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Universal health coverage: A (social insurance) job half done?

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ABSTRACT

Evidence on households' ability to smooth consumption over health shocks is mostly obtained from environments where there is little or no formal insurance of either medical expenses or sickness-related earnings losses. To establish whether households remain economically vulnerable to illness after the introduction of universal health coverage (UHC), we examine the impact of health shocks of different severity on informal workers in Thailand who are entitled to comprehensive public medical care but lack social protection of earnings. Using three years of panel data, we find that the most severe illness that strikes an initially healthy worker reduces household earnings by almost one third and, despite UHC, raises out-of-pocket spending on medical care by around two thirds. However, households are able to protect spending on goods other than medical care by drawing on informal insurance, credit and savings. These coping strategies substitute for the lack of formal earnings insurance and fill gaps in the effective health care coverage. On average, the combination of UHC and informal insurance of the residual risks does a reasonably good job of protecting living standards from the economic impact of illness, at least in the short term.

1. Introduction

A global campaign to extend health care coverage in the developing world was launched in the last decade (World Health Organization, 2010). This push for universal health coverage can potentially improve protection against the risk of unforeseen medical expenses, but it will do little or nothing to reduce exposure to the other major economic risk associated with illness – lost earnings. Nevertheless, if households that acquire health care coverage can cope with the earnings risk by drawing on support networks, credit and savings, then formal health insurance combined with this informal insurance of earnings may be an appropriate policy response to health risks in economies characterized by a large informal sector.

Whether the effective health care coverage in such economies provides sufficient protection against the economic risks associated with illness is currently difficult to establish because most of the existing evidence on households' ability to smooth consumption over health shocks is obtained from environments in which there is little or no formal insurance of either medical expenses or sickness-related earnings losses (Townsend, 1994; Gertler & Gruber, 2002; Asfaw & Von Braun, 2004; Wagstaff, 2007; Genoni, 2012; Islam & Maitra, 2012; Mohanan, 2013; Khan, Bedi, & Sparrow, 2015). Inability to smooth consumption over both risks does not necessarily imply incapacity to cope with one of them. This paper contributes to knowledge of the economic impact of health shocks by examining the extent to which illness continues to threaten living standards in a population that has acquired cover for health care but still completely lacks formal earnings insurance. We establish whether households with health insurance can cope with remaining risks related to sickness. If they

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cannot, then the social insurance job done by universal health coverage (UHC) is only half complete. If they can, then the value of additional formal insurance is low because it would crowd out informal mechanisms that are sufficient to cope with the residual risks.

We address this question by studying the economic impact of health shocks in Thailand, a country that took a major stride towards UHC in 2001 by extending access to reasonably comprehensive medical care benefits to the four fifths of the population not insured through formal sector employment.[1] This landmark reform has been shown to have increased access to medical care sufficiently to produce marked gains in population health (Gruber, Hendren, & Townsend, 2014; Limwattananon et al., 2015) and to have substantially reduced out-of-pocket payments for health care (Limwattananon et al., 2015). But earnings insurance in Thailand, as elsewhere, remains restricted to formal sector employees who form less than a third of the workforce. We estimate the impact of illness on earnings in the other two-thirds of the Thai workforce that is on the margins of poverty and assess the extent to which households can deploy informal insurance mechanisms to maintain expenditure on goods other than medical care despite experiencing a drop in income.

Legislating UHC does not necessarily eliminate the threat to living standards generated by medical expenditure risk. The health system may be overstretched by the attempt to cover a large fraction of the population for a wide array of health services from a very limited budget. Those entitled to health care in the public sector may choose to pay out-of-pocket (OOP) in order to avoid queues, obtain off-list medicines and access care considered to be of higher quality. In Thailand, universal coverage is estimated to have reduced OOP health payments by 28% – well short of 100% (Limwattananon et al., 2015). Our second contribution is to determine the medical expenditure risk that can remain after the introduction of UHC by estimating the extent to which OOP payments increase when illness strikes a Thai informal sector worker who, at least in principle, has cover for public health care.

We use panel data to identify informal sector workers who are struck by the onset of a new health condition and compare their earnings, OOP medical expenses and non-medical expenditures with those of other informal workers who do not experience any deterioration in health. To increase the plausibility of our main identification strategy, which relies on the assumption that the health change is exogenous conditional on the lagged value of the outcome, we select only workers who are initially healthy. We demonstrate robustness to an alternative identification strategy that assumes illness is exogenous conditional on an individual fixed effect.

Our third contribution is to estimate a dose-response relationship by discriminating between the effects of health changes of varying degrees of severity that are distinguished by the extent of any drop in reported health that occurs simultaneous to the onset of the health condition. We demonstrate that greater drops in reported health are observed for more serious conditions. We are aware of only one other study that has examined heterogeneity in the economic impact of illness by severity (Gertler & Gruber, 2002).

Evidence from low- and middle-income countries (LMICs) on the ability of households to smooth consumption in the short term over the economic impact of illness is mixed.[2] Townsend (1994) finds that households in three southern Indian villages can smooth consumption. But this may be due to use of a health measure (sickness days) that does not discriminate major from minor illness. Some later studies that examine more serious health events reject complete smoothing of consumption (Gertler & Gruber, 2002; Wagstaff, 2007; Mete et al., 2008; Gertler, Levine, & Moretti, 2009). The validity of this finding rests on the assumed exogeneity of the health measures used. Two studies with designs intended to deal with endogeneity do not reject full consumption smoothing (Genoni, 2012; Mohanan, 2013).[3] Recently, attention has shifted to how households smooth consumption over illness. If short-term shocks can be absorbed only by depleting long-term productive capacity, for example, by reducing investment in education, or if risk exposure and aversion causes households to divert valuable resources to self-insurance, then ability to keep consumption constant while health fluctuates does not imply that there are no gains to be had from social insurance (Chetty & Looney, 2006). Mohanan (2013) finds that in Karnataka (India) the accumulation of debt is the primary means of maintaining expenditures on food and housing while incurring large medical expenses resulting from health shocks. Mitra, Palmer, Mont, and Groce (2016) find that Vietnamese households experiencing ill-health and hospitalization are able to hold their non-medical expenditure constant, but they spend less on education. Liu (2016) finds a similar result for Chinese households that do not have health insurance. Those that do are able to smooth their consumption over health shocks without sacrificing their children's education.

Most of this literature examines policy environments in which there is little or no formal insurance of health risks. We contribute evidence on the extent to which poor and near-poor Thai households continue to be economically vulnerable to illness after health care coverage is universally provided. The studies of Vietnam (Mitra et al., 2016) and China (Liu, 2016) do examine households' ability to cope with the economic impact of health shocks in contexts where there is some formal health insurance. But compared with Thailand, coverage in these countries during the periods studied was either less universal across the population (Vietnam) or less comprehensive with respect to health services included in the benefit package (China). Informal sector workers in Vietnam who were insufficiently poor to qualify for a tax-financed scheme were covered only if they enrolled voluntarily. And enrolment was very low.[4] The Chinese insurance scheme studied by Liu was restricted to covering mainly inpatient treatments during the period examined.

We find that the onset of a new health condition reduces employment and hours of work. Effects rise substantially with greater drops in reported health. Consistent with previous studies using data from Indonesia (Genoni, 2012) and China (Liu, 2016), we find that reduced labor supply of the person struck by illness is partially compensated by increased work effort of other adults in the household. Fadlon and Nielsen (2018) argue that such intra-household substitution of labor indicates gains that could be obtained from (more) social insurance. Partly as a result of the reallocation of work effort within the household, on average, there is no significant impact on household earnings. However, the most severe health conditions result in a significant 27 percent drop in

[1] In 2003, two years after the UCS was introduced and two years before the beginning of our sample period (2005–07), 95 percent of the Thai population was covered by one of the country's health insurance schemes (Limwattananon et al. (2015) Fig. 1). The UCS benefit package is one of the most comprehensive in countries aspiring to reach UHC (Giedion and Andrés Alfonso (2013) Annex 3). During 2005–7, out-of-pocket payments financed just 20 percent of total health expenditure in Thailand, which is 44 percent below the average in middle-income countries even in 2014 (World Development Indicators).

[2] See Alam and Mahal (2014) for a systematic review of the literature on the economic impact of health shocks in low- and middle-income countries.

[3] Mohanan (2013) has the strongest claim to using exogenous variation in health, which comes from involvement in bus accidents. Genoni (2012) instruments health (changes) with the prices of health services, which need to be interacted with age and sex in order to obtain sufficiently strong instruments.

[4] In 2011, three years after the end of the period examined by (Mitra et al. 2016), around 36 percent of the Vietnamese population remained uninsured (Somanathan et al. 2014).

earnings. Illness raises household OOP medical spending by an estimated 42 percent, although this is from a low absolute level due to universal coverage. Nevertheless, illness burdens some households with substantial medical expenses. It increases the probability that OOP spending absorbs at least a tenth of the household budget by two percentage points, and this effect rises to nine points for the most severe conditions.

Despite the substantial rise in spending on medical care and the fall in earnings resulting from the most severe health conditions, there is no significant impact on non-medical expenditure. Thai households engaged in the informal sector of the economy appear to be able to smooth their consumption over illness. This finding cannot be attributed to a failure to capture the impact of major health problems. The estimated labor supply effect of the most severe health conditions is substantially larger than the effect of a limitation on an Activity of Daily Living (ADL) estimated by Gertler and Gruber (2002) using data from Indonesia. As with other evidence that households are able to smooth consumption over health shocks (Genoni, 2012; Islam & Maitra, 2012; Mohanan, 2013; Liu, 2016; Mitra et al., 2016), interpretation depends on how households are achieving this. In addition to labor substitution within the household, we find a substantial positive impact on non-labor income, which is mainly from remittances and informal transfers from other households. As a result of the intra-household transfer of work effort and the inter-household informal transfers of money, both of which have been found to play important roles in providing protection from the economic impact of health shocks also in Indonesia (Genoni, 2012), there is no significant impact of even the most severe health conditions on total household income. This is evidence of informal insurance through mechanisms that, other than intra-household labor substitution, are considered efficient. We provide indirect evidence that suggests informal transfers are crowded out by formal government transfers. Besides informal transfers, households cope with illness-induced earnings losses and medical expenses by cutting back on saving and by borrowing. On average, the effect on borrowing is similar in magnitude to that on non-labor income. But households experiencing the most severe health conditions, as those in Karnataka studied by Mohanan (2013), rely much more heavily on borrowing. Whether or not this is efficient depends on the terms on which households are borrowing. Access to credit through microfinance programs has been shown to increase the extent to which Indonesian households can smooth consumption over health shocks (Gertler et al., 2009) and to reduce the need to sell livestock to protect consumption when hit by illness in rural Bangladesh (Islam & Maitra, 2012). But borrowing on unfavourable terms could lead to accumulating debt and eventually the sale of productive assets. Overall, our analysis suggests that in Thailand, the combination of formal health care coverage and informal insurance of the residuals risks does a reasonably good job of protecting living standards from the economic impact of illness, at least in the short term.

Our main estimates are obtained by comparing values of an outcome, such as earnings, of individuals who are struck by illness (treatment group) with the outcome values of those who remain healthy (comparison group), after conditioning on the lagged value of the outcome. They are robust to making the treatment and comparison groups observationally equivalent in terms of the pre-illness values of the outcome and covariates by applying inverse probability weights (IPW) based on the estimated propensity score of illness, and also to combining IPW with regression adjustment. In addition, the estimates are generally robust to adoption of a difference-in-differences identification strategy that rests on the counterfactual outcome being independent of illness conditional on time invariant unobservables, rather than the lagged value of the outcome.

The next section outlines the system of social protection of health risks in Thailand and describes the data. The third section explains the identification strategies and estimators employed. Results are presented in the fourth section and the final section concludes.

2. Context and data

2.1. Social protection in Thailand

Social protection varies by employment sector. Separate schemes for public and private sector salaried employees each give entitlement to comprehensive health care benefits, as well as paid sick leave and medical retirement benefits (International Labour Organization, 2013; Social Security Administration, 2013). The two thirds of the workforce that does not get social protection through formal sector employment – mainly low-income households engaged in agriculture or small (family) businesses – has had health care coverage since 2001 through the tax-financed Universal Coverage Scheme (UCS) but does not have any publicly subsidized coverage for illness-related income loss.[5] The UCS provides reasonably comprehensive coverage for ambulatory and hospital care, and delivers medicines from a restricted list. There is no cost-sharing after the removal of a flat fee of 30 Baht (~$0.75) in 2006. Per capita funding is much less generous than that of the schemes for formal sector employees. Possibly due to long waiting times, limited facility opening hours and perceived quality deficiencies, over 40% of UCS intended beneficiaries report forgoing their entitlement and purchasing ambulatory care out-of-pocket, but only 10% do so when accessing more expensive inpatient care.[6]

2.2. Sample

Our data come from the nationally representative Thai Socio-Economic Panel Survey (SEPS) collected by the National Statistical Office. This is a smaller, less detailed but longitudinal version of the cross-sectional Socio-Economic Survey that is used to produce the official estimates of poverty (http://web.nso.go.th/en/survey/house_seco/household_main.htm). The panel is built from a stratified (by province), two-stage (village/block – household) random sample of 6000 households (21,450 individuals) drawn in 2005.[7] In 2006 and 2007, 96% and 93% respectively of the original sample of households were re-interviewed.[8]

We are interested in economic vulnerability to illness in the part of the workforce that is engaged in the informal economy, is mostly poor or on the fringes of poverty, has medical benefits coverage through the UCS and is without earnings insurance. Unfortunately, the survey does not provide information on social insurance coverage in the first two waves of the panel, and so we proxy insurance status by employment sector. Since social protection through the employment-based schemes is low among family workers, micro-enterprise employees, the self-employed and employers, we make these categories of employment the first selection crite-

[5] Since 2010, which is after our estimation period, this group receives a small (500 Baht ~$12.5) monthly pension if permanently disabled.

[6] Authors' analysis of data from the 2009 and 2011 rounds of the Thai Health and Welfare Survey.

[7] Sample weights that vary across sub-districts and make the initial sample representative of the 2005 population are not applied because our goal is to make inference about the population of informal sector workers. We control for geographic differences in all models estimated, which is sufficient to eliminate inconsistency that would arise if there is selection on the outcome indirectly through geographic variation in the sampling probability (Solon et al., 2015).

[8] Households leaving the panel tend to be younger and of higher socioeconomic status. As such, they are less likely to be in the group of interest – informal sector workers.

rion for inclusion in our analytic sample. We refer to these individuals as *informal workers*.[9] In 2007, the only year in which the insurance information is collected, 86 percent of these informal workers reported being insured through the UCS (see online Appendix Table A1). Another 10 percent reported coverage through the employment-based schemes. These are individuals who were retired or had been made redundant in the past year from the formal sector without losing their social insurance protection, as well as some micro-enterprise employees who were members of the scheme for private salaried employees. Since these individuals have more generous medical benefits, as well as sickness pay and disability-related retirement benefits, their inclusion among the group we define as informal workers is expected to downwardly bias the magnitude of our estimates of the impacts of illness on household finances in the informal sector population. About 4.5 percent of informal workers reported being uninsured in the 2007 wave. Since all Thai citizens not covered by an employment-based scheme were entitled to cover through UCS at no cost, those reporting themselves uninsured have probably not yet registered because they have not sought health care.[10]

Employment status is reported for all individuals aged 14 years and above. Since informal work activity in Thailand can start from youth and continue well beyond any pensionable age that applies to salaried employment only, we place no age restriction on the sample. The youngest person in our sample of informal sector workers is 15 and the oldest is 95; 3.9% of the sample is younger than 20 and 7.6% is older than 59.[11]

2.3. Health measure

All household members aged 14 and over are asked if they experienced a health problem over the last 12 months.[12] Those responding positively are asked to describe the condition and answers are categorized by the statistical authority into groups corresponding to broad diagnostic classifications. The respondent is not asked to report a diagnosis nor to describe only conditions that have been diagnosed by a doctor. We use the variable to identify the onset of a new health condition over the course of a year. This indicator is an advantage over some of the related literature that interprets a cross-sectional measure of reported health as a change in health (Skoufias & Quisumbing, 2005; Islam & Maitra, 2012; Sparrow et al., 2014; Wagstaff & Lindelow, 2014; Quintussi, Van de Poel, Panda, & Rutten, 2015; Bonfrer & Gustafsson-Wright, 2016). It also avoids proxying health by hospitalization (Wagstaff, 2007; Mitra et al., 2016), which is likely to be endogenous to the household's ability to pay for the treatment of illness. And it does not rely on a respondent's reported days of disability (e.g. Townsend, 1994; Dercon & Krishnan, 2000; Liu, 2016), which are endogenous to the labor supply response to a given health condition (Genoni, 2012). One further advantage is that the respondent is asked to report any health problem over the past 12 months, which is also the panel wave interval. Consequently, there will be less noise in this measure than there is in another constructed from health reported for the last four weeks that is then differenced over a wave interval of one, two or even four years (e.g. Dercon & Krishnan, 2000; Asfaw & Von Braun, 2004; Liu, 2016).

To identify the effects of illness or disability of different degrees of severity, we combine indicator of the onset of a health condition with the degree of change in self-assessed health, which is obtained from responses to the question: "Compared to other people your age, how would you describe your health state: very good, good, fair, or poor?"The limitation of both health indicators is that they are self-reported. This deficiency is common to all health measures from survey data that have been used to estimate the economic impact of ill-health (O'Donnell, Van Doorslaer, & Van Ourti, 2015) or to test whether households can smooth consumption over health shocks in LMICs (e.g. Townsend, 1994; Gertler & Gruber, 2002; Asfaw & Von Braun, 2004; Wagstaff, 2007; Genoni, 2012; Islam & Maitra, 2012; Liu, 2016; Mitra et al., 2016). Systematic differences in the reporting of health have been a major concern in the literature on the impact of health on employment in high-income countries, where disability insurance creates an incentive to withdraw from the labor force and report poor health (Bound, 1991). This is much less likely to be a source of bias here given the population of interest has no entitlement to disability insurance. We deal with any time invariant differences in the reporting of health by restricting attention to individuals who initially report themselves to be healthy and utilize only information on health changes from that level. Specifically, we restrict the sample to informal workers who initially report to have experienced no health problem in the last 12 months and rate their health as *very good* or *good*. In this estimation sample of 6469 observations, we distinguish *treatment* individuals who report the onset of a health problem at the next wave from *comparison* individuals who do not.[13] We compare outcomes at that wave across these matched groups.

Of the initially healthy informal workers, 6.8% (5.6%) are lost to follow-up in the period 2005–06 (2006–07).[14] Across the three waves of the panel, 13.6% of observations experience the onset of a health problem between consecutive waves (Table 1).[15] We distinguish between health problem onsets that coincide with: (1) a drop in self-assessed health from *very good/good* to *poor* (1.2%); (2) a drop from *very good/good* to *fair* (6.4%); and, (3) self-assessed health remaining as *very good/good* (6.0%). Incidence rates of more serious conditions, such as neurodegenerative disorders, cardiovascular disease, and gastrointestinal, hepatic and renal diseases, are highest among respondents whose self-assessed health falls from *very good/good* to *poor* (online Appendix Table A3), which suggests that this group does indeed consist of the most severe health deteriorations.[16]

[9] Employers and the self-employed are not necessarily engaged in the informal sector. They may have registered their businesses. They can voluntarily enroll in the Social Security Scheme but, as is evident from online Appendix Table A2, very few of them do.

[10] While no health insurance data are available in the 2005 and 2006 waves of the SES panel, the nationally representative Health and Welfare Survey shows that an estimated 95 percent of the Thai population was covered by a health insurance scheme as early as 2003 (Limwattananon et al., 2015).

[11] Only 1.7% is older than 69. Our estimates of the impact of ill-health on work activity and earnings reported in Table III are robust to restricting attention to illness that strikes a prime-aged (20–59 years) informal worker (online Appendix Table A12) and to exclusion of those older than 69 (Appendix Table A13). The estimated effects on labor suppy and earnings are not diluted by the inclusion of less active older individuals with lower initial earnings.

[12] If a household member is not available, then the question is answered on her/his behalf by the main household respondent. Estimates are robust to using only observations for which the health question respondent does not differ over consecutive waves (see online Appendix Table A14).

[13] Observations with no onset of a health problem but with a decline in self-assessed health are retained in the sample and so effectively form part of the comparison group. Excluding them has little effect on the results.

[14] Attrition in this sample is correlated with younger age, better education, a higher propensity to work in white collar occupations and urban residence. We do not re-weight the sample by the probability of attrition given these observable characteristics. Instead, we control for them. Provided attrition is orthogonal to the outcome conditional on controls, it will not render the estimates inconsistent (Solon et al., 2015).

[15] Of the 879 individuals who experience the onset of a health problem, 11 (1.3%) are aged less than 20, 731 (83.2%) are 20–59 and 137 (15.6%) are aged 60+.

[16] For example, 10.3% of those who revise their self-assessed health from *very good/good* to *poor* experience the onset of a neurodegenerative disorder compared with 1.5% of those who continue to report *very good/good* health. The incidence of cardiovascular disease is 9.1% among those who go from *very good/good* to *poor*, while it is 2.6% among those who continue to report *very good/good*. Almost one third of those who continue to rate their health as *very good/good* report a condition that is categorized as musculoskeletal (e.g. back pain). Little more than a fifth of those who revise their health assessment downwards to *poor* report this type of problem.

Table 1
Treatment and comparison observations in individual and household level samples.

	Individuals		Households	
	Observations	%	Observations	%
Treated				
Onset of health problem	879	13.6	773	18.5
& drop to *poor* health	77	1.2	72	1.7
& drop to *fair* health	411	6.4	373	8.9
& remain in *very good/good* health	391	6.0	349	8.4
Comparisons				
No onset of health problem	5590	86.4	3402	81.5
Total	6469	100	4175	100

Notes: The individual level sample consists of individuals who work in the informal sector, report no health problem in the last 12 months and *very good* or *good* health in one wave (2005 or 2006) and who are observed in the subsequent wave. Of the 879 onsets of a health problem, 413 occur between 2005 and 2006, and 466 occur between 2006 and 2007. These frequencies correspond to 13.3% and 13.9% of the number of observations from the respective waves. The household sample consists of households with at least one individual qualifying for the individual sample in one wave who is observed in the subsequent wave. The number of households in which there is an informal worker who experiences the onset of a health problem (773) is less than the sum of the number of households in which there is a health problem onset combined with each of the three potential changes in self-assessed health (72 + 373 + 349) because there can be multiple persons within a household who experience a health problem.

Further, incidence rates of conditions that are generally less severe or life-threatening, such as musculoskeletal problems (e.g. back pain), ear, nose and throat problems and skin disorders, are highest in the group that continues to report *very good/good* health.

As mentioned earlier, only Gertler and Gruber (2002), have previously examined heterogeneity in the economic impact of ill-health by severity. They, like Genoni (2012) and Gertler et al (2009), used self-reported limitations in Activities of Daily Living (ADL) as their main measure of health, and distinguished between less severe limitations in intermediate ADL and more severe limitations in basic ADL. A potential disadvantage of using ADL to examine the economic impact of illness is that they change in response to ageing and the gradual onset of functional problems. A change in ADL does not necessarily capture an unanticipated health shock. We also cannot be sure that the health changes we examine are unanticipated. However, by conditioning on the absence of any health problem and the reporting of *very good* or *good* health at baseline, we increase the likelihood that any health condition subsequently reported corresponds to the onset of a new illness.

Estimation of the impact of health changes on household-level outcomes is done using a sample of households that include at least one informal worker who initially reports no health problem and *very good/good* health, and is observed in the subsequent wave. Within this sample, we distinguish treatment households in which there is at least one informal worker who experiences a health problem onset from comparison households in which this does not occur.[17] Illness strikes in 773 (18.5%) of the 4175 households (Table 1).

Before the onset of illness, the treatment group is older, has more females, is less educated and is more likely to engage in agriculture (Table 2, left-hand panel).[18] We adjust for these differences.

2.4. Outcome variables

We are interested in the economic impact of illness through earnings loss and medical expenses, and in the extent to which households can cushion the impact through informal coping mechanisms. We estimate the effect on individual employment and hours of work. Those subsequently struck by illness initially work fewer hours (Table 2, left column). The impact on individual earnings cannot be estimated because household labor income from agriculture and family business, two important informal sector activities, is not disaggregated to the individual level. We estimate the impact on household labor income, which includes the value of own production that is consumed, as well as on average hours worked by all household members aged 14 years and above.[19] These effects will consist both of any direct effect on the activity of the person struck by illness and any spillover effects on other household members. At baseline, households containing an informal worker who later experienced a health problem worked longer hours than the comparison households (Table 2).

Each person aged 14 or older is asked to report the average monthly amount paid out-of-pocket for health care treatment, medicines and medical equipment over the past 12 months. Payments made for the individual's own treatment are not distinguished from payments for the treatment of others, including children. We aggregate over individuals and estimate the impact on household OOP payments.[20] The mean level of OOP payments does not differ significantly between the treatment and comparison households at baseline (Table 2).[21] The same is true for the mean share of the household budget spent on medical care.

Exercise of coping strategies to mitigate any economic impact of illness is examined by estimating effects on household non-labor income, saving and borrowing. Non-labor income is the sum of unofficial transfers, official social transfers, and incomes from assets, inheritances and lottery winnings.[22] Given our sample is engaged in the informal sector, few receive official transfers. Treatment households are more likely to have a non-labour income source at baseline, but there is no significant difference in the mean amount.

[17] If there is more than one informal worker in the household who is initially healthy, then the household can experience more than one health event. This rarely happens in the sample.

[18] See online Appendix Table A4 for baseline household level means of socio-demographic covariates.

[19] In our sample of individuals, 42% report agriculture as their main occupation and 27% are family workers. Household labor income is the sum across household members of monetary and in-kind incomes from employment, plus profits from non-agricultural business (sales value plus own consumption minus expenses) and the value of household agricultural production (self-consumed and sold) net of expenses. The respondent is asked to report average amounts per month over the past 12 months prior to the day of interview.

[20] We do find statistically significant, positive impacts of illness on OOP payments measured (inadequately) at the individual level. Results can be obtained from the corresponding author upon request.

[21] Lack of significance may be attributable to skewness. The p-value for the test of equality of log OOP payments is 0.119.

[22] As for labor incomes, the respondent is asked to report the monthly average amount received from each source of non-labor income over the 12 months prior to interview.

Table 2
Baseline means of outcomes and covariates for treatment and comparison groups.

Individual				Household			
	Onset of health problem (treatment) (1)	No onset of health problem (comparison) (2)	(1) = (2) p-value		Onset of health problem (treatment) (3)	No onset of health problem (comparison) (4)	(3) = (4) p-value
Outcomes				**Outcomes**			
Hours worked (week)	46.77	48.26	0.012	Hours worked	30.61	29.12	0.010
Covariates				Labor income (Baht)	5532	6290	0.526
Age (years)	46.85	39.40	0.000	OOP amount (Baht)	53.23	49.87	0.577
1 if male	0.469	0.564	0.000	OOP budget share	0.016	0.016	0.614
1 if urban	0.286	0.314	0.093	1 if OOP budget share > .1	0.026	0.028	0.720
Education			0.000	1 if any non-labor income	0.485	0.447	0.057
1 if ≤ primary	0.774	0.662		Non-labor income	642.3	626.7	0.889
1 if secondary	0.157	0.235		Total income	6175	6917	0.540
1 if > secondary	0.069	0.104		1 if any savings	0.678	0.671	0.716
Occupation			0.000	Savings (Baht)	893.6	911.5	0.879
1 if agriculture	0.479	0.416		1 if any borrowing	0.492	0.459	0.096
1 if blue collar	0.115	0.107		Borrowing (Baht)	1129	1027	0.543
1 if retail/trade	0.324	0.350		Non-medical expenditure	2999	3237	0.096
1 if white collar	0.082	0.127					
Employment sector			0.134				
1 if employer	0.084	0.088					
1 if self-employed	0.462	0.428					
1 if family worker	0.267	0.266					
1 if micro firm	0.187	0.218	.				
Observations	879	5590			773	3402	

Notes: Samples as defined in notes to Table 1. Cells show mean values in the wave prior to the onset of a health problem in the treatment group i.e. 2005 or 2006. p-values are for test of equality of means/proportions across groups and are obtained from chi2-test for categorical variables (education, occupation and employment sector). At household level, hours worked is the total hours worked per household member older than 14 years. All monetary amounts are per capita per month. OOP amount is household out-of-pocket spending on medical care and medicines. OOP budget share is OOP spending as proportion of household total expenditure. Borrowing is the amount borrowed in the last month. Means for household level covariates given in online Appendix Table A4. Means of outcomes and covariates by the severity of the health change are given in online Appendix Table A5.

Savings are measured by the average monthly per capita amount saved over the past year. Each household reports the amount of money borrowed formally (from commercial banks, village funds) and informally (from family, friends and loan sharks) during the past year.[23] There is no significant difference in the mean level of saving and of borrowing at baseline between treatment and comparison households, although the former group is significantly more likely to have borrowed anything at all (Table 2).[24]

In addition to the effect on total household income, which includes consumption from own production, we assess the overall impact on household welfare by estimating the effect on per capita expenditure on goods and services other than medical care. This is derived from the amount each household member over 14 years old reports spending on ten broad categories: i) food, beverages and tobacco, ii) housing, iii) clothing and personal care, iv) durables, v) education, vi) transport, vii) recreation, viii) donations to charity, ix) transfers to other households, and x) all other expenditure. For each category, the individual reports the average monthly amount over the past 12 months. We sum over categories and individuals and divide by household size. The mean of the resulting non-medical expenditure per capita is about 7% lower in the sample treatment households at baseline.

3. Identification and estimation

3.1. Identification strategy

We assume that conditional on the initial values of the outcome (Y_{it-1}) and the covariates ($\boldsymbol{X}_{it-1}$) (plus region-specific period effects, δ_{rt}), the onset of illness is not correlated with the counterfactual outcome that would have materialized in the absence of illness (Y_{0it}). Specifically, we identify the average effect of illness on those who succumb to illness under the following conditional independence assumption (Angrist & Pischke, 2008; Imbens & Wooldridge, 2009),

$$E[Y_{0it}|Y_{it-1}, \boldsymbol{X}_{it-1}, \delta_{rt}, Illness_{it}] = E[Y_{0it}|Y_{it-1}, \boldsymbol{X}_{it-1}, \delta_{rt}] \quad (1)$$

where $Illness_{it}$ is an indicator equal to 1 if a health problem is reported in period t. Since the sample is selected to be free of a health problem and to report health as *very good/good* in the previous period, this indicator identifies the onset of ill-health in a sample that is exactly matched between treatment and comparison observations on indicators of initial good health (as well as informal sector employment).

Assumption (1) requires, for example, that for a given level of income in the initial period prior to anyone in the treatment group becoming ill (and given initial values of covariates), the income that would have been earned in the next period in the absence of illness would not have differed between those who actually experience illness and those who do not. Conditioning on the lagged value of the outcome weakens the unconfoundedness assumption relative to an approach that controls only for observable covariates. We need to assume that onset of illness is random across individuals/households that are initially comparable in terms of hours of work, incomes or medical expenses but not across individuals/-households that initially differ in these characteristics.

Exact matching in initial good health also helps weaken the identification assumption. Not doing this would leave a greater risk of comparing individuals whose health and economic outcomes were both trending downward over time – due to low prior investment in both health and human capital, for example – with others following more favourable trends. The lack of formal disability

[23] Monthly average savings and annual borrowing are reported on the individual level. We aggregate to the household level. The annual amount borrowed is divided by 12 for comparability with the monthly averages reported for the other financial variables.

[24] The log amount borrowed does differ significantly.

insurance makes reverse causality less of a threat to identification. There is no incentive for individuals losing employment or experiencing a decline in earnings to report ill-health in order to qualify for disability insurance. Universal health insurance that is not contingent on employment, along with near negligible enrolment in private health insurance (online Appendix Table A1), also reduces the risk of simultaneity bias. Those losing employment or falling on hard times do not lose insurance coverage and subsequently good health due to reduced access to health care.

Individuals may remember and report only the episodes of illness that had a significant economic impact through lost earnings or medical expenses. If this happens and it is due to heterogeneity in the severity of illness, then we identify the average effect of non-trivial illnesses. But it could be that the economic impact of a given severity of illness varies due to circumstances or luck, and only episodes that have the greater impact are reported. In that case, one might anticipate that the estimated effect of *reported* illness will provide an overestimate of the average effect over all those who actually incur illness. However, individuals with non-reported illness would then be included in the comparison group and this would affect the estimate of the counterfactual. Mean employment and earnings would be lower, and medical expenses higher, in the observed comparison group than in the true comparison group consisting exclusively of individuals who do not incur illness. This would bias the estimate downward. So, if this type of endogenous reporting were present, then it would result in two biases that work against each other.

3.2. Estimation

Our main estimator of the causal effect of illness involves specifying a linear model for the actual outcome,

$$Y_{it} = \alpha + \beta Illness_{it} + \theta Y_{it-1} + \boldsymbol{X}_{it-1}\boldsymbol{\gamma} + \boldsymbol{\delta}_{rt} + \varepsilon_{it}. \quad (2)$$

Under the unconfoundedness assumption (1), β corresponds to the average effect of illness on those who become ill. If the regressors are uncorrelated with the error (ε_{it}), then ordinary least squares (OLS) is a consistent estimator of this parameter.[25] We allow for arbitrary correlation of errors within the household by clustering all standard errors at the household level. All continuous outcomes are estimated in logs, with the exception of the medical expenditure budget share.[26]

For employment and hours of work, the unit of analysis is the individual.[27] For the other outcomes, the model is estimated at the household level and in that case $Illness_{it}$ indicates whether there is an informal worker in the household who experiences a health problem and was previously healthy.[28]

In the individual level models, the covariates are those listed in the left-hand panel of Table 2, while in the household level models, the covariates are those listed in (online) Appendix Table A4. One exception is that demographics are controlled for in the individual level analysis by 10 age group × gender indicators and in the household level analysis by the share of household members in 14 age group × gender categories. The region-period effects ($\boldsymbol{\delta}_{rt}$) consist of a set of region indicators, a period dummy and all interactions.[29]

We also estimate models in which the single illness indicator is replaced by three indicators of the onset of a health problem in combination with varying degrees of change in self-assessed health, as described in Section 2.

We examine robustness of the estimated effect of any health problem to using the inverse probability weighting (IPW) estimator based on the estimated propensity score of illness (Rosenbaum, 1987; Imbens, 2004).[30] This makes treatment and comparison observations equivalent in terms of pre-illness values of the outcome and covariates without imposing the regression model restriction on the conditional expectation of the outcome. In addition, we apply the inverse probability weights in least squares estimation of the regression (2) to obtain estimates from a doubly robust estimator (Robins & Rotnitzky, 1995).[31]

3.3. Alternative identification strategy

Although the institutional context and our exploitation of the longitudinal dimension of the data make unconfoundedness more plausible, it does remain an untestable assumption that may be doubted. We therefore examine robustness of the findings to adoption of an alternative difference-in-differences (DID) identification strategy. This rests on the assumption (also untestable) that the change in the (log transformed) outcome under the counterfactual of no illness would have been equal across treatment and comparison observations. Equivalently, the expectation of the counterfactual outcome is assumed independent of illness conditional on an individual (household) fixed effect (λ_i) (Angrist & Pischke, 2008; Imbens & Wooldridge, 2009),

$$E[Y_{0it}|\lambda_i, \boldsymbol{X}_{it}, \boldsymbol{\delta}_{rt}, Illness_{it}] = E[Y_{0it}|\lambda_i, \boldsymbol{X}_{it}, \boldsymbol{\delta}_{rt}]. \quad (3)$$

This is clearly a different assumption from (1). It is not weaker, nor stronger. Plausibility of the common trends assumption (3) may be doubted given differences in some outcomes and covariates at baseline. Making the groups comparable at baseline, as is done by conditioning on lagged values in (2), arguably gives greater credibility to a causal interpretation of the difference in outcomes (Imbens & Wooldridge, 2009). Further potential advantages of the lagged dependent variable (LDV) approach is that identification is less dependent than DID on functional form (Lechner, 2011)[32] and it is more efficient when autocorrelation is low (McKenzie, 2012), as would be expected for medical expenditures and possibly also for informal sector incomes. However, since it is

[25] Note that it is not necessary to assume that the errors are serially uncorrelated for OLS to be consistent in the presence of the lagged dependent variable (LDV) (Wooldridge, 2003, pp. 378–9). If the errors were to follow an AR(1) process, then OLS would be inconsistent and the parameter of interest is likely to be underestimated. But there is no reason why AR(1) is a plausible process for the error, particularly once control is made for the LDV (ibid).

[26] Zero and negative values are replaced by the log of the minimum positive value, minus 0.01. All results are robust to replacement by 0 rather than this value.

[27] Since the sample is restricted to informal sectors workers, the lagged dependent variable is obviously omitted in the model of employment since all observations are initially employed.

[28] Hours of work are estimated at both the individual and household levels.

[29] That is, $\delta_{rt} = \mathbf{R}_i\boldsymbol{\rho} + \tau T_t + (T_t \times \mathbf{R}_i)\boldsymbol{\varphi}$, where $\mathbf{R}_i$ is a vector of four indicators that discriminate between five regions, $T_t = 1$ indicates year 2007 and the final interaction term allows for regional differences in the period effect. Results are generally robust to replacing the region indicators with 76 province indicators, without allowing for different period effects by province.

[30] A weight equal to $w_i/\bar{w}_C$ is given to each comparison observation, where $w_i = \hat{p}(Y_{it-1}, \mathbf{X}_{t-1})/1 - \hat{p}(Y_{it-1}, \mathbf{X}_{t-1})$, $\hat{p}(Y_{it-1}, \mathbf{X}_{it-1})$ is the probit estimated propensity score of experiencing the onset of a health problem, $\bar{w}_C = \frac{1}{N_C}\sum_i(1 - Illness_{it})w_i$ and $N_C = \sum_i(1 - Illness_{it})$. Treated observations are given a weight of 1. The estimator is the treated-comparison group difference in weighted means. Pair cluster (on household) bootstrap standard errors are obtained using 500 replications. We use the standard deviation of the 500 bootstrap estimates.

[31] A consistent estimator is obtained if either the propensity score model for illness or the regression model for the outcome, but not necessarily both, is correctly specified. Pair cluster bootstrap standard errors are calculated. We bootstrap the whole procedure: i) draw random sample of households from data; ii) run probit to estimate the propensity score; iii) compute weights; iv) run weighted least squares regression; v) repeat.

[32] The DID common trends assumption is made with respect to the transformation that is used for the outcome. The lagged dependent variable approach assumes unconfoundedness conditional on the lagged outcome, and this holds for all transformations of the outcome.

impossible to know if either identification strategy is valid, following the advice of Angrist and Pischke (2008), we check whether the estimates are broadly consistent across the approaches.

We implement the DID approach by taking first differences to remove time invariant unobservables, which are assumed away in the LDV approach, and estimating the following model by OLS,

$$\Delta Y_{it} = \mu + \eta Illness_{it} + \Delta \boldsymbol{X}_{it}\boldsymbol{\varphi} + \xi T_t + (T_t \times \boldsymbol{R}_i)\boldsymbol{\psi} + \upsilon_{it} \quad (4)$$

where Δ is the first difference operator, T_t and R_i are year and region indicators respectively, as defined in footnote 34. As with the LDV approach, we restrict the sample to individuals who are healthy at baseline. Consequently, $Illness_{it}$ corresponds to a first difference since it is initially zero for all observations.[33] Given that health shocks potentially impact labour market status, the employment-related covariates, i.e. occupation and employment sector, and the number of workers in the household, are excluded from $\boldsymbol{X}_{it}$.

4. Results

Estimates from the LDV approach implemented by the least squares regression (2) are given in the Tables presented in this section. Estimates obtained from the alternative estimators and identification strategy are given in the online Appendix (Tables A7, A9–A11). Estimates obtained using both identification strategies without conditioning on covariates are presented in online Appendix Table A16.[34]

4.1. Effects on work and earnings

The onset of a health condition is estimated to reduce the probability that an informal worker remains in employment by just less than four percentage points and to reduce hours worked per week by 13 percent (Table 3, top row - all (semi-) elasticities quoted in the text are calculated using $\exp\left(\widehat{\beta}\right) - 1$). Considering that all individuals were initially in employment, these are not particularly large effects. There is, however, substantial heterogeneity by the severity of illness. For those who report a decline from *very good/good* to *poor* health in conjunction with the onset of a health condition, we estimate that the probability of continued employment is reduced by 18 points and the hours coefficient of −0.614 corresponds to a 46 percent decrease in hours worked (Table 3, 2nd row). The estimated effects are substantially smaller for those who report a fall in health to *fair* and there is no statistically significant impact on those who continue to report *very good/good* health. All estimated effects differ significantly across the groups distinguished by the change in self-assessed health with one exception (Table 3, bottom panel). This dose-response relationship lends face validity to the estimates.

At the household level, total hours worked per working aged (14+) person are estimated to be reduced by 5.4% as a consequence of an informal worker falling ill. The fact that this is substantially smaller than the relative effect on the hours worked by the person becoming ill does not necessarily imply that other household members compensate for the lost work of the sick person. The *relative* effect would be smaller at the household level if others were initially working and did not change their labor supply. Retransforming the estimates from the log to the raw scale by applying Duan's smearing estimator (Duan, 1983), gives a highly significant 6.49 (SE = 2.30) reduction in hours worked by the person succumbing to illness compared with an estimated 1.55 (SE = 0.972) fall in total hours worked by all household members.[35] Although these estimates are not significantly different at the 5% level, the substantially lower point estimate of the impact on household hours is at least consistent with other household members partly compensating for the lost work effort of the sick person. Perhaps as a result, there is no significant impact on household labor income despite the reduced work activity of the person struck by illness.[36]

Again, the estimated effects of a health problem in combination with a drop to *poor* health are substantially larger. Household work hours are estimated to be reduced by 18% (exp(−0.193)-1) and labor income is estimated to decrease by 27%. This more severe health event does appear to result in a very substantial loss of household earnings.

Overall, the findings with respect to the impacts of ill-health on labor supply and earnings are robust to the estimator and identification strategy.[37] Estimates of the effects of any health problem on individual employment and work hours obtained from the inverse probability weight estimator without and with regression adjustment are very similar to the unweighted OLS estimates (see online Appendix Table A7). Estimates of the impact on household work hours are also similar in magnitude, but become insignificant with the IPW and doubly robust estimators. For household labour income, we obtain somewhat larger, but still not significant reductions using the IPW estimators.

Adoption of a difference-in-differences identification strategy implemented by OLS estimation of (4) produces estimated effects on individual labor supply and household work hours and earnings that are stronger in magnitude and significance than the main estimates (online Table A7). According to the DID estimates, and inconsistent with the main estimates, it is a health problem that is not accompanied by any deterioration in self-assessed health that has the strongest negative impact on household labor income. With this exception, the DID estimates also show the dose-response relationship one would expect if the health measure is distinguishing between degrees of severity.

Using data from Indonesia in the early 1990s and a DID strategy, Gertler and Gruber (2002) estimate that experiencing any illness symptoms reduces employment by 3 percentage points, which is similar to our estimate of a 4-point reduction following the onset of a health problem. Consistent with our finding that the employ-

[33] While there are good reasons (given above) for identifying the effect of a health change only from comparisons between people who were originally in the same state of health, one may be concerned about potential selection bias arising from restricting the sample to those initially in good health. Therefore, we have checked robustness to not restricting the sample in this way and estimating a fixed effects (FE) regression of each outcome on the health problem indicator (interacted with an indicator of each of the three categories of self-assessed health), plus time varying covariates. Results are available from the authors on request. In general, they display a good deal of consistency in sign and significance, and often also in terms of magnitude, with the LDV estimates presented in the main text and the DID estimates in the Appendix. Exceptions are that the FE estimates of the labor supply and labor income effects are generally smaller in magnitude, and FE gives a rather implausible positive and significant effect of ill-health on non-medical expenditure.

[34] If estimates change a lot when covariates are controlled for, then one may worry that they would change further were it possible to control for yet more observables, or if a different specification from the available covariate set was selected. With the exception of the LDV estimated effects on labor supply and labor income outcomes (see footnote 37), this is generally not the case.

[35] The Duan (1983) smearing estimator is consistent under non-normality of the error but not heteroscedasticity. With a large number of covariates, some of which are continuous, it is not feasible to take account of heteroscedasticity. Pair cluster bootstrap standard errors with 500 replications are used.

[36] Almost half (47%) of illness episodes are experienced by the head of household. The point estimate of the impact on individual labour supply is smaller when it is the head of household that falls ill (see online Appendix Table A6) but the difference is not significant. In fact, for no outcome do we find a significant difference between the effect of illness of the head of household and of someone else.

[37] Generally, the magnitudes of the estimates are substantially larger without conditioning on covariates (see online Appendix Table A16). Since the controls include occupation, employment type, education and region, they should do a good job of characterizing the labor market within which an informal worker operates. Therefore, adding more controls is unlikely to markedly reduce the estimates further. In any case, the estimated effects with these controls are not implausibly large.

Table 3
Estimated effects of the onset of a health problem on labor supply and labor income.

	Individual		*Household*	
	P(works)	Log working hours	Log working hours per member > 14	Log labor income per capita
Onset of a health problem	−0.037***	−0.139***	−0.054*	−0.059
	(0.012)	(0.049)	(0.032)	(0.050)
Onset of a health problem +				
drop to *poor* health	−0.178***	−0.614***	−0.193*	−0.311*
	(0.051)	(0.195)	(0.111)	(0.174)
drop to *fair* health	−0.039**	−0.151**	−0.070	−0.052
	(0.018)	(0.071)	(0.047)	(0.071)
remain in *very good/good* health	−0.007	−0.034	−0.016	−0.024
	(0.016)	(0.064)	(0.040)	(0.070)
p-values for tests of equality of effects				
drop to *poor* = drop to *fair* health	0.020	0.025	0.307	0.170
drop to *poor* = remain in *very good/good* health	0.002	0.005	0.132	0.135
drop to *fair* = remain in *very good/good* health	0.081	0.204	0.368	0.766
N	*6469*	*6469*	*4175*	*4175*

Notes OLS estimates of specifications akin to Eq. (2). No lagged dependent variable in models of employment since all are initially employed. All models control for covariates listed in Table 2 (individual) and online Appendix Table A4 (household). Demographics controls are ten age group × gender indicators (individual) and 14 age group × gender category shares (household). Region effects, a period effect and their interaction are included. Samples as defined in notes to Table 1. Treated households are those in which at least one informal worker experiences the onset of a health problem. Coefficients in the middle panel indicate the effect of a health problem combined with each change in self-assessed health. Standard errors clustered at household level in parentheses. Coefficients on the lagged dependent variable are given in online Appendix Table A15. ***,**,* indicate statistical significance at 1, 5 and 10% levels.

ment effect increases with illness severity, Gertler and Gruber estimate that the inability to perform one ADL reduces the employment probability by 6.7 points, which is only a third the size of our estimate of the employment impact of experiencing a health problem combined with self-reported health deteriorating from the top to the bottom category. Our estimated effects on labor supply at the extensive margin are even larger in comparison with those of Gertler and Gruber.[38]

4.2. Effects on medical expenses

We estimate that an informal worker suffering a health problem results, on average, in a 71% (exp(0.537)-1) increase in household per capita OOP spending on medical care and medicines (Table 4, top row).[39] While this would appear to be a very large increase, the initial mean level of medical spending by the treatment group is rather modest at 200 Baht (∼$5.00) per household per month. We estimate that the onset of illness raises the share of total household expenditure spent on medical care by 0.6 percentage points, or 38% compared to the baseline mean of 1.6 percent.[40] Initially, 2.6 percent of households in the treatment group were spending more than 10% of their budgets on medical care. We estimate that illness raises the percentage incurring these so-called *catastrophic health payments* (Wagstaff & van Doorslaer, 2003) by 2.1 points, a substantial increase.[41]

The effects on medical spending increase with the severity of illness as proxied by the degree of change in self-assessed health, although not always significantly so (Table 4, middle panel). The amount paid is estimated to be raised by almost 100% by a health condition that coincides with a drop to *poor* health. The same severe health event is estimated to raise the share of the household budget spent on medical care by 1.8 percentage points from a base of only 0.8 percent and to raise the probability that this share exceeds 10% by around 9 percentage points from a baseline of less than 1 percent (online Appendix Table A5).

The estimated effects presented in Table 4 are very robust to estimation by inverse probability weights without and with regression adjustment, and also to adoption of a DID identification strategy with OLS estimation of the first difference regression (4) (see online Table A9). One notable difference is that the gradient in the estimated effect by the severity of the health deterioration is even stronger for the first difference estimates.

4.3. Coping with the economic impact of illness

Illness of an informal worker appears to reduce work activity and earnings, and increases medical expenses, with the effects of more severe illness being substantial. Can households deploy risk coping strategies to cushion the short-term impact on wellbeing of these economic costs of ill-health? The point estimate of the impact on the probability of saving and on the (log) amount saved are both negative but neither is significant (Table 5). Note that the effects on all amounts in Tables V and VI are not conditional on the amount being positive. Being struck by illness is estimated to raise the probability of borrowing by 5.6 percentage points and to raise the amount borrowed by a third (exp(0.288)-1) (Table 5). Households appear to partly absorb health shocks by accumulating debt and possibly also by cutting back on savings.

Onset of a health problem is estimated to raise the probability that the household has a non-labor source of income by 4.2 percentage points and to raise the amount received by 31 percent (Table 5, top row, far right column). As is clear from the top panel of Table 6, this is driven by increases in unofficial transfers from relatives and friends, including remittances, and not by official transfers paid by the government, a pension scheme or an employer in compensation for work-related injury/disability or

[38] They do not find any significant effect of symptoms on hours worked but estimate that going from no ADL restrictions to having one ADL reduces work hours by 7.6 percent. We estimate that the onset of any health problem reduces hours by 13 percent, on average, rising to 46 percent when combined with a fall to poor self-reported health.

[39] The point estimate is larger for illness of the head of household (online Appendix Table A6). While this is consistent with greater investment to restore the health of the family head, the difference is not significant.

[40] A 30 Baht (∼$0.75) copay levied on non-poor UCS beneficiaries for each care episode was abolished in 2006. Neither separate analysis of 2005 (copay) and 2006 (no copay) sub-samples, nor interaction of the health problem onset indicator with a year indicator suggests that the copay removal had an impact on the relative or absolute OOP health expenditure effect of a health shock. Results are available on request.

[41] The estimated effects on all three OOP expenditure variables are robust to censoring the OOP distribution at the 99th and 98th percentiles of the distribution to allow for what may be outlier values (see online Appendix Table A8).

Table 4
Estimated effects of the onset of a health problem on household out-of-pocket medical expenditure (OOP).

	Log OOP per capita	OOP/total expenditure	P(OOP/total expenditure > 0.1)
Onset of a health problem	0.537***	0.006***	0.021**
	(0.080)	(0.002)	(0.008)
Onset of a health problem +			
drop to *poor* health	0.684***	0.018***	0.091**
	(0.265)	(0.007)	(0.037)
drop to *fair* health	0.547***	0.007***	0.036***
	(0.108)	(0.002)	(0.013)
remain in *very good/good* health	0.485***	0.003	−0.003
	(0.110)	(0.003)	(0.009)
p-values for tests of equality of effects			
drop to *poor* = drop to *fair* health	0.628	0.114	0.144
drop to *poor* = remain in *very good/good* health	0.486	0.033	0.012
drop to *fair* = remain in *very good/good* health	0.673	0.178	0.008
N	*4175*	*4175*	*4175*

Notes: OLS estimates of specifications akin to Eq. (2). Covariates and controls as described notes to Table 3. Dependent variable in middle column is the share of OOP medical spending in total household expenditure. Dependent variable in right-hand column is an indicator of whether this share exceeds one tenth. Samples as defined in notes to Table 1. Treated households are those in which at least one informal worker experiences the onset of a health problem. Standard errors clustered at household level in parentheses. Coefficients on the lagged dependent variable are given in online Appendix Table A15. ***,** indicate statistical significance at 1% and 5% levels.

redundancy. Illness raises the probability of receiving an unofficial inter-household transfer by three percentage points and increases the amount received by slightly less than a third (exp(0.217)-1).

In contrast, when salaried workers fall sick, they are protected through official transfers. The bottom panel of Table 6 shows estimates of the impact of ill-health on transfer income received by households with at least one formal sector worker approximated by those employed by the public sector or a medium/large private enterprise. The majority, but not all, of these employees have social insurance that compensates for sickness- and disability-related earnings losses.[42] Adopting the same empirical strategy used to estimate effects in the sample of informal workers, we find that when an initially healthy formal sector worker experiences the onset of a health problem, the probability of receiving an official income transfer from the government, pension scheme or employer rises by almost 7 percentage points and the amount received rises by 48% (exp(0.391)-1).[43] More noteworthy is the lack of any increase in the likelihood or amount of unofficial transfers received from other households. This difference across the employment sectors in the source of transfer income received when a worker becomes sick suggests that informal insurance – financial support from friends and relatives – partially fills the gap left by the absence of formal insurance of earnings. Another way of looking at this is that the social insurance crowds out the informal insurance.

It would appear that receipt of unofficial transfers is sufficient to offset the 6 percent decline in labor income (not significant) with the result that there is no significant impact of illness on total household income (Table 7, top row). Drawing on savings and borrowing, in addition to intra-household labor substitution and inter-household informal transfers, enable households to hold expenditure on non-medical goods and services constant (Table 7, top row) despite the 71% increase in mean spending on medical care, corresponding to a 0.6 percentage point increase in the share of the household budget absorbed by medical care. It bears emphasis that these are average effects. It does not necessarily follow that households that incur very high medical expenses as a consequence of illness or injury are able to maintain the same level of spending on other goods. In particular, the estimated 2.1 percent of households induced to spend more than 10% of their budgets on medical care may well have to sacrifice consumption, as well as reducing savings, resorting to borrowing and receiving financial support from acquaintances.

The estimated effects of a health problem accompanied by a decline in self-assessed health to *poor* are generally larger in magnitude (Tables 5 and 7, 2nd row). The negative point estimate of a 7.5 percent (exp(−0.078)-1) reduction in the total income of a household affected by the most serious illness is not significantly different from zero but this may be attributable to imprecision arising from the relatively small number of households (77) experiencing such a steep decline in health.[44] We also could not reject the hypothesis of a substantial one-fifth reduction in total income at the 5% level of significance. Households struck by severe illness may be unable to fully replace the 27% loss of labor income with remittances and other transfers.[45] The monthly savings of these households are reduced by 49% and their borrowing is raised by 92% as a result of severe illness.[46] This heavy resort to debt, as well as the cut back on savings and possibly other coping mechanisms, helps to cushion non-medical spending from the 98% increase in mean spending on medical care, corresponding to a 1.8 percentage point increase in the budget share. The point estimate indicates a 2.5% reduction in non-medical expenditure. Although this estimate is not significantly different from zero, neither could we reject the hypothesis of a 13% cut in non-medical expenditure at the 5% signif-

[42] Employees of the public sector and medium/large private enterprises are the closest approximation we can make to those with employment-based social insurance covering earnings losses. But the approximation is not perfect. The 2007 survey reveals that more than 30% of public sector workers report not to be covered by the civil servants' insurance program and around 20% of those engaged in medium/large private enterprises report that they do not belong to the scheme for private sector salaried employees (see online Appendix Table A2). Nonetheless, social insurance of income losses should certainly be much more widespread in this approximate sample of formal sector workers than it is the sample of informal workers. We again restrict attention to workers who are observed initially to be healthy (i.e. no health problem and *very good/good* reported health), construct the sample as for informal sector workers and estimate the impact of the onset of a health problem.

[43] Estimated effects on work activity are very close to those we estimate for informal sector workers. A health problem is estimated to reduce the probability of being in work by a significant 3.6 percentage points (standard error (SE) = 1.6) and to reduce work hours by a significant 11.7% (coefficient on log hours = −0.125, SE = 0.062). The respective estimates for informal workers are 3.7 points and 13% (Table 3).

[44] Lack of power, in combination with the skewed nature of distributions of some of the outcomes, may also explain why few of the estimated effects differ significantly across the three sub-treatment groups.

[45] For this group, the point estimates indicate increases of 26% and 23% in income received from unofficial and official transfers respectively but neither is remotely significantly different from zero. Again, the lack of significance may be attributable to low power.

[46] The borrowing increase may partly be through uptake of social credit that offers a no-interest loan of up to 40,000 Baht (~1250 US$) to the permanently disabled (Ministry of Labour, 2012).

Table 5
Estimated effects of the onset of a health problem on risk coping strategies.

	Monthly savings		Borrowing		Non-labor income	
	P(any)	Log per capita amount	P(any)	Log per capita amount	P(any)	Log per capita amount
Onset of a health problem	−0.011	−0.115	0.056***	0.288***	0.042**	0.271**
	(0.017)	(0.103)	(0.018)	(0.100)	(0.018)	(0.115)
Onset of a health problem +						
drop to *poor* health	−0.086*	−0.682**	0.174***	0.654**	0.036	0.303
	(0.052)	(0.310)	(0.054)	(0.279)	(0.055)	(0.333)
drop to *fair* health	−0.002	−0.135	0.019	0.081	0.032	0.175
	(0.024)	(0.137)	(0.025)	(0.130)	(0.025)	(0.151)
remain in *very good/good* health	0.002	0.080	0.068***	0.406***	0.053**	0.388**
	(0.023)	(0.145)	(0.025)	(0.145)	(0.026)	(0.166)
p-values for tests of equality of effects						
drop to *poor* = drop to *fair* health	0.137	0.101	0.008	0.060	0.982	0.675
drop to *poor* = remain in *very good/good* health	0.119	0.025	0.074	0.431	0.519	0.646
drop to *fair* = remain in *very good/good* health	0.904	0.261	0.152	0.084	0.280	0.145
N	*4175*	*4175*	*4175*	*4175*	*4175*	*4175*

Notes: OLS estimates of specifications akin to Eq. (2). Covariates and controls as described in notes to Table 3. For each outcome, the left-hand column gives the estimated effect on the probability of a positive value. Right-hand column gives effect on log of amount (zero amounts given log of the minimum positive value, minus 0.01). Samples as defined in notes to Table 1. Treated households are those in which at least one informal worker experiences the onset of a health problem. Standard errors clustered at household level in parentheses. Coefficients on the lagged dependent variable are given in online Appendix Table A15. ***,**,* indicate statistical significance at 1, 5 and 10% levels.

Table 6
Estimated effects of the onset of a health problem on income transfers.

	Unofficial transfers/remittances from other households		Official transfers from government, pension schemes & employer	
	P(any)	Log per capita amount	P(any)	Log per capita amount
Informal sector workers	0.029*	0.217**	0.001	−0.006
	(0.017)	(0.109)	(0.012)	(0.062)
N	*4175*	*4175*	*4175*	*4175*
Formal sector workers	0.001	−0.006	0.067***	0.391***
	(0.012)	(0.062)	(0.022)	(0.141)
N	*2214*	*2214*	*2214*	*2214*

Notes: Formal workers are employees of the public sector or medium/large private enterprises. Both samples are households with at least one worker who reports no health problem in the last 12 months and *very good* or *good* health in one wave and who is observed in the subsequent wave. Treated households are those in which at least one initially healthy worker experiences the onset of a health problem. Notes to Table 5 that do not concern definition of the sample apply.

Table 7
Estimated effects of the onset of a health problem on household total income and non-medical expenditure.

	Log per capita total income	Log per capita non-medical expenditure
Onset of a health problem	0.009	0.020
	(0.028)	(0.020)
Onset of a health problem +		
drop to *poor* health	−0.078	−0.025
	(0.083)	(0.059)
drop to *fair* health	−0.003	−0.000
	(0.039)	(0.027)
remain in *very good/good* health	0.048	0.054**
	(0.037)	(0.027)
p-values for tests of equality of effects		
drop to *poor* = drop to *fair* health	0.999	0.940
drop to *poor* = remain in *very good/good* health	0.484	0.788
drop to *fair* = remain in *very good/good* health	0.246	0.540
N	*4175*	*4175*

Notes: OLS estimates of specifications akin to (2). Covariates and controls as described in notes to Table 3. Samples as defined in notes to Table 1. Treated households are those in which at least one informal worker experiences the onset of a health problem. Standard errors clustered at household level in parentheses. Coefficients on the lagged dependent variable are given in online Appendix Table A15. ** indicates statistical significance at 5% level.

icance level. Informal sector households may well be considerably underinsured against the economic impact of more severe illness.

The estimated effects of any health problem presented in Tables 5 and 7 are very robust to estimation using inverse probability weights both exclusively and with regression adjustment (online Appendix Tables A10 and A11). Relying on the common trends assumption and estimating by OLS in first differences (4) also gives similar results, although the estimated effects on risk coping strategies, particularly borrowing, are somewhat smaller in magnitude and less precise (online Appendix Tables A10 and A11).

5. Conclusion

Our analysis reveals that substantial economic risks associated with illness remain even after universal health coverage is put in place in a middle-income country. These risks do not only take the form of earnings losses that remain (formally) uninsured. There is also residual medical expenditure risk. This does not amount to failure of universal coverage in Thailand. Previous research has demonstrated that the extension of coverage to the informal sector greatly reduced out-of-pocket spending, particularly at the top of the distribution, resulting in a substantial welfare gain from less exposure to medical expenditure risk (Limwattananon et al., 2015). In the absence of universal coverage, informal workers struck by illness would have incurred markedly higher medical expenses. But our estimates make clear that any attempt at full population coverage on a tight budget – public health expenditure was around 2.5% of GDP in Thailand during our estimation period – with minimal or no copayments is likely to result in gaps in effective coverage.

The strength of the case for deepening health care coverage to reduce OOP spending further depends on how potential gains from reduced risk exposure compare with losses from moral hazard. The welfare argument for at least some insurance of sickness-related earnings losses is more straightforward. But as long as a very large proportion of the workforce remains in the informal sector, it will be difficult to compensate for earnings losses. To an extent, our results suggest that the welfare loss from this impasse may not be inordinate. We find that households can call on informal insurance and risk coping strategies to maintain their expenditure on other goods when illness, even serious illness, causes them to lose earnings and spend substantially more on medical care. Social insurance of earnings, if it were feasible, would crowd out this informal insurance. Hence, formal non-contributory health insurance financed by general revenues combined with informal insurance of the residual risks would appear to be reasonably effective in protecting living standards from the economic impact of illness. This hybrid represents a realistic target for the initial development of social protection against health risks in countries with a large informal labour market that makes comprehensive social insurance infeasible.

This conclusion is subject to four important caveats that stem from limitations of the data analyzed. They provide a warning against being overly sanguine about households' ability to cope with the economic impact of illness and so understating the welfare gains that would be generated from the extension of social insurance, if it were feasible. First, while we have designed the study to weaken the identification assumption as much as is feasible and demonstrated robustness to an alternative assumption, in the absence of a credible and strong instrument for ill-health, we are not able to correct any bias that may arise from measurement error in health changes or time varying unobservables correlated with those changes. This potential for bias, which exists in almost all estimates of the economic impact of health shocks, needs to be kept in mind when interpreting our estimates. Second, estimate imprecision arising from the relatively small number of severe illness cases observed means we are not able to rule out a reduction of up to 13% in non-medical expenditure due such illness. Third, the sample size also constrains the extent to which we can examine heterogeneity of the effects. While, on average, households can marshal support networks, draw on savings and borrow to protect their consumption when hit by illness, some households with less extensive social support networks will be much less able to cope. For these households, formal social insurance of earnings and more effective formal health insurance would crowd out informal insurance to a much lesser degree and so would raise welfare to a greater extent. Fourth, the short length of the panel restricts the analysis to the impact in the period that illness strikes. If a health condition is chronic, which is increasingly the case in middle-income countries such as Thailand, then savings, remittances and borrowing will be insufficient to preserve living standards in the long run. Particularly if households absorb the short run impact of such illness by running down savings, which leaves them vulnerable to other economic shocks, and by resorting to high-interest borrowing resulting in rapid accumulation of debt, then there are likely to be substantial welfare gains from further extension of social protection against the risk of illness that permanently reduces earnings and raises medical expenses (Kowalski, 2015).

Conflict of interest statement

None of the study authors have any conflicts of interest regarding the submitted study.

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Appendix A. Supplementary data

Supplementary data associated with this article can be found, in the online version, at https://doi.org/10.1016/j.worlddev.2018.09.004.

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