowing from informal sources	26.3	18.5	19.8	20.5	15.4	16.1
	[1.2]	[0.5]	[0.5]	[1.3]	[0.5]	[0.4]
Loan size per borrowing household (thousand VND)	3,540.5	11,111.7	9,396.3	2,968.9	11,078.6	9,676.9
	[378.3]	[1,151.8]	[899.5]	[247.9]	[748.7]	[629.2]
Distribution of the borrowing households	22.7	77.3	100	17.3	82.7	100
	[1.1]	[1.1]		[1.1]	[1.1]	
Distribution of loan across borrowing households	8.5	91.5	100	5.3	94.7	100
	[1.3]	[1.3]		[0.7]	[0.7]	
Ratio of loan to expenditure	45.9	63.1	61.5	38.0	53.8	52.8
	[4.9]	[9.5]	[8.6]	[3.3]	[3.5]	[3.2]
Ratio of loan to income	33.8	50.0	48.3	26.2	42.6	41.4
	[3.5]	[7.5]	[6.7]	[2.3]	[2.9]	[2.6]
Monthly interest (%)	0.60	0.62	0.61	0.85	0.51	0.53
	[0.11]	[0.09]	[0.08]	[0.19]	[0.05]	[0.05]
Number of observations	1,769	7,419	9,188	1,427	7,762	9,189

Standard errors in brackets. Standard errors are corrected for sampling weights and cluster correlation.
Source: Author's estimation from VHLSS 2004 and 2006.

An interesting style is the coexistence of VBSP and informal credit. Among borrowers from the VBSP, the ratio of households also receiving informal credit was 18 percent in 2006 (Table 4.3). This may indicate that credit from VBSP is not sufficient for households, and they have to resort to informal credit. Similarly, households who do not borrow from VBSP may use informal sources of credit. In 2006, around 16 percent of the non-borrowers from VBSP obtained informal credit.

One important issue in examining the effectiveness of the credit is the use of credit. Tables 4.3 and 4.4 tabulate the loan size by the use purposes which are reported by households. Although credit is fungible, these tables might give some insight into how the credit are used. It shows that a large proportion of VBSP credit is used for investment and production capital. In 2006, the poor and non-poor households used 62 and 43 percent of VBSP loans for agricultural production and investment. However, the non-poor spent more credit on non-farm activities. In 2006, the poor and non-poor used 2 and 15 percent of the VBSP credit for non-farm production and investment, respectively. Credit was also used for debt repayment and important needs such as house construction, healthcare and education. However, the food-poor households report that 7.4 percent of credit is used for consumption.

Table 4.3. The use of VBSP credit.

Activities	**2004**			**2006**		
	Poor	**Non-poor**	**Total**	**Poor**	**Non-poor**	**Total**
Investment and production						
Agriculture/Fishery/Aquaculture	56.5	44.5	47.6	61.7	42.8	47.8
	[4.1]	[2.6]	[2.2]	[4.2]	[2.7]	[2.3]
Service and business	4.9	9.0	7.9	1.5	12.1	9.3
	[1.5]	[1.5]	[1.1]	[0.8]	[2.0]	[1.5]
Other non-farm activities	2.0	5.5	4.6	0.7	2.9	2.3
	[0.9]	[1.3]	[1.0]	[0.5]	[0.9]	[0.7]
Consumption						
Debt repayment	8.0	7.1	7.3	8.1	7.0	7.3
	[2.3]	[1.5]	[1.2]	[2.0]	[1.2]	[1.1]
House construction/purchase	13.9	9.7	10.8	11.3	7.9	8.8
	[3.1]	[1.6]	[1.5]	[2.7]	[1.4]	[1.3]
Education	0.9	4.6	3.6	0.5	3.3	2.6
	[0.7]	[1.2]	[0.9]	[0.3]	[0.8]	[0.6]
Healthcare	1.0	6.2	4.8	1.2	4.1	3.3
	[0.7]	[1.2]	[0.9]	[0.7]	[0.9]	[0.7]
Durable appliances	2.4	4.3	3.8	2.6	5.3	4.6
	[1.0]	[1.1]	[0.9]	[1.4]	[1.4]	[1.1]
Other consumption	10.4	9.2	9.5	12.4	14.6	14.0
	[2.5]	[1.5]	[1.3]	[3.3]	[1.9]	[1.6]
Total	100	100	100	100	100	100
Number of observations	241	484	725	225	537	762

Standard errors in brackets. Standard errors are corrected for sampling weights and cluster correlation.
Source: Author’s estimation from VHLSS 2004 and 2006.

Compared to VBSP credit, a smaller proportion of informal credit was used in production and investment. In 2006, the poor and non-poor households used 22 and 10 percent of informal loans for agricultural production and investment, respectively. Regarding non-farm production, the poor and non-poor used around 3 and 18 percent of the informal credit. Most informal loans are used in consumption, especially house construction and purchase.

Table 4.4. The use of informal credit.

Activities	2004			2006		
	Poor	Non-poor	Total	Poor	Non-poor	Total
Investment and production						
Agriculture/Fishery/Aquaculture	24.1	11.5	12.5	21.5	10.0	10.6
	[3.8]	[2.0]	[1.9]	[3.9]	[1.5]	[1.4]
Service and business	3.6	24.2	22.4	1.6	15.9	15.1
	[1.4]	[6.6]	[6.2]	[1.0]	[3.4]	[3.3]
Other non-farm activities	0.7	11.0	10.1	1.7	2.4	2.4
	[0.4]	[4.4]	[4.1]	[0.9]	[0.7]	[0.7]
Consumption						
Debt repayment	5.3	4.1	4.2	10.3	6.0	6.2
	[1.3]	[1.1]	[1.0]	[2.7]	[1.2]	[1.2]
House construction/purchase	29.4	21.7	22.4	36.4	31.7	32.0
	[5.7]	[3.3]	[3.1]	[5.5]	[3.1]	[3.0]
Education	0.6	1.7	1.6	2.7	3.0	2.9
	[0.3]	[0.3]	[0.3]	[1.1]	[0.5]	[0.5]
Healthcare	7.5	8.0	7.9	5.4	7.8	7.6
	[1.7]	[1.1]	[1.0]	[1.6]	[1.2]	[1.1]
Durable appliances	5.7	2.7	3.0	1.5	3.5	3.4
	[1.7]	[0.5]	[0.5]	[0.8]	[0.6]	[0.6]
Other consumption	23.1	15.3	15.9	19.0	19.7	19.7
	[6.6]	[2.5]	[2.4]	[2.8]	[2.7]	[2.6]
Total	100	100	100	100	100	100
Number of observations	498	1,572	2,070	305	1,363	1,668

Standard errors in brackets. Standard errors are corrected for sampling weights and cluster correlation.

Source: Author's estimation from VHLSS 2004 and 2006.

4.3 Impact evaluation methodology

4.3.1 Impact of credit on expenditures

To assess the impact of VBSP and informal credit, we assume welfare can be specified as follows:

$$Y_{ijt} = \beta_0 + G_t\beta_1 + X_{ijt}\beta_2 + D_{ijt}\beta_3 + C_{jt}\beta_4 + \eta_{ijt} \,, \qquad (1)$$

where Y is expenditure per capita. The subscripts i, j and t refer to household i in commune j at time t, respectively. Note that 'per capita' refers to the average per household member at period t. Per capita expenditure is thus calculated as total household expenditures at period t over the number of household members at period t. G_t is a year dummy, with a one for 2006; this dummy allows common macroeconomic changes between the two years to be controlled for. X and C are vectors of household and community level control variables. The vector D covers per capita VBSP credit and informal credit (i.e. average loan size per household member at period t).

It should be noted that empirical studies sometime assume that income or expenditure follows a log-normal distribution, then (1) becomes:

$$\ln(Y_{ijt}) = \beta_0 + G_t\beta_1 + X_{ijt}\beta_2 + D_{ijt}\beta_3 + C_{jt}\beta_4 + \eta_{ijt} \qquad (1')$$

However, we do not use this semi-log function in this chapter or other chapters, since this function imposes an unrealistic assumption on the increasing marginal impact of credit on income or consumption. We in fact experimented with including squared credit, and interactions between credit and other control variables (*X*) in (1’). However, since these terms appear to be insignificant, we do not present these results. Thus, we do not use semi-log functions. In addition, we do not estimate a double-logs function:

$$\ln(Y_{ijt}) = \beta_0 + G_t\beta_1 + X_{ijt}\beta_2 + \ln(D_{ijt})\beta_3 + C_{jt}\beta_4 + \eta_{ijt}, \qquad (1'')$$

since there are many households without credit, and taking a logarithm of zero returns missing values.

The main problem in estimating Equation (1) is the endogeneity of program participation. Borrowing can be correlated with unobserved characteristics of households, such as motivation for higher income or abilities and skills in business. Failure to control for such factors leads to biased estimates of program impact: if it is, for example, the better entrepreneurs who take a loan, and we do not directly include information on managerial capacity in our regression (because it is not available), a significant and positive coefficient for program participation is at least partly caused by these capacity differences and not by the program itself.

In this study, we use the panel nature of the data to avoid endogeneity bias. A main assumption of the method used is that unobserved variables that are correlated with both outcome and program variables remained unchanged during the period 2004-2006, which is covered by the panel. We feel that it is reasonable to assume that the relevant variables, such as business and production skills or motivation for higher income, were time-invariant during such a short period of time.

To show how the panel nature of the data helps solving the endogeneity problem, suppose the error term can be split into two components: a combined household and commune specific error, $u_{ij} + v_j$, which is correlated with *D* but stable over time, and ε_{ijt}, which is uncorrelated with *D* but is allowed to change over time. Equation (1) then becomes

$$Y_{ijt} = \beta_0 + G_t\beta_1 + X_{ijt}\beta_2 + D_{ijt}\beta_3 + C_{jt}\beta_4 + u_{ij} + v_j + \varepsilon_{ijt}, \qquad (2)$$

or alternatively

$$Y_{ijt} = \beta_{0ij} + G_t\beta_1 + X_{ijt}\beta_2 + D_{ijt}\beta_3 + C_{jt}\beta_4 + \varepsilon_{ijt}, \qquad (3)$$

which can be estimated without bias using fixed effects techniques. Please note that this method will fail to eliminate all endogeneity bias if the unobserved variables affect not only the level of the outcome but also its growth rate. Also, given the dynamic nature of the Vietnamese economy, income opportunities may have changed between 2004 and 2006, and, depending on unobserved characteristics, some households may be better able to use these than other households. Yet, we are confident that the estimation bias possibly resulting from these factors is relatively small.

The marginal impact of credit is measured by β_3. We will also measure the impact of credit by calculating the Average Treatment Effect on the Treated (ATT) (Heckman *et al.*, 1999). ATT is the expected impact of credit on borrowers (with D>0):

$$ATT_t = E\left(Y_{ijt} \middle| D_{ijt} > 0\right) - E\left(Y_{ijt(D=0)} \middle| D_{ijt} > 0\right), \qquad (4)$$

Where $E(Y_{ijt(D=0)} | D_{ijt} > 0)$ is the expected value of the outcome variable of the borrowers, i.e. expenditure per capita had they not received credit. This is not observed and has to be estimated.

Using Equation (1), we get

$$\begin{aligned} ATT_t &= E\left(Y_{ijt}\middle|D_{ijt}>0\right)-E\left(Y_{ijt(D=0)}\middle|D_{ijt}>0\right) \\ &= E\left(\beta_{0ij}+G_t\beta_1+X_{ijt}\beta_2+D_{ijt}\beta_3+C_{jt}\beta_4+\varepsilon_{ijt}\middle|D_{ijt}>0\right) \\ &\quad -E\left(\beta_{0ij}+G_t\beta_1+X_{ijt}\beta_2+C_{jt}\beta_4+\varepsilon_{ijt}\middle|D_{ijt}>0\right) \\ &= D_{ijt}\beta_3. \end{aligned} \tag{5}$$

The ATT at time t is thus estimated by:

$$A\hat{T}T_t = \frac{1}{n_t}\sum_{i=1}^{n_t} D_{ijt}\hat{\beta}_3 , \tag{6}$$

where n_t is the number of the borrowers at the time t.

We estimate the standard error of the ATT estimates by using a non-parametric bootstrap technique. This bootstrap is implemented by repeatedly drawing samples from the original sample of the VHLSS panel data. Since the VHLSS sample selection follows stratified random cluster sampling, communes instead of households are bootstrapped in each stratum (Deaton, 1997). In other words, the bootstrap is made of communes (i.e. clusters) within strata. The number of replications is 500.[31]

4.3.2 The impact of credit on poverty and inequality

We calculate poverty by three Foster-Greer-Thorbecke poverty indexes, which can all be calculated using the following formula (Foster, Greer and Thorbecke, 1984):

$$P_\alpha = \frac{1}{n}\sum_{i=1}^{q}\left[\frac{z-Y_i}{z}\right]^\alpha , \tag{7}$$

where Y_i is a welfare indicator for person i. We use consumption expenditure per capita as the welfare indicator, since, as is well known, consumption is a better proxy for well-being than income. z is the expenditure poverty line, n is the number of people in the sample population, q is the number of poor people, and α can be interpreted as a measure of inequality aversion.

When $\alpha = 0$, we have the headcount index H, which measures the proportion of people below the poverty line. When $\alpha = 1$ and $\alpha = 2$, we obtain the poverty gap PG, which measures the depth of poverty, and the squared poverty gap P_2 which measures the severity of poverty, respectively.

To measure inequality, we use three common measures of inequality: the Gini coefficient, Theil's L index of inequality, and Theil's T index of inequality. The Gini index can be calculated from the individual expenditure in the population:

$$G = \frac{1}{n(n-1)\overline{Y}}\sum_{i>j}\sum_{j}\left|Y_i - Y_j\right| \tag{8}$$

where $\overline{Y}$ is the average per capita expenditure. The double sum in (8) can be hard to calculate if n is relatively large, and an equivalent but computationally more convenient formula is (Deaton, 1997):

$$G = \frac{n+1}{n-1} - \frac{2}{n(n-1)\overline{Y}}\sum_{i=1}^{n}\rho_i Y_i \tag{9}$$

[31] In order to examine the robustness of our bootstrap technique, we also tried to bootstrap households. The results were similar.

where ρ_i is the rank of person *i* in the *Y*-distribution, counting from the richest so that the richest has the rank of 1.

The value of the Gini coefficient varies from 0 when everyone has the same income to 1 when one person has everything. The closer a Gini coefficient is to one, the more unequal is the income distribution.

The Theil L index of inequality is calculated as follows:

$$Theil\_L = \frac{1}{n}\sum_{i=1}^{n}\ln\left(\frac{\bar{Y}}{Y_i}\right), \qquad (10)$$

The Theil L index ranges from 0 to infinity. A higher value of Theil L indicates more inequality.

The Theil T index of inequality is calculated as:

$$Theil\_T = \frac{1}{n}\sum_{i=1}^{n}\frac{Y_i}{\bar{Y}}\ln\left(\frac{Y_i}{\bar{Y}}\right) \qquad (11)$$

The Theil T index ranges from 0 (lowest inequality) to ln(N) (highest inequality).

The impact of credit on the poverty indices of borrowers in period *t* is calculated as follows:

$$\Delta P = P(D_t > 0, Y_t) - P(D_t > 0, Y_{t(D=0)}), \qquad (12)$$

where the first term on the right-hand side of (12) is the poverty measure of the credit receiving households given their credit. This term is observed and can be computed directly from the sample data. However, the second term on the right-hand side of (12) is the counterfactual measure of poverty, i.e. poverty indexes of the borrower had they not borrowed. This term is not observed directly, and is estimated by using equation (1), and substituting these estimates of expenditure into Equation (7).

We also measure the impact of credit on total poverty:

$$\Delta P = P(Y_t) - P(Y_{t(D=0)}), \qquad (13)$$

where $P(Y_t)$ is the observed poverty index of the entire population and $P(Y_{t(D=0)})$ is the poverty index of the entire population if the borrower had not received the credit. The difference between Equations (13) and (12) is that the latter only looks at the effect on borrowers, while the former considers the effect on the entire population. Regarding inequality, we only measure the impact of credit on inequality of the entire population. The impact on the inequality index is given by:

$$\Delta I = I(Y_t) - I(Y_{t(D=0)}), \qquad (14)$$

where $I(Y_t)$ is observed inequality, which is calculated using the observed expenditure data. $I(Y_{t(D=0)})$ is an inequality index in the absence of the credit, which is estimated using the predicted counterfactual expenditure without the credit, using equation (1). The standard errors of the estimates of impacts on poverty and inequality are estimated using the same bootstrap technique as for ATT.

4.4 Credit impact

4.4.1 VBSP credit

To estimate the effects of VBSP and informal credit on per capita expenditure, we regress per capita expenditure on per capita VBSP credit and per capita informal credit and a set of control variables. Control variables include household composition, education of household members, land, villages, urbanity, credit from other sources and regional variables. It should be noted that control variables should be exogenous to credit (Heckman *et al.*, 1999; Ravallion, 2001). Thus, several asset variables such as living areas and housing types are not included as control variables since these variables can be affected by credit (Tables 4.3 and 4.4 show that some households reported the use of credit for housing construction and purchase). We tested whether VBSP credit and informal credit had a different impact in rural and urban areas by including interaction terms for the two types of credit and a dummy for living in an urban area. These estimates indicate that the effects of credit do not differ between urban and rural areas. We, therefore, only present the estimates for the entire sample.

The list of the variables and summary statistics for the borrowing and non-borrowing households are presented in Tables 4.7 and 4.8 in Appendix 4.1. In order to control for inflation, we have deflated all variables in terms of 2004 prices. Table 4.9 presents the regression results. We present both random effects and fixed effects estimates, without and with sampling weight and cluster correlation. Since the Hausman tests strongly favour the fixed effects estimates we focus the discussion on the fixed effects estimates with survey corrections.

The estimates indicate that VBSP credit did not significantly affects borrowers' expenditures, and therefore had no effect on poverty and inequality. This confirms the finding of Pham and Lensink (2008) that VBSP credit did not affect profits from self-employment in the period 2004-2006. Nguyen (2008), on the other hand, found a positive impact of VBSP credit on rural income, consumption and poverty using the VHLSS for 2002 and 2004. When we, however, repeated his estimates correcting for both household and year effects, the significance disappeared. A reason for the lack of impact of VBSP may be that people spend the subsidized money less productively than they claim. Alternatively, loans were used for longer term investments and impact can only be measured when considering a period longer than twelve months, but this is somewhat at odds with the short-term nature of the loans.

4.4.2 Informal credit

Contrary to VBSP credit, informal credit had a positive and statistically significant impact on household expenditure. An increase of 1 VND in per capita informal credit resulted in an increase of 0.05 VND in per capita expenditure, indicating an average rate of return of 5 percent. This is somewhat surprising, as about two-thirds of the households borrowing in the informal sector said that they used the loan for consumption. However, rates of return to investment can be up to 100 percent or higher for relatively poor households, and it appears that at least part of the loans was (indirectly) used for production.

At the household level, informal credit on average increased per capita expenditure by 3 percent, or more precisely 112 and 118 thousand VND in 2004 and 2006, respectively, the difference being the result of a somewhat higher average loan size in the latter year (Table 4.5).

Table 4.5. Impact of informal credit on expenditures per capita measured by ATT.

Year	Y_1	Y_0	ATT (Y_1–Y_0)
2004	3,701.1***	3,589.3***	111.8**
	[69.5]	[83.8]	[56.9]
2006	4,279.8***	4,161.6***	118.2**
	[100.3]	[115.1]	[59.3]

** significant at 5%; *** significant at 1%.
Standard errors in brackets. Standard errors are corrected for sampling weights and estimated using bootstrap (non-parametric) with 500 replications.
Source: Estimation from VHLSS 2004 and 2006.

Table 4.6. Impact of informal credit on poverty and inequality.

	2004			2006		
	With credit	Without credit	Impact	With credit	Without credit	Impact
Poverty of borrowers						
P0	0.2532***	0.2688***	-0.0156*	0.1972***	0.2102***	-0.0138*
	[0.0127]	[0.0154]	[0.0092]	[0.0121]	[0.0151]	[0.0083]
P1	0.0574***	0.0651***	-0.0077	0.0468***	0.0505***	-0.0037
	[0.0040]	[0.0070]	[0.0059]	[0.0040]	[0.0048]	[0.0025]
P2	0.0204***	0.0332***	-0.0129	0.0170***	0.0186***	-0.0016
	[0.0020]	[0.0429]	[0.0430]	[0.0018]	[0.0025]	[0.0017]
All poverty						
P0	0.1949***	0.1981***	-0.0032*	0.1597***	0.1619***	-0.0022
	[0.0053]	[0.0056]	[0.0019]	[0.0051]	[0.0053]	[0.0015]
P1	0.0472***	0.0488***	-0.0016	0.0383***	0.0389***	-0.0006
	[0.0017]	[0.0021]	[0.0012]	[0.0017]	[0.0018]	[0.0004]
P2	0.0170***	0.0196***	-0.0026	0.0137***	0.0140***	-0.0003
	[0.0009]	[0.0087]	[0.0087]	[0.0009]	[0.0009]	[0.0003]
All inequality						
Gini	0.3698***	0.3707***	-0.0008	0.3580***	0.3584***	-0.0005
	[0.0040]	[0.0041]	[0.0007]	[0.0040]	[0.0040]	[0.0004]
Theil L	0.2235***	0.2243***	-0.0007	0.2117***	0.2123***	-0.0006
	[0.0050]	[0.0050]	[0.0005]	[0.0049]	[0.0049]	[0.0004]
Theil T	0.2407***	0.2417***	-0.0010	0.2268***	0.2274***	-0.0006
	[0.0065]	[0.0066]	[0.0009]	[0.0071]	[0.0071]	[0.0005]

* significant at 10%; *** significant at 1%.
Standard errors in brackets. Standard errors are corrected for sampling weights and estimated using bootstrap (non-parametric) with 500 replications.
Source: Author's estimation from VHLSS 2004 and 2006.

As 21 percent of poor households obtained informal credit, at the national level this translated into a decrease in the head count index of poverty for borrowers by around 1.6 and 1.4 percentage points in 2004 and 2006, respectively (Table 4.6). The effects on the other poverty indicators are all negative but very small and mostly not statistically significant. As the non-poor also used informal credit, it did not significantly affect inequality.

4.5 Conclusions

The provision of subsidized loans without formal collateral requirement through the VBSP forms a cornerstone of Vietnam's anti-poverty policy. Yet, little is known about the ultimate impact of these preferential loans on poverty and inequality, as most evaluation reports simply describe the implementation and outputs of the program. Even less information is available on the second important source of credit for the poor: informal loans from moneylenders, relatives and friends. While more expensive, these loans may be more easily accessible for the poor than subsidized loans, which could be siphoned off by wealthier households. Although it is possible to stimulate the availability of informal credit, the Vietnamese government has no policies to do so and its current cheap credit policy may even inhibit the functioning of the informal credit markets by taking its best clients. If indeed informal credit is an important means to increase expenditures for the poor, possibly even more important than subsidized credit, the government may want to reconsider its policy focus.

We use fixed-effects regression to estimate the average effect of informal and VBSP credit on the expenditure of participating households, and subsequently assess their impact on poverty and inequality. In doing so, we intend to eliminate the potential bias caused by differences between participants and non-participants in credit markets. As with similar impact studies before us, the reliability of our estimates may still be disputed. Fixed-effects regression only eliminates endogeneity bias caused by unobserved variables that remained unchanged between survey rounds and that have an additive effect on the outcome. We feel that it is reasonable to assume that the relevant household-level variables, such as business and production skills or motivation for higher income, were time-invariant during the two periods covered in this study. Fixed-effects regressions will, however, fail to eliminate all endogeneity bias if the unobserved variables affect not only the level of the output but also its growth rate. Similarly, depending on unobserved characteristics, some households may be better able to benefit from new opportunities arising between survey rounds than other households are. We are however confident that the estimation bias resulting from these factors is small relative to the bias eliminated by using fixed-effects regression.

We find that the impact of credit on poverty was limited, and the impact on inequality zero. Less than 30 percent of VBSP loans ended up in the hands of the poor. Seven percent of poor households obtained loans from the VBSP, compared to 15 percent that borrowed in the informal sector. Also average loan size was much higher in the informal sector. Not surprisingly, we therefore find that informal credit was most effective in decreasing poverty: it reduced the poverty incidence of borrowers by 1.6 and 1.4 percentage points (or equivalent to 5.9 and 6.6 percent) in 2004 and 2006, respectively, whereas we did not find evidence of an effect of VBSP credit on poverty.

The complete absence of any effect of VBSP credit not only on poverty, but also on the expenditure of recipient households is somewhat surprising, especially since most households claim to use the money in productive activities. Yet these results are in accordance with the finding of Pham and Lensink (2008) that VBSP credit did not affect profits from self employment. Money is fungible, and it is possible that people did not use the loans as productively as they claimed. Alternatively, the effects of VBSP credit may only be measurable over a longer time period, despite the short-term nature of the loans. If the VBSP credit is used to buy production inputs, it is difficult to detect a significant impact on current income and expenditure in the short term. Even so, we would not expect a large negative effect on poverty and inequality, as less than one third of the loans went to the poor and loan size was small compared to informal sources.

Summarizing, we found limited evidence of a positive role of credit in achieving the government objectives of decreasing poverty and inequality. It seems that if credit is to play this role, informal credit is a more likely candidate than government subsidized credit: it already reaches more poor people, provides them with more money, and, contrary to VBSP credit, increases their expenditure. An alternative candidate would be microfinance from non-governmental programs, a source that we have not considered in this chapter. Yet Pham and Lensink (2008) find that, just like VBSP credit, loans from other microfinance programs have no significant impact on self-employment profits. Based on existing studies, informal credit thus seems the best candidate to be a tool for poverty alleviation in Vietnam. It is not clear whether this is also the case for other countries: studies analyzing the effects of informal credit are extremely rare. The results we have obtained for Vietnam justify more research efforts in this direction.

While not directly under public control, financial intermediation through informal lenders is not immune to public policies. Governments can facilitate intermediation through the provision of an important basic infrastructure, such as a system of laws and courts to support the creation and enforcement of property rights and contracts, credit bureaus to publicize information, and prudential regulation of financial institutions (Conning and Udry, 2005). This is, however, not an easy task. Excessive regulation inhibits innovation and raises the costs of intermediation. Moreover, increased competition may undermine previously self-enforcing financial arrangements, unless agents can enter into exclusive contracts (Conning and Udry, 2005). Put differently, new financial institutions or expansion of existing institutions potentially harms the informal sector. This study therefore provides a careful warning against an overenthusiastic adherence to the microfinance miracle.

Appendix 4.1 Descriptive statistics and regression results

Table 4.7. Descriptive statistics of households with and without VBSP credit.

Variables	Type	2004		2006	
		Household with VBSP credit	Household without VBSP credit	Household with VBSP credit	Household without VBSP credit
Household variables					
Ratio of members younger than 16 to total household members	Continuous	0.2978 [0.0082]	0.2633 [0.0027]	0.2928 [0.0092]	0.2369 [0.0027]
Ratio of members older than 60 to total household members	Continuous	0.0593 [0.0048]	0.0962 [0.0020]	0.0592 [0.0047]	0.0999 [0.0020]
Household size	Discrete	5.2395 [0.1117]	4.9945 [0.0271]	5.0383 [0.0687]	4.8531 [0.0304]
Ratio of members with technical degree to total household members	Continuous	0.0463 [0.0052]	0.0587 [0.0020]	0.0528 [0.0052]	0.0682 [0.0021]
Ratio of members with post secondary to total household members	Continuous	0.0077 [0.0018]	0.0343 [0.0018]	0.0124 [0.0021]	0.0360 [0.0017]
Area of annual crop land per capita (m^2)	Continuous	688.5 [36.4]	667.2 [19.6]	740.9 [44.2]	691.6 [20.9]
Area of perennial crop land per capita (m^2)	Continuous	144.6 [38.4]	206.4 [15.5]	183.1 [26.9]	242.7 [15.0]
Forestry land per capita (m^2)	Continuous	432.7 [113.3]	175.8 [21.9]	375.7 [68.3]	200.9 [27.5]
Aquaculture water surface per capita (m^2)	Continuous	25.6 [7.4]	62.2 [7.6]	36.6 [13.6]	63.8 [8.4]
Other credit (thousand VND)	Continuous	116.4 [22.2]	903.3 [54.5]	293.1 [43.1]	1182.9 [97.6]
Commune variables					
Road to village (yes = 1)	Binary	0.6742 [0.0223]	0.5971 [0.0099]	0.7251 [0.0209]	0.6281 [0.0098]
Distance to nearest daily market (km)	Continuous	3.7390 [0.4263]	2.0758 [0.0926]	3.9762 [0.4525]	2.2117 [0.1033]
Regional variables					
Household in Red River Delta	Binary	0.1721 [0.0176]	0.2216 [0.0082]	0.1152 [0.0149]	0.2243 [0.0083]
Household in North East	Binary	0.2309 [0.0197]	0.1048 [0.0052]	0.1984 [0.0186]	0.1082 [0.0054]
Household in North West	Binary	0.0651 [0.0111]	0.0267 [0.0026]	0.0692 [0.0111]	0.0287 [0.0028]
Household in North Central Coast	Binary	0.1734 [0.0197]	0.1253 [0.0070]	0.2125 [0.0212]	0.1252 [0.0071]
Household in South Central Coast	Binary	0.1197 [0.0148]	0.0828 [0.0050]	0.0793 [0.0115]	0.0850 [0.0052]
Household in Central Highlands	Binary	0.0456 [0.0087]	0.0574 [0.0044]	0.0854 [0.0139]	0.0581 [0.0045]
Household in North East South	Binary	0.0559 [0.0104]	0.1672 [0.0085]	0.0885 [0.0162]	0.1652 [0.0084]
Household in Mekong River Delta	Binary	0.1373 [0.0166]	0.2142 [0.0081]	0.1516 [0.0157]	0.2053 [0.0079]
Household in Living in urban areas	Binary	0.1309 [0.0151]	0.2680 [0.0093]	0.1738 [0.0174]	0.2749 [0.0093]
Observations		705	8,483	747	8,442

Standard errors in brackets.
Source: Author's estimation from VHLSS 2004 and 2006.

Table 4.8. Descriptive statistics of households with and without informal credit.

Variables	Type	2004		2006	
		Household with informal credit	Household without informal credit	Household with informal credit	Household without informal credit
Household variables					
Ratio of members younger than 16 to total household members	Continuous	0.2970	0.2579	0.2735	0.2347
		[0.0058]	[0.0028]	[0.0062]	[0.0027]
Ratio of members older than 60 to total household members	Continuous	0.0748	0.0983	0.0670	0.1027
		[0.0035]	[0.0021]	[0.0037]	[0.0021]
Household size	Discrete	5.0194	5.0105	4.8786	4.8650
		[0.0593]	[0.0294]	[0.0551]	[0.0321]
Ratio of members with technical degree to total household members	Continuous	0.0568	0.0581	0.0636	0.0677
		[0.0037]	[0.0020]	[0.0048]	[0.0021]
Ratio of members with post secondary to total household members	Continuous	0.0150	0.0368	0.0183	0.0374
		[0.0020]	[0.0019]	[0.0024]	[0.0018]
Area of annual crop land per capita (m^2)	Continuous	573.9	692.9	627.6	708.9
		[31.0]	[20.7]	[35.6]	[21.8]
Area of perennial crop land per capita (m^2)	Continuous	183.2	206.7	221.7	241.5
		[27.8]	[15.6]	[24.2]	[15.4]
Forestry land per capita (m^2)	Continuous	170.4	200.6	175.9	221.8
		[36.3]	[25.6]	[32.4]	[30.5]
Aquaculture water surface per capita (m^2)	Continuous	27.6	67.6	43.0	65.4
		[5.2]	[8.7]	[13.9]	[9.0]
Other credit (thousand VND)	Continuous	633.6	900.2	655.9	1207.0
		[96.9]	[57.6]	[83.7]	[106.7]
Commune variables					
Road to village (yes = 1)	Binary	0.6735	0.5846	0.7090	0.6208
		[0.0151]	[0.0103]	[0.0157]	[0.0101]
Distance to nearest daily market (km)	Continuous	2.3459	2.1588	2.7613	2.2634
		[0.1534]	[0.1049]	[0.2761]	[0.1091]
Regional variables					
Household in Red River Delta	Binary	0.2606	0.2071	0.2437	0.2105
		[0.0138]	[0.0082]	[0.0145]	[0.0082]
Household in North East	Binary	0.1172	0.1132	0.1340	0.1113
		[0.0092]	[0.0057]	[0.0108]	[0.0056]
Household in North West	Binary	0.0244	0.0308	0.0302	0.0321
		[0.0040]	[0.0030]	[0.0056]	[0.0031]
Household in North Central Coast	Binary	0.1536	0.1225	0.1513	0.1280
		[0.0132]	[0.0070]	[0.0134]	[0.0073]
Household in South Central Coast	Binary	0.0650	0.0907	0.0579	0.0899
		[0.0073]	[0.0056]	[0.0075]	[0.0055]
Household in Central Highlands	Binary	0.0772	0.0512	0.0885	0.0545
		[0.0089]	[0.0042]	[0.0101]	[0.0044]
Household in North East South	Binary	0.1400	0.1640	0.1370	0.1638
		[0.0129]	[0.0087]	[0.0133]	[0.0086]
Household in Mekong River Delta	Binary	0.1620	0.2205	0.1574	0.2099
		[0.0111]	[0.0084]	[0.0117]	[0.0081]
Household in Living in urban areas	Binary	0.2138	0.2693	0.1999	0.2806
		[0.0137]	[0.0096]	[0.0139]	[0.0095]
Observations		1,791	7,397	1,473	7,716

Standard errors in brackets.
Source: Author's estimation from VHLSS 2004 and 2006.

Table 4.9. Regressions of per capita expenditures.

Explanatory variables	Random effect (no sampling weight)	Fixed-effect (no sampling weight)	Fixed-effect with sampling weight and cluster correlation	Random effect (no sampling weight)	Fixed-effect (no sampling weight)	Fixed-effect with sampling weight and cluster correlation
VBSP credit (thousand VND)	-0.045	0.159*	0.107	-0.046	0.086	0.036
	[0.085]	[0.093]	[0.102]	[0.080]	[0.091]	[0.095]
Informal credit (thousand VND)	0.073***	0.045***	0.041	0.072***	0.053***	0.048**
	[0.014]	[0.015]	[0.025]	[0.013]	[0.016]	[0.024]
Ratio of members younger than 16 to total household members				-1,649.79***	-537.467*	-429.962
				[178.755]	[324.129]	[350.014]
Ratio of members older than 60 to total household members				-828.979***	-848.395**	-767.033
				[155.768]	[352.431]	[674.304]
Household size				-632.301***	-984.302***	-
				[65.091]	[108.114]	[169.066]
Household size squared				30.182***	51.914***	65.632***
				[5.788]	[9.390]	[15.154]
Ratio of members with technical degree to total household				3,196.43***	923.764***	945.558**
				[196.832]	[279.558]	[419.991]
Ratio of members with post secondary to total household				8,390.74***	1,550.06***	1,568.71
				[292.248]	[515.527]	[1,059.221]
Area of annual crop land per capita (m^2)				0.098***	0.097***	0.088***
				[0.020]	[0.033]	[0.026]
Area of perennial crop land per capita (m^2)				0.142***	0.109***	0.112***
				[0.024]	[0.034]	[0.035]
Forestry land per capita (m^2)				-0.008	-0.027	-0.033***
				[0.013]	[0.019]	[0.010]
Area of aquaculture water surface per capita (m^2)				0.127***	0.008	0.01
				[0.049]	[0.065]	[0.065]
Other credit (thousand VND)				0.035***	0.016**	0.016
				[0.005]	[0.006]	[0.010]
Road to village (yes = 1)				39.041	9.776	86.264
				[87.570]	[109.494]	[131.785]
Distance to nearest daily market (km)				-11.164**	-2.471	-3.638
				[4.999]	[6.086]	[3.215]
Red River Delta	Base omitted					
North East				-693.759***		
				[128.506]		
North West				-1,184.547***		
				[199.361]		
North Central Coast				-678.43***		
				[135.917]		
South Central Coast				-126.727		
				[145.769]		
Central Highlands				-595.504***		
				[177.455]		
North East South				1,238.25***		
				[137.073]		
Mekong River Delta				123.716		
				[119.813]		
Urban				2,221.648***		
				[118.748]		
Time effect (2006 variable)	627.071***	621.833***	617.212***	468.800***	533.039***	520.804***
	[40.779]	[40.692]	[52.756]	[41.045]	[41.333]	[56.646]
Constant	4,296.560***	4,292.421***	4,553.957***	5,905.78***	7,492.23***	8,050.05***
	[54.577]	[29.779]	[28.586]	[210.578]	[315.777]	[457.584]

Explanatory variables	Random effect (no sampling weight)	Fixed-effect (no sampling weight)	Fixed-effect with sampling weight and cluster correlation	Random effect (no sampling weight)	Fixed-effect (no sampling weight)	Fixed-effect with sampling weight and cluster correlation
Observations	8,432	8,432	8,432	8,432	8,432	8,432
Number of i	4,216	4,216	4,216	4,216	4,216	4,216
R-squared	0.01	0.01	0.01	0.41	0.15	0.15
Hausman test χ^2 (Prob) (H0: Difference in coefficients in fixed and random effects regression not systematic)		43.7(0.000)			316.0(0.000)	

Robust standard errors in brackets.
* significant at 10%; ** significant at 5%; *** significant at 1%.
Source: Author's estimation from VHLSS 2004 and 2006.

Chapter 5 The impact of public and private transfers on poverty and inequality[32]

5.1 Introduction

Income transfers are potentially important means to alleviate poverty and reduce income inequality. A substantial share of poverty is so-called transient poverty, i.e. at any point in time a group of people is poor purely due to 'bad luck' combined with the inability to cope with this downward risk (Dercon and Krishnan, 2000; Dercon, 2003; Jalan and Ravallion, 2000). Targeted transfers may help prevent this type of poverty (Alderman and Haque, 2006). In addition, cash transfers may have persistent effects on chronic poverty if they ease liquidity constraints that prevent the poor from investing in productive activities, generating multipliers on the cash received (Sadoulet *et al.*, 2001; Farrington and Slater, 2006; Lloyd-Sherlock, 2006). Similarly, through the provision of a safety net, transfers may decrease the need of poor households to diversify or skew towards low-risk low-return alternatives that avoid destitution but at the same time inhibit income growth and investment (Carter and Barrett, 2006; Dercon, 2003; Ravallion, 1988).

Income transfers are, however, by no means a panacea. Poor people may receive less from social security programs than people from middle and high income groups (e.g. Friedman and Friedman, 1979; Howe and Longman, 1992; Castles ad Mitchell, 1993). Public transfer programs are often contribution-based and exclude groups without substantial periods of formal sector employment, thus minimizing their coverage of poor and vulnerable social groups (Lloyd-Sherlock 2006). Even for social transfer programs targeted specifically at the poor, there can be a high leakage rate, i.e. the programs may cover a substantial share of ineligible people. Barrientos and DeJong (2006), for example, observed that 20-40 percent of beneficiaries in three different cash transfer programs to support poor households with children of school age were among the non-poor. Similarly, households receiving private income transfers are not necessarily poor: wealthier households may be better integrated in redistributing networks.

Even if it is the poor who receive the transfers, the effect on poverty indicators may be limited. First, the transfers may simply be too small to lift people out of poverty. Second, the increase in income may be smaller than the amount of transfers received. Transfers potentially mitigate the incentive to work thus decreasing non-transfer income (Farrington and Slater, 2006; Lloyd-Sherlock, 2006; Sahn and Alderman, 1996). Public transfers may be especially ineffective in increasing income, as an increase in public transfers may be (partly) cancelled out by an associated decrease in private transfers (Jensen, 2003; Maitra and Ray, 2003). This is particularly important as social transfers compete with other policies for government funds and may ultimately put upward pressure on taxation. Third, increased income may not completely translate into increased expenditure, which is usually used as an indicator in poverty analysis. The effect of different types of transfer and earned income on expenditures may diverge. Often, different income sources accrue to different persons. These persons may have different preferences and pooling may be imperfect (Maitra and Ray 2003).

[32] This chapter is written based on the paper Nguyen, V.C. and Van den Berg M. (2009), 'Measuring the Impact of Public and Private Transfers on Poverty and Inequality in Vietnam', which is currently submitted to a journal for possible publication.

Despite these considerations, few studies systematically assess the combined impact of both private and public transfers on poverty while accounting for potential behavioral responses, and analyses of the relation between transfers and inequality are even more rare. This chapter focuses on the effects of income transfers in Vietnam. The main objective is to estimate and compare the impacts of public and private transfers on poverty and inequality in Vietnam. In addition, the chapter contributes to the existing literature through a stepwise analysis showing not only the ultimate impact of public and domestic private transfers on poverty and inequality in Vietnam but also some of the underlying mechanisms: the distribution of transfers among the poor and the non-poor, the potential effect of public on private transfers, the effect of transfers on work effort, and the different impact of transfers on income and expenditure.

Vietnam has committed itself to a 'growth with equity' strategy of development. The country has achieved high economic growth, with annual GDP growth rates of around 6 percent over the past 10 years. Poverty rates have declined remarkably from 58 to 16 percent between 1993 and 2006. The mass media claim that the extensive social security system maintained by the government has played a key role in this decline. Yet the few existing evaluation studies of the system do not support this claim. Van de Walle (2004) found that social insurance and subsidies were badly targeted at the poor, and that their impact on poverty was negligible during the 1990s. The relationship between transfers and poverty may, however, be different in the twenty-first century, as the pattern of poverty and transfers has changed significantly. As indicated above, poverty rates have declined dramatically, possibly leaving those households poor who are least connected to the outside world and are therefore least affected by any kind of transfers. At the same time several new transfer schemes were introduced, and the targeting of existing schemes may have improved. Evans *et al.* (2006) suggest that the overall effect of public transfers and poverty has improved slightly: they conclude that 2004 poverty rates would have been about 5 percent higher in the absence of social security payments. This estimate could, however, be biased as they use consumption minus transfers as counterfactual and do not account for behavioral responses.

Much less is known about the effect of private transfers. De Brauw and Harigaya (2007) found that without seasonal migration, the estimated poverty rate would have been three percentage points higher than it was in 1998. This reduction in poverty was not associated with an increase in inequality, in part because households that increased participation in migration tended to be in the middle of the expenditure distribution. Seasonal migrants are, however, not the only source of private transfers. Other relatives or friends may also send money.

Hence, while there are previous studies on the impact of transfers on poverty and inequality in Vietnam, sound scientific information is available only for the 1990s, and the rapid transformation of Vietnam since then may imply that this information is outdated. Perhaps more importantly, as for most other countries, these studies sketch only partial pictures. More particularly, they ignore potential interactions between public and private transfers and do not explicitly assess the behavioral responses to transfers. Also, existing information on the impact of private transfers is only indirect and incomplete –through an assessment of the impact of seasonal migration– and the impact of public transfers is only known for poverty and not for inequality. Our study intends to fill these gaps and present a relatively complete picture.

The structure of the chapter is as follows. The next section presents the theory and methodology used. We explain why public transfers may affect private transfers and how we test for this. Next, we explain the procedures for testing the impact of transfers on per capita income, expenditure and work efforts and some mechanisms behind the potential relations. We apply fixed-effects estimators and subsequently compute the expected impacts of transfers on recipients using a non-parametric bootstrap technique to determine their standard errors. Finally, we explain how we use our results to calculate Foster-Greer-Thorbecke poverty indexes and measures of inequality both with and without poverty. Section 3 presents the empirical analysis. We use data from the recent Vietnam Household Living Standard Surveys 2004 and 2006. These surveys form a panel of more than 4200 households that allows us to estimate the impact of public and private transfers correcting for characteristics that are either observed or unobserved but stable between the two survey rounds. In this chapter, we focus on domestic private transfers as opposed to international remittances, as the latter involve an inflow of resources into the country and not a mere redistribution of income. Our empirical analysis consists of five parts. We start by describing the distribution of transfers over poor and non-poor households and then move on to analyzing whether the level of public transfers affect private transfers. As this is not the case, we can thereafter simply estimate the impact of public and private transfers. We first assess whether households adapt working hours to transfers, and secondly focus on the effects on income and expenditures. Finally, we estimate the ultimate effects of transfers on poverty and inequality. Section 4 gives policy implications and a conclusion.

5.2 Theory and methodology

5.2.1 Testing for interaction between public and private transfers

Public transfers may have a limited impact on household income as they may simply replace private transfers. This crowding-out hypothesis is based on theories about altruism and insurance. Transfers of a well-behaved altruistic person to others will decrease if the pre-transfer income of the recipients increases – through public transfers or otherwise (Becker, 1974; Barro, 1974; Stack and Bloom, 1985; Stark, 1995; Lucas and Stark, 1985; Cox, 1987, 1990). Alternatively, if migration is a strategy to cope with economic risks or shocks, migrants will remit more money when those staying behind experience a drop in income (Stark and Levhari, 1982; Stark and Bloom, 1985; Rosenzweig, 1988). Public transfers may make such transfers unnecessary. On the other hand, exchange theory argues that if people give transfers because they expect to get some benefits in return, higher recipient income – and thus higher public transfers – may result in higher private transfers (Cox, 1987, Bernheim *et al.*, 1985; Hoddinott, 1994; De la Brière *et al.*, 2002). Hence, not only the significance but also the direction of the effect of public transfers on private transfers is ultimately an empirical issue.

While public transfers may thus affect private transfers, we do not expect similar effects the other way around. The government is not likely to know about the extent of private transfers and will therefore not be able to adapt its transfers accordingly. We therefore only test whether public transfers crowd in or crowd out private transfers.

In order to do so, we assume that private per capita transfers received by household i in region j at year t are a linear function of a year dummy G, household characteristics X, commune characteristics C and the per capita amount of public transfers received P:

$$T_{ijt} = \alpha_0 + G_t\alpha_1 + X_{ijt}\beta_2 + P_{ijt}\beta_3 + C_{jt}\beta_4 + u_{ij} + v_j + \varepsilon_{ijt}\,, \tag{1}$$

where u_{ij} and v_j are unobserved time-invariant household and commune characteristics, respectively, and ε_{ijt} is an error term. If the coefficient for public transfers is statistically significant and negative, this is evidence of crowding out. Conversely, if the coefficient for public transfers is statistically significant and positive, this provides evidence of crowding in.

Note that 'per capita' refers to the average per household at period t. Per capita transfers are thus calculated as total household transfers divided by the number of household members. The year dummy takes one for 2006 and controls for common macroeconomic changes. Household characteristics include household composition, education of household members, and natural capital. Commune characteristics include urbanity, location in one of the eight main zones of Vietnam, and two village level variables: distance to the nearest market, and a dummy variable indicating whether the village has a road. The VHLSS data sets only provide information on these variables for the rural area. For urban areas, we assume that all communes have a market and a road, which is a reasonable assumption. A description of the explanatory variables can be found in Tables A.1 and A.2.

We use Tobit random effects to account for censoring of the dependent variable and at the same time control for unobserved time-invariant household characteristics. To give an indication of the robustness of the results, we also present standard random effects estimates and fixed effect estimates with sampling weights and cluster correlation. For completeness, we present the estimates of the complete function as well as regressions of per capita private transfers of per capita public transfers only.

5.2.2 Impact of transfers on per capita income, per capita expenditure, labour supply, poverty and inequality

Transfers increase income, but not necessarily by the amount of money transferred. Increased availability of non-labour income results in a rise in the shadow price of household labour. If labour supply is quite flexible, which is the case for self-employed households –an important group in developing countries like Vietnam– this will result in a decrease in labour supply that partly offsets the initial income increase. On the other hand, transfers can also provide working capital investment money for productive activities and therefore increase income by more than the amount transferred. Similarly, an increase in income does not necessarily result in the same increase in expenditure. Households may save part of the transfers, and the savings coefficient may be different from the coefficient for earned income. We therefore estimate not only the ultimate impact of transfers on expenditures, which –together with the distribution of transfers–determines their impact on poverty and inequality, but also the impact on income and labour supply.

We assume a similar specification for estimating the effect of transfers on per capita income, per capita expenditure and labour supply:

$$Y_{ijt} = \beta_0 + G_t\beta_1 + X_{ijt}\beta_2 + D_{ijt}\beta_3 + C_{jt}\beta_4 + u_{ij} + v_j + \varepsilon_{ijt}\,, \tag{2}$$

where Y is a vector including income per capita, expenditure per capita, and different proxies for labour supply, D is a vector of per capita public and private transfers and the remainder variables are identical to Equation (1). Van de Walle (2004) uses a similar equation for her estimates of the impact of public transfers on consumption. Please note that simultaneous inclusion of public and private transfers in a single equation is only possible when they are independent, that is if public transfers do not affect private transfers (Heckman *et al.*, 1999). Otherwise, separate regressions are required for the two types of transfers.

We use fixed and a random effects estimators. If the random effects estimator is used, β_0 is assumed to be the same for all households. In the fixed effects estimator, however, the constant is allowed to differ per household, i.e. $\beta_0 = \beta_{0ij}$. The time invariant household and commune characteristics are then perfectly correlated with the fixed effect and $u_{ij} + v_j$ will drop out of the model. The advantage of using panel data estimators is that they correct for time variant unobserved characteristics, such as diligence and social networks, which affect the choice variables. These characteristics are likely to be correlated with the independent variables in the regression, and if so, they will cause estimates to be biased unless fixed effects regression is used. Fixed effects regression was also used by Van de Walle (2004) in her study on the impact of public transfers on poverty. In addition, she used 1993 transfers as instruments for 1998 transfers in an IV regression. However, as she herself admits, the validity of these results depends on the exogeneity of the instrument. This assumption cannot be tested as there are no other potential instruments. We are not convinced of the suitability of the instrument and therefore do not follow the IV approach and focus on the fixed effect regressions.[33]

The marginal impact of transfers is measured by β_3. We will also measure the impact of transfers by calculating the Average Treatment Effect on the Treated (ATT) (Heckman *et al.*, 1999), i.e. the impact of transfers on the recipients (with *D*>0):

$$ATT_t = E(Y_{ijt} \mid D_{ijt} > 0) - E(Y_{ijt(D=0)} \mid D_{ijt} > 0), \tag{3}$$

Where $E(Y_{ijt(D=0)} \mid D_{ijt} > 0)$ is the expected value of the outcome variable of the transfers recipients, i.e. income per capita, expenditure per capita, or work efforts, had they not received transfers. This is not observed and has to be estimated.

Using Equation (2), we get:

$$\begin{aligned} ATT_t &= E\left(Y_{ijt} \middle| D_{ijt} > 0\right) - E\left(Y_{ijt(D=0)} \middle| D_{ijt} > 0\right) \\ &= E\left(\beta_0 + G_t\beta_1 + X_{ijt}\beta_2 + D_{ijt}\beta_3 + C_{jt}\beta_4 + u_{ij} + v_j + \varepsilon_{ijt} \middle| D_{ijt} > 0\right) \\ &\quad - E\left(\beta_0 + G_t\beta_1 + X_{ijt}\beta_2 + C_{jt}\beta_4 + u_{ij} + v_j + \varepsilon_{ijt} \middle| D_{ijt} > 0\right) \\ &= D_{ijt}\beta_3. \end{aligned} \tag{4}$$

which implies:

$$\hat{ATT}_t = \frac{1}{n_t}\sum_{i=1}^{n_t} D_{ijt}\hat{\beta}_3, \tag{5}$$

where n_t is the number of the remittance recipients at time *t*.

We compliment these point estimates of ATT with standard error estimates generated using a non-parametric bootstrap technique. This bootstrap is implemented by repeatedly drawing samples from the original data. Since the VHLSS sample selection follows stratified random cluster sampling, communes instead of households are bootstrapped in each stratum (Deaton, 1997). The number of replications is 500.[34]

[33] Actually, we tried IV regressions, in which transfers in 2004 are used as instruments for transfers in 2006. However, the estimation results are not robust and reasonable in our data set.

[34] We also tried to bootstrap households instead of communes to examine the robustness to the standard error estimates to bootstrap ways. The results from the different bootstrap methods were very similar.

Finally, the impact of transfers on the poverty and inequality indices of transfer receivers in period t is estimated by the same method which is used to estimate the impact of credit on poverty and inequality in the previous chapter (section 4.3.2 of Chapter 4).

5.3 Income transfers, poverty and inequality in Vietnam

5.3.1 Who are the recipients of transfers?

Vietnam's social security net includes a large number of programs, including both contribution-based and non-contribution-based transfers. Contribution-based social insurance covers health benefits, which are outside the scope of this chapter, and social security schemes, which have been compulsory for employees in State organizations, State-owned enterprises, and private enterprises with ten employees or more since 1995 (Evans *et al.*, 2006). These schemes are mainly paid in cash and include maternity benefits, severance pay, sickness and occupational injury benefits, monthly pensions for the retired, and life insurance (Government of Vietnam, 1993a, 1993b, 1995, 1998, 2003 and 2006). The main non-contributory schemes are the National Targeted Programs (NTP) and social allowances. The NTP provide very diverse support to the poor and are often in kind and difficult to convert to monetary values. In this chapter, we therefore focus on social allowances, which are usually disbursed in cash. These cover support to disadvantaged groups, such as war invalids and heroes, the elderly, children without guardians, disabled people, and households adversely affected by natural calamities (Government of Vietnam, 1993b, 2003).

Table 5.1 presents the distribution of public transfers by the poor and non-poor in 2004 and 2006. The coverage of public transfers remained almost unchanged during this period. About 18 percent of all households received public transfers in both years. In 2004 this was no different for the poor and the non-poor, but in 2006 the percentage of the poor receiving public transfers were decreased to 13.8 percent. In addition, the non-poor received much higher amounts of transfers per capita in both years. Moreover, since the non-poor account for a large proportion of the population, they received more than 95 percent of all public transfers in both years. Yet, the share of public transfers in income and expenditure for those poor receiving transfers is substantial: 24.0 and 38.1 percent of expenditures in 2004 and 2006, respectively, and 17.4 and 23.2 percent of income in 2004 and 2006, respectively.

The large majority of households received domestic private transfers: between 84.0 and 88.7 percent for the poor and the non-poor in 2004 and 2006 (Table 5.2). While the shares of households receiving private transfers did not differ much between the rich and the poor, the non-poor received much higher per capita amounts: their average value of per capita transfers was around 3.5 times more than that of the poor. As a result, the relative contribution of private transfers to total income and expenditures was similar for the rich and the poor.

Summarizing, both the poor and the non-poor received public and private transfers, but a relatively large share of these transfers went to non-poor households. The share of both groups receiving either type of transfer is quite similar, but the average per capita amounts transferred were much larger for the non-poor. However, the analysis is *ex post* and does not take into account that the assignment of households to poverty group is done after accounting for public and private transfers. Transfers may have lifted people out of poverty. In order to test this hypothesis, we need to estimate household income in the absence of these transfers.

Table 5.1. Public transfers by poor and non-poor recipients.

Indicators	2004			2006		
	Poor	Non poor	All	Poor	Non poor	All
% recipient households	18.0	18.3	18.3	13.8	18.8	18.1
	[1.1]	[0.5]	[0.5]	[1.2]	[0.5]	[0.5]
Per capita public transfers	361.2	2,044.3	1,761.5	617.0	2,882.5	2,648.9
(thousand VND)*	[31.3]	[70.2]	[62.4]	[74.7]	[96.1]	[90.6]
Distribution of recipient	16.8	83.2	100	10.3	89.7	100
households	[1.0]	[1.0]		[0.9]	[0.9]	
Distribution of public transfers	4.2	95.8	100	3.1	96.9	100
	[0.4]	[0.4]		[0.4]	[0.4]	
% of public transfers in	24.0	34.3	33.8	38.1	41.7	41.6
household expenditure	[2.0]	[1.1]	[1.1]	[4.4]	[1.5]	[1.5]
% of public transfers in	17.4	26.7	26.2	23.2	31.0	30.8
household income	[1.4]	[0.9]	[0.8]	[2.3]	[1.0]	[1.0]
Number of observations	1,769	7,419	9,188	1,427	7,762	9,189

Note: * in 2004 prices.
Standard errors in brackets (corrected for sampling weight and cluster correlation).
Source: Author's estimation from VHLSS 2004 and 2006.

Table 5.2. Domestic private transfers by poor and non-poor recipients.

Indicators	2004			2006		
	Poor	Non poor	All	Poor	Non poor	All
% recipient households	84.0	86.8	86.3	87.5	88.7	88.5
	[1.1]	[0.5]	[0.5]	[1.0]	[0.5]	[0.5]
Per capita domestic transfers	220.9	815.0	716.3	238.3	800.1	724.7
amount (thousand VND)*	[12.4]	[34.4]	[29.1]	[16.6]	[27.3]	[24.1]
Distribution of receiving	16.6	83.4	100	13.4	86.6	100
households	[0.5]	[0.5]		[0.5]	[0.5]	
Distribution of domestic	5.8	94.2	100	5.1	94.9	100
transfers amount	[0.4]	[0.4]		[0.4]	[0.4]	
% of domestic transfers over	13.7	15.2	15.1	13.9	13.5	13.5
household expenditure	[0.8]	[0.6]	[0.6]	[1.0]	[0.4]	[0.4]
% of domestic transfers over	9.9	11.7	11.6	9.1	10.1	10.0
household income	[0.5]	[0.4]	[0.4]	[0.6]	[0.3]	[0.3]
Number of observations	1,769	7,419	9,188	1,427	7,762	9,189

Note: * in 2004 prices.
Standard errors in brackets (corrected for sampling weight and cluster correlation).
Source: Author's estimation from VHLSS 2004 and 2006.

This is what we will do in the remainder of this chapter. We will first consider possible relations between public and private transfers: does the amount of public transfers received affect private transfers. Subsequently, we will asset the impact of transfers on labour supply, as changes in household labour supply may modify the income effects of transfers. Finally, we determine the ultimate effects of transfers on income and expenditures. To get a general idea of the relation between these different variables, Table 5.3 presents a correlation matrix. As expected, the correlation between per capita income and expenditures is positive and high (0.7). Both per capita public and private expenditures are moderately positively correlated with per capita income and expenditures (0.2 to 0.3) and weakly positively with each other (0.1), confirming our tabular findings that at least *ex post* richer households receive higher transfers of both types. While working hours per productive member and working hours per capita are weakly positively correlated with income and expenditures (0.1 to 0.2), the share of productive household members and income and expenditures are weakly negatively correlated (-0.1 to -0.2). Working hours per productive member, working hours per capita and the share of productive members are weakly negatively correlated with transfers (-0.0 to -0.2). Whether this presents a causal link, and transfers induce people to consume more leisure is investigated in section 5.3.3.

Table 5.3. Correlation matrix of income, expenditures, transfers, and labour supply in 2004 and 2006.

	Per capita income	Per capita expenditure	Per capita public transfers	Per capita private transfers	Members engaged in productive activities/total household members older than 14 (%)	Annual working hours per household member engaged in productive activities	Annual working hours per capita
2004							
Per capita income	1						
Per capita expenditure	0.7224*	1					
Per capita public transfers	0.2258*	0.2355*	1				
Per capita private transfers	0.3283*	0.3532*	0.1429*	1			
Members engaged in productive activities/total household members older than 14 (%)	-0.1241*	-0.2126*	-0.1761*	-0.1820*	1		
Annual working hours per household member engaged in productive activities	0.1854*	0.1650*	-0.0335	-0.0564*	0.3563*	1	
Annual working hours per capita	0.1476*	0.1341*	-0.1086*	-0.1055*	0.0116*	0.6638*	1
2006							
Per capita income	1						
Per capita expenditure	0.6577*	1					
Per capita public transfers	0.2122*	0.2192*	1				
Per capita private transfers	0.2267*	0.3292*	0.1041*	1			
Members engaged in productive activities/total household members older than 14 (%)	-0.0556*	-0.1579*	-0.1793*	-0.1555*	1		
Annual working hours per household member engaged in productive activities	0.1823*	0.1986*	-0.0998*	-0.0760*	0.4100*	1	
Annual working hours per capita	0.1439*	0.1559*	-0.1671*	-0.1578*	0.0743*	0.6822*	1

* Statistically significant at 5%.
Source: Author's estimation from VHLSS 2004.

5.3.2 Public and private transfers: crowding out or crowding in?

To test for the relationship between public and private transfers net of household characteristics, we ran a regression of private transfers on public transfers and control variables as described above. While simple regressions of private transfers on public transfers point towards a significantly positive relationship, the significance disappears after the introduction of household fixed effects or the introduction of household and community controls (Table 5.4 and Table 5.10 in Appendix 5.1). Put differently, the positive association between public and private transfers is the consequence of common association with other characteristics and not of a direct relationship: some households received both more public and more private transfers than others did, but this is due to other factors than the crowding-in of private transfers by public transfers. For example, households with more elderly received both more pensions and more transfers from friends and relatives but the latter sent these not considering the level of public transfers. This result is robust for the estimation method used.

Table 5.4. Regression of per capita domestic transfer.

Explanatory variables	Random effects (no sampling weights)	Tobit random effects (no sampling weights)	Fixed effects (with sampling weights and cluster correlation)	Random effects (no sampling weights)	Tobit random effects (no sampling weights)	Fixed effects (with sampling weights and cluster correlation)
Public transfers per capita	0.122***	0.122***	-0.006	0.01	0.01	-0.05
(thousand VND)	[0.013]	[0.013]	[0.056]	[0.014]	[0.014]	[0.057]
Household controls	No	No	No	Yes	Yes	Yes
Community controls	No	No	No	Yes	Yes	Yes
Dummy variable for 2006	Yes	Yes	Yes	Yes	Yes	Yes
Constant	Yes	Yes	yes	Yes	Yes	Yes
Observations	8,432	8,432	8,432	8,432	8,432	8,432
Number of households	4,216	4,216	4,216	4,216	4,216	4,216
R-squared	0.01		0.01	0.104		0.07

Standard errors brackets.
* significant at 10%; ** significant at 5%; *** significant at 1%.
Author's estimation from VHLSS 2004 and 2006.

5.3.3 Transfers and labour supply

Public and private transfers potentially lower labour supply. To test this relationship, we ran regressions of several indicators of households labour supply on public and private transfers. The labour variables are the ratio of working members in households, the number of working household members, total annual working hours per capita, and total working hours per working household member. The regression results are reported in Tables 5.11 and 5.12 in Appendix 5.1. The results are mostly very similar across different estimation methods. As Hausman tests reject exogeneity of the household effects, we select the fixed effect estimates with survey corrections to estimate ATT.

The ATT estimates are presented in Table 5.5 and show clearly that both public and private transfers decreased work efforts as expected. Receipt of private transfers reduced the ratio of members working in productive activities to total adult household members by nearly 1 percentage point. The impacts of public transfers on the working to all adult ratio was of similar magnitude, but not statistically significant. Both public and private transfers reduced the working hours of the recipients. Public transfers on average reduced working hours per capita by 52 hours, or 5 percent in 2004. The reduction was substantially higher in 2006, when public transfers were higher: 89 hours, or 8 percent. The effect of domestic private transfers was quite small: the receipt of private transfers reduced the working hours per capita by only 1 percent in both years. Not only were private transfers much lower than public transfers, their coefficient in the labour hours equations was also lower indicating that per VND transferred, the decrease in working hours was lowest for private transfers. Possible reasons could be that private transfers are better targeted at those who really need them or that people are afraid that their relatives or friends will not send money again if they notice that the recipients start working fewer hours.

Table 5.5. Impact of public and private transfers on annual working hours (ATT).

	2004			2006		
	Y_1	Y_0	ATT ($Y_1 - Y_0$)	Y_1	Y_0	ATT ($Y_1 - Y_0$)
Impact of public transfers on:						
Members engaged in productive activities/total household members older than 14 (%)	69.6***	70.3***	-0.8	66.5***	67.8***	-1.3
	[0.7]	[1.0]	[0.8]	[0.8]	[1.5]	[1.3]
Annual working hours per household member engaged in productive activities	1,688.7***	1,766.5***	-77.8***	1,732.9***	1,863.2***	-130.3***
	[19.3]	[27.1]	[18.8]	[23.2]	[38.4]	[31.7]
Annual working hours per capita	948.3***	999.8***	-51.5***	961.4***	1,050.0***	-88.6***
	[12.3]	[17.9]	[13.7]	[15.6]	[28.0]	[23.7]
Impact of private transfers on:						
Members engaged in productive activities/total household members older than 14 (%)	76.6***	77.3***	-0.7**	75.1***	75.9***	-0.8***
	[0.3]	[0.4]	[0.2]	[0.3]	[0.4]	[0.2]
Annual working hours per household member engaged in productive activities	1,776.1***	1,784.7***	-8.6**	1,822.5***	1,832.6***	-10.1**
	[10.7]	[11.3]	[3.4]	[10.2]	[11.0]	[4.0]
Annual working hours per capita	1,001.9***	1,013.5***	-11.5***	1,040.6***	1,054.0***	-13.4***
	[6.6]	[7.6]	[3.6]	[7.1]	[8.1]	[4.2]

Note: Members engaged in productive activities are those who are above 14 year olds and working for money, and working hours are those hours spent in productive activities.
* significant at 10%; ** significant at 5%; *** significant at 1%.
Standard errors in brackets. Standard errors are corrected for sampling weights and estimated using bootstrap (non-parametric) with 500 replications.
Author's estimation from VHLSS 2004 and 2006.

5.3.4 The effect of transfers on household income and expenditure

Tables 5.14 and 5.15 in Appendix 5.1 present the regressions of income and expenditure per capita on public and private transfers per capita and other control explanatory variables. As indicated before, we present both random effects and fixed effects estimates, with and without sampling weights and cluster correlation. The results are quite robust with respect to estimation method and inclusion or non-inclusion of control variables. Since the Hausman tests strongly favour the fixed-effects estimates, we focus the discussion on the survey-corrected fixed-effects estimates.

The estimates of marginal effect of public and private transfers on income are 0.75 and 0.72 respectively, and not significantly different (Table 5.14 in the Appendix). This means that an extra VND transferred per capita leads to an increase of just over 0.7 VND in per capita income, irrespective of the source of the transfer. The estimates are statistically significantly smaller than one, indicating that income increased by less than the amount of money transferred. These findings are consistent with the findings that transfers decrease work effort and do not confirm the existence of multiplier effects.

As expected, the impact of public and private transfers on expenditure was lower than the impact on income (Table 5.15 in Appendix 5.1). An increase of 1 VND in per capita public and private transfers resulted in an increase of 0.12 and 0.41 VND in per capita expenditure. This suggests that households use public and private transfers not only for consumption but also for savings and investment. These results are of the same order of magnitude as the findings of Van de Walle (2004) who conclude that the propensity to consume out of public transfers (not including pensions) was 0.37 for Vietnam during the 1990s.

To see the total increase in per capita income and expenditure caused by transfers, we estimated ATT. Since ATT depends on the size of transfers, it may have differed between years and sources, and Table 5.6 presents the ATT estimates for public and private transfers for both years separately. It shows that public transfers increased per capita income of the recipient by around 20 and 24 percent in 2004 and 2006, respectively. The effect of public transfers on per capita expenditure was much lower. Public transfers increased per capita expenditure of the recipient households only by 3 and 4 percent for the years 2004 and 2006, respectively. Again, this finding suggests that most public transfers were saved.

Table 5.6. Impact of transfers measured by ATT.

	2004			2006		
	Y_1	Y_0	ATT ($Y_1 - Y_0$)	Y_1	Y_0	ATT ($Y_1 - Y_0$)
Impact of public transfers on:						
Income per capita	6,279.3***	5,238.0***	1,041.3***	7,962.2***	6,416.5***	1,545.7***
	[167.4]	[194.3]	[120.8]	[192.1]	[247.9]	[183.4]
Expenditure per capita	4,860.6***	4,699.3***	161.3**	5,938.3***	5,698.9***	239.4**
	[117.1]	[136.9]	[76.8]	[140.7]	[174.9]	[102.4]
Difference in ATT between income and expenditure			880.1***			1,306.3***
			[109.5]			[167.6]
Impact of private transfers on:						
Income per capita	5,801.7***	5,412.6***	389.1***	6,809.5***	6,420.6***	388.9***
	[89.1]	[88.4]	[32.3]	[99.3]	[100.9]	[31.9]
Expenditure per capita	4,455.1***	4,229.1***	226.0***	5,034.8***	4,806.4***	228.4***
	[64.9]	[71.9]	[35.9]	[70.5]	[76.7]	[38.1]
Difference in ATT between income and expenditure			163.1***			160.5***
			[37.8]			[37.7]

* significant at 10%; ** significant at 5%; *** significant at 1%.
Standard errors in brackets. Standard errors are corrected for sampling weights and estimated using bootstrap (non-parametric) with 500 replications.
Source: Author's estimation from VHLSS 2004 and 2006.

Private transfers had a much lower effect on per capita income than public transfers – a 7 and 6 percent increase in 2004 and 2006, respectively – simply because private transfers were much smaller on average. The effect of both transfers on expenditures was, however, comparable: private transfers increased recipients per capita expenditure by around 5, public transfers by 3-4 percent. Hence, whereas most public transfers were saved, a relatively large share of private transfers was used for current consumption. This suggests that people sent more transfers to relatives and friends who had a greater need for consumption, as theories of altruism and insurance imply.

Table 5.7. Impact of transfers on poverty.

	2004			2006		
	With public transfers	Without public transfers	Impact	With public transfers	Without public transfers	Impact
Public transfers						
Transfer recipients						
P0	0.1991***	0.2207***	-0.0216**	0.1353***	0.1483***	-0.0130
	[0.0129]	[0.0163]	[0.0108]	[0.0122]	[0.0148]	[0.0092]
P1	0.0565***	0.0608***	-0.0044*	0.0388***	0.0429***	-0.0041
	[0.0050]	[0.0058]	[0.0026]	[0.0055]	[0.0060]	[0.0025]
P2	0.0235***	0.0251***	-0.0015	0.0160***	0.0178***	-0.0018
	[0.0028]	[0.0031]	[0.0010]	[0.0031]	[0.0033]	[0.0012]
All						
P0	0.1949***	0.1987***	-0.0038**	0.1597***	0.1620***	-0.0023
	[0.0053]	[0.0056]	[0.0019]	[0.0051]	[0.0052]	[0.0016]
P1	0.0472***	0.0480***	-0.0008	0.0383***	0.0390***	-0.0007
	[0.0017]	[0.0018]	[0.0005]	[0.0017]	[0.0018]	[0.0004]
P2	0.0170***	0.0173***	-0.0003	0.0137***	0.0140***	-0.0003
	[0.0009]	[0.0009]	[0.0002]	[0.0009]	[0.0009]	[0.0002]
Private transfers						
Transfer recipients						
P0	0.1884***	0.2159***	-0.0275***	0.1574***	0.1780***	-0.0206***
	[0.0060]	[0.0077]	[0.0050]	[0.0062]	[0.0078]	[0.0047]
P1	0.0446***	0.0569***	-0.0123***	0.0366***	0.0457***	-0.0092***
	[0.0019]	[0.0037]	[0.0031]	[0.0019]	[0.0031]	[0.0024]
P2	0.0157***	0.0338***	-0.0181	0.0128***	0.0205***	-0.0077**
	[0.0009]	[0.0125]	[0.0124]	[0.0009]	[0.0038]	[0.0036]
All						
P0	0.1953***	0.2189***	-0.0236***	0.1601***	0.1782***	-0.0182***
	[0.0058]	[0.0071]	[0.0043]	[0.0059]	[0.0074]	[0.0041]
P1	0.0473***	0.0579***	-0.0106***	0.0384***	0.0464***	-0.0081***
	[0.0019]	[0.0033]	[0.0027]	[0.0019]	[0.0030]	[0.0021]
P2	0.0170***	0.0325***	-0.0155	0.0137***	0.0205***	-0.0068**
	[0.0009]	[0.0107]	[0.0106]	[0.0009]	[0.0034]	[0.0032]

* significant at 10%; ** significant at 5%; *** significant at 1%.
Standard errors in brackets. Standard errors are corrected for sampling weights and estimated using bootstrap (non-parametric) with 500 replications.
Source: Author's estimation from VHLSS 2004 and 2006.

5.3.5 Impact on poverty and inequality

Since transfers had a significant impact on per capita expenditure, they are expected to have affected poverty and inequality. Public transfers indeed reduced the head count of poverty of recipients by around 2.2 and 1.3 percentage points in 2004 and 2006, respectively, although the effect was not statistically significant for the latter year (Table 5.7). While the average transfer amounts were higher in 2006, their effect on poverty was higher in 2004, because in this year there were more poor and a larger share of the poor received public transfers. The effect of public transfers on the overall head count was negligible, as was their impact on the other poverty indices. This indicates that not much has changed since the 1990s, for which Van de Walle (2004) obtained a similar result.

Domestic private transfers were slightly more successful in reducing poverty. Almost all impact estimates were negative and statistically significant (Table 5.6). Private transfers reduced the poverty incidence for the recipients by around 2.7 and 2.1 percentage points. Within the group of poor transfer recipients, the effects were greater: their poverty gap decreased by about 20 percent in both years, and the severity of their poverty even declined by 71 and 38 percent in 2004 and 2006, respectively. As more than 80 percent of the poor received domestic private transfers, their effects on total poverty were only slightly smaller than their effects on the poverty of recipients alone.

Public transfers and private transfers had very little impact, if any, on inequality. Inequality of the total population increased by less than one percent due to public transfers, but decreased by around one percent due to private transfers (Table 5.8). Inequality between recipients did not change significantly due to either type of transfer and is therefore not presented in tabular form. The negligible impact of transfers on inequality does not come as a surprise. As we saw before, the relative contribution of transfers to total income and expenditure was similar for poor and non-poor recipients, and the shares of households receiving transfers were also similar across the two groups.

Table 5.8. Impact of transfers on overall inequality.

	2004			2006		
	With transfers	Without transfers	Impact	With transfers	Without transfers	Impact
Public transfers						
Gini	0.3698***	0.3697***	0.0001	0.3580***	0.3576***	0.0004*
	[0.0040]	[0.0040]	[0.0001]	[0.0040]	[0.0040]	[0.0002]
Theil L	0.2235***	0.2234***	0.0002	0.2117***	0.2113***	0.0005*
	[0.0050]	[0.0050]	[0.0002]	[0.0049]	[0.0049]	[0.0003]
Theil T	0.2407***	0.2408***	-0.0001	0.2268***	0.2266***	0.0001
	[0.0065]	[0.0066]	[0.0003]	[0.0071]	[0.0072]	[0.0003]
Private transfers						
Gini	0.3697***	0.3724***	-0.0027**	0.3579***	0.3603***	-0.0024**
	[0.0050]	[0.0052]	[0.0014]	[0.0046]	[0.0047]	[0.0012]
Theil L	0.2235***	0.2238***	-0.0004	0.2118***	0.2137***	-0.0019
	[0.0062]	[0.0064]	[0.0014]	[0.0056]	[0.0056]	[0.0013]
Theil T	0.2405***	0.2444***	-0.0040**	0.2267***	0.2295***	-0.0028*
	[0.0077]	[0.0081]	[0.0021]	[0.0074]	[0.0077]	[0.0016]

* significant at 10%; ** significant at 5%; *** significant at 1%.
Standard errors in brackets. Standard errors are corrected for sampling weights and estimated using bootstrap (non-parametric) with 500 replications.
Source: Author's estimation from VHLSS 2004 and 2006.

5.4 Conclusions

In this study, we investigate how well public transfers and domestic private transfers reached the poor in Vietnam and to what extent these transfers affected poverty and inequality in the mid-2000s. We also show some of the other underlying mechanisms. We estimate the effect of the transfers on both per capita income and expenditure. Neither is straightforward. Cash transfers do not necessarily result in an increase in income with the same value as the transfer. On the one hand, cash transfers may have positive multiplier effects when (part of) the money is used productively. On the other hand, public transfers may crowd out private transfers and lead to a reduction in work effort. We therefore also estimate the effect of public transfers on domestic private transfers and the effect of both types of transfers on work effort. At the same time, the propensity to consume is not necessarily the same for transfers and earned income, as they may accrue to different persons with different preferences, and money may not be perfectly pooled. Last but not least, estimating the effect of transfers on income and expenditures will give biased estimates unless the endogeneity of transfer allocation is accounted for. We therefore use fixed-effects regression to account for time-invariant unobserved variables that are correlated with the independent variables.

Vietnam's extensive social security system is claimed to have played a key role in the extraordinary poverty decline over the past decades. This claim is, however, not substantiated by empirical evidence. Like Van de Walle (2004) has shown for the 1990s, we find that the impact of public transfers on poverty was negligible due to low coverage of poor and relatively low amounts transferred to the poor, but not due to crowding out of private transfers: contrary to studies for other countries, our estimates suggest that public transfers did not affect the level of private transfers. Still, the effect of transfers received on expenditure was small: transfer recipients decreased labour supply and in addition used only a limited amount of the extra income for current consumption.

Domestic private transfers were somewhat more successful in reducing poverty – a decrease of about two percentage points of the head count and quite substantial decreases in the depth and severity of poverty – because a large proportion of the poor received private transfers and a relatively high share of private transfers was used for current consumption.

Our results imply that simply increasing the government budget for transfers will not be very effective in decreasing poverty. A significant share of transfers can leak to the non-poor, even though social subsidies alone are targeting slightly better than the overall transfers we present in the chapter. Moreover, as indicated above, the impact of public transfers received on expenditures is low. The much larger effect of private transfers, however, indicates that better targeting could also improve the link between transfers and expenditures.

Better targeting is however complicated and possibly costly. Decentralization, as suggested by Van de Walle (2004), may not be an easy solution. Public microfinance is allocated through commune authorities to supposedly poor households. But two thirds of the money lent still ends up with the non-poor (Nguyen *et al.*, 2008). Our results suggest that facilitation of money transfers between relatives and friends could be a more efficient method of decreasing poverty.

Despite the fact that most public and private transfer went to non-poor households, inequality was only marginally affected. The group of non-poor is much larger than the group of poor households, and even though the average transfer received is much higher for non-poor recipients, the relative contribution of transfers to total income and expenditure was similar for the poor and the non-poor recipients. Also, the shares of households receiving transfers were similar across the two groups for both public and private transfers.

Finally, we must keep in mind that our poverty and inequality estimates do not cover all effects of transfers on welfare. A substantial share of especially public transfers seems to be saved or invested and may thus lead to future improvements in well-being, which was outside the scope of this study. Also, transfers resulted in a decrease in work effort and thus an increase in leisure, which – like consumption expenditure – adds to current welfare, but is not accounted for in our poverty calculations.

Appendix 5.1 Descriptive statistics and regression results

Table 5.9. Variables of households with and without public transfers.

Variables	Type	2004		2006	
		Households with public transfers	Households without public transfers	Households with public transfers	Households without public transfers
Household variables					
Ratio of members younger than 16 to total household members	Continuous	0.2011	0.2809	0.1674	0.2527
		[0.0092]	[0.0043]	[0.0082]	[0.0042]
Ratio of members who older than 60 to total household members	Continuous	0.1747	0.0771	0.1915	0.0793
		[0.0085]	[0.0029]	[0.0088]	[0.0027]
Household size	Discrete	5.06	4.85	4.92	4.92
		[0.10]	[0.04]	[0.12]	[0.05]
Household size squared	Discrete	29.93	26.51	28.41	27.27
		[1.34]	[0.59]	[1.67]	[0.66]
Ratio of members with technical degree to total household members	Continuous	0.1053	0.0492	0.1310	0.0526
		[0.0079]	[0.0028]	[0.0080]	[0.0026]
Ratio of members with post secondary degree to total household members	Continuous	0.0601	0.0212	0.0770	0.0232
		[0.0076]	[0.0020]	[0.0080]	[0.0020]
Area of annual crop land per capita (m^2)	Continuous	685.7	698.2	605.7	768.1
		[57.0]	[29.6]	[59.0]	[34.9]
Area of perennial crop land per capita (m^2)	Continuous	206.0	215.6	174.0	251.6
		[59.5]	[19.5]	[30.9]	[20.7]
Forestry land per capita (m^2)	Continuous	279.7	195.8	340.4	250.9
		[82.2]	[41.0]	[113.2]	[50.6]
Aquaculture water surface per capita (m^2)	Continuous	43.0	57.8	32.9	73.3
		[13.6]	[10.0]	[10.1]	[14.1]
Village variables					
Road to village (yes = 1)	Continuous	0.5986	0.6157	0.5918	0.6661
		[0.0251]	[0.0145]	[0.0250]	[0.0140]
Distance to nearest daily market (km)	Continuous	2.5999	2.0652	2.2818	2.3390
		[0.3348]	[0.1318]	[0.3275]	[0.1625]
Regional variables					
Red River Delta	Binary	0.2614	0.1977	0.2950	0.1910
		[0.0212]	[0.0113]	[0.0224]	[0.0110]
North East	Binary	0.1518	0.1100	0.1504	0.1106
		[0.0155]	[0.0081]	[0.0164]	[0.0082]
North West	Binary	0.0336	0.0324	0.0246	0.0343
		[0.0081]	[0.0046]	[0.0076]	[0.0048]
North Central Coast	Binary	0.1977	0.1259	0.1992	0.1261
		[0.0215]	[0.0107]	[0.0226]	[0.0106]
South Central Coast	Binary	0.0669	0.0912	0.0637	0.0917
		[0.0115]	[0.0079]	[0.0105]	[0.0080]
Central Highlands	Binary	0.0939	0.0559	0.0532	0.0649
		[0.0175]	[0.0068]	[0.0127]	[0.0076]
North East South	Binary	0.0907	0.1631	0.1087	0.1587
		[0.0160]	[0.0125]	[0.0172]	[0.0123]
Mekong River Delta	Binary	0.1041	0.2238	0.1052	0.2227
		[0.0144]	[0.0122]	[0.0142]	[0.0121]
Living in urban areas	Binary	0.2870	0.2450	0.3329	0.2355
		[0.0230]	[0.0130]	[0.0243]	[0.0129]
Observations		1,687	7,501	1,625	7,564

Standard errors in brackets.
Source: Author's estimation from VHLSS 2004 and 2006.

Table 5.10. Variables of households with and without domestic private transfers.

Variables	Type	2004		2006	
		Households with private transfers	Households without private transfers	Households with private transfers	Households without private transfers
Household variables					
Ratio of members younger than 16 to total household members	Continuous	0.2634	0.2833	0.2370	0.2432
		[0.0043]	[0.0096]	[0.0040]	[0.0111]
Ratio of members older than 60 to total household members	Continuous	0.1005	0.0620	0.1016	0.0786
		[0.0033]	[0.0060]	[0.0029]	[0.0072]
Household size	Discrete	4.87	5.02	4.91	4.97
		[0.04]	[0.09]	[0.05]	[0.09]
Household size squared	Discrete	26.95	28.21	27.46	27.58
		[0.60]	[1.16]	[0.67]	[1.24]
Ratio of members with technical degree to total household members	Continuous	0.0605	0.0534	0.0648	0.0806
		[0.0031]	[0.0057]	[0.0029]	[0.0088]
Ratio of hh. members with post secondary degree to total household members	Continuous	0.0285	0.0268	0.0318	0.0404
		[0.0025]	[0.0042]	[0.0026]	[0.0056]
Area of annual crop land per capita (m^2)	Continuous	676.5	809.9	732.8	794.7
		[28.6]	[68.4]	[33.1]	[92.7]
Area of perennial crop land per capita (m^2)	Continuous	207.6	250.5	231.2	293.4
		[21.0]	[58.9]	[18.1]	[80.9]
Forestry land per capita (m^2)	Continuous	187.3	351.3	254.2	370.7
		[35.0]	[126.7]	[50.0]	[152.1]
Aquaculture water surface per capita (m^2)	Continuous	45.4	112.0	52.0	184.2
		[7.8]	[35.9]	[10.2]	[66.7]
Village variables					
Road to village (yes = 1)	Continuous	0.6214	0.5608	0.6537	0.6472
		[0.0145]	[0.0293]	[0.0141]	[0.0333]
Distance to nearest daily market (km)	Continuous	2.1091	2.4790	2.3392	2.2430
		[0.1261]	[0.3398]	[0.1715]	[0.3432]
Regional variables					
Red River Delta	Binary	0.2162	0.1693	0.2076	0.2243
		[0.0119]	[0.0226]	[0.0115]	[0.0319]
North East	Binary	0.1123	0.1491	0.1109	0.1734
		[0.0084]	[0.0187]	[0.0082]	[0.0238]
North West	Binary	0.0215	0.0976	0.0285	0.0668
		[0.0034]	[0.0159]	[0.0041]	[0.0153]
North Central Coast	Binary	0.1399	0.1335	0.1408	0.1237
		[0.0113]	[0.0206]	[0.0112]	[0.0235]
South Central Coast	Binary	0.0877	0.0812	0.0865	0.0883
		[0.0078]	[0.0164]	[0.0077]	[0.0177]
Central Highlands	Binary	0.0667	0.0402	0.0681	0.0192
		[0.0078]	[0.0115]	[0.0078]	[0.0069]
North East South	Binary	0.1506	0.1456	0.1494	0.1539
		[0.0118]	[0.0224]	[0.0118]	[0.0274]
Mekong River Delta	Binary	0.2051	0.1836	0.2082	0.1504
		[0.0116]	[0.0231]	[0.0116]	[0.0231]
Living in urban areas	Binary	0.2517	0.2582	0.2531	0.2485
		[0.0132]	[0.0256]	[0.0130]	[0.0393]
Observations		7,825	1,363	8,032	1,157

Standard errors in brackets.
Source: Author's estimation from VHLSS 2004 and 2006.

Table 5.11. Regression of per capita domestic transfers.

Explanatory variables	Random effects (no sampling weight)	Tobit random effects (no sampling weight)	Fixed effects with sampling weight and cluster correlation	Random effects (no sampling weight)	Tobit random effects (no sampling weight)	Fixed effects with sampling weight and cluster correlation
Public transfers per capita	0.122***	0.122***	-0.006	0.01	0.01	-0.05
(thousand VND)	[0.013]	[0.013]	[0.056]	[0.014]	[0.014]	[0.057]
Ratio of members younger				-175.680*	-176.004*	175.427
than 16 to total household members				[90.769]	[90.521]	[241.609]
Ratio of members older				587.087***	587.388***	347.955
than 60 to total household members				[78.472]	[78.241]	[357.933]
Household size				-452.273***	-451.976***	-728.132***
				[33.611]	[33.537]	[111.687]
Household size squared				29.519***	29.505***	43.240***
				[2.999]	[2.992]	[8.919]
Ratio of household member				632.373***	632.533***	265.515
with technical degree				[109.205]	[108.979]	[236.676]
Ratio of household member				358.895**	361.075**	-1,156.429*
with post secondary degree				[152.531]	[152.304]	[615.681]
Area of annual crop land				-0.023**	-0.022**	-0.054**
per capita (m^2)				[0.010]	[0.010]	[0.022]
Area of perennial crop land				-0.003	-0.003	-0.036*
per capita (m^2)				[0.013]	[0.013]	[0.021]
Forestry land per capita				0.002	0.002	-0.006
(m^2)				[0.007]	[0.007]	[0.005]
Area of aquaculture water				-0.038	-0.038	-0.047
surface per capita (m^2)				[0.027]	[0.027]	[0.031]
Road to village (yes = 1)				30.069	29.815	114.498
				[49.419]	[49.346]	[76.938]
Distance to nearest daily				0.326	0.319	0.725
market (km)				[2.857]	[2.853]	[2.519]
Red River Delta	Base-omitted					
North East				-184.348***	-184.409***	
				[60.207]	[59.995]	
North West				-157.031*	-157.042*	
				[93.983]	[93.658]	
North Central Coast				-111.363*	-111.382*	
				[63.447]	[63.220]	
South Central Coast				-165.311**	-165.301**	
				[68.170]	[67.926]	
Central Highlands				-138.650*	-138.705*	
				[83.616]	[83.324]	
North East South				156.528**	156.510**	
				[64.277]	[64.049]	
Mekong River Delta				96.697*	96.663*	
				[56.643]	[56.447]	
Living in urban areas				-118.389*	-118.010*	
				[60.634]	[60.492]	
Dummy variable for 2006	60.852**	60.839**	97.859***	39.405	39.399	69.686**
	[27.533]	[27.552]	[32.321]	[27.273]	[27.295]	[33.886]
Constant	495.59***	495.56***	562.86***	1,904.46***	1,903.31***	2,713.51***
	[23.248]	[23.239]	[27.718]	[105.308]	[105.085]	[329.116]
Observations	8,432	8,432	8,432	8,432	8,432	8,432
Number of households	4,216	4,216	4,216	4,216	4,216	4,216
R-squared	0.01		0.01	0.104		0.07

Robust standard errors in brackets. * significant at 10%; ** significant at 5%; *** significant at 1%.
Source: Author's estimation from VHLSS 2004 and 2006.

Table 5.12. Regressions of the ratio and number of household working members.

Explanatory variables	The ratio of working members in households			The number of working members in households		
	Random effects (no sampling weight)	Fixed effects (no sampling weight)	Fixed effects with sampling weight and cluster correlation	Fixed effects Poisson (no sampling weight)	Random effects (no sampling weight)	Fixed effects with sampling weight and cluster correlation
Public transfers per capita (thousand VND)	-0.000016***	-0.000004	-0.000005	-0.000047***	-0.000018	-0.000018
	[0.000002]	[0.000004]	[0.000005]	[0.000007]	[0.000012]	[0.000013]
Domestic transfers per capita (thousand VND)	-0.000015***	-0.000009***	-0.000011***	-0.000029***	-0.000013**	-0.000019**
	[0.000002]	[0.000002]	[0.000003]	[0.000005]	[0.000007]	[0.000008]
Ratio of members younger than 16 to total household members	0.382969***	0.535524***	0.557634***	-2.375293***	-1.719399***	-1.591811***
	[0.014132]	[0.027685]	[0.032667]	[0.047787]	[0.093710]	[0.110253]
Ratio of members older than 60 to total household members	-0.232018***	-0.253573***	-0.260653***	-0.666633***	-0.562667***	-0.555223***
	[0.012449]	[0.030160]	[0.047527]	[0.042089]	[0.102087]	[0.108782]
Household size	-0.087057***	-0.103748***	-0.111368***	0.522134***	0.521758***	0.458883***
	[0.005229]	[0.009347]	[0.012851]	[0.017684]	[0.031637]	[0.064394]
Household size squared	0.005259***	0.006395***	0.006913***	-0.000162	0.001809	0.007073
	[0.000463]	[0.000807]	[0.001099]	[0.001566]	[0.002732]	[0.006610]
Ratio of household member with technical degree	0.076530***	0.099930***	0.105840***	0.228553***	0.341362***	0.358640***
	[0.016207]	[0.023968]	[0.028146]	[0.054835]	[0.081130]	[0.092228]
Ratio of household member with post secondary degree	0.049526**	0.247522***	0.239809***	0.200565**	0.833628***	0.797131***
	[0.023577]	[0.044103]	[0.055843]	[0.079732]	[0.149283]	[0.175584]
Area of annual crop land per capita (m^2)	0.000007***	0.000004	0.000005	0.000010*	-0.000001	0.000002
	[0.000002]	[0.000003]	[0.000003]	[0.000005]	[0.000009]	[0.000010]
Area of perennial crop land per capita (m^2)	0.000003	0.000002	0.000004*	0.000004	0.000002	0.000008
	[0.000002]	[0.000003]	[0.000003]	[0.000007]	[0.000010]	[0.000008]
Forestry land per capita (m^2)	0.000001	0.000001	0.000001	0.000007*	0.000007	0.000007
	[0.000001]	[0.000002]	[0.000001]	[0.000004]	[0.000005]	[0.000007]
Area of aquaculture water surface per capita (m^2)	0.000004	0.000007	0.000004	0.000009	0.000022	0.000012
	[0.000004]	[0.000006]	[0.000008]	[0.000013]	[0.000019]	[0.000026]
Road to village (yes = 1)	0.000412	0.002517	0.002502	0.003726	-0.001028	0.01421
	[0.007113]	[0.009355]	[0.010194]	[0.024074]	[0.031666]	[0.034545]
Distance to nearest daily market (km)	0.000132	-0.001125**	-0.001323***	-0.00175	-0.005325***	-0.006523***
	[0.000407]	[0.000520]	[0.000424]	[0.001378]	[0.001760]	[0.001900]
Red River Delta	Base-omitted					
North East	0.030974***			0.125874***		
	[0.009907]			[0.033478]		
North West	0.029552*			0.087585*		
	[0.015397]			[0.052031]		
North Central Coast	-0.023184**			-0.123077***		
	[0.010475]			[0.035395]		
South Central Coast	-0.011081			-0.05888		
	[0.011256]			[0.038034]		
Central Highlands	0.009184			-0.063221		
	[0.013717]			[0.046356]		
North East South	-0.037660***			-0.146068***		
	[0.010590]			[0.035785]		
Mekong River Delta	-0.016261*			-0.019884		
	[0.009279]			[0.031357]		
Urban	-0.078730***			-0.256460***		
	[0.009370]			[0.031686]		
Time change (dummy 2006)	-0.014336***	-0.013058***	-0.010233***	-0.039823***	-0.026673**	-0.01988
	[0.003451]	[0.003557]	[0.003933]	[0.011692]	[0.012039]	[0.013132]
Constant	1.033248***	1.012347***	1.022314***	0.966159***	0.642359***	0.743344***
	[0.016954]	[0.027480]	[0.036347]	[0.057338]	[0.093016]	[0.151506]
Observations	8,432	8,432	8,432	7,998	8,432	8,432

Explanatory variables	The ratio of working members in households			The number of working members in households		
	Random effects (no sampling weight)	Fixed effects (no sampling weight)	Fixed effects with sampling weight and cluster correlation	Fixed effects Poisson (no sampling weight)	Random effects (no sampling weight)	Fixed effects with sampling weight and cluster correlation
Number of i	4,216	4,216	4,216	3,999	4,216	4,216
R-squared	0.24	0.13	0.17	0.62	0.40	0.39
Test H0: Coefficients of per capita public and private transfers are equal:						
F-test	0.12	1.21	1.17	3.98	0.13	0.01
P-value	0.73	0.27	0.28	0.05	0.72	0.92

Note: Working people are those who are older than 14 and were working during the past 12 months.
Robust standard errors in brackets.
* significant at 10%; ** significant at 5%; *** significant at 1%.
Source: Author's estimation from VHLSS 2004 and 2006.

Table 5.13. Regressions of annual working hours.

Explanatory variables	Annual working hours per capita			Annual working hours per working members		
	Random effects (no sampling weight)	Fixed effects (no sampling weight)	Fixed effects with sampling weight and cluster correlation	Fixed effects Poisson (no sampling weight)	Random effects (no sampling weight)	Fixed effects with sampling weight and cluster correlation
Public transfers per capita (thousand VND)	-0.0522***	-0.0383***	-0.0373***	-0.0621***	-0.0592***	-0.0578***
	[0.0046]	[0.0079]	[0.0092]	[0.0065]	[0.0116]	[0.0131]
Domestic transfers per capita (thousand VND)	-0.0287***	-0.0146***	-0.0212***	-0.0286***	-0.0117*	-0.0159**
	[0.0034]	[0.0042]	[0.0066]	[0.0048]	[0.0062]	[0.0067]
Ratio of members younger than 16 to total household members	-861.0810***	-642.9849***	-622.3706***	109.2682***	20.892	-9.0937
	[30.5052]	[60.6844]	[62.0347]	[42.2936]	[88.7716]	[92.6954]
Ratio of members older than 60 to total household members	-622.6396***	-594.5159***	-622.3082***	-638.8280***	-560.9002***	-632.9537***
	[26.8412]	[66.1097]	[117.3814]	[37.0456]	[96.7079]	[149.1159]
Household size	-84.5288***	-92.4649***	-105.5759***	111.9003***	109.9197***	120.5789***
	[11.2961]	[20.4873]	[28.0074]	[15.7102]	[29.9697]	[33.6507]
Household size squared	4.3051***	4.9081***	6.1223**	-9.7184***	-8.8168***	-9.3359***
	[1.0004]	[1.7691]	[2.3851]	[1.3924]	[2.5880]	[2.7553]
Ratio of household member with technical degree	204.6983***	207.6632***	219.1755***	189.4508***	110.9633	101.5769
	[35.1042]	[52.5378]	[59.8074]	[49.3263]	[76.8545]	[75.2016]
Ratio of household member with post secondary degree	254.9810***	606.3869***	567.0869***	253.9589***	365.4885***	279.2423*
	[50.9199]	[96.6728]	[119.5864]	[70.7382]	[141.4170]	[164.9665]
Area of annual crop land per capita (m^2)	-0.0047	-0.0023	-0.0015	-0.0160***	-0.0003	0.0011
	[0.0034]	[0.0061]	[0.0073]	[0.0048]	[0.0089]	[0.0095]
Area of perennial crop land per capita (m^2)	-0.0045	0.0029	0.0053	-0.0066	0.0052	0.0072
	[0.0042]	[0.0065]	[0.0060]	[0.0059]	[0.0094]	[0.0094]
Forestry land per capita (m^2)	-0.0001	0.0025	0.0044	-0.0052	-0.0021	0.001
	[0.0023]	[0.0035]	[0.0038]	[0.0032]	[0.0051]	[0.0082]
Area of aquaculture water surface per capita (m^2)	0.0026	0.0246**	0.0195	-0.0002	0.0457***	0.0386*
	[0.0086]	[0.0121]	[0.0190]	[0.0122]	[0.0177]	[0.0202]
Road to village (yes = 1)	33.3237**	27.8409	31.555	64.2529***	57.2822*	57.1035*
	[15.4357]	[20.5064]	[21.1830]	[21.8505]	[29.9976]	[31.5280]
Distance to nearest daily market (km)	-0.9876	-1.255	-1.0599	-1.5618	1.1088	1.7374
	[0.8843]	[1.1397]	[1.4075]	[1.2549]	[1.6672]	[1.9975]
Red River Delta	Base-omitted					
North East	70.3721***			46.6676		
	[21.2961]			[29.0598]		
North West	-4.4096			-46.4395		
	[33.1057]			[45.2224]		
North Central Coast	-35.6299			-2.1825		
	[22.5110]			[30.6886]		
South Central Coast	-28.5502			-5.8663		
	[24.1898]			[32.9778]		
Central Highlands	-18.8099			-34.9837		
	[29.4929]			[40.2753]		
North East South	58.3876**			171.9581***		
	[22.7620]			[31.0493]		
Mekong River Delta	-89.0716***			-145.3682***		
	[19.9517]			[27.2551]		
Urban	128.9550***			424.9175***		
	[20.2261]			[28.0585]		
Time change (dummy 2006)	14.6515*	15.4553**	16.8698*	40.6626***	33.9214***	30.2728**
	[7.5408]	[7.7960]	[9.4689]	[10.9984]	[11.4043]	[13.9251]
Constant	1,565.56***	1,528.79***	1,558.68***	1,429.47***	1,497.35***	1,497.67***
	[36.6225]	[60.2355]	[81.8858]	[50.9213]	[88.1150]	[99.4892]

Explanatory variables	Annual working hours per capita			Annual working hours per working members		
	Random effects (no sampling weight)	Fixed effects (no sampling weight)	Fixed effects with sampling weight and cluster correlation	Fixed effects Poisson (no sampling weight)	Random effects (no sampling weight)	Fixed effects with sampling weight and cluster correlation
Observations	8,432	8,432	8,432	7,998	8,432	8,432
Number of i	4,216	4,216	4,216	3,999	4,216	4,216
R-squared	0.23	0.19	0.08	0.21	0.03	0.10
Test H0: Coefficients of per capita public and private transfers are equal:						
F-test	16.8	7.13	2.1	17.29	13.34	9.27
P-value	0.00	0.01	0.15	0.00	0.00	0.00

Note: Working people are those who are older than 14 and were working during the past 12 months.
Robust standard errors in brackets.
* significant at 10%; ** significant at 5%; *** significant at 1%.
Source: Author’s estimation from VHLSS 2004 and 2006.

Table 5.14. Regressions of per capita income.

Explanatory variables	**Random effects (no sampling weight)**	**Fixed effects (no sampling weight)**	**Fixed effects with sampling weight and cluster correlation**	**Random effects (no sampling weight)**	**Fixed effects (no sampling weight)**	**Fixed effects with sampling weight and cluster correlation**
Public transfers per capita (thousand VND)	0.846***	0.764***	0.807***	0.574***	0.718***	0.754***
	[0.052]	[0.086]	[0.085]	[0.052]	[0.086]	[0.086]
Domestic transfers per capita (thousand VND)	0.874***	0.756***	0.734***	0.809***	0.733***	0.717***
	[0.038]	[0.046]	[0.052]	[0.037]	[0.046]	[0.053]
Ratio of members younger than 16 to total household members				-1,209.630***	41.244	794.221
				[340.601]	[654.724]	[1,115.817]
Ratio of members older than 60 to total household members				-2,897.496***	-1,852.319***	-1,744.062*
				[300.478]	[713.258]	[1,039.516]
Household size				-351.140***	-696.568***	-705.331*
				[125.904]	[221.038]	[373.200]
Household size squared				7.478	35.461*	38.979
				[11.145]	[19.087]	[27.459]
Ratio of household member with technical degree				4,369.924***	2,070.701***	2,469.826***
				[389.054]	[566.831]	[697.994]
Ratio of household member with post secondary degree				10,634.727***	4,308.865***	4,787.906***
				[567.907]	[1,043.004]	[1,853.079]
Area of annual crop land per capita (m^2)				0.588***	0.617***	0.661***
				[0.038]	[0.066]	[0.144]
Area of perennial crop land per capita (m^2)				0.428***	-0.036	-0.091
				[0.047]	[0.070]	[0.199]
Forestry land per capita (m^2)				0.031	0.024	0.027
				[0.026]	[0.038]	[0.062]
Area of aquaculture water surface per capita (m^2)				0.664***	0.359***	0.345***
				[0.096]	[0.130]	[0.129]
Road to village (yes = 1)				366.511**	404.257*	551.506**
				366.511**	404.257*	551.506**
Distance to nearest daily				[170.391]	[221.243]	[226.240]
market (km)				-31.360***	-9.752	-9.493*
Red River Delta	Base-omitted				[9.749]	[12.296]
North East				-532.061**		
				[239.992]		
North West				-1,816.938***		
				[372.864]		
North Central Coast				-939.537***		
				[253.812]		
South Central Coast				-3.238		
				[272.737]		
Central Highlands				-760.104**		
				[332.220]		
North East South				1,733.176***		
				[256.558]		
Mekong River Delta				475.643**		
				[224.707]		
Living in urban areas				2,441.156***		
				[225.893]		
Dummy variable for 2006	1,135.392***	1,156.466***	1,089.688***	1,000.029***	1,057.636***	1,003.889***
	[81.878]	[82.434]	[101.077]	[81.995]	[84.111]	[113.807]
Constant	4,980.469***	5,067.907***	5,366.408***	5,220.098***	6,617.513***	6,546.798***
	[92.997]	[67.703]	[64.992]	[408.333]	[649.881]	[1276.726]

Explanatory variables	Random effects (no sampling weight)	Fixed effects (no sampling weight)	Fixed effects with sampling weight and cluster correlation	Random effects (no sampling weight)	Fixed effects (no sampling weight)	Fixed effects with sampling weight and cluster correlation
Observations	8,432	8,432	8,432	8,432	8,432	8,432
Number of households	4,216	4,216	4,216	4,216	4,216	4,216
R-squared	0.11	0.13	0.11	0.3	0.16	0.17
Test H0: Coefficients of per capita public and private transfers are equal:						
F-test	0.18	0.01	0.6	13.6	0.03	0.16
P-value	0.67	0.93	0.44	0.00	0.87	0.69
Hausman test χ^2 (prob) (H0: Difference in coefficients in fixed and random effects regression not systematic)		84.34(0.000)			72.35(0.000)	

Robust standard errors in brackets.
* significant at 10%; ** significant at 5%; *** significant at 1%.
Source: Author’s estimation from VHLSS 2004 and 2006.

Table 5.15. Regression of per capita expenditure.

Explanatory variables	Random effects (no sampling weight)	Fixed effects (no sampling weight)	Fixed effects with sampling weight and cluster correlation	Random effects (no sampling weight)	Fixed effects (no sampling weight)	Fixed effects with sampling weight and cluster correlation
Public transfers per capita (thousand VND)	0.379***	0.136***	0.162***	0.142***	0.100**	0.117**
	[0.028]	[0.041]	[0.056]	[0.026]	[0.041]	[0.056]
Domestic transfers per capita (thousand VND)	0.569***	0.452***	0.447***	0.516***	0.421***	0.412***
	[0.019]	[0.022]	[0.068]	[0.018]	[0.022]	[0.069]
Ratio of members younger than 16 to total household members				-1,562.752***	-630.642**	-540.747
				[171.155]	[310.855]	[333.894]
Ratio of members older than 60 to total household members				-1,323.541***	-1,047.050***	-983.324
				[151.773]	[338.646]	[646.102]
Household size				-359.691***	-705.201***	-824.491***
				[63.056]	[104.946]	[159.557]
Household size squared				12.358**	35.173***	46.497***
				[5.577]	[9.062]	[14.457]
Ratio of household member with technical degree				2,709.662***	711.208***	786.982**
				[192.811]	[269.124]	[389.883]
Ratio of household member with post secondary degree				8,048.395***	1,987.302***	1,985.190**
				[284.784]	[495.205]	[932.498]
Area of annual crop land per capita (m^2)				0.118***	0.119***	0.117***
				[0.019]	[0.031]	[0.024]
Area of perennial crop land per capita (m^2)				0.153***	0.123***	0.128***
				[0.023]	[0.033]	[0.036]
Forestry land per capita (m^2)				-0.01	-0.026	-0.032***
				[0.013]	[0.018]	[0.010]
Area of aquaculture water surface per capita (m^2)				0.153***	0.034	0.032
				[0.047]	[0.062]	[0.067]
Road to village (yes = 1)				9.82	-33.86	33.494
				[83.834]	[105.044]	[124.570]
Distance to nearest daily market (km)				-11.984**	-2.718	-3.923
				[4.785]	[5.838]	[3.172]
Red River Delta	Base-omitted					
North East			-643.976***			
			[122.826]			
North West			-1,126.563***			
			[190.627]			
North Central Coast			-638.732***			
			[130.019]			
South Central Coast			-3.249			
			[139.710]			
Central Highlands			-516.445***			
			[169.883]			
North East South			1,171.448***			
			[131.345]			
Mekong River Delta			112.034			
			[114.875]			
Living in urban areas			2,153.831***			
			[113.708]			
Dummy variable for 2006	526.461***	570.941***	550.712***	441.581***	505.639***	483.726***
	[39.294]	[39.178]	[50.505]	[39.315]	[39.935]	[54.171]
Constant	3,900.198***	4,035.709***	4,272.106***	4,909.112***	6,472.702***	6,900.150***
	[51.859]	[32.177]	[55.191]	[204.735]	[308.556]	[445.349]

Explanatory variables	Random effects (no sampling weight)	Fixed effects (no sampling weight)	Fixed effects with sampling weight and cluster correlation	Random effects (no sampling weight)	Fixed effects (no sampling weight)	Fixed effects with sampling weight and cluster correlation
Observations	8,432	8,432	8,432	8,432	8,432	8,432
Number of households	4,216	4,216	4,216	4,216	4,216	4,216
R-squared	0.16	0.14	0.15	0.46	0.18	0.16
Test H0: Coefficients of per capita public and private transfers are equal:						
F-test	29.98	46.68	15.11	141.03	49.68	16.8
P-value	0.00	0.00	0.00	0.00	0.00	0.00
Hausman test χ^2 (prob) (H0: Difference in coefficients in fixed and random effects regression not systematic)		132.9(0.000)			269.65(0.000)	

Robust standard errors in brackets.
* significant at 10%; ** significant at 5%; *** significant at 1%.
Source: Author's estimation from VHLSS 2004 and 2006.

Chapter 6 The impact of international remittances on poverty and inequality[35]

6.1 Introduction

During the last decade, the development impact of international remittance flows has increasingly become a subject of policy discussions, because these flows represent a substantial part of financial resources, especially from developed to developing countries (Chami *et al.*, 2003). Foreign direct investment is still the largest flow of external funding for the entire group of developing countries, but international remittances represent the second most important external capital flow (Adams, 2006). The average inflow of remittances even surpasses official development flows in middle-income countries, and foreign direct investment in low-income countries. In 2005, the total flow of international remittances amounted to US$ 250 billion, and constituted 5-10 percent of total GDP in developing countries (World Bank, 2005a). The amount of international remittances to developing countries in 2005 was even 50 percent larger than the level of development aid (World Bank, 2008a). The rising trend of international remittances is unlikely to reverse in the medium to long term. It is even expected that remittance flows will keep growing at a 7-8 percent annual rate (World Bank, 2005b).

The significance of remittances for developing countries also becomes clear from the high proportion of households for which remittances are an important source of income. For instance, Rodriguez (1996) shows that 17 percent of Philippines' poor households receive international remittances, Cox *et al.* (1998) estimate that about 25 percent of Peruvian households receive remittances, and Cox and Ureta (2003) find that about 14 percent of the households in El Salvador receive considerable amounts of international remittances.

International remittances are also attracting increasing attention since they are supposed to play a crucial role in improving economic growth and reducing poverty in developing counties. It is even argued that facilitating international remittances may be very important in achieving the Millennium Development goals. Yet, the existing empirical evidence shows that many key questions regarding the impact of international remittances on developing countries remain unanswered. The literature points at beneficial but also detrimental effects of international remittances on the economy of the migrant-sending countries. Several studies conclude that on average remittances positively affect economic growth in developing countries (see e.g. the survey paper by Rapoport and Docquier, 2005, p. 75). The channel by which this occurs is still unclear, though. Some authors argue that remittance inflows directly augment income, and increase capital availability for consumption in receiving countries. Remittance inflows can also create multiplier effects in local economies on GDP, job creation, consumption, income and investment (Stahl and Arnold, 1986; De Vasconcelos, 2005; and Ratha and Shaw, 2007). Remittances may provide finance for investment, notably for small-scale projects, and hence may stimulate production (Solimano, 2003). Some studies, however, argue that remittances are used unproductively and mostly spent on consumption (see Rapoport and Docquier, 2005). Other studies suggest that remittances are used productively.

[35] This chapter is written based on the paper Nguyen, V.C., Van den Berg M. and Lensink R. (2009), 'The Impact of International Remittances on Income, Work Efforts, Poverty and Inequality: New Evidence for Vietnam', which is currently submitted to a journal for possible publication.

Estimates show that around 10 percent of remittance receipts are being saved, invested, and used for entrepreneurial activity (Orozco and Fedewa, 2005). Similarly, based on a survey of the literature, Adams (2006) concludes that international remittances have a more substantial effect on households investments, like education and housing, than on consumption. A large inflow of international remittances can also lower the chance of a financial crisis since it helps to reduce current account reversals (Bugamelli and Paternò, 2005). However, a large inflow of remittances may also have negative effects on growth, since it may reduce export competitiveness in the remittance-receiving country on account of a sharp currency appreciation (World Bank, 2005b; and Cordova and Olmedo, 2006). Moreover, remittances may promote idleness on the part of the recipients, and consequently may have a negative effect on the work efforts of recipients (Chami *et al.*, 2005).

One of the most contentious issues regarding the impact of remittances concerns their effect on poverty reduction and income inequality. Indeed, the impact on poverty reduction and inequality is central in any attempt to examine the overall effect of international remittances in developing countries. It is argued that international remittances may help to reduce poverty in the developing world without increasing debt or administrative burden since remittances are a person-to-person flow of money without government intervention. Yet, it is still far from clear whether and how international remittances reduce poverty and income inequality. Several authors find evidence that the inflow of international remittances reduce poverty. For instance, Anyanwu and Erhijakpor (2008) in a cross-country study for 33 African countries show that international remittances have a significant poverty-reducing effect. In addition, Adams (2006) finds that international remittances reduce poverty in Guatemala and Mexico, e.g. since in these countries international migrants come from the poorest group of households, and remittances are sent to relatively poor households. Moreover, Adams *et al.* (2008) show that international remittances have a poverty-reducing effect in Ghana. In contrast, Cattaneo (2005), in a cross-country study, does not find any effect of international remittances on poverty. Stahl (1982) even argues that international remittances may eventually even lead to an increase in poverty since poor households would not benefit from the inflow of international remittances. The empirical evidence on the impact of international remittances on income inequality even seems to be more pessimistic. Acosta (2007) suggests that the effect of remittances on inequality is mixed. He finds that for some countries remittances increase inequality, whereas for other countries inequality reduces. However, Adams *et al.* (2008) find that international remittances in Ghana increase income inequality. A similar outcome is found by Azam and Gubert (2006). Based on surveys performed in Mali and Senegal, they argue that migrants mainly come from rich families, and that especially the rich families receive most remittances. Hence, the existing studies show a wide diversity of empirical results, which calls for more empirical studies to better understand the economic effects of international remittances.

The aim of this study is to provide new empirical evidence on the impact of international remittances. This study has several special features. First, we concentrate on one country, Vietnam. For several reasons, Vietnam is an interesting case to look at. International remittances to Vietnam are increasing in size and importance. However, there are no recent empirical analyses on the impact of international remittances on welfare in Vietnam available. Niimi *et al.* (2008) investigate the determinants of remittances in Vietnam, but they concentrate on internal remittances. Since, as is e.g. argued by Adams at al. (2008), it is highly likely that international and internal remittances will have differing effects on poverty and inequality, the study by Niimi *et al.* (2008) cannot provide any evidence on the impact of international remittances. Nguyen *et al.* (2008) study the effects of migration. They also deal with remittances, but only indirectly. Vietnam is also interesting to look at since over the past decade Vietnam has achieved a remarkable result in the fight against poverty. The incidence of poverty, according to the international poverty line, declined from 58 percent to 20 percent between 1993 and 2004 (Vietnamese Academy of Social Science, 2007). The impact of international remittances on household welfare in Vietnam remains an open question, though. Our study aims to provide new evidence on this important issue. Second, this study is the first study that uses data from the two most recent Vietnam Household and Living Standard Surveys (VHLSS) of 2004 and 2006 to estimate the impact of remittances. The use of two years of data allows us to use panel techniques. This dramatically improves the estimation strategy since by using panel data biases that arise due to omitted variables, endogeneity and selection can be addressed. Third, we estimate the impact of international remittances on a series of indicators, so that our study provides new evidence for the most important direct effects of remittances. More specifically, we focus on the effect of remittances on per capita income, per capita expenditure (consumption), work efforts, poverty and inequality. We will show that a rise in international remittances increases household income and expenditure. However, we will also show that international remittances decrease work efforts, have no impact on poverty reduction, and lead to a minor increase in inequality. Although the empirical analysis deals with Vietnam, we expect our results to be important for a wider group of emerging and developing economies. At the very least, our study shows that international remittances are not a panacea for poverty reduction, which may have important policy implications.

The remainder of this chapter is organised as follows. The second section presents data on international remittances and poverty in Vietnam. The third section presents impact estimation results and the fourth section draws conclusions.

6.2 Poverty and remittances in Vietnam

Recently, international remittances have become an increasing source of external funding for Vietnam. Figure 6.1 shows that international remittances increased from 26.5 to 57.9 thousand billion VND, in prices of 2001, during the period 2001-2007. Table 6.1 presents the distribution of international remittances over the poor and non-poor in 2004 and 2006. It shows that international remittances are not pro-poor. In 2004 and 2006, the percentage of the poor receiving remittances was only 1.3 and 1.8 percent, respectively. Moreover, the bulk of international remittances went to the non-poor. Some 97 percent of the remittances receiving households are non-poor. In terms of amount, even more than 99 percent of the international remittances inflow is distributed to non-poor households. The average value of remittances of the non-poor is even more than 5 times higher than the average value of remittances to the poor. Also in terms of percentages of household income and expenditure, remittances to the non-poor are much higher than remittances to the poor.

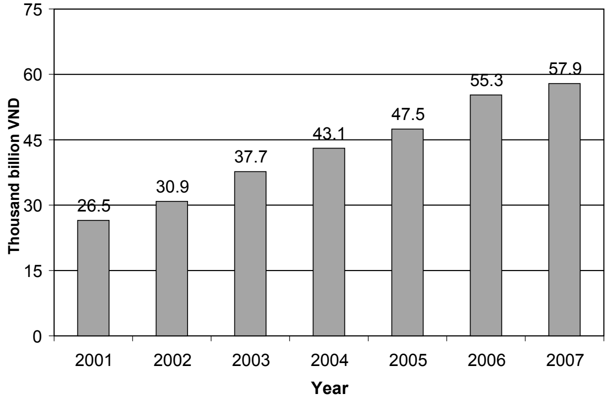

Figure 6.1. International remittances in Vietnam over time (in 2001 prices).[36] *Source: Vietnam Economy Newspapers (www.vneconomy.com.vn).*

Table 6.1. International remittances among the poor and non-poor.

Indicators	2004			2006		
	Poor	Non poor	All	Poor	Non poor	All
% of households receiving remittances	1.3	8.3	7.1	1.8	7.7	6.9
	[0.3]	[0.4]	[0.3]	[0.4]	[0.4]	[0.3]
Remittances per capita (thousand VND)*	851.8	4,744.1	4,626.6	912.0	4,919.2	4,781.3
	[299.1]	[390.1]	[379.9]	[228.5]	[482.8]	[467.7]
Distribution of receiving households	3.0	97.0	100	3.4	96.6	100
	[0.7]	[0.7]		[0.8]	[0.8]	
Distribution of remittance amount	0.9	99.1	100	0.9	99.1	100
	[0.4]	[0.4]		[0.3]	[0.3]	
% of remittances in household expenditure	50.5	52.9	52.8	54.7	60.6	60.6
	[17.5]	[4.0]	[4.0]	[13.8]	[5.9]	[5.9]
% of remittances in household income	27.6	38.2	38.1	24.8	39.8	39.6
	[7.8]	[2.1]	[2.1]	[5.9]	[2.7]	[2.7]
Number of observations	1,769	7,419	9,188	1,427	7,762	9,189

Note: * in 2004.prices.
Standard errors in brackets (corrected for sampling weights and cluster correlation).
Source: Author's estimation from VHLSS 2004 and 2006.

Table 6.2 shows that urban households are more likely to receive remittances than rural households. In 2006, the proportion of households receiving remittances was 11.6 percent and 5.1 percent in the urban and rural areas, respectively. The average size of international remittance inflows was also larger in urban areas.

[36] Note: 1 USD = 15084 VND in 2001

Table 6.2. International remittances received by urban and rural households.

Indicators	2004			2006		
	Urban	Rural	All	Urban	Rural	All
% of households receiving remittances	13.8	4.7	7.1	11.6	5.1	6.9
	[0.9]	[0.3]	[0.3]	[0.9]	[0.3]	[0.3]
Remittances per capita (thousand VND)*	5,352.5	3,861.9	4,626.6	5,320.4	4,319.0	4,781.3
	[633.1]	[392.5]	[379.9]	[828.9]	[497.5]	[467.7]
Distribution of receiving households	51.3	48.7	100	46.2	53.8	100
	[2.7]	[2.7]		[2.7]	[2.7]	
Distribution of remittance amount	56.7	43.3	100	50.0	50.0	100
	[4.6]	[4.6]		[5.8]	[5.8]	
% of remittances in household expenditure	44.9	71.2	52.8	51.3	74.8	60.6
	[4.8]	[6.9]	[4.0]	[7.8]	[8.5]	[5.9]
% of remittances in household income	35.2	43.5	38.1	37.9	41.7	39.6
	[2.9]	[2.8]	[2.1]	[4.2]	[3.0]	[2.7]
Number of observations	2,250	6,938	9,188	2,307	6,882	9,189

Note: * in 2004 prices.
Standard errors in brackets (corrected for sampling weight and cluster correlation).
Source: Author's estimation from VHLSS 2004 and 2006.

Table 6.3 presents changes in welfare and poverty for different household groups over the 2004-2006 period. It appears that households receiving international remittances in both years have higher income and expenditure per capita, and lower poverty than households never receiving remittances. The impact of changes in remittance status is, however, unclear. On the one hand, the strongest decline in the poverty rate is experienced by households receiving remittances neither in 2004, nor in 2006. On the other hand, it appears that households who do receive remittances in 2004, but not in 2006, experience an increase in poverty, whereas the opposite is true for households who receive remittances in 2006, but not in 2004.

If anything, this section suggests that an increase in international remittances will increase income. However, it also suggests that the effects on poverty reduction are probably minor since international remittances primarily go to the non-poor. It may even be the case that an increase in international remittances increases inequality. The remainder of this chapter analyses these issues in detail.

6.3 Impact estimation results

This section presents the estimation results regarding the effects of international remittances on per capita income, per capita expenditure, work efforts, and on aggregate poverty and inequality. We applied the same method of impact measurement which is used to measure the impact of credit in Chapter 4 (presented in section 4.3 of Chapter 4). We use panel data from VHLSS 2004-2006 to regress income per capita, expenditure per capita, and different proxies for work effort, on remittances per capita and other control variables. We use fixed and random effects regression. The advantage of these techniques is that they control for time invariant unobserved variables which are correlated with both income (expenditure) and remittances.

Table 6.3. Per capita expenditure and income (thousand VND), and the poverty indexes of households over the period 2004-2006.

	Not receiving remittances in both 2004 and 2006			Receiving remittances in 2004, but not in 2006			Receiving remittances in 2006, but not in 2004			Receiving remittances in both 2004 and 2006		
	2004	2006	Change	2004	2006	Change	2004	2006	Change	2004	2006	Change
Per capita expenditure*	4,221.8	4,913.4	691.6	8,189.7	8,427.4	237.7	5,488.1	6,724.7	1,236.6	9,437.8	9,468.2	30.3
	[70.5]	[94.9]	[68.5]	[673.9]	[753.4]	[487.8]	[352.0]	[382.5]	[272.1]	[843.5]	[830.9]	[688.5]
Per capita income*	5,542.0	6,740.7	11,98.7	11,015.	10,340.	-6,74.7	6,869.0	11,767.	4,898.4	13,665.	14,633.	968.4
	[111.3]	[121.3]	[90.4]	[874.3]	[827.1]	[675.9]	[513.0]	[1,182.	[1,230.3]	[1,668.0	[1,422.1	[1,106.3]
Poverty rate (%)	19.01	14.35	-4.67	5.10	5.17	0.07	8.31	5.80	-2.51	1.80	1.70	-0.10
	[0.81]	[0.73	[0.63]	[2.07]	[2.04]	[2.22]	[2.07]	[1.93]	[2.57]	[1.26]	[1.21]	[1.00]
Poverty gap index	0.0460	0.0338	-0.0122	0.0118	0.0116	-0.0002	0.0169	0.0100	-0.0069	0.0014	0.0045	0.0030
	[0.0026]	[0.0022	[0.0016]	[0.0055	[0.0048]	[0.0051]	[0.0049]	[0.0039	[0.0051]	[0.0011]	[0.0036]	[0.0026]
Poverty severity index	0.0167	0.0119	-0.0048	0.0040	0.0031	-0.0009	0.0046	0.0027	-0.0019	0.0001	0.0013	0.0012
	[0.0012]	[0.0010	[0.0008]	[0.0021	[0.0014]	[0.0016]	[0.0017]	[0.0013	[0.0017]	[0.0001]	[0.0012]	[0.0011]
Number of observations			3,800			127			170			119

Note: * in 2004 prices.
Standard errors in brackets (corrected for sampling weight and cluster correlation).
The poverty indexes are calculated using per capita expenditure and the expenditure poverty line of WB-GSO. The formulas of the poverty indexes are presented in section 4.
Source: Author's estimation from VHLSS 2004 and 2006.

Control variables include household composition, education of household members, land and housing, villages, urbanity, and regional variables. It should be noted that we use two village level variables, distance to the nearest market, and a dummy variable indicating whether the village has a road. The VHLSS data sets only provide information on these variables for the rural area. Since our sample includes the urban and rural area, we had to come up with estimates for the urban areas. We assumed that for urban areas, the variables 'distance to market' and 'have a road' are equal to 0 and 1, respectively. This is a reasonable assumption given the fact that in all cities there is a market and at least one road.[37]

The complete list of the variables and summary statistics are presented in Table 6.7 in Appendix 6.1. In order to control for inflation, we have deflated all variables in terms of 2004 prices.

6.3.1 Impact on household income and expenditure

Tables 6.8 and 6.9 in Appendix 6.1 present the regression results with respect to the impact of remittances on per capita income and per capita expenditure, respectively. We present both random effects and fixed effects estimates, without and with sampling weight and cluster correlation. Since the Hausman tests strongly favour the fixed effects estimates we focus the discussion on the fixed effects estimates.

International remittances had a significant positive effect on per capita income and per capita expenditure. For all regressions presented, the coefficient for international remittances is highly significant. An increase in remittances had, however, a much smaller impact on consumption than on income. An increase of 1 VND in per capita remittances resulted in an increase of 0.85 VND in per capita income and of only 0.08 VND in per capita expenditure. This suggests that households made only limited use of remittances for daily consumption. Improvement and construction of houses appear to be an important alternative use: these expenses, which are not included in the total expenditure measure used in the remainder in the chapter, increased by 0.3 for each VND of remittances. This accounts for about 40 percent of the difference between income and expenditures. We did not find a significant direct effect of remittances on the purchase of physical assets (tools, implements, etc), but these may require more long-term saving.[38]

Table 6.4 presents the ATT for the effect of remittances on per capita income and per capita expenditure. The advantage of ATT over the regression coefficient is that it gives a better estimate of the total increase in per capita income and expenditure caused by remittances. Since ATT depends on the size of remittances, it differs for 2004 and 2006. The table shows that remittances on average increase per capita income of recipients by 3148 and 3602 thousand VND in 2004 and 2006, respectively. In other words, remittances help increase per capita income of the recipient by around 40 and 47 percent in 2004 and 2006, respectively. The effect of international remittances on per capita expenditure, however, is much smaller: only 285 and 326 thousand VND in 2004 and 2006, respectively.

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[37] We tested whether international remittances have a different impact in rural and urban areas by including interaction terms for international remittances and a dummy for living in an urban area. These estimates indicate that the effects of remittances do not differ between urban and rural areas. We, therefore, only present the estimates for the entire sample.

[38] Estimates available from the authors on request.

Table 6.4. Impact of international remittances measured by ATT.

	2004			2006		
	Y_1	Y_0	ATT ($Y_1 - Y_0$)	Y_1	Y_0	ATT ($Y_1 - Y_0$)
Income per capita	11,052.6***	7,905.0***	3,147.5***	11,281.8***	7,679.9***	3,601.9***
	[500.1]	[432.5]	[317.8]	[507.9]	[364.8]	[432.1]
Expenditure per capita	7,984.8***	7,700.2***	284.7**	7,445.4***	7,119.7***	325.8**
	[331.2]	[349.7]	[132.3]	[289.4]	[314.3]	[158.9]
Difference in ATT between			2,862.9***			3,276.1***
income and expenditure			[309.7]			[413.9]

* significant at 10%; ** significant at 5%; *** significant at 1%.
Standard errors in brackets. Standard errors are corrected for sampling weights and estimated using bootstrap (non-parametric) with 500 replications.
Source: Author's estimation from VHLSS 2004 and 2006.

6.3.2 Impact on work efforts

Table 6.8 in Appendix 6.1 indicates that some crowding out of remittances takes place since there is not a one to one increase in income if remittances increase. This may be caused by a decrease in work efforts. We try to present some evidence for this by presenting regression results on the impact of remittances on different proxies for working time. More specifically, we estimate the effect of remittances on the percentage of household members that work (for persons older than 14), the number of working household members, the total annual working hours per capita, and total working hours per working household member. These regression results are reported in Tables 6.10 and 6.11 in the Appendix. The tables show that remittances lead to a significant reduction in the percentage of household members that work, and in per capita annual working hours.

In order to better understand the effect of remittances on working time, we also calculate the ATT for these variables.[39] The results are presented in Table 6.5. The table shows that remittances reduce the ratio of working people above 14 year olds by around 3 percentage points. As a result, working hours per capita of the receiving households are reduced by 45 and 59 hours in 2004 and 2006, respectively. However, the effect of remittances on the number of annual working hours per working person is small and not statistically significant.

6.3.3 Impact on poverty and inequality

Table 6.6 presents the impact of international remittances on poverty and inequality. The table suggests that an increase in international remittances does not reduce poverty: not one of the three poverty indices is significantly reduced. The table also shows that international remittances have a significant but very small effect on inequality: inequality increased slightly due to an increase in remittances. Both effects do not come as a surprise given that most remittances go to the non-poor and that, although remittances substantially increase income, they have a limited effect on expenditures (see section 3).

[39] We used Hausman specification tests to test the differences in coefficients between the random and fixed-effects regressions. The test statistics strongly reject the null hypothesis that the differences in coefficients between two regressions are not systematic. Thus we use the fixed-effects regressions.

Table 6.5. Impact of remittances on annual working hours (ATT).

	2004			2006		
	Y_1	Y_0	ATT ($Y_1 - Y_0$)	Y_1	Y_0	ATT ($Y_1 - Y_0$)
Ratio of members engaged in productive activities to the total household members older than 14	65.2***	68.0***	-2.7***	65.4***	68.8***	-3.4***
	[1.3]	[1.4]	[0.8]	[1.2]	[1.5]	[0.9]
Annual working hours per working household member	1,985.7***	1,998.2***	-12.5	1,978.1***	1,994.6***	-16.5
	[47.6]	[52.7]	[25.4]	[43.6]	[55.4]	[33.6]
Annual working hours per capita	993.0***	1,038.2***	-45.2**	1,014.6***	1,074.0***	-59.4**
	[30.1]	[34.9]	[19.5]	[31.2]	[40.7]	[25.0]

Note: working people are those who are above 14 year olds and working, and working hours are calculated for working people.
* significant at 10%; ** significant at 5%; *** significant at 1%
Standard errors in brackets. Standard errors are corrected for sampling weights and estimated using bootstrap (non-parametric) with 500 replications.
Source: Author's estimation from VHLSS 2004 and 2006.

Table 6.6. Impact of international remittances on poverty and inequality.

	2004			2006		
	With remittances	Without remittances	Impact	With remittances	Without remittances	Impact
Poverty of remittance recipients						
P0	0.0412***	0.0591***	-0.0179	0.0406***	0.0593***	-0.0188
	[0.0098]	[0.0162]	[0.0129]	[0.0098]	[0.0153]	[0.0122]
P1	0.0081***	0.0111***	-0.0029	0.0105***	0.0193***	-0.0088
	[0.0026]	[0.0054]	[0.0048]	[0.0033]	[0.0137]	[0.0135]
P2	0.0027***	0.0035***	-0.0008	0.0038***	0.0097***	-0.0059
	[0.0010]	[0.0068]	[0.0068]	[0.0015]	[0.0473]	[0.0474]
All poverty						
P0	0.1949***	0.1962***	-0.0013	0.1597***	0.1610***	-0.0013
	[0.0058]	[0.0058]	[0.0009]	[0.0059]	[0.0060]	[0.0009]
P1	0.0472***	0.0474***	-0.0002	0.0383***	0.0389***	-0.0006
	[0.0019]	[0.0019]	[0.0003]	[0.0019]	[0.0021]	[0.0009]
P2	0.0170***	0.0170***	-0.0001	0.0137***	0.0141***	-0.0004
	[0.0009]	[0.0011]	[0.0005]	[0.0009]	[0.0035]	[0.0034]
All inequality						
Gini	0.3698***	0.3687***	0.0012***	0.3580***	0.3577***	0.0003
	[0.0050]	[0.0050]	[0.0004]	[0.0046]	[0.0046]	[0.0005]
Theil L	0.2235***	0.2221***	0.0014**	0.2117***	0.2110***	0.0008
	[0.0062]	[0.0062]	[0.0006]	[0.0056]	[0.0055]	[0.0006]
Theil T	0.2407***	0.2389***	0.0018**	0.2268***	0.2265***	0.0003
	[0.0077]	[0.0077]	[0.0007]	[0.0074]	[0.0074]	[0.0007]

* significant at 10%; ** significant at 5%; *** significant at 1%.
Standard errors in brackets. Standard errors are corrected for sampling weights and estimated using bootstrap (non-parametric) with 500 replications.
Source: Author's estimation from VHLSS 2004 and 2006.

6.4 Conclusions

This chapter estimates the impact of international remittances on per capita income, per capita expenditure (consumption), work efforts, and poverty and inequality in Vietnam, using the two most recent Vietnam Household Living Standard Surveys for 2004 and 2006. We show that an increase in international remittances leads to a significant increase in income. Yet, we do not find evidence that international remittances reduce poverty. Our analysis even suggests that in the short run international remittances may increase inequality. These effects on poverty and inequality seem unfortunate. However, they are not unexpected given the fact that in Vietnam mainly the non-poor are remittances receivers. Moreover, it appears that the direct impact of international remittances on per capita consumption is small since a substantial part of international remittances is being saved and invested.

It should be noted that our estimates only show direct effects. The estimates do not allow for spill-over effects. Especially if international remittances are used productively, indirect effects on the poor may be substantial. On the other hand, we do not control for home earnings had the migrant stayed at home, which may imply that our estimates are too positive. Estimating the indirect effects of international remittances, allowing for spill-over effects, and controlling for home earnings had the migrant stayed at home is beyond the scope of the chapter, but certainly important for future research.

Overall, our analysis suggests that international remittances neither play an important role in reducing poverty, nor in improving equality. International remittances may play an important role in enhancing production and investment, and hence in reducing poverty in the long run. However, to reduce poverty and inequality in the short run, the government of Vietnam would probably be better to rely on income distribution and poverty reduction programs which are targeted at the poor more directly. Also for other developing countries the role that international remittances may play in terms of poverty reduction should not be exaggerated. Especially for some Asian developing countries, such as the Philippines, Indonesia, Lao, and Cambodia, with a similar economic structure to Vietnam there is no reason to expect that international remittances will have a profound poverty-reducing effect. Our study clearly casts doubts on the hypothesis of many academics and politicians who argue that international remittances may play a crucial role in reducing poverty in developing countries. International remittances may have positive economic effects, especially in the longer run, but are certainly not a panacea for poverty reduction in the short term.

Appendix 6.1 Descriptive statistics and regression results

Table 6.7. Descriptive statistics for households with and without international remittances.

Variables	Type	2004		2006	
		Households with remittances	Households without remittances	Households with remittances	Households without remittances
Household variables					
Ratio of members younger than 16 to total household members	Continuous	0.2390	0.2683	0.1969	0.2409
		[0.0136]	[0.0041]	[0.0121]	[0.0039]
Ratio of members older than 60 to total household members	Continuous	0.1107	0.0937	0.1223	0.0973
		[0.0109]	[0.0030]	[0.0110]	[0.0028]
Household size	Discrete	5.0762	4.8768	4.8805	4.9183
		[0.1659]	[0.0406]	[0.1142]	[0.0466]
Household size squared	Discrete	29.7	26.9	26.5	27.6
		[2.2]	[0.6]	[1.3]	[0.7]
Ratio of members with technical degree to total household members	Continuous	0.0885	0.0573	0.0838	0.0651
		[0.0112]	[0.0029]	[0.0118	[0.0028
Ratio of members with post secondary degree to total household members	Continuous	0.0624	0.0258	0.0546	0.0310
		[0.0103]	[0.0022]	[0.0101	[0.0024]
Area of annual crop land per capita (m^2)	Continuous	510.8	709.6	437.4	763.6
		[72.2]	[28.1]	[58.6]	[33.2]
Area of perennial crop land per capita (m^2)	Continuous	290.1	208.2	169.7	243.4
		[141.5]	[17.9]	[35.8]	[19.7]
Forestry land per capita (m^2)	Continuous	37.6	224.0	108.9	279.3
		[26.2]	[39.9]	[45.1]	[51.0]
Aquaculture water surface per capita (m^2)	Continuous	32.7	56.8	75.4	65.4
		[15.9]	[9.3]	[39.5]	[12.2]
Commune variables					
Road to village (yes = 1)	Continuous	0.9456	0.8509	0.9131	0.9049
		[0.0145]	[0.0099]	[0.0211]	[0.0079]
Distance to nearest daily market (km)	Continuous	0.9565	2.2521	1.0502	2.4313
		[0.1818]	[0.1380]	[0.1773]	[0.1723]
Regional variables					
Household in Red River Delta	Binary	0.1786	0.2116	0.1405	0.2149
		[0.0281]	[0.0114]	[0.0244]	[0.0115]
Household in North East	Binary	0.0323	0.1239	0.0862	0.1201
		[0.0102]	[0.0085]	[0.0177]	[0.0084]
Household in North West	Binary	0.0063	0.0345	0.0091	0.0345
		[0.0045]	[0.0046]	[0.0057]	[0.0046]
Household in North Central Coast	Binary	0.1479	0.1383	0.1310	0.1397
		[0.0280]	[0.0108]	[0.0257]	[0.0110]
Household in South Central Coast	Binary	0.0701	0.0880	0.1034	0.0854
		[0.0164]	[0.0075]	[0.0226]	[0.0074]
Household in Central Highlands	Binary	0.0408	0.0645	0.0293	0.0656
		[0.0162]	[0.0072]	[0.0120]	[0.0074]
Household in North East South	Binary	0.3192	0.1374	0.2658	0.1406
		[0.0415]	[0.0108]	[0.0395]	[0.0111]
Household in Mekong River Delta	Binary	0.2048	0.2017	0.2347	0.1993
		[0.0302]	[0.0110]	[0.0289]	[0.0109]
Household in Living in urban areas	Binary	0.4646	0.2370	0.4171	0.2395
		[0.0403]	[0.0121]	[0.0378]	[0.0124]
Observations		563	8,625	584	8,605

Standard errors in brackets.
Source: Author’s estimation from VHLSS 2004 and 2006.

Table 6.8. Impact of remittances on per capita income.

Explanatory variables	Random effect (no sampling weights)	Fixed-effect (no sampling weights)	Fixed-effect (sampling weights and cluster correlation)	Random effect (no sampling weights)	Fixed-effect (no sampling weights)	Fixed-effect (sampling weights and cluster correlation)
International remittances (thousand VND)	0.917*** [0.021]	0.853*** [0.024]	0.849*** [0.041]	0.896*** [0.019]	0.848*** [0.023]	0.848*** [0.043]
Members younger than 16 / total household members				-1,849.669*** [317.177]	-744.764 [592.755]	-448.3 [712.882]
Members older than 60 / total household members				-2,020.407*** [275.009]	-1,446.877** [644.009]	-1,604.552* [927.439]
Household size				-687.284*** [115.694]	-1,140.166*** [197.594]	-1,312.333*** [220.586]
Household size squared				30.233*** [10.292]	64.304*** [17.162]	79.102*** [18.724]
Members with technical degree/ total members				5,398.712*** [351.413]	2,377.630*** [510.939]	2,749.986*** [740.022]
Members with post secondary degree/ total members				11,452.856*** [518.597]	2,970.618*** [942.386]	3,091.188* [1,655.836]
Area of annual crop land per capita (m^2)				0.579*** [0.035]	0.589*** [0.059]	0.622*** [0.144]
Area of perennial crop land per capita (m^2)				0.420*** [0.043]	-0.069 [0.063]	-0.131 [0.192]
Forestry land per capita (m^2)				0.036 [0.024]	0.033 [0.034]	0.033 [0.062]
Area of aquaculture water surface per capita (m^2)				0.639*** [0.088]	0.320*** [0.118]	0.310** [0.126]
Road to village (yes = 1)				357.211** [157.162]	393.357** [200.113]	552.909** [234.068]
Distance to nearest daily market (km)				-29.269*** [8.982]	-7.937 [11.123]	-8.394 [5.629]
Red River Delta	Base-omitted					
North East				-554.983** [225.195]		
North West				-1,817.819*** [349.823]		
North Central Coast				-987.195*** [238.329]		
South Central Coast				-284.653 [255.609]		
Central Highlands				-821.533*** [311.587]		
North East South				1,373.413*** [240.520]		
Mekong River Delta				294.476 [210.262]		
Urban				2,401.531*** [210.365]		
Time effect (2006 variable)	1,272.601*** [74.338]	1,276.351*** [74.227]	1,240.674*** [84.223]	1,041.546*** [74.567]	1,127.202*** [75.390]	1,086.619*** [81.476]
Constant	5,469.512*** [86.716]	5,485.500*** [52.821]	5,756.272*** [44.378]	6,670.533*** [373.428]	8,511.186*** [576.197]	9,023.166*** [651.007]
Observations	8,432	8,432	8,432	8,432	8,432	8,432
Number of i	4,216	4,216	4,216	4,216	4,216	4,216
R-squared	0.17	0.17	0.17	0.39	0.25	0.24
Hausman test χ^2 (prob)	30.0 (0.000)			194.5(0.00)		

Robust standard errors in brackets. * significant at 10%; ** significant at 5%; *** significant at 1%.
Source: Author's estimation from VHLSS 2004 and 2006.

Table 6.9. Impact of remittances on per capita expenditure.

Explanatory variables	Random effect (no sampling weight)	Fixed-effect (no sampling weight)	Fixed-effect with sampling weight and cluster correlation	Random effect (no sampling weight)	Fixed-effect (no sampling weight)	Fixed-effect with sampling weight and cluster correlation
International remittances (thousand VND)	0.161***	0.084***	0.077**	0.166***	0.080***	0.077***
	[0.012]	[0.013]	[0.031]	[0.011]	[0.013]	[0.029]
Members younger than 16/ total household members				-1,751.376***	-653.115**	-587.531*
				[176.039]	[323.282]	[346.572]
Members older than 60 / total household members				-917.851***	-892.519**	-832.818
				[152.759]	[351.235]	[651.192]
Household size				-596.769***	-973.662***	-1139.451***
				[64.184]	[107.765]	[167.845]
Household size squared				27.810***	51.371***	65.656***
				[5.709]	[9.360]	[15.224]
Members with technical degree / total members				3234.433***	903.029***	923.699**
				[194.720]	[278.660]	[420.860]
Members with post secondary degree/ total members				8,450.103***	1,480.861***	1,447.67
				[287.800]	[513.966]	[1,054.883]
Area of annual crop land per capita (m^2)				0.107***	0.102***	0.094***
				[0.020]	[0.032]	[0.025]
Area of perennial crop land per capita (m^2)				0.151***	0.110***	0.112***
				[0.024]	[0.034]	[0.035]
Forestry land per capita (m^2)				-0.008	-0.026	-0.033***
				[0.013]	[0.019]	[0.010]
Area of aquaculture water surface per capita (m^2)				0.139***	0.011	0.013
				[0.049]	[0.064]	[0.065]
Road to village (yes = 1)				25.578	-3.501	73.475
				[86.985]	[109.139]	[132.414]
Distance to nearest daily market (km)				-11.793**	-2.211	-3.582
				[4.970]	[6.066]	[3.269]
Red River Delta	Base-omitted					
North East				-710.320***		
				[125.288]		
North West				-1,176.680***		
				[194.597]		
North Central Coast				-683.058***		
				[132.610]		
South Central Coast				-124.728		
				[142.225]		
Central Highlands				-580.157***		
				[173.332]		
North East South				1,154.754***		
				[133.819]		
Mekong River Delta				107.106		
				[116.963]		
Urban				2,193.406***		
				[116.780]		
Time effect (2006 variable)	616.732***	621.270***	615.082***	467.003***	533.584***	519.708***
	[40.860]	[40.475]	[53.511]	[41.131]	[41.117]	[57.590]
Constant	4,276.946***	4,296.294***	4,548.855***	5,860.681***	7,511.981***	8,091.677***
	[53.202]	[28.803]	[24.810]	[207.203]	[314.251]	[450.432]
Observations	8,432	8,432	8,432	8,432	8,432	8,432
Number of i	4,216	4,216	4,216	4,216	4,216	4,216
R-squared	0.05	0.04	0.04	0.43	0.16	0.15
Hausman test χ2 (prob)			159.9 (0.000)		443.4 (0.000)	

Robust standard errors in brackets. * significant at 10%; ** significant at 5%; *** significant at 1%.
Source: Author's estimation from VHLSS 2004 and 2006.

Table 6.10. Regressions of the ratio of working members and the number of working members.

Explanatory variables	Working members / total members			Working members		
	Random effect (no sampling weight)	Fixed-effect (no sampling weight)	Fixed-effect with sampling weight and cluster correlation	Fixed-effect Poisson (no sampling weight)	Random effect (no sampling weight)	Fixed-effect with sampling weight and cluster correlation
International remittances (thousand VND)	-0.000006***	-0.000006***	-0.000007***	-0.000014***	-0.000013***	-0.000017***
	[0.000001]	[0.000001]	[0.000002]	[0.000003]	[0.000004]	[0.000005]
Members younger than 16 / total household members	0.392005***	0.540145***	0.567058***	-2.353461***	-1.708024***	-1.567524***
	[0.014255]	[0.027693]	[0.032548]	[0.048023]	[0.093696]	[0.110482]
Members older than 60 / total household members	-0.255982***	-0.257242***	-0.262890***	-0.730356***	-0.572812***	-0.560986***
	[0.012320]	[0.030087]	[0.047100]	[0.041473]	[0.101798]	[0.108241]
Household size	-0.078676***	-0.098372***	-0.102891***	0.541307***	0.531192***	0.475342***
	[0.005209]	[0.009231]	[0.012423]	[0.017555]	[0.031233]	[0.063409]
Household size squared	0.004704***	0.006061***	0.006380***	-0.001434	0.001198	0.006005
	[0.000464]	[0.000802]	[0.001073]	[0.001563]	[0.002713]	[0.006576]
Members with technical degree / total members	0.044635***	0.096389***	0.102978***	0.140401***	0.332328***	0.350237***
	[0.015900]	[0.023870]	[0.027643]	[0.053645]	[0.080763]	[0.091373]
Members with post secondary / total members	0.017463	0.260835***	0.260499***	0.103101	0.854753***	0.836195***
	[0.023317]	[0.044027]	[0.053764]	[0.078559]	[0.148962]	[0.170325]
Area of annual crop land per capita (m^2)	0.000007***	0.000004	0.000006*	0.000010*	0.00000	0.000003
	[0.000002]	[0.000003]	[0.000003]	[0.000005]	[0.000009]	[0.000010]
Area of perennial crop land per capita (m^2)	0.000003	0.000003	0.000005*	0.000004	0.000003	0.000009
	[0.000002]	[0.000003]	[0.000003]	[0.000007]	[0.000010]	[0.000008]
Forestry land per capita (m^2)	0.000001	0.000001	0.000001	0.000007*	0.000007	0.000007
	[0.000001]	[0.000002]	[0.000001]	[0.000004]	[0.000005]	[0.000007]
Area of aquaculture water surface per capita (m^2)	0.000004	0.000008	0.000005	0.00001	0.000022	0.000013
	[0.000004]	[0.000006]	[0.000008]	[0.000014]	[0.000019]	[0.000026]
Road to village (yes = 1)	0.000026	0.002227	0.001892	0.002863	-0.001004	0.013625
	[0.007144]	[0.009349]	[0.010078]	[0.024130]	[0.031632]	[0.034266]
Distance to nearest daily market (km)	0.000098	-0.001141**	-0.001336***	-0.001812	-0.005351***	-0.006548***
	[0.000409]	[0.000520]	[0.000423]	[0.001381]	[0.001758]	[0.001896]
North East	0.032107***			0.126874***		
	[0.010025]			[0.033700]		
North West	0.031512**			0.091586*		
	[0.015582]			[0.052389]		
North Central Coast	-0.022212**			-0.121797***		
	[0.010604]			[0.035645]		
South Central Coast	-0.003892			-0.039766		
	[0.011373]			[0.038228]		
Central Highlands	0.01318			-0.052701		
	[0.013877]			[0.046656]		
North East South	-0.033159***			-0.132133***		
	[0.010705]			[0.035987]		
Mekong River Delta	-0.012134			-0.006609		
	[0.009366]			[0.031489]		
Urban	-0.081426***			-0.263615***		
	[0.009449]			[0.031831]		
Time effect (2006 dummy)	-0.016156***	-0.013672***	-0.011090***	-0.044927***	-0.028738**	-0.022221*
	[0.003439]	[0.003522]	[0.003882]	[0.011654]	[0.011917]	[0.012944]
Constant	0.999363***	0.990560***	0.988902***	0.889214***	0.603264***	0.677731***
	[0.016803]	[0.026919]	[0.033991]	[0.056623]	[0.091079]	[0.145530]
Observations	8,432	8,432	8,432	7,998	8,432	8,432
Number of i	4,216	4,216	4,216	3,999	4,216	4,216
R-squared	0.22	0.13	0.14	0.62	0.57	0.39
Hausman test χ^2 (prob)		72.7 (0.000)			272.8 (0.000)	

Robust standard errors in brackets. * significant at 10%; ** significant at 5%; *** significant at 1%.
Source: Author's estimation from VHLSS 2004 and 2006.

Table 6.11. Regressions of annual working hours.

	Annual working hours per working member			Annual working hours per capita		
Explanatory variables	**Random effect (no sampling weights)**	**Fixed-effect (no sampling weights)**	**Fixed-effect (sampling weights and cluster correlation)**	**Random effect (no sampling weight)**	**Fixed-effect (no sampling weights)**	**Fixed-effect (sampling weights and cluster correlation)**
International remittances	-0.0115***	-0.0109***	-0.0110**	-0.0068**	-0.0055	-0.0023
(thousand VND)	[0.0019]	[0.0024]	[0.0045]	[0.0027]	[0.0035]	[0.0065]
Members younger than 16 /	-840.5761***	-637.4136***	-612.3838***	126.7249***	17.7247	-16.5051
total household members	[30.9290]	[60.8238]	[62.2531]	[42.7032]	[89.1169]	[92.7447]
Members older than 60 /	-692.9007***	-615.2529***	-643.0128***	-722.4424***	-591.9096***	-668.4195***
total household members	[26.6990]	[66.0831]	[115.9812]	[36.6304]	[96.8226]	[148.3619]
Household size	-64.3343***	-77.4442***	-82.7503***	134.2960***	128.6281***	145.3145***
	[11.3086]	[20.2755]	[27.8351]	[15.6676]	[29.7070]	[33.4032]
Household size squared	2.9546***	3.9353**	4.6391**	-11.2184***	-10.0354***	-10.9687***
	[1.0067]	[1.7610]	[2.3511]	[1.3961]	[2.5802]	[2.7208]
Members with technical	106.1335***	186.4422***	195.8052***	68.2896	77.6263	63.1636
degree / total members	[34.5807]	[52.4285]	[58.8290]	[48.3976]	[76.8164]	[74.3786]
Members with post	144.966***	621.331***	587.845***	111.616	366.885***	269.328
secondary / total members	[50.5980]	[96.7001]	[118.6326]	[69.9148]	[141.6816]	[166.6653]
Area of annual crop land	-0.0037	-0.0013	0.0001	-0.0148***	0.0009	0.003
per capita (m^2)	[0.0035]	[0.0061]	[0.0074]	[0.0048]	[0.0089]	[0.0096]
Area of perennial crop land	-0.0041	0.0041	0.0068	-0.0062	0.0067	0.0088
per capita (m^2)	[0.0043]	[0.0065]	[0.0061]	[0.0060]	[0.0095]	[0.0094]
Forestry land per capita	-0.0006	0.0019	0.004	-0.0058*	-0.003	0.0003
(m^2)	[0.0023]	[0.0035]	[0.0040]	[0.0033]	[0.0051]	[0.0084]
Area of aquaculture water	0.0037	0.0256**	0.0207	0.0008	0.0468***	0.0397**
surface per capita (m^2)	[0.0087]	[0.0121]	[0.0187]	[0.0123]	[0.0177]	[0.0199]
Road to village (yes = 1)	32.2659**	27.4813	30.0836	63.0966***	56.6440*	55.3039*
	[15.5647]	[20.5340]	[21.2196]	[21.9999]	[30.0857]	[31.6430]
Distance to nearest daily	-1.0156	-1.2624	-1.0739	-1.554	1.1297	1.7424
market (km)	[0.8911]	[1.1414]	[1.4364]	[1.2630]	[1.6723]	[2.0353]
North East	71.1442***			47.1721		
	[21.6774]			[29.3908]		
North West	0.0205			-41.2948		
	[33.7013]			[45.7492]		
North Central Coast	-34.4369			-1.3202		
	[22.9265]			[31.0523]		
South Central Coast	-7.6895			18.1579		
	[24.5881]			[33.3017]		
Central Highlands	-6.9438			-20.608		
	[30.0125]			[40.7286]		
North East South	72.9059***			187.3539***		
	[23.1476]			[31.3731]		
Mekong River Delta	-74.4765***			-128.4896***		
	[20.2564]			[27.4979]		
Urban	120.5362***			414.8475***		
	[20.4997]			[28.3175]		
Time effect (2006 variable)	8.8296	10.4994	11.2493	33.5535***	25.9731**	21.4611
	[7.5328]	[7.7359]	[9.3207]	[11.0055]	[11.3344]	[13.7098]
Constant	1,485.59***	1,469.28***	1,472.23***	1,343.03***	1,425.39***	1,407.75***
	[36.4743]	[59.1247]	[81.8643]	[50.5076]	[86.6274]	[98.4460]
Observations	8,432	8,432	8,432	8,432	8,432	8,432
Number of i	4,216	4,216	4,216	4,216	4,216	4,216
R-squared	0.21	0.17	0.17	0.16	0.08	0.08
Hausman test χ^2 (prob)		48.8(0.000)			89.3 (0.000)	

Robust standard errors in brackets. * significant at 10%; ** significant at 5%; *** significant at 1%.
Source: Author's estimation from VHLSS 2004 and 2006.

Chapter 7 The impact of work migration and non-work migration on poverty and inequality[40]

7.1 Introduction

Migration is potentially an important strategy for fighting poverty and decreasing inequality. Remittances sent by migrants are remarkable capital flows. Nowadays, the amount of international remittances to developing countries is much higher than foreign assistance (DeWind and Holdaway, 2005). In 2005, the total flow of international remittances amounted to US$ 250 billion, and constituted 5-10% of total GDP in developing countries (World Bank, 2005a). It is even expected that remittance flows will keep growing at a 7-8 percent annual rate (World Bank 2005b). International and internal remittances can help households increase income and consumption. Remittances can also help increase investment and production. The theory on exchange motives (Cox, 1987) argues that people give transfers to others because they want to get some return benefits. This implies that remittances can be invested in physical and social assets for the benefit of the migrant and the remittance recipients. In addition, productivity will increase if remittances help relax liquidity constraints and allow households to invest in high-return capital intensive activities (Stark, 1991; Taylor and Martin, 2001; Taylor and Lopez-Feldma, 2007). Through remittances, migration can thus directly and indirectly increase income and consumption in the home area and, if the poor receive at least some remittances, decrease poverty in the home area. If the poor even receive most remittances, not only poverty but also inequality will decrease.

Yet, migration does not necessarily lead to a significant increase in income or reduction in poverty and inequality in home areas. Remittances may not be sufficient to compensate for the loss of local income previously earned by the migrant. In extreme cases, they could even lower the earned income of the recipients if they provide disincentives to work effort (Farrington and Slater, 2006; Lloyd-Sherlock 2006; Sahn and Alderman, 1996). Migration could also result in labour shortages for migrant-sending households, and thus prevent these households from engaging in high-return but labour-intensive activities (Taylor and Lopez-Feldma, 2007). Regarding inequality, migration can even harm inequality of the departure areas if remittances are mostly sent to the non-poor households.

Empirical findings do not settle the theoretical ambiguity of the impact of migration on home country welfare. For example, Acosta (2007) finds that for some countries remittances increase inequality, whereas the opposite happens in other countries. Adams (2006) suggests that international remittances reduce poverty in Guatemala and Mexico, since in these countries international migrants come from the poorest group of households, and remittances are sent to relatively poor households. However, Azam and Gubert (2006) find that in Mali and Senegal migrants mainly come from rich families, and that especially the rich families receive most remittances. Yang (2004) finds that migration reduced labour hours and income in the Philippines. Hence, the effect of out-migration on poverty and inequality remains an empirical question the answer to which depends on the specific nature of migration in an area.

[40] This chapter is written based on the paper Nguyen, V.C., Van den Berg M. and Lensink R. (2009), 'The Impact of Work Migration and Non-Work Migration on Household Welfare, Poverty and Inequality: Evidence from Vietnam', which is currently submitted to a journal for possible publication.

Also the effect of migration on vulnerability is indefinite. According to insurance theory, migration is a strategy to cope with economic risks or shocks in the absence of complete risk and financial markets (Stark and Levhari, 1982, Stark and Bloom, 1985, Rosenzweig, 1988, Stark, 1991). Migrants will then remit more money when their family members who stayed behind experience a decrease in income. On the other hand, recipients may become dependent on remittances, and fall into poverty when the migrant stops sending money.

In Vietnam, both internal and international migration has increased rapidly over the past decades. According to the 1999 Population and Housing Census, around 6.5 percent of the population over 5 years old changed their place of residence during 1994-1999 (Dang *et al.*, 2003). Between 1998 and 2006, the share of the population living in urban areas increased from 22 to 27% (1998 and 2006 Vietnam Household Living Standard Surveys). The annual number of international work migrants increased by 136 percent from 36 to 85 thousand during the period 2001-2007 (Labor Newspaper, 2008). In addition, an increasing number of women married foreigners. Until 2007, 177 thousand women left the country for marriage (Police Newspaper, 2008).

Most studies argue that the main reasons for economic migration in Vietnam are to find better employment and higher wages (e.g. Dang *et al.*, 2003; Cu, 2005; Brauw and Harigaya, 2007). Industrialization and high economic growth in urban areas increasingly attract rural labour (Dang *et al.*, 1997; Dang, 2001; Cu, 2005). Large flows of foreign direct investment move into industrial zones and companies that create employment for rural people. In addition, there are more landless or near landless households (Ravallion and van de Walle, 2006). The increased shortage of land could push farmers to go for non-farm employment in other areas (Cu, 2005).

There are many studies on migration in Vietnam. However, most of them focus on the pattern and determinants of migration (e.g. Guest 1998; Djamba, 1999; Dang *et al.*, 1997; Dang, 2001; Dang *et al.*, 2003; GSO and UNFPA, 2005; Cu, 2005; Dang and Nguyen, 2006). Only two existing studies have assessed the quantitative impact of migration. The first study is Brauw and Harigaya (2007), who find that seasonal migration increases household expenditure using the Vietnam Living Standard Surveys (VLSS) 1993 and 1998. The second and closest to this paper is Nguyen *et al.* (2008), who evaluate the impact of long-term migration on household expenditure and inequality using the Vietnam Household Living Standard Surveys (VHLSS) from 2002 and 2004. They find that migration increases expenditures but also inequality.

The main objective of this chapter is to estimate the impact of long-term migration for both work and non-work reasons on several welfare indicators at household and country level in Vietnam. Compared to Nguyen *et al.* (2008), we provide not only information on the impact of migration on expenditures and inequality, but also on remittances, work effort, income, and poverty. This results in a deeper understanding of the process in which migration affects expenditures. Also, we consider the relationship between migration and vulnerability through the level of income diversification. Finally, we consider additional economy-wide welfare indicators besides inequality: we assess the impact of migration on three different poverty indicators. We do all this using the two most recent Living Standard Surveys: the VHLSS 2004 and 2006.

The remainder of the chapter is organized as follows. Section 2 presents the definition of migration. Section 3 discusses patterns of migration and household welfare in Vietnam.

Next, the methodology employed is discussed in Section 4. Sections 5 and 6 present the estimation results. Finally, we draw conclusions in Section 7.

7.2 Definition of migration

Like other empirical chapters, this chapter relies on data from VHLSS in 2004 and 2006. An important issue when analyzing migration is how to define migration. Although the surveys did not have explicit questions on migration, we could use special features of the panel of 4,216 households to define migration. 1,395 individuals who were part of these households in 2004 were not covered in the 2006 survey (these people do not include dead people). These people are considered migrants. Since a household's members in VHLSS are defined as those who lived with the households for more than 5 months during the past year, the duration of the defined migration was at least 7 months. The 2006 VHLSS includes a question on the reasons why these people left the households: work, marriage/separate stay, and education/other reasons. The questionnaire did not distinguish between internal and international migration. Based on these data, we defined two types of migration: work migration and non-work migration. Households with work migration had someone leaving for work during the period 2004-2006. Households with so-called non-work migration had someone leaving for marriage, separate stay, education or other reasons. The number of households who sent out work migrants and non-work migrants are 295 and 608, respectively. Around 1 percent of households had both types of migration at the same time.

This definition of migration has both disadvantages and advantages. The main disadvantage is that it can underestimate the total migration. Households who did not have migration between 2004 and 2006 can have migration from before 2004. However, other studies which define households with migration as those having someone currently leaving for work can also underestimate the impacts of migration. This is because other households without current migrating members can have migrating members in the past. The chapter's definition of migration has an important advantage. The defined migration clearly refers to the period 2004-2006. Thus, in this chapter, impact estimates will be attached to the impacts of the migration between 2004 and 2006. In many studies, households with migration are those currently with migrating members.[41] As a result, migration can take place in any time before the survey, and it is not clear about how long the migration lasts. As the impact of migration on household welfare may change over time, merging migration over too long a time period may obscure the results of an impact study.

7.3 Migration and household welfare in Vietnam

Figure 7.1 presents the distribution of migrants over different groups of households. The percentage of households with work and non-work migration was 7.1 percent and 14.3 percent, respectively, with very similar shares for poor and non-poor households. As expected, rural areas had a higher proportion of households with work migration, 8.3 percent compared to 3.8 percent, since people tend to move from rural to urban areas for higher income employment. However, the share of households with non-work migration was somewhat higher in urban areas.

[41] For example, in Hoddinott (1994), Barham and Boucher (1995), Niimi and Ozden (2006), and Mora and Taylor (2006).

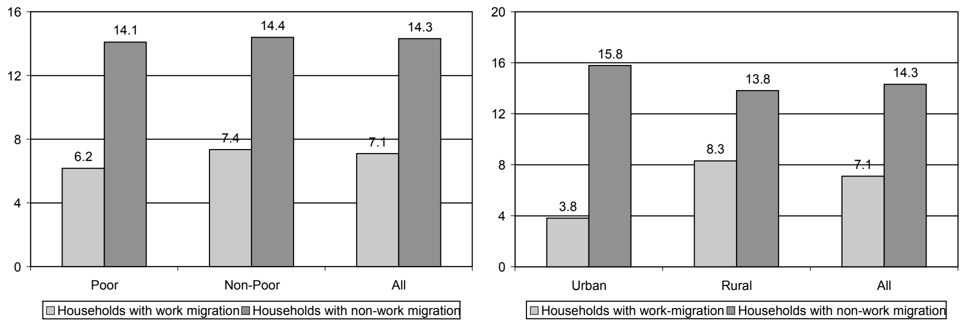

Figure 7.1. Distribution of migrants (% of all households). Source: Authors' calculations using VHLSS 2004 and 2006.

Households with people migrating between 2004 and 2006 could be expected to have experienced an increase in welfare over that period, especially when the migrants left for work. Table 7.1 shows averages of a number of welfare indicators for households with and without work-migration. The indicators include labour efforts, remittances, income, income diversification, consumption expenditure, and poverty indexes.

To examine how the labour efforts of household members change with migration, the labour variables of migration households in 2004 are constructed for only non-migrating members. As can be seen, the ratio of working members in households was quite similar between households with and without work migration. However, annual working hours per labourer for households with migration were slightly lower for households without migration.

Households with work migration experienced a large increase in both internal and international remittances. Internal and international remittances per capita increased by around 78 percent and 561 percent, respectively. Remittances of households without work migration increased only slightly.

The growth rate of income of households with work migration was higher than that of households without migration: 50 percent compared to 20 percent. Migration household income excluding remittances also increased at a higher rate than that of households without migration.

Migration could imply increased income diversification and thus a reduction of risk. To measure income diversification, we use the Simpson index (SI). A larger SI means more income diversification: SI ranges between 0 if there is only one source of income and $(1 - 1/k)$ if a household earns income equally from all k possible income sources (see Appendix 7.2). We define seven mutually exclusive income sources: crops, livestock, fishery/forestry/other agricultural activities, non-farm self employment, wages, remittances, transfers, and other income. Since migration can lead to increased remittances, we estimate the SI for total income and income without remittances.

Table 7.1. Household welfares of household with and without work migrants during 2004-2006.

Welfare indicators	Households with work migrants		Households without work migrants	
	2004	2006	2004	2006
Ratio of members engaged in productive activities to the total household members older than 14 years old (%)	81.7	79.9	81.3	78.6
	[1.4]	[1.5]	[0.5]	[0.4]
Annual working hours per capita	1,023.7	1073.7	1,026.9	1,060.7
	[29.6]	[29.2]	[10.7]	[10.6]
Annual working hours per working member	1,592.1	1,693.1	1,828.1	1,868.6
	[39.4]	[39.2]	[16.1]	[14.6]
Per capita internal remittances	472.0	851.0	423.1	435.8
	[76.9]	[69.2]	[19.1]	[20.8]
Per capita international remittances	123.3	911.9	251.9	302.1
	[71.5]	[388.3]	[46.7]	[64.5]
Per capita remittances (both international and internal)	595.3	1,762.9	675.0	737.9
	[103.8]	[398.7]	[51.4]	[68.1]
Per capita income (household income divided by household size)	4,733.4	7,088.5	5,790.0	6,902.0
	[194.5]	[455.7]	[129.0]	[146.2]
Household income divided by the number of working members (above 14 years old)	8,111.2	13,100.0	11,215.1	13,188.0
	[364.8]	[1,414.3]	[310.7]	[372.9]
Household income excluding remittances divided by the number of working members (above 14 years old)	7,028.9	8,917.7	9,773.2	11,540.5
	[283.2]	[404.3]	[233.2]	[261.9]
Ratio of non-farm income to total income (%)	58.1	61.2	64.0	66.7
	[1.7]	[1.8]	[0.8]	[0.8]
Ratio of non-farm income (excluding remittances) to total income (%)	53.2	49.8	60.1	63.1
	[1.9]	[2.3]	[0.8]	[0.8]
Simpson index 1 (all income sources)	0.5630	0.5561	0.4852	0.4719
	[0.0090]	[0.0100]	[0.0042]	[0.0042]
Simpson index 2 (income sources excluding remittances)	0.5120	0.4670	0.4230	0.4108
	[0.0110]	[0.0122]	[0.0048]	[0.0048]
Per capita consumption expenditure	3,608.9	4,612.5	4,329.1	4,848.0
	[129.8]	[148.4]	[87.8]	[90.5]
P0	0.1727	0.1080	0.2062	0.1625
	[0.0259]	[0.0229]	[0.0096]	[0.0091]
P1	0.0463	0.0222	0.0522	0.0405
	[0.0100]	[0.0084]	[0.0033]	[0.0030]
P2	0.0181	0.0097	0.0195	0.0147
	[0.0055]	[0.0053]	[0.0016]	[0.0015]
Number of observations	295	295	3,921	3,921

Standard errors in brackets (corrected for sampling weight and cluster correlation).
Source: Author's estimation from VHLSS 2004 and 2006.

Households with work migrants had a higher average SI than other households, even before migration. This could be because a large share of these households came from rural areas. The SI was slightly reduced over time for both groups of households. Moreover, the SI for income without remittances decreased remarkably from 0.512 to 0.467 for the households with migration.

Per capita expenditure increased by around 28 percent and 12 percent for households with and without migration during the period 2004-2006, respectively. Poverty, which is measured by three Foster-Greer-Thorbecke poverty indexes, reduced for groups of households (the formulas of these poverty indexes are presented in section 4.2). Migration households experienced a larger decrease in poverty than other households.

Similarly, Table 7.2 presents the welfare indicators of households with and without non-work migration. In general, households with non-work migration experienced a higher increase in per capita remittances and income than those without non-work migration. Per capita expenditure also increased at a higher rate for the migrant-sending households: 26 percent compared to 11 percent for the period 2004-2006. As a result, poverty of the non-work migration households decreased more quickly than poverty of the households without non-work migration.

Table 7.2. Household welfares of household with and without non-work migrants during 2004-2006.

Welfare indicators	Households with non-work migrants		Households without non-work migrants	
	2004	2006	2004	2006
Ratio of members engaged in productive activities to the total household members older than 14 years old (%)	77.6	79.0	82.0	78.7
	[1.2]	[0.9]	[0.5]	[0.4]
Annual working hours per capita	1,066.1	1,152.3	1,021.1	1,047.1
	[29.3]	[22.4]	[10.4]	[10.8]
Annual working hours per working member	1,726.3	1,873.1	1,829.2	1,855.6
	[34.7]	[30.3]	[16.1]	[14.7]
Per capita internal remittances	396.1	802.8	428.9	405.4
	[40.4]	[76.0]	[19.9]	[18.9]
Per capita international remittances	217.2	219.2	249.6	358.2
	[49.1]	[53.8]	[50.5]	[74.8]
Per capita remittances (both international and internal)	613.3	1,021.9	678.5	763.6
	[67.2]	[89.4]	[55.2]	[77.4]
Per capita income (household income divided by household size)	5,520.7	7,093.7	5,754.2	6,884.3
	[208.8]	[266.7]	[132.5]	[152.2]
Household income divided by the number of working members (above 14 years old)	10,000.0	12,500.0	11,138.9	13,292.1
	[533.1]	[617.9]	[315.2]	[403.0]
Household income excluding remittances divided by the number of working members (above 14 years old)	8,924.2	10,700.0	9,703.9	11,497.4
	[468.3]	[568.9]	[232.1]	[268.0]
Ratio of non-farm income to total income (%)	65.8	66.8	63.2	66.1
	[1.3]	[1.4]	[0.8]	[0.7]
Ratio of non-farm income (excluding remittances) to total income (%)	62.3	61.7	59.1	62.2
	[1.4]	[1.5]	[0.8]	[0.8]
Simpson index 1 (all income sources)	0.5020	0.4975	0.4889	0.4749
	[0.0080]	[0.0079]	[0.0043]	[0.0043]
Simpson index 2 (income sources excluding remittances)	0.4450	0.4206	0.4268	0.4147
	[0.0090]	[0.0091]	[0.0049]	[0.0049]
Per capita consumption expenditure	4,260.8	5,387.5	4,284.2	4,746.2
	[163.4]	[170.1]	[86.3]	[88.6]
P0	0.2073	0.1293	0.2037	0.1640
	[0.0202]	[0.0173]	[0.0097]	[0.0093]
P1	0.0550	0.0293	0.0514	0.0409
	[0.0071]	[0.0045]	[0.0033]	[0.0031]
P2	0.0220	0.0100	0.0190	0.0151
	[0.0036]	[0.0019]	[0.0016]	[0.0016]
Number of observations	608	608	3,608	3,608

Standard errors in brackets (corrected for sampling weight and cluster correlation).

Source: Author's estimation from VHLSS 2004 and 2006.

7.4 Methodology of impact evaluation

It is not possible to ascribe the differences between migrant-sending households and other households to the migration, as the two groups are likely to differ in other respects. To address the causal effects of migration on household welfare, poverty and inequality, we used the methodology of difference-in-differences with propensity score matching. In the following section, we discuss this method and the indicators used.

7.4.1 Impact of migration on household welfare indicators

Let *D* be a binary variable indicating whether a household has migrants: *D*=1 if a household has migrants, *D*=0 otherwise. In addition, denote *Y* as the variable of interest, with $Y_i = Y_{i1}$ if household *i* has migrants and $Y_i = Y_{i0}$ if the same household *i* had not had migrants. The impact of migration on household *i* is then measured by:

$$\Delta_i = Y_{i1} - Y_{i0} \tag{1}$$

Following the evaluation literature, the *average* impact of migration on households with migrants is defined by

$$ATT = E(Y_1 - Y_0 | D = 1) = E(Y_1 | D = 1) - E(Y_0 | D = 1) \tag{2}$$

where ATT denotes the Average Treatment Effect on the Treated, or in this case the average effect of migration on households with migrants (Heckman *et al*., 1999).

Estimation of ATT is not straightforward, since $E(Y_0 \mid D = 1)$ is unobservable. $E(Y_0 \mid D = 1)$ is the counterfactual which would give the expected outcome of households with migration if nobody had migrated. A possible solution would be to estimate $E(Y_0 \mid D = 1)$ by the expected value of the variable of interest for those households that do not have migrants, $E(Y_0 \mid D = 0)$, which is observable. Obviously, this approach is only valid if households with and without migrants are similar in all respects but migration, or in other words, if the decision to migrate is not correlated with other variables that are not controlled for in analysis. In practice, this requires selection of a valid control group (the counterfactual).

We use a matching methodology to derive such a control group, which comes down to pairing households with and without migrants on the basis of some observable variables such that both groups become comparable. Such matching is preferable to randomly choosing a control group, since it suffers less from selection bias. The main advantage of matching compared to regression-based methods is that it is a non-parametric method, which avoids specifying the relationship between characteristics and outcome. A second advantage is that matching methods emphasize the common support problem, which implies that they only compare household performance between households with and without migrants when the two groups of households have otherwise similar characteristics.

We use the method of propensity score matching (Rosenbaum and Rubin, 1983). We start by estimating the probability of being a household with migrants at time *t* by using a logit or probit model, $P(D_{it} = 1) = F(X_{it-1})$, where *X* is a vector of observed variables before migration. Let's divide the sample into two groups: a group of households with migrants (say group M) and a group of households without migrants (say group C). The matching methodology pairs each family with migrants to some group of 'comparable' households without migrants and then associates to the outcomes of the treated families the (weighted) outcomes of their neighbours in the comparison group. The matching estimator is defined by

$$\mu = \sum_{i \in M} \left[y_i - \sum_{j \in C} g(p_i, p_j) y_j \right],$$

where p is the probability of having migrants and *g(.)* gives the weights on control family j in forming a comparison with migrant family i. The function *g(.)* differs for the different matching estimators proposed in the literature.

Since we have longitudinal data on the migration and non-migration households, we can estimate the impact of migration by using the method of difference-in-differences with matching. The main advantage of the difference-in-differences method compared to the standard matching estimator in levels is that the former eliminates differences in the variable of interest due to unobserved time-invariant effects. This implies that the difference-in-differences method controls for selection on both observables and time-invariant unobservables, while the standard matching method controls for selection on observables only. Let Δy be the differences between the variable of interest before and after migration. Then the difference-in-differences estimator is given by:

$$\delta = \sum_{i \in M} \left[\Delta y_i - \sum_{j \in C} g(p_i, p_j) \Delta y_j \right]. \tag{3}$$

Different matching estimators can be used. In this chapter, we use nearest-neighbours, kernel matching, and local linear regression matching to examine the sensitivity of the impact estimates. We calculate standard errors using bootstrap techniques. This is common practice in empirical studies, although Abadie and Imbens (2006) show that bootstrap can give invalid standard errors for the nearest neighbour matching estimator, and there is no evidence on the validity of bootstrap standard errors for other matching estimators. We implement the bootstrap by repeatedly drawing samples from the original sample of the VHLSS panel data. Since the VHLSS sample selection follows stratified random cluster sampling, communes instead of households are bootstrapped in each stratum (Deaton, 1997). In other words, the bootstrap is made of communes (i.e. clusters) within strata. The number of replications is 500. We also tried to bootstrap households instead of communes, and the results of both possibilities are very similar.

It should be noted that we are able to construct so-called baseline data before migration since the migration is defined for the period 2004-2006. It is possible that before 2004 both migration and non-migration households already had migrating members. However, through controlling for a large number of household and village variables including receipt of internal and international remittances in 2004, we expect to be able to construct a control group with observed characteristics similar to the migrant group. The main difference between the migrant and control group is that the migrant group had migrating members between 2004 and 2006, while the control group did not. We therefore interpret the estimated differences as the impact of migration during the period 2004-2006.

7.4.2 Impact of migration on poverty and inequality

In this chapter, expenditure poverty is measured by the three Foster-Greer-Thorbecke poverty indexes, and expenditure inequality is measured by the Gini coefficient, Theil's L index, and Theil's T index. The formulas of these indexes are presented in section 4.3 of Chapter 4.

The impact of migration on the poverty indexes of migrant households is calculated as follows:

$$\Delta P_{\alpha} = P_{\alpha}(Y_1^A \mid D=1) - P_{\alpha}(Y_0^A \mid D=1), \tag{4}$$

where the first term on the right-hand side is the poverty measure of households with migration given this migration. This term is observed and can be computed directly from the sample data. The second term on the right-hand side is the counterfactual measure of poverty, i.e. poverty indexes of the migration households had they not had migration. This term is not observed directly. To estimate this term, we estimate Y_0^A for each household as follows:

$$\hat{Y}_{0i}^A = Y_{1i}^A - \hat{\Delta}_i, \tag{5}$$

where Y_{1i}^A is observed per capita expenditure of migration household *i*. $\hat{\Delta}_i$ is the impact estimate of migration on household *i* estimated using the difference-in-differences with matching method described in the previous section. It should be noted that we measure the impact of migration in 2006, i.e. after households sent migrants (and the superscript 'A' means 'after' migration).

We also measure the impact of migration on total poverty:

$$\Delta P_{\alpha} = P_{\alpha}(Y^A) - P_{\alpha}(Y_0^A), \tag{6}$$

where $P_{\alpha}(Y^A)$ is the observed poverty index of the entire population and $P_{\alpha}(Y_0^A)$ is the poverty index of the entire population if there had been no migration at all.

For inequality, we only measure the impact of migration on the entire population:

$$\Delta I = I(Y^A) - I(Y_0^A), \tag{7}$$

where $I(Y^A)$ is the index for observed inequality and $I(Y_0^A)$ the inequality index in the absence of migration, which is estimated using the predicted counterfactual expenditure. The standard errors of the estimates of impacts on poverty and inequality are estimated using the bootstrap technique described in the previous section.

7.5 Determinants of migration and propensity score estimation

The first step in measuring impact is to predict the propensity score, which is the probability that a household had at least one migrating member during 2004-2006. Since the dependent variable is binary, we used logit regression. The main problem we faced was how to select the set of explanatory variables. Two requirements need to be taken into account. First, the explanatory variables should be exogenous to migration (Heckman *et al.*, 1999; Ravallion, 2001). Therefore, we use variables before migration during the 2004-2006 period, i.e. variables in the 2004 VHLSS. Second, the explanatory variables should affect both the outcome variable we are interested in and migration (Ravallion, 2001). Variables which affect the outcome variable but not migration should not be included in the logit regressions. Similarly, variables affecting migration but not the outcome variables should be ignored.

Economic theories of migration suggest that people primarily migrate in order to improve income, or to reduce risk (Harris and Todaro, 1970; Stark, 1980; Katz and Stark, 1986; Stark and Taylor, 1991; Stark, 1991). The new economics of labour migration assumes that migration decisions are determined both by individual and household characteristics, including human and physical assets of the households (Stark, 1991; Mora and Taylor, 2006). In line with the recent literature on determinants of migration (e.g. Hoddinott, 1994; Morra and Taylor, 2006; Sienaert, 2007), our set of explanatory variables includes household income, receipt of remittances, education of household head and head's spouse, age, sex and marriage of the head, household composition, household education, housing and land, village characteristics, regions and urbanity. Several control variables such as household income and remittances are also outcome variables. However, these variables were measured in 2004, i.e.

before migration during the 2004-2006 period, and pre-treatment outcome can be used as a control in the regression of the propensity score (Dehejia and Wahba, 1998; Smith and Todd, 2005).

It should be noted that households with and without migration during the 2004-2006 period may have had migrating members before 2004. Moreover, households who had migrating members before 2004 may be less likely to have had members migrating between 2004 and 2006. Therefore, migration before 2004 should be controlled for. Since the VHLSS do not present information on migration before 2004, we use receipts of remittances in 2004 as a proxy for pre-2004 migration.

Table 7.8 in Appendix 7.1 presents the entire set of explanatory variables, and their means and standard errors of the means. Table 7.9 in the Appendix presents the logit regressions regarding the determinants of work and non-work migration. We start (models 1a and 1b) by including all explanatory variables that are expected to affect migration. Next, we re-estimate the model by only including variables that are statistically significant at the 10% level (models 2a and 2b). We use models 2a and 2b to estimate the propensity scores for work and non-work migration, respectively.

To examine the common support, we present Figures 7.2 and 7.3 of the propensity scores. The bars above the horizontal line represent the density distribution of the propensity score of the migration households, while the bars below the horizontal line represent the density distribution of the propensity score of the non-migration households. The figures show that the common support is large. This means that for each migration household we will be able to find non-migration households with similar propensity scores.

According to Model 2a, households with non-work migrants were less likely to have work migrants. As expected, households with lower per capita income tended to send out work migrants. Households with international remittances in 2004 were less likely to send work migrants during the period 2004-2006, since international remittances in 2004 imply that these households had already sent migrants before 2004. On the contrary, households with internal remittances were more likely to have work migrants. It should be noted that internal remittances as defined in the VHLSS include all internal private transfers received by households. These can be given to households not only by relatives but also by friends, neighbours, etc. Thus, the receipt of internal remittances of a household can be an indicator not only for migrants but also for relationship with other households. Households with a larger network could have a higher probability of migration. Ethnic minorities were less likely to send out migrating members compared to Kinh/Chinese households. Large living areas and annual crop lands were associated with a small probability of work migration.

Non-work migration was not significantly correlated with economic factors such as income and land. Households with a large number of members and higher ratio of members between 15 and 60 years olds were more likely have non-work migration. Smaller living areas tended to increase the probability of non-work migration.

It should be noted that the main aim of the predicted propensity score is to overcome the multidimensionality problem of matching by covariates. The quality of a constructed comparison group should be assessed by testing whether the distribution of the covariates is similar between the comparison and treatment groups given the predicted propensity score. We test the equality of means of covariates between migrant and non-migrant households using t-tests. To examine the sensitivity of the impact estimates to different matching schemes, we will use four matching estimators including 1 nearest neighbour, 5 nearest neighbours, kernel matching with bandwidth of 0.05, and local linear regression matching with bandwidth of 0.05. The results of the balancing test for these estimators are presented in Tables 7.10 to 7.17 in Appendix 7.1. It can be seen that we cannot reject equality of the means of the covariates between migrant and non-migrant households for any of the matching estimators.[42] All estimators achieve similar bias reduction percentages of X. Yet, the matched group from kernel matching is most similar to the migrant group. So we will use the results from the kernel matching scheme in the remainder of this chapter the interpretation. Results from other matching estimators are very similar and presented in Appendix 7.1.

7.6 Impact of migration

Below, we will analyze the impacts of migration on welfare at the household and country level. At household level, we assess the ultimate effects on income and expenditures and the underlying factors work efforts of non-migrating members, remittances, and income diversification. At the national level, we consider poverty of both migrant households and the overall population and total inequality.

7.6.1 Impact of migration on household income and expenditure

Work migration had a significant impact on per capita income, household income per working household member, as well as per capita consumption expenditures (Table 7.3). Migration for work purposes resulted in an average increase in per capita income by 897 thousand VND between 2004 and 2006: an increase of 19 percent. Income per working member even increased by one third. This was solely due to an increase in remittances: non-remittances income did not significantly change due to migration. Per capita consumption increased by 8 percent only, suggesting a high propensity to save out of remittances.

Migration for non-work purposes only significantly affected average per capita consumption expenditures and not income (Table 7.3). The latter is not surprising given our previous finding that work migration affects per capita income through remittances alone. Between 2004 and 2006, per capita consumption increased by 525 thousand VND, or 12 percent, due to migration. Perhaps surprisingly, this indicates that the consumption effect of non-work migrants was higher than the consumption effect on work migrants, even though it did not lead to a significant increase in per capita income. There might be two possible explanations for this. The first explanation is that non-work migration did help migrant-sending households increase their income, but we are not able to detect statistically significant impacts using this data set and this impact measurement method. The second explanation is that households with non-work migrants had on average higher per capita expenditures than households with work migrants. Richer households may experience more household economies of scale and thus decrease total consumption less as a result of a reduction in household size (Deaton and Paxson, 1998).

[42] We relied on the STATA command called 'psmatch2' to perform the matching estimators. However, we do not use the original command for the estimation, since the command does not allow sampling weights. We revised the command to allow for sampling weights. We also tried to estimate the migration impacts without sampling weights. The results are very similar to those using the sampling weights.

Table 7.3. Impact of migration on income and expenditure in 2006 (kernel matching with bandwidth of 0.05).

Welfare indicators (outcome)	2004 Treatment	2004 Matched control	2004 Difference	2006 Treatment	2006 Matched control	2006 Difference	Diff-in-diff
Impact of work migration							
Per capita income (Household income divided by household size)	4,733.4***	4,941.9***	-208.5**	7,088.5***	6,399.7***	688.9*	897.4**
	[194.5]	[185.0]	[86.6]	[455.7]	[169.5]	[445.4]	[424.6]
Household income divided by the number of working members (above 14 years old)	8,111.2***	8,208.0***	-96.9	13,100.0***	10,500.0***	2,614.2*	2,711.0**
	[364.8]	[302.2]	[249.1]	[1,414.3]	[284.5]	[1,406.9]	[1,398.2]
Household income excluding remittances divided by the number of working members (above 14 years old)	7,028.9***	7,466.4***	-437.5*	8,917.7***	9,527.0***	-609.3*	-171.8
	[283.2]	[264.7]	[236.3]	[404.3]	[263.3]	[379.3]	[347.2]
Per capita consumption expenditure	3,608.9***	3,786.5***	-177.5*	4,612.5***	4,487.5***	125.0	302.6***
	[129.8]	[108.6]	[96.8]	[148.4]	[106.6]	[119.7]	[117.0]
Impact of non-work migration							
Per capita income (Household income divided by household size)	5,520.7***	5,433.8***	87.0	7,093.7***	6,748.0***	345.7***	258.8
	[208.8]	[158.3]	[234.0]	[266.7]	[189.5]	[311.7]	[240.1]
Household income divided by the number of working members (above 14 years old)	10,000***	10,100***	141.8	12,500.0***	12,000.0***	492.6	350.8
	[533.1]	[344.2]	[597.9]	[617.9]	[332.1]	[679.2]	[554.0]
Household income excluding remittances divided by the number of working members (above 14 years old)	8,924.2***	8,857.7***	66.5	10,700.0***	10,700.0***	-79.1	-145.7
	[468.3]	[249.0]	[503.1]	[568.9]	[287.3]	[621.7]	[474.9]
Per capita consumption expenditure	4,260.8***	4,153.2***	107.6	5,387.5***	47,54.5***	633.0***	525.4***
	[163.4]	[121.0]	[188.1]	[170.1]	[112.9]	[193.0]	[144.0]

* significant at 10%; ** significant at 5%; *** significant at 1%.
Standard errors in brackets. Standard errors are corrected for sampling weights and estimated using bootstrap (non-parametric) with 500 replications.
Source: Author's estimation from VHLSS 2004 and 2006.

7.6.2 Impact of migration on work efforts

Table 7.4 presents the impact of migration on work efforts of non-migrating members in migrant-sending households. The table suggests that work migration did not have a statistically significant impact on the labour efforts of the non-migrating members. Non-work migration, on the other hand, resulted in an increase in annual working hours per working member (older than 14) by around 7 percent. This suggests that there was a need to compensate for a loss in income due to non-work migration by means of an increase in working hours of the remaining household members. The results of the previous section suggest that this increase was sufficient to compensate for the per capita income loss.

Table 7.4. Impact of work and non-work migration on household work efforts in 2006 (kernel matching with bandwidth of 0.05).

Indicators (outcome)	2004			2006			Diff-in-diff
	Treatment	Matched control	Difference	Treatment	Matched control	Difference	
Impact of work migration							
Ratio of members engaged in productive activities to the	81.7***	79.5***	2.2*	79.9***	78.1***	1.8	-0.5
total household members older than 14 years old (%)	[1.4]	[0.7]	[1.5]	[1.5]	[0.7]	[1.5]	[1.6]
Annual working hours per	1,023.7***	1,091.2***	-67.5**	1,073.7***	1,144.2***	-70.5**	-2.9
capita	[29.6]	[18.3]	[28.1]	[29.2]	[17.7]	[29.0]	[31.2]
Annual working hours per	1,592.1***	1,693.4***	-1,01.3***	1,693.1***	1,765.6***	-72.5*	28.8
working member	[39.4]	[21.4]	[39.7]	[39.2]	[19.2]	[40.9]	[48.9]
Impact of non-work migration							
Ratio of members engaged in productive activities to the	77.6***	75.6***	2.0	79.0***	75.1***	3.8***	1.8
total household members older than 14 years old (%)	[1.2]	[0.7]	[1.3]	[0.9]	[0.6]	[1.1]	[1.4]
Annual working hours per	1,066.1***	1,087.5***	-21.5	1,152.3***	1,113.5***	38.8	60.3**
capita	[29.3]	[17.3]	[31.0]	[22.4]	[19.5]	[26.7]	[29.5]
Annual working hours per	1,726.3***	1,812.5***	-86.2**	1,873.1***	1,835.0***	38.1	124.3***
working member	[34.7]	[21.0]	[40.3]	[30.3]	[21.9]	[35.8]	[40.8]

* significant at 10%; ** significant at 5%; *** significant at 1%.
Standard errors in brackets. Standard errors are corrected for sampling weights and estimated using bootstrap (non-parametric) with 500 replications.
Source: Author's estimation from VHLSS 2004 and 2006.

Table 7.5. Impact of migration on remittances in 2006 (kernel matching with bandwidth of 0.05).

Welfare indicators (outcome)	2004			2006			Diff-in-diff
	Treatment	Matched control	Difference	Treatment	Matched control	Difference	
Impact of work migration							
Per capita internal remittances	472.0***	377.4***	94.6	851.0***	417.4***	433.6***	339.0***
	[76.9]	[26.3]	[72.0]	[69.2]	[31.4]	[74.6]	[95.1]
Per capita International	123.3***	63.6***	59.8	911.9***	166.3***	745.6*	685.8*
remittances	[71.5]	[18.0]	[62.9]	[388.3]	[33.4]	[389.0]	[379.6]
Per capita remittances (both	595.3***	441.0***	154.3	1,762.9***	583.7***	1,179.2**	1,024.9***
international and internal)	[103.8]	[34.6]	[90.5]	[398.7]	[45.3]	[400.1]	[401.6]
Impact of non-work migration							
Per capita internal remittances	396.1***	368.2***	27.9	802.8***	391.9***	410.9***	383.0***
	[40.4]	[24.3]	[45.3]	[76.0]	[27.6]	[81.6]	[85.2]
Per capita international	217.2***	223.4***	-6.2	219.2***	255.9***	-36.7	-30.5
remittances	[49.1]	[71.2]	[84.4]	[53.8]	[49.7]	[73.1]	[96.2]
Per capita remittances (both	613.3***	591.6***	21.7	1,021.9***	647.7***	374.2***	352.5***
international and internal)	[67.2]	[77.0]	[98.6]	[89.4]	[55.3]	[107.8]	[124.5]

* significant at 10%; ** significant at 5%; *** significant at 1%.
Standard errors in brackets. Standard errors are corrected for sampling weights and estimated using bootstrap (non-parametric) with 500 replications.
Source: Author's estimation from VHLSS 2004 and 2006.

7.6.3 Impact of migration on remittances

Table 7.5 shows that both work and non-work migration lead to an increase in remittances. However, remittances from non-work migrants were relatively low. This could explain why in case of non-work migration there was a higher need to compensate for possible income losses by increasing working hours (see above). The table also shows that non-work migration increased internal remittances but not international remittances. This is in accordance with the observation that most non-work migrants left for marriage or separate stay, which mainly involve internal migration.

7.6.4 Impact of migration of income diversification

Migration did not increase the overall Simpson index of income diversification. However, work migration resulted in a decrease of 3 percentage points in the Simpson diversification index of non-remittances income (Table 7.6). This suggests that some work migrants were already involved in different activities than their household members, so that when they left local income became less diversified. This could increase their vulnerability to shocks if the migrant stops sending remittances. We do not find this effect for non-work migrants.

Table 7.6. Impacts of migration on income diversification in 2006 (kernel matching with bandwidth of 0.05).

Welfare indicators (outcome)	2004			2006			Diff-in-diff
	Treatment	Matched control	Difference	Treatment	Matched control	Difference	
Impact of work migration							
Simpson index 1 (all income	0.5630***	0.5280***	0.0354***	0.5561***	0.5080***	0.0481***	0.0128
sources)	[0.0090]	[0.0064]	[0.0098]	[0.0100]	[0.0061]	[0.0108]	[0.0113]
Simpson index 2 (income	0.5120***	0.4804***	0.0313***	0.4670***	0.4640***	0.0029**	-0.0284***
sources excluding remittances)	[0.0110]	[0.0066]	[0.0106]	[0.0122]	[0.0066]	[0.0124]	[0.0122]
Impact of non-work migration							
Simpson index 1 (all income	0.5020***	0.5051***	-0.0031	0.4975***	0.4922***	0.0053	0.0084
sources)	[0.0080]	[0.0054]	[0.0095]	[0.0079]	[0.0050]	[0.0092]	[0.0093]
Simpson index 2 (income	0.4450***	0.4522***	-0.0073	0.4206***	0.4422***	-0.0216**	-0.0143
sources excluding	[0.0090]	[0.0060]	[0.0104]	[0.0091]	[0.0053]	[0.0101]	[0.0100]
remittances)	[163.4]	[121.0]	[188.1]	[170.1]	[112.9]	[193.0]	[144.0]

* significant at 10%; ** significant at 5%; *** significant at 1%
Standard errors in brackets. Standard errors are corrected for sampling weights and estimated using bootstrap (non-parametric) with 500 replications.
Source: Author's estimation from VHLSS 2004 and 2006.

7.6.5 Impact of migration on poverty and inequality

Table 7.7 presents the impact of work and non-work migration on expenditure poverty and inequality. Although work migration did not have statistically significant effects on the incidence of poverty, it did significantly change the poverty gap and severity for the work migration households. Hence, work migration did not lift people out of poverty but it did make their poverty less harsh.

Table 7.7. Impacts of work and non-work migration on poverty and inequality in 2006 (kernel matching with bandwidth of 0.05).

	Households with 'work' migration			Households with 'non-work' migration		
	With migration	Without migration	Impact	With migration	Without migration	Impact
Poverty of households with migration						
P0	0.1074***	0.1346***	-0.0272	0.1277***	0.2166***	-0.0889***
	[0.0219]	[0.0310]	[0.0249]	[0.0159]	[0.0319]	[0.0308]
P1	0.0222**	0.0384***	-0.0162**	0.0293***	0.0701***	-0.0407**
	[0.0084]	[0.0120]	[0.0074]	[0.0045]	[0.0151]	[0.0141]
P2	0.0097**	0.0179**	-0.0082**	0.0100***	0.0327***	-0.0228**
	[0.0053]	[0.0081]	[0.0044]	[0.0019]	[0.0096]	[0.0091]
All poverty						
P0	0.1587***	0.1603***	-0.0017	0.1587***	0.1709***	-0.0122***
	[0.0073]	[0.0074]	[0.0015]	[0.0073]	[0.0087]	[0.0043]
P1	0.0392***	0.0402***	-0.0010**	0.0392***	0.0448***	-0.0056***
	[0.0025]	[0.0026]	[0.0004]	[0.0025]	[0.0033]	[0.0019]
P2	0.0144***	0.0149***	-0.0005	0.0144***	0.0175***	-0.0031***
	[0.0012]	[0.0013]	[0.0003]	[0.0012]	[0.0019]	[0.0013]
All inequality						
Gini	0.3464***	0.3477***	-0.0013**	0.3464***	0.3510***	-0.0046***
	[0.0050]	[0.0051]	[0.0005]	[0.0050]	[0.0053]	[0.0015]
Theil L	0.1984***	0.2002***	-0.0019**	0.1984***	0.2055***	-0.0071***
	[0.0058]	[0.0060]	[0.0009]	[0.0058]	[0.0065]	[0.0026]
Theil T	0.2080***	0.2096***	-0.0016***	0.2080***	0.2135***	-0.0056***
	[0.0073]	[0.0074]	[0.0006]	[0.0073]	[0.0077]	[0.0018]

* significant at 10%; ** significant at 5%; *** significant at 1%
Standard errors in brackets. Standard errors are corrected for sampling weights and estimated using bootstrap (non-parametric) with 500 replications.
Source: Author's estimation from VHLSS 2004 and 2006.

The effects of non-work migration on expenditure poverty were greater, which could be expected given that non-work migration was more widespread and on average resulted in a larger increase in per capita expenditures (although not per capita income). Non-work migration helped reduce the poverty incidence of migration households by around 9 percentage points and in addition reduced their poverty gap and severity indexes by 57 percent and 67 percent, respectively. These numbers translate into a reduction of the overall poverty incidence by 1.2 percentage points and a reduction of the overall poverty gap and severity indexes by 12 percent and 17 percent.

Nguyen *et al.* (2008) found that total migration between 2002 and 2004 resulted in an increase in inequality in 2004: the actual Gini coefficient was 0.42, compared to the counterfactual (no migration) of 0.38. On the contrary, we find that both work and non-work migration slightly decreased expenditure inequality in 2006. The effects we find are, however, extremely small compared to the 0.04 points observed by Nguyen *et al.* The difference could lie in the slightly different time period covered in the two studies or in methodological differences. Nguyen *et al.* use least-squares regression to determine the impact of migration on expenditures to compute inequality, whereas we use difference-in-differences with propensity score matching, which has the advantages that it controls for time-invariant unobservable differences between households with and without migrants in addition to observable differences, and that it does not impose a functional form on the relationship between migration, the covariates, and expenditures.

7.7 Conclusions

While migration is an increasingly important phenomenon in Vietnam, there are surprisingly few studies analyzing its effects quantitatively. In this study, we therefore estimate the impact of long-term migration on welfare at household and country level in Vietnam using the VHLSS of 2004 and 2006. Compared to Nguyen *et al.*(2008), the only other existing study on the impact of long-term migration in Vietnam, we provide not only information on expenditures and inequality, but also on remittances, work effort, income and poverty. Also, we look at income diversification as an indicator of vulnerability.

Work migration resulted in an increase of income per capita by 19 percent and of income per working member by one third. This was purely due to an increase in remittances: migration did not significantly affect non-remittances income per working member, nor did it change the work efforts of the remaining household members. Per capita consumption increased by 8 percent, less than half of the percentage increase in per capita income, suggesting a high propensity to save out of remittances.

Migration for non-work purposes did not significantly affect per capita and per worker income. Remittances were low and per capita working hours increased. This suggests that there was a need to compensate for a loss in per capita income due to the departure of a productive household member. Surprisingly, we found this effect for non-work migrants only. Despite the absence of an effect of non-work migration on per capita income, per capita consumption increased by 12 percent, i.e. more than for work migration, which did significantly increase income. Possibly, households with non-work migrants, which had on average higher per capita expenditures than households with work migrants, experienced more household economies of scale and thus decreased total consumption less as a result of a reduction in household size. It can also be possible that non-work migration did help migrant-sending households increase their income, but we are not able to detect statistically significant impacts using this data set and this impact measurement method.

Migration potentially affects the vulnerability of households to shocks. While neither work nor non-work migration affected overall income diversification as measured by the Simpson index, we could postulate that the covariance between local income sources is higher than the covariance between local income and remittances. Remittances could even be negatively correlated with earned income if migrants remit more when needs are higher. If so, especially work migration, which resulted in most remittances, would have decreased vulnerability.

On the other hand, work migration resulted in a decrease in diversification of non-remittances income. Apparently, before migration some migrants engaged in different activities than their household members, so that when they left local income became less diversified. This could increase the vulnerability of these households to shocks if the migrant stopped sending remittances in the future. This prospect might affect the limited increase in per capita consumption compared to per capita income. We did not find these effects for non-work migration, which was associated with low levels of remittances and did not affect non-remittance income diversification levels.

The household-level effects translated into changes in poverty and inequality at the country level. Due to its even distribution over poor and non-poor households and its relatively large effect on expenditure, non-work migration significantly decreased the incidence, depth and severity of national poverty. The effects of work migration on poverty were much smaller, mainly due to the lower expenditure effects. Still, while work migration did not lift people out of poverty, it did make their poverty less severe.

In addition, we found that migration decreased inequality, although only very slightly. This conflicts with the results of Nguyen *et al.* (2008) who found that total migration resulted in a substantial increase in inequality. Both the two-year difference between the data used for the two studies and methodological differences could explain these results. While we used difference-in-differences with propensity score matching, Nguyen *et al.* applied least-squares regression to determine the impact of migration on expenditures. Our method has the advantages that it controls for time-invariant unobservable differences between households with and without migrants in addition to observable differences, and that it does not impose a functional form on the relationship between migration, the covariates, and expenditures. Yet more research is needed to test the robustness of these contradictory results.

Overall, our analysis suggests that work migration and non-work migration are an important tool to increase household consumption expenditure and to reduce poverty and inequality in Vietnam. Also for other developing countries, especially for some Asian developing countries, such as the Philippines, Indonesia, Lao, and Cambodia, with a similar economic structure as Vietnam, migration may play an important role in terms of poverty and inequality reduction. There are several measures and policies to increase migration. Improvement of transportation and road can promote not only the local market but also the probability of migration from rural to urban areas. Vocational training programs can provide rural people with production and business skills, and rural people are more likely to find employment in urban areas. The government can support migrants with social security programs and protective policies.

Appendix 7.1 Descriptive statistics and regression results

Table 7.8. Descriptive information on the 2004 variables for households with and without migration.

Variable	Description	Type	Household with work migrants	Household without work migrants	Household with non-work migrants	Household without non-work migrants
migration1	Households with work migration (yes = 1)	Binary	1	0	0.0773	0.0687
			[0]	[0]	[0.0108]	[0.0042
migration2	Households with non-work migration (yes = 1)	Binary	0.1593	0.1431	1	0
			[0.0213]	[0.0056]	[0]	[0]
incomepc04	Per capita income (million VND)	Continuous	4.9542	5.8202	5.4337	5.8145
			[0.1898]	[0.0880]	[0.1624]	[0.0930]
dforemit04	Receipt of international remittances	Binary	0.0271	0.0607	0.0691	0.0565
			[0.0095]	[0.0038]	[0.0103]	[0.0038]
ddoremit04	Receipt of internal remittances	Binary	0.8983	0.8452	0.8257	0.8528
			[0.0176]	[0.0058]	[0.0154]	[0.0059]
dagri04	Household involved in agricultural activities (yes = 1)	Binary	0.8780	0.7819	0.7895	0.7885
			[0.0191]	[0.0066]	[0.0165]	[0.0068]
ragri_oc04	Ratio of members involved in agricultural production to total household members	Continuous	0.3839	0.3357	0.3516	0.3370
			[0.0157]	[0.0048]	[0.0119]	[0.0050]
runsk_oc04	Ratio of unskilled workers to total household members	Continuous	0.1313	0.1194	0.1220	0.1199
			[0.0119]	[0.0032]	[0.0081]	[0.0034]
ethnic04	Ethnic minorities (yes = 1)	Binary	0.0983	0.1597	0.1760	0.1519
			[0.0174]	[0.0059]	[0.0155]	[0.0060]
hhsize04	Household size	Discrete	4.9797	4.3532	5.5197	4.2079
			[0.0917]	[0.0274]	[0.0821]	[0.0263]
pchild04	Ratio of members younger than 16 to total household members	Continuous	0.1577	0.2518	0.1803	0.2561
			[0.0103]	[0.0035]	[0.0070]	[0.0037]
pelderly04	Ratio of members older than 60 to total household members	Continuous	0.0709	0.1247	0.0990	0.1247
			[0.0082]	[0.0040]	[0.0068]	[0.0043]
pfemale04	Ratio of female members to total household members	Continuous	0.4582	0.5158	0.5291	0.5088
			[0.0106]	[0.0031]	[0.0070]	[0.0033]
agehead04	Age of household head	Discrete	51.7	48.8	53.0	48.4
			[0.6157]	[0.2252]	[0.4936]	[0.2342]
sexhead04	Gender of household head (male = 1, female = 0)	Binary	0.7898	0.7554	0.7368	0.7614
			[0.0238]	[0.0069]	[0.0179]	[0.0071]
married04	Head lives with spouse (yes = 1)	Binary	0.8305	0.8113	0.7829	0.8176
			[0.0219]	[0.0062]	[0.0167]	[0.0064]
hhedu04	Head completed technical degree or post-secondary degrees	Binary	0.1051	0.1421	0.1250	0.1419
			[0.0179]	[0.0056]	[0.0134]	[0.0058]
hsedu04	Head’s spouse completed technical degrees or post-secondary degrees	Binary	0.1017	0.0808	0.0691	0.0845
			[0.0176]	[0.0044]	[0.0103]	[0.0046]
rtechnical04	Ratio of members with technical degrees to total household members	Continuous	0.0630	0.0645	0.0625	0.0648
			[0.0083]	[0.0025]	[0.0055]	[0.0027]
rposecond04	Ratio of members with post-secondary degrees to total household members	Continuous	0.0244	0.0283	0.0321	0.0274
			[0.0053]	[0.0018]	[0.0045]	[0.0018]
livingarea04	House area per capita (m^2)	Continuous	13.18	15.80	13.05	16.05
			[0.39]	[0.19]	[0.33]	[0.20]
housetype1	House made of permanent materials	Binary	0.1593	0.1900	0.1743	0.1901
			[0.0213]	[0.0063]	[0.0154]	[0.0065]
housetype2	House made of semi-permanent materials	Binary	0.6169	0.5800	0.6217	0.5759
			[0.0284]	[0.0079]	[0.0197]	[0.0082]
housetype3	House of temporary materials	Binary	0.2237	0.2300	0.2039	0.2339

Variable	Description	Type	Household with work migrants	Household without work migrants	Household with non-work migrants	Household without non-work migrants
			[0.0243]	[0.0067]	[0.0164]	[0.0070]
anualand04	Area of annual crop land per capita (thousand m^2)	Continuous	0.5906	0.7631	0.7796	0.7462
			[0.0444]	[0.0243]	[0.0568]	[0.0249]
pereland04	Area of perennial crop land per capita (thousand m^2)	Continuous	0.1899	0.2469	0.2675	0.2388
			[0.0405]	[0.0211]	[0.0462]	[0.0218]
forland04	Forestry land per capita (thousand m^2)	Continuous	0.3691	0.2323	0.3799	0.2186
			[0.1261]	[0.0326]	[0.1246]	[0.0303]
aqualand04	Aquaculture water surface per capita (thousand m^2)	Continuous	0.0309	0.0613	0.0727	0.0568
			[0.0138]	[0.0079]	[0.0241]	[0.0077]
roadv04	Road to village (yes = 1)	Binary	0.8780	0.9095	0.8980	0.9088
			[0.0191]	[0.0046]	[0.0123]	[0.0048]
dmarket104	Distance from village to nearest market (km)	Continuous	2.2102	2.3387	2.1995	2.3517
			[0.2385]	[0.0884]	[0.2010]	[0.0920]
region1	Household in Red River Delta	Binary	0.2203	0.2056	0.1891	0.2095
			[0.0242]	[0.0065]	[0.0159]	[0.0068]
region2	Household in North East	Binary	0.1559	0.1469	0.1414	0.1486
			[0.0212]	[0.0057]	[0.0141]	[0.0059]
region3	Household in North West	Binary	0.0034	0.0487	0.0493	0.0449
			[0.0034]	[0.0034]	[0.0088]	[0.0034]
region4	Household in North Central Coast	Binary	0.2000	0.1109	0.0905	0.1217
			[0.0233]	[0.0050]	[0.0116	[0.0054]
region5	Household in South Central Coast	Binary	0.0983	0.0951	0.0872	0.0967
			[0.0174]	[0.0047]	[0.0114]	[0.0049]
region6	Household in Central Highlands	Binary	0.0305	0.0640	0.0576	0.0624
			[0.0100]	[0.0039]	[0.0095]	[0.0040]
region7	Household in North East South	Binary	0.0508	0.1270	0.1645	0.1145
			[0.0128]	[0.0053]	[0.0150]	[0.0053]
region8	Household in Mekong River Delta	Binary	0.2407	0.2017	0.2204	0.2018
			[0.0249]	[0.0064]	[0.0168]	[0.0067]
Urban04	Household in urban areas (yes = 1)	Binary	0.1492	0.2400	0.2467	0.2314
			[0.0208]	[0.0068]	[0.0175]	[0.0070]
	Number of observations		295	3921	608	3,608

Standard errors in brackets. Standard errors are corrected for sampling weights and cluster correlation.
Source: Author's estimation from VHLSS 2004 and 2006.

Table 7.9. Logit regressions of migration probability.

Explanatory variables	Households with work migration (yes = 1)		Households with non-work migration (yes = 1)	
	Model 1a	Model 2a	Model 1b	Model 2b
Households with work migration (yes = 1)			-0.4371**	-0.4582**
			[0.1980]	[0.1960]
Households with non-work migration (yes = 1)	-0.5537***	-0.5435***		
	[0.2011]	[0.2022]		
Per capita income (million VND)	-0.0288	-0.0406*	-0.0103	
	[0.0252]	[0.0243]	[0.0142]	
Receipt of international remittances	-0.6058*	-0.6020*	0.1462	
	[0.3633]	[0.3626]	[0.2321]	
Receipt of internal remittances	0.4124*	0.4179*	-0.23	
	[0.2177]	[0.2152]	[0.1441]	
Household involved in agricultural activities (yes = 1)	0.6223**	0.5335**	0.0268	
	[0.2534]	[0.2485]	[0.1739]	
Ratio of members involved in agricultural production to total household members	-0.0525		0.022	
	[0.3012]		[0.2706]	
Ratio of unskilled workers to total household members	0.126		-0.0184	
	[0.3676]		[0.2977]	
Ethnic minorities (yes = 1)	-0.7113***	-0.6177***	-0.1073	
	[0.2749]	[0.2336]	[0.2000]	
Household size	0.3200***	0.3053***	0.5408***	0.5376***
	[0.0457]	[0.0431]	[0.0442]	[0.0414]
Ratio of members younger than 16 to total household members	-3.7832***	-3.7498***	-3.8269***	-3.8170***
	[0.4598]	[0.4328]	[0.3466]	[0.3241]
Ratio of members older than 60 to total household members	-2.8967***	-2.9113***	-0.9504***	-0.9939***
	[0.5389]	[0.5168]	[0.3570]	[0.3523]
Ratio of female members to total household members	-1.3120***	-1.2700***	1.1137***	1.1032***
	[0.4052]	[0.3927]	[0.3004]	[0.2907]
Age of household head	0.0245***	0.0257***	0.0118*	0.0121**
	[0.0073]	[0.0066]	[0.0063]	[0.0060]
Gender of household head (male = 1, female = 0)	-0.0664		-0.004	
	[0.1951]		[0.1941]	
Head lives with spouse (yes = 1)	-0.0746		-0.4128*	-0.4218***
	[0.2450]		[0.2262]	[0.1627]
Head completed technical degree or post-secondary degrees	-0.3968	-0.5539**	-0.1683	
	[0.2883]	[0.2360]	[0.2253]	
Head’s spouse completed technical degrees or post-secondary degrees	0.8121**	0.6959***	0.0582	
	[0.3195]	[0.2512]	[0.2595]	
Ratio of members with technical degrees to total household members	-0.5179		0.1513	
	[0.6802]		[0.5436]	
Ratio of members with post-secondary degrees to total household members	-0.4071		0.5876	
	[0.8533]		[0.6973]	
House area per capita (m^2)	-0.0167*	-0.0154*	-0.0151**	-0.0147**
	[0.0096]	[0.0092]	[0.0072]	[0.0067]
House made of permanent materials	Omitted			
House made of semi-permanent materials	0.0307		0.1172	
	[0.1905]		[0.1595]	
House of temporary materials	0.0578		0.068	
	[0.2559]		[0.1999]	
Area of annual crop land per capita (thousand m^2)	-0.2037**	-0.1918**	0.0527	
	[0.0841]	[0.0796]	[0.0338]	
Area of perennial crop land per capita (thousand m^2)	0.0095		0.0299	
	[0.0429]		[0.0322]	
Forestry land per capita (thousand m^2)	0.0583***	0.0600***	0.0328	
	[0.0216]	[0.0209]	[0.0201]	

Explanatory variables	Households with work migration (yes = 1)		Households with non-work migration (yes = 1)	
	Model 1a	Model 2a	Model 1b	Model 2b
Aquaculture water surface per capita (thousand m^2)	-0.4079		0.0675	
	[0.2870]		[0.0992]	
Road to village (yes = 1)	-0.0824		0.1445	
	[0.2416]		[0.1814]	
Distance from village to nearest market (km)	0.0138		-0.0091	
	[0.0193]		[0.0117]	
Household in Red River Delta	Omitted			
Household in North East	0.2446		-0.0669	
	[0.2473]		[0.1892]	
Household in North West	-2.7590*	-2.7510**	-0.2998	
	[1.4406]	[1.3894]	[0.2985]	
Household in North Central Coast	0.6327***	0.5626***	-0.3865**	-0.2929*
	[0.2289]	[0.1820]	[0.1903]	[0.1660]
Household in South Central Coast	0.1375		-0.1572	
	[0.2562]		[0.1914]	
Household in Central Highlands	-0.6128		-0.5495**	-0.4662*
	[0.4879]		[0.2720]	[0.2457]
Household in North East South	-0.8441**	-0.8861***	0.0813	
	[0.3307]	[0.3003]	[0.1929]	
Household in Mekong River Delta	0.1286		-0.1342	
	[0.2384]		[0.1755]	
Household in urban areas	-0.6107***	-0.6583***	-0.0583	
	[0.2254]	[0.2184]	[0.1482]	
Constant	-3.6128***	-3.6164***	-3.9384***	-3.9728***
	[0.6452]	[0.5062]	[0.5325]	[0.4179]
Observations	4,216	4,216	4,216	4,216
R-squared	0.16	0.16	0.16	0.16

* significant at 10%; ** significant at 5%; *** significant at 1%.
Robust standard errors in brackets. Standard errors are corrected for sampling weights and cluster correlation.
Source: Author's estimation from VHLSS 2004 and 2006.

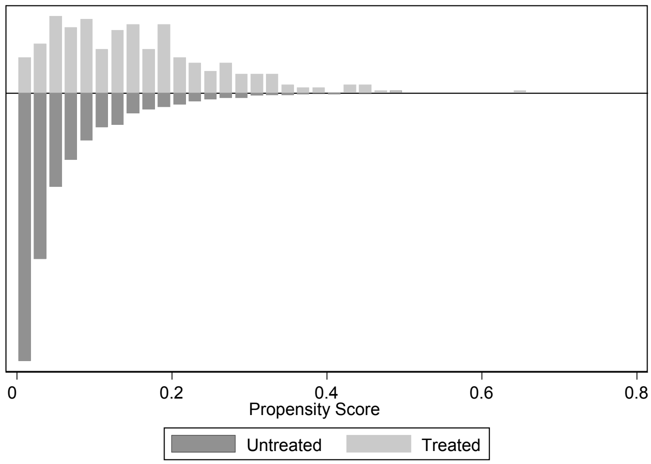

Figure 7.2. Predicted propensity score of households with and without work migration (Model 2a). Source: Author's estimation from VHLSS 2004 and 2006.

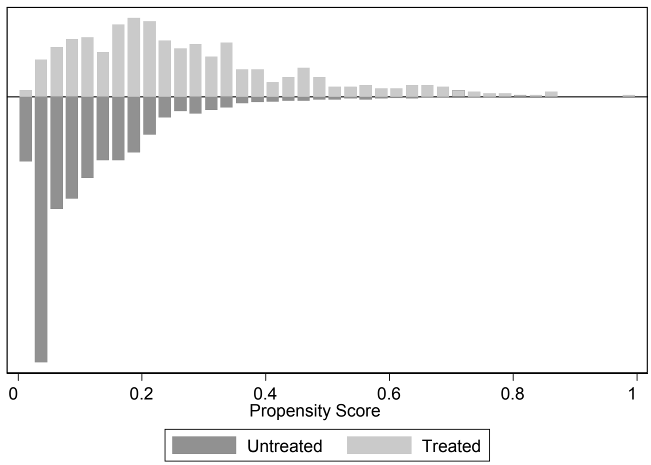

Figure 7.3. Predicted propensity score of households with and without non-work migration (Model 2b). Source: Author's estimation from VHLSS 2004 and 2006.

Table 7.10. Testing balance of the conditioning variables for households with work migrants: 1 nearest neighbour matching (Model 2a).

Variable	Sample	Treated	Control	% bias	% bias reduction	T-statistic	P-value
migration2	Unmatched	0.1593	0.1431	4.5		0.77	0.444
	Matched	0.1593	0.1525	1.9	58.3	0.23	0.821
dforemit04	Unmatched	0.0271	0.0607	-16.4		-2.37	0.018
	Matched	0.0271	0.0271	0.0	100.0	0.00	1.000
ddoremit04	Unmatched	0.8983	0.8452	15.9		2.46	0.014
	Matched	0.8983	0.9288	-9.1	42.6	-1.32	0.188
incomepc04	Unmatched	4,954.2	5,820.2	-19.1		-2.67	0.008
	Matched	4,954.2	4,917.0	0.8	95.7	0.14	0.892
living04	Unmatched	13.2	15.8	-27.0		-3.74	0.000
	Matched	13.2	12.6	5.5	79.7	0.88	0.381
dagri04	Unmatched	0.8780	0.7819	25.8		3.90	0.000
	Matched	0.8780	0.9017	-6.4	75.3	-0.92	0.358
ethnic04	Unmatched	0.0983	0.1597	-18.4		-2.81	0.005
	Matched	0.0983	0.1017	-1.0	94.5	-0.14	0.891
hhsize04	Unmatched	4.9797	4.3532	38.0		6.07	0.000
	Matched	4.9797	5.0271	-2.9	92.4	-0.30	0.761
pchild04	Unmatched	0.1577	0.2518	-47.6		-7.29	0.000
	Matched	0.1577	0.1630	-2.7	94.3	-0.36	0.717
pelderly04	Unmatched	0.0709	0.1247	-26.4		-3.63	0.000
	Matched	0.0709	0.0831	-6.0	77.4	-0.92	0.356
pfemale04	Unmatched	0.4582	0.5158	-30.5		-4.90	0.000
	Matched	0.4582	0.4710	-6.8	77.7	-0.90	0.370
agehead04	Unmatched	51.7	48.8	23.3		3.46	0.001
	Matched	51.7	51.9	-1.6	93.2	-0.21	0.832
hhedu04	Unmatched	0.1051	0.1421	-11.2		-1.77	0.077
	Matched	0.1051	0.0780	8.2	26.6	1.14	0.254
hsedu04	Unmatched	0.1017	0.0809	7.2		1.26	0.209
	Matched	0.1017	0.0780	8.2	-13.8	1.01	0.314
anualand04	Unmatched	0.5906	0.7631	-14.3		-1.93	0.054
	Matched	0.5906	0.5036	7.2	49.6	1.55	0.122
forland04	Unmatched	0.3691	0.2323	6.5		1.10	0.269
	Matched	0.3691	0.2339	6.4	1.2	0.84	0.403
region3	Unmatched	0.0034	0.0487	-28.7		-3.61	0.000
	Matched	0.0034	0.0102	-4.3	85.0	-1.00	0.316
region4	Unmatched	0.2000	0.1109	24.7		4.60	0.000
	Matched	0.2000	0.2068	-1.9	92.4	-0.20	0.838
region7	Unmatched	0.0509	0.1270	-27.0		-3.86	0.000
	Matched	0.0509	0.0441	2.4	91.1	0.39	0.699
urban0404	Unmatched	0.1492	0.2400	-23.1		-3.56	0.000
	Matched	0.1492	0.1220	6.9	70.1	0.96	0.337

Source: Author's estimation from VHLSS 2004 and 2006.

Table 7.11. Testing balance of the conditioning variables for households with work migrants: 5 nearest neighbours matching (Model 2a).

Variable	Sample	Treated	Control	% bias	% bias reduction	T-statistic	P-value
migration2	Unmatched	0.1593	0.1431	4.5		0.77	0.444
	Matched	0.1593	0.1742	-4.2	8.2	-0.48	0.628
dforemit04	Unmatched	0.0271	0.0607	-16.4		-2.37	0.018
	Matched	0.0271	0.0285	-0.7	96.0	-0.10	0.920
ddoremit04	Unmatched	0.8983	0.8452	15.9		2.46	0.014
	Matched	0.8983	0.9085	-3.0	80.9	-0.42	0.677
incomepc04	Unmatched	4,954.2	5,820.2	-19.1		-2.67	0.008
	Matched	4,954.2	4,993.5	-0.9	95.5	-0.14	0.885
living04	Unmatched	13.2	15.8	-27.0		-3.74	0.000
	Matched	13.2	12.8	4.0	85.3	0.66	0.510
dagri04	Unmatched	0.8780	0.7819	25.8		3.90	0.000
	Matched	0.8780	0.8922	-3.8	85.2	-0.54	0.589
ethnic04	Unmatched	0.0983	0.1597	-18.4		-2.81	0.005
	Matched	0.0983	0.1146	-4.9	73.5	-0.64	0.523
hhsize04	Unmatched	4.9797	4.3532	38.0		6.07	0.000
	Matched	4.9797	5.0102	-1.9	95.1	-0.21	0.837
pchild04	Unmatched	0.1577	0.2518	-47.6		-7.29	0.000
	Matched	0.1577	0.1545	1.6	96.7	0.21	0.830
pelderly04	Unmatched	0.0709	0.1247	-26.4		-3.63	0.000
	Matched	0.0709	0.0746	-1.8	93.1	-0.30	0.765
pfemale04	Unmatched	0.4582	0.5158	-30.5		-4.90	0.000
	Matched	0.4582	0.4559	1.2	96.0	0.16	0.873
agehead04	Unmatched	51.7	48.8	23.3		3.46	0.001
	Matched	51.7	51.9	-1.0	95.7	-0.14	0.888
hhedu04	Unmatched	0.1051	0.1421	-11.2		-1.77	0.077
	Matched	0.1051	0.0936	3.5	68.8	0.47	0.641
hsedu04	Unmatched	0.1017	0.0809	7.2		1.26	0.209
	Matched	0.1017	0.0983	1.2	83.7	0.14	0.891
anualand04	Unmatched	0.5906	0.7631	-14.3		-1.93	0.054
	Matched	0.5906	0.5890	0.1	99.1	0.02	0.981
forland04	Unmatched	0.3691	0.2323	6.5		1.10	0.269
	Matched	0.3691	0.4877	-5.6	13.2	-0.45	0.652
region3	Unmatched	0.0034	0.0487	-28.7		-3.61	0.000
	Matched	0.0034	0.0088	-3.4	88.0	-0.84	0.399
region4	Unmatched	0.2000	0.1109	24.7		4.60	0.000
	Matched	0.2000	0.1803	5.5	77.9	0.61	0.544
region7	Unmatched	0.0509	0.1270	-27.0		-3.86	0.000
	Matched	0.0509	0.0549	-1.4	94.7	-0.22	0.826
urban0404	Unmatched	0.1492	0.2400	-23.1		-3.56	0.000
	Matched	0.1492	0.1451	1.0	95.5	0.14	0.889

Source: Author's estimation from VHLSS 2004 and 2006.

Table 7.12. Testing balance of the conditioning variables for households with work migrants: kernel matching with bandwidth of 0.05 (Model 2a).

Variable	Sample	Treated	Control	% bias	% bias reduction	T-statistic	P-value
migration2	Unmatched	0.1593	0.1431	4.5		0.77	0.444
	Matched	0.1593	0.1612	-0.5	88.3	-0.06	0.950
dforemit04	Unmatched	0.0271	0.0607	-16.4		-2.37	0.018
	Matched	0.0271	0.0308	-1.8	89.2	-0.26	0.793
ddoremit04	Unmatched	0.8983	0.8452	15.9		2.46	0.014
	Matched	0.8983	0.8952	0.9	94.2	0.12	0.902
incomepc04	Unmatched	4,954.2	5,820.2	-19.1		-2.67	0.008
	Matched	4,954.2	5,007.5	-1.2	93.8	-0.19	0.851
living04	Unmatched	13.2	15.8	-27.0		-3.74	0.000
	Matched	13.2	13.5	-3.0	89.0	-0.46	0.645
dagri04	Unmatched	0.8780	0.7819	25.8		3.90	0.000
	Matched	0.8780	0.8862	-2.2	91.4	-0.31	0.757
ethnic04	Unmatched	0.0983	0.1597	-18.4		-2.81	0.005
	Matched	0.10	0.12	-5.1	72.3	-0.67	0.505
hhsize04	Unmatched	4.9797	4.3532	38.0		6.07	0.000
	Matched	4.9797	4.9483	1.9	95.0	0.22	0.828
pchild04	Unmatched	0.1577	0.2518	-47.6		-7.29	0.000
	Matched	0.1577	0.1670	-4.7	90.1	-0.62	0.534
pelderly04	Unmatched	0.0709	0.1247	-26.4		-3.63	0.000
	Matched	0.0709	0.0735	-1.3	95.2	-0.21	0.834
pfemale04	Unmatched	0.4582	0.5158	-30.5		-4.90	0.000
	Matched	0.4582	0.4654	-3.9	87.4	-0.50	0.620
agehead04	Unmatched	51.7	48.8	23.3		3.46	0.001
	Matched	51.7	51.3	3.5	85.1	0.47	0.641
hhedu04	Unmatched	0.1051	0.1421	-11.2		-1.77	0.077
	Matched	0.1051	0.1027	0.7	93.6	0.09	0.925
hsedu04	Unmatched	0.1017	0.0809	7.2		1.26	0.209
	Matched	0.1017	0.0933	2.9	59.7	0.34	0.732
anualand04	Unmatched	0.5906	0.7631	-14.3		-1.93	0.054
	Matched	0.5906	0.6324	-3.5	75.8	-0.60	0.550
forland04	Unmatched	0.3691	0.2323	6.5		1.10	0.269
	Matched	0.3691	0.3944	-1.2	81.5	-0.11	0.913
region3	Unmatched	0.0034	0.0487	-28.7		-3.61	0.000
	Matched	0.0034	0.0108	-4.7	83.7	-1.07	0.286
region4	Unmatched	0.2000	0.1109	24.7		4.60	0.000
	Matched	0.2000	0.1854	4.1	83.5	0.45	0.653
region7	Unmatched	0.0509	0.1270	-27.0		-3.86	0.000
	Matched	0.0509	0.0529	-0.7	97.3	-0.11	0.910
urban0404	Unmatched	0.1492	0.2400	-23.1		-3.56	0.000
	Matched	0.1492	0.1402	2.3	90.2	0.31	0.759

Source: Author's estimation from VHLSS 2004 and 2006.

Table 7.13. Testing balance of the conditioning variables for households with work migrants: local linear regression matching with bandwidth of 0.05 (Model 2a).

Variable	Sample	Treated	Control	% bias	% bias reduction	T-statistic	P-value
migration2	Unmatched	0.1593	0.1431	4.5		0.77	0.444
	Matched	0.1593	0.1525	1.9	58.3	0.23	0.821
dforemit04	Unmatched	0.0271	0.0607	-16.4		-2.37	0.018
	Matched	0.0271	0.0271	0.0	100.0	0.00	1.000
ddoremit04	Unmatched	0.8983	0.8452	15.9		2.46	0.014
	Matched	0.8983	0.9288	-9.1	42.6	-1.32	0.188
incomepc04	Unmatched	4,954.2	5,820.2	-19.1		-2.67	0.008
	Matched	4,954.2	4,917.0	0.8	95.7	0.14	0.892
living04	Unmatched	13.2	15.8	-27.0		-3.74	0.000
	Matched	13.2	12.6	5.5	79.7	0.88	0.381
dagri04	Unmatched	0.8780	0.7819	25.8		3.90	0.000
	Matched	0.8780	0.9017	-6.4	75.3	-0.92	0.358
ethnic04	Unmatched	0.0983	0.1597	-18.4		-2.81	0.005
	Matched	0.10	0.1017	-1.0	94.5	-0.14	0.891
hhsize04	Unmatched	4.9797	4.3532	38.0		6.07	0.000
	Matched	4.9797	5.0271	-2.9	92.4	-0.30	0.761
pchild04	Unmatched	0.1577	0.2518	-47.6		-7.29	0.000
	Matched	0.1577	0.1630	-2.7	94.3	-0.36	0.717
pelderly04	Unmatched	0.0709	0.1247	-26.4		-3.63	0.000
	Matched	0.0709	0.0831	-6.0	77.4	-0.92	0.356
pfemale04	Unmatched	0.4582	0.5158	-30.5		-4.90	0.000
	Matched	0.4582	0.4710	-6.8	77.7	-0.90	0.370
agehead04	Unmatched	51.7	48.8	23.3		3.46	0.001
	Matched	51.7	51.9	-1.6	93.2	-0.21	0.832
hhedu04	Unmatched	0.1051	0.1421	-11.2		-1.77	0.077
	Matched	0.1051	0.0780	8.2	26.6	1.14	0.254
hsedu04	Unmatched	0.1017	0.0809	7.2		1.26	0.209
	Matched	0.1017	0.0780	8.2	-13.8	1.01	0.314
anualand04	Unmatched	0.5906	0.7631	-14.3		-1.93	0.054
	Matched	0.5906	0.5036	7.2	49.6	1.55	0.122
forland04	Unmatched	0.3691	0.2323	6.5		1.10	0.269
	Matched	0.3691	0.2339	6.4	1.2	0.84	0.403
region3	Unmatched	0.0034	0.0487	-28.7		-3.61	0.000
	Matched	0.0034	0.0102	-4.3	85.0	-1.00	0.316
region4	Unmatched	0.2000	0.1109	24.7		4.60	0.000
	Matched	0.2000	0.2068	-1.9	92.4	-0.20	0.838
region7	Unmatched	0.0509	0.1270	-27.0		-3.86	0.000
	Matched	0.0509	0.0441	2.4	91.1	0.39	0.699
urban0404	Unmatched	0.1492	0.2400	-23.1		-3.56	0.000
	Matched	0.1492	0.1220	6.9	70.1	0.96	0.337

Source: Author’s estimation from VHLSS 2004 and 2006.

Table 7.14. Testing balance of the conditioning variables for households with non-work migrants: 1 nearest neighbour matching (Model 2b).

Variable	Sample	Treated	Control	% bias	% bias reduction	T-statistic	P-value
migration	Unmatched	0.0773	0.0687	3.3		0.77	0.444
	Matched	0.0773	0.0822	-1.9	42.4	-0.32	0.751
hhsize04	Unmatched	5.5197	4.2079	72.2		18.10	0.000
	Matched	5.5197	5.3553	9.1	87.5	1.44	0.150
pchild04	Unmatched	0.1803	0.2561	-38.5		-8.10	0.000
	Matched	0.1803	0.1565	12.0	68.7	2.40	0.017
pelderly04	Unmatched	0.10	0.12	-11.8		-2.38	0.017
	Matched	0.10	0.09	1.9	83.8	0.44	0.661
pfemale04	Unmatched	0.53	0.51	10.9		2.37	0.018
	Matched	0.5291	0.5275	0.8	92.4	0.15	0.882
agehead04	Unmatched	53.0	48.4	35.3		7.66	0.000
	Matched	53.0	53.1	-0.8	97.7	-0.15	0.880
married04	Unmatched	0.7829	0.8176	-8.7		-2.03	0.042
	Matched	0.7829	0.7944	-2.9	66.9	-0.49	0.623
living04	Unmatched	13.0	16.0	-29.4		-5.95	0.000
	Matched	13.0	13.8	-7.6	74.3	-1.62	0.106
region4	Unmatched	0.0905	0.1217	-10.1		-2.21	0.027
	Matched	0.0905	0.0872	1.1	89.5	0.20	0.840
region6	Unmatched	0.0576	0.0624	-2.0		-0.45	0.649
	Matched	0.0576	0.0395	7.6	-277.3	1.47	0.142

Source: Author's estimation from VHLSS 2004 and 2006.

Table 7.15. Testing balance of the conditioning variables for households with non-work migrants: 5 nearest neighbour matching (Model 2b).

Variable	Sample	Treated	Control	% bias	% bias reduction	T-statistic	P-value
migration	Unmatched	0.0773	0.0687	3.3		0.77	0.444
	Matched	0.0773	0.0799	-1.0	69.3	-0.17	0.865
hhsize04	Unmatched	5.5197	4.2079	72.2		18.10	0.000
	Matched	5.5197	5.4036	6.4	91.1	1.02	0.306
pchild04	Unmatched	0.1803	0.2561	-38.5		-8.10	0.000
	Matched	0.1803	0.1665	7.0	81.8	1.39	0.163
pelderly04	Unmatched	0.0990	0.1247	-11.8		-2.38	0.017
	Matched	0.0990	0.0962	1.3	89.2	0.29	0.769
pfemale04	Unmatched	0.5291	0.5088	10.9		2.37	0.018
	Matched	0.5291	0.5275	0.8	92.4	0.15	0.879
agehead04	Unmatched	53.0	48.4	35.3		7.66	0.000
	Matched	53.0	53.4	-3.3	90.7	-0.61	0.544
married04	Unmatched	0.7829	0.8176	-8.7		-2.03	0.042
	Matched	0.7829	0.7829	0.0	100.0	0.00	1.000
living04	Unmatched	13.0	16.0	-29.4		-5.95	0.000
	Matched	13.0	13.3	-3.1	89.3	-0.68	0.497
region4	Unmatched	0.0905	0.1217	-10.1		-2.21	0.027
	Matched	0.0905	0.0901	0.1	98.9	0.02	0.984
region6	Unmatched	0.0576	0.0624	-2.0		-0.45	0.649
	Matched	0.0576	0.0510	2.8	-37.2	0.51	0.613

Source: Author's estimation from VHLSS 2004 and 2006.

Table 7.16. Testing balance of the conditioning variables for households with non-work migrants: kernel matching with bandwidth of 0.05 (Model 2b).

Variable	Sample	Treated	Control	% bias	% bias reduction	T-statistic	P-value
migration	Unmatched	0.0773	0.0687	3.3		0.77	0.444
	Matched	0.0773	0.0815	-1.6	51.6	-0.27	0.789
hhsize04	Unmatched	5.5197	4.2079	72.2		18.10	0.000
	Matched	5.5197	5.3886	7.2	90.0	1.14	0.254
pchild04	Unmatched	0.1803	0.2561	-38.5		-8.10	0.000
	Matched	0.1803	0.1753	2.5	93.4	0.50	0.620
pelderly04	Unmatched	0.0990	0.1247	-11.8		-2.38	0.017
	Matched	0.0990	0.0983	0.3	97.1	0.08	0.939
pfemale04	Unmatched	0.5291	0.5088	10.9		2.37	0.018
	Matched	0.5291	0.5319	-1.5	86.2	-0.27	0.784
agehead04	Unmatched	53.0	48.4	35.3		7.66	0.000
	Matched	53.0	53.1	-1.1	96.8	-0.21	0.834
married04	Unmatched	0.7829	0.8176	-8.7		-2.03	0.042
	Matched	0.7829	0.7759	1.7	80.0	0.29	0.770
living04	Unmatched	13.0	16.0	-29.4		-5.95	0.000
	Matched	13.0	13.3	-2.4	91.8	-0.53	0.599
region4	Unmatched	0.0905	0.1217	-10.1		-2.21	0.027
	Matched	0.0905	0.0866	1.3	87.6	0.24	0.812
region6	Unmatched	0.0576	0.0624	-2.0		-0.45	0.649
	Matched	0.0576	0.0515	2.5	-25.7	0.46	0.644

Source: Author's estimation from VHLSS 2004 and 2006.

Table 7.17. Testing balance of the conditioning variables for households with non-work migrants: local linear regression with bandwidth of 0.05 (Model 2b).

Variable	Sample	Treated	Control	% bias	% bias reduction	T-statistic	P-value
migration	Unmatched	0.0773	0.0687	3.3		0.77	0.444
	Matched	0.0773	0.0822	-1.9	42.4	-0.32	0.751
hhsize04	Unmatched	5.5197	4.2079	72.2		18.10	0.000
	Matched	5.5197	5.3553	9.1	87.5	1.44	0.150
pchild04	Unmatched	0.1803	0.2561	-38.5		-8.10	0.000
	Matched	0.1803	0.1565	12.0	68.7	2.40	0.017
pelderly04	Unmatched	0.0990	0.1247	-11.8		-2.38	0.017
	Matched	0.0990	0.0949	1.9	83.8	0.44	0.661
pfemale04	Unmatched	0.5291	0.5088	10.9		2.37	0.018
	Matched	0.5291	0.5275	0.8	92.4	0.15	0.882
agehead04	Unmatched	53.0	48.4	35.3		7.66	0.000
	Matched	53.0	53.1	-0.8	97.7	-0.15	0.880
married04	Unmatched	0.7829	0.8176	-8.7		-2.03	0.042
	Matched	0.7829	0.7944	-2.9	66.9	-0.49	0.623
living04	Unmatched	13.0	16.0	-29.4		-5.95	0.000
	Matched	13.0	13.8	-7.6	74.3	-1.62	0.106
region4	Unmatched	0.0905	0.1217	-10.1		-2.21	0.027
	Matched	0.0905	0.0872	1.1	89.5	0.20	0.840
region6	Unmatched	0.0576	0.0624	-2.0		-0.45	0.649
	Matched	0.0576	0.0395	7.6	-277.3	1.47	0.142

Source: Author's estimation from VHLSS 2004 and 2006.

Table 7.18. Impacts of work migration on household welfare in 2006.

	Impact of work migration (Propensity score estimated from Model 2)			Impact of non-work migration (Propensity score estimated from Model 4)		
	1 nearest neighbour matching	5 nearest neighbours matching	Local linear regression matching with bandwidth of 0.05	1 nearest neighbour matching	5 nearest neighbours matching	Local linear regression matching with bandwidth of 0.05
Ratio of members engaged in productive activities to the total household members older than 14 years old (%)	1.0	0.2	-0.9	1.7	1.5	1.6
	[2.4]	[1.9]	[1.6]	[1.9]	[1.6]	[1.4]
Annual working hours per capita	30.8	5.9	-8.4	46.1	43.2	47.4
	[53.1]	[40.0]	[31.7]	[44.1]	[34.5]	[29.3]
Annual working hours per working member	92.1	41.2	27.3	115.1*	118.1**	121.5***
	[76.9]	[60.7]	[49.4]	[61.8]	[48.0]	[42.2]
Per capita internal remittances	548.3***	344.5***	340.6***	388.4***	387.8***	390.8***
	[143.6]	[108.4]	[95.3]	[110.5]	[95.6]	[85.5]
Per capita international remittances	668.5*	721.7*	683.1*	-38.9	-34.0	-23.6
	[392.7]	[380.9]	[379.4]	[186.2]	[134.1]	[100.7]
Per capita remittances (both international and internal)	1,216.8***	1,066.2***	1,023.7***	349.4*	353.7***	367.1***
	[426.5]	[405.2]	[401.1]	[211.9]	[160.0]	[128.5]
Per capita income (household income divided by household size)	1,136.0**	1,005.1**	885.2**	211.7	213.2	262.7
	[548.6]	[476.5]	[420.4]	[395.8]	[292.2]	[246.8]
Household income divided by the number of working members (above 14 years old)	2756.4*	2569.8*	2746.1**	289.3	305.8	402.6
	[1,519.9]	[1,439.3]	[1,389.4]	[898.0]	[664.3]	[558.8]
Household income excluding remittances divided by the number of working members (above 14 years old)	-342.6	-194.8	-153.1	-201.3	-176.0	-127.2
	[674.1]	[483.3]	[348.7]	[698.6]	[542.3]	[479.7]
Ratio of non-farm income to total income (%)	0.9	0.0	-0.2	-1.7	-1.6	-1.4
	[2.3]	[1.8]	[1.5]	[1.6]	[1.3]	[1.1]
Ratio of non-farm income (excluding remittances) to total income (%)	-6.0***	-7.0***	-6.8***	-3.3*	-3.3**	-3.0***
	[2.7]	[2.2]	[1.9]	[1.7]	[1.4]	[1.2]
Simpson index 1 (all income sources)	0.0415**	0.0218	0.0130	0.0062	0.0062	0.0062
	[0.0202]	[0.0154]	[0.0115]	[0.0146]	[0.0113]	[0.0094]
Simpson index 2 (income sources excluding remittances)	-0.0139	-0.0166	-0.0282**	-0.0174	-0.0171	-0.0177*
	[0.0203]	[0.0161]	[0.0125]	[0.0156]	[0.0120]	[0.0100]
Per capita consumption expenditure	257.0	259.9*	311.8**	511.1**	522.2***	530.8***
	[220.0]	[160.2]	[116.4]	[220.0]	[173.3]	[151.7]
Difference between impact on per capita income and impact on per capita expenditure	878.9*	745.2*	573.4	-299.4	-309.0	-268.1
	[494.3]	[439.6]	[394.8]	[335.7]	[242.9]	[194.9]
Difference between impact on per capita income and impact on total remittances per capita	-80.8	-61.1	-138.5	-137.8	-140.6	-104.4
	[359.3]	[268.2]	[188.7]	[341.3]	[262.4]	[222.1]

Note: Because of limited space, this Table reports only the difference-in-differences estimates (similar to the figures in the last column of Table 3). For the observed outcomes of the treated group, see Table 3.
* significant at 10%; ** significant at 5%; *** significant at 1%.
Standard errors in brackets. Standard errors are corrected for sampling weights and estimated using bootstrap (non-parametric) with 500 replications.
Source: Author's estimation from VHLSS 2004 and 2006.

Table 7.19. Impacts of work migration on poverty and inequality in 2006.

	Impact of work migration (Propensity score estimated from Model 2)			Impact of non-work migration (Propensity score estimated from Model 4)		
	1 nearest neighbour matching	5 nearest neighbours matching	Local linear regression matching with bandwidth of 0.05	1 nearest neighbour matching	5 nearest neighbours matching	Local linear regression matching with bandwidth of 0.05
Poverty of households with migration						
P0	-0.0360	-0.0340	-0.0331	-0.0896**	-0.0913***	-0.0931***
	[0.0408]	[0.0311]	[0.0247]	[0.0433]	[0.0357]	[0.0323]
P1	-0.0168	-0.0162*	-0.0161**	-0.0412**	-0.0415**	-0.0421**
	[0.0147]	[0.0085]	[0.0074]	[0.0215]	[0.0173]	[0.0152]
P2	-0.0088	-0.0083*	-0.0082*	-0.0238**	-0.0237**	-0.0240**
	[0.0085]	[0.0045]	[0.0044]	[0.0143]	[0.0113]	[0.0100]
All poverty						
P0	-0.0022	-0.0021	-0.0020	-0.0123**	-0.0125***	-0.0128***
	[0.0025]	[0.0019]	[0.0015]	[0.0060]	[0.0049]	[0.0044]
P1	-0.0010	-0.0010*	-0.0010**	-0.0056*	-0.0057**	-0.0058**
	[0.0009]	[0.0006]	[0.0004]	[0.0030]	[0.0024]	[0.0021]
P2	-0.0005	-0.0005	-0.0005	-0.0033*	-0.0033**	-0.0033**
	[0.0005]	[0.0004]	[0.0003]	[0.0020]	[0.0016]	[0.0014]
All inequality						
Gini	-0.0013	-0.0013*	-0.0013**	-0.0045**	-0.0046***	-0.0047***
	[0.0011]	[0.0007]	[0.0005]	[0.0022]	[0.0018]	[0.0016]
Theil L	-0.0017	-0.0017*	-0.0018**	-0.0073*	-0.0073**	-0.0074***
	[0.0015]	[0.0011]	[0.0009]	[0.0041]	[0.0032]	[0.0029]
Theil T	-0.0016	-0.0016*	-0.0016**	-0.0056**	-0.0056**	-0.0057***
	[0.0013]	[0.0009]	[0.0007]	[0.0028]	[0.0022]	[0.0019]

* significant at 10%; ** significant at 5%; *** significant at 1%.
Standard errors in brackets. Standard errors are corrected for sampling weights and estimated using bootstrap (non-parametric) with 500 replications.
Source: Author's estimation from VHLSS 2004 and 2006.

Appendix 7.2 Simpson index

Simpson's diversity index (also known as Species diversity index) is an index which is often used to measure the biodiversity of a habitat in ecology. It takes into account the number of species present, as well as the relative abundance of each species. In economics, it can be widely used to measure the income diversification. It takes into account both the number of source and the relative amounts of sources. In this chapter, the Simpson index (SI) of income diversity is defined as:

$$SI = 1 - \sum_{i=1}^{k} R_i^2 \qquad (1)$$

where R_i is the ratio of income from source i to the total income, and k is the number of all possible income sources. SI is calculated for all households in the sample data. SI varies between 0 and $(1 - 1/k)$, and larger SI means more income diversification. SI will be equal to 0 if there is only one source of income. On the contrary, SI will be highest at $(1 - 1/k)$ if there are k equal sources of income. In this chapter, k is equal to 7, thus SI will be from 0 to 0.857.[43]

[43] Other diversity indexes can be Shannon-Weaver Index and Herfindahl index. Discussion and application can be found in several studies such as Barrett *et al.* (2000), Barrett and Reardon (2000), Joshi *et al.* (2003), Minot *et al.* (2006).

Chapter 8 Conclusions

8.1 Research objective and questions

Vietnam has committed itself to a 'growth with equity' strategy of development. The country has achieved high economic growth, with annual GDP growth rates of around 6 percent over the past 10 years. At the same time, poverty rates declined remarkably from 58 percent to 16 percent between 1993 and 2006. The incidence of food poverty or ultra poverty decreased from 25 percent to 7 percent during that same period. Unlike other countries such as China, where high economic growth and fast poverty reduction are often associated with a high increase in inequality, Vietnam has achieved this remarkable decrease in poverty with only a slight increase in inequality. The Gini index based on expenditure per capita increased from 0.33 in 1993 to 0.36 in 2006.

In order to reduce poverty, the government of Vietnam has implemented an extensive public safety net with a large number of poverty alleviation programs. An important program of the public safety net is the micro-credit for the poor, which is run by the Vietnam Bank for Social Policies (VBSP). Public transfers to targeted households and people are also an important tool for poverty and inequality reduction. In addition to public programs, private flows such as informal credit, private transfers, remittances and migration are encouraging sources for income growth and poverty reduction.

As mentioned in Chapter 1, the objective of this study is to examine how well economic flows such as micro-credit, public and private transfers, international remittances and migration reach the poor and to measure the extent to which these factors affect household welfare, poverty and inequality in Vietnam. More specifically, the study focuses on four main empirical research questions:

- How extensive is the access of the poor to governmental micro-credit and informal credit? And what is the impact of these credit sources on consumption expenditure, poverty and inequality?
- How effectively do public transfers and domestic private transfers reach the poor? And to what extent do public and private transfers affect household consumption expenditure, poverty and inequality?
- How extensive is the access of the poor to international remittances? And what is the impact of international remittances on household consumption expenditure, poverty and inequality?
- What is the pattern of work and non-work migration of the poor? And what is the impact of work and non-work migration on household consumption expenditures, poverty and inequality?

8.2 Main empirical findings

The thesis is structured into eight chapters. Except for Chapters 1 and 8, which are the introduction and conclusion, respectively, the chapters are written as independent essays on impact evaluation. Chapter 2 reviews the most popular methods of quantitative impact evaluation, which are used to address the problem of program selection. Among the impact evaluation methods, the matching method receives special attention and has been increasingly used in recent years. The traditional literature often deals with the impact evaluation of a single program. However, in reality people can participate in several programs simultaneously. Thus, Chapter 3 contributes to the literature on program impact evaluation by discussing the impact evaluation of multiple programs using regression and matching methods.

The research questions on poverty targeting and the impacts of different economic flows including governmental credit (operated by Vietnam Bank for Social Policies (VBSP)), informal credit, public and private transfers, international remittances, work and non-work migration on poverty and inequality in Vietnam are addressed in Chapters 4 through 7. Poverty is measured by three Foster-Greer-Thorbecke poverty indexes, while inequality is measured by the Gini coefficient, Theil's L and Theil's T indexes. Per capita expenditure is used as a welfare indicator for calculation of poverty and inequality measures. Data used in this study come from Vietnam Household Living Standard Surveys (VHLSS) in 2004 and 2006, which are the most recent nationally representative household surveys.

Regarding the methodology, the study uses fixed-effect regressions to estimate the average effect of credit, transfers and remittances on work effort, income and expenditures of receiving households. The estimation of the impact of credit, transfers and remittances on expenditure poverty and inequality is carried out in several steps. Firstly, we estimate the impact of a program (or an economic flow) on expenditure (using fixed-effect regressions) and construct the counterfactual expenditure in the absence of the flow. Secondly, we estimate a poverty measure or an inequality measure in the state of no-program using this counterfactual expenditure. Thirdly, we assess the impact of the program on the poverty or inequality measure by calculating the difference in the poverty or inequality measure in the presence of the program and the counterfactual poverty or inequality measure in the absence of the program. For migration, which – contrary to the other flows – is defined as a binary variable, we apply difference-in-differences with propensity score matching method instead of fixed-effects regression and otherwise follow the same procedure.

Chapter 4 shows that the impact of governmental credit provided by VBSP on consumption expenditure and poverty is limited. Although VBSP credit is a micro-credit program which is targeted at the poor, the poor receive less VBSP credit than the non-poor. Less than 30 percent of VBSP loans end up in the hands of the poor, which means that up to 70 percent of VBSP loans are obtained by the non-poor. The VBSP program covers only 15 percent of the poor, while informal credit reaches 21 percent of the poor. Not surprisingly, we therefore find that informal credit is more effective in decreasing poverty: it reduces the poverty incidence of borrowers by 1.4 percentage points from 21.1 percent to 19.7 percent. However, the effect of formal credit on total poverty is extremely small and not statistically significant. Regarding VBSP credit, evidence of its effect on poverty is not found. It is possible that the effects of VBSP credit may only be measurable over a longer time period, despite the short-term nature of the loans.

Chapter 5 analyzes the poverty targeting and impacts of public and private transfers. It is found that public transfers do not reach the poor well. Only 14 percent of the poor receive public transfers, while 19 percent of the non-poor obtain these public transfers. In addition, the non-poor receive much higher amounts of transfers per recipient household. Since the non-poor account for a large proportion of the population, they receive around 97 percent of all public transfers. It should be noted that public transfers defined in this study are not limited to transfers provided for the poor. Public transfers can include contribution-based transfers, e.g. pensions, which reach more non-poor than poor people. Although public transfers increase current income of the recipients substantially, they increase current the recipients' expenditures only slightly. As a result, the impact of public transfers on poverty is negligible due to low coverage of poor and relatively low amounts transferred to the poor.

Domestic private transfers are much more successful in reducing poverty. They reduce the poverty rate of the receiving households by 2.1 percentage points from 17.8 percent to 15.7 percent. In addition, private transfers also reduce the incidence of total poverty by 1.8 percentage points from 17.8 percent to 16 percent. Decreases in the depth and severity of poverty due to private domestic transfers are quite substantial. This is because around 88 percent of the poor receive private transfers, and private transfers have a high impact on current expenditures.

Chapter 6 shows that international remittances have a smaller effect on poverty reduction. Yet, international remittances lead to a substantial increase in income and also an increase in consumption. Thus, the main reason for the negligible effect on current expenditure poverty is that in Vietnam mainly the non-poor are remittance recipients. Less than 2 percent of the poor receive international remittances. Moreover, it appears that the direct impact of international remittances on per capita consumption is small since a substantial part of international remittances is saved and invested.

In Chapter 7, we show that both work migration and non-work migration have a positive and significant impact on per capita consumption expenditure of migrant-sending households. Increased expenditure due to migration, both work and non-work, leads to a reduction in poverty. It is interesting that non-work migration is more successful in reducing poverty than work migration, because non-work migration covers a larger proportion of the poor and leads to a higher increase in consumption expenditure than the work-migration. Non-work migration reduces the poverty rate of migrant-sending households by 8.9 percentage points from 21.7 percent to 12.8 percent. Non-work migration reduces the incidence of total poverty by 1.1 percentage points from 17.1 percent to 16 percent.

In this study, the empirical findings on the impact of the economic flows on expenditure inequality are mixed. Both VBSP credit and informal credit have a very small and not statistically significant impact on inequality. It is unexpected that public transfers and international remittances increase inequality slightly. Conversely, domestic private transfers and migration lead to a decrease in inequality.

In short, a flow will have a large impact on poverty and inequality reduction if it covers a large proportion of the poor and has a positive impact on consumption expenditure. In this study, the private flows including informal credit, private domestic transfers and migration are found to lead to a reduction in expenditure poverty. In addition, private domestic transfers and migration reduce inequality. Meanwhile, two public policies on micro-credit and income transfers, which are investigated in this study, do not have the expected effects on poverty and inequality. International remittances are not successful in reducing poverty and inequality either. However, we must keep in mind that this study focuses on an evaluation of the short-run impacts. Credit and transfers can be saved or invested and may thus lead to future increases in income and consumption, which is outside the scope of this study.

8.3 Policy implications

It seems that governmental micro-credit and public transfers are not a strong policy tool to reduce poverty and inequality in the short term. One reason for their low impact on poverty is the low coverage of the poor. In addition, these programs covered a large proportion of the non-poor. Thus, better poverty targeting can increase the effect of the programs on poverty reduction. Improving program targeting requires more accurate classification of household poverty, and more efficient program administration. Disaggregated measures of poverty such as poverty maps at the commune and district levels can be an important tool for the poverty targeting. A recent study by Elbers *et al.* (2007) shows that the impact of budget transferring on poverty is greater when geographic targeting units are smaller such as districts and villages. These findings are also mentioned in other studies, e.g. Baker and Grosh (1994), Bigman and Fofack (2000).

Similarly, international remittances may have positive economic effects, such as enhancing production and investment, especially in the longer run, but are certainly not a panacea for poverty reduction in the short term. To reduce poverty and inequality in the short run, the government of Vietnam would be better to rely on income distribution and poverty reduction programs which are targeted at the poor more directly.

Our results imply that the private flows informal credit, domestic private transfers and migration are more effective in poverty and inequality reduction than public flows. Informal credit is a more likely candidate for poverty reduction in the short-run than government subsidized credit: it already reaches more poor people, provides them with more money, and, contrary to VBSP credit, increases their expenditure. While not directly under public control, financial intermediation through informal lenders is not immune to public policies. Governments can facilitate intermediation by providing an important basic infrastructure, such as a system of laws and courts to support the creation and enforcement of property rights and contracts, credit bureaus to publicize information, and prudential regulation of financial institutions (Conning and Udry, 2005). In addition, the government should have measures to facilitate money transfers between relatives and friends.

Work and non-work migration are important ways to reduce poverty and inequality. Migration leads to increased private transfers and remittances. There are several measures and policies to increase migration. Improvement of transportation and roads can promote not only local markets but also the probability of migration from rural to urban areas. Vocational training programs can provide rural people with production and business skills, and make them more likely to find employment in urban areas. The government can support migrants with social security programs and protective policies for the migrants in the destination areas.

8.4 Limitations and research outlook

This study has several limitations, and further research can be conducted to overcome these. Firstly, we use fixed-effect regression and difference-in-differences with matching to estimate the impact of credit, transfers, remittances and migration on household expenditures, and subsequently assess their impact on poverty and inequality. In doing so, we intend to eliminate the potential bias caused by differences between participants and non-participants in the various flows. Fixed-effects regression only eliminates endogeneity bias caused by unobserved variables that remained unchanged between survey rounds and that have an additive effect on the outcome. We feel that it is reasonable to assume that the relevant household-level variables, such as business and production skills or motivation for higher income and expenditure consumption, were time-invariant during the two periods covered in this study. Fixed-effect regressions will, however, fail to eliminate all endogeneity bias if the unobserved variables which affect expenditures (outcome) and the flows are not time-invariant. It is expected that the estimation bias resulting from these factors is small relative to the bias eliminated by using fixed-effects regression. Yet the availability of valid instrumental variables could improve the accuracy of impact estimates. However, finding good instrumental variables is not an easy task.

Secondly, the study focuses on the short-term impacts of programs. It is possible that credit, cash transfers, and remittances are invested in productive assets. For example, various empirical studies show that international remittances are used to buy land and houses and are invested in financial assets and microenterprises (Adams, 1991, 1996; Dustmann and Kirchkamp, 2002; Woodruff and Zenteno, 2004; Osili, 2007). Cash transfers are also found to have positive impacts on saving and production (Gertler *et al.*, 2006; Soares *et al.*, 2008). If credit, cash transfers and remittances are used in production, it is difficult to detect a significant impact on current income and expenditure in the short term. In this study, we do not estimate the impacts of credit, cash transfers, and remittances on production and assets, since we focus on the impacts on expenditure poverty and inequality.

Thirdly, it should be noted that our estimates only show direct effects. The estimates do not allow for spill-over effects. If credit, transfers and remittances are used productively, indirect effects on the poor may be particularly dramatic. For example, credit, transfers and remittances can be used to finance micro-enterprises, thereby generating employment for the poor. Estimation of the spill-over effects is difficult using household surveys. Alternative methods such as computable general equilibrium (CGE) models could be used.

Fourthly, we must keep in mind that our poverty and inequality estimates do not cover all effects of transfers on welfare. For example, it is found that remittances and public transfers result in a decrease in work effort and thus an increase in leisure, which – like consumptive expenditures – adds to current welfare, but is not accounted for in our poverty calculations.

Finally, like many countries in the world Vietnam has experienced an economic slowdown since 2008. The empirical findings from this study, which are based on data from before the economic slowdown, might not be valid in the context of a stagnating economy. The economic crisis may have caused a structural break in the economy. Capital flows such as transfers and remittances may have decreased or reach different people, and behaviours of the beneficiaries (recipients of programs) may also have changed. When new data become available, these issues can be addressed.

Estimating the long-run effect, indirect and spill-over effects, using different welfare indicators, and investigating how economic crisis can change the effects of credit, transfers, remittances and migration are beyond the scope of the study, but certainly important for future research.

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Summary

Vietnam is often mentioned as an example of a country successful in poverty reduction. According to the Vietnam Household Living Standard Surveys, the poverty incidence fell from 58 percent in 1993 to 29 percent in 2002, and continued to decrease to 16 percent in 2006.[44] In order to reduce poverty, the government of Vietnam has implemented an extensive public safety net with a large number of poverty alleviation programs. An important program within the public safety net is micro-credit for the poor, which is run by the Vietnam Bank for Social Policies (VBSP). Public transfers to targeted households and people are also an important tool for poverty and inequality reduction. In addition to public programs, private flows such as informal credit, private transfers, remittances and migration are encouraging sources of income growth and poverty reduction.

The main objective of this study is to examine how well economic flows including micro-credit, public and private transfers, international remittances and migration reach the poor and to measure the extent to which these economic flows affect household welfare, poverty and inequality in Vietnam. This study uses data from the two most recent Vietnam Household and Living Standard Surveys (VHLSS) of 2004 and 2006 to estimate the impact of different economic factors.

The present thesis is structured into eight chapters. Except Chapters 1 and 8 (the chapters on introduction and conclusions, respectively), the main contents of Chapters 2 through 7 are written as separate essays on impact evaluation. Chapter 2 reviews several popular methods of impact evaluation, which are used to address the problem of program selection in impact estimation. In addition to a randomization-based method, the impact evaluation methods are categorized into methods assuming ‘selection on observables’ and methods assuming ‘selection on unobservables’. Two popular indicators of program impacts discussed in this chapter are the Average Treatment Effect (ATE) and the Average Treatment Effect on Treated (ATT). The chapter discusses how different impact evaluation methods measure ATE and ATT under various identification assumptions. These assumptions are presented in a unified framework of a counterfactual and a two equation model.

Among the impact evaluation methods, the matching method receives special attention and has been increasingly used in recent years. Under the assumption about conditional independence between potential outcomes and program assignment, program impacts measured by ATE and ATT can be identified and estimated using cross-section regression or propensity score matching (PSM). Traditional impact literature often deals with the impact evaluation of a single program. In reality, one can participate in several programs simultaneously and the programs may be correlated. Thus, Chapter 3 contributes to the literature on program impact evaluation by discussing cross-section regression and PSM methods in this general context. It is shown that under the PSM method, the impact of a program of interest can be measured as a weighted average of program impacts on groups with different program statuses. Estimation of impacts of multiple overlapping programs is illustrated using Monte Carlo simulation.

[44] The poverty line is equivalent to the expenditure level that allows for nutritional needs with food consumption securing 2100 calories per day per person and some essential non-food consumption such as clothing and housing. This poverty line is estimated by General Statistics Office of Vietnam and World Bank in Vietnam.

It should be noted that Chapters 2 and 3 do not address the main research questions of the study on the impacts of credit, transfers, remittances and migration on poverty and inequality. These chapters are independent essays which present the literature on program impact evaluation.

The research questions on poverty targeting and the impacts of different economic flows including governmental credit, informal credit, public and private transfers, international remittances, work and non-work migration on poverty and inequality in Vietnam are addressed in Chapters 4 through 7. Poverty is measured by three Foster-Greer-Thorbecke poverty indexes, while inequality is measured by the Gini coefficient, Theil's L and Theil's T indexes. Per capita expenditure is used as a welfare indicator for calculation of poverty and inequality measures.

Regarding the methodology of impact evaluation, the study uses fixed-effect regressions to estimate the average effect of credit, transfers and remittances on work effort, income and expenditures of receiving households. The estimation of the impact of credit, transfers and remittances on expenditure poverty and inequality is carried out in several steps. Firstly, we estimate the impact on expenditure (using fixed-effect regressions) and construct the counterfactual expenditure in the absence of the flow. Secondly, we estimate a poverty measure or an inequality measure in the state of no-program using this counterfactual expenditure. Thirdly, we assess the impact of the program on the poverty or inequality measure by calculating the difference in the poverty or inequality measure in the presence of the program and the counterfactual poverty or inequality measure in the absence of the program. For migration, which – contrary to the other flows – is defined as a binary variable, we apply difference-in-differences with propensity score matching method to instead of fixed-effects regression and otherwise follow the same procedure.

Chapter 4 shows that the impact of governmental credit provided by VBSP on consumption, expenditure and poverty is limited. Although VBSP credit is a micro-credit program which is targeted at the poor, the poor receive less VBSP credit than the non-poor. Less than 30 percent of VBSP loans end up in the hands of the poor, which means that up to 70 percent of VBSP loans are obtained by the non-poor. The VBSP program covers only 15 percent of the poor, while informal credit reaches 21 percent of the poor. Not surprisingly, we therefore find that informal credit is more effective in decreasing poverty: it reduces the poverty incidence of borrowers by 1.4 percentage points from 21.1 percent to 19.7 percent. However, the effect of the informal credit on total poverty is extremely small and not statistically significant. Regarding VBSP credit, evidence of its effect on poverty is not found. It is possible that the effects of VBSP credit may only be measurable over a longer time period, despite the short-term nature of the loans.

Chapter 5 analyzes the poverty targeting and impacts of public and private transfers. It is found that public transfers are ineffective at reaching the poor. Only 14 percent of the poor receive public transfers, while 19 percent of the non-poor obtain these public transfers. In addition, the non-poor receive much higher amounts of transfers per receiving households. Since the non-poor account for a large proportion of population, they receive around 97 percent of all public transfers. It should be noted that public transfers defined in this study are not limited to transfers provided for the poor. Public transfers can include contribution-based transfers, e.g. pensions, which reach more non-poor than poor people. Although public transfers increase the current income of the recipients substantially, they increase the recipients' current expenditures only slightly. As a result, the impact of public transfers on poverty is negligible due to low coverage of the poor and relatively low amounts transferred to the poor.

Domestic private transfers are much more successful in reducing poverty. They decrease the poverty rate of the receiving households by 2.1 percentage points from 17.8 percent to 15.7 percent. In addition, private transfers also reduce the incidence of total poverty by 1.8 percentage points from 17.8 percent to 16 percent. Decreases in the depth and severity of poverty due to private domestic transfers are quite substantial. This is because there are around 88 percent of the poor receiving private transfers, and private transfers have a high impact on current expenditures.

Chapter 6 shows that international remittances have a lesser effect on poverty reduction. Yet, international remittances lead to a substantial increase in income and also an increase in consumption. Thus, the main reason for the negligible effect on current expenditure poverty is that in Vietnam the non-poor are the main remittance recipients. Less than 2 percent of the poor receive international remittances. Moreover, it appears that the direct impact of international remittances on per capita consumption is small since a substantial part of international remittances is being saved and invested.

In Chapter 7, we show that both work migration and non-work migration have a positive and significant impact on per capita consumption expenditure of migrant-sending households. Increased expenditures due to migration, both work and non-work, lead to reduction in poverty. It is interesting that non-work migration is more successful in reducing poverty than work migration, because non-work migration covers a larger proportion of the poor and leads to a higher increase in consumption expenditure than the work-migration. Non-work migration decreases the poverty rate of migrant-sending households by 8.9 percentage points from 21.7 percent to 12.8 percent. Non-work migration reduces the incidence of total poverty by 1.1 percentage points from 17.1 percent to 16 percent.

In this study, the empirical findings on the impact of the economic flows on expenditure inequality are mixed. Both VBSP credit and informal credit have a very small and not statistically significant impact on inequality. It is unexpected that public transfers and international remittances increase inequality slightly. In contrast, domestic private transfers and migration lead to a decrease in inequality.

Chapter 8 presents the main empirical findings on the research questions which are posed by this study. It also proposes some policy implications which are based on the main empirical findings of the study. Finally, a discussion on the limitations of the study and the outlook for future research completes the study.

Samenvatting

Vietnam wordt vaak genoemd als voorbeeld van een land dat succesvol is geweest in de strijd tegen armoede. Volgens de Vietnamese huishoud-levensstandaardenquête (LSMS) is het percentage armen gedaald van 58 in 1993 tot 29 in 2002 en vervolgens tot 16 procent in 2006.[45] Om de armoede te verminderen heeft de Vietnamese overheid een uitgebreid stelsel van sociale zekerheid doorgevoerd met een groot aantal armoedebestrijdingsprogramma's. Een belangrijk programma binnen dit stelsel is microkrediet voor de armen, dat beheerd wordt door de Vietnamese Bank voor Sociaal Beleid (VBSP). Publieke overdrachten aan geselecteerde huishoudens en personen zijn ook een belangrijk middel om armoede en ongelijkheid te verminderen.

Het belangrijkste doel van deze studie is om te analyseren hoe goed economische stromen zoals microkrediet, publieke en private overdrachten, internationale overschrijvingen en migratie de armen bereiken en om te meten in hoeverre deze stromen het welzijn van huishoudens, armoede en ongelijkheid in Vietnam beïnvloeden. Deze studie maakt gebruik van data van de twee meest recente VHLSS van 2004 en 2006 om het effect van verschillende economische factoren te bepalen.

Het proefschrift is verdeeld in acht hoofdstukken. Behalve hoofdstuk 1 en 8 (het introductie- en conclusiehoofdstuk) zijn de hoofdstukken geschreven als afzonderlijke essays over effectmeting. Hoofdstuk 2 behandelt een aantal populaire effectbepalingsmethodes, die gebruikt worden om het probleem van programmaselectie bij effectschattingen op te lossen. De evaluatie methodes zijn geclassificeerd als methodes gebaseerd op randomizering, methodes uitgaande van selectie op basis van geobserveerde variabelen en methodes uitgaande van selectie op basis van niet-geobserveerde variabelen. Twee populaire effectsindicatoren die in dit hoofdstuk besproken worden zijn: het gemiddelde behandelingseffect (ATE) en het gemiddelde behandelingseffect voor de behandelden (ATT) In het hoofdstuk wordt besproken hoe verschillende effectbepalingsmethoden ATE en ATT meten bij verschillende aannames over indentificatie. Deze aannames worden gepresenteerd in een uniform stramien van een hypothetisch alternatief zonder programma en een model met twee vergelijkingen.

De steeds populairder wordende matching methodes krijgen speciale aandacht. Onder de aanname van conditionele onafhankelijkheid tussen potentiële uitkomsten en programmatoewijzing kunnen de programma-effecten gemeten door ATE en ATT worden geïdentificeerd en geschat door middel van cross-sectie regressie of 'propsensity score matching' (PSM). De traditionele effectliteratuur behandelt meestal de effectmeting van een individueel progamma. In werkelijkheid kan iemand tegelijkertijd deelnemen aan verschillende programma's en die programma's kunnen met elkaar gecorreleerd zijn. Hoofdstuk 3 draagt bij aan de literatuur over effectbepaling door cross-sectie regressie en PSM methodes in deze algemene context te bediscussiëren. Het laat zien dat bij de PSM methode het effect van het programma gemeten kan worden als gewogen gemiddelde van de programma-effecten van groepen met verschillende programmastatussen. Het schatten van het effect van verscheidene overlappende programma's wordt geïllustreerd met Monte Carlo simulaties.

[45] De armoedegrens is gelijk aan het uitgavenniveau dat een voedselconsumptie van 2100 calorieën per persoon per dag en wat essentiële niet-voedselconsumptie, zoals kleding en huisvesting, mogelijk maakt.

Hoofdstukken 1 en 2 behandelen niet de hoofdvragen van het onderzoek naar de effecten van krediet, overdrachten, overschrijvingen en migratie op armoede en ongelijkheid. Deze hoofdstukken zijn onafhankelijke essays die de literatuur over effectevaluatie bespreken.

De onderzoeksvragen over het bereiken van de armen en het effect van verschillende economische stromen zoals overheidskrediet, informeel krediet, publieke en private overdrachten, en internationale overschrijvingen, arbeids- en overige migratie op armoede en ongelijkheid in Vietnam worden behandeld in Hoofdstukken 4 tot en met 7. Armoede wordt gemeten door drie Foster-Greer-Thorbecke armoede-indices, terwijl ongelijkheid wordt gemeten door de Gini coefficient, Theil's L en Theil's T indices. Uitgaven per hoofd wordt gebruikt als een welzijnsindicator voor het berekenen van armoede- en ongelijkheidsmaten.

Als methodologie gebruikt de studie fixed-effect regressies om het gemiddelde effect te meten van krediet, overdrachten en overschrijvingen op arbeidsinspanning, inkomen en uitgaven van ontvangende huishoudens. Het schatten van het effect van deze stromen op armoede en ongelijkheid is in verschillende stappen gedaan. Ten eerste schatten we het effect op uitgaven (door fixed-effect regressies) en bepalen de hypothetische alternatieve uitgaven in afwezigheid van de stroom. Ten tweede schatten we een armoedemaat of ongelijkheidsmaat voor de hypothetische situatie zonder programma, gebruik makend van dit hypothetische alternatief. Ten derde bepalen we het effect van het programma op de armoede- of ongelijkheidsmaat door het verschil te berekenen tussen de werkelijke maatstaf met en de hypothetische maatstaf zonder programma. In het geval van migratie, die in tegenstelling tot de andere stromen gedefinieerd is als een binaire variabele, gebruiken we verschil-in-verschil metingen met propensity score matching in plaats van fixed-effect regressie. Verder volgen we dezelfde procedure.

Hoofdstuk 4 laat zien dat het effect van overheidskrediet door VBSP op consumptie, uitgaven en armoede beperkt is. Hoewel VBSP een microfinancieringsprogramma is gericht op de armen, ontvangen de armen minder VBSP krediet dan de niet-armen. Minder dan 30 procent van de VBSP leningen bereikt de armen, wat betekent dat meer dan 70 procent van de VBSP leningen terechtkomen bij de niet-armen. Het VBSP programma bereikt slechts 15 procent van de armen, terwijl 21 procent van de armen informeel krediet krijgt. Het is daarom niet verassend dat we vinden dat informeel krediet effectiever is voor armoedebestrijding: het vermindert het voorkomen van armoede onder leners met 1.4 procentpunten van 21.1 procent tot 19.7 procent. Het effect van informeel krediet op de totale armoede is echter verwaarloosbaar. We hebben geen effect gevonden van VBSP krediet op armoede. Het is mogelijk dat die effecten alleen meetbaar zijn over een langere periode, ondanks dat het om kortetermijnleningen gaat.

In hoofdstuk 5 wordt geanalyseerd in hoeverre publieke en private overdrachten de armen bereiken en wat de effecten van deze overdrachten zijn. We vinden dat publieke overdrachten de armen niet effectief bereiken. Slechts 14 procent van de armen ten opzichte van 19 procent van de niet armen ontvangt publieke transfers. Bovendien ontvangen de niet-armen veel hogere bedragen per ontvangend huishouden. Omdat de niet-armen een veel groter deel van de bevolking vormen, ontvangen zij ongeveer 97 procent van alle publieke overdrachten. De publieke overdrachten zoals gebruikt in deze studie omvatten overigens niet alleen overdrachten gericht op de armen. Ze behelzen ook overdrachten waarvoor contributie vereist is, zoals pensioenen, die meer niet armen dan armen bereiken. Hoewel publieke overdrachten het inkomen van de ontvangers substantieel verhogen, hebben ze slechts een klein effect of de uitgaven. Hierdoor en door het geringe aantal armen dat deze overdrachten ontvangt, is het effect op armoede verwaarloosbaar.

Binnenlandse private transfers hebben veel meer effect op armoede. Ze verlagen het percentage armen onder ontvangende huishoudens met 2.1 procentpunt van 17.8 tot 15.7 procent. Bovendien nemen de diepte en hevigheid van armoede nemen sterk af door private overdrachten. Dit komt doordat ongeveer 88 procent van de armen private overdrachten ontvangen en doordat private overdrachten een sterk effect hebben op bestedingen.

Hoofdstuk 6 laat zien dat internationale overschrijvingen een veel minder sterk effect hebben op armoede. Ze leiden echter wel tot een substantiële verhoging van het inkomen en in mindere mate de consumptie. De belangrijkste reden voor het verwaarloosbare effect van internationale overschrijvingen op armoede is dan ook dat in Vietnam vooral de niet-armen overschrijvingen ontvangen. Minder dan 2 procent van de armen ontvangt internationale overschrijvingen. Het effect van internationale overschrijvingen op consumptie blijkt bovendien klein te zijn doordat een groot deel wordt gespaard of geïnvesteerd.

In hoofdstuk 7 laten we zien dat zowel arbeidsmigratie als overige migratie een positief en significant effect heeft op de consumptiebestedingen per hoofd van de migrantensturende huishoudens. Een toename in uitgave als gevolg van beide typen migratie leidt tot een afname van armoede. Interessant genoeg is dit effect het grootst voor niet-werkgerelateerde migratie. Het armoedepercentage van migrantensturende huishoudens neemt door dit type migratie met 8.9 procentpunten af van 21.7 procent to 12.8 procent and het totale percentage armen met 1.1 procentpunten van 17.1 procent to 16 procent.

De empirische bevindingen van deze studie over het effect van economische stromen op uitgavenongelijkheid zijn gemengd. Zowel VBSP krediet als informeel krediet heeft een zeel klein en niet statistisch significant effect op ongelijkheid.

Tegen de verwachtingen in vergroten publieke transfers en internationale overschrijvingen de ongelijkheid enigszins. Binnenlandse private overdrachten en migratie daarentegen leiden tot een afname van ongelijkheid.

De belangrijkste empirische bevindingen over de onderzoeksvragen zoals gesteld in deze studie worden gepresenteerd in hoofdstuk 8. Dit hoofdstuk suggereert bovendien een aantal consequenties van deze bevindingen voor beleid. Ten slotte worden de beperkingen van de studie en een aantal suggesties voor toekomstig onderzoek besproken.

Curriculum Vitae

Nguyen Viet Cuong (Mr.) was born on 7th June 2009 in Hanoi, Vietnam. He obtained a Bachelor Degree in Trade and International Economics from National Economics University (NEU), Hanoi, Vietnam in 1998. After graduation, he worked as an assistant lecturer for Faculty of Trade and International Economics, NEU. During the period 2000-2002, he studied a master program in development economics at Wageningen University, the Netherlands and received a Master Degree with distinction. He was awarded the Professor H. C. van der Plas award for the best thesis of the Master of Science Courses of Wageningen University in the year 2002. His master thesis is titled 'Is Economic Growth Pro-Poor in Vietnam? Evidence from 1993-1998'. In late 2003, he joined a PhD program at the Development Economics Group, also at Wageningen University.

Currently, Cuong is a lecturer at the Faculty of Trade and International Economics, NEU. He is teaching an undergraduate course named 'Management of Trade Enterprises'. In addition, he is teaching a course 'Impact Evaluation of Development Programs' in the Master Program in Development Economics which is run by NEU and the Institute of Social Studies (ISS), the Netherlands.

Training and supervision plan

Annex to statement
Name: Nguyen Viet Cuong
PhD student, Mansholt Graduate School of Social Sciences (MG3S)
Completed Training and Supervision Plan

Description	Institute / Department	Year	Credit*
Courses:			**18**
Mansholt Introduction course	Mansholt Graduate School of Social Sciences (MG3S)	2002	1
Research Methodology: Designing and conducting a PhD research project	MG3S	2004	2
Econometrics II	Social Sciences, Wageningen University (D050-223)	2001	3
Agricultural Models	Social Sciences, Wageningen University (D150-203)	2001	5
Advanced Impact Evaluation	World Bank Institute, Washington; Korea National Statistical Office	2002	1
Food-Safety Risk Analysis	MG3S	2003	1
Microeconomics I	Tinbergen Institute	2004	3
Impact Evaluation of Program/Project	Georgetown University	2006	2
Presentations at conferences and workshops:**			**3**
Mansholt Multidisciplinary seminar		2008	1
7th international conference of The Association for Public Economic Theory, Hanoi (Impact Evaluation of Multiple Overlapping Programs Using Propensity Score Matching Method)		2006	1
7th international conference of The Association for Public Economic Theory, Hanoi (Poverty Targeting and Impact of the National Micro-Credit program in Vietnam: A non-parametric approach)		2006	1
Total credits			**21**

*One credit on average is equivalent to 40 hours of course work.
** Presented two separate papers in the same conference.