The Effects of Universal Health Insurance on Household Financial Well-being and Investments: Evidence from Rural Thailand*

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Abstract

This paper investigates how public insurance provision affects financial well-being and households' investments and risk-taking decisions. We exploit a major health reform introduced in Thailand in 2001, which triggered exogenous changes in household health insurance coverage, combined with panel data from Townsend Thai Monthly surveys of rural households. We show that households with higher health risk experienced a larger reduction in expected costs of illness and thus benefited more from the reform. Using this variation as a source of identification, we find that households that benefited more gained differential improvement in financial well-being and welfare shown by the differential decline in exposure to health expenditure risk and loan default rate, long-run differential increases in consumption, and short-run differential reduction in the incidence of child labour. These households also shifted their cultivation portfolio towards riskier cash crops at the intensive margin relatively more in the long run.

Keywords: Health insurance, Universal coverage, Thailand, Coping strategies, Consumption, Risk-taking

JEL classifications: D1, G5, H51, I12 and I13

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1 Introduction

Providing financial security is one of the key motivations for policymakers advocating for universal health coverage (World Health Organization, 2010). Spending on medical care can be large and uncertain resulting in uninsured households being exposed to potentially high health care costs and financial distress during sickness. A large literature has studied the effects of health insurance (HI) on health outcomes and utilization of health care, yet relatively less is known about the relationships between HI, financial well-being and investments. Understanding these relationships is important, particularly in a rural and developing country context, where households constantly make investment decisions as they engage in agricultural production activities.

The goal of this paper is to estimate the effect of public HI provision on household financial well-being and investment decisions. The context that we study is rural Thailand, where most households engage in cultivation activities and were previously uninsured. In the absence of HI, adverse health shocks pose a financial burden on households not only through rising medical expenses, but also via decreased household income caused by reduced labour productivity and foregone market opportunities. Because health infrastructure and insurance markets are generally underdeveloped, these shocks can *ex post* force households into costly risk-coping strategies to help maintain their consumption, including, for example, borrowing and using child labour.[1] Such negative consequences can be mitigated by a public HI scheme.[2] *Ex ante*, these shocks constitute a background risk—a risk that is hard to avoid—translating into an earnings risk and a medical expenditure risk. The introduction of HI mitigates the latter risk and possibly also the former if a timely treatment reduces the period of illness. The question is then how the introduction of HI, by containing the background medical expenditure risk, affects household decision making on how many resources to transfer from one period to the other, and through which channels.

To estimate the effects of HI, one major challenge is the well-known identification problem where HI coverage is endogenous and there are unobserved differences between households with and without insurance. For example, households with certain unobserved risk preferences could demand more insurance and self-select into a coverage scheme, but these unobserved traits often also affect their

[1] See Townsend (1995) and Dercon (2002) for a review of the use of risk-coping strategies by households in developing countries.

[2] See, for example, Chetty and Looney (2006) for a theoretical model of how the availability of insurance can be thought of as one channel to insure against health or income shocks in place of costly risk-coping mechanisms.

financial decisions. In addition, datasets that contain detailed information on financial outcomes along with investment and health variables are difficult to come by.

We address these challenges by exploiting a long household panel dataset for rural Thailand and a universal HI reform. To overcome the identification challenge, we rely on Thailand's major health reform introduced in 2001, the '30 Baht' reform. Before the reform, most households in rural Thailand were uninsured, and the rest were covered by pre-existing programs with less generous coverage. The reform replaced out-of-pocket (OOP) expenditures with a fixed co-payment as little as 30 Baht ($\sim$ \$0.73) per visit and increased the capitation budget for hospitals fourfold.[3] Although the reform offers universal HI coverage to the whole population, it causes heterogeneous reductions in expected medical expenditure and productivity loss across households differing in their pre-existing health risk. We classify households into different health risk types by utilizing a battery of health-related symptoms that spans a few years before the reform. Relative to low-risk households, high-risk households should benefit more from the reform in terms of a larger reduction in the expected cost of sickness defined by the sum of OOP health expenditure and earnings loss due to illness.[4] It is this heterogeneous treatment intensity of the reform that we exploit as the source of identification. We show that the reduction of the expected cost of illness (because of the reform) is indeed larger among households with higher health risk. Our main identifying assumption is that changes in the outcome of interest among households with a different degree of health risk would have been the same in the absence of the reform. We provide empirical evidence supporting this assumption.

The data used in this paper come from the Townsend Thai Project. We use the monthly survey of rural households, which contains rich information on financial well-being (such as borrowing, loan repayment and household expenditure) and various types of investments including allocation among different crops in household cultivation portfolio. The data allow us to examine both the short-term and long-term impacts of HI. As our data cover multiple pre-reform periods, we can test the validity of our identifying assumption using an event study analysis. The short recall period (monthly frequency) also provides relatively accurate measures of investments, expenditures and financial variables compared

[3]We use the exchange rate in 2001: 1 US dollar = 41.36 Baht.

[4]Estimating the impact of the reform by only considering its direct effect on OOP spending may not provide a complete picture because the reform also benefited households indirectly in terms of the reduction in earnings loss in that it allows households to get better access to treatments and to recover more quickly from illnesses. Unlike the OOP expenditure that may reflect choices of households about whether to receive or forego treatments when they become ill especially prior to the reform, earnings loss is unaffected by such choices and more directly captures health symptoms that affect household productivity. See Appendix C for details of how the expected cost of sickness variable is constructed.

with less frequently surveyed or annual data used by many previous studies.

Our key findings reveal that the reform led to a relatively larger reduction in the cost of sickness and exposure to risk associated with medical spending for households that benefited more from the reform, with stronger effects in the long run. This reaffirms the role of universal health coverage policy in protecting households against potential financial risk associated with medical expenditure especially for the poor. These households in turn gained differential improvement in financial well-being and welfare as indicated by the differential declines in their loan default rate and loans–assets ratio throughout the post-reform period, as well as in reliance on selling assets or land in the short run. Their durable and non-durable consumption also increased relatively more in the long run.

The second set of our results consider the insurance effects on investment in human capital and cultivation. Following the reform, we find that households that benefited more had differential increases in school enrollment rate and education expenses together with a differential decrease in the incidence of child labour in the short run. Despite the overall scale of cultivation investments and harvests remaining virtually unaltered by the provision of insurance, our results suggest that households that benefited more actually shifted their cultivation portfolio towards riskier cash crops especially at the intensive margin in the long run.[5] Furthermore, these households also worked relatively fewer hours post-reform to generate a relatively similar level of harvests. We show that this household labour supply adjustment is likely through the margin of crop choice rather than capital-labour substitution or a shift to other types of production than cultivation.

Both the increased consumption and the reallocation of the cultivation portfolio towards riskier cash crops can be interpreted as best responses to reduced background risk. As we argue in Section 6, more comprehensive HI lowers the prudence premium.[6] This reduction in the prudence premium lowers the marginal utility of future consumption and thus stimulates current consumption. Under decreasing absolute prudence (DAP), better HI also increases the willingness to take risks in the future and thus stimulates the risky investment channel in total savings. These effects are borne out by the empirical results.

This paper contributes to a large empirical literature estimating the impact of public HI coverage,

[5] Cash crops are defined based on crop location-specific volatility in the value of output per unit of land over the estimation period. See Section 5.3 for more details.

[6] The prudence premium is the amount that can be subtracted from expected consumption to bring the marginal utility in line with the expectation of marginal utilities across health states.

combining microdata with credible research designs.[7] Specifically related to this paper is a small but growing literature that studies the relationship between HI and financial outcomes. Using the Oregon Health Experiment, Finkelstein et al. (2012) find that those living in poverty who gained coverage through lottery reported lower and owed fewer medical bills as well as less self-rated financial strain than those without insurance. Using state-level variation in the timing of Medicaid expansion, Gross and Notowidigdo (2011) show that a 10% increase in Medicaid eligibility led to about 8% reduction in personal bankruptcy. Mazumder and Miller (2016) exploit variation in pre-reform insurance coverage of a major health reform in Massachusetts and use credit report administrative data to find that the reform improved credit scores and decreased third-party collections and personal bankruptcies.[8] These papers focus on HI schemes that affect specific groups in the population in the United States.[9] Due to data limitations, they only analyse broad measures of financial well-being, such as bankruptcy and self-rated financial status, and can only estimate the effects in the short run. In our study, we draw on more detailed and objective measures of household financial outcomes and decisions as well as a universal health reform that affects all households. The data we use also allow us to study the short-term dynamics and the long-run impact of HI.

Also related to this paper are those examining the effects of insurance on agricultural investments and production choices. Karlan et al. (2014) conducted experiments that randomly assigned farmers in Ghana to receive cash grants or opportunities to purchase rainfall index insurance. They find that insurance led to riskier crop production choices and significantly larger agricultural investments. Similarly, using a randomized controlled trial (RCT), Cole, Giné, and Vickery (2017) show that innovative rainfall insurance could mitigate uninsured production risk and induced farmers to increase investments in higher-return but rainfall-sensitive cash crops. Focusing on formal micro-insurance, Cai et al. (2015) find that promoting the adoption of insurance increased sow production with persistent long-run ef-

[7]See Currie and Madrian (1999), Gruber (2000), and Gruber and Madrian (2002) for a review. Examples of papers that use a particular policy reform in a developing country to analyse health-related outcomes include Wagstaff et al. (2009) and Lei and Lin (2009) (China), Wagstaff (2010) (Vietnam), Bauhoff, Hotchkiss, and Smith (2011) (Georgia) and Miller, Pinto, and Vera-Hernández (2013)(Colombia).

[8]A related strand of literature estimates the effects of HI on consumption and savings. Using exogenous variation in Medicaid eligibility during 1984—93 Medicaid expansion, Gruber and Yelowitz (1999) show that HI has negative impact on wealth holdings implied by a strong positive association between Medicaid eligibility and consumption expenditures. In the developing country context, Chou, Liu, and Hammitt (2003) explore the effect of the introduction of National HI reform in Taiwan in 1995 on savings and consumption.

[9]Evidence from developing countries is very limited. In one study, Miller, Pinto, and Vera-Hernández (2013) examines the impact of Colombia's HI program targeting the poor on various outcomes, of which some are related to financial well-being. They find that HI enrollment affects composition of household assets (car and radio), education investments and consumption.

fect.[10] These papers usually involve RCT and insurances that directly target one particular type of agricultural production. We complement this strand of literature by showing that HI can indirectly alter households' production decisions and cultivation portfolio allocations. Our results suggest that, despite not directly affecting cultivation risks like the rainfall index, HI provision can similarly lead households to undertake more risky production options.[11]

Empirical evidence on how HI affects households' risk-coping strategies in developing countries is relatively scarce. Liu (2016) and Garcia-Mandicó, Reichert, and Strupat (2021) examine how HI provision changes household's risk coping strategy following health shocks, focusing on child labor and education.[12] Consistent with their results, we show that HI increased schooling and helped reduce the costly utilization of youth employment especially in the short run. We also examine other risk-coping strategies of households including private transfers and selling assets.

Two other papers have used the same 30 Baht reform. Limwattananon et al. (2015) find that the reform led to a reduction in OOP spending as well as increased health care utilization. Gruber, Hendren, and Townsend (2014) show that the reform led to increased utilization and reduced infant mortality rates, especially among the poor. We examine a different set of outcomes beyond OOP spending and health, studying the impact on informal production activities, loans, investments and risk-taking decisions. Both papers use different datasets, which are collected biannually and are only cross-sectional spanning one year before the reform and at most three years after the reform. We instead use long panel data collected at monthly frequency, which have a short recall period and allow us to study both the short- and long-term effects of the reform as well as testing for possible pre-trend in outcomes (a crucial identifying assumption). Our paper also differs in the identification strategy in that both papers compare government and formal workers against uninsured informal counterparts in

[10] Few papers have studied how investment decisions are affected by health risk and insurance in the developed world context focusing on asset investment portfolio choice. Atella, Brunetti, and Maestas (2012) study the influence of current health and future health risk on the decision to hold risky assets by European retired household members. They use the variation in coverage degrees across European countries as the exogenous variation in the background risk that their citizens are left with. In countries with less protective healthcare systems, health risks affect portfolio choices to a larger extent. Goldman and Maestas (2013) consider the effect of different supplementary coverages to Medicare on the riskiness of the financial portfolio of retirees in the United States. They discover that protective supplementary HI significantly increases the share of risky assets in the portfolio of retirees.

[11] In an RCT setting, households may not respond to the incentive provided due to the response lag and the lack of trust in a non-government insurance provider. In our case, the 30 Baht reform implemented by the government entails credibility. Its longevity and our data also provide sufficient time span for studying investment responses to insurance.

[12] Garcia-Mandicó, Reichert, and Strupat (2021) also consider risk-coping strategies other than the use of child labour including borrowings and remittances, whereas Liu (2016) also investigate household labor supply. Related to these papers are Chen and Jin (2012) and Landmann and Frölich (2015) that study the direct effect of HI on child labor.

a difference-in-difference research design. Instead, we first show that households with higher predicted health risk benefited more from the reform and, analogous to Bleakley (2007), exploit this variation as a source of identification.

The paper proceeds as follows. Section 2 discusses the institutional background of the 30 Baht health reform as well as data and sample construction. Empirical strategies and econometric models are then outlined in Section 3. The estimation results on financial well-being and welfare are reported in Section 4, followed by the results on investments in Section 5 and a discussion of how these results should be interpreted using a theoretical model in Section 6. Section 7 then concludes.

2 Institutional Background and Data

In this section, we first provide an overview of the 30 Baht reform, including how it leads to the design of our empirical strategy, and then describe the data and sample construction.

2.1 Thailand's 30 Baht Reform

The 30 Baht reform is one of the most ambitious and largest universal health care reforms by a developing country (Gruber, Hendren, and Townsend, 2014).[13] The reform was primarily aimed at alleviating the prolonged geographical inequality in the public health care provision and was a key component in the populist election platform of Prime Minister Thaksin Shinawatra, who came into power in February 2001. Through sharp rises in public health spending combined with effective supply-side measures, the scheme offers universal access to a comprehensive care package covering outpatient and inpatient services, accident and emergency, most high-cost treatments and a wide range of preventive cares in public health facilities.[14] As there were four existing public HI schemes prior to the onset of the reform, it is crucial to understand how the reform impacted households with different types of coverage. Table 1 provides an overview of breaking down these households into different pre-reform insurance types.

[13]With gross national income of only $1,990 per capita and tax revenue amounting to just 13% of GDP at the time, Thailand became one of the first middle-income countries to achieve universal health coverage.

[14]Note that the Thai insurance scheme does not include sickness pay as is covered in many European countries. For a complete chronology of coverage extension, see Tangcharoensathien et al. (2018). For more details on the supply-side measures, see Limwattananon et al. (2015).

Previously Uninsured. Sixty-five per cent of households in our data were without any coverage and had to pay for health services out of pocket prior to the reform. Members of these households are essentially those working in agriculture or unregistered small entities, or are self-employed workers in the informal sector. The 30 Baht reform provided them with access to public health services for a fixed cost (co-payment) as little as 30 Baht (∼\$0.73) per visit.[15] This 30 Baht co-payment was subsequently abolished in October 2006 making health care completely free for all. Hence, the reform significantly reduced health care price faced by the previously uninsured unambiguously.

Voluntary Health Card Scheme (VHCS). Households with no coverage had an option to pay an insurance premium of 500 Baht (∼\$12.08) per year per household for enrollment in the pre-existing VHCS, which offered free access to public health care to up to five enrolled household members. The government then contributed 1,000 Baht to supplement each private contribution. However, the combined contributions were often insufficient for providers to offer adequate services, and thus cross-subsidization was required (Donaldson, Pannarunothai, and Tangcharoensathien, 1999; Gruber, Hendren, and Townsend, 2014). After the reform, households in VHCS were merged into the 30 Baht program, which for most cases, lowered their public health care costs from 500 Baht per household per year to 30 Baht per visit. With increased capitation budget for hospitals, coverage and service quality both improved. Only about 5% of households reported paying for this health card at least once during the pre-reform months in our data.

Medical Welfare Scheme (MWS). Primarily based on income criteria, relatively poor households were eligible for free health care provided by the MWS before the reform.[16] In our data, MWS households account for around 21%.[17] The MWS was largely underfunded with an average annual budget per enrollee of only 250 Baht (∼\$6.04) for public hospitals (Damrongplasit and Melnick, 2009; Gruber, Hendren, and Townsend, 2014). Following the reform, MWS enrollees were rolled over to the 30 Baht program but were exempted from contributing the co-payment. The capitation budget for hospitals

[15] People in the scheme receive a gold card that permits them to receive treatments in their health district or to be referred for specialist care elsewhere if required.

[16] MWS covered the poor, children aged below 12 years, elderly over 60 years of age, monks, war veterans and the disabled. The means-tested MWS eligibility criteria on income are that individual lives with monthly income below 2,000 Baht (∼\$1.61/day) per person or a household has monthly income below 2,800 Baht (∼\$2.01/day) per household.

[17] This is proxied by information on service payments at public facilities and pre-reform household income in our data. See footnote to Table 1 for details of how we classify households into different insurance types using our data.

increased fivefold to over 1,200 Baht (∼$29) ensuring adequate health care for all. Although the reform did not directly benefit these households in terms of the reduction in care price, it significantly improved financial outlay and supply-side measures for hospitals (see below for more details).

Employment-based Schemes. The other two public insurance programs were employment-based. Since 1980, the Civil Servant Medical Benefit Scheme (CSMBS) has covered active and retired civil servants and their dependents with a relatively more generous care package and superior annual capitation of almost 2,500 Baht (∼$60).[18] Formal sector workers (but not their dependents) have been protected by the Social Security Scheme (SSS) introduced in 1990.[19] Because households in our data are rural households mainly engaged in informal economic activities, households with at least one CSMBS recipient or with all members being entitled to SSS benefits account for just 9%.[20] The SSS and CSMBS households were left largely unaffected by this reform.

Providers and Financing. Funding for the 30 Baht program comes mainly from government tax revenues. Under the 30 Baht program, health care providers received increased capitation budget of over 1,200 Baht per head replacing either the 250 Baht capitation from former MWS recipients, the 1,500 Baht per household per year from the former VHCS households or the OOP payments from the previously uninsured.[21] In 2003, the 30 Baht capitation budget for providers marked a dramatic 35% rise in real terms above the corresponding 2001 figures of the MWS and VHCS schemes. Through annual increments, this then increased to 2,895 Baht (∼$82) in 2015 (National Health Security Office, 2015). Various supply-side measures including closed-end capitation, gatekeeper for specialist treatments access, in-advance payments of inpatient care for hospitals and a single purchaser in the 30 Baht system were put in place in order to balance out this significant rise in health budget as well as allowing for more efficient run of the programs.[22]

[18] Dependants are parents, spouse and legal children of the recipients aged less than 25.

[19] Employees contribute a small part of their monthly salary, which would then be matched by their employer and the government to the Social Security Fund.

[20] See Section 2.2 or footnote to Table 1 for more details of how CSMBS and SSS households are defined.

[21] In practice, most contracted providers were public hospitals under the Ministry of Public Health (Wagstaff and Manachotphong, 2012). Participation of these public health care units in the 30 Baht program was mandatory, while that of the private hospitals was optional. However, the fraction of private units participating was minimal (Panpiemras et al., 2011).

[22] See Limwattananon et al. (2015) for more details of the supply-side measures. Appendix A provides more detailed information on the reform and how it was financed.

Exogeneity of the Reform. The 30 Baht reform was arguably unanticipated and exogenous. The reform occurred in a rapid fashion and was completed nationwide within a year after implementation. Starting with six pilot provinces in April 2001, the 30 Baht program was then rolled out to cover an additional 15 provinces in June, all remaining provinces in October and completed with all remaining Bangkok districts by April 2002 (Wagstaff and Manachotphong, 2012). Because the reform was launched by the newly elected government within just a few months after the Parliament was dissolved in November 2000, households would not have foreseen the reform materializing earlier. However, because we employ high-frequency monthly data to estimate the impact of the reform, there could be anticipatory effects at play during the months in 2001 when households had already learned the election result. We address this issue and discuss it in more detail in Section 3.3.1. Another advantage of using the 30 Baht reform is that it is a large and sudden reform that extended coverage to most households in our data, and there were no other major health reforms in Thailand during the period that we study.[23]

2.2 Data from the Townsend Thai Project

We employ data from the Townsend Thai Project's monthly surveys of rural households conducted in four Thai provinces distinct in economic conditions.[24] The panel dataset is a clustered, stratified, random sample of around 45 households from each of four villages distributed across each of the four provinces, totalling approximately 720 households.[25] The dataset contains 24 different modules including rich information on household composition, consumption, borrowings in various forms, income, agricultural activities and investments, as well as individual records of education, labour supply, and health-related variables.[26] The surveys began in August 1998 with a baseline interview for initial conditions of each household. The team has then continued to visit these households and interviewed them

[23]Note the Thai government also launched a large micro-credit program in 2001 injecting a uniform transfer of a million Baht (∼$24,000) to all villages in our data by the beginning of 2002. We discuss how we account for the potential effects of this policy in Section 3.3.2.

[24]Lopburi and Chachoengsao are semi-urban provinces located in a more developed Central region close to Bangkok, the capital. By contrast, Srisaket and Buriram are mostly rural and located in a less developed Northeastern region of Thailand bordering Cambodia.

[25]A province is made up of several districts, each of which is a collection of villages with at least one urbanized community at the centre.

[26]Using health as an example to illustrate depth of the data, we have records on all symptoms suffered by each household member in each month along with their duration and severity. Information on each inpatient and outpatient visit including reasons for the visit, types and location of the facility, the number of days hospitalized, treatment and medicines costs and financing methods is available for each individual. Also recorded by reasons for spending at household level are expenditures on over-the-counter medicines, traditional treatments and private insurance fees. For more details on the Townsend Thai data, see Samphantharak and Townsend (2010) and Townsend (2016), or the Townsend Thai Project website: http://townsend-thai.mit.edu/.

to record changes in subsequent months in each module. A separate 'Form' dataset records further details about such changes. For example, when households start growing a new crop, a New Crop Form is issued to record specific details of that crop in the Cultivation module. A Crop Operation Form then stores information of all harvests of each crop plot, different types of plot operations, use of inputs and the associated transaction-level itemized costs and revenues.

The dataset is one of the longest panel surveys from developing countries with data being collected on a monthly basis (weekly for consumption expenditures). Our estimation windows range from September 1998 to December 2000 (28 months) for the pre-reform period, and from January 2002 to December 2010 (108 months) for the post-reform period.[27] This allows us to analyse the short-term changes and the effects of the reform for up to 9 years. The short recall period in our data also generates more accurate measures of variables compared with several previous studies that are based on infrequent surveys and long recall periods. The use of time-averaged measures could also conceal short-term dynamics that can be significant in evaluating the effect of insurance. Moreover, the Townsend Thai project is funded independently of the 30 Baht program. This therefore helps restrict respondents' incentives to misreport regarding the scheme.

Sample. We first drop around 100 households that missed more than 3 months of surveys in either the pre-reform or post-reform periods. As our research design exploits exogenous variations among only the previously uninsured, VHCS and MWS households, we exclude households in which at least one member was a CSMBS recipient during the pre-reform period (CSMBS households). We treat an individual as a CSMBS recipient if, in at least half of the pre-reform months, he/she was a government worker or the village head (also eligible to receive CSMBS benefits), or reported receiving employment-related health benefits that also covered his/her family members. We further exclude very few households in which all members were pre-reform formal private employees (SSS households).[28] Together, these remove 68

[27] In our main analysis, we do not use data for 2001 because of possible anticipatory effects of the reform. see Section 3 for further discussions. Although the data are available up to December 2014, we limit our post-reform period to December 2010 so as to avoid possible confounding from the government's crop insurance provisions implemented from 2011. Note that there was a World Bank pilot project on weather index insurance for corn (but only less than 1% of cultivated acre was insured) and rice (but only in one province, not in our sample) during 2007–2008. Then, in 2011, the government launched a major micro-insurance scheme for rice along due to a major flood in 2011.

[28] We proxy for individual SSS status using the information that, in at least half of the pre-reform months, the person's employer paid for or provided health services when he or she became ill or injured either from work or away from work, and that the person was either a daily or monthly wage employee, or a piece-rate employee who worked for a business organization.

or around 9% of all households from the sample.[29] Our final sample therefore constitutes a panel of 551 rural households spanning 136 months.[30]

3 Empirical Strategies

Our empirical strategy mainly exploits the fact that the 30 Baht reform caused different impacts on the previously uninsured households that differ in terms of their predicted health risk. We conduct our empirical exercises in two steps. First, we predict the household health risk or 'treatment intensity' using a history of pre-reform health conditions. Given that the previously uninsured households faced a similarly reduced price of public care following the reform, those with high health risk would experience a larger reduction in the expected cost of illness and thus benefit more from the reform relative to households with low health risk.[31] Such heterogeneity among the previously uninsured households facilitates a treatment-control strategy in that households that benefited more become those treated more intensively. Hence, in the second step, we exploit this exogenous variation in treatment intensity to estimate the reform impact using various econometric specifications. This treatment intensity approach is similar in spirit to that of Bleakley (2007, 2010), Bütikofer and Salvanes (2020) and Adhvaryu et al. (2020).

We do not compare the excluded CSMBS and SSS households that were virtually unaffected by the reform to those previously uninsured as in Gruber, Hendren, and Townsend (2014) and Limwattananon et al. (2015). Because our data mainly include rural households extensively engaged in informal activities, only less than 10% of households would form the control group using their classification. This would then pose a small-sized control group issue. Moreover, CSMBS and SSS households are quite distinct from the previously uninsured both characteristically and economically.[32]

[29]Under the most relaxed definition where we replace 'in at least half of the pre-reform months' with 'in any of the pre-reform months', only 13% of households are excluded.

[30]The Townsend Thai data also collect annual rural household data of a larger sample of around 960 households, a third of which are the same households as in our sample. Unfortunately, due to the lack of health data important for our estimation in the annual sample, we cannot exploit this larger annual sample.

[31]Household expected cost of sickness is defined as the sum of household monthly OOP health expenditure and the opportunity cost of sickness, where the opportunity cost is measured by earnings loss of all sick individuals in the household. See Section C for details of how the variable is constructed.

[32]Note that, similar to Limwattananon et al. (2015), we do not distinguish between the former MWS, VHCS and previously uninsured households. Similar to households with no insurance, the former VHCS recipients mainly faced a reduction in the price of health care after the reform. For the MWS households, despite not directly benefiting from the reform in terms of price reduction, their capitation health budget at hospitals shot up fivefold allowing them much greater access to better health care.

3.1 Predicting the Health Risk of Households

We use the history of individual health conditions during the pre-reform months to proxy for households' health risk or treatment intensity. Given various measures of health, we follow Blundell et al. (2020) in using principal components analysis (PCA) to estimate an index of household health status from linear combinations of the observed health measures richly available in our data.[33] Our health variables correspond to the objective health measures in the literature which mainly are reported symptoms and diagnosed health conditions.

From the visit- and symptom-level individual health data, we generate and include six health measures specific to each household in the PCA regression. These include whether a household member suffered from any symptom, whether a household member had a symptom that prevented daily-life routines and the duration that household suffered from the symptom(s). For each of these measures, we take the average value over the 28 pre-reform months within each household. We use only the pre-reform health data because it is possible that the reform may have affected the health conditions of households either through changes in behaviours or better treatments, which then would have made the health conditions of household endogenous.[34] We also include whether a household member had any chronic condition and whether a member had any disability condition, both of which we have information at the baseline (month 0). Appendix B provides details of all the health variables used in the PCA regression.

We combine multiple health measure variables into a parsimonious single index of health using the first component of PCA. The predicted index scores are standardized to have mean zero and standard deviation of one.[35] While preserving as much variability as possible, PCA effectively reduces the dimensionality of our health measures to one. We can interpret the index as a proxy for health risk and the potential demand for health care. As a higher value of our health index indicates worse pre-reform health conditions, we also refer to it interchangeably as the 'morbidity' index. A higher morbidity index also implies higher health risk and households' potential health care demand.

We do not include measures such as medical expenditure or those captured purely by health care

[33]Blundell et al. (2020) estimate the effects of health on employment using PCA and factor analysis. They find that both methods give similar results.

[34]Note that this makes our resulting morbidity index becomes time-invariant and specific to each household. However, one important advantage is that the households' predicted health risk is exogenous to the reform.

[35]The first principal component explains around 43% of the total variation in the health measure variables. The second and third components explain further a 24% and 14% respectively.

utilization. This is because these measures could reflect the choices of households that depend on their budget constraint and preferences, and so do not fully capture the health risk of the households. Our baseline survey also contains information on whether individuals can perform various activities of daily living (ADL) and self-rated general health condition. However, this information is available in month 0 only and likely to change over time. As discussed in Blundell et al. (2020), there is also ambiguity as to whether ADL should be included as objective measures. The common practice is to exclude them. Moreover, the self-rated health status can suffer from bias, reporting error and 'cultural conditioning' (Schultz and Tansel, 1997) in that the threshold of 'good' health can vary non-randomly across societies and this threshold tends to be higher for richer households. For these reasons, we exclude both ADL and self-rated health status.

Table 2 presents the summary statistics for household characteristics, health conditions and expenditures variables in the pre-reform period by households' risk types, where high-risk (low-risk) households are those whose morbidity index ranks above (below) the median. High-risk households tend to have more children and elderly members. Unsurprisingly, they also experienced on average more than four times larger OOP health expenditure and total cost of sickness due to the fact that they had more symptoms, chronic and disability conditions, which in turn contribute to a higher morbidity index predicted by PCA, as well as service utilization.[36] The summary of pre-reform statistics of all outcome variables for each household type is presented in Appendix Tables A1 and A2.[37] We discuss each of the outcomes in turn in Sections 4 and 5.

3.2 Visualizing Variations in Treatment Intensity among Households

Given across-household heterogeneity in treatment intensity proxied by the morbidity index, we first test our fundamental hypothesis: *do households with higher health risk experience a larger fall in the cost of sickness and benefit relatively more from the reform than households with lower health risk?*

We divide the sample into ten roughly equal-sized bins ranked by the morbidity index obtained from PCA, where the lowest decile (bin 1) consists of the most healthy households. For each household, we calculate the average total cost of sickness over the pre-reform and post-reform months separately.

[36] Variables used for PCA regression are shown in the top six rows of panel (B).

[37] All monetary variables are converted to December 2004 prices using the monthly and region-specific consumer price index of Thailand's Ministry of Commerce.

Within each bin, we then compute the mean of these averages across households. Panel (a) of Figure 1 plots the average of pre-reform mean total cost of sickness across households in each bin.[38] It is clear that the fitted line exhibits a monotonic upward sloping trend reflecting that high-risk households that had more frequent health issues tend to experience higher total cost of sickness prior to the reform. While households in the first decile faced illness cost on average of close to 10 Baht per month, households in the top two bins experienced roughly 300 Baht per month on average during the pre-reform months.

In panel (b), we plot the change (post- minus pre-reform values) in the average total cost of illness across the ten deciles of households. The linear fit exhibits a monotonic downward sloping trend confirming that households associated with higher predicted health risk tend to have larger reductions in the average total cost of illness following the reform.[39] This forms the key to our estimation, which rests on the fact that households with higher health risk are benefiting more from the reform.

This downward sloping trend in the change in the average cost of sickness is quite robust to excluding the opportunity cost of illness component of the cost of sickness (focusing only on the OOP payments). This is illustrated in Appendix Figure A1 in which we observe the downward sloping trend that is not as monotonic as that of the total cost of sickness variable observed in Figure 1. This is because the OOP health expenditure could partly reflect the behavioural choices of households in whether to receive treatments from health facilities. By contrast, the cost of sickness variable includes the opportunity cost component that directly reflects household health conditions, and so is much less affected by the household's choices. Finally, given that we trim the cost of sickness variable based on removing the top 0.5% of the pre- and post-reform data on OOP health expenditure, Appendix Figure A3 presents further robustness checks that our monotonically downward sloping results still hold when considering different levels of trimming: removing the top 0.25%, 1%, and 2% of the OOP health expenditure data.[40]

[38]Note that we trim the OOP health expenditure component in the total cost of sickness variable at the top 0.5% within the pre- and post-reform periods separately. For robustness, we also consider other methods and levels of trimmings (see below for details).

[39]Note that households in the first two bins actually experienced a small rise in the average cost of sickness. This could primarily be driven by age effects as the panel feature of our data means households become older over time. We will control for these potential age effects in our econometric specifications. It could also be possible that healthier households chose to forego treatments of mild symptoms knowing that they had to pay OOP upon hospital visits in the pre-reform period, while for the very unhealthy households, their symptoms were likely too severe and forced them to seek treatments under any circumstances. As the reform substantially reduced health care costs to just 30 Baht per visit, healthier households no longer had to forego treatments of mild symptoms, thus resulting in the slight increases in the cost of sickness post-reform. Our empirical results in Appendix Table A6 later confirm that the number of inpatient and outpatient visits to a health facility of the healthier households actually increased relatively more following the reform.

[40]Note that we have an outlier in bin 5 in panel (b) of Figure 1. A further detailed examination of the data reveals

3.3 Empirical Specifications

3.3.1 Econometric Model

With the morbidity index obtained from PCA, we can construct the interaction term central to our study:

$$T_i^{Pre} \times Post_t,$$

where i indicates households and t indexes the month, and T_i^{Pre} is the PCA morbidity index. We call the variable T_i^{Pre} 'treatment intensity' to reflect the assumption that households with a higher morbidity index should benefit more following the reform. $Post_t$ is a dummy variable for the post-reform period indicating whether month t is later than December 2001.

Baseline Specification. We identify the effect of HI by comparing the evolution of our outcome of interest across households with a distinct degree of treatment intensity that is given by the morbidity index. We estimate the following baseline reduced-form relationship for household i in year–month t:

$$Y_{it} = \beta(T_i^{Pre} \times Post_t) + X_{it}\Gamma + \delta_i + \delta_t + \epsilon_{it}. \tag{1}$$

The parameter of interest β captures the effect of publicly provided HI on the outcome of interest Y_{it}. The specification includes year–month fixed effect δ_t, which captures economy-wide changes in outcome variables or overall time trend, and the household-specific fixed effect δ_i, which controls for household characteristics such as preferences and self-perception of own health risk that are unobservable. X_{it} is a vector of household-level controls that includes the age of the household head and its squared term, a dummy variable for male household head, a fraction of under 15 years children living in the household, a fraction of over 60 years elderly and a set of dummies for household size. To allow for serial correlation in outcomes within the pre-reform and post-reform periods, standard errors are clustered on household times $Post_t$.

One important identifying assumption of our strategy is the parallel trend assumption that the evolution of the outcome of interest would have been similar in the absence of the reform for households

that this outlier is driven by the health expense and opportunity costs of illness of one individual in a household who suffered from a severe symptom that stopped him from working for a long period during the post-reform window. With higher levels of trimming, we find that the outlier depicted in bin 5 of the figures in the right panel of Appendix Figure A3 lies closer to the linear fit.

with a distinct level of predicted health risk. We include in all our specifications the time-varying control variables, X_{it}, that account for changes in household compositions and possible non-parallel trend in outcomes in the absence of the reform. Suppose, for example, that the morbidity index differs across households but converges as the household head gets older and eventually become equally less healthy. Then changes in an outcome that is also affected by the age of household head, say OOP health expenditure, would not have been parallel between the low-risk and high-risk households even without the reform. If not controlled for, this correlation between age of household head and the treatment intensity variable would invalidate the parallel trend assumption. We further discuss threats to our identification strategy in Section 3.3.2.

Note that in 2001, among the four provinces in our data, the government implemented the reform first in one province (Srisaket) in June followed by the remaining three in October. It is reasonable to assume that households did not anticipate the reform in the months before the government won the election in January 2001. However, as soon as the election result was realized, households could have started anticipating the reform and thereby changing their behaviours in response. For this reason, we exclude all months in 2001 from the estimation in our main results. In Appendix E, we provide results for all health expenditure outcomes using event study diagrams to be described in Section 3.3.2 where we include 2001 in our analysis.

Alternative Specifications. We can split the reform impact into short- (Jan'02–Oct'06) and long-run (Nov'06–Dec'10) effects using the fact that October 2006 is the month in which the 30-Baht co-payment per visit was abolished. This specification decomposes the interaction term in specification (1) into two interaction terms corresponding to the short- and long-run post-reform periods.

We also use a 'binary treatment' specification where we split our households into two groups of high- and low-risk types by the median value of the morbidity index. We replace the treatment intensity variable, T_i^{Pre}, in specification (1) with a dummy variable equal to one if the household is a high-risk household. The high-risk (low-risk) households are those whose morbidity index ranks in the top (bottom) five bins. In this case, the impact parameter of interest then measures the effect of the reform on the households with high predicted health risk relative to the low-risk counterparts. This model does not exploit the full variation in household predicted health risk but classifies households into two categories, thereby making it less sensitive to outliers. Another advantage is that it does not assume a linear relationship between treatment intensity and the outcome of interest. For robustness, we also consider the binary treatment model where we compare the top three bins against the bottom seven bins (roughly top one-thirds vs bottom two-thirds), which allows us to test whether our results are driven by households in the very top bins that potentially demanded the services or benefited much more from the reform.

Interpreting the Reform Effects. In the empirical model, the parameter (β) that is identified is the differential effect of universal HI (the reform) with a one-unit increase in the morbidity index T_i^{Pre} on various outcome variables Y_{it}. For example, because the inter-quartile range of the standardized morbidity index is 1.07, we can interpret the estimates as roughly the effect of moving from a relatively healthy household at the 25th percentile to a less healthy household at the 75th percentile.

One question is how our identified parameter relates to the policy relevant parameter that is informative of the value of the reform. Given that the reform expands HI coverage universally, one policy-relevant parameter would be the average treatment effect (ATE) in period t, $E(Y_{it}^1 - Y_{it}^0 \mid t)$, where Y_{it}^1 and Y_{it}^0 are the potential outcomes for household i in period t with and without universal HI. Absent general equilibrium effects, the ATE of the reform can be linked to our microeconomic causal estimand if we make one additional assumption: the effect of universal HI is zero for households with morbidity index $T_i^{Pre} = T_{\min}$; namely, HI has no effect on households with the smallest health risk. Under this assumption, combined with the functional form assumption that the effect of universal HI is linear in the morbidity index, we have

$$E(Y_{it}^1 - Y_{it}^0 \mid t) = \int E(Y_{it}^1 - Y_{it}^0 \mid T_i^{Pre}, t) dF(T_i^{Pre}) = \int \beta(T_i^{Pre} - T_{\min}) dF(T_i^{Pre}) = \beta E(T_i^{Pre} - T_{\min}) \quad (2)$$

where the second equality holds given the two assumptions made above.[41] Therefore, once scaled up by the mean of $T_i^{Pre} - T_{\min}$, our identified parameter can be interpreted as the ATE of the reform. Because our morbidity index is standardized to have mean zero and standard deviation of one, $E(T_i^{Pre}) = 0$ and it follows that $ATE = -\beta T_{\min}$. From data and PCA, we have $T_{\min} = -1.31$. So, the estimated

[41] Given our empirical model (equation (1)), we have that $E(Y_{it}^1 - Y_{it}^0 \mid T_i^{Pre}, t) = \beta T_i^{Pre} + \alpha$, where α cannot be identified as it collinears with δ_t. Note that we have made an assumption that, at $T_i^{Pre} = T_{\min}$, $\beta T_{\min} + \alpha = 0$. Therefore, we have $\alpha = -\beta T_{\min}$, and so $E(Y_{it}^1 - Y_{it}^0 \mid T_i^{Pre}, t) = \beta(T_i^{Pre} - T_{\min})$. The assumption of linearity is obviously strong (equation (1)). However, if this is a concern, we can easily allow for the effect of the universal insurance to be nonlinear in T_i^{Pre}, by including nonlinear terms of T_i^{Pre} interacted with the Post dummy when estimating equation (1).

ATE is the estimated effect β scaled up by a third.

Because the reform is tax financed, and Thailand has a progressive tax system, and because the households in our sample belong to the lower parts of the income distribution, it is reasonable to believe that the implicit premium these households pay lies substantially below the actuarial HI premium.[42] This means that for many of the households the total expected expenditure on health (OOP + premium) is reduced because of the reform. There are then at least two channels through which the HI provision can affect households' financial decisions and well-being. First, it reduces the expected level of medical spending, which in turn helps relax the budget constraint of households (income transfer channel).[43] Second, the provision of insurance reduces the variability of OOP spending and thereby lowers the background risk associated with medical expenditure faced by households (background risk channel). This in turn can incite households to make riskier types of investments and production choices. Later in Section 6, we sketch a theoretical model that illustrates the reform effect through these two channels. Our ATE of interest (equation (2)) measures the combined effects of HI operating through both channels. This estimated overall impact is an important policy parameter for policymakers.

Outliers Robustness. Because our data display a great degree of variability, our estimates can be very sensitive to one single or a handful of observations. For example, the majority of loans and cultivation costs are quite small. Hence, one large-scale borrowing or investments in cultivation could swamp all these activities at smaller scales. To deal with this issue, similar to Kaboski and Townsend (2012), we remove the top 0.5% of the non-zero values (and along with the bottom 0.5% if the variable spans negative values) for all continuous outcome variables. Nevertheless, for comparisons, we report all estimation results using the untrimmed version for the outcome variables that are subject to trimming in Appendix G.

[42]Based on Thailand's Socio-economic Survey data, the average monthly income for Thai households during 1998–2000 was just below 12,500 Baht ($\sim$ \$302). Our sample consists of rural households with pre-reform average monthly income of only 5,282 ($\sim$ \$128) Baht (and with the 75th and 90th percentiles of just 5,296 and 15,640 Baht respectively).

[43]The introduction of HI may also help to decrease the opportunity cost of sickness (earnings loss due to illness) through better access to treatments and improvement in health. Finkelstein et al. (2012) and Gruber, Hendren, and Townsend (2014) find that coverage extension leads to improvement in health outcomes.

3.3.2 Potential Threats to Identification

In this subsection, we discuss three possible threats to our identification strategy: pre-existing differential trend in outcome, mean reversion and coincidental policy reform.

Non-parallel Trend. Despite the time-varying household controls included in all specifications, it is still possible that there exist pre-reform differential trends in the outcome of interest across households that differ in their morbidity index, and thus violate the crucial parallel trend assumption of our treatment-control strategy. For each outcome variable, we therefore examine this possible pre-reform differential trend using a more flexible event study specification:

$$Y_{it} = \Sigma_{j=-5}^{-2}\beta_j^{Pre}(T_i^{Pre} * \tau_j^{Pre}) + \Sigma_{j=1}^{18}\beta_j^{Post}(T_i^{Pre} * \tau_j^{Post}) + \delta_t + \delta_i + X_{it}\Gamma + \epsilon_{it} \quad (3)$$

where τ_j^{Pre}'s and τ_j^{Post}'s are, respectively, the pre-reform and post-reform period dummies for each half-year interval relative to the year of the reform (year 2001).[44] The omitted half-year period preceding the reform year, period -1 (Jul'00–Dec'00), is the base period. Because we do not include the reform year's data, period 0 is also omitted from the specification.[45] The fixed effects and household controls similar to the baseline specification (1) are included. To test for the extent of pre-reform differential trend, we carry out an F-test for the joint significance of β_j^{Pre}'s with the null hypothesis that $\beta_j^{Pre} = 0 \quad \forall j \in \{-5, -4, -3, -2\}$. A rejection of the null hypothesis would suggest an existence of a pre-trend in the outcome of interest controlling for the household characteristics and fixed effects. For each outcome variable, we provide an event study diagram that plots each of the estimated coefficients β_j^{Pre}'s and β_j^{Post}'s from specification (3) over all the half-year periods relative to the base period -1.[46]

Mean Reversion. The concern is that our estimated impact of insurance could merely be a reflection of some transitory mean-reverting shocks that affect households immediately before the onset of the reform. As an illustration, even in the absence of the reform, households affected by a transitory

[44] j is an index of the number of periods (in half-year interval) relative to the year of the reform (year 2001). For example, j=-5 corresponds to the Sep'98–Dec'98 interval, j=1 corresponds to the Jan'02–Jun'02 interval, while j=18 corresponds to the Jul'10–Dec'10 interval. Note that period -5 is the only one that has 4 months.

[45] In Appendix Figure A4, we show the results obtained from the event study specification when year 2001 data are included. In that specification, the omitted base period (period 0) becomes the second half of year 2001 and the first half of 2001 (now not omitted) becomes period -1 and so on.

[46] The event study graph includes the omitted period -1 at which the coefficient takes the value of zero given that it is the base period.

health shock towards the end of the pre-reform period could incur large OOP health expenditure, and subsequently have near-zero health expenditure when their health rebounds afterwards. This can falsely lead to the implication that the reform led to negative health expenditure changes. The same reasoning can be applied to other outcome variables. Given that we have multiple half-year pre-reform periods, we can test for possible mean reversion resulting from any transitory shock prior to the reform by identifying a significant dip (or spike) in the period just before the reform in the event study diagrams.

For health expenditures outcomes, we also consider leaving out the 4 or 6 months immediately before the reform so that our health intensity variable comes from the PCA morbidity index generated using only the first 24 or 22 months instead of the 28-month pre-reform data. We also tried to add interaction terms between the post dummy and the month-specific health risk index in the last quarter prior to the reform year (PCA morbidity index generated based on data in months 25–28).[47] These interaction terms directly control for changes correlated with health conditions in the last quarter before the reform that could result in mean reversion. In addition, we also tried controlling for changes correlated with treatment intensity during the last six months prior to the reform. We do not find our results to be hampered by this mean reversion concern. We report the estimation results with these robustness checks for mean reversions in Appendix Table A3.

Concurrent Village Fund Policy. Another populist policy launched by the newly elected government around the same time as the 30 Baht reform in 2001 is the Million Baht Village Fund Program. The policy injected a uniform transfer of 1 million Baht ($\sim$\$24,000) into around 77,000 villages across Thailand, and every village in our sample received the fund by the beginning of 2002.[48] The fund was then used to establish an independent village bank that provided loans that could be primarily used for investment funds to households in each village. Especially with outcome variables related to household financial status and production decisions, the effect of Village Fund policy may confound our estimated impact of HI if not accounted for.

Because each village received the same fixed amount regardless of the population of the village, larger

[47]Because data on whether households had chronic or disability conditions are available only at the baseline month 0, the month-specific treatment intensity variables are generated by the PCA regression that does not include these conditions.

[48]Thailand's Village Fund program is one of the largest micro-finance schemes in the world. It aims to improve access to finance and income especially in low-income rural areas where credits are often limited. Across the rural households in the Townsend Thai project, the transfers accounted for around 12% of total annual income on average, and for about 40% of total short-term credit flows. See Kaboski and Townsend (2011) for further details.

villages (made up of more households) obtained a relatively more intense credit injection. Similar to Kaboski and Townsend (2012), we exploit the variation in the intensity of credit injection across the 16 villages in our sample as a proxy for the effect of the Village Fund. As additional controls in our specification, we include the interaction terms between year–month fixed effect and village size group dummies characterized by the number of households in each village.[49] These additional terms capture any changes in the outcome variable at the village level, including changes in village-level resources especially due to the Village Fund scheme. Identification of the effect of health reform will come from comparing households differing in predicted health risk within villages of similar sizes.

In addition, we also consider a more direct way of testing an association between treatment intensity and the propensity of Village Fund take-up. Using only the post-reform data, we regress the dummy variable of whether the household held any Village Fund loan in a given month on treatment intensity along with the controls similar to the baseline and the additional village controls. As shown in Appendix Table A4, we find no significant effect of treatment intensity on the Village Fund take-up. This thus lessens the concern of possible confounding from the effect of Village Fund policy on our outcomes.

4 The Effects of Health Insurance on Financial Well-being

In this section, we report and discuss the estimation results of the reform impact on three categories of financial well-being. We first conduct regression analyses of changes in household health expenditure and exposure to risk associated with it. Then, we consider household borrowing followed by consumption outcomes. We describe how we construct each outcome variable in Appendix C. We present the results using one table for each category of outcome variables. In each table, each cell reports the point estimate of the reform impact obtained from different specifications for each outcome variable.

[49] We use the village size information from Kaboski and Townsend (2012) that is based on Thailand's Community Development Department (CDD) data. We classify a village as small if its number of households is less than 64, medium-sized if the village has between 64 and 100 households, and large if it consists of more than 100 households. This classification of village size is arbitrarily defined so that we have a roughly equal number of households in each group. We also consider other cases where the group dummies are replaced by (i) the continuous village size variable and (ii) the inverse of village size as in Kaboski and Townsend (2012). However, the magnitude and significance of the estimated impact of HI obtained are not significantly different for most outcome variables.

4.1 Health Expenditure

We begin by measuring the direct impact of the reform on household OOP health expenditure and cost of sickness. Consistent with the graphical analysis in Section 3.2, in Table 3, we find that the reform significantly led to higher reduction in both OOP health expenditure and total costs of sickness for the high-risk households relative to the low-risk households. The estimates in row (A) suggest that a one-unit increase (roughly the inter-quartile range) in the morbidity index is associated with a post-reform differential reduction in OOP health expenditure of 28 Baht (column (1)) and a differential decline in cost of sickness of 71 Baht (column (2)) per month on average.[50] In columns (3) and (4), we consider two measures of household's exposure to medical expenditure risk: the budget share of OOP health expenditure and whether the household experiences catastrophic health expense.[51] We find that the effects are negative and highly significant being about 0.5 percentage points for the OOP budget share and 1.8 percentage points for whether household health expenditure was catastrophic.

Short- and Long-run Effects. The specification in panel (B) separates the reform impact into short- and long-run effects. Across all outcomes, we find that the magnitude of the long-run impact is around 1.6–1.9 times larger than the short-run impact, and is more statistically significant in columns (1) and (3). This is not unexpected as the 30-Baht co-payment per visit was abolished making healthcare become free for all from the beginning of the long-run period. In row (C), adding village controls to the baseline specification leads to little change in the magnitude and statistical significance of the estimates across all outcomes. This is consistent with our data in that household loans from the Village Fund policy were rarely used to finance the costs of health care.

Binary Treatments. The estimates in the top row of panel (D) correspond to the specification that bisects households into the low-risk and high-risk groups. These are highly significant and are

[50] All outcomes in this subsection are generated based on the OOP health expenditure variable trimmed at the top 0.5% within the pre- and post-reform periods. The results for corresponding outcomes that are generated from the untrimmed OOP payments variable are presented in Appendix Table A16. The results of log(1+x) of expenditures and the number of visits (health care utilization) are reported in the Appendix Table A5 and Appendix Table A6, respectively. Note that we use log(1+x) transformation, which preserves the usual percentage change interpretation of log(x) for x much larger than 1. For x close to 0, log(1+x) is approximately equal to x. However, using log(1+x) and interpreting the estimates as if it was log(x) work well when the data do not contain too many zeroes (Wooldridge, 2016). Hence, our results using log(1+x) transformation need to be interpreted with caution.

[51] Health expenditure is catastrophic if the OOP health budget share exceeds 10%, where the budget share is calculated as the share out of total non-durable consumption.

about twice as large as the baseline estimates. One way to gauge how large these impacts are is by comparing them with the corresponding pre-reform mean of the high-risk households.[52] The pre-reform average OOP health expenditure for the high-risk households was 275 Baht per month. The coefficient in column (1) implies that, on average, high-risk households experienced a differential reduction in monthly cost of sickness of 57 Baht relative to the low-risk counterparts. This is therefore equivalent to about 21% savings on medical expenditures and is comparable to other studies that find that universal health coverage provision led to around 20–50% savings on OOP payments (Wagstaff, 2010; Bauhoff, Hotchkiss, and Smith, 2011; Limwattananon et al., 2015; Garcia-Mandicó, Reichert, and Strupat, 2021).[53] In column (4), the differential reduction in the incidence of catastrophic health expenditure by 3 percentage points for the high-risk households represents a 40% reduction from the pre-reform mean of 7.5%. In the bottom row of panel (D), defining high-risk households as the top one-third in terms of treatment intensity leads to roughly similar estimates across all the outcomes.

Testing for Differential Pre-trend. The inferences drawn from these coefficients crucially rely on attributing the change in trends to the implementation of the 30 Baht reform in 2001. However, if the outcomes for high- and low-risk households were trending differently prior to 2001, the estimated impact may not be due to the reform. To inspect the existence of differential pre-trends in each dependent variable, Figure 2 presents graphical evidence generated by the event study specification (3) along with the p-value of the F-test for the joint significance of β_j^{Pre}'s. Consistent with the results in Table 3, across all outcomes, the coefficients clearly shift downward beginning in the first post-reform half-year interval and remain below zero throughout the post-reform period but with larger shifts in the long run.[54] Importantly, using the p-value of the F-test, we safely cannot reject the null hypothesis of the joint significance of β_j^{Pre}, which suggests insufficient evidence of a pre-trend. This is corroborated by the fairly flat, near-zero patterns in the coefficients during the pre-reform intervals shown by the event study graphs for these outcome variables.

[52] The pre-reform mean of all the outcome variables is reported in Appendix Tables A1 and A2.

[53] Note that these other studies, such as Limwattananon et al. (2015), obtain the estimated medical expenditure savings by comparing the previously uninsured against the control group of insured households. Our estimate, by contrast, is obtained from comparing the high-risk against low-risk previously uninsured households and is unsurprisingly smaller in magnitude.

[54] The long-run post-reform period is from period 11 onwards.

Detecting Mean Reversion. In Appendix Table A3, we check for mean reversion in the outcomes stemming from the last quarter before the reform period in rows (A) and (B) and from the last half-year prior to the reform in rows (C) and (D). Across most specifications, we find that the magnitude and significance of the estimates across all rows are barely affected.[55] These results reduce the concern over possible mean reversion in our treatment intensity especially that associated with the last quarter before the reform. In addition, based on the event study diagrams in Figure 2, there is also no clear sign of mean reversion for all the outcomes that could be identified by a significant shift or spike in the periods prior to the reform.

Including 2001 Data. As discussed in Section 3.3.1, our results are based on excluding the 2001 data in which anticipatory effects could be at play. We present the corresponding event study diagrams that include all the 2001 data in Appendix Figure A4. Note that the base period here corresponds to Jul'01–Dec'01 instead of Jul'00–Dec'00 used previously. Focusing on the cost of sickness outcome in panel (b), there is a differential decline in the cost of sickness for the high-risk households from period -1 to period 0, which is likely to have been a result of the anticipatory effect that could have come into play since the government won the election at the beginning of the year. Importantly, this contributes to a pre-reform differential trend and the p-value of the F-test of just 0.14. Because it is unclear whether the months in 2001 should be assigned a pre- or post-reform status due to this anticipatory effect, we use data that exclude 2001 for the remainder of the paper.

We have now confirmed that the reform led to a relatively higher reduction in the cost of sickness as well as in exposure to risks associated to medical payments for the households with higher health risk. Using the same research design, we now turn to evaluate the effect of HI on various outcomes related to household financial well-being and investment decisions.

4.2 Borrowing and Loan Repayment

In this section, we assess the effects on several outcomes related to household financing. We consider whether households had outstanding loans and measure the value of loans compared with the value of assets using the loans–assets ratio.[56] We examine households' ability to repay the debt by looking

[55] The only exception is for the specification in row (D) of column (2).

[56] The value of loans (deducting interest paid) and that of assets are obtained directly from the Monthly Financial Accounting dataset.

at whether households defaulted on loans as well as at the fraction they managed to repay out of the required amount.[57] Because in developing rural areas with underdeveloped financial systems households also insure against idiosyncratic shocks through transfers within their network (Townsend, 1994; Gertler and Gruber, 2002; Kinnan and Townsend, 2012; De Weerdt and Dercon, 2006) as well as selling their assets (Mitra et al., 2016; Garcia-Mandicó, Reichert, and Strupat, 2021), we also consider the reform effects on these two risk-coping strategies.

In Table 4, the first column displays the effects on whether households had any outstanding loans that we find to be insignificant across all specifications. Columns (2) and (3) then examine the ability of households to repay their existing loans. Conditional to having to repay their loans in a given month, households associated with higher health risks had a differential decline in the default rate and a differential increase in the fraction of loans repaid out of the amount they were obliged to repay following the reform. These effects are statistically significant across most specifications. In column (2), the size of the effect on the default rate ranges from around 1.6 to almost 4 percentage points with a slightly larger long-run impact than the short-run. The estimate in the top row of panel (D) implies that the reform led to a differential reduction of 3.7 percentage points in the default rate, which is about a 58% decline from the pre-reform mean default rate of 6.4% for high-risk households. In column (3), the fraction of loans repaid for the high risk increased relatively more, by 2.9 percentage points from the 93% pre-reform mean of the high-risk type.

In column (4), we find that the reform had a negative impact on the household loans-to-assets ratio, which measures the extent of a household's leverage conditional on the household having any outstanding loans in a given month.[58] In panel (B), we see that the overall impact on the loans-to-assets ratio is attributable to the significant negative long-run effect of 1.2 percentage point, while the short-run impact almost halves. Using the binary treatment specification in panel (D), we find that the impact is driven by households in the very top bins as the estimate is significant only when we compare the top one-third against the bottom two-thirds. On average, relative to low-risk households, the very high-risk reduced their loans-to-assets ratio by 2.6 percentage points, which is equivalent to

[57]These variables are conditional on the months that households reported that they were supposed to repay their loans. Households defaulted on loans if they failed to repay the full amount that they were obliged to repay in a given month. Note that the fraction repaid can be greater than 1 in months where households repaid more than required.

[58]As households in our sample are extensively engaged in informal agricultural activities and investments, we can treat them as a firm and view the household loans-to-assets ratio as analogous to a firm's debt-to-asset ratio. A high ratio indicates households are highly leveraged.

a 24% reduction from the pre-reform mean of 11%. Note that the value of loans used to calculate the conditional ratio reported in column (4) is based on the amount of outstanding loans after deducting paid interest and hence may reflect households' repayment decisions already captured by columns (2) and (3). We show in Appendix Table A7 that our results on borrowing outcomes are robust to using alternative definitions of the loans–assets ratio, where we replace the evolving value of outstanding loans with the value of principal that is fixed over the loan duration as well as using unconditional ratios instead of the conditional ratio.[59]

The results in columns (2) and (3) that households associated with higher predicted health risk had a differential increase in the ability to repay their debts on time due to the reform possibly suggest that their financial situation had improved. However, it is not always the case that the differential reduction in household leverage measured by the loans-to-assets ratio indicates an improvement in financial well-being as this may also reflect lower investment activities or more limited access to credit markets among high-risk households. In Appendix Table A8, we examine the effect of the reform on different purposes of loans by considering a dummy variable for whether households took out a particular type of loan as well as the fractions out of the total value of loans that were used for different purposes. We find some evidence of a positive impact on cultivation investment loans and a negative impact on non-cultivation investment loans, while there is no significant effect on loans made to finance household durable and non-durable consumption.[60] This potentially suggests that the differential decrease in debts could be driven by non-cultivation investment loans. However, the results also indicate the importance of cultivation investments among households in our sample, of which almost 90% were engaged in cultivation. We will turn to assess the reform impact on household cultivation activities in Subsection 5.2.

Because the Village Fund policy that provided access to ample micro-credits was implemented by the government during the same time as the 30 Baht reform, the decline in household debts were unlikely due to a more limited access to credits. Instead, one might worry whether such a large micro-credit program could confound the estimated effect of HI if not accounted for. Nevertheless, for all outcome variables, our estimates in row (C) illustrate that this is not the case as the inclusion of the

[59]The value of the fixed principal is the total principal value of all outstanding loans (not deducting interest paid) held by households in a given month. This comes from the Borrowing module of the Townsend Thai data.

[60]Cultivation loans are made to finance raw inputs and tractors, while non-cultivation investment loans are for livestock, farm equipment, business, land and re-lending. We also consider loans for health care purpose based on the records of how households financed outpatient and inpatient visits conditional on having to pay for the health care visit. However, the number of cases in which households borrowed to finance their treatment costs is trivial.

additional village controls barely affects the significance and magnitude of the baseline estimates.

We next investigate the existence of the pre-trend and mean reversion in our outcomes using Figure 3. Across all outcomes, the p-value for the F-test of the joint significance suggests that there is not sufficient evidence to support that the coefficients for the pre-reform intervals are jointly significantly different from zero, which implies no issue of pre-trend. Based on these event study diagrams, there is also no clear sign of mean reversion for all the outcomes.

Our results so far have focused on borrowings and repayment responsibility. In Appendix Table A9, we now consider two risk-coping strategies that households may use in response to health-related shocks. In column (1), we find negative effects on the net inflow of private gifts and transfers that is driven by the short-run effects. In column (2), we find some evidence that the reform had a negative impact on whether households sold their assets or lands that is also attributable to the effects in the short run. The estimates in the top row of panel (B) imply that, on average, roughly a one inter-quartile range increase in the morbidity index is associated with differential declines in the private transfers of 600 Baht per month and in the probability of selling assets or land by 1.8 percentage points in the short run. Our results suggest that the provision of HI enhance the finances of households, increasing their ability to repay loans and making them less reliant on asset sales and private transfers.

4.3 Consumption Expenditures

The first column of Table 5 reports the estimated impact of the reform on total non-durable consumption.[61] We find a positive effect on non-durable consumption, which is only significant in the long run, for which we estimate a 207 Baht per month rise with respect to a one inter-quartile range increase in the morbidity index or around a 4% rise from the pre-reform mean of high-risk households. Columns (2)–(4) then disaggregate non-durable consumption by its components. We find in column (2) that the impact on food consumption expenditure, which accounts for over half of the effect on non-durable consumption, is also larger and more significant in the long run. Although the results are robust to adding the village controls as illustrated by the estimates in row (C), the estimates obtained from the binary treatment groups specification in row (D) are not significant. We also consider the overall consumption, which includes both non-durable consumption and net expenditures on durable

[61] Total non-durable consumption is the sum of total consumption expenditure and the value of home production on non-durable goods less health-related expenditures.

household goods in column (5), but find that the estimates are quite similar to those in column (1).[62] Primarily through the income transfer channel, provided that the goods are normal, households with higher predicted health risk now have higher expected disposable income, and so we find that their consumption increases relative to those with lower health risk.

Columns (3) and (4) examine household consumption of alcohol and tobacco, which we find to be positive and significant. In column (3), a rise of one inter-quartile range in the morbidity index translates to around a 16–18 Baht per month differential increase in alcohol expenditure in both short and long runs. The point estimates of the impact on tobacco consumption in column (4) are approximately half the magnitude of those on alcohol but are attributable to the short-run effects.

The implied long run marginal budget shares for alcohol and tobacco are 8.8% and 3.7%, respectively.[63] These are economically significant figures. There are two channels that may explain these behavioural responses. The first is *ex ante* moral hazard: households substitute public HI for self-protection. While theoretically possible, we believe that the serious health risks connected to excess consumption of alcohol and tobacco mitigate such substitution behaviour. The second channel is a standard income effect. Irrespective of the relative importance of these channels, the economic significance of the responses is large enough to suggest that the HI reform could have benefited from a simultaneous increase in excise taxes on demerit goods.

In Figure 4, we present further graphical evidence using an event study analysis. The F-test for joint significance of the pre-reform interaction terms, which is insignificantly different from zero relative to the base period, indicates no sufficient evidence of pre-trend for all outcome variables except in panel (e).[64] There is also no clear evidence of mean reversion for all the variables. Overall, the coefficients obtained from a more flexible event study specification (3) show patterns that are quite consistent with the estimates in Table 5 with total non-durable consumption, food, alcohol and tobacco consumption displaying a clear upward shift in coefficients in the long run.[65]

[62] See Appendix C for more details of each outcome variable.

[63] These figures come from the fact that, for the additional total expenditure of 207 Baht, 18.24 Baht goes to alcohol and 7.76 Baht goes to tobacco consumption.

[64] Note that for alcohol and tobacco consumption expenditure in panels (c) and (d), there appears to be a differential pre-trend in periods furthest from the reform (periods -5 and -4). However, this trend is declining to close to zero from period -3 leading up to the reform year. In panel (e), infrequent records of net expenditures on durables in our data cause a fluctuation pattern resulting in potential pre-trend during the pre-reform months.

[65] The results in this subsection are based on the outcome variables that are trimmed to remove the effects driven by potential outliers. Total consumption in columns (1) and (5) is trimmed at the top and bottom 0.5%, while food, alcohol, and tobacco consumption is trimmed at the top 0.5% within the pre- and post-reform periods. Appendix Table

5 The Effects of HI on Investments

In this section, we investigate the reform impact on two types of investment: human capital and cultivation investment in which we also consider household portfolio allocation among different types of crops. We focus on cultivation activities because almost 90% of households in our data were engaged in cultivation and because of our previous results that cultivation investment loans became relatively more important following the reform.

5.1 Education and Child Labour

Conditional on households that had children aged 10–18 years in a given month, we examine the reform impact on household education expenditures, whether children were enrolled in school as well as whether the household used child labour either at home or as outside-household workers.[66] The results are presented in Table 6.[67]

Overall, we find that the reform led to a differential rise in school enrollment and a differential short-run decline in child employment among households that benefited more from the reform. In column (1), the baseline estimate suggests that an increase of roughly an inter-quartile range in the morbidity index (1.07) is associated with a differential increase in school enrollment rate of 5 percentage points in the short run. This is equivalent to a differential rise of 6.7% from the pre-reform average enrollment rate of 74% for high-risk households. The impact on education enrollment is driven by households in the very top bins with the effects estimated by the binary treatment groups model (bottom row of panel (D)) being significant at the 1% level. This represents a differential increase on average of 11.4 percentage points for households in the top one-thirds relative to the bottom two-thirds ranked by the morbidity index.

In columns (2) and (3), we find that the differential decline in the incidence of child labour is mostly driven by the relative decline in children employed outside households. In column (3), similar to education enrollment, we find larger significant effects in the short run. The estimate for the short-

A17 reports the results obtained from the variables that are not trimmed for comparison. We find more significant and larger effects for overall non-durable, food and alcohol consumption. However, the estimates for tobacco and the overall consumption that includes net expenditures on durable goods are no longer significant, which suggest that the estimates for these two outcomes we find in Table 5 would be quite sensitive to the outliers that we have eliminated.

[66] See Appendix C for details of how we construct these variables from the data.

[67] For robustness, we show that our results in this subsection are robust to using different age groups of children in the Appendix Tables A11 and A10 which, report the results for the 7–18 and the 10–20 age groups, respectively.

run effects indicates a differential decline in the probability that household had child labour away from home of 4.4 percentage points, which is equivalent to almost a 40% reduction from the pre-reform incidence of 11.6% among high-risk households.

In column (4), there is some evidence that HI provision had positive impact on household education expenditure in the short run. The short-run effect (panel (B)) sizes about 42 Baht per month on average, which is equivalent to around a 6% increase from the pre-reform mean of 656 Baht per month for the high-risk households.

We use the results on event study analysis shown in Figure 5 to examine the validity of our results with respect to possible pre-trend. In general, we see patterns that are consistent with the estimates in Table 6. The post-reform coefficients shift upwards for school enrollment (panel (a)), and shift downwards for the child labour outcomes (panels (b) and (c)). The shifts are clearly larger during the short-run post-reform period. Except for education enrollment, we cannot reject the null hypothesis of the F-test for joint significance, indicating that there is not enough evidence of pre-trend.[68] Moreover, across all the outcomes, there is also no evidence of mean reversion.

As medical costs were insured, we would expect households that potentially benefited more from the reform through a higher reduction in the expected costs of sickness to also become relatively less susceptible to earnings capacity risk in addition to having a more relaxed budget constraint.[69] Indeed, we find that these households in turn increased their human capital investments relatively more and became relatively less reliant on the costly risk-coping mechanism of using child labour to help finance their living.[70] One possible reason why we tend to find more sizable and significant effects on human capital investments in the short run is the implementation of nationwide education reform in 2009 that extended the state-school education supports from 12 years to 15 years to cover 3 additional years at

[68]Although the F-test does not support the non-existence of pre-trend for school enrollment (panel (a)), the graphs illustrate that this might be due to differential trend in pre-reform periods furthest from the reform. In periods approaching the reform, the pattern becomes closer to zero, which suggests that the outcome was not trending so differently in comparison to the base period. Note that this evidence of pre-trend in education enrollment could be due to a major education reform enacted by the National Education Act 1999. The Act assured that every student either in public or private school was entitled to the same level of government's support for 12-year basic education covering tuition, books and equipment, and uniforms. The support amount was supposed to be sufficient for providing free education in state schools. In terms of compulsory schooling, the Act declared an increase in compulsory schooling from primary to lower secondary education, or from 6 to 9 years of schooling.

[69]Uninsured adverse health shocks not only create a medical expenditure risk but can also generate an earnings capacity risk or productivity risk, which may translate into disruptions in child education either because such education is costly for parents or because children need to help maintain the income of household.

[70]Such findings are consistent with studies that estimate the effects of HI using the reform in China (Liu, 2016) and Ghana (Garcia-Mandicó, Reichert, and Strupat, 2021).

the pre-primary level. In the long run, the financial burden of households with young kids was thus reduced by the same extent regardless of their health risk.

5.2 Input and Output in Cultivation

Conditional on households having cultivated lands in a given month, the point estimates in the first column of Table 7 indicate that the reform had a significant negative impact on total cultivated land size owned by households in the long run.[71] The estimate in the bottom row of panel (B) indicates a differential decline in the cultivated land size of 1.31 Rai.[72] However, in column (2), we find that the provision of HI had no significant impact on the direct costs of cultivation.[73] This is consistent with the findings in Cole, Giné, and Vickery (2017) who examine how the innovative rainfall insurance index affects production decisions and find no significant insurance effect on cultivation costs even with the insurance directly targeting cultivation. In addition, Appendix Table A12 reports the estimated effects on different types of cultivation costs (seeds, fertilizers, pesticides, hired labour, equipment and agricultural assets), most of which we find to have non-significant effects.

In column (3), we find negative and significant effects of HI on the opportunity cost of family labour. This opportunity cost of family labour reflects the value of household labour and is calculated as the provincial-specific hourly minimum wage rate times the total number of hours that all family labour in a household spent on cultivation activities in each month.[74] The baseline estimate indicates that an increase of roughly one inter-quartile range in the morbidity index is associated with a 108 Baht per month differential decline in the opportunity cost of family labour. The results in Appendix Table A13 further suggest that this decline in the opportunity cost is not due to the reduction in child employment that we found earlier in Section 5.1. We find a statistically significant negative effect on the number of

[71] Total cultivated land size is the sum of all crop-plots' size in Rai, a measure of unit of land in Thailand. Approximately 1 acre is equivalent to 2.52 Rai. The mean size of total cultivated land is 19.95 Rai. We have a panel of crop plots, which records the details of each crop grown by a household along with all plot operations, input uses and the associated costs. Around 3% of all crop plots have missing plot size. These are plots in which households usually grew crops in an unorganized manner (e.g., scattered around the house, circling a fish pond or along the fence). We exclude these plots from our analysis and only use organized crop plots for which households report non-missing plot size to generate all cultivation outcomes. Note that the outcome variables in this subsection are constructed based on excluding observations associated with a cultivated land size that is smaller than 0.007 Rai (0.002 acres or 11.2 sq.m.) or the bottom 0.5%. Appendix Table A18 shows that we still reach the same conclusion for most outcomes when the outcomes are not trimmed.

[72] 1 Rai is approximately 0.4 acres or 1,600 sq.m.

[73] The direct costs of cultivation include different types of cultivation costs listed as outcome variables in Appendix Table A12. Note that this excludes the opportunity cost of family labour.

[74] The hourly minimum wage rate is estimated by the official daily minimum wage rate divided by eight. The minimum wage data are from a series of Notifications on the Minimum Wage Rate of Thailand's National Wage Committee.

hours spent on cultivation by family labour aged above 18 years, but there was no significant impact when restricting the age of family labour to between 10 and 18.[75]

In column (4), the estimates of the effects on harvest value are not significant. However, in column (5), the estimate in panel (B) implies a differential long-run increase in net cultivation income of 1,053 Baht, or 32% compared with the pre-reform mean of the high-risk, which is significant at 5% level.

Panels (a)–(f) of Figure 6 provide the event study diagrams for all the outcomes in this subsection. The p-values of the F-test for joint significance of the pre-reform interaction terms suggest no sufficient evidence of pre-trend across all the outcomes except for the opportunity cost of family labour in panel (c). However, the pattern in panel (c) shows that the possible differential pre-trend for this outcome is arguably less alarming given that the larger deviations from zero in the pre-reform coefficients show up in the pre-reform periods that are furthest from the reform year.

As we find the overall cultivation investments and harvest value to be unaffected by the reform, three possible mechanisms could help explain the differential fall in hours worked in cultivation. First, this could be due to capital-labour substitution where relatively more equipment and machines are used to enhance productivity of labour.[76] However, the result in column (5) of Appendix Table A12 that the costs of equipment and agricultural assets are not significantly affected by the reform does not lend support to this argument. Second, the relative decline in hours worked in cultivation may also suggest a shift in labour allocation away from cultivation and could well be welfare improving provided that households substitute cultivation labour supply for leisure or for other types of production activities that are more profitable. However, our results provided in Appendix Table A14 indicate that shifts towards other types of production might not be the case as the estimated effects of the reform on total net income and net farming income for households with cultivated land are insignificant.[77] Finally, the relative decline in cultivation hours worked can also imply a switch in crop choices towards ones that are less labour-intensive. We investigate this effect on crop portfolio in the next section.

[75]Investigating the opportunity cost of family labour by age groups leads to similar results because the provincial minimum wage rates are quite constant over our estimation period.

[76]Productivity can be defined as the harvest value divided by the number of hours worked in cultivation. The relative improvement in productivity is consistent with our earlier findings in Appendix Table A8 that high-risk households took out investment loans for cultivation to buy more agricultural equipment and machines compared to the low-risk. However, whether such enhancement in productivity was due to relative improvement in health following the reform is an open question.

[77]See Appendix C for details of how household net income is constructed.

5.3 Portfolio of Cultivation Investments

We focus on two outcome variables: whether the crops grown by households are cash crops, and the fraction of cultivated land devoted to cash crops. Again, both outcomes are conditional on households having cultivated land in a given month. This allows us to investigate how the post-reform reduction in household background risk associated with medical expenditure can affect household risk-taking decisions.[78]

Defining Cash Crops. Cash crops reflect higher risk-taking by households and are defined based on region-specific volatility in the value of output per Rai.[79] For each crop grown by each household, we first calculate the within-household year-on-year log difference in the value of output per Rai over the estimation period. We then calculate output volatility as the standard deviation of the year-on-year change in the log value of output per Rai for each region and each crop. A particular crop is then classified as a cash crop if its volatility exceeds the median value of output volatility across all crops within the same region.[80] Our definition of cash crop already accounts for possible differences in volatility that are specific to output across different crops and regions, which may result from the differences in, say, weather, soil quality or pest conditions. Given that we use the value of output to classify cash crops, the volatility in the market price of each crop is also incorporated. Note that, using our definition, a specific crop may be classified as a cash crop in one location but may not be in another.

Column (1) of Table 8 examines the effects at the extensive margin considering whether households grow cash crops conditional on household owning a cultivated land. We find some evidence of a differential increase in the conditional probability of growing a cash crop among high-risk households. The effects across all specifications are positive, but are significant only in the long-run baseline and village control models. The point estimate in panel (B) of column (1) indicates that, in the long run, a one-unit increase in the morbidity index is associated with a differential increase of 2.9 percentage

[78]Ideally, one would wish to consider the costs of investment associated with each crop plot to assess cultivation investment decisions. However, our data do not perfectly capture all associated costs for each crop plot as information on the costs of machinery, equipment, and fixed agricultural assets is not crop-specific.

[79]We have data on over 24 different types of main crops in Thailand including rice, maize, sorghum, sugar cane, mango, coconut, banana, mulberry, tapioca, lemongrass, eucalyptus along with many other fruits, vegetables, and perennial trees. The four provinces in our sample are categorized into two regions: Central and Northeast.

[80]Alternatively, one can define cash crops by incorporating rainfall data. For instance, we can define cash crop as one with output that is more correlated with weather changes. However, at the time of writing, we do not have access to the rainfall data in the study area.

points in the probability that a household with a cultivated land grows a cash crop.

Column (2) considers the fraction of total cultivated land devoted to cash crops (intensive margin). We find that the impact is positive and generally significant with the impact again driven by the long-run effects. The estimate in panel (D) implies that the reform led to a differential increase of 4.9 percentage points in the fraction of land devoted to cash crops among high-risk households in the long run, which is equivalent to around a 13% differential increase from the pre-reform average of 38%.

Figure 7 display the event study diagrams for both outcome variables in this subsection. There are clear upward shifts during the long-run post-reform periods that depict the switch in crop choice towards more risky cash crops. Overall, there are also no clear signs of pre-trend or mean reversion.

The results in this subsection suggest that, despite overall cultivation investment and harvests remaining virtually unchanged by the provision of public HI, higher-risk households did actually shift their crop portfolio relatively more than lower-risk households towards cash crops especially at the intensive margin and in the long run. The mechanism that comes to mind is that the reform could have reduced medical expenditure risk, which forms a substantial part of rural households' background risk, and thereby allowed them to engage in more risky cultivation activities. Combined with the differential decline in hours worked on cultivation among higher-risk households, this differential shift towards cash crops could also imply that growing cash crops was relatively less labour intensive. Finally, our findings on risk-taking are consistent with Karlan et al. (2014) and Cole, Giné, and Vickery (2017) who use a RCT to show that the provision of rainfall index insurance induced farmers to take on riskier crop choices and investments. Although our HI does not directly target cultivation as is the case for rainfall insurance, our results suggest that both health and rainfall insurance similarly reduce one of the risks that households are exposed to and eventually led them to undertake riskier crop options.

6 Interpretation using an Intertemporal Model of Household Choice

To interpret the effects of HI on financial decision making, we may consider the rural household solving an intertemporal decision problem where it decides how much to consume in the current period and how many resources to transfer to future periods, either via a riskless asset or through a risky investment, like cash crop investment. Future consumption then depends on the amount transferred, minus the OOP payments required to finance health treatment in case needed.

What implications does HI have for the consumption-savings decision and the allocation of total savings over the riskless and risky channels? As discussed in Section 3.3.1, the introduction of subsidized HI has two effects: (i) it reduces the variability of medical expenditure (background risk channel), and (ii) it reduces the expected level of total health expenditure (income transfer channel).

It is useful to first focus on the background risk channel by assuming actuarial pricing (no subsidization). Using a two-period version of the problem, it can be shown that (i) prudence is necessary and sufficient for current consumption to increase and total savings to fall, and (ii) decreasing absolute prudence (DAP) is sufficient for risky investment to increase.[81] The critical role of prudence for the first result is well known from the literature on precautionary savings (Kimball, 1990). The critical role of DAP for the second result is more subtle, but not difficult to understand. (Harvest) state-contingent future marginal utility is itself random because of the background risk. The expected state-contingent marginal utility can be expressed as the marginal utility of state-contingent expected consumption minus a prudence premium, i.e., of a state-contingent certainty equivalent.[82] With DAP, the prudence premium in the high return state is smaller than in the low return state. Because optimal risky investment balances the expected rate of return weighted by the marginal utility of consumption (*i.e.*, the risk-adjusted rate of return) with that of the safe return, the reduced background risk tilts the investment decision in favour of the risky channel. In addition, because total savings are reduced, DAP ensures that the *share* of risky investment increases.[83]

However, as argued earlier, most households in our sample only pay a fraction of the actuarial premium. This creates an additional income effect on insurance, which under normality of consumption will be spread out over both periods.[84] As a result, result (i) mentioned above is strengthened (making prudence only sufficient), and, under decreasing absolute risk aversion (DARA) and DAP, result (ii) mentioned above is also strengthened.

Our empirical findings are broadly consistent with these predictions of the two-period model. Non-durable consumption increases, and total cultivation investment goes down, but the share of investment

[81] A decision maker with a vNM utility function $u(\cdot)$ is said to be prudent if $u'''(\cdot) > 0$. DAP means that the coefficient of absolute prudence, defined as $-\frac{u'''(c)}{u''(c)}$, falls in consumption.

[82] The prudence premium for (harvest) state j, ψ_j, is defined as $Eu'(y_j - \widetilde{\varepsilon}) = u'(y_j - E\widetilde{\varepsilon} - \psi_j)$ where y_j is disposable income in state j and $\widetilde{\varepsilon}$ is random health expenditure. Hence, $y_j - E\widetilde{\varepsilon} - \psi_j$ is the prudence certainty equivalent.

[83] More details of the model and its predictions are provided in Appendix D.

[84] In particular if the household has to pay the full actuarial premium, the income effect is zero, leaving only the background risk effect. If the premium is 100% subsidized, the income effect is maximal.

in risky cash crops increases. Our results are important for measuring welfare consequences of introducing or extending HI. Although behavioural responses are of second order when assessing welfare effects of a marginal extension of HI (by the envelope theorem), such responses can no longer be ignored when the HI reform is large. We leave such welfare calculations to future work.

7 Conclusions

In this paper, we exploit a universal health reform in Thailand, which caused heterogeneous impacts among the previously uninsured households that differ in terms of their predicted health risk. We utilize a range of pre-reform health-condition variables to predict this health risk, and show that households associated with higher health risk are those that benefit more from the reform as they experienced a larger reduction in the expected cost of illness. We exploit exogenous variation in this treatment intensity as the source of identification to estimate the effects of health insurance on various household financial well-being and investment outcomes. We perform various checks to confirm that our results support the main identifying assumption of parallel trend, are robust to including the effect of the concurrent Village Fund policy, and are not significantly hampered by mean reversion or outliers.

Our analyses reveal that the reform led to differential declines in OOP health spending and exposure to risk associated with it for households with higher health risk. These households also had differential increases in consumption and ability to repay debts, together with differential declines in their reliance on loans, private transfers and selling assets. These improvements in household financial well-being and welfare serve as empirical evidence that sheds light on the principal role of public health insurance in providing financial security, especially to those in rural or developing areas. But our results also show that the reform resulted in increased consumption of demerit goods like alcohol and tobacco, suggesting that higher excise taxes on these goods could have been beneficial.

Consistent with the precautionary motive model, we show that higher-risk households shifted their cultivation portfolio towards riskier crop choices at the intensive margin relatively more in the long run in response to the reduced background risk of medical expenditure due to HI. Our results further suggest that, rather than capital-labour substitution or a switch to other types of production than cultivation, the margin of crop choice is likely the reason for household labour supply adjustment in that relatively fewer hours were worked by higher-risk households. Importantly, we also find for these households a short-run differential increase in human capital investment and a decline in the use of child labour, which arguably is one of the most costly risk-coping strategies given its potentially adverse impact in later life of the children.

From a policy perspective, our empirical evidence suggests that expanding HI not only have pronounced effects in improving household financial well-being, but its financial implications also spreads beyond health care users and providers and into various aspects of the economy including production and human capital creation. Despite not directly targeting agricultural production risk like rainfall or crop insurances, we show that HI can affect production choices and investments through the channels of income transfers and background medical expenditure risk reduction. Policymakers should thus take these into account when valuing or deciding on public health insurance schemes.

Table 1: Comparison of HI Schemes Pre- and Post-Reform

Pre-Reform	**Description**	**% of Pop.**	**% HHs Data**	**Post-Reform**	**Reform Impacts**
Previously Uninsured	Paid for healthcare out-of-pocket	29	65	Universal health coverage (UHC) with 30 Baht copay (no copayment from 2006)	Households faced reduction in care prices. Hospitals receive 1,200 Baht per head instead of out-of-pocket payments (previously uninsured) or 1,500 Baht per household (VHCS)
VHCS	Voluntarily paid 500 Baht premium per household per year to get free care for up to five members	20	5		
MWS	Free care for the poor, those aged < 12 and > 60, monks, disabled	30	21	UHC with no copayment	Enrollees faced no change in prices, but benefited from increased capitation budget (250 Baht to 1,200 Baht).
SSS	Free care for private sector employees	12	9	SSS	No formal changes
CSMBS	Free care for civil servants and dependents	9		CSMBS	

Note: The percentage of population in each insurance types is based on the pre-reform distribution in 2001 and is from Gruber, Hendren, and Townsend (2014) and Thailand's Health and Welfare Survey. VHCS denotes Voluntary Health Card Scheme. In our data, VHCS households are those reported paying for the health card at least once during pre-reform months. MWS denotes Medical Welfare Scheme. We classify MWS households as those that do not include a member who reported paying for *public* outpatient and inpatient services during pre-reform months, and had average pre-reform monthly household income below the 2,800 Baht ($\sim$\$2.01/day)) eligibility criteria. SSS refers to Social Security Scheme. SSS households are those in which all members are private employees, which can proxied in our data as individuals that, in at least half of the pre-reform months, their employer paid for or provided health services when they became ill or injured either from work or away from work, and that they were either a daily or monthly wage employee, or a piece rate employee who worked for a business organization. CSMBS denotes Civil Servant Medical Benefits Scheme. CSMBS households are those whose at least one member was a CSMBS recipient during the pre-reform period. This is an individual that, in at least half of the pre-reform months, was a government worker or a village head (also eligible to receive CSMBS benefits), or reported receiving employment-related health benefits that also covered their family members.

Table 2: Summary Statistics, Characteristics & Health Conditions of Households (Monthly Pre-reform Data)

	Low-risk	High-risk	Diff.(p-val)
(A) Characteristics of Households			
Household size	4.061	4.384	0.000
	(1.624)	(2.055)	
Number of kids (aged under 15)	1.164	1.267	0.000
	(1.060)	(1.152)	
Number of elderly (aged over 60)	0.474	0.761	0.000
	(0.707)	(0.811)	
Age of household head	50.34	54.86	0.000
	(12.78)	(14.30)	
Male household head	0.750	0.754	0.586
	(0.433)	(0.431)	
Years of head's education	4.401	3.969	0.000
	(2.267)	(1.980)	
(B) Health Conditions & Expenditures of Households			
Member(s) had symptom(s)	0.230	0.560	0.000
	(0.421)	(0.496)	
% of days member(s) had symptom(s)	0.060	0.285	0.000
	(0.190)	(0.468)	
Member(s) had work-limiting symptom(s)	0.059	0.189	0.000
	(0.236)	(0.392)	
% of days member(s) had work-limiting symptom(s)	0.010	0.055	0.000
	(0.058)	(0.190)	
Member(s) with chronic health conditions	0.187	0.496	0.000
	(0.390)	(0.500)	
Member(s) with disability conditions	0.000	0.115	0.000
	(0.000)	(0.319)	
Standardized morbidity index	-0.696	0.685	0.000
	(0.304)	(0.928)	
Number of visits to health facilities	0.134	0.434	0.000
	(0.414)	(0.741)	
Out-of-pocket health expenditure (Baht/month)	65.7	275.6	0.000
	(1,030)	(3,640)	
Total cost of sickness (Baht/month)	83.5	361.7	0.000
	(1,070)	(3,782)	
Number of *pre-reform* observations	7,236	7,205	

Note: The table shows the mean and standard variation (in parentheses) for each variable of interest which are calculated over the 28-month pre-reform period (Sep' 97 - Dec'00). All variables are in monthly format. High-risk (low-risk) households are those whose morbidity index ranks above (below) the median value and thus are predicted to be less (more) healthy and potentially benefited more (less) from the reform. The morbidity index is constructed by the principal component analysis (PCA) using the health condition variables shown in the top six rows of panel (B) during the pre-reform months. See Section 3.1 for more details.

Table 3: Impacts on Health Expenditure Outcomes

Dependent Variables:	(1) OOP health exp	(2) Cost sickness	(3) OOP exp share	(4) Catastrop. exp
(A) Baseline	-27.95***	-71.18***	-0.005***	-0.018***
	(8.79)	(14.44)	(0.002)	(0.004)
(B) Short- & Long-run Effects				
Short-run Effects	-21.12**	-57.37***	-0.004*	-0.015***
	(9.17)	(14.68)	(0.002)	(0.005)
Long-run Effects	-39.72***	-94.94***	-0.007***	-0.024***
	(8.65)	(17.84)	(0.002)	(0.005)
(C) With Village Controls	-27.91***	-70.24***	-0.005***	-0.018***
	(8.85)	(14.16)	(0.002)	(0.004)
(D) Binary Treatment				
High- vs Low-demand	-57.25***	-136.83***	-0.010***	-0.030***
	(9.90)	(31.07)	(0.002)	(0.006)
Top 1/3 vs Bottom 2/3	-50.71***	-155.26***	-0.009***	-0.027***
	(13.64)	(35.14)	(0.003)	(0.008)
Number of Observations	64,886	64,886	64,886	64,886

Note: Robust standard errors, clustering on household times $Post_t$, in parentheses: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. The dependent variables are denoted in the column headings and each cell reports estimates from different regressions. All regressions include household and time (in year-month) fixed effects. Included household-level controls are the age of the household head and its squared term, a dummy variable for household head being male, fraction of under-15 kids living in household, fraction of over-60 elderly, and a set of dummies for household size. The sample consists of previously uninsured households that do not miss more than 3 months of the monthly surveys in the Townsend Thai project within the pre-reform (Sep'98-Dec'00) and the post-reform (Jan'01-Dec'10) periods. Row (A) and (C) report estimates of the interaction between the treatment intensity variable and the post dummy ($T_i^{Pre} \times Post_t$). T_i^{Pre} is the morbidity index generated from the principal component analysis using pre-reform history of household's health conditions. The village controls in (C) is the village size group dummies interacted with time fixed effects. Row (B) short-run and long-run report the estimates for the interaction of T_i^{Pre} with the short-run (Jan'01-Oct'06), and with the long-run (Nov'06-Dec'10) post dummy respectively. Panel (D) reports the estimates of the interaction between the dummy for high-demand household and $Post_t$ in the top row and that between the dummy for households in the top one-thirds ranked by the morbidity index and $Post_t$ in the bottom row. All dependent variables are constructed using household OOP health expenditure trimmed at the top 0.5% within the pre- and post-reform periods. OOP health expenditure is catastrophic when it accounts for over 10% of household total non-durable expenditure.

Table 4: IMPACTS ON BORROWING OUTCOMES

Dependent Variables:	(1) Has loans outstanding	(2) Default on loans	(3) Fraction repaid	(4) Loans-assets ratio
(A) Baseline	-0.016	-0.017***	0.012*	-0.009**
	(0.011)	(0.007)	(0.006)	(0.004)
(B) Short- & Long-run Effects				
Short-run Effects	-0.011	-0.016**	0.012*	-0.007*
	(0.013)	(0.007)	(0.007)	(0.004)
Long-run Effects	-0.023	-0.019***	0.012*	-0.012*
	(0.015)	(0.007)	(0.007)	(0.006)
(C) With Village Controls	-0.010	-0.017**	0.012*	-0.008**
	(0.012)	(0.007)	(0.006)	(0.004)
(D) Binary Treatment				
High- vs Low-demand	-0.024	-0.037***	0.029**	-0.017
	(0.026)	(0.012)	(0.012)	(0.011)
Top 1/3 vs Bottom 2/3	-0.003	-0.014	0.003	-0.026**
	(0.029)	(0.013)	(0.012)	(0.011)
Number of Observations	64,990	15,639	15,639	51,478

Note: Robust standard errors, clustering on household times $Post_t$, in parentheses: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. The dependent variables are denoted in the column headings and each cell reports estimates from different regressions. For different model specifications, see footnote under Table 3. Whether household defaults on loans (2) and fraction of loans repaid out of the amount household is obliged to repay (3) are conditional on months that households are supposed to repay their loans. Loans-assets ratio (4) is the total value of outstanding loans divided by the total value of assets.

Table 5: IMPACTS ON CONSUMPTION OUTCOMES

Dependent Variables:	(1) Non-durable consumption	(2) Food exp.	(3) Alcohol consump.	(4) Tobacco consump.	(5) Durable & non-dur. cons.
(A) Baseline	108.1	74.86**	16.87**	9.25*	113.84
	(72.76)	(34.82)	(7.17)	(4.88)	(84.19)
(B) Short- & Long-run Effects					
Short-run Effects	49.71	57.92	16.06**	10.11**	59.31
	(78.89)	(35.56)	(7.49)	(4.92)	(92.51)
Long-run Effects	207.4**	103.6**	18.24**	7.76	206.77*
	(104.1)	(41.95)	(8.85)	(5.73)	(112.23)
(C) With Village Controls	102.0	74.87**	16.78**	8.49*	97.70
	(75.19)	(35.77)	(7.39)	(4.96)	(85.76)
(D) Binary Treatment					
High- vs Low-demand	2.84	84.99	26.74*	7.81	110.69
	(128.6)	(56.69)	(15.21)	(9.77)	(146.42)
Top 1/3 vs Bottom 2/3	94.73	46.05	12.66	10.71	115.60
	(146.0)	(63.43)	(18.06)	(10.89)	(168.08)
Number of Observations	64,393	64,677	64,692	64,628	64,367

Note: Robust standard errors, clustering on household times $Post_t$, in parentheses: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. The dependent variables are denoted in the column headings and each cell reports estimates from different regressions. For different model specifications, see footnote under Table 3. Total non-durable consumption (1) and total durable and non-durable consumption expenditure (5) are trimmed at the top and bottom 0.5% within the pre- and post-reform periods. Food (2), alcohol (3) and tobacco (4) expenditures are trimmed at the top 0.5% within the pre- and post-reform periods.

Table 6: Impacts on Education & Child Labor (aged 10-18) Outcomes

Dependent Variables:	(1) Whether in school	(2) Child labor (home)	(3) Child labor (outside HH)	(4) Education exp
(A) Baseline	0.041**	-0.001	-0.036**	28.17
	(0.020)	(0.008)	(0.017)	(20.67)
(B) Short- & Long-run Effects				
Short-run Effects	0.050**	-0.014	-0.044**	42.35**
	(0.020)	(0.009)	(0.018)	(23.28)
Long-run Effects	0.029	-0.004	-0.026	9.07
	(0.022)	(0.010)	(0.020)	(26.67)
(C) With Village Controls	0.036*	-0.009	-0.039**	22.98
	(0.019)	(0.008)	(0.017)	(21.18)
(D) Binary Treatment				
High- vs Low-demand	0.039	-0.013	-0.037	8.46
	(0.032)	(0.015)	(0.026)	(51.63)
Top 1/3 vs Bottom 2/3	0.114***	-0.027	-0.070**	100.47
	(0.037)	(0.019)	(0.030)	(62.08)
Number of Observations	41,028	41,028	41,028	41,028

Note: Robust standard errors, clustering on household times $Post_t$, in parentheses: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. The dependent variables are denoted in the column headings and each cell reports estimates from different regressions. For different model specifications, see footnote under Table 3. All outcomes are conditional on household having children aged 10-18 years. Child labor refers to children aged 10-18 years that are not enrolled in school in a given year and are in workforce.

Table 7: Impacts on Cultivation Outcomes

Dependent Variables:	(1) Cultivated land size	(2) Direct costs cultivation	(3) Opp cost family labor	(4) Harvest value	(5)=(4)-(3)-(2) Net income Cultivation
(A) Baseline	-0.90*	-176.93	-107.62**	55.07	339.61
	(0.48)	(116.99)	(46.28)	(325.48)	(276.88)
(B) Short- & Long-run Effects					
Short-run Effects	-0.68	-174.52	-111.07**	-332.27	-46.68
	(0.52)	(115.85)	(46.78)	(340.65)	(293.28)
Long-run Effects	-1.31**	-181.38	-101.23*	770.11	1,052.73**
	(0.62)	(161.54)	(52.82)	(536.78)	(469.84)
(C) With Village Controls	-0.86*	-141.40	-107.31**	83.63	332.34
	(0.48)	(180.50)	(46.14)	(336.56)	(279.60)
(D) Binary Treatment					
High- vs Low-demand	0.32	20.21	-165.23*	906.53	1,051.55
	(1.04)	(544.09)	(90.47)	(795.21)	(696.10)
Top 1/3 vs Bottom 2/3	-0.89	-993.37	-177.08	-10.53	1,159.92
	(1.13)	(777.94)	(108.66)	(897.31)	(1,004.55)
Number of Observations	44,210	44,210	44,210	44,210	44,210

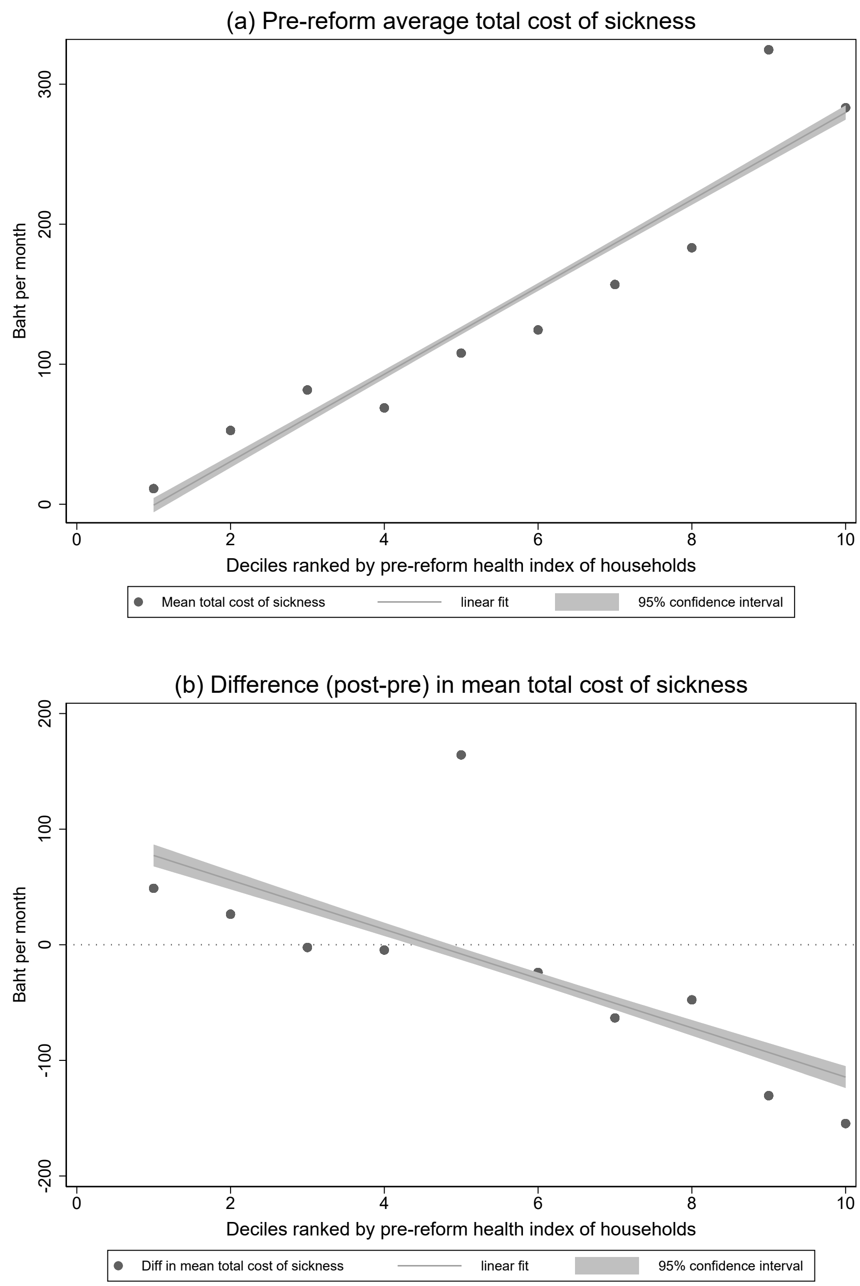
Note: Robust standard errors, clustering on household times $Post_t$, in parentheses: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. The dependent variables are denoted in the column headings and each cell reports estimates from different regressions. For different model specifications, see footnote under Table 3. All outcomes are conditional on household having cultivated land. Observations associated with cultivated land size smaller than 0.007 Rai (bottom 0.5%) are excluded. Direct cultivation costs (2) exclude opportunity cost of labor (3). The opportunity cost of labor is the provincial hourly minimum wage multiplied by the sum of household member's average hours worked times the number of days spent on cultivation activities in a given month.

Table 8: Impacts on Cultivation Portfolio Outcomes

Dependent Variables:	(1) Whether has cash crops	(2) Land frac. cash crops
(A) Baseline	0.015	0.020**
	(0.012)	(0.009)
(B) Baseline: SR & LR Effects		
Short-run Effects	0.008	0.013
	(0.012)	(0.009)
Long-run Effects	0.029*	0.033***
	(0.016)	(0.012)
(C) Village Controls: SR & LR		
Short-run Effects	0.007	0.013
	(0.012)	(0.009)
Long-run Effects	0.027*	0.029**
	(0.016)	(0.012)
(D) High- vs Low-demand: SR & LR		
Short-run Effects	0.006	0.015
	(0.026)	(0.019)
Long-run Effects	0.024	0.049**
	(0.033)	(0.024)
(E) Top 1/3 vs Bottom 2/3: SR & LR		
Short-run Effects	0.018	0.040*
	(0.029)	(0.022)
Long-run Effects	0.040	0.063**
	(0.037)	(0.029)
Number of Observations	44,210	44,210

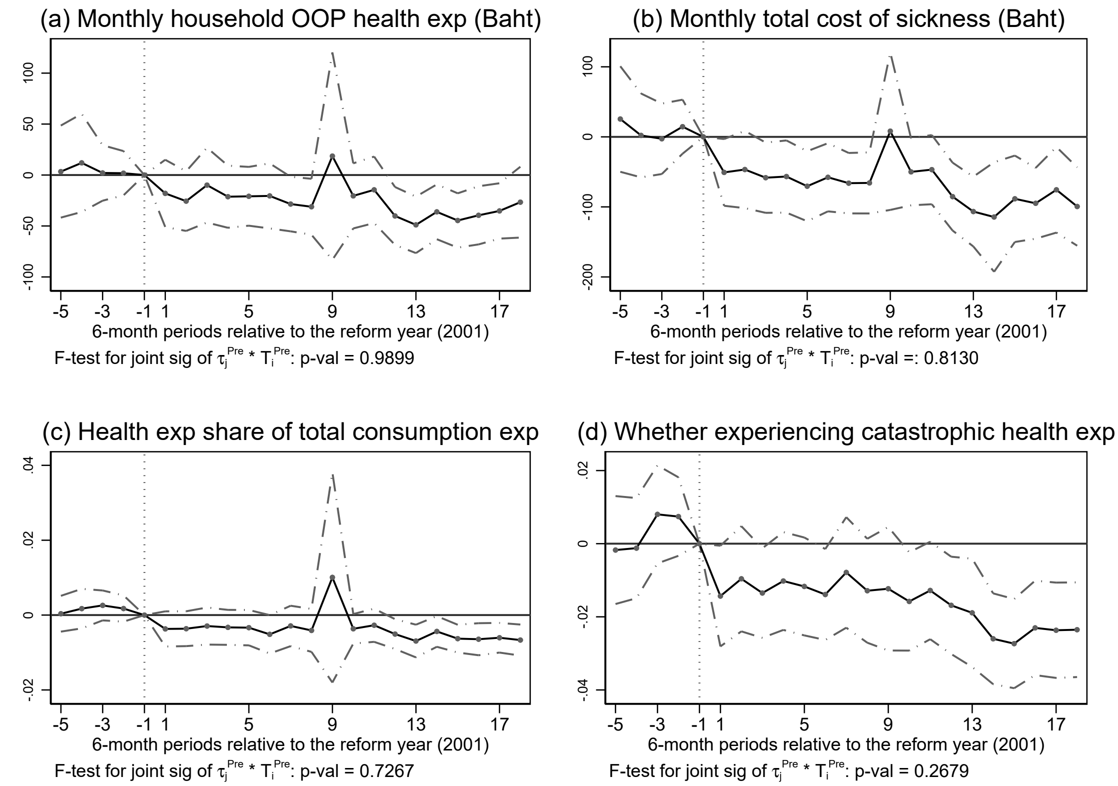
Note: Robust standard errors, clustering on household times $Post_t$, in parentheses: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. The dependent variables are denoted in the column headings and each cell reports estimates from different regressions. All regressions include household and time (in year-month) fixed effects. Included household-level controls are the age of the household head and its squared term, a dummy variable for household head being male, fraction of under-15 kids living in household, fraction of over-60 elderly, and a set of dummies for household size. The base sample consists of households with cultivated land that do not miss more than 3 months of the monthly surveys in the Townsend Thai project within the pre-reform (Sep'98-Dec'00) and the post-reform (Jan'01-Dec'10) periods. Row (A) reports estimates of the interaction between the treatment intensity variable and the post dummy ($T_i^{Pre} \times Post_t$). T_i^{Pre} is the morbidity index generated from the principal component analysis using pre-reform history of household's health conditions. The village controls in (C) is the village size group dummies interacted with time fixed effects. (D) and (E) report the estimates of the interaction between the dummy for high-demand household and $Post_t$ and that between the dummy for households in the top one-thirds ranked by the morbidity index and $Post_t$ respectively. Panel (B), (C), (D) and (E) report the estimates for the interaction of T_i^{Pre} (of high-demand household dummy for (D) and (E)) with the short-run (Jan'01-Oct'06), and with the long-run (Nov'06-Dec'10) post dummy respectively. All outcome variables are conditional on households engaging in cultivation.

Figure 1: Potential Demand for Healthcare & Total Cost of Sickness



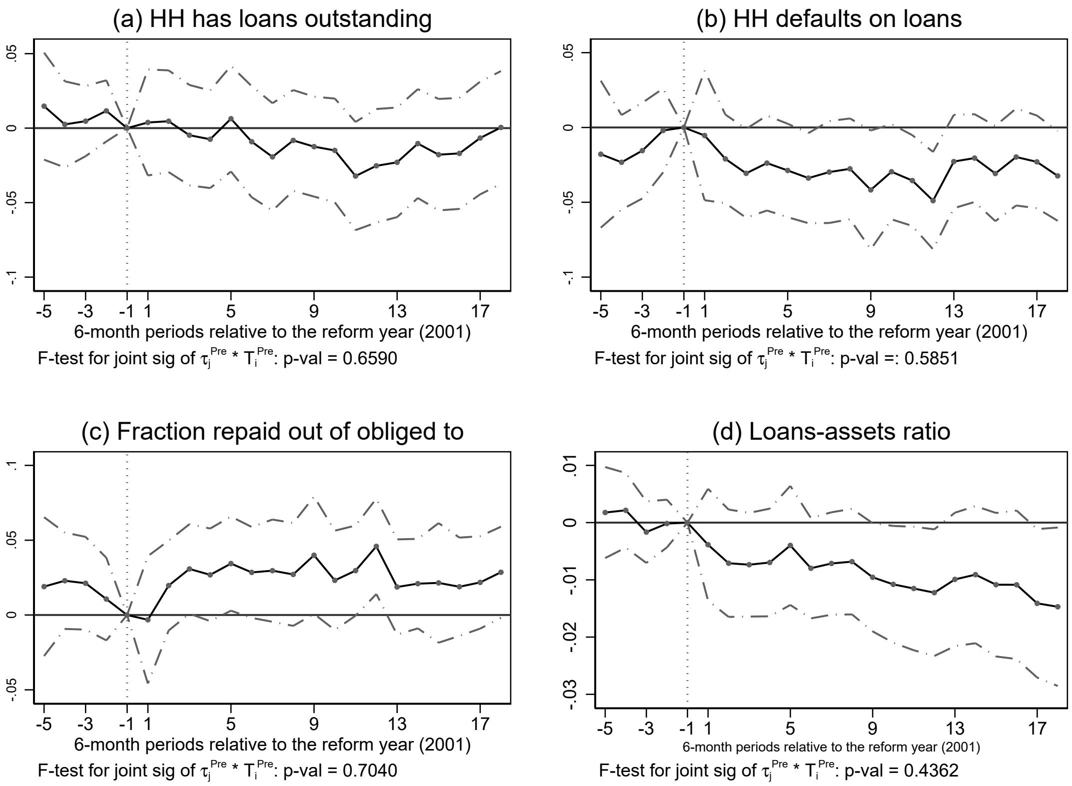
Note: Monthly total cost of sickness is defined as the sum of household out-of-pocket expenditure and the opportunity cost of illness, where household opportunity cost is the sum of individual's loss of days at work resulting from sickness multiplied by his/her average pre-reform earnings. In panel (a), each dot depicts the pre-reform average total cost of sickness across all households in each of the 10 bins ranked by household morbidity index. In panel (b), each dot represents the difference (post- minus pre-) between the average (across households within each bin) of household's total cost of sickness across the pre-reform and post-reform months.

Figure 2: Health Expenditure Outcomes Event Study



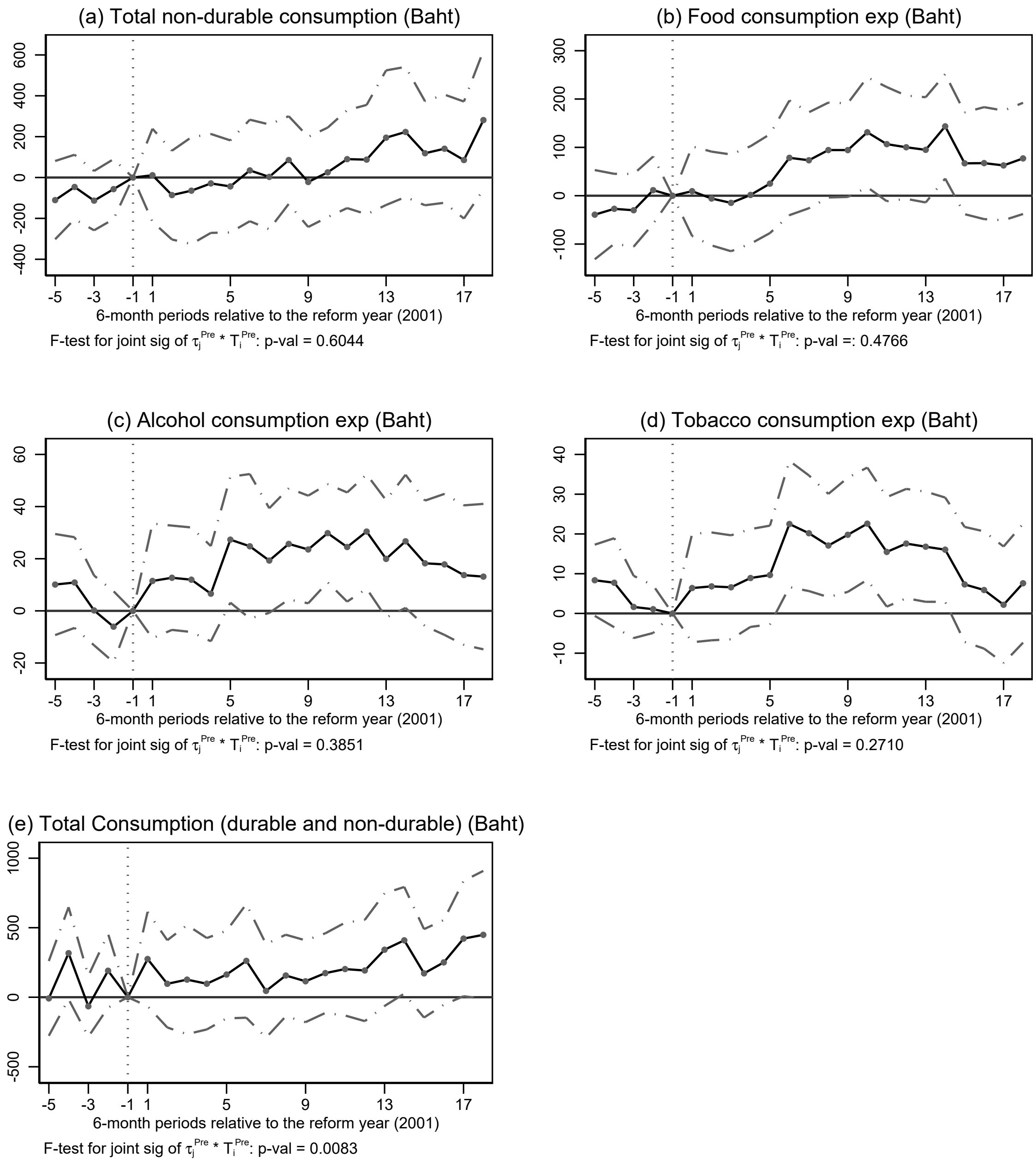
Note: Each graph plots the estimated coefficients of the interaction between half-year period dummies (τ_j) and treatment intensity (T_i^{Pre}) for each half-year period relative to 2001 (the reform year). $\tau_j^{Pre}; j \in \{-5,-4,-3,-2,-1\}$ are the 5 pre-reform half-year period dummies. Note that period -5 only has 4 months (Sep'98-Dec'98). The data span from Sep'98 to Dec'10 and exclude the reform year. Period -1 (Jul'00-Dec'00), the half-year period preceding the reform year, is the base period. The dotted vertical line depicts the end of the pre-reform period.

Figure 3: Borrowings & Transfers Outcomes Event Study



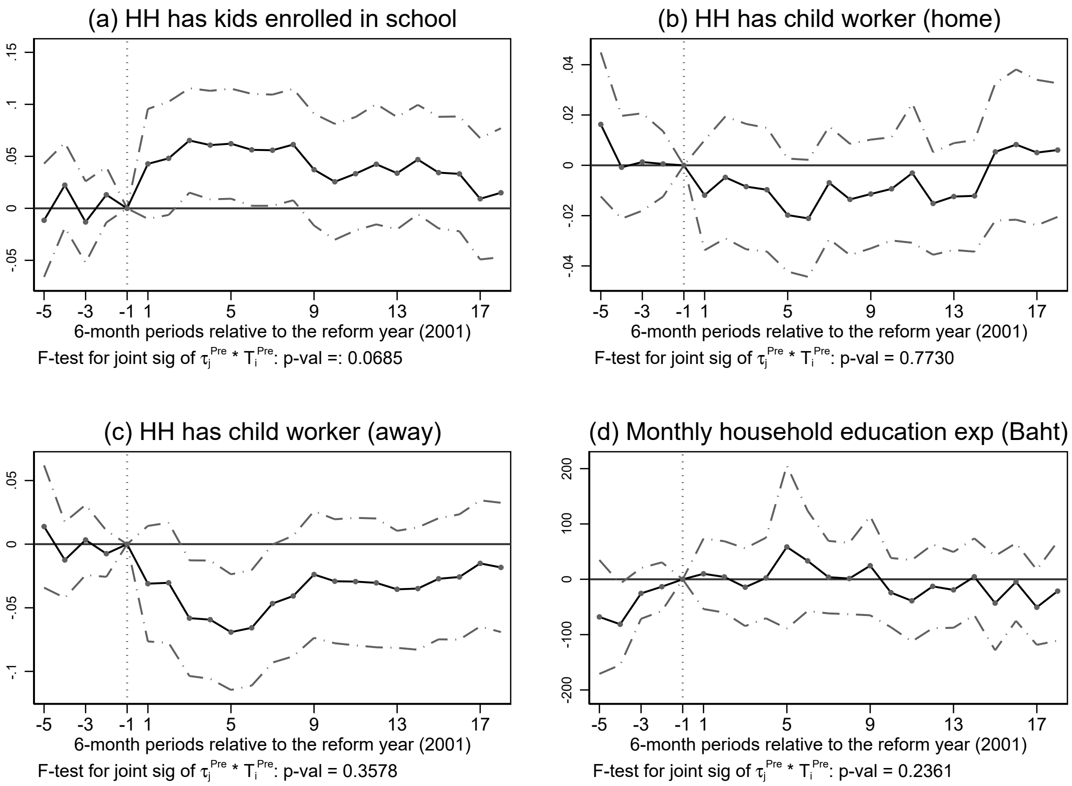
Note: Each graph plots the estimated coefficients of the interaction between half-year period dummies (τ_j) and treatment intensity (T_i^{Pre}) for each half-year period relative to 2001 (the reform year). $\tau_j^{Pre}; j \in \{-5, -4, -3, -2, -1\}$ are the 5 pre-reform half-year period dummies. Note that period -5 only has 4 months (Sep'98-Dec'98). The data span from Sep'98 to Dec'10 and exclude the reform year. Period -1 (Jul'00-Dec'00), the half-year period preceding the reform year, is the base period. The dotted vertical line depicts the end of the pre-reform period.

Figure 4: Consumption Outcomes Event Study



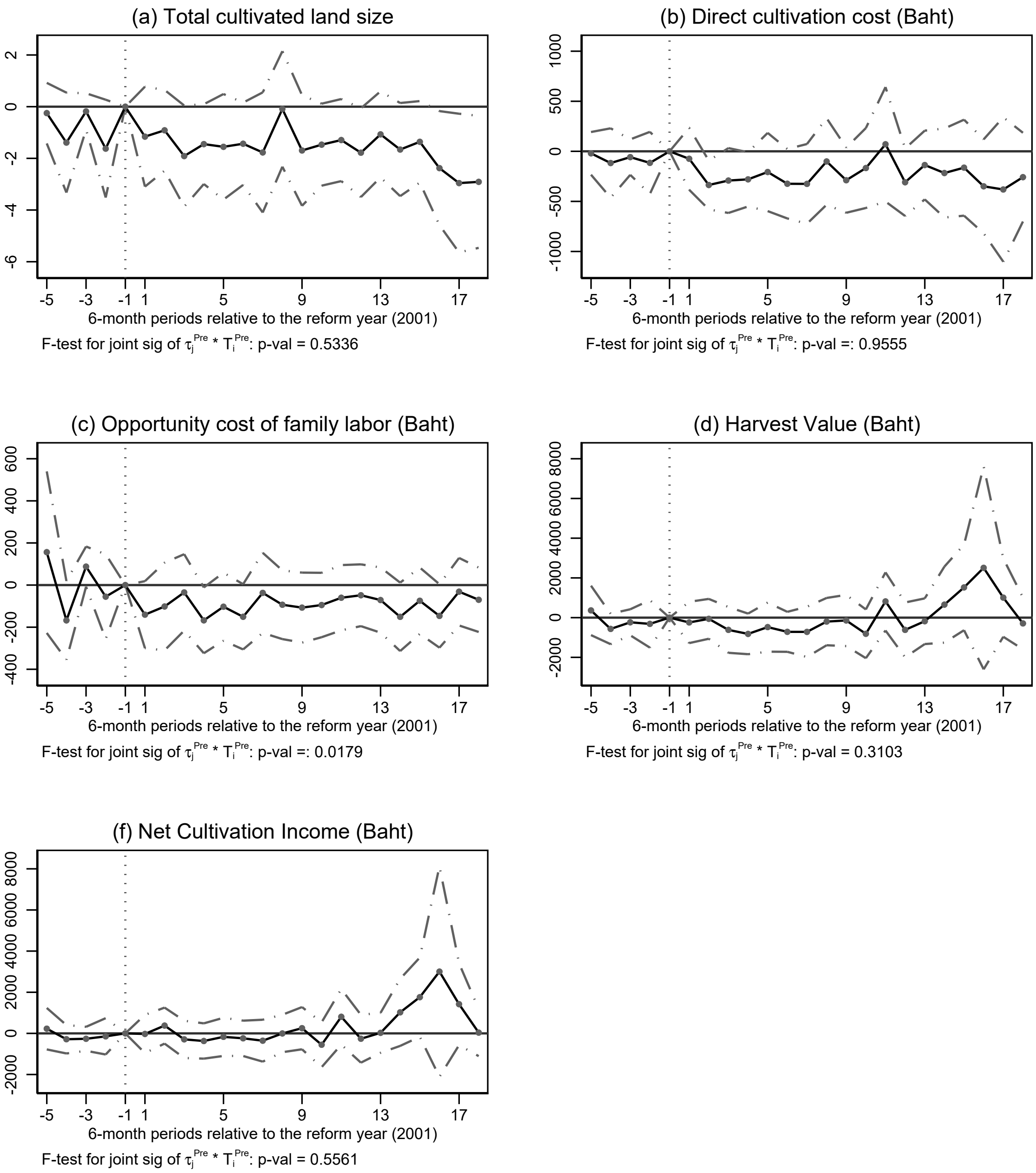
Note: Each graph plots the estimated coefficients of the interaction between half-year period dummies (τ_j) and treatment intensity (T_i^{Pre}) for each half-year period relative to 2001 (the reform year). $\tau_j^{Pre}; j \in \{-5, -4, -3, -2, -1\}$ are the 5 pre-reform half-year period dummies. Note that period -5 only has 4 months (Sep'98-Dec'98). The data span from Sep'98 to Dec'10 and exclude the reform year. Period -1 (Jul'00-Dec'00), the half-year period preceding the reform year, is the base period. The dotted vertical line depicts the end of the pre-reform period.

Figure 5: Education Outcomes Event Study



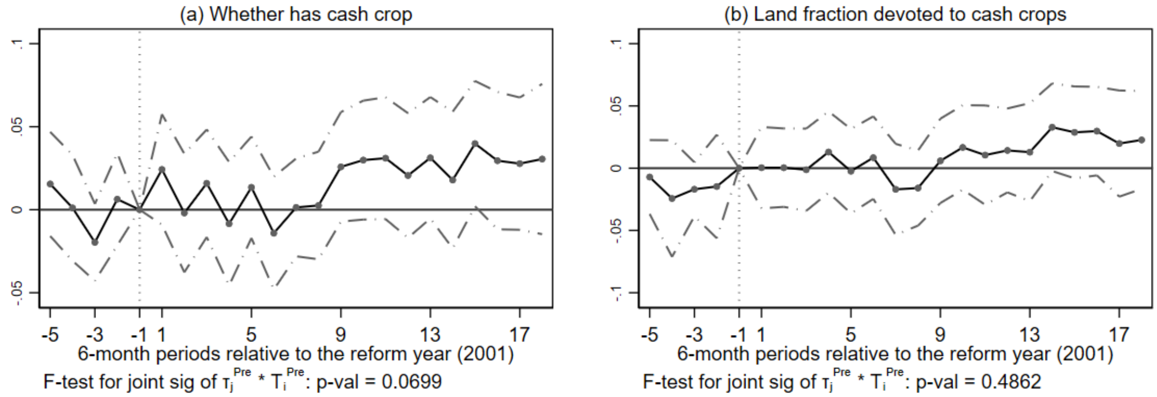
Note: Each graph plots the estimated coefficients of the interaction between half-year period dummies (τ_j) and treatment intensity (T_i^{Pre}) for each half-year period relative to 2001 (the reform year). $\tau_j^{Pre}; j \in \{-5, -4, -3, -2, -1\}$ are the 5 pre-reform half-year period dummies. Note that period -5 only has 4 months (Sep'98-Dec'98). The data span from Sep'98 to Dec'10 and exclude the reform year. Period -1 (Jul'00-Dec'00), the half-year period preceding the reform year, is the base period. The dotted vertical line depicts the end of the pre-reform period. All education outcomes are conditional on households having kids aged 10-18 years.

Figure 6: Cultivation Outcomes Event Study



Note: Each graph plots the estimated coefficients of the interaction between half-year period dummies (τ_j) and treatment intensity (T_i^{Pre}) for each half-year period relative to 2001 (the reform year). $\tau_j^{Pre}; j \in \{-5, -4, -3, -2, -1\}$ are the 5 pre-reform half-year period dummies. Note that period -5 only has 4 months (Sep'98-Dec'98). The data span from Sep'98 to Dec'10 and exclude the reform year. Period -1 (Jul'00-Dec'00), the half-year period preceding the reform year, is the base period. The dotted vertical line depicts the end of the pre-reform period. All production outcomes are conditional on households having cultivated lands.

Figure 7: Risk Outcomes Event Study



Note: Each graph plots the estimated coefficients of the interaction between half-year period dummies (τ_j) and treatment intensity (T_i^{Pre}) for each half-year period relative to 2001 (the reform year). $\tau_j^{Pre}; j \in \{-5, -4, -3, -2, -1\}$ are the 5 pre-reform half-year period dummies. Note that period -5 only has 4 months (Sep'98-Dec'98). The data span from Sep'98 to Dec'10 and exclude the reform year. Period -1 (Jul'00-Dec'00), the half-year period preceding the reform year, is the base period. The dotted vertical line depicts the end of the pre-reform period. All outcomes are conditional on households having cultivated lands.

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Online Appendix (Not for Publication)

A Health Insurance in Thailand & the 30 Baht Reform: Additional Details

Public health facilities in Thailand are governed at provincial and health district levels. Each province has one provincial hospital usually together with at least 1 smaller primary healthcare units in each district. Patients received cares from the local contracted units for primary care (CUPs) or its network that they were registered with, which in most cases were in the patients' area of residence. People in the 30 Baht scheme receive a gold card that permits them to receive treatments in their health district or to be referred for specialist cares elsewhere if required. Included in the package offered by a local provider network are ambulatory treatments, outpatient and inpatient services, maternity benefits, preventive care for health promotion, and prescribed medicines spanning a great proportion of generics (Wagstaff and Manachotphong, 2012; Limwattananon et al., 2015).[85]

Following the reform, Thailand's Public health spending per capita doubled between 2001 and 2010. Coverage by any form of public insurance shot up from 71% in 2001 to 95% in 2003 (Limwattananon et al., 2015). By 2015, National Health Security Office (2015) reported coverage of 99.92%. Out of these, 73.7% were the 30 Baht program recipients with 99.9% of those in the scheme already registered.

Financing the 30 Baht Scheme. The 30 Baht reform have been regarded globally as a major success. Its introduction made considerable adjustments to both financing and the structure of the Thai public healthcare system. Such changes were triggered by the need to balance between providing an effective universal health coverage with a limited budget and controlling government medical expense following such large and rapid expansions (Evans et al., 2012). Consequently various supply-side measures including closed-end capitation, gatekeeper for specialist treatments access, in-advanced payments of inpatient cares for hospitals, and a single purchaser in the system were introduced (Limwattananon et al., 2015).

The main source of funding for the 30 Baht program is from government tax revenues. The National Health Security Office (NHSO) acts as a central purchasing agency who channels funds to local CUPs, of which the annual capitation-based outlay is determined based on the number of registered users (Panpiemras et al., 2011). However, concerns on insufficient funding and poor management in some hospitals, which could lead to compromising service quality and the lack of healthcare staffs, were apparent especially during the early stage (Hughes and Leethongdee, 2007). Despite the new scheme potentially being underfunded, the key to our analysis is that it has brought about significant rises in health budget and different impacts on different households compared to the pre-reform period. At the time of the reform, the annual capitation budget was 1,202 Baht ($\sim$\$29) per registered users. In 2003, the 30 Baht program capitation budget marked a dramatic 35% rise in real terms above the

[85] Renal replacement therapy and heart transplant were not included in the 2001 package, but were later covered in 2008 and 2012 respectively.

corresponding 2001 figures for the superseded MWS and VHCS schemes. Through annual increments, this figure increased to 2,895 Baht (∼\$82) in 2015 (National Health Security Office, 2015).[86]

B Constructing the PCA Morbidity Index

The health module in our data displays a great depth of health-related information for each individual. In each month, we have records of each symptom suffered by each household member along with its duration and severity (whether and how long the symptom prevent the individual from carrying out daily life activities) as well as the initial conditions (month 0 information) on individual self-assessed health status, chronic and disability conditions. Note that only individuals spending at least 15 days over the previous month sleeping in the household were interviewed. Those who migrated away are asked retrospective questions for information since last interviewed when they returned. However, only around less than 2% of all reported symptoms or visits are associated to months during which individuals were away.[87]

We include the following six health measures in the PCA regression where, for each variable, we take the average value over the 28 pre-reform months for each household:

(1) **whether a household member suffered from any symptom or accident over the past month**: The survey asked each household member whether he/she suffered from a broad range of symptoms including headache/dizziness, eye sore, toothache, cough/cold/influenza, fever, diarrhea, nausea/heartburn/abdominal pains, respiratory problem/asthma, rheumatism, infections, skin disorders/scabies/ulcers, chest pains and heart problems. Individuals were asked to specify any other symptoms that were not in the list. Each household member also reported each outpatient and inpatient visit to health facility by reasons. This dummy variable takes the value 1 if in a given month a household member suffered from any symptom or visited an outpatient or inpatient facility due to accident in the previous month.

(2) **the sum of all household members' duration of symptoms in the past month:** The duration of a symptom is calculated as the fraction of days in the past month with the symptom. Since household were not necessarily interviewed on the same date in each month, we divide the total number of days with symptoms by the number of days since they were last interviewed to calculate the fractions of days with symptoms over past month. If an individual reported multiple symptoms, his/her total number of days is then calculated as the sum of all non-redundant number of days suffering from different symptoms.

(3) **whether a household member suffered from any work-limiting symptom:** work-limiting symptoms are those that prevented daily-life routines of households along with hospitalization due to the reported symptoms or accident;

[86]This uses the 2015 exchange rate: 1 dollar = 34.25 Baht.

[87]Individuals also reported detailed information of each inpatient and outpatient visit to healthcare facility including reasons for the visit, types and location of facility, the number of days hospitalized, treatment and medicines costs, and financing methods. Also recorded by reasons for spending are the household-level health-related expenditures on various items including at-the-counter medicines, traditional treatments, and private insurance fees.

(4) **the sum of all members' duration of work-limiting illnesses:** The duration is defined as the fraction of days in past month with the work-limiting illness. If an individual reported being hospitalized due to a reported symptom, we use the number of days hospitalized instead of the number of days the symptoms affected daily-life activities if the former is larger.

(5) **whether any household member had any chronic medical condition;** Examples of chronic health conditions specified in the survey include heart disease, diabetes, asthma, high blood pressure, allergies, and chronic malaria. and

(6) **whether any household member had any disability conditions:** This include any physical or mental disability conditions.

Note that while measures (1) - (3) are variables generated from the monthly data, we only observe measures (5) and (6) in the baseline survey and not in subsequent months. For variable (4), given that the baseline survey also records the number of days over past 12 months that each individual's health made him unable to perform primary activities, we extend (4) to incorporate this previous-12-month information at the baseline by taking a time-weighted average over this and the pre-reform periods. The mean of the sum of all members' fraction of days with work-limiting illnesses is calculated over the extended 40 pre-reform months with the first 12-month period (during which the number of days were reported with 12-month recall period) taking the weight of 12/40 and the latter 28 months taking the weight of 28/40.

Panel (B) of Table 2 displays the statistics for these health conditions variables that are used to construct the PCA morbidity index. The means across all health measure variables reassure that the high-risk households are those with worse pre-reform health conditions.[88] These households experienced more frequent symptoms, were more likely to have members with chronic and disability conditions, and had higher rate of service utilization. These in turn lead to higher standardized morbidity index generated by PCA for the high-risk households.

Our baseline PCA regression uses the average value over the whole 28 pre-reform months of each health measure to generate the morbidity index. We also consider the case in which all monthly health measures are pooled together in the PCA regression to generate a time-varying morbidity index for each household, as well as treating the 12-month retrospective information on the duration of work-limiting symptoms at the baseline (month 0) as a separate additional variable. However, these make no significant difference to our results. In Appendix Figure A2, we illustrate that any pair of these different PCA indices generate scatter plots that produce a pattern that lies quite along the common 45-degree line from the origin.

[88]The high-risk (low-risk) households are those whose morbidity index ranks above (below) the median corresponding to households in the top (bottom) five bins which potentially benefited more (less) from the reform. See Section 3.2 for details.

C Data Appendix

In this Appendix, we describe how each of our main outcome variables are defined and constructed. Unless stated otherwise, the outcomes are generated by the authors using the Monthly Townsend Thai Surveys of rural households data from September 1998 to December 2010. For some variables related to consumption, income, and assets, we use variables from the Monthly Household Financial Accounting dataset that are constructed directly from the Townsend Thai Monthly Surveys.[89] We estimate the impact of HI on various financial well-being, investments and risk taking outcomes. Table 2, A1, and A2 provide the summary statistics of all the control variables and outcomes of interest for our sample of high-risk and low-risk households during the pre-reform period.[90] All monetary variables are in real term and are converted to December 2004 price using monthly and region-specific consumer price index obtained from Thailand's Ministry of Commerce.

Out-of-pocket Health Expenditures. Monthly out-of-pocket health spending is the sum of treatment and medicines costs of each inpatient and outpatient visit due to reported symptoms or accidents of all household members plus other health-related expenditures reported at household level in a given month. Households report other health-related expenditures that are not covered by the reported individual outpatient and inpatient visits in each month. These include over-the-counter medicines, traditional herbs, topical medicines, traditional practitioners, health cards, and insurance premium. We exclude the costs of preventive and prenatal cares, birth giving, and the household-level spending on insurance and health card fees because our health conditions variables are defined based only on reported symptoms and accidents cases. Note that the expenditure for households that did not report visiting any health facility or spend nothing on healthcare is treated as zero.

Total Cost of Sickness Variable. Household's costs of sickness is calculated as household OOP plus the sum of each member's opportunity cost of illnesses. This opportunity cost is measured by individual's loss of days at work resulting from the sickness multiplied by his/her average real earnings during the pre-reform period, which we assume are the earnings that would prevail had the reform not occurred. Individual's monthly earnings are the sum of net income from five possible sources: paid-job(s) outside household, individual's share of household profits from cultivation, livestock, fish and shrimp, and household businesses. Negative profits are treated as zero when calculating the opportunity costs of illnesses. Individual's share of household profits from a household business or

[89] This Financial Accounting dataset is prepared and disseminated by the Research Institute for Policy Evaluation and Design (RIPED) at the University of the Thai Chamber of Commerce. It includes a broad set of financial items in household balance sheet, income statement and cash flow statement such as assets, liabilities, borrowing, and inventories, as well as a number of subcategories of these variables. For more details, see the Townsend Thai Project website: http://townsend-thai.mit.edu/.

[90] For each variable, we report and compare the pre-reform variable means and standard deviations of the low-demand households to those of the high-demand households, where low-demand (high-demand) households are those predicted to have relatively lower (higher) potential demand for healthcare. We use Principal Component Analysis (PCA) which incorporates information on several health conditions of households shown in panel (B) of Table 2 during the pre-reform months to proxy for this potential demand so that high-risk (low-risk) households are those whose morbidity index ranks above (below) the median value. We outline how the PCA is carried out in details in Section 3.1 and Appendix B.

agricultural activity is proportional to the fraction of days the individual spent over the sum of days all household members spent working on that household activity. For individuals or households whose average pre-reform net earnings or income are non-positive, the opportunity cost of illness is assumed to be zero.

OOP Budget Share & Catastrophic Expenditure. The OOP budget share is calculated as the share of monthly OOP health expenditure out of total household expenditures on non-durable consumption. The OOP health expenditure is defined as catastrophic for a household when its OOP budget share exceeds 10%. Both variables help assess household's exposure to risks associated to medical expenditures.

Whether has Outstanding Loans. Dummy variable which is equal to 1 if, in a given month, household has any loan(s) that are not yet fully repaid. Household loans include formal borrowings from financial institutions and village funds as well as informal borrowings from friends and relatives. Account payables such as housing mortgages, selling goods in advance and buying goods on credits are also included.

Whether Household Default on loans. Dummy variable which takes the value of 1, if in a given month in which household reported it was supposed to repay any of its loans in interest or principal, it did not repay anything. The variable is conditional on household being required to repay in a given month. Note that, for each loan held by each household, we observe whether the household had an obligation to repay along with the amount required in each month as well as the amount that was actually repaid by the household.

Fraction Repaid out of the Amount Required. Conditional on household being required to repay in a given month, this is the fraction of the amount actually repaid by the household over the total amount the household was required to repay in that month. Note that this fraction can be greater than 1 in some months where households actually repaid more than what they were supposed to.

Loans-assets Ratio. Loans-assets ratio helps assess the extent of household's leverage analogous to a firm's debt-to-asset ratio. A high ratio indicates households are highly leveraged. Conditional on households having any outstanding loans in a given month, the ratio is calculated as the total outstanding value of loans over the total value of household assets where the value of loans are the value of principal deduct any interests already paid previously. Both the value of loans and assets are obtained from the Financial Accounting dataset, where loans are the sum of household borrowing and account payables while assets include cash, receivables, deposits, lending, inventories, livestock, land, and fixed assets in the household balance sheet. For the results in Appendix Table A7, we also consider alternative definitions of loans-assets ratio, where we replace the evolving value of outstanding loan with the value of principal that is fixed over the loan duration as well as using unconditional ratios instead of the conditional ratio. In such cases, the value of fixed principal is the total principal value of all outstanding loans (not deducting interests paid) held by households in a given month. This comes from the Borrowing module of the Townsend Thai data.

Different Purposes of Loans. In Appendix Table A8, we consider two variables: a dummy variable for whether households took out a particular type of loan and the fractions out of the total value of loans that were used for different purposes. Different purposes for each loan reported by households are grouped into three types: cultivation investments, non-cultivation investments, and consumption expenditures on durable and non-durable goods. Cultivation loans are made to finance raw inputs and tractors, while non-cultivation investment loans are for livestock, farm equipment, business, land, and re-lending.

Risk Coping Strategies. In Appendix Table A9, we consider two strategies households may use to cope with risk associated to medical expenditures: net inflow of private gifts and transfers, and whether household sold their assets or land. The former is obtained directly from household's income statement in the Financial Accounting dataset. Whether household sold assets or land is the dummy variable that equals one if, in a given *year*, household sold one of their assets (fixed agricultural assets and durable goods) or land plots. This comes from the monthly transaction-level data of household expenditures on assets and lands.

Consumption. The consumption module in the dataset collects information on weekly household consumption on 19 different food categories and 7 non-food items such as gasoline, reading materials, lottery and gambling, as well as detailed monthly records of 46 different items on less frequently consumed items including gas, electricity, rents, maintenance, transportation, clothing, education, entertainment, taxes and insurance premium. Our total non-durable consumption variable comes from the Financial Accounting dataset and is the sum of total consumption expenditure and the value of home production on these non-durable goods less health-related expenditures. Food consumption expenditure is the monthly sum of weekly household expenditure paid in cash and credit on the 19 different food categories. We also consider the overall consumption which includes both non-durable consumption and net expenditures on household durable goods, where the net expenditure on household durables are purchases in cash, in goods, or on credit for a range of household items including TV, sofa, air conditioner, refrigerator, telephone, car, motorcycle, truck, boat, etc. These are net of the revenue earned from selling these durable goods in any given month. Alcohol and tobacco consumption is the monthly sum of weekly household expenditure paid in cash and credit on the items as well as the value of home production. Alcohol includes various types of beverages such as beer, wine, whiskey, gin, or local liquors consumed at home and away from home. Tobacco products include cigarettes, tobacco, betelnut, and ya nut (traditional drug).

Education and Child Labor variables. All variables are conditional on households having children aged 10-18 years old in a given month. For robustness, we consider different age groups of children in the Appendix Tables A11 and A10 which report the results for the 7 to 18 and the 10 to 20 age groups respectively.

Whether children were enrolled in school. A dummy variable that equals 1 if the household reports having at least one child at home spending at least 1 day in the previous month attending school or being away for schooling reasons.

Incidence of Child Labor Variables. Child labor refers to children that were not attending school but were working to help finance the households either as home workers (in cultivation, livestock, and household business activities) or outside-household labor (those living at home or away from home who had a job outside the household). We consider two dummy variables: whether the household had child labor working at home and whether the household had child labor working outside the household.

Education Expenditure. Education expenditures are the monthly sum of expenditures on school fees, school equipment, training tuition, pocket money and remittances for children.

Total Cultivated Land Size. Total cultivated land size is the sum of all crop-plots' size in Rai, a measure of unit of land in Thailand.[91] We have a panel of crop plots, which record the details of each crop grown by a household along with all plot operations, input uses, and the associated costs. Around 3 percent of all crop plots have missing plot size. These are plots in which households usually grew crops in an unorganized manner (e.g. scattered around the house, circling a fish pond, along the fence). We exclude these plots from our analysis and only use organized crop plots that households report non-missing plot size to generate all the cultivation outcomes.

Direct Cultivation Cost. The direct costs of cultivation includes the cost of using different types of inputs including seeds, fertilizers, pesticides, hired labor, equipment, animals as well as fixed agricultural assets (such as buildings, tractors) but excluding the opportunity cost of family labor defined below. For each crop plot in each month, we have detailed records of each plot operation (e.g. land preparation, use of itemized inputs, labor tasks) with all associated transaction costs and value of the inputs used in each operation. Total direct cultivation cost is the monthly sum of all these plot-specific operation costs across all the crop plots owned by household plus the flow value of all fixed agricultural assets defined as the depreciation value of household agricultural assets. This depreciation value is obtained from the income statement in the Financial Accounting dataset.

Opportunity Cost of Family Labor. The opportunity cost of family labor reflects the value of household labor and is calculated as the provincial-specific hourly minimum wage rate times the total

[91] Approximately 1 acre is equivalent to 2.52 Rai.

number of hours that all family labors in a household spent on cultivation activities in each month. The hourly minimum wage rate is estimated by the official daily minimum wage rate divided by eight. The minimum wage data come from a series of Notifications on Minimum Wage Rate of Thailand's National Wage Committee.

Harvest Value. Harvest value variable is the cultivation revenue variable obtained directly from the Financial Accounting dataset. It is the total value of all harvests across all crop plots that are produced by household within a given month.

Net Cultivation Revenue. Net cultivation revenue is defined as the total harvest value minus direct cultivation cost and the opportunity cost of family labor.

Portfolio of Cultivation. We focus on two outcome variables: whether the crops grown by households are cash crops, and the fraction of cultivated land devoted to cash crops. The former is the dummy variable which takes the value of 1 if in a given month household own at least one plot that grows a cash crop. The latter is total size of all crop plots that grow cash crop(s) divided by the total size of cultivated land owned by household in a given month. Cash crops are defined based on crop's location-specific average of volatility (within-household standard deviation of the percentage change) in output per Rai over the estimation period. See Section 5.3 for more details of how this volatility in output can be calculated from data.

Household Net Income. Total net income for household is calculated as revenues less costs from 5 household production activities: paid job(s) outside household, cultivation, livestock, fish and shrimp, and household business; net capital gain from production, land, household and financial assets; depreciation of agricultural and business assets; interests; and property and income taxes. Net farming income includes net income from cultivation, livestock, fish and shrimps.

D A Two-period Model of Household Consumption and Investments

To interpret the effects on financial decision making, we consider the rural household solving an two-period decision problem. In period 0, the household has cash-in-hand y and makes a decision about the amount to invest in cash crops (x) of which the return is uncertain, and in a risk free asset, a, with certain gross return R. In period 1, the household receives the gross return on the risk free investment, Ra, and the value of the cash crop harvest. This harvest value depends on the state of the world: In state L, the value is $f_L(x)$, while it is $f_H(x)$ ($> f_L(x)$) in state H (it is also assumed that at the margin, the H-state gives a higher return: $f'_H(x) > f'_L(x) \geq 0$, all x).

In addition to the harvest value risk, which is endogenous through the choice of x, the household also faces a background health risk. With probability π_B, a negative health shock occurs in period 1. It is assumed that medical expenditure required to restore health, Δ, is prioritized so that, if uninsured, the health shock translates into a consumption reduction of equal size.[92] With probability π_G, no health shock occurs. The health expenditure risk is insured at an insurance rate α. Thus in case of a negative health shock the household bears only $(1-\alpha)\Delta$ of the medical expenditure. Expected out-of-pocket health expenditure is then $(1-\alpha)\,\pi_B\Delta$. The actuarial premium, possibly subsidized at rate $1-\lambda$, is $\lambda\alpha\pi_B\Delta$ and paid in the same period. The insurance rate is determined by the government. In the Thai context, the reform consists in raising α from zero to almost unity, at a heavily subsidized premium ($\lambda \ll 1$).[93]

The household has utility function $v(\cdot)$ in period 0 and $u(\cdot)$ in period 1. Both functions are strictly increasing in consumption. The latter function is strictly concave ($u'' < 0$), reflecting risk aversion, and has a strictly convex first derivative ($u''' > 0$), reflecting prudence. The discount factor with period 1 utility is δ. The household then solves

$$\max_{x,a} v(y-x-a)+ \tag{4}$$
$$\delta \sum_{j=L,H} \pi_j[\pi_B u(f_j(x)+Ra-\lambda\alpha\pi_B\Delta-(1-\alpha)\Delta)+\pi_G u(f_j(x)+Ra)-\lambda\alpha\pi_B\Delta]$$

It is assumed that the solution (x^*, a^*) is interior.[94]

After integration over the morbidity index T, the empirical model, provides us with an estimate of

$$Y(\alpha=1,\lambda=0)-Y(\alpha=0,\lambda=0), \tag{5}$$

the effect of the introduction of comprehensive health insurance at a heavily subsidized rate on several outcome variables.

We now present our main results for $Y = x^*$, a^*, and (x^*+a^*). We do this for $\lambda = 0$, assuming that the HI reform is tax financed, with a small share of the burden falling on rural households, but indicate at the end how results change if $\lambda = 1$ (household paying the actuarially fair HI premium). More general results for $\lambda \in [0,1]$, including the effects on a^*, and proofs are available upon request.

It is useful to first take $v'' = 0$ so that intertemporal utility is quasi-linear in period zero consumption. This eliminates any considerations of consumption smoothing between periods, and puts the focus on risk hedging.

Result 1 Assume constant marginal utility in period 0 ($v'' = 0$). Decreasing absolute risk prudence is necessary and sufficient for $\frac{\mathrm{d}x^*}{\mathrm{d}\alpha} > 0$.

Result 2 The effect of an increase in α on total investment (x^*+a^*) is negative.

Risk preferences are said to be prudent if the vNM-utility function has a positive third derivative.

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[92] I.e., we assume the household does not trade-off health care with consumption on other goods.

[93] The model can be extended to a infinite horizon problem with health shocks occurring in every period. It is then assumed that the health shock takes place before the savings/portfolio decisions are made.

[94] The model is related to that studied by Elmendorf and Kimball (2000). They consider the savings *cum* portfolio problem for a consumer whose future period labour earnings are uncertain. The taxation of these earnings reduce the future background risk.

The coefficient of absolute prudence is then defined as $P_a(c) = -\frac{u'''(c)}{u''(c)}$, and decreasing absolute prudence (DAP) means that $P_a'(c) < 0$ (Kimball, 1990). Prudence is necessary for decreasing absolute risk aversion (DARA), a property for which there is ample evidence. Guiso *et al.* (1992) give evidence that labour income uncertainty increases precautionary savings (as prudence suggests) and that this effect is smaller for higher income households (DAP). Experimental support for prudence is given by Noussair *et al.* (2014), Deck and Schlesinger (2014) and Ebert and Wiesen (2011).

For each harvest state, on can define a prudence certainty equivalent which is the certain amount of consumption that gives the same marginal utility as the expected marginal utility over the two health states. It can then be shown that the sign of $\frac{\mathrm{d}x^*}{\mathrm{d}\alpha}$ is governed by the differential compensation (in terms of expected consumption) required to keep the prudence certainty equivalent constant after an increase in α. If the willingness to pay is larger in the low harvest state than in the high harvest state, then the household becomes relatively less vulnerable to the health expenditure risk in the former state and will increase its exposure to risk by investing less in risky cash crops. This happens under DAP and explains Result 1.

Result 2 states that the introduction of HI on the *total* transfer of resources to period 1 is negative. In consequence, HI has a positive effect on the *share* of cash crop investment.

Result 1 holds under absence of consumption smoothing, that is when assuming $v'' = 0$ so that any exogenous increase in period 0 or period 1 cash in hand is absorbed by period 0 consumption. It can also be shown that under DARA the consumption smoothing argument reinforces the effects in Result 1, at least to a first order, while leaving Result 2 unaffected.

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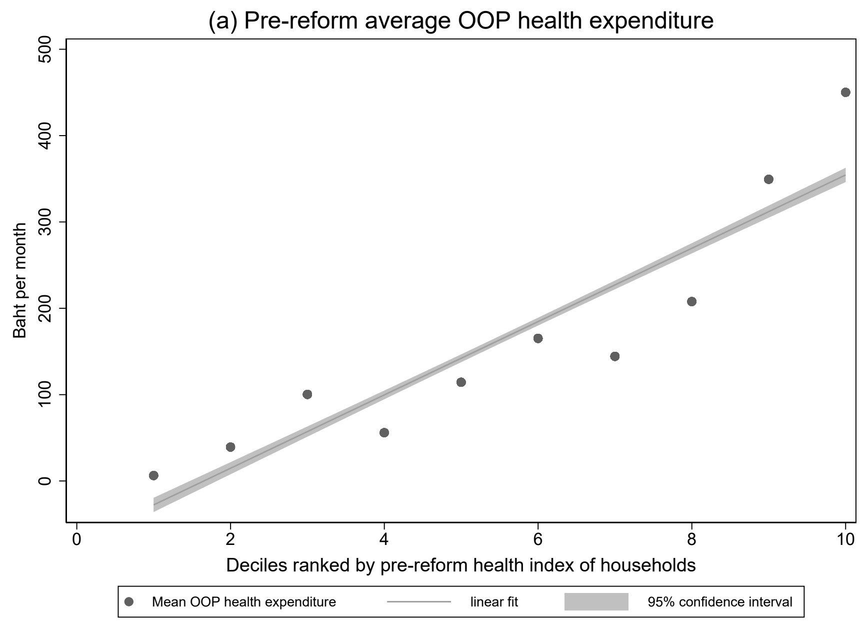
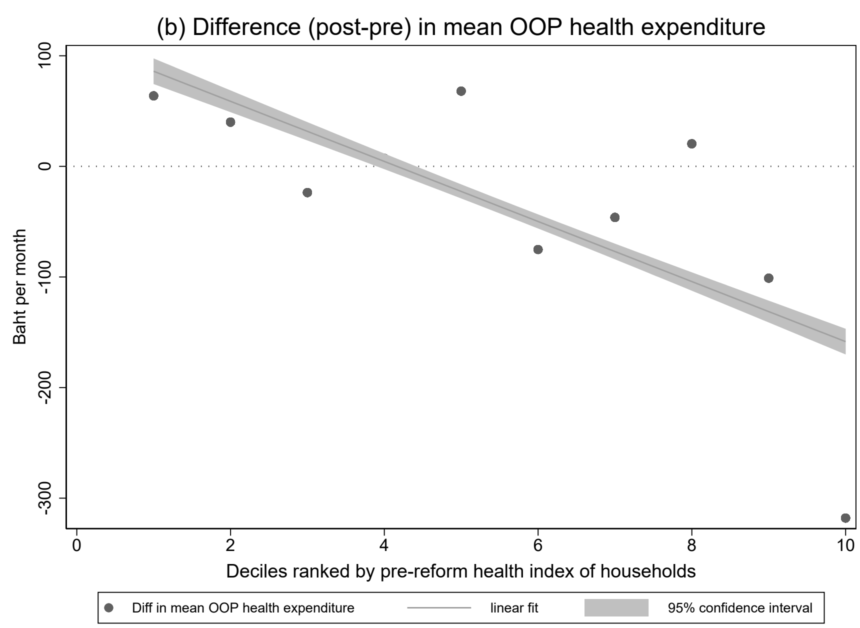
E Appendix Figures

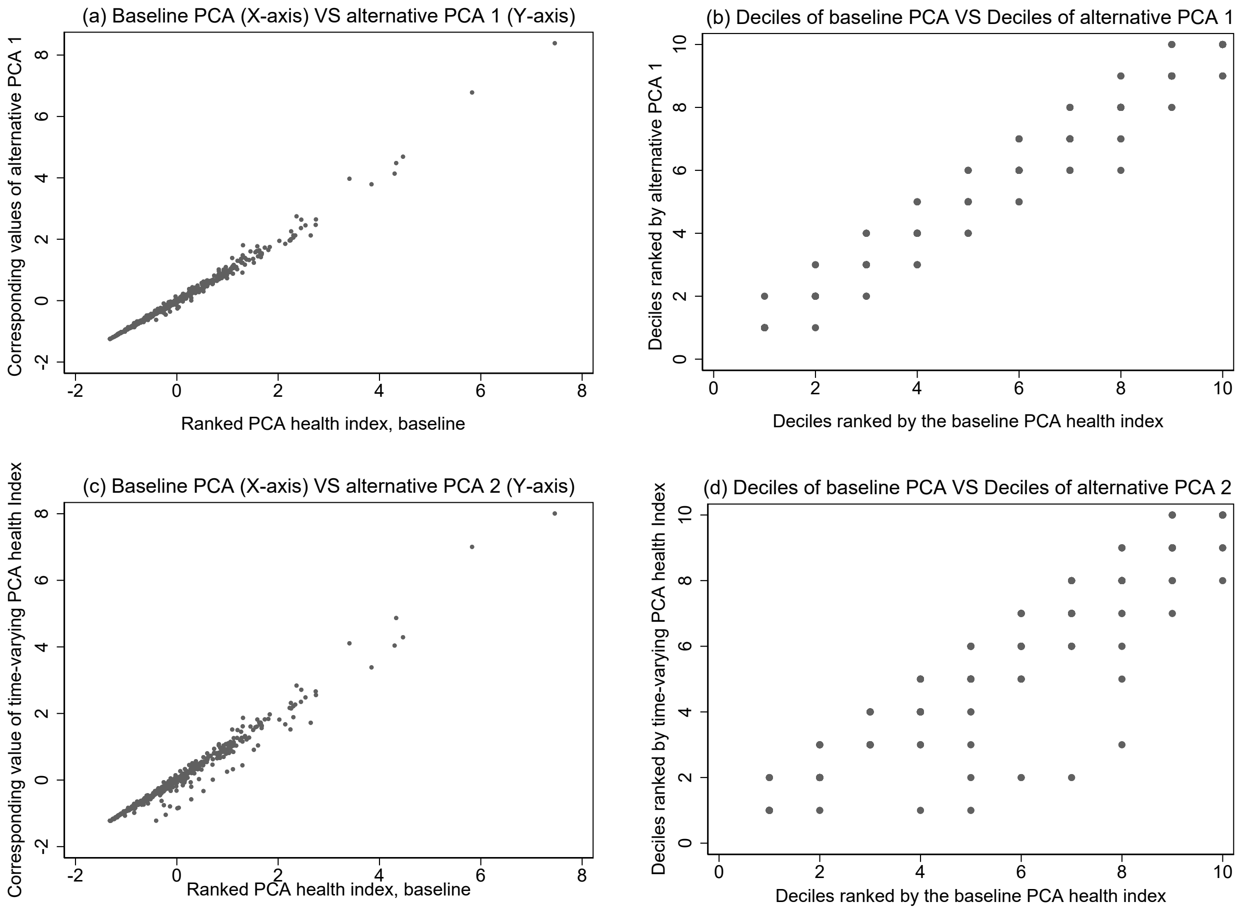
Figure A1: Potential Demand for Healthcare & Out-of-pocket Health Exp





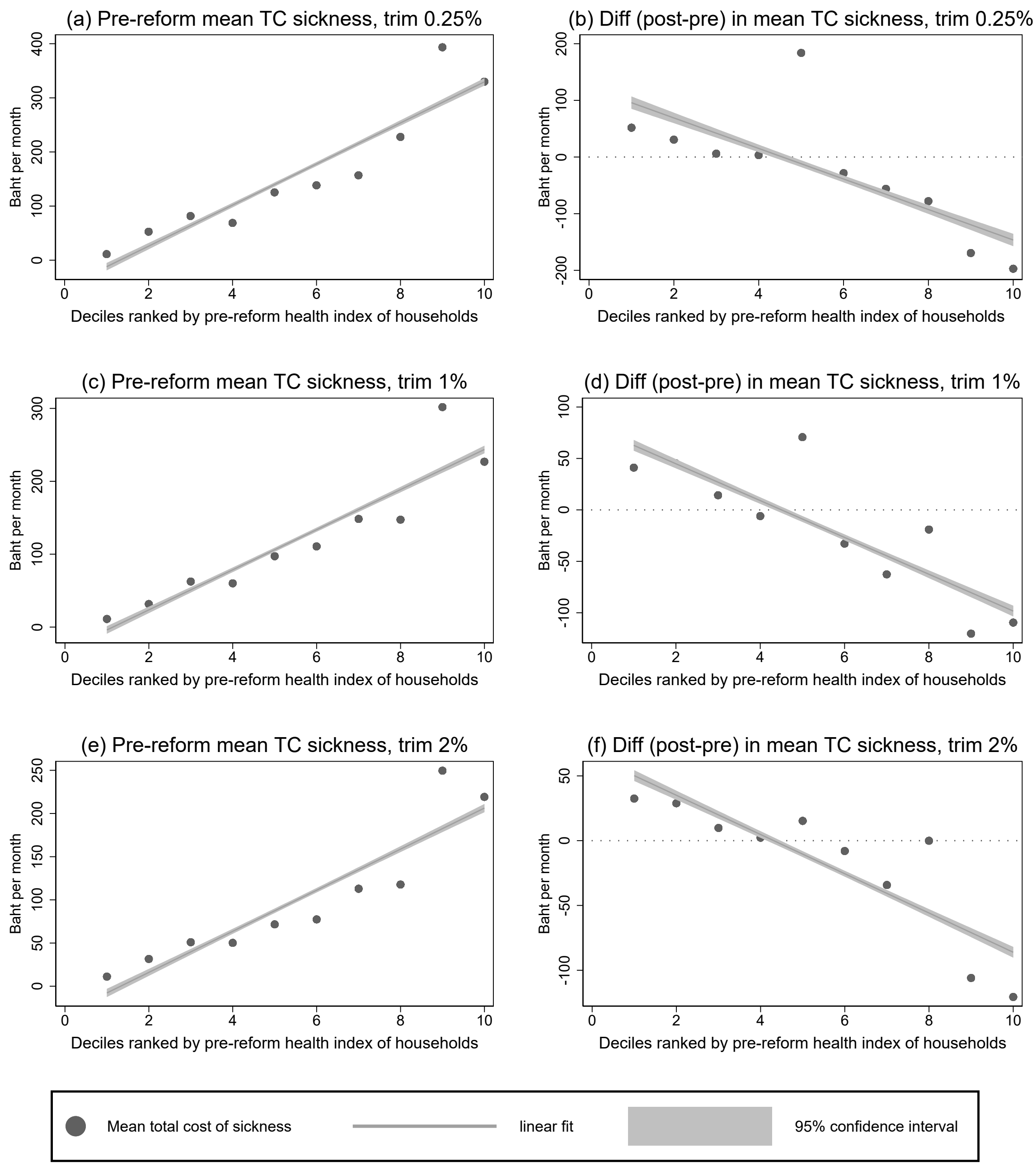
Note: In panel (a), each dot depicts the pre-reform average household out-of-pocket health expenditure across all households in each of the 10 bins ranked by household morbidity index. In panel (b), each dot represents the difference (post-minus pre-) between the average (across households within each bin) of household's out-of-pocket health expenditure across the pre-reform and post-reform months.

Figure A2: Scatter Plots of Baseline PCA index Vs Alternatives



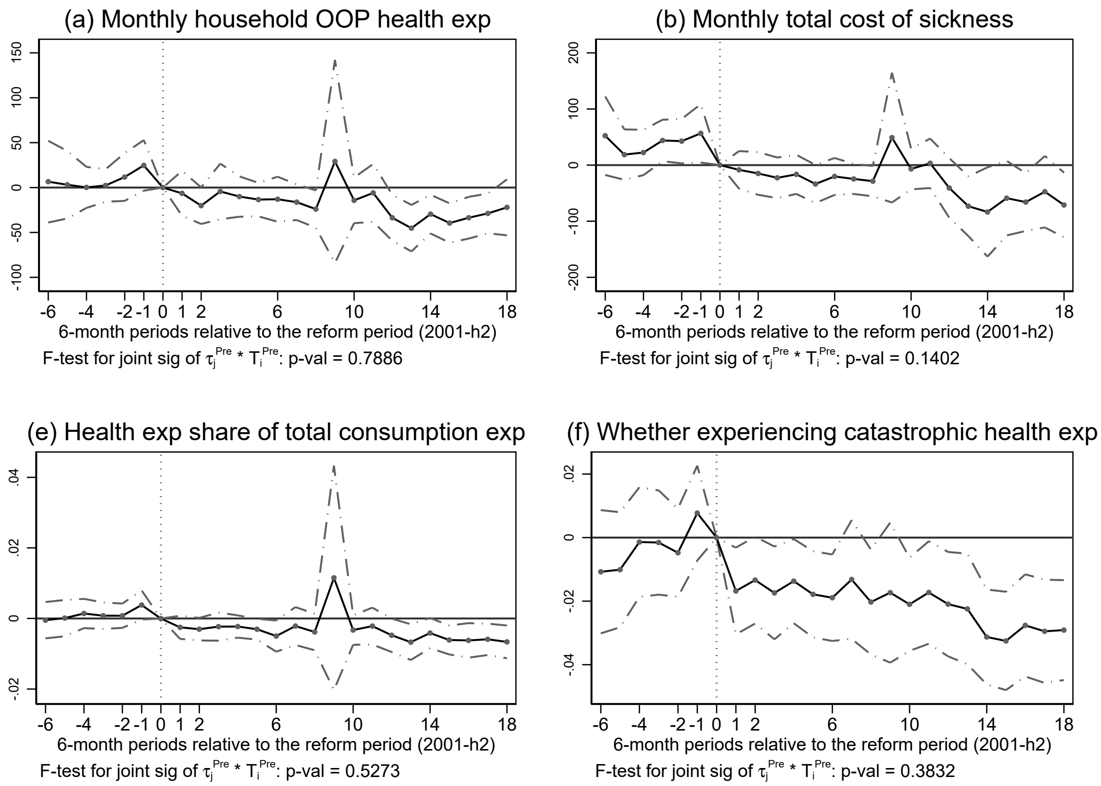
Note: The baseline index is generated from the Principal Component Analysis (PCA) using six health measures as described in Appendix B. These include whether a household member suffered from any symptom, whether a household member had a symptom that prevented daily-life routines, and the duration that household suffered from the symptom(s). For each measure, we take the average value over the 28 pre-reform months within each household. We also include whether a household member had any chronic or disability conditions. For alternative PCA 1 (panel (a) and (b)), the 12-month retrospective information at the baseline on the duration of severe symptoms is treated as an extra variable in the PCA regression. In alternative PCA 2 (panel (a) and (b)), all monthly data points of the six health measures are pooled together to generate a time-varying index. The left panels display the scatter plots of the index values while the right panels illustrate the relationship among different deciles of the indices.

Figure A3: Demand for Healthcare & Total Cost of Sickness, Outlier Robustness



Note: Monthly total cost of sickness is defined as the sum of household out-of-pocket expenditure and the opportunity cost of illness, where household opportunity cost is the sum of individual's loss of days at work resulting from sickness multiplied by his/her average pre-reform earnings. In the left panel, each dot depicts the pre-reform average household total cost of sickness across all households in each of the 10 bins ranked by household morbidity index. In the right panel, each dot represents the difference (post- minus pre-) between the average (across households within each bin) of household's total cost of sickness across the pre-reform and post-reform months. Each row is associated to different levels of top-trimming within the pre- and post-reform period of the total cost of sickness variable.

Figure A4: Health Expenditure Outcomes Event Study (inc. 2001-h2 as base)



Note: Each graph plots the estimated coefficients of the interaction between half-year period dummies (τ_j) and treatment intensity (T_i^{Pre}) for each half-year period relative to the second half of 2001 (the reform year). $\tau_j^{Pre}; j \in \{-6, -5, -4, -3, -2, -1\}$ are the 6 pre-reform half-year period dummies. Note that period -6 only has 4 months (Sep'98-Dec'98). The data span from Sep'98 to Dec'10. Period 0 (Jun'01-Dec'01), the second half of the reform year, is the base period depicted by the dotted vertical line.

F Appendix Tables

Table A1: Summary Statistics, Financial Well-being and Welfare Outcomes (Monthly Pre-reform Data)

	Low-demand	High-demand	Diff.(p-val)
(C) Health Expenditure Outcomes (Monthly Variables)			
Out-of-pocket health expenditure (Baht/month)	65.7	275.6	0.000
	(1,030)	(3,640)	
Total cost of sickness (Baht/month)	83.5	361.7	0.000
	(1,070)	(3,782)	
Consumption budget share of OOP health expense	0.009	0.029	0.000
	(0.045)	(0.083)	
Whether catastrophic health expenditure	0.019	0.075	0.000
	(0.136)	(0.264)	
(D) Borrowing (Monthly Variables)			
Has loan(s) outstanding	0.710	0.611	0.000
	(0.454)	(0.488)	
Whether default on loans†	0.041	0.064	0.014
	(0.198)	(0.246)	
Fraction repaid out of total loans obliged to repay†	0.957	0.937	0.029
	(0.194)	(0.233)	
Loans-assets ratio††	0.114	0.114	0.769
	(0.146)	(0.151)	
Whether sell assets or lands	0.052	0.067	0.000
	(0.221)	(0.249)	
(E) Consumption Expenditure			
Total non-durable consumption (Baht/month)	4,734.5	5,090.7	0.005
	(7,599.6)	(7,470.0)	
Food expenditure (Baht/month)	2,326.7	2,471.9	0.000
	(1,652.6)	(2,134.2)	
Alcohol expenditure (Baht/month)	158.8	159.6	0.943
	(470.0)	(741.3)	
Tobacco expenditure (Baht/month)	86.1	84.6	0.665
	(187.0)	(220.5)	
Total durable & non-durable expenditure (Baht/month)	5,509.4	5,735.0	0.447
	(17,363)	(18,241)	
Number of *pre-reform* observations	7,236	7,205	

Note: The table shows the mean and standard variation (in parentheses) for each variable of interest which are calculated over the 28-month pre-reform period (Sep' 97 - Dec'00). All variables are in monthly format. High-risk (low-risk) households are those whose morbidity index ranks above (below) the median value and thus are predicted to be less (more) healthy and potentially benefited more (less) from the reform. The morbidity index is constructed by the principal component analysis using the pre-reform health condition variables shown in the top six rows of panel (B) of Table 2.

† variables are conditional on households having an obligation to repay their loans in a given month. The number of pre-reform observations for the low- and high-risk group is 1,171 and 1,056 respectively.

†† variable is conditional on household having outstanding loans in a given month. The number of pre-reform observations for the low- and high-risk group is 5,137 and 4,402 respectively.

Table A2: Summary Statistics, Investment in Education and Production Outcomes (Monthly Pre-reform Data)

	Low-demand	High-demand	Diff.(p-val)
(F) Education & Child Labor (aged 10-18)			
Have kids enrolled in school	0.781	0.740	0.000
	(0.414)	(0.439)	
Child labor working at home	0.047	0.066	0.000
	(0.213)	(0.249)	
Child labor working outside household	0.094	0.116	0.002
	(0.292)	(0.321)	
Education expenditure (Baht/month)	558.5	656.1	0.000
	(1,171.4)	(1,135.3)	
Number of *pre-reform* observations	3,924	3,644	
(G) Cultivation & Net Income			
Total cultivated land size (rai)	16.94	23.12	0.000
	(20.69)	(40.69)	
Whether crops grown are cash crops	0.640	0.672	0.001
	(0.480)	(0.470)	
Fraction of cultivated land devoted to cash crops	0.345	0.379	0.000
	(0.418)	(0.423)	
Total cultivation costs (Baht/month)	2,528	3,457	0.000
	(5,533)	(10,054)	
Opportunity costs of family labor (Baht/month)	1,409	1,603	0.001
	(2,592)	(3,090)	
Harvest value (Baht/month)	5,329	7,575	0.000
	(18,556)	(24,809)	
Net cultivation income (Baht/month)	1,393	2,515	0.005
	(16,701)	(22,784)	
Net farming income (Baht/month)	3,150	6,221	0.001
	(22,819)	(57,513)	
Net operating income (Baht/month)	6,131	8,438	0.043
	(41,361)	(67,957)	
Number of *pre-reform* observations	5,071	4,833	

Note: The table shows the mean and standard variation (in parentheses) for each variable of interest which are calculated over the 28-month pre-reform period (Sep' 97 - Dec'00). All variables are in monthly format. High-risk (low-risk) households are those whose morbidity index ranks above (below) the median value and thus are predicted to be less (more) healthy and potentially benefited more (less) from the reform. The morbidity index is constructed by the principal component analysis (PCA) using the health condition variables shown in the top six rows of panel (B) of Table 2 during the pre-reform months. See Section 3.1 for more details.

Table A3: Impacts on Health Expenditure Outcomes, Mean Reversion

Dependent Variables:	(1) OOP health exp	(2) Cost sickness	(3) OOP exp share	(4) Catastrop. exp
(A) First 24-month PCA	-27.93***	-66.90***	-0.005***	-0.018***
	(8.51)	(13.46)	(0.002)	(0.004)
(B) Last 4-pre-reform-month interactions	-30.36***	-50.66***	-0.005***	-0.017***
	(9.65)	(15.13)	(0.002)	(0.005)
(C) First 22-month PCA	-26.41***	-61.29***	-0.005***	-0.018***
	(8.37)	(12.93)	(0.002)	(0.004)
(D) Last 6-pre-reform-month interactions	-21.85**	-26.10	-0.006***	-0.018***
	(9.31)	(18.57)	(0.002)	(0.006)
Number of Observations ((A) and (C))	64,886	64,886	64,886	64,886

Note: Robust standard errors, clustering on household times $Post_t$, in parentheses: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. The dependent variables are denoted in the column headings and each cell reports estimates from different regressions. All dependent variables are constructed based on OOP health expenditure variable trimmed at the top 0.5% within the pre- and post-reform periods. All rows report the estimates of the interaction between the treatment intensity variable and the post dummy ($T_i^{Pre} \times Post_t$). See footnote under Table 3 for included control variables in the baseline. In (A) and (C), treatment intensity comes from the morbidity index generated by the first 24 and 22 months of data respectively, while the whole pre-reform data are used for (B) and (D). Additional controls added in (B) and (D) are the interaction terms of post dummy with month-specific treatment intensity, where we use the last 4 months prior to the reform for (B) and the last 6 months for (D). The number of observations in (B) and (D) are 63,517 and 63,370 respectively. These slight discrepancies from the baseline number of observations are due to missing values of health conditions variables of some households that are used to generate the morbidity index in specific months included as additional interaction terms.

Table A4: Treatment Intensity and the Village Fund Policy

Specifications:	(1) without village controls	(2) with village controls
Treatment Intensity (T_i^{Pre})	-0.030	-0.023
	(0.020)	(0.019)
Number of Observations	50,553	50,553
R-squared	0.072	0.119

Note: Robust standard errors, clustering by household, in parentheses: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. The dependent variable is the dummy variable on whether household held any loan from the Village Fund. The estimated coefficients on treatment intensity (T_i^{Pre}) are reported. T_i^{Pre} is the morbidity index generated from the principal component analysis using pre-reform history of household's health conditions. Both regressions include household and time (in year-month) fixed effects. Included household-level controls (not displayed in the table) are the age of the household head and its squared term, a dummy variable for household head being male, fraction of under-15 kids living in household, fraction of over-60 elderly, and a set of dummies for household size. The village controls included in column (2) are the village size group dummies interacted with the time fixed effects. The sample consists of the previously uninsured households that do not miss more than 3 months of the monthly surveys in the Townsend Thai project within the post-reform (Jan'01-Dec'10) period.

Table A5: Impacts on Log Health Expenses

Dependent Variables:	(1) log(1+x) OOP exp	(2) log(1+x) TC sickness
(A) Baseline	-0.369***	-0.450***
	(0.041)	(0.043)
(B) Short- & Long-run Effects		
Short-run Effects	-0.326***	-0.399***
	(0.044)	(0.046)
Long-run Effects	-0.438***	-0.534***
	(0.048)	(0.051)
(C) With Village Controls	-0.356***	-0.438***
	(0.039)	(0.041)
(D) Binary Treatment		
High- vs Low-demand	-0.571***	-0.682***
	(0.068)	(0.071)
Number of Observations	64,888	64,888

Note: Robust standard errors, clustering on household times $Post_t$, in parentheses: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. The dependent variables are denoted in the column headings and each cell reports estimates from different regressions. For different model specifications, see footnote under Table 3. All dependent variables are constructed based on OOP health expenditure variable trimmed at the top 0.5% within the pre- and post-reform periods.

Table A6: Impacts on Utilization

Dependent Variables:	(1) The number of visits	(2) The number of visits (public)
(A) Baseline	-0.103***	-0.060***
	(0.014)	(0.011)
(B) Short- & Long-run Effects		
Short-run Effects	-0.093***	-0.051***
	(0.015)	(0.011)
Long-run Effects	-0.119***	-0.075***
	(0.017)	(0.014)
(C) With Village Controls	-0.106***	-0.062***
	(0.015)	(0.011)
(D) Binary Treatment		
High- vs Low-demand	-0.159***	-0.094***
	(0.018)	(0.014)
Top 1/3 vs Bottom 2/3	-0.200***	-0.120***
	(0.024)	(0.018)
Number of Observations	64,954	64,959

Note: Robust standard errors, clustering on household times $Post_t$, in parentheses: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. The dependent variables are denoted in the column headings and each cell reports estimates from different regressions. For different model specifications, see footnote under Table 3. Variables are trimmed at the top 0.5%.

Table A7: Impacts on Loans-Assets Ratio, Alternative Definitions

Dependent Variables:	(1) Loans-Assets Ratio (Evolving, Uncond)	(2) Loans-Assets Ratio (Fixed Principal, Uncond)	(3) Loans-Assets Ratio (Fixed, Cond)
(A) Baseline	-0.007**	-0.008**	-0.010**
	(0.003)	(0.004)	(0.005)
(B) Short- & Long-run Effects			
Short-run Effects	-0.005	-0.007*	-0.010*
	(0.003)	(0.004)	(0.005)
Long-run Effects	-0.010**	-0.010**	-0.119*
	(0.004)	(0.005)	(0.007)
(C) With Village Controls	-0.006**	-0.007*	-0.009*
	(0.003)	(0.004)	(0.005)
(D) Binary Treatment			
High- vs Low-demand	-0.015*	-0.015*	-0.019
	(0.008)	(0.009)	(0.013)
Top 1/3 vs Bottom 2/3	-0.015*	-0.0130	-0.024*
	(0.008)	(0.009)	(0.013)
Number of Observations	64,990	64,990	51,478

Note: Robust standard errors, clustering on household times $Post_t$, in parentheses: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. All Loans-assets ratio variables are trimmed at the top 0.5%. (1) is unconditional total outstanding value of loans (interests already paid are deducted) divided by the total value of assets, (2) is the total principle value of loans divided by the total value of assets conditional on households having outstanding loans, and (3) is total outstanding value of loans divided by the total value of assets conditional on having outstanding loans.

Table A8: Impacts on Loan Purposes Outcomes

	Whether has any loans for..			% value of loans for..		
Dependent Variables:	(1) Culti. invest.	(2) Non-culti. invest	(3) consump. exp	(4) Culti. invest.	(5) Non-culti. invest	(6) consump. exp
(A) Baseline	0.021	-0.034**	0.005	0.015	-0.011	0.003
	(0.014)	(0.015)	(0.009)	(0.009)	(0.010)	(0.011)
(B) Short- & Long-run Effects						
Short-run Effects	0.029*	-0.035**	0.002	0.018*	-0.010	0.003
	(0.016)	(0.016)	(0.009)	(0.010)	(0.010)	(0.011)
Long-run Effects	0.007	-0.034*	0.011	0.010	-0.012	0.004
	(0.020)	(0.020)	(0.010)	(0.011)	(0.012)	(0.013)
(C) With Village Control	0.023	-0.039***	0.008	0.017*	-0.017*	0.007
	(0.015)	(0.015)	(0.009)	(0.009)	(0.010)	(0.011)
(D) Binary Treatment						
High- vs Low-demand	0.038	-0.075**	0.026	0.014	-0.019	0.026
	(0.033)	(0.035)	(0.021)	(0.027)	(0.024)	(0.027)
Top 1/3 vs Bottom 2/3	0.109***	-0.063	0.012	0.052**	-0.016	-0.019
	(0.036)	(0.040)	(0.024)	(0.023)	(0.028)	(0.030)
Number of Observations	51,478	51,478	51,478	51,450	51,450	51,450

Note: Robust standard errors, clustering on household times $Post_t$, in parentheses: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. Columns (1) to (3) show results for whether household has any loans that are for different purposes: cultivation, non-cultivation, consumption (including durable and non-durable) expenses respectively. Column (4) to (6) report the fraction of the untrimmed total value of loans for different purposes.

Table A9: Impacts on Risk-coping Strategies

Dependent Variables:	(1) Net private gifts & transfers	(2) Whether selling assets or land
(A) Baseline	-529.4**	-0.012
	(253.7)	(0.007)
(B) Short- & Long-run Effects		
Short-run Effects	-600.0**	-0.018**
	(302.2)	(0.008)
Long-run Effects	-407.7	-0.001
	(442.9)	(0.009)
(C) With Village Controls	-534.0**	-0.013*
	(249.8)	(0.007)
(D) Binary Treatment		
High- vs Low-demand	-917.8*	-0.019
	(518.7)	(0.014)
Top 1/3 vs Bottom 2/3	-863.0	-0.004
	(715.8)	(0.016)
Number of Observations	64,990	64,990

Note: Robust standard errors, clustering on household times $Post_t$, in parentheses: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. The dependent variables are denoted in the column headings and each cell reports estimates from different regressions. For different model specifications, see footnote under Table 3. Net private transfer is the monthly inflow of gifts and private transfers minus outflow.

Table A10: Impacts on Education & Child Labor (aged 10-20) Outcomes

Dependent Variables:	(1) In school	(2) Child labor (home)	(3) Child labor (outside HH)	(4) Education exp
(A) Baseline	0.042**	-0.010	-0.031*	90.41**
	(0.020)	(0.009)	(0.018)	(38.60)
(B) Short- & Long-run Effects				
Short-run Effects	0.053***	-0.015	-0.034*	108.95**
	(0.020)	(0.010)	(0.020)	(43.33)
Long-run Effects	0.028	-0.003	-0.028	66.53*
	(0.023)	(0.012)	(0.022)	(38.01)
(C) With Village Controls	0.039**	-0.008	-0.036*	86.27**
	(0.019)	(0.009)	(0.019)	(40.40)
(D) Binary Treatment				
High- vs Low-demand	0.0371	-0.006	-0.028	60.27
	(0.034)	(0.017)	(0.032)	(56.99)
Top 1/3 vs Bottom 2/3	0.114***	-0.038*	-0.075**	168.75***
	(0.038)	(0.021)	(0.034)	(65.26)
Number of Observations	44,615	44,615	44,615	44,615

Note: Robust standard errors, clustering on household times $Post_t$, in parentheses: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. The dependent variables are denoted in the column headings and each cell reports estimates from different regressions. For different model specifications, see footnote under Table 3. All outcome variables are conditional on household having children aged 10-20 years. Child labor refers to children aged 10-20 years that are not enrolled in school in a given year and are in workforce.

Table A11: Impacts on Education & Child Labor (aged 7-18) Outcomes

Dependent Variables:	(1) In school	(2) Child labor (home)	(3) Child labor (outside HH)	(4) Education exp
(A) Baseline	0.013	-0.006	-0.031**	76.38**
	(0.014)	(0.007)	(0.015)	(34.96)
(B) Short- & Long-run Effects				
Short-run Effects	0.026*	-0.010	-0.038**	93.18**
	(0.015)	(0.008)	(0.016)	(38.87)
Long-run Effects	-0.006	-0.001	-0.022	51.74
	(0.016)	(0.009)	(0.017)	(34.94)
(C) With Village Controls	0.010	-0.006	-0.034**	76.38**
	(0.014)	(0.007)	(0.015)	(36.64)
(D) Binary Treatment				
High- vs Low-demand	0.011	-0.007	-0.024	39.27
	(0.025)	(0.013)	(0.022)	(49.67)
Top 1/3 vs Bottom 2/3	0.079***	-0.015	-0.055**	108.78*
	(0.028)	(0.016)	(0.026)	(58.80)
Number of Observations	47,761	47,761	47,761	47,761

Note: Robust standard errors, clustering on household times $Post_t$, in parentheses: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. The dependent variables are denoted in the column headings and each cell reports estimates from different regressions. For different model specifications, see footnote under Table 3. All outcome variables are conditional on household having children aged 7-18 years. Child labor refers to children aged 7-18 years that are not enrolled in school in a given year and are in workforce.

Table A12: Impacts on Different Types of Cultivation Costs

Dependent Variables:	(1) Seeds	(2) Fertilizers	(3) Pesticides	(4) Hired Labor	(5) Equip.&Assets
(A) Baseline	-18.32	-19.81	-6.39	-42.93	-89.49
	(34.10)	(19.65)	(10.03)	(49.36)	(76.20)
(B) Short- & Long-run Effects					
Short-run Effects	-9.200	-46.27**	-9.11	-30.12	-79.83
	(36.10)	(23.32)	(10.39)	(50.97)	(75.00)
Long-run Effects	-35.14	29.03	-1.37	-66.59	-107.32
	(55.01)	(36.94)	(14.76)	(63.90)	(91.60)
(C) With Village Cont.	-15.57	-23.55	-4.54	-30.02	-67.73
	(34.90)	(20.13)	(10.77)	(49.24)	(59.78)
(D) Binary Treatment					
High- vs Low-demand	95.94	23.74	6.58	136.20	-242.24
	(115.61)	(56.24)	(27.00)	(149.59)	(423.78)
Top 1/3 vs Bottom 2/3	-33.74	-9.14	9.20	-84.70	-874.98
	(136.72)	(59.62)	(30.75)	(171.93)	(697.00)
# of Observations	44,210	44,210	44,210	44,210	44,210

Note: Robust standard errors, clustering on household times $Post_t$, in parentheses: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. The dependent variables are denoted in the column headings and each cell reports estimates from different regressions. For different model specifications, see footnote under Table 3. All outcome variables are conditional on households engaging in cultivation. Observations associated with cultivated land size smaller than 0.007 Rai (bottom 0.5%) are excluded for all outcome variables.

Table A13: Impacts on Opportunity Costs of Cultivation Labor

Dependent Variables:	(1) Cultivation Hrs worked	(2) Hrs worked (age > 18)	(3) Hrs worked (age 10-18)
(A) Baseline	-6.14**	-5.80**	-0.07
	(2.58)	(2.54)	(0.27)
(B) Short- & Long-run Effects			
Short-run Effects	-6.20**	-5.92**	-0.04
	(2.62)	(2.59)	(0.28)
Long-run Effects	-6.05**	-5.58*	-0.13
	(2.96)	(2.88)	(0.31)
(C) With Village Controls	-6.16**	-5.84**	-0.06
	(2.57)	(2.54)	(0.27)
(D) Binary Treatment			
High- vs Low-demand	-9.41*	-8.72*	-0.43
	(5.03)	(4.87)	(0.70)
Top 1/3 vs Bottom 2/3	-10.37*	-9.29	0.04
	(6.05)	(5.86)	(0.77)
Number of Observations	44,210	44,210	44,210

Note: Robust standard errors, clustering on household times $Post_t$, in parentheses: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. The dependent variables are denoted in the column headings and each cell reports estimates from different regressions. For different model specifications, see footnote under Table 3. All outcome variables are conditional on households engaging in cultivation. Observations associated with cultivated land size smaller than 0.007 Rai (bottom 0.5%) are excluded for all outcome variables. The number of hours worked is based on family worker's hours spent on cultivation activities such as land preparation, planting crops, harvesting, etc.

Table A14: Impacts on Household Net Incomes

Dependent Variables:	(1) Net total income	(2) Net farming income
(A) Baseline	579.11	260.75
	(772.10)	(377.30)
(B) Short- & Long-run Effects		
Short-run Effects	532.50	108.38
	(813.52)	(491.98)
Long-run Effects	665.15	542.03
	(884.10)	(706.93)
(C) With Village Controls	449.40	391.74
	(786.61)	(369.94)
(D) Binary Treatment		
High- vs Low-demand	789.25	-1,619.05
	(1,415.14)	(1,756.96)
Top 1/3 vs Bottom 2/3	831.72	1,667.15
	(1,915.19)	(1,517.68)
Number of Observations	44,210	44,210

Note: Robust standard errors, clustering on household times $Post_t$, in parentheses: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. The dependent variables are denoted in the column headings and each cell reports estimates from different regressions. For different model specifications, see footnote under Table 3. Observations associated with cultivated land size smaller than 0.007 Rai (bottom 0.5%) are excluded for all outcome variables.

Table A15: The Predictable Components of Morbidity Index

	(1)
Dependent Variables:	Morbidity Index or Treatment Intensity T_i^{Pre}
Head's age	-0.0414*
	(0.0223)
Head's age squared	0.0004*
	(0.0002)
Male head	-0.0775
	(0.121)
Head & spouse in household	0.0628
	(0.133)
Head's education level (ref category: < 4 yrs):	
exactly 4 years	0.0080
	(0.101)
over 4 years	-0.0198
	(0.150)
Fraction of under-15 kids	-0.0411
	(0.245)
Fraction of over-60 elderly	0.968***
	(0.209)
A member had chronic conditions	0.532***
	(0.0814)
A member had disability conditions	1.569***
	(0.305)
A member was regular smoker	-0.0179
	(0.0807)
Household size (ref. category: 1 member):	
2 members	0.419**
	(0.175)
3 members	0.551***
	(0.199)
4 members	0.538***
	(0.205)
5 members	0.707***
	(0.212)
6 members	0.854***
	(0.271)
7 members	0.819***
	(0.296)
8 members	1.429***
	(0.273)
Constant	-0.0078
	(0.616)
Number of Observations	520
R-squared	0.345

Note: Robust standard errors in parentheses: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. The sample consists of households that do not miss more than 3 months of the monthly surveys in the Townsend Thai project within the pre-reform (Sep'98-Dec'00) and the post-reform (Jan'01-Dec'10) periods, and have available data on initial condition (in the first month) for all the variables used in the model. The morbidity index used as the dependent variable is generated from the principle component analysis using the pre-reform health data. All explanatory variables are valued at their initial condition.

G Appendix Tables on Trimming Robustness

Table A16: Impacts on Out-of-pocket Health Expenditure Outcomes (Untrimmed)

Dependent Variables:	(1) OOP health exp	(2) Cost of sickness	(3) OOP Exp share	(4) Catastrophic Exp
(A) Baseline	-67.12	-109.56**	-0.004**	-0.019***
	(45.09)	(48.06)	(0.002)	(0.004)
(B) Short- & Long-run Effects				
Short-run Effects	-57.94	-92.30*	-0.003	-0.015***
	(44.75)	(47.76)	(0.002)	(0.005)
Long-run Effects	-82.93*	-139.25***	-0.007***	-0.025***
	(48.50)	(52.37)	(0.002)	(0.005)
(C) With Village Controls	-66.88	-108.25**	-0.004*	-0.019***
	(46.83)	(49.76)	(0.002)	(0.005)
(D) Binary Treatment				
High- vs Low-demand	-121.16**	-202.52***	-0.009***	-0.032***
	(61.16)	(72.51)	(0.003)	(0.006)
Top 1/3 vs Bottom 2/3	-137.62	-238.80**	-0.006	-0.029***
	(100.90)	(113.47)	(0.004)	(0.008)
Number of Observations	64,994	64,994	64,994	64,994

Note: Robust standard errors, clustering on household times $Post_t$, in parentheses: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. The dependent variables are denoted in the column headings and each cell reports estimates from different regressions. For different model specifications, see footnote under Table 3. All dependent variables are untrimmed. OOP health expenditure is catastrophic when it accounts for over 10% of household total non-durable expenditure.

Table A17: Impacts on Consumption Outcomes (Untrimmed)

Dependent Variables:	(1) Non-durable consumption	(2) Food exp.	(3) Alcohol consump.	(4) Tobacco consump.	(5) Durables & non-dur. cons.
(A) Baseline	291.5**	157.6***	44.4***	3.8	43.93
	(127.0)	(43.1)	(11.1)	(5.9)	(233.6)
(B) Short- & Long-run Effects					
Short-run Effects	244.8*	155.0***	47.6***	5.1	92.91
	(145.4)	(46.7)	(13.2)	(6.0)	(228.3)
Long-run Effects	371.9**	161.9***	38.9***	1.6	-40.33
	(168.0)	(47.1)	(12.1)	(6.9)	(225.4)
(C) With Village Controls	295.8**	163.2***	46.6***	2.9	30.70
	(131.5)	(44.7)	(12.8)	(6.1)	(229.7)
(D) Binary Treatment					
High- vs Low-demand	98.6	106.8	28.8	0.7	197.7
	(231.8)	(65.9)	(19.8)	(10.9)	(218.7)
Top 1/3 vs Bottom 2/3	129.8	94.6	31.1	-0.3	173.4
	(242.1)	(75.6)	(24.7)	(13.4)	(222.1)
Number of Observations	64,994	64,994	64,804	64,804	64,994

Note: Robust standard errors, clustering on household times $Post_t$, in parentheses: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. The dependent variables are denoted in the column headings and each cell reports estimates from different regressions. For different model specifications, see footnote under Table 3. All outcome variables are not trimmed.

Table A18: Impacts on Cultivation Outcomes (Untrimmed)

Dependent Variables:	(1) Cultivated land size	(2) Cultivation costs	(3) Opp cost of labor	(4) Harvest value
(A) Baseline	-0.904*	-176.4	-108.5**	55.1
	(0.478)	(113.3)	(46.3)	(325.4)
(B) Short- & Long-run Effects				
Short-run Effects	-0.679	-168.3	-111.8**	-332.3
	(0.523)	(113.9)	(46.8)	(340.5)
Long-run Effects	-1.318**	-191.5	-102.4*	770.6
	(0.620)	(169.4)	(52.84)	(536.8)
(C) With Village Controls	-0.861*	-166.2	-108.1**	83.77
	(0.482)	(115.9)	(46.1)	(336.5)
(D) Binary Treatment				
High- vs Low-demand	0.304	59.8	-167.1*	901.3
	(1.036)	(377.6)	(90.4)	(793.7)
Top 1/3 vs Bottom 2/3	-0.905	-362.3	-179.7*	-11.74
	(1.131)	(396.3)	(108.7)	(897.0)
Number of Observations	44,298	44,298	44,298	44,298

Note: Robust standard errors, clustering on household times $Post_t$, in parentheses: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. The dependent variables are denoted in the column headings and each cell reports estimates from different regressions. For different model specifications, see footnote under Table 3. The monthly opportunity cost of labor (3) is calculated as the provincial hourly minimum wage multiplied by the sum of household member's average hours worked times the number of days spent on cultivation activities in a given month. All outcome variables are untrimmed.