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Essays on Health, Healthcare, Job Insecurity and Health Outcomes

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by

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Dedication

This dissertation is dedicated to all my family members, including my wife Yuanzheng Wang, my three children Shizuka Nakamoto, Akira Nakamoto, Yutaka Nakamoto, my two sisters Yanbin Wang, Yanqing Wang and my parents. Without all of you, this work would not have been completed in time. Your support and encouragement motivated me to overcome difficulties with courage and to make improvements with bravery.

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Abstract

This doctoral dissertation proposal is comprised of three separate chapters, all of which uses the nationally representative uniform survey Health and Retirement Survey (HRS) to examine the relationship between health, insurance, health care and health outcomes. Below, the brief introduction for each section is provided:

- Chapter I: Medicare Part D and Patients' Well-being
- Chapter II: Parent's Health Insurance and Informal Care
- Chapter III: Job Insecurity and Health (with Dr. Ayyagari)

In chapter I, I explore how Medicare Part D (MD) affects the well-being of the severely sick patients both in the short- and in the long- term. I employ difference-in-difference (DD) alongside the instrumental variable (IV) model. The estimated results imply MD significantly improves mental health and increases regular drug utilization for the elderly. However, it neither systematically improves out-of-pocket payment (OOP) nor improves mortality across all waves. This suggests that MD provides an efficient mechanism to improve mental health and drug utilization, but might not necessarily enhance survival rate and financial burden for vulnerable patients.

Chapter II investigates the relationship between informal care provided by the children and the take-up of health insurance by the near-elderly and elderly parents, and how the correlation is influenced by parent's Activities of Daily Living (ADLs) and Instrumental Activities of

Daily Living (IADLs). The results indicate that when the endogeneity is controlled for, informal care systematically crowds out the take-up of private long-term care (LTC) insurance whereas “crowds in” the take-up of the total plan including supplement insurance plans (TSP). Nevertheless, the degree of both crowding-out and “crowding-in” effect is reduced when the severity of ADLs/IADLs disability level grows. Our study reflects (a) the strong demand for TSP and more additional health coverage within household budget line (b) and the potential gap between healthcare demands by the parents and the informal care provided by the children and the potential gap between the healthcare demands by the parents and the formal care covered by the insurance. Our estimates are robust to alternative measures of informal care.

The final chapter III examines the causal effect of subjective job insecurity on health, using pooled ordinary least squares (OLS), fixed-effects (FE) and instrumental variable (IV) specifications. The estimate implies that the negative impact of job insecurity is more pronounced for certain outcomes such as mental health and the emergence of new health conditions. Job insecurity provides a powerful prediction on subsequent job displacement and real income loss. Sub-population such as low-employability/better-educated individuals or males responds more to job insecurity than their counterparts.

Chapter I

Medicare Part D and Patients' Well-being: Benefits and Costs

ABSTRACT

This chapter aims to uncover the impact of Medicare Part D (MD) on the understudied well-being of the vulnerable and severely sick patients by investigating both the short- and the long-term effects. We employ difference-in-difference (DD) along with instrumental variable (IV) model to account for unobserved disturbance and endogeneity. The estimated results imply two-sided effects: MD significantly improves mental health both in the short run and in the long run, and increases regular drug utilization in the long run. Countering this, MD neither statistically improves out-of-pocket payment (OOP) nor improves mortality in all periods. This suggests that the benefit of MD might be ambiguous: it potentially provides an effective mechanism to improve mental health and drug utilization, but might not necessarily enhance survival rate and financial burden for ill patients in the long run.

Keywords: Difference-in-Difference, Medicare Part D, Well-being, Health, OOP, Mortality, Drug Utilization

JEL Classification: I11, I13, I18, I38

1 INTRODUCTION

Medicare, commonly referred to Original Medicare (MO), is the federal health insurance program (FHIP) created in 1965 for individuals aged 65 and over, regardless of income, medical history or health status. It was expanded in 1972 to cover individuals under age 65 with permanent disabilities. The enrollment in Medicare is either based on the age threshold of 65 or certain health conditions for below 65-year old individuals. Medicare Part D (MD), also called the Medicare prescription drug benefit, is a U.S. federal government program to subsidize the costs of prescription drugs and insurance premiums for Medicare beneficiaries. MD went into effect on January 1, 2006, by the passage of the Medicare Modernization Act (MMA) of 2003 and thereafter served nearly 41 million of the 57 million people with Medicare in 2016 (Megellas, 2006). The impact of MD is extensive and has been documented by a large literature.

Literature finds that MD affects health, although in a somewhat heterogeneous way. Kaestner et al. (2010) show that MD does not notably impact patients' self-rated health. Countering this, Afendulis, He, Zaslavsky and Chernew (2011) suggest that health is improved through the evidence that nondrug healthcare utilization for the elderly decreased after MD was introduced. Similar applied work dealing with mortality finds that MD reduces the annual mortality of the elderly by 2.2 percent (Huh and Reif, 2017).

In this study, we concentrate on the four measures of the well-being of severely ill patients, including mental health, drug utilization, out-of-pocket payment (OOP) and mortality. All of these are viewed as vital components of the well-being of the patients. The sample of patients in this study incorporates four types of severe diseases: cancer, stroke, heart problem, and diabetes. [1]Studies reveal that these four diseases are among the eight leading causes of death incidences in 2015 in the U.S., with diseases of heart ranking at the top and cancer following the second (Murphy et al., 2017). However, very little is known about whether patients with these conditions can benefit from the MD reform. And if they do, which categories of health outcomes are improved? If they do not, what are the costs?

These concerns remain unanswered in the literature. As such, we contribute to the literature by examining how the formal implementation of MD impacts the various aspects of well-being for the patients. Further, one limitation of the previous literature is that it does not differentiate between the long-run and the short-run impact of MD. Neither can we know whether the level of significance or magnitude evolves over time or not. This study examines the way MD affects patients' well-being both in the short and in the long run. Another concern is that previous work generally considers all near-elderly or elderly cohorts as one research group; however, the degree of heterogeneity across groups might be large and thus the effect may be different for specific vulnerable subgroups such as severely ill patients. These patients generally bear more financial risks and health shocks and thus demand more assistance and care accordingly. By employing data from the 2002-2014 waves of the Health and Retirement Study (HRS) data, our research adds to the literature by comprehensively exploring the impact of MD on the major concerns of well-being alongside comparing the gap between MD-eligible and MD-ineligible patients over time.

The chapter proceeds as follows. Section 2 presents the review of the literature. Section 3 describes the endogeneity problem. In section 4, we show the empirical strategy. Section 5 and 6 introduce the data and the results respectively. Section 7 concludes.

2 LITERATURE REVIEW

MD is an important component for Medicare and a large body of literature examines the impact of MD on medical care utilization and health outcomes. MD has succeeded in multiple ways such as improving the quality of health care for Medicare recipients and substantially increasing the fraction of Medicare beneficiaries on prescription drug coverage (Duggan et al., 2008).

MD pays for most (near 80 percent) of the drug costs, plans bear 15 percent, and enrollees with partial subsidy pay either 5 percent of total drug costs or $3.35/$8.35 for each generic- and brand-name drug respectively after a certain cut-off payment is met (The Henry J. Kaiser Family

[1] The sample is restricted to the patients who have any of the four diseases in the current wave.

Foundation, 2018). [2]Due to the high-coverage cost shared by a health plan, prescription drug coverage (PDC) could significantly drive up the utilization of drugs (Khan, 2006). In contrast, prior to its formal implementation, nearly one-fourth elderly do not have sufficient PDC, most of whom are exposed to soaring OOP stress (Safran et al., 2005). The financial crisis, including OOP, is even worse for drug-dependent patients. With the introduction of the MD, the OOP costs are reduced noticeably for the elderly at the onset though (Ketcham and Simon, 2008).

It has been shown that drug coverage influences the access to medications (see e.g., Yang et al., 2008; Duggan and Morton, 2010) and it might also change the use of prescription drug for the patients(Zhang et al., 2009). Before the MD reform, some senior individuals without drug insurance cannot obtain sufficient and effective treatment due to incomplete coverage. With the introduction of MD through, the OOP stress decreases and this facilitates the access to medications for patients who previously experienced great costs. The study by Khan (2006) finds that PDC, particularly MD, significantly drives up drug utilization, but has no effect on hospital admissions. Countering this, the effect of prescription drug plan on hospitalizations indicates that Part D is associated with an 8 percent reduction in hospital admissions (Robert et al., 2014). Despite its goal to improve medication usage, access to medications through the channel of cost-related non-adherence (CRN) does not improve after the initialization of MD for mental-ill beneficiaries (Zivin et al., 2009). This suggests that the situation of a patient's not filling or refilling a prescription due to the reason of cost is not improved even post MD.

Part D has been shown to reduce the overall rate of hospitalization by 4.1 percent, which is attributable to the positive health effects causing the elderly to reduce the use of nondrug health

[2] Data is accessible from https://www.kff.org/medicare/issue-brief/whats-in-the-administrations-5-part-plan-for-medicare-part-d-and-what-would-it-mean-for-beneficiaries-and-program-savings.

care (Afendulis et al., 2011). Part D could narrow racial or ethnic disparities in hospital utilization as well (Mahmoudi, Jensen, and Tarraf, 2015). Enrollment in MD is associated with lowered utilization of drug when patients reach the “doughnut hole” regardless of coverage in the zone (Zhang et al. , 2009).

On the other hand, since many drugs, including brand-name products, are covered by most of the plans in MD, it could expand beneficiaries' access to cancer treatments as well (Bowman et al., 2006). MD could increase the adherence to medications lowering the risk of cardiovascular events for patients with hypertension, diabetes, and hyperlipidemia (Zhang et al., 2010). MD can facilitate sizable growth in drug utilization for the elderly (Lichtenberg and Sun, 2007). It becomes imperative that the elderly improve the access to treatments achieving desirable outcomes (Aruru and Salmon, 2013). However, other contemporary studies find the utilization of healthcare is the opposite. Ayyagari, Shane and Wehby(2017) explore the impact of MD on the access to the emergency department (ED) and find that the frequency of visiting ED with non-emergency care contracts. This implies that Part D might provide the potential to better manage the exploitation of healthcare. Further evidence comes from the study by Liu et al. (2011), which finds no evidence of improvement in ED for those eligible for Part D during its first year of implementation.

The applied work reveals mixed effects on OOP as well. Medicare is an expensive market and Part D costs the federal government around $108.0 billion gross spending in 2016. On the one hand, Part D is associated with sizable reductions in OOP spending (Engelhardt and Gruber, 2011), and it is also a major benefit to seniors and works well once enrollment is completed (Heiss, McFadden, and Winter, 2006). The study by Mott et al. (2010) shows that it is associated with 17.6 percent reduction in OOP. Another study by Stubbings and Lau (2013) also confirms the cost-lowering effect of MD. The financial cost is a priority in the beneficiaries’ choice of prescription coverage (De Natale, 2007). MD provides competitive options with relatively low cost compared with MO: MD enrollees with earning ($85,000/individual; $170,000/couple) pay an income-related monthly premium surcharge, which ranges between $13.00 and $74.80 in 2018 in addition to the monthly premium for their specific plan (The Henry J. Kaiser Family Foundation, 2015).[3] It saves patients' cost among the elderly (Lichtenberg and Sun, 2007). The addition of MD may help reduce OOP expenses for transplant recipients who have Medicare (Chisholm and Roberts, 2006). Robert Kaestner et al. (2014) also find that Part D is associated with a 7 percent decrease in Medicare expenditures. In contrast, MD is shown to provide only a limited scope of relief for healthcare costs (Briesacher et al., 2010). Ketcham, Kuminoff and Powers (2016) suggest that one of the reasons is that the elderly may place more weight on the foreseeable plan premiums rather than on unforeseeable OOP costs, which counterfactually inflates their ultimate payment.

Mortality has been one major concern as it directly measures the effectiveness of the policy. Part D helps patients prevent or delay the onset of disease and defer mortality (Semilla, Chen, and Dali, 2015). Drug innovation has significantly reduced the cancer mortality rate by 8.0 percent (Lichtenberg, 2014). Dunn and Shapiro (2014) evaluate the impact of MD on mortality for the population over 65 and find that cardiovascular-related mortality drops substantively. Huh and Reif (2017) suggest that MD reduces elderly mortality by 2.2% annually, which was driven primarily by cardiovascular but not cancer. In contrast, Briesacher et al. (2015) find that Part D is not attributable to a sizable reduction in mortality. From the national level, during the period 1955-2014 the U.S. mortality rates for males and females have both declined steadily, among which 65 or older individuals maintain at a stable long-term downward trend. The most recent

[3] Refer to https://www.kff.org/medicare/issue-brief/medicares-income-related-premiums-a-data-note.

SEER cancer statistics review shows that the death rate of cancer decreased by almost 2 percent (Singh, Henley, and Ryerson, 2017). [4]

A study by Ayyagari and Shane (2015) finds that MD systematically relieves depressiveness symptom among elderly individuals. A similar relief impact is found for chronic pain relative activity limitations (Ayyagari, 2016).

The diseases in this study are more severe than other types of health shock on average, causing more social hazard through multiple channels and many patients might have to live with the symptoms for years at the expense of reduced productivity. We hope the research concentrating on the channel of life-threatening diseases could facilitate policy intervention oriented for optimal solution additional to technical advancement.

One issue with the applied research so far is that how the well-being of severely ill patients is comprehensively impacted both in the short term and in the long term is not clarified yet. Neither do we know how effective the MD plan is for vulnerable individuals. We revisit these issues and investigate them from a more in-depth perspective by estimating the impacts over time. The accumulative and time-lagged effect is examined simultaneously. Our paper aims to fill these gaps through a quantitative study.

These severe diseases are particular in that they cause high personal and high social risks. They contribute to one of the major death incidences as well. Uncovering the extent to which the well-being can be improved is one of our major goals. Additionally, our study employs one sole data source for the purpose of minimizing exterior disturbance, which might arise in the case of combining multiple data sources, the ultimate goal in this study is aiming to be the first research in

[4] Data are available through the source: https://seer.cancer.gov/csr/1975_2014/browse_csr.php?sectionSEL=2&pageSEL=sect_02_zfig.03.html.

evaluating the impact of a health reform on under-concerned cohorts and thus providing reference for the intervention oriented for optimal effect.

3 ENDOGENEITY ISSUE AND SOLUTION

The main research question in this study is to assess the impact of MD on severe patients' well-being, including mental health, regular drug utilization, OOP and mortality. However, one of the major concerns is the potential of endogeneity, especially in the case of OLS specification. An endogeneity problem occurs when an explanatory variable is correlated with the unobserved error term. There are well-known reasons for endogeneity. The first is a confounder influencing both independent and dependent variables. The second is a reverse causality between the independent and dependent variables of a model. The third concern is how to address the issue if the endogeneity exists. Though literature deals with partial welfare effect of the patients, it does not address the endogeneity essentially (see e.g., Kircher et al., 2014). Therefore one cannot clarify whether the improved welfare is solely attributable to the MD or other exterior or internal shocks.

The potential endogeneity in this study comes from the PDC of the patients. In this study, we aim to concentrate on the more vulnerable individuals. The major concern about endogeneity is that certain unobserved confounder affects both the MD eligible (65 years old or above patients, excluding below-65 patients with disability conditions. These individuals are defined as our treatment group) and MD ineligible (below-65 cohorts, who are defined as our control group) patients in a different way over time. The question concerning PDC in the prior-2006 surveys is designed as follows: "Have the costs of your prescription medications been completely covered by health insurance, mostly covered, only partially covered, or not covered at all by health insurance?" We define those providing completely covered, mostly covered or partially covered response as the ones with PDC while those not covered at all as the without-PDC patients and those unknown or who refuse to answer are eliminated. Aside from this, another question asks about whether Medicare supplemental or Medigap plans help pay for the prescription drug. The patients covered by either of these two plans are treated as having PDC coverage, therefore the prescription coverage includes the portion from MD as well as the portion from non-MD insurances.

In order to address the potential endogeneity, we employ a Difference-in-Difference (DD) model. The assumption in this study is that the unobserved characteristic associated with the dynamics of the outcome variable is to be balanced between the MD- eligible and ineligible cohorts. The identifying assumption is the parallel trend before the implementation. We estimate the trend gap over periods between the eligible and ineligible cohorts. If the trend gap is not significant at each period prior to MD, then the pre-trends for eligible and ineligible patients are parallel. Thus there is no existence of potential endogeneity impacting both cohorts in a systematically different way over time before MD was implemented.

4 METHODOLOGY

We first estimate the causal effect of MD on patients' well-being through a difference-in-difference (DD) model, which is illustrated in equation (1). The treatment group (MD eligible) consists of the 65-73 years old patients, who are eligible for MD. The control group (MD ineligible) is comprised of 50-64 years old patients and thus is ineligible for MD. To achieve this, we further restrain this cohort to the non-permanent-disability patients due to the reason that below 65-year old patients may qualify for Medicare and thus MD based on adverse health conditions. By excluding them, the control group is systematically not impacted by the MD.

The dependent variable y denotes patient $i's$ well-being at year t, including mental health**,** prescription drug utilization in the past two years, OOP since the last wave, mortality or other health outcomes of concern. The independent variables include the following: one dummy indicator I_D for MD eligible group (1 for the 65-73 years old patients eligible for Medicare, and 0 for the 50-64 years old patients ineligible for Medicare), fixed effects for waves, other control vector of covariates X include socioeconomic status or demographics such as age, age squared, gender (male as the reference group), race indicators for Black/African and other race(White/Caucasian as the reference), marital status indicators for never married and other marital status (the married as the reference), indicators for college or higher education (high school or less education as the reference).

The DD coefficient β_2 is our main parameter of interest and captures the double differences (DOD) effect in our specification: a positive sign implies that the MD-eligible patients are more likely to have a higher outcome of y such as higher value in mental health, greater likelihood of regular utilization of prescription drug, higher OOP payment or higher mortality rate. The term $Post$ denotes post-MD waves, depending on the outcomes we observe therefore the onset of $Post$ might be equal to 2006 or 2008. As for drug utilization and OOP, the ambiguous survey design potentially covers both pre-2006 and post-2006 data, thus we eliminate wave 2006 for these two specific outcomes. T is the waves that $Post$ might theoretically cover at large, depending on the length of periods we are concerned about. As such, the broadest spectrum that T covers is termed as the time set $\{2006,2008,2010,2012,2014\}$, which is the long-run definition. The narrowest spectrum T covers is termed as the set $\{2006\}$ or $\{2008\}$, which is the short-run definition. By the selection of different values of T, we obtain short-run or long-run result respectively.

$$\boldsymbol{y_{it} = \beta_1 + \beta_2 \cdot Post \cdot I_D + \sum_{t=2002}^{T} \beta_{3t} \cdot Year_t + \beta_4 \cdot I_D + \delta \cdot X_{it} + \epsilon_{it}} \quad (1)$$

By design, the DD estimator assumes that no other unobserved factors are systematically driving the differential trends in well-being between the MD-eligible and MD-ineligible population, which is generally called the parallel time-trend assumption. The DD model estimates the intent-to-treat effect, and the eligibility for enrolling in MD is based on the age characteristic of the patients. However, due to heterogeneities, a certain fraction of MD-eligible patients might choose not to participate in the program and the non-participation might be correlated with some factors that researchers do not observe. Therefore, we also estimate the local average treatment effect (LATE) through an instrumental variable (IV) model and present the corresponding result. The technique is based on the methodology in the study by Ayyagari and Shane (2015). The instrumented explanatory variable is one dummy variable indicating PDC and the coverage of drug tends to be endogenous. We employ the interaction term of $Post \cdot I_D$ as the instrumental variable. The exclusion restriction assumptions are: (1) the IV is correlated with the endogeneity of drug coverage, as post-MD more eligible patients are more likely to opt in the program; (2) the instrument is uncorrelated with un-observables as it is exogenously determined.

As discussed so far, without the formal implementation of MD, we assume that there is a parallel time trend for both eligible and ineligible cohorts. To justify this, we test whether the parallel trend assumption holds by testing the model in (2). The coefficient λ_{1t} captures the difference between the eligible and the ineligible individuals within the same wave and the magnitude of the p-value illustrates whether the trend is parallel or not. The dependent y denotes any of the four concerned outcomes. All the other covariates are similar to those previous discussions. Literature reveals that income is prone to be endogenous as individuals with higher income might have better access to healthcare and thus potentially have better health outcomes than otherwise

(Cardak, 2004). To check whether this endogeneity happens in a sizable way, we also include log of the household income to perform a robustness check.[5]

$$\boldsymbol{y_{it} = \lambda_{0t} + \sum_{t=2002}^{T} \lambda_{1t} \cdot Year_t \cdot I_D + \sum_{t=2002}^{T} \lambda_{2t} \cdot Year_t + \lambda_3 \cdot I_D + \eta \cdot X_{it} + \epsilon_{it}} \tag{2}$$

5 DATA

The Health and Retirement Study (HRS) was designed to follow age-eligible individuals (mostly over 50) and their spouses as they made the transition from active workers into retirement and obtained detailed information in a variety of domains: demographics, health status, housing, family structure, employment, income, health, and life insurance. The data used in our research come from 2002-2014 waves RAND V.P including the period both before and after the introduction of MD, which makes the observation over time feasible. The data collection period, which reflects the timing of the interview, varies for each wave. For wave 2004, the interview was conducted from March 2004 through February 2005. The 2006 interview was conducted from March 2006 through February 2007 and it covered February 2008 through February 2009 for the subsequent 2008 interview. Wave 2004 and earlier waves are before the introduction of MD; wave 2008 and later are post-MD implementation. Wave 2006 is an exception as it potentially covers both post- and pre-2006 data due to its past two-year polling in the survey questions. Due to this ambiguousness, we, therefore, eliminate wave 2006 for the two outcomes of OOP and drug utilization. We cluster the regression at the household level.

[5] Household income includes the correspondent and the spouse.

Our final sample includes the following: 24053 observations for mental health, 19843 observations for drug utilization, 15930 observations for OOP, and 3888 observations for mortality respectively.

5.1 HEALTH

We study the mental health of the respondents in this paper. Mental health measures the depression level, based on the Center for Epidemiologic Studies Depression (CES-D) scale (D. E. Steffick, 2000). The CES-D score is commonly used to indicate the level of mental health and it is the summing of five "negative" indicators minus two "positive" indicators. The negative indicators are utilized to measure whether the respondent experienced the following sentiments all or most of the time: depression, everything is an effort, sleep is restless, felt alone, felt sad, and could not get going. The positive indicators are employed to measure whether the respondent felt happy and enjoyed life, all or most of the time. The scale of CES-D in this study varies from 0 to 8. By the nature of its definition, the higher score of CES-D thus denotes a lower level of mental health in the past week.

5.2 DRUG UTILIZATION

In the HRS survey, one direct question asks the regular prescription drug utilization as follows: “Do you regularly take prescription medications in the past two years?” The answer can be “Yes” or “No”. We treat yes-response respondents as regular utilization and set to one, and zero otherwise.

5.3 OOP

OOP is based on the question inquiring about the monthly premium and measured in U.S. dollars, “On average, about how much have you paid out-of-pocket per month for these prescriptions since the last interview/in the last two years?" MD covers prescription drug and thus post the implementation, the utilization of prescription drug is expected to rise.

5.4 MORTALITY

In HRS, there is a question asking the death year of the correspondent, we redefine the mortality status equal to one if the death year is equal or earlier than the interview year and equal to zero otherwise.

5.5 OTHER COVARIATES

Socioeconomic status (SES) can have a positive effect on health status and demonstrate social causation, as is evidenced by (Hay, 1988). We choose the covariates potentially impacting the well-being of the patients such as age, age squared, gender, race, marital status, and education. In the robustness check, we supplement the log of the household income too. There are variables indicating the start-year interview and end-year interview. Part of the interviews continued to the next year, from which the correspondents' ages are calculated. Age squared is added for the purpose of capturing any non-linear correlation between MD and health outcomes. We discard records with missing values for these covariates.

6 RESULTS

6.1 SUMMARY STATISTICS

Table 1 illustrates the summary statistics by year for patients' health outcomes from 2002 to 2014. Table 2 is the summary by MD eligibility.

From Table 1, we can compare the evolution of outcomes over time. For instance, prior-2006, the mean value of mental health is 1.611 in 2004 and post-2006 it gradually reduces from 1.659 in 2006 to 1.543 in 2014. As for regular drug utilization, the fraction of patients increases from 93.9

percent in wave 2004 to 94.9 percent in wave 2008.[6] In the first year of MD implementation, MD take-up is higher than any other single wave, reflecting the potential demand for healthcare coverage.

Taking from the national level, the OOP threshold for catastrophic coverage (patients pay a relatively small coinsurance amount or copayment for covered drugs) increases steadily from $3,600 in 2006 to postulated $5000 in 2018 since MD's initial onset (The Henry J. Kaiser Family Foundation, 2018). In contrast, HRS shows that the expenditure on OOP reduces over time, which peaks at 174.91$/month in 2004, thereafter it gradually reduces to a monthly payment of $80.64 in wave 2014.

Table 2 presents the summary statistics by MD eligibility. It is evident that the average age of the MD-eligible cohort is older than MD-ineligible cohort (69.1 years old versus 58.4 years old). Generally, the patients in the eligible cohorts have lower household incomes, higher ratios of less-educated individuals and lower ratios of married individuals.

6.2 TEST OF THE PARALLEL TRENDS ASSUMPTION

First, Table 3 shows the results of the test of the parallel-trend assumption for each outcome. The reference wave is 2014 for mental health, drug utilization and OOP and the reference wave is 2012 for mortality due to non-availability of death incidence in wave 2014. The parameters investigated here are the interaction term between the MD-eligible cohorts and each wave. As such, we measure the difference between MD-eligible and MD-ineligible patients during the specific wave. The results for $I(Age \geq 65) \times 2002$ and $I(Age \geq 65) \times 2004$ are our major concerns.

In general, the parallel trends are insignificant at conventional levels. For all the outcomes, the estimated results illustrate that the coefficients for $I(Age \geq 65) \times 2002$ and $I(Age \geq 65) \times 2004$ are all

[6] Note that the mortality rate is zero in 2014, and the dependent variable is a discrete dummy indicator thus we eliminate this wave from the sample for mortality-related analysis.

close to zero, indicating that before the MD implementation the trends of outcomes are parallel for the MD-eligible and MD-ineligible cohort.

6.3 RESULT FOR MENTAL HEALTH

We estimate the effect of MD on mental health both in the short term and in the long term based on the specification (1). The results are summarized in Table 4. Since income tends to be the endogeneity potentially impacting the enrollment of MD and health, we add additional control covariate- log of household income- to the baseline models and check the sensitivity for all the outcomes, which is illustrated in Table 5. After controlling this variable we find that our estimate results are robust to this alternative specification. The results of headlines including the subsequent outcomes in the tables are the dependent variables that we are concerned about and these are separate estimation with different post-MD periods. The coefficient for Post×I(Age≥65) is our main coefficient of interest.[7]

Column (1) denotes the post-MD period including wave 2006 only, while column (5) denotes the post-MD period including 2006-2014 waves. Other columns can be deduced in an analogous way. First, the sign of the coefficient for Post×I(Age≥65) is positive and statistically significant for all the periods. MD significantly improves the mental health of patients either in the short run or in the long run. The magnitude of impact grows from -0.189 in wave 2006 to -0.203 in wave 2012. The prior-2006 average mental health for the MD-eligible cohort is 1.624, implying that in the short run patients' mental health is improved by 11.6 percent; while in the long run, the improvement effect is 12.5 percent after the introduction of MD. These improvements are sizable and thus economically significant too.

[7] Other than high-school-or-less and college-or-higher education, there exists other type of education in the HRS but with only one observation in wave 2014, we eliminate this sole one record from the sample.

For other covariates, age contributes to mental health positively and it is statistically significant for the majority of the waves. Mental health increases with age. Age squared is added to check for any potential of nonlinear effect on mental health. Compared with White/Caucasians, Black/African Americans and other race have worse mental condition both in the short-run and in the long-run perspective. The similar trend is observed for women and the cohorts that are not married. In contrast, college or higher educated individuals have better mental health than less-educated patients. The long-run impact is generally consistent with the short-run impact. The positive relationship between MD and mental health evolves over time in a consistent way, implying an accumulating effect.

The impact through the channel of education is consistent with the study by Chevalier and Mayzlin (2006), who show that education reduces the transition to depression as well as substantial returns to education in terms of improved mental health. And all these findings are not in conflict with Ayyagari and Shane (2015).

6.4 RESULT FOR DRUG UTILIZATION

Further, we estimate the impact of MD on prescription drug utilization in the past two years, for which the results are listed in Table 6.

The most important finding is that the coefficient for Post×I(Age≥65) is positive for all the post-2008 waves and statistically significant beginning in 2010. MD does not facilitate drug utilization in the short run but the impact is significant in the long run. The magnitude of impact implies that post MD, the likelihood of growth in drug utilization increases from 1.3 percent in 2008 to 2.8 percent in 2014, indicating an accumulating over-time effect. The prior-2006 average level of drug utilization is 95.4 percent for MD-eligible patients. The economic magnitude is relatively small in contrast with the level of statistical significance.

MD-eligible patients tend to use drugs more regularly than what they did previously. And this trend is progressing over time. The long-run impact is more prominent than the short-run impact. The utilization of drug increases with age. Females tend to use drug more regularly than males. Minorities such as Black/African Americans and married patients are more likely to increase utilization in the long run. The effect on drug utilization does not differentiate through the channel of education.

6.5 RESULT FOR OOP

In Table 7, we obtain the DD outcome for OOP. The DD coefficient for Post×I(Age≥65) is negative over time. It is not statistically significant in all waves. MD does not significantly reduce the financial burden for patients either in the short run or in the long run. However, based on the magnitude of the coefficients, the reduction of OOP is substantive compared with the prior-2006 average value of 168.17, it decreases OOP for MD-eligible patients by about 13.6 percent in the short run (wave 2008) and about 11.03 in the long run (wave 2014). The impact is economically significant for all the periods though.

The impact on OOP presents a unique characteristic different from the applied work, implying that these patients have a differentiated structure of OOP compared with the cohorts in other studies. Women steadily face more severe financial stress than men, as is the same for White patients. The effect on OOP does not differentiate through channels like marital status or education.

6.6 RESULT FOR MORTALITY

Mortality is one major concern in many health insurance reforms. We present the short- and long-run impact on mortality in Table 8. The DD coefficient for Post×I(Age≥65) is negative for all periods but none of them is statistically significant. MD does not improve the mortality for the patients either in the short run or in the long run. From the magnitude of the coefficients, the reduction of mortality is sizable compared with the prior-2006 average value of 1.68 percent. As it reduces mortality for MD-eligible patients by about 1.8 percent in the short run (wave 2008) and about 1.1 percent in the long run (wave 2014). MD seems to have no statistically significant impact through the channel of mortality. One possible explanation for this is the patients in this study generally are either severely or acutely ill cohort needing immediate, persistent or effective assistance, while prescription drug covered by MD can only meet the partial end of demands.

6.7 IV RESULTS

We employ the interaction term of Post×I(Age≥65) as the instrument variable for prescription drug coverage and perform the test of IV specifications for all the outcomes. Results are illustrated in Table 9 to Table 12.

In Table 9, we compare the results of the OLS regression with IV model for mental health, presenting first-stage results of 2SLS. The assumption is that IV is strongly correlated with the endogenous variable but in the meantime not correlated with the unobservables. PDC denotes Prescription Drug Coverage and PT denotes the term Post×I(Age≥65).

The instrument is significantly correlated with PDC in the first stage as the F statistics evolves from 33.40 in the short run to 1646.55 in the long run. In the OLS regression, we find a positive association between PDC and mental health level. When the endogeneity is controlled for, we find that MD is associated with significant growth in mental health either in the short run or in the long run. The significant results in the IV model are generally consistent with the previous finding in the DD model. Most R-squared values in the IV model remain at commensurate levels to those in the DD model. One counterfactual finding is that immediate post-2006, the magnitude is larger than any other periods, one possible explanation is that this is a transitional period of health plans, therefore more fractions of patients transfer to MD and thus the expectation of health improvement is larger than the non-transition period.

A similar comparison of regular drug utilization is summarized in Table 10. The instrumental variable, which is Post×I(Age≥65), is consistently highly correlated with prescription drug coverage as the F statistics are greater than the "rule of thumb" value (10 in most cases) for all the waves. The IV result is consistent with the DD result. MD significantly facilitates regular drug utilization for the patients in the long term. We do observe a similar impact in the short term though.

Table 11 shows the results of OOP in the comparison of IV and OLS. The impacts of the direction and the levels of significance are consistent with our DD model. It is not statistically significant for all the waves but economically significant. On the other hand, the level of significance in OLS is larger than IV and DD. We also present the first-stage outcome of 2SLS, among which all the F-statistics are well above the "rule of thumb" value, and therefore the exclusion restriction assumption of the IV is satisfied.

In Table 12, we find that mortality result remains at insignificant levels, as in the DD estimate. Except for the one below-ten F statistic in wave 2006, all others are well above 10. OLS and IV both support that MD neither improves the mortality in the short run nor in the long run at conventional significance levels.

6.8 ROBUSTNESS CHECKS

To check how sensitive our estimated results are, we supplement alternative specifications and compare the sensitivities for the outcomes in all periods. The results are shown in Table 13 to Table 16. First, we change the fixed effects of the year in the baseline model to an indicator of the post-MD dummy and apply this to all subsequent sensitivity checks. In the second alternative specification, we further add fixed effects of census divisions.[8] In the third set of robustness test, we supplement the census-division level average unemployment rate. Finally, we add the interaction of post-MD indicator and average unemployment rate. We find that the results are robust to these alternative specifications. The estimated outcomes are essentially unchanged both in the short run and in the long run.

6.9 PLACEBO TEST

In the first set of placebo check (not shown), we assume that the 57-64 years old cohort is qualified for MD yet the other 50-56 years old cohort is not, then estimate the same DD baseline model. We do not find statistically significant differences between these two assumed "treatment" and "control" group. None of the DD coefficients for all the outcomes is significant either in the short run or in the long run. We then advance to the second set of placebo test, where we assume that MD is available to the previously defined 65 or older MD-eligible group in wave 2004, all the other conditions are unchanged. By employing the same baseline DD model, we find a consistent insignificant result for all the outcomes. This further justifies the conclusion that the estimated impact in mental health improvement, drug utilization growth, OOP and mortality is attributable to the implementation of MD and the ineligible group is systematically unaffected.

7 CONCLUSIONS AND DISCUSSIONS

First of all, our research suggests that Part D reform does improve severely ill patients' well-being for certain outcomes. The benefits are MD significantly improves mental health and increases regular drug utilization; countering this, MD neither improves OOP nor mortality over time. MD sizably increases patients' mental health both in the short run and in the long run.

[8] Exclude the one census division that is outside the U.S.

Improvements in mental health are observed in 2006 and later. Significant improvement is observed for regular drug utilization but only in the long term. Our finding of mental health is consistent with the documented literature (see e.g., Kaestner et al., 2010; Ayyagari and Shane, 2015), suggesting that the mental health and drug utilization are important channels where MD works well. Nevertheless, both have an economically sizable effect. The sample in this study has more severe patterns of diseases that completely differ from many others. Another caveat is that our study focuses on a limited range of near retirement patients only. How the characteristics vary for other age range might need further investigation.

The study contributes to the literature by examining the impact on patients' well-being, including mental health, drug utilization, financial burden, and mortality from the onset of MD to a relatively long time later. These vulnerable cohorts attract rare concern albeit they might essentially need more attention. We provide evidence that MD does not statistically mitigate the financial burden for the most vulnerable population. Insurance reforms typically are characterized by a lagged effect, indicating that the effect might not be evident until a certain period of time passes. The finding implies that we cannot neglect the potential that the impact might either come into real effect over time or might not be accumulated to a significant level. While comprehensively evaluating reforms of health insurance is one of the major concerns, either observing only short-term effect or observing only long-term effect could be incomplete. Our study quantifies the value of impact over time through observations in a relatively long period, which has not been explored in other studies so far. One implementable avenue in the future is that whether it is attributable to the specific characteristics of the United States, or other countries with similar healthcare system illustrate analogous effects; or whether less severely ill patients trend in a similar way.

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Chapter II

Parent's Health Insurance and Informal Care: Complement or Substitute?

ABSTRACT

This chapter investigates the impact of informal care provided by the children on the take-up of health insurance by the near-elderly and elderly parent, and how the relationship is affected by parent's Activities of Daily Living (ADLs) and Instrumental Activities of Daily Living (IADLs) disability level. How to purchase an optimal cost-efficient portfolio of health plans is one of the major decisions the aging individuals might face in their senior age due to the inherent tradeoff between plans. Our estimate accounts for unobserved heterogeneity by employing individual-level fixed effects and instrument variables (IVs). The results suggest that when the endogeneity is not controlled, neither public nor private plans take-up is sizably affected. In contrast, when the endogeneity is controlled for, informal care systematically crowds out the take-up of private long-term care (LTC) insurance whereas "crowds in" the take-up of total supplement plans (TSP). However, both crowding-out and "crowding-in" effect are reduced if the severity of ADLs/IADLs disability level increases. Our finding indicates one of the reasons the private LTC market shrinks is attributable to the substitutional informal care provided by the children. It also reflects the demands for additional insurance coverage. The results are robust to alternative measures of informal care.

Keywords: Health Insurance, Informal Care, Long-term Care, Crowd In, Crowd Out, Substitute, Complement

JEL Classification: G4, I1, I190, Z1

1 INTRODUCTION

In this study, we concentrate on how informal care (IC) provided by the children affects the take-up of health insurance by the parent.[9] To evaluate how the results vary with different measures of informal care, we observe the impact by informal care presented in multiple ways in this study: (1) specific supports for Instrumental Activities of Daily Living and Activities of Daily Living (IADLs/ADLs); (2) the total hours of help provided by the children last month; (3) the total days of help provided by the children last month. The employment of these different measurements serves the purpose of comparing the extensive and intensive margin of informal care. The effect of informal care provided by the junior generation can be pronounced on the senior generation. One typical instance is that it could affect the take-up decision of the health insurance bundle, such as private and public plans, for the seniors.

Private long-term care (LTC) insurance is a formal care plan that mostly helps people unable to care for themselves for a relatively long period. LTC insurance covers long-term care services and supports promoting independence or quality of life, including personal and custodial care (e.g., non-skilled care) in a variety of settings such as the home, a community organization or other facility.[10] Although the cost of LTC insurance depends on the type, the duration, and the care provider, generally it is expensive compared with other types of health insurance(Brown and Finkelstein, 2009).

There is much concern of uncertainty of being medically dependent and thus needing care when an individual is aging over time, the extent of disability and the long-term care caused (Barr,

[9] Literature might use different measure of informal care. For instance, Bonsang (2009) employs the average total number of hours of informal care received from the children. Since it collects only no more than three informal caregivers, thus the total effect might be underestimated accounting for all received informal care. Our study addresses this issue by incorporating all informal caregivers, and the result is consistent when employing the total time of support provided by the children.

[10] The typical services in LTC include custodial and non-skilled care, such as assisting with normal daily tasks like

2010). Thus far, the take-up rate of LTC insurance is low, which is in sharp contrast with the fact that the likelihood of depending on LTC services is high for aging individuals. Contrary to the paid formal care, the informal care provided by the children usually incurs no additional expenditure. Therefore, if informal care can substitute the services covered by the LTC insurance, a sizable crowding-out effect (i.e., the increase in the provision of informal care causes a reduction in the take-up of LTC insurance) is expected.

That being said, LTC insurance is shown to be incomplete insurance (Costa-Font and Courbage, 2015) and individuals might fail to purchase (see e.g., Meier, 1999; Pestieau and Ponthiere, 2010). The issue has been investigated in a broad context, encompassing both many European countries (e.g., Costa-Font and Courbage, 2015) and the United States (e.g., Brown and Finkelstein, 2011). The former finds trade-offs between the insurance take-up and informal care for individuals who have the need for LTC but without sufficient funds to cover their needs whereas the latter study indicates that the features of public insurance impose an implicit tax on private insurance and crowd out the purchase of private plan like LTC insurance.

In this paper, we aim to identify whether there is any crowding-out (i.e., the substitute) or "crowding-in" effect (i.e., the increase of informal care causes the take-up of public plans to increase accordingly or complement) by informal care on the take-up of health insurance, including both public and private plans. [11]Uncovering the relationship is meaningful as the cost of health plans and optimization of the purchased bundle is often the time one major concern. Public debate intensifies over how to restructure public plans to better address the demands of an aging population while also limiting cost growth (Langa et al., 2001). In the meantime, LTC expenditure grows by more than 100% as a percentage of GDP in many European countries and it is postulated to

dressing, feeding, using the bathroom.

[11] As private LTC is one special case of private plans and is measured independently in our study, we separate it from

remain the same trend until the middle of this century (Comas-Herrera et al., 2003). As such, one suggested solution to decelerate the growth in LTC expenditure is to encourage the provision of informal care provided by the family members to their frail parents. The family is a major source of care for elderly individuals and it is less costly than many formal care arrangements. This solution may lessen LTC expenditure only if informal care is an effective substitute for formal care (Bonsang, 2009). Our study complements the work by showing that the substitution effect (or equivalently, crowding-out effect) on private plans might be accompanied by the complement effect (or equivalently, crowding-in effect) on total supplement plans (these include the supplement plans of private and public insurance) if informal care as a whole substantively reduces the financial burden of the near-elderly and elderly individuals. Thus it is especially fulfilling for many developing areas or financially restricted households in industrialized countries, where individuals do not have sufficient health care coverage or where the insurance market is proven to be incomplete.

The chapter proceeds in the following direction. In section 2, we comprehensively evaluate the previous work that has been completed. Section 3 introduces the empirical framework. In Section 4, we present the data. Results, as well as the robustness check, are shown in section 5. And the policy implications and future extensions are discussed in closing.

2 PREVIOUS RESEARCH

LTC incurs a vital financial risk for the elderly (Huber et al., 2005), the impact of which on the seniors and the juniors can be detrimental. When the elderly dependents exhaust available resources to exploit insurance-covered health service, they might ultimately resort to other feasible help such as the unpaid informal care by their children (Courbage and Eeckhoudt, 2012).

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the general terms of private plans.

That said, applied work has shown that informal care can impact both the benefactors and the beneficiaries. Informal care-relatives, family or friends providing mostly unpaid care support to frail parents compared with those formal care that is mostly paid help-is critical for the adequate continuum of care between informal and formal care (Sinha et al., 2013).

When making the decision of purchase of LTC insurance, the parent might consider the likely response on the children's side such as when caregivers are the children who earn relatively low wages (Zweifel and Strüwe, 1998). Most informal carers are women with a low educational level, low social class or even without employment. Family caregivers might represent an alternative source of care (Pauly, 1990). Although a private LTC market is universally available in the U.S., only a small proportion of people essentially hold these plans.

The market for private LTC insurance does not expand as promisingly as other insurance markets due to its high cost or idiosyncratic reimbursement policy embedded. One evidence is that the average annual expenditure of formal care approximates 60,000 dollars (Kassner, 2004). For those who enter nursing home care market, almost 17 percent of individuals spend more than three years there. These stays are unexpectedly costly: one-year staying in the nursing home typically costs more than $50,000 for a private room and increases over time (MetLife Mature Market Institute, 2010). The national average costs for LTC in the U.S. in 2016 indicate that: (i) $253 a day or $7,698 per month for a private room in a nursing home; (ii) $20.50 an hour for a health aide; and (iii) $20 an hour for homemaker services such as helping with cooking or running errands.[12]

As when becoming dependent is uncertain, the expenditure in LTC varies to a large extent, standard economic models propose that risk-averse individuals place high importance on the capacity to tackle these shocks through insurance (Brown and Finkelstein, 2008). However, the scale

[12] The summary is based on the data from U.S. Department of Health and Human Services. The rates are charged by non-Medicare certified agents.

of this market is small in the U.S., only four percent of LTC expenditures are cost-shared by private plans, with remaining one-third paid by out-of-pocket payment (OOP).

The relationship between LTC and informal care can be rather mixed and complicated though (Courbage and Eeckhoudt, 2012). Help provided by the children to the parent who suffers from adverse health or disabilities might play a crucial role in avoiding the high costs of in-home and in-institution care (Ermisch, 2014). Therefore, it can serve as an effective substitute for formal LTC, which usually operates in paid domestic services or institutional care in the case where care demands of the elderly are relatively low or are not skill-dependent. Informal care can delay nursing home entry (Charles and Sevak, 2005). On the other hand, informal care might be a complement to LTC or paid health care likewise (e.g., nursing care) (Bonsang, 2009) or to doctor and hospital visits (Bolin, Lindgren, and Lundborg, 2008). For instance, seniors who use paid home care service usually are recipients of informal care as well (Canadian Institute for Health Information, 2010). On the opposite side, formal community-based care can drive down the utilization of informal care too(Christianson, 1988). If informal care increases the marginal effect of insured formal care, they can be a complement to each other.

There might be multiple factors impacting the provision of informal care. Pauly (1990) develops an intra-family moral hazard model and suggests that the provision of informal care by the children is endogenous as the extent of LTC purchased is correlated with the extent of provided informal care or the reverse. This relationship directly affects the bias of the crowding-out effect. Under a vNM utility framework, Courbage and Eeckhoudt (2012) propose a model showing that the optimal level of insurance is correlated with the level of informal care, the opportunity cost, the cost of formal care and the share in the bequest when the premium is paid by the parent and children regardless of altruism level.[13] In reality, when the parent has multiple options to select, the optimal insurance might be related to the quality of those services as well.

Our contribution to the literature comes from several channels. The first is that little is known about the "crowding-in" effect if any exists. Even less work discusses both "crowding-in" and crowding-out effect with empirical evidence in a combined way. For instance, Langa et al. (2001) find a complementary relationship between the increase in formal home health care and a relatively high level of social support. Bolin, Lindgren, and Lundborg (2008) find informal care is complementary to doctor and hospital visits. Bonsang (2009) also suggests that informal care decreases low-skilled home care utilization while serves as a complement to high-skilled home care. If informal care reduces the marginal benefit to the health of medical care, the relationship is a substitute. Contrastingly, if informal care increases the marginal benefit to the health of medical care, their relationship is a complement. For the former substitute case, one typical example is that the parent may value paid home or institutional formal care less if a child caregiver is available to support with informal care. For the latter complementary case, it might apply in both inpatient and outpatient care. In the hospital, an informal caregiver serving as an advocate may improve the quality of skill-dependent formal care because the caregiver could help capture the errors in medicine administered or notify the medical staff anything going wrong. For outpatient care, a child informal caregiver may improve a parent's health by transporting the parent to medical appointments. (Van Houtven and Norton, 2004). However, these are the only two studies we found discussing both substitution and complement effect. On the other hand, informal care might observe no statistically significant effect (Mellor, 2001). More others find a substitution effect between informal care and formal care (see e.g., Pezzin and Schone, 1999; Van Houtven, and Norton,

[13] In reality, a child might purchase formal long-term care for the parent if the opportunity cost reaches the extent that it exceeds the cost of the paid formal care itself (Hanley, Wiener, and Ham, 1991). Specifically, when children are

2008; Bolin, Lindgren, and Lundborg, 2008). Our study aims to complement the documented literature by uncovering the coexistence of the substitution and the complement effect.

The second contribution is to investigate how these effects are influenced when the level of ADLs/IADLs disability of the elderly person changes. Literature finds that at a low level of disability the relationship between informal and formal care is complementary, but for highly handicapped individuals the role might shift to a substitute (Bonsang, 2009). The third is the distance-based IVs incorporating co-resident case. Distance-based IVs are employed in the past work (Bonsang, 2009) but co-residency is excluded. In the meantime, Bonsang (2009) uses the number of sons, the number of daughters of the respondents and their interaction with the informal care as the instruments, restraining the sample to above-65 years old individuals without children. One concern is that the first-stage statistics are not reported, thus it is difficult to evaluate the potential weakness of the IVs if any presents. In the case of weak identification, the point estimate is distorted and the results of two-stage least square (2SLS) might be misleading due to bias. The fourth is that much work examines the post-enrollment healthcare utilization for aging individuals, whereas our study explores the take-up or the enrollment of the healthcare plans. And the fifth is the measured informal care presents in multiple ways, all of which are robust by comparing the results of alternative measures.

Our estimates are divided into two clear-cut sections: (i) crowding-out effect consistent with the previous work and “crowding-in” effect for total supplement plans seldom investigated so far; (ii) how the effects are compromised by the disability level.

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financially responsible for their parents, in such case the cost of care enters the child’s concern.

3 METHODOLOGY

In this study, we concentrate on the impact of children's informal care on parent's health-relative events such as the take-up of health plans and the cost of healthcare. The model is characterized by the following (fixed-effects model):

$$\boldsymbol{Y_{it} = \beta_1 + \beta_2 \cdot IC_{it} + \beta_3 \cdot Year_t + \delta \cdot X_{it} + \beta_4 \cdot \alpha_i + \epsilon_{it}} \qquad \textbf{(1)}$$

Where dependent variable Y denotes total supplement plans, private plans, LTC insurance or OOP of parent i at time t. IC is the indicator of children's informal care, including care for ADLs , care for IADLs and amount (hours/month, days/month) of help by the children. Both IADLs and ADLs care are binary variable equal to zero coded as no ADLs/IADLs informal care by any child and equal to one coded as at least one child supports with ADLs/IADLs care in the case of extensive measure. And in the intensive scenario, informal care is the time of help provided by the children during the last month. α and $Year$ denote individual- and wave- level fixed effects respectively.

Covariates X are the controls including demographics, family characteristics and socioeconomic such as parent's age, parent's age squared, parent's marital status and labor status, as these covariates affect either health insurance take-up of the parent or the availability of informal care. Coefficient β_2 captures the impact of informal care on parent's health care take-up, both publicly and privately.

3.1 ENDOGENEITY PROBLEM

We treat informal care provided by the child as endogenous. [14] The baseline model is defined in the specification (1) accounting for individual fixed effects (FE). While FE could absorb

[14] Applied work treats informal care as both endogenous and exogenous. For endogenous case, one can refer to the studies by Van Houtven, Coe and Skira (2013) or (Bolin et al., 2008),whereas for exogenous case, one can refer to the

time-invariant effects, it could not eliminate the potential collinearity between the informal care and other omitted factors. There might be certain un-observables impacting both the parent's take-up of the plans and the children's provision of informal care such as the altruism of the children or the parent. Capturing the degree of bias is difficult in some cases though. The level of informal care provided by caregivers may depend on the extent of health plans held by the parent, which Pauly (1990) terms as an intra-family moral hazard. Instrument Variable (IV) is an alternate methodology to mitigate the endogeneity problem (Costa-Font and Courbage, 2015).

The endogenous variables include the informal care provided by the children. The informal care presents in three ways: (1) ADLs/IADLs informal care by any child; (2) the number of hours children helped the parent last month; (3) the number of days children helped the parent last month. To differentiate the source of impact, the first case of help profile of informal care is divided into three distinct categories: (1) help with ADLs; (2) help with IADLs; (3) help with both ADLs and IADLs. The results in case (2) and case (3) reinforce that the estimates of the case (1) and are robust to alternative measures of informal care. Thus how informal care is measured does not change the analysis essentially.

3.2 INSTRUMENT VARIABLE(S)

To address the endogeneity problem, we employ an IV model corresponding to equation (1), which overcomes both endogeneity and reverse causality issues. The IV for the endogeneity in the baseline model is the distance proximity to the closest child. In our study it is based on the question:" How near is the closest child?" and is categorized as (i) the child co-resides with the parent; (ii) the child lives within 10 miles from the parent; (iii) or lives over 10 miles from the parent. [15]

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study such as (Van Houtven et al., 2013). This study treats it as endogenous, as in the real world, child's informal care is more likely to be correlated with un-observables such as the altruism degree of the child.

[15] The other studies using distance proximity as the IV might not necessarily incorporate the case of co-residence (see e.g., Bonsang, 2009).

The exclusion restriction assumptions for the IVs are: (i) they are correlated with the endogeneity; (ii) uncorrelated with the random error.

While multiple studies show that distance proximity is an important variable for the provision of informal care although it might be endogenous to the supply of informal care (see e.g., Charles and Sevak, 2005; Stern, 1995). We check that the distance-based IVs in our study are not endogenous by showing that the children do not choose to provide informal care based on the parent's health level. By inter-acting the IV with ADLs/IADLs disability levels, it is illustrated how the effect is reduced when the parent's disability condition deteriorates.

4 DATA

The data used in our study come from 2002-2012 waves of the Health and Retirement Study (HRS) on a biennial basis. The survey reports a detailed longitudinal demographic, educational, and labor market information for the individuals aged 50 or older. We use the HRS Rand file containing cleaned and processed variables (Sonnega et al., 2014). Countering to the frail cohort used by many papers (see e.g., Boaz and Muller, 1994; Ettner, 1994; Jr., 1993), our sample is a cohort focusing on the near elderly having at least one living child.

The informal care provided to the parents in this study includes the care for ADLs and IADLs. ADLs are defined as the Activities of Daily Living indices, include bathing, eating, dressing, walking across a room, and getting in or out of bed. Correspondingly, IADLs include activities such as using a telephone, taking medication, handling money, shopping, preparing meals. For ADLs and IADLs, they are based on the following questions: ”Count the number of children who help with ADLs/IADLs.” We re-categorize these two similarly, set them equal to zero if the parent receives no ADLs/IADLs support from any child, and equal to one if the parent receives ADLs/IADLs support from at least one child.

4.1 SAMPLE SELECTION CRITERIA

To isolate other potential sources of informal care and thus concentrate on the effect of children's informal care, we restrain the sample to the 50-72 years old parents who have at least one living child and thus exclude those having no children at the time of the survey. The marital status of parents could be married, spouse-absent married, divorced, separated, widowed or never married.[16] These parents do not receive informal care from other relatives and do not utilize the nursing home service.[17]

4.2 DEPENDENT VARIABLES

Our dependent variables include the take-up of private LTC insurance, total supplement plans, private plans and OOP in the past two years.

(i) PRIVATE LTC

The private LTC insurance measure is based on the question:" Not including government programs, do you now have any long-term care insurance which specifically covers nursing home care for a year or more or any part of personal or medical care in your home?" It thus incorporates the nursing home and at-home health services taken by the individuals. Long-term care policies aim to provide payment assistance with ADLs/IADLs for individuals utilizing a long period of at-home or in-institution care (Brown and Finkelstein, 2009).[18] LTC dependency captures the needs for health care that arise from health problems or disabilities and is supposed to extend over a relatively long period of time.

(ii) TOTAL PLAN INCLUDING SUPPLEMENTS (TSP)

[16] Unknown marital status is eliminated from the sample.
[17] When parents do not reside in nursing home, on average they might not have been heavily depending on formal care service or too disabled to value the informal care.
[18] Wiener, Hanley, Clark and Van Nostrand (1990) have more detailed discussion about ADLs/IADLs and it might differ in measurement across countries.

As for the total insurance plans including supplement plans (hereafter TSP), the question is asked as follows: "Calculate the number of supplement plans for those with Medicare, or the number of private plans for those without Medicare". This variable represents all the plans mentioned but excludes LTC insurance.

(iii) PRIVATE PLAN OTHER THAN LTC

More specifically, the measure of the private plan other than long-term care (hereafter non-LTC private) is according to the response to the following question in the survey: "Now, we would like to ask about all the other types of health insurance plans you might have, such as insurance through an employer or a business, coverage for retirees, or health insurance you buy for yourself, including any (Medigap or other supplemental coverage). If the respondent has Medicare coverage and respondent receives Medicare/Medicaid through an HMO, do not include long-term care insurance. Other than your Medicare HMO you've just told me about, how many other such plans do you have? Or otherwise: do not include long-term care insurance or anything that you have just told me about. How many other such plans do you have?" An insignificant effect denotes that there is no sizable substitution relationship between non-LTC private plans and informal care.

(iv) OOP

For the outcome OOP, respondents are asked to estimate OOP medical expenditures since the previous interview for re-interviews or in the previous two years for new interviews.

For all the questions, respondents can choose to answer "Not known" or "Refuse to answer". We eliminate those observations with vague or missing values.

4.3 EXPLANATORY VARIABLES

Other covariates in the study include age, age squared, marital status, labor status of the respondent, and dummies of waves from 2002 to 2012 in the baseline specification. In the extended sensitivity check, we also include the log of the household asset, the log of the household income, number of living siblings, number of living children, drinking behavior, current smoking status and the self-rated health level of the parent. [19]

The labor status of the respondent is divided into two categories: either continuing paid work or stop working completely. The number of living children is re-categorized into three groups: no living child, one living child, and at least two living children. The number of living siblings is re-termed into two groups: no living sibling or at least one living sibling. With respect to drinking behavior, it is based on the inquiry asking whether the respondent ever drinks. For smoking take-up, it is equal to 0 if the individual answers no smoking in that wave and equal to 1 otherwise. Self-rated health level is based on the question as follows:" Would you say your health is excellent, very good, good, fair, or poor?" The response is divided into five categories: 1 "excellent", 2 "very good", 3 "good", 4 "fair", and 5 "poor".

5 RESULT

5.1 GENERAL SUMMARY

Table 17 and Table 18 present the preliminary evidence and summary statistics in our HRS data, separated by the informal care of IADLs and ADLs.

Table 17 divides the sample by informal care of ADLs. Long-term care insurance take-up ratio is less in the ADLs informal care than in the non-informal care sample (6.1 percent versus 11.0 percent). Whereas the number of total plan take-up is larger for the former (1.642 on average versus 1.550). The average age and the number of siblings are almost at the same levels. More individuals in the former subsample receive a high school or less education (93.6 percent versus 78.1 percent). The household income and asset for the latter are higher than the former, with an

[19] The household income and asset include the respondent and spouse only.

nual income $74009 versus $25714 and total household asset $432928 versus $111532. Generally, the ratio of parents reporting fair or bad health in the informal care sample is greater than the ratio in the non-informal care sample. More parents in the informal care sample have two or more living children.

In Table 18, we illustrate the sample by informal care of IADLs. The trend is similar as in the summary by ADLs, and LTC insurance take-up ratio in the informal care sample is smaller (4.7 percent versus 11.1 percent). The same trend is observed for the number of TSP take-up (1.652 on average versus 1.549). More individuals are less-educated in the informal care subsample (94.1 percent versus 77.9 percent). Their household income and asset are generally lower. And, more parents report fair or bad health in the informal care recipient subsample.

5.2 FIXED-EFFECTS RESULT

Table 19 presents the fixed-effects estimate of children's informal care on parent's take-up of health insurance, including private LTC insurance, TSP, private plans and OOP based on equation (1).

The majority of the results suggest that informal care has no significant effect on all the four outcomes of concern at the conventional levels, implying a neither crowding-out nor "crowding-in" effect by informal care on parent's health insurance take-up or expenditure when the endogeneity of informal care is not controlled for. We present the results by types of informal care: (1) providing ADLs care only; (2) providing IADLs care only; (3) providing both ADLs and IADLs care. For all the types of informal care, the result is consistently insignificant.

5.3 IV RESULT

The instrument variable (IV) we employ in the baseline specification is the geographical proximity from the parent to the closest child.[20] The assumption is that the IV is strongly correlated with informal care but uncorrelated with the unobserved random factors.[21] The IV specification results are shown in Table 20.

The estimated result implies that informal care provided by the children systematically crowds out the take-up of private LTC plan for the parent. It is consistent in all three types of discussed informal care. Generally, the magnitude of effect due to the informal care of ADLs is larger than that of IADLs. ADLs generally include basic living activities, indicating that this type of informal care has a stronger substitution effect on the corresponding formal care covered by insurance and is mostly less skill-dependent. Further, in the case of receiving both types of informal care, the magnitude of the crowding-out effect is larger than either type of informal care, suggesting the more informal care provided the more substitutional relationship between informal care and formal LTC insurance take-up. The add-up effect of two separate care is slightly larger than the case receiving both types of care but generally within a comparative level. Specifically, this is similar for the "crowding-in" effect on TSP. Countering to the effect on LTC plan, informal care counterfactually "crowds in" the take-up of TSP. As such, informal care serves as a complementary role for TSP. The effect is consistent with alternative measures of informal care.

Quantitatively, Table 20 also illustrates that in the case of extensive measure of informal care, receiving ADL informal care reduces the probability of the take-up of LTC insurance by 0.514 and the crowding-out effect is 0.357 for IADL care. In the case of providing both types of care, the

[20] The categories of the IV include: (i) co-resident; (ii) living within 10 miles; (iii) living more than 10 miles.
[21] We employ alternative IVs at later section based on the discussion of the endogeneity of the IV.

reduction effect in probability is 0.747. Contrastingly for the supplement plans, it increases their numbers on average by 1.478, 1.003 and 2.108 respectively.

The crowd-out effect on LTC insurance reflects the strong substitution relationship between informal care and LTC plan and the demand of cost saving by the parent, whereas the “crowd-in” effect reflects that parent’s demand of health services not covered or not readily provided by the private LTC plan. The IV results indicate that informal care serves as the substitution for LTC and complement for the supplement plans after the endogeneity is controlled for. Whether the quality of public plans such as a public LTC insurance is higher than the quality of a private LTC insurance, arouses a debate in the documented literature. Staffing levels are higher in publicly operated homes than private ones, whereas the latter is higher in processual quality (Winblad, Blomqvist, and Karlsson, 2017). Private long-term care plan usually operates under the framework of privatization; in contrast, the public counterpart usually operates accompanied by not-for-profit purpose. While privatization is one way to enhance efficiency, it has the issue of being more prone to lower quality levels for the purpose of reducing costs and generating more profit (Prizzia, 2001).On the other hand, not-for-profit nursing homes, on average, deliver higher quality care than do for-profit counterparts (Comondore et al., 2009; Aaronson, Zinn, and Rosko, 1994). In order to minimize the operation cost, the quality by the private market is generally low (Davis, 1993). LTC recipients in the not-for-profit sector have a lower death rate, lower infections and lower hospitalizations (Spector, Selden, and Cohen, 1998).[22]

To check how the estimated results vary when informal care is measured in different ways. We observe the case of informal care measured in hours of help per month and days of help per month provided by the children as well, the IV estimates from Table 21 to Table 24 are consistent with the case where informal care measured in ADLs/IADLs. Informal care crowds out enrollment of LTC insurance and crowds in enrollment of TSP. Thus, the outcomes are robust regardless of which types of informal care used.

Informal care significantly reduces OOP. This might be an integrated effect of crowding out of LTC insurance and "crowding in" of TSP. In general, the cost increase in TSP is offset by the cost reduction in private LTC. Informal care mitigates the financial burden for the parent.

5.4 INTERACTION EFFECT BY DISABILITY LEVEL

In Table 25 and Table 26, we check whether there is a differentiated effect based on parent's ADLs or IADLs disability level respectively. [23]

To achieve this, we introduce into the empirical model the interaction between informal care and the ADLs or IADLs disability level of the parent. As the model now has one extra endogenous variable and thus requires additional instruments as well, for which we add the interaction of the distance from the closest child and the ADLs/IADLs disability level.

Table 25 presents the impact on the health insurance take-up by informal care based on the level of ADLs disability of the parent. The coefficient for informal care is negative and significant, same as the previous section, indicating a sizable crowding out effect on LTC insurance at the presence of informal care. In contrast, the interaction term between informal care and the ADLs disability level is positive and significant, which implies that the substitution effect is reduced for the parent suffering from a higher disability level. The result of IADLs is similar.[24] In comparison, the direction of impact on TSP is reversed. The coefficient for informal care is significantly positive, which indicates a sizable "crowding in" effect on TSP and the negative and significant sign of interaction term between informal care and the ADLs disability level implies that the "complement

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[22] We are cautious to make the conclusion that the complementary or "crowd-in" effect is causal.

[23] The disability index is based on the number of ADLs/IADLs deficit. ADLs deficit levels range from 0 to 5, whereas IADLs deficit levels range from 0 to 3. The IADL criterion can be counted as a rough proxy for serious cognitive impairment.

effect" is reduced as the disability of the parent becomes more severe.[25] When it changes to IADLs, the outcome is consistent and comparable.

This finding is correlated with the characteristic of the sample. First, in our final sample, the total number of individuals is 19873 and the number of individuals with Medicaid, Medicare or TRI-CARE/CHAMPUS/CHAMP-VA (any of these plans provides public medical care for veterans and their dependents) 12251, the ratio of public plan coverage is 61.6 percent. If breaking out the coverage by age, the number of under-65 years old individuals enrolled in government-plan Medicaid is 1764 (coverage rate 11.4 percent); whereas for Medicare, the number is 1952 (coverage rate 12.6 percent). In contrast, the number of 65 or above elderly enrolled in Medicaid is 1253 (coverage rate 12.4 percent) and the number is 9829 for Medicare-enrolled elderly (coverage rate 90.9 percent). The average number of supplement plans for 65 or above elderly is 2.1 with a maximum number of 8 plans. And the average number of supplement plans for under-65 years old near elderly is 1.2. This implies that other than Medicare or private plans, on average the individuals possess at least one additional supplement plan, either public or private. The under-65 years old individuals might qualify for Medicare by health conditions due to disability.

Among the people having the public plan(s), the number of individuals who have at least one TSP plan (either public or private) is 12208 and thus the fraction of coverage is 99.6 percent. Almost all individuals with the public plan have one or more supplement plans. This reflects the mechanism that individuals potentially demand other types of health insurance not covered by the

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[24] Bonsang (2009) finds a similar mitigating effect on formal care when the disability level of the parent increases.

[25] Staiger and Stock (1997) suggest declaring instruments to be weak if the first-stage F-statistic is less than 10 with one endogenous variable. Stock et al. (2001) propose another procedure of rule of thumb for multiple endogenous variables, from which the characterization of the set of weak instruments is based on the included endogeneity, the number of instrumental variables, and the desired maximal bias of the IV estimator relative to OLS or desired maximal size of a 5% Wald test. Andrews, Moreira, and Stock(2004) also indicate that the assumption of normality can be taken away at the cost of having only asymptotically valid rather than exactly valid tests. The presented first-stage results in TABLE 9 and other relative tables indicate no weak-IV issue based on the 10% critical value proposed by applied work.

basic packages of the public plans, private LTC insurance or informal care. And the demand for coverage of cost-efficient health insurance could be urgent. In our sample, the number of publicly covered people having supplement plan and private LTC plan is 1920, indicating the ratio of coverage is 9.7 percent. This result is similar to the roughly 10 percent of private LTC insurance coverage for elderly individuals in the study by Brown et al. (2004). As such, the cohort satisfies the characteristics of below: the nursing care being primarily paid for through OOP, with a small portion of low-income residents receiving comprehensive LTC insurance through Medicaid, a larger portion of individuals using only limited coverage of LTC plan by Medicare, and remains using private LTC insurance or informal care by family support if they have access to these resources.

In the years when individuals are close to retirement, they will need retirement income to pay for health care, urgent care and skill-dependent care not covered by Medicare or other public plans. In addition, some needy elderly demand more LTC insurance such as plan covering ADLs or IADLs care that they cannot support themselves. Public and private programs pay for merely part of the cost of long-term care. Medicare provides limited coverage for LTC on the skilled nursing facility and home health benefits, which focuses on short-term or skilled nursing care and therapies. Medicaid provides broader coverage for LTC but is only accessible for low- income and assets households. Medicaid helps paid roughly 40 percent of the nation's total long-term care expenditure of $150 billion and 44 percent of the cost on nursing home care in 1998 (Feder, Komisar, and Niefeld, 2018). On the other hand, for those with limited or not qualify for public LTC coverage, the health insurance demands are not necessarily low. As such, they might search other channels of coverage to insure against the potential risks.

Second, whether an individual needs LTC insurance might be correlated with the level of dependency or disability. Limitation in ADLs or IADLs is defined as needing hands-on or standby assistance from others to maintain normal daily activity, and the limitation is expected to last for at least a couple of months. Informal care might be an option for those having children's support; however, the range of the care provided by the children is generally limited, mostly focuses on non-skilled support and covers a relatively short-term period. As when the disability level of the parent increases, the informal care by the children is considered to provide only limited substitution effect for the formal care. When a disabled parent is more dependent than usual case, he/she might require more help and the care usually lasts a longer period than usual. In a highly needy case, individuals are expected to utilize more skill-dependent nursing care with longer stays. As such, disabled parents are more likely to choose to stay in the LTC plan rather than opt-out from it to bailout from the dependency and the burden of healthcare cost.

For TSP, the coefficients on ADLs/IADLs are positive and significant, as indicated by the previous finding. This result consistently implies that more informal care is significantly correlated with more supplement coverage. Nevertheless, this "crowd-in effect" reduces when the disability level increases. This might indirectly reflect the fact that either public or private supplement plans can only address portion of the health insurance demands by the parent with a higher disability or the capacity of addressing health plan demands reduces when parent's disability level increases.

In Table 26, we calculate the disability index by employing the parent's IADLs difficulty levels. Generally, the results are consistent with the case when the disability index is measured by using the parent's ADLs deficiency levels. Informal care serves as a crowding-out effect for LTC insurance and the substitution effect is lower for the parents whose disability level is higher. The impact on TSP is the opposite: “crowd-in effect” for TSP but the “complement effect” is reduced with the growth of severity in parent’s disability level.

Table 27 and Table 28 are the cases where informal care denotes the hours/days that children helped during the last month and their interactions with the parent’s ADLs/IADLs disability respectively. The results do not change essentially though: informal care crowds out LTC insurance and “crowds in” TSP sizably. In the meantime, these effects are significantly reduced when the parent’s ADLs/IADLs disability level increases.

5.5 SENSITIVITY ANALYSIS

To check the sensitivity of our results, in Table 29 and Table 30, we perform the first set of robustness check of the baseline IV estimates. As income or wealth tends to be endogenous, we include these two confounders in the baseline specification to perform the first-round sensitivity test. The resulting outcome indicates it is robust to this alternative specification. Informal care systematically crowds out LTC insurance and “crowds in” TSP. The second concern is about the channel of informal care available to the parent. More living children and more living siblings are likely to impact how readily the parent can access support. Further, health-relative behaviors such as smoking and drinking behaviors tend to affect the health conditions of the parent, and they thus potentially affect the take-up of health insurance. In addition, risk-averse parents might purchase more health insurance due to poor health than otherwise. In the third set of sensitivity check, we incorporate the characteristics of the child such as his/her working status, whether an own child, whether a stepchild and the number of children providing health care support. All of these present consistent results as in the baseline specification: crowding-out effect on LTC insurance and “crowding-in” effect on TSP.

So far, the analysis does not include the parents living in a nursing home who potentially constitute a non-negligible proportion of formal care. It thus may affect the results if the decision to institutionalize an individual represents a substantive substitute of formal care for informal care (Pezzin and Schone, 1999). We then incorporate these individuals into the sample and re-perform the estimate on LTC insurance and TSP, the IV results as well as the corresponding robust check, are essentially comparative to the outcomes excluding the nursing subsample.[26]

One might be concerned about the potential that the children might relocate to live closer in geographic proximity to the parent or the reverse if the informal care from other relatives is not available, if the parent is poorer in health or if the healthcare is not available for the parent (Charles and Sevak, 2005). In this case, the parent's health affects children's relocation preferences. As such, the aforementioned IV based on distance tends to be endogenous. To check, we estimate the number of children living within 10 miles on parent's health levels. The health uses three measures: self-rated health level, ADLs disability level, and IADLs disability level. All the results in Table 31 imply no existence of significant selection effects. Therefore children do not choose to live nearer to the parent or vice versa based on parent's health conditions. [27]

The interaction terms in Table 27 and 28 indicate how the "crowd-in" and crowd-out effects change with parent's disability level based on the intensive measure of informal care. Generally, they are consistent with the distance-based IV results. Either effect is reduced when the parent's disability level increases. The result based on IADLs is similar to that based on ADLs disability. The result based on hours per month is similar to that based on days per month. All of these imply that informal care reduces the enrollment of LTC insurance and increases the take-up of TSP, and the direction of impact reverses when the parent becomes more dependent.

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[26] For conserving space purpose, the estimates are omitted but available upon request.

[27] Charles and Sevak (2005) also find that the probability of children's living within 10 miles is not higher when

6 CONCLUSIONS AND DISCUSSIONS

In this study, we investigate the concern of substitutional and complementary effects of children's informal care on parent's health insurance take-up. Our estimate operates through a fixed-effects specification and its corresponding IV concentrating on the United States market. The estimate indicates that informal care systematically crowds out private LTC insurance and "crowds in" supplement plans including public and private plans. Accounting for the 97.9 and 96.9 percentage points of individuals without ADLs/IADLs informal care, receiving ADLs/IADLs informal care reduces the take-up of LTC insurance by 50.3/34.6 percentage points for these individuals. And increases the average number of supplement plans by 1.45 and 0.97 respectively.

The effects are reduced when parent's ADLs/IADLs disability level increases. The effect of ADLs care is consistent with the effect of IADLs care, and it is also the case when informal care is measured in alternative ways. As when we employ the informal care measured in hours per month or days per month, the outcomes do not change qualitatively. On the other hand, our results suggest that the relationship between formal and informal care is sensitive to the dependency status of the parent, represented by the ADLs/IADLs disability indicator.

We find that when the endogeneity is not controlled, informal care does not crowd out any outcomes being concerned, and the financial burden of the parent is not affected in a significant way. Nevertheless, when the endogeneity is controlled for, informal care serves as the substitution for private LTC insurance and the complement for the public supplement plans. The magnitude of the effects is lowered when the parent's disability level is more severe. This reflects the strong demand for additional insurance coverage across the near-retirement elderly as they enter the senior age.

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regression on parent's health variables.

Further, the positive correlation with total supplement plans reveals that certain unobserved health or financial characteristic is positively linked to the informal care provided by the children. The informal care provided by the children is mostly not skill-dependent and limited in range and sometimes might even be accompanied by high opportunity cost, Thus, the substitute effect on LTC insurance reflects that the parent, on average, has the demand for not skill-dependent care. On the other hand, highly dependent parent, indicated by the ADLs/IADLs disability level, might not count on the support from the children. In contrast, he or she might resort to paid formal care to bail out from the dilemma. Further, the more take-up of supplement plans indirectly reflects the fact the care provided by the children cannot completely cover the healthcare demanded by the parent. Informal care increases the marginal benefit of total supplement plans. Our result about the complementary relationship to the supplement plans is not in confliction with the literature (see e.g., Harold, Houtven, and Norton 2008).

The finding in OOP is consistent with the reduction effect explored by applied work (see e.g., Ermisch, 2014). However, contrary to the "disincentive" of seeking insurance documented in the literature, our study indirectly indicates a new mechanism of active seeking of budget controlling and insuring against health risks. This is accompanied by crowding out LTC insurance and "crowding in" more public/private supplement plans.

This study is meaningful in part is that it formally uncovers the causal effect of informal care on LTC insurance take-up, the correlation with public/private supplement plans and clarifies the possible channels of effect under a family-interaction framework. We quantify how the impact is altered by the severity of the disability level. Our study provides vital two-fold implications for both public health policy and the private health market. The first one is consistent with the most-studied private LTC market, where policy might work well when subsidizing the informal caregiver such as the national family caregiver support program, or reimbursing the provider of private LTC in an appropriate way accounting for the potential issues such as moral hazard. The second is for the concern on the cost growth of public plans. Our study implies that the small-scale private LTC market might be attributable to multiple reasons: (1) commensurate level of informal care provided by the children; (2) high cost of the private LTC insurance. As more individuals deviate to public programs at the receipt of informal care, the financial burden for public financing increases. And how well these public supplements work in an incomplete market might need extra investigation. Public plans such as Medicaid have an interest in limiting their costs (Grabowski, 2007). Our study reflects the demand of cost reduction, increasing coverage of the extra health care for the near-retirement individuals and how these are affected when the individuals progress the degree of dependency. This continues to be the focus of research in the coming decades if more industrialized or developing countries join the list of the aging society.

Compared with the studies focusing on older than 70 of age cohort in the documented literature, the age range of our sample is generally younger and thus less frail on average. This cohort might plan health care at an earlier time. The crowd-out effect is causal but the “crowd-in” effect might not necessarily lead to the same conclusion. The trend is accompanied by an effect of cost reduction, it is more likely that the elderly or near-elderly wish to have more comprehensive health coverage to insure the risks of health shocks within an acceptable budget. Another limitation is that how much detail supports the parent has received from different children, for which we cannot differentiate as the poor information of the measure in the data. In addition, we do not differentiate the concentration level of the long-term care market, as low market concentration signifies that greater numbers of firms operate and severer competition, and thus probably more nursing home bed supplies, higher quality of service and lower cost for the patients within the market (Davis, 1993). The presented effects might be altered by these uncounted factors. Also, the intra-family model in our study is a simplified version; in reality, it might be more complicated though. If the children curtail caring activity in response to the parent's purchase of LTC insurance (expected in case of a low wage rate), the purchase of LTC insurance would run counter to the parent's interest as the need to cater child's response. In contrast, in the case where the children earn high wages, LTC purchase decision may lie in the parent's preference (Zweifel and Strüwe, 1998). Neither do we account for the case when the premiums are paid by the children, which might alter the scenario systematically (Hanley, Wiener, and Ham, 1991). Thus one possible avenue for future work would be to explore the effects accounting for these potential confounders. Another feasible extension might be to clarify whether all the outcomes in this paper present a specific geographic characteristic. All of these to a large extent depend on whether the more commensurate levels of data are measured in these ways.

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Chapter III

Job Insecurity and Health

ABSTRACT

This chapter examines the causal effect of subjective job insecurity on health, including self-rated health, mental health, chronic health conditions, BMI/obesity, drinking and smoking behavior. We employ pooled ordinary least squares (OLS), fixed-effects and instrumental variable (IV) specifications. By employing an indicator for firm-level permanent layoffs as the IV, we find that job insecurity has a negative effect on certain outcomes such as mental health and the emergence of new health conditions. Job insecurity predicts future job displacement and income loss.

Keywords: health, job insecurity, unemployment, endogeneity, instrumental variable, well-being loss

JEL Classification: I100, J3, J200

1 INTRODUCTION

Job insecurity can deteriorate health (Caroli and Godard, 2016). Literature also documents that unemployment has an adverse effect on health and lowers the expectancy of life. Being unemployed can lead to a significant and negative effect on health (see e.g., Otterbach and Sousa-Poza, 2016; Mandal, Ayyagari, and Gallo, 2011)and on income (Jacobson, Lalonde, and Sullivan, 1993). In the most severe situation, it might be associated with an increased risk of mortality (see e.g., Sullivan and Wachter, 2009). For older-male workers, mortality rates in the period after job displacement are more than one-half higher than otherwise would have been (Sullivan and Wachter, 2009). When the impact of dismissal from the job is unexpectedly escalated from an individual level to a sector level, it might even present the characteristic of inter-country "spill-over" effect. The workers in countries with strong employment protection legislation suffer from job insecurity in a degree comparable to the workers in less protected countries (Caroli and Godard, 2016). Though difference exists between the two (unlike unemployment, job insecurity usually involves no essential or immediate loss of income or real social status), the impact of job insecurity can spread sometimes in such a way that its adverse effect tends to be under-estimated as the negative and unobservable components tend to be neglected. Job insecurity is detrimental to health because it can cause stress (Sverke, Hellgren, and Näswall, 2002) and health symptoms such as eyestrain, skin and ear issues and stomach and sleep disorders (Cheng et al., 2005). It is also negatively correlated with depression-related diseases (J. E. Ferrie, 2001).

A large body of literature shows that causal evaluation is challenging as subjective job insecurity tends to be endogenous (see e.g., Caroli and Godard, 2016). Job insecurity can influence various health events; on the opposite side, ill health can negatively affect the subsequent employability or job insecurity of the subject, which is the concern about reverse causality. Another conundrum comes from selection issue when unhealthy workers are systematically selected to be dismissed (Krug and Eberl, 2018) or unhealthy workers select into jobs with low/high insecurity.

To address these concerns, we employ a firm-level IV, which is an indicator of a permanent reduction of labor force inside a specific firm. As the mass layoff within an organization is exogenous to the health of the individual workers, the IV is thus not endogenous and facilitates the identification of causal estimate. As such, we aim to revisit the long-debated issue of the impact on health by subjective job insecurity. Other than the IV, this study also contributes to the literature by observing a much broader range of health outcomes. The major finding is that after controlling for the endogeneity, job insecurity has a negative effect on mental health and the emergence of health conditions.

The chapter proceeds as follows. Section 2 provides the literature review. Section 3 describes the methodology and section 4 presents the data employed in this study. Section 5 reports our estimated results and Section 6 concludes and provides feasible avenues for future work.

2 PREVIOUS RESEARCH

The impact of job insecurity[28] on employees' well-being has received a great deal of attention in psychological and economic research.

Job insecurity is harmful to health. The insecurity (or the fear of being involuntarily fired from the job) is shown to detrimentally impact symptoms such as headaches and skin issues (Caroli and Godard, 2016) and is strongly related to mental distress (Lau and Knardahl, 2008). In addition, a growing literature in psychology, economics, and sociology reveals that job loss and job insecurity correlate with the incidence of depression and heart attack (see e.g., Goldsmith and Darity, 1996; Burgard, Brand, and House, 2009). The detrimental effect of job insecurity on mental health is larger for workers who place high importance on job security. It thus suggests that firms should exert effort to make employees more employable in order to reduce the negative impact of job insecurity (De Jong, Wiezer, and Joling, 2008). [29]High levels of job insecurity may also increase mental health issue such as distress (McDonough, 2000). The increment in fear of unemployment

[28] (Green, 2011) shows that job insecurity might include uncertainty over valued job features in the current job(including fears over promotion and relocation) and lack of continuity of the current job. Job insecurity might arise from job rotation or role difference (Ho, Chang, Shih, and Liang, 2009). However, in this study we focus on the probability of involuntary current job loss for non-self-employed employees (or who working for others).

[29] Literature has heterogeneous measures with respect to mental health. The study by Green (2011) computes mental health based on the feelings in the previous four weeks. The questions ask how much of the time being nervous, feeling down, feeling calm and peaceful and being a happy person. Responses are on a 6-pt scale from "All of the time" to "None of the time". Hellgren and Sverke (2003) use the 12-indicator General Health Questionnaire (GHQ-12; Goldberg, 1979). The scale is a screening test developed for the purpose of detecting non-psychiatric health (Banks et al., 1980). The items are scored ranging from 0 to 3, where 0 indicates no perceptions of mental health complaints and

substantially curtails the mental health status of employees (Reichert and Tauchmann, 2011). Individuals who report insecurity present markedly greater odds of mental and physical health problems (Strazdins et al., 2004) and thus are significantly associated with a worsened risk of poor health (László et al., 2010).

One potential channel of impact comes from job satisfaction (Heaney, Israel, and House, 1994) and absenteeism (Chirumbolo and Areni, 2005). Job insecurity is shown to correlate with negative work attitudes, with insecure employees in management position reporting decreased work effort, trust, career satisfaction and career optimism (Roskies and Guerin, 1990). With increased perceived job insecurity, permanent employees have lower levels of job satisfaction, work engagement accompanied by a deteriorating level of job exhaustion (Mauno et al., 2005). As such, job insecurity is related to reduced levels of identification with the organization, suggesting that employees' organizational citizenship behaviors are partially driven by evaluations about the perceived belongingness to the institution (Piccoli et al., 2017). Permanent employees and those who experienced high levels of subjective job insecurity are more likely than their counterparts to work while ill (Heponiemi et al., 2010). Job insecurity exerts a negative effect on life satisfaction as well (Kassenboehmer and Haisken-DeNew, 2009). Countering this, job satisfaction is boosted when individuals report a high level of optimism with respect to job insecurity (Zheng et al., 2014).

Higher subjective job-loss probabilities are associated with higher earnings declines and provide a significant predictive power on unemployment as well (Stephens, 2004). In the severe case, job insecurity can cause inflated unemployment rate (Luechinger, Meier, and Stutzer, 2010).

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3 indicates frequently perceived health complaints. Others use the depressive symptoms as the indicator of mental health (Burgard, Brand, and House, 2009).

Subjective labor insecurity tracks the dismissal rate and tends to be greater for less-educated older workers (Green, 2009).

Job insecurity can impact behaviors likewise. Job stress is observed to be positively related to continuous smoking behavior among ever smokers (Ayyagari and Sindelar, 2010) and to negative behavior such as decreased work effort (Roskies and Louis-Guerin, 1990). In the extreme case, it is found to be associated with the burnout of self-control (Westman, Etzion, and Danon, 2001).

However, as is shown by previous documenting work, it is a huge challenge to evaluate the causal impact of subjective job insecurity on health outcomes as the variable of job insecurity contributes to the issue of potential endogeneity. The first concern is that different socioeconomic individuals might perceive job insecurity in a different way and thus report different levels of health. In many cases, however, it is hard to capture the degree of bias. Reverse causality is a second major concern because poor-health individuals tend to be less as productive, thus experiencing worse job insecurity or unhealthy workers tend to be hired on insecure/secure occupations. The third comes from the issue of the weak instrument, in such case the power of IV (e.g., the unemployment and labor protection legislation situation) for job insecurity is compromised and thus a precise point estimate on health is hard to achieve (Caroli and Godard, 2016). The fourth issue comes from selection bias as unhealthy workers might select into low/high insecure industries or occupations.

Many studies constrain to the potential threat of a job loss while others extend it to the replacement of employment, encompassing the working employees who have insecure jobs by definition: part-time, or temporary and thus mostly only used to buffer short-term labor demand in the labor market. Workers in such market consider job insecurity as an inseparable component of their career experience and therefore possess relatively consistent beliefs about the labor market and their professional prospects. Mostly, job insecurity captured by this cohort is negatively linked with their health. Contrastingly, workers in the stable labor market, are usually hired based on a long-term basis, and thus perceive an unprepared or unexpected job-insecurity shock at exposure(Jane E Ferrie, 2001). These workers usually experience the change from the feeling of safe work in the firm to distressing insecurity. The rise in job losses and the corresponding adverse impacts among workers in this steady labor market are correlated with the suddenly perceived job insecurity (Wachter, Gordon, Piore, and Hall, 1974).

In this study, we focus on the cohort who is working. [30] We concentrate on this cohort being potentially affected by unemployment but not being dismissed due to the reason that if they indeed experience a worsening in health, then the overall impact of unemployment on health tends to be not fully taken into account (A. Reichert and Tauchmann, 2011).

Second, our study complements recent studies by employing a fine-tuned instrument making the precise point evaluation possible. A firm-level layoff indicator provides hints about how healthy that specific entity is and consequently whether the labor status of one employee extensively “spillovers” to the others, just as in the case of a firm-level mass layoff. It thereby establishes a much clearer causal impact between the perceived labor market and health outcomes than previous applied work. Literature employs either country-level or market-level shock (see e.g., Caroli and Godard, 2016). The macro-level shock might efficiently capture the causal effect if the causal link correctly exists. And individuals are likely to be affected by the whole economy; however, shock at the firm level is more relevant. Reverse causality evolves to be a concern if

[30] Another indispensable component is the workers who are self-employed, as they are the owner of the businesses and the perceived job insecurity is supposed to be disparate from those working for others. Owners on average have more control on the resources within the firms. Compared to paid employees, self-employed individuals have lower job insecurity (Millán, Hessels, Thurik, and Aguado, 2013). In this study, we do not investigate in detail into the part-time market, but focus on the working-for-others individuals and thus not self-employed. In contrast, we use the full-time employees as a sensitivity check.

unhealthy workers are more likely to be employed in insecure/secure jobs or if adverse health causes individuals to fear more they could be displaced.

All of these generate a tough challenge for efficient point estimate. Even the most recent economic literature controlling for unobserved heterogeneity inadequately addresses the potential risk of reverse causality biasing the estimates (see e.g., Green, 2011; Knabe et al., 2010). In the study by Green (2011), for instance, absence IV with multiple endogeneities, they rely on controlled time-invariant effects and FE-RE Heckman specification to identify the causal effect; however, it is possible that there are other time-varying variables associated with both job insecurity and outcome that is not controlled for.[31] Our study aims to provide a more effective causal effect of subjective job insecurity on health by addressing these issues through robust IV specifications. In interpreting this, it is helpful to work from a theoretical and econometric viewpoint explicitly recognizing the possibility that health impact by job insecurity may vary across the subpopulations, depending on such characteristics as gender, education, and employability. This perspective helps to reconcile the varieties of findings in other applied work, and thus illustrates a useful framework for facilitating new insights about the connection between job insecurity and health impact.

The connection between job insecurity and health has found a negative relation. However, the strength of the link varies sizably across studies. Some indicate a weak correlation (see e.g., Hellgren, Sverke, and Isaksson, 1999), others reveal a moderate relationship (Author, Cavanaugh, Noe, Cavanaugh1, and Noe2, 1999), with remains report a strong correlation (Ameen, Jackson, Pasewark, and Strawser, 1995). Given this debated context, revisiting this issue is meaningful. We aim to identify the effect by extending to much broader health outcomes than previous work.

[31] One example is that the existence of time-varying expertise across the workers.

3 METHODOLOGY

The baseline model is defined as the following pooled-OLS specification:

$$\boldsymbol{Y_{it} = \lambda_1 + \lambda_2 \cdot JobInS_{it} + \theta \cdot X_{it} + \lambda_3 \cdot Div + \lambda_4 \cdot Year_t + \varepsilon_{it}} \qquad \textbf{(1)}$$

Where the dependent variable Y denotes health (i.e., self-rated health and mental health,) of individual i at time t, or health behaviors (i.e., any new health conditions since the last interview, BMI, obesity, ever drinking behavior, and smoking behavior during the current wave). $Year$ is the fixed effects for waves and Div is the fixed effects for census divisions. $JobInS$ captures the degree of job insecurity and is measured in one-percent increment from 0 to 100 percent, Coefficient $\boldsymbol{\lambda_2}$ is thus our main parameter of interest. It is expected to be positive based on the hypothesis that job insecurity deteriorates health. Control covariates X in the baseline include a vector of demographic and socioeconomic status such as age, age squared, gender, race, education, marital status, log of the household asset, log of the household income, length of current job tenure in year, whether the respondent is covered by the employer's plan, and firm size, while ϵ_{it} is the random error disturbance.

To capture individual time-invariant effect, we employ the fixed-effects specification in (2). α_i is the individual fixed effects for individual i. The advantages of fixed effects specification include allowing the individual- and time- specific effects to be correlated with explanatory variables. Neither does it require an econometrician to model the correlation patterns (Hsiao, 2007). And if there are some time-invariant effects herein, they increase precision by absorbing substantial variation in the main outcomes and thus avoid completely attributing the impact on the outcome to the sole key variable(s). All the other covariates are similar to those in the previous discussion.

$$Y_{it} = \beta_0 + \beta_1 \cdot JobInS_{it} + \delta \cdot X_{it} + \beta_2 \cdot Div + \beta_3 \cdot Year_t + \beta_4 \cdot \alpha_i + \epsilon_{it} \quad (2)$$

Since the subjective capture of job insecurity tends to be endogenous, we further employ the IV corresponding to the equation (1) and equation (2) respectively to evaluate its under-control impact. The IV for job insecurity is $CFDZ$, which is the indicator of a firm-level mass layoff and is exogenous to workers' health (Sullivan and Wachter, 2009). $CFDZ$ is a dummy indicator and equal to one if the firm does experience a permanent shock in labor reduction and 0 otherwise. To be specific, it is based on the question in the survey:" Has your employer experienced a permanent reduction in employment since [month, year respondent started current job/respondent's last interview month, year/current interview month, 2 years ago /you started working there](permanent employment reductions are sometimes called downsizing)?" The provided answer is either "Yes" or "No". We eliminate those observations with missing or unknown information. The identification assumptions of the IV model are: (i) the IV is correlated with the endogeneity; and (ii) not correlated with the error disturbance.

4 DATA

We use longitudinal data from the Health and Retirement Study (HRS) to estimate the effect of the subjective job insecurity on health and health behaviors. The data used in our research come from the 1998-2014 periods on a biennial basis. Due to the specific design of the questionnaire in wave 2008, we exclude it from our sample as it does not set up the same question as in other waves about the possibility of losing one's job. The HRS was designed to follow age-eligible individuals (mostly > 50) and their spouses as they made the transition from an active worker into retirement and obtained detailed information in a variety of domains: demographics, health status, family structure, employment, and income, and insurance.

Totally there are 10 census divisions in the HRS dataset; we drop the one that is not the U.S. or not inside the U.S. territory, which is not of the major concern in this study. And those whose race characteristic falls to none of the three categories are eliminated as well.

We use the data from RAND V.P HRS, encompassing the workers whose labor status is working but not self-employed at the time of the survey. In Table 32, we list the sample selection criteria, all of which are excluded due to the characteristic of non-major concern in this study or the specific survey design of that wave. The number of individuals is calculated on a person-year basis. The original sample has observations of 159,067. After dropping out non-White/Caucasians, Black/Africans or other race, the number of observation reduces to 159,041. This amount curtails further to 158,821 observations (or 33,458 individuals) if excluding the census division outside the U.S. and reduces to 31,249 when accounting for the missing information of losing one's job and all other covariates. We then constrain to the interested 50-64 years old cohort, generating an observation of 22,711. The final sample size fixes to 21,692 (or 9594 individuals) when only not self-employed working employees are extracted.

4.1 HEALTH MEASURES

The mental health definition in our study comes from the concept of depression based on a score on the Center for Epidemiologic Studies Depression (CES-D), the scale of which can be found in (Diane Elizabeth Steffick, 2003). The CES-D score is the sum of five "negative" indicators minus two "positive" indicators. The negative indicators measure whether the respondent experienced the following sentiments all or most of the time: depression, everything is an effort, sleep is restless, felt alone, felt sad, and could not get going. The positive indicators measure whether the respondent felt happy and enjoyed life, consistently all or most of the time. We use the value of depression index CESD, which varies from 0 to 8 in the sample, as the level of mental health. Thus the higher the value of this score, the worse off mental health and the higher the level of depression it indicates.

For self-rated health, it is treated in a similar way as mental health. In the survey, this variable is divided into five categories: 1 “excellent” 2 “very good” 3 “good” 4 “fair” 5 “poor”. We take the original value of the original five categories to represent the actual level of self-rated health. The question is asked as follows:” Would you say your health is excellent, very good, good, fair, or poor?” A higher number indicates lower self-rated health.

The number of new adverse health conditions measures the set of variables summarizing the increase in the number of health conditions since the last interview. It is reset to 0 if no new health conditions emerge, and equal to 1 if the respondent answers a positive-number response. The health conditions are the sum of indicators for whether a doctor has ever told the respondent that he or she has ever had a particular disease. The eight included diseases are high blood pressure, diabetes, cancer, lung disease, heart disease, stroke, psychiatric problems, and arthritis.

BMI indicates the body mass index and obesity is re-defined as those whose BMI is 30 or above. For smoking take-up, it is equal to 0 if the employee answers no smoking in that wave and equal to 1 otherwise. With respect to drinking behavior, it is asked whether the respondent ever drinks alcohol beverage.

4.2 JOB INSECURITY

As for job insecurity, we employ both continuous and tercile measure to capture the potential of non-linear impact. In the survey, job insecurity is in one percent increment, spanning from zero percent to 100 percent. To quantify this, the survey asks as follows:” On the same scale from 0 to

100 where 0 equals absolutely no chance and 100 equals absolutely certain, what are the chances that you will lose your job during the next year?" While in the tercile measurement, job insecurity is divided into three equal-size subcategories and we use the lowest tertile as the reference.

4.3 OTHER COVARIATES

The variable for work in HRS summarizes the labor force status for the respondent at each wave. Our sample consists of the employees who are working but not self-employed at the time of the survey. The relative question is asked as follows:" Are you working now, temporarily laid off, unemployed and looking for work, disabled and unable to work, retired, a homemaker, or what?" The respondent can choose to answer with the options: 1 "working now", 2 "unemployed and looking for work", 3 "temporarily laid off, on sick or other leave", 4 "disabled", 5 "retired", 6 "homemaker", 7 "other". Our sample is comprised of the employees working currently and working for others. To identify the latter status, we further combine another question concerning self-employment in the study, it is designed as:" On your current main job, do you work for someone else, are you self-employed, or what?" We choose those who responded with "someone else" and exclude the individuals who provided the answer with "self-employed".

Other covariates include gender (females as the reference group), race (with White/Caucasians as the reference), education (high school or less as the reference), marital status (taking never married as the reference), whether the respondent is covered by the employer's plan (no coverage as the reference), log of the household income, log of the household asset,[32] current job tenure length in year (three quantiles and the lowest as the reference) and the size scale of the firm (divided into three categories: small-scale (smaller than 50 employees and taken as reference); medium (between 50 and 100 employees); large-scale (over 100 employees).

[32] The household income and household asset include the respondent and the spouse only.

4.4 INSTRUMENT VARIABLE

The major concern of this study comes from the endogeneity arising from the job insecurity. We instrument perceived job insecurity by another observation about whether the firm experienced permanent reduction with regard to employment. Firm-level employment reduction should be exogenous to individual workers' health developments (Sullivan and Wachter, 2009).To quantify, it is based on the question in the survey:" Has your employer experienced a permanent reduction in employment since [month, year respondent started current job/ respondent's last interview month, year/current interview month, 2 years ago /you started working there]?" [33]The intuition and assumption are that: (1) if a certain firm does cut down the labor on a large scale and thus the unemployment rate is high, the caused level of anxiety and uncertainty of job prospect across the employees is correspondingly high within that specific context, leading to a certain level of response in health-relevant behavior or health outcome. (2) firm-level downsize is exogenous to an individual's health and thus not correlated with the unobservables.

5 RESULTS

5.1 SUMMARY STATISTICS

Table 33 presents the summary statistics for the dependent and independent variables while Table 34 provides the summary for those whose firms experienced permanent mass layoff and those who did not. The data constitution does not show a significant difference across the two compared groups. For the variable CFDZ, which reveals whether current firm experiences permanent labor reduction, males and females do not differ essentially: the percentage is 27.9 and 26.3 respectively. Similarly, the means for self-rated health are 2.434 and 2.414 respectively. As for mental health,

[33] Notice that the endogeneity of job insecurity indicates the upcoming one year frame, whereas the instrument variable of firm downsize varies between periods. Constraining the IV to within-year job insecurity yields comparable results. And the IV is effective in that it satisfies the exclusion restriction assumption: (i) correlated with the endogeneity; and (ii) not correlated with the error disturbance.

the values are .955 and 1.185 respectively, indicating that women have a slightly lower level of mental health on average. The likelihood of losing one's job (or equivalently job insecurity) is 16.61 percent for males and 16.96 percent for females. In either case, the majority is married (84 percent for men versus 70.1 percent for women).

Though the average level of non-permanent reduction in the labor force is a little bit larger for women, the trend is reversed with respect to smoking and drinking. Specifically, a noticeable contrast of 70.4 percent in drinking men compared with 60.7 percent in drinking women. The majority of individuals have a high school or less education: 67.7 percent for the men and 72.1 percent for the women. For both cohorts, more than one-half individuals work in medium-scale (between 50 and 100 employees) or large-scale (larger than 100 employees) firms on average.

In Table 34, we list supplement measure of summary by CFDZ. For the covariates such as BMI, age, and marital status, we do not observe significant gap across the mean values. However, nuance does exist between the two subcategories. For instance, individuals in the non-permanent reduction cohort are more likely to (14.76 percent) report a lower probability of losing their job than their counterparts (22.33 percent). Likewise, the ratio of high school or less education is a little bit greater for the former (71.3 percent versus 67.1 percent). And the household income is slightly lower for the former cohort as well. The employees in either cohort appear to be more likely covered by the employer-provided plan. Figure 1 plots the distribution of job insecurity and the fraction of individuals with zero job insecurity is over 40 percent whereas those with 100% job insecurity constitute less than one-tenth.

5.2 RESULT

We estimate the impact of subjective job insecurity on health for the periods from 1998 to 2014 excluding specific wave 2008, which are presented in Table 35 to Table 44. Table 35 to Table 39 is pooled-OLS or fixed-effects regression outcomes while the remains show corresponding IV results after controlling for the endogeneity.

Table 35 is based on a continuous measure of job security. For each outcome, we control for the same characteristics as the baseline including gender, race, education, age, age squared, marital status, log of the household income, log of the household asset, current job tenure length, firm size, whether the correspondent is covered by the employer's plan. The dummy controls include waves (from 1998 to 2014 with 2008 excluded) and census divisions (with the one outside the U.S. excluded). These are taken as the covariates in the baseline. When the endogeneity is not accounted for, continuous measure of job insecurity significantly impacts self-rated health and mental health: a higher level of job insecurity significantly deteriorates both categories of health. Job insecurity seems to be negatively correlated with almost all adverse health outcomes except for smoking and drinking take-up: it increases the incidence of any new adverse health conditions, BMI, obesity. Coefficients in Table 35 implies that when job insecurity grows by one percentage, self-rated health rises by 0.00301 points on average and mental health grows by 0.00707 points. And the likelihood of having any new health conditions increases by 0.0333 percentage points. Generally, better-educated and wealthier individuals are less likely to experience adverse health shocks than otherwise whereas those having employer-provided plan coverage experience worse health outcomes.[34] When taking into account the average levels of mental-health 0.955 and 1.185 for men and women, this thus implies that 0.74 percent and 0.60 percent of average mental-health deterioration for males and females respectively. [35]

[34] We also take into account the inflation effect over time and transform household incomes and household assets of all waves to 1998-wave dollars, then perform the estimates and the results are essentially not changed. In the subsequent analysis including IV specifications, we apply same logic and obtain similar outcomes.

[35] We perform same POOLED-OLS estimate by using different definitions of outcomes: (a) redefine self-reported health to one if poor or fair and equal to zero otherwise; (b) redefine mental health to one if level is no less than 3 and zero otherwise; (c) number of new health conditions; (d) drinking behavior for the recent three-month period; (e) number of new health conditions ever. The results are essentially not changed.

Table 36 presents the corresponding specification by adding individuals' fixed effects, which are not accounted for so far. High job insecurity consistently reduces the level of mental health significantly. The coefficients in the fixed-effects specification are generally lower than their pooled-OLS counterpart. When fixed individual traits are allowed for, the estimated impact can be reduced (Ferrer-I-Carbonell and Frijters, 2004). On the other hand, when job insecurity is measured in a continuous way, it has no impact on other health outcomes such as self-rated health, the incidence of any new adverse health conditions, BMI, obesity and drinking take-up at the conventional levels. Thus, when individual-level fixed effects are accounted for, the channel of impact seems to concentrate on the deterioration of mental-related diseases or depression symptom if any exists.

Coefficients in Table 36 imply that when job insecurity increases by one percentage, the numeric value of mental health rises by 0.00217. Job insecurity appears to be negatively correlated with the level of mental health. Accounting for the average mental-health levels of .955 and 1.184 for men and women, this indicates a deterioration of 0.229 percent for males and 0.184 percent for females respectively. The impact is statistically significant. The magnitude is relatively small and thus not economically prominent though. Our study suggests that individuals with an increased perception of job insecurity deteriorate their health significantly.

In Table 37, we present the respective tercile estimates of job insecurity on health outcome by employing both pooled-OLS and fixed-effects specification. The results consistently imply that the impact on health by job insecurity seems to be nonlinear. As when job insecurity increases from lower tercile to a higher one, the growth in the coefficients is not linearly proportional with regard to job insecurity. For instance, for mental health at tercile 2 in the fixed-effect specification, the coefficient is -0.0702 whereas it is 0.308 in tercile 3, an over tenfold increase in magnitude.

The impact is not proportional in linearity, generally, at higher tercile, the negative impact on health is more pronounced than that of the lower tercile, reinforcing a positive correlation between poor health and job insecurity.

In Figure 2, we plot how mental health and any new health conditions change with the job insecurity. In a broad way, the graphical presentation is consistent with Table 37 in that, (i) mental health and any new health conditions deteriorate with job insecurity; and (ii) the upward-trend relationship proceeds in a non-linear way.[36]

5.3 RELATIONSHIP BETWEEN JOB INSECURITY AND DISPLACEMENT

Job insecurity is an effective prediction of subsequent unemployment and income loss (Stephens, 2004). In this and the following section, we discuss whether job insecurity accurately predicts the incidence of job loss and income loss. To identify the former, we define the displaced or laid-off workers as those who left their previous employer between waves and the reasons of leaving are either "Business closed/moved" or "Laid off". Nearly 404 workers (roughly 5.3%) in the sample are displaced between waves.[37]

Figure 3 illustrates the direct relationship as well as the predicted marginal effect with a 95-percent confidence interval between the perceived job insecurity and subsequent job displacements. The lower-section dashed line is the ratio of workers suffering a subsequent displacement by job insecurity and the upper-part solid line is the predicted marginal result.[38] Our marginal-effect specification is consistent with the actual trend of incidence. The figure indicates

[36] Job insecurity is rounded to tenth of percentage level. In the subsequent graphical presentations, we follow the similar logic.
[37] For any reasons such as poor health/disabled, better job, family care or retired, we term them as job leavers and totally 744 (around 9.7%) individuals are job leavers.
[38] The marginal effects are derived by calculating at the average level of other covariates.

that there is a positive correlation between the workers' job insecurity and the actual incidence of job displacement. The ratio of layoff increases with job insecurity.[39]

Table 38 is the estimated result of the marginal effect on job displacement by job insecurity, which provides an alternative illustration complementary to Figure 3. Column (3) to (4) is the outcome for layoff. The baseline result using only job insecurity as the covariate is presented in column (3). Column (4) adds the control covariates discussed in the previous baseline specification. The coefficients in both layoff specifications are positive and statistically significant, implying the prediction of displacement by job insecurity is powerful and thus the measured job insecurity in this study is non-trivial. In ideal status, workers perceiving 10 percentage of job insecurity might expect the equivalent percentage of displacement. The flatter trend of actual displacement implies that some portion of the workers exaggerates the subjective perception of job insecurity.[40] Qualitatively, the predictive effect on layoff is consistent with the study by Stephens (2004).[41]

5.4 RELATIONSHIP BETWEEN JOB INSECURITY AND INCOME LOSS

To identify the predictive estimate on income loss by job insecurity, we employ the individual-level actual annual income data. [42]Income loss is termed as one if the income of the upcoming wave is less than the income of current wave for the individual working for the previous employer and zero otherwise. Figure 4 is the comparison between real-income loss and the prediction of marginal effect with a 95-percent confidence interval. The trend patterns are consistent in the two

[39] The acute downturn starting at 60% job insecurity is comparable to the previous trend pattern for mental health, suggesting that individuals with a higher likelihood of displacement are also accompanied by a lower status of mental health (or higher value of depression). This downturn might be attributable to temporary recovery of the economy.

[40] The large-scale economic recession happening in 2000s might partially explain this phenomenon.

[41] In this section and the subsequent income loss marginal effect estimates, we also employ the probit specification and the outcomes are not changed qualitatively.

[42] Individual income data is different from the household-level income in that it only includes the income of the respondent and does not incorporate the income of other family members.

specifications: the likelihood of income loss generally increases with job insecurity. When job insecurity increases above 90 percent, the probability of earning loss is more prominent than in other levels.[43]

Column (1) to (2) in Table 38 is the marginal effect prediction of earnings loss by job insecurity and the result is consistent with Figure 3 in that the probability of annual earning loss systematically increases with job insecurity. Consistently, all the results indicate job insecurity has a statistically significant and positive impact on the likelihood of subsequent income loss.[44] Job insecurity is an efficient prediction of both future job displacement and individual earning loss.[45]

5.5 ANTICIPATION EFFECT OF FIRM-LEVEL LAYOFF

To identify the anticipation effect of firm-level mass layoff on job insecurity, we estimate whether the one-wave later downsize of the current firm (CFDZ) has an impact on current job insecurity and constrain to workers working for the same employer. The methodology is similar to the previous discussion. In the alternative specification, we add contemporaneous CFDZ. The OLS linear estimate in Table 39 indicates subsequent firm-level layoff significantly “anticipates” the present job insecurity in both specifications.[46] Column (1) and (2) estimate job insecurity at time t relating to CFDZ at time t+1, whereas column (3) and (4) add contemporaneous CFDZ at wave t as well. For both specifications, the coefficients for CFDZ are positive and significant. The firm might have multiple options in downsizing decisions when faced with adverse shocks and might take into account the uncertainty about the possibility of being laid off next year affects workers’

[43] The job displacement and income loss both indicate the variations between waves, whereas the time slot for job insecurity in the study is the next one year. Constraining the dependent variables to within-year job insecurity yields comparable results to the presented outcomes.
[44] In alternative specification, we extend the workers to also incorporate the cohort not working for previous employers, the results are essentially similar.
[45] Our income estimate is different from Stephens (2004) in that we employ the actual individual-level earning data, whereas their study only uses the postulated possibility of earning loss and thus our estimate is more powerful although qualitative results are similar.
[46] We also perform the estimate by extending to all workers and the result does not changed qualitatively.

performance this wave. To balance, the timing of downsizing can vary substantially. Specifically, a one-time sweeping cut in the workforce called “big bang” and waves of cuts called “gradualism” are commonly used two policies (Jeon and Shapiro, 2007). Our OLS specification indicates that future mass lay off does have an anticipation effect on current individual job insecurity, suggesting that the firm might exploit the current layoff information to the decision of upcoming layoff. For the magnitude, it seems like that the weight of next-period layoff constitutes at most one-third of the total weight on average. In contrast, in the fixed effect specification, the anticipation effect of a mass layoff at t+1 on job insecurity at time t disappears and the coefficients for layoff at t+1 are insignificant now. Therefore, in the baseline FE model that we employ, firm-level layoff does not have an anticipative power on current job insecurity. For concurrent mass layoff, it still has a systematic impact on contemporaneous job insecurity. Overall, it appears that the rational expectation of job insecurity by combined firm-level layoff lies around the threshold of 10%. This value is commensurate with the case where real layoff happens to an individual worker. Thus previously discussed exaggerated magnitude of jog insecurity might partially attribute to the impact by the future firm-level layoff.

5.6 IV RESULTS

Table 40 is the instrumental variable (IV) estimation for the pooled OLS discussed previously. When instrumenting job insecurity, our estimate suggests that the qualitative impact on health outcomes is not changed systematically. Health deteriorates with job insecurity. As expected, after controlling the endogeneity of job insecurity, all the outcomes except BMI, drinking and smoking take-up systematically increase.[47]

[47] The tercile-IV estimate of job insecurity requires more instrumental variables, however, we are not able to find as many effective IVs in HRS, thus the corresponding tercile-IV results are not presented herein. We perform POOLED-OLS IV estimate by using different definitions of outcomes: (a) redefine self-reported health to one if poor or fair and equal to zero otherwise; (b) redefine mental health to one if level is no less than 3 and zero otherwise; (c)

One might notice that these results are much larger than those estimated by OLS, and the corresponding standard errors are quite large as well. The increase in the coefficients in the IV specification might be attributable to several potential sources of endogeneity. Specifically, measurement error is one major concern in survey data (Olson, 2006). It might lead to bias in the OLS estimate. This is attributable to the reason if there is a gap (or measurement error) between true job insecurity and observed job insecurity, the reliability of observed job insecurity is compromised (Griliches, 1977).

Further major concern focuses on the weak IV issue, as when the F-test of the excluded restriction is less than the "rule of thumb" value 10 then the effectiveness or power of the IV is generally cast with doubt. All of the F statistics in our study are well above 10. The first-stage coefficients range from 8.574 to 8.609 (all significant at conventional levels). F statistics range from 356.66 to 415.85.

Table 41 is the IV estimation for the fixed-effects model accordingly. When accounting for the endogeneity of job insecurity, mental health and any new health conditions statistically significantly deteriorate. For all the other outcomes, we do not observe sizable impact though. Analogous to the aforementioned strategy, we track the potential existence of weak-instrument issue by testing the first-stage specification in the 2SLS. Consistent with the previous finding, F statistics range from 96.00 to 122.23. All the first-stage coefficients are significant at conventional levels.[48]

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number of new health conditions; (d) drinking behavior for the recent three-month period; (e) number of new health conditions ever. The results are qualitatively similar. We estimate for all the outcomes by using different measures of job insecurity as well: (1) redefine to one if job insecurity is no less than 50% and zero otherwise; (2) redefine to one if job insecurity is no less than 90% and zero otherwise; (1) divide the original job insecurity by 100 thus make the scale of job insecurity distribute between 0 and 1. The results do not change essentially.

[48] We perform fixed-effects IV estimate by using different definitions of outcomes: (a) redefine self-reported health to one if poor or fair and equal to zero otherwise; (b) redefine mental health to one if level is no less than 3 and zero otherwise; (c) number of new health conditions; (d) drinking behavior for the recent three-month period; (e) number of new health conditions ever. The results are qualitatively similar. We estimate for all the outcomes by using different measures of job insecurity as well: (1) redefine to one if job insecurity is no less than 50% and zero otherwise; (2) redefine to one if job insecurity is no less than 90% and zero otherwise; (1) divide the original job insecurity by 100 thus make the scale of job insecurity distribute between 0 and 1. The results do not change essentially. We perform the

To evaluate whether job insecurity has a heterogeneous impact on men versus women, we stratify the sample by gender and estimate the fixed-effects IV model separately for each sub-sample. Table 42 is the corresponding result. Job insecurity does not differentiate on mental health by gender and it is deteriorated significantly for both genders at conventional levels, which is not in conflict with the estimate in Table 41. However, women do suffer less from job insecurity than men with respect to the magnitude of impact. Literature documents that males and females systematically differ in their level and profile of job insecurity, the former generally are more insecure than the latter cohort (Rosenblatt, Talmud, and Ruvio, 2010). There is evidence that gender differences in support derived from social participation may partly account for the gap of the mental health level among women compared to men (Kawachi and Berkman, 2001). For men, work and thus earning money for the household is still the core of their role in society, whereas the availability of an alternative role such as taking care of the non-adult children can make job loss not as distressing for women (De Witte, H., and Wets, 1996). Our finding implies a similar result shown in the study by (Ferrie et al., 1995), who reveal less deterioration in health among insecure women. On the other hand, the difference between men and women with respect to mental health does not imply that potential job loss is harmless for the well-being of women as they are more likely than men to extend their concern to non-financial issues (Rosenblatt et al., 2010). And thus are more vulnerable in exposure to unexpected severe health shocks such as cancer, stroke, and heart problem. In our finding, females are more salient and sensitive in the incidence of new health conditions than males when job insecurity grows in levels.

To further estimate whether job insecurity has a differentiated impact through the channel of education, we employ a similar stratification strategy as the gender case. Table 43 illustrates the

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POOLED-OLS, POOLED-OLS IV and fixed-effects IV estimates for the current smoking behavior of ever smokers, none of the outcomes is statistically significant.

corresponding results. When accounting for education, mental health deteriorates significantly both for college or higher educated and high school or lower educated individuals. Job insecurity does not differentiate the impact on health through the channel of education. However, the magnitude of the impact is not the same though; higher-educated individuals suffer a worse-deteriorated effect on these two outcomes. They suffer more lowering in mental health level and emergence of any new health shocks. Another finding is that differentiation validates through the channel of obesity level: high job insecurity systematically increases the likelihood of obesity for better-educated individuals. Literature shows that higher-educated people are more responsive to exterior shock than high-school graduates (Wozniak, 2010). These people are generally more likely to be hired in higher positions such as managers in a firm and might react more strongly to the potential loss of a job as the belief in "meritocratic individualism"(Roskies, 1990). Negative thus career setback would cause self-doubt or even despair, and consequently to a downsize in health, which is called the "status inconsistency" hypothesis by Schaufeli and VanYperen (1992). Our result suggests that the more highly educated are likely to experience much more severe status inconsistency because of their perception of potential risk of being dismissed from the labor force, which in turn results in poorer health and a higher likelihood of health shock. The significant effect on obesity implies that job insecurity can have a twofold negative impact for the higher-educated workers: health deterioration accompanied by losing control of body mass level at the exposure of "status inconsistency".

We further estimate whether the impact on health is heterogeneous based on the employability level of the workers. The relative question about employability asks: "Suppose you were to lose your job this month. What do you think are the chances that you could find an equally good job in the same line of work within the next few months? Zero means absolutely no chance and 100 means absolutely certain."[49] Low-employability workers are termed as 50 percent or lower chances of finding another job, whereas high-employability workers incorporate the individuals responding over 50-percent chances. The result is presented in Table 44. The estimate implies that job insecurity differentiates through the channel of workers' perception of employability. Low-employability workers significantly deteriorate mental health, but it is insignificant for high-employability employees. The negative impact on health is more pronounced for the low-employability cohort.[50]

5.7 PLACEBO TEST

Studies show that maternal morbidity can lead to ill-health of the child (see e.g., World Health Organization, The Partnership for Maternal, Newborn, 2011; Frankel and Wamboldt, 1998) and child's social development (World Health Organisation, 2008). On the other hand, an unhealthy child can cause higher levels of stress for the parent (Bonis, 2016). In the meantime, height is a vital factor indicating the health level of an individual in his or her growing age. However, individuals generally stop piling up in height at a certain age. Therefore, the job insecurity status of an adult individual should not affect his or her height post adulthood or the parent's longevity/mortality status if such a cross-generational or determined link in health exists absence the selection bias based on health. In contrast, when unhealthy workers are selected into certain labor sector systematically, then the job insecurity perceived by this cohort systematically biases the health of the parent or the height of his or her own. As such, a sizable bias reveals the pathway of selection. Complying with this, we perform a placebo test to check whether child's perception of job loss causes a substantial effect on parents' mortality/longevity and working child's height

[49] In the past work, employability might be captured by either the unemployed or the employed (Green, 2011). In this study, we focus on the latter.
[50] Applied work has shown that employability strongly moderates the effects of unemployment and of job insecurity on health literature (see e.g., Green, 2011; Witte and Cuyper, 2015; Cuyper et al., 2008).

level, as are illustrated in Table 45. We do not observe statistically significant effect over all these outcomes; therefore the selection issue based on the child's health conditions is not of existence.

5.8 LAGGED EFFECT

To check whether the current perception of job insecurity has an impact on future health, or equivalently, whether past capture of job insecurity affects current health level, we perform the lagged-effect estimate. The results in Table 46 suggest that the immediate or concurrent impact of job insecurity on health outcome differs from the lagged effect. Neither of the two-year later nor the four-year later effect is statistically significant. However, we do not observe sizable longer than four-year impact as the first-stage specification indicates that it is not significant anymore and F statistics are less than the most acceptable value of "rule of thumb" 10. Although consistency lies in the contemporary negative impact on health development, nuance exists between our lagged impact over time and the finding by Sullivan and Wachter (2009). They reveal that the negative impact on mortality by job displacement continues in a significant way even after a long-term period; instead, our study implies that the concurrent positive correlation with health might be compromised beyond two-wave period. This suggests that actual job displacement might exert a longer time period of negative impact on health than pre-layoff job insecurity.

5.9 SENSITIVITY ANALYSIS

Endogeneity is a major concern when evaluating the causal effect of job insecurity on health. In the most severe case, bias might be unexpectedly huge and thus the sensitivity of the estimate is open to questions. To address this major concern, we consider multiple specifications that control for variables likely to capture the differences. We summarize additional robustness checks reinforcing that our baseline results are not affected by alternative specifications including "prepared" job insecurity.

As job insecurity is an inseparable and consistent component of part-time or temporary workers, if a high dismissal or insecure industries/occupations rely more on part-time workers to meet the needs of labor demand, a disproportionate fraction of the workforce tends to be eliminated from the sample to the extent that we exclude not full-time workers. Therefore in the first set of robustness check, we concentrate on the full-time workers only and the resulting outcome in row (1) of Table 47 indicates that our baseline fixed-effects IV estimate is robust to this alternative specification. Mental health substantively deteriorates, and in the meantime, any new health conditions are increased systematically.

So far, we do not account for the varieties with respect to job industries and occupations. Different occupations and job industries capture job insecurity in their own ways. For instance, certain unstable industries or occupations tend to regularly expose to health-damaging labor environment and thus workers are less productive and less productive workers tend to be less healthy (Sullivan and Wachter, 2009). Or it might be the case less healthy workers select into these insecure industries or occupations. If unhealthy workers are more likely to be employed based on industries/occupations than healthy counterparts, the estimate tends to overestimate the impact (Caroli and Godard, 2016). Therefore we add these two fixed effects then check the sensitivity to these potentials. The result in row (2) is quite consistent after controlling for industries and occupations specifications. It should thus be of little affected if any of these biases exist. A limitation of HRS data is that we do not have a direct measure of "prepared" or "non-prepared" job insecurity. To proceed, in the third set of sensitivity check, we restrict the workers to exclude those having a second paid job to evaluate the bias arising from "prepared" or "pre-perceived" job insecurity by working-age employees. The result in row (3) is robust to adding this specification. An alternative case of "pre-perceived" job insecurity is that our sample covers the cohorts who are close to retiring age, if workers deciding to retire report their job insecurity to a less severe extent, the impact on their health by layoff or business closure should be mediated by this "preparedness" (Michaud, Crimmins, and Hurd, 2016). To explore this issue, we add additional control of post-65 years old subjective work expectations. Row (4) indicates that the impact is not changed essentially. Another concern is that people might tend to choose integral numbers like 0, 50 when provided with multiple options, in such cases the estimates might not reflect the real capture of the subjective job insecurity and thus the results are biased. To check the potential existence of this issue, in row (5) we exclude the patterns including these special integral values and the outcomes do not change essentially.

Bunch of literature reveals that income is prone to be one source of endogeneity (Lundborg, 2013). Although our study does not encounter the similar issue of scarce income information that is vital for reducing the bias (Caroli and Godard, 2016), endogeneity caused by income or asset becomes a concern if the bias is non-negligible. As higher income or asset tend to impact the volume of health services that workers can afford to purchase and thus affect their level of health accordingly. And income pooling within the household can be seen as a source of support in case of employment loss. Insecurity about losing one's job can be expected to decline as the income of other household member increases (Anderson and Pontusson, 2007). Applied work suggests a strong correlation between income and health (see e.g., Deaton, 1999). Analogously, the household total asset might serve as a similar function at the perception of job loss. In the systematic presence of such scenario, our estimate is biased. The direction of bias might depend on the covariance between income/asset and job insecurity. To test whether this bias presents in a significant way, we eliminate the household asset and the household income and then re-perform the evaluation for the fixed-effects IV, and the result in column (2) of Table 48 implies it is robust to this alternative specification.

From the workers' perspective, the employer-provided plan coverage and length of tenure might affect how they capture the job insecurity of mass layoff. At a firm level, the size scale of the firm is another cofounder impacting the worker's subjective perception of labor stability. We then account for these factors and the obtained result is shown in column (3). The third concern is that the perceived job insecurity is heterogeneous across employees with different family characteristics. As workers with more dependents to support might capture job insecurity in a more drastic way, thus, the characteristics of the family might be one of the endogeneities impacting job insecurity as well. Family characteristics such as the labor force status of the spouses and number of living children affect how the employees capture the job insecurity. Column (4) implies that our estimate is not sensitive to this specification. Further, in order to check the bias due to other characteristics of current or past job such as working conditions, experience, the bias due to lower motivation or less productivity, or any other factors relative to job relocation or promotion, we add the fourth set of specification, where we encompass other characteristics of labor force or psychological factors such as whether current job needs moving heavy loads, whether the current job requires lots of physical effort, number of jobs with missing dates, whether moved to less demanding work, whether enjoy work, whether current job requires stoop/kneel/crouch, whether required more difficult things, whether current job requires good eyesight, and whether the respondent is looking for a second job. The resulting set of the robust check is illustrated in column (5) of Table 48. In any specifications, the outcome is consistently stable. Increased job insecurity is sizably related to the significant worsening of mental health and the emergence of new adverse health conditions. Thus, the concerns of endogeneity potentially arising from the sources noted above are not of major concern. Our estimates are robust to these alternative specifications.

The final concern is our estimates are based on a cohort of workers aged between 50 and 64 years old (near elderly and prior to retirement). However, senior workers generally tend to react to job insecurity in a different way than junior counterparts on average as resuming to the status of labor if dismissed is differentiated, accompanied by a lower likelihood of returning back to the labor market for this senior cohort (OECD, 2015). [51] In such a case, people might be concerned that the results herein are driven by a particularly significant effect of job insecurity on health for certain specific age cohorts. We check that our findings with regard to concerned health outcomes, it is robust to the inclusion of a broader range of workers by re-estimating our aforementioned fixed-effects IV on the new cohorts (45-68 years old cohort).[52] The results are qualitatively unchanged. We cannot extend the age to much younger or older individuals due to two reasons: either sample size is too small or the majority of the advanced elderly are not in the labor force already. The inclusion of the latter might bias our estimate downward as this category of individuals usually reaches the retirement age, or the resulting comparative low productivity leads to their expectation of being dismissed from the labor force if any exists.

Overall, we consider the estimates in the sensitivity check as indicating a reasonable degree of robustness to the sets of additional control variables and specifications.

6 CONCLUSIONS AND DISCUSSIONS

Our study provides evidence that subjective job insecurity significantly worsens mental health and raises the likelihood of incidence for new health conditions and thus increases the risk of exposure to severe-adverse health shocks such as cancer, heart disease and stroke for near-elderly workers. We

[51] Denote Organization for Economic Co-operation and Development.
[52] The results are omitted for the purpose of conserving space. The outcomes are available upon request.

do not observe the sizable impact on other health outcomes such as self-rated health, BMI/obesity, smoking and drinking take-up in the IV specification. However, gender- and education- based subpopulation responds to job insecurity in different ways. Male workers are more likely to suffer more in mental health than their counterparts, while better-educated individuals are more likely to increase the likelihood of obesity than less-educated individuals. The negative impact of job insecurity on health outcomes does not imply a time-lagged effect, at least within a four-year period. And employability does not systematically change the qualitative impact on health by job insecurity; however, it compromises the magnitude of negative effect to a large extent. Employability is negatively correlated with job insecurity. Workers with high employability respond to health in a systematically different way compared with workers with low employability. The subjective perception of job insecurity is a highly significant predictor of subsequent job displacement and individual earnings loss. Neither is it a trivial measure in that the firm-level reduction in employment effectively anticipates job insecurity.

Given the severity of the early 2000s economic recession in the U.S., it is feasible that our results somewhat overstate the average impact of job insecurity on negative health shocks. However, the qualitative effects of job insecurity on other aspects of workers' health outcomes are reasonably robust across firms, industries, and occupations. It, therefore, provides a good indication of the direction and rough magnitude of the effects that can be expected for the near-retirement workers. We show the causal effect of job insecurity on various health outcomes in a sample of near-elderly workers in the United States. We instrument job insecurity by the firm-level dismissal indicator, which is a more precise IV than other IV(s) employed (e.g., national or regional unemployment rate) in the documented literature. Investigating data from the longitudinal survey, we indicate that when individual-level fixed effects are not taken into account at all, the potential endogeneity with respect to job insecurity tends to deteriorate almost all of the health outcomes except smoking/drinking take-up in the pooled-OLS estimate. When accounting for the endogeneity of job insecurity in its IV specification, the qualitative result does not change essentially. On the other hand, the fixed-effects IV specification implies that the negative correlation is limited to a certain scope such as mental health when encompassing individual-level unobservable. Specifically, gender- or education- based sub-samples respond to job insecurity in more sensitive and idiosyncratic ways than their counterparts for some health outcomes such as obesity. Some respects the findings of this study reinforce the negative effects of job insecurity on health reported in the documented literature. Thus we provide a complementary and positive reference for possible policy intervention in the ever-changing labor context.

One caveat is that we neither differentiate the impact on the near elderly from those much younger (e.g., younger than 40) nor advanced elderly cohorts (e.g., older than78). Potentially they might not be completely representative of those of the typical job-insecurity workers and the latter two cohorts could show disparate characteristics. Workers of near elderly are generally more prepared for a new transition to retirement. However, for below 50-year old workers, the negative consequences of job insecurity or layoff are much severer. As their increasing family responsibilities and curtailed financial resources are more stressing and their “spillover” cost of real layoff or potential layoff is socially much oppressive and wider. Thus the comparison of the nuance is to be taken as one of the future extensions. Further, neither do we account for cultural effects. Nevertheless, cultural values such as individualism and collectivism could moderate the relationships between job insecurity and personal-level health outcomes (Greenhalgh and Rosenblatt, 2010). Further, although our study does take into account employability, it does not take into account other potential moderators such as unionization status either (Sverke and Hellgren, 2002). Regardless of the level of job insecurity, union co-workers typically express more loyalty to the organization than their non-unionized counterparts. Counterfactually, high job insecurity might lead to enhanced loyalty reactions in order to redress one's attractiveness in the organization and thereby possibly remedying insecurity (Hirschman, 1972). Unfortunately, the missing or poor measure regarding these variables in HRS prohibits a comparable estimation. Thus future research would lie in the impact on the health-damaging effects combined together with relative moderators if the commensurate level of data is available.

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Appendix A

Table 1 HEALTH OUTCOMES SUMMARY BY YEAR

		2002	2004	2006	2008	2010	2012	2014
Mental Health	N	3,119	3,532	3,523	3,322	3,685	3,580	3,272
	Mean	1.635	1.611	1.659	1.538	1.542	1.567	1.543
	Std. Dev.	2.049	2.051	2.088	2.035	2.049	2.058	2.043
Drug Utilization	N	3,453	3,832	3,744	3,483	3,852	3,709	3,394
	Mean	.938	.939	.947	.949	.935	.941	.939
	Std. Dev.	.240	.237	.223	.219	.246	.234	.239
Mortality	N	3,454	3,832	3,744	3,483	3,853	3,715	3,395
	Mean	.0083	.014	.008	.008	.007	.007	0
	Std. Dev.	.0912	.120	.093	.093	.084	.088	0
OOP ($/Month)	N	2,582	2,871	2,304	2,601	2,893	2,784	2,192
	Mean	148.57	174.91	91.42	84.52	92.19	90.61	80.64
	Std. Dev.	1085.34	708.09	323.34	162.93	180.48	165.73	140.58
Part D	N	-	-	3,744	3,483	3,853	3,715	3,395
	Mean	-	-	.260	.216	.167	.162	.164
	Std. Dev.	-	-	.439	.411	.373	.368	.370

Table 2 SUMMARY STATISTICS BY ELIGIBILITY

	Treatment Group (MD eligible)			Control Group (MD ineligible)		
Variable	N	Mean	Std. Dev.	N	Mean	Std. Dev.
Age	14,479	69.1	2.563	11,021	58.4	3.849
Gender:						
Male	14,479	.500	.500	11,021	.462	.498
Female	14,479	.499	.500	11,021	.537	.498
Race:						
White/Caucasians	14,479	.797	.401	11,021	.702	.457
Black/African	14,479	.159	.366	11,021	.198	.398
Other Race	14,479	.042	.201	11,021	.099	.298
Marital Status:						
Married	14,479	.659	.473	11,021	.710	.453
Never Married	14,479	.024	.153	11,021	.039	.194
Other Marriage Type	14,479	.316	.464	11,021	.249	.432
Education:						
High School Or Less	14,479	.818	.385	11,021	.776	.416
College or Higher	14,479	.181	.385	11,021	.223	.416
Household Income	14,479	55574.8	85250.3	11,021	79575.9	169075.9

Table 3 PRIOR-MD PARALLEL TRENDS

	(1) Mental Health	(2) Drug Utilization	(4) OOP	(5) Mortality
I(Age≥65)×2002	0.119	-0.103	2.278	-0.0587
	(0.104)	(0.0738)	(55.09)	(0.0783)
I(Age≥65)×2004	0.0740	-0.0893	15.52	-0.0400
	(0.103)	(0.0769)	(27.25)	(0.0790)
I(Age≥65)×2006	-0.0914	-	-	-0.0701
	(0.105)	-	-	(0.0799)
I(Age≥65)×2008	-0.0497	-0.0919	-13.41	-0.0759
	(0.1000)	(0.0751)	(8.774)	(0.0822)
I(Age≥65)×2010	-0.147	-0.0189	-20.96**	-0.0512
	(0.0898)	(0.0731)	(9.197)	(0.0865)
I(Age≥65)×2012	-0.142*	-0.0197	-0.958	-
	(0.0827)	(0.0699)	(8.316)	-
Year Dummies	X	X	X	X
Control Covariates	X	X	X	X
N	24053	19781	15930	3888
R^2	0.059	0.010	0.005	0.032

Notes: Standard errors in parentheses, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.The estimate results in our study are clustered at the household level. Control covariates including demographics are employed in the estimate but omitted in the result for brevity purpose. The covariates include age, age squared, gender, race, marital status, education, and the indicator for the MD-eligible cohort.

Table 4 MENTAL HEALTH DD RESULT

	(1)Mental Health ~2006	(2)Mental Health ~2008	(3)Mental Health ~2010	(4)Mental Health ~2012	(5)Mental Health ~2014
Post×I(Age≥65)	-0.189**	-0.166**	-0.193***	-0.203***	-0.184***
	(0.080)	(0.072)	(0.068)	(0.067)	(0.067)
Treatment	0.212**	0.245***	0.215***	0.222***	0.223***
	(0.082)	(0.075)	(0.070)	(0.068)	(0.067)
Age	-0.108	-0.128*	-0.107*	-0.107*	-0.095*
	(0.081)	(0.075)	(0.062)	(0.056)	(0.052)
Age Squared	0.001	0.001	0.001	0.001	0.001
	(0.001)	(0.001)	(0.000)	(0.000)	(0.000)
Female	0.357***	0.347***	0.333***	0.331***	0.332***
	(0.052)	(0.048)	(0.043)	(0.041)	(0.039)
Black/African Americans	0.175**	0.130*	0.128**	0.131**	0.119**
	(0.074)	(0.068)	(0.061)	(0.057)	(0.054)
Other Race	0.386***	0.376***	0.370***	0.365***	0.364***
	(0.125)	(0.116)	(0.099)	(0.089)	(0.082)
Never Married	0.555***	0.567***	0.597***	0.565***	0.510***
	(0.168)	(0.155)	(0.138)	(0.121)	(0.112)
Other Marriage Type	0.724***	0.700***	0.673***	0.643***	0.619***
	(0.064)	(0.059)	(0.053)	(0.050)	(0.047)
College or Higher	-0.650***	-0.668***	-0.657***	-0.627***	-0.626***
	(0.055)	(0.050)	(0.046)	(0.044)	(0.042)
N	10180	13505	17195	20777	24053
R^2	0.068	0.067	0.064	0.061	0.059

Notes: Standard errors in parentheses, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.The estimate results in our study are clustered at the household level. Control covariates also include wave dummies but are omitted in the result for the purpose of conserving space.

Table 5 IMPACT OF MD ON PATIENTS' WELL-BEING

	(1)~2006	(2) ~2008	(3) ~2010	(4) ~2012	(5) ~2014
(1-1)Mental Health Baseline	-0.189**	-0.166**	-0.193***	-0.203***	-0.184***
	(0.080)	(0.072)	(0.068)	(0.067)	(0.067)
(1-2)Mental Health Baseline	-0.197**	-0.166**	-0.183***	-0.183***	-0.158**
+Log(income)	(0.079)	(0.071)	(0.067)	(0.066)	(0.066)
(2-1)Drug Utilization Base-	-	0.013	0.023**	0.024***	0.028***
line		(0.011)	(0.009)	(0.009)	(0.009)
(2-2)Drug Utilization Base-	-	0.013	0.023**	0.023***	0.023***
line + Log(income)		(0.011)	(0.009)	(0.009)	(0.009)
(3-1)OOP Baseline	-	-22.936	-27.276	-21.045	-18.552
		(30.549)	(30.008)	(30.140)	(30.206)
(3-2)OOP Baseline	-	-21.532	-25.145	-18.788	-18.788
+Log(income)		(31.471)	(31.422)	(31.606)	(31.606)
(4-1)Mortality Baseline	-0.018	-0.020	-0.017	-0.011	-
	(0.021)	(0.018)	(0.017)	(0.018)	-
(4-2)Mortality Baseline	-0.018	-0.020	-0.016	-0.010	-
+Log(income)	(0.020)	(0.018)	(0.017)	(0.018)	-

Notes: Standard errors in parentheses, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.The estimate results in our study are clustered at the household level. For drug utilization and OOP, wave 2006 is dropped due to ambiguous data coverage. For mortality, data in wave 2014 is not available.

Table 6 DRUG UTILIZATION DD RESULT

	(1)Drug Utilization ~2008	(1)Drug Utilization ~2010	(1)Drug Utilization ~2012	(1)Drug Utilization ~2014
Post×I(Age≥65)	0.013	0.023**	0.024***	0.028***
	(0.011)	(0.009)	(0.009)	(0.009)
Treatment	0.002	-0.006	-0.008	-0.004
	(0.010)	(0.009)	(0.009)	(0.009)
Age	0.046***	0.053***	0.051***	0.045***
	(0.011)	(0.009)	(0.008)	(0.008)
Age Squared	-0.000***	-0.000***	-0.000***	-0.000***
	(0.000)	(0.000)	(0.000)	(0.000)
Female	0.025***	0.026***	0.024***	0.024***
	(0.006)	(0.006)	(0.005)	(0.005)
Black/African Americans	0.012	0.012*	0.011*	0.012*
	(0.008)	(0.007)	(0.007)	(0.006)
Other Race	-0.010	-0.015	-0.016	-0.020*
	(0.015)	(0.012)	(0.011)	(0.011)
Never Married	-0.029	-0.019	-0.010	-0.006
	(0.020)	(0.018)	(0.016)	(0.014)
Other Marriage Type	-0.012*	-0.015**	-0.015***	-0.018***
	(0.007)	(0.006)	(0.006)	(0.005)
College or Higher	0.012	0.010	0.006	0.005
	(0.008)	(0.007)	(0.006)	(0.006)
N	9792	13332	16760	19843
R^2	0.016	0.025	0.026	0.026

Notes: Standard errors in parentheses, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.The estimate results in our study are clustered at the household level. Control covariates also include wave dummies but are omitted in the result for the purpose of conserving space.

Table 7 OOP DD RESULT

	(1)OOP ~2008	(2)OOP ~2010	(3)OOP ~2012	(4)OOP ~2014
Post×I(Age≥65)	-22.936	-27.276	-21.045	-18.552
	(30.549)	(30.008)	(30.140)	(30.206)
Treatment	5.864	7.426	7.700	6.963
	(40.636)	(36.601)	(34.938)	(34.089)
Age	-8.862	-4.996	-6.540	-5.932
	(26.872)	(17.722)	(14.168)	(12.490)
Age Squared	0.075	0.044	0.056	0.052
	(0.205)	(0.136)	(0.110)	(0.097)
Female	25.174**	21.221**	17.132**	15.646**
	(12.673)	(9.661)	(7.911)	(6.951)
Black/African Americans	-47.252***	-30.797***	-20.589**	-17.504**
	(12.735)	(9.445)	(8.083)	(7.080)
Other Race	-32.611*	-12.952	-7.039	-8.010
	(16.748)	(14.259)	(11.087)	(9.241)
Never Married	1.838	-0.422	-7.247	-6.907
	(22.355)	(15.561)	(12.179)	(10.382)
Other Marriage Type	12.175	7.966	3.517	2.365
	(17.016)	(12.563)	(10.094)	(8.763)
College or Higher	16.074	11.269	8.560	6.035
	(30.621)	(21.951)	(17.129)	(14.524)
N	8057	10953	13738	15930
R^2	0.003	0.004	0.004	0.005

Notes: Standard errors in parentheses, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.The estimate results in our study are clustered at the household level. Control covariates also include wave dummies but are omitted in the result for the purpose of conserving space.

Table 8 MORTALITY DD RESULT

	(4)Mortality ~2006	(1) Mortality ~2008	(2) Mortality ~2010	(3)Mortality ~2012
Post×I(Age≥65)	-0.018	-0.020	-0.017	-0.011
	(0.021)	(0.018)	(0.017)	(0.018)
Treatment	0.007	0.017	0.015	0.010
	(0.014)	(0.014)	(0.014)	(0.014)
Age	-0.008	-0.010	-0.010	-0.004
	(0.014)	(0.013)	(0.013)	(0.014)
Female	-0.009	-0.007	-0.012*	-0.009
	(0.007)	(0.007)	(0.007)	(0.007)
Black/African Americans	-0.004	-0.011	-0.017**	-0.020**
	(0.009)	(0.008)	(0.008)	(0.008)
Other Race	-0.007	0.001	0.012	0.004
	(0.019)	(0.020)	(0.021)	(0.021)
Never Married	0.038	0.024	0.013	0.022
	(0.032)	(0.027)	(0.025)	(0.027)
Other Marriage Type	-0.001	0.000	-0.003	-0.002
	(0.008)	(0.008)	(0.008)	(0.008)
College or Higher	0.008	0.006	0.002	0.007
	(0.012)	(0.011)	(0.011)	(0.011)
N	2958	3484	3758	3888
R^2	0.009	0.008	0.013	0.032

Notes: Standard errors in parentheses, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.The estimate results in our study are clustered at the household level. Control covariates also include wave dummies but are omitted in the result for the purpose of conserving space.

Table 9 MENTAL HEALTH LATE EFFECT

		(1) Mental Health ~2006	(2) Mental Health ~2008	(3) Mental Health ~2010	(4) Mental Health ~2012	(5) Mental Health ~2014
OLS	PDC	-0.239***	-0.164***	-0.185***	-0.180***	-0.143***
		(0.056)	(0.044)	(0.039)	(0.037)	(0.036)
	N	10180	13505	17195	20777	24053
	R^2	0.070	0.068	0.065	0.062	0.059
IV-FS	PT	.086***	.426***	.540***	.587***	.606***
		(.017)	(.014)	(.013)	(.012)	(.012)
F Statistics		33.40	416.54	1111.17	1491.84	1646.55
IV-SS	PDC	-2.179**	-0.389**	-0.357***	-0.346***	-0.303***
		(1.011)	(0.168)	(0.125)	(0.114)	(0.110)
	N	10180	13505	17195	20777	24053
	R^2	.	0.066	0.064	0.061	0.058

Notes: Standard errors in parentheses, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.The estimated results in our study are clustered at the household level. FS denotes the first stage and SS denotes the second stage in the 2SLS regression. PDC denotes prescription drug coverage and PT denotes the interaction Post×I(Age≥65). In the regression results shown in the Table, control covariates including demographics are used in the estimate but omitted in the result for the purpose of conserving space. The covariates include age, age squared, gender, race, marital status, education, dummies for waves and indicator for MD-eligible patients. In the first-stage (IV-FS) estimation, the dependent variable is prescription drug coverage (PDC).

Table 10 DRUG UTILIZATION LATE EFFECT

		(1) Drug Utilization ~2008	(2) Drug Utilization ~2010	(3) Drug Utilization ~2012	(4) Drug Utilization ~2014
OLS	PDC	0.092***	0.078***	0.067***	0.060***
		(0.008)	(0.007)	(0.006)	(0.005)
	N	9792	13332	16760	19843
	R^2	0.041	0.041	0.038	0.035
IV-FS	PT	.800***	.771***	.756***	.738***
		(.014)	(.013)	(.013)	(.013)
F Statistics		2421.02	2354.56	2220.47	2081.78
IV-SS	PDC	0.016	0.030**	0.031***	0.037***
		(0.014)	(0.012)	(0.012)	(0.012)
	N	9792	13332	16760	19843
	R^2	0.024	0.035	0.034	0.033

Notes: Standard errors in parentheses, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.The estimated results in our study are clustered at the household level. FS denotes the first stage and SS denotes the second stage in the 2SLS regression. PDC denotes prescription drug coverage and PT denotes the interaction Post×I(Age≥65). In the regression results shown in the Table, control covariates including demographics are used in the estimate but omitted in the result for the purpose of conserving space. The covariates include age, age squared, gender, race, marital status, education, dummies for waves and indicator for MD-eligible patients. In the first-stage (IV-FS) estimation, the dependent variable is prescription drug coverage (PDC).

Table 11 OOP LATE EFFECT

		(1) OOP~2008	(2) OOP~2010	(3) OOP~2012	(4) OOP~2014
OLS	PDC	-102.395***	-78.988***	-66.165***	-57.798***
		(39.372)	(27.937)	(22.399)	(19.558)
	N	8057	10953	13738	15930
	R^2	0.007	0.007	0.006	0.007
IV-FS	PT	.825***	.800***	.782***	.767***
		(.016)	(.015)	(.014)	(.014)
F Statistics		2161.13	2119.96	1990.74	1863.18
IV-SS	PDC	-27.792	-34.060	-26.892	-24.183
		(36.941)	(37.395)	(38.447)	(39.313)
	N	8057	10953	13738	15930
	R^2	0.005	0.006	0.006	0.006

Notes: Standard errors in parentheses, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.The estimated results in our study are clustered at the household level. FS denotes the first stage and SS denotes the second stage in the 2SLS regression. PDC denotes prescription drug coverage and PT denotes the interaction Post×I(Age≥65). In the regression results shown in the Table, control covariates including demographics are used in the estimate but omitted in the result for the purpose of conserving space. The covariates include age, age squared, gender, race, marital status, education, dummies for waves and indicator for MD-eligible patients. In the first-stage (IV-FS) estimation, the dependent variable is prescription drug coverage (PDC).

Table 12 MORTALITY LATE EFFECT

		(1) Mortali-ty~2006	(2) Mortality ~2008	(3) Mortality ~2010	(4) Mortality ~2012
OLS	PDC	0.006	0.006	0.006	0.006
		(0.007)	(0.007)	(0.007)	(0.007)
	N	4535	5393	5875	6104
	R^2	0.007	0.007	0.011	0.030
IV-FS	PT	.145***	.405***	.478***	.502***
		(.042)	(.031)	(.028)	(.027)
F Statistics		8.79	26.68	44.98	52.77
IV-SS	PDC	-0.058	-0.028	-0.017	-0.007
		(0.137)	(0.042)	(0.034)	(0.033)
	N	4535	5393	5875	6104
	R^2	.	0.002	0.009	0.030

Notes: Standard errors in parentheses, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.The estimated results in our study are clustered at the household level. FS denotes the first stage and SS denotes the second stage in the 2SLS regression. PDC denotes prescription drug coverage and PT denotes the interaction term Post×I(Age≥65). In the regression results shown in the Table, control covariates including demographics are used in the estimate but omitted in the result for the purpose of conserving space. The covariates include age, age squared, gender, race, marital status, education, dummies for waves and indicator for MD-eligible patients. In the first-stage (IV-FS) estimation, the dependent variable is prescription drug coverage (PDC).

Table 13 ROBUSTNESS CHECK FOR MENTAL HEALTH

Conditions	(1)Mental Health ~2006	(2)Mental Health ~2008	(3)Mental Health ~2010	(4)Mental Health ~2012	(5)Mental Health ~2014
(1)I(Post)	-0.189** (0.080)	-0.169** (0.072)	-0.184*** (0.068)	-0.193*** (0.067)	-0.173*** (0.067)
(1)I(Post)+ (2)Census Division	-0.187** (0.080)	-0.172** (0.071)	-0.181*** (0.067)	-0.191*** (0.067)	-0.167** (0.066)
(1)I(Post)+ (2)Census Division + (3) unemployment rate	-0.188** (0.080)	-0.170** (0.071)	-0.192*** (0.067)	-0.201*** (0.067)	-0.174*** (0.066)
(1)I(Post)+ (2)Census Division+ (3)unemployment rate+ (4)unemployment rate × I(Post)	-0.188** (0.080)	-0.170** (0.071)	-0.193*** (0.067)	-0.203*** (0.067)	-0.177*** (0.066)

Notes: Standard errors in parentheses,* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.The estimated results in our study are clustered at the household level. I(Post) indicates the period post-MD. Exclude the one census division that is outside the U.S. The average unemployment rate is calculated at census-division level. DD denotes the difference-in-difference coefficient.

Table 14 ROBUSTNESS CHECK FOR DRUG UTILIZATION

Conditions	(1)Drug Utilization ~2008	(2) Drug Utilization ~2010	(3) Drug Utilization ~2012	(4) Drug Utilization ~2014
(1)I(Post)	0.013 (0.011)	0.023** (0.009)	0.023*** (0.009)	0.027*** (0.009)
(1)I(Post)+ (2)Census Division	0.013 (0.011)	0.024** (0.009)	0.024*** (0.009)	0.028*** (0.009)
(1)I(Post)+ (2)Census Division + (3) unemployment rate	0.013 (0.011)	0.023** (0.009)	0.023*** (0.009)	0.028*** (0.009)
(1)I(Post)+ (2)Census Division+ (3)unemployment rate+ (4)unemployment rate × I(Post)	0.013 (0.011)	0.023** (0.009)	0.023*** (0.009)	0.028*** (0.009)

Notes: Standard errors in parentheses,* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.The estimated results in our study are clustered at the household level. I(Post) indicates the period post-MD. Exclude the one census division that is outside the U.S. The average unemployment rate is calculated at census-division level. DD denotes the difference-in-difference coefficient.

Table 15 ROBUSTNESS CHECK FOR OOP

Conditions	(1) OOP ~2008	(2) OOP ~2010	(3) OOP ~2012	(4) OOP ~2014
(1)I(Post)	-23.136 (30.706)	-27.908 (29.945)	-21.586 (30.132)	-18.855 (30.251)
(1)I(Post)+ (2)Census Division	-23.336 (31.230)	-27.951 (30.161)	-21.750 (30.363)	-19.016 (30.572)
(1)I(Post)+ (2)Census Division + (3) unemployment rate	-23.085 (30.611)	-27.557 (30.343)	-21.442 (30.436)	-18.643 (30.600)
(1)I(Post)+ (2)Census Division+ (3)unemployment rate+ (4)unemployment rate × I(Post)+	-23.085 (30.648)	-27.035 (30.212)	-20.907 (30.448)	-18.175 (30.672)

Notes: Standard errors in parentheses, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.The estimated results in our study are clustered at the household level. I(Post) indicates the period post-MD. Exclude the one census division that is outside the U.S. The average unemployment rate is calculated at census-division level. The coefficient denotes the difference-in-difference result.

Table 16 ROBUSTNESS CHECK FOR MORTALITY

	(1) Mortality ~2006	(2) Mortality ~2008	(3) Mortality ~2010	(4) Mortality ~2012	(5) Mortality ~2014
(1)I(Post)	-0.019 (0.021)	-0.022 (0.018)	-0.021 (0.017)	-0.021 (0.017)	-0.021 (0.017)
(1)I(Post)+ (2)Census Division	-0.019 (0.021)	-0.021 (0.018)	-0.020 (0.017)	-0.020 (0.017)	-0.020 (0.017)
(1)I(Post)+ (2)Census Division + (3) unemployment rate	-0.018 (0.021)	-0.021 (0.018)	-0.019 (0.017)	-0.016 (0.017)	-0.016 (0.017)
(1)I(Post)+ (2)Census Division+ (3)unemployment rate+ (4)unemployment rate × I(Post)	-0.017 (0.020)	-0.020 (0.018)	-0.016 (0.017)	-0.014 (0.018)	-0.014 (0.018)

Notes: Standard errors in parentheses, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.The estimated results in our study are clustered at the household level. I(Post) indicates the period post-MD. Exclude the one census division that is outside the U.S. The average unemployment rate is calculated at census-division level. The coefficient denotes the difference-in-difference result.

Appendix B

Table 17 SUMMARY STATISTICS BY ADLS

NO INFORMAL CARE (ADLS)				INFORMAL CARE (ADLS)		
Variable	N	Mean	Std. Dev.	N	Mean	Std. Dev.
Long-term Care Insurance:						
No	61,995	0.890	0.313	1,302	0.939	0.240
Yes	61,995	0.110	0.313	1,302	0.061	0.240
#TSP	61,607	1.550	0.874	1,300	1.642	0.836
#Private Plan	62,005	0.707	0.572	1,302	0.262	0.454
OOP	62,050	3047	10314	1,306	5063	19945
Own Child:						
No		0.032	0.177		0.003	0.055
Yes		0.968	0.177		0.997	0.055
Age		62.0	6.221		61.8	6.371
Labor Status:						
Stop working		0.497	0.500		0.911	0.285
Continue work		0.503	0.500		0.089	0.285
Race:						
White/Caucasian		0.759	0.428		0.492	0.500
Black/African		0.172	0.377		0.371	0.483
Other Race		0.069	0.254		0.137	0.344
Gender:						
Male		0.425	0.494		0.181	0.386
Female		0.575	0.494		0.819	0.386
Education:						
<=High School		0.781	0.414		0.936	0.244
>=College		0.219	0.414		0.064	0.244
# Living Siblings:						
0		0.094	0.292		0.086	0.280
>=1		0.906	0.292		0.914	0.280
# Living Children:						
1		0.103	0.304		0.077	0.266
>=2		0.897	0.304		0.923	0.266
Marital Status:						
married		0.699	0.459		0.391	0.488
married, sp/absent		0.005	0.074		0.008	0.087
separated		0.023	0.150		0.093	0.290
divorced		0.143	0.350		0.223	0.416
widowed		0.106	0.308		0.230	0.421
never married		0.023	0.150		0.055	0.228
Resident Child		0.423	0.767		0.884	0.858
Household Income		74009	189310		25714	43190
Household Asset		432928	1102741		111532	700323
Health Level:						
Excellent		0.122	0.328		0.010	0.099
Very Good		0.312	0.463		0.037	0.188
Good		0.315	0.465		0.119	0.324
Fair		0.185	0.388		0.377	0.485
Poor		0.065	0.247		0.458	0.498
N	62,050			1,306		

Notes: Data are from the HRS sampled from wave 2002 to 2012.

Table 18 SUMMARY STATISTICS BY IADLS

NO INFORMAL CARE (IADLS)				INFORMAL CARE (IADLS)		
Variable	N	Mean	Std. Dev.	N	Mean	Std. Dev.
Long-term Care Insurance:						
No	61,316	0.889	0.314	1,981	0.953	0.212
Yes	61,316	0.111	0.314	1,981	0.047	0.212
#TSP	60,940	1.549	0.874	1,967	1.652	0.841
#Private Plan	61,326	0.711	0.571	1,981	0.274	0.468
OOP	61,374	3024.2	9624.7	1,982	5088.2	26892.3
Own Child:						
No		0.032	0.177		0.012	0.107
Yes		0.968	0.177		0.988	0.107
Age		62.048	6.221		62.598	6.301
Labor Status:						
Stop working		0.492	0.500		0.913	0.282
Continue work		0.508	0.500		0.087	0.282
Race:						
White/Caucasian		0.761	0.427		0.521	0.500
Black/African		0.170	0.376		0.350	0.477
Other Race		0.069	0.253		0.129	0.335
Gender:						
Male		0.428	0.495		0.195	0.396
Female		0.572	0.495		0.805	0.396
Education:						
<=High School		0.779	0.415		0.941	0.236
>=College		0.221	0.415		0.059	0.236
# Living Siblings:						
0		0.094	0.292		0.088	0.284
=0		0.093	0.292		0.088	0.284
>=1		0.906	0.292		0.912	0.284
# Living Children:						
1		0.103	0.304		0.075	0.264
>=2		0.897	0.304		0.925	0.264
Marital Status:						
married		0.703	0.457		0.372	0.484
married, Sp/absent		0.005	0.073		0.013	0.114
separated		0.023	0.150		0.072	0.258
divorced		0.142	0.349		0.234	0.424
widowed		0.104	0.305		0.252	0.434
never married		0.023	0.149		0.057	0.232
Resident Child		0.421	0.767		0.779	0.839
Household Income		74533	190216		25967	44941
Household Asset		436715	1108354		103890	559385
Health Level:						
Excellent		0.124	0.329		0.012	0.107
Very Good		0.315	0.465		0.031	0.173
Good		0.317	0.465		0.132	0.338
Fair		0.182	0.386		0.402	0.490
Poor		0.062	0.241		0.424	0.494
Covariates N	61,374			1,982		

Notes: Data are from the HRS sampled from wave 2002 to 2012. The sample is restricted to those 50-72-year-old individuals, who have at least one living child, do not receive any informal from other relatives and do not reside in a nursing home at the time of the survey. Private denotes the private plans other than long-term care insurance. LTC means private long-term care excluding government plans. OOP denotes out-of-pocket payment during the past two years. TSP means total supplement plans for public and private.

Table 19 INFORMAL CARE AND PARENT'S HEALTH INSURANCE (FE)

	(1)Private	(2)Private	(3)Private	(1)TSP	(2)TSP	(3)TSP
Informal Care(ADL)	-0.0201			-0.00444		
	(0.0144)			(0.0290)		
Informal Care(IADL)		-0.00925			-0.0144	
		(0.0122)			(0.0238)	
ADL and IADL Care			-0.00655			-0.0125
			(0.0178)			(0.0367)
N	63307	63307	63307	62907	62907	62907
R^2	0.069	0.069	0.069	0.076	0.076	0.076
	(1)LTC	(2)LTC	(3)LTC	(1)OOP	(2) OOP	(3) OOP
Informal Care(ADL)	0.0131			155.1		
	(0.00835)			(1012.5)		
Informal Care(IADL)		0.00653			1281.7**	
		(0.00687)			(651.4)	
ADL and IADL Care			0.0131			1319.4
			(0.0101)			(1385.6)
N	63297	63297	63297	63356	63356	63356
R^2	0.005	0.005	0.005	0.002	0.002	0.002

Notes: Standard errors in parentheses,* p<0.10, ** p<0.05, *** p<0.01. Standard errors are clustered at the household level. The covariates in the baseline estimate include the parent's age, age squared, marital status, labor status of the respondent. Fixed effects include dummies of waves from 2002 to 2012 and individuals. Outputs about these co-variates and constant are omitted for the purpose of space conservation. Private denotes the private plans other than long-term care insurance. LTC means private long-term care excluding government plans. OOP denotes out-of-pocket payment during the past two years. TSP means total supplement plans for public and private. ADLs are defined as the Activities of Daily Living indices, including bathing, eating, dressing, walking across a room, and getting in or out of bed. In contrast, IADLs include activities such as using a telephone, taking medication, handling money, shopping, preparing meals.

Table 20 INFORMAL CARE AND PARENT'S HEALTH INSURANCE (IV)

	(1)Private Plan	(2)Private Plan	(3)Private Plan	(1)TSP	(2)TSP	(3)TSP
ADL Care	0.465			1.478*		
	(0.529)			(0.835)		
IADL Care		0.322			1.003*	
		(0.366)			(0.560)	
ADL and IADL Care			0.675			2.108*
			(0.769)			(1.206)
First Stage	.01***	.014***	.007***	.01***	.007***	.007***
	(.0013)	(.0016)	(.001)	(.0013)	(.0012)	(.0012)
F Statistic	55.92	71.02	38.72	54.39	37.521	34.97
N	59447	59447	59447	59071	59071	59071
	(1)LTC	(2)LTC	(3)LTC	(1)OOP	(2) OOP	(3) OOP
ADL Care	-0.514*			-25481.4**		
	(0.264)			(11899.8)		
IADL Care		-0.357**			-17497.5**	
		(0.181)			(8093.0)	
ADL and IADL Care			-0.747*			-36615.5**
			(0.390)			(17409.8)
First Stage	.0096***	.0138***	.0066***	.0095***	.014***	.0066***
	(.001)	(.0016)	(.001)	(.0013)	(.0016)	(.001)
F Statistic	55.91	71.01	38.71	54.28	71.00	38.68
N	59438	59438	59438	59517	59517	59517

Notes: Standard errors in parentheses,* p<0.10, ** p<0.05, *** p<0.01. Standard errors are clustered at the household level. The covariates in the baseline IV estimate include parent's age, age squared, marital status, labor status of the respondent, dummies of waves from 2002 to 2012 and individuals' fixed effects. Outputs about these covariates and constant are omitted for the purpose of space conservation. The IV is the geographic proximity from the parent to the closest child. Private denotes the private plans other than long-term care insurance. LTC means private long-term care excluding government plans. OOP denotes out-of-pocket payment during the past two years. TSP means total supplement plans for public and private. ADLs are defined as the Activities of Daily Living indices, including bathing, eating, dressing, walking across a room, and getting in or out of bed. In contrast, IADLs include activities such as using a telephone, taking medication, handling money, shopping, preparing meals. The disability level increases as the corresponding number of ADLs/IADLs increases.

Table 21 HOURS-BASED INFORMAL CARE ESTIMATE (IV)

	(1) LTC	(2) LTC	(3) LTC	(4) LTC
Hours children helped last month	-0.00257* (0.00133)	-0.00429** (0.00200)	-0.00466** (0.00212)	-0.00458** (0.00210)
First Stage	1.923*** (.294)	1.443*** (.254)	1.381*** (.251)	1.394*** (.253)
F Statistic	42.67	32.27	30.23	30.42
Baseline	Yes	Yes	Yes	Yes
Control A		Yes	Yes	Yes
Control B			Yes	Yes
Control C				Yes
N	59438	52429	52043	52026

Notes: Standard errors in parentheses,* p<0.10, ** p<0.05, *** p<0.01. Standard errors are clustered at the household level. The control covariate A in all of the estimates includes the log of the household income and the log of the household asset. The control covariate B includes the number of living children, the number of living siblings, the number of drinking per day, current smoking status and Self-rated health level. The control covariate C includes the number of own children, the number of step-children, the number of children who help with health cost and the number of children working part-time. Fixed effects include dummies of waves from 2002 to 2012 and individuals. Outputs about these covariates and constant are omitted for brevity purpose. The instrumental variable is the geographic distance to the closest child. LTC denotes long-term care.

Table 22 DAYS-BASED INFORMAL CARE ESTIMATE (IV)

	(1) LTC	(2) LTC	(3) LTC	(4) LTC
Days children helped last month	-0.00990** (0.00501)	-0.0161** (0.00721)	-0.0172** (0.00748)	-0.0171** (0.00749)
First Stage	.499*** (.052)	.385*** (.045)	.374*** (.045)	.374*** (.045)
F Statistic	89.54	72.80	69.58	69.11
Baseline	Yes	Yes	Yes	Yes
Control A		Yes	Yes	Yes
Control B			Yes	Yes
Control C				Yes
N	59438	52429	52043	52026

Notes: Standard errors in parentheses,* p<0.10, ** p<0.05, *** p<0.01. Standard errors are clustered at the household level. The control covariate A in all of the estimates includes the log of the household income and the log of the household asset. The control covariate B includes the number of living children, the number of living siblings, the number of drinking per day, current smoking status and Self-rated health level. The control covariate C includes the number of own children, the number of step-children, the number of children who help with health cost and the number of children working part-time. Fixed effects include dummies of waves from 2002 to 2012 and individuals. Outputs about these covariates and constant are omitted for brevity purpose. The instrumental variable is the geographic distance to the closest child. LTC denotes long-term care.

Table 23 HOURS-BASED INFORMAL CARE ESTIMATE (IV)

	(1) TSP	(2) TSP	(3) TSP	(4) TSP
Hours children helped last month	0.00718*	0.00994*	0.0110*	0.0106*
	(0.00409)	(0.00586)	(0.00619)	(0.00613)
First Stage	1.964***	1.484***	1.423***	1.436***
	(.296)	(.255)	(.253)	(.254)
F Statistic	43.99	33.68	31.64	31.82
Baseline	Yes	Yes	Yes	Yes
Control A		Yes	Yes	Yes
Control B			Yes	Yes
Control C				Yes
N	59071	52107	51723	51708

Notes: Standard errors in parentheses,* p<0.10, ** p<0.05, *** p<0.01. Standard errors are clustered at the household level. The control covariate A in all of the estimates includes the log of the household income and the log of the household asset. The control covariate B includes the number of living children, the number of living siblings, the number of drinking per day, current smoking status and Self-rated health level. The control covariate C includes the number of own children, the number of step-children, the number of children who help with health cost and the number of children working part-time. Fixed effects include dummies of waves from 2002 to 2012 and individuals. Outputs about these covariates and constant are omitted for brevity purpose. The instrumental variable is the geographic distance to the closest child. TSP means total supplement plans for public and private.

Table 24 DAYS-BASED INFORMAL CARE ESTIMATE (IV)

	(1) TSP	(2) TSP	(3) TSP	(4) TSP
Days children helped last month	0.0278*	0.0376*	0.0408*	0.0401*
	(0.0155)	(0.0216)	(0.0224)	(0.0224)
First Stage	.507***	.392***	.381***	.381***
	(.052)	(.045)	(.045)	(.045)
F Statistic	91.73	74.17	71.01	70.46
Baseline	Yes	Yes	Yes	Yes
Control A		Yes	Yes	Yes
Control B			Yes	Yes
Control C				Yes
N	59071	52107	51723	51708

Notes: Standard errors in parentheses,* p<0.10, ** p<0.05, *** p<0.01. Standard errors are clustered at the household level. The control covariate A in all of the estimates includes the log of the household income and the log of the household asset. The control covariate B includes the number of living children, the number of living siblings, the number of drinking per day, current smoking status and Self-rated health level. The control covariate C includes the number of own children, the number of step-children, the number of children who help with health cost and the number of children working part-time. Fixed effects include dummies of waves from 2002 to 2012 and individuals. Outputs about these covariates and constant are omitted for brevity purpose. The instrumental variable is the geographic distance to the closest child. TSP means total supplement plans for public and private.

Table 25 INTERACTION EFFECTS BY DISABILITY LEVEL (ADLS)

		(1)LTC	(2)LTC	(3)TSP	(4) TSP
	ADL Care	-2.253*		7.093*	
		(1.174)		(3.859)	
	ADL Care ×ADL	0.629*		-2.005*	
	Disability	(0.323)		(1.054)	
	ADL Disability	-0.00769	-0.0115*	0.0358*	0.0529**
		(0.00642)	(0.00674)	(0.0188)	(0.0214)
	IADL Care		-0.695**		2.109**
			(0.343)		(1.061)
	IADL Care ×ADL		0.167**		-0.554**
	Disability		(0.0803)		(0.250)
First Stage:	Closest Child	-.0032***	.004***	-.003***	.0046***
(I)ADL Care		(.0009)	(.0013)	(.0009)	(.0013)
	Closest Child ×	.05***	.037***	.05***	.038***
	ADL Disability	(.003)	(.0037)	(.0034)	(.0037)
First Stage:	Closest Child	-.02***	-.013***	-.020***	-.013***
(I)ADL Care ×		(.0024)	(.0024)	(.0024)	(.0024)
ADL Disability	Closest Child ×	.18***	.167***	.19***	.168***
	ADL Disability	(.013)	(.014)	(.0126)	(.014)
	F statistic	10.16	23.36	9.7	23.92
	10% weak ID critical value	7.03	7.03	7.03	7.03
	N	59438	59438	59071	59071

Notes: Standard errors in parentheses,* p<0.10, ** p<0.05, *** p<0.01. Standard errors are clustered at the household level. The control covariates in all of the estimates include parent's age, age squared, marital status, labor status of the respondent, dummies of waves from 2002 to 2012 and fixed effects of individuals. Outputs about these covariates and constant are omitted for brevity purpose. The IVs include the geographic proximity to the closest child and its interaction with the parent's ADLs disability level. LTC means private long-term care excluding government plans. TSP means total supplement plans for public and private. ADLs are defined as the Activities of Daily Living indices, including bathing, eating, dressing, walking across a room, and getting in or out of bed. In contrast, IADLs include activities such as using a telephone, taking medication, handling money, shopping, preparing meals. The disability level increases as the corresponding number of ADLs/IADLs increases.

Table 26 INTERACTION EFFECTS BY DISABILITY LEVEL (IADLS)

		(1)LTC	(2)LTC	(3)TSP	(4) TSP
	ADL Care	-0.829**		2.228*	
		(0.399)		(1.259)	
	ADL Care ×IADL Disability	0.328**		-0.785*	
		(0.141)		(0.439)	
	IADL Disability	-0.0142	-0.00933	0.0641**	0.0504*
		(0.00912)	(0.00798)	(0.0301)	(0.0284)
	IADL Care		-0.530**		1.409*
			(0.255)		(0.785)
	IADL Care ×IADL Disability		0.235**		-0.566*
			(0.100)		(0.310)
First Stage: (I)ADL Care	Closest Child	.005***	.0063***	.005***	.006***
		(.0011)	(.0015)	(.0011)	(.0015)
	Closest Child × IADL Disability	.051***	.083***	.051***	.083***
		(.006)	(.0066)	(.005)	(.0066)
First Stage: (I)ADL Care × ADL Disability	Closest Child	-.004***	-.009***	-.004***	-.009***
		(.0011)	(.0013)	(.0011)	(.0013)
	Closest Child × IADL Disability	.154***	.218***	.154***	.218***
		(.014)	(.015)	(.014)	(.015)
	F statistic	21.04	27.93	20.33	28.68
	10% weak ID critical value	7.03	7.03	7.03	7.03
	N	59435	59435	59068	59068

Notes: Standard errors in parentheses,* p<0.10, ** p<0.05, *** p<0.01. Standard errors are clustered at the household level. The control covariates in all of the estimates include parent's age, age squared, marital status, labor status of the respondent, dummies of waves from 2002 to 2012 and fixed effects of individuals. Outputs about these covariates and constant are omitted for brevity purpose. The IVs include the geographic proximity to the closest child and its interaction with the parent's IADLs disability level. LTC means private long-term care excluding government plans. TSP means total supplement plans for public and private. ADLs are defined as the Activities of Daily Living indices, including bathing, eating, dressing, walking across a room, and getting in or out of bed. In contrast, IADLs include activities such as using a telephone, taking medication, handling money, shopping, preparing meals. The disability level increases as the corresponding number of ADLs/IADLs increases.

Table 27 TIME-BASED INFORMAL CARE DISABILITY-INTERACTION EFFECT (ADLS)

	(1)LTC	(2) LTC	(3)TSP	(4) TSP
Hours helped LM	-0.00697**		0.0205*	
	(0.00353)		(0.0107)	
Hours helped LM × ADL Disability	0.00167**		-0.00516**	
	(0.000836)		(0.00254)	
ADL Disability	-0.00573	-0.00675	0.0315**	0.0356**
	(0.00437)	(0.00456)	(0.0139)	(0.0147)
Days helped LM		-0.0214**		0.0642**
		(0.0105)		(0.0323)
Days helped LM× ADL Disability		0.00531**		-0.0171**
		(0.00253)		(0.00787)
F statistic	16.09	36.95	17.45	38.33
10% weak ID critical value	7.03	7.03	7.03	7.03
N	59438	59438	59071	59071

Notes: Standard errors in parentheses,* p<0.10, ** p<0.05, *** p<0.01. Standard errors are clustered at the household level. The control covariates in all of the estimates include parent's age, age squared, marital status, labor status of the respondent, dummies of waves from 2002 to 2012 and fixed effects of individuals. Outputs about these covariates and constant are omitted for brevity purpose. The IVs include the geographic proximity to the closest child and its interaction with the parent's ADLs disability level. LTC means private long-term care excluding government plans. TSP means total supplement plans for public and private. ADLs are defined as the Activities of Daily Living indices, including bathing, eating, dressing, walking across a room, and getting in or out of bed. In contrast, IADLs include activities such as using a telephone, taking medication, handling money, shopping, preparing meals. The disability level increases as the corresponding number of ADLs/IADLs increases.

Table 28 TIME-BASED INFORMAL CARE DISABILITY-INTERACTION EFFECT (IADLS)

	(1)LTC	(2) LTC	(3)TSP	(4) TSP
Hours helped LM	-0.00409**		0.0108*	
	(0.00202)		(0.00615)	
Hours helped LM × IADL Disability	0.00168**		-0.00416*	
	(0.000750)		(0.00230)	
IADL Disability	-0.0101	-0.00639	0.0583**	0.0463*
	(0.00773)	(0.00654)	(0.0261)	(0.0237)
Days helped LM		-0.0147**		0.0388*
		(0.00698)		(0.0215)
Days helped LM× IADL Disability		0.00622**		-0.0146*
		(0.00259)		(0.00801)
F statistic	18.25	37.12	19.07	38.25
10% weak ID critical value	7.03	7.03	7.03	7.03
N	59435	59435	59068	59068

Notes: Standard errors in parentheses,* p<0.10, ** p<0.05, *** p<0.01. Standard errors are clustered at the household level. The control covariates in all of the estimates include parent's age, age squared, marital status, labor status of the respondent, dummies of waves from 2002 to 2012 and fixed effects of individuals. Outputs about these covariates and constant are omitted for brevity purpose. The IVs include the geographic proximity to the closest child and its interaction with the parent's IADLs disability level. LTC means private long-term care excluding government plans. TSP means total supplement plans for public and private. ADLs are defined as the Activities of Daily Living indices, including bathing, eating, dressing, walking across a room, and getting in or out of bed. In contrast, IADLs include activities such as using a telephone, taking medication, handling money, shopping, preparing meals. The disability level increases as the corresponding number of ADLs/IADLs increases.

Table 29 DISTANCE-BASED IV ROBUSTNESS CHECK

	(1) LTC	(2) LTC	(3) LTC	(4) LTC	(5) LTC	(6) LTC	(7) LTC	(8) LTC	(9) LTC
ADL Care	-0.867**	-0.934**	-0.915**						
	(0.403)	(0.424)	(0.428)						
IADL Care				-0.542**	-0.580**	-0.565**			
				(0.246)	(0.256)	(0.257)			
ADL and IADL Care							-1.251**	-1.328**	-1.301**
							(0.599)	(0.623)	(0.627)
First Stage	.0071***	.007***	.007***	.011 ***	.011***	.011***	.0049***	.0048***	.0048***
	(.0012)	(.0012)	(.0012)	(.0015)	(.0016)	(.0016)	(.001)	(.001)	(.001)
F Statistic	36.87	34.45	33.46	53.44	50.57	49.75	25.58	24.42	23.86
Control A	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Control B		Yes	Yes		Yes	Yes		Yes	Yes
Control C			Yes			Yes			Yes
N	52429	52043	51668	52429	52043	51668	52429	52043	51668

Notes: Standard errors in parentheses,* p<0.10, ** p<0.05, *** p<0.01. Standard errors are clustered at the household level. The control covariate A in all of the estimates includes the log of the household income and the log of the household asset. The control covariate B includes the number of living children, the number of living siblings, the number of drinking per day, current smoking status and Self-rated health level. The control covariate C includes the number of own children, the number of step-children, the number of children who help with health cost and the number of children working part-time. Fixed effects include dummies of waves from 2002 to 2012 and individuals. Outputs about these covariates and constant are omitted for brevity purpose. The instrumental variable is the geographic distance to the closest child. LTC denotes long-term care. ADLs are defined as the Activities of Daily Living indices, including bathing, eating, dressing, walking across a room, and getting in or out of bed. In contrast, IADLs include activities such as using a telephone, taking medication, handling money, shopping, preparing meals.

Table 30 DISTANCE-BASED IV ROBUSTNESS CHECK

	(1) TSP	(2) TSP	(3) TSP	(4) TSP	(5) TSP	(6) TSP	(7) TSP	(8) TSP	(9) TSP
ADL Care	2.033*	2.233*	2.079*						
	(1.191)	(1.252)	(1.260)						
IADL Care				1.275*	1.386*	1.282*			
				(0.735)	(0.764)	(0.764)			
ADL and IADL Care							2.952*	3.187*	2.966
							(1.765)	(1.828)	(1.833)
First Stage	.007***	.007***	.007***	.014***	.012***	.012***	.005***	.0049***	.0049***
	(.0012)	(.0012)	(.0012)	(.0016)	(.0016)	(.0016)	(.001)	(.001)	(.001)
F Statistic	37.52	34.97	33.94	72.84	53.81	50.35	25.64	24.51	23.96
Control A	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Control B		Yes	Yes		Yes	Yes		Yes	Yes
Control C			Yes			Yes			Yes
N	52107	51723	51351	52107	51723	51351	52107	51723	51351

Notes: Standard errors in parentheses,* p<0.10, ** p<0.05, *** p<0.01. Standard errors are clustered at the household level. The control covariates A in all of the estimates include the log of the household income and the log of the household asset. The control covariates B include the number of living children, number of living siblings, number of drinking per day, current smoking status and Self-rated health level. The control covariates C include the number of own children, the number of step-children, the number of children who help with health cost and the number of children working part-time. Fixed effects include dummies of waves from 2002 to 2012 and individuals. Outputs about these covariates and constant are omitted for brevity purpose. The instrumental variable is the geographic distance to the closest child. TSP means total supplement plans for public and private. ADLs are defined as the Activities of Daily Living indices, including bathing, eating, dressing, walking across a room, and getting in or out of bed. In contrast, IADLs include activities such as using a telephone, taking medication, handling money, shopping, preparing meals.

Table 31 DISTANCE-BASED IV ENDOGENEITY CHECK

	(1)# children within 10miles	(2))# children within 10miles	(3))# children within 10miles
Parent’s Health Level	0.00311	0.00432	0.000465
	(0.00579)	(0.00597)	(0.00738)
N	59483	53467	53152
R^2	0.002	0.002	0.003
Disability Level(ADL)	0.00424	0.00677	0.00717
	(0.00835)	(0.00925)	(0.00955)
N	59483	53467	53152
R^2	0.002	0.002	0.003
Disability Level(IADL)	0.00220	0.0202	0.0217
	(0.0167)	(0.0186)	(0.0187)
N	59481	53466	53151
R^2	0.002	0.002	0.003
Baseline	Yes	Yes	Yes
Controls A	No	Yes	Yes
Controls B	No	No	Yes

Notes: Standard errors in parentheses,* p<0.10, ** p<0.05, *** p<0.01. Standard errors are clustered at the household level. The baseline controls include dummies of waves from 2002 to 2012, parent’s age, age squared, marital status, employment status and fixed effects of dummies and individuals. The control covariates A in all of the estimates include the log of the household income and the log of the household asset. The control covariates B include the number of living children, number of living siblings, number of drinking per day, current smoking status and Self-rated health level. The disability level increases as the corresponding number of ADLs/IADLs increases.

Appendix C

Table 32 SAMPLE SIZE AFTER EACH SELECTION CRITERIA

Sample Selection Criteria	N	Number of Individuals (person-wave)
Exclude if wave information missing	159,067	33,484
Exclude if census division is outside the U.S. and exclude if the occupation is army service	158,821	33,458
Exclude if the likelihood of losing one's job information is missing	158,065	33,441
Exclude other missing covariates	31,249	11,955
Restrict to ages from 50 to 64	22,711	9,932
Restrict to working and not self-employed workers only (Final Sample)	21,692	9,594

Table 33 SUMMARY STATISTICS BY GENDER

Male				Female		
Variable	N	Mean	Std. Dev.	N	Mean	Std. Dev.
Self-rated health	9,439	2.434	.948	12,249	2.414	.942
Mental Health	9,438	.955	1.486	12,250	1.185	1.750
Any New Health Conditions	7,815	.166	.372	10,665	.164	.371
BMI	9,409	28.754	4.907	11,974	28.441	6.126
Obesity	9,409	.331	.470	11,974	.338	.473
Drinking	9,439	.704	.456	12,250	.607	.488
Smoking	9,426	.180	.385	12,210	.168	.374
CFDZ		.279	.448		.263	.440
Job Insecurity		16.61	23.546		16.96	24.42
Age		57.4	3.694		57.00	3.847
Race:						
White/Caucasians		.789	.407		.772	.418
Black/Africans		.131	.338		.170	.376
Other Race		.078	.269		.056	.230
Education:						
High School or Less		.677	.467		.721	.448
College or Higher		.322	.467		.278	.448
Marriage:						
Married/Partnered		.840	.366		.701	.457
Divorced/ Separated		.106	.308		.177	.381
Widowed		.015	.123		.076	.265
Never married		.037	.190		.044	.206
Household Income		98007.8	107973.9		85928.9	109042.4
Household Asset		334346.3	736156.6		336728.6	1051027
Job Tenure (year):						
Tercile 1		.2761	.447		.279	.448
Tercile 2		.305	.460		.367	.482
Tercile 3		.418	.493		.353	.477
Firm Size (number of workers):						
<50		.425	.494		.466	.498
(50,100)		.158	.365		.174	.379
>100		.416	.492		.358	.479
Employer Plan Coverage:						
		.220	.414		.320	.466
		.779	.414		.679	.466
N(covariates)	9,440			12,252		

Notes: Sample is restricted to 50-64 years old working employees and not self-employed, from wave 1998 to wave 2014 with 2008 excluded.

Table 34 SUMMARY STATISTICS BY FIRM DOWNSIZE

No Permanent reduction in employment(N=15,837)			Permanent reduction in employment(N=5,855)	
Variable	Mean	Std. Dev.	Mean	Std. Dev.
Job Insecurity	14.76	22.75	22.33	26.44
Gender:				
Male	.429	.495	.450	.497
Female	.570	.495	.549	.497
Age	57.3	3.79	56.8	3.739
Race:				
White/Caucasians	.786	.409	.761	.426
Black/Africans	.147	.354	.170	.375
Other Race	.065	.247	.068	.252
Education:				
High School or Less	.713	.451	.671	.469
College or Higher	.286	.451	.328	.469
Marriage:				
Married/Partnered	.761	.426	.763	.424
Divorced/ Separated	.144	.351	.150	.357
Widowed	.053	.225	.039	.194
Never married	.039	.195	.046	.210
Household Income	89192.65	114099.7	96575.83	92502.3
Household Asset	334525.2	992407.7	338847.6	722035.6
Job Tenure (year):				
Tercile 1	.321	.467	.161	.367
Tercile 2	.339	.473	.342	.474
Tercile 3	.339	.473	.496	.500
Firm Size (number of workers):				
<50	.485	.499	.347	.476
(50,100)	.167	.373	.168	.374
>100	.346	.475	.483	.499
Employer Plan Coverage:				
No	.302	.459	.207	.405
Yes	.697	.459	.792	.405

Notes: Sample is restricted to 50-64 years old working employees and not self-employed. And time ranges from wave 1998 to 2014 with wave 2008 excluded.

Table 35 IMPACT OF JOB INSECURITY ON HEALTH (POOLED OLS)

	(1) Self-rated Health	(2) Mental Health	(3) Any New Health Conditions	(4) BMI	(5) Obesity	(6) Drinking	(7) Smoking
Job Insecurity	0.00301***	0.00707***	0.000333***	0.00462**	0.000322**	-0.000242	-0.00000660
	(0.000297)	(0.000576)	(0.000118)	(0.00195)	(0.000158)	(0.000154)	(0.000129)
Age	0.153***	0.109	0.0301	-0.253	-0.0382	-0.0258	0.0430**
	(0.0487)	(0.0841)	(0.0225)	(0.296)	(0.0253)	(0.0250)	(0.0201)
Age Squared	-0.00125***	-0.00110	-0.000251	0.00182	0.000306	0.000139	-0.000425**
	(0.000424)	(0.000733)	(0.000195)	(0.00257)	(0.000220)	(0.000218)	(0.000175)
Male	0.0327*	-0.155***	-0.000772	0.333***	-0.00418	0.0962***	0.0306***
	(0.0180)	(0.0283)	(0.00570)	(0.121)	(0.00998)	(0.00864)	(0.00787)
Black/African	0.102***	0.0111	0.00113	1.391***	0.0989***	-0.0771***	-0.0185
	(0.0251)	(0.0439)	(0.00845)	(0.192)	(0.0153)	(0.0150)	(0.0124)
Other Race	0.176***	0.145**	-0.00706	-0.416*	-0.0552***	-0.0936***	-0.0233
	(0.0371)	(0.0644)	(0.0114)	(0.233)	(0.0196)	(0.0196)	(0.0145)
College Or Higher	-0.241***	-0.147***	-0.0156**	-0.741***	-0.0526***	0.0235**	-0.0952***
	(0.0207)	(0.0317)	(0.00652)	(0.144)	(0.0118)	(0.0114)	(0.00851)
Divorced/Separated	-0.105***	0.196***	-0.00818	-0.785***	-0.0541***	0.0987***	0.0490***
	(0.0271)	(0.0476)	(0.00841)	(0.184)	(0.0145)	(0.0139)	(0.0125)
Widowed	-0.0897**	0.420***	-0.0176	0.109	0.0121	0.0352	0.0300
	(0.0385)	(0.0837)	(0.0127)	(0.297)	(0.0225)	(0.0216)	(0.0190)
Never Married	-0.110**	0.0463	-0.0113	0.0737	0.00245	0.0612***	0.0286
	(0.0470)	(0.0737)	(0.0147)	(0.334)	(0.0257)	(0.0233)	(0.0208)
log(Household Income)	-0.127***	-0.139***	-0.00381	-0.0721	-0.00623	0.0627***	-0.0152***
	(0.0124)	(0.0203)	(0.00413)	(0.0707)	(0.00587)	(0.00618)	(0.00485)
log(Household Asset)	-0.0810***	-0.0948***	-0.00288	-0.459***	-0.0300***	0.0254***	-0.0234***
	(0.00628)	(0.0108)	(0.00208)	(0.0429)	(0.00332)	(0.00330)	(0.00286)
2/3 quantiles of Current Job Tenure	0.0603***	0.0578*	-0.0111	-0.0138	0.00449	-0.0153	-0.00778
	(0.0186)	(0.0318)	(0.00689)	(0.121)	(0.00981)	(0.00978)	(0.00804)
3/3 quantiles of Current Job Tenure	0.0693***	0.0534	-0.00493	0.189	0.0241**	-0.0135	-0.0317***
	(0.0209)	(0.0348)	(0.00720)	(0.144)	(0.0117)	(0.0112)	(0.00908)
Firm Size(50,100)	-0.0124	0.0654*	0.00605	0.102	0.00496	0.0149	-0.000329
	(0.0233)	(0.0393)	(0.00784)	(0.156)	(0.0128)	(0.0120)	(0.0101)
Firm Size(>100)	0.00187	0.0264	0.00962	0.379***	0.0236**	-0.00445	0.000630
	(0.0190)	(0.0313)	(0.00622)	(0.132)	(0.0107)	(0.0101)	(0.00843)
Employer Plan Coverage	0.00383	0.0169	0.00622	0.547***	0.0316***	-0.00510	-0.00110
	(0.0187)	(0.0314)	(0.00631)	(0.127)	(0.0102)	(0.00952)	(0.00809)
N	21650	21650	18470	21345	21345	21651	21598
R^2	0.106	0.063	0.004	0.067	0.046	0.086	0.056

Notes: Standard errors in parentheses, standard errors are clustered at the household level,* p<0.10, ** p<0.05, *** p<0.01. Control covariates also include dummy indicators of waves (from 1998 to 2014 with 2008 excluded) and census divisions (with the census division outside the U.S. excluded).

Table 36 IMPACT OF JOB INSECURITY ON HEALTH (FIXED EFFECTS)

	(1) Self- Reported Health	(2) Mental Health	(3) Any New Health Conditions	(4) BMI	(5) Obesity	(6) Drinking	(7) Smoking
Job Insecurity	0.000102	0.00217***	-0.0000100	-0.000115	0.0000303	0.0000569	-0.0000121
	(0.000278)	(0.000626)	(0.000195)	(0.000772)	(0.000101)	(0.000131)	(0.0000747)
N	21650	21650	18470	21345	21345	21651	21598
R^2	0.022	0.010	0.004	0.038	0.013	0.008	0.012

Notes: Standard errors in parentheses, standard errors are clustered at the household level,* p<0.10, ** p<0.05, *** p<0.01. Control covariates also include age, age squared, marital status, log of the household asset, log of the household income, current job tenure length in year, firm size, whether the correspondent is covered by the employer's plan. Fixed effects include dummies of waves (from 1998 to 2014 with wave 2008 excluded), individuals and census divisions (with the division outside the U.S. excluded).

Table 37 NON-LINEAR ESTIMATE OF JOB INSECURITY

		(1) Self- Reported Health	(2) Mental Health	(3) Any New Health Conditions	(4) BMI	(5) Obesity	(6) Drinking	(7) Smoking
OLS	tercile 2	-0.00447	-0.0702**	0.000861	-0.132	-0.0152	0.0172*	-0.0172**
		(0.0173)	(0.0276)	(0.00696)	(0.110)	(0.00949)	(0.00934)	(0.00746)
	tercile 3	0.142***	0.308***	0.0121*	0.172	0.00796	-0.00262	-0.0120
		(0.0172)	(0.0306)	(0.00647)	(0.115)	(0.00932)	(0.00906)	(0.00743)
	N	21650	21650	18470	21345	21345	21651	21598
	R^2	0.105	0.062	0.004	0.067	0.047	0.086	0.057
FE	tercile 2	-0.0307**	-0.00233	0.00556	-0.0790*	-0.0129**	0.0141*	-0.00487
		(0.0155)	(0.0307)	(0.0114)	(0.0446)	(0.00623)	(0.00745)	(0.00392)
	tercile 3	-0.0182	0.0889***	-0.00250	-0.0327	-0.00315	0.00971	-0.000405
		(0.0159)	(0.0341)	(0.0113)	(0.0461)	(0.00615)	(0.00781)	(0.00435)
	N	21650	21650	18470	21345	21345	21651	21598
	R^2	0.022	0.010	0.004	0.038	0.013	0.008	0.012

Notes: Standard errors in parentheses, standard errors are clustered at the household level,* p<0.10, ** p<0.05, *** p<0.01. Control covariates also include dummy indicators of waves (from 1998 to 2014 with wave 2008 excluded) and census divisions (with the division outside the U.S. excluded). Control covariates are the ones discussed in the baseline specification. The result of the first lowest tercile is omitted.

Table 38 MARGINAL PREDICTION OF WELL-BEING LOSS

	(1)Income Loss	(2)Income Loss	(3)Layoff	(4)Layoff
Job Insecurity	.00108***	.0009***	.000349***	.000198***
	(.00021)	(.00021)	(.000051)	(.000048)
Year Dummies	No	Yes	No	Yes
Census-division Dummies	No	Yes	No	Yes
Covariates A	No	Yes	No	No
Covariates B	No	No	No	Yes
N	10,163	10,161	21,692	21,654
R^2	0.0029	0.0146	0.0032	0.0704

Notes: Standard errors in parentheses, standard errors are clustered at the household level,* p<0.10, ** p<0.05, *** p<0.01. Control covariates A include age, age squared, gender, race, education, marital status, current job tenure status, firm size, and employer-provided plan coverage status. Control covariates B include age, age squared, gender, race, education, marital status, current job tenure status, firm size, coverage status of the employer-provided plan, log of the household income and log of the household asset. The results shown in the table are the marginal effects.

Table 39 ANTICIPATION EFFECT OF FIRM-LEVEL LAYOFF

		(1) Job Insecurity(t)	(2) Job Insecurity(t)	(3) Job Insecurity(t)	(4) Job Insecurity(t)
OLS	Firm-level Layoff(t+1)	5.500***	5.797***	3.325***	3.830***
		(0.675)	(0.676)	(0.683)	(0.683)
	Firm-level Layoff(t)	-	-	6.302***	6.155***
				(0.678)	(0.687)
	R^2	0.012	0.047	0.027	0.061
FE	Firm-level Layoff(t+1)	-0.609	-0.508	0.412	0.535
		(0.990)	(1.016)	(1.024)	(1.061)
	Firm-level Layoff(t)			4.125***	3.926***
				(1.090)	(1.120)
	R^2	0.000	0.015	0.008	0.021
	Year Dummies	No	Yes	No	Yes
	Census-division Dummies	No	Yes	No	Yes
	Control Covariates	No	Yes	No	Yes
	N	6557	6555	6557	6555

Notes: Standard errors in parentheses, standard errors are clustered at the household level,* p<0.10, ** p<0.05, *** p<0.01. Control covariates include age, age squared, gender, race, education, marital status, current job tenure status, firm size, coverage status of the employer-provided plan, log of the household income and log of the household asset.

Table 40 IMPACT OF JOB INSECURITY ON HEALTH (POOLED-OLS IV)

	(1) Self- Reported Health	(2) Mental Health	(3) Any new health conditions	(4) BMI	(5) Obesity	(6) Drinking	(7) Smoking
Job Insecurity	0.00837***	0.0239***	0.00258***	0.0194	0.00180*	0.00127	0.000231
	(0.00187)	(0.00338)	(0.000774)	(0.0123)	(0.00102)	(0.000960)	(0.000776)
First Stage	8.574***	8.587***	8.609***	8.586***	8.586***	8.589***	8.595***
	(.421)	(.421)	(.455)	(.423)	(.423)	(.421)	(.422)
F Statistic	414.42	415.73	356.66	410.80	410.80	415.85	415.14
N	21650	21650	18470	21345	21345	21651	21598

Notes: Standard errors in parentheses, standard errors are clustered at the household level,* p<0.10, ** p<0.05, *** p<0.01. Control covariates also include gender, race, education, age, age squared, marital status, log of the household asset, log of the household income, current job tenure length in year, firm size, whether the correspondent is covered by employer's plan. Dummy controls include waves (from 1998 to 2014 but excludes 2008) and census divisions. F statistics range from 356.66 to 415.85, and first-stage coefficients range from 8.574 to 8.609 (all significant at conventional levels).

Table 41 IMPACT OF JOB INSECURITY ON HEALTH (FIXED-EFFECTS IV)

	(1) Self- Reported Health	(2) Mental Health	(3) Any new health conditions	(4) BMI	(5) Obesity	(6) Drinking	(7) Smoking
Job Insecurity	0.00210	0.0177***	0.00317*	0.00340	0.000722	0.000608	0.000703
	(0.00242)	(0.00548)	(0.00184)	(0.00696)	(0.000919)	(0.00113)	(0.000659)
First Stage	5.600***	5.610***	5.592***	5.579***	5.579***	5.617***	5.590***
	(.508)	(.508)	(.570)	(.510)	(.510)	(.508)	(.510)
F Statistic	121.54	121.95	96.00	119.32	119.32	122.21	120.20
N	18270	18271	14943	17952	17952	18272	18217

Notes: Standard errors in parentheses, standard errors are clustered at the household level,* p<0.10, ** p<0.05, *** p<0.01. Control covariates also include age, age squared, marital status, log of the household asset, log of household income, current job tenure length in year, firm size, whether the correspondent is covered by the employer's plan. Fixed effects include dummies of waves (from 1998 to 2014 but excludes 2008), individuals and census divisions. In the first-stage specification, the coefficients range from 5.579 to 5.622 (significant at conventional levels), F statistics range from 96.00 to 122.23.

Table 42 HETEROGENOUS EFFECTS BY GENDER
(FIXED-EFFECTS IV)

	Male		Female	
	Job Insecurity	N	Job Insecurity	N
(1)Self -Reported Health	0.000821 (0.00404)	7883	0.00361 (0.00304)	10387
(2)Mental Health	0.0206** (0.00860)	7882	0.0163** (0.00703)	10389
(3)Any New Health Conditions	0.00171 (0.00296)	6221	0.00425* (0.00236)	8722
(4)BMI	0.00197 (0.0101)	7854	0.00217 (0.00934)	10098
(5)Obesity	0.00174 (0.00157)	7854	0.0000924 (0.00114)	10098
(6)Drinking	-0.00171 (0.00174)	7882	0.00208 (0.00148)	10390
(7)Smoking	0.000564 (0.00114)	7870	0.000697 (0.000781)	10347

Notes: Standard errors in parentheses, standard errors are clustered at the household level,* p<0.10, ** p<0.05, *** p<0.01. Control covariates also include age, age squared, marital status, log of household income, log of the household asset, current job tenure length in year, firm size, whether the correspondent is covered by the employer's plan. Fixed effects include dummies of waves (from 1998 to 2014 but excludes 2008), individuals and census divisions. As the chow test cannot be applied to fixed-effect and IV model currently, we only derive the OLS-based chow test results and apply this policy to the subsequent analysis. For the seven outcomes listed, the p-values of comparing the difference of the two groups are 0.0866, 0.0980, 0.6515, 0.4515, 0.9543, 0.2068 and 0.2264 respectively. None of the difference of coefficients is statistically significant.

Table 43 HETEROGENOUS EFFECTS BY EDUCATION (FIXED-EFFECTS IV)

	College or Higher		High School or Lower	
	Job Insecurity	N	Job Insecurity	N
(1)Self -Reported Health	0.00960	5679	0.00000416	12591
	(0.00624)		(0.00258)	
(2)Mental Health	0.0339**	5681	0.0140**	12590
	(0.0137)		(0.00583)	
(3)Any New Health Conditions	0.00771	4590	0.00223	10353
	(0.00600)		(0.00182)	
(4)BMI	-0.00719	5618	0.00671	12334
	(0.0175)		(0.00745)	
(5)Obesity	0.00565**	5618	-0.000468	12334
	(0.00274)		(0.000944)	
(6)Drinking	0.00148	5679	0.000327	12593
	(0.00279)		(0.00120)	
(7)Smoking	-0.000884	5663	0.00109	12554
	(0.00121)		(0.000774)	

Notes: Standard errors in parentheses, standard errors are clustered at the household level,* p<0.10, ** p<0.05, *** p<0.01. Control covariates also include age, age squared, marital status, log of the household income, log of the household asset, current job tenure length in year, firm size, whether the respondent is covered by the employer's plan. Fixed effects include dummies of waves (from 1998 to 2014 but excludes 2008), individuals and census divisions. For the seven outcomes listed, the p-values of comparing the difference of the two groups are 0.9102, 0.3053, 0.5079, 0.0975, 0.0504, 0.3330 and 0.1617 respectively. None of the difference of coefficients is statistically significant.

Table 44 HETEROGENOUS EFFECTS BY EMPLOYABILITY (FIXED-EFFECTS IV)

	LOW EMPLOYABILITY		High EMPLOYABILITY	
	Job Insecurity	N	Job Insecurity	N
(1)Self -Reported Health	0.000671 (0.00354)	10006	-0.000841 (0.00667)	5388
(2)Mental Health	0.0234*** (0.00810)	10010	0.00719 (0.0135)	5386
(3)Any New Health Conditions	0.00368 (0.00263)	8111	0.00207 (0.00529)	4291
(4)BMI	0.00787 (0.00986)	9800	-0.00265 (0.0199)	5310
(5)Obesity	0.00199 (0.00133)	9800	-0.00250 (0.00271)	5310
(6)Drinking	0.0000801 (0.00164)	10009	0.00290 (0.00339)	5388
(7)Smoking	0.000336 (0.000937)	9967	0.00155 (0.00166)	5369

Notes: Standard errors in parentheses, standard errors are clustered at the household level,* p<0.10, ** p<0.05, *** p<0.01. Control covariates also include age, age squared, marital status, log of the household income, log of the household asset, current job tenure length in year, firm size, whether the respondent is covered by the employer's plan. Fixed effects include dummies of waves (from 1998 to 2014 but excludes 2008), individuals and census divisions. For the seven outcomes listed, the p-values of comparing the difference of the two groups are 0.4628, 0.0222, 0.6182, 0.3805, 0.8766, 0.8396 and 0.3187 respectively. The difference of coefficients for mental health is significant and all the others are insignificant.

Table 45 PLACEBO TEST

	(1) Mother's Alive	(2) Mother's Age(at death)	(3) Father's Alive	(4) Father's Age(at death)	(5) Height
Job Insecurity	-0.00134	-0.0183	-0.000393	-0.000478	0.000151
(OLS IV)	(0.00105)	(0.0265)	(0.000857)	(0.0290)	(0.000150)
N	21296	21075	21358	20823	21621
Job Insecurity	0.00119	-0.00259	0.000776	-0.00188	0.0000081
(FE IV)	(0.000929)	(0.00760)	(0.000751)	(0.00610)	(0.000058)
N	17914	17724	18003	17566	18240

Notes: Standard errors in parentheses, standard errors are clustered at the household level,* p<0.10, ** p<0.05, *** p<0.01. Control covariates also include age, age squared, marital status, log of the household asset, log of the household income, current job tenure length in year, firm size, whether the correspondent is covered by the employer's plan. Fixed effects include dummies of waves (from 1998 to 2014 but excludes 2008), individuals and census divisions.

Table 46 LAGGED IMPACT ON HEALTH (FIXED-EFFECTS IV)

	(1) Self- Reported Health(t)	(2) Mental Health(t)	(3) Any New Health Conditions (t)	(4) BMI(t)	(5) Obesity(t)	(6) Drinking (t)	(7) Smoking (t)
Job Insecurity (t-1)	-0.000313	-0.00480	-0.00241	-0.00300	0.00275	0.0000652	-0.00199
	(0.00484)	(0.0104)	(0.00352)	(0.0166)	(0.00221)	(0.00250)	(0.00132)
First Stage	4.250***	4.236***	4.252***	4.073***	4.073***	4.296***	4.289***
	(.786)	(.785)	(.785)	(.783)	(.783)	(.785)	(.787)
F Statistic	29.24	29.09	29.28	27.03	27.03	29.93	29.68
N	7869	7870	7872	7709	7709	7870	7836
Job Insecurity (t-2)	-0.00177	0.00845	-0.00400	0.0130	0.00127	0.00437	-0.000755
	(0.00551)	(0.0115)	(0.00409)	(0.0202)	(0.00260)	(0.00292)	(0.00161)
First Stage	5.632***	5.658***	5.651***	5.408***	5.408***	5.651***	5.724***
	(1.156)	(1.156)	(1.154)	(1.150)	(1.150)	(1.154)	(1.158)
F Statistic	23.70	23.98	23.97	22.12	22.12	23.97	24.42
N	3566	3564	3567	3494	3494	3567	3534

Notes: Standard errors in parentheses, standard errors are clustered at the household level,* p<0.10, ** p<0.05, *** p<0.01. Control covariates also include age, age squared, marital status, log of the household asset, log of the household income, current job tenure length in year, firm size, whether the correspondent is covered by employer's plan. Fixed effects include dummies of waves (from 1998 to 2014 but excludes 2008), individuals and census divisions. In the one-period lagged measure, the first-stage coefficients range from 4.073 to 4.296 (significant at conventional levels), F statistics range from 27.03 to 29.93 Comparatively, the two-period lagged first-stage coefficients range from 5.408 to 5.724 (significant at conventional levels) and F statistics change from 22.12 to 24.42.

Table 47 ROBUSTNESS CHECK I

	(1) Self- Reported Health	(2) Mental Health	(3) Any New Health Conditions	(4) BMI	(5) Obesity	(6) Drinking	(7) Smoking
(1)Job Insecurity	0.00190	0.0168***	0.00359*	-0.000341	0.000619	0.00169	0.000830
	(0.00264)	(0.00584)	(0.00207)	(0.00760)	(0.00102)	(0.00123)	(0.000703)
N(Full-Time Only)	14958	14961	12048	14712	14712	14961	14922
(2)Job Insecurity	0.000228	0.0112*	0.00293	-0.00396	0.00103	0.00132	0.000802
	(0.00292)	(0.00638)	(0.00209)	(0.00825)	(0.00109)	(0.00143)	(0.000869)
N(Add Industry-Occupation FE)	11414	11411	10337	11204	11204	11415	11369
(3)Job Insecurity	0.00222	0.0131**	0.00355*	0.00275	0.000374	0.000685	0.000481
	(0.00269)	(0.00591)	(0.00193)	(0.00767)	(0.00101)	(0.00128)	(0.000753)
N(Second Job Not For Pay)	15516	15517	12713	15230	15230	15517	15467
(4)Job Insecurity	0.00169	0.0165***	0.00326*	0.00377	0.000604	0.000893	0.000668
	(0.00244)	(0.00550)	(0.00183)	(0.00710)	(0.000942)	(0.00114)	(0.000658)
N(post-65 working probability)	17380	17378	14082	17079	17079	17380	17325
(5)Job Insecurity	0.00635	0.0189**	0.00639*	-0.00243	0.00127	0.000615	0.000274
	(0.00444)	(0.00963)	(0.00372)	(0.0136)	(0.00191)	(0.00199)	(0.00118)
N (exclude 0% and 50%)	7033	7033	5606	6918	6918	7034	7000

Notes: Standard errors in parentheses, standard errors are clustered at the household level,* p<0.10, ** p<0.05, *** p<0.01. Control covariates in baseline specification include age, age squared, marital status, log of the household asset, log of the household income, current job tenure length in year, firm size, whether the correspondent is covered by the employer's plan. Baseline Fixed effects include dummies of waves (from 1998 to 2014 but excludes 2008), individuals and census divisions.

Table 48 ROBUSTNESS CHECK II

	(1)	(2)	(3)	(4)	(5)
(1)Self-rated Health	0.00210	0.00210	0.00186	-0.0000353	0.00234
	(0.00242)	(0.00242)	(0.00238)	(0.00332)	(0.00260)
(2)Mental Health	0.0177***	0.0177***	0.0180***	0.0146**	0.0150***
	(0.00548)	(0.00548)	(0.00541)	(0.00723)	(0.00574)
(3) Any New Health Conditions	0.00317*	0.00318*	0.00308*	0.00430*	0.00323*
	(0.00184)	(0.00183)	(0.00182)	(0.00254)	(0.00196)
(4)BMI	0.00340	0.00344	0.00380	0.00250	0.00415
	(0.00696))	(0.00696)	(0.00688)	(0.00926)	(0.00738)
(5)Obesity	0.000722	0.000722	0.000848	0.000941	0.000735
	(0.000919)	(0.000919)	(0.000909)	(0.00131)	(0.000973)
(6)Drinking	0.000608	0.000607	0.000588	0.000863	0.000445
	(0.00113)	(0.00113)	(0.00111)	(0.00162)	(0.00120)
(7)Smoking	0.000703	0.000704	0.000623	0.000373	0.000740
	(0.000659)	(0.000658)	(0.000648)	(0.000914)	(0.000694)
(A)Baseline	Yes	Yes	Yes	Yes	Yes
(B)Exclude characteristics of the household wealth	No	Yes	Yes	No	No
(C)Exclude characteristics of the worker and firm	No	No	Yes	No	No
(D)Include characteristics of other family members	No	No	No	Yes	No
(E)Include other characteristics of labor status, job context, and psychologic factors	No	No	No	No	Yes

Notes: Standard errors in parentheses, standard errors are clustered at the household level,* p<0.10, ** p<0.05, *** p<0.01. Control covariates in baseline specification include age, age squared, marital status, log of the household asset, log of the household income, current job tenure length in year, firm size, whether the correspondent is covered by the employer's plan. Fixed effects include dummies of waves (from 1998 to 2014 but excludes 2008), individuals and census divisions. Characteristics of the household wealth in specification B include log of the household asset and log of the household income, Characteristics of the worker and firm in specification C include whether covered by the employer's plan, current job tenure size scale of the firm. Characteristics of the other family members in specification D include the labor force status of spouse and the number of living children. Other characteristics of labor status, job context and psychologic factors in specification E include whether current job needs moving heavy loads, whether the current job requires lots of physical effort, number of jobs with missing dates, whether moved to less demanding work, whether enjoy work, whether current job requires stoop/kneel/crouch, whether required more difficult things, whether current job requires good eyesight, and whether the respondent is looking for the second job.

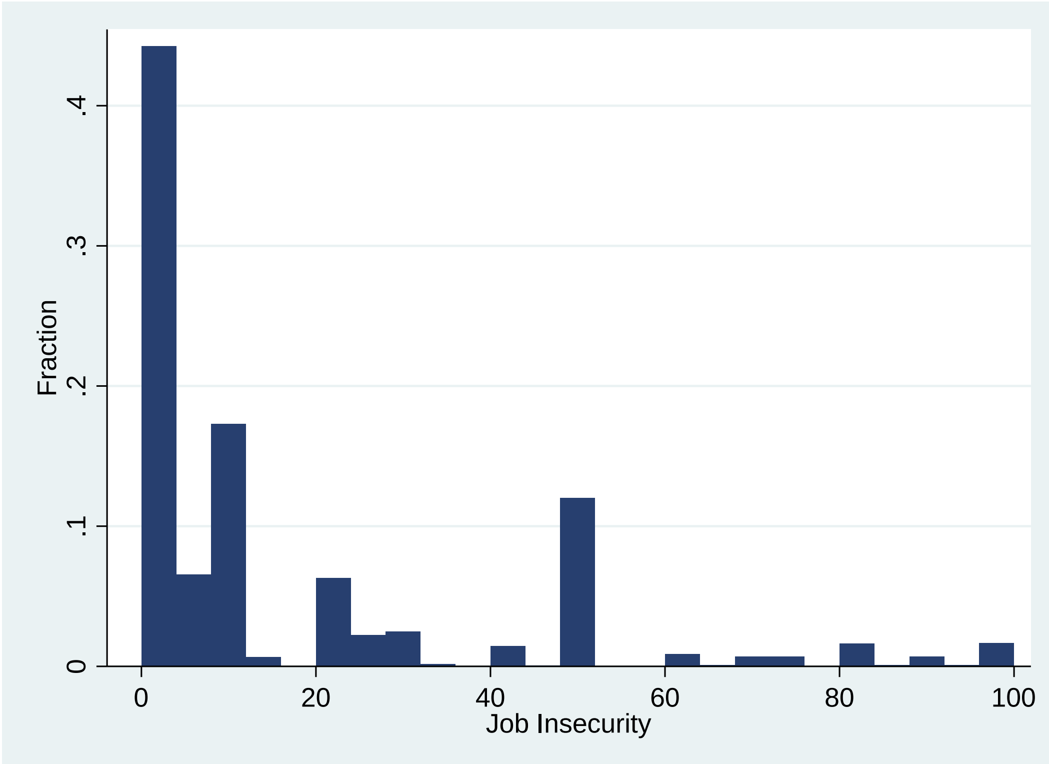

Figure 1 JOB INSECURITY DISTRIBUTION

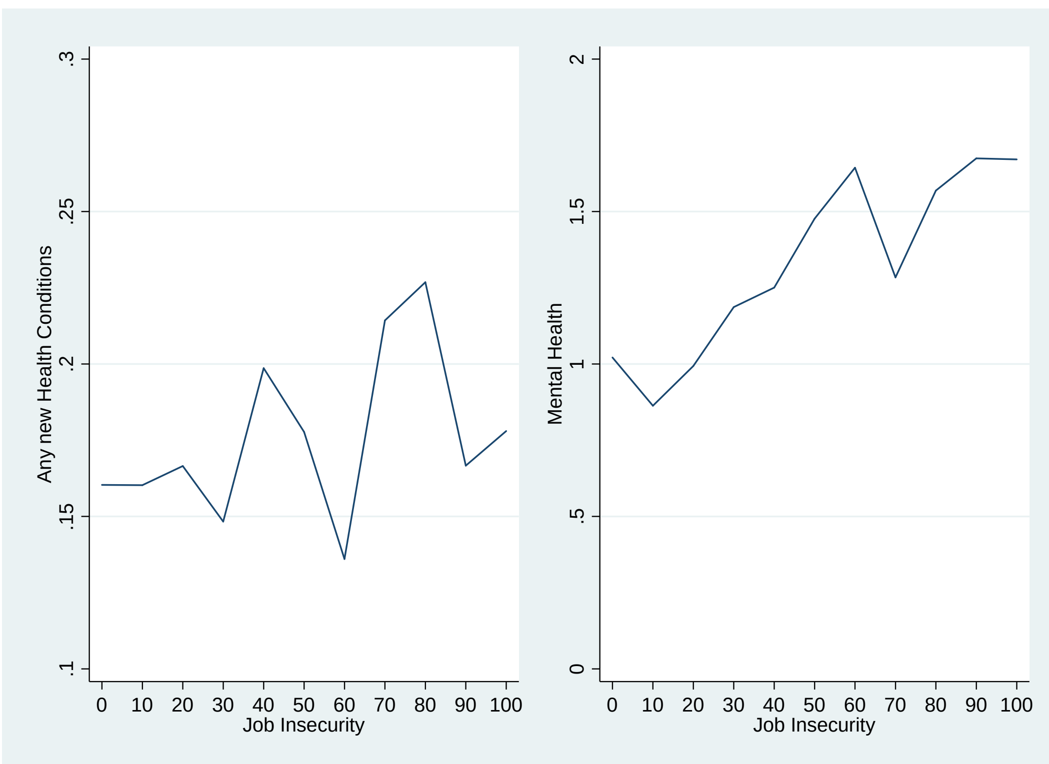

Figure 2 JOB INSECURITY AND HEALTH

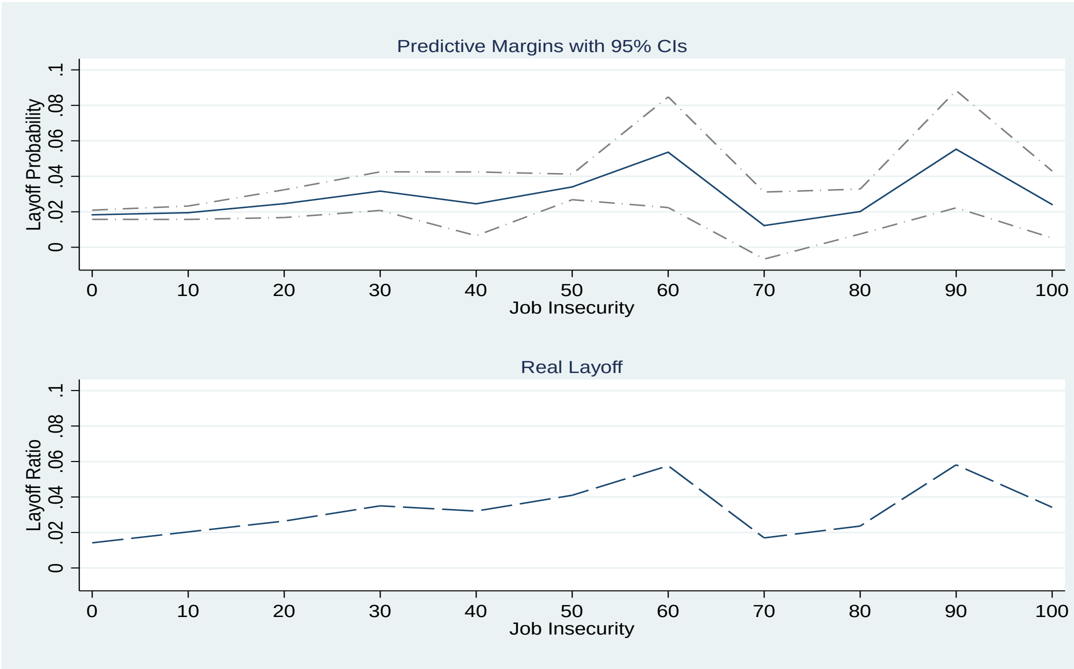

Figure 3 JOB INSECURITY AND LAYOFF

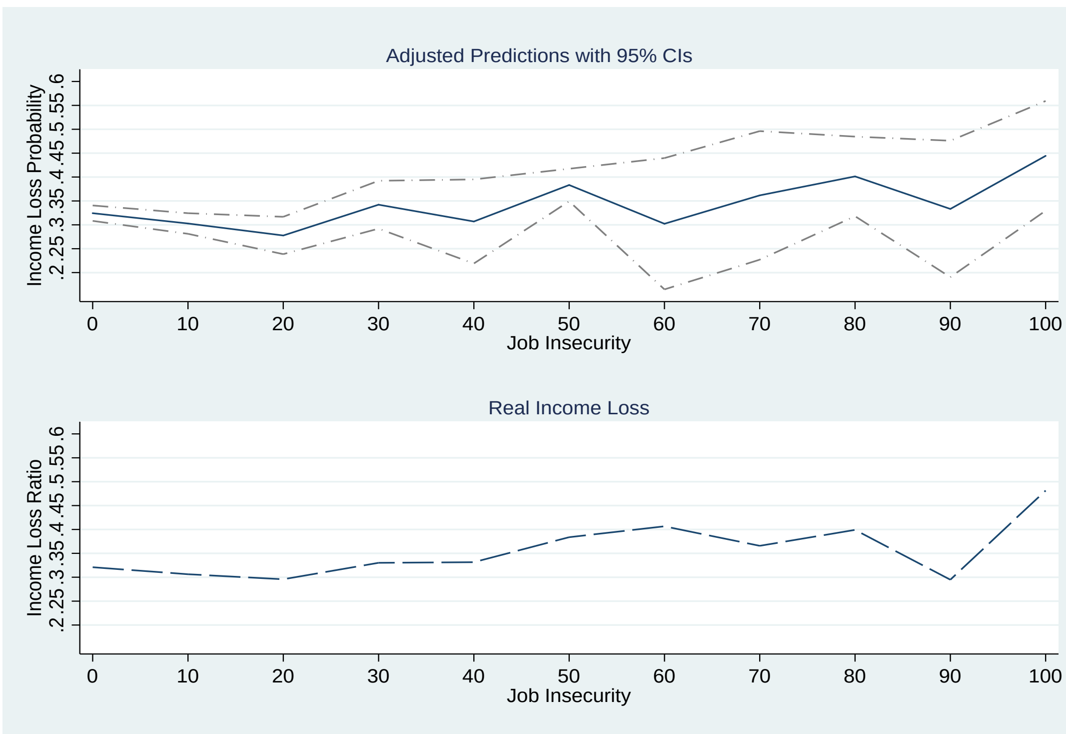

Figure 4 JOB INSECURITY AND INCOME LOSS