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The impact of Universal Health Coverage on healthcare consumption and risky behaviours: evidence from Thailand

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Abstract

Thailand is among one of the first non OECD countries to have successfully introduced a form of Universal Health Coverage (UHC) in 2002. This policy defines a natural experiment for the evaluation of the effects of public health insurance on health behaviours. In the present paper, we look at the impact of the Thai UHC on preventive activities, risky behaviours and healthcare consumption using data from the 1996, 2001 and 2003 Health and Welfare Survey of Thailand. We use double robust estimators combining propensity scores and linear regressions to estimate Difference-in-Differences (DD) and Difference in DD (DDD) models. Results offer important insights. First, previously uninsured men and women increased their preventive activities (check-ups) more than any other groups. At the same time, there is no evidence of either an increase in risky behaviours or a reduction of preventive efforts by the newly insured population. In other words, we find no evidence of *ex ante* moral hazard. Regarding healthcare consumption, we see that hospital admissions increased by 2% and outpatient visits increased by 13% due to the UHC. Overall, these findings imply positive health impacts among the Thai population who entered in the UHC.

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1. Introduction

Government commitment to extend healthcare access to the general population is usually known in the literature as Universal Health Coverage (UHC). Although the central idea of a UHC is to provide a package of basic health services without causing financial hardships to the user (Savedoff *et al*., 2012), regardless of his/her social status or income, the actions and the institutional settings chosen might vary widely from country to country (Moreno-Serra and Smith, 2012). Importantly, any UHC system requires a proper institutional setting and a certain degree of income redistribution. Despite the obvious appeal of the concept, its implementation in low or middle income countries might not be practical and politically straightforward.

Thailand is among one of the first non OECD countries to successfully introduce a form of UHC. In fact, the UHC had long been advocated by health reformists in the Ministry of Public Health and the Health Systems Research Institute. During the electoral campaign, the Thai Rak Thai (TRT) used the catchy slogan of "30 Baht treats all diseases", meaning that the party was willing to introduce a tax-based UHC with a fixed co-payment fee of 30 Thai Baht (US$ 0.75 at the time). By 2002, Thailand successfully provided UHC in all its geographical areas.

This paper examines the impact of the Thai UHC on preventive activities, risky behaviours and healthcare consumption using data from the 1996, 2001 and 2003 Health and Welfare Survey of Thailand. To identify the impact of the programme on the above mentioned outcomes, we need to isolate the impacts of other variables that may have also caused the change in the outcomes. Such variables as household income and other policy changes are usually time-changing confounding factors (Lee, 2005). For this purpose, we adopt the Difference-in-Differences (DD) approach. An additional Difference in DD (DDD) analysis is also performed to relax the DD's typical identifying assumption of parallel trends.

Measuring the impact of the Thai UHC is important for many reasons. First of all, although health protection for everyone represents one of the main *goals* for low and middle income countries, the debate on the best ways to achieve this goal is far from over since a UHC is not the only possible approach. In this sense, two specific, and somehow contrasting concerns, were raised regarding the magnitude of the impact of the programme. On the one hand, budget constraints might have reduced its potential impact, not affecting the utilization of health services among poor households. On the other hand, the UHC's essentially free nature might

set perverse incentives driving to overconsumption of healthcare and a reduction in the preventive efforts by the newly insured (i.e. *ex ante* moral hazard). Since co-payments have not been increased (actually dropped in 2007) and costs of healthcare have been raising constantly, both concerns are still shared among experts from different fields. Indeed, part of these worries were confirmed by the authoritative recent Oregon Experiment (Baicker et al, 2013), where Medicaid has been shown to increase financial protection and accessibility to healthcare services, but with ambiguous impacts on health and healthy behaviours.

The Thai UHC deserves more attention also because it represents an important case study for many other Asian countries involved in the process of universalizing the access to healthcare. In commenting about the strengths and the weaknesses of the Chinese health system, for example, Li *et al*. (2011) draw important lessons from the Thai UHC, defined as a successful experience for a developing country. Also, the Thai way seems to have gained a consistent political consensus both within the ASEAN economic community and across the whole Asia. For example, in a recent ASEAN+3 (China, Japan and South Korea) Health Ministers' meeting in July 2012, it was reported that "Thailand would transfer its knowledge to help regional countries, including Japan, South Korea and China, fully implement universal health coverage" (Sarnsamak, 2012). The extent to which the Thai experience could and should be exported to other Asian contexts obviously depends on its capacity to provide actual health benefits to the general population. Precise impact evaluations of the 2002 policy innovation can then still provide important evidence relevant for the actual health policy and political debate.

Results offer important insights. First, previously uninsured women increased their preventive activities (check-ups) more than any other groups, consistent with the findings of Gruber *et al.* (2012). At the same time, among the informal workers that entered the UHC in 2002, smoking decreased while drinking rate remained stable. Hence, there is no evidence of either an increase in risky behaviours or a reduction of preventive efforts by the newly insured population. In other words, we find no evidence of *ex-ante* moral hazard. Regarding healthcare consumption, we see that hospital admissions and outpatient visits increased due to the UHC. We also provide weak but consistent evidence that the UHC implied a switch from informal (traditional healers) to formal medicine. Overall, we interpret these findings as showing that the UHC increased healthcare consumption without causing significant increases in opportunistic behaviours.

The rest of the paper is organized as follows. Section 2 provides a policy background. Section 3 introduces the data. Identification and the empirical approach are discussed in Section 4 while results are presented in Section 5. The last part of the paper discusses the findings and concludes.

2. Background: the Universal Health Coverage in Thailand

The Universal Health Coverage in Thailand was introduced in 2001. Before, the provision of the UHC and public health insurance was limited to certain groups of citizens. In particular, government and formal sector private employees were fully covered by a mix of private payrolls and government subsidies (through the Civil Servants Medical Benefit Scheme, CSMBS, and the Social Security Scheme, SSS, respectively). In addition, the poor and vulnerable groups (elderly, children below the age of 12, the disabled, war veterans, and monks) were covered by the Medical Welfare Scheme (MWS). These schemes were altogether covering less than two-thirds of the population, leaving informal sector employees and farmers out of any form of health insurance (Evans *et al.*, 2012). Rather than changing the structure of the whole system, the TRT "added" the UHC as another independent public health insurance scheme. The Thai UHC scheme was then to cover everyone not already being insured by either the CSMBS or the SSS. Thus, those who were previously eligible for the MWS were transferred to the UHC. Ten years later, this three-tier scheme still exists, although proposals for setting a unified National Health Insurance system have been repeatedly discussed.

In terms of coverage, the UHC has been very successful. The number of people under the scheme in 2008 was around 47 million (4.2 million for the CSMBS and 9.1 million for the SSS). According to the World Bank, 99.5% of the Thai population is now covered by at least one type of health insurance.

In more details, the UHC provides a comprehensive benefit package stipulated in a contract between purchaser and provider at every level of health services. The benefits can be classified into three groups: the curative package, covering outpatient and inpatient services (with some exclusions); high-cost care, adopting a similar package to the one provided by SSS; disease prevention and health promotion, entailing immunizations, annual physical checkups, counseling, voluntary HIV counseling and testing, and antenatal care and family planning services (Wibulpolprasert and Thaiprayoon, 2008).

Beneficiaries must register with a primary contractor, typically a Community Hospital at the district level, from which they are entitled to free care: up until 2006, a co-payment of 30 Baht (approximately 0.75 US$) was required both for inpatient and outpatient services, with exemptions for previous beneficiaries of the MWS. Moreover, beneficiaries are not allowed into secondary or tertiary care units without referral from the primary contractor, except for accidents or emergency situations. The contracting of primary healthcare services and referral backup are intended to guarantee the rational use of resources by level of care, preventing the bypassing of ordinary care for unnecessary specialist hospital care (Tangcharoensathien *et al.*, 2010).

The payment scheme of the contractors is mixed, comprising capitation for outpatient care, prevention and promotion (plus additional fees for certain services), a fee schedule (centrally managed) for accident and emergency intervention outside the registered network, and global budget plus DRG (Diagnose-Related Groups) for inpatient services. The financing method of the scheme rests on progressively levied general tax revenue rather than on a contributory system, which, in a country with more than 50% of the population in the informal sector and a lively presence of very small establishments, is considered administratively difficult from the point of view of collecting and enforcing contributions (Tangcharoensathien *et al.*, 2010). Moreover, financing out of general tax revenue results in a greater progressivity of health care finance (Tangcharoensathien *et al.*, 2007), a quality that suits well the pro-poor spirit of the reform.

Regarding the impact of the Thai UHC, most of the evidence focuses on the financial impact of the new insurance system on the income of Thai families, especially the poorest. Limwatton *et al*. (2011) show that, while in 1992 health expenditure among the poorest households was 8.17% of their monthly income comparing to 1.27% among the richest decile, in 2006 these figures were 2.23% and 1.07% respectively. General taxation and lack of co-payment mechanisms should also be associated to effective redistributive patterns. The pro-poor nature of the scheme is evident from the consistently progressive Concentration Indexes pointed out by Tangcharoensathien *et al*. (2010) and Limwatton *et al*. (2011). Moreover, catastrophic payments appear to have dropped sharply in all the income quartiles (Somkotra and Lagrada, 2006).

Although positive health effects following the introduction of different UHC schemes in other Asian countries can be found in the literature (e.g. Lee *et al*., 2010), the link between health

insurance and health outcomes is sometimes challenged in the literature (e.g. Levy and Meltzer, 2008). Regarding Thailand, the evidence is surprisingly limited only to the very recent literature. Patcharanarumol *et al.* (2011) shows that Thailand achieved good maternal and child health after the introduction of the policy. More recently, Gruber *et al*. (2012) show that the Thai UHC significantly reduced infant mortality among the poorest, while Wagstaff and Manachotpong (2012) find a negative effect of Universal Coverage on the likelihood of people reporting themselves to be too sick to work.

Finally, the impact of the Thai UHC on healthcare consumption has been analysed by Panpiemars et al (2011) on a sample of 640 hospitals across the whole Thailand. They find that while the outpatient visits increased after the UHC, the number of inpatient and the average length of stay decreased after 2002. Although these findings are only partially in line with ours, it should however be pointed out that all the results in Panpiemars et al (2011) rely on aggregated data. In their case, identification is obtained by comparing aggregated data at hospital level before and after the introduction of the UHC (i.e. before/after study). Our approach here relies on individual-level data, comparing the trends of healthcare consumption between a treatment and a control group. This allows for a more precise identification of the policy impact.

3. Data

The dataset used in this study is the Health and Welfare Survey conducted by the National Statistics Office of Thailand (NSO). This survey is a household survey where all members in the selected household are interviewed. The information asked include each household and each member's demographic and socio-economic characteristics (such as age, gender, income, work status, occupation and education), health status, health care utilization, expenditure on health, smoking, drinking and exercise patterns. A stratified sampling framework was employed for the surveyed households to represent their municipal area, province and the entire country. Each household is given a survey weight. It is worth noting that the Health and Welfare Survey was not collected at a regular frequency during our period of interest, which is before and after 2002 when the UHC was introduced. It was collected every five years before 2001. Then, every year after 2003. The sample size of this survey increased over time. The pre-2001 surveys covered about 45,000 households; while the number increased to approximately 65,000

households by the mid-2000s. In this paper, we use the 1996, 2001 and 2003 data to evaluate the impact of the UHC on preventive activities, risky behaviours and healthcare consumption.

4. Methods

We adopt the Differences-in-Differences (DD) approach to measure the impact of the UHC on preventive activities, risky behaviours and healthcare consumption. By design, the control group has to be formed by those who were unaffected by the UHC. Since the UHC aimed to cover everyone except those who were under either the CSMBS or the SSS, the CSMBS and the SSS beneficiaries would serve as our control group. This group can be directly identified from the Health and Welfare survey.

The treatment group, on the other hand, should cluster all the individuals that before 2001 lacked any form of insurance. These individuals are easy to identify from the 1996 and 2001 surveys. In 2003, however, we do not have complete information about the past insurance status. In particular, we are not able to distinguish between people previously covered by private insurance cards (e.g. Volutary Health Insurance) and people who were not insured before 2002. Our treatment group for 2003 is thus constructed by selecting only the individuals with a sufficiently high probability of being uninsured in 2001. Specifically, from the 2001 survey we calculate, conditional to a set of observable variables, two propensity scores: one for being uninsured and one for having an insurance card. The estimated parameters are then used to predict the two probabilities of reporting being covered by the UHC for the individuals in 2003. Each person is finally assigned to the category for which the estimated probability is higher. Other definitions of treatment group are also used below in order to test the robustness of the results to the definition of the treatment group used here (see Section 6).

Given an outcome variable (Y), we will compare the change in Y for the treatment group with the change in Y for the control group. If the UHC had an impact, the change in Y would not be the same for the treatment and the control group.

It is important to note that any DD analysis relies on the *parallel trend* assumption (Lee, 2005). That is, without the policy, the change in outcomes in the treatment group should be similar to the change in the control group. This way, we can interpret differences in the differences as only due to the treatment/policy. However, the *parallel trend* is a rather strong assumption

since the trends of the outcomes can differ between the treatment and the control groups. For this reason, when possible[1], it is preferable to allow for group-specific pre-treatment trends, resulting in a Differences in the DD model (DDD). In a DDD, instead of assuming that the outcomes in the two groups would change by the same amount, we allow for different pre-treatment trends for the two groups (see, for example, Wagstaff, 2010). More specifically, we assume that the trends we could observe before 2002 (between 1996 and 2001) for each group separately would be maintained also in 2003 if the UHC had not been introduced.

The most important criticism to a DD applied to non-randomized data is that people with particular unobservable characteristics might select themselves into the treatment group (i.e. selection bias). As a result, the treatment effects estimated by the DD method would be biased – picking up not only the real treatment effects but also the unobserved differences. In our case, however, we do not believe that the DD estimation results would be subject to a serious selection bias problem. Firstly, if the treated and control individuals are fundamentally different in their behaviours, the differences in DD (DDD) would be able to account for this through the group-specific pre-treatment trend. Secondly, even if we cannot observe whether people moved out of the formal sector in order to obtain UHC, the likelihood of this happening is rather small. The UHC package is not any better than the existing SSS and CSMBS and having a stable formal job is likely to be considered as more attractive than having an unstable informal job. Indeed, from our perspective, the Thai UHC represents a natural experiment where selection into treatment is ruled out by a unique policy design.

Besides identification, the estimation technique can make a difference to the final results. A good methodology is to reduce the bias from observable confounders. Stratification, regression or matching can be used for such purpose. In our analysis, we adopt the Double-Robust (DR) estimator (Bang and Robins, 2005; Robins, Rotnitzky and Zhao, 1995; Lunceford and Davidian, 2004) as defined in the implementation of Emsley *et al.* (2008).

According to this method, the average causal effect for the difference in the outcomes (Y) between the control and the treatment group in one year is found by a three-step process. In the first step, classical propensity scores p_i (i.e. the probability of being in the treatment group (T) conditional on a set of each individual's characteristics, X) are computed for each

[1] The DDD computes group-specific trends using pre-treatment data. Thus, it requires at least 2 periods of pre-treatment data.

individual. In the second step, the outcome Y is regressed on X for each group. Predicted values of Y, $Y_i\widehat{(T,X)}$ and $Y_i\widehat{(C,X)}$, are computed for each individual within each group. Finally, the Lunceford and Davidian (2004) index for the Double-Robust estimator is calculated for each year (t):

$$DR_t = \frac{1}{N}\sum_{i=1}^{N}\frac{T_{i,t}Y_{i,t} - (T_{i,t} - p_{i,t})\, Y_{i,t}\widehat{(T,X)}]}{p_{i,t}} - \frac{1}{N}\sum_{i=1}^{N}\frac{C_{i,t}Y_{i,t} + (T_{i,t} - p_{i,t})\, Y_{i,t}\widehat{(C,X)}]}{(1 - p_{i,t})}$$

This estimator is called double-robust because it applies both propensity-scores-probability weighting and linear regression. As pointed out by Emsley *et al.* (2008), when both models are correctly specified, the DR estimator is a semiparametric efficient estimator. If only one of the two is valid, the DR estimator is offering protection against misspecification. Obviously, when both models are misspecified, the resulting estimate will be biased, but all alternative methods would be as well. The variance of the DR estimator is provided in Lunceford and Davidian (2004) and is applied here.

Once the DR_t is calculated for each year, the DD and DDD estimators can then be obtained as follows (see, for example, De Preux, 2011):

$$DD = DR_{2003} - DR_{2001}$$

and

$$DDD = (DR_{2003} - DR_{2001}) - (DR_{2001} - DR_{1996})$$

The standard errors of the DD and DDD are obtained from the bootstrapping method.

5. Results

5.1 Descriptive statistics

Descriptive statistics are shown in Table 1. The total sample for the three waves of the Health and Welfare survey includes 94,800 observations. Of these, 65,766 are used for the DD analysis.

TABLE 1 ABOUT HERE

Outcome variables are reported at the beginning of the table. In general, treated individuals display less healthy behaviours than their control counterparts. For example, they are more likely to smoke and less likely to have medical check-ups. Patterns related to drinking, instead, show a partially different picture, as in 2003 treated individuals are found to drink less than control ones, and to engage less in dangerous activities like driving while intoxicated. In terms of health utilization and health outcomes, treated individuals are less likely to have outpatient visits and are more likely to seek treatment from traditional healers. In addition, treated individuals are less likely to be hospitalized (almost 50% less than controls), but at the same time they tend to stay longer in hospital.

The mean out-of-pocket expenditure for inpatient episodes is higher in the treated group. An exception applies to the year 1996 where the CSMBS patients were still allowed to be hospitalized in private hospitals, but were subject to a 50% copayment. This copayment could contribute to out-of-pocket expenditure for the control group. However, from 1998, private inpatient usage was restricted to only emergency cases. Inpatient care for CSMBS patients were only allowed in public hospitals where there was no copayment. This could explain why the out-of-pocket expenditure for hospitalizations decreased sharply after 1998 for the control group.

Socio-demographic variables are relatively stable across groups and years. The control group is in general slightly older than the treatment group, but this difference should not be worrying and it gets smaller for more recent waves. The proportion of females in the treatment sample changes by year. However, in general, women represent around 40-47% of the sample. It is difficult to find a clear pattern for what concerns the civil status of the subjects. The majority are married or single. People working in the informal sector have more children. This is in line with the fact that they are more likely to have no education and less likely to hold a university degree. Government employees are around the 35% of the overall sample. Since they are all covered by the CSMBS, they are all in the control group. Also, note that, as should be expected, the employees in the formal sector are less likely to have no-education and more likely to have a university degree. Finally, regional variations in the distribution of different healthcare schemes, which seem to play a particularly important role in the 1996 survey, tend not to differ significantly between the two groups in later waves.

FIGURE 1 ABOUT HERE

Figure 1 shows the distribution of three main variables of interest (hospitalizations, outpatient visits and smoking) across age and by treatment/control status. The full-body lines represent the non-linear (quadratic) fit of the data aggregated by age for all individuals not covered by either the CSMBS or the SSS, while the dotted lines are for the control group. The comparison between these distributions in 2001 (pre-policy period) and 2003 (post-policy) graphically shows the pattern that is particularly important for our analysis: the gap between the treatment and the control groups tends to narrow down in 2003 as compared to 2001. For hospitalizations and outpatient visits, this gap reduction is present across all age groups and is particularly more accentuated for the elderly.. For smoking, the gap is reduced among the mid-old age, while for the youngsters the smoking gap seems to have reduced less.

5.2 Estimation results

Although, as shown in Table 1, confounders are relatively well distributed in the original sample, we first show how propensity scores perform in our sample. It is worth noting that these scores are not directly used for a matching-based estimation of an average causal effect, but only indirectly as probability weights in the calculation of the DR. The test is based on the standardized bias, which for each covariate computes the difference in means between treated and controls, standardized by the square root of the sum of variances in each group. These biases were computed by performing nearest neighbour matching in each year. Results are reported in Table 2.

TABLE 2 ABOUT HERE

It can be noticed that matching on the propensity scores performs well in reducing biases. As the treatment and control groups are defined essentially on the basis of their work status (which is what determines the eligibility of an individual for CSMBS and SSS insurance), matching does not reduce the bias in variables concerning the work status of individuals, particularly the government employees. Other variables--especially those related to age, family composition and education--are very well balanced across the two groups after matching. Two measures that prove the overall quality of the procedure are the mean and median of the absolute standardized biases. These two statistics jointly consider the biases in all the variables included in the analysis, and should be considerably reduced after matching has been performed. Given a threshold of standardized mean difference of 20 (as suggested in

Rosenbaum and Rubin, 1985), it can be inferred that in all the three years matching is very effective in balancing covariates. This is because the mean absolute biases are all below 15, and the median absolute biases are all below 5. These evidences once again confirm the appropriateness of our procedure.

Table 3 reports the results for both the DD and the DDD analysis. The DD analysis is necessary for all the variables that do not appear in the 1996 surveys.

TABLE 3 ABOUT HERE

For lifestyle indicators, the gap in the prevalence of risky behaviours between the treated and the control group is either not affected or reduced by the UHC. People in the treatment group smoke and drink relatively more than others. However, this is true regardless of the UHC. Smokers and drinkers actually reduced their smoking and drink driving more than the control group. At the same time, no significant impact on drinking is detectable.

The most interesting result here is however related to preventive check-ups, a variable that is more directly linked to the introduction of the UHC. The impact of the policy is clear: between 2001 and 2003 the difference between the treatment and the control group in the probability of going through preventive check-ups reduced by almost 70%. For women, for example, while before the UHC the probability of a check-up was 16% higher for a formal worker, after the enactment of the reform this difference dropped to a 5% (i.e. an impact of more than 11%). The picture emerging from these figures is consistent with the idea of a substantial demand for preventive medicine which could not be met due to the lack of insurance among the informal sector. In this sense, the UHC might also have a significant effect in terms of reduction of future healthcare costs due to earlier and more precise diagnoses.

The Universal Coverage also had an impact on the likelihood of being hospitalized. Before the UHC, there was a 2% difference between groups in the probability of receiving inpatient healthcare, a difference that basically disappears in 2003. Also the average length of stay for the hospitalized informal employees increased (not significantly) after the introduction of the UHC, a result that might be more related to the incentive system designed for providers than to actual patients' demand. Similar results but with greater magnitude in the gap reduction are found for the outpatient visits. In particular, before the Universal Coverage was introduced, there was a clear downward trend in the use of outpatient care for the informal sector. The

parallel trend assumption is not consistent with this picture and the DDD results are more reliable: the UHC boosted an increase in the outpatient care of around 13%. Similarly, we find a marginally significant reduction of 1.7% in the probability of reporting oneself as ill, in line with Wagstaff and Manachotphong (2012). Finally, as found by most of the existing literature, the out-of-pocket expenditure is also found to reduce significantly due to the UHC.

6. Additional evidence

In order to explore more in depth the effects of the policy, the analysis was also performed on specific subsets of the sample, so as to identify whether there were any noteworthy patterns related to certain demographic groups. In particular, the impacts of the Universal Coverage on preventive activities, inpatient and outpatient services were investigated splitting the sample according to age (adults aged between 20 and 45 and between 45 and 80), gender, and municipal vs. rural area.

TABLE 4 ABOUT HERE

Results are reported in Table 4. Smoking decreases in all the groups considered, although almost twice as much among the elderly and women. Drinking is not affected by UHC. This is also consistent with the results obtained from using the entire sample, with the exception of a 5 percentage points decrease in women's drinking behaviour. The likelihood of being hospitalized increases by 3-4 percentage points in all subsamples, and especially so among the elderly, females, and in rural areas. Similarly, out-of-pocket expenditures drop in every group: the most pronounced effect is observed for men (about 7800 Baht decrease), followed by the old and those living in municipal areas.. The average length of the last stay in hospital increases significantly only in the group of women and for people living in towns (5.6 and 6.5 more days spent in the hospital, respectively). Reporting illness, in turn, shows a significant decrease only among the elderly and men. Finally, the likelihood of using outpatient services shows a sizeable increase in all the investigated subsamples: remarkably, in this case the least pronounced effect is found among the elderly (7 percentage point increase), whereas for other groups the DDD estimator is well above 10 percentage points.

The impact of the policy, therefore, appears to be quite evenly distributed across the population, since most of the health indicators under study are found to be significantly affected in the same direction for all subgroups. The differences that arise between one group

and the other may suggest that, despite the widespread effect of the reform, some categories of individuals might be benefiting more. The differences between men and women favour alternatively both groups, depending on the outcome under study. On the other hand, elderly people benefit more than the youngsters in almost all the investigated dimensions as expected.

The fact that in many instances the rural group is outperformed by those living in municipal areas might come as an odd, given the explicit target of the policy towards the rural sector. However, it can be observed that the likelihood to be hospitalized increases more in this group, and that at the same time the reporting of minor illnesses is reduced (although not significantly so) only among these people, pointing to a more pronounced impact of the reform on these dimensions. Moreover, if we imagine that countryside men and women are generally poorer than those in towns and cities, the fact that these people experience a higher increase in their likelihood to be hospitalized clearly reinforces the interpretation of an income effect at work, allowing poorer individuals to expand their budget set and in this way to obtain the treatment they need. This line of thought might appear in contrast with the results for out-of-pocket expenditures. However, it is plausible that less-poor people are more likely to have used more expensive inpatient services before the implementation of the UHC, while the poorest may have not been able to bear high hospitalization expenses, in this way incurring lower costs for medical care.

Another interesting result emerging from a preliminary, descriptive analysis is about traditional medicine. It appears, in fact, that between 2001 and 2003 there was a reduction in the rate of the UHC beneficiaries treating their illness with the services of a traditional healer (from 2.5 to 1.5 percent), together with an increase in the figure for formal outpatient services (from 51.6 to 56 percent). Results for the control group, instead, show a slight increase in usage of traditional medicine (from 1 to 1.1 percent) and an equally thin rise in the rate for formal medical care (from 67 to 67.7 percent). Given that absolute numbers were very low (only 51 observations reported to have used traditional medicine in 2003), we had to forego a formal inferential analysis of these interesting patterns, leaving these issues for future research, maybe with the support of a more specific dataset. However, this evidence is consistent with the idea that access to health insurance, with the drop in expenditures it entailed, could induce a substitution effect between formal medical services and these alternative forms of care, in favour of the former. If these lines of thought are proven correct,

the UHC scheme would entail a beneficial increase in the amount of medical care provided to citizens through a switching of patients from less effective treatments.

One important final issue is related to the definition of the treatment group. The approach taken in this paper is to include in the 2003 treatment group all the individuals that were likely to be uninsured in 2001. However, one might think that other definitions of the treatment group might be equally suitable in this context. In particular, the most relevant alternative is to include all the uninsured, regardless of their insurance status before 2001. After all, the UHC applies also to these people. Obviously, since these individuals were receiving some forms of health insurance, their inclusion in the treatment group is expected to reduce the impact of the UHC. Indeed, this treatment group realistically sets a *lower bound* for the estimation of the impact of the UHC. In table A1 in the Appendix we report the results the DD and the DDD estimation with this definition of the treatment group. The main conclusions from Table 3 do not change substantially, showing that estimates are particularly robust. Specifically, the only relevant difference is in the smoking variable. When this larger treatment group is considered, the UHC does not seem to have any significant impact on smoking prevalence. This suggests that people with voluntary insurance cards in the pre-UHC period were also the ones less likely to smoke. Another variable that is quantitatively affected by the new definition of the treatment group is the out-of-pocket expenditure, though the reduction in the impact of the UHC in this case is expected.

7. Discussion

In this paper we investigate the impact of the introduction of a Universal Health Coverage scheme in Thailand in 2002. The analysis relies on almost 95,000 subjects included in three waves (1996, 2001, 2003) of the Thai Health and Welfare survey. Performing a series of DD and DDD analyses, we are able to identify the effects of the UHC on healthcare consumption and preventive behaviours. If our results are correct, the UHC seems to have levelled the playing field, where not only those holding a formal job could get a subsidised access to health care services. Results however must be considered with caution. Here we tackle a number of points relevant for their interpretation.

The first set of interesting findings is that the UHC does not have a negative impact on the preventive activities of the Thais covered by the new governmental insurance scheme. Indeed,

we could not find any evidence of a change in drinking rate. If anything, we found that the UHC reduced smoking prevalence.

These results can be interpreted within the stream of literature related to healthcare insurance and opportunistic behaviours. Since the original work of Pauly (1968) the idea that an insured individual does not internalize the cost of the health service, thus taking more risks *ex ante* and consuming more healthcare *ex post* (i.e. moral hazard), has influenced the policy debate in many countries. The well-known RAND experiment, detecting significant levels of overconsumption among the people randomly assigned to insurance groups with low or no co-payments, set the stage for the empirical and the experimental evidence on this topic (Manning *et al*., 1987). Regarding *ex ante* moral hazard, the most recent literature has produced some further interesting evidence. The most recent randomized evidence in this sense comes from the Oregon experiment (Baicker et al, 2013), where a group of uninsured low-income adults in Oregon was selected by lottery to be given the chance to apply for Medicaid. Two years after the beginning of the experiment, some of the evidence pointed towards an increase in the smoking prevalence gap between the treatment group selected by the lottery and the control group who lost the lottery. This difference, however, was not statistically significant. Recent non-experimental evidence comes from Spenkuch (2012), who finds moral hazard among Mexican families after the introduction of the Seguro Popular en Salud, while Steinorth (2011) shows that the same phenomenon is observed when looking at the Health Savings Accounts in the US. Evidence on *ex post* overconsumption of healthcare has been found, for example, by Liu *et al.* (2012) in Croatia, van Dijk *et al*. (2012) in the Netherlands and Sapelli and Vial (2003) in Chile. Note, however, that all these studies, with the exception of Spenkunch (2012), identify the effect by looking at changes in the co-payment rate or in the additional insurance. Not much was known about the sudden introduction of a comprehensive UHC among people previously uncovered by any formal health insurance.

Regarding *ex-ante* moral hazard, our findings tell a different story, showing that in the case of Thailand the UHC was not associated to an increase in risky behaviours. Of course the presence of *ex-ante* moral hazard might have been counterbalanced by anti-smoking and anti-drinking campaigns. However, these campaigns should have a different impact on the treatment and the control groups. Moreover, while tobacco control policies might well have been at work in 2002, alcohol has become a public health priority only in more recent times. If anything, our results

thus show that it was relatively straightforward for Thailand to prevent any serious perverse incentive effect of the UHC on the health behaviours of the newly insured.

Besides a lack of support for *ex ante* moral hazard, we find that the number of hospitalizations, check-ups and outpatient visits has significantly increased due to the UHC. Most importantly, the positive impact is mainly driven by the fact that the UHC *reduced the pre-2002 gap* between formal and informal workers regarding the use of these services. In our view, these results confirm the "access motive" pointed out in Nymann (1999), i.e. when insured, people consume more healthcare because they can afford something that without insurance would have been too expensive to buy.

One problem with this view is the sudden and significant picking up of the number of check-up visits following the UHC introduction. Typically, check-up visits by healthy people are sensitive to the phenomenon of *ex post* moral hazard. In theory it can thus be more difficult to interpret this specific result in terms of increased accessibility to health services. However, in our data the likelihood of having a check-up visit for individuals under the UHC is always lower than for individuals in the control group, even in 2003. Unless one is willing to assume significant levels of insurance-driven overconsumption in the SSS and CSMBS schemes, the convergence between different schemes should show that before 2002 *under-consumption* of preventive healthcare among informal workers might have been the norm and that the UHC partially corrected this substandard trend. Moreover, note that these results are confirmed also when looking at women separately, something that appears to be in line with the results in Gruber *et al.* (2012).

Our findings also show a significant increase in the length of stay in hospital for certain subgroups of the population due to the UHC. This result is likely to be driven by the incentive system at the hospital level. Initially in 2002, providers were given the option of receiving reimbursements based on either total capitation or capitation for outpatient services and the DRG for inpatient services at the provincial level. However, UHC began using a single payment system in 2003. On the opposite, the CSMBS used a DRG payment system, which implicitly encourages lower length of stay and fast hospital discharges.

Reducing the financial burden from sickness is one of the main objectives of the UHC. In doing so, however, the system should also work on the extensive margin, including people that would

not have used a service otherwise. Altogether, our results show that the UHC did not only reduce the financial burden related to health expenditures at family level (consistent with the previous literature), but effectively enabled a broader access to medical care without seriously affecting risky behaviours.

Positive conclusions should not be overstretched. The approach taken here is to explicitly prioritize the internal validity of the study, building on a coherent and rigorous identification mechanism. The extent to which the short run effects of the UHC can be projected on the medium and long term remains to be evaluated. The trade-off here is evident: the further away we move from the implementation time, the more difficult it becomes to interpret any DD model as a causal effect. Endogenous UHC innovations such as differential investments or changes in the incentive structure and external factors like the economic cycle or modifications of the pension system might influence individual behaviours even more than the UHC itself, making it difficult to isolate one effect from another. Although questionable, our identification strategy is thus deliberate and explicitly motivated. In addition, it should be pointed out that the subsequent Health and Welfare surveys do not contain comparable questions to the 2001 and 2003 ones. Hence, in analysing medium run impacts we would have had to give up part of our outcome variables.

Finally, one last note regarding the interpretation of our findings. Clearly, the quest for universal access to healthcare is raising and goes beyond the evaluation of its impact. Health represents a fundamental part of human capital and provides a necessary condition for a well-functioning life. Thus, universal access to health care can be the *goal* in itself. Such a goal can be justified independently from its actual impact in terms of usage and moral hazard distortions – relying on a purely ethical *a priori* background (Roger, 2007) or on a well-known "functionings-capabilities" approach (Sen, 1985, 1992). In providing results related to the impact of the UHC, we are by no means claiming that equity and ethical issues should not play a role, or even represent the main drivers for the implementation of a universally accessed healthcare system. However, given an objective (in our case, universal access to health care), it is still true that different policies might in principle be implemented to reach it. Such alternatives may depend, among other things, on the stage of development of the country, on the quality of its institutions and on the political acceptability of the various policy proposals. In this sense, by pointing out the encouraging effects of the Thai UHC, our paper provides evidence that, among the diverse tools available to policy-makers to grant the right to health to

their citizens, a government-based Universal Coverage scheme may well be a valid candidate in this direction. .

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Tables

Table 1: Descriptive Statistics

Outcomes						
VARIABLES	Year 1996		Year 2001		Year 2003	
	Treated	Controls	Treated	Controls	Treated	Controls
Smoking	0.421	0.251	0.36	0.206	0.291	0.185
Drinking	0.37	0.381	0.421	0.382	0.434	0.44
Drinking and Driving	N/A	N/A	0.493	0.475	0.47	0.481
Check-Up	N/A	N/A	0.078	0.363	0.474	0.728
Check-Up (Women)	N/A	N/A	0.09	0.356	0.135	0.334
One Hospitalization	0.038	0.041	0.038	0.064	0.047	0.06
Out-of-Pocket Expenditure	5489.8	9155.5	6286.5	1917.6	2918.7	1438.5
Frequence of Hosp	N/A	N/A	3.894	3.818	1.272	1.216
Days in Hospital	7.335	6.324	6.381	6.181	6.116	5.628
Being Sick	0.123	0.102	0.122	0.108	0.144	0.156
Outpatient	0.544	0.584	0.516	0.671	0.561	0.677
Traditional Healer	0.022	0.01	0.025	0.01	0.015	0.011
CoVariates						
VARIABLES	Year 1996		Year 2001		Year 2003	
	Treated	Controls	Treated	Controls	Treated	Controls
Age	38.09	36.72	39.28	37.76	39.53	39.18
Children	0.891	0.677	0.79	0.656	0.714	0.595
Female	0.477	0.474	0.396	0.496	0.419	0.492
Single	0.177	0.239	0.194	0.231	0.214	0.202
Married	0.758	0.714	0.703	0.715	0.666	0.738
No Education	0.049	0.009	0.043	0.007	0.028	0.008
University	0.023	0.238	0.074	0.403	0.094	0.376
Government Employee	0	0.34	0	0.431	0	0.379
Employer	0.035	0.011	0.074	0.017	0.086	0.023
Bangkok	0.087	0.176	0.098	0.096	0.085	0.101
Observations	20337	8697	22780	27964	5842	9180

Table 2: Balancedness Test - Standardized Percentage Bias

		Year 1996			Year 2001			Year 2003		
VARIABLES	Sample	Treated	Controls	% Bias	Treated	Controls	% Bias	Treated	Controls	% Bias
Age	Unmatched	38.091	36.716	12	39.285	37.759	13.8	39.534	39.176	3.3
	Matched	38.083	38.921	-7.3	39.285	39.168	1.0	39.540	39.254	2.6
Children	Unmatched	0.891	0.677	22.6	0.790	0.656	14.1	0.714	0.595	13.7
	Matched	0.885	0.892	-0.7	0.790	0.782	0.8	0.713	0.724	-1.3
Bangkok	Unmatched	0.087	0.176	-26.5	0.098	0.097	0.7	0.085	0.101	-5.6
	Matched	0.088	0.098	-3	0.098	0.100	-0.5	0.085	0.078	2.3
No Education	Unmatched	0.049	0.009	24.1	0.043	0.007	23.5	0.028	0.008	14.9
	Matched	0.046	0.049	-1.4	0.043	0.032	7	0.028	0.024	2.8
University	Unmatched	0.023	0.238	-67.3	0.074	0.403	-83.7	0.094	0.376	-70.4
	Matched	0.023	0.025	-0.5	0.074	0.075	-0.4	0.094	0.099	-1.2
Female	Unmatched	0.477	0.474	0.7	0.396	0.496	-20.2	0.419	0.492	-14.7
	Matched	0.477	0.463	2.9	0.397	0.371	5.2	0.419	0.397	4.5
Single	Unmatched	0.177	0.239	-15.3	0.194	0.231	-9.1	0.214	0.202	3.1
	Matched	0.177	0.160	4.1	0.194	0.182	2.8	0.214	0.198	4.1
Married	Unmatched	0.758	0.714	9.9	0.704	0.715	-2.5	0.666	0.738	-15.8
	Matched	0.759	0.789	-6.9	0.704	0.725	-4.7	0.667	0.699	-7.1
Mean Absolute	Unmatched			28.4			25.5			27.5
Standardised Bias	Matched			7.9			10.9			11.1
Median Absolute	Unmatched			19.2			13.7			15.9
Standardised Bias	Matched			2.9			2.5			3.2

Table 3: DD and DDD estimates

OUTCOME	DID 2001-1996	[95% Conf. Interval]	DID 2003-2001	[95% Conf. Interval]	DDD	[95% Conf. Interval]
			Preventive Activities			
Smoking	0.029	[-0.003; 0.023]	-0.039**	[-0.035; -0.004]	-0.068**	[-0.057; -0.005]
Drinking	0.032**	[0.006; 0.032]	0.019	[-0.013; 0.025]	-0.012	[-0.034; 0.016]
Drinking and Driving	N/A		-0.060**	[-0.099; -0.009]	N/A	
Check-Up	N/A		0.090**	[0.016; 0.085]	N/A	
Check-Up (women)	N/A		0.111**	[0.078; 0.147]	N/A	
			Inpatient			
One Hospitalization	-0.018**	[-0.026; -0.012]	0.017**	[0.007; 0.023]	0.034**	[0.022; 0.044]
Out-of-Pocket Expenditure	3848.5**	[2055.1; 5863.6]	-2738.3**	[-4690.2; -1280.5]	-6586.8**	[-10587.8; -3993.9]
Days in Hospital	-1.709**	[-4.92; -0.08]	1.601	[-0.68; 4.3]	3.310	[-0.25; 8.04]
Frequency of Hospitalizations	N/A		0.078	[-0.09; 0.275]	N/A	
			Outpatient			
Reporting Illness	0.009**	[0.005; 0.033]	-0.008	[-0.027; 0.002]	-0.017**	[-0.050; -0.008]
Using Outpatient Services	-0.1**	[-0.159; -0.068]	0.035**	[0.007; 0.106]	0.136**	[0.091; 0.242]

**p-value<0.05

Table 4: Heterogeneity Analysis for the DDD

OUTCOME	YOUNG	OLD	MALE	FEMALE	MUNICIPAL	RURAL
			Preventive Activities			
Smoking	-0.047**	-0.091**	-0.056**	-0.08**	-0.076**	-0.075**
Drinking	-0.014	0.034	0.022	-0.053**	0.012	-0.03
			Inpatient			
One Hospitalization	0.034**	0.04**	0.03**	0.045**	0.039**	0.046**
Out-of-Pocket Expenditure	-7075.7**	-7223.2**	-7802**	-6497.6**	-7050.24**	-5923.2**
Days in Hospital	2.955	3.145	0.492	5.601**	6.521**	1.808
			Outpatient			
Reporting Illness	-0.001	-0.061**	-0.031*	-0.006	0.004	-0.027
Using Outpatient Services	0.14**	0.071**	0.12**	0.125**	0.152**	0.149**

*p-value<0.1 **p-value<0.05

Table A1: Lower bound: DD and DDD Estimates

OUTCOME	DID 2001-1996	[95% Conf. Interval]	DID 2003-2001	[95% Conf. Interval]	DDD	[95% Conf. Interval]
			Preventive Activities			
Smoking	0.013	[-0.00; 0.02]	0.003	[-0.01; 0.02]	-0.006	[-0.02; 0.01]
Drinking	0.021**	[0.00; 0.03]	0	[-0.01; 0.01]	-0.19	[-0.03; 0.01]
Drinking and Driving		N/A	-0.075**	[-0.113; -0.038]		N/A
Check-Up		N/A	0.068**	[0.040; 0.092]		N/A
Check-Up (women)		N/A	0.098**	[0.059; 0.120]		N/A
			Inpatient			
One Hospitalization	-0.02**	[-0.03; -0.01]	0.014**	[0.00; 0.02]	0.034**	[0.02; 0.04]
Out-of-Pocket Expenditure	2419**	[119; 4007]	-683	[-1528; 14]	-3102**	[-5262; -880]
Days in Hospital	0.99	[-1.54; 4.25]	-2.24	[-5.26; 0.04]	-3.231	[-9.29; 0.65]
Frequency of Hospitalizations		N/A	0.07	[-0.02; 0.19]		N/A
			Outpatient			
Reporting Illness	0.015**	[0.00; 0.02]	0.001	[-0.01; 0.00]	-0.016	[-0.03; 0.00]
Using Outpatient Services	-0.1**	[-0.15; -0.06	0.017	[-0.05; 0.07]	0.123**	[0.02; 0.20]

**p-value<0.05; N/A: data not available

Figure 1

Hospital visits
2001
2003
0
.05
.1
.15
.2
20
40
60
age
Graphs by yr
Smoking
2001
2003
0
.1
.2
.3
.4
20
40
60
age
Graphs by yr
Outpatient visits
2001
2003
0
.5
1
20
40
60
age
Graphs by yr
trend (treatment group)
trend (control group)
mean (treatment group)
mean (control group)

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