

Estimating the Effects of Universal Health Insurance on Households' Financial Well-being and Investments: Evidence from the Thai 30-Baht Reform

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September 2020

Draft

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1 Empirical Strategies

The goal of this study is to analyze the impact of providing public health insurance on financial well-being. Our empirical strategy mainly exploits the exogenous variation in household potential demand for health services caused by the 30-Baht reform. In a given month, the potential demand for healthcare of households can be predicted by their expected cost of health shock, which in turn is governed by households' self-perception of health risk and healthcare price.¹ *Ceteris paribus*, households that suffer from health shocks more frequently are more likely to perceive themselves as having higher health risk and expected cost of health shock, and thus potentially demand more health services than healthier households. Given that the previously uninsured households faced the same reduction in price of public care following the reform, households with worse health conditions (high-demand type) would therefore experience a larger reduction in their expected cost of health shock and benefit more from the reform relative to healthier households (low-demand type). Such heterogeneity facilitates a treatment-control strategy in estimating the impact of the reform. In this section, we first demonstrate how we predict this potential demand and examine it graphically. Then, we describe our empirical specifications.

1.1 Predicting Potential Demand for Healthcare

We use a history of individual health conditions during the pre-reform months to proxy for households' post-reform health risk and potential demand for healthcare. Given various measures of health, the principal components analysis (PCA) is used to estimate an index of household health status from a linear combinations of the observed health measures richly available in our data.

Principal Components Analysis (PCA). We combine multiple health measures into a parsimonious single health index using the first component of PCA. From the visit- and symptom-level individual data, we first create three health measures that characterize symptoms of household members along with other three measures that indicate the severity of symptoms.² Variables used in PCA are the average value over the 28 pre-reform months for each household of these six health measures:

¹Household expected cost of health shock is defined as the sum of household monthly out-of-pocket health expenditure and the opportunity cost of sickness, where the opportunity cost is measured by earnings loss of all sick individuals in the household. In case the household did not actually use healthcare services, the price of healthcare is the shadow price.

²Only individuals spending at least 15 days over past 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.

- (1) whether a household member suffered from any symptom or visited an outpatient or inpatient facility due to the reported sickness or accident in the past month;
- (2) the sum of all household members' duration of symptoms in the past month;³
- (3) whether a household member suffered from any illness that prevented daily-life routines or were hospitalized due to the reported illness or accident;
- (4) the sum of all members' duration of work-limiting illnesses;⁴
- (5) whether any household member had chronic medical conditions; and
- (6) whether any household member had chronic conditions that limit ability to work or attend school.⁵

Note that while measures (1) - (3) are variables generated from monthly data, measures (5) and (6) only use data from the baseline survey. We have to make an assumption for variables (5) and (6) that chronic conditions for each household are fixed and constant over the pre-reform months because they are observed only in the baseline month 0 and not in subsequent months. For variable (4), given that the baseline survey also records the number of days over past 12 months that each individual's health made him unable to perform primary activities, we extend (4) to incorporate this previous-12-month information at the baseline by taking a time-weighted average over this and the pre-reform periods.⁶

1.2 Specifications

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

$$(\text{PCA Health Index})_i \times (\text{Post-reform Indicator})_t,$$

³The 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.

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

⁵Examples of chronic health conditions specified in the survey include heart disease, diabetes, asthma, high blood pressure, allergies, and chronic malaria.

⁶The 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 28/41.

or $T_i^{Pre} \times Post_t$, where i indicates households and t indexes the month. We call the variable T_i^{Pre} ‘treatment intensity’ to reflect the assumption that households with high health 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. This treatment intensity approach is similar in spirit to that of Bleakley (2007, 2010), Butikofer and Salvanes (2015) and Adhvaryu et al. (2020).

Baseline Specification. We identify the effect of health insurance by comparing the evolution of outcome of interest across households with distinct degree of treatment intensity that is given by household potential demand for healthcare. We estimate the following baseline reduced-form relationship for household i in month t :

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

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

Our identification of the reform impact relies on (a) the exogeneity of the reform and (b) the parallel trend assumption that the evolution of the outcome of interest would have been similar in absence of the reform for households with distinct level of demand for healthcare. Since the 30-Baht reform was implemented nationwide by the government in a rapid and arguably unanticipated fashion in which the households affected did not have a say on their eligibility, it greatly reduces concerns over selection bias whereby coverage might not be exogenous to the outcome variables. Furthermore, our specification include household-level time-varying variables that help control for possible non-parallel trend in outcomes. Suppose, for instance, that the health index differs across households but converge

as household head gets older and eventually become equally less healthy. Then changes in an outcome that is also affected by the age of household head, say OOP health expenditure, would not have been parallel between the low-demand (healthy) and high-demand (unhealthy) households even without the reform. If not controlled for, this correlation between age of household head and the treatment intensity variable would invalidate the parallel trend assumption.

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

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

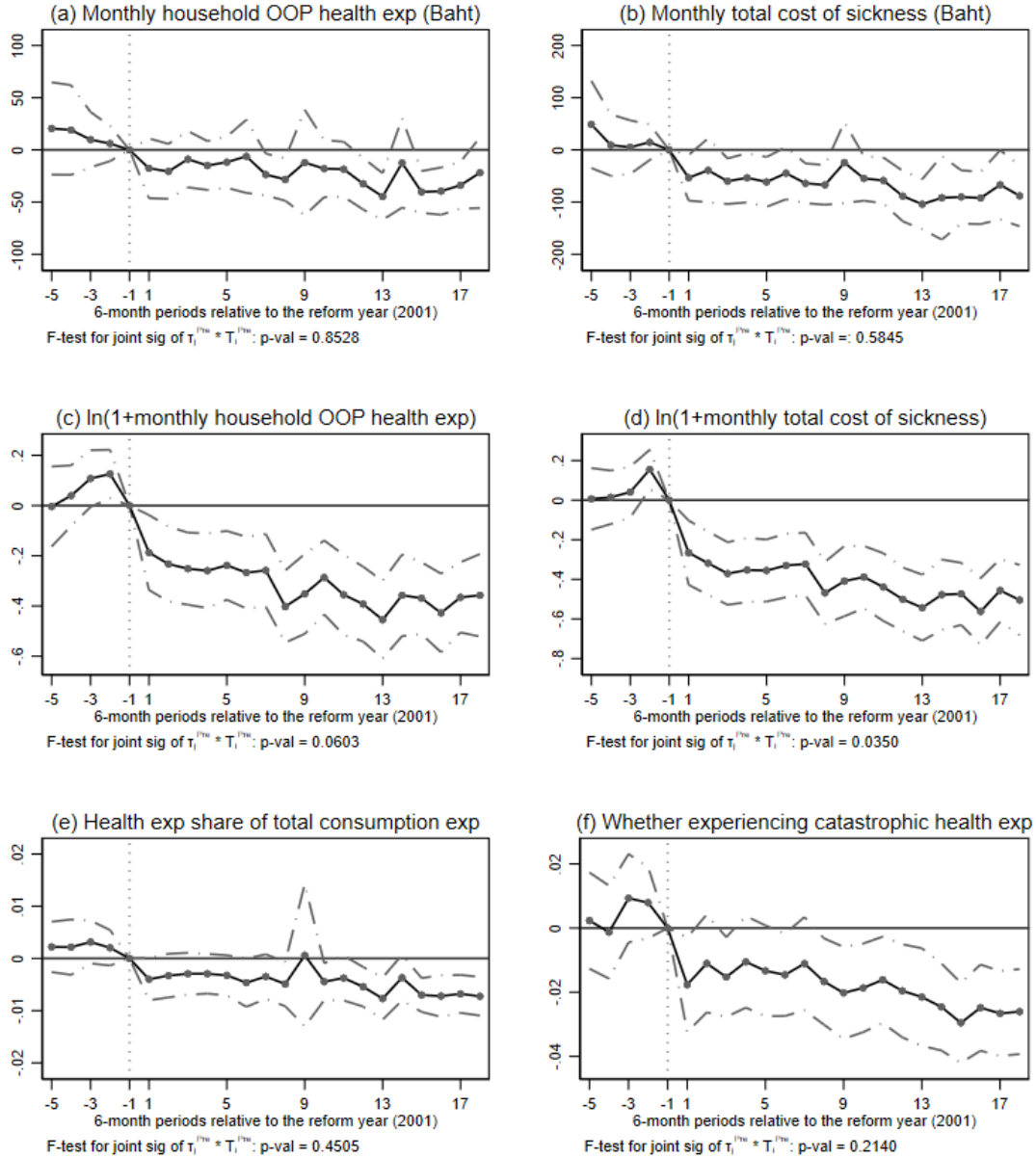
where τ_j^{Pre} 's and τ_j^{Post} 's are respectively the pre-reform and post-reform period dummies for each half-year interval relative to the year of the reform (year 2001).⁷ The omitted half-year period preceding the reform year, period -1 (Jul'00-Dec'00), is the base period. The fixed effects and household controls similar to the baseline specification (1) are included. To test for the extent of pre-reform differential trend, we carry out an F-test for the joint significance of β_j^{Pre} 's with the null hypothesis that $\beta_j^{Pre} = 0 \quad \forall j \in \{-5, -4, -3, -2\}$. A rejection of the null hypothesis would suggest an existence of a pre-trend in the outcome of interest controlling for the household characteristics and fixed effects. For each outcome variable, we also provide an event study diagram which plots each of the estimated coefficients β_j^{Pre} 's and β_j^{Post} 's from specification (2) over all the half-year periods relative to the base period -1.⁸

⁷j is an index of number of periods (in half-year interval) relative to the year of the reform (year 2001). For instance, 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.

⁸The event study graph includes the omitted period -1 at which the coefficient takes the value of zero given that it is the base period.

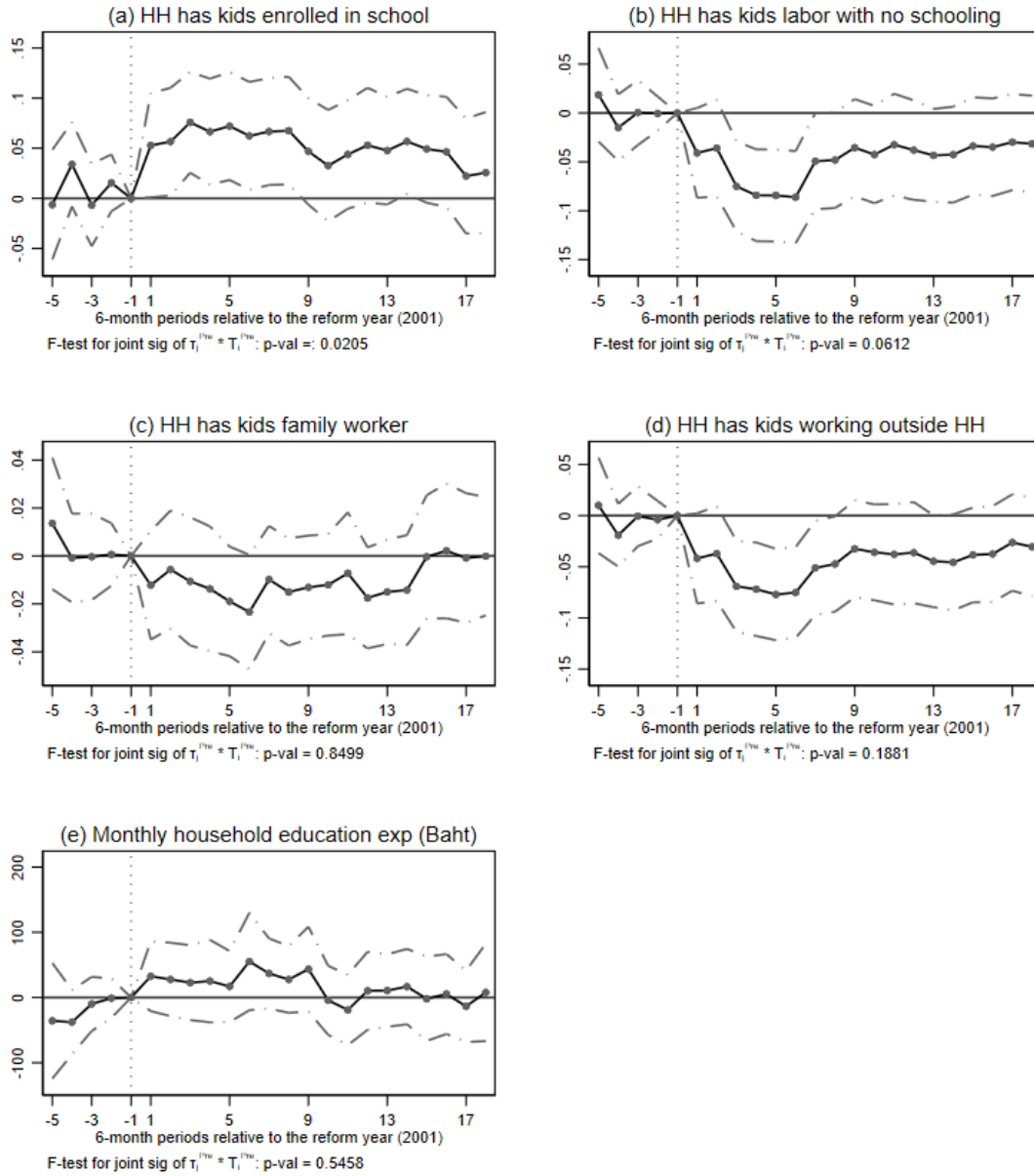
2 Preliminary Results using Event Study Analysis

Figure 1: HEALTH EXPENDITURE OUTCOMES EVENT STUDY



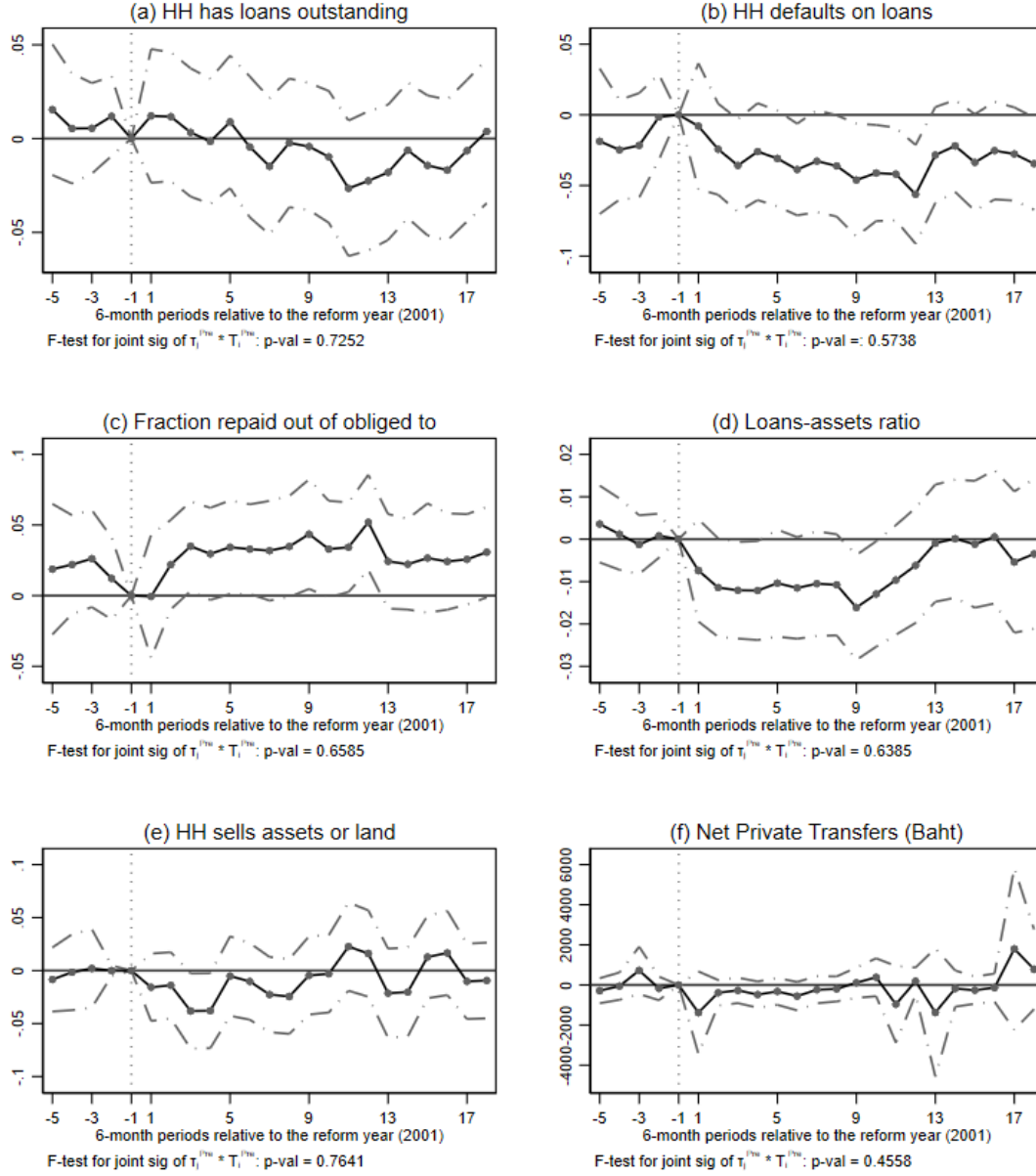
Note: Each graph plots the estimated coefficients of the interaction between half-year period dummies (τ_j) and treatment intensity (T_i^{Pre}) for each half-year period relative to 2001 (the reform year). $\tau_j^{Pre}; j \in \{-5, -4, -3, -2, -1\}$ are the 5 pre-reform half-year period dummies. Note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

Figure 2: EDUCATION OUTCOMES EVENT STUDY



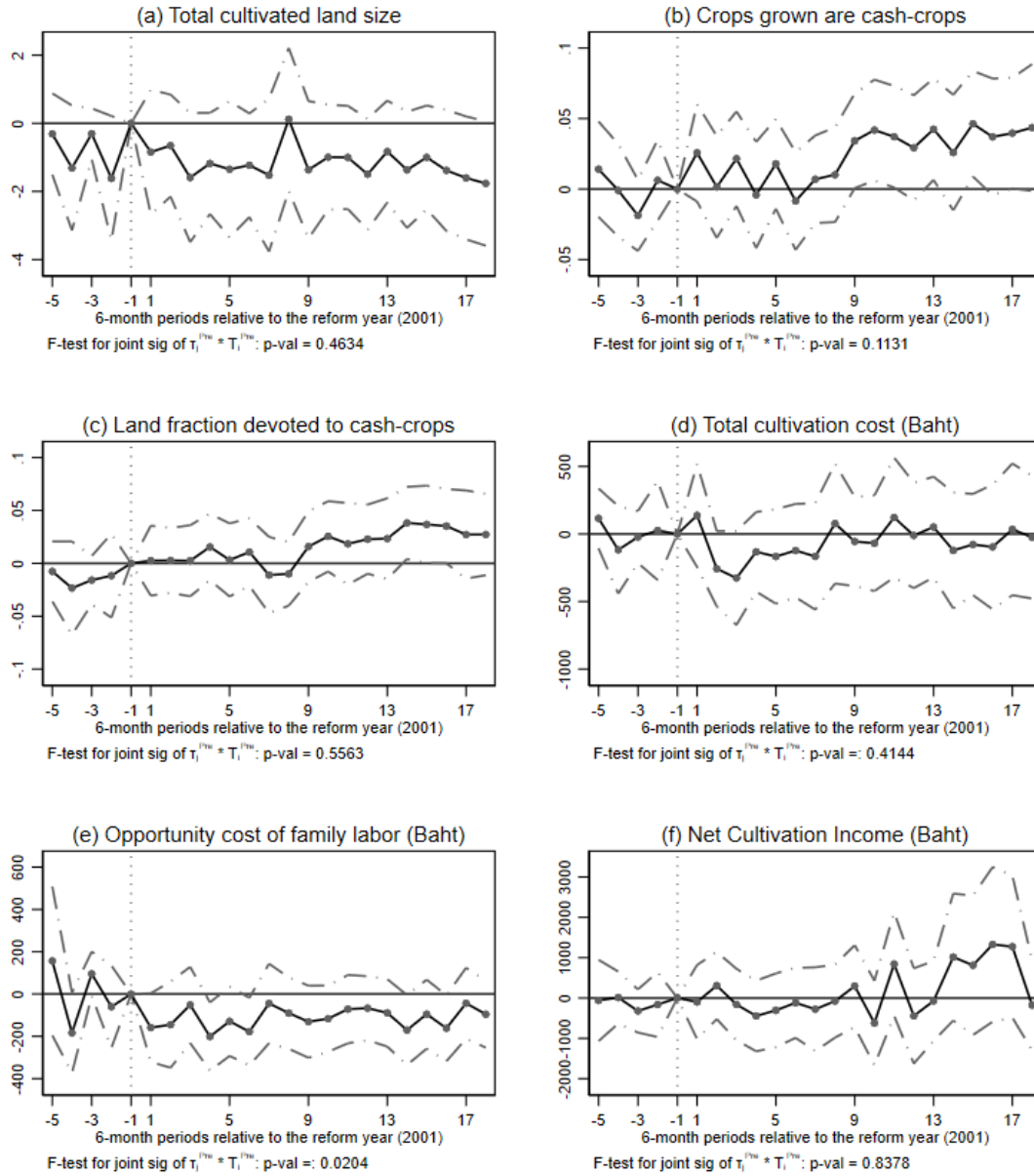
Note: Each graph plots the estimated coefficients of the interaction between half-year period dummies (τ_j) and treatment intensity (T_i^{Pre}) for each half-year period relative to 2001 (the reform year). $\tau_j^{Pre}; j \in \{-5, -4, -3, -2, -1\}$ are the 5 pre-reform half-year period dummies. Note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

Figure 3: BORROWINGS & TRANSFERS OUTCOMES EVENT STUDY



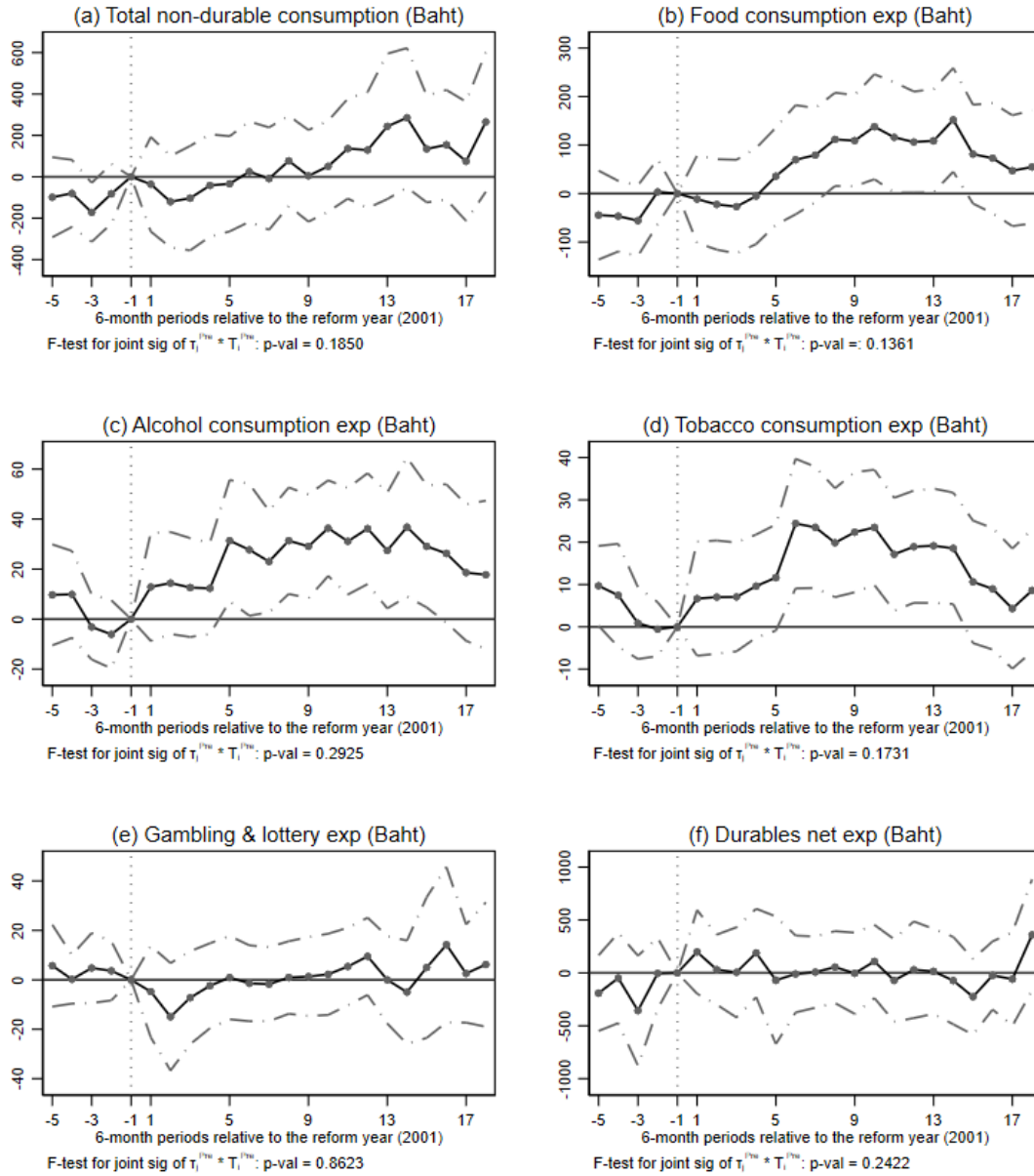
Note: Each graph plots the estimated coefficients of the interaction between half-year period dummies (τ_j) and treatment intensity (T_i^{Pre}) for each half-year period relative to 2001 (the reform year). $\tau_j^{Pre}; j \in \{-5, -4, -3, -2, -1\}$ are the 5 pre-reform half-year period dummies. Note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

Figure 4: CULTIVATION OUTCOMES EVENT STUDY



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

Figure 5: CONSUMPTION OUTCOMES EVENT STUDY



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

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A Appendix: Health Insurance in Thailand & the 30-Baht Reform

Thailand’s 2001 major health reform was primarily aimed at alleviating the prolonged geographical inequality in public healthcare provision as well as providing increased funding to better serve the poor and the previously uninsured. The policy was a key component in the populist election platform of Prime Minister Thaksin Shinawatra, who came into power in February 2001. With gross national income per capita of only \$1,990 per capita and tax revenue amounting to just 13% of GDP at the time, Thailand ambitiously became one of the first lower middle income countries to implement universal health coverage (UHC) scheme. The reform entitled over 18 million previously uninsured citizens with public health insurance coverage primarily replacing out-of-pocket medical expenditures with a fixed copayment as little as 30 Baht ($\sim \$0.73^9$) per visit at local health facility.¹⁰ With sharp and continuous rise in public health spending combined with effective supply-side measures, the scheme was able to offer comprehensive care package covering outpatient and inpatient services, accident and emergency, most high-cost treatments, and a wide range of preventive cares.¹¹

Pre-Reform. There were four existing public health insurance schemes prior to the onset of the 30-baht reform. The main tax-financed scheme targeting the poor and vulnerable groups is the Medical Welfare Scheme (MWS), which covered around 32% of the population in 2001 (Gruber, Hendren, and Townsend, 2014; Limwattananon et al., 2015). Launched in 1975, the scheme provides free healthcare services to the poor, children aged less than 12, secondary school students, elderly aged above 60, monks, war veterans and the disabled. In our data, using information on service payments at public health facility and income eligibility criteria to proxy for MWS status, MWS households accounts for around 21%.¹² The MWS scheme was largely underfunded with an average annual budget per enrollee of only 250 Baht ($\sim \$6.04$) for public hospitals (Damrongplisit and Melnick, 2009; Gruber, Hendren, and Townsend, 2014). This capitation budget was later increased five-fold to over 1,200 Baht ($\sim \$29$) following the reform to ensure adequate healthcare for all.

For those ineligible for the MWS, the Voluntary Health Card Scheme (VHCS) or the 500 Baht program allowed households to voluntarily pay an insurance premium of 500 Baht ($\sim \$12.08$) per year per household in order to receive free public healthcare for up to five enrolled household members. The government then contributed 1,000 Baht to supplement each private contribution. However, with up

⁹We use the exchange rate in 2001: 1 US dollar = 41.36 Baht. 18 million is roughly a quarter of Thailand’s population.

¹⁰Public health facilities in Thailand are governed at provincial and health district levels. Each province has one provincial hospital usually together with at least 1 smaller primary healthcare units in each district. People in the scheme receive a gold card that permits them to receive treatments in their health district or to be referred for specialist cares elsewhere if required.

¹¹Public health spending per capita doubled between 2001 and 2010. For more details on the supply-side measures, see Limwattananon et al. (2015).

¹²The means-tested MWS eligibility criteria on income is that individual lives with monthly income below 2,000 Baht ($\sim \$1.61/\text{day}$) per person or that a household has monthly income below 2,800 Baht ($\sim \$2.01/\text{day}$) per household. As our data do not contain direct information on insurance status, we first use information on individual visits to public facility during the pre-reform months. If a member of households had to pay for treatments or medicines upon a visit for an inpatient or outpatient service, then these households are not classified as an MWS household. Then, for the remaining households, we use the average monthly household income over the pre-reform months to predict households that were eligible for the MWS scheme.

to five enrollees per household, the combined contributions were often insufficient for providers to offer adequate services and thus required cross-subsidization (Donaldson, Pannarunothai, and Tangcharoen-sathien, 1999). Approximately one-fifth of the population were covered by VHCS, but only about 5% of households report paying for the health card at least once during the pre-reform months in our data.

Since 1980, the completely tax-financed Civil Servant Medical Benefit Scheme (CSMBS) has provided comprehensive coverage for active and retired civil servants and their dependents.¹³ The scheme offers relatively more generous care package with a superior annual outlay per capita of almost 2,500 Baht (~\$60) and covered around 8.5% of the population in 2001 (Donaldson, Pannarunothai, and Tangcharoensathien, 1999; Limwattananon et al., 2015). Formal sector workers employed in establishments with more than 10 workers were required to enroll in the Social Security Scheme (SSS) introduced in 1990 by contributing a small part of their monthly salary which would then be matched by their employer and the government. SSS provides free healthcare to these salaried private employees (but not their dependants), about 12% of the population in 2001. Because households in our data are rural households mostly engaged in informal activities, households with at least one CSMBS recipient or with all members entitled to SSS benefits accounts for just 9%.

The remaining 18 million, almost 30% of the population in 2001, were uninsured and had to pay for health services out-of-pocket. These groups are essentially those working in agriculture, unregistered small entities, and self-employed workers in the informal sector. The previously uninsured group accounts for the remaining 65% of households in our data. Table A1 provides a breakdown of different types of coverage and summarizes the impacts of the reform on each group.

Post-Reform. To extend coverage especially to the previously uninsured group working outside the formal sector, the government imminently delivered its promise implementing the reform within 3 months after the election. Started with six pilot provinces in April 2001, the 30-baht (UHC) program superseded the MWS and VHCS, and has provided coverage to all Thais who were not previously insured by the SSS and CSMBS. The rollout was then expanded to cover additional 15 provinces in June, all remaining provinces and 13 districts in Bangkok by October within the same year, then completed with a gradual inclusion of the remaining Bangkok districts between November 2001 and April 2002 (Wagstaff and Manachotphong, 2012). Coverage by any form of public health insurance shot up from 71% in 2001 to 95% in 2003 (Limwattananon et al., 2015). By 2015, National Health Security Office (2015) reported coverage of 99.92%. Out of these, 73.7% were UHC recipients with 99.9% of those in the UHC scheme already registered.

With an exemption for those previously eligible for the MWS (the poor and vulnerable), enrollees in the UHC had to contribute a fixed co-pay of 30 Baht (~\$0.73) per medical service contact and were entitled to receive a near comprehensive health benefit package from the local contracted units for primary care (CUPs) or its network that they are registered with, which in most cases are those in users' area of residence.¹⁴ Included in the package offered by a local provider network are ambulatory

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

¹⁴In practice, most contracted providers are public hospitals under the Ministry of Public Health (Wagstaff and Mana-

treatments, outpatient and inpatient services, maternity benefits, preventive care for health promotion, and prescribed medicines spanning a great proportion of generics (Wagstaff and Manachotphong, 2012; Limwattananon et al., 2015). The 30-Baht co-payment was subsequently abolished making healthcare completely free for all in October 2006.¹⁵

Under the UHC 30-Baht program, hospitals received increased provision capitation of over 1,200 Baht per head replacing either the 250 Baht capitation from former MWS recipients, the out-of-pocket payments from the previously uninsured, or the 1,500 Baht per household per year from the former VHCS households. The SSS was extended to cover those working in smaller establishments in the private sector, while the CSMBS were left largely untouched.

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

The main source of funding for the UHC is from government tax revenues. The National Health Security Office (NHSO) acts as a central purchasing agency who channels funds to local CUPs, of which the annual capitation-based outlay is determined based on the number of registered users (Panpiemras et al., 2011). However, concerns on insufficient funding and poor management in some hospitals, which could lead to compromising service quality and the lack of healthcare staffs, were apparent especially during the early stage (Hughes and Leethongdee, 2007; Panpiemras et al., 2011). Despite the new scheme potentially being underfunded, the key to our analysis is that it has brought about significant rises in health budget and different impacts on different households compared to the pre-reform period. At the time of the reform, the annual capitation budget was 1,202 Baht (~\$29) per registered users. In 2003, the UHC 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 (~\$82) in 2015 (National Health Security Office, 2015).¹⁶

chotphong, 2012). UHC participation of these public healthcare unit is mandatory, while that of the private hospitals are optional. The fraction of private units participating is minimal (Panpiemras et al., 2011).

¹⁵Renal replacement therapy and heart transplant were not included in the 2001 package, but were later covered in 2008 and 2012 respectively. For complete chronology of coverage extension, see Tangcharoensathien et al. (2018).

¹⁶This uses the 2015 exchange rate: 1 dollar = 34.25 Baht.

B Appendix Tables

Table A1: COMPARISON OF HEALTH INSURANCE SCHEMES PRE- AND POST-REFORM

Pre-Reform	Description	% of Pop.	% HHs Data	Post-Reform	Reform Impacts
Previously Uninsured	Paid for healthcare out-of-pocket	29	65	UHC with 30-Baht copayment (no copayment from 2006)	Service users faced reduction in care prices. Hospitals receive 1,200 per head instead of either out-of-pocket payment (from previously uninsured) or annual 1,500 baht per household (from VHCS)
VHCS	Voluntarily paid 500 Baht premium per household per year to get free care for up to five members	20	5		
MWS	Free care for the poor, those aged < 12 and > 60, monks, and disabled	30	21	UHC with no copayment	Enrollees face no change in prices, but benefit from increased hospitals' capitation from 250 Baht to 1,200 Baht.
SSS	Free care for salaried private sector employees of firms with more than workers	12	9	SSS	Extended to cover employees in smaller firms
CSMBS	Free care for civil servants and dependents	9		CSMBS	No formal changes