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Family Spillovers and Long-term Care Insurance

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Abstract

We examine how long-term care insurance (LTCI) affects informal care use and expectations among the insured individuals and co-residence and labor market outcomes of their adult children. We address the endogeneity of LTCI coverage by instrumenting for LTCI with changes in state tax treatment of LTCI insurance policies. We do not find evidence of reductions in informal care use over a horizon of approximately eight years. However, we find that LTCI coverage reduces parents' perceptions of the willingness of their children to care for them in the future and that the behavior of adult children changes, with LTCI resulting in lower likelihoods of adult children co-residing and stronger labor market attachment. These findings provide empirical support for the presence of spillovers of LTCI on the economic behaviors of family members.

Keywords

long-term care insurance; informal care; intra-family behavioral response; co-residence; work; expectations; family behavior; G22; H31; H51; H71; H75; I11; I18; I38; J14

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author statement

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I. INTRODUCTION

Long-term care (LTC) is a large and growing expenditure risk facing older adults. Approximately 70 percent of adults who survive to age 65 develop severe long-term care needs prior to death, defined as having difficulty with two or more activities of daily living (ADLs) expected to last at least 90 days, severe cognitive impairment, or receiving paid long-term care (ASPE 2019). Among this population, 60 percent need care for two years or more (ASPE 2019), which could consist of unpaid care provided by family members or friends, paid care provided at home, or residential care in nursing homes or assisted living facilities. Despite the large financial risk stemming from intensive and enduring care needs, insurance coverage is incomplete. Medicare covers short-term care needs after acute health events, such as hip fracture and stroke, but does not cover longer-term care needs due to chronic illnesses. Medicaid covers long-term care needs, but as a means-tested program, has stringent asset and income requirements. And private long-term care insurance (LTCI) coverage is low – only 7.5 million Americans have some form of long-term care insurance as of Jan 1, 2020—and the market is shrinking (AALTCI 2021).

As a result, many Americans face the choice of paying for care out of pocket or having their care needs met by unpaid caregivers, often spouses, children, or both. In all, $58.1 billion was spent out-of-pocket on home health care and nursing care facilities in 2019 in the US, representing one fifth of total LTC spending (CMS 2021). While the value of total unpaid care provided in the economy is more difficult to value, informal care is an important component of total LTC care provided. The costs of informal care may include reduced labor force participation and changes in location decisions among potential or current caregivers.

In this paper, we examine the role of private long-term care insurance, which reduces the price of formal care at the time of care, on family spillovers including informal caregiving, labor market outcomes, and geographic proximity to aging parents. On one hand, if informal care and formal care are complementary, and the presence of private insurance increases the use of formal care, demand for unpaid caregivers could increase. On the other hand, the reduced price of formal care services may lead people needing care to substitute away from informal care towards formal care. In this case, families – including potential caregivers – may reoptimize work and location decisions in response to lower caregiving expectations, and these effects may appear prior to a parent's care needs materializing.

An empirical challenge that arises when analyzing the impact of private long-term care insurance on family behavior is the potential endogeneity of LTCI. Endogeneity can arise due to the inability of the econometrician to observe, for example, health problems that could make both LTCI coverage and informal care use more likely, or risk aversion that may be associated with higher rates of LTCI coverage and better health (Finkelstein and McGarry 2006). To address the potential endogeneity problem and measure causal effects, we use an instrumental variables technique that exploits exogenous variation in LTCI ownership resulting from the plausibly exogenous implementation of state tax subsidies for LTCI.

Using this empirical strategy, we examine the causal impact of LTCI on a range of outcomes, including informal care use, parents' perceived availability of informal caregivers,

and the location and labor market decisions of their adult children. We find no evidence that LTCI reduces informal care use over an eight-year follow up period. However, we find evidence that LTCI causes respondents to be significantly less likely to report the availability of informal caregivers in the future, suggesting that parents change their expectations regarding informal caregivers.

Consistent with these changing expectations, our results indicate that the likelihood that an adult child coresides with parents is 82 percent lower when parents gain LTCI. We also find that LTCI causes a 34 percent increase in parents reporting all of their children to be working at all (full- or part-time) and a 42 percent increase in all children working full-time. Together, these findings suggest that LTCI ownership can confer economic gains to the next generation regardless of whether a LTC need arises.

Our findings build on several strands of related literature. First, one area of research investigates family interactions and intergenerational behavior around long-term care provision (Hiedemann and Stern 1999, Checkovich and Stern 2002, Engers and Stern 2002, Byrne, Goeree et al. 2009, Heger 2017, Hiedemann, Sovinsky et al. 2018). For example, one theory known as “intra-family moral hazard” is that parents do not buy LTCI because they prefer children to care for them and do not want to enter a nursing home (Pauly 1990). This theory suggests that if the cost of a parent accessing a nursing home were lower because of the purchase of insurance, children would change their behavior and be less likely to provide care. This model predicts a decline in informal care in response to holding LTCI.

Another related literature studies the relationship between informal and formal care use (Engers and Stern 2002, Van Houtven and Norton 2004, Charles and Sevak 2005, Van Houtven and Norton 2008). These studies generally find that receipt of informal care substantially reduces the risk of and/or delays nursing home entry, suggesting that informal care is a substitute for formal care services to a point. Informal care and formal care could also be complements (Bonsang 2009), however, as a drop in the formal care price could increase both formal and informal care, with a change in each leading to a different mix of use between the two resources (Coe, Konetzka et al. 2021). Some studies also show benefits to care recipients, including lower health care utilization and better health outcomes (Eloniemi-Sulkava, Notkola et al. 2001, Van Houtven and Norton 2008, Goncalves and Weaver 2017, Costa-Font, Jimenez-Martin et al. 2018, Coe, Guo et al. 2019). However, there is evidence that providing care also comes at a cost to caregivers: caregivers experience worse health, reduced earnings and fewer labor market opportunities (Pruchno and Resch 1989, Schulz, O’Brien et al. 1995, Coe and Van Houtven 2009, Van Houtven, Coe et al. 2013, van den Berg, Fiebig et al. 2014, Bauer and Sousa-Poza 2015, Schmitz and Westphal 2015, Brenna and Di Novi 2016, de Zwart, Bakx et al. 2017, Heger 2017, Bom, Bakx et al. 2019; Kolodziej, Coe et al 2022). Recent adjacent literature shows that paid family leave can ameliorate the negative long-run effects of spousal disability on household labor supply (Anand, Dague et al. 2021).

Several studies seek to understand the determinants of LTCI coverage. Although factors such as Medicaid crowd-out, high administrative costs in a small market, market stability concerns, and incomplete coverage undoubtedly play a role in explaining low private

coverage no single factor has been found to dominate, and both supply-side and demand-side explanations contribute (Brown and Finkelstein 2011, Stallard 2011, Brown, Goda et al. 2012).

Family relationships may also influence LTCI coverage decisions. Qualitative work finds that parents expressed buying LTCI because they did not want to burden their children with having to provide informal care, and LTCI would leave the role of children to provide attention but not caregiving (Broyles, Sperber et al. 2016, Sperber, Voils et al. 2017). These motives are consistent with the findings in Brown, Goda et al. (2012), who show that 87 percent of respondents wished to not create a financial burden for family members if they needed LTC. However, it is also the case that the presence of potential informal caregivers is associated with lower demand for LTCI. For instance, several studies have documented LTCI coverage decreasing in the number of children (e.g. Mellor 2001).

Finally, there are a few studies that have studied the relationship between LTCI and formal care services. Konetzka, He, Dong and Nyman (2019) test for the presence of traditional moral hazard in the LTCI market by estimating the effect of LTCI on formal care use. They find evidence of moral hazard in formal home care, but not nursing home care. Prior work by Li and Jensen (2011) find modest effects of LTCI on the likelihood of using long-term care services, and no evidence that formal care substitutes for informal care in the presence of LTCI. A study in Japan found that an exogenous policy change introducing LTCI to older adults increased labor force participation of informal caregivers across all age groups, and especially among younger caregivers age 30–39 (8.7 percentage points) (Fu, Noguchi et al. 2017).

Our paper makes two key contributions to this literature. First, to our knowledge, our study is the first to exploit a source of exogenous variation in LTCI to investigate a variety of different family spillovers of LTCI in the U.S. context, including the use of informal care, work and co-residence decisions of adult children, and perceptions regarding the availability of informal care. Second, we provide evidence that insuring long-term care expenditures, or other similar reductions in the cost of formal care services, can impact family members of the insured, and that these effects may occur many years prior to when care is needed. These results highlight the need to account for potential spillovers of policy interventions to family members when assessing the costs and benefits of such interventions.

The rest of our paper proceeds as follows. Section II presents the data, sample selection criteria, and descriptive statistics. Section III describes the potential sources of endogeneity and our instrumental variables strategy. Section IV presents the main results and Section V reports the results of several sensitivity tests. Section VI provides a discussion of the findings and concludes.

II. DATA

We use data from twelve waves of the Health and Retirement Study (HRS) from 1996–2018, a publicly available, bi-annual survey of the near elderly in the U.S.[1] The HRS is the only nationally representative data set with extensive information on family characteristics, long-

term care insurance, expectations about future sources of informal care and informal care use. Respondents were ages 50 and older when they entered the sample initially, thus, they are old enough during the sample period to have formed expectations and decisions about LTC planning and LTCI purchase. Many respondents are observed long enough to have used informal care, formal LTC, or both. We use the *Cross-Wave Geographic Information (State)*[2] file to match respondents to their state of residence. Finally, we have collected individual state income tax return forms from 1996–2018 which contain details of tax incentive programs for private LTCI. The tax incentive variable used in this paper is whether a tax subsidy is available in a given state in a given year (Goda 2011) and we also calculate the average value of the tax subsidy per dollar of insurance. The state LTCI tax subsidy status in each year is merged to individuals based on their state of residence in each year.[3] We use waves 1996–2014 for most models, examining LTCI holding in time t and informal care outcomes in time $t + 1$; however, to examine longer-run effects on informal care, we use waves 2016 and 2018 in order to examine informal care outcomes up to 4 waves (or 8 years) after LTCI coverage is observed.

Sample.

Our identification strategy relies on state tax subsidies for LTCI affecting private LTCI coverage directly. As such, we explored several subsamples among HRS respondents to identify populations for whom the tax subsidies were most impactful, separating the sample by tax filing status and income level. While the state tax subsidy could, in theory, influence the LTCI purchase decisions of all tax filers, prior work has found that those with higher levels of income are most responsive to the tax subsidy (Goda 2011).[4] This is likely because the tax subsidy is generally larger for individuals in higher tax brackets and Medicaid could crowd-out LTCI demand for individuals in the lower portion of the income distribution (Brown and Finkelstein 2008). We examine how the presence of a tax subsidy affects LTCI coverage for different subsamples in Section V (e.g. all HRS respondents; all tax filers; all tax filers with high income; all tax filers with low and middle income). We define high income as having inflation-adjusted income in the top tertile in each age, gender, marital status, and state cell, and low- and middle-income filers are in the bottom two tertiles.

Because the family behavior responses we examine are specific to adult children (expected availability of children to provide informal care if needed, informal care provision from children, and child measures of co-residence, proximity, and work behavior) and in order to maintain a more consistent sample throughout our analysis, we further restrict the sample to those respondents who report having children and those who answer questions regarding the expected availability of informal caregivers.

[1]We do not use waves earlier than 1996 because they have lower quality information on the LTCI question (Finkelstein and McGarry 2006), which is key to our analysis.
[2]We obtained these data by securing authorized access to the HRS Restricted Data.
[3]Tax incentives at the state level require filing taxes in that state and generally do not require itemizing one's deductions.
[4]We define above median income as having real adjusted household income, where we divide by two if the respondent is married or partnered, above the 50th percentile within one's age and gender bin.

Outcome measures.

The outcome measures include the respondent's informal care use (overall and by source), their perception of the availability of informal caregivers if they are needed in the future, and location and labor market choices among adult children.

Informal Care Use.—To define informal care receipt in the HRS we use several questions, first examining whether the respondent gets help with specific ADLs (walking, dressing, bathing, eating, getting in/out of bed and using the toilet) and IADLs (preparing hot meals, shopping for groceries, making telephone calls, taking medications, managing money, driving) and then assessing if it was unpaid assistance. We obtain data on informal care use from children from the RAND Family Data Files, which also contains information on the relationship of the care provider to the respondent and the care provider's gender. We record the provision of any informal care as well as informal care by relationship: spouse, any child, any son, any daughter, and other. We attribute informal care from a daughter-in-law or son-in-law to their spouse when determining the gender composition, and step-children are included.

Informal care outcomes are reported in the HRS over a reference period that covers time since the last interview (or the last two years if the person is interviewed for the first time). Because we aim to measure use after LTCI coverage, we examine informal care receipt reported between the current wave (wave t) and one wave afterward (wave $t + 1$), representing approximately two years after we observe LTCI ownership. In addition, we examine longer run effects of LTCI coverage on informal care by examining the effects of LTCI between wave t and waves $t + 2$, $t + 3$, and $t + 4$, or up to eight years after we observe LTCI ownership.

Expected Availability of Informal Caregivers in the Future.—Respondents are asked: "Suppose in the future, you needed help with basic personal care activities like eating or dressing. Do you have relatives or friends [besides your spouse] who would be willing and able to help you over a long period of time?" If respondents answer yes, they are then asked the relationship of the potential care providers. We examine the presence of any potential caregiver and whether the potential caregiver is a child, relative, or other.

Adult Child Location and Labor Market Decisions.—We construct dependent variables indicating whether at least one adult child lives with the parent(s), whether at least one adult child does not coreside but lives within 10 miles of the parent(s), and whether all children live greater than 10 miles away. We also construct dependent variables indicating whether all adult children work part- or full-time and whether all adult children work full-time. We follow the RAND definitions, where working full-time is 35+ hours per week, 36+ weeks per year.

Explanatory variables.

LTCI.—Respondents in 1996 and later waves answer the following question: "Not including government programs, do you now have any long-term care insurance which specifically covers nursing home care for a year or more or any part of personal or medical care in your

home?" Individuals respond yes or no. LTCI ownership is defined as self-reporting LTCI in time *t*. We focus on holding insurance as the relevant measure rather than the purchase decision for three reasons. First, the family spillovers we examine generally would not be expected to occur only at the time of the purchase. Second, the exact date of purchase is not included in the HRS and therefore must be inferred from an absence of LTCI in one wave and the presence in the next wave. Finally, while the initial decision to purchase LTCI is likely to be more involved, the decision to renew the policy each year may still be influenced by tax incentives.

The models also include binary and categorical controls for marital status (married, divorced, widowed, unmarried), female, number of children (zero, one, two, three, or four or more children), retired status, education (less than high school, high school, some college, college or more), household income (less than 10,000, 10,000–30,000; 30,000+), wealth (indicator for being in the top one-third of the wealth distribution adjusted by age and gender), race (White/Caucasian, Black/African American, Other), health status (fair or poor self-reported health), the presence of one or more limitations in activities of daily living (ADLs), and single year of age fixed effects.

Summary Statistics

Table 1 displays the summary statistics for the control variables used in our analysis, at the person-wave level of observation. The first column shows characteristics of all individuals age 50 and above in HRS Waves 3–12; the second column restricts the sample to tax filers; the third column restricts to tax filers with high income (our analysis sample) and the fourth and final column restricts to those tax filers who have low and middle income.

High income tax filers have much higher rates of private LTCI ownership than the full sample, at 17 percent versus 12 percent. Sixty-eight percent of high income tax filers are married, 57 percent are female, 91 percent are white, and 46 percent are retired. Two percent had at least two limitations in activities of daily living. Almost one-third of the respondents were sampled in a year and state in which there was a tax subsidy for long-term care insurance. High income tax filers were less likely to have three or more children than those in the full sample (52 percent versus 57 percent). As would be expected, a higher proportion of the high-income tax filer sample had household income in the highest category, $30,000 or more (95 percent versus 52 percent of the full sample). In general, compared to the full sample of HRS respondents age 50 and above, high income tax filers were healthier and had completed more education.

We summarize the outcome variables in Table 2, and for comparability show the same categories: all HRS, tax filers, high-income tax filers, and low- and middle-income tax filers. In general, the high-income tax filers are less likely to use informal care than the full sample. More than sixty percent of respondents reported having children, relatives or friends (other than a spouse) willing to provide informal care in the future across all four subsamples. Most expected future informal care to come from a child rather than another source. Among high-income tax-filers, 6 percent receive informal care within two years, and 3 percent receive this care from an adult child. Looking out 2, 3, and 4 waves, the rates of any informal care receipt are higher, with 12, 19, and 28 percent of high-income tax filers

reporting care over this horizon, respectively. Nineteen percent of our sample had at least one child age 19 or over living with them, 48 percent had at least one child living in another household but within 10 miles, and 40 percent reported that all of their children lived more than 10 miles away. About two-thirds reported all of their children worked part time or full time whereas just under half reported that all of their children worked full time.

III. EMPIRICAL METHODS

A. Probit Analysis

The literature has focused on regressions of insurance on outcomes to test the "positive correlation prediction," or the prediction that insurance is positively associated with outcomes such as care utilization. While not a causal estimate, the presence of adverse selection would lead to a positive association between insurance coverage and utilization, even after controlling for available health measures, due to private information on expected future long-term care utilization unobserved by the insurance company and the econometrician (Finkelstein, McGarry et al. 2005; Finkelstein and McGarry 2006). This test is equivalent to testing whether $\alpha_1 > 0$ in Equation (1) below:

$$Y_{it} = \alpha_0 + \alpha_1 LTCI_{it} + \alpha_2 X_{it} + \varepsilon_{it}, \quad (1)$$

where Y_{it} is an outcome measure for individual i in time t, $LTCI_{it}$ is an indicator for whether individual i has private LTCI coverage in time t, and X_{it} is a vector of additional controls for individual-level characteristics. However, either positive or negative selection on health status in LTCI coverage decisions could bias the result from Equation (1) above. Indeed, there is evidence of both types of selection in the LTCI market (Finkelstein, McGarry et al. 2005, Finkelstein and McGarry 2006). We therefore pursue an instrumental variable strategy to address these potential sources of bias.

B. Instrumental Variables:

Addressing bias from unobserved differences.—To address the potential endogeneity problem and measure causal effects, we use instrumental variables estimation techniques which can reduce bias from selection on non-random factors, such as higher likelihood of using long-term care in the future due to unobserved health problems or the inability to control well for risk aversion. Empirically, these unobserved factors could bias the results in either direction. If unobservable poor health is associated with higher rates of LTCI coverage and also with higher rates of informal care use, ignoring endogeneity would lead to a positive bias. However, if unobservable risk aversion is associated with higher rates of LTCI coverage and lower rates of informal care use, we would expect the raw correlation to be biased downwards (Finkelstein and McGarry 2006).

The literature has posed a variety of potential instruments that influence LTCI decisions. The key considerations in the identification of a causal effect are whether the potential instruments strongly predict LTCI coverage and are otherwise uncorrelated with the outcome measures. Researchers have previously used life insurance purchase and the price one faces for a LTCI policy (Li and Jensen 2011), federal tax subsidy for LTCI premiums implemented as part of HIPAA in 1996 (Courtemanche and He 2009), and federal tax

subsidies interacted with tax itemization status (Courtemanche and He 2009, Li and Jensen 2011, Konetzka, He et al. 2019). While these variables predict LTCI purchase, we deem them inappropriate in this setting for three main reasons. These variables are either likely to also influence informal care use independently (life insurance purchase or LTCI prices), be correlated with wealth that in turn impacts long-term care use (tax itemization), or have insufficient individual-level variation since they were implemented at the same time for the whole population (federal tax subsidies).

In this paper, we use variation in the adoption of tax subsidies for LTCI across states as an instrument for LTCI coverage. The state tax subsidies lower the price of insurance, leading to movement along the demand curve for insurance. State tax policies towards LTCI varied greatly during our study period: while only five states had tax incentives for LTCI in 1996, by 2014, 21 states and the District of Columbia had adopted one. Goda (2011) finds that the presence of state tax subsidies, on average, lead to a 28 percent increase in LTCI coverage rates. This increase is measured after controlling for fixed differences in states over time as well as year fixed effects and is therefore identified by variation in the year of adoption across states. Our first stage results (described in detail in Section IV) confirm these earlier findings with a longer time horizon.

In order for the presence of state tax subsidies for LTCI purchase to be a valid instrument, it must also be independent of the outcomes we study (expected availability of informal caregivers in the future, informal care use, work and living arrangements of adult children) except through the effect on LTCI after controlling for a rich set of covariates, including state fixed effects. While impossible to prove empirically, this assumption is plausible: the exact timing of state tax policy changes is likely independent of sharp changes in an individual's preferences for informal care or adult children's decisions regarding work and living arrangements.

The instrumental variables approach would be invalid if there were considerable peer effects due to violation of the exclusion restriction. Given the large percentage of individuals who remain uninsured for LTC following the implementation of subsidies, we believe any peer effects are quantitatively small. In sensitivity analyses (Section V), we explicitly examine the behavior of persons who never hold LTCI.

C. Estimation Strategy

Due to the binary nature of our outcomes and the fact that our outcomes are often in the tails of the distribution, we use nonlinear models and a control function approach as our primary specification (Heckman 1979, Heckman and Robb 1986, Wooldridge 2015), also known as two stage residual inclusion (2SRI) (Terza, Basu et al. 2008). The 2SRI approach is equivalent to two-stage least squares (2SLS) when both the first and second stages are estimated with a linear model. As in 2SLS, the exogenous variation induced by the excluded instrumental variable provides separate variation in the residuals obtained from a reduced form, and these residuals serve as the control functions. By adding these control functions, the endogenous explanatory variables become appropriately exogenous in a second-stage estimating equation (Wooldridge 2015). In the sensitivity tests we examine robustness of the 2SRI results to other specifications.

Our first stage equation is the following probit equation:

$$LTCI_{ist} = \Phi(\beta_0 + \beta_1 Z_{st} + \beta_2 X_{it} + s_s + \lambda_t) \quad (2)$$

Here, $LTCI_{ist}$ represents whether individual i in time t and state s is covered by LTCI. Z_{st} represents whether a tax subsidy is available in state s in time t. X_{it} represents the vector of individual time-varying controls, and S_s and λ_t represent state and year fixed effects, respectively. The year fixed effects account for general differences in coverage rates over time while the state fixed effects account for non-time-varying differences across states. The inclusion of state and year fixed effects implies that we are identifying the effect of LTCI coverage on our outcomes based on variation in LTCI coverage stemming from within-state differences in state tax subsidies over time. In all models, we use population weights and cluster standard errors at the state level (Bertrand, Duflo et al. 2004).

We use the estimated coefficients from the first stage to generate residuals, $\hat{u}_{ist}$. We calculate generalized residuals in the first stage to minimize bias (Garrido, Deb et al. 2012, Basu, Coe et al. 2018).

These residuals are then used in the second stage equation below:

$$LTC_{ist} = \Phi(\alpha_0 + \alpha_1 LTCI_{ist-1} + \alpha_2 \hat{u}_{ist} + \alpha_3 X_{it} + S_s + \lambda_t) \quad (3)$$

where the variables are the same as described above. In addition, we also perform a bootstrap procedure to calculate the standard errors for the second stage with 1,000 iterations.

We estimate probit models, which treat LTCI as exogenous, as well as 2SRI models, which account for the endogeneity of LTCI. For both estimates, we report marginal effects using the recycled prediction approach, which involves computing the marginal effects for each observation and then calculating the sample average of these marginal effects. For small samples, this method is preferred over calculating the marginal effect at the sample mean (Greene 2012).

IV. RESULTS

A. First Stage

In Table 3, we present the first-stage probit results, estimating the effect of state-level tax subsidies for LTCI on LTCI coverage for each of the four samples described above. The table shows the probit coefficient in the top row and report marginal effects below along with the mean of the dependent variable. The mean of the dependent variable is reported for observations in state-year combinations where there is no tax subsidy, representing the base level of LTCI coverage in the "control" group. In Columns (1) and (2), the instrument fails the typical strength tests as illustrated by the low F-statistic in these larger samples (p-value=0.054; p-value=0.020, respectively). Column (4) of Table 3 highlights that for low- and middle-income tax filers, there is no evidence that tax subsidies increase LTCI coverage (p-value=0.600). Only in the sample of high-income tax filers in the top income tertile (Column (3)) is there both a statistically significant increase in LTCI coverage (5

percentage points, or 29 percent), and sufficient strength in the instrument (F-statistic is 15.4, p-value<0.0001).[5]

Due to these results, we conduct our primary analysis on the subsample of high-income tax filers. This restriction means that generalizations from our findings can be made to high-income tax filers who are age 50 and above with children in the U.S., but not to the general population. As Table 1 indicates, our sample is healthier and more educated than a nationally representative sample of those 50 and above in the United States, represented in Column (1). This loss in generalizability is necessary to enhance internal validity and ensures that we are estimating the causal effect of LTCI on the outcomes of interest.

B. Second Stage Regression Results

The main results appear in Tables 4–7, which all follow the same structure. Each column represents a different dependent variable. The first two rows present the marginal effects and 95% confidence intervals from probit models, with no corrections for potential endogeneity of LTCI. The next two rows present the marginal effects and 95% confidence intervals for the 2SRI models. We also report the p-values testing the null hypothesis that the reported marginal effect is equal to zero. Because the sample varies slightly due to individual non-response or sample selection, we also include the first stage marginal effect of the tax subsidy indicator and F-statistic to assess the strength of the first stage for each regression. The subsequent rows include the mean of the dependent variable for individuals without LTCI living in states without tax subsidies. We also show the number of clusters (i.e., states), the number of observations and families, and average number of waves we observe each respondent in each model. The probit results can be interpreted as the association between LTCI and outcomes of interest, whereas the 2SRI estimates are causal effects under the assumption that the adoption of state tax subsidies influence the purchase of LTCI but do not affect the outcomes directly. We discuss the relationship between the probit and 2SRI results in more detail below (Section V).

Informal Care Use—We first examine the short-term effect of LTCI on informal care use by source in Table 4. Our probit estimates show a negative association between LTCI coverage and informal care use overall, as well as specifically from spouses and children of both genders. However, we find no evidence that LTCI reduced the use of informal care over a two-year period after we observe LTCI coverage once we address the endogeneity of LTCI. It is possible that our relatively healthy sample has not yet experienced deteriorating health that would be accompanied by LTC needs in such a short time period. Therefore, we also examine whether LTCI coverage affects informal care use up to four waves (or eight years) into the future and present our results in Table 5. However, we continue to find negative point estimates that are statistically insignificant over the eight-year horizon we examine.[6]

[5]For context, the increase we estimate is in line with the effects of federal tax itemization in Courtemanche and He (2009) and Li and Jensen (2011) which were found to increase LTCI coverage by 29–33 percent, and state tax subsidies using an earlier time period in Goda (2011) which were found to increase LTCI coverage by 28 percent.

[6]Looking over horizons longer than eight years results in losses of sample size and first stage precision due to the need to eliminate some of the later years of our main analysis sample.

Effects on Perceived Availability of Informal Caregivers—We next investigate whether LTCI impacts the respondent's perceived availability of informal caregivers in the future and report our results in Table 6. We find that LTCI results in a statistically significant reduction in the expected availability of having any informal caregivers in Column (1). This effect is present in the probit estimates that do not correct for endogeneity of LTCI coverage but more pronounced in the 2SRI estimates. Specifically, the probit estimates suggest that respondents with LTCI are 3.9 percentage points less likely to report having any relatives or friends willing and able to help over a long period. The 2SRI results that correct for selection into LTCI coverage indicate that LTCI leads to a 40 percentage point reduction (or 63 percent reduction relative to the mean) in having any non-spousal caregivers willing to help. The difference between these results suggest that neglecting to correct for endogeneity leads to a positive bias in the probit estimates. This relationship may emerge if there is adverse selection in LTCI purchase, namely that those with unobservable poor health may be more likely to purchase LTCI and be more likely to use informal care from friends and relatives.

When we examine the effects of LTCI on the perceived availability of different types of informal caregivers (Columns 2–4), we find that LTCI reduces the expected availability of informal caregivers from all sources. Relative to the mean, the drop in perceived availability from children declines by 55 percent.

Spillovers on Adult Children's Location and Labor Market Choices—If the perceptions of respondents are aligned with the expectations of their potential caregivers, parents gaining LTCI may "free" adult children from their potential future informal care responsibilities and therefore lead them to pursue other activities. These effects could occur before the onset of a disability and regardless of whether a LTC need eventually develops; therefore, focusing *only* on actual care use may miss much of the effect of LTCI on the family.

Therefore, we next examine whether LTCI coverage influences the work and living arrangements of adult children in Table 7. Our probit estimates, which do not correct for endogeneity, suggest that there is a significant negative association between LTCI coverage and having at least one co-resident adult child. Our 2SRI results, which address the endogeneity of LTCI, show statistically significant effects that are negative and larger in magnitude for this outcome: LTCI coverage leads to a 16.9 percentage point reduction in the likelihood of living with an adult child. This reduction represents an 82 percent reduction relative to the control mean. While the marginal effects in Columns 2 and 3 are positive, they are statistically insignificant, so we are not able to ascertain whether co-residence is being substituted by children moving close by or farther away. However, together, these results suggest that LTCI results in children living farther away than they otherwise would have, and are consistent with LTCI reducing the need for adult children to provide informal care in the future.

Finally, we examine the effects of LTCI on two measures of labor market choices by adult children: whether all children are working either part- or full-time (Column (4)), and whether all children are working full-time (Column (5)). We find evidence that LTCI

coverage results in a 22.5 percentage point (or 34 percent) increase in the likelihood that all adult children are working in some capacity, and a 20.2 percentage point (or 42 percent) increase in the likelihood of all children working full-time. These results indicate that the adult children of those covered by LTCI display a greater attachment to the labor force than they otherwise would have if their parents did not hold LTCI. Furthermore, additional checks show that the significant effects of LTCI holding on co-residence and labor force persist up to four waves (or eight years) into the future. Our results therefore suggest that by reducing the possibility of informal care provision among children, LTCI confers significant economic spillovers to the next generation in the form of fewer constraints on location and labor market decisions.

V. SENSITIVITY TESTS

We perform a variety of sensitivity tests to examine the robustness of our results to alternative specifications. First, we calculate the value of the tax subsidy per dollar of insurance in each state and year, following (Goda 2011). To do so, we use age- and gender-specific 2018 LTCI premiums, and adjust by U.S. Medical Care CPI to calculate the 1996–2016 premium amounts. We determined a credit value or deduction value based on state income tax rates calculated using NBER TAXSIM, and average individual income by age and gender.[7] Finally, we calculated the tax subsidy value per dollar, representing the average reduction in premiums due to the favorable tax treatment. The average reduction was 5 cents per dollar of insurance. When we substitute this continuous variable for our binary indicator for whether a tax subsidy is present for a particular state and year, our results are both qualitatively and quantitatively similar (see Appendix A).

Second, we conduct a falsification test where we estimate the effect of state tax subsidies on our outcomes among the sample of individuals who never hold LTCI. We would not expect these individuals' informal care use and family spillovers to respond to tax subsidies since they do not hold LTCI. We show reduced form probit results in Appendix B, where the key independent variable is the presence of a tax subsidy. Overall, we find very little evidence of any association between tax subsidies and any of our outcomes. This test further bolsters the argument that our instrumental variable strategy is valid, since never-purchasers are not changing their behavior in response to announcements of tax incentives.

Our main analysis may include individuals who will self-insure no matter what LTCI subsidies are in place and hence the instruments do not induce these persons to purchase or hold LTCI. Brown and Finkelstein (2008) suggest that persons with wealth over $3 million are the most likely to self-insure; we therefore test the robustness of our results by removing this subsample and report our results in Appendix C. Our results are qualitatively and quantitatively similar to our main results. This is not surprising given that this restriction removes less than 400 observations, or approximately 1 percent of the sample.

[7]LTCI premiums from 2018 were retrieved from the American Association for Long-Term Care Insurance (www.aaltci.org), and documentation for NBER TAXSIM is available at http://users.nber.org/~taxsim/. We used the RAND HRS Tax Calculations 2014 file to determine the marginal tax rate for individuals in our sample.

Finally, we examine the robustness of our results to the 2SRI model choice in Appendices D-F. Specifically, we show results using a bivariate probit specification, a control function approach with a probit first stage and linear second stage, and 2SLS where both the first and second stage equations are linear. These results provide evidence that accounting for nonlinearities in the first stage outcomes improves efficiency and precision, and reduces bias by mitigating non-random error introduced in the residual term in the second stage equation. This increase in precision of the estimates is consistent with the earlier literature evaluating the importance of IV model selection in mitigating bias, especially among models with rare outcomes like this setting (Basu, Coe et al. 2018). Importantly, our results are robust to specifications where the first stage equation that predicts LTCI coverage is appropriately specified using a nonlinear model.

VI. DISCUSSION AND CONCLUSION

Caring for aging relatives and family members with disabilities, while often rewarding, can be both emotionally and financially straining. Although a large share of care needs are met by unpaid family members and friends, this pattern of care may not be sustainable due to changing demographics. Smaller families, increased geographic dispersion within families, dual-earner couples, increased divorce rates and delayed fertility all lead to increasing constraints on the traditional supply of informal care providers, namely spouses and adult children (Redfoot, Feinberg et al. 2013, Van Houtven, Coe et al. 2015).

These demographic changes underscore the urgent need to find policy solutions to meet LTC needs, as these needs are likely to increase in the next few decades (Kaye 2012). Many past and proposed policy solutions involve reducing the price of formal care services at the point of care. For instance, the Deficit Reduction Act, signed into law on February 8, 2006, authorized states to offer “Partnership” policies in a (largely unsuccessful) attempt to stimulate demand for private long-term care insurance (Bergquist, Costa-Font et al. 2018). Recent proposals at the state and federal levels call for expanding access to home health care for long-term care needs or creating new public-private partnerships to provide long-term care insurance for older adults. Assessing these policy proposals requires an understanding of both how these policies affect both the recipients of care, and whether gaining access to formal care services creates spillovers to potential unpaid caregivers, even before a caregiving spell begins.

We add to the literature by estimating the causal effects of LTCI on informal care use, perceived informal care availability in the future, and work and location decisions of adult children. We find no evidence that LTCI reduces informal care use over an eight year horizon after LTCI coverage is observed. Unfortunately, our empirical strategy lacks precision in looking at outcomes over a horizon longer than eight years. However, these findings are consistent with the idea that people purchase LTCI when relatively healthy, and longer time horizons are needed in order for a LTC need to arise.

Despite the fact that we do not observe informal care use declining as a result of LTCI, we do find that LTCI reduces perceptions of informal care availability among HRS respondents. In addition, we find that having LTCI leads to changes in the behavior of adult children

that is consistent with a smaller role in planning to care for parents now and in the future (less co-residence and stronger labor force attachment). These results highlight the ability of LTCI coverage to have spillover effects on the next generation even if a LTC need does not arise.

While we conduct multiple sensitivity tests that increase our confidence in our main findings, an important limitation to our study is weak identification in some sub-samples, including households with middle or low income, and that our identification comes from focusing on parents who are induced to hold LTCI due to a slight reduction in price through the provision of tax subsidies. Our sample restriction and empirical strategy allows us to use an instrumental variables approach, which is critical to understanding the causal effect of LTCI on informal care and family behavior spillovers, but they limit the generalizability of our results. In particular, because our sample is limited to individuals who file taxes, have income in the top tertile, and have children, they may not be applicable to the whole age 50 and over population in the United States. In addition, alternative policy actions may target different populations for whom these results may be less relevant. However, we argue that our results are still important for policy, as the implicit tax on LTCI premiums limits the ability of most policy proposals to induce purchase among lower-income populations, and tax subsidies resulted in a significant increase in the number of people covered by LTCI over the past several years.[8]

Our results that LTCI negatively affects parents' expectations of the availability of informal care could be driven in part by marketing campaigns or popular press discussions that led people to expect less care from their children rather than the direct purchase of LTCI. While difficult to rule out this possibility completely, these factors would affect the interpretation of our results only to the extent that these actions occurred differentially when tax subsidies were adopted.

We surpass the rule-of-thumb rule for instrument strength set forth by Stock, Wright et al. (2002), Stock and Yogo (2005); however recent research suggests that may be too low to avoid all problems associated with weak instruments (Andrews, Stock et al. 2019, Lee, McCrary et al. 2021). Weak instruments would lead to a bias in our estimates towards the OLS estimate, and the standard errors would be too small. While our findings are robust using several alternative specifications with different underlying assumptions, such as the bivariate probit model, we could still have bias in these estimates.

It is important to note that our analysis focuses on outcomes and we cannot disentangle the separate effects from changes in the supply of informal caregivers and the demand from parents for children to provide care. However, our results suggest that supply-side factors are at least somewhat at play given the fact that adult children are changing location and work decisions as a result of their parents gaining LTCI, and that parents perceive lower availability among their adult children to provide help if needed.

[8]It is worth noting that tax subsidy policies for LTCI differentially benefit both wealthier parents and the children of wealthier parents, which may result in disparities in family spillovers by parental wealth. Understanding the distributional effects of such policies is an important area of future work.

Overall, our findings suggest that policies that decrease LTC exposure risk, either through increasing LTCI coverage or reducing the cost of formal care, can significantly reduce the burden on adult children anticipating a parent's future LTC need. These spillovers onto adult children may not be internalized by parents in their decision of whether to purchase long-term care insurance, leading to inefficiently low levels of LTCI coverage. In addition, our results suggest that potential spillovers on adult children should be accounted for when analyzing policies that seek to expand access to informal care alternatives.

Supplementary Material

Refer to Web version on PubMed Central for supplementary material.

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Table 1.

Summary Statistics – Controls

	(1) All	(2) Filers	(3) Filers, High Income	(4) Filers, Low/Middle Income
LTCI Coverage	0.12	0.13	0.17	0.11
Age in years	65.85	64.8	65.33	64.41
Married	0.67	0.72	0.68	0.75
Divorced	0.12	0.11	0.11	0.11
Widowed	0.17	0.13	0.16	0.12
Unmarried	0.04	0.04	0.05	0.03
Female	0.57	0.55	0.57	0.54
One child	0.11	0.11	0.12	0.11
Two children	0.31	0.33	0.35	0.31
Three children	0.23	0.24	0.24	0.24
Four+ children	0.34	0.32	0.28	0.35
Retired	0.52	0.49	0.46	0.51
Less than High School	0.18	0.13	0.07	0.18
High School	0.36	0.37	0.29	0.43
Some College	0.23	0.25	0.26	0.24
College Plus	0.22	0.25	0.37	0.16
Income < 10000	0.09	0.05	0.00	0.08
Income 10000–30000	0.39	0.34	0.05	0.55
Income 30000 +	0.52	0.61	0.95	0.37
White/Caucasian	0.86	0.88	0.91	0.86
Black/African American	0.09	0.07	0.06	0.08
Other	0.05	0.04	0.03	0.05
Fair or Poor Health Status	0.22	0.18	0.13	0.23
2 + ADL	0.03	0.02	0.02	0.03
Wealth Top 33 Percentile	0.37	0.42	0.61	0.28
Home Health Use	0.05	0.04	0.04	0.04
Tax Subsidy	0.32	0.33	0.32	0.34
Observations	103,698	78,880	32,149	46,703
Families	25,491	21,420	12,183	17,252
Average #waves families are observed	5.49	5.14	3.88	3.92

Source: HRS Waves 3–12 (1996–2014). The unit of observation is person-wave, thus individuals can appear in the sample more than once.

Table 2.

Summary Statistics - Outcomes

	(1) All	(2) Filers	(3) Filers, High Income	(4) Filers, Low/Middle Income
Informal Care Use (one wave ahead):				
Any	0.10	0.08	0.06	0.09
Spouse	0.05	0.05	0.04	0.06
Child	0.05	0.04	0.03	0.04
Son	0.03	0.02	0.02	0.02
Daughter	0.04	0.03	0.02	0.03
Other	0.01	0.01	0.00	0.01
Long-term Informal Care Use				
Two waves ahead	0.19	0.15	0.12	0.17
Three waves ahead	0.27	0.22	0.19	0.25
Four waves ahead	0.37	0.32	0.28	0.35
Availability of Future Caregivers:				
Any	0.65	0.65	0.63	0.66
Child	0.48	0.48	0.47	0.49
Relative	0.19	0.19	0.18	0.19
Other	0.12	0.13	0.14	0.12
Adult Children Location and Labor Supply:				
At least one co-resident adult child	0.22	0.22	0.19	0.24
At least one child lives within 10 mi	0.53	0.52	0.48	0.55
All children live more than 10 mi. away	0.35	0.36	0.40	0.33
All children work PT/FT	0.62	0.64	0.67	0.62
All children work FT	0.46	0.48	0.50	0.47
Observations	103,698	78,880	32,149	46,703
Families	25,491	21,420	12,183	17,252
Average #waves families are observed	5.49	5.14	3.88	3.92

Source: HRS Waves 3–12 (1996–2014). The unit of observation is person-wave, thus individuals can appear in the sample more than once.

Table 3.

First Stage Regressions: Probit Estimates of Tax Subsidy on Long-Term Care Insurance Coverage

Sample	(1) All	(2) Filers	(3) Filers, High Income	(4) Filers, Low/Middle Income
Tax Subsidy	0.075 *	0.101 **	0.198 ***	0.028
	(0.039)	(0.043)	(0.05)	(0.054)
Marginal Effect	0.014	0.021	0.05	0.005
F-Statistic	3.705	5.383	15.438	0.275
P Value	0.054258	0.020334	0.000085	0.599733
Mean Control DV	0.12	0.134	0.174	0.106
Pseudo R2	0.078	0.068	0.056	0.066
Clusters	50	50	41	50
Observations	103651	78862	32144	46690

Notes: Dependent variable is binary variable for long-term care insurance coverage. Sample includes individuals age 50 and above in HRS Waves 3–12 (1996–2014). Column (2) restricts to those who file taxes. Column (3) further restricts to those with high income, and Column (4) includes those with low income. See text for more details. All specifications include binary controls for marital status, sex, number of children, retirement status, education, income, race, fair/poor health status, ADLs, age, year, and state fixed effects and are estimated using population weights. Standard errors clustered at the state level.

*Significantly different at the 10% level;

**at the 5% level;

***at the 1% level.

Table 4.

Effects of LTCI on Informal Care Use by Source

	(1) Any	(2) Spouse	(3) Child	(4) Son	(5) Daughter	(6) Other
Probit ME	−0.014	−0.008	−0.008	−0.005	−0.006	−0.001
Probit CI	[−0.021;−0.008]	[−0.013;−0.002]	[−0.012;−0.004]	[−0.009;−0.002]	[−0.010;−0.002]	[−0.004;0.002]
2SRI ME	−0.034	−0.025	−0.016	−0.003	−0.009	−0.004
2SRI CI	[−0.108;0.041]	[−0.080;0.029]	[−0.083;0.052]	[−0.098;0.092]	[−0.114;0.095]	[−0.232;0.224]
P Value	0.38	0.368	0.653	0.952	0.86	0.973
FS Marginal Effect	0.0496	0.051	0.0504	0.056	0.0344	0.0405
FS F-Statistic	15.361	16.108	16.887	15.388	7.885	10.627
Mean Control DV	0.0671	0.0392	0.03	0.0178	0.0255	0.00557
Clusters	41	39	39	35	38	34
Observations	32,121	31,958	31,991	25,885	25,613	28,680
Waves	6.16	6.15	6.16	6.19	6.08	5.89
Families	8,509	8,485	8,477	6,861	6,865	7,893

Notes: Dependent variable is as indicated in column heading. Outcomes represent whether informal care was received between the current wave and one wave ahead. Brackets indicate 95% confidence interval. Sample includes individuals age 50 and above who file taxes with high income in HRS waves 3–12 (1996–2014). Long-term care insurance instrumented with indicator for state tax subsidy in 2SRI results. All specifications include binary controls for marital status, sex, number of children, retirement status, education, income, race, fair/poor health status, ADLs, age, year, and state fixed effects and are estimated using population weights. Standard errors clustered at the state level.

Table 5.

Long-Term Effects of LTCI on Any Informal Care Use

	(1) 1 Wave Ahead	(2) 2 Waves Ahead	(3) 3 Waves Ahead	(4) 4 Waves Ahead
Probit ME	−0.014	−0.021	−0.019	−0.01
Probit CI	[−0.021;−0.008]	[−0.030; −0.012]	[−0.031; −0.008]	[−0.027; 0.006]
2SRI ME	−0.034	−0.015	−0.042	−0.037
2SRI CI	[−0.108;0.041]	[−0.126; 0.097]	[−0.165; 0.082]	[−0.184; 0.109]
P Value	0.38	0.799	0.514	0.621
FS Marginal Effect	0.0496	0.0518	0.0427	0.0451
FS F-Statistic	15.361	17.758	10.33	12.177
Mean Control DV	0.0671	0.127	0.191	0.253
Clusters	41	41	41	41
Observations	32,121	30,492	29,016	24,880
Waves	6.16	6.14	6.11	5.62
Families	8,509	8,123	7,762	7,109

Notes: Dependent variable is as indicated in column heading. Outcomes represent whether informal care from any source was received between the current wave and either one, two, three or four waves ahead. Brackets indicate 95% confidence interval. Sample includes individuals age 50 and above who file taxes with high income in HRS waves 3–12 (1996–2014). Long-term care insurance instrumented with indicator for state tax subsidy in 2SRI results. All specifications include binary controls for marital status, sex, number of children, retirement status, education, income, race, fair/poor health status, ADLs, age, year, and state fixed effects and are estimated using population weights. Standard errors clustered at the state level.

Table 6.

Effects of LTCI on Perceived Availability of Informal Caregivers by Source

	(1) Any	(2) Child	(3) Relative	(4) Other
Probit ME	−0.039	−0.035	−0.017	0.008
Probit CI	[−0.058; −0.020]	[−0.057; −0.014]	[−0.031; −0.003]	[−0.006;0.023]
2SRI ME	−0.402	−0.258	−0.18	−0.122
2SRI CI	[−0.550; −0.253]	[−0.411; −0.104]	[−0.260; −0.100]	[−0.203; −0.041]
P Value	5.071e-06	0.002	0.00008238	0.005
FS Marginal Effect	0.0496	0.0496	0.0496	0.0496
FS F-Statistic	15.438	15.438	15.438	15.434
Mean Control DV	0.638	0.47	0.182	0.134
Clusters	41	41	41	41
Observations	32,144	32,144	32,144	32,128
Waves	6.16	6.16	6.16	6.16
Families	8,513	8,513	8,513	8,508

Notes: Dependent variable is as indicated in column heading. Brackets indicate 95% confidence interval. Sample includes individuals age 50 and above who file taxes with high income in HRS waves 3–12 (1996–2014). Long-term care insurance instrumented with indicator for state tax subsidy in 2SRI results. All specifications include binary controls for marital status, sex, number of children, retirement status, education, income, race, fair/poor health status, ADLs, age, year, and state fixed effects and are estimated using population weights. Standard errors clustered at the state level.

Table 7.

Estimates of Effects of LTCI on Location and Labor Market Choices of Adult Children

	(1) At least one co-resident adult child	(2) At least one child lives within 10 mi.	(3) All children live >10 mi away	(4) All children work part time or full time	(5) All children work full time
Probit ME	−0.029	−0.019	0.03	0.02	0.025
Probit CI	[−0.048;−0.010]	[−0.048;0.009]	[0.005;0.054]	[0.001;0.040]	[−0.000;0.051]
2SRI ME	−0.169	0.041	0.066	0.225	0.202
2SRI CI	[−0.271;−0.068]	[−0.147;0.229]	[−0.133;0.265]	[0.089;0.361]	[0.024;0.381]
P Value	0.002	0.673	0.519	0.002	0.032
FS Marginal Effect	0.0438	0.0432	0.0435	0.0428	0.0428
FS F-Statistic	11.799	10.965	11.096	9.579	9.579
Mean Control DV	0.206	0.47	0.403	0.656	0.483
Clusters	41	41	41	41	41
Observations	31,748	31,058	31,026	29,432	29,432
Waves	6.17	6.09	6.09	6.02	6.02
Families	8,383	8,310	8,303	8,043	8,043

Notes: Dependent variable is as indicated in column heading. Brackets indicate 95% confidence interval. Sample includes individuals age 50 and above who file taxes with high income in HRS waves 3–12 (1996–2014). Long-term care insurance instrumented with indicator for state tax subsidy in 2SRI results. All specifications include binary controls for marital status, sex, number of children, retirement status, education, income, race, fair/poor health status, ADLs, age, year, and state fixed effects and are estimated using population weights. Standard errors are clustered at the state level.