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Reference Details

CWPE	2327
Published	20 March 2023
Key Words	Health insurance, Risk-taking, Cultivation, Investments.
JEL Codes	D1, G5, H51, I13, Q12
Website	www.econ.cam.ac.uk/cwpe

Health Insurance and Agricultural Investments: Evidence from Rural Thailand*

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December 13, 2022

Abstract

Exploiting the 2001 universal health insurance reform in Thailand as a source of identification, we estimate the effects of health insurance coverage on agricultural production decisions and welfare. Our estimates suggest that the reform led to long-run increases in total cultivation investments and output, and that households shifted their cultivation portfolio towards riskier crops. We explain these findings using a model of agricultural investment, highlighting the important roles of health insurance in terms of mitigating background medical expenditure risk and improving health. We also find that the reform improved households' welfare by reducing debts and defaults on loans.

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*We would like to thank the participants at several seminars and conferences for helpful comments and discussions. We acknowledge the support and research grant provided by the Cambridge Endowment for Research in Finance. We also thank Ms.Wasinee Juntorn from the Research Institute for Policy Evaluation and Design (RIPED) in Thailand for kind help and support in providing the data. All remaining errors are ours.

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1 Introduction

Providing financial security is a key motivation for policymakers advocating universal health insurance coverage (World Health Organization, 2010). Spending on medical care can be large and uncertain, exposing uninsured households to potential financial distress during periods of illness. In large rural regions of developing countries, households constantly make investment decisions as they engage in agricultural production. Output from agricultural production may be uncertain due to climate and soil conditions and there has been generally a lack of formal insurance against harvest risks.

In this paper we study the effect of public health insurance provision on agricultural decisions and welfare, examining the causal relationships by exploiting a health care reform and providing a theoretical framework to understand the mechanisms at work. The context that we study is rural Thailand, where households engage in cultivation activities and the majority of them did not have health insurance coverage before 2001. In the absence of health insurance, health shocks constitute a major financial background risk – a risk that is hard to avoid. The introduction of universal health insurance in 2001 mitigates this background risk by reducing the variability of out-of-pocket medical expenditure. Since this insurance is financed via a progressive income tax, for many households in rural Thailand the implicit premium is far below the expected reimbursement and therefore the reform also generates an income effect.

We begin by formulating a theoretical framework to examine how an expansion of subsidised health insurance coverage can affect households' agricultural decisions, both in terms of total cultivation investment as well as the portfolio of investments across different types of crops. In our stylised presentation, we assume that there are two types of crops which households can invest in: a risky (cash) crop with an uncertain harvest value, and a riskless crop with a certain harvest value. Our model predicts that, under decreasing absolute risk aversion and decreasing absolute prudence, better health insurance coverage stimulates investment in risky crops. It also predicts a negative effect on total investment. Usually, the latter is related to prudent behaviour—that a mean preserving contraction in background risk reduces the need for precautionary savings—but because the implicit premium is much lower than the expected reimbursement, the income effect will even make an imprudent household to reduce precautionary investment.

To test the predictions from our model, we conduct an empirical assessment of the causal relationships between health insurance coverage, agricultural decisions and welfare. There are two challenges to performing this exercise. The first one is the well-known identification problem due to health insurance coverage being endogenous, for there may be unobserved differences between households with and without insurance.[1] We address the identification problem by exploiting the 2001 universal health insurance reform, known as the '30 Baht' reform. Before the reform, the majority of households in rural Thailand were uninsured, with the rest insured by pre-existing programs with much 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.[2] We classify households into different health risk types by utilising a battery of *pre-reform* health-related symptoms. Relative to low-risk households, high-risk households should benefit more from the reform in terms of a larger reduction in OOP medical expenditure and opportunity cost associated with periods of illness. It is this heterogeneous treatment intensity of the reform that we exploit as the source of identification. We show that the reduction of the 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 tests lending support to this assumption.

The second challenge we face is that datasets that contain detailed information on both agricultural decisions and health are difficult to come by. Agricultural investment decisions often take place at a much higher frequency than the frequency of a typical household survey, and there is also a wide range of time-to-harvest across different types of crops. For instance, while some crops (like rice) can be grown and harvested a couple of times a year, others (like pineapple) can take over a year from planting to harvesting. To overcome this challenge, we use data from the Townsend Thai Project (Townsend, 2016). Its monthly survey contains rich information on health and detailed information on crop-specific inputs and outputs, allowing us to construct a cultivation portfolio for each household and follow it over time. The short recall period (monthly frequency) provides relatively accurate measures of health and investments dynamics compared with annual data used by many previous studies. The panel coverage is long, allowing us to examine the long-term impacts of the reform, and spans a few years before the reform, allowing us to test the validity of our identifying assumption using an event

[1] 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 agricultural production decisions.

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

study analysis.

Our empirical results reveal that the reform led to a differentially larger reduction in the cost of sickness and medical expenditure risk for households that benefited more from the reform (70 Baht per month for one standard deviation increase in the pre-reform health risk). These households then shifted their cultivation portfolio towards riskier cash crops over the long run. Holding fixed total cultivated land, we find differential increases in the probability of investing in cash crops of 5.2 percentage points and in the size of land devoted to growing these risky crops of 0.41 acres. These effects are mainly driven by households with a high level of health risk who benefited the most from the reform. We also find long-run differential rises in total cultivation investments (particularly seeds and fertilisers), output and, ultimately, net income. Our empirical evidence is consistent with the theoretical prediction that risky investment increases following the expansion of health insurance, but at odds with the prediction on total investment. One possible explanation to reconcile this latter difference between the model prediction and data is that health insurance improves the productivity of cultivation investment because of complementarity between this type of investment and health. Using the same identification strategy, we show that the reform indeed differentially improved overall health of individuals in our sample. Therefore, the change in background risk, combined with the improvement in health, can help explain the positive impact on households' overall investments in cultivation.

Given that households that benefited more earned higher net cultivation income, we also assess the reform impact on welfare by examining households' consumption and debts as well as their ability to repay them. Despite no evidence of a significant increase in consumption, our results reveal that households that benefited more experienced differential declines in both the amount of debts and loan default rate. Consistent with Agarwal, Liu, and Souleles (2007), this potentially suggests that the low-income-and-high-debt households in our sample used their extra income to pay down their loans instead of increasing consumption.[3] Altogether, our empirical results suggest that, in addition to providing financial security, public health insurance provision can entail indirect benefits in terms of household production, agricultural investments formation and debt repayments despite the insurance not directly targeting cultivation.

This paper contributes to the literature which studies the effects of insurance on agricultural in-

[3]Almost 80% of households in our sample had loan(s) outstanding during the pre-reform months.

vestments 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 randomised controlled trial (RCT), Cole, Giné, and Vickery (2017) show that innovative rainfall insurance could mitigate uninsured production risk and induces 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 effect, while Cai (2016) illustrates an increase in the production of the insured crop for tobacco farmers from a government weather insurance program. These papers above focus on insurances that directly target one particular type of agricultural production. We complement this strand of literature by showing that health insurance can indirectly alter households' production decisions and cultivation portfolio allocations. Despite not directly affecting cultivation risks like the rainfall or crop insurance, we show that health insurance provision can similarly lead households to undertake more risky production options. A further difference between our paper and the aforementioned ones is that, except for Cai (2016), the crop insurance considered in those papers are evaluated using an RCT. 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.[4] In our case, the 30 Baht reform was implemented by the government; the permanent nature of the reform combined with the long household-level panel data provide sufficient time span for studying the long-term investment responses to insurance provision.

While there is a large literature documenting the effects of health insurance on medical expenditure, consumption and health, little is known about the effects of health insurance on agricultural production.[5] Understanding these relationships is important, particularly in a rural and developing country context, where financial markets are less developed and most households engage in agricultural production, operating effectively as self-managed firms. Existing studies are mostly in the context of developed economies and focus on asset investments and stock portfolio choices. For example, exploiting the US

[4]For example, Cole, Giné, and Vickery (2017) find a take-up rate of 5–10 percent for a rainfall index insurance policy in parts of India. Similar low take-up rates are reported in other experiments (e.g.,Giné, Townsend, and Vickery (2008)).

[5]There is a large empirical literature estimating the impact of public health insurance coverage, combining microdata with credible research designs. See Currie and Madrian (1999), Gruber (2000), and Gertler and Gruber (2002) for a review. Examples of papers that use a particular policy reform in a developing country to analyse health-related outcomes and consumption include Wagstaff et al. (2009), Lei and Lin (2009) and Liu (2016) (China), Wagstaff (2010) (Vietnam), Bauhoff, Hotchkiss, and Smith (2011) (Georgia), Miller, Pinto, and Vera-Hernández (2013)(Colombia) and Garcia-Mandicó, Reichert, and Strupat (2021)(Ghana).

Medicare coverage, health insurance is found to increase the share of risky assets in retirees' portfolio (Goldman and Maestas, 2013), and stock ownership of those with poor health (Angrisani, Atella, and Brunetti, 2018) and with college education (Christelis, Georgarakos, and Sanz-de Galdeano, 2020).[6]

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 utilisation. Gruber, Hendren, and Townsend (2014) show that the reform increased utilisation 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, 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. Instead, we 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 informal counterparts in 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. Section 3 provides a theoretical model illustrating channels through which health insurance can affect households investments. Section 4 describes the data and sample, followed by empirical strategies and econometric models. The estimation results on health expenditure and risks are reported in Section 5, followed by the results and discussions on cultivation decisions in Section 6 and welfare in Section 7. Section 8 concludes.

2 Institutional Background—Thailand's 30 Baht Reform

Thailand's 30 Baht reform implemented in 2001 is one of the most ambitious and largest universal health care reforms by a developing country (Gruber, Hendren, and Townsend, 2014).[7] Funded mainly by

[6]Other examples of papers which find the association between risky asset holdings and health insurance or more protective health care provision include Atella, Brunetti, and Maestas (2012) (10 European countries) and Ayyagari and He (2017) (Medicare Part D).

[7]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.

government tax revenues, it was primarily aimed at alleviating the prolonged geographical inequality in health care provision. The main feature of the reform is that it gave universal access to public health care for a copay of only 30 Baht (∼\$0.73) per visit. It also fixed budget per capita at each provincial public health system at around 1,200 Baht (∼\$29) replacing the pre-existing financing structure. Following the reform, Thailand's public health spending per capita doubled between 2001 and 2010. National Health Security Office (2015) reported national coverage by public insurance of 99.92% by 2015, 73.7% of which were covered by the 30 Baht program.

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 health care units in each district. The National Health Security Office (NHSO) acts as a central purchasing agency who channels funds to local contracted units for primary care (CUPs), of which the annual capitation-based outlay is determined based on the number of registered users (Panpiemras et al., 2011). Patients in the scheme receive a gold card that permits them to receive care from the CUPs that they were registered with, which in most cases were in the patients' area of residence, or to be referred for specialist care elsewhere if required. Through sharp rises in public health spending and effective supply-side measures, the scheme offers 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 (Wagstaff and Manachotphong, 2012; Limwattananon et al., 2015).[8]

Prior to the 30 Baht reform, 65% of households in our data were previously uninsured and had to pay for health care out of pocket (Table 1). The reform provided them with access to public health services for a fixed co-payment as little as 30 Baht per visit.[9] 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.

Relatively poor households were eligible for free health care provided by the Medical Welfare Scheme

[8]Renal replacement therapy and heart transplant were not included in the 2001 package, but were later covered in 2008 and 2012 respectively. For a complete chronology of coverage extension, see Tangcharoensathien et al. (2018).

[9]Beneficiaries are essentially those working in agriculture or unregistered small entities, or are self-employed in the informal sector. They receive a gold card that permits them to receive treatments in their health district or to be referred for specialist care elsewhere if required. Households also had an option to pay an annual premium of 500 Baht (∼\$12.08) for enrollment in Voluntary Health Card Scheme (VHCS) which, combined with government contribution of 1,000 Baht, entitled up to five household members to access free services. The reform merged VHCS into the 30 Baht program. However, only 5% of households in our data reported paying the premium at least once during the pre-reform months.

(MWS) before the reform.[10] In our data, MWS households account for around 21%.[11] The MWS was largely underfunded with an average annual budget per enrollee of only 250 Baht (∼\$6.04) for public hospitals (Gruber, Hendren, and Townsend, 2014). Following the reform, MWS enrollees were rolled over to the 30 Baht program but exempted from contributing the co-payment. The capitation budget for hospitals increased fivefold to over 1,200 Baht (∼\$29) ensuring adequate health care for all.[12] Despite not directly reducing the care price faced by the MWS households, the reform equalised per person funding and significantly improved financial outlay and supply-side measures at public hospitals.

The other two public insurance programs were employment-based and largely unaffected by the reform. Since 1980, the Civil Servant Medical Benefit Scheme (CSMBS) has covered active and retired civil servants and their dependents.[13] Formal sector workers (but not their dependents) have been protected by the Social Security Scheme (SSS) since 1990.[14] Our analysis will exclude CSMBS and SSS households which account for just 9% as most households in our data were in the informal sector.

Financing the 30 Baht Scheme. The main source of funding for the 30 Baht program is from government tax revenues. The reform made considerable adjustments to both financing and the structure of the Thai public health care 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 budgeting, gatekeeper for specialist treatments access, prospective payment system of inpatient cares for hospitals, and a single purchaser in the system were introduced (Limwattananon et al., 2015). Despite the new scheme potentially being underfunded in its early stage, the key to our analysis is that it has brought about significant rises in health budget

[10] 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 the individual has a monthly disposable income below 2,000 Baht (∼\$1.61/day) per person or a household has monthly income below 2,800 Baht (∼\$2.26/day) per household.

[11] 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.

[12] In practice, most contracted providers were public hospitals under the Ministry of Public Health. 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).

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

[14] 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.

and different impacts on different households compared to the pre-reform period.[15] At the time of the reform, the annual capitation budget was 1,202 Baht (~$29) per registered users. In 2003, the 30 Baht program capitation budget marked a dramatic 35% rise in real terms above the corresponding 2001 figures for the superseded MWS and VHCS schemes. Through annual increments, this figure increased to 2,895 Baht (~$70) in 2015 (National Health Security Office, 2015).

Exogeneity of the Reform. The 30 Baht reform was arguably unanticipated and exogenous. It was a key component in the populist election platform of Prime Minister Thaksin Shinawatra, who came into power in February 2001. 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 materialising earlier.[16] 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.[17]

3 Theoretical Framework

In this section, we formulate a model where households make decisions on cultivation investment and consumption under uncertainty.[18] There are two types of uncertainty—health is uncertain which leads to uncertain medical expenditure that reduces consumption; output from cultivation may be uncertain due to climate and soil conditions. In our stylised presentation, we assume that there are two types of

[15]See Hughes and Leethongdee (2007) for further discussions regarding concerns on insufficient funding and poor management in some hospitals, which could lead to compromising service quality and the lack of health care staffs during the early stage of the reform.

[16]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 4.2.2.

[17]The Thai government also launched a large micro-credit program in 2001 injecting a uniform transfer of a million Baht (~$24,178) 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 4.2.2.

[18]The model is related to that studied by Elmendorf and Kimball (2000). They consider the savings *cum* portfolio problem for a consumer facing a future labour earnings risk that is partially insured through an income tax.

crops which households can invest in: a risky (cash) crop with an uncertain harvest value, and a riskless crop with a certain harvest value. Considering crops that differ in their riskiness introduces a portfolio choice problem: for the harvest value any risk can be avoided by investing only in riskless crops. In expectation, however, risky crops are assumed to have a higher return, and this gives an incentive for diversification. Health insurance protects households against the health/medical expenditure risk but not against the harvest risk.[19] The goal of the model is to characterise how much a household is willing to make a trade-off between expected harvest return and harvest risk and to understand how this trade-off is affected by the availability of (subsidised) health insurance.

Consider a household solving an intertemporal decision problem. At the beginning of period 0, the household starts off with cash-on-hand y and decides on the investment in risky (x) and riskless (z) crops. Denoting total investment by $s \stackrel{\text{def}}{=} x + z$, the difference $c_0 = y - s$ is consumed in that period. With probability π_B, a negative health shock occurs in period 1. It is assumed that medical expenditure required to restore health, Δ, is prioritised so that, if uninsured, the health shock translates into a consumption reduction of equal size.[20] 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 subsidised 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 subsidised premium;[21] we therefore take $\lambda = 0$ in what follows.

In period 1, the household also receives harvest values. The harvest value from riskless crop investment is $g(z)$ with $g' > 0 \geq g''$. The harvest value from cash crops depends on the realised 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. We assume that also at the margin, the H-state (L-state) gives a higher (lower) return than crop riskless investment: $f_H'(x) > g'(z) > f_L'(x) \geq 0$, all x, z, and to make the solution non-trivial, that the expected marginal return on cash crops exceeds that on riskless crops. Unlike the health risk, harvest value risk depends

[19] As is common in many developing countries, the public health insurance reform does not include any provision of income support following illness.

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

[21] In Thailand, the health insurance system is financed through a progressive tax. Rural households belong to lower income brackets and pay a small share of the actuarial premium. Thus, as far as the households in the data are concerned, the reform consists in raising α from zero to almost unity, at an ignorable premium.

on the choice of x made in period 0. Harvest values are assumed independent of health.[22]

The household has utility function $v(\cdot)$ in period 0 and $u(\cdot)$ in period 1. Both functions are strictly increasing in consumption and the latter function is strictly concave ($u'' < 0$), reflecting risk aversion, and has a strictly convex first derivative ($u''' > 0$), reflecting prudence. The coefficient of absolute risk aversion is defined as $R^a(c) = -\frac{u''(c)}{u'(c)}$, and decreasing absolute risk aversion (DARA) means that $R^{a\prime}(c) < 0$. Likewise, the coefficient of absolute prudence is defined as $P^a(c) = -\frac{u'''(c)}{u''(c)}$, and decreasing absolute prudence (DAP) means that $P^{a\prime}(c) < 0$ (Kimball, 1990). Prudence is necessary for DARA. Constant relative risk aversion (CRRA) implies constant relative prudence, DARA and DAP;[23] it is a useful empirical benchmark. Experimental support for prudence is given by Noussair, Trautmann, and Van de Kuilen (2014), Deck and Schlesinger (2014) and Ebert and Wiesen (2011). Guiso, Jappelli, and Terlizzese (1992) give evidence that labour income uncertainty increases precautionary savings (as prudence suggests) and that this effect is smaller for higher income households (DAP).

The household chooses the portfolio of crop investment in period 0 by maximising the expected discounted utility from consumption:

$$(s^*, x^*) = \arg\max v(y-s) + \delta \sum_{j=L,H} \pi_j[\pi_B u(f_j(x) + g(s-x) - (1-\alpha)\Delta) + \pi_G u(f_j(x) + g(s-x))], \quad (1)$$

where δ is the discount factor on period 1 utility.

We now present the main results from the model, showing how an marginal increase in the coverage of health insurance can affect the optimal investment in risky crops (x^*) and total investment (s^*) (where both x^* and s^* are interior solutions).

Proposition 1. *If the production function of the riskless crop has constant return to scale (CRS) and household utility in period 1 has decreasing absolute risk aversion and decreasing absolute risk prudence, then* $\frac{dx^*}{d\alpha} > 0$. *If the production function of the riskless crop instead has decreasing return to scale (DRS), then the positive effect on risky investment is attenuated.*

Proof. *See Results 2, 3 and 7 in Appendix A.*

Proposition 1 states that increasing coverage of health insurance increases investment in risky crops,

[22] A background assumption is that the prioritised health care expenditure suffices to restore health. An obvious extension of the model is where health care is a choice variable and that health is not necessarily fully restored, either because this is impossible or too expensive.

[23] CRRA means $u(c) = \frac{c^{1-\rho}-1}{1-\rho}$. Then $R^r(c) = \rho$, $P^r(c) = \rho + 1$, $R^a(c) = \frac{\rho}{c}$, and $P^a(c) = \frac{\rho+1}{c}$.

provided that $u(\cdot)$ in period 1 exhibits DARA and DAP, and decreasing returns to scale in riskless investment is not too strong.

Health insurance raises disposable income in the bad health state. This is an income effect which, to the extent that the household saves, raises total savings (s). The effect of total savings on investment in risky crops is then governed by DARA. This is the standard Arrow effect. Once the extra income in the bad health state is optimally intertemporally allocated according to the Euler equation, the marginal cost of investment is tied down and the remaining effect on risky crop investment is a Frisch effect that is governed by DAP. (Harvest) state contingent future marginal utility is itself random because of the background risk. The expected state contingent marginal utility can be expressed as marginal utility of state contingent expected consumption minus a prudence premium, i.e., of a state contingent certainty equivalent.[24] With DAP, the prudence premium in the high return state is smaller than in the low return state. Since 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 the safe return, the reduced background risk tilts the investment decision in favour of risky crops.

Proposition 2. *Suppose CRS in the production functions of risky and riskless crops. Then* $\frac{ds^*}{d\alpha} < 0$. *If instead the production functions have DRS, then this will strengthen the negative effect on total investment.*

Proof. *See Results 4 and 7 in Appendix A.*

One might be surprised about the absence of prudence as a necessary and sufficient condition for Proposition 2 (CRS). The reason for this absence is that increased insurance coverage lowers expected OOP-payment which is not neutralised by an increased actuarial premium (since the premium is assumed to be heavily subsidised). This creates a real income effect which together with the mean-preserving contraction effect (negative under prudence—see result 5 in Appendix A and Kimball (1990)) results in an unconditional reduction in total investment.

From Propositions 1 and 2 it follows that a marginal expansion of health insurance has a positive effect on the *share* of cash crop investment.

[24]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.

4 Data and Empirical Strategy

4.1 Monthly Survey Data from the Townsend Thai Project

We use data from the Townsend Thai Project's *monthly* surveys of rural households. The data contain a clustered, stratified, random sample of around 45 households from each of 16 villages distributed across four provinces in Thailand, totalling approximately 720 households.[25] Starting from August 1998, the surveys provide monthly information on a rich set of household- and/or individual-level information, including demographics, consumption, health, borrowing, income, agricultural activities and investments.[26]

The monthly survey from the Townsend Thai project consists of 24 different modules. Each module begins with a baseline interview in the first month, collecting information about initial conditions, and continues to collect 'monthly updates' in subsequent months along with a separate 'form' dataset, documenting detailed month-to-month changes. For instance, in the cultivation module, when a household starts growing a new crop, a 'New Crop Form' was issued to record the name and details of that crop including the land used (crop-plot). In subsequent months, a 'Crop-Plot Operation Form' then collects crop-plot level information of harvests, operations, inputs and the associated transaction-level itemised costs and revenues. This continues until that crop-plot is marked finished in the 'Monthly Crop-Plot Updates', which record general household-level changes in cultivation activities. Effectively, these allow us to construct a monthly panel at the crop-plot level (about 20,110 crop-plots), from which we can build an entire cultivation portfolio for each household and trace it overtime. As we illustrate in the Appendix Figure A1, the *duration* of crop-plot—the number of months from when households start planting the crop until it is finished—and thus the frequency of inputs used and outputs harvested by households in our sample, can vary significantly. For example, 92% of our crop-plots have the duration varying between one to twelve months. The average duration of a crop-plot for rice, maize, or sorghum is about 5–6 months, but that of mango, banana, or perennial trees (about 0.7% of our crop-plots) can span the whole of our estimation window (148 months). Therefore, inputs and outputs in a given

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

[26] For further details of the Townsend Thai project, see Samphantharak and Townsend (2010) and Townsend (2016), or the Townsend Thai Project website: http://townsend-thai.mit.edu/. 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.

year differ greatly across households, with more frequent input expenditures than harvests on average (about 4.6 against 3.5 months with positive value). This makes our monthly cultivation data attractive to study agricultural decisions relative to a typical household survey collected annually.

In terms of health, the baseline survey collects retrospective information of past 12-month illnesses and treatments, as well as current chronic conditions and mental/physical disabilities. The monthly updates then provide monthly records of all symptoms suffered by each household member together with their duration and severity. For each inpatient and outpatient visit, we have detailed information on reasons for the visit, types and location of health facilities, the number of days hospitalised, treatments and medicines costs as well as financing methods for each household member. Also recorded at household level by reasons for health spending are expenditures on over-the-counter medicines, traditional treatments and private insurance fees. Therefore, the combined information on health and agricultural activities provides us a unique opportunity to study health insurance and investment, which are difficult to come by particularly in the context of developing countries.

In sum, the short recall period of our data can generate more accurate measures of health and investments when compared with other surveys with relatively long recall periods. Another advantage of the data is that it spans across a few years before and several years after the 30-Baht health reform, allowing us to analyse both the short- and long-term effects of the reform and to test the identifying assumption of empirical strategy. Despite being a long panel, attrition rates are low. The resurvey rate in our data has been over 99% in each year and 602 households out of the 720 original ones in 1998 were still being re-interviewed in 2014 (Townsend, 2016). Finally, the Townsend Thai project is funded independently of the 30 Baht program, limiting respondents' incentives to misreport in response to the 30 Baht program.

Sample Selection. Our estimation window ranges 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. We do not use data for 2001 in our main analysis because of possible anticipatory effects of the reform (See Section 4.2 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 factors from the implementation of government's crop insurance program in 2011.[27]

We 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] Altogether, these sample selection procedures remove 68 or around 9% of all households from the sample. Our final sample consists a panel of 551 rural households spanning 136 months. Of these, 414 households with pre-reform average cultivated land size of above one acre were used for analysing cultivation decisions.

4.2 Empirical Strategy

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 total cost of illness and thus benefited more from the reform relative to households with low health risk.[29] Such heterogeneity among the previously uninsured households facilitates a treatment-control strategy in that households that benefited more were 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

[27]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/she became ill or injured either at 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.

[29]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 Appendix Section D.1 for details of how the variable is constructed.

in spirit to that of Bleakley (2007, 2010), Bütikofer and Salvanes (2020) and Adhvaryu et al. (2020).

Unlike Gruber, Hendren, and Townsend (2014) and Limwattananon et al. (2015), we choose not to compare the previously uninsured with the (excluded) CSMBS and SSS households who were arguably unaffected by the reform using a difference-in-difference design. Our sample includes rural households extensively engaged in informal activities; only less than 10% of households would form the control group using their classification. Moreover, CSMBS and SSS households are quite distinct from the previously uninsured.[30]

4.2.1 Predicting Health Risk of Households

We use the history of individual health conditions to proxy for households' health risk or treatment intensity. We combine multiple health measures richly observed in our data into a parsimonious single index of health using first component of principal components analysis (PCA) as in Blundell et al. (2020).[31] In predicting the health risk, we use individual information on health visits and symptoms before the reform to generate six health variables specific to each household for 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, along with the duration of the symptom(s). 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.[32] 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).

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

[30]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.

[31]Blundell et al. (2020) estimate the effects of health on employment using PCA and factor analysis. They find that both methods give similar results. Our health variables correspond to the objective health measures in the literature which mainly are reported symptoms and diagnosed health conditions. 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.

[32]This makes our resulting 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.

for health care. As a higher value of our health index indicates worse pre-reform health conditions, we refer to it interchangeably as the 'morbidity' index. A higher morbidity index also implies higher health risk and household's potential health care demand. Appendix B provides full details and discussions of the variables used in the PCA regressions. It also demonstrates that our resulting indices are not significantly different when different combinations of variables are used or when all pre-reform monthly health measures are pooled together in the PCA regression to generate a time-varying index.

One potential concern in estimating the reform impact with this index is that high- and low-morbidity households could be different along many dimensions, particularly in terms of age and the number of elderly members in the household. Although we always control for age in our econometric model, the identification of the reform effect would be cleaner if the variation we exploit comes from households of similar ages who differed in initial morbidity. To this end, we adjust for the age composition of households by regressing our morbidity index on age of household head and its squared, fraction of over-60 elderly, fraction of under-15 children and household size evaluated in the last month prior to the reform.[33] We then generate a *age- and composition-adjusted* morbidity index, by adding the predicted residual to the predicted mean morbidity index holding the age of household head fixed at 40, fraction of elderly at 0.17, fraction of kids at 0.25, and household size at 4. This predicted adjusted-index scores are then standardised to have mean zero and standard deviation of one.[34]

Table 2 presents the summary statistics for household characteristics, health conditions and expenditures variables in the pre-reform period separately for households whose health risk ranks above and below the median of the morbidity index. With the composition-adjusted morbidity index, we see in panel (A) that above-median and below-median households are quite similar in terms of household size, the number of children and elderly member as well as the age and gender of household head. This further supports that our adjusted-morbidity index is not systematically correlated with household demographics. In terms of health, the above-median households unsurprisingly experienced much larger OOP health expenditure and total cost of sickness due to the fact that they had more symptoms,

[33] We provide the regression result in Appendix Table A2.

[34] Alternatively, we could normalise the index to make it range between 0 and 1. It is worth noting that both standardising and normalising only affect the scales of the index as the rank ordering of the observations and their relative distances are preserved. The difference, however, lies in the interpretation of the estimated impact of health insurance that we will estimate. With standardised index, our estimated coefficients show the effect on an increase of one standard deviation—a typical increase in the index which corresponds to about an inter-quartile range in our data. Had we normalised the index instead, the coefficient would then show the effect of going from the lowest value to the highest value of the predicted index.

chronic and disability conditions, which in turn contribute to a higher morbidity index predicted by PCA.[35]

4.2.2 Econometric Model

We identify the effect of health insurance 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. Our baseline regression model is:

$$Y_{it} = \beta(M_i \times Post_t) + X_{it}\Gamma + \delta_i + \delta_t + \epsilon_{it}. \tag{2}$$

where i indicates households and t indexes the month, and M_i is the (age- and composition-adjusted) morbidity index. We call the variable M_i '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. The parameter of interest β captures the effect of publicly provided health insurance on the outcome of interest Y_{it}. The specification includes year–month fixed effects δ_t, which absorb economy-wide changes in outcome variables or overall time trend, and the household fixed effects δ_i, which control 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, the fraction of under 15 years children living in the household, the fraction of over 60 years elderly and a set of dummies for household size. To allow for serial correlation in outcomes within households, standard errors are clustered at the household level.[36]

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 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

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

[36]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 realised, households could have started anticipating the reform and thereby changing their behaviours in response. We thus exclude all months in 2001 from the estimation in our main results. In Appendix F, we provide results including the 2001 data for all health expenditure outcomes using event-study diagrams described below.

outcomes in the absence of the reform. Despite the inclusion of time-varying household controls, 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. To this end, we also estimate a more flexible event-study specification:

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

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).[37] 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. 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. The event-study specification also allows us to estimate the dynamic effects of the reform. Household responses to the health reform could either be short-lived, persistent, or take some time to materialise. In addition, we also consider another specification that splits the interaction term in specification (2) into two terms corresponding to the short- (Jan'02–Dec'06) and long-run (Jan'07–Dec'10) effects.

Discrete Risk Types Our baseline specifications assumes a linear relationship between the treatment intensity and the outcome of interest. As the reform effects for households with the highest health risk can differ greatly from those with lower risk, we also use a 'discrete risk types' specification which splits our households into three risk types based on the morbidity index deciles:

$$Y_{it} = \beta^h(D_i^h \times Post_t) + \beta^m(D_i^m \times Post_t) + X_{it}\Gamma + \delta_i + \delta_t + \epsilon_{it}. \quad (4)$$

We replace the regressor $M_i \times Post_t$ in specification (2) with two interaction terms: $D_i^h \times Post_t$ and $D_i^m \times Post_t$, where D_i^h and D_i^m are dummy variables indicating the top and middle bins in the distribution of the morbidity index M_i. We define low-risk households (reference category) as

[37] 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.

those whose adjusted morbidity index ranks in the bottom four deciles, while medium- and high-risk households have their index in the middle and top three deciles, respectively. In this case, the parameter of interest then measures the effect of the reform on the households with high and medium 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 three categories, thereby making it less sensitive to outliers. It also allows us to test whether our results are driven by households in the very top bins that potentially demanded health cares 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 health insurance (the reform) with a one-unit (standard deviation) increase in the morbidity index M_i on various outcome variables Y_{it}. For example, because the interquartile range of the standardised morbidity index is 1.05, 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 health insurance 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 health insurance. 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 health insurance is zero for households with morbidity index $M_i = M_{\min}$; namely, health insurance has no effect on households with the smallest health risk. Under this assumption, combined with the functional form assumption that the effect of universal health insurance is linear in the morbidity index, the estimated ATE is the estimated effect β roughly scaled up by a third. See Appendix C for the full proof.

As illustrated in Section 3, health insurance can affect households' investment decisions through several channels. Our ATE of interest measures the combined effects of health insurance operating through all these channels. This estimated overall impact is an important policy parameter for policymakers.

Mean Reversion. One 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. For instance, even in the absence of the reform, households affected by a transitory 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 use the event-study specification to test for possible mean reversion resulting from any transitory shock prior to the reform (by checking whether there is a significant dip (or spike) in the period just before the reform).

To further ensure that mean reversion is not driving our results, 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).[38] 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.

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 (∼$24,178) into around 77,000 villages across Thailand, and every village in our sample received the fund by the beginning of 2002.[39] 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.

[38]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 excludes these conditions.

[39]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.

Although it is impossible to completely rule out the effects from the other reform, we have done several tests to demonstrate that our results are not driven by the Village Fund policy. First, the effect of Village Fund policy may confound our estimated impact of health insurance if the incidence of Village Fund policy and treatment intensity are correlated. Using only the post-reform data, we test for an association between households' pre-reform health conditions (treatment intensity is defined pre-reform) and post-reform Village Fund take-up. As shown in Table 3, we do not find any evidence that the propensity of Village Fund take-up is correlated with our morbidity index (column 1), even after controlling for village size fixed effect interacted with time fixed effect (column 2). Because each village received the same fixed amount regardless of the village size and that smaller villages (made up of fewer households) could obtain a relatively more intense credit injection, including these additional village controls accounts for any changes in the propensity of Village Fund take-up across villages of different size or with different level of access to the Village Fund credit.[40]

In column (3), we further consider the amount of Village Fund loan(s) conditional on having Village Fund loan(s) in a given month, an intensive margin of Village Fund take-ups. Again, the coefficients on treatment intensity are not significant in the specification that already includes village controls. These findings reassure that, in our case, the funds did not target households with characteristics which are correlated with poor health status.

Second, we then consider and report the estimated impact of health insurance using a specification which includes directly the amount of Village Fund loans held by households in each month as an additional control capturing the potential effect of Village Fund policy. Our estimated impact of health insurance obtained are shown to be robust to these different specifications with village size controls for most outcome variables.[41]

[40]We include the interaction terms between year–month fixed effect and village size group dummies characterised by the number of households in each village. 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 is defined so that we have a roughly equal number of households in each group.

[41]Exploiting the variation in credit injection intensity across the 16 villages as a proxy for the effect of Village Fund, we also consider a specification which includes the year–month fixed effect interacted with village size. 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. Our results are robust to the inclusion of these additional covariates. These additional results are available upon request.

5 The Effects of Health Insurance on Health Expenditure

In this section, we report and discuss the estimation results of the reform impact on health expenditure. We begin by verifying that households with higher pre-reform health risk are those benefiting more from the health reform. We first focus on the reduction in cost of sickness defined as the sum of OOP expenditure and opportunity cost (earnings loss) due to illness[42] We then quantify the direct impact of the reform on household OOP health expenditure, total cost of sickness, and risk associated with it.

5.1 Visualising 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, 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. Within each bin, we then compute the mean of these averages across households. Panel (a) of Figure 1 plots the pre-reform mean total cost of sickness averaged across households in each bin.[43] It is clear that the fitted line exhibits a monotonic upward sloping trend reflecting that households that had more frequent health issues tend to experience higher expected 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.[44] This forms the key to our estimation, which rests on the fact that households with higher health risk are benefiting more from the reform.

[42] The opportunity cost of illness is calculated as the duration of illness times household's pre-reform average earnings.

[43] 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).

[44] 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.

This downward sloping trend in the change in the average cost of sickness is quite robust to excluding the opportunity cost (earnings loss) due to illness as illustrated by Appendix Figure A2 which focuses only on the OOP payments. We still observe the downward sloping trend but not as monotonic as that observed in Figure 1. This is potentially because the impact of the reform on OOP 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 (earnings loss) component that more directly captures household health conditions, and so is 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 expenditure, Appendix Figure A4 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.[45]

5.2 Regression Estimates

We report our estimation results in Table 4. Each cell displays the point estimate of the reform impact on each outcome of interest obtained from different specifications.[46] Consistent with the graphical analysis in Section 5.1, we find that the reform significantly led to higher reduction in both OOP health expenditure and total cost 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 27 Baht (column (1)) and a differential decline in cost of sickness of 70 Baht (column (2)) per month on average.[47] 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 experienced catastrophic health expense.[48] We find that the effects are negative and highly significant sizing about

[45]We have an outlier in bin 5 in panel (b) of Figure 1. A further detailed examination of the data reveals 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 A4 lies closer to the linear fit.

[46]We describe how each outcome variable in our study is constructed in Appendix D.

[47]The results on log expenditures are reported in the Appendix Table A4, respectively. For log expenditures, 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.

[48]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.

0.5 percentage points for the OOP budget share and 2 percentage points for whether household health expenditure was catastrophic.[49]

Short- and Long-run Effects. Figure 2 plots each of the estimated coefficients β_j^{Pre}'s and β_j^{Post}'s from the more flexible event-study specification (3), and reports the p-value of the F-test for the joint significance of β_j^{Pre}'s for each outcome variable.[50] The event study allows us to evaluate the dynamics of the reform impact as well as inspecting the existence of differential pre-trends. We safely cannot reject the null hypothesis of the joint significance of β_j^{Pre}, which suggests insufficient evidence of a pre-trend across all the outcomes. This is corroborated by the fairly flat, near-zero patterns in the coefficients during the pre-reform intervals shown by the graphs. Across all the outcomes, the coefficients clearly shift downward beginning in the first half-year interval post-reform and remain below zero for most of the periods thereafter with larger shifts in the long run.[51] Consistent with this graphical evidence, we see in panel (B) of Table 4 that the magnitude of the average long-run impact is around 1.5–1.9 times larger than the short-run impact. This is not unexpected as the 30-Baht co-payment per visit was abolished making health care become free for all from the beginning of the long-run period.

Village Control. In row (C), adding the village control 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. In this respect, our estimates are unlikely to be confounded by Village Fund take-ups of households.

Discrete Risk Types. The estimates in panel (D) correspond to the discrete-risk-type specification. The top row compares households in the middle three deciles (medium-risk households) to those ranked in the bottom four deciles (low-risk households). In the bottom row, the estimates represent the differential effect of the reform on households in the top three deciles (high-risk households) relative to the low-risk counterparts. These are highly significant and are over twice as large as the estimates in the

[49] 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 robust and are presented in Appendix Table A5.

[50] 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.

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

top row. One way to gauge how large these impacts are is by comparing them with the corresponding pre-reform mean of the medium-risk and high-risk groups. The pre-reform average OOP health expenditure for the high-risk households was 285 Baht per month. The coefficient in column (1) implies that, on average, high-risk households experienced a differential reduction in monthly OOP payments of 63 Baht relative to the low-risk counterparts. This is therefore equivalent to about 22% 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).[52] In column (4), the differential reduction in the incidence of catastrophic health expenditure by 4.3 percentage points for the high-risk households represents a 49% reduction from the pre-reform mean of 8.8% (30% reduction for the medium-risk group).

Detecting Mean Reversion. In Appendix Table A6, 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.[53] 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 4.2.2, 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 A5.[54] 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

[52] 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. The corresponding figure for the high-risk households (middle three bins) is about 17%.

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

[54] Note that the base period here corresponds to Jul'01–Dec'01 instead of Jul'00–Dec'00 used previously.

to a pre-reform differential trend and the p-value of the F-test of just 0.34. 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 differential reduction in the cost of sickness as well as in exposure to medical expenditure risk for the households associated with higher morbidity index. Using the same research design, we now turn to evaluate the effect of health insurance on various outcomes related to household investment decisions.

6 The Effects of Health Insurance on Cultivation Decisions

In this section, we investigate the reform impact on cultivation activities, including (1) cultivation investments, (2) cultivation outputs and (3) cultivation portfolio. We focus on households having at least one acre of cultivation land in the pre-reform period.[55] In addition, we include total cultivated land size as an additional control variable in all our regressions related to cultivation activities. This ensures that we can interpret the impact on total investments and inputs holding total cultivated land size fixed.

Because the uses of inputs and harvests are recorded at the crop-plot level (each plot of each crop grown by a household), we construct a crop-plot panel for each household separately. For each crop-plot, we use a 'flow value' approach in calculating the monthly costs and harvests.[56] We first sum up all the (real) cost or harvest values across each month throughout the *duration* of the crop-plot—from preparation until it is harvested (possibly several times) and finished—and divide it by the duration (in months). We then equally distribute this average value across all months throughout the duration of the crop-plot to obtain the monthly flow value. We then sum up the values across all crop-plots of the same household to arrive at the household-level information in any given month.[57]

[55] Almost 90% of households in our data were engaged in cultivation. 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. Around 3% of all crop-plots have missing plot size. These are plots in which households usually grew crops in an unorganised manner (e.g., scattered around the house, circling a fish pond or along the fence). We exclude these crop-plots from our analysis and only use organised crop plots for which households report non-missing plot size to generate all cultivation outcomes.

[56] A similar approach is described and adopted in Samphantharak and Townsend (2010) for calculating the monthly values of cultivation inputs and outputs in household financial accounting.

[57] Further details of how the cultivation variables are constructed can be found in Appendix D.2.

6.1 Inputs in Cultivation

We first investigate the impact of health insurance on cultivation input outcomes shown in the first five columns of Table 5. In column (1), we find that the reform had a positive long-run impact on total cultivation expenditure, even after controlling for the potential effects of the Village Fund policy. In column (1), the point estimate in the bottom row of panel (B) indicates a differential increase in total cultivation expenditure of 268 Baht with respect to a one-unit increase in the morbidity index (roughly the inter quartile range).[58] However, the short run effects are much smaller and not statistically significant. Our results for the short run are 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 total cultivation costs even with the insurance directly targeting cultivation. Despite their imprecision, the estimates obtained from the discrete-risk-types specification for the medium-risk households (middle three deciles) in the top row of panel (C1) and (C2) are negative and consistent with our theoretical model prediction that total investments fall following the provision of public health insurance (proposition 2). However, this is not true for the high-risk households (bottom row of panel (C1) and (C2)) as the corresponding estimates are positive with the sizable long run differential effects of 1,061 Baht, about 41% of their pre-reform mean.

In columns (2)–(4), we break down total cultivation expenditures into investments on three different types of inputs: raw inputs, physical inputs and hired labour. Again, the short-run effects are not significant across all types of inputs and even become negative when comparing the medium-risk households (middle three bins) against the low-risk counterparts (bottom four bins). Over the long run, we find that the positive effects on total cultivation expenditure are primarily driven by the differential increase in the costs of raw inputs, which account for about over two-thirds based on the estimates in the bottom row of panel (A) and (B) in column (2). These are consistent with the event-study diagrams in panel (a) and (b) of Appendix Figure A6, in which we observe a clear upward shifts in the pattern of coefficients over the long run. Focusing on the high-risk households (bottom row of panel (C2)), we find the long-run differential increases in investments of 699 Baht for the raw inputs and 366 Baht for the hired labour, but close to zero for the physical inputs. In fact, these households differentially

[58] Total cultivation expenditure include costs of raw inputs (seeds, fertilisers, pesticides), physical inputs (equipment rentals, transportation and storage costs), and hired labours. These are the outcome variables in columns (2), (3) and (4). Note that this excludes the opportunity cost of family labour. See Appendix D.2 for more details of variable definition and construction.

reduced their physical inputs investments in the short run by 116 Baht (bottom row of panel(C1) in column (3)), which is about 26% of their pre-reform average.

In Appendix Table A7, we further decompose the costs of raw inputs into expenditures on seeds, fertilisers and pesticides, and break down the costs of physical inputs into equipment rentals and the costs for transport and storage.[59] On average, we find that almost all of the long-run differential rise in raw input investments are attributable to roughly comparable differential increases in the spending on fertilisers and seeds (about 77–97 Baht) although the latter effect is insignificant. For the high-risk households, however, seeds investments dominate, almost doubling those in fertilisers over the long run. Their short-run differential reduction in physical inputs investments were attributable to the transport and storage costs.

Another important type of inputs in agricultural production are family labour inputs. Family labour on farming creates an implicit cost (opportunity cost), which we measure using 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.[60] We find a negative but imprecise effect of health insurance on the opportunity cost of family labour (column (5)), suggesting that reform may not have a strong impact on family labour supply on cultivation.[61]

Appendix Figure A6 provides 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, which are insignificantly different from zero relative to the base period, suggest no sufficient evidence of pre-trend across most outcomes.[62] There is also no clear evidence of mean reversion for all the outcomes. Overall, the graphs are consistent with the point estimates in Table 5.

[59] Seeds expenditure comprises costs of hormones, seeds, and seedlings. Fertilisers include chemical fertilisers and manure, while pesticides incorporate herbicide, fungicide and insecticide. Equipment rentals are expenditures on the use of equipment and animals that do not belong to households in cultivation activities. Transport and storage costs include costs for rental, fuel, repair and maintenance of machinery and equipment, along with transportation, transformation and storage of goods and inputs.

[60] 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.

[61] For robustness, we consider another measure in which the opportunity cost is defined based on the average provincial-specific formal-sector hourly earnings using data from Thailand's Labour Force Surveys. Results remain insignificant.

[62] The only exception is the opportunity cost of family labour in panel (e). However, 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.

6.2 Outputs in Cultivation

With the evidence of the impact of health insurance provision on investments, we now investigate whether and how they translate into the effects on cultivation outputs. Consistent with the results we find for inputs, the point estimates in columns (6) and (7) of Table 5 indicate that the reform had a long-run positive impact on cultivation harvests and net cultivation income (or profits), where the latter is defined by subtracting total cultivation expenditure and opportunity cost of family labour from harvest value. Again, these are robust even after controlling for the potential effects of the Village Fund policy. In panel (A), the baseline estimates imply that approximately an inter-quartile-range increase in the morbidity index is associated with a long-run differential increase in harvest value and net cultivation income of 1,123 and 901 Baht, respectively. The short run effects are positive but about five times smaller on average. In panel (C2), we see that the high-risk households differentially enjoyed more cultivation profits of about 2,708 Baht per month on average compared to the low risks. This long-run differential increase is sizable, accounting for about 97% of high-risk households' average cultivation profits during the pre-reform period.

We compare the estimated impact on cultivation profits to that on total net income and net farm income of households, which we find to be positive both in the short and long run in Appendix Table A8.[63] Interestingly, over the long run, both the effects on total net income and farm profits are not very different in magnitude from that on the profits from cultivation. For instance, the long-run positive effect on total net income of 1,152 Baht is attributable to the positive effects of 1,018 Baht and 901 Baht on farm and cultivation profits, respectively. This reaffirms the importance of cultivation activities in relation to household net income over the long run. This might not be the case in the short run, however, as only less than one fifth of the short-run positive effect on total net income is due to the differential increase in cultivation profits.

Appendix Figure A7 provides the event-study analysis for both outcomes. The pre-reform patterns and p-values of the F-test for joint significance of the pre-reform interaction terms suggest insufficient evidence of pre-trend. There is also no sign of mean reversion for both variables. Consistent with the results in Table 5, the patterns display a clear upward shift in coefficients in periods after the reform,

[63] Total net income includes net operating income from all sources, while farm profits, or net farming income, comprises net income from all farming activities (cultivation, livestock and fisheries). See Appendix D.3 for details of how household net income is constructed.

which becomes larger over the long run periods. We see that the pattern for net cultivation income quite closely resembles that of the harvest value, suggesting that the reform impact on net cultivation income is driven by the effect on harvest value more than that on input investments.

Our estimates on cultivation profits are in contrast to studies using data from rural West Africa which typically find the effects of insurance provision on profits to be negative or insignificant (Karlan et al., 2014). There are two possible reasons that can help reconcile these differences. First, our effects on profits are driven mainly by the long run effects, six to nine years after the reform, whereas Karlan et al. (2014) only consider up to three years of the insurance impact. As our results indicate, the effects on households' cultivation decisions and their resulting profits could take several years to show up; it takes time for households to realise the implicit income transfers, the reduction in background risk, and possibly better health, and then to accordingly adjust their production and investment plans. This is especially true in our case where the provided insurance is aimed at health and not directly at cultivation. Second, our health reform may entail the effects of insurance through productivity channel in that better health improves productivity of cultivation investment due to the complementarity between health and this type of investment, thereby inducing positive effects on outputs and income. We explore more on this potential channel of effect in Section 6.4.

Our empirical results so far suggest that, following the differential decline in both the mean and variance of OOP payments, households with higher initial health risk displayed differential increases in cultivation investments, harvest value, and, ultimately, net cultivation income. A simple cost-benefit analysis presented in Appendix Section E reveals that this differential income gain, combined with the reduced costs of sickness, generates benefits that outweigh the differential government costs of financing the reform per household by about 2.2 times over the long run, implying that the reform is effectual.

6.3 Portfolio of Cultivation Investments

We now examine whether households altered their allocation of crop choices towards more risky cash crops following the provision of health insurance in this subsection. We focus on three types of outcome variables: land size and expenditures devoted to growing cash crops, and harvest values. These allow us to investigate how the post-reform reduction in household background risk associated with medical expenditure, along with the real income effect, can affect household risk-taking decisions.

Defining (Risky) 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.[64] 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.[65] 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) in Table 6 first examines the effect at the extensive margin considering whether households grew cash crop(s). We find strong evidence of a long-run differential increase in the probability of growing cash crop(s), with the baseline estimates identical to the estimates with Village Fund control. An inter-quartile range increase in the morbidity index is associated with a differential rise in the long-run propensity of growing cash crop(s) of 6 percentage points, equivalent to about 11% of the pre-reform average for households in the top five bins ranked by the index. Although the short run effects are positive, they are about four times smaller and are insignificant. The estimates in panel (C2) further suggest that this long-run differential change in risk-taking decision is likely attributable to the high-risk households devoting relatively more resources to cultivating cash crops as their effects (relative to the low-risk households) are 1.6 times larger than those of the medium-risk counterparts.

Columns (2) and (3) consider the size of land devoted to growing cash crop(s) (intensive margin). Consistent with the event-study diagrams in panel (b) and (c) of Appendix Figure A8, we find significant positive effects that are much larger in the long run. The inclusion of Village Fund control barely affects the baseline estimates holding fixed the total cultivated land size. In column (2), the reform effect on the size of cultivated land devoted to growing cash crop(s) is about 1.04 Rai (0.41 acres) for a one-unit

[64] From our data, we can classify crops into 24 different main types including rice, maize, sorghum, sugar cane, mango, coconut, banana, along with many other fruits, vegetables, and perennial trees. See Appendix D.2 for a complete detail. The four provinces in our sample are categorised into two regions: Central and Northeast.

[65] 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.

(inter-quartile range) increase in the morbidity index. The long-run point estimate in the top row of panel (C2) indicates the average differential increase of 1.5 Rai (0.59 acres) in cash-crop cultivated land of the medium-risk relative to that of the low-risk households, which accounts for about 31% of the pre-reform average of the medium-risk households. In column (3), we ask whether households shifted their land towards growing cash crop(s) given that they already grew at least one type of crop in a given month (conditional intensive margin). Again, we find the differential effects that are positive and much larger over the long run. A one-unit increase in the morbidity index is associated with a 3.6 percentage points rise in the fraction of land devoted to growing cash crop(s).

Besides investment in terms of land size, we also examine the effects on cultivation expenditures relating to cash crops, where these expenditures do not include the (implicit) rental price for the land that is used. In column (4), we find a differential increase of about 5 percentage points in the probability of investing in cash-crop cultivation which is associated with a one-unit increase in the morbidity index. In column (5), the long-run differential effect on total cash-crop expenditures is about 120 Baht. Therefore, about 45% of the differential increase in total cultivation expenditure is due to cash crops (relative to 268 Baht in column (1) of Table 5). In Appendix Table A9, we report the reform effects on each component of total expenditure on cash crop(s). Although the overall effects on total cash crop expenditure is imprecise, we find positive and significant effects on seeds expenditure in the short run and on pesticides expenditure in the long run. Interestingly, almost all of the differential increase in pesticide investments and farm equipment rentals are due to those associated with cash crop(s).

Column (6) of Table 6 report the point estimates for total harvest values of cash crop(s). At the baseline, we find that a one-unit increase in morbidity index is associated with a differential increase in the harvest value of cash crop(s) of 507 Baht over the long run. Therefore, cash crops account for about 45% of the long-run differential increase in total harvest value (relative to 1,123 Baht in column (6) of Table 5). Appendix Figure A8 displays the event-study diagrams for all the main outcome variables in this subsection. Overall, the patterns are consistent with the estimates we find in Table 6. Across all the outcomes, we see clear upward shifts during the long-run post-reform periods that depict the switch in crop choice towards more risky cash crops. There are also no signs of pre-trend or mean reversion.

6.4 Discussion

Our results indicate that higher-risk households did actually shift their crop portfolio relatively more than lower-risk households towards cash crops both at the intensive and extensive margins, and especially in the long run. This is consistent with the prediction from our theoretical model in Section 3 (proposition 1), where the reform reduced medical expenditure background risk (as well as generating an income effect from increased expected income), and thereby allowed them to engage in more risky cultivation activities. Although our health insurance does not directly insure against harvest risk, our results suggest that health insurance can eventually lead households to cultivate riskier crops. In this respect, health insurance can play a similar role as rainfall insurance, where researchers have shown that the provision of crop or rainfall index insurance induced farmers to take on riskier crop choices and investments (Karlan et al., 2014; Cai, 2016; Cole, Giné, and Vickery, 2017).

Our theoretical model also predicts a decrease in total cultivation investment (proposition 2) following public health insurance provision. Instead, our empirical evidence in Section 6.1 suggests that overall investment goes up following the reform, especially in the long run.

What could explain the increase in overall investments? One potential channel through which health insurance provision could entail a positive effect on total agricultural investment is health improvement. Health insurance differs from a pure insurance program in that, by inducing people to use more health care facilities, there may be an improvement in their health. If there is complementarity between cultivation investment and health in the production function, an improvement in health will increase the marginal return from investment and hence induce households to invest more.

In Table 7, we provide evidence of the reform effects on health using the same identification strategy, concluding that indeed there is a positive health effect of the reform.[66] In column (1), we find that health insurance provision led to a differential decline in the probability that a household member had any symptom, with larger long-run effect sizing about 12 percentage points. The point estimates in panel (C) indicate that this health effect is driven by the high-risk households, who were on average about 20–22 percentage points relatively less likely to report having symptom(s) compared to the low-risk households. This is equivalent to about 32% reduction from the pre-reform mean of the high-risk

[66] It should be reiterated that, in Table 4, the cost of sickness (column (2)) comprises OOP health expenditure (column (1)) and the opportunity cost (earnings loss) due to illness. This latter component captures the effect on household health. Therefore, in addition to the reduction in OOP payments, the negative effects on the cost of sickness we find in column (2) also implies the differential improvement in health conditions for households with higher morbidity index.

households. In column (2), we focus on the total duration (calculated as the fraction of days of a given month) of all symptom(s) suffered by all household members. Again, we find a strong indication that the reform led to a differential reduction in the total duration of illness suffered by households associated with higher morbidity index with more pronounced effects in the long run.

Column (3) considers whether the households suffered from a work-limiting or severe symptom. The baseline estimate suggests that on average a one standard deviation increase in morbidity index is associated with a differential decline in the probability that a household member stopped going to work or school due to illness of about 7.7 (8.7) percentage points in the short (long) run. Comparing the estimates in column (4) to those in column (2) suggests that about half of the differential reduction in the total duration of symptoms was attributable to that of the severe symptoms. In Appendix Figure A9, the coefficients obtained from a more flexible event-study specification show patterns that are consistent with the estimates across all the outcomes in Table 7, displaying a clear downward shift in coefficients following the reform and slightly more in the long run.[67]

To sum up, the simple model where health insurance only protects households against the medical expenditure risk (Section 3) can explain the increasing investment in risky crops but not the increase in overall investment. Investment in this model plays the same role as savings in the standard intertemporal consumption model, where savings decrease following a mean preserving contraction of the future income distribution (due to the marginal expansion in health insurance coverage) if households are prudent. Furthermore, if the insurance premium is not adjusted (e.g., fully subsidised as in the Thai case), then a marginal increase in insurance coverage will also entail a real income effect, meaning that total investment can go down irrespective of whether the household is prudent or not (result 5 in Appendix A). Our empirical evidence above suggests that health insurance may affect cultivation investment not only via the insurance channel but also via the health channel. The positive health effect could explain the overall increase in investment if there is complementarity between health and investment in agricultural production.[68]

[67]We further check for mean reversion in health in Appendix Table A10. Across all specifications, the magnitude and significance of the estimates are not much different from the baseline estimates in Table 7, suggesting that mean reversion in health is unlikely to drive our results.

[68]Two other papers also find positive effects of the 30-Baht reform on health. Gruber, Hendren, and Townsend (2014) reveal that the reform substantially lowered infant mortality especially among the poorer provinces. Wagstaff and Manachotphong (2012) show that the reform reduced the probability of workers reporting themselves too ill to work.

7 The Effects on Household Welfare

In this section, we assess the impact of the 30-Baht reform on household welfare. In columns (1) and (2) of Table 8, we begin by reporting the estimated impact of the reform on total consumption and non-durable consumption, both of which are standard measures of household welfare.[69] At the baseline, we find positive effects on consumption in both columns, but the estimates are imprecise.[70] For instance, we estimate a 81 Baht per month rise in total consumption (26 Baht in non-durable) with respect to a one standard deviation increase in the morbidity index.

Although the effects on consumption are small and imprecise, low income and high debt households may use the additional cultivation income after the reform to pay down debt instead of increasing consumption (Agarwal, Liu, and Souleles, 2007). Almost 80 % of households in our sample were with debt during the pre-reform months. Given the prevalence of debt that households have, we also examine households' amount of debt and their ability to repay. In column (3), we examine the value of loans compared with the value of assets using the loans–assets ratio, measuring the extent of household leverage.[71] We find that the reform had a negative impact on the household loans-to-assets ratio. At the baseline in panel (A), the overall impact on the loans-to-assets ratio is attributable to the significant negative long-run effect of 1.3 percentage point. The discrete-risk-type specification in panel (C) indicate that the impact is driven by the high-risk households in the very top bins. On average, relative to low-risk households, the high-risk reduced their loans-to-assets ratio by 3.9 percentage points, which is equivalent to a 39% reduction from the pre-reform mean of 10.1%.[72]

In column (4) and (5), we examine the reform impact on the loan default rate as well as on the

[69]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. Total consumption is the sum of net expenditure on durable goods and non-durable consumption. Both consumption measures exclude OOP health expenditures. See Appendix D.3 for more details of each outcome variable.

[70]Note that we trim the top and bottom 0.5% of consumption variables. Without trimming, the effects we find are about five times larger and are significant at 5% level in the long run. However, these are likely driven by outliers in the consumption expenditures data.

[71]As our sampled households are extensively engaged in informal agricultural activities and investments, we can potentially 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. The value of loans (deducting interest paid) and that of assets are obtained directly from the Monthly Financial Accounting dataset of the Townsend Thai project. See Appendix D.3 for further details.

[72]Our results are robust to using an alternative definition 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. 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, and hence does not reflect households' repayment decisions. Our results are also robust to using ratios that are conditional on households having any outstanding loans in a given month.

fraction of loans households managed to repay out of the required amount.[73] In column (4), we find that, conditional on having to repay their loans in a given month, households associated with higher health risks had differentially lower default rate. At the baseline, the differential reduction of 1.8 percentage points is statistically significant over the long run and is slightly larger than the short-run effect. The estimates in panel (C) suggest that this is driven by the high-risk households whose average reduction of 5 percentage points (relative to the low-risk) represents about 68% decline from their pre-reform mean default rate of 7.3 percent. In column (5), we find that the fraction of loans repaid by these high-risk households differentially increased by 4.4 and 3.5 percentage points over the short and long run, respectively, from their pre-reform mean repayment rate of 93%.

One might worry whether the micro-credit Village Fund scheme could confound our estimated effect of health insurance on borrowings. Nevertheless, our results are robust to the inclusion of the additional village controls and we have demonstrated in Section 4.2 that our morbidity index (treatment intensity) is orthogonal to the Village Fund take-ups. In addition, as reported in the Appendix Figure A10, our more flexible event-study specification also suggests no clear existence of pre-trend and mean reversion. Consistent with our estimates in Table 8, the patterns display a downward shift which becomes larger over the long run for the loans-assets ratio in panel (c). In panel (d) and (e), we observe an overall downward and upward shift for the default rate and the repayment rate, respectively.

Despite the limited evidence of an increase in consumption, our empirical findings on debts and increased repayment ability, combined with the rise in net income found earlier, potentially indicate the improvement in household financial situation and welfare following the reform. One should keep in mind, however, that lower household leverage does not always translate into financial improvement as it may also reflect dwindling investment activities or more limited access to credit markets. Nevertheless, our result that overall investments actually increased over the long run plausibly does not suggest so.

8 Conclusions

This paper assessed the causal relationships between health insurance coverage, agricultural decisions and welfare. Exploiting the 2001 universal health insurance reform in Thailand as a source of identifica-

[73] These variables are conditional on the months in which households reported that they were supposed to repay their loan(s). Households defaulted on loans if they failed to repay the full amount that they were obliged to repay in a given month. See Appendix D.3 for more details.

tion, we estimated the effects of health insurance coverage on cultivation investment and output as well as the portfolio of investments across different types of crops. Our estimates suggest that the reform led to rises in total cultivation investments (largely driven by seeds and fertilisers), output and, ultimately, net income. We find that households shifted their cultivation portfolio away from safe crops towards risky crop choices in the long run. The reform also led to an eventual improvement in households' welfare as it increased their ability to repay debts and reduced default on loans.

We also formulated a model of agricultural investment under uncertainty, and used the model to illustrate the mechanisms through which an expansion of subsidised health insurance coverage can affect households' agricultural decisions. The Thai health insurance reform mitigates the background risk by reducing the variability of out-of-pocket medical expenditure. It also generates an income effect as the implicit premium is far below the expected reimbursement. We showed that these two channels can predict the increase in the cultivation of risky crops, consistent with our empirical findings. The same two channels, however, cannot explain the increase in total cultivation investment and we provided empirical evidence that an improvement in health due to the reform may be the additional factor driving the increase in overall cultivation activities.

Altogether, our empirical results therefore demonstrate that, in addition to providing financial security, public health insurance coverage can entail indirect benefits in boosting household agricultural production. Despite not directly insuring against agricultural production risk like rainfall or crop insurances, public health insurance can affect production choices, investments, and ultimately income through the channels of income transfers, background medical expenditure risk reduction, and improvements in health. Therefore, expanding health insurance may not only improve households' financial security, but its financial implications also spread beyond health care users and providers, and into various aspects of the economy including production and investment. Policymakers should thus take these factors into account when valuing or deciding on public health insurance schemes.

Tables & Figures

Table 1: Comparison of health insurance Schemes Pre- and Post-Reform

Pre-Reform	**Description**	**% HHs Data**	**Post-Reform**	**Reform Impacts**
Previously Uninsured	Paid for health care out-of-pocket	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	5		
MWS	Free care for the poor, those aged < 12 and > 60, monks, disabled	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	9	SSS	No formal changes
CSMBS	Free care for civil servants and dependents		CSMBS	

Note: 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 (∼\$2.56/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 for 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 variables (pre-reform data)	Morbidity index: Below-median	Morbidity index: Above-median	Diff.(p-val)
(A) Characteristics of Households			
Household size	4.228	4.216	0.705
	(1.864)	(1.853)	
Number of kids (aged under 15)	1.214	1.217	0.835
	(1.129)	(1.086)	
Number of elderly (aged over 60)	0.607	0.627	0.122
	(0.775)	(0.773)	
Age of household head	52.32	52.87	0.016
	(13.53)	(13.95)	
Male household head	0.756	0.748	0.244
	(0.430)	(0.434)	
Years of head's education	4.347	4.022	0.000
	(2.279)	(1.975)	
(B) Health Conditions & Health Expenditures of Households			
Member(s) had symptom(s)	0.240	0.552	0.000
	(0.427)	(0.497)	
% of days member(s) had symptom(s)	0.068	0.278	0.000
	(0.206)	(0.465)	
Member(s) had work-limiting symptom(s)	0.063	0.186	0.000
	(0.244)	(0.389)	
% of days member(s) had work-limiting symptom(s)	0.011	0.054	0.000
	(0.062)	(0.189)	
Member(s) with chronic health conditions	0.186	0.500	0.000
	(0.389)	(0.500)	
Member(s) with disability conditions	0.000	0.115	0.000
	(0.000)	(0.319)	
Standardised morbidity index	-0.703	0.677	0.000
	(0.352)	(0.906)	
Number of visits to health facility	0.151	0.418	0.000
	(0.445)	(0.73)	
Out-of-pocket health expenditure (Baht/month)	101.3	240.7	0.002
	(1,521.6)	(3,473.5)	
Total costs of sickness (Baht/month)	123.5	322.7	0.000
	(1,562.4)	(3,616.3)	
Consumption budget share of OOP health expense	0.010	0.028	0.000
	(0.052)	(0.08)	
Whether catastrophic health expenditure	0.020	0.075	0.000
	(0.139)	(0.263)	
Number of *pre-reform* observations	7,278	7,163	

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) at December 2004 prices. All variables are in monthly format. Above-median (below-median) 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 4.2.1 for more details.

Table 3: Adjusted Treatment Intensity and the Village Fund Policy

Dependent Variables:	Whether has VFund loan(s)		Amount of VFund loan(s)
	(1)	(2)	(3)
Treatment Intensity (M_i)	-0.030	-0.021	-494.28
	(0.020)	(0.019)	(357.29)
Village controls	No	Yes	Yes
Household characteristics	Yes	Yes	Yes
Time fixed effects	Yes	Yes	Yes
Number of Observations	50,553	50,553	31,725
R-squared	0.072	0.119	0.201

Note: Robust standard errors, clustering by households, in parentheses: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. The dependent variables are the dummy variable on whether household held any loan from the Village Fund in column (1) and (2), and the amount of village fund loans conditional on household having Village Fund loans outstanding in a given month in column (3). The estimated coefficients on treatment intensity (M_i) are reported. M_i is the morbidity index generated from the principal component analysis using pre-reform history of household's health conditions. Included controls (not displayed) are the age of the household head and its squared term, a dummy variable for household head being male, fraction of under-15 children living in household, fraction of over-60 elderly, a set of dummies for household size, and time (in year-month) fixed effects. The village controls included in column (2) and (3) 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 4: Impacts on Health Expenditure

Dependent Variables:	(1) OOP health exp	(2) Cost sickness	(3) OOP exp share	(4) Catastrop. exp
(A) Baseline	-27.39**	-70.01***	-0.005**	-0.020***
	(11.20)	(18.51)	(0.002)	(0.005)
(B) Short- & Long-run Effects				
Short-run Effects	-20.74*	-53.02***	-0.004	-0.017***
	(11.75)	(17.45)	(0.003)	(0.006)
Long-run Effects	-38.93***	-99.48***	-0.007***	-0.025***
	(10.03)	(25.62)	(0.002)	(0.006)
(C) Village Fund Control	-26.96**	-67.66***	-0.005**	-0.020***
	(11.18)	(17.93)	(0.002)	(0.005)
(D) Discrete Risk Types				
Medium Risk	-30.67***	-56.83***	-0.004**	-0.016**
	(11.72)	(39.85)	(0.002)	(0.007)
High Risk	-62.71***	-169.92***	-0.014***	-0.043***
	(17.63)	(47.29)	(0.003)	(0.010)
Number of Observations	64,886	64,886	64,886	64,886

Note: Robust standard errors, clustering on households, 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 children 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 ($M_i \times Post_t$). M_i is the morbidity index generated from the principal component analysis using pre-reform history of household's health conditions. The village control in (C) is the amount of household's village fund loans scaled by 10,000 Baht. Row (B) short-run and long-run report the estimates for the interaction of M_i with the short-run (Jan'01-Dec'06), and with the long-run (Jan'07-Dec'10) post dummy respectively. Panel (D) reports the estimates of the interaction between the dummy for high-risk households (top 3 deciles ranked by the morbidity index) and $Post_t$ in the top row, and that between the dummy for medium-risk households (middle 3 deciles) and $Post_t$ in the bottom row. Both estimates are relative to the low-risk households (bottom 4 deciles). All dependent variables are constructed using household OOP health expenditure trimmed at the top 0.5% within the pre- and post-reform periods. Total cost of sickness (column 2) 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. OOP health expenditure is catastrophic (column 4) when it accounts for over 10% of household total non-durable expenditure.

Table 5: Impacts on Cultivation Outcomes

Dependent Variables:	(1) Total cultivation expenditure	(2) Costs of raw inputs	(3) Costs of physical inputs	(4) Costs of hired labour	(5) Opp. cost of family labour	(6) Harvest value	(7)=(6)-(5)-(1) Net income Cultivation
(A) Baseline:							
Short-run	63.12	26.78	-10.32	46.66	-38.11	215.54	190.53
	(107.47)	(53.35)	(38.89)	(44.95)	(47.14)	(208.11)	(147.84)
Long-run	273.11*	186.21*	36.26	50.64	-51.53	1,122.78**	901.20**
	(157.72)	(96.91)	(48.21)	(55.78)	(52.97)	(475.98)	(375.39)
(B) Village Fund Control:							
Short-run	94.55	41.05	-6.49	59.98	-39.34	290.55	235.35
	(100.88)	(51.13)	(37.89)	(44.26)	(46.96)	(202.85)	(147.22)
Long-run	267.74*	183.65*	35.46	48.63	-51.11	1,109.10**	892.47**
	(157.25)	(97.34)	(47.91)	(55.81)	(52.82)	(485.98)	(382.80)
(C) Discrete Risk Types:							
(C1) Short-run:							
Medium Risk	-372.39	-200.97	-63.56	-107.86	-144.19	-374.86	141.72
	(291.46)	(133.30)	(45.78)	(151.75)	(111.43)	(648.16)	(412.82)
High Risk	157.78	90.99	-116.01**	182.80	-22.26	661.77	526.25
	(354.08)	(167.90)	(50.40)	(184.95)	(120.92)	(779.73)	(483.30)
(C2) Long-run:							
Medium Risk	-141.42	-47.43	-103.44	9.45	-169.63	857.46	1,168.51
	(340.97)	(181.31)	(100.58)	(152.88)	(128.13)	(1,003.46)	(735.48)
High Risk	1,060.94**	698.88**	-3.86	365.92*	-131.75	3,637.47**	2,708.28***
	(521.58)	(304.58)	(133.89)	(208.02)	(134.13)	(1,441.28)	(998.91)
Number of Observations	49,430	49,430	49,430	49,430	49,430	49,430	49,430

Note: Robust standard errors, clustering on households, 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 as described in the footnote under Table 4. Each regression also includes total cultivation plot size as an additional control. Panel (A) and (B) report the estimates for the interaction of M_i with the short-run (Jan'01-Dec'06), and with the long-run (Jan'07-Dec'10) post dummy respectively. M_i is the morbidity index generated from the principal component analysis using pre-reform history of household's health conditions. The village control in (B) is the amount of household's village fund loans. Panel (C1) and (C2) report the estimates of the interaction between the dummy for high-risk households (top 3 deciles ranked by the morbidity index) and $Post_t$ in the top row, and that between the dummy for medium households (middle 3 deciles) and $Post_t$ in the bottom row. Both are relative to the low-risk households (bottom 4 deciles). (C1) reports the short-run while (C2) reports the long-run estimates. Households with total monthly crop-plot size of smaller than 1 acre (2.53 Rai) on average over the pre-reform months are excluded. Total cultivation expenditure (1) is the sum of costs of raw input(2), physical input (3) and hired labour(4), excluding opportunity cost of labour (5). Raw input costs are expenditures on seeds, fertilisers and pesticides. Physical input costs are the sum of rental costs for equipment and animals used in production as well as expenditures on fuel, repair and maintenance of machinery and equipment, and transportation, transformation and storage of goods and inputs. Hired labours (4) are either hired as an individual or as a labour group. The opportunity cost of labour (5) 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 6: Impacts on Cultivation Portfolio Outcomes

Dependent Vars.:	(1) Whether grow cash crops	(2) Land size for cash crops	(3) Land fraction devoted to cash crops	(4) Whether invest in cash crops	(5) Total exp. on cash crops	(6) Harvest values of cash crops
(A) Baseline:						
Short-run	0.014	-0.048	0.004	0.011	-0.85	-23.66
	(0.012)	(0.229)	(0.010)	(0.012)	(26.17)	(78.25)
Long-run	0.060***	1.037*	0.036**	0.052**	119.73	507.29*
	(0.020)	(0.578)	(0.017)	(0.021)	(98.68)	(292.38)
(B) Village Fund Control:						
Short-run	0.014	0.004	-0.039	0.011	6.48	-2.96
	(0.012)	(0.243)	(0.010)	(0.012)	(25.39)	(76.00)
Long-run	0.060***	1.035*	0.036**	0.052**	118.12	502.71*
	(0.020)	(0.578)	(0.017)	(0.021)	(99.37)	(293.45)
(C) Discrete Risk Types:						
(C1) Short-run:						
Medium Risk	-0.004	0.056	-0.000	-0.008	-108.75	-126.34
	(0.031)	(0.518)	(0.023)	(0.031)	(115.40)	(200.63)
High Risk	0.010	-0.287	0.015	0.003	-53.78	-202.17
	(0.035)	(0.811)	(0.026)	(0.035)	(94.94)	(258.83)
(C2) Long-run:						
Medium Risk	0.054	1.524	0.043	0.045	-125.38	427.12
	(0.045)	(1.085)	(0.037)	(0.043)	(234.40)	(620.66)
High Risk	0.090*	2.181	0.058	0.074	305.24	1,473.64
	(0.050)	(2.025)	(0.048)	(0.050)	(339.40)	(952.71)
No. of Observations	49,430	49,430	34,844	49,430	49,430	49,430

Note: Robust standard errors, clustering on households, 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 each model specification, see footnote under Table 5. Households with total monthly crop-plot size of smaller than 1 acre (2.53 Rai) on average over the pre-reform months are excluded. Fraction of land devoted to growing cash crops (3) is conditional on households having cultivated land in a given month. Total expenditure (5) is the sum of costs of inputs, capital and hired labour (excluding opportunity cost of labour) spent on cash crops.

Table 7: Impacts on Health Outcomes

Dependent Variables:	(1) Whether symptom(s)	(2) Fraction of days with symptom(s)	(3) Whether severe symp.	(4) Fraction of days severe symptom(s)
(A) Baseline:				
Short-run	-0.096***	-0.074***	-0.077***	-0.042***
	(0.014)	(0.017)	(0.009)	(0.008)
Long-run	-0.115***	-0.096***	-0.087***	-0.043***
	(0.015)	(0.020)	(0.008)	(0.010)
(B) Village Fund Control:				
Short-run	-0.095***	-0.073***	-0.077***	-0.042***
	(0.014)	(0.016)	(0.009)	(0.008)
Long-run	-0.116***	-0.096***	-0.088***	-0.044***
	(0.015)	(0.020)	(0.008)	(0.010)
(C) Discrete Risk Types:				
(C1) Short-run:				
Medium Risk	-0.055**	0.021	-0.057***	-0.015***
	(0.026)	(0.029)	(0.009)	(0.005)
High Risk	-0.203***	-0.140***	-0.152***	-0.054***
	(0.029)	(0.037)	(0.017)	(0.013)
(C2) Long-run:				
Medium Risk	-0.071**	0.056	-0.036***	0.008
	(0.031)	(0.040)	(0.012)	(0.011)
High Risk	-0.219***	-0.139***	-0.166***	-0.059***
	(0.037)	(0.044)	(0.017)	(0.012)
Number of Observations	49,380	49,380	49,380	49,380

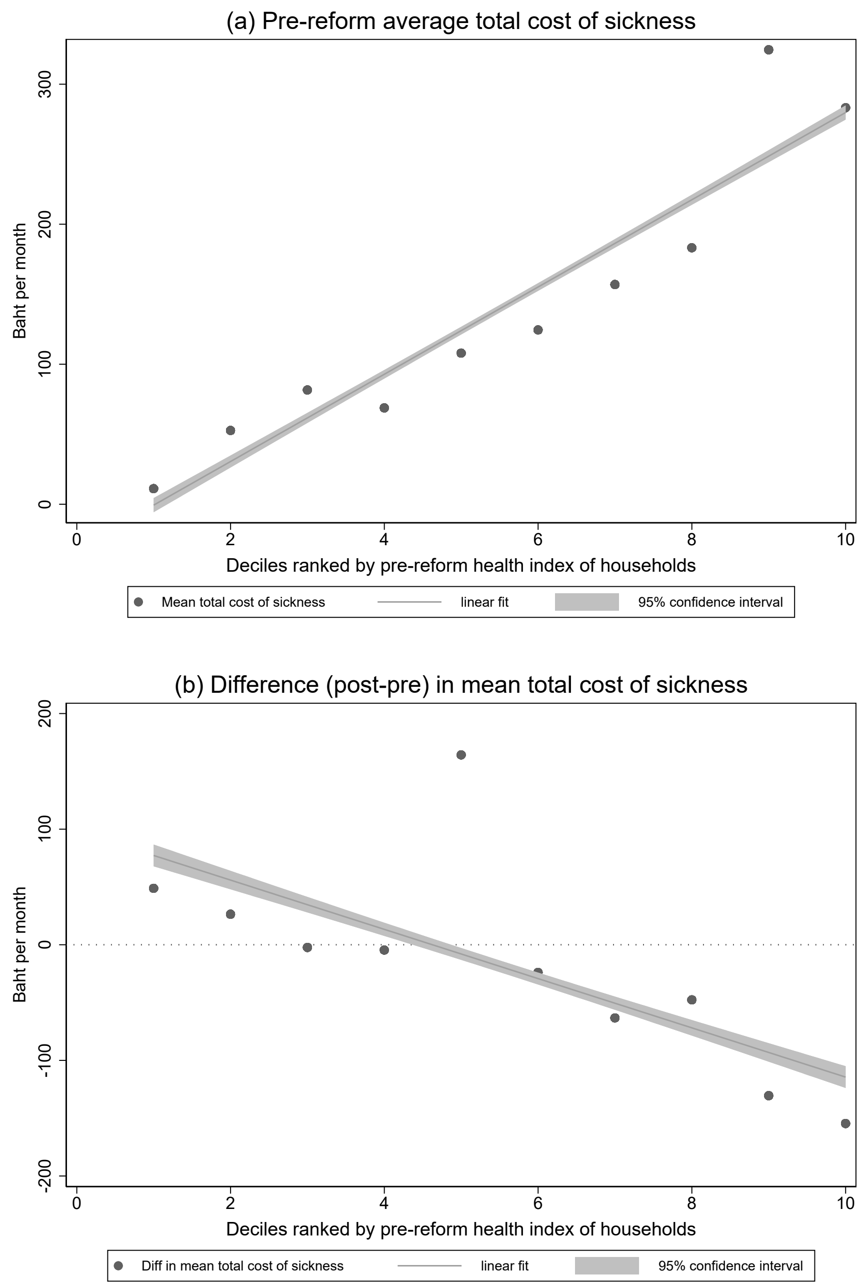
Note: Robust standard errors, clustering on households, in parentheses: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. The dependent variables are denoted in the column headings and each cell reports estimates from different regressions. For each model specification, see footnote under Table 5. Households with total monthly crop-plot size of smaller than 1 acre (2.53 Rai) on average over the pre-reform months are excluded. Whether symptom(s) (1) equals one if any member of household reported having symptom(s) in past month, while (3) is the corresponding variable for symptoms which limit ability to work/study. Column (2) measures fraction of total number of days within a month that a member reported having symptom(s). Column (4) is the corresponding variable for symptoms that affect ability to work/study.

Table 8: Impacts on Welfare Outcomes

Dependent Variables:	(1) Total consumption	(2) Non-durable consumption	(3) Loans-assets ratio	(4) Whether default on loans	(5) Fraction of loans repaid
(A) Baseline:					
Short-run	80.77	26.230	-0.005	-0.014	0.011
	(134.51)	(91.87)	(0.004)	(0.010)	(0.009)
Long-run	47.84	18.62	-0.013*	-0.018**	0.010
	(134.96)	(111.53)	(0.007)	(0.009)	(0.009)
(B) Village Fund Control:					
Short-run	81.60	26.87	-0.004	-0.015	0.013
	(135.08)	(91.62)	(0.004)	(0.010)	(0.009)
Long-run	47.20	18.04	-0.013*	-0.018**	0.010
	(135.20)	(111.85)	(0.007)	(0.009)	(0.009)
(C) Discrete Risk Types:					
(C2) Short-run:					
Medium Risk	-239.78	-93.81	-0.001	-0.015	0.018
	(207.68)	(182.42)	(0.014)	(0.017)	(0.018)
High Risk	97.94	56.51	-0.014	-0.051**	0.044**
	(265.73)	(202.68)	(0.012)	(0.021)	(0.020)
(C2) Long-run:					
Medium Risk	-208.38	-177.68	-0.001	-0.031*	0.032*
	(270.50)	(242.70)	(0.020)	(0.018)	(0.018)
High Risk	-4.30	-19.72	-0.039**	-0.050**	0.035*
	(305.02)	(260.03)	(0.019)	(0.020)	(0.019)
Number of Observations	48,913	48,915	49,430	12,464	12,464

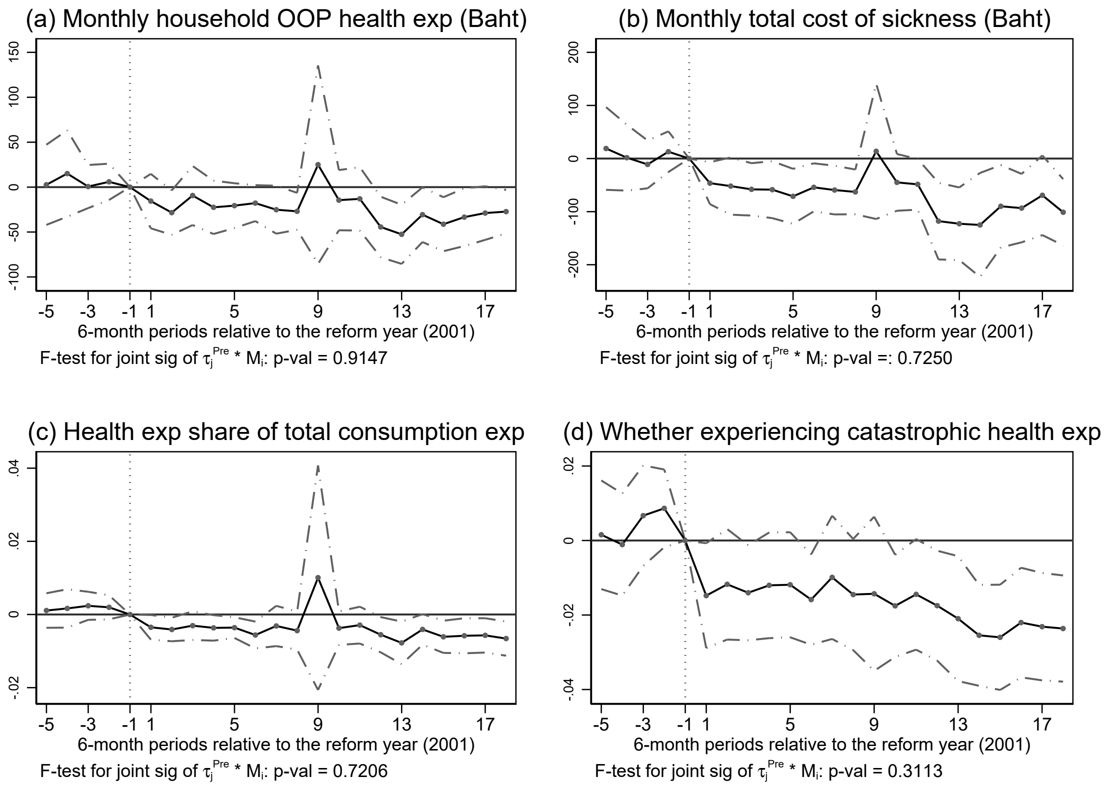
Note: Robust standard errors, clustering on households, in parentheses: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. The dependent variables are denoted in the column headings and each cell reports estimates from different regressions. For each model specification, see footnote under Table 6. Households with total monthly crop-plot size of smaller than 1 acre (2.53 Rai) on average over the pre-reform months are excluded. Total durable and non-durable consumption (1) and Total non-durable consumption (2) exclude out-of-pocket health expenditures and are trimmed at the top and bottom 0.5% within the pre- and post-reform periods. Loans-assets ratio (3) is the total value of outstanding loans divided by the total value of assets. The outcomes in column (4) and (5) are conditional on the months in which households are obliged to repay their loans.

Figure 1: Potential Demand for Health Care & 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: Event Study on Health Expenditure and Associated Risks Outcomes



Note: Each graph plots the estimated coefficients of the interaction between half-year period dummies (τ_j) and treatment intensity (M_i) for each half-year period relative to 2001 (the reform year), along with the 95% confidence intervals. $\tau_j^{Pre}; j \in \{-5, -4, -3, -2, -1\}$ are 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. The p-value of the F-test for joint significance of the coefficients on $\tau_j^{Pre} \times M_i$ in specification (3) are reported.

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Online Appendix (Not for Publication)

A Modeling the effect of the health insurance on saving and portfolio allocation

The model presented in Section 3 considers a household that solves[74]

$$\max_{x,s} F(x, s; \alpha) \overset{\text{def}}{=} v(y - s) + \delta E u((\widetilde{f}(x) + g(s - x)) - \widetilde{OOP}). \quad \text{(A.1)}$$

where $\widetilde{f}(x)$ and $\widetilde{OOP}$ are two independent binary risks:

$$\widetilde{f}(x) = [f_L(x), f_H(x); \pi_L, \pi_H], \text{ and } \widetilde{OOP} = [(1-\alpha)\Delta, 0; \pi_B, \pi_G].$$

Thus, there are two harvest states for risky crops: H and L, and there are two health states for the household: G and B. It is assumed that the solution (x^*, z^*) is interior. We will initially assume that there are constant returns to scale in harvest production ($f_j'' = g'' = 0$, $j = L, H$). At the end of the appendix, we will indicate how decreasing returns to scale affects results.

When presenting the results below, reference will be made to the income behaviour of the coefficient of absolute risk aversion, $R^a(c) \overset{\text{def}}{=} -\frac{u''(c)}{u'(c)}$, and of absolute prudence, $P(c) \overset{\text{def}}{=} -\frac{u'''(c)}{u''(c)}$. Decreasing absolute risk aversion (DARA, $R^{a\prime}(c) < 0$) is considered to be a very weak assumption on risk preferences and is implied by constant relative risk aversion (CRRA). Prudence is necessary for DARA. Decreasing absolute prudence (DAP) means $P^{a\prime}(c) < 0$ (Kimball, 1990) and is implied by constant relative prudence (CRP). CRRA implies CRP, DARA and DAP;[75] it is a useful empirical benchmark. Experimental support for prudence is given by Noussair *et al.* (2014), Deck and Schlesinger (2014) and Ebert and Wiesen (2011). 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).

We are interested in x_α and s_α. Since α works through the OOP-payment, these are two income effects:

$$x_\alpha = \frac{\partial x}{\partial m_B}\Delta, \text{ and } s_\alpha = \frac{\partial s}{\partial m_B}\Delta,$$

where m_B stands for an exogenous income transfer in health state B.

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[74] The model is an extension of 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.

[75] CARA means $u(c) = \frac{1-\exp(-ac)}{a}$ and therefore $R^a(c) = P^a(c) = a$, and $R^r(c) = P^r(c) = ac$ CRRA means $u(c) = \frac{c^{1-\rho}-1}{1-\rho}$. Then $R^r(c) = \rho$, $P^r(c) = \rho + 1$, $R^a(c) = \frac{\rho}{c}$, and $P^a(c) = \frac{\rho+1}{c}$.

A.1 A decomposition result

In period 0, the farmer solves

$$\max_{x,s} F(x,s;\alpha) = v(y-s) + \delta E u((\widetilde{f}(x) + g(s-x)) - \widetilde{OOP}). \tag{A.2}$$

It is useful to break down this problem in two stages. In the 1st stage, the household decides on total investment s. In the 2nd stage, it decides on the amount invested into risky crops x—the residual amount is invested in riskless crops.

We start with the 2nd stage problem. Since x does not appear in period 0 utility, this is similar to the portfolio problem that Arrow (1965) analysed where a given initial wealth (here s) needs to be allocated over two assets. The first order condition w.r.t. investment in cash crops is

$$F_x = E\widetilde{u}'(\widetilde{f}' - g') = 0, \tag{A.3}$$

and the second order condition is (recall that we assume CRS)

$$F_{xx} = E\widetilde{u}''(\widetilde{f}' - g')^2 < 0. \tag{A.4}$$

The optimal value of x will change with s according to

$$\frac{\partial x}{\partial s}|^A = -\frac{F_{xs}}{F_{xx}} = (-\frac{1}{F_{xx}})E\widetilde{u}''(\widetilde{f}' - g')g',$$

where $|^A$ indicates that this is the derivative for the Arrow problem.

In the 1st stage, the household chooses s. The first order condition is the Euler equation

$$F_s = E\widetilde{u}'g' - \frac{v'}{\delta} = 0. \tag{A.5}$$

Before giving the second order condition, we need to acknowledge that x will also affect the value of F_s with

$$F_{sx} = E\widetilde{u}''(\widetilde{f}' - g')g'.$$

Therefore the second order condition for s becomes

$$
\begin{aligned}
SOC_s &= E\widetilde{u}''g'^2 + \frac{v''}{\delta} + E\widetilde{u}'(\widetilde{f}' - g')g'\frac{\partial x}{\partial s} \\
&= E\widetilde{u}''g'^2 + \frac{v''}{\delta} + \left(-\frac{1}{F_{xx}}\right)\left(E\widetilde{u}''(\widetilde{f}' - g')g'\right)^2 \\
&= \frac{1}{F_{xx}}\left[\frac{v''}{\delta}E\widetilde{u}''(\widetilde{f}' - g')^2 + E\widetilde{u}''g'^2E\widetilde{u}''(\widetilde{f}' - g')^2 - \left(E\widetilde{u}''(\widetilde{f}' - g')g'\right)^2\right] \\
&= \frac{1}{F_{xx}}\left[\frac{v''}{\delta}E\widetilde{u}''(\widetilde{f}' - g')^2 + \underbrace{E\widetilde{u}''\widetilde{f}'^2E\widetilde{u}''g'^2 - \left(E\widetilde{u}''\widetilde{f}'g'\right)^2}_{CSD}\right] \qquad \text{(A.6)}
\end{aligned}
$$

The first term inside square brackets is positive due to risk aversion and $v'' \leq 0$. The difference between the 2nd and the 3rd term is positive due to the Cauchy-Schwartz inequality for expectations. (A.4) then guarantees that $SOC_s < 0$. For later reference, we refer to the difference between the 2nd and the 3rd term as CSD ("Cauchy-Schwartz difference").

Also for later reference, it is useful to note that the marginal propensity to consume out of income y in period 0 is given by

$$
\begin{aligned}
MPC &= 1 - \frac{\mathrm{d}s}{\mathrm{d}y} = 1 - \left(-\frac{1}{SOC_s}\right)F_{sy} \\
&= 1 + \left(-\frac{1}{SOC_s}\right)\frac{v''}{\delta} \\
&= 1 - \frac{F_{xx}}{\left[\frac{v''}{\delta}F_{xx} + CSD\right]}\frac{v''}{\delta} \\
&= \frac{CSD}{\frac{v''}{\delta}F_{xx} + CSD}, \qquad \text{(A.7)}
\end{aligned}
$$

and therefore the marginal propensity to save is

$$
1 - MPC = \frac{\frac{v''}{\delta}F_{xx}}{\left[\frac{v''}{\delta}F_{xx} + CSD\right]}. \qquad \text{(A.8)}
$$

We can now ask how the investment policy (x, s) will be affected by a change in a generic parameter p. To find the effect on x, we must acknowledge the indirect effect through s:

$$
\begin{aligned}
\frac{\mathrm{d}x}{\mathrm{d}p} &= \frac{\partial x}{\partial p}|^A + \frac{\partial x}{\partial s}|^A\frac{\partial s}{\partial p} \\
&= \frac{\partial x}{\partial p}|^A + \frac{\partial x}{\partial s}|^A\left(-\frac{1}{SOC_s}\right)\left(F_{sp} + F_{sx}\frac{\partial x}{\partial p}|^A\right) \\
&= \left(1 + \frac{1}{SOC_sF_{xx}}F_{sx}^2\right)\frac{\partial x}{\partial p}|^A - \frac{F_{sx}}{F_{xx}}\left(-\frac{1}{SOC_s}\right)F_{sp},
\end{aligned}
$$

where the third inequality follows from rearranging and $\frac{\partial x}{\partial s}|^A = -\frac{F_{xs}}{F_{xx}} = -\frac{F_{sx}}{F_{xx}}$. From (A.6), we have that $SOC_s F_{xx} = \left[\frac{v''}{\delta}F_{xx} + CSD\right]$. Therefore

$$\begin{aligned}\frac{\mathrm{d}x}{\mathrm{d}p} &= \frac{\frac{v''}{\delta}F_{xx} + CSD + F_{sx}^2}{\frac{v''}{\delta}F_{xx} + CSD}\frac{\partial x}{\partial p}|^A + \left(-\frac{F_{sx}}{F_{xx}}\right)\left(-\frac{1}{SOC_s}\right)F_{sp} \\ &= \frac{\frac{v''}{\delta}F_{xx}}{\frac{v''}{\delta}F_{xx} + CSD}\frac{\partial x}{\partial p}|^A + \frac{1}{\frac{v''}{\delta}F_{xx} + CSD}\left[\left(CSD + F_{sx}^2\right)\frac{\partial x}{\partial p}|^A + F_{xs}F_{sp}\right].\end{aligned}$$

But $CSD + F_{sx}^2$ can be written as

$$\begin{aligned}CSD + F_{sx}^2 &= E\widetilde{u}''\widetilde{f}'^2 E\widetilde{u}''g'^2 - \left(E\widetilde{u}'\widetilde{f}'g'\right)^2 + \left(E\widetilde{u}''(\widetilde{f}' - g')g'\right)^2 \\ &= E\widetilde{u}''\widetilde{f}'^2 E\widetilde{u}''g'^2 - \left(E\widetilde{u}''\widetilde{f}'g'\right)^2 \\ &\quad + \left(E\widetilde{u}''\widetilde{f}'g'\right)^2 - 2E\widetilde{u}''\widetilde{f}'g'E\widetilde{u}''g'^2 + \left(E\widetilde{u}''g'^2\right)^2 \\ &= E\widetilde{u}''g'^2\left(E\widetilde{u}''\widetilde{f}'^2 - 2E\widetilde{u}''\widetilde{f}'g' + E\widetilde{u}''g'^2\right) \\ &= E\widetilde{u}''g'^2 E\widetilde{u}''(\widetilde{f}' - g')^2 \\ &= E\widetilde{u}''g'^2 F_{xx} \\ &= F_{ss}F_{xx},\end{aligned}$$

and $\frac{\partial x}{\partial p}|^A$ as $-\frac{F_{xp}}{F_{xx}}$. Therefore

$$\begin{aligned}\frac{\mathrm{d}x}{\mathrm{d}p} &= \frac{\frac{v''}{\delta}F_{xx}}{\frac{v''}{\delta}F_{xx} + CSD}\frac{\partial x}{\partial p}|^A + \frac{1}{\frac{v''}{\delta}F_{xx} + CSD}\left[-F_{ss}F_{xp} + F_{xs}F_{sp}\right] \\ &= (1 - MPC)\frac{\partial x}{\partial p}|^A + MPC\left[\frac{F_{xs}F_{sp} - F_{ss}F_{xp}}{CSD}\right], \qquad \text{(A.9)}\end{aligned}$$

where the second equality follows from (A.7) and (A.8). This leaves us to identify the meaning of the square bracket term on the RHS.

Consider the following utility maximisation problem:

$$\max_{s,x} \delta Eu((\widetilde{f}(x) + g(s - x)) - \widetilde{OOP}) - \nu s. \qquad \text{(A.10)}$$

This problem (or its equivalent with discounted expected utility multiplied with the "price for utility" $\frac{1}{\nu}$) is called the profit maximisation problem for the household's intertemporal choice (Browning *et al.* 1985). The parameter ν is a virtual exogenous cost of investment funds. This problem can also be coined the Frisch problem because its solution gives the Frisch demands for total and risky investment.[76] It is exactly as the original problem, except that the marginal cost of funds is now fixed at $\frac{\nu}{\delta}$ rather than endogenously determined as $\frac{v'(\cdot)}{\delta}$. Thus, the first order condition for x is exactly as before and

[76] Frisch demands are demands written as a function of prices and the marginal utility of income. See Browning *et al.* (1985).

the one for s is now

$$E\widetilde{u}'g' - \frac{\nu}{\delta} = 0. \tag{A.11}$$

In consequence, the comparative statics expressions are as before but now without the term $\frac{v''}{\delta}$. From the first line of (A.9) it then follows that $\frac{1}{CSD}[F_{xs}F_{sp} - F_{ss}F_{xp}]$ stands for the Frisch effect of p on x, $\frac{\partial x}{\partial p}|^F$, revealing also that CSD corresponds to the determinant of the Hessian of the profit maximisation problem. This means that the comparative statics effect (A.9) can be written as

$$\frac{\mathrm{d}x}{\mathrm{d}p} = (1 - MPC)\frac{\partial x}{\partial p}|^A + MPC\frac{\partial x}{\partial p}|^F. \tag{A.12}$$

Before summarising our results, note that the effect of the generic parameter p on the total amount of investment s, is simply

$$\frac{\mathrm{d}s}{\mathrm{d}p} = MPC\frac{\partial s}{\partial p}|^F, \tag{A.13}$$

because s is not a choice variable in the Arrow problem.

Result 1 For a generic parameter p, the effect on the optimal investment in risky crops x can be decomposed into an Arrow term and a Frisch term, with as weights the marginal propensity to save and to consume, respectively—as in (A.12). The effect on the total investment s is given by the Frisch effect, multiplied with the MPC—as in (A.13).

Intuitively, total investment is used to satisfy the Euler equation of intertemporal utility maximisation. Once expected marginal utility is tied down by s, investment is optimally diversified. Eliminating MPC in (A.12) by means of (A.13), the 2nd RHS term of (A.12) can be written as $\frac{\partial x}{\partial p}|^F \left(\frac{\partial s}{\partial p}|^F\right)^{-1} \frac{\partial s}{\partial p}$, or just as $\frac{\partial x}{\partial s}|_{\mathrm{d}Eu'=0} \times \frac{\partial s}{\partial p}$. With quasi-linear preferences, $v'' = 0$ and (A.12) reduces to $\frac{\partial x}{\partial p}|^F$.

The easiest way to obtain the Frisch comparative statics effects is to totally differentiate the system given by (A.3) and (A.11) and rearrange:

$$\begin{pmatrix} \mathrm{d}x \\ \mathrm{d}s \end{pmatrix} = -\frac{1}{CSD}\begin{pmatrix} E\widetilde{u}''g'^2 & -E\widetilde{u}''(\widetilde{f}' - g')g' \\ -E\widetilde{u}''(\widetilde{f}' - g')g' & E\widetilde{u}''(\widetilde{f}' - g')^2 \end{pmatrix} \times \begin{pmatrix} \partial E\widetilde{u}'(\widetilde{f}' - g')/\partial p \\ \partial E\widetilde{u}'g'/\partial p \end{pmatrix} \mathrm{d}p. \tag{A.14}$$

We can now explain the income effects that make up $x_\alpha = \frac{\partial x}{\partial m_B}\Delta$ and $s_\alpha = \frac{\partial s}{\partial m_B}\Delta$.

A.2 Income effects from m_B

From Result 1 we have that the income effect of m_B is given by

$$\frac{\partial x}{\partial m_B} = (1 - MPC) \times \frac{\partial x}{\partial m_B}|^A + MPC \times \frac{\partial x}{\partial m_B}|^F.$$

The Arrow term $\frac{\partial x}{\partial m_B}|^A$ is the effect of an increase in m_B on x *for a given amount of total investment* s:

$$\begin{aligned}
\frac{\partial x}{\partial m_B}|^A &= \left(-\frac{1}{F_{xx}}\right) E\widetilde{u}''_B(\widetilde{f}' - g')\pi_B \\
&= \left(-\frac{1}{F_{xx}}\right) \left[\pi_L u''_{BL}(f'_L - g') + \pi_H u''_{BH}(f'_H - g')\right] \pi_B \\
&= \left(-\frac{1}{F_{xx}}\right) \left[-\pi_H u''_{BL}\frac{E\widetilde{u}'_H}{E\widetilde{u}'_L}(f'_H - g') + \pi_H u''_{BH}(f'_H - g')\right] \pi_B \\
&= \left(-\frac{1}{F_{xx}}\right) \pi_H E\widetilde{u}'_H(f'_H - g') \left[-\frac{u''_{BL}}{E\widetilde{u}'_L} + \frac{u''_{BH}}{E\widetilde{u}'_H}\right] \pi_B \\
&= \left(-\frac{1}{F_{xx}}\right) \pi_H E\widetilde{u}'_H(f'_H - g') \left[-\frac{u''_{BL}}{u'_{BL}}\frac{u'_{BL}}{E\widetilde{u}'_L} + \frac{u''_{BH}}{u'_{BH}}\frac{u'_{BH}}{E\widetilde{u}'_H}\right] \pi_B \\
&= \left(-\frac{1}{F_{xx}}\right) \pi_H E\widetilde{u}'_H(f'_H - g') \left[R^a_{BL}\frac{u'_{BL}}{E\widetilde{u}'_L} - R^a_{BH}\frac{u'_{BH}}{E\widetilde{u}'_H}\right] \pi_B. \qquad \text{(A.15)}
\end{aligned}$$

It is clear that the sign is given by that of the large square bracket term. This term can be rewritten as

$$\frac{u'_{BL}}{E\widetilde{u}'_L}\left(R^a_{BL} - R^a_{BH}\right) + R^a_{BH}\pi_G\frac{u'_{GL}u'_{GH}}{E\widetilde{u}'_L E\widetilde{u}'_H}\left(\frac{u'_{BL}}{u'_{GL}} - \frac{u'_{BH}}{u'_{GH}}\right). \qquad \text{(A.16)}$$

Without any background health risk, marginal utilities are independent of the health state and we are left with the first round bracket term $R^a_{BL} - R^a_{BH}$, which is positive under DARA. This is the standard wealth effect identified by Arrow (1965) on the demand for a risky asset, except that it now concerns wealth in health state B. Expression (A.16) shows that the presence of a background risk adds an extra term whose sign is given by the big round bracket term.

Claim 1 The ratio $\frac{u'_{Bj}}{u'_{Gj}}$ can be approximated as $1 + R^a_{Gj} \times OOP$.

Proof of claim 1. Since $u'_{Bj} = u'((f_j + g) - (1-\alpha)\Delta) = u'((f_j + g) - OOP)$ and $u'_{Gj} = u'(f_j + g)$, we can take a first order Taylor approximation of u'_{Bj} around $c_{Gj} = f_j + g$:

$$\begin{aligned}
u'_{Bj} &\simeq u'_{Gj} + u''_{Gj} \times [(f_j + g) - OOP - (f_j + g)] \\
&= u'_{Gj} - u''_{Gj} \times OOP
\end{aligned}$$

Therefore

$$\frac{u'_{Bj}}{u'_{Gj}} \simeq 1 + R^a_{Gj} \times OOP. \ \blacksquare$$

Therefore, the large round bracket term in (A.16) is approximately

$$(R^a_{GL} - R^a_{GH}) \times OOP. \qquad \text{(A.17)}$$

This expression, positive under DARA, reinforces the Arrow effect to the extent that the household bears any OOP-payment when ill.

Returning to the sign of (A.16), we obtain

Result 2 The Arrow effect $\frac{\partial x}{\partial m_B}|^A$ is positive under DARA.

The Frisch term $\frac{\partial x}{\partial m_B}|^F$ informs on how the investment policy is affected by m_B *if expected marginal utility is kept constant*, i.e., along the Euler equation. Using (A.14) for $p = m_B$, we get

$$\begin{aligned}\frac{\partial x}{\partial m_B}|^F &= -\frac{1}{CSD}\left(E\widetilde{u}''g'^2 E\widetilde{u}''_B(\widetilde{f}' - g') - E\widetilde{u}''(\widetilde{f}' - g')g'E\widetilde{u}''_B g'\right)\pi_B \\ &= \frac{1}{CSD}\left(E\widetilde{u}''\widetilde{f}'g'E\widetilde{u}''_B g' - E\widetilde{u}''g'^2 E\widetilde{u}''_B\widetilde{f}'\right)\pi_B.\end{aligned}$$

Expanding the expectations over the different states of the world gives

$$\frac{\partial x}{\partial m_B}|_F = \frac{1}{CSD}\pi_B\pi_G\pi_L\pi_H\left(f'_H - f'_L\right)\left(u''_{BL}u''_{GL} - u''_{BH}u''_{GH}\right)g'^2,$$

the sign of which is given by the final round bracket term, which can be written as

$$u''_{GL}u''_{GH}\left(\frac{u''_{BL}}{u''_{GL}} - \frac{u''_{BH}}{u''_{GH}}\right). \tag{A.18}$$

This term vanishes if there is no background health risk at all (then $u''_{Bj} = u''_{Gj}$, $j = H, L$). A similar argument as in Claim 1 shows that

$$\left(\frac{u''_{BL}}{u''_{GL}} - \frac{u''_{BH}}{u''_{GH}}\right) \simeq (P^a_{GL} - P^a_{GH}) \times OOP, \tag{A.19}$$

which is positive under DAP.

Result 3 The Frisch effect $\frac{\partial x}{\partial m_B}|^F$ has the same sign as (A.19) and is positive under DAP.

The income effect on total investment is more straightforward:

$$\frac{\partial s}{\partial m_B} = MPC \times \frac{\partial s}{\partial m_B}|^F,$$

with

$$\frac{\partial s}{\partial m_B}|_F = \frac{1}{CSD}\left(-E\widetilde{u}''\left(\widetilde{f}' - g'\right)\widetilde{f}'E\widetilde{u}''_B g' + E\widetilde{u}''\left(\widetilde{f}' - g'\right)g'E\widetilde{u}''_B\widetilde{f}'\right)\pi_B.$$

Expanding the expectations over the different states of the world gives

$$\frac{\partial s}{\partial m_B}|_F = \frac{1}{CSD}g'\left(f'_H - f'_L\right)\pi_L\pi_H[E\widetilde{u}''_L u''_{BH}\left(f'_L - g'\right) - E\widetilde{u}''_H u''_{BL}\left(f'_H - g'\right)]\pi_B. \tag{A.20}$$

Since $f'_H > g' > f'_L$, this expression is clearly negative.

Result 4 The Frisch effect $\frac{\partial s}{\partial m_B}|^F$ is given by (A.20) and is negative. Hence the total effect $\frac{\partial s}{\partial m_B}$ is negative.

A.3 Income effects from m

The effects of an unconditional rise in period 1 income are useful to assess the effects of α when the health insurance premium is actuarially adjusted at the margin, i.e., $\mathrm{d}m = -\mathrm{d}P = -\pi_B \Delta \mathrm{d}\alpha$.

Let m_G stand for an exogenous income transfer in health state G. Similar derivations as in the previous section show that

$$\begin{aligned} \frac{\partial x}{\partial m_G}|^A &= \left(-\frac{1}{F_{xx}}\right) E\widetilde{u}''_B(\widetilde{f}' - g')\pi_G \\ &= \left(-\frac{1}{F_{xx}}\right) \pi_H E\widetilde{u}'_H(f'_H - g') \left[R^a_{GL}\frac{u'_{GL}}{E\widetilde{u}'_L} - R^a_{BH}\frac{u'_{GH}}{E\widetilde{u}'_H}\right]\pi_G, \end{aligned} \tag{A.21}$$

with the square bracket term being positive under DARA.

Adding (A.15) and (A.21) then gives

$$\frac{\partial x}{\partial m}|^A = \left(-\frac{1}{F_{xx}}\right) \pi_H E\widetilde{u}'_H(f'_H - g') \left[\left(\frac{-Eu''_L}{E\widetilde{u}'_L}\right) + \left(\frac{-Eu''_H}{E\widetilde{u}'_H}\right)\right].$$

The big round bracket term is positive under DARA because the indirect utility function $Eu(\cdot)$ (with expectations over the the health states) inherits DARA from $u(\cdot)$ (see Gollier (2000: 115)).

The Frisch effects are

$$\begin{aligned} \frac{\partial x}{\partial m}|^F &= \left(\frac{-1}{CSD}\right)\left[E\widetilde{u}''g'^2 E\widetilde{u}''(\widetilde{f}' - g') - E\widetilde{u}''(\widetilde{f}' - g')g'E\widetilde{u}''g'\right], \text{ and} \\ \frac{\partial s}{\partial m}|^F &= \left(\frac{-1}{CSD}\right)\left[-E\widetilde{u}''(\widetilde{f}' - g')g'E\widetilde{u}''(\widetilde{f}' - g') + E\widetilde{u}''(\widetilde{f}' - g')^2 E\widetilde{u}''g'\right], \end{aligned}$$

and simplify to

$$\begin{aligned} \frac{\partial x}{\partial m}|^F &= 0, \text{ and} \\ \frac{\partial s}{\partial m}|^F &= -\frac{1}{g'}. \end{aligned}$$

The effect on total investment only consists of Frisch effects, and is given by

$$\begin{aligned} \frac{\partial s}{\partial \alpha}|_{\mathrm{d}P=\pi_B\Delta\mathrm{d}\alpha} &= MPC\left(\frac{\partial s}{\partial m_B}|^F - \frac{\partial s}{\partial m}|^F\ \pi_B\right)\Delta \\ &= \frac{MPC}{CSD}\pi_H\pi_L g'(f'_H - f'_L)\pi_G[Eu''_L\left(u''_{GH} - u''_{BH}\right) + Eu''_H\left(u''_{GL} - u''_{BL}\right)]\pi_B\Delta \end{aligned}$$

Under prudence ($u''' > 0$) the square bracket term, and therefore $\frac{\partial s}{\partial \alpha}|_{\mathrm{d}P=\pi_B\Delta\mathrm{d}\alpha}$ is negative. This squares with the standard result that a mean-preserving contraction of a background risk reduces the need for precautionary savings (Kimball, 1990).

Result 5 The effect of an actuarially fair expansion in HI coverage on total investment is positive under prudence.

The effect on investment in risky cash crops is less transparent. The reason is that it is a weighted average of an Arrow effect and a Frisch effect:

$$\frac{\partial x}{\partial \alpha}|_{\mathrm{d}P=\pi_B\Delta\mathrm{d}\alpha} = \left((1-MPC)\left[\frac{\partial x}{\partial m_B}|^A - \frac{\partial x}{\partial m}|^A \pi_B\right] + MPC\frac{\partial x}{\partial m_B}|^F\right)\Delta \tag{A.22}$$

The square bracket term is the Arrow effect of a mean-preserving contraction in the background risk. It can be shown to equal

$$\frac{\partial x}{\partial \alpha}|^A_{\mathrm{d}P=\pi_B\Delta\mathrm{d}\alpha} = \left(\frac{-1}{F_{xx}}\right)\pi_G\pi_H(f'_H - g')\left(\frac{u''_{BH}-u''_{GH}}{Eu'_H} - \frac{u''_{BL}-u''_{GL}}{Eu'_L}\right)\pi_B\Delta. \tag{A.23}$$

Prudence makes both ratios in the big round bracket term positive. Temperance ($u'''' < 0$) makes the first numerator less than the second one, and risk aversion (u'') makes the first denominator smaller than the second one. Hence the sign of (A.23) is ambiguous. From Result 3, we know that DAP ensures a positive sign for $\frac{\partial x}{\partial m_B}|^F$. Since the numerator of the weight $1-MPC$ involves $\frac{v''}{\delta}$, while that of weight MPC does not, there is no reason to expect that considering the entire weighted sum is going to provide more insight.

Result 6 The effect of an actuarially fair expansion in HI coverage on risky crop investment is ambiguous.

A.4 Decreasing returns to scale in crop investment

So far, we have assumed constant returns in investment. Decreasing returns to scale will add an extra term to the income effects. We can explore the sign of this extra term by assuming away the foreground harvest risk. Then both the Arrow term and Frisch terms vanish. For an interior solution we now need, in addition to $u'' < 0$, at least one of the following inequalities to hold strictly: $f'' \leq 0$, $g'' \leq 0$. Optimal crop diversification then requires $f'(x) = g'(s-x)$, while the optimal savings decision is governed by $E\widetilde{u}'g' = \frac{v'}{\delta}$. For a given x, an increase in m_B lowers $E\widetilde{u}'g'$ and to restore optimality, s must fall. If $g'' < 0 = f''$, the fall in s necessitates an equally large fall in x for the diversification condition to be preserved. Then z remains unchanged. If $f'' < 0 = g''$, the fall in s does not upset the diversification condition. Then x remains unchanged. If $g'' < 0$ and $f'' < 0$, x must fall to restore the diversification. Since f' has increased, so has g', which means that its argument, z, must have fallen.

Result 7 This DRS term for either x or s is negative for the income effect of m_B.

The different results can now be collected in Table A1 for the case of DARA and DAP.

Table A1: SUMMARY OF THE EFFECTS ON x AND s OF A MARGINAL INCREASE IN α WHEN $u(\cdot)$ HAS DARA AND DAP

	$\frac{\partial x}{\partial m_B}\Delta$	$\frac{\partial s}{\partial m_B}\Delta$
Arrow	+ (2)	0
Frisch	+ (3)	− (4)
DRS	− (7)	− (7)

Note: reference to results in parentheses.

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B 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.[77] Our health variables correspond to the objective health measures in the literature which mainly are reported symptoms and diagnosed health conditions. 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.[78]

From the visit- and symptom-level individual health data, 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 within 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;

(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 hospitalised due to a reported symptom, we use the number of days hospitalised instead of the

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

[78]Individuals also reported detailed information of each inpatient and outpatient visit to health care facility including reasons for the visit, types and location of facility, the number of days hospitalised, 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. 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.

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/her 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.[79] These households experienced more frequent symptoms, were more likely to have members with chronic and disability conditions, and had higher rate of service utilisation. These in turn lead to higher standardised morbidity index generated by PCA for the high-risk households.

We do not include measures such as medical expenditure or those captured purely by health care utilisation. 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.[80] Our baseline survey also contains information on whether individuals can perform various activities of daily living (ADL) and self-rated general health condition.[81] 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

[79] 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 5.1 for details.

[80] We acknowledge that diagnosis of chronic conditions (such as diabetes), which we include in the PCA analysis, may also depend on health care use. However, we find that excluding the chronic conditions variables do not lead to significantly different estimation results.

[81] ADL information in the baseline interview includes whether individuals can carry heavy loads, sweep the floor, walk for 5 kilometers, take water from a well, kneel or stoop, cloth themselves, go to toilet without help, and stand from sitting on the floor. For self-rated health status, all individuals were asked to rate whether their current health condition is very healthy, somewhat healthy, somewhat unhealthy, or unhealthy, but again only in the baseline interview (month 0).

and self-rated health status.

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 pre-reform 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 A3, we illustrate that any pair of these different PCA indices and the baseline generate scatter plots that produce a pattern that lies quite along the common 45-degree line from the origin.

C Interpreting the Reform Effects

In the empirical model, the parameter (β) that is identified is the differential effect of universal health insurance (the reform) with a one-unit increase in the morbidity index M_i on various outcome variables Y_{it}. For example, because the inter-quartile range of the standardised 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 health insurance 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 health insurance. 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 health insurance is zero for households with morbidity index $M_i = M_{\min}$; namely, health insurance has no effect on households with the smallest health risk. Under this assumption, combined with the functional form assumption that the effect of universal health insurance is linear in the morbidity index, we have

$$E(Y_{it}^1 - Y_{it}^0 \mid t) = \int E(Y_{it}^1 - Y_{it}^0 \mid M_i, t) dF(M_i) = \int \beta(M_i - M_{\min}) dF(M_i) = \beta E(M_i - M_{\min}) \quad \text{(A.24)}$$

where the second equality holds given the two assumptions made above.[82] Given our empirical model (equation (1)), we have that

$$E(Y_{it}^1 - Y_{it}^0 \mid M_i, t) = \beta M_i + \alpha,$$

where α cannot be identified as it collinears with δ_t. Note that we have made an assumption that, at $M_i = M_{\min}$, $\beta M_{\min} + \alpha = 0$. Therefore, we have $\alpha = -\beta M_{\min}$, and so

$$E(Y_{it}^1 - Y_{it}^0 \mid M_i, t) = \beta(M_i - M_{\min})$$

[82] The assumption of linearity is obviously strong (equation (2)). However, if this is a concern, we can easily allow for the effect of the universal insurance to be nonlinear in M_i, by including nonlinear terms of M_i interacted with the Post dummy when estimating equation (2).

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Therefore, once scaled up by the mean of $M_i - M_{\min}$, our identified parameter can be interpreted as the ATE of the reform. Because our morbidity index is standardised to have mean zero and standard deviation of one, $E(M_i) = 0$ and it follows that $ATE = -\beta M_{\min}$. From data and PCA, we have $M_{\min} = -1.31$. So, the estimated ATE is the estimated effect β scaled up by a third.

D Variable Construction and Definition

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.[83] We estimate the impact of health insurance on various outcomes. Table 2 and A3 provide the summary statistics of all the control variables and outcomes of interest for our sample of households separately for those whose health risk during the pre-reform period was above and below the median risk.[84] 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.

D.1 Health

In the health module, the baseline survey collects retrospective information of past 12-month illnesses and treatments, as well as current chronic conditions and mental/physical disabilities in August 1998. The follow-up monthly updates then collect monthly records of all symptoms suffered by each household member together with their duration and severity. For each inpatient and outpatient visit, we have detailed information on reasons for the visit, types and location of health facilities, the number of days hospitalised, treatments and medicines costs as well as financing methods for each household member.

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

[83] For more details, see the Townsend Thai Project website: http://townsend-thai.mit.edu/.

[84] For each variable, we report and compare the pre-reform variable means and standard deviations of the low-demand (below-median) households to those of the high-demand (above-median) households, where low-demand households are those predicted to have relatively lower potential demand for health care, and vice versa. 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-demand (low-demand) households are those whose morbidity index ranks above (below) the median value. We outline how the PCA is carried out in details in Section 4.2.1 and Appendix B.

reported symptoms and accidents cases. Note that the expenditure for households that did not report visiting any health facility or spend nothing on health care 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 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 expenditure 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 risk associated with medical expenditure.

D.2 Cultivation

In the cultivation module, the uses of inputs and harvests are recorded at the crop-plot level (each land plot of each crop grown by a household). Households in our data grew a wide variety of crops, of which we can classify into 24 main types: rice, rice seedlings, glutinous rice/seedlings, maize, sorghum, grass, tapioca, sunflower, chilli, lemongrass, galangal, sesame, sugar cane, nuts, sapodilla plum, mango, coconut, banana, custard apple, mulberry, eucalyptus, other fruits, other vegetables, and perennial plants/trees. In total, the data contain about 20,110 crop-plots, from which we can construct a monthly panel at the crop-plot level for each household separately. We then sum up all crop-plots of the same household to arrive at the household-level information and build an entire cultivation portfolio for each household to trace it overtime.

For each crop-plot, we define the *duration* of the crop-plot as the number of months from when households started preparing the crop-plot until it is harvested (can occur several times) and finished. We use a 'flow value' approach in calculating the monthly costs of inputs and outputs for each crop-plot.[85] We first sum up all the (real) cost or harvest values across each month throughout the duration of the crop-plot and divide it by the duration (in months) of the crop-plot. We then equally distribute

[85] A similar approach is described and adopted in Samphantharak and Townsend (2010) for calculating the monthly values of cultivation inputs and outputs in household financial accounting.

this average value across all months throughout the duration of the crop-plot to obtain the monthly flow value. It is worth noting that households often grow several types of crops in each month. As shown in the Appendix Figure A1, the duration of crop-plot can vary from one month to several years depending on the type of crops. The median duration for our sample is 5 months. For all the crop-plots observed in the baseline interview (month 0), we have retrospective information of when households started preparing the crop-plot, and so their duration can be deduced.

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.[86] 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 unorganised manner (e.g. scattered around the house, circling a fish pond, along the fence). We exclude these plots from our analysis and only use organised crop plots that households report non-missing plot size to generate all the cultivation outcomes. Note that the outcome variables related to cultivation activities are constructed based on excluding observations associated with total monthly crop-plot size smaller than 1 acre (2.53 Rai) on average over the pre-reform months.

Cultivation Expenditures. Total cultivation expenditures include the cost of using different types of inputs including seeds, fertilisers, pesticides, hired labour, equipment rentals, transport and storage costs but excluding the opportunity cost of family labour to be defined below. Seeds expenditure comprises costs of hormones, seeds, and seedlings. Fertilisers include chemical fertilisers and manure, while pesticides incorporate herbicide, fungicide and insecticide. Equipment rentals are expenditures on the use of equipment and animals that do not belong to households in cultivation activities. Transport and storage costs includes costs for fuel, repair and maintenance of machinery and equipment, and transportation, transformation and storage of goods and inputs. For each crop plot in each month, we have detailed records of each plot operation (e.g. land preparation, use of itemised inputs, labour tasks) with all associated transaction costs and value of the inputs used in each operation. Total cultivation expenditure for each household is calculated as the monthly sum of all these plot-specific operation costs across all the crop plots owned by household plus the monthly costs for rental, fuel, repair and maintenance of machinery and equipment, and transportation, transformation and storage of goods and inputs. For each category of the cost items, we calculate them using the flow value approach described above.

Opportunity Cost of Family labour. The 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 labours 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

[86] Approximately 1 acre is equivalent to 2.53 Rai.

National Wage Committee.

Harvest Value. For each crop-plot of each household, using the flow value approach, we first calculate the sum of the (real) harvest values produced by each crop-plot across all the months within the duration of the crop-plot and divide it by its duration (in months). This average harvest value is then equally assigned to all the months throughout the duration of each crop-plot so that we obtain the flow value of harvests for each crop-plot. The total harvest value is therefore the sum of these flow value of harvests across all crop-plots for each household in each month.

Net Cultivation Income. Net cultivation income is defined as the total harvest value minus total cultivation expenditure and the opportunity cost of family labour in each month.

Portfolio of Cultivation. We focus on outcomes such as 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 6.3 for more details of how this volatility in output can be calculated from our data.

D.3 Income, Consumption & Loans

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.

Consumption. The consumption module in the Townsend Thai monthly 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 Monthly Household Financial Accounting dataset that are constructed directly from the Townsend Thai Monthly Surveys.[87] It is the sum of total consumption expenditure and the value

[87]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/.

of home production on all non-durable goods less health-related expenditures. We also consider net expenditure on household durables which 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.

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. This fraction is capped at 1 in rare cases 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. 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. Loans are the sum of household borrowings and account payables, where borrowings include both formal borrowings from financial institutions and informal borrowings from friends and relatives, whereas account payables comprises housing mortgages, sales of goods in advance and purchases of goods on credits. Assets are composed of cash, receivables, deposits, lending, inventories, livestock, land, and fixed assets in the household balance sheet. We also consider an alternative definition 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. 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, and thus does not reflect households' repayment decisions. In this case, we directly use the loan information from the Borrowing module of the monthly surveys.

E Cost-benefit Analysis

To understand the economic magnitude of our estimates, we examine whether the differential costs of financing the reform are outweighed by the benefits in this section. The universal health insurance provision is effectual if the benefit-cost ratio, the average value of benefits on households per one Baht of government's average expenses on the program, is greater than 1. All our calculations are in 2004 constant prices.[88]

Cost Calculation. We assume that the pre-reform cost of providing public health care only comes from financing the MWS program for the poor to which 23% of households in our sample were entitled.[89] The MWS program costed the government around 263 Baht annually per eligible person (Gruber, Hendren, and Townsend, 2014), thus translating into an average pre-reform cost of 61 Baht per person in our sample (0.23*263). With the pre-reform average size of household of 4.22 members, this is equivalent to 255 Baht per household annually. The reform was accompanied by significant annual increases in real terms in budget per capita for the 30 Baht scheme, averaging 1,341 Baht and 1,918 Baht over the short- and long-run periods.[90] As the post-reform average size of household reduced to 4.04 in the short-run and 3.79 in the long-run, the short- and long-run average cost of financing the 30 Baht scheme were thus approximately 5,420 and 7,268 Baht, respectively. Subtracting the pre-reform cost gives the estimated annual differential cost per household of about 5,165 Baht in the short run and 7,013 Baht in the long run. In monthly terms, these are 430 and 584 Baht per month, respectively.[91]

Benefit Calculation. Our identified parameter of the reform impact measures the monthly differential effect on various outcomes that corresponds with a one-unit increase in the morbidity index M_i. In Section 4.2.2, we have shown that this corresponds to ATE when roughly scaled up by a third under the assumptions of linearity and zero effects of health insurance for the household with smallest predicted health risk. Had we relaxed the latter assumption, our estimated ATE would have been scaled up by a smaller factor. We multiply our identified estimates by 1.31 to arrive at the upper bound for this policy relevant parameter or the value of benefit that is comparable to the program financing costs.

Direct Benefits. Our results reveal a monthly differential decline in OOP health expenditure of 21 Baht per household in the short run. Combined with the effect through the health channel quantified as the differential decline in earnings loss due to illness (opportunity cost of illness), this amounts to 53 Baht per household in the short run. In the long run, the corresponding figure is 99 Baht per household.

Indirect Benefits. In the short run, we find that, as households engaged more in investments and riskier crop production, they consequently experienced a differential increase in net cultivation income of 190 Baht per month, which together with the direct benefit leads to the benefit-cost ratio of 0.88 ([52+235]*1.31/430).[92] In the long run, these effects are amplified. Households ultimately enjoyed a

[88] These are obtained from Thailand's Ministry of Commerce CPI data.

[89] This assumes health care costs were fully borne by the previously uninsured (65% of data) and neglects the coverage costs for VHCS households (5%). Note that our sample does not include SSS and CSMBS households (9%).

[90] Annual public health budget data are from Thailand's National Health Security Office Annual report 2008 and 2010.

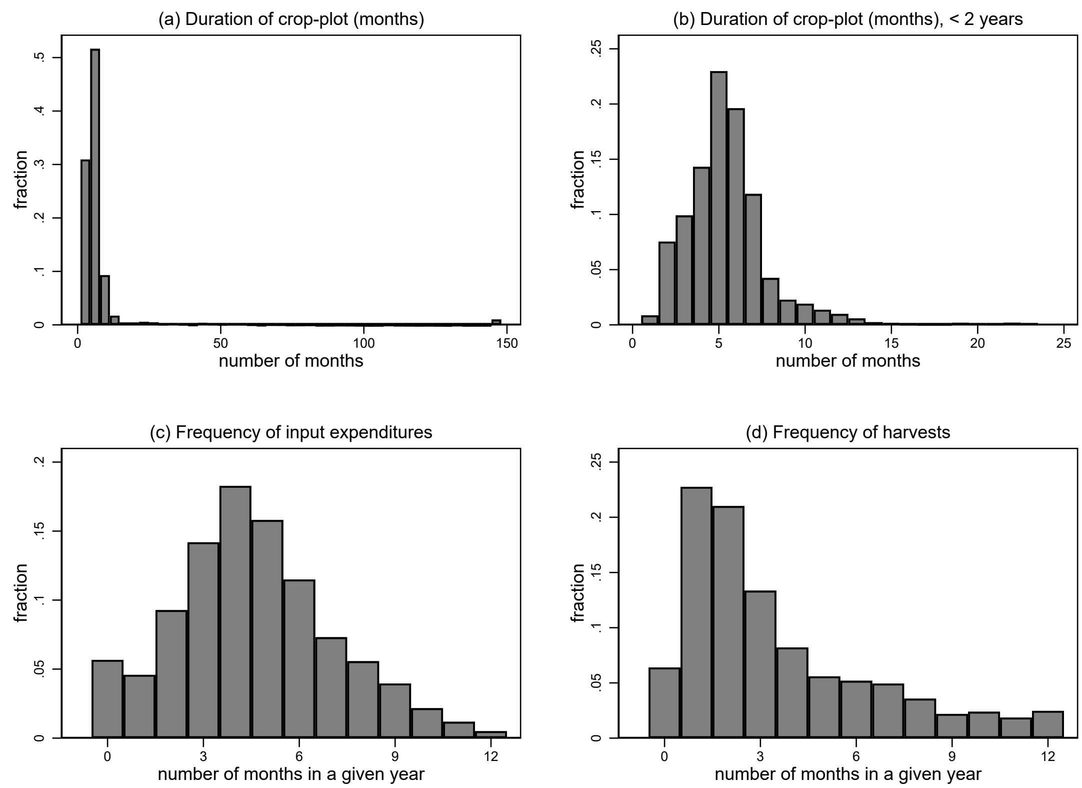
[91] One point to keep in mind is that the above pre-reform program costs do not include government spending at public hospitals for the previously uninsured and the insurance costs for the former VHCS households. Using higher estimates, such as the annual cost of financing the programs in 2000 that were superseded by the 30 Baht scheme of 1,048 per capita as in Limwattananon et al. (2015), would result in even lower estimated differential costs.

[92] One limitation is that the short-run indirect impact of 235 Baht is not precise. It is also estimated based on the cultivating households sub-sample, whereas the direct impact of 58 Baht comes from a larger sample of households.

differential increase in net cultivation income of 892 Baht per month, which translates into the benefit-cost ratio of 2.22 ([99+892]*1.31/584). We conclude that universal health insurance provision through the 30 Baht reform passes the cost-benefit analysis in the context that we study over the long run.

F Appendix Figures

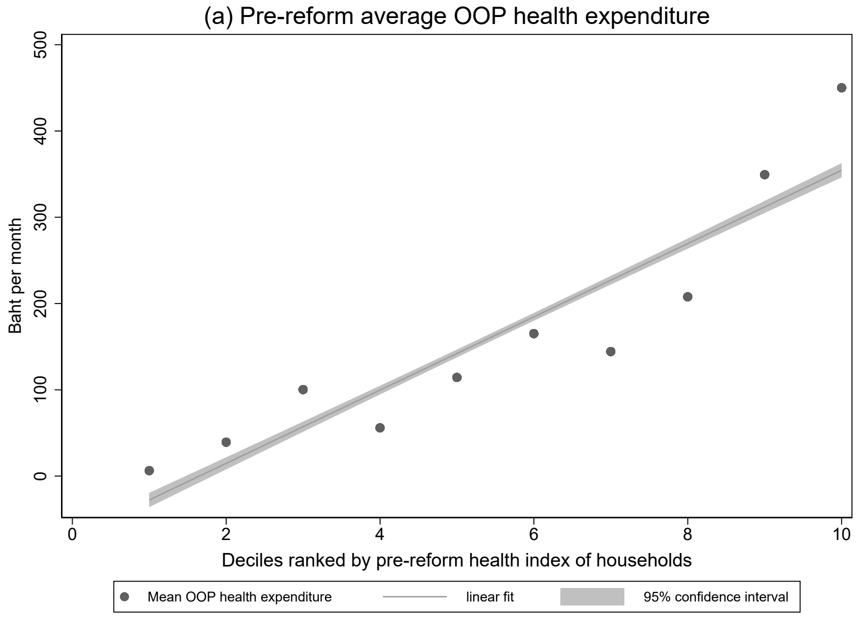
Figure A1: Variations in Duration of Crop-Plot and Frequency of Inputs & Outputs

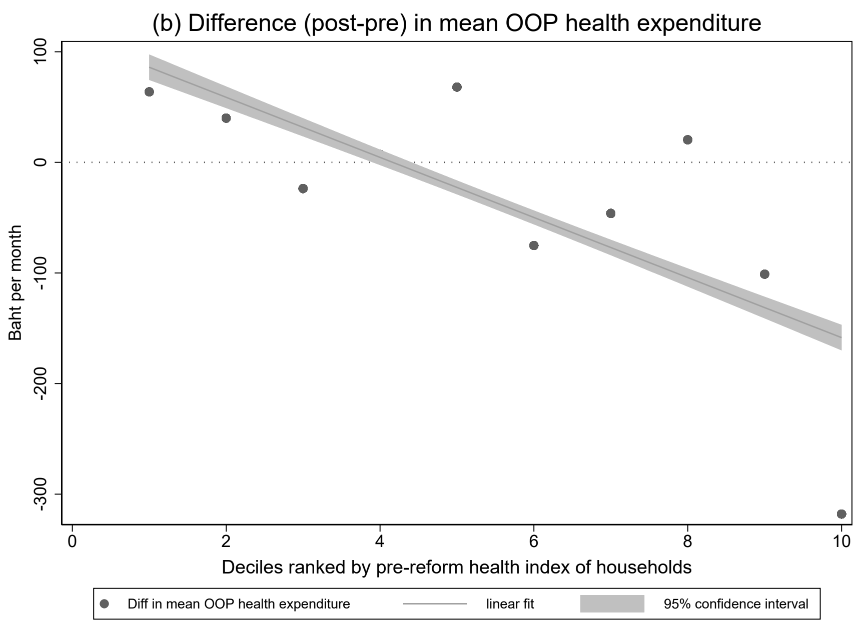


Note: The duration of crop-plot is defined as the number of months from when households start planting the crop until it is finished. Panel (a) shows the histogram of the duration of crop-plot (in months) for all the 20,110 crop-plots in our crop-plot panel. About 95% of them have duration less than 2 years. The median value is 5 months. In panel (b), we only include crop-plots with duration less than 2 years. The frequency of input expenditures (c) and harvests (d) is defined as the number of months in which households had positive cultivation expenditures (c) and harvests (d), respectively, in any given year from 1999–2010 (but excluding 2001, the reform year).

However, we obtain very similar results on health expenditure outcomes and the resulting direct impact when we restrict our sample to cultivating households.

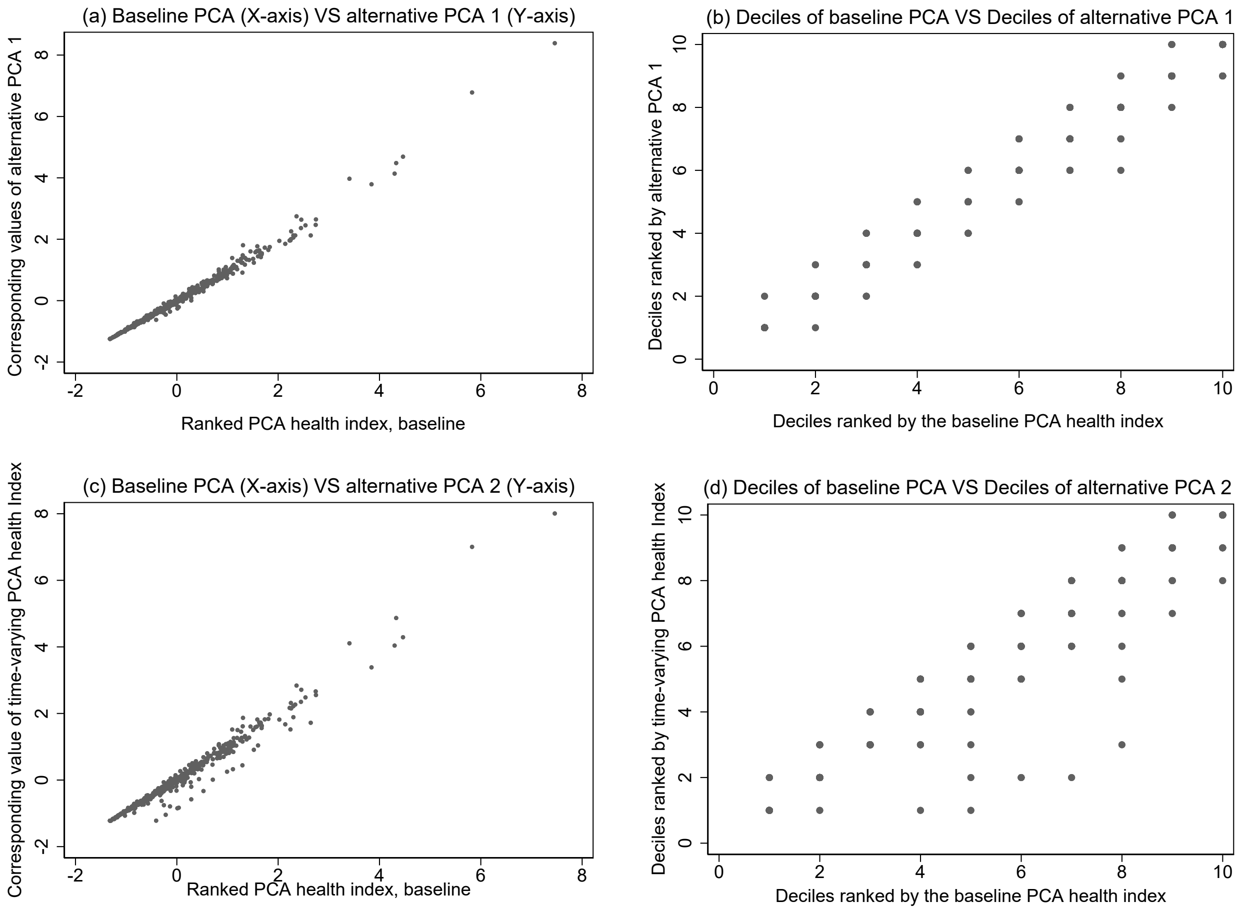
Figure A2: Potential Demand for Health Care & Out-of-pocket Health Expenditure





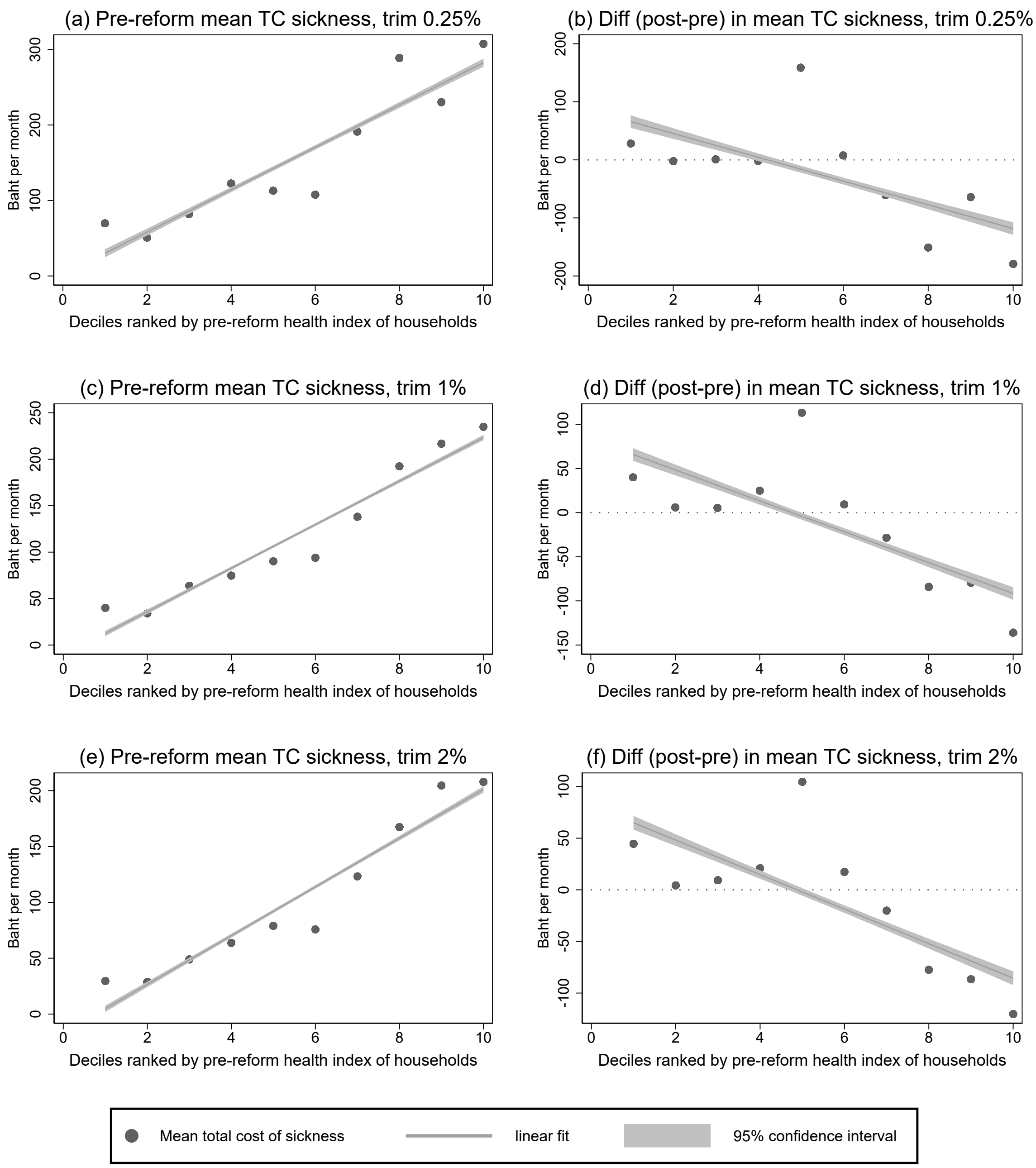
Note: In panel (a), each dot depicts the pre-reform average household out-of-pocket health expenditure across all households in each of the 10 bins ranked by household morbidity index. In panel (b), each dot represents the difference (post- minus pre-) between the average (across households within each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

Figure A3: Scatter Plots of Baseline PCA index Vs Alternatives



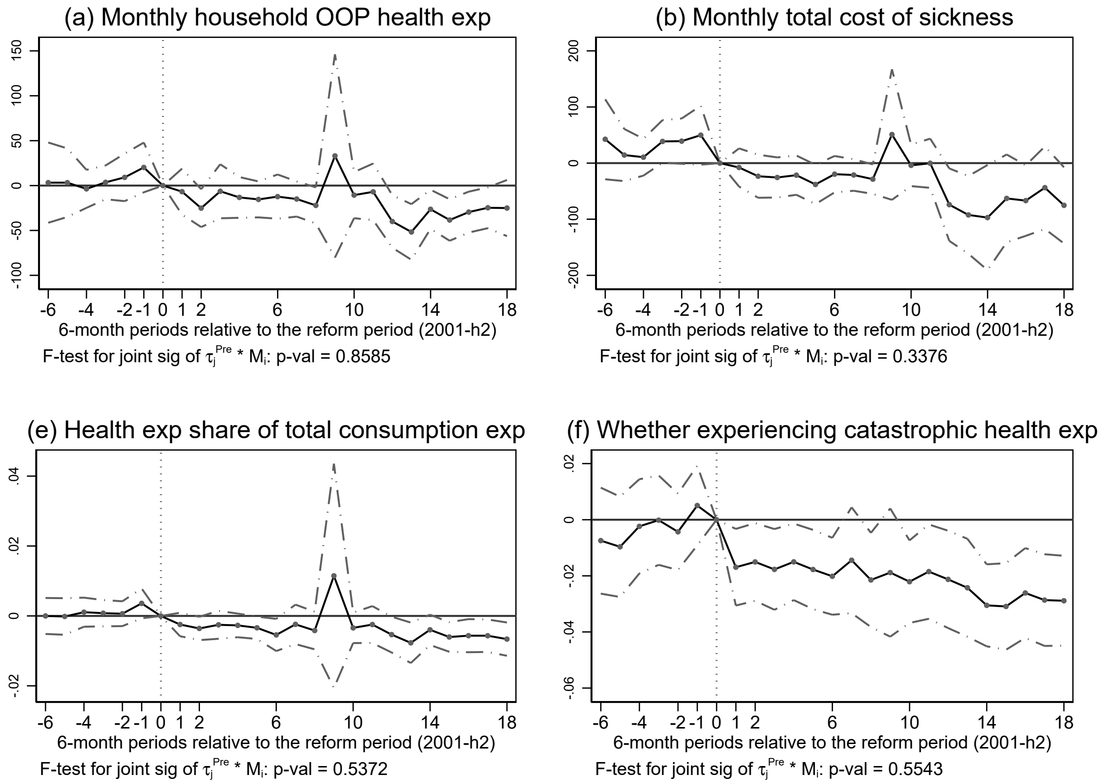
Note: The baseline index is generated from the Principal Component Analysis (PCA) using six health measures as described in Appendix B. These include whether a household member suffered from any symptom, whether a household member had a symptom that prevented daily-life routines, and the duration that 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 A4: Demand for Health Care & Total Cost of Sickness, Outlier Robustness



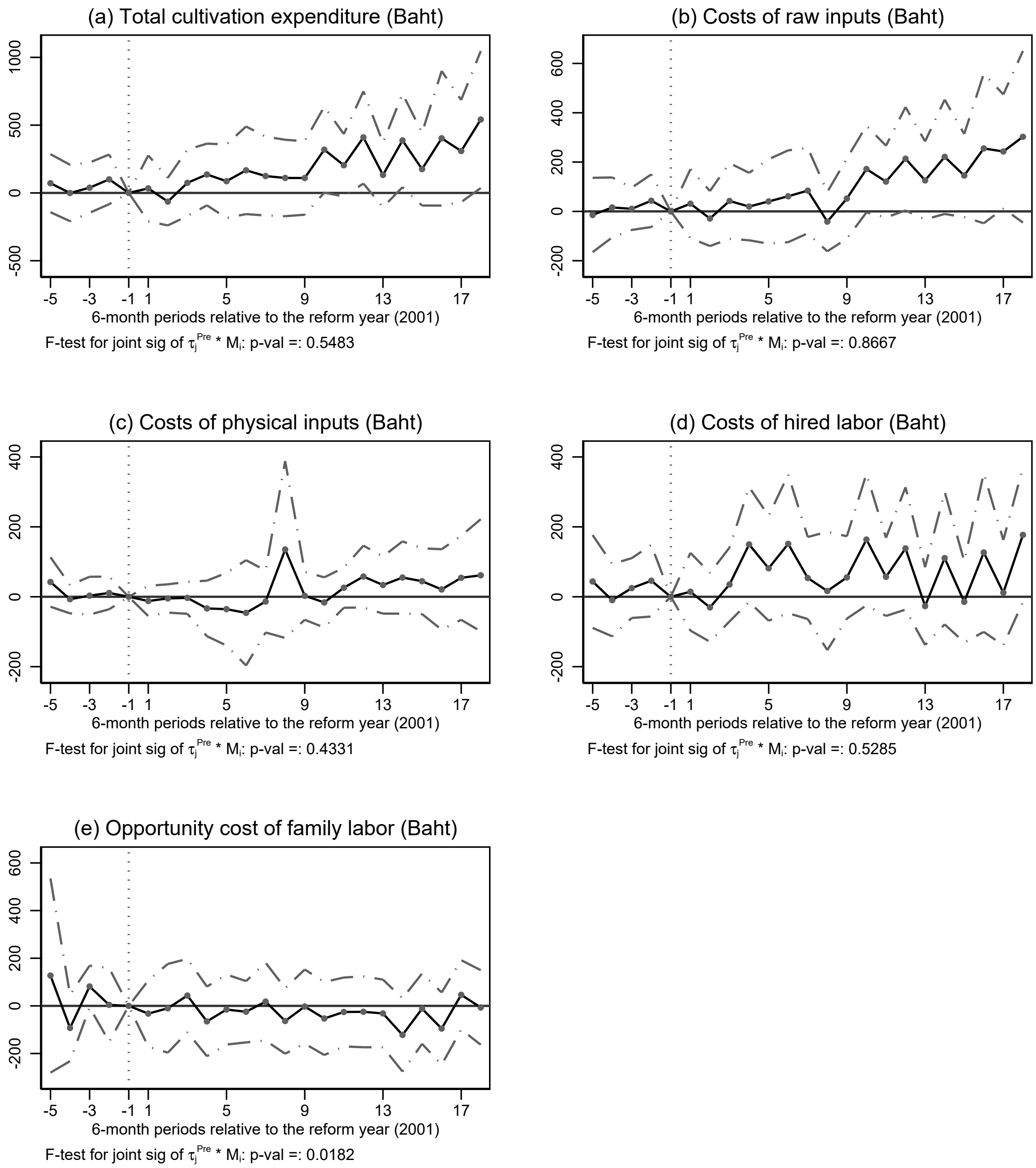
Note: Monthly total cost of sickness is defined as the sum of household out-of-pocket expenditure and the opportunity cost of illness, where household opportunity cost is the sum of individual's loss of days at work resulting from sickness multiplied by his/her average pre-reform earnings. In the left 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 A5: Health Expenditure Outcomes Event Study (inc. 2001-h2 as base)



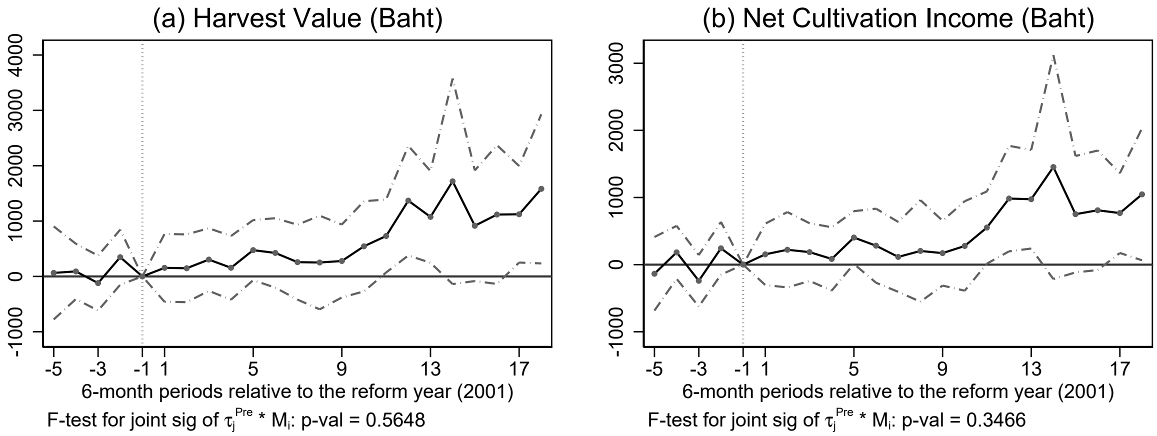
Note: Each graph plots the estimated coefficients of the interaction between half-year period dummies (τ_j) and treatment intensity (M_i) for each half-year period relative to the second half of 2001 (the reform year), along with the 95% confidence intervals. $\tau_j^{Pre}; j \in \{-6, -5, -4, -3, -2, -1\}$ 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. The p-value of the F-test for joint significance of the coefficients on $\tau_j^{Pre} \times M_i$ in specification (3) are reported.

Figure A6: Event Study on Cultivation Investment Outcomes



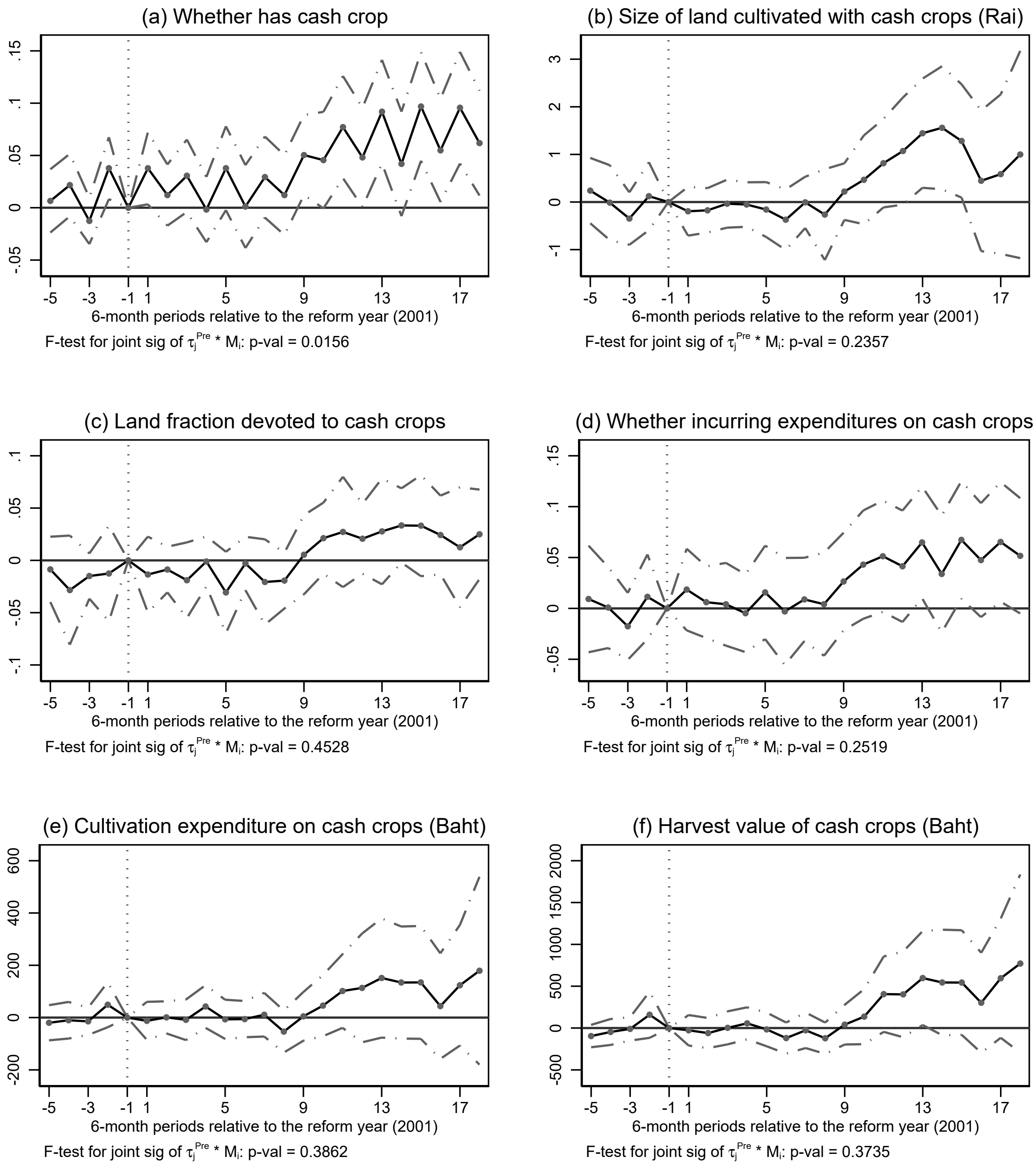
Note: Each graph plots the estimated coefficients of the interaction between half-year period dummies (τ_j) and treatment intensity (M_i) for each half-year period relative to 2001 (the reform year), along with the 95% confidence intervals. $\tau_j^{Pre}; j \in \{-5, -4, -3, -2, -1\}$ are pre-reform 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. The p-value of the F-test for joint significance of the coefficients on $\tau_j^{Pre} \times M_i$ in specification (3) are reported.

Figure A7: Event Study on Cultivation Output Outcomes



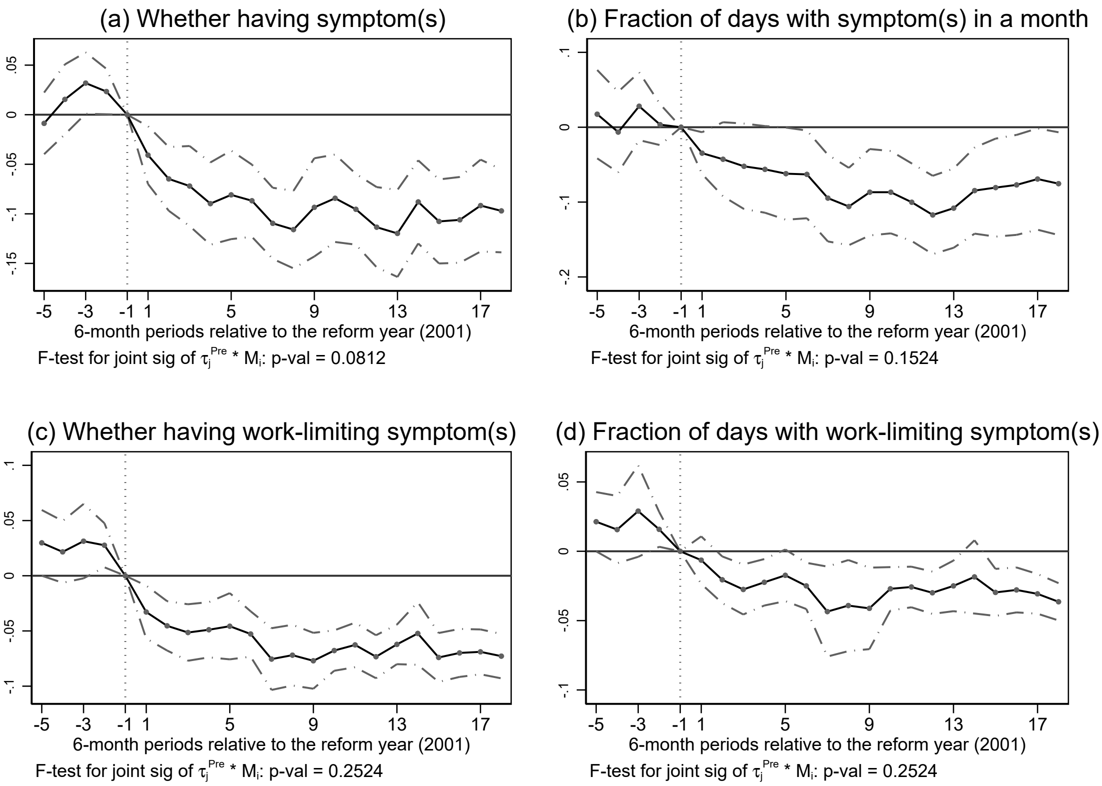
Note: Each graph plots the estimated coefficients of the interaction between half-year period dummies (τ_j) and treatment intensity (M_i) for each half-year period relative to 2001 (the reform year), along with the 95% confidence intervals. $\tau_j^{Pre}; j \in \{-5, -4, -3, -2, -1\}$ are pre-reform 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. The p-value of the F-test for joint significance of the coefficients on $\tau_j^{Pre} \times M_i$ in specification (3) are reported.

Figure A8: Event Study on Cash Crop Outcomes



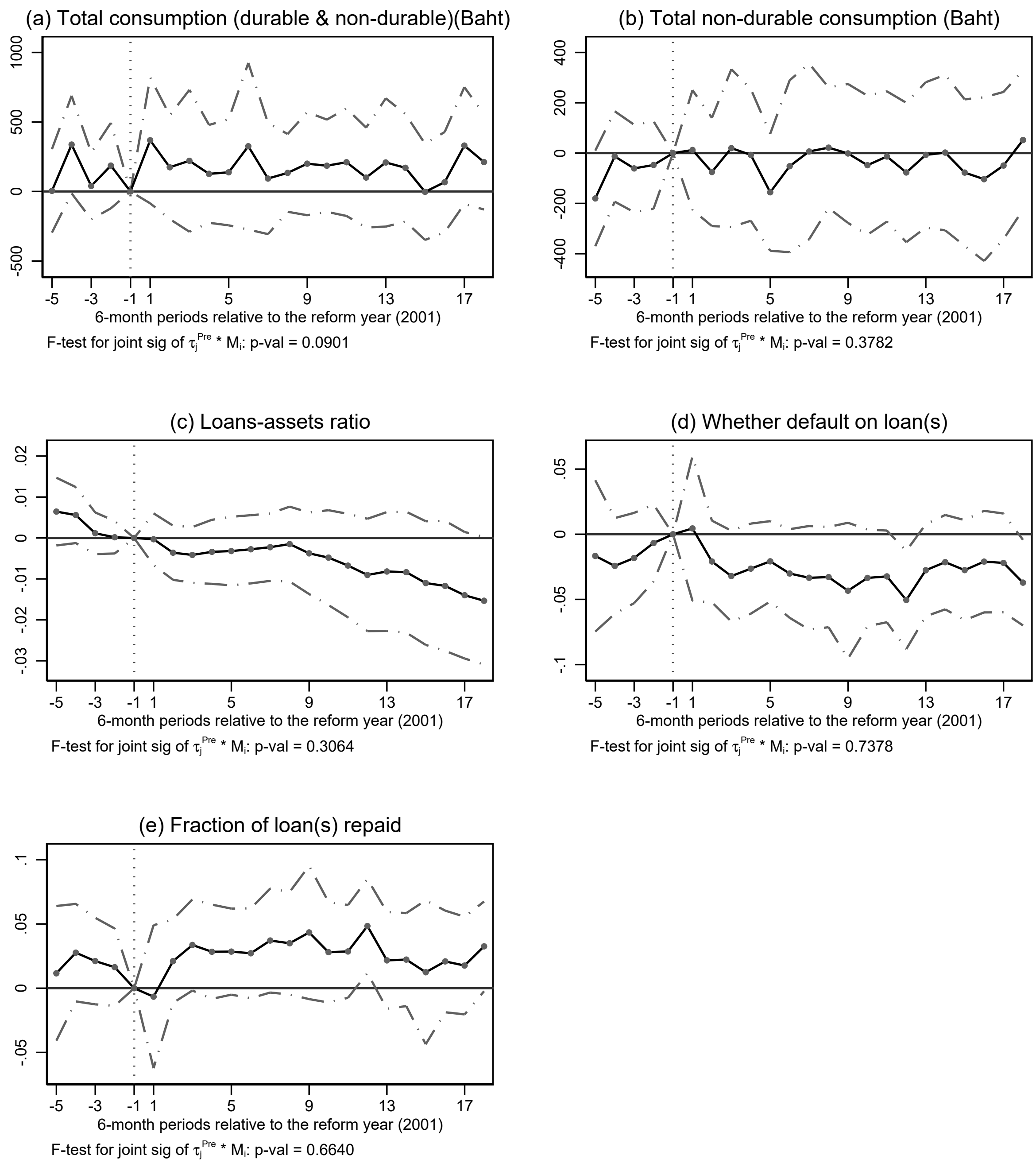
Note: Each graph plots the estimated coefficients of the interaction between half-year period dummies (τ_j) and treatment intensity (M_i) for each half-year period relative to 2001 (the reform year), along with the 95% confidence intervals. $\tau_j^{Pre}; j \in \{-5, -4, -3, -2, -1\}$ are pre-reform 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. The p-value of the F-test for joint significance of the coefficients on $\tau_j^{Pre} \times M_i$ in specification (3) are reported.

Figure A9: Event Study on Health Outcomes



Note: Each graph plots the estimated coefficients of the interaction between half-year period dummies (τ_j) and treatment intensity (M_i) for each half-year period relative to 2001 (the reform year), along with the 95% confidence intervals. $\tau_j^{Pre}; j \in \{-5, -4, -3, -2, -1\}$ are 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. The p-value of the F-test for joint significance of the coefficients on $\tau_j^{Pre} \times M_i$ in specification (3) are reported.

Figure A10: Event Study on Welfare Outcomes



Note: Each graph plots the estimated coefficients of the interaction between half-year period dummies (τ_j) and treatment intensity (M_i) for each half-year period relative to 2001 (the reform year), along with the 95% confidence intervals. $\tau_j^{Pre}; j \in \{-5, -4, -3, -2, -1\}$ are 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. The p-value of the F-test for joint significance of the coefficients on $\tau_j^{Pre} \times M_i$ in specification (3) are reported.

G Appendix Tables

Table A2: Constructing the Adjusted Morbidity Index

	(1)
Dependent Variables:	Morbidity Index or Treatment Intensity
Head's age	-0.0221
	(0.0408)
Head's age squared	0.000284
	(0.000402)
Household size	0.185***
	(0.0408)
Fraction of over-60 elderly	0.904**
	(0.360)
Fraction of under-15 children	-0.243
	(0.363)
Constant	-0.556
	(1.039)
Number of Observations	557
R-squared	0.077

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. 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. The residuals and coefficients obtained from this regression are then used to constructed the adjusted morbidity index.

Table A3: Summary Statistics: Cultivation & Welfare Outcomes

Monthly variables (pre-reform data)	Morbidity index: Below-median	Morbidity index: Above-median	Diff.(p-val)
(C) Cultivation & Net Income			
Total cultivated land (Rai)	14.04	22.59	0.000
	(19.81)	(40.34)	
Total cultivation expenditure (Baht/month)	1,517	2,417	0.000
	(2,808)	(4,362)	
Harvest value (Baht/month)	3,958	6,102	0.000
	(7,820)	(11,256)	
Whether growing cash crop(s)	0.449	0.484	0.000
	(0.497)	(0.5)	
Land fraction devoted to cash crop(s)	0.282	0.313	0.000
	(0.379)	(0.392)	
Net cultivation income (Baht/month)	1,214	2,285	0.000
	(5,888)	(7,957)	
Net farming income (Baht/month)	2,183	3,532	0.000
	(7,305)	(9,571)	
Net operating income (Baht/month)	2,418	2,336	0.905
	(29,076)	(40,101)	
(D) Welfare Outcomes			
Total non-durable consumption (Baht/month)	4,449	4,702	0.058
	(7,017)	(6,787)	
Loans to assets ratio	0.110	0.106	0.282
	(0.198)	(0.182)	
Loan default rate†	0.045	0.063	0.090
	(0.207)	(0.242)	
Fraction repaid out of total loans obliged to repaid†	0.954	0.940	0.137
	(0.199)	(0.227)	
Number of *pre-reform* observations	5,761	5,037	

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) at December 2004 prices. All variables are in monthly format. Above-median (below-median) 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. Variables are conditional on households engaging in cultivation activities.

†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.

Table A4: Impacts on Log Health Expenses

Dependent Variables:	(1) log(1+x) OOP exp	(2) log(1+x) TC sickness
(A) Baseline	-0.348***	-0.442***
	(0.058)	(0.065)
(B) Short- & Long-run Effects		
Short-run Effects	-0.292***	-0.376***
	(0.057)	(0.064)
Long-run Effects	-0.446***	-0.557***
	(0.062)	(0.067)
(C) Village Fund Control	-0.344***	-0.435***
	(0.057)	(0.064)
(D) Discrete Risk Types		
Medium Risk	-0.310***	-0.352***
	(0.091)	(0.093)
High Risk	-0.820***	-1.020***
	(0.104)	(0.105)
Number of Observations	64,888	64,888

Note: Robust standard errors, clustering on households, 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 each model specification, see footnote under Table 4. 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 A5: 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	-57.59	-99.03*	-0.004*	-0.020***
	(48.84)	(53.32)	(0.003)	(0.006)
(B) Short- & Long-run Effects				
Short-run Effects	-51.41	-81.32	-0.003	-0.017***
	(48.11)	(52.10)	(0.003)	(0.006)
Long-run Effects	-68.30	-129.75**	-0.007***	-0.025***
	(52.55)	(59.66)	(0.002)	(0.006)
(C) Village Fund Control	-56.69	-95.91*	-0.004*	-0.020***
	(47.99)	(52.50)	(0.003)	(0.006)
(D) Discrete Risk Types				
Medium Risk	-79.13*	-112.83*	-0.005**	-0.017**
	(45.12)	(65.61)	(0.002)	(0.008)
High Risk	-103.32	-205.08	-0.009*	-0.042***
	(111.78)	(129.76)	(0.006)	(0.010)
Number of Observations	64,994	64,994	64,994	64,994

Note: Robust standard errors, clustering on households, 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 each model specification, see footnote under Table 4. All dependent variables are untrimmed. OOP health expenditure is catastrophic when it accounts for over 10% of household total non-durable expenditure.

Table A6: IMPACTS ON HEALTH EXPENDITURE OUTCOMES, MEAN REVERSION

	(1)	(2)	(3)	(4)
Dependent Variables:	OOP health exp	Cost sickness	OOP exp share	Catastrop. exp
(A) First 24-month PCA	-27.35**	-65.72***	-0.005**	-0.020***
	(10.88)	(17.17)	(0.002)	(0.005)
(B) Last-4-pre-reform-month interactions	-29.84**	-50.36***	-0.006***	-0.018***
	(12.25)	(18.47)	(0.002)	(0.006)
(C) First 22-month PCA	-25.80**	-59.86***	-0.005**	-0.019***
	(10.69)	(16.28)	(0.002)	(0.005)
(D) Last-6-pre-reform-month interactions	-21.75*	-25.64	-0.006**	-0.020***
	(11.98)	(21.06)	(0.002)	(0.007)
Number of Observations ((A) and (C))	64,886	64,886	64,886	64,886

Note: Robust standard errors, clustering on households, 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 ($M_i \times Post_t$). See footnote under Table 4 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. In (B) and (D), we add the interaction terms of post dummy with month-specific treatment intensity, the last 4 months prior to the reform for (B) and the last 6 months for (D), as additional controls, with the whole pre-reform data used to generate the morbidity index. 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 A7: Impacts on More Detailed Types of Cultivation Costs

Dependent Variables:	(1) Seeds	(2) Fertilisers	(3) Pesticides	(4) Equipment Rentals	(5) Transport & Storage
(A) Baseline:					
Short-run	38.77	-12.33	0.35	3.83	-14.15
	(35.06)	(18.26)	(8.21)	(26.31)	(15.18)
Long-run	97.83	78.20**	10.18	10.50	25.76
	(59.93)	(39.10)	(14.06)	(26.42)	(30.92)
(B) Village Fund Control:					
Short-run	48.07	-8.67	1.66	4.56	-11.05
	(33.38)	(18.25)	(8.09)	(26.18)	(14.43)
Long-run	96.25	77.44**	9.95	10.26	25.21
	(60.06)	(39.33)	(14.01)	(26.21)	(31.17)
(C) Discrete Risk Types:					
(C1) Short-run:					
Medium Risk	-87.10	-88.58**	-25.28	-16.34	-47.22*
	(104.03)	(39.09)	(23.99)	(29.21)	(28.36)
High Risk	107.10	-19.56	3.45	-58.10*	-57.92
	(112.70)	(53.95)	(22.86)	(31.55)	(36.05)
(C2) Long-run:					
Medium Risk	-10.44	-32.54	-4.45	-40.87	-62.58
	(113.13)	(84.73)	(44.03)	(47.12)	(82.10)
High Risk	403.52**	252.08**	43.28	-15.47	11.62
	(191.65)	(123.06)	(43.06)	(57.56)	(108.06)
Number of Observations	49,430	49,430	49,430	49,430	49,430

Note: Robust standard errors, clustering on households, 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 each model specification, see footnote under Table 5. Households with total monthly crop-plot size of smaller than 1 acre (2.53 Rai) on average over the pre-reform months are excluded. Seeds expenditure (1) comprises costs of hormones, seeds, and seedlings. Fertilisers (2) include chemical fertilisers and manure, while pesticides (3) include herbicide, fungicide and insecticide. Equipment rentals (4) are expenditures on the use of equipment and animals that do not belong to households in cultivation activities. Transport and storage costs (5) include costs for fuel, repair and maintenance of machinery and equipment, and transportation, transformation and storage of goods and inputs.

Table A8: Impacts on Household Net Incomes

Dependent Variables:	(1) Net total income	(2) Net farming income
(A) Baseline:		
Short-run	1,198.21*	260.33
	(635.73)	(160.07)
Long-run	1,151.83*	1,018.41***
	(587.76)	(383.65)
(B) Village Fund Control:		
Short-run	1,197.73*	295.05*
	(636.69)	(161.97)
Long-run	1,153.15**	1,011.09***
	(585.22)	(390.30)
(C) Discrete Risk Types:		
(C1) Short-run:		
Medium Risk	-250.30	356.61
	(1,273.88)	(467.81)
High Risk	1,939.58**	764.11
	(965.20)	(515.53)
(C2) Long-run:		
Medium Risk	1,696.67	1,644.87**
	(1,355.10)	(815.86)
High Risk	2,911.82**	2,889.84***
	(1,195.49)	(1,027.49)
Number of Observations	49,430	49,430

Note: Robust standard errors, clustering on households, 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 each model specification, see footnote under Table 5. Households with total monthly crop-plot size of smaller than 1 acre (2.53 Rai) on average over the pre-reform months are excluded.

Table A9: Impacts on Different Types of Cultivation Costs for Growing Cash Crops

Dependent Vars:	(1) Seeds	(2) Fertilisers	(3) Pesticides	(4) Hired labour	(5) Equipment Rentals	(6) Transport &Storage
(A) Baseline:						
Short-run	9.12	-0.47	1.01	-2.84	-1.81	-5.85
	(5.94)	(6.73)	(1.59)	(11.43)	(2.50)	(5.94)
Long-run	37.36	41.09	12.02*	8.75	11.01	9.50
	(27.29)	(25.35)	(6.91)	(22.67)	(10.40)	(24.89)
(B) Village Fund Control:						
Short-run	10.48*	0.70	1.32	-0.43	2.01	-3.58
	(6.02)	(6.53)	(1.51)	(11.57)	(2.70)	(5.40)
Long-run	37.07	40.82	11.94*	8.22	11.05	9.02
	(27.34)	(25.46)	(6.93)	(22.83)	(10.41)	(25.23)
(C) Discrete Risk Types:						
(C1) Short-run:						
Medium Risk	-15.42	-27.40	-8.10	-42.26	6.16	-21.72
	(20.53)	(24.53)	(8.57)	(55.42)	(7.10)	(13.70)
High Risk	11.33	-9.07	4.51	-2.89	-18.49	-39.17*
	(23.39)	(23.30)	(5.82)	(45.26)	(11.60)	(23.07)
(C2) Long-run:						
Medium Risk	18.07	-19.89	-1.11	-54.52	7.71	-75.63
	(46.69)	(56.20)	(16.06)	(75.52)	(20.03)	(68.04)
High Risk	130.37	122.59	43.68**	26.362	3.42	-21.18
	(86.28)	(78.04)	(21.12)	(93.51)	(27.21)	(96.06)
No. of Observations	49,430	49,430	49,430	49,430	49,430	49,430

Note: Robust standard errors, clustering on households, 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 each model specification, see footnote under Table 5. Households with total monthly crop-plot size of smaller than 1 acre (2.53 Rai) on average over the pre-reform months are excluded. Seeds expenditure (1) comprises costs of hormones, seeds, and seedlings. Fertilisers (2) include chemical fertilisers and manure, while pesticides (3) include herbicide, fungicide and insecticide. Equipment rentals (5) are expenditures on the use of equipment and animals that do not belong to households in cultivation activities. Transport and storage costs (6) includes costs for fuel, repair and maintenance of machinery and equipment, and transportation, transformation and storage of goods and inputs.

Table A10: Impacts on Health Outcomes, Mean Reversion

Dependent Variables:	(1) Whether symptom(s)	(2) Fraction of days with symptom(s)	(3) Whether severe symp.	(4) Fraction of days severe symptom(s)
(A) First 24-month PCA:				
Short-run	-0.101***	-0.078***	-0.081***	-0.044***
	(0.013)	(0.008)	(0.016)	(0.008)
Long-run	-0.115***	-0.086***	-0.098***	-0.044***
	(0.015)	(0.008)	(0.019)	(0.010)
(B) Last-4-pre-reform-month interactions:				
Short-run	-0.106***	-0.083***	-0.095***	-0.054***
	(0.012)	(0.008)	(0.016)	(0.011)
Long-run	-0.124***	-0.092***	-0.115***	-0.054***
	(0.016)	(0.008)	(0.019)	(0.012)
(C) First 22-month PCA:				
Short-run	-0.100***	-0.077***	-0.081***	-0.043***
	(0.013)	(0.008)	(0.015)	(0.008)
Long-run	-0.112***	-0.084***	-0.097***	-0.044***
	(0.015)	(0.007)	(0.019)	(0.010)
(D) Last-6-pre-reform-month interactions:				
Short-run	-0.094***	-0.073***	-0.091***	-0.050***
	(0.013)	(0.008)	(0.018)	(0.011)
Long-run	-0.111***	-0.082***	-0.111***	-0.050***
	(0.017)	(0.008)	(0.022)	(0.012)
Number of Observations ((A) and (C))	49,380	49,380	49,380	49,380

Note: Robust standard errors, clustering on households, 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. Households with total monthly crop-plot size of smaller than 1 acre (2.53 Rai) on average over the pre-reform months are excluded. All panels report the estimates for the interaction of M_i with the short-run (Jan'01-Dec'06), and with the long-run (Jan'07-Dec'10) post dummy respectively. See footnote under Table 5 for included control variables. In (A) and (C), treatment intensity comes from the morbidity index generated by the first 24 and 22 months of data respectively. In (B) and (D), we add the interaction terms of post dummy with month-specific treatment intensity, the last 4 months prior to the reform for (B) and the last 6 months for (D), as additional controls, with the whole pre-reform data used to generate the morbidity index. The number of observations in (B) and (D) are 48,039 and 47,892 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.