THREE ESSAYS IN HEALTH ECONOMICS

by

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A Dissertation Submitted in

Partial Fulfillment of the

Requirements for the Degree of

Doctor of Philosophy

in Economics

at

The University of Wisconsin-Milwaukee

May 2023

ABSTRACT

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The University of Wisconsin-Milwaukee, 2023
Under the Supervision of Professor Scott J. Adams

In the first chapter of my dissertation, I examine the effects of the Affordable Care Act Medicaid expansion on the labor supply decisions of older workers. To investigate this, I employ a triple-differences (DDD) methodology, utilizing variations in individuals' health insurance status and the expansion choices made by states. The results of my analysis shows that with Medicaid expansion, insured workers without retirement health insurance (RHI) decreased full-time work by 7.06 percentage points relative to those with RHI and those without any employer-sponsored coverage at all. Among those no longer working full-time, 82 percent transitioned to complete retirement.

Moving on to the second chapter of my dissertation, I focus on examining the heterogeneity in the crowd-out of private health insurance by considering individuals' levels of risk aversion in the context of Medicaid expansion under the ACA. Using the Health and Retirement Study (HRS) data, I find that Medicaid expansion led to a decrease in private coverage among risk-loving individuals by 5 percentage points; however, the expansion did not lead to any meaningful change in private coverage for risk-averse individuals. This finding suggests that risk-averse individuals are willing to keep their private coverage even though they become eligible for Medicaid. This suggests sorting into private coverage can have important implications for the effectiveness and cost of the expansion.

In the third chapter of my dissertation, I estimate the causal impact of retirement on measures of health and investigate potential mechanisms. To achieve this, I disentangle the effect of retirement into two distinct components: (i) the part mediated by observable behaviors, which I measure with changes in heavy drinking, exercise habits, and smoking; and (ii) the residual part, which

encompasses factors such as relief from occupational strain and the loss of a sense of purpose. Recognizing the endogeneity issue of retirement with regards to individual health status and health-related behaviors, I employ the eligibility age for social security as an instrumental variable. The comprehensive findings of my analysis indicate a beneficial overall total effect of retirement on both the physical and mental health of both females and males. Additionally, I observe that lifestyle changes triggered by retirement, particularly an increased likelihood of engaging in exercise, amplify the positive impact of retirement on the mental well-being of both female and male individuals.

By engaging with these interconnected themes, my dissertation adds to the growing body of knowledge in the fields of healthcare policy, labor economics, and social well-being. Each chapter contributes unique insights and deepens our understanding of the complex dynamics at play in these domains.

To my best friend, and love of my life, Caglar.

ACKNOWLEDGEMENTS

I would like to extend my heartfelt gratitude to Prof. Scott Adams, my esteemed advisor, for his unwavering support, invaluable guidance, and invaluable feedback throughout the entire journey of my dissertation. I am also deeply appreciative of the contributions made by Prof. John Heywood, Prof. Scott Drewianka, Prof. Jangsu Yoon, and Prof. Kevin Thom. Their invaluable insights, recommendations, and scholarly contributions have greatly enhanced the quality and depth of my work. Their expertise and intellectual guidance have been indispensable in the completion of this endeavor.

Moreover, I wish to express my profound appreciation to my family for their unwavering love, unwavering support, and constant encouragement. I am forever indebted to my parents and brothers for their enduring presence in my life, providing steadfast support throughout both the triumphs and challenges I encountered on this journey.

Lastly, I am indebted to my husband, Caglar Onal, for his unwavering love, understanding, and patience throughout my doctoral pursuit. His unwavering support, encouragement, and sacrifices have been instrumental in enabling me to achieve this significant milestone. I will forever be grateful to him for his unwavering dedication and belief in my abilities.

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1.1 Introduction

In 2013, before the major Affordable Care Act (hereafter, ACA) provisions took hold, those who separated from their employer prior to Medicare eligibility often went without health insurance coverage. According to the Kaiser Family Foundation (2013), only 28% of large firms (200 or more workers) and 5% of small firms (3-199 workers) offered retiree health insurance.[1] For the others, coverage was often unaffordable or inadequate. Having limited and unfavorable health insurance options may have led people to delay retirement until they become eligible for Medicare.

The ACA dramatically altered the U.S healthcare landscape. Beginning in 2014, many provisions of the ACA were implemented to increase the availability of health insurance for those who did not have coverage from their employers or who were not working. Among these, the expansion of Medicaid eligibility was the most pivotal. Before 2014, Medicaid coverage was limited to those who were disabled, elderly, or with dependent children. The expansion of Medicaid eligibility raised the income-eligibility threshold for adults with a dependent child; and low-income adults without dependent children (childless adults) became newly eligible to enroll in Medicaid. However, the 2012 Supreme Court decision on the ACA caused Medicaid expansion to be optional for states, leading to variations across states in when and whether they expand Medicaid eligibility. To date, 39 states, including the District of Columbia, have chosen to expand Medicaid eligibility.[2]

The availability of Medicaid coverage in expansion states serves as an alternative to employer-sponsored coverage for low-income, childless individuals. To that extent, individuals who had

[1] Source: https://www.kff.org/report-section/retiree-health-benefits-at-the-crossroads-overview-of-health-benefits-for-pre-65-and-medicare-eligible-retirees/

[2] Note that health insurance exchanges, the other signature measures in the ACA, provide premium tax credits to eligible people to help them purchase coverage through the market places. Income requirement for premium tax credits eligibility ranges from 138% to 400% of FPL in states that expanded Medicaid, while tax credit eligibility ranges from 100% to 400% of FPL in non-expansion states. The ACA's Medicaid expansion and health insurance exchange were implemented at the same year (2014). However, isolating the effects of Medicaid expansion from health insurance exchanges is not a concern, as the marketplace program is implemented uniformly across all states.

been experiencing ”job lock”, which is when a worker trapped in job because of health insurance, might decrease their work hours, move to bridge jobs, or fully exit the labor force while utilizing Medicaid coverage as a form of employer-sponsored coverage. Therefore, it is hypothesized that the ACA's Medicaid expansion may have an impact on the labor supply, particularly among older workers who place a higher value on health insurance, and it may reduce the extent of job lock for them (Gruber and Madrian 1994; Madrian 1994). The possibility of such reductions in labor supply for older individuals has been one of the concerns surrounding the Medicaid expansion decision.

This paper aims to extend our understanding of the ACA's Medicaid expansion on the labor supply decisions of older workers. To examine the effects of the ACA's Medicaid expansion on the labor supply decisions of older workers, I limit the sample to non-disabled, low-educated, childless adults (without dependent children) ages 50-64. I use education status as a criterion instead of income level to eliminate potential selection bias because individuals could adjust their income by changing their working hours to become eligible for Medicaid. I employ a fixed effect triple-differences (DDD) model that fully exploits the strength of my individual-level panel data and removes time-invariant differences between individuals. The first set of differences is in the within-state comparison of individuals who have employer-sponsored health insurance (ESHI) and have retiree coverage (RHI) from their current or previous employer or spouses' plan, along with those who do not have any employer-sponsored health insurance at all vs. those who have ESHI but not have RHI before and after Medicaid expansion. The third difference is comparing these across Medicaid expansion and non-expansion states. Considering that education is correlated with income, low-educated individuals are more likely to be eligible for Medicaid. Therefore, I focus my analysis primarily on childless individuals with a high school degree or less. My finding suggests that the ACA's Medicaid expansion leads to a decline in full-time work by 7.06 percentage points for the treatment group. Among those no longer working full-time, 82 percent transitioned to complete retirement. The result is robust to several alternative identification strategies.

This finding suggests that reliance on only employer-sponsored health insurance and Medicare was likely limiting older workers' employment and retirement choices. The expansion of Medicaid eligibility creates a public health insurance alternative to employer-sponsored health insurance, which ameliorates these limitations.

It is worth noting that this study focuses on specific subgroups of older individuals who are expected to have larger effects of Medicaid expansion on their labor supply decision. As such, findings may not be representative of the overall impact of Medicaid expansion on the general population. However, results offer valuable insights into the labor supply decisions of older individuals who are more likely to face retirement lock and the impact of Medicaid expansion on their decisions.

The structure of the paper is as follows: Section II provides a literature review related to health insurance and labor supply decisions. Section III describes the data and the identification strategy. Section IV presents the results. Section V discusses placebo tests and robustness checks. Section VI concludes.

1.2 Related Literature on Health Insurance and Labor Supply Decisions

The distribution of health care costs is strongly age-dependent. It rises steadily through one's adult years before it increases exponentially after age 50 (Alemayehu and Warner 2004). Because health insurance is tied to one's employer, the availability of alternative health insurance options is an important factor in labor supply decisions that have been studied frequently.

1.2.1 The Availability of Employer-Sponsored Health Insurance and Labor Supply

Previous studies applied different approaches to examine the effects of health insurance on labor supply decisions of older workers. Many used micro-data and variation in the availability of employer-sponsored retiree health insurance (RHI) to estimate the effects of health insurance on the labor market behavior of older workers. These studies generally conclude that RHI raises the probability of early retirement among pre-Medicare eligible workers (Marton and Woodbury 2013; Leiserson 2013; Strumpf 2010; Kapur and Rogowski 2011; Nyce et al. 2013).

Some studies are particularly important in informing my approach. For example, Gruber and Madrian (1995) exploit the natural experiment generated by the passage of continuation coverage mandates to estimate the effects of health insurance on retirement behavior. Continuation coverage allows workers and their families to continue their health insurance coverage through the employer's plan for a specified period after voluntary or involuntary employment termination. The first law was implemented in 1974. In 1986, the Federal government mandated such coverage at the national level under COBRA. Gruber and Madrian find that one year of mandated continuation benefits raises retirement rates by 20%.

Nyce et al. (2013) use employee-level data from 54 diverse firms and examine the effects of RHI on the turnover rate. The result shows that subsidized retiree coverage substantially reduces employment for pre-Medicare eligible individuals.

Fitzpatrick (2014) analyzes the effects of the availability of RHI for public school teachers in Illinois and shows that it leads employees to retire about 2 years earlier. In other relevant study, Shoven and Slavov (2014) estimate the impact of RHI on public sector workers' labor supply. The result illustrates that RHI leads to higher rates of stopping full-time work among 55-64 years old. The studies of Fitzpatrick (2014) and Shaven and Slavov (2014) add to evidence that RHI encourages early retirement in the public sector, which is consistent with the findings of earlier studies that focused on private sector employees.

Structural estimations of the effects of health insurance on retirement decisions have also been performed. Several early studies, including Lumsdaine, Stock, and Wise (1994) and Gustman and Steinmeier (1994), find that health insurance has a small impact on retirement decisions, but these studies do not account for risk aversion and uncertainty about out-of-pocket medical costs. Therefore, they find that the average employer contribution to health insurance is modest, but the value of employer-sponsored health insurance includes not only the cost paid by the employer but also the reduction in volatile medical expenses. Blau and Gilleskie (2001) address this point by including risk aversion in their study. The result shows that employer-sponsored retiree health insurance raises the exit rate from employment by 6 percentage points if the firm pays the entire cost. Similarly, French and Jones (2011) examine the impact of employer-sponsored insurance, Medicare, and Social Security on retirement decisions. They estimate a dynamic programming model of retirement that considers saving, uncertain medical expenditures, spousal income, and pension benefits. The simulation results show that increasing the Medicare eligibility age from 65 to 67 results in an increase in years of work by 0.074 years over ages 60-69, and removing 2 years' worth of Social Security benefits leads individuals to work extra 0.076 years.

1.2.2 The Availability of Pre-Medicare Public Health Insurance and Labor Supply

Economic theory predicts that cash and in-kind transfer programs generally create labor supply disincentives. The empirical results of a wealth of previous research focusing on labor supply decisions of older individuals support this hypothesized effect. For example, Dague et al. (2017) explore the effects of the temporary expansion of Medicaid to childless adults in Wisconsin, and they find a large decrease in labor supply over age 55 (-17.6 percentage points). Boyle and Lahey (2010) examine the Department of Veterans Affairs (VA) health benefits expansion, and the result shows that increased availability of this public coverage encourages early retirement by 3%. Similarly, Wettstein (2020) explores the effects of the Medicare Part D program on the retirement behavior of those who have retiree health insurance up to age 65 relative to those with insurance for life, before and after Medicare Part D. He finds that those with benefits only to age 65 decrease their full-time work by 8.4 percentage points.

The enactment of the Affordable Care Act provides an opportunity for researchers to reanalyze the labor market behavior of older individuals in the presence of public or heavily subsidized coverage.[3] Gustman et al. (2019) use the Health and Retirement Study (HRS) data from 2010-2014 to estimate the effects of ACA on early retirement expectations. They find no significant effect, but the structural model they present suggests that the ACA leads to an increase in early retirement by less than one percentage point. The authors recognize that the time elapsed might be too short to observe the full effects of ACA. Ayyagari (2019) uses a longer panel of data from 1998 to 2014 and finds 5.6 percentage points decrease in expected retirement age.

These studies identify the initial effects of ACA, and their benchmark analyses do not incorporate Medicaid expansion. There are studies that estimate the impact of the ACA's Medicaid expansion on early retirement (Aslim 2019; Levy et al. 2018; Wood 2019; Bradley and Sabik 2019; Duggan et al. 2021); however, the results are not consistent. Levy, Buchmueller, and Nikpay (2018) find no evidence of change in the probability of retirement among persons aged 50-64 in response to the Medicaid expansion. Their approach is to compare trends in retirement from January 2008 through June 2016 between states that expanded Medicaid and those that did not. It is important to note that the authors observe the fraction of individuals who retired in repeated monthly cross-sections instead of the probability of transition from work into retirement. Therefore, even if there is an increase in the probability of work to retirement transition, a significant change in trends in the stock of retirees may not be instantly observed. The authors point out this limitation and note that it might take time to see the effect of an increase in the probability of work to retirement transition on the fraction of older individuals who are retired.

Aslim (2019) finds no impact of Medicaid expansion on retirement among low-educated, childless men aged 55-64 and finds a small increase in retirement for low-educated childless women. However, the Wald estimates suggest a 10 percentage points increase in the probability of retirement for women. The author considers this finding as an upper bound for the retirement effect.

In contrast to studies that find little to no effect on retirement stemming from the ACA's Medicaid expansion, Wood (2019) finds a 2% and 8% decrease in labor force participation among indi-

[3] There are studies that analyze the effects of ACA on labor force participation among the working-age population rather than focusing on the sample most likely to be observed in retirement lock, older pre-Medicare eligible individuals (ages 50-64). They find little to no effect on labor force participation and small declines in hours worked (Kaestner et al. 2017; Gooptu et al. 2016; Moriya et al. 2016; Leung and Mas 2018).

viduals aged 55-64 resulting from the premium subsidies and Medicaid expansions, respectively. The author measures Medicaid eligibility and premium subsidies based on income. However, relying on the income to estimate Medicaid eligibility or premium subsidies has several potential threats. First, state-level differences in income distribution might be related to Medicaid expansion. In addition, omitted factors such as health might be correlated with family income and tastes for work. The author employs a simulated instruments approach to address these issues and finds consistent results. In a similar vein, Bradley and Sabik (2019) use simulated eligibility as an instrument to explore the effects of pre-ACA Medicaid expansions on labor supply. They find that older low-income women (ages 55-64) with a high-school degree are 17 percentage points less likely to be employed in states that have expanded Medicaid. The data Wood, Bradley and Sabik utilized are American Community Survey (ACS) and the March Current Population Survey (CPS), respectively, but both ACS and CPS collect no data on assets and nearly all income data refer to the previous calendar year self-reported income, rather than the monthly reference period, which is often used to determine Medicaid eligibility. Considering that low-income families are more likely to have fluctuating incomes rather than persistently low ones, utilizing self-reported income variables from CPS or ACS data might prevent the complete simulation of Medicaid eligibility.

Duggan, Goda, and Li (2021) estimate the effects of establishing health care exchanges on labor supply of the near-elderly (ages 60-64) and look at changes that differentiate between Medicaid expansion and non-expansion states. The result shows that the near-elderly reduce their labor force participation rate by 0.6 percentage points, but no significant effects are found related to the Medicaid expansions. It should be pointed out that the authors use this narrower age segment (ages 60-64) due to the higher elasticity of labor supply among the near elderly. However, they are many late-middle-aged adults (ages 50-60) that pursue a job primarily to secure health insurance and are likely to change their labor supply with the expansion.

1.2.3 This Study's Contribution

This study aims to contribute to the mixed literature on this subject. My approach differs from and is somewhat complementary to the approach taken by previous studies. First, the previous literature limits attention to low-educated or low-income individuals to investigate the effects of Medicaid expansion on early retirement. However, retirement lock is not expected to operate on all low-educated or low-income individuals. For example, those eligible for RHI coverage from their employers upon separation or those with health insurance provided by their spouse are not dependent on their employment for insurance. The expansion would not induce a change in the marginal incentive to retire for these groups. Like those with RHI, individuals without ESHI should not experience retirement lock. In the HRS data, individuals with RHI and those without ESHI make up more than half of the low-educated or low-income individuals (See Appendix Figure 1.1). This might also explain the low or null estimates of early retirement incentives found in studies that focus only on low-educated individuals as their treatment group (Aslim 2019; Levy et al. 2018). I address this limitation by exploiting variation in individuals' RHI and ESHI status. Based on individuals' health insurance status, I construct treatment and control groups and compare them across expansion and non-expansion states before and after the expansion. Including the control group in the analysis group can also absorb the effect of increases in the Social Security full retirement age and the impact of the Great Recession.

Second, previous studies on the ACA's Medicaid expansion do not explicitly discuss the potential mechanisms through which Medicaid expansions affect early retirement. The observed decline in labor force participation might be driven by a negative labor demand shock rather than a change in labor supply. Similarly, the potential change in labor demand might offset labor supply responses after the expansion, in turn yielding a null effect on labor market outcome. My identification strategy (triple-differences) absorbs such demand shocks. Nevertheless, I provide further analysis that rules out that the decline in full-time work is driven by a negative labor demand shock rather than a change in labor supply. Third, previous studies use either self-reported retirement status (Gustman et al. 2019; Levy et al. 2018) or an indicator for receiving retirement income in the past 12 months (Aslim 2019). However, using this narrower outcome is likely to result in missing some of the effects of ACA on the elderly labor supply. Specifically, it is true if there are individuals who are not working and do not yet think of themselves as retired or individuals who reduce their working hours or work part-time, but consider themselves as retired. To avoid this kind of misclassification and capture the full effects of expansion on the elderly labor supply, I use individuals' working hours to define their employment status and specifically analyze the transition from full-time work to part-time work or self-employment or complete retirement. And fourth, while existing papers are based on pooled cross-sectional data, my study uses longitudinal individual-level panel data that allow me to track individuals across time and control time-invariant differences between individuals.

Finally, recent literature has shown that difference-in-differences models that rely on the staggered adoption of policies or regulations are susceptible to the biases introduced by treatment effect heterogeneity across time or groups. Estimates can obtain even the opposite sign of the true average treatment effect on the treated (ATT) (De Chaisemartin and d'Haultfoeuille 2020; Sun and Abraham 2021; Goodman-Bacon 2021; Callaway and Sant'Anna 2021). My main empirical approach, in contrast to previous studies exploiting staggered expansion timing, relies on a single treatment period (January 2014). I exclude states that expand Medicaid before and after January 2014. This setting provides unbiased estimates even when there are dynamic treatment effects.

1.3 Data and Empirical Strategy

1.3.1 Data

I use the Rand version of the Health and Retirement Study (HRS) data for the years 2010-2016.[4] The HRS is a nationally representative, longitudinal survey of individuals over the age

[4]The Rand HRS file is derived from all waves of the HRS. It provides a cleaned and user-friendly version of the original data and produced by the RAND Center for the Study of Aging, with funding and support from the National Institute on Aging (NIA) and the Social Security Administration (SSA)

of 50 and their spouses. Interviews are conducted biennially and provide detailed information on individuals' health, employment, insurance status, and demographic characteristics.

State-level geographic identifiers and identification of individuals' insurance status are two crucial variables for my analysis, enabling me to exploit state-level variation in Medicaid and construct treatment and control groups to be used in triple-differences design. The HRS data contains a variable that summarizes whether individuals are covered in retirement under any plan. That could include his or her own plan or a spouse's plan.[5] The questions about coverage in retirement are asked only to the individuals who have ESHI while working and are under age 65. Those who answer the question as "not covered in retirement" are assigned to the treatment group, while those who answer the question as "covered in retirement just to age 65" or "covered in retirement to and over age 65" are assigned to the control group.[6] The control group also includes individuals who do not have ESHI from a current or previous employer or union or their spouses/partners.

In addition, my access to restricted geographic information from the HRS data allows me to determine individuals' state of residence. Since Medicaid expansion would most likely affect low-income adults without dependent children (childless adults), who had been previously excluded from Medicaid in most states, I restrict the sample to non-disabled, low-educated childless adults ages between 50-64.[7] As it is common for the cohort studied, low-educated is defined as having a high school education or less.[8] Disabled individuals are excluded because their employment choices may be impacted by health concerns, which might obscure the actual effect of Medicaid expansion on labor supply. Additionally, disabled individuals were already eligible for Medicaid before the expansion.

[5] Coverage in retirement derived from the variable RwHERET in the Rand HRS 1992-2016.

[6] To assign individuals that have a missing value for the question on retiree insurance for any reason either in the treatment or control groups, I apply the following strategy: If over time individuals retire, it can be inferred whether they have RHI or not by observing whether they are covered by their or spouse's employer plan when they retire. If individuals are retired and covered by their or spouse's employer-sponsored health insurance, I allocate those in the control group.

[7] This age restriction for lower boundary is the same as the previous studies that analyze the effect of RHI on retirement decision (Robinson and Clark 2010; Levy et al. 2018).

[8] Individuals who have missing years of education information are dropped.

The main outcome of interest is the full-time work indicator.[9] I use a full-time work indicator instead of self-reported retirement for two reasons. First, individuals may wish to transit from full-time jobs to retirement gradually. They might reduce working hours in the same job or switch to part-time jobs instead of retiring. Second, it is not easy to define retirement because the word might mean different things to different people. This might lead to misclassification, which would confound the interpretation of my estimates. The definition of full-time work is straightforward. Individuals are considered full-time workers if they report working more than 35 hours a week for more than 36 weeks a year. If they work less than that, they are considered part-time workers. The hours and weeks from the main and second jobs are counted.

The triple-differences method for my empirical model reflects movement in one direction-moving out of full-time work. The absence of a triple difference does not suggest re-entry to full-time work. Therefore, if the individual reenters full-time employment after exits full-time employment, I exclude periods after re-entry to full-time work. [10] For example, assume that an individual works full-time in 2010 and exits full-time employment in 2012 but returns to full-time work in 2014. For this individual, I exclude observations in 2014 and years after 2014. All sample restrictions leave me with a final sample of 4,682 individuals, 3,984 households, and 8,269 person-year observations. Appendix Table A1.1 presents information on sample loss due to each restriction.

1.3.2 Identifying Treatment and Control Group

The fear of losing health coverage does not affect all individuals when making retirement decisions. Those who are eligible for RHI coverage from their employers upon separation, or those who receive health insurance from their spouse, have health insurance that is not tied to their job. Consequently, their retirement choices are less likely to be influenced by health insurance consid-

[9]The full-time work indicator equals 1 if the individual works full-time and 0 if she/he works part-time, is retired, partly retired, unemployed, and not in the labor force.

[10]The results are robust to including the period after re-entry to full-time work (For details, see Appendix Table B1.1)

erations, and they are less likely to change their retirement plans with the expansion of Medicaid. Therefore, these individuals are included in the "control" group of this study. Similarly, individuals who do not have any ESHI do not face retirement lock since they do not rely on their job for health insurance benefits. However, the expansion of Medicaid may affect their labor supply decisions through income channels. If gaining Medicaid coverage reduces their household out-of-pocket medical expenses, Medicaid acts as a positive income shock, and they might reduce their labor supply. Therefore, prior to assigning those who have no ESHI to either the control or treatment group, I further analyze the effects of Medicaid expansion on their total household out-of-pocket medical spending. I employed a log-linear model and quantile estimation method to examine the effects of Medicaid expansion on their total household out-of-pocket medical spending. The results indicate that there is no statistically significant change in total household out-of-pocket medical spending (For details, see Appendix Tables C1.1 and C1.2). This finding suggests that there is minimal or no effective income shock from Medicaid coverage that might influence the labor supply decision of those who have no ESHI at all. Therefore, those without any ESHI are added to the "control" group. Meanwhile, the "treatment" group of individuals is defined as those who have insurance from their employer but do not have retiree coverage. Prior to Medicaid expansion, members of the treatment group were more likely to work until the age of 65 to maintain their employer-sponsored health insurance. Health insurance was guaranteed to them at age 65 or older by Medicare. This might have forced them to continue working even if they were ready to retire, as they couldn't afford to lose their health insurance. However, with the implementation of Medicaid expansion in 2014, members of the treatment group in expansion states were no longer limited to employer-sponsored health insurance options. They could obtain health insurance through Medicaid before becoming eligible for Medicare. As a result, treatment group members were no longer locked into their jobs due to health insurance concerns and out of retirement. Table 1.1 summarizes the characteristics that define the treatment and control groups.

I categorize persons as belonging to the treatment and control groups based on their current health insurance status. However, it is possible that the availability of employer-sponsored health insurance could change due to Medicaid expansion. To allay this concern, I investigate the effects of Medicaid expansion on employer-sponsored health insurance and retiree coverage. The results show that there is no significant effect of Medicaid expansion on ESHI and RHI (For details, see Appendix Table D1.1). These findings are consistent with studies that indicate no effect of Medicaid expansion and health insurance exchange on the likelihood of an establishment offering ESHI or the percentage of its workforce that takes up coverage (Abraham et al. 2019, 2016; Blavin et al. 2015). All of these findings alleviate concerns about the potential endogeneity of the treatment group (those with ESHI but without RHI) to the policy.[11]

Table 1.2 gives descriptive statistics of the sample, which comprises non-disabled, childless adults aged 50-64, with a high school degree or less, divided into two experimental groups: the treatment group and the control group. The control group has a lower annual labor income than the treatment group, but their total wealth is higher, especially in expansion states. The treatment group has a much higher share of women and pensioners (individuals who are currently enrolled in a pension plan through their current job) than the control groups. Furthermore, the rate of full-time employment for the treatment group is higher than the control group. However, these differences between the treatment and control groups do not necessarily violate the identifying assumption of triple-differences estimation. The triple-differences estimator simply requires that the relative outcome of treatment and control groups in the experimental states trend in the same way as the relative outcome of treatment and control groups in the non-experimental states in the absence of treatment. Figure 1.2 in Appendix also clearly illustrates that the treatment and control groups moved parallel in expansion and non-expansion states before the expansion.

[11] Note that the employer mandate, which requires employers with 50 or more full-time workers to provide health insurance or face penalties, was implemented under the ACA in 2015. This policy might affect the composition of working hours. For example, an employer might limit the number of hours employees can work or replace full-time workers with part-time. However, the potential changes in working hours will not threaten my identification because the employer mandate started to be effective nationwide, and the changes would cancel out between expansion and non-expansion states as long as their responses are not significantly distinct. To examine whether there is a differential response in expansion states versus non-expansion states, I compare working hours across expansion and non-expansion states before and after the employer mandate. The result shows that there is no significant difference between expansion and non-expansion states (For details, see Appendix Table E1.1).

1.3.3 Identifying Experimental and Non-Experimental States

States differ in their timing of adopting the ACA Medicaid expansion. Although most states expanded their income eligibility limit to 138% of the Federal Poverty Line (FPL) in January 2014 according to the ACA provisions, six states had previously expanded coverage for childless adults earlier than 2014. These include CT, DE, ME, NY, VT, DC. For the initial analysis, I drop these states for the purpose of a cleaner analysis.[12] In addition, seven states (AK, IN, LA, MI, MT, NH, PA) adopted Medicaid after January 2014, which I refer them as late expansion states. Table 1.3 lists the states with Medicaid expansion to date.

The 20 states that expanded in January 2014 are the initial experimental group of expansion states and the 18 states that did not are the non-expansion states. Although Wisconsin has not expanded Medicaid under the ACA, in 2014, Wisconsin extended its Medicaid program (which is called BadgerCare) to all individuals with income up to 100% FPL (without enrollment cap), and approximately 99,000 childless adults became newly eligible for Medicaid.[13] Therefore, Wisconsin is counted as the experimental state in the main analysis.[14]

1.3.4 Main Specification

The econometric model to estimate the relationship between Medicaid expansion and retirement is written as follows:

[12] Despite the fact that both Hawaii (HI) and Arizona (AZ) expanded Medicaid eligibility earlier, I did not remove them because they temporarily suspended it and then reinstated it to the level required by the Affordable Care Act (ACA) in 2014. In the year 2000, Arizona expanded coverage for childless adults up to 100% of the Federal Poverty Line (FPL), but starting in July 2011, enrollment for adults was capped. Likewise, Hawaii covered coverage for childless adults up to 100% FPL through its QUEST Medicaid managed care waiver program, but enrollment was closed for certain groups in 2012. The results are similar if Arizona and Hawaii are excluded (For details, see Appendix Table G1.1).

[13] Source: https://www.healthinsurance.org/medicaid/wisconsin/

[14] Additionally, I remove Wisconsin from the main sample and the sample that includes both early and late expansion states, and re-estimate the equation (1). The results are qualitatively in line with the main findings (For details, see Appendix Table F1.1).

$$
\begin{aligned}
(1) \quad Y_{ist} = {} & \alpha_0 + \beta_0(Post2014_t \times Treat_{ist} \times Expansion_s) + \beta_1 Expansion_s + \beta_2 Treat_{ist} + \beta_3 Post2014_t \\
& + \beta_4(Expansion_s \times Post2014_t) + \beta_5(Treat_{ist} \times Post2014_t) + \beta_6(Treat_{ist} \times Expansion_s) \\
& + \beta_7 X_{ist} + \gamma_t \times Treat_{ist} + \alpha_a \times Treat_{ist} + \alpha_a + \gamma_t + \mu_i + \phi_{st} + \varepsilon_{ist}
\end{aligned}
$$

In this equation, the subscripts indicate individual i, state s, and year t. Y_{ist} is a full-time work indicator for individual i in state s and time t. $Post2014_t$ and $Expansion_s$ are dummies equal to 1 if and only if the observation is observed in the year 2014 or later, and in experimental states, respectively. $Treat_{ist}$ is a dummy equal to 1 if the individual belongs to the treatment group and 0 for the control group. All specifications further include a full set of age (α_a) and year (γ_t) fixed effects, as well as their interaction with $Treat_{ist}$.[15] μ_i are individual fixed effects, ϕ_{st} is state specific linear time trends, and ε_{ist} is the idiosyncratic error term.

X_{ist} is a vector of additional controls, including a dummy for being married, divorced, or widowed; an indicator for being enrolled in a pension plan from the current job (1 if an individual has any pension plan from the current job, 0 otherwise)[16]; total wealth. Monetary variables are inflated to 2016 prices by the consumer price index. All standard errors are clustered at the individual level, and the significance levels hold for all specifications when standard errors are calculated by using the bootstrap method with multilevel clustering at household and state level and are based on 700 repetitions (P-values of the main parameter of interest, β_0, are reported in all of the tables as a separate row).[17]

Having individual fixed effects in the model allows me to control unobserved heterogeneity

[15] The results are robust when the interaction of the Treat dummy variable with the full set of age and year fixed effects are excluded from the model (For details, see Appendix Table I1.1)

[16] Individuals who skipped the HRS question regarding pension plans due to not having current jobs are identified as having no pension plan. In addition, eighty-four individuals in my sample did not respond to the pension questions even though they have a current job. Those individuals are also identified as having no pension plan. Excluding them from the sample provides similar results to the main finding.

[17] A small number cluster (generally less than 50) may result in having too small standard errors and over-rejection of the null hypothesis (Cameron and Miller 2015; Donald and Lang 2007; McCaffrey and Bell 2006). In my case, the number of state clusters is 37, so I apply block-bootstrapped standard errors by household and state groups based on 700 replications to alleviate this concern.

across individuals. However, a linear fixed effects estimator should not be applied when an outcome variable is a non-repeated event. To account for the potential that leaving full-time work is a non-repeated event, I estimate Cox proportional hazard model as a robustness check.

1.3.5 Identification Assumption

The triple-differences estimation was introduced by Gruber (1994), and it is an extension of double differences. The triple-differences estimator can be computed by taking the difference of two difference-in-differences estimators. However, the triple-differences estimator does not require two parallel trend assumptions to interpret estimates as causal. The reason is that taking the difference between two biased difference-in-differences estimators will cancel out the bias as long as the bias is the same in both estimators. Therefore, the triple-differences estimator only requires that the trend of the relative outcome in the treatment and control groups in the experimental states must be parallel to the trend of the relative outcome of treatment and control groups in the non-experimental states, in the absence of treatment.

To investigate whether differences in outcomes between treatment and comparison groups trend similarly in expansion and non-expansion states, I conduct an event study using data from 2000 to 2016. Shifting the analysis sample back to 2000 (the main analysis includes data from 2010 to 2016) allows me to better validate pre-treatment trends.[18] I estimate the following regression:

$$(2) \quad Y_{ist} = \alpha_0 + \sum_{n \neq 2012} \beta_0^n (I_{t=n} \times Treat_{ist} \times Expansion_s) + \sum_{n \neq 2012} \beta_1^n (I_{t=n} \times Treat_{ist})$$
$$+ \sum_{n \neq 2012} \beta_2^n (I_{t=n} \times Expansion_s) + \beta_3 (Treat_{ist} \times Expansion_s) + \beta_4 Treat_{ist}$$
$$+ \beta_5 Expansion_s + \gamma_t + \mu_i + \varepsilon_{ist}$$

$I_{t=n}$ is an indicator for each year (other than the 2012-base year). γ_t and μ_i are time and individual fixed effects, respectively. The coefficients interest are pre-2014 β_0^n estimates (β_0^{2000},

[18]The results are robust when the sample is constrained to the year from 2010 to 2016.

β_0^{2002}, β_0^{2004}, β_0^{2006}, β_0^{2008}, β_0^{2010}). Table 1.4 illustrates the results; the coefficients of the triple interaction terms except for β_0^{2002} are statistically indistinguishable from zero. F-test p-value for joint tests of significance for pre-treatment coefficients indicates they are not jointly statistically significant, which provides evidence that the parallel trend assumption is valid.

1.4 Results

Table 1.5 presents the results of the triple-differences estimation. Column 1 shows the raw results without individual controls and state-specific linear time trends, and column 3 shows the baseline specification of equation (1). The baseline specification indicates that Medicaid expansion leads to a fall of 7.06 percentage points in full-time work for the treatment group. Reassuringly, the effect of Medicaid expansion on the control group is not statistically significant, which alleviates the concern that the result in the treatment group is influenced by other unobserved changes rather than relaxation of retirement lock with Medicaid expansion.

To analyze how much reduction in full-time work is due to individuals shifting into complete retirement and how much is due to individuals shifting into part-time work or self-employment, I estimate equation (1) with any work, part-time work, and self-employment indicator as the dependent variable, respectively.[19] Table 1.6 shows that Medicaid expansion leads to a fall of 5.79 percentage points in any work for the treatment group and no statistically significant change in part-time work and self-employment. This finding illustrates that 82 percent of those who no longer work full-time replace their full-time work by transitioning into complete retirement, rather than transitioning to part-time work or self-employment.

One natural question that can be asked whether this decline in full-time work is driven by labor supply or labor demand channel. The existing literature on the ACA does not explicitly address

[19]The part-time work indicator equals 1 if the individual works part-time or is partly retired and 0 if she/he works full-time, is retired, unemployed, and not in the labor force. Similarly, the any work indicator equals 1 if the individual works full-time or part-time or is partly retired and 0 if she/he is retired, unemployed,or not in the labor force. Self-employment indicators are constructed based on individuals' self-report. The HRS survey asks respondents whether they are self-employed or work for someone else. Respondents' possible answers are either self-employed or working for someone else. There are some cases where answers are missing because respondents refuse to answer or do not know. Therefore, the sample size is smaller when the dependent variable is self-employed.

the potential change in labor demand. However, the employer mandate implemented under the ACA in 2015 might have led an increase in layoffs or part-time staffing arrangements. The study of Mulligan (2020) shows that between 28,000 and 50,000 businesses nationwide reduce their number of full-time-equivalent employees because of the mandate. Similarly Dillender, Heinrich, and Houseman (2022) find that the ACA increase low-hours, involuntary part-time employment in retail, accommodations, and food services.

Using a triple-differences estimator provides primary evidence that the change in full-time work can be attributed to the labor supply shift. This is because general shocks to labor demand would impact both expansion and non-expansion states,resulting in their cancellation out between expansion and non-expansion states. Furthermore, demand shock that occurs only in expansion states would impact the labor outcome of both the control and treatment groups. Thus, the triple-differences estimator should absorb demand shocks.

Nevertheless, there might be unobserved demand shock that differentially affects the treatment group relative to the control group in expansion states. I explore the existence of such labor demand shocks indirectly by analyzing changes in the average wage, as in Garthwaite, Gross, and Notowidigdo (2014). The negative demand shock leads to a decline in wages, so the observed reduction in wages indicates a negative demand shock rather than a negative supply.

Columns 1-3 of Table 1.7 show the effect of Medicaid expansion on annual labor earnings, while columns 4-6 illustrate the effect on wages.[20] Conditional on positive wages, there is no decrease in wages for the treatment group. This lack of decrease in wages is inconsistent with a labor demand shock. Therefore, the point estimates do not suggest that the observed decrease in full-time work for the treatment group following expansion is driven by a fall in demand for their labor. It is worth mentioning that both labor supply and demand could decrease in a similar magnitude, in turn yielding a null effect on wage. However, large standard errors prevent decisive conclusion.

[20] Earnings are very right-skewed, so I top code earnings at the ninety-fifth percentile among full-time workers ($100,000). In addition, I exclude observations with over 70 usual hours of work per week due to misreporting is highly likely.

1.5 Placebo Tests and Robustness Checks

1.5.1 Placebo Tests

To further assess the validity of my empirical approach, I conduct two placebo tests. First, I analyze the effects of Medicaid expansion on high-educated adults. As these individuals likely have incomes above the Medicaid income eligibility threshold, the expansion should have either no effect or a limited effect on their full-time work. I re-estimate equation (1) for this sample, and the results are presented in columns 1,2, and 3 of Table 1.8. As predicted, I observe no effect of the Medicaid expansions on the full-time work of high-educated adults.

Next, I restrict the analysis to the pre-Medicaid expansion period. I construct the data the same way as the main analysis, but the sample period is constrained from 2008 to 2012. A placebo date of Medicaid expansion, the year 2010 rather than 2014, is used to construct a variable indicating that when states expand Medicaid, it corresponds to $Post2014_t$ in equation (1). I re-estimate equation (1) with the assumption that Medicaid expansion occurred in 2010 instead of 2014. The results of this placebo test utilizing the pre-Medicaid expansion period are presented in columns 4-6 of Table 1.8. As expected, the results of the estimate are not statistically significant.

1.5.2 Robustness Checks

In this section, I perform several robustness checks to further assess the sensitivity of the main findings.

1.5.3 The Inclusion of Early and Late Expansion States

I re-estimate equation (1) by including early expansion (CT, DE, MN, NY, VT, DC) and late expansion states (AK, IN, LA, MI, MT, NH, PA). The inclusion of early and late expansion states creates variation in the timing of the expansion. Therefore, the variables $Expansion_s$ and $Post2014_t$ in equation (1) are replaced with $Expansion_{st}$, which represents when Medicaid expansion happens in a state.[21]

Columns 1, 2, and 3 of Table 1.9 present the estimation results. The result in column 3 shows that Medicaid expansion leads to a decrease in full-time work by 6.16 percentage points for the treatment groups, which is consistent with the main findings.

1.5.4 Alternate Sample Definition of Individual Affected by Expansion

I constrain the sample based on household income level instead of education level and re-estimate equation (1). To avoid concerns about self-selection into Medicaid through the manipulation of income levels via working hours, I constrain the sample based on household income level in 2012. The sample is restricted to childless individuals with annual household income equal to or less than \$50K in 2012 .[22] While this restriction results in a smaller sample size, it allows me to test the robustness of the main finding.

Table 1.10 displays the estimate for childless individuals with annual total household income equaled to \$50K or less in 2012. The results are consistent with the main findings. It is important to note that the sizes of the estimated effects are relatively larger when the sample is constrained based on current household income level, which might be a sign of self-selection into Medicaid.

1.5.5 Alternative Control Group

Thus far, all the triple-differences regressions have used a control group consisting of individuals who have RHI through their employer or spouse's plan, or those who do not have ESHI from any source. The rationale for the latter group being assigned to the control group is that they should be equally unaffected by the relaxation of the retirement lock. However, those of advanced ages without ESHI may be less comparable to the treatment group. Applying the triple-differences

---

[21]The new equation is as in following :
$Y_{ist} = \alpha_0 + \beta_0(Treat_{ist} \times Expansion_{st}) + \beta_1 Expansion_{st} + \beta_2 Treat_{ist} + \beta_3 X_{ist} + \gamma_t \times Treat_{ist} + \alpha_a Treat_{ist} + \alpha_a + \gamma_t + \mu_i + \phi_{st} + \varepsilon_{ist}$

[22]The Federal Poverty Level (FPL) changes with the number of individuals in the household, so does the Medicaid income eligibility threshold. I chose an annual household income of \$50K as an upper bound of the annual household income level because that 138% of FPL for a household with seven people is 50,687\$ for contiguous states and the District of Columbia in 2016. Note that monetary variables in the sample are inflated to 2016 prices.

method addresses the possible bias arising from pre-treatment differences between the treatment and control groups.

Nevertheless, I exclude those who do not have ESHI at all and re-estimate equation (1) to test the robustness of the main result. Column 3 in Table 1.11 confirms that the main results hold using this alternative control group. Medicaid expansion leads to a fall of 7.13 percentage points in full-time work for the treatment group.

1.5.6 Accounting for Recovery from the Great Recession

The Medicaid expansions occurred during the recovery from the Great Recession, perhaps confounding the interpretation of my estimates. Furthermore, as seen in column 3 of Table 1.5, there is a significant positive change in non-expansion states for the treatment group, raising concerns that unobserved factors in non-expansion states may influence the full-time working decision of the treatment group. There is one way that I can test the extent to which these might affect my estimates.

The Great Recession did not equally affect all parts of countries or all demographic groups (Elsby et al. 2010). Therefore, it is likely that recovery from the recession varies across states or demographic groups. The inclusion of unique time trends for different demographic groups and states accounts for the varying effects of the recovery from the recession on states and demographic groups. Therefore, to evaluate whether the recovery from the Great Recession or unobserved factors in non-expansion states drives the result, I add separate time trends for each state and state-specific effects of being in the treatment group to the main specification. Columns 1 and 2 in Table 1.12 show that the results are similar to the main finding.

1.5.7 Hazard Models

The Cox proportional hazard model is an alternative to the main estimation with individual fixed effects. Table 1.13 presents estimates of the effect of Medicaid expansion on full-time work (columns 1 and 2) and any work (columns 3 and 4) using a hazard model (failure is leaving full-time work in columns 1 and 2 and leaving work completely in columns 3 and 4). The results of these estimations are in line with the main findings, with the Medicaid expansion hazard of leaving full-time work increasing by 12 percent for the treatment group. Similarly, the hazard of leaving any work increases with Medicaid expansion by 26 percent for the treatment group.

1.6 Conclusion

Expansion of Medicaid eligibility was the primary means by which health care coverage was expanded under the ACA. In this paper, I explore the relationship between Medicaid expansion and the labor supply decisions of older Americans. The estimation suggests that Medicaid expansion leads to a decrease in full-time work by 7.06 percentage points for the treatment group, of which 82 percent was due to transition to complete retirement.

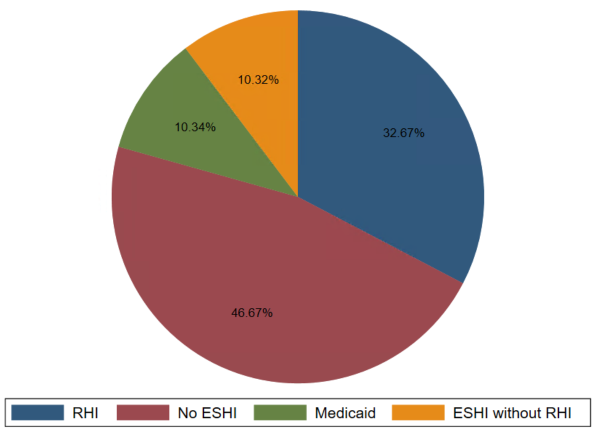
The high early retirement incentive with Medicaid expansion implied by my estimates are consistent with results found in retirement health insurance studies. In addition, studies that examine the effect of public health insurance on the labor supply of the elderly provide similar results to my analysis (Dague et al. 2017; Boyle and Lahey 2010; Wettstein 2020; Wood 2019). However, my estimates are in contrast to the low or null estimates of early retirement incentives found in by Aslim (2019) and Levy, Buchmueller, and Nikpay (2018). Aslim (2019) finds a 0.6 percentage point increase in retirement among low-educated, childless women aged between 55-64 with the ACA's Medicaid expansion. Levy, Buchmueller, and Nikpay (2018) find no significant effect of Medicaid expansion on retirement.

I contend that the likely reason for the differences in my findings is that previous studies do not limit attention to subsets of the population that experience retirement lock and would likely be affected by the expansion. Indeed, I find a null effect when I limit attention to low-educated individuals ages 50-64 and compare them across expansion and non-expansion states (See Appendix Table J1.1). In the current paper, I use individuals' health insurance status to isolate those likely to alter their retirement decision with the expansion.

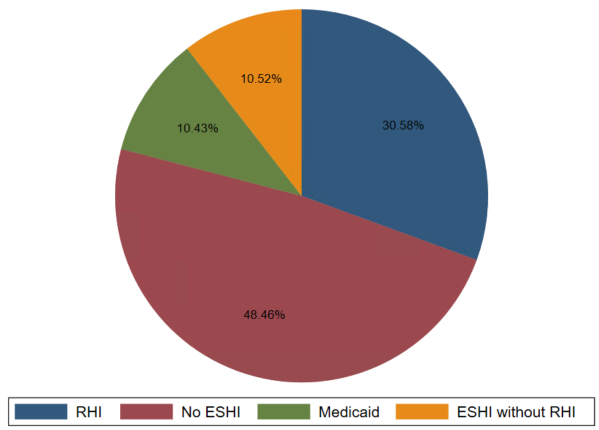
This paper provides evidence of retirement lock stemming from an employer-sponsored insurance system. This can signify that the employer-based health insurance system inefficiently allocates jobs to reluctant older workers at the expense of younger workers who are eager to replace them. At the same time, the observed trend toward earlier retirement has implications for the design of social security. The aging of the U.S population already raises concern about social security solvency. A trend towards earlier retirement ages might increase financial pressure on the social security system. However, it is noteworthy that my estimation results, suggesting the high willingness to retire with Medicaid expansion, should be interpreted as an effect only on lower-income, less-educated Americans. This would likely lower the impact on the social security system since they contribute less and have a lower life expectancy.

It is important to note that the results of this study provide an average treatment effect. However, it is likely that the intensity of retirement lock varies across individuals due to differences in their health status or demographic factors so does the effect of Medicaid expansion. One limitation of this study is the lack of heterogeneity analysis. I could not explore heterogeneities with respect to demographics or health measures due to small sample size limitations.

Figure 1.1: Distribution of Insurance Categories



Low-educated individuals ages 50-64

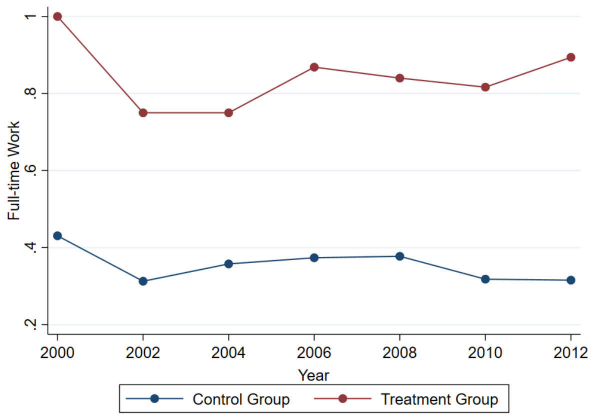


Low-income individuals ages 50-64

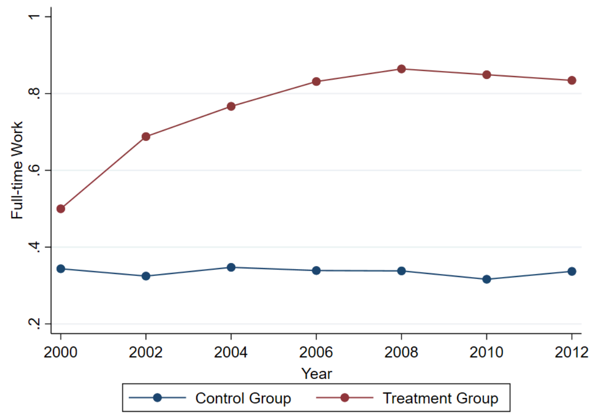
Figure 1.1 above represents the share of low-educated and low-income individuals aged 50-64 in the years 2010-2016 in each insurance category displayed in a legend. Low education is defined as having a high school degree or less, and low income is defined as having an annual household income equal to or less than $50K.

RHI represents individuals who have retiree coverage. No ESHI consists of those who do not have any employer-sponsored health insurance at all. Medicaid comprises individuals who have Medicaid coverage before the expansion. Finally, ESHI without RHI includes individuals with employer-sponsored health insurance but no retiree coverage. Note that observations with missing data regarding retiree health coverage are dropped.

Figure 1.2: Medicaid Expansion on Full-time Work



Expansion States



Non-Expansion States

Table 1.1: Treatment & Control Group

Treatment Group	Control Group
∗ Individuals who have employer-sponsored health insurance from their current or previous employer but do not have retiree coverage from their current or previous employer or their spouse.	∗ Individuals who have employer-sponsored health insurance and have retiree coverage from their current or previous employer or their spouse. ∗ Individuals who do not have employer-sponsored health insurance from a current or previous employer or union or their spouses/partners.

Table 1.2: Descriptive Statistics for Sample Years 2010-2016

Panel A: Expansion States	Treatment	Control	*t-value* (difference)
Working Full-Time	0.84	0.35	23.03
	(0.36)	(0.47)	
Married	0.26	0.53	-11.4
	(0.44)	(0.49)	
Divorced	0.35	0.17	9.43
	(0.48)	(0.38)	
Widowed	0.067	0.069	-0.16
	(0.25)	(0.25)	
Women	0.62	0.57	2.2
	(0.48)	(0.49)	
Age	56.4	57.9	-7.9
	(3.57)	(3.89)	
Annual Labor Earnings	4.42	1.92	15.4
	(4.31)	(3.23)	
Total Wealth per 10.000	13.53	22.85	-3.25
	(37.8)	(62.7)	
Share of Pensioner	0.66	0.19	24.2
	(0.47)	(0.39)	
N	505	3,448	

Panel B: Non-expansion States	Treatment	Control	*t-value* (difference)
Working Full-Time	0.84	0.36	22.5
	(0.36)	(0.48)	
Married	0.33	0.53	-8.96
	(0.47)	(0.49)	
Divorced	0.37	0.17	7.97
	(0.46)	(0.38)	
Widowed	0.067	0.074	-0.5
	(0.25)	(0.26)	
Women	0.64	0.56	3.46
	(0.48)	(0.49)	
Age	56.6	57.7	-5.7
	(3.57)	(3.96)	
Annual Labor Earnings	3.5	1.69	13.9
	(3.27)	(2.7)	
Total Wealth per 10.000	10.09	13.5	-2.21
	(17.8)	(35.3)	
Share of Pensioner	0.61	0.19	22.46
	(0.48)	(0.39)	
N	545	3,771	

The sample is restricted to non-disabled, childless adults, ages 50-64, with a high school degree or less. All monetary values are inflated to 2016 dollars using the consumer price index. Standard deviations are in parentheses.

Table 1.3: Timing of State Medicaid Expansion for Childless Adults

Early Expansion States (Before January 2014)	Expansion States (January 2014)	Late Expansion States (After January 2014)	Non-Expansion States (As of December 2016)
Connecticut	Arizona	Alaska (September 1, 2015)	Alabama
Delaware	Arkansas	Indiana (February 1, 2015)	Florida
Minnesota	California	Louisiana (July 1, 2016)	Georgia
New York	Colorado	Michigan (April 1, 2014)	Idaho
Vermont	Hawaii	Montana (January 1, 2016)	Kansas
Washington, DC	Illinois	New Hampshire (August 15, 2014)	Maine
n=6	Iowa	Pennsylvania (January 1, 2015)	Mississippi
	Kentucky	*n=7*	Missouri
	Maryland		Nebraska
	Massachusetts		North Carolina
	Nevada		Oklahoma
	New jersey		South Carolina
	New Mexico		South Dakota
	North Dakota		Tennessee
	Ohio		Texas
	Oregon		Utah
	Rhode Island		Virginia
	Washington		Wyoming
	West Virginia		*n=18*
	Wisconsin*		
	n=20		

*Although Wisconsin has not expanded Medicaid under the ACA, in 2014, Wisconsin extended its Medicaid program (which is called BadgerCare) to all individuals with income up to 100% FPL (without enrollment cap), so Wisconsin treated as an expansion states.
Source: https://www.healthinsurance.org/medicaid/wisconsin/

Table 1.4: Testing Parallel Trend for Triple Differences (DDD)

Full-time work	(1)
$Treatment \times Expansion \times 2000$	-0.02
	(0.27)
$Treatment \times Expansion \times 2002$	-0.16*
	(0.08)
$Treatment \times Expansion \times 2004$	0.002
	(0.07)
$Treatment \times Expansion \times 2006$	-0.12
	(0.08)
$Treatment \times Expansion \times 2008$	-0.05
	(0.07)
$Treatment \times Expansion \times 2010$	-0.02
	(0.07)
$Treatment \times Expansion \times 2014$	-0.03
	(0.05)
$Treatment \times Expansion \times 2016$	-0.10
	(0.07)
P-Value: Joint Lead Test	0.17
Observations	14,229
Number of Clusters	7,250

Notes: Robust standard errors clustered at the level of the individual are in parentheses.

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

Table 1.5: Triple Differences Estimates of the ACA's Medicaid Expansions on Full-time Work

Dependent Variable: Full-time Work	(1)	(2)	(3)
Treatment × Expansion × Post2014	-7.9**	-8.2**	-7.06*
	(4.05)	(4.1)	(3.8)
Post2014 × Expansion	1.6	1.97	2.11
	(1.8)	(2.56)	(2.38)
Treatment × Post2014	4.6	4.5	8.02*
	(4.5)	(4.6)	(4.4)
Treatment × Expansion	3.9	4.2	3.2
	(4.5)	(4.5)	(3.8)
Post2014	-22***	-17***	-18***
	(6.5)	(6.8)	(6.01)
Treatment	12.8	12.7	0.04
	(8.2)	(8.1)	(8.5)
Expansion	2.4	10.4	1.8
	(7.5)	(17.6)	(15.4)
P-Value from multiway clustering at household and state level$*$	0.052	0.044	0.05
Individual controls	No	No	Yes
Year, age and individuals fixed effects	Yes	Yes	Yes
Year and age indicators × Treatment	Yes	Yes	Yes
State specific linear time trends	No	Yes	Yes
Observations	8,269	8,269	8,261
Number of clusters	4,682	4,682	4,677

Notes: This table presents triple-differences estimates of the effects of the ACA's Medicaid expansion on full-time work. Early and late expansion states are excluded from the analysis (see Table 3). Individual controls include dummies for being married, divorced, or widowed; an indicator for being enrolled in a pension plan from the current job; and total wealth (Appendix Table H1.1 displays coefficient estimates of individual controls). Robust standard errors clustered at the level of the individual are in parentheses.

$*$ Multiway clustering was implemented using the *reghdfe* Stata estimator, in which singleton groups are dropped from the regression sample (Correia 2015).

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

Table 1.6: Triple Differences Estimate of Medicaid Expansion Effect On Labor Supply

Dependent Variable	Full-Time Work		Part-Time Work		Any Work		Self-Employed	
Specification	No Controls	Baseline	No Controls	Baseline	No Controls	Baseline	No Controls	Baseline
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment × Expansion × Post2014	-7.9**	-7.06*	0.09	1.3	-7.8**	-5.79	0.8	1.12
	(4.05)	(3.83)	(3.54)	(3.56)	(3.84)	(3.65)	(1.8)	(1.86)
P-Value from multiway clustering at household and state level	0.052	0.05	0.98	0.71	0.052	0.063	0.64	0.62
Year,age and individual fixed effects	Yes	Yes	Yes	Yes	Yes	Yes		
Year and age indicators × Treatment	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State specific linear time trends	No	Yes	No	Yes	No	Yes	No	Yes
Observations	8,269	8,008	8,269	8,008	8,269	8,008	8,258	7,998
Number of clusters	4,682	4,583	4,682	4,583	4,682	4,583	4,677	4,579

Notes: This table presents triple-differences estimates of the effects of the ACA's Medicaid expansion on full-time work (columns 1 and 2), part-time work (columns 3 and 4), and any work (columns 5 and 6) and self-employed (Columns 7 and 8). Individual controls include dummies for being married, divorced, or widowed; an indicator for being enrolled in a pension plan from the current job; and total wealth. Robust standard errors clustered at the level of the individual are in parentheses.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 1.7: The Effect on Annual Labor Earnings

Dependent Variable	Annual labor earnings			Wages		
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment × Expansion × Post2014	713.71	524.72	955.07	4.21	3.96	3.88
	(2860)	(2828)	(2805)	(2.83)	(2.98)	(2.96)
P-Value from multiway clustering at household and state level	0.84	0.84	0.73	0.18	0.28	0.32
Individual controls	No	Yes	Yes	No	No	Yes
Year, age and individual fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Year and age indicators × Treatment	Yes	Yes	Yes	Yes	Yes	Yes
State specific linear time trends	No	Yes	Yes	No	Yes	Yes
Observations	8,269	8,269	8,261	4,638	4,638	4,509
Number of clusters	4,682	4,682	4,677	2,893	2,893	2,827

Notes: This table presents triple-differences estimates of the effects of the ACA's Medicaid expansion on annual labor earnings and wages. Dollars are inflated to 2016 prices by the consumer price index. The dependent variable of columns 1, 2, and 3 is annual earnings. The dependent variable of columns 4,5, and 6 is wages, defined as: $w_{it} = AnnualLaborEarnings_{i,t} / (UsualWeeklyHours_{i,t} \times 52)$. Individual controls include dummies for being married, divorced, or widowed; an indicator for being enrolled in a pension plan from the current job; and total wealth. Robust standard errors clustered at the level of the individual are in parentheses.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 1.8: Placebo Tests

Dependent Variable: Full-time Work	High-educated Adults (1)	(2)	(3)	False Treatment Time (4)	(5)	(6)
Treatment $\times$ Expansion $\times$ Post2014	-5.03	-2.6	5.36	8.4	7.9	-0.02
	(5.9)	(5.98)	(5.5)	(6.8)	(6.9)	(7.77)
P-Value from multiway clustering at household and state level	0.41	0.62	0.33	0.23	0.3	0.99
Individual controls	No	No	Yes	No	No	Yes
Year, age and individual fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Year and age indicators $\times$ Treatment	Yes	Yes	Yes	Yes	Yes	Yes
State specific linear time trends	No	Yes	Yes	Yes	Yes	Yes
Observations	3,759	3,759	3,754	8,394	8,394	5,794
Number of clusters	2,191	2,191	2,191	5,309	5,309	3,708

Notes: This table presents triple-differences estimates of the effects of the ACA's Medicaid expansion on full-time work for higher education adults (columns 1-3) and uses pre-ACA years of the data with false treatment time (columns 4-6). Individual controls include dummies for being married, divorced, or widowed; an indicator for being enrolled in a pension plan from the current job; and total wealth. Robust standard errors clustered at the level of the individual are in parentheses.

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

Table 1.9: Robustness Check: The Inclusion of Early & Late Expansion States

Dependent Variable: Full-time work	(1)	(2)	(3)
Treatment $\times$ Expansion	-5.42*	-5.58*	-6.16*
	(3.19)	(3.20)	(3.16)
P-Value from multiway clustering at household and state level	0.09	0.055	0.017
Individual controls	No	No	Yes
Year, age and individual fixed effects	Yes	Yes	Yes
Year and age indicators $\times$ Treatment	Yes	Yes	Yes
State specific linear time trends	No	Yes	Yes
Observations	10,236	10,236	10,228
Number of clusters	5,825	5,825	5,820

Notes: This table presents triple-differences estimates of the effects of the ACA's Medicaid expansion on full-time work. Individual controls include dummies for being married, divorced, or widowed; an indicator for being enrolled in a pension plan from the current job; and total wealth. Robust standard errors clustered at the level of the individual are in parentheses.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 1.10: Robustness Checks: Low Income Group

Dependent Variable: Full-time work	(1)	(2)	(3)
Treatment × Expansion × Post2014	-14***	-13.2**	-9.87*
	(5.1)	(5.26)	(5.07)
P-Value from multiway clustering at household and state level	0.008	0.02	0.03
Individual controls	No	No	Yes
Year and individual fixed effects	Yes	Yes	Yes
Year and age indicators × Treatment	Yes	Yes	Yes
State specific linear time trends	No	Yes	Yes
Observations	4,241	4,241	4,237
Number of clusters	1,893	1,893	1,892

Notes: This table presents triple-differences estimates of the effects of the ACA's Medicaid expansion on full-time work. The sample is restricted to low-income (annual household income equal to or less than $50K in 2012) childless adults ages 50-64 years old. Individual controls include dummies for being married, divorced, or widowed; an indicator for being enrolled in a pension plan from the current job; and total wealth. Robust standard errors clustered at the level of the individual are in parentheses.

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

Table 1.11: Robustness Checks: Alternative Control Group

Dependent Variable: Full-time work	(1)	(2)	(3)
Treatment × Expansion × Post2014	-6.32	-7.97*	-7.13*
	(4.44)	(4.55)	(4.25)
P-Value from multiway clustering at household and state level	0.18	0.08	0.07
Individual controls	No	No	Yes
Year, age and individual fixed effects	Yes	Yes	Yes
Year and age indicators × Treatment	Yes	Yes	Yes
State specific linear time trends	No	Yes	Yes
Observations	3,587	3,587	3,585
Number of clusters	2,247	2,247	2,245

Notes: This table presents the triple-differences estimates of the effects of the ACA's Medicaid expansion on full-time work relative to a control group of individuals who have RHI (individuals who have no ESHI are excluded from the control group). Individual controls include dummies for being married, divorced, or widowed; an indicator for being enrolled in a pension plan from the current job; and total wealth. Robust standard errors clustered at the level of the individual are in parentheses.

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

Table 1.12: Robustness Checks: Accounting for the Great Recession

Dependent Variable: Full-time work	(1)	(2)
Treatment × Expansion × Post2014	-7.45*	-7.03*
	(4.15)	(3.89)
P-Value from multiway clustering at household and state level	0.08	0.077
Individual controls	No	Yes
Year, age and individual fixed effects	Yes	Yes
Year and age indicators × Treatment	Yes	Yes
Year, Treatment indicators × States Indicators	Yes	Yes
Observations	8,269	8,261
Number of clusters	4,682	4,677

Notes: This table presents the triple-differences estimates of the effects of the ACA's Medicaid expansion on full-time work, accounting for the Great recession. Individual controls include dummies for being married, divorced, or widowed; an indicator for being enrolled in a pension plan from the current job; and total wealth. Robust standard errors clustered at the level of the individual are in parentheses.

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

Table 1.13: Hazard Models

Dependent Variables:	Full-time Work		Any Work	
	(1)	(2)	(3)	(4)
Treatment × Expansion × Post2014	1.12*	1.12*	1.26**	1.26**
	(0.07)	(0.071)	(0.15)	(0.14)
P-Value from multiway clustering at household and state level	0.075	0.077	0.048	0.045
Individual controls	No	Yes	No	Yes
Observations	4,063	3,932	5,650	5,593
Number of Clusters	2,339	2,283	3,128	3,098

Notes: This table presents estimates of the effect of the ACA Medicaid expansion on full-time work (columns 1 and 2) and on any work (columns 3 and 4) using a hazard model (failure is leaving full-time work in columns 1 and 2, and leaving work completely in columns 3 and 4). Individual controls include an indicator of gender, race, marital status (married, widowed, divorced); indicators for being enrolled in a pension plan from the current job; full set of age dummy variables; years of education; and total wealth. Robust standard errors clustered at the level of the individual are in parentheses.

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

A Data Appendix

The Rand HRS data from 2010-2016 includes 42,052 individuals and 168,208 person-year observations. Restricting the sample to non-disabled, low-educated, childless adults ages 50-64 yields a final sample of 4,682 individuals, 3,984 households, and 8,269 person-year observations. Table A1.1 illustrates information on sample loss due to each restriction.

Table A1.1: Sample Size after Each Sample Selection Criteria

Sample Selection Criteria	Sample Size (Year-person observations)
Total number of observation between 2010-2016	168,208
Exclude observations with missing state identifier	98,954
Exclude Early & Late Expansion States	77,795
Exclude any periods after re-entry to full-time work	74,533
Exclude observations with missing age information	61,665
Exclude individuals if they have Medicare and aged below 65	58,083
Exclude individuals who are disabled	56,981
Exclude observations with missing information on RHI (dropping missing values of treatment variable)	42,412
Exclude individuals who has missing education information	42,279
Restrict to low-educated, childless adults aged between 50-64	8,269

B Including the Period After Re-Entry to Full-time Work

In the main analysis, I exclude periods after re-entry to full time work because the triple-difference technique utilized in my empirical model is designed to reflect movement in one

Table B1.1: Triple Differences Estimates of the ACA's Medicaid Expansions on Full-time Work

Dependent Variable: Full-time Work	(1)	(2)	(3)
Treatment × Expansion × Post2014	-9.6*	-10**	-9.2*
	(4.09)	(5.02)	(4.7)
Post2014 × Expansion	2.2	3.9	3.8
	(2.1)	(3.4)	(3.2)
Treatment × Post2014	10.7*	11.2 **	12.7**
	(4.5)	(5.6)	(5.3)
Treatment × Expansion	8.02	8.6*	6.6
	(4.9)	(4.9)	(4.3)
Post2014	-7.6	-3.2	-3.5
	(7.6)	(7.9)	(7.1)
Treatment	14.2	13.3	-0.08
	(9.5)	(9.8)	(10.3)
Expansion	-3.7	15.3	6.2
	(8.2)	(20.5)	(18.6)
P-Value from multiway clustering at household and state level∗	0.056	0.022	0.05
Individual controls	No	No	Yes
Year, age and individuals fixed effects	Yes	Yes	Yes
Year and age indicators × Treatment	Yes	Yes	Yes
State specific linear time trends	No	Yes	Yes
Observations	9,115	9,115	9,107
Number of clusters	4,879	4,879	4,874

Notes: This table presents triple-differences estimates of the effects of the ACA's Medicaid expansion on full-time work. Early and late expansion states are excluded from the analysis (see Table 1.3). Individual controls include dummies for being married, divorced, or widowed; an indicator for being enrolled in a pension plan from the current job; and total wealth (Appendix Table H1.1 displays coefficient estimates of individual controls). Robust standard errors clustered at the level of the individual are in parentheses.

∗ Multiway clustering was implemented using the *reghdfe* Stata estimator, in which singleton groups are dropped from the regression sample (Correia 2015).

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

direction-departing from full-time work. However, this might lead selection bias if the ACA's Medicaid expansion influence post-retirement labor supply decision. I re-estimate the equation (1) by including all observations in the analysis to test the robustness of results.

Table B1.1 presents triple-difference estimates that include all observations. Table B1.1 confirms that the results hold when using all observations in the analysis.

C The Impact of the ACA's Medicaid Expansion on Total Household Out-of-Pocket Medical Spending

To analyze the effects of Medicaid expansion on the household out-of-pocket medical spending of low-educated, childless individuals without any ESHI, I utilize a log-linear model and quantile estimation. Table C1.1 shows the effects of Medicaid expansion on the log of total household out-of-pocket medical spending. Similarly, Table C1.2 illustrates the change in expenditure at every fifth quantile of the distribution of total household out-of-pocket medical spending associated with Medicaid expansion. Tables C1.1 and C1.2 illustrate that Medicaid expansion did not affect household out-of-pocket medical spending of low-educated, childless individuals who do not have health insurance from their employer or their spouse.

The HRS data provide information regarding individuals' and their spouses' total out-of-pocket medical spending. Total household out-of-pocket total medical spending is constructed by summing individuals' and their spouses' total out-of-pocket medical spending.

Table C1.1: The Effect of Medicaid Expansion on Total Household Out-of-Pocket Medical Spending

Dependent Variable: Log(OOP Spending)	(1)
Expansion $\times$ Post2014	-0.09
	(0.093)
Expansion	0.18
	(0.28)
Post2014	-0.18**
	(0.08)
Individual controls	Yes
Year and individual fixed effects	Yes
Observations	5,667
Number of Clusters	3,215

Notes: This table presents the effects of the ACA's Medicaid expansion on the log of total household out-of-pocket medical spending. Individual controls include dummies for being married, divorced, or widowed; an indicator for being enrolled in a pension plan from the current job; and total wealth . The sample includes non-disabled, low-educated, childless individuals, ages 50-64, with no ESHI. Robust standard errors clustered at the level of the individual are in parentheses.

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

Table C1.2: The Effect of Medicaid Expansion on Total Household Out-of-Pocket Medical Spending

Quantile	5th	15th	25th	35th	45th	55th	65th	75th	85th	95th
Expansion × Post2014	-624	-588	-579	-566	-551	-513	-452	-433	-401	-361
	(30116)	(24145)	(22545)	(20309)	(17871)	(11431)	(1864)	(2675)	(7751)	(14468)
Expansion	630	633	634	635	637	640	646	648	650	654
	(51515)	(41301)	(38565)	(34739)	(30569)	(19554)	(3189)	(4576)	(13259)	(25728)
Post2014	-39	-140	-167	-204	-245	-355	-528	-582	-674	-788
	(29121)	(23348)	(21801)	(19638)	(17281)	(11054)	(1803)	(2587)	(7495)	(13990)
Individual controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Individual Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Standard errors are in parentheses. Individual controls include dummies for being married, divorced, or widowed; an indicator for being enrolled in a pension plan from the current job; and total wealth. The sample includes non-disabled, low-educated, childless individuals, ages 50-64, with no ESHI.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

D The Impact of the ACA's Medicaid Expansion on ESHI and RHI

Table D1.1 illustrates the effects of the ACA's Medicaid expansion on employer-sponsored health insurance (Column 1) and retiree coverage (Column 2).

Panel A of Table D1.1, illustrates the results for the full sample; panel B for those who are low-educated, childless ages 50-64.

Table D1.1: The Effect of Medicaid Expansion on ESHI and RHI

Dependent Variable:	ESHI	RHI
Panel A : Full Sample		
Expansion × Post2014	0.0003	0.006
	(0.005)	(0.022)
Expansion	0.007	-0.003
	(0.02)	(0.05)
Post2014	-0.09***	0.03
	(0.004)	(0.022)
Year and individual fixed effects	Yes	
Observations	58,522	7,873
Number of Clusters	20,266	4,711
Panel B : Low-educated, childless adults ages 50-64		
Expansion × Post2014	-0.02	-0.016
	(0.013)	(0.04)
Expansion	0.008	0.013
	(0.07)	(0.03)
Post2014	-0.04***	0.028
	(0.01)	(0.04)
Individual controls		
Year and individual fixed effects	Yes	Yes
Observations	12,780	3,411
Number of Clusters	6,009	2,159

Notes: This table presents the effect of the ACA's Medicaid expansion on employer-sponsored health insurance (ESHI) and retiree coverage (RHI). Robust standard errors clustered at the level of the individual are in parentheses.

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

E The impact of Employer Mandate on Working Hours

Table E1.1 illustrates data on working hours by expansion, non-expansion states, and time periods. Working hour counts the number of hours per week a person works at her/his main and second job. The years 2010,2012, and 2014 are defined as the periods before the employer mandate, while the year 2016 counts as the period after the employer mandate.

Panel A of Table E1.1 illustrates the results for the full sample; panel B for those who are

Table E1.1: Difference-Difference Estimates of Employer Mandate Implication on Working Hours

Group/year	Before Employer Mandate	After Employer Mandate	Difference
Panel A : Full Sample			
Expansion States	16.03	16.82	0.798
	(0.14)	(0.25)	(0.28)
Non-expansion States	14.23	15.68	1.45
	(0.136)	(0.28)	(0.27)
Difference-in-Difference			-0.65
			(0.40)
Number of Observations			63,228
Panel B : Low-educated, childless adults ages 50-64			
Expansion States	26.78	28.34	1.56
	(0.3)	(0.52)	(0.65)
Non-expansion States	26.68	27.72	1.04
	(0.32)	(0.56)	(0.64)
Difference-in-Difference			0.52
			(0.92)
Number of Observations			12,616

Notes: Before the employer mandate includes the years 2010, 2012, and 2014 while after the employer mandate includes 2016. Robust Standard errors are shown in parentheses.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

low-educated, childless ages 50-64.

The difference-in-differences estimates in the third row of panel A and B indicates that there is no significant difference in the response of expansion and non-expansion states regarding workings hours to the employer mandate.

F Excluding Wisconsin from the Sample

Prior to 2014, the Medicaid program in Wisconsin was limited to children, pregnant women, and parents with dependent children. Wisconsin did not expand Medicaid under the ACA; however, in 2014, it extended its Medicaid program to all individuals with income up to 100% FPL. As a result, Wisconsin is the only non-expansion state without a coverage gap.[23] Therefore, I treat

[23]Source: https://www.healthinsurance.org/medicaid/wisconsin/

Table F1.1: Triple Differences Estimates Excluding Wisconsin

	Excluding Early & Late Expansion States			Including Early & Late Expansion States		
Dependent Variable: Full-time work	(1)	(2)	(3)	(3)	(4)	(5)
Treatment × Expansion × Post2014	-6.5	-6.87*	-5.55	-4.5	-4.65	-5.36*
	(4.02)	(4.05)	(3.8)	(3.18)	(3.2)	(3.1)
P-Value from multiway clustering at household and state level	0.11	0.09	0.059	0.16	0.13	0.04
Individual controls	No	No	Yes	NO	No	Yes
Year,age and individual fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Year and age indicators × Treatment	Yes	Yes	Yes	Yes	Yes	Yes
State specific linear time trends	No	Yes	Yes	No	Yes	Yes

Notes: This table presents triple-differences estimates of the effects of the ACA's Medicaid expansion on full-time work using the sample excluding Wisconsin. In columns 1-3, the sample is limited to states that expanded Medicaid in 2014 under the ACA (no early and late expansion states), while in columns 3-5, early and late expansion states are added to the sample. Individual controls include dummies for being married, divorced, or widowed; an indicator for being enrolled in a pension plan from the current job; and total wealth. Robust standard errors clustered at the level of the individual are in parentheses.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Wisconsin as an experimental state so far.

To test the robustness of my findings, I remove Wisconsin from the sample and re-estimate the equation (1). Since a large number of childless adults, approximately 99,000, became newly eligible for Wisconsin Medicaid with its recent expansion, defining Wisconsin as a non-expansion state might be misleading; therefore, for robustness check, I exclude Wisconsin instead of defining it as a non-expansion state.

Table F1.1 presents triple-differences estimates that exclude Wisconsin. Columns 1-3 of Table F1.1 present the result for the sample excluding early and late expansion states, while columns 3-5 display the result for the sample, including early and late expansion states. Table F1.1 confirms that the qualitative results hold using the sample excluding Wisconsin. These findings indicate -5.55 percentage points decline in full-time work for the sample excluding early and late expansion states, and -5.36 percentage points decline in full-time work for the sample, including early and late expansion states.

Table G1.1: Triple Differences Estimates Excluding Hawaii and Arizona

Dependent Variable: Full-time work	(1)	(2)	(3)
Treatment $\times$ Expansion $\times$ Post2014	-11.5**	-12.04**	-10**
	(5.07)	(5.1)	(4.8)
P-Value from multiway clustering at household and state level	0.019	0.015	0.044
Individual Controls	No	No	Yes
Year, age and individuals fixed effects	Yes	Yes	Yes
Year and age indicators $\times$ Treatment	Yes	Yes	Yes
State specific linear time trends	No	Yes	Yes

Notes: This table presents triple-differences estimates of the effects of the ACA's Medicaid expansion on full-time work, excluding Hawaii and Arizona from the sample. Individual controls include dummies for being married, divorced, or widowed; an indicator for being enrolled in a pension plan from the current job; and total wealth. Robust standard errors clustered at the level of the individual are in parentheses.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

G Excluding Arizona and Hawaii from the Sample

In the main analysis, I exclude early and late expansion states for the purpose of clear analysis. However, I did not drop Hawaii and Arizona as early expansion states because they closed their enrollment and were reinstated at the ACA level in 2014. Though Hawaii is quite small, Arizona's eligibility changes might confound the results given its size and large retirement communities.

To test the robustness of my findings, I remove Arizona and Hawaii from the sample and re-estimate equation (1). Table G1.1 presents triple-differences estimate that exclude Arizona and Hawaii. The results are similar to the main finding.

H The Main Result with Coefficient Estimates of Individual Controls

Table H1.1 display the triple-differences estimates of the effects of the ACA Medicaid expansion on full-time work. Individual controls include an indicator for being married, divorced, or widowed; indicator for being enrolled in a pension plan from the current job; and total wealth.

Table H1.1: Triple Differences Estimates of the ACA Medicaid Expansions on Full-time Work

Dependent Variable: Full-time work	(1)	(2)	(3)
Treatment × Expansion × Post2014	-0.079**	-0.082**	-0.0706*
	(0.04)	(0.041)	(0.038)
Post2014 × Expansion	0.016	0.0197	0.02
	(0.018)	(0.026)	(0.024)
Treatment × Post2014	0.046	0.045	0.08*
	(0.045)	(0.046)	(0.044)
Treatment × Expansion	0.039	0.042	0.032
	(0.045)	(0.045)	(0.038)
Post2014	-0.22***	-0.17***	-0.17***
	(0.065)	(0.068)	(0.06)
Treatment	0.128	0.127	-0.0004
	(0.082)	(0.081)	(0.085)
Expansion	0.024	0.104	0.018
	(0.075)	(0.176)	(0.154)
Married	-	-	-0.04
			(0.032)
Divorced	-	-	0.01
			(0.031)
Widowed	-	-	-0.04
			(0.04)
Pension	-	-	0.42***
			(0.024)
Total Wealth*	-	-	-0.000017
			(0.00011)
Year, age and individual fixed effects	Yes	Yes	Yes
Year and age indicators × Treatment	Yes	Yes	Yes
State specific linear time trends	No	Yes	Yes
Observations	8,269	8,269	8,008
Number of clusters	4,682	4,682	4,583

Notes: This table presents triple-differences estimates of the effects of the ACA's Medicaid expansion on full-time work. Robust standard errors clustered at the level of the individual are in parentheses. *Total wealth is scaled by dividing 10.000.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

I Alternative Identification

In the main model, I include a full set of age and year fixed effects along with their interaction with the Treat dummy variable. The reason for adding interaction terms in the model is to capture the heterogeneity of being in the treatment group over time and across age groups, which allows me to account for the non-linear and time-varying nature of being in the treatment group. However, given the relatively small sample size, including a full set of age, year fixed effect, and their interaction with the Treat dummy variable plus individual fixed effect and state-specific time

Table I1.1: Alternative Identification

Dependent Variable: Full-time work	(1)	(2)	(3)
Treatment × Expansion × Post2014	-8.09**	-8.42**	7.14 *
	(4.02)	(4.04)	(3.8)
P-Value from multiway clustering at household and state level	0.04	0.034	0.038
Individual controls	No	No	Yes
Year, age and individual fixed effects	Yes	Yes	Yes
State specific linear time trends	No	Yes	Yes
Observations	8,269	8,269	8,261
Number of clusters	4,682	4,682	4,677

Notes: This table presents triple-differences estimates of the effects of the ACA's Medicaid expansion on full-time work. Individual controls include dummies for being married, divorced, or widowed; an indicator for being enrolled in a pension plan from the current job; and total wealth. Robust standard errors clustered at the level of the individual are in parentheses.

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

trends might result in a saturated model. To alleviate this concern, I re-estimate equation (1) without interaction of the Treat dummy variable with age and year fixed effects. Table I1.1 illustrates that the results are consistent with the main findings when the interaction terms are not excluded from the model.

J Difference-in-Differences Estimates the Impact of the ACA's Medicaid Expansion on Full-time Work

Table J1.1 shows the effects of the ACA's Medicaid expansion on full-time work. I restrict the sample to low-educated, childless individuals ages 50-64. Note that the expansion and non-expansion states I used in this analysis are the same as the ones I used in the main analysis (See Table 1.3).

Table J1.1: The Effect of Medicaid Expansion on Full-time Work

Dependent Variable: Full-time Work	(1)
Expansion × Post2014	0.0048
	(0.06)
Expansion	-0.04
	(0.07)
Post2014	-0.08
	(0.06)
Year and individual fixed effects	Yes
Observations	12,957
Number of Clusters	6,09

Notes: This table presents the effects of the ACA's Medicaid expansion on retirement. The sample includes non-disabled, low-educated, childless individuals ages 50-64. Robust standard errors clustered at the level of the individual are in parentheses.

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

2. Heterogeneity in Crowd-Out by Risk Aversion: Assessing the Effect of the ACA Medicaid Expansion

2.1 Introduction

In 2009, before the Affordable Care Act (ACA), 46 million persons under age 65 lacked health insurance (Cohen et al. 2009). Medicaid eligibility expansion under the ACA was designed to address this. However, the expansion is not limited to the previously uninsured, so crowding-out pre-existing private health insurance might occur. That is, individuals might replace their existing private health coverage with Medicaid coverage. In the case of extensive crowd-out, the expansion merely shifts coverage from the private to the public sector, which increases government health care spending but does not lead to a change in net insurance coverage rate.

The literature studying crowd-out from Medicaid expansions has produced mixed results that are sensitive to empirical methods, the data set used, and the definition of crowd-out. In addition, most of these studies aimed to determine the average treated crowd-out rate. However, when risk preference differs across individuals, analyzing average crowd-out rates is insufficient. Varying risk preferences across people might lead to individuals' responses to Medicaid expansion being vastly different. Indeed, studies that focus on selection in insurance markets call the assumption that individuals are homogeneous in their risk preferences into question (Cohen and Einav 2007; De Meza and Webb 2001; Davidoff and Welke 2004; Finkelstein and McGarry 2006; Einav et al. 2007). These papers show that individuals vary not only with their expected risk but also their risk preference, which can result in favorable sorting into insurance markets.

In theory, risk-averse individuals demand more insurance, and therefore, they are more likely to be fully or more comprehensively insured compared to risk-loving individuals. As a consequence, when Medicaid becomes available to them, it might work differentially for risk-averse individuals compared to risk-loving individuals. Risk-averse individuals might perceive Medicaid coverage as an inferior health insurance, while it might be a better option for risk-loving individuals relative to their limited health insurance coverage. This might mute the Medicaid take-up effect among risk-averse individuals. Similarly, risk-averse individuals are likely to be reluctant to change their insurers or doctors, which discourage them from enrolling in Medicaid.

Knowing the response of individuals with varied risk preferences levels to public insurance expansion also has implications on the cost of expansion. There are two pathways whereby the effect of risk preferences determines healthcare utilization. First, there should be a positive relationship between the propensity to engage in risky behaviors, such as not wearing a helmet when riding a motorcycle, excessive drinking, and illicit drug use and risk aversion. This pathway gives rise to the so-called *ex-ante* moral hazard problem. Since highly risk-averse individuals intend to avoid risky behavior or exert effort to prevent losses, actions deemed as ”self-protection” in the economic term, the extent of *ex-ante* moral hazard would be smaller for individuals whose risk aversion is higher. This would result in lower health care utilization.

The second and less obvious relationship between risk aversion and health care consumption can be explained by *ex-post* moral hazard. *Ex-post* moral hazard refers to a change in consumer demand for health care in response to the out-of-pocket price the consumer has to pay for that care (Pauly 1968; Cutler and Zeckhauser 2000). The magnitude of *ex-post* moral hazard, therefore, depends on the price elasticity of demand for health care services. Highly risk-averse individuals are likely to seek medical care regardless of price, so their demand for health care tends to be inelastic. In contrast, less risk-averse individuals tend to ignore minor health problems when faced with a high cost of medical care; hence their demand for health care is likely more elastic. Thus, risk-loving individuals are likely to change their health care utilization when they have access to free or lower prices for health care through Medicaid coverage.

In sum, risk-loving individuals would have higher health care utilization compared to highly risk-averse individuals due to higher *ex-ante* and *ex-post* moral hazard. If risk-loving individuals respond more to public insurance expansion, the cost of expansion, therefore, might be higher than the expected cost.

In this paper, using the Health and Retirement Study (HRS) data, I quantify how much private health insurance decreased and how much Medicaid coverage increased as a causal effect of the ACA Medicaid expansion among risk-averse versus risk-loving individuals. Prior to the ACA Medicaid expansion, eligible individuals for Medicaid were primarily pregnant women, parents, the elderly, and the disabled. The ACA Medicaid expansion removed categorical exclusions, and everyone making less than 138 percent of the federal poverty line became qualified for Medicaid coverage. The expansion was initially formulated to occur nationwide, but a 2012 Supreme court decision on the ACA made it optional for states. To date, 39 states, including the District of Columbia, adopted the Medicaid expansion, and 12 states have opted not to expand Medicaid. I exploit this variation in the timing and expansion decisions of states by employing a difference-in-differences (DD) approach. To estimate heterogeneity in Medicaid crowd-out effects, I utilize a direct measure of risk aversion by the HRS questions that designed to elicit risk preferences. I focus my analysis primarily on low-educated, childless, and working individuals aged under 65, given this is the group most likely affected by the expansion.

I find that the Medicaid expansion led to a decrease in private coverage by 5 percentage points among risk-loving individuals; however, there is no evidence that the expansion crowded out private coverage for highly risk-averse individuals. Similarly, the expansion increased Medicaid coverage by 3 percentage points among risk-loving individuals, while no change was observed for risk-averse individuals. These findings suggest that risk-averse individuals prefer to keep their private coverage even though they become eligible for Medicaid; however, risk-loving individuals respond to Medicaid expansion and drop their private coverage in favor of Medicaid coverage. This is consistent with the results of Wettstein (2016). He used Medicare Part D expansion to examine heterogeneity in crowd-out along the dimension of risk aversion and found that risk-aversion is associated with 5 percentage points less crowd-out over a base crowd-out rate of 50-60%.

This paper contributes to the literature that studies crowd-out from Medicaid expansion in several ways. First, I explore the implications of heterogeneity in risk aversion on the effects of Medicaid expansion on private health insurance. Second, most recent studies analyze the early years of the ACA Medicaid expansion. Drawing a conclusion regarding the effect of expansion might be premature when focusing on a short period of time after the expansion. Individuals might exercise caution in relying on Medicaid, especially when there is uncertainty regarding the continuation of the expansion. For example, Medicaid expansion in Kentucky was enacted in 2014. However, the Republication candidate for governor, Matt Bevin, campaigned on a healthcare pledge that would eliminate or change the Medicaid expansion. Therefore, the election of a new governor in 2015 has brought into question the future of Medicaid expansion. In such an atmosphere, individuals might adopt a wait-and-see attitude before abandoning their private insurance. In this paper, I utilize a longer-time period (years 2000-2018) to observe the full effect of the expansion. Third, unlike existing papers utilizing pooled cross-sectional data, I use individual-level panel data that allow me to track individuals across time and control time-invariant differences between individuals. Finally, the results of my paper provide further evidence that risk preference—and how it varies across individuals—is an important factor in determining insurance demand.

2.2 Background and Previous Research

2.2.1 Background

In 1965, the United States Congress created the Medicaid program, which grants funding to states to provide low-income people access to health care. In March 2010, during the Obama administration, the United States Congress enacted the Affordable Care Act (ACA) law to make healthcare more affordable and accessible for a large number of people. Three of the most important elements of the ACA are: (1) an expansion of Medicaid; (2) reforms designed to improve the functioning of private non-group markets, including community rating, coverage standards, the introduction of exchanges, subsidies, and purchase mandates and (3) a mandate for a large employer (50 or more full-time employees) to offer health insurance to their worker, and subsidies for small employers.

My analysis focuses on the effect of expanding the eligibility of Medicaid under the ACA. Before the expansion, the coverage of Medicaid was limited to those who were disabled, elderly, or with dependent children. After the ACA's Medicaid expansion, qualification was extended to 138 percent of the federal poverty level (FPL). Medicaid expansion was designed to be nationwide; however, 2012 Supreme Court decisions caused it to be optional for states. Most states expanded their income eligibility limit to 138% of the FPL in January 2014. Six states previously expanded coverage before 2014. These include CT, DE, ME, NY, VT, DC. In addition, there are 18 states that provided limited coverage for parents and/or childless adults, mainly access to primary care services, prior to the expansion. Table 2.1 illustrate the complete list of states.

In this analysis, I classify the 32 states that expanded Medicaid under the ACA as the treatment states and 18 states that did not adopt expansion at all as the control states. I exclude Wisconsin because it did not expand Medicaid under the ACA. In 2014, Wisconsin extended its Medicaid program (which is called BadgerCare) to all individuals with income up to 100% FPL. As a result, Wisconsin is the only non-expansion state without a coverage gap. [1]

2.2.2 Prior Research on Crowd-Out from Medicaid Expansions

Cutler and Gruber (1996) were the first to assess to the extent of crowd-out arising from the expansion of Medicaid. They used the expansion of Medicaid to pregnant women and children over the 1987-1992 period. Using March the current population survey (CPS) data, they simulated eligibility through computing average state-level eligibility measures for a fixed national random sample based on each state's Medicaid eligibility rules. They found the crowd-out rate of private insurance was almost 50 percent. Subsequent papers questioned the high rates of crowd-out Cutler and Gruber (1996) found and reanalyzed the effects of Medicaid expansions for children in the late 1980s and early 1990s. These studies used different approaches and/or data, and the consensus of these studies was that there was a small or statistically insignificant crowd-out (Dubay and Kenney 1996; Ham and Shore-Sheppard 2005; Shore-Sheppard 2008; Card and Shore-Sheppard 2004; Yazici and Kaestner 2000).

The introduction of the State Children's Health Insurance Program (SCHIP) presented another

[1] Source: https://www.healthinsurance.org/medicaid/wisconsin/

opportunity to assess the potential crowd-out effect. SCHIP was introduced as a means for states to provide coverage for children whose families earned beyond the cutoffs for Medicaid but likely still below an amount where they could afford private coverage. SCHIP was implemented by states in different years between 1997 and 2000. This extension of benefits led researchers to revisit the extent of crowd-out. Some of these studies found significant crowd-out rates ranging from 20-60% (Gruber and Simon 2008; Sasso and Buchmueller 2004; Bansak and Raphael 2007), while others found no statistically significant evidence of crowd-out of private health insurance (Hamersma and Kim 2013).[2]

With the implementation of the ACA's provisions, a growing body of research has begun to examine change in coverage. Several states (California, Connecticut, Minnesota, and Washington, D.C) adopted Medicaid expansion under the ACA before 2014. Sommers et al. (2014) utilized these expansions and found that private coverage decreased by 2 percentage points after Medicaid expansion in Connecticut, while that was not the case in D.C. In addition, they observed far less crowd-out of private health insurance among those with health problems. Abraham et al. (2019) used the 2010-2015 Medical Expenditure Panel Survey-Insurance component to estimate the effect of the ACA Medicaid expansion on three employer-sponsored health insurance (ESHI) outcomes: providing health insurance, eligibility, and take-up. The authors employed a difference-differences method and found no effect of expansion on these three outcomes. Similarly, Blavin et al. (2015) analyzed the effect of Medicaid expansion and market subsidies on employers' incentives to offer health insurance and workers' incentive take-up. They found no change in the offer and take-up rates. Kaestner et al.(2017) utilized the difference-in-differences method and a sample of low socioeconomic-status individuals to identify the effect of the ACA Medicaid expansion on insurance coverage. Among a sample of low-educated (high school education or less) individuals from the 2010-2015 American community survey (ACS) and the current population survey (CPS), they found that Medicaid expansion was associated with approximately a 4 percentage points increase

[2]Unlike to previous studies, Dilender (2017) examined the financial implication of crow-out. He found that families that transferred from private coverage to Medicaid could spend significantly less on health insurance expenses, \$4124–4284 per year on average.

in Medicaid coverage while they detected crowd-out of roughly 25% for parents; less for childless adults. Using the ACS data from 2011 to 2015, Wehby and Lyu (2018) applied the difference-differences method to examine the heterogeneity in the ACA Medicaid expansion effects by demographic factors (age, gender, race). The authors stratified the data by demographic groups and estimated separate models for each group. The results show that Medicaid take-up increased across all examined groups by age, gender, race/ ethnicity, and private coverage declined only for certain groups. However, private coverage declined significantly for most subgroups, and it decreased by 2.5 percentage points on average when they excluded states that had prior partial or full expansions from both the treatment and control groups.[3]

Unlike previous studies focusing specifically on the ACA Medicaid expansion, Courtemanche et al. (2017) estimated the aggregate impact of all 2014 elements of the ACA on insurance coverage. They found that at the average pre-treatment uninsured rate, non-Medicaid components of the ACA increased the coverage rate by 2.8 percentage points, and Medicaid expansion added another 3.1 percentage points. Further, the authors investigated heterogeneity in the ACA's impacts. The results suggest that coverage gains were larger among individuals without a college degree, nonwhites, young adults, unmarried individuals, and those without children in the home.

Similar to Courtemanche et al. (2017), Frean, Gruber, and Sommers (2017) estimated the effects of three components of the ACA in 2014—subsidized premiums for Marketplace coverage; the individual mandate; and the Medicaid expansion—using a triple-differences estimation strategy. The authors used both variations in the state decisions to expand Medicaid and the differential impacts of these decisions across income and family structure. The results show that the combined impact of three ACA policies of interest was a 2.3 percentage point increase in coverage, of which roughly 60 percent could be attributed to the Medicaid expansion, 40 percent to the premium subsidies, and essentially none to the individual mandate. In addition, they found no evidence of significant crowd-out of employer-sponsored coverage by the new premium subsidies and no ev-

[3]There are descriptive studies that report the change in insurance coverage from before and after the 2014 components of the ACA were implemented and found an increase in coverage rate (Long et al. 2015; Smith et al. 2014; Courtemanche et al. 2016).

idence of crowd-out of either employer coverage or non-group private coverage by the Medicaid expansion.

2.3 Data and Empirical Strategy

2.3.1 Data

The primary source of data for this analysis is the Rand version of the Health and Retirement Study (HRS) from 2000-2018.[4] The HRS is a biennial longitudinal survey of a nationally representative sample of older Americans and their spouses. The HRS data provide detailed information on individuals' health, insurance, employment status, and demographic characteristics. In addition, the HRS includes questions that are designed to elicit individuals' risk preferences, which allow me to construct a risk aversion index.

I obtained access to restricted geographic information from the HRS to identify individuals' states of residence.[5] The sample is restricted to low-educated, childless, and working people aged below 65 years old. Low-educated is defined as having a high school education or less. Working people include those who are working full-time, part-time, and partly retired. Retired, disabled, unemployed, and those who are not in the labor force are excluded from the sample. I focus on working people because most private coverage is provided through employment rather than purchased individually. I aim to estimate the possible transfer from private coverage to Medicaid by focusing on the group who had been most likely to be the target of Medicaid expansion. I also exclude older workers aged 65 and over because they are eligible for Medicare.

Private health insurance and Medicaid coverage are not mutually exclusive. Individuals might have both private health insurance and Medicaid coverage, which creates the overlap issue. The existence of an overlap between private insurance and public insurance categories alters the in-

[4]Rand HRS file is derived from all waves of the HRS. It provides a cleaned and user-friendly version of the original data and produced by the RAND Center for the Study of Aging, with funding and support from the National Institute on Aging (NIA) and the Social Security Administration (SSA)

[5]The HRS cross-wave geographic information merged with the Rand HRS data

terpretation of the extent of crowd-out based on how the overlap group is treated. Therefore, my dependent variables are indicators for Medicaid only (no overlap with private health insurance) and private health insurance only (no overlap with Medicaid). Columns 1 and 2 of Table 2.2 present descriptive statics of key variables for low-educated, childless individuals and low-educated childless working individuals, respectively. Since most private insurance is provided by employers, the share of individuals with private coverage is higher among working individuals. Similarly, the share of individuals with Medicaid coverage is lower among working individuals.

2.3.2 Measure of Risk Aversion

I construct a risk aversion measure that relies on a hypothetical gambling question in the HRS data. In the HRS data, respondents are asked the following question form to elicit their risk preferences: "*Suppose that you are the only income earner in the family. Your doctor recommends that you move because of allergies, and you have to choose between two possible jobs. The first would guarantee your current total family income for life. The second is possibly better paying, but the income is also less certain. There is a 50-50 chance the second job would double your total lifetime income and a 50-50 chance that it would cut it by x%. Which job would you take- the first job or the second job?*" The potential loss of income, x, ranges from 10% to 75%. Based on the answers to this question, individuals are categorized into six groups by increasing risk aversion (1 least risk-averse, 2nd least risk-averse, 3rd least risk-averse, 3rd most risk-averse, 2nd most risk-averse, and most risk-averse.

Respondents were selected to answer this question in waves 1, 4, 5, 6, 7, and 8, so the latest year respondents were surveyed was in 2008 (wave 8). I assume that risk aversion is largely stable over time. This assumption allows me to impute the missing risk variables by carrying forward individuals' answers from previous years.[6] Based on the risk aversion score, I then construct a dichotomous "risk-averse" and "risk-lover" variable. Those in the highest category of risk aversion are defined as risk-averse individuals, while others (those in 1 least risk-averse, 2nd least risk-

[6]The R^2 resulted from regressing risk-aversion score only on its lag is 0.87.

averse, 3rd least risk-averse, 3rd most risk-averse, 2nd most risk-averse) are categorized as risk-loving individuals.

To investigate to what extent the degree of risk aversion varies with individuals' characteristics, I estimate the risk-averse variable against a set of demographic and socio-economic variables. Table 2.3 shows that risk aversion is correlated with gender, marital status, race, household income, and wealth. It is worth underlining that heterogeneity in crowd-out may also operate along these individual characteristics. For example, women might be affected differently by the expansion. And the result show that women have a substantially higher degree of risk aversion than men. In such a case, the estimated effect would not necessarily represent a causal effect of risk aversion; such demographic factors might drive it. Utilizing exogenous variation in risk aversion would be an ideal way to estimate heterogeneity in crow-out by risk aversion. However, it is difficult to determine the factors that produce exogenous variations. As a second-best approach to addressing this concern, I introduce interaction terms in the model and re-estimate the model as a robustness check.

In addition, the result shows that risk aversion levels fall with increasing household income. Both linear and quadratic terms are significantly negative. With respect to wealth, which is correlated with income, I also found a significant effect.[7]. However, if individuals' attitudes toward risk heavily depend on their income or wealth, then the estimation result would not yield a causal effect of risk-aversion but rather represent an income effect. To address this issue, I use the residualized risk aversion variable against total wealth and income, which excludes any income and wealth effect as a robustness check.

It is noteworthy that my measure of risk aversion has some limitations. First, even though the extent of the literature assumes constant risk aversion across time, there are studies showing that risk aversion might change due to financial or health shocks. If risk aversion varies across time, my measure of risk aversion is likely subject to measurement error due to imputation done under the assumption of largely stable risk aversion. Second, individuals' risk preferences might not

[7]These findings are consistent with previous studies that found that increased income reduces risk aversion (Wright 2017; Binswanger 1980, 1981; Donkers et al. 2001; Hartog et al. 2002)

translate perfectly across domains. In other words, individuals might have different risk aversion levels depending on whether he or she is faced with a financial decision compared with risk aversion in the domain of his own health insurance related choices, her own health or other domains (Barseghyan et al. 2011; Einav et al. 2012). Finally, the third is that the accuracy of self-reported survey measures depends on self-awareness and honest reporting, so it might not provide a perfect measure of risk aversion.

2.3.3 Empirical Methods

I use a difference-in-differences research design where non-expansion states provide a control group to states which expanded Medicaid. To estimate heterogeneity in risk aversion, I estimate the following regressions separately for risk-averse and risk-loving individuals:

$$(1) \quad Coverage_{ist} = \alpha_0 + \beta_1 Expansion_{st} + \beta_2 X_{it} + \alpha_a + \gamma_t + \delta_i + \varepsilon_{ist}$$

In this equation, the subscripts indicate individual i, state s, and year t. $Coverage_{ist}$ is indicators for Medicaid and private coverage. It equals one for Medicaid coverage if the individual had Medicaid coverage only (no overlap with private insurance) and zero in other cases. Similarly, it equals one for private coverage if the individual had private coverage only (no overlap with Medicaid) and zero in other cases. States' decision to adopt Medicaid expansion is expressed by a dummy variable, $Expansion_{st}$, which equals one if state s adopted the Medicaid expansion in or before year t. (δ) is individual fixed effects, (α_a) and (γ_t) are a full set of age and year fixed effects, respectively.

X_{it} is a vector of additional controls. These include the forms of dummy variables for marital status (married, divorced, and widowed) and measures of self-reported health status (excellent, very good, good, fair, poor), sick (which equals one if individuals have at least one of the following conditions: high blood pressure, heart disease, lung disease, diabetes, or psychiatric conditions, and zero if individuals do not have any of these conditions).Additional control variables are household income, total wealth, and their squares. Monetary variables are inflated to 2018 prices by a consumer price index. All standard errors are calculated using the bootstrap method with two-way clustering at the household and state level with 200 repetitions.[8]

A necessary condition for identifying casual effects rests on the assumption that pre-intervention trends in outcomes are parallel between the treated and the comparison groups. I test for the presence of pre-treatment trend differences between the treatment and control states.

Panel A of Figure 2.1 shows the full set of event study estimates for private coverage. The visual evidence in panel A of Figure 2.1 supports my identification strategy: there is no evidence of differential trends in private coverage for both risk-loving and risk-averse individuals. Similarly, Panel B of Figure 2.1 shows an event study analysis for Medicaid coverage. Reviewing the estimates, I find that pre-period coefficients are not statistically significant individually for risk-averse individuals. However, the coefficients for the relative years -12, -10, and -8 are statistically significant for risk-loving individuals, but the magnitude of coefficients is close to zero and in the opposite direction of the treatment effects.

Recent literature has shown that in an empirical setting such as mine, in which states are adopting the expansion at different points in time, a problem can emerge regarding heterogeneous treatment effects across time or groups (De Chaisemartin and d'Haultfoeuille 2020; Sun and Abraham 2021; Goodman-Bacon 2021; Callaway and Sant'Anna 2021). Specifically, it has been shown that in this setting, the two-way fixed effect estimator represents a weighted sum of the average treatment effects (ATE) in each group and period, and there is the potential for the weights to be negative. Due to negative weights, the two-way fixed effect (TWFE) estimator could be biased when the treatment effect is sufficiently heterogeneous across time or groups. Thus, when the number and size of the negative weights attached to a regression increase, the risk of obtaining a biased estimate also increases under heterogeneous treatment effects. To mitigate the concern, I

[8]Having a small sample of clusters can lead to over-rejecting the null hypothesis and producing standard errors that are too small (Cameron and Miller 2015; Donald and Lang 2007; McCaffrey and Bell 2006). To address this issue in my own study, where there are 51 state clusters, I apply block-bootstrapped standard errors by household and state groups based on 200 replications.

test for the prevalence and significance of negative weights within my regression, as proposed by de Chaisemartin and d'Haultfoeuille (2020). I find that the average treatment effect on the treated (ATT) is the weighted sum of 276 and 399 estimated average treatment effects for the risk-averse and risk-loving samples, respectively. Of those, 274 of 276 and 398 of 399 estimates receive a positive weight. Additionally, the sum of negative weights is around -0.0003 representing a very little contribution to the overall ATT estimate, as the total of all weights sums to one. Thus, it is likely the case that my estimator is robust in light of potential treatment effect heterogeneity.

2.4 Results

Table 2.4 shows the results of estimating heterogeneous Medicaid expansion effects by risk aversion on Medicaid take-up (columns 1-4) and private health insurance (columns 5-8). Across the first four columns, there is clear evidence that Medicaid expansion does not lead to any change in Medicaid coverage among risk-averse individuals. In contrast, the expansion increases Medicaid coverage by 3.4 percentage points for risk-loving individuals. Similarly, there is no evidence of expansion crowd-out among risk-averse individuals while it leads to a decrease in private coverage by 5.2 percentage points for risk-loving individuals.[9]

These findings suggest that risk-averse individuals are not willing to drop their private health insurance in favor of Medicaid coverage. There are several possible explanations for a lower crowd-out rate with an increase in the degree of risk aversion. First, individuals demand insurance mainly to get protection against uncertainty, so the insurance demand reflects the demand for certainty. This implies that the more risk-averse individuals are, the more coverage they will buy. Thus the effect of Medicaid expansion is expected to be different for risk-averse individuals relative to risk-loving ones. Risk-averse individuals are likely to be fully or completely insured, so they might not be willing to take the risk of switching from their private coverage to Medicaid. However, risk-loving individuals consider Medicaid as a better substitute for their possibly limited

[9] It is important to note that in practice, there are several conceptions of crowd-out and several ways to measure it. In this study, I interpret the coefficient on DID ($Expansion_{st}$) in the private health insurance equation as a measure of crowd-out.

coverage. Second, considering that physicians are likely to decrease the quantity or intensity of services supplied to Medicaid patients due to the low Medicaid reimbursement rate (Gruber et al. 1999; McGuire and Pauly 1991), risk-averse individuals might worry about switching from their private coverage to Medicaid even though there is little or no cost-sharing with Medicaid. Second, Medicaid coverage might not cover the health care providers individuals currently visit, which can discourage risk-averse individuals from enrolling in Medicaid.

A potential alternative explanation, however, may stem from the fact that most private health insurance is provided through employment. Therefore, the availability of Medicaid might lead some to quit a job that was providing them with highly valued health insurance. The choice to quit a job that you were holding merely for insurance coverage might vary by level of risk aversion. Risk averse individuals might be reluctant to quit their job, while risk-loving individuals might leave their job in search of a better job, being self-employed when there is available public health insurance. However, I did not find significant evidence that Medicaid expansion leads to job switches (see Appendix Table A2.1), which is consistent with the Gooptu et al. (2016) finding. Therefore, no evidence of crowd-out of private insurance with the introduction of the ACA Medicaid expansion among the risk-averse might be explained by the uncertainty associated with the quality of Medicaid coverage.

Another alternative explanation that I rule out is the concurrent role of insurance exchanges. State health insurance exchanges and Medicaid expansion were both passed under the ACA. Health insurance exchanges provide premium tax credits to eligible people to help them purchase coverage through the market places. Income requirement for premium tax credits eligibility ranges from 138% to 400% of FPL in states that expanded Medicaid (people with incomes below 138% of FPL are eligible for Medicaid in expansion states; therefore, they can not receive premium tax credits), while tax credit eligibility ranges from 100% to 400% of FPL in non-expansion states. Lower-income individuals (in the 100–138% income range) in non-expansion states are able to receive premium subsidies as a fallback option to Medicaid, which might increase private coverage. Therefore, a significant increase in private coverage in non-expansion states might appear as a form of crowd-out when exploiting variation in Medicaid expansion by using a simple binary Medicaid expansion vs. non-expansion independent variable (Frean et al. 2017).

To assess this concern, I analyze the change in private coverage in non-expansion states before and after the health insurance exchange. There is no increase in private coverage in non-expansion states; in fact, there is a significant decrease in private coverage (see Appendix Table A2.2). A decrease in private health insurance in non-expansion states might be explained by the wood-work effect. Individuals who were already eligible for coverage but had previously not enrolled can choose to transfer from their existing private insurance to Medicaid in non-expansion states.

2.4.1 Controlling Other Dimensions of Heterogeneity

In the main specification, to examine the heterogeneity in the Medicaid expansion effects by risk-aversion, I split the sample and estimate the treatment effects separately for risk-averse and risk-loving individuals. While the method provides valuable insight, the inability to control other dimensions of heterogeneity might confound the result. To address this concern, I augment equation 1 by adding the interaction of the difference-in-differences (Expansion) with the following individual's characteristics: a dummy for being sick, being female, being single, household income, and total wealth as independent variables.

Table 2.5 presents the results of the estimation. Medicaid expansion results in an 10 percentage points decrease in private health insurance among risk-loving individuals; however, there is no significant change in private coverage for risk-averse individuals. Similarly, the expansion leads to an increase in Medicaid coverage by 11 percentage points for risk-loving individuals, while there is no change for risk-averse individuals. This suggests that the estimated effect of risk aversion on crowd-out is driven by risk-aversion itself rather than its correlation with other individuals' characteristics.

2.4.2 Residualized Risk Aversion Variable Against Household Total Wealth and Income

The simple descriptive analysis I performed suggests that risk aversion is decreasing with respect to income (see Table 2.3). The existing literature also provides evidence that income does influence individuals' risk preference, but to what extent is still very much disputed. If risk preference is primarily driven by income, the results would not necessarily represent a causal effect of risk-aversion because Medicaid eligibility was based on household income. To alleviate this possible concern, I regress the risk aversion variable against household income, total wealth, and their squares, then estimate the residual, which excludes any income and wealth effect. I assign individuals as highly risk-averse if their estimated residual risk aversion is higher than the average. Similarly, individuals are assigned as risk-loving if their estimated residual risk aversion is equal to or below the average.

Table 2.6 illustrates the results of the estimation. The estimated heterogeneity of crowd-out is similar to the results in the main finding. Medicaid expansion crowds-outs private health insurance by 5 percentage points for risk-loving individuals; however, there is no crowd-out effect for risk-averse individuals. Likewise, the expansion increases Medicaid coverage by 3.4 for risk-loving individuals but no change for risk-averse individuals.

2.4.3 Exclude the States with Prior Full Expansion for Childless Individuals

I re-estimate equation (1) by excluding the states with full expansion for childless individuals before the year 2014 (CT, DE, MN, NY, VT, DC). Therefore, the treatment group is now only composed of states with no prior expansion for childless adults and those with prior limited expansion for childless adults. Table 2.7 presents the results of the estimation, which are consistent with the main finding.

2.5 Conclusion

The ACA included a major increase in public health insurance eligibility through Medicaid among people who are already covered with private plans. This raises the concern that Medicaid may be crowding out these existing sources of health insurance, which would suggest some of the spendings on Medicaid is ineffective at achieving the stated goal of increased coverage. A large body of literature has explored potential crowd-out from early Medicaid expansions, but their findings are very mixed. Recent studies take the ACA Medicaid expansion as an opportunity to estimate the crowd-out rate. Some of these studies detected no crowd-out rate (Courtemanche et al. 2017; Frean et al. 2017), while others found a moderate crowd-out rate (Wehby and Lyu 2018; Kaestner et al. 2017; Sommers et al. 2014).

In this paper, I estimate heterogeneity in crowd-out of private health insurance by risk-aversion. I find that ACA Medicaid expansion leads to a decline in private health insurance by 5 percentage points for risk-loving individuals but detects no crowd-out for risk-averse individuals. No crowd-out of private health insurance among highly risk-averse individuals implied by my estimates is in line with the findings of Wettstein (2016). He estimated heterogeneity in the Medicare Part D drug program by risk aversion and found that an increase of one standard deviation in risk aversion was associated with almost 5 percentage points less crowd-out, over a base crowd-out rate of 50%-60%.

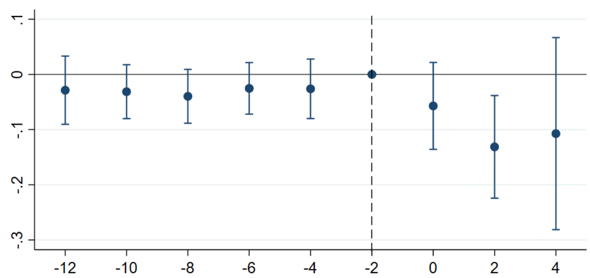
This substantially differential crowd-out of private health insurance by risk aversion has several policy implications. First, it suggests that highly risk-averse individuals are willing to keep their private insurance even though they become eligible for Medicaid, which might be a sign of sorting into private coverage. If highly risk-averse individuals are healthier, this sorting suggests advantageous selection into private coverage. In addition, high willingness to switch from private insurance to Medicaid among low risk-averse individuals might change the risk pool of Medicaid coverage so does total spending. Finally, it provides valuable information for identifying the need for corrective interventions or anti-crowd-out provisions.

Note, however, that there is one limitation to my analysis. I use an individual-level model when analyzing crowd-out due to data limitations.[10] However, individual level models do not incorporate possible family spillover effects. Therefore, the crowd-out rate might be higher when the entire family's eligibility is considered. Gruber and Simon (2008) emphasized the importance of family-level analysis. They used family eligibility measures and found that crowd-out estimates were much larger when family members' eligibility is considered.

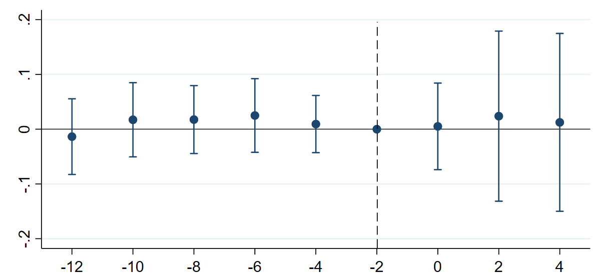
[10]The HRS data does not follow up with younger respondents, so information for younger members of the family is not available across the wave, which limits my ability to construct household-level model.

Figure 2.1: Event Study Plot of Medicaid Expansion on Coverage Categories

Panel A. Private Coverage

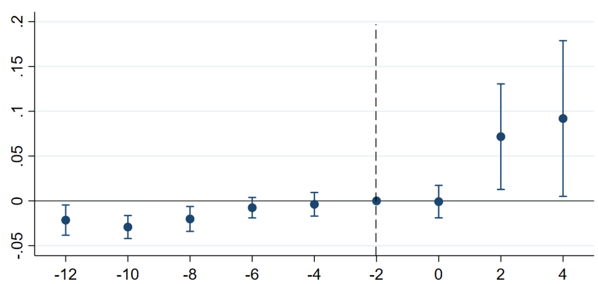


Risk Lover

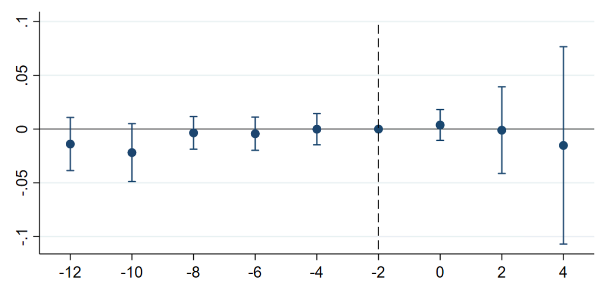


Risk Averse

Panel B. Medicaid Coverage



Risk Lover



Risk Averse

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Table 2.1: Classification of states into treatment and control groups

Treatment States (expansion on January 1, 2014 or later)

Prior full expansion for childless adults	No prior expansion		Prior limited expansion for childless adults	
Connecticut	Alaska*	Nevada	California	Michigan*
Delaware	Arizona†	New Hampshire*	Hawaii	New Jersey
Minnesota	Arkansas	North Dakota	Illinois	New Mexico
New York	Colorado†	Ohio	Indiana*	Oregon
Vermont	Kentucky	Pennsylvania*	Iowa	Rhode Island
Washington, DC	Louisiana*	West Virginia	Maryland	Washington
	Montana*		Massachusetts	
n=6	*n=13*		*n=13*	

Control States (as December 2018)

No prior expansion			Prior limited expansion for parents and/or childless adults
Alabama	Mississippi	South Dakota	Maine
Florida	Missouri	Virginia	Oklahoma
Georgia	Nebraska	Texas	Tennessee
Idaho	North Carolina	Wyoming	Utah
Kansas	South Carolina		Wisconsin⋆
	n=14		*n=5*

* Expansions taking place after January 1, 2014 include: Michigan (April 1, 2014), New Hampshire (August 15, 2014), Pennsylvania (January 1, 2015), Indiana (February 1, 2015), Alaska (September 1, 2015), Montana (January 1, 2016), and Louisiana (July 1, 2016)
† In 2000, Arizona expanded Medicaid coverage for childless adults below 100% FPL. However, on July 8, 2011, Arizona decided to freeze enrollment. Colorado provided Medicaid coverage to jobless childless adults below 10% FPL in May 2012, but it capped enrollment to 10,000 adults.
⋆ Although Wisconsin has not expanded Medicaid under the ACA, in 2014, Wisconsin extended its Medicaid program (which is called BadgerCare) to all individuals with income up to 100% FPL (without enrollment cap). Therefore, I exclude Wisconsin.
Source: `https://www.healthinsurance.org/medicaid/wisconsin/`
Note: If states adopted the ACA's Medicaid expansion after July 1, I assigned expansion time the following calender year.

Table 2.2: Descriptive Statistics for Sample Years 2000-2018

	Low-educated, childless individuals	N	Low-educated, childless working individuals	N
Risk Averse	0.48	22,216	0.46	12,035
	(0.49)		(0.5)	
Risk Lover	0.52	22,216	0.54	12,035
	(0.49)		(0.5)	
Medicaid coverage only	0.133	41,122	0.04	22,826
	(0.34)		(0.19)	
Private health insurance only	0.58	41,247	0.74	22,828
	(0.49)		(0.44)	
Age	57.3	41,513	56.6	22,919
	(5.09)		(5.02)	
Women	0.60	41,513	0.56	22,919
	(0.49)		(0.5)	
Married	0.59	41,513	0.64	22,919
	(0.49)		(0.48)	
Divorced	0.18	41,470	0.16	22,901
	(0.38)		(0.37)	
Widowed	0.07	41,470	0.05	22,901
	(0.25)		(0.22)	
Annual Labor Earnings per 10.000	2.15	41,513	3.55	22,219
	(3.5)		(4.02)	
Total Wealth per 10.000	26.7	41,513	29.2	22,919
	(84.9)		(9.96)	

Standard deviations are in parentheses.

Table 2.3: Regression Analysis of Risk Aversion

Dependent Variable: Risk Aversion	(1)
Female	0.05 ***
	(0.016)
Black	-0.017
	(0.036)
White	-0.05 *
	(0.03)
Married	0.004
	(0.02)
Divorced	-0.056**
	(0.029)
Widowed	-.0.05
	(0.035)
Age	0.008
	(0.016)
Age^2	-0.000034
	(0.001)
Household Income *	-0.006 ***
	(0.001)
$(\text{Household Income})^2$	0.00003***
	(7.56e-06)
Total Wealth *	-0.003**
	(0.001)
$(\text{Total Wealth})^2$	1.47e-07*
	(7.82e-08)
Sick ⋆	-0.01
	(0.015)
Intercept	0.19
	(0.46)
R-square	(0.0176)
Observations	12,000

Notes: This table presents the estimates of the OLS regression of risk aversion indicator against individuals' demographic characteristics. The sample is restricted to low-educated, childless, working individuals aged below 65 years in the 2000-2018. All standard errors are clustered across individuals.

* Total household income and wealth are scaled by diving 10,000.

⋆ Sick is a binary variable that equals one if individuals have at least one of the following conditions: high blood pressure, heart disease, lung disease, diabetes, or psychiatric conditions, and zero if individuals do not have any of these conditions.

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

Table 2.4: Heterogeneity in Crowd-Out by Risk Aversion

Dependent Variable:	Medicaid only				Private only			
	Risk Averse		Risk Lover		Risk Averse		Risk Lover	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Expansion	0.004	0.005	0.033**	0.034**	0.007	0.0015	-0.05*	-0.052 **
	(0.009)	(0.011)	(0.016)	(0.015)	(0.04)	(0.035)	(0.028)	(0.023)
Individual, age and year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Individual controls	No	Yes	No	Yes	No	Yes	No	Yes
Observations	4,620	4,603	5,579	5,565	4,617	4,600	5,580	5,566

Notes: This table presents estimates of the effects of Medicaid expansion on Medicaid coverage and private health insurance for risk-averse and risk-loving individuals. The sample is restricted to low-educated, childless, working individuals aged below 65 years in the 2010-2018. Individual controls include dummies for marital status (married, divorced, and widowed), self-reported health on a scale of 1-5 from poor to excellent, household income, total wealth and their squares, and a sick dummy (which equals one if individuals have at least one of the following conditions: high blood pressure, heart disease, diabetes, or psychiatric conditions, and zero if individuals do not have any of these conditions). All standard errors are calculated using the bootstrap method with two-way clustering at household and state level with 200 repetitions.

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

Table 2.5: Robustness Check - Controlling Other Dimension of Heterogeneity

Dependent Variable:	Medicaid only		Private only	
	Risk Averse	Risk Lover	Risk Averse	Risk Lover
Expansion	0.023	0.11 ***	0.021	-0.10 **
	(0.023)	(0.04)	(0.05)	(0.046)
Individual, age and year fixed effects	Yes	Yes	Yes	Yes
Individual controls	Yes	Yes	Yes	Yes
Expansion × demographic controls	Yes	Yes	Yes	Yes
Observations	4,603	5,565	4,600	5,566

Notes: This table presents estimates of the effects of Medicaid expansion on Medicaid coverage and private health insurance for risk-averse and risk-loving individuals. The sample is restricted to low-educated, childless, working individuals aged below 65 years in the 2010-2018. Individual controls include dummies for marital status (married, divorced, and widowed), self-reported health on a scale of 1-5 from poor to excellent, household income, total wealth and their squares, and a sick dummy (which equals one if individuals have at least one of the following conditions: high blood pressure, heart disease, diabetes, or psychiatric conditions, and zero if individuals do not have any of these conditions). Demographic controls are a dummy for being female, being single, being sick, household income, and total wealth. All standard errors are calculated using the bootstrap method with two-way clustering at household and state level with 200 repetitions.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 2.6: Robustness Check - Residualized Risk Aversion

Dependent Variable:	Medicaid only				Private only			
	Risk Averse		Risk Lover		Risk Averse		Risk Lover	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Expansion	0.004	0.005	0.033**	0.034**	0.007	0.0015	-0.05*	-0.52 *
	(0.007)	(0.007)	(0.016)	(0.014)	(0.037)	(0.044)	(0.03)	(0.027)
Individual, age and year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Individual controls	No	Yes	No	Yes	No	Yes	No	Yes
Observations	4,620	4,603	5,579	5,565	4,619	4,602	5,580	5,566

Notes: This table presents estimates of the effects of Medicaid expansion on Medicaid coverage and private health insurance for risk-averse and risk-loving individuals. The sample is restricted to low-educated, childless, working individuals aged below 65 years in the 2000-2018. Individual controls include dummies for marital status (married, divorced, and widowed), self-reported health on a scale of 1-5 from poor to excellent, household income, total wealth and their squares, and a sick dummy (which equals one if individuals have at least one of the following conditions: high blood pressure, heart disease, diabetes, or psychiatric conditions, and zero if individuals do not have any of these conditions). All standard errors are calculated using the bootstrap method with two-way clustering at household and state level with 200 repetitions.

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

Table 2.7: Robustness Check-Exclude States With Prior Full Expansion

Dependent Variable:	Medicaid only				Private only			
	Risk Averse		Risk Lover		Risk Averse		Risk Lover	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Expansion	0.007	0.008	0.025*	0.025**	0.001	-0.002	-0.047*	-0.047*
	(0.01)	(0.008)	(0.015)	(0.012)	(0.03)	(0.022)	(0.025)	(0.027)
Individual, age and year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Individual controls	No	Yes	No	Yes	No	Yes	No	Yes
Observations	4,316	4,300	5,227	5,214	4,313	4,297	5,228	5,215

Notes: This table presents estimates of the effects of Medicaid expansion on Medicaid coverage and private health insurance for risk-averse and risk-lover individuals. The sample is restricted to low-educated, childless, working individuals aged below 65 years in the 2000-2018. Individual controls include dummies for marital status (married, divorced, and widowed), self-reported health on a scale of 1-5 from poor to excellent, household income, total wealth and their squares, and a sick dummy (which equals one if individuals have at least one of the following conditions: high blood pressure, heart disease, diabetes, or psychiatric conditions, and zero if individuals do not have any of these conditions). All standard errors are calculated using the bootstrap method with two-way clustering at household and state level with 200 repetitions.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

APPENDIX TO CHAPTER 2

Table A2.1: Difference-in-Difference Estimates of Medicaid Expansion on Job Switches

Dependent Variable: Job Switch	(1)	(2)
Expansion	0.0016	0.0006
	(0.01)	(0.013)
Individual, age and year fixed effects	Yes	Yes
Individual Controls	No	Yes
Observations	19,938	19,909

Notes: This table presents estimates of the effect of Medicaid expansion on job switching. The sample is restricted to low-educated, childless, working individuals aged below 65 years in the 2000-2018. Individual controls include dummies for marital status (married, divorced, and widowed), self-reported health on a scale of 1-5 from poor to excellent, household income, total wealth and their squares. All standard errors are calculated using the bootstrap method with two-way clustering at household and state level with 200 repetitions.

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

Table A2.2: Difference-in-Difference Estimates of Health Insurance Exchange on Private Coverage

Group/Year	Before 2014	After 2014	Difference
Expansion States	0.79 ***	0.69 ***	-0.10 ***
	(0.005)	(0.007)	(0.008)
Non-Expansion States	0.73 ***	0.69***	-0.04 ***
	(0.006)	(0.008)	(0.008)
Difference-in-Difference			-0.06 ***
			(0.01)
Number of Observations			20,936

Notes: Expansion states include 25 states that expanded Medicaid in 2014 under the ACA, while non-expansion states include 18 states that have not adopted the expansion except Wisconsin (see Table 1 for details).The sample is restricted to low-educated, childless, working individuals aged below 65 years in the 2000-2018.

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

3. How Changes in Lifestyle During Retirement Affect Mental and Physical Health

3.1 Introduction

Retirement means relief from occupational strain and having more leisure time. Retirees might use their abundant free time by investing in health (e.g., exercising more, having healthy habits, and maintaining social contention). At the same time, retirement is often associated with a decrease in social interactions, a reduction in daily activities, and a loss of purpose, which all may have negative impacts on physical and mental health. Therefore, the overall effect on health is ambiguous.

The health capital model by Grossman (1972) emphasizes that individuals invest in health (e.g., exercise, diet) for consumption benefits (people feel better and enjoy all consumption more) as well as production benefits (people with better health can invest more and better in the labor market that good health provides, consequently raising their money earnings). The incentive to invest in health for production purposes disappears upon permanent withdrawal from the labor force. However, since health enters individuals' preference functions as a consumption commodity, retirees may still invest in their health during post-retirement. The effect of retirement on health status, therefore, depends on the marginal benefits and costs of health capital. In the presence of this theoretical ambiguity, the effect of retirement on health status remains an empirical question.

Uncovering the causal relationship between retirement and health is complicated. Because retirement and health are jointly determined. That is, poor health may bring about retirement. The literature has employed various empirical methods, such as instrumental variables or regression discontinuity techniques, to deal with this issue. This strand of literature focuses on physical health measured by both subjective (e.g., self-assessed health) and objective (diagnoses of specific diseases, physical limitation) indicators, mental health, and cognitive functioning. However, there is no consensus among empirical results. Several studies estimated that retirement leads to improvement in physical or mental health measures (e.g., Charles 2004; Johnston and Lee 2009; Neuman 2008; Coe and Lindeboom 2008; Coe and Zamarro 2011; Garrouste et al. 2012; Grip et al. 2012; Latif 2013) while other studies reported negative effects on health measures (e.g., Behncke 2012; Heller-Sahlgren 2012; Dave et al. 2008). Similarly, Rohwedder and Willis (2010), Bonsang et al (2012), and Bingley and Martinello (2013) investigated the relationship between retirement and cognitive functioning. They found that retirement leads to a decrease in cognitive functions.

The aforementioned papers focused on estimating the overall effect of retirement on health and provided theoretical arguments on why retirement might affect health. However, knowing the mechanisms behind the causal effect of retirement is important for the individual as well as the government or pension funds' budgets and health care expenditures. There are studies focusing on changes in lifestyle upon retirement to shed light on the mechanisms that potentially drive the health effect of retirement. For example, Insler (2014) showed that retirement is associated with more exercise and less smoking; Eibich (2015) found a decrease in cigarette consumption; Zins et al. (2011) found significant changes in alcohol consumption at retirement; Barnett et al. (2012), Kampfen, Maurer (2016), Celidoni (2017), Zhu (2016), and Celidoni and Rebba (2017) reported a positive association between physical exercise and retirement; Godard (2016) showed changes in body mass index, and, consequently, Atalay et al. (2019) found that moving into retirement leads women to increase the time spent in mental and household activities. However, these results provide only suggestive evidence and rely on the assumption that the change in health behaviors or time use upon retirement would increase or deteriorate individuals' health status. A formal mediation analysis is required to measure the magnitude of the potential mechanism that drives the health effect of retirement.

In contrast to previous studies, Delugas and Balia (2019) provided a formal mediation analysis. They investigated the causal mechanism through which retirement operates on individuals' health. They found that the overall effect of retirement is detrimental to general mental health and cognitive functioning. The total retirement effect runs through lifestyle channels, exacerbating the detrimental long-term effect of retirement. It is important to note that Delugas and Balia used crossnational data[1] and exploited cross-country variations in the eligibility age for retirement benefits as instruments. However, individuals in different countries face different norms, labor markets, and economic incentives embedded in their pension systems. This heterogeneity is likely to influence individuals' psychical and mental health status and to be systematically correlated with differences in eligibility ages for retirement benefits, which invalidate the exclusion restrictions and result in bias estimation.

In this paper, I estimate the causal impact of retirement on health measures and investigate potential mechanisms driving the health effects of retirement. I decompose the retirement effect into two parts: (i) the part mediated by observable behaviors which I measure with change in heavy drinking, exercise, and smoking; (ii) a residual, which includes, for instance, relief from occupational strain, and loss of purpose. I use data from the Health and Retirement Study (HRS), a longitudinal survey among individuals aged 50+ living in the US. To address the endogeneity of retirement on individual health status or health-related behaviors, I employ the eligibility age for social security as an instrument. The panel dimension of the data allows me to control unobservable time-invariant heterogeneity. Besides, unlike previous studies focusing on cross-country data, my analysis uses data from a single country with individuals facing the same institutional settings and constraints.

3.2 Data and Empirical Strategy

3.2.1 Data

I use fourteen waves (1992-2018) from Health and Retirement Study (HRS). The data include a wide range of information about individuals' mental and physical health, employment status, financial situation, insurance status, and health behaviors.

I restricted the sample to respondents aged between 50 and 75. In the United States, many individuals rely on their employers for health insurance coverage, but upon retirement, most of these

[1] They use data of the Survey of Health, Aging and Retirement in Europe (SHARE).

individuals lose this coverage. While individuals over the age of 65 become eligible for Medicare, and some individuals may become eligible for Medicaid before reaching Medicare eligibility age, it is important to note that a significant number of individuals do not become eligible for Medicaid before reaching this age. Therefore, the retirement effect on health might actually be driven by the change in health insurance status.[2] In order to isolate the retirement effect, I focus on individuals who report having health insurance (private or public insurance) in all waves.

In addition, individuals who re-entered work after retiring were dropped because those individuals are likely to stay active in the labor market (e.g., looking for a job) during their non-working time. Moreover, individuals who never worked and those who reported having left their last job before the age of 50 were excluded.

The Retirement Variable

There are various definitions of retirement. In this study, I follow Lazear (1986), which defines individuals as being retired if he/she is out of the labor force with the intention of remaining out permanently. Therefore, an individual is defined as ”Retired” if she/he reports not working for pay and ”Working” if she/she claims to be currently working for pay. HRS includes information about the year and the month the individual's last job ended. I use this information to define the individual's retirement duration. Then I create a dummy variable, ”retired for at least one year,” which equals 1 if the individual has been retired for at least one year and 0 otherwise.

Health Measures

I use the eight-item Center for Epidemiologic Studies Depression (CESD) score, which is a measure of mental health, and indicators for self-rated physical health as health measures.

In the HRS, respondent was asked the following questions with response options of 'yes' or 'no': 1) Much of the time during the past week, I felt depressed; 2)I felt everything I did was an effort; 3) my sleep was restless; 4) I was happy; 5) I felt lonely; 6) I enjoyed life; 7) I felt sad; 8) I

[2]The RAND experiment, which provides ”gold standard” evidence of the impacts of health insurance, found that more generous health insurance translated into improved health for individuals with poor vision or high blood pressure.

could not get going. The CESD score is the sum of respondents' answers to these eight questions. Therefore, the higher the score, the more negative the respondents' feelings were in the past week.

For self-reported health status, respondents are asked how they would describe their current health status on a scale of 1-5 from poor to excellent. For this analysis, I create a "satisfactory health" dummy variable, which equals 1 if individuals' reported health status belongs to one of the best three categories (excellent, very good, good) and zero for the worst two categories (fair, poor).

Health Behavior

The HRS provides various measures of health behaviors. In this analysis, I use data on exercise, alcohol consumption, and smoking as measures of health behaviors.

The HRS asks how many drinks the individual drinks in a day on which the individual drinks (set to 0 when the individual reports are never drinking). I define heavy drinking as 1 if individuals report consuming 5 or more drinks for males and 4 or more for females.

Exercise is a dummy variable that equals 1 if individuals exercise at least twice a week and 0 otherwise. Smoking habit is defined by binary variables that take 1 if the individual is a current smoker and 0 otherwise.

Table 3.1 illustrate descriptive statistics for each outcome. The mean values for satisfactory health and mental health are similar for both men and women. Additionally, the proportion of individuals who engage in exercise and smoke is nearly the same for both genders, with 79% of females and 75% of males reporting that they engage in exercise, while 15% of females and 16% of males report that they smoke However, there is a noticeable gender difference when it comes to heavy drinking, as only 1.3% of females reported engaging in this habit, while the corresponding figure for males was much higher at 4%.

3.2.2 Empirical Strategy

In this section, I discuss an identification strategy that aims to estimate the effect of retirement on health measures and the share of this effect that can be attributed to the change in lifestyle. Such an exercise to unpack mechanism is called ”mediation analysis”- where retirement and lifestyle, i.e., the *mediator*, jointly cause health measures. The following figure illustrates the causal chain :

$$\begin{tikzcd} Retirement \arrow[r] \arrow[rr, bend right] & Lifestyle \arrow[r] & Health \end{tikzcd}$$

The main issue here is that retirement and lifestyle are not exogenous in health determination. Therefore, it is not possible to estimate the relationship of the causal path by means of OLS. IV estimators might be applied to conduct casual path analysis, but overcoming the under-identification problem might be challenging because two instrumental variables are required – one for retirement status and the other for lifestyle. In this study, I follow the approach of Tubeuf et al. (2012). I first estimate the effect of retirement on lifestyle by using the eligibility age for social security as an instrument. Then, I introduce the estimated residual of the lifestyle equation - which excludes any retirement effect - into the health equation. In this equation, I use the eligibility age for social security as an instrument for retirement status. The econometric model to estimate to total retirement effect and its fraction that is explained by lifestyle as follows:

$$(1) \quad H_{it} = \beta R_{it} + f(age_{it}) + \mu_i + \mu_t + \varepsilon_{it}$$

$$(2) \quad L_{it} = \lambda R_{it} + f(age_{it}) + \mu_i + \mu_t + \upsilon_{it}$$

$$(3) \quad H_{it} = \alpha R_{it} + \gamma \hat{\upsilon} + f(age_{it}) + \mu_i + \mu_t + u_{it}$$

In these equations, the subscripts indicate individual i, and year t. H_{it} is the vector of health measures which are mental health score and satisfactory health dummy. L_{it} is the vector of health-related behavior, namely heavy drinking, exercise, and smoking. R_{it} is a dummy variable for retirement status, and $f(age_{it})$ is a smooth function of age: age and age squared to capture any non-linear effects of age on health and health-related behavior. All specifications further include year (μ_t) and individual (μ_i) fixed effects.

In equation (1), β represents the overall effect of retirement on health. Similarly, in equation (2), λ represents the overall effect of retirement on lifestyle. In the health equation (3), $\hat{\upsilon}$ is the predicted error term of the lifestyle equation that no longer includes any retirement effect, and γ is the mediating coefficient. To compute the indirect effect of retirement λ from equation (2) and γ from equation (3) has to be multiplied. All standard errors are clustered at the individual level. [3]

To identify the causal effect of retirement on lifestyle and health measures, it is necessary for the error term to be independent of retirement, age, and unobserved time-invariant heterogeneity. However, this requirement is unlikely to be met due to several factors. Firstly, the aforementioned endogeneity of retirement because of reverse causality between retirement, health, and lifestyle, namely health behaviors. Secondly, there may be a correlation between retirement and unobserved heterogeneity. Applying fixed effects (FE) estimators control for time-invariant individual heterogeneity. To deal with the endogeneity of retirement, I apply IV methods.

Figure 3.1 illustrates the estimated retirement probability changes as individuals get one year older between the ages of 56 and 70. As shown in previous studies, retirement spikes at age 62 and again, though to a smaller degree, at age 65. Financial incentives induced by social security have been found to play a significant role in explaining such spikes (Burtless and Moffitt 1984; Peracchi and Welch 1994; Ruhm 1995; Gruber and Wise 2009; Coile and Gruber 2001). In this study, I utilize these key retirement ages as identifying instruments for the retirement decision. These specific age values are likely to have a direct effect on retirement decisions, but it is not expected to have a particular influence on mental and physical health or health behaviors.

Other than the endogeneity of retirement in health and lifestyle equations, identifying the causal effect of retirement involves another issue: the changes in lifestyles are not immediate and are likely to translate progressively into changes in mental health or general health status. Therefore, I

[3] To produce the standard errors of the mediating coefficients, I generate a bootstrap sample, compute λ and γ, and store the product value. I repeat the process again 1000 times. Then I simply use the sample standard deviation of 1000 $\lambda \times \gamma$ to approximate the standard error.

focus on individuals who being retired for at least one year. In such a case, the instruments should then become threshold dummies for reaching 63 years and the normal age of retirement plus one.

3.3 Results

In the first step, I estimate the overall effect of retirement on mental health and satisfactory health status by using the ordinary least squares (OLS) method with fixed effects. The results, which are presented in Table 3.2, suggest that retirement for one year or more is associated with an increase in depressive symptoms and a decrease in satisfactory health status for both males and females. However, as discussed earlier, individuals with mental health problems or low life satisfaction may choose to retire, which can lead to biased estimation results due to sample selection. To isolate the causal effect of retirement, I employ the two-stage least squares (2SLS) within estimator.

Table 3.3 presents the coefficients of the first-stage equation describing the probability of being retired (for at least one year). The instruments, i.e., the eligibility ages (plus one) for social security, have large and significant effects on the probability of being retired for at least one year for both females and males. At the bottom panel, I report the F test of the joint significance of the instruments and the p-value of the Sargan–Hansen test of overidentifying restriction. The F test results confirm that the instruments are significant predictors of retirement, and the Sargan–Hansen test of overidentifying restriction does not reject the hypothesis that my instruments are valid.

Table 3.4 illustrate the effect of being retired one year or more on mental health and satisfactory health for both females and males. The results show that retirement is associated with 1.32 fewer depressive symptoms for females (column 1). Compared to a pre-retired mean CESD score of 1.32, these changes represent a 100% decline in the count of depressive symptoms. Similarly, retirement results in 0.5 fewer depressive symptoms for males (column 2). This corresponds to about a 60% decrease in depressive symptoms compared to the average sample score (0.83). Moreover, I find that retirement led to a significant improvement in satisfactory health status for females, while there is no statistically significant effect for males.

To estimate the effect of retirement on health-related behaviors, I use the two-stage least squares (2SLS) within estimator. Table 3.5 displays the estimated effects of the three health-related behaviors, namely exercise, heavy drinking, and smoking.[4] For health-related behaviors, I find that retirement increases the probability of exercise by 33 and 17 percentage points for females and males, respectively. In addition, the probability of heavy drinking increase with retirement by 6 percentage points for males, but no significant evidence for females. Finally, with regard to the relationship between smoking and retirement, no significant effects are observed for females and males.

To unravel the effect of change in lifestyle upon retirement on health outcomes (indirect effect), λ from equation (2) and γ from equation (3) have to be multiplied. Table 3.6 reports the total effect and indirect effects of lifestyle. [5]

The decomposition shows that doing exercise has a significant positive effect on the mental and satisfactory health status of females and males. They explain 9.46% and 6.2% of the total retirement effect on the mental health status of females and males, respectively. Similarly, exercise account for 10.4% of the total retirement effect on the satisfactory health status of the female. However, the total effect of retirement is not significant for the satisfactory health status of the male.

The findings suggest that retirement increases the likelihood of engaging in exercise for both males and females, and an increase in the probability of doing exercise intensifies the beneficial effect of retirement on mental health for both genders. However, there is a potential bias arising from self-selection in exercise, as individuals with mobility impairments are less likely to engage in regular exercise, which could confound the estimation results. To alleviate this concern, I utilize the HRS mobility index variable, which is constructed by asking respondents whether they have difficulty performing the following physical functioning tasks: walking one block, walking several

[4]Table A3.1 in the Appendix shows the first-stage regression analysis.

[5]Table B3.1 in the Appendix illustrates the results from estimating the effect of retirement, conditional on changes in lifestyle, on mental and satisfactory health status (equation 3).

blocks, walking across a room, climbing one flight of stairs, and climbing several flights of stair activities. I then re-estimate the models by including a dummy variable I created by using the mobility index (1 if individuals do not have any difficulties performing the physical functioning tasks, 0 otherwise). The results are consistent with the main findings, confirming the positive effect of retirement on exercise and its subsequent positive impact on mental health. [6]

3.3.1 Controlling For Other Mediator Variable

In the main analysis, I have hypothesized that retirement often leads to lifestyle changes, which in turn may affect an individual's health during their retirement period. To investigate this relationship, I have focused on changes in exercise behavior, smoking, and heavy drinking, and their impact on health during retirement. However, there are other potential factors that may change with retirement and also have an effect on an individual's health. For instance, an individual's wealth stock is likely to change significantly after retirement, and this change may have a significant impact on their health. Similarly, the presence of a spouse may also influence an individual's retirement decision and may subsequently impact their health during retirement.

To further explore the relationship between retirement, lifestyle changes, and health outcomes, I have included additional mediator variables such as wealth stocks and presence of a spouse in my analysis. By re-estimating the main models with these additional mediator variables, I aim to gain a more complete understanding of the factors that affect an individual's health during their retirement years.

Table 3.7 displays the total and indirect effects of exercise, smoking, heavy-drinking, wealth stock, and the presence of a spouse on mental and satisfactory health status. The results indicate no significant effect of smoking, heavy-drinking, wealth stock and the presence of a spouse on mental and satisfactory health status for both females and males. Exercise account for 9.4% and 2.3% of the total effect retirement on mental health status for females and males, respectively. It is important to note that although the coefficient of the presence of a spouse and wealth stock are not

[6] For details, see Appendix Table C3.1

significant for males, the inclusion of wealth stock and the presence of a spouse as mediators in the model reduces the proportion of the total effect of retirement on male mental health that can be explained by exercise from 6.2% to 2.3%. Similarly, exercise explains 8.7% of the total retirement effect on the satisfactory health status of female,compared to 10.4% in the main model that did not include wealth stock and the presence of a spouse.

3.3.2 Heterogeneity Across Occupational Groups

The health effects of retirement and the mechanisms underlying them are likely to vary across different occupational groups. This is because the burden of job-related stress and physical demands varies significantly among different occupations, and so does the impact of retirement on individuals' health and well-being. For instance, individuals working in physically demanding jobs are likely to benefit differently from retirement than those working in office-based jobs, and their lifestyle changes after retirement may also differ significantly. Previous research has highlighted this potential source of heterogeneity and shown that retirement has an immediate beneficial effect on both mental and physical health and on cognitive abilities for individuals in more physically demanding jobs. However, for the rest of the workforce, retirement has negative long-run effects (Mazzonna and Peracchi 2017).

In this section, I examine whether the estimated effects on health and the mechanisms differ with respect to occupational groups. In order to analyze the heterogeneity of retirement behaviors across occupations, I divide respondents into two groups: those who are (or were) employed in blue-collar jobs and those who are (or were) employed in white-collar jobs. Equations (1), (2), and (3) are estimated for women in blue color jobs, women in white color jobs, men in blue color jobs, and men in white color jobs separately.

Heterogeneity for mental health is reported in figure 3.2. Retirement effect on mental health is larger for women in blue color jobs. Similarly, men in blue color jobs exhibit higher positive effects relative to men in white color jobs. Figure 3.3 illustrates heterogeneity for satisfactory health status. As in the estimation result for mental health status, the positive effect of retirement on satisfactory status is higher among those in blue color jobs.

Concerning health behavior, I do not find any significant effect apart from exercise (Figure 3.4-3.6). Figure 3.4 shows that retirement increase the probability of doing exercise for women in the blue color job, which might explain a stronger positive effect of retirement on mental health and satisfactory health status among women in blue color jobs.

3.3.3 The Sample Selection

In the main specification, I exclude individuals who observed returning to work during the sample period. Therefore, non working individuals are defined as retired in this study. However, if individuals did not return to the labor force because of physical or mental health problems that prevented them from finding a job, sample selection bias could arise. To test sensitivity my results, I keep individuals observed going back to work during the sample period and defined their non-working duration as retired. In such cases, the results are similar to the main findings : Retirement leads to increase in the probability of exercise for both female and male and these increases in exercise behavior explain a significant portion of the overall effect of retirement on mental health. Specifically, they account for 11% and 12.5% of the total retirement effect on the mental health status of females and males, respectively (For details, see Appendix Table D3.1).

3.4 Conclusion

This paper provides empirical evidence on the causal impact of retirement on health measures and explores the underlying mechanisms. Using longitudinal data on older Americans from 1992 to 2018 (HRS), I find that retirement has a positive effect on both physical and mental health for both females and males. In addition, I find that changes in lifestyle, specifically an increase in exercise, play an important role in intensifying the beneficial effect of retirement on mental health. The results are consistent with previous studies that have found a positive effect of retirement on mental or physical health,[7] as well as studies that have shown an association between retirement and an increase in physical activity. [8]

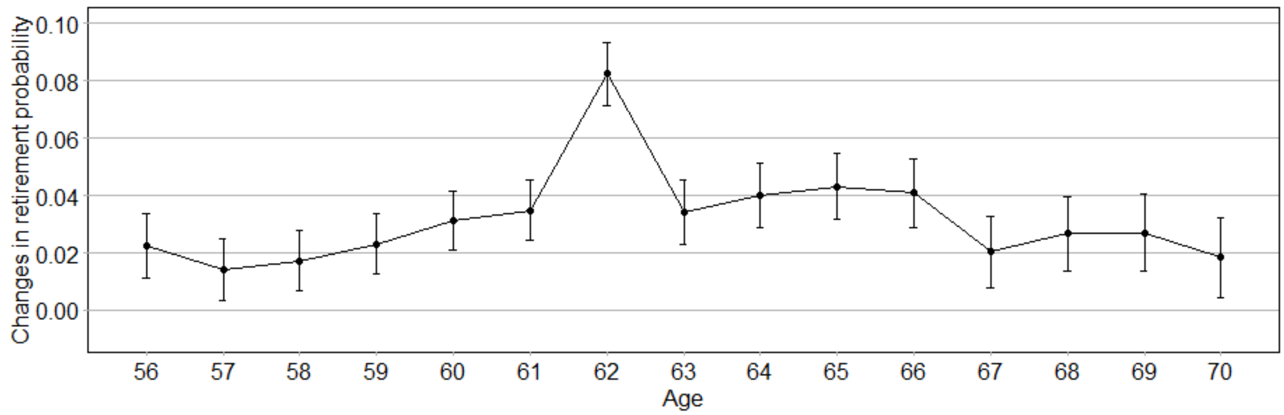
My findings have important implications that extend beyond the effects of retirement on mental health. They indicate that individuals can play a role in managing their mental health status through activities they engage in, such as exercise. Thus, there is scope for policy interventions to affect the state of mental health.

It is important to highlight that the healthcare costs of the elderly pose a significant burden on public health, especially with the current trend of population aging. Policies aimed at promoting labor force participation among older individuals could contribute to ensuring the sustainability of social security systems. However, my findings demonstrate that it is also important to consider the potential challenges to welfare that may arise from the positive effect of retirement on mental health.

[7](e.g., Charles 2004; Johnston and Lee 2009; Neuman 2008; Coe and Lindeboom 2008; Coe and Zamarro 2011; Garrouste et al. 2012; Grip et al. 2012; Latif 2013)

[8](e.g., Insler 2014; Barnett et al. 2012; Kämpfen and Maurer 2016; Celidoni and Rebba 2017; Zhu 2016; Celidoni and Rebba 2017)

Figure 3.1: Changes in retirement probability by age



Note: The sample includes all individuals aged between 55 and 70 in 1998-2018 HRS data. The figures show the coefficient estimates and the corresponding 95%-confidence interval from the following model: $Y_{it} = \alpha_i + \sum_{a=56}^{70} \gamma_a d_{it}^a + \varepsilon_{it}$, where y_{it} is the retirement dummy.

Figure 3.2: Mental Health by Occupation Groups

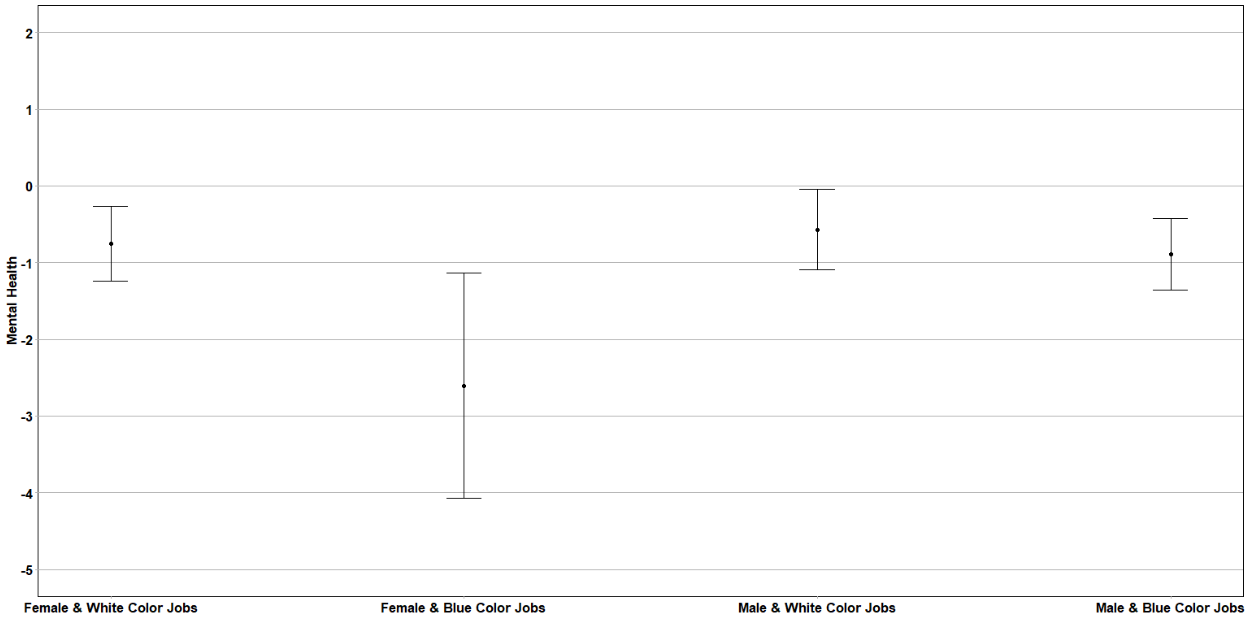


Figure 3.3: Satisfactory Health Status by Occupation Groups

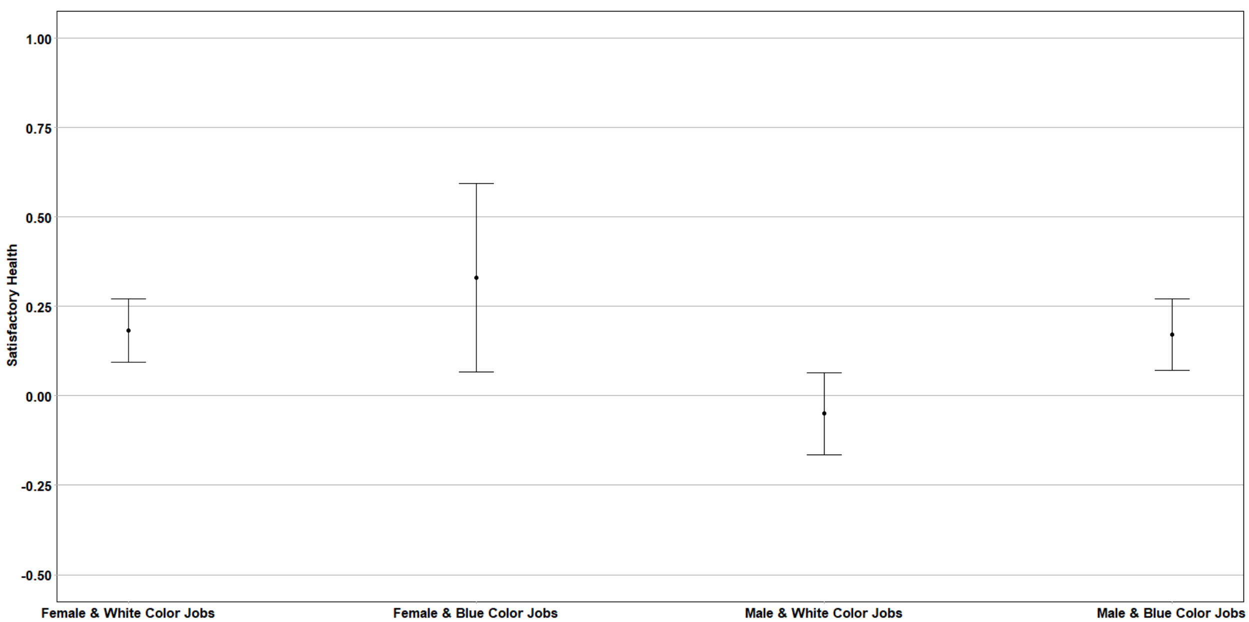


Figure 3.4: Exercise by Occupation Groups

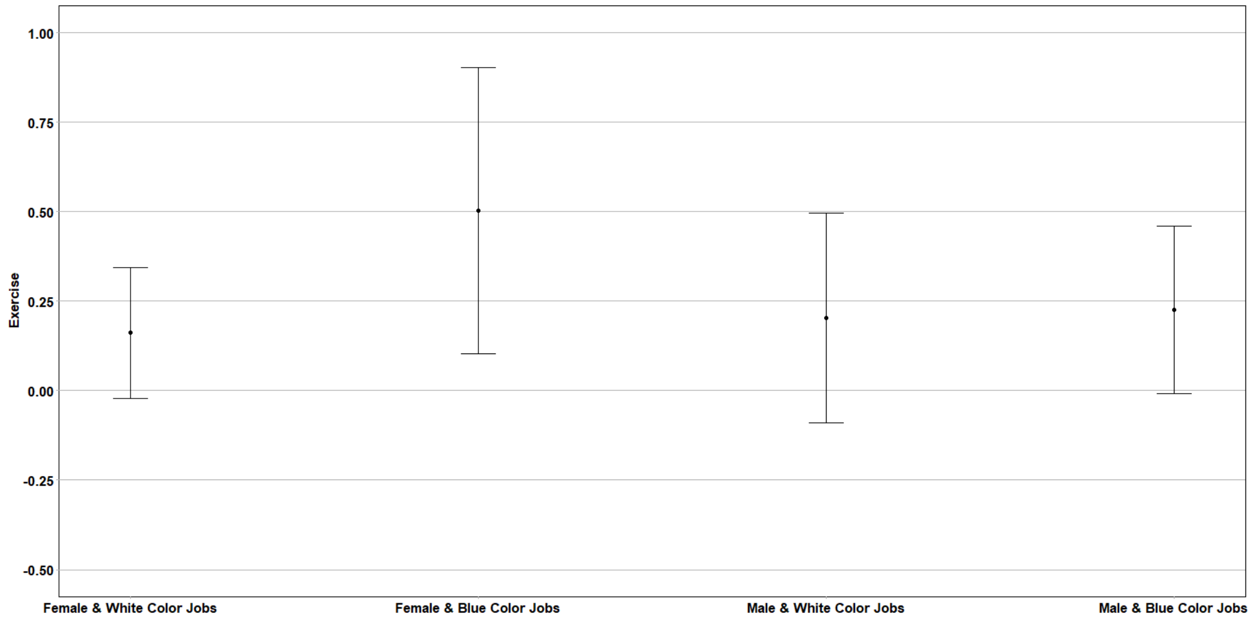


Figure 3.5: Smoking by Occupation Groups

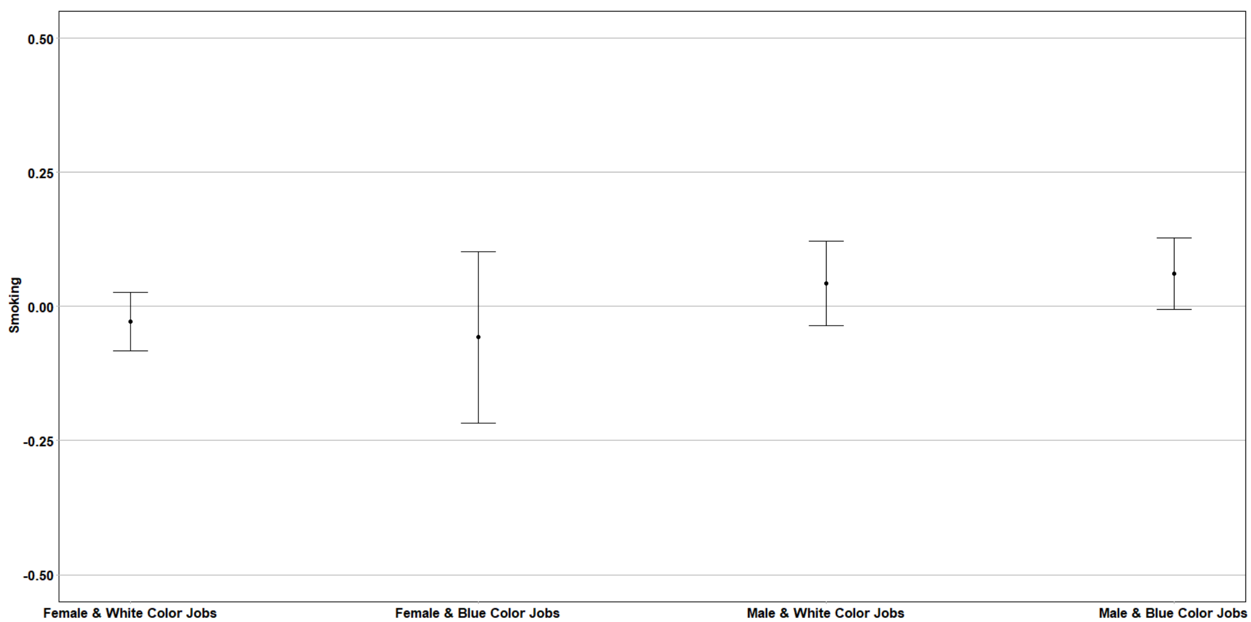


Figure 3.6: Heavy Drinking by Occupation Groups

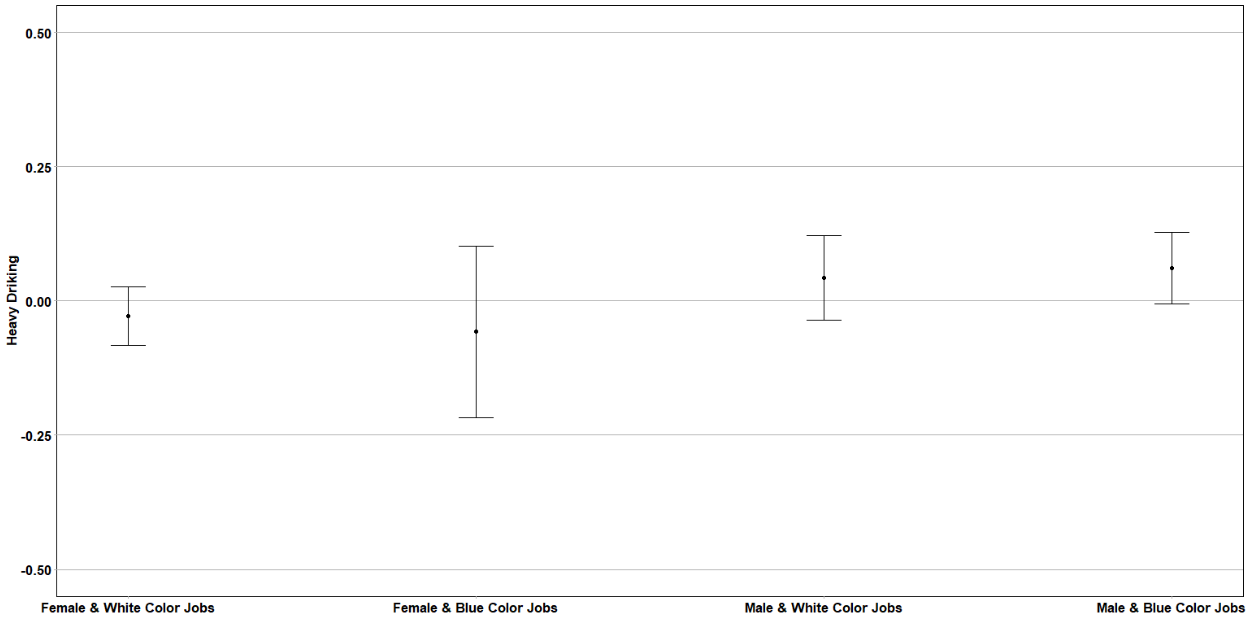


Table 3.1: Summary Statistics

	Female		Male	
Variables	Mean	N	Mean	N
A. Health				
Satisfactory Health	0.78	48,776	0.78	46,091
	(0.41)		(0.41)	
Mental Health	1.37	45,160	1.01	38,828
	(1.92)		(1.6)	
B. Health Behavior				
Heavy Drinking	0.013	45,513	0.04	41,276
	(0.11)		(0.20)	
Exercise	0.79	27,927	0.75	24,215
	(0.40)		(0.43)	
Smoking	0.15	48,608	0.16	45,842
	(0.15)		(0.37)	

Standard deviations are in parentheses.

Table 3.2: The Effect of Retirement on Health - OLS with FE

	Mental Health		Satisfactory Health	
	Female	Male	Female	Male
Retired	0.11***	0.115***	-0.03***	-0.05***
	(0.03)	(0.03)	(0.006)	(0.007)
Controls				
Age	-0.11***	-0.09***	0.024***	0.012*
	(0.033)	(0.03)	(0.006)	(0.007)
Age^2	0.0011***	0.0009***	-0.00016***	-0.00017***
	(0.0002)	(0.0002)	(0.00004)	(0.00005)
Year fixed effects	Yes	Yes	Yes	Yes
Individual fixed effects	Yes	Yes	Yes	Yes
Observations	45,137	38,799	48,751	46,059

Notes: This table presents the parameter estimates of the model estimated by an ordinary least square (OLS) model with fixed-effects (FE). The sample is restricted to individuals ages 50-75. Robust standard errors clustered at the level of the individual are in parentheses.

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

Table 3.3: The Effect of Retirement on Health - First Stage

A. First Stage	Mental Health		Satisfactory Health	
	Female	Male	Female	Male
Instruments				
$>$ 62 years old	0.093***	0.12***	0.098***	0.12***
	(0.007)	(0.008)	(0.007)	(0.007)
$>$ Normal age of retirement	0.07***	0.087***	0.07***	0.08***
	(0.007)	(0.008)	(0.007)	(0.007)
Controls				
Age	-0.0024	-0.016*	-0.002	-0.018**
	(0.008)	(0.008)	(0.007)	(0.0078)
Age^2	-0.00014 ***	-0.00002	-0.00013***	6.02e-06
	(0.00005)	(0.00006)	(0.00005)	(0.00005)
Year fixed effects	Yes	Yes	Yes	Yes
Individual fixed effects	Yes	Yes	Yes	Yes
Observations	45,137	38,799	48,751	46,059
F-test	219.93	151.47	262.67	201.09
Test of overidentifying restriction (p-value)	0.88	0.705	0.91	0.77

Table 3.4: The Effect of Retirement on Health - IV Model

	Mental Health		Satisfactory Health	
	Female	Male	Female	Male
Retired	-1.32***	-0.5***	0.23***	0.06
	(0.26)	(0.17)	(0.05)	(0.04)
Controls				
Age	-0.13***	-0.11***	0.03***	0.15*
	(0.04)	(0.03)	(0.007)	(0.007)
Age^2	0.0011***	0.0009***	-0.00017***	-0.00019***
	(0.0002)	(0.0002)	(0.00004)	(0.00005)
Year fixed effects	Yes	Yes	Yes	Yes
Individual fixed effects	Yes	Yes	Yes	Yes
Observations	45,137	38,799	48,751	46,059

Notes: This table presents the parameter estimates of the model estimated by two-stage least squares within estimator. The sample is restricted to individuals ages 50-75. Robust standard errors clustered at the level of the individual are in parentheses.

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

Table 3.5: Health Behaviors - IV Model

	Exercise		Smoking		Heavy drinking	
	Female	Male	Female	Male	Female	Male
Retired	0.33***	0.17**	-0.032	0.043	0.02	0.06***
	(0.087)	(0.08)	(0.03)	(0.027)	(0.015)	(0.022)
Controls						
Age	0.05***	0.04***	-0.003	-0.005	-0.002	-0.0018
	(0.012)	(0.013)	(0.004)	(0.005)	(0.0023)	(0.004)
Age^2	-0.0004***	-0.0003 ***	0.00002	0.00003	0.00002	9.66e-06
	(0.00007)	(0.00008)	(0.00003)	(0.00003)	(0.00001)	(0.00002
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Individual fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	27,919	24,207	48,583	45,811	45,494	41,255

Notes: This table presents the parameter estimates of the model estimated by two-stage least squares within estimator. The sample is restricted to individuals ages 50-75. Robust standard errors clustered at the level of the individual are in parentheses.

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

Table 3.6: The Decomposition of the Retirement Effect

	Total effect (α)		Indirect effect via lifestyles($\lambda \times \gamma$)	
	Female	Male	Female	Male
A. Mental Health				
Retired	-1.32 ***	-0.5 ***	-	-
	(0.26)	(0.17)		
Exercise			-0.125 ***	-0.031**
			(0.04)	(0.015)
Heavy drinking			-0.0014	-0.006
			(0.0032)	(0.005)
Smoking			-0.0028	-0.0086
			(0.005)	(0.007)

	Total effect (α)		Indirect effect via lifestyles ($\lambda \times \gamma$)	
	Female	Male	Female	Male
B. Satisfactory Health				
Retired	0.23***	0.06	-	-
	(0.05)	(0.04)		
Exercise			0.024***	0.0051*
			(0.008)	(0.003)
Heavy drinking			0.0007	0.001
			(0.00076)	(0.0012)
Smoking			-0.0006	0.002
			(0.00098)	(0.0015)

Table 3.7: Decomposing Retirement Effects on Health with Additional Mediators

	Total effect (α)		Indirect effect via lifestyles($\lambda \times \gamma$)	
	Female	Male	Female	Male
A. Mental Health				
Retired	-1.32 ***	-0.5 ***	-	-
	(0.26)	(0.17)		
Exercise			-0.11***	-0.031 *
			(0.04)	(0.016)
Heavy drinking			0.001	-0.005
			(0.0033)	(0.005)
Smoking			-0.0002	-0.001
			(0.004)	(0.007)
Total Wealth per 10.000			0.0003	0.001
			(0.003)	(0.002)
Spouse Present			-0.014	-0.003
			(0.01)	(0.007)

	Total effect (α)		Indirect effect via lifestyles ($\lambda \times \gamma$)	
	Female	Male	Female	Male
B. Satisfactory Health				
Retired	0.23***	0.06	-	-
	(0.05)	(0.04)		
Exercise			0.02 ***	0.005
			(0.007)	(0.003)
Heavy drinking			0.0006	0.001
			(0.0008)	(0.0012)
Smoking			0.001	0.001
			(0.0015)	(0.0013)
Total Wealth per 10.000			0.0001	0.0001
			(0.0007)	(0.0003)
Spouse Present			0.0006	0.0.0001
			(0.0011)	(0.0005)

APPENDIX TO CHAPTER 3

A Health Behaviors - First Stage Regression

Table A3.1 shows the estimated coefficients of the first-stage equation describing the probability of being retired for at least one year. As it is seen, the instruments have a highly significant effect on the probability of being retired for at least one year. The F test results confirm that the instruments are significant predictors of retirement, and the Sargan–Hansen test of overidentifying restriction does not reject the hypothesis that my instruments are valid. All of which hints that the choice of my instruments correctly identifies the models.

Table A3.1: Health Behaviors - First Stage

A. First Stage	Exercise		Smoking		Heavy drinking	
	Female	Male	Female	Male	Female	Male
Instruments						
$>$ 62 years old	0.084***	0.10***	0.099***	0.12***	0.09***	0.12***
	(0.009)	(0.01)	(0.007)	(0.007)	(0.007)	(0.007)
$>$ Normal age of retirement	0.083***	0.09***	0.07***	0.08***	0.07***	0.08***
	(0.0098)	(0.010)	(0.007)	(0.007)	(0.007)	(0.007)
Controls						
Age	-0.022**	-0.04***	-0.0017	-0.02**	0.0006	-0.02*
	(0.009)	(0.01)	(0.007)	(0.008)	(0.007)	(0.008)
Age^2	0.00002	0.00013*	-0.00014 ***	0.000012	-0.00016***	-0.00001
	(0.00006)	(0.00007)	(0.00005)	(0.00005)	(0.00005)	(0.00005)
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Individual fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	27,919	24,207	48,583	45,811	45,494	41,255
F-test	120.75	98.03	261.00	201.20	204.58	151.19
Test of overidentifying restriction (p-value)	0.46	0.96	0.53	0.15	0.72	0.99

B The Effects of Retirement, Conditional on Changes in Lifestyle, on Mental and Satisfactory Health Status

To measure the effect of retirement, conditional on changes in lifestyle, on mental and satisfactory health status, I estimate equation 3 using the 2SLS-FE estimator. Table B3.1 presents the results of both the first-stage regression and the IV regression for these two outcomes. The first-stage regression illustrates that the instruments are significant predictors of the probability of being retired. In addition, the F-test results (shown in the bottom panel A) verify that the instruments are highly related to the probability of being retired. The Sargan-Hansen test of overidentifying restrictions also does not reject the hypothesis that my instruments are valid (shown in the bottom panel A). Columns 1 and 2 in Panel B of Table B3.1 display that the effect of retirement on mental and satisfactory health status, conditional on changes in lifestyle, is positive for both males and females, while it only affects females' satisfactory health status positively.

Table B3.1: Estimation Results of Regression Analysis: Effects of Retirement, Conditional on Changes in Lifestyle, on Mental and Satisfactory Health Status

A. First Stage	Mental Health		Satisfactory Health	
	Female	Male	Female	Male
Instruments				
> 62 years old	0.086***	0.097	0.085***	0.10***
	(0.009)	(0.01)	(0.009)	(0.01)
> Normal age of retirement	0.08***	0.095***	0.081***	0.09***
	(0.009)	(0.01)	(0.009)	(0.01)
Controls				
Age	-0.025 ***	-0.043***	-0.022**	-0.046***
	(0.009)	(0.01)	(0.009)	(0.01)
Age^2	0.00004	0.0001	0.00003	0.00014
	(0.00006)	(0.00007)	(0.00006)	(0.00007)
Exercise	-0.164***	-0.06***	-0.16***	-0.07***
	(0.006)	(0.006)	(0.006)	(0.006)
Smoking	0.09***	-0.16***	0.098***	-0.16***
	(0.015)	(0.017)	(0.015)	(0.016)
Heavy drinking	-0.12***	-0.13***	-0.12***	-0.13***
	(0.02)	(0.015)	(0.019)	(0.015)
Year fixed effects	Yes	Yes	Yes	Yes
Individual fixed effects	Yes	Yes	Yes	Yes
Observations	27,174	22,349	27,753	23,936
F-test	117.45	82.69	121.32	91.59
Test of overidentifying restriction (p-value)	0.33	0.63	0.29	0.1

B. IV	Mental Health		Satisfactory Health	
	Female	Male	Female	Male
Retired	-1.108***	-0.73***	0.17**	0.009
	(0.34)	(0.027)	(0.07)	(0.06)
Controls				
Age	-0.17***	-0.18***	0.05***	0.024**
	(0.05)	(0.04)	(0.009)	(0.012)
Age^2	0.0014***	0.0014 ***	-0.0003***	-0.0003***
	(0.0003)	(0.0002)	(0.00006)	(0.00007)
Exercise	-0.381 ***	-0.18***	0.074***	0.03***
	(0.06)	(0.03)	(0.013)	(0.008)
Smoking	0.088	-0.20**	0.018	0.04 *
	(0.09)	(0.08)	(0.018)	(0.02)
Heavy drinking	-0.070	-0.099	0.035	0.018
	(0.13)	(0.075)	(0.024)	(0.018)
Year fixed effects	Yes	Yes	Yes	Yes
Individual fixed effects	Yes	Yes	Yes	Yes

Notes: Robust standard errors are clustered two-way at the state and state pairs level.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

C Including Mobility Index as a Control Variable in the Model: Results

Table C3.1: The Effect of Retirement on Health

A. First Stage	Mental Health		Satisfactory Health	
	Female	Male	Female	Male
Instruments				
$>$ 62 years old	0.094***	0.12***	0.094***	0.12***
	(0.007)	(0.008)	(0.007)	(0.007)
$>$ Normal age of retirement	0.07***	0.089***	0.07***	0.08***
	(0.007)	(0.008)	(0.007)	(0.007)
Controls				
Age	-0.0018	-0.016*	-0.002	-0.016*
	(0.008)	(0.009)	(0.008)	(0.008)
Age^2	-0.00014***	-0.00004	-0.00014***	-0.00004
	(0.00005)	(0.00006)	(0.00005)	(0.00005)
Mobility Index	-0.024***	-0.036***	-0.024***	-0.035***
	(0.005)	(0.006)	(0.005)	(0.005)
Year fixed effects	Yes	Yes	Yes	Yes
Individual fixed effects	Yes	Yes	Yes	Yes
Observations	42,571	37,017	43,645	40,809
F-test	199.34	143.20	208.30	160.67
Test of overidentifying restriction (p-value)	0.96	0.77	0.71	0.78

B. IV	Mental Health		Satisfactory Health	
	Female	Male	Female	Male
Retired	-1.15***	-0.49 ***	0.20***	0.06
	(0.26)	(0.17)	(0.05)	(0.04)
Controls				
Age	-0.13***	-0.097***	0.03***	0.007
	(0.04)	(0.03)	(0.007)	(0.008)
Age^2	0.0010***	0.0009***	-0.00018***	-0.00015***
	(0.0002)	(0.0002)	(0.00004)	(0.00005)
No difficulties	-0.28***	-0.32***	0.09***	0.14***
	(0.025)	(0.03)	(0.005)	(0.007)

Table C3.2: Health Behaviors

A. First Stage	Exercise		Smoking		Heavy drinking	
	Female	Male	Female	Male	Female	Male
Instruments						
> 62 years old	0.084***	0.10***	0.095***	0.12***	0.09***	0.12***
	(0.009)	(0.01)	(0.007)	(0.007)	(0.007)	(0.008)
> Normal age of retirement	0.08***	0.09***	0.07***	0.08***	0.07***	0.08***
	(0.01)	(0.010)	(0.007)	(0.007)	(0.007)	(0.007)
Controls						
Age	-0.03***	-0.05***	-0.0017	-0.017**	-0.002	-0.02**
	(0.009)	(0.01)	(0.008)	(0.008)	(0.008)	(0.008)
Age^2	0.00007	0.00015	-0.00015***	-0.00004	-0.00015***	-0.00002
	(0.00006)	(0.00007)	(0.00005)	(0.00005)	(0.00005)	(0.00006)
Mobility Index	-0.015**	-0.02***	-0.024***	-0.035***	-0.024***	-0.034***
	(0.006)	(0.007)	(0.005)	(0.006)	(0.005)	(0.006)
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Individual fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	26,204	23,071	43,481	40,562	42,301	38,798
F-test	97.30	82.27	206.90	160.68	178.10	137.16
Test of overidentifying restriction (p-value)	0.89	0.91	0.26	0.13	0.8	0.86

B. IV	Exercise		Smoking		Heavy drinking	
	Female	Male	Female	Male	Female	Male
Retired	0.31***	0.16**	-0.026	0.04	0.009	0.054**
	(0.088)	(0.08)	(0.03)	(0.029)	(0.016)	(0.024)
Controls						
Age	0.04***	0.03**	-0.006	-0.006	-0.002	-0.0014
	(0.012)	(0.014)	(0.005)	(0.005)	(0.002)	(0.004)
Age^2	-0.0003***	-0.0003 ***	0.00002	0.00004	0.00002	9.49e-06
	(0.00007)	(0.00008)	(0.00003)	(0.00004)	(0.000014)	(0.00003)
Mobility Index	0.045***	0.06***	0.009***	0.017***	0.0008	0.004
	(0.008)	(0.01)	(0.003)	(0.004)	(0.0016)	(0.003)

Table C3.3: Estimation Results of Regression Analysis: The Effects of Retirement, Conditional on Change in Lifestyle, On Mental and Satisfactory Health Status

A. First Stage	Mental Health		Satisfactory Health	
	Female	Male	Female	Male
Instruments				
> 62 years old	0.086***	0.099***	0.085***	0.10***
	(0.009)	(0.01)	(0.009)	(0.01)
> Normal age of retirement	0.08***	0.097***	0.08***	0.09***
	(0.0099)	(0.01)	(0.009)	(0.01)
Controls				
Age	-0.03 ***	-0.045***	-0.028***	-0.05***
	(0.0097)	(0.01)	(0.009)	(0.01)
Age^2	0.00008	0.00012	0.00007	0.00016**
	(0.00006)	(0.00007)	(0.00006)	(0.00007)
Exercise	-0.15***	-0.06***	-0.15***	-0.06***
	(0.006)	(0.006)	(0.006)	(0.006)
Smoking	0.07***	-0.14***	0.08***	-0.14***
	(0.015)	(0.017)	(0.015)	(0.017)
Heavy drinking	-0.06*	-0.12***	-0.06***	-0.12***
	(0.02)	(0.015)	(0.019)	(0.015)
Mobility Index	-0.016***	-0.026***	-0.015**	-0.02***
	(0.006)	(0.007)	(0.006)	(0.007)
Year fixed effects	Yes	Yes	Yes	Yes
Individual fixed effects	Yes	Yes	Yes	Yes
Observations	25,490	21,265	26,048	22,809
F-test	92.27	68.66	95.92	76.35
Test of overidentifying restriction (p-value)	0.51	0.59	0.36	0.19

B. IV	Mental Health		Satisfactory Health	
	Female	Male	Female	Male
Retired	-0.95***	-0.67**	0.12	-0.007
	(0.35)	(0.27)	(0.07)	(0.06)
Controls				
Age	-0.17***	-0.17***	0.04***	0.02*
	(0.05)	(0.05)	(0.009)	(0.011)
Age^2	0.0014***	0.0014 ***	-0.0003***	-0.0003***
	(0.0003)	(0.0003)	(0.00006)	(0.00007)
Exercise	-0.34 ***	-0.16***	0.06***	0.02***
	(0.06)	(0.035)	(0.013)	(0.008)
Smoking	0.04	-0.16**	0.03*	0.04 *
	(0.08)	(0.08)	(0.017)	(0.019)
Heavy drinking	-0.0013	-0.02	0.017	0.01
	(0.13)	(0.07)	(0.022)	(0.02)
Mobility Index	-0.26***	-0.29***	0.07***	0.12***
	(0.03)	(0.04)	(0.006)	(0.009)
Year fixed effects	Yes	Yes	Yes	Yes
Individual fixed effects	Yes	Yes	Yes	Yes

Notes: Robust standard errors are clustered two-way at the state and state pairs level.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table C3.4: The Decomposition of the Retirement Effect

	Total effect (α)		Indirect effect via lifestyles($\lambda \times \gamma$)	
	Female	Male	Female	Male
A. Mental Health				
Retired	-1.15 ***	-0.49 ***	-	-
	(0.26)	(0.17)		
Exercise			-0.105 ***	-0.026*
			(0.036)	(0.014)
Heavy drinking			-0.000012	-0.0018
			(0.002)	(0.005)
Smoking			-0.001	-0.006
			(0.004)	(0.007)

	Total effect (α)		Indirect effect via lifestyles ($\lambda \times \gamma$)	
	Female	Male	Female	Male
B. Satisfactory Health				
Retired	0.20***	0.06	-	-
	(0.05)	(0.04)		
Exercise			0.019***	0.0032
			(0.006)	(0.002)
Heavy drinking			0.000153	0.0005
			(0.0005)	(0.001)
Smoking			-0.0008	0.0016
			(0.0012)	(0.0014)

D The Sample Selection

Table D3.1: The Effect of Retirement on Health

A. First Stage	Mental Health		Satisfactory Health	
	Female	Male	Female	Male
Instruments				
> 62 years old	0.088***	0.11***	0.093***	0.12***
	(0.006)	(0.007)	(0.006)	(0.007)
> Normal age of retirement	0.07***	0.062***	0.055***	0.06***
	(0.007)	(0.007)	(0.007)	(0.007)
Controls				
Age	-0.014*	-0.015*	-0.014**	-0.019***
	(0.007)	(0.008)	(0.007)	(0.007)
Age^2	-0.00007	-0.00004	-0.00007	-9.92e-06
	(0.00005)	(0.00005)	(0.00005)	(0.00005)
Year fixed effects	Yes	Yes	Yes	Yes
Individual fixed effects	Yes	Yes	Yes	Yes
Observations	56,005	47,941	60,387	56,718
F-test	228.7	150.01	268.19	200.59
Test of overidentifying restriction (p-value)	0.41	0.71	0.57	0.32

B. IV	Mental Health		Satisfactory Health	
	Female	Male	Female	Male
Retired	-1.38***	-0.4 **	0.26***	0.089**
	(0.26)	(0.17)	(0.05)	(0.04)
Controls				
Age	-0.16***	-0.09***	0.03***	0.02***
	(0.03)	(0.03)	(0.006)	(0.007)
Age^2	0.0011***	0.0009***	-0.00019***	-0.00019***
	(0.0002)	(0.0002)	(0.00004)	(0.00004)

Table D3.2: Health Behaviors

A. First Stage	Exercise		Smoking		Heavy drinking	
	Female	Male	Female	Male	Female	Male
Instruments						
$>$ 62 years old	0.083***	0.10***	0.094***	0.12***	0.09***	0.12***
	(0.009)	(0.01)	(0.006)	(0.007)	(0.007)	(0.007)
$>$ Normal age of retirement	0.07***	0.07***	0.054***	0.06***	0.05***	0.06***
	(0.009)	(0.010)	(0.007)	(0.007)	(0.007)	(0.007)
Controls						
Age	-0.035***	-0.04***	-0.015	-0.02***	-0.011	-0.02**
	(0.009)	(0.01)	(0.007)	(0.007)	(0.007)	(0.008)
Age^2	0.00009	0.00009	-0.00006	-3.03e-06	-0.00008	-0.00002
	(0.00006)	(0.00007)	(0.00005)	(0.00005)	(0.00004)	(0.00005)
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Individual fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	34,516	29,602	60,136	56,431	56,259	50,784
F-test	118.30	91.21	266.60	200.53	219.11	151.55
Test of overidentifying restriction (p-value)	0.53	0.60	0.60	0.36	0.42	0.57

B. IV	Exercise		Smoking		Heavy drinking	
	Female	Male	Female	Male	Female	Male
Retired	0.34***	0.22**	-0.04	0.03	0.002	0.066***
	(0.082)	(0.08)	(0.03)	(0.027)	(0.015)	(0.022)
Controls						
Age	0.05***	0.04***	-0.005	-0.006	-0.0004	-0.004
	(0.011)	(0.012)	(0.004)	(0.005)	(0.0019)	(0.004)
Age^2	-0.0003***	-0.0003 ***	0.00001	0.00003	4.92e-06	0.00002
	(0.00006)	(0.00007)	(0.000025)	(0.00003)	(0.000011)	(0.00002)

Table D3.3: Estimation Results of Regression Analysis: The Effects of Retirement, Conditional on Change in Lifestyle, On Mental and Satisfactory Health Status

A. First Stage	Mental Health		Satisfactory Health	
	Female	Male	Female	Male
Instruments				
> 62 years old	0.085***	0.096***	0.085***	0.10***
	(0.008)	(0.01)	(0.008)	(0.01)
> Normal age of retirement	0.07***	0.07***	0.07***	0.07***
	(0.009)	(0.01)	(0.009)	(0.01)
Controls				
Age	-0.04 ***	-0.04***	-0.035***	-0.044***
	(0.009)	(0.01)	(0.009)	(0.01)
Age^2	0.00011*	0.00007	0.0001	0.0001
	(0.00006)	(0.00007)	(0.00006)	(0.00007)
Exercise	-0.22***	-0.13***	-0.22***	-0.13***
	(0.006)	(0.006)	(0.006)	(0.006)
Smoking	0.13***	-0.14***	0.13***	-0.14***
	(0.015)	(0.016)	(0.015)	(0.016)
Heavy drinking	-0.034*	-0.22***	-0.03*	-0.22***
	(0.02)	(0.017)	(0.017)	(0.017)
Year fixed effects	Yes	Yes	Yes	Yes
Individual fixed effects	Yes	Yes	Yes	Yes
Observations	33,631	27,451	34,272	29,304
F-test	151.00	95.77	154.78	106.08
Test of overidentifying restriction (p-value)	0.08	0.71	0.44	0.41

B. IV	Mental Health		Satisfactory Health	
	Female	Male	Female	Male
Retired	-1.10***	-0.60**	0.20***	0.06
	(0.32)	(0.026)	(0.07)	(0.06)
Controls				
Age	-0.19***	-0.16***	0.05***	0.03**
	(0.05)	(0.04)	(0.008)	(0.009)
Age^2	0.0015***	0.0013 ***	-0.0003***	-0.0003***
	(0.0003)	(0.0002)	(0.00005)	(0.00006)
Exercise	-0.45 ***	-0.23***	0.098***	0.04***
	(0.08)	(0.044)	(0.016)	(0.01)
Smoking	0.11	-0.17**	0.0097	0.04 **
	(0.09)	(0.07)	(0.016)	(0.017)
Heavy drinking	-0.06	-0.15*	0.035*	0.048**
	(0.11)	(0.08)	(0.019)	(0.02)
Year fixed effects	Yes	Yes	Yes	Yes
Individual fixed effects	Yes	Yes	Yes	Yes

Notes: Robust standard errors are clustered two-way at the state and state pairs level.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table D3.4: The Decomposition of the Retirement Effect

	Total effect (α)		Indirect effect via lifestyles($\lambda \times \gamma$)	
	Female	Male	Female	Male
A. Mental Health				
Retired	-1.38 ***	-0.4 **	-	-
	(0.26)	(0.17)		
Exercise			-0.153 ***	-0.05**
			(0.04)	(0.02)
Heavy drinking			-0.00012	-0.0099
			(0.002)	(0.007)
Smoking			-0.00038	-0.00
			(0.0056)	(0.005)

	Total effect (α)		Indirect effect via lifestyles ($\lambda \times \gamma$)	
	Female	Male	Female	Male
B. Satisfactory Health				
Retired	0.26***	0.089**	-	-
	(0.05)	(0.04)		
Exercise			0.033***	0.0088**
			(0.01)	(0.004)
Heavy drinking			-0.00007	0.0032*
			(0.0006)	(0.0017)
Smoking			-0.00038	0.0012
			(0.0009)	(0.0013)

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