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To my parents, husband, and friends, who helped make this dissertation possible.

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Abstract

Population aging is a major global issue faced by almost all countries in the world. By 2013, 11.7 percent of people worldwide are aged 60 years or over, this number is expected to be 21.1 percent by 2050 (United Nations, 2013). As a result, the old-age support ratios in both developed and developing countries are expected to continue to fall in the next few decades, placing heavy burden on their long-term care (LTC) systems. In response to this challenge, countries have taken different approaches to strengthen their LTC systems. In this dissertation, we discuss the characteristics and problems of the LTC systems in the United States and China, the largest developed country and the largest developing country in the world, and then study the approaches they have taken to cope these problems.

Chapter 2 presents findings of a study evaluating the financial protection effects of private long-term care insurance (LTCI) in the United States. Although private LTCI is often discussed as a potential solution to the need for LTC financing, there exists remarkably little empirical evidence on the economic consequences of being insured. We use U.S. Health and Retirement Study data to examine how LTCI affects key financial outcomes of insured individuals, including asset accumulation. Using an instrumental variable (IV) approach to account for the endogeneity of LTCI purchase, we find that LTCI leads to consistently positive effects on assets, consistently negative effects on Medicaid and Food Stamps enrollment and parent-child financial transfers, and ambiguous effects on out-of-pocket (OOP) medical payments. These results suggest that although private LTCI is ineffective at protecting insured individuals against large medical expenditures, it improves the general financial well-being of insured individuals potentially by reducing Medicaid-related disincentives to asset accumulation, motivating them to save more and reduce asset transfers.

Chapter 3 examines the impact of formal home care use on key physical and mental health outcomes for spouses of care recipients in the United States. Over the past three decades, there has been a large expansion in noninstitutional LTC, and public financing of long-term care services has been shifting away from nursing home toward home and community-based services (HCBS). However, given that spouses of care recipients play an important role in making LTC decisions, there exists remarkably little empirical evidence on the effects of different LTC settings on spousal health outcomes. We use U.S. Health and Retirement Study data to examine how home health use affects key physical and mental health for spouses of care recipients. Using an IV approach to account for the endogeneity of home health use, we find that home health use leads to mostly insignificant, yet consistently negative effects on spousal physical health, which may be caused by increased informal care responsibilities. We also find improved spousal mental health outcomes, especially in depression symptoms, which may be caused by increased satisfaction derived from providing more intensive informal care, from enabling care recipients to stay in their preferred LTC setting, or from living together with their partners. Our results are important in estimating the potential cost and effectiveness of HCBS expansion.

Chapter 4 examines the impact of family size on informal care supply in China, as China recently abolished the one-child policy (OCP) and allowed all couples to have two children partly in response to its growing aging population and increasing demand for informal caregivers. Because only recently has the aging of parents subject to the OCP created significant need for long-term care among this generation, evidence on whether having more children increases the probability of receiving LTC among the elderly in modern China remains limited. We use data from the China Health and Retirement Longitudinal Study (CHARLS) to examine how family size affects LTC support for elderly parents. Using an IV approach to account for the potential endogeneity of fertility choice, we find that compared to parents with only one child, parents with two or more children are 17 percentage points more likely to receive care from their adult children. This effect is larger among rural residents but insignificant among urban residents. However, having more children does not increase the parents' overall probability of getting care— care from spouses largely compensates/substitutes for care from adult children. Our results suggest that the new two-child policy (TCP) may not necessarily lead to an increase in total informal care supply, but may instead transfer the caregiving burden from spouses to adult children. The end of the OCP will likely not reduce the need for further investment in developing the formal LTC system in China.

Chapter 1 The Long-Term Care Crisis: Challenges in Financing and Delivery

The population in both the United States and China is aging rapidly, raising concerns about their long-term care (LTC) financing and delivery systems. Various proactive public policies have been enacted to build a high-performing system of LTC services and supports.

Chapter 2 and 3 focus on the evaluation of LTC policies in the United States. LTC is expensive in the United States, yet there is only limited insurance coverage for LTC services, making LTC costs a substantial source of financial risk faced by elderly people and their families (Brown & Finkelstein, 2007). Therefore, a number of proposed solutions have been advanced over the past few decades to reduce individuals' financial burdens, curb Medicaid spending on LTC, and deliver high quality care at a lower price. Our study focuses on 2 potential solutions: (1) expanding the private long-term care insurance (LTCI), and (2) shifting public financing from institutional care toward home and community-based services (HCBS). The first solution rests on the assumption that LTCI is effective in protecting the insured against catastrophic medical expenditures, which is arguably the primary purpose of any health insurance. However, prior research in this area has examined mainly the influences of LTCI on health service utilization and informal caregiver outcomes, little is known about the economic consequences of having LTCI. Therefore, to evaluate private LTCI policies and inform policymakers of the potential costs and benefits of extending LTCI coverage, Chapter 2 examines the effects of LTCI on key financial outcomes for elderly people, and seeks potential explanatory mechanisms behind these effects. The second solution rests on the assumption that HCBS (versus institutional care) is a more efficient way of delivering LTC services. The basic rationale for this assumption is based mainly on prior findings that: (1) LTC users generally prefer HCBS to institutional care, and (2) for nursing home residents with less intensive care needs, HCBS are cheaper

(Grabowski, 2006). However, it is still unclear whether and how HCBS (versus nursing home care) may affect health outcomes for the spouses of care recipients—who are usually also old and fragile, yet play an important role in taking care of the care recipients and making LTC decisions. HCBS use may affect spousal health outcomes if it directly changes their physical or mental demands, or changes health-seeking behavior (Coe & Van Houtven, 2009). We could not estimate the overall costs and benefits of HCBS expansion without understanding how spousal well-being is affected. Therefore, Chapter 3 describes the characteristics of home care and nursing home users and their spouses, and examines the causal impact of different care settings on physical and mental health outcomes for spouses of care recipients.

Chapter 4 is motivated by the new two-child family planning policy in China, and examines how family size may affect informal care supply. Population aging is an urgent issue in China, as it is driven both by a sharp decline in fertility and by a steady increase in life expectancy (The World Bank, 2015). However, the social insurance, social assistance and formal LTC systems in China remain underdeveloped. Therefore, although the former family-oriented LTC organization is not as sustainable as it used to be due to demographic transition, it will continue to be the primary means of old-age support over the next few decades (Glass, Gao, & Luo, 2013). These new challenges create a great caregiving burden for adult children and potential risk for parents—parents whose single child is unable to care for them may face a lack of support in old age. Partly in response to this aging issue, China replaced the one-child policy (OCP) with the new two-child-policy (TCP), which allows all families to have two children. It is expected that two children can share the caregiving burden, boosting the supply of informal LTC and providing elderly parents with greater old-age support. While this explanation may seem intuitively appealing, we know surprisingly little about the actual consequences of having more children on LTC support in modern China. Therefore, Chapter 5 examines the causal impact of having additional children (versus 1 child) on receiving LTC support. By focusing on individuals who need help with daily activities, our study also informs policymakers about the potentially unmet demand for formal LTC services.

Chapter 2 Effects of Long-Term Care Insurance on Financial Well-Being

2.1. Introduction

Since more and more people need long-term care (LTC) services,[1] LTC expenditures have become one of the largest financial risks faced by elderly people and their families in the United States (Brown & Finkelstein, 2007). Although LTC is expensive,[2] there is limited public insurance coverage for LTC—Medicare covers only post-acute care up to 100 days, and Medicaid covers only individuals who have spent down most of their assets. On the other hand, only 13 percent of individuals age 65 years and older have private long-term care insurance (LTCI) (Congressional Budget Office, 2013). As a result, current LTC financing relies heavily on out-of-pocket (OOP) expenditures by individuals and their families until they spend down their assets and qualify for Medicaid. In 2010, Medicare enrollees spent an average of $900 OOP on LTC, representing one-third of their total OOP medical spending (Congressional Budget Office, 2013; Kaiser Family Foundation, 2014). Further, it is estimated that about 6 percent of people turning 65 in 2005 would spend a total of $100,000 or more of their own money on LTC (Kemper, Komisar, & Alecxih, 2005). The high cost of formal LTC might also limit an individual's choice of service; more than two-thirds of the most disabled seniors receive solely informal care, which might be inappropriate for individuals in need of more intensive services (Thompson, 2004).

LTC financing is also an urgent issue faced by the U.S. government. At $192 billion dollars in 2011, LTC for elderly people accounts for 8 percent of total health expenditures, and

[1] About 11 million Americans need LTC, and most of them are over 65 years old. It is estimated that 70 percent of people age 65 and over will eventually need LTC services at some point during their lifetimes (Centers for Medicare and Medicaid Services, 2014; Kaye, Harrington, & LaPlante, 2010).
[2] In 2014, the average monthly cost is $6,000 for nursing home care and $4,000 for home-based care (Genworth Financial, 2014).

1.3 percent of total GDP. Medicaid and Medicare currently pay for about two-thirds of this total spending. As Baby Boomers age and the need for LTC grows, the two insurance programs are expected to grow by an average of more than 5 percent per year, which places a heavy burden on government budgets (Congressional Budget Office, 2013).

To reduce individuals' financial burdens and curb Medicaid and Medicare spending on LTC, policymakers have discussed private LTCI as one solution since it shifts part of the responsibility of financing LTC from the public sector to the private sector. Purchase of private LTCI may not be broadly appealing for a variety of reasons, including the typical structure of the policies themselves, which usually limit benefits to a set dollar amount per day for a limited number of years. Individuals generally purchase policies several decades before needing care because their risk—and the corresponding price—is lower at that point, and they are able to lock in the lower rate. However, individuals in their 50s and 60s often have competing demands in terms of spending on children and their own parents, and might not see the policies as a valuable priority at that time in their lives (Sperber et al., 2014). The result is that many individuals explicitly or implicitly rely on Medicaid for their future LTC needs (Brown & Finkelstein, 2004), despite the associated requirement of spending down assets to qualify. Furthermore, reliance on Medicaid might in turn dampen incentives to accumulate assets. Therefore, various federal- and state-level programs have been used to fix these demand-side and supply-side issues to stimulate the demand for private LTCI. For example, federal government and half of the state governments offer tax subsidies to lower the effective purchase price for LTCI premiums, the Partnership for Long-Term Care program allows LTCI policyholders to keep more assets when they turn to Medicaid after their private policy benefits are exhausted, and the LTC awareness campaign promotes general awareness of LTC and LTCI.

From a policy perspective, the desirability of increasing private LTCI is often taken as self-evident in the current policy environment. However, prior research in this area has examined mainly the influences of LTCI on health service utilization and informal caregiver outcomes. Although financial protection is arguably the primary purpose of any health insurance (Zeckhauser, 1970), there is little empirical evidence on the economic consequences of having LTCI. Therefore, to evaluate private LTCI policies and inform policymakers of the potential costs and benefits of extending LTCI coverage, we need to understand whether and how LTCI affects key financial outcomes for elderly people. In this paper, we ask the following research questions: Does private LTCI improve the general financial well-being of insured individuals? If so, what are the mechanisms behind these effects? Specifically, we examine two types of financial outcomes—individuals' assets and their likelihood of enrolling in safety net programs (Medicaid and Food Stamps) as an extreme outcome. It is critical to understand how LTCI influences safety net program enrollment because it informs policymakers regarding whether LTCI prevents insured individuals against depleting their wealth, and informs the potential social benefits of expanding the LTCI market. Further, LTCI may affect insured individuals' wealth accumulation by affecting their saving and spending behaviors. Due to data limitations, we examine only two explanatory mechanisms— OOP medical expenditures and asset transfer behavior. Individuals who have fewer OOP medical expenditures and make fewer financial transfers are expected to be wealthier.

One big challenge of identifying the causal relationship between LTCI ownership and financial outcomes is addressing the potential endogeneity of owning LTCI; since LTCI ownership is a choice variable, it might be subject to selection bias. That is, individuals purchase LTCI based on their private information of their risk types and insurance preferences, which might also affect their financial outcomes. Therefore, failure to control for them might result in selection bias. We address this concern by using an instrumental variable (IV) approach, which mimics the random assignment process and leads to plausibly unbiased estimates (Angrist, Imbens, & Rubin, 1996). Our IV is based on the exogenous variation in LTCI ownership caused by state tax subsidies for LTCI purchase.

Our results suggest that LTCI might be effective as a financial management tool since it might increase the assets of policyholders and reduce their likelihood of enrolling in safety net programs by potentially removing incentives for reducing personal savings and making parent-child financial transfers to spend down their assets voluntarily. However, it might not protect the insured against large OOP medical expenditures. In addition, public policies designed to encourage LTCI purchase should consider additional savings associated with reduced safety net program enrollment and increased personal savings as potential social gains.

Our paper proceeds as follows. In Section 2, we discuss the background and conceptual framework. In Section 3, we discuss the data, sample selection criteria, measures, and estimation strategy. Section 4 presents the main results and Section 5 presents conclusions.

2.2. Background and conceptual framework

2.2.1. Background

2.2.1.1. Asset planning for seniors

Since Medicare does not cover LTC and only few seniors have private LTCI, Medicaid becomes the most important potential alternative source of LTC financing for elderly people. However, Medicaid LTC rules require individuals with substantial assets to exhaust any assets above the

Medicaid qualifying levels[3] to be eligible, which leave their community-dwelling spouses and joint assets at risk. As a result, some seniors use asset planning strategies to shelter their assets (Centers for Medicare and Medicaid Services, 2008; Timothy & Korbin, 2006). One commonly used approach is to invest in assets that are not counted by Medicaid. Because Medicaid does not include assets such as individual's primary home[4], car, and personal items in the asset total to determine eligibility, seniors may spend more money on exempt assets to accelerate Medicaid qualification. They may also transfer assets (e.g., money, gifts, and home ownership) to their relatives or a third party for free or for less than fair market value. The elderly may still claim the home as their "life estate" and retain a right to live in the home for the rest of their lives. However, transfers made within 60 months prior to Medicaid application might be subject to penalties under the Medicaid Deficit Reduction Act of 2005 (i.e., the "look-back" period). Last but not least, the Medicaid/ institutionalized spouse may divorce the well/ community-dwelling spouse and split their assets in favor of the well spouse to avoid impoverishing the well spouse (i.e., "Medicaid divorce").

2.2.1.2. How LTCI may affect financial outcomes?

Theoretically, LTCI might influence the insured's financial well-being through several mechanisms. It might protect insured individuals against large LTC expenditures, and reduce their OOP medical expenses. It might also mitigate saving disincentives associated with the existence of Medicaid, and encourage individuals to reduce consumption, increase personal savings, and reduce voluntary asset spend-down/transfers. Our study is related to two strands of

[3] In most states, an individual can keep $2,000 in countable assets, and married couples who are still living in the same household can keep $3,000 in countable assets.
[4] Some states require the equity value of the home to be $500,000 or less

literature on these mechanisms. The first strand of literature examines the financial protection effects of conventional acute care health insurance schemes in the U.S.. Generally, these studies find positive results. For example, Goldman & Zissimopoulos (2003) find that poor, elderly people with Medicaid spend less on OOP expenditures than their counterparts without Medicaid do. Barcellos & Jacobson (2014) find that Medicare reduces total OOP medical expenditures, and reduces the likelihood of having problems paying medical bills. Another well-known study, the Oregon Health Insurance Experiment, finds that Medicaid reduces the amount of OOP spending, the probability of having an unpaid medical bill sent to a collection agency, the probability of having to borrow money or skip paying other bills because of medical expenses, and the probability of having catastrophic OOP medical expenditures[5] (Baicker et al., 2013; Finkelstein et al., 2011).

However, LTCI is distinct from acute care health insurance, and these results might not apply to it (Konetzka, 2014). For one thing, compared to most acute health insurance policies, LTCI policies are less comprehensive in terms of their benefits rules, and may therefore provide insufficient protection against medical spending risk[6]. For another thing, price elasticity for acute care may be different from that for LTC. Therefore, the two types of insurance may have different effects on health service utilization, and further cause different OOP costs.

[5] Defined as OOP medical expenses that exceed 30% of income.

[6] First, most private LTCI policies have an elimination period, usually 30 to 100 days, which is the waiting period from the time the policyholder starts to use LTC until the time the policy begins paying for the care. Second, most policies have a preset daily coverage limit, as well as a lifetime maximum benefit period. Since, on average, policyholders who use LTC start to use the service 15 years after they first purchase the policies, policies that do not have inflation protection might be insufficient to cover the daily cost of LTC services, and leave many elderly individuals exposed to significant OOP risk. Third, policyholders who fail to pay premiums at some point after initial purchase are liable to forfeit any right to future benefits (Brown & Finkelstein, 2009). As a result, Brown & Finkelstein (2007) estimate that the typical insurance policy purchased by a 65-year-old in 2002 covers only 13 percent of their expected LTC costs.

The second strand includes literature on precautionary saving and social insurance. Theoretical studies imply that the implicit tax of holding assets inherent in Medicaid LTC rules should lead to fewer personal savings and more voluntary asset transfers among elderly people (Hubbard, Skinner, & Zeldes, 1995). That is, since Medicaid covers only individuals with minimal assets, to qualify for Medicaid, individuals with more assets must pay for LTC with their own money until they meet their states' asset eligibility levels, which means an implicit tax rate of 100 percent on their previous savings above the asset threshold. Therefore, individuals who have a higher perceived probability of using Medicaid in the future should have greater incentives to save less, and transfer assets to their children to spend down/hide their assets voluntarily. Also, since transferring assets shortly before Medicaid enrollment may disqualify individuals from Medicaid,[7] elderly people should start this process years before they need LTC. There is extensive empirical research supporting these hypotheses. For example, a study by Gruber & Yelowitz (1999) finds that Medicaid eligibility has a significant negative effect on wealth holdings, and a positive effect on consumption expenditures, and these effects are stronger in the presence of an asset eligibility test. A recent study by Greenhalgh-Stanley (2015) also finds that elderly people who live in states with more assets protections[8] have more total assets, financial assets, and home equity. Bassett (2007) finds that people with a higher self-assessed probability of entering a nursing home are more likely to make an asset transfer, suggesting that individuals voluntarily spend down their assets to qualify for Medicaid LTC coverage.

However, none of these prior studies has examined the role of private LTCI as a potential

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[7] Gifts or transfers of assets made within 60 months prior to Medicaid application might be subject to penalties under the Deficit Reduction Act of 2005.
[8] Spouses are allowed to keep a certain amount of the couple's combined financial resources after institutionalized spouses enroll in Medicaid.

offset to Medicaid-related disincentives to asset accumulation. Since LTCI ownership removes some of the incentives inherent in Medicaid LTC rules, we would expect insured individuals to save more and make fewer voluntary transfers. We test these hypotheses and discuss them as explanatory factors for the change in general wealth.

Taken together, our study extends the literature by examining how LTCI affects the general financial well-being of insured individuals for the first time, and considers the mechanisms behind these effects. We control for potential endogeneity of LTCI ownership using an IV approach.

2.2.2. Conceptual framework

Based on a standard, expected-utility framework, an individual decides to purchase LTCI if his expected utility with insurance is greater than without insurance. The predominant theoretical model for demand for LTCI is by Pauly (1990), in which a consumer is assumed to choose consumption to maximize his lifetime utility function (EU). Adapted from the original, this can be given by:

$$EU = \sum_{t=1}^{H} p_t^h \, U(C_t) + \sum_{t=1}^{H} p_t^s \, \bar{U}^s \tag{1}$$

s.t. the budget constraint:

$$\bar{A} \geq \sum_{t=1}^{H-S} C_t + S\bar{X} \tag{2}$$

where H is the maximum length of life, p_t^h is the probability of surviving to period t in a healthy state, C_t is the dollars of consumption in period t, p_t^s is the probability of surviving to period t in a sick state (in need of LTC), and $\bar{U}^s$ is the utility level if one is sick with chronic illness and consuming $\bar{X}$ worth of LTC per time period (the only desired consumption in the sick state). $\bar{A}$ is initial assets, S is time in a sick state, and $\bar{W}$ is greater than $S\bar{X}$ such that the individual is unlikely to qualify for Medicaid. Then, conditional on having LTCI or not, the consumer faces several additional decisions that will affect his or her wealth. One is how much to spend (e.g., on OOP medical expenditures, other goods, financial transfers), and another is how much to save, which equals the net value of earnings less spending.

2.2.2.1. How does LTCI ownership affect OOP LTC expenditures?

In economics, the primary motivation for purchasing health insurance is arguably to protect policyholders against financial losses associated with large medical expenditures. On the one hand, LTCI reduces the net purchase price of LTC to the insured by offering full or partial coverage. On the other hand, it might also lead to a demand-side, ex post moral hazard and increase care utilization. That is, health insurance might stimulate additional use of medical care, defined as care that would not be demanded if paid for completely OOP (McGuire, 2011). In the long-term care context, this means an insured person is likely to receive more LTC and/or care in a more desirable (sometimes, more expensive) setting because the person's choice set has been expanded due to the availability of the insurance payoff (Konetzka, He, Guo, & Nyman, 2014). For example, it has been found that LTCI leads to more nursing home use (Konetzka et al., 2014; Y. Li & Jensen, 2011), and nursing homes catering to private residents are preferred on a number of characteristics over those catering to Medicaid residents (Mor, Zinn, Angelelli, Teno, & Miller, 2004; R Tamara Konetzka, 2009). Since the OOP payment equals the effective price of care times the quantity of care consumed, how LTCI will affect OOP LTC payments depends on whether the decrease in effective price offsets the increase in quantity caused by the moral hazard effect, which further depends on the cost-sharing benefit design and the insured's price elasticity of demand for LTC. Ultimately, whether the effect on OOP LTC expenditures is negative, positive, or zero must be determined empirically.

2.2.2.2. How does LTCI ownership affect wealth accumulation?

Hubbard et al.'s (1995) model explains how the presence of asset-based, means-testing transfer programs such as Medicaid might discourage household savings. In a two-period model, for a means-tested transfer program in time s, the amount of government transfer TR_s depends on the maximum of 0 and the sum of the minimum consumption floor $\bar{C}$ and medical spending M_s less all available sources of money (including assets from the previous period $A_{s-1}(1+r)$ and new earnings E_s). That is, the government transfer, if made, guarantees only the minimum consumption $\bar{C}$ once medical spending is covered.

$$TR_s = max\{0, (\bar{C} + M_s) - [A_{s-1}(1+r) + E_s]\} \quad (3)$$

Therefore, one fewer unit of consumption in the current period yields 1+r more units of assets in the next period if no government transfer is received in the next period, but zero more units if a transfer is received. In other words, savings are subject to an implicit tax rate of 100 percent in the event of receiving Medicaid in the next period. As a result, Medicaid creates incentives for reducing wealth and making voluntary asset transfers among households that expect to turn to Medicaid in the future[9]. We therefore test the hypothesis that having LTCI removes these incentives inherent in Medicaid LTC rules, and leads to more savings and fewer asset transfers.

[9] Brown & Finkelstein (2004) find that Medicaid crowd-out private LTCI for the bottom two-thirds of the wealth distribution, suggesting a large number of households depend on Medicaid for their LTC.

2.3. Methods

2.3.1. Data

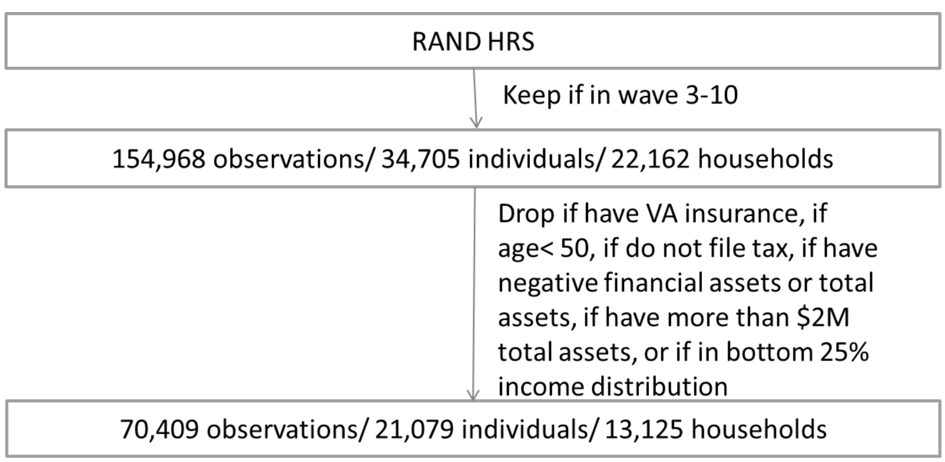
Our primary source of data is from the Health and Retirement Study (HRS), which is a longitudinal study that surveys a national representative sample of more than 20,000 Americans over the age of 50, and their spouses, since 1992. Respondents are initially drawn from the non-institutionalized population. They are then interviewed biennially, and are followed after they move in and out of nursing homes. The HRS dataset collects detailed information on individual's socioeconomic status, health, health service utilization, healthcare expenditures, health insurance, and family structure. It is the only publicly available dataset that includes consistently worded questions on LTCI. The RAND Center for the Study of Aging developed a longitudinally consistent dataset from the original HRS dataset, including imputations of missing data (http://hrsonline.isr.umich.edu/index.php). We use the RAND HRS data file (version N) in conjunction with the original HRS data for most variables. We also use the Cross-Wave Geographic Information (detail) file to match respondents to their state and county of residence, and use the state-level instrument. In addition, we collect the county-level nursing home beds supply from the Area Health Resource File (AHRF), and collect state tax policies from the literature (Goda, 2010; Grabowski & Gruber, 2005) and manual searches of state tax return forms. We use data from waves 3 to 10 (1996–2010) for our analyses because the LTCI questions in waves 1 and 2 are inconsistent and subject to substantial measurement error (Finkelstein & McGarry, 2006).

2.3.2. Sample

We use observation-level, pooled sample from all waves, and account for the multiple observations of the same individuals in the error structure in our regression analyses. We also conduct sensitivity analyses to study individuals' financial outcomes 4 years after holding LTCI.

A schematic of our analysis sample is pictured in Figure 2.1. From the combined HRS sample spanning 1996–2010 (154,968 observations), we limit our sample for our primary analyses to those observations who (1) were not on veterans' insurance, (2) were over 50 years of age, (3) filed tax, (4) had positive total assets and financial assets, (5) had less than $2 million in total assets, and (6) were not in bottom 25 percent income distribution[10]. These exclusions enable us to focus on respondents who are more likely to purchase LTCI to improve the strength of our instruments (Brown & Finkelstein, 2004). Our final sample includes 70,409 observations. We applied other restrictions in certain models. For parent-child financial transfer models, we also exclude individuals who have no children.

Figure 2.1. Deriving the Analysis Sample



[10] Very poor people have lower opportunity cost to enroll in Medicaid, and very rich people can afford to self-insure (Brown & Finkelstein, 2004)

2.3.3. Variables

2.3.3.1. Dependent variables

We have two types of financial outcome measures: those that directly measure an individual's general financial well-being and those that explain the potential mechanisms of the change in the insured's financial well-being.

Household non-housing financial assets and total assets. We use the dollar amount of household non-housing financial assets and total assets as measures of general financial well-being. Respondents are asked to report their household-level asset ownerships and values. Those who do not provide an exact amount are then asked unfolding bracket questions. Therefore, the raw data contain valid zero-value responses, exact amounts, complete and incomplete bracket responses, claim of ownership only, and unknown ownership. RAND imputes a consistent measure of wealth across all waves using bracketed responses and imputation models if an exact amount is not reported. Specifically, for respondents for whom nothing is known, RAND first imputes their asset ownership. Given ownership, RAND then imputes brackets and exact amounts. The same set of covariates are used in all asset model specifications (ownership, bracket, and amount for all asset types). Covariates in imputation models include: husband and wife's education, health status, age, race, marital status, occupation class, cognition, bequest expectations, a number of income amounts (imputed, when necessary), and indicators of pension or government benefit receipt. Comparison of the distributions of imputed assets with distributions from the Survey of Consumer Finances indicates high imputation quality[11]. We use

[11] See the RAND HRS Data Version N for a detailed description of the imputation method (RAND Center for the Study of Aging, 2014).

RAND-imputed household financial assets and total assets during our analyses. Specifically, non-housing financial assets are defined as the net value of stocks, mutual funds, investment trusts, bank accounts, certificates of deposit, Treasury Bills, government bonds, bonds, and bond funds, less debt. Total assets are defined as the net value of non-housing financial assets, housing, real estate, vehicles, businesses, and IRA/Keogh accounts, less home loans. Since all asset measures are reported in nominal dollars, we inflate them to 2010 dollars using the CPI-U. The measurement error due to imputation and self-report may bias results if it is correlated with individual's LTCI ownership. We address this concern by controlling for a similar set of covariates and by using an instrumental variable approach.

Medicaid and Food Stamps enrollment. We construct dichotomous variables for enrollment in time t using HRS questions that ask directly about Medicaid and Food Stamps coverage at any time since the previous wave.

Large total OOP medical expenditures[12]. Respondents are asked to report their individual-level spending on hospitals, nursing homes, doctors, dentists, outpatient surgery, prescription drugs, home-based care, and special facilities since the previous wave. For individuals who do not provide an exact value, RAND imputes a consistent measure of OOP medical expenditures across all waves using bracketed responses and imputation models. Covariates in imputation models include: age, age-squared, education, subjective health status, gender, marital status, race, whether an individual has any health insurance, whether an individual reported a hospital or nursing home stay, and number of doctor visits. Goldman, Zissimopoulos, & Lu (2011) compare the distribution of RAND imputed measures of OOP

[12] We use total OOP medical costs instead of OOP LTC costs because: (1) HRS does not have consistent measures for OOP nursing home costs and OOP home care costs, (2) LTC costs are the largest component of total OOP medical costs, and (3) total OOP medical costs more directly relate to elderly people's financial security.

spending to the Medical Expenditure Panel Survey (MEPS) and Medicare Current Beneficiary Survey (MCBS), and find that OOP spending data are of high quality. We inflate OOP expenditures to 2010 dollars using the CPI-U. To further account for potential bias due to measurement error (e.g., recall bias and imputation), we define our two OOP variables dichotomously, indicating whether the respondent's total OOP payments for healthcare exceed $10,000 or $25,000.

Parent-child financial transfers. We define the financial transfer variable dichotomously using a question from HRS that asks directly about the presence of any parent-child financial transfers totaling $500 or more since the previous wave.

2.3.3.2. Treatment variable

We define a dichotomous variable for LTCI enrollment in time t using a question in HRS that asks whether the respondent currently has LTCI. Specifically, respondents are asked: "Not including government programs, do you now have any insurance which specifically pays any part of long-term care, such as, personal or medical care in the home or in a nursing home?". Respondents who indicate having long-term care insurance are then asked: "Does this plan cover care in a nursing home facility only, personal or long-term care at home, or both in-home and nursing home care?". In our main specifications, LTCI purchase is defined as answering "yes" to the LTCI ownership question, regardless of their answers to the follow-up coverage question.

2.3.3.3. Control variables

We also control for (or in some cases, stratify by) a rich set of individual-level variables available in the HRS that might be related to both holding LTCI and financial outcomes.

Specifically, we control for age, gender, race, ethnicity, education, employment status, marital status, number of children, any health insurance coverage, number of diagnosed chronic conditions,[13] number of limitations in activities of daily living (ADLs), self-rated health status, county-level number of nursing home beds per thousand 65-year-olds (proxy for LTC service availability), life insurance coverage (proxy for risk aversion). We also control for state and year fixed effects to account for general time trends in individuals' financial well-being and unobserved time-invariant state characteristics such as time-invariant state tax policies.

2.3.4. Empirical strategy

2.3.4.1. Structural model setup

We begin by estimating the effects of LTCI on financial outcomes using ordinary least squares (OLS) and logistic regression models. Specifically, we use OLS models (4) for continuous outcomes (financial assets and total assets) and logistic regression models (5) for binary outcomes (i.e., Medicaid and Food Stamps enrollment, large OOP medical expenditures, and parent-child transfers).

$$Y_{it} = \alpha_0 + \alpha_1 LTCI_{it} + \alpha_2 X_{it} + Year_t + State_{it} + \delta_{it} \tag{4}$$

$$logit(P(Y_{it} = 1)) = \alpha_0 + \alpha_1 LTCI_{it} + \alpha_2 X_{it} + Year_t + State_{it} \tag{5}$$

Here, Y_{it} is an outcome measure for individual i at time t. $LTCI_{it}$ represents whether that individual is covered by LTCI. X_{it} is a vector of controls for individual-level characteristics. $Year_t$ and $State_{it}$ represent year and state fixed effects. δ_{it} represents the error term.

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[13] Number of diagnosed chronic conditions out of a list of 8 conditions, including hypertension, diabetes, cancer, lung diseases, heart problems, stroke, psychiatric problems, and arthritis.

2.3.4.2. Instrumental variables design

The main concern with naïve OLS or logistic regression estimates is that LTCI ownership might be endogenous to individual characteristics that also affect financial outcomes. According to Finkelstein & McGarry (2006), there exists both preference- and risk-based selection in the LTCI market. That is, individuals are more likely to purchase LTCI if they have private information that they are high risk and/or if they have strong tastes for insurance. For example, insured individuals might have higher likelihood of needing LTC, or might prefer formal care to informal care because they value autonomy for themselves and their children (Sperber et al., 2014), and might therefore have more medical spending, more financial problems, and fewer parent-child transfers. Alternatively, the insured might be wealthier at the baseline, and more risk averse, and might therefore have fewer medical expenditures, fewer financial problems, and more parent-child transfers. Furthermore, bequest motives may also affect seniors' saving and LTCI holding decisions. It is often assumed that individuals with strong bequest motives are more likely to purchase LTCI since LTCI insures bequests (Pauly, 1990). However, a recent study by Lockwood (In press) finds that bequest motives are positively correlated with savings and negatively correlated with LTCI ownership because they make self-insurance more attractive.

Failing to control for such factors might result in selection bias, although it is unclear whether the estimates will be biased upward or downward since these unobserved confounders act in offsetting directions.

We use an instrumental variable approach to address this potential regressor endogeneity issue. This approach uses one or more instruments, which affect LTCI coverage but do not directly affect the financial outcomes, to mimic a randomization of individuals to different likelihoods of having LTCI. The ideal instrument(s) should affect one's LTCI ownership but should not affect his financial outcomes without altering LTCI status. That is, the instrument(s) should be exogenous and should be uncorrelated with the unobserved confounders in the structural model. When validity assumptions are met, IV approach uses only exogenous sources of variation, and allows us to obtain unbiased treatment effects (McClellan, McNeil, & Newhouse, 1994). Adapted from Coe, Goda, & Van Houtven (2015) and Goda (2010), our instrument is based on the exogenous variation in LTCI ownership caused by state tax subsidies for LTCI purchase.

Specifically, our instrument is a dichotomous variable indicating the availability of any state tax subsidies for LTCI purchase in a specific year. It is assumed that variation in state subsidies for the purchase of LTCI might result in variation in propensity to purchase LTCI. As shown in Table 2.1, there is great variation in tax policies over state and time. In 1996, among the 41 states and the District of Columbia that levied a broad-based personal income tax[14], only 3 states offered tax deductions, and 1 state offered both tax deduction and tax credit for LTCI purchase. By 2010, 15 states and the District of Columbia offered tax deductions, 8 states offered tax credits, and 2 states offered both tax deductions and tax credits for LTCI purchase. Goda (2010) finds that the average value of the tax subsidies was 4.6% of premiums, and the average state subsidy leads to 2.7 percentage points (or approximately 28 percent increase at the pre-subsidy coverage rate of 9.5 percent) increase in LTCI purchase.

[14] AK, FL, NV, SD, TX, WA, and WY did not have an income tax, and NH and TN collected income tax only on interest and dividend income

Table 2.1. Summary of state tax subsidies for LTCI purchase

State	1996	1998	2000	2002	2004	2006	2008	2010
Alabama	D	D	D	D	D	D	D	D
Colorado			C	C	C	C	C	C
District of Columbia						D	D	D
Iowa		D	D	D	D	D	D	D
Idaho				D	D	D	D	D
Illinois	D	D	D	D	D			
Indiana			D	D	D	D	D	D
Kansas						D	D	D
Kentucky			D	D	D	D	D	D
Louisiana				C	C	C	C	C
Maryland			C	C	C	C	C	C
Maine	D	D	D	D	D	D	D	D
Minnesota		C	C	C	C	C	C	C
Missouri			D	D	D	D	D	D
Mississippi							C	C
Montana	D	D	D	D	D	D	D	D
North Carolina			C	C	C	C	C	C
North Dakota	CD	CD	CD	CD	CD	CD	CD	CD
Nebraska						D	D	D
New Mexico			D	D	D	D	D	D
New York	D	D	D	C	C	C	C	C
Ohio			D	D	D	D	D	D
Oregon			C	C	C	C	C	C
Utah			D	D	D	D	D	D
Virginia			CD	CD	CD	CD	CD	CD
Wisconsin		D	D	D	D	D	D	D
West Virginia			D	D	D	D	D	D

C = Credit, D = Deduction

Although this instrument is a state-level variable, which is less likely to be confounded by individual-level unobserved confounders (e.g., risk aversion and unmeasured health), there may be some threats to IV exogeneity. The main concern is that states might adopt tax subsidies for savings and investments and/or change Medicaid policies around the same time that the LTCI tax subsidies were enacted, which may lead to change in wealth independently of the effect through LTCI (i.e., policy endogeneity). In terms of Medicaid policies, Goda (2010) finds that the implementation of state LTCI tax subsidies was unrelated to changes in Medicaid eligibility or age ratios (which are related to rating regulations) (Coe et al., 2015; Goda, 2010). In terms of other tax policies that may directly lead to changes in individuals' wealth, we examine state tax treatments of income from Social Security benefits and pensions between 1998 and 2010 since they constitute the major sources of income of seniors[15]. We find that those policies are very consistent over time. In 1998, among the 41 states and the District of Columbia that levied a broad-based personal income tax, 26[16] states and the District of Columbia provided a full exclusion for income from Social Security, and 10 states[17] provided a full exclusion for income from pensions. As of 2010, only the State of Wisconsin had changed its tax policies and started to offer full exclusion for Social Security income in 2008 (Baer, 2001; Edwards & Wallace, 2004; McNichol, 2006; Penner, 2000; Snell, 2011; Snell & Waisanen, 2009). Another concern is that the instrument may be correlated with formal LTC supply in local market, which may further affect individuals' LTC expenditures and financial well-being. Therefore, we control for

[15] In 2013, 34% of the aggregate income of the older population comes from Social Security benefits, 33% comes from earnings, 22% comes from pensions, and 11% comes from assets. Social Security benefits also account for 90% or more of the income received by 35% of beneficiaries (Administration on Aging, 2016).
[16] The 26 states are: AL, AZ, AR, CA, DE, GA, HI, ID, IL, IN, KY, LA, ME, MD, MA, MI, MS, NJ, NY, NC, OH, OK, OR, PA, SC, and VA
[17] The 10 states are: AL, HI, IL, KS, LA, MA, MI, MS, NY, and PA

county-level number of nursing home beds per thousand 65-year-olds as proxy for formal LTC supply[18]. Finally, although it is unlikely that seniors will choose where to live and move solely based on state tax subsidies for LTCI purchase, we still test the robustness of results excluding individuals who moved between waves. Overall, it is theoretically plausible to consider the tax subsidies for LTCI purchase to be exogenous.

Empirically, although the exogeneity of our IV cannot be tested directly, we present a similar "IV balance check" table as that presented in the McClellan et al. (1994) study. Table 2.2 shows the correlation coefficients and p values between the IV and each of the independent variables while controlling for the other independent variables. Intuitively, this table highlights whether an observed confounder (e.g., education) would be correlated with the IV and lead to IV endogeneity if it were unobservable and not controlled for. Despite significant differences between the two groups in age and nursing home supply, the differences in all the other observed characteristics are not statistically significant at the 0.10 level. These results show that the IV approach has greatly improved sample balance in observed characteristics/ confounders (compared to those shown in Table 2.3), which also suggest a good balance in unobserved characteristics/ confounders and provide support for the validity of the tax instrument.

[18] Although the IV is a state-level variable, but the county-level nursing home supply affect individuals' LTC/medical consumption more directly.

Table 2.2. IV balance check

Independent variables	Marginal effects	P value
SES		
Age	0.003	0.017
Age^2	-0.000	0.025
Female	0.001	0.735
Black	0.004	0.172
Hispanic	-0.002	0.503
Education (less than high school is the reference)		0.474
GED	-0.005	
High school	-0.001	
Some college	-0.004	
College and above	-0.001	
Retired	-0.002	0.363
County-level LTC supply		
NH beds per 1,000 people 65+ (bottom 20% is the reference)		<0.001
20%-40%	0.023	
40%-60%	0.007	
60%-80%	0.032	
Top 20%	0.009	
Family		
Number of children (0 is the reference)		0.403
1	-0.001	
2	-0.003	
3+	-0.005	
Married or partnered	-0.001	
Health insurance		
Uninsured	-0.003	0.516
Health		
Diagnosed disorder	-0.000	0.920
Self-rated health (excellent is the reference)		0.356
Very good	0.002	
Good	0.005	
Fair	0.004	
Poor	-0.001	
Number of ADLs(0 is the reference)		0.309
1	-0.007	
2	-0.004	
3+	-0.004	
Risk aversion		
Has life insurance	0.001	0.742

2.3.4.3. Instrumental variable models

Since we have a binary treatment variable and in some models, binary dependent variables, traditional two-stage least squares regression (2SLS) models might result in biased results. We therefore use two-stage residual inclusion (2SRI) methods (Terza, Basu, & Rathouz, 2008).

Our first-stage, logistic regression model uses instrumental variable(s) to predict LTCI status:

$$logit(P(LTCI_{it} = 1)) = \beta_0 + \beta_1 IV_{it} + \beta_2 X_{it} + Year_t + State_{it} \tag{6}$$

where IV_{it} is the instrumental variable for individual i at time t, and the other variables are the same as described in equations (4) and (5). We then calculate the response residuals $\widehat{r_{it}}$ from the first-stage model, which are the difference between the predicted probabilities and observed LTCI values. These residuals are included in the second-stage models below as additional regressors to produce the correct adjustment for the endogeneity in the outcome equations:

$$Y_{it} = \alpha_0 + \alpha_1 LTCI_{it} + \alpha_2 X_{it} + Year_t + State_{it} + \widehat{r_{it}} + u_{it} \tag{7}$$

$$logit(P(Y_{it} = 1)) = \alpha_0 + \alpha_1 LTCI_{it} + \alpha_2 X_{it} + Year_t + State_{it} + \widehat{r_{it}} \tag{8}$$

where Y_{it} is an outcome measure, and the other variables are the same as described above. We use the second-stage model to estimate the causal effects of LTCI on our financial outcomes. We use equation (7) to model linear outcomes, and use equation (8) to model binary outcomes. In all of our regression models, we cluster the standard errors on the individual identifier. Finally, we perform a bootstrap procedure for both stages, with 500 iterations to correct standard errors (Efron, 1981).[19]

Our main models estimate the effect of owning LTCI at time t on outcomes in time t

[19] In our bootstrap process, we resample at the observation level. Our results are robust to resampling at the individual and household levels, which might approximate the real data generating/sampling mechanism better.

across the sample (where the LTCI policy might have been purchased recently or many years prior). We also construct numerous alternative specifications to test the robustness of our results. First, to control for unmeasured baseline health further, we run our models on those who have ever exhibited potential need for LTC[20] and are more likely to be affected by LTCI. Second, we examine long-run effects and measure outcomes 4 years after holding LTCI. Third, we test the robustness of results excluding individuals who moved from another state 2 years before holding LTCI. Finally, although the IV approach should correct for measurement error in our endogenous treatment variable, we also test an alternative measure of LTCI holding that is defined as individuals who reported having LTCI and were able to answer the LTCI coverage question[21]. This follow-up question helps us to confirm individuals' LTCI status.

2.4. Results

2.4.1. Descriptive statistics

Tables 2.3 and 2.4 provide descriptive statistics for our sample. Overall, by construction, our sample has higher education levels, income, and assets than average Americans of a similar age. In addition, there are significant differences between respondents with and without LTCI in their socioeconomic status, county-level formal LTC supply, family structure, un-insurance rates, health, and risk preferences, which suggest the potential endogeneity of LTCI and the necessity of using the IV approach.

[20] Defined as individuals who need help with ADL/IADL, who have been diagnosed with memory-related diseases, and who have bad self-reported memory

[21] We drop individuals who indicated having LTCI but were not able to answer the coverage question

Table 2.3. Descriptive statistics of independent variables, by LTCI ownership

Independent variables	LTCI=0 (N=60,484)	LTCI=1 (N=9,925)	P value
SES			
Age (Mean)	65.7(9.7)	68.1(9.5)	<0.001
Female (%)	53.4	57.5	<0.001
Black (%)	9.6	7.3	
Hispanic (%)	6.3	2.3	<0.001
Education (%)			<0.001
Less than HS	15.8	8.4	
GED	4.2	2.6	
High school	34.5	30.5	
Some college	23.4	23.2	
College and above	22.2	35.3	
Retired (%)	37.6	48.9	<0.001
County-level LTC supply			
NH beds per 1,000 people 65+ (%)			<0.001
Bottom 20%	19.3	20.0	
20%-40%	20.2	18.6	
40%-60%	19.7	18.4	
60%-80%	21.3	18.6	
Top 20%	19.6	24.4	
Family			
Number of children (%)			<0.001
0	6.1	7.7	
1	9.7	9.8	
2	28.6	31.6	
3+	55.7	50.9	
Married or partnered (%)	77.6	75.2	0.005
Health insurance			
Uninsured (%)	6.6	2.0	<0.001
Health			
Diagnosed disorder (Mean)	1.7(1.3)	1.7(1.3)	0.002
Self-rated health (%)			<0.001
Excellent	13.8	14.8	
Very good	32.9	38.1	
Good	32.6	30.9	
Fair	15.6	12.7	
Poor	5.2	3.5	
Number of ADLs (%)			<0.001
0	89.3	90.7	
1	5.8	5.4	
2	2.2	1.7	
3+	2.7	2.2	
Risk aversion			
Has life insurance (%)	72.1	75.9	<0.001

The comparisons between the two treatment arms are calculated based on simple two-sample t-tests or chi-squared test.
Standard deviations in parentheses.

Table 2.4. Descriptive statistics of dependent variables, by LTCI ownership

Dependent variables	**LTCI=0 (N=60,484)**	**LTCI=1 (N=9,925)**	**Total (N=70,409)**
Financial assets ($)	129,784 (217,928)	197,138 (261,880)	139,278 (225,862)
Total assets ($)	430,487 (415,032)	596,475 (463,674)	453,885 (426,157)
Medicaid (%)	1.4	0.6	1.3
Food Stamp (%)	1.1	0.4	1.0
Total OOP expenditures >$10k (%)	6.7	7.9	6.9
Total OOP expenditures >$25k (%)	1.7	2.0	1.8
Transfers (%)	44.5	48.3	45.1

Standard deviations in parentheses.

2.4.2. Regression results

Regression results of the first-stage model are in Table 2.5. We find that state tax incentives lead to a 1.6 percentage points increase in LTCI ownership, which is similar to the 2.7 percentage points increase found in the Goda (2010) study using a richer sample.

Tables 2.6 and 2.7 present regression results of the influence of LTCI coverage on financial outcomes: (1) non-housing financial assets, (2) total assets, (3) receives Medicaid, (4) receives Food Stamps, (5) total OOP medical expenditures greater than $10,000, (6) total OOP medical expenditures greater than $25,000, and (7) any parent-child transfers. Results of our primary specifications are displayed in Table 2.6. For each outcome, we show results from the naïve models with no instrument and from the tax IV models. Regression results of sensitivity test models are displayed in Table 2.7. For all models, we present marginal effects of LTCI ownership, bootstrapped standard errors, first-stage F statistics, mean values of the dependent variables, and numbers of observations (full first- and second-stage results are available on request).

Table 2.5. First-stage estimates

	LTCI
Tax IV	0.016***
	(0.006)
Age	0.022***
	(0.003)
Age^2	-0.000***
	(0.000)
Female	0.033***
	(0.005)
Race (white is the reference)	
Black	-0.014**
	(0.007)
Other races	-0.032***
	(0.012)
Hispanic	-0.046***
	(0.009)
Education (less than high school is the reference)	
GED	0.010
	(0.011)
HS	0.028***
	(0.006)
Some college	0.047***
	(0.007)
College and above	0.106***
	(0.008)
Retired	0.037***
	(0.005)
NH beds per 1,000 people 65+ (bottom 20% is the reference)	
20%-40%	-0.006
	(0.007)
40%-60%	-0.004
	(0.008)
60%-80%	-0.017**
	(0.008)
Top 20%	-0.000
	(0.009)
Number of children (0 is the reference)	
1	-0.019*
	(0.011)
2	-0.013
	(0.010)
3+	-0.034***
	(0.010)
Married or partnered	0.002
	(0.005)
Uninsured	-0.069***
	(0.006)
Number of diagnosed diseases	-0.001
	(0.002)

Table 2.5. First-stage estimates (continued)

	LTCI
Self-rated health (excellent is the reference)	
Very good	0.003
	(0.005)
Good	-0.014**
	(0.006)
Fair	-0.020***
	(0.007)
Poor	-0.038***
	(0.009)
Number of ADLs(0 is the reference)	
1	-0.007
	(0.007)
2	-0.023**
	(0.010)
3+	-0.012
	(0.010)
Has life insurance	0.026***
	(0.005)
Observations	68,780

* p<0.1; ** p<0.05; *** p<0.01. Standard errors in parentheses

Table 2.6. Estimates of marginal effects, base models

Model	(1) Financial assets	(2) Total assets	(3) Medicaid	(4) Food Stamp	(5) OOP>$10k	(6) OOP>$25k	(7) Transfers
No IV							
Marginal effect	34,344***	97,922***	-0.005***	-0.004***	0.009***	0.002	0.026***
S.E.	(4,132)	(7,293)	(0.001)	(0.001)	(0.003)	(0.002)	(0.007)
State tax IV							
Marginal effect	45,859***	123,635***	-0.000	-0.007**	-0.006	0.002	-0.048**
Bootstrap S.E.	(7,480)	(14,886)	(0.007)	(0.003)	(0.009)	(0.005)	(0.020)
First-stage F statistics	11.0	11.0	11.0	11.0	11.0	11.0	10.5
Mean of dependent variable	139,835	455,254	0.014	0.010	0.068	0.017	0.452
Observations	68,780	68,780	64,441	67,291	68,743	68,693	63,989

* p<0.1; ** p<0.05; *** p<0.01. Standard errors in parentheses.

Table 2.7. Estimates of marginal effects, sensitivity test models

Model	(1) Financial assets	(2) Total assets	(3) Medicaid	(4) Food Stamp	(5) OOP>$10k	(6) OOP>$25k	(7) Transfers
Need LTC							
Marginal effect	39,678***	120,812***	-0.001	-0.009***	-0.002	0.003	-0.037*
Bootstrap S.E.	(9,555)	(17,245)	(0.007)	(0.003)	(0.011)	(0.006)	(0.022)
First-stage F statistics	11.5	11.5	11.5	11.5	11.5	11.5	11.4
Mean of dependent variable	137,647	438,574	0.016	0.011	0.075	0.020	0.432
Observations	56,178	56,178	52,640	53,811	56,156	55,972	52,634
4 years later							
Marginal effect	68,846***	99,179***	-0.014***	-0.006	0.008	0.008	0.026
Bootstrap S.E.	(19,840)	(35,515)	(0.005)	(0.007)	(0.012)	(0.008)	(0.023)
First-stage F statistics	7.9	7.9	8.3	7.9	10.7	10.7	7.1
Mean of dependent variable	140,842	464,117	0.010	0.007	0.058	0.013	0.453
Observations	50,970	50,970	50,656	49,935	50,945	50,926	47,696
Drop if moved 2 years ago							
Marginal effect	44,261***	120,528***	-0.001	-0.007**	-0.005	0.002	-0.049**
Bootstrap S.E.	(8,739)	(15,494)	(0.006)	(0.003)	(0.009)	(0.005)	(0.020)
First-stage F statistics	11.6	11.6	11.6	11.6	11.6	11.6	11.1
Mean of dependent variable	139,391	454,834	0.014	0.010	0.068	0.017	0.451
Observations	67,894	67,894	63,632	66,458	67,845	67,825	63,145
Alternative LTCI definition							
Marginal effect	48,419***	127,344***	0.000	-0.007**	-0.004	0.003	-0.048**
Bootstrap S.E.	(8,574)	(15,387)	(0.008)	(0.003)	(0.009)	(0.005)	(0.020)
First-stage F statistics	9.0	9.0	9.0	9.0	9.0	9.0	8.3
Mean of dependent variable	139,995	455,574	0.014	0.010	0.068	0.017	0.452
Observations	68,202	68,202	63,884	66,719	68,165	68,117	63,452

* p<0.1; ** p<0.05; *** p<0.01. Standard errors in parentheses.

Our conceptual framework predicts that LTCI leads to increased savings. In line with this hypothesis, we find consistently higher financial assets and total assets for insured individuals using our base models, and the magnitudes of the effects are substantial. Specifically, we find LTCI ownership leads to a $45,859 increase in household financial assets (on a mean of $139,835) and a $123,635 increase in household total assets (on a mean of $455,254). These represent a sizable 33 percent increase in financial assets and a 27 percent increase in total assets. We further examine how changes in various types of assets contribute to the $123,635 increase in household total assets. Besides the $45,859 increase in household financial assets, LTCI also leads to a $48,842 significant increase in IRA/Keogh values (on a mean of $72,548), and a $12,813 significant increase in primary residence values (on a mean of $158,527) (full regression results are available on request). Our results suggest that LTCI has greater impact on "liquid assets" (e.g., IRA/Keogh and financial assets) than housing assets. One possible explanation may be that since Medicaid does not consider individuals' primary residence as countable assets[22], it has greater negative impact on liquid asset accumulation, and LTCI removes this disincentive. Furthermore, we see similar effects among less healthy individuals who need LTC. The effects on assets 4 years after holding LTCI are also significantly positive. We find that, in the long-run, the increase in total assets is primarily driven by the increase in financial assets. The diminished effects over time in total assets might be due in part to policy lapses between the time we measure LTCI ownership and the time we measure outcomes. Instrumental variable estimates using the alternative definition of LTCI status and excluding those who moved between waves are also robust.

Next, we examine the effect of LTCI on safety net program enrollment. We find that

[22] Although state Medicaid programs are required to seek recovery of payments from the individual's estate for LTC services.

LTCI reduces insured's likelihood of enrolling in Medicaid and Food Stamps programs, although the marginal effects are not statistically significant in the Medicaid model. The lack of significance/statistical power in our results might be due in part to low prevalence rates of outcomes. We find a much greater effect on Medicaid enrollment 4 years ahead, suggesting greater impact in the long-run. The IV estimates of other sensitivity models are generally consistent with those of our base specifications. Overall, our findings on safety net programs are consistent with those on assets, suggesting LTCI is effective as a financial management tool to improve the overall economic well-being of insured individuals.

Finally, we examine the potential explanatory mechanisms of these effects. Our IV results reveal a negative relationship between LTCI and $10,000+ OOP payments and a positive relationship between LTCI and $25,000+ OOP payments, but no consistent pattern that is statistically significant nor meaningful in magnitude[23][24]. The IV estimates of the sensitivity models are somewhat noisy and insignificant. One potential explanation for the lack of change in total OOP expenditures may be the moral hazard effect. That is, although insured individuals pay only a proportion of the total LTC costs, they may use more (formal) or higher-quality LTC services and spend the same on OOP LTC expenditures. This assumption is consistent with prior findings that the demand for paid LTC services is very price sensitive, and there exists significant moral hazard (Konetzka et al., 2014; Y. Li & Jensen, 2011). Another explanation may be the income effect— since insured individuals accumulate more assets, they may have a stronger demand for all types of health services. Last but not least, changes in LTC costs and

[23] Because many LTCI policies have an elimination period, usually 30 to 100 days, during which the policyholders still have to pay their LTC OOP, we also try a higher OOP threshold, $50,000, to capture impact of LTCI on OOP payments. We still find no significant effect.

[24] We also try continuous version of total OOP medical payments as an outcome, finding that LTCI leads to a $143 insignificant increase in total OOP costs (on a mean of $3,841).

utilization may also affect utilization and costs of other types of health services. For example, receiving LTC may enable recognition of problems and reminders of screenings that lead to greater use (and higher costs) of outpatient routine or acute care. Alternatively, access to LTC may reduce the probability of having an avoidable event that leads to outpatient or acute care utilization. Overall, our findings suggest that private LTCI is ineffective at protecting insured individuals against large OOP medical expenditures, which is consistent with the market-level finding that LTCI provides only very limited coverage relative to the total LTC expenditure risk (Brown & Finkelstein, 2007). Therefore, findings for OOP payments might not explain the increase in wealth.

On the other hand, our findings on parent-child financial transfers support our findings with respect to Medicaid-related wealth accumulation incentives. The IV results of our base models imply that LTCI induces a significant 4.8 percentage point reduction in the probability of giving financial help to children (on a mean of 45.2 percent). This negative effect is not observed in the 4-year model, suggesting LTCI delays asset transfers but does not substantially influence whether parents ultimately transfer assets to their children in the long-run. Furthermore, it is worth noting that the effects of LTCI on transfers, while not in comparable units to the effects on assets, are modest in magnitude. This might suggest that our finding of increased assets is not driven mainly by the change in parent-child financial transfers, and there might be a net increase in household savings due to LTCI.

2.5. Conclusions

Although policymakers often consider private LTCI a solution to LTC financing, no empirical study has focused on economic consequences caused by holding LTCI. In this paper, we estimate the causal effects LTCI has on individuals' wealth, and then assess the potential explanatory mechanisms of these effects. Our results are consistent with incentives associated with the existence of Medicaid as disincentives to asset accumulation (or incentives to transfer/hide assets). Individuals with LTCI face fewer incentives to spend down their assets, and are therefore expected to accumulate more assets and delay/reduce asset transfers. We also find evidence that LTCI is effective as an anti-poverty tool since it reduces insured individuals' likelihood of enrolling in safety net programs.

The effects on OOP payments are somewhat insignificant and small in magnitude. The lack of negative findings suggests that typical LTCI policies leave policyholders with part of the financial risk. Furthermore, although our analysis is not a direct assessment of moral hazard, our results are consistent with the existence of a moral hazard effect. Since insured individuals face a lower effective price for LTC and an expanded LTC choice set (as more services become affordable), they may use more formal LTC services. The potentially increased LTC use may be welfare-increasing and desirable among lower-income people or people with higher risk who might underuse formal LTC without insurance (Nyman, 2003; Pauly, 1968). In addition, increased use of formal LTC might also relieve the burden of informal caregivers in the form of time, effort, forgone wages, and other economic costs, which might further improve insured individuals' financial outcomes, and increase social welfare. Coe, Goda, et al. (2015) find that LTCI coverage induces less informal caregiving.

From a policy perspective, our study indirectly informs policymakers of whether the social gains from a tax subsidy for LTCI premiums would outweigh the cost of the tax subsidy. Using simulations, Goda (2010) estimates that each dollar of state tax subsidy for LTCI premiums produces approximately $0.84 in Medicaid savings, and the return is more for individuals with moderate wealth, and less for individuals with very high or very low wealth. However, her calculation does not take into account social gains from increased savings among LTCI policyholders, reduced utilization of other safety-net programs (e.g., Food Stamps), and increased tax revenue from caregivers who can provide less informal care and work more. We therefore expect a higher return than what she estimated if we take into account these desired spillover effects.

Our results are subject to several potential limitations, but we employ strategies in each case to address or minimize them to the extent possible. First, our sample is limited to individuals with moderate wealth, and we can estimate only local average treatment effects (LATEs) for subpopulation groups that are induced by our instruments to change the LTCI status (i.e., the compliers) (Imbens & Angrist, 1994). That is, we can only estimate the treatment effects among individuals who have LTCI because they live in a state that offers tax subsidies for LTCI purchase, and would not have LTCI otherwise. However, making these sample restrictions and using the IV approach are necessary, and it allows us to focus on seniors who are more likely to respond to tax subsidies. Second, there might be concern that the null hypothesis is incorrectly rejected when we consider a set of statistical inferences simultaneously. However, since our findings are robust across different model specifications, our significant findings are unlikely to be a statistical artifact, and the multiple testing problem does not appear to be an issue in our study. Third, due to data limitation, we do not know the exact time when individuals start and stop to purchase LTCI, and cannot precisely measure the changes in their LTCI enrolment and their length of holding LTCI. Therefore, we do not directly exploit the panel nature of the data and changes in financial well-being for specific individuals over time due to changes in their LTCI ownerships, but we conduct sensitivity analyses to study the long-run effect of LTCI holding.

Overall, our findings on the effects of LTCI on financial outcomes have several key implications. One is that current LTCI policy design might be insufficient to protect the insured against large medical expenditures. However, it might improve the general financial well-being of insured individuals by encouraging them to save more and reduce/delay asset transfers. Furthermore, public policies designed to encourage LTCI purchase to cover LTC services should consider additional savings associated with reduced safety-net program enrollment, increased labor participation due to reduced use of informal care, and increased personal savings.

Chapter 3 Effects of Long-term Care Setting on Spousal Health Outcomes

3.1. Introduction

Over the past three decades, there has been a large expansion in noninstitutional long-term care (LTC), and public financing of long-term care services has been shifting away from nursing home toward home and community-based services (HCBS). Medicaid, the primary payer for LTC for elderly people, spent 46 percent of its total LTC dollars on HCBS as of 2013[1], up from 13 percent in 1990 (Reaves & Musumeci, 2015). The Affordable Care Act will further allow the expansion on noninstitutionally-based LTC (Kaiser Family Foundation, 2013). The basic economic rationale for this expansion is based mainly on two assumptions: (1) LTC users generally prefer HCBS to institutional care, and (2) for nursing home residents with less intensive care needs, HCBS are cheaper (Grabowski, 2006). It is hoped that HCBS will provide high-quality care at a lower cost.

However, the desirability of HCBS among family members of care recipients is often taken as self-evident in the current policy environment. Prior studies in this area have mainly focused on the preferences and health outcomes of LTC users in different settings. We know surprisingly little about whether and how HCBS (versus nursing home care) may affect health outcomes for the family members of care recipients—especially the spouses—who are usually also old and fragile, yet play an important role in providing care and making LTC decisions. It is clear that we cannot estimate the overall costs and benefits of HCBS expansion without understanding how spousal well-being is affected.

Theoretically, HCBS may affect spousal health well-being through several mechanisms. On the one hand, since substituting HCBS for nursing home care may inevitably place greater

[1] With states spending between 21 percent and 78 percent.

caregiving burden on the spouses and providing informal care may lead to both short-term and long-term negative health outcomes (e.g., Brodaty, Green, & Koschera, 2003; Coe & Van Houtven, 2009; Gallant & Connell, 1998; Harwood et al., 1998), it is possible that this shift may have large, but unstudied, negative effects on spousal health. On the other hand, spouses may derive an internal satisfaction and better mental health outcomes from providing more intensive informal care ("warm-glow giving" (Andreoni, 1989, 1990)), from enabling care recipients to stay in their preferred LTC setting ("altruistic spouse" (Becker, 1974, 1992)), and from utility gains from living together with the care recipients ("cohabitation model" (Brien, Lillard, & Stern, 2006)). Because these theories provide potentially conflicting directions of effect, this question must be studied empirically.

To inform policy makers about the potential costs and benefits of HCBS expansion for spouses of HCBS recipients and help them to design programs to better support spouses, a rigorous evaluation of the impact of HCBS (versus nursing home) on spousal health outcomes is needed. Our study aims to examine the causal impact of different care settings on physical and mental health outcomes for spouses of care recipients. To the best of our knowledge, this is the first study to examine this causal relationship[2].

A key challenge of identifying the causal relationship is addressing the potential endogeneity of the choice of care setting. In most cases, the care setting is chosen by care recipients and/or their family members and may, therefore, be correlated with factors that may also affect physical and mental health for the spouses. Unless the endogeneity issue is addressed,

[2] HCBS include services such as formal home care, adult day care, and adult foster care, with formal home care being the most common. In this study, we only focus on formal home care due to lack of questions on other types of HCBS in our dataset. Furthermore, we intentionally estimate outcomes for all spouses but not just those who provide informal care to the care recipients, as caregiving itself may not be the only mediator through which care setting affects spousal health outcomes.

the estimated effect is subject to selection bias. We address this concern using an instrumental variable (IV) approach, which mimics the random assignment process and leads to plausibly unbiased estimates (Angrist et al., 1996). Our instrument is based on the exogenous variation in home care use caused by differences in nursing home supply in the local market. Furthermore, a set of socioeconomic status, family structure, health status attributes, and year and state fixed effects is controlled for to reduce bias further and increase precision.

We find that home care use leads to mostly insignificant, yet consistently negative effects on spousal physical health, which may be caused by increased informal care responsibilities. We also find improved spousal mental health outcomes, especially in depression symptoms, which may be caused by increased satisfaction derived from providing more intensive informal care, from enabling care recipients to stay in their preferred LTC setting, or from living together with their partners.

Our paper proceeds as follows. In Section 2, we discuss the background and conceptual framework. In Section 3, we discuss the data, sample selection criteria, measures, and estimation strategy. Section 4 presents the main results and Section 5 presents conclusions.

3.2. Background and conceptual framework

3.2.1. Background

Our study relates closely to two strands of literature. The first strand includes studies on informal caregiver health effects. Generally, these studies find negative physical and mental health outcomes. For example, Coe & Van Houtven (2009) examine the health effects of providing LTC to an elderly mother using an instrumental variable approach and find that continued caregiving leads to worse depression symptoms and self-rated health for married children. They also find persistently increased incidence of heart conditions for single sons. Connell (1994) examines the impact of caring for a spouse with progressive dementia on caregivers' health and finds that caregiving is associated with arthritis, cardiac and back problems, as well as stress-related health issues such as migraines and colitis. Gallant & Connell (1998) further find negative effects in terms of health behaviors (exercise, sleep, smoking, drinking, and weight maintenance) among spousal caregivers, and consider these behaviors as mechanisms through which caregiving stress is associated with adverse health outcomes. Furthermore, greater caregiver burden and worse health outcomes are found among female caregivers (Harwood et al., 1998; Lalonde & Kasprzyk, 1993), spousal caregivers (Barnes, Given, & Given, 1992; Miller, McFall, & Montgomery, 1991), older and sicker caregivers (Lalonde & Kasprzyk, 1993), those with prolonged caregiving time (Gaynor, 1990), and more intensive caregiving (Majerovitz, 1995). However, a few studies find the opposite results. For example, Fredman et al. (2008) find that caregivers and noncaregivers do not differ in rates of all-cause mortality or incident mobility limitations. However, when adjusting for amount of physical activity, caregivers have worse health outcomes. Therefore, they argue that caregiving behavior may increase physical activities and result in health improvements, which mask the adverse effects of high-intensity caregiving on health. However, since most of these cross-sectional studies (e.g., Connell, 1994; Gallant & Connell, 1998; Harwood et al., 1998; Lalonde & Kasprzyk, 1993) do not account for the potential endogeneity of providing informal care, the negative effects on health they find may not always be caused by caregiving. In addition, results from studies that focus on other informal caregivers (e.g. adult children) should also be interpreted carefully since spouse caregivers may differ from other caregivers significantly with regard to socioeconomic status, types and intensity of care they provide, and emotional bond with the care recipients (Pinquart & Sörensen, 2011).

Finally, caregiving is only one mechanism through which HCBS use may affect spousal health, whether spouses of HCBS users have better or worse health still needs to be studied directly.

The second strand includes literature on cost-effectiveness comparisons between HCBS and institutional care. There has been a long-standing debate about whether shifting public financing from institutional care toward HCBS would save money or even be cost-neutral. One general concern is that HCBS would attract qualified individuals who would not otherwise use institutional care (i.e., the woodwork effect) (Grabowski, 2006). Therefore, a number of studies have been conducted to evaluate the cost and/or effectiveness of various state HCBS programs. However, due to great heterogeneity of the HCBS programs and the enrollees, and the potential selection bias, there are no consistent findings on the cost or the effectiveness of HCBS programs. As for the cost studies, for example, Kane et al. (2013) find that in states with increased spending in Medicaid HCBS 1915(c) waivers, the decrease in nursing home service payments was offset by the increase in HCBS waiver and state plan expenditures, leading to a net increase in total Medicaid LTC expenditures from 2001 to 2005. Similarly, Guo, Konetzka, & Manning (2015) find that the use of Medicaid-financed HCBS may reduce but only partially offset utilization and Medicaid expenditures on nursing facility services. However, Eiken, Burwell, & Sredl (2013) find no strong evidence that shifting toward HCBS significantly increased or decreased overall Medicaid LTC spending from 1999 to 2007. Further, LaPlante (2013) observes a “woodwork effect” for people with intellectual and developmental disabilities (IDD) but not for people with disabilities other than IDD and elderly people. As for the effectiveness studies, Frytak, Kane, Finch, Kane, & Maude-Griffin (2001) find no difference in growth trajectories for ADLs, pain and discomfort, and psychological well-being between residents in assisted living facilities and residents in nursing homes. Pruchno & Rose (2000) find that physical and mental health outcomes for people living in assisted living facilities and nursing homes do not change at different rates. However, Wieland, Boland, Baskins, & Kinosian (2010) find that, when adjusting for risks, HCBS users have longer survival time than their counterparts in nursing home settings. Overall, the lack of consistent findings lends little clarity to the debate about HCBS expansion. On the other hand, it is likely that LTC setting will also affect the health well-being for family members of the care recipients. Yet, to the best of our knowledge, no study has compared the health outcomes and burdens for the spouses under different settings. Therefore, without assessing outcomes for spouses, who play an important role in making LTC decisions for care recipients and are inevitably affected by those decisions, the evidence base is insufficient to judge the costs and benefits of HCBS.

Taken together, we begin to fill a key gap in the literature, comparing spousal health outcomes under different LTC settings. By focusing on spouse caregivers, we are able to study the caregiver group that is potentially most vulnerable to HCBS expansion. We also carefully control for potential endogeneity of care setting choice using an IV approach, which plausibly leads to unbiased causal inferences. In addition, our study indirectly contributes to emerging knowledge of measuring outcomes of HCBS expansion.

3.2.2. Conceptual framework

Adapted from the Grossman Health Capital model (Grossman, 1972) and the Coe & Van Houtven (2009) study, we use the reduced-form model (1) to examine the effects of HCBS use on spousal health outcomes.

$$H_{s,t} = \beta_0 + \beta_1 HomeCare_{p,t} + \beta_2 H_{s,t-1} + \beta_3 X_{s,t} + \beta_4 X_{p,t} + \varepsilon_{it} \tag{1}$$

In this model, the spouse's current health ($H_{s,t}$) is a function of his/her previous health ($H_{s,t-1}$)

and new health investments. β_2 represents the natural rate of health deterioration, and the other control variables ($HomeCare_{p,t}$, $X_{s,t}$, and $X_{p,t}$) represent LTC setting and various spouse- and user-level characteristics that affect the spouse's decision on health investments. Therefore, home care use may have an effect on the spouse's decision about health investments, if it directly changes physical or mental demands, or changes health-seeking behavior, which will affect health consequently (Coe & Van Houtven, 2009).

3.3. Methods

3.3.1. Data

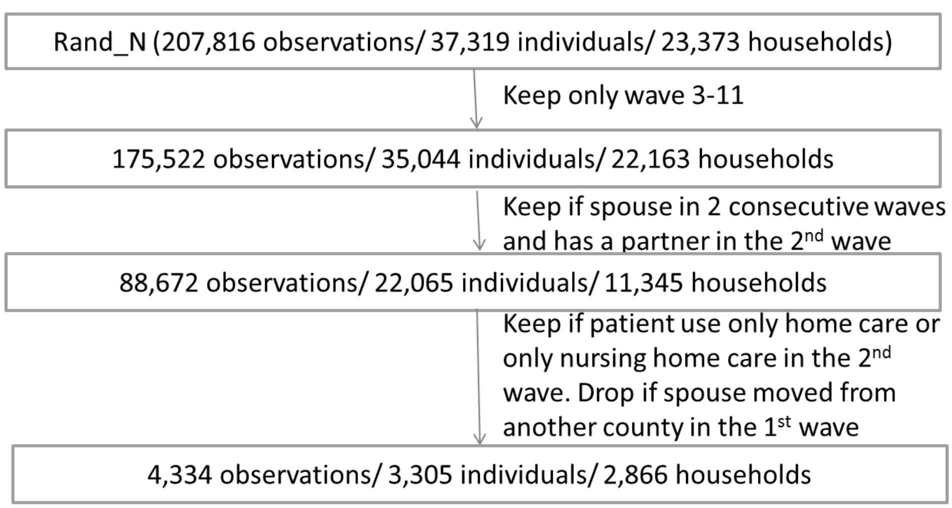
Our primary source of data is from the Health and Retirement Study (HRS), which is a longitudinal study that surveys a nationally representative sample of Americans over the age of 50 and their spouses since 1992. The HRS collects detailed information on respondents' socioeconomic status, health, health service utilization, healthcare expenditures, and family structure. Respondents are surveyed every 2 years until death, regardless of institution and cognitive function (proxy interviews may be used). The RAND Center for the Study of Aging developed a longitudinally consistent dataset from the original HRS dataset, including imputations of missing data (RAND Center for the Study of Aging, 2014). We use the RAND HRS data file (version N) when available. We also use the original HRS data and the Cross-Wave Geographic Information (detail) file to match respondents to their geographic information. In addition, we collect the county-level number of skilled nursing home beds per 1,000 elderly people aged 65+ (the instrumental variable) from the Area Health Resource File (AHRF). We use data from waves 3 to 11 (1996–2012) for our analyses because some key questions in waves 1 and 2 are inconsistent and subject to substantial measurement error.

3.3.2. Sample

We use observation-level, pooled sample from waves 3-11, and account for the multiple observations of the same individuals in the error structure in our regression analyses.

A schematic of our analysis sample is pictured in Figure 3.1. From the combined HRS sample spanning 1996–2012 (175,522 observations), we limit our sample for our primary analyses to spouses (1) if they are in 2 consecutive waves and have a partner in the later wave[3], (2) if the care recipients used only home care or only nursing home care in the later wave, and (3) if they lived in the same county for both waves. These exclusions enable us to focus on respondents who are less likely to move due to different availability of nursing homes to improve the validity of our instrument. Our final sample includes 4,334 observations of 3,305 spouses and 2,866 households. Most of the 3,305 spouses have a partner that used home care or nursing home services for 1 wave, only 531 (16 percent) of them have a partner that used LTC services for 2 or more waves.

Figure 3.1. Deriving the Analysis Sample



[3] More than 99 percent of spouses have a partner for both waves.

3.3.3. Variables

3.3.3.1. Dependent variables

We have two types of health outcomes: those that measure an elderly person's general physical health and those that measure general mental health.

Self-rated health. Respondents are asked to rate their general health as excellent, very good, good, fair, or poor. We use a dichotomous variable to indicate if the respondent has good or better self-rated health.

Need help with Activities of Daily Living (ADLs). Respondents are asked whether they need help with bathing, eating, dressing, walking across a room, and getting in or out of bed. We use a dichotomous variable to indicate if the respondent needs help with at least 1 ADL.

Need help with Instrumental Activities of Daily Living (IADLs). Respondents are asked whether they need help with using a telephone, taking medication, handling money, shopping, and preparing meals. We use a dichotomous variable to indicate if the respondent needs help with at least 1 IADL.

High blood pressure. We use a dichotomous variable to indicate if the respondent has reported the onset of high blood pressure or hypertension since last interview.

Heart problems. We use a dichotomous variable to indicate if the respondent has reported the onset of heart attack, coronary heart disease, angina, congestive heart failure, or other heart problems since last interview.

Stroke. We use a dichotomous variable to indicate if the respondent has reported the onset of stroke or transient ischemic attack (TIA) since last interview.

Psychiatric problems. We use a dichotomous variable to indicate if the respondent has reported the onset of emotional, nervous, or psychiatric problems since last interview.

CESD8 index. We use the well-established Center for Epidemiologic Studies Depression (CESD) scale to measure depression. It is the sum of 6 self-report “negative” measures (whether the respondent experienced depression, everything is an effort, sleep is restless, felt alone, felt sad, and could not get going, all or most of the time over the week prior to the interview) minus 2 “positive” measures (whether the respondent felt happy and enjoyed life, all or most of the time over the week prior to the interview). We use a dichotomous variable to indicate if the respondent has a score of 3 or above, which is consistent with probable clinical depression (Lewinsohn, Seeley, Roberts, & Allen, 1997; Radloff, 1977). Because CESD scale is a short self-report scale, it is useful and sensitive in screening for depression in adults.

3.3.3.2. Treatment variable

The HRS question for formal home care is “Since the previous interview, has any medically-trained person come to your home to help you?”. The HRS question for nursing home care is “Since the previous interview, have you been a patient overnight in a nursing home, convalescent home, or other long-term health care facility?”. We use a dichotomous variable to indicate whether the recipient has used home-based LTC since last interview. Spouses of recipients who used only home care are in the treatment group, and spouses of recipients who used only nursing home care are in the control group. We do not include spouses of recipients who used both services in our sample.

3.3.3.3. Control variables

Health stock. Wagstaff (1986) estimates a multiple indicator version of the structure and reduced form of the Grossman (1972) model. He uses 4 health indicators that reflect physical mobility, mental health, respiratory health, and the presence of pain to represent the unobserved stock of health. Adapted from his model and adjusted to fit our aging population, we use 4 health measures (at t-1) to represent the spouse's health stock. The 4 variables are: (1) a variable indicating the number of diagnosed chronic diseases (ranges between 0 and 8)[4], (2) a variable indicating the number of mobility tasks that the spouse had problem with (ranges between 0 and 5)[5], (3) a dichotomous variable indicating whether the spouse had been diagnosed with emotional, nervous, or psychiatric problems, and (4) a dichotomous variable indicating whether the spouse had problem with pain.

We also control for (or in some cases, stratify by) a rich set of spouse- and user-level variables available in the HRS that might be related to both LTC setting and the spouse's health outcomes. We use year and state fixed effects to account for general time trends in spousal health and unobserved time-invariant state characteristics that may affect spousal health outcomes.

3.3.4. Empirical strategy

3.3.4.1. Structural model setup

We begin by estimating the effects of care settings on spousal health outcomes using naïve logistic regression models.

$$logit(P(H_{st} = 1)) = \beta_0 + \beta_1 HomeCare_{p,t} + \beta_2 H_{s,t-1} + \beta_3 X_{s,t} + \beta_4 X_{p,t} + Year_t + State_{st} \qquad (2)$$

where $HomeCare_{p,t}$ is the treatment variable, indicating the LTC setting, $H_{s,t-1}$ is a vector of

[4] The 8 diseases are high blood pressure, diabetes, cancer, lung disease, heart disease, stroke, psychiatric problems, and arthritis.
[5] The 5 mobility tasks are walking several blocks, walking one block, walking across the room, climbing several flights of stairs, and climbing one flight of stairs.

health measures for spouse s at time t-1 (health stock), $X_{p,t}$ and $X_{s,t}$ are vectors of user- and spouse-level controls at time t. We also control for state and year fixed effects.

3.3.4.2. Instrumental variables design

The main concern with naïve logistic regression estimates is that care setting might be endogenous to recipient and spouse characteristics that also affect spousal health outcomes. For example, since home care usually requires more intensive informal care provision than nursing home care, spouses of home care users may have better initial health, which may affect his/her current health. Also, the health status and LTC needs of the care recipients may also be correlated with both the care setting and spousal health outcomes. Last but not least, household financial status may also affect LTC setting and spousal health outcomes. Therefore, failing to control for all potential confounders may result in selection bias, although it is unclear whether the estimates will be biased upward or downward since these unobserved confounders act in offsetting directions.

We use an instrumental variable approach to address this potential endogeneity issue. This approach uses one or more instruments, which affect the LTC settings but do not directly affect the spouses' health outcomes, to mimic a randomization of LTC users to different likelihoods of receiving home care. Therefore, this approach can identify balanced sources of variation in the treatment and control groups, and allows us to obtain unbiased treatment effects (McClellan et al., 1994). The ideal instrument(s) should predict treatment (home care versus nursing home care) but should not affect the spouse's health after the observed confounders are controlled for. Theoretically, if an IV balances both measured and unmeasured characteristics between treatment and control groups, then the selection bias can be minimized (Newhouse &

McClellan, 1998). Therefore, our instrument is based on the exogenous variation in LTC setting choice caused by county-level supply for skilled nursing home beds.

We use the county-level number of skilled nursing facility beds per 1,000 people aged 65+ as our IV. The nursing home care market option that a family faces has a direct impact on their demand for nursing home care and ability to get alternative care arrangements. Therefore, our instrument should be negatively correlated with the home care use by spouses. There is substantial variation in our IV: the county-level numbers of skilled nursing home beds per 1,000 people aged 65+ range between 0 and 142.5, with a mean of 42.9 and a standard deviation of 18.1. Regression results of the first-stage model are shown in Table 3.1. We find that a 1 unit increase in county-level skilled nursing home beds per 1,000 elderly people aged 65+ leads to a 0.2 percentage point reduction in likelihood of using HCBS, and a 1 standard deviation increase in the IV leads to a sizable 3.2 percentage points reduction in likelihood of using HCBS.

Table 3.1. First-stage estimates

	Home Care
Instrumental variable	-0.002***
	(0.001)
Spouse SES	
Age	0.004
	(0.007)
Age^2	-0.000
	(0.000)
Female	-0.023*
	(0.014)
Race (White is the reference)	
Black	0.016
	(0.020)
Other races	-0.041
	(0.039)
Hispanic	0.034
	(0.026)
Education (Less than high school is the reference)	
GED	0.025
	(0.035)
High school	0.021
	(0.018)
Some college	0.022
	(0.020)
College and above	0.007
	(0.023)
Retired	-0.019
	(0.015)
Household wealth	
Log total financial assets	0.003**
	(0.001)
Log total income	0.018***
	(0.006)
Family	
Number of children (0 is the reference)	
1	-0.004
	(0.037)
2	0.027
	(0.033)
3+	0.041
	(0.031)
Spouse health insurance	
Uninsured	-0.005
	(0.035)
Has LTCI	-0.031
	(0.019)

Table 3.1. First-stage estimates (continued)

	Home Care
Spouse health	
Lagged # diagnosed disorder	-0.001
	(0.005)
Lagged # mobility tasks cannot do	0.005
	(0.004)
Lagged any psychiatric problems	-0.025
	(0.018)
Lagged any pain problems	-0.022*
	(0.013)
Care recipeint health	
# diagnosed disorder	0.008*
	(0.005)
# mobility tasks cannot do	-0.031***
	(0.004)
Any psychiatric problems	-0.073***
	(0.017)
Any pain problems	0.053***
	(0.012)
Observations	4,334

* p<0.1; ** p<0.05; *** p<0.01. Standard errors in parentheses

Although, intuitively, our IV should be exogenous as it would be difficult to find a way in which spousal health would be affected by the market availability of nursing homes except through the care setting, there may be some threats to IV exogeneity. First, the IV would be endogenous if people actually move to counties with their preferred care settings, so people are not “randomly assigned” to different care settings based on where they live. Therefore, we exclude individuals who moved from a different county 2 years before they used LTC services to exclude those people, and we further exclude individuals who moved from a different county 4 years before they used LTC services in our sensitivity test models. Second, the IV would be endogenous if supply of nursing homes responds to demand. However, the supply of nursing homes is often constrained due to state moratoria on building and Certificate of Need regulation (Cimasi, 2005) and high entry costs [6]. We find that the average annual change in number of

[6] We use the supply of nursing home beds but not the supply of home health agencies as our IV, as the nursing home market is more stagnant.

nursing home beds is only 0.8 percent during our study period. Finally, the IV would be endogenous if it is correlated with county-level socioeconomic status variables (e.g., wealth and education) that may also affect spousal health. Therefore, we carefully control for a rich set of socioeconomic status variables, and we conduct the following test to see whether there may be such correlation.

Empirically, although we cannot directly test the exogeneity of our IV, we present an "IV balance check" table to assess whether treatment and control observations are plausibly balanced on observable characteristics, lending support to the idea that our instrument pseudo-randomizes observations. Table 3.2 shows the correlation coefficients and p values between the nursing home supply IV and each of the independent variables while controlling for the other independent variables, as the exogeneity requirement is conditional. While a few differences in observable characteristics for counties with more or fewer nursing home beds emerge as statistically significant (ethnicity; partner's mobility and pain), the differences in all the other observed characteristics are not statistically significant at the 0.10 level. These results show that the IV approach has greatly improved sample balance in observed characteristics/ confounders (compared to those shown in Table 3.3), providing support for the validity of the nursing home supply IV. These variables are also included as controls to account for any residual confounding.

Table 3.2. IV balance check

Independent variables	Correlation coefficients	P value
Spouse SES		
Age	0.030	0.881
Age^2	-0.000	0.821
Female	0.429	0.367
Race (White is the reference)		0.999
Black	0.002	
Other races	0.070	
Hispanic	-3.258	0.018
Education (Less than high school is the reference)		0.689
GED	1.716	
High school	-0.157	
Some college	-0.105	
College and above	-0.086	
Retired	-0.042	0.929
Household wealth		
Log total financial assets	-0.001	0.980
Log total income	0.335	0.118
Family		
Number of children (0 is the reference)		0.541
1	-0.195	
2	0.068	
3+	0.625	
Spouse health insurance		
Uninsured	0.868	0.439
Has LTCI	-0.834	0.165
Spouse health		
Lagged # diagnosed disorder	0.181	0.300
Lagged # mobility tasks cannot do	-0.083	0.597
Lagged any psychiatric problems	0.902	0.137
Lagged any pain problems	-0.655	0.156
Care recipient health		
# diagnosed disorder	0.034	0.829
# mobility tasks cannot do	0.336	0.008
Any psychiatric problems	-0.148	0.781
Any pain problems	-1.077	0.013

3.3.4.3. Instrumental variable models

Because we have binary dependent variables and a binary treatment variable, traditional two-stage least squares regression (2SLS) models might result in biased results. Therefore, we use two-stage residual inclusion (2SRI) methods (Terza et al., 2008).

Our first-stage, logistic regression model uses the IV to predict home care use:

$$logit(P(HomeCare_{pt} = 1)) = \alpha_0 + \alpha_1 IV_{pt} + \alpha_2 H_{s,t-1} + \alpha_3 X_{s,t} + \alpha_4 X_{p,t} + Year_t + State_{st} \qquad (3)$$

where IV_{pt} is the county-level number nursing home beds per 1,000 people 65+ for LTC user p at time t, the other variables are the same as described in equation (2). We then calculate the response residuals $\widehat{r_{it}}$ from the first-stage models, which are the difference between the predicted probabilities and observed uses of home care services. These residuals are included in the second-stage models below as additional regressors to produce the correct adjustment for the endogeneity in the outcome equations:

$$logit(P(H_{st} = 1)) = \beta_0 + \beta_1 HomeCare_{p,t} + \beta_2 H_{s,t-1} + \beta_3 X_{s,t} + \beta_4 X_{p,t} + Year_t + State_{st} + \widehat{r_{st}} \qquad (4)$$

where H_{st} is a health outcome, and the other variables are the same as described above. We use the second-stage model to estimate the causal impact of spousal home care use on individual's health outcome. In all of our regression models, we estimate Huber-White standard errors to account for multiple observations per individual. Finally, we perform a bootstrap procedure for both stages, with 500 iterations to correct standard errors (Efron, 1981).[7]

Our main models estimate the effects of home care use at time t on the spouse's health outcomes at time t across the sample. We also construct numerous alternative specifications to test the robustness of our results. First, we examine longer-run effects and measure health

[7] In our bootstrap process, we resample at the observation level. Our results are robust to resampling at the individual and household levels, which might approximate the real data generating/sampling mechanism better.

outcomes 2 years later. Second, we test the robustness of IV estimates excluding people who moved from another county 4 years ago (instead of 2 years ago as in our main models). Third, we exclude nursing home use due to post-acute rehabilitative purpose to compare care recipients who received LTC mainly for chronic care purpose. However, HRS questions do not distinguish the 2 types of care. In practice, because Medicare covers rehabilitation of up to 100 days in a nursing home with a substantial copayment after the 20th days, most nursing home stays for rehabilitation purpose are shorter than 30 days (Konetzka, Park, Ellis, & Abbo, 2013). Therefore, in our sensitivity analysis, we exclude spouses of care recipients who used nursing home services for less than 30 days.

3.4. Results

3.4.1. Descriptive statistics

Tables 3.3 and 3.4 provide descriptive statistics of our sample. There are significant differences between spouses of home care users and spouses of nursing home users. On average, spouses of home care users are younger, are more educated, are less likely to be retired, have more income, have more children, are less likely to have LTCI or any other health insurance, have better lagged health, and the LTC users are also healthier. The many differences in observed variables suggest the existence of self-selection and the necessity of using the IV approach.

Table 3.3. Descriptive statistics of independent variables, by LTC setting

Independent variables	Home Care=0 (N=781)	Home Care=1 (N=3,553)	P value
Spouse SES			
Age (Mean)	75.7(10.0)	69.7(10.2)	<0.001
Female (%)	51.6	54.3	0.163
Race (%)			0.383
White	83.5	82.2	
Black	11.9	13.7	
Other races	4.6	4.1	
Hispanic (%)	8.3	9.3	0.396
Education (%)			0.001
Less than HS	32.0	25.1	
GED	3.6	4.5	
High school	31.4	31.5	
Some college	17.9	20.7	
College and above	15.1	18.2	
Retired (%)	69.5	55.9	<0.001
Household wealth			
Log total financial assets ($)	7.6(5.0)	7.7(5.0)	0.444
Log total income ($)	10.4(1.1)	10.7(1.0)	<0.001
Family			
Number of children (%)			0.002
0	5.5	3.5	
1	10.8	8.1	
2	25.1	24.9	
3+	58.6	63.5	
Spouse health insurance			
Uninsured (%)	2.3	4.3	0.009
Has LTCI (%)	15.0	11.7	0.011
Spouse health			
Lagged # diagnosed disorder (Mean)	2.3(1.5)	2.0(1.4)	<0.001
Lagged # mobility tasks cannot do (Mean)	1.4(1.6)	1.1(1.5)	<0.001
Lagged any psychiatric problems (%)	21.3	17.5	0.012
Lagged any pain problems (%)	37.8	34.4	0.078
Care recipient health			
# diagnosed disorder (Mean)	3.1(1.5)	3.0(1.5)	0.001
# mobility tasks cannot do (Mean)	3.0(1.9)	2.2(1.8)	<0.001
Any psychiatric problems (%)	32.0	22.6	<0.001
Any pain problems (%)	42.8	48.0	0.007

The comparisons between the two treatment arms are calculated based on simple two-sample t-tests or chi-squared test.
Standard deviations in parentheses.

Table 3.4. Descriptive statistics of dependent variables, by LTC setting

Dependent variables	Home Care=0 (N=781)	Home Care=1 (N=3,553)	Total (N=4,334)
Good Health (%)	62.3	68.3	67.2
Any ADLs (%)	27.1	19.1	20.6
Any IADLs (%)	28.7	17.7	19.7
Hypertension (%)	6.3	4.7	5.0
Heart Problems (%)	4.4	3.4	3.6
Stroke (%)	2.8	1.4	1.7
Psychiatric Problems (%)	4.2	2.6	2.9
CESD>=3 (%)	35.9	24.9	26.8

3.4.2. Regression results

Table 3.5 and 3.6 show regression results of home care use (vs nursing home use) on spousal health outcomes: (1) self-rated good or better health, (2) need help with any ADLs, (3) need help with any IADLs, (4) has reported the onset of high blood pressure since last interview, (5) has reported the onset of heart problems since last interview, (6) has reported the onset of stroke since last interview, (7) has reported the onset of psychiatric problems since last interview, and (8) has a CESD score of 3 or more. Results of our primary specifications are displayed in Table 3.5. For each outcome, we show results from the naïve logistic regression models and from the IV models. IV estimates of sensitivity test models are shown in Table 3.6. For all models, we present marginal effects of home care use, bootstrapped standard errors, first-stage F statistics, mean values of the dependent variables, and numbers of observations (full first- and second-stage results are available on request).

Table 3.5. Estimates of marginal effects, base models

Model	(1) Good Health	(2) Any ADLs	(3) Any IADLs	(4) High blood pressure	(5) Heart Problems	(6) Stroke	(7) Psychiatric Problems	(8) CESD>=3
No IV models								
Marginal effect	-0.008	0.004	-0.018	-0.011	-0.003	-0.005	-0.006	-0.068
S.E.	(0.017)	(0.014)	(0.014)	(0.009)	(0.007)	(0.006)	(0.007)	(0.018)***
Observations	4,399	4,400	4,384	4,352	4,238	3,527	4,075	3,952
IV models								
Marginal effect	-0.092*	0.068	0.032	0.029	-0.032	0.019	-0.022	-0.123**
Bootstrap S.E.	(0.048)	(0.043)	(0.041)	(0.028)	(0.027)	(0.021)	(0.028)	(0.055)
First-stage F statistics	10.6	10.2	10.2	10.1	10.1	10.1	10.4	8.6
Mean of dependent variable	0.672	0.206	0.197	0.050	0.036	0.017	0.029	0.268
Observations	4,327	4,330	4,330	4,312	4,204	3,519	4,029	3,899

* p<0.1; ** p<0.05; *** p<0.01. Standard errors in parentheses.

Table 3.6. Estimates of marginal effects, sensitivity test models

Model	(1) Good Health	(2) Any ADLs	(3) Any IADLs	(4) High blood pressure	(5) Heart Problems	(6) Stroke	(7) Psychiatric Problems	(8) CESD>=3
t+1 models								
Marginal effect	-0.006	0.085*	0.113**	-0.006	-0.005	0.018	0.025	-0.045
Bootstrap S.E.	(0.063)	(0.048)	(0.047)	(0.034)	(0.038)	(0.029)	(0.024)	(0.065)
First-stage F statistics	12.5	12.2	12.2	12.3	12.0	12.2	12.3	9.1
Mean of dependent variable	0.681	0.204	0.193	0.048	0.039	0.021	0.027	0.272
Observations	3,220	3,219	3,217	3,111	3,072	2,650	3,034	2,920
Dropped if moved 4 years ago								
Marginal effect	-0.043	0.052	0.044	0.010	-0.020	0.031	-0.036	-0.068
Bootstrap S.E.	(0.056)	(0.050)	(0.047)	(0.029)	(0.030)	(0.027)	(0.035)	(0.059)
First-stage F statistics	16.7	16.2	16.2	16.1	16.0	16.1	16.4	13.4
Mean of dependent variable	0.675	0.210	0.201	0.049	0.039	0.018	0.029	0.256
Observations	3,482	3,484	3,481	3,420	3,387	2,699	3,227	3,107
NH>30 days								
Marginal effect	-0.076	0.037	0.021	0.024	-0.001	0.015	-0.029	-0.138***
Bootstrap S.E.	(0.046)	(0.041)	(0.036)	(0.028)	(0.021)	(0.019)	(0.031)	(0.052)
First-stage F statistics	10.2	10.1	10.1	10.0	10.0	9.9	10.1	9.3
Mean of dependent variable	0.672	0.207	0.195	0.049	0.037	0.017	0.029	0.269
Observations	3,989	3,993	3,991	3,898	3,885	3,095	3,032	3,604

* p<0.1; ** p<0.05; *** p<0.01. Standard errors in parentheses.

We expect spouses of home care users to have worse physical health outcomes due to potentially greater informal caregiving responsibility. In line with our hypothesis, we find that home care use leads to a 9.2 percentage points reduction in the likelihood of having good or better self-rated health, a 6.8 percentage points increase in the likelihood of needing help with ADLs, a 3.2 percentage points increase in the likelihood of needing help with IADLs, a 2.9 percentage points increase in the likelihood of having had hypertension since last interview, a 3.2 percentage points reduction in the likelihood of having had heart problems since last interview, and a 1.9 percentage points increase in the likelihood of having had stroke since last interview. Although only the effect on self-rated health is statistically significant, the other effects are large in magnitude when compared to the means of dependent variables. There are some potential explanations for the lack of statistical significance: (1) some health outcomes are rare, and we have limited power to detect differences in outcomes given the small sample size, and (2) we only examine the effects of 1 wave of home care use, which may be too short to significantly cause chronic diseases. Our t+1 model estimates show that most of these negative physical health effects do not persist in the longer-run and become much smaller. But the negative effects on ADLs and IADLs become larger and more significant at t+1, suggesting long-run/accumulation effects in functional disability. The IV estimates of the 2 sensitivity test models (exclude spouses who moved 4 years ago, and exclude spouses of care recipients who used nursing home services for less than 30 days) are generally consistent with those of our base models in their direction and magnitude. Overall, our results show that home care use leads to mostly insignificant, yet consistently negative effects on spousal physical health, especially in functional status in the long-term. These effects may potentially be caused by increased informal care responsibilities. Our findings suggest that although short-term home health use may not significantly negatively affect some common spousal physical health measures, it would not protect spousal physical health and may have an accumulation effect on their functional ability.

On the other hand, we find that home care use leads to a 2.2 percentage points insignificant reduction in the likelihood of having had diagnosed emotional, nervous, or psychiatric problems since last interview but a 12.3 percentage points significant reduction in having a CESD score of 3 or more (an indicator for depression). The effect on spousal CESD score is stronger. Some potential explanations include: (1) the CESD is a screening test for depression and depressive disorder and is designed to be more sensitive, and (2) psychiatric disorders are usually underdiagnosed, especially among informal caregivers. In the longer-run models, home care use still leads to a lower likelihood of having a high CESD score, but a higher likelihood of having diagnosed emotional, nervous, or psychiatric problems. It is unclear why the direction of the effect on psychiatric outcome changed 2 years later. But since psychiatric problems are usually underdiagnosed and the outcome is rare, the results may be noisy. The estimates of the other 2 sensitivity test models are consistent with those of the base models. It is worth mentioning that a high CESD score is different from clinical diagnosis of depression. According to Radloff (1977), the CESD scale is designed to measure current levels of depressive symptomatology in the general population. The symptoms in the scale are among those on which a clinical diagnosis of depression is based, but other diagnoses may also be necessary to determine the disease. Interpretations of individual CESD scores should not be made, and group average scores should be interpreted in terms of the level of symptoms which accompany depression but not rates of depression. Overall, we find home care use leads to better mental health, especially in depression symptoms.

3.5. Conclusions

Although there has been a large expansion in noninstitutional LTC during the past several decades, no HCBS cost-effectiveness research has focused on health or well-being of the spouses. In this paper, we estimate the causal effects of home care use on spousal key physical and mental health outcomes. Our findings show that home care use leads to mostly insignificant, yet consistently negative effects on spousal physical health, especially in functional status in long-term, which may potentially be caused by increased informal care responsibilities. We also find improved spousal mental health outcomes, especially in depression symptoms, which may potentially be caused by increased satisfaction derived from providing more intensive informal care, from enabling care recipients to stay in their preferred LTC setting, or from living together with their partners. Although the estimated effects of care setting on spousal health are limited, these results are less surprising given the fact that 84 percent of the care recipients used home care or nursing home services for only 1 wave.

Our results are subject to several potential limitations, but we employ various strategies in each case to address or minimize them to the extent possible. First, our sample is limited to individuals/families with moderate wealth, and we can estimate only local average treatment effects (LATEs) for subpopulation groups that are induced by our instruments to change their LTC settings (i.e., the compliers) (Imbens & Angrist, 1994). That is, we can only estimate the treatment effects among spouses of care recipients who used nursing home services because there is more nursing home supply and who would have used home care if otherwise. However, making these sample restrictions and using the IV approach are necessary, and it allows us to focus on seniors who do not have a strong preference on care settings (i.e., the always-takers and the never-takers), and are therefore more likely to respond to programs/incentives that aim to expand noninstitutional LTC. Second, due to small sample size, we are not able to study the spousal health outcomes by care recipients' diseases and LTC needs. It is possible that HCBS use by recipients with different physical and cognitive impairments may affect spousal health differently. Third, the commonly used health measures that are available in HRS may not be sensitive enough to capture the small effects of short-term home health use. Future studies are needed to examine the effects of continued home health use on spousal health outcomes. Last but not least, our results in t+1 models may be biased if spouses of home health users have a different likelihood of withdrawing from the study at t+1 due to death (i.e., attrition bias). We find that the death related withdrawal rate is 5.9 percent for spouses of home health users and 11.3 percent for spouses of nursing home users. However, we find no statistical difference in death related withdrawal rate between control and treatment groups when age and preexisting conditions are controlled for. Therefore, the difference in spouse withdrawal rates does not appear to be caused by use of home care. Since we control for age and previous health of the spouses, our results should not subject to attrition bias.

Overall, our findings on the effect of HCBS on spousal health outcomes have several key implications. Our findings on spousal health outcomes are important in estimating the cost and effectiveness of HCBS expansion. Future studies should examine the health service utilization and medical costs for spouses of home health users. Finally, HCBS programs should consider support for spouses and other family members of care recipients, focusing on their physical health and ways to avoid declining functional status.

Chapter 4 Unintended Consequence of China's One-Child Policy: Effects on Long-Term Care

4.1. Introduction

China's population is aging, creating growing needs for services for the elderly. During the past 40 years, China has shifted from a comparatively young society to an aging society due to demographic shifts and an epidemiological transition. In 2010, 13 percent of the total population was elderly people aged 60 years or over, and 19 percent of the elderly people needed assistance with daily activities (M. Li et al., 2013; National Bureau of Statistics of China, 2011). These numbers are projected to grow rapidly in the next few decades due to the aging of China's baby boomers of the 1950s and 1960s[1].

These trends present new challenges for China's Long-Term Care (LTC) system. On the one hand, most LTC has long been provided informally by family members and friends of care recipients in China. Influenced by Confucian filial norms (Chinese: Xiao), adult children, especially the sons and the daughters-in-laws, are expected to live with the parents and take care of them (M. Li et al., 2013). However, this practice has come under pressure due to the emergence of three new social phenomena that have greatly reduced the supply of family caregivers. First, the one-child family planning policy (OCP) of 1979, which allowed most families to have only one child, led to a sharp fertility drop. The old age dependency ratio[2] has increased from 0.09 in 1980 to 0.11 in 2010, and is projected to reach 0.49 in 2060 (United Nations Department of Economic and Social Affairs, 2013). As a result, in many families, one

[1] The total fertility rate (TFR) in China was 5.8 in 1950. It declined to 3.3 in 1961 due to the Great Chinese Famine, and then peaked at 7.5 in 1963 (Zeng, 2008). The total population had risen from 540 million in 1950 to 850 million in 1970 (Zhu, 2003).

[2] The ratio of the total number of people aged 65 years and older to the number of people aged between 15 and 64 years.

adult child is responsible for taking care of two parents and four grandparents (as known as the "4-2-1" problem). Second, the explosive growth of labor migration also challenges long-standing caregiving and living arrangements. It is estimated that 160 million workers (about 35 percent of total working-age rural population) left their rural homes for urban jobs in 2012 (Cai & Wang, 2015). As a result, many elderly people in rural areas become "empty nesters" and lack LTC resources (Chen, Jin, & Yue, 2010). Last but not least, increases in the female employment rate further reduce the availability of family caregivers (Zhang, 2007).

On the other hand, compared with industrialized countries that experienced more gradual development and were wealthier when their populations aged, the social insurance, social assistance and formal LTC systems in China remain underdeveloped. As a result, although the former family-oriented LTC organization is not as sustainable as it used to be, it will continue to be the primary means of old-age support over the next few decades (Glass et al., 2013). These new challenges create a great caregiving burden for adult children and potential risk for parents—parents whose single child is unable to care for them may face a lack of support in old age.[3]

Partly in response to concerns about meeting the demand for old-age support and an imbalanced sex ratio[4], China has gradually relaxed the OCP since the early 1980s to allow certain families to have two children (e.g. if both parents are single children themselves or if the first-born is a daughter in the rural areas of some provinces) (Glass et al., 2013; Hesketh & Zhu,

[3] They also create a problem called "shidu", which refers to parents who have lost their only child and are unable to have another child due to age or other reasons. Currently, there are about one million "shidu" families in China. "Shidu" parents suffer mentally and physically, and losing their only child means there may be no one to care for them when they are old (Song, 2014).

[4] In the early 1980s, when the OCP was at its most stringent, the sex ratio at birth was almost 120 boys to 100 girls. Empirical studies suggest that due to son preference, the OCP indirectly leads to issues such as sex-selective abortion, female infanticide and unreported female births, especially in rural China.

1997; Hvistendahl, 2010). This relaxation gained pace in recent years as the first generation whose childbearing years began during the period of the OCP gradually moved into their elderly years and aging-related family and social problems became urgent. In January 2016, the 37-year-old OCP was finally replaced by the new two-child-policy (TCP), which allows all families to have two children. It is estimated that the TCP would yield a total fertility rate of 1.7 over the next two decades, which would help to normalize the sex ratio and mitigate the "4-2-1" problem (Greenhalgh & Bongaarts, 1992; Hesketh, Lu, & Xing, 2005; Song, 2014).

Many policymakers expect that two children can share the caregiving burden, boosting the supply of informal LTC and providing elderly parents with greater old-age support.
While this explanation may seem intuitively appealing, we know surprisingly little about the actual consequences of having more children on LTC support in modern China. Prior studies on family size in China mainly focus on its impact on living arrangements and financial support from children (Chou, 2009; M. Guo, Chi, & Silverstein, 2009; Y. J. Lee & Xiao, 1998; Lei, Strauss, Tian, & Zhao, 2011; Logan & Bian, 2003; Ren et al., 2015; Zimmer & Kwong, 2003). However, due to the lack of efficient formal LTC system in many areas, financial support from children alone may not solve the old-age support issues, and a very different form of investment – direct provision of informal care by adult children – may be necessary (Gruijters, 2015). The few studies on the relationship between family size and LTC support in China find mixed results (M. Guo et al., 2009; Ren et al., 2015; Zimmer & Kwong, 2003). Two reasons may underlie the lack of consistent estimates: first, these studies do not account for the potential endogeneity of family size, and do not fully account for differences in parents who have more children versus fewer children that may affect their likelihood of receiving care, so they may not identify the true causal effects of family size. Second, the data used in these studies are outdated and/or not

nationally representative. Because the social and demographic structure in China is very different across regions and has changed a lot over the past several decades, their data may lack relevance to contemporary China. Other research has studied the effects of number of children on old-age support in other Asian countries and share the same endogeneity problems (Knodel, Chayovan, & Siriboon, 1992; Knodel, Friedman, Anh, & Cuong, 2000; Lillard & Willis, 1997). But more fundamentally, the effects of family size on LTC support are largely dependent on cultural context, and the level of development of social support systems. Only recently has the aging of parents subject to the one-child policy created significant need for long-term care, and has nationally representative data become available to study this question in contemporary China.

In addition, although many seniors no longer depend solely on their children for LTC due to changes in family structure and living arrangements, few studies have examined and emphasized the roles of other potential caregivers in modern China, potentially due to the perceived primacy of adult children as caregivers (Gruijters, 2015). Family size may not meaningfully affect the receipt of LTC by elderly adults if other caregivers may serve as substitutes for adult children. One of the few recent empirical studies on alternative caregivers suggests that married individuals are mostly cared for by their spouse, even if they live with their adult children (Gruijters, 2015). Therefore, it is important to understand whether family size may also affect the use of LTC provided by other caregivers, and who they are.

To inform policymakers about the potential impact of the new TCP on informal old-age support, we examine the causal impact of having additional children on receiving LTC support using recent, nationally representative survey data. By focusing on individuals who need help with daily activities, our study also informs policymakers about the potentially unmet demand for formal LTC. To the best of our knowledge, this is the first study to address this question in modern China. Specifically, we study the following research questions: (1) Does having more children (compared to having one child) increase one's probability of receiving LTC from his children? (2) Does having more children (compared to having one child) affect one's overall probability of receiving LTC? Who are these alternative caregivers?

One big challenge of identifying the causal relationship is addressing the potential endogeneity of family size. Since, in most cases, parents decide the number of children they want, it might be subject to selection bias. That is, the fertility level is chosen endogenously by parents and may, therefore, be correlated with parental characteristics that may also affect their likelihood of receiving assistance from children later in life. For example, parents who live in a society that values filial piety may have a stronger preference for informal care from children and may have more children. Their children may be more likely to provide informal care if they are also influenced by the filial piety tradition.[5] Failure to control for these confounders may lead to biased estimates. We address this concern by using an instrumental variable (IV) approach, which mimics the random assignment process and leads to plausibly unbiased estimates (Angrist et al., 1996). Adapted from Qian (2009) and Islam & Smyth (2014), our instrument is based on the exogenous variation in family size caused by the OCP in 1979 and sex of the first-born.

We find that compared to parents with only one child, parents with two or more children are more likely to receive care from their adult children. This effect is larger among rural residents but insignificant among urban residents. However, having more children does not increase the parents' overall probability of receiving care— care from spouses largely substitutes

[5] Alternatively, parents who have more children are more likely to have lower socioeconomic status, and may therefore be less likely to use *inter vivos* transfers and bequests to invoke attention and care from children (Bernheim, Shleifer, & Summers, 1985). Also, parents who have more children are less likely to have extremely poor health when they were young (otherwise they might not be able to have many children), and are also less likely to have extremely poor health when they are old and need less intensive care.

for care from adult children. Our results suggest that the new TCP may not necessarily lead to an increase in informal care supply, but may transfer the caregiving burden from spouses to adult children. The end of the OCP will likely not reduce the need for further investment in developing the formal LTC sector in China.

Our paper proceeds as follows. In Section 2, we discuss the background and conceptual framework. In Section 3, we discuss the data, sample selection criteria, measures, and estimation strategy. Section 4 presents the main results and Section 5 presents conclusions.

4.2. Background and conceptual framework

4.2.1 Background

4.2.1.1. Family planning policies in China

In the 1970s, after two decades of rapid population growth, policymakers in China employed various population policies to curb its population growth and alleviate its social, economic, and environmental problems. In 1972, China introduced the so-called "late, long, and few" policy (Chinese: Wan, Xi, Shao), which encouraged (but did not mandate) later childbearing, longer spacing, and fewer children. It also offered economic incentives to parents who space the birth of their children at least 4 years apart (Zhu, 2003). However, the fall in the total fertility rate was not significant enough, and the population continued to grow rapidly. Therefore, in 1979, China replaced the voluntary "late, long, and few" policy with the mandatory OCP, which applied to individuals of Han ethnicity, who comprise 92 percent of the total population. The policy was enforced at the provincial and county level through fines, forced abortion and other types of punishments (e.g. violators may lose their jobs) (Banister, 1987; Greenlaugh, 1986). Since the policy was enforced at the provincial level, some provinces had relaxed the restrictions and allowed certain groups to be exempted from the policy since the early 1980s. For example, in some rural areas, couples were allowed to have a second child if their first child was a girl or was disabled or if both of them were themselves single children. In 1984, the central government officially granted local governments the flexibility to make exemptions and allowed all families nationwide to have a second child if their first child was disabled or if both parents were single children (Greenlaugh, 1986). As of 2010, there were 22 exceptions qualifying a couple for more children, but 63 percent of all couples were still restricted to the OCP (Hvistendahl, 2010). Recently, as the parents of the first generation of law-enforced only-children became aged, some population aging-related issues began to be seen as more urgent, and China further relaxed its population policy. In November 2013, the central government allowed families to have two children if one parent is an only child. Finally, starting in January 2016, all couples are allowed to have two children under the new TCP. Many policymakers expect that two children may share the caregiving burden, and elderly parents will have greater old-age support.

4.2.1.2. China's formal old-age support system

The social insurance, social assistance and formal LTC systems in China remain underdeveloped. First, although China's public health insurance schemes cover more than 90 percent of the rural and urban population, the average reimbursement rate is less than 40 percent of costs (Hou & Li, 2011), and there is no insurance coverage for LTC services[6]. Second, there are great disparities in the social security system and public safety net system. The pension system covers less than 8 percent of the rural population[7]. Although pension coverage is much

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[6] The private insurance market in China is very small.

[7] Usually, health insurance and pension are based on contributions of both employees and employers. Therefore, social insurance is hard to extend to rural residents, who are mostly self-employed agricultural workers with no stable income (Pei & Tang, 2012).

higher for urban employees, a large number of former state-owned enterprises employees who were laid off during the process of privatization in the 1990s still suffer from pension arrears(Cai, Giles, & Meng, 2006). Third, the formal LTC infrastructure is poor. As of 2012, there were about 42,000 nursing homes[8] with 3.65 million beds nationwide (Liu & Sun, 2014), much less than the demand for nursing home care, which has been estimated at 8 million beds (Glass et al., 2013). Since most of these nursing homes are in big cities such as Beijing and Shanghai, shortages are even greater in small cities and rural areas. In addition, most of these nursing homes have insufficient resources: less than 60 percent of them have medical treatment rooms, and only 30 percent of caregivers have received the professional training required to properly provide LTC (Liu & Sun, 2014). Although rising LTC demand and purchasing power in China have created incentives and opportunities for several US-based firms and private equity funds to invest in high-end western-style nursing home facilities in big cities, issues such as unaffordable service costs, cultural incongruity, and an uncertain regulatory environment may limit the development of this private nursing home industry (Feng, Liu, Guan, & Mor, 2012). In addition, public programs that support home- and community-based services (HCBS) are rare and largely limited to urban areas (Feng et al., 2012).

4.2.1.3. Differences in sources of old-age support for the urban and rural elderly

The elderly people in rural areas are more vulnerable and have fewer sources of old-age support than their counterparts in urban areas. As we discussed above, elderly people in rural areas have worse access to government pensions and formal LTC services. As a result, they depend much

[8] Nursing homes in China range from board-and-care homes providing little professional and medical care to modern nursing homes with skilled personnel (Feng, Liu, Guan, & Mor, 2012).

more on their own labor income and family support for old-age care and financial security. Elderly people in rural areas have longer working lives[9] and save a substantial share of income across their life cycle. However, due to disparities in socio-economic development, the rural elderly have much higher poverty rates than urban seniors, especially in the 71-80 year old age group. Although there has been substantial rural-to-urban migration since the 1990s and having migrant children has a positive effect on senior parents' financial well-being[10], migration also has changed living arrangement and informal care pattern in rural China. The coresidence rate of rural elderly with adult children has dropped dramatically, from 70 percent in 1991 to 40 percent in 2006 (Cai, Giles, O'Keefe, & Wang, 2012).

4.2.1.4. How does one's fertility affect one's probability of receiving LTC?

According to Caldwell (1978), "descendants are the most valuable protection that a couple can have against destitution in old age" in many societies. Indeed, the desire to receive old-age support from children is one of the main motivations for childbearing (Caldwell, 1976; Cunningham, Yount, Engelman, & Agree, 2013). This logic suggests that the decline in fertility in China would reduce the availability of caregivers for both children and elderly parents (Glass et al., 2013; Liu & Sun, 2014; Wong & Leung, 2012; Zhan, 2004). However, this may not necessarily be the case. For one thing, as family size increases, each child receives fewer material resources, opportunities, and parental attention and interventions that may be critical to their intellectual development (Keister, 2003). The impact of these diluted investments is

[9] Younger rural seniors depend more on their own labor income, and most of them work in agriculture related fields. However, even among seniors aged between 70 and 75, nearly a quarter of them report labor income as main source of income (*Chinese Labor and Social Security Yearbook 2006*, 2007).
[10] Rural households with migrant children are less likely to have incomes below the poverty line or become poor because of health shocks (Cai, Giles, O'Keefe, & Wang, 2012).

evidenced both immediately and over time— literature has documented the negative effects of sibling size on resources, education attainment, health and wealth in both childhood and adulthood (Blake, 1981; Downey, 1995; Keister, 2003; Rosenzweig & Zhang, 2009; Sandefur & Wells, 1999; Teachman, 1987). As a result, the resources and care that these children are able to provide to their parents may be reduced. For another thing, many studies find that, in China, intergenerational support tends to center on parents' needs. These findings are also consistent with Becker's (1974, 1992) altruism model (Hermalin, 2002; Y. J. Lee & Xiao, 1998; Y.-J. Lee, Parish, & Willis, 1994), which suggests that an altruistic child maximizes his own utility in part by maximizing his parents'. Therefore, children provide support to their parents mainly based on the need of the parents. If most children value their parents' health and LTC needs greatly, parents with only one child would also receive appropriate care, and having two or more children may not necessarily lead to significantly greater support from children (Chou, 2009). This model is also consistent with the Chinese traditional philosophy of filial piety, which regards caring for elderly parents as a moral obligation (Lang, 1946). In addition, adult children are legally obliged to support and assist their parents under the Marriage Law ("The Marriage Law of the People's Republic of China (1980)," 1984). These official policies also enhance parents' access to informal care regardless of the family size. Ultimately, whether parents with more children are more likely to receive informal care from their children must be determined empirically.

4.2.1.5. Previous studies

Our study is closely related to literature that examines the relationship between family size and children's support for elderly parents in China. These studies find inconsistent results. For example, using the 1992 survey of the support for the elderly in rural and urban China, Zimmer

& Kwong (2003) find that having more children increases one's probability of receiving both LTC and financial support from the children. Similarly, using data from the China Survey on Support Systems for the Elderly in 1992, Lee & Xiao (1998) find that family size is positively correlated with the incidence and amount of financial support from children, and financial transfers from children compensate for inequality in access to public resources in both urban and rural China. The study by Lei et al. (2011) uses 2008 China Health and Retirement Longitudinal Study (CHARLS) pilot data and finds that having more children is associated with a higher likelihood of living with a child or having a child nearby and higher likelihood of receiving financial transfers from children. On the other hand, using 2005 survey data from 619 elderly people in Dujiangyan city, China, Ren et al. (2015) find that although having more children is positively correlated with one's probability of receiving financial support from children, it is not significantly correlated with one's probability of receiving physical or emotional care from children. Further, using longitudinal survey data from 1,006 older Chinese people in the rural areas in Anhui province, China, (M. Guo et al., 2009) find that parents with declining income are more likely to receive financial support from children, and parents experiencing widowhood and difficulties with instrumental activities of daily living are more likely to receive LTC support from children. However, family size is not significantly correlated with one's probability of receiving LTC support from children. All of these studies fail to account for the endogeneity of family size, and the data used in these studies are outdated and/or not nationally representative. Therefore, the lack of consistent results may be due to selection bias and limited external validity.

Taken together, our study extends the literature by assessing the causal impact of family size on parents' old-age support, and by using recent nationally representative data with a focus on seniors who are in need of care in both rural and urban areas for the first time. We control for the potential endogeneity of family size using an IV approach, which plausibly leads to unbiased causal inferences.

4.2.2. Conceptual framework

We use a modified Anderson behavioral model to determine the use of informal LTC services (Andersen, 1968). The Anderson model was first developed to analyze families' aggregate use of health service because the medical care that an individual receives is a function of the demographic, social and economic characteristics of the family as a unit (Andersen, 1995). However, due to the heterogeneity of family members the model then focuses on individuals as the unit of analysis, with important family characteristics attached. It is one of the most widely used frameworks to predict health care utilization.

The Anderson model posits that the amount of health services used by an individual is a function of the predisposing and enabling characteristics of the individual and his need for medical care. Specifically, the predisposing variables include those that describe the propensity of an individual to use the services (e.g. age/sex/race of the individual and other family members, social class, and health beliefs); the enabling variables include resources that an individual has available to him/her for the use of services (e.g. income, health insurance, and physician density); and the health care need refers to health status or illness (e.g. major illness, symptoms, and disabilities), which is the most direct and important cause of health service use. In most cases, family size is considered as an exogenous predisposing factor in the Anderson model. However, in our context, we consider children as a source of old-age support, and consider family size to be an enabling factor. Therefore, to study how family size affects receiving LTC from adult children, we control for a rich set of predisposing, enabling and health care need variables that is available in our data and fits the context of modern China.

4.3. Methods

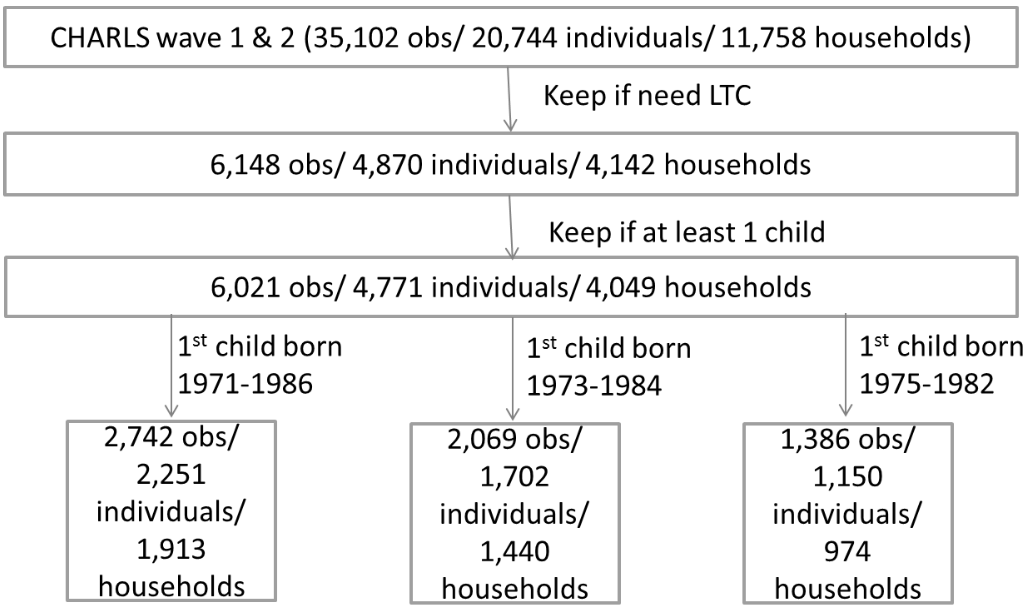
4.3.1. Data

We use data from the China Health and Retirement Longitudinal Study (CHARLS), which is the first publicly available nationally representative data on elderly parents in China (Smith, Shen, Strauss, Yang, & Zhao, 2010). It resembles the U.S. Health and Retirement Study (HRS) and surveys non-institutionalized individuals aged 45 years or over and their spouses biennially since 2011. CHARLS covers individuals from 150 counties in 28 out of 30 provinces in China. It collects information on individuals' socioeconomic status, health and functioning, health service utilization, health insurance, consumption, and family structure (Zhao et al., 2013). We use both waves of the CHARLS data, 2011 and 2013.

4.3.2. Sample

We use observation-level, pooled sample from two waves, and account for multiple observations of the same individuals in the error structure in our regression analyses. A schematic of our analysis sample is pictured in Figure 4.1. From the combined CHARLS sample spanning wave 1 & 2 (35,102 observations), we limit our sample for our primary analyses to those observations from individuals who have problems with at least one activity of daily living (ADL), instrumental activities of daily living (IADL) or cognitive function, and have at least one child. These exclusions enable us to focus on respondents who are more likely to need LTC. To further reduce the heterogeneity among the parents and strengthen our IV validity, we only include individuals who have their first child between 1975 and 1982 (i.e., 4 years before or after the OCP) in our base model[11]. Our final sample includes 1,386 observations. We include individuals who have their first child between 1973 and 1984 (i.e., 6 years before or after the OCP), and between 1971 and 1986 (i.e., 8 years before or after the OCP) in our sensitivity test models.

Figure 4.1. Deriving the Analysis Sample



4.3.3. Variables

4.3.3.1. Dependent variables

In our base models, the dependent variable is a binary variable indicating whether respondents with LTC needs receive help with daily activities from their children. To further study whether care from alternative caregivers may also be affected by family size, we also use binary variables indicating whether respondents with LTC needs receive help from (1) anyone, (2) their children or children-in-law, and (3) their spouses as the dependent variables. Specifically, respondents are asked whether they have difficulties with each ADL (dressing, bathing, eating, getting in/out of

[11] This is similar to the regression discontinuity design (RDD).

bed, toileting, and continence) and IADL (doing housework, cooking, shopping, managing money, and taking medication). Respondents who state that they have difficulties and need help are then asked to name up to three persons[12] that most often help them with their ADLs and IADLs, and their relationship to those people (e.g. spouse, child, child-in-law, parent, sibling, and volunteer).

4.3.3.2. Treatment variable

Our key independent variable is the number of living children the respondent has. Number of children is categorized as having 1, or 2+ children in our base models, and having 1, 2, or 3+ children in our sensitivity test models. We treat number of children categorically instead of continuously to study the nonlinear marginal effect of each additional child[13]. We also want to examine if there is a threshold above which additional children will not affect the likelihood of receiving LTC from children. Specifically, to evaluate the potential impact of the new TCP, our categories are designed to study whether having two children (instead of one child) will increase one's probability of receiving care from children, and whether having three or more children will further increase this likelihood.

4.3.3.3. Control variables

We also control for (or in some cases, stratify by) a rich set of individual-level variables

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[12] Most respondents only named one helper

[13] We also considered using "number of children ever born" as our independent variable. In our sample, 5% of the respondents have at least one deceased child, but only 2% would have a different value for the independent variable if we use "number of children ever born" instead of "number of living children" (e.g. if a person had 4 children but 1 died, he will have the same value for the independent variable under different definitions, since the independent variable is binary). Therefore, we prefer using "number of living children" as the more directly relevant measure of potential supply of LTC.

available in the CHARLS that might be related to both family size and receiving LTC from children. Specifically, adapted from the Anderson's model, our predisposing control variables include: age, sex, marital status, place of residence (rural/urban), and level of education of the individual. Our enabling control variables include: annual household consumption (in 2013 Chinese RMB), and whether has health insurance. Our healthcare need control variables include: self-rated health status and number of limitations in ADLs. We also control for year and province fixed effects to account for general trends in LTC provision and unobservable time-invariant province characteristics such as state formal LTC system and filial piety tradition that may vary systematically.

4.3.4. Empirical strategy

4.3.4.1. Structural model setup

We begin by estimating the (potentially biased) association between family size and the probability of receiving care from adult children using logistic regression models.

$$logit(P(LTC_{it} = 1)) = \alpha_0 + \alpha_1 Children_i + \alpha_2 X_{it} + Province_{it} + Year_i \qquad (1)$$

Here, LTC_{it} is the outcome measure for individual i at time t. $Children_i$ represents the number of children that individual has. X_{it} is a vector of controls for individual-level characteristics. We also control for year and province fixed effects.

4.3.4.2. Instrumental variables design

The main concern with the naïve logistic regression estimates is that one's fertility choice might be endogenous to individual characteristics that also affect LTC outcomes.

Therefore, we use an instrumental variable approach to address this potential regressor endogeneity issue. The IV approach uses one or more instruments, which affect the family size but do not directly affect the LTC outcomes, to mimic a randomization of individuals to having different numbers of children. When validity assumptions are met, this approach uses only exogenous sources of variation and allows us to obtain unbiased treatment effects (McClellan et al., 1994). The ideal instrument(s) should affect one's family size but should not affect one's probability of receiving informal care without altering the family size. That is, the instrument(s) should be exogenous and should be uncorrelated with the unobserved confounders in the structural model after controlling for other covariates. Adapted from Qian's (2009) and Islam & Smyth's (2014) studies, our instrument is based on the exogenous variation in family size caused by the OCP in 1979 and sex of the first-born.

Our IV is the interaction term of the eldest child's sex and year of birth: boy*1979, where boy is a binary variable indicating whether the eldest child is a boy and 1979 is a binary variable indicating whether the eldest child was born in or after 1979. The year dummy variable relates to the mandatory OCP in 1979. Due to the boy preference and the OCP, we assume that: (1) the family is more likely to be affected by the OCP and have fewer children if the eldest child is a boy, and he was born in or after 1979, and (2) the family is more likely to have more children if the eldest child is a girl, or if the eldest child was born before 1979.

The instrument would be invalid if the sex of the eldest child is not random in the potential presence of sex-selective abortion caused by OCP and boy preference. However, in many rural areas, where boy preference is more common, families with girl firstborns were allowed to have a second child, and therefore sex selection in firstborns should be less common (Chen et al., 2010). Ebenstein (2011) finds that the sex of the firstborns is balanced and unchanged between 1982 and 2000 in China, and the observed imbalanced sex ratio is mostly driven by sex selection for the second and later-borns. We also find a balanced sex ratio of firstborns in our sample[14]. Another concern is that the sex of the first-born may affect LTC provision through changing the sex composition of children. Since son(s) are expected to be the LTC providers according to the filial piety tradition, all else equal, parents with son(s) may be more likely to receive care than parents without a son. Therefore, in our sensitivity models, we also account for sex of the children. It is plausible to consider the sex of the eldest child to be exogenous after the existence of son(s) is controlled for. In one sensitivity model, we directly control for the existence of son(s). This model may be theoretically plausible, although it may be empirically impossible to increase the family size while keep the existence of son(s) unchanged. Therefore, we also conduct a different sensitivity test and only include individuals who have son(s). Since our base model estimate is consistent with our sensitivity model estimates, our base model estimate is robust to accounting for sex of the children.

The instrument would also be invalid if parents who have their first child before 1979 are different from parents who have their first child in or after 1979 in ways that affect LTC provision independently of the effect through family size. Because there have been substantial changes in socioeconomic status, sources of old-age support, living arrangements, and in the health care system in China in the past several decades, we cannot rule out this possibility. Therefore, we only include individuals who have their first child in a small time interval around the time the OCP was initiated (4 years before or after 1979) [15] to enhance our sample homogeneity. Conceptually, this can be seen as a fuzzy regression discontinuity design. We show that, in our sample, parents who have their first child before 1979 are on average only 3.8

[14] The sex ratio of the firstborns in our sample is 107 male to 100 female.
[15] Further sample restriction leads to small sample size issues and a weak first-stage correlation between family size and the IV

years older than parents who have their first child in or after 1979 (and we control for their age and age squared), and all of the other observed individual and family characteristics are balanced (Table 4.1). Therefore, it is plausible to consider the year of birth of the eldest child to be exogenous. We also conduct sensitivity tests to include individuals who have their first child 6 or 8 years before or after the OCP, and our IV estimates are robust to different sample restrictions. In addition, as suggested by (Qian, 2009), if couples were affected by the previous "late, long, and few" policy and had a 4-year birth spacing, they may still be affected by the OCP even if their first child was born between 1975 and 1978. This may not violate the IV exclusion restriction, but may make the IV weaker due to a smaller proportion of compliers[16]. Therefore, in our sensitivity model, we include individuals who have their first child between 1971 and 1974, and between 1979 and 1982 to allow for the effect of birth spacing[17].

[16] Individuals who are induced by our instrument to change their fertility decision.
[17] In this case, IV=1 if the eldest child is a son and was born between 1979 and 1982, and IV=0 if the eldest child is a daughter or if the child was born between 1971 and 1974.

Table 4.1. Descriptive statistics of independent variables, by eldest child year of birth

	Born 1975-1978 (N= 651)	Born 1979-1982 (N= 735)	p-value
SES			
Age (Mean)	60.7 (4.6)	56.9 (4.6)	<0.001
Female (%)	55.5	58.0	0.347
Education (%)			0.104
No education	39.5	35.1	
Primary school	40.9	40.2	
Middle school	14.1	16.8	
High school or higher	5.5	7.9	
Urban (%)	35.2	34.6	0.809
Wealth			
Annual household consumption (Mean $)	31,633 (60,841)	28,699 (38,578)	0.278
Family			
Married or partnered (%)	91.6	93.2	0.248
Health insurance			
Uninsured (%)	5.1	3.8	0.254
Health			
Number of ADLs (%)			0.320
Problem with 0 ADL	69.9	71.0	
Problem with 1 ADL	16.4	18.4	
Problem with 2 ADLs	6.1	5.1	
Problem with 3+ ADLs	7.6	5.5	
Self-rated health (%)			0.571
Excellent or very good	2.2	3.1	
Good	6.0	5.3	
Fair	35.9	34.1	
Poor or very poor	55.9	57.5	

The comparisons between the two treatment arms are calculated based on simple two-sample t-tests or chi-squared test.
Standard deviations in parentheses.

Although the exogeneity of our IV cannot be tested directly, we present a similar “IV balance check” table as that presented in the McClellan et al. (1994) study. Table 4.2 shows sample characteristics for individuals whose eldest child is a son and was born between 1979 and 1982 (IV=1) and individuals whose eldest child is a daughter or was born between 1975 and 1978 (IV=0). Despite significant difference between the two groups in age, which is supposed to be different due to the design of the IV, the differences in all the other observed characteristics are not statistically significant. This suggests that the IV approach has improved sample balance in observed characteristics/ confounders (compared to those shown in Table 4.3), providing support for the validity of the instrument.

Table 4.2. Descriptive statistics of independent variables, by IV

	IV=0 (N=1,019)	IV=1 (N=367)	p-value
SES			
Age (Mean)	59.4 (5.0)	56.8 (4.5)	<0.001
Female (%)	55.8	59.4	0.238
Education (%)			0.066
No education	38.7	33.1	
Primary school	40.2	41.3	
Middle school	15.2	16.4	
High school or higher	5.9	9.3	
Urban (%)	34.8	34.9	0.989
Wealth			
Annual household consumption (Mean $)	30,438 (55,131)	29,074 (33,336)	0.656
Family			
Married or partnered (%)	93.0	90.7	0.154
Health insurance			
Uninsured (%)	5.0	2.7	0.068
Health			
Number of ADLs (%)			0.050
Problem with 0 ADL	68.7	75.4	
Problem with 1 ADL	17.9	16.2	
Problem with 2 ADLs	6.0	4.5	
Problem with 3+ ADLs	7.5	3.9	
Self-rated health (%)			0.336
Excellent or very good	2.3	3.8	
Good	5.5	6.0	
Fair	34.5	36.1	
Poor or very poor	57.7	54.1	

The comparisons between the two treatment arms are calculated based on simple two-sample t-tests or chi-squared test.
Standard deviations in parentheses.

4.3.4.3. Instrumental variable models

Since we have nonlinear dependent and treatment variables, traditional two-stage least squares regression (2SLS) models might result in biased results. We, therefore, use the two-stage residual inclusion (2SRI) method (Terza et al., 2008).

Our first-stage logistic regression models use instrumental variable to predict binary number of children:

$$\mathrm{logit}(\mathrm{P}(\mathrm{Children} = 2+)) = \beta_0 + \beta_1 \mathrm{IV}_{it} + \beta_2 \mathrm{X}_{it} + \mathrm{Year}_t + \mathrm{Province}_{it} \qquad (2)$$

where IV_{it} is the instrumental variable for individual i at time t, and the other variables are the same as described in equations (1). We then calculate the response residuals $\widehat{r_{it}}$ from the first-stage models, which are the difference between the predicted probabilities and observed values. These residuals are included in the second-stage models below as additional regressors to produce the correct adjustment for the endogeneity in the outcome equations:

$$\text{logit}(P(\text{LTC}_{it} = 1)) = \alpha_0 + \alpha_1 \text{Children}_{it} + \alpha_2 X_{it} + \text{Year}_t + \text{Province}_{it} + \widehat{r_{it}} \qquad (3)$$

where LTC_{it} indicates whether the respondent received LTC from his children, and the other variables are the same as described above. We use the second-stage models to estimate the causal effects of family size on the likelihood of receiving LTC from adult children (or other caregivers). For sensitivity test models in which the number of children is categorized as 1, 2, and 3+, we use ordered logistic regression models in the first stage, and calculate the response residuals for each category of choice except the reference category (two residuals for three categories). We then include both residuals in the second stage models (Terza et al., 2008). In all of our regression models, we cluster the standard errors on the individual identifier to account for the fact that some individuals have two observations. Finally, we perform a bootstrap procedure for both stages, with 500 iterations to correct standard errors (Efron, 1981).[18]

Our main models estimate the effect of family size on the likelihood of receiving LTC from adult children among parents who have LTC needs. We also construct numerous alternative specifications to test the robustness of our results. (A) To test the appropriate bandwidth and see whether our IV estimates hold, we relax our sample restrictions and run our models among individuals whose first child was born between 1973 and 1984 (6 years before or after OCP), and between 1971 and 1986 (8 years before or after OCP). (B) To account for the potential impact of

[18] In our bootstrap process, we resample at the observation level. Our results are robust to resampling at the individual and household levels, which might approximate the real data generating/sampling mechanism better.

the 4-year birth spacing policy, we do not include individuals whose first child was born between 1975 and 1978, but whose first child was born between 1971 and 1974 instead. (C) To further study the nonlinear marginal effect of additional children, we also categorize number of children as 1, 2 and 3+. (D) Instead of using pooled observations from both waves and clustering the standard errors on the individual identifier, we keep one observation per individual to account for the panel nature of the data[19]. (E) We account for sex of the children by either controlling for the existence of son(s) or including only individuals with son(s) in our sample.

Given the large rural urban disparities in health care and pension systems and the difference in living arrangements, having more children may affect LTC provision differently in rural and urban areas (Zimmer & Kwong, 2003). Therefore, we estimate treatment effects on rural and urban samples separately.

We also examine whether family size affects one's overall likelihood of receiving care from alternative caregivers, and use "whether receiving any care" as the outcome. Since our results show that individuals who have more children are no different in their overall likelihood of receiving LTC than individuals with only one child— suggesting that LTC from other caregivers may substitute for LTC from children, we next want to know who the alternative caregivers are. We use "receiving care from children or children-in-law" and "receiving care from spouses" as outcomes to see whether care from children-in-law and/or spouses may substitute for care from children.

[19] 20 percent individuals in our sample have 2 observations. If an individual has two observations, we keep the second one.

4.4. Results

4.4.1. Descriptive statistics

Table 4.3 provides descriptive statistics of dependent variables for our base model sample (first child born 1975-1982). There are significant differences between respondents with one child and respondents with two or more children: parents with only one child are older, more likely to live in urban areas, and need help with more ADLs. These suggest the potential endogeneity of family size and the necessity of using the IV approach. Table 4.4 provides descriptive statistics of independent variables.

Table 4.3. Descriptive statistics of independent variables, by family size

	1 child (N=190)	2+ children (N=1,196)	p-value
SES			
Age (Mean)	60.7 (6.0)	58.4 (4.7)	<0.001
Female (%)	55.3	57.0	0.649
Education (%)			0.131
No education	33.7	37.7	
Primary school	37.9	40.9	
Middle school	18.4	15.1	
High school or higher	10.0	6.3	
Urban (%)	51.1	32.3	<0.001
Wealth			
Annual household consumption (Mean $)	31,815 (47,479)	29,801 (50,724)	0.608
Family			
Married or partnered (%)	90.5	92.7	0.287
Health insurance			
Uninsured (%)	4.2	4.4	0.890
Health			
Number of ADLs (%)			0.021
Problem with 0 ADL	60.6	72.0	
Problem with 1 ADL	23.4	16.5	
Problem with 2 ADLs	8.0	5.2	
Problem with 3+ ADLs	8.0	6.3	
Self-rated health (%)			0.393
Excellent or very good	2.6	2.7	
Good	5.8	5.6	
Fair	29.5	35.8	
Poor or very poor	62.1	55.9	

The comparisons between the two treatment arms are calculated based on simple two-sample t-tests or chi-squared test.
Standard deviations in parentheses.

Table 4.4. Descriptive statistics of dependent variables, by family size

Base sample (born 1975-1982)	1 child (N=190)	2+ children (N=1,196)	Total
Received LTC from children (%)	20.5	17.1	17.6
Received LTC from children or children-in-law (%)	24.7	20.8	21.4
Received LTC from spouses (%)	61.6	59.4	59.7
Received LTC from anyone (%)	77.9	71.0	71.9
Sensitivity sample (born 1973-1984)	1 child (N=289)	2+ children (N=1,780)	Total
Received LTC from children (%)	18.3	17.1	17.3
Sensitivity sample (born 1971-1986)	1 child (N=361)	2+ children (N=2,381)	Total
Received LTC from children (%)	18.3	17.1	17.2
Sensitivity sample (born 1971-1974 & 1979-1982)	1 child (N=162)	2+ children (N=1,359)	Total
Received LTC from children (%)	19.8	17.6	17.8
Sensitivity sample (have at least 1 son)	1 child (N=119)	2+ children (N=1,058)	Total
Received LTC from children (%)	18.5	16.3	16.5
Rural sample	1 child (N=93)	2+ children (N=810)	Total
Received LTC from children (%)	24.7	16.8	17.6
Urban sample	1 child (N=97)	2+ children (N=386)	Total
Received LTC from children (%)	16.5	17.9	17.6

The comparisons between the two treatment arms are calculated based on simple two-sample t-tests.
Standard deviations in parentheses.

4.4.2. Regression results

Table 4.5 presents regression results of the first-stage model. We find that having an eldest son born in or after 1979 leads to a sizable 9.2 percentage points (or approximately 11 percent) reduction in the likelihood of having more than 1 children.

As shown in Table 4.6, our main specification reveals a positive and significant effect of family size on the probability of receiving LTC from children. Individuals with two or more children are 17.4 percentage points more likely to receive informal care from their children than their counterparts with only one child. This represents a roughly 100 percent increase over the sample baseline mean probability of receiving care from children of 17.6 percent.

As shown in Table 4.7, the results of our sensitivity analyses are generally consistent with our main results. For model As, when we relax the sample restriction to include individuals who have their first child 6 or 8 years before or after the OCP, our marginal effects become slightly different, but are still significant and meaningful in size. The IV estimates also become more precise as the sample sizes increase and the standard errors shrink. Overall, even if we use a wider bandwidth, our conclusion about the causal effects of the family size would not be violated. These consistent findings also suggest that our sample is relatively homogenous. For model B, when we take into account the potential impact of the (voluntary) 4-year birth spacing policy and use the 4 years before 1979 as the washout period, as expected, our 1st-stage F score become larger, suggesting more compliers. Our IV estimate is similar in magnitude to the base model. For model C, when we use three categories of family size (1, 2, or 3+ children) instead of a dichotomous indicator (1 or 2+ children), the results reveal diminishing marginal effects of having additional children— compared to parents with one child, parents with two children are 20.2 percentage points more likely to receive care from children, and parents with three or more children are 29.7 percentage points more likely to receive care from children. Therefore, although having two children will greatly increase one's probability of receiving care from children (compared to having one child), having more than two children will not meaningfully further increase this probability. Our findings also suggest that further relaxation of the current TCP may not be necessary for increasing the supply of children caregivers. Model D that includes one observation per individual also yields similar IV estimates. Finally, estimates of model Es that account for sex of the children are also consistent with the estimate of the base model. Overall, across all specifications, the positive and large effects on the probability of receiving LTC from adult children are consistent and robust.

Table 4.5. Estimates of marginal effects, first-stage model

	2+ children
Instrumental variable	-0.092***
	(0.030)
Age (centered around the mean)	-0.011***
	(0.003)
Age^2	-0.000
	(0.000)
Female	-0.047*
	(0.026)
Education (no education is the reference)	
Primary	-0.010
	(0.026)
Secondary	-0.027
	(0.036)
Higher	-0.046
	(0.054)
Urban	-0.082***
	(0.024)
Married or partnered	0.028
	(0.040)
Uninsured	0.014
	(0.040)
Self-rated health (excellent or very good is the reference)	
Good health	-0.118**
	(0.057)
Fair health	-0.062
	(0.043)
Poor or very poor health	-0.093**
	(0.043)
Number ADLs (problem with 0 ADL is the reference)	
1 ADL	-0.047*
	(0.028)
2ADLs	-0.068
	(0.049)
3+ ADLs	-0.012
	(0.041)
Household annual consumption in 1,000 RMB	-0.000
	(0.000)
Observations	1,226

* p<0.1; ** p<0.05; *** p<0.01. Standard errors in parentheses.

Table 4.6. Estimates of marginal effects, base models

	No IV models	IV models
Base model (1st child born 1975-1982)		
2+ children	-0.003	0.174**
	(0.032)	(0.077)
1st-stage Chi2		10.6
Observations	1,256	1,212

* p<0.1; ** p<0.05; *** p<0.01. Standard errors in parentheses.

Table 4.7. Estimates of marginal effects, sensitivity test models

Sensitivity test models	No IV models	IV models
Sensitivity model As, different restrictions on YOB of 1st child		
A1) 1st child born 1971-1986		
2+ children	0.007	0.167***
	(0.023)	(0.053)
1st-stage Chi2		34.2
Observations	2,465	2,382
A2) 1st child born 1973-1984		
2+ children	0.012	0.216***
	(0.026)	(0.041)
1st-stage Chi2		19.5
Observations	1,871	1,805
Sensitivity model B, account for birth spacing		
2+ children	0.035	0.219***
	(0.032)	(0.031)
1st-stage Chi2		15.5
Observations	1,373	1,279
Sensitivity model C, categorical number of children		
2 children	-0.014	0.202**
	(0.032)	(0.081)
3+ children	0.017	0.297**
	(0.036)	(0.129)
1st-stage Chi2		25.8
Observations	1,256	1,256
Sensitivity model D, one observation per individual		
2+ children	-0.021	0.187**
	(0.037)	(0.082)
1st-stage Chi2		10.0
Observations	1,042	1,005
Sensitivity model E, account for sex of the children		
E1) Control for the availability of son(s)		
2+ children	0.015	0.144**
	(0.031)	(0.065)
1st-stage Chi2		28.7
Observations	1,256	1,212
E2) Only include individuals with son(s)		
2+ children	-0.000	0.147**
	(0.037)	(0.061)
1st-stage Chi2		27.9
Observations	1,054	1,012

* p<0.1; ** p<0.05; *** p<0.01. Standard errors in parentheses.

As shown in Table 4.8, our IV estimates on rural and urban subsamples suggest that increased family size leads to a 17.2 percentage point increase in the likelihood of receiving care from children among rural seniors, and a 5.1 percentage point insignificant reduction in the likelihood of receiving care from children among urban seniors. The first-stage F score is insignificant in the rural sample model, suggesting a weak correlation between the IV and the likelihood of having 2+ children. Potentially, this may be because the OCP had been less rigorously enforced in rural areas and the subsample size is small. Nevertheless, our results are consistent with our hypothesis that having more children may have greater implications for rural residents because the current formal LTC system favors urban residents and urban residents are less dependent on their children for informal care. However, since we do not have a strong IV for the rural sample model, the results, especially the magnitude of the IV estimate, should be interpreted carefully.

Further, Table 4.9 shows regression results of the impact of family size on LTC from alternative caregivers. Model A examines the impact of having 2+ children on individual's overall likelihood of receiving care when needed. We see that on average, 71.9 percent individuals who need LTC receive care (Table 4.4). Although individuals with more children are 4.0 percentage points less likely to receive care, this effect is insignificant and neither large nor meaningful. Since having more children will only affect one's probability of receiving care from his children but not his overall probability of receiving LTC, there must be some alternative caregivers who may substitute for the care provided by children. Therefore, in model B, we examine the impact on receiving care from children or children-in-law. We find that having 2+ children will increase one's likelihood of receiving care from children or children-in-law by 11.9 percentage points, or about 50 percent when considering that the mean of the dependent variable is 21.4 percent. Our results suggest that children-in-law may, to some extent, substitute children's roles as caregivers, but that still could not explain the large difference between the control and treatment groups. Therefore, in Model C, we further examine the causal relationship between family size and the probability of receiving care from spouses. We find that having two or more children will reduce one's likelihood of receiving care from spouse by 17.4 percentage points or 30 percent when considering that the mean of the dependent variable is 59.7 percent, although not statistically significant. Overall, our results show that spouses are the primary caregivers in modern China—consistent with the findings by Gruijters (2015)—and that although having more children increases LTC provision by children, care provided by spouses may greatly substitute for care provided by additional adult children.

Table 4.8. Estimates of marginal effects, subsample models

Subsample models	No IV models	IV models
Model A, rural only		
2+ children	-0.044	0.172*
	(0.045)	(0.102)
1st-stage Chi2		1.2
Observations	808	808
Model B, urban only		
2+ children	0.039	-0.051
	(0.048)	(0.197)
1st-stage Chi2		14.1
Observations	407	407

* p<0.1; ** p<0.05; *** p<0.01. Standard errors in parentheses.

Table 4.9. Estimates of marginal effects, alternative care givers

Alternative caregiver models	No IV models	IV models
Model A, receiving any care		
2+ children	-0.022	-0.040
	(0.041)	(0.244)
1st-stage Chi2		10.6
Observations	1,185	1,145
Model B, receiving care from children or children-in-law		
2+ children	-0.021	0.119
	(0.035)	(0.127)
1st-stage Chi2		10.6
Observations	1,256	1,212
Model C, receiving care from spouse		
2+ children	0.024	-0.174
	(0.042)	(0.176)
1st-stage Chi2		10.6
Observations	1,268	1,224

* p<0.1; ** p<0.05; *** p<0.01. Standard errors in parentheses.

4.5. Conclusion

Although policymakers in China have considered the relaxation of OCP as a potential solution for meeting the increasing demand for LTC, no empirical study has examined the causal impact of family size on the probability of receiving LTC from adult children and other caregivers in modern China. In this paper, we estimate the causal effects of having more children on LTC reception. We find that compared to parents with only one child, parents with two or more children are more likely to receive care from their adult children. This effect is larger among rural residents but insignificant among urban residents. However, having more children does not increase the parents' overall probability of getting care— care from spouses largely compensates/substitutes for care from adult children. Our results suggest that the new TCP may not necessarily lead to an increase in total informal care supply, but may instead transfer the caregiving burden from spouses to adult children. Therefore, the end of the OCP will likely not reduce the need for further investment in developing the formal LTC sector in China.

Our results are subject to several potential limitations, but we use strategies in each case to address or minimize them to the extent possible. First, by using an instrumental variable design, we may only estimate the local average treatment effects (LATEs), which is the average treatment effects among individuals who are induced by our instrument to change their fertility decision (i.e., the "compliers")(Imbens & Angrist, 1994). As we discussed earlier, those people are individuals who have one child if their first born is a son who was born after OCP. They would have two or more children if their first child was a daughter or if their first child was born before OCP. These compliers are those with a boy preference who obey the OCP, and they represent a large proportion of people in China. Second, we do not examine the effect of family size on the amount/quantity of care provided by caregivers due to concerns about data quality. Although respondents were asked to report the amount of care they received during the last month, we are not able to use these variables because the data quality of hours of care is suspect, with many cases falling at 0 or above 720 hours. This may suggest that respondents have a problem understanding this question and provide inconsistent answers. Third, because most parents with first child born around 1979 are still in their 50s or 60s, we cannot study the LTC support for the oldest-old. It is possible that the oldest-old, who have limited access to care provided by their spouses, have to depend more on their children. So family size may play a greater role in determining their old-age support. Studies are needed to focus on this population when data becomes available in the future. Finally, our study does not aim to estimate whether people will have two children under the new TCP, and which people are more likely to have two children. Future studies are needed to estimate how this new policy may change the supply of informal care.

Overall, our findings on the effects of family size on informal LTC reception have several key implications. One is that if more people will have two children under the new TCP, the policy may help more people to receive care from their children. However, in modern China, adult children are no longer the primary caregivers for their parents (at least for parents who are younger seniors), and the LTC support from spouses may greatly substitute for LTC support from additional children. Therefore, the new TCP will likely not increase the total supply for informal care nor reduce the need for further investment in developing the formal LTC system. In addition, further relaxation of the TCP to allow families to have more than two children may also not help with stimulating informal care supply from children.

Chapter 5 Conclusions and Policy Implications

Given the importance of the issue of population aging, different countries have implemented various policies to strengthen their LTC systems. Therefore, there is an urgent need to understand the problems and challenges that different LTC systems are facing and evaluate these approaches. These three essays examine the economics of LTC and the family under different intervention programs.

In Chapter 2, we estimate the causal effects that LTCI has on individuals' wealth, and then assess the potential explanatory mechanisms of these effects. We find that insured individuals accumulate more assets and delay/reduce asset transfers, and are less likely to enroll in safety net programs. However, LTCI is ineffective in protecting insured individuals against high OOP medical expenditures. Overall, our findings on the effects of LTCI on financial outcomes have several key implications. One is that current LTCI policy design might be insufficient to protect the insured against large medical expenditures. However, it might improve the general financial well-being of insured individuals by encouraging them to save more and reduce/delay asset transfers. Furthermore, LTCI cost-effectiveness studies should also consider additional savings associated with reduced safety-net program enrollment, increased labor participation due to reduced use of informal care, and increased personal savings.

In Chapter 3, we estimate the causal effects of home care use on key physical and mental health outcomes for spouses of care recipients. Our findings show that home care use leads to mostly insignificant, yet consistently negative effects on spousal physical health. The negative effect on spousal functional status also increases over time. On the other hand, we find improved spousal mental health outcomes, especially in depression symptoms. Although the estimated effects of care setting on spousal health are limited, these results are less surprising given the fact that most of the care recipients used home care or nursing home services for only 1 wave. From a policy perspective, our findings on spousal health outcomes are important in estimating the cost and effectiveness of HCBS expansion. Future studies should examine the health service utilization and medical costs for spouses of home health users. Finally, HCBS programs should consider support for spouses and other family members of care recipients, focusing on their physical health and ways to avoid declining functional status.

In Chapter 4, we estimate the causal effects of having more children on LTC reception in China. We find that compared to parents with only one child, parents with two or more children are more likely to receive care from their adult children. However, having more children does not increase the parents' overall probability of getting care— care from spouses largely compensates/substitutes for care from adult children in smaller families. Our results suggest that the new TCP may not necessarily lead to an increase in total informal care supply, but may instead transfer the caregiving burden from spouses to adult children. Therefore, the end of the OCP will likely not reduce the need for further investment in developing the formal LTC sector in China.

One substantial and common challenge in conducting evidence-based research to assist decision making for policymakers is to examine the causal effects of the policies in situations where randomization is not feasible. In these cases, it is important to use rigorous quasi-experimental design. By using instrumental variable methods in these three analyses, our study results are not likely to be confounded by selection bias, and our estimates of treatment effects are plausibly causal. These causal effects lend themselves to more informed decision making about policies that can increase access to LTC services and financing in the future.

The LTC systems in China and the U.S. are facing different challenges. The two countries have taken approaches that are based on their cultural and development contexts to strengthen their LTC and social support systems. Our research provides new evidence on the financial protection effect of private LTCI, the spousal health outcomes of home care use, and the impact of family size on LTC provision. Future studies are needed to track progress of these interventions, and quantify social costs and benefits.

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