The Supply-Side Effects of Long-Term Care Insurance in China*

Yu Chen Feng Huang Shilong Sun

Abstract

Although public long-term care insurance (LTCI) programs are prevalent globally, their effects on market supply of and access to long-term care (LTC) remain understudied. We analyze China's pilot public LTCI program, which reimbursed LTC services at regulated prices for elderly individuals, using administrative establishment registration data and a large-scale labor market survey. Exploiting variation from the program's staggered rollout, we find that the number of quarterly LTC provider entrants increased by 245% and the LTC employment share by 120%. We estimate a model of elderly individuals' choice between informal and formal care. Our estimates indicate that supply expansion and improved affordability driven by LTCI increased the aggregate share of formal care use among eligible beneficiaries. LTCI benefits for informal care did not discourage formal care providers' market entry.

Keywords: Long-term care insurance, Firm entry, China

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*Chen: School of Public Administration, Zhejiang University of Finance and Economics, 18 Xueyuan Road, Xiasha District, Hangzhou 310018, China (email: yu.chen@zufe.edu.cn); Huang: School of Economics, Shanghai University of Finance and Economics, 777 Guoding Road, Yangpu District, Shanghai 200433, China (email: huang.feng@mail.shufe.edu.cn); Sun: Compass Lexecon, 555 12th Street NW, Washington, DC 20004 (email: ssun@compasslexecon.com). We thank Robert Clark, Ricard Gil, Jia Xiang, Junjian Yi, and seminar participants at International Industrial Organization Conference, Meeting of the Society of Economics of the Household, International Symposium on Health Economics and Policy (HUST), Queen's University and Shanghai University of Finance and Economics for their helpful comments. Huang gratefully acknowledges the financial support from the National Natural Science Foundation of China (71973092). The views expressed in this paper are those of the authors and do not purport to represent the views of Compass Lexecon.

1. Introduction

The global share of the population aged 65 and over has risen from 5.02% in 1960 to 9.80% in 2022, with a projected increase to 16% by 2050 (United Nations, 2022). Over the same period, global fertility has decreased by almost 50%. This demographic shift has important implications for the demand for long-term care (LTC) services among elderly individuals. While the majority of the elderly population currently relies on informal care provided by family members (Gruber et al., 2023), there is a foreseeable transition towards formal market-provided care in the near future. To address this, numerous countries have implemented public long-term care insurance (LTCI) programs, aiming to enhance the affordability and provision of formal LTC.[1]

Although public insurance has improved the affordability of care services,[2] concerns about the lack of supply-side responses have been raised in a few healthcare contexts. When service supply is unresponsive to insurance provision, expanded demand under broader insurance coverage may not lead to greater accessibility of LTC services (Garthwaite, 2012; Buchmueller et al., 2016), and public insurance programs may not ultimately improve individual health status (Kondo and Shigeoka, 2013). Despite its global prevalence and longstanding theoretical foundation (e.g., Arrow, 1963; Feldstein, 1971), public LTCI has not been comprehensively evaluated in terms of its effects on care provision: how does LTCI affect the supply of and access to formal care services, and do informal care subsidies have unintended effects of discouraging formal care providers' market entry?

This paper addresses this literature gap by examining China's public LTCI pilot program from 2016 to 2019. The pilot provided public insurance for individuals aged 60 and above to reimburse LTC services at regulated prices. In 2016, the Chinese central government initiated this program to assess the feasibility and sustainability of a potential nationwide LTCI scheme, as China had an estimated 20.61 million elderly individuals (8.1% of the old-age population) in need of assistance for activities of daily living in 2020 (Gong et al., 2022). The pilot was implemented in 35 cities across 14 provinces in a staggered fashion by 2019. We exploit the temporal and geographical variations in LTCI availability arising from the staggered implementation of the program to uncover the causal impacts of LTCI on LTC supply and individual formal care utilization.

[1] LTCI is commonly used to refer to the private long-term care insurance system in many countries. However, since China's long-term care insurance is almost entirely government-run, we use LTCI throughout this paper to refer specifically to the public long-term care insurance system, without explicitly mentioning its public nature.

[2] See Gruber and McGarry (2023), Bakx et al. (2023), Fu et al. (2023) for evidence in the Netherlands, the US, and Japan, respectively, and Gruber et al. (2023) for a summary of evidence from ten developed countries.

Our analysis starts with an analytical framework inspired by Arrow (1963) to illustrate how public insurance can stimulate the supply of care services. The framework yields three key predictions. First, LTCI reduces out-of-pocket costs through cost sharing and hence boosts demand for formal care. The profits of incumbent firms increase, consequently prompting the entry of new care providers into the market. Second, the entry of new care providers enhances the overall availability of LTC, thereby increasing individual formal care service utilization. Together, the expanded formal care supply and the improved affordability through cost sharing contribute to greater formal care utilization.

Our empirical analysis leverages data compiled from four sources. First, we manually extract the timing of LTCI policy enactment at the city level from government websites and policy reports. Second, to assess the supply-side effects of the policy, we identify new LTC providers using administrative data on establishment registration. Third, we gauge LTC labor market outcomes using the China Migrants Dynamic Survey (CMDS), a comprehensive annual nationwide survey that collects demographic and employment information of approximately 200,000 migrant households. As documented in He and Luo (2020), in China, migrant workers constitute 90% of the domestic service workforce and are therefore the primary source of LTC workers. Finally, we measure care utilization patterns using the Chinese Longitudinal Healthy Longevity Survey (CLHLS), which contains data on the demographic characteristics, socioeconomic status, family background, health conditions, and healthcare and long-term care utilization of individuals aged 65 and above.

We use a staggered difference-in-differences (DID) framework to investigate the program's impact on the entry of new care providers. We estimate an increase of 0.967 in quarterly LTC provider entries after introducing LTCI. However, unobserved changes to the LTC industry may confound our ability to determine the causality of this estimate. To address this concern, we adopt four strategies. First, we include a time trend dependent on city characteristics before the pilot in 2010. Second, we estimate the average treatment effect and conduct event studies using the state-of-the-art methodology proposed by Callaway and Sant'Anna (2021) and find that supply-side outcomes remained stable before the program's implementation. Third, we apply the methodology developed by Rambachan and Roth (2023) to assess the sensitivity of our findings to potential violations of the parallel trends assumption. Lastly, we show that the order of LTCI pilot implementation is unrelated to city-level characteristics.

To substantiate the finding that provider entry reflects greater service capacity, we next examine whether the LTC labor market expanded after LTCI was implemented. We find that the LTC employment share increased by 120% following the pilot. The income and working hours of LTC workers also increased, but the statistical significance is marginal.

As a placebo test, we look for evidence of labor market changes in other industries and find none, emphasizing the specificity of the impact of LTCI availability on labor dynamics within the LTC sector. The results are robust to parallel trend tests and alternative model specifications.

The pilot's effects varied by city characteristics. Cities with higher GDP per capita, average wages, and a larger elderly population experienced greater supply expansions. Wider eligibility increased market entry. Specifically, subsidies for informal care did not deter formal care entry, contrasting with findings in healthcare settings (e.g., Freedman et al., 2015; Geddes and Schnell, 2023). Structural analysis highlights two mechanisms: (1) the elderly exhibit limited responsiveness to informal care benefits, and (2) formal care supply expansion does not induce a shift from informal to formal care.

We conclude our analysis by evaluating whether the expansion of formal care service capacity induced by LTCI translates into greater access to formal care services. To achieve this, we link longitudinal CLHLS data on the demographics, health status, and care choices of elderly individuals with the number of formal care providers available to them. Using this dataset, we examine how elderly individuals' care choices respond to eligibility for LTCI benefits (demand-side improvements in affordability) and the availability of LTC providers (supply-side expansion driven by LTCI).

Building on our theoretical framework, which formalizes Arrow (1963)'s conjecture, we estimate a discrete choice model in which individuals choose between informal care, formal care, or no care. Age, gender, LTCI eligibility, coresidence with children, and local provider availability enter the utility of each option. To disentangle demand- and supply-side effects, we adopt a control-function approach following Petrin and Train (2010) and its extension to endogenous entry by Aguirregabiria et al. (2025). Identification exploits shift-share type variation that combines LTCI implementation with pre-pilot supply shifters, generating exogenous variation in LTC supply across cities and over time. We estimate marginal effects of LTCI benefits and supply expansion on care choices and find that both channels divert elderly individuals away from informal care and no care.

This paper is related to the substantial literature documenting that health insurance expansion has led to significant supply responses (e.g., Finkelstein, 2007; Triyana, 2016; Freedman et al., 2015).[3][4] Despite the evidence on supply responses to health insurance

[3]Finkelstein (2007) reports that the introduction of Medicare, with its generous cost-based reimbursements for hospitals, led to a significant expansion in the US hospital sector. Similarly, Triyana (2016) finds that Indonesia's Conditional Cash Transfer program which incentivized the use of midwives as primary delivery attendants increased the supply of midwives in the market. In contrast, Freedman et al. (2015) find that Medicaid expansion slows hospitals' adoption of new technology when newly Medicaid-insured may have come from the privately insured population.

[4]There is also a large literature that studies the Children's Health Insurance Program (CHIP) in the United

expansion, limited research has examined whether these supply expansions translate into greater access to care services. This study fills that gap by assessing the effectiveness of public insurance in addressing the under-provision of long-term care through two distinct channels: incentivizing supply expansion and improving the affordability of care services.

This paper also contributes to the growing literature assessing the impact of LTCI. The current literature primarily focuses on demand-side (individual) outcomes in developed countries and suggests that public LTCI programs can enhance the welfare of LTCI beneficiaries by reducing avoidable hospital admissions and medical expenditures (Kim and Lim, 2015; Serrano-Alarcón et al., 2022; Bakx et al., 2023), lowering mortality risk (Sohn et al., 2020), improving general mental health (Barnay and Juin, 2016) and self-rated health (Wang et al., 2023), as well as enhancing overall care-related quality of life (Forder et al., 2018).[5] Recent studies find that China's LTCI pilot has reduced the likelihood of older adults' needs for care being unmet (Lei et al., 2022), decreased hospital care and medical expenditures (Lu et al., 2020; Feng et al., 2020a) and increased non-health consumption of elderly households (Liu et al., 2023). Different from these papers, we explicitly model older adults' choice between informal and formal care and quantify how LTCI contributes to greater utilization of formal care through demand-side improvements in LTC affordability and supply-side expansion in LTC service capacity.

In addition to these studies, our paper is closely related to Hackmann et al. (2023), who examine the expansion of LTC providers following the introduction of universal LTC insurance in Germany. While both papers study the welfare effects of LTCI, they differ in emphasis. Hackmann et al. (2023) analyze LTCI in the *input* market, viewing the policy as a form of industrial policy and fiscal stimulus. Using a labor market search model, they show that, in a high-unemployment environment, the LTC sector expanded without increasing wages and generated positive fiscal externalities. Our analysis differs along two dimensions. First, we study the effects of LTCI in the *output* market, focusing on consumer access to care and distinguishing between demand-side subsidies and supply expansion. Second, we discuss in our context the effect heterogeneity across variation in LTCI benefit design. Broader benefit eligibility does not lead to greater entry, and subsidies for informal care do not deter entry by formal providers.

The rest of this paper proceeds as follows. Section 2 introduces the institutional back-

States. Some studies show that its expansion resulted in increased pediatrician participation in Medicaid through the program (Garthwaite, 2012) and an 18% higher likelihood of newly trained pediatricians subspecializing and entering private practice (Chen et al., 2018). However, White (2012) find that the expansion of CHIP did not improve patients' physician utilization.

[5]In line with these findings, a study by Miyawaki et al. (2020) investigating a contractionary LTCI reform in Japan that reduced coverage benefits for low-level care needs find that the reform resulted in increased time dedicated to informal caregiving, negatively impacting caregivers' health.

ground of LTCI and LTC provision in China. Section 3 presents a stylized theoretical model to illustrate how public insurance can stimulate the supply of care services. Section 4 describes the data and construction of the sample used for empirical analysis. Section 5 discusses our empirical strategies and presents our estimated effects of LTCI on provider entry and the labor market. Section 6 explores the implication of supply expansion on the older population's care choices. Lastly, Section 7 concludes with final remarks.

2. Institutional Background

In the following three subsections, we introduce the public LTCI pilot and the LTC industry in China.

2.1. The Pilot Program

Prior to the pilot, there was no public insurance covering individual LTC expenses. To address the anticipated increase in demand for long-term care and to assess the feasibility and sustainability of a potential nationwide LTCI scheme, the Chinese government launched a pilot LTCI program in June 2016. The Ministry of Human Resources and Social Security issued a policy directive detailing the objectives, criteria for pilot city eligibility, and the process for pilot rollout.[6]

Initially implemented in 15 cities, the program expanded over time. Between June 2016 and January 2020, an additional 20 cities in Shandong and Jilin provinces joined the pilot. In September 2020, the central government further expanded the program to include 14 more cities, bringing the total number of pilot cities to 49. Figure 1 illustrates the geographical locations of cities that started the LTCI pilot each year from 2016 to 2020.[7]

City and municipality governments applied to the State Council for eligibility as pilot cities. The selection process evaluated applications based on: (1) the city's economic development and financial capacity to ensure self-sufficiency; (2) the demographic profile, particularly the aging population and the share of disabled elderly individuals; and (3) the representativeness of the city for the region. The Guidelines issued by the State Council highlighted the experimental nature of the program that limited the scale of pilot during our sample period: nearly all second-round pilots (2016–2020) were drawn from provinces not included in the first round, and only one city per province could be selected.

[6] See "Guiding Opinions on Piloting the Long-Term Care Insurance System" (Document No. [2016]80).

[7] Qingdao, Dongying, and Weifang initiated their own LTCI programs before the 2016 pilot and are therefore excluded from the treatment group in our research design.

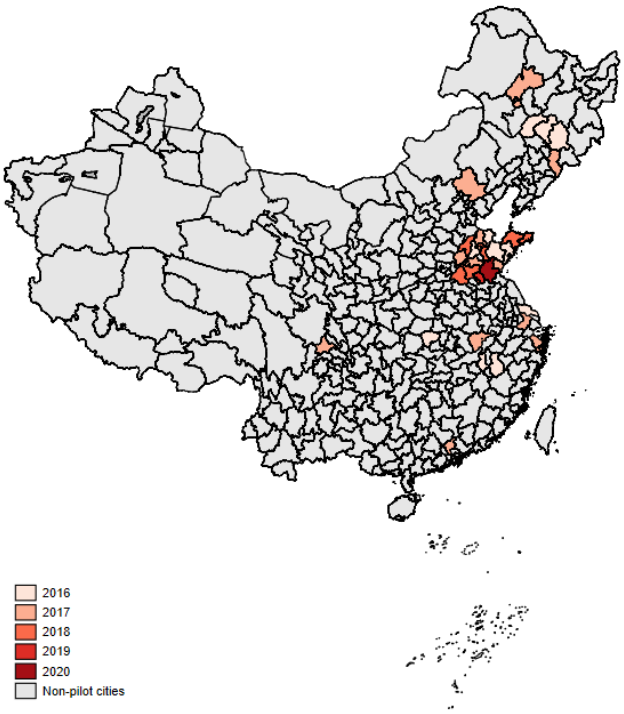


Figure 1: LTCI pilot cities during 2016–2020

Notes: The timing of city-level pilot enactment is collected by the authors from policy documents released by the central and local governments. Detailed data is in Online Appendix C.1.

2.2. Public Long-Term Care Insurance in China

2.2.1. Overview

The LTCI program operates within the infrastructure of China's public health insurance systems: Urban Employee Basic Medical Insurance (UEBMI) and Urban-Rural Resident Basic Medical Insurance (URRBMI). UEBMI is a mandatory, employment-based program for urban workers, primarily funded through payroll taxes contributed by both employers and employees.[8] In contrast, URRBMI is an optional program for other citizens (e.g., the unemployed and rural residents), primarily financed by central and local government subsidies for individual premiums. As of 2016, when the pilot program commenced, the two systems provided health insurance coverage to over 95% of the population but offered minimal coverage for LTC expenses.

In 2021, the LTCI program in the 49 pilot cities had 144.607 million participants, including

[8]LTCI premiums are automatically withheld through mandatory payroll deductions for UEBMI enrollees. While opt-outs are generally not permitted, several policy features are in place to address potential affordability concerns among low-income individuals. First, LTCI premium is affordable. The province-level *hourly* minimum wage ranged from 9.9 to 19 Chinese Yuan. The *annual* LTCI premium was equivalent to 1.57 to 9.09 times the minimum wage. Second, means-tested subsidies are provided to low-income enrollees.

1.087 million beneficiaries.[9] The income of the LTCI fund amounted to 26.06 billion Chinese Yuan, with expenditures totaling 16.84 billion Chinese Yuan. Figure 2 depicts the national level time trends in the number of LTCI enrollees (Panel A) and beneficiaries (Panel B), as well as LTCI fund income (Panel C) and expenditures (Panel D).

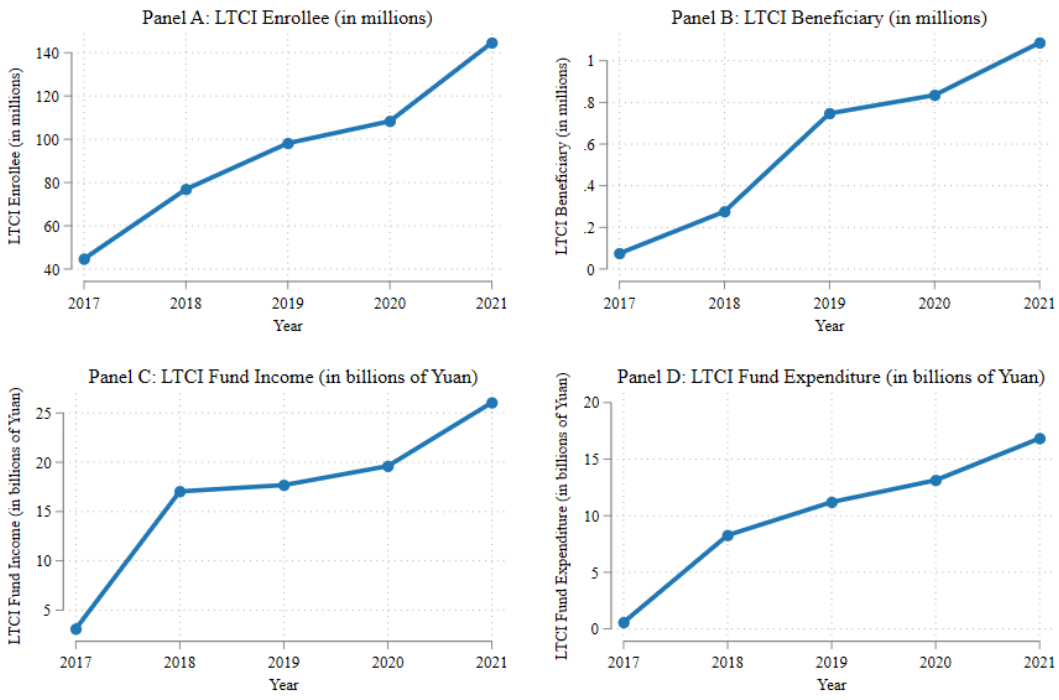


Figure 2: Aggregate statistics on the numbers of LTCI enrollees and beneficiaries, and LTCI fund income and expenditure

Note: Data is compiled from the website of the National Healthcare Security Administration (http://www.nhsa.gov.cn/).

2.2.2. Premium Contribution

The primary goal of the LTCI is to provide financial protection for individuals with functional impairments who require costly LTC services. The program is integrated into UEBMI (and in half of the pilot cities, also URRBMI), with enrollees aged above 60 automatically becoming beneficiaries. Enrollees are required to make a flat annual premium contribution (e.g., 30 to 90 Chinese Yuan) or a proportionate contribution (0.3% to 1% of their annual salary).[10] In addition to premiums, other funding sources include government subsidies, public welfare funds from welfare lotteries, and charitable donations.

[9]During the Covid-19 pandemic, most cities temporarily suspended long-term care services due to restrictions imposed by stay-at-home orders.

[10]Based on the surplus of their medical insurance funds and financial capacity, pilot cities can design the implementation of LTCI, including enrollees' contribution levels and premium calculation methods.

2.2.3. Price Regulation and Benefits

Insured LTCI beneficiaries become eligible for LTC expense reimbursement if they have experienced a certain degree of disability, as determined by the Barthel Index of Activities of Daily Living, for at least six months. LTCI's cost-sharing structure follows the framework of these two public health insurance systems: beneficiaries pay out-of-pocket at the point of service, and the remaining costs are automatically reimbursed by LTCI.

Pilot cities have adopted three reimbursement models for LTCI: fee-for-service, per-diem, and capitation. Although reimbursement models differ in their benefit formulas—conditioning payments on either service type or disability level—they are consistent with precedent care service pricing guidelines. These guidelines, comprised of a standardized service catalog and pricing standard, ensure broad consistency across regions in pricing and subsidies while allowing for limited local discretion.[11]

Under the fee-for-service model, reimbursement is tied to the actual quantity and type of services delivered. Rates typically range from 70% to 90%, substantially reducing out-of-pocket expenditures and enabling individualized care. The model features high transparency but poses risks of overprovision, which are often mitigated using daily or monthly expenditure ceilings.

The per-diem model pays a fixed daily amount conditional on service provision, whereas the capitation model assigns a fixed monthly payment based on the assessed disability level. Both models strengthen incentives for cost control and reduce administrative burdens, but require more sophisticated providers to deliver cost-effective care plans while maintaining quality.

2.3. Long-Term Care Providers: Historical Overview and Regulatory Frameworks

The landscape of LTC service providers in China has undergone significant changes in the recent decade. Before the pilot, public facilities traditionally dominated the formal care sector. Aimed at transitioning from a direct provider to a regulator of LTC services, the pilot program was designed to encourage private entry into the industry and achieve a market-based solution to LTC under-provision (State Council, 2013; Feng et al., 2020b). Specifically, three types of care services are reimbursed: home care, community care, and institutional care.

[11] See National Development and Reform Commission (2012), "National Standard of Medical Service Pricing Items (2012)," Section 1.3 (Care Services), available at https://www.ndrc.gov.cn/fggz/tzgg/ggkx/201205/t20120518_1063839.html.

Home, community, and institutional care services encompass a shared set of services, including fundamental daily living assistance (e.g., feeding, dressing, and bathing) and basic medical services (e.g., nursing care, rehabilitation exercises, and health counseling). The distinction lies in the location of services: home and community care are provided at a beneficiary's home and in their residential community, respectively, while institutional care is delivered within designated residential care facilities or nursing homes with long-term residences.

Insured beneficiaries become eligible as soon as they meet the waiting period. To receive LTCI benefits, eligible beneficiaries are required to obtain formal care services from designated providers. New providers are granted qualifications for LTCI enrollment on a monthly or quarterly basis.[12] Upon approval as designated providers, they are obligated to adhere to regulated pricing. For instance, the Shanghai municipality government set prices for 27 daily living assistance services and 15 basic medical services.

3. Conceptual Framework

In this section, we present a conceptual framework that formalizes the mechanisms through which public insurance can stimulate the supply of care services and affect equilibrium LTC utilization. The framework builds on the seminal insights of Arrow (1963) and is micro-founded on elderly individuals' care choices and potential providers' entry decisions. A technical version of the model and the corresponding comparative statics are provided in Appendix B.

We begin by describing demand for formal care, micro-founded by elderly individuals' discrete choice among informal care, formal care, or no care in a given period. An individual's decision depends on health status, economic resources, the out-of-pocket price of formal services (net of insurance reimbursement), and the availability of providers. Aggregate demand is obtained by summing individual choices across the population and over time. On the supply side, entry follows a free-entry condition: new providers enter when incumbent profits are temporarily high, and entry continues until profits are driven down to the level of entry costs.

Given these primitives, the introduction of an LTCI scheme has three effects. The first is a demand-side effect: LTCI lowers out-of-pocket prices for formal care, increasing the likelihood that elderly individuals choose formal care even when market structure is fixed in the short run. The second is a supply-side effect: higher demand raises incumbent providers' profits,

[12] Details are provided in Appendix C.4.

inducing entry by new LTC providers in pilot cities. The third is an equilibrium utilization effect: supply-side expansion, through greater provider availability, further increases formal care utilization among the elderly.

The empirical relevance and relative importance of these three effects are not obvious nor predetermined by the theoretical framework. Instead, their magnitudes depend on key economic primitives. The demand-side effect—the impact of LTCI on aggregate formal care demand—is governed by consumer responsiveness to LTCI benefits and the demographic composition of a city. A greater share of eligible benefits among the elderly and stronger responsiveness to benefits generate larger demand expansion.

The supply-side effect—new entry prompted by LTCI's demand-side effect—is related to the level of entry costs and the degree of substitutability between LTC providers. When substitutability is large, new entrants primarily divert existing formal care consumers from incumbents rather than extending services to previously underserved populations. In this case, profits decline more quickly with the number of active providers, limiting the extent of new provider entry.

Finally, the equilibrium utilization effect—the increase in formal care demand arising from supply expansion—depends on the extent to which greater provider availability raises individuals' propensity to choose formal care. A more salient change in propensity translates into larger incremental utilization through this supply channel. In our empirical analysis, we provide evidence on these effects.[13]

4. Data, Sample Construction, and Measurement

4.1. Administrative Data on Firm Registration

The firm registration information used in this paper is sourced from the National Enterprise Credit Information Publicity System (NECIPS), which is developed and maintained by the State Administration for Market Regulation of China. This system contains comprehensive information on all business and non-profit establishments in China. We extract firms' registration dates, Chinese Industry Classification codes, operating business descriptions, and locations from this database.

We follow the methodology of Beraja et al. (2023), who utilize NECIPS data to identify Chinese firms specializing in artificial intelligence. To isolate LTC providers in our dataset, we

[13]While the three effects are interconnected through economic primitives, they are not necessarily of similar magnitudes. A canonical example, provided by Berry and Waldfogel (1999), is that when products are minimally differentiated and prices are fixed, free entry will generate excessive supply relative to a social planner's allocation, without increasing overall consumer demand.

first select firms that meet both of the following criteria: (1) inclusion of terms "elderly care" and "care/nursing" in operating business descriptions; (2) being regulated by the "Industrial and Commercial Bureau." Through this process, we obtain a sample of 43,472 observations.

Next, we narrow down our sample based on Chinese Industry Classification (CIC) codes. The National Bureau of Statistics of China published the *Statistical Classification of the Eldercare Industry* in 2020.[14] The guidelines defined three categories of LTC services: home-based, community-based, and institutional-based services (also summarized in Section 2.3), and provided CIC codes associated with each category. We retain providers from the initial selection whose CIC codes align with the Bureau's guidelines and label them as home- and community-based service (HCBS) and/or institutional providers accordingly (following Feng et al., 2020a).[15] Appendix A provides detailed information on our sample construction procedures based on CIC codes. After applying this criterion, we identify a sample of 12,494 unique LTC providers entering the market between 2010 and 2019.

Lastly, we aggregate this data to counts of provider entries at the city-quarter level. The resulting sample encompasses 265 prefecture-level cities over 40 quarters spanning 2010 to 2019, totaling 10,600 observations. Among these cities, 36 have implemented LTCI over the sample period (henceforth the treatment group), while the remaining cities constitute the control group.

Panel A of Table 1 presents the average numbers of LTC provider entries among the two groups, both in the pre-pilot (2010–15) and post-pilot (2016–19) periods. Between 2010 and 2015, there were 0.122 and 0.395 entrants among cities in the control and the treatment groups, respectively (see Columns (1) and (3)). Between 2016 and 2019, the numbers increase to 1.072 and 3.106, respectively (see Columns (2) and (4)). The simple difference-in-differences $[((4)-(3))-((2)-(1))]$ is reported in Column (5). Similar patterns in terms of the discrepancy between the treatment and control groups in HCBS and institutional provider entry are observed. The simple difference-in-differences terms are significant at the 1% level for all three measures.

[14]Source: https://www.gov.cn/gongbao/content/2020/content_5503559.htm (in Chinese).

[15]Home-based service and community-based service providers have one Chinese Industry Classification code in common. We consolidate the classification to avoid double-counting. The two types of services are close in nature, as they both operate within residential communities and serve local residents. In contrast, institutional providers, commonly known as nursing homes, require a more substantial upfront installment of equipment and provide a broader spectrum of services. For this reason, Feng et al. (2020a) also adopt the same classification.

4.2. Labor Market Survey

We utilize the China Migrants Dynamic Survey (CMDS) from 2013 to 2018 to explore the labor market effects of LTCI. The CMDS is a comprehensive nationwide annual survey administered by the National Health Commission of China, covering demographic and employment information of approximately 200,000 migrant households in each wave.

This dataset is particularly suitable for our study for two main reasons. First, as highlighted in He and Luo (2020), migrant workers constitute 90% of the domestic service workforce in China, including roles such as nursing, house cleaning, and maternity care. Many of these positions lack formal labor contracts, involve a heavy workload, but offer low wages. Consequently, workers who participate in these occupations are mostly migrant workers with limited access to other job opportunities. This phenomenon is not unique to China and is observed worldwide (see Bakx et al. 2023 and Banks et al. 2023 for evidence in the Netherlands and England, respectively, and Gruber et al. 2023 for a summary).[16] Second, the CMDS is the only labor market survey that offers nationwide coverage of Chinese cities during both the pre- and the post-pilot periods in our research design.

We calculate the LTC employment share at the city level. LTC workers are identified based on both their industry of employment and occupation description. Specifically, we define LTC workers in CMDS as individuals who (1) are employed in LTC-related industries as defined by two-digit CIC codes (refer to Appendix A for the list) and (2) hold occupations listed as "domestic service worker" or "personal care assistant with activities of daily living."

Descriptive statistics for the CMDS are reported in Table 1, Panel B. Between 2013 and 2018, the survey covered 227 cities in the control group and 36 cities in the treatment group.[17] Prior to the pilot, the average share of LTC workers in treatment cities exceeded that in control cities by 4.8 percentage points. In the post-pilot period, this gap declined to 3.1 percentage points. Working hours and hourly wages rose modestly in control cities and increased by more in treatment cities, although the difference in growth rates across groups is not statistically significant. These patterns indicate shifts in employment outcomes in the LTC market over the sample period; however, they do not by themselves establish an effect of LTCI implementation. We examine this question formally in Section 5.

[16] Gruber et al. (2023) summarize evidence from ten developed countries and conclude that many countries have relied on immigrant populations for formal long-term caregivers.

[17] The number of prefecture-level cities covered by CMDS varies little across survey waves.

4.3. The Chinese Longitudinal Healthy Longevity Survey (CLHLS)

To complement the previous supply-side datasets, we incorporate data on the elderly's care choices. Our primary source for individual-side data is the Chinese Longitudinal Healthy Longevity Survey (CLHLS), conducted by the Center for Healthy Aging and Development Studies at Peking University in China. CLHLS surveys individuals aged 65 and above, collecting their demographic characteristics, socioeconomic status, family background, health, and other relevant information. Following the approach of previous LTC literature (e.g., Lei et al., 2022), we utilize the CLHLS data in 2011, 2014, and 2018, which represent the three most recent waves of CLHLS. In the control group cities, there are 3,071 individuals surveyed at least in one wave, while in the treatment group cities, there are 2,166 such individuals.[18] Note that the CLHLS dataset includes only one wave of data following the pilot (2018). As a result, our analysis of demand responses should be interpreted as short-term effects.

In Panel C of Table 1, we present summary statistics for key indicators related to care needs and choices used in this paper. We first examine weekly hours of informal care received. Before the pilot, the average informal care hours were 5.8 and 8.781 in the control and treatment groups, respectively. After 2016, they grew to 15.062 and 16.283, respectively. We then consider two indicators of whether elderly individuals opt for informal or formal care as the primary source of elderly care. The share of individuals choosing each type of care increased over time, while the treatment-control difference in care choices diverged after the pilot: the difference in informal care narrowed, and the difference in formal care remained constant. Male individuals constituted a larger share of observations after the pilot in treated cities, and household income increased more in control cities than in treated cities. Lastly, we examine individual independence in ADL. We define having difficulty in performing at least one of the six basic activities as ADL impairment (same as in Liu et al., 2023) and find that the share of individuals with ADL impairment grew in treated cities from the pre- to post-pilot period.[19]

4.4. Other Data

We augment our dataset with macroeconomic and demographic characteristics at the city-level prior to the LTCI pilot. We extract baseline socio-economic variables, including GDP per capita, fiscal expenditure per capita, and healthcare facilities, from the 2010 China City Statistical Yearbook. Moreover, we compute demographic characteristics using the

[18] Among these respondents, 2,601 were interviewed in 2011 and survived until 2018, and 898 were first interviewed in 2014, of whom 522 survived until 2018.

[19] The six activities are eating, dressing, bathing, getting in/out of bed, using the toilet, and controlling urination and defecation.

2010 census data, including the proportion of the population aged 65 and above, and the dependency ratio.

Panel D of Table 1 reports summary statistics for city-level characteristics. Cities that implemented LTCI differ from those that did not. Treatment cities have higher GDP per capita, greater fiscal expenditure, more hospital beds per 10,000 residents, higher dependency ratios, and a larger share of the population aged 65 and above. We account for these cross-city differences in the empirical analysis that follows.

5. The Effects of LTCI on Provider Entry and LTC Labor Market

5.1. Empirical Strategy

We employ a difference-in-differences (DID) strategy to estimate the effects of LTCI on provider entry and the LTC labor market, exploiting temporal and geographic variation generated by the staggered rollout of the program across cities. Because LTCI was introduced at different dates across pilot cities, our baseline specification adopts a staggered DID design. Recent work shows that conventional two-way fixed effects estimators may yield biased estimates in such settings.[20]

To address these concerns, we estimate treatment effects using the cohort-based DID estimator of Callaway and Sant'Anna (2021). Let g denote the first period in which a city adopts LTCI and t the calendar period. For each (g, t), the estimator obtains the group-time average treatment effect on the treated, $\text{ATT}(g, t)$, which compares outcomes for cities first treated in period g to never-treated cities, based on observable characteristics. This approach delivers consistent estimates in the presence of staggered adoption and heterogeneous treatment effects.

Our baseline results report an aggregated effect that averages $\text{ATT}(g, t)$ across cohorts and post-adoption periods. The overall effect is given by

$$\text{ATT} = \sum_{g} \sum_{t \geq g} w(g, t) \cdot \text{ATT}(g, t), \tag{1}$$

where $w(g, t)$ are probability weights proportional to the share of cities first treated in cohort g. Technical details are provided in Appendix D.1.

[20] Two-way fixed effect models implicitly form weighted averages of cohort-specific treatment effects, potentially with negative weights in the presence of treatment effect heterogeneity (e.g., Callaway and Sant'Anna, 2021; De Chaisemartin and d'Haultfoeuille, 2020; Goodman-Bacon, 2021; Sun and Abraham, 2021).

Table 1: Descriptive statistics

	(1)	(2)	(3)	(4)	(5)
	Control		Treatment		DID
	Pre	Post	Pre	Post	Coef.
Panel A: LTC provider entrants					
Total entrants	0.122	1.072	0.395	3.106	1.762***
Institutional entrants	0.021	0.436	0.074	1.368	0.879***
HCBS entrants	0.100	0.635	0.321	1.738	0.883**
Total providers	1.440	10.848	5.727	33.679	18.54***
Institutional providers	0.137	3.495	0.802	11.307	7.148***
HCBS providers	1.302	7.354	4.925	22.372	11.40**
Number of cities	229		36		
Panel B: LTC workers					
City-level					
Share of LTC workers	0.252	0.191	0.300	0.222	-0.0163
Working hours	59.060	59.835	55.198	62.278	6.306
Hourly wages	14.469	15.296	12.686	14.659	1.146
Worker-level					
Age	32.985	34.291	31.860	35.386	2.220
Female	0.518	0.529	0.495	0.516	0.00940
Primary school	0.070	0.112	0.053	0.050	-0.0449
Middle school	0.543	0.563	0.514	0.528	-0.00584
High school	0.270	0.201	0.302	0.278	0.0455
College	0.107	0.112	0.129	0.141	0.00748
Number of cities	227		36		
Panel C: The elderly population					
Informal care hours	5.800	15.062	8.781	16.283	-1.852
1(Informal care)	0.120	0.260	0.206	0.237	-0.107**
1(Formal care)	0.007	0.017	0.005	0.011	0.00114
Providers per 1000 pop. 65+	0.005	0.041	0.009	0.068	0.0227**
Age	80.609	85.562	81.267	84.334	-1.915***
Male	0.464	0.470	0.469	0.512	0.0375***
Coresidence with children	0.771	0.753	0.806	0.720	-0.0706***
Household income (Chinese Yuan)	23306.4	28660.4	23561.3	25502.1	-3413.2**
ADL impairment	0.078	0.205	0.134	0.198	-0.0566**
Number of individuals	2,029		1,074		
Panel D: Socioeconomic and Demographic characteristics (in 2010)					
GDP per capita (10K RMB)	3.129		4.406		
Fiscal expenditure per capita (10K RMB)	0.251		0.382		
Healthcare facilities per 10K people	4.989		4.769		
Hospital beds per 10K people	292.184		324.254		
Dependency rate	3.232		3.523		
Population share aged 65+	0.086		0.089		

Notes: Panel A describes LTC entrants in the NECIPS (2010–2019). Panel B describes city-level LTC worker information from the CMDS (2013–2018). Panel C describes LTC choices and demographic characteristics of the elderly population in the CLHLS (2011, 2014, and 2018). Panel D describes city-level pre-pilot characteristics sourced from the 2010 City Statistical Yearbooks and the 2010 population census. Columns (1) and (3) provide summary statistics for the pre-pilot period (2010–2015), and Columns (2) and (4) the post-pilot period (2016–2019). Column (5) reports the simple pre- and post-pilot difference in the treatment-control differences for each variable. Significance levels in Column (5) are computed from standard errors clustered at the city level: * p<0.1, ** p<0.05, *** p<0.01.

To assess whether provider entry reflects increased service capacity, we next examine labor market responses to the LTCI pilot. Given the labor-intensive nature of the LTC sector, employment effects are informative about capacity expansion following insurance provision (Hackmann, 2019; Hackmann et al., 2023). We estimate Equation (1) using CMDS data at the annual level.

5.1.1. Identification for the DID Strategy

Unobserved changes in the LTC industry may confound causal interpretation if pilot adoption is correlated with pre-existing trends in provider entry or labor market outcomes. To address this concern, we adopt two complementary strategies.

First, when estimating $\text{ATT}(g,t)$, we control for differential trends across cities by interacting baseline demographic and economic characteristics measured in 2010—GDP per capita, fiscal expenditure per capita, healthcare facilities and hospital beds per 10,000 people, the dependency ratio, and the share of the population aged 65 and above (denoted $W_{c,2010}$)—with a linear time trend τ_t. This specification absorbs variation in provider entry driven by differential pre-treatment conditions. Identification relies on the assumption that, conditional on these interactions, the exact timing of LTCI implementation is independent of transitory LTC supply shocks. Following Barwick et al. (2022), Appendix Figure D5 documents the absence of systematic relationships between pilot timing and city characteristics measured in 2010, supporting this assumption.

Second, we estimate dynamic treatment effects using the event-study version of the Callaway and Sant'Anna (2021) estimator. This approach traces the evolution of treatment effects before and after LTCI implementation and provides a direct assessment of differential pre-trends between treated and untreated cities. Definition 2 in Appendix D.1 details the construction of the estimator.

5.2. Baseline Results

5.2.1. LTC Provider Entry

Table 2 presents the regression results based on Equation (1). In Column (1), we report the impact of the LTCI pilot on the entry of all types of LTC providers. The LTCI pilot results in a significant quarterly increase of 0.967 (245%) more LTC provider entrants in pilot cities.[21] This substantial growth suggests a positive association between the introduction of LTCI and the expansion of service providers. The results align with the estimates in Hackmann

[21] In Table 1, Panel A, the average number of entrants is 0.395 for treated cities in the pre-pilot period. The growth in the number of all entrants is equal to $0.967/0.395 = 2.45$.

et al. (2023), which find that a 10-percentage-point expansion in the share of insured elderly individuals leads to a 7% increase (0.05) in the number of inpatient LTC firms per 1,000 elderly individuals.

Columns (2) and (3) further distinguish LTC providers into home- and community-based service (HCBS) providers and institutional providers, as described in Section 4.1. The growth in HCBS providers closely mirrors the overall increase, while institutional providers experience a more moderate growth, approximately half of the overall growth. This differential supply expansion may reflect the Chinese tradition of relying on informal family care and the moral obligation of children to support their parents. This differential growth is also consistent with the institutional context that the vast majority of elderly Chinese individuals receive care from home or community care centers.[22]

In Columns (4)–(6), we repeat the analysis for the total number of providers. Consistent with the summary statistics reported in Table 1, we find statistically significant increases in the stock of all providers, HCBS providers, and institutional providers.

Table 2: The impact of LTCI on firm entry

	Net entry			Providers		
	(1)	(2)	(3)	(4)	(5)	(6)
	All	HCBS	Institutional	All	HCBS	Institutional
ATT	0.967***	0.547***	0.421	12.597**	7.258**	5.339***
	(0.318)	(0.180)	(0.297)	(5.197)	(3.474)	(1.939)
N	10600	10600	10600	10600	10600	10600
Dependent var. mean	0.635	0.392	0.242	6.793	4.834	1.959
Trend $\times W_{c,2010}$	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table presents estimates of the treatment effect of LTCI on firm entry following Callaway and Sant'Anna (2021). Data is from NECIPS (2010–2019). The city-level variables, measured in 2010, that are interacted with a linear time trend include GDP per capita, healthcare facilities per 10,000 people, hospital beds per 10,000 people, fiscal expenditure per capita, dependency rate, and the proportion of the population aged 65 and above. Standard errors, clustered at the city level, are reported in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

[22] As documented in Nie and Zhao (2023) and Lobanov-Rostovsky et al. (2023), among other research, the central government of China depicts its eldercare landscape as a "90-7-3" model, with 90% of elderly individuals using home-based care, 7% using community-based care, and 3% using institutional care. The reimbursement rate for home-based care is slightly more generous than that of institutional care. For instance, in Shanghai, home-based care is reimbursed at a rate of 90%, while institutional care is reimbursed at 85%.

5.2.2. LTC Labor Market

The estimation results for Equation (1) are reported in Table 3. Column (1) shows that LTCI implementation significantly increases the employment share of LTC-related jobs in pilot cities by 120%, compared to non-pilot cities with similar pre-pilot characteristics.[23] Column (2) indicates that LTCI has no detectable effect on working hours.[24] Column (3) of Table 3 also shows an increase in average city-level wages. However, the increase is not broad-based: Columns (4)–(6) of Panel A indicate that the wage effects are concentrated in the upper percentiles of the earnings distribution, with no statistically significant changes at the 25th percentile. Panel B of Table 3 and the worker-level evidence in Table 4 clarify the nature of this upper-quartile wage increase. In Panel B of Table 3, we document shifts in aggregate worker characteristics following LTCI implementation—most notably, a decline in the share of workers with middle-school education and an increase in the share of high-school-educated workers.

The worker-level regressions reported in Table 4 reinforce this interpretation. Once demographic characteristics are controlled for, the ATT estimate in Column (1) for individual wages is close to zero and statistically insignificant. In other words, LTCI does not raise wages *within* worker types. The higher average wages observed at the city level arise primarily because pilot cities shifted toward worker profiles associated with higher wage premiums after the policy rollout. Moreover, in Column (2), we report coefficient estimates from a Mincerian wage regression in which log wages are regressed on worker characteristics—potential experience, experience squared, gender, and educational attainment. This regression includes city–industry fixed effects, which capture aggregate wage levels and regional labor market conditions, and year fixed effects, which absorb macroeconomic factors.

Taken together, these results suggest that LTCI raises labor income mainly through workforce expansion and compositional change, rather than through providers offering higher wage premia to existing workers. The expansion of LTC services draws better-educated workers into the sector, shifting the upper tail of the wage distribution without increasing wages for comparable workers. This mechanism is consistent with the documented increase in employment and aligns with prior evidence that insurance expansions stimulate labor demand in long-term care markets.

[23] The change in employment share is $\exp(0.787) - 1 = 1.20$.

[24] These findings are consistent with Hackmann et al. (2023), who document that a 10 percentage point expansion in the share of insured elderly individuals leads to a 13 percent increase in LTC employment, equivalent to four additional workers per 1,000 elderly.

Table 3: Employment, income, and worker characteristics (city-level)

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Employment and income						
	Log		Log hourly wage			
	Share	Work hours	Mean	p25	p50	p75
ATT	0.787**	0.038	0.473***	0.085	0.293**	0.613***
	(0.401)	(0.086)	(0.136)	(0.166)	(0.116)	(0.191)
N	1473	974	966	966	966	966
Panel B: Worker characteristics						
	Experience	Share	Share of worker educational attainment			
		Female	Primary	Middle	High	College
ATT	-0.615	−0.285**	-0.042	-0.155	0.178**	0.009
	(1.846)	(0.125)	(0.107)	(0.140)	(0.084)	(0.052)
N	1473	1473	1473	1473	1473	1473

Notes: This table presents regression estimates of the impact of LTCI on the labor market following Callaway and Sant'Anna (2021). Data come from CMDS (2013–2018). Panel A reports the overall treatment effect of LTCI implementation on city-level employment and income. Panel B reports the effect on city-level average worker characteristics. City-level variables measured in 2010 are controlled for in constructing the estimator, including GDP per capita, healthcare facilities per 10,000 people, hospital beds per 10,000 people, fiscal expenditure per capita, the dependency rate, and the share of the population aged 65 and above. Standard errors are clustered at city-level and reported in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

Table 4: Wage analysis (worker-level)

	(1)	(2)
	log wage	log wage
ATT	-0.003	
	(0.017)	
Experience		0.032***
		(0.001)
Experience^2		-0.001***
		(0.000)
Female		-0.213***
		(0.005)
Middle school		0.085***
		(0.005)
High school		0.224***
		(0.008)
College		0.503***
		(0.031)
N	698,862	713,566

Notes: Notes: This table presents coefficient estimates, with standard errors clustered at the city level in parentheses. Data come from CMDS (2013–2018). Column (1) reports the Callaway and Sant'Anna (2021) estimator of the treatment effect of LTCI on workers' log wages, controlling for demographics. Column (2) reports coefficient estimates from a Mincerian wage regression in which log wages are regressed on demographic characteristics (job experience, experience squared, gender, and education levels), controlling for year fixed effects and city–industry fixed effects. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

5.3. Robustness Checks

In the preceding sections, we established that LTCI substantially boosted the formal care service capacity in pilot cities. In this section, we discuss several potential threats to the validity of our empirical approach, along with robustness checks.

5.3.1. Parallel Trend Analysis

A key identifying assumption underlying the DID strategy is that, absent the pilot, outcomes in treatment and control cities would have followed parallel trends after accounting for observable differences. If treatment cities were on a systematically steeper trajectory due to unobserved factors correlated with LTC outcomes, the estimated effects of LTCI would be biased upward. We address this concern using two complementary approaches.

First, we report event-study estimates in Figures 3 and 4. For provider entry (Figure 3), Panels (1)–(3) show net entry of total, institutional, and HCBS providers, respectively, while Panels (4)–(6) report levels of total providers, institutional providers, and HCBS providers. Across all entry outcomes, we find no evidence of differential pre-trends prior to program implementation. Moreover, post-adoption effects on provider counts are statistically indistinguishable from zero by the twelfth quarter after pilot implementation.

Figure 4 presents event-study estimates for labor market outcomes. Panel (1) reports results for log employment share; Panels (2)–(4) for mean and quantiles of log wages; and Panels (5)–(6) for the share of female workers and the share of workers with at most a high school education. As with provider entry, we find no evidence of differential pre-trends for any labor market outcome.

Second, we assess the sensitivity of our estimates to potential violations of the parallel trends assumption using the approach of Rambachan and Roth (2023). This method formalizes the notion that counterfactual post-treatment trends should not diverge substantially from pre-treatment trends by allowing for bounded deviations in linear trends between pilot and non-pilot cities. We construct confidence sets for the average treatment effect on the treated under alternative degrees of trend divergence. Implementation details are provided in Appendix D.3, and results are shown in Appendix Figure D4. For the estimates reported in Panel B of Table 2, the effects for all providers and institutional providers (Columns (1), (3), (4), and (6)) remain robust even when treatment and control trends differ by up to 0.5 standard errors of the baseline estimate. For HCBS providers (Columns (2) and (5)), the corresponding threshold ranges from 0.3 to 0.35 standard errors.

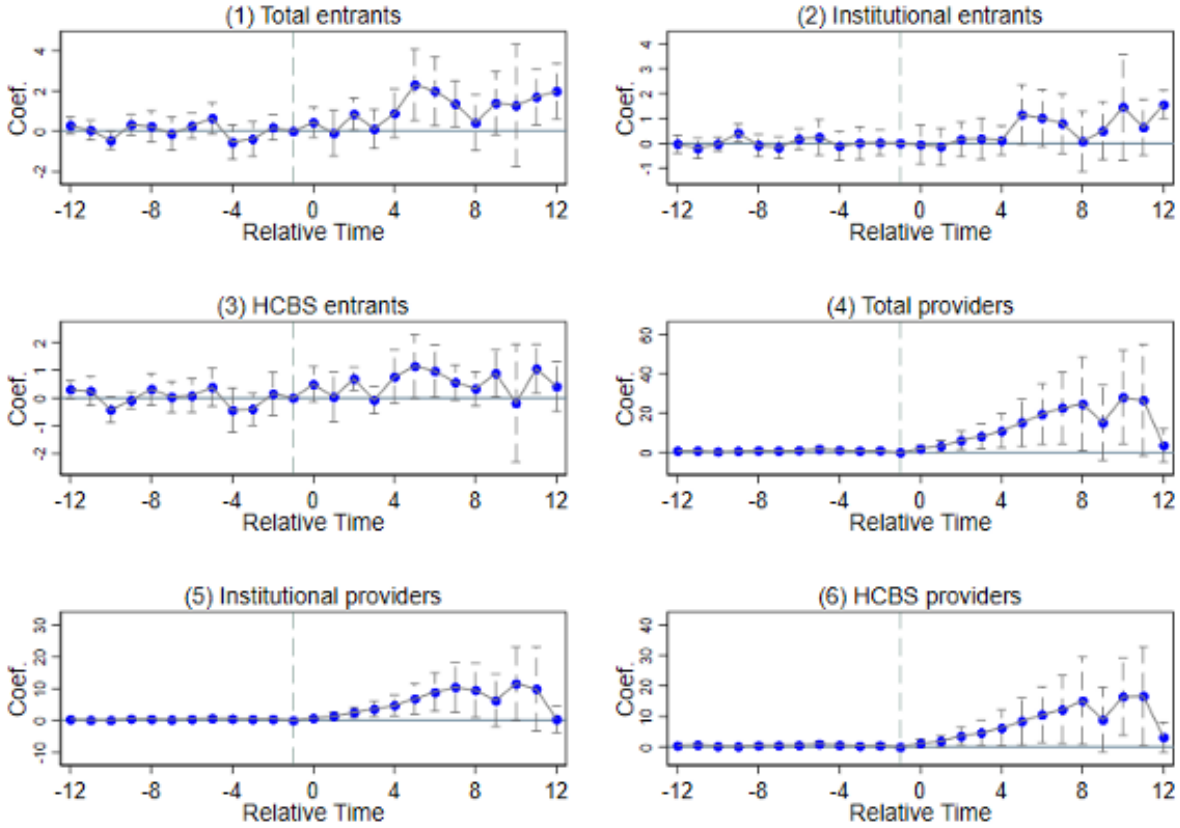


Figure 3: Effects of the LTCI pilot on provider entry: event-study estimates

Notes: This figure reports dynamic treatment effect estimates of the LTCI pilot on provider entry using the estimator of Callaway and Sant'Anna (2021). The data are from NECIPS (2010–2019). The horizontal axis denotes quarters relative to pilot implementation. Panels (1)–(3) show net entry of total, institutional, and HCBS providers, respectively. Panels (4)–(6) report the number of total providers, institutional providers, and HCBS providers. Points indicate coefficient estimates and bars denote 95% confidence intervals based on standard errors clustered at the city level.

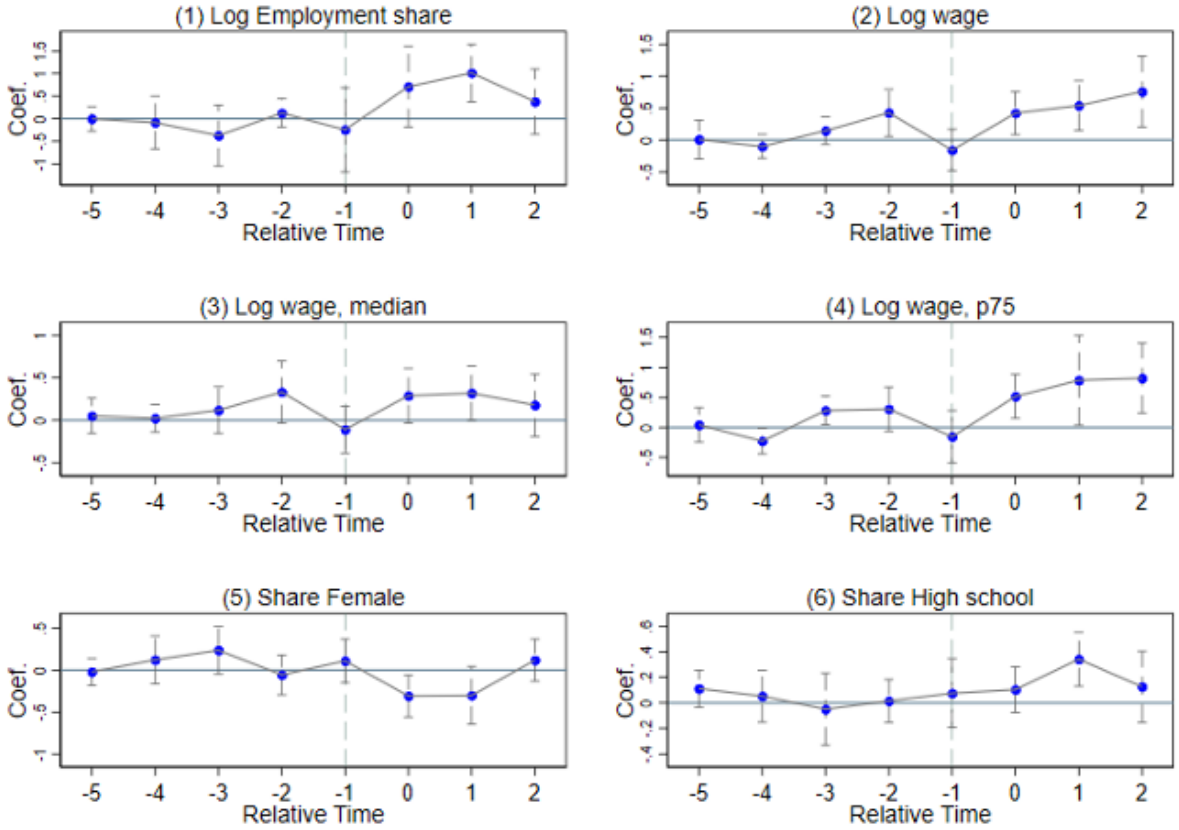


Figure 4: Labor market effects of the LTCI Ppilot: event-study estimates

Notes: This figure reports dynamic treatment effect estimates of the LTCI pilot on labor market outcomes using the estimator of Callaway and Sant'Anna (2021). The data are from CMDS (2013–2018). The horizontal axis denotes years relative to pilot implementation. Panel (1) reports log employment share. Panels (2)–(4) report the mean, median, and 75th percentile of log wages. Panels (5)–(6) report the share of female workers and the share of workers with at most a high school degree. Points indicate coefficient estimates and bars denote 95% confidence intervals based on standard errors clustered at the city level.

5.3.2. Alternative Control Groups

We enhance the baseline DID analysis with two matched-sample designs to address concerns about control group comparability. These designs exploit specific institutional features: while our review of government documents confirms that pilot selection relies on socioeconomic and demographic metrics, strict quotas (e.g., typically one city per province) and regional representativeness criteria exclude many otherwise comparable cities. This creates plausibly exogenous variation among cities with similar ex ante likelihoods of selection.

First, we implement a standard propensity score matching (PSM) approach. We estimate treatment probabilities using a logit model based on the predetermined socioeconomic characteristics driving selection and match each treated city to two control cities with similar scores.

Second, we conduct a more restrictive border-city matched analysis. For each treated city, we limit the control pool to geographically adjacent cities and apply one-to-one nearest-neighbor PSM within this restricted set. The validity of this strategy is supported by balance test results in Table D2: when projecting treatment on predetermined characteristics while controlling for constructed "city-cluster" fixed effects, covariates are not predictive of treatment, confirming strong within-cluster balance.

Using these two matched samples, we re-estimate the baseline DID specification. Results (Appendix Table D3) remain statistically significant and closely aligned with the baseline results.

5.3.3. Anticipation

A further concern in staggered DID designs is anticipation: potential entrants may enter in advance of policy implementation. If present, such behavior would bias estimated treatment effects downward, rendering our baseline estimates conservative. We address this concern with three pieces of evidence.

First, we restrict the control group to settings in which anticipation is most likely—cities that implemented LTCI shortly after the end of our sample period. As reported in Panel B of Appendix Table D5, estimates increase in magnitude when the control group is limited to cities that had not implemented LTCI by 2023. This pattern is consistent with anticipation effects, if present, biasing baseline estimates toward zero. We find similar results for labor market outcomes in Panel B of Appendix Table D6.

Second, the event-study estimates—using the period immediately prior to treatment as the reference period—provide little support for anticipatory entry. Coefficients in periods leading up to treatment are economically small and statistically insignificant, and the full set

of pre-treatment estimates is consistent with the parallel trends assumption.

Third, we further assess anticipation by sequentially excluding the second and third pre-treatment periods from the estimation sample. As shown in Panels C–D of Appendix Table D5, estimated treatment effects remain statistically significant and close in magnitude to the baseline estimates. Point estimates increase only modestly as pre-treatment periods are excluded, indicating that any anticipatory entry is economically limited. We find similar patterns for labor market outcomes in Panels C–D of Appendix Table D6.

5.3.4. Alternative Period Fixed-Effects and Time Trend Specifications

We investigate the robustness of our baseline estimates to alternative fixed-effects specifications, as reported in Panels A–C of Appendix Table D4. In Panel A, we parameterize the time trends in Equations (1) as a linear time trend interacting with a province-specific coefficient. In Panel B, we present estimates of the coefficient on the pilot dummy, allowing the period fixed effects in the regression models to be province-specific by including province-period fixed effects. This specification allows temporal shocks to be province-specific, rather than common to all provinces as in the baseline specification. Lastly, in Panel C, we use the most flexible fixed-effects specification for the entry regression: city-year fixed effects and quarter fixed effects. Since the labor market regression is at the city-year level, we cannot apply this specification for the regression analysis of labor market outcomes. The results in all specifications remain similar to our baseline results.

5.3.5. Placebo Test

Finally, we assess the statistical significance of our baseline results using a set of placebo tests. We begin by examining the effects of the LTCI pilot on a set of placebo outcomes—variables that should plausibly remain unaffected by the introduction of LTCI. These include city-level demographic indicators (e.g., the natural population growth rate), macroeconomic indicators (e.g., GDP per capita), and employment shares in non-LTC-related industries (e.g., mining, manufacturing, and financial services).

We estimate treatment effects using the Callaway and Sant'Anna (2021) estimator, controlling for the same baseline city characteristics and linear time trends as in the main analysis. To ensure comparability across outcomes, we plot the t-statistics associated with the estimated ATT for each placebo outcome Appendix Figure D6. All placebo estimates lie within the insignificant range, supporting the parallel trends assumption and indicating that our baseline results are unlikely to be driven by concurrent unobserved shocks to local economic or labor market conditions.

We further evaluate statistical significance using a randomization-based placebo procedure following Chetty et al. (2009) and Rosenbaum (2007). As emphasized in these studies, such placebo tests provide a benchmark for inference that does not rely on distributional assumptions about unobservables. We compare the estimated effect of the actual LTCI pilot with the distribution of effects obtained under placebo treatment assignments, using the same baseline specification.

Specifically, in each placebo iteration, we randomly assign an LTCI indicator and adoption timing—drawn from the set of actual treatment assignments—to cities in the control group only, while setting the LTCI indicator to zero for cities that were actually treated. Using the resulting sample, we estimate the baseline specification in Equation (1) and record two outputs: the pseudo treatment effect and its associated p-value.[25] We repeat this procedure 1,000 times, yielding a sample of 1,000 pairs of placebo estimates and corresponding p-values.

Appendix Figure D7 presents a scatterplot of the simulation results. Each dot corresponds to one placebo iteration, with the pseudo treatment effect on the horizontal axis and the associated p-value on the vertical axis. The dashed vertical reference line denotes the 10% significance threshold. Two patterns emerge. First, the vast majority of placebo estimates are statistically insignificant, with most p-values exceeding 10%. Second, the placebo estimates are economically small and centered near zero. Together, these findings indicate that random assignment rarely generates effects comparable in magnitude or significance to those observed following the actual LTCI pilot.

5.4. Effect Heterogeneity by City Demographics and Policy Design

We further explore the effect heterogeneity along two dimensions: city socioeconomic characteristics and LTCI policy design.

We first divide the sample into two sub-samples based on pre-pilot indicators such as GDP per capita, average wage, and the share of the population aged 65 and above: the subsample with above-median indicators and the subsample with below-median indicators. These indicators are intuitively correlated with market size or expected profit for potential entrants. We re-estimate the baseline model (Equations (1) and examine the heterogeneous effects of the policy. The results are presented in Figure 5. Point estimates of the impact of LTCI on the respective outcomes, along with the 95% confidence intervals calculated from standard errors clustered at the city level, are shown in all panels.

In Panels (a)–(c) of Figure 5, we report how the effect varies among cities with different baseline economic statuses. We find that, on average, pilot cities with higher GDP per

[25]The null hypothesis is that the treatment effect equals zero. Test statistics are constructed using standard errors clustered at the same level as in the baseline specification.

capita, and average wage experienced more provider entry after the pilot. These results align with the conventional wisdom of entry literature in industrial organization that markets of a larger size have more firms in equilibrium (e.g., Bresnahan and Reiss, 1991; Berry, 1992). In Panels (d)–(f), we report the heterogeneity analysis for provider counts. The results are quantitatively similar to those in Panels (a)–(c).

We next examine whether heterogeneity in LTCI policy design generates differential supply responses. Figure 6 presents the corresponding estimates. The six panels correspond to the outcomes reported in Table 2. In each panel, we present three sets of estimates: (1) the baseline effects, (2) the pilot's ATT for cities with broader LTCI eligibility, and (3) the pilot's ATT for cities that offer cash transfers to formal and informal care in addition to the standard LTCI design.

Among cities with broader LTCI eligibility—where beneficiaries include both UEBMI and URRBMI enrollees—the net entry of both HCBS and institutional providers is comparable to the benchmark estimates. Although point estimates for HCBS entry are slightly smaller, they are statistically indistinguishable from the baseline.

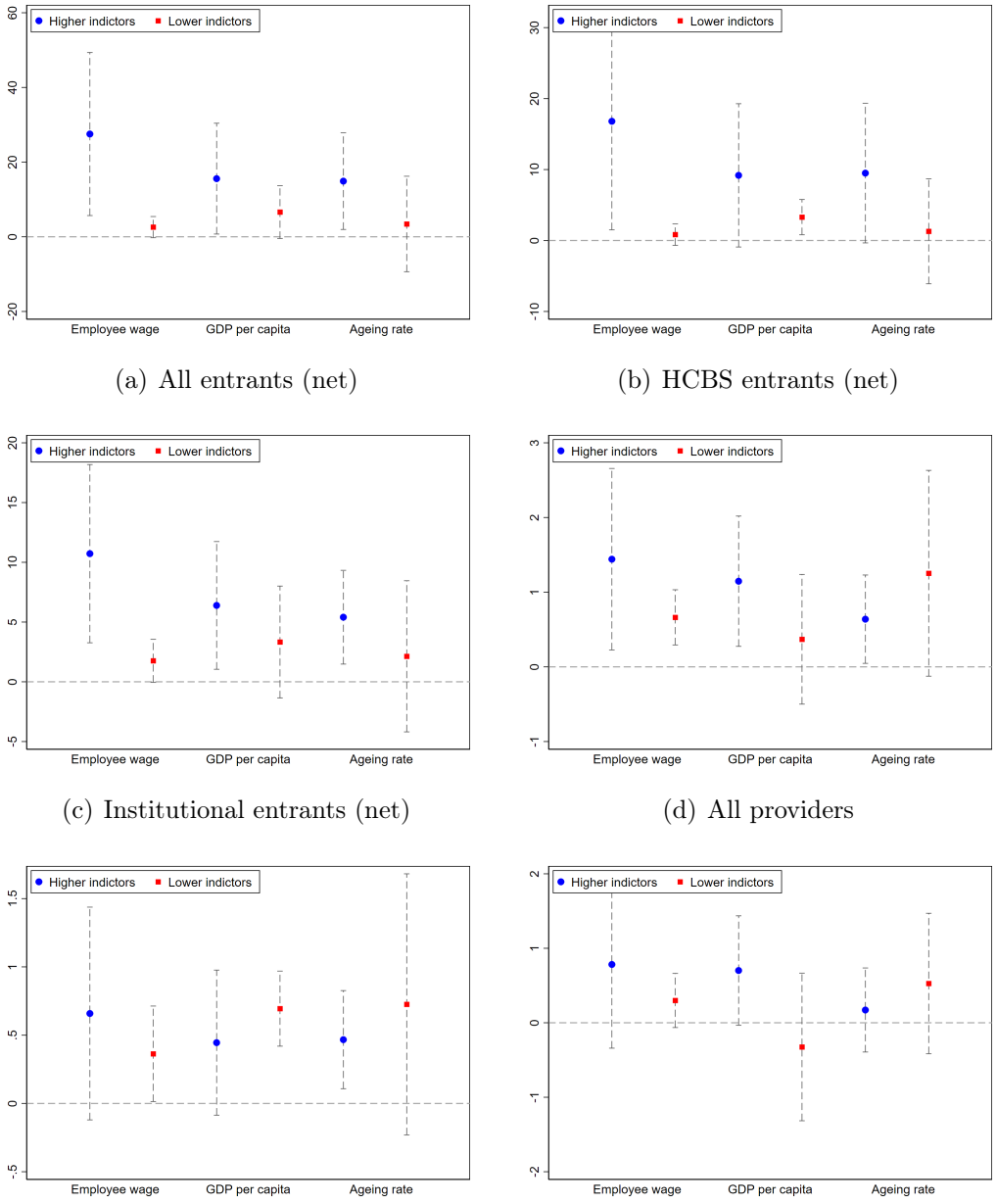
For cities providing cash transfers, contrary to concerns that subsidizing informal care may crowd out formal supply, we find that entry of both HCBS and institutional providers is larger in magnitude relative to the benchmark. In particular, the point estimates for institutional provider entry are substantially higher than in the baseline specification, reversing the pattern of relatively lower entry observed in earlier estimates, though the confidence intervals are wider. Overall, these results suggest that subsidizing informal care does not deter entry by formal LTC providers and may instead complement formal supply.

6. Implications on LTC Accessibility

The final part of our analysis applies the theoretical framework developed in Section 3 to the CLHLS data. We empirically assess the role of affordability (demand-side subsidies) and accessibility (LTCI-induced supply expansion) in shaping formal care utilization. Specifically, we estimate how long-term care demand responds to LTCI benefits and to expansions in long-term care supply.

6.1. Choice Model

For notational simplicity, the time subscript is suppressed in the exposition below. Individual care choices are indexed by j, where $j = 0, 1, 2$ correspond to the outside option (i.e., not choosing any care), informal care, and formal care, respectively.



(a) All entrants (net)

(b) HCBS entrants (net)

(c) Institutional entrants (net)

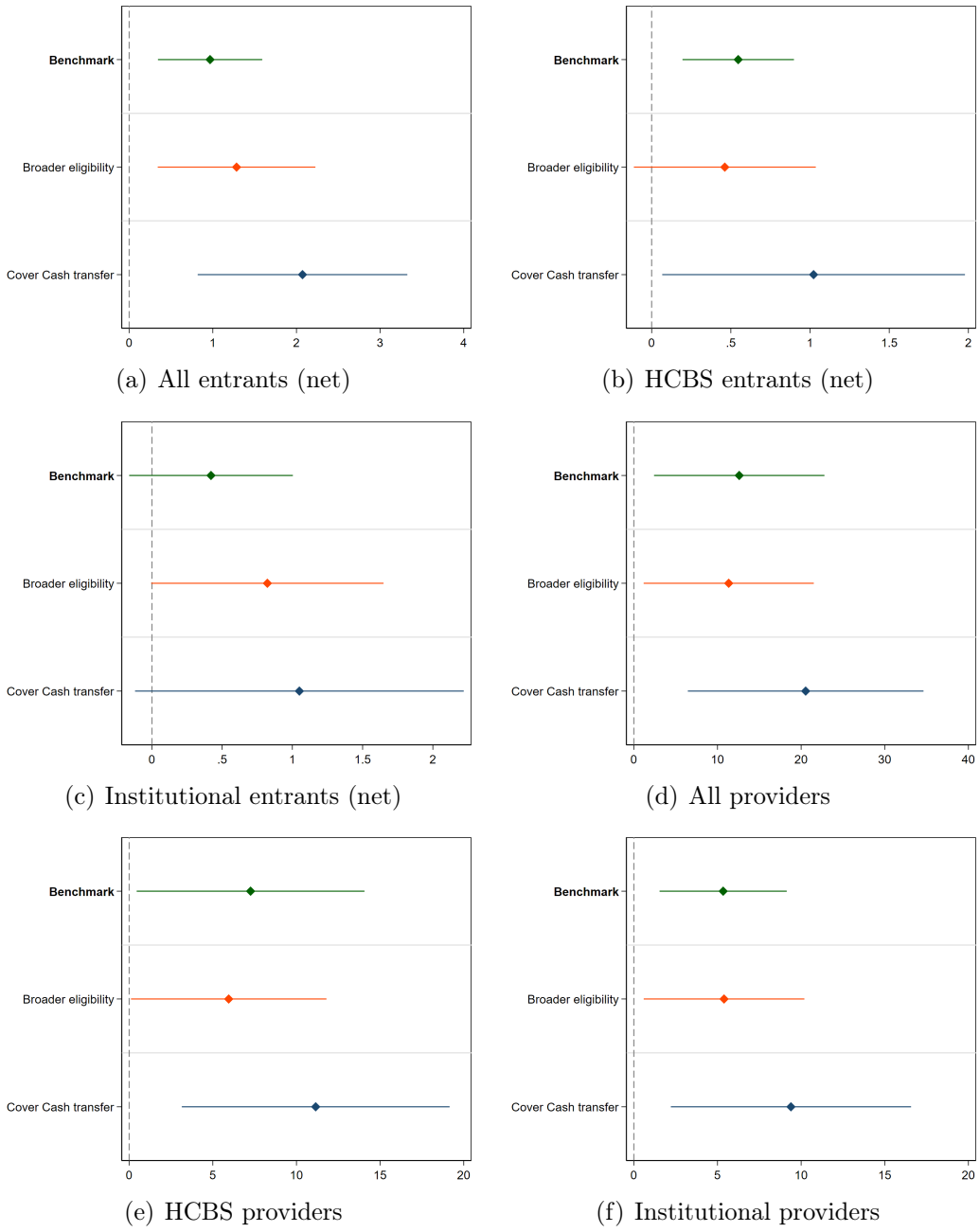
(d) All providers

(e) HCBS providers

(f) Institutional providers

Figure 5: Effect heterogeneity (providers)

Notes: This figure shows the pilot's heterogeneous effect on LTC provider entry. Data is from NECIPS (2010–2019). We divide the sample into two sub-samples with above- and below-median socioeconomic characteristics prior to the pilot: average wage, GDP per capita, and the share of the population aged 65 and above. In all panels, the dots represent the point estimates of the impact of LTCI on the respective outcomes, and the dashed lines show the 95% confidence intervals calculated from standard errors clustered at the city level. Among them, the blue dashed lines represent the group with higher indicators, and the orange dashed lines represent the group with lower indicators.



(a) All entrants (net)

(b) HCBS entrants (net)

(c) Institutional entrants (net)

(d) All providers

(e) HCBS providers

(f) Institutional providers

Figure 6: Effect heterogeneity, by policy design, providers

Notes: This figure shows the estimates of the treatment effect using Callaway and Sant'Anna (2021)'s DID estimator. Data is from NECIPS (2010–2019). The heterogeneity analysis is conducted by categorizing pilot cities based on specific variations in policy implementation: (i) all cities; (ii) cities with broader eligibility; (iii) cities providing cash transfer for care services. In all panels, the dots represent the point estimates of the impact of LTCI on the respective outcomes, and the dashed lines show the 95% confidence intervals calculated from standard errors clustered at the city level.

In the model, an elderly individual's need for long-term care depends on a set of observed need shifters, z_i, including age and gender. These shifters affect the relative utility of the outside option ($j = 0$) and the inside options ($j = 1, 2$). We specify that a vector of demographic and socio-economic characteristics, x_i, affects the utility derived from informal and formal care differently. This vector includes income, coresidence with children. Lastly, we also allow the density of LTC providers (denoted by N_c) to affect utility from formal care but not that from informal care.

To capture how demand-side improvements affect individual care choices, we include an indicator for individual i's eligibility for LTCI benefits for care type j, denoted by $LTCI^*_{ij}$.[26] The model allows for differential responses to cost-sharing benefits for informal care (captured by the coefficient γ_1) and formal care (captured by the coefficient γ_2). Finally, we incorporate Type-1 Extreme Value utility shocks, ε_{ij}, which are assumed to be independent across individuals, choices, and years.[27]

6.1.1. Unobserved Heterogeneity

The model also accounts for unobserved heterogeneity along two dimensions. First, we include year fixed effects to absorb aggregate national shocks and common time trends affecting all cities, and city fixed effects to control for time-invariant local characteristics. Second, we allow for individual-specific demand shocks that are unobserved to the econometrician, denoted by $v_i \sim \mathcal{N}(0, \sigma_v^2)$.

6.1.2. Choice Probabilities

Let U_{ij} denote the indirect utility of an old-age individual i in city c from the three options, taking the following form:

$$U_{ij} = \begin{cases} \varepsilon_{i0} & \text{if } j = 0 \\ \underbrace{z_i\beta + x_i'\beta_1 + \gamma_1 LTCI^*_{ij} + v_i}_{u_{i1}} + \varepsilon_{i1} & \text{if } j = 1 \\ \underbrace{z_i\beta + x_i'\beta_2 + \gamma_2 LTCI^*_{ij} + \theta N_c + v_i}_{u_{i2}} + \varepsilon_{i2} & \text{if } j = 2 \end{cases} \tag{2}$$

Under the distributional assumption on ε_{ij} and conditional on the realization of v_i, the

[26] The indicator $LTCI^*_{ij}$ equals one if the city where individual i resides has implemented LTCI and offers cost sharing for care service j, and if individual i is eligible for LTCI benefits.

[27] Following the convention in the discrete-choice literature, the scale parameter of the ε_{ij} distribution is normalized to one (Train, 2009).

probability of individual i choosing option j is given by the logit formula

$$p_{ij}(x_i, z_i, LTCI^*_{ij}, N_c; v_i) = \frac{\exp(u_{ij})}{1 + \sum_{j'=1,2} \exp(u_{ij'})}. \tag{3}$$

and the ex-ante choice probability is

$$p_{ij}(x_i, z_i, LTCI^*_{ij}, N_c, \sigma_v) = \int_v p_{ij}(x_i, z_i, LTCI^*_{ij}, N_c; v) f(v; \sigma_v) dv,$$

where $f(v; \sigma_v)$ is the probability density function of $\mathcal{N}(0, \sigma_v^2)$.

Note that although the option-specific insurance benefit $LTCI^*_{ij}$ enters the utility of option j, Equation (3) makes clear that the choice probability p_{ij} depends on the availability of insurance benefits across *all* care options.

6.2. Marginal Effects in Binary Comparison

To illustrate how long-term care demand responds to LTCI benefits and to expansions in long-term care supply, we quantify two marginal effects that capture the change in probability driven by different variables.

Definition 1 (Marginal effect of LTCI on elderly choices)**.** This measures the shift in the probability of selecting care option j when an individual becomes eligible for LTCI benefits. It captures the demand-side response driven by the change in LTCI benefit availability:

$$\delta_j^{LTCI} = \int \left[p_{ij}(x_i, z_i, LTCI^*_{ij} = 1, N_c; v_i) - p_{ij}(x_i, z_i, LTCI^*_{ij} = 0, N_c; v_i) \right] f(v_i; \sigma_v) dv_i,$$

where F_i denotes the distribution of individuals over which the marginal effect is evaluated.

Definition 2 (Marginal effect of supply expansion on elderly choices)**.** This measures the marginal shift in the probability of selecting care option j induced by a continuous increase in local provider density. It captures the supply-side response driven by the change in provider density:

$$\delta_j^{N_c} = \int \frac{\partial p_{ij}(x_i, z_i, LTCI^*_{ij}, N_c; v_i)}{\partial N_c} f(v_i; \sigma_v) dv_i$$

6.3. Identification Strategy: A Control Function Approach

To estimate the effect of provider density on individual care choices while accounting for endogenous provider entry, we adopt a control function approach within our structural discrete

choice model. A primary identification concern is that unobserved city-level demand shocks may simultaneously drive both provider entry and individual formal care utilization.

We address this concern with a control-function approach as in Crawford et al. (2018) and Cuesta and Sepúlveda (2021). This control function approach builds on Petrin and Train (2010) and its recent extension to endogenous entry settings by Aguirregabiria et al. (2025), and exploits supply shifters that are plausibly orthogonal to demand shocks (after we control for city fixed effects and year fixed effects).

6.3.1. Control Function Specification

We exploit heterogeneous provider entry responses documented in Section 5.4, which arise from differences in pre-pilot market conditions. We interact these supply shifters with the LTCI pilot indicator.[28] The resulting control function captures variation in provider counts across markets and over time that is orthogonal to transitory demand shocks. This approach parallels the identification strategy in Finkelstein (2007) and is very similar to the instrumental variable design in Duflo (2001).[29]

Our key explanatory variable for provider entry is the interaction between $LTCI_{ct}$—an indicator for LTCI implementation in city c and year t—and Z_c, a vector of pre-pilot market characteristics measured in 2010. These characteristics include the share of the population aged 60 and above, total population, and the log number of hospitals. Identification relies on differential supply responses to the policy shock across markets with heterogeneous pre-pilot conditions. Consistent with the reduced-form evidence, LTCI implementation generates significantly larger supply expansions in markets with greater growth potential.

We therefore estimate a fixed-effects regression of provider counts N_{ct} on the interaction $LTCI_{ct} \times Z_c$, controlling for city and year fixed effects, and recover the residual $\hat{N}_{ct}$. This residual captures variation in provider entry that is not explained by exogenous supply shifters. Following the control function literature, we interpret $\hat{N}_{ct}$ as a proxy for endogenous supply variation, which we include as a control in the main estimating equation.

6.3.2. Correction for Endogeneity

In the main estimating equation, we address endogeneity by introducing a control function into the individual utility specification. Specifically, we include the estimated residual, $\hat{N}_{ct}$,

[28] The CLHLS is observed in three waves—2011, 2014, and 2018. Only the 2018 wave occurs after the pilot's introduction, so the design does not involve staggered treatment timing.

[29] Finkelstein (2007) study the effects of Medicare by comparing states with different pre-Medicare private insurance coverage rates before and after Medicare's introduction. Using Indonesia's large-scale school construction program, Duflo (2001) identify returns to education by instrumenting years of schooling with regional variation in school construction interacted with cohort exposure induced by program timing.

as an additional regressor in the utility function of the discrete choice model. With this correction, the deterministic part of utility for formal care in choice probability Equation (3) is given by $u_{i2} = z_i\beta + x_i'\beta_2 + \gamma_2 LTCI_{ij}^* + \theta N_c + \theta'\hat{N}_c + v_i$. Petrin and Train (2010) shows, after controlling for variation in market structure that is not explained exogenous supply shifters, the coefficient on N_c can be consistently estimated.

6.4. Results

The full set of coefficient estimates is reported in Online Appendix A. In this subsection, we characterize the effects of the LTCI pilot by computing individual-level differences in marginal effects between formal care and the two alternative options—informal care and no care. These binary comparisons allow us to assess whether demand- and supply-side improvements induced by LTCI primarily reallocate individuals from informal care arrangements or draw new users from the outside option.

Results are summarized in Table 5. For the demand-side effect of LTCI eligibility, we report $\delta_2^{LTCI} - \delta_1^{LTCI}$ and $\delta_2^{LTCI} - \delta_0^{LTCI}$. A positive value indicates that LTCI eligibility increases the probability of choosing formal care relative to the alternative option. Analogously, for the supply-side channel, we compute $\delta_2^{N_c} - \delta_1^{N_c}$ and $\delta_2^{N_c} - \delta_0^{N_c}$, capturing the differential response of formal care demand to an expansion in provider density.

The results show that LTCI diverts elderly individuals away from both informal care and no care, with slightly larger effects on informal care along both the demand and supply margins. In contrast, subsidies for informal care under LTCI exhibit limited diversion effects, consistent with modest behavioral responses along this margin. Finally, the magnitude of the effects increases with age, indicating stronger responses among older individuals.

7. Conclusion

Addressing the growing imbalance between the supply and demand for LTC services amid rapid population aging has become a global challenge. While LTCI is the most widely adopted policy response, there is limited evidence on whether—and to what extent—public insurance expands the market provision of formal care.

This paper fills this gap by studying China's LTCI pilot program from 2016 to 2019. Exploiting temporal and geographic variation from the staggered rollout of the program, we find that LTCI increases the quarterly number of LTC provider entrants by 245 percent and raises the LTC employment share by 120 percent. We further estimate a model of elderly individuals' choice between informal and formal care. The results indicate that LTCI-induced

Table 5: Demand- and Supply-Side Effects of LTCI on Care Choices

	$\delta_2 - \delta_1$	$\delta_2 - \delta_0$
LTCI eligibility	0.034	0.026
	(0.016)	(0.012)
LTCI informal care benefit	-0.005	-0.000
	(0.032)	(0.001)
Provider density (N_c)	0.068	0.050
	(0.040)	(0.030)

Notes: This table reports average differences in individual-level marginal effects derived from the structural discrete choice model. $\delta_2 - \delta_1$ compares the marginal effect on the probability of choosing formal care ($j = 2$) relative to informal care ($j = 1$), while $\delta_2 - \delta_0$ compares formal care to the outside option ($j = 0$). The first two rows correspond to demand-side effects driven by LTCI eligibility and informal care benefits, respectively. The third row reports supply-side effects associated with a marginal increase in provider density (N_c). Robust standard errors are reported in parentheses and are clustered at the city level. The sample consists of elderly individuals from the CLHLS (2011, 2014, and 2018).

supply expansion and improved affordability increase the aggregate use of formal care among eligible beneficiaries. Importantly, LTCI benefits for informal care do not discourage entry by formal care providers.

The effects of LTCI are heterogeneous across cities. Supply expansion is larger in cities with characteristics indicative of greater LTC market size, including higher GDP per capita, higher average wages, and a larger share of the population aged 65 and above. We also examine whether policy design features—such as broader eligibility or subsidies for *informal* care—generate differential supply responses. While broader eligibility further incentivizes market entry, subsidies for *informal* care do not deter the entry of formal care providers, contrasting with findings in the healthcare literature. We provide evidence for two mechanisms underlying this null spillover effect: (1) elderly individuals exhibit limited responsiveness to informal care benefits, and (2) formal care supply expansion does not induce those relying on informal care to switch to formal care.

Our findings suggest several promising directions for future research. First, the magnitude of supply-side effects we document raises questions about whether similar mechanisms operate in other national contexts. Many countries established LTCI systems in the 1990s and early 2000s. Since our demand-side estimates align with prior evidence from these settings, it is important to examine whether comparable supply-side effects also emerged, or whether distinctive features of China's system account for its unique impacts. Second, because China's LTCI policy is relatively recent, our results should be viewed as short-term effects. These findings are especially relevant for policymakers seeking to stimulate the care market and address the immediate needs of an aging population. The long-run effects, however, may

differ: while demographic aging is likely to sustain demand growth and potentially induce further supply expansion, rising dependency ratios also raise the risk of under-provision of LTC practitioners, which could cause the effects to taper off. Understanding how these demographic dynamics shape both the demand- and supply-side impacts of LTCI at the aggregate level remains an important direction for future research.

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Appendix A. Sample Construction for LTC Provider Data

A.1. Textual Information in Operating Business

To isolate LTC providers in our dataset, we first select firms that meet both of the following criteria: (1) inclusion of terms "elderly care" (养老) and "care/nursing" (护理) in operating business descriptions; (2) being regulated by the "Industrial and Commercial Bureau" (工商行政管理局).

A.2. Industry Codes in the National Bureau of Statistics of China (2020)'s Guidelines

Our selection process begins by identifying all establishments under the Chinese Industry Classification (CIC) codes categorized as elderly care-related industries, following the National Bureau of Statistics of China (2020) guidelines. The guidelines outline twelve major categories comprising 103 four-digit CIC (GB/T 4754-2017) industries.

It is important to note that the NBS's classification extends beyond the coverage scope of the public LTCI. We retain only LTC-related industries, excluding entertainment, pension and insurance, public welfare, financial services, scientific research and technical services, media, legal assistance, construction, and miscellaneous industries.

The CIC classification codes for the two types of providers in the guidelines are summarized as follows.

Home- and community-based service (HCBS) providers are associated with codes:

- 8010 (Home Services 家庭服务)
- 8219 (Other Cleaning Services 其他清洁服务)
- 8521 (Social Care and Assistance Services 社会看护与帮助服务)
- 6299 (Other Unspecified Food Services 其他未列明餐饮业)
- 8090 (Other Residential Services 其他居民服务业)
- 8521 (Social Care and Assistance Services 社会看护与帮助服务)

Institutional care providers are associated with codes:

- 8416 (Sanatoriums 疗养院)
- 8511 (Cadre Rest Homes 干部休养所)
- 8512 (Nursing Home Services 护理机构服务)
- 8514 (Elderly and Disabled Care Services 老年人、残疾人养护服务)

After applying the initial screening criterion based on textual information, there are no observations with CIC code 8090, as it is not closely related to the core business of LTC.

A.3. Crosswalk between NECIPS and the NBS's Guidelines by CIC Code

NECIPS records only the first three of the four-digit CIC (GB/T 4754-2017) code. We select LTC providers based on the three-digit CIC codes. Fourteen codes are retained: 624, 801, 809, 821, 621, 622, 629, 851, 852, 841, 724, 842, 843, and 849.

A.4. LTC Provider Classification

The last step of sample construction is to categorize LTC providers into different categories. NBS specifies three categories in its 2020 guidelines: home-, community-, and/or institutional-based providers using three-digit CIC codes.

- Home-based LTC providers are associated with codes 624, 801, 809, 821, or 852.
- Community-based LTC providers are associated with codes 621, 622, 624, 629, 809, 851, or 852.
- Institutional LTC providers are associated with codes 841 or 851.

Appendix B. Details on the Theoretical Model

B.1. Model Setup

B.1.1. Demand

In a market with M elderly individuals who have the potential need for long-term care, each faces three options: not receiving any care, getting informal care from family members, and purchasing formal care services, indexed by $j = 0, 1, 2$, respectively. Formal care services is purchased from the market at an administered price p under a reimbursement rate r (i.e., paying $(1 - r)p$ out of pocket).[30]

The *indirect* utility individual i derives from options $j = 1, 2$ is specified as $U_{ij} = u_j(N, p, r) + \varepsilon_{ij}$, where N is the number of LTC providers in the market and ε_i is a utility shock identically distributed following $f(\varepsilon)$ and independent across individuals. The utility from not receiving any care is normalized to 0, following the discrete choice literature convention (Train, 2009).

The specification that the number of LTC providers enters individual utility captures how a greater number of providers makes an individual more likely to choose formal care. This relationship can be micro-founded using a model in which providers are probabilistically available to serve elderly individuals. Alternatively, it can be derived from a model where providers are geographically differentiated, with N representing the gain in available varieties of LTC providers accessible to individual i.[31] Consequently, the term associated with N in the indirect utility equation accommodates explanations where a larger number of providers enhances utility or convenience for elderly individuals, or increases the likelihood of choosing formal care.

The market demand for LTC is proportional to the share of individuals who attain higher

[30] The cost of informal care can be interpreted as the opportunity cost of family caregivers' time, primarily due to reduced work hours (Chari et al., 2014; Mommaerts, 2025). In pilot cities that provide benefits for informal care, the out-of-pocket cost is effectively the difference between forgone wages and the LTCI subsidy.

[31] Appendix B provides an analytical example for each microfoundation: (i) a rationing model where provider availability is probabilistic, and (ii) providers are differentiated and N captures local provider density. Both approaches yield the reduced-form relationship between N and care utilization.

utility from LTC facilities:

$$\begin{aligned} D(N,p,r) =& M \times \Bigg\{ P(u_0 > u_1 + \varepsilon_1) \int_{u_2+\varepsilon_2>u_0} f(\varepsilon_2) d\varepsilon_2 \\ &+ P(u_0 \leq u_1 + \varepsilon_1) \int_{u_2+\varepsilon_2>u_1+\varepsilon_1} f(\varepsilon_1) f(\varepsilon_2) d\varepsilon \Bigg\} \\ =& M \times \Bigg\{ \int_{\varepsilon_1<u_0-u_1} \int_{\varepsilon_2>u_0-u_2} f(\varepsilon_1) f(\varepsilon_2) d\varepsilon_2 d\varepsilon_1 \\ &+ \int_{\varepsilon_1\geq u_0-u_1} \int_{\varepsilon_2>u_1+\varepsilon_1-u_2} f(\varepsilon_1) f(\varepsilon_2) d\varepsilon_2 d\varepsilon_1 \Bigg\} \end{aligned} \tag{4}$$

Example 1 (Variety/choice set expansion): Assuming the indirect utility that individual i receives from choosing provider j in the formal care market is $u_{ij} = u_{i2} + \varepsilon_{ij}$ Providers may deliver same utility (e.g., standardized service) and are differentiated by the utility shock ε_{ij}. The ex ante inclusive value of choosing formal care is $U_{i2} = \log\left(\sum_j \exp(u_{ij})\right) = \log(N) + u_{i2}$ with N reflecting local provider density. Similar specifications appear in studies of sales force deployment (Hastings et al., 2017), movie show time allocation (Hodgson and Sun, 2022; Chen et al., 2024), and insurance network breadth (Serna, 2024).

Suppose there are three options:

$$\begin{aligned} U_{i0} &= \delta_0 + \lambda_{i0} + u_{i0}, \\ U_{i1} &= \delta_1 + \lambda_{i1} + u_{i1}, \\ U_{i2} &= \max_{A_2 \in \{1,\ldots N\}} \{U_{i2,A_2} = \delta_2 + \lambda_{i2} + u_{i2,A_2}\}. \end{aligned}$$

where $(\delta_0, \delta_1, \delta_2)$ are mean utilities from the three options, and $(\lambda_{i0}, \lambda_{i1}, \lambda_{i3})$ are individual-specific utility from option j (e.g., individuals with worse health status benefit more from option 2), and $(u_{i0}, u_{i1}, u_{i2,A_2})$ are i.i.d. Type I extreme value draws.

Given this indirect utility specification, the probability that i chooses option j is

$$\Pr(i \text{ chooses } j) = \frac{N \exp(\delta_2 + \lambda_{i2})}{\exp(\delta_0 + \lambda_{i0}) + \exp(\delta_1 + \lambda_{i1}) + N \exp(\delta_2 + \lambda_{i2})}.$$

Hastings et al. (2017) rewrite it as

$$\Pr(i \text{ chooses } j) = \frac{\exp(\lambda_{i2} + \delta_2(N))}{\exp(\delta_0 + \lambda_{i0}) + \exp(\delta_1 + \lambda_{i1}) + \exp(\lambda_{i2} + \delta_2(N))},$$

which yields an observationally equivalent indirect utility representation:

$$U_{i2} = \lambda_{i2} + \delta_2(N).$$

Example 2 (Rationing): Suppose each provider j can serve individual i with probability $\phi(N)$. The ex ante inclusive value of choosing any provider is $U_{i2} = \log\left(\sum_j \phi(N)\exp(u_{ij})\right)$. When $u_{ij} = u_i$ for all j, this simplifies to $U_{i2} = u_i + \log(N\phi(N))$. This approach follows the literature on incomplete product availability (Conlon and Mortimer, 2013; Goeree, 2008). Note that U_{i2} increases in N when $\phi'(N) > 0$.

B.1.2. Supply

On the supply (LTC provider) side, we assume, without loss of generality, that providers are symmetric and face zero marginal costs for serving an individual. Under these assumptions, each firm's profit is a function of the price of formal care p, reimbursement rate r, and the cost for a potential entrant to enter the market $c(N)$,

$$\pi(N; p, r) = \frac{pD(N, p, r)}{N} - c(N). \tag{5}$$

B.2. Assumptions on LTC Demand and Entry Cost

We make the following assumptions:

Assumption 1: Firms in a market are symmetric.

Assumption 2: $0 \leq c'(N) < \infty$.

Assumption 3: $\frac{\partial u}{\partial p} < 0$, $\frac{\partial u}{\partial N} > 0$, $\frac{\partial u}{\partial N}\big|_{N=0} = \infty$, $\frac{\partial u}{\partial r} > 0$.

These assumptions provide conditions under which the equilibrium profit curve under regulated prices and the entry cost curve have a unique intersection (as plotted in Figure 1(a)). Assumption 1 implies that at the regulated price p, each of the N firms has a profit of $pD(N, p, r)/N$. Assumption 2 implies that the cost of entry is finite and weakly increasing as the market becomes more saturated (i.e., a higher number of incumbent firms). Lastly, Assumption 3 implies that individuals dislike higher prices or lower reimbursement levels, and having more providers in the market benefits individuals. When there are no formal care providers in the market, the marginal utility of having one is infinite.

B.3. Public Insurance, Provider Entry, and Equilibrium LTC Demand

We make mild assumptions about individual utility and entry costs: consumers prefer lower prices, higher reimbursement rates, and more providers in the market. Additionally, we assume that marginal utility is very high when there are no LTC providers and entry costs

increase with the number of providers in the market. In Appendix B.5, we formulate these assumptions mathematically and prove $\frac{\partial D}{\partial N}|_{N=0} = \infty$, $\frac{\partial D}{\partial N} > 0$ and $\frac{\partial^2 D}{\partial N^2} < 0$. Figure 1(a) illustrates how demand $D(N, p, r)$ varies with N.

Under the assumptions outlined above, potential entrants enter the market until the profit in Equation (5) falls below the entry cost for the marginal entrant (i.e., the free-entry condition). The equilibrium number of providers N^* is determined accordingly by $pD(N^*, p, r)/N^* = c(N^*)$. Equivalently,

$$D(N^*, p, r) = \frac{N^* c(N^*)}{p}. \tag{6}$$

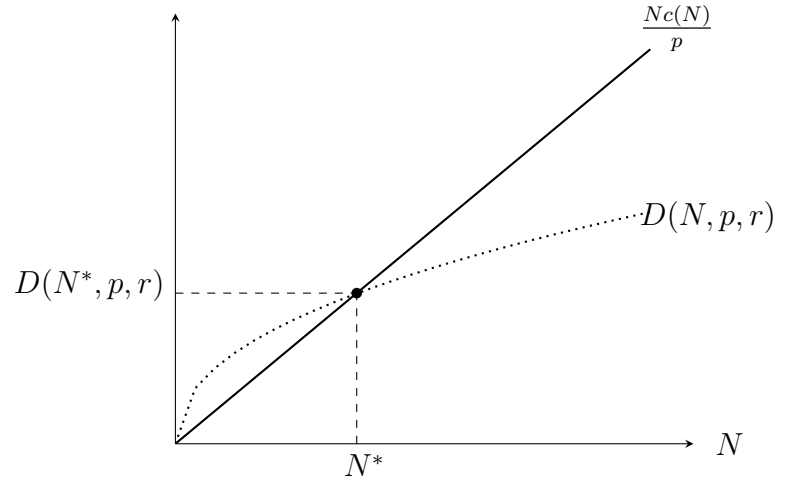
Figure 1(a) plots how the right-hand side term of Equation (6) varies with N. The intersection of the two curves in Figure 1(a) characterizes the equilibrium number of firms N^* and the market demand $D(N^*, p, r)$.

B.4. The Effect of an LTCI Pilot

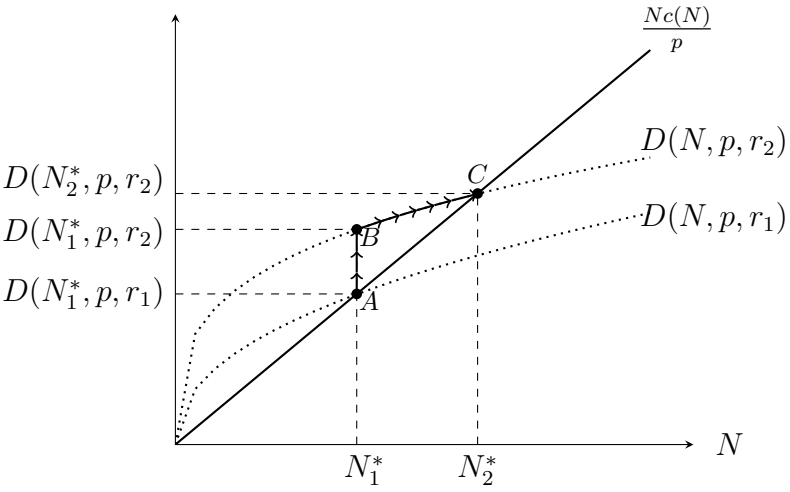
This simple model helps illustrate the potential effects of an LTCI pilot on market structure and equilibrium elderly LTC demand. In the context of administered price, we model LTCI provision as increasing the reimbursement rate from $r_1 = 0$ to $r_2 > 0$, consistent with the institutional background of China's LTCI as described in Section 2.2. The main takeaways are shown in Figure 1(b). Additional proofs and derivations are presented in Appendix B.5.

The original equilibrium under $r_1 = 0$ depicted in Figure 1(a) is replicated in Figure 1(b). Upon raising the reimbursement rate to r_2, formal care demand is higher for any number of providers N. In other words, the demand curve shifts from $D(N, p, r_1)$ to $D(N, p, r_2)$. In the short run, with the market structure fixed, the market transitions from the initial state A $(N_1^*, D(N_1^*, p, r_1))$ to an interim state B $(N_1^*, D(N_1^*, p, r_2))$. This initial expansion on the demand side generates profits that exceed entry costs, thereby attracting new entrants until the free-entry condition (Equation (6)) is satisfied. Subsequently, the market evolves from the interim state B $(N_1^*, D(N_1^*, p, r_2))$ to a final state C $(N_1^*, D(N_2^*, p, r_2))$.

Furthermore, the elasticity of demand with respect to the reimbursement rate is positively associated with supply expansion. In markets where the elasticity is high (e.g., larger market size, larger fraction of the elderly population), we can expect a greater response to public LTCI. In summary, expansion on the demand side leads to a corresponding expansion on the supply side, enhancing individual access to LTC facilities and increasing LTC demand in equilibrium.



(a) Equilibrium market demand and number of providers



(b) Comparative statics

Figure B1: Effects of LTCI expansion on formal care provider entry and market demand for LTC

B.5. Propositions and Proofs

Based on the above assumptions, we prove two propositions on the slope and curvature of the demand curve. In Proposition 1, we first show that demand is increasing in the number of LTC providers in the market. In Proposition 2, we show that the demand-expansion effect of new LTC providers is smaller when there are more incumbent providers.

Proposition 1: Demand is increasing in the number of incumbent providers

$$\frac{\partial D}{\partial N} > 0.$$

Proof: by Leibniz Integral Rule, the first and second derivatives of market demand can be expressed as

$$\begin{aligned}\frac{\partial D(N,p,r)}{\partial N} =& M \times \left\{ \int_{\varepsilon_1 < u_0 - u_1} f(\varepsilon_1) f(u_0 - u_2) u_2'(N) d\varepsilon_1 \right. \\ &\left. + \int_{\varepsilon_1 \geq u_0 - u_1} f(\varepsilon_1) f(u_1 + \varepsilon_1 - u_2) u_2'(N) d\varepsilon_1 \right\} > 0\end{aligned}$$

Proposition 2: Under mild regularity conditions, the rate of demand increase is lower as a market accommodates more incumbent firms

$$\frac{\partial^2 D}{\partial N^2} < 0.$$

Proof:

$$\begin{aligned}\frac{\partial^2 D(N,p,r)}{\partial N^2} =& MT \times \left\{ \int_{\varepsilon_1 < u_0 - u_1} f(\varepsilon_1)[-f'(u_0 - u_2)(u_2'(N))^2 + u_2''(N) f(u_0 - u_2)] d\varepsilon_1 \right. \\ &\left. + \int_{\varepsilon_1 \geq u_0 - u_1} f(\varepsilon_1)[-f(u_1 + \varepsilon_1 - u_2)(u_2'(N))^2 + u_2''(N) f(u_1 + \varepsilon_1 - u_2)] d\varepsilon_1 \right\}.\end{aligned}$$

The sign of $\frac{\partial^2 D(N,p,r)}{\partial N^2}$ is indeterminate, depending on the sign of $f'(u_0 - u_2)$. Nevertheless, $\frac{\partial^2 D(N,p,r)}{\partial N^2} < 0$ if and only if

$$\frac{f'(u_0 - u_2)}{f(u_0 - u_2)} > \frac{u_2''(N)}{(u_2'(N))^2},$$

which holds for canonical discrete-choice demand models in empirical applications where $\frac{\partial^2 u}{\partial N^2} < 0$ and $f(\cdot)$ is the probability density function of the type-I extreme value distribution.

Appendix C. Institutional Background

C.1. Timing of Pilot Rollout

Table C1 reports the rollout timing among pilot cities.

Table C1: Timing of city-level LTCI implementation

Province	City	Implement date	Province	City	Implement date
Anhui	Anqing	2017/1	Jilin	Tonghua	2017/9
Guangdong	Guangzhou	2017/8		Meihekou	2017/9
Hebei	Chengde	2017/7		Changchun	2015/5
Heilongjiang	Qiqihaer	2017/10		Songyuan	2016/6
Hubei	Jingmen	2016/11		Jilin	2016/11
Jiangxi	Shangrao	2016/11		Huichun	2017/12
Shanghai	Shanghai	2017/1	Jiangsu	Suzhou	2017/6
Sichuan	Chengdu	2017/7		Nantong	2016/1
Chongqing	Chongqing	2017/12	Xinjiang	Shihezi	2017/1
Shandong	Jinan	2016/6		Urumqi	2019/7
	Qingdao	2012/7	Zhejiang	Ningbo	2017/9
	Zibo	2018/1	Beijing	Shijingshan	2020/10
	Zaozhuang	2018/6	Tianjin	Tianjin	2020/12
	Heze	2018/5	Shanxi	Jincheng	2021/2
	Dongying	2014/1	Inner Mongolia	Hohhot	2021/4
	Yantai	2018/6	Liaoning	Panjin	2020/12
	Weifang	2015/1	Fujian	Fuzhou	2020/12
	Jining	2018/1	Henan	Kaifeng	2020/12
	Taian	2017/11	Hunan	Xiangtan	2020/12
	Weihai	2018.7	Guangxi	Nanning	2021/3
	Rizhao	2017/12	Guizhou	Qianxinan	2020/11
	Linyi	2020/1	Yunnan	Kunming	2020/12
	Dezhou	2018/12	Shaanxi	Hanzhong	2021/1
	Liaocheng	2017/10	Gansu	Gannan	2020/12
	Binzhou	2017/12			

Notes: The city rollout data for the LTCI implementation are from the authors' collection of various policy documents released by the governments. Qingdao, Dongying, and Weifang initiated their own LTCI programs prior to the 2016 pilot and are therefore excluded from the treatment group in our research design.

C.2. More Background on LTCI

C.2.1. Premium Contribution

The *annual* LTCI premium (equivalent to 1.57–9.09 times the provincial *hourly* minimum wage in 2015) was relatively affordable, either as flat annual premiums (e.g., 30 to 90 Chinese Yuan) or as a proportion of annual salary (0.3% to 1%). Contribution schemes vary across pilot cities.

- Jilin City: Individuals pay 10 CNY annually, supplemented by a 5:1 government subsidy, for a total of about 12 CNY in annual funding.

- Tonghua, Songyuan, and Meihekou: Individuals pay 10 CNY annually.
- Shanghai: Contributions are set at 0.1% of the employee's monthly UEBMI base (e.g., 8 CNY for a base of 8,000 CNY).
- Qingdao: The 2024 contribution base floor is 8,847 CNY, with both employer and employee contributing 0.1%, yielding about 18 CNY monthly.
- Shenzhen: a 0.1% rate is applied but caps the contribution base at 26,541 CNY, such that employers pay up to 26.5 CNY monthly.
- Hangzhou: UEBMI enrollees contribute 90 CNY per year (shared equally between individuals and employers), while URRBMI enrollees pay 30 CNY annually with an additional 60 CNY government subsidy.
- Jingmen: Individuals pay 36 CNY annually, co-funded by the medical insurance fund (37.5%) and government subsidies (37.5%).
- Chongqing: A flat 70 CNY annual premium is levied.
- Hanzhong: UEBMI participants pay 100 CNY annually (50 CNY from individuals, 30 CNY from employers, and 20 CNY from government subsidies).

C.3. LTCI Reimbursement Models, Pricing Regulation, and Subsidy Standards Across Pilot Cities

We first report cities that use the three models of reimbursement and summarize their corresponding generosity levels based on reimbursement rates (fee-for-service), per-diem subsidy standards, and capitation-based monthly payments.

- **Fee-for-service**: Shanghai (HCBS 90%), Qingdao (UEBMI 90%; URRBMI 75–80%), Jinan (UEBMI 90%; URRBMI 70%), Liaocheng (UEBMI 90%), Yantai (90%), Nantong (100%), Jingmen (Level I 80%; II 50%; III 40%), Anqing (70%), Tianjin (75%), Fuzhou (85%), Jincheng (self-care 100%; home-visit 70%), Kaifeng (75%), Nanning, Chengde (70%), Linyi, Qiqihar (50%), Guangzhou (up to 90% by level), Changchun (UEBMI ∼90%; URRBMI ∼80%), Ningbo (97%).
- **Per-diem**: Shanghai (institutional 85%), Chongqing (fixed 50/day for home-visit), Qingdao (bed-day 65; special care 180–210/day), Jinan (general 70/day; special care 220–260/day), Liaocheng (general 60/day; special 120/day), Nantong (institutional 40–70/day depending on severity), Jingmen (nursing up to 100/day; hospital 150/day), Anqing (50/day; medical 60/day), Tianjin (70/day), Jincheng (100/day), Nanning (bed-day 57–66 by level), Chengde (50–60/day depending on provider), Chongqing (institutional 50/day), Linyi (institutional 65/day), Qiqihar (25–30/day), Guangzhou (ceiling 120/day), Suzhou (severe 26/day; moderate 20/day).

- **Capitation**: Shangrao (self-care 450/month; home care 900/month), Shihezi (450/630/810/900 per month by level), Hanzhong (home 450/month; institutional home-visit 800/month; elderly care 1,100/month; medical 1,200/month), Chengdu (severe I 1,577/month; moderate II 660/month; moderate III 1,118/month), Kaifeng (900–1,500/month depending on care mode), Beijing–Shijingshan (1,044/month), Kunming (institutional/integrated care ceiling 2,600/month), Dezhou (UEBMI 600/month; URRBMI 400/month), Fuzhou (home care 1,800/month), Jincheng (home-visit 1,500/month).

C.4. Provider Eligibility for LTCI

Becoming a designated service institution for LTCI generally involves an application and approval process. While the specific steps may vary by region, the typical procedure includes the following key stages:

1. **Qualification Compliance:** The institution must meet all necessary qualifications for providing long-term care services, including a valid business license and any relevant industry-specific licenses.
2. **Submit Application:** An application must be submitted to the relevant local LTCI management department to request designation as an approved service institution.
3. **Service Capacity Assessment:** The institution will undergo a thorough assessment by the appropriate authorities, evaluating service capacity, facility conditions, staff qualifications, and other relevant factors.
4. **Sign Service Agreement:** Upon approval, the institution enters into a service agreement with the LTCI management department, outlining the rights and obligations of both parties.
5. **Supervision and Assessment:** After designation, the institution is subject to regular supervision and performance assessments by relevant departments to ensure compliance with service quality standards. Failure to comply may result in the revocation of its designation.

C.5. LTCI Benefits for Informal Care

Only nine pilot cities in our sample period provided benefits for informal care services. There are two types of benefits: direct cash transfers and pay-for-service. Direct cash transfers are provided to eligible families who choose to care for their elderly members at home. These transfers are typically monthly lump-sum payments, usually equivalent to 30% to 60% of the

monthly minimum wage. For example, Nantong paid 15 Chinese Yuan per day in 2016.[32] Chengdu paid 288–1,437 Chinese Yuan per month according to its 2020 guidelines.[33]

The pay-for-service program provided benefits based on the level of care required by the elderly individual. For example, Chengdu paid 360–1,796 Chinese Yuan per month to eligible elderly individuals who received care from a certified and trained family member.

There are several anti-fraud mechanisms to ensure that benefits are used for their intended purposes and to prevent abuse of the system. First, as with formal care benefits, individuals must undergo LTCI eligibility verification. This includes mandatory ADL assessments and cross-checking with medical or disability records. Furthermore, all cities used electronic application systems to enhance monitoring. For instance, Tianjin required family caregivers to upload daily care logs. Lastly, as with formal care benefits, all cities implemented random audits to ensure compliance with the program's rules and regulations.

[32]Source: https://ylbzj.nantong.gov.cn/ntsylbzj/xwfbh/content/7e621a75-deac-4745-8a4c-3d86994e47c6.html, retrieved June 8, 2025.

[33]The LTCI benefit for formal care is approximately 122% to 268% higher than informal care benefits in Chengdu. Source: https://mp.weixin.qq.com/s?__biz=MzA4NTIzNjg3Ng==&mid=2671844149&idx=2&sn=ddd47a865eba retrieved June 8, 2025.

Appendix D. Additional Empirical Results

D.1. Callaway and Sant'Anna (2021) Estimator

Define g as the first period in which a unit receives treatment (city) and t as the calendar period (quarter in the entry regressions and year in the labor market regressions). The group-time average treatment effect on the treated ($\text{ATT}(g,t)$) is estimated using inverse probability weighting:

$$\text{ATT}(g,t) = \mathbb{E}\left[\left(\frac{G_g}{\mathbb{E}[G_g]} - \frac{p_g(X)C}{(1-p_g(X))\mathbb{E}\left[\frac{p_g(X)C}{1-p_g(X)}\right]}\right)(Y_t - Y_{g-1})\right]$$

where $p_g(X)$ is the probability of being treated in cohort g, conditional on covariates X,

$$p_g(X) = P(G_g = 1 \mid X, G_g + C = 1),$$

where C is a dummy variable equal to 1 for units that are never treated, and G_g is an indicator variable equal to 1 if the unit is treated for the first time in period g.

In the regression tables and event-study plots, we estimate two different parameters that summarize the treatment effects. These estimates are constructed as $\text{ATT}(g,t)$, weighted across cohorts and time periods, following the equation:

$$\theta = \sum_g \sum_{t=2} w(g,t) \cdot \text{ATT}(g,t),$$

where $t \geq 2$, since no units are treated in the first period ($t = 1$).

The overall treatment effect is obtained by aggregating $\text{ATT}(g,t)$ across cohorts and periods. It is formally defined as:

Definition 1 (Overall Average Treatment Effect)**.**

$$\theta_W^O = \frac{1}{K}\sum_g \sum_{t=2}^{T} 1\{t \geq g\} \cdot \text{ATT}(g,t) \cdot P(G = g \mid G \leq T),$$

where $1\{t \geq g\}$ indicates whether the period t occurs after the cohort g, and $P(G = g \mid G \leq T)$ represents the probability of being in cohort g given treatment is assigned no later than T.

Definition 2. (Dynamic treatment effect) Let $e = t - g$ denote the time elapsed since

treatment. The dynamic treatment effect is defined as:

$$\theta_e^s = \sum_g 1\{g + e \leq T\} \cdot P(G = g \mid G + e \leq T) \cdot \text{ATT}(g, g + e)$$

D.2. Propensity Score Matching Augmented Estimation

D.2.1. Balance Check

To assess the validity of our identification strategy, we examine the balance of covariates between treated and control cities within the matched sample. Figure **??** illustrates the distribution of propensity scores for the treatment and control groups before and after matching. The visual evidence demonstrates a substantial improvement in the overlap of the distributions following the matching procedure, indicating that the matched sample shares common support in terms of observable characteristics.

Table D2 formally tests this balance by regressing the treatment indicator on pre-treatment city characteristics measured in 2010, controlling for border-city group fixed effects. The results indicate that the vast majority of socioeconomic and demographic variables—including per capita GDP, employee wages, education levels, and public employment shares—are not statistically significant predictors of treatment status. While the dependency rate and hospital resource metrics show statistical significance, the coefficients are generally small, and the lack of systematic differences across the broader set of economic indicators supports the validity of the border-city matched design in accounting for local confounding factors.

D.2.2. Parallel-Trend Analysis

To assess the validity of the identification strategy, we examine the parallel trends assumption using an event study specification applied to the PSM sample. This approach allows us to verify whether the treatment and control cities followed similar trajectories in provider market dynamics prior to the implementation of the LTCI pilot.

Figure D3 plots the estimated coefficients from the dynamic DID model. The horizontal axis represents the years relative to the policy implementation ($t = 0$), while the vertical axis represents the estimated impact on provider entry and market stock. Across all panels, the coefficients for the pre-treatment periods are small in magnitude and statistically indistinguishable from zero, as the 95% confidence intervals consistently include zero. This lack of differential pre-trends confirms that, conditional on the PSM matching, the pilot and non-pilot areas were comparable prior to the reform.

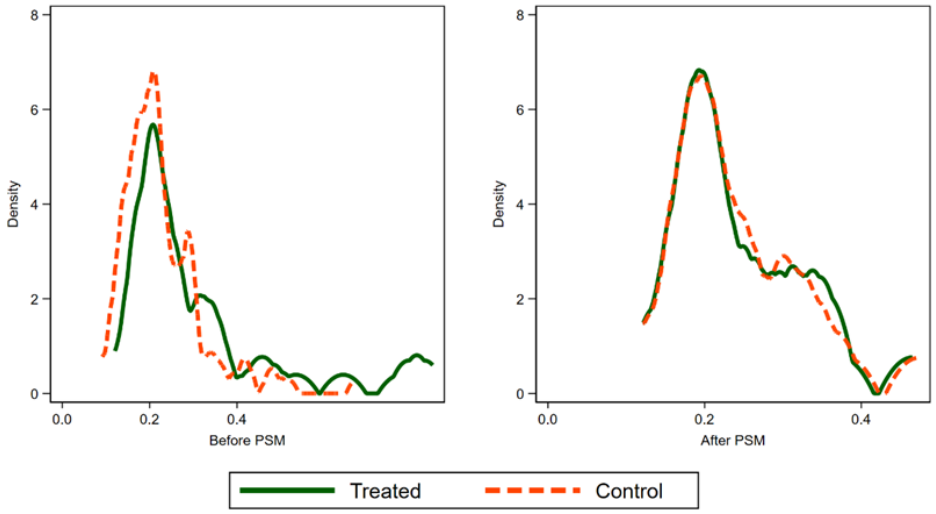


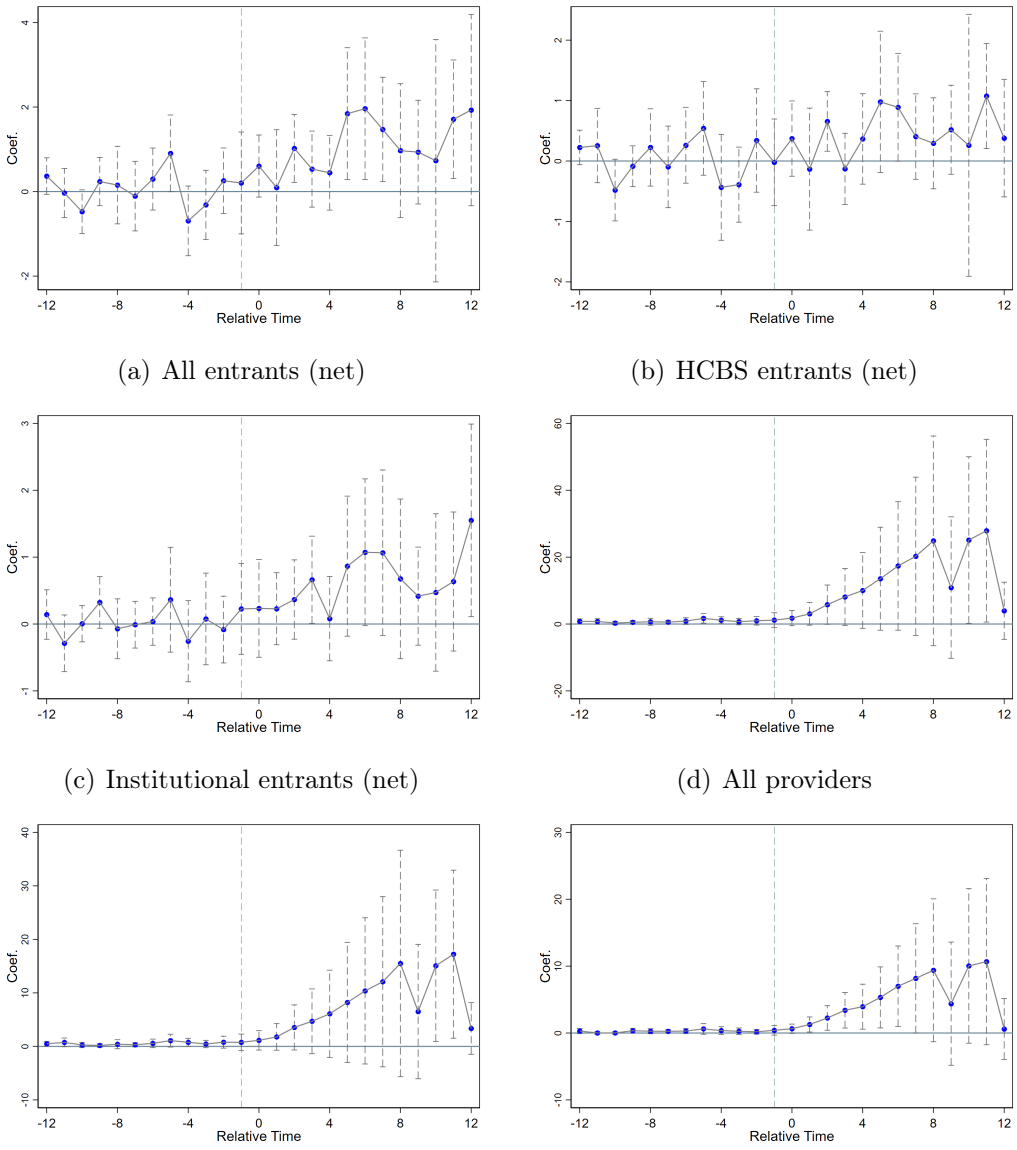
Figure D2: Balance Check of Propensity Scores Before and After Matching

Notes: This figure presents the results of the balance check for propensity score matching (PSM). The left and right panels show the distribution of propensity scores for the treated (in green line) and untreated (in orange line) group before and after the matching procedure, using board cities sample.

Table D2: Balance Test (Dependent Variable: Treat)

Variable	**Coefficient**	**Std. Error**
Gender Ratio	-0.952	(3.165)
Illiteracy Rate	0.223	(1.954)
Primary Education Rate	-1.660	(2.040)
Average Years of Education	0.188	(0.311)
Population Aged 65+ Share	1.705	(4.210)
Dependency Rate	0.320**	(0.151)
Population	0.000	(0.000)
Per Capita GDP	0.006	(0.026)
Public Employment Share	-0.002	(0.001)
Public Health Employment Share	0.095	(0.065)
Employee Wage	0.034	(0.112)
Per Capita Fiscal Expenditure	-0.101	(0.245)
Hospitals per Capita	0.034***	(0.010)
Hospital Beds per Capita	-0.001**	(0.000)
Observations	5,800	
R-squared	0.436	

Notes: This table presents the results of regressing the treatment variable on pre-treatment characteristics of cities, controlling for border city group fixed effects. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.



(a) All entrants (net)

(b) HCBS entrants (net)

(c) Institutional entrants (net)

(d) All providers

(e) HCBS providers

(f) Institutional providers

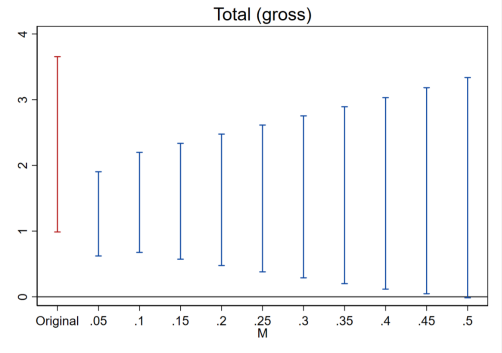
Figure D3: The event study estimates on the matched sample

Notes: This figure reports the event study coefficients and their corresponding 95% confidence intervals derived from a dynamic DID specification estimated on the PSM-matched sample. Panels represent the entry and counts of of all providers, HCBS providers, and institutional providers, respectively. The x-axis denotes the time relative to the implementation of the LTCI pilot ($t = 0$). The solid dots represent point estimates, and the vertical bars indicate 95% confidence intervals (clustered at the city level).

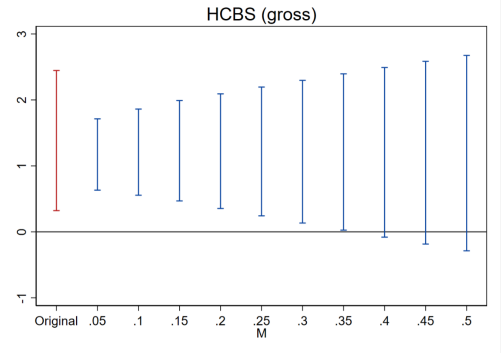
D.3. Sensitivity Check of Violations to the Parallel Trends Assumption

Rambachan and Roth (2023) introduces a method for assessing violations of the parallel trends assumption and evaluating their impact on point estimates and confidence intervals of the parameter of interest. The proposed approach involves two key steps: The first step defines Δ, the set of possible deviations from the parallel trends assumption. The second step constructs confidence intervals of the parameter of interests after accounting for these deviations in parallel trends.

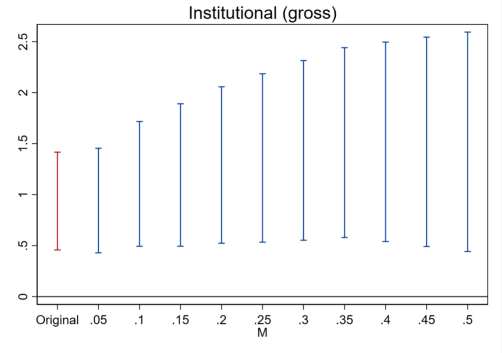
For our main parameter of interests, the overall treatment effect of the LTCI pilot reported in Table 2, we compute confidence intervals that allow for deviations from linearity up to a parameter M. The trend is represented by δ, and $\Delta^{SD}(M) = \{\delta : |(\delta_{t+1} - \delta_t) - (\delta_t - \delta_{t-1})| \leq M, \forall t\}$. The parameter M can vary from zero (indicating linear pre-trends) to half of the standard error of the coefficient of interest. The confidence intervals for the parameter θ_W^O (Definition 1) are shown in Figure D4.



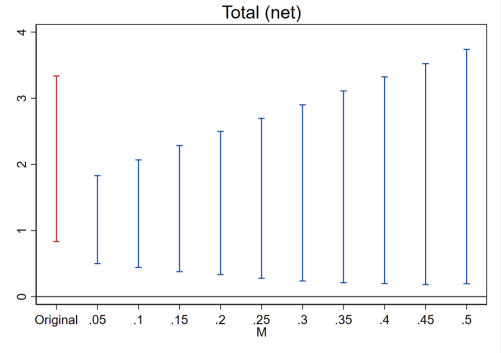
(a) All entrants (gross)



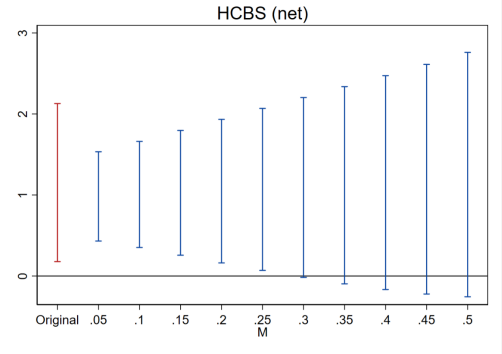
(b) HCBS entrants (gross)



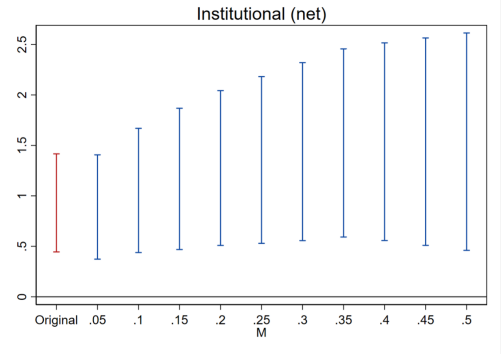
(c) Institutional entrants (gross)



(d) All entrants (net)



(e) HCBS entrants (net)



(f) Institutional entrants (net)

Figure D4: The effects of LTCI pilot on provider entry: event study estimators

Notes: This figure presents a sensitivity analysis of the confidence intervals for the overall treatment effect estimates reported in Panel B of Table 2, constructed using the methodology of Rambachan and Roth (2023). The red series corresponds to the confidence intervals reported in Table 2. The horizontal axis represents deviations from the linear parallel trend assumption, ranging from 0.05 to 0.5 standard errors of the estimated overall effect. The red series correspond to the confidence intervals constructed under these deviations.

D.4. Supplementary Tables and Figures

Table D3: Alternative Control Group Specifications

	Net entry			Providers		
	(1)	(2)	(3)	(4)	(5)	(6)
	All	HCBS	Institutional	All	HCBS	Institutional
Panel A: Benchmark						
ATT	0.967***	0.547***	0.421	12.597**	7.258**	5.339***
	(0.318)	(0.180)	(0.297)	(5.197)	(3.474)	(1.939)
N	10600	10600	10600	10600	10600	10600
Panel B: PSM						
ATT	0.951**	0.402**	0.548*	11.064	7.200	3.864
	(0.370)	(0.187)	(0.300)	(7.163)	(4.971)	(2.415)
N	2440	2440	2440	2440	2440	2440
Panel C: PSM among Border Cities						
ATT	1.478***	0.650***	0.827*	6.797	2.458	4.339**
	(0.491)	(0.245)	(0.478)	(4.702)	(2.637)	(2.202)
N	1880	1880	1880	1880	1880	1880

Notes: This table reports the estimated effects of LTCI on LTC provider entry using alternative control group specifications using the Callaway and Sant'Anna (2021) estimator. Data is from NECIPS (2010–2019). The dependent variables in Columns (1)–(3) represent the quarterly count of net entrants. The dependent variables in Columns (4)–(6) represent the total number of providers. Panel A reproduces the benchmark estimates. Panel B reports estimates using a sample constructed via propensity score matching (PSM) based on 2010 city characteristics. Panel C reports estimates using a matched border-city sample, which includes city-cluster specific linear time trends, where a cluster is defined as a pilot city and its matched border control. Standard errors, clustered at the city level, are reported in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

Table D4: Additional robustness checks, alternative specification, providers

	Net entry			Providers		
	(1)	(2)	(3)	(4)	(5)	(6)
	All	HCBS	Institutional	All	HCBS	Institutional
Panel A: Controlling for province-specific linear time trend						
ATT	0.986**	0.752***	0.234	14.795**	9.087**	5.707**
	(0.383)	(0.274)	(0.327)	(5.748)	(3.757)	(2.258)
N	10600	10600	10600	10600	10600	10600
Panel B: Controlling for province-by-year fixed effect						
ATT	0.737**	0.418**	0.319	14.105***	8.812**	5.293***
	(0.336)	(0.211)	(0.303)	(5.190)	(3.501)	(1.935)
N	10600	10600	10600	10600	10600	10600
Panel C: Controlling for city-by-year fixed effects and quarter fixed effects						
ATT	1.137***	1.114***	0.023	17.872***	11.230***	6.643***
	(0.372)	(0.192)	(0.304)	(4.658)	(3.109)	(1.872)
N	10600	10600	10600	10600	10600	10600

Notes: This table presents robustness checks for the impacts on LTC provider entry and the labor market following Equation (1). Observations are at the city-quarter level. The dependent variables in Columns (1)–(3) represent counts of net LTC, HCBS, and institutional LTCI provider entry, respectively. The dependent variables in Columns (4)–(6) represent counts of LTC, HCBS, and institutional LTCI providerS, respectively. Panel A replaces time trends in the baseline model with province-specific linear time trends. Panel B replaces temporal fixed effects in the baseline model at the province-period level. Panel C replaces temporal fixed effects with city-year and quarter-level effects. Standard errors, clustered at the city level, are reported in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

Table D5: Additional robustness checks: anticipation effects, providers

	Net entry			Providers		
	(1)	(2)	(3)	(4)	(5)	(6)
	All	HCBS	Institutional	All	HCBS	Institutional
Panel A: Baseline						
ATT	0.967***	0.547***	0.421	12.597**	7.258**	5.339***
	(0.318)	(0.180)	(0.297)	(5.197)	(3.474)	(1.939)
N	10600	10600	10600	10600	10600	10600
Panel B: Excluding cities that implemented LTCI after 2020						
ATT	1.023***	0.570***	0.454	13.183**	7.508**	5.675***
	(0.322)	(0.182)	(0.297)	(5.210)	(3.482)	(1.941)
N	10200	10200	10200	10200	10200	10200
Panel C: Excluding $t-2$						
ATT	1.018***	0.563***	0.455	13.195**	7.504**	5.690***
	(0.321)	(0.180)	(0.296)	(5.215)	(3.482)	(1.946)
N	10174	10174	10174	10174	10174	10174
Panel D: Excluding $t-3$						
ATT	1.015***	0.558***	0.458	13.205**	7.502**	5.704***
	(0.320)	(0.178)	(0.295)	(5.217)	(3.481)	(1.950)
N	10148	10148	10148	10148	10148	10148

Notes: This table reports robustness checks for the effects of LTCI on provider entry and employment under the main DID specification by sequentially : (i) excluding cities that implemented LTCI after 2020 (Panel B); (ii) excluding the second period prior to the pilot $t-2$ (Panel C); (iii) excluding the third period prior to the pilot $t-3$ (Panel D). Standard errors are reported in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

Table D6: Additional robustness checks: anticipation effects, Labor market

	(1)	(2)	(3)	(4)	(5)	(6)
	Log Employment share	Log wage	Log wage, median	Log wage, p75	Female share	Share High school
Panel A: Baseline						
ATT	0.787**	0.473***	0.293**	0.613***	−0.285**	0.178**
	(0.401)	(0.136)	(0.116)	(0.191)	(0.125)	(0.084)
N	1473	966	967	966	1473	1473
Panel B: Excluding cities that implemented LTCI after 2020						
ATT	0.798**	0.478***	0.297**	0.619***	−0.286**	0.182**
	(0.399)	(0.137)	(0.117)	(0.192)	(0.125)	(0.084)
N	1417	928	929	928	1417	1417
Panel C: Excluding $t-2$						
ATT	0.807**	0.475***	0.297**	0.619***	−0.290**	0.187**
	(0.394)	(0.138)	(0.117)	(0.191)	(0.125)	(0.082)
N	1384	909	909	908	1384	1384
Panel D: Excluding $t-3$						
ATT	0.814**	0.478***	0.299**	0.623***	−0.293**	0.190**
	(0.396)	(0.139)	(0.117)	(0.192)	(0.125)	(0.082)
N	1350	898	898	897	1350	1350

Notes: This table reports robustness checks for the effects of LTCI on labor market under the main DID specification by sequentially: (i) excluding cities that implemented LTCI after 2020 (Panel B); (ii) excluding the second period prior to the pilot $t-2$ (Panel C); (iii) excluding the third period prior to the pilot $t-3$ (Panel D). Standard errors are reported in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

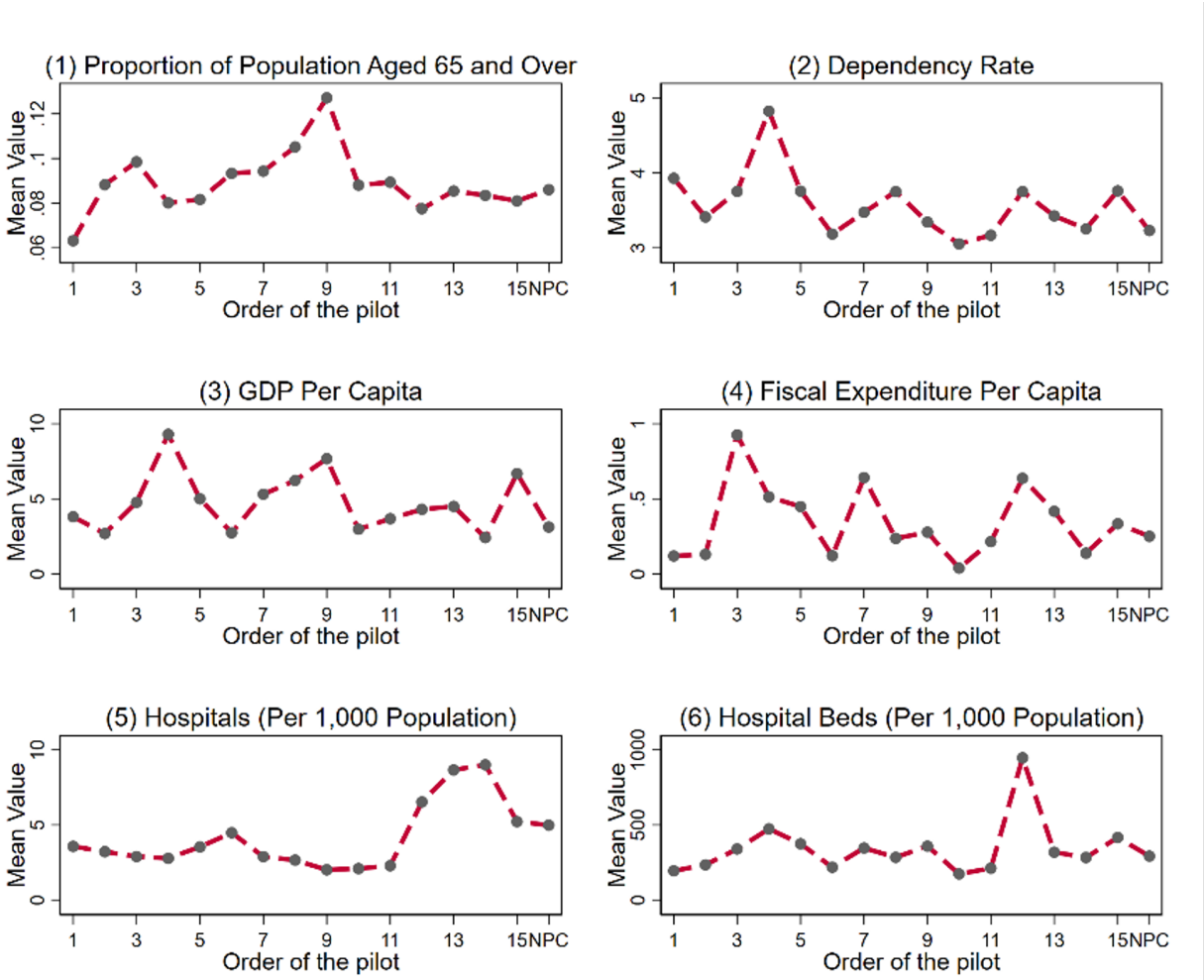


Figure D5: Pilot rollout patterns

Notes: This figure shows the relationship between city-level characteristics in 2010 and the order of the pilots. The horizontal axis represents the order in which the pilot was rolled out, with the last (i.e., 33) representing the control cities. The dashed line connects the mean statistics among cities with the same pilot timing, and the averages are taken for all non-pilot cities.

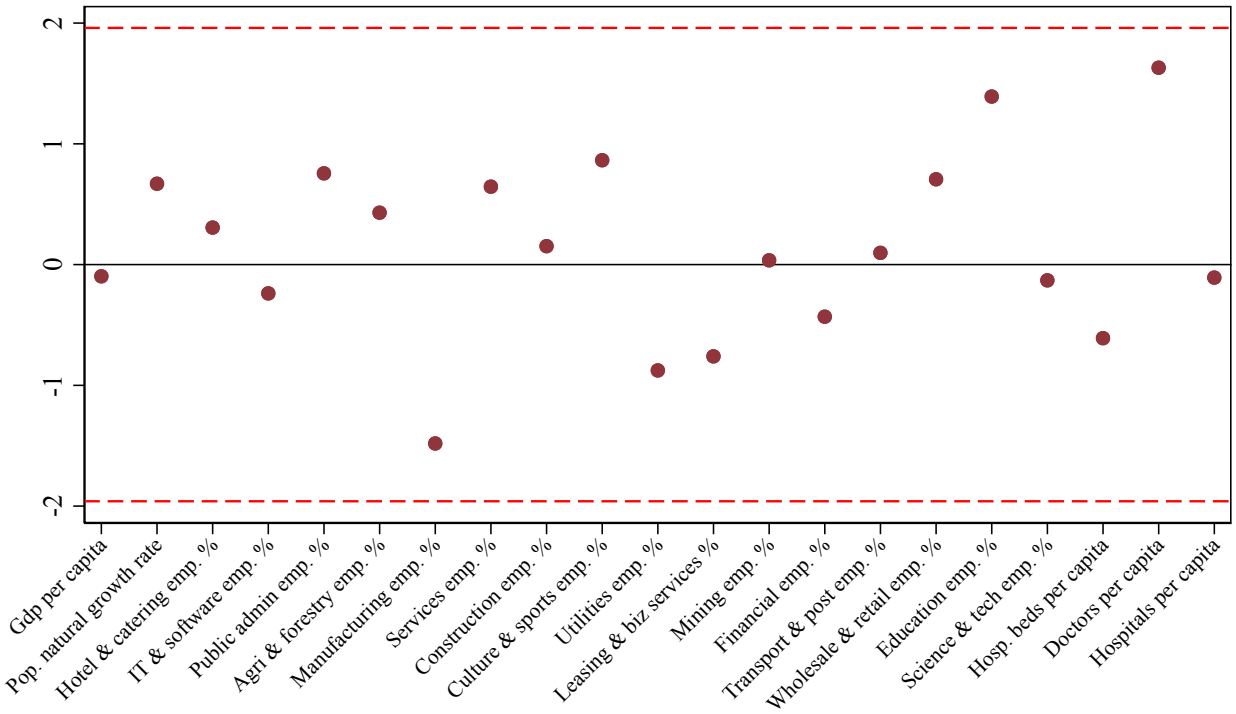
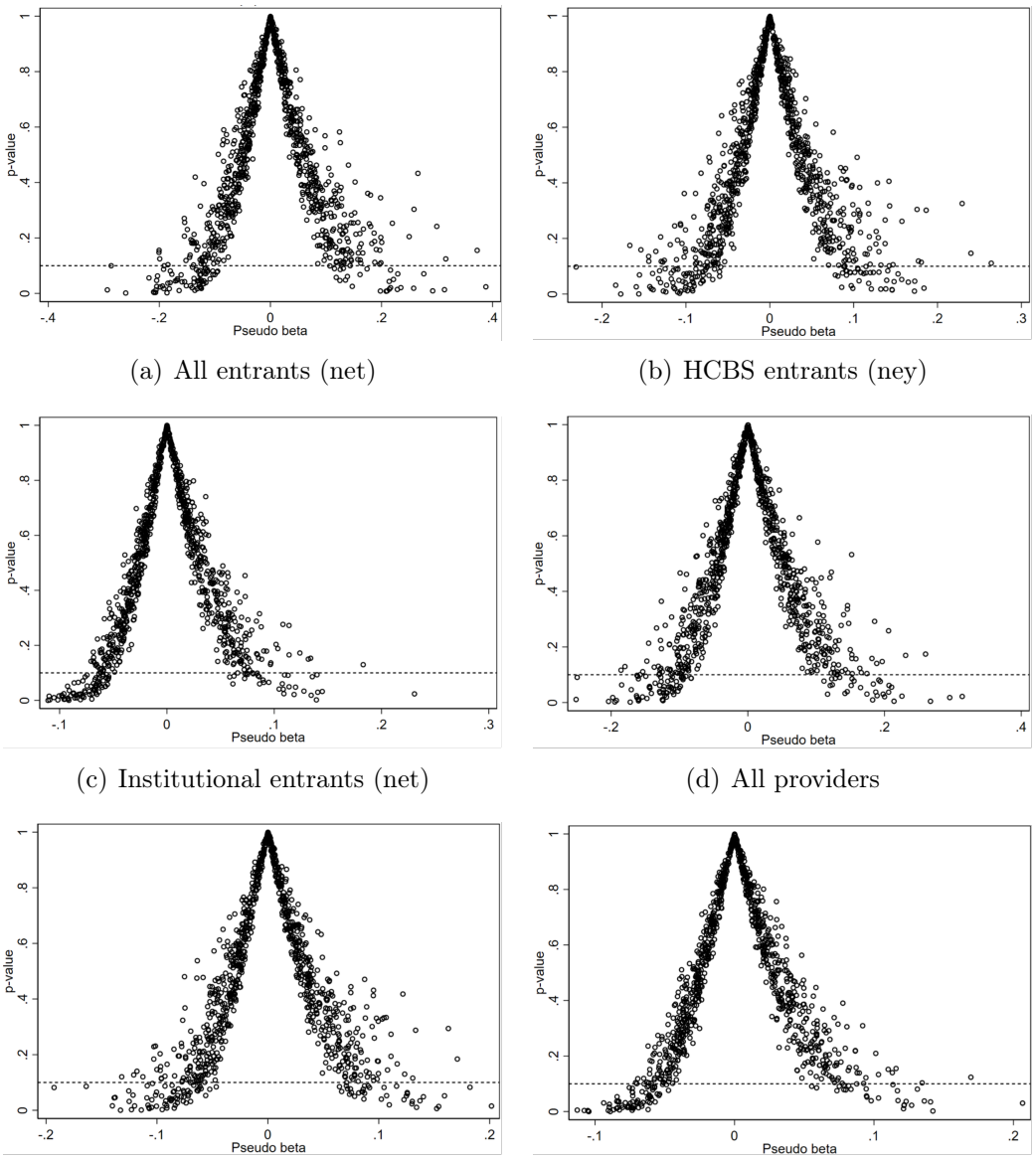


Figure D6: Falsification test: the impact of LTCI on non-LTC outcomes

Notes: This figure presents t-statistics for the estimated ATT of the LTCI pilot on a set of placebo outcomes. The estimates are obtained using the Callaway and Sant'Anna (2021) estimator with standardized outcome variables (z-scores). The placebo outcomes include city-level demographic variables (Natural Growth Rate), economic indicators (GDP per capita), and employment shares in non-LTC industries (e.g., Manufacturing, Mining, Financial Services). The red dashed lines indicate the critical values for statistical significance at the 5% level ($t = \pm 1.96$). Data is sourced from the China City Statistical Yearbook and annual local statistical bulletins.



(a) All entrants (net)

(b) HCBS entrants (ney)

(c) Institutional entrants (net)

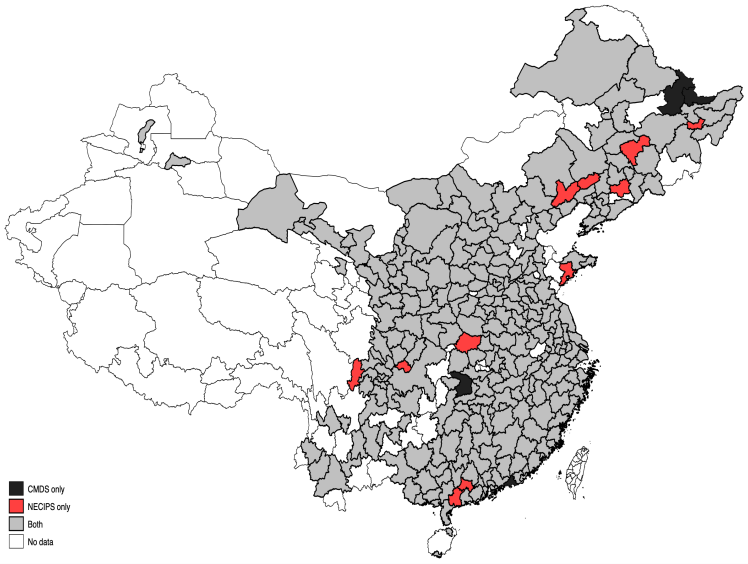
(d) All providers

(e) HCBS providers

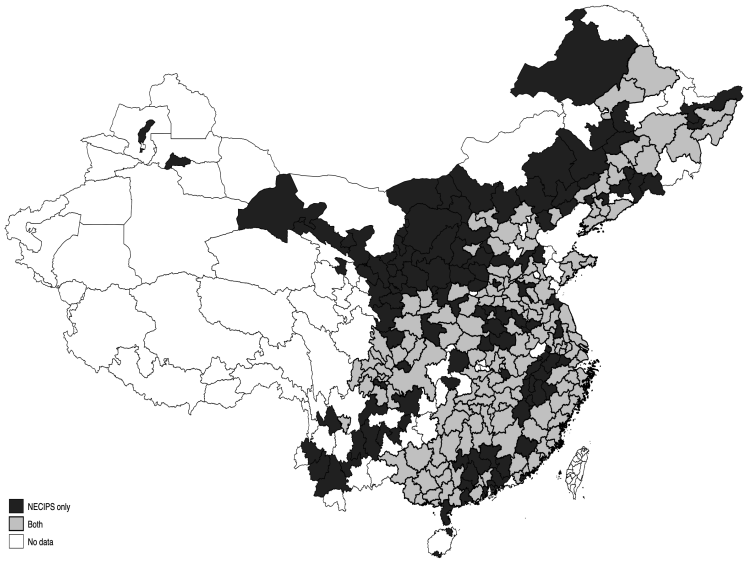
(f) Institutional providers

Figure D7: Pseudo-treatment versus actual pilot

Notes: This figure shows the distribution of pseudo point estimates (horizontal axis) and p-values (vertical axis) for the coefficient on the policy indicator, estimated using 1,000 pseudo samples generated by randomly assigning treatment status and timing to each city. The dashed lines in all panels represent the 10% p-value threshold.



(a) CMDS and NECIPS



(b) NECIPS and CLHLS

Figure D8: City coverage across datasets

Notes: Panel (a) shows cities covered in both the CMDS and NECIPS datasets. Panel (b) shows cities covered in both the NECIPS and CLHLS datasets.

Online Appendix

Online Appendix A. Details on Demand Model

OA.1. Estimation Results

The parameters to be estimated in Equation (2) are $(\beta, \beta_1, \beta_2, \gamma_1, \gamma_2, \sigma_v)$. Given a parameter guess, the likelihood contribution of observation i is given by Equation (3), integrating out v_i with respect to its distribution

$$\mathcal{L}_i(\beta, \beta_1, \beta_2, \gamma_1, \gamma_2, \sigma_v) = \int_{-\infty}^{\infty} p_{ij}(\beta, \beta_1, \beta_2, \gamma_1, \gamma_2; v)\phi\left(\frac{v}{\sigma_v}\right) dv, \tag{7}$$

where $\phi(.)$ is the probability density function of a standard normal random variable. The log-likelihood function of observing individual choices is given by

$$\ell(\beta, \beta_1, \beta_2, \gamma_1, \gamma_2, \sigma_v) = \sum_i \log\left(\mathcal{L}_i(\beta, \beta_1, \beta_2, \gamma_1, \gamma_2, \sigma_v)\right).$$

We estimate the model using the (simulated) maximum likelihood method.[34] The estimates are reported in Table OA1.[35] We estimate two models that only differ in the specification of choice-specific covariates (x_i). In Specification (2), we control for access to LTC providers, measured by the number of providers per 1,000 elderly population (denoted as N_c).

We first discuss coefficient estimates on LTC need shifters. The need for care services increases as an individual ages. Individuals with ADL impairment are more likely to have care needs. We do not find evidence of different care needs across genders.

Next, we discuss the estimated choice-dependent parameters $(\hat{\beta}_1, \hat{\beta}_2, \hat{\gamma}_1, \hat{\gamma}_2)$. In both specifications, we find that providing insurance benefits for informal and formal care increases the likelihood of choosing the respective care type. Elderly individuals are more responsive to formal care benefits than to informal care benefits. This finding is consistent with international evidence suggesting that the demand for informal care is generally less price-sensitive (Roquebert and Tenand, 2017), whereas the demand for private LTCI and resulting formal care utilization are more price-sensitive (Courtemanche and He, 2009; Goda, 2011). It is also aligned with the institutional context, where informal care benefits under LTCI are typically much lower than formal care benefits. For example, as documented in Footnote 33, LTCI

[34] The integral in Equation (7) is approximated with Gauss–Hermite quadrature.

[35] Based on our description of the data used in this section in footnote 18, our sample consists of 9,223 observations, of which 271 do not have income information (a covariate in Equation (2)). This brings the sample size for the estimation in Table OA1 to 8,952.

beneficiaries in Chengdu received 122% to 268% more in benefits for formal care than for informal care under its 2020 guidelines.

The coefficient on the number of providers per 1,000 elderly individuals is both statistically and economically significant. In the next subsection, we quantify the effects of LTCI eligibility and supply expansion by reporting several reduced-form elasticities. Moreover, coresidence with children increases the likelihood of choosing informal care and decreases the likelihood of choosing formal care (as in Mommaerts, 2018). Finally, we find no evidence that household income affects the pattern of care service choices.

Table OA1: LTCI, care providers, and individual care choices

	(1)		(2)		(3)	
	Informal	Formal	Informal	Formal	Informal	Formal
Common coefficients						
Wave 2014	0.633		0.633		0.612	
	(0.060)		(0.060)		(0.074)	
Wave 2018	0.567		0.558		0.704	
	(0.072)		(0.072)		(0.079)	
Alternative-specific coefficients						
LTCI*		1.245		1.101		1.475
		(0.403)		(0.412)		(0.421)
LTCI* × Family care	0.0375		0.0469		0.0348	
	(0.244)		(0.244)		(0.245)	
N_c				3.625		5.593
				(1.977)		(3.281)
Age	0.0861	0.0924	0.0861	0.0924	0.0772	0.0739
	(0.004)	(0.011)	(0.004)	(0.011)	(0.004)	(0.014)
Male	-0.294	-0.0465	-0.294	-0.0604	-0.290	-0.346
	(0.060)	(0.216)	(0.060)	(0.218)	(0.064)	(0.263)
Coresidence with children	0.616	-1.847	0.614	-1.825	0.538	-1.679
	(0.077)	(0.229)	(0.077)	(0.229)	(0.082)	(0.277)
Log household income	-0.00285	0.281	-0.00249	0.270	0.0154	0.0832
	(0.026)	(0.107)	(0.026)	(0.107)	(0.028)	(0.124)
Random Coefficient						
σ_v^2	0.779		0.779		0.475	
	(0.120)		(0.120)		(0.109)	
City fixed effects	Yes		Yes		Yes	
Control function for N_c	No		No		Yes	
Observations	11,740		11,740		9,661	
Log-likelihood	-5889.1		-5887.9		-4530.3	

Notes: This table reports estimates for parameters in Equation 2. Variable N_c is the number of providers per 1,000 population aged 65 and above. Specification (3) use policy and supply shifters to construct a control function for N_c in the maximum likelihood estimation. Data is from the CLHLS (2011, 2014, and 2018). Robust standard errors are reported in parentheses.

OA.2. Quantifying the Effects of LTCI

In this subsection, we combine our estimates in Table OA1 with the theoretical framework in Section 3 (particularly Figure 1(b)) to conduct a back-of-the-envelope quantification of the demand- and supply-side effects of LTCI. Based on the estimates in Column (2), we can calculate $p_{ij}(LTCI^*_{ij}, N_c; \hat{\beta}, \hat{\beta}_1, \hat{\beta}_2, \hat{\gamma}_1, \hat{\gamma}_2, \hat{\sigma}_v)$, the probability of an individual choosing different care options under different LTCI enactment status and the availability of providers using Equation (7).[36]

For cities that implemented LTCI by 2018, we construct a counterfactual 2018 provider density that assumes no supply-side effects. We do this by subtracting the estimated LTCI effect on entry from the observed provider count $\tilde{N}_c = N_c - \hat{\beta}^{\text{LTCI}} \times t_c/\text{Population}_c^{65+}$, where $\hat{\beta}^{\text{LTCI}}$ is the coefficient estimate in Table **??** Column (1), t_c is the number of quarters since the LTCI pilot in city c, and $\text{Population}_c^{65+}$ is the population aged 65 and above (in thousands) in city c.

We then calculate three sets of probabilities for the residents in city c:

$$\begin{aligned}
\check{p}_{ij} &= p_{ij}(LTCI^*_{ij} = 0, \tilde{N}_c; \hat{\beta}, \hat{\beta}_1, \hat{\beta}_2, \hat{\gamma}_1, \hat{\gamma}_2, \hat{\sigma}_v), \\
\hat{p}_{ij} &= p_{ij}(LTCI^*_{ij} = 1, \tilde{N}_c; \hat{\beta}, \hat{\beta}_1, \hat{\beta}_2, \hat{\gamma}_1, \hat{\gamma}_2, \hat{\sigma}_v), \\
p_{ij} &= p_{ij}(LTCI^*_{ij} = 1, N_c; \hat{\beta}, \hat{\beta}_1, \hat{\beta}_2, \hat{\gamma}_1, \hat{\gamma}_2, \hat{\sigma}_v).
\end{aligned}$$

That is, the choice probability without demand- or supply-side effects, with only demand-side effects, and with both demand- and supply-side effects. We average choice probabilities across individuals and obtain the aggregate share of formal care, informal care, and the outside option among pilot cities: $D_j(LTCI^*_{ij} = 0, \tilde{N}_c)$, $D_j(LTCI^*_{ij} = 1, \tilde{N}_c)$, and $D_j(LTCI^*_{ij} = 1, N_c)$, corresponding to points A, B, and C in our simple theoretical framework (Figure 1(b)).

We then calculate the percentage difference between the aggregate demands. The increase in formal care demand due to demand-side improvement is measured by

$$\left(\frac{D_j(LTCI^*_{ij} = 1, \tilde{N}_c)}{D_j(LTCI^*_{ij} = 0, \tilde{N}_c)} - 1 \right) \times 100\%,$$

representing the change from point A to B in Figure 1(b). The increase in formal care demand

[36] To be specific, the probability of individual i with demographic characteristics (x_i, z_i), LTCI eligibility $LTCI^*$, and the number of providers per 1,000 elderly population N_c choosing option j can be expressed as $p_{ij}(LTCI^*_{ij}, N_c; \hat{\beta}, \hat{\beta}_1, \hat{\beta}_2, \hat{\gamma}_1, \hat{\gamma}_2, \hat{\sigma}_v) = \int_{-\infty}^{\infty} p_{ij}(x_i, z_i, LTCI^*_{ij}, N_c; \hat{\beta}, \hat{\beta}_1, \hat{\beta}_2, \hat{\gamma}_1, \hat{\gamma}_2) \phi\left(\frac{v}{\hat{\sigma}_v}\right) dv$.

due to the supply-side effects is measured by

$$\left(\frac{D_j(LTCI_{ij}^*=1,\tilde{N}_c)}{D_j(LTCI_{ij}^*=1,N_c)}-1\right)\times 100\%,$$

representing the change from point B to C in Figure 1(b).

In addition to these aggregate statistics, we also compute the standard deviation across pilot city elderly individuals to measure the distributional features of the above two effects.[37] In Table OA2, we report the demand- and supply-side effects, measured by changes in aggregate demand and their standard deviations across elderly individuals in pilot cities (in parentheses). We present these results for three sets of treated cities. In Columns (1) and (2), we report the results for all treated cities. In Columns (3) and (4), we report results for treated cities with broader LTCI eligibility. In Columns (5) and (6), we report results for cities that provide reimbursement for informal care.

Among all treated cities, LTCI increases the aggregate share of formal care use by 14.72% through demand-side improvements. Supply expansion led by LTCI increases the aggregate share of formal care use by 2.24%. There is considerable heterogeneity across individuals, with standard deviations roughly 50–60% of the aggregate effects. The effects are quantitatively similar in cities with broader eligibility. Notably, in cities that provide reimbursement for informal care, the demand-side effect is the smallest, likely because out-of-pocket costs for both formal and informal care are reduced.

We also report the effects of LTCI on informal care choices. Overall, the implementation of LTCI has minimal impact on informal care usage. However, in cities with reimbursement for informal care, the demand expansion effect is significant, at around 5.96%. Formal care supply expansion does not affect informal care choices.

Finally, we find evidence that, in cities that provide benefits for informal care, the increase in formal and informal care choices is accompanied by a diversion from the outside option (a 1.74% decrease). This phenomenon is not observed in other cities and is consistent with previous studies showing that China's LTCI pilot has reduced the likelihood of unmet care

[37] For option j, the standard deviation of the demand-side effect is calculated as

$$\sqrt{\frac{1}{n-1}\sum_{i=1}^{n}\left[\left(\frac{\hat{p}_{ij}}{\check{p}_{ij}}-1\right)-\left(\frac{1}{n}\sum_{i=1}^{n}\frac{\hat{p}_{ij}}{\check{p}_{ij}}-1\right)\right]^2},$$

and the standard deviation of the supply-side effect is calculated as

$$\sqrt{\frac{1}{n-1}\sum_{i=1}^{n}\left[\left(\frac{p_{ij}}{\hat{p}_{ij}}-1\right)-\left(\frac{1}{n}\sum_{i=1}^{n}\frac{p_{ij}}{\hat{p}_{ij}}-1\right)\right]^2}.$$

needs among older adults (Lei et al., 2022).

Table OA2: The demand- and supply-side effects of LTCI in the pilot cities

	(1)	(2)	(3)	(4)	(5)	(6)
Treated cities	All		Broader LTCI eligibility		Reimbursement for informal care	
Channel	Demand	Supply	Demand	Supply	Demand	Supply
Formal care	24.78	24.19	25.08	22.83	7.51	36.61
	(11.68)	(28.08)	(16.76)	(34.62)	(3.79)	(44.16)
Informal care	1.62	-0.82	-0.89	-0.83	14.07	-0.93
	(8.29)	(0.49)	(1.16)	(0.8)	(19.31)	(0.42)
Outside option	-0.77	-0.12	-0.06	-0.13	-3.17	-0.08
	(2.3)	(.47)	(1.11)	(0.75)	(5.22)	(0.4)

Notes: This table reports the aggregate demand- and supply-side effects of LTCI. Data is from the CLHLS (2011, 2014, and 2018). The demand-side effect is defined as the percentage difference between (1) the aggregate share of an option if there were no LTCI and supply expansion driven by LTCI and (2) the share if there were only LTCI but no supply expansion, holding all else fixed. The supply-side effect is defined as the percentage difference between (1) the aggregate share if there were only LTCI but no supply expansion and (2) the share if there were both. Standard deviations across elderly individuals are reported in parentheses.