Universal Health Care and Informal Labor Markets: The Case of Thailand

Adam Wagstaff

and Wanwiphang Manachotphong

Faculty of economic Thammasat University
ertc@econ.tu.ac.th

Universal Health Care and Informal Labor Markets:
The Case of Thailand

by

Adam Wagstaff[a] and Wanwiphang Manachotphong[b]

[a] Development Research Group, The World Bank, Washington DC, USA
[b] Department of Economics, Thammasat University, Bangkok, Thailand

July 2012

Abstract

We explore the possibility that universal coverage (UC) may inadvertently lead to distorted labor market choices, with workers preferring informal over formal employment, leading to negative effects on investment and growth, and reduced protection against both non-health risks and the income risks associated with ill health. We explore this hypothesis in the context of the Thai UC scheme, which was rolled out in four waves over a 12-month period starting in April 2001. We identify the scheme's effects through the staggered rollout, and gain statistical power by using no less than 32 consecutive labor force surveys, each containing an average of 160,000 respondents. We find that UC encouraged employment especially among married women, reduced formal-sector employment among married men but not among other groups, and increased informal-sector employment especially among married women. We see the largest positive informal-sector employment effects in the agricultural sector.

Corresponding author and contact details: Adam Wagstaff, World Bank, 1818 H Street NW, Washington, D.C. 20433, USA. Tel. (202) 473-0566. Fax (202) 522-1153. Email: awagstaff@worldbank.org. Wanwiphang Manachotphong, Department of Economics, Thammasat University, 2 Prachan Rd., Bangkok, 10200, Thailand. Tel.(02)613-2453. Fax (02)224-9428. Email: wanwiphang@econ.tu.ac.th.

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). Since everyone in almost every country has some coverage through the partially tax-financed network of public facilities that are accessible by everyone, the agenda in practice is about reducing the gap in de facto coverage between the section of the population relying on public facilities and the sections of the population covered by more generous and more explicit coverage schemes. The latter group typically includes civil servants who receive health coverage at the taxpayer's expense as a perk of their job, and formal-sector workers, many (if not most or all) of whom are required to enroll in a contributory scheme with contributions linked to earnings, split between the employee and employer, and supplemented by subsidies from tax-payers. While the details of the arrangements vary from country to country, a common emerging pattern is for governments to use general revenues to provide more generous – and sometimes more explicit – coverage to at least some individuals not covered by schemes for civil servants and formal-sector workers and their dependents. In some countries this has involved setting up a third scheme (Mexico and Thailand are examples[1]), while in others the hitherto "uncovered" individuals are brought into an existing scheme (Vietnam is an example[2]). In some cases, everyone without formal coverage is included in the new program at no cost to the individual (Thailand is an

---

[1] On Mexico's reform, see Frenk et al. (2006). On Thailand's, see Damrongplasit and Melnick (2009).
[2] On Vietnam's reform, see Lieberman and Wagstaff (2009).

example); in others only some are given the full subsidy, and the rest are expected to contribute at least part of the cost (this is the case in Mexico and Vietnam).

The goal of these initiatives, of course, is to narrow gaps in coverage and hence improve people's health and their protection against the financial consequences of ill health. But the question arises as to whether these well-intentioned initiatives may have unintended negative side-effects. If they do, one would want to know how large they are relative to the benefits of the policies, and whether these benefits could be achieved at lower cost through some alternative policy. Levy (2008) has argued that one unintended consequence of such policies is that they provide people with an incentive to work in the informal sector rather than the formal sector, since people can obtain similar if not identical health coverage without making any additional specific health contribution. (They have to non-payroll pay taxes, of course, but they have to do this anyway.) Inadvertently therefore universal health coverage may skew employment and investment away from the formal sector, and – according to Levy – reduce economic growth. This informalization of the labor force may also leave workers with less protection against the income losses associated with health shocks if the universal coverage program covers health care costs but not sickness absence, disability benefits, and so on. It may also leave them with less protection against the financial consequences of non-health shocks (e.g. unemployment, and old age) insofar as the various social insurance programs are bundled together.

Mexico – where Levy served as deputy finance minister – is the country where this issue has been researched and debated the most, although there has been some work undertaken in other Latin American countries (see Aterido et al. (2011) for a review) and

in Europe and central Asia (Wagstaff and Moreno-Serra 2009). Aterido et al. (2011) provide a review of the Mexican literature and other Latin American studies. They also provide a new set of estimates for Mexico whose *Seguro Popular* (SP) or People's Insurance provides explicit health coverage to the majority (55%) of the Mexican population not covered by the formal-sector health insurance program (the latter covers dependents as well as workers) (see e.g. King 2007; King *et al.* 2009). SP was launched in 2001 as a pilot program and was subsequently rolled out across the country over a period of several years; only in 2012 does the government expect to achieve 100 percent coverage of households not covered by the formal-sector or social security program. The staggered rollout of SP provides a credible way of identifying the impacts of the program on informality. Studies to date have found some effect, but the magnitude is very small. The study by Aterido et al. is the most recent and arguably the most credible, not least because it uses the longest time period, and exploits the panel nature of the data to eliminate individual- or household-specific effects. Aterido et al. find that SP reduced the probability of being in the formal sector by just half of one percent.

In this paper we explore the labor impacts of another universal health coverage program, namely the Thai Universal Coverage (UC) or 30-Baht scheme (cf. e.g. Pannarunothai *et al.* 2004). Like *Seguro Popular*, Thailand's UC scheme has become one of the world's most discussed universal coverage initiatives: the two reforms have in effect become the poster children of universal coverage reforms, with the architects of the systems engaging in extensive international public relations exercises to promote their reform model. The Thai reform is an especially interesting one from a labor market perspective. The Thai UC scheme differs from *Seguro Popular* in three respects, all of which

are likely to make for larger informality effects. First, there are minimal copayments and no joining fee in Thailand's UC scheme – everyone is covered automatically with a 100 percent subsidy. By contrast, in *Seguro Popular* households with the means to pay are supposed to contribute, so opting out of the formal sector scheme does not eliminate contributions.[3] Second, the relative generosity of the two schemes differs. Both are less generous than the social security scheme, but the gap is wider in Mexico: in 2000, spending per enrollee in *Seguro Popular* was just 66 percent that of the social security program; by contrast, on a per-beneficiary basis Thailand's UC scheme spends 85 percent of the amount spent by the social security scheme.[4,5] Third, the two countries' social security schemes treat dependents differently: Mexico's covers dependents while Thailand's does not. Thus in Mexico once one household member is in the formal sector, there is no incentive – from a health coverage perspective – for additional members to seek a formal-sector job. In Thailand, by contrast, at least prior to the UC reform, the health coverage rules gave each household member an incentive to seek a formal-sector job. Health coverage rules prior to the reform thus created an incentive for Mexican households to limit the number of formal-sector workers to one, but for Thai households to keep adding formal-sector workers. The informalizing effect of the Thai reform ought therefore to have been greater – a larger fraction of workers will have been encouraged to formalize prior to the reform, but discouraged from formalizing after the reform.

---

[3] It needs to be acknowledged that in practice contributions are less than would be expected from application of the means-testing rule to household survey data, and are a good deal lower for most workers than the social security contribution.

[4] The Thai scheme excludes a few high-cost interventions (e.g. renal transplants) but for the most part the schemes are fairly comparable in terms of their generosity.

[5] One feature of the Mexican SP program that may limit the disincentive to formalize is that it provides access to a different network of health facilities – sometimes these may be more accessible for a household than the facilities operated by the formal-sector health scheme.

We use an informal expected-utility framework that captures the risk of medical care expenses associated with informal employment prior to UC to explore the possible effects of the UC reform: we conclude that UC could cause unmarried individuals to prefer the informal sector over the formal sector, and may cause a married couple to favour having both of them working in the informal sector in preference to having the husband work in the formal sector and the wife not working. We explore empirically the impacts of the UC scheme on the probabilities of working, working in the informal sector, and working in the informal sector. Our identification strategy comes from the staggered rollout of the UC scheme over a period of 12 months, with six provinces adopting the program in April 2001, 15 provinces doing so in June 2001, 55 provinces and 13 districts in Bangkok following in October 2001, and the final set of Bangkok's districts adopting UC between November 2001 and April 2002. We estimate the labor market effects of UC by linking an individual's labor market status at a particular date to their length of exposure to the UC "regime", which will depend 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 at each of the 32 quarterly survey dates. In addition, since the month variable is available from 2001, we use this variable to increase the number of effective survey dates to 68 months. Each contains about 53,000 respondents and covers every province in the country. The accuracy of our estimates is enhanced by the fact that 13 of the 32 quarterly surveys we use predate the launch of UC: this allows us our regressions to capture the pre-reform trends in our labor market outcomes. We control for the potentially confounding effects of the Thai Village Fund that was also rolled out from 2001 onwards.

We find that UC appears to have encouraged employment especially among married women, to have reduced formal-sector employment among married men but not among other groups, and to have increased informal-sector employment especially among married women. We see the largest positive informal-sector employment effects in the agricultural sector. These results are consistent with our hypothesis that by eliminating the medical expenditure risks associated with informal-sector employment, UC has encouraged couples to work in the informal sector in a rural setting rather than live in the city and rely on earnings from a single breadwinner's formal-sector job. Our results suggest that UC increased informal-sector employment overall by two percentage points in the year of UC adoption rising to just under 10 percentage points after three years. This effect is, as expected, larger than that found for *Seguro Popular*. We end with some thoughts on the policy implications.

II. THAILAND'S UNIVERSAL COVERAGE SCHEME

Before the introduction of Universal Coverage (UC) in 2001, Thais were either not covered for their health care expenses (more than 25 percent of the population fell into this category) or were covered by one or more of the schemes operating at the time: see Hanvoravongchai and Hsiao (2007) and Table 1. The largest of the pre-UC schemes was the Medical Welfare Scheme (WHS) which provided tax-financed cover for various poor and vulnerable groups, including the poor, the elderly, children below the age of 12, secondary school students, the disabled, war veterans, and monks. These groups comprised around 33 percent of the population. The next largest scheme was the Health Card Scheme (HCS) –

a public voluntary insurance program for nonpoor households who were ineligible for the WHS. This accounted for around 12 percent of the population. The third largest scheme was the Civil Servants Medical Benefits Scheme (CSMBS) in which health insurance was provided as a fringe benefit to current and retired civil servants and their dependents. This scheme covered around 11 percent of the population in 2000. The final scheme was the Social Security Scheme (SSS). This scheme 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). Around 10 percent of the population was covered through this scheme in 2000. 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.[6]

Frustrated with the low coverage of the HC scheme and with the resultant large number of Thais without coverage[7], 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) fixed-fee per visit (hence the initial name – the Thirty 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 SSS schemes. The government budgeted as much as Baht 1,309 per enrollee in the UC scheme, a

---

[6] This masks the cross-subsidies from the public hospitals to the last two schemes.
[7] 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.

dramatic increase on the per-enrollee outlays of the HC and MWS schemes, and equal to 85 percent of the expenditure per enrollee in the SSS. 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). UC and SS enrollees are similarly restricted in their choice of provider, being required to choose a contracted hospital or its network; the contracted providers differ, however, between the two schemes. In both schemes, registration with a regular provider is required, and enrollees in both schemes have access to both public and private providers, though in practice most UC contracted providers have been Ministry of Public Health hospitals. One difference between the UC and SS schemes is that the latter but not the former provides certain cash benefits payable in the event of sickness, disability and death.

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.

III. **HYPOTHESIZED LABOR MARKET EFFECTS OF UNIVERSAL COVERAGE**

We explore in this section the possible labor supply effects of UC using a simple expected utility framework. We argue that UC could cause unmarried individuals to prefer

the informal sector over the formal sector, and may cause a married couple to prefer both working in the informal sector to having the husband working in the formal sector and the wife not working.

Consider first the case of an unmarried individual. If the person works in the informal sector, they get an income I_I but face a risk p of incurring medical costs equal to M. In the example in Figure 1, it is assumed for simplicity that $p=0.5$. The expected utility associated with informal-sector employment is EU_I in Figure 1. If the individual works in the formal sector, they get an income I_F, and in the event of illness all their medical bills are covered by insurance. Their (certain) utility associated with formal-sector employment is U_F which in Figure 1 exceeds EU_I; the individual therefore chooses the formal sector even though the income associated with formal-sector employment is lower than that associated with informal-sector employment. After UC, the risk associated with medical care costs is eliminated, and the individual can attain $U(I_I)$ with certainty by working informally, which is what they choose to do. This example illustrates that by eliminating the risk of medical care expenses UC may cause unmarried individuals to switch from formal-sector to informal-sector employment.

Consider next the case of a married couple. If both work in the informal sector, they get an income $I_{I,I}$ but face a risk (assumed to be 0.5 as before) of incurring medical costs equal to $2M$. The expected utility associated with husband and wife working in the informal sector is therefore $EU_{I,I}$ in Figure 2. An alternative is that the husband works in the formal sector, though this may require the couple live in an urban location and the wife may end up not working, either because of limited employment opportunities or because of limited

childcare and eldercare. In this case the couple gets an income equal to $I_{F,N}$. If illness strikes, all the husband's medical expenses are covered by insurance, but the wife's are not. The couple's income in the event of the wife falling ill is equal to $I_{F,N}$-M, and the couple's expected utility from the husband being in the formal sector and the wife not working is equal to $EU_{F,N}$, which in this example exceeds $EU_{I,I}$. The couple therefore chooses to have the husband in formal-sector employment and the wife not working even though this option yields a lower income than that associated with them both in informal employment. After UC, the risk associated with medical care costs is eliminated, and the couple can attain $U(I_{I,I})$ with certainty by both working informally, which is what they decide to do. This example illustrates that by eliminating the risk of medical care expenses UC may cause a couple to prefer to have both working in the informal sector rather than having the husband in the formal sector and the wife not working.

There is one factor that will likely limit – but not eliminate – the reduction in the incentive to be in the formal sector versus the other two labor market states, namely that the social security scheme likely provides greater access to private providers (cf. Table 2), which is likely to be considered a plus.

IV. METHODS

While a national program, UC was phased in, as explained above, over a period of nine months, with one set of provinces implementing the policy in April 2001, a second set two months later, a third set four months later, and the final set three months afterwards. We use this staggered rollout to identify the impacts of UC. From January 2002 (the date

the fourth and last wave of provinces implemented 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 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 labor market outcomes 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 for each quarter or 53,000 for each month on average), and a very large number of survey dates (32 quarterly or 68 effective 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 outcomes of interest.

When linking labor market outcomes to exposure to UC, 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; however, their inclusion does help to improve precision (cf. e.g. Angrist and Pischke 2009, p.237) so we include the obvious individual- and household-level covariates. Rather, our concern with omitted variable bias lies with events and/or programs that may have also had labor market effects and whose timing in a specific location coincides with the introduction of UC. The confounder we are most worried about is the 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. The VF aimed to stimulate local economies by providing to every village and urban community one million baht (around $22,500) in seed money for a locally-run micro-credit association (cf. Boonperm *et al.* 2012). Villagers could add this fund to their existing village's micro-credit institution, or if the village did not have one at the time, the money could be used to set one up. According to the socio-economic survey collected by the Thai national statistical office in 2004, 50 percent of VF borrowers used the loan to fund agricultural activities, 30 percent used the loan to fund daily expenses, while 14 percent used the loan to fund non-farm business (Boonperm *et al.* 2012). Given this, we expect that the VF may have had a positive impact on the number of informal and – to a lesser extent –formal jobs. We discuss below how we try to capture the effects of the VF in our estimations so that our UC impact estimates are not biased.

Let y_{ipt} be our outcome of interest for individual *i* in province *p* at quarter *t*. Our estimating equation takes the form:

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

where X_{ipt} is a vector of covariates at the household- and individual-level, the UC_{pi}^τ and VF_{pt}^α 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.[8] 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 $UC^{\tau}=0$ for $\tau\neq0$. In the quarter after implementation, $UC^1=1$ and $UC^{\tau}=0$ for $\tau\neq1$. In the quarter before implementation, $UC^{-1}=1$ and $UC^{\tau}=0$ for $\tau\neq-1$; however, we omit this UC dummy. In the quarter before this, $UC^{-2}=1$ and $UC^{\tau}=0$ 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 Mexico's *Seguro Popular*. 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. Like Bosch and Campos-Vázquez, 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 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 equalling one when the quarter is three or more years before

---

[8] This specification is similar to that used by Campos-Vázquez (2010), and is in effect a mix of the models discussed by Wooldridge (2002 p.317) and Angrist and Pischke (2009 p.237).

implementation (UC^{-3}); the second equalling one when the quarter is two years before UC implementation (UC^{-2}); the third equalling one when the quarter is between zero and 4 quarters after implementation (UC^0); the fourth equalling one when the quarter is between 4 and 8 quarters of implementation (UC^1); the fifth equalling one when the quarter is between 8 and 12 quarters of implementation (UC^2); and the sixth equalling 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.

V. DATA

Our data are from Thailand's Labor Force Survey (LFS) conducted by the National Statistical 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.[9]

Our outcomes of interest are whether the individual is working, and if so whether

they are in the formal sector or informal sector. LFS respondents are asked whether they

worked during the previous week, and if not whether they received a salary or wage and

whether they have a job to return to. If the individual was not working and had no job to

return to, we classified them as not working. Respondents who said they were employed

were asked about their type of employment, the categories being: (i) employer; (ii) self-

employed; (iii) unpaid family helper; (iv) government employee; (v) government

enterprise employee; and (vi) private employee. We classified (i)-(iii) as informal sector,

and (iv)-(v) as formal sector. As for (vi), the private employees, we classified those who

earned monthly salary as formal and those who earned hourly or daily wage in an

establishment with less than 10 workers as informal[10].

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

---

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

[10] Our definition of "formal sector" focuses on whether the workers are provided health welfare as well as other work-related welfare according to the Thai Labor Protection Act. This is because we aim to measure the impacts of UC. In the early 2000s, it was unlikely for daily or hourly workers to be registered under the SSS. The number of SSS insurers calculated using our definition is reasonably close to the actual number. For example, in 2003, our definition would suggests around 7.8-8.0 million workers under the SSS, whereas the actual number reported by the Social Security Office was 7.6 million. However, some may suggest that we count all private employees as formal workers (see for example, Chandoevwit(2004)) because they are all protected under the Thai Labor Protection Act. We tried using this definition for our estimations as well, but the change does not give significantly different results.

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 VF implementation[11]. 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.

---

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

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 over the time period 1997-2005. For each variable, we report their population-weighted mean and standard deviation. As discussed earlier, the dependent variables include whether the individual is working, whether they work in the formal or informal sector, and which industrial sector they are in. About 69 percent of all Thai population age 15 and above were working, while about 18.2 percent were in the formal sector.[12] In terms of industrial sectors, about 29 percent were in agriculture. The manufacturing, commerce and service sectors each employed about 11 percents of workers; while the construction sector employed about 4 percent of workers. 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, labor scarcity and the relative attractiveness of jobs in each province. The non-municipal area dummy variable, on the other hand, should highly correlate with the industrial sector the worker is in. For example, agricultural workers are more likely to live in non-municipal areas. The number of children under age 6, the number

---

[12] We categorize workers who were covered by the Civil Servant Medical Benefit Scheme (CSMBS) and the Social Security Scheme (SSS) as formal. The numbers of workers under the SSS are close to those given by the Social Security Office. For example, by using our definition, the number of SSS insurers would be between 7.8-8.0 million in 2003, while the actual number was 7.6 million.

of children ages 6-15, and the 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 the 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.

VI. **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 increases the likelihood of being in the formal sector, rural residents are less likely to be in the formal sector, and having school-age children reduces the likelihood of people being in the formal sector, as does having elderly household members. The Village Fund apparently increased the likelihood of working, but interestingly more in the formal sector than the informal sector.

Table 4 reports the estimates of the impacts of UC on the probability of working, and the probabilities of being employed in the formal and informal sectors. The latter are not conditional on working, so in the case of formal-sector employment the other states are not employed and informally employed, while in the case of informal-sector employment the other states are not employed and formally employed. The coefficients are the estimates of

the δ_τ in eqn (1) with the restrictions placed on the δ_τ as explained in section IV. The estimates show the "impact" of UC three and two years prior to its implementation (these coefficients ought not to be significantly different from zero), and the impacts of UC zero, one, two and three years after implementation.

Figure 3, Figure 4 and Figure 5 show the corresponding estimates and 95% confidence intervals for the more flexible version of equation (1) where the δ_τ are unrestricted. In all cases, we control for the effects of the village fund, as well as 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 some significant "effects" of UC prior to UC implementation which ought not to be evident if the effects we are estimating are causal relationships – unless they capture anticipatory effects. However, the number of significant prior "effects" in Table 4 is relatively few – just five out of 24 (20%). By contrast, there is a fairly high rate (just under 60%) of significant effects of UC after UC implementation, suggesting we can be reasonably confident we are estimating genuine causal effects.

Table 4 and to a lesser extent Figure 3 suggest that UC increased employment, especially among married women. This is consistent with our hypothesized effects of UC in section IV, namely that UC offers married couples the possibility of returning from the city where only the man may work (due to limited job opportunities or limited childcare) to the countryside where informal employment is available without the risk of medical expenses, and childcare may be available through elderly parents (or less necessary because the home is also the place of employment). Despite the positive effect of UC on the probability

of working, Table 4 suggests that UC reduced formal-sector employment, though only significantly so among men. The effect appears only with a lag, and is somewhat larger for married men. This gender-specific result and the larger effects among married men are also consistent with our hypothesized effects of UC in section IV. These effects are also apparent in the more general specification in Figure 4. Finally, Table 4 suggests that UC had a positive effect on informal employment. The effect increases with the length of "exposure" to UC, beginning at around 1-3% in the year of implementation, and rising to 7-13% three years after implementation. These effects – which are also evident in Figure 5 – are evident for both men and women, and are statistically significant among all four groups two and three years after implementation, and statistically significant among three of the four groups one year after implementation. The effects on informal employment are most pronounced among married women – consistent with our hypothesized effects of UC prompting a return of families from the city to the countryside.

Table 5 shows the effects of UC on formal- and informal-sector employment for the sample as a whole. For the sample as a whole we find a negative but insignificant impact of UC on formal-sector employment, but a positive and significant impact of UC on informal-sector employment starting at two percentage points in the year of implementation rising to 9.7 percentage points three years after. The formal and informal effects are not the mirror image of the other because the zero-coded category in each case includes not just the other category but also those not working. Also shown in Table 5 are the estimates of the effect of UC on formal- and informal-sector employment by sector. We see significant effects on formal-sector employment only in the case of the manufacturing sector, and the reductions implied are very small. By contrast, we see sizeable significant positive impacts

of UC on informal-sector employment, but only in the case of agriculture with an effect starting at three percentage points in the year of implementation rising to 13 percentage points after three years[13].

VII. **CONCLUSIONS**

Our analysis exploits the phased rollout of the Thai Universal Coverage policy to estimate the labor market effects of the policy. We control for the effects of the Village Fund scheme which began to be rolled out around the same time; we are able to separate out the effects of the two programs through the spatial and temporal differences in program rollout. Our results point to labor market effects: UC appears to have encouraged employment especially among married women, to have reduced formal-sector employment at least among married men, and to have increased informal-sector employment especially among married women. We see the largest positive informal-sector employment effects in the agricultural sector. These results are consistent with our hypothesis that by eliminating the medical expenditure risks associated with informal-sector employment, UC has allowed couples to switch from a situation where the breadwinner works in an urban formal-sector job and the spouse co-locates but does not work to having both working in informal-sector jobs in a rural setting.

---

[13] Since Thailand counts those who worked at least one hours during the week before as employed, this definition could result in a very high employment rate (especially in Thailand where the informal sector is large). We also explored the possibility that our results could be biased toward over-counting employment in the informal sector by only counting those who worked full-time (worked at least 40 hours during the week before). This did not change the results. We still see informal employment increased by 10.8 percentage points overall and by 13.8 percentage points in the agricultural sector.

Our effects suggest that UC increased informal-sector employment by two percentage points initially rising to just under 10 percentage points after three years. This effect is larger than that found in recent work for Mexico's *Seguro Popular* universal health coverage reform – there the negative effects on formal-sector employment were around half of one percent. However, as indicated in the Introduction, there are some reasons to expect a larger effect in Thailand. The gap in the benefit package in Mexico means that in Figure 1 the individual still faces some risk in an informal sector job even after the SP reform. So while SP reduces M it does not reduce it to zero. $U(I_I)$ is not attainable and the Mexican informal-sector worker has an expected utility after the reform that will exceed EU_I but may fall short of U_F. The fact that the Mexican informal-sector worker has to pay to join the SP scheme means that with insurance the individual's income in the state where illness does not occur is less than I_I; this further reduces the likelihood that the informal-sector job – even after the SP reform – dominates the informal-sector job. Finally, as mentioned in the Introduction, the fact that social security provides health coverage to all household members in Mexico but only to the worker in Thailand means that Thai households had an incentive prior to the UC reform to get additional household members into formal-sector jobs, while in Mexico the incentive was to get just one household member into the formal sector. This made for a bigger pool of workers in Thailand who would be incentivized by the UC reform to switch out of the formal sector.

Thailand's UC scheme – like Mexico's *Seguro Popular* scheme – does appear to have encouraged growth in informal-sector employment, although the negative effects on the formal sector in Thailand appear to have been very small. Formal-sector employment is not, of course, an end its own right, but nonetheless having a health insurance program that

biases employment growth toward the informal sector may well be considered unattractive. Several policy options suggest themselves. Some of these are likely to be considered unacceptable to Thai policymakers. One such option would be to deliberately encourage a per-capita funding gap between the UC scheme and the SS scheme, so that relying on the UC scheme has a "price" associated with it, in the form of a less generous benefit package. Another option would be to levy a contribution on UC enrollment, perhaps linked to the household's living standards through a "proxy means" test. This is also unlikely to be considered acceptable by the Thai government, and evidence from Mexico suggests that collecting means-tested contributions is expensive and hard to enforce. Successful enforcement might deter the very people the government is seeking to cover from enrolling. A more palatable option would be to merge the two schemes at some point in time and delink the payroll tax from health care entitlements, either by scaling back the payroll tax so it raises just enough to finance the occupational pension, or by treating what is currently the health part of the payroll tax as just a tax on labor that helps to fund government programs.

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.
Work status (of all population age ≥ 15)		
By employment (= 1, if not = 0)		
Work during the survey week or have a job to return to	0.69	0.46
By formality (= 1, if not = 0)		
Formal	0.18	0.38
Informal	0.50	0.50
By industry (= 1, if not = 0)		
Agriculture	0.29	0.45
Manufacturing	0.11	0.31
Construction	0.04	0.19
Commerce	0.11	0.31
Service	0.11	0.31
All other	0.04	0.21
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	1.50	0.50
Education - primary	0.62	0.49
Education - some secondary	0.17	0.38
Education - secondary	0.10	0.30
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	0.80
No. of elderly (age > 64)	0.31	0.60
Age (15-29)	0.36	0.48
Age (30-49)	0.40	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: Estimates of impact of UC on employment

			Years since implementation					
			-3	-2	0	1	2	3
Probability of working	single male	coef	-0.053*	-0.026	0.024**	0.038**	0.050*	0.070*
		t	-1.72	-1.46	2.35	1.98	1.84	1.91
	married male	coef	-0.010	-0.007	0.003	0.008	0.015	0.016
		t	-1.09	-1.12	0.72	1.10	1.39	1.05
	single female	coef	-0.012	-0.020*	0.011	0.023*	0.036**	0.075***
		t	-0.76	-1.91	1.37	1.74	2.02	2.71
	married female	coef	-0.060*	-0.025	0.025**	0.061**	0.080**	0.116**
		t	-1.68	-1.09	2.15	2.41	2.30	2.37
Formal employment	single male	coef	-0.021*	-0.011	-0.009	-0.018	-0.026	-0.030*
		t	-1.82	-1.18	-1.10	-1.26	-1.59	-1.75
	married male	coef	0.006	0.004	-0.002	-0.015	-0.033*	-0.032
		t	0.37	0.35	-0.25	-1.14	-1.71	-1.64
	single female	coef	-0.009	-0.011**	0.002	0.001	-0.005	-0.001
		t	-1.40	-2.11	0.54	0.26	-0.55	-0.12
	married female	coef	-0.015	-0.001	0.003	-0.003	-0.009	-0.004
		t	-1.44	-0.15	0.43	-0.28	-0.70	-0.23
Informal employment	single male	coef	-0.030	-0.013	0.034**	0.058**	0.078**	0.102**
		t	-0.86	-0.67	2.41	2.08	2.16	2.14
	married male	coef	0.006	0.008	0.014	0.040*	0.069**	0.074*
		t	0.17	0.34	1.20	1.74	2.10	1.80
	single female	coef	0.004	-0.003	0.011	0.026	0.046**	0.082***
		t	0.20	-0.27	1.19	1.62	2.02	2.75
	married female	coef	-0.043	-0.022	0.024	0.067**	0.093**	0.125**
		t	-1.05	-0.88	1.56	2.08	2.09	2.06

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 IV. The regressions include – in addition to the UC dummies – the VF dummies, province- and quarter-specific fixed effects, as well as 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.

Table 5: Estimates of impact of UC on employment, by sector

			Years since implementation					
			-3	-2	0	1	2	3
By Formality	Formal	coef	-0.008	-0.003	-0.000	-0.008	-0.019	-0.018
		t	-0.83	-0.36	-0.07	-0.81	-1.43	-1.33
	Informal	coef	-0.014	-0.006	0.021*	0.051**	0.075**	0.097**
		t	-0.46	-0.30	1.76	2.13	2.23	2.17
Formal sector By Industry	Agriculture	coef	-0.004	-0.003	0.001	0.000	-0.003	-0.001
		t	-0.54	-0.57	0.28	0.00	-0.37	-0.14
	Manufacturing	coef	0.001	-0.001	-0.002	-0.007	-0.011	-0.012*
		t	0.20	-0.76	-0.68	-1.30	-1.51	-1.92
	Construction	coef	0.000	0.001	0.001	-0.001	-0.001	-0.000
		t	0.27	0.97	0.91	-0.82	-0.91	-0.32
	Commerce	coef	0.001	0.001	0.000	0.002	0.001	0.001
		t	0.27	0.46	0.08	1.06	0.27	0.42
	Service	coef	-0.002	0.001	0.002	0.000	-0.004	-0.003
		t	-0.74	0.60	1.32	0.14	-1.45	-1.07
	All other	coef	-0.004*	-0.001*	-0.002**	-0.003*	-0.001	-0.002
		t	-1.78	-1.70	-2.24	-1.84	-0.77	-0.69
Informal sector By Industry	Agriculture	coef	-0.044	-0.019	0.026	0.063**	0.092**	0.129**
		t	-1.15	-0.84	1.59	2.02	2.17	2.42
	Manufacturing	coef	0.011**	0.006**	-0.001	-0.004	-0.009	-0.015**
		t	1.99	2.32	-0.47	-0.73	-1.25	-2.10
	Construction	coef	0.009**	0.005**	-0.003	-0.006	-0.009	-0.014*
		t	2.03	1.97	-1.18	-1.50	-1.60	-1.85
	Commerce	coef	0.006*	0.002	0.000	0.001	0.000	-0.004
		t	1.70	0.60	0.14	0.17	0.03	-0.57
	Service	coef	0.003	-0.001	-0.000	-0.002	0.001	0.002
		t	0.88	-0.37	-0.25	-0.77	0.25	0.45
	All other	coef	0.002	0.000	-0.001	-0.001	-0.000	-0.001
		t	1.06	0.39	-1.33	-0.81	-0.01	-0.73

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 IV. The regressions include – in addition to the UC dummies – the VF dummies, province- and quarter-specific fixed effects, as well as 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: The effects of UC on the labor supply decision of an unmarried individual
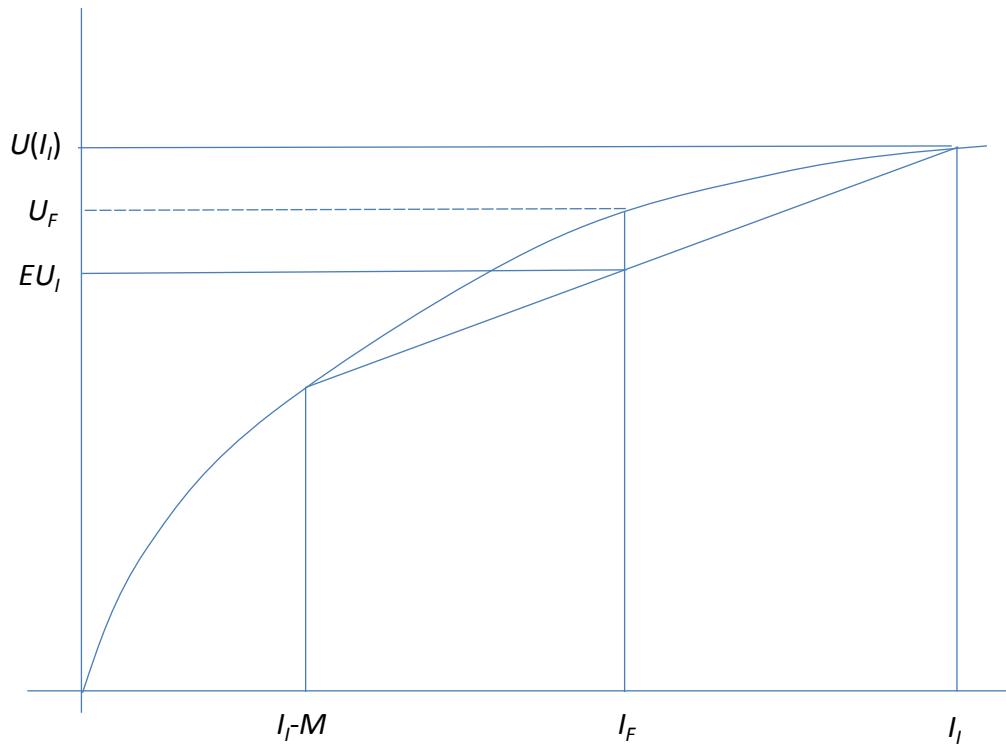
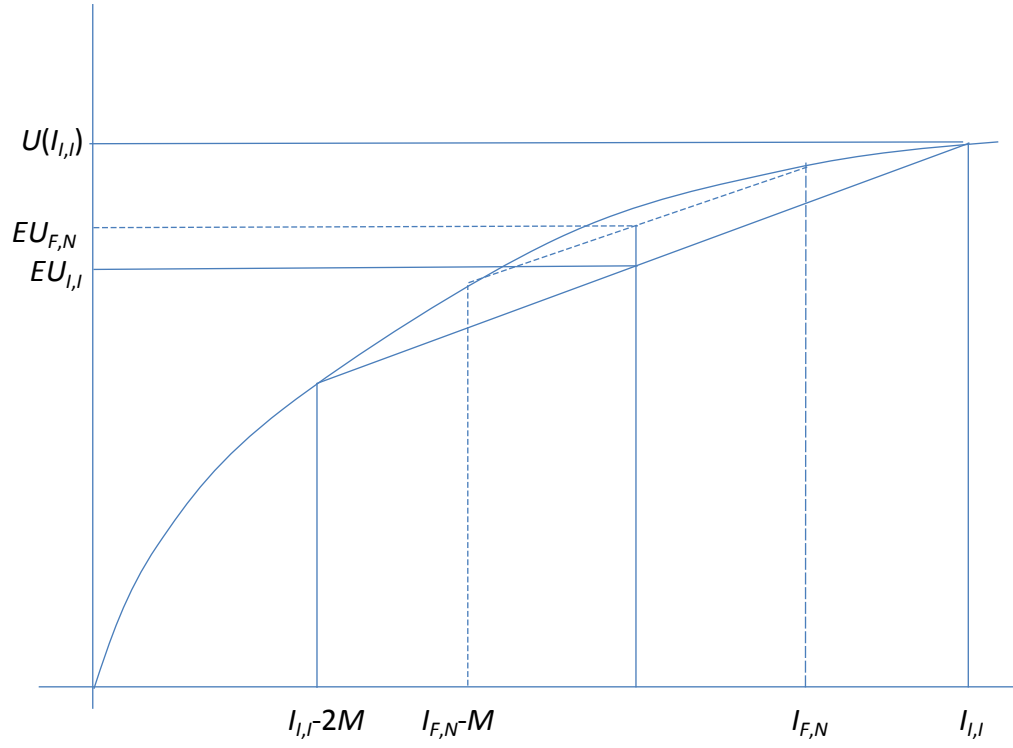Figure 2: The effects of UC on the labor supply decisions of a married couple

Figure 3: Impacts of UC on probability of working, by gender and marital status
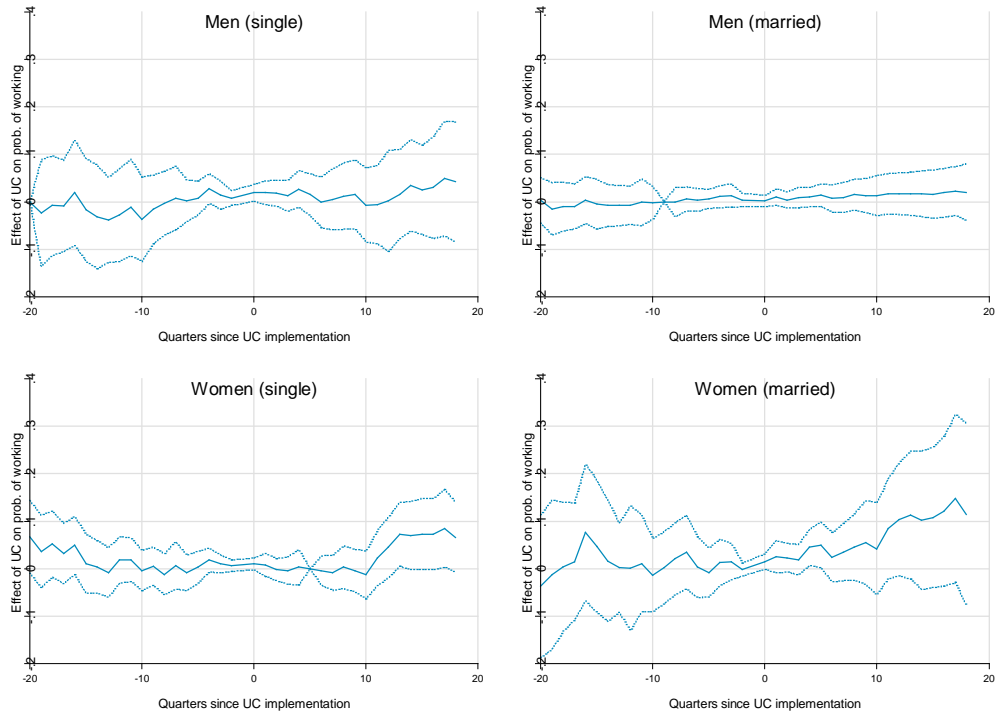
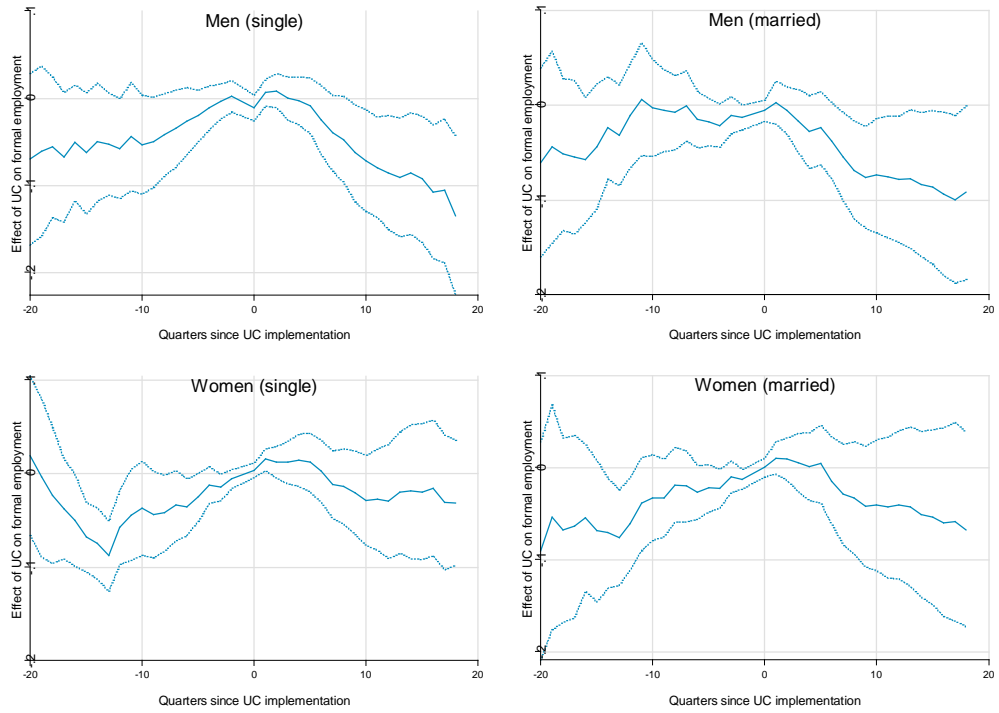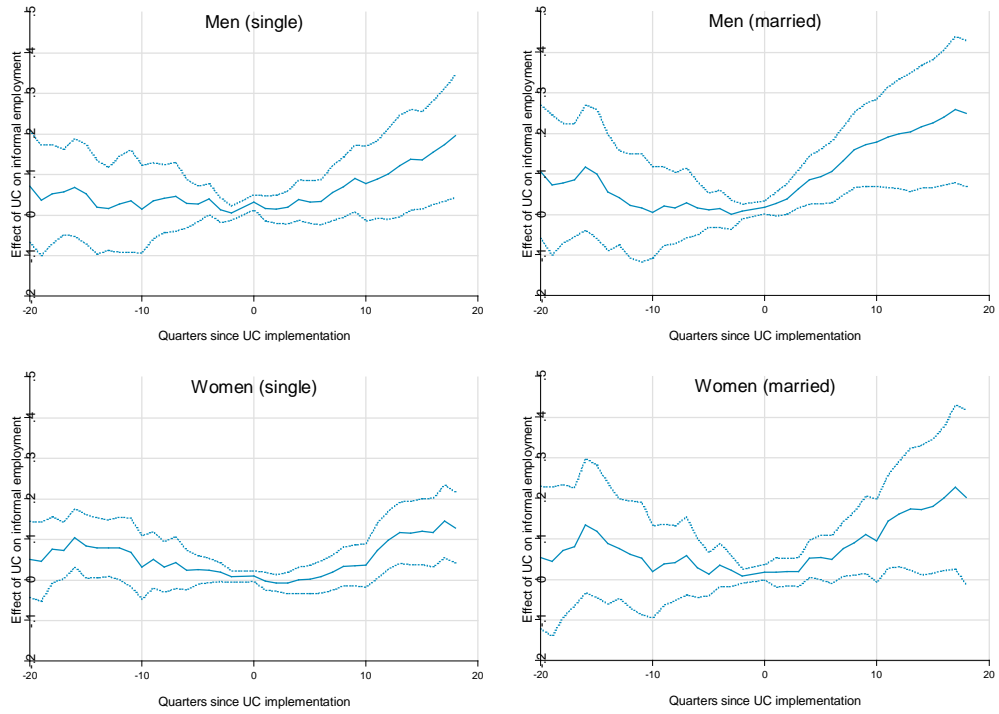Figure 4: Impacts of UC on formal employment, by gender and marital status

Figure 5: Impacts of UC on informal employment, by gender and marital status

References

Angrist, J. D. and J. S. Pischke (2009). Mostly Harmless Econometrics: An Empiricist's Companion. Princeton, NJ, Princeton University Press.

Aterido, R., M. Hallward-Driemeier and C. Pages (2011). Does expanding health insurance beyond formal-sector workers encourage informality ? measuring the impact of Mexico's Seguro Popular. The World Bank, Policy Research Working Paper Series, 5785.

Boonperm, J., J. Haughton, S. R. Khandker and P. Rukumnuaykit (2012). Appraising the Thailand village fund. The World Bank, Policy Research Working Paper Series, 5998.

Bosch, M. and R. M. Campos-Vázquez (2010). "The trade-offs of social assistance programs in the labor market: The case of the "Seguro Popular" program in Mexico."

Chandoevwit (2004). "Labor Market Issues in Thailand" TDRI Quarterly Review **19**(2): 10-15.

Damrongplasit, K. and G. A. Melnick (2009). "Early results from Thailand's 30 Baht Health Reform: something to smile about." Health Aff (Millwood) **28**(3): w457-66.

Frenk, J., E. Gonzalez-Pier, O. Gomez-Dantes, M. A. Lezana and F. M. Knaul (2006). "Comprehensive reform to improve health system performance in Mexico." Lancet **368**(9546): 1524-34.

Hanvoravongchai, P. and W. C. Hsiao (2007). Thailand: Achieving universal coverage with social insurance. In: Social Health Insurance for Developing Nations. W. C. Hsiao and R. P. Shaw (ed). Washington, D.C., World Bank**:** xi, 172.

King, G. (2007). "A 'Politically Robust' Experimental Design for Public Policy Evaluation, with Application to the Mexican Universal Health Insurance Program." Journal of Policy Analysis and Management **26 3**: 479-506.

King, G., E. Gakidou, K. Imai, J. Lakin, R. T. Moore, C. Nall, N. Ravishankar, M. Vargas, M. M. Tellez-Rojo, J. E. Avila, M. H. Avila and H. H. Llamas (2009). "Public policy for the poor? A randomised assessment of the Mexican universal health insurance programme." Lancet **373**(9673): 1447-54.

Levy, S. (2008). Good intentions, bad outcomes: Social policy, informality, and economic growth in Mexico. Washington, DC, Brookings Institution.

Lieberman, S. S. and A. Wagstaff (2009). Health Financing and Delivery in Vietnam: Looking Forward. Washington, DC, World Bank.

Na Ranong, V. and et al. (2004). The First Year of Universal Health Coverage in Thailand. The Monitoring and Evaluation of Universal Health Coverage in Thailand, Second Phase 2003-04. Bangkok, Thailand Development Research Institute (TDRI).

Pannarunothai, S., D. Patmasiriwat and S. Srithamrongsawat (2004). "Universal health coverage in Thailand: ideas for reform and policy struggling." Health Policy **68**(1): 17-30.

Sakunphanit, T. (2006). Universal Health Care Coverage Through Pluralistic Approachs: Experience from Thailand. Bangkok, ILO Subregional Office for East Asia.

Social Security Office (2003). Annual Report 2003 Social Security Office. Nonthaburi Thailand, SSO.

Satsanguan, P. (2006). Specialized Financial Institutions: A Policy Tool to Fill the Gap Left by the Commercial Bank? Toward a Decade after the Economic Crisis: Lessons and Reforms. Bangkok, TDRI.

Srithamrongsawat, S. (2002). The Health Card Scheme: A subsidized voluntary health insurance scheme. In: Health Insurance Systems in Thailand. P. Pramualratana and S. Wibulpolprasert (ed). Bangkok Health Systems Research Institute (HSRI)**:** 79-93.

Wagstaff, A. and R. Moreno-Serra (2009). Social health insurance and labor market outcomes: Evidence from central and eastern Europe, and central Asia. In: <u>Innovations in Health System Finance in Developing and Tranisitional Economies. Advances in Health Economics and Health Services Research (Volume 21)</u>. D. Chernichovsky and K. Hanson (ed). Bingley, United Kingdom, Emerald Group Publishing 83-106.

Wooldridge, J. M. (2002). <u>Econometric analysis of cross section and panel data</u>. Cambridge, Mass., MIT Press.

World Health Organization (2010). <u>World health report: health systems financing: the path to universal coverage</u>. Geneva, WHO.