Journal of Health Economics 32 (2013) 1180–1193

Contents lists available at ScienceDirect

Journal of Health Economics

journal homepage: www.elsevier.com/locate/econbase

Financial protection of patients through compensation of providers: The impact of Health Equity Funds in Cambodia

Gabriela Flores[a,b], Por Ir[c,d], Chean R. Men[e], Owen O'Donnell[f,g,h,*], Eddy van Doorslaer[b,g,h]

[a] Institute of Health Economics and Management, University of Lausanne, Switzerland
[b] Institute of Health Policy and Management, Erasmus University Rotterdam, The Netherlands
[c] Institute of Tropical Medicine, Antwerp, Belgium
[d] National Institute of Public Health, Ministry of Health, Cambodia
[e] URC & Centre for Advanced Studies, Cambodia
[f] University of Macedonia, Greece
[g] Erasmus School of Economics, Erasmus University Rotterdam, The Netherlands
[h] Tinbergen Institute, The Netherlands

ARTICLE INFO

Article history:
Received 26 November 2011
Received in revised form 7 January 2013
Accepted 16 September 2013
Available online 6 October 2013

JEL classification:
H42
H51
I18

Keywords:
Health Financing
User fees
Financial protection
Health care
Cambodia

ABSTRACT

Public providers have no financial incentive to respect their legal obligation to exempt the poor from user fees. Health Equity Funds (HEFs) aim to make exemptions effective by giving NGOs responsibility for assessing eligibility and compensating providers for lost revenue. We use the geographic spread of HEFs over time in Cambodia to identify their impact on out-of-pocket (OOP) payments. Among households with some OOP payment, HEFs reduce the amount paid by 35%, on average. The effect is larger for households that are poorer and mainly use public health care. Reimbursement of providers through a government operated scheme also reduces household OOP payments but the effect is not as well targeted on the poor. Both compensation models raise household non-medical consumption but have no impact on health-related debt. HEFs reduce the probability of primarily seeking care in the private sector.

1. Introduction

De jure, the poor are often exempt from user fees levied at public health facilities in low income countries. *De facto*, exemptions are seldom respected. The primary reason is that providers are charged with responsibility for establishing exemption eligibility but are not compensated for revenue lost from exemptions granted. The incentives to be vigilant in honouring legal rights to fee waivers are not strong. They are further weakened by the often vague criteria for eligibility status and the heavy reliance of health facilities on user fee revenue not only to finance supplies but also to provide incomes to staff whose low salaries may be paid intermittently (Creese, 1991; Gilson et al., 1995; Russell and Gilson, 1997). As a consequence, poor households are left exposed to out-of-pocket (OOP) health payments that threaten to drive them further into poverty. They may opt for unqualified, but ostensibly cheap, providers of health care and for self-medication, or even forgo treatment altogether.

Making exemptions effective would appear to require both separation of responsibility for assessment of exemption eligibility from that of provision of care and compensation of providers for lost fee revenue. Health Equity Funds (HEFs), which have been operating in Cambodia since 2000 and have a lesser presence in Lao and Vietnam, are based on this logic. They are mostly financed by international donors and operated by local Non-Governmental Organisations (NGOs), which have responsibility for selecting patients whose fees at selected public health facilities are paid from the fund. Besides having their fees paid, HEF beneficiaries may also be reimbursed for their transport and food costs.

* Corresponding author at: University of Macedonia, Egnatia 156, Thessaloniki 54006, Greece. Tel.: +30 2310 891478.
E-mail addresses: odonnell@ese.eur.nl, ood@uom.gr (O. O'Donnell).

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http://dx.doi.org/10.1016/j.jhealeco.2013.09.012

This subsidy model has spread rapidly in Cambodia over the last decade. Three-quarters of the population is now resident in areas in which HEFs operate and the government is pursuing a target of nationwide coverage by 2013. HEFs are expanding not only geographically but also from coverage of district hospitals to include health centres. For Cambodia, as well as other low income countries wrestling with the problem of financing public health services while shielding poor patients from prohibitive user fees, it is imperative to establish whether HEFs are succeeding in their primary objective of offering financial protection to poor households and their secondary one of improving access to qualified providers. The existing literature generally argues that HEFs fulfil their promise (Annear, 2010). This conclusion is largely based on small scale studies, which, while providing valuable detail on the operation of HEFs, make only descriptive comparisons between areas with and without a HEF, or of a single area before and after the introduction of a HEF. There has been no country-wide evaluation with a design sufficient to identify the *impact* of HEFs on health care payments and utilisation.

The effectiveness of HEFs in financially protecting the poor from health care costs cannot be taken as given. The model may fail to meet its objectives for a number of reasons. First, targeting of the poor could be weak. All methods that have been employed by HEFs to identify the poor give voice to the community and much latitude in the definition of poverty. While in many respects laudable, this could be exploited to direct subsidies towards cronies. Second, initially most HEFs established eligibility only when someone presented at the hospital for treatment – so-called 'post-identification' (Jacobs and Price, 2008). Many poor may have been unaware that they would be granted exemption from fees. Third, most people in Cambodia do not immediately resort to public health care when sick. Distance to the district hospital and the often unreliable service on offer there, and not only the cost, discourage usage and encourage substitution with medicines purchased from usually unqualified, but convenient, local vendors (Yanagisawa et al., 2004). Waiving fees may not be sufficient to overcome the other deterrents to utilisation of public health care. Fourth, providers may attempt to charge illegally and still claim fees from the HEF. Finally, the NGO itself is usually paid in relation to inputs and estimated workload but compensates facilities on a fee per case basis. To an extent, the disincentive to encourage utilisation by the exempted poor is shifted backward from the providers to the HEF operator.

This paper exploits the geographic spread of HEFs over the last decade to compare changes in outcomes in areas that acquire a HEF with changes in outcomes in areas that remain without a HEF. We implement this difference-in-differences (DID) identification strategy using household data from four nationally representative cross-sectional surveys conducted between 2004 and 2009. Effects on OOP payments for health care, health-related debt, non-medical consumption and health care utilisation are estimated.

We find that HEFs do not reduce the propensity to incur health care payments, which is anticipated since HEFs mainly cover care at the district hospital and will not eliminate all health care expenses, particularly those on self-medication and private sector care. But HEFs do reduce the amount spent on health care by a substantial 35% averaged over all households making some payment. The effect is larger for the poorer households (42%) that HEFs are intended to target. It is also larger for households that mainly rely on public health care (57%), which is what HEFs cover. A government financed and operated funded scheme that reimburses providers for granted fee exemptions is estimated to reduce OOP payments by 29% but, unlike for NGO operated HEFs, the effect on the poor is smaller suggesting a lack of target efficiency. This is important since a policy of tax financed health care for the poor implemented through expansion of the government subsidy scheme is under consideration.

Household non-medical consumption is estimated to increase as a result of HEFs. This suggests that medical expenditures are financed, at least in part, by sacrificing other consumption and so subsidisation of health care has a positive impact on living standards. Despite the fact that the average payment for a hospitalisation in Cambodia has been estimated as equivalent to more than 40 times the daily earnings of a field labourer (Hardeman et al., 2004), a cost that could only be met by most households through borrowing and other coping strategies, we find no significant effect of HEFs on health-related debt, although the point estimate is negative. HEF subsidisation of public care is estimated to reduce the propensity to mainly rely on private care providers when sick, although there is no significant increase in reliance on public care.

In the next section we provide some background on health care financing and the operation of HEFs in Cambodia. In the third section we sketch our identification strategy and describe the data. The models and estimators are set out in Section 4. Results are presented in Section 5. The final section concludes with implications for the financing of health care in Cambodia and further afield.

2. Health Financing and HEFs in Cambodia

2.1. Health Financing

Cambodia, which has a population of a little less than 15 million, is one of the poorest countries in south-east Asia, with GDP per capita in 2009 of only $1915 at purchasing power parity exchange rates (PPP) (US$706), and 28% of the population living on less than $1.25 per day in 2007 (World Bank, 2011). Total expenditure on health per capita is low in absolute terms at only $122 (PPP) in 2009, but at 6% of GDP is the highest relative spending of any ASEAN country except Vietnam (World Health Organization, 2011b). Over 70% of health expenditure is financed from OOP payments (*ibid*), which are mainly for self-medication and private sector care. Around two-thirds of the remainder is estimated to be financed by government, and the rest from external resources.

Utilisation of curative, but not preventive, public health services is low (World Health Organization, 2011a). This reflects perceived low quality of care and unreliability of service provision. Combined with often long distances to public health facilities and cultural preferences for care at home and traditional healers, there is a strong bias towards private sector, often unqualified, providers and self-medication (Annear et al., 2006). This bias is maintained and encouraged by low paid public sector staff moonlighting in the private sector.

Public health facilities are financed through a combination of government funding of salaries, drug supplies and recurrent costs, direct subsidies from international donors and user fees paid by patients and HEFs, as well as some payments through voucher schemes and community based health insurance. The right to charge user fees was established by the 1996 National Health Financing Charter with the objective of providing revenue for the operation of hitherto poorly resourced facilities and to motivate staff paid very low salaries (Jacobs and Price, 2004). All but 1% of user fee revenue is retained by the facility; 60% can be used to provide staff incentives and 39% to supplement operational budgets (Ministry of Health, 2009a). Fees can only be charged after approval by both a local committee, including elected community representatives, and the Ministry of Health (MoH) (Jacobs and Price, 2004). MoH approval is conditional on establishing a system of exemptions of the poor, or rather the poorest.

The opportunity to charge fees is taken up by almost all public health facilities. Fees are estimated to generate around 30%

of public health facility revenue (Annear et al., 2006). Case studies claim that user fees substitute for informal payments, reduce price uncertainty by replacing opaque under-the-table payments with a published price schedule, possibly even reduce costs to patients, raise hospital revenue and, consequently, service quality and utilisation (Akashi et al., 2004; Annear et al., 2006; Barber et al., 2004; Jacobs and Price, 2004). But there is also a widespread belief that exemption schemes were ineffective, in large part because providers were not compensated for the lost revenue. Around 10% of revenue is estimated to be lost due to the fee exemptions that are actually granted (Annear et al., 2006). In the years immediately following the introduction of user fees, the average proportion exempted was around half the official poverty rate (Annear et al., 2006). Official exemption schemes are designed at the Operational District (OD) level within the health system, resulting in a great deal of geographic variation in exemption rates, as well as means testing rules.

Case studies suggest that, prior to the introduction of HEFs, fees deterred utilisation by the poor (Jacobs and Price, 2004) and that OOP payments were a major cause of impoverishment and indebtedness (Van Damme et al., 2004).

2.2. Health Equity Funds

The HEF model recognises the importance of user fees in providing resources and incentives to public health facilities, but also in leaving poor households exposed to health care costs. By funding fee waivers HEFs not only support demand for public health care, they also provide incentives for suppliers to respect exemption entitlements and for staff to be present in facilities to meet the induced effective demand and so obtain resulting revenues. Financed mostly by international donors, HEFs basically act as purchasers of public health care on behalf of the poor. This role is typically carried out by local NGOs, who also screen households, or patients, to identify those entitled to subsidies. During the period studied, providers were usually paid a fixed amount per case, which was very broadly defined. Recently, payments have been related more closely to procedures.

Autonomous at the OD level, HEFs spread rapidly from operating in only 2 of 77 ODs in 2000 to 61 by 2010. The pattern of this expansion was not based on any explicit selection criteria. Given reliance on external funding, HEFs were often located in ODs where there was already some externally funded health project operating. The first HEF pilot schemes were initiated between 2000 and 2003 in Phnom Penh Municipal Hospital and in referral hospitals of six ODs where external financial and technical support to improve service delivery had previously been provided but financial barriers to accessing hospital services by the poor persisted (Ir et al., 2010; Noirhomme et al., 2007). Between 2004 and 2007, which is within our study period, HEFs were introduced in nine ODs as part of a Cambodian-Belgian bilateral health project that included some performance-based financing and other supply-side interventions, and in several other ODs under a USAID-funded health system strengthening project. After 2007, HEFs were often extended to ODs that already had a form of contracting in operation at the public referral hospital. Because of the frequent presence of some other externally funded health project, the ODs in which HEFs were introduced tended to be the better performing ones. Our difference-in-differences strategy allows for these level differences and we also control for the introduction of contracting and other interventions the effects of which might otherwise be attributed to the HEFs.

Twelve ODs without HEFs and six national hospitals are supported by a government subsidy scheme (known as SUBO) that operates through the Ministry of Health and compensates public facilities for fees waived for poor patients at hospitals and some health centres. This is a different model of reimbursement from that of an externally funded HEF operating as an independent third-party purchaser. The often slow and inefficient public administration may cause delays in the payment of providers (Men et al., 2011) and the subsidy does not cover the transport and food costs of patients. But the scheme offers a potential way of unifying the diverse HEF models in operation and its expansion is the most likely means of realising the recent government commitment to tax finance health care for the poor at public facilities. We estimate its effect on OOP payments alongside that of HEFs using the same DID strategy.

OD level autonomy and mostly external funding creates a great deal of heterogeneity in the design and operation of HEFs. They vary in the facilities covered (district hospitals only or also health centres), the size of the budget, the means test used to identify the poor, the point at which the poor are identified (prior to or at presentation for treatment), and whether transport and food costs are reimbursed. With this in mind, a typical HEF benefit package covers district referral hospital medical services, transport costs from health centre to referral hospital, food for patients and carers, and sometimes funeral costs. In four ODs beginning in 2008, UNFPA funds what is referred to as a 'HEF from reproductive health services'. This is effectively a voucher scheme for ante and post natal care and delivery in a public facility. Given the narrowly defined benefit package covering pregnant women only, we do not include this scheme within our definition of a HEF but we do control for it, along with other maternity care voucher schemes, in the estimation.

In 2008, 38% of all hospitalised patients and 25% of deliveries at covered referral hospitals were funded by HEFs (Ministry of Health, 2009b).[1] Where case study sample sizes permit disaggregation, which is seldom, it is evident that women are the largest group of HEF beneficiaries, followed by children. On average, around one-quarter of user fee revenue is estimated to flow from HEFs (Annear et al., 2006), although there is a great deal of variation (Annear et al., 2007; Ir, 2008. In additional to this substantial contribution of HEFs to user fee revenue, the fact this reimbursement is received directly from the NGO operator, as opposed to through the Ministry of Health bureaucracy, provides facilities with a strong incentive to grant exemptions to patients covered by the HEF.

If public hospitals were operating at full capacity, then HEF subsidisation of the poor would be expected to crowd out non-subsidised patients. But utilisation of public sector care is low and hospitals typically have spare capacity. HEF funding that increases facility revenue and, consequently, staff attendance can then generate benefits that extend beyond the direct recipients of the subsidy. It is not the public sector user fee that causes non-poor patients to seek private sector alternatives, since the latter are typically more expensive, but rather the perceived difference in quality. A better stocked and more reliable public sector may therefore increase utilisation and reduce OOP payments of households that are not identified as HEF beneficiaries.

Assessment of subsidy eligibility is carried out before (pre-identification) and/or after (post-identification) presentation for treatment. In the early years, most HEFs only operated post-identification. This involved a means test, based on asset ownership, applied at the NGO office on the hospital site. Pre-identification of households eligible for fee exemptions has increased over time and is implemented usually with a

[1] Cited by Annear (2010, p. 5). Ministry of Health data for 2010 indicate that 6.3% of outpatient cases, 27% of inpatient cases and 15.6% of deliveries at all public health facilities (not only referral hospitals) were funded by HEFs.

Table 1
Health Equity Fund coverage and sample selection.

	2004[a]	2007	2008	2009
Operational districts (77 in total) with				
Health Equity Fund (HEF)[b]	9	27	37	42
Government Subsidy Scheme (SUBO)	0	2	7	9
Communes				
Number in Cambodia Socioeconomic Survey (CSES)	692	293	291	621
With information on HEF coverage	684	290	288	617
Included in 2004 CSES and at least once 2007–2009	472	290	288	340
Without HEF coverage in 2004 (estimation sample[c])	303	187	179	217
With HEF	0	48	66	104
With SUBO	0	3	22	65
Households				
In estimation sample[c]	6305	2063	2000	4038
As % of total cross-section CSES sample	42.7%	58.2%	57.2%	34.0%
In commune with HEF coverage	0	546	776	1899
In commune with SUBO coverage	0	30	229	596

Source: Authors' calculations from Cambodian Socioeconomic Surveys (CSES).
Notes: (a) November 2003-January 2005. (b) The number of ODs with HEF coverage at the beginning of the sampling period of the first wave and the number with HEF cover at the end of the sampling periods of the other cross-sections. (c) Communes/households that did not already have HEF coverage in the first survey wave and with valid responses on all outcomes and control covariates at village/household level.

combination of means test screening of a population and consultation with community representatives on which households are considered poor. There is usually still an opportunity for patients to apply for HEF coverage at the hospital. While reducing exclusion errors, this may increase inclusion errors if there is gaming of the means test to qualify for exemption when treatment is needed. In fact, inclusion errors have been estimated at only around 10% (Annear, 2010; Jordanwood et al., 2009), and most HEF beneficiaries are amongst the poorest households (Hardeman et al., 2004). Exclusion errors are estimated to be much higher, at 25% or so (Annear, 2010; Jordanwood et al., 2009).

In the early years of operation, the means test and community consultation procedures used to identify the poor varied a great deal with the funder and operator of the HEF. From 2007, the Ministry of Planning began implementing standardised procedures (known as IDPoor) to identify poor households and establish entitlement to a variety of government and non-government assistance programmes, including HEFs. Households are screened by a common means test, the score from which is taken into account by a Village Representative Group, whose recommendation on households to be classified as poor is considered and revised after consultation at the village level, and finally approved following review by the Commune Council (Ministry of Planning, 2007). IDPoor has spread geographically, with HEFs playing an important role in its implementation, but by 2009 it did not yet cover the whole country.

3. Empirical strategy and data

3.1. Identifying information and sample selection

We use data from the 2004, 2007, 2008 and 2009 Cambodian Socio-Economic Surveys (CSES). At the beginning of the study period, HEFs were already operating in 9 ODs (out of a total of 77), which had less than one-fifth of the population. By the end of 2007, 18 more ODs had HEFs and by the end of the study period a further 15 HEFs had been established (Table 1) and almost three-fifths of the population was living in areas covered by HEFs. The government subsidy scheme (SUBO) was legislated in 2006 and was functioning at the time of the 2007 CSES in 2 ODs. By 2009, it was operating in 9 ODs where there were no HEFs. We use the variation in HEF and SUBO coverage both between districts and across time in a difference-in-differences (DID) strategy comparing changes in outcomes occurring in areas in which a HEF (SUBO) comes into operation with changes in outcomes in areas that remain without coverage by either a HEF or SUBO.

Each CSES is nationally representative and all follow the same stratified sampling design. Primary sampling is at the 'village' level, which defines neighbourhoods of towns and cities as well as rural villages. The 2004 and 2009 surveys sampled around 720 villages and 12,000 households, while in 2007 and 2008 half the number of villages, and just less than one-third of the number of households, were sampled. In each of 2007 and 2008 the villages sampled were a subset of those sampled in 2004. The 2009 survey sampled (potentially) different villages. In order to reduce the risk of compositional bias, we would like to keep the sample of villages constant between 2004 and 2009. This is not quite possible since the village codes are not consistent across surveys. There are consistent identifiers at the commune level, which represents a slightly wider geographic area.[2] In our estimation sample we include only those communes sampled in 2009 that were also sampled in 2004.[3]

While HEFs are implemented at the OD level, within a survey wave there is variation in HEF coverage across communes within an OD due to differences in the interview date, which can differ by more than 12 months.[4] Among the 33 ODs that acquired a HEF during our study period, in 9 the HEF was functioning in only some of the communes at the time they were surveyed in the initial year of operation. For example, in one OD one commune was surveyed three months before a HEF came into operation, while another commune in the same OD had been exposed to the HEF for seven months by the time it was surveyed later in the sampling period. We identify a commune as being covered if the HEF has been operating for at least one month at the time of the survey.

[2] In the 2004–2009 CSES samples used in estimation there are, on average, 1.4 villages within each commune and 72% of communes consist of only one village.
[3] A compositional bias could theoretically arise if HEF coverage is a motivation for migration between districts. This seems unlikely. If it does exist, then, presuming that those with higher than average health payments move into areas with HEFs, we will underestimate any negative impact on medical expenditure.
[4] The '2004' survey was conducted between November 2003 and January 2005.

A HEF covers the whole poor population within the catchment area of the district referral hospital. In some ODs, particularly in more recent years, HEFs cover care delivered at health centres, in addition to the district hospital. Typically not all health centres within the OD are covered. We do not have data on which health centres are covered and how communes map to health centre catchment areas and so cannot identify households located close to a HEF covered health centre.

There are very few communes for which we could not establish HEF coverage (Table 1). These are dropped from the sample. Selecting only communes sampled in 2004 and in at least one other year, we lose around 30% of communes from the 2004 CSES and 45% of those from the 2009 sample (Table 1). We arrive at the estimation sample by dropping the 36% of communes that already had HEF coverage in 2004 or have missing values for any of the (village level) covariates. Those with coverage in 2004 provide no variation that is helpful in identifying the parameter of interest. The DID estimates are thus derived from comparisons between communes that acquire a HEF after 2004 with those that remain without a HEF in subsequent survey waves. Around a quarter of the estimation sample of communes (and households) that did not have HEF coverage in 2004 was covered by 2007. Just less than one-half was covered by 2009 (Table 1). By that time, the government subsidy scheme was operating in 30% of our estimation sample of communes, where 15% of the households used in the estimation were resident.

The estimation sample corresponds to 43% of the full CSES cross-section sample of households in 2004, around 58% in 2007 and 2008, and a little more than one-third in 2009 (Table 1). An important exclusion is the capital city, Phnom Penh. Operation of HEFs in the capital is confined to the slum areas, which cannot be identified in the survey and were already covered by 2004. Households and health facilities in Phnom Penh obviously differ from those in the periphery and so our estimates cannot be presumed to be indicative of the average impact of HEFs across the whole country.

3.2. HEF coverage at the household level

Not the whole population of an area in which a HEF or the government subsidy scheme operates are direct beneficiaries. At least in principle, both HEF and SUBO subsidies are targeted on the poor. Since the CSES did not ask about receipt or entitlement to subsidised health care until 2009, it is not possible to identify treatment status, in the form of HEF/SUBO funded exemptions, at the household level. The 2009 data can be used to examine the extent to which HEFs make the poor's legal entitlement to user fee exemptions effective. In areas in which a HEF was operating, 7.1% of households reported receipt of free or subsidised health care in the last 12 months, and a further 2.2% reported entitlement without receiving treatment. In areas with no HEF, the corresponding figures are 2.0% and 0.01%. Clearly, there is a strong correlation between HEF operation and reported subsidisation.

To assess the targeting of subsidies on the poor in 2009 we construct an indicator of household living standards from a principal components analysis (PCA) of household characteristics, including housing materials, ownership of durables, land and livestock (see Appendix, Table A1), that are also weighted in the IDPoor programme. We use PCA because the CSES does not have the complete information necessary to apply the IDPoor means test. Subsidies are disproportionately directed to the poor. The poorest quintile of households by the wealth index account for 43% of all those in receipt or entitled to subsidies. Moving up the wealth distribution, the proportion of subsidised households falls monotonically. However, 17% of those reporting receipt or entitlement to subsidies is in the top two quintiles of the wealth index distribution. Since the PCA score is only an imperfect indicator of poverty, and not that used to establish HEF subsidy entitlement, we cannot interpret these numbers as unbiased measures of the target inefficiency of the subsidies. They do suggest, however, that there is substantial leakage to non-poor households.

In the DID analysis, we examine whether the impact of HEFs (and SUBO) varies between households below and above the 40th percentile of the year specific wealth index distribution. This threshold is chosen because data from IDPoor reveal that, on average, around 30% of households are identified as poor and entitled to subsidisation. However, the proportion does vary substantially across districts. Further, our wealth index does not correspond to the IDPoor score or to the assessment employed by HEFs for much of the period of analysis. Allowing for these discrepancies, the 40th percentile seems a reasonable threshold around which to consider variation in the HEF effect. Of course, we certainly cannot rule out that there are HEF beneficiaries in the top 60% of the wealth index distribution and there may be an indirect impact on better-off households through improvements in service quality due to subsidy revenue.

3.3. Outcomes

We estimate impacts on four outcomes: payments for health care, health-related debt, non-medical consumption and health care utilisation. Health payments are recorded as the total amount spent on health care for each household member in the last four weeks. We aggregate across individuals to get total household per capita spending on health care. In 2008, spending on health care was not asked in the health module of the survey resulting in an inconsistency that forces us to drop this wave from the analysis of payments. In 2009, payments for health services and medicines were recorded separately from health care-related transport costs. Comparison of descriptive statistics suggests that respondents in previous waves included transport costs in the amounts reported. We therefore aggregate payments and transport costs in 2009 and rely on a year dummy variable in the models to control for any discrepancy. On average, transport costs are around 20% of total health care costs in 2009. Payments are deflated to constant 2000 prices using the non-food consumer price index.[5]

In Table 2 we split households into those living in communes that acquired a HEF during the study period and those in communes that remained without coverage by either a HEF or the government subsidy scheme. We present means of the outcomes by these 'treatment' and 'control' groups in 2004 and 2009. This is done to give a general impression of any baseline and trend differences between households living in areas that get HEF coverage and those in areas that do not.[6]

At the beginning of the period, a larger proportion of the control than the treatment group incurred any payments for health care in the last month and while the proportion increased over time for the control group, it did not for the treatment group. Conditional on making any payment, the mean per capita amount paid per month was 1386 Riel ($0.36) greater in the treatment group in 2004. Over time, the amounts paid increased substantially for both groups but to a greater extent in both absolute and relative terms for those living in areas that did not acquire a HEF such that by the end of the period payments were higher, on average, in those areas.

[5] Unfortunately, the only available price index for the 2004–09 period is specific to Phnom Penh. There was very rapid inflation in food prices in 2008. Non-food inflation was much less marked.

[6] To save on space and because it is not the intervention of main interest, we do not present means for households resident in areas that acquire coverage by the government subsidy scheme.

Table 2
Means of outcomes by Health Equity Fund coverage.

	2004	2009	Change	% Change
Households				
Percentage with any health payments				
HEF starts 2004–2009 (treatment)	44.6	44.5	−0.13	0.29
No HEF or SUBO 2004–2009 (control)	49.5	51.9	2.39	4.83
Mean monthly health payment per capita (if > 0) (Riel)				
Treatment	9746	16,457	6711	68.9
Control	8360	21,699	13,339	159.6
Percentage with any health-related debt				
Treatment	5.02	3.74	−1.28	−25.5
Control	6.01	4.27	−1.74	−29.0
Mean health-related debt (if > 0) (Riel)				
Treatment	99,462	217,826	118,364	119.0
Control	107,081	202,239	95,158	88.9
Mean monthly non-medical consumption per capita (Riel)				
Treatment	51,780	124,392	72,612	140.2
Control	49,015	116,586	67,571	137.9
Individuals				
Percentage reporting illness, injury or health problem ("ill") in last 4 weeks				
Treatment	17.2	15.3	−1.92	−11.2
Control	18.4	17.5	−0.94	5.1
Percentage seeking health care if ill				
Treatment	64.7	90.0	25.3	39.1
Control	66.7	94.2	27.6	41.3
Percentage usually seeking public health care if ill and seek care				
Treatment	19.0	18.3	−0.72	−3.8
Control	15.6	13.6	−1.98	−12.7
Percentage usually seeking private health care if ill and seek care				
Treatment	20.5	13.6	−6.91	−33.7
Control	14.4	17.2	2.79	19.4
Percentage usually going to pharmacists/drug vendor if ill and seek care				
Treatment	25.3	34.7	9.40	37.2
Control	30.3	36.3	6.03	19.9

Source: Authors' calculations from Cambodia Socioeconomic Survey.
Notes: The 'treatment group' includes household/individuals resident in communes in which there was no HEF coverage in 2004 but there was by 2009. The 'control group' had no coverage from either HEFs or the government subsidy scheme (SUBO) over the whole study period. Amounts deflated to December 2000 prices using non-food price index specific to Phnom Penh. In 2000, the official exchange rate was 3841 Riel to the US$.

We measure household living standards by per capita household consumption net of payments for health care. This is the aggregate of expenditures on food and non-food items, plus the value of food produced for household consumption. In 2004, non-medical consumption per capita was slightly higher (2765 Riel = $0.72) in the treatment group. Between 2004 and 2009, real consumption increased by around 140% in both treatment and control areas (Table 2).

The CSES asks whether the household has any loans that were taken out principally because of illness or injury. No distinction is made between borrowing to pay for health care and to replace earnings lost due to work incapacity. Nonetheless, this provides the opportunity to test for an impact of HEFs on health-related debt. We sum the amount borrowed through all outstanding loans taken out primarily because of illness or injury to get the total health-related debt incurred.

In 2004, 5% of treatment households and 6% of control households had health-related debt (Table 2). The prevalence of debt fell for both groups over time and to a slightly greater extent in areas that remained without a HEF. Among households with health-related debt, the mean amount was around 10,000 Riel ($2.61) in both treatment and control areas in 2004. Over time, the amount of debt increased by 119% the treatment areas and by 89% in the control areas.

The household head or spouse is asked whether each household member experienced any illness, injury or health problem in the last four weeks. He or she is also asked whether each individual sought health care over the same period. In the 2004 survey, the question on utilisation (and payments) was posed only if sickness was reported for an individual. We examine the probability of seeking health care among individuals for whom sickness is reported. When health care use is recorded, the respondent is asked from which provider care is usually sought, with options including different types of hospital, health centre, drug store, traditional healer, etc. We distinguish between respondents reporting their main provider to be in the public sector (national, provincial and district hospitals and health centres), the private sector (private hospitals and clinics), and those mainly using pharmacies and drug vendors.

In 2004, around 18% of individuals in the treatment and control groups reported illness, injury or a health problem in the last four weeks (Table 2). By 2009, this reported sickness rate had fallen to a greater extent in areas that had acquired a HEF. At the beginning of the period, around two-thirds of individuals reporting illness sought care. Fortunately, the propensity to use health care when sick increased dramatically over time and by roughly the same degree in both treatment and control areas. Of the individuals that sought care, the proportion that did so in the public sector fell to

a greater degree in areas that remained without a HEF. Utilisation of private sector care increased over time in the control areas but decreased in the treatment areas. These trends are consistent with HEFs shifting patterns of utilisation towards the public sector. However, there is no sign of reduced reliance on pharmacies and drug vendors. An increasing proportion of both groups reported usually seeking treatment from these providers and the increase is greater among those in the treatment group.

Overall, the unconditional differences-in-differences are consistent with HEFs constraining the increase in health care payments but not with a positive impact on non-medical consumption or a negative effect on health-related debt. Sickness rates came down by slightly more in the areas acquiring a HEF and there is some sign of a greater movement away from private sector care in these localities.

4. Estimation and specification

4.1. Estimation

We estimate modified two-part models (Mullahy, 1998) of OOP payments with fixed effects. Let I_{ict} be an indicator of whether household i, in commune c at time period t incurs any OOP payments and assume this is determined as follows:

$$I_{ict} = 1\left(\beta_1 HEF_{ct} + \alpha_1 SUBO_{ct} + \mathbf{Z}_{ct}\boldsymbol{\gamma}_1 + \mathbf{X}_{ict}\boldsymbol{\theta}_1 + \tau_{1t} + \lambda_{1c} + \varepsilon_{ict} > 0\right) \quad (1)$$

where $1(\cdot)$ is the indicator function and the error (ε_{ict}) is assumed to follow a logistic distribution. The time varying dummy variable HEF_{ct} is equal to one in periods in which a HEF operates in a commune and $SUBO_{ct}$ indicates operation of the government subsidy scheme. We control, through dummy variables, for year and month effects (τ_{1t}) that are common across all communes and commune specific effects (λ_{1c}) that are fixed across all periods. The bias due to the latter incidental parameters is likely to be very small by virtue of the large number of households (38 on average) per commune (Coupé, 2005; Greene, 2004; Heckman, 1981; Katz, 2001). Time varying commune level covariates ($\mathbf{Z}_{ct}$) (see next Section) are included to increase plausibility of the DID identifying assumption by controlling for changes in determinants of health care expenditure and utilisation that are correlated with, but not caused by, the introduction of a HEF.

Time varying household level determinants ($\mathbf{X}_{ict}$) are included both to gain precision and to avoid bias in the instance that their year specific commune level averages are correlated with the introduction of HEFs. The vector includes, among other characteristics (see next Section), a set of dummies indicating the wealth index quintile of the household. Since the wealth indices are year specific and because the trends in outcomes might differ by wealth, we allow the wealth effects to be time varying.

The average effect of a HEF on the probability of incurring OOP payments across the population in HEF covered areas during periods of operation is estimated by the average partial effect:

$$\widehat{ATT}_1 = \frac{1}{N_{HEF}} \sum_{i \in S_{HEF}} \Lambda(\hat{\beta}_1 + \mathbf{Z}_{ct}\hat{\boldsymbol{\gamma}}_1 + \mathbf{X}_{ict}\hat{\boldsymbol{\theta}}_1 + \hat{\tau}_{1t} + \hat{\lambda}_{1c}) - \Lambda(\mathbf{Z}_{ct}\hat{\boldsymbol{\gamma}}_1 + \mathbf{X}_{ict}\hat{\boldsymbol{\theta}}_1 + \hat{\tau}_{1t} + \hat{\lambda}_{1c}), \quad (2)$$

where $\Lambda(.)$ is the logistic distribution function, S_{HEF} is the set of observations for which $HEF_{ct} = 1$ and N_{HEF} is the number of such observations. The average effect of the government subsidy scheme is estimated analogously with $\hat{\alpha}_1$ replacing $\hat{\beta}_1$ and the averaging done over the corresponding sub-sample in areas with SUBO coverage.

The expectation of household OOP payments (y_{ict}) per capita over their positive range is specified as an exponential function:

$$E[y_{ict}|y_{ict} > 0] = \exp(\beta_2 HEF_{ct} + \alpha_2 SUBO_{ct} + \mathbf{Z}_{ct}\boldsymbol{\gamma}_2 + \mathbf{X}_{ict}\boldsymbol{\theta}_2 + \tau_{2t} + \lambda_{2c}). \quad (3)$$

The error of this Generalised Linear Model (GLM) is assumed to follow a gamma distribution. The average effect of a HEF on mean positive payments among those with such payments in the treated population is estimated by:

$$\widehat{ATT}_2 = \frac{1}{N_{HEF_+}} \sum_{i \in S_{HEF_+}} \exp(\mathbf{Z}_{ct}\hat{\boldsymbol{\gamma}}_2 + \mathbf{X}_{ict}\hat{\boldsymbol{\theta}}_2 + \hat{\tau}_{2t} + \hat{\lambda}_{2c})[\exp(\hat{\beta}_2) - 1], \quad (4)$$

where S_{HEF_+} is the subset of S_{HEF} with positive OOP payments in the last four weeks and N_{HEF_+} is the number of such households. In order to assess the relative magnitude of the effect, we also estimate the ATT relative to the counterfactual of no HEF exposure, which is simply $\widehat{RATT}_2 = \exp(\hat{\beta}_2) - 1$. This gives the proportionate impact on health payments relative to the average amount that would have been paid by the treatment group in the absence of a HEF. Treatment effects of the government scheme are again estimated in analogous fashion.

Standard errors for the average treatment effects are calculated by a delta method that takes account of sample clusters (Korn and Graubard, 1999). Despite allowance for commune fixed effects, the cluster adjustment is intentionally not applied at the commune-year level but one level up at the commune level in order to allow for possibly serially correlated commune-level shocks that would result in overstatement of the precision of the DID estimate, which is identified from variation in HEF exposure at the commune, rather than the household, level (Angrist and Pischke, 2009; Bertrand et al., 2004). Since $\widehat{ATT}_2$, like $\widehat{ATT}_1$, is derived from a nonlinear model, it is a function not only of the coefficient on the treatment indicator but of all the model coefficients and covariates, including the time and commune fixed effects. The sampling variance of such a nonlinear function over a large number of estimates and covariates averaged over the sample may be high (Greene, 2010). The $RATT_2$, which is a function of the coefficient on the treatment indicator only, can be estimated with greater precision.

Both HEFs and the government scheme are intended to target the poor and should have greater impacts on the poor population. To allow for this explicitly, beyond effect heterogeneity arising from non-linearity of the estimators, we extend the specifications to include second-order terms between each of the treatment indicators and poverty status, such that Eq. (3) becomes:

$$E\left[y_{ict}\left|y_{ict} > 0\right.\right] = \exp\left(\beta_2 HEF_{ct} + \delta_2 HEF_{ct} \times POOR_{ict} + \alpha_2 SUBO_{ct} + \eta_2 SUBO_{ct} \times POOR_{ict} + \mathbf{Z}_{ct}\boldsymbol{\gamma}_2 + \mathbf{X}_{ict}\boldsymbol{\theta}_2 + \tau_{2t} + \lambda_{2c}\right) \quad (5)$$

and Eq. (1) is extended analogously. $POOR_{ict}$ is an indicator of poverty status defined, as explained in the previous section, by location below the 40th percentile of the (year specific) wealth index. Keep in mind that $\mathbf{X}$ includes indicators of wealth index quintiles interacted with the year effects. So, we are allowing outcomes and their trends to differ by wealth level in both treatment and control areas. Given our imprecise notation that uses β_2 to represent different parameters in (3) and (5), the estimated treatment effects for the non-poor are as above except for summation over the non-poor subset of the defined samples. For the poor, the treatment

effect, which is the incremental effect of HEF_{ct}, is also a function of the second-order term, δ_2.

Treatment effects on the outcomes other than OOP payments are estimated using the same general specifications but with different estimators. Because relatively few households incur health-related debt (Table 2) there is insufficient within commune variation to make inclusion of a full set of commune dummies in the second part of the model feasible. Instead, we control for time invariant differences between treatment and control communes through a dummy to indicate residence in a commune that eventually gets a HEF (SUBO), in addition to the dummy indicating HEF (SUBO) coverage at the time of the survey. The relatively small sample size ($n = 590$) and highly skewed distribution of debt also prompts us to adopt a robust GLM estimator (Cantoni and Ronchetti, 2006) that is less sensitive to outliers for this outcome. All the health care utilisation outcomes are binary and are modelled by logit with dummies to control for the commune effects. For these outcomes, the analysis is at the individual, rather than the household, level.

4.2. Control variables

As we pointed out in Section 2.2, HEFs were often introduced where some other externally funded health project, often contracting, was already in operation. At the beginning of the study period, the management of public health services was contracted to an NGO in 10 of the 33 ODs that acquired a HEF between 2004 and 2009. In contrast, none of the 35 ODs that remained without a HEF had contracting in operation in 2004. The fixed effects estimator is intended to eliminate differences arising from contracting, or any other health service initiatives, that operated over the full observation period. But interventions other than HEFs or SUBO introduced during the observation period could potentially create bias. By 2009, contracting had been introduced in a further 9 of the 33 ODs that acquired a HEF between 2004 and 2009, while it was established in only 3 of the 35 ODs that remained without a HEF. To minimise the risk of bias, we control directly for the operation of contracting within the OD in which the household is located at the time of the survey.

Within the study period, the model of contracting in operation involved the Ministry of Health signing a contract with a NGO for the management of public health services within an OD. Public and donor funding continued to be directed exclusively to public health facilities but responsibility for the management of these providers was taken outside of the public bureaucracy and handed to an NGO. The latter was paid conditional on the realisation of targets specified in the contract. Performance indicators were defined in relation to utilisation of maternal and child health services (e.g. antenatal care, deliveries in facilities, immunisation), as well as utilisation of treatment services by the poor and the overall facility contact rate. A review of contracting in eleven ODs found that seven met all targets, and only two were deficient in two or more targets (SBK Research and Development, 2009). One study conducted before our observation period found that contracting improved targeted outcomes, and reduced OOP health expenditures (Bhushan et al., 2007). There is some evidence indicative of contracting raising public hospital occupancy rates (Annear et al., 2006) and it has been claimed that it works best when operating in conjunction with HEF subsidisation of user fees (Annear et al., 2007).

The provision of vouchers for maternity care at public health facilities is a second important health service initiative introduced within the study period. Pregnant women, in some places only those assessed as sufficiently poor, are issued with vouchers that can be exchanged for ante natal care, delivery and post natal care at public health centres or posts.[7] The first such schemes started in 2007. Unlike contracting, they were not more likely to be introduced alongside HEFs. By the end of 2009, vouchers had been introduced in 12 ODs that had a HEF or SUBO, and in 11 with no HEF or SUBO. We control for an indicator of whether there is any maternity care voucher scheme in operation at the time of the survey in the district in which the household is located.

In addition to controlling for contracting and maternity vouchers, the DID identification strategy is further strengthened by the availability of a village level survey conducted as part of the CSES that makes it possible to take account of changes in health care supply, public health programmes, epidemics, credit institutions and development projects that could potentially be correlated with both the outcomes examined and the introduction of HEFs. Descriptions, means and changes in means of these village level control variables are given in Table A2 in the Appendix separately for communes that acquired a HEF after 2004 ('treatment group') and those that remained without a HEF ('control group').[8] At baseline, the communes that eventually got HEF coverage tended to be closer to a district hospital and to a health centre. Over time, there was a significant increase in 'control' communes located within 2 km of a health centre indicating that more new health centres were opened in districts that remained without a HEF. At the beginning of the period, villages in the treatment districts were more likely to have a bank. During the study period, there was a significant increase in the likelihood of a bank being found in both treatment and control villages. Malaria receded as a health problem in both the villages that acquired HEF coverage and those that did not. The propensity for dengue fever to be reported as a major health problem increased significantly only in the control villages. Without control for this differential change, there would be a risk of attributing increased health care expenditures in areas that remained without a HEF and also experienced a dengue epidemic to the constraining effect of HEF coverage in areas that obtained it and were also lucky enough to avoid a dengue epidemic. On the other hand, the operation of public health programmes, which by providing preventive care should reduce health expenditures, increased by more in control areas.

Control for differences between treatment and control areas in changes in village level observables is clearly an advantage. But, in combination with the non-random selection of HEF locations acknowledged in Section 2.2, the differences that are observed may raise concerns about the plausibility of the parallel trends identification assumption regarding unobservables.[9] To assess the extent to which such concerns are justified, we use the village survey data from the pre-treatment period to examine whether the treatment and control areas differ with respect to changes in health services reported by village representatives. Across the treatment villages, 61% report that, in the past five years, health services for the people of the village improved (as opposed to remaining the same or deteriorating), compared with 66% of the control villages. While these point estimates suggest that health services were slightly less likely to be improving in the treatment

[7] One such scheme, sponsored by the UNFPA, is referred to as a 'HEF for reproductive health' but is, in effect, a voucher scheme. It covers maternity care only. We do not identify this as a HEF but as a voucher scheme.

[8] The 'control group' also excludes those communes in which the government subsidy scheme came into operation by 2009.

[9] Note that in the nonlinear models estimated, the DID identification assumption is not that the outcome follows parallel trends in the treatment and control groups since the (partial) time effect differs across all observations in such models (Puhani, 2012). Rather, identification is given by the restriction that the time coefficient in the index function does not vary with treatment status.

villages prior to the introduction of HEFs, the difference is not significant (Pearson $\chi^2(2) = 1.66$, $p = 0.437$). The village representative is asked to identify the main problems with the village health services. Since we are mostly interested in the impact of HEFs on payments for health care, we examine whether there is any difference between treatment and control villages in the change over the pre-treatment period in the extent to which the main problem is identified as "health services are too expensive". The null of no difference in the trends is not rejected indicating that, prior to the introduction of HEFs, the cost of health care was not becoming more or less of a problem in the treatment villages compared with the control villages.[10] While it is impossible to test the DID identification assumption, the pre-treatment trends in the village level data provide no reason to doubt it.

At the household level, we control for household demographics, housing, urban/rural location, economic activity, wealth index quintiles, and head of household demographics, education, employment, occupation, disability status and ethnicity (Table A2).[11] The areas that remained without a HEF between 2004 and 2009 are almost entirely rural, while almost one-third of those that obtained a HEF are urban. As a consequence, households in the treatment areas are more likely to rent housing and are slightly better educated. Over the study period, there are significant changes in household demographics, occupation, employment and education for both groups. Differences in these changes could have biased the estimates if it were not possible to control for these covariates.

5. Results

5.1. Health payments

The estimated effects of HEFs and the government subsidy scheme (SUBO) on payments for health care are presented in Table 3. There is no indication of an effect of either intervention on the probability that a household makes any payment for health care. This is to be expected. HEFs mainly pay for care at district hospitals and subsidise related transport and food costs, while SUBO does not even cover the latter costs. It is unlikely that these subsidies will eliminate all expenditures related to an episode of illness. Individuals may self-medicate and be treated privately before seeking care at the public facility (Annear, 2010).

Among households that make some payment for health care, on average, HEFs reduce the monthly per capita amount paid by almost 8000 Riel, equivalent to $2. While this may not seem much, it represents a 35% reduction from a baseline average of around 23,000 Riel ($6). The magnitude and significance of this estimated effect is robust to estimation by least squares rather than GLM.[12] Combining the estimated effects on the probability of any payment and the conditional amount of payment, we estimate that, on average over all households, a HEF reduces the amount paid by 1770 Riel ($0.46), or 26%.[13]

The estimated average effect of a HEF on health payments (conditional on there being any payment) is much larger in both absolute and relative terms for the poor, defined as households in the bottom 40% ranked by the wealth index.[14] For these households, the positive amount of health payments is estimated to fall by a substantial and highly significant 42%. For households above the 40th percentile of wealth, payments are estimated to fall by 23% and this is significant only at the 10% level.[15] Subject to the caveat that our ranking of households based on possession of assets, housing conditions and economic activity is only indicative of relative poverty status, it appears that HEFs are indeed successful in targeting financial protection on the most poor. Stratifying by an indicator that more closely corresponds to the poverty assessment conducted by HEFs would presumably reveal even greater bias towards the 'poor' but would beg the question of which assessment is more accurate in identifying households experiencing the greatest deprivation.

Since HEFs subsidise care received at public health facilities, principally district hospitals, one expects their impact on payments to be greater for those obtaining care mainly from the public sector. But, to the extent that HEFs encourage substitution of public for private care and for self-medication, payments to these other providers may also be reduced. Lack of data on payments made specifically to each type of provider makes it difficult to reach firm conclusions about the extent to which subsidisation of public care leads to its substitution for other sources of treatment. Nonetheless, comparing the impact of HEFs on *total* OOP payments per capita across households that differ in the provider from which they report *usually* seeking care is informative.

Among households in which a public provider is reported as the main source of care for at least one person, the introduction of a HEF is estimated to reduce OOP payments for health care by a highly significant 23,852 Riel ($6.21), or 57% (Table 3).[16] These suggest very substantial direct gains from the subsidy to public care. For those relying principally on private clinics, the point estimates are negative but are not remotely significant, such that the null of no impact of subsidised pubic care on spending on private care is not rejected. One must keep in mind, however, that the effect is estimated for those that continue to rely mainly on private care.

For households that report usually seeking treatment from a pharmacy or drug vendor, there is a significant reduction of 4757 Riel ($1.24) in total OOP payments per capita. While this is one-fifth of the average decrease in payments among households that mainly rely on public providers, it still represents a 37% reduction relative

[10] The test is implemented by regressing the proportion of villages within each commune that identifies "health services are too expensive" as the main problem in the pre-treatment period on commune fixed effects, year effects and interactions between the year indicators and treatment status. The p-value from the test that all interactions are zero is 0.28. With the dependent variable defined in terms of the expense of health services being one of the three main problems, the p-value is 0.53.

[11] The models for utilisation of health care include the village and household level covariates listed in Table 3 except that for adults the individual's own education, economic activity and occupation are used rather than those of the head of household and demographics are represented by age-sex specific dummies.

[12] Least squares regression of log OOP gives a coefficient on the HEF indicator of 0.355, which is significant at 1% and implies a 43% reduction in (positive) health payments.

[13] The absolute effect is estimated by: $\widehat{ATT} = \frac{1}{N_{HEF}} \sum_{i \in S_{HEF}} \left[\Lambda\left(\hat{\beta}_1 + \mathbf{Z}_{ct}\hat{\boldsymbol{\gamma}}_1 + \mathbf{X}_{ict}\hat{\boldsymbol{\theta}}_1 + \hat{\tau}_{1t} + \hat{\lambda}_{1c}\right) \exp(\hat{\beta}_2) - \Lambda\left(\mathbf{Z}_{ct}\hat{\boldsymbol{\gamma}}_1 + \mathbf{X}_{ict}\hat{\boldsymbol{\theta}}_1 + \hat{\tau}_{1t} + \hat{\lambda}_{1c}\right)\right] \times \exp\left(\mathbf{Z}_{ct}\hat{\boldsymbol{\gamma}}_2 + \mathbf{X}_{ict}\hat{\boldsymbol{\theta}}_2 + \hat{\tau}_{2t} + \hat{\lambda}_{2c}\right)$. The relative effect is given by $\widehat{RATT} = \frac{1}{N_{HEF}} \sum_{i \in S_{HEF}} \frac{\Lambda\left(\hat{\beta}_1 + \mathbf{Z}_{ct}\hat{\boldsymbol{\gamma}}_1 + \mathbf{X}_{ict}\hat{\boldsymbol{\theta}}_1 + \hat{\tau}_{1t} + \hat{\lambda}_{1c}\right)}{\Lambda\left(\mathbf{Z}_{ct}\hat{\boldsymbol{\gamma}}_1 + \mathbf{X}_{ict}\hat{\boldsymbol{\theta}}_1 + \hat{\tau}_{1t} + \hat{\lambda}_{1c}\right)} \exp(\hat{\beta}_2) - 1$.

[14] Effects on the probability of incurring payments are not significant for either the poor or the non-poor and are not significantly different from each other, and so only effects on the conditional amount of payments by poverty status are presented.

[15] Model (5), with explicit heterogeneity of effects by poverty status, is preferred to the restricted model (3) with the null of $\delta_2 = 0$ being rejected at the 10% level of significance.

[16] Conditional on seeking care and so reporting a usual provider, almost all observations make some payment and so the first part of the model is not interesting. In any case, for all the models estimated permitting explicit interaction effects, we never found any significant impact on the probability of incurring OOP payments for any group and so report only the conditional mean estimates in Table 3.

Table 3
Effects on payments for health care (OOP).

	Pr(OOP > 0) logit		E[OOP|OOP > 0] GLM			
			Riel (3841 = $1)		Relative effect	
	ATT_1	SE	ATT_2	SE	$RATT_2$	SE
HEF effects						
All	0.045	0.041	−7895**	3211	−0.346***	0.091
Poor			−8915**	3842	−0.418***	0.103
Non-poor			−4904	3195	−0.227*	0.117
Household's usual health care provider						
Public			−23,852***	6907	−0.573***	0.083
Private			−8205	8203	−0.150	0.180
Pharmacy			−4757***	1760	−0.366***	0.094
SUBO effects						
All	0.040	0.072	−8763	5780	−0.289*	0.148
Poor			−5060	6843	−0.180	0.223
Non-poor			−9990*	6024	−0.324**	0.144
Household's usual health care provider						
Public			−11,101	10,859	−0.216	0.209
Private			−17,801	13,758	−0.337	0.205
Pharmacy			−5277***	2637	−0.377***	0.131
N	12,310					5646

Notes: Estimated from 2004, 2007 and 2009 CSES. OOP is household OOP payments for health care and related transport and food costs per capita in the last four weeks. GLM specified with Gamma distribution and log-link. Average treatment effects on the treated (ATT) estimated as in Eqs. (2) and (4). All models control for covariates listed in Table A2 plus commune fixed effects, month effects, year effects and wealth quintile effects with the latter two interacted. Standard errors (SE) are computed by delta method and corrected for clustering at the commune level. Effects by poverty status and type of provider derived from model (5), or analogous, with partial effects averaged over appropriate sub-samples.

* $p < 0.1$.
** $p < 0.05$.
*** $p < 0.01$.

to a lower average.[17] This could derive from reduced payments for public care among those who rely mainly, but not exclusively, on the purchase of medicines from drug vendors. It could also result from reduced reliance on self-medication or even a reduction in prices charged by drug vendors in response to the subsidised public care. Unfortunately, with the data at hand it is not possible to cast light on the relative importance of these potential explanations.

Since the impact of a HEF might be expected to vary with its length of operation and a household's proximity to a district hospital, where the HEF is typically located and the services of which it mainly covers, we also considered model specifications that explicitly allow heterogeneity of the effect in these dimensions. There was no support for this with the null of a zero second order term between the HEF indicator and each of these characteristics not being rejected.[18] There was also no significant interaction between the operation of a HEF and the contracting of public health services.

The point estimate of the absolute effect of the government subsidy scheme on (positive) OOP payments is negative but not significant (Table 3). The relative effect is 29% and is significant at 10%. The difference in significance of the absolute and relative effects is due to the dependence of the former on all coefficients and covariate values.[19] Split by poverty status, contrary to what is observed for HEFs, the estimated effect of the government scheme is larger for the non-poor. In fact, there is no significant impact on the poor. Some caution is called for here since the sample exposed to SUBO is much smaller than that exposed to HEFs and this scheme mainly came into operation between the last two cross-sections used for estimation making it more difficult to estimate its effect with precision, particularly for sub-samples. This said, the smaller and non-significant estimated effect on the poor is consistent with a review of the government scheme that observed that despite its burdensome bureaucracy that increases the administrative costs of applying for compensation and the delay before receiving it, there is no effective monitoring of claims, leaving the scheme vulnerable to leakage of funds and spending on fictitious patients (Men et al., 2011).

The government scheme has no significant impact on OOP payments for households that report usually being treated by public or private health services but it reduces payments made by those relying mainly on pharmacies and drug vendors by as much in relative terms as the impact of HEFs. This group would appear to be particularly sensitive to subsidised public care.

While the study is not purposefully designed to identify the effect of contracting the management of public health services, a by-product of controlling for contracting in a commune fixed effects model is an estimate of the effect of introducing contracting. Over the whole sample, there is no significant effect on OOP payments but the estimates indicate a strongly significant impact among the non-poor. The partial effect averaged over all households above the 40th percentile of the wealth index indicates that contracting reduces (positive) OOP payments per capita by 6337 Riel (SE = 3173), equivalent to a relative effect of -0.302 (SE = 0.119).[20] This is consistent with the large negative effect

[17] A model analogous to (5) is preferred to a restricted one without explicit interactions between the HEF indicator and type of usual health care provider in the sense that the coefficient on each interaction term is significant at 1%.

[18] Given nonlinearity of the model, even in the absence of second-order terms in the index function, the estimated effects will differ across sub-samples simply because of differences in the covariate values. But such heterogeneity does not imply that, all else equal, the HEF effect varies with its length of operation or a household's distance from the district hospital.

[19] The test of the null of the index function parameter on the SUBO indicator being zero gives a *p*-value of 0.102.

[20] Both the absolute and relative effects are significant at 5% for the non-poor. The contracting coefficient differs significantly (1%) between the poor and non-poor, and the effect is not significant for the poor.

Table 4
Effects on health-related debt.

	Pr(debt > 0) logit		E[debt|debt > 0] Robust GLM		
	ATT_1	SE	ATT_2	$RATT_2$	SE
HEF	0.005	0.009	−34,833	−0.160	0.143
SUBO	−0.004	0.015	−17,021	−0.115	0.272
N	10,670		590		

Notes: Estimated from 2004, 2007, 2008 and 2009 CSES. Standard errors for ATT_2 derived from robust GLM estimator have not been computed and those for the $RATT_2$ are not adjusted for clustering at the commune level. Otherwise, notes as for Table 3.

Table 5
Effects on non-medical consumption.

	ATT	SE	RATT	SE
HEF	6718*	3777	0.064*	0.038
SUBO	15,941*	8561	0.145*	0.075
N	13,791			

Notes: Estimated from 2004, 2007, 2008 and 2009 CSES. Dependent variable is total household consumption per capita in last four weeks excluding costs of health care. Otherwise, notes as for Table 3.
* indicates significant at 10% level.

estimated by (Bhushan et al., 2007), who attribute it to contracting encouraging substitution of public for private providers in response to the improved quality of service available from the former. Their study found that contracting reduced staff absence at public facilities, decreased the propensity to use unqualified providers and increased the probability of using qualified public sector providers. The reason why the effect appears to be confined to the non-poor is not immediately obvious. One possibility is that only the better-off households have the economic means to take advantage of the improved quality of public health services.

5.2. Health-related debt and non-medical consumption

HEFs succeed in reducing payments for health care, particularly for poorer households. Does this result in less borrowing to pay for health care and/or increased spending on other goods and services? There is no evidence whatsoever that either HEFs or the government subsidy scheme reduce the probability of incurring health-related debt (Table 4). Conditional on the existence of health-related debt, the point estimate of the effect is negative for both HEF and SUBO and is larger in both absolute and relative terms for the former. But the effect is not close to significance for either intervention. The lack of significance may simply be due to the relatively few households available from which to estimate the level effect, particularly since adoption of a robust estimator comes at the cost of precision (Cantoni and Ronchetti, 2006).[21] Subject to this caveat, despite reducing spending on health care, there is no evidence that HEFs lift the burden of debt on households.

A positive and significant (at 10%) estimated impact of HEFs on non-medical consumption (Table 5) is consistent with the sacrifice of other consumption being an important means of financing spending on health care. The relative effect indicates that per capita non-medical consumption rises by 6.4%, on average, when a HEF is introduced.[22] Given that the average household budget share spent on health care is 5.7% and we estimate that HEFs reduce OOP payments by 2.6% averaged over those with and without any such payments, this estimate seems rather large. The estimated effect of the government scheme on consumption is even larger at 14.5%. It is unlikely that the magnitudes of these estimates reflect causal effects of the interventions alone. If HEFs and SUBO were introduced in areas that would have experienced more rapid growth of consumption even in their absence, then the effects on consumption will be overestimated. In this case, the estimated effects on payments for health care are unlikely to be upwardly biased in magnitude. Areas experiencing greater than average growth of incomes would have more resources available to spend on health care. Yet, we find that the introduction of both HEFs and SUBO reduces health expenditures.

Table 6
Effects on sickness and health care utilisation.

	ATT_1	SE
Probability of:		
Illness, injury or other health problem in last 4 weeks		
HEF	−0.007	0.018
SUBO	−0.005	0.029
N	69,587	
Seeking health care if ill in last 4 weeks		
HEF	0.057	0.046
SUBO	0.022	0.046
N		10,773
Usually treated at public provider if ill and seek care		
HEF	−0.003	0.041
SUBO	0.001	0.057
N		8686
Usually treated at private clinic/hospital if ill and seek care		
HEF	−0.112**	0.049
SUBO	0.01	0.059
N		7925
Usually treated at pharmacy/drug vendor if ill and seek care		
HEF	0.079	0.055
SUBO	0.035	0.088
N	8397	

Notes: Estimated from CSES 2004, 2007, 2008 and 2009. Observations are individuals. ATT_1 estimated from dummy variable logit models as in Eq. (2). All models control for covariates listed in Table A2 plus commune fixed effects, month effects, year effects and wealth quintile effects with the latter two interacted. For adults, the individual's own education, economic activity and occupation is used rather than that of the head of household. In addition, demographics are controlled for through indicators for 18 age-sex categories. Standard errors (SE) are corrected for clustering at the commune level. Sample sizes differ due to dropping communes for which there is no within variation in the outcome.
** indicates significant at 5% level.

5.3. Health care utilisation

In Table 6 we present estimates of the impact of HEFs and SUBO on the probability of reporting illness and utilisation of health care. There is no significant effect on the probability of reporting sickness. Conditional on reporting illness, the point estimate of the impact on the probability of seeking care is positive for both HEFs and SUBO but in neither case is it significant. This is perhaps not surprising since HEFs and the government subsidy are more likely to influence the pattern of health care seeking behaviour than the aggregate rate of utilisation. We test for this by conditioning on reporting illness and seeking care and modelling the propensity to seek care mainly from a public provider, a private provider or

[21] For computational reasons, standards errors for the ATT have not been estimated. Neither the index function coefficients nor the RATT is statistically significant for both interventions. It is very unlikely that the ATT, which is a function of all coefficients and covariates, would be significant when the coefficient on the treatment indicator is not. The estimated ATT is not remotely significant using the standard (non-robust) GLM estimator. Nor is the RATT estimated using either standard GLM or OLS on the log of debt.

[22] An OLS regression of log non-medical consumption with commune fixed effects gives estimated effect of 5.8%, which is also significant at 10%.

a pharmacy/drug vendor. There is no significant effect of either intervention on the probability of seeking public care. HEFs, but not SUBO, have a significant negative effect on the probability of seeking care from a private provider.[23] The magnitude of the effect is large at around 40% of the baseline probability of 0.27 of usually seeking care from the private sector. This suggests that HEFs may substantially discourage utilisation of private providers but the lack of a significant positive impact on public care is not consistent with this occurring through substitution of public for private care. Contrary to the desired reduced reliance on self-medication, the point estimate of the effect on resorting to a pharmacy or drug vendor when sick is positive for both interventions but is not significant. The significant negative impact of HEFs on total OOP payments among those mainly relying on pharmacies observed in Table 3 does not therefore appear to be due to any impact of HEFs on the composition of this group.

6. Conclusion

Poorly resourced health systems struggle to achieve an appropriate balance between raising revenue essential for service provision and financial protection of the poorest households from health care costs. Because user fees are such an important source of revenue, providers have no incentive to grant exemptions from them even when they are statutorily obliged to do so. Compensation of providers for the revenue lost through exemptions and separation of responsibilities for service provision and adjudication of exemption eligibility are essential ingredients of an effective fee waiver system. Health Equity Funds possess these characteristics.

We evaluate the impact of HEFs on households' payments for health care, their debts, non-medical consumption and health care utilisation in Cambodia. We find that HEFs have a significant and substantial negative impact on health care payments. On average over households making payments for health care in covered areas, HEFs reduce the amount paid by 35%. The effect is even larger for poor households and those mainly making use of public care.

A government financed and operated compensation scheme has a smaller relative impact than NGO operated HEFs and does not appear as well targeted on the poor. This suggests that the arm's length relationship of HEFs with public providers is an important ingredient of their success. But it also poses a dilemma for their sustainability. In Cambodia, HEFs have been mainly funded by external donors. This leads to much heterogeneity in their operation and also means that the continued subsidisation of poor households is uncertain. In the long run, government funding is probably the only sustainable source of finance. The government of Cambodia has recently committed to tax financed care for the poor at public facilities with an expansion of the government subsidy scheme the most likely means of implementation. Our analysis suggests that this may weaken financial protection of the poorest from health care costs. One reason is that the government scheme does not cover transport and food costs. A second is that it blurs the distinction between the finance and the provision of care and requires that facilities seek compensation for user fee exemptions through a bureaucratic and slow public administration with little effective monitoring of claims.

Rather than bring HEFs within the public system, it may be preferable to more clearly define their role as purchasers of health care for the poor within a system that also demarcates responsibility for the public funding of health care, including the subsidisation of the poor, and the provision of services, management of which could be contracted out. HEFs would continue to operate independently, receiving a public subsidy to pay the user fees of the poor and relying on external funding to reimburse transport and food costs (Men et al., 2011). The evidence presented here is not sufficient to conclude decisively in favour of this tripartite model but our findings that independent HEFs are more effective in reducing medical expenditures incurred by the poor do suggest that autonomy and division of responsibilities are important ingredients of success.

[23] The restricted model was not rejected in favour of a more general one with an explicit interaction between the treatment indicators and, in turn, poverty status and gender.

Table A1
Household characteristics used in principal components analysis construction of wealth index with means and factor weightings in 2004.

	Mean	Factor weighting
Primary construction material of the roof of the housing/dwelling unit occupied by the household is		
Thatch	0.208	−0.185
Tiles	0.268	0.047
Fibrous cement	0.051	0.061
Galvanised iron	0.352	0.032
Salvaged materials	0.002	0.003
Mixed but mainly made of galvanised iron/aluminium, tiles or fibrous cement	0.011	0.003
Mixed but mainly made of thatch/leave/grass or salvaged materials	0.005	−0.025
Concrete	0.042	0.237
Other	0.062	−0.085
Primary construction material of the wall of the housing/dwelling unit occupied by the household is		
Bamboo, thatch	0.271	−0.179
Wood or logs	0.252	0.041
Plywood	0.197	0.032
Concrete, brick, stone	0.093	0.314
Galvanised iron	0.022	−0.001
Fibrous cement	0.002	−0.002
Makeshift, salvaged	0.014	−0.021
Other	0.150	−0.110
Floor area of the housing (m^2)	43.013	0.265
Area of the plot used for vegetable gardening, agricultural or farming (m^2)	21.60	−0.004
Percentage of household members economically inactive (%)	0.416	0.039
Durable goods per household, number of		
Radio	0.379	0.092
Television	0.496	0.295
Cell phones	0.201	0.372
Videos	0.069	0.260
Stereo	0.238	0.225
Camera (picture/video)	0.030	0.213
Bicycle	0.847	0.057
Motorcycle	0.350	0.322
Cart	0.251	−0.075
Car	0.033	0.267
Jeep	0.007	0.095
Rowing boat	0.085	−0.034
Motor boat	0.024	0.007
Tractor	0.003	0.008
Hand tractor	0.042	−0.007
Purchase value of all means of transportation/vehicle (current Riel prices)	1,410,381	0.285
Livestock per household, number of		
Cattle	1.140	−0.068
Buffaloes	1.041	−0.004
Horses, Ponies	0.009	−0.011
Pigs	0.913	−0.005
Goats	0.019	−0.003

Notes: PCA is conducted separately for each year. Means and factors weightings shown for 2004 only.

Table A2
Control variables – baseline means and changes in means by treatment status.

	Baseline 2004		Change: 2009–2004	
	HEF after 2004	No HEF	HEF after 2004	No HEF
Village level variables (%)				
Nearest district hospital ≤5 km	45.51	31.30**	1.19	10.11
Nearest district hospital [5,10 km]	20.79	22.90	−1.07	−1.74
Nearest health centre ≤2 km	48.88	37.40**	4.85	11.18+
Nearest health centre [2,5 km]	28.65	41.22**	3.23	−2.62
Has bank or loan credit union	38.76	29.01*	11.58++	14.98++
Any non-health govt. development project	37.64	29.77	-2.63	7.42
Any non-health NGO development project	29.78	25.95	0.38	−1.96
Malaria one of most important health problems	38.76	24.43***	−11.67++	−12.62++
Dengue one of most important health problems	64.04	56.49	1.22	15.64++
Any public health programme†	73.03	64.12*	7.67	23.33+++
Has private clinic, drug shop/vendor	40.45	36.64	1.21	−9.51
Household level variables				
Log of household size	1.53	1.51	−0.06+++	−0.02
# Children < 6 years	0.53	0.47***	0.05+	0.11+++
# Children 6–11 years	0.62	0.61	0.01	0.03
# Adults ≥45 years	0.36	0.40**	0.05+++	0.05++
House is rented (%)	1.54	0.38***	2.05++	0.22
Rural (%)	68.60	95.26***	4.06	−8.37+++
Household runs company/business (%)	42.04	38.29	−5.67++	−2.62
Household does fishing/aquatic farming (%)	47.34	51.04	4.37	5.70
Head of household (%)				
Male	77.83	78.08	0.46	2.75+
Log of age (in years)	3.74	3.77**	0.01	0.01
Moderately or severely disabled	7.56	9.48*	−1.41	−2.06+
Elementary occupation‡	8.62	6.82*	0.71	1.72
Unpaid family worker	3.12	3.48	−2.87+++	−3.15+++
Civil servant	11.04	12.62	−4.21+++	−7.95+++
Unemployed	10.68	12.48	−4.51+++	−4.89+++
No schooling	30.40	30.86	−4.53+	−5.74++
School grades one to five	34.27	37.20*	3.04+	1.65
School grades six to twelve	33.42	30.00*	0.77	4.72++
Not Khmer	3.69	4.74	1.05	0.81

Notes: 'No HEF' excludes villages/households in areas that obtained SUBO coverage between 2004 and 2009.
*, **, *** Indicate significant difference between HEF and No HEF areas in baseline means at 10%, 5% and 1% levels.
+, ++, +++ Indicate significant difference between 2004 and 2009 means at 10%, 5% and 1% levels.
† Programme for immunisation, mother and child health, family planning, HIV/AIDS testing, iodine deficiency/Goitre.
‡ As defined by International Standard Classification of Occupations includes (i) cleaners, (ii) agricultural, forestry and fishery labourers, (iii) labourers in mining, construction, manufacturing and transport, (iv) food preparation assistants, (v) street and related sales and service workers, (vi) refuse workers.

Our estimate of a 35% reduction in mean (positive) OOP payments may seem surprisingly large given only around 9% of households in HEF covered areas report receipt of, or entitlement to, subsidised care and HEFs cover only public health services, while households make heavy use of private care and self-medication. One explanation is that many households may be ignorant of their HEF entitlement. It is estimated that around two-fifths of hospitalisations and one-quarter of deliveries at covered hospitals are financed through HEFs (Ministry of Health, 2009b). Benefits from this substantial inflow of revenue are likely to extend beyond the households receiving the subsidy. Fee paying patients may be induced by increased stocks of supplies and better staff attendance to seek less expensive treatment from public providers rather than the more convenient but less medically effective private clinics and drug vendors.

The estimated magnitude of the effect would be overestimated if HEFs were purposively located in areas in which health expenditures were expected to decline. HEF operators have little incentive to engage in such strategic location and their ability to forecast changes in health care utilisation and expenditures is likely to be limited in any case. More probable is that locations are selected on the basis of initial conditions. This does not jeopardise the consistency of our difference-in-differences estimator. The parallel trends assumption required for identification of the effect is made more plausible not only by our ability to follow the same communes over time but also by control for changes in their observable characteristics that could be correlated with health care utilisation and expenditures. These include public health programmes and development projects, which may be established in the same locations as HEFs in order to exploit economies of agglomeration arising from administrative infrastructures. Nonetheless, we cannot completely rule out the possibility that there are other projects attracted to the HEF sites that are not documented in the data. If this is the case and if these interventions reduce health expenditures, then the estimated impact of HEFs on OOP payments will be overestimated.

We find that HEFs, which subsidise public care, reduce the probability resorting to the private sector but find no significant effect on use of public care. The latter may be attributable to limitations of the data that identify only the 'usual provider' of care. It could be that HEF coverage raises utilisation of public health care but this goes undetected since the increase is not sufficient to substantially raise the probability that the public sector is reported as the usual provider of care.

Heterogeneity is a feature of the HEFs that have been implemented in Cambodia. Given this, one must be careful in interpreting the average treatment effects presented here. They are averages over the effects of different HEF designs. Lack of data has prevented us from examining heterogeneity in relation to whether the HEF covers health centres and transport costs, and if there is pre- or post-identification of the poor. Determination of how the

effectiveness of a HEF varies with these parameters would be valuable in considering further extension of the model in Cambodia and its implementation elsewhere.

Acknowledgements

This research is part of the EU FP7 financed project entitled Health Equity and Financial Protection in Asia (HEALTH-F2-2009-223166). The authors are grateful to the Cambodian National Institute of Statistics for permission to use the data from the Cambodian Socioeconomic Surveys and to Tim Johnston (World Bank, Phnom Penh) and Maurits van Pelt for help in acquiring and interpreting information. We are grateful for comments received at presentations made at iHEA (Toronto), HSRA (Hong Kong), HEFPA workshops and the University of Lausanne.

Appendix.

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