ESSAYS ON CONSEQUENCES AND RESPONSES TO ECONOMIC SHOCKS

A Dissertation

Presented to the Faculty of the Graduate School

of Cornell University

in Partial Fulfillment of the Requirements for the Degree of

Doctor of Philosophy

by

Armand Arief Sim

August 2020

ESSAYS ON CONSEQUENCES AND RESPONSES TO ECONOMIC SHOCKS

Armand Arief Sim, Ph.D.

Cornell University 2020

This dissertation consists of three chapters that study the consequences of income or price shocks on important economic dimensions of villages, households, and individuals, and how they respond to these shocks.

The first chapter investigates whether local governments respond to local economic shocks. I use heterogeneity in the effects of rice import restriction on rice price shocks across Indonesian villages to investigate public resources provision responses to local price shock. To this end, I combine village agro-climatic conditions for growing rice with provincial rice price over time to construct plausibly exogenous price shocks at the village level. Using a comprehensive longitudinal dataset of 53,000 villages, I find that an increase in rice price is associated with negative income growth in villages less suited for growing rice, and local governments responded to it by increasing public resources – public health facilities and financial capital assistance – towards those villages. The effects on public health facilities (financial assistance) are only significant in high (low)-inequality villages. Increased social capital only in low-equality villages can provide a plausible explanation for the heterogeneous result on financial assistance projects. I also show that an increase in public health facilities was associated with a reduction in infant mortality suggesting evidence of good targeting by local governments.

The second chapter (co-authored with Patrick Asuming and Hyuncheol Bryant Kim) investigates the long-run effects of a health insurance subsidy in

Ghana, where mandates are not enforceable. We randomly provide different levels of subsidy (1/3, 2/3, and full), with follow-up surveys seven months and three years after the intervention. We find that a one-time subsidy promotes insurance enrollment for all treatment groups, but long-run health care service utilization increases only for the partial subsidy groups. Selection explains this pattern: those who were enrolled due to the subsidy, especially the partial subsidy, are more ill and have greater health care utilization. A careful enforcement of mandatory enrollment is necessary to prevent selection.

The third chapter (co-authored with Asep Suryahadi and Daniel Suryadarma) measures the effect of child market work on the long-term growth of human capital, focusing on the output of the human capital production: mathematics skills, cognitive skills, pulmonary function, and educational attainment. Our full sample is drawn from a rich longitudinal dataset Indonesia Family Life Survey (IFLS). We address endogeneity of child market work using provincial legislated minimum wage as the instrument. Our instrumental variable estimation shows that child labor negatively affects mathematics skills and pulmonary function, but not cognitive skills and educational attainment. We find heterogeneities in type of work. Those who work outside of family business have lower educational attainment than those working for family business.

BIOGRAPHICAL SKETCH

Armand Sim is a native of Jakarta, Indonesia. He joined the doctoral program in Dyson School of Applied Economics and Management at Cornell University in 2014. Prior to pursuing his doctorate, Mr. Sim worked on various issues in microeconomic development as a researcher at the SMERU Research Institute in Jakarta, Indonesia. Previously he worked as a research assistant at the Vice President Office of the Republic of Indonesia. He holds a Bachelor of Arts in economics from University of Indonesia and Master of Arts in economics from Vanderbilt University, where he received Fulbright scholarship.

To the memory of my father, Sim Hon Liong, who passed away a few months before I started my PhD.

ACKNOWLEDGEMENTS

I am very grateful to my advisers, Ravi Kanbur, Nancy Chau, Hyuncheol Bryant Kim, and Doug Miller, for their guidance and support over the course of this dissertation. I am especially grateful to Bryant Kim for his constant support, advice, and challenges on my works and ideas since my second year. His support during job market season was invaluable, especially by allowing me to use his office during his leave at Asian Development Bank. Otherwise, I would not have been able to finish my job market paper on time. I also thank the Indonesia Endowment Fund for Education (LPDP-RI) for their financial support.

My experience in the program would not have been the same without my peers and friends. I am especially grateful to Abhishek Ananth, Esteban Mendez Chacon, Kyun Suk Chang, Jee-Hun Choi, Yu She, Xiaolu Wang, and Nahim Zahur for their friendship and generosity. I would miss our Friday dinners.

I thank my late father, Sim Hon Liong, and mother, Jauw Njun Jin, for their support and love as well as their appreciation towards education. Also thank to my brother, Ardy Nugraha, for his selfless care for our mother during my study. Finally, I thank my wife, Sekar Talenta, for her love and selflessness to support me and postpone her career pursuit; and my daughter, Alma Aurelius Sim, whose presence always gives me joy even during the exhausting first few months of her arrival.

TABLE OF CONTENTS

LIST OF TABLES

LIST OF FIGURES

CHAPTER 1

IMPORT RESTRICTION, PRICE SHOCK, AND LOCAL POLICY RESPONSES: EVIDENCE FROM INDONESIA

1.1 Introduction

Large-scale trade liberalization has been attributed to much of the massive eradication of poverty in developing countries, especially in India and China (Goldberg and Pavcnik, 2007). Barriers to trade, on the other hand, impose negative effects on economy and efficiency (Ethier, 1982; Melitz, 2003; Amodio et al., 2020). Recent studies, however, show that trade policy in any form – trade liberalization or restriction – has adverse consequences along several dimensions, such as poverty, health, and educational outcomes, that are unevenly distributed within country depending on the intensity of regional exposure to the policy (Edmonds et al., 2010; Topalova, 2010; Kis-Katos and Sparrow, 2015; Dix-Carneiro and Kovak, 2017; Anukriti and Kumler, 2019; Amodio et al., 2020).[1] Central governments have launched various programs to help adversely affected individuals and households through social transfers (Autor et al., 2013) or vocational training (McKenzie, 2017). However, programs that directly address concerns on communities, such as investment in social infrastructure and public goods, are relatively rare (Pavcnik, 2017).[2] This is unfortunate given that

[1]See Goldberg and Pavcnik (2016) for survey literature on the impacts of international trade policy and Goldberg and Pavcnik (2007) for impacts on poverty and inequality.

[2]One possible explanation is because local governments' policy responses are constrained by decreased local tax revenues in times of negative economic shock. Lack of responses could amplify adverse consequences of negative income shock. For example, (Feler and Senses, 2017) find that localities in the US more heavily hit by import competition from China provided less

public investment can play an important role in mitigating risk in communities, especially in developing countries, where levels of social safety are low and insurance markets are imperfect.

I offer empirical evidence in this issue by investigating whether local governments in Indonesia respond to trade-induced local price shocks by providing larger public resources – health facilities and small-scale development projects (e.g., financial capital assistance) – to more adversely affected villages.[3] I focus on health facilities because they provide key public services in Indonesian villages, but their quality and availability varies across regions (Booth et al., 2019). Unequal access to public health facility may exacerbate inequality in health outcomes especially in the poor and remote regions. This could translate to higher income inequality since health is an important determinant for individual earnings (Strauss and Thomas, 1998). I also study the effects on development projects, especially projects that provided financial capital assistance, because they are important in mitigating income risk in villages. Finally, I focus on these particular public resources because they are mostly aimed to be distributed and implemented at the village level by the local governments.

I investigate the effects of trade-induced local price shocks by studying the relationship between rice import restriction and domestic rice price movement in Indonesia.[4] In 2004, the Indonesian government banned rice import with the

public goods, such as policing, due to a decline in property and sales tax revenues. Reduction in policing in turn leads to increased property crime amplifying the economic costs of trade-induced income shock.

[3]Public health facilities are mainly determined by district governments, while small-scale development projects by village governments.

[4]Using product price movement to study the potential effects of trade policy has been used extensively in the literature (e.g., see Leamer and Levinsohn, 1995 for review and Edmonds,

purported goal of protecting farmers by increasing return to farming.[5] The policy contributed to a significant increase and considerable variation in domestic price of rice exceeding government's initial expectation.[6] The resulting increase in price of rice had important welfare implication. For example, existing studies attribute increased poverty rate to increased price of rice driven by import restriction (McCulloch, 2008; Warr and Yusuf, 2014).

Studying rice import restriction provides an ideal context for two main reasons. First, it generates price and income shocks to a large proportion of Indonesian population because rice is a staple food. Second, the policy environment in this setting is ideal because it was implemented during the decentralization era. In this era district governments rely heavily on transfer grants from the central government in the forms of General Allocation Fund (*Dana Alokasi Umum, DAU*) and Shared Natural Resource Revenue (*Dana Bagi Hasil Sumber Daya Alam, DBH SDA*) especially from oil and gas production. Income shocks resulting from rice import restriction did not have significant impacts on district revenue or the economy in general (Warr and Yusuf, 2014), which allows me to examine local governments responses to the consequences of price shocks at village level. Additionally, decentralization alters mechanisms and allocation decision of public goods provision at the village level. Once centralized, district

2007 for impacts of trade liberalization in rice on child labor in Vietnam).

[5]The Indonesian government has always protected rice sector regardless of the state of Indonesian economy. Despite many failures, self-sufficiency in rice has always been a main policy objective in agricultural sector up to the point of being considered as a policy emotionally driven by a sense of nationalism (Fane and Warr, 2008).

[6]Rice price change after the import ban in 2004 cannot be fully accounted by tariffs and transportation cost alone indicating a significant role of non-tariff barriers, i.e., import ban (Patunru, 2018). Some studies estimate that import restriction contributed to rice price change as much as 37 % in 2006 (Fane and Warr, 2008) and 64 % in 2015 (Marks, 2017).

governments now have the authorities to allocate public goods to villages. Village governments, once suppressed, have more freedom to express their political aspiration and request for more public goods. Overall, rice import restriction can have important implications on changes in provision of public resources at the village level.

To establish causal relationship between price shocks and public resources, I use two plausibly exogenous variations to construct exogenous price shocks at the village level. First, I use considerable increase and variation in rice price across provinces and years. I assume that villages within the same province are exposed to the same price. Using this variation alone is not enough because the effect of rice price change on income is theoretically ambiguous. For example, rice price hike is likely to benefit net sellers but harm net buyers (Deaton, 1997). To help address this problem I use exogenous variation in village agro-climatic condition for growing rice or rice suitability which could approximate rice elasticity of supply and predict the proportion of net-sellers farmers in a village. I interact rice suitability with rice price variation to construct the key independent variable providing differential effects of rice price change on village outcomes that arise from comparing high with low suitability villages.[7]

To implement my empirical design, I assemble a comprehensive longitudinal village-level dataset covering more than 53,000 villages spanning from 2000 to 2014 drawing on a variety of sources. I use six waves of village census *Podes* (2000, 2003, 2005, 2008, and 2014) to obtain information on public goods and development projects. Time-invariant village-level rice suitability data that mea-

[7]Throughout this paper, villages in the 10^{th} percentile of rice suitability distribution are considered low-suitability villages, while those in the 90^{th} percentile are considered high-suitability villages.

sures potential rice yields comes from FAO-GAEZ project. Monthly domestic price of rice across provinces from 2000 to 2014 comes from the Central Bureau of Statistics (BPS). In addition, I use complete records of various census data (i.e., the 2003 and 2013 Agricultural Census as well as the 2000 and 2010 Population Census) to construct other village characteristics, such as land ownership, proportion of net seller rice farmers, and ethnic diversity. Finally, to complement analysis at the less aggregate level, I use nationally representative household survey, the National Socioeconomic Survey (*Susenas*). Overall, the longitudinal nature of the dataset allows me to associate changes in public goods and projects with differential price shocks induced by changes in price of rice and variation in rice suitability.

The main results show evidence that local governments responded to adverse local price shocks by increasing public resources. District governments were more likely to distribute public health facilities – but not health personnels – to villages less suited for growing rice. The effects are economically meaningful. Low suitability villages were 4 % and 5.4 % more likely to receive the overall and support health facilities than high-suitability villages. Intensive margin analysis suggests that the effects do not extend to villages that already had one. I interpret these results as an attempt to evenly distribute health facilities among adversely affected villages.

The second set of results suggest that adversely affected villages empowered themselves by launching more development projects. Less-suited village governments were 14 % more likely to launch projects that provided financial capital assistance.[8] I also find significant increases on intensive margin: 8.6 %, 9.1%,

[8]Unlike information on public goods, information on development projects was only available starting in the 2008 *Podes*. Thus, the study period here refers to 2008-2014.

and 17 % for all types of project, infrastructure, and capital assistance project, respectively. The significant intensive margin results on development projects are in contrast to the null results on public goods. This difference is probably due to villages having more authority in the provision of development projects. Villages traditionally decide projects implementation through some forms of democratic system, whereas districts hold the final decision on public goods provision despite request or pressure from villages.

A large literature has documented the role of land or wealth inequality as an important determinant of distribution in public goods and local projects. High land inequality can contribute to higher level of public goods received from higher-tier governments (Banerjee and Somanathan, 2007; Dell, 2010), but it can also lead to lower local public goods and projects (e.g., Alesina and La Ferrara, 2000). It is likely that my main results are also masked by heterogeneity in wealth inequality given large variation of landholding across villages – Gini coefficient of 0.54 with standard deviation of 0.18 in 2003.

My results align with the mixed evidence in the literature. The effects on the probability of receiving support health facilities are significant only among high-inequality villages. On the other hand, the effects on capital assistance projects (at extensive and intensive margins) are only significant among low-inequality villages. As suggested by the literature, these different responses may be attributed to the political economy dynamics attributed with different level of inequality. Large landowners in high-inequality villages may use their political influence to lobby for public goods from higher tier-governments, and this is more relevant during the decentralization era. Collective action, which has been documented to be higher in low-inequality villages (e.g., Bardhan, 2000; Dayton-Johnson, 2000; Khwaja, 2004), may contribute to development projects outcomes since villages have more control over development projects than over public goods (Cruz et al., 2020).

To test this hypothesis, I examine whether social capital – a variety of collective action measure – mediates the heterogeneous effects of inequality level on development projects. Price or income shock has been documented to play an important role in shaping social capital in both developing (e.g., Cassar et al., 2017) and developed countries (e.g., Whitt and Wilson, 2007). An increase in social capital has also been documented to strengthen social cohesion and cooperation that can lead to higher provision of public goods and development projects (Tavits, 2006; Khwaja, 2009; Casey et al., 2012; Cameron et al., 2019). The result of mediation analysis lends support to my hypothesis: Individuals in adversely affected villages express higher social capital, especially trust level, and the effects are significant only in low-inequality villages.

There are multiple plausible mechanisms to explain why rice price hike led to larger increase in public services in low suitability villages. I provide suggestive evidence for one specific mechanism, changes in local income. I find that an increase in rice price is associated with lower aggregate income for villages less suited for growing rice, as indicated by decreased coverage and intensity of nighttime lights and higher number of people eligible for health insurance program for the poor. The evidence is consistent at the household level. Households in low-suitability villages had lower nutrient intake food consumption per capita, as measured by calories and protein, than those in high-suitability villages.

Finally, having shown that health facilities were distributed more heavily towards more negatively affected villages, I investigate whether those villages actually benefited from it. Using complete record of the 2010 population census, I focus on two health outcomes: infant (IMR) and maternal mortality rates (MMR). I show that price shock is associated with increased presence of both IMR and MMR by 5.2 % and 15.5 %, respectively. The presence of public health facilities only mitigated the presence of IMR, but not MMR. Overall, I interpret these results as a suggestive evidence of district governments targeting villages effectively.

This paper connects to several strands of literature. First, this paper contributes to the literature studying policies or compensation schemes in response to adverse effects of international trade or globalization. Most studies in the literature evaluate the effects of policies for individuals or households, such as social transfers (e.g., Autor et al., 2013) and active labor market policies (Crépon and Van Den Berg, 2016; McKenzie, 2017). I contribute to this literature by providing the first evidence on policy responses and the effects of those policies at village level in a developing country context. Understanding policy responses is crucial because adverse effects on communities can be amplified in the setting with low investment in social infrastructure or public goods provision.

Second, this paper contributes to the literature studying the role of wealth or land inequality in provision of public goods or local development projects (e.g., Galasso and Ravallion, 2005; Banerjee and Somanathan, 2007; Banerjee et al., 2007; Araujo et al., 2008). Much of the studies in the literature take land or wealth inequality as relatively constant due to absence of exogenous shocks. In this paper, I find that exogenous change in relative values of land due to rice price shocks can amplify the effect of inequality in provision of public goods and local projects.

Finally, this study is also related to the literature on decentralization, targeting performance, and public goods provision by local governments (Bardhan, 2002; Besley and Coate, 2003; Gadenne and Singhal, 2014). Decentralization is considered relatively better than centralized system in terms of accountability and knowledge on the local communities. These factors are likely to improve targeting performance either in government programs or public goods provision. I complement this literature by showing that price and income shocks may contribute to local governments' ability to identify problem and effectively distribute public goods.

The remainder of this paper is organized as follows. Section 1.2 discusses the context: rice import restriction and village institutional settings. Section 1.3 discusses data and measurement. Section 1.4 discusses estimation framework. Section 1.5 discusses main results. Section 1.6 discusses the mechanisms. Section 1.7 discusses the mitigating effects of public health facilities on mortality rates. Section 1.8 concludes.

1.2 Context

1.2.1 Rice Import Restrictions

Rice is the most important agricultural commodity in terms of its proportion to expenditure, income, and employment. First, rice is the staple food for the majority of Indonesian population, and it constitutes more than 20% of the food expenditure of the poorest 40% of the population (McCulloch, 2008).[9] Second, rice is an important source of income and employment among farmers. The 2003 agricultural census reveals that 55% of agricultural households are rice farmers, more than any other commodities. Out of those rice growing households, more than 70% are net producers (McCulloch, 2008).[10]

Given the importance of rice, the government has long been concerned with policies to increase domestic production and limit its dependency on international market through various rice intensification programs (e.g., mass guidance program or *Bimas*) or other protection measures like tariff or non-tariff measures (Timmer, 2005). Despite those policies, Indonesia has regularly been a net importer, as seen in Figure A.1.[11]

Before the 1997/1998 economic crisis, the state logistic agency, *Bulog* (Badan Urusan Logistik), was the sole importer.[12] Following financial agreement with the IMF in 1998, the government was forced to abolish *Bulog*'s monopoly role and allow private sectors to participate in rice import business. However, the policy only lasted for less than two years. The growing influence of pro-farmers groups purportedly pressured the government to implement a series of rice import restriction policies aiming to protect farmers.

---

[9] As in many developing countries, food constitutes a large share of total expenditure in Indonesia: well above 60% for more than half of the Indonesian population (McCulloch, 2008)

[10] The proportion of farmers in urban areas is nonzero. The 2004 National Socioeconomic Survey (*Susenas*) records that 16 % of urban population work as farmers, where 50 % of them are rice farmers. The proportion of the urban poor that work as farmers is higher, 37 %, where 50 % of them are rice farmers.

[11] For a comprehensive overview on historical Indonesian rice cultivation and related policies, see Mears (1984) and Simatupang and Peter Timmer (2008).

[12] In addition to rice, *Bulog* controlled other commodities, such as sugar, maize, and soybeans.

In 1999, the government introduced a 20% tariff for imported rice, which was sharply raised to approximately 75% in 2003 (Warr, 2005; Fane and Warr, 2008). In 2004, importing rice was effectively banned. While private sectors were completely prohibited to import rice, *Bulog* could import limited quantities of rice only during certain periods with the goal to secure rice supply (Warr, 2005, 2011). The ban was originally intended to be a seasonal policy to protect rice farmers,[13] but the policy had been repeatedly extended and had not been completely revoked (Warr and Yusuf, 2014).[14] Figure A.1 shows that Indonesia's net rice import fell sharply following the ban, but it appears that domestic production increased in response to higher domestic price. Specifically, between 2004 to 2014 domestic production and yield increased by about 31 % and 13 %, respectively.

Indonesia had rarely imported rice exceeding 5% of total national consumption, but the import managed to stabilize domestic price (Dawe, 2008). It is, thus, unsurprising that the domestic rice price increased significantly following the ban. Figure 1.1 shows that domestic price started to climb in 2005 as the stocks of rice from previous year were starting to thin.[15] Between 2000 and 2014, domestic rice price increased annually by 0.1 log points, as shown in Panel B of Table 1.1. Some estimates suggest that import ban contributed to the price hike by 37% and 64% in 2006 and 2015, respectively (Fane and Warr, 2008; Marks, 2017).[16]

[13]The Ministry of Trade and Industry regulation No.9/MPP/Kep/1/2004 stipulates that rice import was prohibited one month prior to, during, and two months after the harvest season.

[14]Instead of lifting the ban, the government has been imposing import quotas which vary over time depending on, for example, domestic rice supply and demand.

[15]Due to heavy reliance on domestic supply, the increasing trend in domestic price appears unaffected by the brief period of sharp increase of the global rice price between 2008-2011.

[16]The estimates in both studies are measured in terms of nominal rate of protection (NRP),

In addition, import ban also imposed substantial price variation across provinces, as shown in Figure 1.4. There was relatively negligible variation in domestic rice price pre-ban in contrast to that of the post-ban, which indicates a lack of arbitrage by domestic traders in the post-ban era.[17] There are three plausible explanations. First, a weaker role of the state logistic agency, *Bulog*, in stabilizing domestic price. Second, a disruption to the thriving relationship between the private and international traders during the more liberal trade regime prior to 2004 (Bazzi, 2017).[18] Third, the overall low elasticity of supply (0.2-0.4); it varies across regions depending on soil characteristics and land types. As a comparison, the elasticity of demand for Thailand's rice exports varies between -2.5 to -5 (Warr, 2005).

---

which measures the effect of the government trade policy at any given nominal exchange rate compared to a situation in absence of said policy. See Fane and Warr (2008) and Marks (2017) for details.

[17]The price variation post-ban is persistent and follows random walk as suggested by test results that fail to reject the null hypothesis of unit root (p-value > 0.85 for all cities). This alleviates some concerns that the significant effects of price are false positive.The results come from the two unit root tests. First, the augmented Dickey-Fuller test. Second, acknowledging the fact that rice prices across cities are less likely to be independent, I apply heterogeneous panel unit root tests suggested by Im et al. (2003).

[18]Bazzi (2017) demonstrates that areas closer to domestic ports and shipping distance to Bangkok and Ho Chi Minh experienced higher increases in price suggesting high dependency to import rice.

1.2.2 Institutional Setting

Decentralization

The political context during the period of this study corresponds to the decentralization reform period that started in 1999. Following the fall of the President Soeharto's authoritarian regime (1967-1998) — known as the New Order —, there was a massive urge to decentralize responsibilities to local government. Fiscal decentralization to provincial and district governments increased the share of total expenditures managed by subnational governments to 36 % in 2011, a 50 % increase from the mid-1990s (World Bank, 2003). Decentralization of political power allows districts and villages to merge or proliferate to form a new district or village. This resulted in a significant increase in the number of the new governments at all level.[19] By 2014, Indonesia is divided into 34 provinces and 511 districts. Each district is divided into subdistricts that are further divided into villages. There are two types of villages based on observable characteristics: *desa*, which is more rural, and *kelurahan*, which is more urban.[20] Following the decentralization reform in 1999, the number of villages increased significantly from more than 66,000 in 2000 to more than 80,000 in 2014, where

[19] See Fitrani et al. (2005) for more details on decentralization.

[20] While the head of *desa* is decided through local election, the head of *kelurahan* is directly appointed by district mayor. The categorization of villages into *desa* and *kelurahan* were initiated after the passage of the Village Law No.5 of 1979. The law stipulates that all villages were *desa*, and some of them were categorized as *kelurahan* by the central government. The conversion of *desa* into *kelurahan* stopped in 1992 mainly due to financial reasons (Niessen, 1999). See Martinez-Bravo (2014) for more detailed explanation on the historical formation and differences between *desa* and *kelurahan*.

well above 85% of them are *desa*.[21]

Decentralization has significant impacts on villages because it alters the decision process on the allocation of public goods and development projects as well as the financial resources for villages.

Public Goods The decision-making process for the provision of public goods in Indonesian villages has evolved since the fall of the New Order era. Prior to decentralization, the main source of funding for village public goods came from the national budget and was highly centralized. Virtually every important decision regarding village budget and public goods provision required approval from the district mayors (Antlöv, 2003; Tajima et al., 2018). In addition to budget allocation from the central government, villages could submit a proposal to the National Development Planning Process (*P5D*) for public goods provision. However, village officials were mainly unaware of this mechanism hampering them to submit high-quality proposals that led to undesirable outcomes for their village (Evers, 2000).

Decentralization changed the process for public goods provision in villages, especially concerning the roles of district and village governments. While district governments remains central in funding and allocating public goods, village governments also play an important role in initiating and leading maintenance of the public goods, especially infrastructure, such as roads and bridges (World Bank, 2010). Overall, to some degree, villages still relied on higher-tier governments for public goods provision both before and after decentralization.

[21] Decentralization reform, which marked the end of Soeharto government in 1998, provided massive far-reaching autonomy to local governments, including fiscal responsibility and splitting or forming new local government.

However, this does not rule out the influence of village governments in improving the level of public goods provision. This has been documented even before decentralization.

Despite top-down and centralized approach in the New Order era, recent studies find that villages could influence public goods provision through the roles of educated heads (Martinez-Bravo, 2017) and inter-village competition indirectly induced by the level of ethnic segregation (Tajima et al., 2018). The influence of villages is theoretically larger after decentralization due to more freedom in the expression of political aspiration and collective actions. This has been documented in an extensive longitudinal local level institutions study covering 40 villages over more than a decade (Wetterberg et al., 2014).[22] The study finds that, among other factors, income shocks, shifts in sources of income, and distribution of power and assets within a village contribute to the collective capacity of villagers that can affect level and choice of public goods and development projects.

Public Healthcare Public health system providing basic primary health care consists of hospitals, clinics, and smaller facilities. The main health centers or clinics, *Puskesmas*, are staffed by at least one physician and roughly five nurses providing primary care. The smaller supporting facilities, *Pustu*, are staffed by one to three nurses and received monthly visit from a physician. *Pustu* helps

[22]Local-level Institution study is a study on the relationship between local institutions, poverty, and village governance in rural areas in Indonesia combining descriptive and quantitative methods. The study was conducted in 3 provinces, 6 districts, 40 villages, and 1,200 households. It has been conducted three times: 1996/1997, 2000/2001, and 2012.

provide basic services to villages or areas that are out of reach by *Puskesmas*.[23]

Development Projects The main goals of development projects are to empower village communities in the forms of infrastructure, capital assistance, and employment-related assistance projects. There are several mechanisms for a village to launch projects. First, villages obtain financial resources from central government programs, such as the National Community Empowerment Program (*PNPM*, formerly known as the *Kecamatan (subdistrict) Development Program or KDP*). Every year, each village within a participating subdistrict writes a proposal for small-scale projects in, for instance, infrastructure and capital assistance (Olken, 2007). Each proposal is ranked in an intervillage forum within subdistrict according to predetermined criteria such as the number of beneficiaries and project cost. All projects are funded until block grants are exhausted, with the top ranked project receives priority. Second, villages write proposals to district governments through *P5D*. Third, villages receive irregular grants from district governments or from other parties, such as NGOs. Those grants are then allocated to projects of their choice. While it is useful to be able to distinguish financial source for each project, the data unfortunately does not allow me to do it. Overall, villages have relatively more control over the allocation of development projects than that of public goods controlled by district governments.

Financial Resources Village financial sources have been evolving over time. Following the first major political reform concerning village governance, Law 5/1979 stipulated that each village received block grant from district govern-

[23]Other facilities include village health posts (poskesdes), village maternity posts (polindes), and neighborhood health posts (posyandu). These facilities are usually run by communities and volunteers and may not even have permanent locations.

ment. The massive decentralization reform provided more financial sources to villages. Law 22/1999 stipulated that each village had the autonomy to raise its own revenues in addition to receiving block grants from district government. In 2004, each village was set to receive additional grants from the central government, as mandated by Law 32/2004.

In summary, there are two broad sources of village financial resources: village own-source revenues and transfer grants (from district and central governments). Villages raise own incomes through the following sources: own-managed traditional markets, charges on small scale public transportation vehicles that pass through their jurisdictions, and other fees related to administrative services (Antlöv et al., 2016). Most of village revenue comes from transfer grants from higher-level government, especially district government. Unfortunately, there are many missing observations and inconsistent structures in data collection of village revenues information in *Podes* preventing me to conduct further analysis on types of revenues.

1.3 Data and Measurement

This section presents information on data sets and measurement, construction of the study sample, descriptive statistics, and estimation framework. I combine multiple data sets that include population and village census as well as gridded data to form the basis of the main empirical analysis. The unit of analysis is at village-year level.

1.3.1 Public Goods and Development Projects

The data on the main outcomes, public goods and development projects, come from the village census (*Podes*). This dataset has been collected roughly three times every decade starting in 1980.[24] In each wave *Podes* collects rich information on village characteristics, such as the land size, geographic location, population, existing infrastructure projects, public goods, and development projects.[25] The information comes from the official village documentation and interviews with the village head. The *Podes* sample size has increased over time following the decentralization reform that allow villages to split and merge to form a new village. The 2000 wave covers more than 66,000 villages, but it has expanded to 82,000 in 2014. For the purpose of this study, I use the 2000, 2003, 2005, 2008, 2011, and 2014 waves.

The main outcomes include sets of health public goods and development projects.[26] I focus on health public goods that are consistently collected across

[24]The latest wave was recently completed in 2018. *Podes* has three main themes which alternate every wave: agriculture, economy, and population. For example, the 2003 wave focuses on agriculture. In that year, *Podes* collects detailed information on village agriculture, such as production yields of cash crops and land plots allocated for each crop. The agriculture module was not collected in the 2006 wave, for example, because in that year *Podes* focuses on village economy and collects more detailed information on the small enterprises, for instance.

[25]There are some information that are not consistently collected every wave. For example, detailed information on village budget allocation, such as for construction or maintenance of infrastructure, and development projects were only available starting in 2008. The number of health facilities and officials were not collected in 2008.

[26]In general, development projects can include both public and excludable goods (Chavis, 2010; Araujo et al., 2008). As documented in *Podes*, the projects include public goods, such as maintenance or building road infrastructure, and excludable goods, such as capital assistance to the eligible villagers. To avoid confusion, I separate these two outcomes in the analysis even

waves. These include health care personnels (medical doctors) and health care facilities. I examine two types of facilities: the main facilities (*Puskesmas*) and the supporting facilities (*Pustu*).

The development project consists of three broad sets of outcomes. First, infrastructure project. This project includes maintenance or construction of the following public infrastructures: road, bridge, schools, sanitation, traditional market, irrigation, and other economic support facilities. Second, capital assistance project. This project aims to increase village economic capacity by providing loans for agriculture, non-agriculture, and other types of enterprises. Third, employment assistance project This project includes training program to increase production and marketing capacity as well as enhancement of civic engagement. To be consistent across waves, I restrict my analysis to projects that are not funded by *PNPM*.[27]

Flow and Stock Variables Changes in public goods and development projects are examined in its extensive and intensive margin.[28] For intensive margin analyses, I follow Cassidy (2019) by dividing the outcomes into stock and flow variables. Flow variables include variables that are probably only present for limited time, such as development project and health personnel outcomes. For example, the funds for development projects or contracts for medical doctors might not be perpetually renewed. In contrast, stock variables, such as school

though some products of the development projects are public goods.

[27]Information on development projects was only introduced in 2008.

[28]In addition to the three broad measures of development project, I also create two additional variables: 1) an indicator variable for whether a village receives any kind of project in particular year (extensive margin) and 2) a continuous variable that sums up all projects that are available in a village (intensive margin).

buildings or health care center, might remain indefinitely. Thus, by definition, both variables are constructed differently to reflect changes between periods. The outcome flow variables Y_{vpt} at village v and province p at the end of period t reflects the number of the variable at that period. On the other hand, the annual change in stock variables in period t must take into account the stock of that variable in the previous period, t_0, which is calculated as follows:

$$Y_{vpt} = \frac{1}{t_1 - t_0}\left(Y_{vpt_1} - Y_{vpt_0}\right) \tag{1.1}$$

1.3.2 Rice Price and Suitability

Rice Price The monthly domestic rice price data is collected by the central bureau of statistics (BPS) from a major representative city in each province. This practice is common in many developing countries (Deaton, 1997). Because data on commodity and food crop prices at village level is not available and observable, I assume that villages within the same province are exposed to the same price. I further assume that provincial price is exogenous to each village. This assumption is not too stringent because a village is less likely to determine the price of rice at the province level.

I use the monthly retail price data that spans from January 2000 to March 2014 to construct a key independent variable, price change.[29] The variable is defined as the annualized growth in the log rice price between *Podes* waves. For example, to examine the effects on village outcomes in 2003, the price change mea-

[29]While it is probably more ideal to use farmgate than retail price, regional farmgate price data is not available. Figure A.2 shows that it might not pose an estimation problem because the movement of retail price is highly correlated with that of farmgate price.

sures growth January 2000 to March 2003. For outcomes in 2005, price change is constructed from April 2003 to March 2005. Price changes for subsequent waves follow the same construction method.[30] Figure A.3 illustrates the distribution of annual price changes from 2000 to 2014, where the darker shade implies higher price change than the lighter shade. The annual price change does not seem to be permanently attributed to certain provinces, whether rice producing or not.

Rice Suitability Data for rice suitability, which measures potential or maximum attainable yields (ton/hectare) of rice, comes from the FAO-GAEZ project.[31] This measure is arguably exogenous as it is climate-driven productivity, not observed by the actual pattern of production. The climatic record is based on daily weather records observed in each year from 1961 to 1990, which provides good approximation for historical condition (Nunn and Qian, 2011; Costinot et al., 2016; Fiszbein, 2017). To obtain rice suitability information at the village level, I aggregate the suitability information across grids using area weights, i.e., the total area of the grid overlapping with the village, divided by the total village area. Figure 1.2 describes geographic variation of rice suitability in Indonesia, where darker shades indicate higher values. Rice suitability appears to be a good proxy for rice production (ton/hectare), at least for Indonesian villages, as illustrated by Figure 1.3.

[30] Data collection for *Podes* generally commenced in the first quarter of the year, around March or April.

[31] The FAO-GAEZ project provides worldwide grid cells information on predicted yields on various crops by combining various high-resolution geographic data with agronomic models, described in detail by Costinot et al. (2016).

1.3.3 Other Variables

To measure the differential impacts of rice price change on aggregate income, I use two proxy variables. First, I follow the standard approach in the literature by analyzing nighttime lights, which has been increasingly shown to be a reliable indicator for economic development, especially in areas with shortage of quality data (Henderson et al., 2012). I measure extensive margin — indicator for presence of lights — and intensive margin — intensity of lights — of nighttime lights. The data comes from the National Oceanic and Atmospheric Administration (NOAA) Defense Meteorological Satellite Program. The data used in this paper spans from 2000 to 2011.[32]

Second, the number of health card (*Kartu Sehat*) issued in the last year. The health card program was launched in 1998 as part of the social safety net program (*Jaringan Pengaman Sosial*) intended to protect the poor during the economic crisis. The benefits of health card beneficiaries include various free services at public health care providers, such as outpatient and inpatient care (Sparrow, 2008; Bah et al., 2018). This variable comes from *Podes*.

I use additional complete census records to construct additional village agriculture and demographic characteristics. First, the 2003 agricultural census to construct inequality in land ownership and other agricultural-related variables.[33] Second, the 2000 population census to construct ethnic diversity measures.

[32]The latest available data is 2013, but to allow comparability with the *Podes* waves, I only use the data up to 2011.

[33]The agricultural census has been conducted every decade since 1963 with the latest wave completed in 2013. The 2003 wave records landholding information on 40 million households.

1.3.4 Sample Construction and Summary Statistics

The main analysis is based on a balanced panel of 53,152 villages out of 298 districts and 26 provinces matched across the *Podes* waves. In total, the final sample has 318,912 village-year observations. To improve accuracy and quality of the data I impose some restrictions. First, I exclude Papua and West Papua provinces due to unreliable data. Second, I drop villages that amalgamated within the study period. To maintain comparability of institutions, I exclude provinces with special autonomy status which may affect provision and distribution of public goods and development projects.[34] To maintain a consistent unit of observation, village outcomes are aggregated up to the 2000 borders.

Table 1.1 displays summary statistics. An average district is divided into more than 270 villages. On average, each village has a total population of more than 3,500. The Gini coefficient of 0.54 in landholding indicates high wealth inequality within a village. The state of public health facilities and personnels is quite worrying. Only 20 % and 44 % of villages have at least one doctor and health care center. However, development projects are well distributed. Almost every village has at least one development project (84 %), where infrastructure maintenance project is the most popular (70 %).

[34]Provinces with special autonomy status include the capital of Indonesia, DKI Jakarta, Nanggroe Aceh Darussalam, and DI Yogyakarta.

1.4 Estimation Framework

My empirical strategy is similar in spirit to the standard difference-in-difference method but with continuous treatment intensity.[35] This method estimates whether changes in rice price affect provision of health public goods and development projects disproportionately in villages more suitable for rice production.

This approach requires plausibly exogenous shocks that vary across time and villages. The time variation comes from the movement in annual rice price. I exploit a sudden and major rice import restriction that contributes to substantial rice price variation across provinces, which is arguably exogenous to villages because national rice production and consumption are not driven by a small fraction of villages. The cross-sectional variation comes from geographic variation in rice suitability, which measures potential or maximum attainable rice yields, across villages. I interact both time and spatial variations as an indirect way to measure price shocks at the village level. A large increase in rice price in low (high) suitability villages is considered negative (positive) shock.

Villages with higher rice suitability are assumed to be more likely to benefit from higher rice price. However, this is not guaranteed because an increase in rice price does not necessarily translate to an increase in net income. Theoretically, it depends on whether a household is a net producer or a net consumer. Rice price hike benefits net producers but hurts net consumers (Deaton, 1989). Because variable that informs net-producer status at village level is not available, I use a proxy variable indicating whether the majority of farmers in

[35]This approach is commonly used to analyze the effects of commodity or food price shocks (Dube and Vargas, 2013; McGuirk and Burke, 2017; Sviatschi, 2018).

a village both sell and consume agricultural products.[36] The 2003 and 2005 *Podes* document that eight out of ten villages are mainly agricultural suggesting that the proxy variable is representative at the national level. More importantly, Figure A.4 shows that rice suitability is positively correlated with the majority share of farmers selling and consume their products. To see whether this pattern holds, I use complete record of the 2013 Agriculture Census and demonstrate that the relationship between rice suitability and proportion of net sellers remains positive in 2013 even after adjusting for the proportion in the same village in 2003, as illustrated in Figure A.5.[37] Overall, these show that my approach to approximate whether a village potentially benefits from rice price hike is reasonable.

Equation (1.2) presents the estimation specification.

$$Y_{vpt} = \beta_0 + \beta_1 Price_{pt} + \beta_2 Price_{pt} \times RiceSuit_{vp} + \theta X_{vpt} + \gamma_v + \delta_t + \sigma_d t + \epsilon_{vpt} \qquad (1.2)$$

where Y_{vpt} denotes public goods and development projects variables in village v, province p, and year t. $RiceSuit_{vp}$ is time-invariant measure of rice suitability, measured in thousands of tons per hectare. $Price_{pt}$ is the annualized log growth of domestic rice price in province p. $\mathbf{X}_{vpt}$ are time-varying covariates that in-

[36] This variable comes from the 2005 *Podes* and conditions on a village being an agricultural village. While the variable does not have information on specific agricultural products being sold and consumed, it is reasonable to assume that rice drives up the number given that it is the most dominant agricultural product among Indonesian farmers McCulloch (2008).

[37] A household is considered a net seller of rice if it sells some or all of the harvested rice. This variable is not available in the 2003 Agriculture Census, which explains why I use indicator from 2003 *Podes*.

clude (log) population to account for scale effects and access to public goods and projects,[38] (log) distance to district capital and (log) distance to sub-district capital to account for political influence of physical distance on local resources (Stasavage, 2010; Campante and Do, 2014; Henn, 2018).[39] I also control for the interaction between the following time-invariant variables and year fixed effects: (log) village size and (log) harvested lands for rice.[40] These covariates respectively control for changes in the pattern of usage of lands and incentives to plant rice that may affect outcomes.

Village fixed effects, γ_v, and year fixed effects, δ_t, account for time-invariant village characteristics and common nationwide shocks, respectively. District-specific time-trends, $\sigma_d t$, account for potential omitted variables at district level that may cause upward trends in the distributive policies (e.g., public goods provision), such as shifts in political preferences.[41] Robust standard errors ϵ_{vpt} are clustered at the district level to control for potential serial correlation over time and across villages within a district. This approach is somewhat stringent given that the cross-sectional variation in the key independent variable is at the village level.[42] The identifying assumption is that, after accounting for time-invariant factors at the village level and common trending factors at the district

[38]Including (log) population also indirectly controls for migration inflow that might be affected by better economic opportunities induced by increased rice price.

[39]Distances between village and district and sub-district capital vary because village and district splits over time.

[40]Total village size (in km^2) and total harvested lands for rice (in thousands of hectare) are constructed from the village map in 2000 and the FAO-GAEZ project, respectively.

[41]As a robustness check, I substitute district-specific time-trends with village-specific time trends. The main results hold.

[42]This approach, however, is useful because the decision for public goods provision varies across districts.

level, variation in rice price is not correlated with unobserved factors that also affect public goods and development projects.

The key coefficient of interest, β_2, captures differential effects of rice price change on outcomes that arise from comparing villages with varying rice suitability. In all specifications, $\beta_2 < 0$ implies that an increase in rice price leads to a larger positive change in outcomes, i.e., health public goods and development projects in villages less suitable for growing rice.[43]

1.5 Results

1.5.1 Public Goods

I start by presenting the estimated differential effects of rice price change on health public goods, i.e., the interaction term $Price \times Suitability$, in Equation 1.2. Figure 1.6 summarizes the results. While price shock can explain changes in extensive margin (Panel A), it cannot explain changes in intensive margin (Panel B). Figure 1.7 shows that the effects (extensive margin) on support health facilities become more visible – relative to that in 2000 and 2003 – as rice price started increasing sharply in 2005, illustrating the significance of import restriction.

Table 1.2 presents the regression results from a linear probability model. Panel A reports results on changes in extensive margin, while Panel B reports results on changes in intensive margin. The coefficient of -0.085 (column 4 of Panel A) on any health facility is statistically significant ($p < 0.01$) and economically

[43]Because most public goods experienced a nationwide increase during decentralization period, a negative coefficient should generally be interpreted as a smaller increase.

meaningful. To measure the magnitude of the estimated coefficient, consider, for example, the rise in health facilities associated with the rise in rice price. I compare high (90^{th} percentile) with low suitability villages (10^{th} percentile). A high-suitability village has the mean suitability of 5.93 tons per hectare, while a low suitability village has the mean suitability of 3.86 tons per hectare. During the period of the study, 2000 to 2014, yearly price of rice increased by 0.10 log points. Thus, the coefficient of -0.085 in column 4 implies that the price rise led to an increase of 1.7 percentage points in total health center, which accounts for 4 % relative to the mean.[44] The effect is larger for *Pustu*, i.e., a 1.8 percentage points or 5.4 % relative to the mean.

The effect on *Puskesmas* is not detectable and small (column 2) implying that the effect on any health facility is entirely driven by *Pustu*. This is interesting because *Pustu* is more ubiquitous than *Puskesmas*.[45] The most likely explanation is the lower cost of building *Pustu* than that of *Puskesmas*. Result on column 1 suggests that an increased presence of health center is not necessarily accompanied by an increase in the presence of doctors, which makes sense because doctors are not directly assigned to *Pustu*. An alternative explanation is that district government might have preferred more easily visible public goods (i.e., health facilities) to the less visible ones (i.e., doctors) to gain political supports, which is not uncommon in developing countries (e.g., Williams, 2017). Testing this hypothesis requires information on voting data at the village level, which is unfortunately unavailable.

[44]The magnitude is obtained by the following calculation: $0.017 = (0.1 \times -2.07 \times -0.085)$.

[45]In 2000, prior to decentralization reform, the ratio of villages to the number of facilities for *Pustu* was lower than that of *Puskesmas*, 3 vs. 8 (Tajima et al., 2018). The ubiquity of *Pustu* can be explained by the legacy of Soeharto's *Inpres* program in 1970s which focused on building and funding *Pustu* (Shah et al., 1994).

In summary, adversely affected villages were more likely to receive public goods in the forms of health facilities but not health personnels. The results of intensive margins analysis means that the effects do not apply to villages that already had a health facility, suggesting that the district government attempted to provide a more even distribution of health public goods across negatively affected villages.

1.5.2 Development Projects

I now turn to discuss the results on development projects, summarized in Figure 1.8. Panel A plots the effects on the extensive margin, while Panel B on the intensive margin. To reiterate, the intensive margin measures the number of development projects, not the amount of funding. The overall results show a somewhat different pattern than that of public goods. I find evidence on both margins. Panel A indicates that rice price increases led to higher probability of launching a capital assistance project in villages where lands are less suited for rice production. I do not find evidence on the other projects. However, Panel B shows that the coefficients on intensive margin for other projects are negative and significant, except for the employment assistance project.

Table 1.3 presents the regression results. Columns 1 to 4 report the results on extensive margin, while columns 5 to 8 on intensive margin. The coefficient of -0.357 (column 3) is both statistically and economically significant. Between 2008 and 2014, the period in which data on development projects is available, yearly rice price increased by 0.12 log points. Thus, rice price hike translates to an increase of 8.8 percentage points in likelihood of launching a capital assistance project in less suitable villages, which accounts for 14 % relative to the mean. The effects on the intensive margin are also significant: 8.6 %, 9.1 %, and 17 % for any projects (column 5), infrastructure (column 6), and capital assistance projects (column 7), respectively.

There are two key points that are worth highlighting. First, the largest impact is on the capital assistance project. This reveals that villages that did not benefit from rice price hike suffered from capital problem and preferred projects that could help relax financial constraints. Second, compared to the null effects on public goods, the significant results on the intensive margin analysis, especially that of capital assistance, are not surprising. Villages have different degree of influence over provision of public goods and development projects. While village communities can put pressure on district governments for public goods, districts hold the final decision. On the other hand, communities traditionally hold more power in projects implementation. Majority of village communities in Indonesia engage in some forms of democracy in deciding a project or policy.[46] They identify what they need and decide which projects to launch. This practice can potentially result in more projects to be implemented. Olken (2010) finds that Indonesian communities randomly assigned to a more democratic system, i.e., plebiscites, to decide development projects reportedly

[46]Based on the 1997 Indonesia Family Life Survey (IFLS), a nationally representative dataset for more than 80 % of Indonesia population, more than 70 % of villages engage in either voting or "consensus building" (*musyawarah*), by which villagers are involved in group deliberation leading to consensus. In remaining villages, a policy is decided by elites or village head (27 %). Note that the 1997 IFLS was conducted before the fall of Soeharto, which means that more villages are more likely to engage in democracy after implementation of decentralization reform. Unfortunately, the subsequent wave of IFLS conducted after 1997 does not have information on communities policy decision making process.

had higher satisfaction, more knowledge about the projects, better perception of the benefits, and higher willingness to contribute compared to communities whose projects were decided through representative-based meetings.

1.5.3 Heterogeneity Treatment Effects

Wealth Inequality

Studies have shown that landholding inequality is an important determinant for provision of public goods and local projects, but the evidence is mixed. High inequality is positively correlated with resources or public goods received from higher-tier governments due to the political connection and lobbies from large landowners (Banerjee and Somanathan, 2007; Dell, 2010). On the other hand, high inequality may contribute to reduction in collective action that could lead to lower provision of public goods and local projects (e.g., Alesina and La Ferrara, 2000).[47]

In this subsection, I explore how provision in public resources Results presented thus far help explain the main results through income effects at both village and household level. However, additional analysis is necessary to better understand potential mechanisms. I focus on wealth inequality to account for variation of landholding – a proxy for wealth – across Indonesian villages. Specifically, I examine how the main results differ by the variation in landholding inequality. I use complete records of the 2003 Agricultural Census to construct Gini coefficient to measure landholding inequality at the village level

[47] Many studies have argued the role of inequality on public goods provision or local projects in various contexts (Galasso and Ravallion, 2005; Banerjee et al., 2007; Araujo et al., 2008)

prior to import ban. The land inequality is relatively high, 0.54, as shown in Table 1.1. For easier interpretation, Gini coefficient is transformed to a binary variable taking the value of one if the value is above the median.

Public Goods Figure 1.9 plots coefficients from subsample regressions for health public goods. The corresponding regression results are presented in Table 1.6. The effects are concentrated among high-inequality villages. Column 3 shows that the effect is higher than that of the main result, -0.123 vs -0.085 (Column 3 of Table 1.2), but the difference is not statistically significant. There is no evidence that price shock has any effects on the presence of health facilities among low-inequality villages (Columns 1 to 4). Interestingly, those villages that already had at least one facility received *less* additional health facilities (Column 6). All these results are consistent with existing literature suggesting that large landowners, which are more common in more unequal communities, have relatively bigger political clout to lobby for public goods, and this is even more relevant in the context of decentralization era in Indonesia.

Development Projects Figure 1.10 plots coefficients from subsample regressions for development projects. The corresponding regression results are presented in Table 1.7. Unlike the results from public goods analysis, the effects of local price shock are concentrated among *low*-inequality villages. They were more likely to launch capital assistance projects. This finding also extends to intensive margin result, as illustrated in Panel B of Figure 1.10. The contrasting role of land inequality in public goods and development projects heterogeneity results is not surprising – it is in line with mixed evidence in the literature. More equal communities are more likely to reach decision in launching and choosing projects that are of greater benefit for them, probably because low inequality is correlated with high collective action that may have positive influence over provision of local projects (e.g., Bardhan, 2000; Dayton-Johnson, 2000; Khwaja, 2004). Further, collective action is more likely to have more influence over policy-making process that has greater relevance to communities, i.e., development projects. In this paper, villages have relatively more control over types and number of projects to launch than that of public goods, where district governments hold the final decision. To test this hypothesis, I analyze whether social capital – a variety of collective action measure – mediates the heterogeneous effects of inequality level on development projects.

Social capital A higher level of social capital – broadly defined as information, trust, and norms of reciprocity in one's social networks that enabled people to act collectively (Woolcock, 1998; Woolcock and Narayan, 2000) – has been documented to strengthen social cohesion and cooperation which could increase governments responsiveness potentially resulting in increases in projects or public goods (Tavits, 2006; Khwaja, 2009; Casey et al., 2012; Cameron et al., 2019).

For empirical analysis, I merge village-level data with *Susenas*. To obtain broader measures of social capital, I construct eight variables that appear in both the 2009 and 2012 sociocultural module of National Socioeconomic Survey (*Susenas*).[48] In particular, I construct the following variables: trust towards local village governments, trust the neighbors to watch one's house when all house-

[48]The sociocultural module is included in *Susenas* every three years. The module was introduced in 2006, but it does not include village identifier preventing me to use it in the analysis. After merging with *Podes* and excluding special status provinces, the final sample includes more than 200,000 households out of 15,000 unique villages.

hold members are away, trust the neighbors to care for one's children (aged 0-12) when adults are not home, willingness to help neighbors in need, frequency of participation in community activities (e.g., religious, sports, ROSCA, etc.), and feelings towards activities of people from different ethnicities. Some variables are measured in 1-4 scale, while others in 1-5 scale. Higher value reflects stronger support for each variable. For easier interpretation, I standardize all variables. Then I construct a mean index by taking the average out of all variables as the main measure of social capital.[49]

I begin by showing the relationship between price shock and social capital. Table 1.8 presents the results. The top row reports the effects of price shock using full sample. Individuals in villages less suited for growing rice had higher overall social capital level (column 1) that appears to be driven by trust (columns 2 and 3) and tolerance toward different ethnicity (column 8). Similar pattern has also been documented in developing and developed countries settings where individuals experience major income shocks. For example, Cassar et al. (2017) find higher level of trust after the 2004 massive tsunami among people in rural Thailand partly due to reciprocity of help from neighbors and others during the difficult situation, while Whitt and Wilson (2007) document increased group cooperation among Hurricane Katrina refugees in the US.

Next, I explore whether heterogeneous effects in land inequality on development projects differ by social capital. The result confirms my hypothesis: an increase in social capital is only detected in low-inequality villages, as illustrated in Figure 1.11. This finding is in line with a recent paper highlighting the role of social capital in the provision of sanitation facilities in Indonesian communities.

[49]I estimate equation 1.3, but instead of household covariates I include individual covariates, such as sex indicator, age, and age square.

Using a randomized experiment on community-led program intended to create demand for sanitation, Cameron et al. (2019) show that villages with higher initial social capital were more responsive to health information by building public toilets.

Price Shock Magnitude and Ethnic Diversity

Next, I turn to examine heterogeneity treatment effects in price shock magnitude and ethnic diversity for two reasons. First, district governments may react differently to severity of price shock by distributing disproportionately more resources to low suitability villages. Second, it is natural to examine the influence of ethnic diversity given the diversity of ethnicity in Indonesia and numerous studies having documented ethnic diversity as a determinant of public good provision (e.g., Alesina et al., 1999; Miguel and Gugerty, 2005; Habyarimana et al., 2007), including in Indonesia (e.g., Bandiera and Levy, 2010; Tajima et al., 2018).

The magnitude of price shock refers to the interaction term between rice price and rice suitability, $Price \times RiceSuit$. Ethnic diversity is measured by ethnolinguistic fractionalization (ELF). Information on self-reported ethnicity is obtained from complete record of the 2000 Population Census.[50] ELF reflects the probability that two randomly selected individuals from a population belong to different groups (Alesina et al., 2003). Higher value implies higher diversity.[51]

[50] In total, there are more than 1,000 self-reported ethnicities recorded in the 2000 Census.

[51] ELF is calculated as follows

$$\text{ELF}_j = 1 - \sum_{i=1}^{N} s_{ij}^2$$

where s_{ij} is the share of ethnic i(i=1 ... N) in village j.

Figures A.6 and A.7 plot coefficients from subsample regressions for health public goods and development projects. The corresponding regression results are presented in Tables A.2 and A.3. Two interesting results emerge. First, in general, the effects on health public goods do not differ by the magnitude of price shocks implying that district governments opted for more even distribution across villages. Instead, some unobserved factors, such as political preferences or lobby from large landowners, may play some role in the allocation decision. Second, the effects on development projects, especially on capital assistance projects, are concentrated among less ethnically-diverse villages. This finding can plausibly be explained by a theory proposed by Bandiera and Levy (2010).[52] Exogenous income decline and low diversity may give rise to the projects that benefit general population because low diversity among the poor make it hard for local elites to form a stable coalition with the poor that may give rise to the elite-specific projects. This theory relies on the assumption that rice price hike hits the poor more heavily in the less suitable villages such that they needed capital assistance more than the elites.

1.5.4 Robustness

Sample Selection Bias In the main sample, I exclude provinces with special autonomy status, such as Nanggroe Aceh Darussalam, Special Capital Region of Jakarta (*Daerah Khusus Ibukota Jakarta or DKI Jakarta*), and the Special Region of Yogyakarta (*Daerah Istimewa Yogyakarta or DIY*), because these provinces have

[52]Bandiera and Levy (2010) find that in the democratic Indonesian villages, the level of ethnic diversity is positively correlated with the provision of public goods closer to the preference of the wealthy elites.

special arrangement different from other provinces. One may have concerns of sample selection bias affecting the main results. To address these concerns, I include those special-status provinces. The results are presented in Tables A.2.1 and A.2.2

Alternative Specifications Next, I conduct additional robustness tests to examine whether my main results change with various alternative specifications. Specifically, I make two changes. First, I substitute district-specific trends in the main estimating Equation 1.2 with village-specific trends to address concerns of omitted variable bias at the village level may drive upward trends on the main outcomes. Results are presented in Tables A.2.3 and A.2.4. Second, in a separate regression, to address concerns on the sensitivity of results to price change definition, I construct an alternative definition, where the price change is defined as the difference between log price in $t+1$ and t. Results are presented in Tables A.2.5 and A.2.6.

Transitory Shocks Short-term transitory shocks can have short-term effects on village income which might affect the main results. To address this concern, I adjust the main estimation with rainfall shock. Following Levine and Yang (2014), I define rainfall shock as the deviation from its long-term mean, which is calculated from 1953-2014 but excludes rainfall in the given year. I focus on rainfall during wet season, which varies across provinces. The main precipitation information is obtained from Global Land Precipitation and Temperature, University of Delaware. The dataset covers monthly global temperature at 0.5 x 0.5 degree resolution or 55 km around the equator (Matsuura and Willmott, 2015). I use Version 4, which is available for 1900-2014. Results are presented in

Tables A.2.7 and A.2.8.

Differences in Baseline Local Income The effects of rice price shocks on health facilities and development projects may occur due to differences in initial local income. For example, the main results in health public goods distribution could reflect the targeting policy by district governments to low-income villages regardless of local price shocks. To test this hypothesis, I add an interaction term between baseline nighttime lights measures (coverage and intensity) and year fixed effects in the main specification.[53] Results for including interaction term of lights intensity and year fixed effects are presented in Tables A.2.9 and A.2.10. Results for including interaction term of lights coverage and year fixed effects are presented in Tables A.2.11 and A.2.12.

Price Shocks and Local Income Rice price hike may affect results through channels other than increased income related to rice-growing capacity. For example, rice price hike can affect variables that can serve as proxies for preexisting state of the local economy, such as local investment. To address this concern, I adjust main estimation by including interaction term between baseline nighttime lights intensity and year fixed effects. Tables A.2.13 and A.2.14 report the results for health public goods and development projects, respectively.

Specific to Rice Growing Areas Lastly, I include rice suitability specific time-trend to rule out alternative explanation that my main results are driven by rice-growing villages. Results for health public goods and development projects are

[53] I consider the year 2000 as the baseline. This choice is reasonable because Indonesia experienced regime change and major economic crisis in 1998.

shown in Tables A.2.15 and A.2.16, respectively.

Overall, the results from these robustness tests show that my main results and conclusion remain unchanged suggesting that local rice price shocks indeed have significant impacts on provision of public resources across villages.

1.6 Mechanisms

1.6.1 Aggregate Income

There are multiple plausible mechanisms through which price shocks can affect public resources in villages with varying rice-suitability. I provide suggestive evidence for one specific mechanism, changes in local income. Detecting changes in aggregate income in small areas, i.e., villages, without reliable income data is challenging. I follow the standard in the literature by examining two measures of nighttime lights: yearly growth in *coverage* and *intensity*. Bazzi et al. (2016) document that nighttime lights is a reliable proxy variable for income across Indonesian villages. In addition, I also examine the number of health insurance cards issued for the poor which can provide a rough indicator for poverty incidence at the village level. Higher number of health cards implies higher number of people eligible to receive social protection programs for the poor indicating higher poverty incidence.[54]

[54]This measure is by no means perfect. *Leakage* (inclusion error) and *undercoverage* (exclusion error) are common problems that affect targeting performance of social protection programs in developing countries, including health card program in Indonesia (Sparrow, 2008).

Table 1.4 presents the results. Across columns, I find positive impacts of rice price change on villages more suitable for rice production. As can be seen, the interaction coefficient on $Price \times Suitability$ is statistically significant and positive in the first two columns. The effects of rice price hike go beyond the extensive margin (column 1). It also leads to growth in the intensive margin (column 2). The coefficients of 0.476 and 0.249 in columns 1 and 2 imply that the rice price hike led to an increase of 14.8 % and 3.9 % in the coverage and intensity of nighttime lights relative to the mean.[55] These estimates reflect the differential effects on local economic growth between high and low suitability villages.

Negative coefficient on the number of health cards for the poor (column 3) indicates reduction in poverty or decreased demand for health insurance as local economy improves. Together, all these findings suggest that rice price hike increased local aggregate income for villages more suitable for rice production. The effects are monotonically increasing with rice suitability, as illustrated in Figure 1.5.

The resulting increase in local aggregate income can probably be explained by wage growth, especially in the agricultural sector. Bazzi (2017) finds that increased domestic rice price attributed to rice import restriction in Indonesia can explain positive wage growth in agricultural sector. Another study that examines the effect of major disruption in rice supply and price in a part of Java, Indonesia, also finds a faster wage growth for individuals working in the agricultural sector (Kirchberger, 2017). Even though the effects on both studies operate through different channel, their findings can shed light on why rice

[55] The numbers are obtained by the following calculation: $0.148 = (0.1 \times 0.207 \times 0.476)/0.663$; $0.039 = (0.1 \times 0.207 \times 0.249)/1.329$.

price increases local income in more suitable villages.[56]

1.6.2 Nutrient Consumption Intake

I have shown that rice price shock has significant effects on aggregate income across villages of varying rice suitability. This may help explain why less-suitable villages launched more capital assistance projects. However, it is less clear why those villages were more likely to receive public health centers. To address this concern, I analyze the effects of local price shocks on people's health. However, health outcomes are endogenous to the presence of public health centers. Thus, I examine the effects on household nutrient consumption intake, a primary input of health, in its stead. Health outcomes are highly correlated with its input, i.e., nutrition (e.g., Grossman, 1972; Strauss and Thomas, 1998), so the results of this analysis can approximate the direction – not necessarily magnitude – of the effects on health.

To conduct the analysis, I merge the main village-level dataset with the nationally representative household-level data, National Socioeconomic Survey (*Susenas*). I leverage detailed information on calorie and protein contents of more than 200 foods based on the seven-day recall period from the consumption module of *Susenas*.[57] I use all available module in the year that closely

[56] Note that the findings of both studies are not representative at the village level. Unfortunately, there is no nationally representative data that provides information on wages that can be aggregated at the village level.

[57] Until 2008, the consumption module was collected every three years, but it has since been collected annually.

corresponds to the *Podes* waves: 2002, 2005, 2008, and 2011.[58,59,60]

Because *Susenas* is a cross-sectional household survey, it does not cover all villages over time preventing me to employ within-village analysis as in the main specification. I slightly modify Equation 1.2.

$$Y_{hvpt} = \beta_0 + \beta_1 Price_{pt} + \beta_2 Price_{pt} \times RiceSuit_{vp} + \theta X_{vpt} + \theta Z_{hvpt} + \gamma_d + \delta_t + \sigma_d t + \epsilon_{vpt} \quad (1.3)$$

The household outcome variables Y_{hvpt} include the total amount of daily calories and protein consumption per capita (log) and share of food expenditure per capita (log).[61,62] In addition to village covariates X_{vpt} as in Equation 1.2, I also add a vector of household covariates Z_{hvpt} that control for factors affecting the amount and quality of household food consumption: an indicator for wife's education attainment (primary, junior and senior high school, university, and post-graduate education), wife's age and age squared, an indicator for marital status of the household head (not married, married, divorced, widowed), and indicators for the number of household members aged 0-4, 5-9, 10-14, 15-55, and above 55. I include district fixed effects γ_d instead of village fixed effects. I also control for district-specific trends $\sigma_d t$. Standard errors are clustered at the district level.

[58] I am unable to use the 2014 *Susenas* because it does not include village identifier.

[59] The final sample includes more than 300,000 households out of 25,000 unique villages.

[60] I use variables from the 2000 *Podes* to correspond with the 2002 *Susenas*.

[61] To obtain per capita measure, I adjust household size with equivalent scales as suggested by Deaton (1997). Equivalent scales dictates that household member aged 0-4 years old is equivalent to 0.4 adult, 0.5 for 5-14 years old, and 1 for above 15.

[62] Nutrition measures are constructed from the following food groups: cereals (e.g., rice), roots and tubers, fish and seafood, meat, eggs and milk, vegetables, pulses, legumes and nuts, fruits, oil/fats, sugar/honey, and others (e.g., bread).

Table 1.5 reports the results. In line with existing evidence (e.g., Stillman and Thomas, 2008), I find that households in villages more suited for growing rice consumed better nutrient content both in terms of calory and protein (columns 1 and 2).[63] There is no statistical difference in share of food expenditure per capita (column 3). Overall, these results complement the evidence on income effects of local rice price shocks at the household level.

1.7 Did Public Health Centers Mitigate Effects on Mortality?

Evidence presented thus far have shown strong negative relationship between price shocks and public health centers in less suitable villages. However, it is less clear whether these facilities help alleviate negative price effects on communities. To address this question I examine the effects on infant (IMR) and maternal mortality rate (MMR) at the village level.

I combine the main data with complete record of the 2010 Population Census that provides information on deaths in the past year (2009 and 2010).[64] To construct MMR, I follow the standard in the literature by restricting the sample for women aged 15 to 49 years old who died while pregnant, during delivery or the 2 months after birth. The sample contains more than 8,000 pregnancy-

[63] I do not find evidence that the effects on household nutrient consumption differ by the status of food acquisition, bought or self-produced, as shown in Table A.1.

[64] More importantly, the 2010 Census provides information on pregnancy-related deaths allowing me to construct maternal mortality rate (MMR). Measure on MMR is constructed from the following question: Has there been a death in this household since January 1st, 2009? If yes, and the person who died was female and over 10 years old: Did [name] die while pregnant, during delivery or the 2 months after birth?

related deaths and more than 5.8 million surviving pregnancy and delivery.[65]

Figure 1.12 plots coefficients of the effects on extensive (Panel 1.12a) and intensive margins (Panel 1.12b) by different subsamples: all villages (full sample) and villages with and without at least one public health center.[66] Results using full sample show that price shock is only associated with increased presence of IMR and MMR in low suitability villages by the magnitude of 5.2 % and 15.5 %, respectively (see Columns 1 and 2 of baseline specification of Table 1.9). These findings are in line with existing evidence in developing countries (e.g., Baird et al., 2011). Moreover, these results are supported by evidence from micro level (see Table 1.5): households in low suitability villages consumed lower calories and protein per capita. Nutrient intake is an essential input for health production function and a reliable predictor for health outcomes (Strauss and Thomas, 1998).

Subsample regressions indicate significance of public health center on alleviating some of negative consequences of price shocks. In absence of public health center, the effect of income shock on the presence of IMR is negative and significant suggesting that IMR increased in the adversely affected villages. However, the effect becomes insignificant when there is at least one public health center in a village. This pattern, however, does not extend to MMR. Overall, these results imply good targeting performance by the district government in identifying vil-

[65]Both IMR and MMR are calculated per 1,000 live births.

[66]I use price change variable measured between 2006 to 2009, which increased on average by 0.084 log points, to correspond with the first recorded birth and death in January 2009. Measures on the support and total public health centers are based on the 2008 *Podes*. Control variables include village-level covariates in the main specification obtained from the 2008 *Podes*, all regressions also include indicator for urban village, proportion of employment in agricultural sector, and proportion of high educated people (higher than primary school) from the 2010 Census.

lages that may benefit from public health centers.

1.8 Conclusion

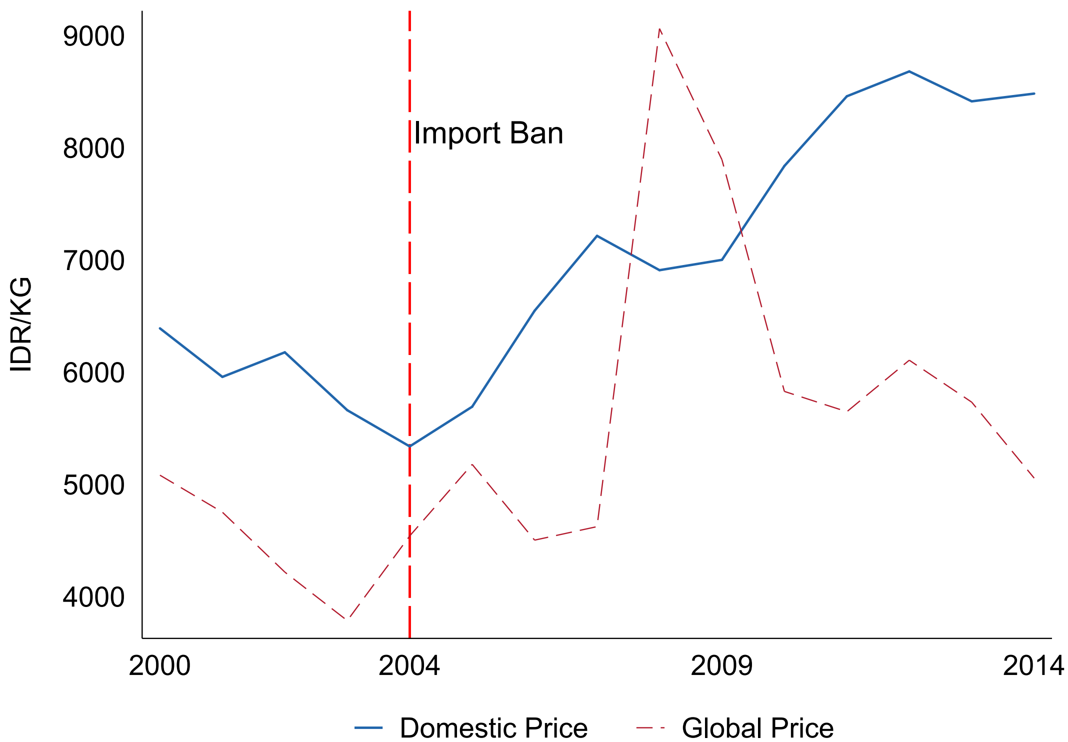
This paper provides evidence that local governments responded to local economic shocks by distributing more resources toward more adversely affected communities. Specifically, in the context of rice import restriction in Indonesia – a policy that imposed sharp increase and high variation in domestic rice prices –, villages less suited for growing rice were more likely to receive public health facilities, but only if they did not previously have one facility. Less-suited villages also launched more development projects to empower themselves, especially through capital assistance projects. I explore several plausible mechanisms to understand these outcomes: income effects at village and household level, land inequality, and social capital. I find suggestive evidence that the presence of public health facilities mitigates some of adverse effects on infant mortality suggesting effective targeting performance by district governments. While political motivation, such as collecting votes for election, has been documented to determine governments' allocation decision of public goods, my dataset prevents me to explore this channel.

I draw two important lessons for policy making process that are especially relevant for developing countries. First, local economic shocks help district governments allocate public goods provision to adversely affected communities when they have legitimate authorities. This implies that decentralization reform is somewhat crucial. Theoretically, allocation decision by district governments may be more difficult during the New Order era when virtually every decision concerning villages required approval from the central government. Second, my findings highlight the roles of social capital and wealth inequality in helping communities empower themselves in the presence of economic adversity. The design of social protection programs should enhance integration and minimize adverse effects of inequality within communities.

1.9 Figures and Tables

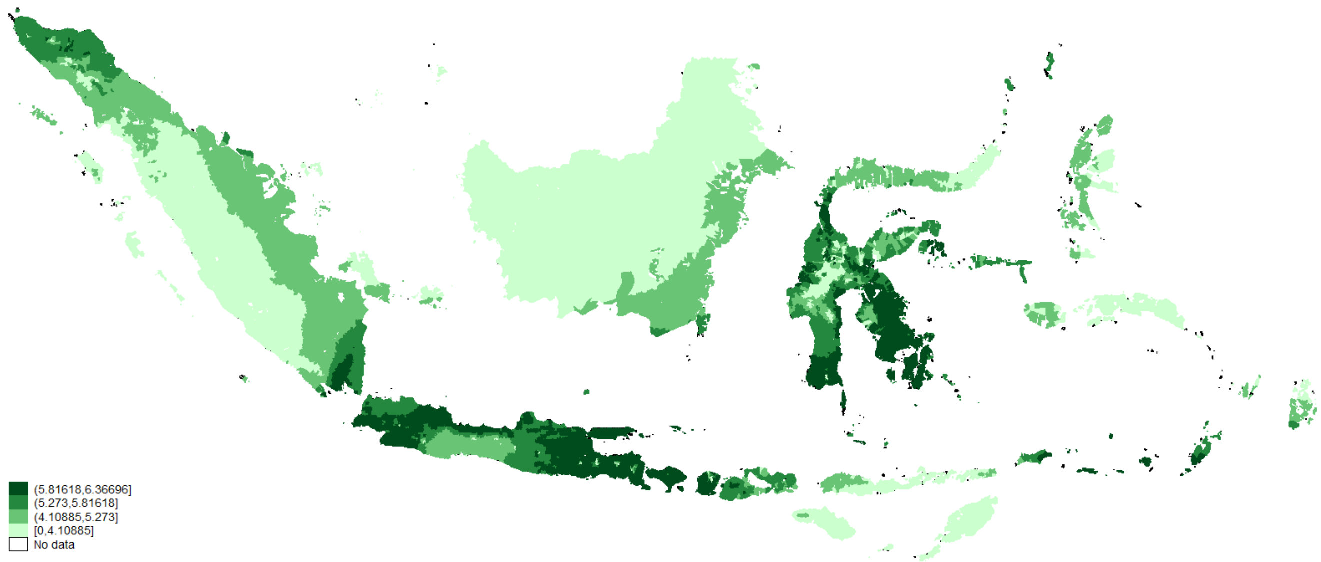
1.9.1 Figures

Figure 1.1: Domestic and Global Prices of Rice, 2000-2014



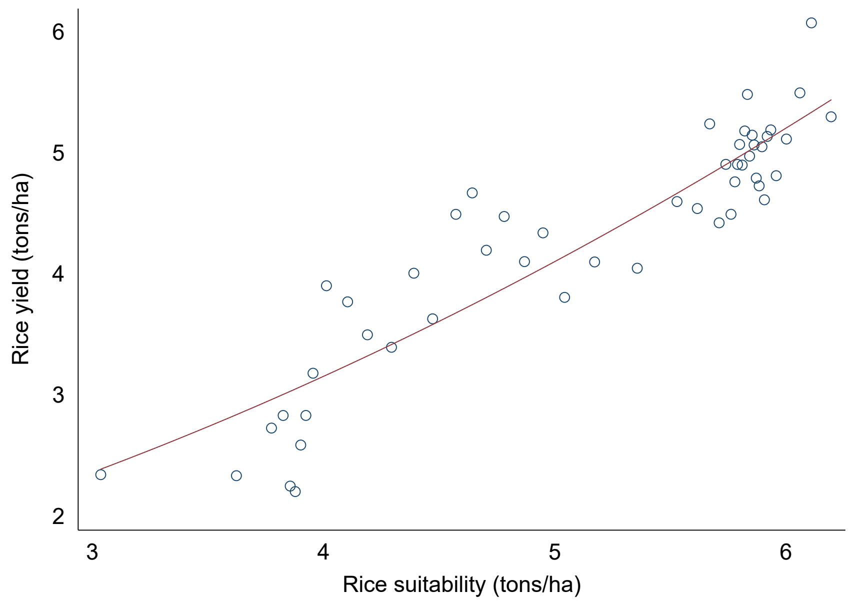
Note: This figure shows the movement of domestic and global rice price from 2000 to 2014. Nominal rice prices are deflated by the national CPI. Global price and domestic rice price are in IDR/Kilogram. Global price refers to price of Thailand milled rice in US $ converted to IDR in current prices using market exchange rate and converted to retail price by adding $20/ton for shipping and a 10 % of mark-up from wholesale to retail (Dawe, 2008). Domestic price is the average of retail prices collected from major markets. Source: Central Bureau of Statistics (BPS) obtained via CEIC database for domestic price and IMF Statistics for global price and market exchange rate (IDR/USD).

Figure 1.2: Rice Suitability Distribution.



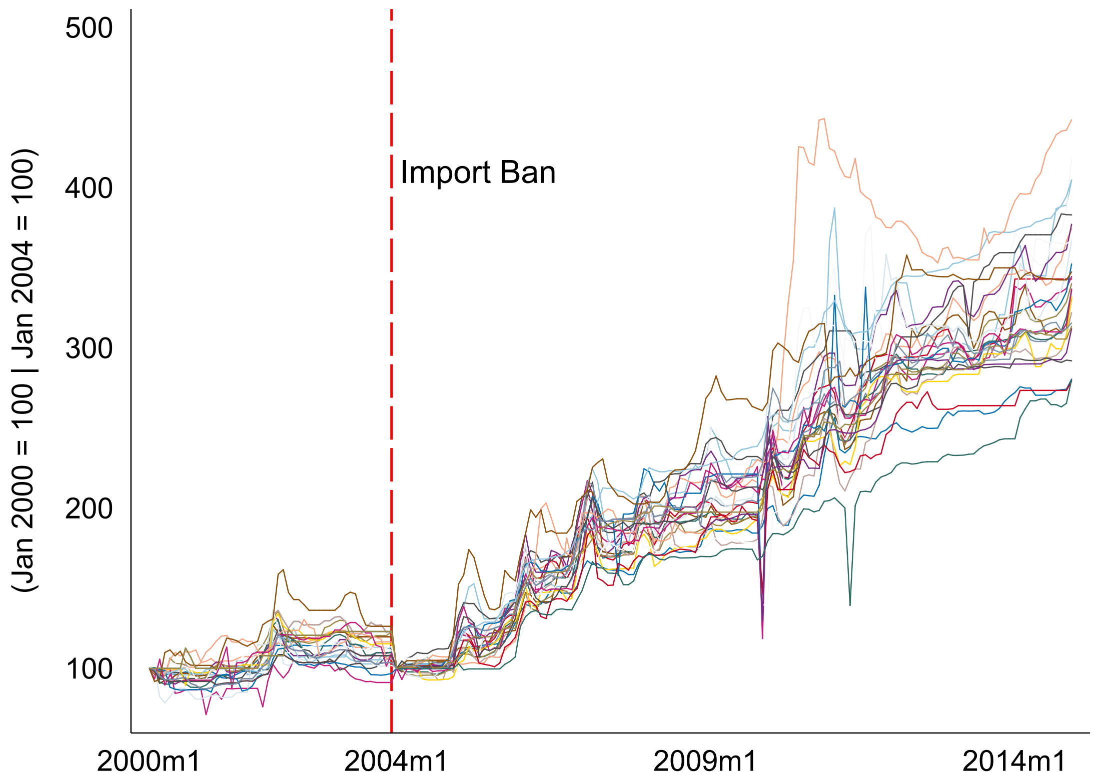
Note: This figure shows distribution of rice suitability in Indonesian villages in 2000, excluding Papua island. Rice suitability measures potential or maximum attainable yields in tons per hectare. Darker shade implies higher suitability than that of lighter shade. Source: FAO-GAEZ.

Figure 1.3: Rice Productivity and Rice Suitability



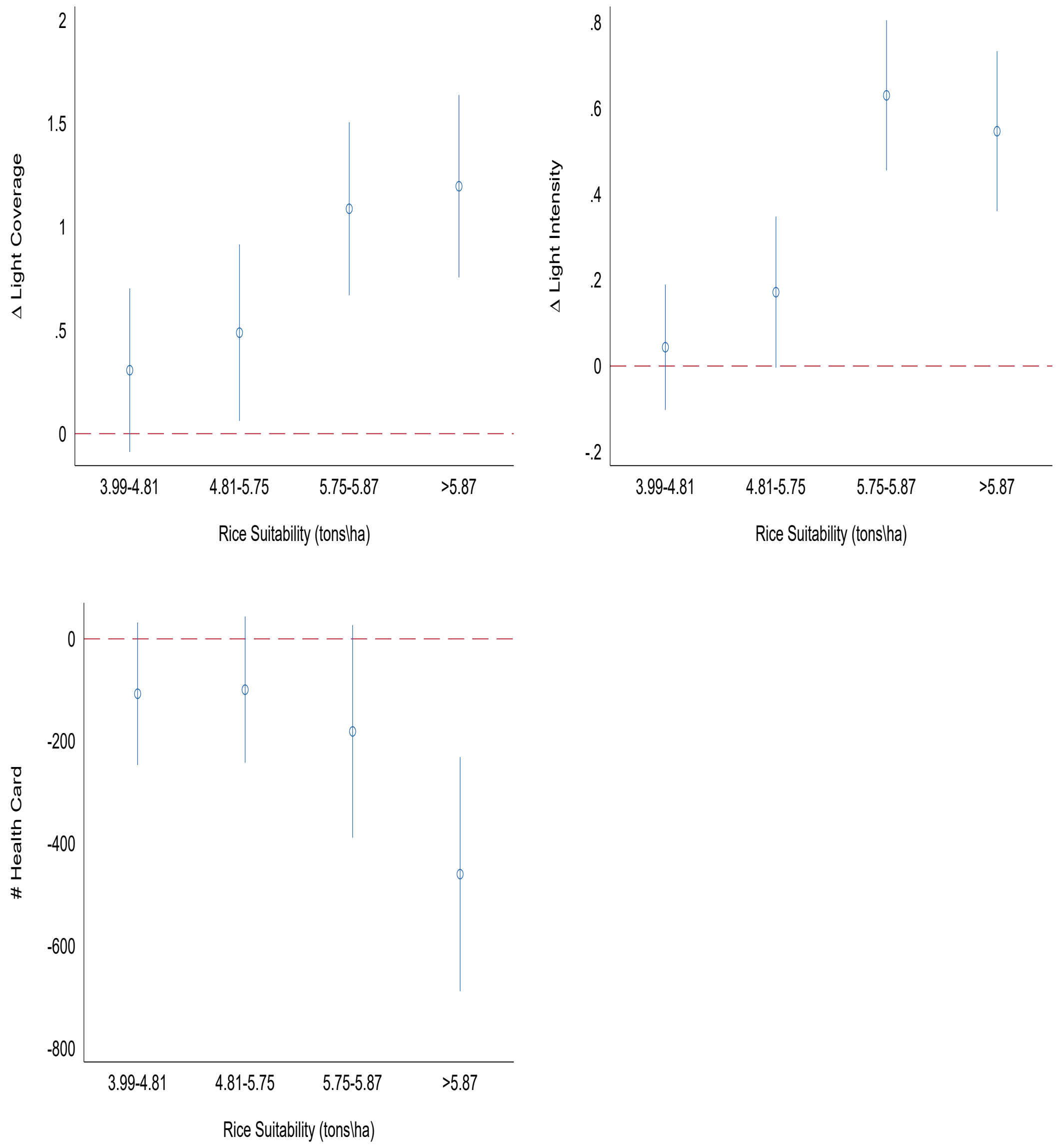
Note: This figure summarizes relationship between rice suitability and productivity, as measured by rice yields. Both variables are measured in tons per hectare. Source: rice suitabiltiy (FAO-GAEZ) and rice yields (the 2003 PODES).

Figure 1.4: Domestic Annual Rice Price Change: 2000-2014



Note: This figure shows monthly price of rice in Indonesia across cities in Indonesia, 2000-2014. Prices are normalized to 100 in January 2000 and again in January 2004 to emphasize the evolution of price before and after the import ban. Source: Central Bureau of Statistics (BPS) obtained via CEIC database.

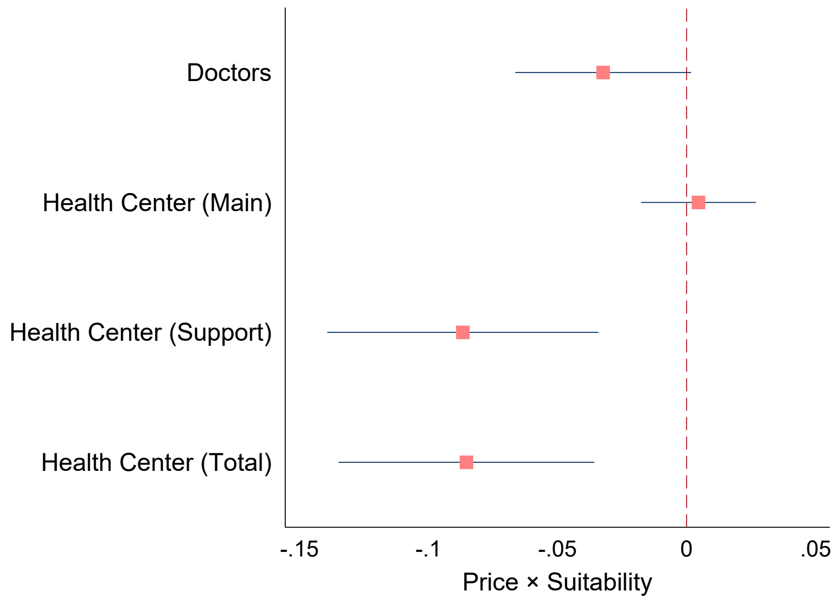
Figure 1.5: Rice Suitability and Aggregate Income Indicators



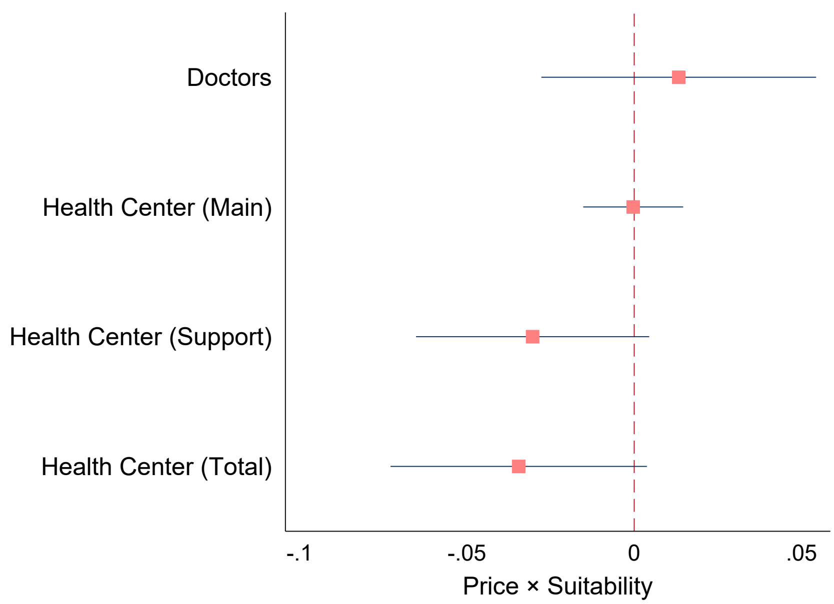
Note: These graphs figure show regression coefficients of estimating Equation (1.2) by quantiles of rice suitability (tons/ha). Standard errors are clustered at the district level with 90% confidence interval.

Figure 1.6: Effects on Public Health Facilities and Personnel

(a) Changes in Extensive Margin

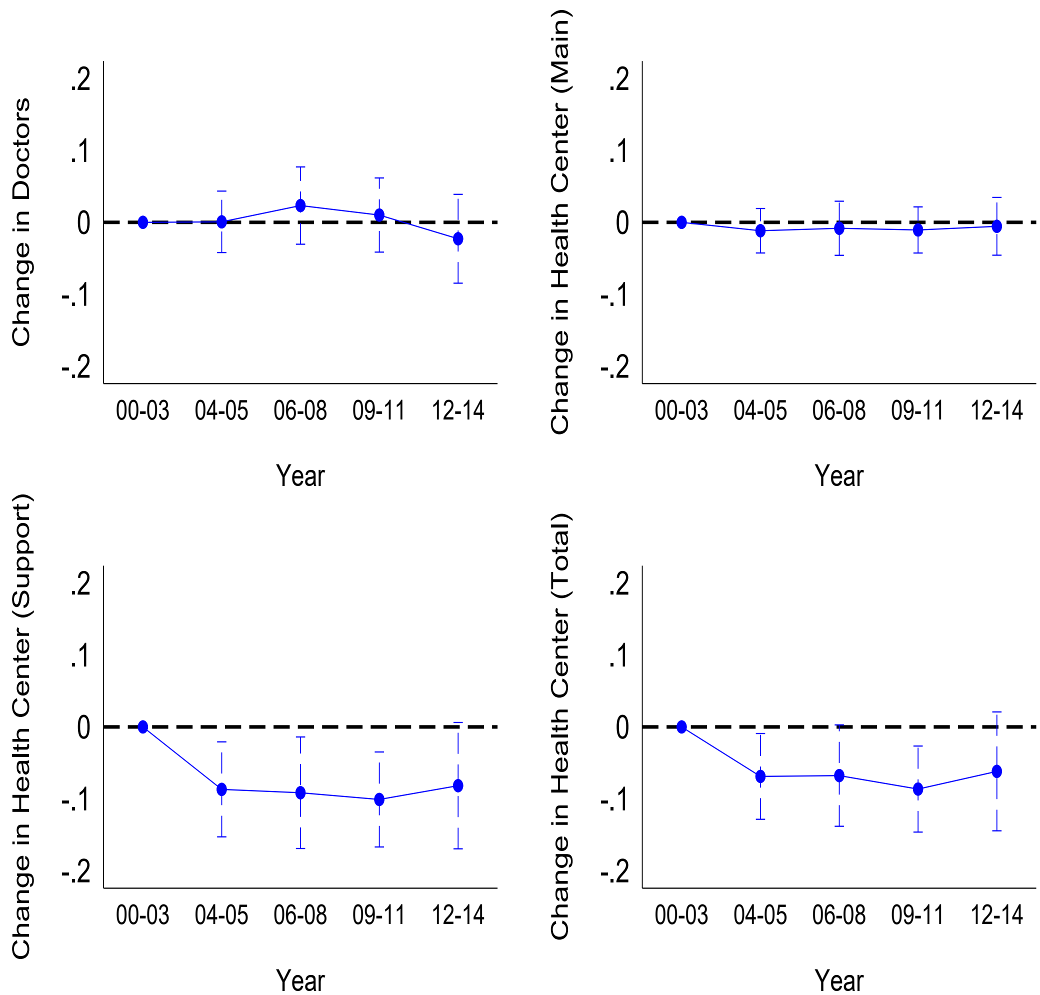


(b) Changes in Intensive Margin



Note: This figure plots regression coefficients of estimating Equation (1.2). Each coefficient comes from each regression conducted separately. Panel (a) and Panel (b) plot effects on changes in the presence and number of public health facilities and personnel, respectively. Standard errors are clustered at the district level with 90% confidence interval.

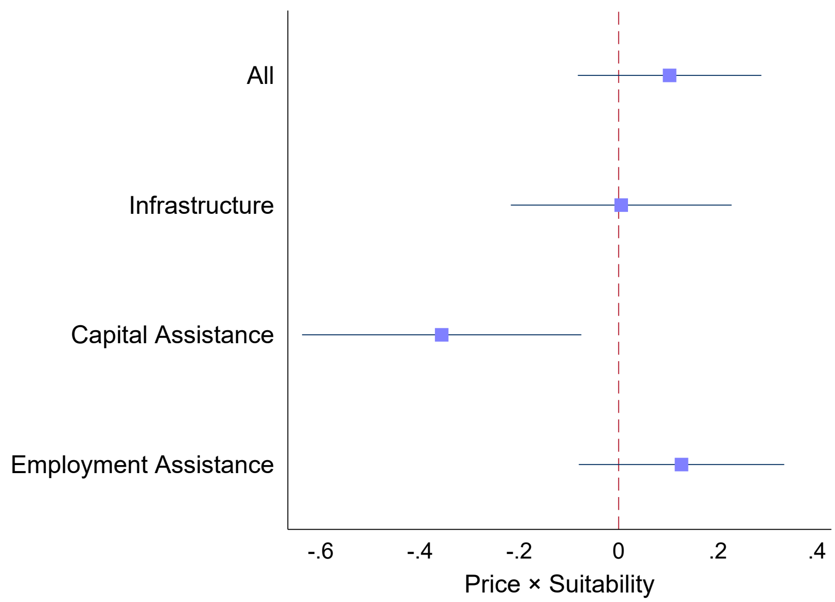
Figure 1.7: Effects on Public Health Facilities and Personnel by Year – Extensive Margin



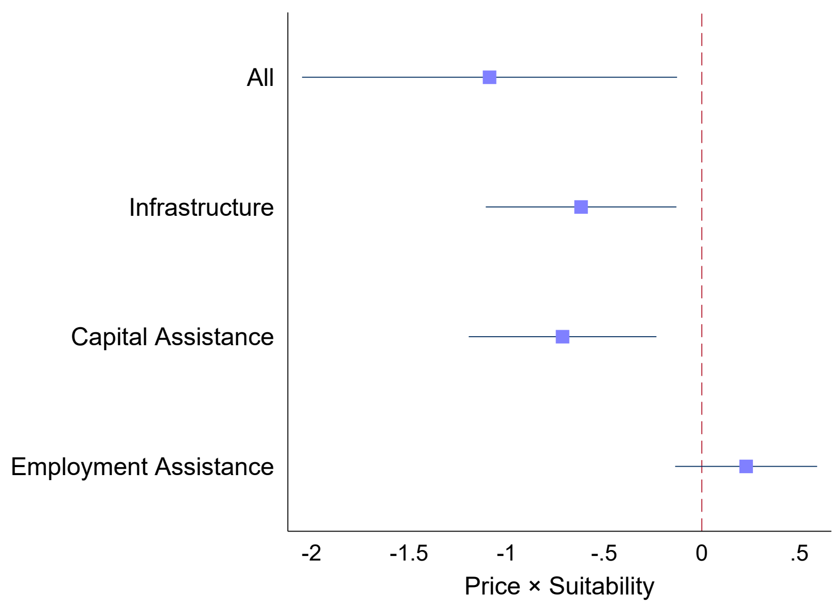
Note: Each regression coefficient corresponds to the change in the gradient between local price shock ($Price \times Suitability$) and growth of public health facilities (extensive margin) relative to the period 2000-2003. Standard errors are clustered at the district level with 90% confidence interval.

Figure 1.8: Effects on Development Projects

(a) Changes in Extensive Margin



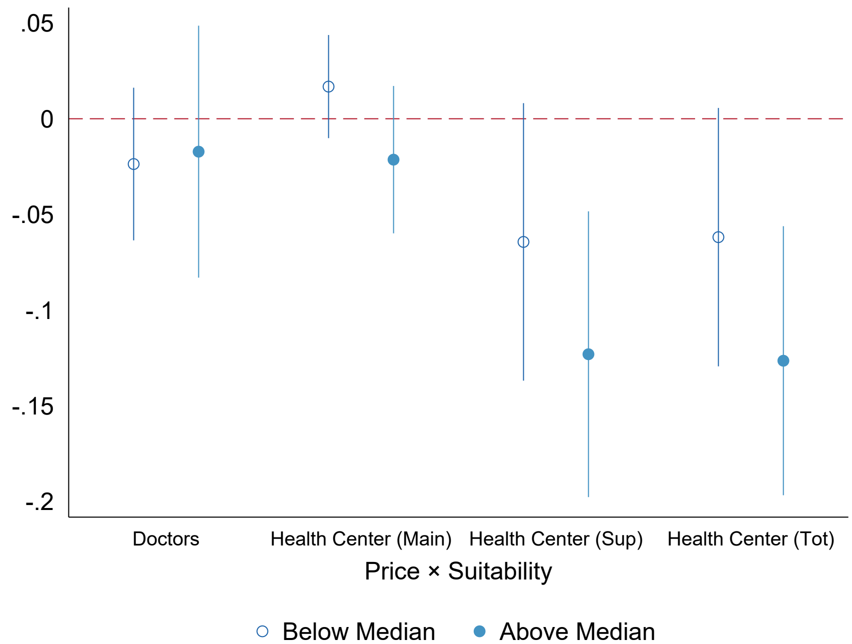
(b) Changes in Intensive Margin



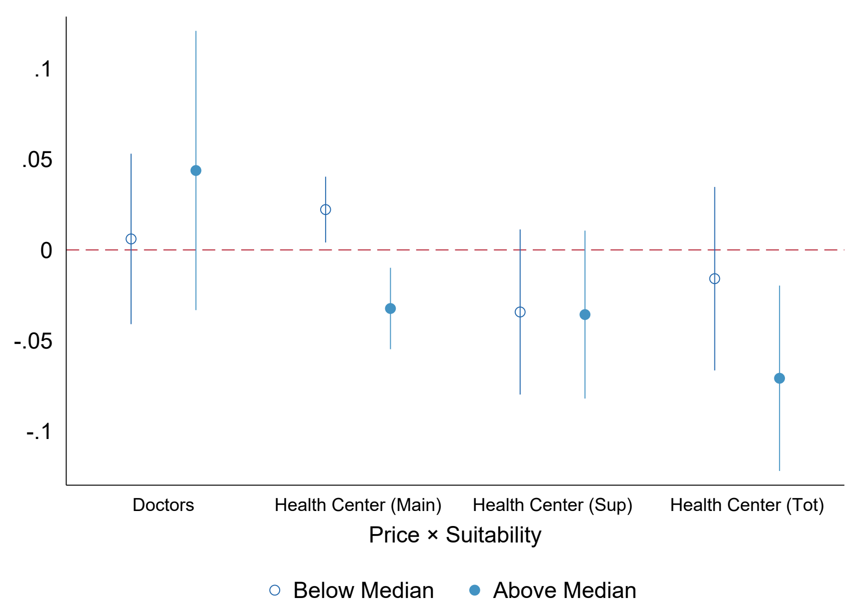
Note: This figure plots regression coefficients of estimating Equation (1.2). Each coefficient comes from each regression conducted separately. Panel (a) and Panel (b) plot effects on changes in the presence and number of development project, respectively. Standard errors are clustered at the district level with 90% confidence interval.

Figure 1.9: Effects on Public Health Facilities and Personnel by Land Inequality

(a) Changes in Extensive Margin



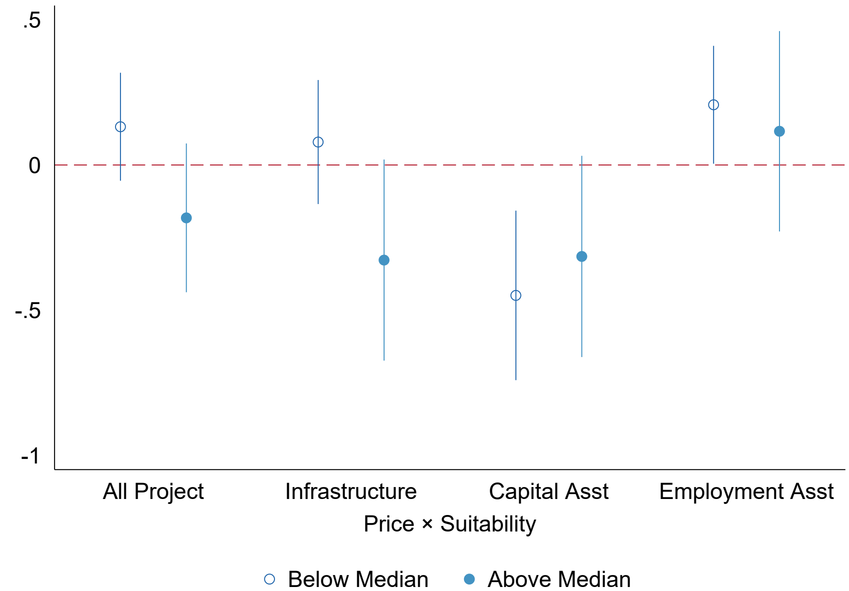
(b) Changes in Intensive Margin



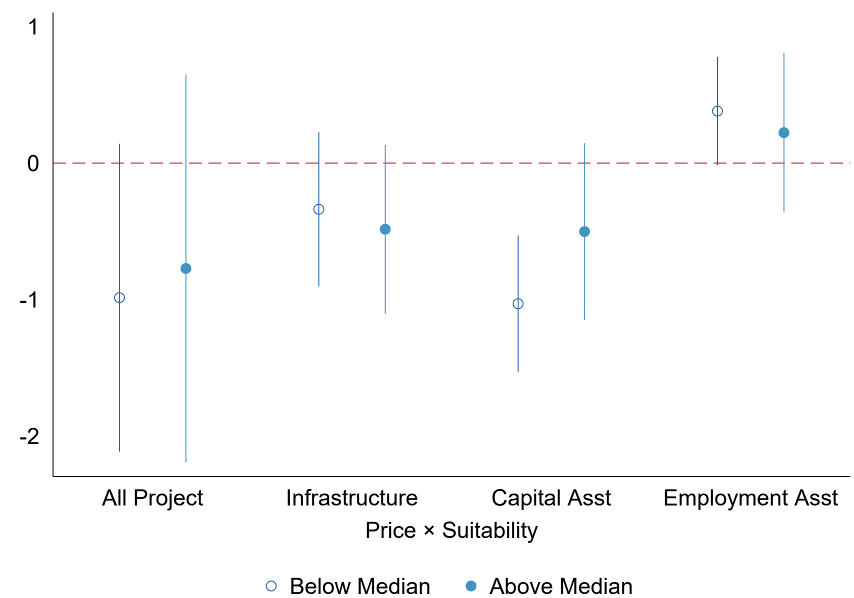
Note: This figure plots regression coefficients of estimating Equation 1.2. Each coefficient comes from each regression conducted separately. Panel (a) and Panel (b) plot effects on changes in the presence and number of public health facilities and personnel, respectively. Standard errors are clustered at the district level with 90% confidence interval.

Figure 1.10: Effects on Development Projects by Land Inequality

(a) Changes in Extensive Margin

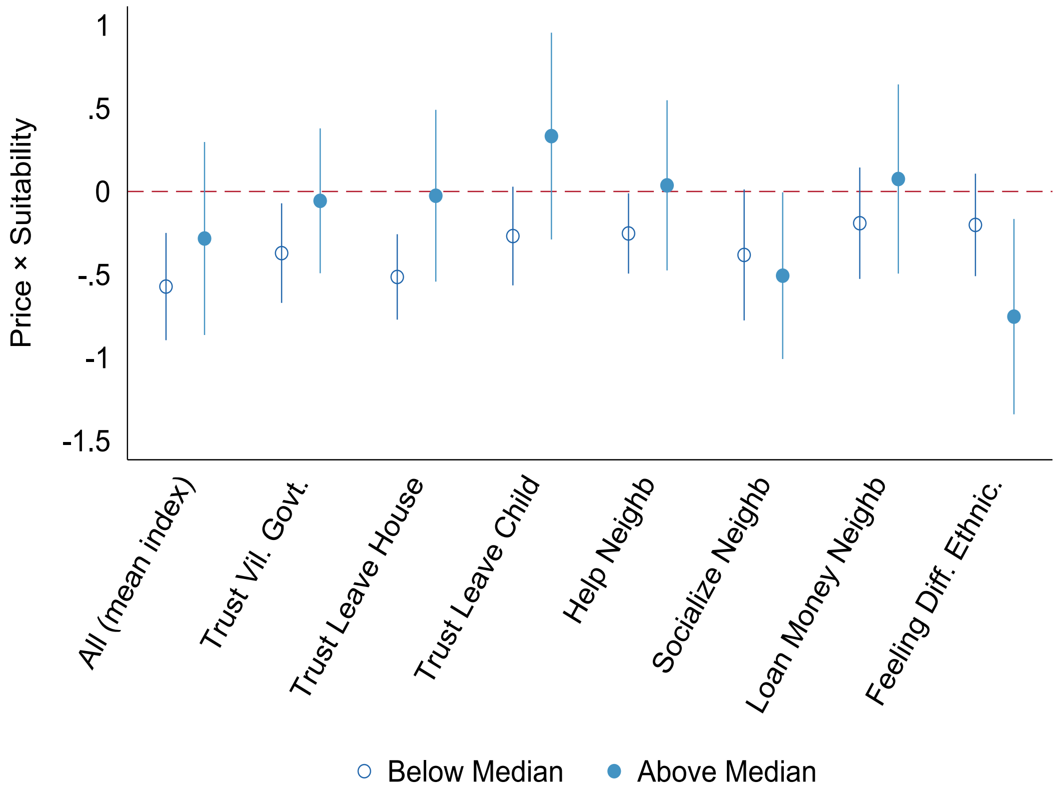


(b) Changes in Intensive Margin



Note: This figure plots regression coefficients of estimating Equation 1.2. Each coefficient comes from each regression conducted separately. Panel (a) and Panel (b) plot effects on the presence and number of development projects, respectively. Standard errors are clustered at the district level with 90% confidence interval.

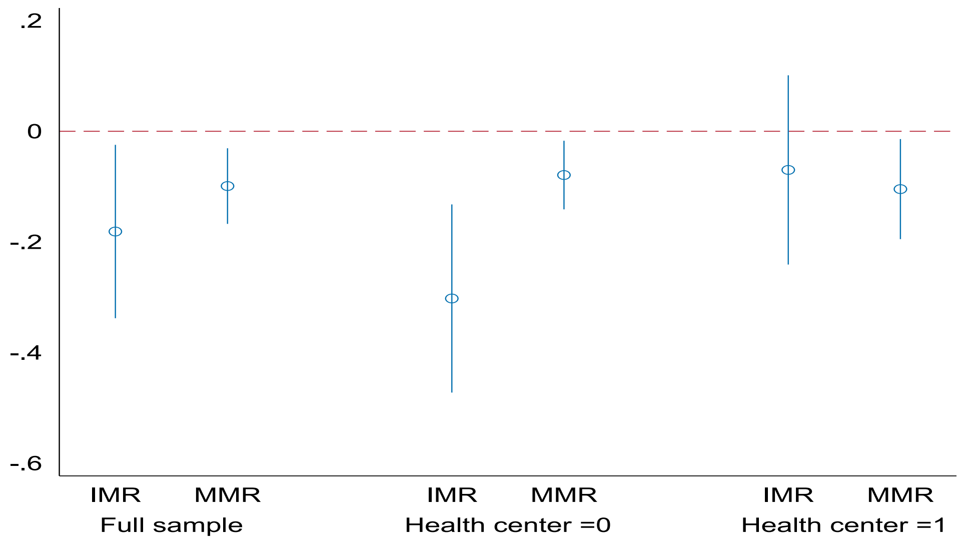
Figure 1.11: Effects on Social Capital by Land Inequality



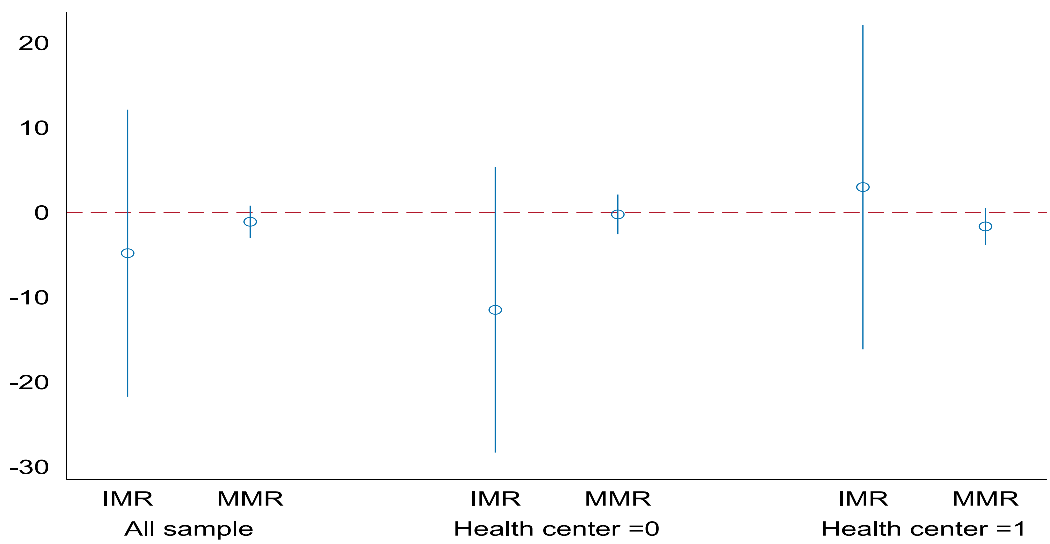
Note: This figure shows regression coefficients obtained from estimating effects on social capital by land inequality. Each coefficient comes from each regression conducted separately. Standard errors are clustered at the district level with 90% confidence interval.

Figure 1.12: Effects on Infant and Maternal Mortality by Public Health Facilities

(a) Extensive Margin



(b) Intensive Margin



Note: This figure plots regression coefficients obtained from estimating effects on presence and number of infant and maternal mortality by different subsamples: 1) whole sample, 2) presence, and 3) absence of health facility. Panel (a) and Panel (b) plot extensive and intensive margins coefficients, respectively. Each coefficient comes from each regression conducted separately. Standard errors are clustered at the district level with 90% confidence interval. Source: IMR and MMR are from the 2010 Population Census.

1.9.2 Tables

Table 1.1: Summary Statistics

	Mean	SD	Obs.
Panel A: Demographic and administrative characteristics			
Number of population (thousands)	3.59	4.21	318912
Number of villages (hundreds)	2.71	1.31	318912
Ethnic fractionalization	0.18	0.24	317418
Proportion of high education (> primary school)	0.23	0.15	317418
Proportion of Muslim	0.84	0.33	317418
Urban village	0.17	0.38	318899
Distance to district capital (km)	41.31	45.84	318629
Distance to subdistrict capital (km)	8.66	11.91	318580
Panel B: Agricultural characteristics			
Price change (annual growth), 2000-2014	0.10	0.06	265285
Potential rice yields (suitability) (tons/ha)	5.06	0.89	309786
Paddy production (tons/ha)	4.26	3.98	252801
Paddy harvested area (thousands ha)	0.57	0.58	316896
Gini coefficient of land ownership	0.54	0.18	301128
Share of HH plant sawah (wetland) >0	0.37	0.31	301533
Share of HH plant palawija >0	0.29	0.31	301533
Share farmers sell and consume ag prod.	0.78	0.41	264928
Panel C: Public goods and development projects			
Presence of...			
Doctors	0.20	0.40	318912
Health center (Main)	0.13	0.33	318912
Health center (Support)	0.33	0.47	318912
Health center (Total)	0.44	0.50	318912
Development project (Total)	0.84	0.37	159456
Infrastructure project	0.71	0.46	159456
Capital asst. project	0.64	0.48	159456
Employment asst. project	0.27	0.45	159456
Number of...			
Doctors	0.54	1.53	265760
Health center (Main)	0.13	0.33	265760
Health center (Support)	0.35	0.50	265760
Health center (Total)	0.47	0.56	265760
Development project (Total)	3.14	2.61	159456
Infrastructure project	1.68	1.56	159456
Capital asst. project	1.04	1.04	159456
Employment asst. project	0.40	0.76	159456

Note: Number of observations varies due to variation in availability of some variables in village census (*Podes*) waves

Table 1.2: Public Health Facilities and Personnel

	Δ Presence				Δ Number			
	Doctors (1)	Health Center (Main) (2)	Health Center (Support) (3)	Health Center (Total) (4)	Doctors (5)	Health Center (Main) (6)	Health Center (Support) (7)	Health Center (Total) (8)
Price	0.167*	-0.044	0.461***	0.417***	-0.098	-0.005	0.169*	0.181*
	(0.094)	(0.067)	(0.153)	(0.149)	(0.126)	(0.043)	(0.098)	(0.109)
Price × Suitability	-0.032	0.005	-0.087***	-0.085***	0.013	-0.000	-0.030	-0.035
	(0.021)	(0.013)	(0.032)	(0.030)	(0.025)	(0.009)	(0.021)	(0.023)
Village FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District-specific Trend	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	237063	237063	237063	237063	190383	188955	188955	188955
R-Squared	0.080	0.096	0.075	0.073	0.131	0.155	0.130	0.136
Mean of Dep. Var.	0.199	0.129	0.335	0.442	0.639	0.131	0.350	0.482

Note: This table presents the effects on changes in public good provision in health facilities and personnels. Columns 1 to 4 present estimation results of changes in extensive margins. Columns 5 to 8 present estimation results of changes in intensive margins. All regressions include population (log), distance to district capital (log), distance to sub district capital (log), village area (log) interacted with year fixed effects, harvested areas (log) allocated for planting rice interacted with year fixed effects. Dependent variables in columns 5 to 8 of Panel B are not available in PODES 2008. Standard errors, reported in parentheses, are robust to heteroskedasticity and clustered at district level. $*p < 0.10, **p < 0.05, ***p < 0.01$

Table 1.3: Development Projects

	Presence				Number			
	All project	Infrastructure	Capital Asst	Employment (Asst)	All project	Infrastructure	Capital Asst	Employment (Asst)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Price	-0.063	0.124	1.691**	-0.593	4.037	1.850	3.292**	-1.072
	(0.509)	(0.634)	(0.780)	(0.599)	(2.673)	(1.352)	(1.327)	(1.052)
Price × Suitability	0.103	0.005	-0.357**	0.127	-1.088*	-0.619**	-0.714**	0.227
	(0.112)	(0.135)	(0.171)	(0.125)	(0.582)	(0.296)	(0.291)	(0.221)
Village FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District-specific Trend	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	145654	145654	145654	145654	145654	145654	145654	145654
R-Squared	0.563	0.766	0.506	0.455	0.693	0.736	0.560	0.468
Mean of Dep. Var.	0.840	0.705	0.636	0.274	3.144	1.680	1.044	0.403

Note: The sample for all regressions only include PODES 2008 onwards because information on development projects only available starting in 2008. All regressions include population (log), distance to district capital (log), distance to sub district capital (log), village area (log) interacted with year fixed effects, harvested areas (log) allocated for planting rice interacted with year fixed effects. All regressions include village and year fixed-effects as well as district-specific trends. Standard errors, reported in parentheses, are robust to heteroskedasticity and clustered at district level.. $*p < 0.10, **p < 0.05, ***p < 0.01$

Table 1.4: Night Light Intensity and Health Insurance Enrollment for the Poor

	Δ Lights Coverage	Δ Lights Intensity	Δ Health Card
	(1)	(2)	(3)
Price	-2.342***	-1.143***	506.316**
	(0.429)	(0.176)	(198.090)
Price × Suitability	0.476***	0.249***	-116.880***
	(0.081)	(0.035)	(41.340)
Village FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
District-specific Trend	Yes	Yes	Yes
N	188076	188076	237063
R-Squared	0.143	0.299	0.295
Mean of Dep. Var.	0.663	1.329	435.137

Note: This table presents the results of changes in the presence and intensity of nighttime lights as well as the number of membership of health insurance for the poor. The sample for dependent variables in columns 1 and 2 are only up to 2011. All regressions include population (log), distance to district capital (log), distance to sub district capital (log), village area (log) interacted with year fixed effects, harvested areas (log) allocated for planting rice interacted with year fixed effects. Standard errors, reported in parentheses, are robust to heteroskedasticity and clustered at district level. $*p < 0.10, **p < 0.05, ***p < 0.01$

Table 1.5: Nutrient Intake and Share of Food Expenditure per Capita

	Calorie	Protein	Share of Food Exp per capita.
	(1)	(2)	(3)
Price	-0.072	-0.215	-0.014
	(0.167)	(0.198)	(0.053)
Price × Suitability	0.034*	0.050**	0.008
	(0.019)	(0.022)	(0.006)
District FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
District-specific Trend	Yes	Yes	Yes
N	305460	305460	305460
R-Squared	0.177	0.170	0.253

Note: This table presents the effects on nutritional status and the share of food expenditure. Nutrition status is measured by per capita calorie (log) and protein (log) intake in the last seven days at the household level using data from the consumption module of the 2002, 2005, 2008, and 2011 National Socioeconomic Survey (Susenas). The sample covers 25,821 unique villages. To obtain per capita measures, household size is adjusted by equivalent scales. Calorie and protein intakes are converted from various food groups. In addition to the village-level covariates in the main specification, the regression specification also includes household covariates: indicator for wife's education attainment, wife's age and age squared, indicator for marital status of head of household (not married, married, divorced, widowed), and indicators for the number of household members aged 0-4, 5-9, 10-14, 15-55, and above 55. Standard errors, reported in parentheses, are robust to heteroskedasticity and clustered at the district level.. $*p < 0.10, **p < 0.05, ***p < 0.01$

Table 1.6: Effects on Public Health Facilities and Personnel by Land Inequality

	Δ Presence				Δ Number			
	Doctors	Health Center (Main)	Health Center (Small)	Health Center (Total)	Doctors	Health Center (Main)	Health Center (Small)	Health Center (Total)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Land Inequality: Above Median								
Price $\times$ Suitability	-0.017	-0.021	-0.123***	-0.126***	0.044	-0.032**	-0.036	-0.071**
	(0.040)	(0.023)	(0.045)	(0.043)	(0.047)	(0.014)	(0.028)	(0.031)
N	110679	110679	110679	110679	89028	88000	88000	88000
R-Squared	0.081	0.097	0.076	0.074	0.133	0.161	0.132	0.143
Land Inequality: Below Median								
Price $\times$ Suitability	-0.024	0.017	-0.064	-0.062	0.006	0.022**	-0.034	-0.016
	(0.024)	(0.016)	(0.044)	(0.041)	(0.028)	(0.011)	(0.028)	(0.031)
N	114400	114400	114400	114400	91709	91415	91415	91415
R-Squared	0.080	0.097	0.077	0.074	0.136	0.152	0.130	0.134

Note: This table presents efects on development projects by land inequality. All regressions include population (log), distance to district capital (log), distance to sub district capital (log), village area (log) interacted with year fxed efects, harvested areas (log) allocated for planting rice interacted with year fxed efects, village and year fxed-efects as well as district-specifc trends. Standard errors, reported in parentheses, are robust to heteroskedasticity and clustered at district level. $*p < 0.10, **p < 0.05, ***p < 0.01$

Table 1.7: Effects on Development Projects by Land Inequality

	Presence				Number			
	All project	Infrastructure	Capital Asst	Employment (Asst)	All project	Infrastructure	Capital Asst	Employment (Asst)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Land Inequality: Above Median								
Price × Suitability	-0.182	-0.327	-0.315	0.116	-0.772	-0.485	-0.502	0.223
	(0.155)	(0.210)	(0.210)	(0.209)	(0.861)	(0.375)	(0.392)	(0.355)
N	68705	68705	68705	68705	68705	68705	68705	68705
R-Squared	0.544	0.752	0.493	0.438	0.681	0.746	0.542	0.455
Land Inequality: Below Median								
Price × Suitability	0.132	0.079	-0.449**	0.207*	-0.986	-0.338	-1.031***	0.381
	(0.113)	(0.129)	(0.177)	(0.123)	(0.684)	(0.343)	(0.303)	(0.240)
N	69500	69500	69500	69500	69500	69500	69500	69500
R-Squared	0.576	0.784	0.507	0.458	0.694	0.727	0.564	0.468

Note: This table presents effects on development projects by land inequality. All regressions include population (log), distance to district capital (log), distance to sub district capital (log), village area (log) interacted with year fixed effects, harvested areas (log) allocated for planting rice interacted with year fixed effects, village and year fixed-effects as well as district-specific trends. Standard errors, reported in parentheses, are robust to heteroskedasticity and clustered at district level. $*p < 0.10, **p < 0.05, ***p < 0.01$

Table 1.8: Effects on Social Capital by Land Inequality

	All (Mean)	Trust Vil. Govt.	Trust Leave House	Trust Leave Child	Help Neighbor	Socialize Neighbor	Loan Money Neighbor	Feeling Diff. Ethnic
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Baseline Specification (all sample)								
Price × Suitability	-0.523***	-0.290*	-0.412***	-0.119	-0.182	-0.361	-0.172	-0.448***
	(0.181)	(0.159)	(0.135)	(0.163)	(0.139)	(0.230)	(0.191)	(0.168)
N	229905	220092	197147	219876	209373	228846	198156	220741
R-Squared	0.074	0.041	0.083	0.053	0.049	0.059	0.055	0.065
Land Inequality: Above Median								
Price × Suitability	-0.283	-0.056	-0.026	0.333	0.037	-0.507*	0.075	-0.753**
	(0.352)	(0.264)	(0.313)	(0.377)	(0.310)	(0.304)	(0.345)	(0.356)
N	106898	101727	90026	101866	96795	106391	89910	102392
R-Squared	0.080	0.050	0.084	0.056	0.055	0.069	0.067	0.081
Land Inequality: Below Median								
Price × Suitability	-0.572***	-0.371**	-0.514***	-0.268	-0.253*	-0.382	-0.191	-0.201
	(0.196)	(0.182)	(0.156)	(0.180)	(0.146)	(0.239)	(0.203)	(0.187)
N	109223	105225	95027	104790	99709	108725	96544	104908
R-Squared	0.087	0.051	0.096	0.069	0.068	0.071	0.072	0.075

Note: This table presents baseline and heterogeneity effects on social capital by land ineqaulity using sociocultural module of the 2009 and 2012 National Socioeconomic Survey (Susenas). The sample covers 15,088 unique villages. Column 1 measures mean index of all social capital variables (columns 2 to 8). Price is omitted due to collinearity with district-specific trends. The sample varies across outcomes due to non-responses. The regression specification adds an indicator variable for being male, age, and age squared. Additional village-level covariates include those in the main specification. For ease of interpretation, dependent variables are standardized. All regressions include village and year fixed effects as well as district-specific trends. Standard errors, reported in parentheses, are robust to heteroskedasticity and clustered at the district level. $*p < 0.10, **p < 0.05, ***p < 0.01$

Table 1.9: Effects on Infant and Maternal Mortality by Presence of Health Centers

	Extensive Margins		Intensive Margins	
	Infant Mortality	Maternal Mortality	Infant Mortality	Maternal Mortality
	(1)	(2)	(3)	(4)
Baseline Specification (all sample)				
Price × Suitability	-0.181*	-0.099**	-4.786	-1.077
	(0.095)	(0.041)	(10.259)	(1.149)
N	52285	52285	52233	52267
R-Squared	0.127	0.037	0.029	0.007
Mean of Dep. Var	0.607	0.111	30.644	1.957
Health Centers (All): Present				
Price × Suitability	-0.070	-0.104*	3.011	-1.628
	(0.104)	(0.055)	(11.596)	(1.312)
N	25004	25004	24990	25002
R-Squared	0.122	0.039	0.031	0.009
Health Centers (All): Not Present				
Price × Suitability	-0.302***	-0.079**	-11.471	-0.212
	(0.103)	(0.038)	(10.199)	(1.420)
N	27281	27281	27243	27265
R-Squared	0.119	0.029	0.030	0.007

Note: This table presents baseline (uninteracted) and heterogeneity treatment effects on infant and maternal mortality by the presence of public health centers. Information on public health centers are taken from the 2008 *Podes*. In addition to the village-level covariates in the main specification, all regressions include indicator for urban village, proportion of employment in agricultural sector, proportion of high educated people (higher than primary school). *Price*, which measures log annualized price change from 2006 to 2009, is included but not shown. Standard errors, reported in parentheses, are robust to heteroskedasticity and clustered at district level. $*p < 0.10, **p < 0.05, ***p < 0.01$

CHAPTER 2

SELECTION AND TREATMENT EFFECTS OF HEALTH INSURANCE SUBSIDIES IN THE LONG RUN: EVIDENCE FROM A FIELD EXPERIMENT IN GHANA

2.1 Introduction

Many poor households in developing countries lack access to health insurance, and their poverty is exacerbated by health-related problems (Dercon, 2002). Many developing countries have increasingly been instituting social health insurance schemes (SHIs) to help mitigate the effects of adverse health shocks, especially for the poor (WHO, 2005, 2010).[1] However, even though SHIs are theoretically mandates and offer low sign-up costs and generous benefits to increase enrollment, take-up and retention rates remain very low in many countries (Fenny et al., 2016), especially among the poorest households (Acharya et al., 2013).[2]

Achieving universal coverage or a high enrollment rate is important in terms of risk pooling and the sustainability of social health insurance; however, it is often difficult for developing countries to successfully impose mandates, primarily due to administrative constraints. For example, if those who are more

[1]Recent examples of countries that have instituted SHIs include Ghana, Kenya, Nigeria, Tanzania, and Vietnam. Countries in the process of instituting SHIs include Cambodia, Laos, Malaysia, Zimbabwe, and South Africa (Wagstaff, 2010).

[2]In a rural district of Northern Ghana, our study area, the annual fees and premiums of the SHI are about $5; the program covers almost 95% of disease conditions without deductibles or copayments. However, by the end of 2010, the total active membership reached only 34% of the total population (NHIA, 2011).

ill or with larger health care service utilization are selected into social health insurance, the financial burden of the program will increase and become less sustainable. Recent studies find that various efforts to promote health insurance enrollment and health care utilization have limited impact (Wagstaff et al., 2016; Capuno et al., 2016; Thornton et al., 2010).[3]

Even after successfully promoting health insurance enrollment in the short run, retention and sustainable improvements in health service utilization and health status remain a challenge. The long-run effects of an intervention to promote health insurance enrollment have important implications for policy. For example, an increased retention rate may yield economic and health benefits when individuals engage in health services on a regular and timely basis, which may improve the sustainability of the health insurance program. On the other hand, selective retention – those who are more ill and have larger health expenditure remain enrolled – could threaten the sustainability of the insurance scheme. Nevertheless, this topic remains relatively understudied.

Subsidy is one of the few successful types of interventions used to promote health insurance enrollment in developing countries (e.g., Thornton et al., 2010; Banerjee et al., 2019). However, there are two important aspects of the effects of subsidy intervention that remain relatively less understood. The first aspect is the effects of subsidy level (price). It is important because different subsidy level may attract people with different characteristics, and this selection may affect health care service utilization and health outcomes among the insured.[4]

[3] Wagstaff et al. (2016) and Capuno et al. (2016) find subsidy and information do not successfully promote health insurance enrollment. Thornton et al. (2010) find subsidy increases short-term enrollment but does not increase health care service utilization.

[4] In addition, as Dupas (2014) explains, the price level may affect the long-run adoption of

The screening effect of subsidy level has been studied for a few health products and services, such as facility delivery (Grépin et al., 2019), bed nets (Cohen and Dupas, 2010), and chlorine for water purification (Ashraf et al., 2010), but has not been intensively investigated for health insurance in a developing country setting. One exception is Banerjee et al. (2019) that, among other interventions, randomly provides half and full subsidy of health insurance fees to households to examine health insurance enrollment in urban areas in Indonesia.

The second aspect is the effects of receiving temporary subsidy on the long-run adoption or take-up of the health insurance and its consequences. The long-run effects are potentially ambiguous in theory. Temporary subsidy can reduce take-up in the long run if people consider the temporarily subsidized price as the reference points that could affect their future reservation price (Simonson and Tversky, 1992; Kőszegi and Rabin, 2006). On the other hand, temporary subsidy can increase demand in the long run for an experience good, i.e., health insurance, if people using the subsidized good subsequently learn the value and the benefits of it. Despite the importance of understanding the long-run effects of temporary subsidy, there is relatively scarce evidence on its application and evaluation on long-run health insurance enrollment in developing countries.

This study complements the literature through a field experiment. We employ experimental variations in access to a health product and examine the behavioral responses in the long run. Specifically, we randomly selected communities for the subsidy intervention and randomized different levels of subsidy (one-third, two-thirds, and full subsidy) for the insurance premiums and fees for one-year's coverage at the household level. To measure the impact of these

health products through the "anchoring" mechanism, where a previously encountered price may act as anchor and affect people's valuation of a product regardless of its intrinsic value.

interventions, we conduct a baseline survey and two follow-up surveys at seven months and at three years after the initial intervention.

This experiment has two main objectives. First, we study whether a subsidy for premiums and fees promotes health insurance enrollment in short and long run. Second, we study whether the level of subsidy affects health insurance enrollment, health care service utilization, and self-reported health status to shed light on the potential selection effect of the level of health insurance subsidy. Three sets of results emerge. First, we find a significant increase in short-run insurance take-up. Those receiving one-third, two-thirds, and a full subsidy were 39.3, 48.3, and 53.8 percentage points, respectively, more likely to enroll in health insurance in the short-run. Three years after the initial intervention, we still observe increased enrollment. Those who received one-third, two-thirds, and a full subsidy were 17.3, 14.0, and 18.9 percentage points, respectively, more likely to enroll in health insurance in the long run.

Second, we find evidence of selection. Those who enrolled due to our intervention (compliers) are more ill and have larger health expenditures than those who did not enroll regardless of intervention (never-takers). Among compliers, individuals in the partial subsidy group are particularly more ill and have larger health expenditures than those in the full subsidy group. This evidence suggests that having to pay positive amount of premium and fees induces individuals to engage in selective enrollment and maximize net expected benefits of having insurance. This is somewhat consistent with a recent study in Indonesia (Banerjee et al., 2019). They find that individuals who received half of health insurance subsidy submitted more medical claims than those who received full subsidy. We also find that selection patterns are more prominent in the long run, which is consistent with the idea of health insurance as an experience good. In the long run individuals have more time to learn about their health types as well as cost and benefits of health insurance.

Third, we do not find evidence of improvement in health status despite increased health care expenditures in the long run, especially for the partial-subsidy group. One possible explanation is that people become more sensitive to symptoms and/or aware of their illness, which may lead to a reporting problem: misperception of symptoms of an illness. This could happen when people with coverage make frequent contacts with health facilities (Dow et al., 1997; Finkelstein et al., 2012).

In summary, this study shows that a short-term subsidy intervention can successfully sustain health insurance enrollment even when mandates are not enforceable. However, we also observe a selection pattern on observable characteristics, especially in the partial subsidy group, which negatively affects the financial sustainability of social health insurance program.

Our study contributes to three strands of literature. First, our study contributes to the literature on sustainability of health intervention programs. This study is, to our knowledge, among the first to document evidence of the long-run effects of interventions on insurance enrollment retention in a developing country.[5] While the idea of promoting sustainability is attractive, it is difficult to achieve in practice. The challenges in promoting sustainable health insurance enrollment could be even greater because health care services in developing

[5] One exception is Banerjee et al. (2019) that studies the longer-run (32 months post-intervention) effects of different level of temporary subsidy (half and full), assisted registration, and information on participation in health insurance program in urban areas in Indonesia. They also evaluate healthcare utilization and health outcomes.

countries are generally of low quality and unreliable.[6] The few studies on this topic include those by Kremer and Miguel (2007) and Dupas (2014). In contrast to Kremer and Miguel (2007), who find limited evidence that a subsidy promotes long-run adoption of worm treatment, Dupas (2014) finds that a one-time subsidy is effective in boosting long-run adoption of bed nets.[7]

Second, our study also contributes to the literature on selection by price (or level of subsidy). Specifically, we study whether the characteristics of people who are enrolled in health insurance vary by the level of subsidy in the short and long run. The effect of prices on utilization of health products and services has received considerable attention recently. While proponents of user fees argue that cost-sharing is necessary for the sustainability of health programs (Bank, 1993; Easterly, 2006), there is a concern that even a small fee may prevent those most in need from purchasing the product. Several studies aiming to test the existence of the screening effects of higher prices on health product utilization find mixed results. For example, while Ashraf et al. (2010) find that high prices stimulate product use through a screening effect in chlorine for water sanitation, Cohen and Dupas (2010) find no effect of higher prices on the use

[6]See, for example, Banerjee et al. (2004), Goldstein et al. (2013), and Das et al. (2016) for illustrations of low health care quality in developing countries. Alhassan et al. (2016) provides illustrations for Ghana.

[7]It is important to note, however, that the long-run effect of a one-time health insurance intervention is quite different from that of health product adoption such as worm treatment and malaria bed nets. Having health insurance does not necessarily result in improved health status. To be successful, health insurance enrollment should promote health care service utilization and prevent moral hazard behaviors. In addition, learning about the effects of other health products, such as deworming medicine, bed nets, and water disinfectants, could be less setting-specific than the case of health insurance, where the quality of health care services could vary considerably across settings.

of bed nets.

Lastly, our study contributes to the broad empirical literature on the effects of health insurance coverage on health outcomes, which has so far produced mixed evidence. Some studies find insignificant impacts of insurance coverage on health outcomes (Thornton et al., 2010; Fink et al., 2013; King et al., 2009), while others find positive impacts (e.g., Miller et al., 2013; Gruber et al., 2014). In terms of OOP expenses, some studies observe no or adverse effects of insurance on such expenses (e.g., Thornton et al., 2010; Fink et al., 2013), while others find the opposite (e.g., Galárraga et al., 2010). Our study is among the first to examine the effects of insurance coverage on both short- and long-run health outcomes in a low-income setting, while the existing literature largely focuses on short-run outcomes.[8]

The remainder of this paper is structured as follows. Section 2.2 outlines the research context. Section 2.3 describes the experimental design and data. Section 3.4 presents the empirical strategy and Section 3.5 presents the main results. Section 3.7 concludes the paper.

[8]In the US setting, a RAND experiment reports insignificant effects of insurance coverage on average health outcomes but finds negative effects on health outcomes for the more vulnerable subgroups (Newhouse et al., 1993). More recent studies find positive effects of exposure to public health insurance during childhood on various long-term health outcomes (Currie et al., 2008; Miller and Wherry, 2019; Boudreaux et al., 2016).

2.2 Institutional Background

2.2.1 National Health Insurance Scheme (NHIS) in Ghana

The National Health Insurance Scheme (NHIS) in Ghana was established by the National Health Insurance Act (Act 560) in 2003. It aims to improve access to and the quality of basic health care services for all citizens, especially the poor and vulnerable (MoH, 2004). The law mandates that every citizen enroll in at least one scheme. However, in practice, there are no penalties for those who do not enroll. Most of the 170 administrative districts of Ghana operate their own District Mutual Health Insurance Scheme (DMHIS) (Gajate-Garrido and Owusua, 2013).[9] Each DMHIS accepts and processes applications, collects premiums (and fees), provides membership identification cards, and processes claims from accredited facilities for reimbursement.

Annual means-tested premiums, which are charged to informal sector workers, range from \$5 to \$32. However, owing to the lack of information on household incomes, rural districts tend to charge the lowest premiums, while urban districts charge higher premiums. Indigents, pregnant women, children under 18 years, and the elderly over 70 years are exempt from premiums but not reg-

[9]There are three types of insurance schemes in Ghana: District Mutual Health Insurance Schemes (DMHIS), Private Mutual Health Insurance Schemes (PMHIS), and Private Commercial Insurance Schemes (PCHIS). The focus of this study is DMHIS, which represents 96 percent of insurance coverage. They are operated and subsidized by the government through the National Health Insurance Fund (NHIF). PMHIS are non-profit non-subsidized schemes run by NGOs, religious bodies and cooperative societies. PCHISs are for profit schemes that do not receive government subsidies.

istration fees.[10] All members, except for indigents and pregnant women, are required to pay registration fees when they first register and when they renew. Those who do not renew their membership by the due date pay penalties when they eventually renew their memberships.

The benefits package of the NHIS, which is the same across DMHISs, is very generous, albeit new members wait for three months before they can enjoy the insurance benefits. As described in Appendix B, the package covers: 1) full outpatient and inpatient (surgery and medical) treatments and services, 2) full payment for medications on the approved list, 3) payments for referrals on the approved list, and 4) all emergencies. The NHIA (2010) estimates that 95% of disease conditions that affect Ghanaians are covered by the scheme. Those who enroll do not pay deductibles or copayments for health care service utilization by law; however, according to the NHIA (2011), health care providers often charge unauthorized fees that are inaccurately described as copayments.

Despite the low premiums and generous benefits, enrollment in the NHIS remains low. By the end of 2010, the total active membership stood at 34% of the population of Ghana (NHIA, 2011). Enrollment is particularly low among the poorest. A 2008 nationwide survey found that only 29% of the individuals in the lowest wealth quintile were active members of the scheme compared to 64% of households in the highest quintile (NDPC, 2009).

[10]The law defines an indigent as "a person who has no visible or adequate means of income or who has nobody to support him or her and by the means test." Specifically, an indigent is a person who satisfies all of these criteria: i) unemployed and has no visible source of income, ii) does not have a fixed place of residence according to standards determined by the scheme, iii) does not live with a person who is employed and who has a fixed place of residence, and iv) does not have any identifiably consistent support from another person.

In addition to the lack of affordability, negative perceptions of the NHIS explain the low enrollment rate. For example, Alhassan et al. (2016) note that those enrolled in the NHIS generally perceive they are not receiving good-quality health care, for reasons such as long wait times and the poor attitudes of health staff towards patients. Additionally, Fenny et al. (2016) observe that perceived quality of service and socio-cultural factors such as trust, bad attitudes of health facility staff, and drug shortage contribute to low enrollment and retention rates in Ghana.

2.2.2 Setting

This study was conducted in Wa West, a poor and remote rural district in Northern Ghana (Figure B.1). It covers an area of approximately 5,899 km^2 and had a population of about 81,000 in 2010. Settlement patterns are highly dispersed, with most residents living in hamlets of about 100-200 people. This high dispersion, coupled with the poor road network, makes traveling within the district difficult and expensive. The economy is largely agrarian, with over 90% of the population working as farmers. Estimates from the 2006 Ghana Living Standard Survey indicate that average annual per-capita income and health expenditure in a rural savannah locality like Wa West were about $252 and $26, respectively (GSS, 2008).

In the study area, even though the Community-Based Health and Planning Services (CHPS) has increased accessibility to health care services,[11] there

[11] CHPS are community health facilities that provide primary health care. They are located within rural communities with limited access to larger hospitals and are manned by nurses. Among the services offered are treatment of common ailments (malaria and diarrheal diseases)

are only six public health centers and no tertiary health facility.[12] During the study period, the district had only 15 professional nurses and no medical doctor (Nang-Beifua, 2010). The district also has a high disease burden. The most common cause of outpatient visits in the region is malaria, which accounts for one third of outpatient visits. Other common causes of outpatient visits are acute respiratory-tract infections and skin diseases.

The Wa West DMIHS was introduced in January 2007. In 2011, it charged a uniform premium of \$5.46 (GHC 8.20) for adults (18-69) and a processing fee of \$2.67 (GHC 4) for first-time members and \$0.60 (GHC 1) for renewals. Late renewals incur a fee of \$1.30 (GHC 2) in addition to full premiums for all years for which membership was not renewed.[13] The baseline enrollment rate in 2011 for the study sample is 20%.

2.3 Research Design

In this section, we discuss the original study, data collection, definition and construction of key variables, descriptive statistics as well as the balance test of baseline characteristics.

---

and maternal and child care services.

[12]About 75% of the communities in the study sample were within 6 km (3.73 mi) of a health facility.

[13]The exchange rate at the time of the study was \$1 = GHC 1.5.

2.3.1 Interventions

We begin by discussing the original study aimed to analyze short-run outcomes (Asuming, 2013). Three different interventions were introduced to 4406 individuals of 629 household in 59 communities: a subsidy for the insurance premiums and fees (*Subsidy*), an information campaign on the national health insurance (*Campaign*), and an option for individuals to sign up in their community instead of traveling to the district capital (*Convenience*). Interventions were overlapping and randomized at the community level. Figure B.2 summarizes our original research design.[14]

The *Subsidy* intervention was conducted in two stages. In the first stage, subsidy was randomly provided to households across communities. In the second stage, the level of subsidy was randomized at the household level within the *Subsidy* communities: one-third ($2.67), two-thirds ($5.40), or full ($8.13) subsidy (see Figure 2.1). Subsidies were given in the form of vouchers, which were distributed between November 2011 and January 2012, valid for two-month, and redeemable at the Wa West DMHIS center.[15]

The subsidy voucher specified the names, ages, and genders of all household members, expiration date, and place of redemption. Households that did not receive a full subsidy were informed about the extra amount needed to register all

[14]The initial intention of the original study was to analyze the effects of single intervention and complementarity among interventions. We also estimate the long-term effects of all intervention. Results are shown in the Appendix B.

[15]The voucher could also be used to either initiate or renew insurance membership. Those who did not enroll at the baseline (80%) could use the voucher anytime. Those who had already enrolled at the baseline (20%) could only use the voucher if their existing renewal was due within the voucher's validity period. Otherwise they could not use the voucher.

members.[16] Although subsidy was provided at the household level, enrollment had to be specified for each household member. Households do not necessarily insure all members. For example, only about 8%, 36%, and 16% of households enrolled all members in the baseline, first, and second follow ups, respectively.

We extended the original study by implementing a long-term follow-up survey and focus only on the *Subsidy* intervention in this paper for two reasons. First, to obtain estimates with sufficient statistical power. Second, to avoid potential few-cluster problem which could lead to downward-biased standard errors and over-rejection of null hypotheses.

To formally support our approach, we conducted two empirical exercises. First, complementarity tests between *Subsidy* and other treatments. Obtaining unbiased causal effects of the *Subsidy* intervention requires no complementarity between *Subsidy* and the other treatments. Table B.3.1. shows that eight complementarity tests (e.g., *Sub* + *Camp* = *Sub* & *Camp*) fail to reject the null hypothesis of no complementarity at the 5% level.

Second, restricting the sample to the control and Subsidy only groups (i.e., excluding *Subsidy* + *Campaign*, *Subsidy* + *Convenience*, and *Subsidy* + *Campaign* + *Convenience*) to investigate the cleaner effects of subsidy variation. We provide the estimation results in Tables B.3.2 (effects on enrollment), B.3.3 and B.3.4 (short- and long-run effects on health care utilization), and B.3.5 and B.3.6

[16]For one-third or two-thirds subsidy households, vouchers took one of two forms: specified and unspecified. If a household received a specified subsidy voucher, its members were listed on the voucher, along with the specific amount of subsidy for each of them. Thus, reallocation of a subsidy within a household was not possible. If a household received an unspecified subsidy voucher, reallocation of the subsidy was possible because the voucher only showed the total amount of subsidy for the whole household, not the specific amount for each member.

(short- and long-run effects on health outcomes). The interpretation of our main findings and conclusion holds. Overall, the results of these two exercises lend support for our approach.

2.3.2 Data Collection

The study sample includes 2,954 individuals from 418 households in 44 communities. We conducted the baseline survey in September 2011 and implemented the intervention in October 2011. We conducted two follow-up surveys seven months and three years after the intervention. The baseline survey collected information on demographic characteristics, employment, health status, health care service utilization, enrollment in the NHIS, and health behaviors for all household members.

The first follow-up survey collected information on health care service utilization, health status, and health behaviors. In the second follow-up survey, we collect sets of information similar to those in the first follow-up survey but with greater detail to improve the quality of the data. For example, we asked for specific dates and the respondent's status since the first follow-up for up to three episodes of several important illnesses, such as malaria, acute respiratory diseases, and skin diseases. As a result, there are some differences in the construction of short- and long-run utilization measures that prevent a direct comparison of health care service utilization and health status in these survey periods.[17]

[17]The health facility visit variable in the first follow-up survey is constructed from the following question: "The last time (in the last four weeks/last six months) (NAME) was ill or injured, did he/she visit any health facility?". In the second follow-up survey, the same variable

The main outcome variables of interest, measured at the individual level, are health insurance enrollment, health care service utilization, and self-reported health status. Health care service utilization is measured by health facility visits in the last four weeks and last six months as well as OOP health expenditure. Health status is measured by the number of days of illness in the last four weeks, the indicator and the number of days an individual was unable to perform normal daily activities due to illness, and self-rated health status.[18] The measure of inability to perform normal daily activities is essentially similar to the measure of Activities of Daily Living (ADL) that is commonly used in the literature as an objective measure for health status.[19]

The attrition rate in the first follow-up survey was relatively low (5%) but increases in the second follow-up survey (21%), as shown in Table B.2.1.[20] The short- and long-run attrition rates are not systematically correlated with our interventions.

---

is constructed from questions about respondents' visits during illness episodes. For example, an individual is said to visit a health facility in the last six months if his/her illness episode occurred in the last six months and he/she sought treatment in the health facility.

[18]Self-rated health status, which is restricted to those aged 18 years or older, is only available in the follow-up surveys.

[19]In the literature ADL are usually constructed from asking respondents questions about their ability to perform basic daily activities such as self-feeding, ambulation, dressing and undressing etc. The variables used here are derived from the following questions "During the last four weeks did (NAME) have to stop his/her usual activities because of this (illness/injury)" and "For how many days (in the last one month) was name unable to do his/her usual activities".

[20]The main reasons for attrition in the first follow-up survey are deceased (17%), traveled (61%), relocated to other districts (16%), and others (6%). Information on reasons for attrition is not available in the second follow-up survey.

2.3.3 Baseline Characteristics and Balance Test

Table 2.1 presents the summary statistics of baseline characteristics and balance tests between the intervention and control groups. Panels A, B, and C report the average values of the individual, household, and community characteristics. Columns 1 and 2 report the average characteristics for all respondents and control groups, respectively. The average respondent is about 24 years old and 48% are male. About 20% were enrolled in the NHIS at the baseline survey, and 36% had ever registered with the scheme. In terms of health characteristics, 12% reported a sickness or injury in the last four weeks, about 4% visited a health facility in the last month, and 14% made a positive OOP health expenditure. The average household lives within 5.4 km of a health facility and 20 km from the district capital.

Our empirical approach requires a balance of baseline characteristics between the intervention and control groups that could affect outcome variables. To test this assumption, we compare the means of the variables at the baseline (Table 2.1). Columns 3 to 5 present results from regressions of each variable on control and subsidy level indicators. Column 6, which reports the p-values from the equality test, shows that only 2 out of 31 tests are statistically significant at the 10% level. We also compare the baseline differences between each subsidy level group in Columns 8 to 10. 5 out of 93 t-tests are statistically significant at the 10% level. Overall, these results suggest that our randomization is successful in creating balance across the control and treatment groups.

2.4 Estimation Framework

To measure the effects of our intervention on various outcomes, we estimate the following reduced-form intent-to-treat (ITT) effect of each level of subsidy:

$$\begin{aligned} y_{ihc} = \gamma_0 + &\gamma_1 1/3Subsidy_{ihc} + \gamma_2 2/3Subsidy_{ihc} + \gamma_3 FullSubsidy_{\mathrm{ihc}} \\ &+ \theta X_{\mathrm{ihc}} + \delta Z_{hc} + \omega V_c + \epsilon_{ihc} \end{aligned} \tag{2.1}$$

where y_{ihc} denotes the outcomes for individual i of household h in community c. The outcomes of interest include NHIS enrollment, health care service utilization, health status, and health behaviors. X_{ihc} denotes a vector of baseline individual covariates, such as indicator variables for age, gender, religion, ethnicity, and schooling. Household covariates Z_{hc} include household size and a wealth index indicator (poor third, middle third, and rich third).[21] Community covariates V_c include distance to the nearest health facility and to the NHIS registration center.[22] We also control for a baseline measure of the dependent variable to improve precision. The results are robust when we exclude the baseline controls (results not shown). Estimations employ a linear probability model. For each outcome, we present its short- and long-run estimations.

We cluster standard errors at the community level to account for possible

[21] The wealth index is obtained through a principal components analysis with dwelling characteristics (e.g., number of rooms and bedrooms in the house), enterprise (e.g., ownership of any private non-farm enterprise), livestock (e.g., number of chickens and pigs), and other assets (e.g., motorcycles and bicycles).

[22] In addition, we controlled for indicators for *Subsidy + Campaign*, *Subsidy + Convenience*, and *Subsidy + Campaign + Convenience*.

correlation in the error terms within the same community.[23] We also perform 1,000 draws of a wild-cluster bootstrap percentile t-procedure suggested by Cameron et al. (2008) to address concerns about small number of clusters, which could lead to downward-biased standard errors (Bertrand et al., 2004; Cameron et al., 2008).

To obtain the effects of insurance coverage for compliers, we conduct a two-stage least squares (2SLS) regression, where the first-stage regression equation is Equation 2.1 with health insurance enrollment in the short run as the dependent variable. We estimate the following second-stage regression:

$$y_{ihc} = \alpha_0 + \alpha_1 \widehat{Enrolled}_{\text{ihc}} + \theta X_{\text{ihc}} + \delta Z_{hc} + \omega V_c + \epsilon_{\text{ihc}} \tag{2.2}$$

where we instrument for short-run enrollment. Then, we capture the local average treatment effect for those who were induced to enroll in health insurance as results of our subsidy intervention.[24]

Because we estimate Equation (2.1) for many different outcome variables in health care utilization and health status domains, a multiple hypothesis testing problem may occur. The probability we incorrectly reject at least one null hypothesis is larger than the conventional significance level. We address this concern using two methods. First, we group outcome variables into a domain and take the average standardized treatment effect in each domain, as suggested by Kling et al. (2007) and Finkelstein et al. (2012). For the health care utilization

[23]To account for correlation within household, we also cluster standard errors at the household level. The results do not change our main conclusion (results available upon request).

[24]We assume that income effect of the subsidy ($2.7 - $8.1) is small and negligible given that average income of the households in catchment area is $252.

domain, we group five outcome measures including intensive and extensive measures of health facility visits in the last four weeks and last six months and OOP expense incidence. For the health status domain, we group four outcomes including self-rated health status, number of days of illness, inability to perform normal activities, and the number of days lost to illness. Second, we apply the free step-down resampling procedure to adjust the family-wise error rate, that is, the probability of incorrectly rejecting one or more null hypotheses within a family of hypotheses (Westfall and Young, 1993). Family-wise adjusted p-values of each family are obtained from 10,000 simulations of estimations.[25]

2.5 Results

2.5.1 Impacts on Insurance Take-up, Sustainability, and Price Elasticity

Figure 2.2 shows the enrollment rates of the control and treatment groups at the baseline, short-run follow-up, and long-run follow-up surveys by level of subsidy. In general, it shows that enrollment rate increases with subsidy level in the short and long run, but the impacts attenuate over time. We observe the largest incremental increase in enrollment rate between receiving zero (control group) and one-third subsidy in the short run, but smaller incremental increases in the

[25]These two methods serve different objectives. The first method is relevant for drawing general conclusions about the treatment effects on health care utilization and health status. The second method is more appropriate for examining the treatment effect of a specific outcome belonging to a set of tests.

subsequent levels of subsidy. In the long run, the treatment group is still more likely to enroll in health insurance, but the differences among the one-third, two-thirds, and full subsidy groups become insignificant. Table 2.2 presents the formal regression results. We present robust standard errors in parentheses as well as two-tailed wild cluster bootstrap p-values in square brackets. Our results show that the effects on enrollment attenuate but are sustained over time. Column 1 of Panel A shows that overall subsidy intervention increases short-run insurance enrollment by 43.6 percentage points (160%). Long-run enrollment also increases by on average 20.6 percentage points (90%) (Column 2).

In terms of the level of subsidy, receiving a one-third, two-thirds, and full subsidy is associated with, respectively, a 39.3, 48.3, and 53.8 percentage points higher likelihood of enrolling in insurance than that of the control group in the short run (Column 1). Even though the enrollment rate of the one-third subsidy group is lower than that of the two-thirds and full subsidy groups in the short run, the enrollment rate of the one-third subsidy group is at least as large as those of the two-thirds and full-subsidy groups in the long run (p-value > 0.6).

Enrollment rates decrease in the long run as interventions were one-time subsidy, but they are still higher than that of the control group. Receiving a one-third, two-thirds, and full subsidy is associated with, respectively, 17.3, 14.0, and 18.9 percentage point higher likelihood of enrolling in insurance than that of the control group (Column 2). There is no statistical difference in the effects of receiving different subsidy level (p-value > 0.481).

The short-run arc elasticities are large. Overall, when price decreases from \$8.13 to \$0, demand for health insurance increases from 27.2% to 81% (arc elasticity is -0.54).[26] The estimated arc elasticity is close to the elasticity of preventive health products in developing countries, such as -0.6 for chlorine, a disinfectant that prevents water-borne diseases in Zambia (Ashraf et al., 2010), and -0.37 for insecticide-treated bed nets for malaria prevention in Kenya (Cohen and Dupas, 2010). The estimated arc elasticity is also similar to that of preventive health products in developed countries, such as -0.17 and -0.43 for preventive health care in the United States (Newhouse et al., 1993) and -0.47 for cancer screening in Korea (Kim and Lee, 2017).

Our finding that a larger subsidy may lead to higher health insurance enrollment corresponds to studies in both developed and developing countries (Finkelstein et al., 2019; Banerjee et al., 2019). However, our finding is contradictory to the special zero price argument suggesting that individuals act as if pricing a good as free not only decreases its cost but also adds to its benefit (Shampanier et al., 2007). For example, several studies find a larger decrease between zero and small non-zero prices in demand for bed nets (Dupas, 2014) and HIV testing (Thornton, 2008).

In contrast, we find a large incremental increase in enrollment between zero and the one-third subsidy (full and two-thirds price) but no difference between the two-thirds and full subsidy (one-third and zero price). One possible ex-

[26] Arc elasticity estimates were obtained using the following formula: $[(Y_a - Y_b) / (Y_a + Y_b)] / [(P_a - P_b) / (P_a + P_b)]$, where Y and P denote enrollment rate and price, respectively. The short-run arc elasticity estimates when price increases from \$0 to \$2.67, \$2.67 to \$5.40, and \$5.40 to \$8.13 are 0.04, 0.19, and 2.10, respectively. Comparing the arc elasticity in a zero-price setting to those in other settings could be problematic because the denominator, $(P_a - P_b) / (P_a + P_b)$ is always 1 if P_b =0. Moreover, people tend to treat a zero price not only as a decrease in cost but also as an extra benefit (Shampanier et al., 2007). These results must be interpreted with this caveat.

planation for this finding is the framing of the price of health insurance. Unlike Thornton (2008) and Dupas (2014), our subsidy intervention focuses on the level of subsidy instead of the level of price, and, therefore, the largest response to the intervention is found between zero and a small (one-third) subsidy.

2.5.2 Selection into Health Insurance

Selection into social health insurance could have important implications for the financial sustainability of the program, especially when mandates are not enforceable, in that people who are more ill or those with larger health care service utilization could be more likely to select into the program.

We first show evidence of selective retention in health insurance by level of health care service utilization. Those who have larger health care service utilization are more likely to remain enrolled in health insurance, as shown by the standardized treatment effects (Panels A and B of Table B.2.2).

To gain further insight on selection into health insurance, we compare the individual and household characteristics of compliers, always-takers, and never-takers. The impacts we estimate are driven by compliers who enroll in health insurance due to our subsidy intervention. Following Almond and Doyle (2011) and Kim and Lee (2017), we calculate the mean characteristics and test the differences among compliers, always-takers, and never-takers.

To do so, we first define a binary variable T, an indicator for whether an individual is assigned to the treatment group (*Subsidy*). Next, we define a binary variable H, an indicator for whether an individual is enrolled in health insurance. Lastly, we define HT as the value H would have if T were either 0 or 1. Hence, $\mathrm{E}(X|H_1 = 1)$ presents the mean value characteristics of treated individuals who enrolled in health insurance. Under the assumption of existence of the first stage, monotonicity, and independence, $\mathrm{E}(X|H_1 = 1)$ can be written as:

$$\begin{aligned} \mathrm{E}(X|H_1 = 1) = & \mathrm{E}(X|H_1 = 1, H_0 = 1) \cdot P(H_0 = 1|H_1 = 1) \\ & + \mathrm{E}(X|H_1 = 1, H_0 = 0) \cdot P(H_0 = 0|H_1 = 1) \end{aligned} \tag{2.3}$$

Equation (2.3) implies that $\mathrm{E}(X|H_1 = 1)$ is a sum of always-takers and compliers components. $\mathrm{E}(X|H_1 = 1, H_0 = 0)$ represents the characteristics of compliers. $\mathrm{E}(X|H_1 = 1, H_0 = 1) = \mathrm{E}(X|H_0 = 1)$ holds from the monotonicity assumption. $P(H_0 = 1)$, the proportion of always-takers, and $P(H_1 = 0)$, the proportion of never-takers, can be directly measured from the sample. $P(H_0 = 1)$, the proportion of always-takers can be thus measured by P_a, the proportion of insurance takers in the control group. Similarly, the proportion of never-takers, $P(H_1 = 0)$, can also be measured by P_b, the proportion of insurance non-takers in the treatment group. The proportion of compliers is 1-P_a-P_b. Therefore, $P(H_0 = 0|H_1 = 1)$ and $P(H_0 = 0|H_1 = 1)$ are $\frac{P_a}{P_c+P_a}$ and $\frac{P_c}{P_c+P_a}$, respectively.[27]

By rearranging Equation (2.3), the characteristic of compliers can be calculated as follows:

$$\mathrm{E}(X|H_1 = 1, H_0 = 0) = \frac{P_c + P_a}{P_c} \times \left[\mathrm{E}(X|H_1 = 1) - \frac{P_a}{P_c + P_a} \times \mathrm{E}(X|H_0 = 1) \right] \tag{2.4}$$

[27]The estimated share of compliers, always-takers, and never-takers are 47.4%, 27.1%, and 25.5% in the short run, and 24.3%, 23.0%, and 52.7% in the long run, respectively.

Table 2.3 presents the summary statistics of the entire sample, compliers, always-takers, and the never-takers for short-run selection (Columns 1 to 4) and long-run selection (Columns 8 to 11). Columns 5 to 7 report the t-statistics for the mean comparison between compliers and always-takers, compliers and never-takers, and always-takers and never-takers in the short run. Columns 12 to 14 report similar statistics in the long run. By comparing compliers and never-takers, we find that our subsidy intervention attracted people who were more ill and had larger health expenditure, especially in the long run (Column 13). For example, compliers were more likely to have limited daily activities in the last four weeks compared to never-takers, and the differences became larger and more significant in the long run.

Next, we explore the selection pattern by level of subsidy by comparing compliers of the full subsidy and partial subsidy intervention. To do so, we restrict the sample to those who were insured in the *Subsidy* treatment group. The reasonable assumption that we impose is that always-takers in the full and partial subsidy groups are the same. Since we restrict our sample to those insured in the treatment group, which consists of compliers and always-takers, any difference between full and partial subsidy groups in the restricted sample is due to the compositional changes of compliers. Table 2.4 presents the results of 24 regressions where we regress each health characteristic on an indicator of full subsidy. The last two rows in Panels A and B report the average standardized effects for health status and health care utilization in the short and long run, respectively. The results show that partial subsidy compliers are more likely to be ill and have larger health care expenditure in the long run but not in the short run.

In summary, we find that, in general, compliers are more ill and have larger health expenditures than never-takers. Among the compliers, those in the partial subsidy group are more ill and have larger health expenditure than those in the full subsidy group. In addition, Tables 2.3 and 2.4 show that the selection patterns are more prominent in the long run, suggesting heterogeneous impacts of interventions on health care utilization by level of subsidy, especially in the long run.

Stronger selection in the partial subsidy group compared to the full subsidy group is not surprising. Those in the partial subsidy group need to pay positive amount of insurance premiums and fees, compared to zero-cost for the full-subsidy group. Those with partial subsidy may enroll in health insurance only if they expect the net gain to be positive, that is, expected benefits of health insurance are greater than the cost. Stronger selection in the long run could be because health insurance is an experience good, a service where product characteristics are easier to observe upon consumption (Nelson, 1970). For example, those with health insurance can afford to make more frequent contacts with medical services. They can collect more private health information and learn more about costs and benefits of health insurance than those without insurance.

2.5.3 Impacts on Health Care Services Utilization

Table 2.5 presents the effects on utilization of health care services in the short run (Columns 1 to 6). Column 6 presents average standardized effects; Panels A and B present ITT and 2SLS results, respectively. We report bootstrap and family-wise p-values in square and curly brackets, respectively. The long-run effects are presented in Table 2.6.

We find that insurance coverage leads to an increase in utilization of health care services in both the short and long runs, which corresponds to the fact that health insurance enrollment is sustained in the long run (Panel A1 of Tables 2.5 and 2.6). It is worth noting that an increase in health care service utilization in the long run is at least as high as that in the short run (Columns 6 of Tables 2.5 and 2.6, respectively) even though the enrollment rate decreased.

We find some interesting results regarding health care utilization by level of subsidy in the long run (Panels A2 and A3 of Table 2.6). Even though the increase in health insurance enrollment is similar across subsidy levels, an increase in health care utilization is only significant for the partial subsidy group, not the full subsidy group. These results suggest that selection could be important for explaining the increase in health care utilization through health insurance promotion.

We also study the impacts on OOP expenses (Column 5). We find limited evidence that health insurance prevents OOP expenses either in the short or long run.[28] There are a few possible explanations for this finding. First, as we de-

[28] Again, the size effects in the short- and long-run are not directly comparable because the short- and long-run OOP expenses are constructed differently. In the short run, respondents were asked about more general OOP expenses, but in the long run, OOP expenses only included those related to the treatment of several important illnesses (e.g., malaria, skin diseases, and acute respiratory infection). Specifically, for the short-run OOP expense, we use the individual's response to the following question: "On (NAME's) most recent visit to a health facility, did he/she pay any money from his/her own pocket at a health facility in the last six months?" On the other hand, to construct the long-run OOP expense, we use information on whether individuals made positive OOP expenses in each illness episode (i.e., malaria, acute respiratory infection, and skin diseases) that occurred in the last six months.

scribed earlier, most services are free under the NHIS, but health care providers often charge unauthorized fees as copayments. Second, medicine is often in short supply at the public health centers, and those who receive a diagnosis may purchase medicine from a private pharmacy. Third, those without health insurance often use traditional or herbal medicine which is inexpensive, and therefore, substitution from traditional medicine to formal health care does not decrease OOP expenses.

2.5.4 Impacts on Health Status

Table 2.7 presents the effects on health status in the short run (Columns 1 to 5). Column 5 presents average standardized effects. The long-run effects are presented in Table 2.8. Panel A1 of Table 2.7 shows that insurance coverage improves health status in the short run. However, Panel A1 of Table 2.8 shows that short-run positive health effect seems to disappear in the long run even though health insurance enrollment and health care service utilization continue to increase, as shown in Tables 2.2, 2.5, and 2.6. Panel B, which shows the 2SLS results, confirms a similar pattern: the emergence of short-run positive health effects (although most are not statistically significant) dissipate in the long run. We even find negative health effects on the number of sick days and daily activities in the long run (Columns 2 to 4 of Table 2.8). The negative health consequences in the long run are mainly driven by those in the partial subsidy group (Columns 2 to 4 of Panels A2 and A3 of Table 2.8) who also experienced an increase in health care utilization.[29]

[29]To help shed light on the lack of long-run health outcomes, we investigate individuals' health behaviors – 12 years old or older – regarding the use of bed nets and safe water tech-

Negative health consequences despite increased health care utilization in the long run appears contradictory. However, given the subjective nature of our health status outcomes, these results are not surprising. This could happen when people make frequent contacts with health facilities. Those who experience health care services could learn about the specific symptoms of illnesses, and thus become more sensitive about their health status (Dow et al., 1997; Finkelstein et al., 2012). Also, those who receive a diagnosis could be more aware of the times or periods they were sick. As a result, they are more likely to report being ill. Unfortunately, we are unable to test these conjectures in our data. More research is needed to verify more precise mechanisms through which health insurance enrollment and health care utilization may result in a decline in self-reported health status.

2.6 Conclusion

This study examines the long-term consequences of one-time short-run subsidy interventions on health insurance enrollment, health care service utilization, and self-reported health outcomes in the long run, especially when mandates are not enforceable. In addition, we study the role of pricing in health insurance by measuring important behavioral responses to different levels of subsidy (i.e., one-third, two-thirds, and full subsidy). In Northern Ghana, we implement three randomized subsidy interventions to promote health insurance enrollment. We then use the resulting variation in insurance coverage to esti-

nologies. We find some suggestive evidence on the decrease in the overall health investments in the full subsidy group, which is not consistent with the results in health utilization and status (Column 5 of Table B.2.3).

mate the effect of insurance coverage on utilization of health care services, OOP expenses, and health status and behaviors.

We highlight three main findings. First, our interventions significantly promoted enrollment in the short run, and while the impacts attenuate, the positive impacts remained three years after the initial intervention implementation. Specifically, those treated with one-third, two-thirds, and full subsidies were 39.3, 48.3, and 53.8 percentage points, respectively, more likely to enroll health insurance in the short run, and 17.3, 14.0, and 18.9 percentage points, respectively, more likely to enroll in the long run.

Second, we find evidence of selection, especially in the long run. Compliers are more ill and have larger health expenditures than never-takers, and this pattern is more prominent in the long run. Among compliers, individuals in the partial subsidy group are particularly more ill and have larger health expenditures than those in the full subsidy group. As a result, health care expenditures of the partial subsidy group, who more selectively enrolled in health insurance, increases in the long run, even though health insurance enrollment rates are similar across levels of subsidy. Third, we do not find evidence of improvement in self-reported health status despite the increase in health care utilization in the long run.

Critics of the Ghanaian NHIS have argued that the scheme is overly generous and financially unsustainable because of the huge percentage of NHIS members under premium exemption without co-payment (Alhassan et al., 2016). Policy makers should be cautious of the presence of selection and behavioral responses since they are often difficult to predict and, importantly, may endanger financial stability of an insurance program, especially when mandates are not enforceable.

Taken together, these findings highlight that even though short-run interventions successfully increase health insurance enrollment, their long-run success in improving health status could depend on behavioral responses such as selection. Our findings suggest that as health insurance continues to be introduced in developing counties, both careful enforcement of mandatory health insurance enrollment to prevent selection and establishment of policies to encourage desirable health behaviors need to be considered.

2.7 Figures and Tables

2.7.1 Figures

Figure 2.1: Study Design

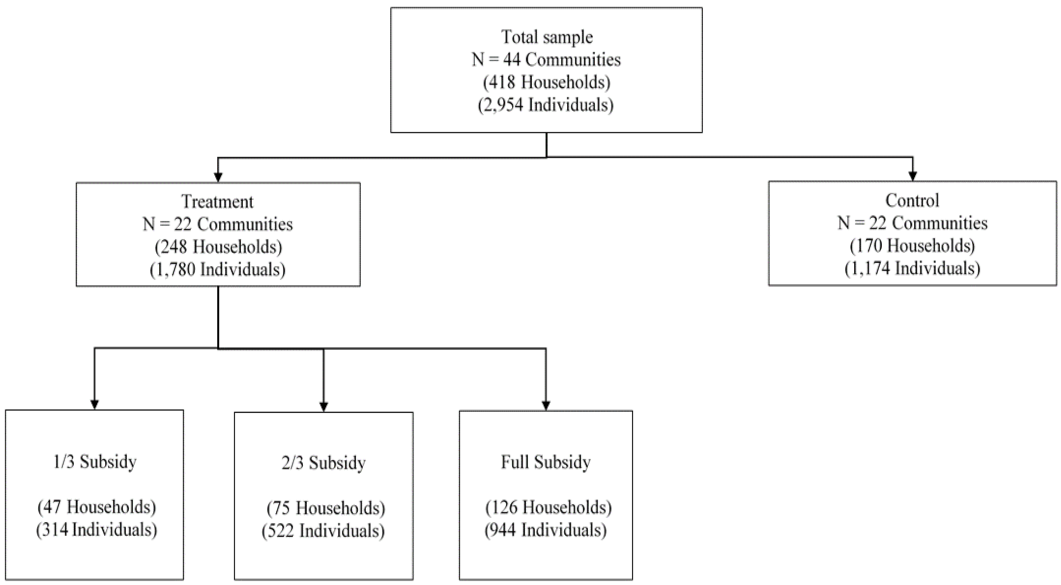
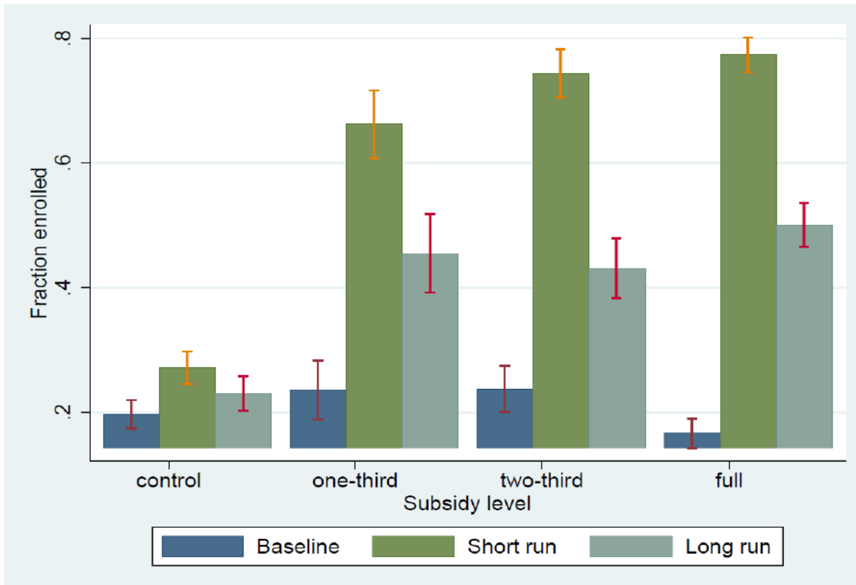


Figure 2.2: Enrollment Rate by Subsidy Level at Baseline, Short Run, and Long Run



Note: This figure shows means of enrollment rates of each subsidy-level group at baseline, short run, and long run. Sample includes those who received subsidy and the control group. The vertical lines indicate 95% confidence intervals.

2.7.2 Tables

Table 2.1: Baseline Characteristics and Balance Check

Variable	Mean		Difference between subsidy level and control				N	Difference between each subsidy level		
	Full	Control	One-third	Two-third	Full	p-value		One-third vs Two-third	One-third vs Full	Two-third vs Full
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Panel A: Individual Characteristics										
Age	23.780	24.310	1.180	-0.775	-1.620	0.390	2,954	1.955	2.800	0.844
Male	0.481	0.475	0.009	-0.010	0.022	0.578	2,954	0.019	-0.013	-0.031
Christian	0.417	0.373	0.073	0.102	0.058	0.801	2,954	-0.029	0.015	0.044
Dagaaba (ethnic group)	0.517	0.458	0.153	0.208	0.017	0.370	2,954	-0.055	0.136	0.191
Has some formal education	0.335	0.337	-0.022	-0.015	0.009	0.976	2,954	-0.007	-0.031	-0.025
Has a health condition ($\geq$ 6 months)	0.071	0.072	-0.002	-0.002	-0.004	0.996	2,954	-0.001	0.001	0.002
Probably sick next year	0.441	0.436	0.030	0.023	-0.005	0.809	2,845	0.007	0.035	0.028
Overall illness										
Ill in the last month	0.123	0.105	0.039	0.048	0.016	0.532	2,954	-0.010	0.023	0.032
No. of days ill in the last month	0.918	0.846	0.505	0.208	-0.056	0.565	2,927	0.296	0.560	0.264
Could not do normal activities in the last month	0.076	0.060	0.011	0.039	0.023	0.428	2,919	-0.028	-0.012	0.016
No. of days could not perform normal activities in the last month	0.544	0.480	0.134	0.138	0.079	0.867	2,815	-0.004	0.055	0.060
Malaria										
Ill in the last month	0.046	0.041	-0.006	0.028	0.004	0.483	2,931	-0.034	-0.010	0.024
No. of days ill in the last month	0.243	0.220	-0.049	0.182	-0.011	0.721	2,909	-0.231	-0.037	0.194
Could not do normal activities in the last month	0.025	0.018	-0.002	0.023	0.011	0.431	2,919	-0.025	-0.013	0.012
No. of days could not perform normal activities in the last month	0.146	0.128	-0.036	0.056	0.036	0.693	2,815	-0.092	-0.072	0.020
Visited health facility in the last month	0.039	0.036	0.033	0.023	-0.015	0.301	2,435	0.010	0.048	0.038
Visited health facility in the last six months	0.074	0.074	0.025	0.008	-0.014	0.639	2,954	0.016	0.038	0.022
Number of visits in the last month	0.066	0.063	0.062	0.042	-0.036	0.117	2,443	0.020	0.098	0.078*
Visited health facility in the last month for malaria treatment	0.010	0.011	-0.004	0.002	-0.004	0.925	2,435	-0.006	-0.0005	0.006
Made out of pocket expense in the last six months	0.136	0.133	-0.009	0.059	-0.021	0.306	2,954	-0.067	0.012	0.079*
Ever enrolled in NHIS	0.358	0.302	0.179**	0.084	0.071	0.090*	2,954	0.096	0.108	0.012
Currently enrolled in NHIS	0.198	0.197	0.039	0.041	-0.030	0.690	2,954	-0.002	0.069	0.071
Slept under mosquito nets (12 years old or older)	0.501	0.448	0.192**	0.140	0.025	0.111	1,720	0.053	0.168	0.115
Use safe drinking water technology (12 years old or older)	0.024	0.039	-0.039	-0.019	-0.020	0.109	1,286	-0.020	-0.020	0.001
Panel B: Household Characteristics										
HH Size	8.703	8.454	-0.187	0.051	0.813	0.517	2,953	-0.238	-0.999	-0.761
Number of children under 18	5.141	4.952	0.054	-0.125	0.641	0.578	2,954	0.179	-0.587	-0.766
Owns farming land	0.553	0.506	0.118	-0.007	0.112	0.363	2,674	0.125	0.006	-0.119
Owns mosquito net	0.711	0.690	0.020	0.146	-0.032	0.139	2,477	-0.125*	0.052	0.177*
Household assets (principal component score)	0.601	0.266	0.580	0.269	0.705**	0.061*	2,953	0.311	-0.126	-0.436
Panel C: Community Characteristics										
Distance to NHIS regist (km)	20.010	20.370	4.347	3.447	-4.466	0.303	2,954	0.900	8.812	7.912
Distance to health facility (km)	5.394	5.166	0.221	-0.687	1.017	0.149	2,954	0.908	-0.796	-1.704**

Note: Columns 1 and 2 report mean of all respondents and control group. Columns 3 to 5 present results from regressions of each variable on control and subsidy level indicators (1/3, 2/3, and full). Column 6 reports the p-value from a joint test of equality of the three coefficients reported in Columns 3 to 5. Column 7 reports total number of observations. Columns 8 to 10 present results from separate regressions of each variable on one-third and two-thirds subsidy levels (Column 8), on one-third and full subsidy levels (Column 9), and on two-thirds and full subsidy levels (Column 10). Robust standard errors are clustered at community level. *, **, and *** denote statistical significance at 10 %, 5 %, and 1 %. levels, respectively.

Table 2.2: Effects of Subsidy on NHIS Enrollment

	Enrollment	
	Short-run	Long-run
	(1)	(2)
Panel A		
Any Subsidy	0.436***	0.206***
	(0.048)	(0.059)
	[0.000]	[0.007]
R-squared	0.342	0.160
Panel B		
Partial subsidy (positive price)	0.444***	0.154*
	(0.054)	(0.079)
	[0.000]	[0.094]
Full subsidy (free)	0.530***	0.192*
	(0.060)	(0.097)
	[0.000]	[0.117]
R-squared	0.351	0.183
Panel C		
1/3 subsidy	0.393***	0.173**
	(0.072)	(0.083)
	[0.001]	[0.080]
2/3 subsidy	0.483***	0.140
	(0.060)	(0.086)
	[0.000]	[0.153]
Full subsidy (free)	0.538***	0.189*
	(0.057)	(0.097)
	[0.000]	[0.108]
R-squared	0.353	0.183
Number of observations	2,785	2,304
Mean	0.555	0.380
Control group mean	0.272	0.230
P-values on test of equality:		
Partial subsidy = Full subsidy	0.097	0.525
1/3 subsidy = 2/3 subsidy	0.196	0.604
1/3 subsidy = Full subsidy	0.016	0.807
2/3 subsidy = Full subsidy	0.339	0.481

Note: All regressions include a standard set of covariates (individual, household, and community) and baseline measure of dependent variable. P-values for the equality of effect estimates for various pairs of treatment groups are also presented. Robust standard errors clustered at community level are reported in parentheses. Wild-cluster bootstrap-t p-values are reported in square brackets. *, **, and *** denote statistical significance at 10 %, 5 %, and 1 % levels, respectively.

Table 2.3: Characteristics of Compliers, Always Takers, and Never Takers

	Short-run							Long-run						
	Mean				t-stat			Mean				t-stat		
	Total	Complier	Always	Never	C=A	C=N	A=N	Total	Complier	Always	Never	C=A	C=N	A=N
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
Proportion	100	47.4	27.1	25.5				100	24.3	23.0	52.7			
Panel A: Individual Characteristics														
Age	23.78	24.34	20.48	24.39	3.61	-0.05	-2.58	23.78	18.90	21.46	27.08	-1.79	-10.12	-3.43
Male	0.48	0.51	0.47	0.48	1.18	1.17	-0.15	0.48	0.51	0.44	0.51	1.90	-0.04	-1.71
Christian	0.42	0.43	0.51	0.40	-2.74	1.24	2.90	0.42	0.37	0.53	0.42	-4.58	-2.38	3.00
Dagaaba (ethnic group)	0.52	0.61	0.54	0.44	2.14	6.89	2.79	0.52	0.59	0.53	0.51	1.70	4.39	0.51
Has some formal education	0.34	0.38	0.35	0.26	0.81	5.61	2.78	0.34	0.42	0.35	0.30	2.16	7.61	1.43
Has a health condition ($\geq$ 6 months)	0.07	0.08	0.05	0.06	2.09	1.37	-0.61	0.07	0.04	0.07	0.09	-1.57	-4.62	-0.96
Probably sick next year	0.44	0.44	0.47	0.45	-1.99	-1.56	0.62	0.44	0.41	0.46	0.45	-2.62	-4.34	0.56
Illness														
Ill in the last four weeks	0.12	0.13	0.11	0.14	1.21	-0.13	-0.98	0.12	0.14	0.17	0.11	-1.17	3.05	2.26
No. of days ill in the last four weeks	0.92	1.03	0.75	0.89	1.62	0.91	-0.58	0.92	0.85	1.05	0.93	-0.73	-0.57	0.38
Could not do normal activities in the last four weeks	0.08	0.10	0.04	0.08	5.80	1.93	-2.28	0.08	0.10	0.10	0.07	0.23	3.01	1.04
No. of days could not perform normal activities in the last four weeks	0.54	0.74	0.33	0.43	3.39	3.96	-0.69	0.54	0.42	0.68	0.57	-1.18	-1.39	0.47
Illness due to Malaria														
Ill in the last four weeks	0.05	0.05	0.05	0.04	-0.40	0.63	0.70	0.05	0.07	0.08	0.03	-0.48	7.78	2.69
No. of days ill in the last four weeks	0.24	0.29	0.32	0.16	-0.30	3.12	1.63	0.24	0.34	0.53	0.14	-1.05	4.16	2.05
Could not do normal activities in the last four weeks	0.03	0.04	0.02	0.02	3.23	2.16	-0.71	0.03	0.07	0.03	0.01	3.64	14.82	1.29
No. of days could not perform normal activities in the last four weeks	0.15	0.17	0.16	0.13	0.16	1.05	0.40	0.15	0.30	0.31	0.05	-0.10	13.68	1.51
Visited health facility in the last four weeks	0.04	0.04	0.05	0.03	-0.43	1.52	1.14	0.04	0.04	0.06	0.03	-1.31	1.53	1.74
Visited health facility in the last six months	0.07	0.07	0.09	0.06	-1.16	0.73	1.37	0.07	0.05	0.13	0.05	-3.40	-0.26	3.12
Number of visits in the last four weeks	0.07	0.06	0.08	0.05	-0.80	0.81	1.07	0.07	0.02	0.10	0.06	-2.24	-2.78	0.99
Visited health facility in the last four weeks for malaria treatment	0.01	0.01	0.01	0.01	0.43	1.73	0.50	0.01	0.01	0.03	0.00	-1.19	8.31	2.13
Made out of pocket expense in the last six months	0.14	0.15	0.12	0.14	1.55	0.89	-0.57	0.14	0.15	0.16	0.13	-0.20	1.99	1.03
Ever enrolled in NHIS	0.36	0.30	0.65	0.30	-12.98	0.23	10.02	0.36	0.40	0.41	0.40	-0.22	-0.25	0.08
Currently enrolled in NHIS	0.20	0.05	0.47	0.15	-14.99	-6.38	9.39	0.20	0.14	0.30	0.17	-5.09	-2.34	3.79
Slept under mosquito nets (12 years old or older)	0.50	0.60	0.45	0.49	3.64	3.27	-0.80	0.50	0.62	0.53	0.51	1.76	4.80	0.44
Panel B: Household Characteristics														
HH Size	8.70	9.35	8.34	8.57	5.11	5.41	-0.92	8.70	10.18	7.99	8.86	12.989	12.29	-4.07
Number of children under 18	5.14	5.42	5.17	5.08	1.37	2.77	0.39	5.14	5.84	5.12	5.26	3.2613	6.10	-0.57
Male head HH	0.83	0.84	0.87	0.87	-0.46	-0.54	0.02	0.83	0.76	0.92	0.85	-2.699	-2.98	1.04
Owns farming land	0.55	0.69	0.48	0.53	7.08	6.45	-1.24	0.55	0.79	0.48	0.56	8.6766	13.45	-1.94
Owns mosquito net	0.71	0.74	0.73	0.67	0.39	2.86	1.37	0.71	0.52	0.91	0.74	-18.36	-18.15	6.04
Knowledge about NHIS	0.59	0.57	0.61	0.61	-2.00	-2.34	0.00	0.59	0.59	0.60	0.59	-0.351	-0.21	0.25
Household assets (principal component score)	0.60	1.09	0.77	0.59	2.56	5.69	1.19	0.60	2.29	0.16	0.59	23.213	71.18	-3.50

Note: This table presents the mean individual (Panel A) and household (Panel B) characteristics of the entire sample, compliers and always takers, and never takers. The mean characteristics of compliers are estimated from Equation (2.4). Columns 5-7 and 12-14 present the t-statistics from the two-sample t-test comparing compliers with always takers, compliers with never takers, and always takers with never takers, respectively.

Table 2.4: Characteristics of Compliers, Always Takers, and Never Takers

Sample	Among those enrolled in the short run				
Independent variable: Received full subsidy	Coefficient	Standard error	bootstrap p-values	N	R-squared
	(1)	(2)	(3)	(4)	(5)
Panel A: Short run					
Healthy or very healthy	-0.022	(0.027)	0.376	413	0.037
# Days ill last month	-0.034	(0.092)	0.775	1,238	0.005
Could not perform normal daily activities due to illness last month	-0.007	(0.019)	0.741	1,244	0.010
# days could not perform normal daily activities in the last month	-0.008	(0.262)	0.978	1,244	0.003
# Days ill last month (Malaria)	-0.045	(0.034)	0.257	1,237	0.011
Could not perform normal daily activities due to illness last month (Malaria)	-0.006	(0.011)	0.587	1,238	0.009
# days could not perform normal daily activities in the last month (Malaria)	-0.031	(0.107)	0.800	1,238	0.003
Visited health facility in last four weeks	-0.010	(0.017)	0.600	1,152	0.017
Visited health facility in last six months	-0.001	(0.027)	0.978	1,223	0.025
# of visits in last six months	0.004	(0.012)	0.794	1,148	0.010
Visited Facility for malaria treatment in the last four weeks	-0.018	(0.011)	0.169	1,200	0.008
Made an out-of-pocket for health service in the last six months	-0.006	(0.016)	0.754	1,244	0.008
Standardized treatment effects (health status)	-0.003	(0.005)		7,852	0.006
Standardized treatment effects (health care utilization)	-0.007	(0.008)		5,967	0.009
Sample	Among those enrolled in the long run				
Independent variable: Received full subsidy	Coefficient	Standard error	bootstrap p-values	N	R-squared
	(1)	(2)	(3)	(4)	(5)
Panel B: Long run					
Healthy or very healthy	0.210*	(0.103)	0.117	174	0.078
# Days ill last month	-1.106***	(0.338)	0.012	674	0.049
Could not perform normal daily activities due to illness last month	-0.085*	(0.044)	0.117	674	0.027
# days could not perform normal daily activities in the last month	-0.730*	(0.411)	0.066	674	0.033
# Days ill last month (Malaria)	-0.613***	(0.206)	0.032	674	0.037
Could not perform normal daily activities due to illness last month (Malaria)	-0.079**	(0.033)	0.034	674	0.034
# days could not perform normal daily activities in the last month (Malaria)	-0.522**	(0.222)	0.023	674	0.037
Visited health facility in last four weeks	-0.122***	(0.034)	0.018	674	0.044
Visited health facility in last six months	-0.242***	(0.075)	0.033	674	0.088
# of visits in last six months	-0.099**	(0.041)	0.080	674	0.033
Visited Facility for malaria treatment in the last four weeks	-0.090**	(0.037)	0.073	674	0.033
Made an out-of-pocket for health service in the last six months	-0.034	(0.023)	0.152	674	0.018
Standardized treatment effects (health status)	-0.076**	(0.031)		4,218	0.032
Standardized treatment effects (health care utilization)	-0.116***	(0.034)		3,370	0.038

Note: This table reports estimation results of running regression of each selected health characteristics on an indicator variable that takes value of one if receiving full subsidy (zero price) and zero if receiving partial subsidy (positive price). We control for indicators of other interventions involving subsidy: *Subsidy + Campaign*, *Subsidy + Convenience*, and *Subsidy + Campaign + Convenience*. Sample is restricted to those who received partial and full subsidy. Panel A summarizes regression results when sample is restricted to those who enrolled in the short run. Panel B summarizes results when sample is restricted to those who enrolled in the long run. Standardized treatment effects on health status and health care utilization in the short and long run are reported in the last two rows of Panels A and B, respectively. Robust standard errors clustered at community level reported in parantheses. Wild-cluster bootstrap-t p-values are reported in Column 3. *, **, and *** denote statistical significance at 10 %, 5 %, and 1 % level respectively.

Table 2.5: Effects on Healthcare Services Utilization (Short Run)

	Short run					
	Visited health facility in last four weeks	Visited health facility in last six months	# of visits in last four weekss	Visited facility for malaria treatment in the last four weeks	Made out-of-pocket for health service in the last six months	Standardized treatment effects
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: ITT results						
Panel A1						
Any subsidy	0.020	0.053**	0.031	0.018**	0.011	0.021*
	(0.017)	(0.021)	(0.022)	(0.008)	(0.013)	(0.011)
	[0.294]	[0.035]	[0.247]	[0.069]	[0.457]	
	{0.500}	{0.202}	{0.475}	{0.254}	{0.500}	
R-squared	0.099	0.119	0.062	0.066	0.092	0.054
Panel A2						
Partial subsidy	-0.008	0.009	-0.003	0.008	-0.009	-0.0001
	(0.013)	(0.019)	(0.024)	(0.009)	(0.015)	(0.010)
	[0.591]	[0.623]	[0.883]	[0.387]	[0.598]	
	{0.944}	{0.944}	{0.944}	{0.869}	{0.944}	
Full subsidy	0.008	0.001	0.007	0.001	-0.013	0.004
	(0.022)	(0.031)	(0.038)	(0.012)	(0.023)	(0.016)
	[0.716]	[0.963]	[0.875]	[0.973]	[0.585]	
	{0.990}	{0.998}	{0.998}	{0.998}	{0.979}	
R-squared	0.106	0.129	0.065	0.074	0.094	0.058
Panel A3						
1/3 subsidy	-0.0003	0.009	-0.012	0.009	-0.015	0.001
	(0.016)	(0.022)	(0.023)	(0.011)	(0.014)	(0.011)
	[0.974]	[0.668]	[0.629]	[0.464]	[0.325]	
	{0.986}	{0.918}	{0.918}	{0.881}	{0.803}	
2/3 subsidy	-0.014	0.009	0.004	0.007	-0.004	-0.001
	(0.014)	(0.023)	(0.030)	(0.010)	(0.020)	(0.011)
	[0.390]	[0.697]	[0.901]	[0.480]	[0.842]	
	{0.851}	{0.965}	{0.974}	{0.898}	{0.974}	
Full subsidy	0.007	0.001	0.008	0.0004	-0.012	0.004
	(0.021)	(0.031)	(0.038)	(0.011)	(0.023)	(0.016)
	[0.746]	[0.962]	[0.830]	[0.970]	[0.678]	
	{0.996}	{0.998}	{0.996}	{0.998}	{0.984}	
R-squared	0.106	0.129	0.065	0.074	0.094	0.058
Number of observations	2,130	2,710	2,124	2,252	2,805	11,008
Panel B: 2SLS results						
Enrolled in NHIS	-0.007	0.011	0.005	0.011	-0.019	0.002
	(0.024)	(0.041)	(0.050)	(0.015)	(0.035)	(0.021)
First-stage F-statistics	26.646	25.134	25.559	26.421	26.608	27.884
Control group mean	0.038	0.102	0.032	0.019	0.046	-0.011
P-values on test of equality:						
Partial subsidy = Full subsidy	0.491	0.751	0.783	0.543	0.825	0.784
1/3 subsidy = 2/3 subsidy	0.419	0.987	0.500	0.893	0.515	0.819
1/3 subsidy = Full subsidy	0.743	0.786	0.560	0.480	0.821	0.854
2/3 subsidy = Full subsidy	0.437	0.769	0.908	0.625	0.710	0.770

Note: Panels A and B report ITT and 2SLS results, respectively. Panels A1, A2, and A3 report the effects of receiving any subsidy, partial and full subsidy, and each subsidy level (1/3, 2/3, and full), respectively. All regressions include a standard set of covariates (individual, household, and community) and baseline measure of dependent variable. Standardized treatment effects are reported in Column 6. *P*-values for the equality of effect estimates for various pairs of treatment groups are also presented. Robust standard errors clustered at community level are reported in parentheses. Wild-cluster bootstrap-t *p*-values are reported in square brackets. Family-wise *p*-values are reported in curly brackets. *, **, and *** denote statistical significance at 10 %, 5 %, and 1 % level respectively.

Table 2.6: Effects on Healthcare Services Utilization (Long Run)

	Long run					
	Visited health facility in last four weeks	Visited health facility in last six months	# of visits in last four weekss	Visited facility for malaria treatment in the last four weeks	Made out-of-pocket for health service in the last six months	Standardized treatment effects
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: ITT results						
Panel A1						
Any subsidy	0.038***	0.079***	0.033***	0.028***	0.009	0.038***
	(0.009)	(0.018)	(0.010)	(0.010)	(0.008)	(0.010)
	[0.000]	[0.000]	[0.009]	[0.022]	[0.378]	
	{0.018}	{0.012}	{0.054}	{0.077}	{0.375}	
R-squared	0.077	0.084	0.060	0.064	0.087	0.062
Panel A2						
Partial subsidy	0.048***	0.108***	0.038***	0.035**	0.013	0.048***
	(0.013)	(0.025)	(0.012)	(0.014)	(0.010)	(0.012)
	[0.001]	[0.001]	[0.004]	[0.024]	[0.256]	
	{0.062}	{0.033}	{0.080}	{0.151}	{0.287}	
Full subsidy	-0.029	-0.013	-0.033	-0.035	-0.038*	-0.037
	(0.020)	(0.053)	(0.023)	(0.021)	(0.022)	(0.023)
	[0.222]	[0.849]	[0.243]	[0.201]	[0.057]	
	{0.630}	{0.854}	{0.630}	{0.630}	{0.630}	
R-squared	0.094	0.102	0.078	0.084	0.103	0.079
Panel A3						
1/3 subsidy	0.020	0.085**	0.016	0.019	0.025	0.036*
	(0.015)	(0.034)	(0.017)	(0.016)	(0.027)	(0.021)
	[0.297]	[0.057]	[0.441]	[0.328]	[0.719]	
	{0.603}	{0.410}	{0.681}	{0.615}	{0.681}	
2/3 subsidy	0.070***	0.125***	0.056***	0.048***	0.004	0.058***
	(0.016)	(0.032)	(0.017)	(0.018)	(0.011)	(0.015)
	[0.000]	[0.001]	[0.010]	[0.019]	[0.794]	
	{0.029}	{0.041}	{0.058}	{0.114}	{0.812}	
Full subsidy	-0.025	-0.010	-0.030	-0.033	-0.039	-0.035
	(0.020)	(0.054)	(0.024)	(0.021)	(0.024)	(0.024)
	[0.319]	[0.913]	[0.265]	[0.249]	[0.100]	
	{0.647}	{0.894}	{0.647}	{0.647}	{0.647}	
R-squared	0.099	0.103	0.081	0.086	0.105	0.080
Number of observations	2,228	2,688	2,231	2,228	2,688	11,140
Panel B: 2SLS results						
Enrolled in NHIS	0.051	0.145**	0.038	0.031	-0.017	0.041
	(0.033)	(0.061)	(0.031)	(0.033)	(0.020)	(0.032)
First-stage F-statistics	33.381	34.796	32.094	31.355	32.844	35.857
Control group mean	0.017	0.050	0.036	0.010	0.013	-0.021
P-values on test of equality:						
Partial subsidy = Full subsidy	0.000	0.010	0.004	0.002	0.085	0.001
1/3 subsidy = 2/3 subsidy	0.015	0.355	0.110	0.177	0.539	0.417
1/3 subsidy = Full subsidy	0.068	0.092	0.093	0.049	0.197	0.063
2/3 subsidy = Full subsidy	0.000	0.006	0.003	0.001	0.019	0.000

Note: Panels A and B report ITT and 2SLS results, respectively. Panels A1, A2, and A3 report the effects of receiving any subsidy, partial and full subsidy, and each subsidy level (1/3, 2/3, and full), respectively. All regressions include a standard set of covariates (individual, household, and community) and baseline measure of dependent variable. Standardized treatment effects are reported in Column 6. *P*-values for the equality of effect estimates for various pairs of treatment groups are also presented. Robust standard errors clustered at community level are reported in parentheses. Wild-cluster bootstrap-t *p*-values are reported in square brackets. Family-wise *p*-values are reported in curly brackets. *, **, and *** denote statistical significance at 10 %, 5 %, and 1 % level respectively.

Table 2.7: Effects on Health Status (Short Run)

	Short run				
	Healthy or very healthy	# of days ill last four weeks	Could not perform normal daily activities due to illness last four weeks	# of days could not perform normal daily activities due to illness in the last four weeks	Standardized treatment effects
	(1)	(2)	(3)	(4)	(5)
Panel A: ITT results					
Panel A1					
Any subsidy	0.059	-0.275**	0.007	-0.355	-0.018
	(0.042)	(0.116)	(0.017)	(0.280)	(0.013)
	[0.182]	[0.019]	[0.743]	[0.289]	
	{0.501}	{0.179}	{0.741}	{0.501}	
R-squared	0.176	0.085	0.079	0.092	0.063
Panel A2					
Partial subsidy	0.130***	-0.309**	-0.012	-0.076	-0.025*
	(0.037)	(0.135)	(0.017)	(0.368)	(0.013)
	[0.003]	[0.036]	[0.567]	[0.850]	
	{0.035}	{0.145}	{0.717}	{0.865}	
Full subsidy	0.118**	-0.409*	-0.021	-0.511	-0.041*
	(0.044)	(0.211)	(0.030)	(0.508)	(0.022)
	[0.009]	[0.071]	[0.527]	[0.357]	
	{0.099}	{0.222}	{0.543}	{0.488}	
R-squared	0.192	0.086	0.080	0.094	0.064
Panel A3					
1/3 subsidy	0.121***	-0.399**	-0.011	-0.383	-0.034**
	(0.041)	(0.158)	(0.023)	(0.411)	(0.017)
	[0.011]	[0.033]	[0.68]	[0.408]	
	{0.071}	{0.094}	{0.688}	{0.546}	
2/3 subsidy	0.136***	-0.241	-0.013	0.166	-0.018
	(0.044)	(0.174)	(0.020)	(0.429)	(0.015)
	[0.011]	[0.245]	[0.623]	[0.773]	
	{0.090}	{0.540}	{0.741}	{0.757}	
Full subsidy	0.119***	-0.395*	-0.021	-0.453	-0.040*
	(0.044)	(0.214)	(0.030)	(0.508)	(0.022)
	[0.01]	[0.115]	[0.534]	[0.425]	
	{0.097}	{0.256}	{0.554}	{0.554}	
R-squared	0.192	0.086	0.080	0.095	0.064
Number of observations	861	2,768	2,775	2,677	8,824
Panel B: 2SLS results					
Enrolled in NHIS	0.238***	-0.691**	-0.039	-0.536	-0.067**
	(0.074)	(0.311)	(0.037)	(0.806)	(0.028)
First-stage F-statistics	22.737	26.609	27.019	26.858	27.464
Control group mean	0.817	0.617	0.081	1.379	-0.019
P-values on test of equality:					
Partial subsidy = Full subsidy	0.723	0.477	0.710	0.179	0.278
1/3 subsidy = 2/3 subsidy	0.722	0.409	0.921	0.212	0.400
1/3 subsidy = Full subsidy	0.970	0.979	0.701	0.848	0.731
2/3 subsidy = Full subsidy	0.664	0.333	0.764	0.100	0.221

Note: Panels A and B report ITT and 2SLS results, respectively. Panels A1, A2, and A3 report the effects of receiving any subsidy, partial and full subsidy, and each subsidy level (1/3, 2/3, and full), respectively. All regressions include a standard set of covariates (individual, household, and community) and baseline measure of dependent variable. Standardized treatment effects are reported in Column 5. *P*-values for the equality of effect estimates for various pairs of treatment groups are also presented. Robust standard errors clustered at community level are reported in parentheses. Wild-cluster bootstrap-t *p*-values are reported in square brackets. Family-wise *p*-values are reported in curly brackets. *, **, and *** denote statistical significance at 10 %, 5 %, and 1 % level respectively.

Table 2.8: Effects on Health Status (Long Run)

	Long run				
	Healthy or very healthy	# of days ill last four weeks	Could not perform normal daily activities due to illness last four weeks	# of days could not perform normal daily activities due to illness in the last four weeks	Standardized treatment effects
	(1)	(2)	(3)	(4)	(5)
Panel A: ITT results					
Panel A1					
Any subsidy	-0.076	0.221**	0.030***	0.178**	0.036***
	(0.050)	(0.092)	(0.010)	(0.074)	(0.012)
	[0.166]	[0.035]	[0.009]	[0.028]	
	{0.194}	{0.132}	{0.055}	{0.132}	
R-squared	0.289	0.071	0.084	0.059	0.054
Panel A2					
Partial subsidy	-0.156**	0.294***	0.048***	0.284***	0.058***
	(0.058)	(0.087)	(0.013)	(0.082)	(0.013)
	[0.166]	[0.035]	[0.009]	[0.028]	
	{0.051}	{0.031}	{0.029}	{0.031}	
Full subsidy	-0.130	-0.352*	-0.006	-0.179	-0.028
	(0.093)	(0.175)	(0.020)	(0.185)	(0.027)
	[0.042]	[0.025]	[0.007]	[0.024]	
	{0.566}	{0.379}	{0.820}	{0.604}	
R-squared	0.301	0.083	0.099	0.077	0.068
Panel A3					
1/3 subsidy	-0.081	0.205	0.035**	0.240*	0.042**
	(0.067)	(0.141)	(0.016)	(0.129)	(0.020)
	[0.254]	[0.127]	[0.823]	[0.535]	
	{0.396}	{0.396}	{0.286}	{0.325}	
2/3 subsidy	-0.221***	0.362***	0.058***	0.318***	0.071***
	(0.074)	(0.100)	(0.016)	(0.108)	(0.018)
	[0.29]	[0.212]	[0.063]	[0.123]	
	{0.051}	{0.032}	{0.033}	{0.051}	
Full subsidy	-0.140	-0.339*	-0.004	-0.171	-0.026
	(0.090)	(0.170)	(0.020)	(0.176)	(0.026)
	[0.021]	[0.008]	[0.017]	[0.033]	
	{0.497}	{0.387}	{0.872}	{0.614}	
R-squared	0.307	0.084	0.100	0.077	0.068
Number of observations	658	2,666	2,661	2,564	8,309
Panel B: 2SLS results					
Enrolled in NHIS	-0.306**	0.173	0.067**	0.289	0.066*
	(0.120)	(0.226)	(0.031)	(0.202)	(0.036)
First-stage F-statistics	42.373	34.195	33.371	33.838	33.695
Control group mean	0.791	0.413	0.013	0.096	0.011
P-values on test of equality:					
Partial subsidy = Full subsidy	0.764	0.000	0.010	0.015	0.002
1/3 subsidy = 2/3 subsidy	0.140	0.351	0.278	0.650	0.275
1/3 subsidy = Full subsidy	0.397	0.009	0.089	0.015	0.012
2/3 subsidy = Full subsidy	0.476	0.000	0.008	0.028	0.002

Note: Panels A and B report ITT and 2SLS results, respectively. Panels A1, A2, and A3 report the effects of receiving any subsidy, partial and full subsidy, and each subsidy level (1/3, 2/3, and full), respectively. All regressions include a standard set of covariates (individual, household, and community) and baseline measure of dependent variable. Standardized treatment effects are reported in Column 5. *P*-values for the equality of effect estimates for various pairs of treatment groups are also presented. Robust standard errors clustered at community level are reported in parentheses. Wild-cluster bootstrap-t *p*-values are reported in square brackets. Family-wise *p*-values are reported in curly brackets. *, **, and *** denote statistical significance at 10 %, 5 %, and 1 % level respectively.

CHAPTER 3

THE CONSEQUENCES OF CHILD MARKET WORK ON THE GROWTH OF HUMAN CAPITAL

3.1 Introduction

Child labor is one of the most pressing problems in the developing countries. In 2012 the global number indicates that about 168 million children were in child labor, almost 11 percent of total child population in the world (ILO-IPEC, 2013). More importantly, more than half of them, 85 million children, worked in hazardous sectors. In the literature, attention on child labor has been increasing in the last fifteen years (Edmonds, 2008). He explains that emergence of theoretical works on child labor help generate awareness in this topic, especially on its relation to human capital (Basu and Van, 1998; Baland and Robinson, 2000).

The majority of studies use education attainment or school enrollment as a proxy for human capital (Basu, 1999; Edmonds, 2008). The use of education attainment or school enrollment as a proxy for human capital has two shortcomings. First, they are measures of input into human capital production (Edmonds, 2008; Gunnarsson et al., 2006). The problem is that in countries where school quality is low, input is a poor measure of output, in this case human capital (Dumas, 2012). Secondly, several studies find that the output of the human capital production is a better measure of the level of a country's human capital. These studies also find that the variation in the output provides a better explanation for the variation on personal income and economic growth (Hanushek and Woessmann, 2008; Glewwe, 2002).

Studies in the literature also examine the effect of child labor on health, the second aspect of human capital. However, most use subjective measures of health (Wolff and Maliki, 2008) or objective measures whose trajectory are determined early in life, such as height (Beegle et al., 2009; O'Donnell et al., 2005). Ideally, the health measures used must be objective and could still be affected well into adolescence.

In addition to the difficulties in determining the appropriate outcomes on which the effect of child labor should be estimated upon, the literature has also found conflicting results. Conceptually, the effect of child labor on human capital is ambiguous. On one hand, working can displace schooling. Even in the case where working and schooling go hand-in-hand, the negative effect of working can come through reducing time available for studying, playing, and sleeping (Edmonds and Pavcnik, 2005). On the other hand, child labor may provide household with sufficient income to keep children in school. Indeed, many studies cited in the literature reviews by Basu (1999) and Edmonds (2008) find zero or positive effect of child labor on school enrollment and education attainment.

With regards to health, child labor can impart stress on a young body, as a consequence of contacts with hazardous material, or cause exhaustion (O'Donnell et al., 2005). However, the additional income can be used to maintain the health of children and buy sufficient food. Grootaert and Kanbur (1995) note that if survival depends on work in the informal sector, then the most sensible solution is to take children out from school and put them to work.

In this paper, we estimate the effect of child labor on the accumulation of human capital. Our paper makes several contributions to the literature. First, we measure the effect of child labor on the growth of human capital over a seven-year period using a rich longitudinal dataset from Indonesia. Only few studies in the literature examine the effect of child labor on the growth of human capital (e.g., Beegle et al., 2009; O'Donnell et al., 2005), while most only look at the contemporaneous effect of child labor on human capital due to the general lack of longitudinal dataset in developing countries.

Second, we focus on the output of human capital production: mathematics skills, cognitive skills, and an objective measure of health that may be directly affected by child labor: pulmonary function as measured through lung capacity. We believe this is a better measure of the potential adverse effect of child labor on health, as lung capacity can be affected well into adulthood. Finally, we also include the traditional measure of human capital, education attainment.

Thirdly, the data allow us to begin the initial step in distinguishing the heterogeneous effect of child labor based on whether the work is for wage outside the household or for the household business. This may only address the issue of the human capital effects of hazardous or the worst forms of child labor (Dessy and Pallage, 2005) in a very limited way, but still important given the lack of empirical evidence on this particular type of heterogeneity in the literature thus far.

In this study we use nominal provincial minimum wage as the instrument to treat the endogeneity problem in our estimation. Our 2SLS estimation results show that child labor has significant impacts on the long-term growth of mathematics skills and lung capacity. We find that compared to working in family business, children who are in wage sectors have lower educational attainment. We, however, cannot draw meaningful inferences about other heterogeneities effects of child labor.

We organize the rest of the paper as follows. Section 3.2 describes the datasets used in the paper. Section 3.3 discusses child labor in Indonesia, while Section 3.4 outlays the estimation strategy. Section 3.5 presents the main estimation results, while section 3.6 examines heterogeneities in the effect of child labor. Section 3.7 concludes.

3.2 Data

The first dataset that we use is the National Labor Force Statistics (*Sakernas*), which is an annual, nationally representative, repeated cross-sectional labor force survey that records the activity of individuals older than 10 years old in the sample households. We use *Sakernas* to show the share of children ages 10 – 14 who were engaged in market work between 1986 and 2007. Although less than ideal because *Sakernas* does not record the activities of individuals younger than 10, it is the only nationally representative dataset that allows us to observe the annual child market work trend in Indonesia over the past two decades.

The second dataset is the Indonesia Family Life Survey (IFLS), a longitudinal household survey that began in 1993. Three full follow-up waves were conducted, in 1997, 2000, 2007, and 2014. In this paper, we only use the 2000 and 2007 waves. The first wave represented about 83 percent of Indonesia's 1993 population, and covered 13 of the nation's then 27 provinces. This initial round interviewed roughly 7,200 households. By 2007, the number of households had grown to 13,000 as the survey endeavored to re-interview many members of the original sample that form or join new households. Household attrition is quite low; only around 5 percent of households were lost each wave. Overall, 87.6 percent of households that participated in IFLS1 were interviewed in each of the subsequent three waves (Strauss et al., 2009).

IFLS added a specific child labor module (B5A-DL4) in the 2000 wave. The module was administered to children younger than 15 years old, and recorded market work both inside and outside the household. In addition, the module collected information on the age at which a child worker began working, hours worked in the past week, and wage rate of the children who work outside the household. We define a child work if he or she had engaged in economic work in the past month. The definition of economic work is participation in the production of economic goods and services (Edmonds, 2008). Market work can be conducted both inside and outside the household. In the case of child workers, market work inside the household is usually unpaid.

IFLS also conducted mathematics and cognitive tests to 7-14 year old individuals (EK1) and 15-24 year olds (EK2). The former contains five numeracy problems and 12 shape matching problems, while the latter contains five numeracy problems and eight shape matching problems. The numeracy problems in EK2 are significantly more complex than those in EK1. These modules were first included in 2000. The identical modules were then re-enumerated to individuals in the 2007 survey round, on the following procedure. Individuals who had taken EK1 in 2000 were asked to retake EK1 in 2007. In addition, individuals who were already at least 15 years old in 2007 were also asked to answer EK2. Note that these individuals had been 7-14 years old in 2000 and were around 14-21 years old in 2007. Similarly, individuals who had answered EK2 in 2000 were also asked to work on EK2 in 2007. Finally, EK1 was administered to individuals who were 7-14 years old in 2007. In this paper, we use EK1 results in 2000 and 2007 for individuals who were first tested in 2000.

Given that household surveys in developing countries rarely administer identical tests to the same individuals twice in a seven-year period, IFLS allows us to go beyond most studies by assessing skills accumulation of the same individuals over a relatively long period.

Finally, IFLS measured various health outcomes. In this paper, we use growth in lung capacity as our health measure. We argue that lung capacity, which measures pulmonary function (Lebowitz, 1991) and respiratory health (He et al., 2010; Rojas-Martinez et al., 2007; Schwartz, 1989), is a better measure of health because unlike height, whose trajectory is determined early in life, lung capacity growth can still be adversely affected by low air quality or excessive physical exertion well into adolescence.[1]

The third dataset is the village census (*Podes*), which records infrastructure availability and demographic data of every village in Indonesia. Statistics Indonesia conducted *Podes* three times every decade. We use the dataset collected in 2000 to construct our measures of district level infrastructure availability.

3.3 Child Market Work in Indonesia

Similar to developing countries in general (Edmonds, 2008), child market work in Indonesia is related to poverty (Kis-Katos and Sparrow, 2011; Suryahadi et al.,

[1]IFLS uses a device called peak flow meter, which measures expiratory flow rate. Expiratory flow rate depends on gender, age, and height, and measures how well the lungs are working. Peak flow readings are measured in liters per minute.

2005). We begin this section by presenting the participation rate in market work for children 10-14 from 1986 to 2007. Figure 3.1 shows the participation rate by gender. The rate for males was always higher than females throughout the period, and they exhibited the same pattern. After slightly increasing between 1986 and 1989, child market work participation rate began to decline between 1990 and 1996, during Indonesia's high economic growth period when annual output growth reached close to seven percent and the headcount poverty rate declined from 32 percent to 17 percent (Suryahadi et al., 2009). During this period, the decline in child market work was around 35 percent proportionally for males, from five percent to 3.2 percent, and around 37 percent proportionally for females, from 3.5 to 2.2 percent.

The child market work participation rates then soared to 9.1 percent for males and 6.4 percent for females during the economic crisis in 1997 and 1998. During the same period, the economy contracted by 14 percent in 1998 and remained stagnant in 1999 (Strauss et al., 2004) and headcount poverty rate reached 27 percent in 1999 (Suryahadi et al., 2009). In addition to the dramatic increase in 1997, another notable change in the market work participation pattern is that the rate of increase between 1996 and 1997 was higher for males than females, as shown by the steeper slope between the two years. This was then accompanied by a higher rate of decline for males between 1999 and 2000 as the economy recovered.

Child market work participation rate had continued to decrease between 2000 and 2006, reaching 2.6 percent, before dramatically reversing in 2007. While the participation rate in 2006 was lower than 2000, the rate in 2007 was double the rate in 2006. The explanation does not seem to lie in the economy contracting or an increase in adult unemployment, because the economy grew by 6.3 percent in 2007, higher than in 2006 when growth was six percent, and adult open unemployment rate was lower in 2007 compared to 2006 (Kong and Ramayandi, 2008). While understanding the cause of this trend reversal is important, we leave such endeavor for the future.

The second important issue in child labor is the occupation sector of the child workers. We again use information on sectoral share from Sakernas. Similar to other developing countries as mentioned in (Edmonds and Pavcnik, 2005), the majority of child workers in Indonesia are in agriculture (63 percent in 2000, 62 percent in 2007). Outside the agricultural sector, the next three sectors that employ most of the child workers are manufacturing, trade, and other services. Together, these four sectors employed between 96 and 97 percent of child workers in 2000 and 2007.

Although the occupation sector shares of child workers appear to be relatively constant between 2000 and 2007, we observe considerable heterogeneity in the pattern by gender. Figure 3.2 shows the distribution of child workers by gender in 2000 and 2007 in agriculture, manufacturing, and trade. The share of male child workers in agriculture is significantly higher than the share of female child workers in the sector. The gap was around 15 percentage points in 2000 and has since widened to 25 percentage points by 2007 as female child workers moved out of agriculture and male child workers moved into agriculture. In contrast, there are significantly more female child workers in manufacturing and trade. The share of female child workers in both sectors was almost double that of male child workers in 2000, and the gaps have slightly widened by 2007. Different from the contrasting gender trend in agriculture, however, it appears that both female and male child workers' participation in manufacturing slightly declined, while their participation in trade increased.

The sectoral gender difference is more striking when we examine the rest of the occupation sectors, as shown in Figure 3.3. The largest increase took place in the other services sector, which includes occupations like domestic helper.[2] In 2000, about 2 percent and 3.4 percent of male and female child workers respectively were working in this sector. By 2007, the share for male child workers reached 2.8 percent while the share for female child workers almost tripled to 9.1 percent. On the other hand, the share of male child workers in the other occupations declined between 2000 and 2007, while the share of female child workers increased in all other sectors except construction.

Linking the information of occupation sectors to strenuous and hazardous work, the higher participation rate of male child workers in construction and mining sectors may imply a larger health effect of child labor on males than females. In addition, it may also be possible that the kind of work that male and female child workers are engaged in is different even in the same occupation sector. These observations provide the motivation for examining gender heterogeneity in the effect of child labor on human capital growth.

To conclude, we find that child market work participation rate in Indonesia, averaging 4.3 percent between 1986 and 2007, is smaller than most developing countries listed in (Edmonds, 2008). Despite the low child market work participation rate in Indonesia, more than 2.7 million children between 5 and 14 were engaged in market work in 2007. Therefore, the empirical question of whether

[2] Formally, Statistics Indonesia includes the following occupations in the other services: government, education, health, social work, international agencies, and domestic duties.

child market work adversely affects human capital accumulation remains important.

3.4 Estimation Strategy

Given our focus on the effect of market work on the growth of skills and health between 2000 and 2007, our main child worker sample consists of those who were engaged in market work in 2000 while the comparison group consists of those who were not working in 2000. We employ the following value-added model:

$$\frac{Y_{ijk,2007}}{\sigma_{2000}} = f\left(W_{ijk,2000}, \frac{Y_{ijk,2000}}{\sigma_{20000}}, X_{ijk}, P_{ijk}, D_k, \varepsilon_{ijk}\right) \tag{3.1}$$

where the dependent variable is individual i's outcomes of interest (mathematics skills, cognitive skills, and lung capacity) in 2007, divided by the standard deviation of each particular outcome in 2000. Our main independent variable is $W_{ijk,2000}$, the working status of the individual in 2000, which is equal to one if the individual was working in 2000 and zero otherwise. Our value-added model conditions upon the individual's outcomes in 2000. The exogenous control variables include X_{ijk}, the individual's age and gender; P_{ijk}, the father's education attainment as measured through years of completed schooling; and D_k, a vector of various district characteristics where individual i resided in 2000, as well as the real GDP per capita in 1996; and ε_{ijk} is the residual.[3] Table 3.1 presents

[3]As we mention below, close to 80 per cent of the child workers in our sample began working between 1997 and 1999. Therefore, we choose to condition on GDP per capita levels in 1996

summary statistics.

Instrument The economic literature on child labor widely acknowledges that estimating an Ordinary Least Squares (OLS) on Equation (3.1) produces biased estimates due to the endogenous nature of child market work. Studies in the literature (e.g., Akabayashi and Psacharopoulos, 1999; Beegle et al., 2009; Gunnarsson et al., 2006; Kana et al., 2010; O'Donnell et al., 2005; Wolff and Maliki, 2008) use various instrumental variables such as household land holdings, local economy, prices, or labor market conditions, school quality and availability, and compulsory school starting age.

In this paper, we use the provincial legislated minimum wage levels as the instrument. The choice is motivated by the theoretical work by (Basu, 2000) showing that an increase in minimum wage can either increase or decrease child labor. The main argument for the former is that a rise in minimum wage can increase adult unemployment rate which, in turn, increases child labor incidence because parents only send their children to work if they are on the brink of poverty. Basu (2000) explains further that this adverse effect may amplify if an increased supply of child labor displaces more adult labor, which in effect sends more children to work. This is typically the case in developing countries where unemployment benefits do not exist. On the other hand, increasing the minimum wage can also decrease supply of child labor because increased minimum wage translates to improved conditions of adult workers. Consequently, parents do not have to send their children to work (e.g., Goldin, 1979; Ray, 2000). Magruder (2013) provides further support to use minimum wage as the instrument in this study. Using a difference in spatial difference analysis, he finds that

in order to ensure the exogeneity of the variable.

in Indonesia an increase in minimum wage increases formal employment rate and decreases informal employment rate. Therefore, based on above arguments and evidence we should expect minimum wage to be correlated with child labor in Indonesia.

Since IFLS provides information on the year that each child worker began working, we match the minimum wage level in the particular year and province where the child worker began working. The majority of child workers in our sample, 67 percent, began working between 1997 and 1999, at the height of the economic crisis in Indonesia. For the non-child workers, we assign the minimum wage values according to their province of residence and imputed year that they would have begun working, based on their birth year.[4]

In order to be a valid instrument, legislated minimum wage must fulfill two conditions. First, it must be relevant, or in other words have a statistically significant relationship with child labor. Second, it must not have a direct causal relationship with the dependent variables or be correlated with the residual in Equation (3.1). This is the exclusion restriction. We now discuss the validity of the instrument with regards to these conditions.

We first present the relevance of the instrument, as shown in Table 3.2. The results in the first column do not condition for any covariates, and show that a one-standard deviation increase in minimum wage (about Rp. 26,000) is associated with a 3.3 (=0.26 x 0.128) percentage-point increase in the probability of child market work. This accounts for about 25 percent increase from the base

[4]We impute the year for non-child workers by regressing the year started working on the birth year of the child workers, and then use the estimated coefficient to predict the starting year that the non-child workers would have begun working had they been sent to work.

probability.

The positive and statistically significant correlation between minimum wage and child labor remains after we condition on individual characteristics, parental education, and district GDP per capita (Column 2) or various district level variables (Column 3) that may confound the observed correlation, which we calculate using *Podes* 2000.[5] We find that the positive correlation between minimum wage and child labor remains robust, even strengthened, after conditioning upon these variables.

In contrast to the relevance condition, the exclusion restriction of an instrument is fundamentally untestable. Therefore, understanding the process in determining provincial minimum wage in Indonesia is important in order to understand whether it may be directly correlated with any component in the residual. According to Suryahadi et al. (2003), minimum wage in Indonesia is calculated based on a bundle of consumption items deemed essential for the livelihood of a single worker, around 2,600 to 3,000 calories per day. Until the end of 2000, each province has a single minimum wage level, determined through a tripartite discussion process attended by employee representatives, employers, and the government. Therefore, the level of legislated minimum wage is the result of province-specific conditions and the between-province variation in minimum wages reflects the variation in prices and negotiation results. From the process described above, minimum wage is unlikely to have a direct correlation with the dependent variables.

To tease out the validity of this hypothesis, we regress dependent variables

[5] The administrative regions in Indonesia consist of villages, sub-districts, districts, and provinces.

in 2000 on minimum wage and a host of district control variables. The idea of this exercise is that minimum wages in 2000 or earlier should only affect human capital outcomes in 2007 through child labor status in 2000. Thus, if minimum wage satisfies exclusion restriction we should see insignificant correlation between minimum wage and human capital outcomes in 2000. The results of this exercise support our hypothesis, as shown in Table 3.3. We do not find statistically significant effects of minimum wage on all dependent variables (Columns 1-4). This implies that minimum wage arguably satisfies exclusion restriction condition. Minimum wage also satisfies relevance condition, as described above. Together, we can claim that minimum wage is an arguably valid instrument for this study.

Our instrumental variable specification is as follows :

$$W_{ijkp,2000} = g\left(MW_p, X_{ijkp}, P_{ijkp}, D_{kp}, v_{ijkp}\right) \tag{3.2}$$

$$\frac{Y_{\text{likp,2007}}}{\sigma_{2000}} = f\left(\hat{W}_{\text{lik,2000}}, \frac{Y_{\text{lik,,2000}}}{\sigma_{2000}}, X_{\text{likp}}, P_{\text{likp}}, D_{kp}, \varepsilon_{\text{likp}}\right) \tag{3.3}$$

where MW_p is the legislated minimum wage in province p.

3.5 The Effect of Child Market Work on Human Capital

The two-stage least squares (2SLS) estimation results are shown in Table 3.4, while the OLS results are shown in Table 3.8. Comparing the OLS with the 2SLS estimation results, we find some contrasting results. We compare coefficients of child work on mathematics and lung capacity specifications because these two outcomes are statistically significant in 2SLS results. Following Clogg et al. (1995), we use z-test to verify the difference between OLS and 2SLS results, and find that the differences are statistically significant.[6]

Examining the 2SLS results, we find that the instrument performs strongly with first-stage F-statistics ranging from 52 to 54. Column 1 shows that children who were engaged in market work in 2000 experienced 0.37 standard deviations lower growth in mathematics skills by 2007 compared to children who were not engaged in market work in 2000. The effect is especially substantial when measured in years of schooling. According to Suryadarma (2010), one additional year of schooling in Indonesia increases mathematics skills by about 0.13 standard deviations. Therefore, the effect of child market work on mathematics skills accumulation is worth about three years of schooling. This effect is large considering our panel is only seven years. Even more importantly, our estimates on the impact of child labor on education attainment (Column 4) is not statistically significant.

Assessing the health effects of child market work, we find growth in the lung capacity among child workers between 2000 and 2007 to be 0.38 standard deviations lower than non-child workers (Column 3). Based on the literature on children lung function growth (He et al., 2010), the estimates indicate that child workers may be working in environments with higher air pollution and

[6] z-score is obtained by calculating the following:

$$z-\text{score} = \frac{\beta_{IV} - \beta_{OLS}}{\sqrt{SE(\beta_{IV})^2 + SE(\beta_{OLS})^2}}$$

excessive physical activity, resulting in lower respiratory health compared to non-child workers. If this health effect is irreversible later in life, then the associated health costs or the loss from early mortality resulting from market work may be substantial.[7] Effects of child work on cognitive skills and educational attainment are negative, but these estimates are not statistically different from zero (Columns 2 and 4, respectively).

3.6 Heterogeneity Effects

Gender Heterogeneity We do not observe significant gender differences in terms of child market work participation rate. However, we may still observe gender heterogeneity in the effects of child market work due to other reasons, such as participation in different tasks (Edmonds, 2008) or employment in different sectors, as shown in Figures 3.2 and 3.3. We investigate this issue by adding an interaction term between child labor status and a gender indicator variable, which takes a value of one if a child is male and zero if female. Because the interaction term is endogenous, we add another instrument, interaction between provincial minimum wage and gender variable, into our estimation. We cannot draw meaningful inferences from the results, shown in Table 3.5. Low first-stage F-statistics in all specifications suggest that our instruments are weak. Moreover, coefficients on interaction term and child labor status are not statisti-

[7] In a study in the United States, Evans and Smith (2005) find that the long-term effects of exposure to air pollution include heart attack and angina. In addition, Jayachandran (2009) finds that air pollution is responsible for early-life mortality in Indonesia.

cally significant.

Location of Residence Heterogeneity The second aspect of heterogeneity that we consider is location of residence. Children may be engaged in different kinds of work depending on whether they live in a rural or an urban area. For example, most of those working in rural areas may be engaged in agriculture, while those working in urban areas may be working in manufacturing. Since working in factories may expose children to more pollution than working in agriculture, these children may suffer worse health effects. Similar to estimation of gender heterogeneity effects, to examine heterogeneity in residence effects of child work, we add interaction term between child labor status and residence location indicator variable. Interaction between residence location and minimum wage serves as an additional instrument. Table 3.6 shows that our instruments are weak. First-stage F-statistics are very low. Thus, we cannot interpret anything from our estimation results.

Type of Work Heterogeneity Heterogeneity in the effect of child market work may also take place between child workers who work for the family business and those who work outside their household. As an example, the child workers who are working for their parents, although unpaid, may not work as intensely as those who are working for pay outside the family.[8]

[8]The assumption that working for wage outside the household is worse than working for the household business may or may not be true. As an example, injury rate from child market work in agriculture – which may include working in household-owned land – is higher than the injury rate in child market work in manufacturing – which most likely falls under working for wage (Ashagrie, 1998). However, most of the worst forms of child labor, such as bonded labor, prostitution, combat, or involvement in pornography, are done outside the household (ILO, 2002).

In this section, we examine whether type of work heterogeneity in the effect of child market work on human capital accumulation exists. However, we do not explicitly model the decision to work inside or outside the household. To the extent that the decision is related to the outcomes that we are measuring and have no controls for, the estimates may be inconsistent. However, we believe that this is an important yet largely unexplored aspect in the research of the effect of child labor. To investigate this issue we simply regress our outcomes on type of work indicator variable, which takes one if wage work and zero if family business work, and other control variables. The results are presented in Table 3.7.

We find that in terms of educational attainment child workers whose jobs are outside of family business suffer worse than those who help their family business. The difference is quite large, about 1.5 years of schooling (column 4). This represents 20 percent loss in schooling during the period of our study. Columns 1-3 suggest that working for wage and for family business do not differ statistically in terms of other human capital outcomes. Nevertheless, we find suggestive evidence that working outside the family business is a worse form of child labor.

3.7 Conclusion

In this paper we examine the effect of child labor on the long-term growth in human capital, which is widely accepted as an important determinant of earnings. Different from most studies in the literature, we use measures of output of the human capital production: mathematics skills, cognitive skills, and pulmonary function.

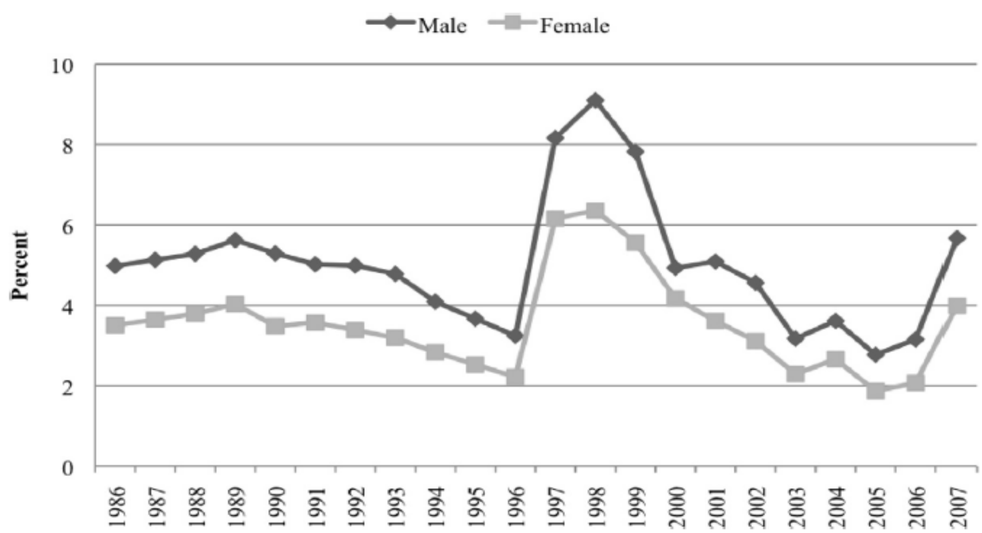
We find strong negative effects of child work on the growth of mathematics skills and lung capacity in the next seven years. We are not able to draw meaningful gender and residence heterogeneous effects of child labor. However, we find large adverse effects of working for wage outside the family on educational attainment compared to those who work in family business.

Therefore, departing from many studies that focus on the input to the human capital production function, we discover large negative effects of child labor. Moreover, we observe these substantial negative effects despite the fact that close to 90 percent of child workers in Indonesia work for the family business. This means two things. First, even the kind of child labor that is considered as relatively acceptable already has large negative effects on long-term human capital accumulation. Second, the results also imply that the effects of child labor on human capital accumulation may be much worse in other developing countries at lower levels of development than Indonesia, where a higher share of children are working and more child workers are working for wage in factories or other locations outside the household. Thus, child labor remains a phenomenon that needs to be seriously addressed by policymakers, especially in developing countries.

3.8 Figures and Tables

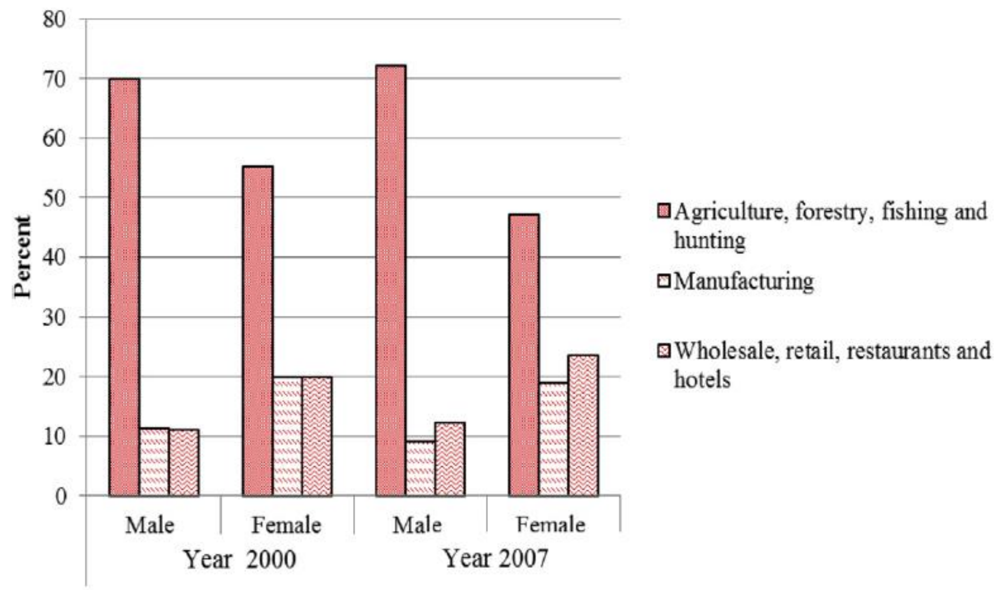
3.8.1 Figures

Figure 3.1: Market Work Participation Rate of 10- to 14-year-olds, By Gender 1986–2007



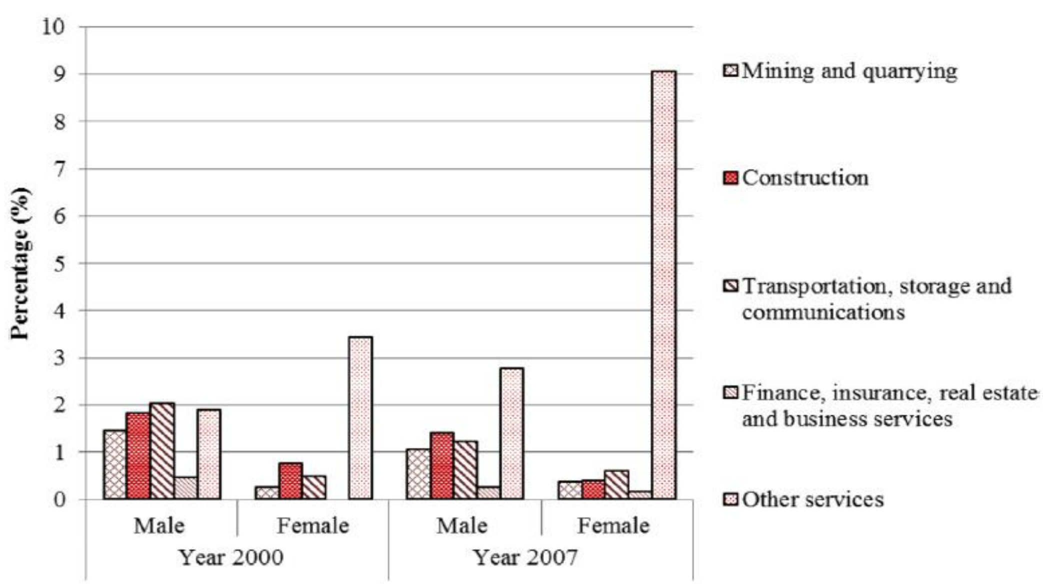
Note: Authors' calculation from *Sakernas* 1986–2007.

Figure 3.2: Three Most Popular Occupation Sectors of Child Workers 2000 and 2007, By Gender



Note: Authors' calculation from *Sakernas* 2000 and 2007.

Figure 3.3: The Rest of Occupation Sectors of Child Workers 2000 and 2007, By Gender



Note: Authors' calculation from *Sakernas* 2000 and 2007.

3.8.2 Tables

Table 3.1: Summary Statistics

	Full Sample		N	Children not working in 2000		N	Children working in 2000		N	Mean difference signif at 5 %
	Mean	Std. Dev		Mean	Std. Dev		Mean	Std. Dev		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Mathematics score in 2000 (min = 0, max = 5)	3.08	1.34	2794	3.07	1.35	2438	3.10	1.26	356	No
Mathematics score in 2007 (min = 0, max = 5)	3.13	1.34	2794	3.14	1.35	2438	3.07	1.27	356	No
Cognitive Score in 2000 (min = 0, max = 12)	8.38	2.89	2794	8.39	2.89	2438	8.31	2.87	356	No
Cognitive Score in 2007 (min = 0, max = 12)	9.74	2.39	2794	9.74	2.38	2438	9.74	2.48	356	No
Lung Capacity in 2000 (l/min)	223.24	63.03	2794	220.39	61.93	2438	242.72	67.04	356	Yes
Lung Capacity in 2007 (l/min)	337.13	98.19	2794	337.15	97.60	2438	336.95	102.34	356	No
Schooling in 2000 (years)	4.74	1.94	2794	4.67	1.95	2438	5.22	1.83	356	Yes
Schooling in 2007 (years)	9.92	2.89	2794	9.97	2.84	2438	9.57	3.17	356	Yes
Child labour status (=1)	0.13	0.33	2794	NA	NA	2438	1.00	1.00	356	
Work for wage outside family (=1)	0.11	0.31	2794	NA	NA	2438	0.21	0.41	356	
Work in family business (=1)	0.03	0.16	2794	NA	NA	2438	0.86	0.35	356	
Male (=1)	0.50	0.50	2794	0.50	0.50	2438	0.49	0.50	356	No
Age in 2007	18.79	1.86	2794	18.65	1.84	2438	19.74	1.72	356	Yes
School attendance in 2000	0.94	0.23	2764	0.96	0.20	2414	0.84	0.36	350	Yes
Mother's schooling in 2000 (years)	5.61	4.09	2794	5.81	4.13	2438	4.26	3.54	356	Yes
Father's employment status (=1)	0.87	0.25	2794	0.88	0.25	2438	0.86	0.24	356	No
Mother's employment status (=1)	0.89	0.23	2794	0.89	0.23	2438	0.87	0.22	356	No
PC monthly HH expenditure in 2000 (in 00'000 IDR)	2.47	2.31	2788	2.45	2.22	2432	2.58	2.85	356	Yes
District GDP per capita in 1996 in 1993 (in millions IDR)	2.25	2.71	2794	2.31	2.80	2438	1.89	1.92	356	Yes
District adult unemployment rate	0.07	0.05	2794	0.07	0.05	2438	0.06	0.04	356	Yes
District population (thousand)	938.36	664.94	2794	942.89	671.67	2438	907.34	616.91	356	No
Proportion of villages in the district with:										
A market building	0.25	0.18	2794	0.25	0.18	2438	0.25	0.16	356	No
Year-round roads	0.96	0.07	2794	0.97	0.07	2438	0.96	0.08	356	No
Bank	0.26	0.25	2794	0.27	0.25	2438	0.24	0.24	356	No
Public health center	0.20	0.19	2794	0.20	0.20	2438	0.18	0.17	356	Yes
A primary and secondary school	0.91	0.55	2794	0.91	0.56	2438	0.92	0.50	356	No
Instrument										
Provincial monthly legislated minimum wage (in 00'000 IDR)	1.43	0.26	2794	1.42	0.23	2438	1.50	0.41	356	Yes

Note: Mean difference is calculated from a *t*-test or a chi-squared test for binary variables, where *Ho* is equality of means.

Table 3.2: Relevance of Instrument

	Child Labor (=1)			
	(1)	(2)	(3)	(4)
Provincial monthly legislated minimum wage (in 00‘000 IDR)	0.128***	0.264***	0.353***	0.379***
	(0.049)	(0.070)	(0.056)	(0.052)
Male (=1)		-0.004	-0.003	-0.002
		(0.012)	(0.013)	(0.013)
Age in 2007		0.044	0.048***	0.049***
		(0.005)	(0.004)	(0.003)
Mother's schooling in 2000 (years)		-0.008***	-0.008***	-0.008***
		(0.002)	(0.001)	(0.001)
Father's employment status (=1)		-0.015	-0.028	-0.036
		(0.067)	(0.059)	(0.067)
Mother's employment status (=1)		(-0.012)	(-0.002)	(0.004)
		(0.068)	(0.062)	(0.069)
District GDP per capita in 1996 (in 1993 Rupiah)		-0.014	-0.012	-0.010
		(0.005)	(0.007)	(0.006)
District adult unemployment rate			-0.837**	-0.777
			(0.368)	(0.355)
District population			-0.000	-0.000
			(0.000)	(0.000)
Share of villages in the district with market			0.052	0.169
			(0.108)	(0.114)
Share of villages in the district with year-round roads				0.137
				(0.125)
Share of villages in the district with banks				-0.140
				(0.058)
Number of primary and secondary schools in the district (thousand)				0.021
				(0.056)
Number of observations	2,794	2,794	2,794	2,794
Adjusted R-squared	0.010	0.086	0.107	0.110

Note: The provincial minimum wage depends on the year that a child worker began working or a non-child worker is predicted to have begun working. Robust standard errors are clustered at the province level. *, **, and *** denote statistical significance at 10 %, 5 %, and 1 % level respectively.

Table 3.3: Exclusion Restriction of Instrument

	Mathematics Score in 2000	Cognitive Score in 2000	Lung Capacity in 2000	Education (Years) in 2000
	(1)	(2)	(3)	(4)
Provincial monthly legislated minimum wage (in 00'000 IDR)	-0.168	0.090	-0.143	-0.047
	(0.142)	(0.084)	(0.171)	(0.034)
Number of observations	2,794	2,794	2,794	2,794
Adjusted R-square	0.126	0.382	0.133	0.645

Note: The provincial minimum wage depends on the year that a child worker began working or a non-child worker is predicted to have begun working. All regressions include full covariates. Robust standard errors are clustered at the province level. *, **, and *** denote statistical significance at 10 %, 5 %, and 1 % level respectively.

Table 3.4: Child Labor and Human Capital Accumulation, 2SLS Results

	Mathematics Score in 2007	Cognitive Score in 2007	Lung Capacity in 2007	Schooling in 2007 (years)
	(1)	(2)	(3)	(4)
Child labor status (=1)	-0.372*	-0.243	-0.384***	-1.305
	(0.191)	(0.282)	(0.124)	(0.860)
Mathematics score in 2000, standardized	0.251***			
	(0.027)			
Lung capacity in 2000, standardized		0.222***		
		(0.027)		
Cognitive score in 2000, standardized			0.462***	
			(0.038)	
Years of education in 2000				1.145***
				(0.050)
Male (=1)	-0.104***	0.035	1.146***	0.096
	(0.035)	(0.035)	(0.034)	(0.074)
Age in 2007	-0.009	0.003	-0.001	-0.523***
	(0.009)	(0.014)	(0.009)	(0.049)
Mother's schooling in 2000 (years)	0.029***	0.031***	0.010***	0.198***
	(0.005)	(0.007)	(0.003)	(0.016)
Father's employment status (=1)	-0.286	-0.226	-0.140	0.609
	(0.264)	(0.253)	(0.156)	(0.752)
Mother's employment status (=1)	0.318	0.136	0.174	-0.933
	(0.280)	(0.301)	(0.171)	(0.854)
Per capita district GDP 1996, millions IDR, at 1993 constant price	-0.006	-0.005	-0.024**	0.010
	(0.007)	(0.007)	(0.012)	(0.017)
District adult unemployment rate	-0.305	-0.495	-0.722	-0.934
	(0.594)	(0.410)	(0.488)	(1.265)
District Population (thousand)	0.000	0.000	0.000	-0.000**
	(0.000)	(0.000)	(0.000)	(0.000)
Share of villages in the district with market	-0.027	0.107	0.183	0.719
	(0.121)	(0.134)	(0.115)	(0.686)
Share of villages in the district with year-round roads	0.993***	0.325	-0.232	1.352
	(0.326)	(0.475)	(0.308)	(0.834)
Share of villages in the district with banks	-0.028	-0.021	-0.025	0.267
	(0.115)	(0.108)	(0.186)	(0.382)
Number of primary and secondary schools in the district (thousand)	-0.108	-0.147	-0.177*	0.375
	(0.140)	(0.132)	(0.107)	(0.341)
Number of observations	2,794	2,794	2,794	2,794
Adjusted R-square	0.097	0.112	0.528	0.476
First-stage F-statistics	54.669	52.413	53.329	52.154

Note: The instrumental variable is provincial minimum wage in the year that a child worker began working or a non-child worker is predicted to have begun working. Dependent variables are mathematics score in 2007 (Column 1), cognitive score (Column 2), lung capacity (Column 3), and completed years of schooling in 2007 (Column 4); the mathematics score, cognitive score, and lung capacity are standardized to the standard deviation of respective scores in 2000. The provincial minimum wage depends on the year that a child worker began working or a non-child worker is predicted to have begun working. Robust standard errors are clustered at the province level. *, **, and *** denote statistical significance at 10 %, 5 %, and 1 % level respectively.

Table 3.5: Child Labor and Human Capital Accumulation By Gender, 2SLS Results

	Mathematics Score in 2007	Cognitive Score in 2007	Lung Capacity in 2007	Education (Years) in 2007
	(1)	(2)	(3)	(4)
Child labor*Male	0.247	-0.704	-1.654	2.205
	(0.895)	(1.061)	(1.260)	(1.912)
Child labor status (=1)	-0.504	0.131	0.490	-2.469
	(0.521)	(0.764)	(0.755)	(1.469)
Male (=1)	-0.135	0.125	1.352**	-0.182
	(0.123)	(0.126)	(0.192)	(0.290)
Constant	0.668	2.118**	2.009**	12.477**
	(0.372)	(0.486)	(0.355)	(0.708)
Number of observations	2,794	2,794	2,794	2,794
Second-stage R-square	0.098	0.099	0.444	0.466
First-stage F-statistics	3.105	3.123	3.114	3.075

Note: The instrumental variable is provincial minimum wage in the year that a child worker began working or a non-child worker is predicted to have begun working. Dependent variables are mathematics score in 2007 (Column 1), cognitive score (Column 2), lung capacity (Column 3), and completed years of schooling in 2007 (Column 4); the mathematics score, cognitive score, and lung capacity are standardized to the standard deviation of respective scores in 2000. The provincial minimum wage depends on the year that a child worker began working or a non-child worker is predicted to have begun working. All regressions include full covariates. Robust standard errors are clustered at the province level. *, **, and *** denote statistical significance at 10 %, 5 %, and 1 % level respectively.

Table 3.6: Child Labor and Human Capital Accumulation By Location of Residence, 2SLS Results

	Mathematics Score in 2007	Cognitive Score in 2007	Lung Capacity in 2007	Education (Years) in 2007
	(1)	(2)	(3)	(4)
Child labor*Urban	4.368*	-5.403	-19.328*	0.782
	(22.054)	(24.155)	(67.473)	(42.599)
Child labor status (=1)	(-2.012)	(1.830)	(7.057)	(-1.566)
	(8.527)	(9.254)	(26.007)	(16.588)
Urban (=1)	0.142	0.165	0.145	0.394
	(0.152)	(0.153)	(0.316)	(0.289)
Constant	0.762	1.992**	1.340**	12.280**
	(0.612)	(0.740)	(2.417)	(3.664)
Number of observations	2,794	2,794	2,794	2,794
Second-stage R-square	-0.653	-1.026	-9.828	0.473
First-stage F-statistics	0.039	0.035	0.042	0.058

Note: The instrumental variable is provincial minimum wage in the year that a child worker began working or a non-child worker is predicted to have begun working. Dependent variables are mathematics score in 2007 (Column 1), cognitive score (Column 2), lung capacity (Column 3), and completed years of schooling in 2007 (Column 4); the mathematics score, cognitive score, and lung capacity are standardized to the standard deviation of respective scores in 2000. The provincial minimum wage depends on the year that a child worker began working or a non-child worker is predicted to have begun working. All regressions include full covariates. Robust standard errors are clustered at the province level. *, **, and *** denote statistical significance at 10 %, 5 %, and 1 % level respectively.

Table 3.7: Child Labor and Human Capital Accumulation By Type of Work, OLS Results

	Mathematics Score in 2007	Cognitive Score in 2007	Lung Capacity in 2007	Education (Years) in 2007
	(1)	(2)	(3)	(4)
Child labor*Type of Work	-0.013	0.095	-0.118	-1.468*
	(0.081)	(0.074)	(0.104)	(0.320)
Number of observations	356	356	356	356
Second-stage R-square	0.167	0.148	0.599	0.546

Note: The instrumental variable is provincial minimum wage in the year that a child worker began working or a non-child worker is predicted to have begun working. Dependent variables are mathematics score in 2007 (Column 1), cognitive score (Column 2), lung capacity (Column 3), and completed years of schooling in 2007 (Column 4); the mathematics score, cognitive score, and lung capacity are standardized to the standard deviation of respective scores in 2000. The provincial minimum wage depends on the year that a child worker began working or a non-child worker is predicted to have begun working. All regressions include full covariates. Robust standard errors are clustered at the province level. *, **, and *** denote statistical significance at 10 %, 5 %, and 1 % level respectively.

Table 3.8: Child Labor and Human Capital Accumulation, OLS Results

	Mathematics Score in 2007	Cognitive Score in 2007	Lung Capacity in 2007	Schooling in 2007 (years)
	(1)	(2)	(3)	(4)
Child labor status (=1)	0.028	0.065	-0.066	-0.120
	(0.041)	(0.044)	(0.048)	(0.124)
Mathematics score in 2000, standardized	0.248***			
	(0.028)			
Cognitive score in 2000, standardized		0.224***		
		(0.027)		
Lung capacity in 2000, standardized			0.458***	
			(0.040)	
Years of education in 2000				1.161***
				(0.048)
Male (=1)	-0.103**	0.035	1.147***	0.102
	(0.036)	(0.037)	(0.035)	(0.071)
Age in 2007	-0.023**	-0.008	-0.011	-0.576***
	(0.008)	(0.008)	(0.008)	(0.034)
Mother's schooling in 2000 (years)	0.033***	0.033***	0.013***	0.207***
	(0.005)	(0.006)	(0.003)	(0.022)
Father's employment status (=1)	-0.275	-0.220	-0.132	0.640
	(0.275)	(0.265)	(0.167)	(0.827)
Mother's employment status (=1)	0.314	0.135	0.172	-0.940
	(0.288)	(0.319)	(0.175)	(0.928)
Per capita district GDP 1996, millions IDR, at 1993 constant price	-0.005	-0.004	-0.023*	0.012
	(0.007)	(0.007)	(0.012)	(0.016)
District adult unemployment rate	-0.138	-0.370	-0.589	-0.466
	(0.626)	(0.412)	(0.486)	(1.234)
District Population (thousand)	0.000	0.000	0.000	-0.000*
	(0.000)	(0.000)	(0.000)	(0.000)
Share of villages in the district with market	-0.052	0.089	0.163	0.648
	(0.129)	(0.127)	(0.109)	(0.630)
Share of villages in the district with year-round roads	0.987**	0.320	-0.238	1.326
	(0.330)	(0.489)	(0.316)	(0.881)
Share of villages in the district with banks	-0.023	-0.019	-0.021	0.274
	(0.116)	(0.107)	(0.191)	(0.369)
Number of primary and secondary schools in the district (thousand)	-0.133	-0.167	-0.197	0.293
	(0.130)	(0.131)	(0.116)	(0.331)
Number of observations	2,794	2,794	2,794	2,794
Adjusted R-square	0.122	0.129	0.540	0.494

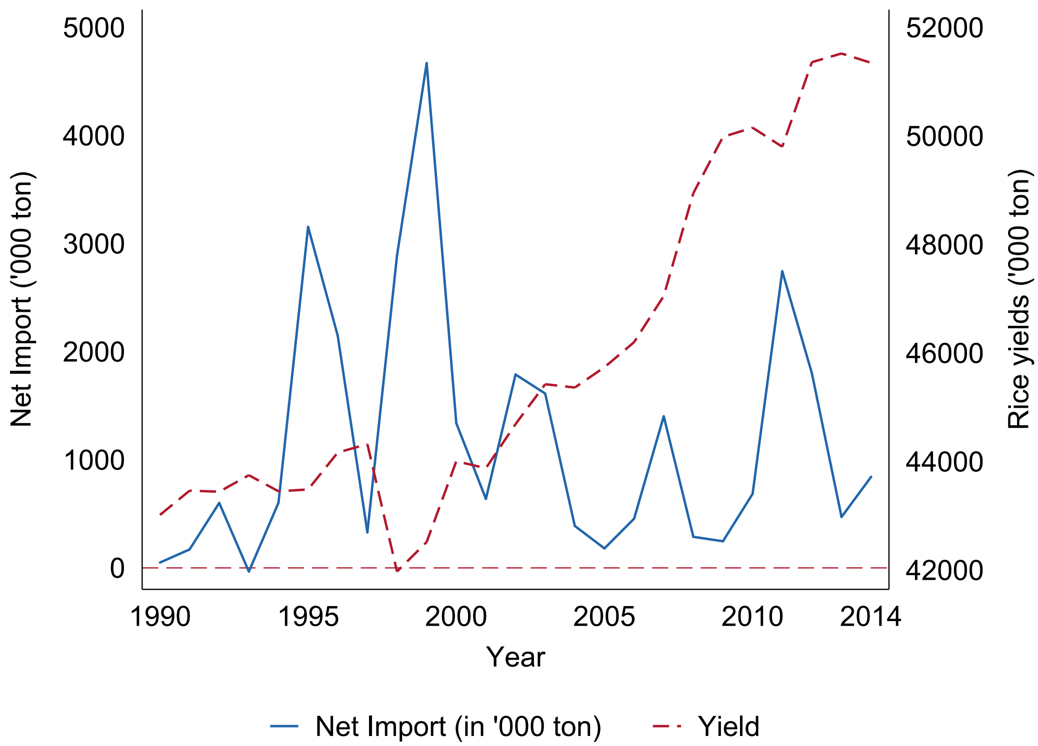
Note: Dependent variables are mathematics score in 2007 (Column 1), cognitive score (Column 2), lung capacity (Column 3), and completed years of schooling in 2007 (Column 4); the mathematics score, cognitive score, and lung capacity are standardized to the standard deviation of respective scores in 2000. Robust standard errors are clustered at the province level. *, **, and *** denote statistical significance at 10 %, 5 %, and 1 % level respectively.

APPENDIX A

CHAPTER 1 OF APPENDIX

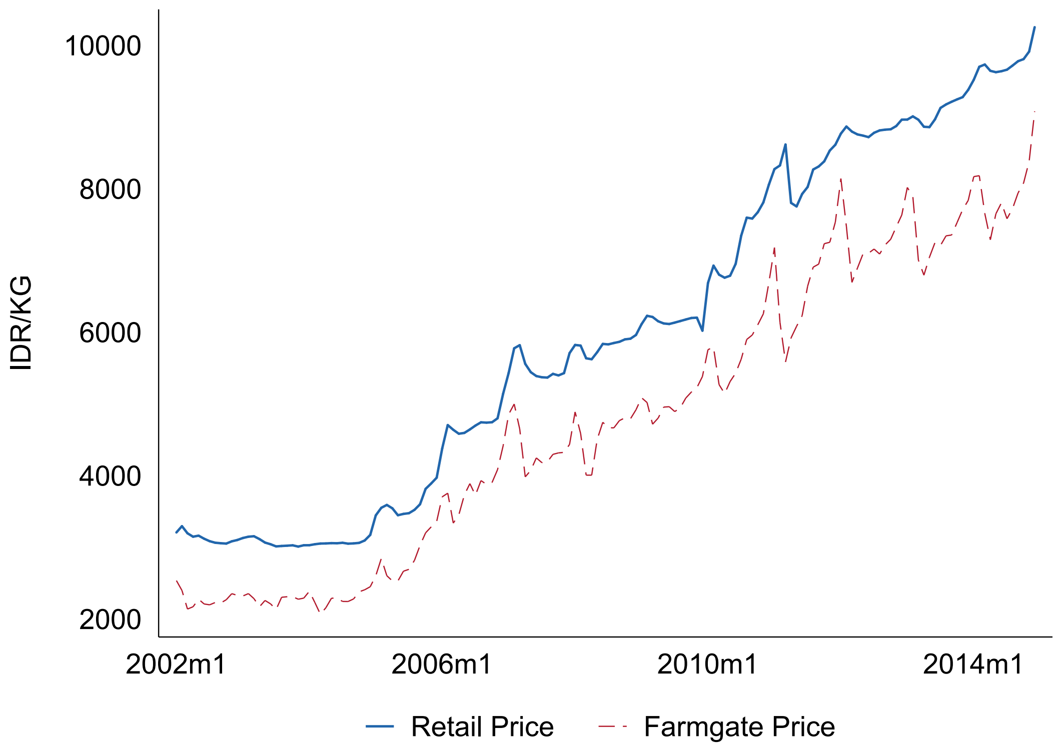
A.1 Figures and Tables

Figure A.1: Net Rice Imports and Rice Yields, 1990-2014.



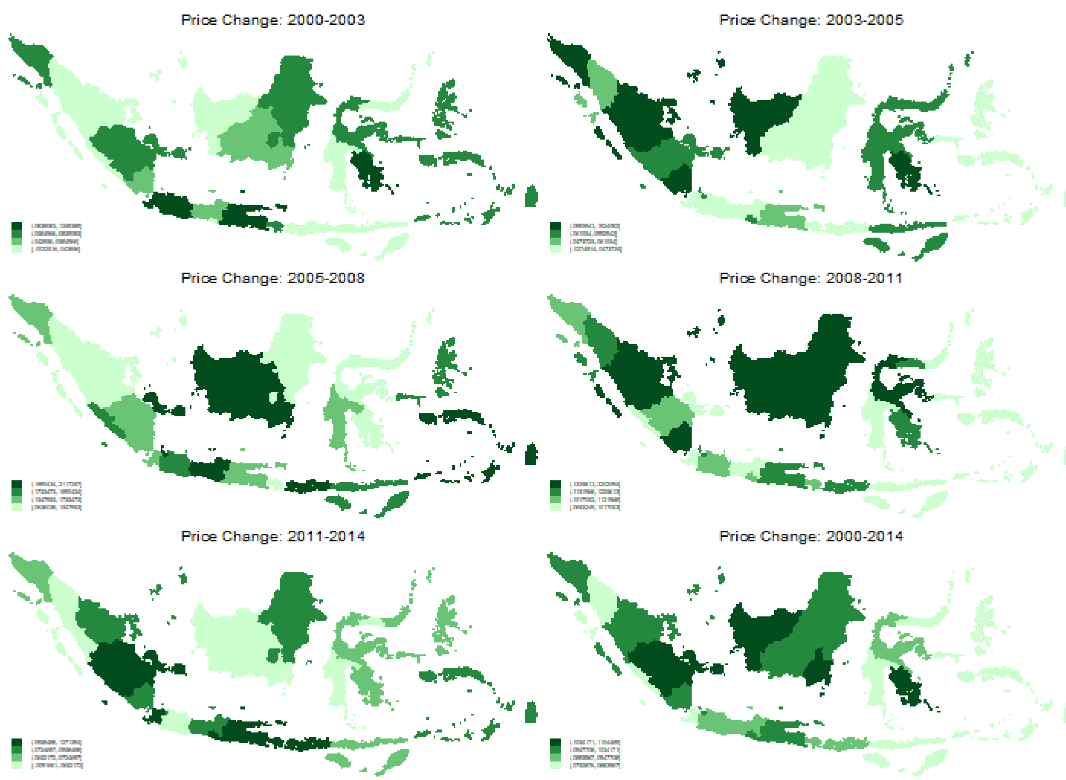
Note: This figure shows Indonesia's net import of rice and rice yields from 1990 to 2014. The rice yields reflect the amount of rice after going through a drying and milling process that converts 100 kilograms of wet paddy to roughly 55 kilograms of rice. Source: FAOSTAT.

Figure A.2: Domestic Retail and Farmgate Price: 2002-2014



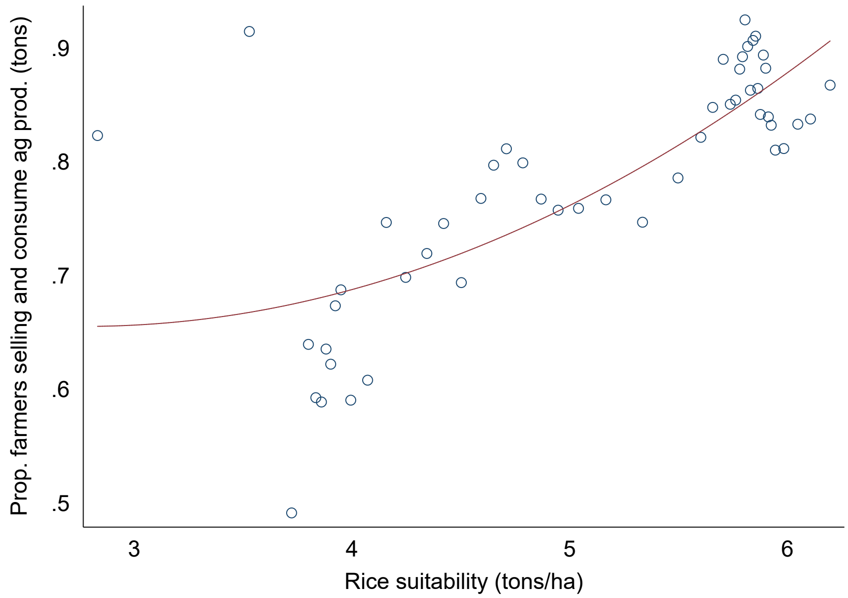
Note: This figure shows the close relatonship between national farmgate and retail rice prices from 2002 to 2014. Farmgate prices are quoted in wet paddy. Drying and milling process converts 100 kilograms of wet paddy to roughly 55 kilograms. Source: Central Bureau of Statistics (BPS) and CEIC database.

Figure A.3: Domestic Annual Rice Price Change Distribution: 2000-2014



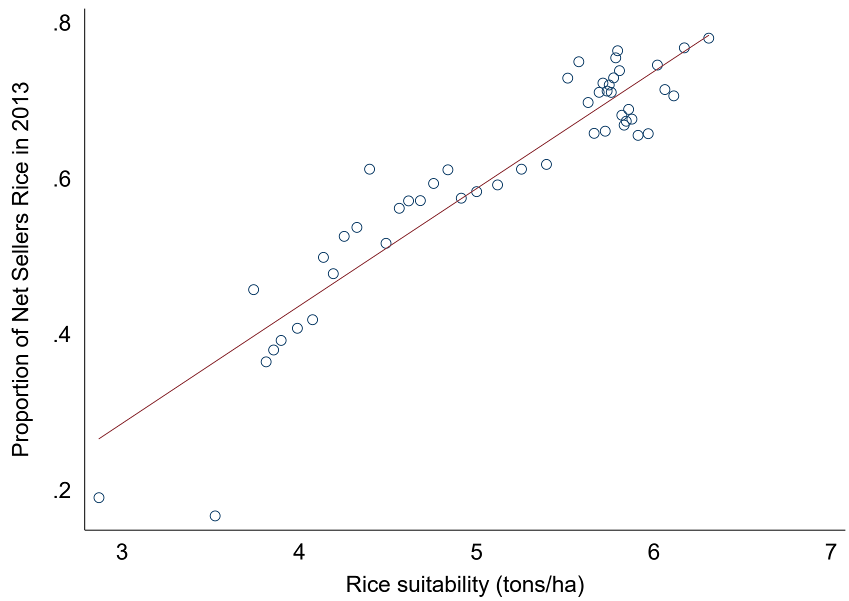
Note: his figure shows log annualized price change (log points) between 2000 to 2014 across provinces in Indonesia, excluding Papua island. Darker shade implies higher price change than that of lighter shade. Source: Central Bureau of Statistics (BPS) obtained via CEIC database.

Figure A.4: Proportion of Producer and Consumer Farmers and Rice Suitability.



Note: This figure summarizes relationship between rice suitability and proportion of farmers selling and consuming their agriculture products conditional on majority of villagers working in agricultural sector. Source: FAO-GAEZ (rice suitability) and PODES 2005 (proportion of farmers sellers).

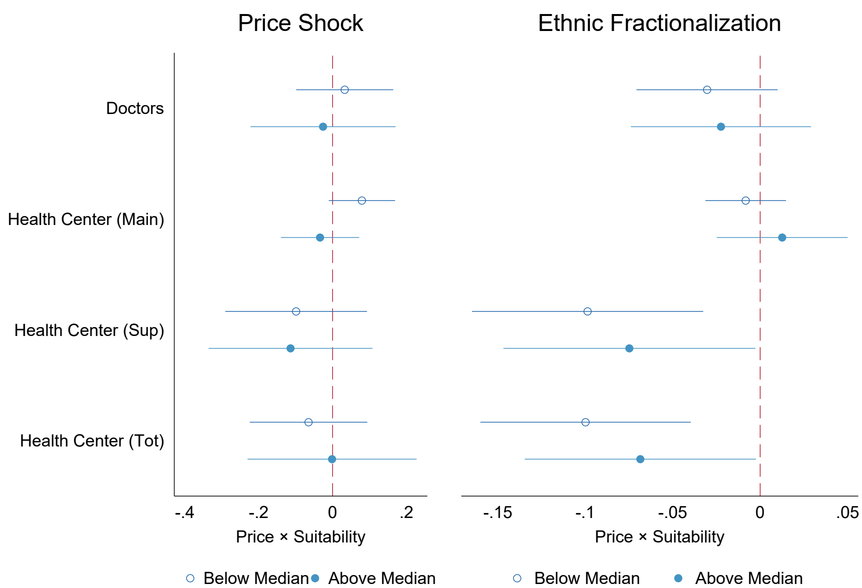
Figure A.5: Proportion of Net Seller Rice Farmers and Rice Suitability in 2013.



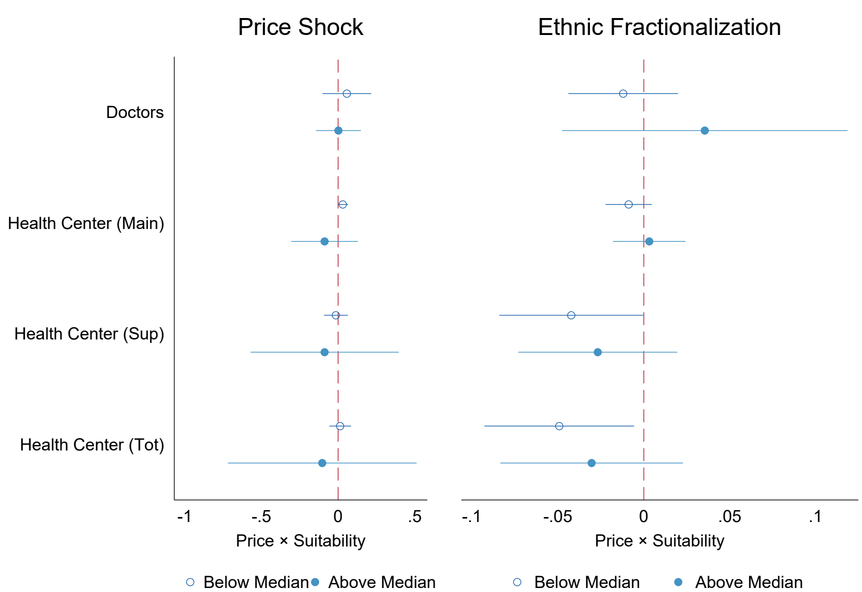
Note: This figure summarizes relationship between rice suitability and proportion of net seller rice farmers. Net seller is an indicator for whether a household sells some or all of their harvested rice. Source: FAO-GAEZ (rice suitability) and Agricultural Census 2013 (proportion of net sellers).

Figure A.6: Effects on Health Public Goods by Price Shock Magnitude and Ethnic Diversity

(a) Changes in Extensive Margin



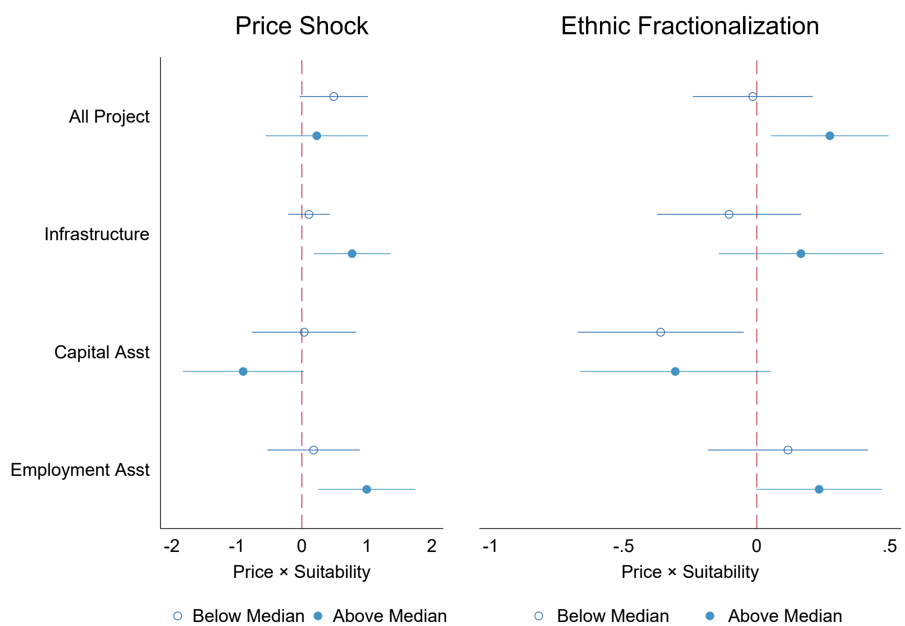
(b) Changes in Intensive Margin



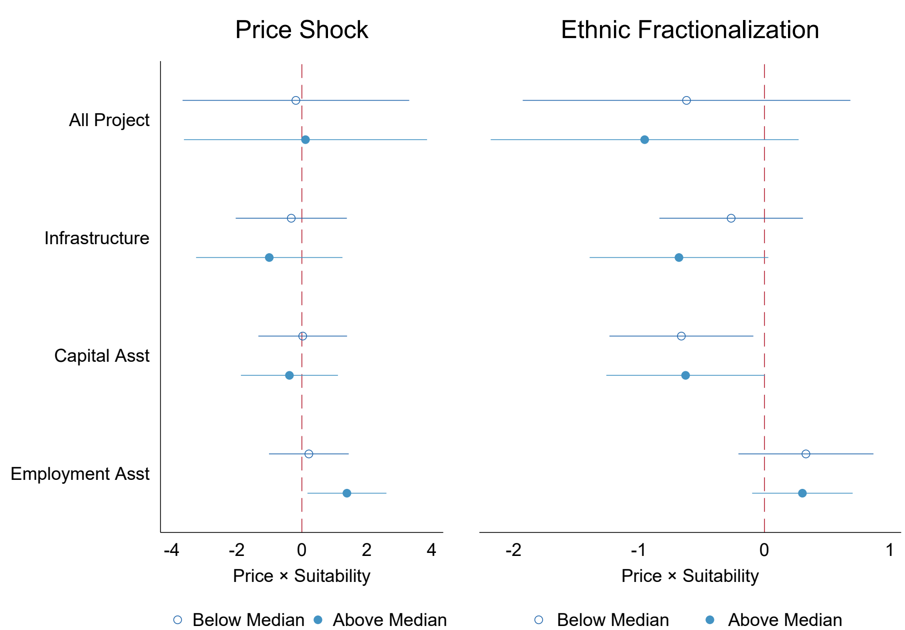
Note: This figure plots regression coefficients of estimating Equation 1.2. Each coefficient comes from each regression conducted separately. Panel (a) and Panel (b) plot effects on changes in the presence and number of public health facilities and personnel, respectively. Standard errors are clustered at the district level with 90% confidence interval.

Figure A.7: Effects on Development Projects by Price Shock Magnitude and Ethnic Diversity

(a) Changes in Extensive Margin



(b) Changes in Intensive Margin



Note: This figure plots regression coefficients of estimating Equation 1.2. Each coefficient comes from each regression conducted separately. Panel (a) and Panel (b) plot effects on changes in the presence and number of public health facilities and personnel, respectively. Standard errors are clustered at the district level with 90% confidence interval.

Table A.1: Nutrient Intake: Bought and Own Food

	Calorie Bought (All) (1)	Calorie Own (All) (2)	Calorie Bought (Unprocessed) (3)	Calorie Own (Unprocessed) (4)	Protein Bought (All) (5)	Protein Own (All) (6)	Protein Bought (Unprocessed) (7)	Protein Own (Unprocessed) (8)
Price	-0.036	0.084	0.052	-0.144	-0.237	0.647	-0.319	0.284
	(0.232)	(0.810)	(0.412)	(0.888)	(0.267)	(0.730)	(0.307)	(0.800)
Price × Suitability	0.014	0.201**	-0.040	0.145	0.039	0.087	-0.015	0.069
	(0.024)	(0.079)	(0.041)	(0.096)	(0.027)	(0.087)	(0.032)	(0.101)
District FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District-specific Trend	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	305292	208476	304488	185987	305255	207581	304486	185977
R-Squared	0.190	0.200	0.135	0.206	0.211	0.189	0.149	0.207

Note: This table presents the effects on nutritional status in the forms of per capita calorie (log) and protein (log) intake in the last seven days at the household level using data from the consumption module of the 2002, 2005, 2008, and 2011 National Socioeconomic Survey (Susenas). The sample covers 25,821 unique villages. To obtain per capita measures, household size is adjusted by equivalent scales. Calorie and protein intakes are converted from all and unprocessed food groups. All-food group includes processed and unprocessed food. Both food groups are further divided into whether the food are bought or obtained from household own production. In addition to the village-level covariates in the main specification, the regression specification also includes household covariates: indicator for wife's education attainment, wife's age and age squared, indicator for marital status of head of household (not married, married, divorced, widowed), and indicators for the number of household members aged 0-4, 5-9, 10-14, 15-55, and above 55. Standard errors, reported in parentheses, are robust to heteroskedasticity and clustered at the district level.. $*p < 0.10, **\, p < 0.05, ***p < 0.01$

Table A.2: Effects on Public Health Facilities and Personnel by Price Shock Magnitude and Ethnic Diversity

	Δ Presence				Δ Number			
	Doctors	Health Center (Main)	Health Center (Small)	Health Center (Total)	Doctors	Health Center (Main)	Health Center (Small)	Health Center (Total)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Price Shock: Above Median								
Price × Suitability	-0.026	-0.034	-0.114	-0.001	0.002	-0.088	-0.088	-0.104
	(0.119)	(0.064)	(0.134)	(0.139)	(0.089)	(0.132)	(0.293)	(0.373)
N	110728	110728	110728	110728	97680	59818	59818	59818
R-Squared	0.218	0.277	0.248	0.245	0.245	0.372	0.353	0.361
Price Shock: Below Median								
Price × Suitability	0.033	0.080	-0.099	-0.065	0.057	0.031	-0.015	0.013
	(0.080)	(0.054)	(0.116)	(0.096)	(0.096)	(0.020)	(0.047)	(0.043)
N	106375	106375	106375	106375	64752	103456	103456	103456
R-Squared	0.289	0.289	0.272	0.268	0.431	0.279	0.260	0.265
Ethnic Fractionalization: Above Median								
Price × Suitability	-0.022	0.013	-0.075*	-0.069*	0.036	0.003	-0.027	-0.030
	(0.031)	(0.023)	(0.044)	(0.040)	(0.050)	(0.013)	(0.028)	(0.032)
N	114400	114400	114400	114400	91990	91103	91103	91103
R-Squared	0.083	0.098	0.078	0.075	0.135	0.160	0.134	0.143
Ethnic Fractionalization: Below Median								
Price × Suitability	-0.030	-0.008	-0.099**	-0.100***	-0.012	-0.009	-0.042*	-0.049*
	(0.024)	(0.014)	(0.040)	(0.036)	(0.019)	(0.008)	(0.025)	(0.026)
N	122013	122013	122013	122013	97869	97335	97335	97335
R-Squared	0.079	0.095	0.073	0.072	0.126	0.148	0.126	0.130

Note: This table presents heterogenous treatment effects on changes in public good provision (extensive margins): health facilities and personnels as well as public schools. All regressions include population (log), distance to district capital (log), distance to sub district capital (log), village area (log) interacted with year fixed effects, harvested areas (log) allocated for planting rice interacted with year fixed effects, village and year fixed-effects as well as district-specific trends. Standard errors, reported in parentheses, are robust to heteroskedasticity and clustered at district level. $*p < 0.10, **p < 0.05, ***p < 0.01$

Table A.3: Effects on Development Projects by Price Shock Magnitude and Ethnic Diversity

	Presence				Number			
	All project	Infrastructure	Capital Asst	Employment Asst	All project	Infrastructure	Capital Asst	Employment Asst
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Price Shock: Above Median								
Price × Suitability	0.230	0.773**	-0.903	0.998**	0.112	-1.002	-0.382	1.387*
	(0.477)	(0.358)	(0.562)	(0.452)	(2.265)	(1.363)	(0.904)	(0.735)
N	87071	87071	87071	87071	87071	87071	87071	87071
R-Squared	0.620	0.803	0.562	0.504	0.719	0.772	0.595	0.514
Price Shock: Below Median								
Price × Suitability	0.491	0.110	0.036	0.180	-0.183	-0.326	0.026	0.216
	(0.317)	(0.193)	(0.486)	(0.429)	(2.105)	(1.032)	(0.824)	(0.741)
N	26249	26249	26249	26249	26249	26249	26249	26249
R-Squared	0.657	0.724	0.662	0.629	0.774	0.754	0.710	0.630
Ethnic Fractionalization: Above Median								
Price × Suitability	0.275**	0.166	-0.306	0.234	-0.955	-0.682	-0.629	0.303
	(0.134)	(0.187)	(0.217)	(0.143)	(0.744)	(0.432)	(0.383)	(0.243)
N	70775	70775	70775	70775	70775	70775	70775	70775
R-Squared	0.562	0.753	0.504	0.458	0.683	0.717	0.563	0.478
Ethnic Fractionalization: Below Median								
Price × Suitability	-0.015	-0.104	-0.361*	0.117	-0.621	-0.265	-0.663*	0.331
	(0.137)	(0.164)	(0.189)	(0.182)	(0.792)	(0.347)	(0.348)	(0.326)
N	74483	74483	74483	74483	74483	74483	74483	74483
R-Squared	0.568	0.782	0.507	0.453	0.706	0.761	0.557	0.461

Note: This table presents effects on development projects by price shock magnitude and ethnic diversity. All regressions include population (log), distance to district capital (log), distance to sub district capital (log), village area (log) interacted with year fixed effects, harvested areas (log) allocated for planting rice interacted with year fixed effects, village and year fixed-effects as well as district-specific trends. Standard errors, reported in parentheses, are robust to heteroskedasticity and clustered at district level. $*p < 0.10, **p < 0.05, ***p < 0.01$

A.2 Robustness Tests Results

Table A.2.1: Public Goods: Health Facilities and Personnel – All Provinces

	Δ Presence			
	Doctors	Health Center (Main)	Health Center (Support)	Health Center (Total)
	(1)	(2)	(3)	(4)
Panel A: Extensive Margins				
Price	0.162*	-0.057	0.438***	0.378**
	(0.091)	(0.067)	(0.150)	(0.146)
Price × Suitability	-0.032	0.007	-0.083***	-0.078***
	(0.020)	(0.013)	(0.031)	(0.030)
Village FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
District-specific Trend	Yes	Yes	Yes	Yes
N	263364	263364	263364	263364
R-Squared	0.080	0.097	0.075	0.073
Mean of Dep. Var.	0.194	0.126	0.318	0.424
	Δ Number			
Panel B: Intensive Margins				
Price	-0.098	-0.023	0.144	0.138
	(0.128)	(0.043)	(0.095)	(0.108)
Price × Suitability	0.011	0.004	-0.025	-0.025
	(0.025)	(0.009)	(0.021)	(0.023)
Village FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
District-specific Trend	Yes	Yes	Yes	Yes
N	211429	209966	209966	209966
R-Squared	0.131	0.155	0.130	0.136
Mean of Dep. Var.	0.669	0.130	0.333	0.463

Note: This table presents the effects on changes in public good provision in health facilities and personnels. Panel A presents estimation results of changes in extensive margins. Panel B presents results of changes in intensive margins. All regressions include population (log), distance to district capital (log), distance to sub district capital (log), village area (log) interacted with year fixed effects, harvested areas (log) allocated for planting rice interacted with year fixed effects. Dependent variables in columns 1 to 4 of Panel B are not available in PODES 2008. Standard errors, reported in parentheses, are robust to heteroskedasticity and clustered at district level. $*p < 0.10, **p < 0.05, ***p < 0.01$

Table A.2.2: Development Projects – All Provinces

	Presence				Number			
	All project	Infrastructure	Capital Asst	Employment Asst	All project	Infrastructure	Capital Asst	Employment Asst
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Price	-0.340	0.254	1.556**	-0.475	5.329**	3.002**	3.214**	-0.808
	(0.496)	(0.658)	(0.749)	(0.586)	(2.685)	(1.451)	(1.287)	(1.027)
Price $\times$ Suitability	0.166	-0.026	-0.328**	0.097	-1.404**	-0.894***	-0.698**	0.160
	(0.109)	(0.140)	(0.163)	(0.123)	(0.589)	(0.320)	(0.282)	(0.216)
Village FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District-specific Trend	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	161761	161761	161761	161761	161761	161761	161761	161761
R-Squared	0.563	0.756	0.503	0.462	0.697	0.734	0.568	0.475
Mean of Dep. Var.	0.837	0.700	0.639	0.264	3.074	1.627	1.042	0.389

Note: The sample for all regressions only include PODES 2008 onwards because information on development projects only available starting in 2008. All regressions include population (log), distance to district capital (log), distance to sub district capital (log), village area (log) interacted with year fixed effects, harvested areas (log) allocated for planting rice interacted with year fixed effects. All regressions include village and year fixed-effects as well as district-specific trends. Standard errors, reported in parentheses, are robust to heteroskedasticity and clustered at district level. $*p < 0.10, **p < 0.05, ***p < 0.01$

Table A.2.3: Public Goods: Health Facilities and Personnel – Village-Specific Trend

	Δ Presence			
	Doctors	Health Center (Main)	Health Center (Support)	Health Center (Total)
	(1)	(2)	(3)	(4)
Panel A: Extensive Margins				
Price	0.164	-0.051	0.475***	0.429**
	(0.108)	(0.077)	(0.175)	(0.171)
Price × Suitability	-0.032	0.007	-0.090**	-0.087**
	(0.024)	(0.015)	(0.036)	(0.035)
Village FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Village-specific Trend	Yes	Yes	Yes	Yes
N	237063	237063	237063	237063
R-Squared	0.229	0.237	0.204	0.198
Mean of Dep. Var.	0.199	0.129	0.335	0.442
	Δ Number			
Panel B: Intensive Margins				
Price	-0.140	-0.018	0.184	0.180
	(0.141)	(0.051)	(0.119)	(0.134)
Price × Suitability	0.023	0.003	-0.033	-0.034
	(0.028)	(0.011)	(0.026)	(0.029)
Village FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Village-specific Trend	Yes	Yes	Yes	Yes
N	190383	188955	188955	188955
R-Squared	0.383	0.393	0.311	0.324
Mean of Dep. Var.	0.639	0.131	0.350	0.482

Note: This table presents the effects on changes in public good provision in health facilities and personnels and schools. Panel A presents estimation results of changes in extensive margins. Panel B presents results of changes in intensive margins. All regressions include population (log), distance to district capital (log), distance to sub district capital (log), village area (log) interacted with year fixed effects, harvested areas (log) allocated for planting rice interacted with year fixed effects. Dependent variables in columns 1 to 4 of Panel B are not available in PODES 2008. Standard errors, reported in parentheses, are robust to heteroskedasticity and clustered at district level. $*p < 0.10, **p < 0.05, ***p < 0.01$

Table A.2.4: Development Projects – Village-Specific Trend

	Presence				Number			
	All project	Infrastructure	Capital Asst	Employment Asst	All project	Infrastructure	Capital Asst	Employment Asst
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Price	0.399	0.712	3.104**	-0.478	6.982	2.564	4.978**	-0.441
	(0.828)	(1.087)	(1.344)	(1.029)	(4.289)	(2.254)	(2.240)	(1.675)
Price × Suitability	0.005	-0.119	-0.655**	0.104	-1.705*	-0.771	-1.067**	0.098
	(0.180)	(0.233)	(0.293)	(0.218)	(0.944)	(0.492)	(0.493)	(0.354)
Village FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Village-specific Trend	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	145654	145654	145654	145654	145654	145654	145654	145654
R-Squared	0.838	0.888	0.766	0.745	0.860	0.869	0.799	0.757
Mean of Dep. Var.	0.840	0.705	0.636	0.274	3.144	1.680	1.044	0.403

Note: The sample for all regressions only include PODES 2008 onwards because information on development projects only available starting in 2008. All regressions include population (log), distance to district capital (log), distance to sub district capital (log), village area (log) interacted with year fixed effects, harvested areas (log) allocated for planting rice interacted with year fixed effects. All regressions include village and year fixed-effects as well as Village-specific trends. Standard errors, reported in parentheses, are robust to heteroskedasticity and clustered at district level. $*p < 0.10, **\ p < 0.05, ***p < 0.01$

Table A.2.5: Public Goods: Health Facilities and Personnel – Alternative Price Change Definition

	Δ Presence			
	Doctors	Health Center (Main)	Health Center (Support)	Health Center (Total)
	(1)	(2)	(3)	(4)
Panel A: Extensive Margins				
Price	0.055	-0.016	0.142**	0.124**
	(0.035)	(0.024)	(0.057)	(0.056)
Price × Suitability	-0.010	0.002	-0.025**	-0.024**
	(0.008)	(0.005)	(0.012)	(0.011)
Village FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
District-specific Trend	Yes	Yes	Yes	Yes
N	237063	237063	237063	237063
R-Squared	0.080	0.096	0.075	0.073
Mean of Dep. Var.	0.199	0.129	0.335	0.442
	Δ Number			
Panel B: Intensive Margins				
Price	-0.024	-0.002	0.020	0.026
	(0.042)	(0.016)	(0.035)	(0.040)
Price × Suitability	0.002	-0.000	-0.002	-0.003
	(0.008)	(0.003)	(0.008)	(0.009)
Village FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
District-specific Trend	Yes	Yes	Yes	Yes
N	190383	188955	188955	188955
R-Squared	0.131	0.155	0.129	0.136
Mean of Dep. Var.	0.639	0.131	0.350	0.482

Note: This table presents the effects on changes in public good provision in health facilities and personnels. Panel A presents estimation results of changes in extensive margins. Panel B presents results of changes in intensive margins. All regressions include population (log), distance to district capital (log), distance to sub district capital (log), village area (log) interacted with year fixed effects, harvested areas (log) allocated for planting rice interacted with year fixed effects. Dependent variables in columns 1 to 4 of Panel B are not available in PODES 2008. Standard errors, reported in parentheses, are robust to heteroskedasticity and clustered at district level. $*p < 0.10, **p < 0.05, ***p < 0.01$

Table A.2.6: Development Projects – Alternative Price Change Definition

	Presence				Number			
	All project	Infrastructure	Capital Asst	Employment Asst	All project	Infrastructure	Capital Asst	Employment Asst
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Price	-0.015	0.044	0.622**	-0.211	1.495	0.654	1.236**	-0.387
	(0.192)	(0.240)	(0.295)	(0.225)	(1.005)	(0.506)	(0.502)	(0.395)
Price × Suitability	0.037	0.002	-0.132**	0.045	-0.408*	-0.227**	-0.269**	0.082
	(0.042)	(0.051)	(0.064)	(0.047)	(0.218)	(0.110)	(0.110)	(0.083)
Village FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District-specific Trend	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	145654	145654	145654	145654	145654	145654	145654	145654
R-Squared	0.563	0.766	0.506	0.455	0.693	0.736	0.560	0.468
Mean of Dep. Var.	0.840	0.705	0.636	0.274	3.144	1.680	1.044	0.403

Note: The sample for all regressions only include PODES 2008 onwards because information on development projects only available starting in 2008. All regressions include population (log), distance to district capital (log), distance to sub district capital (log), village area (log) interacted with year fixed effects, harvested areas (log) allocated for planting rice interacted with year fixed effects. All regressions include village and year fixed-effects as well as district-specific trends. Standard errors, reported in parentheses, are robust to heteroskedasticity and clustered at district level.. $*p < 0.10, **p < 0.05, ***p < 0.01$

Table A.2.7: Public Goods: Health Facilities and Personnel – Control for Rainfall Shock

	Δ Presence			
	Doctors	Health Center (Main)	Health Center (Support)	Health Center (Total)
	(1)	(2)	(3)	(4)
Panel A: Extensive Margins				
Price	0.170*	-0.046	0.465***	0.420***
	(0.095)	(0.068)	(0.154)	(0.149)
Price × Suitability	-0.033	0.005	-0.088***	-0.086***
	(0.021)	(0.014)	(0.032)	(0.030)
Village FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
District-specific Trend	Yes	Yes	Yes	Yes
N	237063	237063	237063	237063
R-Squared	0.080	0.096	0.075	0.073
Mean of Dep. Var.	0.199	0.129	0.335	0.442
	Δ Number			
Panel B: Intensive Margins				
Price	-0.089	-0.008	0.170*	0.180*
	(0.126)	(0.044)	(0.097)	(0.109)
Price × Suitability	0.009	0.001	-0.031	-0.034
	(0.025)	(0.009)	(0.021)	(0.023)
Village FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
District-specific Trend	Yes	Yes	Yes	Yes
N	190383	188955	188955	188955
R-Squared	0.131	0.155	0.130	0.136
Mean of Dep. Var.	0.639	0.131	0.350	0.482

Note: This table presents the effects on changes in public good provision in health facilities and personnels. Panel A presents estimation results of changes in extensive margins. Panel B presents results of changes in intensive margins. All regressions include population (log), distance to district capital (log), distance to sub district capital (log), village area (log) interacted with year fixed effects, harvested areas (log) allocated for planting rice interacted with year fixed effects, and rainfall shock. Dependent variables in columns 1 to 4 of Panel B are not available in PODES 2008. Standard errors, reported in parentheses, are robust to heteroskedasticity and clustered at district level. $*p < 0.10, **p < 0.05, ***p < 0.01$

Table A.2.8: Development Projects – Control for Rainfall Shock

	Presence				Number			
	All project	Infrastructure	Capital Asst	Employment Asst	All project	Infrastructure	Capital Asst	Employment Asst
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Price	-0.056	0.119	1.702**	-0.585	4.053	1.828	3.316**	-1.057
	(0.513)	(0.634)	(0.777)	(0.602)	(2.672)	(1.358)	(1.321)	(1.055)
Price × Suitability	0.103	0.005	-0.357**	0.127	-1.088*	-0.618**	-0.714**	0.227
	(0.112)	(0.135)	(0.170)	(0.126)	(0.582)	(0.297)	(0.290)	(0.221)
Village FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District-specific Trend	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	145654	145654	145654	145654	145654	145654	145654	145654
R-Squared	0.563	0.766	0.506	0.455	0.693	0.736	0.560	0.468
Mean of Dep. Var.	0.840	0.705	0.636	0.274	3.144	1.680	1.044	0.403

Note: The sample for all regressions only include PODES 2008 onwards because information on development projects only available starting in 2008. All regressions include population (log), distance to district capital (log), distance to sub district capital (log), village area (log) interacted with year fixed effects, harvested areas (log) allocated for planting rice interacted with year fixed effects, and rainfall shock. All regressions include village and year fixed-effects as well as district-specific trends. Standard errors, reported in parentheses, are robust to heteroskedasticity and clustered at district level.. $*p < 0.10, **p < 0.05, ***p < 0.01$

Table A.2.9: Public Goods: Health Facilities and Personnel – Controlling for Baseline Lights Coverage Interacted with Year Fixed Effects

	Δ Presence			
	Doctors	Health Center (Main)	Health Center (Small)	Health Center (Total)
	(1)	(2)	(3)	(4)
Price	0.046	-0.052	0.435***	0.374**
	(0.096)	(0.075)	(0.160)	(0.156)
Price × Suitability	-0.007	0.005	-0.081**	-0.077**
	(0.021)	(0.015)	(0.033)	(0.032)
Village FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
District-specific Trend	Yes	Yes	Yes	Yes
N	237063	237063	237063	237063
R-Squared	0.080	0.096	0.075	0.073
Mean of Dep. Var.	0.199	0.129	0.335	0.442
Panel B: Intensive Margins				
Price	-0.244*	-0.017	0.184*	0.188*
	(0.134)	(0.044)	(0.100)	(0.111)
Price × Suitability	0.046*	0.001	-0.034	-0.037
	(0.027)	(0.009)	(0.022)	(0.024)
Village FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
District-specific Trend	Yes	Yes	Yes	Yes
N	190383	188955	188955	188955
R-Squared	0.131	0.155	0.130	0.136
Mean of Dep. Var.	0.639	0.131	0.350	0.482

Note: This table presents the effects on changes in public good provision in health facilities and personnels. Panel A presents estimation results of changes in extensive margins. Panel B presents results of changes in intensive margins. All regressions include population (log), distance to district capital (log), distance to sub district capital (log), village area (log) interacted with year fixed effects, harvested areas (log) allocated for planting rice interacted with year fixed effects, and baseline (in 2000) nighttime lights coverage interacted with year fixed effects. Dependent variables in columns 1 to 4 of Panel B are not available in PODES 2008. Standard errors, reported in parentheses, are robust to heteroskedasticity and clustered at district level. $*p < 0.10, **p < 0.05, ***p < 0.01$

Table A.2.10: Development Projects – Controlling for Baseline Lights Coverage Interacted with Year Fixed Effects

	Presence				Number			
	All project	Infrastructure	Capital Asst	Employment Asst	All project	Infrastructure	Capital Asst	Employment Asst
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Price	0.229	0.323	1.091	-0.337	2.907	0.512	2.857**	-0.576
	(0.505)	(0.654)	(0.711)	(0.616)	(2.762)	(1.389)	(1.290)	(1.096)
Price × Suitability	0.036	-0.040	-0.219	0.068	-0.828	-0.311	-0.613**	0.113
	(0.111)	(0.139)	(0.159)	(0.129)	(0.603)	(0.299)	(0.286)	(0.230)
Village FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District-specific Trend	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	145654	145654	145654	145654	145654	145654	145654	145654
R-Squared	0.563	0.767	0.507	0.455	0.693	0.736	0.560	0.468
Mean of Dep. Var.	0.840	0.705	0.636	0.274	3.144	1.680	1.044	0.403

Note: The sample for all regressions only include PODES 2008 onwards because information on development projects only available starting in 2008. All regressions include population (log), distance to district capital (log), distance to sub district capital (log), village area (log) interacted with year fixed effects, harvested areas (log) allocated for planting rice interacted with year fixed effects, and baseline (in 2000) nighttime lights coverage interacted with year fixed effects. All regressions include village and year fixed-effects as well as district-specific trends. Standard errors, reported in parentheses, are robust to heteroskedasticity and clustered at district level. $*p < 0.10$, $**p < 0.05$, $***p < 0.01$

Table A.2.11: Public Goods: Health Facilities and Personnel –Controlling for Baseline Lights Intensity Interacted with Year Fixed Effects

	Δ Presence			
	Doctors	Health Center (Main)	Health Center (Small)	Health Center (Total)
	(1)	(2)	(3)	(4)
Price	0.197	0.035	0.376**	0.369**
	(0.133)	(0.100)	(0.187)	(0.180)
Price × Suitability	-0.036	-0.021	-0.066*	-0.080**
	(0.029)	(0.020)	(0.039)	(0.037)
Village FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
District-specific Trend	Yes	Yes	Yes	Yes
N	110127	110127	110127	110127
R-Squared	0.176	0.190	0.179	0.176
Mean of Dep. Var.	0.199	0.129	0.335	0.442
Panel B: Intensive Margins				
Price	-0.164	0.054	0.012	0.113
	(0.168)	(0.051)	(0.112)	(0.117)
Price × Suitability	0.032	-0.016	0.002	-0.024
	(0.035)	(0.011)	(0.024)	(0.025)
Village FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
District-specific Trend	Yes	Yes	Yes	Yes
N	85019	84580	84580	84580
R-Squared	0.233	0.242	0.227	0.232
Mean of Dep. Var.	0.639	0.131	0.350	0.482

Note: This table presents the effects on changes in public good provision in health facilities and personnels. Panel A presents estimation results of changes in extensive margins. Panel B presents results of changes in intensive margins. All regressions include population (log), distance to district capital (log), distance to sub district capital (log), village area (log) interacted with year fixed effects, harvested areas (log) allocated for planting rice interacted with year fixed effects, and baseline (in 2000) nighttime lights coverage interacted with year fixed effects. Dependent variables in columns 1 to 4 of Panel B are not available in PODES 2008. Standard errors, reported in parentheses, are robust to heteroskedasticity and clustered at district level. $*p < 0.10, **p < 0.05, ***p < 0.01$

Table A.2.12: Development Projects Controlling for Baseline Lights Intensity Interacted with Year Fixed Effects

	Presence				Number			
	All project	Infrastructure	Capital Asst	Employment Asst	All project	Infrastructure	Capital Asst	Employment Asst
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Price	0.044	0.170	1.818**	-0.873	4.690	1.848	4.166**	-1.291
	(0.575)	(0.657)	(0.914)	(0.690)	(2.932)	(1.623)	(1.655)	(1.091)
Price × Suitability	0.071	-0.011	-0.374*	0.160	-1.263**	-0.608*	-0.889**	0.220
	(0.124)	(0.137)	(0.199)	(0.143)	(0.637)	(0.357)	(0.358)	(0.230)
Village FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District-specific Trend	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	60361	60361	60361	60361	60361	60361	60361	60361
R-Squared	0.611	0.791	0.556	0.509	0.726	0.768	0.606	0.522
Mean of Dep. Var.	0.840	0.705	0.636	0.274	3.144	1.680	1.044	0.403

Note: The sample for all regressions only include PODES 2008 onwards because information on development projects only available starting in 2008. All regressions include population (log), distance to district capital (log), distance to sub district capital (log), village area (log) interacted with year fixed effects, harvested areas (log) allocated for planting rice interacted with year fixed effects, and baseline (in 2000) nighttime lights coverage interacted with time trend. All regressions include village and year fixed-effects as well as district-specific trends. Standard errors, reported in parentheses, are robust to heteroskedasticity and clustered at district level. $*p < 0.10, **p < 0.05, ***p < 0.01$

Table A.2.13: Public Goods: Health Facilities and Personnel – Controlling for Price Change Interacted with Baseline Lights Coverage

	Δ Presence			
	Doctors	Health Center (Main)	Health Center (Small)	Health Center (Total)
	(1)	(2)	(3)	(4)
Price	0.110	-0.038	0.465^{***}	0.418^{***}
	(0.094)	(0.068)	(0.153)	(0.148)
Price × Suitability	-0.008	0.002	-0.089^{***}	-0.086^{***}
	(0.020)	(0.014)	(0.033)	(0.031)
Village FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
District-specific Trend	Yes	Yes	Yes	Yes
N	237063	237063	237063	237063
R-Squared	0.080	0.096	0.075	0.073
Mean of Dep. Var.	0.199	0.129	0.335	0.442
Panel B: Intensive Margins				
Price	-0.162	-0.004	0.175^{*}	0.190^{*}
	(0.133)	(0.043)	(0.097)	(0.107)
Price × Suitability	0.043	-0.001	-0.034	-0.040^{*}
	(0.027)	(0.009)	(0.021)	(0.023)
Village FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
District-specific Trend	Yes	Yes	Yes	Yes
N	190383	188955	188955	188955
R-Squared	0.131	0.155	0.130	0.136
Mean of Dep. Var.	0.639	0.131	0.350	0.482

Note: This table presents the effects on changes in public good provision in health facilities and personnels. Panel A presents estimation results of changes in extensive margins. Panel B presents results of changes in intensive margins. All regressions include population (log), distance to district capital (log), distance to sub district capital (log), village area (log) interacted with year fixed effects, harvested areas (log) allocated for planting rice interacted with year fixed effects, and price change interacted with baseline (in 2000) nighttime lights coverage. Dependent variables in columns 1 to 4 of Panel B are not available in PODES 2008. Standard errors, reported in parentheses, are robust to heteroskedasticity and clustered at district level. $*p < 0.10, **p < 0.05, ***p < 0.01$

Table A.2.14: Development Projects – Controlling for Price Change Interacted with Baseline Lights Coverage

	Presence				Number			
	All project	Infrastructure	Capital Asst	Employment Asst	All project	Infrastructure	Capital Asst	Employment Asst
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Price	-0.058	0.132	1.661**	-0.606	3.939	1.806	3.259**	-1.099
	(0.510)	(0.633)	(0.751)	(0.603)	(2.636)	(1.343)	(1.303)	(1.056)
Price × Suitability	0.092	-0.011	-0.300*	0.152	-0.901	-0.536*	-0.651**	0.278
	(0.114)	(0.137)	(0.163)	(0.128)	(0.587)	(0.297)	(0.291)	(0.226)
Village FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District-specific Trend	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	145654	145654	145654	145654	145654	145654	145654	145654
R-Squared	0.563	0.767	0.507	0.455	0.693	0.736	0.560	0.468
Mean of Dep. Var.	0.840	0.705	0.636	0.274	3.144	1.680	1.044	0.403

Note: The sample for all regressions only include PODES 2008 onwards because information on development projects only available starting in 2008. All regressions include population (log), distance to district capital (log), distance to sub district capital (log), village area (log) interacted with year fixed effects, harvested areas (log) allocated for planting rice interacted with year fixed effects, and price change interacted with baseline (in 2000) nighttime lights coverage. All regressions include village and year fixed-effects as well as district-specific trends. Standard errors, reported in parentheses, are robust to heteroskedasticity and clustered at district level.. $*p < 0.10, **p < 0.05, ***p < 0.01$

Table A.2.15: Public Goods: Health Facilities and Personnel – Controlling for Time Trends Interacted with Rice Suitability

	Δ Presence			
	Doctors	Health Center (Main)	Health Center (Small)	Health Center (Total)
	(1)	(2)	(3)	(4)
Price	0.157*	-0.047	0.463***	0.411***
	(0.095)	(0.068)	(0.154)	(0.149)
Price × Suitability	-0.030	0.005	-0.087***	-0.084***
	(0.021)	(0.014)	(0.032)	(0.030)
Village FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
District-specific Trend	Yes	Yes	Yes	Yes
N	237063	237063	237063	237063
R-Squared	0.080	0.096	0.075	0.073
Mean of Dep. Var.	0.199	0.129	0.335	0.442
Panel B: Intensive Margins				
Price	-0.099	-0.008	0.174*	0.178
	(0.126)	(0.044)	(0.099)	(0.111)
Price × Suitability	0.013	0.000	-0.031	-0.034
	(0.025)	(0.009)	(0.022)	(0.024)
Village FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
District-specific Trend	Yes	Yes	Yes	Yes
N	190383	188955	188955	188955
R-Squared	0.131	0.155	0.130	0.136
Mean of Dep. Var.	0.639	0.131	0.350	0.482

Note: This table presents the effects on changes in public good provision in health facilities and personnels. Panel A presents estimation results of changes in extensive margins. Panel B presents results of changes in intensive margins. All regressions include population (log), distance to district capital (log), distance to sub district capital (log), village area (log) interacted with year fixed effects, harvested areas (log) allocated for planting rice interacted with year fixed effects, and rice suitability interacted with time-trend. Dependent variables in columns 1 to 4 of Panel B are not available in PODES 2008. Standard errors, reported in parentheses, are robust to heteroskedasticity and clustered at district level. $*p < 0.10, **p < 0.05, ***p < 0.01$

Table A.2.16: Development Projects – Controlling for Time Trends Interacted with Rice Suitability

	Presence				Number			
	All project	Infrastructure	Capital Asst	Employment Asst	All project	Infrastructure	Capital Asst	Employment Asst
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Price	0.367	0.785	2.796***	-0.440	6.687**	2.530	4.771***	-0.529
	(0.656)	(0.859)	(1.055)	(0.801)	(3.351)	(1.803)	(1.749)	(1.302)
Price × Suitability	0.011	-0.135	-0.591**	0.094	-1.649**	-0.763*	-1.027***	0.112
	(0.144)	(0.184)	(0.231)	(0.169)	(0.738)	(0.394)	(0.385)	(0.275)
Village FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District-specific Trend	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	145654	145654	145654	145654	145654	145654	145654	145654
R-Squared	0.563	0.767	0.506	0.455	0.693	0.736	0.560	0.468
Mean of Dep. Var.	0.840	0.705	0.636	0.274	3.144	1.680	1.044	0.403

Note: The sample for all regressions only include PODES 2008 onwards because information on development projects only available starting in 2008. All regressions include population (log), distance to district capital (log), distance to sub district capital (log), village area (log) interacted with year fixed effects, harvested areas (log) allocated for planting rice interacted with year fixed effects, village area (log) interacted with year fixed effects, and rice suitability interacted with time-trend. All regressions include village and year fixed-effects as well as district-specific trends. Standard errors, reported in parentheses, are robust to heteroskedasticity and clustered at district level.. $*p < 0.10, **\ p < 0.05, ***p < 0.01$

APPENDIX B

CHAPTER 2 OF APPENDIX

B.1 NHIS Service Coverage

Out-Patient Services

- General and specialized consultation and review
- Requested investigation (including laboratory investigations, x-rays and ultrasound scanning)
- Medication (prescription drugs on the NHIS Drug List)
- HIV/AIDS symptomatic treatment for opportunistic infection
- Out-patient/Day Surgery Operations including hernia repairs, incision and drainage, hemorrhoidectomy
- Out-patient physiotherapy

In-Patient Services

- General and specialist in-patient care
- Requested investigations
- Medication (prescription drugs on NHIS Drug List)
- Cervical and Breast Cancer Treatment
- Surgical Operations
- In-patient physiotherapy

- Accommodation in general ward
- Feeding (where available)

Oral Health Services

- Pain relief which includes incision and drainage, tooth extraction and temporary relief
- Dental restoration which includes simple amalgam, fillings and temporary dressing

Eye Care Services

- Refraction, visual fields and A-Scan
- Keratometry
- Cataract removal
- Eye lid surgery

Maternity Care

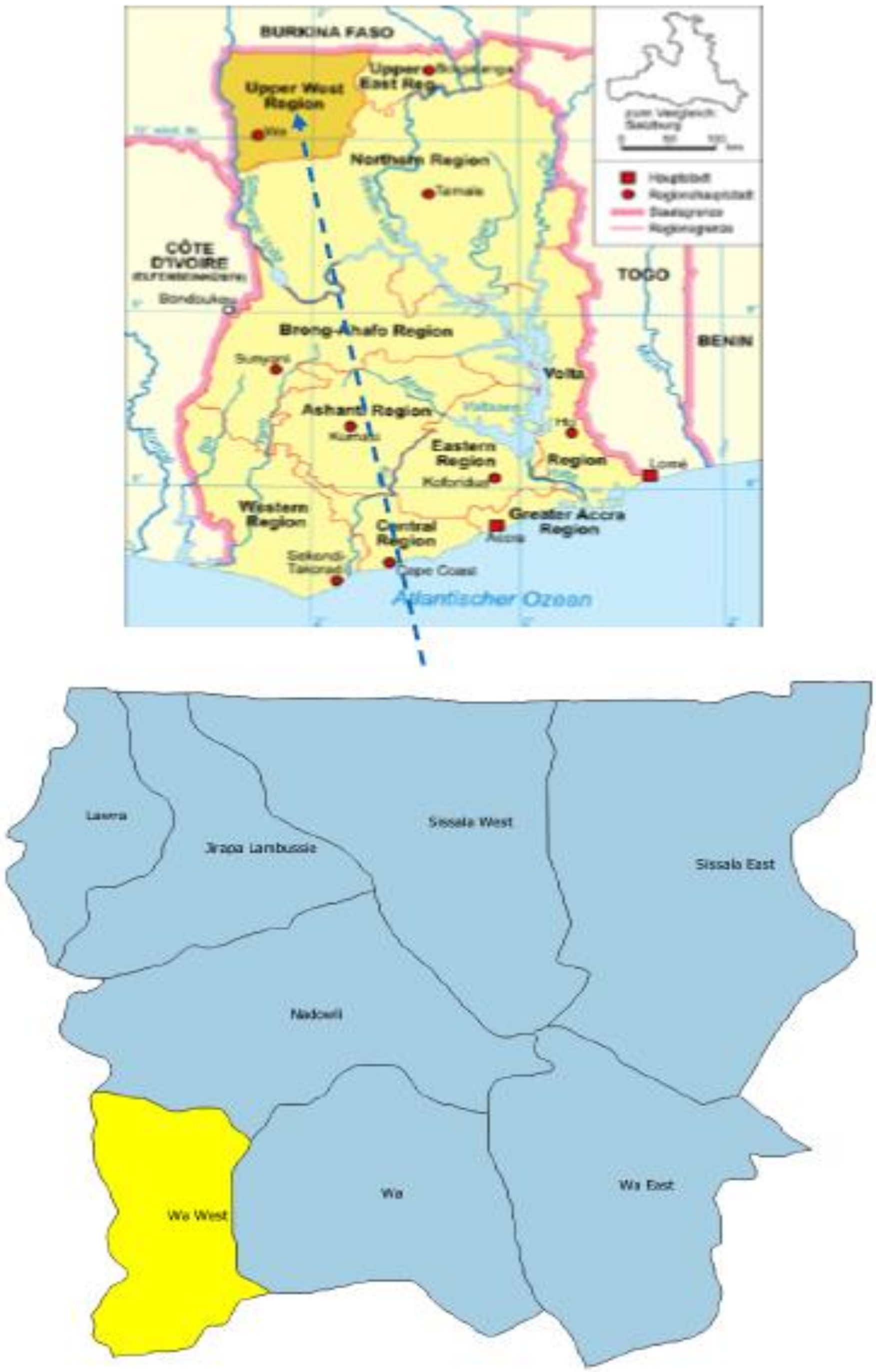
- Antenatal care
- Deliveries (normal and assisted)
- Caesarian section
- Postnatal care

Emergencies

- Medical emergencies
- Surgical emergencies including brain surgery due to accidents
- Pediatric emergencies
- Obstetric and gynecological emergencies
- Road traffic accidents
- Industrial and workplace accidents
- Dialysis for acute renal failure

B.2 Figures and Tables

Figure B.1: Ghana and Wa West District Map



Note: This map shows Ghana (upper panel) and the Upper West region of Ghana (lower panel), which includes Wa West district (highlighted).

Figure B.2: Original Study Design

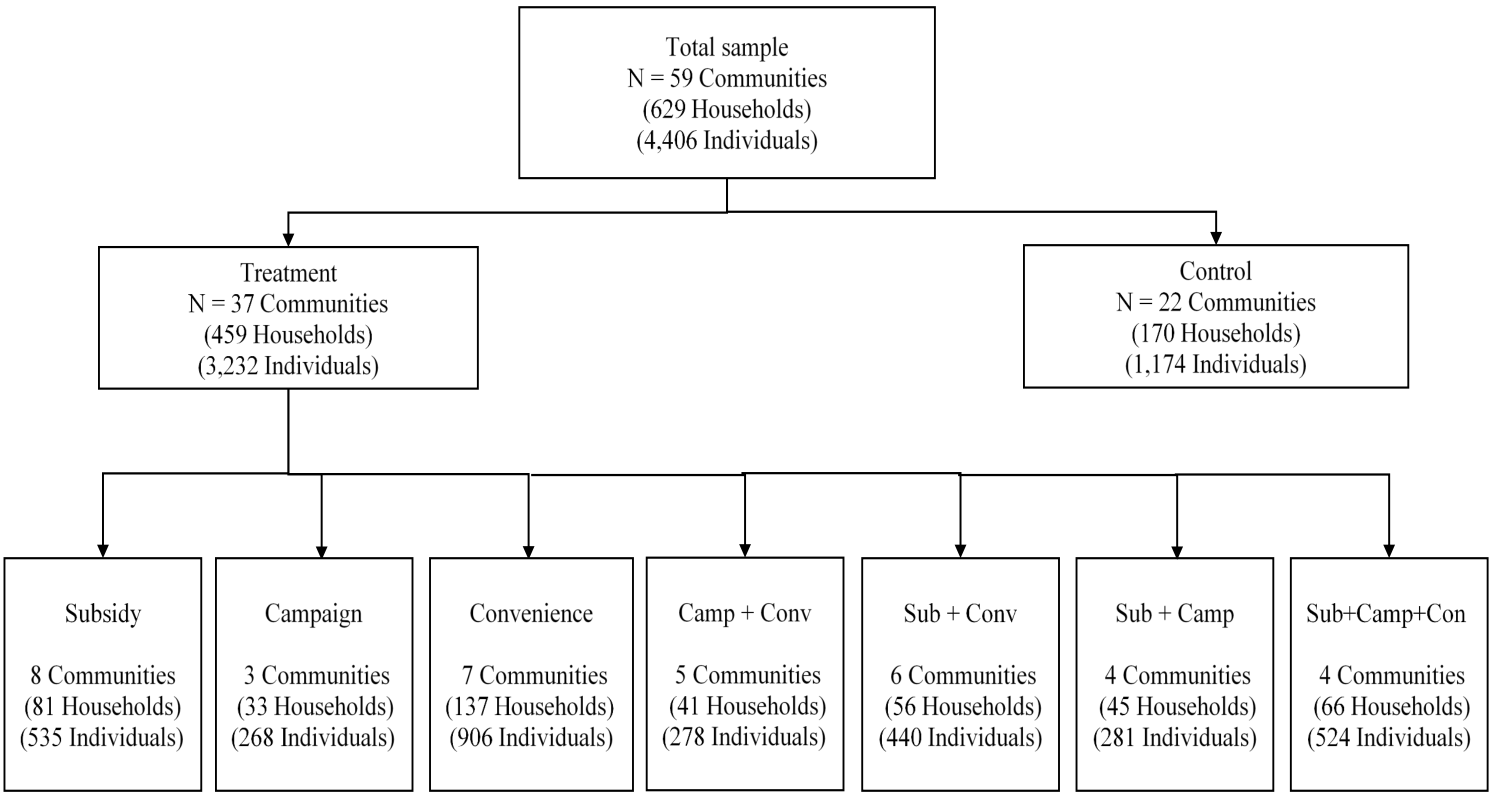


Table B.2.1: Attrition

	Short run	Long run
	(1)	(2)
Panel A		
Any subsidy	0.005	-0.045
	(0.021)	(0.036)
	[0.855]	[0.259]
R-squared	0.133	0.102
Panel B		
Partial subsidy	0.004	-0.042
	(0.025)	(0.038)
	[0.895]	[0.302]
Full subsidy	0.013	-0.065
	(0.044)	(0.053)
	[0.809]	[0.311]
R-squared	0.144	0.104
Panel C		
1/3 subsidy	-0.005	-0.039
	(0.043)	(0.051)
	[0.924]	[0.533]
2/3 subsidy	0.011	-0.045
	(0.024)	(0.042)
	[0.664]	[0.314]
Full subsidy	0.014	-0.066
	(0.044)	(0.051)
	[0.779]	[0.298]
R-squared	0.144	0.104
Mean	0.05	0.21
Number of observations	2953	2953

Note: Dependent variable is a binary variable indicating whether an individual had been attrited in the short- and long-run follow-up surveys. All regressions include a standard set of covariates (individual, household, and community). Robust standard errors clustered at community level reported in parantheses. *, **, and *** denote statistical significance at 10 %, 5 %, and 1 % levels, respectively.

Table B.2.2: Selective Retention of Health Insurance by Characteristics

Sample	Among those enrolled in the baseline				
Independent variable: Enrolled at the first follow-up	Coefficient	Standard error	bootstrap p-values	N	R-squared
	(1)	(2)	(3)	(4)	(5)
Panel A: Short run					
Healthy or very healthy	0.020	(0.059)	0.740	161	0.001
# Days ill last month	-0.354	(0.340)	0.338	531	0.004
Could not perform normal daily activities due to illness last month	0.029	(0.038)	0.507	535	0.002
# days could not perform normal daily activities in the last month	-0.037	(0.683)	0.979	535	0.00001
# Days ill last month (Malaria)	-0.064	(0.119)	0.617	531	0.001
Could not perform normal daily activities due to illness last month (Malaria)	0.017	(0.019)	0.444	532	0.002
# days could not perform normal daily activities in the last month (Malaria)	-0.002	(0.271)	0.990	532	0.0000001
Visited health facility in last four weeks	0.035	(0.025)	0.219	497	0.004
Visited health facility in last six months	0.114**	(0.044)	0.027	513	0.017
# of visits in last six months	0.037*	(0.021)	0.121	494	0.004
Visited Facility for malaria treatment in the last four weeks	0.034*	(0.020)	0.146	511	0.005
Made an out-of-pocket for health service in the last six months	-0.010	(0.029)	0.849	535	0.001
Standardized treatment effects (health status)	-0.001	(0.017)		3,357	0.00001
Standardized treatment effects (health care utilization)	0.030*	(0.017)		2,550	0.004
Sample	Among those enrolled in the short run				
Independent variable: Enrolled at the second follow-up	Coefficient	Standard error	bootstrap p-values	N	R-squared
	(1)	(2)	(3)	(4)	(5)
Panel B: Long run					
Healthy or very healthy	-0.008	(0.067)	0.910	360	0.0001
# Days ill last month	0.210	(0.167)	0.253	1,305	0.003
Could not perform normal daily activities due to illness last month	0.021	(0.018)	0.239	1,305	0.003
# days could not perform normal daily activities in the last month	0.119	(0.135)	0.520	1,305	0.002
# Days ill last month (Malaria)	0.049	(0.093)	0.625	1,305	0.0003
Could not perform normal daily activities due to illness last month (Malaria)	0.016	(0.015)	0.305	1,305	0.002
# days could not perform normal daily activities in the last month (Malaria)	0.066	(0.088)	0.557	1,305	0.001
Visited health facility in last four weeks	0.047**	(0.018)	0.003	1,305	0.012
Visited health facility in last six months	0.139***	(0.038)	0.000	1,305	0.042
# of visits in last six months	0.038**	(0.018)	0.017	1,305	0.008
Visited Facility for malaria treatment in the last four weeks	0.030*	(0.016)	0.044	1,305	0.006
Made an out-of-pocket for health service in the last six months	-0.025*	(0.013)	0.074	1,305	0.007
Standardized treatment effects (health status)	0.013	(0.012)		8,190	0.001
Standardized treatment effects (health care utilization)	0.038**	(0.018)		6,525	0.006

Note: This table reports estimation results of running univariate regression of each selected health characteristics on an enrollment indicator in short and long-run. Panel A summarizes regression results when sample is restricted to those who enrolled in the baseline. Panel B summarizes results when sample is restricted to those who enrolled in the short run. Standardized treatment effects on health status and health care utilization in the short and long run are reported in the last two rows of Panels A and B, respectively. Robust standard errors clustered at community level reported in parantheses. Robust standard errors clustered at community level reported in parantheses. Wild-cluster bootstrap-t p-values are reported in Column 3. *, **, and *** denote statistical significance at 10 %, 5 %, and 1 % level respectively.

Table B.2.3: Effects on Health Behaviors

	Short run	Long run			
	Sleep under mosquito nets (1)	Have mosquito nets (2)	Sleep under mosquito nets (3)	Water safe to drink (4)	Standardized treatment effects (5)
Panel A: ITT results					
Panel A1					
Any subsidy	0.085	-0.035	0.016	-0.072	-0.032
	(0.065)	(0.088)	(0.072)	(0.049)	(0.041)
	[0.251]	[0.752]	[0.85]	[0.218]	
		0.933	0.933	0.588	
R-squared	0.233	0.258	0.235	0.257	0.155
Panel A2					
Partial subsidy	0.098	0.039	0.036	-0.071	-0.018
	(0.113)	(0.094)	(0.123)	(0.045)	(0.044)
	[0.457]	[0.758]	[0.806]	[0.149]	
		0.926	0.926	0.522	
Full subsidy	0.227*	-0.269**	-0.044	-0.014	-0.117**
	(0.118)	(0.106)	(0.118)	(0.068)	(0.051)
	[0.113]	[0.064]	[0.749]	[0.878]	
		0.241	0.946	0.946	
R-squared	0.247	0.318	0.259	0.275	0.179
Panel A3					
1/3 subsidy	0.020	0.146	0.072	-0.054	0.021
	(0.110)	(0.117)	(0.127)	(0.057)	(0.047)
	[0.886]	[0.343]	[0.664]	[0.394]	
		0.716	0.724	0.724	
2/3 subsidy	0.158	-0.065	0.009	-0.087*	-0.055
	(0.141)	(0.089)	(0.131)	(0.044)	(0.049)
	[0.396]	[0.567]	[0.959]	[0.061]	
		0.796	0.956	0.355	
Full subsidy	0.238**	-0.294***	-0.050	-0.017	-0.127**
	(0.118)	(0.097)	(0.120)	(0.068)	(0.051)
	[0.088]	[0.022]	[0.723]	[0.829]	
		0.143	0.931	0.931	
R-squared	0.252	0.333	0.260	0.276	0.182
Number of observations	1,422	1,101	1,092	497	2,069
Panel B: 2SLS results					
Enrolled in NHIS	0.306	-0.184	0.027	-0.091	-0.085
	(0.204)	(0.148)	(0.219)	(0.083)	(0.078)
First-stage F-statistics	29.175	38.614	28.639	42.639	56.943
Control group mean	0.447	0.290	0.661	0.080	0.007
P-values on test of equality:					
Partial subsidy = Full subsidy	0.274	0.001	0.179	0.166	0.003
1/3 subsidy = 2/3 subsidy	0.303	0.043	0.382	0.482	0.008
1/3 subsidy = Full subsidy	0.096	0.0001	0.131	0.490	0.00004
2/3 subsidy = Full subsidy	0.544	0.011	0.340	0.106	0.049

Note: Health behaviors are measured for those aged 12 years and above. Dependent variable in Column 4 is an indicator variable of whether a household member does anything to their water to make it safe to drink. Panels A and B report ITT and 2SLS results, respectively. Panels A1, A2, and A3 report the effects of receiving any subsidy, partial and full subsidy, and each subsidy level (1/3, 2/3, and full), respectively. All regressions include a standard set of covariates (individual, household, and community) and baseline measure of dependent variable. Standardized treatment effect in the long run is reported in Column 5. P-values for the equality of effect estimates for various pairs of treatment groups are also presented. Robust standard errors clustered at community level are reported in parentheses. Wild-cluster bootstrap-t p-values are reported in square brackets. Family-wise p-values are reported in curly brackets. *, **, and *** denote statistical significance at 10 %, 5 %, and 1 % level respectively.

B.3 Tables

Table B.3.1: Effects of the Original Interventions on Enrollment

	Enrollment	
	Short-run	Long-run
	(1)	(2)
Subsidy only	0.436***	0.160*
	(0.046)	(0.082)
Campaign only	0.161**	0.044
	(0.080)	(0.066)
Convenience only	0.007	0.195***
	(0.066)	(0.072)
Campaign & Convenience	0.231	0.182
	(0.165)	(0.159)
Subsidy & Convenience	0.347***	0.155**
	(0.078)	(0.064)
Subsidy & Campaign	0.520***	0.080
	(0.075)	(0.094)
Subsidy & Camp & Conven	0.458***	0.397***
	(0.064)	(0.083)
R-squared	0.318	0.166
Mean	0.504	0.379
Control group mean	0.272	0.230
Number of observations	4,168	3,415
P-value on test of equality		
Sub + Camp = Sub & Camp	0.477	0.330
Sub + Conv = Sub & Conv	0.323	0.090
Camp + Conv = Camp & Conv	0.756	0.770
Sub + Camp + Conv = Sub & Camp & Conv	0.211	0.991

Note: This table presents the effects of original intervention on enrollment in health insurance in short and long run. All regressions include a standard set of covariates (individual, household, and community) and baseline measure of dependent variable. *P*-values for the equality of effect estimates are also presented. Robust standard errors clustered at the community level are reported in parentheses. *, **, and *** denote statistical significance at 10 %, 5 %, and 1 % levels, respectively.

Table B.3.2: Effects on Enrollment with Restricted Sample

	Enrollment	
	Short-run	Long-run
	(1)	(4)
Panel A		
Any Subsidy	0.424***	0.135*
	(0.044)	(0.074)
R-squared	0.399	0.228
Panel B		
Partial subsidy (positive price)	0.405***	0.095
	(0.047)	(0.067)
Full subsidy (free)	0.514***	0.317***
	(0.090)	(0.105)
R-squared	0.401	0.238
Panel C		
1/3 subsidy	0.387***	0.137*
	(0.084)	(0.080)
2/3 subsidy	0.419***	0.063
	(0.062)	(0.067)
Full subsidy (free)	0.514***	0.316***
	(0.090)	(0.105)
R-squared	0.401	0.239
Mean	0.405	0.290
Control group mean	0.272	0.230
Number of observations	1,614	1,304

Note: This table corresponds to Table 2.2, but the sample is restricted to subsidy only and control groups. All regressions include a standard set of covariates (individual, household, and community) and baseline measure of dependent variable. Robust standard errors clustered at community level are reported in parentheses. *, **, and *** denote statistical significance at 10 %, 5 %, and 1 % levels, respectively.

Table B.3.3: Effects on Healthcare Services Utilization with Restricted Sample (Short Run)

	Short run					
	Visited health facility in last four weeks	Visited health facility in last six months	# of visits in last four weekss	Visited Facility for malaria treatment in the last four weeks	Made an out-of-pocket for health service in the last six months	Standardized treatment effects
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A						
Any subsidy	-0.011	-0.009	-0.007	0.011	-0.018	-0.003
	(0.010)	(0.020)	(0.024)	(0.008)	(0.016)	(0.009)
R-squared	0.121	0.137	0.142	0.113	0.139	0.086
Panel B						
Partial subsidy	-0.012	-0.005	-0.004	0.014	-0.011	0.001
	(0.012)	(0.021)	(0.028)	(0.010)	(0.017)	(0.011)
Full subsidy	-0.001	-0.028	-0.025	-0.010	-0.049*	-0.020
	(0.020)	(0.044)	(0.019)	(0.013)	(0.025)	(0.013)
R-squared	0.121	0.137	0.143	0.114	0.141	0.086
Panel C						
1/3 subsidy	-0.019	-0.014	-0.027	0.016	-0.021	-0.002
	(0.020)	(0.023)	(0.038)	(0.012)	(0.016)	(0.013)
2/3 subsidy	-0.006	0.001	0.014	0.013	-0.003	0.003
	(0.015)	(0.030)	(0.032)	(0.011)	(0.024)	(0.013)
Full subsidy	-0.001	-0.028	-0.025	-0.010	-0.049*	-0.020
	(0.019)	(0.044)	(0.018)	(0.013)	(0.025)	(0.013)
R-squared	0.121	0.138	0.144	0.114	0.141	0.086
Control group mean	0.038	0.101	0.033	0.018	0.046	-0.011
Number of observations	1,200	1,566	1,196	1,263	1,622	6,191

Note: This table corresponds to Table 2.5, but the sample is restricted to subsidy only and control groups. All regressions include a standard set of covariates (individual, household, and community) and baseline measure of dependent variable. Robust standard errors clustered at community level are reported in parentheses. *, **, and *** denote statistical significance at 10 %, 5 %, and 1 % level respectively.

Table B.3.4: Effects on Healthcare Services Utilization with Restricted Sample (Long Run)

	Long run					
	Visited health facility in last four weeks	Visited health facility in last six months	# of visits in last four weekss	Visited Facility for malaria treatment in the last four weeks	Made an out-of-pocket for health service in the last six months	Standardized treatment effects
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A1						
Any subsidy	0.041***	0.096***	0.031**	0.028*	0.003	0.047***
	(0.013)	(0.026)	(0.012)	(0.014)	(0.006)	(0.014)
R-squared						0.077
Panel A2						
Partial subsidy	0.045***	0.086***	0.033**	0.030**	0.005	0.049***
	(0.013)	(0.020)	(0.012)	(0.014)	(0.007)	(0.014)
Full subsidy (free)	0.020	0.146**	0.024	0.020	-0.006	0.037
	(0.018)	(0.060)	(0.020)	(0.019)	(0.009)	(0.024)
R-squared						0.077
Panel A3						
1/3 subsidy	0.012	0.071***	0.012	0.013	-0.001	0.024**
	(0.010)	(0.025)	(0.009)	(0.009)	(0.009)	(0.010)
2/3 subsidy	0.070***	0.097***	0.048**	0.043*	0.009	0.069***
	(0.018)	(0.032)	(0.019)	(0.021)	(0.009)	(0.023)
Full subsidy (free)	0.020	0.146**	0.024	0.020	-0.006	0.038
	(0.019)	(0.060)	(0.020)	(0.020)	(0.009)	(0.024)
R-squared	0.117	0.123	0.110	0.107	0.091	0.079
Control group mean	0.014	0.044	0.011	0.009	0.012	-0.026
Number of observations	1,236	1,546	1,238	1,236	1,546	6,180

Note: This table corresponds to Table 2.6, but the sample is restricted to subsidy only and control groups. All regressions include a standard set of covariates (individual, household, and community) and baseline measure of dependent variable. Robust standard errors clustered at community level are reported in parentheses. *, **, and *** denote statistical significance at 10 %, 5 %, and 1 % level respectively.

Table B.3.5: Effects on Health Status with Restricted Sample (Short Run)

	Short run				
	Healthy or very healthy	# Days ill last four weeks	Could not perform normal daily activities due to illness last four weeks	# days could not perform normal daily activities due to illness in the last four weeks	Standardized treatment effects
	(1)	(2)	(3)	(4)	(5)
Panel A					
Any subsidy	0.148***	-0.421**	-0.025	-0.315	-0.037**
	(0.043)	(0.185)	(0.017)	(0.447)	(0.017)
R-squared	0.346	0.139	0.136	0.141	0.107
Panel B					
Partial subsidy (positive price)	0.152***	-0.445**	-0.021	-0.203	-0.033*
	(0.044)	(0.196)	(0.019)	(0.497)	(0.018)
Full subsidy (free)	0.130*	-0.308	-0.041	-0.854	-0.058**
	(0.076)	(0.283)	(0.038)	(0.619)	(0.027)
R-squared	0.346	0.139	0.136	0.142	0.107
Panel C					
1/3 subsidy	0.157***	-0.740**	-0.044	-0.916	-0.061**
	(0.047)	(0.300)	(0.031)	(0.728)	(0.027)
2/3 subsidy	0.147**	-0.225	-0.005	0.343	-0.011
	(0.061)	(0.250)	(0.021)	(0.517)	(0.021)
Full subsidy (free)	0.130	-0.298	-0.040	-0.836	-0.057**
	(0.077)	(0.287)	(0.038)	(0.617)	(0.027)
R-squared	0.346	0.141	0.137	0.145	0.109
Control group mean	0.818	0.616	0.082	1.376	0.011
Number of observations	478	1,597	1,603	1,549	5,081

Note: This table corresponds to Table 2.7, but the sample is restricted to subsidy only and control groups. All regressions include a standard set of covariates (individual, household, and community) and baseline measure of dependent variable. Robust standard errors clustered at community level are reported in parentheses. *, **, and *** denote statistical significance at 10 %, 5 %, and 1 % level respectively.

Table B.3.6: Effects on Health Status with Restricted Sample (Long Run)

	Long run				
	Healthy or very healthy	# Days ill last four weeks	Could not perform normal daily activities due to illness last four weeks	# days could not perform normal daily activities due to illness in the last four weeks	Standardized treatment effects
	(1)	(2)	(3)	(4)	(5)
Panel A					
Any subsidy	-0.128**	0.249**	0.042***	0.243**	0.050***
	(0.047)	(0.097)	(0.013)	(0.090)	(0.014)
R-squared	0.416	0.095	0.132	0.094	0.083
Panel B					
Partial subsidy	-0.133**	0.296***	0.045***	0.268**	0.055***
	(0.058)	(0.095)	(0.013)	(0.097)	(0.014)
Full subsidy	-0.111	0.019	0.027*	0.122	0.026
	(0.126)	(0.157)	(0.013)	(0.097)	(0.020)
R-squared	0.416	0.096	0.133	0.095	0.084
Panel C					
1/3 subsidy	-0.115**	0.287**	0.027***	0.253**	0.045***
	(0.051)	(0.107)	(0.010)	(0.109)	(0.014)
2/3 subsidy	-0.149	0.303**	0.059***	0.279**	0.063***
	(0.088)	(0.128)	(0.018)	(0.123)	(0.019)
Full subsidy	-0.110	0.019	0.027*	0.123	0.026
	(0.127)	(0.157)	(0.013)	(0.098)	(0.020)
R-squared	0.416	0.096	0.135	0.095	0.084
Control group mean	0.792	0.355	0.012	0.083	-0.019
Number of observations	416	1,531	1,530	1,475	4,814

Note: This table corresponds to Table 2.8, but the sample is restricted to subsidy only and control groups. All regressions include a standard set of covariates (individual, household, and community) and baseline measure of dependent variable. Robust standard errors clustered at community level are reported in parentheses. *, **, and *** denote statistical significance at 10 %, 5 %, and 1 % level respectively.

BIBLIOGRAPHY

Acharya, A., S. Vellakkal, F. Taylor, E. Masset, A. Satija, M. Burke, and S. Ebrahim (2013). The impact of health insurance schemes for the informal sector in low-and middle-income countries: a systematic review. *The World Bank Research Observer 28*(2), 236–266.

Akabayashi, H. and G. Psacharopoulos (1999). The trade-off between child labour and human capital formation: A tanzanian case study. *The Journal of Development Studies 35*(5), 120–140.

Alesina, A., R. Baqir, and W. Easterly (1999). Public goods and ethnic divisions. *The Quarterly Journal of Economics 114*(4), 1243–1284.

Alesina, A., A. Devleeschauwer, W. Easterly, S. Kurlat, and R. Wacziarg (2003). Fractionalization. *Journal of Economic growth 8*(2), 155–194.

Alesina, A. and E. La Ferrara (2000). Participation in heterogeneous communities. *The quarterly journal of economics 115*(3), 847–904.

Alhassan, R. K., E. Nketiah-Amponsah, and D. K. Arhinful (2016). A review of the national health insurance scheme in ghana: what are the sustainability threats and prospects? *PloS one 11*(11).

Almond, D. and J. J. Doyle (2011). After midnight: A regression discontinuity design in length of postpartum hospital stays. *American Economic Journal: Economic Policy 3*(3), 1–34.

Amodio, F., L. Baccini, and M. Di Maio (2020, 01). Security, Trade, and Political Violence. *Journal of the European Economic Association*. jvz060.

Antlöv, H. (2003). Village government and rural development in indonesia: The new democratic framework. *Bulletin of Indonesian Economic Studies 39*(2), 193–214.

Antlöv, H., A. Wetterberg, and L. Dharmawan (2016). Village governance, community life, and the 2014 village law in indonesia. *Bulletin of Indonesian Economic Studies 52*(2), 161–183.

Anukriti, S. and T. J. Kumler (2019). Women's worth: Trade, female income, and fertility in india. *Economic Development and Cultural Change 67*(3), 687–724.

Araujo, M. C., F. H. Ferreira, P. Lanjouw, and B. Özler (2008). Local inequality and project choice: Theory and evidence from ecuador. *Journal of Public Economics 92*(5-6), 1022–1046.

Ashagrie, K. (1998). Statistics on working children and hazardous child labour in brief.

Ashraf, N., J. Berry, and J. M. Shapiro (2010). Can higher prices stimulate product use? evidence from a field experiment in zambia. *American Economic Review 100*(5), 2383–2413.

Asuming, P. O. (2013). Getting the poor to enroll in health insurance, and its effects on their health: Evidence from a field experiment in ghana. *Job Market Paper–Columbia University*.

Autor, D., D. Dorn, and G. H. Hanson (2013). The china syndrome: Local labor market effects of import competition in the united states. *American Economic Review 103*(6), 2121–68.

Bah, A., S. Bazzi, S. Sumarto, and J. Tobias (2018, 04). Finding the Poor vs.

Measuring Their Poverty: Exploring the Drivers of Targeting Effectiveness in Indonesia. *The World Bank Economic Review*.

Baird, S., J. Friedman, and N. Schady (2011). Aggregate income shocks and infant mortality in the developing world. *Review of Economics and statistics 93*(3), 847–856.

Baland, J.-M. and J. A. Robinson (2000). Is child labor inefficient? *Journal of Political Economy 108*(4), 663–679.

Bandiera, O. and G. Levy (2010). Diversity and the power of the elites in democratic societies: A model and a test.

Banerjee, A., A. Deaton, and E. Duflo (2004). Wealth, health, and health services in rural rajasthan. *American Economic Review 94*(2), 326–330.

Banerjee, A., A. Finkelstein, R. Hanna, B. A. Olken, A. Ornaghi, and S. Sumarto (2019, August). The challenges of universal health insurance in developing countries: Evidence from a large-scale randomized experiment in indonesia. Working Paper 26204, National Bureau of Economic Research.

Banerjee, A., L. Iyer, and R. Somanathan (2007). Public action for public goods. *Handbook of development economics 4*, 3117–3154.

Banerjee, A. and R. Somanathan (2007). The political economy of public goods: Some evidence from india. *Journal of development Economics 82*(2), 287–314.

Bank, W. (1993). World development report: Investing in health. Technical report, World Bank.

Bardhan, P. (2000). Irrigation and cooperation: An empirical analysis of 48 irrigation communities in south india. *Economic Development and cultural change 48*(4), 847–865.

Bardhan, P. (2002). Decentralization of governance and development. *Journal of Economic perspectives 16*(4), 185–205.

Basu, K. (1999). Child labor: cause, consequence, and cure, with remarks on international labor standards. *Journal of Economic Literature 37*(3), 1083–1119.

Basu, K. (2000). The intriguing relation between adult minimum wage and child labour. *The Economic Journal 110*(462), C50–C61.

Basu, K. and P. H. Van (1998). The economics of child labor. *American Economic Review*, 412–427.

Bazzi, S. (2017). Wealth heterogeneity and the income elasticity of migration. *American Economic Journal: Applied Economics 9*(2), 219–55.

Bazzi, S., A. Gaduh, A. D. Rothenberg, and M. Wong (2016). Skill transferability, migration, and development: Evidence from population resettlement in indonesia. *American Economic Review 106*(9), 2658–98.

Beegle, K., R. Dehejia, and R. Gatti (2009). Why should we care about child labor? the education, labor market, and health consequences of child labor. *Journal of Human Resources 44*(4), 871–889.

Bertrand, M., E. Duflo, and S. Mullainathan (2004). How much should we trust differences-in-differences estimates? *The Quarterly Journal of Economics 119*(1), 249–275.

Besley, T. and S. Coate (2003). Centralized versus decentralized provision of local public goods: a political economy approach. *Journal of public economics 87*(12), 2611–2637.

Booth, A., R. M. Purnagunawan, and E. Satriawan (2019). Towards a healthy indonesia? *Bulletin of Indonesian Economic Studies 55*(2), 133–155.

Boudreaux, M. H., E. Golberstein, and D. D. McAlpine (2016). The long-term impacts of medicaid exposure in early childhood: Evidence from the program's origin. *Journal of Health Economics 45*, 161–175.

Cameron, A. C., J. B. Gelbach, and D. L. Miller (2008). Bootstrap-based improvements for inference with clustered errors. *The Review of Economics and Statistics 90*(3), 414–427.

Cameron, L., S. Olivia, and M. Shah (2019). Scaling up sanitation: evidence from an rct in indonesia. *Journal of development economics 138*, 1–16.

Campante, F. R. and Q.-A. Do (2014). Isolated capital cities, accountability, and corruption: Evidence from us states. *American Economic Review 104*(8), 2456–81.

Capuno, J. J., A. D. Kraft, S. Quimbo, C. R. Tan Jr, and A. Wagstaff (2016). Effects of price, information, and transactions cost interventions to raise voluntary enrollment in a social health insurance scheme: A randomized experiment in the philippines. *Health economics 25*(6), 650–662.

Casey, K., R. Glennerster, and E. Miguel (2012). Reshaping institutions: Evidence on aid impacts using a preanalysis plan. *The Quarterly Journal of Economics 127*(4), 1755–1812.

Cassar, A., A. Healy, and C. Von Kessler (2017). Trust, risk, and time preferences after a natural disaster: experimental evidence from thailand. *World Development 94*, 90–105.

Cassidy, T. (2019). How forward looking are local governments? evidence from indonesia. Unpublished.

Chavis, L. (2010). Decentralizing development: Allocating public goods via competition. *Journal of Development Economics 93*(2), 264–274.

Clogg, C. C., E. Petkova, and A. Haritou (1995). Statistical methods for comparing regression coefficients between models. *American Journal of Sociology 100*(5), 1261–1293.

Cohen, J. and P. Dupas (2010). Free distribution or cost-sharing? evidence from a randomized malaria prevention experiment. *Quarterly Journal of Economics 125*(1), 1.

Costinot, A., D. Donaldson, and C. Smith (2016). Evolving comparative advantage and the impact of climate change in agricultural markets: Evidence from 1.7 million fields around the world. *Journal of Political Economy 124*(1), 205–248.

Crépon, B. and G. J. Van Den Berg (2016). Active labor market policies. *Annual Review of Economics 8*, 521–546.

Cruz, C., J. Labonne, and P. Querubin (2020). Social network structures and the politics of public goods provision: evidence from the philippines. *American Political Science Review 114*(2), 486–501.

Currie, J., S. Decker, and W. Lin (2008). Has public health insurance for older children reduced disparities in access to care and health outcomes? *Journal of health Economics 27*(6), 1567–1581.

Das, J., A. Holla, A. Mohpal, and K. Muralidharan (2016). Quality and accountability in health care delivery: audit-study evidence from primary care in india. *American Economic Review 106*(12), 3765–99.

Dawe, D. (2008). Can indonesia trust the world rice market? *Bulletin of Indonesian Economic Studies 44*(1), 115–132.

Dayton-Johnson, J. (2000). Determinants of collective action on the local commons: a model with evidence from mexico. *Journal of Development Economics 62*(1), 181–208.

Deaton, A. (1989). Rice prices and income distribution in thailand: a non-parametric analysis. *The Economic Journal 99*(395), 1–37.

Deaton, A. (1997). *The Analysis of Household Surveys: A Microeconometric Approach to Development Policy*. World Bank Publications.

Dell, M. (2010). The persistent effects of peru's mining mita. *Econometrica 78*(6), 1863–1903.

Dercon, S. (2002). Income risk, coping strategies, and safety nets. *The World Bank Research Observer 17*(2), 141–166.

Dessy, S. E. and S. Pallage (2005). A theory of the worst forms of child labour. *The Economic Journal 115*(500), 68–87.

Dix-Carneiro, R. and B. K. Kovak (2017). Trade liberalization and regional dynamics. *American Economic Review 107*(10), 2908–46.

Dow, W., P. Gertly, R. F. Schoeni, J. Strauss, D. Thomas, et al. (1997). Health care prices, health and labor outcomes: Experimental evidence. Technical report.

Dube, O. and J. F. Vargas (2013). Commodity price shocks and civil conflict: Evidence from colombia. *The Review of Economic Studies 80*(4), 1384–1421.

Dumas, C. (2012). Does work impede child learning? the case of senegal. *Economic Development and Cultural Change 60*(4), 773–793.

Dupas, P. (2014). Short-run subsidies and long-run adoption of new health products: Evidence from a field experiment. *Econometrica 82*(1), 197–228.

Easterly, W. (2006). *The White Man's Burden: Why the West's Efforts to Aid the Rest Have Done So Much Ill and So Little Good*. Penguin.

Edmonds, E. (2008). Child labor. *Handbook of Development Economics 4*, 3609–3639.

Edmonds, E. V. (2007). Child labor. *Handbook of development economics 4*, 3607–3709.

Edmonds, E. V. and N. Pavcnik (2005). Child labor in the global economy. *Journal of Economic Perspectives 19*(1), 199–220.

Edmonds, E. V., N. Pavcnik, and P. Topalova (2010). Trade adjustment and human capital investments: Evidence from indian tariff reform. *American Economic Journal: Applied Economics 2*(4), 42–75.

Ethier, W. J. (1982). National and international returns to scale in the modern theory of international trade. *The American Economic Review 72*(3), 389–405.

Evans, M. F. and V. K. Smith (2005). Do new health conditions support mortality–air pollution effects? *Journal of Environmental Economics and Management 50*(3), 496–518.

Evers, P. (2000). Resourceful villagers, powerless communities: Rural village governance in indonesia. Technical report, Mimeo. World Bank Office Jakarta.

Fane, G. and P. Warr (2008). Agricultural protection in indonesia. *Bulletin of Indonesian Economic Studies 44*(1), 133–150.

Feler, L. and M. Z. Senses (2017). Trade shocks and the provision of local public goods. *American Economic Journal: Economic Policy 9*(4), 101–43.

Fenny, A. P., A. Kusi, D. K. Arhinful, and F. A. Asante (2016). Factors contributing to low uptake and renewal of health insurance: a qualitative study in ghana. *Global health research and policy 1*(1), 18.

Fink, G., P. J. Robyn, A. Sié, and R. Sauerborn (2013). Does health insurance improve health?: Evidence from a randomized community-based insurance rollout in rural burkina faso. *Journal of health economics 32*(6), 1043–1056.

Finkelstein, A., N. Hendren, and M. Shepard (2019). Subsidizing health insurance for low-income adults: Evidence from massachusetts. *American Economic Review 109*(4), 1530–67.

Finkelstein, A., S. Taubman, B. Wright, M. Bernstein, J. Gruber, J. P. Newhouse, H. Allen, K. Baicker, and O. H. S. Group (2012). The oregon health insurance experiment: evidence from the first year. *The Quarterly journal of economics 127*(3), 1057–1106.

Fiszbein, M. (2017). Agricultural diversity, structural change and long-run development: Evidence from the u.s. Working Paper 23183, National Bureau of Economic Research.

Fitrani, F., B. Hofman, and K. Kaiser * (2005, April). Unity in diversity? The creation of new local governments in a decentralising Indonesia. *Bulletin of Indonesian Economic Studies 41*(1), 57–79.

Gadenne, L. and M. Singhal (2014). Decentralization in developing economies. *Annu. Rev. Econ. 6*(1), 581–604.

Gajate-Garrido, G. and R. Owusua (2013). The national health insurance scheme in ghana: Implementation challenges and proposed solutions.

Galárraga, O., S. G. Sosa-Rubí, A. Salinas-Rodríguez, and S. Sesma-Vázquez (2010). Health insurance for the poor: impact on catastrophic and out-of-pocket health expenditures in mexico. *The European Journal of Health Economics 11*(5), 437–447.

Galasso, E. and M. Ravallion (2005). Decentralized targeting of an antipoverty program. *Journal of Public economics 89*(4), 705–727.

Glewwe, P. (2002). Schools and skills in developing countries: Education policies and socioeconomic outcomes. *Journal of Economic Literature 40*(2), 436–482.

Goldberg, P. K. and N. Pavcnik (2007). Distributional effects of globalization in developing countries. *Journal of economic Literature 45*(1), 39–82.

Goldberg, P. K. and N. Pavcnik (2016). The effects of trade policy. In *Handbook of commercial policy*, Volume 1, pp. 161–206. Elsevier.

Goldin, C. (1979). Household and market production of families in a late nineteenth century american city. *Explorations in Economic History 16*(2), 111–131.

Goldstein, M., J. Graff Zivin, J. Habyarimana, C. Pop-Eleches, and H. Thirumurthy (2013). The effect of absenteeism and clinic protocol on health outcomes: The case of mother-to-child transmission of hiv in kenya. *American Economic Journal: Applied Economics 5*(2), 58–85.

Grépin, K. A., J. Habyarimana, and W. Jack (2019). Cash on delivery: Results of a randomized experiment to promote maternal health care in kenya. *Journal of health economics 65*, 15–30.

Grootaert, C. and R. Kanbur (1995). Child labour: an economic perspective. *International Labour Review 134*, 187.

Grossman, M. (1972). On the concept of health capital and the demand for health. *Journal of Political economy 80*(2), 223–255.

Gruber, J., N. Hendren, and R. M. Townsend (2014). The great equalizer: Health care access and infant mortality in thailand. *American Economic Journal: Applied Economics 6*(1), 91–107.

GSS (2008). Ghana living standards survey report of the fifth round. Ghana Statistical Service.

Gunnarsson, V., P. F. Orazem, and M. A. Sánchez (2006). Child labor and school achievement in latin america. *The World Bank Economic Review 20*(1), 31–54.

Habyarimana, J., M. Humphreys, D. N. Posner, and J. M. Weinstein (2007). Why does ethnic diversity undermine public goods provision? *American Political Science Review 101*(4), 709–725.

Hanushek, E. A. and L. Woessmann (2008). The role of cognitive skills in economic development. *Journal of Economic Literature 46*(3), 607–68.

He, Q.-Q., T. W. Wong, L. Du, Z.-Q. Jiang, Y. Gao, H. Qiu, W.-J. Liu, J.-G. Wu, A. Wong, and T.-S. I. Yu (2010). Effects of ambient air pollution on lung function growth in chinese schoolchildren. *Respiratory Medicine 104*(10), 1512–1520.

Henderson, J. V., A. Storeygard, and D. N. Weil (2012). Measuring economic growth from outer space. *American economic review 102*(2), 994–1028.

Henn, S. (2018). Complements or substitutes: State presence and the power of traditional leaders. *Job Market Paper, Harvard University*.

ILO (2002). Every child counts: New global estimates on child labour.

ILO-IPEC (2013). Making progress against child labor: Global estimates and trends 2000-2012.

Im, K. S., M. H. Pesaran, and Y. Shin (2003). Testing for unit roots in heterogeneous panels. *Journal of econometrics 115*(1), 53–74.

Jayachandran, S. (2009). Air quality and early-life mortality evidence from indonesia's wildfires. *Journal of Human resources 44*(4), 916–954.

Kana, M., H. Phoumin, and F. Seiichi (2010). Does child labour have a negative impact on child education and health? a case study in rural cambodia. *Oxford Development Studies 38*(3), 357–382.

Khwaja, A. I. (2004). Is increasing community participation always a good thing? *Journal of the european economic Association 2*(2-3), 427–436.

Khwaja, A. I. (2009). Can good projects succeed in bad communities? *Journal of public Economics 93*(7-8), 899–916.

Kim, H. B. and S.-m. Lee (2017). When public health intervention is not successful: Cost sharing, crowd-out, and selection in korea's national cancer screening program. *Journal of health economics 53*, 100–116.

King, G., E. Gakidou, K. Imai, J. Lakin, R. T. Moore, C. Nall, N. Ravishankar, M. Vargas, M. M. Téllez-Rojo, J. E. H. Ávila, et al. (2009). Public policy for the poor? a randomised assessment of the mexican universal health insurance programme. *The lancet 373*(9673), 1447–1454.

Kirchberger, M. (2017). Natural disasters and labor markets. *Journal of Development Economics 125*, 40–58.

Kis-Katos, K. and R. Sparrow (2011). Child labor and trade liberalization in indonesia. *Journal of Human Resources 46*(4), 722–749.

Kis-Katos, K. and R. Sparrow (2015). Poverty, labor markets and trade liberalization in indonesia. *Journal of Development Economics 117*, 94–106.

Kling, J. R., J. B. Liebman, and L. F. Katz (2007). Experimental analysis of neighborhood effects. *Econometrica 75*(1), 83–119.

Kong, T. and A. Ramayandi (2008). Survey of recent developments. *Bulletin of Indonesian Economic Studies 44*(1), 7–32.

Kőszegi, B. and M. Rabin (2006). A model of reference-dependent preferences. *The Quarterly Journal of Economics 121*(4), 1133–1165.

Kremer, M. and E. Miguel (2007). The illusion of sustainability. *The Quarterly journal of economics 122*(3), 1007–1065.

Leamer, E. E. and J. Levinsohn (1995). International trade theory: the evidence. *Handbook of international economics 3*, 1339–1394.

Lebowitz, M. D. (1991). The use of peak expiratory flow rate measurements in respiratory disease. *Pediatric pulmonology 11*(2), 166–174.

Levine, D. I. and D. Yang (2014). The impact of rainfall on rice output in indonesia. Technical report, National Bureau of Economic Research.

Magruder, J. R. (2013). Can minimum wages cause a big push? evidence from indonesia. *Journal of Development Economics 100*(1), 48–62.

Marks, S. V. (2017). Non-tariff trade regulations in indonesia: Nominal and effective rates of protection. *Bulletin of Indonesian Economic Studies 53*(3), 333–357.

Martinez-Bravo, M. (2014). The role of local officials in new democracies: Evidence from indonesia. *American Economic Review 104*(4), 1244–87.

Martinez-Bravo, M. (2017). The local political economy effects of school construction in indonesia. *American Economic Journal: Applied Economics 9*(2), 256–89.

Matsuura, K. and C. J. Willmott (2015). Terrestrial precipitation: 1900-2014 gridded monthly time series. *WWW document] URL http://climate. geog. udel. edu/˜ climate/html_pages/Global2011/Precip_revised_3* 2.

McCulloch, N. (2008). Rice prices and poverty in indonesia. *Bulletin of Indonesian Economic Studies 44*(1), 45–64.

McGuirk, E. and M. Burke (2017). The economic origins of conflict in africa. Working paper, National Bureau of Economic Research.

McKenzie, D. (2017). How effective are active labor market policies in developing countries? a critical review of recent evidence. *The World Bank Research Observer 32*(2), 127–154.

Mears, L. A. (1984). Rice and food self-sufficiency in indonesia. *Bulletin of Indonesian Economic Studies 20*(2), 122–138.

Melitz, M. J. (2003). The impact of trade on intra-industry reallocations and aggregate industry productivity. *econometrica 71*(6), 1695–1725.

Miguel, E. and M. K. Gugerty (2005). Ethnic diversity, social sanctions, and public goods in kenya. *Journal of Public Economics 89*(11-12), 2325–2368.

Miller, G., D. Pinto, and M. Vera-Hernández (2013, October). Risk protection, service use, and health outcomes under colombia's health insurance program for the poor. *American Economic Journal: Applied Economics 5*(4), 61–91.

Miller, S. and L. R. Wherry (2019). The long-term effects of early life medicaid coverage. *Journal of Human Resources 54*(3), 785–824.

MoH (2004). Legislative instrument on national health insurance.

Nang-Beifua, A. (2010). Health sector half-year performance report - upper west region.

NDPC (2009). 2008 citizen's assessment of the national health insurance scheme.

Nelson, P. (1970). Information and consumer behavior. *Journal of political economy 78*(2), 311–329.

Newhouse, J. P. et al. (1993). *Free for all?: lessons from the RAND health insurance experiment*. Harvard University Press.

NHIA (2010). National health insurance scheme: Annual report 2009.

NHIA (2011). National health insurance scheme: Annual report 2010.

Niessen, N. (1999). *Municipal government in Indonesia: Policy, law, and practice of decentralization and urban spatial planning*. Number 77. Research School CNWS, School of Asian, African, and Amerindian Studies ....

Nunn, N. and N. Qian (2011). The potato's contribution to population and urbanization: Evidence from a historical experiment. *The Quarterly Journal of Economics 126*(2), 593–650.

O'Donnell, O., F. C. Rosati, and E. Van Doorslaer (2005). Health effects of child work: Evidence from rural vietnam. *Journal of Population Economics 18*(3), 437–467.

Olken, B. A. (2007). Monitoring corruption: evidence from a field experiment in indonesia. *Journal of political Economy 115*(2), 200–249.

Olken, B. A. (2010). Direct democracy and local public goods: Evidence from a field experiment in indonesia. *American political science review 104*(2), 243–267.

Patunru, A. A. (2018). Is greater openness to trade good? what are the effects on poverty and inequality? *Trade, Poverty and Income Distribution: The Indonesian Experience*.

Pavcnik, N. (2017). The impact of trade on inequality in developing countries. Technical report, National Bureau of Economic Research.

Ray, R. (2000). Analysis of child labour in peru and pakistan: A comparative study. *Journal of Population Economics 13*(1), 3–19.

Rojas-Martinez, R., R. Perez-Padilla, G. Olaiz-Fernandez, L. Mendoza-Alvarado, H. Moreno-Macias, T. Fortoul, W. McDonnell, D. Loomis, and I. Romieu (2007). Lung function growth in children with long-term exposure to air pollutants in mexico city. *American Journal of Respiratory and Critical Care Medicine 176*(4), 377–384.

Schwartz, J. (1989). Lung function and chronic exposure to air pollution: a cross-sectional analysis of nhanes ii. *Environmental research 50*(2), 309–321.

Shah, A., Z. Qureshi, A. Bagchi, B. Binder, and H.-f. Zou (1994). Intergovernmental fiscal relations in indonesia. *World Bank Discussion Paper 239*.

Shampanier, K., N. Mazar, and D. Ariely (2007). Zero as a special price: The true value of free products. *Marketing Science 26*(6), 742–757.

Simatupang, P. and C. Peter Timmer (2008). Indonesian rice production: Policies and realities. *Bulletin of Indonesian Economic Studies 44*(1), 65–80.

Simonson, I. and A. Tversky (1992). Choice in context: Tradeoff contrast and extremeness aversion. *Journal of Marketing Research 29*(3), 281–295.

Sparrow, R. (2008, 03). Targeting the poor in times of crisis: the Indonesian health card. *Health Policy and Planning 23*(3), 188–199.

Stasavage, D. (2010). When distance mattered: geographic scale and the development of european representative assemblies. *American Political Science Review 104*(4), 625–643.

Stillman, S. and D. Thomas (2008). Nutritional status during an economic crisis: Evidence from russia. *The Economic Journal 118*(531), 1385–1417.

Strauss, J., K. Beegle, A. Dwiyanto, Y. Herawati, D. Pattinasarany, E. Satriawan, B. Sikoki, F. Witoelar, et al. (2004). *Indonesian living standards: before and after the financial crisis*, Volume 137. Institute of Southeast Asian Studies.

Strauss, J. and D. Thomas (1998). Health, nutrition, and economic development. *Journal of economic literature 36*(2), 766–817.

Strauss, J., F. Witoelar, B. Sikoki, and A. M. Wattie (2009). The fourth wave of the indonesia family life survey: Overview and field report.

Suryadarma, D. (2010). *Labor Market Returns, Marriage Opportunities, Or the Education System?: Explaining Gender Differences in Numeracy in Indonesia*. Centre for Economic Policy Research.

Suryahadi, A., A. Priyambada, and S. Sumarto (2005). Poverty, school and work: Children during the economic crisis in indonesia. *Development and Change 36*(2), 351–373.

Suryahadi, A., D. Suryadarma, and S. Sumarto (2009). The effects of location and sectoral components of economic growth on poverty: Evidence from indonesia. *Journal of Development Economics 89*(1), 109–117.

Suryahadi, A., W. Widyanti, D. Perwira, and S. Sumarto (2003). Minimum wage policy and its impact on employment in the urban formal sector. *Bulletin of Indonesian economic studies 39*(1), 29–50.

Sviatschi, M. M. (2018). Making a narco: childhood exposure to illegal labor markets and criminal life paths. *Unpublished manuscript, Princeton University*.

Tajima, Y., K. Samphantharak, and K. Ostwald (2018). Ethnic segregation and public goods: Evidence from indonesia. *American Political Science Review 112*(3), 637–653.

Tavits, M. (2006). Making democracy work more? exploring the linkage between social capital and government performance. *Political Research Quarterly 59*(2), 211–225.

Thornton, R. L. (2008). The demand for, and impact of, learning hiv status. *American Economic Review 98*(5), 1829–63.

Thornton, R. L., L. E. Hatt, E. M. Field, M. Islam, F. Solís Diaz, and M. A. González (2010). Social security health insurance for the informal sector in nicaragua: a randomized evaluation. *Health Economics 19*(S1), 181–206.

Timmer, C. (2005). Food security and economic growth: an asian perspective. *Asian-Pacific Economic Literature 19*(2), 1–17.

Topalova, P. (2010). Factor immobility and regional impacts of trade liberalization: Evidence on poverty from india. *American Economic Journal: Applied Economics 2*(4), 1–41.

Wagstaff, A. (2010). Social health insurance reexamined. *Health economics 19*(5), 503–517.

Wagstaff, A., H. T. H. Nguyen, H. Dao, and S. Bales (2016). Encouraging health insurance for the informal sector: a cluster randomized experiment in vietnam. *Health economics 25*(6), 663–674.

Warr, P. (2005). Food policy and poverty in indonesia: a general equilibrium analysis. *Australian Journal of Agricultural and Resource Economics 49*(4), 118588.

Warr, P. (2011). Food security vs. food self-sufficiency: The indonesian case. Departmental working papers, The Australian National University, Arndt-Corden Department of Economics.

Warr, P. and A. A. Yusuf (2014). World food prices and poverty in indonesia. *Australian Journal of Agricultural and Resource Economics 58*(1), 1–21.

Westfall, P. H. and S. S. Young (1993). *Resampling-based multiple testing: Examples and methods for p-value adjustment*, Volume 279. John Wiley & Sons.

Wetterberg, A., J. R. Jellema, and L. Dharmawan (2014). The local level institutions study 3: Overview report. Technical report.

Whitt, S. and R. K. Wilson (2007). Public goods in the field: Katrina evacuees in houston. *Southern Economic Journal*, 377–387.

WHO (2005). Sustainable health financing, universal coverage and social health insurance: World health assembly resolution 58.33.

WHO (2010). The world health report: health systems financing: the path to universal coverage. Technical report, World Health Organization.

Williams, M. J. (2017). The political economy of unfinished development projects: Corruption, clientelism, or collective choice? *American Political Science Review 111*(4), 705–723.

Wolff, F. and Maliki (2008). Evidence on the impact of child labor on child health in indonesia, 1993–2000. *Economics & Human Biology 6*(1), 143–169.

Woolcock, M. (1998). Social capital and economic development: Toward a theoretical synthesis and policy framework. *Theory and society 27*(2), 151–208.

Woolcock, M. and D. Narayan (2000). Social capital: Implications for development theory, research, and policy. *The world bank research observer 15*(2), 225–249.

World Bank (2003). Decentralizing indonesia : A regional public expenditure review overview report. Technical report.

World Bank (2010). Village capacity in maintaining infrastructure: evidence from rural indonesia. Technical report.