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RESEARCH ARTICLE

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Re-examining the effects of public health insurance: The case of nonpoor children in Vietnam

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Abstract

This paper focuses on the effects of a 2005 health insurance reform in Vietnam. Through this reform, public health insurance was newly offered to nonpoor children under 6 years old, but it required the use of community health facilities. This requirement potentially limited the value of the insurance. Employing difference-in-discontinuities and triple-difference methods and using data from 2002, 2004, and 2006, I show that, despite health coverage among nonpoor children increasing by nearly three times, there is little or no evidence that the reform significantly increased health care utilization, changed care locations from private to public sites, lowered out-of-pocket costs, or improved health status for nonpoor young children. My results suggest a "bypassing" phenomenon whereby nonpoor families skipped free health care at low-quality facilities.

KEYWORDS

children, difference in discontinuities, health reform, health care utilization, medical expenses, nonpoor, public health insurance, triple difference, Vietnam

1 | INTRODUCTION

Individuals in developing countries have been known to delay medical care because of large out-of-pocket (OOP) costs, and many are impoverished by health care expenses. In the early 2000s, the World Bank provided technical support to its member states for using public funds to subsidize insurance premiums for vulnerable populations. A wave of health insurance reforms has since swept through developing countries in Africa, Asia, and Latin America (Han, 2012; Hsiao & Shaw, 2007). Unlike health systems in developed countries, those in most developing countries are burdened not only by financing issues but also by inefficiency and poor quality of services, which can discourage their use (Escobar, Griffins, & Shaw, 2010). Although a growing literature exists on the impacts of public health insurance in developing countries, the evidence on whether it encourages individuals to seek medical care more frequently and improves their health status is mixed (Giedion, Alfonso, & Diaz, 2013; Mitra, Palmer, Pullaro, Mont, & Groce, 2017). On the one hand, reforms in Colombia, Peru, and Thailand were found to increase service utilization, with the effects being more pronounced for the use of curative and prenatal care (Miller, Pinto, & Vera-Hernandez, 2013; Bernal, Carpio, & Klein, 2017; Limwattananon et al., 2015). On the other hand, there is no evidence that beneficiaries used more health care services in China and Mexico (King et al., 2009; Wagstaff & Yu, 2007). Public health insurance lowered young children's mortality in Brazil and Thailand and reduced the incidence of low birth weight in newborns in Colombia (Reis, 2014; Ruiz, Amaya, & Venegas, 2007; Miller et al., 2013; Gruber, Hendren, & Townsend, 2014), but it had no significant impact on health outcomes of the overall target population in China and Mexico (King et al., 2009; Wagstaff & Yu, 2007). My paper contributes to this literature by evaluating how the expansion of public health insurance to nonpoor young children in Vietnam affected health care utilization, OOP expenditures, and health outcomes in the target population.

In 2003, Vietnam offered free health insurance to households below the poverty line, including poor children of all ages. In 2005, the eligibility for this insurance was expanded to cover children under the age of 6 from nonpoor families as well. The share of insured young children in the population subsequently rose sharply, from 18% to 85% between 2004 and 2006 (Table 1, columns 3 and 5). Given the limited resources and capacity of health delivery systems in developing countries, it is important to determine whether devoting efforts and resources to subsidize the nonpoor population translates to desirable outcomes.

Several studies have sought to evaluate the 2005 reform by using regression discontinuity (RD) and difference-in-differences (DiD) approaches (Nguyen & LoSasso, 2017; Nguyen & Wang, 2013; Palmer, Mitra, Mont, & Groce, 2015). However, some significant empirical challenges undermine their identifying strategies. Using pre-policy data, my study shows that the RD estimates are confounded by a large preexisting difference in health coverage between the treatment and control groups because 6 years of age is the cutoff for another health-related intervention, namely, school health insurance. By testing the parallel assumption in the pre-policy period, my study also provides evidence that the DiD estimates are likely to be confounded by differential trends in outcomes between the two age groups.

To address these issues, I employ two strategies: (a) difference-in-discontinuities approach and (b) triple difference, which relax the identifying assumptions of RD and DiD, respectively. These strategies allow me to distinguish the 2005 reform effects from other factors that potentially had differential effects on the treated and untreated.

Unlike past work, my study reveals that the policy of expanding public health insurance to nonpoor young children in 2005 did not lead to significant changes in the use of outpatient or inpatient services or that of public or private facilities despite its large positive impact on health coverage. I also document negative but not significant effects of the policy on OOP expenditures and the number of sick days. My results are robust to a number of specification checks.

One potential explanation for my findings is that nonpoor families skipped free health services because public health insurance required them to begin the health care-seeking process at registered commune health centers (CHCs), which tend to be of low quality (Lieberman & Wagstaff, 2009). The findings are consistent with literature. Beneficiaries of public health insurance skipping free services at public facilities, known as the “bypassing” phenomenon, has been observed in a number of developing countries (O'Donnell, 2007).

2 | BACKGROUND

2.1 | The health care delivery system

Health services in Vietnam are delivered primarily through a dominant public sector and a small private sector. The public health system operates on three tiers. The first point of health service contact is an extensive network of nearly 10,000 CHCs throughout the country, which provides simple outpatient care, preventive care, delivery, and family

TABLE 1 Selected health-related outcomes of nonpoor children aged 0–11

	2002		2004		2006	
	Under 6, nonpoor	Ages 6–11, nonpoor	Under 6, nonpoor	Ages 6–11, nonpoor	Under 6, nonpoor	Ages 6–11, nonpoor
	(1)	(2)	(3)	(4)	(5)	(6)
Health insurance	—	—	0.178 (0.008)	0.608 (0.008)	0.854 (0.008)	0.747 (0.008)
Any health care services	0.198 (0.006)	0.124 (0.004)	0.445 (0.010)	0.298 (0.008)	0.544 (0.011)	0.358 (0.009)
Any services at public facilities	0.136 (0.005)	0.079 (0.003)	0.325 (0.010)	0.186 (0.006)	0.420 (0.011)	0.258 (0.008)
Any services at private facilities	0.078 (0.004)	0.051 (0.002)	0.177 (0.008)	0.135 (0.006)	0.196 (0.009)	0.128 (0.006)
OOP expenditure >0	0.196 (0.006)	0.122 (0.004)	0.436 (0.010)	0.292 (0.007)	0.424 (0.011)	0.295 (0.008)
OOP expenditure (in VND)	66,755 (4,796)	40,986 (4,200)	146,426 (24,387)	71,489 (15,480)	166,353 (29,098)	130,291 (23,837)
Number of sick days	—	—	6.37 (0.418)	3.26 (0.578)	7.84 (0.491)	3.49 (0.258)
Observations	8,891	14,458	2,662	4,196	2,445	3,305

Note. This table provides selected outcomes of interest for nonpoor children aged 0-11. The statistics are weighted using the sample weights provided in VHLSSs. Standard errors are in parentheses. Exchange rate: 1 USD=15,500 VND. Abbreviations: OOP, out of pocket; VND, Vietnam Dong; VHLSSs, Vietnam Household Living Standard Surveys.

planning at the commune level (7,000 to 12,000 people). Only 70% of the CHCs have a medical doctor. CHCs disproportionately serve the poor and have a very low number of consultations per year. More than 500 district hospitals form the second tier of the health care delivery network. Lastly, the tertiary tier consists of approximately 200 provincial hospitals, national hospitals, and specialty hospitals (Lieberman & Wagstaff, 2009).

The private sector makes up less than 18% of the health care market and also greatly varies in the quality of services (Lieberman & Wagstaff, 2009). For example, the quality of foreign-owned hospitals is well above that of any public hospitals, but practices of rural traditional medicine practitioners may fail an acceptable hygiene standard. People, however, are fond of private providers because they offer more flexible time of services (Nguyen, 2013).

2.2 | Health insurance for children in Vietnam

Prior to 2003, children in Vietnam were covered by two voluntary insurance schemes: (a) the dependent plan, which was only available to children of public sector employees, and (b) school health insurance, which was cheaper and had been available to all school children since 1994. Children under 6 years old typically received insurance through the former channel, while older children mainly had insurance through the latter (Lieberman & Wagstaff, 2009).

In 2003, the government provided free public health insurance to families below the poverty line, which accounted for 20% of the population. Consequently, poor children of all ages were entitled to free health insurance. However, only 18% of nonpoor children under 6 years old had insurance in 2004 (Wagstaff & Doorslaer, 2003). In 2005, the government expanded the insurance eligibility to cover children under the age of 6 in nonpoor families. The proportion of insured children under 6 rapidly increased and reached 96% of all children in this age group by 2007 (Lieberman & Wagstaff, 2009).

The public health insurance benefit package was the same for all beneficiaries. It covered most inpatient and outpatient services, associated laboratory tests, and drugs approved by the Ministry of Health, as long as the total health care expenses did not exceed 7 million Vietnam Dong (Lieberman & Wagstaff, 2009).

To receive the benefit package, beneficiaries were required to register at a CHC. They were also required to first seek medical care at their registered unit. To receive free services at higher level hospitals, beneficiaries needed to obtain a referral from their registered unit. If they chose to seek care at nonregistered facilities without a referral, they would have to pay OOP for the services used (Lieberman & Wagstaff, 2009). According to Sepehri, Sarma, and Serieux (2009), half of the publicly insured population forgo their benefits to seek medical attention at nonregistered facilities.

Because nonpoor children over 6 are the key comparison group, examining how their voluntary health coverage changed over this period is useful. These children do not qualify for free public health insurance, but students can get voluntary health insurance through their schools. Based on my calculations from the Vietnam Household Living Standard Surveys, the proportion of nonpoor children over 6 with health insurance steadily expanded from 67% in 2004 to 76% in 2006. This observed trend suggests that the control group was not static during this time period even though there was no policy change with regard to this voluntary scheme. Thus, it is particularly important to account for time effect on health insurance or the possibility of differential trends between the treatment and control groups.

2.3 | Possible effects of the reform

The direction and extent of the reform impacts depend on several factors: whether the beneficiaries choose to use the free services associated with the public health insurance, and if they do, how that affects their health care utilization, OOP medical costs, and health outcomes. I will first discuss the former and then the latter.

Because the policy required that every treatment process begins at the registered CHCs, which tend to be lower quality than other public facilities (Lieberman & Wagstaff, 2009), it offered nonpoor families two discrete options: low-price, low-quality care associated with the public health insurance, and high-price, high-quality care without public health insurance. Because medical costs represent a significant portion of the budget for low-income families, the marginal utility of consumption lost to pay for the higher quality health care is likely high. As income rises, the marginal rate of substitution of consumption for health diminishes, holding health constant. Given that health care is a normal good, Gertler and van der Gaag's (1990) behavioral model in a discrete choice situation predicts that higher income individuals are more likely to choose the high-price, high-quality option, and lower income individuals are more likely to choose the low-price, low-quality option.

In Vietnam, a single visit to a public hospital can cost about 45% of nonfood expenditure for a family in the lowest economic quintile, whereas this figure is only 4% for a family in the highest quintile (World Bank, 2001). Families in lower economic quintiles thus are more likely to choose the low-price, low-quality health service package rather than to give up their essential consumption. Alternatively, families in higher economic quintiles are more likely to choose the high-price, high-quality package because they do not have to give up much for it. However, nonpoor families in Vietnam are far from being wealthy. So whether they choose to make use of free services offered by the reform is an empirical question.

For families who choose to use the public health insurance benefits, the effects on their health care utilization, OOP expenditures, and health status are still ambiguous. The reform substantially subsidized health care expenses, simultaneously decreasing the relative price of health care to other goods and services and increasing the family disposable income. Both these substitution and income effects should increase the utilization of health care services given that health care is a normal good. However, if people receive more preventive care or better nutrition, which improves health, the demand for other types of health care can potentially decrease, thereby making the net effect on health care use and expenses unclear.

Because the policy requires every treatment process to begin at the registered unit and only subsidizes expenses for services at public facilities, the reform is expected to lead to an increase in beneficiaries seeking care in the public sector and a decrease in beneficiaries seeking care in the private sector. At the same time, as the beneficiaries flow to public facilities, waiting times and congestion might rise, causing some segments of the population to shift away from public services and toward private providers, making the net effect on location of care and service utilization ambiguous. If the reform leads households to seek care at CHCs instead of higher level public hospitals, the quality of the services used tends to fall, which likely adversely affects health outcomes. However, if households switch from the private sector to CHCs, the quality will change in an unknown way because quality varies greatly across private providers.

In summary, both the questions of whether nonpoor families actually use the health care services offered by the reform and whether their utilization of the health care services lead to any changes in their OOP expenditures and health outcomes cannot be answered definitively by theory and need to be investigated empirically.

3 | DATA

This study uses data from the nationally representative Vietnam Household Living Standard Surveys 2002, 2004, and 2006. The samples include 132,384 individuals from 30,000 households in 2002 and 40,000 individuals from approximately 9,180 households in 2004 and 2006. In addition to collecting information about demographic characteristics, education, and household expenditures, the surveys provide a wide range of health utilization and medical expenditure information from the previous 12 months. The surveys also include self-reported data on whether households are beneficiaries of the government's pro-poor programs, including free public health insurance for the poor. Identification of poor households had been consistently conducted by the government every year during the study period, and the poverty criteria were revised every 5 years (International Monetary Fund, 2004).

Table 1 provides summary statistics of selected outcomes in 2002–2006.[1] Several trends are worth noting. First, large gaps in most outcomes between the treatment and control groups existed in 2004, the year prior to the reform (see Table 1, columns 3 and 4). Specifically, health coverage was substantially lower among the treatment group, whereas health care utilization and OOP expenditures in the treatment group were higher than those in the control group. These discrepancies suggest that comparing the outcomes in the post-policy period only, without accounting for the preexisting differences between these two groups, would lead to underestimation of the effects on health coverage and OOP expenditures and overestimation of the effects on health care utilization. Second, because OOP expenses as well as the probability of using health care or any services in public and private facilities increased more rapidly among young nonpoor children than older nonpoor children in the pre-policy period 2002–2004 (see Table 1, columns 1 through 4), DiD estimates would also overestimate the positive effects on health care utilization and the use of public and private facilities and underestimate the negative effects on OOP expenditures because the pre-trends would be falsely assumed to be similar between the two groups in the absence of the reform.

4 | EMPIRICAL SPECIFICATIONS

This section first presents evidence that RD and DiD approaches likely produce biased estimates of the policy effects (Sections 4.1.1 and 4.2.1). It next describes difference-in-discontinuities and triple-difference models (Sections 4.1.2 and 4.2.2) that address the issues affecting RD and DiD models.

[1]Detailed descriptive statistics by treatment status, bandwidth, and year are presented in Online Appendix Tables B1 and B2.

4.1 | Difference-in-discontinuities approach

4.1.1 | Issues with RD

Because the 2005 reform made 6 years of age the eligibility threshold for public health insurance, the natural first approach is to use an RD model (Palmer et al., 2015). For RD estimates to be unbiased, an important condition is that no concurrent policies are present that use the same threshold and could have an impact on the outcomes of interest. This condition appears to be violated in the context of Vietnam.

In Vietnam, children over 6 attend school and are eligible for school health insurance, which is cheaper and not as restrictive as the dependent plan. These children are thus more likely to have health coverage. These different insurance statuses might cause differential health care-seeking behaviors and outcomes between the treatment and control groups, leading to a violation of the RD model's assumption.

As a placebo test for their specification, Palmer et al. (2015) use an RD design to estimate the reform effect in the pre-period based on data from 2004 and find a significant discontinuity in health insurance coverage between the treatment and control groups in the pre-policy period. They also discuss the possibility that the pre-treatment discontinuity in 2004 was caused by 6 years of age also being the threshold for eligibility for school health insurance. I replicate their falsification test and come to the same conclusion (see Online Appendix A1). I find a large discontinuity between health coverage of children aged under 6 and those over 6 in 2004 (Online Appendix Figure 1A and Table B5).

This result indicates that the RD coefficient on health coverage relying entirely on the post-policy period data would be underestimated due to the omission of the preexisting gap in health coverage prior to the policy. This finding raises several concerns about Palmer et al.'s (2015) second-stage estimates. First, it is unclear whether their second-stage estimates capture the reform effects or the effects of the school health insurance plan since both policies have the same eligibility cutoff at 6 years of age. Second, the underestimated first-stage coefficient will cause the second-stage estimates to be scaled up too much and result in overestimation. And third, because service utilization and OOP expenditures in the treatment group were higher than those in the control group prior to the reform (see Table 1), an RD approach that uses only post-policy data would mistakenly capture these preexisting gaps as the reform effects, consequently overestimate the effects on health care utilization but underestimate the effects on OOP expenditures. Altogether, the RD estimates would likely overestimate the policy effects on health care utilization and bias the effects on OOP expenditures in an ambiguous direction. In practice, I find a smaller and less significant effect on health care utilization and larger but less significant effect on OOP expenditures than Palmer et al.'s (2015) results (see Section 5).

4.1.2 | Difference-in-discontinuities model

To address the concern with the RD approach, I apply a difference-in-discontinuities design (Grembi, Nannicini, & Troiano, 2016) for nonpoor children in the period 2004–2006 with the bandwidths of 48 and 24 months on each side of the 72-month threshold. The scatterplots of the data (Online Appendix Figures 1, B1, and B2) support a linear specification of the function of age effects. I thus estimate the following equation:

$$\begin{aligned} Y_{it} = {} & \alpha_0 + \alpha_1 UNDER6_{it} + \alpha_2 AGE_{it} + \alpha_3 (UNDER6_{it} \times AGE_{it}) + \beta_0 YEAR2006_t + \beta_1 (YEAR2006_t \times UNDER6_{it}) \\ & + \beta_2 (YEAR2006_t \times AGE_{it}) + \beta_3 (YEAR2006_t \times UNDER6_{it} \times AGE_{it}) + \delta X_{it} + \varepsilon_{it}. \end{aligned} \tag{1}$$

In these models, Y_{it} denotes the outcomes of interest. $UNDER6_{it}$ is the indicator for children aged under 6. The running variable is AGE_{it}, calculated as the number of months from individual age to the cutoff of 72 months. $YEAR2006_t$ is an indicator for year 2006. To introduce the before/after effects, I interact $YEAR2006_t$ with other variables in the model. The interactions between AGE_{it}, $UNDER6_{it}$ and $YEAR2006_t$ are added to mitigate bias caused by age-related factors. Vector X_{it} presents individual and household characteristics, namely, child age and gender, residency, and ethnicity. I control for household size, gender, age, education, and marital status of household heads in certain specifications.

The coefficient of interest, β_1, provides the difference between the discontinuity in outcomes of children just under 6 and over 6 in 2006 and the discontinuity in 2004. By controlling for the gaps of outcomes in 2004, I overcome the challenge encountered with the RD approach. The difference-in-discontinuities estimates would be unbiased even if discontinuities existed at the threshold in the pre-policy period.

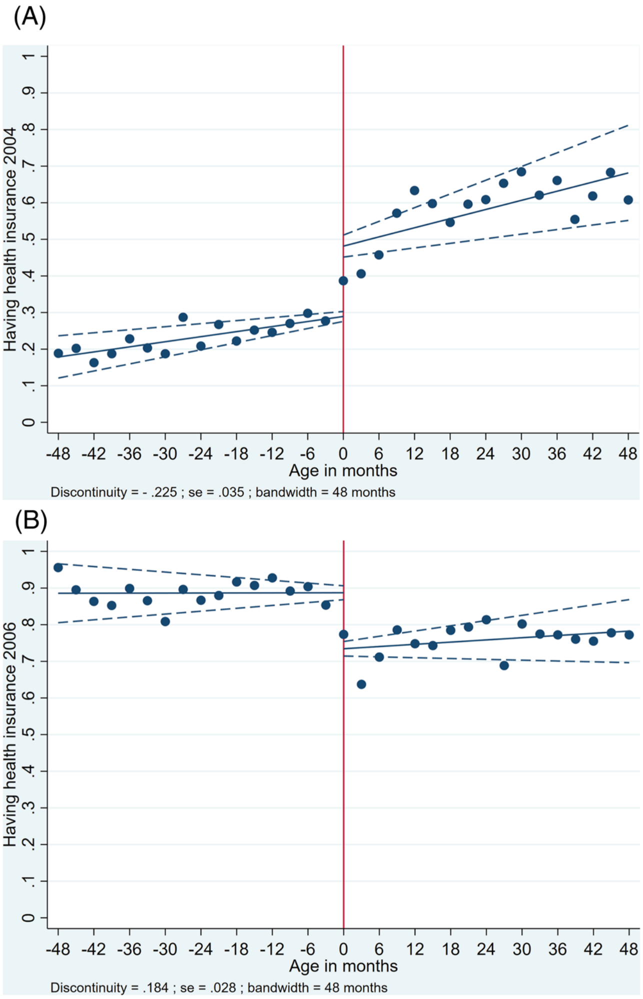

FIGURE 1 Health insurance among nonpoor children aged 2–9 (2004–2006). (A) Health insurance among nonpoor children aged 2–9 in 2004. Panel (a) reports the percentage of nonpoor children aged 2–9 with health insurance by age in 2004. Age is centered at 72 months old, so the treated group has a negative age and the control has a positive age. The discontinuity at the threshold presents the difference in health coverage between the treatment and control groups in 2004. (B) Health insurance among nonpoor children aged 2–9 in 2006. Panel (b) plots an analogous figure for nonpoor children aged 2–9 in 2006. The difference between the discontinuities in panels (B) and (A) illustrates the difference-in-discontinuities estimate of the reform effect on health insurance displayed in Table 2, panel A, column (2). The dashed lines present the 95% confidence interval (CI) of the fitted lines [Colour figure can be viewed at wileyonlinelibrary.com]

I also estimate another difference-in-discontinuities model that allows for quadratic age effects as a robustness check:

$$\begin{aligned}Y_{it} &= \alpha_0 + \alpha_1 UNDER6_{it} + \alpha_2 AGE_{it} + \alpha_3(UNDER6_{it} \times AGE_{it}) + \beta_0 YEAR2006_t + \beta_1(YEAR2006_t \times UNDER6_{it}) \\ &\quad + \beta_2(YEAR2006_t \times AGE_{it}) + \beta_3(YEAR2006_t \times UNDER6_{it} \times AGE_{it}) + \gamma_1 AGE_{it}^2 + \gamma_2\left(UNDER6_{it} \times AGE_{it}^2\right) \\ &\quad + \gamma_3\left(YEAR2006_t \times AGE_{it}^2\right) + \gamma_4\left(YEAR2006_t \times UNDER6_{it} \times AGE_{it}^2\right) + \delta X_{it} + \varepsilon_{it}.\end{aligned} \tag{2}$$

The identifying assumption for the difference-in-discontinuities models is that the discontinuities in outcomes between children just over and under 6 years old in 2006 would be the same as the discontinuities in 2004 without the 2005 reform. I directly test this assumption, and the test results support the validity of my assumption (see Section 6.1). In addition, I employ a triple-difference strategy that relaxes this assumption.

4.2 | Triple-difference approach

4.2.1 | Issues with DiD

Another common approach in addition to RD is DiD, which assumes that the outcomes of the treatment (nonpoor under 6) and control groups (nonpoor over 6) do not change differently over time (Nguyen & Wang, 2013; Nguyen &

LoSasso, 2017). However, changes in outcomes of the nonpoor over 6 might not be good counterfactuals for those of the nonpoor under 6 because children start schooling at 6. Being exposed to different settings, children over 6 may develop different health patterns over time relative to children under 6. In addition, the trends of being covered might diverge because the two main health insurance schemes covering these two groups might have different outreach activities.

To test the parallel assumption crucial for the DiD strategy to be valid, I estimate a DiD model using data from 2002 and 2004 (see Online Appendix A2). I find that the probability of using health care service, the OOP expenditure, and the use of public services among the nonpoor under 6 grew significantly faster than among the nonpoor over 6 in the absence of the policy (Online Appendix Table B6).[2] The presence of these differential trends over time casts doubt on the validity of the DiD estimates in the post-policy period. These results predict that the DiD estimates in the 2004–2006 period would likely overestimate the positive effects on health care utilization and the use of public and private facilities and underestimate the negative effects on OOP expenditures because the pre-trends would be falsely assumed to be similar between the two groups in the absence of the reform.

4.2.2 | Triple-difference model

Because poor children of all ages had access to public health insurance in 2003, they would not be affected by the 2005 reform. This scenario allows me to control for the differential time trends between the nonpoor under 6 and nonpoor over 6 by controlling for the differential trends between the poor under 6 and poor over 6. My triple-difference model takes the following form:

$$\begin{aligned} Y_{it} = {} & \alpha_0 + \alpha_1 UNDER6_{it} + \alpha_2 YEAR2006_t + \alpha_3 (UNDER6_{it} \times YEAR2006_t) + \beta_0 NONPOOR_{it} \\ & + \beta_1 (NONPOOR_{it} \times UNDER6_{it}) + \beta_2 (NONPOOR_{it} \times YEAR2006_t) + \beta_3 (NONPOOR_{it} \times UNDER6_{it} \times YEAR2006_t) \\ & + \delta X_{it} + \varepsilon_{it}, \end{aligned} \tag{3}$$

where Y_{it} denotes the outcomes of interest; $YEAR2006_t$ is the year 2006 fixed effect; $UNDER6_{it}$ is the indicator for a child under age 6; and $NONPOOR_{it}$ is the indicator for not being a member of a poor family. The model also includes a full set of the interactions between these three fixed effects. β_3 is the coefficient of interest. X_{it} is the vector of individual and household characteristics described in Section 4.1.2.

Relative to the DiD approach, the triple-difference specification relies on a more flexible assumption that the discrepancy in outcomes of the nonpoor over 6 and under 6 would evolve similarly to that of the poor over 6 and under 6 without the reform. The concern about differential trends due to exposure to different health settings or health policies that may cause bias in the DiD and difference-in-discontinuities models would not be present in this specification. Triple difference is hence the preferred strategy for my analysis.

5 | RESULTS

Table 2 presents estimation results from the two alternative approaches: difference-in-discontinuities and triple difference. Columns (1) and (2) show the difference-in-discontinuities estimates with the 48-month bandwidth; columns (3) and (4) show the difference-in-discontinuities estimates with the 24-month bandwidth[3]; and columns (5) and (6) present the coefficients of the triple-difference approach. The models in columns (1), (3), and (5) exclude household covariates, whereas models in columns (2), (4), and (6) include them.[4]

Regardless of which approach is used and whether household covariates are included, conclusions are similar across specifications. I observe a large positive effect of the 2005 reform on health coverage (Table 2, panel A). My preferred specification (column 6) suggests that treated children were 50.9 percentage points more likely to have health insurance.

The data plot of health service utilization (Online Appendix Figure B1) indicates that health care utilization did not rise among nonpoor children despite increased coverage. The coefficients on having any outpatient visit are positive, but none is even remotely significant, and all are small in absolute magnitude (Table 2, panel B). For the probability of

[2]The pre-trend for health insurance cannot be examined since the 2002 data do not cover this information.

[3]Results from estimation of difference-in-discontinuities with bandwidths that are smaller than 48 months are similar and available upon request.

[4]Summary statistics for the difference-in-discontinuities and the triple-difference samples are presented in Online Appendix Tables B1 and B2, respectively.

TABLE 2 Effects of the expansion of public health insurance on nonpoor children aged under 6

	Difference in discontinuities					
	48 month		24 month		Triple difference	
	(1)	(2)	(3)	(4)	(5)	(6)
	Panel A: Health Insurance					
Having health insurance coverage	0.411*** (0.040)	0.408*** (0.040)	0.396*** (0.059)	0.389*** (0.058)	0.502*** (0.035)	0.509*** (0.036)
	[0.209]		[0.239]		[0.178]	
	Panel B: Healthcare Utilization					
Using health care services at least once per year	0.016 (0.047)	0.009 (0.046)	0.007 (0.067)	0.006 (0.065)	0.010 (0.046)	0.025 (0.044)
	[0.434]		[0.407]		[0.445]	
Using outpatient services at least once per year	0.012 (0.047)	0.006 (0.046)	0.012 (0.067)	0.011 (0.065)	0.015 (0.045)	0.030 (0.043)
	[0.407]		[0.387]		[0.415]	
Using inpatient services at least once per year	−0.007 (0.019)	−0.008 (0.020)	0.009 (0.026)	0.009 (0.026)	−0.0001 (0.024)	−0.001 (0.024)
	[0.055]		[0.043]		[0.059]	
	Panel C: Locations of Care					
Using health care services at any public facilities	0.018 (0.044)	0.009 (0.044)	−0.024 (0.062)	−0.032 (0.062)	0.020 (0.044)	0.030 (0.043)
	[0.301]		[0.267]		[0.325]	
Using health care services at any private facilities	0.006 (0.037)	0.007 (0.036)	−0.005 (0.053)	0.001 (0.051)	−0.026 (0.029)	−0.020 (0.027)
	[0.183]		[0.184]		[0.177]	
	Panel D: Out-of-Pocket Expenditures					
Total OOP medical expenditure >0	−0.009 (0.047)	−0.015 (0.045)	−0.030 (0.066)	−0.034 (0.064)	−0.011 (0.044)	0.002 (0.043)
	[0.425]		[0.396]		[0.436]	
Log (Total medical expenditure +1)	0.106 (0.534)	0.043 (0.521)	−0.212 (0.754)	−0.262 (0.737)	−0.101 (0.484)	0.007 (0.471)
	[61,286]		[49,199]		[59,004]	
Log (Outpatient expenditure+1)	0.099 (0.523)	0.037 (0.509)	−0.048 (0.739)	−0.078 (0.720)	−0.277 (0.455)	−0.153 (0.442)
	[51,720]		[43,477]		[49,102]	
Log (Inpatient expenditure+1)	−0.024 (0.207)	−0.030 (0.207)	0.036 (0.283)	0.031 (0.281)	0.142 (0.261)	0.128 (0.261)
	[9,566]		[5,732]		[9,902]	
	Panel E: Health Status					
Number of sick days	−2.257 (1.444)	−2.418* (1.435)	−1.003 (1.870)	−1.097 (1.848)	−0.164 (1.555)	−0.218 (1.568)
	[5.74]		[4.90]		[6.37]	
Individual characteristics	Y	Y	Y	Y	Y	Y
Household characteristics	N	Y	N	Y	N	Y
Observations	8,020		3,880		14,726	

Note. Difference-in-discontinuities coefficients from estimation of Equation (1) are displayed in columns (1) through (4). Triple-difference coefficients from estimation of Equation (3) are displayed in columns (5) and (6). The means of dependent variables of treatment group in 2004 are provided in square brackets. Individual covariates include age, gender, and ethnicity. Household covariates include household size, region, and household head's age, gender, education, and marital status. OOP expenditure data are trimmed at the 99th percentile of the distribution. Regressions are weighted using the sampling weights provided in the VHLSSs. Robust standard errors in parentheses.

Abbreviations: OOP, out of pocket; VHLSSs, Vietnam Household Living Standard Surveys.

*$p < .1$. **$p < .05$. ***$p < .01$.

hospitalization, the coefficients are very small and also insignificant. These estimates suggest that being eligible for public health insurance was associated with minor and insignificant changes in the probability of using any outpatient or inpatient services.

The effects on redistribution of services between the public and private sectors are presented in Table 2, panel C. The beneficiaries were given free services only when they started the health care-seeking process at a CHC. Results from my preferred specification suggest a 2.0 percentage point increase in using services at public sites and a 3.0 percentage point decrease in the probability of using services at private sites. These estimates have the expected signs but are statistically insignificant.

Next, I examine the effects on OOP expenditures (Table 2, panel D). First, I investigate whether the policy changed the probability of spending any money on health care services. In 2004, 34% of children had positive health care costs. The result shows that the reform had an ignorable and insignificant impact (0.2 percentage points) on this probability. Second, I explore whether OOP expenditures were reduced. Because the data on OOP expenditure form an extremely long right tail, I exclude the OOP outpatient and inpatient costs beyond the 99th percentile of the distribution for each year to reduce the noise.[5] As illustrated in Online Appendix Figure B2, OOP expenditures of the treatment group were higher than those of the control group before the reform, but the gap closed afterward. Results from panel D of Table 2 suggest that the expanded eligibility was associated with a 15.3% decrease in outpatient cost but a 12.8% increase in inpatient cost. Both effects are quite large but imprecisely measured. Altogether, children less than 6 years old experienced an almost zero change in total OOP spending (0.7%) as well as the probability of having a positive OOP expenditure (0.2%). Finally, the treatment group reported a lower number of sick days, but the effect was small and also statistically insignificant (Table 2, panel E).[6] Online Appendix Table B4 provides estimates from nonweighted regressions, which give similar implications as these weighted estimates.

Overall, although the eligibility expansion had large, positive, and significant impacts on health coverage, it did not lead to any statistically significant changes in utilization of medical care, location of care, or health status of nonpoor children. The reform was associated with a large decrease in OOP expenses, but that impact is statistically insignificant.

My findings differ from those of past studies in several ways. First, I find larger impact on health coverage than Palmer et al.'s (2015) RD estimates, which is expected because (a) both authors of the said paper and I find a preexisting gap in health coverage between the treatment and control groups, which is not captured by the RD estimates, and (b) Palmer et al.'s sample includes poor children who had received free health insurance in 2003 and should not have been affected by this eligibility expansion to nonpoor children. Whereas my results suggest no significant impacts on utilization of health care at public and private facilities, past work finds positive and significant effects on these outcomes.[7] In addition, my results suggest a negative impact on total and outpatient OOP expenditures, whereas past work offers mixed conclusions on effects of the reform on medical expenses, including increases in total OOP expenditures (Palmer et al., 2015) and outpatient OOP expenditures (Nguyen & LoSasso, 2017). However, the discrepancies between the conclusions from past work and my findings are expected. Recall that in Sections 4.1.1 and 4.2.1, my investigation of the preexisting gaps and trends between the treatment and control groups using pre-policy data suggests that DiD and RD estimates likely overstate the impact of the reform on utilization of any health care services and on the use of public and private facilities, while they likely understate the impact on OOP expenditures.

6 | SENSITIVITY ANALYSIS

6.1 | Falsification test for difference-in-discontinuities models

The identifying assumption of the difference-in-discontinuities estimation is that the trends in the outcomes would have been the same between children just under 6 and just over 6 in the absence of the 2005 reform. To test this assumption, I re-estimate Equation (1) in the pre-period using 2002–2004 data. Health insurance status and the number of sick days are excluded from the outcomes of the falsification tests because the information is not available for 2002.

The 2002–2004 difference-in-discontinuities estimates are presented in Table 3. They indeed indicate that no significantly differential trends existed between the treatment and control groups. The results of this falsification exercise support the validity of the 2004–2006 difference-in-discontinuities estimation.

6.2 | Robustness check for triple-difference models

Next, I address the concern related to a portion of the control group, children ages 6–7, having been treated in 2005. I drop this group of children from the sample and perform a "donut" triple-difference analysis using Equation (3). Restricting the control group to children over 7 instead of children over 6 left the results unchanged. Moreover, the estimates are similarly robust to the inclusion of household size and household head's age, gender, education, and marital status (Table 4).

[5]The coefficients estimated when the OOP data are untrimmed have the same signs, are much larger, and have the same significance level (not statistically significant at the 10% level).

[6]Estimates from quadratic specifications are presented in Online Appendix Table B3. The conclusion does not change regardless of which specification is used.

[7]All previous three papers find significant and positive impacts on health care utilization and the use of public facilities; two find significant and positive impacts on the use of private facilities.

TABLE 3 Effects of the expansion of public health insurance on nonpoor children aged under 6 in the pre-policy period: Difference-in-discontinuities estimates

	Difference in discontinuities			
	48 month		24 month	
	(1)	(2)	(3)	(4)
Using health care services at least once per year	0.036 (0.036)	0.042 (0.035)	0.019 (0.051)	0.023 (0.050)
	[0.185]		[0.158]	
Using outpatient services at least once per year	0.041 (0.035)	0.046 (0.034)	0.015 (0.050)	0.019 (0.049)
	[0.148]		[0.129]	
Using inpatient services at least once per year	-0.0005 (0.014)	0.001 (0.015)	0.004 (0.019)	0.004 (0.019)
	[0.046]		[0.035]	
Using health care services at any public facilities	0.016 (0.032)	0.022 (0.032)	0.046 (0.045)	0.052 (0.044)
	[0.124]		[0.105]	
Using health care services at any private facilities	0.026 (0.027)	0.026 (0.027)	0.016 (0.039)	0.014 (0.038)
	[0.076]		[0.064]	
Total OOP medical expenditure >0	0.023 (0.036)	0.028 (0.035)	0.008 (0.051)	0.012 (0.049)
	[0.184]		[0.156]	
Log (Total medical expenditure+1)	0.206 (0.404)	0.259 (0.394)	0.227 (0.573)	0.255 (0.556)
	[23,342]		[18,596]	
Log (Outpatient expenditure+1)	0.282 (0.392)	0.332 (0.381)	0.152 (0.560)	0.184 (0.543)
	[15,172]		[12,802]	
Log (Inpatient expenditure+1)	−0.064 (0.152)	−0.046 (0.152)	0.040 (0.192)	0.053 (0.192)
	[8,169]		[5,794]	
Individual characteristics	Y	Y	Y	Y
Household characteristics	N	Y	N	Y
Observations	19,872		9,970	

Note. This table provides estimates of the reform effects in the pre-policy period as a falsification test. Coefficients from estimation of Equation (1) are displayed. The means of dependent variables of treatment group in 2002 are provided in square brackets. Individual covariates include age, gender, and ethnicity. Household covariates include household size, region, and household head's age, gender, education, and marital status. OOP expenditure data are trimmed at the 99th percentile of the distribution. Regressions are weighted using the sampling weights provided in the VHLSSs. Robust standard errors in parentheses.

Abbreviations: OOP, out of pocket; VHLSSs, Vietnam Household Living Standard Surveys.

*$p < .1$. **$p < .05$. ***$p < .01$.

7 | CONCLUSION

Understanding whether health insurance can improve health access and outcomes of target populations is crucial to any health reform. This study evaluates the public health insurance expansion in Vietnam in 2005, which targeted young children in nonpoor families. My findings suggest that the reform was successful in raising health insurance, but it did not effectively increase health service utilization, reduce OOP costs, or improve children's health.

When choosing between high-price, high-quality and low-price, low-quality options, higher income individuals are more likely to select the former, and lower income individuals are more likely to select the latter, given that health is a normal good (Gertler & van der Gaag, 1990). The insurance reform in Vietnam seems to have presented this corner solution to nonpoor households. Consistent with the observation of Sepehri et al. (2009) that 50% of the insured in Vietnam bypass their registered units and seek care from alternative sources, my results suggest that many beneficiary families opted not to use public health insurance, which required them to seek care at low-quality public facilities. This bypassing issue has been observed in other developing countries, such as Sri Lanka, Indonesia, and Pakistan where low quality of public care is prevalent (O'Donnell, 2007), and it presents a waste of resources.

These findings raise several important questions. First, given limited resources and capacity of health delivery systems in developing countries, should public insurance target nonpoor individuals? Second, can expanded health insurance alone increase access to health care in developing countries? Appropriate allocation of public funds and adequate quality are essential to the success of health insurance reform. Therefore, spending needs to be directed to the most responsive groups. In addition to solving demand-side problems, it might be important for developing countries to pay attention to the supply side to ensure that the quality of services being provided is acceptable.

TABLE 4 Effects of the expansion of public health insurance on nonpoor children aged under 6: "Donut" triple-difference estimates

	"Donut" triple difference		
	(1)		(2)
Having health insurance coverage	0.491*** (0.036)	[0.178]	0.498*** (0.036)
Using health care services at least once per year	0.004 (0.046)	[0.445]	0.024 (0.045)
Using outpatient services at least once per year	0.011 (0.045)	[0.415]	0.031 (0.044)
Using inpatient services at least once per year	−0.001 (0.024)	[0.059]	−0.003 (0.024)
Using health care services at any public facilities	0.015 (0.044)	[0.325]	0.027 (0.044)
Using health care services at any private facilities	−0.024 (0.029)	[0.177]	−0.014 (0.028)
Total OOP medical expenditure >0	−0.008 (0.044)	[0.415]	0.009 (0.043)
Log (Total medical expenditure+1)	−0.072 (0.492)	[59,004]	0.097 (0.479)
Log (Outpatient expenditure+1)	−0.226 (0.462)	[49,102]	−0.038 (0.449)
Log (Inpatient expenditure+1)	0.119 (0.263)	[9,902]	0.103 (0.264)
Number of sick days	0.273 (1.571)	[6.37]	0.225 (1.586)
Individual characteristics	Y		Y
Household characteristics	N		Y
Observations		13,566	

Note. Coefficients from estimation of Equation (3) are displayed. Children ages 6–7 are excluded from the sample. The means of dependent variables of the treatment group in 2004 are provided in square brackets. Individual covariates include age, gender, and ethnicity. Household covariates include household size, region, and household head's age, gender, education, and marital status. OOP expenditure data are trimmed at the 99th percentile of the distribution. Regressions are weighted using the sampling weights provided in the VHLSSs. Robust standard errors in parentheses.

Abbreviations: OOP, out of pocket; VHLSSs, Vietnam Household Living Standard Surveys.

*$p < .1$. **$p < .05$. ***$p < .01$.

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CONFLICT OF INTEREST

None.

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SUPPORTING INFORMATION

Additional supporting information may be found online in the Supporting Information section at the end of the article.

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