2022

THE IMPACTS OF PUBLIC POLICIES ON HEALTH-RELATED OUTCOMES

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Digital Object Identifier: https://doi.org/10.13023/etd.2022.337

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THE IMPACTS OF PUBLIC POLICIES ON HEALTH-RELATED OUTCOMES

DISSERTATION

A dissertation submitted in partial fulfillment of the requirements for the degree of Doctor of Philosophy in the College of Business and Economics at the University of Kentucky

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ABSTRACT OF DISSERTATION

THE IMPACTS OF PUBLIC POLICIES ON HEALTH-RELATED OUTCOMES

This dissertation consists of three chapters that study how public policies affect health-related outcomes in China. The first two papers examine the impacts of the country's Critical Illness Insurance program on health-related outcomes. The third paper studies the effects of maternal education on children's early childhood health outcomes and cognitive skills.

The first chapter evaluates the impact of a recent social health insurance benefits expansion program on middle-aged and elderly individuals. In 2012, China launched a Critical Illness Insurance (CII) program, which aimed to reduce out-of-pocket costs for individuals covered by the government's health insurance programs. This chapter utilizes the staggered timing of CII adoption across cities to estimate the impact of CII on medical costs and health services for middle-aged and elderly individuals. For rural insured people, I find that CII had a negligible impact on the likelihood of using inpatient care but decreased out-of-pocket inpatient costs by 45% for those who did receive inpatient care. The reductions are largest for individuals over 60 years old, those who live in poor households, and enrollees with chronic illnesses. I do not find evidence of similar effects on urban insured residents' inpatient and outpatient expenditures or utilization, which is expected since the urban insured are less affected by the CII program, given the higher pre-existing benefits in the urban insurance program.

The second chapter examines the impact of CII on health outcomes, health service utilization, and risky health behaviors. I explore the staggered timing of CII adoption across the cities and utilize generalized difference-in-differences and event-study models to provide the first estimates of CII's health and behavioral impacts. Using a nationally representative longitudinal dataset, I show that CII has little effect on self-reported health, probability of getting diagnosed with any disease, inpatient care use, smoking and drinking behaviors, and BMI. These results suggest that the income effect from CII reducing out-of-pocket inpatient costs for middle-aged and elderly adults, as documented in the first chapter, has little improvement on health and no discernible impact on risky health behaviors.

The third chapter studies how maternal education affects children's early childhood health outcomes and cognitive skills. I take advantage of the college expansion in China, which creates credible exogenous variation in access to colleges that improves educational attainment. Utilizing the difference in the number of colleges across provinces and cohorts, I employ an instrumental variable approach to examine how maternal education improves children's outcomes. The results show that maternal education reduces the probability of infant low birth weight and improves children's early cognitive skills development for mothers of rural origins. In contrast, little impact is found for mothers who grew up in urban areas. I investigate several mechanisms which could explain the outcomes and find that maternal education is strongly associated with assortative marriage and rural-urban migration.

KEYWORDS: Public Health Insurance; Medical Expenditures; Health Outcomes; Risky Behaviors; Maternal Education; Early Childhood Outcomes

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August 12, 2022

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To my grandma, Xiuqun Feng.

ACKNOWLEDGMENTS

I want to express my sincere gratitude to my advisor, Dr. Charles Courtemanche, for his invaluable guidance and unwavering patience. The numerous conversations I had with him helped me to better understand different research topics, empirical approaches, and policy analyses. I also thank Dr. Olga Malkova. I benefited a lot from being her TA in the first year I came to the U.S. and from the RA opportunity working with her. Studying Labor Economics with Olga trained me to enjoy reading papers and think deeply. I thank Dr. Steven Lugauer for always encouraging me and giving me timely feedback. I want to thank Dr. Teresa Waters for her constructive advice on my dissertation and job application. I also thank Dr. Craig Adam for being my outside examiner. Many thanks and appreciation also go to Dr. Carlos Lamarche, Dr. Chris Bollinger, Dr. James Ziliak, and Dr. Aaron Yelowitz for their insightful comments during my practice talks. I would also like to express my gratitude to Dr. Gail Hoyt, Dr. Alejandro Dellachiesa, and Dr. Darshak Patel for sharing their valuable teaching experience.

I am grateful to have had Fang-Yu Yeh and Jinwoo Song in my first three years of Ph.D. study. I will never forget those evenings we spent in the office, discussing homework questions and teaching skills. I would like to thank Anh Leh and Shiyu Cheng for their generous help. I also thank all the other friends who have accompanied me on this journey.

I thank my parents, Kai Song and Ping Wang, for their unlimited support. I would also like to thank my grandparents, Lihou Song, and Xiuqun Feng. They gave me endless love during my childhood when I was a left-behind child. Finally, I want to thank my husband, Xiaozhou Ding, for his encouragement through many challenging times. His hard work and intelligence have inspired me to be a better person since 2010.

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Chapter 1 Does Increasing the Generosity of Public Health Insurance Affect Medical Expenditures? Evidence from China's Critical Illness Insurance Program

1.1 Introduction

The aging population in China has attracted worldwide attention. According to the latest 2020 Population Census, there are 264 million people over 60 years old in China, accounting for 18.7% of the country's population, compared to 13.3% in 2010.[1] At the same time, decreased fertility rates and increased life expectancy have led to dramatic changes in the structure of Chinese families. Rapid population aging, changing family structures, and the financial burden of aging-related diseases strongly suggest that the government should address these issues through health policies. This paper examines the impact of a newly expanded public health insurance program, the Critical Illness Insurance (CII) program in China, which provides more generous benefits to enrollees in two public health insurance programs, the New Rural Cooperative Medical Insurance Scheme (NCMS) and the Urban Resident Basic Medical Insurance Scheme (URMI). In particular, this paper focuses on the most vulnerable groups in China—the rural middle-aged and elderly—who are at high risk of illness and medical costs.

Unlike developed countries with specific insurance schemes (Medicare in the US) or extra benefits (lower cost-sharing plans in Japan) for the elderly population, China has no national health insurance program that is specific to seniors.[2] Although 93% of middle-aged and older Chinese adults had some health insurance in 2011 (Zhang et al., 2017), high deductibles and coinsurance rates and low benefits ceilings impede the use of healthcare services while having little effect of reducing medical costs (Wagstaff et al., 2009a; Liu and Zhao, 2014). The World Bank also reported that China's universal health insurance system lacks financial protection (World Bank, 2010) . Compared to the elderly insured by urban URMI, the elderly with rural NCMS have shallow pensions and face more financial

[1]National Bureau of Statistics, 7th Population Census summary statistics report. The aging population over 60 years old ratio in rural areas is 23.81%, which is 7.99 percentage points higher than in urban areas. `http://www.stats.gov.cn/ztjc/zdtjgz/zgrkpc/dqcrkpc/ggl/202105/t20210519l817702.html`

[2]Some cities have lower cost-sharing plans for the elderly covered by Urban Employee Basic Medical Insurance (UEMI), while some cities offer free basic medical insurance coverage for rural elderly who meet local requirements. For example, in the cities in Henan province, elderly rural residents who are extremely poor, childless, and unable to work (*Wu Bao Hu*) are eligible for full, zero-premium coverage with the New Rural Cooperative Medical Scheme (NCMS).

constraints.[3] Rural families of chronically ill people over the age of 60 are three times more likely to experience catastrophic medical expenditures compared to urban residents (Hu et al., 2008; Li et al., 2014b). Furthermore, the aging group in rural China encounters additional barriers to accessing high-quality health care facilities, which are mostly located in urban areas and have low reimbursement rates for rural NCMS enrollees (Smith et al., 2013).

China launched the CII program in an effort to reduce high out-of-pocket medical costs in 2012, and it was gradually rolled out across cities and established nationwide by the end of 2015. As the largest expansion of benefits since the introduction of the NCMS and URMI, CII has covered 1.05 billion Chinese citizens.[4] NCMS and URMI enrollees get extra reimbursement if their cumulative annual out-of-pocket medical costs exceed the local CII deductible. Although CII is an extension of both NCMS and URMI schemes, the potential size of benefits for enrollees in these two groups differs. People insured by NCMS are rural *hukou* residents and usually live in rural areas.[5] Based on the design of NCMS, insured individuals face higher copayments in urban medical facilities, which tend to be more comprehensive and of better quality, than those in rural areas. Meanwhile, the urban *hukou* residents covered by URMI face lower copayments at these quality health facilities. Therefore, CII should reduce the medical costs of rural residents more substantially than for urban residents.

This paper uses survey data from a nationally representative dataset of middle-aged and elderly individuals combined with novel data on CII city-level phased rollout dates to study the effects of increased public health insurance generosity on medical costs and health service utilization. Since the primary goal for CII is reducing financial risks incurred by potentially high medical costs, I first examine the impact of CII on out-of-pocket inpatient and outpatient costs for rural NCMS-insured individuals. With the large mass of zeros and heavy skewness for these outcomes, I follow Strumpf et al. (2017) and Deb and Norton (2018) to use the two-part model in a difference-in-differences and event-study framework. I find that CII decreased out-of-pocket inpatient costs by around 366 yuan (USD$56) for the rural middle-aged and elderly in China. If we only consider the group

[3] From Huang and Zhang (2021), pension for the rural elderly is only 55 yuan (USD$9) per month.

[4] CII's covered population number was calculated by the end of September 2016. This number is from a press conference report, available at http://www.gov.cn/xinwen/2016-10/19/content 5121874.htm.

[5] *Hukou* is a registration system in China. The Hukou system records each citizen's birth, death, marriage status, education level, core family relations, family address, and permanent hukou migration history. The Hukou system divides the residents into urban and rural areas with separate social health insurance benefits, social pension schemes, and educational support. A Hukou status change usually has to meet strict requirements set by local governments. More details about China's hukou system can be found in Song (2014).

of people who had any costs, CII significantly reduced the out-of-pocket inpatient costs by 2753 yuan (USD$426). This implies an approximate 45% decrease from the pre-CII period. There is no evidence showing that CII affected outpatient costs; even though I find a negative effect, it is not statistically significant.

I then use the negative binomial model with a difference-in-differences and event-study framework to study the effect of CII on the number of healthcare visits for NCMS enrollees. I do not find any significant changes in the number of inpatient and outpatient visits in either the probability of having any visits or the number of visits. This suggests that the reductions in medical costs are separate from changes in healthcare utilization, indicating lower prices for inpatient services. The findings help support the effectiveness of CII in reducing medical burdens without increasing healthcare utilization as a result of a more generous program.[6]

Next, I apply the same empirical strategy to examine the impact of CII on URMI-insured urban residents. There is no statistical sign of a reduction in either inpatient or outpatient out-of-pocket medical costs. Also, I do not find any significant changes in the number of inpatient or outpatient visits. The results for the urban URMI group confirmed my hypothesis that the rural NCMS-insured group has substantial benefits.

I perform a wide range of robustness checks and placebo tests. The results are qualitatively similar to my main results. Finally, I evaluate the heterogeneity in the effects of CII through subsample analyses. I find rural elderly people over 60 years old, rural individuals who live in poor households, and rural chronically ill enrollees benefit most from CII in reducing out-of-pocket inpatient costs. While derived from a developing country's context, my main findings are consistent with the literature showing that the increase in health insurance generosity leads to lower out-of-pocket inpatient costs for the elderly in developed countries like Japan (Shigeoka, 2014; Fukushima et al., 2016).

My study contributes to several strands of existing literature using quasi-experimental designs to examine the effects of public health insurance. First, it adds to the health insurance literature about how public health insurance affects the elderly, who use more health services and incur more healthcare costs than others. Few studies are focusing on the impact on the elderly, concentrating on developed countries, and they have consistently found that health insurance reduces medical costs among elderly people. For example, Card et al. (2008) document an increase in healthcare service and health improvement with eligibility for Medicare, but it is unknown whether the impact on the elderly is from having insurance at all or because the insurance plan is more generous than before.

[6] I also examine the effects of CII on subjective physical health and mental health, and I do not find any significant changes in these two health measures. The results are presented in Appendix Table A3.

Finkelstein and McKnight (2008) find a substantial reduction in out-of-pocket medical expenses for the elderly covered by Medicare, and Engelhardt and Gruber (2011) show a similar, smaller result with Medicare Part D. However, research on the old-age population in developing countries is scarce. Cheng et al. (2015) study the effect of China's NCMS introduction on the elderly and show no evidence of any reduction in out-of-pocket medical costs, which contrasts with the findings in the US. Although CII is not specific to the elderly, this paper helps us better understand the impact of health insurance on aging people in a country that lacks dedicated public insurance support for the elderly.

Second, this paper contributes to existing literature about the effects of changes along the intensive margin (i.e., increased generosity) of health insurance. Although a large body of literature has studied the impact of the introduction or expansion of health insurance programs, we know little about the effects of increased generosity in public health insurance.[7] This paper is most related to one strand of literature that examines the impact of cost-sharing on health insurance plans. The famous RAND experiment shows that lower cost-sharing is associated with more medical costs and health service utilization, but that was only for a limited sample of people aged under 62 years old and was conducted 40 years ago (Manning et al., 1988). Shigeoka (2014) and Fukushima et al. (2016) study the effect of reduced cost-sharing for the elderly in Japan; they find consistent results that a more generous insurance plan reduced out-of-pocket expenditures and increased healthcare utilization but had no significant effects on health. Feng et al. (2020) conduct a similar study using a decreased cost-sharing plan for the Urban Employment Basic Insurance (UEMI) in Shanghai, China, and they find comparable effects to Japan. However, no clear evidence shows what would happen with increased health insurance benefits for the relatively poor middle-aged and elderly populations in developing countries. As generosity increases for basic medical insurance schemes, CII offers a unique opportunity to study the impact. In addition, China has the largest aging population in the world; the experience of increasing health insurance benefits for this vulnerable population provides valuable guidance for other developing countries.

Third, this is the first paper using a quasi-experimental design to study the impact of the CII program on medical costs in China. While previous literature has only examined the correlation between CII and medical costs, this paper explores the causal effects of CII on medical expenses and healthcare utilization. This allows us to understand whether

[7]For the literature regarding to health insurance expansion, Courtemanche et al. (2018a) has a detailed literature review about the US health insurance interventions before the 2014 ACA expansion; Gruber and Sommers (2019) presents a summary review of the ACA literature showing the various impacts of ACA with different research designs; Erlangga et al. (2019b) has a systematic review about the impact of social health insurance on low and middle income countries.

changes in patients' behavior drive the changes in medical costs after CII implementation. Unlike other studies that use short-term effects of one year, such as Zhao (2019), and one or two city case studies (Zhong et al., 2021), this paper uses a longer post-period of nationally representative survey data, with data through 2018. The extended data period offers an advantage for examining the relatively longer post-treatment period nationally rather than locally.

The paper proceeds as follows: Section 2 introduces the institutional background and literature review. Section 3 describes the data and provides some descriptive analyses. Section 4 outlines the empirical strategy. Section 5 presents the results, and Section 6 offers a conclusion.

1.2 Institutional Background

Over the past three decades, China's healthcare system has undergone tremendous changes in conjunction with the country's economic reforms. Current basic health insurance schemes in China consist of the Urban Employee Basic Medical Insurance Scheme (UEMI) for urban employed workers; the New Rural Cooperative Medical Scheme (NCMS) for rural hukou residents; and the Urban Residents Basic Medical Insurance Scheme (URMI) for urban hukou residents without formal jobs. These three government-run basic medical insurance schemes have covered more than 98% of the population in China, with approximately 1.35 billion Chinese protected by one of the schemes.

Urban Employee Basic Medical Insurance Scheme (UEMI) The employment-status based UEMI was introduced nationwide in 1998. Prior to 1998, urban workers employed by the government and state institutions were insured by the Government Insurance Scheme (GIS) with no premium needed. The Labor Insurance Scheme (LIS) covers urban employees in state-owned or collectively owned enterprises and public sectors. These two schemes provided comprehensive benefits with minimal cost-sharing (Liu, 2002). However, small risk pools, overuse of health services, and immense financial burdens for enterprises led to reforms in urban health insurance (Yip and Hsiao, 1997). After 1998, UEMI gradually replaced GIS and LIS; private-sector employees were also eligible to enroll. As of the end of 2018, 316.8 million urban workers were insured by UEMI.

Studies have shown that UEMI enrollees are sensitive to price changes in health services. Huang and Gan (2017) find that under the increased cost-sharing plan, UEMI decreased outpatient care and medical costs. A recent study confirmed this by showing that a decreased cost-sharing ratio of primary care provider visits for UEMI enrollees in one large city in China leads to more primary care visits and higher primary care costs (Shen et al., 2020). Other research demonstrates no discernible effects of UEMI on formal health service utilization, out-of-pocket medical expenses, or health (Liu and Zhao, 2006).

New Rural Cooperative Medical Scheme (NCMS) and Urban Residents Basic Medical Insurance Scheme (URMI) UEMI covers only around 25 percent of enrollees in the basic insurance schemes, while the majority of China's urban and rural residents are covered by the New Rural Cooperative Medical Scheme (NCMS) and the Urban Residents Basic Medical Insurance Scheme (URMI).

China had the Rural Cooperative Medical System (CMS) before the introduction of NCMS. CMS was initiated by the rural communities during the 1950s to 1970s to protect rural peasants. With the collapse of the collective system in rural China in 1979, CMS was terminated due to lack of funds, and China's rural residents were uninsured for more than 20 years (You and Kobayashi, 2009). Meanwhile, gradually increasing medical costs pushed many rural families back into poverty (Yip and Hsiao, 2009). In 2003, the Chinese government initiated NCMS to protect against the high inpatient medical costs for rural hukou residents. Another group of people, the unemployed individuals in urban areas, were excluded from the previous two basic insurance programs. This unemployed urban group includes mainly children, students, and the elderly. Starting in 2007, URMI was gradually expanded, and by the end of 2009, it provided basic medical insurance nationwide for urban hukou residents without jobs (Liang and Langenbrunner, 2013).

NCMS and URMI share many similarities. First, these two basic insurance schemes are dependent on hukou registration status. Rural hukou residents are only eligible to enroll in NCMS, while urban hukou residents can only participate in URMI. NCMS enrollees cannot switch to URMI unless their hukou registration status is changed from rural to urban.[8] Second, these two insurance schemes are heavily subsidized by the government. Premiums for both schemes include a small portion of individual contributions and a large portion of government subsidies.[9] The premiums usually vary by population, with the poor, elderly, and disabled experiencing lower premiums. Third, local governments have the authority to design and implement these two basic public insurance programs. Fourth, these two programs are both based on voluntary participation. Fifth, they both mainly aim to reduce the high medical costs of inpatient services with limited outpatient services.

[8]Previous NCMS enrollees who find a formal job in urban areas can switch to UEMI if their employers offer this plan.

[9]For example, the national required minimum NCMS annual premium is 150 yuan (USD$23.22) in 2010. The state government subsidized 60 yuan (USD$9.29), the local government subsidized 60 yuan (USD$9.29), and the rural residents only needed to pay 30 yuan (USD$4.64). http://www.gov.cn/gongbao/content/2010/content1555968.htm.

Table 1.1 summarizes more details about the general design of the basic medical insurance schemes in China.

Several studies have consistently found that the NCMS and URMI in China have null impacts on health expenditures, especially on out-of-pocket medical costs (Lei and Lin, 2009; Yip and Hsiao, 2009; Cheng et al., 2015; Pan et al., 2016). There are a number of possible explanations for the null effects, including the limited financing level with low actual reimbursement rates, more health service utilization from higher-level providers (Liu and Zhao, 2014), and supply-side incentives for health providers to offer unnecessary drugs and high-tech care services (Wagstaff et al., 2009a).

Literature also documents that NCMS increased preventive care (Lei and Lin, 2009; Donato and Rokicki, 2016; Babiarz et al., 2010), as well as outpatient and inpatient services (Wagstaff et al., 2009a). But there are mixed findings on the introduction of URMI: Liu and Zhao (2014) argue that it increased inpatient and outpatient services, while Pan et al. (2016) find no significant effects of URMI on primary check-up, or inpatient and outpatient services. Other studies also confirmed the null effects of URMI (Dong et al., 2018). The inconsistent results regarding healthcare services may be due to the different datasets used or different measures of health service utilization.

Only a few studies have explored how NCMS and URMI affect health outcomes. Although Pan et al. (2016) find that URMI increased the enrollees' probability of reporting good health and Cheng et al. (2015) show there is an improvement in physical health status for the rural elderly with NCMS, other papers conclude that neither NCMS nor URMI has significant effects on health outcomes (Lei and Lin, 2009; Donato and Rokicki, 2016). In other further studies related to the impact on mortality, there is no evidence showing that NCMS has affected the mortality rates for pregnant women, young children (Chen and Jin, 2012), or rural elderly (Zhou et al., 2017). Lei and Lin (2009) suggest that the null effect of NCMS on health might be because of its minor impact on formal medical care utilization due to high deductibles, small budgets on the insurance schemes, and little effect on the enrollees' out-of-pocket medical costs.

Critical Illness Insurance (CII) In an effort to provide affordable and equitable basic healthcare for all Chinese people, the central government initiated ambitious healthcare reform in April 2009 (Yip et al., 2012)[10]. One crucial part of this reform is expanding the basic health insurance benefit coverage (Meng et al., 2019). In August 2012, the National Development and Reform Commission, together with five other ministries, issued "Guid-

[10]The State Council issued "Opinions on Deepening Health System Reform" in March 2009. More details about this reform can be found at: `http://www.china.org.cn/government/scio-press-conferences/2009-04/09/content17575378.htm`.

ing Opinions on Carrying Out Critical Illness Insurance for Urban and Rural Residents." The Opinions point out that CII is an intensive change and development of NCMS and URMI, and it aims to provide extra reimbursement for high medical expenses. Local governments take the main role of designing their own CII plans, including the premium, funding source, pooling level, and coverage scope; commercial insurance companies provide CII using market mechanisms and professional advantages. More importantly, for the high out-of-pocket costs that exceed the CII deductible line, the Opinions require that the reimbursement rate shall not be less than 50%.[11]

Starting in 2012, local governments gradually established CII. In August 2015, the State Council issued another document entitled "Opinions on the Full Implementation of Critical Illness Insurance for Urban and Rural Residents," which reinforces the importance of CII and requires that all the NCMS- and URMI-insured population shall be covered by CII by the end of 2015.[12] Based on an estimation from 2015, the real reimbursement rate of inpatient services for NCMS and URMI was around 50% before CII, and it can be more than 70% after implementation.[13]

There are three distinct CII features. First, no premium is needed for enrolling in CII. People covered by NCMS and URMI will be automatically enrolled in CII, and local governments will use a specific amount of premium from NCMS or URMI as the premium for CII. Second, CII is not a disease-based but a fee-based plan; in other words, if the eligible out-of-pocket medical costs for NCMS or URMI enrollees are higher than the local CII deductible line, usually the previous year's local GDP per capita, then NCMS and URMI enrollees can obtain a second reimbursement based on their local CII plans. Third, given the different economic development and local health insurance funds' sustainability, CII plans vary in premium, deductible, and reimbursement rate among cities. Table A1 and Table A2 in the Appendix present the differences of CII plans in three example cities.

In the relatively short time since the full establishment of CII, not many studies have comprehensively examined the impact of this program, and most of them are correlation analyses. Li et al. (2019b) use hospitalization data from one city to show that CII has no significant effect on reducing out-of-pocket costs or reimbursement rates. Another study utilizes four cities' insurance reimbursement data before-and-after the CII, presenting that the CII increased medical safety to some extent but had limited effects on the incidence of catastrophic health expenditure. In contrast, Jiang et al. (2019) employ a before-after

[11]The full text of this document is available at: http://www.gov.cn/gzdt/2012-08/31/content2214223.htm.

[12]The full text of this document is available at: http://www.gov.cn/zhengce/content/2015-08/02/content10041.htm.

[13]From a report about CII that was originally published by the National Health Commission in 2015. http://www.gov.cn/zhengce/2015-08/02/content2907557.htm.

comparison showing that CII reduced the financial burden for patients with high medical costs.[14] As for the indirect effects of CII, Zhao (2019) argue that CII increased daily household consumption per person in rural China, but there is no evidence of changes in rural household healthcare expenditures.

In summary, plenty of studies have examined the impact of basic health insurance provisions on various outcomes based on gaining coverage (extensive margin), but the effect of health insurance's benefits increasing (intensive margin) is understudied. Limited coverage and small budgets in NCMS and URMI have been demonstrated to have negligible effects on health-related outcomes. With the gradual development of these two insurance programs and the establishment of CII, it is necessary to examine the impact of CII among NCMS-and URMI-insured populations. The effects of CII for the rural middle-aged and elderly should be emphasized because the population is rapidly aging but with limited old-age supports. More generally, China's experience of providing extra protection for high medical costs can also shed light on the health insurance reforms in other developing countries, particularly in addressing healthy aging and welfare for the elderly.

1.3 Data

Overview of Data The main data used in this paper is from the China Health and Retirement Longitudinal Survey (CHARLS), which is designed in a similar way to the US Health and Retirement Survey.[15] The survey includes seven sections: demographic background; family; health status and functioning; health care and health insurance; income; expenditures and assets; and interviewer observations. It is a nationally representative longitudinal survey of people aged 45 or older and their living spouses (Zhao et al., 2014).[16] So far, CHARLS collected baseline survey data in 2011 and three follow-up surveys in 2013, 2015, and 2018. In the baseline wave, 17,708 individuals in 10,257 households in 450 village/urban communities were surveyed. CHARLS added a small share of new respondents in the follow-up surveys to make the entire sample representative of the 45+ age group.[17] In addition to the basic demographic and socio-economic variables, one essen-

[14] In Jiang et al. (2019), the financial burden includes five measures: out-of-pocket payment, the ratio of out-of-pocket to income, the prevalence of catastrophic health expenditure (CHE), average gap of CHE, the relative gap of CHE, and medical debt.

[15] CHARLS data can be accessed through the official website. `http://charls.pku.edu.cn/index/en.html`. CHARLS does not survey Tibet, Hong Kong, Macao, or Taiwan.

[16] The main respondent's spouse's age can be less than 45 years old. So in the final dataset, we can observe some respondents aged below 45 years. I drop the observations with age lower than 45 for the main analysis.

[17] From the CHARLS wave four user's guide, with the increase of refresher samples, the total number of individuals (main respondents plus spouses) has increased from 17,078 in wave one to 19,817 in wave

tial part of this survey is its detailed information about the respondent's health insurance status, health care utilization, and health expenditures. This helps to evaluate the impact of CII on overall health-related outcomes.

The unique city-level CII policy information is hand collected from the official documents and media coverage archives. The official CII documents are extracted from the PKU Law website for each prefecture city.[18] For cities without any official CII implementation documents in the PKU Law database, I supplement those documents by searching the local government websites or media reports. The effective date of CII is defined using the date listed on the official documents, and usually it is January 1st of the CII start year. Figure 1.1 shows the staggered time variation of CII rollout among cities in China for the NCMS group.[19] Combining the city-year level CII policy dataset with CHARLS data shows that two cities rolled out CII in 2012, 37 cities in 2013, 55 cities in 2014, and 29 cities in 2015.

Outcome Variables The outcome variables include medical costs and health service utilization. Medical costs are measured as out-of-pocket inpatient costs last year and out-of-pocket outpatient costs last month.[20] The health service utilization category includes the total frequency of inpatient services last year and the total number of outpatient visits last month. I also examine the effects of CII on health, and respondents' health condition is measured by self-reported physical health status and an overall mental health score. Self-reported physical health status is categorized as very good, good, fair, poor, and very poor. Here I create a dummy variable of fair or poor self-reported health, which equals one if the respondent's answer is fair, poor, or very poor. If a respondent answers this question as good or very good, this self-reported health dummy variable equals 0. The CESD-10 depression scale measures self-reported mental health. A higher score means worse mental health.[21] I standardized this score for ease of comparison. The effects of CII

four.

[18] PKU Law is a Chinese law search system. It includes all the laws and regulations released by central and local governments. `http://en.pkulaw.cn/`.

[19] NCMS and URMI were administered by different government agencies. The Health and Family Planning Commission administered NCMS, and the Ministry of Human Resources and Social Security administered URMI. These two agencies were merged with other agencies as the Nation Healthcare Security Administration in 2018. Before 2018, because NCMS and URMI were under different administration agencies, CII rollout time may have differed by one year for these two insurance schemes in the same city. Since my main study sample is rural hukou people (insured by NCMS), I only list the NCMS-CII time information. In the latter part, when I study the impact of CII on the URMI group, I use URMI-CII time to define the treatment status.

[20] All the expenditures are inflated to the 2015 price level.

[21] The 5th and 8th questions in the CESD scale are different from others; I code the lower score for these two questions as higher. For example, in question 5, "I felt hopeful about the future," if the respondent

on these two health measures are presented in the Appendix Table A3.

Control Variables I control for the respondents' basic demographic and social-economic variables, including age, gender, education, marital status, residence location (rural or urban area), and individual annual income. Education is grouped into four levels: elementary and lower, middle school, high school, college and above. The respondent's objective physical health condition is also controlled in the model as a dummy of whether the individual has any disabilities and/or any doctor-diagnosed chronic diseases. The insured population covered by the basic medical insurance schemes can also purchase additional supplement plans or private medical insurance from the commercial insurance companies to enjoy more benefits; the model also includes these two variables, which could affect the outcome variables. I add city-level economic variables such as the GDP per capita, number of hospital beds, number of health workers, and average employee wage to control for the time-varying city characteristics that could affect respondents' medical costs and health utilization. City-level economic variables are obtained from the Economy Prediction System (EPS) database.[22] In addition, considering that some cities have been gradually integrating the NCMS and URMI into one basic health insurance scheme—the Urban-Rural Resident Basic Health Insurance Scheme (URRMI)—since 2009, I also add a dummy variable indicating whether the city has completed the integration. The dates for integrating NCMS and URMI at the city level are mainly extracted from official documents obtained from the PKU Law website. The city-fixed effects and year-fixed effects are used to control for the unobserved variations, which may occur because of geographical differences or time differences. Individual sample weights are included to get population-representative estimates.

Sample Selection This paper mainly focuses on rural middle-aged and elderly residents, so I only use the sample who reported being insured by the NCMS in the main analysis. I first drop the groups covered by UEMI (around 13% in all four waves) and URMI (around 8%). Even though it is usually exclusive to have two or more basic medical insurance schemes simultaneously, a few individuals are reported to have more than one basic medical insurance scheme. To isolate the potential effects that may come from other multiple schemes, I exclude the sample insured by NCMS and other basic medical insurance schemes (around 0.4%). CHARLS requires that the main respondent's age should be 45 years old or older, but some of the respondent's spouses with ages lower than 45

answers "1-rarely or no none of the time," I code it as 4; "2-some or little of the time" as 3; "3-occasionally or a moderate amount of the time" as 2; "4-most or all of the time" as 1.

[22] EPS database is available at: `https://www.epsnet.com.cn`

years old will also be included in the survey. Therefore, I drop the group of people under 45 years old (around 2.5 %). With the staggered rollout timing of CII, the earliest group, which started CII in 2012, has one calendar year before the treatment and six calendar years after the treatment, while the last group, which rolled out in 2015, has four calendar years before and three calendar years after the CII program.

Despite the fact that some cities have been working on helping migrants get the basic coverage reimbursed in the city in which they reside, most medical costs outside the basic health insurance enrollment city cannot be reimbursed or have low reimbursement rates compared to the health expenses incurred at their local health facilities. Therefore, in the robustness checks, I only keep the sample who reported being covered by NCMS and currently living in the NCMS enrollment city (i.e., there is no sample of migration from hukou place to current living place in this analysis).

Since NCMS enrollment is tied to hukou status, urban hukou residents typically enjoy higher social benefits and are unlikely to change their hukou status to enroll in NCMS. Moreover, NCMS benefits are much lower than UEMI benefits, and urban employed individuals are not likely to change their insurance status to get CII benefits. Therefore, changes in the composition of the NCMS group are likely to come from previously uninsured individuals being insured by NCMS. Another potential change in composition could come from the integration of NCMS and URMI in some cities. To deal with this, I first control for a dummy variable of whether the city started the integration in the survey year to account for such potential effects on outcome variables. Due to the early stage of integration, previously NCMS-insured may report being covered by URRMI in later surveys. In the robustness checks, I consider the sample that reported being covered by NCMS in previous surveys but reporting being covered by URRMI in the latter survey as the broad NCMS group affected by CII.

Descriptive Statistics Figure 1.4 Panel A shows the trend of health expenditures over the years since CII implementation for NCMS-insured individuals. It shows a gradual increase in weighted average out-of-pocket inpatient costs over years, and the out-of-pocket outpatient costs appear to decrease slightly after CII. Consistent with the trends in health expenditures, Panel B shows a gradual increase in hospitalization visits but a decrease in outpatient visits after CII. The gradual decline in out-of-pocket outpatient costs seems to be driven by the decrease in outpatient service utilization. The relatively slow increase in inpatient visits is also consistent with a gradual increase in out-of-pocket inpatient costs before and after the CII implementation. However, based on preliminary trend plots, we cannot conclude whether CII has a causal effect on health care expenditures or utilization. Table 1.2 presents summary statistics for the basic characteristics of the main sample used in this paper. It also provides baseline means and standard deviations for cities that launched the CII in different years. As the table shows, the baseline levels of all control variables for cities that participated in the CII program in 2012 are very different from those of cities that rolled out later. This is because only two cities in the CHARLS data belong to the 2012 group, and this small sample size may lead to statistical differences with other cities. In addition, economic variables at the city level differ in the period prior to CII, with cities with CII in 2013 having better economic conditions and health service provision levels than those in 2014 and 2015. Cities that introduced CII in 2014 also had better city-level characteristics than those in 2015. Other than these two aspects, most of the control variables show a very similar baseline level among cities that rolled out CII in different years. Table 1.3 shows the summary statistics for all outcome variables as well as their baseline means and standard deviations. There is not much difference in the outcome variables across cities with different CII rollout timings, especially considering the large standard deviations in health expenses.

1.4 Methodology

With the staggered rollout of the CII program across cities, I can compare outcomes for individuals living in the cities before and after CII implementation after controlling for city and year effects. To estimate the impacts of CII on middle-aged and older people, the general form of the model with a difference-in-differences framework can be written as follows:

$$E(Y_{ict} \mid \chi_{ict}) = exp(\alpha + \beta CII_{ct} + \gamma X_{ict} + \mu G_{ct} + \pi I_{ct} + \eta_c + \lambda_t) \tag{1.1}$$

where Y_{ict} are outcome variables I described above. CII_{ct} is an indicator variable that measures whether or not respondent i living in city c at year t has been covered by CII. X is a vector of control variables that include individual specific demographics and social-economics variables. G_{ct} controls for time-varying city-level economics variables. I include a dummy variable I_{ct} indicating whether city c at year t has integrated the URMI and NCMS or not. The model also adds year fixed effects λ_t and city fixed effects η_c to account for heterogeneity across city and year specific unobservables, respectively. The parameter of interest is β which measures the overall impact of CII on medical costs, healthcare utilization, and health.

This generalized difference-in-differences framework is similar to Shah and Steinberg (2021) which studies the impact of staggered rollouts of India's public works program on individuals' schooling outcomes. It is built on the first difference that compares the outcomes of individuals living in the early and late cities before and after CII in the early cities. The second difference compares individual outcomes in the early and late cities before and after CII was extended to the whole country. The identification comes from comparing the differences in the outcome variables between the early and late cities during the rollout (when the earlier cities had CII, but the later cities did not) to before the rollout (when no city had the program) and after the rollout (when every city had the program). The identifying assumption is that, in the absence of the CII program, the difference in the individual outcomes I examine between the early and late cities in this time period would have been the same as the differences before and after the CII rollout. A flexible event-study model is useful to test for any pre-trend that might occur before the program starts, and it helps to understand the dynamic effects of CII. So here, I also employ a flexible event-study model, and the specification is as below.

$$E(Y_{ict} \mid \chi_{ict}) = exp(\alpha + \sum_{\tau=-4,\tau\neq-1}^{4} \gamma_\tau CII_{ct} + \delta X_{ict} + \zeta G_{ct} + \theta I_{ct} + \phi_c + \psi_t) \tag{1.2}$$

Y_{ict} refers to the outcome variables as before. A set of year dummies for each CII year t before or after implementation are included in the model. The year immediately prior to the treatment year is omitted. γ_τ measures the dynamic effects of CII on the outcome variables if t is τ years after city c participates in CII. X_{ict}, G_{ct} and I_{ct} are same as before. ϕ_c, and ψ_t are city fixed effects and year fixed effects, respectively. Standard errors in all regressions are clustered at the city level to allow for within-city correlations. All the regressions are weighted by the weights provided in CHARLS. CHARLS is a longitudinal survey that allows me to control the individual fixed effects in the regression analysis. Nevertheless, considering that there is around a 15% share of new samples added into the survey each wave, the attrition issue for four waves of data, and the medical costs or healthcare utilization (especially inpatient costs or visits) are unlikely to repeat every year, I do not control for the individual fixed effects in the main regression model.[23] However, I do control for the individual fixed effects in the robustness checks section.

Healthcare Expenditures Health expenditure data is usually highly right-skewed. Many people have zero cost if they do not use any health services during a specific period. Figure 1.2 displays the distribution of the medical expenses for out-of-pocket inpatient and outpatient services. Panel A shows the distribution of any inpatient or outpatient

[23] Huang and Zhang (2021) also use CHARLS data to study the impact of New Rural Pension Scheme on some health measures. As they discuss, "Including individual fixed effects may exaggerate the attenuation bias caused by measurement error".

costs. Among my main study sample, only 12% of people had any inpatient costs last year, and 18% of people had outpatient costs last month. In panel B, which shows the distribution of medical costs if positive, a large portion of the sample reported having out-of-pocket inpatient/outpatient medical costs falling into the left tail. Figure 1.3 Panel A depicts the distribution of medical expenses for users. Deb et al. (2017) describes two main issues when using the ordinary least squares model (OLS) with highly skewed data: Firstly, OLS produces a negative prediction; secondly, there are large variations within the sample. Following Deb et al. (2017) and Deb and Norton (2018), this paper uses a two-part model within a generalized difference-in-differences framework to examine the effects of CII on medical cost measures. The event-study analyses for the health expenses also use two-part models to study the dynamic treatment effects. The two-part model assumes that the density of healthcare expenditures is a mixture of the first step generating zeros and the second step that only generates positive values. The first part of this model, which estimates the probability of having any positive healthcare expenditures $Pr(y_{ict}>0 \mid X_{ict})$, is usually estimated by a probit or logit model, while the second part of this model $E(y_{ict} \mid y_{ict}>0, X_{ict})$, which estimates the amount of expenditures conditional on having positive medical costs, can be estimated through a linear model, a log-liner model, or a generalized linear model (GLM). The combined effects can then be estimated from both parts of the model $E(y_{ict} \mid X_{ict}) = Pr(y_{ict}>0 \mid X_{ict}) \times E(y_{ict} \mid y_{ict}>0, X_{ict})$. Compared with the linear model, GLM is a better alternative in the second part to analyze the highly skewed data in two ways and has been widely used in the health economics literature (Strumpf et al., 2017; Ozluk, 2017; McCullagh and Nelder, 2019). First, GLM allows the expected value of the dependent variable to be changed as a function of linear independent variables. Second, GLM incorporates heteroskedasticity by allowing the variance of the dependent variable as a function of its predicted value based on the distribution family. By avoiding transforming the dependent variables into the log format, GLM estimates give the raw scale coefficients, which also facilitates interpretation (Deb et al., 2017). Mihaylova et al. (2011) summarizes the statistical methods for analyzing healthcare expenditures, and Deb et al. (2017) describes the detailed application of two-part models. I use logit estimation for the first part of the model to study the probability of having any inpatient/outpatient costs (extensive margin).[24] Then, in the second part model, for the intensive margin, GLM models with log-link function and gamma distribution family are used to study the effects of CII on medical costs based on having any costs. Following Deb and Norton (2018), I use Box-Cox and modified Park tests to determine the link function

[24]There is little difference between using logit and probit regarding the estimated marginal effects and predicted values (Norton and Dowd, 2018).

and distribution family. In both medical cost outcome variables, the Box-Cox tests show the estimated coefficients are very close to zero, and the modified Park tests give the estimated coefficients around 2. These two tests together suggest that it is appropriate to use the log link function and gamma distribution in the GLM estimations. Finally, I use the "twopm" command to obtain the combined marginal effects and standard error, which include both parts.[25]

Counts of Health Service Utilization Healthcare utilization is typically measured as counts of non-negative integers. Similar to the medical costs data, there is a large mass of zero values among the healthy respondents, and thus it is right-skewed. This paper focuses on the number of inpatient visits last year and outpatient visits last month. Figure 1.3 Panel B displays the distribution of the number of inpatient/outpatient visits. More than 80% of the sample had zero inpatient visits last year, and around 80% of the sample had zero outpatient visits last month. The majority had fewer than five inpatient visits, and the number of outpatient visits was lower than 10 for most respondents who had any visits. Cameron and Trivedi (2013) and Deb et al. (2017) outline the potential approaches to study the count data. Here I use negative binomial regression as my preferred model to estimate the effects of CII on healthcare utilization. This is because it offers a more efficient estimator compared to Poisson regression by relaxing the restrictive mean-variance property of Poisson regression (Deb and Norton, 2018). In the robustness checks, I use a Poisson model and a hurdle model to check if the results are sensitive to the model specification changes.

1.5 Results

1.5.1 Main Specification Results

Effects of CII for NCMS I first investigate the effects of CII on out-of-pocket inpatient costs and inpatient visits among the NCMS insured population. Table 1.4 Panel A reports the main results from estimating Equation 1.1. Columns (1) and (2) show the average marginal effects of CII on medical costs. I find that CII has no discernible effects on the extensive margin of having any inpatient costs. However, CII has largely reduced the inpatient out-of-pocket costs on the intensive margin. That is, after CII, for those with positive inpatient medical costs, out-of-pocket inpatient costs were reduced by 2,753 yuan (USD$426). This translates into a 45% reduction in out-of-pocket hospitalization costs from the baseline mean, and it is also around 82% of the average individual income and 45%

[25] Stata "twopm" command is developed by Belotti et al. (2015).

of the average household income per person. Column (3) shows the combined average marginal effects of CII on the overall NCMS insured population, which considers the two parts together. The results show a reduction in out-of-pocket hospitalization costs of 366 yuan (USD$56). The two-part model results indicate that reductions in the out-of-pocket inpatient costs were due to the decrease in costs for those who had any inpatient costs but not the probability of having any inpatient costs. Column (4) presents the effect of CII on the number of inpatient visits using the negative binomial model. The result implies a small and insignificant effect on hospitalization service utilization. Next, I examine the effects of CII on out-of-pocket outpatient costs and outpatient visits among the NCMS-insured population. The results are presented in Table 1.4 Panel B. Similar to the inpatient costs, there is no significant change after CII in the share of individuals having positive outpatient spending. A reduction of 142 yuan (21%) is shown for out-of-pocket outpatient costs on the intensive margin, even though it is not statistically significant. Moving to the overall effects of CII on out-of-pocket outpatient costs, there is also no statistically significant reduction. From Column (4), I see no discernible effects on the number of outpatient visits.

Then I report the results from the event-study specification using two-part models for the medical costs and visits. The flexible event-study model enables me to test the key identifying assumption of any pre-existing trend before CII implementation. Figure 1.5 Panels (a) and (b) show the combined average marginal effects of CII on out-of-pocket inpatient and outpatient costs four years before and four years after its implementation.[26] I omit the year before CII as the base year. Although the pre-trend has a small fluctuation, I do not see any significant difference in the pre-CII period. Figure 1.5 Panel (c) and Panel (d) show the dynamic changes in the number of inpatient and outpatient visits over the years. There are no clear changes before or after CII, except for the estimated marginal effects on outpatient visits four years later. CII significantly decreased out-of-pocket inpatient costs after implementation, especially after two or three years. At the same time, CII had minimal effect on the number of inpatient visits, as is shown in Panel (c). Consistent with results in Table 1.4, Panel (b) indicates barely significant changes in out-of-pocket outpatient costs over the years. A few pre-trends are showing up before CII, and the combined average marginal effects are fairly stable over the years. Figure 1.5 Panel (d) shows the impact of CII on outpatient visits. There is a gradual increase in the number of outpatient visits after CII, even though these increases are not statistically significant in most post periods. Overall, these event-study results provide supportive evidence for the

[26] I group the cities that have six calendar years and five calendar years after CII. In the event-study estimation process, I add the dummy variable of “post-5 years” in the regression but do not report it here because it has a small observation size in this group.

underlying assumption that there is no clear pre-trend before the early treated group or the late treated group. In addition, all the results in the event-study figures are consistent with the findings from Table 1.4, showing that CII decreased the out-of-pocket inpatient costs but had indistinguishable effects on inpatient health service utilization, outpatient costs, and the number of outpatient visits. This large and significant reduction in out-of-pocket inpatient costs could be because the basic medical insurance programs in China are government-run programs and have considerable market power, which has led to lower drug prices in recent years. If this is true, then the significant drop in inpatient costs I find might be influenced by other factors rather than a dominant effect of CII. I test this by doing a placebo test that uses a non-affected group (UEMI) as the analysis sample. If any other concurrent policies could drive a reduction in inpatient costs, this would also affect the UEMI group. The results are shown in Table 1.9. I do not find any significant changes in any of the outcomes for the UEMI group, which indicates that no other policies had significant impacts on medical costs. More details about this point are discussed in the robustness checks section.

Effects of CII for URMI As I previously mentioned, the NCMS-insured group should have a more considerable impact from CII than URMI-insured people because URMI-insured people face lower copayments for quality healthcare providers. I test this hypothesis by using the two-part models and negative binomial models in a difference-in-differences framework for URMI group people. Table 1.7 show the main results of CII for the URMI group. I find CII has insignificant effects on medical costs and inpatient or outpatient visits. In addition, the results show an increase in out-of-pocket inpatient and outpatient costs, contrary to the findings of the NCMS group. Thus, the results confirm the hypothesis that the NCMS group benefits more from the CII program, but there are small and insignificant impacts on the URMI group.

1.5.2 Robustness Checks

In this section, I test the robustness of the results for the NCMS group in three ways: changing the model specification, restricting study samples, and discussing the newly proposed difference-in-difference estimators with staggered timing.

Model Specifications Since my main results suggest that CII significantly decreased inpatient costs while having zero impact on other outcomes, I start the robustness checks with medical costs. Table 1.5 provides the effect of CII on medical costs by adjusting the control variables, fixed effects, and model specifications. The first three columns for each medical cost outcome variable use the two-part model to find the combined average marginal treatment effects while incorporating different control variables. Column (1) excludes all individual-and city-level controls and only includes the city and time-fixed effects. Column (2) adds the city-level integration dummy variable in addition to the city and time fixed effects, which helps test whether integration of NCMS and URMI has any large impact on the medical costs. Compared to my main model specification, Column (3) only excludes the city-level economics variables since these could potentially be endogenous to the CII rollout time. The results of out-of-pocket inpatient costs in Table 1.5 Panel A Columns (1)-(3) show very similar findings to the main results reported for inpatient costs. I then estimate CII effects on medical costs using the OLS model with the same control variables, fixed effects, and cluster level as the two-part model I used in the main analyses. Column (4) provides very close estimates of inpatient costs as my main results in Table 1.4. Next, I add the individual fixed effects using the OLS model, and the results are presented in Column (5). I find the results are quantitatively and qualitatively consistent with my main finding for the out-of-pocket inpatient costs. As for out-of-pocket outpatient cost results presented in Table 1.5 Panel B, the results are very similar to the results shown in Table 1.4 in terms of the significance level. However, the magnitudes of out-of-pocket outpatient costs from this table are slightly smaller, except that the OLS model results have flipped signs. This further supports the fact that CII has no impact on outpatient costs. For the outcomes of healthcare utilization, I present the Poisson model estimation results and the intensive and extensive changes in Table 1.6. As shown, the estimated marginal effects on the number of visits are very similar among different model specifications. There are no significant changes in the extensive margin nor the intensive margin of the number of inpatient and outpatient visits.

Sample Restriction I use two different ways to redefine the sample used for the analysis. First, I use a more broadly defined CII-affected group for the rural residents. China started to integrate NCMS and URMI in recent years, which requires that NCMS and URMI groups share the same administrative agency, premium, and benefits package. Since few cities have completed the integration of NCMS and URMI in the last wave of survey data, the respondents in these cities may report their insurance status as URRMI after integration. However, some respondents still report their insurance status as covered by NCMS. By taking advantage of the longitudinal data, if the respondent reported being insured by URRMI in the survey wave after integration and having NCMS in previous waves, I treat them as the rural sample and include them in the analysis for robustness checks. Second, CII benefits are an additional benefit to NCMS and are attached to hukou status. Insured people living in different places may enjoy fewer reimbursement benefits based on the local design of health insurance schemes. Therefore, I restrict the sample to those who report having NCMS and currently live in the same city as they did when they enrolled in NCMS. Table 1.8 Panel A and Panel B show the results for all the outcome variables. The effects of CII using different analysis samples are broadly similar to my main findings in terms of significance and magnitude.

TWFE with Staggered Timing The emerging literature has discussed the potential issues with using two-way fixed effects (TWFE) with varying treatment timing (staggered difference-in-differences).[27] As discussed in Goodman-Bacon (2021), TWFE estimator is a weighted average of all potential 2×2 DD estimates. The weights are based on the group size and variance for each treatment group at varying treatment times. The variation used to identify the causal effects of CII in this paper only comes from the different CII rollout timing, and there is no pure control group. There will only be the early treated group with the latter group as the control and the later treated group with the early treated group as its control group. The estimated effects will be biased if the control group (early treated group) has heterogeneous treatment effects. As suggested in Baker et al. (2021), researchers should conduct a "Bacon-Decomposition" to run the diagnostic test. However, the dataset I use in this paper is not balanced panel data, which does not allow me to conduct this diagnostic test. Other alternative estimators proposed by Callaway and Sant'Anna (2020) and Sun and Abraham (2020) address the potential issues related to the canonical TWFE estimator and offer unbiased difference-in-differences estimates by exploring the potential control groups. If no control group exists, which is the case in this paper, Callaway and Sant'Anna (2020) use the set of not-yet-treated groups as the potential control units, while Sun and Abraham (2020) only use the last treatment group as the control unit. When using these two estimators, the data from the last treatment year and the year after will not be used in the main analysis since all the groups are treated eventually. In this paper, the last treatment year of CII is 2015, which is the third wave of CHARLS data. If I eliminate the data in 2015 and after, there will be only the first two waves of CHARLS data available for estimation. Therefore, this analysis can only identify the CII effects for the groups that started in 2012 and 2013 using the groups treated in later years as control units. In addition, I am unable to use the two-part model and negative binomial model in the newly proposed generalized difference-in-difference framework. Based on the intuition behind these two new estimators, I only use the data before the

[27] For example, see Goodman-Bacon (2021),Callaway and Sant'Anna (2020),De Chaisemartin and d'Haultfoeuille (2020), Sun and Abraham (2020). Baker et al. (2021) summarize some of the papers related to staggered difference-in-differences.

last treatment group starts CII and use the last treatment group treated in 2015 as the control group to test the impact of CII in the first two treatment years.[28] The results are presented in Table 1.8 Panel C. Using data from the first two waves, I still find that the early cities with the CII program (cities that started in 2012 and 2013) reduced out-of-pocket inpatient costs compared to the group treated in 2015. This is consistent with my main findings, even though the sample size is not that large.

1.5.3 Placebo Tests

One potential threat to this identification strategy is that time-varying unobservables lead some cities to adopt CII early while others start later. These unobserved time-varying differences among different places may also affect the individual's medical costs and health care utilization. For example, the early-adopting cities may have a stronger health consensus and have other general local health policies that my primary regression can not fully capture. One way to test for unobservables is to employ the main specification on the non-affected group (the group insured by UEMI) and check whether CII has any effects on this group.[29] It would be sensible to believe that people living in the same cities share the same local health policies. The placebo test estimation results are reported in Table 1.9. Following the same strategy used in the main analysis, I find no significant results on any of the outcome variables. Moreover, for those significant effects on out-of-pocket inpatient costs, which I find in the main results, the placebo tests show the opposite signs of magnitude for the UEMI group people. The placebo test results suggest that no other unobservable characteristics drive the main results.

1.5.4 Heterogeneous Effects

This section examines the heterogeneity of the CII effects; I mainly focus on the NCMS-insured individuals. One potential explanation for a large number of null results in some outcome variables is that the full sample I studied earlier includes different groups of people with varying probabilities of receiving benefits from the health insurance program

[28] I do not use the 2014 treatment group as the potential group because I am worried that anticipation effects might contaminate the 2014 treatment group. In the initial period of the CII program, media coverage reported this newly expanded health insurance nationwide. For example, see `http://tv.cctv.com/2013/03/23/VIDE1363996680943400.shtml`. Moreover, some cities had major illness insurance that only covered major diseases in 2013 but changed to CII based on the total health expenditures in 2014. (For example, all the cities in Shandong province: `http://hrss.shandong.gov.cn/articles/ch00580/201411/40038.shtml`.)

[29] There is around 1.6% of sample in four waves of CHARLS data reported to be covered by Government Medical Insurance (GMI). I combine this group of people with UEMI because UEMI in most cities gradually replaces GMI.

(Courtemanche et al., 2018a). In that case, it would be more valuable to check the CII effects across groups.

Elderly and Middle-aged I first stratify the sample into two age groups: elderly (>=60 years old) and middle-aged (45–60 years old).[30] Research has documented that the elderly are strongly affected by health insurance and likely to use more health services in China (Zhang et al., 2017). More importantly, a lack of old-age social security programs makes poverty more prominent among the rural elderly in China (Huang and Zhang, 2021).[31] As the level of medical insurance security increases, it is fair to believe that CII would significantly affect the elderly more than other populations. The impacts of CII on the elderly are shown in Table 1.10 Panel A. Notably, the magnitudes of out-of-pocket inpatient and outpatient medical costs are larger for the elderly; especially for the out-of-pocket inpatient costs: the average out-of-pocket inpatient cost was largely reduced by 709 yuan ($109) after CII.[32] If we compare this amount with the average individual income and average household income per person for this group, it comprises 34% of annual income or 15% of household income per person. For most of the other outcomes, the magnitudes of CII effects are also larger but still not statistically significant. The regression results of adults below 60 years old are presented in the Table 1.11 Panel A. As I expected, although middle-aged individuals still experienced a reduction of 36.87 yuan ($6) in out-of-pocket inpatient costs after CII, it is minimal compared to the effects on the elderly and insignificant. I also find no significant changes in other outcomes for the middle-aged group of people.

Lower and Higher Income As described in Xu et al. (2018), poor households are more likely to experience catastrophic health expenditures than relatively affluent households in China. Therefore, I split the sample based on their reported household income per person; if the respondent's annual household income per person is higher than the median level of the city of residence, then I categorize that respondent into a higher-income group. Otherwise, the respondent is in the lower-income group. The results for lower-income

[30]There are two reasons why I divide the sample by using 60 years old as the threshold. First, the statutory retirement age is 60 years old for men and 50/55 years old for women. Second, the eligible age for receiving a rural or urban pension is 60 years old.

[31]There was only an old-age social security program for urban employees before 2012 in China. Starting in 2009, China initiated the New Rural Pension Scheme, and it expanded to all rural residents by the end of 2012. Then, in 2011, a national pilot of the Urban Resident Pension Scheme (URPS) started. More details about China's pension system can be found in Dorfman et al. (2012); Huang and Zhang (2021).

[32]This may seem like a huge reduction, but if we look at the relatively large pre-period standard deviation of out-of-pocket inpatient costs, the estimate is reasonable. The 95% level confidence interval for the reduction in out-of-pocket inpatient costs is (-1203,-214).

people are presented in Table 1.10 Panel B. I find a larger effect on out-of-pocket inpatient costs compared to my full sample results. Individuals living in poor households experienced a reduction of 516 yuan ($81) in out-of-pocket inpatient costs, which is very close to the average annual individual income and household income per person. Although there are no statistically significant effects on outpatient costs, I still see the reduction in its magnitude. I find a marginally significant decrease in the number of inpatient visits, and the magnitude is very small, only 0.045. There is no impact on the number of outpatient visits. The results for the higher income group are displayed in Table 1.11 Panel B. None of the outcome variables show a significant impact from CII. There are still reductions in out-of-pocket inpatient costs but increases in outpatient costs.

Has Chronic Diseases or Not Hu et al. (2008) and Li et al. (2014b) document that rural families with a chronically ill elderly member have a higher probability than urban families of experiencing extremely high medical costs. With increased benefits in the basic health insurance plan, I expect chronically ill people to be more affected by a considerable reduction in the out-of-pocket inpatient costs because of the potentially high medical costs they already have. Thus, based on the respondent's self-reported chronic condition status, I divide the sample into two groups, the first being those with at least one chronic disease and the other being those without any chronic diseases. The results for the group with chronic illnesses are shown in Table 1.10 Panel C. I find that CII largely reduced the total and out-of-pocket inpatient costs for patients with chronic conditions, with no change in the number of hospitalizations. The reduction in out-of-pocket inpatient costs is also larger than the main finding: 515 yuan ($81) after CII. There is no evidence that CII affected the outpatient costs and number of visits for the group with any chronic diseases. The results for the group without chronic diseases are displayed in Table 1.11 Panel C. None of the outcome variables show a significant impact from CII. Taking these two results together suggests that, as I expected, CII has relatively large effects on the chronically ill but a small and insignificant impact on the relatively healthy group.

Combining all the subgroup results above shows that the CII program significantly reduced the out-of-pocket inpatient costs for the elderly, individuals living in poor households, and chronically ill people. The statistically significant results I find in these three vulnerable groups are also different from the other three opposite groups in terms of the smaller standard errors. Besides, I conduct event study analyses for all the different subgroups with these four outcomes. The event study graphs can be found in the Appendix Figure A2-Figure A5. I do not see any clear pre-trend for different groups over 24 regressions, although the pre-treatment effects are estimated to be more noise for a few groups.

In cases where I find a clear pre-trend in out-of-pocket outpatient costs and the number of outpatient visits for the elderly group and out-of-pocket costs for non-chronically ill individuals, I do not see any significant effects in my main or subgroup results. Some of them show upward trends, which is also different from my previous results. Overall, there is no clear evidence showing that there is any pre-trend contaminating my main sub-sample analyses.

1.6 Conclusion

With the unique health insurance expansion program affecting intensive margin change in China, I explore the impact of a more generous health insurance plan on middle-aged and older people. Using nationally representative data from CHARLS and exploring staggered CII program rollouts across cities, I find strong evidence that CII reduced out-of-pocket inpatient costs by 366 yuan (USD$56) among the NCMS insured population. The reduction is mainly from the decrease in costs among the users of inpatient services. Descriptive statistics show that the out-of-pocket ratio of inpatient costs for NCMS individuals has decreased from 68% in CHARLS wave one to 51% in wave four for those who had any inpatient costs, confirming the large reduction in rural residents' medical burden for inpatient care users. However, there are no significant changes in outpatient costs or the use of health services.[33] Combining the first part of having any medical costs or the number of visits and the second part of examining the changes conditional on any positive values, the subsample results which consider both parts show that the reduction effects of out-of-pocket hospitalization expenses are largest for the elderly group, individuals living in poor households, and the chronically ill population. These heterogeneity results provide evidence that CII has greatly benefited the vulnerable groups in terms of reducing high inpatient costs. The findings I show in this paper are consistent with the literature, which studies the impact of Medicare or low-cost sharing plans on elderly people (Finkelstein and McKnight, 2008; Shigeoka, 2014; Fukushima et al., 2016; Feng et al., 2020). Unlike the rural residents insured by NCMS, I do not find evidence that the urban middle-aged and elderly population insured by URMI have benefited from CII. This might be because urban residents had better access to public health services, higher benefits packages, and health conditions even before the CII program; therefore, generous changes in basic health insurance schemes may not have such significant effects on them. The different impact of CII for rural and urban samples implies that increased health insurance benefits have a greater impact on the more vulnerable group and can play a prominent role in reduc-

[33] The out-of-pocket ratio of outpatient costs is 89% in CHARLS wave one data, and it is 83% in CHARLS wave four data for NCMS-insured outpatient users.

ing the medical burden for the rural aging population. As presented in the Appendix Table A3, although I do not find significant improvement in physical health and mental health, there is reason to believe that health will improve in the long run as the program grows and medical financial pressures are reduced. Overall, my findings suggest that the CII program has achieved its goal of reducing the medical burden for the middle-aged and elderly population in rural China, especially for those vulnerable groups of people, but it has had limited impacts on the insured urban population. While most developed countries already have comprehensive social health insurance systems in place, some developing countries have been attempting to establish nearly universal health insurance systems. Most of the developing countries focused on broad coverage of health insurance at the beginning and then moved to improve the health insurance system later on.[34] How improvements in health insurance benefits affect enrollees' outcomes has been largely overlooked in the literature. The findings from this paper show that an innovative way of increasing social health insurance benefits can help reduce the potentially high medical costs for those who had shallow insurance benefits before, like the rural residents who are insured by NCMS. This provides practical experience for other developing countries in offering extra social safety protection to the poor elderly. This paper still has some limitations. First, it would be valuable to study the impact of CII on household catastrophic medical expenses or occurrences. Due to the tradition of inter-generational transfer from adult children to elderly parents to support their livelihoods (Wu and Li, 2014), it is hard to define real annual household income for rural households. This makes it impossible to know whether the incurred medical expenses can be defined as catastrophic expenditures. Second, I cannot apply the newly proposed staggered difference-in-differences estimators well due to the data structure of CHARLS (biennial survey data) and the consecutive CII rollout years (2012-2015). If better annual data are available in the future, combined with the time variation of CII, there should be some more precise estimates of CII's impact. Third, the health effects of CII may not have been realized yet due to the short duration of the study after CII, but this program may have a more sizable long-term impact since health improvement is a cumulative process. Also, there may be meaningful effects on mortality in the long run. Hence, future studies should pay more attention to the CII's impact on health measures.

[34]For example, Seguro Popular in Mexico, Universal Health Insurance in Thailand, and Voluntary Health Insurance and Free Health Insurance in Vietnam.

1.7 Tables

Table 1.1: Main Features of China's Three Basic Health Insurance Schemes

Schemes	Urban Employee Basic Medical Insurance(UEMI)	Urban Residents Basic Medical Insurance(URMI)	New Cooperative Medical Insurance Scheme (NCMS)
Launch Year	1998	2007	2002
Insured Population	Urban employee with formal jobs and retired	Urban hukou residents without formal jobs or self-employed	Rural hukou residents
Pooling Level	City	City	County
Premiums Contribution	Employee and employer	Individual and government subsidies	Individual and government subsidies
Mandatory	Yes	No	No
Inpatients Service	Yes	Yes	Yes
Outpatients Service	Yes	Yes (Limited)	Yes (Limited)
Coverage Level	High	Medium	Low
Critical Illness Insurance	No	Yes	Yes

Note: This table is adapted from Fang et al. (2018) and Liang and Langenbrunner (2013). It describes three main basic public health insurance schemes in China. Starting from 2016, URMI and NCMS have been gradually integrated together as the Urban and Rural Residents Basic Medical Insurance Schemes (URRMI). In 2010, the UEMI average premium employer contribution was 7.43%, and the employee contribution was 2% of the employee's total wage. The URMI premium in 2010 included minimum government subsidies of around 120 yuan (USD$19), and individual contributions varied from 100 yuan to 400 yuan (USD$15-$61). The NCMS average premium in 2010 included a minimum government subsidy of 120 yuan (USD$19) and an individual contribution of 36 yuan (USD$5.5) from the China Health Statistics Yearbook. Limited outpatient service in URMI and NCMS means they only cover part of diseases' outpatient services, like chronic or fatal diseases.

Table 1.2: Summary Statistics for Control Variables

	Full sample	CII in 2012 Pre-CII only	CII in 2013 Pre-CII only	CII in 2014 Pre-CII only	CII in 2015 Pre-CII only
A. Individual-level controls					
Age	60.650	56.610	59.530	59.610	59.860
	(9.841)	(8.683)	(9.731)	(9.770)	(9.984)
Male	0.463	0.463	0.460	0.455	0.447
	(0.499)	(0.500)	(0.498)	(0.498)	(0.497)
Urban location	0.267	0.316	0.253	0.284	0.210
	(0.442)	(0.466)	(0.435)	(0.451)	(0.407)
Married	0.867	0.932	0.867	0.872	0.868
	(0.340)	(0.253)	(0.339)	(0.334)	(0.338)
Education level	1.316	1.374	1.252	1.302	1.297
	(0.587)	(0.636)	(0.537)	(0.570)	(0.570)
Supplemental	0.032	0.000	0.057	0.024	0.014
insurance	(0.176)	0.000	(0.231)	(0.152)	(0.115)
Commercial	0.010	0.011	0.010	0.010	0.024
insurance	(0.102)	(0.102)	(0.101)	(0.101)	(0.152)
Ever had disability	0.289	0.216	0.189	0.215	0.182
	(0.453)	(0.412)	(0.392)	(0.411)	(0.386)
Has chronic diseases	0.615	0.705	0.695	0.674	0.692
	(0.487)	(0.457)	(0.460)	(0.469)	(0.462)
Personal annual	3,331	456	1,268	1,622	1,386
income (yuan)	(11,057)	(1,868)	(4,427)	(7,671)	(6,084)
B. City-level controls					
GDP per capita (yuan)	43,040	51,426	36,446	32,187	30,186
	(28,835)	(18,745)	(19,959)	(17,756)	(20,560)
Num of hospital bed	25,448	14,322	22,653	20,497	16,052
	(22,577)	(1,676)	(21,967)	(12,180)	(11,821)
Num of licensed	12,324	9,181	10,757	9,967	8,958
health physician	(10,865)	(461)	(10,153)	(6,995)	(7,022)
Average annual employee	50,444	42,044	35,963	34,270	34,624
wage (yuan)	(16,052)	(3,524)	(6,344)	(5,968)	(7,612)
Integration dummy	0.311	0.000	0.037	0.053	0.003
	(0.463)	0.000	(0.190)	(0.225)	(0.051)
Observations	44,132	190	2,895	5,297	2,295

Note: This table shows the summary statistics for the control variables. The sample presented in this table is the NCMS sample used for the main analysis. Standard deviations are in parentheses. Pre-CII means the first survey wave in 2011.

Table 1.3: Summary Statistics for Outcome Variables

	Full sample	CII in 2012 Pre-CII	CII in 2013 Pre-CII	CII in 2014 Pre-CII	CII in 2015 Pre-CII
OOP inpatient costs	798.9	588.3	449.7	506.9	485.7
	(5270.3)	(2604.2)	(3063.0)	(4165.0)	(3285.4)
OOP outpatient costs	152.5	195.8	128.6	102.1	199.4
	(1550.6)	(644.4)	(1254.7)	(855.3)	(3939.0)
Num of inpatient visit	0.196	0.174	0.118	0.137	0.125
	(0.683)	(0.500)	(0.446)	(0.622)	(0.633)
Num of outpatient visit	0.455	0.379	0.431	0.513	0.440
	(1.463)	(0.786)	(1.432)	(1.568)	(1.243)
Fair/poor	0.781	0.794	0.782	0.785	0.810
self-reported health	(0.413)	(0.406)	(0.413)	(0.411)	(0.392)
Standardized	0.003	0.150	(0.038)	0.095	0.119
mental score	(0.992)	(1.034)	(1.000)	(1.014)	(0.950)
Observations	44,132	190	2,895	5,297	2,295

Note: This table shows the summary statistics for outcome variables. The sample presented in this table is the NCMS sample used for the main analysis. Standard deviations are in parentheses. Pre-CII means the first survey wave in 2011. All the medical costs are measured in Chinese yuan.

Table 1.4: Average Marginal Effects of CII on Medical Costs and Number of Visits

Panel A. Inpatient	OOP inpatient costs			Inpatient visits
	Extensive	Intensive	Combined	Combined
	(1)	(2)	(3)	(4)
CII	-0.00653	-2,753***	-365.7***	-0.00961
	(0.00768)	(1,031)	(137.3)	(0.0169)
Pre mean	0.0806	6,062	488.8	0.130
Pre SD	0.272	11621	3688	0.580
Observations	44,111	5,031	44,111	44,132
Panel B. Outpatient	OOP outpatient costs			Outpatient visits
	Extensive	Intensive	Combined	Combined
	(1)	(2)	(3)	(4)
CII	0.00860	-142.5	-18.61	0.00315
	(0.00938)	(162.8)	(31.48)	(0.0351)
Pre mean	0.194	679.2	131.9	0.473
Pre SD	0.396	4574	2033	1.456
Observations	44,121	8,133	44,121	44,132
Individual income	mean= 3330.13		median= 0	
HH income per person	mean= 6067.92		median= 1134.48	

Note: This table shows the average marginal effects of CII medical costs and the number of visits for the NCMS group. The data is cross-sectional four-wave CHARLS data (2011, 2013, 2015, and 2018), and individual cross-sectional weights are used in the regression. Extensive margin effects are estimated using the logit model, and intensive margin effects are estimated using GLM. The combined marginal effects on medical costs are estimated using the "twopm" command. In Column (4), the marginal effects of CII on the number of visits are estimated using a negative binomial model. All the regressions include city fixed effects, year fixed effects, and controls. Control variables include individual controls and city-level controls listed in Table 1.2. Robust standard errors are clustered at the city level and are shown in parentheses. Pre-treatment mean and standard deviation are calculated based on the first wave survey data (2011). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 1.5: Combined Average Marginal Effects of CII on Medical Costs

	TPM			OLS	
	(1)	(2)	(3)	(4)	(5)
Panel A.OOP inpatient costs					
CII	-344.1***	-370.9***	-382.8***	-402.2***	-424.5**
	(131.4)	(137.0)	(138.8)	(142.1)	(173.8)
Pre mean	479.9	479.9	480.2	488.3	452
Pre SD	3611	3611	3613	3686	3626
Panel B. OOP outpatient costs					
CII	-7.738	-5.004	-2.915	-9.807	18.42
	(29.56)	(29.98)	(30.40)	(37.22)	(34.27)
Pre mean	127.5	127.5	127.7	131.9	126.9
Pre SD	1976	1976	1977	2033	2101
Observations	47,184	47,184	47,170	44,132	40,386
Integration dummy	N	Y	Y	Y	Y
Individual controls	N	N	Y	Y	Y
City-level controls	N	N	N	Y	Y
Individual FE	N	N	N	N	Y

Note: This table shows the combined average marginal effects of CII on medical costs and the number of visits for the NCMS group with changing control variables and model specifications. The data is cross-sectional four-wave CHARLS data (2011, 2013, 2015, and 2018), and individual cross-sectional weights are used in the regression. Columns (1)-(3) are estimated using two-part models and report the combined average marginal effects. Columns (4) and (5) are estimated using OLS models. All the regressions include the city fixed effect and the year fixed effect. Individual controls and city-level controls are listed in Table 1.2. Robust standard errors are clustered at the city level and are shown in parentheses. Pre-treatment mean and standard deviation are calculated based on the first wave survey data (2011). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 1.6: Average Treatment Effects of CII on Inpatient and Outpatient Visits

	Inpatient visits			Outpatient visits		
	Extensive	Intensive	Poisson	Extensive	Intensive	Poisson
	(1)	(2)	(3)	(4)	(5)	(6)
CII	-0.00646	0.118	-0.00841	0.00493	0.0621	0.0222
	(0.00804)	(0.103)	(0.0166)	(0.0105)	(0.182)	(0.0350)
Pre mean	0.0862	1.438	0.130	0.209	2.265	0.473
Pre SD	0.281	1.358	0.580	0.406	2.471	1.456
Observations	44,111	5,681	44,132	44,121	8,763	44,132

Note: This table shows the average treatment effects of CII on the number of visits with different model specifications for the NCMS group. Columns (1)-(2) and (4)-(5) are estimated using the hurdle model, where the extensive margin is estimated using the logit model and the intensive margin is estimated using the truncated-Poisson model. All four columns report the average marginal treatment effects. Columns (3) and (6) are estimated using the Poisson model and report the combined average marginal effects. The data is cross-sectional four-wave CHARLS data (2011, 2013, 2015, and 2018), and individual cross-sectional weights are used in the regression. All the regressions include city fixed effects, year fixed effects, and controls. Control variables include individual controls and city level controls listed in Table 1.2. Robust standard errors are clustered at the city level and are shown in parentheses. Pre-treatment mean and standard deviation are calculated based on the first wave survey data (2011). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 1.7: Combined Average Marginal Treatment Effects of CII for URMI group

	Medical Costs		Num of visits	
	OOP inpatient	OOP outpatient	Inpatient	Outpatient
	(1)	(2)	(3)	(4)
CII	246.4	60.45	0.00990	-0.118
	(579.5)	(83.03)	(0.0599)	(0.114)
Pre mean	921.6	182.9	0.159	0.409
Pre SD	6587	1192	0.511	1.414
Observations	2,596	2,592	2,717	2,717
Individual income	mean= 8608.46		median= 682.35	
HH income per person	mean= 10396.77		median= 5354.37	

Note: This table shows the results of the combined average treatment effects of CII medical costs and the number of visits for the less affected URMI group. Medical cost columns (1) and (2) are estimated using the two-part model. Columns (3) and (4) are estimated using the negative binomial model. The data is cross-sectional four-wave CHARLS data (2011, 2013, 2015, and 2018), and individual cross-sectional weights are used in the regression. All the regressions include city fixed effects, year fixed effects, and controls. Control variables include individual controls and city level controls listed in Table 1.2. Robust standard errors are clustered at the city level and are shown in parentheses. Pre-treatment mean and standard deviation are calculated based on the first wave survey data (2011). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 1.8: Combined Average Marginal Treatment Effects of CII: Robustness Checks

	Medical Costs		Num of visits	
	OOP inpatient	OOP outpatient	Inpatient	Outpatient
	(1)	(2)	(3)	(4)
A.Add part of URRMI group	-365.8**	-10.47	-0.0136	0.00474
	(142.6)	(29.66)	(0.0173)	(0.0368)
Pre mean	132.1	132	0.130	0.472
Pre SD	2030	2030	0.579	1.454
Observations	46,421	46,431	46,443	46,443
B. Live in insurance set-up place	-293.8**	-17.34	-0.00156	-0.0184
	(130.8)	(31.70)	(0.0165)	(0.0367)
Pre mean	134.8	134.8	0.131	0.481
Pre SD	2080	2080	0.587	1.476
Observations	40,867	40,871	40,882	40,882
C. Group 2015 as control group and first two waves data	-213.5*	13.98	-0.0206	-0.0395
	(127.0)	(44.24)	(0.0337)	(0.0541)
Pre mean	161.6	161.6	0.123	0.433
Pre SD	2738	2738	0.535	1.336
Observations	11,510	11,510	11,530	11,530

Note: This table shows the combined average marginal treatment effects of CII on medical costs and the number of visits for the NCMS group using different sample restrictions. Medical cost results are estimated using two-part models, and the number of visits results are estimated using negative binomial models. The data is cross-sectional four-wave CHARLS data (2011, 2013, 2015, and 2018), and individual cross-sectional weights are used in the regression. All the regressions include city fixed effects, year fixed effects, and controls. Control variables include individual controls and city-level controls listed in Table 1.2. Robust standard errors are clustered at the city level and are shown in parentheses. Pre-treatment mean and standard deviation are calculated based on the first wave survey data (2011). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 1.9: Combined Average Marginal Treatment Effects of CII for UEMI group

	Medical Costs		Num of visits	
	OOP inpatient	OOP outpatient	Inpatient	Outpatient
	(1)	(2)	(3)	(4)
CII	186.3	29.88	-0.142	0.0743
	(363.7)	(69.30)	(0.123)	(0.0810)
Pre mean	816.4	164.8	1.638	0.459
Pre SD	3856	1305	53.33	1.643
Observations	7,996	8,049	8,123	8,123
Individual income	mean= 24162.51		median=21090.8	
HH income per person	mean= 18019.26		median=11817.54	

Note: This table shows the placebo test results of the combined average treatment effects of CII medical costs and the number of visits for the non-affected UEMI group. Medical cost columns (1) and (2) are estimated using the two-part model. Columns (3) and (4) are estimated using the negative binomial model. The data is cross-sectional four-wave CHARLS data (2011, 2013, 2015, and 2018), and individual cross-sectional weights are used in the regression. All the regressions include city fixed effects, year fixed effects, and controls. Control variables include individual controls and city-level controls listed in Table 1.2. Robust standard errors are clustered at the city level and are shown in parentheses. Pre-treatment mean and standard deviation are calculated based on the first wave survey data (2011). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 1.10: Heterogeneous Combined Average Marginal Treatment Effects of CII-Vulnerable Groups

	Medical costs		Num of visits	
	OOP inpatient	OOP outpatient	Inpatient	Outpatient
	(1)	(2)	(3)	(4)
A. Elderly	-708.9***	-36.87	-0.00522	-0.0431
(age>=60)	(252.3)	(42.77)	(0.0253)	(0.0581)
Pre mean	563.1	101.6	0.172	0.556
Pre SD	3965	626.6	0.676	1.667
Observations	22,492	22,481	22,505	22,505
Individual income	mean= 2085.63		median= 660	
HH income per person	mean= 4704.43		median= 827.23	
B. Lower income	-515.9***	-52.93	-0.0452*	-0.00331
	(179.8)	(45.58)	(0.0269)	(0.0523)
Pre mean	546.6	130	0.138	0.506
Pre SD	3880	1169	0.566	1.512
Observations	22,786	22,778	22,795	22,795
Individual income	mean= 548.61		median= 0	
HH income per person	mean= 581.39		median=206.77	
C. Has Chronic	-515.3***	-24.62	-0.0161	0.0679
diseases	(196.3)	(45.30)	(0.0230)	(0.0530)
Pre mean	132.4	132.6	0.160	0.587
Pre SD	988.5	988.7	0.620	1.639
Observations	27,120	27,123	27,135	27,135
Individual income	mean= 2690.33		median= 0	
HH income per person	mean= 5484.17		median= 978.49	

*Note:*This table shows the heterogeneous combined average treatment effects of CII on different groups of people among the NCMS insured. Panel A shows the results for the elderly group, which has age greater or equal to 60 years old. Panel B shows the results for the lower-income group of people. The lower-income individual is defined as a person who lives in a household with a per-person income that is lower than the local median household income per person. Panel C presents the results for the group of people who reported having at least one chronic disease. Medical cost columns (1) and (2) are estimated using the two-part model. The number of visits columns (3) and (4) are estimated using a negative binomial model. The data is cross-sectional four-wave CHARLS data (2011, 2013, 2015, and 2018), and individual cross-sectional weights are used in the regression. All the regressions include city fixed effects, year fixed effects, and controls. Control variables include individual controls and city level controls listed in Table 1.2. Robust standard errors are clustered at the city level and are shown in parentheses. Pre-treatment mean and standard deviation are calculated based on the first wave survey data (2011). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 1.11: Heterogeneous Combined Average Marginal Treatment Effects of CII-Other Groups

	Medical costs		Num of visits	
	OOP inpatient	OOP outpatient	Inpatient	Outpatient
	(1)	(2)	(3)	(4)
A. Middle-aged	-38.71	6.453	-0.000411	0.0465
(age<60)	(151.0)	(39.95)	(0.0163)	(0.0413)
Pre mean	427.9	157.6	0.0942	0.403
Pre SD	3444	2698	0.481	1.248
Observations	21,522	21,618	21,627	21,627
Individual income	mean= 4618.85		median= 0	
HH income per person	mean= 7476.25		median= 2150.79	
B. Higher income	-163.7	-8.682	0.0275	-0.00234
	(202.8)	(39.72)	(0.0240)	(0.0425)
Pre mean	427.9	134.2	0.121	0.438
Pre SD	3474	2658	0.594	1.394
Observations	21,301	21,321	21,337	21,337
Individual income	mean= 6287.88		median= 620.32	
HH income per person	mean= 11902.84		median= 6120	
C. No chronic	-135.9	11.77	0.00268	-0.0518
diseases	(141.5)	(34.30)	(0.0192)	(0.0394)
Pre mean	131.8	130.6	0.0646	0.225
Pre SD	3331	3312	0.476	0.897
Observations	16,862	16,981	16,997	16,997
Individual income	mean= 4329.77		median= 0	
HH income per person	mean= 6951.49		median= 1529.70	

Note: This table shows the heterogeneous combined average treatment effects of CII on different groups of people among the NCMS insured. Panel A reflects the results for the middle-aged group, which has age of less than 60 years old. Panel B shows the results for the higher-income group of people. A higher-income individual is defined as a person who lives in a household with a per-person income that is higher than the local median household income per person. Panel C presents the results for the group of people who reported having no chronic diseases. Medical costs columns (1) and (2) are estimated using the two-part model. The number of visits columns (3) and (4) are estimated using a negative binomial model. The data is cross-sectional four-wave CHARLS data (2011, 2013, 2015, and 2018), and individual cross-sectional weights are used in the regression. All the regressions include city fixed effects, year fixed effects, and controls. Control variables include individual controls and city-level controls listed in Table 1.2. Robust standard errors are clustered at the city level and are shown in parentheses. Pre-treatment mean and standard deviation are calculated based on the first wave survey data (2011). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

1.8 Figures

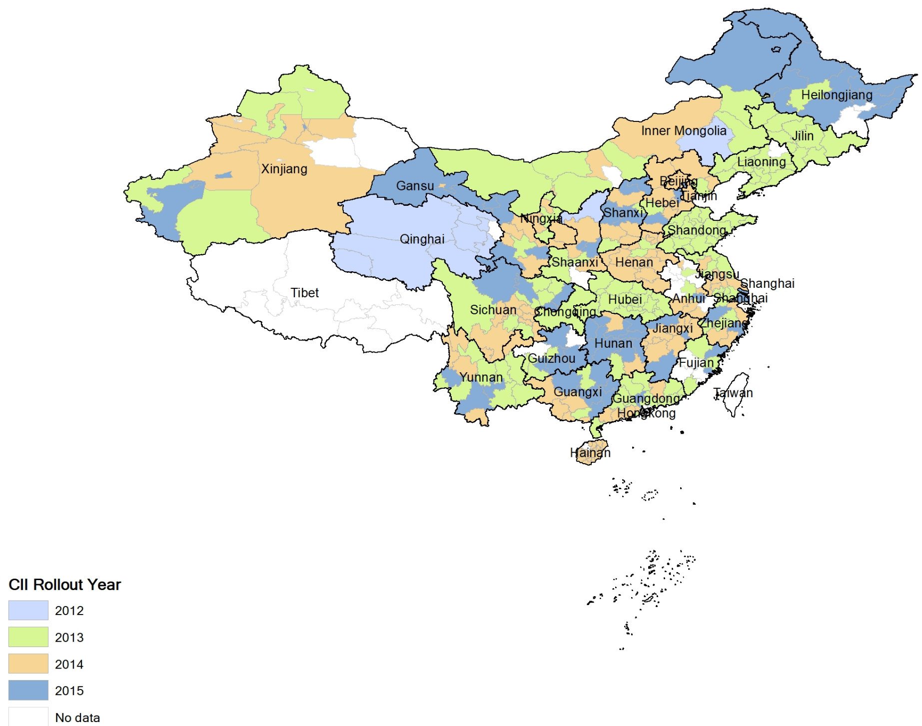
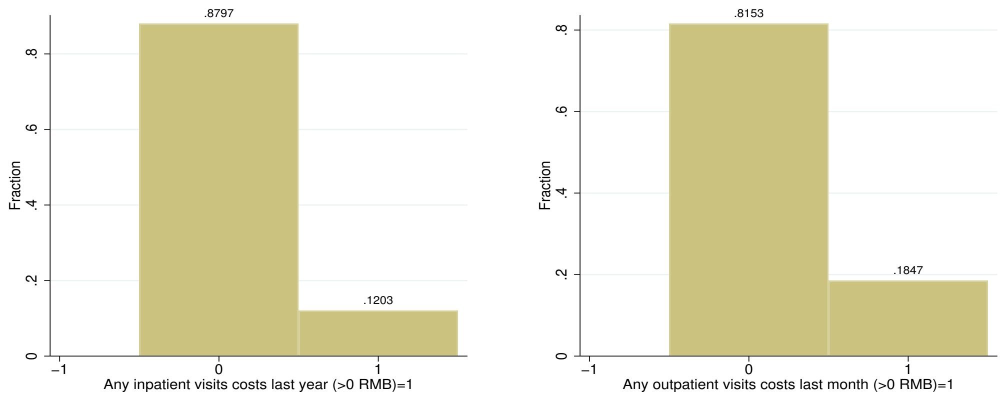
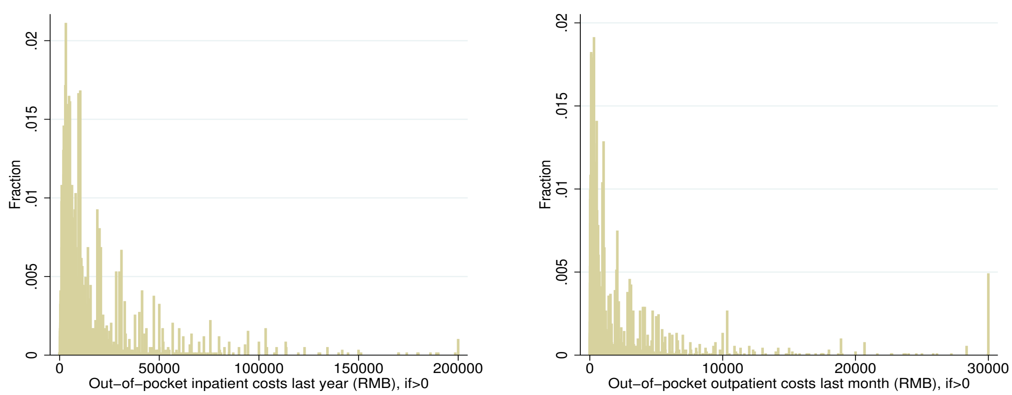


Figure 1.1: City-Level CII Roll Out Information

Note: This map shows the time variation that cities implemented the CII program in China for the NCMS group. The CII information is collected by the author from the PKU Law website. From the unique CII dataset, there are 11 cities that rolled out CII in 2012, 145 cities in 2013, 124 cities in 2014, and 93 cities in 2015 nationwide. There are also a few cities' exact CII effective dates that cannot be verified.



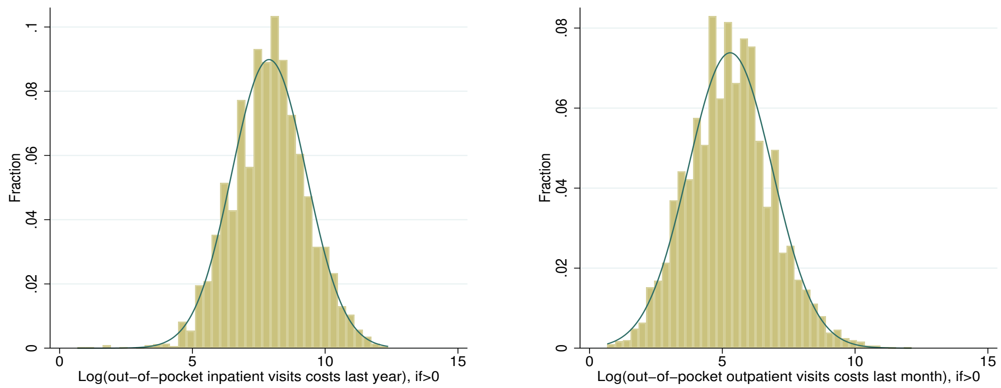
(a) Any inpatient/outpatient costs



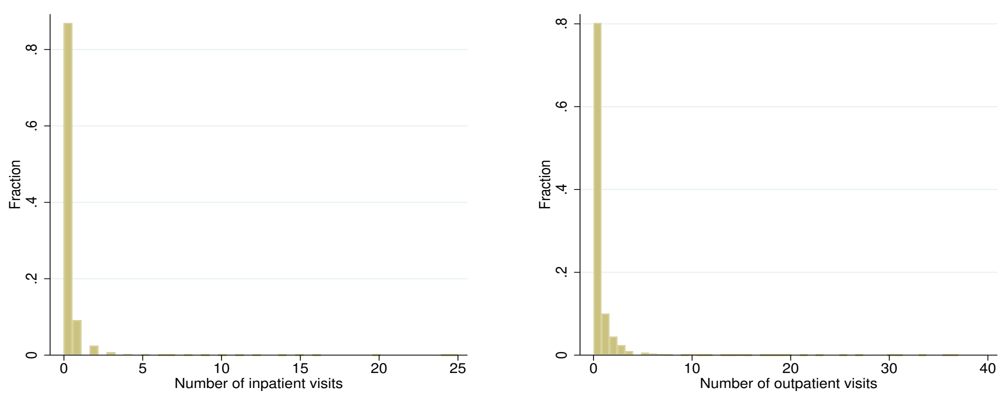
(b) Total out-of-pocket inpatient and outpatient costs (RMB), if >0

Figure 1.2: The Distribution of Out-of-Pocket Inpatient and Outpatient Health Expenditures

Note: This graph shows the distribution of out-of-pocket inpatient and outpatient costs. Panel (a) shows the distribution of having any inpatient or outpatient medical costs. Panel (B) shows the distribution of out-of-pocket inpatient and outpatient costs if there are any positive values. The inpatient costs are top coded at 200,000 yuan (US$3095), and the outpatient costs are top coded at 30,000 yuan (US $464) for graphing. The data is from the author's calculation of four waves of CHARLS data (2011, 2013, 2015, and 2018).



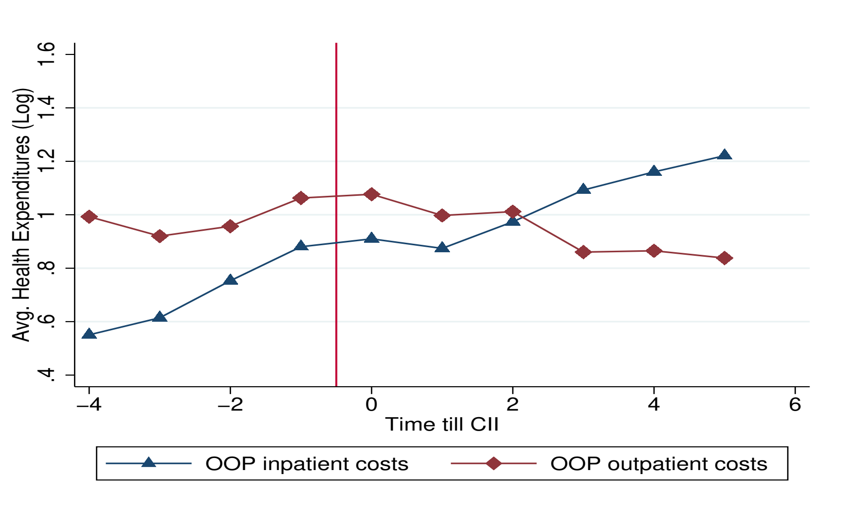
(a) Log (Out-of-pocket inpatient and outpatient costs (RMB)), if >0



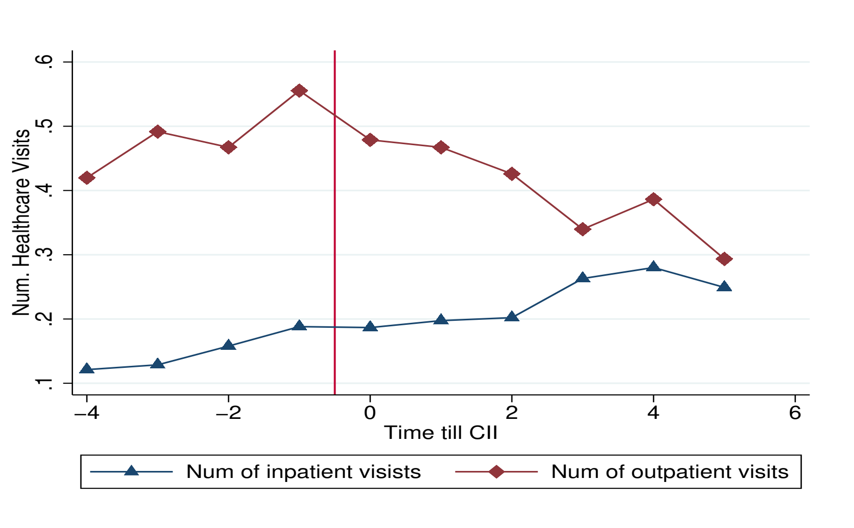
(b) Number of inpatient/outpatient visits

Figure 1.3: The Distribution of Medical Costs and Number of Visits

Note: This graph shows the distribution of medical costs (log) and the number of visits. Panel (a) shows the distribution of the log value of out-of-pocket inpatient and outpatient medical costs if there is a positive value. Panel (b) shows the distribution of inpatient and outpatient visits. The data is from the author's calculation of four waves of CHARLS data (2011, 2013, 2015, and 2018).



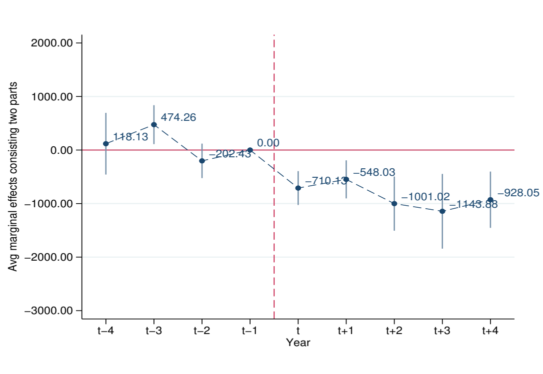
(a) Medical costs trend



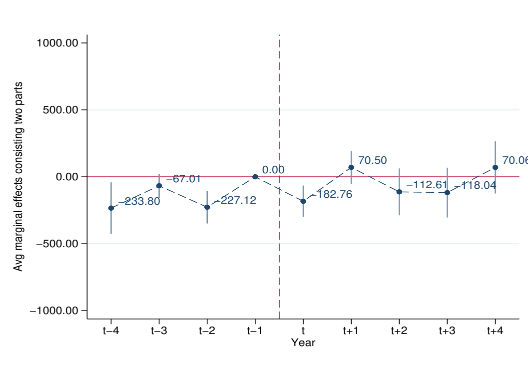
(b) Number of visits trend

Figure 1.4: Medical Costs and Number of Visits Trends Before and After CII

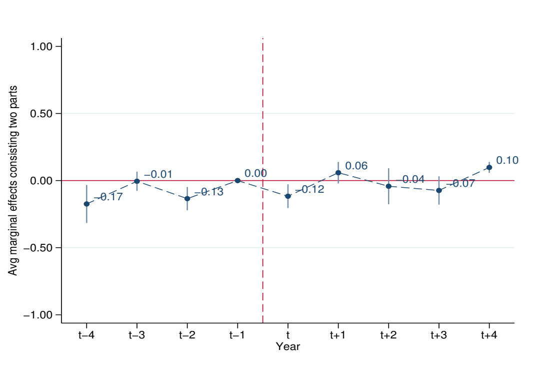
Note: This graph shows the overall average of out-of-pocket (OOP) medical costs and the number of visits trends before and after the CII program. Panel (a) shows the trends in average out-of-pocket inpatient and outpatient costs. Panel (b) shows the trends in the number of inpatient and outpatient visits over the years. The data is from the author's own calculation from four waves of CHARLS data (2011, 2013, 2015, and 2018).



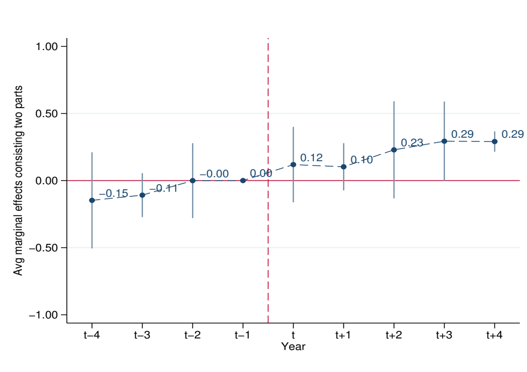
(a) OOP inpatient costs



(b) OOP outpatient costs



(c) Inpatient visits last year



(d) Outpatient visits last month

Figure 1.5: The Effects of CII on Medical Costs and Number of Visits

Note: This graph shows the dynamic average marginal effects of CII on medical costs and the number of visits. The data is cross-sectional four waves CHARLS data (2011, 2013, 2015, and 2018), and individual cross-sectional weights are used in the regression. The combined marginal effects of medical costs are estimated using "twopm" command. The number of visits regressions are estimated using negative binomial models. All the regressions include city fixed effect, year fixed effect, and controls. Control variables include individual controls and city level controls listed in Table 1.2. Robust standard errors are clustered at the city level and shown in parentheses. All graphs are plotted at 90% confidence interval.

Chapter 2 Impact of Increasing Generosity of Public Health Insurance on Health-Related Outcomes: Evidence from China's Critical Illness Insurance Program

2.1 Introduction

Achieving universal health insurance coverage (UHC) is many countries' primary goal for health reforms. UHC requires that families do not experience excessive financial strain from receiving necessary medical treatment (Wagstaff et al., 2016). Starting from 1998, China is also on its way to pursuing this objective. Although more than 98 percent of the population (around 1.35 billion residents) has been covered by one of the public health insurance programs, the World Health Organization reports that China's universal health insurance system is still lacking financial protection (WHO, 2017). To increase the financial protection level of existing public health insurance programs, China's central government launched a nationwide health insurance benefits increase program, the Critical Illness Insurance (CII), in 2012.[1] This paper examines the impact of this recent insurance expansion program on the self-reported health outcomes, health service utilization, and risky health behaviors of adults in China.

It has been well studied about the impact of getting public health insurance coverage (i.e., the extensive margin of coverage) on a wide variety of health-related outcomes in different countries with different programs, but less evidence exists on the impact of increasing generosity of existing public health insurance programs (i.e., the intensive margin).[2] The RAND Health Insurance Experiment found that reducing patient cost-sharing has minimal effects on health status for the general population, with the exception of improving health for the sickest and poorest, and it also has no impact on smoking and drinking behaviors (Manning et al., 1987). However, this experiment occurred roughly 40 years ago and involved participants from a developed Western country. Studying the intensive margin effects of the CII program in China provides more recent evidence from a rapidly developing East Asian country. Knowing whether and to what extent the benefits increase in public health insurance can affect different outcomes is informative for policy designs, especially for developing countries that first increased their insurance coverage

[1] This program is also called the Catastrophic Medical Insurance (*Da Bing Yi Bao*).

[2] For example, Sommers et al. (2017) and Soni et al. (2020) reviewed the literature which uses quasi-experimental designs to study the impact of the Affordable Care Act (ACA) in the United States; Erlangga et al. (2019a) summarize the literature of public health insurance coverage expansion in developing countries.

rate but with a limited benefits coverage package. The CII program provides additional benefits to New Cooperative Medical Insurance Scheme (NCMS) and Urban Residents Basic Medical Insurance Scheme (URMI) enrollees. Those insured by these two insurances will automatically be insured by CII once their local government starts this program without any premium. After getting the first reimbursement from their basic medical insurance schemes, either NCMS or URMI, the insured will get a second reimbursement if their annual accumulated, eligible out-of-pocket (OOP) medical costs are higher than their local CII deductible. CII aims to increase the reimbursement ratio for NCMS and URMI schemes and help widely insured participants avoid illness-induced poverty.[3] CII expansion is also considered an essential part of China's ongoing health system reform, and it is seen as an important way to better protect the health of people in need (Meng et al., 2019).

Increasing health insurance generosity could affect health-related behaviors and outcomes in multiple ways. For example, better access to health care could increase its utilization, thereby improving health, but could also lead to riskier behaviors because of the reduced financial downside risk of illness (ex-ante moral hazard) (Courtemanche et al., 2018b). However, in Chapter 1, I found no evidence that CII increased health care utilization, seemingly ruling out such mechanisms. Instead, Chapter 1 showed that CII's effect is to reduce out-of-pocket (OOP) medical costs among the rural middle-aged and elderly, as the government bore a larger share of the costs. Therefore, any effects on health behaviors or health outcomes are likely attributable to an income effect. In addition, society lacks comprehensive social security benefit coverage, and old-age support usually comes from multi-generational cash transfers. The income effects may apply not only to the middle-aged and elderly individuals who are insured but also to non-elderly individuals through spillover effects.

Theoretically, increased income enables people to spend more on healthcare service utilization, consume more healthy products or services, as well as participate in more leisure activities, all of which could contribute to an improvement in people's overall health (Grossman, 2017). But the income effect could also make insured people increase their risky health behaviors using the money that would otherwise be used for medical services, which may impair health, such as smoking, drinking, illicit drugs, or junk food consumption (Cawley and Ruhm, 2011). The evidence on the effect of income on health-related behaviors and outcomes is mixed. For instance, associational research has documented that increased income leads to better physical and mental health with more health care services (Ettner, 1996; Smith, 1999), but it also increases alcohol consump-

[3]The total number of government-run basic medical insurance scheme participants in China is 1.36 billion, and NCMS and URMI covered 1.01 billion (around 75%) participants by the end of 2020.

tion (Ettner, 1996) and has an inverted U-shaped relationship with body mass index (Lakdawalla and Philipson, 2002). Researchers use different types of income shocks to study the effects on health outcomes. One type of exogenous positive income shock from lottery prize winnings shows that higher income leads to better health and no changes in weight (Lindahl, 2005), positive impact on self-reported health but no changes in smoking or drinking behaviors (Kim and Koh, 2021). However, Apouey and Clark (2015) use similar positive income shock only found better mental health but no changes in self-reported health status, as well as increased smoking and drinking behaviors. Frijters et al. (2005) use plausibly exogenous jump in income in East Germany relative to West Germany after German reunification as an income shock and finds a small positive effect of income on health satisfaction. Kim and Ruhm (2012) and Carman (2013) explore the impact of inheritance on health, and they find no evidence that increased income has improved health. The income effects led by Earned Income Tax Credit (EITC) show improvement in self-reported health (Lenhart, 2019), positive impacts on Body Mass Index (BMI) and obesity for eligible women (Schmeiser, 2009), decreased maternal smoking (Averett and Wang, 2013), more cigarettes smoked by smokers and lower probability of cessation (Kenkel et al., 2014), while Collin et al. (2021) find the state level EITC has no impact on self-rated health status, psychological distress, alcohol consumption, and smoking. Literature study the impact of ACA expansion at its earlier stages, showing that it has no impact on risky behavior change (Simon et al., 2017; Courtemanche et al., 2018a) and a modest impact on self-reported health (Simon et al., 2017). In the later years of ACA expansion, Courtemanche et al. (2019) show it increased risky drinking, while Cotti et al. (2019) find little impact on risky health behaviors, and Soni et al. (2020) show the expansion reduced smoking and increased the probability of exercise. Over the life cycle, income increase does not lead to any changes in subjective or objective health measures, but it leads to more tobacco and alcohol consumption (Adda et al., 2009). Therefore, the causal effect of income on health and risky behaviors is ambiguous and varies from context to context.

The objective of this chapter is to provide new evidence on the impact of the health insurance benefit expansion on self-assessed physical and mental health outcomes, healthcare utilization, and risky behaviors of adult Chinese people. By exploiting the phased rollout of the CII program across cities in China, I use a generalized difference-in-difference design accompanied by a flexible event study to examine its causal impact. Using the unique city-level CII program information matched with the individual-level data from a representative national survey data, I find CII has little improvement in the probability of reporting poor health status, and there is no evidence showing that CII has had any significant effects on other self-reported health outcomes or risky behaviors, such as mental health, smoking, or risky drinking behaviors. The results are robust to the different model specifications. Specifically, people who are insured by URMI, the elderly, and the lowest income group experienced larger impacts from CII. Although consistent with the main results, most insignificant heterogeneity results indicate that CII had little help to the relatively vulnerable groups of people. The results suggest that CII has not led to significant changes in health service utilization or health improvement, and there is no ex-ante moral hazard issue with the newly expanded CII program.

This paper contributes to the literature on both the effects of health insurance and income on health-related behaviors and outcomes by focusing on a novel intervention: a medical insurance program that provides additional reimbursement to the insured if they still have high OOP medical costs after getting the first reimbursement from existing health insurance programs. As previously discussed, existing literature has extensively studied the health and behavioral impacts of income effects, and an abundance of studies focus on the health impacts of gaining public health insurance coverage. Examples include the Medicare Card et al. (2008), Oregon Health Insurance Experiment (Finkelstein et al., 2012), Massachusetts healthcare reform (Courtemanche and Zapata, 2014), and ACA expansion (see Soni et al. (2020) for a review of this literature). However, only a few recent studies focus on the benefit changes of medical insurance along the intensive margin. Shigeoka (2014) and Fukushima et al. (2016) focus on the cost-sharing plan change for elderly in Japan, and Feng et al. (2020) study the similar reduced cost-sharing for previous employed urban elderly. But the literature lacks evidence of the income effects induced by such health insurance generosity changes on health and risky behaviors. As the first paper provides estimates of the impact of such a nationwide program on health and behavior changes in a developing country, it enriches our understanding of the comprehensive effects of the ongoing healthcare reforms.

In addition, this paper uses a different dataset that surveys a broader age range sample (above 18-year-old population) compared to the main dataset I used in Chapter 1 to further look into the impact of CII on health service utilization. The finding of a null impact on healthcare utilization for the general adult population is consistent with the main result in Chapter 1 that there is only a decrease in OOP inpatient costs but no changes in utilization for middle-aged and elderly people. The two findings from different chapters both show that CII has a negligible impact on healthcare service utilization.

The rest of the paper is organized as follows: Section 2 introduces the public health insurance system in China and related literature. Section 3 describes the data part, and Section 4 discusses the empirical strategy. Section 5 presents the main results, robustness checks, placebo tests, and heterogeneity analysis. Section 6 concludes.

2.2 Public Health Insurance System in China

In this section, I first discuss China's public health insurance system and its recent expansion. Then, I discuss the related literature, which evaluates the impacts of China's different health insurance schemes on health outcomes and risky behaviors. Finally, I explain more detailed contributions of this paper based on the literature that studies the effects of public health insurance in China.

China's nearly universal health insurance coverage system includes three main public health insurance schemes. The Urban Employee Basic Medical Insurance Scheme (UEMI) covers urban employees who have formal jobs. Urban residents without a formal job who are not eligible for the UEMI can participate in the Urban Residents Basic Medical Insurance Scheme (URMI). For example, students and self-employed workers who have urban hukou are usually insured by URMI. Rural Hukou residents are eligible to enroll in the New Rural Cooperative Medical Scheme (NCMS).[4]

Because of the relatively high premium of UEMI compared to the other two residents' health insurance schemes, the coverage and benefit level are usually higher for the beneficiaries of UEMI. There are mixed findings on the impact of the introduction of UEMI on health status. He and Nolen (2019) show that UEMI increased the use of preventative health care and decreased the probability of being sick for non-state-owned workers. However, Liu and Zhao (2006) and Huang and Gan (2017) argue that UEMI had null effects on the probability of reported poor health. Unlike UEMI, NCMS and URMI are highly subsidized by the government, and participants only need to pay a small portion of the premium. Therefore, the financing level of these two insurance schemes is lower than UEMI, and thus the benefits and coverage are relatively lower. Existing literature has documented that the introduction of NCMS and URMI has greatly increased health service utilization, including preventive care (Babiarz et al., 2010), outpatient and inpatient services (Wagstaff et al., 2009a; Liu and Zhao, 2014). Although the main goal of NCMS and URMI is to reduce the high OOP medical costs of rural and urban residents without formal jobs, the findings consistently suggest that these two public health insurance programs have little impact on reducing OOP expenditures (Wagstaff et al., 2009a; Lei and Lin, 2009; Cheng et al., 2015; Pan et al., 2016).

There is limited evidence on the impact of NCMS and URMI on health outcomes and risky behaviors, and the results are mixed. Pan et al. (2016) find URMI increased enrollees' probability of reporting good health and Cheng et al. (2015) show there is an improvement in physical health status. Other papers conclude that neither NCMS nor URMI has signifi-

[4]In recent years, some cities have gradually combined URMI and NCMS into the Urban and Rural Residents Basic Medical Insurance Scheme (URRMI).

cant effects on self-reported health status (Lei and Lin, 2009), self-reported sick or injuries experience (Cheng et al., 2015), and other objective health measures (Donato and Rokicki, 2016). One explanation for the null impact on health is that the remaining high enrollees' OOP medical costs limited the formal medical healthcare utilization (Lei and Lin, 2009). As for the behaviors change after gaining health insurance coverage, on the one hand, Qin and Lu (2014) find NCMS increased rural enrollees' tendency towards smoking, excessive drinking, physical inactivity, consuming junk food, and being overweight. Similar results are found when focusing on rural elderly samples only (Fu et al., 2017). On the other hand, Dong et al. (2018) show evidence that URMI has no effect on these risky behaviors but only slightly increases the probability of urban enrollees spending more time in sedentary activities. Starting in 2012, the local government gradually established CII to further reduce the financial burden of medical costs for URMI and NCMS enrollees, and this program was established nationwide by the end of 2015. With the coverage of CII, URMI and NCMS enrollees will automatically receive additional reimbursement based on their local CII plan if their annual OOP medical costs are higher than the local CII deductible. Typically, the local deductible is the previous year's local disposable income per capita.[5] Only a few studies evaluate the effects of the CII program on medical costs, and they find CII has little or small effects on reducing OOP costs using different cities' medical costs data (Li et al., 2019a; Jiang et al., 2019; Yu et al., 2021). One quasi-experimental design paper shows that this program increased rural household consumption but not healthcare consumption (Zhao, 2019). No research exists that looks at how CII affects health outcomes and risky behaviors.

All that said, literature has shown ample evidence of the effects of basic health insurance programs on various outcomes in China, but little is known about the effects of health insurance's benefits increase from the intensive margin. As I have presented in Chapter 1 that CII largely decreased insured rural middle-aged and elderly people's OOP costs, it is reasonable to expect that CII may have some effects on either health outcomes or unhealthy behaviors.

More specifically, this paper contributes to China's public health insurance study in three ways. First, to the best of my knowledge, this paper is the first to use a quasi-experiment design to examine the health effects of CII. Recent studies related to CII mainly focus on the direct impact of CII on medical expenditures (Fang et al., 2018; Zhao et al., 2019), but without further analysis of its impact on health measures. Second, the findings of this paper not only complement the health effects of China's public health insurance programs but also provide new evidence about their impact on smoking, drinking, and

[5]Table A1 in the Appendix presents the differences in CII plans among three example cities.

BMI. These unhealthy behaviors relate to ex-ante moral hazards in the health insurance literature and have rarely been studied based on China's context. Qin and Lu (2014) and Fu et al. (2017) find the introduction of NCMS increased the probability of rural enrollees engaging in risk behaviors, such as smoking, heavy drinking, sedentary activities, and being overweight. While Dong et al. (2018) argue that urban residents did not change their smoking and drinking behaviors after gaining coverage of URMI but only increased their probability of sedentariness. CII increased the generosity of NCMS and URMI for all enrollees, not only the high-risk elderly. With the income effects and spillover effects, it is valuable to understand the impacts on health and risky behaviors for the general adult population. The third contribution is that this paper uses more extended post-treatment national representative survey data, which allows me to examine the relatively longer-post treatment effects on broad measures of health status and risky behaviors nationwide. In contrast to the short-run, one-year effects study like Zhao (2019), and one or two city case studies (Fang et al., 2018; Zhao et al., 2019; Yu et al., 2021), this paper presents the longer-term dynamic impacts change of the CII program.

2.3 Data

Overview of Data This paper uses data from the China Family Panel Study (CFPS) survey. CFPS is a nationally representative bi-annual longitudinal survey launched by the Institute of Social Sciences at Peking University.[6] The CFPS baseline survey was conducted in 2010, and a total of 14,960 households and 42,590 individuals were surveyed. Until now, CFPS has had one baseline survey and four complete follow-up surveys in 2012, 2014, 2016, and 2018.[7] CFPS surveys 25 provinces in mainland China, representing 95 percent of the total population.[8]

There are five modules in CFPS: community, family roster, family, child, and adult. Samples in these five modules are surveyed independently (Xie and Hu, 2014b). This paper uses data from both the family and adult module data. The household head usually

[6] CFPS data can be accessed from `http://www.isss.pku.edu.cn/cfps/en/about/introduction/index.htm`. Since I only obtained county-level information for CFPS 2010, for the other four waves, I excluded samples for which county-level information could not be identified based on CFPS 2010. I thank the Institute of Social Science Survey at Peking University for providing the data.

[7] According to the CFPS-39 technical report, the CFPS 2018 household cross-sectional interview rate is 69.3%, and the cross-wave follow-up interview rate is 86.6%, as measured by the completion of the questionnaire at the household level. The individual-level cross-sectional response rate is 67.4%, and the cross-wave individual follow-up rate is 80.8%. When focusing only on baseline genetic members, which was defined in CFPS 2010, the completion rate is 64.5%.

[8] CFPS excludes Xinjiang, Tibet, Qinghai, Inner Mongolia, Ningxia, Hainan, Hongkong, Macao, and Taiwan.

answers the family questionnaire, while the adult questionnaire is responded to by individuals over 16 years of age living in this household.

CFPS is well suited for this study because it asks a wide range of survey questions related to health insurance status, physical and mental health conditions, health service utilization, and risky health behaviors in the adult module. In addition, CFPS is the most recent available survey data with a time period that spans three calendar years before and five calendar years after the earliest CII implementation in its surveyed cities. This relatively long time frame allows me to trace the dynamic impact of CII on respondents' comprehensive health-related outcomes.

CII Rollout Time CII rollout data is mainly obtained from the PKU Law database. This database includes all laws and regulations that the central and local governments have released.[9] By searching the PKU Law database for all official documents related to CII policies at the prefecture city-level, I use the effective dates listed on these official documents to define the timing of CII rollout in each city. For cities that didn't have any CII policy-related documents in this database, I added online news articles and official documents from local government websites.[10] Figure 2.1 presents the time variations of CII rollout across cities.

As I discussed earlier in section 2, CII is a program for both NCMS-and URMI-insured people. URMI is mostly run by the city government at the prefecture level, while NCMS is mostly run by the county government. County-level NCMS-CII policies usually follow their local upper-level prefecture city's policies, with a few exceptions.[11] These two insurance-scheme groups in the same city may not have CII started simultaneously due to differences in administrative agencies. There are 48 cities among 127 cities in CFPS 2010 that have NCMS- and URMI- CII programs started at different years. Usually, it is a one-or two-year difference. Since I can observe the county information for the individ-

[9] This database is available at `https://www.pkulaw.com/`. Huang and Wu (2020) uses this source to retrieve the effective date of urban-rural health insurance integration at the prefecture city level. Liu et al. (2021) uses this database to construct a unique health policy database to study the overall impact of healthcare policies on healthcare spending in China.

[10] There are 23 prefecture-level cities out of a total of 228 cities with available information that lack official CII documents in the PKU Law database, and their CII information is supplemented using additional resources. The population in these 23 prefecture-level cities accounts for around 7.61% of the total population in 2010.

[11] Unlike in the United States, a county (*Xian*) is within the prefecture city (*Shi*), and usually counties are administered by the prefecture cities. There are five tiers of administrative division units in China. The first tier of administrative division units, at the provincial level, includes provinces, autonomous regions, and municipalities directly under the Central Government. The second tier division is at the prefecture level, with prefecture-level cities, autonomous prefectures, prefectures, and leagues. The third tier division is at the county level, which usually has districts, county-level cities, and counties, autonomous counties. The fourth tier division is at the township level, and the fifth division is at the basic level of autonomy.

uals in CFPS, for the counties where NCMS has different CII rollout times compared to their prefecture city-level time, I use effective CII data found in county local news reports, government reports, or official regulations to define their CII treatment time. Of the 127 cities in the CFPS, 23 counties administered by 23 cities have NCMS-CII information that differs from their prefecture city-level timing, and their CII information is supplemented by the county-level program information.

I then match the CII information with the individual-level survey data in CFPS. The earliest cities started CII in 2013, and the last wave of CII implementation was in 2015. For the most populated affected insurance group-NCMS, from the CFPS survey data, 60 counties rolled out CII in 2013, 49 counties in 2014, and the rest of 53 counties in 2015.

Outcome Variables This paper focuses on three categories of outcome variables: health conditions, health care utilization and total medical costs, and risky health behaviors.

The first set of outcomes is self-reported health conditions. Although it is a subjective measure of a respondent's actual health status, self-assessed health status has been widely used in the literature because it is a strong predictor of an objective health measure of mortality (Idler and Benyamini, 1997; Hurd and McGarry, 2002; DeSalvo et al., 2006; Phillips et al., 2010; Barbaresco et al., 2015). Self-reported health outcomes include three dummy variables reflecting self-reported current health status as poor, self-reported current health condition as poor/fair, any physical discomfort in the past two weeks, and one standardized variable measuring mental health score. Table 2.1 shows how the different outcome variables are constructed, especially how the self-reported health measures are constructed based on CFPS survey questions. Table A4 in the Appendix part describes more details about constructing this standardized mental health score using the related questions asked in the questionnaires.

The second set of outcome variables is about health service utilization and its total costs. This includes whether the respondent had inpatient services last year and was diagnosed by any doctor with any disease in the past six months.[12] With the increased public health insurance benefits, especially with developed coverage of high medical costs, it is reasonable to assume that insured individuals tend to use more health services because they face relatively lower prices for medical services. The following outcome variable is health expenditure as the measure of the total medical costs. The primary goal of CII is

[12]For the outcome of doctor-diagnosed diseases, the original survey question in CFPS is "During the past six months, have you had any doctor-diagnosed chronic disease," and a follow-up question is "What was your doctor's diagnosis of the disease you suffered from?". Based on respondents' answers, some of the diseases listed in the follow-up question are not considered traditional chronic diseases. Thus, I define it as a binary indicator of whether there has been any doctor-diagnosed disease in the past six months.

to reduce the high medical costs for the insured. The direct effect of CII should be the reduction of OOP costs. However, CFPS survey questions regarding individual OOP costs are not consistent among the five waves of data. Therefore, I cannot directly examine whether CII decreased the OOP medical costs for the affected groups of people.[13] A relatively consistent measure of health expenditure is total medical costs (starting from 2012 wave data). It asks for the total medical cost of all diseases and injuries in the past year. Since there are many zero values in the reported total medical costs, I use an inverse hyperbolic sine transformation (IHS) for these values. IHS is similar to the logarithm, but it allows to keep the zero observations (Bellemare and Wichman, 2020). The medical costs are inflated to the 2015 price level.

The third category of the outcomes is a set of risky health behaviors, consisting of a dummy variable of whether they smoked last month, a dummy variable of whether they often drink (at least three times a week), and a continuous variable that measures body mass index. Smoking, excessive drinking, and being overweight are the leading causes of major chronic diseases such as cardiovascular diseases, diabetes, and cancers worldwide. Studies show that these three risky behaviors are also key factors in chronic diseases in China (Murray and Lopez, 1997; Li et al., 2012). Considering that health insurance benefits have been increased after introducing the CII program, insured people may change their health behaviors in response to lower medical costs than before. Table 2.1 describes more details about constructing the different outcome variables.

Control Variables The wide range of control variables include basic demographic information and household background information. Demographic information consists of the respondent's age, gender, marital status (a dummy variable of married or unmarried), education years, and ethnicity (a dummy of whether the respondent is han).[14] In addition, I control for whether the respondent has private supplemental insurance. Household characteristics are household income per capita, residence location (whether the individual lives in urban areas), an individual's employment status (a dummy of whether employed or not), and family size.[15]

[13]There is a question asking about the OOP hospitalization expenses in 2010 and 2012. But starting in 2014, they do not ask about out-of-pocket hospitalization expenses anymore. Instead, CFPS asks about OOP medical expenses, including inpatient and outpatient expenses.

[14]Han is dominant ethnic group in mainland China. In 2010, nearly 91% of the population was categorized as Han. The other 55 ethnic groups are usually grouped as minority groups.

[15]Family size is a continuous variable. For households with a family size equal to or greater than 6, I code it as 6.

Sample Selection The primary question I use in the adult module to define the affected group is based on their reported medical insurance status in baseline wave 2010 or the first time they were surveyed in the CFPS.[16]

The three basic insurance schemes (UEMI, URMI, and NCMS) are typically exclusive. However, anyone with or without the basic insurance scheme can purchase private supplemental insurance. First, I exclude the sample who reported having UEMI but also had URMI or NCMS at the same time.[17] Second, because the CII only affects those insured by URMI or NCMS in the main regression, I keep the sample reported to be insured by URMI or NCMS in the 2010 baseline wave. In the robustness check section, I include the sample reported insured by the UEMI to test whether these two groups of people are affected by the implementation of CII.[18] Third, I also exclude the respondents who started getting into the CFPS survey after their local treatment year. For example, individual i residing in county c may have first received the survey in 2014 and then stayed in the CFPS in 2016 and 2018, but county c received treatment in 2014 and thereafter. In this case, that observation appears in the CFPS dataset only after its initial CII treatment, and no data is observed for the pre-treatment status of the observation.[19] Additionally, I exclude the samples with missing data for any of the control variables.

Table 2.2 describes the summary statistics for the control variables. This table also provides information on the different CII treatment time groups, which offers a good way to check whether counties that adopted CII early are different from the later counties. As seen in Table 2.2, counties that rolled out CII in different years have similar characteristics. Among the three different treatment year groups, only two variables, han (ethnicity) and urban location show a slight difference. Even though the biggest difference in han between the 2013 and 2014 treatment groups, the pre-treatment period sample mean difference is 0.042, which is only about 0.1 of the standard deviation.

[16] Switching insurance schemes between years is not common in China. Based on CFPS data from 2010 (wave 1) to 2012 (wave 2), 8% of the sample reported having a different insurance scheme from the previous wave. Around 11% of the sample reported having different insurance between 2012 (wave 2) and 2014 (wave 3). These numbers in later waves are 10% and 11%, respectively. The original survey question about insurance status is, "Do you have any of the following medical insurance? [Select all that apply]". The answers are public medical insurance, urban employee basic medical insurance, urban residents' basic medical insurance, supplementary medical insurance, new rural cooperative medical insurance, and none of the above. Public Medical Insurance (PMI) is a similar type of Government Insurance Scheme (GIS). Supplemental medical insurance refers to private commercial medical insurance.

[17] The dropped group of people who have UEMI and also URMI/NCMS at the same time is around 0.94% in the baseline 2010 wave data.

[18] Some provinces still have public medical insurance (PMI) for government officials. However, it is gradually being replaced by UEMI, and some cities' PMI reforms are underway. Given that PMI shares similar characteristics as UEMI, I consider the PMI group as the UEMI group in this paper.

[19] These observations are 3% of all observations in CFPS.

2.4 Methodology

The staggered treatment timing of CII varied across counties from 2013-2015 in CFPS, providing a natural quasi-experiment to identify the causal effects of CII on comprehensive health-related outcomes using a generalized difference-in-differences (DD) approach.

$$Y_{irct} = \beta_0 + \beta_1 CII_{rct} + \beta_2 X_{irct} + \alpha_c + \mu_t + \delta_i + \theta_{rt} + \epsilon_{irct} \tag{2.1}$$

Where Y_{irct} refers to different outcome variables I described above in the data section, CII_{irct} is a dummy variable that equals one if insurance r (NCMS or URMI) at county c in year t started CII. X is a vector of the aforementioned covariates, including individuals' specific demographics and household background information that may affect the comprehensive health outcomes. The model also includes year fixed effects μ_t, county fixed effects α_c, individual fixed effects δ_i. Since two insurance groups are affected by CII and these two groups of people may experience different changes over time, I also control for insurance time trend θ_{rt}, and ϵ_{irct} is an error term. All specifications are weighted by the cross-sectional weights, and standard errors are clustered at the county level. The coefficient I am interested in is β_1, which captures the differential changes in the comprehensive health-related outcomes after the CII implementation on the affected groups.

To understand the dynamic effects of CII, I also employ a flexible event study model. The event study model also allows for testing the pretends. The event study specification is as below.

$$Y_{irct} = \alpha + \sum_{\tau=-3, \tau \neq -1}^{3} \gamma_\tau (CII_{rc,t-\tau}) + \delta X_{irct} + \sigma_i + \phi_c + \psi_t + \zeta_{rt} + \xi_{irct} \tag{2.2}$$

Y_{irct} are outcome variables as before. Year dummies for each event year t before or after the CII implementation are included in the model. The year immediately prior to the treatment year is omitted. γ_τ measures the dynamic effects of CII on the outcome variables if year t is τ years after county c's insurance schemes r (NCMS/URMI) starts the CII program. Counties that started CII as early as 2013 have three calendar years before and five calendar years after the CII implementation. While the counties which rolled out CII in 2015 have five years before and three years after the CII treatment. The sample size is very small for 4-5 years before and after CII treatment, so I only control for the three years before and three years after CII. X_{irct} is same as in Equation 2.1. σ_i, ϕ_c, and ψ_t, and ζ_{rt} are individual fixed effects, county fixed effects, year fixed effects, and insurance time trends separately. ξ_{irct} is the error term.

Two placebo tests are conducted to validate the identification strategy. The first one is a permutation test, which assigns the treatment time earlier than the true treatment year to examine whether there are any effects of CII on these different outcomes. I also run the main specification using the sample insured by UEMI, which is the group not affected by the CII program, and see whether there are any impacts on them.

2.5 Results

2.5.1 Main Estimates

Table 2.3 reports the generalized DD model results of CII on three categories of health-related outcomes. In addition to the main estimates, this table also provides the pre-treatment mean and standard deviations for the outcome variables, which offer relative effect size comparability. The first four columns show the impact on self-reported health conditions. From columns (1) and (2), I find CII increased the probability of reporting poor health by around 2.3 percentage points and poor/fair health by 1.2 percentage points. Only the coefficient estimate of reporting poor health is marginally statistically significant, though. Compared to the pre-treatment mean, it is around a 12 percent increase. Columns (3) and (4) show the results of physical discomfort and self-assessed mental health, and no significant evidence shows CII has changed these two outcomes.[20]

Columns (5) and (6) in Table 2.3 present the effect of CII effects on health service utilization for any disease and hospitalization. The point estimates show that the affected insured was 0.7 percentage points more likely to get a doctor-diagnosed disease in the past six months and 0.7 percentage points more likely to have inpatient service last year, even though these two estimates are not statistically significant. Despite the fact that CII has no statistically significant effects on health service utilization, the positive magnitudes of health service usage results are consistent with findings in Medicaid expansion and the introduction of NCMS or URMI in China (Wagstaff et al., 2009b; Liu and Zhao, 2014; Courtemanche et al., 2017). The next column shows that CII has decreased medical costs, but still, it is not statistically significant. Since the dependent variable of total medical costs has been inverse hyperbolic transformed, following Bellemare and Wichman (2020), the interpretation of the estimates now refers to a 5.6% decrease in the total medical costs after CII.

The remaining three columns in Table 2.3 contain the coefficient estimates of risky health behaviors, including drinking often, smoking last month, and BMI. I do not observe any statistically significant effects on these three outcomes. They have very small

[20] CFPS also asks, "How would you rate your health status compared with one year ago?" When I run the main specification on this outcome, the results are very similar, and people are more likely to report worse health compared with one year ago, but the estimate is still not significant.

magnitudes, and the effects of drinking and smoking are close to zero. The null results indicate that there is no evidence of ex-ante moral hazard.

In order to further test whether there are any significant effects of CII on these health-related outcomes, I use factor analysis and principal component analysis (PCA) to examine its impact on these ten outcome variables. Additionally, I obtain eight z-scores of health-related outcomes based on three different outcome categories. The comprehensive measures generated from factor analysis and PCA can help to improve the statistical power by aggregating multiple measures of health (Huang and Zhang, 2021). Panel A in Table 2.4 shows the results of CII effects on the indexes using factor analysis. Panel B presents the impact of CII on the indexes using principal component analysis. The outcome variable in column (1) is an overall health index using the ten outcomes presented before. The next three columns use outcome variables constructed from three categories of outcome variables: self-reported health conditions, health service utilization and medical costs, and risky behaviors separately.[21] Not surprisingly, I find no evidence of significant effects on the overall health index, self-reported health index, health service utilization index, or risky behavior index.

Most of the negligible effects of CII on various outcomes I found before might be because the impacts are very small, or they may appear right after the program and then gradually fade out over the years. Then, I turn to the event study analysis, which allows me to observe the dynamic effects of CII on different outcomes before and after CII implementation. Figure 2.2 displays the event study results of the four self-reported health outcome variables and health service utilization. It is clear that all of the coefficient estimates before CII implementation are not statistically different from zero, which implies no change among the self-reported health conditions before getting covered by CII. Graphs (a) and (b) show an increase in the probability of reporting health as fair and reporting health status as poor or fair after CII. I cannot argue that CII leads people to be more likely to report poor health because the confidence intervals become gradually large in the later years. Graphs (c) and (d) present the results of physical discomfort and mental health. Consistent with the results of self-reported health, I find minimal and insignificant effects on physical discomfort in the past two weeks and mental health status after CII. Graphs (e) and (f) show the change in the CII effects on health service utilization. I do see there is an increasing trend of doctor-diagnosed diseases and a decreasing trend of hospitalization services after CII with no statistical significance. But the hospitalization services in the past years need to be read with caution because there had been a downward trend before

[21] Self-reported health conditions include self-reported fair health, self-reported fair health/food, physical discomfort, and mental health. The health service utilization group includes any diseases, hospitalizations, and medical costs. Risky behaviors group includes smoked last month, drinking often, and BMI.

CII. Similar to the main results in Table 2.3, there are no statistically significant effects found on healthcare service utilization. Figure 2.3 exhibits the results of the remaining four dependent variables. Graph (a) indicates that the total medical cost gradually increased after CII and its magnitudes became even bigger in the later years. With a slightly increasing pre-trend before CII, I cannot rule out any other potential confounders that may affect the total medical costs. As for smoking, drinking behaviors, and BMI, it seems there is no significant change in these three behaviors over time. This supports the main finding that there is no ex-ante moral hazard after the program.

Combining the results of factor analysis, principal component analysis, and event study analysis, I find consistent non-significant results as the main generalized DD regression results. All of these results support the main finding that there is no evidence that CII implementation led to improvements in self-reported health conditions, use of health services, or risky behaviors.

2.5.2 Robustness Checks

To check the sensitivity of the main results, I employ four robustness checks. First, I exclude all the individual and family control variables in the main regression, and Table 2.5 Panel A presents the estimation results. The second robustness check is that I move the treatment year one year forward (e.g., if one city had CII in 2013, here I set its treatment year as 2014.), and its results are shown in panel B. I do this because even though local NCMS/URMI started CII in different treatment years, there might be lagged policy effects, and the insured people may not fully understand this policy in a short period. Thirdly, I use the baseline insurance status as the IV for the current insurance status, and now the treatment status is defined based on the current insurance plan. In this case, I have to assume that the current insurance status as NCMS/URMI is the only mechanism through which CII affects overall health-related outcomes. This assumption is hard to test, but it is informative to see whether most of the null results I found earlier still show no significant effects. The IV results are shown in panel C. The last thing I do for the sensitivity of the specification is that I use the balanced CFPS panel data, and the results are shown in panel D.[22]

Table 2.5 suggests that the main results I found earlier are robust among different model specifications and samples. The significance level and coefficient magnitudes are very similar to the main results. It is worth noting that the results using balanced panel

[22]For the sample size consideration, I use the unbalanced longitudinal sample in the main analyses. The attrition rate in the baseline survey (2010) and follow-up survey (2012) is around 19%. For the other three consecutive waves, the attrition rate stays around 13%.

data indicate that the insured population is three percentage points more likely to report their health status as poor, which is statistically significant. In addition, the coefficient magnitudes of any disease, every hospitalization, and total medical costs are relatively larger compared with the main findings, even though they are not statistically significant. This may indicate that the same individual used more health services after the implementation of CII and that they had a better understanding of their health status, which may have led them to report a worse health status than before.

2.5.3 Placebo Tests

The first placebo test I conduct to validate the identification strategy is running a permutation test that assigns treatment time one year before the actual policy year, and I only use the sample treated in 2013. In other words, I set the CII treatment year as 2012 for those counties which started CII in 2013. And I do not consider the counties treated in 2014 and 2015 in this permutation test.

One potential threat to my main identification strategy is that although the models I presented earlier control for a lot of timing-varying observables, which have an impact on various health-related outcomes, there could be some time-varying unobservables that may lead to changes in health outcomes, healthcare utilization, and risky behaviors. These unobservables may also relate to the timing of CII rollouts in different cities. It is hard to test how large the impact of these unobservables has on the outcome variables I study in this paper, but I test it by employing the main specification on the non-affected UEMI group and seeing if CII has any effect on them during the study period. If there are any significant changes in outcomes for this non-affected group, then the main results I found earlier would be biased and driven by other factors.

The two placebo test estimation results are reported in Table 2.6. As expected, the results of a randomly assigned treatment year in 2012 show no significant effects on nine outcome variables, and only smoking behavior is marginally significant.[23] And most of the non-significant estimates show the opposite signs of magnitude as the main results. Using the unaffected group sample, I also do not find any evidence of significant impacts of CII on their comprehensive health-related outcomes. These two placebo tests support that no other unobservable characteristics drive the main results.

[23]Total medical costs are measured starting from the second wave of the CFPS survey in 2012. So it is not examined in this permutation test.

2.5.4 Heterogenous Effects

NCMS and URMI Groups CII aims to increase the basic insurance protection level for NCMS and URMI groups, while URMI enrollees face relatively higher premiums and coverage rates than NCMS enrollees (Meng et al., 2015). It would be reasonable to think that NCMS enrollees are more likely to get real benefits from CII. Moreover, the NCMS group accounts for around 84% of the main sample, which may drive the main results. Table 2.7 Panel A summarizes CII effects on the NCMS group. The results are very similar to the main analysis, but the coefficient estimator of reporting health status as poor is no longer statistically significant.

Furthermore, I find that the effects of CII are even more prominent for the URMI group, and Table 2.8 Panel A shows the results. One possible explanation might be that URMI enrollees, who usually live in urban areas, are more sensitive to health policy changes relative to their rural counterparts. Or the local governments in the urban areas have widely advocated the newly started CII program but not in the rural areas.

Elderly and Young Adult Groups The elderly are the most at-risk group of people who are likely to have severe diseases and thus incur high medical costs. At the same time, unlike the elderly in developed countries with generous old-age social security support, the elderly in China usually have limited pension income, and most of the old-age support comes from adult children's financial support. The CII program can not only alleviate the financial burden of medical care for the elderly, but it may also have an impact on other adults in the family. So, I stratify the sample into two age groups: those aged 60 or over and those under 60. The impacts on the elderly are shown in Table 2.7 Panel B. The results are close to the main results. But notably, the magnitudes are larger for the elderly. The probability for them to report fair health status has increased by 4.35 percentage points, and it is around a 13% increase relative to the pre-period. For most other outcomes, the magnitudes are also larger but still not statistically significant. The regression results of adults below 60 years old are presented in Table 2.8, and there are minimal and insignificant effects of CII for the younger people.

Income Group The main goal of CII is to help the insured reduce medical costs and decrease the probability of getting into poverty because of high medical costs. Therefore, low-income group people may experience a larger income effect compared with high-income group people because of the CII program. Then, I consider splitting the sample into five different income groups. I present and describe the effects of CII on the lowest income group in this part. The highest income quintile group result is shown in Table

2.8. As can be seen from Table 2.7 Panel C, similar to the main results, I do not find any significant results on the health status, health service utilization, or risky behaviors for the lowest income group of people.

Self-Reported Health Condition Consider that the data I use in this paper is longitudinal survey data, which allows me to examine how people's health-related outcomes change over the years. By restricting the data to the balanced panel data, I split the sample into good and not good (poor/fair) health groups based on their self-reported health status in the pre-treatment period.[24] Table 2.7 Panel D presents the main effects of CII for the group of people who reported not having a good health status before CII. I do not find statistically significant results for CII on most outcome variables. Two exceptions are that the probability of reporting poor health has statistically increased by around 3.62 percentage points, and the standard mental health score has statistically decreased by 0.135 standard deviations. This implies that CII did not improve the physical and mental health of people who already have poor or fair health conditions.

2.6 Conclusion

In this paper, I use five waves of China Family Panel Study data to examine the effects of the CII program on self-reported physical and mental health conditions, health service utilization, medical costs, and risky behaviors. By taking advantage of the time variations of CII implementation in different cities and using a generalized DD strategy combined with event study analysis, I find there is little impact of the CII program on self-reported health outcomes. Although literature based on the US context finds that health insurance programs improve health status immediately after gaining coverage (Finkelstein et al., 2012; Courtemanche and Zapata, 2014), my findings regarding the health impacts of income effect from the increased benefits program are consistent with the impact of public health insurance introduction in China, which shows little evidence of improving health conditions (Liu and Zhao, 2014; Lei and Lin, 2009). This paper also presents no evidence showing that CII has significantly changed formal health service utilization, decreased total medical costs, or affected smoking, heavy drinking, and BMI. The null results on healthcare utilization for the adult population further support my finding in Chapter 1 that there is no evidence that CII increased health service utilization. This suggests that even with the introduction of a better reimbursement plan (extra protection of health insurance), there is no statistical improvement in health service utilization and medical

[24] The pre-treatment period here means the second wave survey, which was conducted in 2012.

costs. This little impact on healthcare utilization and medical costs is also consistent with existing literature which studies the impact of getting coverage for public health (Pan et al., 2016; Liu and Zhao, 2014; Li et al., 2019a). Meanwhile, I do not find any ex-ante moral hazard that would suggest an increase in unhealthy behaviors with better insurance benefits. Similar to the findings in Dong et al. (2018); Yu and Zhu (2018), the basic residents' insurance schemes in China have negligible effects on smoking or drinking behaviors. The few changes in health improvement could also be attributed to these null effects. The heterogeneity analyses also provide evidence that this program has minimal impacts across subgroups, except that CII has a relatively significant and larger effect on people in fair or poor health. Furthermore, CII did not improve health or change risky behaviors in vulnerable groups of people, such as the poor and the elderly.

One potential explanation for the null results of CII on health might be that health improvement is a cumulative process. Thus, it takes longer post periods to experience changes after health interventions in China. Another explanation for the lack of health impacts or behavior changes might be that the group of people who can get CII benefits are the "marginal people" who tend to have extremely high medical costs, which only accounts for a small portion of the affected group. As shown in the Appendix Table A2, CII reimbursement will be incurred once the OOP costs are higher than the local CII's deductibles. Therefore, only those people who have higher medical expenses are more likely to be affected by and benefit from CII. Meanwhile, there would not be any benefits from CII for the group of people who experience high OOP costs but lower than their local CII deductibles. In other words, as Fang et al. (2018) described, the high deductibles may be the main barriers to limiting the number of CII beneficiaries. Although I find CII has reduced the inpatient OOP costs by around 45% for the middle-aged and elderly NCMS-insured people in Chapter 1, the effect of the reduced financial burden on this specific population may not also appear among the general adult population. In addition, the income effects and spillovers brought about by the increased generosity of public health insurance may offset each other or the size of the impacts is too small to be statistically detected in the early years after CII. This is similar to Courtemanche et al. (2018a) that finds null impacts of ACA expansion on risky behaviors and self-reported health in its first two years.

There are still some limitations to this paper. First, in general, I find there are no positive effects of CII on health outcomes, healthcare utilization, or risky health behaviors. The relatively large standard errors of different estimates suggest that this paper may have had insufficient power to detect the effects of the CII program in its early years. Therefore, I cannot argue these are "true" zero effects. If we take the main results I found in Chapter

1 that CII decreased the OOP inpatient costs by 366 yuan (USD 56) for rural middle-aged and elderly people, and we assume that the general population also experiences such an OOP inpatient cost reduction, the results from this paper imply that 365 yuan (USD 56) is going to lead the probability of reporting poor health at most by 4.6 percentage points, and at least less than 0.04 percentage points with a 95% confidence interval. Suppose we take heavy drinking as another example. In that case, the income effects of 366 yuan (USD 56) will at most increase the probability of being a heavy drinker by 3.1 percentage points, and the lower end change will decrease it by 1.9 percentage points. In other words, the income effects driven by the general decrease in OOP inpatient costs may result in very small effect sizes in the early years of the CII program. Second, it would be better to examine the impact of CII on OOP medical costs, particularly inpatient costs, because CII reimbursement is most likely to occur for the high cost of hospitalization services. Chapter 1 finds that CII has reduced the inpatient OOP costs for insured rural enrollees over 45 years old, but it would also be valuable to see if this significant impact can also be found among younger adults. This will give us a better understanding of whether there are any real benefits that young adults can get from the CII program, which could further affect their health and behaviors. Because CFPS has an inconsistency in out-of-pocket hospitalization costs, I can not do so directly. Third, it is also important to study the impact of the CII program on saving or consumption behaviors since it increased the generosity of existing public health insurance programs. Gruber and Yelowitz (1999) document the positive relationship between health insurance coverage and consumption behaviors. Similar results are also found with the introduction of China's rural insurance program (Bai and Wu, 2014). Future research using detailed individual consumption or saving data can shed light on such behavior changes as the response to insurance benefits increases. In addition to addressing these limitations, future work should use data from a longer post-treatment period to focus on health effects or behavioral changes, as insured individuals may not respond to benefit changes in the first few years.

2.7 Tables

Table 2.1: Outcome Variables

Variable name	Survey Question	Definition
A. Self-reported health condition		
Poor health	How would you rate your health status?	1=poor, 0=fair/good
Poor/fair health		1=poor/fair, 0=good
Physical discomfort	During the past two weeks, have you felt any physical discomfort?	1=yes, 0=no
Mental health	How often have you felt depressed and could not cheer up in the past month? etc.	standardized self-reported mental health score
B. Health service utilization		
Any doctor diagnosed diseases	Have you had any doctor-diagnosed chronic disease during the past six months?	1=yes, 0=no
Ever hospitalized	In the past year, were you ever hospitalized due to illness/injury?	1=yes, 0=no
C. Health expenditure		
Medical costs	How much was the total medical costs in the past year?	Inverse hyperbolic sine transformed total medical costs
D. Risky Behaviors		
Smoked last month	Did you smoke cigarettes in the past month?	1=yes, 0=no
Drink often	Did you drink alcohol at least 3 times a week in the past month?	1=yes, 0=no
BMI	What is your current height/weight?	BMI=current weight (kg)/square of current height (m)

Notes: (1) Self-reported current health status as poor and self-reported current health status as poor/fair is constructed from self-reported current health status. (2) The original question in the survey answer is scaled into five categories. From 1 to 5, means “excellent”, “very good”, “good”, “fair”, “poor”. To make it more understandable, I reverse the answer order. 1“poor” 2“fair” 3“good” 4“very good ” 5“excellent”. (3) The self-reported health status question measure is different in the baseline survey 2010 and the other four follow-up surveys. In 2010, the answer scale was “1” very unhealthy, “2” unhealthy, “3” relatively unhealthy, “4” fair, “5” excellent; while in the follow-up surveys, the measure of this same question was changed as 1 “poor,” 2 “fair,” 3 “good,” 4 “very good,” 5 “excellent.” To make all five waves data comparable, I reassign the numerical value. In 2012, 2014, 2016, and 2018 waves data. For the answers with 3 “good” 4 “very good ” 5 “excellent”, I categorize them into 3, which means “good”. So the final code for the self-reported health status is that 1“poor,” 2 “fair” and 3“good.” (4) Standardized mental score is constructed from several different mental health questions. For more mental health questions, see the appendix. (5) Medical expenditures are converted into 2015 value based on the CPI.

Table 2.2: Summary Statistics

	Full sample all waves	Treated in 2013 Pre-treatment waves only	Treated in 2014 Pre-treatment waves only	Treated in 2015 Pre-treatment waves only
Age	48.630	46.610	46.290	46.200
	(15.040)	(14.930)	(15.540)	(15.070)
Male	0.487	0.477	0.491	0.487
	(0.500)	(0.499)	(0.500)	(0.500)
Han	0.912	0.929	0.911	0.870
	(0.284)	(0.257)	(0.284)	(0.336)
Urban location	0.360	0.350	0.308	0.321
	(0.480)	(0.477)	(0.462)	(0.467)
Married	0.853	0.850	0.840	0.842
	(0.354)	(0.357)	(0.367)	(0.365)
Education year	6.073	5.747	5.692	5.696
	(4.475)	(4.314)	(4.546)	(4.440)
Employed	0.677	0.571	0.530	0.543
	(0.468)	(0.495)	(0.499)	(0.498)
Family size	4.162	4.069	4.376	4.305
	(1.480)	(1.408)	(1.387)	(1.394)
Supplementary	0.007	0.004	0.005	0.003
medical insurance	(0.082)	(0.060)	(0.072)	(0.056)
Family income	10.340	10.040	10.170	10.160
per capita (log)	(1.189)	(1.172)	(1.166)	(1.152)
Observations	88,179	14,274	15,290	9,020

Notes: This table shows the summary statistics of the sample used for the main analysis. Male is a dummy variable, and it equals 1 if the respondent is a male; otherwise, 0. Urban location equals 1 if the respondent currently lives in urban areas, and it equals 0 if the respondent lives in rural areas. Married equals 1 if the respondent's marital status is married, and it equals 0 if the respondent is never married, cohabitation, divorced, or widowed. The education year is the highest education year the respondent has. If the respondent has a family size greater than 6, then it is classified into 6. Supplementary medical insurance equals 1 if the respondent reports having any supplemental medical insurance in addition to the basic medical insurance; otherwise, 0. Standard deviations are in parentheses. Pre-treatment period means before the first round of CII implementation and is calculated based on 2010 and 2012 two waves data.

Table 2.3: Effects of CII on Comprehensive Outcomes

	Poor health (1)	Poor/fair health (2)	Physical discomfort (3)	Mental health (4)	Any disease (5)
CII	0.0231*	0.012	-0.001	-0.036	0.007
	(0.012)	(0.016)	(0.026)	(0.072)	(0.016)
Pre mean	0.197	0.459	0.299	0.012	0.136
Pre SD	0.398	0.498	0.458	1.020	0.343
Observations	88,167	88,167	85,830	84,907	85,813
	Ever Hospitalized (6)	**Medical costs (7)**	**Smoked last month (8)**	**Drink often (9)**	**BMI (10)**
CII	0.007	-0.111	0.009	0.006	-0.022
	(0.011)	(0.176)	(0.009)	(0.013)	(0.048)
Pre mean	0.086	5.273	0.312	0.163	22.350
Pre SD	0.281	3.046	0.463	0.370	3.363
Observations	85,833	63,801	85,775	85,771	83,733

Notes: This table shows the results of CII effects on self-reported health conditions, health service utilization, medical costs, and risky behaviors. The data is cross-sectional 5 waves CFPS data (2010, 2012, 2014, 2016, and 2018), and weights are used in the regression. All the regressions include individual fixed effects, county fixed effects, year fixed effects, and urban/rural-year time trends, as well as controls. Control variables include individual age, gender, ethnicity(han), living place location, marital status, education year, employment status, family size, supplementary private health insurance, and log family income per capita. Robust standard errors are clustered at the county level and shown in parentheses. Pretreatment mean and the standard deviation is calculated based on the first two waves' data (2010 and 2012). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 2.4: Effects of CII on Comprehensive Outcomes Using Indexes

	Index for all outcome measures (1)	Index for self-reported health (2)	Index for health service utilization (3)	Index for risky behaviors (4)
A. Factor analysis	-0.014	0.027	-0.005	0.019
	(0.040)	(0.040)	(0.046)	(0.023)
Pre mean	-0.072	0.060	-0.193	0.029
Pre SD	0.895	1.009	0.603	1.011
Observations	61,473	84,912	63,792	82,953
B. Principal component analysis	-0.022	0.039	-0.006	0.022
	(0.066)	(0.057)	(0.059)	(0.027)
Pre mean	-0.137	0.086	-0.249	0.033
Pre SD	1.454	1.446	0.775	1.161
Observations	61,473	84,912	63,792	82,953

Notes: This table shows the results of CII effects on comprehensive outcomes using indexes generated from factor analysis and principal component analysis. The outcome variable in column (1) is the index for all 10 outcome variables presented in Table 2.3. Column (2) uses the index for the first four outcomes in Table 2.3 of self-reported health conditions as the outcome variable. The outcome variable of column (3) is the index of health service utilization constructed from "chronic disease," "ever hospitalized," and "medical costs" variables in Table 2.3. Column (4) represents the outcome variable of the index for risky behaviors of the last three outcome variable-"smoked last month," "drink often," and "BMI" in Table 2.3. Panel A presents the results of using indexes generated from factor analysis, and panel B show the regression results of using indexes constructed from the principal component analysis. The data is cross-sectional 5 waves CFPS data (2010, 2012, 2014, 2016, and 2018), and weights are used in the regression. All the regressions include individual fixed effects, county fixed effects, year fixed effects, and urban/rural-year time trends, as well as controls. Control variables include individual age, gender, ethnicity(han), living place location, marital status, education year, employment status, family size, supplementary private health insurance, and log family income per capita. Robust standard errors are clustered at the county level and shown in parentheses. Pretreatment mean and the standard deviation is calculated based on the first two waves' data (2010 and 2012). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 2.5: Robustness Checks-Effects of CII on Comprehensive Outcomes

	Poor health	Poor/fair health	Physical discomfort	Mental health	Any disease	Every hospitalized	Medical costs	Smoked last month	Drink often	BMI
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
A. No controls	0.0219*	0.010	-0.001	-0.035	0.006	0.006	-0.120	0.008	0.004	-0.026
	(0.012)	(0.016)	(0.025)	(0.070)	(0.016)	(0.011)	(0.178)	(0.008)	(0.013)	(0.049)
B. Move treatment year	0.0189*	0.007	0.000	-0.031	0.012	-0.004	-0.068	0.008	0.010	-0.055
one year forward	(0.010)	(0.014)	(0.020)	(0.066)	(0.016)	(0.009)	(0.185)	(0.010)	(0.011)	(0.047)
C. IV	0.021	0.013	-0.001	-0.033	0.009	0.000	-0.115	0.011	0.006	-0.027
	(0.014)	(0.018)	(0.029)	(0.078)	(0.017)	(0.012)	(0.194)	(0.009)	(0.014)	(0.049)
D. Balanced panel	0.0313***	0.009	-0.001	-0.046	0.009	0.015	0.204	-0.001	-0.009	-0.030
	(0.008)	(0.018)	(0.021)	(0.070)	(0.019)	(0.013)	(0.171)	(0.013)	(0.012)	(0.036)
Pre Mean	0.193	0.471	0.307	-0.002	0.140	0.083	5.373	0.309	0.168	22.59
Pre SD	0.395	0.499	0.461	1.006	0.347	0.275	2.988	0.462	0.374	3.314
Observations	45,532	45,532	45,311	45,050	45,305	45,307	35,466	45,290	45,288	44,314

Notes: This table shows the robustness check results of CII effects on self-reported health conditions, health service utilization, medical costs, and risky behaviors. The first row shows CII effects without controlling the individual demographic controls and basic family background information. Weights and robust standard errors cluster level are the same as Table 2.3. The second row shows CII effects on the comprehensive outcomes moving the real treatment time one year forward since there might be lagged policy effects. Control variables, weights, and robust standard errors cluster level are the same as Table 2.3. The third row presents the IV results of using baseline insurance status as the IV of the current insurance status. Control variables, weights, and robust standard errors cluster level are the same as Table 2.3. The first stage IV coefficient is 0.99, and the first stage KP Wald F statistic is 125,350. The fourth row shows the results of CII effects using 5 waves balanced panel CFPS data (2010, 2012, 2014, 2016, and 2018). Panel weights are used in the regression. All the regressions include county fixed effect, year fixed effect, individual fixed effects, urban/rural-time trend, and control variables. The control variables and robust standard errors level are the same as Table 2.3. Sample size, pretreat mean and standard deviations in the first three rows are similar to the main estimates in Table 2.3. The Pretreatment mean and standard deviation shown in the table above are calculated based on the first two waves of balanced panel data (2010 and 2012).* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 2.6: Falsification Tests

	Poor health (1)	Poor/fair health (2)	Physical discomfort (3)	Mental health (4)	Any disease (5)
A. Treatment 2012	-0.019	-0.029	0.038	-0.005	-0.010
	(0.014)	(0.033)	(0.027)	(0.083)	(0.017)
Observations	37,338	37,330	35,692	34,562	35,674
B. UEMI and PBMI	0.008	-0.020	0.053	-0.087	0.024
	(0.026)	(0.041)	(0.033)	(0.123)	(0.039)
Pre mean	0.129	0.456	0.29	-0.129	0.187
Pre SD	0.335	0.498	0.454	0.852	0.39
Observations	16,700	16,700	16,606	16,500	16,605
	Ever Hospitalized (6)	**Medical costs (7)**	**Smoked last month (8)**	**Drink often (9)**	**BMI (10)**
A. Treatment 2012	0.013		0.0218*	0.013	0.013
	(0.011)		(0.012)	(0.011)	(0.011)
Observations	35,690		35,626	35,622	33,420
B. UEMI and PBMI	0.003	0.426	0.007	-0.002	-0.024
	(0.025)	(0.342)	(0.017)	(0.023)	(0.163)
Pre mean	0.106	5.459	0.317	0.189	23.39
Pre SD	0.308	3.276	0.465	0.392	3.357
Observations	16,605	11,892	16,588	16,585	16,566

Notes: This table shows the placebo test results of CII effects. The first row shows the estimates of moving the first treatment year one year before the real treatment year. The data used is 2 waves cross-sectional CFPS data (2010, 2012). The second row shows the estimates of CII effects on the group of people who are insured by Urban Employee Basic Medical Insurance (UEMI) which is not affected by CII. The control variables, weights, fixed effects, robust standard errors cluster levels, and the calculation of pretreatment mean and standard deviations are the same as Table 2.3. The pretreatment mean and standard deviation shown in the table are calculated based on the group of UEMI. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 2.7: Heterogeneous Effects-Effects of CII on Vulnerable Groups

	Poor health (1)	Poor/fair health (2)	Physical discomfort (3)	Mental health (4)	Any disease (5)	Every hospitalized (6)	Medical costs (7)	Smoked last month (8)	Drink often (9)	BMI Index (10)
A. NCMS	0.021	0.020	0.004	-0.025	0.006	0.003	-0.075	0.009	0.006	-0.001
	(0.013)	(0.017)	(0.028)	(0.077)	(0.018)	(0.011)	(0.194)	(0.009)	(0.014)	(0.049)
Pre mean	0.201	0.456	0.299	0.029	0.133	0.085	5.25	0.317	0.165	22.27
Pre SD	0.401	0.498	0.458	1.033	0.339	0.28	3.027	0.465	0.371	3.355
Observations	80,944	80,944	78,633	77,800	78,616	78,636	58,930	78,584	78,581	76,631
B. Elderly	0.0435*	-0.023	-0.016	-0.146	0.008	0.013	-0.023	-0.001	0.014	-0.133
(60+)	(0.026)	(0.025)	(0.034)	(0.112)	(0.039)	(0.030)	(0.226)	(0.017)	(0.018)	(0.134)
Pre mean	0.341	0.637	0.394	0.079	0.216	0.134	6.043	0.31	0.172	21.74
Pre SD	0.474	0.481	0.489	1.126	0.412	0.34	2.845	0.462	0.378	3.552
Observations	21,607	21,607	21,525	21,022	21,514	21,527	16,614	21,515	21,515	20,116
C. Family income	0.024	-0.014	-0.028	-0.118	-0.014	-0.017	-0.169	-0.002	0.021	0.011
1st quintile	(0.029)	(0.031)	(0.034)	(0.123)	(0.041)	(0.023)	(0.322)	(0.017)	(0.023)	(0.141)
Pre mean	0.295	0.542	0.355	0.218	0.160	0.104	5.572	0.332	0.16	21.87
Pre SD	0.456	0.498	0.479	1.154	0.367	0.306	2.931	0.471	0.367	3.299
Observations	17,391	17,391	17,101	16,765	17,093	17,100	12,448	17,077	17,077	16,290
D. Poor/fair health	0.0362**	0.000	0.015	-0.135*	0.011	0.023	0.210	-0.005	-0.008	-0.012
	(0.014)	(0.019)	(0.025)	(0.076)	(0.026)	(0.018)	(0.204)	(0.017)	(0.012)	(0.066)
Pre mean	0.323	0.768	0.430	0.175	0.205	0.108	6.013	0.281	0.143	22.53
Pre SD	0.468	0.422	0.495	1.088	0.404	0.31	2.781	0.45	0.35	3.419
Observations	24,582	24,582	24,484	24,312	24,480	24,481	19,010	24,478	24,477	23,802

Notes: This table shows the heterogeneous CII effects. Panel A of the table shows the results of CII on people who are insured by NCMS only. Panel B shows the results for elderly people who are aged 60 and above. Panel C presents the results for people who have the lowest family income per capita quintile. The data, control variables, weights, fixed effects, robust standard errors cluster levels, and the calculation of pretreatment mean and standard deviations are the same as Table 2.3. Panel D shows the results of using balanced panel data who reported poor or fair health status in their baseline survey wave. The data, control variables, weights, fixed effects, robust standard errors cluster levels, and the calculation of pretreatment mean and standard deviations are the same as Table 2.5 balanced panel data part. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 2.8: Heterogeneous Effects-Effects of CII on Other Groups

	Poor health (1)	Poor/fair health (2)	Physical discomfort (3)	Mental health (4)	Any diseases (5)	Every hospitalized (6)	Medical costs (7)	Smoked last month (8)	Drink often (9)	BMI (10)
A. URMI	0.0442*	-0.046	-0.037	-0.147	0.028	0.031	-0.448	0.004	0.001	-0.141
	(0.024)	(0.037)	(0.040)	(0.113)	(0.036)	(0.031)	(0.346)	(0.020)	(0.021)	(0.143)
Pre mean	0.161	0.488	0.299	-0.141	0.168	0.095	5.456	0.263	0.152	23.000
Pre SD	0.368	0.500	0.458	0.894	0.374	0.293	3.214	0.440	0.359	3.351
Observations	8,953	8,953	8,861	8,766	8,860	8,861	6,523	8,855	8,854	8,783
B. Non-elderly	0.015	0.021	-0.002	0.000	0.006	0.005	-0.128	0.010	0.005	0.011
	(0.011)	(0.019)	(0.028)	(0.071)	(0.013)	(0.010)	(0.206)	(0.008)	(0.014)	(0.050)
Pre mean	0.155	0.405	0.272	-0.010	0.111	0.072	5.015	0.312	0.158	22.500
Pre SD	0.362	0.491	0.445	0.986	0.314	0.259	3.069	0.463	0.365	3.297
Observations	64,445	64,445	62,196	61,762	62,190	62,197	45,151	62,153	62,149	61,466
C. Family income	-0.005	0.032	-0.008	-0.001	0.013	0.027	-0.242	0.011	0.009	-0.030
5st quintile	(0.019)	(0.032)	(0.034)	(0.072)	(0.022)	(0.018)	(0.318)	(0.015)	(0.019)	(0.133)
Pre mean	0.131	0.405	0.259	-0.181	0.130	0.084	5.234	0.280	0.172	22.720
Pre SD	0.337	0.491	0.438	0.862	0.336	0.277	3.158	0.449	0.378	3.356
Observations	17,077	17,077	16,556	16,437	16,553	16,556	12,359	16,552	16,550	16,418
D. Good health	0.0218***	-0.008	-0.023	0.065	0.007	-0.001	0.179	-0.002	-0.012	-0.043
	(0.008)	(0.018)	(0.023)	(0.083)	(0.016)	(0.018)	(0.268)	(0.014)	(0.018)	(0.069)
Pre mean	0.040	0.120	0.161	-0.212	0.064	0.052	4.657	0.342	0.198	22.670
Pre SD	0.196	0.325	0.367	0.853	0.244	0.222	3.048	0.474	0.399	3.186
Observations	20,868	20,868	20,745	20,657	20,743	20,744	16,376	20,730	20,729	20,435

Notes: This table shows the heterogeneous CII effects. Panel A of the table shows the results of CII on people who are insured by URMI. Panel B shows the results for adults who are aged less than 60 years old. Panel C presents the results for people who have the highest family income quintile (income classification is based on their pretreatment income). The data, control variables, weights, fixed effects, robust standard errors cluster levels, and the calculation of pretreatment mean and standard deviations are the same as Table 2.3. Panel D of the table shows the results of using balanced panel data who reported good health status before CII implementation. The data, control variables, weights, fixed effects, robust standard errors cluster levels, and the calculation of pretreatment mean and standard deviations are the same as Table 2.5 balanced panel data part. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

2.8 Figures

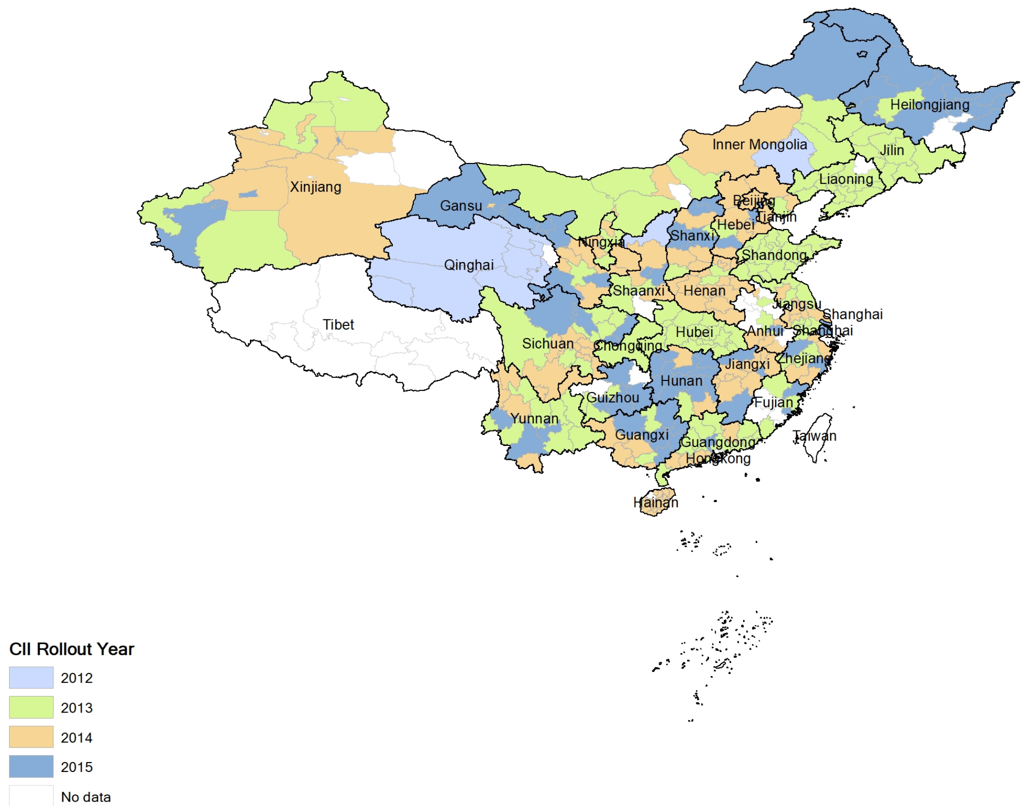
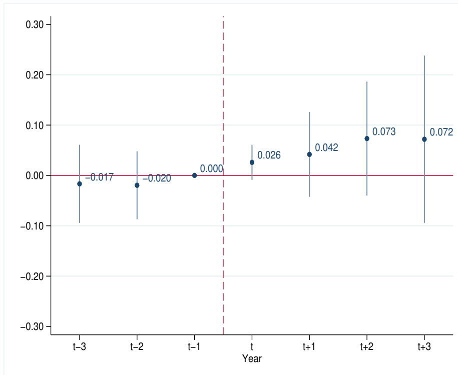
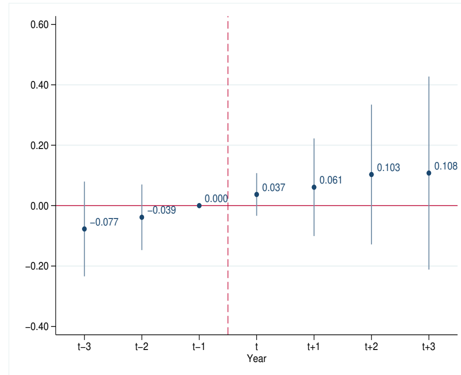


Figure 2.1: CII Rollout Time

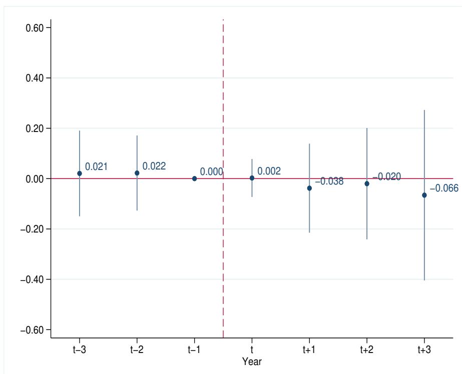
Notes: This map shows the time variations that cities roll out the CII program for the NCMS group. The main analysis uses county-level CII rollout information. Since there is usually only one county surveyed underneath the city level and the majority of the counties' CII rollout time directly follows their upper-level city's rollout time, this map plots the city-level CII rollout information for ease of reading. The CII data is hand collected by the author from the PKU Law database.



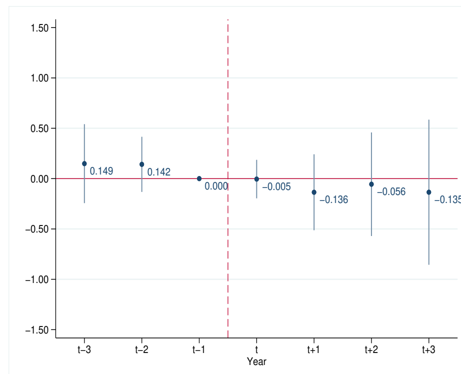
(a) Self-reported health status as poor



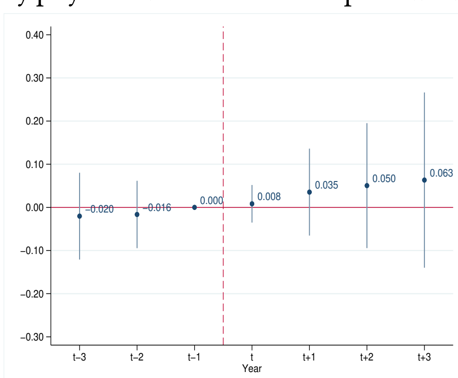
(b) Self-reported health status as poor/fair



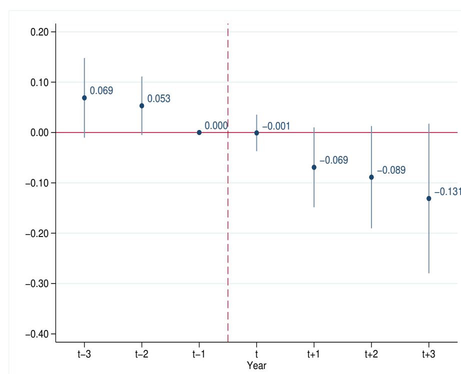
(c) Any physical discomfort in the past two weeks



(d) Standardized mental health score

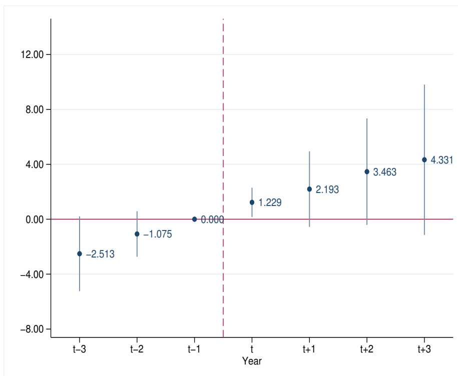


(e) Any doctor diagnosed diseases in the past 6 months

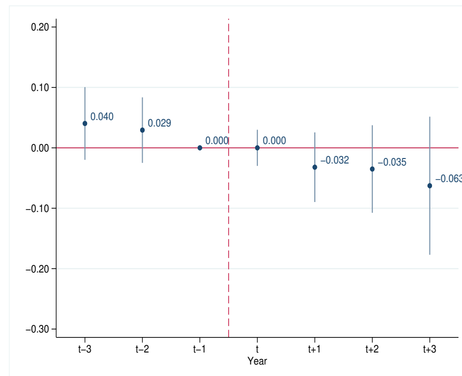


(f) Whether hospitalized in past 1 year

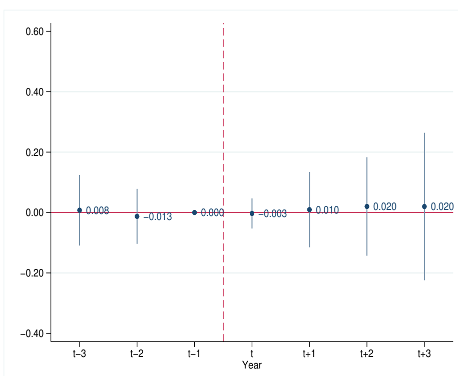
Figure 2.2: The Effects of CII on Self-reported Health and Health Service Utilization



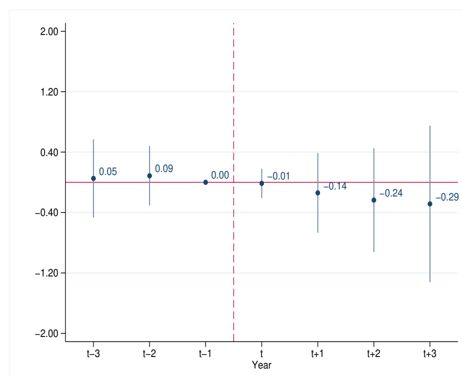
(a) Total medical costs last year(ihs transformed)



(b) Smoked last month



(c) Drink often



(d) BMI

Figure 2.3: The Effects of CII on Medical Costs and Risky Behaviors

Chapter 3 Intergenerational Transmission of Education and Early Childhood Outcomes: Evidence from China

3.1 Introduction

[1] Extensive research has documented that the early childhood environment is vital for later life outcomes (Almond and Currie, 2011; Currie, 2011). Children's health conditions at birth and during their early childhood are especially important as they could improve educational attainment, income, labor supply, and health status (Behrman and Rosenzweig, 2004; Case et al., 2005; Black et al., 2007; Oreopoulos et al., 2008; Smith et al., 2012). Equally important is the early mastery of cognitive and non-cognitive skills, which also significantly affect those outcomes later in life (Heckman et al., 2013). Among various factors that shape children's birth conditions and skills development, maternal education is of special interest to researchers as it plays a major role in determining those early childhood outcomes (Almond et al., 2018).

There is a long-lasting discussion related to the intergenerational transmission of human capital that focuses on how parental factors influence children's outcomes. Theoretically, Becker and Tomes (1979, 1986) demonstrate that parental investment in children is crucial to human capital accumulation. Parents' education, especially maternal education, is essential to a child's health and cognitive skills because more educated people are more efficient producers of health and are more likely to have better health and pass it on to the next generation (Grossman, 1997). In contrast, children with less educated parents tend to have poorer health conditions (Currie, 2009). However, as discussed in Grossman (2006), the empirical challenge is to establish the causal relationship between parental education and children's outcomes; the endogeneity issue of the omitted variables that affect parental education and children's outcomes would invalidate the estimates.

Two main strands of literature use different methods to address this issue. One strand of recent studies utilizes family structure to causally identify the effect of parental education on children's outcomes. Behrman and Rosenzweig (2002) use twins sample to isolate nature effect from nurture effect and a few other studies use adoptees to obtain the exogenous variation in parental education (Sacerdote, 2002, 2007; Chen and Li, 2009). Another strand of the literature uses government policy changes that create plausible exogenous variation in educational attainment for the affected group of people.[2] However, as pointed

[1]This chapter is joint work with Xiaozhou Ding.

[2]Recent literature falling into this strand includes, but not limited to, Currie and Moretti (2003); Black et al.

out by Currie (2009), most of these studies focus on the change induced by policies that increase the lower levels of education, such as compulsory education law reforms, and these studies have found mixed results on the impact of intergenerational transmission of education. Only a few studies investigate the impact of the higher level of education attainment (Currie and Moretti, 2003).[3] For countries that experienced one educational reform, this may not be an issue since differences in adult educational attainment are likely caused by such a reform. However, if there are multiple shocks that affect educational attainment for certain cohorts, the estimate would be biased because those cohorts that are affected by the earlier policy are also likely to be affected by the later reform as well. For instance, China implemented compulsory education reform and higher education expansion in a short period of time, and some cohorts were affected by both policies. Therefore, it is challenging to disentangle the effect of improved education due to the earlier reform from the latter one.

This paper studies the impact of maternal education on a comprehensive set of child outcomes in China, following the second strand of literature. I take advantage of higher education expansion, a recent educational reform implemented in 1999 that massively increased the probability of attending college, to examine the impact on child birth weight, gestational age, and health conditions before age one, along with cognitive skills such as the time the child begins speaking, counting, walking, and self-urinating, via an instrumental variable (IV) approach. To address the endogeneity issue of education and children's outcomes, I utilize the number of colleges in each mother's college-going year and province induced by the expansion as an instrument of the mother's education. It overcomes several challenges in the existing studies. First, the instrument provides variation across both provinces and years. Second, as previous literature established, college expansion in China is an unexpected central government's social and economic development strategy, and it is independent of provincial characteristics, which creates credible exogenous variation in educational attainment, and it is unlikely to affect child outcomes. Third, given that there was a compulsory education reform before the higher education expansion and some cohorts were exposed to both policies, using the most recent reform would deliver a more accurate estimate of the impact of the mother's education on the next generation. I use the China Family Panel Studies (CFPS) 2010 wave as the main data set. Mother's province is defined as her reported province at age 12 since this is the closest year I could get before the college-going year, and migration between age 12 and before

(2005); Lindeboom et al. (2009); Chou et al. (2010); McCrary and Royer (2011); Andrabi et al. (2012); Güneş (2015); Keats (2018).

[3] A recent paper by Cowan and Tefft (2020) follows Currie and Moretti (2003) but look at the impact of college openings on adult health.

college is rare in China. The mother's college-going year is from her self-reported high school graduation year. If she did not attend high school, I use the predicted college-going year. Next, I match the province-year number of colleges with the mother's college-going year and province to perform the two-stage least-squares estimation and obtain the causal effect of maternal education on child outcomes.

Results from the Ordinary Least Square (OLS) show that maternal education is negatively correlated with the probability of having an abnormally weighed baby and preterm birth, though the low birth weight is less precise. Maternal education is also associated with a higher likelihood of reporting the child's having any sickness or hospitalization before age one. The OLS estimates also present a strong negative correlation between maternal education and the time that the child starts to walk independently, speak a full sentence, count from 1–10, and self-urinate. But I do not find similarly statistically significant effects in IV estimations for the full sample. However, the sub-sample analyses show that, for mothers who originally come from rural areas, a one-year increase in education decreases the probability of having a low birth weight child by two percentage points, and it also shortens the time that the child first walks by 0.307 months, speaks a full sentence by 0.675 months, and counts from 1-10 by 1.216 months. To avoid the potential confounding factor of China's Compulsory Schooling Law, I perform robustness checks by restricting the sample to mothers born after 1975 who were fully exposed to this reform. The results are both quantitatively and qualitatively similar to my main findings. Further investigation of the potential channels reveals that assortative marriage and rural-urban migration are the driving forces related to a mother's education, whereas maternal education has a limited impact on their own self-reported health and risky health behaviors.

This paper contributes to the literature in three ways. First, to my best knowledge, this study provides the first estimate using higher education expansion to measure the effects of maternal education on child outcomes in China. Unlike most of the literature that uses people who were affected by policies in the lower-level education distribution, this paper's focus is on the policies that exposed people more to the upper level of the education distribution. Chevalier and O'Sullivan (2007) and Lindeboom et al. (2009) use the same policy and find that mother's education year increase led by the change in minimum school leaving age in the U.K., has limited effects on child health. A similar small effect is confirmed in the U.S. by McCrary and Royer (2011) which use the age at school entry policies in California and Texas, showing that a mother's education has negligible effects on infant birth weight, prematurity, and infant mortality rate. However, other studies in developing countries generally show that maternal education leads to a smaller child mortality rate (Chou et al., 2010; Grépin and Bharadwaj, 2015), reduction in very low birth weight (Güneş, 2015), increased likelihood of having a normal birth weight child, and decreased birth defects rate (Huang et al., 2018), increased completed childhood immunization rate (Özer et al., 2018). The mixed findings related to the mother's education on child health outcomes might be that these studies usually rely on the exogenous variation in education induced by education reforms which increase low levels of education (Currie, 2009). Only limited evidence exists on the impact of mother's education on child outcomes using higher-level maternal education distribution changes. Currie and Moretti (2003) show that the maternal education increase induced by new college openings can improve infant health as measured by birth weight and gestational age. Meanwhile, Carneiro et al. (2013) use local market conditions and college tuition as instrumental variables of mother's education, and they find similar improvements in children's outcomes associated with maternal education. No current study focuses on the impact of post-compulsory education increases on child outcomes in developing countries.

Second, this paper not only studies the impact of maternal education on infant health outcomes but also on early childhood cognitive skills development, which is crucial to later life achievement (Heckman et al., 2013; Campbell et al., 2014). Despite the importance of the development of these skills, little evidence is found. Carneiro et al. (2013) show that there is no evidence that maternal education improves early childhood motor and social skills development up to 24 months in the U.S. This paper provides the first evidence of the effect of maternal education on early childhood cognitive skills development in a Chinese context.

Third, this paper complements existing studies that look at the effects of China's college expansion by providing new evidence of the effects of intergenerational transmission of education. Recent literature on the expansion mainly examines its impacts on intergenerational education mobility (Guo et al., 2019), college premium Hu and Bollinger (2021), and migration choice (Ding, 2021). This paper adds to the literature by providing the effect of college expansion from a different perspective. More than twenty years after China's higher education expansion began, it is valuable to assess its impact on the next generation's outcomes through the lens of intergenerational transmission of education.

The rest of this paper is organized as follows. Section 2 describes the background of higher education expansion in 1999. Section 3 presents the data and main variables, including the summary statistics. Section 4 outlines the identification strategy. The results are explained in detail in Section 5, and Section 6 concludes.

3.2 Higher Education Expansion in China

The formal education timeline in China typically starts with six years of elementary school education. Students can enroll in elementary school once they reach age six but no later than age seven. After completing elementary school, students must attend three years of middle school as required by the Compulsory Schooling Law. Then, if students want to get into college, they must attend three years of high school and take the college entrance exam.

Unlike most developed countries, the central government in China plays an essential role in the development of higher education and its related policies (Che and Zhang, 2018). Since China's sweeping economic reform in the late 1970s, the accompanied state-owned enterprises' reform has created massive scale layoffs. Following the 1997 Asian financial crisis, the central government launched the Higher Education Expansion (HEE) program in July 1999 in order to reduce labor market stress by encouraging more high school graduates to enroll in college and to help economic development due to the demand for highly skilled workers. As suggested by the economist of the Asian Development Bank Mission in China, Ming Tang, the HEE could lead to more education consumption as well as induce more investment in services, constructions, and other industries, which could eventually increase the aggregate demand and therefore help the economic growth in the short term. The unexpected HEE was initiated in 1999 by the central government, and it reflects the government's social and economic development strategy.

The number of colleges went up simultaneously to support the expansion of higher education. As described in the official document published by the Chinese Ministry of Education in 1999, "Action Plan for Education Revitalization in the 21st Century," the central and local governments should gradually increase educational expenditures. The central government should raise the proportion of education spending to total government spending by three percentage points in 2000. The local government's higher educational expenditure should be increased even more based on their economic conditions.[4] Figure 3.1 plots the number of colleges in China from 1978 to 2010, and Figure 3.2 shows the number of colleges in selected provinces during this period. The number of colleges in each province remained almost unchanged before HEE, while it went up rapidly right after HEE. In order to finance the construction or expansion of a large number of higher education institutions, in addition to the financial support from governments, it is encouraged that colleges borrow from banks.[5]

[4]See the news report from People's Daily at
`http://52.34.104.77/renminribao/1999/3/2/11/`.

[5]From the 2007 China Education Blue Book, higher education institutions had around 450 billion to 500

The central government strictly restricted higher education admission to meet economic development needs. The government allocates admission quotas to different higher education institutions. Before the HEE, college students usually got assigned jobs once they completed their college education. There was also no tuition and fees for college education before the early 1990s, while nearly all higher education institutions started to charge tuition and fees to cover their expenses in the mid-1990s. After HEE, the tuition and fees gradually increased in later years (Wan, 2006). HEE was unanticipated because it was first proposed in November 1998, then approved by the central government in January 1999, and finally implemented in July 1999. The initial plan for the 1999 expansion was to increase college enrollment by 0.23 million compared to 1998; then, this number was finally increased to 0.567 million in the middle of June 1999. Prior to 1999, the average increase in college enrollment rates was about 8%. In 1999, the enrollment rate jumped by 47.4% and kept growing every year.

This expansion has led to millions of students from both urban and rural areas getting into college, and it has also increased educational attainment at various levels because of the high probability of getting into college after the expansion. As we can see from Figure 3.3, new enrollment in high schools jumped dramatically after the higher education expansion. From 2010 Population Census data, the number of college students per ten thousand increased to 15,467 compared to 8,930 in the 2000 Population Census. I will mainly take advantage of the unexpected HEE as an exogenous shock to examine the impacts of maternal education on early childhood outcomes.

One concern regarding HEE-induced education attainment is that the Compulsory Schooling Law (CSL) in 1986, which requires all children to have nine years of education, could potentially confound the estimation of the impact of maternal education. For example, a mother who was born after 1980 should be affected by CSL as well as HEE. Therefore, the sizeable potential impact of maternal education might be attributed to the HEE and also the CSL. In this study, I address this concern by using the exogenous variation led by HEE, which happened in relatively recent years. Moreover, I restrict my sample to mothers who were fully exposed to the CSL in the robustness checks. The main results are quantitatively and qualitatively similar to my main findings.

billion RMB in loans by the end of 2006.

3.3 The Data and Variables

3.3.1 Overview of the Data

The dataset I use for this study is the baseline wave of China Family Panel Studies (CFPS) survey data. CFPS is a national representative longitudinal survey conducted by Peking University. The national baseline CFPS survey was conducted in 2010, and it surveys 25 provinces of China.[6] These 25 provinces consist of 95 percent of the total population of China, which can be viewed as a representative sample of China (Xie and Hu, 2014a). There are five modules in CFPS, including community, family roster, family, child, and adult. This paper mainly uses survey data from child and adult modules. Child survey questions are answered by the adult family member who is the child's primary caregiver. The survey children themselves will answer part of the questions if the child is more than 10 years old. Adult survey questions are answered by the adults who live in the chosen interviewed family.

The CFPS survey data is ideal for this research because it offers several advantages. First, it has detailed information on the timing and level of adults' education, which helps match external provincial data. Second, it tracks an adult's past residence locations at birth, age three, and age twelve before becoming an adult. Third, CFPS asks a wide range of questions related to the child's birth outcomes and early childhood cognitive skills, which provides the latest available child's outcome information for those mothers whose education was affected by the HEE. Fourth, this dataset has rich information about adult social-economic status and health-related outcomes, which allows me to examine the potential channels of the effects of maternal education on children's health outcomes and cognitive skills.

College expansion does not only increase access to college but also induces people to stay longer in school (Xing, 2014). For those mothers who did not finish high school and did not take the college entrance exam, I follow Ding (2021) to get college-going year information. I use the end year of their education to predict their high school graduation year so that I can match college-going year external variables to them. Although CFPS does not have the province information of the mother's college-going year, I proxy the location with their reported province at age 12. With this mother's college-going time and location information, I can match it with provincial level macroeconomics variables and control for such aggregate provincial level changes. Provincial-level economics variables data are from China Statistical Years Book. I also obtain the number of colleges, college

[6] The administrative areas that are not in the survey are Hong Kong, Macao, Taiwan, Xinjiang, Tibet, Inner Mongolia, Ningxia, and Hainan.

admission students, and high school graduates for each province and year from the China Yearly Statistical Book and the Educational Statistical Book of China.

3.3.2 The Outcome Variables

In this paper, I mainly study the impact of maternal education on two sets of outcomes: early childhood health outcomes and early childhood cognitive skills.

The first set of health outcomes, following the literature (Currie and Moretti, 2003; Chou et al., 2010; McCrary and Royer, 2011; Güneş, 2015), includes a dummy variable of low birth weight (LBW), a dummy variable of very low birth weight (VLBW), a dummy variable of macrosomia, and a dummy variable of premature birth. LBW equals one if the infant's birth weight is lower than 2,500 grams; otherwise, zero. VLBW is defined as one if the infant's birth weight is lower than 1,500 grams and as zero if it is higher. Macrosomia is defined as an infant's birth weight exceeding 4,000 grams. A child's gestational age is less than 36 weeks is defined as premature birth.[7] I also study whether the child had any sickness before age one and whether the child had any hospitalization before age 1. These two outcomes measure whether the child is healthy or not before age 1. As for the second set of early childhood outcomes, I focus on four measures that show early childhood cognitive skills development. CFPS asks the child's primary caregiver how many months after birth the child started to perform these skills. It includes the month the child first started walking independently, speaking a full sentence, counting from 1-10, and self-urinating.

3.3.3 The Independent Variables

The variable of interest is maternal education. I focus on two measures of maternal education. The first measure is a continuous variable that measures the mother's total years of schooling, and the second one is an indicator variable that equals one if the mother has more than nine years of education (some high school or above). I focus on the mother's education years above nine because HEE not only increased the number of college students but also increased the number of high school students since they had a higher probability of getting into college after 1998. I do not specifically use the education measure of college attainment because there are only 7% mothers in the final sample who have a college degree.

I control for the mother's ethnicity (an indicator variable equals one if the mother's ethnicity is Han), the child grandmother's education level (elementary school, middle

[7]Since CFPS asks for gestational age in months but not weeks, here I define the variable of gestational age as less than 36 weeks equals one if the infant was reported to be born less than nine months.

school, high school, college, and above), and the mother's hukou status (urban or rural) at age 12. I do not control for the father's education since it is highly correlated with the mother's education, and it is a potential channel that could affect children's outcomes (Chou et al., 2010). In addition, I add provincial-level economics variables in the mother's college-going year in all specifications because these could be related to the number of colleges in that province and also potentially affect the mother's choice of college location.

Furthermore, I include the mother's birth year fixed effects, the province at age 12 fixed effects, and the child's province-year of birth fixed effects in the main specifications. Including mother's province fixed effects and year fixed effects absorb unobserved heterogeneity at the aggregate level. Including the mother's childhood province fixed effects account for geographical heterogeneity, which includes cultural and social norms that might affect the child's behaviors. The mother's year of birth fixed effects controls for common shock at the time dimension, which is likely to improve both the educational attainment of young mothers and other factors that affect future children's outcomes. For example, younger cohort mothers are more likely to be affected by the one-child policy, which reduces sibling size and encourages parents to spend more resources on them. In addition, children's province-year of birth fixed effects control for the time-varying changes in children's birth year and province, which could affect their birth outcomes and early childhood cognitive outcomes. Meanwhile, there could be provincial-cohort-specific variables that might be correlated with the number of colleges in that province in a given year and also potentially affect children's outcomes. Therefore, I add two provincial-level trends to control for such unobservables. The first is the interaction of mother's year-of-birth dummies with the provincial college degree population ratio in the 1995 mini Census, and the second is the interaction of mother's year-of-birth dummies with the provincial level college admission rate in 1998.

3.3.4 Sample Descriptive Statistics

The main sample used in this paper is child survey data matched with their parents' information using CFPS 2010 baseline data. I exclude the child sample with missing values of their mother's education information. I also exclude mothers who were born before 1970 (aged more than 40 years old).[8] The youngest mother in CFPS 2010 was born in 1994. To eliminate the potential effects of the Compulsory Education Law in 1986 on mothers' educational attainment, in the robustness checks part, I further restrict the sample to mothers

[8]This is around 5% of the matched sample.

who were born after 1975.[9]

Table 3.1 presents the summary statistics of children's birth outcomes, early childhood cognitive skills, and the independent variables. Among the study sample, around 27% of the sample used in this analysis was exposed to HEE. The average number of colleges for the mother's college-going year before HEE was 40, and this number increased to 50 for those mothers exposed to HEE. For mothers who were exposed to HEE, 80% of their college-going years were between 1999 and 2005. Therefore, this number of college increases only reflects changes in the early years after HEE.

According to Panel A, approximately 5% of children have low birth weight, and only 0.3 % of children have very low birth weight. There are around 4% children who were born overweight and around 4% children whose gestational age is lower than 36 weeks. When comparing children's early childhood health outcomes between two groups of mothers, I do not see much difference in these outcomes between mothers who were exposed to HEE and those who were not. Panel B in Table 3.1 shows the summary statistics for the four measures of cognitive skills. It is clear that the average month of children's first walking independently, speaking a full sentence, counting from 1-10, and self-urinating was shortened for mothers exposed to HEE.

3.4 Empirical Strategy

I am interested in the impact of maternal education on a set of early childhood outcomes, which can be specified in the following equation:

$$y_{ijt} = \beta_0 + \beta_1 Medu_{ipk} + \boldsymbol{\beta}_2 \mathbf{X}_i + \boldsymbol{\beta}_3 \mathbf{Z}_{pc} + \boldsymbol{\zeta}_p + \boldsymbol{\zeta}_k + \boldsymbol{\zeta}_{jt} + u_{ijt} \tag{3.1}$$

Where y_{ijt} is the outcome of the child i living in province j who was born in year t, $Medu_{ipk}$ represents the education of child i's mother whose childhood province is p and was born in year k.[10] Throughout this paper, I mainly focus on two measures of a mother's education: a continuous measure of years of schooling and a discrete measure of whether the mother's education level is above middle school (i.e., she has ever attended high school or above). $\mathbf{X}_i$ represents a set of variables controlling for maternal characteristics for child i, including mother's ethnicity, grandmother's education level, and mother's hukou

[9]There are only two provinces, Hunan and Gansu, which had the Compulsory Education Law to be effective in 1991, and their first eligible birth cohort is 1976 (Ma, 2017).

[10]I do not use the mother's birth province because there could be a migration that happened during their childhood period. Meanwhile, I am unable to observe their hukou province in their college-going year. Therefore, I use the most recent observable province information before their college-going year to identify their location information.

status at age 12. $\mathbf{Z}_{pc}$ refers to mother's provincial economics variables at their college-going year c including GDP growth rate, employment growth rate, and population growth rate, so that I can control for the impact of improved economic conditions on children's health and other outcomes. $\boldsymbol{\zeta}_p$, $\boldsymbol{\zeta}_k$, and $\boldsymbol{\zeta}_{jt}$ denote the mother's childhood province, birth year, and child's birth province by cohort fixed effects, respectively. I also include cohort-specific trends by interacting the initial year provincial-level college degree population ratio and provincial college admission rate with the mother's year of birth to account for potential diverging trends resulting from different initial conditions of higher education and population.

Even though with these fixed effects and cohort trends, I can absorb time-invariant unobserved heterogeneity across different provinces and common shocks across different cohorts, the estimate of β_1 may still be biased due to omitted variables. There might be factors that affect both the mother's education and the potential outcome of the child.

I employ an instrumental variables (IV) approach to address the endogeneity concerns by utilizing the exogenous variation in mothers' education attainment induced by the HEE in China. At the province-year level, I search for instruments that are correlated with the mother's education but unlikely to affect the child's later outcomes. The instrument I use is the number of colleges in the mother's province p in her college-going year c.[11] The identifying assumption is that there is no direct impact on these early childhood outcomes from the mother's exposure to HEE. The only channel that could affect these outcomes is via the changes in maternal education.

Therefore I am able to estimate a two-stage least squares model with the first-stage regression equation being

$$Medu_{ipk} = \pi_0 + \pi_1 Numcollege_{pc} + \boldsymbol{\pi}_2 \mathbf{X}_i + \boldsymbol{\pi}_3 \mathbf{Z}_{pc} + \boldsymbol{\xi}_p + \boldsymbol{\xi}_k + \boldsymbol{\xi}_{jt} + \varepsilon_{ipk}. \tag{3.2}$$

There are several reasons why I use the number of colleges as an instrument for a mother's education. First, there is strong evidence linking college expansion and improved educational attainment in China at various levels (Xing, 2014; Liu and Wan, 2019) and it is unlikely that this swift policy reform was anticipated and endogenously determined by provincial socioeconomic conditions (Feng and Xia, 2022). Ding (2021) also confirms this by a validity test showing that college enrollment is not correlated with provincial-level GDP and employment growth. Second, unlike existing studies focusing on compulsory educational reforms that affected educational attainment at an early stage of an individual,

[11] I first calculate the starting year of each education level by using the end year and subtracting the duration. I assume the mother's college year is the same year when high school is completed for those mothers without a college degree. For those mothers who did not finish high school, I assigned age eighteen to them, which is the year they become adults.

college expansion was more recent and came much later in effect. Since those cohorts that were affected by compulsory education laws were also likely to be affected by later educational reforms, previous estimates may overestimate the impact of adult education on children's outcomes. By exploiting the variation in maternal educational attainment induced by the number of colleges before and after HEE, I am able to capture a more accurate relationship between education and children's outcomes.

3.5 Results

In this section, I present the empirical results of intergenerational transmission of education on children's outcomes. First, I show that, in both continuous and discrete measures of educational attainment, the number of colleges has strong predictive power on maternal education. Then I proceed with the instrumental variable estimation and find that, on average, there is no significant impact of intergenerational transmission of education on children's birth outcomes. However, this is masked by the heterogeneity of mothers' urban-rural origins, as the sub-sample analyses yield a strong effect for mothers from rural areas. Next, I test several mechanisms that lead to such results, and I find assortative marriage and rural-urban migration play an important role in understanding the intergenerational transmission of education.

3.5.1 First-Stage Result of Number of Colleges on Maternal Education

Table 3.2 summarizes the IV first-stage results following Equation (3.2). Panel A uses years of schooling as the dependent variable, while panel B uses a discrete variable that equals one if the mother's educational attainment is above nine years, which indicates the mother has at least completed middle school education and started high school. Column (1) controls all individual characteristics and fixed effects. Column (2) adds a cohort-specific trend in terms of college population ratio in the 1995 census. Column (3) additionally adds a cohort-specific trend related to the admission rate prior to the college expansion in 1998 and is the preferred specification. All three specifications reveal similar quantitative results on the impact of access to college. A one-unit increase in the number of colleges would raise 0.05 years of education and the probability of going to high school by one percentage point. Though the coefficients seem to be small, given that the average years of schooling for the sample prior to the expansion is less than seven years, and the average increase in the number of colleges after 1999 is more than 10, translating the impact to 0.5 years, equivalent to a 7% increase. The impact on the high school-going rate is even more significant (10 percentage points), approximately 70% compared to the pre-expansion average (14 percent). This set of results is consistent with the compulsory education reform literature where Güneş (2015) show that affected mothers' years of education increased by 0.04 years and primary school completion by 1.4-1.7 percentage points. Liu and Wan (2019) find the impact of higher education expansion on years of schooling is around 0.09 years. Therefore, the first stage confirms that there is a strong relationship between college expansion and maternal education, and the magnitude is not negligible.

3.5.2 The Impact of Maternal Education on Children's Outcomes

Birth Outcomes Table 3.3 reports the impact of maternal education on their child's early childhood health outcomes. Panel A shows OLS estimates, and Panel B provides the corresponding IV results. For each column, I examine how years of schooling and high school attendance affect different health measures. An inspection of the first two columns in panel A reveals that there is no significant correlation between a mother's education and low birth weight, even though the direction of the impact is expected. A one-year increase in schooling will decrease low birth incidence by only 0.2 percentage points and very low birth weight by 0.03 percentage points. Attending high school reduces low birth weight by 0.7 percentage points, and very low birth weight is almost zero and not statistically significant. However, given that the average of very low birth weight children only takes 0.3 percent of the whole sample, it is not a surprising result. I do find years of schooling is correlated with the incidence of reduction of macrosomia, while the high school attendance effect is less precise. Having more education does not significantly reduce the chance of a child being born early.[12] There is also a positive correlation between maternal education and reporting the child being sick and hospitalized in the first year after birth. It is likely due to selection that more educated mothers have better care for their children, and they are also more cautious about any illness.

Panel B presents the results from the instrumental variable regression. Inspection of the F statistics indicates there is a strong power of our first stage estimation as the F statistics exceed ten (Andrews et al., 2019). That the F statistics associated with high school attendance are far larger than those associated with years of education suggests the effect is strongest for people who are marginally affected by the higher education expansion (Ding, 2021). Similar to the literature, I do not find a substantial effect of increased maternal education on infant health at birth or in the first year of birth (Lindeboom et al., 2009). All IV estimates lack statistical power to make causal inferences.

[12]It is worth noting that CFPS does not report gestational age in weeks but in months. The conventional threshold for premature birth is 37 weeks, which is the first week of the 9th month. Here I use 9 months as the cut-off time.

Cognitive Skills Table 3.4 shows the impact of maternal education on early childhood cognitive skills. I focus on how long it takes the child to start walking independently, speaking a full sentence, counting numbers from 1-10, and self-urinating. Coefficients from OLS regressions imply a strong correlation between a mother's education and children's cognitive skills. Having one more year of schooling will reduce the time of starting walking by 0.137 months, speaking by 0.248 months, counting by 0.79 months, and self-urinating by 0.4 months. The impact of high school attendance is more pronounced. Children with a mother who at least went to high school will start speaking 1.6 months earlier than children from a less educated mother, almost 8 percent of the mean effect. The effect for counting numbers is approximately 16 percent and for self-urinating is 10 percent. The IV estimates are consistent with OLS results in terms of direction and magnitude. I still cannot claim there is an average effect of a mother's education on their children's cognitive skills because of the lack of statistical power. However, as I will confirm in the heterogeneity analyses next, we shall not be surprised by the null results found in the IV estimations since I expect that education is more salient for people on the margin affected by the college expansion.

3.5.3 Heterogeneous Impacts of Maternal Education on Children's Outcomes

Rural/Urban Origins Even though there has been rapid urbanization in China over the past few decades and economic transformation has improved education access and living conditions in rural areas, there still exists a large gap in terms of both education access and attainment between rural and urban areas, especially among females (Guo et al., 2022). Given the vast differences in educational opportunities across locations, we should not expect the effect of college expansion to be uniform across all individuals. To investigate the heterogeneous effect of education on children's outcomes, I first look into how mothers' rural-urban origins influence infants' health outcomes and the forming of cognitive skills in early childhood.

Table 3.5 shows the IV results for mothers from rural and urban areas separately. For those females whose childhood was in the rural area, one more year of schooling reduces low birth weight incidence by 2.1 percentage points, equivalent to a 36 percent reduction compared to the mean. Completing middle school and attending high school or above results in an even larger effect of 10.7 percent, almost twice as the mean effect. A similar impact is also found in very low birth weight. I do not find significant impacts for other birth outcome measures. Empirical studies have found that improved education due to college expansion has profound impacts on moving rural people to urban areas through migration (Ding, 2021), which potentially improves access to health care and other services that deliver better outcomes for infants.

Table 3.6 presents the results for children's cognitive skills, where I see significant improvements in these outcomes for mothers with more education. An increase in years of education slightly reduces the time for children beginning to possess those skills and attending high school largely decreases the time. Children with more educated mothers from rural are seen to have 1.6 months, 4 months, and 7 months earlier than their counterparts with less educated mothers to start walking independently, speaking a full sentence, and counting numbers from 1-10 . This set of results shows the important transmission of a mother's education to a child's skills in the early stage. More educated mothers may possibly pass on their increased human capital to their children by teaching them to perform these activities earlier than others. Although I do not find significant effects from urban mothers, that is possibly due to less sample size and smaller averages.

High/Low Impact Provinces There may also be differential effects for provinces with varying levels of initial college access. Places with a higher admission rate before HEE may have better amenities that are positively correlated with health-related outcomes for those mothers, and they pass them on to the next generation. Even though the previous study shows that college expansion is independent of provincial economic conditions (Ding, 2021), preexisting high education levels could affect the outcomes of affected individuals in a different way.

To test this hypothesis, I split provinces into two categories. The high-impact provinces are those with high admission rates in 1998, the year prior to the expansion. The low-impact provinces are those with relatively low admission rates. The effects of a mother's education on her children's birth outcomes are more notable in high-impact provinces. Though I do not find there is an improvement in children's birth outcomes through maternal education in the full sample, here I identify a non-trivial effect that one more year of schooling lifts a child from low birth weight by 4 percentage points and attending high school improves it by 19 percentage points. I also find that mothers from high-impact provinces are also more likely to send children to hospitals. However, there is no substantial effect in low-impact provinces.

As can be seen in Table 3.8, the only significant effect on cognitive skills is the time children start counting numbers for high-impact provinces. Children with mothers who have one more year of schooling start counting 3 months ahead of others, and there is almost an 18-month advancement for children whose mother has at least some high school. Other skills are noisier compared to counting, and I again find no significant effect for low-impact provinces.

3.5.4 Robustness Checks

In this section, I perform different specification checks to test if excluding cohort-specific trends and using the restricted sample alter the main results.

First, I do not control the trends that account for differential effects originating from the initial college population ratio and admission rate. If it significantly changes our results, it implies the potential selection of college expansion at the provincial level, which would invalidate my instrument variable. The results are presented in Table 3.9. Consistent with what I have found, I do not see the causal effect of intergenerational transmission of a mother's education on early childhood health outcomes for the full sample. Only very low birth weight from years of education and the time to start speaking from high school attendance show marginally significant effects, and all coefficients estimated are very similar to the magnitude in Tables 3.3 and 3.4. Panel B shows the rural sample test where I confirm my main result is valid since there are no statistical differences from the estimates in panel A Table 3.5 and 3.6.

Next, I restrict the sample to those mothers who were born after 1975. The restricted sample helps to avoid the confounding impact of the Compulsory Schooling Law (CSL) in 1986. CSL was gradually rolled out across provinces starting in 1986. The earliest eligible birth cohort is mothers who were born in 1970, and the latest eligible birth cohort is mothers who were born in 1976 (Ma, 2017). All children born after 1975 are fully exposed to CSL. Table 3.10 reports the IV results using the restricted sample. I find similar results as in Table 3.3 and Table 3.4. Maternal education decreases the likelihood of an infant's very low birth weight and improves the cognitive skills of mothers with rural hukou at age 12.

3.6 Mechanisms

My main results show that, on average, there is no meaningful evidence of intergenerational transmission of education on children's outcomes through college expansion, but more educated mothers from rural areas are less likely to have low birth weight children and prepare their children's speaking, walking, and counting skills earlier than less educated mothers. In this section, I seek to understand what factors could explain those results by exploring several mechanisms. I first examine if the college expansion promotes assortative marriage in China as a common cause found in the literature. Next, I investigate if increased access to education improves labor market outcomes so that mothers have more resources to take care of their children. Then I test if there are any place-based factors behind these effects. Last, I look into whether access to college changes the self-reported health and health behaviors of affected mothers.

Assortative Marriage Columns (1) and (2) in Table 3.11 provide evidence of assortative marriage in terms of educational attainment. The full sample analysis shows one more year of schooling increases spousal education years by 0.9 years, and the effect is magnified by almost 5 times for mothers from rural areas who at least attended high school. A mother's education is also causally related to the discrete measure of the spouse's education. On average, a one-year increase in a mother's education leads to a 13 percentage point increase in the father's attendance of high school, and mothers who have at least some high school are 62 percentage points more likely to marry a similar or higher-educated father. These results are driven by the rural sample, which verifies my findings in the heterogeneous effect and is consistent with existing studies (Nie and Xing, 2019).

Labor Market Outcomes Columns (3) and (4) present the results of maternal education on the mother's working status and log income. The full sample estimates show a significant impact of education on employment, while such an effect is muted for mothers from rural areas in panel B.

In contrast to the mixed evidence of job status, returns to education have a quantitatively large but noisy effect. One year of schooling increases a mother's income by 15.5 percent and 17.8 percent in full and the rural sample, respectively. Attending high school will almost double the income compared to those mothers that are less educated. These estimates are within the range among studies that looked into the labor market consequences of the high education expansion (Shi and Xing, 2010).

The existence of assortative marriage but no statistically significant impact on labor market outcomes, especially for rural mothers, is not surprising. Literature suggests that mothers tend to spend more time with children and sacrifice labor market outcomes, which in turn could transfer their human capital to the next generation. It is also worth mentioning that mothers in this sample may be selective as I only use women who have children, which would underestimate the effect of education on labor market outcomes.

Location Recent literature suggests place-based factors could affect health outcomes (Deryugina and Molitor, 2021; Finkelstein et al., 2021). As I have confirmed in my previous results, mothers who came from rural areas were more affected by the college expansion and transmitted human capital to the outcomes of the next generation. Most Chinese universities are located in large cities, and attending college requires migrating to those cities (Xing and Zhang, 2017). Is it because they are likely to stay in cities after they receive college education there, so they have better health services that improve children's outcomes? I test if the mother's current hukou registration status is affected by her education.

Column (5) in Table 3.11 shows it is indeed this case. One more year of education on average increases current urban hukou status by 10.8 percentage points, and high school attendance increases having urban hukou by 35 percentage points. These effects are mostly driven by rural mothers, as seen in panel B, where the estimates are larger than those obtained in the full sample.

Adult's Health and Risky Behaviors Existing studies have shown that improved access to education, either through compulsory education law or college, likely changes adults' health (Huang, 2015), and risky behaviors such as smoking and drinking (Cowan and Tefft, 2020). Meanwhile, parents with worse health status may put fewer resources on their children, and therefore, this might be negatively related to health or early cognitive skills development for the next generation.

However, I do not find such channels of effect in the Chinese context. Column (6) presents that there is little impact of the mother's own education on her self-reported health as good, although the positive magnitudes are consistent with the literature (Huang, 2015). Columns (7) and (8) show that there is no causal effect of education on health behaviors for mothers. Considering that the average percentage of smokers is only 0.68 percent for the full sample and 0.61 percent for the rural sample, it is not surprising to see the null effect. In addition, the null impact of education on self-reported health and health behaviors is also consistent with the existing findings in Turkey (Cesur et al., 2014), and Romanian (Malamud et al., 2021).[13]

3.7 Conclusion

Although there is a large body of literature studying the intergenerational transmission of education on various outcomes, there are only a few studies focusing on maternal education on early-childhood outcomes. In particular, current literature lacks evidence of mothers' post-compulsory education attainment on their children's outcomes in earlier stages. Knowing whether maternal education has any impact on different outcomes is extremely important. From the fetal origins hypothesis, helping mothers would be one

[13] I also examine whether mother's education has any impact on the probability of a child being born in a hospital and breastfeeding length. There is no evidence that a mother's education leads to any changes to these two outcomes, which could be helpful to early childhood health outcomes.

way to help their children throughout their life course (Almond and Currie, 2011). Making child-bearing age women more educated is a cost-effective way to improve child health.

This paper analyzes the impact of maternal education on early childhood health outcomes and cognitive skills. Using the higher education expansion in China as the exogenous shock to maternal education and applying the IV estimation. This paper finds that, for mothers from rural areas, a one-year increase in the mother's education leads to a 2 percentage point decrease in the probability of having a low birth weight child, 0.31 months earlier to start to walk independently, 0.675 months earlier to speak a full sentence, and 1.22 months earlier to count from 1-10. I test several channels through which maternal education could affect child outcomes. The results show that education leads to assortative marriage and rural-urban migration, which could contribute to the improvement of children's health outcomes and cognitive skills development.

Similar to the findings that examine the impact of college education attainment (Currie and Moretti, 2003; Carneiro et al., 2013), and the increase in the lower margin of educational attainment (Chou et al., 2010; Güneş, 2015; Huang et al., 2018), results in this paper indicate that the post-compulsory educational attainment in China has a large impact on infant birth weight and children's early cognitive skills development for mothers from rural origins. The mechanism analysis results are also consistent with the existing finding about the impact of higher education expansion in China, which vastly improves the social economics status with the increase in education (Li et al., 2014a; Ding, 2021).

As pointed out in Black et al. (2017), 250 million children younger than 5 years old who live in low- and middle-income countries (LMICs) have not reached their development potential. Insufficient attention placed on nurturing care during a child's rapid brain development and learning under the age of three is the major concern in many LMICs. A variety of early childhood development programs have been adopted in these countries, while the findings in this paper demonstrate that improving mothers' post-compulsory education will be a helpful way to improve children's early childhood development and thus further help children's health development and human capital accumulation. One thing that needs to be noted is that the sample mothers I used in the main analysis are mainly from the early cohort who benefited from the higher education expansion, and I show that mothers with high school attendance have shown considerable improvement in the outcome variables of interest. It would be reasonable to believe that college attainment should have an even larger impact on these outcomes, and it deserves further study in the future.

3.8 Tables

Table 3.1: Summary Statistics

	Full sample			Mother's college year before 1999			Mother's college year after 1999		
	N	Mean	Std. Dev.	N	Mean	Std. Dev.	N	Mean	Std. Dev.
A. Health outcomes									
LBW	4,638	0.057	0.232	3,371	0.056	0.231	1,267	0.058	0.235
VLBW	4,638	0.003	0.055	3,371	0.004	0.060	1,267	0.002	0.040
Macrosomia	4,638	0.039	0.194	3,371	0.039	0.194	1,267	0.039	0.195
Premature	5,329	0.037	0.189	3,950	0.032	0.175	1,379	0.052	0.223
Any Sick	4,574	0.692	0.462	3,449	0.670	0.470	1,125	0.758	0.428
Any Hospitalization	4,564	0.610	0.488	3,442	0.585	0.493	1,122	0.687	0.464
B. Cognitive skills outcomes									
Walk	4,753	14.328	4.615	3,748	14.509	4.822	1,005	13.653	3.525
Speak	4,490	20.487	8.032	3,651	20.642	8.251	839	19.810	6.926
Count	4,112	34.479	14.858	3,419	35.284	15.212	693	30.508	12.477
Self-Urinate	4,262	32.986	13.019	3,506	33.633	13.365	756	29.985	10.726
C. Instrumental variable									
Num.College	5,329	42.834	17.326	3,950	39.637	14.443	1,379	51.992	21.205
D. Explanatory variables									
Mother's ethnicity as Han	5,329	0.871	0.335	3,950	0.872	0.335	1,379	0.871	0.335
Grandma's education level	5,329	1.194	0.506	3,950	1.143	0.441	1,379	1.341	0.636
Mother's Hukou at age 12	5,329	0.098	0.297	3,950	0.090	0.286	1,379	0.120	0.326
Some high school or above	5,329	0.179	0.384	3,950	6.594	4.320	1,379	8.403	4.487
Mother's education years	5,329	7.062	4.435	3,950	0.138	0.345	1,379	0.297	0.457
Population growth rate	5,329	0.012	0.008	3,950	0.014	0.007	1,379	0.007	0.009
Employment growth rate	5,329	0.027	0.065	3,950	0.029	0.056	1,379	0.020	0.085
GDP growth rate	5,329	0.161	0.079	3,950	0.177	0.081	1,379	0.116	0.052

Notes: This table shows the summary statistics for the outcome variables, instrumental variable and main explanatory variables. Standard deviations are in parentheses. The sample presented in this table is the CFPS 2010 survey data for the main analysis.

Table 3.2: First Stage-Number of College on Maternal Education

	(1)	(2)	(3)
Panel A: Education Years			
Number of College	0.0514***	0.0491***	0.0462***
	(0.0121)	(0.0121)	(0.0121)
	(4)	(5)	(6)
Panel B: High school attendance			
Number of College	0.00987***	0.00982***	0.0100***
	(0.00122)	(0.00122)	(0.00121)
Mother's cohort×college population ratio in 1995	N	Y	Y
Mother's cohort×admission rate in 1998	N	N	Y
Observations	5,329	5,329	5,329

Notes: This table shows the first stage results of the number of colleges on mother's education. The data is from CFPS 2010, and cross-sectional weights are used in the regression. All regressions control for the mother's characteristics, including ethnicity, grandmother's education level, mother's hukou status at age 12, provincial-level employment rate, GDP growth rate, and population growth rate. In addition, the mother's birth year fixed effects, the mother's childhood province fixed effects, and the child's birth province by year fixed effects are included. Robust standard errors are clustered at the mother's province by cohort level and are shown in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 3.3: The Impact of Maternal Education on Children's Birth Outcomes

	LBW (1)	VLBW (2)	Macrosomia (3)	Premature (4)	Any sick (5)	Any hospitalization (6)
Panel A: OLS						
1. Education Years	-0.00223	-0.000355	-0.00191**	0.00017	0.00901***	0.0115***
	(0.00137)	(0.000242)	(0.000851)	(0.00103)	(0.00274)	(0.00269)
2. High school attendance	-0.00781	0.000002	-0.0148	0.00524	0.0289	0.0414
	(0.0111)	(0.00173)	(0.00922)	(0.01121)	(0.0285)	(0.0280)
Panel B: IV						
1. Education Years	-0.0131	-0.00199	-0.00372	0.0331	-0.00660	0.00325
	(0.00929)	(0.00121)	(0.00629)	(0.0244)	(0.0272)	(0.0259)
F-statistics	18.79	18.79	18.79	14.46	20.38	20.72
2. High school attendance	-0.0652	-0.00990	-0.0185	0.1525	-0.0332	0.0165
	(0.0471)	(0.00626)	(0.0309)	(0.117)	(0.137)	(0.132)
F-statistics	73.01	73.01	73.01	69.18	76.61	76.23
Observations	4,633	4,633	4,633	5,329	4,566	4,556
Sample Mean	0.0568	0.00302	0.0393	0.037	0.692	0.610
Sample SD	0.231	0.0549	0.194	0.189	0.462	0.488

Notes: This table presents the main results of OLS and IV estimations. It shows the effects of maternal education on children's early childhood health outcomes. The data is from CFPS 2010, and cross-sectional weights are used in the regression. All regressions control for the mother's characteristics, including ethnicity, grandmother's education level, mother's hukou status at age 12, provincial-level employment rate, GDP growth rate, population growth rate, mother's birth cohort dummies interacted with provincial-level college population ratio in 1995 and mother's birth cohort dummies interacted with provincial-level admission rate in 1998. In addition, the mother's birth year fixed effects, the mother's childhood province fixed effects, and the child's birth province by year fixed effects are included. Robust standard errors are clustered at the mother's province by cohort level and are shown in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 3.4: The Impact of Maternal Education on Children's Early Childhood Cognitive Skills

	Walk (1)	Speak (2)	Count (3)	Self-urinate (4)
Panel A : OLS				
1. Education Years	-0.137***	-0.248***	-0.790***	-0.422***
	(0.0234)	(0.0557)	(0.0812)	(0.0690)
2. High school attendance	-0.404**	-1.666***	-5.711***	-3.367***
	(0.183)	(0.432)	(0.705)	(0.696)
Panel B: IV				
1. Education Years	-0.00251	-0.481	-0.801	-0.679
	(0.175)	(0.340)	(0.612)	(0.576)
F-statistics	16.88	17.15	20.16	18.72
2. High school attendance	-0.0131	-2.841	-4.918	-4.027
	(0.914)	(1.960)	(3.859)	(3.403)
F-statistics	62.77	50.92	61.78	52.73
Observations	4,748	4,483	4,101	4,253
Sample Mean	14.33	20.49	34.51	33.01
Sample SD	4.614	8.035	14.86	13

Notes: This table presents the main results of OLS and IV estimations which study the effects of maternal education on children's early childhood cognitive skills. The data is from CFPS 2010 and cross-sectional weights are used in the regression. Independent variable and fixed effects follow Table 3.3. Robust standard errors are clustered at the mother's province by cohort level, and are shown in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 3.5: The Impact of Maternal Education on Children's Birth Outcomes by Mother's Hukou Origin

	LBW (1)	VLBW (2)	Macrosomia (3)	Premature (4)	Any sick (5)	Any hospitalization (6)
Panel A: Rural						
1. Education Years	-0.0210*	-0.00334**	0.00469	0.0420	-0.0178	0.00489
	(0.0117)	(0.0017)	(0.0072)	(0.0327)	(0.0300)	(0.0278)
F-statistics	12.46	12.46	12.46	9.329	14.77	15.03
2. High school attendance	-0.107*	-0.0171**	-0.0185	0.197	-0.0945	0.0262
	(0.0574)	(0.0082)	(0.0309)	(0.1604)	(0.1580)	(0.1490)
F-statistics	47.07	47.07	47.07	44.03	50.93	50.56
Observations	4,116	4,116	4,116	4,803	4,083	4,074
Sample Mean	0.0586	0.00316	0.0384	0.036	0.691	0.606
Sample SD	0.235	0.0561	0.192	0.186	0.462	0.489
Panel B: Urban						
1. Education Years	-0.0177	-0.00186	-0.0285	-0.0103	-0.0237	-0.0539
	(0.0164)	(0.0031)	(0.0281)	(0.0182)	(0.0558)	(0.0635)
F-statistics	11.7	11.7	11.7	12.01	8.973	8.973
2. High school attendance	-0.157	-0.0189	-0.289	-0.1002	-0.199	-0.452
	(0.1390)	(0.0307)	(0.3060)	(0.1777)	(0.2910)	(0.6290)
F-statistics	44.73	5.954	5.954	6.791	6.887	6.887
Observations	432	432	432	439	396	396
Sample Mean	0.0463	0.00231	0.0417	0.045	0.702	0.646
Sample SD	0.21	0.0481	0.2	0.209	0.458	0.479

Notes: This table presents the IV results of maternal education on early childhood health outcomes by mother's urban or rural hukou origin. Mothers with a rural hukou at age 12 are defined as being of rural hukou origin, while mothers with an urban hukou at age 12 are considered to be of urban hukou origin. The data is from CFPS 2010, and cross-sectional weights are used in the regression. Independent variable and fixed effects follow Table 3.3.Robust standard errors are clustered at the mother's province by cohort level and are shown in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3.6: The Impact of Maternal Education on Children's Early Childhood Cognitive Skills by Hukou Origin

	Walk (1)	Speak (2)	Count (3)	Self-urinate (4)
Panel A: Rural				
1. Education Years	-0.307**	-0.675*	-1.216*	-0.749
	(0.1500)	(0.4050)	(0.7170)	(0.5590)
F-statistics	11.75	13.17	14.63	13.37
2. High school attendance	-1.676**	-4.093*	-7.266*	-4.445
	(0.8150)	(2.3000)	(4.2340)	(3.2870)
F-statistics	39.32	35.72	50.8	37.9
Observations	4272	4039	3670	3826
Sample Mean	14.45	20.6	35.43	33.26
Sample SD	4.714	8.146	14.99	13.1
Panel B: Urban				
1. Education Years	0.216	0.976	0.207	0.277
	(0.6100)	(1.0680)	(1.8920)	(1.8930)
F-statistics	6.44	5.28	6.061	6.059
2. High school attendance	2.447	14.13	3.443	3.111
	(7.0120)	(19.6500)	(32.0400)	(21.2200)
F-statistics	8.158	3.492	1.147	8.636
Observations	387	361	348	349
Sample Mean	13.14	19.56	27.16	30.99
Sample SD	3.27	6.549	11.01	11.9

Notes: This table presents the IV results of maternal education on early childhood cognitive skills by the mother's urban or rural hukou origin. A mother with a rural hukou at age 12 is defined as being of rural hukou origin, while a mother with an urban hukou at age 12 is considered to be of urban hukou origin. The data is from CFPS 2010, and cross-sectional weights are used in the regression. Independent variables and fixed effects follow Table 3.3. Robust standard errors are clustered at the mother's province by cohort level and are shown in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3.7: The Impact of Maternal Education on Children's Birth Outcomes by High/Low Admission Rates in 1998

	LBW	VLBW	Macrosomia	Premature	Any sick	Any hospitalization
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: High						
1. Education Years	-0.0409*	-0.00437	-0.0164	-0.0521	0.0451	0.0619
	(0.0233)	(0.00318)	(0.0154)	(0.0484)	(0.0330)	(0.0388)
F-statistics	5.048	5.048	5.048	3.413	7.740	7.951
2. High school attendance	-0.191**	-0.0204	-0.0765	-0.205	0.231	0.323*
	(0.0954)	(0.0152)	(0.0636)	(0.175)	(0.166)	(0.186)
F-statistics	23.79	23.79	23.79	22.98	29.94	29.58
Observations	2,274	2,274	2,274	2,782	2,109	2,103
Sample Mean	0.0616	0.00352	0.0365	0.0331	0.655	0.582
Sample SD	0.240	0.0592	0.188	0.1789	0.476	0.493
Panel B: Low						
1. Education Years	-0.000625	-0.00162	0.00266	0.0161	-0.0461	-0.0526
	(0.0112)	(0.00147)	(0.00714)	(0.0268)	(0.0351)	(0.0326)
F-statistics	14.68	14.68	14.68	15.79	14.47	14.21
2. High school attendance	-0.00299	-0.00773	0.0127	0.0815	-0.238	-0.270*
	(0.0538)	(0.00683)	(0.0337)	(0.140)	(0.187)	(0.156)
F-statistics	64.07	64.07	64.07	64.31	53.78	53.80
Observations	2,265	2,265	2,265	2,450	2,369	2,365
Sample Mean	0.0521	0.00265	0.0411	0.040	0.726	0.636
Sample SD	0.222	0.0514	0.198	0.197	0.446	0.481

Notes: This table presents the IV results of maternal education on early childhood health outcomes by mother's college province. A mother is defined as from a high college admission rate province if this province's college admission rate in 1998 was higher than the median college admission rate in that year across all provinces. If that province's college admission rate in 1998 was lower than the median, then I define that province as a lower admission rate province. The data is from CFPS 2010, and cross-sectional weights are used in the regression. Independent variables and fixed effects follow Table 3.3. Robust standard errors are clustered at the mother's province by cohort level and are shown in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3.8: Maternal Education on Children's Early Childhood Cognitive Skills By High/Low admission rate in 1998

	Walk (1)	Speak (2)	Count (3)	Self-urinate (4)
Panel A: High				
1. Education Years	-0.378	-1.445	-3.444*	-0.761
	(0.323)	(1.268)	(1.924)	(1.069)
F-statistics	2.284	2.108	2.516	2.847
2. High school attendance	-1.567	-7.023	-18.39**	-4.075
	(1.293)	(4.911)	(8.248)	(6.108)
F-statistics	15.04	9.702	9.503	10.92
Observations	2,179	2,063	1,902	1,945
Sample Mean	14.13	20.40	32.18	32.39
Sample SD	4.778	8.373	14.25	12.86
Panel B: Low				
1. Education Years	0.0707	-0.176	0.916	-0.649
	(0.207)	(0.303)	(0.661)	(0.737)
F-statistics	19.49	26.50	35.93	23.68
2. High school attendance	0.420	-1.180	6.371	-4.390
	(1.220)	(2.054)	(4.207)	(4.879)
F-statistics	53.23	57.34	67.84	42.38
Observations	2,482	2,338	2,123	2,227
Sample Mean	14.53	20.65	36.66	33.66
Sample SD	4.488	7.762	15.05	13.04

Notes: This table presents the IV results of maternal education on early childhood cognitive skills by mother's college province. A mother is defined as from a high college admission rate province if this province's college admission rate in 1998 was higher than the median college admission rate in that year across all provinces. If that province's college admission rate in 1998 was lower than the median, then I define that province as a lower admission rate province. The data is from CFPS 2010, and cross-sectional weights are used in the regression. Independent variables and fixed effects follow Table 3.3. Robust standard errors are clustered at the mother's province by cohort level and are shown in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3.9: Robustness Checks
Maternal Education on Early Childhood Outcomes without Controlling Trends

	(1) LBW	(2) VLBW	(3) Macrosomia	(4) Premature	(5) Any Sick	(6) Any Hospitalization	(7) Walk	(8) Speak	(9) Count	(10) Self-Urinate
Panel A: Full Sample										
1. Education Years	-0.0132	-0.00186*	-0.00307	-0.0170	-0.00146	0.00278	-0.0409	-0.482	-0.819	-0.850
	(0.00857)	(0.00110)	(0.00559)	(0.0230)	(0.0240)	(0.0229)	(0.158)	(0.293)	(0.575)	(0.533)
F-statistics	22.91	22.91	22.91	18.13	23.61	23.87	21.05	20.74	22.83	21.26
2. High school attendance	-0.0731	-0.0103	-0.0169	-0.0887	-0.00807	0.0155	-0.238	-3.087*	-5.365	-5.393
	(0.0477)	(0.00624)	(0.0306)	(0.118)	(0.133)	(0.128)	(0.927)	(1.843)	(3.864)	(3.402)
F-statistics	69.57	69.57	69.57	65.93	70.43	70.19	61.71	51.25	62.95	50.24
Observations	4,633	4,633	4,633	5,329	4,566	4,556	4,748	4,483	4,101	4,253
Sample Mean	0.0568	0.00302	0.0393	0.037	0.692	0.610	14.33	20.49	34.51	33.01
Sample SD	0.231	0.0549	0.194	0.189	0.462	0.488	4.614	8.035	14.86	13
Panel B: Rural Sample										
1. Education Years	-0.0193*	-0.00283*	0.00429	-0.0227	-0.00984	0.00727	-0.302**	-0.693**	-1.268*	-0.921*
	(0.0108)	(0.00148)	(0.00642)	(0.0276)	(0.0257)	(0.0235)	(0.137)	(0.336)	(0.680)	(0.510)
F-statistics	16.46	16.46	16.46	13.05	18.48	18.71	15.32	15.96	16.00	15.45
2. High school attendance	-0.112*	-0.0164*	0.0248	-0.125	-0.0581	0.0433	-1.852**	-4.585**	-8.128*	-5.938*
	(0.0601)	(0.00853)	(0.0364)	(0.145)	(0.151)	(0.140)	(0.905)	(2.111)	(4.282)	(3.286)
F-statistics	48.38	48.38	48.38	45.34	51.32	51.09	40.45	36.68	50.29	36.15
Observations	4,116	4,116	4,116	4,803	4,083	4,074	4,272	4,039	3,670	3,826
Sample Mean	0.0586	0.00316	0.0384	0.036	0.691	0.606	14.45	20.60	35.43	33.26
Sample SD	0.235	0.0561	0.192	0.186	0.462	0.489	4.714	8.146	14.99	13.10

Notes: This table presents the IV results of maternal education on early childhood health outcomes and cognitive skills without including two economic variable trends. The data is from CFPS 2010, and cross-sectional weights are used in the regression. Independent variables and fixed effects follow Table 3.3. Robust standard errors are clustered at the mother's province by cohort level and are shown in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3.10: Robustness Checks
Maternal Education on Early Childhood Outcomes with Restricted Sample

	(1) LBW	(2) VLBW	(3) Macrosomia	(4) Premature	(5) Any Sick	(6) Any Hospitalization	(7) Walk	(8) Speak	(9) Count	(10) Self-Urinate
Panel A: Full Sample										
1. Education Years	-0.0105	-0.00216	-0.00921	-0.0271	-0.0228	0.00315	-0.126	-0.641	-0.645	-0.377
	(0.0116)	(0.00161)	(0.00837)	(0.0323)	(0.0312)	(0.0311)	(0.141)	(0.444)	(0.705)	(0.590)
F-statistics	14.03	14.03	14.03	10.94	15.08	15.55	15.61	12.31	16.16	12.85
2. High school attendance	-0.0476	-0.00982	-0.0419	-0.112	-0.102	0.0143	-0.631	-3.452	-3.627	-2.053
	(0.0525)	(0.00755)	(0.0355)	(0.126)	(0.142)	(0.141)	(0.710)	(2.167)	(3.945)	(3.179)
F-statistics	56.51	56.51	56.51	55.17	60.84	60.70	53.29	38.80	51.97	38.41
Observations	2,956	2,956	2,956	3,287	2,794	2,790	2,760	2,515	2,227	2,358
Sample Mean	0.0541	0.00304	0.0389	0.041	0.714	0.635	14.10	20.13	32.79	31.96
Sample SD	0.226	0.0551	0.193	0.198	0.452	0.481	4.352	7.750	13.68	12.06
Panel B: Rual Sample										
1. Education Years	-0.0194	-0.00352*	0.00172	-0.0377	-0.0370	0.00549	-0.244*	-0.738	-1.224	-0.563
	(0.0140)	(0.00195)	(0.00831)	(0.0387)	(0.0331)	(0.0319)	(0.142)	(0.488)	(0.777)	(0.587)
F-statistics	10.61	10.61	10.61	8.081	12.14	12.55	13.35	12.05	13.99	12.64
2. High school attendance	-0.0957	-0.0173*	0.00848	-0.167	-0.182	0.0275	-1.342*	-4.304*	-6.959*	-3.253
	(0.0659)	(0.00948)	(0.0409)	(0.153)	(0.163)	(0.159)	(0.751)	(2.568)	(4.129)	(3.295)
F-statistics	37.00	37.00	37.00	36.08	41.02	40.91	35.81	29.91	48.06	31.95
Observations	2,639	2,639	2,639	2,965	2,506	2,503	2,485	2,273	1,992	2,126
Sample Mean	0.0553	0.00303	0.0371	0.038	0.709	0.628	14.21	20.17	33.52	32.12
Sample SD	0.229	0.0550	0.189	0.193	0.454	0.483	4.430	7.815	13.88	12.14

Notes: This table presents the IV results of maternal education on early childhood health outcomes and cognitive skills by restricting the sample to mothers who were born after 1975. The data is from CFPS 2010, and cross-sectional weights are used in the regression. Independent variables and fixed effects follow Table 3.3. Robust standard errors are clustered at the mother's province by cohort level and are shown in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3.11: The Mechanisms of Maternal Education on Children's Outcomes

	(1) Father's education years	(2) Father's high school attendance	(3) Mother Employed	(4) Mother's income if employed	(5) Current urban Hukou	(6) Self-reported good health	(7) Ever smoked	(8) Alcohol often
Panel A : Full Sample								
1. Education Years	0.911***	0.133***	0.0660*	0.155	0.108***	0.0157	-0.000588	0.0117
	(0.276)	(0.0442)	(0.0379)	(0.174)	(0.0212)	(0.0111)	(0.00456)	(0.0103)
F-statistics	9.401	9.401	16.32	10.53	23.10	14.46	16.03	14.62
2. High school attendance	4.267***	0.622***	0.321**	0.833	0.348***	0.0722	-0.00277	0.0540
	(1.185)	(0.140)	(0.155)	(0.951)	(0.111)	(0.0499)	(0.0214)	(0.0448)
F-statistics	39.37	39.37	68.93	43.56	69.12	69.17	72.85	69.79
Observations	4,131	4,131	5,145	2,791	5,322	5,329	5,247	5,318
Sample Mean	8.231	0.231	0.549	7.334	0.195	0.900	0.00686	0.0216
Sample SD	3.913	0.421	0.498	3.476	0.396	0.300	0.0826	0.145
Panel B: Rural Sample								
1. Education Years	1.028***	0.138**	0.0568	0.178	0.0940***	0.0115	0.000119	0.0148
	(0.374)	(0.0549)	(0.0424)	(0.211)	(0.0290)	(0.0136)	(0.00545)	(0.0136)
F-statistics	5.542	5.542	11.32	7.497	9.196	9.329	10.05	9.382
2. High school attendance	4.901***	0.658***	0.286	0.937	0.439***	0.0540	0.000564	0.0694
	(1.567)	(0.169)	(0.183)	(1.132)	(0.136)	(0.0644)	(0.0258)	(0.0582)
F-statistics	23.40	23.40	46.09	33.30	43.97	44.03	46.64	44.37
Observations	3,728	3,728	4,626	2,488	4,798	4,803	4,733	4,792
Sample Mean	7.850	0.186	0.546	7.076	0.111	0.898	0.00613	0.0219
Sample SD	3.747	0.389	0.498	3.512	0.314	0.302	0.0780	0.146

Notes: This table presents the IV results of maternal education on various social-economical outcomes and adult health-related outcomes. The data is from CFPS 2010, and cross-sectional weights are used in the regression. Independent variables and fixed effects follow Table 3.3. Robust standard errors are clustered at the mother's province by cohort level and are shown in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

3.9 Figures

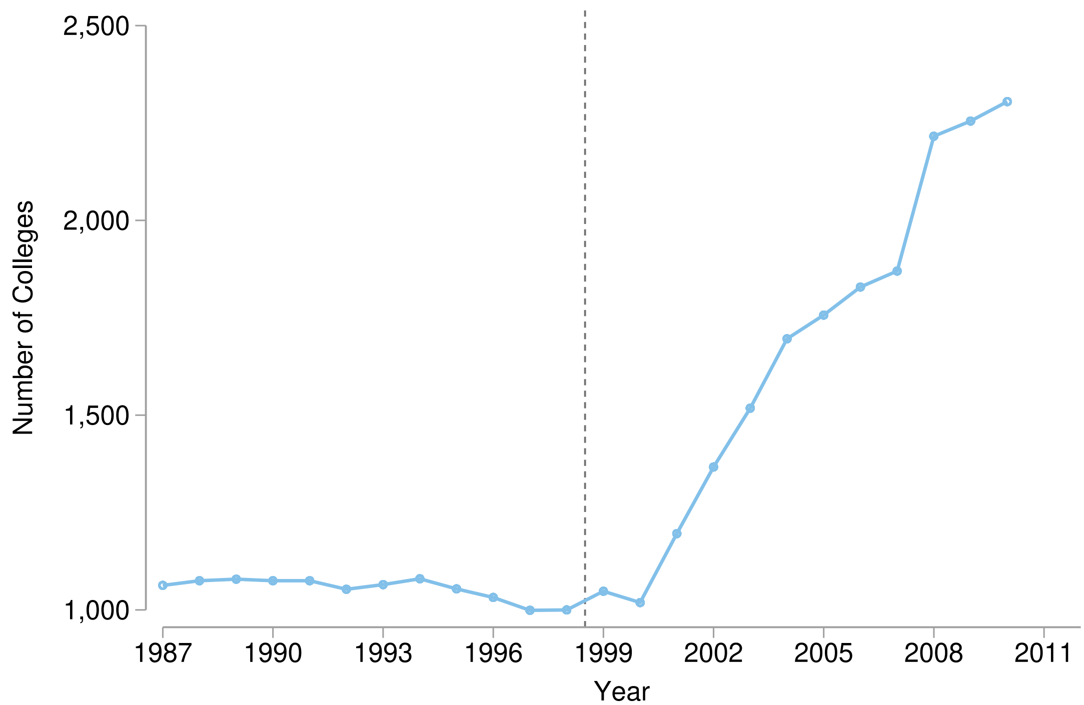


Figure 3.1: National Number of Colleges

Notes: This figure plots the national number of colleges from 1987 to 2010. The data is from China Yearly Statistical Book.

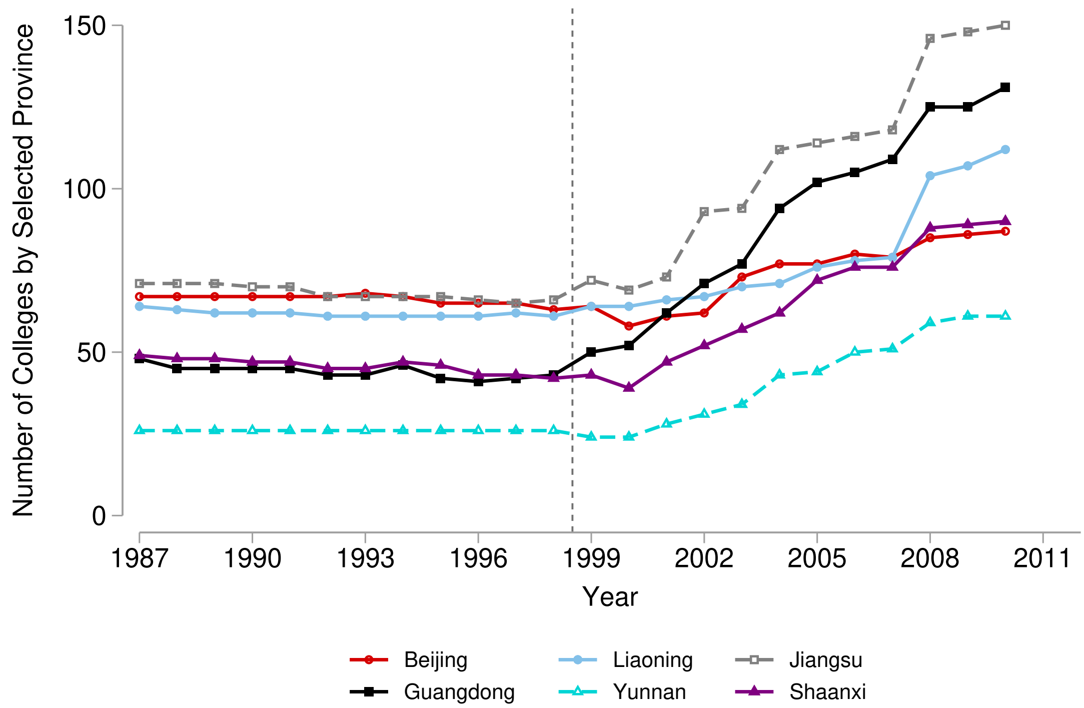


Figure 3.2: Number of Colleges by Selected Provinces

Notes: This figure plots the number of colleges from 1987 to 2010 in the selected province. The data is from the China Yearly Statistical Book and Educational Statistical Book of China. There are six regions in mainland China. Each province presented above is a representative province in different regions.

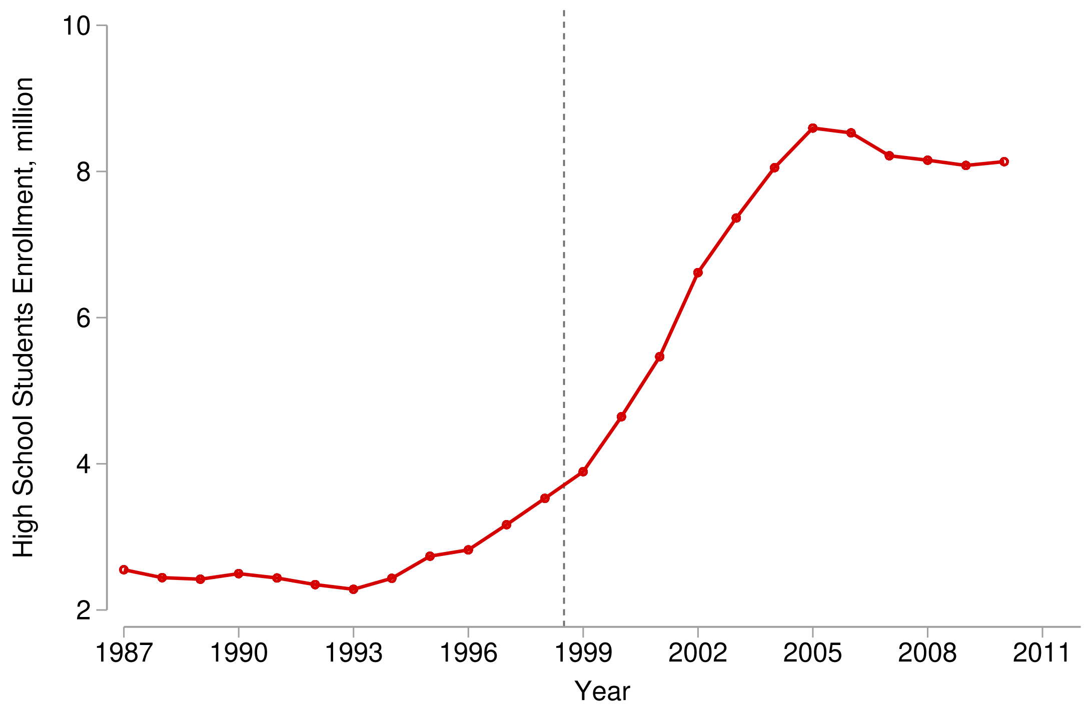


Figure 3.3: National High School Students New Enrollment

Notes: This figure plots the total number of high school admissions from 1987 to 2010. The data is from the Educational Statistical Book of China.

Appendices

Appendix A: Supplemental Institutional Background for Chapters 1 and 2

The three basic medical insurance schemes, the Urban Employment Basic Insurance Scheme (UEMI), the New Rural Cooperative Basic Medical Insurance (NCMS), and the Urban Residents Basic Medical Insurance (URMI) in China, are provided by the government. These three basic medical insurances cover more than 95% population in China.

People are free to purchase private commercial health insurance plans. Public health facilities usually accept all three basic health insurance schemes. In-network health services typically mean health services provided by most public health facilities within the prefecture-level city's jurisdiction. Out-of-network medical services are available to enrollees but may result in higher out-of-pocket costs and less reimbursement. Inpatient and outpatient services and prescription drugs are subject to different deductibles, co-payments, and reimbursement plans depending on the jurisdictions, plans, and types of health facilities. Co-payments for outpatient physician visits are typically \$3-\$5(USD), and co-payments for inpatient services are typically higher and depend on the services used by the patient. For medical expenses incurred at some providers, patients only pay out-of-pocket costs after doctor visits or inpatient treatment. However, at other providers that do not yet have such payment systems in place, patients have to pay all medical costs first and then get the reimbursement later (typically within 1-2 months).

Patients can easily access specialist care without going through a referral system. In lower-level health facilities, such as community health centers or clinics, appointments are not required to see a doctor. For higher-tier hospitals (usually the higher quality hospitals), general practitioner visits are usually available with walk-in visits or the day after an appointment. However, in recent years, specialist visits usually require an appointment.

Table A1 and Table A2 show the differences in CII programs across different cities. For example, Resident A lives in Beijing and is enrolled in the NCMS in 2014. The total medical costs were about \$75,559, including \$5,200 for outpatient visits and \$70,359 for inpatient stays. NCMS paid \$26,304, including \$304 outpatient costs and \$26,000 inpatient costs. The out-of-pocket cost after NCMS coverage is \$49,255. This high out-of-pocket cost exceeds the local GDP per capita in 2013 (USD\$6,166), which is the deductible for the 2014 Beijing CII plan. Then this rural resident is eligible for a second reimbursement from CII. With Beijing's 2014 CII reimbursement plan, the total deductible after CII is \$24,895(USD). After CII reimbursement, the total out-of-pocket costs are reduced by about 50%.

If we consider the other two residents, B living in Wuhan and C living in Yinchuan in 2013, whose out-of-pocket costs after basic health insurance are also $49,225(USD), the total CII reimbursement in Wuhan and Yinchuan in 2013 would be $16,609 and $21,637(USD), respectively, based on their local CII policies. Even with the same out-of-pocket costs after the basic health insurance plan, the final out-of-pocket costs after CII are reduced the most in Wuhan's plan and the least in Beijing's CII plan. This means that the benefits from CII may not be evenly distributed across the population in different cities.

Table A1: CII Plans in Different Cities

City	Financing level	Deductibles	Cap Line	Reimbursement Method
Beijing	7.65 USD (50 RMB)	7,647 USD (40,321 RMB)	No	One year later
Wuhan	4.13 USD (27 RMB) for urban residents 3.67 USD (24 RMB) for rural residents	1,237 USD (8,000 RMB)	45,880 USD (300,000 RMB)	Patients only pay the OOP costs after CII
Yinchuan	3.82 USD (25 RMB)	917 USD (6,000 RMB)	No	Patients only pay OOP costs after CII from 2016

Notes: (1) Beijing is one of the four municipalities in China, and it is located in the east region of China. Wuhan city in Hubei province is located in the middle region of China. Yinchuan city in Ningxia province is located in the northwestern part of China; (2) The CII plan listed above in Beijing is in 2014, while Wuhan and Yinchuan's CII plans listed above were in 2013.; (3) All of these CII plans shown above are in their first policy year; (4) Only the out-of-pocket costs incurred within the scope of basic medical insurance coverage can be reimbursed from CII. The out-of-pocket costs used in the example above are considered as incurred within the basic medical insurance scope.

Table A2: CII Reimbursement Plans in Different Cities

City	Reimbursement Plan		Reimbursement Ratio (%)	Benefits Example	
	OOP Costs (USD)	OOP Costs (RMB)		Total OOP costs after basic medical insurance =49,225 USD	Total OOP costs after CII
Beijing	7,647-13,813	40,321-90,321	50%	(13,813-7,647) *50%=3,083 USD	49,225-24,330=24,895 USD
	>13,813	>90,321	60%	(49,225-13,813) *60%=21,247 USD	OOP cost has decreased by 50%
				Total amount of CII reimbursement = 24,330 USD	
Wuhan	1,237-4,588	8,000-30,000	50%	(4,588-1,237) *50%=1,676 USD	49,225-32,616=16,609 USD
	4,588-7,647	30,000-50,000	60%	(7,647-4,588) *60%=1,835 USD	OOP cost has decreased by 66%
	>7,647	50,000.00	70%	(49,225-7,647) *70%=29,105 USD	
				Total amount of CII reimbursement=32,616 USD	
Yinchuan	918-3,059	6,000-20,000	50%	(3,059-918) *50%=1,071 USD	49,225-27,588=21,637 USD
	3,059-7,647	20,000-50,000	52%	(7,647-3,059) *52%=2,386 USD	OOP cost has decreased by 56%
	7,647-15,293	50,000-100,000	54%	(15,293-7,647) *54%=4,129 USD	
	15,293-30,587	100,000-200,000	57%	(3,0587-15293) *57%=8,718 USD	
	30,587-45,878	200,000-300,000	60%	(45,878-30,587) *60%=9,175 USD	
	45,878-61,173	300,000-400,000	63%	(49,225-45,878) *63%=2,109 USD	
	61,173-76,467	400,000-500,000	66%	Total amount of CII reimbursement=27,588 USD	
	>76,467	>500,000	70%		

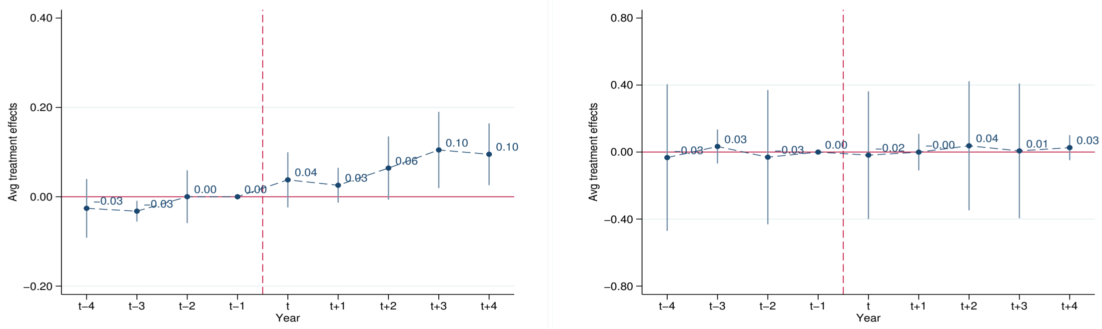
Notes: (1) Beijing is one of the four municipalities in China, and it is located in the east region of China. Wuhan city in Hubei province is located in the middle region of China. Yinchuan city in Ningxia province is located in the northwestern part of China; (2) The CII plan listed above in Beijing is in 2014, while Wuhan and Yinchuan's CII plans listed above were in 2013.; (3) All of these CII plans shown above are in their first policy year; (4) Only the out-of-pocket costs incurred within the scope of basic medical insurance coverage can be reimbursed from CII. The out-of-pocket costs used in the example above are considered as incurred within the basic medical insurance scope.

Appendix B: Supplemental Tables and Figures for Chapter 1

Table A3: Combined Average Treatment Effects of CII on Health

	NCMS		URMI		UEMI	
	Self-reported poor/fair health	Std. Mental health score	Self-reported poor/fair health	Std. Mental health score	Self-reported poor/fair health	Std. Mental health score
	(1)	(2)	(3)	(4)	(5)	(6)
CII	-0.000887	-0.0169	-0.0520	-0.0824	0.0161	0.0188
	(0.0129)	(0.0321)	(0.0328)	(0.120)	(0.0217)	(0.0633)
Observations	43,871	45,170	2,692	2,799	7,776	8,118
Pre mean	0.776	0.0117	0.789	0.0672	0.436	0.938
Pre SD	0.417	1.017	0.408	1.015	0.745	0.112

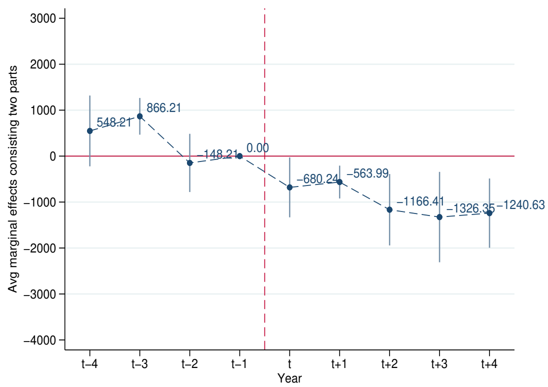
Notes: This table shows the combined average treatment effects of CII on different groups of the insured population. All the regressions are estimated using the OLS model. The data is cross-sectional four waves CHARLS data (2011, 2013, 2015, and 2018), and individual cross-sectional weights are used in the regression. All the regressions include city fixed effect, year fixed effect, and controls. Control variables include individual controls and city level controls listed in Table 1.2. Robust standard errors are clustered at the city level and shown in parentheses. Pre mean and standard deviation are calculated based on the first wave survey data (2011). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.



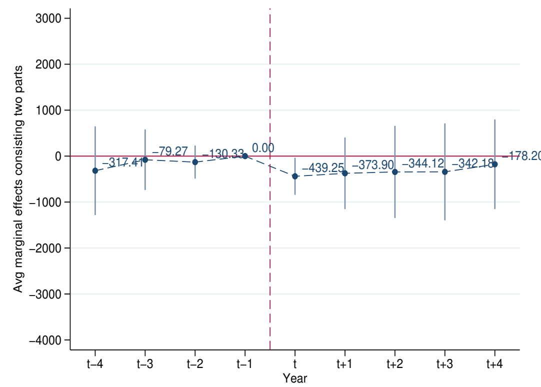
(a) Self-reported health as poor/fair (b) Standardized mental health score

Figure A1: The Effects of CII on Health

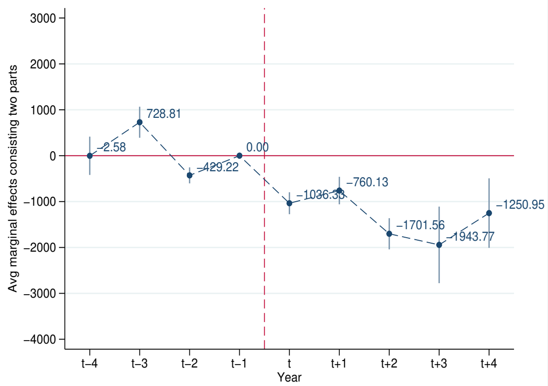
Notes: This graph shows the average dynamic effects of CII on self-reported physical health status and mental health status for NCMS insured population. The data is cross-sectional four waves CHARLS data (2011, 2013, 2015, and 2018), and individual cross-sectional weights are used in the regression. All the regressions include city fixed effects and year fixed effects and controls. Control variables include individual controls and city level controls listed in Table 1.2. Robust standard errors are clustered at the city level and shown in parentheses. All graphs are plotted at 90% confidence interval.



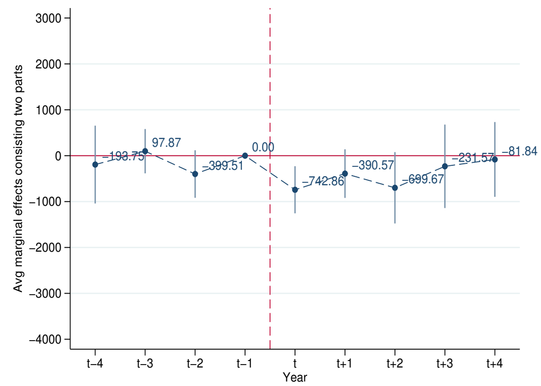
(a) Elderly



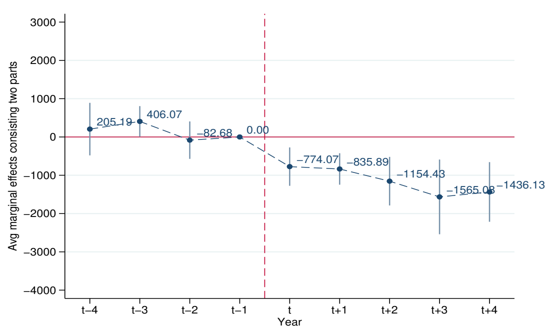
(b) Middle aged



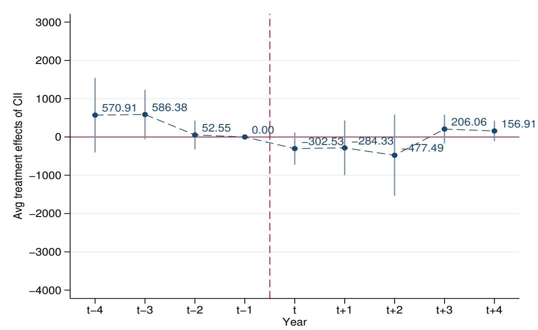
(c) Poor income



(d) Non-Poor Income



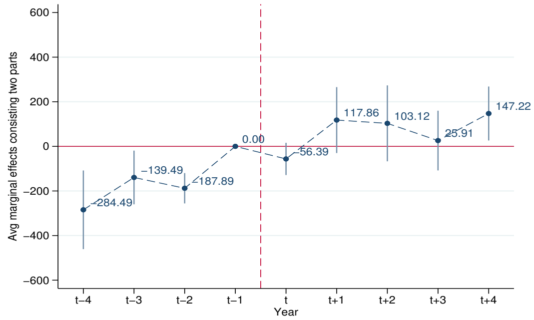
(e) Has Chronic diseases



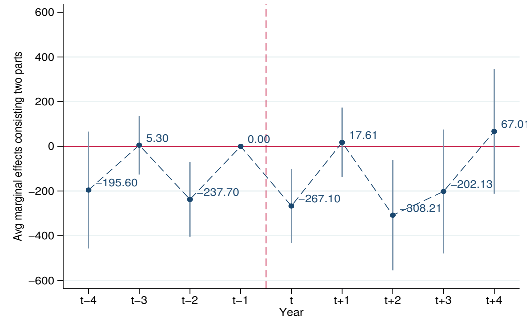
(f) No chronic diseases

Figure A2: Average Marginal Effects of CII on Out-of-Pocket Inpatient Costs

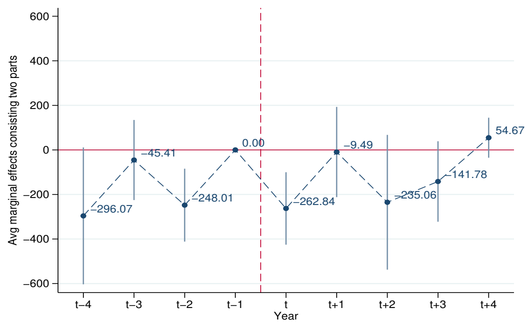
Notes: This graph shows the average dynamic effects of CII on out-of-pocket inpatient costs for different subgroups of the NCMS insured population. The data, control variables, fixed effects, regression specifications, and cluster level are the same as Figure 1.5 Panel (a) and (b). All graphs are plotted at 90% confidence interval.



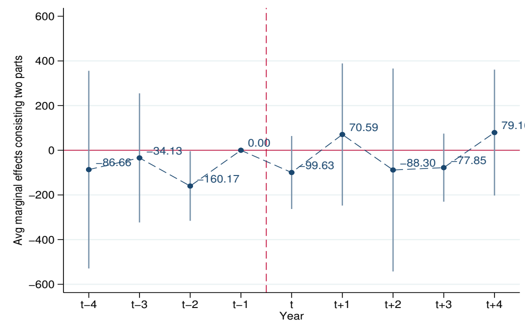
(a) Elderly



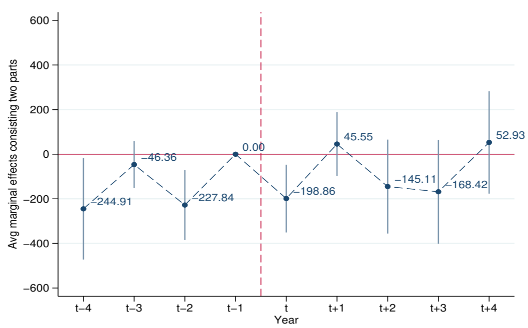
(b) Middle aged



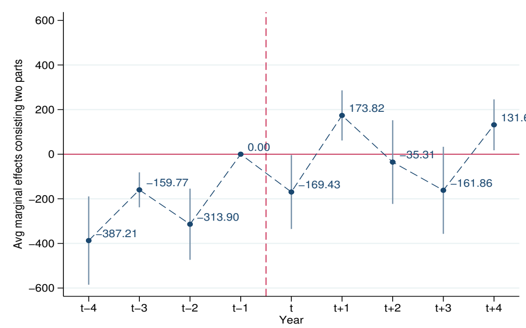
(c) Poor income



(d) Non-Poor Income



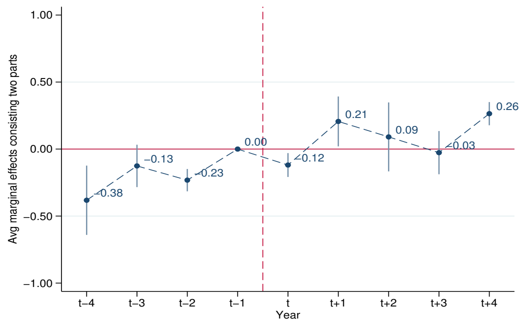
(e) Has Chronic diseases



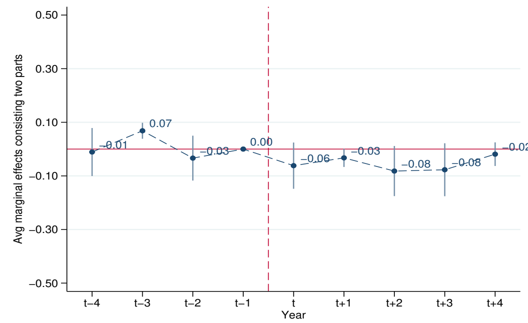
(f) No chronic diseases

Figure A3: Average Marginal Effects of CII on Out-of-Pocket Outpatient Costs

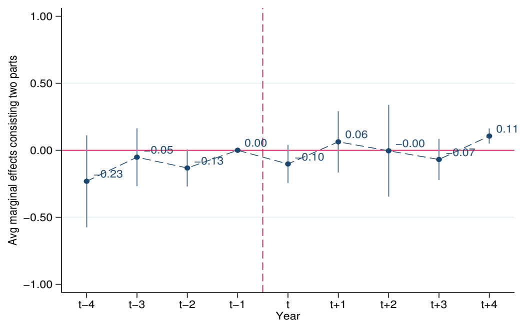
Notes: This graph shows the average dynamic effects of CII on out-of-pocket outpatient costs for different subgroups of the NCMS insured population. The data, control variables, fixed effects, regression specifications, and cluster level are the same as Figure 1.5 Panel (a) and (b). All graphs are plotted at 90% confidence interval.



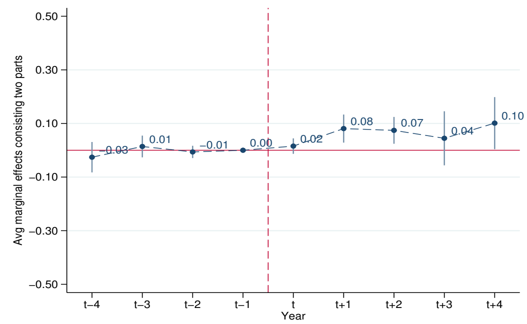
(a) Elderly



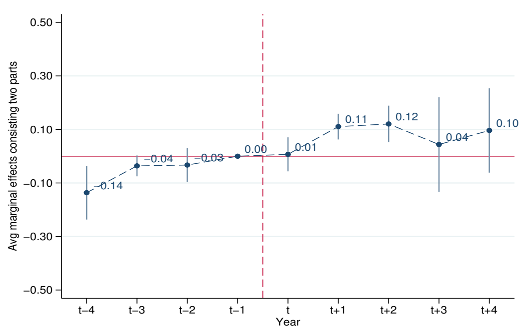
(b) Middle aged



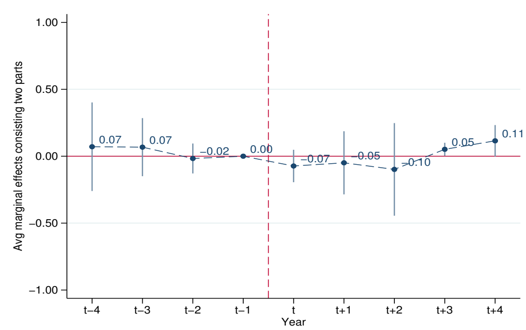
(c) Poor income



(d) Non-Poor Income



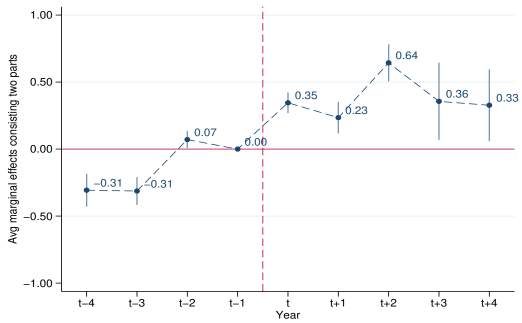
(e) Has Chronic diseases



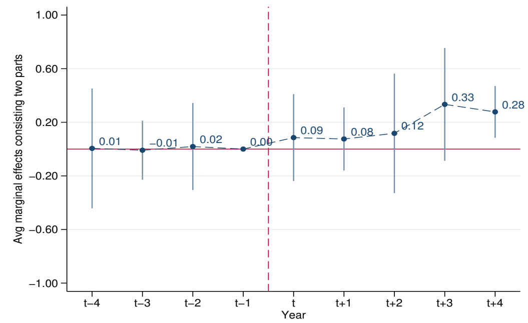
(f) No chronic diseases

Figure A4: Average Marginal Effects of CII on Number of Inpatient Visits

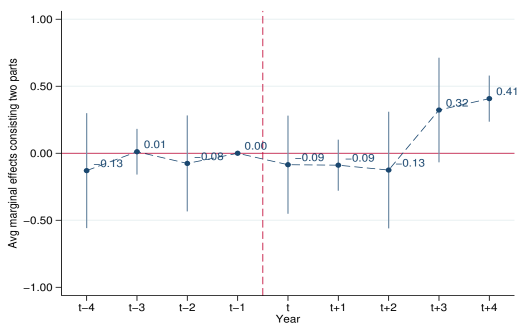
Notes: This graph shows the average dynamic effects of CII on the number of inpatient visits for different subgroups of NCMS of the insured population. The data, control variables, fixed effects, regression specifications, and cluster level are the same as Figure 1.5 Panel (c) and (d), except that the results for the non-poor income and non-chronic diseases groups are estimated using an OLS model because the negative binomial models do not converge. All graphs are plotted at 90% confidence interval.



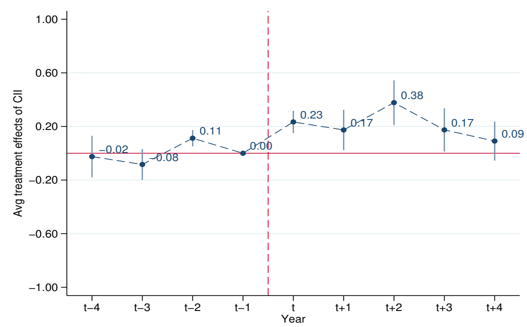
(a) Elderly



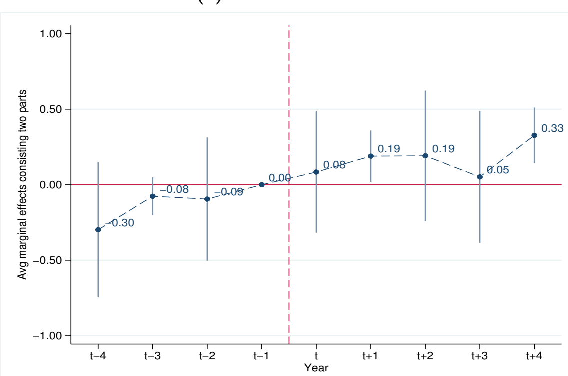
(b) Middle aged



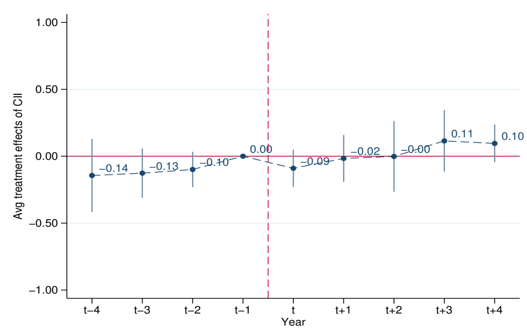
(c) Poor income



(d) Non-Poor Income



(e) Has Chronic diseases



(f) No chronic diseases

Figure A5: Average Marginal Effects of CII on Number of Outpatient Visits

Notes: This graph shows the average dynamic effects of CII on the number of outpatient visits for different subgroups of the NCMS insured population. The data, control variables, fixed effects, regression specifications, and cluster level are the same as Figure 1.5 Panel (c) and (d), except that the results for the non-poor income and non-chronic diseases groups are estimated using an OLS model because the negative binomial models do not converge. All graphs are plotted at 90% confidence interval.

Appendix C: Mental Health Variable for Chapter 2

The mental health measure in this paper is a standardized mental health score. There is a set of CES-D measures of self-reported questions for the current level of depressive symptomatology in CFPS questionnaires. For two consecutive waves of surveys, the CES-D scale questions are different. To be more comparable across the years, I standardized all the mental health scores. Table A4 below lists all the survey questions CFPS uses across five waves of data.

Table A4: Mental Health Survey Questions in CFPS

Survey Questions	Answers
Panel A. 2010, 2014 waves	
Here are some feelings or activities you may have experienced before.	
Please tell us how often you experienced them in the past month.	
1. Feel depressed and cannot cheer up	1 Never; 2 Sometimes;
2. Feel nervous	3 Half of the time; 4 Ofthen;
3. Feel agitated or upset and cannot remain calm	5 Almost daily
4. Feel hopeless about the future	
5. Feel that everything is difficult	
6. Think life is meaningless	
Panel B. 2012, 2016, 2018 waves	
Here are some descriptions of people's mental statuses.	
Please select according to your statuses in the past week.	
1. I am annoyed by some trifles.	1 Never (less than one day) ; 2 Sometimes (1-2 days);
2. I don't want to eat and have a poor appetite.	3 Often (3-4 days) ; 4 Most of the time (5-7 days)
3. I feel depressed even though I receive help from my family and friends.	
4. I feel that I am better than someone else.	
5. I find it hard to focus on what I am doing.	
6. I feel depressed.	
7. I find it difficult to do anything.	
8. I am hopeful about the future.	
9 .I feel that I have been a loser all the time.	
10. I feel scared.	
11. I have a poor sleep.	
12. I am happy.	
13. I talk less than usual.	
14. I feel lonely.	
15. I find people are unfriendly to me.	
16. I have a happy life.	
17. I have cried or want to cry.	
18. I feel sad.	
19. I find others dislike me.	
20. I feel that I am unable to keep on with my life.	

Notes: In the 2010 and 2014 surveys, CFPS asks respondents all six questions. The higher the score, the lower the mental health. The 2012 survey asks all 20 questions presented above. The 2016 survey randomly chooses either question 6,7,11,12,14,16,18,20 or question 1-20 from 2012 survey questions. In 2018 survey, all the respondents will be asked question 6,7,11,12,14,16,18,20 about mental health part.

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