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The Impact on Chinese Children of Extending Social Medical Insurance Coverage: A Difference-in-Difference with Staggered Treatment Analysis

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The impact on Chinese children of extending social medical insurance coverage: A difference-in-difference with staggered treatment analysis[1]

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ABSTRACT

Although the impact of introducing social medical insurance schemes in developing countries has attracted considerable research interest, less attention has been paid to the extension and consolidation of such policies. This paper estimates the effect on children's health of the Chinese Urban and Rural Resident Basic Medical Insurance (URRBMI), which was gradually implemented across Chinese provinces. Using recent econometric methods that avoid the problem of negative weights associated with staggered treatments, we find that the URRBMI policy reduced sickness frequency while increasing insurance participation. However, results are statistically insignificant when we employ a traditional two-way fixed effect (TWFE) model. Results highlight the benefits of extending insurance policies in China and the need to evaluate them using accurate estimation techniques.

Keywords: URRBMI; staggered adoption; Difference-in-Difference.

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1. Introduction

While universal health coverage is an aspiration in most countries (Aman et al., 2019), there have been divergent approaches to how it is to be achieved (Frenk, 2015; Lagomarsino et al., 2012). Thus, some patterns include using tax revenues to subsidise target populations, steps towards broader risk pools, or incentives to purchase services through demand-side financing mechanisms (Lagomarsino et al., 2012). In developing countries, it is common to rely on social insurance as the policy instrument (Giedion et al., 2013; Huang & Wu, 2020; Shaw, 2007). The extension of social insurance is commonly associated with unified coverage, funds, benefits and more variety of medical services; see, for example, the discussion about the URRBMI unified coverage by Pan et al. (2016) and Wang (2018). Thus, evaluating the impact of these policies on children can be deemed as a relevant empirical question because of the importance of children as economic assets in developing economies, which has been discussed in a large literature (Galor, 2005; Moav, 2005).

Some of the most relevant studies about children's health focus on the US. They assess, for example, how expanding eligibility for Medicaid affects children's coverage (Card & Shore-Sheppard, 2004; Miller, 2012), health use (Dafny & Gruber, 2000; Kaestner et al., 2001), and health outcomes (Hamersma & Ye, 2021). Unlike this literature, most papers evaluating the impact of social insurance on the Chinese population focus on the introduction (rather than the expansion) of policies (Chen & Jin, 2012; Liu & Zhao, 2014; Peng & Conley, 2016). However, a more recent, but still embryonic, literature has estimated the impact of the URRBMI, which integrated previous schemes for the rural and urban population, on health status among middle-aged and older rural

residents (Huang & Wu, 2020) and labour force (Li & Jin, 2022).[4] As will be explained in Section 2, the URRBMI was gradually adopted in different years across Chinese provinces. In order to study the effect of the implementation of the policy, Huang and Wu (2020) and Li and Jin (2022) employ traditional difference-in-differences (DD) and DD-matching approaches based on comparing individuals in treated and untreated provinces. Overall, they found an apparent positive effect on the labour force and a more limited effect on middle-aged and older rural residents. However, subsequent contributions in the literature have raised concerns about employing traditional DD methods to evaluate staggered treatments. The reason for this is that estimates are susceptible to being biased because the aggregate effects can be decomposed as a weighted sum of different periods and groups but some of these weights can be negative (de Chaisemartin & D'Haultfoeuille, 2020).

This paper analyses the impacts of the URRBMI on health status among Chinese children under the age of 16. To do so, we use a longitudinal database from the China Family Panel Studies (CFPS), which was carried out at two-yearly intervals between 2010 and 2020 (inclusive). In our analysis, we compare health outcomes of individuals in provinces participating in the URRBMI scheme with those in provinces employing the two previous social schemes to better appraise the benefits of the new policy for insured individuals. This research complements previous papers studying the impact of the URRBMI policy in two ways. First, it provides a more comprehensive analysis by looking at both urban and rural children and including information about the last wave of the sample in 2020. Second, and most importantly, to tackle the potential bias

[4] H. Chen, Xing, Yang, & Zhan (2021) is another paper that also focuses on the URRBMI policy. However, unlike the literature cited here, it estimates the impact of the decision to participate, rather than the implementation of the URRBMI.

associated with standard DD estimators, it employs methods proposed by Callaway and Sant'Anna (2021) and de Chaisemartin and D'Haultfoeuille (2020). Although these two approaches rely upon different econometric techniques to obtain causal estimates, both propose aggregation schemes that avoid the problem of negative weights. This is a fundamental issue as traditional two-way fixed effect (TWFE) regressions can mislead conclusions about the impact of treatment effects (Baker et al. (2022).

Estimation results based on these methods indicate that the URRBMI policy significantly reduced sick frequency in children. Moreover, we also find that it increased the probability of insurance participation. Further, health outcome results are robust to the introduction of sample bias corrections. Importantly, and consistent with the discussion in the previous paragraph, this significant effect cannot be identified using a TWFE model, which highlights the relevance of using approaches that deal with the negative weight problem (Callaway & Sant'Anna, 2021). We also found that URRBMI was associated with better coordination of administrative resources and reduced out-of-pocket expenditure. At the same time, it did not significantly impact hospital use. Overall, these results suggest a more efficient use of resources as one potential mechanism that explains the beneficial impact of the URRBMI policy.

This paper proceeds as follows. The following section describes the Chinese social medical insurance systems. Section 3 discusses the two methodologies employed to estimate the health impact of the URRBMI policy. Section 4 presents our database and the variables considered in the research. Section 5 displays and analyses the main results, and Sections 6 and 7 explore transmission mechanisms and other additional extensions. Finally, Sections 8 and 9 respectively discuss the results and propose future

lines of research.

2. Integration policy of social medical insurance

In 1998 China initiated reform of the social medical insurance scheme, a reform aiming to tackle the increase in health care costs as a consequence of the sharp drop in medical insurance coverage (Li & Zhang, 2013). Consequently, in 2003 the Chinese government established the new cooperative medical scheme (NCMS) in rural China and later, in 2007, the urban residents' basic medical insurance system (URBMI) in urban China. These two social medical schemes were separate from each other and have become popular across the Chinese population. More specifically, over 1.3 billion Chinese, 95% of the total population, have participated in the social medical insurance schemes (The State Council Information Office of PRC, 2017). We pay particular attention to the child population. They account for 20% of the Chinese population, most of whom are also covered by insurance schemes (The National Bureau of Statistics of PRC, 2019).

The two schemes (NCMS and URBMI) share three features: (1) both are voluntary and funded by enrolees' premiums and subsidies from central and local governments; (2) both require full household participation;[5] (3) schemes are territory-based. However, the covered population, benefits, and management differ substantially between the two schemes.

The separation of the NCMS and the URBMI led to a lack of coordination and

[5] It means that children are either included or excluded from the programme depending on their parents' involvement (Li & Zhang, 2013; Zhang et al., 2017). However, partial participation is also observed in both insurance schemes due to household members' migration or different registration types (Chen & Jin, 2012). Involvement decisions are made annually at the end of the year.

coherence in the medical insurance policy, distorting hospitals' and doctors' incentives (Wang, 2018). In 2016, China formally established the URRBMI, which integrated the NCMS and URBMI by launching a unified basic medical insurance for urban and rural residents. In particular, the URRBMI unified coverage, funds, benefits, drug and medical services lists, health providers, and management by the merging of administrative resources of NCMS with URBMI, such as offices, staff, and information systems (Pan et al., 2016; Wang, 2018). Specifically, the Ministry of Human Resources and Social Security (subordinates of MoHRSS) and the Municipal Department of Health were responsible for the NCMS and URBMI administration. After the integration, MoHRSS becomes solely responsible for the medical insurance administration (Huang & Wu, 2020).

The administration improvement induced by the URRBMI reform has reached many of the insured individuals. For example, it doubled the number of covered drugs in Shandong, Guangdong, and Ningxia and tripled in Tianjin and Inner Mongolia. It increased the benefit levels and reimbursement rates to meet medical needs, meanwhile providing quick and simple steps to get medical treatment and reimbursement. The expansion of the funds benefited medical insurance authorities in negotiating with pharmaceutical suppliers, distribution sectors, and medical service agencies. Furthermore, unified health provider selection is conducive to evaluating and improving healthcare quality. A detailed explanation of these benefits can be found in Li et al. (2017).

However, and essential for our analysis, the URRBMI system was introduced in different provinces at different times. Chongqing was the earliest adopter in 2007 but it

was not until 2019 that all provinces of Mainland China had introduced the new system. Table 1 shows how the speed of reform varied across provinces.

Table 1 Introduction of the URRBMI policy by provinces

Province	Year	Province	Year	Province	Year
Chongqing	2007	Guizhou	2016	Shaanxi	2016
Tianjin	2009	Hainan*	2016	Shanxi	2016
Ningxia*	2010	Hebei	2016	Sichuan	2016
Guangdong	2012	Heilongjiang	2016	Xinjiang	2016
Qinghai*	2013	Henan	2016	Yunnan	2016
Shandong	2013	Hubei	2016	Zhejiang	2016
Shanghai	2015	Hunan	2016	Beijing	2017
Anhui	2016	Inner Mongolia*	2016	Tibet*	2018
Fujian	2016	Jiangsu	2016	Liaoning	2019
Gansu	2016	Jiangxi	2016		
Guangxi	2016	Jilin	2016		

Sources: from the local governments' official websites.
* Not available in the CFPS dataset

3. Methodology

Difference-in-differences (DD) has become one of the most popular methods to evaluate the causal effect of political interventions. In its canonical form, DD considers two periods and two groups. While no group is treated in the first period, only one group receives treatment in the second period. In this setting, the average treatment effect for the treated population (ATT) is obtained by comparing the output change in the two periods for the treated and control groups; see Heckman et al. (1998); Heckman et al. (1997) and Abadie (2005), among many others.

However, extending the canonical DD design to settings with more than two time periods and variations in treatment timing is complicated. More specifically, this study focuses on estimating the health effect of the URRBMI extension that occurred in different Chinese provinces at different points in time. It is a staggered design because,

once treated, each province remains treated for the whole analysis period. In our longitudinal panel, the unit of analysis is the individual child. Thus, the focus is on estimating the health impact of the URRBMI extension on Chinese children compared to the traditional NCMS and URBMI schemes. Accordingly, our treatment group includes insured individuals under 16 in provinces that have adopted the URRBMI extension. In contrast, the control group includes insured individuals of the same age living in provinces that had not yet implemented it.[6]

3.1. An event study with static effects

We start our analysis with a two-way fixed effects (TWFE) specification. In this case, the treatment, $D_{i,t}$ is a binary variable indicating that the i^{th} individual lives in a province that had already adopted the URRBMI extension by period *t*. Using the definition, the TWFE takes the following form:

$$Y_{i,t} = \gamma_i + \lambda_t + \beta D_{i,t} + u_{i,t} \quad (1)$$

where $Y_{i,t}$ denote the health outcome for individual i at time *t*, γ_i and λ_t are individual and time fixed effects, respectively, and $u_{i,t}$ is the error component. Note that specification (1) allows estimating the total impact of the reform.

Despite the popularity of the TWFE approach, as discussed by de Chaisemartin and D'Haultfoeuille (2020) and Callaway and Sant'Anna (2021), the TWFE estimate of β in (1) is a weighted average of different DD estimates comparing different early-treated to later-treated units. It is especially problematic that some of the weights may be negative, which can result, for example, in negative estimates of β even if the true ATE

[6] This study is at the individual, rather than the province, level. Similarly, Callaway and Sant'Anna (2021) estimate the impact of minimum wages (set at the state level) on US counties. Another similar approach based on individuals (but using traditional DD models) was employed by Huang and Wu (2020).

or ATT are positive for all units.

3.2. Aggregating group-time average treatment effects

3.2.1. The approach in Callaway and Sant'Anna (2021)

Callaway and Sant'Anna (2021) propose an alternative to TWFE for DD with staggered design based on the aggregation of group-time average treatment effects. For the convenience of exposition, we denote this method by CS hereafter. For a simple description of this approach, we define a binary variable, $G_{i,g}$, that takes the value one when individual i is first treated in period g, i.e. in the first period when individual i under the new regime. The health outcome of individual i at time t (the date of the interview) if it is not yet affected by the reform is denoted by $Y_{i,t}(0)$. Thus, following Callaway and Sant'Anna (2021), the observed and potential outcomes for each individual i can be expressed

$$Y_{i,t} = Y_{i,t}(0) + \sum_{g=2}^{T} \left(Y_{i,t}(g) - Y_{i,t}(0)\right).G_{i,g} \quad (2)$$

As it is typical in observational studies, our analysis is affected by the fundamental missing data problem (Holland, 1986) as we can observe only one potential outcome path for each individual. Callaway and Sant'Anna (2021) define a natural generalisation of the average treatment effect for the treated subpopulation (ATT) to setups with multiple treatment groups and multiple periods as

$$ATT(g,t) = E\left[Y_t(g) - Y_t(0) | G_g = 1\right] \quad (3)$$

The identification of $ATT(g,t)$ relies on three basic assumptions that can be interpreted as a generalisation of the canonical DD with two periods. The first one is

limited treatment anticipation. This assumption is weaker than the no-anticipation assumption. It allows for some degree of anticipatory behaviour, for example, two years before, as long as we have a good understanding of the anticipation horizon. The second assumption is the conditional parallel trend based on "Not-Yet-Treated" Groups.[7] This hypothesis states that, after conditioning on covariates, trend differences in health output between treated and untreated individuals are only explained by the effect of treatment. Finally, the overlap assumption restricts treatment probabilities from taking extreme values.

Based on these assumptions, we estimate different ATTs (g, t) using an improved doubly robust (DR) approach.[8] More specifically, this estimator applies the inverse probability of tilting[9] to weight observations. It involves estimating propensity scores for each individual and time, using pretreatment covariates and including the same set of variables in the subsequent regression (weighted by the inverse probability of treatment) for health outcomes. Although the expected ATT under the DR approach is identical to other methods such as weighted least square outcome regression or inverse probability of tilting, Callaway and Sant'Anna explain that, compared to these alternatives, the DR estimator usually enjoys additional robustness against model-misspecifications. Based on this, the present study employs the DR approach.

The estimated ATTs (g, t) are aggregated for different events and periods using positive

[7] Note that there are no "Never-Treated" Groups in our study.
[8] This analysis was conducted using the default method (drimp) of the csdid package in Stata.
[9] Inverse probability tilting (IPT) is equivalent to the inverse probability weighting (IPW) estimator, except that it replaces the conditional maximum likelihood estimate of the propensity score with an alternative method of moments estimate. We employ the method of unconditional moments to estimate propensity score parameters because it is locally efficient and remains consistent even if the propensity score is misspecified. These properties cannot be found in standard propensity score methods ((Graham et al., 2012).

weights that sum up to one following CS. This method tackles troubling issues associated with negative weights.

3.2.2. The approach in de Chaisemartin and D'Haultfoeuille (2020)

For robustness, we also estimate the ATE of the URRBMI extension using the approach proposed by de Chaisemartin and D'Haultfoeuille (2020) and later extended by de Chaisemartin and D'Haultfoeuille (2022) for instantaneous and dynamic effects. This approach is denoted by CD hereafter. This methodology provides an alternative way to deal with the negative weight problem in standard TWFE regressions when the ATEs are heterogeneous across groups or periods. Unlike CS, CD do not estimate propensity scores to make treatment and control groups comparable in terms of observables. Instead, CD is more general as it can be applied to any TWFE regression, rather than only those with staggered design. Their estimation relies on the existence of stable groups. Given that our focus is on a staggered design, in our setting, this condition only requires that, between each pair of consecutive periods, if there is a joiner, i.e., a province switching from being untreated to being treated, then there should be at least another province that is untreated in both periods. Based on this condition, we can compute the DDs estimates between any two periods. Then, the final ATE estimate is obtained as a weighted average of the DDs using the *fuzzydid* and *did_multiplegt* Stata packages. Although CD mainly focus on unconditional DD designs, they show it is also possible to extend the analysis to conditional DD designs and sharp and fuzzy designs.

4. Data

This study uses data from the China Family Panel Studies (CFPS) conducted by the Institute for Social Science Survey (ISSS) of Peking University. It officially launched

its baseline survey in April 2010, and full-scale follow-up interviews took place every other year, with the last one happening in 2020. It includes all family members in a representative group of households in 30 provinces in Mainland China.

The CFPS provides information at individual, family and community levels. We aim to evaluate the impact of the insurance extension on those individuals who already had either the NCMS or URBMI schemes. For that purpose, we restrict our attention to individuals younger than sixteen who ever appeared in the sample and were already covered by rural or urban social medical insurance in the survey year (on average, this represents 70% of the population under 16). It is a longitudinal database such that the same individual can be identified in different years. Still, some individuals may disappear from the data set for reasons such as migration, death or attainment of majority. Furthermore, new individuals are included in the sample each wave for reasons such as marriage or divorce happening in their family. The CFPS sample is self-renewing, based on the natural changes of the baseline Chinese families. Still, these situations can be deemed to be exogenous in our analysis. More detailed information about the CFPS database can be found in Xie and Hu (2014).

Response variable:

The response variable is health condition measured by sick frequency in the last month (denoted by *Sickfreq*). Sick refers to feeling physically uncomfortable and receiving treatment through medicine or other methods reported by the child's parents. Fig. 1 shows the distribution of the response variable in the treatment and control groups. As expected, the young Chinese population is generally healthy and not affected by health issues. More specifically, roughly 70% of individuals in both groups were not sick in

the previous month.

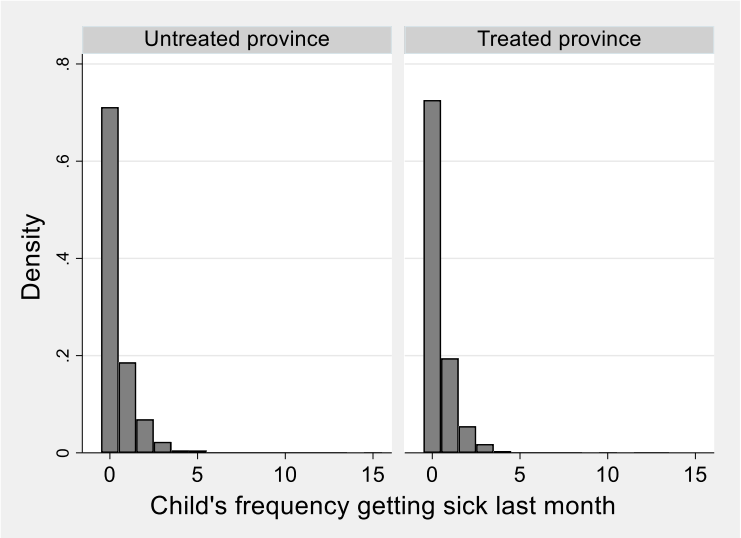


Fig. 1 Distribution of response variables for individuals in treated and untreated provinces
Note: Treatment refers to the implementation of the URRBMI extension.

Treatment variable:

This variable indicates whether the individual lives in a province that has implemented the URRBMI system. Consistently with expression (2), we define groups by the period when the individuals living in the province first applied the URRBMI extension in the year of the interview. We use individuals in not yet treated provinces as the control group. All provinces are eventually treated during the analysis period.

Control variables:

We employ two big groups of control variables: personal characteristics and socio-economic indicators. Personal characteristics variables include *Age*, *Squared age*, *Male*, *Han*, *Education level*, *Height* and *Weight*. *Age* is a potential determinant of health because younger children may have a more deficient immune system. Moreover, parents may make different insurance purchase decisions according to their children's

age. *Squared age* captures the non-linear effect of *Age*. We include a male indicator (*Male*) to account for gender differences in health outcomes. For example, males may be more involved in risky behaviour, or females' mortality rate may be higher due to environmental disadvantages in remote areas (Waldron, 1983). Therefore, in principle, the influence of *Male* on health is ambiguous. *Han* is a binary variable that takes the value one for Han people (the largest group) and zero for the remaining groups. Non-Han people usually live in underdeveloped provinces with relatively primitive medical facilities, which may lead to less use of health care and poor health status (Li, 2021). There are three education categories, indicating the highest education level completed, for Illiterate/Semi-literate; Primary school, Junior high school, and above. Thus, two dummy variables are generated for Primary school and Junior high schools with Illiterate/Semi-literate as the control group. Finally, *Height* and *Weight* are traditionally important indicators of children's health (Lobstein et al., 2015).

Regarding socio-economic indicators, we include *Education cost* (k Yuan); *Registration type* (*Hukou*); *Urban area*; *Father education level*; *Mother education level*; and *Commercial insurance*. *Education cost* is measured by child's total educational expenditure in the past 12 months. Typically, schools with high tuition fees offer special commercial insurance for their students. This variable could affect health outcomes in two possible ways. First, a greater education cost may increase the financial burden of the families; therefore, they may reduce their expenditure on insurance products. A second possibility is that a higher education cost indicates that children come from higher social status and, therefore, they have the financial ability to get more insurance coverage. Two dummy variables, *Hukou* and *Urban area*, denote agriculture registration and current urban location, respectively. Interestingly, they identify the

origin and the present child's location, which is relevant given the two different types of insurance schemes described in Section 2. *Father education level* and *Mother education level* indicate years of education of the children's father and mother. These variables significantly predict children's health outcomes according to previous research (Desai & Alva, 1998). *Commercial insurance* is measured by whether or not the individual has commercial health insurance. It is also expected to affect health outcomes.

A typical microdata issue is the existence of missing values in different observations for different regressors. It imposes a severe limitation on the degrees of freedom of the regression. Due to this problem, we apply the EM algorithm to tackle data irregularities (Graham, 2009). Following Von Hippel (2007), we exclude response variables from the imputation model since the correlation between the control and response variables can contaminate our imputation results. In general, imputation increases the number of observations without significantly impacting variable characteristics. Table 2 presents the sample means using the dataset after and before imputation, the number of observations of each variable before imputation, and the differences in sample means. The imputation does not significantly alter the mean of the variables at the conventional levels for most of the variables except for *Height*, *Education cost*, *Father education level and Mother education level*. However, even in these cases the differences in magnitudes are almost negligible. For instance, the mean difference of *Height* is less than one centimetre before and after imputation. For this reason, reported estimates in the following sections are based on imputed data. However, results are not materially

affected by this consideration.[10]

Table 2 Summary statistics before and after imputation. Total number of observations after imputation is 33306.

	(1)	(2)	(3)	(4)	(5)
Variables	Mean (after impu.)	Mean (before impu.)	#Obs. (before impu.)	Mean Diff.	t_value(1)
Sickfreq	0.417	0.417	33306	0.000	0.00
Age	7.83	7.831	33305	0.000	0.00
Squared age	79.242	79.245	33305	-0.004	0.00
Male	0.535	0.535	33306	0.000	0.00
Han	0.876	0.879	33264	-0.003	-1.25
Education level					
Primary school	0.168	0.168	33151	0.000	-0.05
Junior & above	0.025	0.025	33151	0.000	-0.10
Height (cm)	121.056	120.298	31986	0.758***	3.20
Weight (0.5kg)	54.472	54.484	32404	-0.013	-0.05
Exp_edu (k Yuan)	3.103	3.282	29256	-0.178***	-3.55
Hukou	0.806	0.806	33286	0.000	0.00
Urban area	0.411	0.413	32744	-0.002	-0.55
Edu_father	8.745	8.866	21904	-0.121***	-3.50
Edu_mother	7.828	7.934	23285	-0.106***	-2.80
Commercial ins.	0.160	0.160	33115	0.000	-0.05

Notes: (1) Welch's t-test
* p<0.1 ** p<0.05 *** p<0.01

5. Results

5.1. The effect of the URRBMI policy on sick frequency

We initiate the empirical analysis with the estimation of the overall effect of the URRBMI policy on children's health. Table 3 shows the estimated causal effects employing the CS and CD methods as well as the TWFE estimation. For completeness, we have run the TWFE model in expression (1) with and without the set of confounding variables defined in the data section. Interestingly, the beneficial effect of the reform is not found under the TWFE regression in expression (1). However, as explained in

[10] In particular, it does not change the main conclusions of our analysis in Sections 5 and 6 about the overall positive and significant impact at the conventional level of social insurance on health outcomes and participation. However, due to the lower number of observations, the dynamic causal estimates are not significant in some cases.

Section 3.1, this regression has no clear causal interpretation. The CS approach allows for the ATT estimation from a simple weighted average and the aggregation of group-, event-, and calendar-specific effects. To evaluate the influence of the set of confounders defined in the Data section in Table 2 on the final estimates, we show the estimation conditional and unconditional results. Results overwhelmingly show a significant negative effect of the URRBMI extension on sick frequency except for the unconditional aggregated calendar effect. Such an estimate is still negative but not significant at the conventional levels.

The CD approach obtains estimates from the aggregation of event effects. Both conditional and unconditional results align with the CS estimates suggesting a negative and significant impact of the URRBMI extension on sick frequency. This approach also shows two placebo estimates that compare outcome trends of individuals in the treated and control provinces for one and two periods before the first survey year when the provinces were treated. Both placebos are insignificant at the conventional levels providing evidence that the null hypothesis of parallel trends can be accepted.

Table 3 Aggregate effect of the URRBMI extension on children's sick frequency.

	Unconditional	Conditional
TWFE (1)	0.002 (0.03)	0.003 (-0.05)
CS Event study (2)	-0.207 (-3.10) ***	-0.197 (-3.24) ***
CS Simple weighted (2)	-0.108 (-2.02) **	-0.134 (-2.08) **
CS Group effect (2)	-0.105 (-1.85) *	-0.134 (-2.01) **
CS Calendar effect (2)	-0.063 (-1.44)	-0.079 (-1.65) *
CD Event study (3)	-0.127 (-2.21) **	-0.125 (-2.21) **
CD Placebo1 (3)	-0.051 (-0.500)	-0.056 (-0.545)
CD Placebo2 (3)	0.217 (1.472)	0.213 (1.380)

Notes: (1) Estimated coefficients on a post-treatment dummy variable from a two-way fixed effects regression with clustered standard errors at the province and wave levels. (2) ATT estimates using the approach in Callaway & Sant'Anna (2021) approach. (3) ATE estimates using the approach in de Chaisemartin & D'Haultfœuille (2022). In the Callaway & Sant'Anna (2021) estimates, the row 'Simple weighted' reports the weighted average (by group size) of all available group-time average treatment effects. The row 'Group effect' summarises average treatment effects by the timing of the implementation of the extension of medical insurance. The row 'Event study' reports average treatment effects by the length of exposure to the implementation to the extension of medical insurance. The row 'Calendar effect' reports average treatment effects by year. The conditional estimation uses the improved doubly robust estimator. The approach in de Chaisemartin & D'Haultfœuille (2022) shows the estimated aggregated ATE. Placebo1 and Placebo 2 correspond to the ATEs estimated one and two years before the implementation of the URRBMI extension. t and z statistics in parentheses.
* $p<0.1$ ** $p<0.05$ *** $p<0.01$

Although the previous analysis answers the central question of this research about the aggregate impact of the URRBMI extension, a dynamic analysis provides additional information about how each event period contributes to the aggregate effects. Fig. 2 shows the event-specific estimates using the CS and CD methods. It should be noted that the CS approach employs propensity scores to compare individuals in the treated and control group at different periods. However, the CD approach compares joiners and not-joiners individuals in each pair of consecutive waves. For this reason, there is a different number of periods in the x-axis of Fig. 2 under the two methods. Despite these differences, both the CS and CD estimates indicate a predominantly negative impact of the URRBMI extension on sick frequency.[11] Interestingly, the reaction is slow and it only becomes apparent after 4 years (2 waves). One possible explanation is that health outcome needs some time to react to the new policy. It should also be noticed that this dynamic analysis is not without limitations, as one of the unconditional pretreatment periods (-4) under the CS approach is significantly different from zero at the conventional levels.[12]

[11] Note that the reported CS estimates define the first year of being treated using the first time they appear in the biannual CFPS waves. However, in a nonreported exercise, rather than using the survey years, we define the first year of being treated according to Table 1 to get static and dynamic estimates under the CS estimation approach. Results are not materially affected by this consideration.

[12] A similar concern was also present in Callaway and Sant'Anna (2021) in their estimation of the effect of minimum wage policy decisions on teen employment.

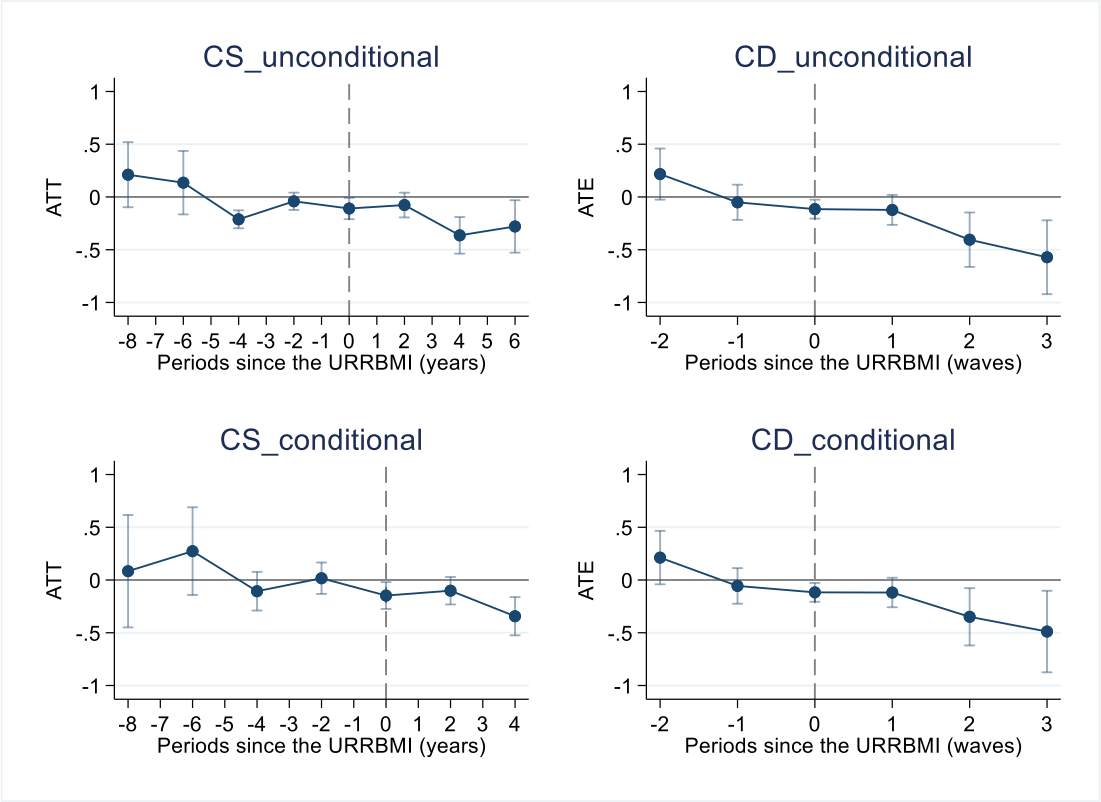


Fig. 2 Dynamic effects of URRBMI extension on sick frequency: Event specific causal estimates using the CS and CD methods

Blue lines provide point estimates and 90% confidence bands for the treatment effect.

5.2. The effect of the URRBMI policy on participation

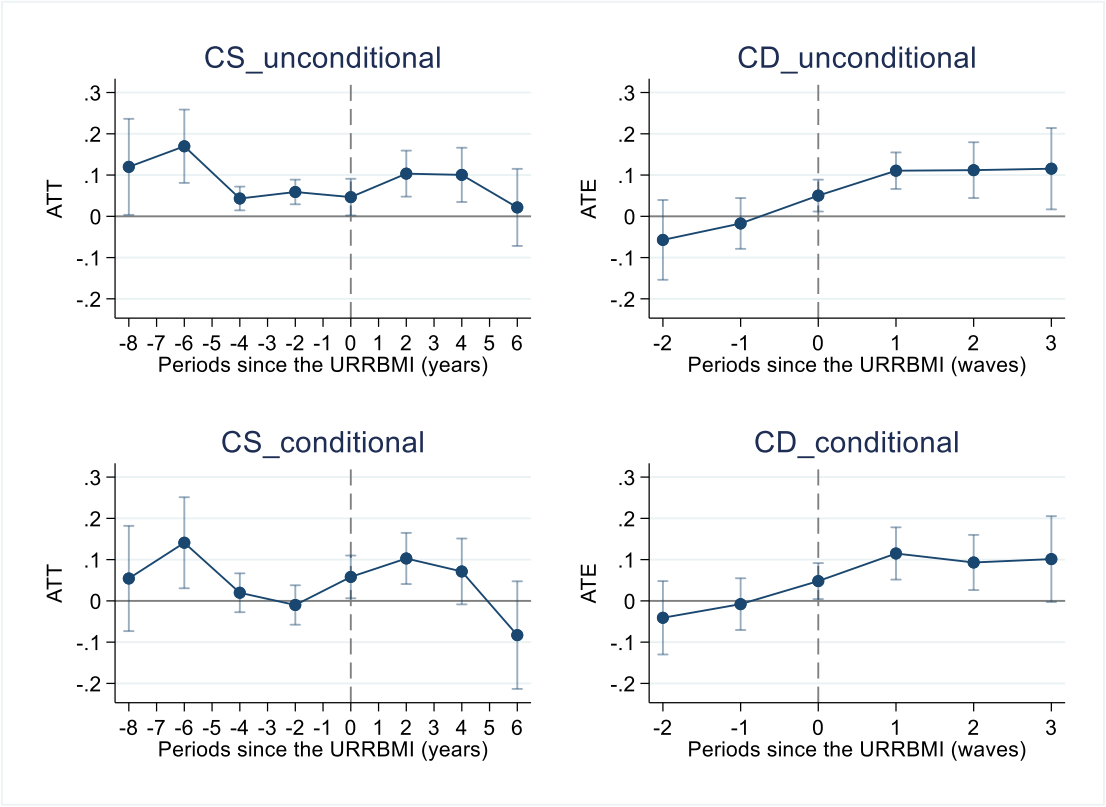
The previous analysis is based on comparing the health outcomes of insured individuals before and after applying the URRBMI system. However, such policy decisions could also affect individual incentives to participate, providing additional health benefits to the province. Therefore, using the whole child population sample (insured and non-insured), we estimate the impact of the URRBMI policy on the individual decision to participate under the CS and the CD approaches. We employ a dichotomous response variable for this analysis that takes values 1 and 0 for participating and non-participating individuals.

Table 4 reports the estimation results. It stands out that TWFE conditional estimates show an insignificant impact of the URRBMI policy on the probability of participation. This result does not agree with the CS and CD estimates that show a positively significant effect of the URRBMI policy on the probability of participating. We observe a nonsignificant calendar effect and event effect under the CS approach that could be explained by the URRBMI extension having a less clear long-term effect, as shown in Fig. 3. This is reasonable as the coverage rate of social medical insurance has been increasing over the years, and the long-term impact could shrink with the improving coverage rate. Moreover, the placebo estimates in CD indicate that the increase in participation associated with the URRBMI policy could not have been anticipated in the previous two years. Overall, these results suggest an additional beneficial effect of URRBMI as it not only improves the health outcomes of NCMS and URBMI insurance individuals but also incentivises the probability of participating in social medical insurance.

Table 4 Aggregate effect of the URRBMI extension on children being insured.

	Unconditional	Conditional
TWFE (1)	-0.036 (-0.72)	-0.036 (-0.72)
CS Event study (2)	0.068 (2.42)**	0.037 (1.05)
CS Simple weighted (2)	0.072 (2.81)***	0.073 (2.44)**
CS Group effect (2)	0.085 (3.05)***	0.090 (2.77)***
CS Calendar effect (2)	-0.001 (-0.07)	-0.011 (-0.53)
CD Event study (3)	0.079 (3.82)***	0.078 (2.80)***
CD Placebo1 (3)	-0.017 (-0.46)	-0.008 (-0.21)
CD Placebo2 (3)	-0.057 (-0.97)	-0.041 (-0.76)

Notes: (1) Estimated coefficients on a post-treatment dummy variable from a two-way fixed effects regression with clustered standard errors at the province and wave levels. (2) ATT estimates using the approach in Callaway & Sant'Anna (2021) approach. (3) ATE estimates using the approach in de Chaisemartin & D'Haultfœuille (2022). In the Callaway & Sant'Anna (2021) estimates, the row 'Simple weighted' reports the weighted average (by group size) of all available group-time average treatment effects. The row 'Group effect' summarises average treatment effects by the timing of the implementation of the extension of medical insurance. The row 'Event study' reports average treatment effects by the length of exposure to the implementation to the extension of medical insurance. The row 'Calendar effect' reports average treatment effects by year. The conditional estimation uses the improved doubly robust estimator. The approach in de Chaisemartin & D'Haultfœuille (2022) shows the estimated aggregated ATE. Placebo1 and Placebo 2 correspond to the ATEs estimated one and two years before the implementation of the URRBMI extension. t and z statistics in parentheses.
* p<0.1 ** p<0.05 *** p<0.01.



Blue lines provide point estimates and 90% confidence bands for the treatment effect.

Fig. 3 Dynamic effects of URRBMI extension on insurance participation: Event specific causal estimates using the CS and CD methods

6. Transmission mechanisms

As discussed in Section 2, adopting the URRBMI policy improved the allocation of health resources. Thus, the policy could improve individuals' health by providing healthcare services (Lei & Lin, 2009) and reducing medical expenses (Agüero & Valdivia, 2010; Arkes, 2007; Catalano et al., 2003). Rather than focusing on institutional decisions, this section conducts a mechanism study by looking into two individual reactions to URRBMI: (1) hospital use and (2) out-of-pocket expenditure. To perform such analysis, we measure hospital use by hospital visit frequency due to illness in the last month and out-of-pocket expenditure by the amount of payment for the medical expenditure of the child (yuan) in the past year, excluding reimbursed or

will-be-reimbursed costs. Table 5 displays the results of this analysis.[13]

Focusing on the CS and CD estimation, the URRBMI extension has significantly reduced out-of-pocket expenditure by around 800 yuans. A decline in medical expenses increases individuals' disposable income, which may further improve individuals' health by enlarging health investment (Kawachi et al., 2010; Lee et al., 2021). However, the impact of URRBMI on health through its effect on hospital use is insignificant at the conventional values. While the latter estimation result is clear, its interpretation is purely speculative. Therefore, a plausible explanation is that improvements in primary health attention could also make the use of hospitals less necessary. However, it is also possible that the estimation result simply reflects that the accessibility and quality of hospitals take longer to improve (Thaddeus & Maine, 1994; Yang et al., 2015). Further research employing a more detailed database is required to clarify this debate.

Table 5 Aggregate effect of the URRBMI extension on children's hospital utilisation and out-of-pocket expenditure. Conditional estimates.

	Hospital use	Out-of-pocket expenditure (yuan)
TWFE (1)	0.029 (0.25)	-56.666 (-0.37)
CS Event study (2)	0.057 (0.29)	-797.036 (-2.31)**
CS Simple weighted (2)	0.044 (0.23)	-784.523 (-2.22)**
CS Group effect (2)	0.044 (0.23)	-796.964 (-2.26)**
CS Calendar effect (2)	0.057 (0.29)	-371.423 (-1.80)*
CD Event study (3)	-0.134 (-0.68)	-557.351 (-2.17)**
CD Placebo1 (3)	-0.279 (-0.70)	665.948 (1.37)

[13] Although results are not affected by non-inclusion of confounding variables in the econometric models, for conciseness, Sections 6 and 7 only report conditional estimates.

CD Placebo2 (3)	Na	-724.131 (-0.47)

Notes: (1) Estimated coefficients on a post-treatment dummy variable from a two-way fixed effects regression with clustered standard errors at the province and wave levels. (2) ATT estimates using the approach in Callaway & Sant'Anna (2021) approach. (3) ATE estimates using the approach in de Chaisemartin & D'Haultfœuille (2022). In the Callaway & Sant'Anna (2021) estimates, the row 'Simple weighted' reports the weighted average (by group size) of all available group-time average treatment effects. The row 'Group effect' summarises average treatment effects by the timing of the implementation of the extension of medical insurance. The row 'Event study' reports average treatment effects by the length of exposure to the implementation to the extension of medical insurance. The row 'Calendar effect' reports average treatment effects by year. The conditional estimation uses the improved doubly robust estimator. The approach in de Chaisemartin & D'Haultfœuille (2022) shows the estimated aggregated ATE. Placebo1 and Placebo 2 correspond to the ATEs estimated one and two years before the implementation of the URRBMI extension. t and z statistics in parentheses. Na: not available due to the small number of observations.
* p<0.1 ** p<0.05 *** p<0.01. Na: estimates not available due to a low number of observations.

7. Extended analysis

We further test whether previous estimates of the impact of the URRBMI policy on sick frequency are robust to sample selection and the inclusion of month-specific effects aiming to control for the seasonality of some diseases. We also study the differential impact of this policy on urban and rural children.

Regarding the first issue, sample selection could underestimate the impact of the URRBMI policy if it does not consider the additional beneficial effect of the policy through increasing participation. To account for this concern, we followed the two-stage estimation procedure proposed by Heckman (1974). Thus, in the first stage, we estimate the Mills ratio for insurance participation using father and mother social medical insurance participation as the instrumental variables. Then, in a second stage, we repeat the CS and the CD analysis adding the estimated Mills ratio to the set of covariates to correct for the different individual probabilities of participating. Estimation results are shown in the first column of Table 6. The inclusion of the Mills ratio does not alter our previous results about the impact of the URRBMI extension on children's health. The second column of Table 6 also indicates that our conclusions are not affected by the inclusion of monthly dummy variables in the model.

Table 6 Aggregate effect of the URRBMI extension on children's health (extended analysis). Conditional estimates.

	(1)	(2)	(3)	(4)
	Heckman correction	Month dummies	Urban	Rural
TWFE [(1)]	-0.002 (-0.04)	-0.002 (-0.05)	-0.041 (-0.95)	0.023 (0.34)
CS Event study [(2)]	-0.197 (-3.56)***	-0.125 (-2.15)**	-0.214 (-2.40)**	-0.005 (-0.05)
CS Simple weighted [(2)]	-0.145 (-2.47)**	-0.162 (-2.67)***	-0.211 (-2.43)**	-0.003 (-0.03)
CS Group effect [(2)]	-0.146 (-2.41)**	-0.171 (-2.66)**	-0.211 (-2.45)**	0.010 (0.10)
CS Calendar effect [(2)]	-0.086 (-1.88)*	-0.086 (-1.71)*	-0.144 (-2.08)**	-0.018 (-0.18)
CD Event study [(3)]	-0.126 (-2.21)**	-0.119 (-2.05)**	-0.187 (-1.83)*	-0.087 (-1.14)
CD Placebo1 [(3)]	-0.056 (-0.55)	-0.066 (-0.64)	-0.056 (-0.40)	0.013 (0.08)
CD Placebo2 [(3)]	0.205 (1.32)	0.216 (1.43)	0.025 (0.08)	0.146 (0.65)

Notes: (1) Estimated coefficients on a post-treatment dummy variable from a two-way fixed effects regression with clustered standard errors at the province and wave levels. (2) ATT estimates using the approach in Callaway & Sant'Anna (2021) approach. (3) ATE estimates using the approach in de Chaisemartin & D'Haultfœuille (2022). In the Callaway & Sant'Anna (2021) estimates, the row 'Simple weighted' reports the weighted average (by group size) of all available group-time average treatment effects. The row 'Group effect' summarises average treatment effects by the timing of the implementation of the extension of medical insurance. The row 'Event study' reports average treatment effects by the length of exposure to the implementation to the extension of medical insurance. The row 'Calendar effect' reports average treatment effects by year. The conditional estimation uses the improved doubly robust estimator. The approach in de Chaisemartin & D'Haultfœuille (2022) shows the estimated aggregated ATE. Placebo1 and Placebo 2 correspond to the ATEs estimated one and two years before the implementation of the URRBMI extension. t and z statistics in parentheses.
* p<0.1 ** p<0.05 *** p<0.01.

In the final experiment, we conducted separate estimations for children living in urban and rural areas to identify which group benefited more from the URRBMI extension. The third and fourth columns of Table 6 present the aggregate effects indicating that urban children benefitted more from the URRBMI extension than rural children. We also find some weak evidence of health improvement among rural children, which is not significant at the conventional values. This differential impact might be explained because rural areas take more time to improve their medical and health outcomes, probably due to the lack of health infrastructure structure or information networks.

We conduct a dynamic analysis to further explore the less significant results for rural children. Fig. 4 shows these estimates suggesting that rural children can also benefit from the URRBMI extension even if the policy takes longer to reach benefits. Overall, we find that both rural and urban children benefited from the URRBMI extension.

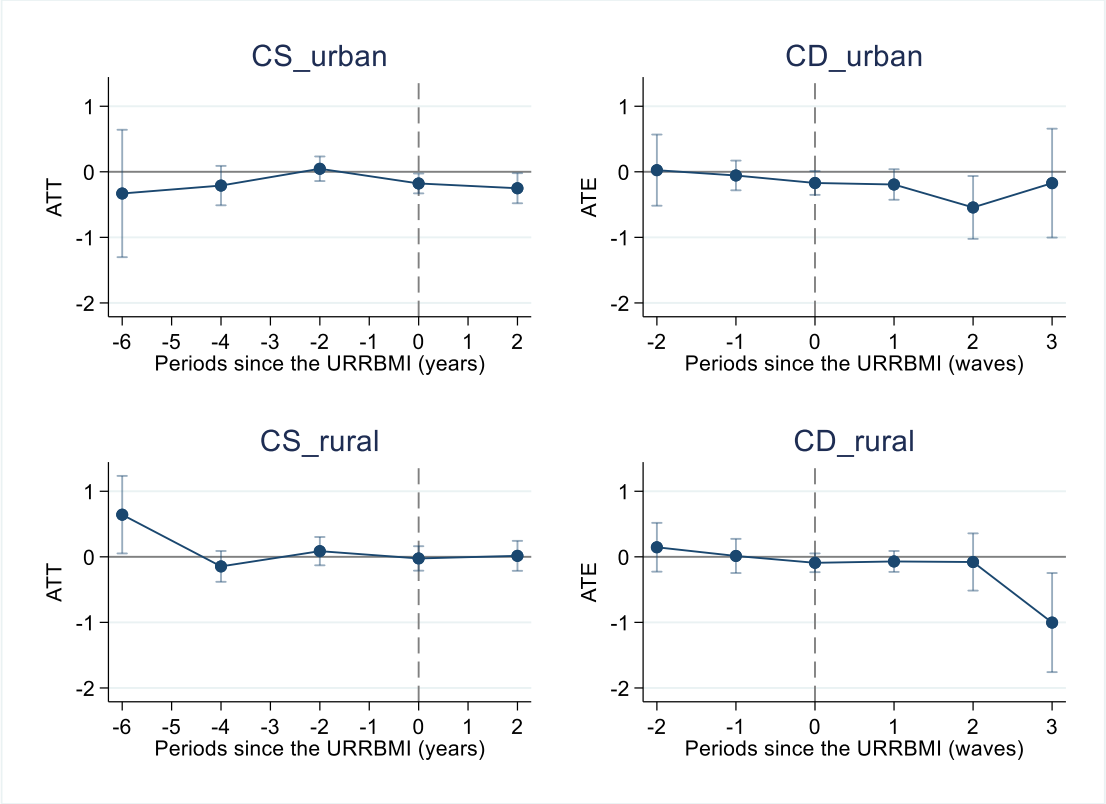


Fig. 4 Dynamic effects of URRBMI extension on urban and rural children's health outcome: Event specific causal estimates using the CS and CD methods. Conditional estimates.
Blue lines provide point estimates and 90% confidence bands for the treatment effect.

8. Discussion

The URRBMI policy has integrated the access to health of urban and rural people, making medical services more accessible to all individuals. The gradual implementation of the URRBMI policy across Chinese provinces facilitates causal identification because the experiment is repeated several times. Thus, it reduces the possibility of misattribution due to unrelated shocks at the time of intervention. However, employing TWFE for this analysis can bias estimation results as control and treatment groups differ across the analysis period.

To tackle this problem, we took advantage of the approaches proposed by the CS and

the CD, finding that the URRBMI policy reduces sick frequency and increases insurance participation. The health effect is significant at the conventional levels using both the CS and the CD approaches. Moreover, its magnitude is also substantial, representing around 0.125-0.25 of a standard deviation (around 0.1-0.2 divided by 0.8, the standard deviation of the response variable). An additional dynamic analysis (Fig. 2) indicates that the positive impact on health is sustained in the subsequent periods. Moreover, the placebo tests proposed by CD indicate that this effect could not have been anticipated in the previous two years.

We also found that the URRBMI policy contributed to increased insurance participation in the regions where it was implemented. However, this effect is only robust under the CD approach, which finds an 8% increase in participation due to the policy. In contrast, the CS approach shows different estimated magnitudes depending on the aggregation scheme. Although we found evidence in the previous literature that URRBMI is associated with better use of resources, increase in the number of covered drugs and benefits levels and reimbursement rates, we also explored the transmission mechanism of this policy through its effect on individual behaviour. In particular, we found that URRBMI affected health by reducing medical expenses, while its impact on hospital use was insignificant at the conventional levels. Estimation results were robust to the inclusion of controls for sample selection (Heckman, 1974) and monthly dummy variables. We also found that the impact of the URRBMI extension has a more significant aggregate effect on children's health in urban than in rural areas. However, the positive impact on rural children takes longer to emerge. This suggests that improving the level of medical service facilities and services is the key to the next step of health policy reform.

9. Future research

We hope the present paper incentivises future research about the effect of medical insurance extension in a setting where its implementation is staggered and heterogeneous. The new methods proposed by CS and CD constitute a change in paradigm in the analysis of policies with staggered implementation that could modify established results in the empirical literature.

A promising research avenue to extend the present analysis is to study the impact of different levels of medical insurance and their interactions with the characteristics of the population and other health decisions. For example, it could analyse the effect of the dramatic changes in recent years due to the ageing of the population, improvement in living standards, the development of the commercial health insurance market, and demographic policy, i.e., moderation of China's one-child policy. Specifically, part of the funding from social medical insurance is used to develop long-term care insurance (Feng et al., 2020). Furthermore, it should capture the impact of policies on the increasing role that commercial health insurance plays as a supplement to social medical insurance. For instance, the government promotes the development of inclusive medical insurance with a low price and zero restriction of age, occupation, household registration (hukou) and previous health conditions for people covered by social medical insurance (Yu et al., 2021). Studying the overall effect of these changes should be fundamental to guiding future policy decisions. However, this analysis would need a different type of data and the requirement to deal with new identification challenges to tackle endogeneity.

Another avenue of research could consider evaluating different types of childhood

illnesses, including mild and serious ones, and hospital use behaviour (i.e., selection of hospitals at different levels and length of hospitalisation). Thus, it is possible that a significant impact of social medical insurance could be found for a particular disease which could be especially relevant for decision-makers. Again, a more detailed database would be needed for such analysis.

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