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The Health Effects of Universal Health Care: Evidence from Thailand

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Abstract

We exploit the staggered rollout of Thailand's universal health coverage scheme (UC) to estimate its impacts on whether individuals report themselves as being too ill to work. Our statistical power comes from the fact that there is an average of 160,000 respondents in the labor force survey at each survey date and no less than 32 survey dates. We find that UC reduced the likelihood of people reporting themselves to be too sick to work: we estimate the effect to be -0.004 one year after UC and -0.007 three years after. Our estimated effects are much larger among those aged 65 and over. We find that UC had a much larger effect on health (about four times larger) than the Village Fund scheme, which provided free credit to rural households through a subsidized microcredit scheme and which was rolled out around the same time as UC.

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I. **INTRODUCTION**

The developing world – and some of the developed world too – is in the midst of a major push toward universal health coverage (cf. World Health Organization 2010). A key objective of these universal coverage (UC) initiatives is to narrow gaps in coverage, and hence improve population health. Such health improvements would be of value in their own right. But they could also have an economic benefit: better health may enable people to work who would otherwise be too ill to work; it may allow people to take less time off work due to sickness; and it may lead to increased labor productivity.

It is certainly plausible that health insurance expansion might yield health benefits: cost-sharing at the point of use may lead people to delay seeking care, discontinue treatment prematurely, or forgo it altogether. Any or all of these could conceivably lead to worse health outcomes. Yet two recent reviews (Levy and Meltzer 2008; Giedion and Diaz 2011) conclude that there is surprisingly little hard evidence on whether health insurance does indeed lead to better health, and if so how large the impacts are. As Levy and Meltzer (2008) remark, "many of the studies claiming to show a causal effect of health insurance on health do not do so convincingly because the observed correlation between insurance and good health may be driven by other, unobservable factors." People with adverse unobservables may deliberately seek out insurance coverage, or local government officials administering public insurance may successfully target those with adverse unobservables.

Random assignment of insurance would, of course, eliminate a concern over selection bias. However, randomized control trials (RCTs) of insurance are few and far

between. The RAND Health Insurance Experiment (HIE), conducted in California in the 1970s, is the most famous exception. It randomly assigned participants to different insurance plans, and found that cost-sharing had only limited impacts on health outcomes among the general population (Brook *et al.* 1983). However, as Chernew and Newhouse (2008) note, with the technological advances that have occurred in medicine since the study, the negative effects of cost-sharing on health outcomes are likely to be larger today than they were in the 1970s; furthermore, the HIE did find negative effects of cost-sharing among those on low incomes. Randomized encouragement is an alternative to randomized assignment. King et al. (2009) used this technique to estimate the impacts of Mexico's *Seguro Popular* health insurance program, distributing leaflets about the program to households in randomly chosen health facility catchment areas. They found no difference in health between the intervention and control group; they acknowledged, however, that the short follow-up period (just 10 months) may be the reason. Finkelstein et al. (2011) exploited Oregon's decision to give a random selection of people the chance to apply for Medicaid. They found that 12 months after the lottery, among those given the chance to take out Medicaid around 25 percent did so. This "treatment" group reported greater use of preventive and curative medical care than the control group, and reported themselves to be in better physical and mental health than the control group.

Randomization is *sufficient* to eliminate concerns over selection bias but not *necessary*; what matters is that a setting can be found where an individual's insurance status is exogenous to his or her health. One such setting is where a program is rolled out in a staggered fashion so that over time the number of people entitled to or possessing health insurance increases, but the individuals affected do not decide their eligibility and may not

even have any say in whether they are covered. Currie and Gruber (1996) and Finkelstein and McKnight (2008) exploited the staggered rollout of the Medicaid and Medicare programs in the US: Currie and Gruber found that increased Medicaid eligibility led to "a sizeable and significant reduction in child mortality", while Finkelstein and McKnight found that "the establishment of universal health insurance for the elderly had no discernible impact on elderly mortality".

In this paper we exploit the staggered rollout of Thailand's Universal Coverage (UC) scheme (cf. e.g. Pannarunothai *et al.* 2004) to estimate its impacts on one measure of health status – whether individuals report themselves as being too ill to work. By using general revenues to cover everyone not covered by the civil servants scheme or the social security scheme, Thailand achieved universal coverage within a period of 12 months. The fact that the expansion was achieved in a staggered fashion in four waves enables us to identify the effects of the UC reform on health status. We estimate the health effects of UC by linking an individual's self-reported health limitations at a particular date to their length of exposure to the UC "regime"; this depends on the survey date and the person's province of residence. Our statistical power comes from the fact that we have an average of 160,000 respondents in each of our quarterly labor force survey covering the period 1997 - 2005[1]. Since the "month" variable is also available from 2001, we can precisely identify the timing of UC implementation in each province. The accuracy of our estimates is enhanced by the fact that 17 of the 32 surveys we use predate the launch of UC: this allows our regressions to capture the pre-reform trends in our health limitations variable.

[1] We only have the first and the third quarter of LFS1997 and LFS1998.

We find that UC reduced the likelihood of people reporting themselves to be too sick to work. In our estimation, we allow the effect to change with increased exposure to the UC regime. We find that one year after UC, the effect of the program on the probability of being too ill to work is -0.004; this effect rises to -0.007 after two years and stays at the same level in the third year since implementation. We find much larger effects among those aged 65 and over.

Our effects are mostly statistically significant at the 10% level if not the 5% level. But are they large in economic terms? In part to answer this question, we take into account and report the effects on self-reported health limitations of another major public program that was introduced around the same time as UC, namely the Village Fund (VF). This provided Thai villages with seed money (around $22,500 per village) to set up a microcredit scheme; survey data suggest villagers used the money mostly to fund agricultural activities, though the VF was also used by significant numbers of villagers to finance daily expenses and non-farm businesses (cf. Boonperm *et al.* 2012). While not a health intervention, the VF might have been expected to have had some beneficial effects on health status, by for example increasing food consumption and giving households extra income to purchase medicines and other medical goods not covered by the UC scheme. We find that in practice the effects of the VF on the probability of someone reporting themselves as too ill to work is zero in the year of implementation and in the following two years; only in the third year after implementation do we see an effect. On average, the UC effect is around four times the size of the VF effect.

II. THAILAND'S UNIVERSAL COVERAGE SCHEME

Before the introduction of Universal Coverage (UC) in 2001, more than 25 percent of the Thai population was not covered for their health care expenses. The rest were covered by at least one of four schemes. The largest was the Medical Welfare Scheme (WHS) which covered 33 percent of the population. The Health Card Scheme (HCS), the Civil Servants Medical Benefits Scheme (CSMBS) and the Social Security Scheme (SSS) each covered about 12, 11 and 10 percent of the population respectively: see Hanvoravongchai and Hsiao (2007) and Table 1.

The Medical Welfare Scheme (WHS) provided tax-financed coverage to the poor and vulnerable groups, including the poor, the elderly, children below the age of 12, secondary school students, the disabled, war veterans, and monks. The Health Card Scheme (HCS) was a public voluntary insurance program for nonpoor households who were ineligible for the WHS. The Civil Servants Medical Benefits Scheme (CSMBS) was provided as a fringe benefit to current and retired civil servants and their dependents. The Social Security Scheme (SSS) aimed to cover employees of establishments with more than 10 workers, but not their dependents, and was – and still is – financed through a payroll tax (1.5 percent paid by the employer, 1.5 percent paid by the employee) and a subsidy (the government also pays 1.5 percent). Outlays per enrollee varied considerably across the schemes: the CSMBS recorded the highest at baht 2,106; the SSS recorded the second highest at baht 1,558; and the HC and MWS recorded much lower outlays per enrollee of just baht 534 and baht 363 respectively.[2]

[2] This masks the cross-subsidies from the public hospitals to the last two schemes.

Frustrated with the low coverage of the HC scheme and with the resultant large number of Thais without coverage[3], Thailand's government decided to introduce a UC scheme to cover people not covered by the SS and CSMBS schemes. The scheme was to be funded largely by general revenues, with a minimal copayment of 30 baht (about USD 0.75 at the time) per visit (hence the initial name – the 30 Baht Scheme). Once it was rolled out, the UC scheme covered around 70 percent of the Thai population.

Table 2 summarizes the main features of the UC scheme and compares it with the CSMBS and SS schemes. The government budgeted as much as baht 1,309 per enrollee in the UC scheme, a dramatic increase on the per-enrollee outlays of the HC and MWS schemes. On paper at least all three schemes provide comprehensive medical coverage to their members, and the UC and SS schemes are similar in terms of their coverage of maternity benefits (both cover them), annual physical checkup (neither does), and prevention and promotion (both cover health education and immunization). Similar to the SS scheme, UC enrollees are required to choose a contracted hospital or its network. Enrollees have access to both public and private providers, though in practice most UC contracted providers have been Ministry of Public Health hospitals. However, unlike the CSMBS and SS schemes, the UC scheme does not provide certain cash benefits payable in the event of sickness, disability and death.

[3] Srithamrongsawat (2002) also found that those who joined the health card scheme (HCS) had a significantly higher utilization rate than those under the social security scheme (SSS), hence suggesting that there was an adverse selection problem.

III. **METHODS**

The UC scheme was rolled out in four phases. Phase 1 started in April 2001 and involved six of Thailand's provinces. Phase 2 started in June 2001 and involved a further 15 provinces. Phase 3 started in October 2001 and involved the remaining 55 provinces and 13 districts in Bangkok. The rest of Bangkok's districts gradually began implementation between November 2001 and April 2002. We use this staggered rollout to identify the impacts of UC. From January 2002 (the date the fourth and last wave of provinces started to implement UC), we have an exposure difference of nine months between the first-wave provinces and the last-wave provinces. Assuming the effects of UC were similar across provinces, we should see changes in self-reported health limitations appearing first in the first-wave provinces, the next set of changes two months later in the second-wave provinces, and so on. In other words, we can estimate the impacts of UC by linking the self-reported health of individual *i* at time *t* to the amount of time individual *i* has been exposed to the UC "regime" at time *t*. Exposure will vary depending on when the survey was done, but also on which of the four groups of provinces the person lives in. Clustering at the provincial level will, of course, be an issue from the point of view of statistical power. However, with a very large number of respondents at each survey date (160,000 on average for each quarterly survey date or 53,000 on average for each monthly survey date), and a very large number of survey dates (68 monthly survey dates, of which a majority – 51 – are after the start of the rollout of UC and a month apart), we should have sufficient statistical power to detect any effect that increased exposure to the UC "regime" has on our outcome variable.

We need to be careful to control for variables that may be correlated with UC rollout. Household- and individual-level variables are unlikely to cause omitted variable bias, but their inclusion does help to improve precision (cf. e.g. Angrist and Pischke 2009, p.237) so we include the obvious ones. Our concern with omitted variable bias actually lies with events and/or programs that may have also had effects on health status, and whose timing in a specific location coincides with the introduction of UC. One potential confounder is the aforementioned Village Fund (VF) program, which was also launched in 2001 and whose rollout could conceivably have coincided – at least in some areas – with the rollout of UC and whose effects could conceivably include improvements in health status.

Let y_{ipt} be the self-reported health of individual *i* in province *p* at quarter *t*. Our estimating equation takes the form:

$$(1) \qquad y_{ipt} = X_{ipt}\gamma + \sum_{\tau=-m}^{n} \delta_{\tau} UC_{pt}^{\tau} + \sum_{\omega=-k}^{l} \alpha_{\omega} VF_{pt}^{\omega} + \lambda_p + \theta_t + e_{ipt} \qquad ,$$

where X_{ipt} is a vector of covariates at the household- and individual-level, the UC_{pt}^{τ} and VF_{pt}^{ω} are a series of dummies capturing the UC and VF policies defined in a way that we explain below, λ_p and θ_t are province- and period-specific effects (each quarter is allowed its own fixed effect), and e_{it} is an error capturing unobservable variables and noise. Our interest is in the δ_{τ} which capture the effect of UC (we discuss their interpretation below). In our estimation of eqn (1), we heed Angrist and Pishcke's (2009 p.94 ff) advice and use OLS rather than a limited dependent variable model to estimate the marginal effects of interest.

We adjust standard errors for clustering at the province level, since this is the level of variation in the UC program.

We capture UC through a series of dummies that reflect the quarters until or after UC implementation.[4] Thus UC^{τ} equals 1 at time t in province p if UC has been in force exactly τ quarters. In the quarter of implementation, UC^{0} =1 and UC^{τ} =0 for $\tau \neq 0$. In the quarter after implementation, UC^{1} =1 and UC^{τ} =0 for $\tau \neq 1$. In the quarter before implementation, UC^{-1} =1 and UC^{τ} =0 for $\tau \neq -1$; however, we omit this UC dummy. In the quarter before this, UC^{-2} =1 and UC^{τ} =0 for $\tau \neq -2$. If UC has a causal effect on y, we would expect the δ_{τ} for quarters before UC to be zero, and the δ_{τ} for quarters after UC to be non-zero, and not necessarily equal to one another; the effect of UC may build up over time, for example, reflected in a growth of the δ_{τ} with time since UC implementation. We control for the effects of the VF program in the same way.

We also estimate a version of eqn (1) with constraints imposed on the δ_{τ} similar to the constraints imposed by Bosch and Campos-Vázquez (2010) in their analysis of the labor market effects of Mexico's *Seguro Popular* scheme. We constrain the δ_{τ} and the α_{ω} in each quarter to be the equal to one another, giving UC and VF variables that capture years (rather than quarters) to or since UC and VF implementation. We constrain the δ_{τ} and the α_{ω} to be the same for 3+ years before UC/VF implementation and for 3+ years after UC/VF implementation. Bearing in mind that we omit the UC dummy for the period prior to UC

[4] This specification is similar to that used by Campos-Vázquez (2010) in their study of the labor market effects of Mexico's *Seguro Popular* insurance program, and is in effect a mix of the models discussed by Wooldridge (2002 p.317) and Angrist and Pischke (2009 p.237).

implementation, and that we have quarterly data prior to UC and monthly data thereafter, the constrained version of eqn (1) includes six dummies: the first equaling one when the quarter is three or more years before implementation (UC^{-3}); the second equaling one when the quarter is two years before UC implementation (UC^{-2}); the third equaling one when the quarter is between zero and 4 quarters after implementation (UC^{0}); the fourth equaling one when the quarter is between 4 and 8 quarters of implementation (UC^{1}); the fifth equaling one when the quarter is between 8 and 12 quarters of implementation (UC^{2}); and the sixth equaling one when the quarter is 12 quarters or more after implementation (UC^{3}). If UC has a causal effect on *y*, we would expect δ_{-3} and δ_{-2} to be zero, and at least some of δ_0 , δ_1 , δ_2 and δ_3 to be nonzero, depending on the time profile of UC impacts.

IV. **DATA**

Our data are from Thailand's Labor Force Survey (LFS) conducted by the National Statistics Office of Thailand (NSO). The data were collected quarterly from 1985 until present. From 2001, the data includes a month variable which allows us to identify which month a given sample was collected (UC was launched in the first provinces on April 2001). Each round of the survey covers all provinces and all industries in the entire country. The sample size is between 500,000 and 650,000 persons per year, with some rotation across surveys. The LFS gives a weight variable which allows us to gross up to the population. In this paper, we use the data from the start of 1997 to the end of 2005. This time frame covers the four years and three months prior to the earliest implementation of UC, the nine months of staggered implementation across the remaining provinces, and four years after

the last province acquired UC. This gives us a sample size of 4.7 million individuals. Our effective sample size in terms of UC impacts is, of course, much smaller because the program operates at the province level, and there are only 76 provinces. Offsetting this is the fact we have data for 32 periods: 13 quarters covering the period 1997-2000 and the first quarter of 2001 up to the launch of UC in April, and then 19 quarters during and after UC implementation.[5]

Our outcome of interest is whether the individual reports himself as being too ill to work. LFS respondents who said they were not available for work during the survey week were asked why there not available; the list of possible answers included "Illness, disability". We use this to construct our self-reported health limitations measure.

Our UC "treatment" variable is constructed from the province of residence of the respondent, the timing of the UC implementation in the individual's province, and the date (month and year) of the LFS. We construct variables corresponding to the numbers of months, quarters, half years, and years since implementation of UC in the individual's province as of the LFS in question. From these we construct the dummy variables in eqn (1).

Our VF variable is constructed from the province of residence of the respondent, the timing of the arrival of the first VF funds, and the date (month and year) of the LFS. We define the VF variable analogously to the UC variable so if, for example, the UC variable is defined as years from UC implementation, the VF variable is also be defined as years from

[5] We have only the first and the third quarters of LFS 1997 and LFS 1998.

VF implementation[6]. In practice, pinning down the date of VF implementation was not straightforward. We were able to obtain from the National Village and Urban Community Fund Office the date that each village registered with the Thai government. However, we could not identify the date that each village actually received its 1-million baht fund. What we do know is that, on average, villages received their money within 2-3 months of registration. We therefore assume that the "implementation" date of VF is 3 months after the registration date. We define the VF variable at the provincial level because we do not know the identity of the village in our LFS data; we define the VF variable as the proportion of villages in the province that had received the first tranche of VF money. The first fund was transferred in July 2001; by the end of 2002, about 73,941 villages (or 93.79 percent of all villages) received the money (Satsanguan 2006). Our VF variable – unlike the UC variable – exhibits variation within each of the blocks of provinces that comprise the four waves of the UC rollout; this extra geographic variation helps us separate the effects of UC from the effects of the VF program.

Table 3 shows the descriptive statistics of our dependent variables and covariates. The statistics are calculated from the total sample size of 4,770,735 individuals (who are older than 15 years old) over the time period 1997-2005. For each variable, we report their population-weighted mean and standard deviation. The dependent variable is whether the individual is too ill to work. For each survey period, about 31 percent of the respondents would report that they were not available to work. About 5-7 percent of this pool provided

[6] The value of this village fund variable would be equal to zero for municipal areas because villages are in non-municipal areas only.

"too ill or disabled" as the reason (see Table 4). In other words, about 1.5-2 percent of the population over age 15 was too-ill-too-work at a given time.

Our covariates, also reported in Table 3, include province-level minimum wage, non-municipal area dummy, number of children under 6 years old in the household, number of children between 6-15 years old in the household, and number of elderly older than 64 years old in the household. The covariates also include the individual gender, age and education level.

The minimum wage in Thai baht per day is obtained from the Thai Ministry of Labor. The Ministry of Labor reconsiders the minimum wage at least once a year. This reconsideration is done at the province level. Thus, minimum wage could potentially control for the trend of living cost in each province. The non-municipal area dummy could control for proximity to hospitals and availability of health resources. It could also account for any health-related factors that differ between rural and urban areas such as life style and pollution. The number of children under age 6, number of children age 6-15, and number of elderly older than 64 are to control for the claims on resources of the household.

As for individual-specific covariates, we include gender, age and education. For ease of analysis but without much loss of generality, we group age into 15-29 years old, 30-49 years old, 50-64 years old and over 64 years old. We also group education attainment based on the highest level of education achieved. The categories include: less than primary education; primary education; some secondary education; completed secondary education; vocational education; and university education and above.

V. **RESULTS**

We focus in our presentation of our results on the impacts of UC. The signs of the coefficients of the non-UC variables in our regressions are broadly as expected. Education reduces the likelihood of reporting limiting health though the effects of additional education are very small, having school-age children increases the likelihood of people reporting their health preventing them working, while having elderly household members reduces the probability. Unsurprisingly, the elderly (65+) are substantially more likely to report limiting health.

Table 5 reports the estimates of the impacts of UC and the VF on the probability of reporting ill health or disability preventing the person from working. The coefficients are the estimates of the δ_τ and the α_ω in eqn (1) with the restrictions placed on the δ_τ and the α_ω as explained in section III. The estimates show the "impacts" of UC and VF three and two years prior to its implementation (these coefficients ought not to be significantly different from zero), and the impacts of UC and VF zero, one, two and three years after implementation.

Figure 1 shows the corresponding estimates and 95% confidence intervals for the more flexible version of equation (1) where the δ_τ and α_ω are unrestricted. In all cases, we control for the effects of the provincial minimum wage, educational attainment, age, the demographic mix of the respondent's household, area of residence (rural versus urban), and province and year. There are no significant "effects" of UC and VF for the sample as a

whole prior to implementation; this is as it should be if the effects we are estimating are causal relationships. There are a few effects for specific age groups, but there are very few.

Table 5 and to a lesser extent Figure 2 suggest that among the sample as a whole UC reduced the likelihood of being too ill to work. The effect is most pronounced for those over 65, though there is a perceptible effect too among those aged 50-64. Among the over-65s, the effect is sizeable – the numbers suggest that among this age group UC may have reduced the probability of someone being too ill to work by as much as 5-6 percentage points. For the sample as a whole, the effect is much smaller – between 0.5 and 0.7 of a percentage point.

Whether the sample impact is small or large is hard to say without a yardstick to compare it against. The VF provides one yardstick. The VF was not a health intervention per se but by raising living standards of rural residents it can reasonably be expected to have had some health effects. Table 5 suggests that any health effects that the VF had were a good deal smaller than those of UC – on average, the effect of the VF on self-reported health was around one quarter that of the UC scheme.

VI. **CONCLUSIONS**

Our analysis exploits the phased rollout of the Thai UC policy to estimate its effects on health, specifically on the probability of a person reporting themselves as too sick to work. We control for province and quarterly effects, as well as multiple individual- and household-level variables. We also estimate the effects on our self-reported health limitations variable of the Thai Village Fund (VF) scheme which began to be rolled out

around the same time and which provided free credit to rural residents; we are able to separate out the effects of the two programs through the spatial and temporal differences in program rollout.

Our results suggest that UC did reduce the likelihood of people being too ill to work, especially among people aged 65 and over. We find that UC had larger effects on health than the VF – around four times as large. A full comparison of the two programs would, of course, require a comparison of the costs as well as a fuller comparison of the benefits; the VF may have had relatively larger effects on other health indicators, and presumably had larger effects on non-health outcomes. Nonetheless, the comparison is interesting because in some quarters it has been fashionable to argue that interventions outside the health sector – including those that simply increase household purchasing power – may have larger health benefits than health sector interventions.[7] We leave for future research the question of whether the estimated health effects of UC are large or small against other yardsticks.

[7] See Cutler, Deaton and Lleras-Muney (2006) for a thoughtful – and rather critical – look at this view.

Table 1: Health coverage of the Thai population in 2000

Type of Health Coverage	Number (million persons)	Percentage of Population
Medical Welfare Scheme (WHS)	20	33%
Health Card Scheme (HCS)	7-8	12%
Civil Servant Medical Benefit Scheme (CSMBS)	~7	11%
Social Security Scheme (SSS)	6	10%
Private Health Insurance	~5.9	~10%
No coverage	>15.5	>25%
Eligible for more than 1 type of coverage*	?	?
Total	61.5	100%

Source: Na Ranong et al. (2004).

* For example, those who covered by CSMBS from being a parent or a child of a civil servant would be covered by CSMBS but also automatically covered by WHS.

Table 2: Characteristics of different health insurance schemes

Characteristics	**Government Employees (CSMBS)**	**Social Security Scheme (SSS)**	**Universal Health Coverage (UC)**
Population Coverage	Civil Servant, pensioners and their dependents (parents, spouse, children)	Formal sector private employee	The rest of Thai population who are not eligible for CSMBS and SSS.
Benefits			
Ambulatory services	Public Only	Public & Private	Public & Private
Inpatient services	Public & Private (emergency only for private)	Public & Private	Public & Private
Choice of provider	Free choice	Contracted hospital or its network with referral line, registration required	Contracted hospital or its network with referral line, registration required
Cash benefit	No	Yes	No
Maternity benefits	Yes	Yes	Yes
Annual Physical checkup	Yes	No	Yes
Prevention Health Promotion	No	Yes	Yes
Services not covered	Special nurse	Private bed, special nurse	Private bed, special nurse, eye glasses
Copayment	Yes (inpatient at private hospital only)	Maternity, emergency services	30-baht/visit*
Financing			
Source of funds	General tax	Employee & Employer	General tax
Financing body	Comptroller General Department, Ministry of Finance	Social Security Office	National Health Security Office
Expenditure per capita (in 2006)	8,785	1,738	1,659
Per capita tax subsidy (in 2006)	8,785 (plus administrative cost)	579 (plus administrative cost)	1,659 (plus administrative cost)

Source: Sakunphanit (2006).

* The 30-baht copayment was eliminated in 2006.

Table 3: Descriptive Statistics

	Mean	Std. Dev.
Not available to work during the survey week (of all population age ≥ 15)		
By reason (= 1, if not = 0)		
Having household, family duty	0.09	0.28
In school,student	0.09	0.29
Seasonal worker	0.01	0.10
Too young or too old	0.06	0.24
Illness, disability	0.02	0.13
Resting	0.01	0.09
Other reasons	0.03	0.18
Total not available to work during the survey week	0.31	0.46
Determinants of work status (= 1, if not = 0, except minimum wage)		
Province-level minimum wage (THB/day)	140.53	13.77
Non-municipal area	0.67	0.47
Female	1.5	0.5
Education - primary	0.62	0.49
Education - some secondary	0.17	0.38
Education - secondary	0.1	0.3
Education - vocational	0.05	0.21
Education - university	0.06	0.23
No. of children (age ≤ 5)	0.95	1.04
No. of children (age 6-14)	0.58	0.8
No. of elderly (age > 64)	0.31	0.6
Age (15-29)	0.36	0.48
Age (30-49)	0.4	0.49
Age (50-64)	0.16	0.36
Age (>64)	0.08	0.28
N (unweighted sample size)	4,770,735	

Source: Thai Labor Force Survey (1997 - 2005)

Notes: N = unweighted sample size (individual respondents) for 32 quarters from 1997-2005.

Mean and Std. Dev. are calculated using population weight given by the survey.

Table 4: Reason for not available to work during the survey week (1997-2005)

Reason/Year	Percentage of respondents, given that they were not available to work									
	1997	1998	1999	2000	2001	2002	2003	2004	2005	2006
Having household, family duty	30.9	30.3	28.7	28.1	28.9	29.3	29.5	30.3	30.9	29.5
In school, student	30.4	31.0	30.6	30.3	30.8	32.1	32.6	32.4	31.2	31.3
Seasonal worker	4.6	5.1	5.0	5.3	4.2	2.7	2.2	1.3	1.5	3.4
Too young or too old	21.3	21.8	21.9	22.1	21.9	21.9	22.5	22.0	22.1	22.0
Illness, disability	6.5	5.7	5.7	5.5	6.6	6.3	6.5	7.1	7.1	6.4
Resting	3.3	3.4	3.8	3.7	2.4	2.7	2.8	2.9	3.0	3.1
Other reasons	2.9	2.6	4.2	5.0	5.2	5.0	4.0	4.0	4.2	4.3

Source: raw data from the Thai LFS, authors' tabulation.

Table 5: Estimates of impacts of UC and VF on probability of being too ill to work

			Years since implementation					
			-3	-2	0	1	2	3
UC and VF effects for full sample	UC	coef	0.001	0.002	-0.001	-0.004**	-0.007**	-0.007*
		t	0.39	1.12	-0.60	-2.10	-2.32	-1.75
	VF	coef	-0.001	0.000	-0.000	-0.000	-0.001	-0.004*
		t	-0.44	0.39	-0.00	-0.03	-0.48	-1.89
UC effects by age group	age (15-29)	coef	-0.000	-0.000*	-0.000	-0.001***	-0.001***	-0.000*
		t	-0.91	-1.79	-0.74	-3.59	-2.98	-1.68
	age (30-49)	coef	-0.000	-0.000	0.000**	0.000**	0.000**	0.000**
		t	-0.39	-0.47	2.55	2.43	2.25	2.07
	age (50-64)	coef	0.019***	0.009**	0.001	-0.005	-0.012*	-0.012
		t	2.68	1.96	0.19	-1.01	-1.73	-1.19
	age (65+)	coef	-0.004	0.016	-0.022	-0.045**	-0.056*	-0.058
		t	-0.16	1.16	-1.10	-2.02	-1.79	-1.47

Notes: *** p<0.01, ** p<0.05, * p<0.1. Results are from estimates of eqn (1) with the coefficients on UC and VF constrained as described in section III. The regressions include – in addition to the UC and VF dummies – province- and quarter-specific fixed effects, as well as the provincial minimum wage, educational attainment, age, the demographic mix of the respondent's household, area of residence (rural versus urban). Standard errors are adjusted for clustering at the province level.

Figure 1: Impacts of UC and VF on probability of being too ill to work

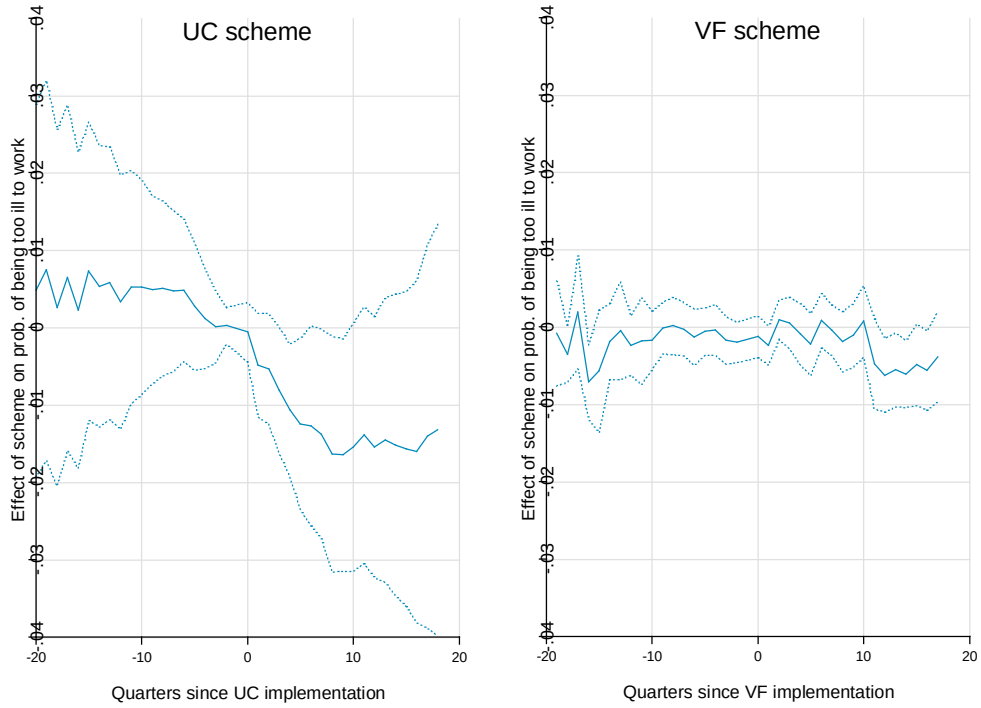
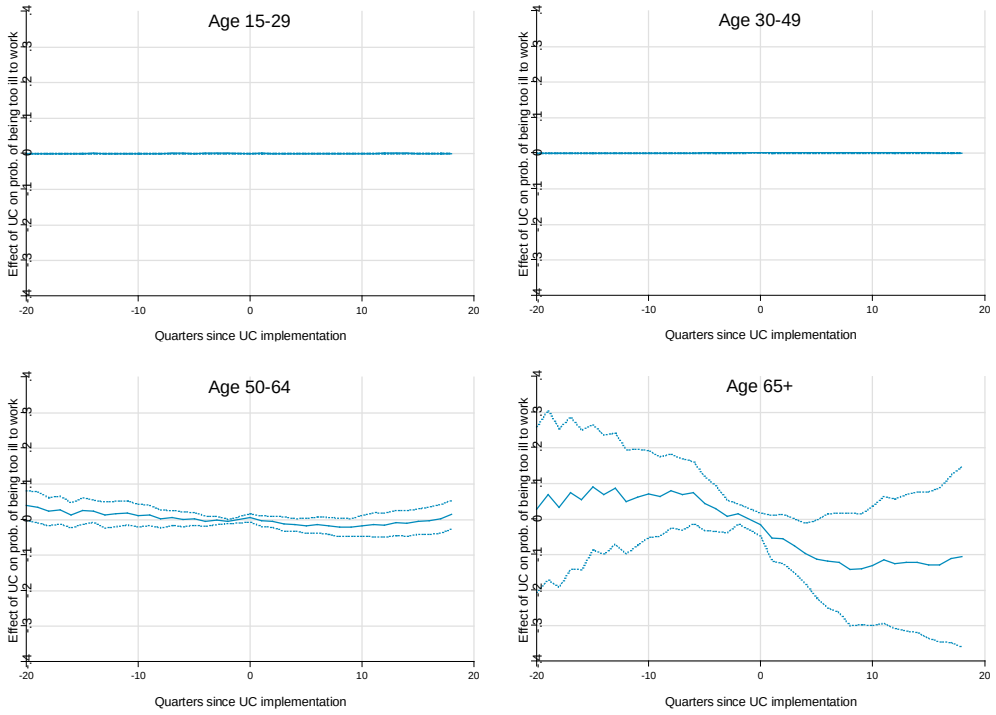


Figure 2: Impacts of UC on probability of being too ill to work, by age



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