UNIVERSAL COVERAGE ON A BUDGET: IMPACTS ON HEALTH CARE UTILIZATION AND OUT-OF-POCKET EXPENDITURES IN THAILAND

SUPON LIMWATTANANON[a,b], SVEN NEELSEN[c], OWEN O'DONNELL[d,e,f,*], PHUSIT PRAKONGSAI[a], VIROJ TANGCHAROENSATHIEN[a], EDDY VAN DOORSLAER[c,d,e], VUTHIPHAN VONGMONGKOL[a]

a International Health Policy Program, Ministry of Public Health, Thailand
b Khon Kaen University, Thailand
c Institute of Health Policy and Management, Erasmus University Rotterdam, the Netherlands
d Erasmus School of Economics, Erasmus University Rotterdam
e Tinbergen Institute, the Netherlands
f Department of Balkan, Slavic and Oriental Studies, University of Macedonia, Greece

May 14, 2013

Abstract

We estimate the impact on health care utilization and out-of-pocket (OOP) expenditures of a major reform in Thailand that extended health insurance to one-quarter of the population to achieve universal coverage while keeping health spending below 4% of GDP. Identification is through comparison of changes in outcomes of groups to whom coverage was extended with those of public sector employees and their dependents whose coverage was not affected. The reform is estimated to have reduced the probability that a sick person goes without formal treatment by 3.2 percentage points (11%). It increased the probability of receiving public ambulatory care by 2.7 ppt (5%) and of admission to a public hospital by 1 ppt (18%). OOP expenditures were reduced by one-third on average, as was the probability of spending more than 10% of the household budget on health care, while spending at the very top of the OOP distribution was reduced by one-half representing substantial reductions in exposure to medical expenditure risk. Supply-side measures implemented with the coverage extension are likely to have helped realize these effects from an increased, but still very tight, budget.

JEL Classification: H42, H51, I18

Keywords: Health Insurance, Health Care, Medical Expenditures, Universal Coverage, Thailand

Acknowledgements: The research reported in this paper was financed by the EU FP7 project entitled *Health Equity and Financial Protection in Asia* (HEALTH-F2-2009-223166). Comments received at the following conferences and seminars are gratefully acknowledged: ISS (the Hague), CESifo (Munich), SSPH+ (Grindelwald), iHEA 2011 (Toronto), HEFPA (Hanoi), the 2nd Global Symposium on Health Systems Research (Beijing) and Erasmus University Rotterdam.

* Corresponding author: University of Macedonia, Egnatia 156, Thessaloniki 54006, Greece.
Email address: odonnell@ese.eur.nl

1. INTRODUCTION

Thailand achieved universal health insurance in 2001-02 by extending coverage to 18 million previously uninsured citizens representing almost one-quarter of the population. At the time, the country had a GDP per capita of $5036 (PPP) and was spending only $169 per capita on health care.[1] Aware of the difficulty of making good on the promise of universal coverage on such a tight budget, the coverage expansion was accompanied by supply-side measures intended to constrain costs and deliver cost-effective care. We examine whether this major health care reform was able to increase access to medical care and improve financial protection against medical expenditure risks for Thai citizens without jeopardising the health of the public finances.

There is increasing advocacy of universal health coverage being recognised as a key development goal. It has the backing of the United Nations General Assembly (2012), the World Health Assembly (2005; 2011), the World Health Organization (2010; 2013) and the World Bank (2013). Governments in low and middle income countries are likely to hear this rallying cry with some trepidation of the fiscal consequences of comprehensive social health insurance. Finkelstein (2007) estimates that the introduction of Medicare – public health insurance for the elderly in the US – increased total spending on hospitals by 37% within five years and, by extrapolation, claims that the expansion of health insurance coverage could account for one-half of the sixfold increase in real spending on medical care in the US between 1950 and 1990. From the estimated impact of the introduction of universal insurance on health care utilization in Japan, Kondo and Shigeoka (2013) warn countries contemplating a major expansion of health insurance to expect a "surge in health care expenditures." The large and growing structural deficits of both the US and Japan may give politicians in emerging economies pause for thought before following these two countries in legislating entitlements to public health insurance.

In this context, the experience of Thailand is of considerable interest. The 2001 reform aimed at universality with respect to both the breadth of population coverage and the depth of services covered. The Universal Coverage Scheme (UCS) granted entitlement to a near comprehensive benefit package of most medical treatments and prescribed medicines to all Thai citizens not covered by (formal sector) employment-related public insurance schemes. UCS beneficiaries were required to make a copayment of only 30 Baht (~$0.75) per patient contact, from which the poor, the elderly and children were exempted, and even this was abolished in 2006.

Legislating universal coverage is easier than making it effective. A large extension of tax-financed entitlements can be expected to release demand to which supply may respond resulting in an escalation of public spending on health that may undermine the financial sustainability of the policy. Alternatively, if the lid is kept too tightly on medical spending, such that supply cannot

[1] Amounts in current (2001) prices and international $ using PPP conversion factors from World Development Indicators (www.databank.worldbank.org accessed April 9, 2013).

respond to the inflated demand, then the public medical care offered may be grossly lacking in quality or accessible only after long delays. In that case, universal coverage will be a chimera. Supposed beneficiaries will still have to pay to access effective medical care, or else they will forgo care altogether. The impact on health care utilization and financial protection will be muted.

The Thai reform attempted to avoid both scenarios by expanding coverage while maintaining tight control of supply and constraining medical prices with the aim of squeezing the most out of the resources available. Three features of the reform design were seen as key to the effort to constrain costs (Health Insurance System Research Office, 2012). First, a tax-financed single-payer with a fixed budget had both the incentive to contain costs and the monopsony power to constrain payments to health care providers and pharmaceutical suppliers. Second, payment of mainly public providers by capitation for outpatient care and prospectively at a fixed price per condition (i.e. Diagnostic Related Groups) under a global budget for inpatient care gave providers little incentive to inflate demand or deliver treatments of questionable medical effectiveness. Third, a gatekeeper, holding the capitation budget, at the district hospital level was intended to ration access to expensive tertiary level treatment and shift the balance of care toward the primary level.

Despite these measures, spending on medical care did rise. Total health expenditure per capita approximately doubled in real terms between 2001 and 2010 (International Health Policy Program, 2011). But GDP per capita also grew at an average rate of 5.2% per annum over this period. Health spending relative to GDP increased by little more than half a percentage point and was still under 4% in 2010 (*ibid*). This is a tight budget from which to deliver universal coverage. Has it been achieved or have the constraints on public spending left households with limited access to quality health care and little financial protection from the costs of seeking effective treatment?

We identify the impact of the UCS reform on health care utilization and household out-of-pocket medical expenditures through a difference-in-differences comparison of population groups covered by the UCS with a control group of public sector workers and their dependents that had coverage prior to the reform. We find that the reform reduced the probability that a sick individual goes without ambulatory care by 11% and increased the probability of an inpatient admission to a public hospital by 18%. There is no evidence of crowding out of private sector care.

The Thai universal coverage reform is often trumpeted as a success (World Health Organization, 2010; Health Insurance System Research Office, 2012) but this claim is based simply on upward trends in health care utilization and downward trends in household OOP medical spending (Limwattananon et al., 2007; Somkotra T, 2008; Damrongplasit, Melnick, 2009; Panpiemras et al., 2011; Health Insurance System Research Office, 2012). Gruber et al. (2012) present the only other evaluation of the reform's causal impact on health care utilization. They concentrate on the effect on inpatient admissions and attempt to estimate differential effects between those exempt and not exempt from the 30-Baht copay. These groups are presumed to correspond respectively to those covered by a less well-financed programme for the poor, children and elderly – the Medical Welfare Scheme (MWS) – prior to the UCS and others who were previously uninsured or held voluntary insurance. The authors find a greater positive impact on the exempt group, which is estimated to be even larger for women of childbearing age and infants. Prompted by this finding, they use province level data to estimate the impact of the UCS on infant mortality for those previously covered by the MWS and find a very large effect. On the basis of these results, Gruber et al. argue that the most important component of the Thai reform was not the extension of coverage to the previously uninsured but what they consider to be a massive increase in funding for those enrolled in the MWS that made their previously nominal coverage effective.

This interpretation has important implications for international health policy beyond understanding what happened in Thailand. It suggests that health care access and health outcomes can be dramatically improved by putting more resources into health systems that offer subsidized care to poor and vulnerable populations without changing entitlements and extending population coverage. This is an interesting hypothesis that deserves attention but it may be premature to conclude in its favour. We explain in section 3 that with the data available is it difficult to distinguish those previously covered by the MWS from those previously uninsured. Further, taking account of cross-subsidization of the MWS in the pre-reform period and of the allocation of salary costs, the per capita rise in funding for those previously insured is likely to have been much less than the four-fold increase claimed by Gruber et al..

We estimate the effect of the UCS on the utilization of ambulatory care, as well as inpatient admissions, distinguishing between public and private care, and between different levels of care within the public sector. The latter reveals that the gatekeeper system appears to have been effective in redirecting treatment from higher cost provincial hospitals to district hospitals. Ambulatory care has shifted from sub-district health centres to district hospitals, which is consistent with patients seeking more effective, now affordable, treatment options.

This is the first paper to present estimates of the impact of the Thai reform on OOP medical spending. We find that the UCS left the probability of a household making any OOP payments unchanged but reduced the mean amount spent by those incurring payments by 31%. The effect on the 95^{th} percentile of (positive) expenditures is even higher at 51%. Prior to the reform, 5.8 percent of the target group spent at least one-tenth of their household budgets on health care. The UCS reduced these so-called catastrophic health payments by two percentage points. These effects suggest that the extension of insurance coverage brought substantial gains in welfare from reduced exposure to medical expenditure risk, consistent with the finding of Finkelstein and McKnight (2008) for US Medicare.

The evidence presented is of direct relevance to the policy discourse arising from the current push for universal health coverage being led by the World Health Organization (World Health Organization, 2010; The Lancet, 2012). It is more relevant than evidence on the effects of health insurance obtained from marginal changes in coverage, even when this is obtained experimentally (Newhouse, 1993; Finkelstein et al., 2012), because supply-side responses can make the impact of a large scale expansion of coverage disproportionate to that of a small scale increase (Finkelstein, 2007; Kondo, Shigeoka, 2013). Evidence from a middle-income country that implemented universal coverage a decade ago is more relevant to emerging economies contemplating this step than is evidence from the experience of the US and Japan fifty years ago (Finkelstein, 2007; Finkelstein, McKnight, 2008; Kondo, Shigeoka, 2013), and even from Taiwan (Chen et al., 2007; Chou et al., 2011; Chang, 2012; Keng, Sheu, 2013) that implemented universal coverage in 1995 at a much higher level of income than that of Thailand in 2002. Our evidence is distinguished from that obtained from major expansions of health insurance in the middle income settings of Colombia and Mexico not only by geography and the lower income of Thailand, but also by the nature of the reform in Colombia, which offered a means-tested subsidy for the purchase of private insurance covering care at a restricted network of providers (Miller et al., 2012), and the nature of the evidence in Mexico, which comes from an experiment that offers only a ten-month window within which to identify the effect on treated individuals offered the opportunity to voluntarily enroll relative to controls (King et al., 2009; Barofsky, 2011). Evidence from China informs of effects of insurance when, unlike in Thailand, the provider payment system gives little incentive to deliver cost-effective care (Wagstaff, Lindelow, 2008; Wagstaff et al., 2009).

With the exceptions of evaluations of universal coverage reforms in Massachusetts (Kolstad, Kowalski, 2012) and Japan (Kondo, Shigeoka, 2013), most studies are restricted to identifying the impact of an extension of public insurance to certain demographic or income groups. Since the Thai UCS covers all citizens not in the formal employment sector, we can estimate differential effects by age, poverty status and urban/rural location.

The rest of the paper is structured as follows. Section 2 describes the organization and financing of health care in Thailand before and after the UCS reform. Section 3 describes the empirical strategy and data used to identify the effects on utilization and presents the results obtained. Section 4 presents analysis of effects on household OOP medical spending. Section 5 concludes.

2. THE THAI HEALTHCARE SYSTEM

2.1 Before universal coverage

Over four decades prior to the introduction of the UCS in 2001, the supply of health care increased substantially. By 1977 there was a provincial hospital in all 76 provinces and rapid building of district hospitals had begun. In the ten years prior to the UCS reform around 150 such hospitals were opened (Ministry of Public Health, Various years). The number of doctors increased steadily from around 17 per 100,000 population in 1977 to around 30 in 2001 (ibid). Nursing staff increased even more dramatically from 40 to 170 nurses per 100,000 population (ibid).

Various public health insurance schemes had succeeded in extending coverage to a substantial share of the population even before the introduction of the UCS. Ten years before the reform, two-thirds of Thai citizens had no formal health insurance. Just before the reform, this fraction had been reduced to less than 30% (Figure 1). The single largest scheme in 2001 was the Medical Welfare Scheme (MWS) that entitled the elderly (60+), children (<12, and secondary school students), the poor, and the disabled to comprehensive free care in public facilities.[2] This tax-financed scheme covered 32% of the population in 2001. In 1998, the average annual budget per enrollee was just 273 Baht (~$6.82), raising concerns of severe underfunding (Donaldson et al., 1999; Pannarunothai, 2002).[3] To fill the financing gap, provincial health authorities and hospital staff redirected substantial funds from general budgets and other public schemes. As a result of this cross-subsidization, it is estimated that MWS expenditure per enrollee was around 70% greater than the official budget (Donaldson et al., 1999). The funding increase to provide care for those moving from MWS to UCS coverage was substantial but much less than the fourfold rise apparent from the comparison of official per capita subsidies cited by Gruber et al (2012).

The second largest program prior to the UCS was the Voluntary Health Card Scheme (VHCS) covering 21% of the population in 2001. For 500 Baht ($12.50) per year, households could purchase a health card that entitled up to 5 household members to free care at public facilities. The private contribution was matched by a 1,000 Baht tax-financed government subsidy. With over four enrollees per card on average, the VHCS budget was often insufficient to provide adequate care and, similar to the MWS, there was substantial cross-subsidisation (Donaldson et al., 1999).

The Civil Servant Medical Benefit Scheme (CSMBS) is tax-financed and provides free care at public facilities for active and retired government employees, their spouses, parents, and up to three of their children below the age of 21. It covered 8.5% of the population in 2001. Operating under an uncapped fee-for-service system with a generous benefit package, CSMBS spending per

[2] Army veterans, monks and village leaders and their dependents also had MWS entitlement.
[3] Salary costs were not covered from this budget.

capita was almost 2,500 Baht ($62.50) per enrollee in 1998 (Donaldson et al., 1999). Finally, the Social Security Scheme (SSS) covers salaried private sector employees but not their dependents. In April 2002, just around the time of the completion of the UCS-reform, the restriction that the SSS covered only enterprises with ten or more employees was removed.

The 18 million Thais that remained without health insurance coverage in 2001 could access healthcare in two ways. First, they could purchase care out-of-pocket at private or public facilities. Second, the so-called Type B Exemption enabled public health facility staff to exercise discretion in granting free or subsidized care to apparently poor individuals with no proof of MWS membership. Type B Exemption expenditure amounted to about 2% of total public health expenditure in 1997 (Donaldson et al., 1999).

Without overlooking considerable achievements in the provision of health care and the extension of population coverage, there were structural weaknesses in the Thai healthcare system at the turn of the millennium. Almost 30% of the population had no insurance coverage and underfunding of the MWS and the VHCS left many poor and near-poor with a lack of effective coverage. Targeting of the MWS on the poor, elderly and children was far from perfect (Kongsawat et al., 2000) and the voluntary nature of the VHCS left it vulnerable to adverse selection (Donaldson et al., 1999). The system was also characterized by large and persistent regional disparities in both resources and health outcomes (Wibulpolprasert, 2002).

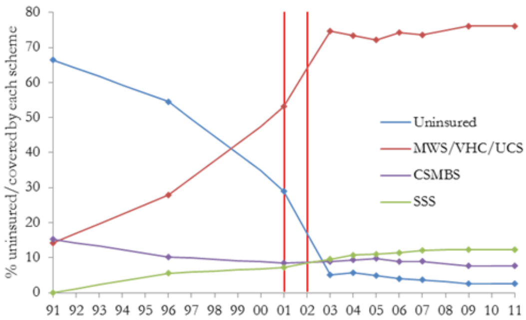

Fig. 1: Percentage of population uninsured and covered by each public health insurance scheme

Source: Authors' estimates using Health and Welfare Surveys
Notes: Sample weights applied. MWS – Medical Welfare Scheme, VCHS – Voluntary Health Card Scheme, UCS – Universal Coverage Scheme, SSS – Social Security Scheme, CSMBS – Civil Servants Medical Benefit Scheme.

2.2 After universal coverage

On coming to power in January 2001, the populist Thai Rak Thai party immediately embarked on an ambitious health reform. Starting in April 2001 with the introduction of the UCS in 6 pilot provinces, the scheme was expanded to cover 15 provinces in June 2001 and the remaining provinces by October of that year. The rollout was completed within a year with the inclusion of the capital Bangkok in April 2002. The UCS covers all Thai citizens not covered by the employment-based schemes – the CSMBS and SSS. The percentage of the population covered by some form of health insurance jumped from 71% in 2001 to 95% in 2003, and by 2011 coverage had risen to over 98% (Figure 1).

Beneficiaries of the UCS are entitled to inpatient treatment, ambulatory care and medicines (prescribed) at facilities within a local provider network. The benefit package is near comprehensive. It includes high-cost treatments like open-heart surgery and chemotherapy.[4] Medicines can be prescribed from the National List of Essential Medicines, which contains a large proportion of generics. Fully tax-financed, the UCS initially levied a fixed charge of 30 Baht (~$0.75) per service contact on a little more than half of its enrollees. Those meeting the criteria of the former MWS were exempt from this charge. This charge was abolished in 2006 and reinstated in 2012, but rather symbolically with payment being voluntary.

On top of the coverage extension, the reform made substantial changes to the financing and organization of the public healthcare system. These were largely motivated by an awareness of the difficulty of achieving a balance between making universal coverage effective on a modest budget and preventing medical spending from rocketing in response to a huge expansion of coverage (Health Insurance System Research Office, 2012). The main ingredients of the supply side measures were: i) a closed-end capitation-based budget; ii) gatekeeper control of access to specialist care; iii) prospective payment of hospitals for inpatient treatment; and, iv) from 2006, a single public purchaser of care and medicines for UCS beneficiaries.

Consistent with the aims of increasing the role of primary care and containing costs, the backbone of the reformed system is the Contract Unit for Primary Care (CUP), which typically consists of a district hospital and affiliated health centers. Most CUPs consist of a network of public facilities, except in some parts of large cities where demand is estimated to exceed the capacity of public providers and adequate private provision is available.[5] The CUP issues a UCS gold card to residents of its catchment area that gives entitlement to care within the network. Access to other CUPs and higher level providers, including provincial hospitals, requires referral from the CUP of residence (Hughes, Leethongdee, 2007; Hughes et al., 2010).

Outpatient and preventive care are paid for from a capitation budget allocated to CUPs. Hospitals are paid for inpatient care prospectively at rates specific to Diagnostic Related Groups

[4] Anti-retroviral treatment and renal replacement therapy have been covered since 2003 and 2008 respectively. Organ transplantation is one of the few essential treatments still excluded.

[5] In 2010, 55% of the UCS members in Bangkok were registered with private hospitals and clinics. But nationally only 5.7% of UCS members are registered with private networks (Health Insurance System Research Office, 2012) p.53.

(DRG) for referrals made by the CUPs but subject to the global capitation budget.[6] Originally, salary costs were to be paid from the distributed UCS capitation budget with the intention of correcting geographic inequalities in access by providing financial incentives for the redeployment of health personnel to understaffed rural regions. This put severe financial strain on many urban providers whose entire capitation payment could easily be consumed by salary costs at existing staffing levels (Hughes et al., 2010). An intense lobbying effort by the medical profession led to a repeal of the new financing arrangement in 2002/3 and a return to salary budgets being determined by current staffing levels and administered centrally and separately from the service budgets.[7]

From the outset, the intention was to achieve a purchaser-provider split but the central purchasing authority – the National Health Security Office (NHSO) – did not begin to operate until 2006, which is after our estimation period. This agency negotiates the capitation rate with the Ministry of Finance annually. Constrained by its budget, the NHSO has an incentive to flex its purchasing power to obtain lower prices from service providers and, particularly, suppliers of medicines.

Besides changing the financial architecture of the Thai healthcare system, the reform was accompanied by an increase in public health spending. The UCS began with a capitation of 1,202 Baht ($30.50) in 2001-03.[8] In real terms, this had been increased by 6.3% by 2005, the end of our study period, and by 71% by 2011 (Health Insurance System Research Office, 2012). Total spending on the UCS in 2003 was 35% greater in real terms than total public expenditure on the MWS and VHCS schemes it replaced had been in 2001 (International Health Policy Program, 2011). The increase in expenditure per insured person was much more modest since around 23% more of the population was covered by the UCS. There was a sharp rise in real total (and public) health expenditure per capita at the time the UCS was introduced between 2001 and 2002 (Figure 2a). Health expenditures continued to rise, but at a less rapid rate, in the post-reform period, such that real total expenditure per capita doubled between 2001 and 2010, and public health expenditure per capita increased by almost 170%. With rapid economic growth over this period, spending on medical care would most probably have increased even in the absence of the coverage extension. As a percentage of GDP, total health expenditure increased by only 0.6 of a percentage point from 2001 to 2010 and public health expenditure increased by one percentage

[6] In the first year of operation, CPUs could be given the budget for inpatient care, as well that for outpatient and preventive care. From 2002-03, PHOs could not pass the inpatient care budget down to CUPs (Hughes et al., 2010).

[7] The majority of urban facilities successfully requested emergency funding to cover salary costs in the first year of the reform prior to the repeal. Afterwards, the UCS budget was top-sliced to take out a contribution toward salary costs. Inconsistent treatment of salary costs is a further reason why it is likely that Gruber et al (2012) overstate the funding increase coincident with the move from the MWS to the UCS.

[8] Salary costs had to be covered from this budget, initially directly and subsequently through its top-slicing.

point (Figure 2b). Extension of coverage to one-quarter of the population has not resulted in an explosion of expenditure on medical care, although the increase in public spending is undoubtedly contributing to budgetary pressures (Lindelow et al., 2012).

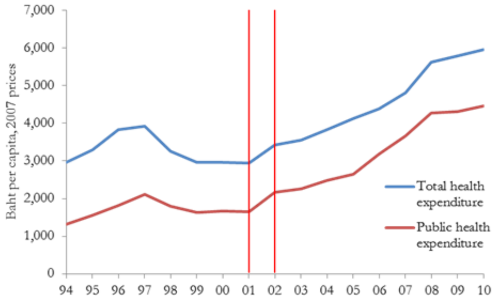

Fig. 2a: Total and public health expenditure per capita, Baht (2007 prices)

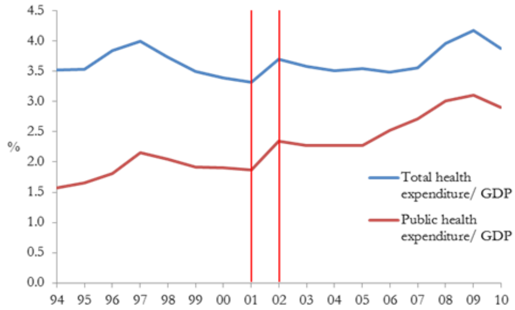

Fig. 2b: Total and public health expenditure as a percentage of GDP

Source: Thai National Health Accounts 1994-2010 (International Health Policy Program, 2011)

3. UCS Impact on Healthcare Utilization

The impact of the health insurance extension on the level and pattern of health care utilization is contingent on a number of factors. The UCS reduced the price of public care for those previously uninsured and it may have increased effective access to quality public care for those previously insured through the MWS and the VHCS that were less well-financed. These groups are expected to be less likely to forgo care when needed, but also to switch from care paid for privately to that provided publicly. The impact on total utilization will depend on the degree of crowd-out of private care. The impact on utilization will also depend on the supply side's ability and incentives to respond to the increased demand. We noted in the previous section that capacity had been developed over four decades prior to universal coverage, but if the UCS budget, and consequently the DRG payment rates, were set too low, then providers would have little incentive to respond to any increased demand, which may not even materialize if the perception of the quality of care that could be delivered was low (Panpiemras et al., 2011).

Since the flat rate fee of 30 Baht represented a larger decrease in the price of more expensive treatments for those who had to pay prior to the UCS, demand would be expected to shift to higher levels of treatment. The CUP gatekeeper was designed to resist any movement toward provincial hospitals, and with the DRG prospective payment gave district hospitals, which effectively control the CUPs, the financial incentive to provide inpatient treatment rather than make referrals to the provincial hospital.

3.1 Treatment and Control Groups

Our strategy for identifying the impact of the UCS on health care utilization is to compare before and after differences between population groups whose coverage was changed as a result of the reform and beneficiaries of the CSMBS whose coverage did not change. We use data from a nationally representative cross-sectional household survey – the Health and Welfare Survey (HWS) – conducted by the National Statistical Office (NSO).

Treatment Group

The HWS records the health insurance coverage of each household member. Our treatment group consists of all individuals not covered by one of the two employment-related schemes (CSMBS and SSS) and so entitled to coverage through the UCS. In the pre-UCS period, it comprises those covered by the MWS and the VHCS, as well as the uninsured. In the post-UCS period, it includes those covered by the UCS and those who remain uninsured because they are not yet registered with a CUP. Since the latter are part of the target population, they belong to the treatment group under the intention-to-treat principle.

Gruber et al. (2012) use the same dataset and essentially the same identification strategy to estimate the impact of the UCS on inpatient admissions, but they focus on differential effects by pre-reform insurance status. They construct one treatment group from individuals identified in the survey as MWS enrollees before the reform plus those reporting to be in the UCS and exempt from the 30 Baht copay post-reform (UCSE). Since the criteria for qualification for the MWS and the UCS copay exemption were the same, this treatment group is presumed to consist of individuals whose nominal coverage did not change but whose effective coverage may have increased because the UCS is better financed than was the MWS. A second treatment group is constructed by combining VHCS enrollees and the uninsured pre-reform with those reporting to be in the UCS and not exempt from the copay post-reform (UCSP). The separation of these two treatment groups may be problematic. Inefficiency of the MWS in targeting the poor was one motivation for the reform. If there was any difference in the target efficiency of the two schemes, then survey participants identified as MWS enrollees in the pre-reform period will not correspond to those reporting UCSE coverage in the post-reform period. The 2003 HWS data reveal, for example, that 15% of the formerly uninsured became UCSE beneficiaries. Differences in the composition of the specific treatment groups over time potentially invalidate the difference-in-differences identification strategy. Relying on reported UCS exemption status can also be problematic. While all children automatically became UCSE beneficiaries through the reform, 92% of the children of UCSP parents are reported as UCSP in the HWS.

Because differential pre-reform coverage is related to demographics and income levels, we present separate estimates of the impact of the UCS for children (<16), working-age adults (16-

59), and the elderly (60+), and for the poor and non-poor[9]. Since geographic reallocation of resources was an original aim of the reform, we also distinguish between rural and urban dwellers.

Control group

Individuals belonging to the CSMBS, either as a government employee, a retiree from public sector employment or a dependent of one of those two populations, form our control group. Their insurance status did not change with the introduction of the UCS. Between 2001 and 2005, our period of analysis, there were no changes to the CSMBS and the proportion of the population covered by it varied by just over one percentage point. The expansion of the SSS around the time of the UCS reform to cover all formal private sector employees and the resulting increase in the proportion of the population covered by it implies a change in the composition of beneficiaries. For this reason, we do not include individuals covered by SSS in the control group. Those reporting coverage by private insurance, employer benefits and other insurance types (2.1% in 2001) are also dropped from the estimation sample.

3.2 Data

Our analysis uses one cross-section of the HWS before the introduction of the UCS (2001) and three cross-sections afterwards (2003, 2004 and 2005). The estimation sample size in 2001 (203,106) is about three times larger than that in each of the post-UCS waves, giving us an approximately equal number of observations pre- and post-reform (Table 1). The 2001 wave was conducted between April and June, before the nationwide implementation of UCS in October.[10] Implementation began in April 2001 in six pilot provinces. With utilization of ambulatory and inpatient treatment recorded for the previous two weeks and one year respectively, recall periods for some pilot province observations span both pre and post reform. UCS was, however, not effectively implemented in the pilot provinces at the time of the survey. We thus treat all 2001 data as pre-reform. [11]

Analysis of health care utilization is at the individual level. The HWS records whether each household member had used each of various types of ambulatory care and whether s/he has had an inpatient admission in the last year. The information on ambulatory care is routed through a question about whether an illness that is considered not to have required hospitalization has been experienced. The recall period for this question is two weeks in 2001 and one month in 2003-

[9] Broadly consistent with the official poverty rate during our observation period, we define the poor as those in the bottom tertile of per capita household income distribution.
[10] In all other waves used, sampling was conducted in April only. We deal with any seasonal effects by entering two indicators in the regressions for observations sampled in May and in June.
[11] Observations from the pilot provinces make up 7.2% of the 2001 sample of which only 11.5% identified themselves as UCS enrollees. Redefining these observations as being within the treatment period for the estimation of effects on ambulatory utilization, for which the recall period was two weeks, had no impact on the estimates. Our results are also robust to exclusion of observations from the pilot provinces.

2005. We rely on year effects to take account of this inconsistency. Since this change coincides with the reform, identification of the effect of the latter relies on the assumption that the change in recall period affects the responses of treatment and control observations equally. There are no obvious reasons to doubt this. We further reduce the importance of the change in the sickness recall period by modeling the probability of using ambulatory care given reported illness. The number of different providers that a respondent reporting an illness could identify in the survey increased from two to three in 2003. Again, we must rely on year effects and the assumption that any effect of this change on reporting is the same for the treatment and control groups.

For the subsample reporting illness, we test for UCS effects on three categories of ambulatory care. The first category indicates if an individual has forgone care, self-medicated (retail pharmacies, drug vendors, herbal medicines) or visited a traditional healer but has received no formal treatment from a public facility or private clinic. We refer to this category as *no formal ambulatory care* hereafter. The second category indicates if an individual received care from formal private providers but did not visit a public facility and the third corresponds to receipt of any care from a public provider. Because the UCS reform reduced the price and/or increased the funding of care mainly provided at public facilities for the treatment group (see footnote 5), we expect it to result in a switch from the first and second categories to the third.

Separately for the treatment and control groups, Table 2 shows pre- and post-reform sample proportions reporting sickness and, conditional on reporting sickness, the proportions in each of the three categories of ambulatory care. The proportion reporting sickness rises by about 30% for both the treatment and control groups, presumably attributable to the aforementioned longer recall period in the post UCS surveys. The proportion with no formal ambulatory care is higher in the treatment group in both periods but drops significantly, by almost three percentage points, only for this group after the reform. For both groups, utilization of private care rises and utilization of public care falls. The decrease in the probability to use public care is substantially larger for the control group (4.8 percentage points) than it is for the treatment group (1.6) and almost entirely eliminates the pre-reform difference between the two groups.[12]

For the subsample visiting a public facility, we investigate whether the pattern of utilization across levels of care changes differently between treatment and control groups. We distinguish between visits to i) a health center but no other public facility, ii) a district hospital but not to a provincial or university hospital, and iii) a provincial or university hospital. Because the reform reduced the relative price of higher level treatments but made district hospitals the point of entry

[12] Gruber et al (2012) conduct an uncontrolled difference-in-differences analysis of the probability of using ambulatory care. They do not condition on the reporting of illness and so the changes in utilization rates reflect the change in sickness recall period. Neither do they distinguish between types of ambulatory care. Compared with the CSMBS conrol group, they find that the rise in utilization of any ambulatory care is significantly greater for their treatment group presumed to be previously covered by the MWS and is significantly lower for those presumed previously to be uninsured or covered by the VHCS.

to even higher levels of care, we expect both upstream (from health centers) and downstream (from provincial/university hospitals) shifts of ambulatory care towards district hospitals.

Table 1: Probability of reporting illness and ambulatory care utilization

	Pre-UCS (2001)	Post-UCS (2003-05)	Difference	*Sample size (by group)*
Proportion reporting illness[a]				
Treatment	.1640	.2057	.0418***	*326,213*
(Uninsured/MWS/VHCS/UCS)	(.0009)	(.0010)	(.0014)	
Control (CSMBS)	.1516	.2037	.0520***	*59,171*
	(.0021)	(.0024)	(.0031)	
Sample size (by period)	*203,106*	*182,278*		*385,384*
Proportion of ill obtaining				
no formal ambulatory care[b]				
Treatment	.2966	.2692	-.0274***	*59,887*
	(.0027)	(.0025)	(.0037)	
Control	.2286	.2258	-.0029	*10,483*
	(.0062)	(.0054)	(.0083)	
ambulatory care at public facility				
Treatment	.5456	.5294	-.0162***	
	(.0030)	(.0028)	(.0041)	
Control	.5679	.5200	-.0479***	
	(.0073)	(.0065)	(.0098)	
ambulatory care at a private facility only				
Treatment	.1534	.1861	.0327***	
	(.0021)	(.0022)	(.0031)	
Control	.1956	.2243	.0287***	
	(.0059)	(.0054)	(.0080)	
Sample size (by period)	*32,931*	*37,439*		*70,370*
Proportion of those obtaining public care treated at				
health center				
Treatment	.3242	.3776	.0534***	*32,163*
	(.0038)	(.0038)	(.0053)	
Control	.0629	.1202	.0574***	*5,670*
	(.0048)	(.0059)	(.0076)	
district hospital				
Treatment	.2865	.4162	.1297***	
	(.0036)	(.0038)	(.0053)	
Control	.2260	.3214	.0954***	
	(.0082)	(.0084)	(.0118)	
provincial/university hospital				
Treatment	.3893	.2062	-.1831***	
	(.0039)	(.0031)	(.0050)	
Control	.7111	.5583	-.1528***	
	(.0089)	(.0090)	(.0126)	
Sample size (by period)	*18,069*	*19,764*		*37,833*

Source: Authors' calculations from Health and Welfare Survey.

Notes: [a] Reporting illness considered not to require hospitalization. Two-week recall period in 2001 HWS, one-month recall period in 2003-05 HWS. [b] Includes not seeking any care, self-medication, retail pharmacies drug vendors, herbal medicines, and traditional healers.

Categories are mutually exclusive within each panel. Visit to a private facility excludes those also visiting any public facility. Health center excludes those also visiting a higher level public facility. District hospital excludes those also visiting a provincial/university level hospital. Robust standard errors reported in parentheses. *** Indicates that the difference over time is significantly different from zero at 1%.

Patterns of utilization differ strikingly by insurance coverage (Table 2, bottom panel). Individuals in the treatment group are much more likely to use health centers and district hospitals and so are much less likely to visit provincial hospitals than those covered by the CSMBS. Post-reform, for both groups, the proportions using health centers and district hospitals increase. The increase in the probability to visit a district hospital and the decrease in the probability to visit a provincial hospital are both greater in magnitude for the treatment group, which is consistent with the UCS shifting the pattern of care toward the lower level hospitals.

In the 2001 and 2003 surveys, respondents were asked whether they had any inpatient admission in the last year and, if so, to which type of hospital. From 2004, the respondent could report admissions to up to three types of hospitals.[13] For the 2004-05 cross-sections, we use all admissions reported and rely on the year dummy variables to absorb the effect of this change in the question. We distinguish between no admission, admission to a private but not a public hospital, and any inpatient stay in a public hospital. If the UCS reform succeeded in making inpatient care affordable to individuals who would otherwise have forgone treatment, then admissions to public facilities will have risen. In addition, if individuals who could otherwise afford inpatient treatment in private hospitals are induced by the lower relative price and better funding to switch to public hospitals, then there will be a crowding-out effect on private care.

Table 2: Probability of inpatient admission

	Pre-UCS (2001)	Post-UCS (2003-05)	Difference	*Sample size (by group)*
Proportion admitted to				
public hospital				
Treatment	.0560 (.0006)	.0607 (.0006)	.0047*** (.0008)	*326,213*
Control	.0801 (.0016)	.0805 (.0016)	.0005 (.0022)	*59,171*
private hospital				
Treatment	.0064 (.0002)	.0070 (.0002)	.0006** (.0003)	
Control	.0089 (.0005)	.0093 (.0006)	.0004 (.0008)	
Sample size (by period)	*203,106*	*182,278*		*385,384*
Proportion of those admitted to public hospital treated at a district hospital				
Treatment	.3284 (.0048)	.4947 (.0052)	.1664*** (.0070)	*18,990*
Control	.1862 (.0079)	.2915 (.0094)	.1053*** (.0123)	*4,752*
Sample size (by period)	*12,102*	*11,640*		*23,742*

Source: Authors' calculations from Health and Welfare Survey.
Notes: Robust standard errors in parentheses. *** (**) indicates that the difference over time is significantly different from zero at 1% (5%).

There is a significant increase in the probability of admission to a public hospital in the treatment group, but not the control group, after the UCS reform (Table 2). There is a significant increase in the probability of admission to a private hospital also only in the treatment group, which is not

[13] In the 2004-05 data, 18% of those with at least one admission report additional hospital stays.

consistent with a crowding-out effect.[14] Furthermore, the second panel of the table shows that a much higher proportion of individuals in the treatment group use district hospitals rather than provincial or university hospitals and that, consistent with the gatekeeper role of district hospitals in the UCS, there is an increase in the probability to be admitted to this type of hospital after the reform that is greater for the treatment group than for the control group.

3.3 Estimation

For the sample reporting a sickness not considered to have required hospitalization, we estimate a multinomial logit (MNL) model of the three mutually exclusive categories of ambulatory care: no formal ambulatory care $(y_{it1}=1)$, care at a private clinic or hospital but not at a public facility $(y_{it2}=1)$ and care at a public facility $(y_{it3}=1)$, where *i* and *t* are indicators of individuals and time respectively. Probabilities of these three outcomes are defined as

$$P\left(y_{itj}=1\right)=\Lambda_j\left(\mathbf{X}_{it}\boldsymbol{\theta}\ \right)=\frac{\exp\left(\mathbf{X}_{it}\boldsymbol{\theta}_j\right)}{\sum_{k=1}^{3}\exp\left(\mathbf{X}_{it}\boldsymbol{\theta}_k\right)}. \quad (1)$$

We set the no formal care option as the reference ($\boldsymbol{\theta}_1=0$) and define the linear index as follows,

$$\boldsymbol{X}_{it}\boldsymbol{\theta}_j=\alpha_j D_{it}+\beta_j D_{it}\times UC_t+\boldsymbol{Z}_{it}\boldsymbol{\gamma}_j+\delta_{tj},\ \ j=2,3 \quad (2)$$

where D_{it} is a dummy variable indicating membership of the target treatment group (MWS/VHCS /uninsured/UCS), UC_t is an indicator of the post-treatment period (>2001), δ_{tj} is a year effect[15] and $\mathbf{Z}_{it}$ is a vector of control variables included to increase precision and to allow for any differences between the treatment and control groups in trends in observable determinants.[16] The effect of the UCS on the probability of each of the three possible outcomes for the target treatment group at the time of treatment is given by (Puhani, 2012)

$$\Delta P\left(y_{itj}=1\right)=\Lambda_j\left(\boldsymbol{\alpha}+\boldsymbol{\beta}+\boldsymbol{Z}_{it}\boldsymbol{\gamma}+\boldsymbol{\delta}_{t\in UC}\right)-\Lambda_j\left(\boldsymbol{\alpha}+\boldsymbol{Z}_{it}\boldsymbol{\gamma}+\boldsymbol{\delta}_{t\in UC}\right),\quad j=1,2,3 \quad (3)$$

where $t\in UC$ is some point in the post-reform period. We compute these effects by averaging over observations in the UCS target group within the post-UCS period (2003-05).

---

[14] Identification of the crowding-out effect is subject to the caveat that a minority of UCS enrolees (5.3% in 2010) have access to a private provider network that contracts with the NHSO.

[15] Month effects are also included to take account for the sampling undertaken in May and June (in addition to April) in 2001 only.

[16] For the full sample, and separately by treatment status, Appendix Table A1 reports means of the control variables. The observed employment and occupation differences between treatment and control groups derive from the fact that the UCS covers those outside the formal employment sector. The control group contains non-public sector workers because CSMBS coverage extends to public sector retirees and dependents of public sector workers. The 2.3% of individuals who report to be public employees not covered by the CSMBS could either be temporary public workers or the result of misreporting. Besides the occupation differences, individuals in the treatment group are on average younger, less likely to live in urban areas and less educated than individuals in the control group.

We estimate analogous models for the type of public facility used by those with any ambulatory visit to a public facility (health center only (reference) vs. district hospital (but not provincial hospital) vs. provincial hospital) and for inpatient care (no admission (reference) vs. admission to a private (but not a public) hospital vs. admission to a public hospital). For those admitted to a public hospital, we estimate a binary logit model for admission to a district hospital as opposed to a provincial or university hospital.

Adoption of the multinomial logit model implies the potentially restrictive Independence of Irrelevant Alternatives (IIA) assumption (McFadden, 1984). For example, the relative probability of seeking ambulatory care at a private clinic as opposed to forgoing formal treatment may not be independent of the availability and quality of the third treatment option from public facilities. While the mixed logit (McFadden, Train, 2000) would be an attractive more flexible alternative, identification of this model is likely to prove difficult in the absence of covariates that are specific to the care options. The nested logit (McFadden, 1984) also succeeds in relaxing the IIA assumption but requires imposition of a hierarchy that may not be consistent with actual choices. For example, one could assume that there is a first stage decision to seek any formal sector ambulatory care followed by a second stage decision to seek care from either a public or private sector facility. But for some, perhaps poor and rural dwellers, the only feasible choice may be between no treatment and care at an inexpensive local public health center. We prefer to avoid the risk of misspecification arising from the imposition of incorrect hierarchies.[17]

3.4 Results

Ambulatory Care

In Table 3 we present the estimated effects of the UCS on the probability of: i) reporting an illness not requiring hospitalization (top panel); ii) each of the three defined ambulatory treatment responses to such an illness (middle); and, iii) for the subsample utilizing public care, visiting each of the three types of public provider (bottom). There is a significant effect on the probability of reporting illness only for the poor. The absence of any significant impact on reported illness for the full sample and all other sub-samples suggests that no selection bias is induced by estimating the effect of the UCS on ambulatory care utilization among those reporting illness. The increased likelihood of the poor to report illness could be because sickness is more likely to be acknowledged when there is an opportunity to seek treatment. But it could also be that the poor respond differentially to the extension of the recall period. In any case, we concentrate on the treatment response conditional on reported illness.

[17] We have estimated the impact of the UCS on all utilization outcomes using the linear probability model (LPM) of binary choices. Qualitatively, and for the most part quantitatively, LPM estimates are consistent with those presented from the multinomial and binary logits.

Table 3: Average effects of the Universal Coverage Scheme on the probability of reporting an illness and receiving ambulatory care

	All	*Children*	*Adult*	*Elderly*	*Rural*	*Urban*	*Poor*	*Non-Poor*
Report illness	.0023	-.0107	-.0056	.0165	.0091	-.0051	.0211**	-.0033
	(.0037)	(.0081)	(.0047)	(.0104)	(.0080)	(.0041)	(.0101)	(.0038)
Sample size	152,842							
Given illness, probability of receiving:								
no formal care	-.0321***	-.0308	.0007	-.0720***	-.0458**	-.0254**	-.0667***	-.0226**
	(.0097)	(.0194)	(.0150)	(.0169)	(.0191)	(.0119)	(.0220)	(.0111)
public care	.0270**	.0069	-.0063	.0805***	.0258	.0250**	.0662***	.0105
	(.0106)	(.0250)	(.0154)	(.0183)	(.0216)	(.0123)	(.0239)	(.0119)
private care	.0052	.0238	.0056	-.0086	.0199	.0005	.0004	.0120
	(.0079)	(.0202)	(.0114)	(.0126)	(.0151)	(.0105)	(.0145)	(.0100)
Sample size	29,411							
Given receive public care, probability of treatment at:								
health center	-.0692***	-.0376	-.1102***	-.0314	-.0712*	-.0431*	-.0899*	-.0762***
	(.0220)	(.0493)	(.0342)	(.0364)	(.0393)	(.0229)	(.0478)	(.0245)
district hospital	.1048***	.0400	.1588***	.0751**	.0781**	.0932***	.1124***	.1117***
	(.0192)	(.0431)	(.0304)	(.0312)	(.0353)	(.0211)	(.0432)	(.0214)
provincial/university hospital	-.0355***	-.0023	-.0485***	-.0438**	-.0070	-.0501***	-.0225	-.0354***
	(.0100)	(.0208)	(.0163)	(.0171)	(.0152)	(.0146)	(.0161)	(.0137)
Sample size	16,664							

Notes: Estimates are average treatment effects on the treated in the post-treatment period derived from binary (top panel) and multinomial logit (middle and bottom panels) model estimates. Models include the control variables listed in Table A1, plus indicators of province, year, month and treatment/control group. Robust standard errors reported in parenthesis. ***, **, * indicate significance at 1%, 5% and 10% respectively. Sample sizes give the number of treated observations in the post-treatment period over which the treatment effects are averaged. The estimation samples are larger including control observations and treated observations in 2001 and correspond to the sum of the pre- and post-UCS observations given in Table 1. Sub-sample sizes are given in Appendix Table A2.

The introduction of the UCS is estimated to have reduced the probability of going without formal ambulatory care when sick by 3.2 percentage points (11%) on average, an effect primarily driven by an increase in the probability of seeking care at a public facility. There is no evidence of the UCS resulting in the crowding out of private ambulatory care in the full or any sub-samples, although it should be kept in mind that a minority of UCS beneficiaries are registered with a private provider network. The shift from no formal ambulatory care to treatment at public facilities is confined to the elderly, for whom the UCS increased the probability of receiving public care by 8 percentage points (13%). The elderly were, in principle, exempt from user fees both before (through the MWS) and after the introduction of the UCS. Apparently the MWS did not provide effective coverage to the elderly component of its target population. MWS targeting of the elderly was indeed very poor with about one-third of this group not enrolled (Donaldson et al., 1999). However, MWS targeting of children was also deficient and the UCS did extend coverage to many working-age adults. The degree of coverage increase does not seem sufficient to explain why the impact on ambulatory care is restricted to the elderly. An additional explanation is that the greater morbidity among the elderly implies a larger pool of unmet need for any given degree of underinsurance. It may also be the case that the elderly attached greater stigma to the MWS, widely perceived as the poor man's scheme, and so reacted more strongly to the UCS entitlement granted to all citizens.

In rural areas, it is estimated that the UCS reduced the probability of going without formal care by 4.6 percentage points (17%). The effect is driven by almost equally sized non-significant increases in the probabilities of seeking care at private and public providers. The smaller reduction in the probability of forgoing formal treatment in urban areas derives solely from increased utilization of public facilities. The UCS is estimated to reduce the probability that the poor sick go without formal treatment by 6.7 percentage points (25%). The effect on the non-poor is one-third of this magnitude.

Conditional on receiving ambulatory care in the public sector, the location of treatment shifts from health centres and provincial hospitals to district hospitals. The (conditional) probability of being treated at a district hospital is raised significantly in the full sample (11 ppt, 38%) and for all subsamples, except for children. With the exceptions of the elderly and urban subsamples, the shift in treatment is mainly from health centres to district hospitals. This is consistent with the larger price reduction of district hospital care. It may also reflect the greater budgetary power granted to district hospitals under the reformed system and the consequent shift in resources toward them that further encourages patients to seek care at that level rather than at insufficiently equipped health centers.[18] In all but the rural and poor subsamples, which overlap considerably, there is statistically significant evidence that the increase in utilization of district hospitals also partly stems from reduced use of provincial hospitals. This is consistent with what one would expect from the UCS referral system that made district hospitals the point of entry to higher level care.

Inpatient Care

In Table 4 we present average effects of the UCS on the probability of inpatient admissions in the post-reform target population. A clear pattern emerges consistent with increased affordability inducing greater inpatient treatment in public hospitals without crowding out care in the private sector. On average, the UCS is estimated to raise the probability of receiving inpatient treatment by 1 percentage point (18%). Across age groups, the effect varies from 2 percentage points for the elderly to 0.6 points for children but in relative terms all effects are in the range of 15% (working-age adults) to 18% (children). The effect is just short of significance at 10% for children. The larger relative impact on public inpatient care compared to that on ambulatory care is presumably due to a greater pre-reform financial barrier to obtaining the former.

The positive impact of the UCS on the probability of receiving public inpatient care is confined to urban settings, where it is 1.1 percentage points (21%). The lack of any significant impact in

[18] Hughes et al. (2010) anecdotally report complaints by health centre staff that district hospitals abused their new budgetary powers to cut funding for largely preventive care at health centers.

rural areas may reflect geographic barriers that continue to block access to care at distant hospitals despite the reduced price of treatment, and possibly also the persistence of supply constraints as a result of the reform's failed efforts to redistribute human resources and funds towards rural regions. The UCS succeeded in improving access to public inpatient care for both the poor and the non-poor. The slightly lower impact on the poor reflects the overlap between the poor and rural populations.[19]

Table 4: Average effects of the Universal Coverage Scheme on the probability of inpatient admission

	All	*Children*	*Adult*	*Elderly*	*Rural*	*Urban*	*Poor*	*Non-Poor*
public hospital	.0099***	.0059	.0081***	.0201***	.0041	.0114***	.0089*	.0100***
	(.0020)	(.0037)	(.0027)	(.0062)	(.0041)	(.0022)	(.0052)	(.0020)
private hospital	.0005	.0010	.0003	.0008	-.0009	.0020*	.0005	.0008
	(.0007)	(.0014)	(.0009)	(.0021)	(.0008)	(.0011)	(.0011)	(.0009)
Sample Size	152,809							
If public, probability treated at district hospital	.0511***	-.0668	.0489**	.0776***	.0603*	.0292	.0864**	-.0353*
	(.0176)	(.0482)	(.0250)	(.0295)	(.0346)	(.0204)	(.0365)	(.0202)
Sample size	9,289							

Notes: Estimates are average treatment effects on the treated in the post-treatment period derived from multinomial (top panel) logit and logit (bottom panel) model estimates. Notes on control variables, standard errors, significance and sample sizes as for Table 3.

In order to gauge the magnitude of the increase in inpatient treatment relative to the price reduction that brings it about, we estimate the impact of the UCS on the mean payment for inpatient care in a public hospital using the same years of the HWS and the same empirical strategy adopted to identify the impacts on utilization.[20] The estimator is the modified two-part model described in the next section. We estimate that the UCS reduced the mean payment for inpatient treatment across all those in the target population admitted to a public hospital by 14%. Setting the estimated 18% increase in the probability of receiving public inpatient care against this price reduction implies a price elasticity of -1.29. The price decrease is modest since the average includes individuals who made no payment both before and after the reform. These individuals were always exempt and if their utilization did not change, then the price elasticity is overestimated. Restricting attention to strictly positive payments, the average effect on the treated is 37%, which implies a price elasticity of -0.48 if one assumes that the rise in utilization comes from individuals who expect to make a reduced positive payment after the reform. Even this estimate indicates a substantial degree of price sensitivity.[21]

[19] While not reported in the table, we find a particularly large increase for the urban poor that amounts to 1.8 percentage points.

[20] This analysis is conducted only for inpatient treatment because the HWS did not collect data on payments for ambulatory care prior to 2003. Details of the analysis of payments and the results are available from the authors on request.

[21] Both estimates are substantially larger in magnitude than the price elasticity of inpatient care of around -0.15 estimated from the famous, but now rather dated, RAND Health Insurance Experiment in the US

As with ambulatory care, the UCS induced a shift in the location of public inpatient care from provincial/university to district hospitals (Table 4). The 5 percentage point (16%) increase in the probability of being treated at a district hospital (conditional on being admitted to a public hospital) is most likely a result of the new referral system. This relocation of treatment is not observed for children and for the non-poor there is a marginally significant reduction in the probability of being treated at a district hospital. The reason for the latter effect is not immediately obvious. It could be that wealthier individuals are more successful in negotiating referral to higher level hospitals possibly being induced to do so by the increased utilization of district hospitals.

4. UCS IMPACT ON OUT-OF-POCKET EXPENDITURES

4.1 Data and identification strategy

To estimate the impact of the reform on household OOP medical expenditures we again adopt a difference-in-differences strategy, this time using data from the nationally representative Socioeconomic Survey (SES) conducted by the NSO. We use the 2000 SES cross-section for the pre-UCS period, and the 2004 cross-section for the post-UCS period. Sample sizes are just under 25,000 households in 2000 and 35,000 households in 2004.

Medical expenditures are available at the household level. The 2000 SES did not record health insurance status and so we use employment sector to proxy insurance coverage. Treatment group households are defined as those with no public sector worker and not every member is a private sector salaried employee. These households should not be fully covered by either the CSMBS or the SSS, although there could be partial coverage from these schemes. This occurs, for example, if the household includes a retired civil servant living with his/her adult offspring, if a child of a public sector worker does not live in the same household as this parent, or if a formal private sector employee lives with dependents. With these exceptions, individuals living in the treatment group households belong to the population entitled to UCS coverage. They include the self-employed, informal workers, unemployed, retired private sector and informal workers, elderly and child dependents of these groups and of private salaried employees. Our comparison group includes households consisting of a public sector employee(s) and his or her dependents, which should be covered by the CSMBS. Households consisting only of formal private sector

(Manning et al., 1987). The discrepancy could be due to the difference in contexts and the fact that the RAND experiment examined response to much more limited variation in prices. Our elasticity estimates are broadly consistent with evidence from low and middle income countries. Lavy and Quigley (1993) estimate a price elasticity of inpatient care of -1.82 in Ghana in 1987 and the elasticity of both outpatient and inpatient care at public hospitals is often estimated to be around unitary or higher in magnitude (Gertler et al., 1987; Sauerborn et al., 1994; Sahn et al., 2003; Asfaw et al., 2004; Mocan et al., 2004). A recent study of Chile estimates elasticities of ambulatory care of around -2 but of close to zero for specific surgeries (Duarte, 2012).

employees should be covered by the SSS and are excluded because of the expansion of this scheme at the time of the UCS reform.

Because the 2004 SES wave does identify insurance status, we can check the validity of approximating health insurance enrollment by employment sector. The method works quite well. Over three-quarters of individuals in households allocated to the treatment group are indeed UCS enrollees and the same proportion of the comparison group is covered by the CSMBS (Table 5). A further 5% of the treatment group individuals are uninsured and correctly fall into the treatment group under the intention-to-treat principle. SSS and CSMBS beneficiaries account for 7% and 10% respectively of individuals in the treatment group households. These are the exceptions described in the previous paragraph. Moreover, 9% of individuals in the comparison group households are enrolled in the SSS and 14% are UCS members. The latter are co-residents of public sector workers not covered by the CSMBS: ≥ 4^{th} child, adult offspring, in-laws, siblings and non-family household members. The inclusion of these individuals means that we do not have a control group of households completely unaffected by the reform. For this reason, we refer to a 'comparison group' whose exposure to the reform is much less than that of the treatment group. If the UCS does provide financial protection from medical expenses, this should be apparent from comparisons of changes in household expenditures between the two groups. Identification of the effect of differential exposure to the reform is inevitable given treatment – insurance status – is defined at the individual level, while the outcome – medical expenditures – is measured at the household level.

Table 5: Percentage of individuals in treatment and comparison households covered by each insurance scheme, 2004

	Treatment[a]	**Comparison[b]**
UCS	78.3	14.3
CSMBS	9.6	75.5
SSS	6.7	8.8
Uninsured	5.4	1.4
Sample size (households)	*30,785*	*1,631*

Source: Authors' calculations from Thailand Socioeconomic Survey 2004
Notes: [a] All households except those including a public sector employee or including only formal private sector employees. [b] Households including only public employees and their dependents.

The inclusion of CSMBS/SSS enrollees in the treatment group and of UCS enrollees in the comparison group does not necessarily imply that the estimate of the impact of the UCS on *household* medical expenditures will be biased. The impact at the household level will indeed vary with the number of individuals in the household with UCS coverage. Eighty-four percent of individuals in the treatment group households are in the UCS target population, while the same proportion of individuals in the comparison households are not entitled to coverage through the UCS. We are identifying the effect of the reform on households whose main health insurance entitlement is provided by the UCS relative to households mainly covered by employment-based insurance that does not change. This is an interesting treatment effect, which can be anticipated to be smaller than the effect on a household fully covered by the UCS relative to one without any coverage from the UCS. But the latter effect is relevant to a much smaller number of households than the one we estimate.

The SES identifies out-of-pocket payments for ambulatory care and medicines in the last month and for inpatient care in the last year. No distinction is made between payments for public and private care. We compute household per capita expenditures. Pre- and post-reform means for the treatment and comparison groups are given in Table 6.

Table 6: Descriptive statistics – Household OOP medical expenditures per capita
(per month, Baht constant 2004 prices)

	Pre-reform (2000)		**Post-reform (2004)**		**Difference**		*Sample size*
Expenditures on medicines							
Proportion making any payment							
Treatment	.5541	(.0033)	.4659	(.0028)	-.0881***	(.0044)	*52,964*
Comparison	.3407	(.0141)	.3120	(.0114)	-.0286	(.0181)	*2,758*
Mean expenditure if >0							
Treatment	31.97	(0.838)	31.84	(0.745)	-.1305	(1.12)	*26,633*
Comparison	68.48	(5.322)	94.30	(10.29)	25.82**	(11.58)	*893*
Expenditures on ambulatory care							
Proportion making any payment							
Treatment	.2936	(.0031)	.3291	(.0026)	.0355***	(.0041)	
Comparison	.2032	(.0120)	.2177	(.0102)	.0145	(.0158)	
Mean expenditure if >0							
Treatment	203.7	(7.485)	172.2	(5.467)	-31.45***	(9.269)	*16,643*
Comparison	446.5	(52.20)	523.3	(43.16)	76.82	(67.73)	*584*
Expenditures on inpatient care							
Proportion making any payment							
Treatment	.0854	(.0019)	.1192	(.0018)	.0339***	(.0026)	
Comparison	.0905	(.0085)	.0901	(.0071)	-.0004	(.0111)	
Mean expenditure if >0							
Treatment	274.3	(20.75)	227.0	(19.60)	-47.25*	(28.54)	*5,564*
Comparison	443.5	(71.41)	357.2	(82.89)	-86.29	(109.4)	*249*
Total medical expenditures							
Proportion making any payment							
Treatment[a]	.7135	(.0030)	.6736	(.0027)	-.0399***	(.0040)	
Comparison[b]	.5102	(.0149)	.4800	(.0124)	-.0301	(.0194)	
Mean expenditure if >0							
UCS	141.5	(4.28)	146.4	(4.78)	4.91	(6.41)	*36,561*
CSMBS	302.2	(27.19)	365.6	(27.82)	63.40	(38.90)	*1,358*
Medical expenditure / total household expenditure							
Treatment	.0257	(.0004)	.0201	(.0003)	-.0056***	(.0004)	
Comparison	.0130	(.0010)	.0138	(.0009)	.0007	(.0010)	
Proportion with (Medical expenditure / total household expenditure)>0.1							
Treatment	.0578	(.0016)	.0446	(.0012)	-.0131***	(.0020)	
Comparison	.0240	(.0046)	.0294	(.0042)	.0054	(.0062)	

Source: Authors' calculations from Thailand Socioeconomic Survey.
Notes: Treatment and comparison households defined as in notes to Table 5. Robust standard errors reported in parenthesis. Where sample sizes not given they are as in first two rows.

Prior to the reform, the treatment group was more likely to pay OOP for medicines in the last month. This probability declined for both groups but only significantly so and to a greater degree for the treatment group. Also consistent with an effect of the UCS on spending on medicines, the mean positive amount paid remained constant for the treatment group but increased by 37% in the comparison group. The probability of paying for ambulatory care in the last month was also higher for the treatment group at baseline and it increased only for this group. This is consistent with the impact of the UCS on ambulatory care utilization identified in the previous section and with the 30 Baht copay per service contact. Indicative of reduced exposure to larger expenditures on ambulatory care, the mean positive amount paid by the treatment group fell by 15%, while it increased (not significantly) by 17% for the comparison group. Before the UCS, the treatment and comparison groups faced approximately equal probabilities of incurring OOP expenditures on inpatient care over a year. This probability rose only for the treatment group, which again is likely to reflect the positive impact of the UCS on admissions combined with the 30 Baht copay. The mean amount paid for inpatient care fell significantly after the reform only for the treatment group, although in relative terms the fall in the sample means is roughly the same for both groups. We calculate total household OOP medical expenditure per capita by dividing payments for inpatient care reported for one year by 12 and adding this to payments for ambulatory care and medicines each reported for the last month, and dividing the result by household size. The probability of having any OOP medical spending was higher for the treatment group at baseline but declined significantly only for this group indicating that the reduced probability of paying for medicines dominates the increases in the probabilities of paying for ambulatory and inpatient care. There is no significant increase in the mean OOP payment for either group, although the rise in the sample mean is larger for the comparison group.

The treatment group spends less on health care than the comparison group in absolute terms but relative to household budgets, the burden of medical spending is higher for the treatment group, which is poorer. Prior to the UCS, the treatment households spent 2.6% of their budgets on medical care, on average, compared to only 1.3% for the comparison households. After the introduction of the UCS, the mean medical expenditure budget share fell by one-fifth among the treatment households and remained constant in the comparison group. Prior to the reform, 5.8% of the treatment households spent at least 10% of their budgets on health care – sometimes referred to as 'catastrophic health payments' (Wagstaff, van Doorslaer, 2003) – compared with only 2.4% of comparison households. Exposure to such high relative medical expenditures fell only in the treatment group.

4.2 Estimation

We estimate modified two-part models (Mullahy, 1998) of household OOP medical expenditures (in total and separately for medicines, ambulatory care, and inpatient care). The probability of making any payment is modeled by a logit with the linear index specified analogous to equation (2) and the partial effect of the reform being analogous to equation (3), which is averaged over the treatment group of households in the post-reform period. The impact on the expectation of OOP payments (m_{it}) over their positive range is estimated by a Generalized Linear Model (GLM) with a log link function,

$$E\left[m_{it} \mid m_{it} > 0\right] = \exp\left(\xi D_{it} + \eta D_{it} \times UC_t + \mathbf{Z}_{it}\zeta + \tau_t\right) \tag{4}$$

where, i now indicates a household, D_{it}, UC_t and $\mathbf{Z}_{it}$ are defined as above[22], τ_t is the year effect and the error of the model is assumed to follow a gamma distribution.[23] The average effect of the UCS on mean positive expenditures among those with such spending in the treated population is estimated by

$$\widehat{ATT} = \frac{1}{N_{D_+}} \sum_{i \in S_{D_+}} \exp\left(\hat{\xi} + \mathbf{Z}_{it}\hat{\zeta} + \hat{\tau}_t\right)\left[\exp(\hat{\eta}) - 1\right] \tag{5}$$

where S_{D^+} is the subset of households within the treatment group $(D_{it} = 1)$ with positive expenditures and N_{D^+} is the number of such households. We also estimate the average effect on positive expenditures relative to the counterfactual of no reform, which is simply the term in square brackets in (5).[24] The average effect of the UCS on mean expenditures (including zero) among the population exposed to the reform is estimated by combining estimates from the two parts of the model (Flores et al., 2011).

We take two approaches to test whether the UCS was successful in reducing exposure to particularly high medical expenses, as opposed to merely decreasing the mean amount spent. First, we use a logit model – the linear index again specified analogous to (2) – to estimate the impact on the probability that the household spends at least 10% of its budget on health care. Second, we use the quantile difference-in-differences approach (Athey, Imbens, 2006) to estimate effects on conditional (on covariates) quantiles of positive expenditures, concentrating on effects at higher quantiles. Each conditional quantile is specified as a linear function analogous to the term within parentheses on the right-hand-side of (4). Under the assumption that, in the absence of the reform, (conditional) quantile q of positive OOP payments of treated households would have changed as did quantile q of payments of comparison households, the effect on that quantile is identified by the parameter on the reform indicator $(D_{it} \times UC_t)$, which is estimated by the respective quantile regression (Koenker, Basset, 1978). To estimate the relative impact on a

[22] Descriptive statistics by treatment and control group of the control variables used in the regression analyses reported below are given in Table A3 of the Appendix.
[23] We have also estimated the effect on the log of positive expenditures using ordinary least squares. The results are consistent with those obtained from GLM with respect to direction and significance of effects and the magnitudes of the relative effects obtained are broadly similar.
[24] An additional advantage of the relative ATT is that it is a function only of the coefficient on the treatment indicator and so will be estimated with greater precision than the ATT, which depends on all covariates and parameters in the model.

conditional quantile, we use the regression estimates to predict expenditure for each household at that quantile and average over the treated households in the post-reform period assuming that the UCS had not been implemented. We then express the treatment effect on the quantile as a proportion of the mean counterfactual prediction.

4.3 Results

We present average effects of the UCS on medical expenditures in the post-reform treated households in Table 7. There is no significant impact on the probability of incurring any medical expenditure, which is the result of a significant reduction in the probability of spending on medicines being cancelled by increases in the probability of spending on inpatient and on ambulatory (not significant) care. That the UCS significantly increased the probability of incurring payments for inpatient care is consistent with our finding of a positive impact on inpatient admissions and the small UCS copayment. The offsetting reduction in the probability of spending on medicines is consistent with our finding that the reform reduced reliance on self-medication – included in the 'no formal treatment' category in section 3 – and with the inclusion of prescribed medicines in the UCS benefit package at no further charge after payment of the 30 Baht copay for a visit.

Table 7: Average effects of the Universal Coverage Scheme on household out-of-pocket medical expenditure per capita (OOP)

		Total	Medicines	Ambulatory care	Inpatient care
Pr(OOP>0)		-.0076	-.0519**	.0279	.0438***
		(.0171)	(.0206)	(.0206)	(.0141)
	Sample size	*30,774*			
OOP amount if >0					
	ATT	-51.84**	-11.55***	-63.27**	-78.90
		(20.48)	(3.936)	(29.69)	(53.59)
	Relative Effect	-.3146***	-.3106***	-.3250***	-.3278*
		(.1027)	(.0877)	(.1249)	(.1819)
	Sample size	*20,731*	*14,339*	*10,131*	*3,671*
OOP amount					
	ATT	-44.34	-8.980	-19.58	-.4043
	Relative Effect	-.3227	-.3847	-.2598	-.0017
Pr(OOP/Total hhold. exp.>0.1)		0.0199*			
		(0.0103)			

Notes: Estimates are average treatment effects on the treated in the post-treatment period derived from logit model (top & bottom panels) and GLM (gamma and log link) estimates. Relative effect is the ATT relative to the counterfactual prediction of OOP for the treatment group in the post-treatment period in the absence of treatment. Models include the control variables listed in Table A3, plus indicators of province, year and treatment/comparison group. Robust standard errors in parentheses. Standard errors not computed for effect OOP amount unconditional on OOP>0, which is a combination of the effects from the two parts of the model. Sample sizes give the number of treated observations in the post-treatment period over which the treatment effects are averaged. The estimation samples are larger including comparison observations and treated observations in 2000 – see Table 6. Sample size for bottom two panels is the same as the top panel.

Among households making some payment, mean medical expenditure per capita is reduced by 52 Baht ($1.30) per month, or 31%. The impacts on the amounts spent on ambulatory and inpatient (not significant) care are larger in absolute terms than that on spending on medicines but in relative terms the effects are similar across all categories of expenditure. These are large effects considering that they refer to expenditures on both public and private care. Among those incurring some medical expenses, the extension of coverage for public care has reduced the total amount spent on all types of care and medicines by almost one-third. The 52 Baht reduction in total per capita medical expenditure is equivalent to 1.3% of the treatment group's total household per capita monthly expenditure in the post-reform period.

Taking into account that around one-third of households incur no medical expenses and that the reform has no significant impact on this probability, the mean household monthly medical expenditure per capita is estimated to be reduced by 44 Baht, or 32%.[25] For ambulatory care, there is no significant impact on the payment probability and the overall mean payment is reduced by 20 Baht per person per month, or 26%. For inpatient care, the increased probability to make any payment entirely offsets the reduction in the mean positive amount paid. In relative terms, the largest overall effect is on payments for medicines, with the reduced probability of spending reinforcing the negative impact on the amount spent to produce a 38% reduction in the mean expenditure across all treatment households.

The probability of spending at least 10% of the household budget on health care is reduced by 2 percentage points (Table 7, bottom row), which is a very large effect relative to a 5.7% prevalence of so-called 'catastrophic payments' among pre-reform treatment group households.[26] This effect may be generated not only through the UCS reducing the numerator of the medical spending budget share but also by increasing the denominator. The latter could occur, for example, because of a reduction in precautionary saving in response to increased formal health insurance coverage. While this indirect effect cannot be ruled out, the other estimates in Table 7 indicate a direct effect on medical expenditures.

The UCS has a significant negative effect on all conditional quantiles estimated (Table 8). In absolute terms, the magnitude of the effect increases monotonically moving from the 10th quantile to the 95th quantile. This is to be expected given the rise in the level of expenditure moving up the distribution. The relative effect increases from a 14% reduction in the 10th quantile to a 51% reduction in the 95th quantile. The reform had its greatest impact at the top of

[25] We have not computed standard errors for the combined effects across the two parts of the models since they would reflect sampling variability of the estimators of all parameters across both parts of the model and would also be a function of all covariate values. It is more informative to conduct inference for each component of the model separately.

[26] The probability of spending at least 15% of the household budget on health care is reduced by 1.95 ppt (significant at 5%), which is a reduction of approximately two-thirds relative to the pre-reform probability. OLS estimates of the effects are very similar to the logit ones presented.

the distribution of medical expenditures and thus has provided financial protection against the risk of particularly burdensome expenses. This indicates a compression of the distribution of OOP payments and reduced exposure to medical expenditure risk.

Table 8: Effects of the Universal Coverage Scheme on conditional quantiles of positive household out-of-pocket medical expenditures per capita (OOP)

	Conditional quantile OOP>0 distribution						
	0.1	0.5	0.6	0.7	0.8	0.9	0.95
Effect (Baht)	-1.160*	-15.75***	-30.17***	-50.26***	-89.85***	-217.2***	-536.2***
	(.6322)	(2.738)	(3.934)	(5.514)	(9.165)	(18.55)	(37.02)
Relative effect	-.1360	-.2621	-.3162	-.3385	-.3646	-.4140	-.5078
Sample size	*39,993*						

Notes: Standard errors in parentheses (not computed for relative effects). Relative effect is the absolute effect as a proportion of the prediction of the conditional quantile averaged over the treatment group in the post-treatment period under the counterfactual of no treatment. Quantile regressions include the control variables listed in Table A3, plus indicators of province, year and treatment/comparison group.

5. CONCLUSION

We have estimated the impact on health care utilization and household out-of-pocket medical expenditures of a major health insurance reform in Thailand that extended coverage to one-quarter of the population and secured universal coverage at levels of national income and aggregate spending on health below those of countries that have achieved that landmark hitherto. The reform reduced the likelihood that someone goes without formal treatment when sick by 11% and increased inpatient admissions by 18%. These effects are largest for the elderly population. The increase in ambulatory care utilization is greatest for the poor and rural populations, while there is a significant impact on inpatient care only in the urban population. The absence of an effect on inpatient treatment in rural areas may be attributable to continued barriers to access. Although physical obstacles are not large with most of the population within 50 km of a district hospital, beds are limited and capacity may easily be reached. Also pertinent is the failure of the reform to realize its ambition of redistributing medical manpower, particularly specialists and surgeons, to relatively understaffed hospitals in less densely populated areas. While these explanations are plausible, the result could also reflect a statistical limitation due to the greater difficulty of finding public sector worker controls in rural areas when estimating the impact on inpatient admission, which is a relatively infrequent event. There is no evidence of crowding out of private formal care. The reform made care more accessible to individuals previously financially deterred from utilization but removal of the public care price tag appears not to have induced users of private facilities to switch to the public sector, which may indicate continued quality and convenience gaps between public and private providers.[27]

[27] This finding is subject to the caveat that the Universal Coverage Scheme does cover a minority of enrollees for care at private facilities.

In addition to raising access to health care, the reform reduced households' exposure to medical expenditure risks. Mean household medical expenditures were reduced by one-third, as was the probability of spending more than 10% of the household budget on health care, while spending at the very top of the distribution of medical expenditures (i.e. the 95th conditional quantile) was reduced by one-half. The welfare gain from this reduction in risk exposure is potentially large relative to the modest cost (Finkelstein, McKnight, 2008). In addition, there may be health gains from the increased use of medical care, particularly the steep rise in inpatient treatment of the elderly. Gruber et al (2012) identify very large reductions in infant mortality arising from the reform and Wagstaff and Manachotphong (2012a) find that it reduced labour force nonparticipation due to illness or disability.[28] Identification of impacts on more general health outcomes would better be achieved with a longer follow-up than the three years post implementation we have used to estimate effects on the more immediate outcomes of medical care use and expenditures.

The experience of other countries shows that a major expansion of health insurance cover does not inevitably raise medical care utilization and reduce household OOP spending on health. US Medicare did achieve this (Finkelstein, 2007; Finkelstein, McKnight, 2008), as does Medicaid – health insurance for the poor – at least in one state (Finkelstein et al., 2012), but in Japan utilization was increased without any reduction in OOP spending (Kondo, Shigeoka, 2013). There is also evidence from China of increased utilization of medical care without any fall in OOP payments (Wagstaff et al., 2009), and even of greater exposure to very high medical expenses resulting from increased insurance cover, which seems to be attributable to the fee-for-service payment of providers (Wagstaff, Lindelow, 2008). The opposite appears to have occurred in Mexico – a reduction in OOP health expenditures but no increase in utilization (King et al., 2009; Barofsky, 2011). In Colombia, exposure to medical expenditure risk was reduced and preventive care increased, but there was no significant impact on the utilization of curative care (Miller et al., 2012). Set against this evidence, the example of Thailand suggests that the extent to which increased health insurance coverage both raises utilization and reduces OOP spending is contingent on the way in which the provision of medical care is financed and organized.

The evidence we present on the shift in the location of public care from provincial to district hospitals is consistent with an effect of the gatekeeper system instituted by the reform. Much remains to be learned about exactly how this and other supply-side measures – a single purchasing agency, closed-end capitation budgets and prospective payment of hospitals for inpatient care – have been successful in realizing the promise of universal coverage with modest

[28] Against the positive effects of the reform through reduced risk exposure and health, a full welfare analysis would take account of potential distortionary impacts on the labour market through reduced incentives for formal sector employment to obtain health insurance coverage. Wagstaff and Manachotphong (2012b) find only weak evidence of a reduction in formal sector employment for men and a positive impact on employment overall.

spending on health care that has increased but remains below 4% of GDP. The Thai experience confirms that effective universal coverage on a budget is feasible, but other middle income countries considering embarking on this major reform will be keen to learn exactly how it can be achieved.

APPENDIX

Table A1: Means of control variables by treatment status from Health and Welfare Surveys, 2001-2005

	Treatment group (Uninsured/MWS/ VHCS/UCS)	*Control group (CSMBS)*
Demographics[a]		
Male	.473	.462
Age (years)	32.2	39.7
Urban	.541	.802
Education[b]		
None	.056	.021
Primary	.587	.256
Secondary	.319	.366
Post-secondary	.038	.357
Employment[c]		
Inactive	.236	.297
Self-employed/family enterprise	.361	.120
Employer	.141	.025
Formal private	.233	.031
Public	.030	.527
Occupation[c]		
None	.319	.327
Production/equipment	.079	.039
Agriculture/fishery	.264	.165
Clerk/sales/services	.248	.177
Professional/technical	.090	.293
Income[d]		
Real household per capita income (2005-Baht)	4,093	11,194
% poor (in bottom income tertile)	30.1	10.8
Sample size	*326,213*	*59,171*

Notes: Sample weights applied.
[a] Models are estimated using dummies to indicate age-sex categories.
[b] For a child under 15 years old, the maximum education level per his/her household was used.
[c] For a child under 15 years old, the highest employment/occupation category of any household member was assigned with the following rankings imposed: a) Employment sector: public > private > employer > self-employed / family enterprise > unemployed; and b) Occupation: professional / technical > clerk / sales / services > agriculture / fishery > production / equipment > no occupation.
[d] Household money income per equivalent adult (OECD scale). Models include dummies to indicate income deciles.

Table A2: Sizes of sub-samples over which treatment effects in Table 3 & 4 are calculated

	All	*Children*	*Adult*	*Elderly*	*Rural*	*Urban*	*Poor*	*Non-Poor*
Reporting illness	152,842	44,717	88,409	19,716	73,197	79,645	46,617	106,225
Ambulatory care	29,411	7,908	14,474	7,029	15,067	14,344	10,996	18,415
Type of public care	16,664	4,589	7,514	4,561	9.634	7,030	7,391	9,273
Inpatient admission	152,809	44,712	88,387	19,710	73,181	79,628	46,611	106,198
IP district hospital	9,289	1,788	5,214	2,287	4,726	4,563	3,489	5,800

Notes: Sample sizes give the number of treated observations in the post-treatment period over which the treatment effects are averaged. The estimation samples are larger including control observations and treated observations in 2001.

Table A3: Means of control variables from Socioeconomic Surveys 2000 and 2004

	Treatment households[a]	Comparison households[b]
Urban	.603	.853
Household size[c]	2.328	1.477
Assets index[d]		
bottom quintile	.142	.058
2nd bottom quintile	.164	.094
middle quintile	.186	.132
2nd top quintile	.228	.231
top quintile	.280	.485
Proportion of adult household members with		
No education	.069	.003
Primary education	.566	.125
Secondary education	.277	.363
Post-secondary education	.088	.509
Head of household		
Male	.699	.699
Age	50.6	42.3
No education	.073	.001
Primary education	.649	.099
Secondary education	.200	.359
Post-secondary education	.078	.541
Sample size	*52,964*	*2,758*

Notes: Sample weights applied.

[a] All households except those including a public sector employee or including only formal private sector employees.

[b] Households including only public employees and their dependents.

[c] Models also include the proportion of the household in 20 age-sex groups.

[d] Assets index given by first principle component from analysis of the following items: house construction material, toilet, bed, gas and electric stoves, electric and cooking pots, water boiler, microwave oven, refrigerator, iron, electric fan, radio, television, video machine, washing machine, air conditioner, computer, line and mobile telephones, fluorescence lamp and light bulb, bicycle, motorcycle, car, truck and boat. Both treatment and comparison groups appear to have more than 20% in the top quintile due to the application of sample weights.

References

Asfaw, A., von Braun, J., Klasen, S. 2004. How big is the crowding-out effect of user fees in the rural areas of Ethiopia? Implications for equity and resource mobilization. World Development 32, 2065–81.

Athey, S., Imbens, G.W. 2006. Identification and Inference in Nonlinear Difference-in-Differences Models. Econometrica 74 (2), 431-497.

Barofsky, Jeremy. 2011. Estimating the impact of health insurance in developing nations: Evidence from Mexico's Seguro Popular. Harvard School of Public Health, Cambridge, MA.

Chang, S. 2012. The effect of Taiwan's National Health Insurance on mortality of the elderly: revisited. Health Economics 21 (11), 1257-1270.

Chen, L., Yip, W., Chang, M., Lin, H., Lee, S., Chiu, Y., Lin, Y. 2007. The effects of Taiwan's National Health Insurance on access and health status of the elderly. Health Economics 16 (3), 223-242.

Chou, S. -Y, M. Grossman and J. -T Liu. 2011. The Impact of National Health Insurance on Birth Outcomes: A Natural Experiment in Taiwan. NBER Working Paper No. 16811. Cambridge, MA.

Damrongplasit, K., Melnick, G.A. 2009. Early Results From Thailand's 30 Baht Health Reform: Something To Smile About. Health Affairs 28, w457-w466.

Donaldson, D., S. Pannarunothai and V. Tangcharoensathien. 1999. Health financing in Thailand. Thailand: Health Financing and Management Study Project, ADB#2997 THA. Health Systems Research Institute, Nonthaburi, Thailand.

Duarte, F. 2012. 2012. Price elasticity of expenditure across health care services. Journal of Health Economics 31 (6), 824-41.

Finkelstein, A. 2007. The aggregate effects of health insurance: Evidence from the introduction of Medicare. Quarterly Journal of Economics CXXII (1), 1-37.

Finkelstein, A., McKnight, R. 2008. What did Medicare do? The impact of Medicare on mortality of out of pocket medical spending. Journal of Public Economics 92, 1644-1668.

Finkelstein, A., Taubman, S., Wright, B., Bernstein, M., Gruber, J., Newhouse, J.P., Allen, H., Baicker, K., Oregon Health Study Group. 2012. The Oregon Health Insurance Experiment: Evidence from the first year. The Quarterly Journal of Economics 127 (3), 1057-1106.

Flores, G., P. Ir, C. R. Men, O. O'Donnell and E. van Doorslaer. 2011. Protection of Patients through Compensation of Providers: The Impact of Health Equity Funds in Cambodia. Tinbergen Discussion Paper 11-169/3. Tinbergen Institute, Amsterdam.

Gertler, P., Locay, L., Sanderson, W. 1987. Are user fees regressive? The welfare implications of health care financing proposals in Peru. Journal of Econometrics 36 (1-2), 67-88.

Gruber, Jonathan, Nathaniel Hendren and Robert M. Townsend. 2012. The great equalizer: Health care access and infant mortality in Thailand. Harvard University, Cambridge, MA. http://scholar.harvard.edu/files/hendren/files/the_great_equalizer_vfinal.pdf.

Health Insurance System Research Office. 2012. Thailand's Universal Coverage Scheme: Achievements and Challenges. An independent assessment of the first 10 years (2001-2010). Health Insurance System Research Office, Nonthaburi, Thailand.

Hughes, D., Leethongdee, S. 2007. Universal Coverage In The Land Of Smiles: Lessons From Thailand's 30 Baht Health Reforms. Health Affairs 26, 999-1008.

Hughes, D., Leethongdee, S., Osiri, S. 2010. Using economic levers to change behaviour: The case of Thailand's universal coverage health care reforms. Social Science and Medicine 70, 447-454.

International Health Policy Program. 2011. National Health Accounts, Thailand 1994-2010. IHPP, Ministry of Public Health, Nonthaburi, Thailand.

Keng, S., Sheu, S. 2013. The effect of National Health Insurance on mortality and the SES-health gradient: Evidence from the elderly in Taiwan. Health Economics 22 (1), 52-72.

King, G., Gakidou, E., Imai, K., Lakin, J., Moore, R.T., Nall, C., Ravishankar, N., Vargas, M., Téllez-Rojo, M.M., Hernández Ávila, J.E., Hernández Ávila, M.H., Hernández Llamas, H.

2009. Public policy for the poor? A randomized assessment of the Mexican universal health insurance programme. The Lancet 373, 1447-54.
Koenker, R., Basset, G. 1978. Regression quantiles. Econometrica 46, 33-50.
Kolstad, J.T., Kowalski, A.E. 2012. The impact of health care reform on hospital and preventive care: Evidence from Massachusetts. Journal of Public Economics 96 (11–12), 909-929.
Kondo, A., Shigeoka, H. 2013. Effects of universal health insurance on health care utilization, and supply-side responses: Evidence from Japan. Journal of Public Economics 99, 1-23.
Kongsawat, S., P. Rodsawaeng, S. Khanonggnut and W. Charusomboon. 2000. Effectiveness and the process of issuing the Low Income Card scheme, Round 6 (1998-2000). Bureau of Health Policy and Planning, Ministry of Public Health., Nonthaburi, Thailand.
Lavy, V. and J. M. Quigley. 1993. Willingness to Pay for the Quality and Intensity of Medical Care; Low- Income Households in Ghana. LSMS working paper no. LSM 94. The World Bank., Washington, DC. http://documents.worldbank.org/curated/en/1993/03/699083/willingness-pay-quality-intensity-medical-care-low-income-households-ghana.
Limwattananon, S., Tangcharoensathien, V., Prakongsai, P. 2007. Catastrophic and poverty impacts of health payments: results from national household surveys in Thailand. Bulletin of the World Health Organization 85, 600-606.
Lindelow, Magnus, Loraine Hawkins and Sutayut Osornprasop. 2012. Government spending and central-local relations in Thailand's health sector. Health, Nutrition and Poverty Discussion Paper 75680. World Bank, Washington D.C.
Manning, W.G., Newhouse, J.P., Duan, N., Keeler, E.B., Leibowitz, A. 1987. Health Insurance and the Demand for Medical Care: Evidence from a Randomized Experiment. American Economic Review 77 (3), 251-277.
McFadden, D.L. 1984. Econometric Analysis of Qualitative Response Models. In: Griliches, Z., Intriligator, M. (Eds.), Handbook of Econometrics. Vol. II. North Holland, Amsterdam.
McFadden, D.L., Train, K. 2000. Mixed MNL models for dicrete response. Journal of Applied Econometrics 15, 447-470.
Miller, Grant, Diana Pinto and Marcos Vera-Hernández. 2012. Risk protection, service use and health outcomes under Colombia's Health Insurance Program for the Poor. NBER Working Paper 15456. Cambridge, MA.
Ministry of Public Health. Various years. Annual Reports on Health Resource Surveys. Bureau of Policy and Strategy, MOPH, Nonthaburi, Thailand.
Mocan, H.N., Tekin, E., Zax, J.S. 2004. The Demand for Medical Care in Urban China. World Development 32 (2), 289–304.
Mullahy, J. 1998. Much ado about two: reconsidering retransformation and the two- part model in health econometrics. Journal of Health Economics 17, 247-281.
Newhouse, J.P. 1993. Free for all? Lessons from the RAND Health Insurance Experiment. Harvard University Press, Cambridge, MA.
Pannarunothai, S. 2002. Medical Welfare Scheme: Financing and Targeting the Poor. In: Pramualratana, P., Wibulpolprasert, S. (Eds.), Health Insurance System in Thailand.Health Systems Research Institute, Nonthaburi, Thailand. pp. 62-78.
Panpiemras, J., Puttitanum, T., Samphantharak, K., Thampanishvong, K. 2011. Impact of universal health care coverage on patient demand for health services in Thailand. Health Policy 103, 228-235.
Puhani, P.A. 2012. The treatment effect, the cross difference, and the interaction term in nonlinear "difference-in-differences" models. Economics Letters 115 (1), 85-87.
Sahn, D.E., Younger, S.D., Genicot, G. 2003. The Demand for Health Care Services in Rural Tanzania. , Oxford Bulletin of Economics and Statistics 65 (2), 241–260.
Sauerborn, R., Nougtara, A., Latimer, E. 1994. The elasticity of demand for health care in Burkina Faso: differences across age and income groups. Health Policy and Planning 92 (2), 185-92.
Somkotra T, L.L. 2008. Payments for health care and its effect on catastrophe and impoverishment: Experience from the transition to Universal Coverage in Thailand. Social Science & Medicine 67, 2027-2035.
The Lancet. 2012. Themed issue: Universal Health Coverage. The Lancet 380 (9845).

United Nations General Assembly. 2012. Sixty-seventh session, Global health and foreign policy. A/67/L.36. United Nations: New York. http://www.un.org/ga/search/view_doc.asp?symbol=A/67/L.36.

Wagstaff, Adam and Wanwiphang Manachotphong. 2012a. The Health Effects of Universal Health Care: Evidence from Thailand. World Bank Policy Research Working Paper 6119. World Bank, Washington D.C.

Wagstaff, Adam and Wanwiphang Manachotphong. 2012b. Universal Health Care and Informal Labor Markets: The Case of Thailand. World Bank Policy Research Working Paper 6116. World Bank, Washington D.C.

Wagstaff, A., van Doorslaer, E. 2003. Catastrophe and impoverishment in paying for health care: with applications to Vietnam 1993-1998. Health Economics 12 (11), 921-933.

Wagstaff, A., Lindelow, M. 2008. Can insurance increase financial risk?: The curious case of health insurance in China. Journal of Health Economics 27 (4), 990-1005.

Wagstaff, A., Lindelow, M., Jun, G., Ling, X., Juncheng, Q. 2009. Extending health insurance to the rural population: An impact evaluation of China's new cooperative medical scheme. Journal of Health Economics 28 (1), 1-19.

WHO/World Bank. 2013. WHO/World Bank convene ministerial meeting to discuss best practices for moving forward on universal health coverage. March 27, 2013. http://www.who.int/mediacentre/news/statements/2013/uhc_20130219/en/index.html.

Wibulpolprasert, S. 2002. Health Care Systems in Thailand . In: Pramualratana, P., Wibulpolprasert, S. (Eds.), Health Insurance System in Thailand.Health Systems Research Institute, Nonthaburi, Thailand. pp. 3-27.

World Bank. 2013. Universal Health Coverage Studies Series. March 27, 2013. http://web.worldbank.org/WBSITE/EXTERNAL/TOPICS/EXTHEALTHNUTRITIONANDPOPULATION/0,,contentMDK:23352920~pagePK:210058~piPK:210062~theSitePK:282511,00.html.

World Health Assembly. 2011. Sixty-fourth session: Sustainable health financing structures and universal coverage. WHA 64.9. United Nations: New York. http://apps.who.int/gb/ebwha/pdf_files/WHA64/A64_R9-en.pdf.

World Health Assembly. 2005. Fifty-eight session: Sustainable health financing, universal coverage and social health insurance. WHA58.33. http://apps.who.int/iris/bitstream/10665/20383/1/WHA58_33-en.pdf.

World Health Organization. 2010. The World Health Report - Health systems financing: the path to universal coverage. World Health Organization, Geneva.