

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

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## Abstract

Although public long-term care insurance (LTCI) programs are prevalent globally, their effects on the supply of and access to long-term care (LTC) remain understudied. We analyze China's pilot public LTCI program, which reimbursed LTC services for older adults at regulated prices, using administrative establishment-registration data and a large-scale labor-market survey. Exploiting variation from the program's staggered rollout, we find that quarterly LTC provider entry increased by 245% relative to the pre-pilot mean in treated cities and that the share of service-sector workers employed in LTC increased by 14.8 percentage points. We estimate a model of older adults' choices between informal and formal care. Our estimates indicate that the LTCI increased the aggregate share of eligible beneficiaries using formal care. LTCI benefits for informal care did not discourage formal-care provider entry.

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

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# 1. Introduction

The global share of the population aged 65 and older rose from 5.02% in 1960 to 9.80% in 2022 and is projected to reach 16% by 2050 (United Nations, 2022). Over the same period, global fertility declined by almost 50%. This demographic shift has important implications for the demand for long-term care (LTC) services among older adults. Although most older adults currently rely on informal care provided by family members (Gruber et al., 2023), demand is expected to shift toward formal market-provided care. To address this need, numerous countries have implemented public long-term care insurance (LTCI) programs to improve the affordability and availability of formal LTC.<sup>1</sup>

Although public insurance has improved the affordability of care services,<sup>2</sup> concerns about limited supply-side responses have arisen in several healthcare contexts. When service supply is unresponsive to insurance expansion, increased demand may not improve access to LTC services (Garthwaite, 2012; Buchmueller et al., 2016), and public insurance programs may not ultimately improve individual health (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 unintentionally discourage formal-care provider entry?

This paper addresses this gap by examining China’s public LTCI pilot program from 2016 to 2019. The pilot provided public insurance that reimbursed LTC services for individuals aged 60 and older at regulated prices. In 2016, the central government launched the program to assess the feasibility and sustainability of a potential nationwide LTCI scheme. In 2020, an estimated 20.61 million older adults in China (8.1% of the older population) needed assistance with activities of daily living (Gong et al., 2022). By 2019, the pilot had been implemented in 39 cities across 14 provinces. We exploit temporal and geographic variation in LTCI availability arising from the staggered rollout to estimate the causal effects of LTCI on LTC supply and formal-care utilization.

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

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<sup>1</sup>In many countries, LTCI commonly refers to private long-term care insurance. Because China’s long-term care insurance system is almost entirely government-run, we use LTCI throughout this paper to refer specifically to the public system.

<sup>2</sup>See Gruber and McGarry (2023), Bakx et al. (2023), and Fu et al. (2023) for evidence from the United States, the Netherlands, and Japan, respectively, and Gruber et al. (2023) for a summary of evidence from ten developed countries.

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 draws on four data sources. First, we manually collect city-level LTCI implementation dates from government websites and policy reports. Second, to assess the policy’s supply-side effects, we identify new LTC providers using administrative establishment-registration data. Third, we measure LTC labor-market outcomes using the China Migrants Dynamic Survey (CMDS), a comprehensive annual nationwide survey that collects demographic and employment information from approximately 200,000 migrant households. As documented by [He and Luo \(2020\)](#), migrant workers constitute 90% of China’s 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 information on the demographic characteristics, socioeconomic status, family background, health, and care utilization of individuals aged 65 and older.

We use a staggered difference-in-differences (DID) framework to investigate the program’s effect on new provider entry. We estimate that LTCI increased net quarterly entry by 0.967 providers per city. We adopt four strategies to address the concern that pilot timing may be correlated with unobserved changes in local LTC supply. First, we include time trends interacted with pre-pilot city characteristics measured in 2010. Second, we estimate average treatment effects and conduct event-study diagnostics using the method developed by [Callaway and Sant’Anna \(2021\)](#). The event-study estimates provide no evidence of systematic or monotonic pre-trends in supply-side outcomes between treated and control cities before implementation. Third, we apply the method developed by [Rambachan and Roth \(2023\)](#) to assess the sensitivity of our findings to potential violations of the parallel-trends assumption. Fourth, we show that the order of LTCI pilot implementation is unrelated to baseline city characteristics.

To assess whether provider entry was accompanied by greater extensive-margin labor input, we next examine the LTC labor market. We find that the LTC employment share increased by 14.8 percentage points, relative to a pre-policy mean of 30 percent in treated cities, while average weekly hours did not change significantly. Taken together, these estimates are more consistent with adjustment through the number of workers than through longer hours per worker. We also conduct cross-industry falsification and spillover checks and find no evidence of broader employment spillovers, underscoring the specificity of the LTCI

effect on labor dynamics within the LTC sector. The results are robust to parallel-trend tests and alternative specifications. At the city level, average LTC worker income increased, but this reflects a compositional shift toward better-educated workers with higher wage premiums rather than higher wages for comparable existing workers.

The pilot’s effects vary across city characteristics. Cities with higher GDP per capita or larger older populations experience larger increases in provider counts but not in provider entry. The effect on total net entry is also larger in pilot cities that offer LTCI benefits for informal care.

We conclude our analysis by evaluating whether the expansion of formal-care capacity induced by LTCI translates into greater use of formal-care services. We link longitudinal CLHLS data on older adults’ demographics, health status, and care choices to the number of formal-care providers available in their cities. Using these data, we show that eligibility for LTCI benefits increases formal-care use, although the increase is smaller when LTCI also subsidizes informal care.

Finally, building on our theoretical framework, which formalizes the conjecture in [Arrow \(1963\)](#), we estimate a discrete-choice model in which individuals choose among informal care, formal care, and no care. Age, gender, LTCI eligibility, and coresidence with children enter the utility of each option. The model estimates are consistent with the descriptive patterns in older adults’ care choices. The results also align with the key mechanism in [Arrow \(1963\)](#): demand-side policies stimulate consumer demand, thereby increasing the expected profitability of market entry.

This paper relates to a substantial literature documenting significant supply responses to health insurance expansions (e.g., [Finkelstein, 2007](#); [Triyana, 2016](#); [Freedman et al., 2015](#)).<sup>34</sup> Despite this evidence, limited research has examined whether supply expansions translate into greater access to care. This study fills that gap by assessing whether public insurance addresses the underprovision of long-term care by inducing provider entry and lowering the price that patients pay for covered care.

This paper also contributes to the growing literature on the effects of LTCI. This literature focuses primarily on individual outcomes in developed countries and suggests that

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<sup>3</sup>[Finkelstein \(2007\)](#) reports that the introduction of Medicare, with its generous cost-based hospital reimbursements, led to a significant expansion of the US hospital sector. Similarly, [Triyana \(2016\)](#) finds that Indonesia’s Conditional Cash Transfer program, which encouraged the use of midwives as primary birth attendants, increased the supply of midwives. In contrast, [Freedman et al. \(2015\)](#) find that Medicaid expansion slowed hospitals’ adoption of new technology when newly insured Medicaid patients may previously have had private insurance.

<sup>4</sup>A large literature also studies the Children’s Health Insurance Program (CHIP) in the United States. Its expansion increased pediatrician participation in Medicaid ([Garthwaite, 2012](#)) and raised the likelihood that newly trained pediatricians would subspecialize and enter private practice by 18% ([Chen et al., 2018](#)). However, [White \(2012\)](#) find that CHIP expansion did not improve patients’ use of physician services.

public LTCI programs can improve beneficiaries’ welfare 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 mental and self-rated health (Barnay and Juin, 2016; Wang et al., 2023), and enhancing care-related quality of life (Forder et al., 2018).<sup>5</sup> Recent studies find that China’s LTCI pilot has reduced unmet care needs among older adults (Lei et al., 2022), decreased hospital use and medical expenditures (Lu et al., 2020; Feng et al., 2020a), and increased non-health consumption among older households (Liu et al., 2023). Unlike these studies, we explicitly model older adults’ choices between informal and formal care and quantify how LTCI increases formal-care utilization through improved affordability and expanded service capacity.

Our paper builds most directly on two important preceding studies of supply-side responses to demand-side policy interventions: Hackmann et al. (2025) and Geddes and Schnell (2023). Hackmann et al. (2025) provide a comprehensive analysis of how universal LTCI expanded the German nursing-home sector, jointly examining provider entry, employment, wages, and worker composition. Their labor-market search model offers a framework for understanding how insurance-induced demand mobilizes workers in a market with search frictions and pre-existing excess labor supply. They find a substantially larger employment response than provider-entry response, little evidence of wage increases, and a shift toward lower-skilled workers drawn from unemployment or nonparticipation. Their framework is especially relevant for interpreting our entry and employment estimates. We find a different pattern—provider entry expands more strongly relative to employment, while aggregate wages rise alongside a shift toward workers with higher educational attainment<sup>6</sup>—but their insight helps rationalize this contrast: more limited labor availability in China may have constrained policy-driven employment growth relative to provider entry.

Geddes and Schnell (2023) make an important contribution by demonstrating that the supply response to insurance expansion depends critically on the prices and payer segments affected by the policy. They show that expanded private insurance coverage at high, market-based prices can strongly encourage provider entry by reducing patients’ out-of-pocket costs and increasing their willingness to pay. At the same time, their analysis provides a sharp qualification to the conventional prediction of supply expansion: when insurance shifts patients from a market-based payer segment to a lower-priced regulated segment, provider

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<sup>5</sup>Consistent with these findings, Miyawaki et al. (2020) study a contractionary LTCI reform in Japan that reduced benefits for low-level care needs and find that the reform increased time devoted to informal caregiving, adversely affecting caregivers’ health.

<sup>6</sup>We find that provider entry expands substantially relative to employment, while aggregate wages increase alongside a shift toward workers with higher educational attainment.

profitability may fall and exit may increase.<sup>7</sup> Their findings highlight the central role of reimbursement design and the pre-existing market environment in determining firm responses. Our setting also features regulated prices, but differs in an important respect: before China’s LTCI pilots, no public insurance covered individual LTC expenses. LTCI therefore created insured demand in a previously uninsured market rather than primarily shifting patients from an existing market-based insurance segment to a regulated segment. This distinction helps explain why we observe an expansionary supply response and illustrates the broader relevance of the mechanism emphasized by [Geddes and Schnell \(2023\)](#).

The rest of the paper proceeds as follows. Section 2 describes the institutional background of LTCI and LTC provision in China. Section 3 presents a stylized analytical framework of how public insurance can stimulate the supply of care services. Section 4 describes the data and sample construction. Section 5 presents the empirical strategy and estimates of LTCI’s effects on provider entry and the labor market. Section 6 examines the implications of supply expansion for older adults’ care choices. Section 7 concludes.

## 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. Formal care was financed primarily out of pocket by older adults, with public medical insurance covering services that qualified as medical care. 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, eligibility criteria for pilot cities, and rollout process.<sup>8</sup>

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

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<sup>7</sup>In their two-tier outpatient-care setting, the contractionary effects of Medicaid expansion are concentrated in markets with relatively low Medicaid reimbursement rates.

<sup>8</sup>See “Guiding Opinions on Piloting the Long-Term Care Insurance System” (Document No. [2016]80).

locations of cities that started the LTCI pilot each year from 2016 to 2020.<sup>9</sup>

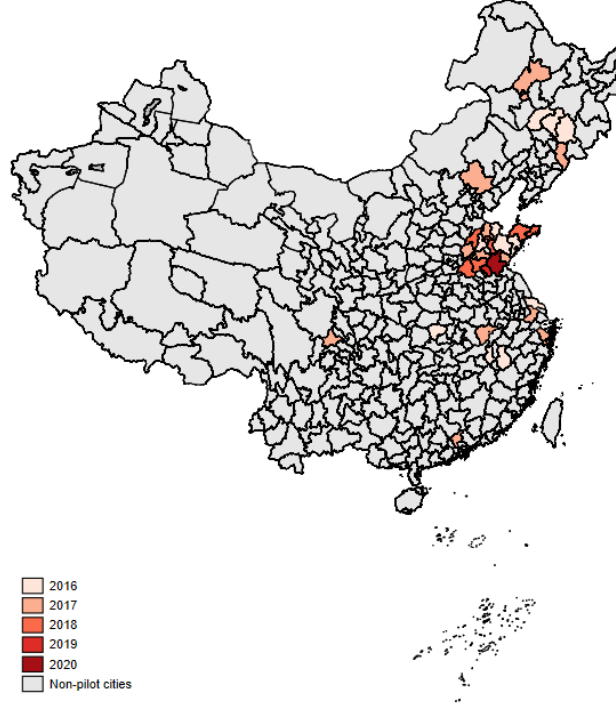


Figure 1: Staggered rollout of the LTCI pilots

Notes: The map reports the city-level LTCI benefit implementation dates used in the empirical analysis, compiled by the authors from policy documents issued by central and local governments. See Appendix C.1 for details.

City and municipal governments applied to the State Council for designation as pilot cities. Applications were evaluated on the basis of (1) local economic development and financial capacity; (2) demographic characteristics, particularly population aging and the share of older adults with disabilities; and (3) regional representativeness. The guidelines issued by the State Council emphasized the program’s experimental nature and limited the scale of the 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. The guidelines also established specific requirements for contribution and benefit schemes but did not impose a uniform national design.<sup>10</sup> Once selected, cities developed local insurance schemes in accordance with the guidelines. These schemes were implemented after

<sup>9</sup>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.

<sup>10</sup>For example, see “Guiding Opinions on Piloting the Long-Term Care Insurance System,” Section 2.8 (“The long-term care insurance (LTCI) fund pays a specified share of the costs incurred by care institutions and care workers in providing eligible care services to insured individuals. Differential benefit policies are established based on factors such as the level of care required and the mode of service delivery. For eligible long-term care expenses, the fund generally covers approximately 70% of the costs. The specific eligibility requirements and reimbursement rates are determined by the local pilot regions.”)

provincial approval and filing with the central government.

## 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.<sup>11</sup> 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 1.087 million beneficiaries.<sup>12</sup> Similar to pension insurance, enrollment in the insurance pool does not by itself confer eligibility for LTCI benefits. Participants are UEBMI/URRBMI enrollees who are mandated to participate in the LTCI insurance pool in pilot cities. Most participants are younger than 60 and contribute premiums to the system but are not yet eligible to receive LTCI benefits. Beneficiaries are a much smaller subset of participants who are aged 60 or older and meet the relevant disability criteria. Thus, the distinction is analogous to that between individuals who contribute to a pension insurance system and those who currently receive pension benefits.

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).

### 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

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<sup>11</sup>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.

<sup>12</sup>During the COVID-19 pandemic, most cities temporarily suspended long-term care services due to restrictions imposed by stay-at-home orders.

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).<sup>13</sup> In addition to premiums, other funding sources include government subsidies, public welfare funds from welfare lotteries, and charitable donations.

### *2.2.3. Price Regulation and Benefits*

To become eligible for LTC expense reimbursement, an individual must apply for an assessment and meet a locally specified threshold for persistent functional impairment, typically based on activities of daily living or a Barthel-type instrument. Most cities require severe functional disability, while some also cover individuals with moderate disability (e.g., a Barthel score of 40 or below).<sup>14</sup>

Pilot cities specified, in accordance with the national guideline, reimbursement rules that determined both provider settlement and patient cost-sharing. A city typically applied a common reimbursement model to providers within a given care category (e.g., institutional or community-based). Three primary models were used: (1) under fee-for-service, LTCI reimburses a specified share of the regulated service price. Reimbursement rates typically range from 70% to 90%, substantially reducing out-of pocket expenditures and enabling individualized care; (2) under a per-diem model, providers receive a fixed payment per bed-day; and (3) under capitation or fixed-monthly arrangements, LTCI provides a fixed package amount that typically varies by service type, or beneficiary disability severity.<sup>15</sup>

Although these models differ in how provider payments are determined, they operate within pre-existing care-service pricing guidelines. These guidelines, which specify a standardized service catalog and pricing standards, promote broad consistency in service pricing

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<sup>13</sup>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. See also Appendix C.3 for examples of premiums across pilot cities.

<sup>14</sup>For example, Guangzhou required an impairment expected to last at least six months and generally limited basic-care benefits to applicants with a Barthel score no greater than 40. Shanghai tied eligibility to specified levels of its unified care-needs assessment. See [https://www.gz.gov.cn/zwgk/gzsrnzfgbn/2017/24/content/post\\_10638644.html](https://www.gz.gov.cn/zwgk/gzsrnzfgbn/2017/24/content/post_10638644.html) and [https://rsj.sh.gov.cn/tzcwj\\_17116/20200617/t0035\\_1371103.html](https://rsj.sh.gov.cn/tzcwj_17116/20200617/t0035_1371103.html). Appendix Table C2 summarizes the policy components, and Appendix Table C3 separates patient liability from provider settlement in four representative cities.

<sup>15</sup>See Appendices C.2, C.4 and C.6 for an overview of city-level differences, examples of benefit plans in pilot cities, and benefits for informal care, respectively. For example, Shanghai covered 90% of covered HCBS charges and 85% of institutional charges; Guangzhou covered 90% of home care up to 115 yuan per day and 75% of institutional care up to 120 yuan per day. Chengdu used fixed monthly payments for institutional care based on assessed disability rather than services delivered. See [https://rsj.sh.gov.cn/tzcwj\\_17116/20200617/t0035\\_1371103.html](https://rsj.sh.gov.cn/tzcwj_17116/20200617/t0035_1371103.html), [https://www.gz.gov.cn/zwgk/gzsrnzfgbn/2017/24/content/post\\_10638644.html](https://www.gz.gov.cn/zwgk/gzsrnzfgbn/2017/24/content/post_10638644.html), and [https://www.sic.gov.cn/sic/81/455/0131/11794\\_pc.html](https://www.sic.gov.cn/sic/81/455/0131/11794_pc.html).

and reimbursement while allowing for limited local discretion.<sup>16</sup>

A patient’s out-of-pocket payment equals the regulated service charge under the fee-for-service arrangement, or the applicable local payment standard under per-diem and capitation arrangements, net of the LTCI reimbursement, subject to applicable copayments and reimbursement limits. Beneficiaries generally do not pay the full bill upfront: the LTCI fund settles the covered portion directly with the provider, while the beneficiary pays only the applicable copayment or other uncovered charges at the point of service.

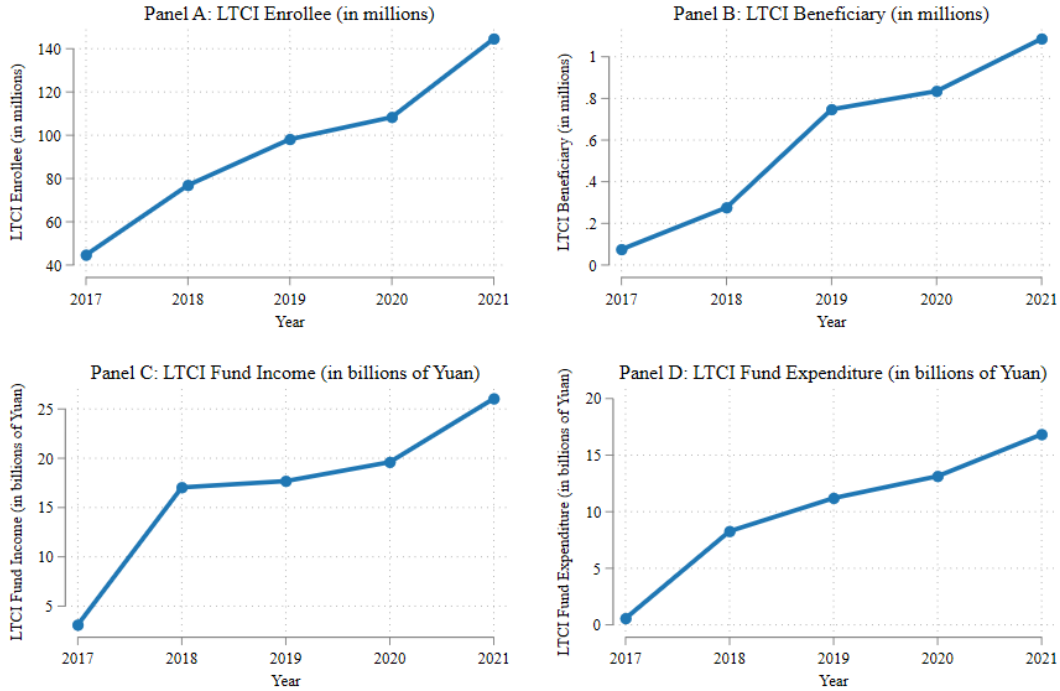


Figure 2: Aggregate LTCI participation, benefit receipt, fund income, and expenditure

Notes: Data are compiled from the website of the National Healthcare Security Administration (<http://www.nhsa.gov.cn/>).

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

The landscape of LTC service provision in China has changed substantially in recent decades. Before the pilot, public facilities dominated the formal-care sector. As the government shifted from directly providing LTC services toward regulating them, the pilot program was designed to encourage private entry and promote a market-based response to LTC underprovision

<sup>16</sup>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](https://www.ndrc.gov.cn/fggz/tzgg/ggkx/201205/t20120518_1063839.html).

(State Council, 2013; Feng et al., 2020b). Three types of care are reimbursed: home, community, and institutional care.

Home, community, and institutional care encompass a common set of services, including assistance with activities of daily living (e.g., feeding, dressing, and bathing) and basic medical services (e.g., nursing care, rehabilitation exercises, and health counseling). The categories differ primarily in where care is delivered: home and community care are provided in a beneficiary’s home and residential community, respectively, whereas institutional care is delivered in designated residential-care facilities or nursing homes.

Insured individuals become eligible for benefits once they are aged 60 or older and satisfy the applicable disability criteria. To receive LTCI benefits, eligible beneficiaries must obtain formal-care services from designated providers. New providers are approved for participation in the LTCI program on a monthly or quarterly basis.<sup>17</sup> Once designated, providers must adhere to regulated prices. For example, the Shanghai municipal 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 microfounded on older adults’ 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, microfounded by older adults’ discrete choices among informal care, formal care, and 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 provider availability. 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 fall to the level of entry costs.

Given these primitives, the introduction of an LTCI scheme has three effects. First, LTCI lowers the out-of-pocket price of formal care, increasing the likelihood that older adults choose formal care even when market structure is fixed in the short run. Second, higher demand raises incumbent providers’ profits, potentially inducing intensive-margin adjustments such as increased staffing and extensive-margin adjustment through new provider entry. Third,

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<sup>17</sup>Details are provided in Appendix C.5.

the demand- and supply-side changes together increase equilibrium formal-care utilization among older adults.

The focus of our paper is the supply-side effect. The empirical relevance of this effect and the relative importance of the extensive and intensive margins are not predetermined by the theoretical framework; instead, they depend on key economic primitives, such as beneficiaries’ responsiveness to LTCI benefits and the demographic structure.

We also investigate the equilibrium utilization effect—the increase in formal care demand after the LTCI pilot. The extent to which actual utilization rises depends on the supply-side response; if supply capacity on the extensive margin (e.g., provider entry and workforce expansion) does not expand to meet the subsidy-induced demand shock, the intended utilization gains of the LTCI policy will be constrained.<sup>18</sup>

## 4. Data, Sample Construction, and Measurement

### 4.1. Administrative Data on Firm Registration

The firm-registration data used in this paper come from the National Enterprise Credit Information Publicity System (NECIPS), which is developed and maintained by China’s State Administration for Market Regulation. The system contains comprehensive information on business and nonprofit establishments in China. We extract firms’ registration dates, Chinese Industry Classification (CIC) codes, business-scope descriptions, and locations. Because NECIPS records establishment registration rather than operational inputs, it does not allow us to observe capacity utilization or expansion within incumbent providers.

We combine registered business-scope descriptions at registration and renewal with CIC codes to identify LTC providers. Following the text-based classification method of [Beraja et al. \(2023\)](#), who use NECIPS data to identify Chinese firms specializing in artificial intelligence, we first select firms that meet both of the following criteria: (1) their business descriptions include terms for both “elderly care” and “care/nursing”; and (2) they are regulated by the Industrial and Commercial Bureau. This process yields 43,472 establishments.<sup>19</sup>

Next, we refine the sample using CIC codes. The National Bureau of Statistics of China published the *Statistical Classification of the Eldercare Industry* in 2020.<sup>20</sup> The guidelines

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<sup>18</sup>While these 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.

<sup>19</sup>CIC codes are broadly defined, and we document in Appendix A.2 that certain codes are not exclusive to LTC (e.g., other cleaning services).

<sup>20</sup>Source: [https://www.gov.cn/gongbao/content/2020/content\\_5503559.htm](https://www.gov.cn/gongbao/content/2020/content_5503559.htm) (in Chinese).

define three categories of LTC services—home-based, community-based, and institutional services—and provide the CIC codes associated with each category (see also Section 2.3). We retain providers from the initial selection whose CIC codes align with these guidelines and classify them as home- and community-based service (HCBS) providers, institutional providers, or both (following Feng et al., 2020a).<sup>21</sup> Appendix A provides further details on the CIC-based sample-construction procedure. After applying this criterion, we identify 7,775 unique LTC providers that entered the market between 2010 and 2019: 2,461 in the 36 treated cities and 5,314 in the control cities.<sup>22</sup>

Finally, we aggregate these data into city-quarter counts of provider entry. The resulting sample covers 265 prefecture-level cities over 40 quarters from 2010 through 2019, yielding 10,600 observations. Of these cities, 36 are classified as ever treated because the empirical sample includes cities from later rollout cohorts. Cities that implemented LTCI after 2019 remain untreated throughout the observed panel and serve as not-yet-treated comparison cities during this period; the other 229 cities are classified as never treated.

Panel A of Table 1 presents the average number of LTC provider entries in the treatment and control groups during the pre-pilot (2010–15) and post-pilot (2016–19) periods. Between 2010 and 2015, the average number of entrants was 0.122 in control cities and 0.395 in treatment cities (Columns (1) and (3)). Between 2016 and 2019, these numbers increased to 1.072 and 3.106, respectively (Columns (2) and (4)). Column (5) reports the baseline difference, (3) – (1), and Column (6) reports the corresponding  $p$ -value for the test of statistical significance. Similar patterns are observed for HCBS and institutional provider entry. Thus, pilot cities had significantly higher provider entry before the reform. This pattern is consistent with the city-level characteristics in Panel D, which show that pilot cities had higher dependency ratios and GDP per capita in 2010, and motivates our controls for baseline city characteristics in the subsequent analysis.

## 4.2. Labor Market Survey

We use the 2013–2018 China Migrants Dynamic Survey (CMDS) to examine the labor-market effects of LTCI. The CMDS is a comprehensive annual nationwide survey administered by

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<sup>21</sup>Home- and community-based providers share one CIC code. We combine these categories to avoid double-counting. The two service types are similar because both operate within residential communities and serve local residents. In contrast, institutional providers, commonly known as nursing homes, require greater upfront investment in facilities and equipment and provide a broader range of services. Feng et al. (2020a) adopt the same classification.

<sup>22</sup>The National Health Commission’s 2020 *National Aging Development Bulletin* reports 4,845 LTC service providers across the 49 pilot cities. Although the figures are not directly comparable—the bulletin reports providers operating in 2020, whereas our NECIPS measure captures providers entering between 2010 and 2019—the official statistic provides a useful external benchmark for the scale of our provider sample.

China’s National Health Commission that collects demographic and employment information from approximately 200,000 migrant households in each wave.

This dataset is particularly suitable for our study for two reasons. First, as highlighted by [He and Luo \(2020\)](#), migrant workers constitute 90% of China’s domestic-service workforce, including workers in nursing, housecleaning, and maternity care. Many of these jobs lack formal labor contracts, involve heavy workloads, and offer low wages. Consequently, these occupations are staffed primarily by migrant workers with limited access to other employment opportunities. This pattern is not unique to China (see [Bakx et al. 2023](#) and [Banks et al. 2023](#) for evidence from the Netherlands and England, respectively, and [Gruber et al. 2023](#) for a summary).<sup>23</sup> Second, the CMDS is the only labor-market survey that provides nationwide coverage of Chinese cities during both the pre- and post-pilot periods in our research design.

We calculate the LTC employment share at the city level as the share of service-sector workers employed in LTC-related jobs. Unlike NECIPS, the CMDS does not contain free-text business-scope or industry descriptions, so we cannot apply the keyword-based refinement used to identify LTC providers. We therefore define LTC workers through the intersection of two reported variables: employment within an LTC-related two-digit CIC industry (detailed in [Appendix A](#)) and an occupational classification of either “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 39 cities in the treatment group.<sup>24</sup> Prior to the pilot, the average share of LTC workers in treatment cities exceeded that in control cities by 4.7 percentage points. In the post-pilot period, this gap declined to 3.1 percentage points. Working hours, measured as average weekly hours among LTC workers within a city-year, and hourly wages, measured as their average hourly wage, increased modestly in control cities and more in treatment cities, although the difference in growth rates is not statistically significant. Average weekly hours exceed 55 in every treatment-control and pre-post cell, consistent with the long working hours documented for Chinese migrant workers by [Meng \(2012\)](#). This high baseline level leaves limited scope for further increases in hours per worker following LTCI implementation. These patterns indicate changes in LTC labor-market outcomes over the sample period but do not by themselves establish an effect of LTCI implementation. We examine this question formally in [Section 5](#).

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<sup>23</sup>[Gruber et al. \(2023\)](#) summarize evidence from ten developed countries and conclude that many countries rely heavily on immigrants as formal long-term caregivers.

<sup>24</sup>The number of prefecture-level cities covered by CMDS varies little across survey waves. The city coverage of NECIPS and CMDS is mapped in [Appendix Figure D13](#).

### 4.3. The Chinese Longitudinal Healthy Longevity Survey (CLHLS)

To complement the supply-side data, we incorporate information on older adults’ care choices from the Chinese Longitudinal Healthy Longevity Survey (CLHLS), conducted by the Center for Healthy Aging and Development Studies at Peking University. The CLHLS surveys individuals aged 65 and older and collects information on their demographic characteristics, socioeconomic status, family background, health, and care needs. Following prior LTC research (e.g., [Lei et al., 2022](#)), we use the 2011, 2014, and 2018 waves. The analytic data contain 3,499 unique respondents: 2,029 in control-group cities and 1,470 in treatment-group cities.<sup>25</sup> After excluding observations with missing variables, the reduced-form demand sample contains 8,635 person-wave observations from 3,045 individuals. Because the CLHLS includes only one post-pilot wave (2018), our analysis of demand responses should be interpreted as capturing short-term effects.

Descriptive statistics for the CLHLS are reported in Panel C of Table 1. We classify care as formal when a respondent identifies a paid organization or nonfamily caregiver as the primary source of assistance with activities of daily living, and as informal when the primary caregiver is a family member or another unpaid individual. Both indicators are constructed from the same caregiver-source question in every survey wave used in the analysis. Because the analytic sample includes all surveyed respondents rather than only individuals with substantial care needs or LTCI beneficiaries, formal and informal care account for only 1–2% and 11.9–25.6% of the full sample, respectively. Age and gender distributions are similar before and after 2016 in both groups. Because the CLHLS surveys individuals aged 65 and older, the sample naturally declines over time as respondents age and die, while informal care hours increase from the pre- to post-period. Household income is similar across groups before 2016, although it is higher in control cities. Finally, we report the shares of individuals eligible for formal and informal LTCI benefits separately; eligibility depends on enrollment in UEBMI/URRBMI, the individual’s ADL impairment, and whether the pilot city provides coverage for informal care, as described in Section 2.2.1.

### 4.4. Other Data

We augment our dataset with city-level macroeconomic and demographic characteristics measured before the LTCI pilot. We obtain baseline socioeconomic variables, including GDP per capita, fiscal expenditure per capita, and healthcare facilities, from the 2010 *China City Sta-*

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<sup>25</sup>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. The city coverage of NECIPS and CLHLS is mapped in Appendix Figure D13.

*tistical Yearbook*, and demographic characteristics, including the share of the population aged 65 and above and the dependency ratio, from the 2010 Census. Table 1, Panel D reports these characteristics. Treatment cities had significantly higher dependency ratios and GDP per capita in 2010; we control for these baseline differences in the subsequent analysis.

## 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.<sup>26</sup>

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  $t$  denote the calendar period. For each  $(g, t)$ , the estimator obtains the group-time average treatment effect on the treated,  $ATT(g, t)$ , by comparing outcomes for cities first treated in period  $g$  with those for never-treated cities, conditional on observable characteristics. This approach delivers consistent estimates under staggered adoption and heterogeneous treatment effects.

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

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

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.

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 extensive margin responses following insurance

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<sup>26</sup>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: Summary statistics

|                                              | Control cities |           | Treatment cities |           | Baseline comparison |                 |
|----------------------------------------------|----------------|-----------|------------------|-----------|---------------------|-----------------|
|                                              | Pre            | Post      | Pre              | Post      | Difference          | <i>p</i> -value |
| <b>Panel A: Provider entry and stock</b>     |                |           |                  |           |                     |                 |
| All entrants (net)                           | 0.122          | 1.072     | 0.395            | 3.106     | 0.273               | 0.039           |
| HCBS entrants (net)                          | 0.100          | 0.635     | 0.321            | 1.738     | 0.220               | 0.065           |
| Institutional entrants (net)                 | 0.021          | 0.436     | 0.074            | 1.368     | 0.053               | 0.009           |
| All providers                                | 1.440          | 10.848    | 5.727            | 33.679    | 4.287               | 0.034           |
| HCBS providers                               | 1.302          | 7.354     | 4.925            | 22.372    | 3.622               | 0.048           |
| Institutional providers                      | 0.137          | 3.495     | 0.802            | 11.307    | 0.665               | 0.039           |
| Number of cities                             | 229            |           | 36               |           |                     |                 |
| <b>Panel B: LTC workers</b>                  |                |           |                  |           |                     |                 |
| <i>City-level</i>                            |                |           |                  |           |                     |                 |
| Share of LTC workers                         | 0.252          | 0.191     | 0.300            | 0.222     | 0.047               | 0.323           |
| Working hours                                | 59.060         | 59.835    | 55.198           | 62.278    | -3.862              | 0.146           |
| Hourly wages                                 | 15.506         | 15.447    | 18.068           | 14.659    | 2.562               | 0.526           |
| <i>Worker-level</i>                          |                |           |                  |           |                     |                 |
| Age                                          | 32.985         | 34.291    | 31.860           | 35.386    | -1.125              | 0.224           |
| Female                                       | 0.518          | 0.529     | 0.495            | 0.516     | -0.023              | 0.689           |
| Primary school                               | 0.070          | 0.112     | 0.053            | 0.050     | -0.017              | 0.302           |
| Middle school                                | 0.543          | 0.563     | 0.514            | 0.528     | -0.029              | 0.608           |
| High school                                  | 0.270          | 0.201     | 0.302            | 0.278     | 0.032               | 0.636           |
| College                                      | 0.107          | 0.112     | 0.129            | 0.141     | 0.022               | 0.590           |
| Number of cities                             | 227            |           | 39               |           |                     |                 |
| <b>Panel C: The elderly population</b>       |                |           |                  |           |                     |                 |
| Informal care hours                          | 5.724          | 14.868    | 8.774            | 18.369    | 3.050               | 0.018           |
| 1(Informal care)                             | 0.119          | 0.256     | 0.213            | 0.226     | 0.095               | 0.007           |
| 1(Formal care)                               | 0.012          | 0.017     | 0.004            | 0.016     | -0.007              | 0.308           |
| Providers per 1000 pop. 65+                  | 0.005          | 0.041     | 0.009            | 0.075     | 0.004               | 0.184           |
| Age                                          | 80.633         | 85.520    | 81.137           | 85.378    | 0.504               | 0.548           |
| Male                                         | 0.461          | 0.467     | 0.477            | 0.494     | 0.016               | 0.560           |
| Coresidence with children                    | 0.766          | 0.751     | 0.799            | 0.732     | 0.033               | 0.321           |
| Household income (Chinese Yuan)              | 23306.428      | 28660.438 | 23070.277        | 26726.956 | -236.151            | 0.929           |
| ADL impairment                               | 0.083          | 0.203     | 0.133            | 0.215     | 0.050               | 0.036           |
| LTCI formal care elig.                       | 0.000          | 0.000     | 0.004            | 0.069     | 0.004               | 0.251           |
| LTCI informal care elig.                     | 0.000          | 0.000     | 0.000            | 0.007     | 0.000               | —               |
| Number of individuals                        | 2029           |           | 1470             |           |                     |                 |
| <b>Panel D: City characteristics in 2010</b> |                |           |                  |           |                     |                 |
| Population share aged 65 and over            | 0.086          | —         | 0.089            | —         | 0.003               | 0.276           |
| Dependency rate                              | 3.232          | —         | 3.523            | —         | 0.291               | 0.008           |
| GDP per capita                               | 3.129          | —         | 4.406            | —         | 1.277               | 0.002           |
| Fiscal expenditure per capita                | 0.251          | —         | 0.382            | —         | 0.132               | 0.149           |
| Healthcare facilities per capita             | 4.989          | —         | 4.769            | —         | -0.220              | 0.842           |
| Hospital beds per capita                     | 292.184        | —         | 324.254          | —         | 32.070              | 0.327           |
| Number of cities                             | 229            |           | 36               |           |                     |                 |

Notes: Columns (1)–(4) report group means before and after 2016. “Difference” is the treatment-group pre-period mean minus the control-group pre-period mean; the final column reports the corresponding *p*-value. Panel A uses city-quarter NECIPS data for 2010–2019. Panel B uses city-year CMDS data for 2013–2018; means and baseline regressions are weighted by the CMDS city-level sampling weight. Panel C uses the 2011, 2014, and 2018 CLHLS; the counts in its final row refer to unique respondents. Panel D uses city characteristics measured in 2010 and therefore has no post-period statistics. GDP per capita and fiscal expenditure per capita are measured in 10,000 RMB; healthcare facilities and hospital beds are measured per 10,000 residents. The *p*-values for tests of pre-period differences are based on standard errors clustered at the city level.

provision ([Hackmann, 2019](#); [Hackmann et al., 2025](#)). 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  $ATT(g, t)$ , 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  $\tau_t$ . 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 D2 in Appendix D.1 details the construction of the estimator.

## 5.2. Baseline Results

#### 5.2.1. LTC Provider Entry

Table 2 reports the effects of LTCI on provider flows and stocks. LTCI increased total net entry by 0.967 providers per city-quarter and HCBS net entry by 0.547. The institutional net-entry estimate is 0.421 (standard error 0.297) and is not statistically distinguishable from zero. Columns (2) and (3) distinguish HCBS providers from institutional providers, as described in Section 4.1. The HCBS estimate is positive and statistically significant, whereas the institutional estimate has a smaller magnitude and is not statistically significant. The larger HCBS point estimate is consistent with the predominance of home- and community-based care in China, although the estimates do not by themselves identify why the two

provider types respond differently.<sup>27</sup>

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.

The contrast between the imprecisely estimated institutional net-entry effect and the statistically significant increase in the institutional-provider stock is not necessarily inconsistent. The aggregate net-entry ATT averages quarterly flow effects across post-treatment periods, so relatively small or temporary increases in net entry may be estimated imprecisely when stronger effects in some quarters are offset by weaker effects in others. Nevertheless, even modest positive net-entry effects can accumulate over time and produce a persistent, statistically detectable increase in the provider stock.

Table 2: Effects of LTCI on provider entry and stocks

|                           | Net entry           |                     |                  | Providers           |                    |                     |
|---------------------------|---------------------|---------------------|------------------|---------------------|--------------------|---------------------|
|                           | (1)                 | (2)                 | (3)              | (4)                 | (5)                | (6)                 |
|                           | All                 | HCBS                | Institutional    | All                 | HCBS               | Institutional       |
| ATT                       | 0.967***<br>(0.318) | 0.547***<br>(0.180) | 0.421<br>(0.297) | 12.597**<br>(5.197) | 7.258**<br>(3.474) | 5.339***<br>(1.939) |
| Observations              | 10600               | 10600               | 10600            | 10600               | 10600              | 10600               |
| Cities                    | 265                 | 265                 | 265              | 265                 | 265                | 265                 |
| 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 are 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.

### 5.2.2. LTC Labor Market

To assess whether the extensive-margin supply response—provider entry—is accompanied by an intensive-margin response such as employment, Table 3 reports city-level labor-market effects. Column (1) shows that LTCI increased the share of service-sector workers employed

<sup>27</sup>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%.

in LTC by 14.8 percentage points, corresponding to a 49.3% increase relative to the pre-2016 mean of 0.3 in treated cities. Column (2) reports that the estimated effect on average weekly hours among observed LTC workers is 0.038 log points and is not statistically different from zero. Because this measure captures hours per observed worker rather than total sector hours, the results are more consistent with adjustment through the number of workers than through longer hours per worker, although the imprecise hours estimate does not rule out moderate changes in either direction.

Column (3) of Table 3 also shows an increase in average city-level wages (measured as average hourly wage among LTC workers within a city-year). 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-quantile 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 female workers 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. Column (1) also reports the coefficients from the parametric outcome regression models used within the Callaway and Sant’Anna (2021) estimator.<sup>28</sup> 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

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<sup>28</sup>These coefficients hence reflect the relationship between worker characteristics and outcome trends in the comparison group to compute  $ATT(g, t)$ . Because the estimator constructs separate first-stage models across different group-time cells, we report for each coefficient its average in the first row, and the 2.5th and 97.5th percentiles in parentheses.

long-term care markets.

The results in Section 5.2.1 measure the extensive margin through the entry of new providers, whereas employment captures the labor input associated with both new entrants and incumbent providers that expand their services. Because formal LTC is labor intensive, employment growth being substantially larger than provider entry would suggest an important role for expansion within existing providers (e.g., [Hackmann et al., 2025](#)). In our setting, provider entry outpaces employment growth, which may indicate that entry exceeded the supply of LTC workers, consistent with widely documented labor-supply constraints. This pattern also implies that the number of LTC workers per provider declined on average as providers entered the market, suggesting a potential deterioration in staffing-based quality ([Hackmann, 2019](#)). At the same time, workforce composition shifted toward more highly educated workers, suggesting improved worker qualifications. These opposing patterns leave the overall effect on care quality ambiguous.

Table 3: Effects of LTCI on aggregate employment, earnings, and worker composition

|                                         | (1)                | (2)                 | (3)                 | (4)               | (5)                | (6)                 |
|-----------------------------------------|--------------------|---------------------|---------------------|-------------------|--------------------|---------------------|
| <b>Panel A: Employment and earnings</b> |                    |                     |                     |                   |                    |                     |
|                                         | Employment         |                     | Log hourly wage     |                   |                    |                     |
|                                         | LTC share          | Log hours           | Mean                | p25               | Median             | p75                 |
| ATT                                     | 0.148**<br>(0.068) | 0.038<br>(0.086)    | 0.478***<br>(0.136) | 0.083<br>(0.169)  | 0.280**<br>(0.116) | 0.602***<br>(0.191) |
| Dependent-variable mean                 | 0.224              | 4.035               | 2.771               | 2.289             | 2.612              | 2.942               |
| Observations                            | 1,563              | 974                 | 974                 | 972               | 974                | 974                 |
| Cities                                  | 265                | 259                 | 259                 | 259               | 259                | 259                 |
| <b>Panel B: Worker composition</b>      |                    |                     |                     |                   |                    |                     |
|                                         | Experience         | Female              | Primary             | Middle            | High school        | College             |
| ATT                                     | -0.615<br>(1.846)  | -0.285**<br>(0.125) | -0.042<br>(0.107)   | -0.155<br>(0.140) | 0.178**<br>(0.084) | 0.009<br>(0.052)    |
| Dependent-variable mean                 | 23.340             | 0.512               | 0.100               | 0.516             | 0.247              | 0.125               |
| Observations                            | 1,473              | 1,473               | 1,473               | 1,473             | 1,473              | 1,473               |
| Cities                                  | 262                | 262                 | 262                 | 262               | 262                | 262                 |

Notes: The table reports overall average treatment effects estimated using the staggered-adoption estimator of [Callaway and Sant’Anna \(2021\)](#). The data are city-year observations constructed from the 2013–2018 CMDS. The LTC employment share is the share of service-sector workers employed in LTC-related jobs. Log hours, wage measures, and worker-composition outcomes are calculated among observed LTC workers; city-year outcomes use CMDS sampling weights. The estimator conditions on linear time trends interacted with the city characteristics measured in 2010 and listed in Table 1, Panel D. Standard errors are clustered at the city-level and reported in parentheses. \*\*\*, \*\*, and \* denote significance at the 1%, 5%, and 10% levels, respectively.

Table 4: Worker-Level wage effects

|                                                   | (1)                              | (2)                      |
|---------------------------------------------------|----------------------------------|--------------------------|
| Dependent variable: log hourly wage               | Staggered DID                    | Mincerian Regression     |
| <b>Panel A: Effects of LTCI</b>                   |                                  |                          |
| ATT                                               | -0.009<br>(0.017)                |                          |
| <b>Panel B: Effects of Worker Characteristics</b> |                                  |                          |
| Experience                                        | 0.0303<br>[0.0266, 0.0353]       | 0.032***<br>(0.001)      |
| Experience <sup>2</sup>                           | -0.00056<br>[-0.00064, -0.00050] | -0.00059***<br>(0.00002) |
| Female                                            | -0.239<br>[-0.268, -0.213]       | -0.213***<br>(0.005)     |
| Middle school                                     | 0.086<br>[0.052, 0.106]          | 0.085***<br>(0.005)      |
| High school                                       | 0.224<br>[0.169, 0.263]          | 0.224***<br>(0.008)      |
| College                                           | 0.522<br>[0.400, 0.582]          | 0.503***<br>(0.031)      |
| Estimation observations                           | 698,862                          | 713,566                  |

Notes: 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. The coefficients on worker characteristics in Column (1) are from the parametric outcome-regression models used within the estimator to model outcome evolution among comparison-group workers. They are nuisance-function coefficients used to estimate  $ATT(g, t)$ . Because the estimator estimates separate models for different group-time  $(g, t)$  cells, Column (1) reports the average coefficient across these models in the first row, with the 2.5th and 97.5th percentiles in brackets. Column (2) reports coefficient estimates from a conventional Mincerian wage regression of log wages on worker characteristics (experience, experience squared, gender, and education), controlling for industry fixed effects and city-wave fixed effects. \*\*\*, \*\*, and \* denote significance at the 1%, 5%, and 10% levels, respectively.

### 5.3. Robustness Checks

The baseline estimates show increases in provider entry, provider stocks, and the LTC employment share. We next examine whether these findings depend on differential pre-trends, the composition of the comparison group, anticipation, time-varying regional shocks, or broader city-level changes outside LTC.

#### 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 outcomes, Figure 3 plots estimates from 12 quarters before through 12 quarters after LTCI implementation.<sup>29</sup> Panels (1)–(3) show net entry for all, institutional, and HCBS providers, respectively, while Panels (4)–(6) report the corresponding provider stocks. Across the entry outcomes, we find no evidence of differential pre-trends before program implementation. Moreover, the post-adoption effects on provider stocks are statistically indistinguishable from zero by the twelfth quarter after implementation.

Note that the set of treated cities becomes small at longer event horizons: only four early pilot cities contribute observations 12 quarters after treatment. We therefore report estimates using an alternative event window of  $\pm 8$  quarters in Appendix Figure D4.<sup>30</sup>

Figure 4 presents event-study estimates for labor-market outcomes. Panel (1) reports the employment share; Panels (2)–(4) report the mean and quantiles of log wages; and Panels (5)–(6) report the female share 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

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<sup>29</sup>The pre-treatment pseudo-ATTs, including the estimate at  $t = -1$ , are estimated under the varying-base-period construction of Callaway and Sant’Anna (2021);  $t = -1$  is not an omitted reference category and therefore need not equal zero.

<sup>30</sup>Appendix Figure D3 reports the number of treated cities at each event time, and Appendix Table D7 reports the corresponding coefficients.

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 D2. For the specifications reported in 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.

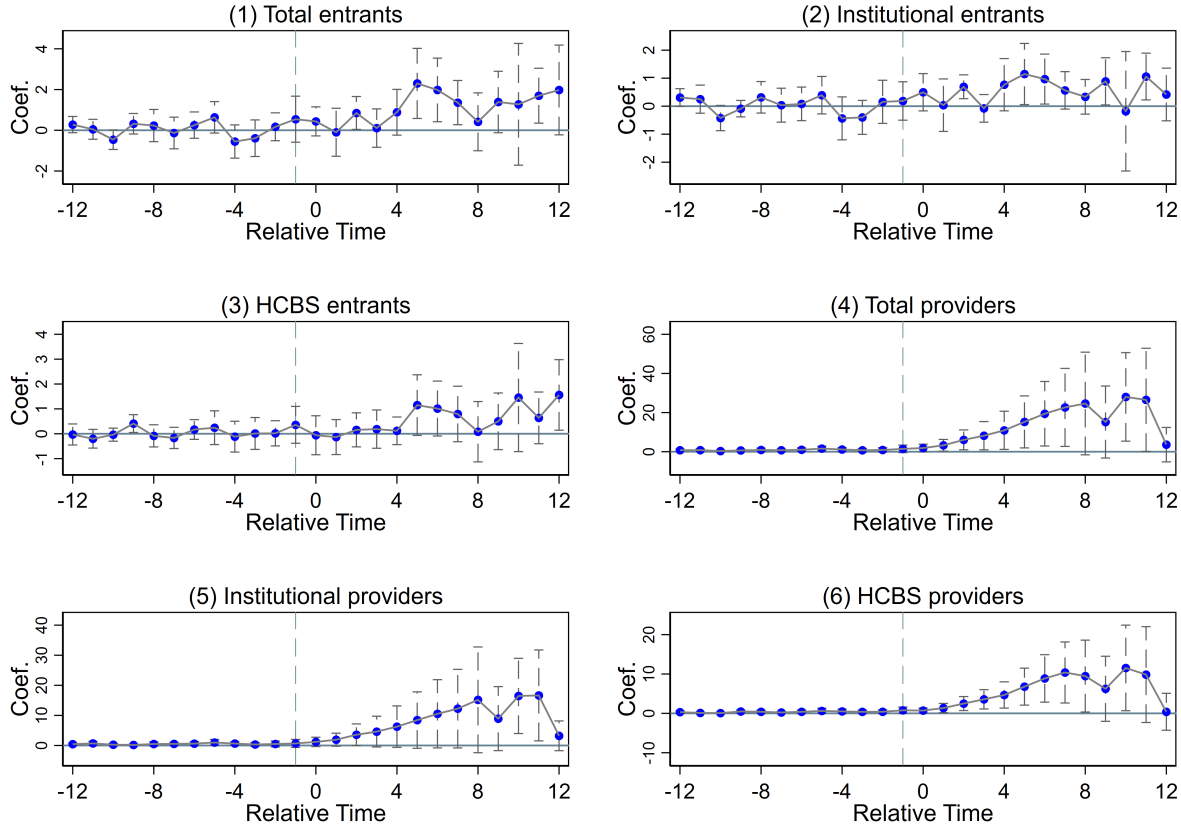


Figure 3: Effects of the LTCI pilot on provider entry and stocks: event-study estimates  
Notes: The figure reports event-time effects estimated using [Callaway and Sant’Anna \(2021\)](#) on NECIPS city-quarter outcomes for 2010–2019. Panels (1)–(3) report net entry for all, institutional, and HCBS providers; Panels (4)–(6) report the corresponding active provider stocks. Event time is measured in quarters relative to LTCI implementation, and the figure covers the unrestricted window from  $t = -12$  through  $t = 12$ . Points are estimates and bars are 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

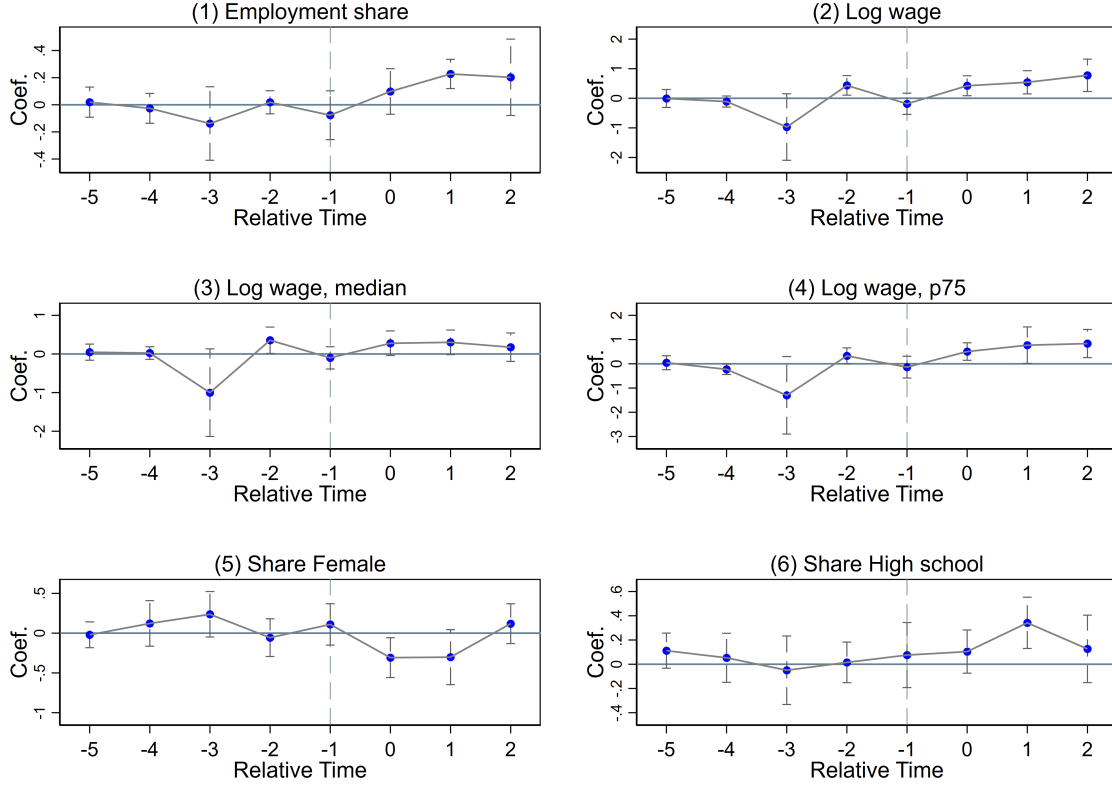


Figure 4: Labor-market effects of the LTCI pilot: event-study estimates

Notes: The figure reports event-time effects estimated using [Callaway and Sant'Anna \(2021\)](#) on city-year CMDS outcomes for 2013–2018. Panels (1)–(6) report the LTC employment share in levels, mean log wage, median log wage, 75th-percentile log wage, female share, and high-school share, respectively. The specification conditions on baseline city characteristics interacted with time and uses available not-yet-treated cities as comparisons. Event time is measured in survey waves relative to LTCI implementation. Pre-treatment estimates are pseudo-ATTs under the varying-base-period construction. Points are estimates and bars are 95% confidence intervals based on standard errors clustered at the city level; sample sizes vary across outcomes.

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. The propensity score estimation and implementation details are outlined in Appendix D.2.

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 balance-test results in Table D1 support the comparability of the matched groups: when we regress treatment on predetermined characteristics while controlling for constructed “city-cluster” fixed effects, the covariates are not jointly predictive of treatment.

Using these two matched samples, we re-estimate the baseline DID specification, with results summarized panel by panel in Appendix Table D2. Panel A reproduces our benchmark estimates, showing significant positive effects for overall and HCBS net entry, alongside significant increases across all provider stock categories (All, HCBS, and Institutional). In Panel B, we apply the propensity score matched (PSM) sample; while net-entry estimates remain positive and statistically significant across all three categories, all provider-stock estimates lose their statistical significance. Panel C further restricts the matched sample to border cities, where net-entry estimates remain robustly positive and highly significant. In contrast, most provider-stock estimates remain statistically insignificant in this border-matched sample, with only the institutional-provider stock maintaining statistical significance. We therefore view net entry as comparatively more stable across specifications.

The robustness checks for the labor-market estimates are reported in Appendix Table D6. Panel A reproduces the baseline estimates, showing statistically significant increases in the employment share, average wages (mean, median, and 75th percentile), and high-school share, alongside a significant decrease in the female share. Panel B applies the propensity-score-matched sample; the estimates remain consistent in sign and statistical significance across all labor-market outcomes. In Panel C, however, nearly all estimates lose statistical significance when we restrict the matched sample to border cities because the sample size shrinks substantially.

### 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 D4, 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 D5.

Second, the event-study estimates in the pre-treatment periods 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 assess robustness to providers' anticipation using the limited-anticipation specification of Callaway and Sant'Anna (2021), allowing anticipation windows of one or two quarters. Under this approach, cities leave the comparison group when they enter their anticipation window before treatment, while cities that have not yet entered that window remain eligible controls. Appendix Table D4 reports the results. Panel A reproduces the baseline estimates. Panel B excludes from the control pool cities that implemented LTCI in 2020 or later. Panels C and D show that the estimated treatment effects remain positive and generally increase modestly relative to the baseline.

Appendix Table D5 reports the corresponding labor-market results. Panel A reproduces the baseline estimates. Panel B excludes from the control pool cities that implemented LTCI in 2020 or later, yielding estimates nearly identical to the baseline. Panels C and D show that, when we allow for one- and two-year anticipation, the estimated treatment effects generally decrease in magnitude and lose statistical significance. The estimated anticipation effects are statistically indistinguishable from zero at the 10% level across all labor-market outcomes; all  $p$ -values exceed 0.10 (the lowest is  $p = 0.111$  for mean log wage in Panel D).

### 5.3.4. Alternative Period Fixed-Effects and Time Trend Specifications

We examine the robustness of our baseline estimates to alternative fixed-effects specifications in Panels A–C of Appendix Table D3. In Panel A, we interact a linear time trend with province indicators. In Panel B, we include province-period fixed effects, allowing temporal

shocks to vary across provinces rather than imposing common shocks as in the baseline specification. In Panel C, we use the most flexible specification for the provider-entry regression: city-year fixed effects and quarter fixed effects. Because the labor-market regression is estimated at the city-year level, we cannot apply this specification to the labor-market outcomes. The results remain similar to the baseline estimates across specifications.

#### *5.3.5. Cross-Industry Falsification and Spillover Checks*

Finally, to ensure our baseline findings are not driven by concurrent unobserved shocks to local economic or labor market conditions, we examine whether the implementation of LTCI coincided with broad changes outside the LTC sector. We begin by analyzing city-level demographic indicators (e.g., the natural population growth rate), macroeconomic measures (e.g., GDP per capita), and employment shares in non-LTC-related industries (e.g., mining, manufacturing, and financial services). The LTC employment share is measured relative to total service-sector employment, whereas each comparison-industry share is measured relative to total employment. Thus, an increase in the LTC share does not mechanically imply a decline in the comparison-industry shares, although labor reallocation across sectors could still contribute to the change. We also estimate the effect of LTCI on service-sector employment as a share of total employment and find that it is statistically indistinguishable from zero, indicating that the LTC employment result is not driven by a change in the denominator. Appendix Figure D6 plots the coefficient estimates and 95 percent confidence intervals for these outcomes. Most estimates are statistically indistinguishable from zero and exhibit no consistent sign or pattern.

In a complementary placebo check, we examine whether concurrent shocks affected local firm dynamics by analyzing net entry and provider stocks in non-LTC industries. As shown in Appendix Figure D7, these estimates are overwhelmingly indistinguishable from zero and display no consistent direction. Although we cannot rule out all cross-industry spillovers, the combined placebo tests provide no systematic evidence of a broad city-level expansion or contraction outside the LTC sector.

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.<sup>31</sup> We repeat this procedure 1,000 times, yielding a sample of 1,000 pairs of placebo estimates and corresponding  $p$ -values.

Appendix Figure D12 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 Characteristics and Policy Design

We next examine effect heterogeneity along two dimensions: pre-pilot city characteristics and LTCI policy design. We first divide the sample into above- and below-median groups based on pre-pilot GDP per capita, average wages, and the share of the population aged 65 and older. These indicators are correlated with market size and the expected profitability of entry. We re-estimate the baseline model in Equation (1) for each subgroup. Figures D8 and D9 present the resulting estimates and 95% confidence intervals based on standard errors clustered at the city level. The figures also report  $p$ -values from direct tests of equality across groups.

For provider outcomes (Figure D8), none of the cross-group differences in net entry (Panels (a)–(c)) is statistically significant. For provider stocks (Panels (d)–(f)), however, the effects are significantly larger in cities with higher pre-policy wages and larger older populations. Differences in provider-stock effects by GDP per capita are not statistically significant.

For the labor-market outcomes (Figure D9), the LTC employment-share response does not differ significantly across any city-characteristic split. Selected compositional differences emerge: the effects on mean and 75th-percentile log wages and on the high-school share differ significantly by pre-policy wages, while the effect on the female share differs by GDP per capita. Overall, although some provider-stock, wage, and worker-composition outcomes exhibit heterogeneity, higher baseline economic indicators do not uniformly amplify the effects of LTCI.

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<sup>31</sup>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.

We next examine whether LTCI policy design generates differential supply and labor-market responses. Figures D10 and D11 present the baseline effects alongside the pilot’s ATT for cities with broader LTCI eligibility (where beneficiaries include both UEBMI and URRBMI enrollees) and for cities that offer cash transfers for informal care in addition to the standard LTCI design. The figures also report  $p$ -values from direct cross-group comparisons.

For provider outcomes (Figure D10), direct statistical tests reveal that broader LTCI eligibility does not generate significant differences in net entry or provider stocks across any category (all  $p$ -values  $> 0.10$ ). For cities providing cash transfers, contrary to concerns that subsidizing informal care might crowd out formal supply, we find no evidence of a reduction in provider entry. While point estimates for cash-transfer cities appear larger in magnitude, only the difference in total net entry is statistically significant at the 5% level ( $p = 0.037$ ). The comparisons for HCBS and institutional entrants individually, as well as all provider stock measures, remain statistically insignificant. Overall, these results suggest that subsidizing informal care does not deter formal LTC provider entry, though the evidence that it uniformly amplifies supply is limited.

Turning to the labor-market outcomes (Figure D11), the LTC employment-share response does not differ significantly across any policy-design split. We do, however, observe selected differences in worker composition: the effect on the female share differs significantly under both broader eligibility ( $p = 0.001$ ) and cash-transfer coverage ( $p = 0.010$ ), and the effect on the high-school share differs by cash-transfer coverage ( $p = 0.021$ ). The wage effects, by contrast, do not differ significantly by policy design.

## 6. Demand Responses to the LTCI Pilot

While the provider and labor-market analyses document adjustments in provider entry and labor inputs, they cannot by themselves establish whether the supply response is accompanied by a comparable change in formal-care utilization. Nor do they explain how LTCI benefit design affects choices between formal and informal care—the core demand-side mechanism underlying the supply response in our conceptual framework (Section 3). We therefore present a parsimonious discrete-choice model, reduced-form estimates, and event-study evidence. The choice model jointly analyzes the mutually exclusive options of no care, informal care, and formal care, allowing us to characterize substitution across all three options, which is less straightforward when estimating separate reduced-form effects for each outcome. Together, the results support the mechanism in our analytical framework: LTCI creates demand-side incentives that increase demand for formal care and thereby strengthen providers’ incentives

to enter or expand. The analysis also illustrates how benefit designs, such as coverage of informal care, shape equilibrium care choices.

## 6.1. Choice Model

For notational simplicity, we suppress the time subscript below. Individual care choices are indexed by  $j$ , where  $j = 0, 1, 2$  corresponds to the outside option (i.e., no care), informal care, and formal care, respectively.

### 6.1.1. Demand Specifications

Following the canonical approach in the health-economics literature on long-term care utilization, the model separates biological need shifters from constraints related to economic resources and family structure. We include age, gender, and limitations in activities of daily living (ADLs) in  $z_i$  as primary determinants of baseline care needs (Liu et al., 2023; Lei et al., 2022). The vector  $x_i$  includes income and coresidence, both of which affect care-arrangement choices in the existing literature (Mommaerts, 2018, 2025; Chari et al., 2014).

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  $LTCI_{ij}^*$ .<sup>32</sup> Because some cities cover informal care in addition to formal care, both  $LTCI_{i1}^*$  and  $LTCI_{i2}^*$  can equal one. The two indicators enter the utility functions for informal and formal care, respectively. Finally, we incorporate type-I extreme-value utility shocks,  $\varepsilon_{ij}$ , which are assumed to be independent across individuals, choices, and years.<sup>33</sup>

### 6.1.2. Unobserved Heterogeneity

The model also accounts for two sources of unobserved heterogeneity. 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  $v_i \sim \mathcal{N}(0, \sigma_v^2)$ .

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<sup>32</sup>As detailed in Section 2.2, this indicator equals one if three conditions are met: (1) the city in which individual  $i$  resides has implemented the LTCI pilot by year  $t$ ; (2) individual  $i$  is aged 60 or older and meets the basic enrollment criteria (i.e., is enrolled in UEBMI and/or URRBMI); and (3) individual  $i$  meets the disability threshold for claiming benefits.

<sup>33</sup>Following the convention in the discrete-choice literature, the scale parameter of the  $\varepsilon_{ij}$  distribution is normalized to one (Train, 2009).

### 6.1.3. Choice Probabilities

Let  $U_{ij}$  denote the indirect utility that individual  $i$  in city  $c$  receives from option  $j$ :

$$U_{ij} = \begin{cases} \varepsilon_{i0} & \text{if } j = 0 \\ \underbrace{z_i\beta + x'_i\beta_1 + \gamma_1 LTCI_{i1}^* + v_i}_{u_{i1}} + \varepsilon_{i1} & \text{if } j = 1 \\ \underbrace{z_i\beta + x'_i\beta_2 + \gamma_2 LTCI_{i2}^* + v_i}_{u_{i2}} + \varepsilon_{i2} & \text{if } j = 2 \end{cases} \quad (2)$$

Under the distributional assumption on  $\varepsilon_{ij}$  and conditional on the realization of  $v_i$ , the probability that individual  $i$  chooses option  $j$  is given by the logit formula:

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

The ex ante choice probability is

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

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.

### 6.1.4. Marginal Effects in Binary Comparison

To illustrate how long-term care demand responds to LTCI benefits, we quantify the resulting changes in choice probabilities.

**Definition 1** (Marginal effect of LTCI on care 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:

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

## 6.2. Identification Strategy

The identification strategy closely follows the framework described in Section 5.1.1, with one minor distinction: almost all treated-city individuals have only one survey wave in the

post-implementation period. We include survey-year fixed effects to capture common temporal shocks to care demand and, as in our baseline specifications, interactions between a linear time trend and baseline city-level characteristics. To account for unobserved individual heterogeneity, we include individual fixed effects in the reduced-form treatment-effect estimations. In the structural discrete-choice model, we instead include individual-specific, time-invariant random effects, as detailed in Section 6.1.2, together with city fixed effects. Conditional on these fixed effects and covariates, identification hinges on the assumption that time-varying, idiosyncratic shocks to individual care choices are orthogonal to LTCI benefit eligibility.

### 6.3. Results

#### 6.3.1. Reduced-Form Results

Table 5 presents reduced-form two-way fixed-effects estimates of the effect of LTCI eligibility on individuals' care choices.<sup>34</sup> The first row shows that eligibility for formal-care benefits increases the probability of using formal care by 3.2 percentage points (Column 3), while reducing both the intensive and extensive margins of informal-care use (Columns 1 and 2). Because relatively few individuals are eligible for LTCI benefits, these estimates are imprecise. The second row highlights the role of benefit design in shaping substitution patterns. When LTCI also subsidizes informal care, formal-care utilization falls by 7.4 percentage points ( $p < 0.01$ ; Column 3), while reliance on informal care increases by 12.5 percentage points (Column 2).

Figure 5 presents event-study estimates of LTCI eligibility on care choices. The Wave 1 pre-treatment estimates are statistically indistinguishable from zero across all three outcomes, providing no evidence of differential pre-trends. Following implementation in Wave 3, the estimates are consistent with the pooled results: eligibility increases formal-care utilization (Figure 5(c)) and is accompanied by directional declines in both the extensive and intensive margins of informal care (Figures 5(b) and 5(a)). The formal-care effect is statistically significant, while the informal-care effects are estimated less precisely.

The difference in statistical significance between the event-study and TWFE estimates reflects their treatment of the pre-policy periods. The event study uses Wave 2 (2014) as the omitted baseline, whereas the TWFE specification uses both Waves 1 and 2 as the pre-treatment comparison period. Because Wave 1 exhibits greater variance, including it in the

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<sup>34</sup>Panel C of Table 1 reports the share of older adults eligible for LTCI benefits before and after 2016. For the model-free event-study analysis, we use a standard two-way fixed-effects (TWFE) estimator rather than the estimator of Callaway and Sant'Anna (2021). The very low baseline share of individuals choosing formal care causes numerical instability in the latter estimator.

Table 5: The effects of LTCI on care choices

|                                | (1)<br>Log (1+informal care hours) | (2)<br>Informal care | (3)<br>Formal care   |
|--------------------------------|------------------------------------|----------------------|----------------------|
| LTCI formal care eligibility   | -0.056<br>(0.226)                  | -0.015<br>(0.055)    | 0.032<br>(0.028)     |
| LTCI informal care eligibility | -0.067<br>(0.497)                  | 0.125<br>(0.137)     | -0.074***<br>(0.028) |
| Individual FE                  | Yes                                | Yes                  | Yes                  |
| Year FE                        | Yes                                | Yes                  | Yes                  |
| Trend $\times W_{c,2010}$      | Yes                                | Yes                  | Yes                  |
| Number of individuals          | 3,045                              | 3,045                | 3,045                |
| $N$                            | 8,635                              | 8,635                | 8,635                |

Notes: This table reports the effect of LTCI eligibility on older adults' care choices. The dependent variables in Columns (1)–(3) are  $\log(1 + \text{weekly informal-care hours})$ , an indicator for using informal care as the primary mode of care, and an indicator for using formal care as the primary mode of care, respectively. All specifications include individual fixed effects, survey-year fixed effects, and interactions between a linear time trend and baseline city-level characteristics ( $W_{c,2010}$ ). Data are from the 2011, 2014, and 2018 waves of the CLHLS. Standard errors clustered at the individual level are reported in parentheses. \*, \*\*, and \*\*\* denote significance at the 10%, 5%, and 1% levels, respectively.

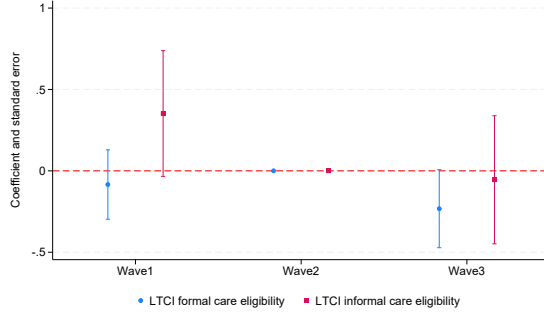
pooled comparison adds noise to the baseline and may reduce the precision of the estimated treatment effect. Using the more proximate Wave 2 baseline therefore yields more precise estimates of post-treatment changes.

### 6.3.2. Care Choice Model Estimation Results

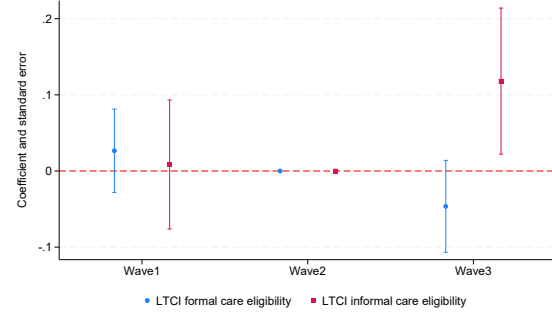
Table 6 reports estimates of the choice model in Equation (2). We estimate individuals' choices among informal care, formal care, and no care using a sample of 3,362 individuals. The specification includes city fixed effects and interactions between a linear time trend and baseline city-level characteristics.

We specify survey-wave indicators as common need shifters. The alternative-specific coefficients indicate that the use of both informal and formal care increases with age. Men are less likely to choose informal care, while we find no statistically significant gender difference in formal-care choices. Insurance benefits for informal and formal care increase the utility of the respective care type relative to no care. The coefficients on formal- and informal-care eligibility are both positive and statistically significant, suggesting that LTCI benefits shift choices toward the type of care they cover.

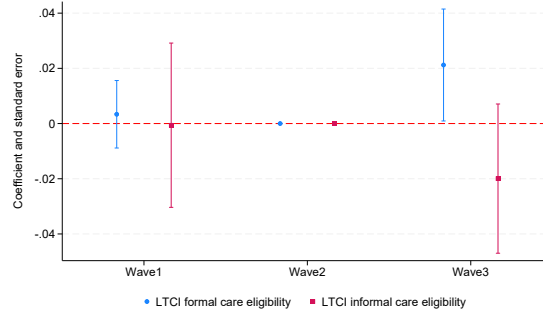
Moreover, coresidence with children increases informal-care use and decreases formal-care use (as in Mommaerts, 2018), while household income has no statistically significant effect. The estimated random-effect variance is 0.337 (standard error 0.102), indicating persistent



(a) Informal care hours



(b) Informal care



(c) Formal care

Figure 5: Event-study estimates of LTCI eligibility on care choices

Notes: This figure plots event-study coefficients and 95% confidence intervals based on standard errors clustered at the individual level. Panel (a) reports effects on  $\log(1 + \text{weekly informal-care hours})$ . Panels (b) and (c) report effects on indicators for using informal and formal care as the primary mode of care, respectively. The omitted baseline period is Wave 2 (2014). All specifications include individual fixed effects, survey-year fixed effects, and interactions between a linear time trend and baseline city-level characteristics.

unobserved heterogeneity in care preferences.<sup>35</sup>

Lastly, we discuss the marginal effect estimates  $\delta_2 - \delta_1$  and  $\delta_2 - \delta_0$ .  $\delta_2 - \delta_1$  measures the effect of LTCI (formal or informal) care benefit eligibility on diverting elderly individuals from informal care to formal care.  $\delta_2 - \delta_0$  measures the effect of LTCI (formal or informal) benefit eligibility on diverting elderly individuals from no care to formal care. Model estimates show that LTCI formal care benefit eligibility shifts older adults away from both the outside option ( $\delta_2 - \delta_0 = 0.032$ ) and informal care ( $\delta_2 - \delta_1 = 0.018$ ). In contrast, subsidies for informal care under LTCI significantly reduce the relative likelihood of choosing formal over informal care ( $\delta_2 - \delta_1 = -0.699$ ) while generating limited diversion from formal care toward the outside option ( $\delta_2 - \delta_0 = -0.008$ ).

## 7. Conclusion

Population aging has made the financing and provision of long-term care an increasingly pressing policy issue. Although public insurance can reduce the price of care faced by older people, coverage expansion will not improve access unless care providers and workers respond. Yet evidence on how care supply responds to insurance expansion remains limited, particularly in developing countries such as China, where formal long-term care markets are still emerging.

This paper studies the supply- and demand-side effects of China’s LTCI pilot program from 2016 to 2019. Exploiting temporal and geographic variation from the staggered roll-out of the program, we find that LTCI increases net provider entry by 0.967 providers per city-quarter. The pilot also raises the share of service-sector workers employed in LTC by 14.8 percentage points, with no significant change in average weekly hours. Although average LTC worker income increases, this reflects a compositional shift toward better-educated workers with higher wage premiums rather than higher wages for comparable existing workers. Provider entry therefore expands faster than the LTC workforce, consistent with fewer workers per provider on average and a potential deterioration in staffing-based quality. At the same time, workforce composition shifts toward more highly educated workers, suggesting improved worker qualifications. These opposing patterns leave the overall effect on care quality ambiguous.

The supply response should be interpreted with caution. Our data do not measure facility capacity over time, so we identify capacity expansion only along the extensive margin through

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<sup>35</sup>Adding the ADL impairment indicator slows convergence and drives the estimated random-effect variance to zero. Because the ADL impairment indicator strongly predicts care need (a z statistic of 34) and is highly persistent, it absorbs much of the time-invariant heterogeneity otherwise captured by the random effect.

Table 6: LTCI and individual care choices

| <b>Panel A: Coefficient estimates</b>      |                       |                       |
|--------------------------------------------|-----------------------|-----------------------|
|                                            | (1)                   | (2)                   |
|                                            | Informal care         | Formal care           |
| <b>Common coefficients (need shifters)</b> |                       |                       |
| Wave 2014                                  | 0.572<br>(0.315)      |                       |
| Wave 2018                                  | 0.521<br>(0.594)      |                       |
| <b>Alternative-specific coefficients</b>   |                       |                       |
| LTCI formal care eligibility               |                       | 1.687<br>(0.433)      |
| LTCI informal care eligibility             | 3.705<br>(1.143)      |                       |
| Age                                        | 0.0677<br>(0.004)     | 0.0655<br>(0.014)     |
| Male                                       | -0.316<br>(0.068)     | -0.287<br>(0.273)     |
| Coresidence with children                  | 0.480<br>(0.084)      | -1.762<br>(0.283)     |
| Log household income                       | 0.0111<br>(0.029)     | 0.0831<br>(0.126)     |
| $W_{2010} \times$ Trend                    |                       | Yes                   |
| City fixed effects                         |                       | Yes                   |
| Number of individuals                      |                       | 3,362                 |
| Number of observations                     |                       | 8,940                 |
| Log-likelihood                             |                       | -4028.5               |
| <b>Random coefficient</b>                  |                       |                       |
| $\sigma_v^2$ (individual random effects)   |                       | 0.337<br>(0.102)      |
| <b>Panel B: Marginal effects</b>           |                       |                       |
|                                            | $\delta_2 - \delta_1$ | $\delta_2 - \delta_0$ |
| LTCI formal care eligibility               | 0.018 (0.010)         | 0.032 (0.015)         |
| LTCI informal care eligibility             | -0.699 (0.164)        | -0.008 (0.001)        |

Notes: This table reports estimates for parameters in Equation 2 and 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 eligibility for formal- and informal-care benefits, respectively. Data are from the 2011, 2014, and 2018 waves of the CLHLS. Standard errors clustered at the individual level are reported in parentheses.

net new provider entry, not the intensive margin through capacity expansion by incumbent providers. The estimated entry effect therefore captures an important dimension, but not the entirety, of the supply response. The employment results provide complementary evidence on the labor input supporting this expansion.

The supply response is stronger in cities with higher GDP per capita or larger older populations. On the demand side, we estimate a model in which older adults choose among no care, informal care, and formal care. Eligibility for formal-care benefits shifts choices toward formal care, while coverage of informal care attenuates this shift. Despite this substitution in care choices, we find no evidence that informal-care benefits reduce entry by formal-care providers.

Finally, the reduced-form and discrete-choice estimates consistently indicate that eligibility for formal-care benefits shifts older adults toward formal care and away from informal care or no care. When LTCI also covers informal care, this shift toward formal care is attenuated, suggesting that benefit design affects substitution between care arrangements. Because formal-care use and LTCI eligibility are rare in the survey sample and each pilot city contributes only one post-implementation observation, we interpret these estimates as short-term, complementary evidence on the demand-side mechanism rather than as a comprehensive assessment of dynamic utilization responses.

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 may differ; however, our study covers only the early post-pilot period and does not estimate long-run effects. Understanding how the effects documented in our paper may evolve as the market matures remains an important direction for future research.

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## Appendix A. Sample Construction for LTC Provider Data

### A.1. Textual Information in Business-Scope Descriptions

To isolate LTC providers, we first select firms that meet both of the following criteria: (1) their business-scope descriptions include terms for both “elderly care” (养老) and “care/nursing” (护理); and (2) they are regulated by the Industrial and Commercial Bureau (工商行政管理局).

### A.2. Industry Codes in the 2020 Guidelines of the National Bureau of Statistics of China

Following the 2020 guidelines of the National Bureau of Statistics of China, our selection process begins by identifying establishments with Chinese Industry Classification (CIC) codes categorized as eldercare-related industries. The guidelines identify 12 major categories comprising 103 four-digit CIC (GB/T 4754-2017) industries.

The NBS classification extends beyond the scope of public LTCL. We retain only LTC-related industries and exclude entertainment, pensions 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 digits of each four-digit CIC (GB/T 4754-2017) code. We therefore select LTC providers using three-digit CIC codes. We retain 14 codes: 624, 801, 809, 821, 621, 622, 629, 851, 852, 841, 724, 842, 843, and 849.

### **A.4. LTC Provider Classification**

The final step of sample construction assigns LTC providers to categories. The 2020 NBS guidelines specify three categories based on three-digit CIC codes: home-based, community-based, and institutional providers.

- 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  $M$  older adults who may need long-term care, each individual faces three options: no care, informal care from family members, and formal care purchased in the market, indexed by  $j = 0, 1, 2$ , respectively. Formal care is purchased at an administered price  $p$  and reimbursement rate  $r$ , so the out-of-pocket price is  $(1 - r)p$ .<sup>36</sup>

The *indirect* utility that individual  $i$  derives from option  $j \in \{1, 2\}$  is  $U_{ij} = u_j(N, p, r) + \varepsilon_{ij}$ , where  $N$  is the number of LTC providers in the market and  $\varepsilon_{ij}$  is a utility shock drawn independently across individuals and options from the distribution  $f(\varepsilon)$ . Following the convention in the discrete-choice literature, we normalize the utility from no care to zero (Train, 2009).

Allowing the number of LTC providers to enter individual utility captures how greater provider availability can increase the likelihood that an individual chooses formal care. This relationship can be microfounded using a model in which providers are probabilistically available to serve older adults. Alternatively, it can be derived from a model of geographically differentiated providers, with  $N$  representing the number of LTC varieties accessible to individual  $i$ .<sup>37</sup> Thus, the term associated with  $N$  in the indirect-utility equation captures settings in which additional providers improve convenience, expand available options, or otherwise increase the likelihood of choosing formal care.

The market demand for LTC is proportional to the share of individuals who attain higher utility from LTC facilities:

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

**Example 1 (Variety/choice-set expansion):** Suppose that the indirect utility indi-

<sup>36</sup>The cost of informal care can be interpreted as the opportunity cost of family caregivers' time, primarily through reduced work hours (Chari et al., 2014; Mommaerts, 2025). In pilot cities that provide informal-care benefits, the out-of-pocket cost is effectively the difference between forgone wages and the LTCI subsidy.

<sup>37</sup>Appendix B provides an analytical example for each microfoundation: (i) a rationing model in which provider availability is probabilistic and (ii) a differentiated-provider model in which  $N$  captures local provider density. Both approaches yield the reduced-form relationship between  $N$  and care utilization.

vidual  $i$  receives from choosing formal-care provider  $j$  is  $u_{ij} = u_{i2} + \varepsilon_{ij}$ . Providers may deliver the same expected utility (e.g., standardized services) and differ only through the utility shock  $\varepsilon_{ij}$ . The ex ante inclusive value of formal care is  $U_{i2} = \log \left( \sum_j \exp(u_{ij}) \right) = \log(N) + u_{i2}$ , where  $N$  reflects local provider density. Similar specifications appear in studies of sales-force deployment (Hastings et al., 2017), movie-showtime allocation (Hodgson and Sun, 2025; Chen et al., 2024), and insurance-network breadth (Serna, 2024).

Suppose there are three options:

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

Here,  $(\delta_0, \delta_1, \delta_2)$  are the mean utilities of the three options,  $(\lambda_{i0}, \lambda_{i1}, \lambda_{i2})$  are individual-specific utilities (e.g., individuals in worse health benefit more from formal care), 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 individual  $i$  chooses formal care is

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

Hastings et al. (2017) rewrite this probability as

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

which yields an observationally equivalent indirect utility representation:

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

**Example 2 (Consideration set).** Suppose there are  $N$  symmetric providers, each of which enters individual  $i$ 's consideration set independently with Bernoulli probability  $\phi(N)$ . The utility of provider  $j$  is  $u_i + \varepsilon_{ij}$ . Our goal is to derive the inclusive value of formal LTC:

$$IV_{\text{LTC}} = E \left[ \max_{j: Z_j=1} \{u_i + \varepsilon_{ij}\} \right].$$

where  $Z_j \sim \text{Bernoulli}(\phi(N))$  indicates whether option  $j$  enters the consideration set and  $\varepsilon_{ij}$

are i.i.d. type-I extreme-value errors. Let  $V_i = \max_{j:Z_j=1} \{u_i + \varepsilon_{ij}\}$ . Following Goeree (2008, Equation 3), the CDF of  $V_i$  is

$$F_V(x) = \left[1 - \phi(N) \left\{1 - \exp \left[-e^{-(x-u_i)}\right]\right\}\right]^N. \quad {}^{38}$$

Consider a sequence  $\phi(N)$ , which converges to 0 at a rate between  $1/N$  and  $1/\sqrt{N}$  as  $N \rightarrow \infty$  such that  $N\phi(N) \rightarrow \infty$  (note that  $\phi(N) \equiv 1$  corresponds to all providers being considered by individual  $i$ ). Then using the property of exponential series,<sup>39</sup> for large  $N$  (a significant number of providers)

$$F_V(x) \approx \exp \left\{ -N\phi(N) \left[1 - e^{-e^{-(x-u_i)}}\right] \right\}.$$

Because  $-N\phi(N) \rightarrow -\infty$ , for small  $x$ ,  $F_V(x) \rightarrow 0 \approx \exp \left\{ -N\phi(N)e^{-e^{-(x-u_i)}} \right\}$ . For large  $x$ , using Taylor expansion  $1 - e^{-e^{-t}} \sim e^{-t}$ ,  $F_V(x) \approx \exp \left\{ -N\phi(N)e^{-e^{-(x-u_i)}} \right\}$ . Thus, when  $N$  is large,  $V_i$  is type-I extreme-value distributed with location parameter  $u_i + \log(N\phi(N))$  and scale parameter one:

$$V_i \sim \text{T1EV}(u_i + \log(N\phi(N)), 1).$$

It follows that

$$IV_{\text{LTC}} = u_i + \log(N\phi(N)).$$

---

<sup>38</sup>Let  $C$  denote the set of available choices. The maximum utility among choices in  $C$  is below  $x$  if and only if every choice in  $C$  yields utility below  $x$ :

$$\Pr(V_i \leq x \mid C) = \prod_{j \in C} \Pr(u_i + \varepsilon_{ij} \leq x) = \prod_{j=1}^N [\Pr(u_i + \varepsilon_{ij} \leq x)]^{Z_j} \equiv \Pr(V_i \leq x \mid Z_1, \dots, Z_N).$$

Because the  $Z_j$  variables are assumed to be i.i.d., the CDF of  $V_i$  is

$$\begin{aligned} F_V(x) &= E_{Z_1, \dots, Z_N} \Pr(V_i \leq x \mid Z_1, \dots, Z_N) = E_{Z_1, \dots, Z_N} \left[ \prod_{j=1}^N (\Pr(u_i + \varepsilon_{ij} \leq x))^{Z_j} \right] \\ &= \prod_{j=1}^N E_{Z_j} [(\Pr(u_i + \varepsilon_{ij} \leq x))^{Z_j}] = [E_{Z_j} [(\Pr(u_i + \varepsilon_{ij} \leq x))^{Z_j}]]^N. \end{aligned}$$

Plugging in the definition of Bernoulli expectation,

$$\begin{aligned} E_{Z_j} [(\Pr(u_i + \varepsilon_{ij} \leq x))^{Z_j}] &= \phi(N) \times \Pr(u_i + \varepsilon_{ij} \leq x) + (1 - \phi(N)) \times 1 \\ &= 1 - \phi(N)(1 - \Pr(u_i + \varepsilon_{ij} \leq x)), \end{aligned}$$

Finally,  $\Pr(u_i + \varepsilon_{ij} \leq x) = F_\varepsilon(x - u_i)$ , where  $F_\varepsilon(t) = e^{-e^{-t}}$  is the standard type-I extreme-value CDF. Thus,

$$F_V(x) = \left[1 - \phi(N) \left\{1 - e^{-e^{-(x-u_i)}}\right\}\right]^N.$$

<sup>39</sup> $\lim_{N \rightarrow \infty} (1 + x/N)^N = e^{-x}$

Hence, limited consideration affects the value of formal LTC through the expected number of considered providers,  $N\phi(N)$ . Provider entry raises the inclusive value whenever  $N\phi(N)$  increases.

### 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  $c(N)$ ,

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

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

These assumptions provide conditions under which the profit curve under regulated prices and the entry-cost curve have a unique intersection, as shown in Figure B1(a). Assumption 1 implies that, at the regulated price  $p$ , each of the  $N$  firms earns  $pD(N, p, r)/N$ . Assumption 2 implies that entry costs are finite and weakly increase with market saturation. Finally, Assumption 3 implies that individuals prefer lower prices, higher reimbursement rates, and greater provider availability. When no formal-care providers operate in the market, the marginal utility of adding the first provider 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 B1(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,

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

Figure B1(a) plots how the right-hand side term of Equation (6) varies with  $N$ . The intersection of the two curves in Figure B1(a) characterizes the equilibrium number of firms  $N^*$  and the market demand  $D(N^*, p, r)$ .

#### B.4. The Effect of an LTCI Pilot

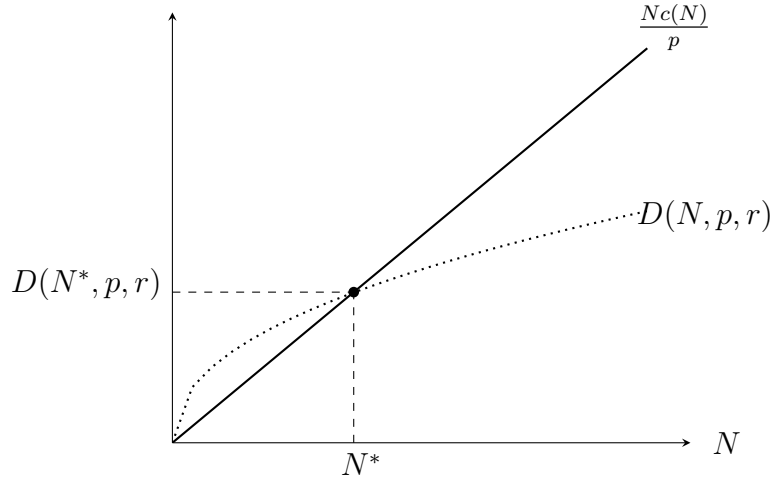
This simple model illustrates the potential effects of an LTCI pilot on market structure and equilibrium LTC demand. Because pilot cities regulate prices, we model LTCI as increasing the reimbursement rate from  $r_1 = 0$  to  $r_2 > 0$ , consistent with the institutional background described in Section 2.2. Figure B1(b) presents the main results. Additional proofs and derivations appear in Appendix B.5.

Figure B1(b) reproduces the original equilibrium under  $r_1 = 0$  from Figure B1(a). When the reimbursement rate rises to  $r_2$ , formal-care demand is higher for any number of providers  $N$ ; the demand curve shifts from  $D(N, p, r_1)$  to  $D(N, p, r_2)$ . In the short run, with market structure fixed, the market moves from the initial state A,  $(N_1^*, D(N_1^*, p, r_1))$ , to the interim state B,  $(N_1^*, D(N_1^*, p, r_2))$ . This demand expansion generates profits above entry costs, attracting new entrants until the free-entry condition in Equation (6) is restored. The market then moves from state B to the final state C,  $(N_2^*, D(N_2^*, p, r_2))$ .

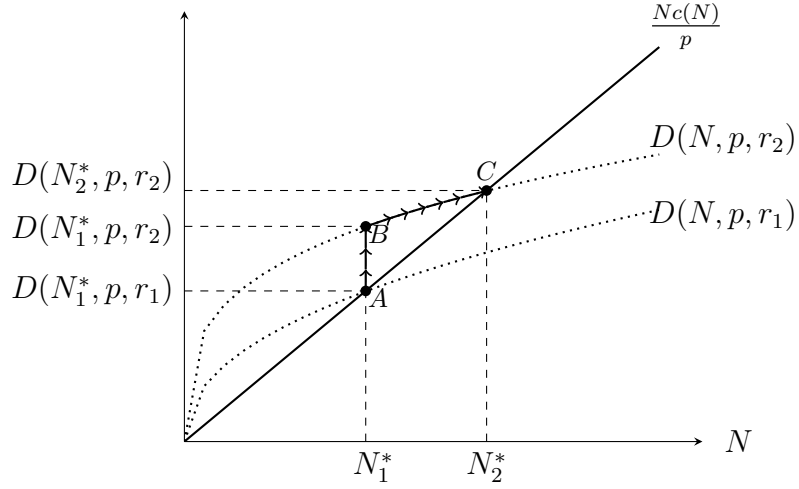
Furthermore, the elasticity of demand with respect to the reimbursement rate is positively associated with supply expansion. In markets where the elasticity is high (e.g., larger market size, larger fraction of the elderly population), we can expect a greater response to public LTCI. In summary, expansion 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.

#### 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.



(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

**Proposition 1:** Demand increases with 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 marginal increase in demand declines as the number of incumbent firms rises:

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

**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. Official Pilot Rollout and Implementation Timing

Table C1 reports the rollout timing among pilot cities.

Table C1: Timing of city-level LTCI implementation

| Province     | City      | Implementation date | Province       | City        | Implementation 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-level LTCI implementation dates were compiled by the authors from government policy documents. 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.

## C.2. Variation in LTCI Design

Table C2: LTCI rules across pilot cities

| Policy feature         | How cities differed                                                                               | Why it matters for the analysis                                                                               |
|------------------------|---------------------------------------------------------------------------------------------------|---------------------------------------------------------------------------------------------------------------|
| Policy timing          | Administrative designation and citywide benefit implementation could occur in different years     | Prevents an administrative batch label from being used mechanically as the treatment date                     |
| Insurance coverage     | UEBMI enrollees only or inclusion of URRBMI and other residents                                   | Determines the size and composition of the population exposed to the demand subsidy                           |
| Benefit eligibility    | Disability instrument, qualifying threshold, persistence requirement, and covered severity levels | Separates enrollment from effective demand for covered care                                                   |
| Care mode              | Home, community, residential, and medical-institution care                                        | Implies that new entry and expansion within existing providers may differ between HCBS and institutional care |
| Patient cost sharing   | Covered-service price, reimbursement share, copayment, and daily or monthly ceiling               | Determines the reduction in the price faced by eligible beneficiaries                                         |
| Provider payment       | Itemized service payment, per diem, or fixed monthly payment                                      | Determines how additional beneficiaries and services translate into provider revenue and cost exposure        |
| Provider designation   | Application, capacity and staffing review, service agreement, monitoring, and claims settlement   | Determines which providers can receive LTCI payments; market registration alone is insufficient               |
| Informal-care benefits | No benefit, cash transfer, or payment for trained family care                                     | May alter substitution between family care and services purchased from formal providers                       |

Notes: The table lists the policy components that varied across pilot cities. It does not classify a city as operating a single payment regime because local rules could differ across care modes and beneficiary groups.

### C.3. Premium Contributions

The *annual* LTCI premium (equivalent to 1.57–9.09 times the provincial *hourly* minimum wage in 2015) was relatively affordable. Cities used either flat annual premiums (e.g., 30 to 90 Chinese yuan) or contributions proportional to annual salary (0.3% to 1%). Contribution schemes varied 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.
- 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.4. Provider Payment and Cost Sharing

The following list summarizes provider-payment formulas and subsidy standards by care setting and beneficiary group.

- **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).
- **Fixed monthly amount:** 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).

Table C3: Patient cost sharing and provider settlement in selected pilot cities

| City      | Care setting                                       | Patient liability                                                                                                                                                                               | Payment to designated providers                                                                                                                                                                                                                           |
|-----------|----------------------------------------------------|-------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------|-----------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------|
| Shanghai  | HCBS and institutional care                        | The patient paid 10% of covered home- and community-care charges and 15% of covered institutional-care charges, plus noncovered charges.                                                        | The insurance center and designated providers agreed service prices and LTCI payment standards; providers recorded covered charges for fund payment.                                                                                                      |
| Guangzhou | Home, institutional, and medical nursing           | The patient paid 10% of covered home care and 25% of covered institutional care, plus amounts above the daily ceilings and noncovered charges.                                                  | The fund paid covered home care up to 115 yuan per day and institutional care up to 120 yuan per day; approved medical-nursing items were settled separately, subject to a 1,000 yuan monthly ceiling.                                                    |
| Jinan     | Home, institutional, and intensive medical nursing | Employee-insurance beneficiaries paid 10% of qualifying charges; the policy had no deductible.                                                                                                  | The 2016 rules paid fixed daily packages of 50 yuan for home care, 60 yuan for institutional care, and 200 yuan for intensive medical nursing. Ten percent of approved monthly payment was retained as a quality guarantee and released after assessment. |
| Nantong   | Home and institutional care                        | The patient paid 40% of qualifying charges in a medical-institution care bed and 50% in an eldercare-institution bed; home-care liability depended on the service schedule and monthly ceiling. | Institutional care was settled at a 70 yuan bed-day amount; qualifying home services were paid item by item within a 1,200 yuan monthly ceiling. The patient portion and fund portion were settled through the social-security card.                      |

Notes: “Patient liability” refers to the part of qualifying charges not paid by LTCI, plus charges outside the covered standard. “Payment to designated providers” refers to the formula used by the insurance administrator to settle with the provider. The two need not be the same percentage or unit. Sources: Shanghai LTCI Pilot Measures, Articles 14–19; Guangzhou LTCI Trial Measures, Articles 15–21; Jinan 2016 implementation measures, Articles 14–18; and Nantong Government Document No. 73 of 2015, Sections III–IV. All sources are linked in the main text.

### C.5. 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.6. LTCI Benefits for Informal Care

Only nine pilot cities in our sample period provided benefits for informal care. These benefits took two forms: direct cash transfers and payments for services. Direct cash transfers were provided to eligible families that cared for older household members at home. The transfers were typically monthly lump-sum payments equivalent to 30%–60% of the monthly minimum wage. For example, Nantong paid 15 Chinese yuan per day in 2016.<sup>40</sup> Under its 2020 guidelines, Chengdu paid 288–1,437 Chinese yuan per month.<sup>41</sup>

Payments for services varied with the recipient’s assessed care needs. For example, Chengdu paid 360–1,796 Chinese yuan per month to eligible older adults who received care from a certified and trained family member.

Several antifraud mechanisms help ensure that benefits are used for their intended purposes. First, as with formal-care benefits, individuals must undergo LTCI eligibility verification, including mandatory ADL assessments and cross-checks against medical or disability records. In addition, all cities used electronic application systems to strengthen monitoring. For example, Tianjin required family caregivers to upload daily care logs. Finally, all cities conducted random audits to verify compliance with program rules.

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<sup>40</sup>Source: [Nantong policy notice](#), retrieved June 8, 2025.

<sup>41</sup>The LTCI benefit for formal care is approximately 122%–268% higher than the benefit for informal care in Chengdu. Source: [Chengdu 2020 policy guidance](#), retrieved June 8, 2025.

## Appendix D. Additional Empirical Results

### D.1. Callaway and Sant’Anna (2021) Estimator

Let  $g$  denote the first period in which a city receives treatment and  $t$  denote the calendar period (a quarter in the entry regressions and a 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  $X$ :

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

Here,  $C$  is an indicator equal to one for units that are never treated, and  $G_g$  is an indicator equal to one if the unit is first treated 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 D1** (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 D2.** (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

As a diagnostic for the propensity-score-matched sample, Figure D1 compares the distributions of estimated propensity scores before and after matching. The distributions overlap more closely after matching, providing a visual check on common support in the estimated score. This check concerns the propensity score only and does not establish balance in every observed or unobserved determinant of treatment.

Table D1 formally assesses balance by regressing the treatment indicator on the six pre-treatment city characteristics reported in Panel D of Table 1, while controlling for matched border-pair fixed effects. None of the six coefficients is individually statistically significant at conventional levels. We do not interpret this result as evidence of exact balance; rather, we do not detect systematic differences in these observed pre-policy characteristics within the matched border-city sample.

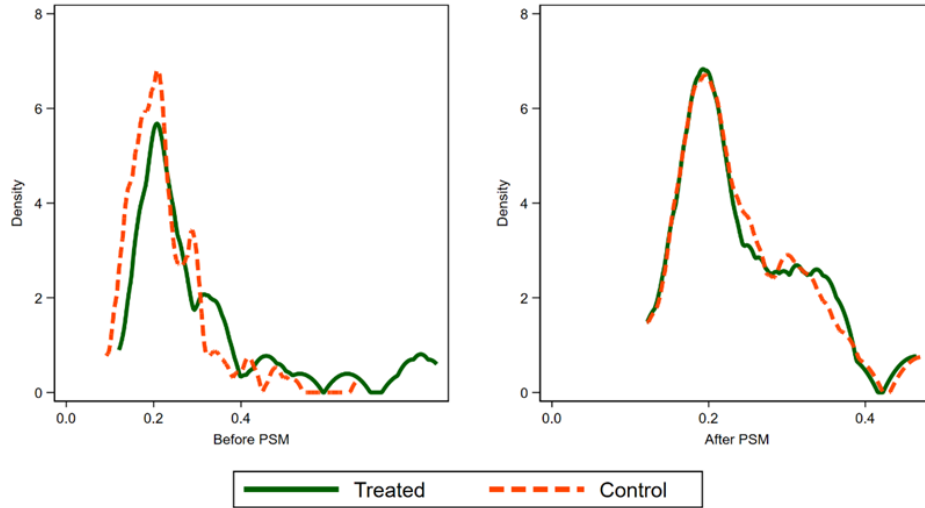


Figure D1: Propensity-score distributions before and after matching

Notes: The left and right panels show the distributions of estimated propensity scores for treated cities (green) and comparison cities (orange) before and after propensity-score matching, respectively. Greater overlap after matching is a diagnostic for common support in the estimated score; it does not imply exact covariate balance.

Table D1: Balance test in the matched border-city sample

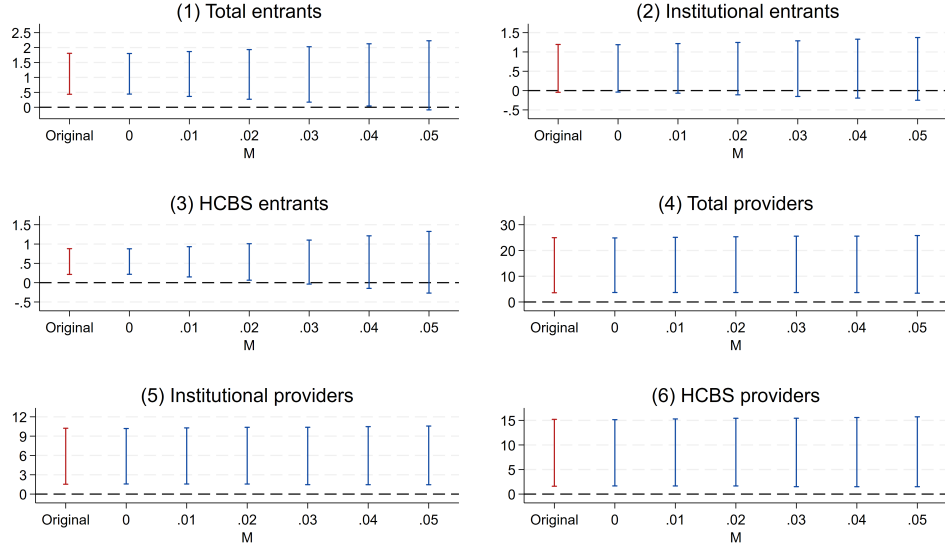
|                                     | Treatment indicator |
|-------------------------------------|---------------------|
| Population share aged 65 and over   | -1.696<br>(5.258)   |
| Dependency rate                     | -0.043<br>(0.173)   |
| GDP per capita                      | 0.109<br>(0.099)    |
| Fiscal expenditure per capita       | -0.601<br>(0.625)   |
| Hospitals per 10,000 population     | 0.062<br>(0.042)    |
| Hospital beds per 10,000 population | -0.000<br>(0.001)   |
| Matched border-pair fixed effects   | Yes                 |
| Observations                        | 1,880               |
| Cities                              | 47                  |

Notes: The dependent variable is an indicator for LTCI pilot status. All covariates are measured in 2010 and use the definitions and units in Table 1, Panel D. The regression includes matched border-pair fixed effects. Matching is one-to-one with replacement; because several treated cities share the same nearest eligible control, the estimation sample contains 1,880 unique city-quarter observations. Standard errors clustered at the city level are in parentheses.

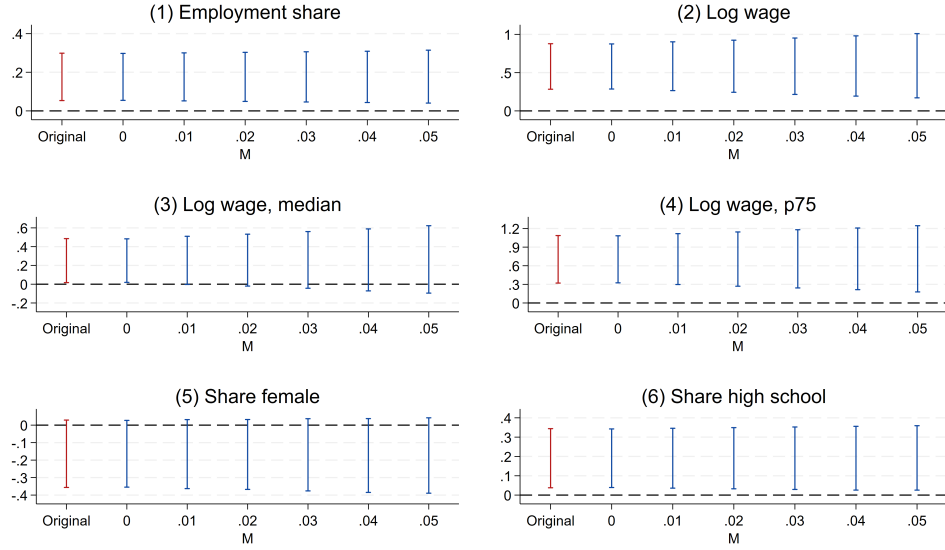
### D.3. Sensitivity Check of Violations to the Parallel Trends Assumption

Rambachan and Roth (2023) introduce a method for assessing violations of the parallel-trends assumption and evaluating their effects on point estimates and confidence intervals. The approach has two steps. First, it defines  $\Delta$ , the set of possible deviations from parallel trends. Second, it constructs confidence intervals for the parameter of interest that account for these deviations.

For our main parameter of interest—the overall treatment effect of the LTCI pilot reported in Table 2—we compute confidence intervals that allow deviations from linearity up to a parameter  $M$ . The trend is represented by  $\delta$ , and  $\Delta^{SD}(M) = \{\delta : |(\delta_{t+1} - \delta_t) - (\delta_t - \delta_{t-1})| \leq M, \forall t\}$ . The parameter  $M$  ranges from zero (indicating linear pre-trends) to one-half of the standard error of the coefficient of interest. Figure D2 reports confidence intervals for  $\theta_W^O$  (Definition D1).



(a) LTC provider



(b) LTC workers

Figure D2: Robustness of the LTCI effect: HonestDiD sensitivity analysis

Notes: This figure reports the sensitivity of the overall treatment-effect estimates to violations of parallel trends, following [Rambachan and Roth \(2023\)](#). Panel (a) reports results for LTC provider entry and Panel (b) reports results for LTC workers. The horizontal axis,  $\bar{M}$ , indexes the allowed deviation from a linear pre-trend. The red series shows the original confidence intervals reported in [Tables 2 and 3](#); the blue series show robust confidence intervals constructed for each value of  $\bar{M}$ .

#### D.4. Supplementary Tables and Figures

Table D2: Alternative Control Group Specifications

|                                         | Net entry           |                     |                   | Providers           |                    |                     |
|-----------------------------------------|---------------------|---------------------|-------------------|---------------------|--------------------|---------------------|
|                                         | (1)                 | (2)                 | (3)               | (4)                 | (5)                | (6)                 |
|                                         | All                 | HCBS                | Institutional     | All                 | HCBS               | Institutional       |
| <b>Panel A: Benchmark</b>               |                     |                     |                   |                     |                    |                     |
| ATT                                     | 0.967***<br>(0.318) | 0.547***<br>(0.180) | 0.421<br>(0.297)  | 12.597**<br>(5.197) | 7.258**<br>(3.474) | 5.339***<br>(1.939) |
| Observations                            | 10,600              | 10,600              | 10,600            | 10,600              | 10,600             | 10,600              |
| Cities                                  | 265                 | 265                 | 265               | 265                 | 265                | 265                 |
| <b>Panel B: PSM</b>                     |                     |                     |                   |                     |                    |                     |
| ATT                                     | 0.951**<br>(0.370)  | 0.402**<br>(0.187)  | 0.548*<br>(0.300) | 11.064<br>(7.163)   | 7.200<br>(4.971)   | 3.864<br>(2.415)    |
| Observations                            | 2,440               | 2,440               | 2,440             | 2,440               | 2,440              | 2,440               |
| Cities                                  | 61                  | 61                  | 61                | 61                  | 61                 | 61                  |
| <b>Panel C: PSM among Border Cities</b> |                     |                     |                   |                     |                    |                     |
| ATT                                     | 1.478***<br>(0.491) | 0.650***<br>(0.245) | 0.827*<br>(0.478) | 6.797<br>(4.702)    | 2.458<br>(2.637)   | 4.339**<br>(2.202)  |
| Observations                            | 1,880               | 1,880               | 1,880             | 1,880               | 1,880              | 1,880               |
| Cities                                  | 47                  | 47                  | 47                | 47                  | 47                 | 47                  |

Notes: This table reports estimated effects of LTCI on provider entry under alternative comparison-group specifications using the estimator of [Callaway and Sant’Anna \(2021\)](#). Data are from NECIPS (2010–2019). The dependent variables in Columns (1)–(3) are quarterly net-entry counts; Columns (4)–(6) report active provider stocks. Panel A reproduces the benchmark estimates. Panel B uses a sample constructed through propensity-score matching on 2010 city characteristics. Under the common-support and caliper restrictions, Panel C matches 23 treated cities with 24 control cities, each observed for 40 quarters, yielding 1,880 city-quarter observations. Panel C includes year fixed effects and matched-border-group-specific linear time trends; standard errors are clustered at the matched-border-group level. Standard errors in Panels A and B are clustered at the city level. \*\*\*, \*\*, and \* denote significance at the 1%, 5%, and 10% levels, respectively.

Table D3: Additional robustness checks, alternative specification, providers

|                                                                                      | Net entry           |                     |                  | Providers            |                      |                     |
|--------------------------------------------------------------------------------------|---------------------|---------------------|------------------|----------------------|----------------------|---------------------|
|                                                                                      | (1)                 | (2)                 | (3)              | (4)                  | (5)                  | (6)                 |
|                                                                                      | All                 | HCBS                | Institutional    | All                  | HCBS                 | Institutional       |
| <b>Panel A: Controlling for province-specific linear time trend</b>                  |                     |                     |                  |                      |                      |                     |
| ATT                                                                                  | 0.986**<br>(0.383)  | 0.752***<br>(0.274) | 0.234<br>(0.327) | 14.795**<br>(5.748)  | 9.087**<br>(3.757)   | 5.707**<br>(2.258)  |
| Observations                                                                         | 10,600              | 10,600              | 10,600           | 10,600               | 10,600               | 10,600              |
| Cities                                                                               | 265                 | 265                 | 265              | 265                  | 265                  | 265                 |
| <b>Panel B: Controlling for province-by-year fixed effect</b>                        |                     |                     |                  |                      |                      |                     |
| ATT                                                                                  | 0.737**<br>(0.336)  | 0.418**<br>(0.211)  | 0.319<br>(0.303) | 14.105***<br>(5.190) | 8.812**<br>(3.501)   | 5.293***<br>(1.935) |
| Observations                                                                         | 10,600              | 10,600              | 10,600           | 10,600               | 10,600               | 10,600              |
| Cities                                                                               | 265                 | 265                 | 265              | 265                  | 265                  | 265                 |
| <b>Panel C: Controlling for city-by-year fixed effects and quarter fixed effects</b> |                     |                     |                  |                      |                      |                     |
| ATT                                                                                  | 1.137***<br>(0.372) | 1.114***<br>(0.192) | 0.023<br>(0.304) | 17.872***<br>(4.658) | 11.230***<br>(3.109) | 6.643***<br>(1.872) |
| Observations                                                                         | 10,600              | 10,600              | 10,600           | 10,600               | 10,600               | 10,600              |
| Cities                                                                               | 265                 | 265                 | 265              | 265                  | 265                  | 265                 |

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 fixed effects. Standard errors, clustered at the city level, are reported in parentheses. \*\*\*, \*\*, and \* denote significance at the 1%, 5%, and 10% levels, respectively.

Table D4: Robustness to limited anticipation: provider outcomes

|                                                                  | Net entry           |                     |                     | Providers           |                    |                     |
|------------------------------------------------------------------|---------------------|---------------------|---------------------|---------------------|--------------------|---------------------|
|                                                                  | All                 | HCBS                | Institutional       | All                 | HCBS               | Institutional       |
| <b>Panel A: Baseline</b>                                         |                     |                     |                     |                     |                    |                     |
| ATT                                                              | 0.967***<br>(0.318) | 0.547***<br>(0.180) | 0.421<br>(0.297)    | 12.597**<br>(5.197) | 7.258**<br>(3.474) | 5.339***<br>(1.939) |
| Observations                                                     | 10,600              | 10,600              | 10,600              | 10,600              | 10,600             | 10,600              |
| Cities                                                           | 265                 | 265                 | 265                 | 265                 | 265                | 265                 |
| <b>Panel B: Excluding cohorts first treated in 2020 or later</b> |                     |                     |                     |                     |                    |                     |
| ATT                                                              | 1.023***<br>(0.322) | 0.570***<br>(0.182) | 0.454<br>(0.297)    | 13.183**<br>(5.210) | 7.508**<br>(3.482) | 5.675***<br>(1.941) |
| Observations                                                     | 10,200              | 10,200              | 10,200              | 10,200              | 10,200             | 10,200              |
| <b>Panel C: Allowing one-quarter anticipation</b>                |                     |                     |                     |                     |                    |                     |
| ATT                                                              | 1.545***<br>(0.543) | 0.857**<br>(0.341)  | 0.688**<br>(0.338)  | 14.880**<br>(6.139) | 8.421**<br>(4.140) | 6.460***<br>(2.256) |
| Observations                                                     | 10,200              | 10,200              | 10,200              | 10,200              | 10,200             | 10,200              |
| Cities                                                           | 255                 | 255                 | 255                 | 255                 | 255                | 255                 |
| Anticipation-effect $p$ -value                                   | 0.388               | 0.619               | 0.390               | 0.120               | 0.284              | 0.061               |
| <b>Panel D: Allowing two-quarter anticipation</b>                |                     |                     |                     |                     |                    |                     |
| ATT                                                              | 1.703**<br>(0.706)  | 0.921*<br>(0.518)   | 0.782***<br>(0.251) | 16.067**<br>(6.723) | 9.051*<br>(4.633)  | 7.016***<br>(2.418) |
| Observations                                                     | 10,200              | 10,200              | 10,200              | 10,200              | 10,200             | 10,200              |
| Cities                                                           | 255                 | 255                 | 255                 | 255                 | 255                | 255                 |
| Anticipation-effect $p$ -value                                   | 0.646               | 0.819               | 0.619               | 0.207               | 0.537              | 0.094               |

Notes: Variables and the sample period (2011–2019) are defined as in Table 2. Panel A reproduces the estimates from Table 2. In Panels B–D, we exclude from the control group cities that implemented LTCI in 2020 or later (10 cities in total). Panels C and D additionally allow for anticipation effects one and two quarters before actual implementation, respectively. The anticipation-effect  $p$ -values correspond to tests of whether the effect during the anticipation window is zero. 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, Labor market

|                                                                  | (1)                | (2)                 | (3)                | (4)                 | (5)                 | (6)                |
|------------------------------------------------------------------|--------------------|---------------------|--------------------|---------------------|---------------------|--------------------|
|                                                                  | Employment share   | Log wage            | Log wage, median   | Log wage, p75       | Female share        | Share high school  |
| <b>Panel A: Baseline</b>                                         |                    |                     |                    |                     |                     |                    |
| ATT                                                              | 0.148**<br>(0.068) | 0.478***<br>(0.136) | 0.280**<br>(0.116) | 0.602***<br>(0.191) | −0.285**<br>(0.125) | 0.178**<br>(0.084) |
| Observations                                                     | 1,563              | 974                 | 974                | 974                 | 1,473               | 1,473              |
| Cities                                                           | 265                | 259                 | 259                | 259                 | 262                 | 262                |
| <b>Panel B: Excluding cohorts first treated in 2020 or later</b> |                    |                     |                    |                     |                     |                    |
| ATT                                                              | 0.151**<br>(0.069) | 0.482***<br>(0.137) | 0.284**<br>(0.117) | 0.607***<br>(0.192) | −0.286**<br>(0.125) | 0.182**<br>(0.084) |
| Observations                                                     | 1503               | 936                 | 936                | 936                 | 1417                | 1417               |
| Cities                                                           | 255                | 249                 | 249                | 249                 | 252                 | 252                |
| <b>Panel C: Allowing for one-year anticipation</b>               |                    |                     |                    |                     |                     |                    |
| ATT                                                              | 0.036<br>(0.064)   | 0.101<br>(0.177)    | 0.105<br>(0.107)   | 0.169<br>(0.249)    | 0.022<br>(0.087)    | 0.022<br>(0.050)   |
| Observations                                                     | 1500               | 915                 | 915                | 915                 | 1423                | 1423               |
| Cities                                                           | 254                | 244                 | 244                | 244                 | 253                 | 253                |
| Anticipation-effect $p$ -value                                   | 0.411              | 0.317               | 0.507              | 0.556               | 0.413               | 0.576              |
| <b>Panel D: Allowing for two-year anticipation</b>               |                    |                     |                    |                     |                     |                    |
| ATT                                                              | 0.032<br>(0.042)   | 0.347<br>(0.236)    | 0.310<br>(0.217)   | 0.390<br>(0.299)    | −0.086<br>(0.108)   | 0.022<br>(0.082)   |
| Observations                                                     | 1507               | 1125                | 1125               | 1125                | 1429                | 1429               |
| Cities                                                           | 255                | 240                 | 240                | 240                 | 254                 | 254                |
| Anticipation-effect $p$ -value                                   | 0.694              | 0.111               | 0.221              | 0.271               | 0.748               | 0.890              |

Notes: Variables and the sample period (2013–2018) are defined as in Table 3. Panel A reproduces the estimates in Table 3. In Panels B–D, we remove from the control group the 10 cities that implemented LTCI in 2020 or later. Panels C and D allow for anticipation one and two years before policy implementation, respectively. The anticipation-effect  $p$ -values test whether the effect during the anticipation window is zero. Standard errors clustered at the city level are reported in parentheses. \*\*\*, \*\*, and \* denote significance at the 1%, 5%, and 10% levels, respectively.

Table D6: Alternative control-group specifications: labor-market outcomes

|                                                 | Employment<br>share | Log mean<br>wage    | Log median<br>wage  | Log p75<br>wage     | Female<br>share     | High-school<br>share |
|-------------------------------------------------|---------------------|---------------------|---------------------|---------------------|---------------------|----------------------|
| <b>Panel A: Baseline</b>                        |                     |                     |                     |                     |                     |                      |
| ATT                                             | 0.148**<br>(0.068)  | 0.478***<br>(0.136) | 0.280**<br>(0.116)  | 0.602***<br>(0.191) | -0.285**<br>(0.125) | 0.178**<br>(0.084)   |
| Dependent-variable mean                         | 0.224               | 2.771               | 2.612               | 2.942               | 0.512               | 0.247                |
| Observations                                    | 1,563               | 974                 | 974                 | 974                 | 1,473               | 1,473                |
| Cities                                          | 265                 | 259                 | 259                 | 259                 | 262                 | 262                  |
| <b>Panel B: Propensity-score matched sample</b> |                     |                     |                     |                     |                     |                      |
| ATT                                             | 0.159**<br>(0.064)  | 0.485***<br>(0.145) | 0.282***<br>(0.093) | 0.476**<br>(0.188)  | -0.379**<br>(0.185) | 0.224*<br>(0.131)    |
| Dependent-variable mean                         | 0.240               | 2.807               | 2.646               | 2.967               | 0.506               | 0.258                |
| Observations                                    | 411                 | 262                 | 263                 | 263                 | 398                 | 398                  |
| Cities                                          | 70                  | 68                  | 68                  | 68                  | 69                  | 69                   |
| <b>Panel C: Matched border-city sample</b>      |                     |                     |                     |                     |                     |                      |
| ATT                                             | 0.060<br>(0.083)    | 0.300<br>(0.243)    | 0.178<br>(0.223)    | 0.323*<br>(0.193)   | -0.333<br>(0.291)   | 0.024<br>(0.115)     |
| Dependent-variable mean                         | 0.240               | 2.757               | 2.600               | 2.925               | 0.523               | 0.259                |
| Observations                                    | 344                 | 219                 | 218                 | 217                 | 327                 | 327                  |
| Cities                                          | 59                  | 57                  | 57                  | 57                  | 58                  | 58                   |

Notes: The table reports overall average treatment effects estimated using [Callaway and Sant'Anna \(2021\)](#). Outcomes are city-year CMDS measures defined as in Table 3; the employment share is in levels. Panel B uses propensity-score matching on 2010 city characteristics. Panel C restricts controls to matched neighboring cities and includes matched border-pair-specific linear trends. Standard errors clustered at the city level are in parentheses. \*\*\*, \*\*, and \* denote significance at the 1, 5, and 10 percent levels, respectively.

Table D7: Balanced-cohort event-study estimates: provider entry and stock

| Event time           | Net entry          |                    |                   | Providers           |                     |                     |
|----------------------|--------------------|--------------------|-------------------|---------------------|---------------------|---------------------|
|                      | All                | HCBS               | Institutional     | All                 | HCBS                | Institutional       |
| -8                   | 0.710<br>(0.719)   | 0.665<br>(0.520)   | 0.045<br>(0.237)  | 1.090<br>(0.871)    | 0.942<br>(0.705)    | 0.147<br>(0.174)    |
| -7                   | -0.662<br>(0.659)  | -0.727<br>(0.539)  | 0.065<br>(0.178)  | 0.428<br>(0.357)    | 0.216<br>(0.291)    | 0.212<br>(0.138)    |
| -6                   | 0.111<br>(0.488)   | 0.325<br>(0.460)   | -0.214<br>(0.156) | 0.539<br>(0.688)    | 0.541<br>(0.642)    | -0.002<br>(0.079)   |
| -5                   | 1.137**<br>(0.576) | 0.828<br>(0.530)   | 0.309<br>(0.205)  | 1.676<br>(1.043)    | 1.369<br>(0.995)    | 0.307*<br>(0.182)   |
| -4                   | -0.102<br>(0.570)  | -0.073<br>(0.594)  | -0.029<br>(0.139) | 1.574**<br>(0.645)  | 1.296***<br>(0.499) | 0.278<br>(0.251)    |
| -3                   | -0.324<br>(0.499)  | -0.484<br>(0.363)  | 0.160<br>(0.361)  | 1.250**<br>(0.595)  | 0.811**<br>(0.412)  | 0.439<br>(0.287)    |
| -2                   | 0.289<br>(0.423)   | 0.164<br>(0.540)   | 0.125<br>(0.334)  | 1.539*<br>(0.828)   | 0.975<br>(0.720)    | 0.564<br>(0.347)    |
| -1                   | 0.642<br>(0.718)   | 0.455<br>(0.441)   | 0.187<br>(0.483)  | 2.181<br>(1.420)    | 1.430<br>(0.965)    | 0.750<br>(0.545)    |
| 0                    | 0.394<br>(0.494)   | 0.083<br>(0.358)   | 0.311<br>(0.479)  | 2.575*<br>(1.454)   | 1.513<br>(1.179)    | 1.062**<br>(0.429)  |
| 1                    | -0.926<br>(0.728)  | -0.823<br>(0.580)  | -0.103<br>(0.288) | 3.841*<br>(2.196)   | 2.130<br>(1.621)    | 1.711**<br>(0.797)  |
| 2                    | 0.316<br>(0.452)   | 0.481**<br>(0.225) | -0.165<br>(0.395) | 6.320*<br>(3.705)   | 4.024<br>(2.646)    | 2.295*<br>(1.206)   |
| 3                    | -0.305<br>(0.409)  | -0.288<br>(0.289)  | -0.016<br>(0.298) | 8.325<br>(5.134)    | 5.256<br>(3.640)    | 3.069*<br>(1.624)   |
| 4                    | 0.494<br>(0.413)   | 0.355<br>(0.395)   | 0.139<br>(0.350)  | 11.051<br>(6.851)   | 7.086<br>(4.919)    | 3.965*<br>(2.128)   |
| 5                    | 1.558*<br>(0.812)  | 0.902<br>(0.668)   | 0.656<br>(0.626)  | 14.759*<br>(8.853)  | 9.394<br>(6.417)    | 5.365**<br>(2.662)  |
| 6                    | 2.092**<br>(0.964) | 0.866*<br>(0.486)  | 1.227*<br>(0.709) | 19.060*<br>(10.479) | 11.712<br>(7.394)   | 7.347**<br>(3.341)  |
| 7                    | 1.595**<br>(0.649) | 0.587<br>(0.397)   | 1.008<br>(0.686)  | 22.863*<br>(12.189) | 13.751*<br>(8.138)  | 9.111**<br>(4.275)  |
| 8                    | 0.544<br>(0.727)   | 0.410<br>(0.332)   | 0.134<br>(0.615)  | 25.615*<br>(13.352) | 15.614*<br>(8.968)  | 10.001**<br>(4.633) |
| Treated cities       | 17                 | 17                 | 17                | 17                  | 17                  | 17                  |
| Never-treated cities | 229                | 229                | 229               | 229                 | 229                 | 229                 |

Notes: The table reports event-time effects estimated using [Callaway and Sant'Anna \(2021\)](#) for a fixed-composition sample observed from eight quarters before through eight quarters after LTCI implementation. The sample includes 17 treated cities from six treatment cohorts and 229 never-treated cities. Event time is measured in quarters. Pre-treatment estimates, including  $t = -1$ , are pseudo-ATTs under the varying-base-period construction and are not normalized to zero. Standard errors clustered at the city level are reported in parentheses. \*\*\*, \*\*, and \* denote significance at the 1%, 5%, and 10% levels, respectively.

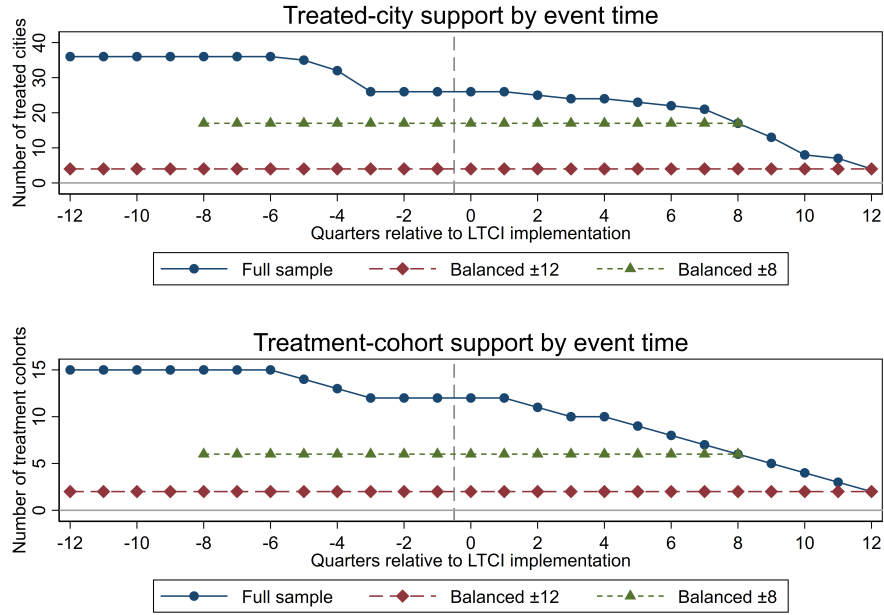


Figure D3: Treatment support across provider event time

Notes: The figure shows identifying support for the unrestricted provider event study. Event time is measured in quarters relative to LTCI implementation. The upper panel reports the number of treated cities, and the lower panel reports the number of treatment cohorts contributing to each event-time estimate. The comparison group consists of 229 never-treated cities. Identifying support declines from 26 treated cities in 11 cohorts at implementation to 17 cities in six cohorts at quarter 8 and four cities in two cohorts at quarter 12.

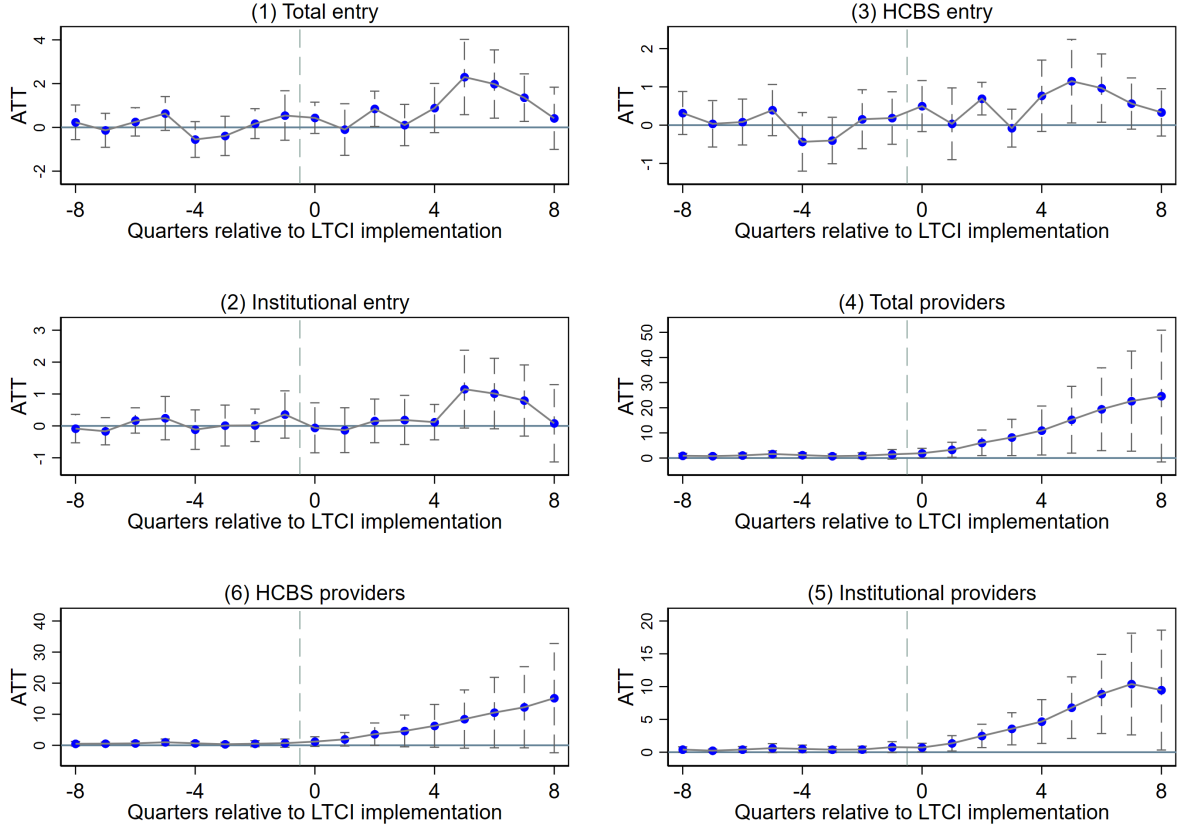


Figure D4: Provider entry and stocks: an alternative event window

Notes: The figure reports event-time effects estimated using [Callaway and Sant'Anna \(2021\)](#) for a fixed-composition sample observed from eight quarters before through eight quarters after LTCI implementation. Panels (a)–(c) report net entry for all, HCBS, and institutional providers; Panels (d)–(f) report the corresponding active provider stocks. The sample includes 17 treated cities from six treatment cohorts and 229 never-treated cities. Pre-treatment estimates, including  $t = -1$ , are pseudo-ATTs under the varying-base-period construction and are not normalized to zero. Points are estimates and bars are 95% confidence intervals based on standard errors clustered at the city level.



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.

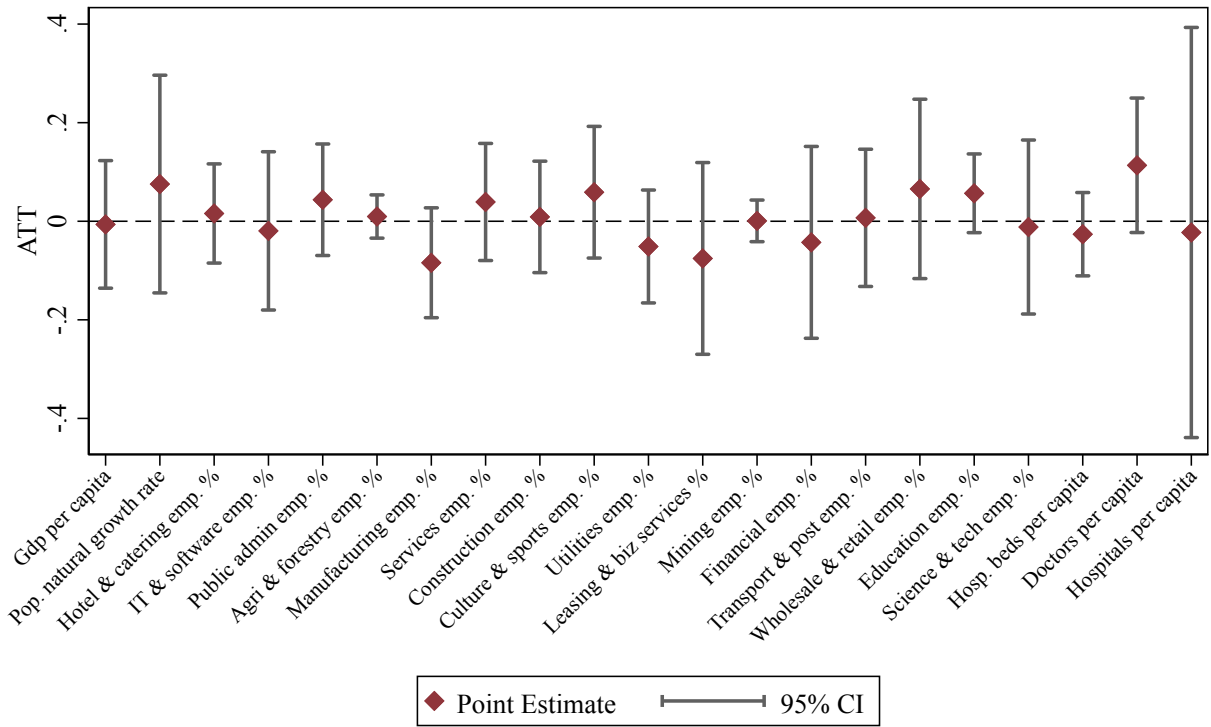
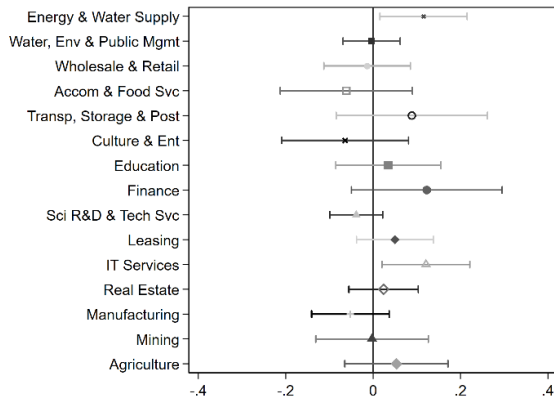
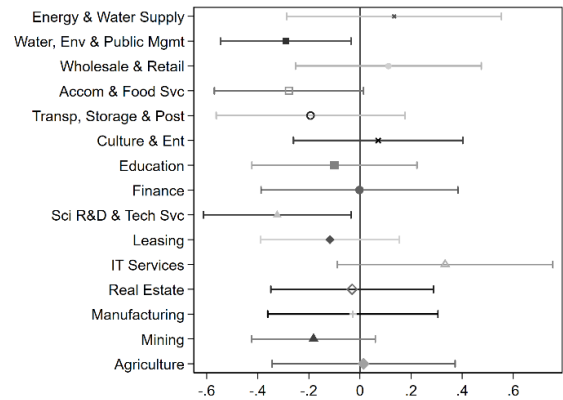


Figure D6: Cross-industry falsification and spillover effects

Notes: This figure presents estimated ATTs of the LTCI pilot for a set of standardized outcomes ( $z$ -scores), using the estimator of [Callaway and Sant'Anna \(2021\)](#). The outcomes include a city-level demographic measure (natural population growth), an economic indicator (GDP per capita), service-sector employment as a share of total employment, and employment shares in non-LTC industries (e.g., manufacturing, mining, and financial services). The LTC employment share used in the main analysis is measured relative to total service-sector employment; the service-sector and comparison-industry shares reported here are measured relative to total employment. Points are estimates, and bars are 95% confidence intervals based on standard errors clustered at the city level. Data come from the *China City Statistical Yearbook* and annual local statistical bulletins.



(a) Net entry



(b) Provider stocks

Figure D7: Effects of LTCI on providers in non-LTC industries

Notes: The figure reports overall average treatment effects estimated using [Callaway and Sant'Anna \(2021\)](#) on city-quarter establishment-registration outcomes. Panel (a) reports effects on net entry, and Panel (b) reports effects on provider stocks for industries outside the LTC sector. Points are estimates and horizontal bars are 95% confidence intervals based on standard errors clustered at the city level; the vertical line marks zero. The estimates show no consistent sign or pattern across industries.

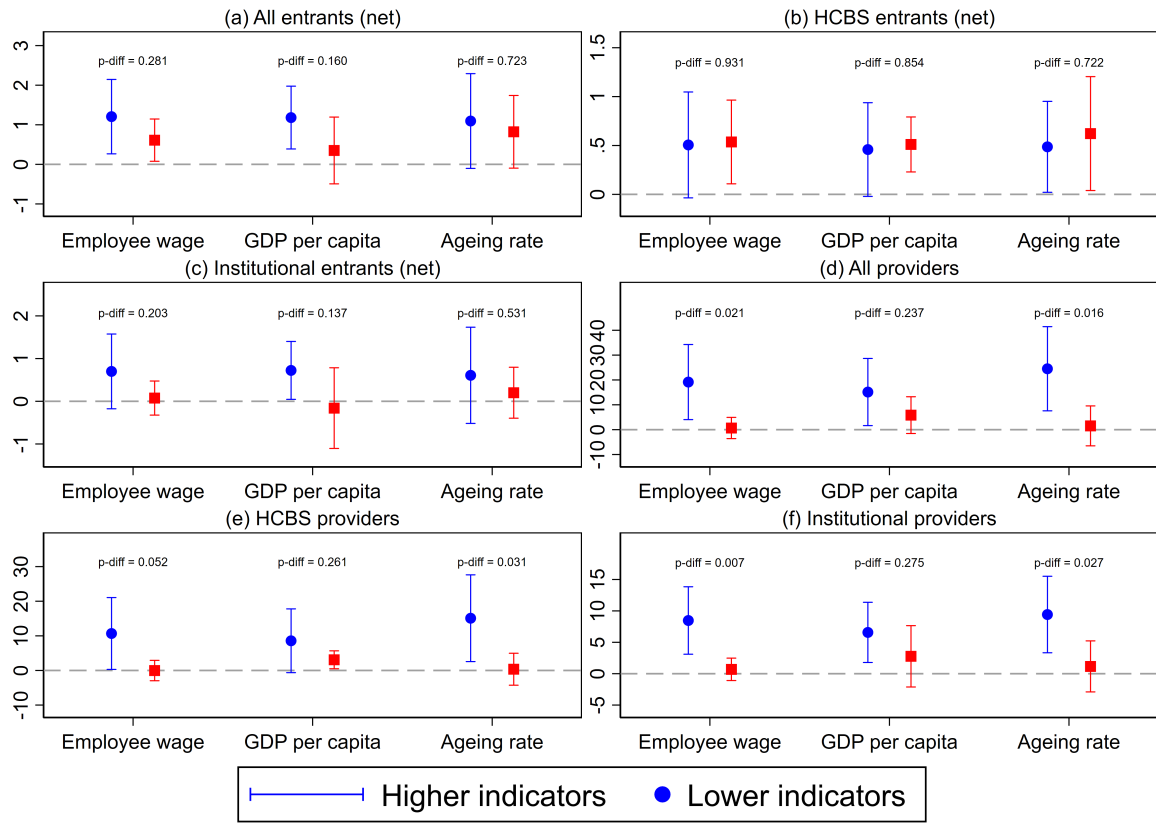


Figure D8: Provider heterogeneity by pre-policy city characteristics

Notes: The figure reports subgroup average treatment effects estimated using Callaway and Sant'Anna (2021) with NECIPS data for 2010–2019. Cities are split at the sample median of pre-policy GDP per capita, employee wages, or the share of the population aged 65 and older. Points are subgroup estimates, and bars are 95% confidence intervals based on standard errors clustered at the city level. Reported  $p$ -values test equality of the two subgroup effects for each outcome.

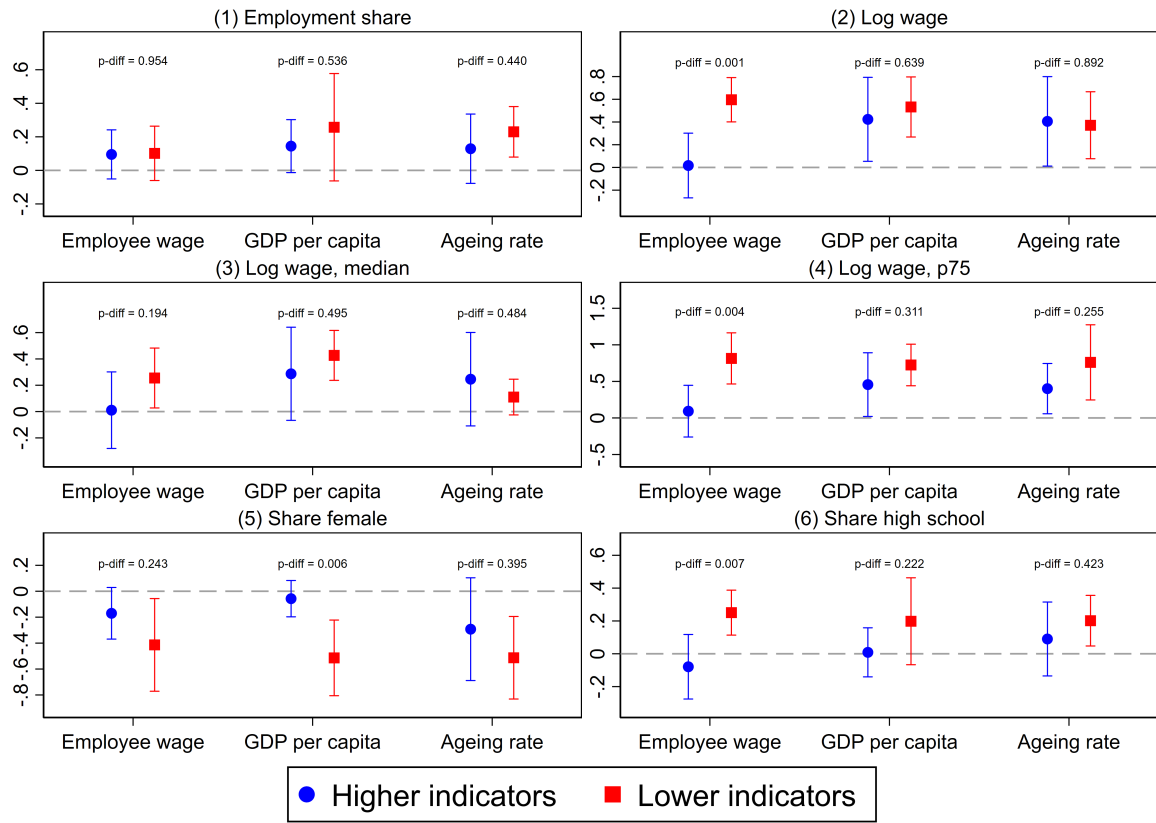


Figure D9: Labor-market heterogeneity by pre-policy city characteristics

Notes: The figure reports subgroup average treatment effects estimated using Callaway and Sant'Anna (2021) with city-year CMDS outcomes for 2013–2018. Cities are split at the sample median of pre-policy employee wages, GDP per capita, or the share of the population aged 65 and older. Panels report the LTC employment share in levels, mean log wage, median log wage, 75th-percentile log wage, female share, and high-school share. Points are subgroup estimates, and bars are 95% confidence intervals based on standard errors clustered at the city level. Reported  $p$ -values test equality of the effects for the higher- and lower-value groups for each outcome.

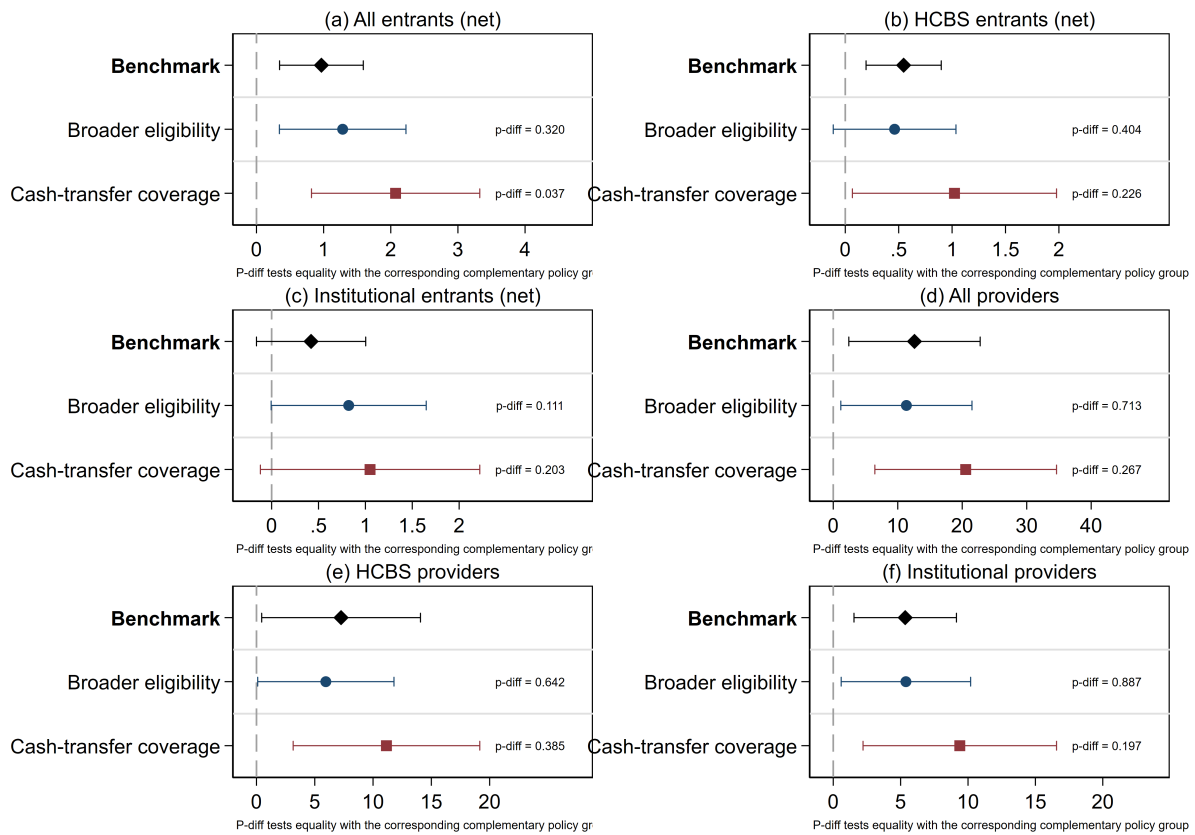


Figure D10: Provider heterogeneity by LTCI policy design

Notes: The figure reports subgroup average treatment effects estimated using Callaway and Sant'Anna (2021) with NECIPS data for 2010–2019. Subgroups are defined by whether LTCI covers residents outside UEBMI and whether the pilot permits cash benefits. Points are subgroup estimates and bars are 95% confidence intervals based on standard errors clustered at the city level. Reported  $p$ -values test equality of the two subgroup effects for each outcome.

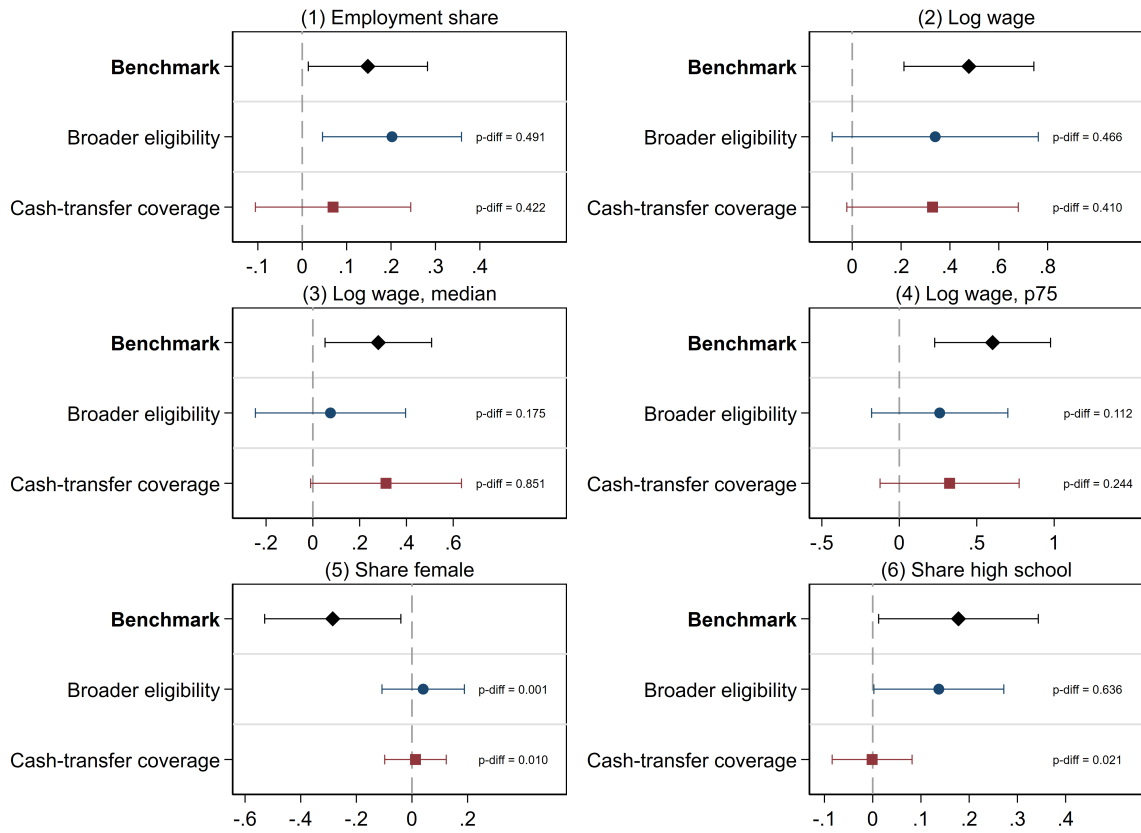
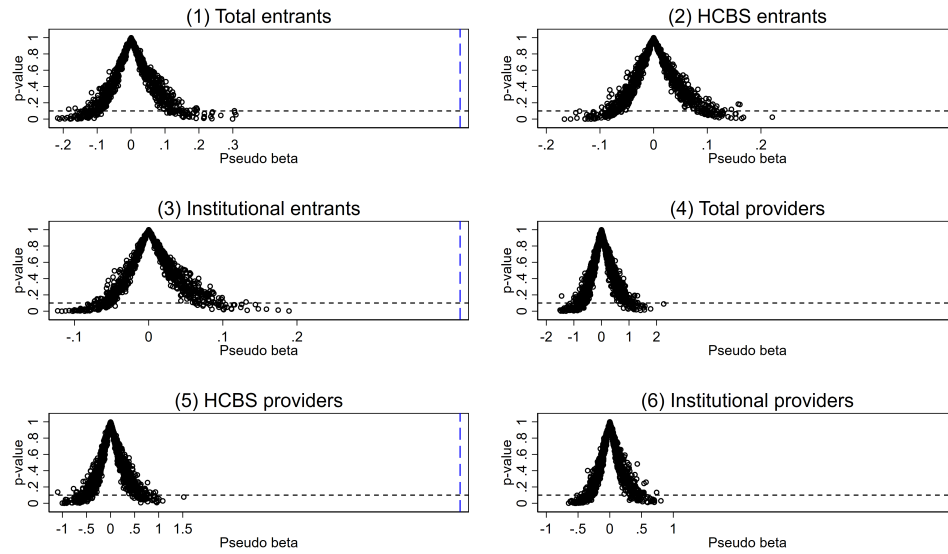
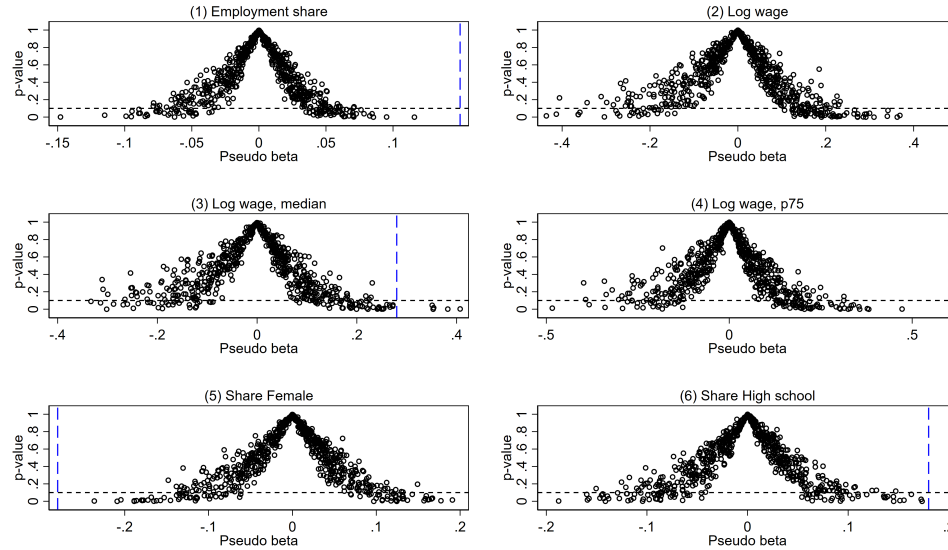


Figure D11: Labor-market heterogeneity by LTCI policy design

Notes: The figure reports subgroup average treatment effects estimated using [Callaway and Sant'Anna \(2021\)](#) with city-year CMDS outcomes for 2013–2018. The benchmark estimate uses all eligible comparison cities; the two policy-design estimates correspond to pilots with broader eligibility and those with cash-transfer coverage. Panels report the LTC employment share in levels, mean log wage, median log wage, 75th-percentile log wage, female share, and high-school share. Points are estimates and bars are 95% confidence intervals based on standard errors clustered at the city level. Reported  $p$ -values test equality between broader- and narrower-eligibility pilots and between cash- and non-cash-transfer pilots for each outcome.



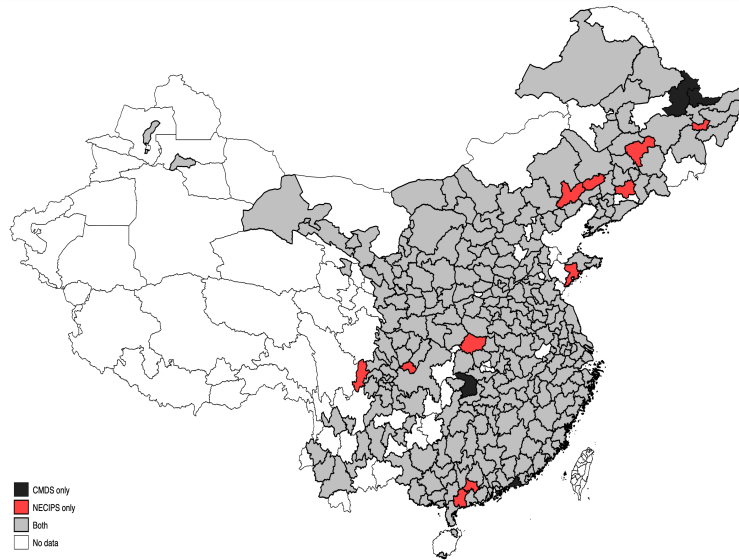
(a) LTC provider



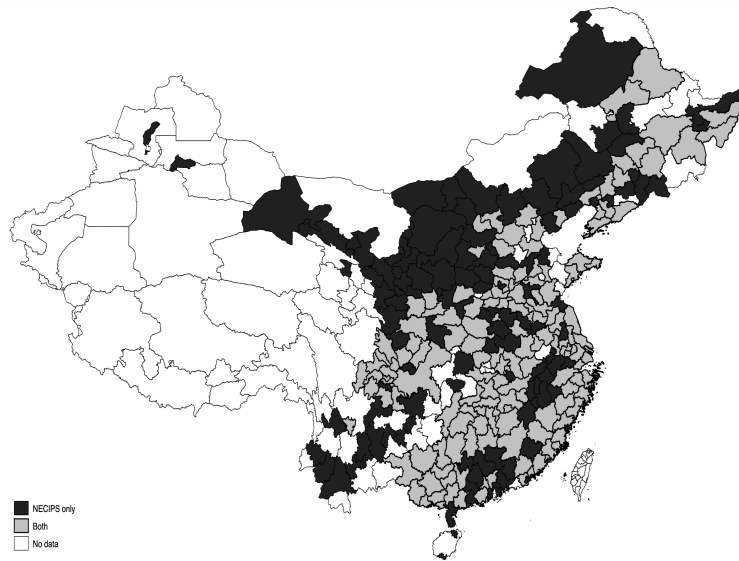
(b) LTC workers

Figure D12: Pseudo-treatment versus actual pilot

Notes: This figure shows pseudo-treatment point estimates on the horizontal axis and their associated  $p$ -values on the vertical axis. The estimates are obtained from 1,000 placebo samples generated by randomly assigning treatment status and timing across cities. The dashed lines indicate the 10% significance threshold.



(a) CMDS and NECIPS



(b) NECIPS and CLHLS

Figure D13: 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.