The effects of public long-term care insurance in China on adult children's labor supply

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Abstract

This study examines how China's public long-term care insurance (LTCI) affects adult children's labor supply. Leveraging staggered city-level policy implementation and variation in older parents' eligibility, I employ a triple-differences framework. I find that parental LTCI eligibility increases adult children's probability of working by 3.9 percentage points, with effects concentrated among daughters. However, LTCI does not increase the probability of working among children whose parents have severe health problems during the COVID-19 pandemic. Evidence on mechanisms suggests that adult children spend less time providing care while offering more financial support to parents. Transfers from parents to children also increase, although this effect weakens during the pandemic. Regarding policy design, compared with strictly in-kind LTCI, programs combining in-kind and cash benefits lead to larger increases in adult children's probability of working and greater reductions in caregiving time, but lower life satisfaction among parents.

1 Introduction

The global population of older adults is growing rapidly. According to the World Population Prospects 2020 Revision, individuals aged 60 and above are projected to comprise 16 percent of the global population by 2030 and approximately 20 percent by 2050 (Yang et al., 2023; Ai et al., 2024). As the increase in longevity, the prevalence of functional limitations also rises, as older adults are more likely to experience functional limitations: evidence from OECD (2019) shows that 50 percent of people aged 65 and over in OECD countries report some limitations in daily activities, with 17 percent experiencing severe limitations. Together, population aging and rising disability imply a sustained increase in the demand for long-term care (LTC).

Meeting this growing demand of LTC imposes substantial financial and time costs on households. As indicated by previous research, the cost of LTC ranges from one-half to five times the median disposable income of individuals aged 65 and over in OECD countries (OECD, 2020). Formal LTC services are particularly expensive, for instance, in the United States, LTC expenditures reached $379 billion in 2018 (Mommaerts, 2025). In China, the average LTC cost amounts to 26 to 37 percent of GDP per capita (İmrohoroğlu and Zhao, 2018). As a result, many households turn to informal care, typically provided by family members such as spouses or children. However, informal care imposes implicit costs, including lost wages due to time spent caregiving, reduced tax revenues, and adverse effects on caregivers' physical and mental health.

These burdens raise a central policy question: can public long-term care insurance (LTCI) reduce reliance on family caregiving and thereby alter caregivers' time allocation? This study examines how parents' eligibility for China's public LTCI program affects the labor supply of their adult children. [1] China provides an important setting for this question because rapid population aging, the large generation of only children, and strong norms of intergenerational support have intensified the tension between paid work and family caregiving. The staggered rollout of LTCI provides policy variation for estimating these effects. The central government selected 15 pilot cities in 2016 and another 14 in 2020, while some local governments had introduced their own programs earlier.

Economic theory predicts that public LTCI may affect adult children's labor supply through both substitution and income effects. The substitution effect arises when formal care replaces care previously provided by adult children, relaxing their time constraints and allowing them to reallocate time from caregiving to market work. LTCI may also affect labor supply ex ante. By insuring parents' future access to formal care, the program reduces adult children's expected caregiving burden and may make it easier for them to work or live farther from their parents. In contrast, the income effect may reduce labor supply because LTCI benefits lower expected out-of-pocket care expenditures and increase the household's disposable resources. The overall effect on adult children's labor supply is therefore theoretically ambiguous.

[1] TLTCI eligibility generally requires enrollment in public medical insurance and functional limitations in activities of daily living (ADLs) or instrumental activities of daily living (IADLs) lasting at least six months. The program is financed through existing health insurance funds rather than new taxes. Most pilot cities provide home-based or institutional care, with benefits typically covering 70% to 90% of care expenditures. By 2018, the average benefit was approximately 1.5 times the median annual income of older households in China (Liu et al., 2023).

For identification, since the timing and location of LTCI adoption may be correlated with city-specific time-varying factors, I employ a triple-differences design by leveraging variation along three dimensions: the staggered implementation of LTCI across cities, differences in insurance-based eligibility within cities, and changes over time. In cities without an LTCI program, I classify households with at least one parent enrolled in UEBMI as potentially eligible, consistent with the eligibility rule used in most pilot cities.

My result shows that parental LTCI eligibility increases the likelihood of adult children's labor supply statistically significant by 3.9 percentage points (standard error = 0.018), which is about 4.4% of the control mean (88.4%). In addition, among adult children with parents who have any ADL/IADL limitations, the public LTCI program increases the likelihood of working statistically significant by 4.1 percentage points. The labor-supply effects of LTCI are larger and statistically significant among women, less-educated adult children, and those with parents aged 75 or older. These groups are more likely to provide informal care in the absence of public LTCI. The effects are also larger among older children, those who coreside with their parents, and those with fewer siblings. Among women, the effect decreases with the number of sisters, suggesting that sisters share caregiving responsibilities when formal care is unavailable. No comparable pattern is observed among men.

Motivated by Browning et al. (1985), I examine whether the labor-supply effects of LTCI vary with parental care needs and the COVID-19 period. Neither parental ADL limitations nor the COVID-19 period independently changes the effect of LTCI. However, during the pandemic, each additional parental ADL limitation significantly attenuates the program's positive labor-supply effect. No comparable pattern emerges when care needs are measured using IADL limitations. The results are similar when the number of parental chronic conditions is used instead of ADL limitations.

To shed light on the underlying mechanisms, I examine informal caregiving and intergenerational transfers. LTCI reduces the time that children and grandchildren spend providing care, although this effect is smaller during the COVID-19 period. This pattern is consistent with formal care substituting for informal care under normal conditions and, together with the main labor-supply results, suggests that the substitution effect dominates the income effect. LTCI also increases financial transfers from adult children to their parents, with the increase along the intensive margin amounting to 31% of the baseline mean. Transfers from parents to children also increase in normal times, but this effect weakens during COVID-19, consistent with stronger precautionary-saving motives and higher medical expenditures during the pandemic (Ameriks et al., 2020; Liu and Hu, 2022; Liu et al., 2023; Li et al., 2024).

Since most of pilot cities provide in-kind LTC support while some offer both in-kind and cash support, I compare these two types policy design. The results show that both designs increase adult children's labor supply but providing both in-kind and cash yields a larger positive effect than in-kind only. Additionally, households in in-kind-only cities are more likely to use institutional or community care, whereas households in cities with both benefits are more likely to purchase paid care. However, in cities with both benefits, children's or grandchildren's caregiving declines significantly and parents

report lower life satisfaction. Overall, there is a trade-off: program that offer both in-kind and cash benefits produce larger gains in adult children's labor supply but are associated with lower parental life satisfaction due to reduced children's caregiving time.

This study contributes to the literature on LTCI and caregivers' labor supply. Existing studies reach different conclusions depending on the institutional setting, benefit design, and population examined. In the United States, Coe et al. (2023) find that private LTCI increases the labor supply of adult children. Fu et al. (2017) similarly show that public LTCI increases caregivers' labor supply in Japan. In contrast, Geyer and Korfhage (2015) find that Germany's public LTCI program, which allows beneficiaries to receive cash benefits, reduces labor supply among male caregivers and has no significant effect among female caregivers. These findings suggest that the relative importance of the substitution and income effects varies across policy settings.

Evidence from China is also mixed across affected family members. Ai et al. (2024) find that public LTCI reduces labor supply, particularly full-time employment, among older adults. By contrast, Han et al. (2025) show that the program increases the labor supply of spouses of older adults with functional limitations, primarily through self-employment. Most closely related to this study, Yi et al. (2025) find that China's public LTCI program increases the labor supply of adult children.

My study extends this literature in four respects. First, whereas Yi et al. (2025) consider centrally designated pilot cities observed before the COVID-19 pandemic, I incorporate programs initiated by both central and local governments as well as later pilot expansions. Second, the inclusion of data collected during the pandemic allows me to examine whether the labor-supply effects of LTCI vary with parental care needs and disruptions to formal care. Third, I analyze changes in informal caregiving and intergenerational financial transfers to clarify the mechanisms underlying the labor-supply response. Finally, I compare programs that provide only in-kind services with those that offer both in-kind and cash benefits, evaluating their effects on adult children's labor supply and parents' life satisfaction.

This study also contributes to the evaluation of public LTCI, particularly in China. Much of the broader LTCI literature examines the determinants of insurance demand, including the crowd-out effect of Medicaid on private coverage (Brown and Finkelstein, 2008; Brown et al., 2012; Coe et al., 2015; Mommaerts, 2025). Recent studies of China's public LTCI program find improvements in beneficiaries' health (Lei et al., 2022; Wang et al., 2023), reductions in healthcare utilization (Han et al., 2024; Li et al., 2024) and health-related expenditures (Liu and Hu, 2022), and increases in non-health consumption (Liu et al., 2023). I extend this literature by examining spillover effects on adult children's labor supply and by comparing programs that provide only in-kind benefits with those that also offer cash benefits.

Finally, this study contributes to the literature on the allocation of formal and informal care. Pauly (1990) predicts that access to family-provided care reduces demand for LTCI, while Pezzin and Schone (1999) show that access to formal care can reduce reliance on informal care, particularly care provided by women. Empirical studies also document substitution between formal and informal

care (Engers and Stern, 2002; Charles and Sevak, 2005; Van Houtven and Norton, 2008). I provide evidence from a public LTCI program and distinguish among different forms of formal care. Although substitution between children's care and institutional or community-based care is limited, paid home care substitutes strongly for care provided by adult children.

The remainder of the paper is organized as follows. Section 2 provides institutional background on China's public LTCI program. Section 3 describes the identification strategy. Section 4 introduces the CHARLS data and presents descriptive statistics. Section 5 reports the main results and heterogeneity analyses. Section 6 examines the underlying mechanisms, focusing on informal caregiving and intergenerational financial transfers. Section 7 compares the effects of different LTCI benefit designs. Section 8 presents robustness checks, and Section 9 concludes.

2 Public LTCI program in China

LTC is commonly understood as personal, social, and medical support for individuals who require help with activities of daily living (ADLs) and instrumental ADLs (IADLs) because of physical or cognitive limitations (Zimmerman and Sloane, 2007; Mommaerts, 2025). China is facing a rapidly aging population: the share of people aged 65 or older increased from approximately 9.7% in 2013 to around 15.4% in 2023. Since older adults are more likely to experience functional limitations in their daily activities, the rapid increase in the aging population contributes to a substantial rise in the demand of LTC. The sharp increase in LTC demand has created serious challenges for households. First, formal care services are very expensive for older households in China. For example, the cost of purchasing home-based or community-based formal care corresponds to around 43.9% to 300% of the average household wage (Ai et al., 2024). Choosing formal LTC therefore imposes a severe economic burden on households.

Instead, households often rely on informal care provided by family members, especially adult children, due to the traditional norm that children should care for their parents. According to Ai et al. (2024), over 90% of disabled older people receive care at home, typically from their children. However, demographic shifts such as declining fertility, smaller household sizes, and increased labor mobility places a heavy burden on children who must balance caregiving with being the main earners. This informal care generates substantial implicit costs, as children devote significant time to caregiving and often experience high stress and adverse health effects.

To ensure that all disabled older adults can access LTC and to reduce the burden on families, both financially and in terms of time, especially for caregivers, the Chinese government initiated a public LTCI program. The first wave of LTCI pilot cities consisted of 15 cities announced by the central government in 2016, followed by a second wave in 2020 that added 14 more cities. In addition, some local governments launched their own LTCI pilots, and a few cities, such as Qingdao and Weifang, began pilot with LTCI even before the national announcement. The Figure 2 shows the distribution of city-level GDP (left-hand side) and the timing of LTCI implementation (right-hand side) for all cities in my sample. As the Figure 2 indicates, both more developed cities, such as cities

in eastern China, and less developed cities, such as cities in northern and central–western China, were selected as pilots.

In all pilot cities, eligibility for LTCI depends on enrollment in a public health insurance program. In all pilot cities, this is the Urban Employee Basic Medical Insurance (UEBMI), and in some cities, coverage is extended to enrollees with the Urban and Rural Resident Basic Medical Insurance (URRBMI). The LTCI fund is financed mainly through existing public health insurance pools without imposing additional taxes. The annual premium per enrollee was around 180 RMB in 2021, which is about 0.5% of average per capita disposable income (Liu et al., 2023). Thus, the LTCI program does not impose a substantial additional financial burden on households.

The LTCI program in all pilot cities provides in-kind services, and in some cities it also offers an in-cash benefit option. Covered services include home care, institutional care, and community-based care, with reimbursement rates typically ranging from 50% to 90% (Liu et al., 2023). The reimbursement rates and benefits depend on the level of disability, the type of care the household chooses, and the city. In general, higher levels of disability are associated with greater benefits, and institutional care usually has a relatively lower reimbursement rate than home care on average. By 2021, the average annual reimbursement per beneficiary was approximately 1,500 RMB (US$2,400), equivalent to 139% of average caregiving expenditures prior to the program and about 1.5 times the median income of elderly households in China (Liu et al., 2023). To receive benefits, individuals generally must be assessed as having functional limitations in their daily activities for at least six months. The assessment criteria vary across cities: most rely on ADLs, while some cities also include IADLs in order to expand eligibility.

The LTCI program plays an increasingly important role in China, with a growing number of people covered by the program and actually receiving benefits. Figure 3 shows the yearly numbers of enrollees (blue bars) and beneficiaries (orange line). The number of enrollees increased from 44 million in 2017 to 183 million in 2023, and the number of beneficiaries rose from 0.1 million to 1.3 million over the same period. Hence, the LTCI program has expanded rapidly in both coverage and utilization, indicating its growing importance in meeting long-term care needs.

3 Empirical strategy

3.1 Empirical models

The objective of this study is to evaluate the treatment effect of parental LTCI eligibility on their adult children's labor supply. Since I cannot observe whether parents actually receive the LTC benefits, the treatment effect identified in this study is of the intent-to-treat variety. The variation in LTCI implementation occurs along three dimensions: (1) across pilot and non-pilot cities; (2) over time, as different cities adopted the policy at different points; and (3) within pilot cities, where some households are eligible for LTCI and others are not.

Because a DiD specification limited to pilot cities (comparing eligible and ineligible households) may be affected by shocks that vary by eligibility status over time, and a DiD specification limited

to eligible households (comparing pilot and non-pilot cities) may be affected by shocks that vary by city over time, I adopt a triple-difference (DDD) specification as my main empirical model. My primary DDD regression specification is:

$$\begin{aligned} Y_{ict} = \alpha + \beta_1 E_i \times Pilot_c \times Post_{ct} + \beta_2 E_i \times Pilot_c \times Post_{ct} \times COVID_t \\ + X'_{ict}\gamma + \eta_{ct} + \mu_{Et} + \delta_i + \varepsilon_{ict}, \end{aligned} \tag{3.1}$$

where Y_{ict} denotes the labor supply outcome of individual i, whose parents reside in city c, in year t. E_i is an indicator equal to one if individual i has at least one parent who is eligible for LTCI (and zero otherwise). In non-pilot cities, I analogously set $E_i = 1$ if at least one parent is covered by the Urban Employee Basic Medical Insurance (UEBMI), since in most pilot cities LTCI eligibility is restricted to UEBMI enrollees. $Pilot_c$ is an indicator for whether the parents' city c is a pilot city. $Post_{ct}$ is an indicator that equals one in years after LTCI is implemented in city c. The coefficient β_1 is the parameter of interest and captures the effect of parental LTCI eligibility on the labor supply of their adult children during the pre-COVID period. The indicator $COVID_t$ equals one in years affected by the COVID-19 pandemic (2020) and zero otherwise. The coefficient β_2 identifies the additional treatment effect during the COVID-19 period, that is, the differential impact of parental LTCI eligibility in the pandemic period relative to the pre-COVID period.

X_{ipt} is a vector of adult children's characteristics, including sex, age, type of *hukou*, marital status, education level, family size, an indicator for having any children, and an indicator for having at least one elderly family member aged 60 or above. Additionally, δ_i denotes individual fixed effects. For the DDD identification, I also include two further sets of fixed effects: η_{ct}, city-by-year fixed effects, and μ_{Et}, eligibility-group-by-year fixed effects. These two sets of fixed effects control for both city-by-year and group-by-year shocks.

In addition to the triple-difference design, I estimate two complementary difference-in-differences specifications. The first DiD is limited to children whose parents reside in pilot cities, the effects of LTCI can be identified by comparing changes in labor supply among children from eligible households to those from ineligible households, before and after the implementation period of the LTCI policy. The empirical DiD model is given by:

$$\begin{aligned} Y_{ict} = \alpha + \beta_1^{Elig} E_i \times Post_{ct} + \beta_2^{Elig} E_i \times Post_{ct} \times COVID_t \\ + X'_{ict}\gamma + M'_{ct}\lambda + \omega_{pt} + \theta_t + \delta_i + \varepsilon_{ict}, \end{aligned} \tag{3.2}$$

where Y_{ict} is the outcome of interest, and the coefficient β_1^{Elig} is the parameter of interest, and β_2^{Elig} indentifies the additional treatment effect during the COVID-19 period. I control for city-by-year characteristics, including GDP and the unemployment rate, summarized in M_{ct}. Because COVID-19 policies in China were typically implemented at the province level, I additionally include province-by-year fixed effects, ω_{pt}, to account for differential COVID-19 policy shocks during the pandemic period. In addition, δ_i denotes individual fixed effects and θ_t denotes year fixed effects.

The second DiD specification restricts the sample to children whose parents are eligible for LTCI and compares outcomes across pilot and non-pilot cities. The corresponding DiD specification is as

follows:

$$
\begin{aligned}
Y_{ict} = \alpha + \beta_1^{Pilot} Pilot_c \times Post_{ct} + \beta_2^{Pilot} Pilot_c \times Post_{ct} \times COVID_t \\
+ X'_{ict}\gamma + M'_{ct}\lambda + \omega_{pt} + \theta_t + \delta_i + \varepsilon_{ict},
\end{aligned}
\tag{3.3}
$$

where Y_{ict} is the outcome of interest, and the coefficient β_1^{Pilot} is the parameter of interest, and β_2^{Pilot} indentifies the additional treatment effect during the COVID-19 period. The key identifying assumption is that, in the absence of the LTCI program, the labor supply of children with eligible parents in pilot and non-pilot cities would have followed parallel trends over time (conditional on controls and fixed effects).

Each of the two DiD specifications relies on distinct identifying assumptions that may be restrictive.

The first DiD specification assumes that, in the absence of the LTCI program, the labor supply of children with at least one eligible parent and that of children without eligible parents would have followed parallel trends over time within pilot cities. This assumption may be violated if eligibility status is correlated with unobserved factors that differ between eligible and ineligible households and that affect labor supply differently across the two groups.

The second DiD specification assumes that pilot cities were not selected based on city-level characteristics that differentially affect labor supply trends among eligible households. However, pilot city selection may be correlated with administrative capacity, fiscal conditions, or demographic structure, raising concerns that policy adoption may be endogenous to local labor market conditions.

In contrast, the DDD design compares eligible and ineligible households within the same city over time and across pilot and non-pilot cities. By differencing out both city-level shocks and group-specific time-invariant heterogeneity, the DDD estimator mitigates concerns arising from endogenous pilot selection as well as from differential trends between eligible and ineligible households. The DDD specification relies on the assumption that, absent the LTCI program, the eligible–ineligible labor supply gap would have evolved similarly in pilot and non-pilot cities. I discuss the empirical tests of this identifying assumption in the next section.

3.2 Assumption for DDD

The identification of the DDD empirical strategy relies on a generalized parallel-trends assumption: in the absence of the LTCI program, the evolution of the labor-supply gap between adult children with eligible parents and those without eligible parents would have been the same in pilot and non-pilot cities. In other words, absent treatment, the difference in labor supply between eligible and ineligible children in pilot cities would have followed the same time path as the corresponding difference in non-pilot cities. To assess the plausibility of this assumption, I estimate a triple-differences event-study specification following League and Shi (2025). The empirical model is:

$$Y_{ict} = \alpha + \sum_{e=-7}^{4} \beta_{1e}^{ES} T_{ct}(e) \times Elig_i + \sum_{e=-7}^{4} \beta_{2e}^{ES} T_{ct}(e) + X'_{ict}\gamma + M'_{ct}\lambda + \omega_{pt} + \mu_{Et} + \theta_t + \delta_i + \varepsilon_{ict} \tag{3.4}$$

where $T_{ct}(e)$ is a set of event-time indicators equal to 1 if city c is e years away from the LTCI implementation year (event time e). Because the city-by-year fixed effects in the baseline DDD specification would absorb the event-time variation, this event-study model replaces the city-by-year fixed effects with a full set of event-time indicators, allowing the treatment effect to vary flexibly over time. To account for time-varying local conditions, I include city-by-year controls M_{ct} (including GDP and the unemployment rate) as well as province-by-year fixed effects ω_{pt}, and μ_{Et} are eligibility-group-by-year fixed effects. δ_i denotes individual fixed effects, and θ_t denotes year fixed effects. Relative to the baseline DDD specification, this event-study model replaces the city-by-year fixed effects with a full set of event-time indicators interacted with pilot status, allowing the treatment effect to vary flexibly over time. The event-time window ranges from $e = -7$ to $e = 4$, with the omitted period serving as the reference category.

A limitation of the event-study analysis based on CHARLS is that, because cities adopt LTCI in different years while CHARLS is not an annual survey, the coefficients β_{1e}^{ES} and β_{2e}^{ES} at different event times e are identified from different sets of treated cities. For example, observations in cities that implemented LTCI in 2017 only contribute to the estimation of β_{1e} and β_{2e} at event times $e \in \{-6, -4, -2, 1, 3\}$. As a result, the event-study estimates are less precise, and the coefficients can be sensitive to the choice of the omitted (baseline) period, because for any given baseline event time some treated cities do not have observations in that period. To assess robustness, I report event studies based on two normalizations: one using $e = -1$ as the baseline period, which is standard in the literature, and another using $e = -2$ as the baseline period, since for most cities that adopted LTCI in 2017 the $e = -2$ period is observed (Among the 25 pilot cities, 1 city was treated in 2012, 1 in 2015, 2 in 2016, 12 in 2017, 4 in 2018, 4 in 2019, and 1 in 2020.).

4 Data and descriptive statistics

4.1 Data and sample

The data used in this study is from the China Health and Retirement Longitudinal Study (CHARLS), designed by the National School of Development at Peking University. CHARLS is equivalent to the Health and Retirement Study (HRS) in the United States and is a nationally representative longitudinal survey of individuals aged 45 and above, as well as their spouses, across 126 cities (out of a total of 333 cities) in 28 provinces in China. collects extensive information on the elderly and their spouses and includes data on their children, both those living within and outside the household. CHARLS comprises five waves, conducted in 2011, 2013, 2015, 2018, and 2020. My analysis includes all five waves, capturing both pre- and post-COVID-19 periods. I examine the

causal effects of the Long-Term Care Insurance (LTCI) policy with and without including the 2020 wave to assess whether the COVID-19 pandemic influences the policy's estimated impact. The sample is restricted to households with both elderly individuals aged 50 or older and at least one child aged 16 to 55, as 16 is the legal working age in China, and 55 is the typical retirement age for female blue-collar workers (60 for men). Additionally, I drop all children who are in school or both in school and working. Based on the question about children's work in CHARLS, CHARLS defines work as being a farmer, doing work for salary, self-employed or assisting in family business without getting paid.

Regarding policy data, I identify pilot cities based on official announcements from both the central and local governments, following the criteria established by Ai et al. (2024) and cross-checked with information from official government websites. A city is included as a pilot only if both sources are consistent. Cities with conflicting information, unclear implementation status, or unspecified start dates are excluded. Based on these criteria, the dataset includes 120 cities, among which 23 are pilot cities. Of the pilot cities, 20 implemented LTCI before 2019, while 3 implemented it during or after 2019 (and are treated as such in wave 5).

Eligibility for LTCI is determined by individuals' enrollment in either the UEBMI or the URRBMI. All 23 pilot cities in my dataset treat UEBMI enrollees as eligible for LTCI, and 8 of these cities also extend eligibility to URRBMI enrollees. UEBMI status is relatively stable among older adults because it is tied to employment history. I therefore treat UEBMI as time-invariant for three reasons. First, older individuals tend to be risk-averse and are unlikely to withdraw from public health insurance once enrolled. Second, individuals close to retirement are unlikely to change their employment status. Third, most of my sample consists of retired individuals, for whom UEBMI status is unlikely to change post-retirement. Compared with UEBMI, URRBMI status is more fluid because it must be renewed annually and depends on individuals' preferences. However, based on the assumption that older individuals are unlikely to drop coverage once enrolled, I also treat URRBMI status as fixed from the first year of enrollment. To avoid selection concerns arising from changes in insurance coverage around the time of LTCI adoption, I drop households in which any parent first become eligible (enroll UEBMI or URRBMI in some pilot cities) after the LTCI policy implementation date in their pilot city. In a later section, I relax the assumption that older individuals remain continuously enrolled once they enter UEBMI or URRBMI and re-estimate the models without this restriction to assess the potential magnitude of selection bias.

Based on the sample selection criteria, the analytical sample includes 23 pilot cities and 95 non-pilot cities, and 88,679 child-year observations. Among the full sample, 4,589 observations are children with at least one LTCI-eligible parent living in a pilot city (treated group), and 10,180 observations are children with no eligible parent living in a pilot city. There are 13,708 observations for children with at least one eligible parent living in a non-pilot city and 60,202 observations for children with no eligible parent living in a non-pilot city. Restricting attention to children who ever have any parents with any ADL/IADL limitation, the sample contains 71,786 child-year observations. Within this subgroup, 3,008 observations are children with at least one eligible parent living in a

pilot city (treated group), 8,170 are children with no eligible parent living in a pilot city, 9,862 are children with at least one eligible parent living in a non-pilot city, and 50,746 are children with no eligible parent living in a non-pilot city. My main analysis focuses on adult children with any ADL/IADL-limited parents, as this group is directly targeted by the LTCI policy.

4.2 Descriptive statistics

Table 1 presents summary statistics for the full sample. Among all child-year observations, 53% are male. The average age is 38 years, and 86% of children are married. About 76% hold a rural Hukou, which in China generally indicates whether an individual is registered in a rural or urban area. The average family size is 3 members. In addition, 77% of observations have at least one elderly parent aged 60 or above, and 87% have at least one child. Regarding education, 39% of children have less than middle school education (i.e., did not graduate from middle school), 34% have completed middle school, 14% have completed high school, and 13% have graduated from college or above. The CHARLS also reports parents' public health insurance status. Based on their parents' UEBMI and URRBMI enrollment, 19% of children have at least one parent covered by UEBMI, and 27% have at least one parent covered by URRBMI. Additionally, 17% of observations have parents living in pilot cities. The CHARLS records each child's employment status (whether the child is currently working), and in my sample, 88% of observations are working.

The CHARLS further collects information on each parent's ADL and IADL limitations. I define a child as having any ADL (IADL) parent if at least one parent reports difficulty in any ADL (IADL) activity. At the household level, I measure the number of ADL or IADL limitations as the maximum number of limitations reported by any parent. In the full sample, 66% of observations have at least one parent with an ADL limitation, and 73% have at least one parent with an IADL limitation. Moreover, the average number of ADL limitations among parents is 1.8, and the average number of IADL limitations is 1.9. Moreover, Table A1 in Appendix A reports summary statistics for adult children with any ADL/IADL-limited parents.

Since I employ three empirical strategies to study how parents' LTCI eligibility affects adult children's labor supply, I consistently define the treated group as children from households with at least one LTCI-eligible parent living in a pilot city. I then construct three corresponding control groups for identification. In the first DiD specification, I study the effect of LTCI among adult children living in pilot cities. The control group consists of children from households in pilot cities without any eligible parents. In the second DiD specification, I focus on the effect of LTCI among children from eligible households. Here, the control group is composed of children from households with at least one eligible parent but not living in pilot cities (i.e., eligible households in non-pilot cities). In the triple-differences specification, I combine both sources of variation and use all children in the two DiD control groups as the control group: children from ineligible households in pilot cities and children from eligible and ineligible households in non-pilot cities.

Figure 1 plots the employment rate over time for four groups of children, defined by their households' LTCI eligibility and pilot-city status. The red curve shows the trend for the treated

group: children from eligible households in pilot cities. The blue curve shows the control group for the first DiD: children from ineligible households in pilot cities. The green curve shows the control group for the second DiD: children from eligible households in non-pilot cities. The black curve shows part of the control group for the triple-differences specification: children from ineligible households in non-pilot cities. The figure indicates that, within pilot cities, the pre-policy employment trends for children from eligible and ineligible households are similar, but there is a noticeable increase in employment for the treated group between 2013 and 2015. By contrast, the trends for children from eligible households in pilot and non-pilot cities differ more sharply. After the implementation of LTCI, all control groups exhibit decreasing employment trends, whereas the treated group shows an increasing trend. From 2018 to 2020, employment declines across all groups, likely reflecting the impact of the COVID-19 pandemic.

5 Main Results

5.1 Effects of LTCI on Adult Children's Labor Supply

Table 2 presents estimates of the effects of LTCI on adult children's labor supply, measured by the probability of working. Because wave 5 corresponds to 2020 (the COVID-19 period), I include an interaction between the treatment indicator and a COVID-19 indicator to allow the treatment effect to differ during the pandemic. This specification allows me to examine whether and how the COVID-19 pandemic alters the effects of LTCI on adult children's labor supply.

Column 1 reports estimates from the baseline DDD specification. The results indicate that parental LTCI eligibility increases adult children's probability of working by 3.9 percentage points in pilot cities during the pre-pandemic period. This effect is statistically significant at the 5% level and represents approximately 4.4% of the control group's mean employment rate (88.4%). During the COVID-19 period, the positive effect of LTCI declines significantly by 3.3 percentage points, suggesting that the program has a limited impact on adult children's probability of working during the pandemic. Overall, these findings suggest that parental LTCI eligibility encourages adult children to work, but its positive effect is attenuated during the COVID-19 pandemic.

Column 2 reports DiD estimates using a sample restricted to adult children whose parents reside in pilot cities. The results indicate that parental LTCI eligibility increases adult children's probability of working by 3.6 percentage points, and the effect is statistically significant. During the COVID-19 period, the estimated effect declines by only 0.5 percentage points, and this attenuation is not statistically significant. Although the DiD specification does not detect a significant change in the treatment effect during the pandemic, the estimated main effect of parental LTCI eligibility is consistent in magnitude and direction with the baseline DDD results.

The last column reports DiD estimates using a sample restricted to adult children with at least one LTCI-eligible parent. The results indicate that LTCI implementation in pilot cities increases adult children's probability of working by 4.4 percentage points relative to those whose parents reside in non-pilot cities. This effect is statistically significant. During the COVID-19 period, the estimated

treatment effect declines by 2.7 percentage points, although this attenuation is not statistically significant. Overall, the estimated main effect is consistent in magnitude and direction with the baseline DDD results.

Figure 4 presents the event-study estimates from the DDD specification. The first panel uses $e = -1$ as the baseline period, while the second uses $e = -2$. In both specifications, the estimates suggest that LTCI increases adult children's probability of working following implementation, although the positive effects are attenuated during the COVID-19 period. The pre-treatment coefficients are statistically indistinguishable from zero, consistent with the parallel-trends assumption. Also, Figure 5 presents the corresponding DDD event-study estimates for the subsample of adult children with at least one parent experiencing an ADL/IADL limitation. The results are broadly consistent with those in Figure 4, with no statistically significant pre-treatment effects.

Overall, the results suggest that parental eligibility for public LTCI increases adult children's probability of working. This finding is consistent with substitution effects outweighing income effects on adult children's labor supply in China. By alleviating caregiving time constraints, public LTCI may enable adult children to reallocate time from informal caregiving toward work.

5.2 Effects of LTCI Among Households with Parental ADL/IADL Limitations

Since China's public LTCI program primarily targets older adults with functional limitations, this section focuses on households with at least one parent experiencing an ADL/IADL limitation. These households are more likely to have long-term care needs and benefit from LTCI coverage, making this particularly relevant for assessing LTCI's effects on adult children's labor supply.

Table 3 presents estimates of the effects of public LTCI on adult children's probability of working among households with at least one parent with any ADL/IADL limitation. Column 1 shows that parental LTCI eligibility increases adult children's probability of working by 4.1 percentage points, statistically significant at the 5% level. This effect represents approximately 4.6% of the control group's mean employment rate. During the COVID-19 period, the estimated treatment effect declines by 3.1 percentage points. Although this attenuation is not statistically significant, the estimate is negative and similar in magnitude to the full-sample result reported in Table 2. Overall, the estimated positive effect of LTCI is slightly larger among adult children with parents experiencing ADL/IADL limitations than in the full sample, while the estimated attenuation during the pandemic follows a similar pattern.

Columns 2 and 3 report estimates from two DiD specifications for the subsample of adult children with at least one parent experiencing an ADL/IADL limitation. Column 2 shows that parental LTCI eligibility increases adult children's probability of working by 4.0 percentage points, a statistically significant effect, while the estimated treatment effect declines by 0.8 percentage points during the COVID-19 period. Similarly, Column 3 shows a 6.0-percentage-point increase in the probability of working, with an estimated attenuation of 2.5 percentage points during the pandemic. Hence, these DiD estimates are consistent with the baseline DDD results, showing positive effects of LTCI on adult children's probability of working, with point estimates suggesting attenuation during the

COVID-19 period.

Overall, across all three specifications, the estimated positive effects of LTCI on adult children's probability of working are somewhat larger among households with at least one parent experiencing an ADL/IADL limitation than in the full sample. One possible explanation is that parents with functional limitations are closer to the target population of LTCI and are more likely to receive benefits. Another explanation is that their adult children may face greater informal caregiving responsibilities in the absence of public LTCI. By substituting formal care for informal family care, LTCI may alleviate caregiving time constraints, allowing adult children to reallocate time toward work and increasing their probability of working.

Moreover, Table 4 presents estimates of the effects of LTCI on adult children's probability of working, restricting the sample to households with at least one parent experiencing an ADL limitation. ADL limitations generally reflect more severe functional impairments than IADL limitations, and adult children in these households may face greater caregiving responsibilities in the absence of public or privately purchased long-term care. Compared with the broader ADL/IADL subsample in Table 3, Columns 1 and 2 show smaller and less statistically significant positive effects of LTCI on adult children's probability of working. Specifically, the baseline DDD estimate indicates an increase of 2.5 percentage points, although this effect is not statistically significant. The estimated treatment effect further declines by 2.8 percentage points during the COVID-19 period. The estimate in the last column is statistically significant but smaller in magnitude than the corresponding estimate for the broader ADL/IADL subsample. One possible explanation is that parents with ADL limitations have more severe care needs that may not be fully addressed by public LTCI. Consequently, adult children may continue to face substantial caregiving responsibilities, limiting the positive effects of LTCI on their probability of working.

Table 5 further restricts the sample to adult children with at least one parent who both experiences an ADL/IADL limitation and is eligible for LTCI. Because LTCI eligibility is determined at the individual rather than the household level, parents with ADL/IADL limitations may not necessarily qualify for LTCI if they are not enrolled in UEBMI or, in some pilot cities, URRBMI. To assess whether this additional eligibility restriction affects the results, I re-estimate the models using this subsample. Column 1 shows that LTCI increases adult children's probability of working by 3.9 percentage points, while the estimated treatment effect declines by 3.6 percentage points during the COVID-19 period. Column 2 reports a 3.2-percentage-point increase, with only limited attenuation during the pandemic. Similarly, Column 3 shows a 7.1-percentage-point increase, with an estimated decline of 5.2 percentage points during the COVID-19 period. Overall, these estimates are broadly consistent with those for the broader ADL/IADL subsample in Table 3, suggesting positive effects of LTCI on adult children's probability of working, with attenuation during the pandemic. Moreover, the additional eligibility restriction results in only a small reduction in sample size, suggesting that most households in the broader ADL/IADL subsample also satisfy the LTCI eligibility criteria.

5.3 Heterogeneity by Adult Children's and Family Characteristics

In this section, I examine heterogeneity in the effects of LTCI on adult children's labor supply by their individual characteristics and family structure. The analysis is restricted to adult children who have ever had at least one parent with an ADL or IADL limitation, as these families are more likely to have long-term care needs targeted by the LTCI program.

Figure 6 presents the heterogeneous effects of parental LTCI eligibility on adult children's probability of working by gender, education, and co-residence status, with 90% confidence intervals.

The first pair of coefficients (circle markers) shows that LTCI increases daughters' probability of working by 6.3 percentage points, a statistically significant effect, compared with a smaller and statistically insignificant increase of 1.2 percentage points among sons. One possible explanation is that daughters traditionally bear greater responsibility for caring for aging parents, while sons are more likely to engage in market work. Public LTCI may therefore substitute for daughters' informal caregiving time, generating a stronger substitution effect and encouraging them to work.

The second pair of coefficients (diamond markers) presents heterogeneity by children's baseline education level. I classify children who did not complete middle school as having low education and those with at least a middle school education as having high education. The results indicate that LTCI significantly increases the probability of working among low-educated children by 17.3 percentage points, compared with a smaller increase of 2.8 percentage points among highly educated children. A possible explanation is that low-educated children tend to have lower expected wages and may therefore be more likely to provide informal care in the absence of LTCI. By reducing their caregiving responsibilities, public LTCI may encourage greater participation in market work.

The last pair of coefficients (triangle markers) examines heterogeneity by whether adult children lived with their parents in the baseline period. LTCI significantly increases the probability of working among co-residing children by 4.9 percentage points, compared with a statistically insignificant increase of 3.4 percentage points among those living separately. Although the effect is slightly larger among co-residing children, the difference in magnitude is relatively small. One possible explanation is that children living separately but near their parents may still provide substantial informal care. However, data limitations prevent a more detailed analysis based on the geographic distance between parents and children.

Overall, the estimates suggest larger positive effects of LTCI on the probability of working among daughters, low-educated children, and those co-residing with their parents. These groups may face greater informal caregiving responsibilities in the absence of public LTCI. The estimated positive effects are also attenuated during the COVID-19 period, potentially reflecting increased caregiving needs during the pandemic. Detailed regression results, including the COVID-19 interaction terms, are reported in Appendix B Table B1.

In addition, Figure 7 presents the heterogeneous effects of parental LTCI eligibility on adult children's probability of working by family characteristics, including parental age, number of siblings, and birth order.

The first pair of coefficients (diamond markers) examines heterogeneity by whether children

have at least one parent aged 75 or older. The results show that LTCI significantly increases the probability of working among children with older parents by 9.1 percentage points, while the effect is limited among children whose parents are all younger than 75. One possible explanation is that older parents tend to have greater caregiving needs, which may impose stronger time constraints on their adult children. Public LTCI may therefore substitute for informal family care, allowing these children to reallocate more time toward work.

The second pair of coefficients (circle markers) examines heterogeneity by the number of siblings. I classify children from families with at most two children (including themselves) as having fewer siblings, and those from families with three or more children as having more siblings. The results indicate that LTCI significantly increases the probability of working among children with fewer siblings by 5.4 percentage points, compared with a smaller and statistically insignificant increase of 3.2 percentage points among those with more siblings. This pattern is consistent with the idea that children with fewer siblings face greater caregiving responsibilities because they have fewer family members with whom to share the caregiving burden. Consequently, public LTCI may generate stronger substitution effects for these children. Consistent with this interpretation, the estimated effect is particularly large among only children, reaching 14.1 percentage points.

The last group of coefficients (triangle, square, and cross markers) examines heterogeneity by children's birth order. I classify children into three groups: the oldest child (first-born), the second-oldest child (second-born), and younger children (third-born or later). The results show that LTCI significantly increases the probability of working among first-born children by 6.1 percentage points, compared with a smaller increase of 3.7 percentage points among second-born children and only limited effects among younger children. The estimated effects decline with birth order, suggesting that earlier-born children may shoulder greater caregiving responsibilities in the absence of public LTCI. By reducing these responsibilities, LTCI may have a larger positive effect on their probability of working.

Overall, the estimates suggest larger positive effects of LTCI among adult children with older parents, fewer siblings, and earlier birth order. These groups may face greater informal caregiving responsibilities in the absence of public LTCI. During the COVID-19 period, the positive effect remains relatively stable among children with older parents but is attenuated among those with fewer siblings or earlier birth order. This pattern may reflect increased family caregiving needs during the pandemic, limiting the extent to which public LTCI substitutes for informal care. Detailed regression results, including the COVID-19 interaction terms, are reported in Appendix B Table B2.

5.4 Parental Health and Pandemic-Related Changes in LTCI Effects

In this section, I examine how parental health status and the COVID-19 pandemic affect the impact of LTCI on adult children's labor supply. Building on the framework of labor supply under uncertainty in Browning et al. (1985), I consider whether parental health conditions and pandemic-related disruptions alter adult children's labor supply responses to LTCI. I use an indicator for the COVID-19 period, corresponding to Wave 5 (2020) of CHARLS.

To measure parental health status, I use ADL and IADL limitations reported in CHARLS. These measures capture functional impairments that may increase the need for informal care from family members, particularly adult children. ADL limitations generally reflect more severe difficulties with basic self-care activities. Moreover, China's public LTCI program primarily targets older adults with functional limitations, making these measures particularly relevant to the analysis.

I construct a household-level measure of the total number of parental ADL/IADL limitations (ranging from 0 to 11) and interact it with the treatment and COVID-19 indicators. Column 1 of Table 6 reports a positive LTCI effect of 3.8 percentage points, while the separate interactions with COVID-19 and parental limitations are small. However, the triple interaction is negative and statistically significant, indicating that each additional ADL/IADL limitation is associated with a 1.1-percentage-point further reduction in LTCI's effect on adult children's probability of working during COVID-19. Columns 2 and 3 show similar patterns. These findings suggest that greater parental care needs may limit LTCI's ability to alleviate caregiving constraints and encourage adult children to work during the pandemic.

Because ADL limitations reflect more severe functional impairments, I further examine whether parental ADL burden, measured by the number of ADL limitations or an indicator for severe ADL limitations, alters the effects of LTCI on adult children's labor supply. Table 7 presents results using the number of parental ADL limitations. Column 1 reports a positive LTCI effect of 4.5 percentage points, statistically significant at the 5% level. The separate interactions with COVID-19 and parental ADL limitations are small and negative. The triple interaction is also negative, suggesting that each additional ADL limitation is associated with a further 1.5-percentage-point reduction in LTCI's effect on adult children's probability of working during COVID-19, although this estimate is not statistically significant. Columns 2 and 3 provide stronger evidence, with statistically significant negative triple interactions of 1.7–1.8 percentage points. Overall, these findings suggest that greater parental ADL limitations may reduce the positive labor-supply effects of LTCI during the pandemic, potentially reflecting increased caregiving needs that public LTCI cannot fully address.

I also examine whether parental IADL limitations affect adult children's labor supply responses to LTCI, restricting the sample to households without parental ADL limitations to isolate the role of IADLs. The results suggest that neither the number nor the severity of IADL limitations substantially alters the positive effects of LTCI, including during the COVID-19 period. Detailed results are reported in Appendix Tables C1 and C2.

Overall, the results suggest that parental health conditions and the COVID-19 pandemic jointly shape the labor-supply effects of LTCI. During the pandemic, greater parental functional limitations are associated with weaker positive effects of LTCI on adult children's probability of working.

6 Mechanisms: Informal Caregiving and Intergenerational Transfers

This section provides further evidence indicating that the LTCI policy increases adult children's likelihood of working. First, I examine how LTCI policy affect adult children's or grandchildren's

caring time on their parents to test the substitution effect of public LTC. The substitution effect means that when the government provides LTC for elderly parents with any ADL or IADL limitations, their children reduce the time spent on caring and allocate that time to other activities. Second, I examine the effect of LTCI policy on intergenerational transfers, specifically the transfers from children to parents and from parents to children. If the policy encourages adult children to work outside the home, they are more likely to become economically independent, rely less on financial support from their parents, and provide more support to their parents. This may be especially salient in China, where adult children are typically the primary income earners in most families.

6.1 Effects of LTCI on children's or grandchildren's care

Column 1 of Table 8 examines the extensive margin of caregiving, measured by whether older parents receive any care from their children or grandchildren. The results show that LTCI reduces the probability of receiving care by 1.7 percentage points (3.3% of the control-group mean), although this effect is not statistically significant. During the COVID-19 period, the estimated effect increases by 7.7 percentage points, but this change is also statistically insignificant. These findings suggest that LTCI has limited effects on whether children or grandchildren provide care.

Column 2 examines the intensive margin, measured by the number of caregiving days per month among parents already receiving care from children or grandchildren. Parental LTCI eligibility significantly reduces caregiving time by 9.4 days per month, equivalent to 59.9% of the control-group mean. However, this reduction is attenuated by 10.8 days during the COVID-19 period, suggesting that the substitution from informal care to formal LTC may be weaker during the pandemic.

Column 3 reports the total effect, combining the extensive and intensive margins. LTCI significantly reduces caregiving time by 5.0 days per month, corresponding to 67.4% of the control-group mean. During the COVID-19 period, this reduction is attenuated by 5.6 days, consistent with the pattern observed for the intensive margin.

Overall, these findings suggest that public LTCI reduces informal caregiving time, consistent with the substitution of formal LTC services for care provided by children or grandchildren. Combined with the positive effects on adult children's probability of working, the results suggest that LTCI may alleviate caregiving constraints and allow adult children to reallocate time toward market work. The decline in caregiving time also weakens an alternative explanation that LTCI increases reported employment by allowing parents to hire their children as caregivers. If this mechanism dominated, caregiving time would be expected to increase rather than decrease.

Appendix D.2 Table D2 reports estimates separately excluding and including the COVID-19 period. The results are broadly consistent with Table 8, showing reductions in caregiving time before the pandemic and weaker effects when COVID-19 observations are included.

6.2 Effects of LTCI on Intergenerational Transfers

Table 9 examines the effects of LTCI on intergenerational financial transfers. Panel A focuses on transfers from children to parents, while Panel B examines transfers from parents to children.

Column 1 reports the extensive margin, measured by whether any financial support is provided. Column 2 examines the intensive margin, conditional on providing positive support, and Column 3 combines both margins to estimate the overall effect on transfer amounts.

Transfers from children to parents. Panel A of Table 9 shows that LTCI has positive but statistically insignificant effects on financial transfers from children to parents. Column 1 reports an increase in the probability of providing financial support by 4.3 percentage points, equivalent to 5.5% of the control-group mean. Column 2 shows a 27% increase in the amount of support conditional on providing any support, corresponding to approximately 1,394 CNY annually. Combining both margins, Column 3 reports a 42% increase in total financial support, equivalent to approximately 1,709 CNY annually, although this estimate is also statistically insignificant. The COVID-19 interaction terms suggest only limited attenuation of these positive effects. Overall, the estimates suggest that LTCI may encourage adult children to provide greater financial support to their parents, although the effects are imprecisely estimated.

Transfers from parents to children. Panel B shows that LTCI also increases financial support from parents to children, with more pronounced changes during the COVID-19 period. Column 1 reports a small and statistically insignificant increase of 2.7 percentage points in the probability of providing financial support, while the COVID-19 interaction indicates a significant decline of 13.7 percentage points. Column 2 shows that LTCI significantly increases the amount of support conditional on providing any support by 66.5%, equivalent to approximately 2,421 CNY annually. However, this positive effect is significantly attenuated during COVID-19. Column 3 shows a similar pattern for total transfers, with a positive estimated effect that declines significantly during the pandemic. One possible explanation is that LTCI reduces precautionary savings and health-related expenditures (Liu et al., 2023), allowing parents to provide greater financial support to their children in normal times. During the pandemic, however, parents may reduce such transfers in response to greater financial uncertainty.

Net economic support. I further examine whether LTCI changes net financial transfers from children to parents, defined as transfers received from children minus transfers provided to children. Column 4 of Panel A in Table 9 shows a small negative estimated effect, with limited changes during the COVID-19 period. However, the net support measure is highly skewed and sensitive to extreme observations. To address this concern, I also consider a log-ratio measure of relative transfers:

$$y_{it} = \ln\left(\frac{\text{Support received from children}_{it} + 1}{\text{Support given to children}_{it} + 1}\right), \tag{6.1}$$

where transfers are annual amounts in RMB and the "+1" adjustment allows for zero transfers. Larger values indicate greater financial support from children relative to support provided by parents. Table 10 shows no discernible effect of LTCI on relative transfers in the pre-pandemic period, but a positive estimated effect during COVID-19. This pattern appears to be driven primarily by

reductions in financial support from parents to children rather than increased transfers from children.

Overall, these findings suggest that LTCI may increase financial transfers in both directions, although the evidence is stronger for transfers from parents to children. Combined with the positive labor-supply effects, the results are consistent with adult children having greater capacity to support their parents financially. However, LTCI does not clearly increase net financial support received by parents in normal times. During COVID-19, relative financial support shifts toward parents, primarily because parents reduce transfers to their children. Additional estimates excluding and including COVID-19 observations are reported in Appendix D.3 Tables D3 and D4.

7 Policy Evaluation: Two Designs of LTC Support

There are two types of LTC support designs in China. Some pilot cities provide in-kind benefits only, while others offer both in-kind and cash benefits, from which eligible households must choose one. In-kind benefits are more likely to generate a substitution effect by reducing the need for informal care from family members. Cash benefits can generate both substitution and income effects: households may use cash to purchase formal care, but government transfers may also reduce incentives to work. Geyer and Korfhage (2015) find that Germany's cash-based LTCI program reduces adult children's labor supply. Han et al. (2024) find that China's LTCI increases spouses' labor supply in "normal LTCI" pilot cities but has limited effects in "generous LTCI" cities. They also find larger positive effects on spouses' self-employment in cities offering both in-kind and cash benefits.

Focusing on adult children may lead to different predictions. First, adult children in China are often the main earners in their households, so reducing their caregiving burden may encourage them to reallocate time toward market work. Second, cash benefits are relatively small compared with typical earnings. Monthly cash benefits are usually about 300–900 CNY, compared with average monthly urban wages of 6,872 CNY in non-private units and 4,131 CNY in private units in 2018. Hence, the income effect of cash benefits on adult children's labor supply is expected to be relatively small.

Table 11 compares the effects of LTCI across the two benefit designs. Column 1 examines adult children's labor supply at the individual level, while Columns 2–5 examine children's caregiving time, institutional care use, paid care use, and parents' life satisfaction at the household level. Based on CHARLS and Harmonized CHARLS definitions, I distinguish between paid care, which includes paid helpers such as nannies, and institutional care, which includes facility staff or volunteers, nursing home care, and community-based care.

Column 1 shows that, among adult children whose parents have ADL/IADL limitations, LTCI increases the probability of working by 2.8 percentage points in pilot cities offering in-kind benefits only, although this effect is not statistically significant. In contrast, the estimated effect is 6.6 percentage points and statistically significant in cities offering both in-kind and cash benefits. The COVID-19 interactions are negative but statistically insignificant in both groups, at approximately

-3.0 and -3.4 percentage points, respectively. These results suggest larger positive labor-supply effects in cities providing both benefit options, consistent with the findings on spouses' labor supply in Han et al. (2024).

Column 2 examines the intensive margin of caregiving from children. In cities providing in-kind benefits only, LTCI reduces caregiving time by approximately 5.2 days per month, although the estimate is not statistically significant. During COVID-19, the estimated effect increases by 14.2 days, and this change is statistically significant. In contrast, in cities offering both in-kind and cash benefits, LTCI significantly reduces caregiving time by approximately 13.5 days per month. This reduction becomes significantly smaller by 8.2 days during the COVID-19 period. These results are consistent with the labor-supply findings, suggesting that programs offering both in-kind and cash benefits may be more effective in reducing adult children's caregiving burden and encouraging market work in normal times.

Columns 3 and 4 examine formal care utilization. Column 3 shows that LTCI increases institutional care use by 2.5 percentage points in cities providing in-kind benefits only, statistically significant at the 10% level, but has no detectable effect in cities offering both in-kind and cash benefits. Column 4 shows little effect on paid care use under the in-kind-only design. In contrast, LTCI increases paid care use by 1.3 percentage points in cities offering both benefit options, equivalent to approximately 33% of the control-group mean, although this estimate is not statistically significant. The estimated effect increases by another 1.8 percentage points during COVID-19. Overall, households in cities providing in-kind benefits only appear more likely to use institutional care, while those offering both options may encourage households to purchase paid care. Combined with the reductions in children's caregiving time, these patterns are consistent with substitution between paid care and informal caregiving, although the evidence on paid care use is statistically imprecise.

Since programs offering both in-kind and cash benefits increase adult children's labor supply but reduce their caregiving time, an important question is whether this reduction affects parents' well-being. I use parents' self-reported life satisfaction, measured on a 1–5 scale in CHARLS, to examine this possibility. Column 5 shows that LTCI has little effect on parents' life satisfaction in cities providing in-kind benefits only. In contrast, in cities offering both in-kind and cash benefits, LTCI reduces life satisfaction by approximately 0.31 points, or 10% of the control-group mean, statistically significant at the 10% level. This negative effect becomes smaller by approximately 0.26 points during COVID-19, when the reduction in children's caregiving time is also attenuated. Furthermore, Figure 8 presents a residualized binned-scatter plot showing a positive association between parents' life satisfaction and children's caregiving time, after controlling for the covariates and fixed effects in the main regression. This pattern is consistent with the possibility that reductions in children's caregiving may be accompanied by lower parental life satisfaction.

Overall, the results suggest a potential trade-off between encouraging adult children's labor supply and protecting parents' well-being across LTCI benefit designs. Programs providing in-kind benefits only have limited effects on children's labor supply and caregiving time, but do not appear to reduce parents' life satisfaction. In contrast, programs offering both in-kind and cash benefits

significantly increase adult children's probability of working and reduce their caregiving time, but are also associated with lower parental life satisfaction. In China, where adult children are often the main earners in their households, particularly in one-child families, reducing caregiving burdens may ease financial pressures and encourage market work. However, less time spent caring for parents may come at a cost to parental well-being. These findings highlight the importance of balancing the goals of reducing informal caregiving burdens, encouraging labor supply, and improving older adults' well-being when designing LTCI benefits.

8 Robustness Checks

This section presents several robustness checks for the main findings on the effects of LTCI on adult children's labor supply, using both the full sample and the subsample of adult children with ADL/IADL-limited parents.

First, I include additional controls for parental household characteristics, including mean age, the share with rural hukou, family size, number of children, the share of non-Han members, and baseline ADL and IADL measures. Panel A of Table 12 shows that LTCI significantly increases adult children's probability of working in both samples, with estimated effects close to the baseline results.

Second, I use an alternative control group consisting of cities that implemented LTCI after 2020 or were designated by the central government in 2020 but had not yet implemented the program. This restriction helps address concerns that pilot cities differ systematically from non-pilot cities. Panel B shows positive effects on adult children's probability of working in both samples. Although the estimated magnitudes are larger than the baseline results, the findings remain consistent with a positive labor-supply effect of LTCI.

Third, I exclude adult children whose parents live in Qingdao and Weifang, which implemented LTCI before the program was first announced by the central government. Excluding these early adopters helps address potential concerns about selection into treatment. Panel C shows that LTCI continues to have statistically significant positive effects in both samples, with magnitudes similar to the baseline estimates.

Fourth, LTCI eligibility depends on enrollment in UEBMI and, in some pilot cities, URRBMI. In the baseline analysis, I assume that once an older adult is first observed to be enrolled in either program, their enrollment status remains unchanged thereafter. This assumption is more plausible for UEBMI, which is relatively stable among older adults, than for URRBMI, which is typically renewed annually. To assess the sensitivity of the results to potential changes in URRBMI enrollment, I exclude households in which parents are covered exclusively by URRBMI. Panel D shows that LTCI still significantly increases adult children's probability of working in both samples, with estimated effects close to the baseline results.

Fifth, I exclude the COVID-19 period (Wave 5) to examine the effects of LTCI outside the pandemic. Panel E shows statistically significant positive effects in both samples, with magnitudes larger than the baseline estimates.

Finally, I implement a propensity score matching and triple-difference (PSM–DDD) approach following Liu et al. (2023). Using 2013 as the baseline year, I estimate propensity scores based on observable characteristics and perform five-nearest-neighbor matching. I then re-estimate the DDD specification using the matched sample and matching weights. Panel F shows statistically significant positive effects on adult children's probability of working in both samples. Although the estimated magnitudes are larger than the baseline results, the findings remain consistent with the main analysis.

Overall, these robustness checks provide consistent evidence that LTCI increases adult children's probability of working, including among those with ADL/IADL-limited parents. The positive effects remain robust to alternative controls, control-group definitions, sample restrictions, and matching-based estimation.

9 Conclusion

This study provides evidence on how China's public LTCI program affects the labor-supply decisions of adult children, one of the main groups of informal caregivers. It examines the program's effect on adult children's likelihood of working, analyzes two factors that shape this effect, and compares the effectiveness of two benefit designs.

Exploiting the quasi-experimental rollout of China's public LTCI program and using CHARLS data from 2011 to 2020, I find that parental LTCI eligibility significantly increases adult children's likelihood of working. The positive effect remains robust when the analysis is restricted to adult children whose parents have ADL/IADL limitations, the group most likely to be targeted by the program.

The heterogeneity analysis shows that the program generates larger labor-supply effects among women, less-educated individuals, and adult children who co-reside with their parents. These groups are more likely to provide informal care in the absence of public LTCI. The effects are also larger among adult children with fewer siblings, older adult children, and those with very old parents. These patterns are consistent with the theoretical predictions and suggest that public LTCI particularly encourages labor-force participation among adult children who would otherwise face greater caregiving burdens.

The results for children's caregiving time show that parental LTCI eligibility significantly reduces caregiving on the intensive margin: adult children provide care on fewer days per month. This finding further supports the main labor-supply results, suggesting that adult children increase their labor supply while reducing the time they spend caring for their parents. It also indicates that, in China, the substitution effect of public LTCI is larger than the income effect, in contrast to the negative labor-supply effect found for Germany by Geyer and Korfhage (2015). Moreover, the results for intergenerational transfers suggest that LTCI encourages adult children to provide greater financial support to their parents. At the same time, parents appear to adjust their transfers to children more flexibly: they generally provide more support but sharply reduce the additional transfers during the

COVID-19 period. The increase in parental support to children is consistent with Liu et al. (2023), who find that LTCI eligibility reduces precautionary saving and increases non-health consumption among older households. During the COVID-19 period, however, parents appear to adjust these behaviors in response to heightened risk. Overall, the results indicate that LTCI reduces adult children's caregiving time and alleviates households' financial burden.

The study also shows that the labor-supply effects of LTCI are not uniform. The program's impact on adult children depends on both parental health status and pandemic-related shocks. Specifically, during the COVID-19 period, each additional parental ADL limitation significantly attenuates the program's positive labor-supply effect. Among adult children whose parents have severe ADL limitations, parental LTCI eligibility has no detectable effect on labor supply during this period. This pattern is consistent with the theoretical framework of Browning et al. (1985), which emphasizes that individuals' labor-supply decisions respond to family members' health conditions. Evaluations of LTCI policies should therefore account for heterogeneity in parental health and the presence of large aggregate shocks.

Furthermore, this study evaluates two benefit designs: an in-kind-only program and a program that offers both in-kind and cash benefits. Relative to the in-kind-only design, the mixed in-kind-and-cash design has a larger positive effect on adult children's labor supply and significantly reduces their caregiving time. This reduction in caregiving is associated with a significant decline in parents' life satisfaction. Policymakers should therefore carefully consider the trade-off between increasing adult children's labor supply and preserving parental well-being. Larger labor-supply gains may come at the cost of substantial reductions in caregiving time, which parents value and which may contribute to their life satisfaction. Future policy development and research should seek benefit designs that better balance these objectives.

Overall, the findings show that China's public LTCI program increases adult children's labor supply on average, although the magnitude of this effect depends on parental health and pandemic-related shocks. The study sheds light on how public long-term care programs shape the labor-supply decisions of working-age caregivers and, more broadly, labor-market outcomes. The findings also alleviate a common policy concern that LTCI may weaken work incentives: on average, the estimated effects are positive. At the same time, the analysis highlights the importance of benefit design. Programs that generate larger labor-supply responses among adult children may do so by reducing caregiving time, with potentially adverse consequences for parents' well-being. Policymakers should therefore consider the trade-off between increasing caregivers' labor supply and maintaining the caregiving time that supports parents' life satisfaction. One limitation of this study is that it identifies effects based on LTCI eligibility in pilot cities rather than actual benefit receipt or benefit intensity. Future research could use administrative or program data to identify households that receive LTCI benefits, measure benefit amounts, and examine how variation in benefit intensity affects adult children's labor supply.

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10 Figures and Tables

10.1 Figures

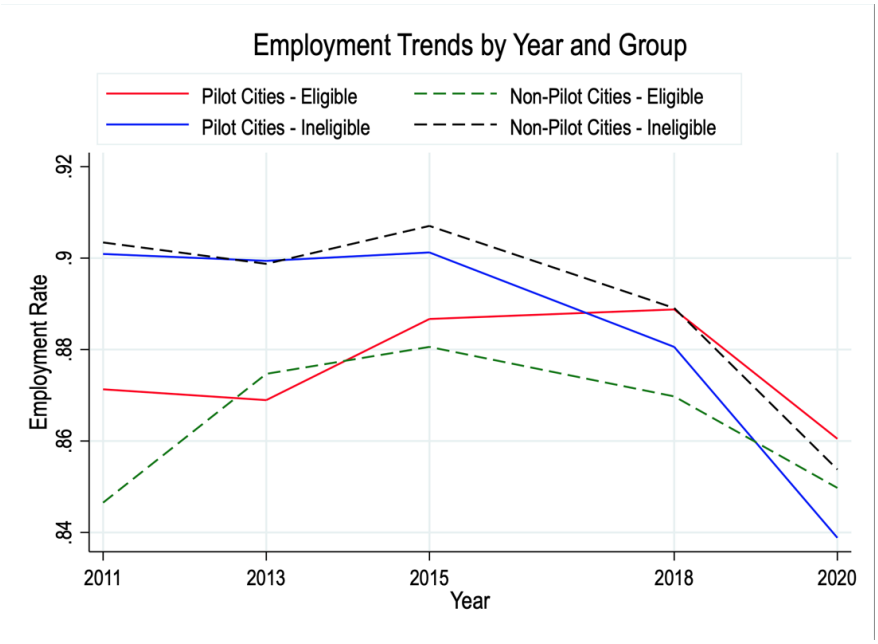


Figure 1: Employment Trends by Year and Group

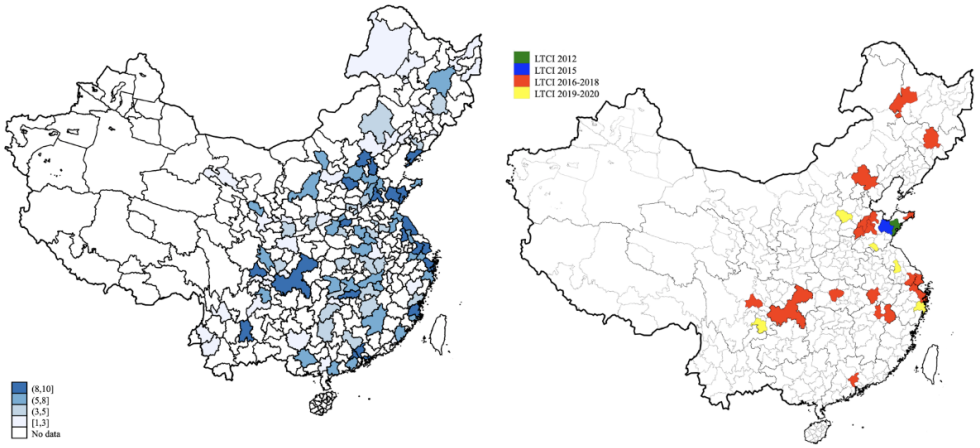


Figure 2: City-level GDP and LTCI implement

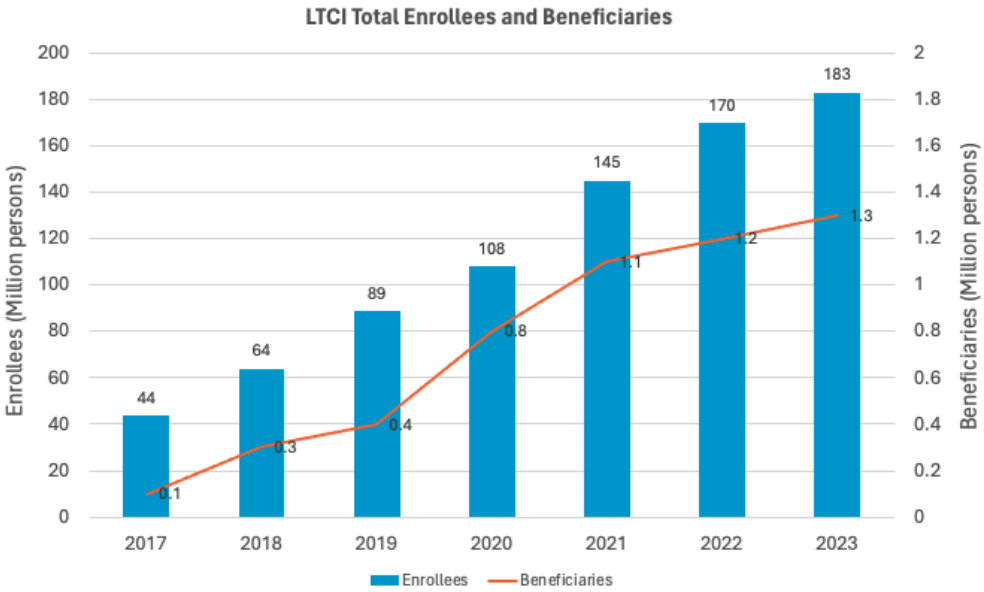


Figure 3: LTCI enrollees and beneficiaries

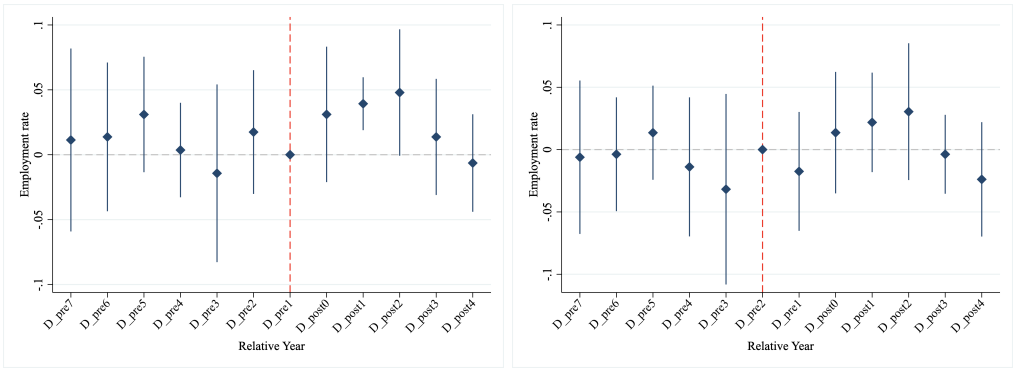


Figure 4: DDD event study

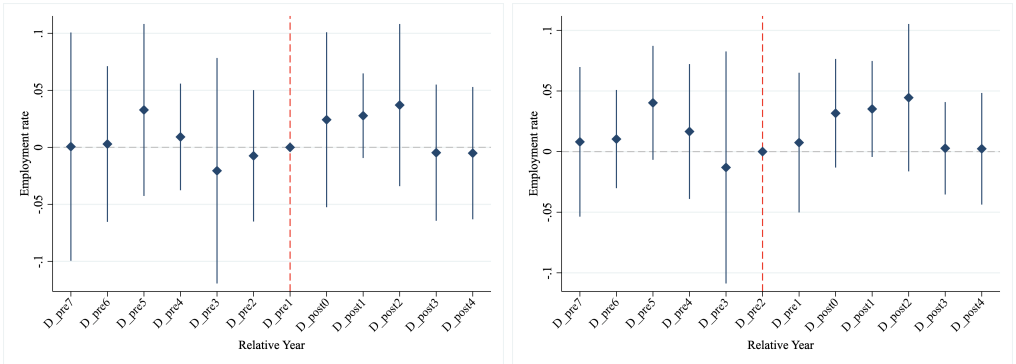


Figure 5: DDD event study among adult children with ADL/IADL parents

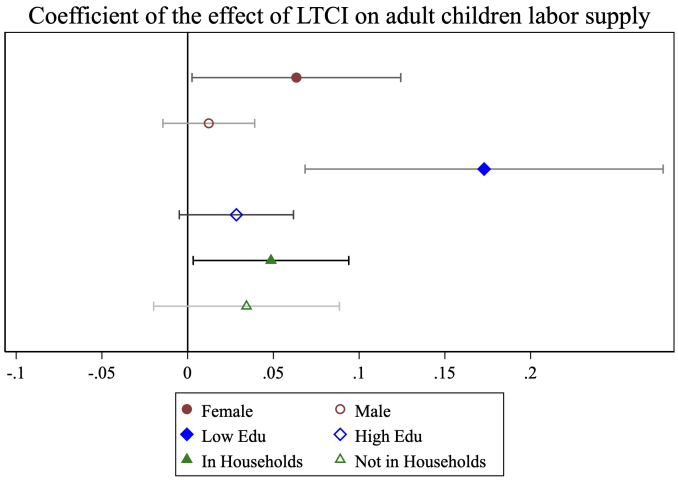


Figure 6: Effects of LTCI program on labor supply based on children's characteristics

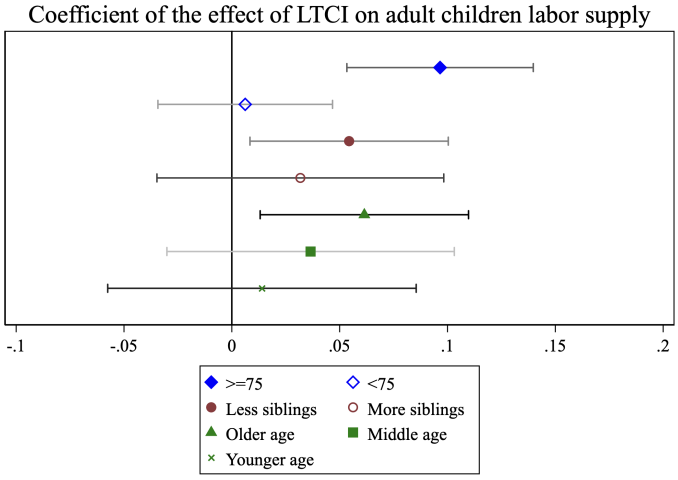


Figure 7: Effects of LTCI program on labor supply based on children's family structures

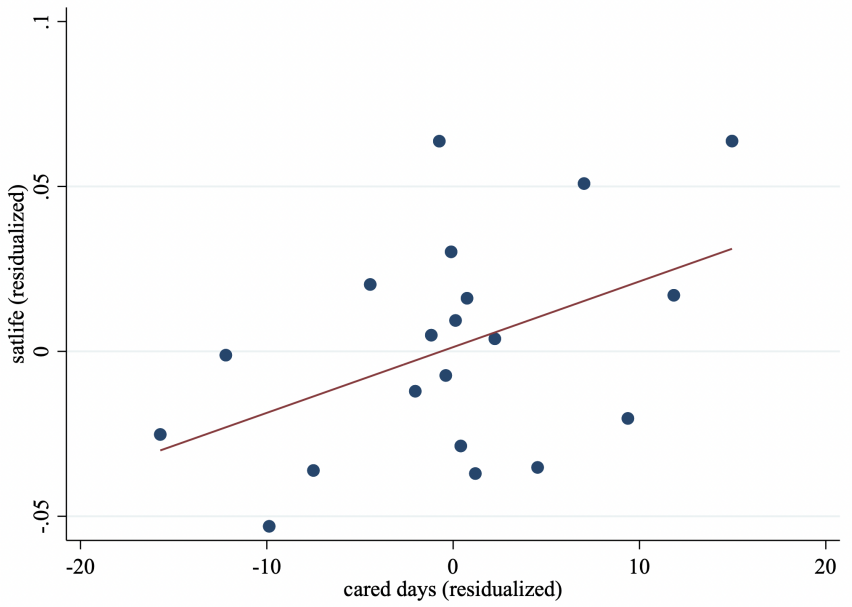


Figure 8: Residualized binned-scatter: parents' life satisfaction and adult children's caregiving time

10.2 Tables

10.2.1 Summary Statistics

Table 1: Summary Statistics (Full Sample)

Variable	**Obs**	**Mean**	**Std. Dev.**
Demographics			
Sex (Male=1)	88,679	0.530	0.499
Age	88,679	38.387	8.066
Marriage	88,679	0.862	0.345
Rural Hukou	88,679	0.762	0.426
Family size	88,679	3.096	1.769
Less than middle school	88,679	0.387	0.487
Middle school	88,679	0.341	0.474
High school	88,679	0.143	0.350
College or more	88,679	0.129	0.335
Any elderly (60+)	88,679	0.774	0.418
Any child	88,679	0.865	0.342
Eligibility and employment			
UEMI	88,679	0.190	0.393
URRMI	88,679	0.265	0.441
With eligible parent	88,679	0.206	0.405
From pilot cities	88,679	0.167	0.373
Employment (working)	88,679	0.884	0.320
Parents' ADL/IADL			
HH with any ADL	88,679	0.663	0.473
HH with any IADL	88,679	0.726	0.446
Number of ADLs	88,679	1.769	1.875
Number of IADLs	88,679	1.920	1.718

Table 2: Effects of LTCI on adult children's labor supply

	DDD	DiD (Among pilot cities)	DiD (Among eligible HH)
Elig*Pilot*Post	0.039**		
	(0.018)		
Elig*Pilot*Post*COVID-19	-0.033*		
	(0.018)		
Elig*Post		0.036***	
		(0.014)	
Elig*Post*COVID-19		-0.005	
		(0.015)	
Pilot*Post			0.044**
			(0.016)
Pilot*Post*COVID-19			-0.027
			(0.021)
Number of observations	88,679	14,769	18,297
Adjusted R^2	0.320	0.311	0.396
City by year FE	YES	NO	NO
Province by year FE	NO	YES	YES

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3: Effects of LTCI on adult children's labor supply with any ALD/IADL parents

	DDD	DiD (Among pilot cities)	DiD (Among eligible HH)
Elig*Pilot*Post	0.041**		
	(0.019)		
Elig*Pilot*Post*COVID-19	-0.031		
	(0.021)		
Elig*Post		0.040**	
		(0.015)	
Elig*Post*COVID-19		-0.008	
		(0.016)	
Pilot*Post			0.060***
			(0.020)
Pilot*Post*COVID-19			-0.025
			(0.031)
Number of observations	71,786	11,178	12,870
Adjusted R^2	0.317	0.315	0.399
City by year FE	YES	NO	NO
Province by year FE	NO	YES	YES

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 4: Effects of LTCI on adult children's labor supply with any ADL parents

	DDD	DiD (Among pilot cities)	DiD (Among eligible HH)
Elig*Pilot*Post	0.025		
	(0.023)		
Elig*Pilot*Post*COVID-19	-0.028		
	(0.027)		
Elig*Post		0.025	
		(0.011)	
Elig*Post*COVID-19		-0.002	
		(0.022)	
Pilot*Post			0.055***
			(0.025)
Pilot*Post*COVID-19			-0.027
			(0.038)
Number of observations	58,757	8,615	10,189
Adjusted R^2	0.313	0.323	0.332
City by year FE	YES	NO	NO
Province by year FE	NO	YES	YES

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 5: Effects of LTCI on adult children's labor supply with any ADL/IADL eligible parents

	DDD	DiD (Among pilot cities)	DiD (Among eligible HH)
Elig*Pilot*Post	0.039*		
	(0.022)		
Elig*Pilot*Post*COVID-19	-0.036		
	(0.024)		
Elig*Post		0.032*	
		(0.017)	
Elig*Post*COVID-19		-0.003	
		(0.017)	
Pilot*Post			0.071***
			(0.021)
Pilot*Post*COVID-19			-0.052*
			(0.031)
Number of observations	69,503	10,786	10,587
Adjusted R^2	0.314	0.312	0.400
City by year FE	YES	NO	NO
Province by year FE	NO	YES	YES

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 6: LTCI effects by parental ADL/IADL and COVID-19

	DDD	DiD (Pilot cities)	DiD (Eligible HH)
Elig*Pilot*Post	0.038*		
	(0.021)		
Elig*Pilot*Post*COVID-19	-0.006		
	(0.018)		
Elig*Pilot*Post*Num of DL	0.001		
	(0.002)		
Elig*Pilot*Post*Num of DL*COVID-19	-0.011***		
	(0.004)		
Elig*Post		0.031*	
		(0.017)	
Elig*Post*COVID-19		0.024	
		(0.014)	
Elig*Post*Num of DL		0.002	
		(0.002)	
Elig*Post*Num of DL*COVID-19		-0.012***	
		(0.004)	
Pilot*Post			0.036**
			(0.017)
Pilot*Post*COVID-19			0.007
			(0.020)
Pilot*Post*Num of DL			0.003
			(0.002)
Pilot*Post*Num of DL*COVID-19			-0.013***
			(0.004)
Number of observations	88,679	14,769	18,297
Adjusted R^2	0.320	0.311	0.396

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 7: LTCI effects by parental ADL and COVID-19

	DDD	DiD (Pilot cities)	DiD (Eligible HH)
Elig*Pilot*Post	0.045**		
	(0.022)		
Elig*Pilot*Post*COVID-19	-0.017		
	(0.019)		
Elig*Pilot*Post*Num of ADL	-0.004		
	(0.005)		
Elig*Pilot*Post*Num of ADL*COVID-19	-0.015		
	(0.010)		
Elig*Post		0.039**	
		(0.018)	
Elig*Post*COVID-19		0.012	
		(0.016)	
Elig*Post*Num of ADL		-0.002	
		(0.005)	
Elig*Post*Num of ADL*COVID-19		-0.017*	
		(0.010)	
Pilot*Post			0.045***
			(0.015)
Pilot*Post*COVID-19			-0.006
			(0.021)
Pilot*Post*Num of ADL			0.000
			(0.006)
Pilot*Post*Num of ADL*COVID-19			-0.018*
			(0.010)
Number of observations	88,679	14,769	18,297
Adjusted R^2	0.320	0.311	0.396

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 8: The effect of LTCI on children's care (DDD)

	Extensive (Indicator)	Intensive (days per month)	Total (days per month)
Elig*Pilot*Post	-0.017	-9.432**	-5.026*
	(0.091)	(3.670)	(2.727)
Elig*Pilot*Post*COVID-19	0.077	10.851***	5.609**
	(0.093)	(3.683)	(2.412)
Number of observations	9,020	2,858	8,184
Adjusted R^2	0.252	0.287	0.300

Table 9: The effect of LTCI on economic support

	Extensive (Indicator)	Intensive ($ln(CNY)$)	Total $ln(CNY+1)$
Panel A: Support from children to parents			
Elig*Pilot*Post	0.043	0.236	0.352
	(0.040)	(0.148)	(0.356)
Elig*Pilot*Post*COVID-19	-0.029	-0.067	-0.067
	(0.049)	(0.161)	(0.397)
Number of observations	23,004	17,360	23,004
Adjusted R^2	0.349	0.855	0.605
Panel B: Support from parents to children			
Elig*Pilot*Post	0.027	0.510**	0.481
	(0.047)	(0.243)	(0.327)
Elig*Pilot*Post*COVID-19	-0.137*	-0.790**	-1.258**
	(0.069)	(0.329)	(0.539)
Number of observations	23,004	6,945	23,004
Adjusted R^2	0.188	0.721	0.286

Table 10: The effect of LTCI on economic support

	Net children sup CNY	Log-ratio
Elig*Pilot*Post	-750.63	-0.129
	(1278.33)	(0.415)
Elig*Pilot*Post*COVID-19	1441.874	1.190**
	(1087.27)	(0.491)
Number of observations	23,004	23,004
Adjusted R^2	0.232	0.324

Table 11: The effect of LTCI on employment

	Labor supply (Indv)	Child care (HH)	Institutional care (HH)	Paid care (HH)	Life satisfaction (HH)
Panel A: In-kind only					
Elig*Pilot*Post	0.028	-5.163	0.025*	0.004	-0.039
	(0.025)	(5.124)	(0.014)	(0.027)	(0.155)
Elig*Pilot*Post*COVID-19	-0.030	14.238***	0.014	0.003	0.008
	(0.026)	(4.805)	(0.023)	(0.012)	(0.173)
Number of observations	67,955	2,697	10,970	10,970	8,700
Adjusted R^2	0.318	0.366	0.145	0.280	0.318
Panel B: In-kind & cash					
Elig*Pilot*Post	0.066***	-13.537***	0.009	0.013	-0.305*
	(0.019)	(3.144)	(0.005)	(0.012)	(0.173)
Elig*Pilot*Post*COVID-19	-0.034	8.221**	-0.015	0.018	0.264*
	(0.028)	(4.135)	(0.018)	(0.011)	(0.154)
Number of observations	64,439	2,605	10,538	10,538	8,388
Adjusted R^2	0.321	0.379	0.174	0.256	0.323

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 12: Effects of LTCI on adult children's labor supply with any ADL/IADL

	All children	Children with ADLs/IADLs parents
Panel A: Additional control variables		
Elig*Pilot*Post	0.039**	0.042**
	(0.018)	(0.019)
Number of observations	88,679	71,786
Adjusted R^2	0.320	0.317
Panel B: Alternative control group		
Elig*Pilot*Post	0.063***	0.064**
	(0.022)	(0.024)
Number of observations	18,740	14,204
Adjusted R^2	0.328	0.335
Panel C: Exclude Qingdao & Weifang		
Elig*Pilot*Post	0.044**	0.041**
	(0.019)	(0.019)
Number of observations	87,744	71,786
Adjusted R^2	0.320	0.317
Panel D: Exclude only URRBMI households		
Elig*Pilot*Post	0.041**	0.041**
	(0.018)	(0.020)
Number of observations	87,264	70,961
Adjusted R^2	0.321	0.318
Panel E: Excluding the COVID-19 period		
Elig*Pilot*Post	0.052***	0.054***
	(0.019)	(0.020)
Number of observations	66,062	54,797
Adjusted R^2	0.304	0.298
Panel F: PSM-DDD		
Elig*Pilot*Post	0.064*	0.066**
	(0.033)	(0.030)
Number of observations	14,484	10,316
Adjusted R^2	0.327	0.340

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

A Summary Statistics

Table A1 reports summary statistics for adult children with any ADL/IADL-limited parents. In this subsample, the share of children with at least one parent covered by UEBMI is 17%, which is slightly lower than in the full sample, while the share with at least one parent covered by URRBMI remains 27%. The fraction of observations with any eligible parent is 16%, also slightly lower than in the full sample. The mean of the working indicator is 0.88, implying that about 88% of children are employed, which is very similar to the full sample. Furthermore, 82% of observations have at least one parent with an ADL limitation, and nearly 90% have at least one parent with an IADL limitation. The average number of ADL limitations among parents is 2.2, and the average number of IADL limitations is 2.4.

Table A1: Summary Statistics (With any ADL/IADL parents)

Variable	**Obs**	**Mean**	**Std. Dev.**
Demographics			
Sex (Male=1)	71,786	0.530	0.499
Age	71,786	39.102	7.987
Marriage	71,786	0.873	0.333
Rural Hukou	71,786	0.790	0.407
Family size	71,786	3.108	1.777
Less than middle school	71,786	0.428	0.495
Middle school	71,786	0.341	0.474
High school	71,786	0.130	0.336
College or more	71,786	0.101	0.302
Any elderly (60+)	71,786	0.815	0.388
Any child	71,786	0.884	0.320
Eligibility and employment			
UEMI	71,786	0.168	0.374
URRMI	71,786	0.265	0.442
With eligible parent	71,786	0.179	0.384
From pilot cities	71,786	0.156	0.363
Employment (working)	71,786	0.882	0.323
Parents' ADL/IADL			
HH with any ADL	71,786	0.819	0.385
HH with any IADL	71,786	0.897	0.305
Number of ADLs	71,786	2.185	1.852
Number of IADLs	71,786	2.371	1.605

B Heterogeneous Effects of LTCI

Table B1: Heterogeneous effects: Children's characteristics (DDD)

	Female	Male	Low Edu	High Edu	In HHs	Not in HHs
Elig*Pilot*Post	0.063*	0.012	0.173***	0.028	0.049*	0.034
	(0.037)	(0.016)	(0.063)	(0.020)	(0.027)	(0.033)
Elig*Pilot*Post*COVID-19	-0.062	0.011	-0.253***	-0.039	-0.062**	-0.020
	(0.041)	(0.015)	(0.074)	(0.027)	(0.024)	(0.031)
Number of observations	33,705	38,081	25,872	33,264	21,138	49,873
Adjusted R^2	0.307	0.263	0.287	0.342	0.328	0.317

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table B2: Heterogeneous effects: Children's family structures (DDD)

	≥ 75	≤ 75	Less sib	More sib	One child	Older	Middle	Younger
Elig*Pilot*Post	0.091***	0.006	0.054*	0.032	0.141**	0.061*	0.037	0.014
	(0.032)	(0.024)	(0.028)	(0.040)	(0.065)	(0.029)	(0.035)	(0.043)
Elig*Pilot*Post*COVID-19	-0.021	-0.040	-0.065**	-0.053	-0.175**	-0.077**	0.023	-0.020
	(0.048)	(0.035)	(0.031)	(0.036)	(0.081)	(0.035)	(0.044)	(0.053)
Number of observations	23,765	48,018	22,462	46,641	3,557	24,881	22,232	24,663
Adjusted R^2	0.319	0.329	0.347	0.313	0.321	0.337	0.323	0.297

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

C Further Evidence on Parental ALD/IADL

Table C1: LTCI effects by parental IADL and COVID-19

	DDD	DiD (Pilot cities)	DiD (Eligible HH)
Elig*Pilot*Post	0.051		
	(0.037)		
Elig*Pilot*Post*COVID-19	-0.023		
	(0.035)		
Elig*Pilot*Post*Num of IADL	-0.002		
	(0.011)		
Elig*Pilot*Post*Num of IADL*COVID-19	-0.001		
	(0.011)		
Elig*Post		0.043	
		(0.029)	
Elig*Post*COVID-19		-0.002	
		(0.029)	
Elig*Post*Num of IADL		0.000	
		(0.013)	
Elig*Post*Num of IADL*COVID-19		-0.002	
		(0.012)	
Pilot*Post			0.016
			(0.023)
Pilot*Post*COVID-19			-0.031
			(0.026)
Pilot*Post*Num of IADL			0.008
			(0.012)
Pilot*Post*Num of IADL*COVID-19			-0.008
			(0.014)
Number of observations	29,922	6,154	8,106
Adjusted R^2	0.324	0.292	0.382

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table C2: LTCI effects by parental high IADL level and COVID-19

	DDD	DiD (Pilot cities)	DiD (Eligible HH)
Elig*Pilot*Post	0.050		
	(0.037)		
Elig*Pilot*Post*COVID-19	-0.024		
	(0.033)		
Elig*Pilot*Post*High IADL	-0.050		
	(0.051)		
Elig*Pilot*Post*High IADL*COVID-19	0.040		
	(0.048)		
Elig*Post		0.043	
		(0.029)	
Elig*Post*COVID-19		-0.003	
		(0.028)	
Elig*Post*High IADL		-0.035	
		(0.034)	
Elig*Post*High IADL*COVID-19		0.033	
		(0.038)	
Pilot*Post			0.020
			(0.022)
Pilot*Post*COVID-19			-0.035
			(0.027)
Pilot*Post*High IADL			-0.031
			(0.026)
Pilot*Post*High IADL*COVID-19			0.027
			(0.027)
Number of observations	29,922	6,154	8,106
Adjusted R^2	0.324	0.292	0.381

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

D Additional Results on Informal Caregiving and Intergenerational Transfers

This section provides further evidence on the effects of parental LTCI eligibility on informal care received from adult children and intergenerational transfers between parents and their adult children.

D.1 Summary Statistics

Table D1 presents summary statistics on informal caregiving and intergenerational financial transfers by parental LTCI eligibility and pilot city status. In pilot cities, eligible parents are less likely to receive care from their children and receive fewer caregiving days per month than ineligible parents. Eligible parents are also less likely to receive financial support from their children but more likely to provide financial support to them. Similar differences in caregiving and financial transfers are observed between eligible and ineligible parents in non-pilot cities. These descriptive patterns provide preliminary evidence of differences across groups, without implying causal effects of LTCI.

Table D1: Summary Statistics by Eligibility and Pilot City Status

Variable	Elig, Pilot	Inelig, Pilot	Elig, Non-Pilot	Inelig, Non-Pilot
	Mean (S.D.)	Mean (S.D.)	Mean (S.D.)	Mean (S.D.)
Children's care	0.384 (0.487)	0.506 (0.500)	0.446 (0.497)	0.521 (0.500)
Observations	354	1,163	1,188	6,315
Children's care (D/M)	6.054 (11.625)	7.276 (12.300)	6.988 (12.206)	7.591 (12.528)
Observations	334	1,075	1,147	5,897
Children's care (D/M)	13.945 (14.202)	13.510 (14.027)	14.313 (14.157)	13.619 (14.124)
Observations	145	579	560	3,287
Econ support from children	0.673 (10.469)	0.764 (0.424)	0.700 (0.458)	0.813 (0.390)
Observations	1,184	3,043	3,458	15,319
Econ support to children	0.435 (0.496)	0.354 (0.478)	0.482 (0.500)	0.369 (0.483)
Observations	1,184	3,043	3,458	15,319
Amount from children	4723.97 (6207.44)	4346.63 (5844.48)	5406.90 (6429.15)	4641.11 (5279.88)
Observations	733	2,118	2,218	11,256
Amount to children	6493.54 (9268.99)	2884.72 (5971.48)	5720.47 (8211.80)	2705.89 (5786.46)
Observations	451	995	1,494	5,184

D.2 Additional Results on Informal Caregiving

Table D2 presents additional estimates of LTCI's effects on caregiving provided by children or grandchildren, comparing results from samples that exclude and include the COVID-19 period. Column 1 shows no significant effects on the extensive margin when COVID-19 observations are

excluded, while including them yields a small positive estimate of 3.0 percentage points (5.9% of the control-group mean). Column 2 shows that LTCI significantly reduces caregiving time by approximately 10 days per month (63.7% of the control-group mean) when COVID-19 observations are excluded, whereas the estimated reduction is smaller when these observations are included. Similarly, Column 3 shows a reduction in total caregiving days when COVID-19 observations are excluded, while the estimate remains negative but statistically insignificant when they are included. Overall, these findings suggest that LTCI primarily reduces the intensity of informal caregiving, with weaker effects when the COVID-19 period is included.

Table D2: The effect of LTCI on children's care (DDD)

	Extensive (Indicator)	Intensive (days per month)	Total (days per month)
Panel A: Including COVID-19			
Elig*Pilot*Post	0.030	-3.076	-1.590
	(0.083)	(4.248)	(2.119)
Number of observations	9,020	2,858	8,184
Adjusted R^2	0.252	0.286	0.299
Panel B: Before COVID-19			
Elig*Pilot*Post	-0.017	-10.200**	-5.980*
	(0.090)	(4.894)	(3.606)
Number of observations	5,970	1,753	5,354
Adjusted R^2	0.312	0.235	0.267

D.3 Additional Results on Intergenerational Transfers

Appendix Tables D3 and D4 present additional estimates of LTCI's effects on intergenerational financial transfers, comparing specifications that exclude and include COVID-19 observations.

Transfers from children to parents. Table D3 shows that LTCI has positive but statistically insignificant effects on the probability that children provide financial support to parents, both excluding and including COVID-19 observations. The estimated effect is smaller when the pandemic period is included. Columns 2 and 3 also report positive but statistically insignificant effects on the intensive margin and total transfer amounts, respectively, with limited differences across the two samples. The estimates for net financial support similarly show limited effects, while the relative transfer measure indicates a positive change during the pandemic. Overall, these findings suggest that LTCI may encourage financial transfers from children to parents, although the estimates are statistically imprecise.

Transfers from parents to children. Table D4 shows that LTCI has limited effects on the extensive margin when COVID-19 observations are excluded. When these observations are included, the estimated effect becomes negative at 5.1 percentage points, equivalent to 13.3% of the control-group mean, although it is not statistically significant. On the intensive margin, LTCI increases transfers from parents to children by approximately 50% when COVID-19 observations are excluded, whereas the estimated effect becomes limited when they are included. Column 3 shows a similar pattern for total transfers, with a small positive effect when excluding COVID-19 observations and a negative estimated effect when including them. These results suggests that LTCI increases parental financial support in normal times, but this positive effect weakens during the COVID-19 period.

Overall, the additional estimates are consistent with the main findings that LTCI may increase intergenerational financial transfers, particularly from parents to children, while the inclusion of COVID-19 observations weakens the estimated positive effects on parental transfers.

Table D3: The effect of LTCI on economic support from children

	Extensive (Indicator)	Intensive ($ln(CNY)$)	Total $ln(CNY+1)$	Net sup CNY	Log-ratio
Panel A: Including COVID-19					
Elig*Pilot*Post	0.027	0.198*	0.313	76.29	0.554*
	(0.024)	(0.115)	(0.200)	(824.70)	(0.306)
Number of observations	23,004	17,360	23,004	23,004	23,004
Adjusted R^2	0.349	0.855	0.605	0.232	0.324
Panel B: Excluding COVID-19					
Elig*Pilot*Post	0.043	0.205	0.344	-270.60	0.235
	(0.040)	(0.161)	(0.355)	(1399.39)	(0.450)
Number of observations	17,421	12,180	17,421	17,421	17,421
Adjusted R^2	0.335	0.882	0.609	0.210	0.328

Table D4: The effect of LTCI on economic support to children

	Extensive (Indicator)	Intensive ($ln(CNY)$)	Total $ln(CNY+1)$
Panel A: Including COVID-19			
Elig*Pilot*Post	-0.051	0.031	-0.241
	(0.034)	(0.220)	(0.243)
Number of observations	23,004	6,945	23,004
Adjusted R^2	0.188	0.721	0.286
Panel B: Excluding COVID-19			
Elig*Pilot*Post	-0.013	0.407	0.109
	(0.048)	(0.371)	(0.331)
Number of observations	17,421	4,023	17,421
Adjusted R^2	0.159	0.748	0.262

E Further Placebo Tests

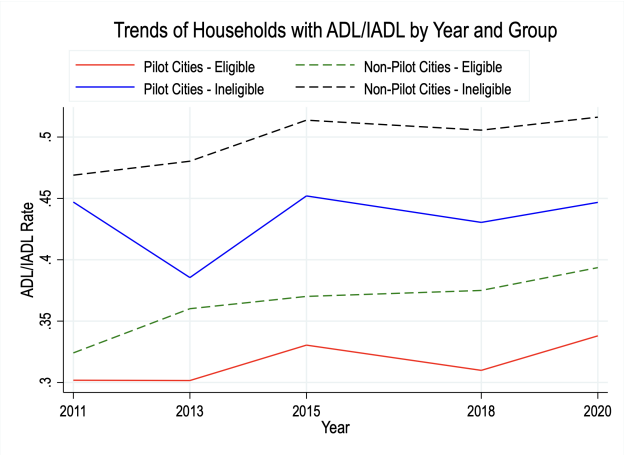


Figure E1: Trend of ADL/IADL

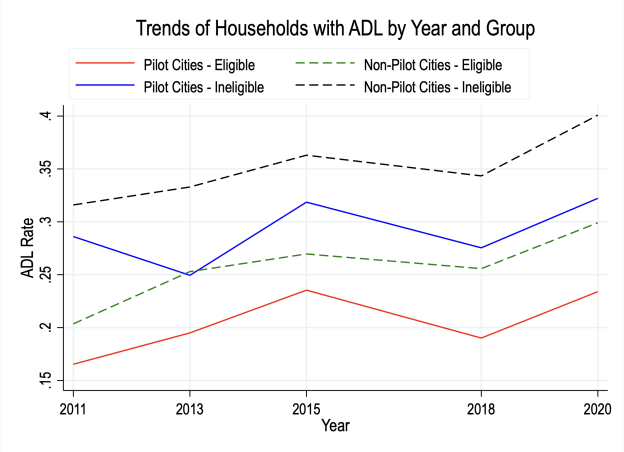


Figure E2: Trend of ADL

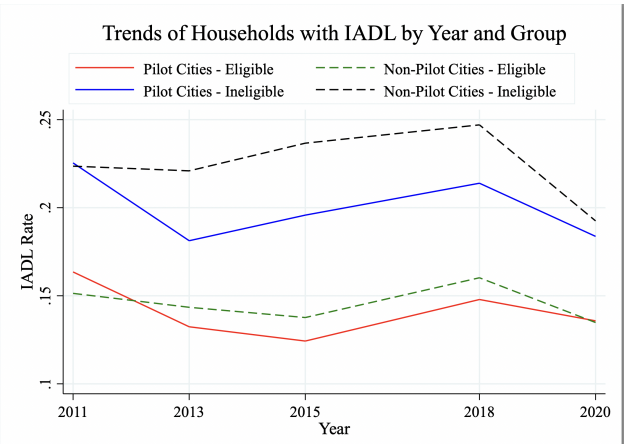


Figure E3: Trend of IADL

Table E1: Placebo test based on ADL/IADL indicator

	DiD (Pilot cities)	DiD (Eligible HH)	DDD
Panel A: Including COVID-19			
Elig*Post	0.025		
	(0.027)		
Pilot*Post		0.012	
		(0.025)	
Elig*Pilot*Post			0.014
			(0.033)
Number of observations	6,133	7,588	30,497
Adjusted R^2	0.327	0.372	0.329
Panel B: Before COVID-19			
Elig*Post	0.053		
	(0.039)		
Pilot*Post		0.034	
		(0.034)	
Elig*Pilot*Post			0.022
			(0.043)
Number of observations	3,731	5,747	23,129
Adjusted R^2	0.326	0.377	0.329
City by year FE	NO	NO	YES
Province by year FE	NO	YES	NO

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table E2: Placebo test based on ADL indicator

	DiD (Pilot cities)	DiD (Eligible HH)	DDD
Panel A: Including COVID-19			
Elig*Post	0.005		
	(0.017)		
Pilot*Post		0.002	
		(0.016)	
Elig*Pilot*Post			-0.007
			(0.029)
Number of observations	6,133	7,588	30,497
Adjusted R^2	0.397	0.455	0.408
Panel B: Before COVID-19			
Elig*Post	0.036		
	(0.036)		
Pilot*Post		0.027	
		(0.029)	
Elig*Pilot*Post			0.027
			(0.045)
Number of observations	3,731	5,747	23,129
Adjusted R^2	0.314	0.365	0.332
City by year FE	NO	NO	YES
Province by year FE	NO	YES	NO

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table E3: Placebo test based on the number of ADL

	DiD (Pilot cities)	DiD (Eligible HH)	DDD
Panel A: Including COVID-19			
Elig*Post	0.012		
	(0.058)		
Pilot*Post		-0.018	
		(0.049)	
Elig*Pilot*Post			-0.031
			(0.095)
Number of observations	6,133	7,588	30,497
Adjusted R^2	0.397	0.445	0.408
Panel B: Before COVID-19			
Elig*Post	0.061		
	(0.137)		
Pilot*Post		0.036	
		(0.077)	
Elig*Pilot*Post			0.002
			(0.170)
Number of observations	3,731	5,747	23,129
Adjusted R^2	0.401	0.487	0.422
City by year FE	NO	NO	YES
Province by year FE	NO	YES	NO

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table E4: Placebo test based on IADL indicator among households with no ADL

	DiD (Pilot cities)	DiD (Eligible HH)	DDD
Panel A: Including COVID-19			
Elig*Post	0.023		
	(0.030)		
Pilot*Post		0.008	
		(0.025)	
Elig*Pilot*Post			0.035
			(0.034)
Number of observations	4,327	5,459	19,394
Adjusted R^2	0.193	0.216	0.172
Panel B: Before COVID-19			
Elig*Post	0.029		
	(0.030)		
Pilot*Post		0.017	
		(0.028)	
Elig*Pilot*Post			0.001
			(0.043)
Number of observations	2,549	4,119	14,656
Adjusted R^2	0.192	0.195	0.151
City by year FE	NO	NO	YES
Province by year FE	NO	YES	NO

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table E5: Placebo test based on the number of IADL among households with no ADL

	DiD (Pilot cities)	DiD (Eligible HH)	DDD
Panel A: Including COVID-19			
Elig*Post	0.039		
	(0.063)		
Pilot*Post		0.029	
		(0.052)	
Elig*Pilot*Post			0.092
			(0.075)
Number of observations	4,327	5,459	19,394
Adjusted R^2	0.290	0.270	0.242
Panel B: Before COVID-19			
Elig*Post	0.108		
	(0.071)		
Pilot*Post		0.083	
		(0.063)	
Elig*Pilot*Post			0.102
			(0.074)
Number of observations	2,549	4,119	14,656
Adjusted R^2	0.284	0.251	0.219
City by year FE	NO	NO	YES
Province by year FE	NO	YES	NO

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table E6: Placebo test based on IADL indicator

	DiD (Pilot cities)	DiD (Eligible HH)	DDD
Panel A: Including COVID-19			
Elig*Post	0.061*		
	(0.031)		
Pilot*Post		0.038	
		(0.027)	
Elig*Pilot*Post			0.048
			(0.034)
Number of observations	6,133	7,587	30,496
Adjusted R^2	0.320	0.335	0.310
Panel B: Before COVID-19			
Elig*Post	0.077*		
	(0.039)		
Pilot*Post		0.044	
		(0.028)	
Elig*Pilot*Post			0.030
			(0.046)
Number of observations	3,731	5,747	23,129
Adjusted R^2	0.330	0.346	0.312
City by year FE	NO	NO	YES
Province by year FE	NO	YES	NO

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table E7: Placebo test based on the number of IADL

	DiD (Pilot cities)	DiD (Eligible HH)	DDD
Panel A: Including COVID-19			
Elig*Post	0.143**		
	(0.066)		
Pilot*Post		0.087	
		(0.062)	
Elig*Pilot*Post			0.124**
			(0.063)
Number of observations	6,133	7,587	30,496
Adjusted R^2	0.446	0.445	0.418
Panel B: Before COVID-19			
Elig*Post	0.218**		
	(0.086)		
Pilot*Post		0.148**	
		(0.061)	
Elig*Pilot*Post			0.144*
			(0.079)
Number of observations	3,731	5,747	23,129
Adjusted R^2	0.463	0.478	0.440
City by year FE	NO	NO	YES
Province by year FE	NO	YES	NO

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table E8: Placebo Tests

	ADL/IADL (Indicator)	ADL (Indicator)	ADL (Number)	IADL (Indicator)	IADL (Number)
Panel A: Including COVID-19					
Eligible × Pilot × Post	0.014	-0.007	-0.031	0.035	0.092
	(0.033)	(0.029)	(0.095)	(0.034)	(0.075)
Observations	30,497	30,497	30,497	19,394	19,394
Adjusted R^2	0.329	0.408	0.408	0.172	0.242
Panel B: Before COVID-19					
Eligible × Pilot × Post	0.022	0.027	0.002	0.001	0.102
	(0.043)	(0.045)	(0.170)	(0.043)	(0.074)
Observations	23,129	23,129	23,129	14,656	14,656
Adjusted R^2	0.329	0.332	0.422	0.151	0.219

Standard errors are reported in parentheses.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

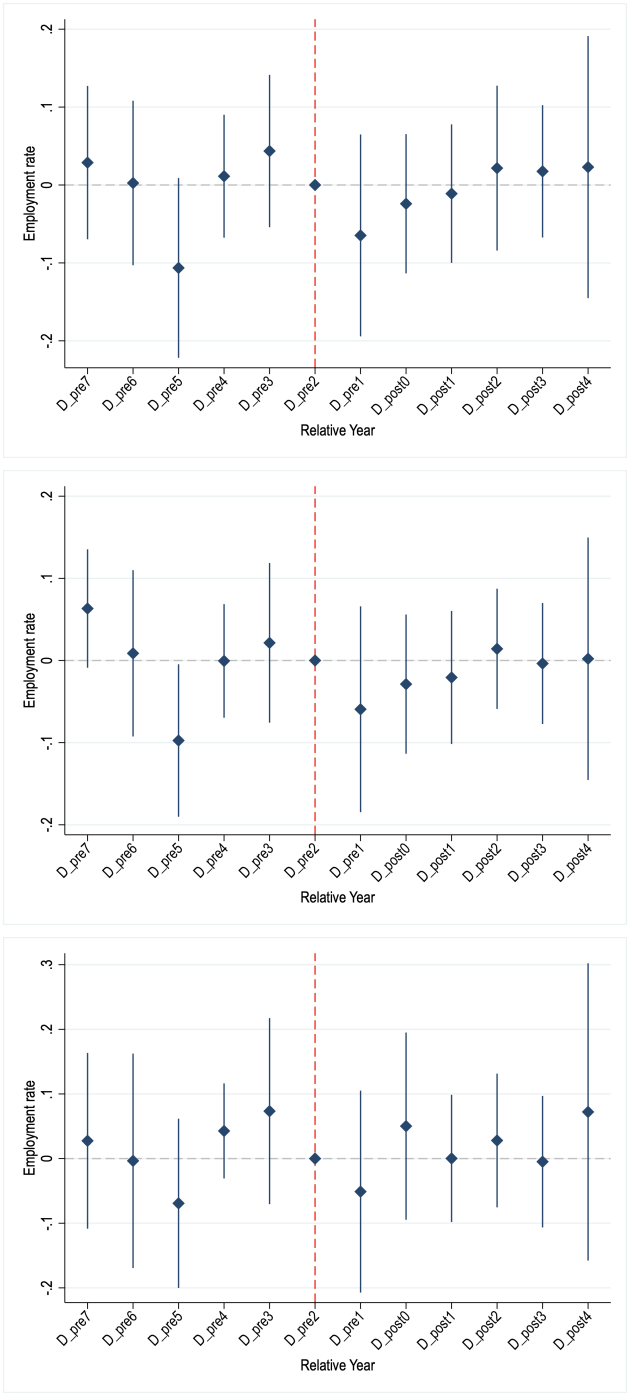


Figure E4: Placebo event study for ADL/IADL

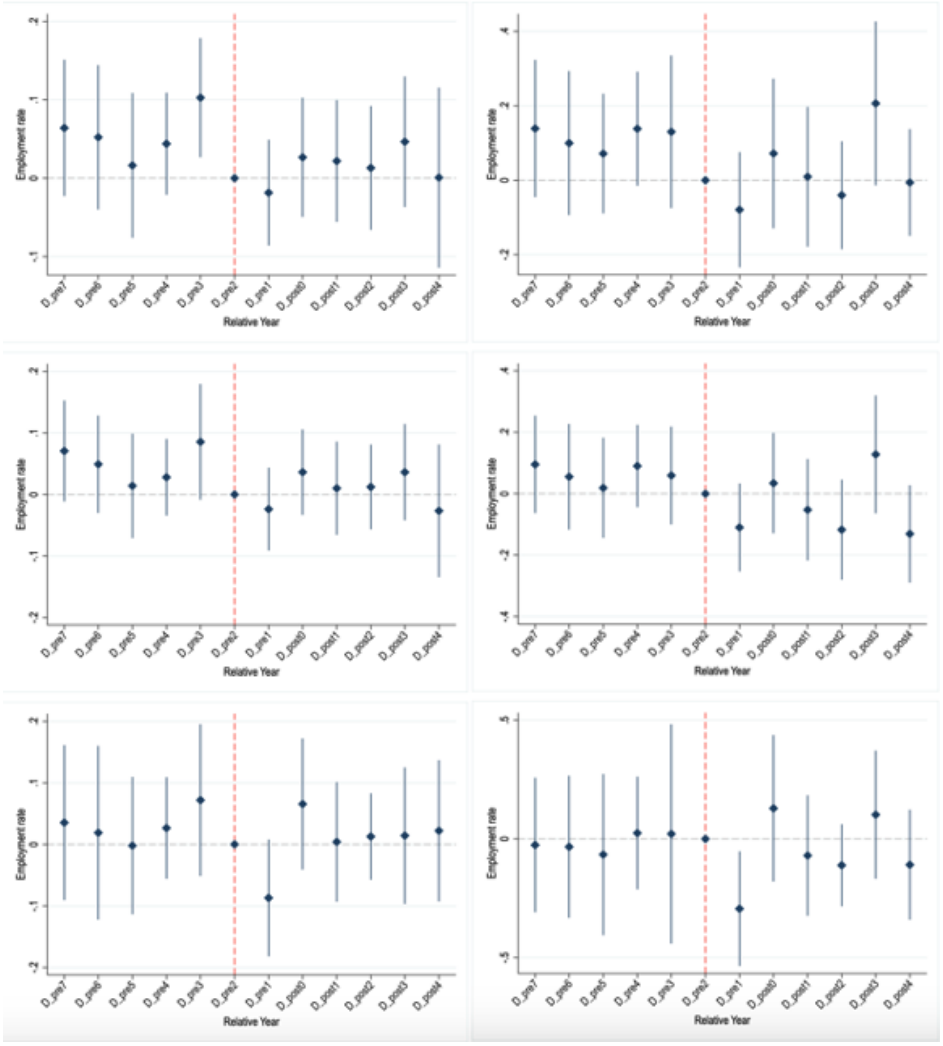


Figure E5: Placebo event study for ADL and the number of ADL

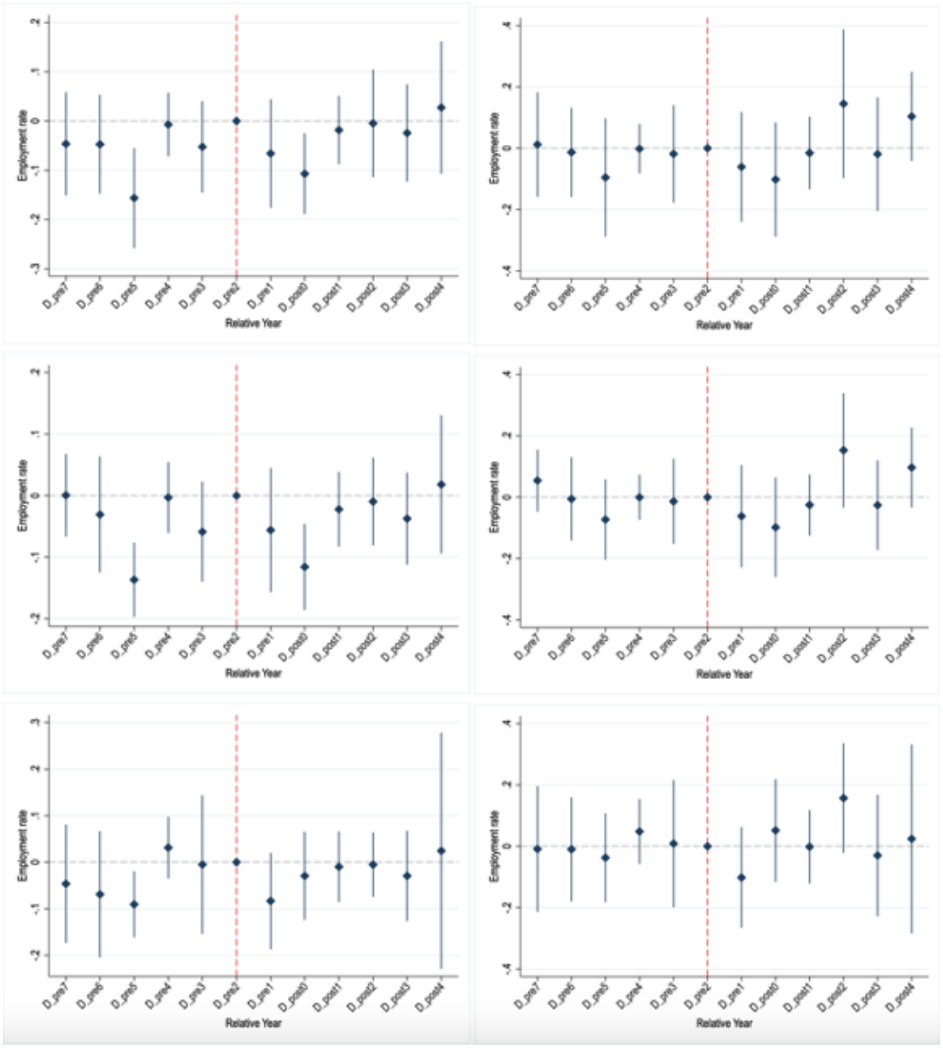


Figure E6: Placebo event study for IADL and the number of IADL