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Impact of Health Insurance for the Poor on Labor Market Out comes: Evidence from Indonesia.

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Impact of Health Insurance for the Poor on Labor Market Outcomes:

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by

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Table of Contents

List of Tables

List of Figures

Abstract

I examine the impact of a health insurance program for the poor implemented in Indonesia in 2005 on labor supply and informal work measured by employment status outside of the private and public formal sector. As a first step to its ambitious plan for universal coverage, this program extended subsidized health insurance coverage to a large proportion of Indonesia's poor and near-poor population. Using a rich longitudinal survey, I estimate the average treatment effect on the treated using a combined propensity score matching method with difference in differences. The richness of the data allows me to control for a set of observable characteristics used by the government to allocate the benefit as well as an extensive combination of controls at the individual, household and community level. I find a significant negative impact on labor force participation. This impact is driven by women, both at the intensive and extensive margin of labor supply. These results are in line with the fact that individuals with higher value for health insurance are more inclined to modify their labor market behavior. This decrease of labor force participation has important policy implications as it may cause a negative impact on economic development, poverty and socio-economic status of women. The results are not suggestive of an impact on informality. This lack of an effect on informal status is encouraging for developing countries in the verge of implementing universal care reform.

Chapter One: Introduction

Several developing countries have pushed towards expanding health insurance coverage to universal levels in recent years. Member nations of the World Health Organization (WHO) vowed, in 2005, to reform their health systems in order to enhance access to health services for their populations and promote financial protection through expanded coverage (WHO, 2010). In most developing countries, health insurance coverage is provided exclusively through the formal labor market following a contributory "Bismarckian" model in which the insurance is financed by tax contributions that are levied from employers and employees (Frolich, Kaplan, Pages, Rigolini, and Robalino, 2014). This system leaves a large proportion of the population without coverage, increasing the financial risk from illness. The World Health Organization estimates that, each year, more than 100 million individuals are forced into poverty due to financial catastrophe, having to make unexpected out-of-pocket expenditures for expensive emergency care (WHO, 2010).

In order to deal with this issue, some low and middle-income countries have expanded coverage to the remaining population through subsidized schemes financed from general tax revenues. Two prominent examples of such expansions are Mexico's Seguro Popular[1] and the Thai Universal Care Scheme[2] (30 Bahts Scheme). Seguro

[1] See Bosch, Cobacho and Pages (2012) for a background on the health insurance reform in Mexico.
[2] See Hanvoravongchai (2013) for a description of the health insurance program in Thailand.

Popular, established in 2003, provided a comprehensive benefit package of broad health services for about 50 million Mexicans formerly uncovered and, as a consequence, Mexico reached near universal coverage levels (Bosch et al. 2012). In 2001, the government of Thailand introduced the 30 Bahts Scheme, a universal health care coverage program, by extending coverage to 18.5 million previously uninsured Thais (Towse, 2004). Indonesia showed similar intent in recent years, through the implementation of its Health Insurance for the Poor (Askeskin[3]) program. It sets the first step towards universal health coverage.

Indonesia implemented one of the largest health insurance expansions in the developing world. Following the Asian Financial Crisis of 1997, the government of Indonesia decided to invest in safety net programs in order to protect the chronic poor and vulnerable near poor from the adverse impacts of the crisis (Sumarto and Bazzi, 2011). This led to the Social Security Reform of 2004, which paved the way for universal health care through the introduction of Askeskin in 2005. This subsidized health insurance program targeted the poor and near poor and was designed to complement previously provided social health insurance schemes for public and private sector employees. Unlike the social insurance schemes for these employees, Askeskin was entirely funded by the government (Sparrow, Suryahadi and Widyanti, 2013), and hence, did not require a contribution from the recipients.

There has been an ongoing debate among policymakers on whether large expansions of health insurance can have unintended impacts on the labor market

[3] Askeskin stands for Asuransi Kesehatan Masyarakat Miskin or Health Insurance for the Poor. Health Insurance for the Poor and Askeskin will be used interchangeably for the purpose of this study.

decisions of households in low and middle-income countries. In particular, economists conjectured two potential key effects, a movement of workers towards the informal sector of the labor market and a decrease in labor supply (Wagstaff and Manachotphong, 2012). The rationale behind such effects is intuitive. First, the existence of such noncontributory programs render informal employment more attractive since fringe benefit contribution is not required (Duval and Smith, 2011). Askeskin provides a comprehensive benefit package free of charge, and holding everything else equal, renders informal employment relatively more appealing. Therefore, I expect an effect on the decision to uptake informal jobs at the margin. Second, government subsidized health insurance provides less incentives to work for individuals that place a high value on health insurance. For individuals that value health insurance and spend a considerable amount of their resources on health care, the free provision of health insurance is analogous to an increase in income. This should affect both the intensive (the quantity of work) and extensive margins (the decision to participate) of labor supply negatively. Overall, such effects may have adverse impacts on growth and investment, increase risks faced by poor households and lead to more poverty and inequality. Moreover, these negative behavioral labor responses are possible mechanisms through which safety net programs can generate poverty traps and ultimately impede economic development. As a result, the presence of these potential effects is worth investigating since the policy implications of such effects are important.

Utilizing the Indonesian Family Life Survey, a rich ongoing longitudinal survey that collects a large representative sample at the individual-level, I use a propensity score matching method with difference in differences in order to estimate the impact on labor supply and informal status. This methodology has proven efficient to estimate the impact of health insurance programs for the poor in Indonesia (Johar, 2009) but has also been extensively used in the program evaluation literature on labor outcomes (Smith and Todd, 2000; Smith and Todd, 2005; Heckman et al., 1997, Heckman et al.,1998). Taking advantage of the large countrywide expansion of health insurance to the poor and near poor, the analysis does not show evidence an effect on informal status. However, I find evidence of an effect on labor supply both at the intensive and extensive margins driven by women and individuals living in urban areas. Overall, the results are consistent with the fact that individuals that value health insurance more should exhibit a larger impact. Such a decrease in labor supply goes against the redistributive and welfare improving goals of the program.

Chapter 2 presents Indonesia’s institutional structure as well as a description of the program. Chapter 3 discusses the potential labor impacts of the program. Chapter 4 provides an overview of the current state of the literature. Chapter 5 describes the empirical methodology used in this paper. Chapter 6 introduces the Indonesian Family Life Survey and the sample used in the analysis. Chapter 7 exposes preliminary regressions. Chapter 8 exhibits the assumption tests, descriptive statistics and the results. Chapter 9 displays limitations of the study. Chapter 10 concludes.

Chapter Two: Institutional Background and Introduction of Askeskin

2.1. Indonesia's Health Care Environment

Indonesia is an archipelago that covers over 1.81 million kilometers squared[4] and consists of more than 17,000 islands. As of 2007, its population counted 232 million people, making it the fourth most populated country in the world. It contains a very diverse population ethnically, culturally, economically and socially due to its rich history and unique geography. Bahasa Indonesian is the official language of the country. As is usually the case in low and middle-income countries, it is characterized by a large rural population. In 2007, 52 percent of its population lived in rural areas[5].The country is divided in 33 provinces (since 2006[6]) which are each directed by a governor. The provinces encompass 405 districts, distributed into some 6,543 sub-districts in which there are almost 75,244 villages[7]. In 2007, Indonesia's GDP per capita was 1,860 dollars, making it a lower middle-income country. However, the country has since then enjoyed annual GDP growth of over 5%[8]. Indonesia's total health expenditure is under 3% of GDP in 2007[9], which is low compared to the average of OECD countries[10].

[4] data.worldbank.org
[5] data.worldbank.org
[6] Between 2001 and 2006, the country expanded from 27 to 33 provinces.
[7] depdagri.go.id
[8] data.worldbank.org
[9] data.worldbank.org
[10] www.oecd.org/els/health-systems/Briefing-Note-INDONESIA-2014.pdf

According to a study conducted by the Indonesian Joint Committee on Reducing Maternal and Neonatal Mortality in Indonesia (2013), the Indonesia's health care system is characterized by its focus on primary health care. In 1968, the government introduced the Community Health Centers (Puskesmas) as primary health care providers (Triratnawati, 2006). These facilities are assisted by hospitals and other types of health care centers at the community level. It is at the village level that the most basic level of primary care is situated. Most of the facilities are community based and offer the most basic level of curative and preventive care. This is usually also the first point of contact between patients and providers. The health center has several goals. It emphasizes basic preventive and curative care procedures, the promotion of health, sanitation, mother and childcare and family planning, and minor emergencies (Joint Committee on Reducing Maternal and Neonatal Mortality in Indonesia, 2013).

Secondary and tertiary care are usually provided based on referrals in Indonesia (Harimurti, Pambudi, Pigazzini, and Tandon, 2013). Secondary health care services are offered by about 2000 hospitals[11] and tertiary health care services are only available at the province level and major cities. The higher-level institution in charge of the health care monitoring in Indonesia is the Ministry of Health (MoH) whose responsibility is to manage health care programs and policy and to oversee the general functioning of the health care system (Joint Committee on Reducing Maternal and Neonatal Mortality in Indonesia, 2013).

[11] The Indonesian health care system is a combination of public and private institutions. Indonesia comprises 1,632 secondary-care hospitals, of which about (838 private). Indonesia has about 376 tertiary hospitals (76 of which are private) (Harimurti, 2013).

The Indonesian health care system is characterized by its inequity in access to health care. There is a large schism in health care provision between the affluent part of the population and the indigent due to uneven distribution of health care services and low coverage among the poor. A similar split exists between urban and rural areas.

2.2. Social Health insurance in Indonesia and Health Insurance for the Poor

In 1997, Asian countries were hit by a serious financial crisis (Asian Financial Crisis), which had severe impacts across the continent. In Indonesia, the situation was particularly disastrous, the Indonesian GDP fell by 15% in 1998 (Thomas and Frankenberg, 2007) and poverty rate went from 15.7% in 1996 to 27.1% in 1999 (Suriyahadi and Sumarto, 2003). In order to tackle rising poverty rates, Indonesia established several programs to protect its population. These social assistance programs were aimed to support the poor and vulnerable[12]. They were designed to pull the poor out of poverty and prevent the vulnerable from becoming poor. This desire to reduce poverty would later lead to the introduction of the Health Insurance for the Poor or Askeskin (this will be one of the largest program in scale and in terms of resources allocated). Before the introduction of Askeskin, only about 10% of the population had coverage, generally through the formal sector of the labor market (Sparrow et al, 2013). Health Insurance for the Poor was introduced as part of the larger goal to cover the entire population. It is considered a first step to universal health care coverage. In fact, unlike other temporary social safety net programs, the vision for Askeskin was long-term. Askeskin was meant

[12] Several programs were introduced across several years after the crisis, most notable ones being: The subsidized rice program (Raskin), Health insurance for the Poor (Askeskin), the cash transfers for poor students (BSM), a conditional cash transfer (PKH), the temporary (unconditional) cash transfer (BLT) and the Health Card program.

to provide a level of coverage similar to ones enjoyed by formal sector employees, and provide long-term health coverage to the bulk of its uncovered population.

The social security and health insurance system in Indonesia has historically been insufficient. This lack of adequate protection represented a paramount challenge for Indonesia. Individuals used their extended family or their communities as last resort to seek protection (ILO, 2008). This precariousness and uncertainty exacerbated poverty, ill health and lead to higher mortality (ILO, 2008). Since health insurance was only limited to the formal employment sector, which accounts for a small proportion of the labor market, a large majority of workers were excluded from coverage. This kept the population vulnerable to impoverishment. In order to understand the institutional environment in Indonesia prior to the introduction of Askeskin, the next section introduces the existing formal sector schemes: Askes and Jamsostek.

2.2.1. Askes

Askes is a contributory social security scheme for public sector workers, which includes comprehensive health insurance coverage among other programs (employment injury, death insurance, pension and retirement) (ILO, 2009). It was introduced in 1968 in order to provide comprehensive protection for civil servants against several risks. In 2007, Askes covered about 8.28% of the population[13]. It represents approximatively 19.2 million people. The contributions that fund this program are split between employees and employers, who each pay 2% of the base salary. In terms of health insurance coverage, **Table 1** shows that the program provides comprehensive coverage for both outpatient

[13] Based on authors' estimates from the fourth wave of the Indonesian Family Life Survey.

and inpatient care exclusively through public providers and requires copayments for certain medical services. Askes also covers the armed forces and police through a separate fund called Asabri (ILO, 2009). This program is funded both through contributions from beneficiaries and government subsidies.

2.2.2. Jamsostek

Jamsostek is a social security program for employees operating in the private sector and insurance four types of risk: old age, work-related injury, health, and death (ILO, 2009). In 2007, Jamsostek covered approximatively 4.84% of the population[14].This represents about 11.18 million individuals. In this scheme, the burden of the premium is borne only by the employer at a level of either of 3% or 6% percent depending on whether the employee has dependents or not, respectively (Aji et al., 2013). This scheme extends a comprehensive benefit package. It offers access to both public and private facilities for outpatient care but only public facilities for inpatient care (ABD, 2007). It is mandatory for firms that have ten workers or more or a payroll of over one million rupiahs per month[15]. However, there is an option to choose private insurance if the employer can provide better coverage to their employees (Thabrany, 2011). This led to certain firms with more than ten workers to provide cheaper alternative coverage through the private insurance market. As a result, Jamsostek suffered from relatively low take up. Moreover, the enforcement of the mandate was not perfect even among firms with more than ten workers leading to some workers not having coverage altogether. Several issues affected the enforcement of the mandate. ILO (2008) points out that the private sector suffered

[14] Based on authors' estimates from the Fourth Wave Indonesian Family Life Survey.
[15] https://www.ssa.gov/policy/docs/progdesc/ssptw/2010-2011/asia/indonesia.html

from contribution evasion through under declaration of contributory wages which led to some workers working for formal firms without being formally registered. In addition, Jamsostek lacked proper monitoring and suffered from insufficient resource allocation which caused various abuses. There were no inspectors under its supervision to ensure compliance, and its monitoring depended on the labor inspectors that roll under the regional government to ensure the proper application of the legislation. This understaffing led to compliance issues (ILO, 2008).

2.2.3. Private Health Insurance

As it is usually the case in developing countries, private health insurance markets are underdeveloped and prohibitively expensive for the majority of the population. In most countries, they provide a negligible part of the population's health coverage. In Indonesia, there is a private health insurance market, but it only accounts for less than 1% of the population (based on the fourth wave of the Indonesian Family Life Survey) and, hence, I forgo describing it in detail for the purpose of this study.

2.2.4. The Health Insurance for the Poor (Askeskin)

Before the introduction of Askeskin, health insurance coverage in Indonesia was low and Indonesian authorities realized the need to reform the system. In August 1998, in the midst of the Asian Financial Crisis, the government made an attempt at increasing coverage for the poor through reforming and revitalizing a previously existing fee waiver program (Health Card[16] program) for public health care facilities (Sparrow, 2008). The purpose of this program was to provide indigent households basic health care provision

[16] The Health Card program was originally implemented in 1994 to cover the poorest Indonesians.

at the community level. However, it provided only a limited benefit package and had several organizational issues. The program was discontinued in 2005 to pave the way to Askeskin (World Bank, 2006). In 2004, the Indonesian government ratified the National Social Security System Law, which confirmed the aspiration of the government to expand social health insurance to a universal level (ILO, 2008). The first step towards this goal was the establishment of the Askeskin program, which targeted the poor and near poor. It was implemented in the beginning of 2005 and made substantial progress in the first year of implementation. The target setting and implementation of the program followed several steps.

From the beginning of January to the end of May 2005, the government set a target of 36.1 million enrolled individuals. This number represented the initial estimate of the poor population in Indonesia by the Central Bureau of Statistics (BPS[17]). This amounted to about 17 percent of the population. The benefit was allocated at the district level, with districts-level quotas based on an estimate of the number of indigent individuals per district. Local authorities played an active role as they delivered lists of qualifying individuals to local branches of Askes (the state-owned social insurance body in charge of providing the benefit) (ILO, 2008). After June 2005, the government decided to increase the target to 76.4 million individuals in order to include the near poor in this program (ILO, 2008). Based on data from the fourth wave of the Indonesian Family Life Survey, about 18.4% of the population received the benefit by the end of 2007 (this represents about 43

[17] Central Bureau of Statistics and BPS will be used interchangeably in this study.

million individuals). **Table 2** shows health insurance coverage before and after this reform.

Figure 1 and **Table 3** show the trend in public health expenditure between 1995 and 2007 based on World Bank estimates (2008). Health expenditure as a fraction of total national spending rose from 2.6 percent in 2001 to 4.2 percent in 2005 (the year of implementation of the Askeskin). The years following the implementation of Askeskin up to the end of 2007 exhibit a large increase in public health expenditure. In fact, real national health expenditures more than doubled from an initial 9.3 trillion rupiahs (about 1 billion dollars) in 2001, to 20.1 trillion rupiahs (about 2.1 billion dollars) in 2007. These figures provide evidence of the scale of the program and the important material impact associated with it.

Askeskin covered health services from the primary to the tertiary level. More precisely, it provided free outpatient and inpatient care at public health centers and hospitals, but it also offered access to a third of private health facilities (ILO, 2008; Sparrow et al., 2013). Secondary provider claims (hospitals) were paid through a limited negotiable fee-for-service, whereas primary health centers were compensated based on capitation[18].

Targeting for the allocation of membership in Askeskin was based on district level targeting in which eligible individuals were selected within a specific district. The mechanism used within each district was based on a proxy-means tested method computed by the Central Bureau of Statistics (Harimurti et al., 2013). This proxy-means

[18] It is a health care payment system based on a fixed fee per patient per unit of time paid to the physician for the delivery of medical care.

tested score was used to determine district quotas. It is composed of the weighted average of several poverty indicators that were provided by the Central Bureau of Statistics[19]. The targeting and eligibility process is described in detail in **Section 2.3**.

The financing of the program was based on a monthly premium paid by the central government of an amount of 5,000 rupiahs (approximately US$ 0.50) with no contribution from individual beneficiaries (ILO, 2008). It was paid for mainly by savings from fuel subsidy removal (Perdana, 2014). The entire budget that the government allotted for 2005 was set at 3 trillion Rupiahs (around US$ 300 million). PT Askes, a state insurance company, pooled risk at the district level.

Table 1 provides a description of the specific characteristics of the three different health insurance schemes (Askes, Jamsostek, Askeskin). The benefit package provided through Askeskin is more comprehensive than the other schemes (Aji et al, 2003). **Table 2** provides the proportion of individuals enrolled in the main health insurance programs before and after the implementation of Askeskin based on the fourth Wave of the Indonesian Family Life Survey. Two years after its implementation, it became the largest health insurance program in size in Indonesia.

Askeskin has been shown to improve access to health care and consumption of outpatient care by the poor (Sparrow et al., 2013). Aji et al. (2013) explore the impact of all Indonesian schemes on out-of-pocket expenses and find that Askeskin decreased out-of-pocket spending by 34%. They also find that Askes decreased out-of-pocket expenditures by 54% whereas Jamsostek did not have any effect on out-of-pocket

[19] Appendix A provides a description of the computation used in the proxy means tested score.

expenditures. This shows that Askeskin has had meaningful success in ameliorating access to health services and relieving households from financial costs of health care.

2.3. Targeting Mechanism and Eligibility

Targeting was decentralized at the district level (geographic targeting) where resources were allocated to district officials to select eligible households within each district (Sparrow, 2013). The poor and near poor were targeted through proxy-means testing and criteria from local governments (Harimurti et al., 2013).

2.3.1. Proxy-Means Testing Score Calculation

In order to effectively reach the intended population for the safety net programs, the government needed to create a reliable targeting mechanism that could distinguish the poor and near poor from the non-poor. Effective targeting increases the chances that the correct households (poor and near poor) will receive assistance. However, the government lacked data on key metrics to properly identity households. In fact, in developing countries, due to widespread informality, significant household production, and absence of reliable data, it is problematic to produce a dependable measure of income. Households are more likely to underreport their income and attempt to appear as poor in order to be eligible for social welfare programs. Therefore, it is common that the governments of low and middle-income countries use a statistical method called proxy-means testing in order to identify poor and near-poor households.

Proxy means testing uses data on household characteristics to proxy for household income, consumption or expenditures. The evident benefit of proxy means testing is that, in low and middle income countries, “good” predictors of welfare such as socio-economic characteristics, demographic characteristics, housing characteristics, and ownership of household durables are easier to gather and verify than direct measures like income. There is evidence that proxy means testing is among the most effective targeting methods (Alatas, Banerjee, Hanna, Olken, and Tobias, 2011). Coady, Grosh and Hoddinott (2004) assess several cash transfer programs in developing countries. The study finds that it performs better in targeting households than many other methods.

Cameron and Shah (2014) provide an excellent description of the proxy means tested methodology used for targeting in Indonesia. It was implemented to better target all safety net programs in Indonesia from 2005 on. It was first intended and utilized for the Unconditional Cash Transfer program (BLT) which started in 2005, ran for 12 months (from 2005 to 2006) and provided 10$ a month to 19 million poor and near poor households (a total of $120). In order to target households, the Central Bureau of Statistics (BPS in Indonesian) conducted a National Poverty Census Survey (PSE05) in order to assess the poverty status of households based on observable characteristics. In order to produce the survey, the government first combined data from the comprehensive annual Susenas[20] survey for three consecutive years prior to the introduction of the cash transfer program "to identify 14 variables that together had the greatest capacity to predict household expenditure" (Cameron and Shah, 2014). This identification process relied on running logit regressions models for each of the districts in Indonesia (Cameron and Shah, 2014). The variables identified for this purpose were:

1) Households floor area

2) Households floor type

[20] The National Socioeconomic *Survey* (*SUSENAS*) are large-scale multi-purpose socioeconomic household surveys. They were introduced in 1963-1964 and been fielded on a yearly basis. It is a rotating panel of a sample of 60000 individuals (www.rand.org).

3) Type of wall

4) Type of toilet facility

5) Water source

6) Source of lighting

7) Type of cooking fuel

8) Frequency of meat consumption

9) Frequency of food consumption

10) Frequency of clothes purchase

11) Accessibility to health center

12) Household head education level

13) Household head employment sector

14) Presence and ownership of five different durable assets: television, motor vehicle savings account, livestock and precious metals.

This process produced district-level weights that were subsequently used to compute a value for the score at the household level. The survey was then tailored for the collection of those data. **Appendix A** provides a description of the construction of the index. In my analysis of Askeskin, I use the original weights computed by the BPS in the production of the proxy means tested score[21].

2.3.2. Overall Targeting Method

As mentioned above, the targeting was implemented at the district-level. The BPS requested lists of potential poor households from community leaders and village officials. Then, the BPS sent enumerators to those villages to assess their poverty level using the

[21] The weights were provided for this analysis by the BPS.

PSE05 Survey. After collection of the data, it was sent to the BPS headquarters where the district level weights were used to assign each household a score (Cameron and Shah, 2014). The eligibility of each household was based on that score. Households meeting a certain cut-off point were considered to be indigent or near poor.

Following this process, the national government (BPS) produced lists of eligible households based on the estimated number of poor households in the district. They also produced quotas from those lists, which they delivered to the district governments (Harimurti et al., 2013). The district governments then confirmed those data using their own methods to meet their budget and quotas. They then delivered the final lists to PT Askes (state-owned social insurance enterprise) which produced cards to be disbursed at the community level (by local branches of PT Askes). Askeskin started before the proxy-means tested score was implemented[22]. As a result of the unavailability of the data in the first semester of Askeskin's implementation, the proxy-means tested score was only used starting around the end of 2005 and onwards. Therefore, in practice, even though many districts used the proxy-means tested method; there were variations across districts and communities in the way households were targeted. If the proxy-means tested score was not used, districts generally used a subset of the indicators related to the ones in the proxy-means tested method or criteria based on the Health Card program[23]. However, there is anecdotal evidence that in some villages the allocation was needs

[22] It was first used in the BLT program that started in October 2005.

[23] The Health Card program used the criteria from the National Family Planning Coordination Board (BKKBN), which were used to allocate several social programs prior to Askeskin. This targeting method is based on five basic needs criteria (food consumption, the quality of the house's building materials, ownership of clothes, and religious practices, households head educational status) (ASEAN and World Bank, 2009). Households are deemed poor if five one basic needs condition is not met. This information is collected on a census basic across the whole country.

based. Bachtiar, Wibisana and Pujiyanto (2006) indicate that, in some cases, Askeskin was allocated based on health status.

In evaluating the targeting of Askeskin, Sparrow et al. (2013) find that Askeskin was successful at targeting the "poor and those most vulnerable to catastrophic out-of-pocket health payments" (Sparrow et al., 2013). Even though there was evidence of leakage, Askeskin targets the poor accurately since about 70% Askeskin recipients are part of the 40% least well off of the country (Sparrow et al., 2013).

Chapter Three: Health Insurance for the Poor and Potential Labor Market Outcomes

The Health insurance for the Poor has the potential to affect the labor market decisions of households on several dimensions. Providing free health benefits may affect the decision of searching for jobs, supplying one's labor and choosing between formal and informal employment (Aterido, Hallward-Driemeier, and Pages, 2012). This study focuses on examining the potential effects on an individual's decision to work and, conditional on working, on the individual's decision to take up informal sector employment.

3.1. Labor Supply

3.1.1. Model of the Decision between Employment and Non Employment

Provision of government-subsidized health insurance is equivalent to an increase in income as it increases the value of non-monetary benefits from unemployment and reduces the opportunity cost of non-employment. In fact, the decision of reducing the quantity of work or dropping out of the labor force entirely would partially depend on the value that the individuals associate to the benefit. The more comprehensive the coverage and the lower the contribution, the higher the perceived benefit and value to the recipient. The more a person values the benefit offered, the more important the expected effect on labor supply.

I modify a model by Azuara and Marinescu (2013) used to characterize the impact of Seguro Popular on informality in order to illustrate the decision of employment versus non-employment. Let us assume that the utility from working is given by:

$$U_e = w_e + \delta h_e$$

Where w_e is the wage rate received if the individual decides to take up employment, h_e is the non-monetary benefits received from working, and δ is the value that workers place on an unit of non-monetary benefits.

The utility from non-employment is given by:

$$U_n = \rho_n + \delta h_n$$

Where ρ_n is the benefit from non-employment (psychic benefit) and, h_n is a non-monetary benefit received by the individual such as Askeskin in the case of Indonesia. The individual will choose not to work if $U_n - U_e > 0 \;\; or \; (\rho_n - w_e) + \delta(h_n - h_e)$>0.

The implementation of Askeskin increases the value of non-monetary benefits from unemployment (represented by h_n) because it provides a comprehensive benefit package without cost sharing and no contributions (premiums, copayment or deductibles). In fact, considering **Table 1**, it appears that the program is at least as generous as the formal sector programs previously in place. In addition, the benefit gives access to any public health center in the country and some private health centers. It also covers all dependents in the household as well as the spouse. Hence, everything else held equal, a person receiving the health insurance benefit is more likely to choose not to participate as opposed to participate. A decrease in labor supply, because of the introduction of Askeskin, is expected.

The effect of Askeskin also depends on the value of δ. In particular, the more a person values health insurance, the more important the expected effect on labor supply. As a result, groups that have higher propensity of health care consumption or higher probability of illness should be particularly affected. For instance, women of childbearing age (particularly married women) and older individuals should have a higher valuation of health insurance. Older individuals consume more health care products and services and have a higher probability of suffering from a health issue. This results in a higher associated value to health insurance and a higher potential sensitivity of labor supply to provision of health insurance. Women of childbearing age might have a higher value for health insurance since they expect to spend considerable amounts on health care due to the costs of health care services related to pregnancy and childcare. In some cases, they might even be responsible for the health care of their spouse. They are therefore more likely to reduce their labor supply[24]. This trade-off between homemaking/childcare and labor market activities is even more important in a developing country context.

3.1.2. Labor Supply of Women

In Indonesia, the labor force condition of women is particularly precarious. They suffer from less job security due to the uncertainty of their employment and as a result to lower and less stable social security coverage. Therefore, women's health insurance coverage is uncertain as it depends on their ability to hold their position (if their job provides health insurance) or on their ability to benefit from the coverage of their spouse.

[24] In their review of the U.S. literature on the impact of health insurance on labor supply, Gruber and Madrian (2002), conclude that "health insurance is an important determinant of the labor behavior" of individuals of retirement age and married women.

In 2007, they had significantly lower labor force participation than men (50% vs 80%) and sustained a higher unemployment rate than men (11.8 percent vs 8.5% respectively) (ILO, 2008). Women workers had a higher likelihood of working in the informal sector (61% for men vs 68% for women) and to partake in unpaid activities. Dependence on work in the informal economy results in them being less likely to be protected by social security systems, even though they are more exposed to risks related to their role in the family and their life cycle. Therefore, their vulnerability would lead to a higher value for health insurance. This relative vulnerability should be reflected by a higher value of δ (which, all else held equal, would lead to a higher impact on labor supply).

3.1.3. The Intensive Margin of Labor Supply

As mentioned above, the provision of Askeskin could affect the decision to participate in the labor force. This decision between employment and unemployment is referred to as the extensive margin of labor supply. Nevertheless, health insurance expansion may also affect the intensive margin of labor supply (hours worked or weeks worked) if the person is employed. The labor supply effect could be observed on the intensive margin where individuals decrease their quantity of time working as the “extra” income that would be spent on health insurance and health care is saved. This would lead to the substitution of some labor market time to either leisure, house-care or family caring activities as health insurance reduces the costs of health care and the probability of catastrophic loss due to an unexpected event. This is relevant in the case of Indonesia since households, especially poor and near-poor households, spend a relatively significant fraction of their income to purchase health care products and services. Poor households are also more likely to suffer from catastrophic health spending. As such, they place a high value on health insurance coverage. In many cases, individuals cannot afford to completely drop out of the labor force and therefore the impact may be only observed at the intensive margin where certain members of the households decide to reduce their time spent in labor activities to partake in other activities.

3.2. Labor Informality

3.2.1. Model of the Decision between Formal and Informal Work

The provision of health insurance does not only have the potential to affect the decision to work and the amount of working, but it also has the potential to affect the sector in which the individual decides to provide his labor. The extension of subsidized health insurance beyond the formal sector of the labor market increases the "implied wage" in the informal sector. As a result, holding everything else equal, a person receiving the health insurance benefit is more likely to choose informal employment over formal employment since the utility from informal work increases due to the fringe benefit provision.

In their study of Seguro Popular, Azuara and Marinescu (2013) present a simple model to illustrate the potential impact of subsidized health insurance availability on the decision between formal and informal work.

Let us assume that the utility of working in a formal job is as follow:

$$U_f = w_f + \gamma h_f$$

Where w_f is the formal sector wage, h_f are non-monetary benefits received by formal sector employees, and γ is the value of non-monetary benefits to workers. Likewise, the utility of being employed in the informal sector follows the below relationship:

$$U_i = w_i + \gamma h_i$$

For a person to choose to work in the informal sector the following condition needs to hold:

$$U_i - U_f > 0 \quad or \quad w_i - w_f + \gamma\left(h_i - h_f\right) > 0$$

The introduction of Askeskin increases the value of benefits in the informal sector (represented by h_i) increasing the utility from informal work. Therefore, a person receiving the health insurance benefit is more likely to choose informal employment over formal employment.

Theoretically, this effect could emerge in Indonesia for two reasons. First, the introduction of Health Insurance for the Poor should have increased the value of h_n because it provides a comprehensive benefit package (comparable to those available for public sector employees and private sector employees), with no premium and without cost sharing. Moreover, it is portable (giving access to any public health center in the country and some private health centers) and covers all dependents. In addition, firms in the formal sector are only required to cover their employees if they have more than ten workers. Therefore, there are workers in registered firms that are not covered by social security or health insurance.

3.2.2. Informal Work and Segmentation of the Labor Market

This effect is expected to be observed only if the labor market in Indonesia is integrated (there is no segmentation between the informal sector and formal sector of the labor market). The labor market in developing countries is viewed as a dual market where the formal sector is a superior choice (with a greater potential for earnings), whereas the informal sector is considered as unattractive and transitory (in wait for a “better” formal job). The informal sector can comprise a wide range of jobs from assembly and manufacturing unregistered workers in larger registered and unregistered firms (for example in the garment and construction industry), agricultural or mining seasonal workers, small producers or petty traders such as street vendors to a range of casual employment arrangements. Informal work can be found in most industries in developing countries. In some cases, the informal economy also consists of small-scale entrepreneurs that have a substantial capacity for innovation and growth. The formal sector is composed of workers in both smaller and larger firms in the private sector as well as the public sector. For example, employees for large corporations and smaller registered companies in virtually all industries as well as individuals working in civil services, public sector units and government services are all considered formal sector employees. Liberal professions such as doctors, lawyers and architects would also be part of formal sector employment.

The formal sector cannot absorb all employees that wish to enter it because wages are set above market clearing levels due to structural or institutional factors. The jobs are limited and the demand for formal jobs is greater than the supply. Thus, not all individuals that demand a formal sector job can obtain one. This rationing leads to workers being excluded from the formal sector, which pushes them to integrate informal jobs instead. The latter accommodates those who are incapable of integrating a formal sector job but need to work until they can find better employment.

There is an extensive literature on the nature of formal and informal labor markets in developing countries. Harris and Todaro (1970) model the segmentation of the market in developing countries, with a more desirable formal sector, in order to explain the labor movements from rural to urban areas in developing countries. Fields (1975) introduces an urban informal sector to characterize the labor market accurately in a developing country's urban setting. In his work, the urban informal sector serves as "transitory" employment for individuals that are unable to find formal sector jobs and need to remain employed for survival purposes. This mainstream dual market framework has framed labor market policy in several developing countries, in which government institutions have tried to reduce the size of the informal labor sector by reforming labor market institutions and laws.

More recent research went against the idea that informal work is an employment of last resort and represented the informal labor market as a sector in which individuals decide to work intentionally in order to make themselves better off. Magnac (1991) shows that earnings differential that is observed between the informal and formal sector is due to unobserved heterogeneity between employees. Thus, controlling for unobserved characteristics, wages are not different between the two sectors. Using longitudinal data from Mexico, Maloney (1999) claims that a portion of informal workers in Mexico willingly take up jobs in the informal sector and are not actively queuing for formal sector jobs.

Previous research has argued that informality could be beneficial as it allows the provision of employment for everyone that needs a job. For instance, it can provide flexible work arrangements for women that need proximity to their household for family reasons (Alatas & Newhouse, 2010) or low-skilled workers that need employment for subsistence and meeting their family's needs. Therefore, the informal sector may play a favorable role as it can absorb a large proportion of the population that are vulnerable to financial risks and help them subsist. Nevertheless, a large empirical literature finds that an increasing informal sector may cause lower economic growth, low tax revenues and public goods provision.

In the case of Indonesia, the current consensus among researchers is that the labor market is segmented and informal jobs are “inferior” as workers in the informal sector would prefer to access a formal sector job. The informal sector is seen to perpetuate poverty, because wages are lower than they would be in the formal sector (Newhouse and Alatas, 2010), they do not offer employment security and do not cover them against risks of illness, disability, old age, disability and death (OECD 2008; ILO 2008). The Indonesian government has made a priority of limiting the size of the informal sector through structural or institutional reform tailored towards the creation of more formal sector jobs (Newhouse and Alatas, 2010). However, recently, using the Indonesian Family Life Survey, Sharma (2013) finds that the Indonesian labor market does not exhibit a wage differential after controlling for firm size. This new evidence indicates the possibility that the Indonesian Labor market is integrated. Therefore, more research is required to reach a definite answer on the nature of the Indonesian labor market. This is an important question, as the extent to which the Health Insurance for the Poor or any future program can distort the labor market incentives is directly dependent on the structural differences between formal and informal sector.

Chapter Four: Literature Review

The goal of this section is to analyze the existing research related to the topic. This study relates to two streams of literature that examine the impact of health insurance and poverty alleviation programs, respectively, on labor supply and informality. There is scarce evidence on the impact of health insurance or poverty alleviation programs on labor supply decisions in developing countries. The majority of the literature examines this question in the US setting. Even though there is evidence of an impact of health insurance expansion on labor supply in the US, it cannot be extended to developing countries due to the large heterogeneity in institutional and market structure. The impact of health insurance expansions on informality is a topic that has been studied in a limited set of low and middle-income countries. This is due to the small number of large-scale universal-like health insurance expansions in the developing world. There is suggestive evidence of an effect on informality in certain studies. However, the effect is small and highly reliant on the structure of the program and the institutional environment of the country. This renders the results difficult to generalize to other settings.

4.1. Social Health Insurance and Labor Market Outcomes in Developing Countries

It is in the context of Mexico that the topic has been studied the most. Aterido et al. (2011) exploited the Mexican Seguro Popular social program to investigate its impact on labor market decisions. Seguro Popular (SP) is a social health insurance scheme introduced in 2003 that provides coverage to the 55% of the population that are not employed in the formal sector. It was launched as a pilot program but was progressively extended across the country. In 2010, the government estimated that every Mexican household that was not covered by a formal scheme had coverage through Seguro Popular. This study exploits panel data from the National Employment Survey over a period of nine years and finds that Seguro Popular reduced the probability of being in the formal sector by approximatively half of one percent. Their analysis also uncovers lower labor force participation. This is possibly the most robust study on the impact of social health insurance on labor market outcomes as it uses panel data to control for individual and households fixed effects. It also uses the longest period to study the effect.

Parker and Scott (2008) utilized the Rand Mexican Family Life Survey panel to study the impact of Seguro Popular. They investigate differential effects on informality between urban and rural areas. They find a positive effect on the probability of informal employment in rural municipalities, but the absolute magnitude of the effect is small. They do not observe any effect in urban areas. They also use a data set from the 2000 and 2005 censuses and do not find any significant results.

Barros (2008) investigated the effect of Seguro Popular on labor outcomes. He used four repeated cross-sectional surveys over the period spanning from 2000 to 2006 and employed a triple-difference estimation method but did not find significant effects on the probability of being in the formal sector, hours worked or on labor force participation.

Wagstaff and Manachotphong (2012) examine the impact of the roll-out of the Universal Health Care reform in Thailand (30 Bahts Scheme). They utilize 68 labor force surveys, and exploit the staggered roll-out of the social insurance expansion to identify labor market distortions caused by the new universal health care scheme implemented in

2001. Their findings indicate that the reform has encouraged employment (mainly for married women), has led to reduced formal sector work for married men, and increased informality (mainly for married women).

A notable study by Azuara and Marinescu (2013) examine the impact of Seguro Popular on labor informality and wages. They exploit the gradual roll-out at the municipality level using a difference in differences estimation and do not find any effect of Seguro Popular on informality in the overall population and across the majority of their specifications. They do find, though, that informality increases by 1.7% for less educated workers.

There are also several studies of health insurance expansions or poverty alleviation programs on informality or other labor outcomes conducted in other Latin American countries, Europe or Asia. Camacho, Conover and Hoyos (2009) investigate the effect of a subsidized health insurance for the poor (Regimen Subsidiado or SR) in Colombia on informality. Regimen Subsidiado, which was implemented in 1993, was provided to the poor and the unemployed and was financed from both government funds and formal sector contributions. The benefit package provided was less generous than in the formal sector but covered all individuals living in the household. Camacho et al. (2009) utilize repeated cross sections from the Colombian household survey covering a period from 1992 to 2005. Their estimation results indicate an increase in informality after the reform of about 4 percentage points.

Gasparini et al. (2007) do not study a health insurance program but a poverty alleviation program with similar expected effects. They investigate the effect of a conditional cash transfer program (Programa Jefes de Hoga or PJH), on informal sector employment in Argentina. This program provided 150 pesos to each individual that met eligibility criteria on a monthly basis. It targeted poor families and was implemented to reduce the level of poverty in Argentina. They use a different estimation strategy that exploits observable eligibility criteria. They employ those criteria using matching techniques (propensity score matching) and panel data for their empirical strategy. The authors use a difference in differences estimation technique in investigating whether program recipients are less likely to be employed compared to similar units. They used a dataset consisting of two rotating panels from the national household survey and found some evidence of an increase in informal sector work but the results were not robust to all their specifications.

Dabalen, Kilicb Kalep and Waly (2008) analyze a poverty alleviation program in Albania. Using the 2002 and 2005 waves of a nationally representative survey (Albanian Living Standards Measurement Survey), they observe a negative labor supply response from the program especially among women and urban residents. They uncover that the program decreased the labor supply of urban female workers by, on average, 2.8 hours a week and 2.8 weeks a year. They also find that the probability of labor force participation declines by 5.8 percent for the full sample.

A study by Chou and Staiger (2001) examine the impact of health coverage on the supply of labor of married women in Taiwan, a middle-income country, and find that insurance can be a powerful work incentive. They take advantage of an expansion of health insurance to government employees' spouses. They uncover that the provision of insurance led to decrease in labor force participation of married women with a larger impact in low-income households.

4.2. Social Health Insurance and Labor supply in the US

The effect of Heath insurance on labor supply and mobility is a topic that has been widely studied in the US. Gruber and Madrian (2002) provide an extensive literature survey on the impact of health coverage on job mobility and the supply of labor. The literature focuses on the impact of health insurance on the labor supply of groups that associate a high value to health insurance. These groups comprise older individuals, married women and low-income single mothers. Gruber and Madrian (2002) argue that health benefits are important to explain retirement decisions. Across studies, health insurance increases the likelihood of retirement by 30 to 80%. For married women, they indicate that even though studies are similar in their estimation strategies the literature is consistent in finding an effect of health insurance on the labor supply of married women. They finally point to the fact that studies of Medicaid on labor supply are inconclusive in finding an effect on labor supply of low-income single mothers.

4.3. Other Studies of the Effects of Health Insurance Expansions in Indonesia

Studies that examine the effect of health insurance expansions for the poor in Indonesia are relevant to this dissertation.

A notable study by Johar (2009) analyses the impact of the Health Card program that was introduced in Indonesia in 1994 on the utilization of health care. They exploit the propensity score matching with difference in differences method that has been extensively utilized in the government programs evaluation on labor outcomes (Smith and Todd, 2000; Smith and Todd, 2005; Heckman et al., 1997; Heckman et al., 1998). They find little impact of the Health Card program on the utilization of health care. I employ a similar method in this study since it has proven appropriate in previous research on labor outcomes.

Aji et al. (2013) explore the impact of all the prominent health insurance programs in Indonesia on out-of-pocket spending. They find that the introduction of Askeskin led to a decrease in out-of-pocket expenditures of 34%. They estimate that Askes decreased out-of-pocket expenditures by 54%. However, they did not find any evidence of an effect of Jamsostek. This is evidence of the effectiveness of Askeskin in protecting its population from health risk.

Sparrow et al. (2013) use two waves of a national socioeconomic survey (Susenas) and utilize a propensity score matching method with difference in differences to study the targeting efficiency of the Askeskin program. They also investigate out-of–pocket expenditures and access to health care. They show that Askeskin improves access to health care and utilization of outpatient care among the poor. The results also prove that the targeting was pro-poor.

4.3. Contribution

Evidence of an impact of social insurance or other poverty alleviation programs on labor markets in developing countries is scarce. Only recently have governments of low and middle-income countries started showing interest in large-scale health insurance expansions. Moreover, due to large differences across health care systems, design of health reforms and overall institutional environments, impacts vary from one country to another. In fact, the literature focuses on a handful of countries (mainly Mexico) and, as a result, studies from other developing nations would contribute significantly to the knowledge base.

This dissertation contributes to the literature in at least three ways. First, it provides new estimates from a different institutional setting. Indonesia's expansion of health insurance to the poor is, in size, one of the largest provisions of health insurance in the developing world. The scale of the program and its generosity offers a unique opportunity to provide new robust estimates and contribute to the existing knowledge.

Second, this study departs from the literature by exploiting a propensity score matching with difference in differences methodology. This method has been used in the study of health insurance for the poor in Indonesia (Johar, 2009) and proven efficient to study labor outcomes (Smith and Todd, 2000; Smith and Todd, 2005; Heckman et al., 1997; Heckman et al., 1998). The propensity score matching with difference in differences estimation method used in this study provides several advantages over previously used methods. This method tackles the selection issue by only drawing inference on comparable units. Off-support (non-comparable) units are excluded from the analysis. Regression-based approaches may suffer from bias because units that may have never benefited from the program even if they intended to participate are included. In addition, the differencing using individual level data renders this estimation method robust to individual time invariant unobservable factors that may be correlated with the decision of partaking in the program. Unobservable individual heterogeneity is a common source of bias in such studies due to the known endogenous nature of health insurance. Due to the imperfect targeting of Askeskin, several unobservable factors, such as taste for medical care or risk aversion, could lead to selection into the program. Unobservable factors cannot be accounted for and may have an important impact on the decision to select the program. The propensity score matching with difference in differences method is robust to such factors as long as they do not vary with time. Lastly, due to its non-parametric nature, propensity score matching with difference in differences provides more flexible estimates than regression-based approaches as it does not impose a functional form on the relationship in question.

Third, the dataset used in this study is very rich. It allows us to reproduce and control for the attributes used in the eligibility process by the central and district governments. The Central Bureau of Statistics' proxy means tested score and other criteria used in the eligibility process are used as explanatory variables in the computation of propensity score. Since the allocation of the program was based on the governments' discretion and no formal application process was in place, the selection on observables assumption is reasonable in this study. Moreover, households were contacted by the districts independently and based on the lists provided by local governments. Households had little power to influence the allocation of the benefit and could hardly self-select into the program. Therefore, controlling for all the criteria used in this allocation may reduce bias greatly. In addition, the richness of the data allows me to control for an extensive set of controls at the individual, household and community level that have the potential to affect both the participation in the program as well as labor market outcomes.

Chapter Five: Empirical Strategy and Methodology

5.1. Selection Bias

5.1.1. Average Treatment Effect on the Treated and Selection Bias

The aim of this dissertation is to estimate the impact of health insurance for the poor on labor market outcomes with negligible bias. Ideally, the most effective way to reach this goal is to simultaneously compare the outcome of the enrollee to what would be observed if they were not treated. However, this theoretical outcome cannot be observed. In order to circumvent this issue, empirical economists have compared the outcomes of recipients and non-recipients in order to estimate the impact of the program. If the benefit is distributed purely randomly, then an unbiased estimate of the program impact can be obtained by comparing these two groups. However, if the benefit allocation fails the randomness condition, the estimate would suffer from selection bias and the "true" estimate would be difficult to discern from the effects of selection.

To illustrate this, following Angrist and Pischke (2009), for each individual i, let the treatment variable be represented by the binary variable T_i= {0, 1}. Let Y_i represent the labor market outcome of interest. For any individual i there are two potential outcomes possible: The outcome that would be observed if the individual participated in the program and the outcome in the counterfactual case. As shown below:

$$\begin{cases} Y_{1i} \text{ if } T_i = 1 \\ Y_{0i} \text{ if } T_i = 0 \end{cases}$$

The issue is that, for any given individual, it is not possible to observe both outcomes at the same time because in a state of the world only one of the two can occur. Therefore, we will only be able to observe the outcome if the person was treated or not treated.

We could write the observed outcomes in terms of potential outcomes with the below expression

$$\mathrm{Y_i} = \begin{cases} \mathrm{Y_{1i}} \text{ if } \mathrm{T_i} = 1 \\ \mathrm{Y_{0i}} \text{ if } \mathrm{T_i} = 0 \end{cases}$$

$$= \mathrm{Y_{0i}} + (\mathrm{Y_{1i}} - \mathrm{Y_{0i}})\mathrm{T_i}$$

The difference between the two outcomes above $\mathrm{Y_{1i}} - \mathrm{Y_{0i}}$ would be the causal effect on labor outcomes of the program. In practice, a portion of the population will be treated and the remaining will be untreated. Since the counterfactual is never observed for any given individual, we must compare the averages between treated and untreated individuals. The average treatment effect between treated and untreated can be illustrated by the below relation:

$$\mathrm{E}\,[\,\mathrm{Y_i}|\mathrm{T_i} = 1] - \mathrm{E}[\mathrm{Y_i}|\mathrm{T_i} = 0] = \mathrm{E}\,[\,\mathrm{Y_{1i}}|\mathrm{T_i} = 1] - \mathrm{E}[\mathrm{Y_{0i}}|\mathrm{T_i} = 1] + \mathrm{E}[\mathrm{Y_{0i}}|\mathrm{T_i} = 1] - \mathrm{E}[\mathrm{Y_{0i}}|\mathrm{T_i} = 0]$$

This becomes,

$$\mathrm{E}[\mathrm{Y_{1i}} - \mathrm{Y_{0i}}|\mathrm{T_i} = 1] + \mathrm{E}[\mathrm{Y_{0i}}|\mathrm{T_i} = 1] - \mathrm{E}[\mathrm{Y_{0i}}|\mathrm{T_i} = 0]$$

The first term $\mathrm{E}[\mathrm{Y_{1i}} - \mathrm{Y_{0i}}|\mathrm{T_i} = 1]$ represents the average treatment effect on the treated, namely, the average causal effect on the individuals that were treated. This would be what we would observe if we could simultaneously observe the outcome with treatment and without treatment for the same individual. However, by comparing averages between two distinct individuals (one that benefited from the program and the other excluded from it), it will add a bias term if the assignment is not random. This selection bias is given $\mathrm{E}[\mathrm{Y}_{\mathrm{Oi}}|\mathrm{T}_{\mathrm{i}} = 1] - \mathrm{E}[\mathrm{Y}_{\mathrm{Oi}}|\mathrm{T}_{\mathrm{i}} = 0]$. It represents the inherent differences between treatment and control individual that cause them to select into the program. This term can be so large that it could conceal the true treatment effect. Empirical economists' goal is to find ways to overcome selection bias and estimate the average treated effect on the treated

given above by $\mathrm{E}[\mathrm{Y}_{1\mathrm{i}} - \mathrm{Y}_{\mathrm{Oi}}|\mathrm{T}_{\mathrm{i}} = 1]$.

Random assignment of the benefit (represented by T_{i}) would fix this issue. In the benefit was randomly distributed, then the treatment variable would be exogenous:

$$\mathrm{Y}_{\mathrm{i}} \perp\!\!\!\perp \mathrm{T}_{\mathrm{i}}$$

In fact, if T_{i} is randomly assigned, it would be independent of the potential outcomes. Using our previous expression of the average treatment effect on the treated and the selection bias, Independence allows us to swap $\mathrm{E}[\mathrm{Y}_{\mathrm{Oi}}|\mathrm{T}_{\mathrm{i}} = 0]$ for $\mathrm{E}[\mathrm{Y}_{\mathrm{Oi}}|\mathrm{T}_{\mathrm{i}} = 1]$

$$\mathrm{E}[\mathrm{Y}_{1\mathrm{i}} - \mathrm{Y}_{\mathrm{Oi}}|\mathrm{T}_{\mathrm{i}} = 1] + \mathrm{E}[\mathrm{Y}_{\mathrm{Oi}}|\mathrm{T}_{\mathrm{i}} = 1] - \mathrm{E}[\mathrm{Y}_{\mathrm{Oi}}|\mathrm{T}_{\mathrm{i}} = 0]$$

$$= \mathrm{E}[\mathrm{Y}_{1\mathrm{i}} - \mathrm{Y}_{\mathrm{Oi}}|\mathrm{T}_{\mathrm{i}} = 1] + \mathrm{E}[\mathrm{Y}_{\mathrm{Oi}}|\mathrm{T}_{\mathrm{i}} = 1] - \mathrm{E}[\mathrm{Y}_{\mathrm{Oi}}|\mathrm{T}_{\mathrm{i}} = 1]$$

$$= \mathrm{E}[\mathrm{Y}_{1\mathrm{i}} - \mathrm{Y}_{\mathrm{Oi}}|\mathrm{T}_{\mathrm{i}} = 1]$$

$$= \mathrm{E}[\mathrm{Y}_{1\mathrm{i}} - \mathrm{Y}_{\mathrm{Oi}}]$$

This last expression provides the average treatment on the treated and is an unbiased estimate of the impact of the program.

5.1.2. Selection Bias in Askeskin

The allocation process of Askeskin may not be random and could suffer from selection bias for several potential reasons:

5.1.2.1. Heterogeneity in the Targeting Criteria Used

Though the many district governments used the proxy means tested score or a related set of variables, there were accounts of districts using other criteria. The eligibility process was decentralized and varied across communities. The benefit started to be provided several months before the proxy means tested score was effectively in place. As a result, district officials provided the benefit to recipients using a subset of the proxy-means tested score variables present, criteria used for targeting of previous programs (such as the Health Card program[25]), or criteria at the discretion of the local governments.

5.1.2.2. Lack of Compliance with Eligibility Rules in Certain Districts

In fewer cases, certain districts and communities combined the official eligibility criteria and guidelines with informal targeting methods due to budget constraints. The rationale behind such methodology was to provide the benefit based on need since official criteria might be imprecise in perfectly identifying indigent and unhealthy individuals. Anecdotal evidence from Bachtiar et al. (2006) point out to occurrences in certain districts where local health centers and medical staff provided the benefit to the uncovered based on observable health status.

[25] See footnote 18.

5.1.2.3. Organizational and Structural Issues

Arifianto, Budiyati, Marianti, and Tan (20105) point to deficiencies in the implementation of Askeskin in its initial year. Targeting was implemented at the household level, and in eligible households, each member was eligible to be enrolled in the program. Nevertheless, due to costs associated with the obtaining the cards, poorer households may have decided to enroll only certain members. Moreover, even though health services were completely free under Askeskin, transportation costs to points of health service, rendered health services costly for some households (Bachtiar et al., 2006). As a result, some indigent households only opted for partial coverage.

Another reason for the potential presence of selection bias is related to the structure of the health care market in Indonesia. The quality of care in general differs between public and private providers but also between subsidized and out of pocket care. Subsidized care is generally viewed as lower quality as compared to the care received by patients paying out of pocket. In some poor areas, there were accounts of certain services in the Askeskin benefit package that failed to be delivered (Arifianto et al., 2005). This could have affected the decision to accept the benefit.

Another organizational factor that could have influenced the indigent's decision to partake in Askeskin is related to the awareness and information on the program. There were important deficiencies in the overall information campaign for Askeskin that led some beneficiaries and health care providers to be uninformed about the procedures and responsabilities related to the program (Bachtiar et al., 2006).

5.1.2.3. Other Reasons

As is usually the case in developing countries, it would not be far-fetched to believe that some village leaders provided the benefit to their relatives and extended social network.

All these factors would lead to selection bias and would require an estimation method that is robust to self-selection and potential confounding factors. The method used in this dissertation, propensity score matching with difference in differences is robust to time-invariant unobservable variables and provides estimates of the average treatment effect on the treated accurately if certain conditions are met. In the next section, I will illustrate the identification strategy.

5.2. Propensity Score Matching with Difference in Differences

As mentioned above, I follow an empirical strategy similar to that used by Johar (2009) in studying the effectiveness of the Health Card program on the utilization of health care services. It is based on comparing the change in outcomes for individuals that received the program to the change in outcomes for members of a matched control group. This method was widely used in the impact evaluation of government programs on labor outcomes (Smith and Todd, 2000; Smith and Todd, 2005; Heckman et al., 1997; Heckman et al., 1998). It is a difference in differences estimator of the average treatment effect on the treated conditional on observable characteristics. They introduced an estimator that matches the changes in outcomes before and after treatment for recipients to the average of weighted changes of outcomes for controls units while conditioning on a set of characteristics that determine treatment (for instance, the proxy-means tested score[26]). The differences are matched on the probability of treatment conditional on the propensity score and non-parametric weights are applied. The longitudinal nature of the IFLS survey allows the use of this method as it requires a pre-program period and post-program period.

Let $t = 0$ represent the pre-treatment period and $t = 1$ represent the post-treatment period. Let $\mathrm{Y}_{\mathrm{it}}^{\mathrm{R}}$ denote the outcome for recipient individual i at time t and $\mathrm{Y}_{\mathrm{it}}^{\mathrm{C}}$ represent the outcome without treatment for individual i at time t. Let R be an indicator variable for receiving treatment ($\mathrm{R} = 1$ for treatment and $\mathrm{R} = 0$ for lack thereof). Let X denote a set covariates used as conditioning variables. Finally, let $\mathrm{P}(\mathrm{X}) = \mathrm{Pr}(\mathrm{R} = 1|\mathrm{X})$ be the propensity score of receiving treatment conditional on the set of characteristics of interest X. The key parameter of interest is the mean treatment effect on the treated conditional on a set of characteristics.

$$\theta_{\mathrm{R}=1}(\mathrm{X}) = \mathrm{E}[\mathrm{Y}_1 - \mathrm{Y}_0|\mathrm{X}, \mathrm{R} = 1]$$

Certain assumptions are required for the method to return unbiased estimates. The next sections present the underlying assumptions.

[26] If the proxy-means tested score were purely exogenous, an adequate estimation strategy would be to use the original scores computed by the BPS and search for a discontinuity at the threshold between the near poor and non-poor. Then, use a fuzzy regression discontinuity design to estimate the impact on Askeskin near the threshold. This estimation method is not feasible in my case. Researchers do not have access to the original scores computed by the BPS used to assign the benefits to the poor. Since I am using a household survey to simulate the assignment variable, there might be some measurement error incorporated in it. I searched for a potential threshold in the simulated proxy means tested score from the IFLS survey at both the national level and each of the 225 districts included in the survey. I do not find a clear threshold neither at the national level nor at the district level.

5.2.1. Assumptions

5.2.1.1. Assumption 1: Ignorability

$$\mathrm{Y_{i1}} - \mathrm{Y_{i0}} \perp\!\!\!\perp \mathrm{R}_i|\mathrm{X}$$

Or the mean version,

$$\mathrm{E}\left[\mathrm{Y_{i1}^C} - \mathrm{Y_{i0}^C} \; \middle| \mathrm{X}, \mathrm{R} = 1\right] = \mathrm{E}[\mathrm{Y_{i1}^C} - \mathrm{Y_{i0}^C} \mid \mathrm{X}, \mathrm{R} = 0]$$

This assumption is the usual 'ignorability' assumption, which states that, conditional on covariates X, the treatment can be considered as random (exogenous). In the case of propensity score matching with difference in differences, this assumption also tells us that the average outcomes for treated and controls would have followed similar paths without treatment. This is a crucial identifying restriction in difference in differences models and also known as "Parallel Paths" assumption. Distribution of Askeskin may not be random; nevertheless, the allocation of the benefit is exogenous if X includes all of the variables that affect the allocation and are correlated to labor outcome. This assumption is reasonable in this case since the allocation of Askeskin was based on several observable variables that are contained in the survey and that are controlled for in this study. Moreover, since the allocation of the benefit was centralized at the national and district government level independently of households' decision to enroll, it is sound to assume that controlling for eligibility characteristics should greatly reduce bias. They could hardly manipulate the assignment of the benefit[27]. Additionally, X must be exogenous to the treatment.

[27] It is appropriate to assume that individuals had little power to manipulate the assignment of the program. Only after being selected, do they have the possibility to refuse the benefit. It is sound to assume that the large majority of individuals that were offered the benefit accepted it.

5.2.1.2. Assumption 2: Common Support

For the ATT to be unbiased, at least fraction of the population must be treated. Moreover, since the identification is based on controlling for the effect of covariates included in X, some proportion of the control units must be untreated for the identification strategy to be valid. This allows us to find the appropriate "counterfactual" for each treated unit. The second assumption is called the "common support" assumption and is given by the below expression:

$$0 < \Pr(R = 1|X) < 1$$

This common support assumption guarantees that the attributes of the treated and untreated units overlap sufficiently for appropriate matches to be found. Matching estimation does not infer relationships based on off support units. This is on amelioration over regression methods. This method avoids making inference from incomparable treated and control units, which would lead to biased average treatment on the treated estimates. Crump, Hotz, Imbens, and Mitnik (2009) claim that propensity score matching estimators should be bound to the regions of "thick" overlap. According to their research, this region is the efficient bound for semi-parametric and parametric estimators. They suggest that the range between 0.1 and 0.9 is optimal for several applications. Black and Smith (2004) examine a more restrictive interval of overlap. Units in the 0 to 0.1 area may be too dissimilar to infer proper relationship. Including them may bias the estimates. Therefore, due to sample restrictions from the Indonesian Family Life Survey, this study will use 0.1 to 0.9 as a common support.

5.2.2. Average Treatment Effect on the Treated

In the case of Askeskin, treatment effects need to be estimated by matching a large number of variables. This can render the estimation difficult. This is called the curse of dimensionality. Rosenbaum and Rubin (1983) developed a way of overcoming the curse of dimensionality by estimating a conditional probability of participating in the program, called the propensity score $P(X)$, conditional on all the covariates X. They suggest selecting a propensity score based on attributes that determine treatment from the sample, then matching two individuals with this propensity score, one treated and one untreated. This is a way of summarizing information given by X. the probability of participating in the program is estimated by a parametric procedure. In this study, the method used to obtain a propensity score is a probit regression. This allows the problem of matching to be reduced to a one-dimensional, nonparametric estimation problem instead of a multi-dimensional estimation problem. As mentioned before, the propensity scores require choosing a set X of that are not influenced by the program. The exogeneity of X is crucial to ensure unbiased estimates of the average treatment effect on the treated. In order to guarantee the exogeneity of X, pre-treatment characteristics should be used as conditioning variables (Johar, 2009; Caliendo and Kopeinig, 2005). This would be both relevant even more relevant in the Askeskin program as it is even more reliant on observable characteristics. One useful result (Rosenbaum and Rubin, 1983) is that the “ignorability” assumption still applies when using propensity scores.

$$Y_{i1} - Y_{i0} \perp\!\!\!\perp R_i | P(X)$$

Alternatively, the mean version:

$$E\left[Y_{i1}^{C} - Y_{i0}^{C} \;\middle|P(X), R = 1\right] = E[Y_{i1}^{C} - Y_{i0}^{C} \,|P(X), R = 0]$$

For regular propensity score matching, the selection on observable or ignorability assumption is not robust to the selection due to time-invariant latent variables. The propensity score matching with difference in differences method allows for selection on time-invariant unobservables and hence is less restrictive than the cross-sectional version of propensity score matching. This is true because differencing nullifies the effect of time-invariant characteristics that are unobservable. To the extent that treatment is determined by time-invariant characteristics, the propensity score matching with difference in differences matching technique will be robust to this type of bias by eliminating it. For instance, decision to seek coverage by Askeskin could be determined by the unobserved "taste" for medical care or risk aversion, which might not change much over time. Much of the appeal of the propensity score matching with difference in differences estimator lies in being able to cancel out those time-invariant characteristics correlated to the treatment and estimate the effect of the program based on comparable units.

In order to calculate Average Treatment effects on the treated we rely on the mean conditional independence assumption above. The difference in differences version of the Average Treatment effect on the Treated is given by:

$$\mathrm{ATT} = \mathrm{E}\left[\mathrm{Y}_{i1}^{R} - \mathrm{Y}_{i0}^{R} \mid \mathrm{P}(\mathrm{X}_i), \mathrm{R} = 1\right] - \mathrm{E}\left[\mathrm{Y}_{i1}^{C} - \mathrm{Y}_{i0}^{C} \mid \mathrm{P}(\mathrm{X}_i), \mathrm{R} = 0\right]$$

Given that the assumptions are not violated, the equation above provides an unbiased and consistent estimator of the average treatment effect on the treated. The next section describes the matching estimators used in estimating the impact of Askeskin on labor market outcomes.

5.2.3. Matching Estimators

The propensity score algorithm searches for units that are closest in characteristics for matching appropriately. Let i denote a treated individual and R denote the set of treated individuals ($i \in R$). Let j denote a non-treated individual and C denote the set of non-treated individuals ($j \in C$). The sample equivalent for the average treatment effect on the treated can be written as :

$$ATT = \sum_{i \in R} [(Y_{i1}^{R} - Y_{i0}^{R}) - \sum_{j \in c} W_{ij}(Y_{j1}^{C} - Y_{j0}^{C})] \, N_R{}^{-1}$$

The term W_{ij} represents the weight assigned to a control unit j that is associated to a treated unit i. This expression effectively compares change in outcomes from recipient units to the weighted average of changes in outcomes of the control units. The weights reflect the propensity score of the unit in question and are determined by the specific matching method used (Dehejia and Wahba, 2002). If the matching method utilizes only the most comparable unit based on propensity score, the weight will be equal to 1 for the closest unit and 0 for all other units. This matching algorithm is called the nearest neighbor matching method because it only uses the outcome of the most comparable unit j for each treated unit i. However, there exists a more flexible estimator. This estimator is the kernel estimator and it uses the weighted average of the outcomes of all control units j, associating higher weights for control units with closer propensity score (i.e. more comparable units). Some hybrid methods of nearest neighbor matching and kernel matching exist. These utilize a weighted average of the outcomes of control individuals in a certain neighborhood of the propensity score of treated units. These methods are called caliper-matching methods. Kernel matching has the advantage that it minimizes the standard errors and, hence, renders the estimator more efficient. I will only present the kernel matching estimators. Gaussian weights are used.

A closed form of the formula of the standard errors of the average treatment effect on the treatment estimates does not exist for kernel estimators. Therefore, I must rely on calculating the standard errors using the bootstrapping method with 400 repetitions for inference purposes. As mentioned above, only observations that lie in the region of common support from 0.1 to 0.9 are kept in the study.

Chapter Six: Data

6.1. Description of the Indonesia Family Life Survey

The estimation method requires a longitudinal dataset containing a pre-exposure period and a post–exposure period. Since the inauguration of the program occurred in 2005, the ideal survey should provide a wave before and a wave after 2005.

The Indonesia Family Life Survey is an ongoing longitudinal household survey which started in 1993. It collects a very rich set of individual, household and community level socioeconomic and health data. It is a sample representative of 83% of the Indonesian population that is collected in 13 of the 27 provinces of the country (Strauss, Sikoki, Witoelar, and Watie, 2009). The data were gathered by the RAND Corporation in cooperation with American and Indonesian universities[28]. The sampling was stratified at the province level and then randomly sampled within provinces. This sampling was undertaken with the aim to maximize representation of the population and capture the cultural and socioeconomic diversity of the country. The first wave of the IFLS was conducted in 1993 (IFLS1), with a sample of 7224 households and data collected for 22000 individuals in those households. There were three subsequent waves: in 1997 (IFLS2), 2000 (IFLS3), and 2007(IFLS4). Each subsequent wave contains re-contacted IFLS1 households (the original households) and their split-offs (Strauss et al, 2009). A large proportion of the original individuals were re-contacted (around 95%). Due to

[28] http://www.rand.org/labor/FLS/IFLS.html

households splitting off over time, the sample size for the 2000 survey increased to 10400 households and is composed of 39000 individuals. In 2007, 13535 households are present containing 44,103 individuals.

Main fieldwork went on from June through November 2000 for IFLS3 and between November 2007 and April 2008 for IFLS4 (Strauss, 2009), which constitutes a period of about 7 years between the pre-treatment period and post-treatment periods. Restricting to the sample of individuals that were present in the original survey (IFLS1), the sample is made up of 33,902. This is necessary for accurately addressing the issue of attrition. In addition, I restrict the sample to working-age individuals. Since the minimum legal age for labor participation is 15 and the age of retirement is approximately 65, I drop from this study all the individuals that are either younger than 15 years of age or older than 65 years of age in both the pre and post surveys.

Apart from its longitudinal nature, this dataset's main appeal is its richness. It includes a comprehensive set of variables at the individual, household and community level. The ignorability assumption requires that all observable variables that may affect treatment and outcome be included in X for the ATT estimates to be unbiased. As mentioned previously, IFLS includes the majority of the variables officially used in the eligibility of recipients of the program (namely, the proxy means tested score). The rich nature of the data also allows controlling for a comprehensive set of complementary variables that may be correlated with the program allocation and labor market outcomes. The next section provides a description of the variables used in the analysis.

6.2. Dependent Variables

6.2.1. Informal Status Variables

No unanimously accepted characterization or measurement of informal status exists, even in higher income countries. In countries where social security appropriately covers the majority of the formal sector, the notion of informal sector employment is defined by the absence of social security coverage. In other places, informality would be more accurately identified based on the individuals' employment status and occupation. The definition of informality is highly dependent on the country and institutions in place (OECD 2008).

A survey by the Organisation of Economic Cooperation and Development (OECD,2008), argues that the best characterization of informality in Indonesia would be based on employment status and occupation of workers due to the fact that the social security mechanisms are under developed and many key benefits are either missing or limited (such as unemployment insurance and retirement benefits). Moreover, workers seldom enter in official agreements with their employers in Indonesia. Only a negligible fraction of the labor force has a written agreement with their employers. In 2007, 5% of active employers and employees report themselves as having their employment tied by a permanent contract. Whereas, 8% of the active labor force has any type of agreement (Newhouse et al., 2010). As mentioned earlier, enrollment in a formal social security scheme is far from widespread. Social security in the private sector (Jamsostek) is only mandatory in firms with 10 or more employees. In general, social insurance is much more common in larger private enterprises and the public sector and a large share of formal sector personnel is left out without social insurance. Based on the IFLS4, only about 55%

of private employees work in firms with 10 or more workers. Moreover, even in firms, that have 10 workers, the social security is not well enforced. As mentioned in section **2.2.2**, there were serious compliance issues that led to some employers to fail to register certain employees for social security. Based on the IFLS survey only 20% of individuals in the formal sector have Jamsostek. This makes social security coverage or other legal protection such as legal agreements inaccurate measures of formal status.

For the reasons above, I primarily use employment status as a definition of informal status. The Indonesian Central Bureau of Statistics' (BPS) official definition of informal status is based on the primary working status. **Table 4** provides a simple description of how informality is defined officially in Indonesia.

The Central Bureau of Statistics (Winarsih and Lisna, 2015) distinguishes between formal and informal self-employment. Self-employed with permanent workers are deemed to be part of formal sector whereas self-employed with temporary, family or unpaid workers are considered a part of the informal sector. All casual workers in agriculture or non-agriculture are considered informal. Likewise, own account workers and family workers are considered informal. Finally, the Central Bureau of Statistics considers all employees within the salaried sector to be a part of formal employment – including employees of small and medium enterprises, large firms and the entire public sector.

The IFLS provides the employment status of the individual. I define informal status as a dichotomous variable that takes a value of 1 if the individual is self-employed with no employees, self-employed with unpaid family members or temporary members, an unpaid family worker, or a casual worker. The informal status variable takes a value of 0 if the individual is a private worker, a government worker or self-employed with permanent workers.

6.2.2. Labor Supply Variables

I investigate both the intensive and extensive margins of labor supply. To measure the decision to work at the extensive margin, I use two variables. The first one takes a value of 1 if the individual was employed for pay in the week preceding the survey and 0 otherwise. The second variable that takes a value of 1 if the person was employed for pay in the year preceding the survey and 0 otherwise.

To measure the decision to work at the intensive margin, I use the hours worked in the week preceding the survey in the primary job of the respondent. I also include the typical number of weeks worked per year in the primary employment of the respondent. These variables are based on an estimate provided by the respondent.

6.3. Explanatory Variables

The independent variable of interest is a dichotomous variable for whether the household in which the individual lived received the Askeskin program. The variable takes a value of one for Individuals that received Askeskin (treatment group) and zero for non-recipients (control group).

In order to ensure the strict exogeneity of the control variables with respect to treatment, only pre-treatment values are used (i.e. as of 2000) in the estimation of the propensity score. The pre-treatment values are recorded four years before the implementation of the program which renders the exogeneity assumption reasonable, as the lag is long before the implementation of the program. The appropriate variables to include as covariates are all the variables that are correlated with both the dependent variable (labor market outcome) and the treatment variable. In fact, in order to minimize the likelihood of a bias due to omitted variables, all variables that meet this condition need to be accounted for. Socioeconomic status, welfare status, demographic conditions, measures of well-being and infrastructure conditions are the main categories of covariates that are susceptible to determine both eligibility in the program and labor market outcomes.

6.3.1. Individual Level Control Variables

The control variables at the individual level include a dummy for gender, age, a dummy for marital status, four dummy variables for highest education level education completed (elementary education, junior high school, senior high school, and higher education). The inclusion of the personal and socio-economic characteristics is important as they are essential in predicting not only need and propensity to enroll in the program but also the labor status of individuals.

Bachtiar et al. (2011) point to anecdotal evidence that Askeskin has been allocated, in some cases, based on health status. There are reasons to believe that if there was some degree of freedom in determining eligibility at the village level, then health should be considered as it should be an important factor, and thus it needs to be controlled for. Therefore, I added a variable measuring health of the individual in the analysis. It is a measure of self-reported general health status that takes values from 1 to 4 depending on whether the person’s health is very healthy, somewhat healthy, somewhat unhealthy, and unhealthy respectively.

I also control for dichotomous variables signaling the enrollment of the individual to the other main types of health insurance programs: the public sector program (Askes) and the private sector program (Jamsostek). Keeping individuals with other health insurance programs than Askeskin in the sample while controlling for their enrollment in those programs allows to maximize the sample used in the study. Dropping them from the sample would significantly reduce the sample size of the study.

6.3.2. Household Level Control Variables

The household-level variables included in the study are: household size, the age of the head of the household, a dummy variable for whether the head of the household is a female, and the number of children aged less than 12 in the household. Household composition is correlated with the availability of health insurance in households as well as labor market decisions within the household. Individuals that live in larger households and with children may be more prone to self-select into health insurance as they expect larger health care expenses. Individuals with older head of household will also be more likely to seek health insurance coverage as they expect large medical expenditures.

The proxy means tested score was also computed at the household level. To construct the simulated proxy-means tested score, I use the original district – level weights provided by the Central Bureau of Statistics (225 different sets of weights for each districts in my sample). The Indonesian Family Life Survey includes all of the variables[29] that were used in the production of the proxy means tested score. The index values ranges from zero to 100, 0 corresponding to the richest individuals and 100 the poorest

[29] Or closely related proxy variables.

individuals. The simulated proxy-means tested score does not perfectly predict the eligibility for several reasons. First, the eligibility rule was not strictly enforced for reasons discussed in **Section 5.1.2**. Second, proxy means testing can lead to some targeting errors, where some non-poor receive the benefit (inclusion error), and some poor do not (exclusion error). This is because the indicators used in the score might not be measured perfectly and fail to capture all the variation between the poor and the non-poor. In addition, the variables used to construct the proxy means tested score may not predict poverty perfectly. Third, using a household survey with several years of lag might add some measurement error.

In my analysis of the Askeskin program, I control for the enrollment Health Card program that was implemented prior to the institution of Askeskin. This allows disentangling the potential effects of the Health Card program. Thus, I create a dichotomous variable that takes a value of one if the person lived in a household that received the Health Card and zero otherwise. Controlling for it is necessary since Individuals that obtained the Health Card are more likely to be enrolled in Askeskin.

I control for the unconditional cash transfer program (BLT) for which enactment coincided with Askeskin. The unconditional cash transfer was another safety net program designed to reduce poverty and help indigent families financially. As mentioned before, this program started in January 2005, ran for 12 months (from 2005 to 2006) and provided 10$ a month to 19 million poor and near poor households (a total of $120). It has been documented that poverty alleviation programs that provide households with monetary transfers might affect labor market outcomes (Dabalen et al., 2007), therefore controlling for it would account for changes in labor market outcomes related to its implementation.

If this program has a large enough impact on labor supply or informality, omitting to control for this variable may lead to bias as the labor impact observed and associated with this program would coincide with the introduction of Askeskin and may be mistakenly attributed to Askeskin.

6.3.3. Community Level Control Variables

I add several controls to account for community level characteristics. There are large disparities between rural and urban areas in Indonesia. In fact, even within urban areas and rural areas, communities may differ substantially. These characteristics might have an effect on both the likelihood of employment and the propensity of receiving Askeskin. The economy and the labor market are in general more dynamic in urban areas and jobs are easier to find. Moreover, the population in rural areas is likely to be low skilled and less educated. The supply of health services is lower in rural areas as there less health care facilities and professionals and access is more difficult. Infrastructure are more rudimentary in rural areas and the quality of health services lower. These factors would render the enrollment in Askeskin relatively less desirable. As a result, not accounting for this spatial heterogeneity might lead to omitted variable bias and endogeneity of my coefficient of interest. Adding infrastructure and supply of health care variables at the community level as controls should capture most of the community-level differences in propensity to receive targeting as well as differences in labor market decisions.

I include a dichotomous variable indicating whether the individual lives in an urban or rural community, a dichotomous variable for the presence of an asphalt road in the village, the percentage of households that have electricity in the community, the presence of a sewage system in the community and the presence of piped water in the community. These variables should account for the infrastructural differences between communities. Areas with better infrastructure should have better access to health care service due to better transportation routes. This would render the benefit more desirable. Additionally, I include the number of health centers present in the town and an indicator for the presence of a midwife[30]. The larger the quantity of health providers and the better the quality of health care provided, the more valuable the benefit to potential recipients. I also include a variable at the community's subjective wealth. Considering the current conditions of the village population, this variable asks a village official to rank their village on a scale from one to six, one corresponding to the village where the population is poorest, and six representing the village where the population is richest.

Finally, since there is a large spatial disparity in the distribution of the population and important socioeconomic heterogeneity across provinces, I include dummies for all provinces where the survey has taken place.

[30] A midwife is a person that is trained to assist women in childbirth. Due to high maternal and child mortality, maternal care has been a priority for the government for decades. In 1989, the government implemented a program in which midwives were placed in birth facilities in most villages across the country. Due to low access to care in remote areas, the midwife sometimes provides basic health care when a physician is not present.

Chapter Seven: Preliminary Regressions

This section provides preliminary regression results using different methodologies both cross-sectional and longitudinal. The purpose of this exercise is expositional in nature. **Chapter 8** provides the main ("preferred") results of this dissertation based on the propensity score matching with difference in differences discussed in earlier sections. **Tables 5** to **10** present cross sectional regression (with and without controls), fixed effects and propensity score matching estimates. The propensity score estimates only use the 2007 values for the dependent variables and the pre-treatment values for the independent variables. The regressions are estimated using OLS (i.e. linear probability model for binary dependent variables). The propensity score matching estimates use the kernel matching method. The results are presented for the full sample, for subsamples by gender and by residence status (urban vs. rural) in order to investigate potential heterogeneous impacts. In fact, as commonly known, labor markets are seen as heterogeneous and segmented in developing countries since the labor market conditions faced by different groups (men vs. women, urban vs. rural, skilled vs. unskilled) may be different. Fields (2011) argues that the overall labor market in developing countries is a network of interconnected labor market segments that are connected by the potential mobility of firms and workers. The segments that make up the labor market differ from one another by the level of income and benefits and the employment arrangements. Labor mobility between the better segments (usually formal) and the less desirable segments is assumed to be limited (i.e. informal). In these less desirable segments, underemployed is ubiquitous. Certain groups within these segments are less privileged than others. In fact, women are usually disadvantaged in developing countries' labor markets; they usually earn less, work more often in the informal market and are more likely to hold irregular positions. There are also spatial differences in labor markets. The nature of the industries available in the rural areas is more rudimentary. Individuals are more likely to be engaged in agricultural activities. In addition, informality is more prevalent in the rural world. The urban world is characterized by greater wage labor and greater formal employment. Therefore, it is important to investigate the impact on different samples.

7.1. Cross-sectional OLS Estimation

If the treatment were purely random, a simple cross sectional regression framework would return unbiased results. As mentioned above, random assignment would only necessitate comparing the means between treated individuals and untreated individuals. I estimate an OLS specification without controls first and then using a full set of controls[31]. I use the 2007 values of the variables controlled for and use robust standard errors for estimation. The model estimated is the below:

$$Y_i = \alpha + Askeskin_i \delta + X_i \beta + \varepsilon_i$$

Where δ is the coefficient of interest, X_i is a set of explanatory covariates and Y_i the labor market variable of interest.

[31] Linear probability model in the case of binary dependent variables.

7.1.1. Full Sample

As shown in **table 5**, the relationship between receipt of Askeskin and informality when no controls are added appears to be strong and positive; however, it changes signs and is still significant at the 10% level when a full set of control variables is added to the model. The counter-intuitive negative sign in the regression equation for informality can be explained by the fact that OLS can only provide a partial picture since it cannot account for bias caused by unobservable variables. Therefore, a method that is robust to unobservable confounders would provide better indication on the true nature of this relationship. Later, when applying the more robust propensity score matching methods, this significant negative impact disappears as expected. Concerning labor supply at the extensive margin, there appears to be a marginally significant negative relationship for the probability of working in the previous week when no controls are added, but no significant effect with a full set of controls. For the number of hours worked per week, we observe a strong negative relationship without controls. The relationship stays significant at the 10% level when a full set of controls is added to the equation. It appears that receiving Askeskin led to a decrease of 1.1 hours worked per week. There is a strong relationship (1% level) for the weeks worked per year without controls. When controls are added, it stays significant at the 5% level. Individuals appear to be working 0.78 weeks less per year due to Askeskin.

7.1.2. By Gender

As shown in **Table 7**, there is a strong positive relationship (significant at the 1% level) on informality for women when no controls are added, however, there is no significant relationship apparent when a full set of controls are added. Concerning labor supply at the extensive margin, the relationship is not significant for any of the specifications whether with controls or not. However, the signs are negative as expected. The regression equation on the intensive margin of labor supply provide significant and negative (1% level) estimates when no controls are included and no significance when controls are added. However, they exhibit the expected sign (i.e. negative).

As shown on **Table 6**, the specification with informal status as a dependent variable exhibits a strong positive relationship for the sample of men when no controls are used. Yet, the relationship becomes negative but still significant (5% level) when a full set of controls is added. Similarly, to the effect observed for the full sample, this change in sign could be caused by confounding factors. The estimates on labor supply at the extensive margin are insignificant with and without controls and the magnitude is very close to zero. This is may be an indication that men do not change their labor force participation because of their enrollment in Askeskin. Regarding labor supply at the intensive margin, only the specifications without controls return very significant (1% level) and negative estimates. However, the signs for the estimates for both the regression equations on hours worked per week and weeks worked per year have negatives sign as expected.

7.1.3. By Residence Status

Using the sample of individuals living in urban areas, **Table 8** shows that the specification with informality as a dependent variable and without controls returns very significant positive estimates. They are large in magnitude (9% increase), which indicates that the effects associated to other variables are entangled in this estimate. However, when control variables are added, the estimates become insignificant and change signs.

The specification with labor supply at the extensive margin as a dependent variable returns insignificant results with and without controls for the probability of working in the preceding year, whereas only the specification without controls appear to be marginally significant for the probability of working in the preceding week. The signs are negative in all cases. Regarding labor supply at the intensive margin, individuals living in urban areas appear to work 4.51 hours less when no controls are used (very significant at the 1% level) and the estimates decrease to a negative 1.66 hours per week when we use a full set of controls (marginally significant at the 10% level). For weeks worked per year only the estimate without control is significant (at the 1% level), the estimates become smaller and non-significant when a full set of controls are added. For labor supply at the intensive margin, the estimates are also all in the expected direction.

For rural areas, the coefficient on informality is large, positive, and significant without controls and becomes smaller, negative and insignificant when we account for all control variables. Concerning labor supply at the extensive margin, the coefficient on the probability of working in the week preceding the survey is marginally significant without controls but becomes insignificant when controls are added. The specification with working in the preceding year as a dependent variable is insignificant in both cases. Concerning labor supply at the intensive margin, OLS estimates without controls are very significant for both hours per week and weeks per year and only stay significant with controls for weeks per year. It appears that individuals in rural areas decrease their labor supply by 1.27 weeks per year due to the availability of Askeskin.

Overall, the signs of the variables are as expected for the labor supply (negative). In a few cases, the informal status specifications return significant and counterintuitive signs (full sample and sample of men). A robust estimation technique is necessary in order to uncover the true relationship and eliminate different sources of bias. Overall, when controlling for a full set of controls most specifications return insignificant estimates. This is probably due to omitted variable bias due to unobservables, which may bias the standard errors upwards and consequently bias the t-ratios downwards. Consequently, it is less likely to reject the null hypothesis when it is false. Moreover, the OLS method is based on strong assumptions. It is inappropriate because it infers relationships from incomparable units. It includes a large number of control units that would not have never received the program even if they intended to and individuals that received the program that should not have received it since they do not have the appropriate profile. In the next section, Propensity score matching estimates are presented. This method provides an enhancement over regular OLS, as it compares observations that are similar.

7.2. Propensity Score Matching

Table 10 presents the results for the propensity score-matching model. The Propensity score matching estimates provide two enhancements over the regular OLS regression estimation. As mentioned above, it does not compare individuals that are not comparable (outside of the common support) and it uses non-parametric estimation that does not assume any functional relationship between dependent and independent variables. Misspecified functional form can lead to bias and would invalidate the regression estimates. Hence, using a method that does not impose functional restrictions on the regression model can enhance the estimates. In order to estimate the cross-sectional PSM model, I use as controls the baseline dataset variables (2000 values) in order to ensure the exogeneity of the treatment variable with respect to the explanatory variables.

7.2.1. Full Sample

For the full sample, the specifications of the informality and labor supply at the extensive margin return insignificant estimates. The estimates for labor supply at the extensive margin are both in the correct direction but insignificant (though the probability of working in the preceding week is only marginally insignificant). Concerning labor supply at the intensive margin, both the hours worked per week and the weeks worked per year provide estimates in the expected direction (negative). They are significant at the 1% level and 5% level respectively.

7.2.2. By Gender

Looking at the sample by gender, the specification with informality as a dependent variable returns insignificant negative estimates for both men and women. Regarding labor supply at extensive margin, only the probability of working in the previous week provides significant estimates for both men and women. These estimates are marginally significant (at the 10% level). Being enrolled in Askeskin leads to a decrease in the probability of working in the previous week of 2.7 percentage points for both men and women. Being enrolled in Askeskin also appears to lead to a decrease in the numbers of hours worked and weeks worked for women of 1.82 hours a week and 1.25 weeks per year. These estimates are significant at the 1% and 5% level respectively. For men, only the specification with hours worked per week as a dependent variable returns significant results (at 5% level). Being enrolled in Askeskin appears to lead to a decrease in the numbers of hours worked for men of 1.246 hours per week.

7.2.3. By Residence Status

There appears to be no evidence of an effect of Askeskin on either informality or labor supply at extensive margin for neither urban nor rural areas as the coefficient are non-significant. Concerning the labor supply at the intensive margin, only the hours worked show a significant effect (at the 1% level) for urban areas. It appears that receiving Askeskin leads to a decrease of 2.691 hours per week. For individuals living in rural areas, only the specification with weeks worked as a dependent variable shows a significant effect (at the 1% level). Receiving Askeskin leads to a decrease in the weeks worked per year of 1.353 based on this model.

The propensity score matching estimates are suggestive that an effect exists on labor force participation and quantity of work. This impact seems to be present for both men and women. If unobservable variables are correlated to both the treatment variable and dependent variables, the results returned by OLS regressions and PSM in a cross-sectional setting could be unreliable due endogeneity, mainly due to the selection bias arising from unobservable variables (omitted variable bias). Therefore, one must be careful in interpreting these estimates as causal estimates. For cross-sectional data, the usual solution for this problem is the use of an instrument as a remedy to the selection issue (or a regression discontinuity framework). However, in the case of Indonesia, the lack of strong instruments and the presence of longitudinal data renders panel data models preferable. Methods using panel data are robust to time invariant unobservable and are more effective in correcting the main cause of selection bias. The next section presents the fixed effects estimates using a baseline survey before the introduction of the program. This method is more compelling as it accounts for time invariant unobserved heterogeneity.

7.3. Fixed Effects Estimation

Given the longitudinal nature of the data, I can implement a fixed-effect estimation strategy. The main advantage of this strategy is that it is robust to the time invariant unobserved heterogeneity across individuals. The fixed effect equation is as follows:

$$Y_{it} = \alpha + Askeskin_{it}\delta + X_{it}\beta + u_i + v_t + \varepsilon_{it}$$

For individual i and period t=1,2. Y_{it} is our dependent variables of interest (labor market outcomes in our case). X_{it} is a set of individual, household and community level controls. u_i represents the group fixed-effects which controls for unobservable differences between groups. v_t are time fixed effects which provides the impacts common to all groups but is variable across time. Finally, ε_{it} embodies the idiosyncratic error. The panel is strongly balanced.

7.3.1. Full Sample

Table 5 provides the fixed effects estimates for the full sample. For the specification with informality as a dependent variable, the fixed effects estimates are close to marginally significant (but not significant) and exhibit the expected (positive) sign, this result (compared to OLS estimates) can be explained by the reduction of bias due to unobservables. For instance, if risk aversion to health events is the confounding factor, and more risk averse individuals are more likely to work in the formal sector and more prone to self-select into Askeskin then not accounting for this unobservable factor (risk aversion) could attenuate the coefficient. If the effect is large enough it could lead to a negative effect on informality. This was observed in the cross-sectional estimation case. As a result, cancelling out the effect of the confounders by differencing using fixed effects estimation leads to positive coefficients.

Regarding labor supply at both the extensive margin and intensive margins, the coefficients are all insignificant but in the correct direction (negative).

7.3.2. By Gender and by Residence Status

By segmenting the data by gender and region, it appears that there is not any significant effect for any of the dependent variables.

Similarly to cross sectional OLS, the issue with fixed-effects estimation is that it includes a large number of observations with low propensity score ($p<0.1$). These off-common support observations can bias the estimates. In fact, several individuals with low propensity score that did not receive the program cannot be used as control units as they would not have received the program even if they intended to. These individuals are too far off the eligibility threshold and should not be used as controls. Similarly, low propensity score treated units should not have received the benefit in the first place and are not valid observations for inference purposes. Fixed effects estimation imposes a linear relationship between the dependent and independent variables. If the true relationship is not linear, fixed effects regression would lead to biased estimates. The standard errors would also be larger, rendering the t-ratios too small to be significant.

An estimation method that can correct these issues is the propensity score matching with difference in differences as it allows to only compare individuals that are comparable (by restricting the common support), does not impose a functional form and is robust to time invariant unobservable variables. This method provides an enhancement over the regular cross-sectional propensity score matching as its difference in differences nature renders it robust to unobserved heterogeneity. It also improves the fixed-effects estimates by only drawing inference from comparable units. The next section presents the results from the propensity score matching with difference in differences estimation method.

Chapter Eight: Propensity Score Matching with Difference in Differences

8.1. Testing Assumptions

8.1.1. Ignorability of Treatment Assumption

A crucial identifying restriction in the propensity score matching with difference in differences model states that, conditional on explanatory variables, the assignment of treatment becomes random or that the average outcomes for treated and controls would have followed similar paths in absence of the treatment. This is an important assumption since it helps disentangle the effect of the program from trends that would lead the outcome variable to follow a certain path regardless of the effect of the treatment variable. The IFLS provides an opportunity to test for this assumption as it contains a wave right before the baseline survey. The strategy used to test for the validity of the ignorability assumption is to examine whether an effect is present using the survey prior to the baseline survey (1997) and the baseline survey (2000). If the effects observed in our main analysis are only due to trends in the outcome variables, I should observe such effect between 1997 and 2000. Using the sample of individuals that were treated in 2005 and a similar control group, I test whether there is an effect using the same estimation strategy (propensity score matching with difference in differences). **Table 11** shows that none of the coefficients are significant for any of the subsamples and dependent variables. This provides a reasonable indication that the ignorability assumption is valid. Therefore, it is safe to claim that the effects observed in the subsequent sections are not due to a trend in the dependent variables that are not related to the enrollment in Askeskin.

8.1.2. Common Support Assumption

The treatment and control groups need to overlap and thus, share a region of common support on the propensity score. However, for my estimates to be accurate only individuals that have a high enough probability of receiving the treatment need to be included as comparisons. In fact, observations with low levels of P(x) could bias the estimates. Estimation of average treatment effects is often undermined by lack of overlapping of the covariate distributions. In propensity score matching, the choice of the control units is the key determinant in obtaining sound estimates. Using control observations that have low propensity score can render the estimates invalid since they are not comparable and would most likely not be able to enroll in the program even if they desired to. Recently, Crump et al. (2009) proposed a systematic ad hoc approach that addresses the problem of lack of overlap. They showed that for a wide range of distributions, an appropriate ad hoc method is to remove all units with estimated propensity scores that are not in the interval [0.1, 0.9]. This provides a good approximation to the optimal rule. This is also referred as “trimming” the common support. Therefore, in this study, the sample is limited to observations propensity score equal to at least 0.1 but no more than 0.9. This ensures that every treated observation have at least one comparable untreated observation in the sample.

Figures 2 and **3** present the kernel density graph of the estimated propensity score of both the treated and non-treated groups. Before applying the ad-hoc rule restricting the common support from 0.1 to 0.9, a large number of observations are located in the 0 to

0.1 area. Those are mainly control observations; however, some treated observations are also contained in that area. These observations can cause biased estimates and should be excluded from the analysis. After restricting the sample to the [0.1, 0.9] area, 3380 observations are excluded, 3154 untreated and 226 treated. **Figure 3** shows that there is significant overlap between treated and untreated observations. The common support area for the propensity scores is [0.1, 0.785], hence, the upper bound does not require to restrict the sample as there are no observations with P(x)>0.785.

8.1.3. Balancing Property

One important condition for the propensity score matching method to return unbiased results is that, given an equal propensity score, observations must have the same distribution of both observable and unobservable characteristics independently of treatment status. This is called the balancing condition. However, it is impossible to test for the balancing of unobservable characteristics. Therefore, I test the balancing condition of observable characteristics based on a method presented by Becker and Ichino (2002). This is based on the *pscore* command in Stata. The test consists in splitting the sample into several intervals called “blocks” for which the average propensity score is equal. Then the program statistically tests (using a two-sample t-test) that the mean value between treated and control units do not differ for each covariate in each block. The balancing property is not rejected only in the case that it holds for all the covariates. In this study, the balancing property is satisfied[32].

[32] Due to the large number of t-tests related to this method, the results are not presented.

8.1.4. Testing for Attrition

Panel surveys have traditionally suffered from sample attrition. If attrition is non-random and severe enough, it can render the sample non-representative and could invalidate the estimates. The reason for this is that respondents that drop out of the longitudinal survey may differ systematically from individuals that are re-interviewed. Therefore, results of studies that only incorporate continuing panel respondents may suffer from severe attrition bias. The problem of attrition is especially widespread in household surveys conducted in developing countries due to communication means being underdeveloped. It is not easy to track individuals that have moved from one survey to another. Tracking movers can implicate substantial investment in terms of time and money.

Using the first two waves of the Indonesia Family Life Survey, Thomas, Frankenberg and Smith (2001) showed that panels in developing countries are not all necessarily contaminated by high rates of attrition. Statistics from the IFLS show an optimistic picture. 94% of the households interviewed in 1993 were re-interviewed in 1997. This rate of re-contact tops even the best surveys in the United States.

Since the study uses the last two waves of the IFLS, I test for the presence attrition between the third wave (baseline) and fourth wave (post-treatment) of IFLS. I first examine descriptive statistics by comparing the group of attritors and non-attritors across multiple variables measured at the baseline. Then, I estimate a binary dependent variable model of attrition as a function of variables measured at the baseline in order to examine whether differences between attritors and non-attritors hold after controlling for a comprehensive set of socio-demographic characteristics. The model used includes demographic characteristics, health characteristics, labor force variables, household composition and resource variables, as well as spatial and community characteristics in 2000.

Based on the IFLS, 93.46% of individuals in the 2000 data are successfully re-interviewed in the 2007 wave. Therefore, the attrition rate between these two surveys is 6.54%, which is similar to the 94% that Thomas et al. (2001) estimates using the first and second wave. **Table 12** presents the comparison of descriptive statistics and t-test between the group of attritors and group of non-attritors. It appears that for most variables the group of attritors are significantly different. The individual demographic characteristics point out to the fact that older married individuals with higher education level are more likely to be attritors. There is no clear difference in health status. The labor force participation variables indicate that attritors are more likely to work and work more hours and less likely to work in the informal sector. Household composition and resource variables appear to indicate that attritors are wealthier (very significant difference in proxy means tested score), live in smaller households, and have less children. Community characteristics appear to show that individuals that are attritors live in areas that are wealthier and have better overall infrastructure. Attritors are also more likely to live in urban areas. However, these results are only indicative at best, as they only provide a comparison of means individually. It is important to control for all other variables that may cause attrition.

Table 13 provides estimates of a binary dependent variable model of attrition as a function of variables measured at the baseline wave. After estimating a full model, it appears that some differences remain between the group of attritors and non-attritors.

Age, proxy-means tested score, hours worked per week and number of health centers show significant differences that are small in magnitude. Attritors are more likely to be married, more likely to have higher education, are less likely to have children, are less likely to work in the informal sector, are more likely to have piped water and are more likely to live in an urban area. However, the magnitude of these variables is small.

The presence of attrition (even though small) can bias our estimates. Thus, it is necessary to apply a method to correct the sample from attrition bias. IFLS provides sampling weights that allow to account for attrition in the survey. Strauss, Sikoki, Witoelar, and Watie (2009) describe the procedure in order to correct the attrition bias specific to the IFLS Survey. They compute weights specific to each survey to be used by researchers in order to obtain a representative sample. The inverse probability weights provided in the Indonesia Family Life Survey dataset correct for both sampling bias as well as attrition bias. The methodology to compute the longitudinal analysis individual weights is the following: In order to correct for in between-survey attrition, they first estimated a logistic model of the probability that an individual found in a baseline wave of IFLS was found in a subsequent wave, conditional on basic individual and household characteristics at the baseline. They then calculated the predicted probability that the individual was found. From that predicted probability, they computed the inverse-probability-of-attrition weights for each individual. I use the weights provided in the IFLS in order adjust for attrition. Conditional on these weights, attrition can be considered as ignorable and random.

Finally, because all respondents who were interviewed in the later waves but were not in the original household roster (IFLS1) are not assigned longitudinal weights, it is necessary to restrict the analysis to only the individuals that were present in the original survey (1993).

8.2. Descriptive Statistics

8.2.1. Explanatory Variables

For comparison purposes, **Table 14** presents descriptive statistics for the full sample and by treatment status without restricting the area of common support (trimming) as described in section 8.1.2. **Table 15** presents the descriptive statistics for the full sample and by treatment status after restricting the area of common support. As mentioned previously, the sample is restricted to the thick overlap area of [0.1, 0.785]. Restricting the sample has reduced the differences in characteristics between the treated and untreated. However, there are still significant differences in the distribution of characteristics between the treated and control groups. This is consistent with the fact that the program targets the poor and near-poor population. The treated individuals are materially poorer than the non-treated as indicated by their higher proxy-means tested score. Other socio-economic characteristics such as education show significant differences as well. The difference in socio-economic status is also true at the community level. Differences in statistical significance of infrastructure and subjective community well-being variables suggest that treated individuals come from poorer areas with less developed infrastructures. Overall, the statistical significance of the difference in the means of the socio-economic, infrastructure and household characteristics across these two groups suggests that the governments targeting strategy was appropriate and successful. A small portion of the individuals that received the benefit in 2005 was part of the formal sector social insurance program in the pre-treatment period. It is indicative that certain individuals enrolled formerly in the formal sector insurance programs come from poor households. Working in the formal sector does not fully protect from economic shocks that could lead to vulnerability or poverty. In fact, a non-negligible portion of individuals working in the formal sector could easily fall into vulnerable status and become eligible for the program. Certain household could fall into poverty in between waves. Households could also decide to drop out of the formal sector and move to the informal sector even if they are not poor.

Due to the overall difference in the distribution of characteristics between treated and untreated, employing a linear regression method or fixed effects would lead to biased estimates as the treated individuals are fundamentally different from untreated individuals in the sample. Propensity score matching would allow the balancing on these differences by only comparing observations that are similar in characteristics.

8.2.2. Dependent Variables

Tables 16.a to **d** display the descriptive statistics and t-test for the pre and post-treatment labor market outcomes by treatment status before and after trimming the common support. Trimming reduces the significance of the differences of means between treated units and untreated units. However, the majority t-statistics stay significant even after restricting the area of common support.

Table 16.d shows the post-treatment labor market outcomes means by treatment status after trimming. At first glance, a t-test of the differences in the means between recipients and non-recipients of Askeskin suggest that there is a significant difference in the means of post-treatment values for Informal status and Labor supply measured by the numbers of hours worked per week or weeks worked per year. The means in the post-treatment

level of labor supply at the extensive margin do not exhibit any difference. Nevertheless, one must refrain from drawing inference from a tabulation or a test of means of post-treatment dependent variables values. Post-treatment values do not take into account the initial level of the variable of interest. As shown in **Table 16.c**, the difference in characteristics between treated and untreated individuals in the pre-treatment wave are considerable. In fact, treated individuals are actually less likely to work in the informal sector prior to implementation of the program. They are more likely to participate in the labor force as shown by the probability of working the preceding month and preceding year. They also work less hours per week and more weeks per year. All of these variables are significant. Both the direction and magnitude in the pre-treatment values for the dependent variable would invalidate drawing inference from the significant relationship at the cross-sectional level for post treatment outcomes. The difference in differences method is useful in the sense that it takes into consideration the initial condition and allows to "net" these differences.

Moreover, comparing post-treatment differences in means does not provide a full picture as it does not take into account fundamental differences in the two groups (treated and control). Therefore, not controlling for all potential confounding variables leads to completely biased estimates. In order to reach an unbiased estimate constructing a control group by computing a propensity score from a comprehensive set of characteristics is desirable. The next sections provide the results from propensity score matching with difference in differences.

8.3. Results

8.3.1. Results of the Matching Equation

Table 17 presents the results of the matching equation used to compute the propensity score. As mentioned earlier, the propensity score is computed by probit and the explanatory variables are measured using their pre-treatment values. As expected, most of the variables are statistically significant. Significance of covariates in the matching equation means that treated individuals are materially different from non-treated individuals. Hence, this reinforces the rationale behind the use of propensity score matching with difference in differences, as regression based methods would return biased results. Some variables at individual, household and community level return insignificant results. However, some of these variables such as age, gender and marital status are important determinant of labor market outcomes and there is no valid reason to remove them from the matching equation. In fact, it is known that unless there is a robust reason of excluding certain variables from the equation, it is preferable to keep them if their presence is sound based on previous empirical research or economic theory.

Concerning individual level variables, education and the proxy means tested score are very significant (with a negative and positive coefficient respectively) which is expected since the program is supposed to target the poor. Household composition as measured by household size has a positive significant coefficient since poorer households are usually larger due to a larger number of children but also the presence of the extended family. At the community level, the infrastructure variables are negative and significant which is in line with the fact that individuals living in poorer communities are more likely to be enrolled in Askeskin. Finally, the coefficient on previous programs is positive and very significant since those programs are pro-poor and some households may to be enrolled in several programs.

8.3.2. Informal Status

Table 18 displays the impact of Askeskin on informal status by gender and by residence status. The propensity score matching with difference in differences estimates are insignificant across all samples but exhibit the correct sign in most cases (i.e. positive). Several explanations to this lack of an effect on informality are possible.

The first potential reason is that Askeskin was not perceived to offer any benefit to the population and thus did not provide the necessary incentive to entice workers to move out of the formal sector. This reason is implausible, as research has shown that Askeskin decreased out-of-pocket expenditures between 11% to 34% (Aji et al., 2013) and provided value to beneficiaries by covering them against risk associated with illness. In fact, as shown **Table 1**, the package provided by Askeskin is at least as good as the other insurance programs available and free of charge.

The second explanation could be that since Askeskin does not prevent individuals from working in the formal sector, poorer households working in the formal sector could have been selected for coverage. Therefore, individuals could have kept their formal sector jobs while enrolling in Askeskin. In IFLS, out of all individuals that are in the labor force, 29% work in the formal sector. Therefore, it is possible that no effect is present because individuals can keep their perceivably more valuable formal sector job and receive the benefit. However, this reason is also implausible as the vast majority of individuals enrolled in Askeskin work in the informal sector based on the definition of Central Bureau of Statistics. Based on the IFLS, a simple tabulation of the enrollment in

Askeskin versus the informal status shows that only 30% of the recipients work in the formal sector whereas 70% of the recipient work in the informal sector. This large proportion of informality for Askeskin recipients may also be reason for the absence of evidence of an effect in informality. In fact, since the original proportion of recipient individuals working in the informal sector is high, it is less likely to find an effect at the margin.

A third possibility is that individuals are not willing to move or stay in the informal sector because informal sector jobs are less desirable. Formal sector jobs are more prized by workers and, hence, workers are reluctant to abandon them. This absence of an effect across all samples could be due to the presence of segmented labor markets in Indonesia. Harris and Todaro (1970) posited that in developing countries, individuals are queuing to enter formal sector jobs and when these jobs are attained, it is very unlikely that individuals would move towards informal sector jobs. Individuals in the informal sector would prefer formal to informal jobs under most circumstances. In the Indonesian context, many workers are trapped in informality, as they cannot get jobs in the formal sector. Therefore, the initially large proportion of informal sector workers and lack of mobility towards the formal sector could be also be a reason for the lack of significant evidence on informality. Since most individual's value formal sector jobs more than informal sector jobs, they would require a large compensation in order to move from formal to informal. Thus, implicit incentives such as Askeskin might not be enough to have an impact on informal status.

8.3.3. Labor Supply

Table 18 exhibits the impact of Askeskin on labor supply at the extensive and intensive margin by gender and by residence status in order to investigate the possible heterogeneity in impacts on labor supply. We first investigate the impact at the extensive margin of labor supply by presenting the results of propensity score matching with difference in differences.

The results show an impact for the entire sample of 3% for the probability of working in the preceding week. This effect shown is significant at the 5% level. It is only driven by women and urban areas. Women exhibit an impact on both the probability of working in the week preceding the survey and the year preceding the survey. This effect is negative and very significant. There is a decrease of 5.8% (at the 1% level) in the probability of working in the week preceding the survey and a decrease 4.7% (at the 5% level) in the probability of working in the year preceding the survey. For the probability of working in the preceding year, the effect in urban areas is negative and significant at the 5% level. The magnitude of the effect is 3.4% on average.

Concerning hours worked per week and weeks worked per year, the results are not significant for the full sample and the sample of men. However, for the sample of women and individuals living in urban areas, there is an effect on the number of hours worked per week. The effect is negative and significant at the 10% level. Women decreased their labor supply by 2.52 hours on average due to the program coverage and individuals in urban areas decreased their labor supply by 2.63 hours on average. There is also a negative significant impact on the number of weeks worked per year for women. Overall, women decrease their labor supply by 2.11 weeks per year due to Askeskin.

Table 19 shows the results by further breaking down by gender in urban areas or rural areas. The effect is mainly driven by women in urban areas. However, in this case, since the sample is smaller, the results are less precise leading to less significance.

The results for the intensive margin of labor supply are suggestive of the fact that women decrease the number of hours worked or weeks worked in order to engage in other activities. This is tantamount to an increase in income, which leads to substitution of work for leisure or other activities (for instance, family care activities). Due to the importance of expected health care costs as a proportion of income in developing countries and the prohibitively high cost of private insurance due to missing markets, this income effect can be important as shown by the estimates. The extensive margin estimates reveal that women may altogether decide to not participate in or exit the labor force if insurance coverage and health care consumption are determining factor for labor work. Since Askeskin provides medical goods and services free of charge, certain households that plan to pay for those goods or services may decrease the amount of work or cease labor force participation altogether. Additionally, individuals may decrease their labor supply or drop out of the labor force if they are risk averse to health care risk. This is because Askeskin eliminates risk by providing coverage to recipients, their spouses and dependents. For certain women, if the income effect from the benefit and the opportunity cost of working are high enough they may decide to drop out of the labor force and rely on the income of the primary earner.

There are several possible explanations for these observations. The first one is that health insurance is valued by women higher than its expected value owing to a mix of aversion to risk, highly unpredictable health care expenses and missing market for private health insurance in Indonesia. Women have higher expected health expense because they have the responsibility for both their own health care expenses and their dependents. The opportunity cost of labor work is also higher for women than men as they need to spend time in childcare or homemaking activities. As a result, the value associated to health insurance by women is expected to be high.

The second reason is that certain women are working to provide incremental income to their household and at the margin; these women would prefer not to work or work part-time. In this case, as well, there could be an impact both at the extensive margin and intensive margin as this could determine the decision between working and not working (if the whole household is covered).

Household dynamics can also explain the fact that we mainly observe an impact for women. In developing countries, traditionally, a woman's status as primary earner may not be perceived well. In those societies, social norm dictates that men should be the main provider of the households. This way of thinking is still common in the lower income strata of most developing countries. As a result, although theoretically we should observe an impact for both men and women in a developing setting, it is much more likely that women will be more impacted. Therefore, in households where both household heads are working, it is more likely that women will decrease their labor supply in response to this benefit. This appears to be the case for Indonesia.

This effect is observed primarily in urban areas for several potential reasons. The value of the benefit in urban areas is higher as there is more availability of health services at proximity. In rural areas, individuals are more likely to not seek enrollment or not value it as much because there is a high hidden cost associated with Askeskin in terms of transportation, lower quality of care and less health supply available overall. Sumarto et al. (2011) point to the fact that in rural areas, transportation cost can be considerable for indigent families and discourage them from taking part in Askeskin. Moreover, he highlights the fact that the quality of care provided under Askeskin was lower in some cases. In some poor areas, there were accounts of certain services in the Askeskin benefit package that failed to be delivered.

A second reason could be that in rural areas, the extended family is present to take care of the household when both heads of households are out working. In urban areas, this may not be the case and individuals need to coordinate themselves in order to take care of their households and children. Therefore, the wage earned in the urban labor market might not be attractive enough for women (especially of low socio-economic). Their large opportunity cost of working may cause them to drop out or reduce their labor supply.

8.3.4. Effect by Education Level

Individuals with higher education levels have a wider selection of jobs available to them in the labor market. The job search process is also easier. Moreover, they have access to jobs that require higher levels of skill. Those jobs are likely to be formal and provide better benefits than lower skill jobs. Individuals that have attained lower levels of education have a restricted selection of jobs available to them and those jobs are of lower quality and provide little to no benefits. They are also more likely to be low wage and informal.

Individuals with lower levels of education have lower economic status and are less likely to accumulate savings or have any significant initial economic endowment. They necessitate more work in order to provide for their family. They also are likely to work more hours in order to compensate for the lower wages.

However, individuals with lower levels of education might value Askeskin more than the more educated individuals since they face important barriers of entry to the health insurance industry. In fact, prior to Askeskin, health insurance was provided exclusively through the formal sector and private insurance is prohibitively expensive or altogether missing. As a result, provision to Askeskin may be perceived as a large increase in wage as it provides coverage for an important portion of the households' budgets. We therefore expect to see an effect on labor supply for individuals with lower educational status.

Table 20 presents the results by educational level. There is no significant effect for individuals that have completed junior high school or more. However, we see effect on labor force participation in the previous year and for the number of weeks worked per year for individuals with less than elementary high school. The effects are significant at the 5% level. Individuals with less than elementary school education are 3.8% less likely to work in the preceding week and work 1.883 weeks less per year on average because of receiving Askeskin.

8.3.5. Effect for Individuals with Higher Valuation for Askeskin

We further investigate the hypothesis that individuals that may theoretically value the benefit more should exhibit an important effect. The groups examined are married women, individuals with low health status, older individuals.

For married women, **Table 21** shows that the only impact observed is on labor supply at the extensive margin. The impact is large and very significant impact (at the 1%

level). Married women are 7.2% less likely to participate in the labor market in the precedent week and the precedent year. This impact intuitive because these households have higher expected health care costs. Therefore, households that were previously not covered by health insurance find their households' budget relieved from health care costs. Medical care costs can be large and impose a large burden upon lower income households. The financial effect of Askeskin leads a portion of married women to leave the workforce. The larger magnitude of the effect for the sample of married women as opposed to all women is intuitive as married women are much more likely to have children and higher health care costs. This reinforces the idea the individuals with higher value of insurance should exhibit a larger impact.

The sample of individuals with lower health status is obtained by restricting the sample to individuals that are somewhat unhealthy or unhealthy using the self-reported health status variable. The sample of individuals with lower health status is small. However, we still find an impact on labor supply at the extensive margin at the 5% level. Individuals with lower health status are 7.5% less likely to work in the precedent week due to enrollment in Askeskin. Individuals with lower health status have much lower propensity to work and exhibit lower productivity. Therefore, it is more likely that individuals with lower health status would abandon their employment after receiving the benefits. As a result, for these individuals, receiving Askeskin provides strong incentives to drop out of the labor force altogether.

Older individuals are defined as individuals that are still in working age but in the older tranche of the labor force. In order to conserve a large enough sample, we restrict our sample to individuals that are 35 to 65 years of age. Based on **Table 21**, we observe a very significant impact at both the extensive margin and the intensive margin of labor supply. Older individuals are 0.9% less likely to work in the precedent year due to the availability of Askeskin. This impact is significant at the 1% level. In addition, Askeskin leads to a decrease of 1.653 weeks worked per year on average for recipients. The coefficient is significant at the 5% level. This impact is consistent with previous research in the US by Gruber and Madrian (2002) that find that availability of subsidized insurance increases the odd of retirement. In this case, individuals that were working solely to retain their benefits are likely either to drop out or to reduce their quantity of work.

8.3.6. Labor Market Transitions

Following Azuara and Marinescu (2013), I test for potential impacts on labor market transitions. **Table 22** provides propensity score matching estimates of transition probabilities between different employment statuses (informal, formal and unemployment). As discussed above, theoretically, the provision of free health insurance should render informality more desirable, and as a result, an effect on the transitions towards informality may be observed. Thus in this section, we investigate the impact of Askeskin on various transition probabilities towards informality: unemployed to informal status, formal to informal status, informal to informal status. As a sanity check, we also investigate the transitions towards formality, namely: from informal to formal status and from unemployed to formal status. I generated several dichotomous variables that take value 1 for a certain transition (change in labor status from the pre-treatment period to the post-treatment period), conditional on being in a certain status in the first wave.

Consistent to my results on informality, I do not observe any effect on any transitions. As expected, the transitions from formal to informal are positive in sign however the magnitude is very small and insignificant. Therefore, we cannot infer any movement from the formal sector to the informal sector. The coefficients on the transition from informal to formal and from unemployment to formal are positive. In addition, the movement from unemployment to informal sector is negative. However, the estimates are not significant, which is not suggestive of any movement between these employment statuses. The sign of these coefficients could be suggestive that despite the introduction of Askeskin in the informal sector, formal jobs are still more desirable and hardly substitutable to informal sector jobs. Hence, the benefit provided through Askeskin might be barely enough to push individuals to move to the informal sector. Since the signs of all the coefficients on transitions are statistically insignificant, we conclude that Askeskin does not have an impact on the transitions across different sectors of the labor market.

Chapter Nine: Study Limitations

The first limitation of this study is the lag between the two surveys used. The seven-year lag could be an obstacle in identifying an impact if individuals changed significantly in the seven-year lapse. For instance, an individual that was 18 in the first survey would be more likely to be in the labor force in the second survey, as they would have graduated. However, this may not be an important issue in this study for two reasons. I am comparing the outcomes of Askeskin recipients with outcomes of comparable non-recipients. Askeskin targets individuals that are poor or near poor. These individuals are more likely to be out of the schooling early and to join the workforce at an early age. The proportion of Askeskin recipients that have attended high school or higher education is low. Based on the IFLS4, about 12% of Askeskin recipient have completed high school and only about 2% have completed higher education. Moreover, the effects on labor force participation and quantity of work are still significant when restricting the sample to individuals that are 35 to 65 (for which the propensity of retention in the workforce is homogenous).

In addition, it could be argued that since the economic environment has changed significantly during those seven years, the impacts observed could be due to changes in macroeconomic conditions instead of real changes in labor market behavior due to the availability of Askeskin. The proxy-means tested score and community level variables allow controlling for several infrastructure and economic factors that would be affected by the changes in macroeconomic conditions. These could be considered suitable proxies for macroeconomic conditions. Moreover, residence dummies (urban vs. rural) and province dummies that control for spatial differences in demographic and economic conditions are included. One of the robustness checks was to test whether there was an effect for Askeskin recipient using two surveys prior the program implementations. Those two surveys were much closer in time and the economic conditions were greatly varying. The period between 1997 and 2000 include the important South Asian recession with a large decrease in economic activity and the recovery period starting in 1999. Therefore, if changes in labor supply were related to changes in economic condition we would have observed an impact on labor force participation, quantity of work or informality during that period. The lack of impact suggests that the empirical strategy and controls used in the study successfully account for changes in macroeconomic condition.

The second limitation is related to the estimation method. Although propensity score matching with difference in differences is robust to time invariant unobservable variables, it is not robust to time variant unobservable variables. If certain factors that affect both labor outcomes and eligibility or enrollment in Askeskin are unobservable and vary with time, this could cause our estimates to be biased. It is difficult to account for it. An example of a time-varying factor would be the talent or skill of individuals that could change over time as they become more skilled through either the school system, their life experience or self-training. As mentioned, the test for ignorability assumption using the two surveys prior to the start of Askeskin (IFLS2 and IFLS3) provides an indication that this effect may not be as important. In fact, if the time-varying unobservables were driving the changes observed in our main analysis, I would observe some effect between those two surveys. However, no effect is observed. To conclude, it is difficult to rule out that the effect is not partially due to time varying unobservable as it is difficult to account for it accurately. Only a perfectly designed randomized control trial or a perfect random assignment of Askeskin based on a natural experiment would rule out this effect.

Chapter Ten: Conclusion

This paper has analyzed the impact of the expansion of social health insurance for the poor on labor supply and informality in Indonesia. The results of study suggest that expanding health insurance to the uncovered portion of the population in a lower middle-income country where health insurance is mainly provided by the formal sector may have an impact on labor outcomes. When the country extends government-funded health insurance to a large proportion of vulnerable individuals, this can provide disincentives to work. The results indicate that the effect emerges mostly in the trade-off between work and non-work and on the quantity of work. Nevertheless, the effect on informality is inconclusive as the coefficients obtained from the analysis are statistically insignificant. In the wake of large pushes for universal health reform across the developing world, governments should anticipate a decline in labor force participation. Expansions that provide health insurance to groups that associate a higher value to the health insurance benefit should observe an even larger impact as suggested by the Indonesian experience. These effects need to be considered while implementing those programs as they can be impeding economic growth due to an adverse impact on labor force participation. The impact of Askeskin seem to be present only for women and urban areas. Women from recipient households are more likely to drop out of the labor force because the benefit provides them with the ability to receive all of the health care they and their family need at virtually no cost. In addition, these impacts appear to be more concentrated among demographics that are more vulnerable (namely married women, older individuals, less educated, and poor and near poor) which can have nefarious consequences and help perpetuate the poverty trap. Moreover, in most countries, the labor force participation of women is already lower than men, which leaves them in a vulnerable position. A program that decreases further the labor force participation of women may worsen the condition of this demographic group.

Concerning the incentive to work in the informal sector, no effect is observed. Since all estimates are statistically insignificant, the introduction of Askeskin does not suggest an effect on the choice between informal and formal sector employment. This may be due to the segmented nature of the labor market in Indonesia where formal jobs are more desirable. This is a hopeful message for policy makers and governments around the world as they prepare their transition towards universal health care. In fact, a considerable impact on informality would have important implications on government tax revenues as well as poverty levels (Devicienti, Fernando and Groisma, 2010). It could have unintended effects and work against the policy makers' agenda. Informal jobs are precarious and less stable, leave workers unprotected from economic shocks, and do not provide any protection. There is suggestive evidence that informal jobs are related to more poverty and ultimately impede economic development (World Bank, 2006). For these reasons, the results this study on the impact of health insurance for the poor on informality may be reassuring for policy makers.

Table 1: Description of the Three Major Existing Health Insurance Plans in Indonesia

Attribute	Askes	Jamsostek	Askeskin
Establishment	1968	1992	2005
Population Coverage	19.12 million (about 8.28% of the population) in 2007*	11.18 million (about 4.84% of the population) in 2007*	42.48 million (about 18.3% of the population) in 2007*
Participation	Mandatory	Mandatory for firms with more than 10 workers or payroll of over 1M rupiahs per month; opt-out option for employers with better benefit plans	Social Insurance, selected based on government determined criteria
Organization Carrier	State-owned company (PT Askes Indonesia)	State-owned company (PT Jamsostek Indonesia)	Ministry of Health
Beneficiaries	Civil servants, pensioners of civil servants and armed forces	Formal private employees	Identified Poor and Near-Poor
Eligible dependents	Spouse and 2 oldest children over 21 years of age or over 25 years if student	Spouse and 3 oldest children over 21 years of age	Whole Household
Contribution	Borne by employee and government. Members: 2% of basic salary. Government: 2% of basic salary.	Borne by the employer: Single member- 3% of salary. Member with dependents: 6% of salary.	Borne by the government. No contribution by the beneficiary.
Benefit Package	Outpatient and inpatient care at public providers only	Outpatient care at both public and private providers networks, and for inpatient care at public providers	Outpatient and inpatient care at public providers and a third of private providers
Copayment	Yes, if members want to upgrade class, branded drugs, renal dialysis, heart surgery and transplants	None, but does not cover high cost treatments such as cancer treatment, heart surgery and renal dialysis	None
Negative list	Cosmetic surgery, physical check-up, alternative medicine, dental prostheses, fertility treatment, non-basic immunization	General check-up, cancer treatment, heart surgery, renal dialysis, prostheses, non-basic immunization, transplantation, fertility treatment	Cosmetic surgery, physical check-up, alternative medicine, dental prostheses, fertility treatment

Note: Based on Rokx et al (2009) and Aji et al. (2013), *Authors own estimates from the IFLS.

Table 2: HI Coverage Before and After the Implementation of Askeskin

Health Insurance Program	2000	2007
Askeskin	0.00%	18.39%
Askes	7.96%	8.28%
Jamsostek	3.41%	4.84%
Private Insurance	0.39%	0.99%

Note: Data from the third wave and fourth waves of the IFLS

Table 3: Trend in Public Health Expenditure between 1995 and 2007

Rp trillion

	2001	2002	2003	2004	2005	2006	2007
National Nominal Health Expenditures	9.3	11.0	16.0	17.7	22.2	31.8	39.0
Real National Health Expenditures (2001=100)	9.3	9.8	13.4	14.0	15.9	20.1	23.2
Annual Rate Growth Real Health Expenditures (%)	42.8	6.3	36.5	4.2	13.3	27.0	15.4
Health Expenditures as % of Total Expenditures	2.6	3.3	4.0	4.0	4.2	4.5	5.0
National Health Expenditures as % of GDP	0.5	0.6	0.8	0.8	0.8	1.0	1.1
Overall National Health Expenditures	353.6	337.6	405.4	441.8	533.6	699.5	786.9
Overall Real National Expenditures (2001=100)	353.6	301.8	340.0	348.9	381.4	443.2	469.2

Source: World Bank (2008)

Table 4: Central Bureau of Statistics' Official Definition of Informality

Official Definition of Informal Status of the Indonesian Government	
Formal Workers:	- Self-employed with permanent workers - Individual employed in the formal private sector or by the government
Informal Workers:	- Self-employed without employees - Self-employed assisted by a temporary worker - Casual and Family workers

Note: Based on the BPS' 2015 "Statistics on Informality in Indonesia" official report.

Table 5: OLS and FE Results for the Full Sample

	Worked (previous week)			Worked (previous year)			Informality			Hours per week			Weeks per year		
	OLS	OLS	FE	OLS	OLS	FE	OLS	OLS	FE	OLS	OLS	FE	OLS	OLS	FE
Askeskin	-0.02*	-0.01	-0.03	-0.00	-0.01	-0.02	0.09***	-0.02*	0.05	-3.27***	-1.10*	-1.52	-1.89***	-0.78**	0.50
	(0.01)	(0.01)	(0.02)	(0.01)	(0.01)	(0.02)	(0.01)	(0.01)	(0.03)	(0.50)	(0.61)	(1.79)	(0.30)	(0.37)	(1.20)
Age		0.00*	0.01***		0.00	0.01***		0.01***	0.01***		-0.11***	0.01		0.14***	0.19***
		(0.00)	(0.00)		(0.00)	(0.00)		(0.00)	(0.00)		(0.03)	(0.05)		(0.02)	(0.03)
Male		0.27***	0.25***		0.28***	0.24***		-0.07***	-0.07***		3.34***	5.06***		0.39	-0.29
		(0.01)	(0.01)		(0.01)	(0.01)		(0.01)	(0.02)		(0.47)	(0.97)		(0.29)	(0.65)
Married		0.02*	0.13***		0.02*	0.16***		0.05***	0.05*		1.28*	2.73*		0.86**	1.93**
		(0.01)	(0.02)		(0.01)	(0.02)		(0.01)	(0.03)		(0.70)	(1.40)		(0.43)	(0.93)
Elementary School		-0.02	0.05**		-0.02	0.04*		0.00	0.10***		1.13	2.79		-0.21	0.35
		(0.01)	(0.02)		(0.01)	(0.02)		(0.01)	(0.03)		(0.82)	(1.74)		(0.50)	(1.17)
Junior High School		-0.08***	-0.01		-0.07***	-0.00		-0.01	0.10**		0.93	0.82		-0.10	-0.41
		(0.02)	(0.03)		(0.02)	(0.03)		(0.02)	(0.04)		(1.01)	(2.18)		(0.62)	(1.46)
Senior High School		-0.04**	0.01		-0.05***	0.03		-0.11***	0.01		0.09	0.09		-0.44	1.41
		(0.02)	(0.03)		(0.02)	(0.03)		(0.02)	(0.04)		(1.06)	(2.24)		(0.65)	(1.50)
Higher Education		0.06***	0.10**		0.04**	0.14***		-0.33***	-0.15***		-5.41***	-7.31***		0.34	0.80
		(0.02)	(0.04)		(0.02)	(0.04)		(0.02)	(0.05)		(1.25)	(2.64)		(0.77)	(1.77)
Health Status		-0.05***	-0.05***		-0.03***	-0.04***		-0.00	-0.01		-0.71	-1.15		-0.46	0.12
		(0.01)	(0.01)		(0.01)	(0.01)		(0.01)	(0.02)		(0.47)	(1.04)		(0.29)	(0.69)
Proxy Means Tested Score		-0.00	-0.00		0.00	0.00*		0.00***	-0.00		-0.11***	-0.14***		-0.05***	-0.07***
		(0.00)	(0.00)		(0.00)	(0.00)		(0.00)	(0.00)		(0.02)	(0.03)		(0.01)	(0.02)
Health Card		-0.02*	-0.02		-0.02**	-0.02		-0.00	-0.02		-0.38	-0.96		-0.94***	-1.04
		(0.01)	(0.02)		(0.01)	(0.02)		(0.01)	(0.02)		(0.58)	(1.16)		(0.36)	(0.78)
Askes		-0.01	0.00		-0.02*	-0.05**		-0.26***	-0.25***		-1.87**	-0.65		1.63***	1.67
		(0.01)	(0.02)		(0.01)	(0.02)		(0.02)	(0.04)		(0.91)	(1.82)		(0.56)	(1.22)
Jamsostek		0.11***	0.18***		0.10***	0.18***		-0.43***	-0.41***		4.43***	6.96***		4.58***	4.94***
		(0.02)	(0.03)		(0.02)	(0.03)		(0.02)	(0.04)		(1.02)	(2.18)		(0.63)	(1.46)
Unconditional Cash Transfer		0.00	0.03		0.01	0.03		-0.01	0.06*		-1.37**	-1.32		-0.26	-1.75
		(0.01)	(0.02)		(0.01)	(0.02)		(0.01)	(0.03)		(0.59)	(1.67)		(0.36)	(1.11)
Household's Head Age		-0.00**	-0.00***		-0.00	-0.00***		0.00***	0.00		-0.07***	-0.08		-0.07***	-0.06*
		(0.00)	(0.00)		(0.00)	(0.00)		(0.00)	(0.00)		(0.02)	(0.05)		(0.02)	(0.03)
Household Size		-0.00**	-0.00		-0.01***	-0.01*		-0.00	0.00		0.05	-0.14		-0.03	-0.09
		(0.00)	(0.00)		(0.00)	(0.00)		(0.00)	(0.00)		(0.10)	(0.21)		(0.06)	(0.14)
Household Head is Female		0.02	0.13***		0.04***	0.13***		-0.02	-0.07**		-0.53	1.77		0.41	0.56
		(0.01)	(0.02)		(0.01)	(0.02)		(0.01)	(0.03)		(0.81)	(1.59)		(0.50)	(1.07)
Number of Children <12 Years		-0.02***	-0.02**		-0.02***	-0.01*		0.01	-0.02		0.24	0.82		-0.19	0.11
		(0.00)	(0.01)		(0.00)	(0.01)		(0.00)	(0.01)		(0.26)	(0.54)		(0.16)	(0.36)
Presence of Asphalt Road		-0.03**	-0.01		-0.02	0.01		-0.06***	0.03		2.16**	0.12		1.82***	1.31
		(0.01)	(0.02)		(0.01)	(0.02)		(0.01)	(0.03)		(0.84)	(1.58)		(0.51)	(1.06)
Presence of a Midw ife		0.00	0.03*		0.00	0.02		0.00	0.06**		-2.37***	-0.52		0.08	-0.79
		(0.01)	(0.02)		(0.01)	(0.02)		(0.01)	(0.02)		(0.62)	(1.17)		(0.38)	(0.78)
% of Households w ith Electricity		-0.00***	-0.00**		-0.00***	-0.00**		-0.00***	-0.00*		0.07***	0.07**		-0.01	-0.04*
		(0.00)	(0.00)		(0.00)	(0.00)		(0.00)	(0.00)		(0.01)	(0.03)		(0.01)	(0.02)
Presence of a Sew age System		0.00	-0.02		-0.01	-0.02		0.03***	0.01		0.84	1.37		0.03	-0.68
		(0.01)	(0.02)		(0.01)	(0.02)		(0.01)	(0.02)		(0.56)	(1.24)		(0.34)	(0.83)
Presence of Piped Water		-0.01	-0.05***		0.00	-0.04**		-0.02	-0.08***		-1.48**	-0.32		0.26	0.33
		(0.01)	(0.02)		(0.01)	(0.02)		(0.01)	(0.02)		(0.62)	(1.27)		(0.38)	(0.85)
Number of Health Centers		-0.00*	0.00		-0.00***	-0.00		-0.00***	0.00		0.07	0.09		-0.03	-0.13**
		(0.00)	(0.00)		(0.00)	(0.00)		(0.00)	(0.00)		(0.05)	(0.09)		(0.03)	(0.06)
Subjective Village Wealth		0.00	-0.01		0.01*	-0.00		0.00	-0.01		0.13	-0.93		-0.44**	-0.33
		(0.01)	(0.01)		(0.00)	(0.01)		(0.01)	(0.01)		(0.32)	(0.71)		(0.20)	(0.48)
Urban Area		-0.05***	0.03		-0.05***	0.00		-0.09***	-0.11***		3.33***	3.95***		0.23	0.88
		(0.01)	(0.02)		(0.01)	(0.02)		(0.01)	(0.03)		(0.62)	(1.30)		(0.38)	(0.87)
Province Dummies		X	X		X	X		X	X		X	X		X	X
Observations	18,172	12,317	14,109	19,714	12,309	14,107	13,828	9,554	10,042	14,059	9,698	10,109	14,055	9,696	10,109
R-Squared	0.00	0.14	0.17	0.00	0.16	0.21	0.01	0.31	0.24	0.00	0.07	0.09	0.00	0.05	0.08

***** p<0.01, ** p<0.05, * p<0.1. Robust standard errors in parentheses.**

Table 6: OLS and FE Results for the Sample of Men

	Worked (previous week)			Worked (previous year)			Informality			Hours per week			Weeks per year		
	OLS	OLS	FE	OLS	OLS	FE	OLS	OLS	FE	OLS	OLS	FE	OLS	OLS	FE
Askeskin	-0.01	-0.00	-0.01	0.01	0.01	0.02	0.10***	-0.04**	0.02	-2.81***	-1.03	2.11	-1.61***	-0.66	1.20
	(0.01)	(0.01)	(0.04)	(0.01)	(0.01)	(0.04)	(0.01)	(0.02)	(0.07)	(0.63)	(0.78)	(3.24)	(0.40)	(0.50)	(2.22)
Age		-0.00***	0.00***		-0.00***	0.01***		0.01***	0.01***		-0.17***	0.05		0.15***	0.11
		(0.00)	(0.00)		(0.00)	(0.00)		(0.00)	(0.00)		(0.04)	(0.10)		(0.02)	(0.07)
Male		0.00	0.00		0.00	0.00		0.00	0.00		0.00	0.00		0.00	0.00
		(0.00)	(0.00)		(0.00)	(0.00)		(0.00)	(0.00)		(0.00)	(0.00)		(0.00)	(0.00)
Married		0.14***	0.26***		0.12***	0.27***		0.04**	-0.05		3.18***	3.54		1.50**	2.72
		(0.01)	(0.04)		(0.01)	(0.03)		(0.02)	(0.06)		(1.03)	(2.83)		(0.66)	(1.94)
Elementary School		-0.01	-0.01		-0.01	0.07		-0.03	-0.02		-0.51	3.16		-1.82**	-4.62*
		(0.02)	(0.06)		(0.01)	(0.05)		(0.02)	(0.08)		(1.27)	(3.98)		(0.82)	(2.74)
Junior High School		-0.06***	-0.03		-0.04***	0.04		-0.04	-0.05		-0.89	2.52		-1.81*	-4.87
		(0.02)	(0.06)		(0.02)	(0.06)		(0.03)	(0.10)		(1.45)	(4.63)		(0.93)	(3.17)
Senior High School		-0.01	-0.04		-0.04**	0.04		-0.11***	-0.15		-0.67	2.22		-1.58	-2.91
		(0.02)	(0.06)		(0.02)	(0.06)		(0.03)	(0.10)		(1.50)	(4.56)		(0.96)	(3.13)
Higher Education		-0.01	0.08		-0.03*	0.14**		-0.27***	-0.13		-6.21***	-3.69		-1.00	-7.13**
		(0.02)	(0.07)		(0.02)	(0.07)		(0.03)	(0.11)		(1.77)	(5.23)		(1.14)	(3.61)
Health Status		-0.06***	-0.07***		-0.04***	-0.09***		0.00	0.02		-1.01*	-3.28		-0.06	1.03
		(0.01)	(0.03)		(0.01)	(0.02)		(0.01)	(0.04)		(0.61)	(2.00)		(0.39)	(1.36)
Proxy Means Tested Score		-0.00	0.00		-0.00	0.00		0.00***	-0.00		-0.08***	-0.05		-0.03***	-0.08**
		(0.00)	(0.00)		(0.00)	(0.00)		(0.00)	(0.00)		(0.02)	(0.06)		(0.01)	(0.04)
Health Card		-0.02	0.00		-0.01	0.02		0.01	-0.04		-0.42	-3.01		-1.02**	-1.42
		(0.01)	(0.03)		(0.01)	(0.03)		(0.01)	(0.04)		(0.75)	(2.13)		(0.48)	(1.46)
Askes		-0.03**	-0.07		-0.06***	-0.12***		-0.33***	-0.39***		-2.13*	-1.47		1.64**	2.50
		(0.02)	(0.04)		(0.01)	(0.04)		(0.02)	(0.07)		(1.20)	(3.30)		(0.78)	(2.27)
Jamsostek		0.06***	0.11**		0.04***	0.07		-0.44***	-0.41***		1.39	4.40		4.57***	4.78*
		(0.02)	(0.06)		(0.01)	(0.05)		(0.03)	(0.09)		(1.29)	(4.05)		(0.83)	(2.77)
Unconditional Cash Transfer		-0.00	0.01		-0.00	-0.02		0.00	0.06		-1.04	-2.20		-0.36	-1.77
		(0.01)	(0.04)		(0.01)	(0.04)		(0.01)	(0.06)		(0.76)	(2.92)		(0.49)	(1.99)
Household's Head Age		-0.00	-0.00**		-0.00	-0.00		0.00***	-0.00		-0.07**	-0.02		-0.08***	0.01
		(0.00)	(0.00)		(0.00)	(0.00)		(0.00)	(0.00)		(0.04)	(0.09)		(0.02)	(0.06)
Household Size		-0.00***	-0.00		-0.00***	-0.01**		-0.00	-0.00		0.17	-0.37		-0.08	-0.20
		(0.00)	(0.01)		(0.00)	(0.00)		(0.00)	(0.01)		(0.13)	(0.35)		(0.08)	(0.24)
Household Head is Female		-0.01	0.06		-0.01	0.01		-0.03	-0.06		-2.93**	2.97		1.11	-0.88
		(0.02)	(0.05)		(0.01)	(0.04)		(0.03)	(0.08)		(1.42)	(3.59)		(0.92)	(2.46)
Number of Children <12 Years		0.01*	0.01		0.00	0.02*		-0.00	-0.03		0.64*	2.19**		0.08	0.27
		(0.00)	(0.01)		(0.00)	(0.01)		(0.01)	(0.02)		(0.34)	(0.96)		(0.22)	(0.66)
Presence of Asphalt Road		-0.01	0.02		0.01	0.02		-0.07***	0.03		3.19***	-2.06		1.44**	2.74
		(0.02)	(0.04)		(0.01)	(0.04)		(0.02)	(0.06)		(1.08)	(2.97)		(0.69)	(2.04)
Presence of a Midwife		0.00	0.00		0.01	0.00		0.01	0.07		-2.04**	0.54		0.54	-0.59
		(0.01)	(0.03)		(0.01)	(0.03)		(0.02)	(0.05)		(0.80)	(2.12)		(0.52)	(1.45)
% of Households with Electricity		-0.00***	-0.00		-0.00***	-0.00		-0.00**	-0.00		0.06***	0.09		-0.00	-0.05
		(0.00)	(0.00)		(0.00)	(0.00)		(0.00)	(0.00)		(0.02)	(0.06)		(0.01)	(0.04)
Presence of a Sewage System		0.02	0.05		-0.00	0.03		0.04**	0.03		1.23*	4.52**		-0.15	-0.27
		(0.01)	(0.03)		(0.01)	(0.03)		(0.01)	(0.05)		(0.72)	(2.14)		(0.46)	(1.46)
Presence of Piped Water		-0.01	-0.06**		0.00	-0.08***		-0.02	-0.07		-1.62**	-1.02		0.27	-1.98
		(0.01)	(0.03)		(0.01)	(0.03)		(0.02)	(0.05)		(0.80)	(2.21)		(0.52)	(1.51)
Number of Health Centers		-0.00	0.00		-0.00***	-0.00		-0.00***	0.00		0.23***	0.32*		-0.08**	0.02
		(0.00)	(0.00)		(0.00)	(0.00)		(0.00)	(0.00)		(0.07)	(0.17)		(0.04)	(0.12)
Subjective Village Wealth		0.01**	0.01		0.02***	0.00		0.01	0.02		-0.28	-0.33		-0.05	-0.12
		(0.01)	(0.02)		(0.00)	(0.02)		(0.01)	(0.03)		(0.41)	(1.31)		(0.26)	(0.90)
Urban Area		-0.05***	-0.03		-0.02***	-0.00		-0.11***	-0.21***		2.86***	5.05**		0.01	1.39
		(0.01)	(0.03)		(0.01)	(0.03)		(0.02)	(0.05)		(0.80)	(2.33)		(0.51)	(1.60)
Province Dummies		X	X		X	X		X	X		X	X		X	X
Observations	8,425	5,640	6,599	9,297	5,636	6,599	7,649	5,106	5,498	7,813	5,205	5,542	7,812	5,205	5,544
R-Squared	0.00	0.07	0.25	0.00	0.09	0.34	0.01	0.30	0.25	0.00	0.08	0.11	0.00	0.05	0.11

***** p<0.01, ** p<0.05, * p<0.1. Robust standard errors in parentheses.**

Table 7: OLS and FE Results for the Sample of Women

	Worked (previous week)			Worked (previous year)			Informality			Hours per week			Weeks per year		
	OLS	OLS	FE	OLS	OLS	FE	OLS	OLS	FE	OLS	OLS	FE	OLS	OLS	FE
Askeskin	-0.02	-0.02	-0.03	-0.00	-0.02	-0.06	0.08***	0.00	0.06	-3.83***	-1.24	-0.96	-2.23***	-0.91	3.20
	(0.01)	(0.02)	(0.05)	(0.01)	(0.01)	(0.05)	(0.02)	(0.01)	(0.07)	(0.80)	(0.95)	(4.65)	(0.46)	(0.56)	(2.87)
Age		0.00	0.01***		-0.00	0.01***		0.01***	0.01***		-0.10**	-0.08		0.12***	0.17**
		(0.00)	(0.00)		(0.00)	(0.00)		(0.00)	(0.00)		(0.04)	(0.13)		(0.02)	(0.08)
Male		0.00	0.00		0.00	0.00		0.00	0.00		0.00	0.00		0.00	0.00
		(0.00)	(0.00)		(0.00)	(0.00)		(0.00)	(0.00)		(0.00)	(0.00)		(0.00)	(0.00)
Married		-0.05***	0.02		-0.04***	0.08**		0.09***	0.15***		0.00	-0.55		-0.16	-1.19
		(0.02)	(0.04)		(0.02)	(0.04)		(0.02)	(0.05)		(1.06)	(3.37)		(0.63)	(2.08)
Elementary School		-0.01	0.05		0.00	0.07		0.02	0.15**		2.19*	5.54		0.54	3.37
		(0.02)	(0.05)		(0.02)	(0.05)		(0.02)	(0.06)		(1.12)	(3.98)		(0.66)	(2.46)
Junior High School		-0.08***	-0.03		-0.08***	0.00		0.01	0.11		2.74*	6.15		1.00	0.88
		(0.02)	(0.06)		(0.02)	(0.06)		(0.02)	(0.09)		(1.49)	(5.30)		(0.88)	(3.26)
Senior High School		-0.04	-0.00		-0.05*	0.05		-0.11***	0.06		1.01	1.87		-0.19	1.53
		(0.03)	(0.06)		(0.03)	(0.06)		(0.02)	(0.09)		(1.59)	(5.68)		(0.94)	(3.50)
Higher Education		0.14***	0.13*		0.13***	0.27***		-0.40***	-0.37***		-4.61**	-4.28		0.84	2.71
		(0.03)	(0.08)		(0.03)	(0.08)		(0.03)	(0.11)		(1.83)	(6.54)		(1.08)	(4.03)
Health Status		-0.03***	-0.05*		-0.01	-0.04		-0.02	-0.04		-0.31	0.23		-0.94**	-1.14
		(0.01)	(0.03)		(0.01)	(0.03)		(0.01)	(0.04)		(0.72)	(2.57)		(0.42)	(1.59)
Proxy Means Tested Score		-0.00	-0.00		0.00	0.00		-0.00	-0.00**		-0.13***	0.00		-0.07***	-0.06
		(0.00)	(0.00)		(0.00)	(0.00)		(0.00)	(0.00)		(0.02)	(0.08)		(0.01)	(0.05)
Health Card		-0.02	0.01		-0.03*	-0.00		-0.01	-0.10**		-0.35	2.71		-0.85	-2.89
		(0.01)	(0.03)		(0.01)	(0.03)		(0.01)	(0.05)		(0.90)	(2.95)		(0.53)	(1.82)
Askes		0.01	0.05		0.01	-0.03		-0.21***	-0.23***		-1.89	6.71		1.64**	8.60***
		(0.02)	(0.05)		(0.02)	(0.05)		(0.02)	(0.07)		(1.39)	(4.48)		(0.82)	(2.76)
Jamsostek		0.13***	0.31***		0.13***	0.31***		-0.40***	-0.46***		7.72***	7.69		4.42***	0.25
		(0.03)	(0.07)		(0.03)	(0.07)		(0.03)	(0.09)		(1.63)	(5.64)		(0.96)	(3.48)
Unconditional Cash Transfer		0.01	0.05		0.02	0.06		-0.03**	0.06		-1.67*	-5.70		-0.15	-2.97
		(0.01)	(0.05)		(0.01)	(0.05)		(0.01)	(0.07)		(0.91)	(4.42)		(0.53)	(2.72)
Household's Head Age		0.00	-0.00**		0.00	-0.00*		-0.00	-0.00		-0.02	-0.10		-0.04**	-0.14*
		(0.00)	(0.00)		(0.00)	(0.00)		(0.00)	(0.00)		(0.04)	(0.12)		(0.02)	(0.08)
Household Size		-0.00	-0.00		-0.00**	-0.01		-0.00*	0.01		-0.02	-0.33		0.01	0.16
		(0.00)	(0.01)		(0.00)	(0.01)		(0.00)	(0.01)		(0.15)	(0.59)		(0.09)	(0.36)
Household Head is Female		-0.01	0.11***		0.02	0.14***		0.00	0.04		-0.35	-1.62		-0.10	-2.79
		(0.02)	(0.04)		(0.02)	(0.04)		(0.02)	(0.06)		(1.10)	(3.67)		(0.65)	(2.26)
Number of Children <12 Years		-0.05***	-0.04**		-0.05***	-0.03*		0.02***	-0.02		-0.38	0.07		-0.56**	-1.79**
		(0.01)	(0.02)		(0.01)	(0.02)		(0.01)	(0.02)		(0.41)	(1.45)		(0.24)	(0.90)
Presence of Asphalt Road		-0.05**	-0.03		-0.04*	0.02		-0.05**	-0.02		0.93	-1.45		2.10***	-0.56
		(0.02)	(0.05)		(0.02)	(0.05)		(0.02)	(0.06)		(1.30)	(3.89)		(0.77)	(2.40)
Presence of a Midwife		0.01	0.08**		0.00	0.04		-0.01	0.01		-2.60***	-3.61		-0.40	-3.91**
		(0.02)	(0.03)		(0.02)	(0.03)		(0.01)	(0.05)		(0.95)	(2.80)		(0.56)	(1.72)
% of Households with Electricity		-0.00***	-0.00*		-0.00***	-0.00		-0.00***	-0.00		0.07***	-0.01		-0.02*	-0.02
		(0.00)	(0.00)		(0.00)	(0.00)		(0.00)	(0.00)		(0.02)	(0.07)		(0.01)	(0.05)
Presence of a Sewage System		-0.01	-0.04		-0.03*	-0.04		0.02	0.03		0.43	-1.63		0.23	-2.32
		(0.01)	(0.04)		(0.01)	(0.04)		(0.01)	(0.05)		(0.88)	(3.09)		(0.51)	(1.91)
Presence of Piped Water		-0.01	-0.04		0.00	-0.03		-0.01	-0.06		-1.45	-0.55		0.27	0.46
		(0.02)	(0.04)		(0.02)	(0.04)		(0.02)	(0.05)		(0.97)	(3.27)		(0.57)	(2.01)
Number of Health Centers		-0.00	0.00		-0.00	0.01**		-0.00	-0.00		-0.10	-0.27		0.03	-0.30**
		(0.00)	(0.00)		(0.00)	(0.00)		(0.00)	(0.00)		(0.08)	(0.24)		(0.05)	(0.15)
Subjective Village Wealth		-0.00	-0.02		0.00	-0.02		0.00	0.01		0.58	1.51		-0.84***	0.83
		(0.01)	(0.02)		(0.01)	(0.02)		(0.01)	(0.03)		(0.49)	(1.86)		(0.29)	(1.15)
Urban Area		-0.05***	0.09**		-0.07***	0.03		-0.08***	-0.01		3.75***	6.11*		0.48	-0.21
		(0.02)	(0.04)		(0.02)	(0.04)		(0.02)	(0.05)		(0.97)	(3.26)		(0.57)	(2.01)
Province Dummies		X	X		X	X		X	X		X	X		X	X
Observations	9,747	6,677	7,510	10,417	6,673	7,508	6,179	4,448	4,544	6,246	4,493	4,567	6,243	4,491	4,565
R-Squared	0.00	0.07	0.13	0.00	0.09	0.16	0.00	0.34	0.37	0.00	0.07	0.12	0.00	0.06	0.15

***** p<0.01, ** p<0.05, * p<0.1. Robust standard errors in parentheses.**

Table 8: OLS and FE Results for the Urban Sample

	Worked (previous week)			Worked (previous year)			Informality			Hours per week			Weeks per year		
	OLS	OLS	FE	OLS	OLS	FE	OLS	OLS	FE	OLS	OLS	FE	OLS	OLS	FE
Askeskin	-0.02*	-0.02	0.07	-0.01	-0.01	0.02	0.09***	-0.02	0.15	-4.51***	-1.66*	-4.19	-1.39***	-0.62	4.37
	(0.01)	(0.02)	(0.06)	(0.01)	(0.01)	(0.06)	(0.02)	(0.02)	(0.10)	(0.75)	(1.00)	(4.88)	(0.44)	(0.59)	(2.91)
Age		-0.00	0.01***		-0.00**	0.01***		0.01***	0.01***		-0.14***	0.03		0.17***	0.32***
		(0.00)	(0.00)		(0.00)	(0.00)		(0.00)	(0.00)		(0.04)	(0.13)		(0.02)	(0.08)
Male		0.28***	0.24***		0.32***	0.25***		-0.10***	-0.12**		2.67***	1.13		-0.43	-0.23
		(0.01)	(0.03)		(0.01)	(0.03)		(0.01)	(0.05)		(0.75)	(2.46)		(0.44)	(1.47)
Married		0.01	0.10**		0.01	0.13***		0.05***	-0.01		1.61	5.72		0.82	0.37
		(0.02)	(0.04)		(0.02)	(0.04)		(0.02)	(0.07)		(1.06)	(3.59)		(0.62)	(2.14)
Elementary School		0.01	0.12		-0.00	0.10		0.01	-0.00		-0.63	5.19		-0.03	4.13
		(0.03)	(0.08)		(0.02)	(0.08)		(0.03)	(0.12)		(1.66)	(6.10)		(0.98)	(3.68)
Junior High School		-0.05*	0.12		-0.07**	0.11		0.00	0.01		0.67	1.98		-0.59	2.58
		(0.03)	(0.08)		(0.03)	(0.08)		(0.03)	(0.13)		(1.87)	(6.84)		(1.10)	(4.12)
Senior High School		-0.03	0.14*		-0.05*	0.13		-0.13***	-0.12		-1.24	2.52		0.02	6.09
		(0.03)	(0.08)		(0.03)	(0.08)		(0.03)	(0.13)		(1.87)	(6.64)		(1.10)	(4.01)
Higher Education		0.07**	0.25***		0.06*	0.29***		-0.29***	-0.19		-6.65***	-2.94		0.82	4.44
		(0.03)	(0.09)		(0.03)	(0.09)		(0.04)	(0.14)		(2.07)	(7.13)		(1.21)	(4.33)
Health Status		-0.04***	-0.03		-0.02**	-0.02		-0.01	0.01		0.06	-2.96		-0.02	-0.93
		(0.01)	(0.03)		(0.01)	(0.03)		(0.01)	(0.05)		(0.72)	(2.63)		(0.42)	(1.57)
Proxy Means Tested Score		-0.00	0.00		-0.00	-0.00		0.00	-0.00		-0.08***	-0.09		-0.06***	-0.03
		(0.00)	(0.00)		(0.00)	(0.00)		(0.00)	(0.00)		(0.03)	(0.09)		(0.01)	(0.05)
Health Card		-0.01	-0.00		-0.02	-0.01		-0.01	-0.04		-1.61*	-0.76		-0.16	2.04
		(0.01)	(0.04)		(0.01)	(0.04)		(0.02)	(0.06)		(0.93)	(3.02)		(0.54)	(1.82)
Askes		-0.01	0.04		-0.03	-0.02		-0.27***	-0.33***		-3.19***	-5.67		1.08	0.59
		(0.02)	(0.05)		(0.02)	(0.05)		(0.02)	(0.09)		(1.20)	(4.43)		(0.70)	(2.64)
Jamsostek		0.11***	0.17***		0.10***	0.16***		-0.37***	-0.25***		3.19**	6.55		4.26***	3.15
		(0.02)	(0.06)		(0.02)	(0.06)		(0.02)	(0.09)		(1.31)	(4.54)		(0.77)	(2.71)
Unconditional Cash Transfer		0.01	0.01		0.03*	0.02		-0.02	-0.01		-2.60**	1.20		0.03	-4.27
		(0.02)	(0.06)		(0.02)	(0.06)		(0.02)	(0.09)		(1.02)	(4.70)		(0.59)	(2.81)
Household's Head Age		-0.00**	-0.00**		-0.00**	-0.00**		0.00**	-0.00		-0.08**	-0.03		-0.08***	-0.08
		(0.00)	(0.00)		(0.00)	(0.00)		(0.00)	(0.00)		(0.04)	(0.13)		(0.02)	(0.08)
Household Size		-0.00	-0.01		-0.00*	-0.00		-0.01*	-0.00		0.15	-0.33		0.02	0.22
		(0.00)	(0.01)		(0.00)	(0.01)		(0.00)	(0.01)		(0.14)	(0.58)		(0.08)	(0.35)
Household Head is Female		0.04**	0.09*		0.05***	0.06		-0.04*	-0.14*		-1.04	-0.78		-0.10	0.29
		(0.02)	(0.05)		(0.02)	(0.05)		(0.02)	(0.08)		(1.19)	(4.12)		(0.69)	(2.46)
Number of Children <12 Years		-0.01**	-0.00		-0.02***	-0.02		0.02**	0.02		-0.56	-0.44		-0.43*	-0.02
		(0.01)	(0.02)		(0.01)	(0.02)		(0.01)	(0.03)		(0.41)	(1.52)		(0.24)	(0.91)
Presence of Asphalt Road		-0.03	-0.00		-0.01	-0.02		-0.02	0.47***		3.82	10.27		10.47***	-3.68
		(0.07)	(0.11)		(0.07)	(0.10)		(0.08)	(0.16)		(4.48)	(8.08)		(2.61)	(4.86)
Presence of a Midwife		0.02	-0.02		0.02	-0.01		-0.00	0.02		-2.61***	0.75		0.10	-2.68
		(0.01)	(0.04)		(0.01)	(0.04)		(0.02)	(0.07)		(0.91)	(3.33)		(0.53)	(1.99)
% of Households with Electricity		-0.00	-0.00		-0.00	0.00		-0.00***	-0.01		0.05	0.03		0.08**	-0.07
		(0.00)	(0.00)		(0.00)	(0.00)		(0.00)	(0.00)		(0.07)	(0.25)		(0.04)	(0.15)
Presence of a Sewage System		0.02	-0.07		-0.01	-0.08*		0.03	0.17**		2.98***	7.81*		0.05	2.40
		(0.02)	(0.05)		(0.02)	(0.05)		(0.02)	(0.08)		(1.15)	(4.07)		(0.67)	(2.43)
Presence of Piped Water		0.02	-0.03		0.03	-0.01		-0.01	-0.03		-0.49	-7.32*		-0.95	-1.13
		(0.02)	(0.05)		(0.02)	(0.05)		(0.02)	(0.08)		(1.12)	(4.18)		(0.65)	(2.50)
Number of Health Centers		-0.00**	-0.00		-0.00***	-0.00		-0.00**	0.00		0.10*	0.20		-0.05	-0.08
		(0.00)	(0.00)		(0.00)	(0.00)		(0.00)	(0.00)		(0.06)	(0.17)		(0.04)	(0.10)
Subjective Village Wealth		0.00	-0.05		0.00	-0.04		-0.00	0.02		0.24	-3.27		-0.06	-1.86
		(0.01)	(0.03)		(0.01)	(0.03)		(0.01)	(0.05)		(0.50)	(2.42)		(0.29)	(1.45)
Urban Area															
Province Dummies		X	X		X	X		X	X		X	X		X	X
Observations	9,735	5,802	6,859	10,716	5,798	6,857	7,041	4,218	4,556	7,199	4,310	4,598	7,192	4,302	4,595
R-Squared	0.00	0.13	0.19	0.00	0.17	0.22	0.00	0.27	0.24	0.01	0.06	0.11	0.00	0.05	0.15

***** p<0.01, ** p<0.05, * p<0.1. Robust standard errors in parentheses.**

Table 9: OLS and FE Results for the Rural Sample

	Worked (previous week)			Worked (previous year)			Informality			Hours per week			Weeks per year		
	OLS	OLS	FE	OLS	OLS	FE	OLS	OLS	FE	OLS	OLS	FE	OLS	OLS	FE
Askeskin	-0.02*	-0.01	-0.01	-0.01	-0.01	0.00	0.05***	-0.02	0.05	-1.31**	-0.75	-0.32	-2.13***	-1.27***	-2.03
	(0.01)	(0.01)	(0.05)	(0.01)	(0.01)	(0.04)	(0.01)	(0.01)	(0.05)	(0.66)	(0.76)	(3.14)	(0.42)	(0.49)	(2.15)
Age		0.00***	0.01***		0.00***	0.01***		0.00***	0.01***		-0.09***	-0.08		0.12***	0.13*
		(0.00)	(0.00)		(0.00)	(0.00)		(0.00)	(0.00)		(0.03)	(0.10)		(0.02)	(0.07)
Male		0.25***	0.30***		0.26***	0.26***		-0.05***	-0.07**		3.82***	5.40***		0.99**	-1.16
		(0.01)	(0.03)		(0.01)	(0.02)		(0.01)	(0.03)		(0.60)	(1.87)		(0.39)	(1.28)
Married		0.03*	0.17***		0.03*	0.23***		0.05***	-0.02		1.31	5.89**		0.92	0.58
		(0.02)	(0.04)		(0.01)	(0.04)		(0.02)	(0.05)		(0.94)	(2.86)		(0.61)	(1.96)
Elementary School		-0.03*	-0.02		-0.01	0.00		-0.00	-0.00		1.76*	4.14		-0.36	-2.43
		(0.02)	(0.04)		(0.01)	(0.04)		(0.02)	(0.05)		(0.92)	(2.87)		(0.59)	(1.97)
Junior High School		-0.08***	-0.01		-0.06***	0.03		-0.03	-0.01		0.42	3.73		0.14	-3.68
		(0.02)	(0.05)		(0.02)	(0.05)		(0.02)	(0.07)		(1.21)	(3.85)		(0.78)	(2.64)
Senior High School		-0.03	-0.06		-0.04**	-0.05		-0.07***	-0.07		0.28	-0.96		-1.16	-2.23
		(0.02)	(0.06)		(0.02)	(0.05)		(0.02)	(0.07)		(1.37)	(4.11)		(0.88)	(2.82)
Higher Education		0.05	0.02		0.01	0.11		-0.42***	-0.46***		-5.64***	-3.30		-0.19	-6.50*
		(0.03)	(0.08)		(0.03)	(0.07)		(0.03)	(0.09)		(1.76)	(5.20)		(1.13)	(3.56)
Health Status		-0.05***	-0.05*		-0.03***	-0.03		0.00	-0.01		-1.54**	-0.79		-0.75*	0.84
		(0.01)	(0.03)		(0.01)	(0.03)		(0.01)	(0.03)		(0.61)	(1.89)		(0.39)	(1.28)
Proxy Means Tested Score		-0.00	-0.00		0.00	-0.00		0.00***	-0.00		-0.12***	-0.25***		-0.05***	-0.11***
		(0.00)	(0.00)		(0.00)	(0.00)		(0.00)	(0.00)		(0.02)	(0.06)		(0.01)	(0.04)
Health Card		-0.02*	0.02		-0.02	-0.00		0.00	0.03		0.74	4.50*		-1.27***	-1.45
		(0.01)	(0.03)		(0.01)	(0.03)		(0.01)	(0.04)		(0.74)	(2.37)		(0.48)	(1.62)
Askes		0.02	0.01		0.01	-0.04		-0.26***	-0.22***		0.63	-0.64		1.82*	4.86*
		(0.02)	(0.06)		(0.02)	(0.05)		(0.02)	(0.07)		(1.48)	(4.00)		(0.95)	(2.75)
Jamsostek		0.11***	0.30***		0.11***	0.22**		-0.58***	-0.52***		6.45***	3.30		5.65***	6.42
		(0.03)	(0.10)		(0.03)	(0.09)		(0.03)	(0.12)		(1.74)	(6.84)		(1.12)	(4.69)
Unconditional Cash Transfer		-0.01	0.03		-0.01	0.04		-0.01	0.08		-0.59	-2.35		-0.33	-0.91
		(0.01)	(0.04)		(0.01)	(0.04)		(0.01)	(0.05)		(0.71)	(2.88)		(0.46)	(1.97)
Household's Head Age		-0.00	-0.00***		0.00	-0.00*		0.00*	-0.00		-0.05	-0.06		-0.05**	-0.08
		(0.00)	(0.00)		(0.00)	(0.00)		(0.00)	(0.00)		(0.03)	(0.09)		(0.02)	(0.06)
Household Size		-0.00	0.01		-0.01***	0.00		0.00	0.00		-0.06	-0.57		-0.11	-0.10
		(0.00)	(0.01)		(0.00)	(0.01)		(0.00)	(0.01)		(0.13)	(0.36)		(0.08)	(0.25)
Household Head is Female		-0.00	0.10**		0.02	0.13***		-0.00	-0.03		0.15	1.39		0.92	-2.95
		(0.02)	(0.04)		(0.02)	(0.04)		(0.02)	(0.06)		(1.11)	(3.20)		(0.71)	(2.19)
Number of Children <12 Years		-0.02***	-0.03*		-0.02***	-0.03**		-0.00	-0.00		0.97***	2.30**		0.09	0.42
		(0.01)	(0.01)		(0.01)	(0.01)		(0.01)	(0.02)		(0.34)	(0.97)		(0.22)	(0.66)
Presence of Asphalt Road		-0.02	-0.04		-0.02	-0.01		-0.04***	0.13***		2.79***	-1.86		1.40**	3.06*
		(0.01)	(0.04)		(0.01)	(0.04)		(0.01)	(0.05)		(0.86)	(2.66)		(0.55)	(1.82)
Presence of a Midwife		-0.02	0.05		-0.01	0.06		0.01	0.06		-1.51	0.82		0.01	-0.52
		(0.02)	(0.04)		(0.01)	(0.04)		(0.02)	(0.05)		(0.93)	(3.05)		(0.60)	(2.09)
% of Households with Electricity		-0.00***	-0.00**		-0.00***	-0.00**		-0.00***	-0.00		0.08***	-0.11*		-0.01	0.02
		(0.00)	(0.00)		(0.00)	(0.00)		(0.00)	(0.00)		(0.02)	(0.06)		(0.01)	(0.04)
Presence of a Sewage System		-0.00	0.04		-0.02*	0.03		0.03***	0.02		-0.08	-4.07*		0.32	-0.45
		(0.01)	(0.03)		(0.01)	(0.03)		(0.01)	(0.04)		(0.65)	(2.17)		(0.42)	(1.49)
Presence of Piped Water		-0.03*	-0.07*		-0.02*	-0.07**		-0.03**	-0.07		-1.67**	1.43		0.62	1.16
		(0.01)	(0.04)		(0.01)	(0.04)		(0.01)	(0.05)		(0.80)	(2.79)		(0.51)	(1.91)
Number of Health Centers		-0.00	0.01		-0.00	0.01*		-0.00**	0.02***		-0.13	0.49		0.04	-0.11
		(0.00)	(0.01)		(0.00)	(0.01)		(0.00)	(0.01)		(0.11)	(0.37)		(0.07)	(0.25)
Subjective Village Wealth		0.01	-0.04		0.02***	-0.02		0.01*	-0.00		-0.35	0.93		-1.20***	-2.75**
		(0.01)	(0.03)		(0.01)	(0.03)		(0.01)	(0.03)		(0.47)	(1.94)		(0.30)	(1.33)
Urban Area															
Province Dummies		X	X		X	X		X	X		X	X		X	X
Observations	8,437	6,515	7,250	8,998	6,511	7,250	6,787	5,336	5,486	6,860	5,388	5,511	6,863	5,394	5,514
R-Squared	0.00	0.14	0.23	0.00	0.16	0.27	0.00	0.26	0.23	0.00	0.08	0.15	0.00	0.06	0.11

***** p<0.01, ** p<0.05, * p<0.1. Robust standard errors in parentheses.**

Table 10: Cross-Sectional Propensity Score Matching Estimates

Dependent Variable	Full Sample			Men			Women			Urban			Rural		
	Obs.	ATT s.e.	t-stat	Obs.	ATT s.e.	t-stat	Obs.	ATT s.e.	t-stat	Obs.	ATT s.e.	t-stat	Obs.	ATT s.e.	t-stat
Worked (Previous Week)	T=3,243 C=14,929	-0.016 (0.01)	-1.60	T=1,493 C=6,932	-0.027* (0.016)	-1.69	T=1,750 C=7,997	-0.027* (0.016)	-1.69	T=1,552 C=8,183	-0.011 (0.016)	-0.69	T=1,691 C=6,746	-0.02 (0.014)	-1.43
Worked (Previous Year)	T=3,515 C=16,199	-0.005 (0.01)	-0.50	T=1,634 C=7,663	-0.018 (0.018)	-1.00	T=1,881 C=8,536	-0.018 (0.018)	-1.00	T=1,696 C=9,020	0.008 (0.016)	0.50	T=1,819 C=7,179	-0.014 (0.013)	-1.08
Informality	T=2,477 C=11,357	-0.017 (0.012)	-1.42	T=1,374 C=6,275	-0.026 (0.016)	-1.63	T=1,103 C=5,076	-0.003 (0.015)	0.07	T=1,120 C=5,921	-0.018 (0.018)	-1.00	T=1,357 C=5,430	-0.016 (0.013)	-1.23
Hours per Week	T=2,493 C=11,566	-1.64*** (0.63)	-2.60	T=1,386 C=6,427	-1.246** (0.573)	-2.17	T=1,107 C=5,139	-1.82* (0.8)	-1.66	T=1,133 C=6,066	-2.691*** (0.833)	-3.23	T=1,360 C=5,500	-0.779 (0.884)	-0.88
Weeks per Year	T=2,493 C=11,562	-1.00** (0.408)	-2.45	T=1,385 C=6,427	-0.798 (0.563)	-1.42	T=1,108 C=5,135	-1.246** (0.573)	-2.16	T=1,133 C=6,059	-0.515 (0.587)	-0.88	T=1,360 C=5,503	-1.35*** (0.444)	-3.05

***** p<0.01, ** p<0.05, * p<0.1. propensity score matching estimates by Kernel matching. ATT: average treatment effect on the treated. Standard errors in parentheses. C represents the control units whereas T represents the treated units.**

Table 11: Testing the Ignorability of Treatment Assumption Using IFLS2 and IFLS3

Dependent Variable	Full Sample			Men			Women			Urban			Rural		
	Obs.	ATT s.e.	t-stat	Obs.	ATT s.e.	t-stat	Obs.	ATT s.e.	t-stat	Obs.	ATT s.e.	t-stat	Obs.	ATT s.e.	t-stat
Worked (Previous Week)	T=2264 C=6547	-0.004 (0.013)	-0.33	T=1,038 C=3,014	-0.016 (0.018)	-0.88	T=1226 C=3533	0.003 (0.02)	0.16	T=1054 C=3298	0.003 (0.018)	0.14	T=1210 C=3249	-0.009 (0.019)	-0.47
Worked (Previous Year)	T=2264 C=6548	0 (0.012)	0.03	T=1038 C=3015	-0.005 (0.014)	-0.37	T=1226 C=3533	0.003 (0.02)	0.16	T=1054 C=3299	0 (0.021)	-0.01	T=1210 C=3249	0 (0.017)	-0.02
Informality	T=1723 C=4753	-0.009 (0.015)	-0.63	T=921 C=2620	-0.018 (0.019)	-0.91	T=802 C=2133	0.001 (0.02)	0.06	T=748 C=2253	0.015 (0.024)	0.63	T=975 C=2500	-0.024 (0.022)	-1.11
Hours per Week	T=1722 C=4749	-0.056 (0.894)	-0.06	T=920 C=2618	-0.586 (1.291)	-0.45	T=802 C=2131	0.566 (1.306)	0.43	T=748 C=2249	-0.305 (1.565)	-0.20	T=974 C=2500	0.005 (1.042)	0.01
Weeks per Year	T=1723 C=4748	-0.865 (0.594)	-1.46	T=921 C=2619	-0.564 (0.759)	-0.74	T=802 C=2129	-1.35 (0.92)	-1.47	T=748 C=2250	-1.181 (0.894)	-1.32	T=975 C=2498	-0.653 (0.72)	-0.91

***** p<0.01, ** p<0.05, * p<0.1. propensity score matching estimates by Kernel matching. ATT: average treatment effect on the treated. Standard errors in parentheses.**
C represents the control units whereas T represents the treated units.

Table 12: Descriptive Statistics and T-Test Between Attritors and Non-Attritors

Variable	Present in both surveys			Present only in 2000			t-test
	Observations	Mean	Std. Dev.	Observations	Mean	Std. Dev.	
Age	35090	29.19	20.34	2448	30.29	21.09	-2.52
Male	36510	0.49	0.50	2557	0.46	0.50	3.27
Married	35060	0.46	0.50	2437	0.41	0.49	4.17
Elementary School	36511	0.38	0.49	2557	0.27	0.45	12.20
Junior High School	36511	0.15	0.36	2557	0.14	0.35	1.25
Senior High School	36511	0.14	0.35	2557	0.21	0.41	-7.91
Higher Education	36511	0.05	0.22	2557	0.12	0.33	-11.29
Proxy Means Tested Score	33879	35.73	18.84	2235	31.18	20.40	10.26
Health Status	17576	2.07	0.46	928	2.09	0.50	-1.41
Household Size	36511	6.70	2.94	2557	6.37	3.53	4.71
Household' Head Age	36463	47.50	13.46	2541	47.40	16.76	0.30
Household Head is Female	36511	0.14	0.34	2557	0.21	0.41	-8.95
Number of Children under 12 years old	36511	1.42	1.23	2557	0.99	1.07	19.22
Worked in the Previous Week	17583	0.61	0.49	935	0.52	0.50	5.25
Hours per Week	12261	37.00	24.71	545	42.49	27.23	-4.62
Informal Status	12281	0.76	0.43	546	0.59	0.49	8.08
Presence of Asphalt Road	28349	0.80	0.40	1402	0.91	0.28	-13.93
% of Households with Electricity	28127	84.27	21.56	1394	90.77	15.10	-15.33
Number of Health Centers	28347	7.36	6.58	1396	9.10	7.80	-8.16
Presence of Piped Water	28507	0.55	0.50	1402	0.79	0.40	-22.04
Presence of a Sewage System	28507	0.54	0.50	1402	0.77	0.42	-19.61
Subjective Village Wealth	28507	3.17	0.71	1402	3.29	0.71	-6.07
Urban Area	36511	0.48	0.50	2557	0.73	0.44	-28.03

Table 13: Binary Dependent Variable Model of Attrition Using Baseline Values

Variables	P(Absent in 2007 | Present in 2000)
Age	0.00***
	(0.00)
Male	-0.01
	(0.00)
Married	0.02***
	(0.01)
Elementary School	-0.01
	(0.01)
Junior High School	0.00
	(0.01)
Senior High School	0.00
	(0.01)
Higher Education	0.04***
	(0.01)
Proxy means tested Score	0.00**
	(0.00)
Health Status	-0.00
	(0.00)
Household Size	-0.00
	(0.00)
Household' Head Age	0.00
	(0.00)
Household Head is Female	0.01
	(0.01)
Number of Children <12	-0.01***
	(0.00)
Worked in the previous Week	-0.01
	(0.01)
Hours per Week	0.00**
	(0.00)
Informal Status	-0.01***
	(0.00)
Presence of Asphalt Road	-0.00
	(0.01)
% of Households with electricity	0.00
	(0.00)
Number of Health Centers	0.00***
	(0.00)
Presence of Piped Water	0.01**
	(0.00)
Presence of a Sewage System	0.01
	(0.00)
Subjective Village Wealth	-0.00
	(0.00)
Urban Area	0.02***
	(0.01)
Obervations	10,428
R-squared	0.03

Note: *** p<0.01, ** p<0.05, * p<0.1. Standard errors in parentheses.

Table 14: Descriptive Statistics by Treatment Status (without Trimming)

Explanatory Variables	Full Sample		Non Treated		Treated		
	Mean	Std. Dev.	Mean	Std. Dev	Mean	Std. Dev	t-stat
Individual Characteristics							
Age	33.21	12.11	33.05	12.07	33.86	12.23	-3.05
Male	0.46	0.50	0.46	0.50	0.46	0.50	0.35
Married	0.67	0.47	0.67	0.47	0.68	0.47	-1.01
Elementary School	0.43	0.49	0.41	0.49	0.52	0.50	-10.85
Junior High School	0.20	0.40	0.20	0.40	0.18	0.39	2.15
Senior High School	0.23	0.42	0.25	0.43	0.14	0.35	13.81
Higher Education	0.06	0.24	0.07	0.26	0.02	0.15	13.03
Health Status	2.03	0.43	2.02	0.43	2.05	0.43	-2.92
Askes	0.10	0.30	0.10	0.30	0.06	0.23	12.63
Jamsostek	0.04	0.21	0.05	0.22	0.02	0.14	8.08
Household Characteristics							
Proxy means tested Score	34.78	18.61	33.43	18.58	40.86	17.48	-19.35
Household's Head Age	46.60	12.09	46.56	12.03	45.33	12.51	-0.89
Household Size	6.33	2.69	6.34	2.69	6.26	2.70	1.43
Household Head is Female	0.12	0.33	0.12	0.32	0.14	0.35	-3.31
Number of Children Under 12 Years	1.21	1.13	1.21	1.12	1.23	1.16	-0.95
Health Card	0.21	0.41	0.18	0.39	0.32	0.47	-14.24
Unconditional Cash Transfer	0.21	0.41	0.14	0.35	0.51	0.50	-35.52
Community Level Characteristics							
Presence of Asphalt Road	0.81	0.39	0.81	0.39	0.80	0.40	0.97
Presence of a Midwife	0.55	0.50	0.53	0.50	0.63	0.48	-9.78
Percentage of Households with Electric	84.77	21.75	85.63	21.12	80.83	24.07	9.38
Presence of a sewage system	0.56	0.50	0.57	0.50	0.52	0.50	4.12
Presence of Piped Water	0.59	0.49	0.60	0.49	0.53	0.50	5.95
Number of Health Centers	7.76	6.65	7.93	6.81	6.96	5.82	7.41
Subjective Village Wealth	3.18	0.68	3.19	0.67	3.13	0.73	3.92
Urban Area	0.50	0.50	0.51	0.50	0.44	0.50	6.50
Province Dummies							
Lives in North Sumatra	0.06	0.25	0.07	0.25	0.04	0.20	5.80
Lives in Yogyakarta	0.06	0.24	0.06	0.24	0.07	0.26	-2.70
Lives in West Sumatra	0.05	0.21	0.05	0.22	0.04	0.20	1.94
Lives in East Java	0.16	0.36	0.17	0.37	0.12	0.32	6.66
Lives in South Sumatra	0.04	0.20	0.04	0.19	0.06	0.24	-5.06
Lives in West Nusa Tenggara	0.07	0.25	0.05	0.23	0.12	0.32	-9.68
Lives in Jakarta	0.10	0.30	0.11	0.31	0.05	0.21	11.56
Lives in South Kalimantan	0.04	0.20	0.04	0.21	0.03	0.18	3.22
Lives in West Java	0.13	0.33	0.13	0.33	0.14	0.34	-1.49
Lives in South Sulawesi	0.06	0.23	0.05	0.22	0.07	0.26	-3.68
Lives in Central Java	0.14	0.35	0.13	0.34	0.18	0.39	-6.29
Lives in Bali	0.06	0.23	0.06	0.24	0.04	0.20	3.76
Observations	13963		11289		2612		

Table 15: Descriptive Statistics by Treatment Status (after Trimming)

Explanatory Variables	Full sample		Non Treated		Treated		
	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev	t-stat
Individual Characteristics							
Age	33.60	12.40	33.45	12.44	34.00	12.25	-1.84
Male	0.46	0.50	0.46	0.50	0.46	0.50	0.08
Married	0.67	0.47	0.67	0.47	0.68	0.46	-1.21
Elementary School	0.52	0.50	0.51	0.50	0.56	0.50	-3.78
Junior High School	0.20	0.40	0.21	0.41	0.18	0.38	3.27
Senior High School	0.14	0.35	0.15	0.36	0.11	0.32	5.24
Higher Education	0.02	0.13	0.02	0.13	0.01	0.09	4.05
Health Status	2.05	0.43	2.05	0.43	2.05	0.44	-0.06
Askes	0.06	0.24	0.07	0.26	0.05	0.22	4.19
Jamsostek	0.02	0.14	0.02	0.14	0.02	0.13	0.26
Household Characteristics							
Proxy means tested Score	40.32	17.43	39.64	17.50	42.70	16.83	-19.35
Household's Head Age	46.73	12.30	46.75	12.27	46.68	12.38	0.21
Household Size	6.34	2.74	6.37	2.78	6.25	2.63	1.91
Household Head is Female	0.14	0.34	0.13	0.34	0.15	0.35	-1.48
Number of Children < 12	1.21	1.15	1.21	1.14	1.24	1.17	-1.24
Health Card	0.30	0.46	0.27	0.45	0.36	0.48	-7.67
Unconditional cash transfer	0.34	0.47	0.25	0.43	0.59	0.49	-29.17
Community Level Characteristics							
Presence of Asphalt Road	0.81	0.39	0.81	0.39	0.80	0.40	0.98
Presence of a Midwife	0.62	0.49	0.61	0.49	0.65	0.48	-3.63
% of Households with Electricity	81.60	23.35	82.21	22.84	79.46	24.83	4.64
Presence of a sewage system	0.52	0.50	0.52	0.50	0.50	0.50	4.12
Presence of Piped Water	0.54	0.50	0.55	0.50	0.51	0.50	5.95
Number of Health Centers	6.80	5.19	6.83	5.24	6.60	4.98	1.86
Subjective Village Wealth	3.15	0.69	3.16	0.67	3.12	0.74	2.53
Urban Area	0.46	0.50	0.47	0.50	0.43	0.50	2.88
Province Dummies							
Lives in North Sumatra	0.04	0.19	0.04	0.19	0.03	0.17	1.25
Lives in Yogyakarta	0.08	0.27	0.08	0.27	0.07	0.26	1.11
Lives in West Sumatra	0.05	0.22	0.05	0.23	0.04	0.19	3.78
Lives in East Java	0.11	0.31	0.11	0.31	0.10	0.30	1.02
Lives in South Sumatra	0.06	0.23	0.05	0.23	0.07	0.25	-2.42
Lives in West Nusa Tenggara	0.10	0.30	0.09	0.28	0.14	0.34	-5.96
Lives in Jakarta	0.04	0.20	0.04	0.20	0.03	0.17	2.71
Lives in South Kalimantan	0.04	0.19	0.04	0.20	0.03	0.17	2.50
Lives in West Java	0.14	0.35	0.14	0.35	0.15	0.36	-1.27
Lives in South Sulawesi	0.08	0.27	0.08	0.28	0.08	0.27	0.34
Lives in Central Java	0.18	0.39	0.18	0.38	0.20	0.40	-1.82
Lives in Bali	0.05	0.22	0.05	0.22	0.04	0.20	2.67
Observations	8874		6548		2264		

Table 16.a: Descriptive Statistics for Pre-Treatment Labor Market Outcomes by Treatment (before Trimming)

Dependent Variables	Observations (Non-Treated)	Observations (Treated)	*Non-Treated*		*Treated*		t-stat
			Mean	Std. Dev	Mean	Std. Dev	
Informality	8049	1973	0.58	0.49	0.56	0.50	-1.67
Worked(Previous year)	11289	2612	0.67	0.47	0.72	0.45	-5.55
Worked(Previous yeek)	11288	2612	0.62	0.49	0.65	0.48	-3.02
Hours per week	8044	1972	37.95	24.15	35.35	24.31	4.27
Weeks per year	8044	1973	41.00	15.21	39.38	16.24	4.01

Table 16.b: Descriptive statistics for Post-Treatment Labor Market Outcomes by Treatment (before Trimming)

Dependent Variables	Observations (Non-Treated)	Observations (Treated)	*Non-Treated*		*Treated*		t-stat
			Mean	Std. Dev	Mean	Std. Dev	
Informality	8247	1954	0.65	0.48	0.72	0.45	-6.31
Worked(Previous year)	11282	2608	0.78	0.42	0.79	0.41	-1.84
Worked(Previous week)	10787	2506	0.74	0.44	0.74	0.44	0.13
Hours per week	8384	1964	39.23	22.76	36.03	22.34	5.70
Weeks per year	8381	1967	42.61	13.42	40.82	14.51	4.99

Table 16.c: Descriptive statistics for Pre-treatment Labor Market Outcomes by Treatment (after Trimming)

Dependent Variables	Observations (Non-Treated)	Observations (Treated)	*Non-Treated*		*Treated*		t-stat
			Mean	Std. Dev	Mean	Std. Dev	
Informality	4753	1723	0.62	0.49	0.59	0.49	2.15
Worked(Previous Year)	6548	2264	0.68	0.46	0.73	0.44	-4.10
Worked(Previous Week)	6547	2264	0.62	0.49	0.65	0.48	-2.83
Hours per Week	4749	1722	36.60	24.35	34.80	24.14	2.64
Weeks per Year	4748	1723	39.90	15.74	39.04	16.44	1.88

Table 16.d: Descriptive Statistics for Post-Treatment Labor Market Outcomes by Treatment (after Trimming)

Dependent Variables	Observations (Non-Treated)	Observations (Treated)	*Non-Treated*		*Treated*		t-stat
			Mean	Std. Dev	Mean	Std. Dev	
Informality	4854	1704	0.72	0.45	0.74	0.44	-1.82
Worked(Previous Year)	6545	2262	0.78	0.41	0.79	0.41	-0.70
Worked(Previous Week)	6298	2506	0.74	0.44	0.74	0.44	0.38
Hours per Week	4919	1714	38.07	22.52	35.77	22.20	3.67
Weeks per Year	4912	1716	42.03	13.84	40.59	14.71	3.55

Table 17: Results of the Matching Equation

Explanatory variables	Dependent variable: Askeskin			
	Coeff.	Std. error	z	P>z
Age	(0.00)	0.00	-1.12	0.261
Male	0.05	0.03	1.53	0.127
Married	0.02	0.04	0.34	0.731
Elementary School	(0.09)	0.05	-1.73*	0.084
Junior High School	(0.12)	0.06	-1.83*	0.067
Senior High School	(0.24)	0.07	-3.42***	0.001
Higher Education	(0.46)	0.15	-2.97***	0.003
Proxy means tested Score	0.01	0.00	5.92***	0
Household Size	0.02	0.01	2.25**	0.025
Household's Head Age	(0.00)	0.00	-1.48	0.14
Presence of Asphalt Road	0.16	0.05	3.47***	0.001
Subjective Village Wealth	0.09	0.03	3.54***	0
Household Head is Female	0.04	0.05	0.81	0.421
Health Status	0.03	0.04	0.93	0.351
Health Card	0.31	0.03	9.14***	0
Presence of a Midwife	(0.04)	0.04	-1.01	0.314
Number of Children < 12	(0.04)	0.02	-2.43	0.015
Number of Health Centers	(0.02)	0.00	-3.95***	0
% Households with Electricity	(0.00)	0.00	-2.32**	0.02
Presence of Piped Water	(0.01)	0.04	-0.22	0.829
Presence of a sewage system	(0.17)	0.04	-4.26***	0.00
Urban Area	0.20	0.04	4.57***	0
Lives in North Sumatra	0.14	0.12	1.18	0.237
Lives in Yogyakarta	0.72	0.12	6.05***	0
Lives in West Sumatra	0.16	0.12	1.34	0.179
Lives in East Java	0.19	0.10	1.86*	0.063
Lives in South Sumatra	0.70	0.11	6.28***	0
Lives in Bali	0.42	0.12	3.45***	0.001
Lives in West Nusa Tenggara	0.83	0.11	7.8***	0
Lives in Jakarta	0.26	0.13	2.06**	0.04
Lives in South Kalimantan	0.20	0.12	1.63	0.104
Lives in West Java	0.58	0.10	5.68***	0
Lives in South Sulawesi	0.48	0.11	4.38***	0
Lives in Central Java	0.50	0.10	5.04***	0
Askes	(0.05)	0.08	-0.61	0.542
Jamsostek	0.12	0.11	1.07	0.284
Unconditional cash transfer	0.92	0.03	27.66***	0
Constant	(1.84)	0.18	-10.04***	0
Observations	8673			
Log likelihood	-4399			
Pseudo R-Squared	0.1127			

Table 18: Propensity Score Matching with Difference in Differences Results for the Different Subgroups

Dependent Variable	Full Sample			Men			Women			Urban			Rural		
	Obs.	ATT s.e.	t-stat	Obs.	ATT s.e.	t-stat	Obs.	ATT s.e.	t-stat	Obs.	ATT s.e.	t-stat	Obs.	ATT s.e.	t-stat
Worked (Previous Week)	T =2184 C=6298	-0.03** (0.015)	-1.97	T= 987 C=2850	0.005 (0.019)	0.284	T=1197 C=3448	-0.058*** (0.022)	-2.585	T=1017 C=3152	-0.033 (0.024)	-1.407	T=1167 C=3146	-0.026 (0.02)	-1.30
Worked (Previous Year)	T=2262 C=6545	-0.02 (0.014)	-1.457	T=1036 C=3014	0.013 (0.011)	1.12	T=1226 C=3531	-0.047** (0.021)	-2.27	T=1053 C=3297	-0.034** (0.02)	-1.675	T=1209 C=3248	-0.008 (0.018)	-0.42
Informality	T=1704 C =4854	0.002 (0.02)	0.105	T=919 C=2578	0.002 (0.023)	0.09	T=785 C=2276	0.001 (0.021)	0.068	T=743 C=2294	-0.035 (0.027)	-1.303	T=961 C=2560	0.028 (0.022)	1.29
Hours per Week	T=1714 C=4919	-1.188 (0.972)	-1.222	T= 926 C=2620	-0.247 (1.408)	-0.176	T=788 C=2299	-2.522* (1.523)	-1.657	T=750 C=2336	-2.631* (1.484)	-1.773	T=964 C=2583	-0.097 (1.189)	-0.08
Weeks per Year	T=1716 C=4912	-1.071 (0.66)	-1.619	T=927 C=2618	-0.287 (0.839)	-0.342	T=789 C=2294	-2.11** (0.976)	-2.163	T=750 C=2329	-1.156 (1.098)	-1.052	T=966 C=2583	-0.981 (0.927)	-1.06

***** p<0.01, ** p<0.05, * p<0.1. Propensity score matching estimates by Kernel matching. ATT: average treatment effect on the treated. Standard errors in parentheses.**
C represents the control units whereas T represents the treated units.

Table 19: Propensity Score Matching with Difference in Differences Results by Gender and Urban Status

Dependent Variable	Men in Urban Area			Men in Rural Area			Women in Urban Area			Women in Rural Area		
	Obs.	ATT s.e.	t-stat	Obs.	ATT s.e.	t-stat	Obs.	ATT s.e.	t-stat	Obs.	ATT s.e.	t-stat
Worked (Previous Week)	T=450 C=1452	-0.045* (0.025)	-1.83	T=537 C=1398	0.029 (0.021)	1.37	T =567 C=1700	-0.038 (0.027)	-1.40	T=630 C=1748	-0.059 (0.026)	-2.31
Worked (Previous Year)	T=473 C=1553	-0.027 (0.02)	-1.345	T=563 C=1461	0.024* (0.014)	1.651	T=580 C=1744	-0.046** (0.022)	-2.123	T=646 C=1787	-0.042* (0.023)	-1.801
Informality	T=349 C=1081	-0.004 (0.032)	-0.128	T=481 C=1218	0.03 (0.029)	1.053	T=245 C=681	0.024 (0.03)	0.796	T=338 C=921	0.025 (0.025)	1.006
Hours per Week	T=350 C=1105	-2.778 (1.74)	-1.597	T=483 C=1231	1.356 (1.508)	0.899	T=248 C=692	-3.994* (2.169)	-1.841	T=338 C=930	-0.989 (1.971)	-0.502
Weeks per Year	T=350 C=1103	-0.512 (1.115)	-0.459	T=485 C=1232	-0.015 (0.916)	-0.016	T=248 C=689	-0.363 (1.245)	-0.292	T=339 C=928	-1.415 (0.979)	-1.445

***** p<0.01, ** p<0.05, * p<0.1. propensity score matching estimates by Kernel matching. ATT: average treatment effect on the treated.**
C represents the control units whereas T represents the treated units. Standard errors in parentheses.

Table 20: Propensity Score Matching with Difference in Differences Results by Education Level

Dependent Variable	Elementary school and less			Junior High School			Senior High School and Higher		
	Obs.	ATT s.e.	t-stat	Obs.	ATT s.e.	t-stat	Obs.	ATT s.e.	t-stat
Worked (Previous Week)	T=1529 C=3768	-0.038** (0.017)	-2.23	T=345 C=1,208	-0.014 (0.035)	-0.39	T=310 C=1,321	-0.01 (0.042)	-0.23
Worked (Previous Year)	T=1577 C=3909	-0.013 (0.015)	-0.885	T=359 C=1,250	-0.052 (0.038)	-1.392	T=326 C=1,386	-0.008 (0.047)	-0.18
Informality	T=1054 C=2558	0.032 (0.024)	1.292	T=203 C=678	-0.05 (0.042)	-1.193	T=156 C=665	-0.004 (0.062)	-0.063
Hours per Week	T=1057 C=2587	-0.942 (1.229)	-0.767	T =204 C = 696	-1.803 (2.784)	-0.648	T=158 C=675	-3.527 (3.03)	-1.164
Weeks per Year	T=1058 C=2579	-1.883** (0.893)	-2.11	T=206 C=697	-0.08 (1.828)	-0.044	T=158 C=676	0.919 (2.392)	0.384

***** p<0.01, ** p<0.05, * p<0.1. propensity score matching estimates by Kernel matching. ATT: average treatment effect on the treated.**
C represents the control units whereas T represents the treated units. Standard errors in parentheses.

Table 21: Propensity Score Matching with Difference in Differences Results for Groups with Higher Value for Askeskin

Dependent Variable	Married Women			35 to 65 years of age			Lower Health Status		
	Obs.	ATT s.e.	t-stat	Obs.	ATT s.e.	t-stat	Obs.	ATT s.e.	t-stat
Worked (Previous Week)	T=890 C=2658	-0.072*** (0.026)	-2.75	T=1330 C=3710	-0.017 (0.017)	-0.99	T=307 C=600	-0.075** (0.038)	-1.99
Worked (Previous Year)	T=904 C=2702	-0.072*** (0.023)	-3.141	T=1370 C=3843	-0.009*** (0.016)	-3.122	T=311 C=845	-0.005 (0.027)	-0.172
Informality	T=563 C=1727	-0.007 (0.019)	-0.382	T=1068 C=2953	-0.012 (0.013)	-0.894	T=153 C=489	-0.064 (0.046)	-1.383
Hours per Week	T=564 C= 1745	-0.931 (1.68)	-0.554	T=1073 C=3003	-0.648 (1.184)	-0.547	T=253 C=646	0.293 (2.448)	0.12
Weeks per Year	T=565 C=1742	-1.253 (1.266)	-0.99	T=1074 C=2997	-1.653** (0.731)	-2.261	T=254 C=644	-0.846 (1.636)	-0.517

*** p<0.01, ** p<0.05, * p<0.1. propensity score matching estimates by Kernel matching. ATT: average treatment effect on the treated.
C represents the control units whereas T represents the treated units. Standard errors in parentheses.

Table 22: Propensity Score Matching Estimates of Transition Probabilities between Formality and Non Employment

Transitions	Obs.	ATT s.e.	t-stat
Formal to Informal	T=572 C= 1492	0.004 (0.024)	0.166
Informal to Informal	T=841 C=2409	-0.016 (0.015)	-1.05
Informal to Formal	T=841 C=2409	0.016 (0.015)	1.088
Unemployment to Informal	T=334 C=1152	-0.017 (0.033)	-0.532
Unemployment to Formal	T=334 C=1152	0.017 (0.031)	0.559

***** p<0.01, ** p<0.05, * p<0.1. propensity score matching estimates by Kernel matching.**

C represents the control units whereas T represents the treated units.

ATT: average treatment effect on the treated. Standard errors in parentheses.

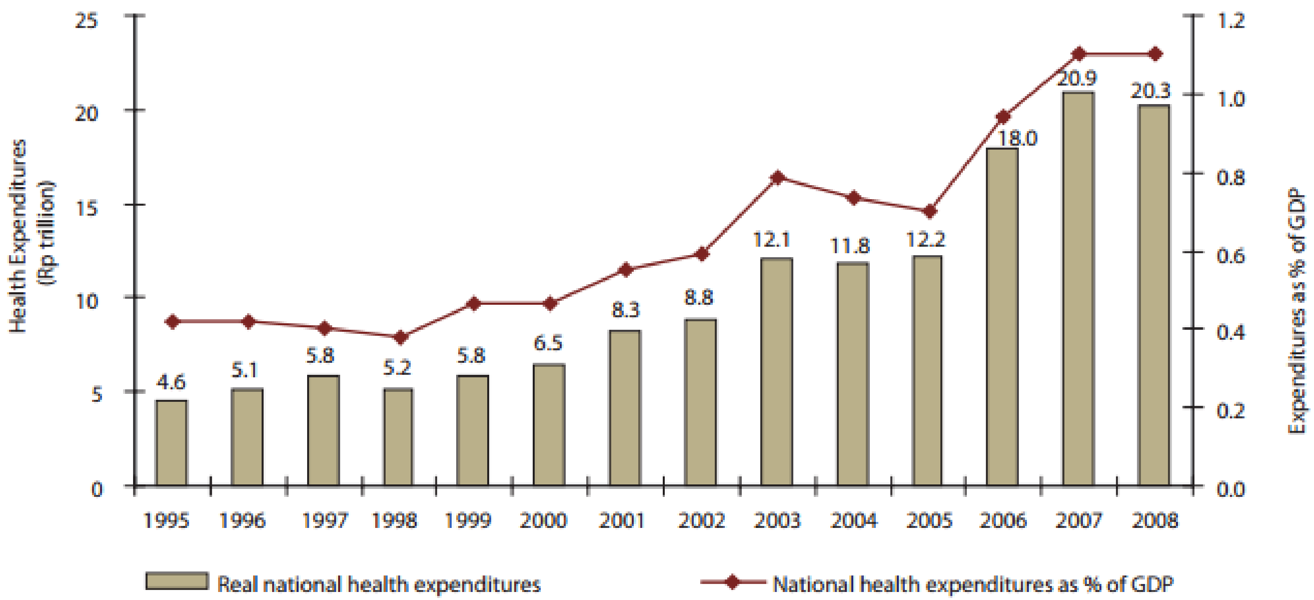

Figure 1: Histogram of Public Health Expenditure in Indonesia between 1995 and 2007

Source: World Bank (2008)

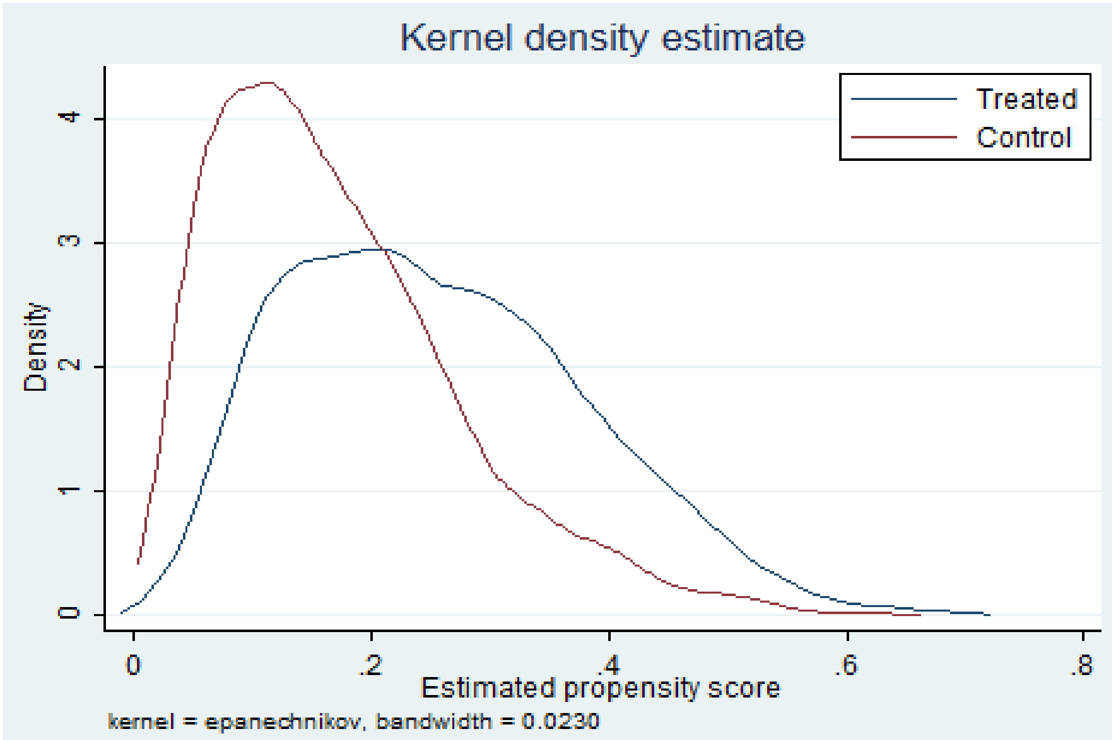

Figure 2: Kernel Density Graph of the Propensity Score without Trimming the Common Support Area

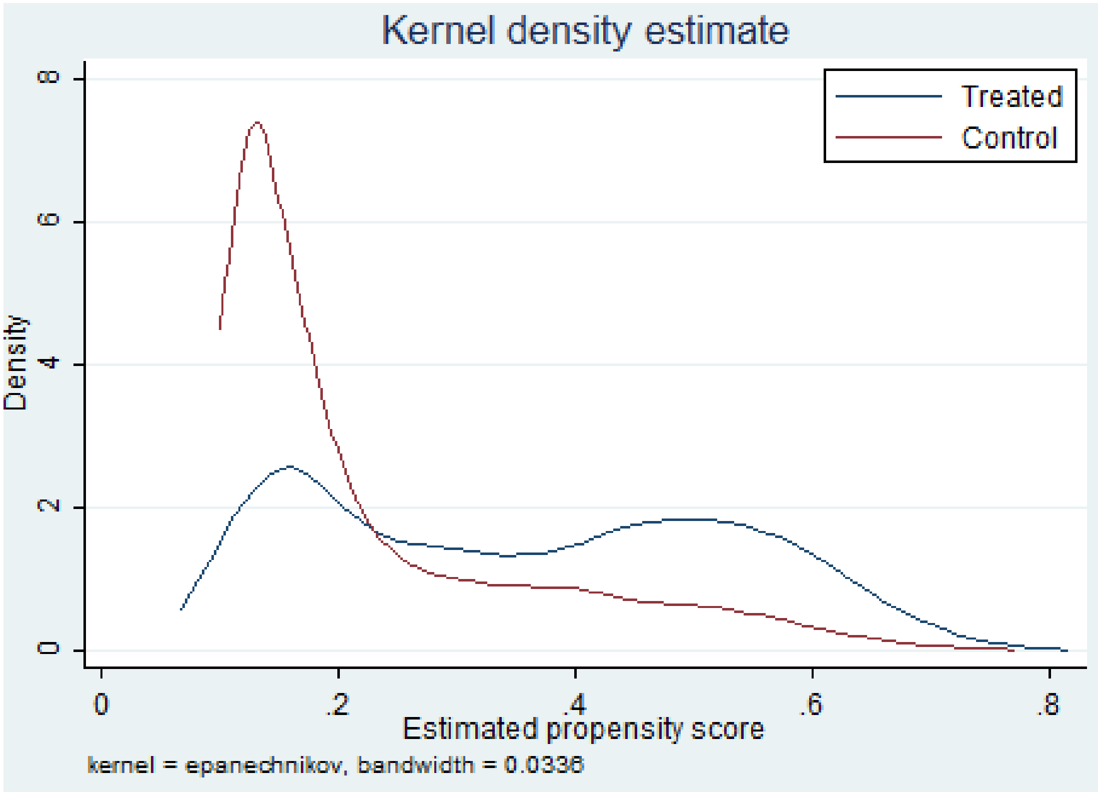

Figure 3: Kernel Density Graph of the Propensity Score after Trimming the Common Support Area

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Appendix A: Details of the Computation of the Proxy Means Tested Score

Data from several waves of the Susenas survey was combined for several years prior to the implementation of the program and a binary dependent variable model (logit) was implemented on household expenditure in order to determine the best predictor variables for each district (Cameron and Shah, 2014). Weights at the district level were obtained from such estimations. Consequently, a survey was designed to collect data on those variables.

For targeting purposes, community leaders recommended to the BPS a list of poor households. The government sent enumerators to survey the households using the PSE05. The answers to the survey were converted the reclassification in **Table A.1**.

I used the original weights computed by the BPS, to compute the simulated proxy means tested score in my analysis. Let $\mathrm{X_{ij}}$ denote one of the $\mathrm{i^{th}}$ indicator variable for household j, let $\mathrm{W_{ik}}$ denote the weight calculated for the $\mathrm{i^{th}}$ variable in district k. The computation of the proxy-means-tested score in calculated in the following way:

$$Proxy - means\ tested\ score = 100 * \sum_{\mathrm{i=1}}^{18} \mathrm{W_{ik}} * \mathrm{X_{ij}}$$

Table A.1: Proxy Means Tested Scoring System Used for Identification of Recipients of Government-Sponsored Poverty Allocation Programs:

Variable	Scoring System	
	Poor	**Non-Poor**
House Floor Area	1 if the floor area < 15 meters squared	0 if the floor area > 15 meters squared
House Floor Type	1 if the floor type is made of soil	0 if the floor type is a non-soil material
House Wall Type	1 if the wall is made of bamboo	0 if the wall is made of concrete or wood
Household Toilet Facility	1 if the toilet is public or other	0 if the toilet is private
Drinking Water Source	1 if it is an unprotected spring or river	0 if it is mineral, piped or a protected spring
Source of Lighting	1 if the light source is electricity	0 if it isn't electricity
Fuel Used	1 if it is wood or charcoal	0 if it is gas or electricity
Frequency of Meat Purchased	1 if HH never buys meat or once a week	0 if it is twice a week or more
Meal Frequency	1 if the HH consumes 1 or 2 meals a day	0 if it is more than 2 meals a day
Frequency of Clothes Purchased	1 if never or once (previous year)	0 if more than once (previous year)
Accessibility to Health Center	1 if non-accessible	0 if accessible
Employment Sector of HH	1 if agriculture	0 if non-agriculture
Highest Education Level of HH	1 if junior high school or below	0 if above junior high school
Asset Possession: Savings	1 if not in possession	0 if in possession
Asset Possession: Gold	1 if not in possession	0 if in possession
Asset Possession: Television	1 if not in possession	0 if in possession
Asset Possession: Livestock	1 if not in possession	0 if in possession
Asset Possession: Vehicle	1 if not in possession	0 if in possession