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

by

Simone Ghislandi [a], WanwiphangManachotphong[b], Viviana M.E. Perego[a]

[a]ECONPUBBLICA, Department of Policy Analysis and Public Management, Bocconi University, Milan, Italy
[b] Department of Economics, Thammasat University, Bangkok, Thailand

Keywords: Universal Health Coverage; Moral Hazard; Healthcare Consumption; Thailand

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Abstract

Thailand is among the first non-OECD countries to have introduced a form of Universal Health Coverage(UHC). This policy employs a natural experiment to evaluate the effects of public health insurance on health behaviours. In this paper, we examine the impact of Thailand's UHC programme on preventive activities, unhealthy or risky behaviours and healthcare consumption using data from the Thai Health and Welfare Survey. We use doubly robust estimators that combine propensity scores and linear regressions to estimate Differences-in-Differences (DD) and Differences-in-DD (DDD) models. Our results offer important insights. First, UHC increases individuals' likelihood of having an annual check-up, especially among women. Regarding healthcare consumption, we observe that UHC increases hospital admissions by over 2% and increases outpatient visits by 13%. However, there is no evidence that UHC leads to an increase in unhealthy behaviours or a reduction of preventive efforts. In other words, we find no evidence of *ex ante* moral hazard. Overall, these findings suggest positive health impacts among the Thai population covered by UHC.

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Introduction

Thailand is among the first non-OECD countries to have introduced a form of Universal Health Coverage(UHC). This policy employs a natural experiment to evaluate the effects of public health insurance on health behaviours–especially in a middle-income country. In this paper, we examine the impact of Thailand's UHC programme on preventive activities, unhealthy or risky behaviours and healthcare consumption using data from the Thai Health and Welfare Survey.

Before 2001 when the UHC was introduced, public health insurance was limited to certain groups of citizens. In particular, government employees and their dependents were covered by the Civil Servants Medical Benefit Scheme (CSMBS). Private employees (in the formal sector) were covered by the Social Security Scheme (SSS). The poor and the vulnerable groups were covered by the Medical Welfare Scheme (MWS), whereas workers in the informal sector could pay a small fee join the Voluntary Health Card scheme (VHC). These schemes, nonetheless, were covering less than two-thirds of the population. Most workers in the informal sector were uninsured (Evans *et al.*, 2012). Thailand's UHC scheme was then aimed at filling this gap. The scheme covered everyone not already insured by either the CSMBS or the SSS. In other words, it insured all the informal workers and people not in the labour force. By 2008, 99.5% of the Thai population were covered by one of the insurance schemes–about 70% were under UHC.

The range of benefits of UHC include curative services, high-cost treatments such as coronary artery bypass, and a wide range of preventive care. Initially, there was a 30-Baht (approximately 0.75 US$) co-payment for both inpatient and outpatient services, but it was eliminated in 2006.

Although its introduction dates more than ten years ago, the assessment of UHC in Thailand goes beyond its historical and geographical context. First, the Thai system has gained political

consensus within the ASEAN economic community: at the ASEAN+3 (ASEAN plus China, Japan and South Korea) Health Ministers' meeting held 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). In this light, precise impact evaluations of Thai UHC are crucial to prove the worth of such knowledge transfer.

The main contribution of this study lies in the identification of UHC impact on healthcare consumption and healthy behaviours. Our approach is to treat the UHC as a quasi-experiment. We carefully specify a treatment and a control group. Then, employ the treatment effects method to identify the impact of UHC on health behaviours and consumptions. Evidence from Thailand is particularly useful because it provides a useful case study of a government funded scheme in a middle-income country–where financial and medical resources may be limited. Our results suggest that Thailand's UHC system leads to positive impacts on health consumption without inducing unhealthy behaviours.

Moreover, our results also contribute to the debate regarding impact of public insurance schemes on moral hazard. 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 debate has recently been supported by the so-called Oregon Health Experiment conducted in the USA (Beicker, 2013). In this sense, the Thai UHC provides a good and still under-researched quasi-experiment for measuring whether and how people change their behaviours following free access to healthcare.

Most existing studies explore the impact of the Thai UHC on financial outcomes, e.g. households' income, especially the poorest. Most studies find that the Thai UHC largely benefits

the poor (Limwatton *et al*., 2011; Somkotra and Lagrada, 2006; Tangcharoensathien *et al*., 2010). Evidence on accessibility, health outcomes and health activities, on the other hand, is surprisingly rather limited and recent. Patcharanarumol *et al.* (2011) show that Thailand achieved adequate maternal and child health after the introduction of the UHC. Gruber *et al*. (2012) find that the Thai UHC significantly reduced infant mortality among the poorest. Wagstaff and Manachotphong (2012a) find the scheme reduces the likelihood of people reporting themselves as too ill to work. Finally, using a hospital-level dataset, Panpiemras *et al*. (2011) find that UHC increased outpatient visits, while reduced the number of inpatients.

In this study, we use an individual-level dataset to examine the impact of the Thai UHC on preventive activities, unhealthy behaviours, and healthcare consumption. In particular, the outcomes that we assess are the probability of having an annual check-up, drinking, smoking, drink-and-drive, out-of-pocket expenditure on health, and frequency of hospitalizations. Unlike previous studies, our analysis also account for possible difference in the trends (of the outcomes of interest) between a treatment and a control group. This allows for a more precise identification of the policy impact.

Data and Methods

The dataset used in this study is the Health and Welfare Survey (HWS) conducted by the National Statistics Office of Thailand. The HWS is a household survey for which all members in the selected household are interviewed. The information asked includes basic household's information and each member's characteristics, health status, healthcare utilisation, and lifestyle patterns. A stratified sampling framework was employed for the surveyed households to represent their municipal area, their province and the entire country.

We use data from the 1996, 2001 and 2003 rounds of the survey: our total sample thus includes 94,800 observations. Descriptive statistics are shown in Table 1 (outcome variables are reported at the beginning of the table).

TABLE 1 ABOUT HERE

The Differences-in-Differences (DD) approach is adopted to measure the impact of UHC. For each outcome (*Y*), the impact is estimated by comparing the change in *Y* over time within the treatment group with the change in *Y* within the control group. If UHC had an impact, the change in *Y* would differ between the treatment and control groups. When 1996 data were available, in order to relax the well-known assumption of parallel trend (e.g. Lee, 2005), we also correct the DD for the different pre-UHC trends in the outcomes in the two groups with a Differences-in-DD (DDD) model (see, for example, Wagstaff, 2010).

Our control group is composed of the CSMBS and the SSS beneficiaries. These individuals may be directly identified by the "type of health insurance" variable in our data. The treatment group should include all the individuals who were uninsured before 2001. These individuals are easy to identify from the 1996 and 2001 surveys. For the 2003 survey, however, we are unable to distinguish between people who were previously covered by a voluntary insurance (i.e., VHC) and people who were previously uninsured. Our treatment group for 2003 is then constructed by selecting only the individuals with a sufficiently high probability of being uninsured in 2001. Specifically, from the 2001 survey we calculate two propensity scores (conditional to a set of observable variables): one for being uninsured and one for having a voluntary insurance card. The estimated parameters are then used to predict the two probabilities for the individuals that in 2003 report themselves as being covered by the UHC. Each person is finally assigned to the category for which the estimated probability is higher. A

broader definition of the treatment group, which include all the UCS beneficiaries, is also used as a robustness check in the last part of the analysis.

Once the treated and the control individuals were identified, we first employed the Double-Robust (DR) estimator to estimate the intra-period group differences in each outcome (see supplemental material and also Lunceford and Davidian (2004) and Emsley*et al.* (2008)). The set of covariates used for the estimation is reported in Table 1. After the DR_t is calculated for each year, the DD and DDD estimators were then obtained as follows (see De Preux (2011)):

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

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

Standard errors were obtained by bootstrapping.

Results

Figure 1 preliminarily shows the trends of three main outcome variables: inpatient admission, outpatient visits and smoking. Although results must still be tested formally, the effect of the UHC on inpatient and outpatient visits seems evident.

FIGURE 1 ABOUT HERE

Matching works well, the balancedness test shows that the treatment and the control groups contain comparable samples (Appendix 2). Table 2 reports the final results for the DD and DDD analysis. Simple DD is performed for those outcome variables that are not available in the 1996 survey.

TABLE 2 ABOUT HERE

Concerning indicators related to unhealthy or risky behaviours, UHC does not increase the tendency to smoke, drink, or drink-and-drive. In fact, smoking prevalence is reduced. The UHC increases the likelihood that a person has at least one annual check-up by 9%. This impact is greater (11%) among women. In terms of inpatient usage, UHC increases the likelihood of hospitalisation by 1.7%. This represents approximately 30% of the post-UHC hospitalization rates among the treated. UHC also reduces out-of-pocket expenditure per inpatient episode by 6,586 Thai Baht (THB). As for outpatient care, UHC raises the likelihood that an individual will seek formal outpatient care for his/her illness (as opposed to other actions such as doing nothing or visiting a traditional healer) by 13.6%. A small but significant reduction of 1.7% can also be observed in the probability of reporting oneself as ill. In Table 3 the same analysis is performed on different subsamples.

TABLE 3 ABOUT HERE

The comparison of UHC impacts across different subsamples may cast doubt on whether UHC actually benefits the poor more, e.g. out-of-pocket expenditure decreased less among rural residents (who usually have lower income) than among municipal residents. However, the pre-treatment out-of-pocket expenditure among rural residents was lower to begin with–about 5,195 THB per hospitalisation vs. 6,853 THB per hospitalisation for municipal residents. With respect to the pre-treatment values, the reduction in out-of-pocket expenditures is in fact greater among rural residents. Moreover, the fact that the likelihood of being hospitalised increases more, and the reporting of minor illnesses is reduced (although not significantly) among rural residents actually support that UHC benefits the poor more.

Another important issue is our definition of the treatment group. In this paper, the treated individuals are those who were covered by UHC in 2003 but were likely to be uninsured in

2001. The most relevant alternative is to include all UHC beneficiaries, regardless of their previous insurance status (see Table 4). Overall, however, results in Table 4 are very consistent with our main findings drawn from Table 2, and thus confirm the validity of our findings.

TABLE 4 ABOUT HERE

A final point worth mentioning is about the usage of traditional medicine. According to the descriptive statistics (Table 1), it appears that, between 2001 and 2003, the rate of UHC beneficiaries seeking treatments from traditional healers dropped from 2.5% to 1.5%. At the same time, the usage of formal outpatient services increases from 51.6% to 56%. As for the control group, these rates remain almost unchanged. Given a very small relevant sample of observations with reports of using traditional medicine in 2003, we had to forego the formal inferential analysis of these interesting patterns. However, this evidence suggests that UHC may encourage beneficiaries to switch away from informal to formal medical treatments: an additional benefit of UHC might therefore be a reduction in the use of ineffective treatments in favour of more effective ones.

Discussion

This paper investigates the impacts of Thailand's UHC scheme on preventive activities, healthcare consumption and unhealthy behaviours. Our work is among the few that assess the impact of an introduction of a comprehensive UHC scheme to a previously uninsured population. The results suggest that UHC increases preventive activities and improves access to healthcare.

Regarding impacts on unhealthy or risky behaviours, we find that UHC does not increase the tendency to smoke, drink, or driving drunk. In fact, the programme reduces smoking. These

results provide a counter-evidence to the recent findings by Baicker et al. (2013), who find a positive (although not statistically significant) impact of the Medicaid programme on smoking prevalence using data from the Oregon Experiment. Overall, our findings may be considered as an instance in which the *ex ante* moral hazard theory might not apply.

Moreover, similarly to many existing studies on *ex post* moral hazard (e.g. Manning *et al.*, 1987;Sapelli and Vial, 2003; Liu *et al.*, 2012; van Dijk*et al*., 2012), we find that UHC increases healthcare consumption, i.e. the number of annual check-ups, the likelihood of hospitalisations, the duration of hospital stays, and the tendency to utilise formal outpatient care. However, it is difficult to prove whether these increases were due to *ex post* moral hazard or the under-consumption observed before the implementation of UHC. In fact, healthcare consumption among the uninsured is consistently substantially lower than that of the CSMBS and SSS beneficiaries: the introduction of UHC only reduces the consumption gap between the already insured and the newly insured. Unless one is willing to assume significant levels of insurance-driven overconsumption in the existing CSMBS and SSS, the convergence between different schemes appears to support the pre-UHC under-consumption explanation. This is consistent with what Nyman (1999) calls the "access motive": given insurance, low-income people consume more healthcare because they can afford something that otherwise would have been too expensive to buy.

Altogether, our results show that UHC did not only reduce the financial burden on health expenditures at the family level, but also effectively enabled a broader access to healthcare, including people that would not have used it otherwise. Some caveats, however, need to be made. This study only assesses the short-run impacts of UHC, and the extent to which these may be projected onto the medium and long term remains unevaluated. This, nevertheless, would not be straightforward, as the further we move away from the date of the programme's

implementation, the more difficult it would become to interpret any DD result as a causal effect.

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Tables

Table 1: Descriptive Statistics

VARIABLES	Year 1996		Year 2001		Year 2003	
	Treated	Controls	Treated	Controls	Treated	Controls
Outcomes						
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
At least 1 annual check-up	N/A	N/A	0.078	0.363	0.474	0.728
At least 1 annual check-up (Female)	N/A	N/A	0.09	0.356	0.135	0.334
At least one hospitalisation last year	0.038	0.041	0.038	0.064	0.054	0.06
Out-of-Pocket Expenditure (THB for the most recent hospitalisation)	5489.8	9155.5	6286.5	1917.6	2918.7	1438.5
Frequency of Hospitalisations (times in the past 12 months)	N/A	N/A	3.894	3.818	1.272	1.216
Days in Hospital (days for the most recent hospitalisation)	7.335	6.324	6.381	6.181	6.116	5.628
Report being sick during the past month	0.123	0.102	0.122	0.108	0.144	0.156
Soughtformal outpatient care	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						
Age	38.09	36.72	39.28	37.76	39.53	39.18
Number of 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
Government Employee	0	0.34	0	0.431	0	0.379
Resident of Bangkok	0.087	0.176	0.098	0.096	0.085	0.101
Observations	20337	8697	22780	27964	5842	9180

Table 2: DD and DDD estimates

OUTCOME	DD 2001-1996	[95% Conf. Interval]	DD 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 Hospitalisation	-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 Hospitalisations		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 3:Heterogeneity Analysis for the DDD

OUTCOME	By Age		By Gender		By Municipality	
	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 Hospitalisation	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**

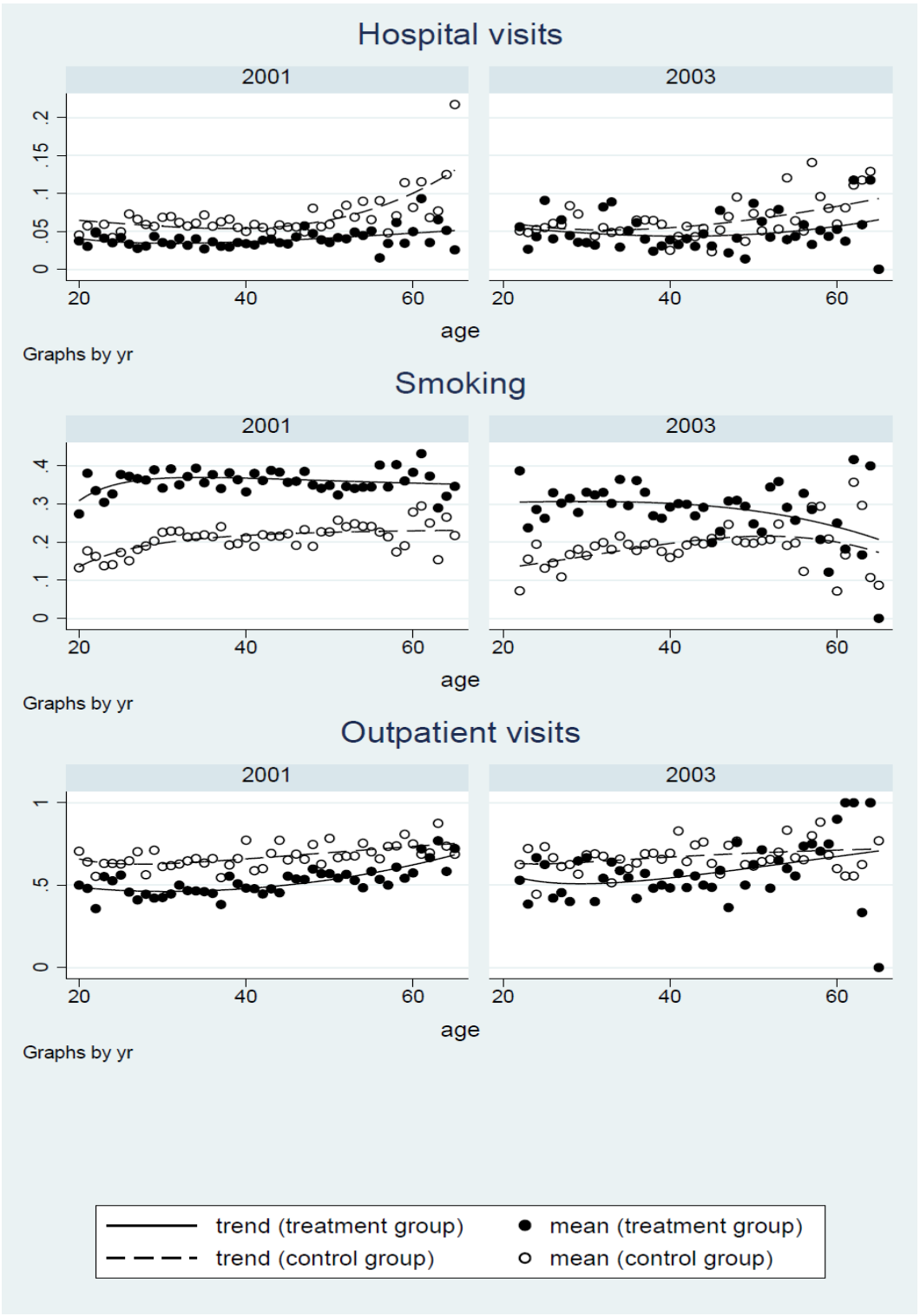
*p-value<0.1 **p-value<0.05

Table 4: DD and DDD Estimates with the treated group including everyone covered by UHC in 2003, regardless of their pre-UHC insurance status.

OUTCOME	DD 2001-1996	[95% CI]	DD 2003-2001	[95% CI]	DDD	[95% CI]
			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-Ups		N/A	0.068**	[0.040; 0.092]		N/A
Check-Ups (women)		N/A	0.098**	[0.059; 0.120]		N/A
			Inpatient			
One Hospitalisation	-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 Hospitalisations		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: Distribution of hospitalisations, outpatient visits, and smoking across age and by treatment vs. control group



Supplemental material

Appendix 1: The Double Robust Estimator

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 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.

Appendix 2: Balance between treated and controls

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