THE IMPACT OF LABOR INCOME REWARD PLAN:

EITC EXPERIMENT SERIES IN CHINA

A Dissertation

by

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ABSTRACT

China currently has no in-work benefit nor social welfare programs such as the Earned Income Tax Credits (EITC) in the United States. Yet, the literature and global evidence have suggested EITC-type programs' potentials on poverty alleviation and work incentives. This dissertation examines the first-ever EITC field experiment series in China with a variety of interventions and rigorous validation procedures. The experiment series shows that the participation of the program increases employment and hours worked. It also finds significant increases in household earnings and expenditures. These effects are substantially larger than the existing literature partially because of the strong treatment intensity. This dissertation contributes to a large literature that studies the effect of in-work benefits.

DEDICATION

This dissertation is dedicated to my mother, who has devoted everything to allow me to pursue my success and happiness.

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TABLE OF CONTENTS

Page

LIST OF FIGURES

FIGURE Page

LIST OF TABLES

TABLE Page

1. INTRODUCTION AND LITERATURE REVIEW

1.1 Introduction

China currently has no in-work benefits, such as Earned Income Tax Credits (EITC) in the United States. Yet, the literature and global evidence have suggested EITC-type programs' substantial potentials on poverty alleviation and work incentives. This dissertation studies the first-ever EITC-type experiment series that evaluates its feasibility and effect in China. This section will introduce a quick overview of the dissertation, including motivation, research question, design, result, and contribution.

1.1.1 Motivation

One distinguishing fact about China that makes this experiment particularly critical is that China currently spends very little on social spending (3.05% vs. 12.42% for OECD average[1]), particularly on cash transfer programs (0.40% of GDP vs. 3.04% in the US[2]). All of the current welfare programs in China (e.g., Dibao) are means-tested programs. Moreover, China has no incentive-compatible transfer program, while the US has many. Importantly, the evaluations of the current means-tested programs in China indicate that their performance is very unsatisfactory (e.g., Ravallion et al., 2006). An opportunity arises if China can substantially increase cash transfer. Hence, a comprehensive evaluation through experiments to evaluate an EITC-type program-a

[1] I use public social expenditure (cash and in-kind) in OECD Social Expenditure Database (SOCX) minus pension to calculate the percentages for OECD countries. China's percentage is calculated by incorporating all the transfers that fall in OECD classifications. The data for China is from the National Government Final Accounts (Ministry of Finance, 2017). OECD's classification of social expenditure includes old-age, survivors, incapacity-related benefits, health, family, active labor market programs, unemployment, housing, and expenditures on other social policies. Tax credits are not included. Please see the OECD SOCX Manual for more information.

[2] I calculate China's percentage by incorporating all the cash transfer that are listed in National Government Final Accounts (Ministry of Finance, 2017). The US's percentage is calculated by incorporating cash transfer that is listed in SOCX plus EITC. A program is included if and only if China has one for a similar purpose. Therefore, I am maximizing the transfers in China while censoring the number for the US. Cash transfers that are considered for China include Minimum Subsistence (Dibao), incapacity-related cash benefits, Extreme Poverty Allowance, Agricultural Income Stabilization Subsidy, Grain for Green Cash Subsidy, Catastrophic Supplemental Aid, and other subsistence aids. Cash transfers that are considered for the US include incapacity-related cash benefits, survivor cash benefits, other cash benefits (SNAP and other), unemployment cash benefits, family cash benefits (TANF), and Earned Income Tax Credit (EITC).

world-popular incentive-compatible cash transfer program that has long been proven effective and successful has important policy implications.

1.1.2 Research Question

This dissertation aims to answer the following questions through three field experiments: A) will EITC policy work in China, i.e., will EITC policy increase the labor supply? B) If the policy increases the labor supply, will it increase the earning? C) If it increases the earning, will it increase the expenditure?

1.1.3 Study Design

Between 2014 and 2018, China Household Finance and Survey (CHFS) at Southwestern University of Finance and Economics conducted a series of experiments that aim to test the effects of an EITC-type policy. Each experiment has a different design and population focus. There are a total of 1,798 households that participated in the experiments. Households in the treatment group enrolled an EITC-type program-Labor Income Reward Plan, which is one of the typical in-work benefits among OECD countries. Upon enrollment, households started receiving cash rewards regularly. The amount of reward depends on household earning in the last month. The offered EITC programs contain three stages, including phase-in, plateau, and phase-out[3]. The amount of reward will first increase with earning. Then, it reaches the maximum and no longer changes with the earning (plateau). Eventually, the reward will start decreasing as households earn more and move into the phase-out stage. Once the earning exceeds the break-even point, the household becomes ineligible for the program automatically. Enrolled households are required to report their earnings and employment status every month to get the rewards. Each claim needs supporting materials such as proof of employment and income (see Appendices). Household surveys were conducted among households in both the treatment and the control group to track changes such as employment, hours worked, earning, consumption, and self-reported health conditions. Finally, regression analysis and distribution analysis are performed to identify the impacts of the EITC programs on a

[3]One exception is that the EITC that was offered in Mabian/Muchuan Experiment contains only a phase-in stage and a plateau.

variety of outcomes.

The designs have two important and unique features that are worth noting. First, the experiments implement a relatively strong treatment intensity compared to the existing studies (3%-7% of the income). Second, the designs employed a comprehensive measure to audit and validate each earning claim. Given that China has a large portion (47%[4]) of self-employed jobs, this validation process is particularly crucial for the experiments since it is relatively easier for the self-employed to manipulate earnings compared to the employed with formal contracts.

1.1.4 Main Result

The results show that Labor Income Reward Plan increases labor supply significantly on both extensive margin and intensive margin. Estimates show that program participation increases the average number of employed per household and household monthly hours worked. Consequently, household monthly earning (without the benefit) and monthly expenditure have also increased. The size of the effects seems to be a lot larger than the existing literature, which is partially due to the strong treatment intensity.

1.1.5 Contribution

This dissertation primarily contributes to the literature that studies the effects of incentive-compatible transfer programs using field experiments and non-experimental methods by presenting the first-ever EITC field experiment series in developing countries with rigorous validation procedures. The dissertation yields important policy implications for governments and policymakers.

The next section will briefly summarize China's current anti-poverty policies, challenges, and solutions. Then, it will discuss the Earned Income Tax Credit policy in the United States and relevant studies. Chapter Two, Chapter Three, and Chapter Four cover three EITC experiments that are conducted in China, including Wutongqiao Experiment, Mabian/Muchuan Experiment, and Leibo Experiment. Each of the three chapters will first describe the design of the experiment and the general implementation procedure. Then, it presents the methodology that is used for

[4]See The World Bank (2019) for data source.

analysis and the main results. Next, there will be a section that explores specific parts of the experiment. The last section will summarize the findings of the experiment. Finally, Chapter Five concludes.

1.2 Background: China's War on Poverty and EITC

China's current objective for anti-poverty policies is to lift all registered poor households and counties out of poverty and solve the regional poverty problem by 2020. According to the official agenda, a household is considered as being lifted out of poverty if the household has a stable source of income, income per capita higher than the national poverty line, does not have food and clothing issue, and guaranteed access to compulsory education, basic healthcare, and housing safety. The national poverty line was 2,300 RMB in 2015 when the objective was announced. Estimation shows that there were 70 million people in China who lived under the national poverty line. The goal is to lift all of them out of poverty by 2020. In order to do so, the government has released a poverty alleviation plan for the 13th Five-Year Plan period (2016-2020) that describes the detailed policies in each sector.

China's current anti-poverty policies can be categorized as regional and household policies. According to the official, China has 14 poor areas, 832 poor counties (30% of the total), 148 poor villages (21% of the total), and 29.5 million registered poor households (7% of the total). These entities are heavily overlapped with each other. More importantly, household policies only apply to registered poor households. Other policies are regional, which means they apply to geographical areas instead of a specific group of people or households.

1.2.1 Regional Policies

Regional policies mainly involve sectors such as industry, employment, education, healthcare, ecosystem restoration, and infrastructure construction. Policies are largely implemented in forms of government-oriented construction projects, investments, and subsidies. Industry policies focus on supporting poor areas to develop agricultural production and tourism. For agricultural production, poor villages can apply for subsidies from governments to develop their own unified, large

scale agricultural production projects that produce local specialty products. For tourism, the government has selected around 6,000 poor villages to make investment and construct infrastructure to develop tourist sites. Employment policies mainly consist of job training programs that are subsidized by governments. Education policies focus on supporting kindergarten constructions in poor areas. Meanwhile, each student in the compulsory education system is provided with a 4 RMB (0.6 USD) food subsidy per day to improve nutrition. For higher education, poor areas can enjoy exclusive admission quotas among public universities. Healthcare policies focus on supporting hospital constructions in poor areas. Ecosystem restoration mainly involves several large scale restoration projects. The purpose of the projects is to reverse the degeneration of ecosystems by planting vegetation. In addition, populations that reside in habitat conservation areas will be moved to other places. Governments will provide cash compensations as well as new houses and relevant infrastructure constructions. Infrastructure construction largely involves government-invested projects, including roads, railways, airports, irrigation systems, drinking water systems, power plants, electricity transmissions, communication networks, farming and public facilities. Lastly, local governments have invested and installed mini solar power stations and hydroelectric power stations for households in some areas. By selling unused electricity to the grid companies, these power stations can generate additional income for the poor households continuously.

1.2.2 Household Policies

Household policies mainly involve sectors such as job training, Minimum Subsistence, house renovation, moving, healthcare guarantee, and micro-finance discount. Policies are implemented in forms of privileges. First of all, the government will organize farming technique training programs so that registered poor households with able-bodied persons are guaranteed to master at least one useful skill to start farming. Registered poor households can enroll in technical or vocational schools for free. Meanwhile, registered poor households enjoy scholarships and subsidies for living costs while studying in universities. Once graduated, there will be guaranteed employment arrangements. For education, registered poor households can waive the miscellaneous fees if they enroll in senior high schools. Some majors at some of the technical schools have exclusive quotas

specially allocated for the poor households. In addition, poor households are guaranteed scholarships and student loans in universities. For healthcare, governments provide full subsidy to poor households for health insurance. The households also enjoy lower deductible levels and higher reimbursement rates as well as a catastrophic disease aid. Lastly, poor households can apply for small loans without providing securities. Registered poor households can also apply for soft loans or loans with discounted interest rates.

1.2.3 Inaccurate Identification and Welfare Dependency

Regional policies are made to deal with underdeveloped economies with problems such as lack of public infrastructure (e.g., education and healthcare facilities), lack of natural resources, and severely degenerated ecosystems, which are typical among poor areas. However, regional policies cannot fully accomplish the goal of poverty reduction. First of all, the government's identification of poor areas is inaccurate. Wang et al. (2007)'s study shows that, in 2001, about 25% of poor counties are over-qualified. About 48% of poor villages are not actually the poorest. Even if the identifications were accurate, regional policies could not fully cover all poverty in the country. Theoretically, the 14 poor areas and counties can only cover 50% to 60% of the poor population (Li et al., 2017). This means that almost half of the poor population cannot receive benefits directly from the regional policies. The policies also have some issues. On the one hand, most regional policies are implemented in forms of government-oriented construction projects, investments, and subsidies. Usually, the policies only put restrictions on items such as dates of completion or amount of investments. Policies do not require reasonable evaluations to investigate the effects on poverty reduction. Therefore, it is almost impossible to evaluate these policies through quantitative analysis. On the other hand, almost all of the funds are financed by the government. This makes the poor areas rely heavily on governments' aids and lack of motivation to reduce poverty.

Household policies are made to help families recover from the ordeals. Most of the policies can be characterized as subsistence. Consequently, household policies can only help the poor to some extent. On top of that, the identification of registered poor households is very inaccurate. Many poor households are not registered. Wang and Guo (2015) conducted a survey of 1,200

households. They found that about 40% of the households are over qualified. About 58% eligible households below the poverty lines are not registered as poor. Moreover, there is almost no policy that provides households with incentives to increase their incomes. As a result, there has been almost no increase in income among registered poor households. Thus, the welfare dependency issue arises, and the pressure for the households to return to poverty has been growing. For example, the Rural Minimum Income Guarantee is a typical cash transfer program that can be applied by any household that has income below the poverty line. For poor households, an increase in income means facing a greater risk of losing cash grants. Some poor households refused to be lifted from poverty deliberately because of this reason. Other households, realizing the loss of the cash grants, decided to return to poverty and start relying on welfare again. This fact was reflected by the decrease in income among the low-income group on the macro-level. From 2013 to 2016, China's rural disposable income per capita has been increased by 7% on average every year. However, the income of the bottom tier has been decreasing. This indicates that the welfare dependency issue has been a prevalent phenomenon.

1.2.4 Solution: Incentives-compatible Programs

China's current regional and household anti-poverty policies share two common issues. One issue is inaccurate identification. The other issue is dependency. The government has been making improvements such as using dynamic identification methods to update the poor areas routinely and putting more restrictions on eligibility for Rural Minimum Income Guarantee. Although these efforts helped alleviate the problems, they touched a little the fundamental problem of policy rent-seeking among regions and households. The key to solving the problems once and for all is to implement an incentives-compatible transfer program/policy, so that poor households have incentives to increase their earnings while other households do not have incentives to lie. Thus, it removes the identification issue and the dependency issue from the source.

1.2.5 Earned Income Tax Credit

EITC, or Earned Income Tax Credit, is a typical incentive-compatible transfer payment policy in the United States (Nichols and Rothstein, 2015). It is a refundable tax credit program that was launched during Ford's administration in 1975. The idea is to use tax credits as a form of reward to encourage households to increase their earnings. The more the households earned, the larger the credits they will receive. This design is incentive-compatible. On the one hand, it resolves the inaccurate identification issue since there will be no incentives for the households to pretend to be poor since under-reporting leads to fewer credits. On the other hand, It resolves the welfare dependency since the credit is no longer guaranteed, and households will always try to increase the earnings to get more credits.

EITC was originated from president Lyndon Johnson's idea to end poverty as a part of his initiatives of Great Society in 1964. Johnson argued, in his 1964 Economic Report, that providing cash grants to the poor will not solve the problem from the root. As he put it, it would be far better, even if more difficult to equip and permit the poor of the Nation to produce and earn the additional money required to escape from the poverty (Ventry, 2000). His advocate is closely related to heavy welfare dependency at the time. One of the widely discussed cash grant alternatives is the Negative Income Tax scheme. However, the design was criticized for being too expensive, and for not being consistently incentive-compatible when combined with other social welfare programs. In order to resolve the dispute, starting from 1968, the Office of Economic Opportunity conducted several large scale social experiments in New Jersey, Iowa, Washington, Colorado, and Indiana. However, much of these efforts have gone wasted due to data manipulation, after which NIT was terminated. It was not until 1975, when Ford's administration sought for a major tax cut to stimulate the economy, that the EITC came out with a low profile, and it was considered to be a huge success. After which, EITC continued to expand and become one of the major anti-poverty policy today. In the year of 2018, there were 25 million eligible workers and families received about $63 billion in EITC (IRS, 2018), which is about $2,400 on average. It is proved that EITC can effectively promote labor participation, increase income, and lift households out of poverty.

Today, its achievements in anti-poverty and employment have been highly regarded all over the world. There are at least 16 countries that have adopted similar policies such as the United Kingdom, Canada, New Zealand, Austria, France, Netherlands, Hungary, Ireland, Slovak, Spain, Italy, Belgium, Denmark, Finland, Korea, and Sweden.

1.3 Literature

There is a large body of non-experimental studies that backs up the success of EITC. Defining EITC-type programs is difficult given the variety of forms it could take (Nichols and Rothstein, 2016. For this reason, I focus on studies that are closely related to EITC and experiment studies.

The EITC in the US was initially introduced in 1975. However, there was no EITC experiment until 1995. The program experienced several major expansions in 1986, 1990, 1993, and 2009. Therefore, most early studies use quasi-experiment designs to examine the impacts of EITC. From 1995, there have been three EITC experiments in North America, including Canada Self-Sufficient Project (SSP), Minnesota Family Investment Program (MFIP), and New York Paycheck Plus Experiment. For non-experimental studies, Eissa and Hoynes (2006) and Nichols and Rothstein (2016) have complied reviews that cover most of the topics, including EITC history, the detailed provisions, and studies. Studies that are more focused on specific topics are also very helpful to understand EITC, such as the excellent historical review by Ventry (2000), and the discussion of contemporary welfare reforms by Meyer and Rosenbaum (2001). These quality works provide researchers insightful knowledge and perspectives about EITC.

1.3.1 Labor Participation

The results from the experimental studies are unambiguously positive in terms of employment and earning (Card and Robins, 1996; Lin, 1998; Gennetian and Miller, 2000 Michalopoulos et al., 2000; Gennetian et al., 2002; Michalopoulos et al., 2002; Card and Hyslop, 2005; Gennetian et al., 2005; Miller et al., 2018).

As for non-experimental studies, most of the findings support the ideas that EITC has positive impact on labor force participation (extensive margin) (Eissa and Liebman, 1996; Eissa and

Hoynes, 1998; Ellwood, 2000; Meyer and Rosenbaum, 2000; Meyer and Rosenbaum, 2001; Grogger, 2003; Eissa and Hoynes, 2004; Rothstein, 2005; Hotz and Scholz, 2006; Bastian, 2017; Hoynes and Patel, 2018) with few exceptions (Cancian and Levinson, 2005; Kleven, 2019). While most positive effects are concentrated on single mothers and married men, several studies find that labor force participation has reduced among married mothers (Eissa and Hoynes, 1998; Ellwood, 2000; Eissa and Hoynes, 2004).

Many early studies use the Difference-in-Differences approach to examine the impact of single or multiple EITC expansions on labor supply (Eissa and Liebman, 1996; Ellwood, 2000; Hotz and Scholz, 2000; Meyer and Rosenbaum, 2000; Rothstein, 2005). Most of the studies use single mothers with fewer children as the control group and single mothers with more children as the treatment group. For example, Eissa and Liebman (1996), Hotz and Scholz (2000), Meyer and Rosenbaum (2001) compared single mothers with one child and single mothers with two or more children. Ellwood (2000) and Rothstein (2005) use the variation of the credits across the income group, and compared single mothers with different earning and working skills. Results show that labor participation among single mothers increased 16% between 1993 and 2000. Meanwhile, labor participation among singe mothers without children remained steady during the same period (Eissa and Hoynes, 2006). Many researchers have found that single mothers with children who have less income and education increased labor participation most during the expansion. More importantly, they are also the ones that benefited the most from the expansions (Ellwood, 2000; Rothstein, 2005; Meyer and Rosenbaum, 2000).

In general, the studies above showed that EITC has strong positive impacts on labor supply among single mothers. Moreover, while the methodologies and subjects are different, the conclusions are unambiguously consistent. As for magnitudes of the impact, it largely depends on which expansion the study is examining. Eissa and Liebman (1996) studied the expansion in 1986, which indicates that EITC increased labor participation by 2.8%. Meyer and Rosenbaum (2001) find that, on average, EITC increased labor participation by 8.7% annually between 1984 and 1996. Its impact between 1992 and 1996 was relatively small, only 3.5%.

Later reforms introduce different benefit schemes for families with different structures. This allows researchers to investigate how EITC can affect labor participation differently across different households. For example, Eissa and Hoynes (2004) examine the labor participation response of married couples to EITC expansions between 1984 and 1996. Results show that the EITC expansions reduced the total labor supply of married couples, which is offset by the declines in labor supplies among the wives, implying that the EITC is effectively subsidizing married mothers to stay at home. More importantly, Eissa and Hoynes (2004)'s study shows that EITC schemes do not necessarily provide work incentives to individuals homogeneously because labor supply decisions were made jointly within households.

1.3.2 Hours Worked

In theory, EITC will decrease hours worked. However, both experimental and non-experimental studies show little evidence that confirms the prediction. For example, Card and Robins (1996) and Miller et al. (2018) find that EITC increases hours worked while Gennetian and Miller (2000) and Gennetian et al. (2005) find the opposite. In addition to experiments with direct intervention (program participation), Chetty and Saez (2013) implement an indirect intervention (information) to identify the effects of EITC. The result shows that providing information about tax incentives through tax preparers does not systematically affect earnings.

Similarly, most non-experimental studies also find no effect on hours worked (Eissa and Liebman, 1996; Meyer and Rosenbaum, 2000; Cancian and Levinson, 2005; Rothstein, 2005) or mixed results (Meyer and Rosenbaum, 2001). Eissa and Liebman (1996) use the Difference-in-Differences approach and examine how EITC affects the hours worked. They find a significant positive effect, but the size is very small. When they focus on less-educated single mothers, there is no effect at all. Meyer and Rosenbaum (2000) find both positive and negative effects on hours worked, but they are not significant. Rothstein (2005) finds no significant difference when comparing single mothers with children and single mothers without children across different income levels. One of the main challenges here is that estimating the hours worked response of workers to the EITC budget constraint is fundamentally a harder empirical problem (Eissa and Hoynes, 2006).

Why has labor participation increased while the hours worked decreased? Eissa and Hoynes (2006) reviewed four possible explanations. First of all, women's labor supply could be inelastic, which means the increase in hours, if any, was too small to be significant. Consequently, the increase in labor supply was largely reflected by changes in employment status. Secondly, workers' decisions on working are not continuous. Therefore, it could be difficult for them to make adjustments. Thirdly, self-reported hours could generate measurement errors. Because the annual labor supply is calculated by timing weeks worked per year and hours worked per week, attempting to average the answer could make measurement errors more than the ordinary recall error. Lastly, taxpayers might not be familiar with the EITC schemes, which could explain the inconsistency between the theoretical prediction and actual behavior. Meyer (2010) thinks the lack of understanding of EITC schemes is the main reason why hours worked has decreased. As he put it, the recent EITC instructions are as long as 14 pages. Thus, only a few people would spend time and finish reading the whole tax document.

1.3.3 Earning

Without considering the changes in other policies and welfare programs, theoretically, taxpayers who fall in the phase-out stage will reduce their income[5]. However, there are only a few studies that focus on the impact on earnings. The results are somewhat mixed, too. Some studies find positive impacts on earning (LaLumia, 2009; Rothstein, 2005; Chetty et al., 2013; Hoynes and Patel, 2018), other studies find either no effect (Cancian and Levinson, 2005) or even strong negative effect (Leigh, 2004). For example, Leigh (2004) and Rothstein (2005) have examined EITC's impact on pre-tax income. Leigh utilizes the variation of EITC schemes across different states and finds that EITC has a very strong negative effect on earning. However, Rothstein studied the EITC expansion in 1993 and find that earning and labor participation have increased simultaneously among low-skilled female workers. This result is later confirmed by one of the recent studies by Hoynes and Patel (2018). Hoynes and Patel compare the states with different EITC

[5] A phase-out stage is a range of income in which the amount of credits decreases as the earning increases. Similarly, a phase-in stage is a range of income in which the amount increases with the earning.

generosities and find that EITC has increased income for population ranged from 75% to 150% of the poverty line. There are reasons that can explain these mixed results. For instance, EITC might have affected taxing behaviors differently across groups. Joulfaian and Rider (1998) use tax auditing data in 1998 and examine the impact of EITC on tax evasion. Results show that the employers in the phase-out range tend to under-report the income, but only a very little. There was no under-reporting on the employee side. No over-reporting was found in the phase-in stage, neither. The results are reasonable in the sense that there is a little room for the employers and employees with formal contracts to manipulate their earnings, compared with the self-employed. LaLumia (2009) uses tax return data and examine EITC's impact on income among self-employed taxpayers. She uses the Difference-in-Differences approach and compares taxpayers with children and taxpayers without children. The result shows that the reported earning has increased by 3.2% to 4.1% among the group that has the lowest income. This implies that the change in taxing behavior was more prevalent among self-employed populations.

1.3.4 Consumption

Researchers are also curious about how EITC changes consumption. Although there are relatively fewer studies that focus on consumption due to data limitations, the findings are consistent. Researchers have found that households with EITC spend more on both durable (Barrow and McGranahan, 2000; Goodman-Bacon and McGranahan, 2008) and non-durable (Goodman-Bacon and McGranahan, 2008; McGranahan and Schanzenbach, 2013) goods. For durable goods, Barrow and McGranahan (2000) estimate and compare expenditure patterns between households with different eligibility using Consumer Expenditure Survey (CES) data. They find households spent more on total expenditures, durable goods, and big-ticket items. Goodman-Bacon and McGranahan (2008) use the same data and directly compare the spending between households with different eligibility. The results show that households that are eligible for EITC spend more on durables than non-durables compared with ineligible households. In particular, households with EITC eligibility are more likely to purchase vehicles. For nondurable goods, McGranahan and Schanzenbach (2013) exploits the fact that EITC benefits are not paid out evenly across the calendar year to investigate

its effect on food expenditure patterns of households. They find that households spend relatively more on healthy food such as fresh fruit and vegetables. These findings are consistent with the descriptive analysis (Smeeding et al., 2000; Linnenbrink et al., 2006; Mammen and Lawrence, 2006). At last, Barrow and McGranahan (2000)'s results suggest that households tried to smooth their expenditures after receiving EITC benefits.

2. WUTONGQIAO EXPERIMENT

Wutongqiao Experiment was conducted by China Household Finance and Survey (CHFS) between 2014 and 2017. 259 households were sanctioned by the local government for the experiment. The households were later randomly assigned to the treatment group and the control group. All the households in the treatment group enrolled in an EITC-type program-Labor Income Reward Plan. Upon enrollment, households started receiving cash rewards every month. The amount of reward depends on household earning in the last month. The offered EITC contains three stages, including a phase-in, a plateau, and a phase-out. The amount of reward will first increase with the earning (40%-50% phase-in rate). Then, it reaches the maximum (160 RMB or 200 RMB per capita depending on the number of children) and no longer changes with the earning (plateau). Eventually, the reward will start decreasing as households earn more and move into the phase-out stage (40%-50% phase-out rate). Once the earning exceeds the break-even point (1,000 RMB per capita), the household becomes ineligible for the program automatically. Enrolled households are required to report their earnings and employment status every month to get the rewards. Each claim needs supporting materials such as proof of employment and income (see Appendix A.5). Household surveys were conducted periodically among households in both the treatment and the control group to track changes such as employment, labor supply, earning, consumption, and self-reported health conditions. Finally, I compare outcomes between the two groups to examine the impacts of the EITC program.

The design has two important and unique features that are worth noting. First, it implements a relatively strong treatment intensity (average reward is 20% of the monthly income) compared to the existing studies (3%-7% of the income). Second, it employs a comprehensive measure to audit and validate each claim. Given that China has a large portion of self-employed jobs, this validation process is particularly crucial for the experiment since it is relatively easier for the self-employed to manipulate earning compared to the employed with formal contracts.

Results show that Labor Income Reward Plan increases labor supply significantly on both

extensive margin and intensive margin. Estimates show that the number of earners per household has increased by 0.34 person (14% increase), and total hours worked has increased by 81 hours per month (40%). Consequently, the monthly total earning (without the benefit) has increased by 569 RMB (82 USD), and the total expenditure has increased by 436 RMB (63 USD). The size of the effects seems to be a lot larger than the existing literature, which is partially due to the strong treatment intensity.

2.1 Experiment Design

Wutongqiao[1] Experiment is conducted by China Households Finance Survey (CHFS) at Southwestern University of Finance and Economics, China. The project started in May 2014. Households in the samples were randomly offered with Labor Income Reward Plan. The program will allow households to receive a cash reward every month that depends on their earnings of last month. The scheme is incentive-compatible and similar to the EITC in the United States, and it is expected to provide work incentives. On average, enrolled households received a 350 RMB (50 USD) cash reward per month. The amount is roughly equivalent to 20% of households' monthly income. It is the first-ever EITC field experiment in a developing country.

2.1.1 Treatment

The scheme of the Labor Income Reward Plan is similar to EITC in the United States. To start with, when a household has zero earning, the initial reward is zero. The reward will increase with earning until it reaches a maximum level. Then, the reward remains at the maximum level regardless of the changes in earning (plateau). Later, the reward begins to decrease when the earning is higher than a certain threshold (phase-out stage). The reward eventually diminishes to zero as the earning continues to grow and passes the break-even point, at which point the household is no longer eligible for the program. Namely, the reward will be a 50% of the earnings for households with less one child, and a 40% for those with two or more children for the first 400 RMB (80 USD)

[1]Wutongqiao is a district in Leshan City in Sichuan Province with an area roughly the same as San Antonio. It is a major chemical industry area in Leshan City. Wutongqiao has around 320,000 population with a GDP per capita of 3,750 USD in 2010. In 2015, around 1.3% of its population lived under the national poverty-line (Wutongqiao People's Government, 2013).

earning per capita. The reward is 200 RMB per capita for households with less than one child and 160 RMB per capita for households with two or more children when the earning per capita is between 400 RMB to 600 RMB. Then, the reward will start to decrease as earning increases. The phase-out rate is 50% for households with less than one child and 40% for households with two or more children. The reward will become zero once the earning reaches 1,000 RMB regardless of the number of children. Figure 2.1 demonstrates how the reward per capita changes at different stages.

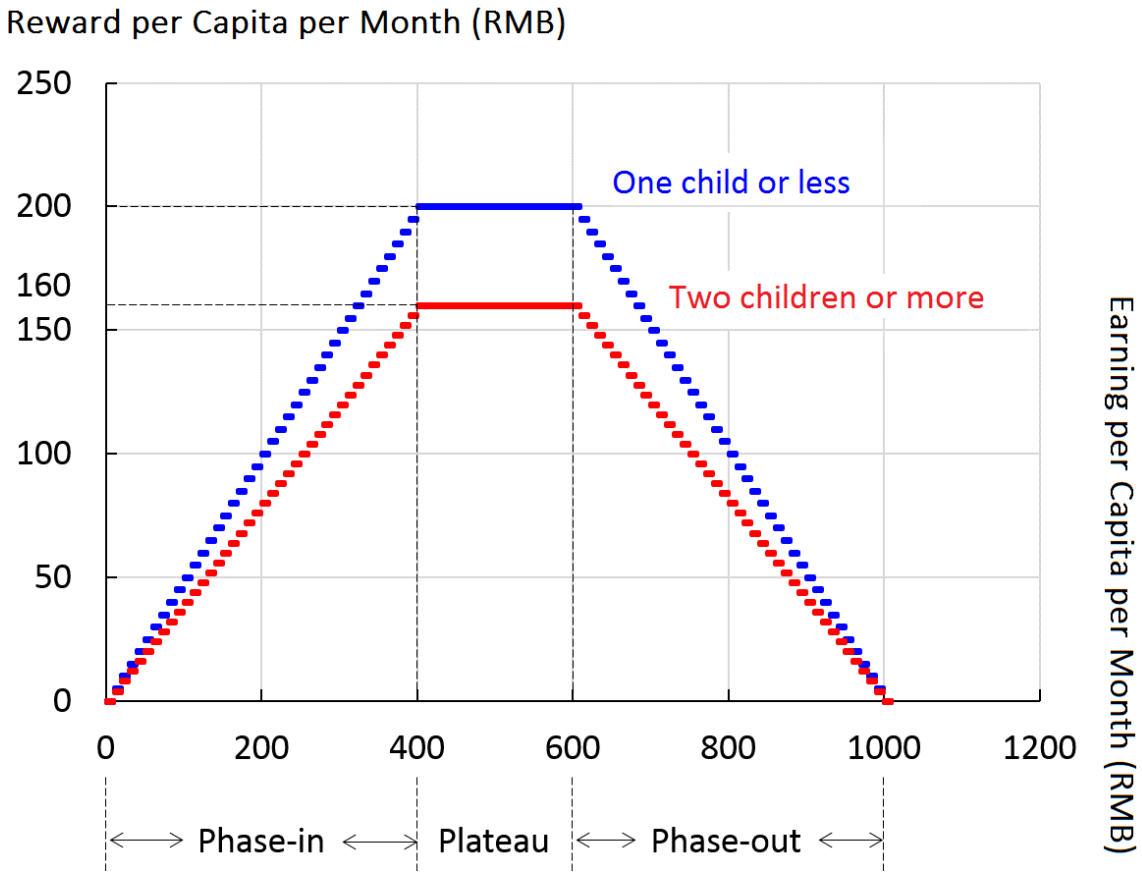

Figure 2.1: Labor Income Reward Plan 2016 (Wutongqiao, The Experiment)

Although the general setup is similar to EITC in the US, there is an important difference between the Labor Income Reward Plan and the US schemes. Specifically, after reporting household earning each month, a per capita earning will be calculated by dividing the total earning by the family size. Only spouse, parents, dependents, and members that have lived and shared expenditure together will be considered as family members. Any senior member who receives a pension more than 1,000 RMB per month will not be considered as a household member in spite of eligibility. Then, the earning per capita is applied to Figure 2.1 to find out the amount of the reward per capita for this particular household. The final amount of the reward will be the product of the reward per capita and the family size. For example, assume a nuclear family (a couple with one child) has no other source of income other than the 1,500 RMB earning from the father's employment last month. Then, the per capita earning of this household is $1500 \div 3 = 500$ RMB per capita. According to Figure 2.1, the household should be rewarded with 200 RMB per family member. Finally, the total amount of reward is $200 \times 3 = 600$ RMB. It is important to point out that having more children not only reduces the per capita earning but also grants the family an additional share of reward directly.

2.1.2 Timeline

The project started with a baseline survey among 808 households that reside in Wutongqiao District. The sample was selected by a three-stage stratified random sampling method. The main purpose of the baseline survey is to collect the basic socioeconomic status of the local population. More importantly, this step is crucial for the project team to estimate the income eligibility for the program and make sure the pending expenditure is feasible for the limited budget. Then, 65 households were selected for the pilot in December 2014. The experiment came ten months after the pilot in October 2015. The sample for the experiment contains 194 households that reside in Wutongqiao District. Finally, 1,362 households were added to the sample in March 2017. There is no sample overlap between the pilot, the experiment, and the expansion. The EITC programs that were offered are largely similar across different phases. Before March 2017, the experiment was funded by donations, after which the local government took it over and expended the program to all

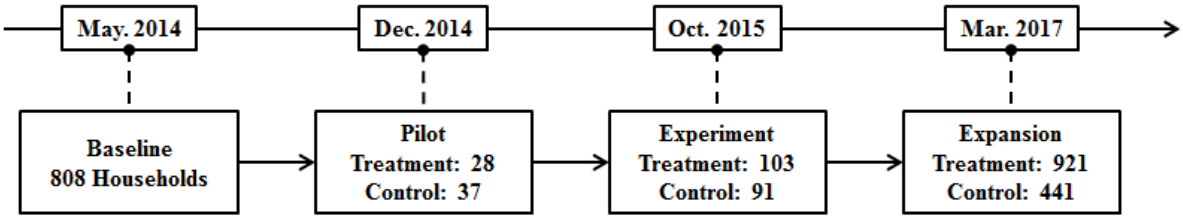

Figure 2.2: Wutongqiao Experiment Timeline (2014-2017)

registered poor households in their entire jurisdiction. Therefore, this study will be only focusing on the pilot and the experiment and only use survey data collected prior to 2017. Figure 2.2 describes the four major phases of Wutongqiao Experiment.

2.2 The Pilot

The purpose of the pilot is to figure out all policy parameters that will fit the budget, evaluate the feasibility of the design, and anticipate potential problems during the implementation to reduce the uncertainty of the experiment. Generally, it takes three steps to select the sample for the pilot.

STEP 1: A baseline survey was performed in May 2014. By using a three-stage stratified random sampling method, the study sampled 808 households (476 from urban areas and 332 from rural areas) from 16 communities across 4 (out of 13) towns in the Wutongqiao Districts. The survey collects a variety of information such as demographics, employment status, assets, child education, social security, income, and expenditure, etc.

STEP 2: The project team calculated the earning per capita for each household using the survey data. Households with monthly earnings more than 1,200 RMB per capita were dropped[2]. By controlling age, marriage, gender, income, social security, and household register, the research team predicted the probability of employment for each individual in unemployed households. Then, households were ranked by the probability of employment from the highest to the lowest. The first 130 households were chosen as the pre-screened pool. To guarantee the accuracy and credibility of

[2]This cut-off was chosen as the average income per person in the sample

the information, the survey team re-visited these 130 households in October. The team also asked the households if they are willing to take part in the experiment.

STEP 3: Based on their eligibility and their willingness to participate, the team eventually selected 65 households and randomly assigned them into the treatment group and the control group. The treatment group will enroll in the Labor Income Reward Plan. The control group was not offered any form of reward. Other welfare programs remain the same for households in both of the groups.

Pilot treatment: The scheme in the pilot has parameters slightly different from the scheme in the experiment. First, the Labor Income Reward Plan that was offered in the pilot does not distinguish the number of children in the households. Second, the maximum amount of reward is 250 RMB instead of 200 RMB in the experiment. Both the phase-in rate and the phase-out rate are 50%. Therefore, the reward per capita reaches its maximum when the earning per capita reaches 500 RMB and start declining when it reaches 700 RMB. The break-even point is 1,200 RMB instead of 1,000 RMB as it is in the experiment. Figure A.18 describes the scheme that was used in the pilot. Finally, the pilot treatment started in January 2015.

2.2.1 Data for the Pilot

Table 2.1 presents the data used in the pilot analysis[3]. The data is collected from the household surveys that were conducted before and after the intervention started. The first round of the survey was conducted upon enrollment in December 2014. Another survey was conducted four months later in March 2015. Table A.4 presents the balance of covariates between the treatment and the control groups in the pilot sample. I find no statistical difference between the two groups in terms of demographics, labor supply, earning, and expenditure. The randomization is successful.

2.2.2 Results from the Pilot

By nature of the incentive-compatible design, the EITC program should provide work incentives. Therefore, it is expected to increase labor supply. It is uncertain how earning would be

[3]There are some households that were not observed in the data due to their absences of the survey.

Table 2.1: Data Used in the Pilot

# of Households	Treatment	Control
Dec. 2014 (pre)	27	32
Mar. 2015 (post)	21	32

affected. On the one hand, wage income may increase with employment. On the other hand, wage income only captures the changes among the employed. Households may prefer other methods to increase their earnings, such as farming and business. Moreover, even if a household increased working hours immediately, there is no guarantee that the earning will rise. For formal employees, it is hard to adjust work time. For self-employed individuals, a shop keeper may extend the business hour immediately, but the revenue may not increase because there were no more customers. In the following sections, I will first discuss the program's effects on labor supply (on both extensive margin and intensive margin). Then, I will discuss its effects on household earning.

The effects on labor supply are examined on both extensive and intensive margins. The extensive margin of household labor supply is measured by the number of earners in the household during the last month. The intensive margin of household labor supply is measured by the total hours worked for the primary job and the secondary job during the last month. The hours worked are aggregated to the household-level so that it measures the sum of the hours across each family member in the household. Figure 2.3 presents the treatment effects on household labor supply estimated by OLS estimators. The result indicates that the Labor Income Reward Plan has positive and significant impacts on the labor supply. Specifically, Labor Income Reward Plan increases the number of earners per household by 0.336 and increases total hours worked for the primary job by 105.1 hours per month. There is a positive but small effect (1.9 hours) on hours worked for the secondary job. However, it is not statistically significant.

Household earning is measured by the earnings from the primary and the secondary job, profits from farming as well as family-owned small businesses during the last month. Similar to total

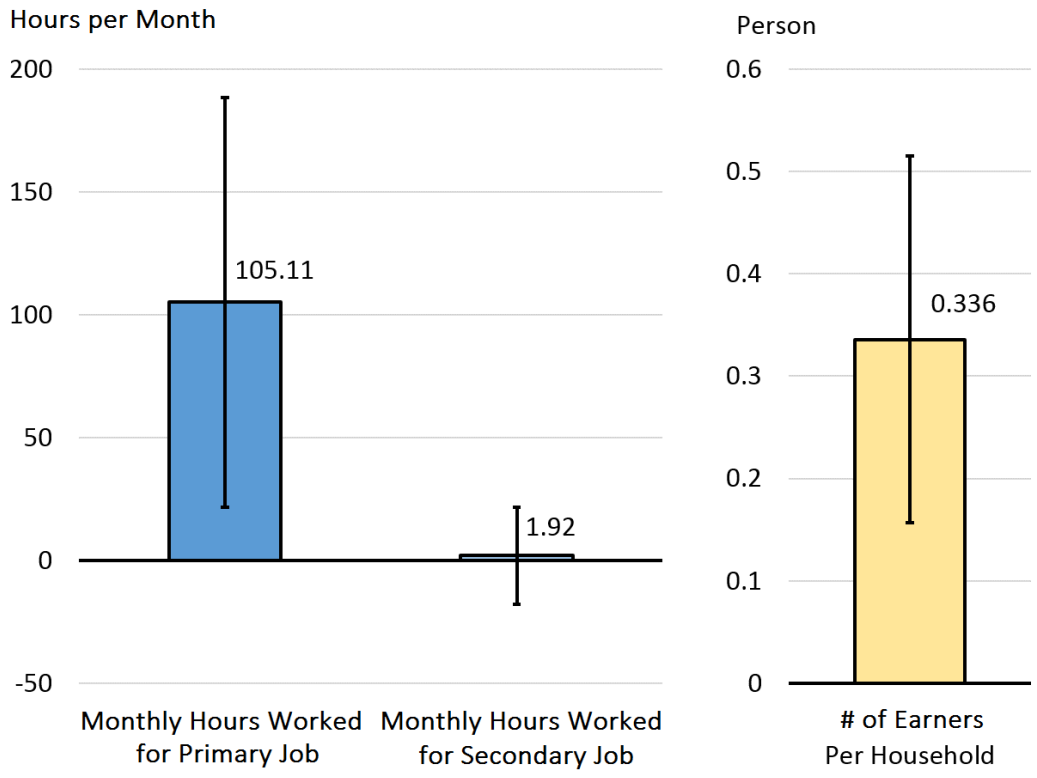

Figure 2.3: Treatment Effects on Household Labor Supply (The Pilot)

hours worked, they are aggregated to the household-level so that they measure the sums of the earnings across each family member in the household. Figure 2.4 presents the treatment effects on household earning estimated by OLS estimators. The result shows that the Labor Income Reward Plan has a positive and significant impact on earnings from the secondary job. Specifically, Labor Income Reward Plan has increased the earning by 66.6 RMB per month. There is a positive and larger effect on earning from the primary job. However, it is not statistically significant. There is no effect on household earning in terms of farming profit and business profit. The estimates are very small and insignificant.

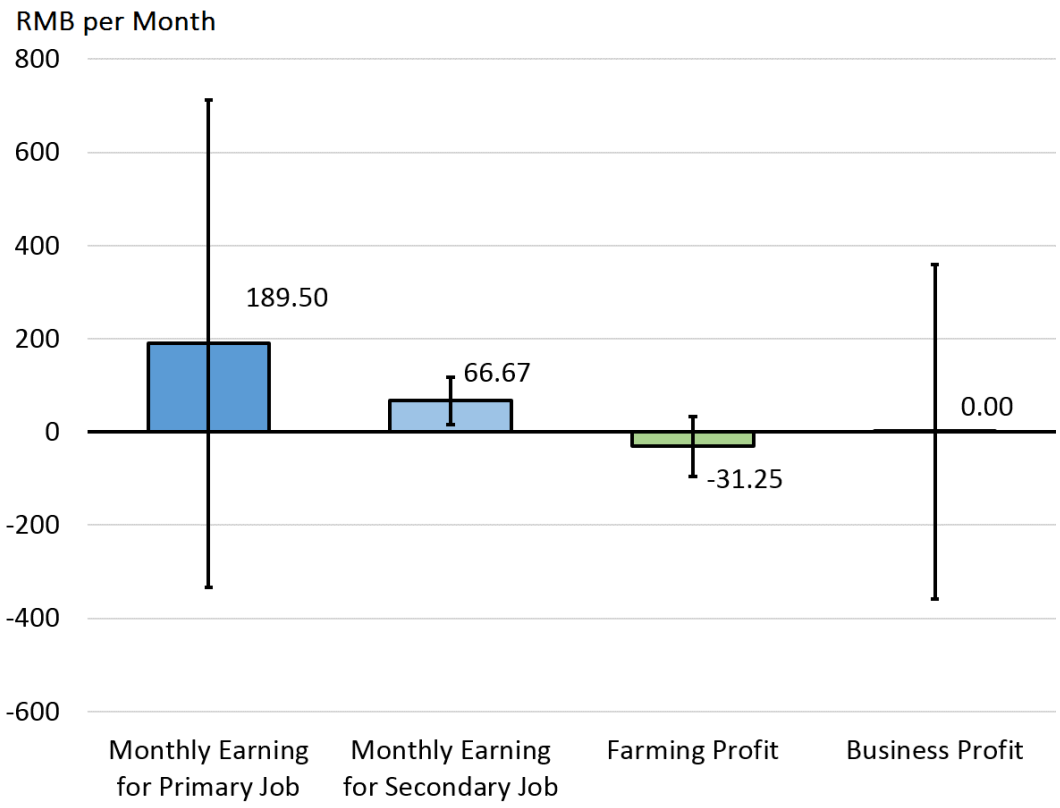

Figure 2.4: Treatment Effects on Household Earning (The Pilot)

2.3 The Experiment

The experiment started in October 2015, after the pilot was completed. It also involves three steps to select the sample. However, the procedure is completely different. Moreover, there is no overlap between the pilot sample and the experiment sample.

STEP 1: Unlike the pilot sampling method, the households in the experiment sample are referred by the local community committees[4]. Then, a baseline survey that is similar to the pilot was completed among the referred households.

STEP 2: Based on the baseline survey, the research team computed the per capita income for each household. Ineligible households were dropped. This includes households that have earning

[4]Households were referred based on committees' knowledge about the economic conditions of these families and their probabilities to get employed.

per capita higher than 1,000 RMB, households that lost their abilities to work, and households that do not wish to get employed.

STEP 3: Eligible households are ranked according to earning per capita from the highest to the lowest. Households were split into odd-rank and even-rank group. Finally, the even-rank group was randomly selected to be the treatment group.

Experiment treatment: To reiterate, the reward will be a 50% of the earnings for households with less one child, and a 40% for households with two or more children for the first 400 RMB (80 USD) earning per capita. The reward is 200 RMB per capita for households with less than one child and 160 RMB per capita for households with two or more children when the earning per capita is between 400 RMB to 600 RMB. Then, the reward will start to decrease as earning increases. The phase-out rate is 50% for households with less than one child and 40% for households with two or more children. At this rate, the reward will decrease to zero once the earning reaches 1,000 RMB per capita regardless of the number of children.

2.3.1 Data for the Experiment

Table 2.2 presents the data used in the experiment analysis[5]. The data is also collected from the household surveys that were conducted before and after the intervention started. The first round of the survey was conducted upon enrollment in November 2015. The following surveys were conducted four months later in March 2016. I compared demographics, labor supply, earning, and expenditures between households who always respond to the surveys and households who missed at least one survey. I do not find any statistical difference between the twos (Table A.5).

Given the findings from the pilot, one should expect to see similar effects on labor supply and earnings in the experiment. Figure A.1 shows the distribution of total hours worked among the treatment and the control group. The hollow columns represent the frequency of the observations in the pre-intervention period. The solid columns represent the frequency of the observations in the post-intervention period. By visual comparison, the distribution of the treatment group

[5]Similar to the pilot, there are some households that were not observed in the data due to their absences of the surveys.

Table 2.2: Data Used in the Experiment

# of Households	Treatment	Control
Nov. 2015 (pre)	103	91
Mar. 2016 (post)	94	76
May. 2016 (post)	82	65
Sep. 2016 (post)	79	63
Nov. 2016 (post)	77	54
Jan. 2017 (post)	78	61

has skewed to the right after the intervention, while the control group remained relatively the same. This indicates that there exists a potential treatment effect on hours worked. Figure A.2 shows the distribution of household earning among the treatment and the control group. Similar to Figure A.1, the distribution of the treatment group skewed to the right after the intervention. Its counterpart remained relatively the same. This indicates that there exists a potential treatment effect on household earning, too. At last, Figure A.3 shows the distribution of household total expenditures among the treatment and the control group. There is no notable difference between the treatment and the control group.

2.3.2 Methodology

The treatment assignment in the experiment was originally random. However, around 20% of the sample was switched at the request of the local government. The idea is to benefit as many poor households as possible. Consequently, the treatment group is relatively poorer (Table A.6).

Since the treatment assignment in the sample is only random prior to the switching, it is likely that OLS estimates would be biased due to non-random switching. However, I can still recover the intention-to-treat effect by using the initial assignment as an instrument. Namely, I aggregate the data to the household-level and use the initial assignment to instrument for the actual treatment status. Meanwhile, I am taking advantage of the panel data and controlling for household fixed effect as well as time fixed effect. The coefficients are to be estimated by the regressions below:

$$\text{First-stage:} \quad T_{i,t} = \alpha_i + \lambda_t + \beta D_{i,t} + \epsilon_{i,t} \tag{2.1}$$

$$\text{Reduced form:} \quad Y_{i,t} = \alpha_i + \lambda_t + \delta D_{i,t} + \xi_{i,t} \tag{2.2}$$

where $T_{i,t}$ is actual treatment status for household i at time t. $D_{i,t}$ is the initial treatment status for household i at time t. α_i is household fixed effect. λ_t is time fixed effect. $Y_{i,t}$ is outcome variable. $\epsilon_{i,t}$ and $\xi_{i,t}$ are the error terms.

For the instrument to work, the relevance assumption (the instruments predict the endogenous variables well) and the exclusion restriction (the instruments only affect the outcome variables through the instrumented variables) must be satisfied. The relevance assumption can be tested during the first-stage regression. I show that the relevance assumption is satisfied by a strong first-stage (Table A.8). For the exclusion restriction, I show that the randomization for the initial assignment is successful (Table A.7). Therefore, the initial assignment only affects the outcome variables via the actual treatment status.

Later, I allow the treatment effects to vary across time periods to recover dynamic treatment effects. However, this will also introduce additional endogenous variables, which means that the first-stage is going to be different. Namely, in the new first stage, $\beta D_{i,t}$ is replaced with $\sum_{t=1}^{T} \beta_t Treatment_{0,i} \times I_t(time = t)$, and $T_{i,t}$ is replaced with $Treatment_i \times I_t(time = t)$. $Treatment_i$ is a dummy variable that equals one if household i is actually assigned to the treatment group, and $Treatment_{0,i}$ (instrument) is a dummy variable that equals one if household i is initially assigned to the treatment group. $I_t(time = t)$ is a dummy variable that equals one if the data is observed in time t. There are five regressions in the first-stage estimation. I dropped the first period to avoid perfect multicollinearity problem and show that the relevance assumption is satisfied with a strong first-stage (Table A.9).

2.3.3 Results from the Experiment

Although the sample and identification strategies are different, one should expect that the results from the experiment to be similar to the pilot. The results in the pilot show that the Labor Income Reward Plan increases the number of earners in the household, hours worked for the primary job, and earnings for the secondary job. In the following section, I will discuss the impacts of the Labor Income Reward Plan on labor supply, earning, and expenditure.

2.3.3.1 Impacts on Household Labor Supply

Figure 2.5 shows the treatment effects on household labor supply. The result indicates that the Labor Income Reward Plan has positive and significant impacts on the labor supply. Specifically, Labor Income Reward Plan increases the number of earners per household by 0.329 and increases hours worked for the primary job by 86.0 hours. There is a negative but small effect (5.3 hours) on hours worked for the secondary job. However, it is not statistically significant. I consider these results are similar to the pilot.

Figure A.4 shows the dynamic treatment effects on the number of earners per household. The baseline period (Nov-15, pre) is dropped to avoid perfect multicollinearity problem. The result indicates that the effects are persistent throughout time periods. The magnitude of the effect ranges from 0.269 to 0.451. Not surprisingly, I find similar persistence in dynamic treatment effects on total hours worked (Figure A.5). Moreover, there are increases in all dimensions (weeks worked per month, days worked per week, and hours worked per day) in terms of labor supply for the primary job implying the increase in labor supply is primarily driven by the increase in the number of earners per household (Table A.16).

2.3.3.2 Impacts on Household Earning

Figure 2.6 presents the treatment effects on household earning. The result shows that the Labor Income Reward Plan has a positive and significant impact on earning from the primary job. Specifically, Labor Income Reward Plan increases the earning from the primary job by 560 RMB per month. The effects on households monthly earning for the secondary job is negative

Figure 2.5: Treatment Effects on Household Labor Supply

but relatively small (14 RMB), and the estimate is statistically insignificant. As for profits from farming and business, the effects are small and positive but statistically insignificant. This result is consistent with the findings in household labor supply since the increase in hours worked for the primary job is both large and statistically significant while the increase in hours worked for the secondary job is small and insignificant.

Figure A.6 shows the dynamic treatment effects on household earning from the primary job. The magnitude of the effect ranges from 250 RMB to 840 RMB. The dynamic treatment effects are persistent throughout times except for September, making the effects less persistent than labor supply. I can think of three potential explanations. First, compared to measuring hours worked, the measurement error issue is often more complicated when measuring earnings. Second, the statistical power might not be large enough to produce significance in the panel analysis. Finally,

earning from self-employment jobs might not be paid regularly throughout the year. It is possible that a part of the effect is conditioned on seasonality.

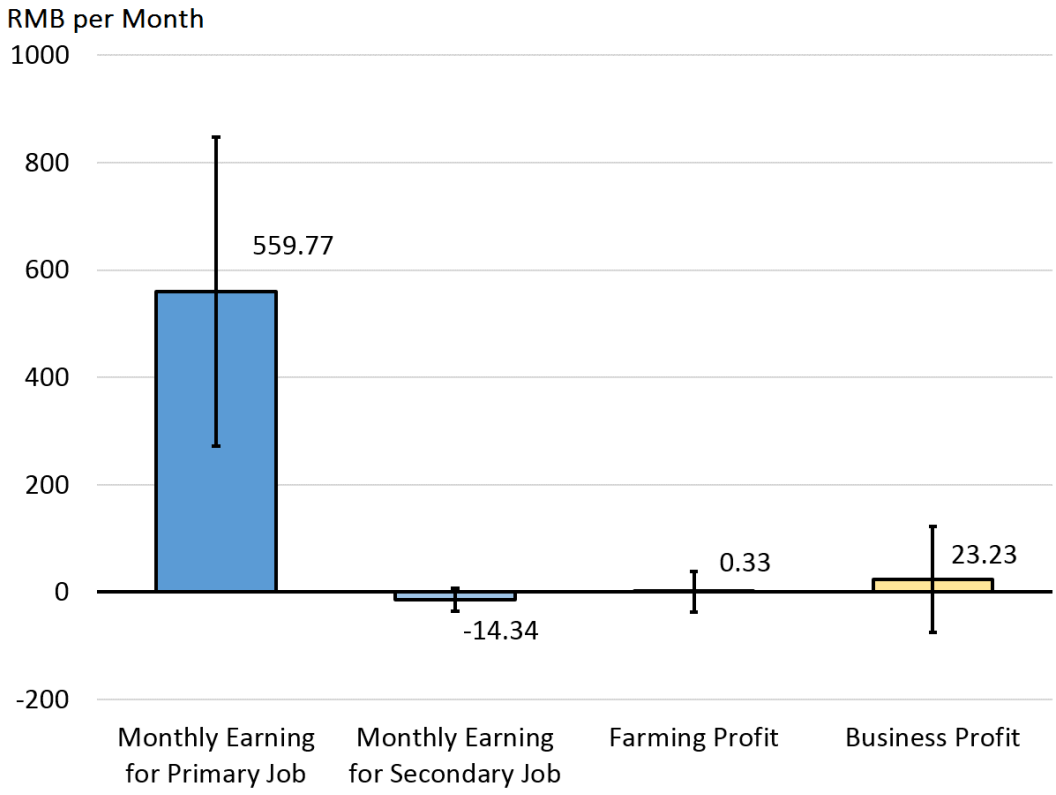

Figure 2.6: Treatment Effects on Household Earning

2.3.3.3 Impacts on Household Expenditure

Figure 2.7 presents the treatment effects on household expenditure. The result indicates that households in the treatment group have significantly increased the expenditure on food (159 RMB) and education (226 RMB). The expenditure on transportation has also increased, but the estimate is statistically insignificant. This insignificant effect might be partially driven by work-related transportation costs such as fuel for rickshaw taxis, which is quite common among self-employed workers in the local area. However, there is available data to support such an argument. I find

Labor Income Reward Plan has small and positive but insignificant effects on expenditures on eating-out, utilities, sundries, communication, and clothing.

Figure A.10 shows the dynamic treatment effects on food expenditure. Since it is unlikely that I have enough power to yield significance, I will focus on the magnitude only. The pattern suggests that the size of the effect is potentially related to seasonality. I consider this reasonable since households tend to rely more on their own supplies after the harvesting season started.

Figure A.11 shows the dynamic treatment effect on education expenditure. There was a relatively larger effect in September 2016 when the fall semester began. The second-largest effect was in March 2016 when the spring semester began. The rest of the effects are small and statistically insignificant. By the look of the timing, it seems that the increases in education expenditure are semester-related such as tuition. If this increase in education expenditure is tuition, I would like to verify that these effects are re-occurring. However, the data is not long enough for me to check if this is true. It might be the case in which the treatment group happens to have more kids in colleges. I show that the treatment group has more individuals that fall in the college-age group (18-22) (Table A.22). For this reason, I divide the sample into the "college group" and the "non-college group" and perform the same regression. Figure A.12 shows that it is "the college group" that was driving the treatment effects with the exact same pattern of occurrences while there is no effect in the other group. This means the treatment effect is not driven by the fact that the treatment group has more college students.

Since it is obvious that a large part of the effect is driven by the education expenditure (Figure A.7), I excluded the education expenditure and performed the same regression again. Figure A.8 shows the dynamic treatment effects on household total expenditure without education. The effects are less persistent throughout time, but the overall effect is still positive and significant (Figure A.9).

2.3.3.4 Impacts on Health

Since households have spent more on food, I am also curious about whether the nutrition status has improved. Unfortunately, I do not have direct measures of any nutrition intake, but I do have

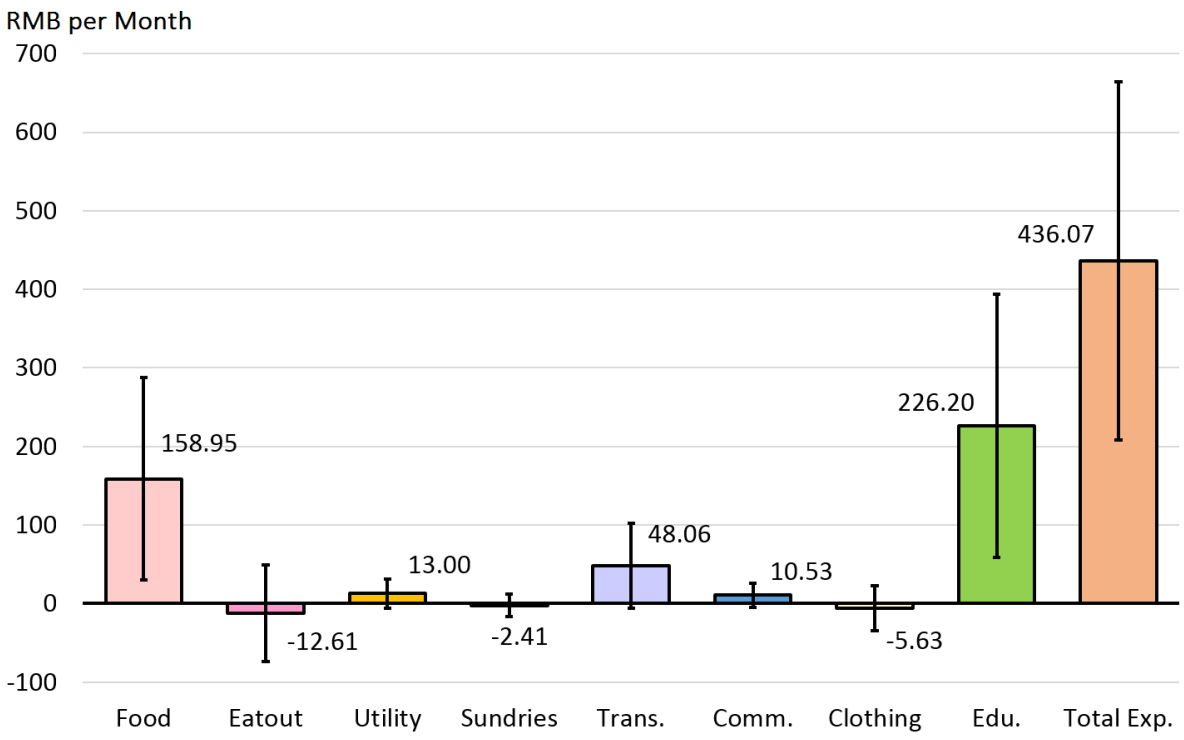

Figure 2.7: Treatment Effects on Household Expenditure

some measures on health outcomes from the household surveys. Since health outcomes cannot be aggregated to the household-level, I use similar specifications but with individual-level data and replace household fixed effect with individual fixed effect. I show that first-stage regressions using the individual-level data satisfy the relevance assumption (Table A.18 and A.19). Figure 2.8 shows the treatment effect on health outcomes. Since health outcomes are measured in occurrences of symptoms, which are negative outcomes, a negative coefficient indicates a relief of the symptom, while a positive one indicates a deterioration. The result shows that the number of claims of fever and heart palpitation has significantly decreased among households in the treatment group. The claims of discomfort and the claims of cough have decreased while the number of claims of diarrhea has increased. However, the estimates are small and statistically insignificant. Although there are medical studies that back up the reliefs in fever (Wu et al., 2004; Victora et al., 2008) and heart palpitation (Webb et al., 1986; Amare et al., 2015; Sze et al., 2018), I do not want to

over-interpret these results since the data does not observe the nutrition status directly. In addition, these effects might not be from the increases in food intake alone but also other aspects such as labor participation. For example, some of the labor-intensive jobs can also be seen as physical exercises. These physical exercises can potentially strengthen the immune system and reduce the chance of fever as well. Figure A.13 and A.14 show the dynamic treatment effects on the claims of fever and heart palpitation. The pattern of the effects on heart palpitation seems to be seasonal. A potential explanation is that heart palpitation is one of the common allergic reactions, and allergic symptoms are usually seasonal.

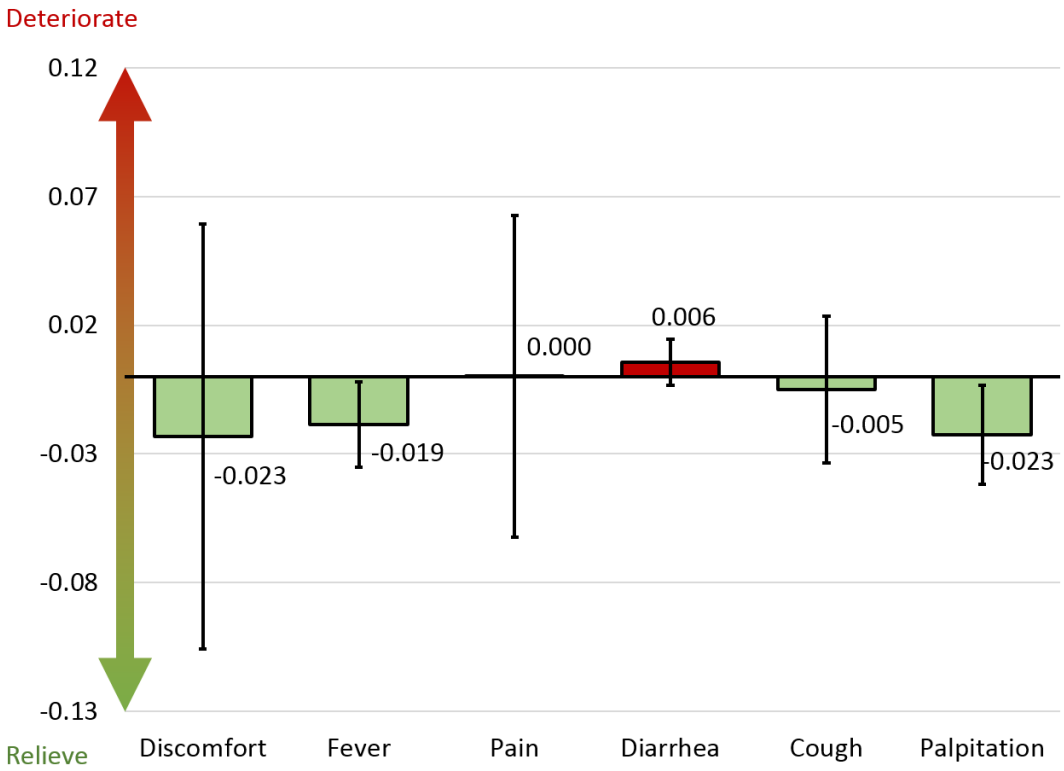

Figure 2.8: Treatment Effects on Health

2.3.3.5 Impacts on Employment Type

There is another thing I am particularly interested in, which is the effects on employment type. In the US EITC literature, researchers find no effect or mixed results. One explanation is that workers are unable to choose continuous hours worked (Eissa and Hoynes, 2006). I find that the average earning from the employed jobs (1,357 RMB per month) is twice as high as self-employed ones (646 RMB per month). Thus, for those who are already working, a better way to increase earning (instead of increase hours) is to switch to a better-paid employment type. Therefore, I expect to see differences in terms of switching behavior between the treatment and the control group.

Figure A.15 to A.17 show the percentages of individuals in the treatment/control group with a certain type of switching behavior[6]. For example, the first two columns in Figure A.15 show that, for people who were previously unemployed before the treatment, 82.0% of the individuals in the treatment group remained unemployed after the intervention while 85.5% individuals in the control group have done so. In other words, for those who did not have a job prior to the intervention, compared with the control group, fewer people in the treatment group remained unemployed. Slightly more people in the treatment group became either self-employed or employed after the intervention. For those who were self-employed prior to the intervention, compared with the control group, fewer individuals in the treatment group became unemployed after the intervention. More people in the treatment group remained either self-employed or became employed after the intervention (Figure A.16). For those who were employed prior to the intervention, compared with the control group, fewer people in the treatment group became unemployed. More people in the treatment group became self-employed, and fewer people in the treatment group remained employed (Figure A.17). The three figures depict a picture in which previously unemployed individuals in the treatment group are only doing slightly better than their counterparts in the control

[6]It is tricky to present the switching since there are multiple panels, and an individual can switch for multiple times. For this reason, I only consider the first two panels in the data to make things easier. I use the number of individuals who did a certain type of switching as the numerator and use the total number of able-bodied persons as the denominator to adjust for the compositional differences between the treatment and the control group.

group. Previously self-employed individuals in the treatment group are doing a lot better in staying self-employed and switching to employed jobs. When previously employed individuals change their current work type, more individuals in the treatment group preferred self-employment rather than exiting the labor force.

Figure 2.9 shows the treatment effect on employment type. I use individual-level data for this regression because employment types cannot be aggregated. Here, the dependent variables are dummies that indicates one of the tree employment types (unemployed, employed, and self-employed) for individual i at time t. The result shows that the Labor Income Reward Plan significantly decreases the probability of unemployed. I find positive but insignificant effects on the probabilities of employed and self-employed.

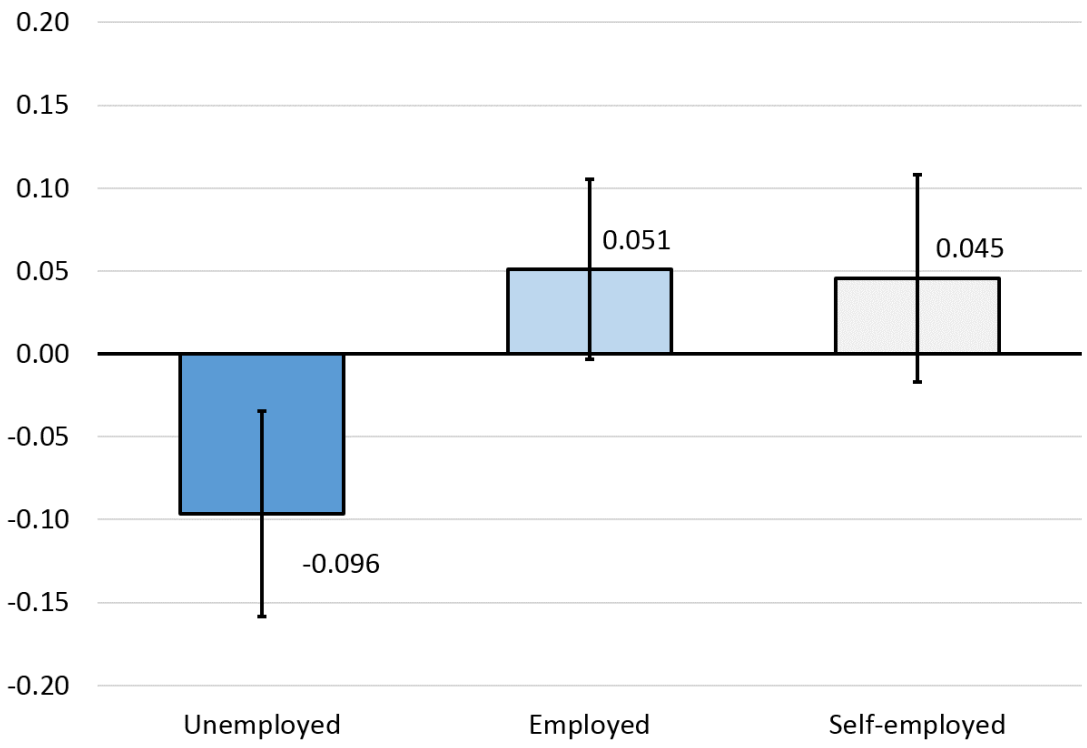

Figure 2.9: Treatment Effects on Employment Type

2.3.4 Why There Are So Many Self-employed Lost Their Jobs?

It is notable that 40% of the individual in the treatment group have lost their jobs after the intervention (Figure A.16). I argue this phenomenon is largely due to the labor division among the dual-income households[7]. To better understand why, the first column on the left in Figure A.19 shows that dual-income households earned more than single-income households. This makes sense because there are more earners in dual-income households. When comparing earnings from employed jobs, people in single households earned roughly the same as their counterparts in dual-income households. However, when comparing earnings from self-employed jobs, people in dual-income households earned considerably less than those who are in single-income households. Moreover, the data showed that individuals who have self-employed jobs in the two groups worked about the same length of time each month. This indicates that self-employed individuals in two types of households are doing different jobs. To put the pieces together, it looks like that, within dual-income households, there is usually a primary earner who has an employed job and a secondary earner who has a self-employed job. Although there is only scattered information in the data that allows me to have some idea about what exactly these jobs are, the information indicates that they are something such as holding a street stall. These jobs are typically low labor-intensive but could have very long working hours. These jobs are often something people can do while performing housework or taking care of children at the same time. It is possible that it is relatively easier for these secondary earners to quit their jobs occasionally because of the uncertainty besides the fact that there is a primary source of income in the household. However, this is not true anymore when households enrolled in the Labor Income Reward Plan. This explains why treated individuals who were previously employed would rather switch to self-employed jobs instead of becoming unemployed (Figure A.17). If the argument is true, I should be able to see that the effect on labor supply is larger among dual-income households. I show that this is the exact case (Figure A.20).

[7]Households have more than one source of income.

2.3.5 Do Households React to Different Marginal Reward?

By design, the Labor Income Reward Plan has a positive marginal reward in the phase-in stage, a zero marginal reward in the plateau, and a negative marginal reward in the phase-out stage. Theoretically, households would only increase their labor supply when the marginal reward is positive. I divide the households into three groups based on which stage they were prior to the treatment. Figure A.21 shows sub-group estimates for three major outcome variables (employment, earning, and consumption). The results show that it is households on the phase-out stage that are more responsive to the program. This is not surprising since the labor supply decisions are not made based on the Labor Income Reward Plan scheme alone. For example, there might be many other constraints that bind the households from increasing their labor supplies such as children, illness, education, and economic opportunity. I argue that one of the main reasons why phase-out households are more responsive to the program is that most of the dual-income families are located in the phase-out stage (Table A.21). This implies that marginal reward makes little difference in households' decisions compared with the program's ability to prevent self-employed from occasional unemployment.

2.4 Is It Worth Investing Money?

One of the prize-winning questions is whether it is worth investing money in the Labor Income Reward Plan? Unfortunately, I am unable to answer this question using regressions due to the data limitation, but I do know that households involved in the analysis are getting an average reward of 348 RMB per month on average. With the results from the previous sections, I adjust the effects with the cost accordingly. Without considering administration cost, I estimated that each 100 RMB reward per month for a family, would yield 0.1 additional earners, 23 hours worked, 163 RMB of additional earning. More importantly, I calculate that each 100 RMB spent on the Labor Income Reward Plan would increase 125 RMB in consumption. This amount is larger than 107 RMB-the estimated return of a 100 RMB in fixed capital investment (STCN, 2017). There is no way that these calculations to be taken seriously, but it looks promising, especially the effects on earning

and consumption. The evidence supports my argument at the beginning-an incentive-compatible transfer program will help the poor and the consumption at the same time.

2.5 Summary

China currently has no in-work benefits nor social welfare programs such as the Earned Income Tax Credits in the United States. Yet, the literature and global evidence have suggested EITC-type programs' potentials on poverty alleviation and work incentives. More importantly, the performance of current transfer programs is very unsatisfactory. An opportunity arises if China can substantially increase cash transfer. This experiment presents the first-ever EITC field experiment in China with strong treatment intensity (20% of household income) and rigorous validation procedure.

The pilot shows that the Labor Income Reward Plan significantly increases household labor supply on both extensive margin and intensive margin. The number of earners per household has increased by 0.336, and total hours worked has increased by 105.1 hours per month. As for earning, household earning from secondarys job has increased by 66.6 RMB per month.

The experiment finds results that are similar to the pilot. Results show that Labor Income Reward Plan significantly increased the number of earners per household by 0.329, total hours worked for the primary job by 86.0 hours per month. It also significantly increases the household earning by 560 RMB per month. This result is consistent with the findings in household labor supply since the increase in household labor supply for the primary job is also large and statistically significant. The effects are persistent throughout different periods. As for consumption, I find an overall positive effect on total expenditure. Specifically, households in the treatment group increase food expenditures by 160 RMB per month. Results also show that households in the treatment group increase education expenditures by 226 RMB per month. Evidence suggests that these expenditures are college-related.

Finally, I explored the effects on employment type. I find individuals in the control group are more likely to become unemployed compared with the treatment group. The evidence suggests that the Labor Income Reward Plan not only provides work incentives but also prevents households

from quitting jobs. This prevention effect is especially important among dual-income households. The study has important policy implications and contributes to a large literature that studies the effect of in-work benefits.

3. MABIAN/MUCHUAN EXPERIMENT

Mabian/Muchuan Experiment was conducted by China Household Finance and Survey (CHFS) between 2017 and 2018. The experiment was conducted in Mabian county and Leshan county. Both counties are located in Leshan City, Sichuan Province, China. Each county had four villages that participated in the experiment. Half of the villages are treated. Namely, there are totally eight villages that participated in the experiment. Four of the eight villages are chosen as the treatment villages. The rest of the villages will serve as the control villages. All registered poor households in the treatment villages are offered with a bundle that contains an EITC-type program-Labor Income Reward Plan. Upon enrollment, households will start receiving cash rewards every quarter (for three quarters). The amount of the EITC depends on household earning in the last month. The offered EITC contains only a phase-in stage with a phase-in rate of from 10% to 15%. The rest of the bundle contains a 50% of kick-starter fund subsidy and a price subsidy for farm products. Households can choose to receive reward/subsidy through either method or all methods. However, there is a maximum amount of reward per quarter. In addition, there is a limit to the total amount of the reward for three quarters combined. The limits apply to the bundle, which means it accounts for both EITC and other subsidies above. Each claim needs supporting materials such as proof of employment and income (see Appendix B.3). Finally, household surveys were conducted among households in both the treatment villages and the control villages prior and post to intervention.

The design has several important and unique features. First, it implements a relatively strong treatment intensity (17% of the monthly income). Second, it employs a comprehensive measure to audit and validate each earning claim. Given that China has a large portion of self-employed jobs, this validation process is particularly crucial for the experiment since it is relatively easier for the self-employed to manipulate earnings compared to the employed with formal contracts. Third, the EITC policies were offered with other supplemental components that vary across the counties. This gives me a valuable opportunity to see how EITC interacts with other typical anti-poverty policies in China. Finally, this is the first EITC experiment that was conducted in China with a

focus on rural/minority populations.

Results show that Labor Income Reward Plan increases the labor supply significantly on both extensive margin and intensive margin. Estimates show that the average number of earners per household has increased by 0.30 person (13% increase), and the total hours worked has increased by 77 hours (41%). Consequently, households' monthly wage earnings (without the benefit) has increased by 466 RMB (63 USD). The size of the effects seems to be a lot larger than the existing literature, which is partially due to the strong treatment intensity. Moreover, the effects are very similar to the findings from the previous Wutongqiao Experiment. Distribution tests suggest that the program may have increased households' expenditures in food and transportation and reduced expenditures on wedding and funeral. Although I only find suggestive evidence that the program increases in household expenditure, the results suggest that the EITC has achieved its designed purpose, which is to increase the labor supply and earning.

3.1 Experiment Design

The Mabian/Muchuan[1] Experiment was conducted by China Households Finance Survey (CHFS), at Southwestern University of Finance and Economics, China. The experiment started in April 2017. All registered poor households in the treatment villages were offered with Labor Income Reward Plan. The program will allow households to receive a cash reward every quarter. On average, enrolled households received a 211 RMB (30 USD) cash reward each month. The amount is roughly equivalent to 17% of households' monthly income. This is the first EITC experiment that was conducted in China with a focus on rural/minority populations.

3.1.1 Treatment

All registered poor households are offered a policy bundle that contains either two or three components depending on the county the households reside in. The first component is the EITC component. The amount of the reward is a fixed percentage of household earning in the last quar-

[1]Mabian is a minority autonomous region in China with an area similar to the city of Jacksonville in the US. Mabian county has a population of around 220,000. The GDP per capita is 2,480 USD in 2017 (Mabian People's Government, 2016). Muchuan is a county with an area slightly larger than Phoenix. Muchuan county has a population of around 260,000 with a GDP per capita of 2,460 USD in 2011 (Muchuan People's Government, 2011).

ter. The rate is either 6% or 10%, depending on the county. Unlike the typical EITC scheme, the EITC scheme in this particular experiment only contains a phase-in stage. The EITC is distributed once every quarter. However, households are required to report their earnings monthly. The second component is the price subsidy for farm products. This means that households can receive a percentage of the revenue they received from selling their farm products. For example, if a household earned 1,500 RMB by selling ducks. They can apply for a price subsidy that equals 8% of the sales. The rates of subsidies vary across products and county. Similar to the EITC, the price subsidy is usually distributed quarterly, and households are required to report their sales revenue monthly. The last component is the kick-starter fund subsidy that equals 50% of the cost of the farm project, including products such as cash-crop growing, greenhouse, and livestock raising. The subsidy only covers one-time investment, such as seed and pen construction, and it does not cover costs such as labor and fertilizer. This component is distributed quarterly and is only available to households in Muchuan County. Appendix B.1 and B.2 show the bundles that were offered to households in Mabian and Muchuan Counties. In addition, the maximum reward for each quarter is 1,500 RMB (2,500 RMB for Muchuan County). The maximum reward for three quarters combined is 7,000 RMB (4,000 RMB for Muchuan County). When the calculated amount exceeds the quarterly limit, the extra amount will be forwarded to the next quarter.

3.1.2 Timeline

The experiment started with a baseline survey in April 2017. Then, the intervention started immediately after the baseline survey. Households were required to report earnings as well as provide supporting materials monthly during the intervention period. Community staff will review the integrity and validity of the submitted materials. The reported earnings, supporting materials, and the calculated reward were audited and approved by CHFS quarterly, after which the reward was distributed within the first ten days of each quarter. There are totally three quarters in the intervention period. The intervention stopped in January 2018. Finally, a follow-up survey was conducted in April 2018. Figure 3.1 describes the major phases of Mabian/Muchuan Experiment.

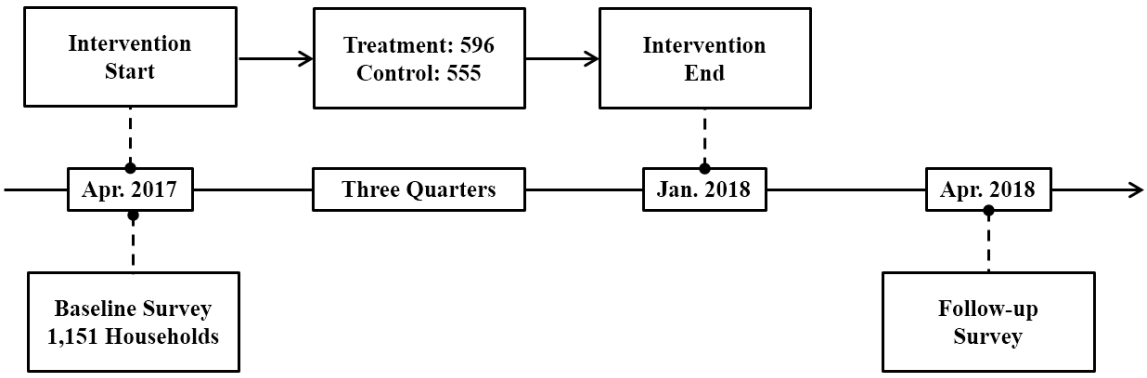

Figure 3.1: Mabian/Muchuan Experiment Timeline (2017-2018)

3.1.3 Implementation

Although the policy bundles in two counties are different, the implementation procedure is identical. This section will briefly discuss how the policy was implemented during the intervention period and how households can access the program.

Preparation - Eligible households (all registered poor households in the treatment villages) first applied for participation through CHFS prior to the intervention started. Then, information sessions were carried out to inform the participated households about the program mechanism. A team made of eight to ten local community staff was appointed to administer the program in each treatment village. The staff was appointed by the local authorities. Each team participated in a training session before the experiment. In addition, a cross-validation procedure was implemented within the team. Namely, the staff from the same team will check and review the works of others. Finally, both information sessions and training sessions were carried out by CHFS.

First-time applicant - Upon enrollment, households were required to sign the consent form (Appendix B.2) and fill out the application form (Appendix B.2). Then, the forms were reviewed and signed by the community staff.

Income report - Households are required to document and report the income every month as

well as providing the supporting materials (see Appendix B.3). All the required documents need to be prepared properly, signed, and submitted to the community staff between the 20th and 24th of each month. Upon acceptance, the community staff will also sign and date the documents. Late submission will be forwarded to the next month. The community staff will then calculate the earnings and upload the data to the audit system in the next five days.

Approval - CHFS will review all the materials and calculate the reward within the first ten days of each quarter (including forwarding the excess amount). Then, results will be sent to the community staff in each village. The community staff will confirm the amount of the reward and compile the forms (Appendix B.2) to be approved then submit them to the township governments in the next five days. The reward for each household will be published and open to the public for any inquiry for five days, after which the governments will seal the approvals and submit them to the county's office of poverty alleviation. Finally, the office will directly transfer the reward to the bank accounts of the applicants.

3.2 Survey Data

The survey sample was restricted to the villages that participated in the experiment. Each county had two treatment villages and two control villages. Boxiang, Ciwan, Gantianba, and Qianguang are the treatment villages. Erping, Fengxi, Hejia, and Jinpen are the control villages. Households were randomly selected for surveys for each of the eight villages. There were 2,778 households that were selected for the household surveys. This sample size is calculated based on the power of 95%.

Table 3.1 presents the data used in the analysis[2]. The data is collected via two rounds of household surveys that were conducted before and after the intervention. The first round of the survey was conducted prior to the intervention in April 2017. The follow-up surveys were conducted after the intervention in April 2018.

The first column in Table B.4 presents the household characteristics in the sample, including labor supply, demographics, and socioeconomic status. 1,511 households have responded to the

[2]A proportion of the households absented either the baseline survey or the follow-up survey or both.

Table 3.1: Household Survey Data for Mabian/Muchuan Experiment

	Baseline	Follow-up
Treatment Villages		
Boxiang	242	197
Ciwan	71	66
Gantianba	199	135
Qianguang	84	77
Control Villages		
Erping	60	30
Fengxi	250	171
Hejia	175	145
Jinpen	70	62
Sample Size	1,511	883

baseline survey. There are several aspects that are worth mentioning. First, the average family size (4.26) tends to be larger compared with the urban sample in Wutongqiao Experiment (2.91). Consequently, the average number of able-bodied persons is higher (2.66). This is one of the main differences between households in rural and urban areas. Second, 37% of the households in the sample are registered poor households. Moreover, more than half (55.7%) of the registered poor households reported the main causes of their poverty are health-related. Third, the wage income contributes only a small proportion of the total income primarily because the experiment site is in rural areas. Moreover, more than half of the households in the sample are minorities. Thus, the language barrier makes entering the labor market even more difficult for these families. Finally, the self-reported expenditure tends to be larger than the self-reported income, which is typical among household surveys.

There is a notable decline in sample size in the follow-up survey, which is primarily due to the no-shows in the follow-up survey. The main concern of this loss of sample is that it might

introduce bias when estimating the treatment effects. Two-sample tests show that households that did not show up during the follow-up survey are statistically different from households that attended both surveys in many aspects (Table B.3). Namely, the no-show households tend to have less income, less labor supply, fewer jobs, and fewer able-bodied persons. No-show households are less likely to be registered poor households, less likely to come from the treatment villages, more likely to be a minority household, and less likely to have a pensioner in the household. However, these differences indicate that the disadvantaged (lower labor supply and income) households in the control villages are more likely to miss the follow-up survey. This means that the outcome variables, such as labor supply and income in the control villages, will be over-estimated in the follow-up survey. Consequently, the treatment effects are likely to be underestimated. Therefore, it is unlikely that the effects are driven by the selection in no-shows.

3.3 Methodology

Given the fact that the treatment was assigned on the village-level and the fact that there were only eight villages, the design of the experiment is more of a quasi-experiment study rather than a field experiment. Thus, a classic approach to identify the treatment effects is by using the Difference-in-Differences (DD) estimation. The DD estimation imposes the identification assumption that the changes in the control group will provide a good counterfactual for the changes in the treatment group in the absence of the intervention, which is usually referred to as the "common trend" assumption. However, there is no way to test this assumption due to the fact that there are only two panels of survey data (Table 3.1). A second-best solution is to compare the characteristics of the households in the treatment villages and the control villages. If there is no statistical difference in household characteristics (except for outcome variables such as income and labor supply) between the treatment group and the control group, it can be argued that the identification assumption would hold. Table B.4 shows the balance of covariates of households in the treatment villages and the control villages. Unfortunately, results show that households in the treatment group are less likely to be a minority household and tend to have fewer jobs. The differences are statistically significant. This means any effect that could be identified by the DD estimation is caused by either

the policy or the differences in household characteristics, or both, which will severely hamper the validity of the analysis. The last resort is to control for these differences and hope that controlling for the observables will also resolve the bias caused by the unobservables. However, it is generally considered that such an estimation is not reliable. To alleviate this problem, I will use DD estimation with extensive controls and village fixed-effects to recover the treatment effect. Meanwhile, I will estimate the treatment effect using three other methods, including Propensity-score Matching (PSM), DD estimation with Propensity-score Matching (PSM-DD), and Difference-in-Difference-in-Differences (DDD). Given that most outcome variables are household-level outcomes, I aggregated the data to the household-level. There will be an independent analysis in the next sections that discuss the impacts on individual-level outcomes.

PSM is generally considered to be less-than-ideal primarily because the matching on observables does not guarantee the balances on unobservables. In order to alleviate this concern, I will use a one-to-one matching method and match observations based on all of the outcome variables as well as all of the household characteristics. Then, I will use additional tests to ensure the match is successful.

PSM-DD is a combination between PSM and DD estimation. The DD estimation resolves a part of the potential bias (observable and unobservable) caused by the differences between the treatment and the control group. Since PSM-DD should yield the same estimates if PSM successfully removes the bias, PSM-DD also serves as a check of PSM estimation. In order to make sure any difference between the two estimations is not caused by the differences in matching methods, I will use the exact same matching result from the PSM for PSM-DD. Finally, both PSM and PSM-DD will be matching the eligible households (registered poor households in the treatment villages) with other households since it comes with a larger pool of candidates that increases the chance of successful matching.

Since only registered poor households are eligible for the program, it provides a third difference other than the time difference (pre/post) and the village difference (the treatment/control villages). Compared with the DD estimation, the DDD estimation resolves a part of the bias that causes the

difference in "trends". However, it only resolves the part of the bias that brings the same difference between the two groups. If the DD estimation is without this sort of bias, the DDD estimation should give similar (not identical) results. Thus, the DDD estimation provides a comparison to the DD estimation. In order to make the estimations as comparable as they can be, I will use the same set of factors that were used in PSM and PSM-DD as the controls for the DD estimation and the DDD estimation. Finally, it is important to point out that all estimation recover the Treatment Effect on the Treated (ATT) except for the DD estimation, which recovers the Average Treatment Effect (ATE).

3.4 Results

The results from the Wutongqiao Experiment show that the Labor Income Reward Plan increases the number of earners in the household, hours worked, earning, and expenditure. Although the samples and identification strategies are different, one should expect that the results to be similar to the Wutongqiao Experiment. In the following section, I will discuss the impacts of the policy on labor supply, earning, production, and expenditure.

3.4.1 Matching

Table B.5 shows the results of the matching. The households are matched based on both outcome variables and household characteristics in the pre-period. Results indicate that the covariates between the two groups are balanced after matching. In addition, there are two additional tests to evaluate the overall matching success. Rubins' B measures the absolute standardized difference of the means of the linear index of the propensity score between the two groups. Rubin's R measures the ratio of the variances of the propensity-score index between the two groups. It is recommended that a successful matching should have a B less than 25 and an R between 0.5 and 2 (Rubin, 2001). The result shows that the Rubins' B for the matching is 25.0, and the R is 1.03. Therefore, the two groups are sufficiently balanced after matching.

3.4.2 Impacts on Labor Participation

The number of earners and the number of jobs in the household reflect the extensive aspect of the labor supply. An individual is defined as earner if he/she reported that he/she had worked in the past year. The number of earners is the total number of earners in the household. Similarly, the number of jobs is the total number of jobs in the household. The last two rows of Table 3.2 presents the treatment effects on extensive labor supply. The result indicates that the Labor Income Reward Plan has positive and significant impacts on extensive labor supply. Namely, program participation increases the number of earners and the number of jobs. More importantly, the magnitudes and the significance of the estimates are consistent across the regressions. The magnitude of the effect on the number of earners ranges from 0.30 person to 0.54 persons per household. The estimated effect on the number of jobs ranges from 0.44 jobs to 0.96 jobs per household. The results from the PSM estimation are slightly smaller than the ones from the PSM-DD estimation. This suggests that there exists a potential unaddressed bias in the PSM estimation. However, it tends to bias the result downward. Therefore, this should not be a concern. The results from the DDD estimation are larger than their counterparts in the DD estimation, which is expected since the DD estimation yields ATE. The size of the effect falls between the estimates from the PSM estimation and the PSM-DD estimation. Therefore, these findings are reasonably convincing.

3.4.3 Impacts on Hours Worked

The total hours worked reflects the intensity aspect of the labor supply. The variable is calculated based on four questions in the survey. Namely, household members were first asked how many months they had been working in the past year (2016 or 2017). Then, household members were asked, on average, how many weeks they had been working per month. Next, household members were asked, on average, how many days they usually work per week. Finally, household members were asked, on average, how many hours they usually worked per day in the past year. For each individual, hours worked is the product of the months worked in the past year, weeks worked per month, days worked per week, and hours worked per day. The total hours worked

Table 3.2: Treatment Effects on Labor Supply and Earning (Mabian/Muchuan)

	PSM	PSM-DD	DD	DDD
Wage Income	5,598***	7,145***	4,669**	6,261***
Total Hours Worked	925**	1,610**	890***	1228**
# of Earners	0.30**	0.54**	0.00	0.37***
# of Jobs	0.44**	0.96***	0.38***	0.57**
Controls			Yes	Yes
Village FE			Yes	Yes
Sample Size	883	883	1,151	1,151

*** p<0.01, ** p<0.05, * p<0.1

is the sum of hours worked for each individual in the household. The second row of Table 3.2 presents the treatment effect on total hours worked. The result indicates that the Labor Income Reward Plan increases the intensive labor supply significantly. Namely, program participation increases total hours worked. More importantly, the magnitude and significance are consistent across the regressions. The magnitude of the effect ranges from 925 hours to 1,228 hours per year (or 77 hours to 102 hours per month) per household. The results from the PSM estimation are slightly smaller than the ones from the PSM-DD estimation. This suggests that there exists a potential unaddressed bias in the PSM estimation. However, it tends to bias the result downward. Therefore, this should not be a concern. The results from the DDD estimation are larger than their counterparts in the DD estimation, which is expected since the DD estimation yields ATE. The size of the effect falls between the estimates from the PSM estimation and the PSM-DD estimation. Noticed that the differences between the estimates from different regressions are moving in identical directions as they did in terms of extensive labor supply, I consider this evidence that is consistent with the findings in extensive labor supply and, therefore, convincing.

3.4.4 Impacts on Earning

An income is considered to be wage income if it is earned via either employment and self-employment, including part-time and freelance jobs. The income that is earned via farming and business is not included since they are documented on the household-level. The total earning is the sum of the earning of each individual in the household. Earning is particularly important since the EITC incentivizes households directly through earning (instead of employment or working hours). The first row of Table 3.2 presents the treatment effects on household earning. The result indicates that the Labor Income Reward Plan has a positive and significant impact on household earning. More importantly, the magnitude and the significance of the estimates are consistent across the regressions. The size of the effect ranges from 5,598 RMB to 7,145 RMB per year (or 466 RMB to 595 RMB per month) per household. Again, the results from the PSM estimation are slightly smaller than the ones from the PSM-DD estimation. The estimate from the DDD estimation falls between the estimates from the PSM estimation and the PSM-DD estimation. The differences between the estimates from different regressions move in identical directions as they did in terms of extensive and intensive labor supply. Therefore, this result is consistent with the previous findings.

3.4.5 Impacts on Production Type

It is important to examine how households would change their production types for two reasons. First, the Labor Income Reward Plan that was offered to the households contains components that reward through both employment and farming. This gives me a unique opportunity to better understand how households determine and organize production when facing economic incentives. Second, Mabian/Muchuan Experiment is the first EITC experiment that was conducted in China with a focus on rural populations where farming is a major source of income. There are several reasons to expect that the rural population would react differently (with or without the farming reward). For instance, the employment opportunity in rural areas may not be as abundant as it is in urban areas. Thus, rural households might find it difficult to increase earning via additional em-

ployment. Table B.8 presents the treatment effects on production type. The first three outcomes in the table are indicators that identify whether a household engages in crop farming only, husbandry only, or both. The fourth row is an indicator that equals one if a household produces more than one type of farm product. The last four rows are indicators that equal to one if a household engages in food-crop farming, cash-crop farming, forestry, or husbandry. The result indicates no significant impact on household production. Food-crop is defined as farm products that are grown for food consumption. Cash-crop is defined as farm products that are grown for their economic values. Yield is the reported market value of the farm products. Profit is the market sales revenue less the cost of the input. There is some evidence that suggests that program participation increases households' engagements in food-crop farming, forestry, and husbandry. In addition, households tend to diversify their farming products. However, these findings are inconsistent across regressions in terms of either magnitudes and significance. This implies that households reacted to the program primarily by increasing earning via employment. Moreover, this is consistent with the administrative record in which 80% of the reward was collected via the EITC component. There are three possible explanations of why households prefer employment rather than farming. First, expanding farming production might be particularly difficult due the constrains such as labor, farmlands, and funds. The subsidy rate of the kick-starter fund may not be high enough to lift the households over the barrier. Second, since it takes a significant amount of time for the household to profit from the yield compared with employment, the liquidity constraints might have prevented the households from expanding farm productions. Finally, households might be concerned about the risk of engaging in new farming activities without sufficient training and experience. Consequently, there has been a little change in terms of yield and profit through farming (Table B.8).

3.4.6 Impacts on Individuals

Individual-level data allows me to examine outcomes that cannot be aggregated to the household-level. The matching for the individual-level regressions is similar to the ones for the household-level data. The only difference is that household-level outcomes were placed by individual-level outcomes. Table B.6 shows that the Rubins' B for the matching is 16.7, and the R is 0.97. There-

fore, the two groups are sufficiently balanced after matching. In this section, I will first discuss outcomes that had been covered before, including earning, employment, the number of jobs. Then, I will discuss the treatment effects on work types. Finally, I will breakdown the total hours worked and look at the treatment effects on each component.

The first three rows in Table B.9 presents the treatment effects on individual earning and employment. The results indicate that the Labor Income Reward Plan has positive and significant impacts on individual earning and the number of jobs. However, the estimates are less consistent compared with the previous regressions. Particularly, the treatment effect on employment becomes small and insignificant. This change implies that program participation has heterogeneous treatment effects across different types of households. Since the number of earners has increased significantly in the household-level regression, the small and insignificant effect on individual employment indicates that the amount of people who became employed is roughly close to the ones that became unemployed in general. However, the two groups of individuals are distributed differently among households. On the one hand, individuals who became employed are more evenly distributed among households so that the increase in earners would have a higher weight. On the other hand, individuals who became unemployed are from fewer households, so that the decrease in earners would have a lower weight. Thus, it shows an overall positive effect on the number of earners in the household. Moreover, the increase in the number of jobs implies that there may have been a substitution effect within households. This means that there are household members who took up more jobs while other members from the same household quit working. Finally, The fourth and the fifth row in Table B.9 presents the treatment effects on individual work type. An individual has an employed job if he/she is hired by an employer. An individual is self-employed if he/she is farming or doing business. The results indicate no significant change in work types.

The last five rows in Table B.9 present the treatment effects on individuals' labor supply. Individual labor supply is reflected by five measurements that describe different aspects. All of the measurements are constructed based on the four questions in Section 3.4.3. The magnitude of the effect on total months worked ranges from 0.47 to 0.62 months per year. The magnitude of the

effect on total days worked ranges from 9 to 18 days per year. The magnitude of the effect on total hours worked ranges from 157 to 179 hours per year. The size of the effect on the number of days worked per month ranges from 1.29 to 1.87 days per month. The magnitudes and the significance of the estimates are consistent across the regressions. However, evidence suggests that individuals' number of hours worked per day has decreased.

3.4.7 Impacts on Expenditure

Previous results show that program participation significantly increases household earning. Therefore, it is expected that household expenditure would also increase. However, the estimates for household expenditures are mostly inconsistent across regressions in terms of either magnitudes and significance (Table B.10). There are two potential explanations for this inconsistency. First, the self-reported expenditures usually come with large measurement errors. Thus, the effects may not be large enough to overcome the noise. Second, it is possible that the policy has heterogeneous effects across different types of households. Therefore, a better way is to test the effects on different quantiles. However, there is not enough statistical power to conduct meaningful comparisons. A second-best solution is to compare the distributions between the two groups. Here, I employed a method invented by Abadie (2002) to test for equality of the distributions, as well as first and second order stochastic dominance.

Figure B.1 to Figure B.3 present the Cumulative Density Function (CDF) of changes in household expenditures. Namely, I calculated the changes in household expenditures between the follow-up survey and the baseline survey for each household. Then, I plot the CDF of the changes for both the treatment villages and the control villages. There exist notable differences in CDFs of expenditures on food, utilities, sundries, transportation, and communication. Table B.10 presents the results of the Kolmogorov-Smirnov test for differences in CDFs between the treatment villages and the control villages. The results indicate that CDFs of changes in food expenditure, changes in transportation expenditure, and changes in wedding/funeral expenditure are statistically different. The distribution of changes in food expenditure and the distributions of changes in transportation expenditure of the control villages dominate the treatment villages in both first and second order.

The distribution of changes in wedding/funeral expenditure of the treatment villages dominates the control villages in both first and second order. This suggests that the EITC may have increased households' expenditures on food and transportation and reduced the expenditure on wedding and funeral.

3.5 Heterogeneous Treatment Effects

Table B.12 presents the heterogeneous treatment effects from the PSM-DD estimations. Here, the sample is divided into subgroups based on different criteria. The high-income group is defined as households with a total income higher than the median. The high-education group is defined as households that have at least one member who has received secondary education. "Multiple able-bodied" is defined as households with at least two individuals between 16 and 65 years old who are able to work. "Multiple earners" means that households have at least two individuals who have been working prior to the intervention. Results show that treatment effects vary across different subgroups.

First, high education households responded similarly to low education households in terms of labor supply. However, the effect on wage income among high education households is a lot larger than the low-education group. This may imply that there exists a fundamental difference in abilities to increase earning through employment if an individual holds a high school diploma or a higher degree. This also implies that disadvantaged households benefit less from the program in terms of earning.

Second, EITC has similar effects on labor supply among households with different amount of able-bodied individuals, but the effect on wage income is a lot larger among households with more than one able-bodied individual. This implies that the additional wage incomes are from very different jobs. For example, households with single labor might be forced to choose part-time jobs due to their time constraints, with which the wage is generally lower than full-time jobs. Similar patterns can be observed among households with different earners. However, the differences are less appealing.

Finally, heterogeneous treatment effects indicate that households that increase income through

farming are mainly the ones that have more able-bodied persons but fewer earners prior to the experiment. However, there is no increase in farm profit.

3.6 Summary

Mabian/Muchuan Experiment was conducted by China Household Finance and Survey (CHFS) between 2017 and 2018. Eight villages participated in the experiment, of which four villages were treated. The design employs a strong treatment intensity and a rigorous validation process. This experiment is also the first EITC experiment that was conducted in China that focuses on rural/minority populations.

Results show that Labor Income Reward Plan increases labor supply significantly on both extensive margin and intensive margin. Estimates show that the number of earners per household has increased by 0.30 person (13% increase). The number of jobs per has increased by 0.38 (27% increase) job per household, and total hours worked has increased by 77 hours (41% increase). Consequently, households' monthly wage earnings (without the benefit) has increased by 466 RMB (63 USD) per month. These results are consistent across different methods in terms of both magnitudes and significance. Moreover, the size of the effects is very similar to the findings from the previous Wutongqiao Experiment. Distribution tests suggest that the EITC may have increased household expenditure on food and transportation. However, it reduced expenditure on weddings and funeral. Although I do not find a significant increase in household expenditure, the results suggest that EITC has achieved its designed purpose, which is to increase the labor supply and earning. Most importantly, the outcome of the experiment proves that EITC can work soundly not just in urban areas in China but in rural areas as well.

4. LEIBO EXPERIMENT

Leibo Experiment was conducted by China Household Finance and Survey (CHFS) between 2017 and 2018. The experiment was conducted in Leibo County. The county is located in Liangshan Yi Autonomous Prefecture, Sichuan Province, China. Four villages (390 households) participated in the experiment, of which two villages were treated. All registered poor households in the treatment villages are offered with an EITC policy that contains two versions. Upon enrollment, households will start receiving cash rewards every quarter (for consecutive four quarters). The amount of the EITC depends on household earning in the last month. The offered EITC contains three stages, including a phase-in, a plateau, and a phase-out. The amount of reward will first increase with earning (phase-in rate=20% or 33% depending on the number of children). Then, it reaches the maximum (300 RMB or 495 RMB depending on the number of children) and no longer changes with the earning (plateau). Eventually, the reward will start decreasing as households earn more and move into the phase-out stage (phase-out rate=10%). Once the earning exceeds the break-even point (5,500 RMB or 5,800 RMB depending on the number of children), the household becomes ineligible for the program automatically. The other version is an EITC scheme but with one phase-in stage and two plateau stages. The scheme starts with a basic amount of 100 RMB (first plateau). Then, it enters a phase-in stage (phase-in rate=15%). The reward stops increasing once it reaches 2,000 RMB, after which it enters the second plateau. Each claim needs supporting materials such as proof of employment and income (see Appendix C.3). Finally, surveys were conducted among households in both the treatment villages and the control villages prior to and post to the intervention.

The design has several important and unique features. First, it implements a relatively strong treatment intensity (18% of the monthly income). Second, it employs a comprehensive measure to audit and validate each earning claim. Given that China has a large portion of self-employed jobs, this validation process is particularly crucial for the experiment since it is relatively easier for the self-employed to manipulate earnings compared to the employed with formal contracts. Third, the

EITC that was offered to the households contains two different schemes. Households can choose to receive EITC through one or both of the schemes. This feature offers a unique opportunity for researchers to better understand households' preferences of EITC schemes. Finally, the sample focus on rural and minority populations.

Distribution analysis suggests that program participation has improved household labor supply both on extensive and intensive margins. Namely, the result indicates the distributions of changes in farm yield, changes in the number of earners, changes in the number of jobs, and changes in total hours worked for the control villages stochastic dominates the treatment villages in both first and second order. In addition, program participation improves household earning. The distribution of changes in household wage income for the control villages stochastic dominates the treatment villages in both first and second order. However, the regression analysis shows that the estimates are inconsistent in terms of both magnitude and significance.

4.1 Experiment Design

The Leibo[1] Experiment was conducted by China Households Finance Survey (CHFS) at Southwestern University of Finance and Economics, China. The experiment started in June 2017 and ended in January 2018. All registered poor households in the treatment villages were offered with Labor Income Reward Plan. The program will allow households to receive a cash reward every quarter.

4.1.1 Treatment

All registered poor households are offered with a policy bundle that contains two versions of EITCs. The first version is an EITC that can be applied only through earning from employment. The amount of the EITC depends on household earning in the last month. The offered EITC contains three stages, including a phase-in, a plateau, and a phase-out. The amount of reward will first increase with earning (phase-in rate=20% or 33% depending on the number of children). Then, it reaches the maximum (300 RMB or 495 RMB depending on the number of children)

[1]Leibo is a county in China with an area twice as large as Phoenix in the US. The county has a population of around 240,000. The GDP per capita is 4,112 USD in 2018 (Leibo People's Government, 2018).

and no longer changes with the earning (plateau). Eventually, the reward will start decreasing as households earn more and move into the phase-out stage (phase-out rate=10%). Once the earning exceeds the break-even point (5,500 RMB or 5,800 RMB depending on the number of children), the household becomes ineligible for the program automatically. The second version is an EITC that can be applied through income either from crops farming, husbandry, or business. This version is an EITC scheme but with one phase-in stage and two plateau stages. The scheme starts with a basic amount of 100 RMB (first plateau). Then, it enters a phase-in stage (phase-in rate=15%). The reward stops increasing once it reaches 2,000 RMB, after which it enters the second plateau and continues forever. The EITC is distributed once every quarter. However, households are required to report their earnings monthly. In addition, the maximum reward for each quarter is 1,000 RMB. The maximum reward for two quarters combined is 4,000 RMB. When the calculated amount exceeds the quarterly limit, the extra amount will be forwarded to the next quarter.

Figure 4.1 and Figure 4.2 demonstrate how reward changes across earning and family structures. An eligible child is defined as any child below 16 years old or any child above 16 years old but is currently enrolled in education (up to undergraduate equivalency). The earning from farming and business is calculated as the sum of the sale revenue (instead of profit). Poultry and livestock animals must be sold in the presence of the community staff to be valid for the reward.

4.1.2 Timeline

The experiment started with a baseline survey in June 2017. Then, the intervention started immediately after the baseline survey. Households were required to report earnings as well as provide supporting materials monthly during the intervention period. Community staff will review the integrity and validity of the submitted materials. The reported earnings, supporting materials, and the calculated rewards were audited and approved by the research team from CHFS quarterly, after which the reward was distributed within the first ten days of each quarter. There are totally four quarters in the intervention period. The intervention stopped at the end of September 2018. Finally, a follow-up survey was conducted in November 2018. Figure 4.3 describes the major phases of the Leibo Experiment.

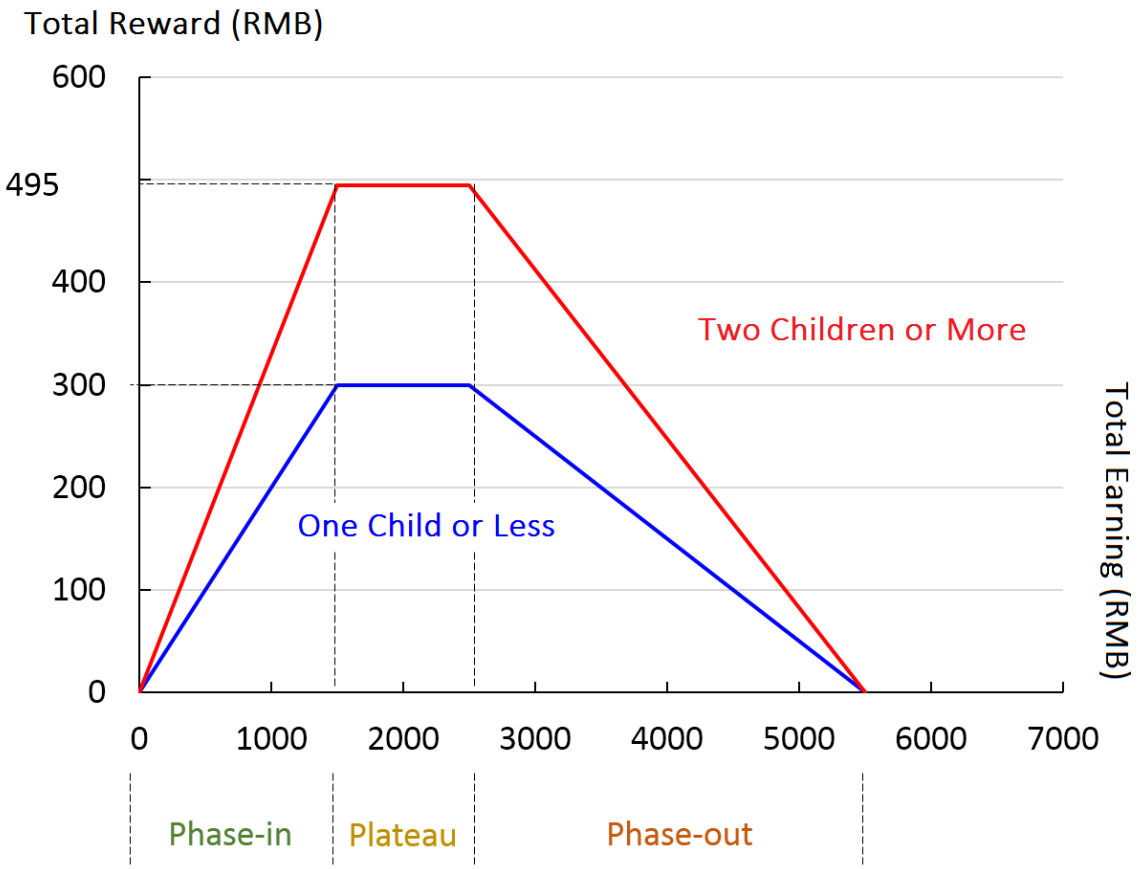

Figure 4.1: Labor Income Reward Plan in Leibo (Employment Reward)

4.1.3 Implementation

The implementation procedure is largely identical to Mabian/Muchuan Experiment. This section will briefly discuss how the policy is implemented during the intervention period and how households can access the program.

Preparation - Eligible households (all registered poor households in the treatment villages) first applied for participation through CHFS prior to the intervention started. Then, information sessions were carried out to inform the participated households about the program mechanism. A team made of three to five local community staff was appointed to administer the program in each treatment village. The staff was appointed by the local authorities. Each team participated in a

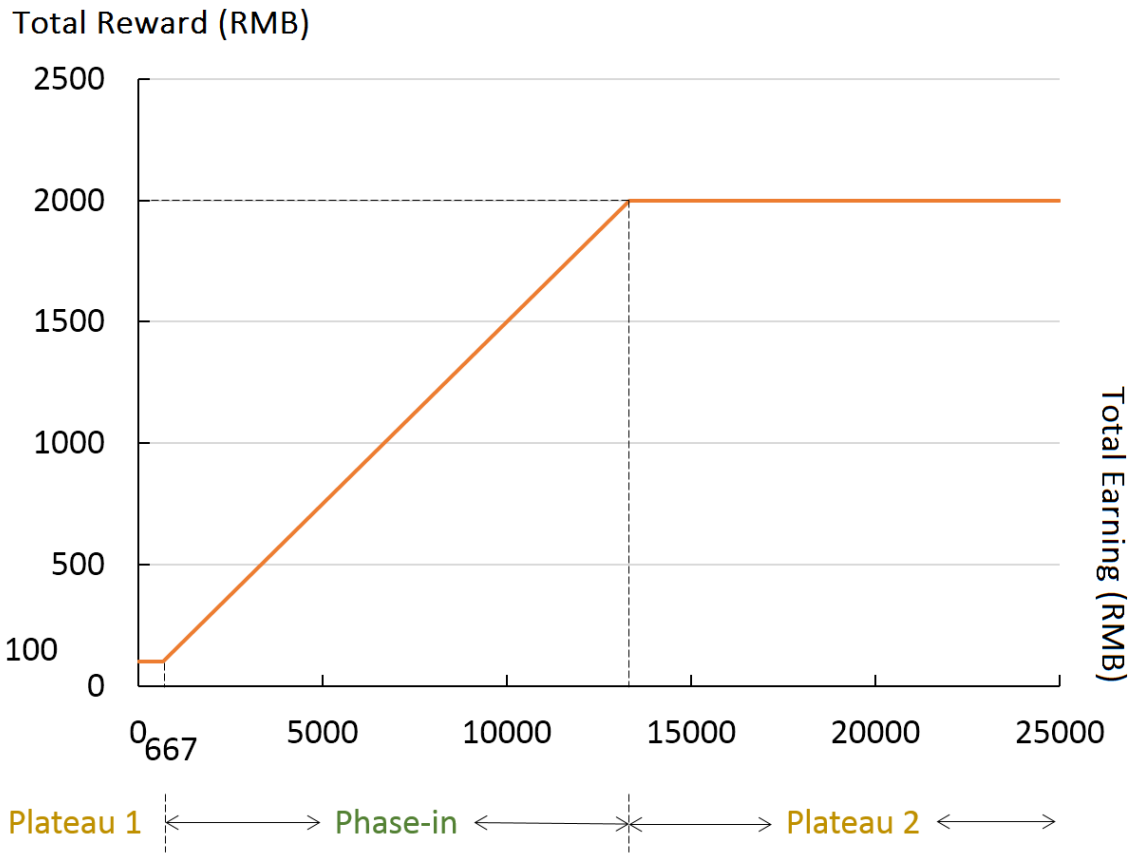

Figure 4.2: Labor Income Reward Plan in Leibo (Farming and Business)

training session before the experiment. Finally, both information sessions and training sessions were carried out by CHFS.

First-time applicant - Upon enrollment, households were required to sign the consent form (Appendix C.2) and fill out the application form (Appendix C.2). Then, the forms were reviewed and signed by the community staff.

Income report - Households are required to document and report the income every month as well as providing the supporting materials (see Appendix C.3). All the required documents need to be prepared properly and signed before the end of each month, then submitted to the community staff within the first four days of each month. Upon acceptance, the community staff

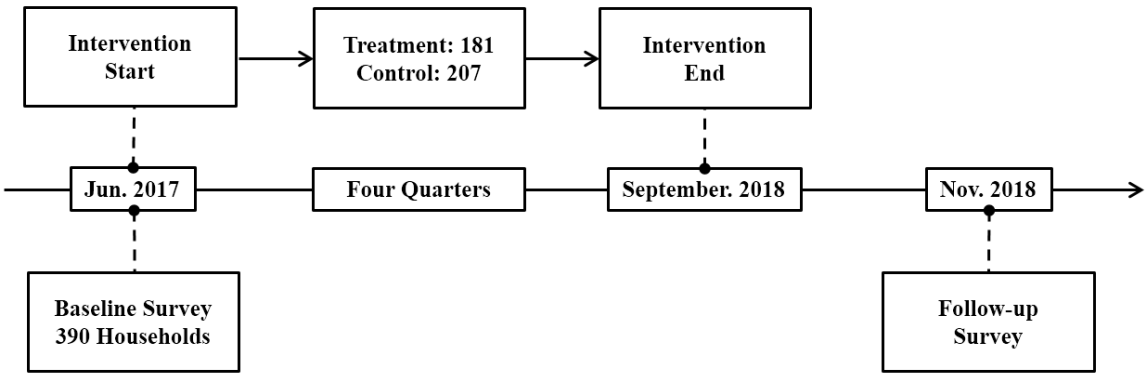

Figure 4.3: Leibo Experiment Timeline (2017-2018)

will also examine the validity of the documents before sign and date. The community staff will then calculate the earnings and upload the data to the audit system in the next five days.

Approval - CHFS will review all the materials and calculate the reward within the first ten days in each quarter (including forwarding the excess amount). Then, results will be sent to the community staff in each village. The community staff will have applicant households confirm the amount of the reward and compile the forms (Appendix C.2) to be approved then submit them to the township governments in the next five days. The reward for each household will be published and open to the public for any inquiry for five days, after which the governments will seal the approvals and submit them to the county's office of poverty alleviation. Finally, the office will directly transfer the reward to the bank account of the household applicants.

4.2 Survey Data

The survey sample was restricted to the villages that participated in the experiment. Two treatment villages and two control villages participated in the experiment. Moshi and Shuikouba are the treatment villages. Luohangou and Meiyi are the control villages. Households were randomly selected for surveys. There were 390 households that were selected. This sample size is calculated based on the power of 95%.

Table 4.1: Household Survey Data for Leibo Experiment

	Baseline	Follow-up
Treatment Villages		
Moshi	86	61
Shuikouba	99	84
Control Villages		
Luohangou	108	48
Meiyi	95	59
Sample Size	388	252

Table 4.1 presents the data used in the analysis[2]. The data is collected via two household surveys that were conducted before and after the intervention. The first survey was conducted prior to the intervention in June 2017. The follow-up surveys were conducted after the intervention in November 2018.

The first column in Table C.3 presents the household characteristics in the sample, including labor supply, demographics, and socioeconomic status. 388 households have responded to the baseline survey. It can be noticed that the average family size (4.84) tends to be larger compared with the urban sample in Wutongqiao Experiment (2.91). Consequently, the average number of able-bodied persons is higher (2.71). This is one of the main differences between households in rural and urban areas. In addition, 64% of the households in the sample are registered poor households. The poverty rate is substantially larger than the sample in Mabian/Muchuan Experiment, indicating that the households in Leibo sample are a lot poorer. Consequently, income and expenditure are relatively lower. Finally, the self-reported expenditure tends to be larger than the self-reported income, which is typical among household surveys.

There is a notable decline in sample size in the follow-up survey, which is primarily due to the

[2]A proportion of the households absented either baseline or follow-up surveys or both.

no-shows. The main concern of this loss of sample is that it might introduce bias when estimating the treatment effects. Two-sample tests show that households that did not show up during the follow-up survey are not statistically different from households that attended both surveys with two exceptions (Table C.2). Namely, households that missed the follow-up survey are less likely to be registered poor households and less likely to perform farming. This is potentially problematic since it means that the treatment households are over-represented in the follow-up survey, which could bias the treatment effect upward. To address the problem, I restrict the sample to households that participated in both surveys. However, the results are robust to this restriction.

4.3 Methodology

Given the fact that the treatment was assigned on the village-level and the fact that there were only four villages, the design of the experiment is more of a quasi-experiment study rather than a field experiment. Thus, a classic approach to identify the treatment effects is by using the Difference-in-Differences (DD) estimation. The DD estimation imposes the identification assumption that the changes in the control group will provide a good counterfactual for the changes in the treatment group in the absence of the intervention, which is usually referred to as the "common trend" assumption. However, there is no way to test this assumption due to the fact that there are only two panels of survey data (Table 4.1). A second-best solution is to compare the characteristics of the households in the treatment villages and the control villages. If there is no statistical difference in household characteristics (except for outcome variables such as income and labor supply) between the treatment group and the control group, it can be argued that the identification assumption would hold. Table C.3 shows the balance of covariates of households in the treatment villages and the control villages. Unfortunately, results show that households in the treatment group are less likely to be a registered poor household and more likely to be a minority household. The results also show that households in the treatment villages are less likely to perform farming and tend to have fewer hours worked and able-bodied persons but more farming profit. The differences are statistically significant. This means any effect that could be identified by the DD estimation is caused by either the policy or the differences in household characteristics, or both, which will severely

hamper the validity of the analysis. The last resort is to control for these differences and hope that controlling for the observables will also resolve the bias caused by the unobservables. However, it is generally considered that such an estimation is not reliable. To alleviate this problem, I will use DD estimation with extensive controls and village fixed-effects to recover the treatment effect. Meanwhile, I will estimate the treatment effect using two other methods, including Propensity-score Matching (PSM) and DD estimation with Propensity-score Matching (PSM-DD). Given that most outcome variables are household-level outcomes, I aggregated the data to household-level.

PSM is generally considered to be less-than-ideal primarily because the matching on observables does not guarantee the balances on unobservables. In order to alleviate this concern, I will match on all of the outcomes variables as well as all of the household characteristics[3]. Then, I will use additional tests to ensure the match is successful.

PSM-DD is a combination between PSM and DD estimation. The DD estimation resolves a part of the potential bias (observable and unobservable) caused by the differences between the treatment and the control group. Since PSM-DD should yield the same estimates if PSM successfully removes the bias, PSM-DD also serves as a check of PSM estimation. In order to make sure any difference between the two estimations is not caused by the differences in matching methods, I will use the exact same matching result from the PSM for PSM-DD. Finally, both PSM and PSM-DD will be matching the eligible households (registered poor households in the treatment villages) with other households since it comes with a larger pool of candidates that increases the chance of successful matching.

It would be important to test for effects at different quantiles. However, there is not enough statistical power to conduct meaningful comparisons. A second-best solution is to compare the distributions between the two groups. In order to compare the distributions between the treatment villages and the control villages, I first calculated the changes in outcomes between the follow-up survey and the baseline survey for each household. Then, I plot the Cumulative Density Function (CDF) of the changes for both the treatment villages and the control villages. Finally, I employed

[3]Here, I used the kernel method to match the observations instead of one-to-one since rigorous matching reduces the chance of success drastically.

a method invented by Abadie (2002) to test for equality of the distributions, as well as first and second order stochastic dominance.

4.4 Results

The results from the Wutongqiao Experiment and Mabian/Muchuan Experiment show that the Labor Income Reward Plan increases the number of earners in the household, hours worked, and earning. Therefore, one should expect the results to be similar to the two experiments. In the following section, I will discuss the impacts of the Labor Income Reward Plan on labor supply, earning, and farming production.

4.4.1 Matching

Table B.5 shows the results of the matching. The households are matched based on both outcome variables and household characteristics in the pre-period. Results indicate that the covariates between the two groups are balanced after matching. In addition, there are two additional tests to evaluate the overall matching success. Rubins' B measures the absolute standardized difference of the means of the linear index of the propensity score between the two groups. Rubin's R measures the ratio of the variances of the propensity-score index between the two groups. It is recommended that a successful matching should have a B less than 25 and an R between 0.5 and 2 (Rubin, 2001). The result shows that the Rubins' B for the matching is 19.6, and the R is 1.17. Therefore, the two groups are sufficiently balanced.

4.4.2 Impacts on Labor Participation

The number of earners and the number of jobs in the household reflect the extensive aspect of the labor supply. An individual is defined as earner if he/she reported to having in the past year. The number of earners is the total number of earners in the household. Similarly, the number of jobs is the total number of jobs in the household. The second and the third row of Table 4.2 presents the treatment effects on extensive labor supply. The result indicates that the Labor Income Reward Plan has positive and significant impacts on the number of earners. The magnitudes and the significance of the estimates are largely consistent across the regressions. The magnitude

of the effect on the number of earners ranges from 0.29 persons to 0.48 persons per household. Although the estimated treatment effects on the number of jobs are positive, most of the estimates are statistically insignificant.

Table 4.2: Treatment Effects on Labor Supply and Earning (Leibo)

	PSM	**PSM-DD**	**DD**
Wage Income	3,745*	3,164	6,363***
# of Earners	0.29	0.45**	0.48***
# of Jobs	0.25	0.48	0.93***
Total Hours Worked	94	863	332
Farm	-0.25	-0.02	0.03
Farm Yield	-853	1,627	2,764*
Farm Profit	-877	-605	1,524**
Controls			Yes
Village FE			Yes
Sample Size	252	252	252

Figure C.1 presents the CDFs of changes in the number of earners and the number of jobs. There exist notable differences in both outcomes. Table C.1 presents the result of the Kolmogorov-Smirnov test for differences in CDFs between the treatment villages and the control villages. The results indicate that CDFs of changes in the number of earners and the number of jobs are statistically different. The distribution of changes in the number of earners and the changes in the number of jobs of the control villages dominate the treatment villages in both first and second order. This

suggestive evidence is consistent with the previous causal analysis.

4.4.3 Impacts on Hours Worked

The total hours worked reflects the intensity aspect of the labor supply. The variable is calculated based on four questions in the survey. Namely, household members were first asked how many months they had been working in the past year (2016 or 2017). Then, household members were asked, on average, how many weeks they had been working per month. Next, household members were asked, on average, how many days they usually work per week. Finally, household members were asked, on average, how many hours they usually worked per day in the past year. For each individual, hours worked is the product of the months worked in the past year, weeks worked per month, days worked per week, and hours worked per day. The total hours worked is the sum of hours worked for each individual in the household. Table 4.2 presents the treatment effect on total hours worked. The result indicates that the Labor Income Reward Plan increases the intensive labor supply. Namely, program participation increases total hours worked. However, none of the results are significant.

The left panel of Figure C.2 presents the CDFs of changes in total hours worked. There exist notable differences at the left tail. The test results in Table C.1 indicates that CDFs of changes in total hours worked are statistically different. The distribution of changes in total hours worked of the control villages dominates the treatment villages in both first and second order. This suggests that the EITC may have increased households' intensive labor supply.

4.4.4 Impacts on Earning

An income is considered to be wage income if it is earned via either employment and self-employment, including part-time and freelance jobs. The income that is earned via farming and business is not included since they are documented on the household-level. The total earning is the sum of the earning of each individual in the household. Earning is particularly important since the EITC incentivizes households directly through earning (instead of employment or working hours). The first row of Table 4.2 presents the treatment effects on household wage income. Most

estimates are positive and with some significance. The size of the effect ranges from 3,164 RMB to 6,363 RMB per year (or 263 RMB to 530 RMB per month) per household. Given that the previous finding suggests that program participation increases labor participation, this result is reasonable and consistent with the previous findings.

The right panel of Figure C.2 presents the CDFs of changes in total wage income. The differences between the treatment villages and the control villages are quite appealing. The test results in Table C.1 indicates that CDFs of changes in total wage income is statistically different. The distribution of changes in total wage income of the control villages dominates the treatment villages in both first and second order. This suggests that the EITC may have increased households' wage income.

4.4.5 Impacts on Farm Production

It is important to examine how households would change their production type for two reasons. First, households can choose to receive EITC either through earnings from employment, business, farming, or multiple ways. This gives me a unique opportunity to better understand how households decide productions. Second, Leibo Experiment was conducted in rural areas in China where farming is a major source of income. There are several reasons to expect that the rural population would react differently compared with their counterparts in urban areas. For instance, the employment opportunity in rural areas may not be as abundant as it is in urban areas. Thus, rural households might find it difficult to increase earning via additional employment. The last three rows in Table 4.2 presents the treatment effects on farm production. The first outcome is an indicator that identifies whether a household engages in farming only. Farm yield is the reported market value of the farm products, and farm profit is the market sales revenue less the cost of the input. The inconsistent and insignificant results imply that the policy has no effect on farm production.

Figure C.3 presents the CDFs of changes in farm yield and changes in farm profit. There seem to be some differences between the distributions of the treatment villages and the control villages, especially the left tail of the farm yield. The test results in Table C.1 indicate that CDFs of changes in farm yield is statistically different. The distribution of changes in farm yield of the control

villages dominates the treatment villages in both first and second order. This suggests that the EITC may have increased farm yield. However, the test results show that the CDFs of changes in farm profit are not statistically different from each other. This suggests that households reacted to the program primarily by increasing earning via employment.

4.5 Heterogeneous Treatment Effects

Table C.5 presents the heterogeneous treatment effects from the PSM-DD estimation. Here the sample is divided into subgroups based on different criteria. The high-income group is defined as households with a total income higher than the median. "High able-bodied" is defined as households with more than two (median) individuals between 16 and 65 years old who are able to work. "Multiple earners" means that households have more than two (median) individuals who have been working prior to the intervention. Results show that treatment effects vary across different subgroups. First, the treatment effects on labor supply and wage income are a lot larger among high-income households compared with others. This implies the poor households generally benefit less from the EITC compared with households with higher income. Households with more income also tend to increase more in terms of farm yield and farm profit. Second, the treatment effects on labor supply are a lot larger among households with fewer able-bodied persons. However, the households tend to earn less additional wage income compared with the ones with more able-bodied persons. This implies that the additional wage incomes are from very different jobs. For example, households with single labor might be forced to choose part-time jobs due to their time constraints, with which the wage is generally lower than full-time jobs. Similar patterns can be found between households with high and low earners, which is reasonable since the two categorizations are closely related. Finally, households with fewer able-bodied persons/earners tend to increase more in farm yield and farm profit.

4.6 Summary

Leibo Experiment was conducted by China Household Finance and Survey (CHFS) between 2017 and 2018. Four villages (390 households) participated in the experiment, of which two

villages were treated. The design employs a strong treatment intensity and a rigorous validation process.

Results show that Labor Income Reward Plan increases labor supply on the extensive margin. Estimates show that the number of earners per household has increased by 0.29 persons (12% increase), and the number of jobs has increased by 0.25 (9% increase). Consequently, households' monthly wage income (without the benefit) has increased by 263 RMB (38 USD) per month. These results are mostly consistent across different methods. Moreover, the size of the effects is very similar to the findings from the previous Wutongqiao Experiment and Mabian/Muchuan Experiment. Distribution tests suggest that the EITC may have increased intensive labor supply (total hours worked) and farm yield. The results suggest that EITC has achieved its designed purpose, which is to increase the labor supply and earning.

5. CONCLUSIONS

China currently has no in-work benefits nor social welfare programs such as the Earned Income Tax Credits (EITC) in the United States. Yet, the literature and global evidence have suggested EITC-type programs' potentials on poverty alleviation and work incentives. More importantly, the performance of current transfer programs is very unsatisfactory. An opportunity arises if China can substantially increase cash transfer. Hence, a comprehensive evaluation through experiments to evaluate an EITC-type program-a world-popular incentive-compatible cash transfer program that has long been proven effective and successful, has important policy implications.

Between 2014 and 2018, China Household Finance and Survey (CHFS) at Southwestern University of Finance and Economics conducted a series of experiments that aim to test the effects of an EITC-type policy-Labor Income Reward Plan. Each experiment has a different design and population focus.

The designs have several important and unique features. First, the experiments implement a relatively strong treatment intensity compared to the existing studies. Second, the designs employ a comprehensive measure to audit and validate each earning claim. Third, the EITC policies were offered with either other supplemental components or different schemes, which provides unique opportunities for researchers to better understand how EITC interacts with other typical anti-poverty policies in China.

Wutongqiao Experiment shows that Labor Income Reward Plan significantly increases labor supply significantly on both extensive margin and intensive margin. Namely, program participation has increased the number of earners per household by 0.32, and total hours worked has increased by 86 hours per month. It also significantly increases the household earning by 560 RMB (84 USD) per month. Finally, household expenditure has increased by 436 RMB (65 USD) per month. The effects are persistent throughout the intervention period.

Mabian/Muchuan Experiment shows that Labor Income Reward Plan increases labor supply significantly on both extensive margin and intensive margin. Estimates show that the number of

earners per household has increased by 0.30 persons (13% increase). The number of jobs per household has increased by 0.38 (27% increase) job per household. Total hours worked has increased by 77 hours per month (41% increase). Consequently, households' monthly wage income (without the benefit) has increased by 466 RMB (63 USD) per month. These results are consistent across different methods in terms of both magnitudes and significance. Moreover, distribution tests suggest that the EITC may have increased household expenditure in food and transportation and reduced expenditure in wedding and funeral.

Leibo Experiment shows that Labor Income Reward Plan increases labor supply on the extensive margin. Estimates show that the number of earners per household has increased by 0.29 persons (12% increase), and the number of jobs has increased by 0.25 (9% increase). Consequently, households' monthly wage income (without the benefit) has increased by 263 RMB (38 USD) per month. These results are mostly consistent across different methods. Moreover, distribution tests suggest that the EITC may have increased intensive labor supply (total hours worked) and farm yield.

Most importantly, three experiments yield similar results regardless of designs and methodologies (Table 5.1). Together, the EITC experiment series presents the first and unambiguously strong evidence that EITC has worked and achieved its designed purpose in China. First, EITC increases the labor supply both on the extensive and intensive margin. Second, EITC increases earning and work well as an anti-poverty policy. Finally, EITC will increase household consumption, especially in terms of food and education.

The dissertation yields important policy implications for governments and policymakers and contributes to the literature that studies the effects of incentive-compatible transfer programs using field experiments and non-experimental methods.

Table 5.1: Results Comparison Across Experiments

Treatment Effects	**Wutongqiao**	**Mabian/Muchuan**	**Leibo**
Bundle	EITC	EITC + Price Sub. + Kickstart	EITCs
Wage Income	559	466	263
Ttl Hours worked	86	77	-
# of Earners	0.32	0.30	0.29
# of Jobs	0.26	0.38	0.25
Sample Size	259	1,151	388

In addition, Wutongqiao Experiment was funded by donations before March 2017, after which the local government took it over and expended the program to all registered poor households in their entire jurisdiction. Appendix A.1 includes a photograph of an identification plate outside the enrolled household issued by the local ministry of civil affairs. Moreover, more local governments have joined the experiment with various plans. In 2019, there are additional 9,000 households newly enrolled in the program, and the total enrolled households have reached more than 72,000 (Table 5.2). The forthcoming evaluations and experiments studies about EITC in the future shall continue to help researchers and policymakers deepen the understanding and make improvements of EITC.

Table 5.2: Current On-going Experimental Programs in China

District		Start	Households Enrolled
Leshan City			
Wutongqiao Dist.	Pilot Study (Completed)	August, 2014	27
	Experiment (Completed)	November, 2015	103
	Full Implementation (On-going)	February, 2017	1,052
Mabian County	Gantianba Village (Completed)	April, 2017	64
	Boxiang Village (Completed)	April, 2017	122
	Houchi Village (On-going)	January, 2018	107
Muchuan County	Qianguang Village (Completed)	April, 2017	34
	Ciwan Village (Completed)	April, 2017	39
	Full Implementation (On-going)	January, 2018	68,000
Xiangcheng County, Ganzi	Shagong Village (Completed)	Septmber, 2017	127
(Minority Autonomous Prefecture)	Shagong, Baiyi, Reda (Implemented)	April, 2018	835
Leibo County, Liangshan	Moshi Village (On-going)	April, 2017	74
(Minority Autonomous Prefecture)	Shuikouba Village (On-going)	April, 2017	64
Shehong County	Suining City (Implemented)	April, 2018	1,400
Total			72,048

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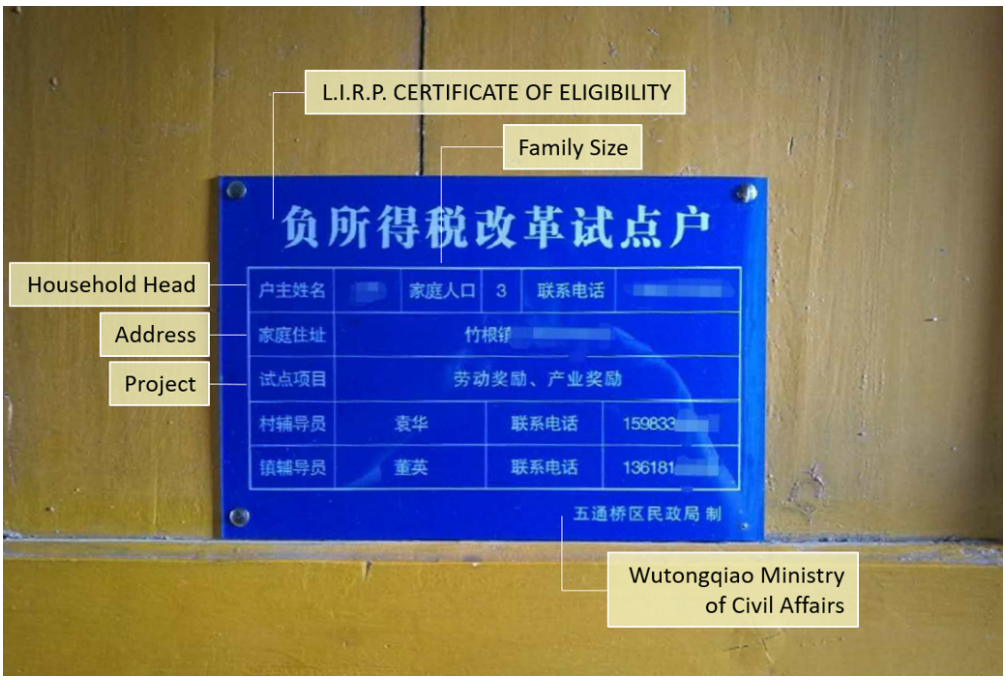
APPENDIX A

APPENDIX FOR CHAPTER 2

A.1 Photo



An Identification Plate Outside an Enrolled Household

A.2 Figures

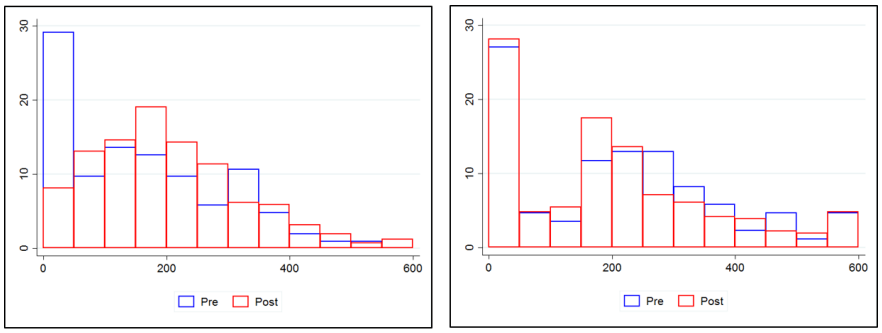


Figure A.1: Household Monthly Hours Worked Distribution

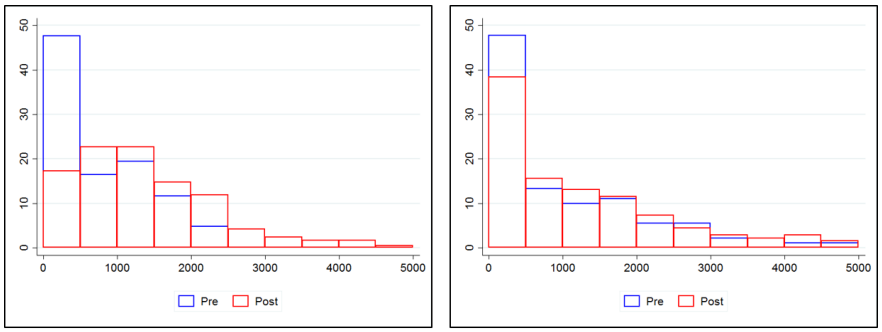


Figure A.2: Household Monthly Earning Distribution

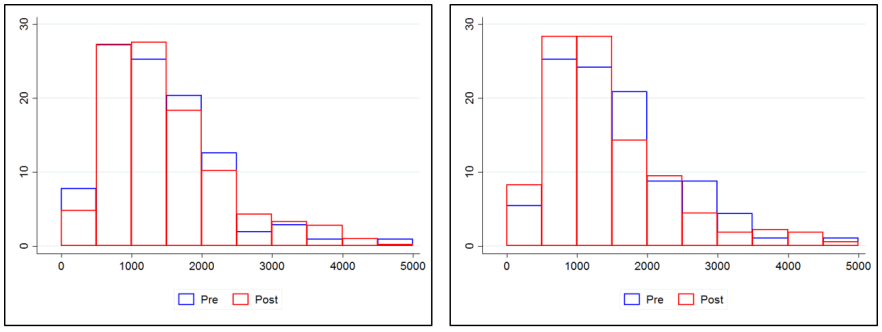


Figure A.3: Household Monthly Expenditure Distribution

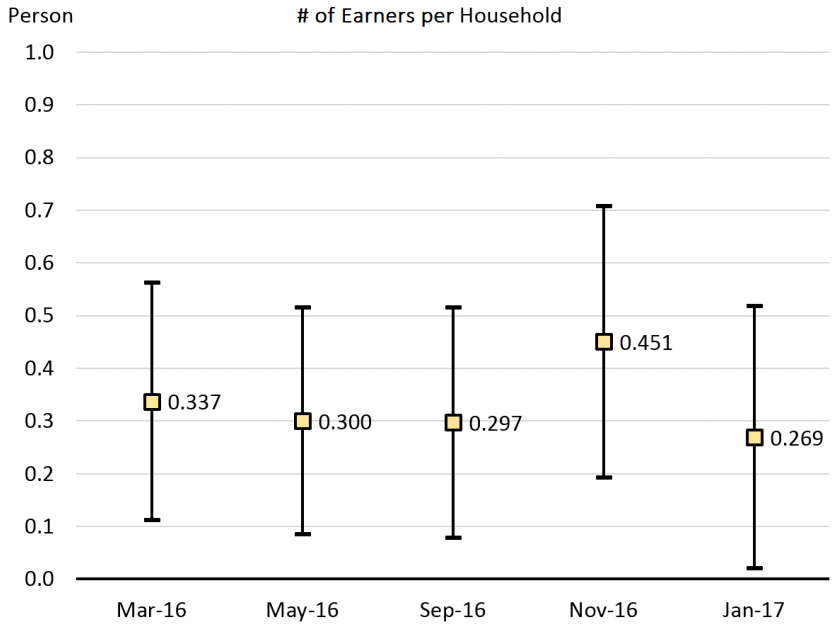


Figure A.4: Dynamic Treatment Effects on Number of Earners

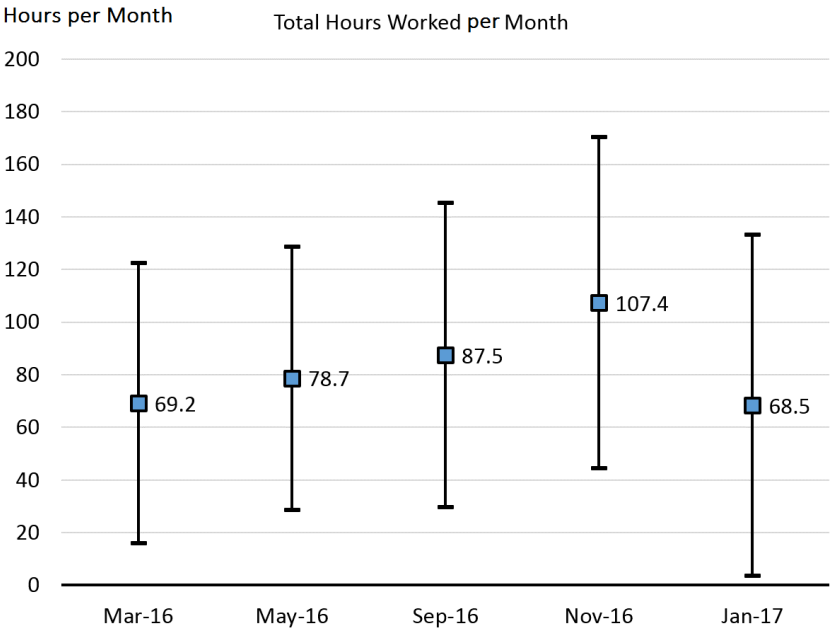


Figure A.5: Dynamic Treatment Effects on Household Total Hours Worked

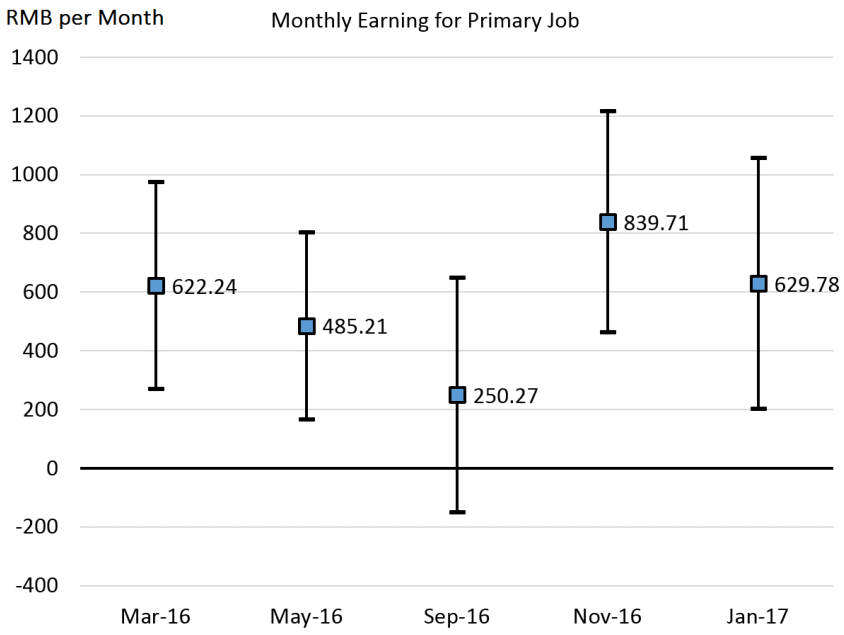


Figure A.6: Dynamic Treatment Effects on Household Earning

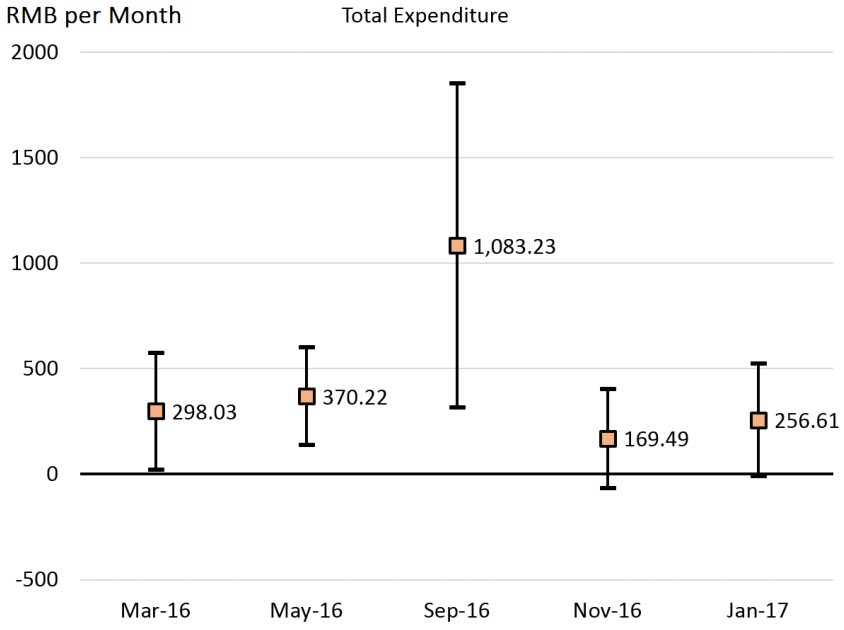


Figure A.7: Dynamic Treatment Effects on Household Total Expenditure

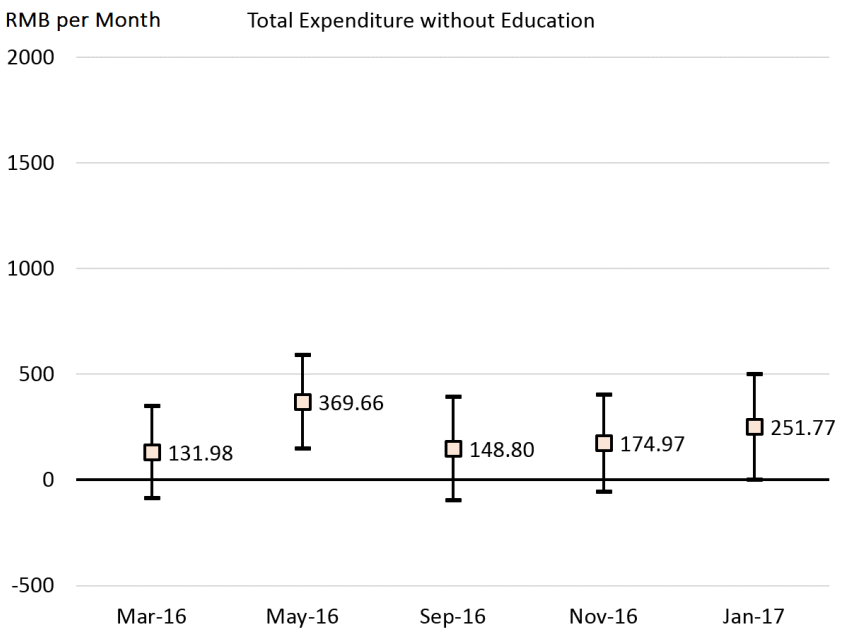


Figure A.8: Dynamic Treatment Effects on Household Total Expenditure (No Education)

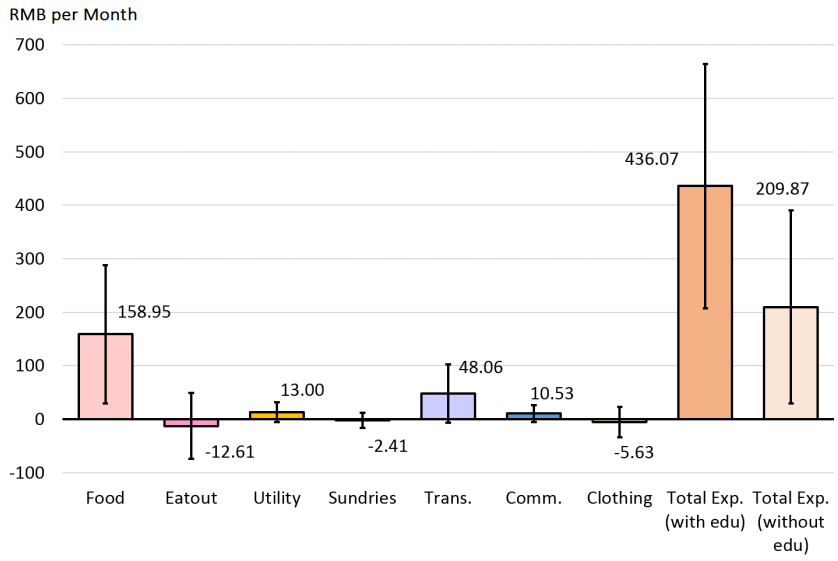


Figure A.9: Treatment Effects on Household Total Expenditure (No Education)

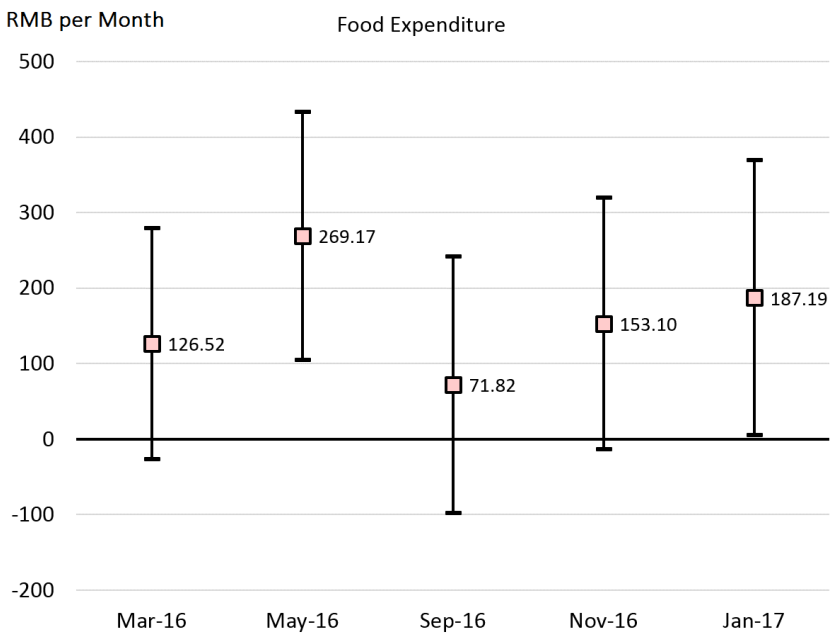


Figure A.10: Dynamic Treatment Effects on Food Expenditure

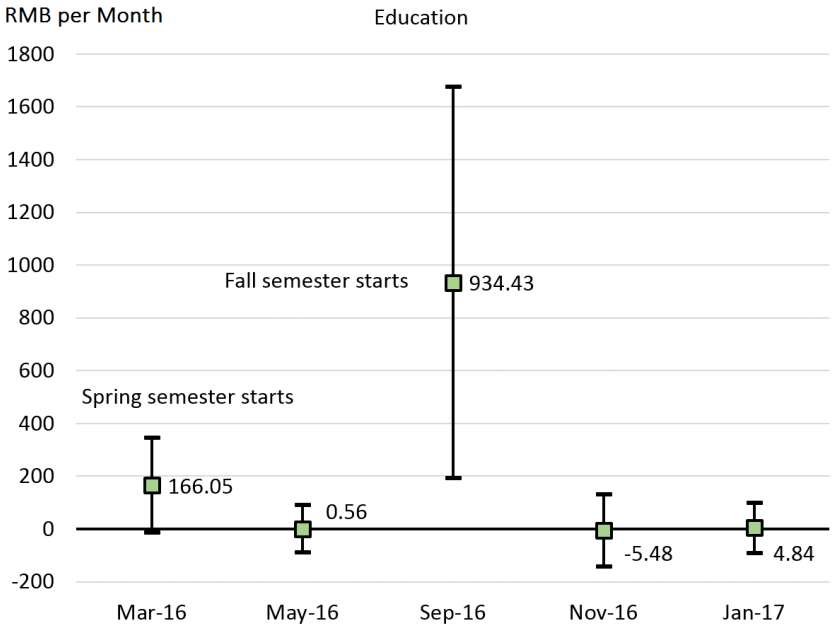


Figure A.11: Dynamic Treatment Effects on Education Expenditure

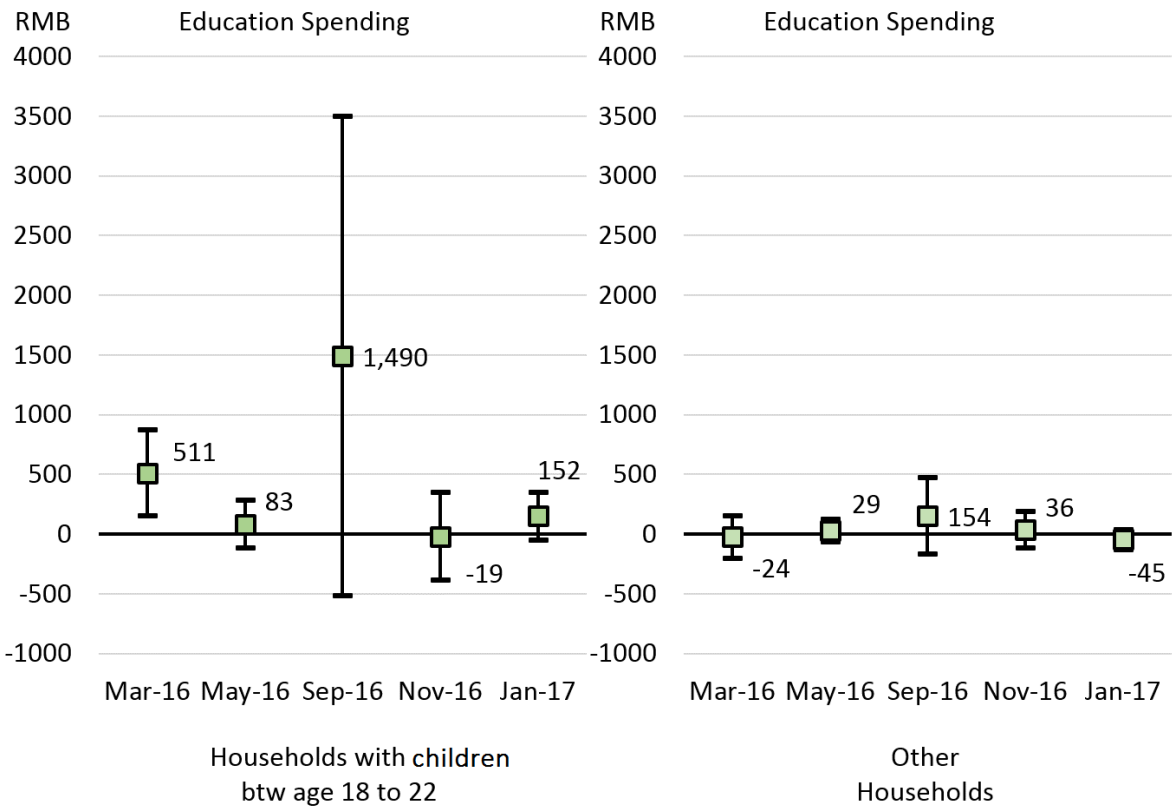


Figure A.12: Dynamic Treatment Effects on Education Expenditure (College vs. Others)

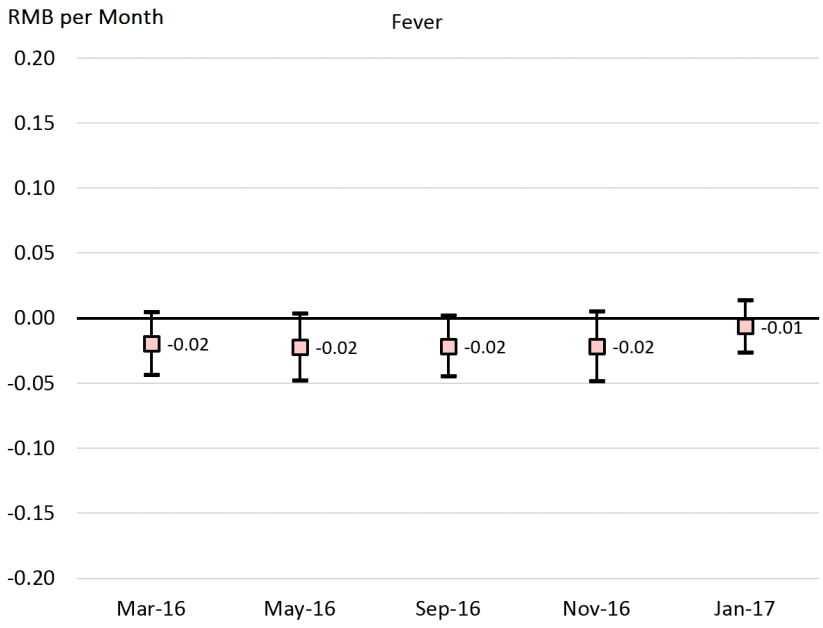


Figure A.13: Dynamic Treatment Effects on Claims of Fever

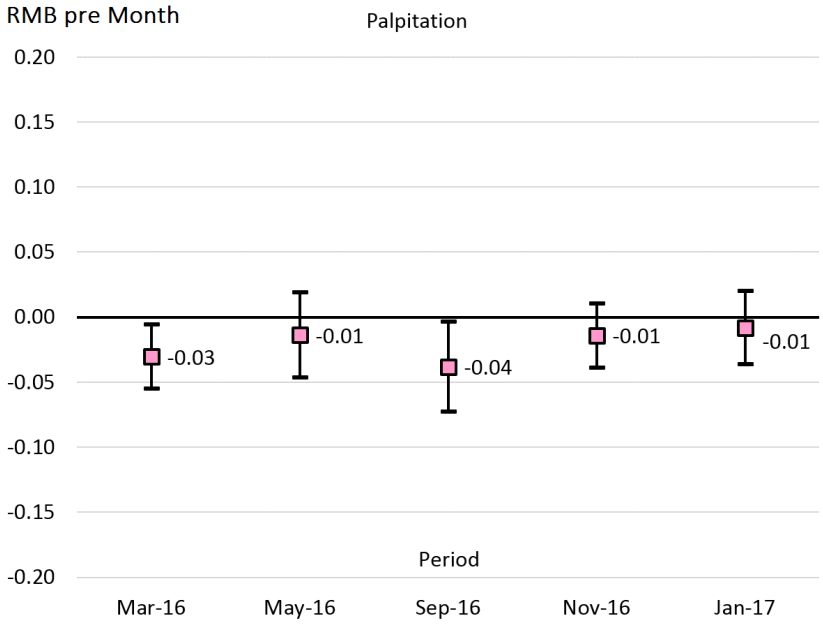


Figure A.14: Dynamic Treatment Effects on Claims of Palpitation

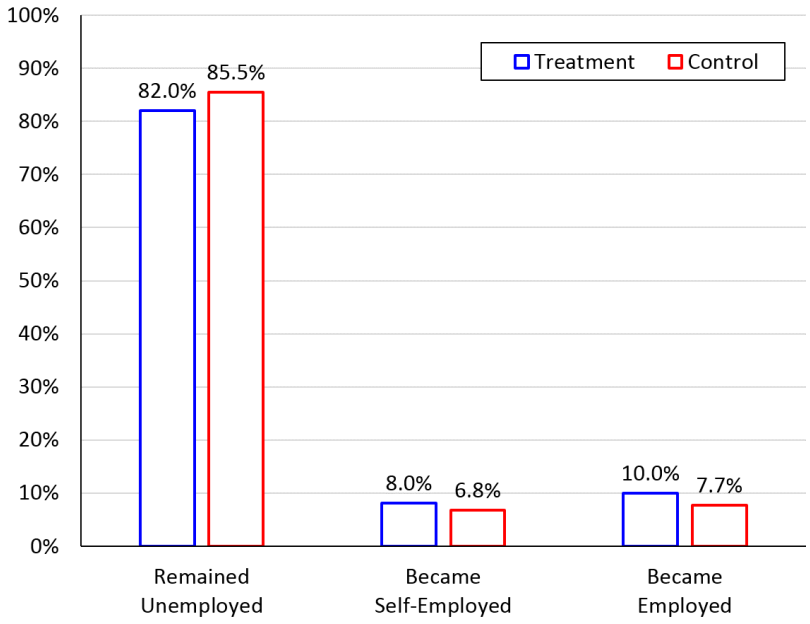


Figure A.15: Previously Unemployed End Up with (Treatment vs. Control)

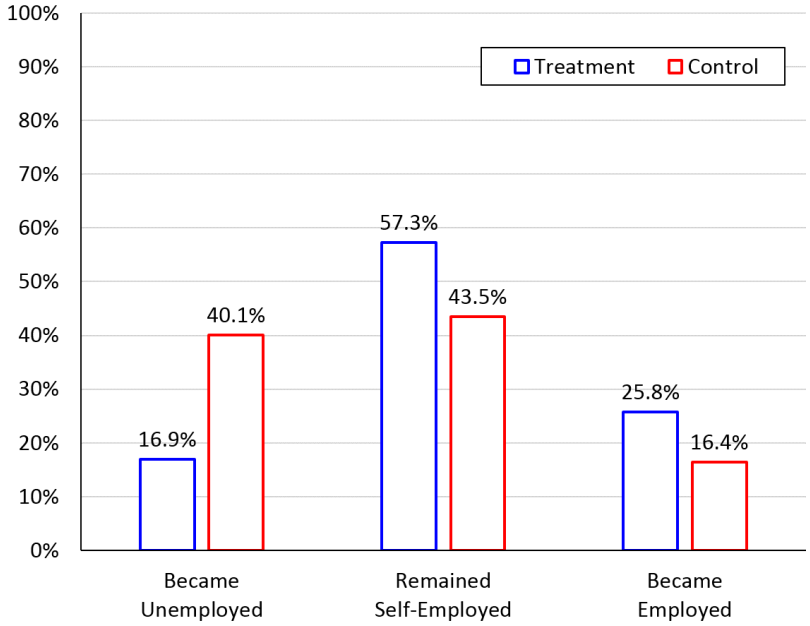


Figure A.16: Previously Self-employed End Up with (Treatment vs. Control)

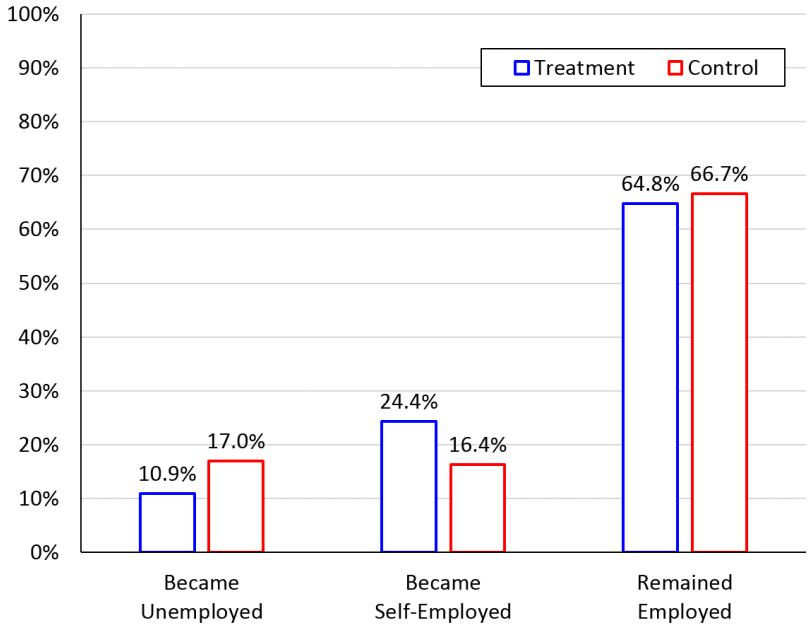


Figure A.17: Previously Employed End Up with (Treatment vs. Control)

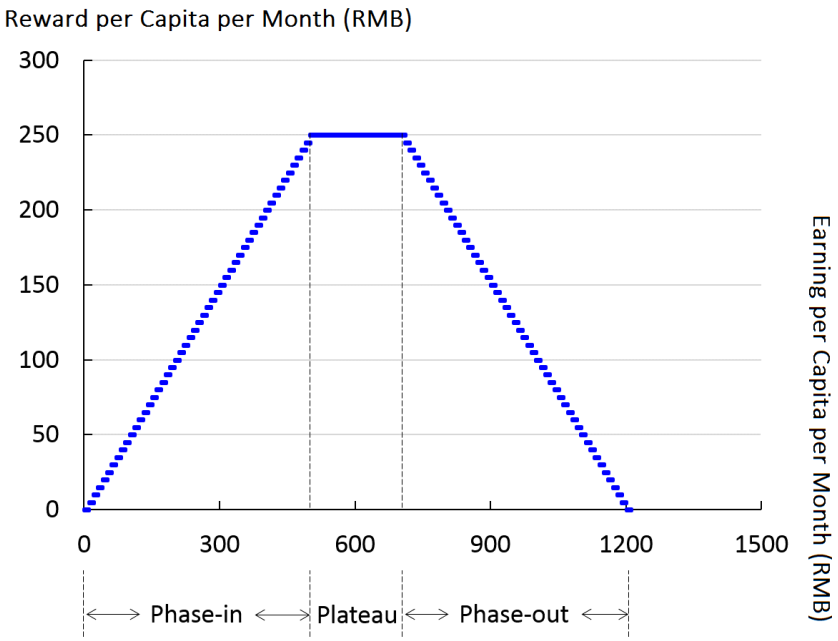


Figure A.18: Labor Income Reward Plan (The Pilot)

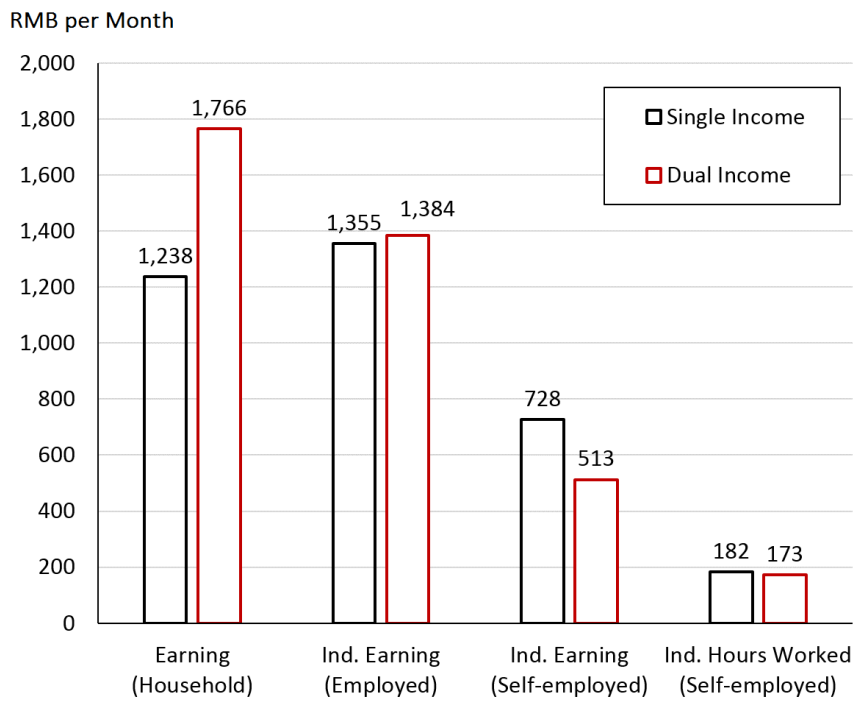


Figure A.19: Labor Division among the Dual-income Families

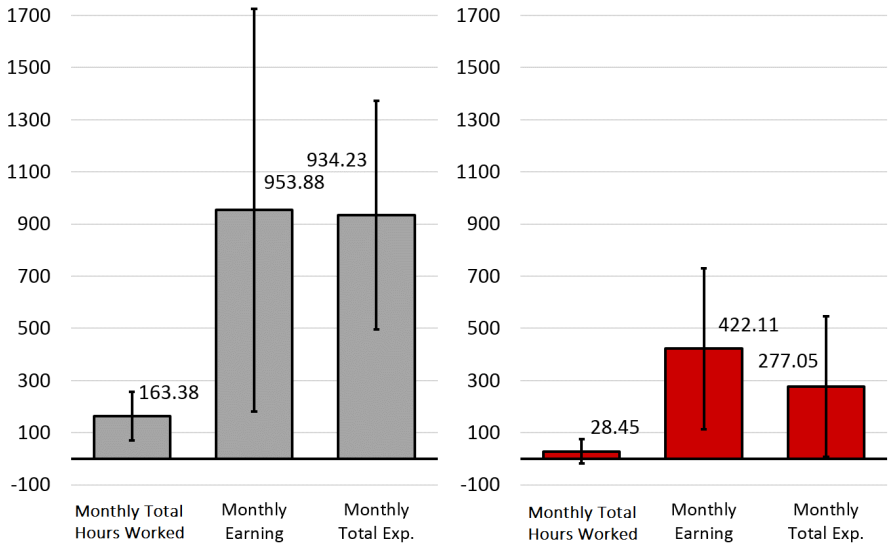


Figure A.20: Heterogeneous Treatment Effects (Dual-income vs. Non-Dual)

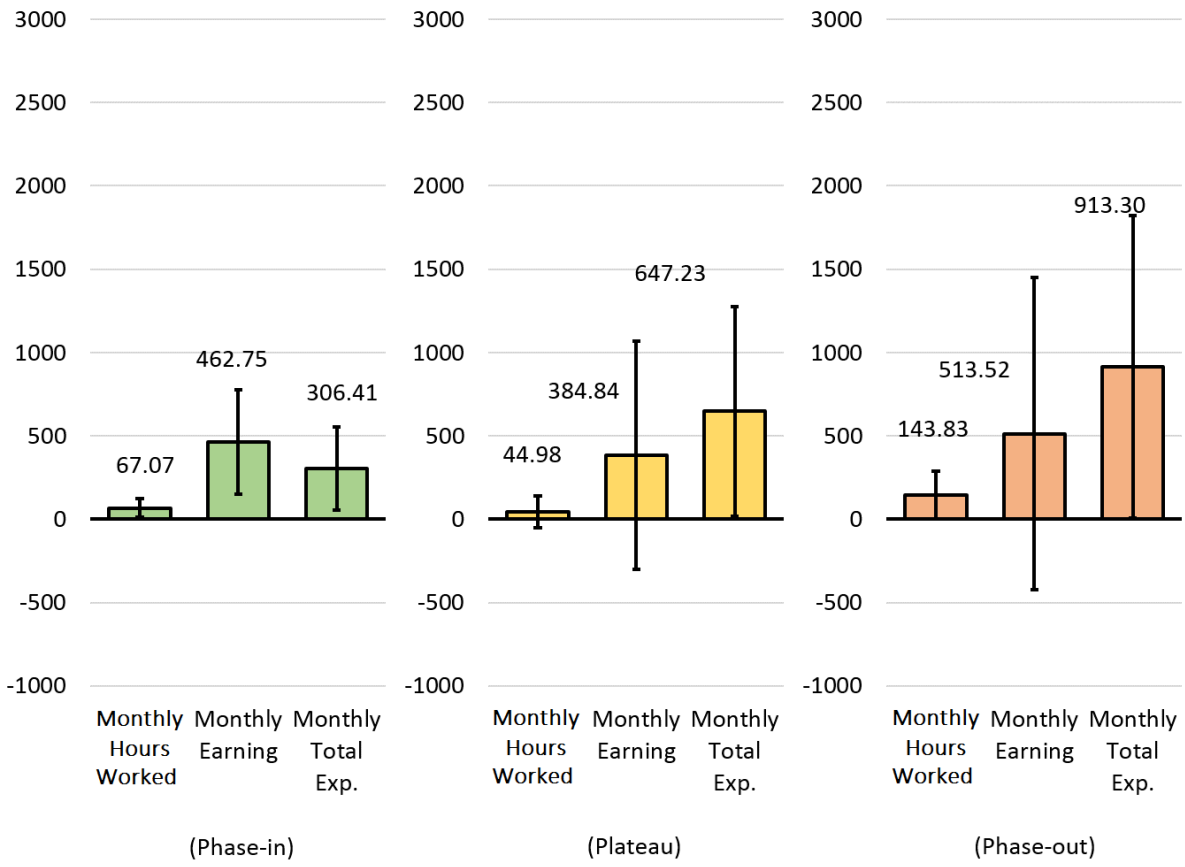


Figure A.21: Heterogeneous Treatment Effects (Phase-in vs. Plateau vs. Phase-out)

A.3 Tables

Table A.1: Treatment Effects on Household Labor Supply (a) (The Pilot, OLS)

	# of Earners per Household	Total Hours Worked	Monthly Hours Worked for Primary Job	Monthly Hours Worked for Secondary Job
treatment	0.336*	80.7**	105.1**	1.9
	(0.11)	(27.87)	(50.69)	(12.03)
N	53	53	53	53

*** p<0.01, ** p<0.05, * p<0.1; Standard errors in parentheses

Table presents the OLS estimates of the treatment effects on household labor supply using only the data post to the intervention in the pilot. Results show that households significantly increased the household total labor supply through monthly hours worked for the primary job. This is potentially due to the significant increase in the number of earners per household.

Table A.2: Treatment Effects on Household Labor Supply (b) (The Pilot, OLS)

	Primary Job			Secondary Job		
	Weeks Worked	Days Worked	Hours Worked	Weeks Worked	Days Worked	Hours Worked
treatment	1.68**	1.98**	3.10*	-0.12	-0.31	-0.20
	(0.45)	(0.72)	(1.10)	(0.12)	(0.20)	(0.28)
N	53	53	53	53	53	53

*** p<0.01, ** p<0.05, * p<0.1; Standard errors in parentheses

Table presents the OLS estimates of the treatment effects on household labor supply using only the data post to the intervention in the pilot. Results show that households significantly increased their weeks worked per month, days worked per week, and hours worked per day for the primary job.

Table A.3: Treatment Effects on Household Earning (The Pilot, OLS)

	Monthly Earning for Primary Job	Monthly Earning for Secondary Job	Farming Profit	Business Profit
treatment	189.50	66.67*	-31.25	0.00
	(317.68)	(30.63)	(38.71)	(218.45)
N	53	53	53	53

*** p<0.01, ** p<0.05, * p<0.1; Standard errors in parentheses

Table presents the OLS estimates of the treatment effects on household earning using only the data post to the intervention in the pilot. Results show that households significantly increased their earnings from the secondary job.

Table A.4: Balance of Covariate (The Pilot, Treatment vs. Control)

	Overall	Treatment	Control	T-C	p-value
Family Size	3.29	3.52	3.09	0.42	0.204
	(0.17)	(0.26)	(0.21)	(0.33)	
Number of Children	0.46	0.52	0.41	0.11	0.530
	(0.09)	(0.15)	(0.10)	(0.18)	
Number of Seniors	0.41	0.41	0.41	0.00	0.994
	(0.08)	(0.11)	(0.11)	(0.16)	
Household has Handicapped	0.02	0.00	0.03	-0.03	0.362
	(0.02)	(0.00)	(0.03)	(0.03)	
Number of Earners	1.32	1.26	1.38	-0.12	0.560
	(0.10)	(0.13)	(0.15)	(0.20)	
Weeks Worked Per Month	4.93	4.74	5.09	-0.35	0.664
	(0.40)	(0.52)	(0.60)	(0.81)	
Days Worked Per Weeks	7.63	7.29	7.90	-0.61	0.618
	(0.60)	(0.76)	(0.92)	(01.22)	
Hours Worked Per Day	10.67	10.41	10.91	-0.50	0.774
	(0.86)	(01.38)	(01.09)	(01.74)	
Household is Farming	0.03	0.00	0.06	-0.06	0.192
	(0.02)	(0.00)	(0.04)	(0.05)	
Household is Doing Business	0.31	0.37	0.25	0.12	0.325
	(0.06)	(0.09)	(0.10)	(0.12)	
Total Wage	1175.89	1210.48	1146.71	63.76	0.849
	(164.91)	(251.84)	(221.07)	(353.80)	
Total Pension	781.70	918.17	666.36	251.61	0.424
	(155.34)	(247.67)	(197.24)	(312.76)	
Total Earning Income	1507.83	1543.95	1477.34	66.61	0.827
	(150.14)	(233.92)	(197.43)	(303.88)	
Earning Per Capita	469.15	430.87	501.45	-70.57	0.358
	(37.93)	(44.85)	(58.86)	(76.22)	
Sundries	1492.11	1716.11	1303.12	412.98	0.071
	(114.30)	(194.53)	(125.59)	(224.89)	
Education	279.32	413.70	174.37	229.32	0.114
	(72.22)	(137.03)	(62.66)	(143.04)	
Total Expenditure	2493.42	3062.11	2013.54	1048.51	0.023
	(233.30)	(438.75)	(187.93)	(451.51)	
Sample Size	59	27	32		

Table presents the balance of covariates between the treatment group and the control group in the pilot sample. The first three columns present the means and the standard errors (in parentheses) for the whole sample, the treatment group, and the control group during the pre-intervention period. The fourth column presents the mean difference between the treatment group and the control group. The fifth column presents the p-value of the two-sample t-test. Labor supply (e.g., weeks worked per month) is calculated based on the jobs that provide primary earnings. There is no statistical difference between the two groups in terms of demographics, labor supply, earning, and expenditure. The randomization is successful. There is no extremely poor household in the pilot sample. The expenditure on food is not available in the pilot surveys.

Table A.5: Balance of Covariate (The Experiment, Always Answered vs. Others)

	Overall	Attritted	Non-Attritted	A-N	p-value
Family Size	2.91	3.00	2.84	0.16	0.270
	(0.07)	(0.12)	(0.09)	(0.15)	
Number of Children	0.36	0.34	0.38	-0.04	0.575
	(0.04)	(0.06)	(0.05)	(0.08)	
Number of Seniors	0.23	0.29	0.18	0.11	0.108
	(0.03)	(0.06)	(0.04)	(0.07)	
Household has Handicapped	0.04	0.01	0.06	-0.05	0.088
	(0.01)	(0.01)	(0.02)	(0.03)	
Extreme Poverty	0.02	0.02	0.01	0.01	0.469
	(0.01)	(0.02)	(0.01)	(0.02)	
Number of Earners	1.12	1.15	1.11	-0.04	0.709
	(0.06)	(0.10)	(0.06)	(0.12)	
Weeks Worked Per Month	3.89	3.98	3.81	0.17	0.664
	(0.21)	(0.35)	(0.24)	(0.41)	
Days Worked Per Weeks	6.48	6.40	6.38	0.21	0.752
	(0.34)	(0.59)	(0.40)	(0.68)	
Hours Worked Per Day	9.03	9.29	8.80	0.49	0.632
	(0.51)	(0.91)	(0.55)	(01.03)	
Household is Farming	0.11	0.13	0.10	0.04	0.388
	(0.02)	(0.04)	(0.03)	(0.05)	
Household is Doing Business	0.16	0.13	0.18	0.05	0.384
	(0.03)	(0.04)	(0.04)	(0.05)	
Total Wage	701.96	656.77	755.28	98.51	0.514
	(75.04)	(83.06)	(131.34)	(150.81)	
Total Pension	246.80	309.62	193.55	116.07	0.257
	(50.87)	(76.50)	(67.93)	(102.00)	
Total Earning Income	820.25	871.85	776.52	95.33	0.520
	(74.17)	(131.33)	(80.34)	(149.08)	
Earning Per Capita	271.57	274.87	268.76	06.11	0.892
	(22.26)	(37.04)	(26.73)	(44.79)	
Food	970.48	1034.50	916.21	118.28	0.143
	(40.21)	(65.19)	(49.34)	(80.46)	
Sundries	55.37	58.51	53.09	05.42	0.512
	(04.11)	(07.07)	(04.68)	(08.26)	
Education	79.79	73.65	85.00	-11.35	0.685
	(13.89)	(18.49)	(20.40)	(27.94)	
Total Expenditure	1454.67	1498.24	1436.22	62.02	0.607
	(60.02)	(80.24)	(80.24)	(120.68)	
Sample Size	189	95	94		

Table presents the balance of covariates of the experiment sample between households that answered all of the surveys and other households. The first three columns present the means and the standard errors (in parentheses) for the whole sample, households that did not respond to the survey at least once, and households that responded to all of the surveys during the pre-intervention period. The fourth column presents the mean difference between the last two groups. The fifth column presents the p-values of the two-sample t-test. All covariates are aggregated to the household-level. Wage income only includes earnings from employment. Earning includes earnings from employment, self-employment, farming, and business. Labor supply (e.g., weeks worked per month) is calculated based on the jobs that provide primary earnings. The statistics in this table imply that there is no statistical difference between households that did not respond to the survey at least once and households that responded to all of the surveys during the pre-intervention period.

Table A.6: Balance of Covariate (The Experiment, Treatment vs. Control)

	Overall	Treatment	Control	T-C	p-value
Family Size	2.91 (0.07)	2.90 (0.10)	2.92 (0.11)	-0.02 (0.15)	0.890
Number of Children	0.36 (0.04)	0.37 (0.06)	0.35 (0.05)	0.02 (0.08)	0.825
Number of Seniors	0.23 (0.03)	0.18 (0.04)	0.29 (0.05)	-0.10 (0.07)	0.143
Household has Handicapped	0.04 (0.01)	0.04 (0.02)	0.03 (0.02)	-0.01 (0.03)	0.582
Extreme Poverty	0.02 (0.01)	0.02 (0.01)	0.01 (0.02)	0.01 (0.02)	0.637
Number of Earners	1.12 (0.05)	1.03 (0.07)	1.23 (0.09)	-0.20 (0.11)	0.066
Weeks Worked Per Month	3.89 (0.21)	3.39 (0.23)	4.45 (0.34)	-0.11 (0.41)	0.009
Days Worked Per Weeks	6.48 (0.34)	5.75 (0.39)	7.31 (0.56)	-1.56 (0.67)	0.020
Hours Worked Per Day	9.03 (0.51)	7.62 (0.52)	10.62 (0.89)	-2.99 (01.00)	0.003
Household is Farming	0.11 (0.02)	0.14 (0.00)	0.09 (0.03)	0.05 (0.05)	0.295
Household is Doing Business	0.16 (0.03)	0.18 (0.04)	0.13 (0.04)	0.05 (0.05)	0.320
Total Wage Income	701.96 (75.04)	498.15 (67.88)	932.64 (136.81)	434.49 (147.45)	0.003
Total Pension	246.79 (50.86)	190.61 (60.71)	310.39 (83.75)	-119.78 (101.82)	0.240
Total Earning	820.25 (74.17)	660.07 (66.89)	1001.55 (136.89)	-341.47 (146.96)	0.021
Earning Per Capita	271.56 (22.26)	228.42 (22.31)	320.39 (39.70)	-91.96 (44.22)	0.038
Food	970.48 (40.21)	904.46 (50.53)	1045.20 (63.25)	-140.74 (80.15)	0.080
Sundries	55.57 (04.11)	57.98 (05.53)	52.85 (06.14)	05.13 (08.25)	0.534
Education	79.79 (13.89)	93.25 (20.85)	64.56 (17.88)	28.69 (27.83)	0.304
Total Expenditure	1464.67 (60.02)	1424.07 (80.28)	1510.63 (90.32)	-86.55 (120.42)	0.473
Sample Size	194	103	91		

Table presents the balance of covariates of the experiment sample between the actual treatment group and the actual control group. The first three columns present the means and the standard errors (in parentheses) for the whole sample, the actual treatment group, and the actual control group during the pre-intervention period. The fourth column presents the mean difference between the last two groups. The fifth column presents the p-value of the two-sample t-test. All covariates are aggregated to the household-level. Wage income only includes earnings from employment. Earning includes earnings from employment, self-employment, farming, and business. Labor supply (e.g., weeks worked per month) is calculated based on the jobs that provide primary earnings. The statistics in this table implies potential selection bias.

Table A.7: Balance of Covariate (Instrument, Treatment vs. Control)

	Overall	Treatment	Control	T-C	p-value
Total Wage	661.29 (73.44)	571.26 (78.59)	752.27 (124.23)	-181.00 (146.67)	0.219
Total Pension	253.32 (52.13)	205.20 (301.96)	301.96 (81.21)	-96.76 (104.30)	0.354
Total Earning	782.70 (72.86)	737.35 (76.53)	828.54 (124.68)	-91.20 (145.95)	0.533
Earning Per Capita	254.86 (20.94)	253.06 (25.59)	256.68 (33.37)	-03.62 (42.00)	0.931
Number of Earners	01.11 (0.05)	01.08 (0.07)	01.14 (0.08)	-0.06 (0.11)	0.564
Weeks Worked Per Month	03.79 (0.20)	03.51 (0.24)	04.07 (0.34)	-0.56 (0.42)	0.181
Days Worked Per Weeks	06.33 (0.34)	05.94 (0.41)	06.73 (0.55)	-0.79 (0.69)	0.254
Hours Worked Per Day	08.86 (0.55)	08.06 (0.58)	09.68 (0.92)	-01.62 (01.09)	0.140
Household is Farming	0.12 (0.02)	0.14 (0.04)	0.10 (0.03)	0.04 (0.05)	0.381
Household is Doing Business	0.29 (0.07)	02.96 (0.11)	02.88 (0.10)	0.09 (0.15)	0.569
Family Size	02.92 (0.07)	02.97 (0.11)	02.88 (0.10)	0.09 (0.15)	0.569
Number of Children	0.36 (0.04)	0.37 (0.06)	0.35 (0.05)	0.02 (0.08)	0.827
Number of Seniors	0.23 (0.04)	0.19 (0.05)	0.28 (0.05)	-0.09 (0.07)	0.215
Household has Handicapped	0.04 (0.01)	0.05 (0.02)	0.02 (0.01)	0.03 (0.03)	0.256
Extreme Poverty	0.02 (0.01)	0.02 (0.01)	0.01 (0.01)	-0.01 (0.02)	0.569
Food	954.62 (39.95)	917.05 (54.01)	992.59 (58.95)	-75.54 (79.92)	0.346
Sundries	55.70 (04.19)	59.92 (05.81)	51.43 (06.04)	8.48 (08.38)	0.313
Education	77.67 (13.95)	97.00 (22.38)	58.14 (16.49)	38.86 (27.87)	0.165
Total Expenditure	1446.90 (60.64)	1458.68 (86.00)	1435.00 (85.97)	23.69 (121.60)	0.846
Sample Size	189	95	94		

Table presents the balance of covariates of the experiment sample across initial treatment status. The first three columns present the means and the standard errors (in parentheses) for the whole sample, the initial treatment group, and the initial control group during the pre-intervention period. The fourth column presents the mean difference between the treatment group and the control group. The fifth column presents the p-value of the two-sample t-test. All covariates are aggregated to the household-level. Wage income only includes earnings from employment. Earning includes earnings from employment, self-employment, farming, and business. Labor supply (e.g., weeks worked per month) is calculated based on the jobs that provide primary earnings. The statistics in this table implies that there is no statistical difference between the households that were initially in the treatment group and the those that were initially in the control group. The initial randomization is successful.

Table A.8: First-stage Regression (Household-level)

treated	**Coef.**	**Robust Std. Err.**	**t**	**P> |t|**
treated0	0.8637	0.0386	22.4000	0.0000
t2	0.1040	0.0333	3.1300	0.0020
t3	0.1008	0.0332	3.0400	0.0030
t4	0.0979	0.0324	3.0300	0.0030
t5	0.1005	0.0333	3.0200	0.0030
t6	0.1045	0.0342	3.0600	0.0030
_cons	-0.0035	0.0145	-0.2400	0.8100
N	897	**F-statisic**	=	416.53
# of Households	189	**Prob>F**	=	0.0000

Table presents the estimation result of the first-stage regression using household-level data. Here, the dependent variable (treated) is the interaction of the treatment group indicator and the post-treatment indicator that equals one if the panel is observed post to the intervention. The main independent variable (treated0) is the instrument, which is the interaction of the initial treatment group indicator and the post-treatment indicator. Variables t2 to t6 are the time dummies. t1 is dropped. The first-stage regression shows the F-statistic is 416.53, which is relatively large compared to the 10 in "Rule of Thumb" by Staiger and Stock (1994). This implies the instrument predicts the endogenous variable well.

Table A.9: First-stage Regression (Household-level, Dynamic Treatment Effects)

Variable	**F-statistic**	**p-value**
treat_t2	162.17	0.0000
treat_t3	314.32	0.0000
treat_t4	483.52	0.0000
treat_t5	247.04	0.0000
treat_t6	93.02	0.0000
Kleibergen-Paap rk LM statistic	**(Underidentification test)**	96.396
Cragg-Donald Wald F-statistic	**(Weak indentification test)**	376.259

Table presents the estimation result of the first-stage regressions using household-level data. Here, the dependent variables (treat_t2 to treat_t6) are the interactions of $Treatment_i$ and $I_t(time = t)$. $Treatment_i$ is a dummy variable that equals one if household i is actually assigned to the treatment group. $I_t(time = t)$ is a dummy variable that equals one if the data is observed in time t. With regressions in the first-stage estimation, I use two additional tests for weak instruments, which are more appropriate when there are multiple endogenous variables. Both tests show that the instruments predict the endogenous variables well in the new first-stage estimation. Please see Kleibergen and Paap (2006) and Stock and Yogo (2002) for discussion.

Table A.10: Treatment Effects on Household Labor Supply (a) (The Experiment, IV)

	# of Earners per Household	Total Hours Worked	Monthly Hours Worked for Primary Job	Monthly Hours Worked for Secondary Job
treated	0.329***	80.7***	86.0***	-5.3
	(0.11)	(27.87)	(27.54)	(4.77)
Household FE	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes
# of Households	174	174	174	174
N	882	882	882	882

*** p<0.01, ** p<0.05, * p<0.1; Standard errors in parentheses

Table presents the IV estimates of the treatment effects on household labor supply using data prior and post to the intervention in the experiment. Results show that households significantly increased the number of earners, total hours worked, and monthly hours worked for the primary job.

Table A.11: Treatment Effects on Household Labor Supply (b) (The Experiment, IV)

	Primary Job			Secondary Job		
	Weeks Worked	Days Worked	Hours Worked	Weeks Worked	Days Worked	Hours Worked
treated	1.68***	1.98***	3.10***	-0.12	-0.31	-0.20
	(0.45)	(0.72)	(1.10)	(0.12)	(0.20)	(0.28)
Household FE	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes
# of Households	174	174	174	174	174	174
N	882	882	882	882	882	882

*** p<0.01, ** p<0.05, * p<0.1; Standard errors in parentheses

Table presents the IV estimates of the treatment effects on household labor supply using data prior and post to the intervention in the experiment. Results show that households significantly increased their weeks worked per month, days worked per week, and hours worked per day for the primary job.

Table A.12: Treatment Effects on Household Earning (The Experiment, IV)

	Monthly Earning for Primary Job	Monthly Earning for Secondary Job	Farming Profit	Business Profit
treated	559.77***	-14.34	0.33	23.23
	(174.83)	(13.22)	(23.11)	(59.74)
Household FE	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes
# of Households	174	174	174	174
N	882	882	882	882

*** p<0.01, ** p<0.05, * p<0.1; Standard errors in parentheses

Table presents the IV estimates of the treatment effects on household earning using data prior and post to the intervention in the experiment. Results show that households significantly increased their earnings from the primary job.

Table A.13: Treatment Effects on Household Expenditure (a) (The Experiment, IV)

	Food	Eatout	Utility	Sundries	Transportation
treated	158.95**	-12.61	13.00	-2.41	48.06
	(78.43)	(37.46)	(11.29)	(8.66)	(33.07)
Household FE	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes
# of Households	174	174	174	174	174
N	882	882	882	882	882

*** p<0.01, ** p<0.05, * p<0.1; Standard errors in parentheses

Table presents the IV estimates of the treatment effects on household expenditure using data prior and post to the intervention in the experiment. Results show that households significantly increased expenditure on food.

Table A.14: Treatment Effects on Household Expenditure (b) (The Experiment, IV)

	Education	Communication	Clothing	Total Exp. (with edu.)	Total Exp. (without edu.)
treated	226.20**	10.53	-5.63	436.07***	209.87**
	(101.78)	(9.39)	(17.29)	(138.74)	(109.59)
Household FE	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes
# of Households	174	174	174	174	174
N	882	882	882	882	882

*** p<0.01, ** p<0.05, * p<0.1; Standard errors in parentheses

Table presents the IV estimates of the treatment effects on household expenditure using data prior and post to the intervention in the experiment. Results show that households significantly increase expenditure on education. Total expenditure increased significantly with or without considering expenditure on education.

Table A.15: Treatment Effects on Health (The Experiment, IV)

	Discomfort	Fever	Pain	Diarrhea	Cough	Palpitation
treated	-0.023	-0.019*	0.000	0.006	-0.005	-0.023*
	(0.050)	(0.010)	(0.038)	(0.005)	(0.017)	(0.012)
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes
# of Individual	531	531	531	531	531	531
N	2,562	2,562	2,562	2,562	2,562	2,562

*** p<0.01, ** p<0.05, * p<0.1; Standard errors in parentheses

Table presents the IV estimates of the treatment effects on individual health using data prior and post to the intervention in the experiment. Results show that households significantly decreased the incidence of fever and palpitation.

Table A.16: Dynamic Treatment Effects on Household Labor Supply (The Experiment, IV)

	# of Earners per Household	Total Hours Worked per Month	Monthly Hours Worked Primary Job	Monthly Hours Worked for Secondary Job	Weeks Worked per Month (Primary Job)	Days Worked per Month (Primary Job)	Hours Worked per Day (Primary Job)	Weeks Worked per Month (Secondary Job)	Days Worked per Month (Secondary Job)	Hours Worked per Day (Secondary Job)
treat_t2	0.34**	69.20**	71.73**	-2.53	1.56***	2.09**	2.77**	-0.05	-1.58	-0.06
	(0.14)	(32.34)	(32.05)	(5.50)	(0.53)	(0.87)	(1.30)	(0.14)	(0.24)	(0.32)
treat_t3	0.30**	78.70***	84.06***	-5.36	1.58***	1.66**	3.16**	-0.13	-0.33*	-0.21
	(0.13)	(30.39)	(29.95)	(4.15)	(0.51)	(0.81)	(1.30)	(0.12)	(0.18)	(0.29)
treat_t4	0.30**	87.53**	91.70***	-4.17	1.67***	1.76**	2.87**	-0.06	-0.21	-0.11
	(0.13)	(35.08)	(35.07)	(5.08)	(0.56)	(0.90)	(1.30)	(0.13)	(0.20)	(0.31)
treat_t5	0.45***	107.42***	115.28***	-7.86	2.29***	2.60***	4.45***	-0.20	-0.49*	-0.34
	(0.16)	(38.22)	(37.86)	(5.16)	(0.65)	(1.01)	(1.44)	(0.13)	(0.27)	(0.33)
treat_t6	0.27*	68.46*	76.8***	-8.35	1.44**	1.85**	2.53**	-0.23*	-0.47**	-0.37
	(0.15)	(39.41)	(39.11)	(5.18)	(0.61)	(1.01)	(1.45)	(0.13)	(0.24)	(0.28)
Household FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
# of Households	174	174	174	174	174	174	174	174	174	174
N	882	882	882	882	882	882	882	882	882	882

*** p<0.01, ** p<0.05, * p<0.1; Standard errors in parentheses

Table presents the IV estimates of the dynamic treatment effects on household labor supply using data prior and post to the intervention in the experiment. Results show that households significantly increased the number of earners per household, total hours worked, and monthly hours worked for the primary job. These effects are persistent throughout the intervention period. Moreover, there are increases in all components in terms of labor supply for the primary job, which implies the increase in labor supply is largely driven by the increase in the number of earners.

Table A.17: Dynamic Treatment Effects on Household Earning and Expenditure (The Experiment, IV)

	Monthly Earning for Primary Job	Monthly Earning for Secondary Job	Farming Profit	Business Profit	Food Expenditure	Eating-Out	Utility	Sundries	Transportation	Communication	Clothing	Education	Total Expenditure	Total Expenditure without Education
treat_t2	622.24***	-10.81	2.11	-108.47	126.52	-46.56	7.46	3.38	55.56	3.63	-18.01	166.05	298.03**	131.98
	(213.62)	(14.18)	(20.80)	(70.35)	(93.19)	(50.18)	(15.51)	(11.86)	(47.19)	(10.17)	(21.86)	(108.74)	(168.97)	(131.96)
treat_t3	485.21**	-18.54	30.87	78.10	269.17***	23.37	27.24*	-5.10	54.23*	17.02	-16.27	0.56	370.22*	369.66***
	(193.95)	(13.02)	(63.23)	(97.27)	(100.11)	(48.88)	(15.88)	(11.76)	(31.41)	(10.78)	(18.54)	(54.47)	(141.37)	(133.93)
treat_t4	250.27	-13.27	-14.86	130.06	71.82	23.63	20.04	-24.99	46.00	7.15	5.14	934.43**	1083.23**	148.80
	(242.67)	(14.10)	(20.73)	(85.88)	(103.22)	(45.34)	(14.65)	(18.31)	(34.15)	(11.56)	(22.80)	(451.44)	(467.13)	(148.13)
treat_t5	839.71***	-12.80	-2.43	27.22	153.10	-47.74	-2.17	9.43	36.93	15.15	10.28	-5.48	169.49	174.97
	(228.50)	(14.75)	(24.61)	(98.04)	(101.22)	(43.02)	(15.62)	(10.42)	(28.88)	(11.01)	(21.30)	(83.45)	(143.08)	(139.37)
treat_t6	629.78**	-17.79	-18.01	46.67	187.19*	-7.43	11.84	5.63	41.74	13.5	-0.70	4.84	256.61	251.77*
	(259.94)	(13.61)	(31.45)	(76.96)	(110.76)	(46.12)	(16.57)	(10.67)	(32.82)	(13.77)	(22.35)	(57.77)	(162.09)	(152.17)
Household FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
# of Households	174	174	174	174	174	174	174	174	174	174	174	174	174	174
N	882	882	882	882	882	882	882	882	882	882	882	882	882	882

*** p<0.01, ** p<0.05, * p<0.1; Standard errors in parentheses

Table presents the IV estimates of the dynamic treatment effects on household earning and expenditure using data prior and post to the intervention in the experiment. Results show that households significantly increase monthly earning for the primary job. The effect is persistent throughout the intervention period.

Table A.18: First-stage Regression (Individual-level)

treated	**Coef.**	**Robust Std. Err.**	**t**	**P>|t|**
treated0	0.871684	0.038559	22.40	0.000
t2	0.092944	0.033260	3.13	0.002
t3	0.092605	0.033186	3.04	0.003
t4	0.090101	0.032353	3.03	0.003
t5	0.091293	0.033270	3.02	0.003
t6	0.097394	0.034150	3.06	0.003
_cons	-0.004983	0.014073	-0.35	0.724
N	2628	**F-statisic**	=	434.71
# of Individual	597	**Prob>F**	=	0.0000

Table presents the estimation result of the first-stage regression using the individual-level data. Here, the dependent variable (treated) is the interaction of the treatment group indicator and the post-treatment indicator that equals one if the panel is observed post to the intervention. The main independent variable (treated0) is the instrument, which is the interaction of the initial treatment group indicator and the post-treatment indicator. Variables t2 to t6 are the time dummies. t1 is dropped. The first-stage regression shows the F-statistic is 434.71, which is relatively large compared to the 10 in "Rule of Thumb" by Staiger and Stock (1994). This implies the instrument predicts the endogenous variable well.

Table A.19: First-stage Regression (Individual-level, Dynamic Treatment Effect)

Variable	**F-statistic**	**p-value**
treat_t2	258.23	0.0000
treat_t3	179.26	0.0000
treat_t4	260.73	0.0000
treat_t5	190.68	0.0000
treat_t6	108.18	0.0000
Kleibergen-Paap rk LM statistic	**(Underidentification test)**	96.396
Cragg-Donald Wald F-statistic	**(Weak indentification test)**	376.259

Table presents the estimation result of the first-stage regressions using individual-level data. Here, the dependent variables (treat_t2 to treat_t6) are the interactions of $Treatment_i$ and $I_t(time = t)$. $Treatment_i$ is a dummy variable that equals one if individual i is actually assigned to the treatment group. $I_t(time = t)$ is a dummy variable that equals one if the data is observed in time t. With regressions in the first-stage estimation, I use two additional tests for weak instruments, which are more appropriate when there are multiple endogenous variables. Both tests show that the instruments predict the endogenous variables well in the first-stage estimation. See Kleibergen and Paap (2006) and Stock and Yogo (2002) for discussion.

Table A.20: Dynamic Treatment Effects on Health (The Experiment, IV)

	Discomfort	**Fever**	**Pain**	**Diarrhea**	**Cough**	**Palpitation**
treat_t2	0.008	-0.02	-0.003	0.005	-0.005	-0.03*
	(0.056)	(0.015)	(0.042)	(0.005)	(0.020)	(0.016)
treat_t3	-0.059	-0.022	-0.009	0.007	0.020	-0.014
	(0.066)	(0.016)	(0.043)	(0.006)	(0.024)	(0.020)
treat_t4	0.002	-0.021	0.026	0.008	-0.001	-0.039*
	(0.067)	(0.014)	(0.053)	(0.006)	(0.024)	(0.022)
treat_t5	-0.041	-0.021	0.023	0.008	-0.008	-0.015
	(0.067)	(0.016)	(0.05)	(0.006)	(0.028)	(0.015)
treat_t6	-0.047	-0.006	-0.035	0 .000	-0.037	-0.009
	(0.069)	(0.012)	(0.046)	(0.008)	(0.034)	(0.018)
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes
# of Individual	531	531	531	531	531	531
N	2562	2562	2562	2562	2562	2562

*** p<0.01, ** p<0.05, * p<0.1; Standard errors in parentheses

Table presents the IV estimates of the dynamic treatment effects on individual health outcomes using data prior and post to the intervention in the experiment.

Table A.21: Correlation Matrices (Income Source vs. Scheme Phase)

t=1	Phase-in	Plateau	Phase-out
No Income	0.2233*	-0.1736*	-0.1214*
Single Income	0.0380	0.0719	-0.1265*
Dual Income	-0.2395*	0.0726	0.2486*

t=2	Phase-in	Plateau	Phase-out
No Income	0.2142*	-0.1675*	-0.1060
Single Income	0.0975	0.0751	-0.2071*
Dual Income	-0.2891*	0.0505	0.3274*

t=3	Phase-in	Plateau	Phase-out
No Income	0.2545*	-0.1677*	-0.1469*
Single Income	0.1979*	-0.0262	-0.2273*
Dual Income	-0.3743*	0.1355	0.3368*

t=4	Phase-in	Plateau	Phase-out
No Income	0.3004*	-0.1961*	-0.1687*
Single Income	0.1091	0.1369	-0.2886*
Dual Income	-0.3298*	-0.0110	0.4322*

t=5	Phase-in	Plateau	Phase-out
No Income	0.3395*	-0.1938*	-0.2175*
Single Income	0.1138	0.0292	-0.1610*
Dual Income	-0.3979*	0.1209	0.3533*

t=6	Phase-in	Plateau	Phase-out
No Income	0.3932*	-0.2397*	-0.2516*
Single Income	0.0846	0.1175	-0.2179*
Dual Income	-0.4507*	0.0880	0.4694*

Table presents the matrices of correlations between types of income structures (no-income, single-income, and dual-income) and the location in scheme phases (phase-in, plateau, and phase-out). Significance with the level above 10% is marked with asterisks. The table shows that dual-income households always tend to end up in the phase-out stage regardless of time periods. This explains why the phase-out families have a larger treatment effect.

Table A.22: Balance of Covariate (Child Age Group, Treatment vs. Control)

Age Group	Overall	Treatment	Control	T-C	p-value
6 to 12 (Elementary)	0.21	0.21	0.22	-0.01	0.876
	(0.04)	(0.05)	(0.05)	(0.07)	
12 to 15 (Junior High)	0.09	0.06	0.13	-0.07	0.170
	(0.02)	(0.03)	(0.04)	(0.05)	
15 to 18 (Senior High)	0.19	0.21	0.17	0.04	0.587
	(0.03)	(0.05)	(0.05)	(0.07)	
18 to 22 (College)	(0.34	0.40	0.27	0.13	0.121
	(0.04)	(0.06)	(0.06)	(0.08)	
Sample Size	138	78	60		

Table presents the balance of covariates of the experiment sample across initial treatment status. Each row presents the average number of children that fall in a particular age group. The first three columns present the means and the standard errors (in the parenthesis) for the whole sample, the initial treatment group, and the initial control group during the pre-intervention period. The fourth column presents the mean difference between the last two groups. The fifth column presents the p-value of the two-sample t-test. The table shows that there are more households in the treatment group who have members in the 18-22 age group even though the difference is not statistically significant. This indicates that the treatment effect on education expenditure might have been driven by the fact that more households in the treatment group happen to have more students in colleges.

Table A.23: Treatment Effects on Education Expenditure (College vs. Non-College)

	Education Expenditure College Group	Education Expenditure Non-college Group
treat_t2	511.10**	-23.58
	(870.79)	(156.82)
treat_t3	82.96	29.30
	(282.89)	(124.14)
treat_t4	1490.48	153.71
	(3497.54)	(471.79)
treat_t5	-19.36	36.02
	(346.28)	(189.85)
treat_t6	151.52	-44.68
	(350.67)	(37.88)
Household FE	Yes	Yes
Time FE	Yes	Yes
# of Households	174	174
N	882	882

Table presents the IV estimates of the dynamic treatment effects on household education expenditure using data prior and post to the intervention in the experiment. Results show that college households have effects that are similar to the previous result, while the other households do not show any effect. This indicates that it is households with college students that are driving the overall effect. The overall effect is not driven by the fact that more households in the treatment group happen to have more students in colleges.

Table A.24: Treatment Effects on Employment Type

	Unemployed	Employed	Self-employed
treated	-0.096**	0.051	0.045
	(0.038)	(0.033)	(0.038)
Individual FE	Yes	Yes	Yes
Time FE	Yes	Yes	Yes
# of Individual	531	531	531
N	2562	2562	2562

*** p<0.01, ** p<0.05, * p<0.1; Standard errors in parentheses

Table presents the IV estimates of the treatment effects on employment type using data prior and post to the intervention in the experiment. Results show that the number of unemployed individuals decreased significantly.

Table A.25: Treatment Effects by Income Structures and Scheme Phases

	Dual-income	Non-dual	Phase-in	Plateau	Phase-out
Monthly Hours Worked	163.38***	28.45	67.07**	44.98	143.83*
	(56.22)	(28.53)	(33.53)	(59.07)	(86.64)
Monthly Earning	953.88**	422.11**	462.75**	384.84	513.52
	(469.10)	(187.74)	(189.88)	(417.05)	(568.26)
Monthly Total Exp.	934.23***	277.05*	306.41**	647.23*	913.30*
	(266.65)	(163.79)	(151.53)	(383.28)	(552.37)
Household FE	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes
# of Households	174	174	174	174	174
N	882	882	882	882	882

*** p<0.01, ** p<0.05, * p<0.1; Standard errors in parentheses

Table presents the IV sub-group estimates of the treatment effects on major outcomes using data prior and post to the intervention in the experiment. Results indicate that dual-income families have larger effects compared with the others, and households that were initially in the phase-out range prior to the intervention have larger effects compared with the others.

A.4 Application Procedure

Households are required to report their employment status and earnings monthly with supporting materials. A review committee will verify the validity of the materials before issuing the rewards. In addition, an inspection team will contact the employers to verify the reported earning before the applications were approved.

First-time applicants - Upon the first-time application, local community committee staffs will visit the eligible households in person and deliver the forms to be prepared, including the consent form and the confidentiality form. Households will be asked to provide their household registers, IDs, and the bank account of the heads before completing the forms. The staff will be assisting the household heads to fill out the forms. For heads who are illiterate, the forms will be completed by narrations and filled by the staff.

Submission - Households are required to report their employment status and earnings of the last month during the first week of each month. Specifically, members in the enrolled households are required to submit earning certificates or payroll records that are signed and sealed by their employers. Less formal jobs such as part-time jobs are required to provide payment certificates which indicate hours worked and payments, signed and sealed by the employers, too. The certificate templates are designed and provided by the review committee (See Appendix A.5). Households that perform farming or own business are required to submit sales records or tax certificates, indicating daily costs and quantities along with profits. The record templates are also designed and provided by the review committee. When the materials are ready, the staff from the local community committees will visit the households and collect the documents.

Preliminary check - After the submission, the staff from the local community committees will begin checking the integrity and the validity of the materials and see if the numbers make sense. This process is to be completed in three workdays. Specifically, first, they will check if the households have complete their submissions. Then, check if the submitted materials are valid, such as if the documents are signed and sealed by the employers and possibilities to forge the documents. For any incomplete submission or invalid material, the staff will document the records,

reject, and inform the households the reasons before having them submitted again. Meanwhile, if there is any significant difference in earning between two adjacent months, the staff will also ask the households and document the reasons. All the staff who participated in the program were enrolled in a brief training session one month prior to the implementation.

Review - An inspection team with several members from the research center and student workers will be verifying the submitted materials during the reviewing stage. For employees who are hired by formal companies and organizations, the inspection team will contact and inquire about their employment status and wage earnings for that particular month. For those who are hired by individuals or performing part-time jobs, the inspection team will contact their employers to verify hours worked and payments. Most inquiries were made by phone calls. The rest was done by field investigations. For individuals who perform farming activities or own small businesses, the inspection team will ask the nearby households to verify business hours first. Then, randomly visit the households two or three times per month (no less than one hour per visit) to see how well the business is going. Specifically, the team will observe the type of business the household is doing, the cost and the revenue, etc. By comparing with other households that are also performing similar activities, the team will be able to find out a reasonable range of income. For activities such as rickshaw taxi that does not have a fixed operating location, the team will visit the workers multiple times to inquire its working hours and income to speculate the range.

Approval - When the inspection team completed the work, the review committee will make the final approvals of the applications. The review committee includes the community committee and the inspection team. The final decisions will be made based on several aspects, including the staffs' impressions on households' living standards, any unqualified material, reports from the inspection team. Based on the validity and authenticity of the materials, the final decision is made. All the decisions are well documented, along with the reasons for rejections.

Distributing the reward - The review committee will submit the final decisions to the research team with all the supporting materials, reports, and documented records. Then, the team will distribute the reward according to the submitted decisions within the first two weeks of each month.

The distributions were accomplished by bank transfers through household heads' accounts in co-operation with Leshan City Commercial Bank. Then, the research team will compile a register that records households and corresponding rewards for that month and submit to the local authority. For rejected applications, the team will contact the household to inquire about the reasons before taking further actions.

Penalties for noncompliance - There are incentives for the households to manipulate their earning to get more rewards. For example, households within the phase-out stage have incentives to under-report the earnings. Households within the phase-in stage have incentives to over-report the earnings. The households also have the incentives to over-report family sizes. In the case of intentional misreporting, a set of rules were made to prevent the households from doing so. Each rule describes a certain condition that will result in households being expelled from the program. The review committee also reserves the right to withdraw the rewards that were given previously. Namely, households are not allowed to intentionally misreport earning, provide any invalid or forged materials, leak information to irrelevant personnel, obstruct implementation, gamble, participate or purchase any goods or service that are not appropriate for aided families, purchase commercial real estate, refuse to get employed without appropriate reason, conceal changes in family size, obstruct inspection team from making inquiries, or any form of cheating.

Integrity citation - Households that continuously submit valid and authentic materials throughout the year will be awarded integrity citations annually. In addition, the awarded households will be receiving a small amount of bonus issued by the research center.

Household surveys - In addition to the reward applications, the research team will conduct and carry out household surveys among all households that participated in the experiment once every two months. The surveys cover a variety of information such as demographics, employment, labor supply, income, asset, daily expenditures, disease, and healthcare expenditures. Households are required to respond to the questions based on the knowledge of the last month.

Information sessions - Households in the treatment group were asked to enroll an information session to familiarize the reward mechanism before the experiment began. The sessions include an

explanation to inform the households about their rights and responsibilities, particularly the confidentiality of the experiment and underlying consequences. Then, the reward plans were explained to households, emphasizing that the total amount of reward will increase as they work more. An illustration of the reward application procedure was also introduced to households. Particularly, it emphasizes the importance of filing the actual earning. The last section involves explaining the notion of family size to the households, informing households about the conditions under which a person will be considered as a household member.

A.5 Documents and Forms

Wutongqiao Labor Income Reward Plan Experiment Commitment

I, _____, hereby declare my commitment to take part in Wutongqiao Labor Income Reward Plan. During the experiment, my household members and I promise:

1. Full cooperation with the government officials to complete the relevant work involved in Wutongqiao Labor Income Reward Plan;
2. Fully comply with the relevant laws and provisions;
3. Take no extreme measures or disturb public order in the case of being expelled from the program due to provision violations;
4. Provide household information and proof of income as required.
5. Cooperate with authorized personnel to verify household income and income sources;
6. Guarantee the authenticity and validity of the information that is provided and will NOT provide any false information; and
7. Will not use the reward received from the program to carry out illegal activities such as gambling, drug abuse, and prostitution.

Name (Print): _____ Signature: _____

ID: _____ Date: _____

Labor Income Reward Plan

Earned Income Statement Form

Name:		**Telephone:**	
Family Size:		**Address:**	
Income Sources:			
1.	**Work away from home:**		
2.	**Rickshaw taxi:**		
3.	**Domestic service:**		
4.	**Grocery store:**		
5.	**Farming product:**		
6.	**Others:**		
	Monthly Total:		

Date: _____ Community Staff Signature: _____

Wutongqiao Labor Income Reward Plan Experiment
Reward Approval

Household Head:		**Gender:**		**Date of Birth:**			
ID No.:			**Welfare Type:**		☐ **Minimum Subsistence** ☐ **Registered Poor Household**		
Account No.:			**Bank:**				
County:		**Township:**			**Village:**		
Address:							
Household Register:	☐ **Urban** ☐ **Rural**						
Phone:		**Family Size:**			**Attachment(s):**		
Earning Proof:		**Work Type:**	☐ **Employed** ☐ **Self-employed** ☐ **Farming (Animal)** ☐ **Farming (Crop)** ☐ **Others**				
Earning:		**Reward:**					
Village Committee Approval:	**Operator: _____ Inspector: _____ (Official Seal)** **Telephone (Office): _____ Date:**						
Township Committee Approval Check:	**Operator: _____ Inspector: _____ (Official Seal)** **Telephone (Office): _____ Date:**						

PEOPLE'S GOVERNMENT OF WUTONGQIAO DISTRICT

(2016) No. 201600001

People's Government of Wutongqiao District
Letters of Verification for Employees' Labor Income

Hello _____,

Mr. (Mrs.) _____ has been selected to take part in the Wutongqiao Labor Income Reward Plan Experiment conducted by Southwest University of Finance and Economics and the Wutongqiao Government. The participation of the experiment requires the participants to have their monthly earning verified. Please fill out the relevant documents and provide actual work information for this person.

Thank you for your cooperation!

Sincerely,

People's Government of Wutongqiao District

December 2014

Wutongqiao Labor Income Reward Plan Experiment

Proof of Income

_____ (Employer's Name)

hereby prove that Mr. (Mrs.) _____ is the (1. full-time; 2. contract; 3. Part-time) employee. This employee worked ___ days in the last month with ___ hours every day. The base wage for this employee in the last month was _____ RMB; The bonus was _____ RMB; The total earning from the work in the last month was _____ RMB.

The employer confirms that the information above is authentic and valid.
Please call _____ (Phone) or go to _____ (Address) for inquiries or disputes.

Regards,

(Employer's Seal)
Representative's Signature: _____
December 2014

Wutongqiao Labor Income Reward Plan Experiment
Daily Sales Record

Date:	**SID:**	**Household Head:**

Item	**Cost**	**Quantity**	**Price**	**Revenue**
Total				

Wutongqiao Labor Income Reward Plan Experiment
Monthly Sales Record

Household Head:		**SID:**		
Item	**Cost**	**Quantity**	**Stock**	**Revenue**
Total Cost:			**Total Revenue:**	
Net Profit:				

Wutongqiao Labor Income Reward Plan Experiment
Application Form

SID:		**Community:**			**Date:**		
Household Head:		**Gender:**		**Contact:**			
				Address:			
Household Member Information:							
	Head	**1**	**2**	**3**	**4**	**5**	**6**
Name:							
Relationship with the Head:							
Able-bodied:							
Employment:							
Employer:							
Employer's Address:							
Days Worked Per Month:							
Hours Worked Per Day:							
Earning:							
Wage:							
Pension:							
Welfare:							
Others:							
Average Business Revenue:				**Family Size:**			
Average Labor Income:				**Average Total Income:**			
Average Labor Income Per Capita:				**Average Income Per Capita:**			
Note:				**Commitment**		**I confirm that all the information above is authentic and valid. I am aware and willing to take responsibility for any form of falsification.**	
Surveyors and Verification Only							
Community Staff Check and Note:				**Community Staff Signature:**			
CHFS Approval:	**CHFS confirms this family's actual revenue from business is _____, household total income is _____, income per capita is _____.**						

Wutongqiao Labor Income Reward Plan Experiment
Monthly Reward Application Form

SID:				**Community:**		**Phone:**					
Household Head:				**ID No.:**		**Address:**					
Work and Earning Information	**Name**	**Emp. Last Month**	**Emp. This Month**	**Change Note:**	**Earning**					**Other**	**Amount**
					Wage	**Pension**	**Welfare**		**Subsidy**		
Other Income	**Project**	**Business Revenue**			**Cost**			**Profit**			
Summary	**Total Earning**		**Earning Per Capita**		**Total Income**			**Income Per Capita**			
Other Change Note:											
Surveyors and Verification Only											
Community Staff Check:							**Staff Signature:**				
Surveyor Check:							**Surveyor Signature:**				
Approval Note:							**Reviewer Signature:**				
Approval:											

Wutongqiao Labor Income Reward Plan Experiment Inspection Form

SID:				**Community:**			**Date:**	
Name:				**ID No. :**			**Address:**	
Employment Information Verification	**Formal Employer**	**Name**	**Employer Exists**	**Contact Information Check**	**Working**	**Days Worked Per Month**	**Hours Worked Per Day**	**Wage After Tax**
	Surveyor's Note							
	Individual Employer	**Name**	**Employer Exists**	**Employment Status**	**Job**	**Days Worked Per Month**	**Hours Worked Per Day**	**Payment After Tax**
	Surveyor's Note							

Family Business Revenue	**Household Owns Business**	**Main Business**	**How is the Business**				**How Many Employees?**	**Business Hour**
			Good	**Okay**	**Bad**	**Very Bad**		
	Main Cost	**Surveyor: Please Pick Two Different Times to Make Inquiries Among the Residents Who Live Nearby (At Least 1 Hour Each Time)**	**First Time**	**Business Revenue**			**Second Time**	**Business Revenue**
	Estimated Business Revenue Per Day	**Project-relevant Cost**				**Total Cost**		**Net Profit**
		Project1	**Project2**	**Project3**	**Project4**			
	Note							

APPENDIX B

APPENDIX FOR CHAPTER 3

B.1 Figures

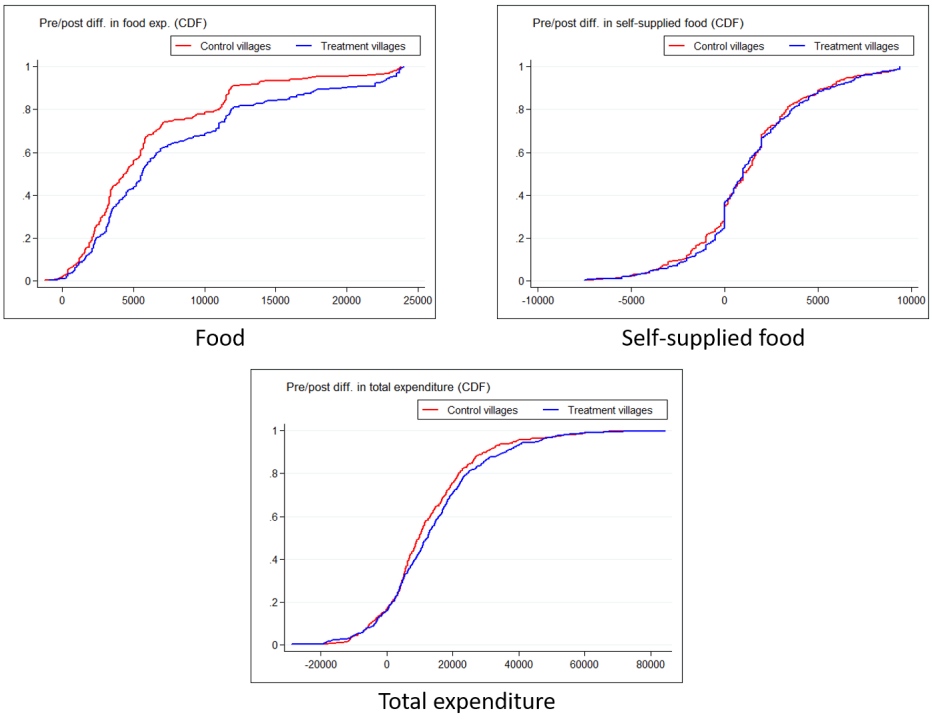


Figure B.1: Cumulative Density Function of Changes in Household Expenditures (A)

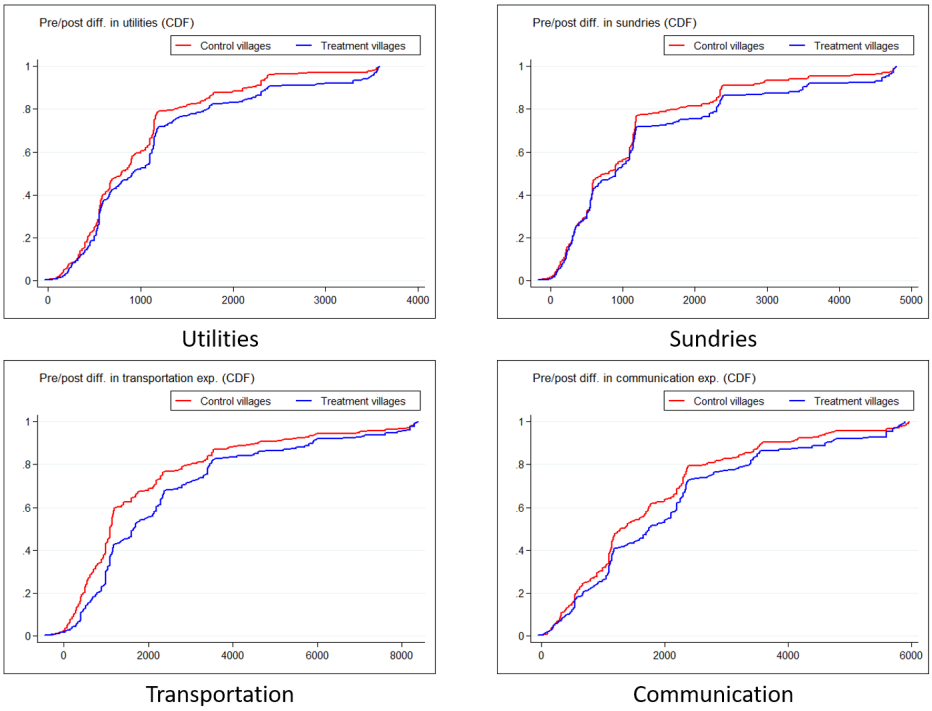


Figure B.2: Cumulative Density Function of Changes in Household Expenditures (B)

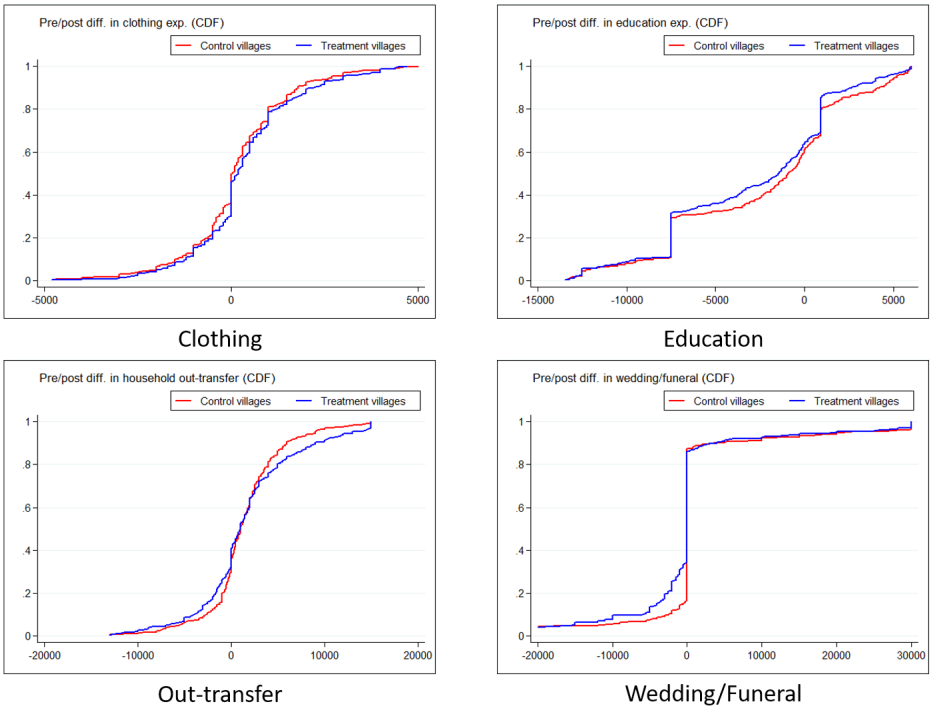


Figure B.3: Cumulative Density Function of Changes in Household Expenditures (C)

B.2 Tables

Table B.1: Labor Income Reward Plan (Mabian)

Component		Reward	Distribution
Price Subsidies			
	Chicken/Duck/Goose	8% of Revenue	Quarterly
	Cow	6% of Revenue	Quarterly
	Goat	10% of Revenue	Quarterly
	Pig	8% of Revenue	Semi-annually
	Honey	50 RMB per box	Annually
	Tea	8% of Revenue	Quarterly
	Yam	8% of Revenue	Quarterly
EITC		6% of Earning	Quarterly

Table B.2: Labor Income Reward Plan (Muchuan)

Component		Reward	Distribution
Kick-starter Subsidy		50% of Cost	Quarterly
Price Subsidy			
	Chicken/Duck/Goose	10% of Revenue	Quarterly
	Goat	10% of Revenue	Quarterly
	Pig	10% of Revenue	Semi-annually
	Tea	10% of Revenue	Quarterly
	Bamboo Shoots	10% of Revenue	Quarterly
	Pum	10% of Revenue	Quarterly
	Turmeric	10% of Revenue	Annually
	Kiwi Fruit	10% of Revenue	Annually
EITC		10% of Earning	Quarterly

Table B.3: Balance of Covariate (Mabian/Muchuan, Show vs. No-show)

	All	No-show	Others	N-O	p-value
Wage Income	1,993	1,228	2,224	-995	0.003
	(4,811)	(3,515)	(5,119)	(334)	
Total Income	14,254	11,079	15,217	-4,138	0.003
	(20,501)	(17,783)	(21,172)	(1,425)	
Total Expenditure	10,759	11,222	10,619	603	0.384
	(9,925)	(10,230)	(330)	(692)	
Total Hours Worked	2,216	1,837	2,331	-493	0.015
	(2,902)	(2,854)	(2,908)	(202)	
# of Earners	2.25	2.16	2.27	0.11	0.189
	(1.16)	(1.06)	(1.19)	(0.08)	
# of Jobs	1.36	1.15	1.43	-0.28	0.007
	(1.47)	(1.41)	(1.48)	(0.10)	
Poor Housholds	0.37	0.29	0.40	-0.11	0.001
	(0.48)	(0.45)	(0.49)	(0.03)	
Treatment Village	0.52	0.45	0.54	-0.09	0.013
	(0.50)	(0.50)	(0.50)	(0.03)	
Family Size	4.26	4.34	4.23	0.10	0.414
	(1.82)	(1.86)	(1.81)	(0.13)	
# of Females	2.03	2.09	2.01	0.08	0.382
	(1.30)	(1.36)	(1.28)	(0.09)	
Minority	0.56	0.72	0.52	0.20	0.000
	(0.50)	(0.45)	(0.50)	(0.03)	
Secondary Education	0.69	0.72	0.68	0.04	0.240
	(0.46)	(0.45)	(0.47)	(0.03)	
# of Able-bodied	2.66	2.52	2.71	-0.18	0.026
	(1.18)	(1.08)	(1.21)	(0.08)	
# of Children	0.62	0.66	0.61	0.05	0.148
	(0.48)	(0.47)	(0.49)	(0.03)	
# of Seniors	0.33	0.29	0.34	-0.04	0.170
	(0.47)	(0.46)	(0.47)	(0.03)	
Receive Pension	0.22	0.16	0.24	-0.08	0.008
	(0.41)	(0.37)	(0.43)	(0.03)	
Sample Size	1151	268	883		

Table presents the balance of covariates of the households that answered both surveys and other households. The first three columns present the means and the standard deviations (in parentheses) for the whole sample, households that did not show up in the follow-up survey, and the rest. The fourth column presents the mean difference between the last two groups. The fifth column presents the p-values of two-sample t-tests. All covariates are aggregated to the household-level. Wage income only includes earnings from employment and self-employment. "Minority" is an indicator that equals one if there is at least one household member who is a minority. "Secondary education" is an indicator that equals one if the household has at least one member who has education higher than junior high. "Able-bodied" is defined as anyone who is between 16 and 65 years old and except for individuals who have completely lost their abilities to work. "Receive pension" is an indicator that equals one if the household has at least one pensioner. The statistics in this table imply that there is a statistical difference between households that did not show up in the follow-up survey and households that responded to both of the surveys.

Table B.4: Balance of Covariate (Mabian/Muchuan, Treatment Villages vs. Control Villages)

	All	Treatment	Control	T-C	p-value
Wage Income	1,993 (4,811)	1,992 (4,832)	2,079 (4,791)	-167 (283)	0.555
Total Income	8,691 (13,982)	9,446 (13,982)	7,881 (12,897)	1,565 (794)	0.049
Total Expenditure	15,413 (10,195)	16,386 (10,708)	14,368 (9,514)	2,018 (598)	0.000
Total Hours Worked	2,216 (2,902)	2,184 (3,072)	2,250 (2,711)	-65 (171)	0.701
# of Earners	2.25 (1.16)	2.26 (1.28)	2.23 (1.03)	0.03 (0.07)	0.705
# of Jobs	1.36 (1.47)	1.23 (1.51)	1.51 (1.41)	-0.28 (0.09)	0.001
Family Size	4.26 (1.82)	4.20 (1.82)	4.32 (1.81)	-0.12 (0.11)	0.260
# of Females	2.03 (1.30)	2.01 (1.32)	2.05 (1.28)	-0.04 (0.08)	0.584
Minority	0.56 (0.50)	0.60 (0.49)	0.52 (0.50)	0.08 (0.03)	0.006
Secondary Education	0.69 (0.46)	0.66 (0.47)	0.71 (0.45)	-0.05 (0.03)	0.084
# of Able-bodied	2.66 (1.18)	2.67 (1.23)	2.66 (1.13)	0.01 (0.07)	0.866
# of Children	0.62 (0.48)	0.6 (0.49)	0.64 (0.48)	-0.04 (0.03)	0.170
# of Seniors	0.33 (0.47)	0.33 (0.47)	0.33 (0.47)	0.00 (0.03)	0.975
Receive Pension	0.22 (0.41)	0.21 (0.41)	0.23 (0.42)	-0.02 (0.02)	0.434
Sample Size	1,151	596	555		

Table presents the balance of covariates of households that are in the treatment villages and the control villages. The first three columns present the means and the standard deviations (in parentheses) for the whole sample, treatment-village households, and control-village households. The fourth column presents the mean difference between the last two groups. The fifth column presents the p-values of two-sample t-tests. All covariates are aggregated to the household-level. Wage income only includes earnings from employment and self-employment. "Minority" is an indicator that equals one if there is at least one household member who is a minority. "Secondary education" is an indicator that equals one if the household has at least one member who has education higher than junior high. "Able-bodied" is defined as anyone who is between 16 and 65 years old and except for individuals who have completely lost their abilities to work. "Receive pension" is an indicator that equals one if the household has at least one pensioner. The statistics in this table implies that there is a statistical difference between treatment-village households and control-village households prior to the intervention.

Table B.5: Matching Results (Mabian/Muchuan)

	Match	Treatment	Control	p-value
Wage Income	Before	2,761	4,373	0.009
	After	2,761	2,544	0.792
Total Earning	Before	3,165	4,812	0.008
	After	3,165	2,864	0.618
Total Hours Worked	Before	1,650	2,534	0.000
	After	1,650	1,505	0.564
# of Earners	Before	2.08	2.32	0.011
	After	2.08	2.09	0.905
# of Jobs	Before	0.93	1.57	0.000
	After	0.93	0.97	0.756
Family Size	Before	4.04	4.28	0.091
	After	4.04	4.18	0.442
Grow Food-crop	Before	0.66	0.75	0.006
	After	0.66	0.64	0.678
Grow Cash-crop	Before	0.26	0.31	0.213
	After	0.26	0.27	0.824
Forestry	Before	0.12	0.25	0.000
	After	0.12	0.11	0.759
Husbandary	Before	0.42	0.46	0.270
	After	0.42	0.42	1.000
# of Females	Before	1.96	2.02	0.572
	After	1.96	1.97	0.939
Minority	Before	0.54	0.50	0.388
	After	0.54	0.52	0.766
Secondary Education	Before	0.59	0.70	0.004
	After	0.59	0.59	1.000
# of Able-bodied	Before	2.59	2.74	0.121
	After	2.59	2.63	0.750
# of Children	Before	0.56	0.62	0.096
	After	0.56	0.57	0.764
# of Seniors	Before	0.38	0.32	0.128
	After	0.38	0.41	0.544
Sample Size		203	608	

Rubins' B=25.0; Rubins' R=1.03

Table presents the results of Propensity-score matching. The second and the third column present the variable means of the eligible households and other households. The last column presents the p-values of the two-sample tests between the last two groups. Each variable has two rows that compare the means before and after the matching. Results show that the covariates are balanced after the matching.

Table B.6: Matching Results (Mabian/Muchuan, Individuals)

	Match	Treatment	Control	p-value
Wage Income	Before	639	1,088	0.000
	After	639	832	0.171
Is Working	Before	0.49	0.50	0.635
	After	0.49	0.51	0.585
# of Jobs	Before	0.21	0.35	0.000
	After	0.21	0.21	1.000
Not Working b/c Temporal Reasons	Before	0.06	0.02	0.000
	After	0.06	0.05	0.528
Not Working b/c Retirement	Before	0.02	0.01	0.087
	After	0.02	0.02	0.627
Total Months Worked	Before	2.00	2.98	0.000
	After	2.00	2.07	0.752
Total Days Worked	Before	47	70	0.000
	After	47	48	0.835
Total Hours Worked	Before	388	564	0.000
	After	388	396	0.854
Hours Worked per Day	Before	13	11	0.000
	After	13	13	0.931
Days Worked per Month	Before	4.70	7.46	0.000
	After	4.70	4.73	0.945
Female	Before	0.46	0.44	0.206
	After	0.46	0.46	0.960
Age	Before	33	31	0.036
	After	33	33	0.733
Able to Work	Before	0.61	0.59	0.297
	After	0.61	0.60	0.647
Secondary Education	Before	0.28	0.32	0.020
	After	0.28	0.29	0.784
Minority	Before	0.59	0.57	0.247
	After	0.59	0.61	0.476
Number of Females	Before	2.34	2.43	0.095
	After	2.34	2.42	0.219
Secondary Education (household)	Before	0.73	0.81	0.000
	After	0.73	0.76	0.330
# of Able-bodied	Before	2.84	3.02	0.000
	After	2.84	2.81	0.599
Has Children in the Household	Before	0.70	0.75	0.010
	After	0.70	0.73	0.293
Has Seniors in the Household	Before	0.38	0.31	0.000
	After	0.38	0.41	0.335
Family Size	Before	4.70	4.97	0.000
	After	4.70	4.77	0.365
Sample Size		809	2,816	

Rubins' B=16.7; Rubins' R=0.97

Table presents the results of Propensity-score matching for individual-level regressions. The second and the third column present the variable means of the eligible individuals and other individuals. The last column presents the p-values of the two-sample tests between the two groups. Each variable has two rows that compare the means before and after the matching. Results show that the covariates are balanced after the matching.

Table B.7: Matching Results (Mabian/Muchuan, Household Expenditure)

	Match	Treatment	Control	p-value
Food	Before	645	643	0.954
	After	645	640	0.917
Food (Self-supplied)	Before	1828	2107	0.093
	After	1828	1931	0.600
Education	Before	6532	6050	0.293
	After	6532	6519	0.981
Ultilities	Before	75	87	0.046
	After	75	77	0.760
Sundries	Before	83	82	0.884
	After	83	79	0.576
Clothing	Before	1122	1384	0.007
	After	1122	1171	0.644
Transportation	Before	189	208	0.239
	After	189	194	0.784
Communication	Before	127	145	0.041
	After	127	114	0.200
Minority	Before	0.54	0.50	0.388
	After	0.54	0.56	0.619
# of Females	Before	1.96	2.02	0.572
	After	1.96	2.13	0.205
Secondary Education	Before	0.59	0.70	0.004
	After	0.59	0.65	0.308
# of Able-bodied	Before	2.59	2.74	0.121
	After	2.59	2.59	1.000
Has Children	Before	0.56	0.62	0.096
	After	0.56	0.62	0.190
Has Seniors	Before	0.38	0.32	0.128
	After	0.38	0.38	0.919
Family Size	Before	4.04	4.28	0.091
	After	4.04	4.15	0.544
Sample Size		203	680	

Rubins' B=29.4; Rubins' R=1.30

Table presents the results of Propensity-score matching for household expenditure regressions. The second and the third column present the variable means of the eligible households and other households. The last column presents the p-values of the two-sample tests between the two groups. Each variable has two rows that compare the means before and after the matching. Results show that the covariates are balanced after the matching.

Table B.8: Treatment Effects on Production Type

	PSM	PSM-DD	DD	DDD
Crop Only	0.02	0.02	0.01	-0.12*
Husbandry Only	0.00	-0.02	0.00	-0.01
Crop & Husbandry	0.02	0.07	0.04	0.18**
Product Diversity	0.04	0.07**	0.05**	0.02
Food-crop	0.02	0.10**	0.10***	0.12*
Cash-crop	0.00	0.03	-0.03	0.11
Forestry	0.05	0.15**	0.10***	0.10
Husbandry	0.01	0.05	0.03	0.17**
Crop Yield (Food + Cash)	-365	216	351*	156
Crop Profit (Food + Cash)	35	35	360***	-78
Husbandry Yield	104	104	-541***	94
Husbandry Profit	-186	-186	-221**	294
Farm Yield (Crop + Husbandry)	-186	279	-184	7.38
Farm Profit (Crop + Husbandry)	-141	-177	351*	156
Controls			Yes	Yes
Village FE			Yes	Yes
Sample Size	883	883	1,151	1,151

*** p<0.01, ** p<0.05, * p<0.1

Table presents the treatment effects on production type. The first three outcomes in the table are indicators that identify whether a household engages in crop farming only, husbandry only, or both. The fourth row is an indicator that equals one if a household produces more than one type of farm product. The last four rows are indicators that equal to one if a household engages in food-crop farming, cash-crop farming, forestry, or husbandry. Food-crop is defined as farm products that are grown for food consumption. Cash-crop is defined as farm products that are grown for their economic values. Yield is the reported market value of the farm products. Profit is the market sales revenue less the cost of the input. The result indicates no significant impact on household production. There is some evidence that suggests that program participation increases households' engagements in food-crop farming, forestry, and husbandry. In addition, households tend to diversify their farming products. However, these findings are inconsistent across regressions in terms of either magnitudes and significance.

Table B.9: Treatment Effects on Employment and Earning (Individual-level)

	PSM	PSM-DD	DD	DDD
Wage Income	1,115**	1,216***	1,217***	558**
Is Working	0.04	0.02	0.00	-0.04***
# of Jobs	0.04	0.08*	0.11***	0.06**
Employed Jobs	0.04**	0.00	-0.01	-0.00
Self-employed Jobs	0.02	0.04	0.00	-0.00
Not Working b/c Temporal Reasons	0.00	0.00	-0.01*	0.01*
Not Working b/c Retired	0.00	0.00*	0.00	0.00
Not Working b/c Housework	0.00	0.00	0.00	0.00
Total Months Worked	0.47	0.62*	0.92***	0.50**
Total Days Worked	15	18*	22***	9
Total Hours Worked	178*	179*	271***	157***
Hours Worked per Day	0.35	0.01	-0.46***	-0.53**
Days Worked per Month	1.29*	1.87**	2.63***	1.36***
Controls			Yes	Yes
Village FE			Yes	Yes
Sample Size	3,625	3,625	3,625	3,625

*** p<0.01, ** p<0.05, * p<0.1

Table presents the treatment effects on individual outcomes. The results indicate that the Labor Income Reward Plan has positive and significant impacts on individual earning and the number of jobs. The last five rows present the treatment effects on individuals' labor supply. Individual labor supply is reflected by five measurements that describe different aspects of the intensive labor supply. All of the measurements are constructed based on the four questions in the survey. Namely, household members were first asked how many months they had been working in the past year (2016 or 2017). Then, household members were asked, on average, how many weeks they had been working per month. Next, household members were asked, on average, how many days they usually work per week. Finally, household members were asked, on average, how many hours they usually worked per day in the past year. The results indicate that the Labor Income Reward Plan has positive and significant impacts on total months worked, total hours worked as well as days worked per month. However, evidence suggests that individuals' number of hours worked per day has decreased.

Table B.10: Treatment Effects on Household Expenditure

	PSM	PSM-DD	DD	DDD
Total Expenditure	2,329	2,991**	1,743*	719
Food	753	782	1,828***	-614
Self-supplied Food	183	452	144	496
Utilities	40	53	167***	-161
Sundries	124	123	192**	-207
Clothing	-152	126	175**	30
Transportation	-65	-45	525***	-189
Communication	-55	-34	278***	-210
Education Expenditure	74	-287	-709**	-168
Wedding/Funeral	969	1,355**	-951	1,278
Household Out-transfer	382	429	268	-371
Controls			Yes	Yes
Village FE			Yes	Yes
Sample Size	883	883	1,151	1,151

*** p<0.01, ** p<0.05, * p<0.1

Table presents the treatment effects on household expenditure. Household out-transfer is the sum of the amount of cash the household gave away to other households due to a variety of reasons. Evidence suggests that program participation increases total expenditure. However, most estimates are inconsistent across regressions in terms of either magnitudes and significance.

Table B.11: Distribution Test for Differences in CDFs (Mabian/Muchuan)

		Equality	FSD(TC)	SSD(TC)	FSD(CT)	SSD(CT)
Food	Difference	0.8168	0.0237	0.039	0.8168	22.0441
	p-value	0.087	0.959	0.773	0.043	0.005
Self-supplied Food	Difference	0.2945	0.2395	0.0967	0.2945	13.247
	p-value	0.987	0.771	0.775	0.687	0.428
Utilities	Difference	0.5571	0.013	0.013	0.5571	77.849
	p-value	0.514	0.995	0.822	0.252	0.046
Sundries	Difference	0.4078	0.0632	-0.0021	0.4078	43.2453
	p-value	0.848	0.974	0.785	0.487	0.152
Clothing	Difference	0.4568	0.0473	0.0129	0.4568	25.6654
	p-value	0.714	0.982	0.811	0.374	0.117
Transportation	Difference	1.0718	0.0237	0.0843	1.0718	173.6696
	p-value	0.022	0.992	0.810	0.012	0.000
Communication	Difference	0.6578	0.0912	0.2904	0.6578	107.6578
	p-value	0.372	0.938	0.736	0.207	0.034
Education	Difference	0.4527	0.4527	66.6211	0.0493	0.148
	p-value	0.846	0.471	0.120	0.964	0.823
Wedding/Funeral	Difference	1.151	1.151	30.5224	0.0942	0.0811
	p-value	0.000	0.000	0.005	0.858	0.716
Out-transfer	Difference	0.5366	0.5366	28.2499	0.4559	6.169
	p-value	0.544	0.271	0.228	0.388	0.561
Total Expenditure	Difference	0.5421	0.1079	2.1789	0.5421	183.1275
	p-value	0.612	0.948	0.693	0.330	0.065

Table presents the result of the Kolmogorov-Smirnov test for differences in CDFs of the treatment and the control group. All CDFs describe the distributions of the changes in certain household expenditures during the intervention period. The first column shows the differences and the p-values of distribution equality tests. The rest of the columns shows differences and p-values of the first order stochastic dominance tests and the second order stochastic dominance tests, among which the first two columns present the test results that indicate whether the treatment group dominants the control group (TC). The next two columns present the test results that indicate whether the control group dominates the treatment group (CT). Differences and the p-values of Kolmogorov-Smirnov tests are presented for each test. The results indicate that CDFs of changes in food expenditure, changes in transportation expenditure, and changes in wedding/funeral expenditure are statistically different. The distribution of changes in food expenditure and the distributions of changes in transportation expenditure of the control villages dominate the treatment villages in both first and second order. The distribution of changes in wedding/funeral expenditure of the treatment villages dominates the control villages in both first and second order. This suggests that the EITC may have increased households' expenditures in food and transportation and reduced the expenditure in wedding and funeral.

Table B.12: Heterogeneous Treatment Effects (Mabian/Muchuan)

	Income		Education		Able-bodied		Earner	
	High	**Low**	**High**	**Low**	**Single**	**Multiple**	**Single**	**Multiple**
Wage Income	9,636***	8,043***	8,884***	5,325*	4788***	8,702***	5,131**	6,958***
Total Hours Worked	2,306***	1,792***	1,959***	1,865***	237	2,087***	1,571**	1,889***
# of Earners	0.61***	0.56***	0.64***	0.53***	0.56*	0.53***	0.62***	0.44***
# of Jobs	1.18***	1.20***	0.95***	1.40***	1.26***	0.94***	1.14***	0.98***
Product Diversity	0.01	0.10*	0.04	0.13*	0.30*	0.01	0.15	0.01
Farm Yield	1267	-104	920	1,604	-378	1,265**	2,050**	-337
Farm Profit	-323	-374*	91	-475	-279	-3.96	-38	-319
Food Expenditure	-208	71	404	-750	-699	165	925	-401
Education Expenditure	-503	-12	9.11	338	-1,284	-18	-923	-326
Total Expenditure	2,185	1,314	-1,133	1,566	4,259	-317	3,726	-1,508
Sample Size	441	442	599	284	111	772	212	671

*** p<0.01, ** p<0.05, * p<0.1

Table presents the heterogeneous treatment effects from the PSM-DD estimation. Here, the sample is divided into subgroups based on different criteria. The high-income group is defined as households with a total income higher than the median. The high-education group is defined as households that have at least one member who has received secondary education. "Multiple able-bodied" is defined as households with at least two individuals between 16 and 65 years old who are able to work. "Multiple earners" means that households have at least two individuals who have been working prior to the intervention. Results show that treatment effects vary across different subgroups.

B.3 Documents and Forms

Mabian/Muchuan County

Labor Income Reward Plan Experiment Commitment

After careful consideration, I hereby wish to participate Labor Income Reward Plan Experiment. During the experiment, My household members and I promise:

1. Full cooperation with the government officials to complete the relevant work involved in Labor Income Reward Plan;
2. Fully comply with the relevant laws and provisions;
3. Take no extreme measures or disturb public order in the case of being expelled from the program due to provision violations;
4. Provide household information and proof of income as required.
5. Cooperate with authorized personnel to verify household income and income sources;
6. Guarantee the authenticity and validity of the information that is provided and will NOT provide any false information; and
7. Will not use the reward received from the program to carry out illegal activities such as gambling, drug abuse, and prostitution.

Name (Print): _____ Signature: _____

ID: _____ Date: _____

PEOPLE'S GOVERNMENT OF MABIAN/MUCHUAN

(2017) No. 201700001

People's Government of Mabian/Muchuan
Letters of Verification for Employees' Labor Income

Hello _____,

Mr. (Mrs.) _____ has been selected to take part in Labor Income Reward Plan. The participation of the experiment requires the participants to have their monthly earning verified. Please fill out the relevant documents and provide actual work information for this person.

Thank you for your cooperation!

Sincerely,
People's Government of Mabian/Muchuan
Date:

Leibo Labor Income Reward Plan Experiment

Proof of Income

_____ (Employer's Name)

hereby prove that Mr. (Mrs.) _____ is the (1. full-time; 2. contract; 3. Part-time) employee. This employee worked ___ days in the last month with ___ hours every day. The base wage for this employee in the last month was _____ RMB; The bonus was _____ RMB; The total earning from the work in the last month was _____ RMB.

The employer confirms that the information above is authentic and valid.
Please call _____ (Phone) or go to _____ (Address) for inquiries or disputes.

Regards,

(Employer's Seal)
Representative's Signature: _____
Date:

Leshan City Mabian/Muchuan County Labor Income Reward Plan Application

Household ID					**Community**			**Date**	
Head			**Gender**		**Contact**			**Address**	
Household Member Employment Information	**Name**	**Relationship with Head**	**Ability to Work**	**Employed**	**Employer**	**Employer Address**	**Days Worked**	**Hours Worked Per Day**	**Earning**
	Household Total Earning								
Income	**Crop Profit**					**Husbandry Profit**			
	Business Profit					**Total Earning**			
	Welfare					**Asset Income**			
	Other					**Total Income**			
	Family Size					**Number of Earners**			
	Earning per Capita					**Income per Capita**			
Note						**Head's Commitment and Signature**			**Commitment: I declare that the household information provided above is correct and accurate. I am willing to face any legal consequences for any forgery activity.**
									Signature:
Surveyors and Verification Only									
Note									**Signature:**
Audit	**Business profit for this household is _____RMB; Total income is _____RMB; Income per catpiat is _____RMB.**								

Labor Income Reward Quarterly Verification Summary

SID			**Serial No.**		**Community**		
Head			**Contact**		**ID**		
Household Member Employment Information	**Name**	**Labor Supply**			**Wage Income**	**Verified Income**	**Eligibility**
		Weeks Worked	**Days Worked Per Week**	**Hours Worked Per Day**			
	Total Earning						
Household Income Information	**Husbandry Profit**				**Verified Profit**		
	Crop Profit				**Verified Profit**		
	Business Profit				**Verified Profit**		
Total	**Total Earning (Reported)**				**Total Earning (Verified)**		

Reviewer:

Mabian/Muchuan Labor Income Reward Plan Approval

Head		**Gender**		**Date of Birth**	
ID			**Household Type**	**Ordinary ☐; Registered Poor ☐; Minimum Income Guarantee ☐**	
Account			**Bank**		
Approval					
Address					
Household Register	**Urban ☐; Rural ☐**				
Contact		**Family Size**		**Attachments**	
Proof of Income	**See the Attachments**		**Work Type**	**Employment ☐; Business ☐; Husbandry ☐; Crop ☐; Other ☐**	
Earning			**Reward**		
Village Committee Approval	**Operator: _____ Inspector: _____ (Official Seal)** **Telephone (Office): _____ Date:**				
Township Committee Approval	**Operator: _____ Inspector: _____ (Official Seal)** **Telephone (Office): _____ Date:**				

Minor Revenue Record

Date	Goods/Services	Quantity	Price	Revenue	Buyer/Employer		
					Name	Contact	Address

Name: _____

I hereby declare that all the information above is correct and accurate. I am willing to face any legal consequences for any forgery activity.

Instructions: This form is only for income part-time jobs or high-frequent transactions that are impossible to be verified in the presence of the staff. Each transaction should be no larger than 200 RMB.

Major Revenue Record

Date	Goods/Services	Quantity	Price	Revenue	Buyer/Employer		
					Name	Contact	Address

Name: _____

I hereby declare that all the information above is correct and accurate. I am willing to face any legal consequences for any forgery activity.

Instructions: This form is for transactions that are larger than 200 RMB.

APPENDIX C

APPENDIX FOR CHAPTER 4

C.1 Figures

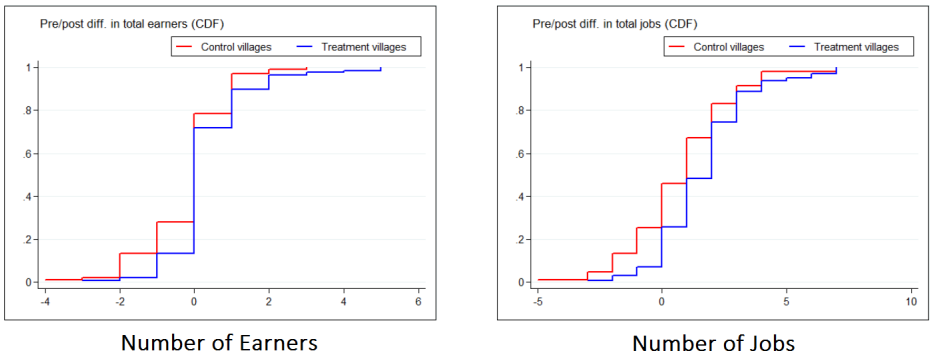


Figure C.1: CDFs of Changes in Number of Earners and Number of Jobs

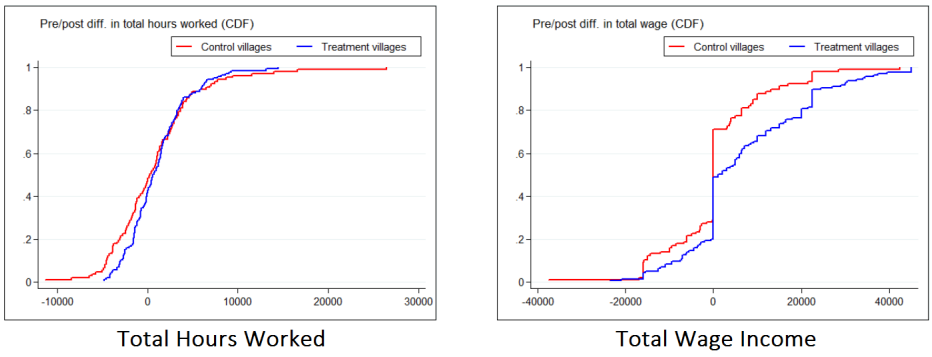


Figure C.2: CDFs of Changes in Total Hours Worked and Wage Income

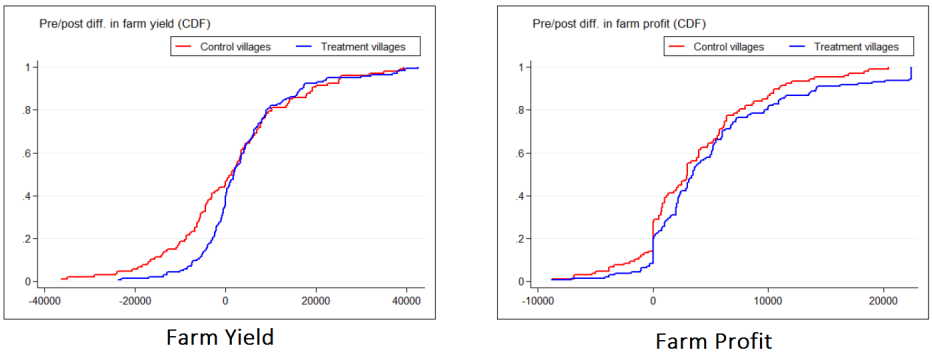


Figure C.3: CDFs of Changes in Farm Yield and Farm Profit

C.2 Tables

Table C.1: Distribution Test for Differences in CDFs (Leibo)

		Equality	FSD(TC)	SSD(TC)	FSD(CT)	SSD(CT)
Farm Profit	Difference	0.7467	0.0273	0.0123	0.7467	50.3147
	p-value	0.305	0.978	0.832	0.150	0.024
Farm Yield	Difference	1.3398	0.3951	-0.0574	1.3398	86.7005
	p-value	0.007	0.577	0.924	0.004	0.006
# of Eaners	Difference	0.9174	0.0000	-0.0574	0.9174	2.9699
	p-value	0.015	0.951	0.892	0.009	0.001
# of Jobs	Difference	1.2456	0.0000	-0.0574	1.2456	5.6745
	p-value	0.002	0.963	0.900	0.000	0.000
Total Hours Worked	Difference	0.9079	0.2352	-0.0574	0.9079	71.0928
	p-value	0.138	0.808	0.941	0.072	0.027
Total Wage Income	Difference	1.4032	0.0273	-0.0451	1.4032	51.5563
	p-value	0.002	0.98	0.907	0.000	0.000

Table presents the result of the Kolmogorov-Smirnov test for differences in CDFs of treatment and control groups. All CDFs describe the distributions of the changes in certain outcomes during the intervention period. The first column shows the differences and the p-values of distribution equality tests. The rest of the columns shows differences and p-values of the first order stochastic dominance tests and the second order stochastic dominance tests, among which the first two columns present the test results that indicate whether the treatment group dominants the control group (TC). The next two columns present the test results that indicate whether the control group dominates the treatment group (CT). Differences and the p-values of Kolmogorov-Smirnov tests are presented for each test. The results indicate that distributions of changes in farm yield, changes in the number of earners, changes in the number of jobs, changes in total hours worked, and changes in total wage income are statistically different. Distributions of changes in farm yield, changes in the number of earners, changes in the number of jobs, changes in total hours worked, and changes in total wage income of the control villages dominate the treatment villages in both first and second order. These results are both reasonable and consistent, suggesting that the EITC may have increased labor supply on both extensive and intensive margin as well as wage income.

Table C.2: Balance of Covariate (Leibo, Show vs. No-show)

	All	No-show	Others	N-O	p-value
Family Size	4.84	4.71	4.91	-0.20	0.263
	(1.73)	(1.73)	(1.73)	(0.18)	
# of Females	2.38	2.29	2.43	-0.14	0.311
	(1.28)	(1.17)	(1.34)	(0.14)	
Secondary Education	0.08	0.10	0.07	0.03	0.220
	(0.27)	(0.31)	(0.25)	(0.03)	
Minority	0.94	0.95	0.94	0.01	0.633
	(0.24)	(0.22)	(0.24)	(0.03)	
# of Able-bodied	2.71	2.69	2.73	-0.04	0.778
	(1.17)	(1.18)	(1.16)	(0.12)	
# of Children	0.77	0.77	0.77	0.00	0.891
	(0.42)	(0.42)	(0.42)	(0.05)	
# of Seniors	0.25	0.26	0.25	0.01	0.874
	(0.44)	(0.44)	(0.43)	(0.05)	
Receive Pension	0.14	0.10	0.16	-0.06	0.108
	(0.35)	(0.31)	(0.37)	(0.04)	
Registered Poor Households	0.64	0.49	0.73	-0.24	0.000
	(0.48)	(0.50)	(0.45)	(0.05)	
# of Earners	2.35	2.24	2.40	-0.16	0.165
	(1.1)	(1.17)	(1.05)	(0.12)	
# of Jobs	2.60	2.46	2.68	-0.22	0.129
	(1.33)	(1.39)	(1.30)	(0.14)	
Total Hours Worked	4,120	4,024	4,171	-147	0.632
	(2,875)	(3,031)	(2,792)	(306)	
Total Wage	6,868	7,644	6,450	1,194	0.281
	(10,385)	(11,426)	(9,776)	(1,104)	
Farming	0.86	0.81	0.89	-0.08	0.030
	(0.35)	(0.39)	(0.31)	(0.04)	
Farm Profit	1,041	1,191	960	231	0.354
	(2,340)	(2,635)	(2,166)	(249)	
Farm Yield	9,393	9,943	9,096	847	0.423
	(9,913)	(10,826)	(9,393)	(1,055)	
Total Income	10,757	11,768	10,211	1,557	0.297
	(14,011)	(15,174)	(13,341)	(1,490)	
Total Expenditure	14,297	15,526	13,634	1,892	0.516
	(27,314)	(23,509)	(29,185)	(2,908)	
Sample Size	388	181	207		

Table presents the balance of covariates of the sample between households that answered both surveys and other households. The first three columns present the means and the standard deviations (in parentheses) for the whole sample, households that did not show up in the follow-up survey, and the rest. The fourth column presents the mean difference between the last two groups. The fifth column presents the p-values of two-sample t-tests. All covariates are aggregated to the household-level. Wage income only includes earnings from employment and self-employment. "Minority" is an indicator that equals one if there is at least one household member who is a minority. "Secondary education" is an indicator that equals one if the household has at least one member who has education higher than junior high. "Able-bodied" is defined as anyone who is between 16 and 65 years old and except for individuals who have completely lost their abilities to work. "Receive pension" is an indicator that equals one if the household has at least one pensioner. The statistics in this table imply that there is no statistical difference between households that did not show up in the follow-up survey and households that responded to both of the surveys except for the proportion of registered poor households and proportion of households that perform farming.

Table C.3: Balance of Covariate (Leibo, Treatment Villages vs. Control Villages)

	Overall	Treatment	Control	T-C	p-value
Total Wage Income	6,869	6,267	7,558	-1,291	0.222
	(10,385)	(10,164)	(10,619)	(1,056)	
Receive Pension	0.14	0.12	0.17	-0.05	0.120
	(0.35)	(0.32)	(0.38)	(0.04)	
Total Income	10,757	10,207	11,387	-1,180	0.409
	(14,011)	(14,330)	(13,650)	(1,426)	
Total Expenditure	14,298	14,365	14,221	144	0.959
	(27,314)	(19,682)	(34,069)	(2,783)	
Poor Household	0.64	0.52	0.78	-0.26	0.000
	(0.48)	(0.50)	(0.41)	(0.05)	
# of Earners	2.35	2.27	2.44	-0.17	0.115
	(1.10)	(1.07)	(1.12)	(0.11)	
# of Jobs	2.60	2.55	2.66	-0.11	0.408
	(1.33)	(1.37)	(1.29)	(0.14)	
Total Hours Worked	4,120	3,819	4,465	-646	0.027
	(2,875)	(2,583)	(3,148)	(291)	
Farming	0.86	0.81	0.92	-0.11	0.003
	(0.35)	(0.39)	(0.28)	(0.03)	
Farm Profit	1,042	1,242	813	429	0.071
	(2,340)	(2,525)	(2,093)	(237)	
Farm Yield	9,393	9,838	8,884	954	0.345
	(9,913)	(10,682)	(8,955)	(1,008)	
Family Size	4.84	4.87	4.80	0.07	0.678
	(1.73)	(1.72)	(1.75)	(0.18)	
# of Female	2.38	2.42	2.35	0.07	0.606
	(1.28)	(1.27)	(1.30)	(0.13)	
Minority	0.94	1.00	0.87	0.13	0.000
	(0.24)	(0.00)	(0.33)	(0.02)	
# of Able-bodied	2.71	2.60	2.84	-0.24	0.047
	(1.17)	(1.15)	(1.18)	(0.12)	
# of Children	0.77	0.80	0.73	0.07	0.148
	(0.42)	(0.40)	(0.44)	(0.04)	
# of Seniors	0.25	0.24	0.27	-0.03	0.443
	(0.44)	(0.43)	(0.45)	(0.04)	
Sample Size	388	181	207		

Table presents the balance of covariates of households that are in the treatment villages and the control villages. The first three columns present the means and the standard deviations (in parentheses) for the whole sample, treatment-village households, and control-village households. The fourth column presents the mean difference between the last two groups. The fifth column presents the p-values of two-sample t-tests. All covariates are aggregated to the household-level. Wage income only includes earnings from employment and self-employment. "Minority" is an indicator that equals one if there is at least one household member who is a minority. "Secondary education" is an indicator that equals one if the household has at least one member who has education higher than junior high. "Able-bodied" is defined as anyone who is between 16 and 65 years old and except for individuals who have completely lost their abilities to work. "Receive pension" is an indicator that equals one if the household has at least one pensioner. The statistics in this table implies that there is a statistical difference between treatment-village households and control-village households prior to the intervention.

Table C.4: Matching Results (Leibo)

	Match	Treatment	Control	p-value
Wage Income	Before	6,866	6,215	0.612
	After	6,866	5,795	0.457
# of Earners	Before	2.27	2.47	0.141
	After	2.27	2.24	0.840
# of Jobs	Before	2.51	2.77	0.136
	After	2.51	2.45	0.738
Total Hours Worked	Before	3,668	4,456	0.031
	After	3,668	3,883	0.565
Farm	Before	0.87	0.89	0.712
	After	0.87	0.87	0.926
Farm Yield	Before	7,847	9,802	0.113
	After	7,847	7,776	0.955
Farm Profit	Before	695	1,110	0.144
	After	695	666	0.915
Family Size	Before	4.65	5.05	0.081
	After	4.65	4.66	0.981
Minority	Before	0.90	0.95	0.084
	After	0.90	0.91	0.834
Secondary Education	Before	0.07	0.06	0.654
	After	0.07	0.06	0.782
# of Able-bodied	Before	2.67	2.75	0.568
	After	2.67	2.64	0.882
# of Females	Before	2.23	2.54	0.071
	After	2.23	2.27	0.838
# of Children	Before	0.70	0.80	0.078
	After	0.70	0.72	0.796
# of Seniors	Before	0.36	0.18	0.002
	After	0.36	0.39	0.631
Sample Size		86	150	

Rubins' B=19.6; Rubins' R=1.17

Table presents the results of Propensity-score matching. The second and the third column present the variable means of the eligible households and other households. The last column presents the p-values of the two-sample tests between the two groups. Each variable has two rows that compare the means before and after the matching. Results show that the covariates are balanced after the matching.

Table C.5: Heterogeneous Treatment Effects (Leibo)

	Income		Able-bodied		Earner	
	High	**Low**	**High**	**Low**	**High**	**Low**
Wage Income	8,185**	447	4,157	3,287	6,580	3,613*
Total Hours Worked	1,905*	493	-76	1568***	911	1,410***
# of Earners	0.67**	0.43*	0.33	0.49***	0.58	0.45***
# of Jobs	1.06**	0.41	0.18	0.83***	0.77	0.59**
Farm	0.02	-0.02	-0.01	-0.01	0.03	-0.04
Farm Yield	3,628	-899	1,121	1,827	697	1,231
Farm Profit	835	-1,290	-1,708	1,041	-2,270	121
Sample Size	121	131	121	131	97	155

*** p<0.01, ** p<0.05, * p<0.1

Table presents the heterogeneous treatment effects from the PSM-DD estimation. Here, the sample is divided into subgroups based on different critera. High-income group is defined as households with a total income higher than the median. "High able-bodied" is defined as households with more than two (median) individuals between 16 and 65 years old who are able to work. "Multiple earners" means that households have more than two (median) individuals who have been working prior to the intervention. Results show that treatment effects vary across different subgroups.

C.3 Documents and Forms

Leibo County

Labor Income Reward Plan Experiment Commitment

After careful consideration, I hereby wish to participate Labor Income Reward Plan Experiment. During the experiment, My household members and I promise:

1. Full cooperation with the government officials to complete the relevant work involved in Labor Income Reward Plan;
2. Fully comply with the relevant laws and provisions;
3. Take no extreme measures or disturb public order in the case of being expelled from the program due to provision violations;
4. Provide household information and proof of income as required.
5. Cooperate with authorized personnel to verify household income and income sources;
6. Guarantee the authenticity and validity of the information that is provided and will NOT provide any false information; and
7. Will not use the reward received from the program to carry out illegal activities such as gambling, drug abuse, and prostitution.

Name (Print): _____ Signature: _____

ID: _____ Date: _____

PEOPLE'S GOVERNMENT OF LEIBO

(2017) No. 201700001

People's Government of Leibo
Letters of Verification for Employees' Labor Income

Hello _____,

Mr. (Mrs.) _____ has been selected to take part in Labor Income Reward Plan. The participation of the experiment requires the participants to have their monthly earning verified. Please fill out the relevant documents and provide actual work information for this person.

Thank you for your cooperation!

Sincerely,
People's Government of Leibo
Date:

Leibo Labor Income Reward Plan Experiment

Proof of Income

_____ (Employer's Name)

hereby prove that Mr. (Mrs.) _____ is the (1. full-time; 2. contract; 3. Part-time) employee. This employee worked ___ days in the last month with ___ hours every day. The base wage for this employee in the last month was _____ RMB; The bonus was _____ RMB; The total earning from the work in the last month was _____ RMB.

The employer confirms that the information above is authentic and valid.
Please call _____ (Phone) or go to _____ (Address) for inquiries or disputes.

Regards,

(Employer's Seal)
Representative's Signature: _____
Date:

Liangshan Yi Autonomous Prefecture Leibo County Labor Income Reward Plan Application

SID:			Household Type: Ordinary ☐; Registered Poor ☐; Minimum Income Guarantee ☐				Community (Village):				Date:		
Account:			Family size:		Able-bodied:		Address:						
Name	Relationship with head	DOB	Gender	Ethnicity	Education	Currently in school	Disabled	Able to work	MIG	BLS	EPA	ID	Contact
			☐ Male; ☐ Female			☐ Yes ☐ No	☐ Yes ☐ No	☐ Yes ☐ No	☐ Yes ☐ No	☐ Yes ☐ No	☐ Yes ☐ No		
			☐ Male; ☐ Female			☐ Yes ☐ No	☐ Yes ☐ No	☐ Yes ☐ No	☐ Yes ☐ No	☐ Yes ☐ No	☐ Yes ☐ No		
			☐ Male; ☐ Female			☐ Yes ☐ No	☐ Yes ☐ No	☐ Yes ☐ No	☐ Yes ☐ No	☐ Yes ☐ No	☐ Yes ☐ No		
			☐ Male; ☐ Female			☐ Yes ☐ No	☐ Yes ☐ No	☐ Yes ☐ No	☐ Yes ☐ No	☐ Yes ☐ No	☐ Yes ☐ No		
			☐ Male; ☐ Female			☐ Yes ☐ No	☐ Yes ☐ No	☐ Yes ☐ No	☐ Yes ☐ No	☐ Yes ☐ No	☐ Yes ☐ No		
			☐ Male; ☐ Female			☐ Yes ☐ No	☐ Yes ☐ No	☐ Yes ☐ No	☐ Yes ☐ No	☐ Yes ☐ No	☐ Yes ☐ No		
			☐ Male; ☐ Female			☐ Yes ☐ No	☐ Yes ☐ No	☐ Yes ☐ No	☐ Yes ☐ No	☐ Yes ☐ No	☐ Yes ☐ No		
			☐ Male; ☐ Female			☐ Yes ☐ No	☐ Yes ☐ No	☐ Yes ☐ No	☐ Yes ☐ No	☐ Yes ☐ No	☐ Yes ☐ No		
			☐ Male; ☐ Female			☐ Yes ☐ No	☐ Yes ☐ No	☐ Yes ☐ No	☐ Yes ☐ No	☐ Yes ☐ No	☐ Yes ☐ No		
			☐ Male; ☐ Female			☐ Yes ☐ No	☐ Yes ☐ No	☐ Yes ☐ No	☐ Yes ☐ No	☐ Yes ☐ No	☐ Yes ☐ No		
Household member employment information													
Name	Employment		Location		Occupation		Employment		Days worked		Earning		Note

MIG=Minimum Income Guarantee; BLS=Basic Living Subsistence; EPA=Extreme Poverty Aid

Household Farming and Business

Crop farming	Product	Quantity	Estimated market value	Husbandry	Product	Quantity	Estimated market value	Business	Project	Quantity	Estimated market value

Note		**Commitment: I declare that the household information provided above is correct and accurate. I am willing to face any legal consequences for any forgery activity.**
		Signature:
Approval:		**Signature:**
Surveyors and Verification Only		
Attachment and note:		**Signature:**
CHFS approval:		

Note:
1. **Please verify bank account.**
2. **Employment type: farming; employed; self-employed; unemployed.**

Leibo Labor Income Reward Plan Approval

Head		**Gender**		**Date of Birth**	
ID			**Household Type**	**Ordinary ☐; Registered Poor ☐; Minimum Income Guarantee ☐**	
Account			**Bank**		
County			**Township**	**Village**	
Address					
Household Register	**Urban ☐; Rural ☐**				
Contact		**Family Size**		**Attachments**	
Proof of Income	**See the Attachments**		**Work Type**	**Employment ☐; Business ☐; Husbandry ☐; Crop ☐; Other ☐**	
Earning			**Reward**		
Household Approval	**Name:** **Contact:** **Date:**				
Village Committee Approval	**Operator: _____ Inspector: _____ (Official Seal)** **Telephone (Office): _____ Date:**				
Township Committee Approval	**Operator: _____ Inspector: _____ (Official Seal)** **Telephone (Office): _____ Date:**				

Minor Revenue Record (Date: ____ / ____)

County: **Township:** **Village:** **Head:** **SID:**

Date	Earner	Goods/Services	Location	Quantity	Price	Sales	Buyer/Employer		
							Name	Contact	Address

Name: _____

I hereby declare that all the information above is correct and accurate. I am fully aware that I will be fined, losing eligibility, and be expelled from the program in case of any form of forgery.

Instructions:

1. **This form is only for income part-time jobs or high-frequent transactions that are impossible to be verified in the presence of the staff. Each transaction should be no larger than 200 RMB.**
2. **If the goods are stock animals, quantity should be converted to weight. For example, if a household sold two chickens that weight 3 kilograms. The quantity should be 3 kg instead of 2 chicken. If the service is part-time jobs, the quantity should be converted to days worked. For example, if a member worked for two days in the town. The quantity should be 2 days.**

Minor Revenue Record (Date: ____ / ____)

County: **Township:** **Village:** **Head:** **SID:**

Date	Earner	Goods/Services	Location	Quantity	Price	Sales	Buyer/Employer		
							Name	Contact	Address

Name: _____

1. **I hereby declare that all the information above is correct and accurate. I am fully aware that I will be fined, losing eligibility, and be expelled from the program in case of any form of forgery.**
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Leibo County Labor Income Reward Plan Proof of Income

Provided _____(goods/services) for Cash _____, and transaction _____.

Buyer (Signature): _____
Buyer's contact: _____
Seller (Signature): _____

Date and time: _____(Year)/_____(Month)/ _____(Day) _____(Time)
Instruction:
1. **Time should be in 24-hour scale. For example, 1 pm afternoon should be 13:00.**
2. **This form should only be used in case of a buyer cannot provide a receipt.**

Leibo County Labor Income Reward Plan Proof of Income

Provided _____(goods/services) for Cash _____, and transaction _____.

Buyer (Signature): _____
Buyer's contact: _____
Seller (Signature): _____

Date and time: _____(Year)/_____(Month)/ _____(Day) _____(Time)
Instruction:
3. **Time should be in 24-hour scale. For example, 1 pm afternoon should be 13:00.**
4. **This form should only be used in case of a buyer cannot provide a receipt.**

Leibo County Labor Income Reward Plan Proof of Income

Provided _____(goods/services) for Cash _____, and transaction _____.

Buyer (Signature): _____
Buyer's contact: _____
Seller (Signature): _____

Date and time: _____(Year)/_____(Month)/ _____(Day) _____(Time)
Instruction:
5. **Time should be in 24-hour scale. For example, 1 pm afternoon should be 13:00.**
6. **This form should only be used in case of a buyer cannot provide a receipt.**