Does the ACA Medicaid Expansion Encourage Early Retirement?

Abstract

In this study, I examine the effect of the Affordable Care Act Medicaid expansion on the retirement plans of low-educated, childless adults, aged 50 to 64. I employ a triple-differences (DDD) methodology, exploiting variation in individuals' health insurance status and the timing of the expansion decisions of states. I find that with Medicaid expansion, insured workers without retirement health insurance (RHI) prior to the expansion decreased full-time work by 8.4 percentage points relative to those with RHI and those without any employer-sponsored coverage whatsoever. Among those no longer working full-time, 77 percent transitioned to complete retirement.

Key Words: Affordable Care Act, Medicaid, Retirement, Labor Force Participation.

JEL Classification Codes: I130, I180, J2, J220.

I 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. Only 28 percent of large firms (200 or more workers) and 5 percent of small firms (3 to 199 workers) offered retiree health insurance (McArdle et al., 2014). 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 health care 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 employees 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, which created variation 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.

This paper aims to extend our understanding of the ACA Medicaid expansion on early retirement. To examine the effect of ACA Medicaid expansion on the retirement plan of adults aged 50 to 64 (without children in the home), 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 the within state comparison of individuals who have retiree health insurance (RHI) through their current or previous employer or spouses' plan along with those who have no employer-sponsored health insurance (ESHI) whatsoever vs. those who do 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. This restriction also alleviates the concern of isolating the effect of Medicaid from simultaneously implemented health insurance exchanges.[1] Although, it might not already be an issue due to the universality of the marketplace program across all states. My finding suggests that Medicaid expansion leads to a decline in full-time work by 8.4 percentage points for the treatment groups. The result is robust to several alternative identification strategies.

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

The structure of the paper is as follows: Section II provides a literature review related to health insurance and early retirement. 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.

II RELATED LITERATURE ON HEALTH INSURANCE AND EARLY RETIREMENT

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 (Meerding et al., 1998). Because health insurance is tied to one's employer, the availability of alternative health insurance options is an important factor in retirement decisions that have been studied frequently.

II.1 The availability of retiree health insurance and retirement

Previous studies applied different approaches to examine the effect of health insurance on retirement. Many utilized micro-data and variation in the availability of employer-provided retiree

[1] Health insurance exchanges provide premium tax credits to eligible people to help them purchase coverage through the marketplaces. Income requirement for premium tax credits eligibility ranges from 138 percent to 400 percent of FPL in states that expanded Medicaid (people with incomes below 138 percent of FPL are eligible for Medicaid in expansion states; therefore, they can not receive premium tax credits), while tax credit eligibility ranges from 100 percent to 400 percent of FPL in non-expansion states.

health insurance (RHI) to estimate the effect of health insurance on retirement. 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).

Some studies will be 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 effect 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, and 20 states passed it. In 1986, the Federal government mandated such coverage at the national level under COBRA. Gruber and Madrian find that the availability of continuation coverage accounts for 60 percent of the net rise in retirement for males aged between 55 and 64 years old in 1980. The fraction of retired among 55 to 64 years old increases by 3.3 percentage points with COBRA.

Kapur and Rogowski (2011) utilized the Health and Retirement Study (HRS) data to analyze the impact of RHI on female and male workers' retirement decisions. The result shows that RHI increases retirement rates by between 3 to 5 percentage points depending on gender and marital status.

Nyce et al. (2013) utilized employee-level data from 54 diverse firms that are clients of Tower Watson and examine the effect of RHI on the turnover rate. The data set the authors use allow them to control various firm-level characteristics and service eligibility criteria for retiree health coverage. The result shows that subsidized retiree coverage decreases total employment among 58 to 64 years old by 5.6 percentage.

Fitzpatrick (2014) analyzes the effect of the availability of RHI for public school teachers in the state of Illinois. She uses administrative data from Illinois Public School (IPS) and utilizes a difference-in-differences approach that compares the labor supply of teachers eligible for retiree health insurance programs for teachers and other public school employees (TRHIP) to those who are ineligible before and after the introduction of TRHIP. The result shows that RHI leads employees to retire about 2 years earlier, or 8 percent, earlier on average. Shoven and Slavov (2014) estimate the impact of retiree health insurance on public sector workers' labor supply. The author utilizes the HRS data, and the result illustrates that RHI leads to higher rates of stopping full-time work among 55 to 64 years old. The Fitzpatrick (2014) and Shaven and Slavov (2014) studies add evidence that RHI encourages early retirement in the public sector, which is consistent with the findings of earlier studies that focused on employers in the private sector.

Structural estimations of the effect 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 explicitly. Therefore, they find that the average employer contribution to health insurance is modest, but the value of employer-provided 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 explicitly including risk aversion in their study. Moreover, the authors explicitly model heterogeneity in health insurance preferences. The result shows that employer-provided retiree health insurance raises the exit rate from employment by 6 percent if the firm pays the entire cost. However, the authors assume that there is no capital market. In this way, they avoid an individual's ability of consumption smoothing through saving. Since saving provides individuals the ability to self-insure against future medical expenses shock and reduce the volatility of consumption, not allowing saving in the model might lead to overestimation of the value of health insurance, and in turn the effect of health insurance on retirement. The authors also omit family consideration from the model, which reduces the positive effect of health insurance on employment decisions because married couples can sometimes benefit from their partner's employer-provided health insurance. It is likely that the married couple coordinates their employment decisions.

Addressing these points, French and Jones (2011) examine the impact of employer-provided insurance, Medicare, and Social Security on retirement decisions, and they estimate a dynamic programming model of retirement that considers saving, uncertain medical expenditures, and spousal income and pension benefits. The simulation result shows that increasing Medicare eligibility age from 65 to 67 results in an increase in years of work by 0.074 years over ages 60 to 69, and removing 2 years worth of Social Security benefits leads individuals to work extra 0.076 years.

II.2 The availability of public health insurance in retirement decisions

Economic theory predicts that cash and in-kind transfer program generally create labor supply disincentives.[2] The empirical results of a wealth of previous research focusing on labor supply decisions of elderly individuals support this hypothesized effect. For example, Dague et al. (2017) explore the effect of the temporary expansion of Medicaid to childless adults in Wisconsin, and they find a large decrease in labor supply of over age 55 (-17.6 percentage points). Boyle and Lahey (2010) examine an expansion of Veteran's health insurance to all service members in the 1990s, and the result shows that increased availability of this public coverage encourages early retirement by 3 percent. Similarly, Wettstein (2020) explore the effect of the Medicare Part D program on 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 full-time work by 8.4 percentage points.

The enactment of the Affordable Care Act provide an opportunity for researchers to reanalyze the retirement behavior of individuals to the provision of public or heavily subsidized coverage.[3] The findings of these studies provide mixed evidence on whether the introduction of the ACA encourages early retirement. Gustman et al. (2019) utilized the HRS data to estimate the effect of the ACA on retirement behavior for the years 2010 to 2014. The authors applied a difference-in-differences (DD) approach that compares individuals with health insurance at work but not in retirement to two other groups: those who, before the ACA, had RHI and those without health

[2]Matsudaira and Blank (2015) provide helpful discussion and analysis of unintended effects on work effort of cash and in-kind transfer programs.

[3]There are studies that analyze the effect of the ACA on labor force participation among the working-age population rather than focus on the sample that is most likely to be observed in retirement lock, older pre-Medicare eligible individuals (ages 50 to 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).

insurance either at work or in retirement before and after the ACA. The authors not only analyze the effect of the ACA on observed retirement decisions but also examine the impact of the ACA on expected retirement. The results show that the ACA does not lead to change in either propensity to retire or the retirement expectations, but they note that the time elapsed might be too short to observe internalization of the incentives under the ACA. On the other hand, the structural model they present suggests that the ACA led to an increase in the probability of retirement, but the increase in retirement is quite small, only about half a percentage point at each year of age.

One closely related study to Gustman et al. (2019) is Ayyagari (2019), who analyzes the impact of the ACA on the subjective probability of working past age 62. The author uses the HRS data from 1998 through 2014 waves. The sample restricted individuals between 45 and 60 years old and worked full- time in 2008. Applying a DD approach that compares individuals with and without employer-sponsored retiree benefits before the ACA, the author finds that the ACA leads to a decrease in the subjective probability of working past the age of 62 by 5.6 percentage points. Contrary to Gustman et al. (2019), on average, the ACA affects individuals' expected retirement age, which is 3.6 to 7.6 months earlier.

Levy, Buchmueller, and Nikpay (2018) find no evidence of change in the probability of retirement among persons aged 50 to 64 years 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. The authors pooled all individuals together in the sample, which might confound the estimates because parents were eligible for Medicaid prior to the ACA, which could make retirement decisions driven by the woodwork effects.[4]

Aslim (2019) finds no impact of Medicaid expansion on retirement among low-educated, childless men aged 55 to 64 years and finds a small increase in retirement for low-educated childless women. However, the Wald estimates suggest a 10 percentage point increase in the probability

[4]Individuals who were previously eligible for coverage but for some reason had not enrolled it or may not have even been aware that they could so choose to sign up with the expansion of public program eligibility, which is called the "woodwork" effect. Several recent studies have also highlighted the importance of considering woodwork effects when analyzing the effects of public health insurance expansion (Sonier et al., 2013; Leung and Mas, 2018; Frean et al., 2017).

of retirement for women. The author considers this finding as an upper bound for the retirement effect.

In contrast to existing studies that find little to no effect of increased retirement behavior (Levy et al., 2018; Aslim, 2019; Gustman et al., 2019), Wood (2019) finds a 2 percent and 8 percent decrease in labor force participation among individuals aged 55 to 64 resulting from the premium subsidies and Medicaid expansion, respectively. The author measures Medicaid eligibility and premium subsidies based on income. However, relying on 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 uses a simulated instrument to address these issues and finds consistent results. In a similar vein, Bradley and Sabik (2019) utilize simulated eligibility as an instrument to explore the effect of Medicaid expansion on labor supply. They find that older low-income women (aged 55 to 64 years) with a high-school degree 17 percentage points less likely to be employed in states that had expanded Medicaid. It is important to note that the data Wood and Bradley, and Sabik utilized are American Community Survey (ACS) and the March Current Population Survey (CPS), but both ACS and CPS collects no data on assets or expenditure, and nearly all income data refer to the previous calendar year self-report income, rather than the monthly reference period 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 impact of ACA on the labor supply of the near-elderly (ages 60 to 64). They utilize variation across geographic areas in the preexisting level of insurance and use those aged 65 to 69 years as a within region control. The result shows that the near-elderly reduce their labor force participation rate by 0.6 percentage.

My paper contributes to health insurance and retirement literature by focusing on the ACA Medicaid expansion that directly affects health insurance availability for low-income adults. Although there are studies that focus on the impact of the ACA Medicaid expansion on early retirement, existing papers utilize 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. In addition, much of previous studies simply compare labor market outcomes of those who are likely to be eligible for Medicaid (treatment group) in expansion and non-expansion states before and after the expansion. However, this approach is not able to control for any systematic shock to labor-market outcomes of the treatment group in the expansion states that are correlated with, but not due to, the expansion. Utilizing a triple differences approach allows me to control state-specific shocks, which are correlated with the expansion decision of states. In addition, it allows me to identify the most relevant subgroups, which avoids the possible underestimation of policy impact. Additionally, I control key variables such as pension plan coverage, health status, etc. These variables highly influence individuals' retirement behavior and are likely correlated with the availability of RHI.

III DATA AND EMPIRICAL STRATEGY

The starting point of my empirical work approach is identifying the groups of individuals affected by the expansion (treatment group) and the states that adopt Medicaid expansion (experimental states) vs. control group and control states, respectively. This will create the groups that will allow for the implementation of the triple differences approach. The next two sections provide more details.

III.1 Identifying treatment and control groups

Not all individuals face constraints in retirement due to fear of losing health coverage. Those who are eligible for RHI coverage from their employers upon separation, along with those who have health insurance provided by their spouse, have health insurance that is not tied to their job. Their retirement decision is more likely to be detached from considerations of health insurance, and they are less likely to alter their retirement decision with Medicaid expansion. Similarly, individuals who have no ESHI whatsoever do not experience retirement lock, a situation when a worker remains in the labor force due to the potential loss of employer-sponsored health insurance. However, Medicaid expansion might influence their labor supply decision with income channel. 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 either control or treatment group, I analyze the effect of Medicaid expansion on their household out-of-pocket medical spending. I utilize quantile estimation and find that there is no statistically significant change in expenditure at every fifth quantile of the distribution of out-of-pocket spending (for details, see Table B1 in the Appendix).[5] 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. Therefore, those with retiree health coverage through their previous or current employer or their spouse and those who have no ESHI whatsoever are the "control" group in the study.

Table 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 any employer-sponsored health insurance. This includes insurance from a current or previous employer or union, of one's own or of one's spouse.

Note: Individuals who have Medicare is excluded since those who have Medicare when they aged under 65 might be disabled or medically needy individuals, and their retirement decision can be involuntary or forced due to their health issue.

I define a "treatment" group of individuals who have insurance from their employer but do not have retiree coverage. Tables 1 summarizes the characteristics that define treatment and control groups. Before Medicaid expansion, members of the treatment group were more likely to intend

[5]All appendices are available at the end of this article as it appears in JPAM online. Go to the publisher's website and use the search engine to locate the article at http://onlinelibrary.wiley.com.

to work until they reached age 65 to keep their employer health insurance. Health insurance was guaranteed to them at age 65 or older by Medicare. Therefore, if maintaining health insurance was sufficiently important for them (or they did have enough assets to cover their possible medical expenses), they had to keep working until the age of Medicare eligibility or else lose health insurance.

In contrast, with Medicaid expansion in 2014, members of the treatment group in expansion states were not constrained with only employer health insurance options. They could have health insurance through Medicaid before Medicare eligibility. Therefore, with the Medicaid expansion, members of the treatment group were no longer locked into their job and out of retirement.

I categorize persons as belonging to the treatment and control group based on their current health insurance status. It is important to note that if the availability of employer-sponsored health insurance changes as a result of Medicaid expansion, the estimate would be confounded. However, recent studies show that Medicaid expansion and establishment of health insurance exchange do not lead to a change in the offer, take-up, or coverage rates of employer-sponsored health insurance (Abraham et al., 2019, 2016; Blavin et al., 2015).[6] These findings alleviate the concern about the potential endogeneity of the treatment group (those with ESHI but without RHI) to the policy.

III.2 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 percent of the Federal poverty line (FPL) in January 2014 according to the ACA provisions, six states had previously expanded eligibility for coverage

[6]Note that the employer mandate, requiring employers with 50 or more full-time workers to provide health insurance or be subject to penalties, was implemented under the ACA in 2015. This policy might change the composition of working hours. For example, an employer might restrict the number of hours employees can work or replace their full-time workers with part-time. However, the possible 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 different. To test whether there is a differential response in expansion states compared to non-expansion states, I compare working hours in 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 C1).
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and

Table 2. Timing of state Medicaid expansion for childless adults

Early Expansion States (Before January 2014)	Expansion States (January 2014)	Late Expansion States (After January 2014)	None-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/`

had them in place as the other states expanded in 2014. These include CT, DE, ME, NY, VT, DC.

For the initial analysis, I drop these for the purpose of a cleaner analysis.[7] 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 2 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 percent FPL (without enrollment cap), and approximately 99,000 childless adults became newly eligible for Medicaid (Norris, 2020). Therefore, Wisconsin counted as the experimental state in the main analysis.[8]

The decision to initially exclude the early and late expansion states and leave the cleaner set of experimental and non-experimental states is meant to more clearly identify the timing of surges in Medicaid enrollment.

III.3 Data

I use the Rand version of the Health and Retirement Study (HRS) data for the years 2010 to 2016.[9] [10] The HRS is a nationally representative, longitudinal survey of individuals over 50 year of age 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

[7] Although HI and AZ also expanded earlier, I do not drop these because they suspended and reinstated at ACA level in 2014. In 2000, Arizona had expanded coverage for childless adults up to 100 percent FPL, but beginning in July 2011, enrollment in adults was capped. Similarly, Hawaii had covered childless adults up to 100 percent FPL under its QUEST Medicaid managed care waiver program, but enrollment was closed except for certain groups in 2012. The results are similar if Arizona and Hawaii are excluded (for details, see Appendix Table F1).
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[8] I also remove Wisconsin from the main sample and the sample including early and late expansion states and re-estimate equation (1). The results are qualitatively in line with main findings. For details, see Appendix Table E1.
All appendices are available at the end of this article as it appears in JPAM online. Go to the publisher's website and use the search engine to locate the article at http://onlinelibrary.wiley.com.

[9] 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)

[10] For information on dependent children, I refer to the HRS core final release data.

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, either his or her own plan or a spouse's plan.[11] 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.[12] The control group also includes individuals who have no ESHI whatsoever. This includes insurance from a current or previous employer or union, of one's own or of one's spouse. Those who report that they do not have health insurance from a current or previous employer, a union of one's own or of one's spouse, are allocated to the control group.

In addition, my access to restricted geographic information from the HRS data allows me to determine an 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 low-educated childless adults aged between 50 to 64 years old.[13] As is common for the cohort studied, low-educated is defined as having a high school education or less.[14] Note that disabled individuals are excluded because their employment decision can be involuntary or forced due to their health issues.

The main outcome of interest is the full-time work indicator.[15] I utilize a full-time work indicator instead of retirement for two reasons. First, individuals may wish to gradually step down

[11] Coverage in retirement derived from the variable RwHERET in the Rand HRS 1992 to 2016.

[12] 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.

[13] 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).

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

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

from full-time jobs to retirement. They might reduce working hours in the same job or switch to part-time jobs instead of retired. 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.

Table 3 gives descriptive statistics of the sample (low-educated, childless adults aged between 50 to 64) of two experimental groups: the treatment and control groups before and after the expansion. There are pre-expansion differences between treatment and control groups both in expansion and non-expansion states. 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 than the control groups. Furthermore, the rate of full-time employment for the treatment group is higher than the control group. However, these pre-existing differences between the treatment and control groups do not necessarily violate the identifying assumption of triple differences estimation. The triple difference 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.

The triple differences method for my empirical model provides estimations in only one direction—moving out of full-time work. The absence of a triple difference does not suggest reentry to full-time work. Therefore, if the individual reenters full-time employment after existing full-time employment, I exclude periods after re-entry to full-time work.[16]

[16]For example, let's assume that the individual works full-time in 2010 and exist 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.

Table 3. Descriptive statistics for sample years 2010 to 2016

	Pre-Expansion		Post-Expansion	
Panel A: Expansion States	Treatment	Control	Treatment	Control
Working Full-Time	0.84	0.35	0.85	0.33
	(0.36)	(0.5)	(0.36)	(0.47)
Women	0.65	0.56	0.59	0.59
	(0.47)	(0.5)	(0.49)	(0.491)
Age	56.4	57.69	56.6	58.29
	(3.63)	(3.89)	(3.50)	(3.86)
Annual Labor Earnings per 10.000	4.50	2.3	4.32	1.8
	(4.75)	(3.2)	(3.66)	(3.26)
Total Wealth per 10.000	14.27	21.48	12.57	24.57
	(47.10)	(57.21)	(20.60)	(69.1)
Share of Pensioner	0.68	0.21	0.63	0.17
	(0.46)	(0.41)	(0.48)	(0.38)
N	285	1,924	220	1,524

	Pre-Expansion		Post-Expansion	
Panel B: Non-expansion States	Treatment	Control	Treatment	Control
Working Full-Time	0.84	0.37	0.85	0.345
	(0.37)	(0.48)	(0.36)	(0.47)
Women	0.69	0.57	0.59	0.56
	(0.46)	(0.49)	(0.49)	(0.5)
Age	56.6	57.5	56.7	57.94
	(3.84)	(3.98)	(3.62)	(3.9)
Annual Labor Earnings	3.45	1.68	3.57	1.70
	(2.31)	(2.65)	(4.16)	(2.87)
Total Wealth per 10.000	9.74	12.94	10.5	14.15
	(18.4)	(34.48)	(17.12)	(36.2)
Share of Pensioner	0.63	0.19	0.59	0.18
	(0.48)	(0.39)	(0.49)	(0.38)
N	301	2,022	244	1,749

The sample is restricted to low-educated, childless adults, aged 50 to 64. The pre-expansion sample includes the years 2010 and 2012, while the post-expansion sample consists of the years 2014 and 2016. All monetary values are inflated to 2016 dollars using the consumer price index. The number of individuals is the number of unique individuals included in the baseline of equation (1). Standard deviations are in parentheses.

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 presents information on sample loss due to each restriction.[17]

III.4 Main Specification

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

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

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 $Treat_{ist}$. 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);[18] a set of dummies for self-reported health

[17] All appendices are available at the end of this article as it appears in JPAM online. Go to the publisher's website and use the search engine to locate the article at http://onlinelibrary.wiley.com.

[18] Individuals who skipped the HRS question regarding pension plans due to not having a current job 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 with the main findings.

on a scale of 1 to 5 from poor to excellent; body mass index; and a fifth-order polynomial in 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 of the main result are the same when standard errors are calculated at the household level and using the bootstrap method with multilevel clustering at household and state level and are based on 700 repetitions (see Appendix Table D1).[19] [20]

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

III.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 of 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.

[19] 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 that I apply block-bootstrapped standard errors by household and state groups based on 700 replications to alleviate this concern.

[20] All appendices are available at the end of this article as it appears in JPAM online. Go to the publisher's website and use the search engine to locate the article at http://onlinelibrary.wiley.com.

Table 4. Testing parallel trend for triple differences (DDD)

Full-time work	(1)
$Treatment \times Expansion \times 2010$	0.0008
	(0.05)
$Treatment \times Expansion \times 2014$	-0.06
	(0.04)
$Treatment \times Expansion \times 2016$	-0.11
	(0.06)
$Treatment \times Expansion$	0.05
	(0.05)
$Treatment \times 2010$	-0.11
	(0.03)
$Treatment \times 2014$	0.08^{**}
	(0.03)
$Treatment \times 2016$	0.11^{***}
	(0.04)
$Expansion \times 2010$	0.003
	(0.02)
$Expansion \times 2014$	0.013
	(0.018)
$Expansion \times 2016$	0.03
	(0.03)
Treatment	0.11^{***}
	(0.03)
Expansion	0.04
	(0.07)
2010	0.11^{***}
	(0.013)
2014	-0.09^{***}
	(0.013)
2016	-0.18^{***}
	(0.02)
Observations	8,269
Number of Clusters	4,682

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

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

To test for parallel trends in the triple difference, I conduct event study and estimate the following regression:

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

$I_{t=n}$ 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 coefficient interest is pre-2014 β_0^n estimates (β_0^{2010}). Table 4 illustrates the results; the coefficient of the triple interaction term (β_0^{2010}) is statistically indistinguishable from zero, which provides evidence that the parallel trend assumption is valid.

IV RESULTS

Table 5 presents the result of the triple-differences estimation. Column 1 shows the raw results without individual controls, and column 2 shows the baseline specification of equation (1).

The baseline specification indicates that Medicaid expansion leads to a fall of 8.4 percentage points in full-time work for the treatment group, of which 77 percent was due to transition to complete retirement.[21]

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.

[21] To analyze how much reduction in full-time work is due to individuals shifting to complete retirement and how much of it 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, respectively as the dependent variable. The result shows that Medicaid expansion leads to a fall of 6.5 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 77 percent of those who no longer work full-time replace their full-time work with shifting into complete retirement rather than transition to part-time work or self-employment (for details, see Appendix Table G1).
All appendices are available at the end of this article as it appears in JPAM online. Go to the publisher's website and use the search engine to locate the article at http://onlinelibrary.wiley.com.

Table 5. Triple differences estimates of the ACA Medicaid expansions on full-time work

Dependent variable: Full-time work	(1)	(2)
Treatment × Expansion × Post2014	-8.2**	-8.4**
	(4.06)	(3.93)
Post2014 × Expansion	1.97	2.49
	(2.56)	(2.44)
Treatment × Post2014	4.54	11.37***
	(4.61)	(4.19)
Treatment × Expansion	4.23	3.54
	(4.55)	(3.82)
Post2014	-17.7***	-18.46***
	(6.81)	(6.13)
Treatment	12.7	0.5
	(8.11)	(8.41)
Expansion	10.4	4.75
	(17.6)	(15.8)
Individual Controls	No	Yes
Year, age and individuals fixed effects	Yes	Yes
Year and age indicators × Treatment	Yes	Yes
State specific linear time trends	Yes	Yes
Observations	8,269	8,008
Number of clusters	4,682	4,583

Notes: This table presents triple-differences estimates of the effect of the ACA Medicaid expansion on full-time work. Early and late expansion states are excluded from the analysis (see Table 2). Individual controls include dummies for being married, divorced, or widowed; an indicator for being enrolled in a pension plan from the current job; a set of dummies for self-reported health on a scale of 1-5 from poor to excellent; body mass index; and a fifth-order polynomial in total wealth (Appendix Table H1 displays coefficient estimates of individual controls). Robust standard errors clustered at the level of the individual are in parentheses. All appendices are available at the end of this article as it appears in JPAM online. Go to the publisher's website and use the search engine to locate the article at http://onlinelibrary.wiley.com.

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

The finding is robust to a number of alternative specifications as well. I test:

- including early and late expansion states;
- utilizing low-income individuals group instead of low-educated individuals group;
- using an alternative control group of individuals with no RHI (individuals with no ESHI is excluded);
- estimating a hazard model of leaving full-time work and leaving any work;

A. Earnings and wage estimates

Having observed the reducing effect of Medicaid expansion on full-time work, one natural question can be asked whether this decline in full-time work is driven by labor supply or the labor demand channel. Utilizing a triple differences estimator provides primary evidence that the change in full-time work can be attributed to the labor supply shift. Because general shock to labor demand would impact both expansion and non-expansion states so it would cancel out between expansion and non-expansion states. In addition, demand shock that occurs only in expansion states would impact the labor outcome of both control and treatment groups. Thus, the triple-differences estimator should absorb demand shocks.

Table 6. The effect on annual labor earnings

Dependent Variable	Annual Labor Earnings		Wages	
	(1)	(2)	(3)	(4)
Treatment × Expansion × Post2014	-5,252	-5,614	1.524	0.95
	(4842)	(5015)	(3.46)	(3.6)
Individual controls	No	Yes	No	Yes
Year, age and individual fixed effects	Yes	Yes	Yes	Yes
Year and age indicators × Treatment	Yes	Yes	Yes	Yes
State specific linear time trends	Yes	Yes	Yes	Yes
Observations	8,269	8,008	4,638	4,509
Number of clusters	4,682	4,583	2,893	2,827

Notes: This table presents triple-differences estimates of the effect of 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 and 2 is annual earnings. The dependent variable of columns 3 and 4 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; a set of dummies for self-reported health on a scale of 1-5 from poor to excellent; body mass index; and a fifth-order polynomial in total wealth. Robust standard errors clustered at the level of the individual are in parentheses.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

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 shock 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 and 2 of Table 6 show the effect of Medicaid expansion on annual labor earnings, while columns 3 and 4 illustrate the effect on wages. There is no statistically significant effect on wages of the treated group. These findings suggest that the observed decrease in full-time due to the Medicaid expansion is driven by labor supply. It is also consistent with the results of Aslim's study (2020) that show transitions from full-time employment resulting from Medicaid expansions are due to personal (voluntary) reasons rather than economic, (involuntary) reasons

V PLACEBO TESTS AND ROBUSTNESS CHECKS

V.1 Placebo tests

To further assess the validity of my empirical approach, I conduct two placebo tests. First, I analyze the effect of Medicaid expansion for the population group that was not likely to be targeted by the expansion. I restrict the sample to high-educated adults. These individuals most likely have income above the Medicaid income eligibility threshold, so that the expansion should have no effect (or a limited effect) on the full-time work of higher-educated adults. I re-estimate equation (1) for this sample, and the result is presented in columns 1 and 2 of Table 7. As predicted, I observe no effect of the Medicaid expansions on the full-time work of higher-educated adults.

Table 7. Placebo tests

	Higher Education Adults		False Treatment Time (year 2010)	
Dependent Variable: Full-time work	(1)	(2)	(3)	(4)
Treatment × Expansion × Post2014	-2.9	5.3	7.9	-0.06
	(5.05)	(5.05)	(6.9)	(7.96)
Individual controls	No	Yes	No	Yes
Year, age and individual fixed effects	Yes	Yes	Yes	Yes
Year and age indicators × Treatment	Yes	Yes	Yes	Yes
State specific linear time trends	Yes	Yes	Yes	Yes
Observations	3,759	3,719	8,394	5,602
Number of clusters	2,191	2,182	5,309	3,611

Notes: This table presents triple-differences estimates of the effect of Medicaid expansion on full-time work for higher education adults (columns 1 and 2) and using pre-ACA years of the data with false treatment time (columns 3 and 4). Individual controls include dummies for being married, divorced, or widowed; an indicator for being enrolled in a pension plan from the current job; a set of dummies for self-reported health on a scale of 1-5 from poor to excellent; body mass index; and a fifth-order polynomial in total wealth. Robust standard errors clustered at the level of the individual are in parentheses.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Next, I restrict the analysis sample 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 that corresponds to $Post2014_t$ in equation (1). I re-estimate equation (1) as if Medicaid expansion occurred in 2010 rather than 2014. The result of this placebo test utilizing the pre-Medicaid expansion period is presented in columns 3 and 4 of Table 7. As expected, the result of the estimate is not statistically significant.

V.2 Robustness checks

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

A. 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.

Table 8. Robustness check: the inclusion of early & late expansion states

Dependent variable: Full-time work	(1)	(2)
Treatment × Expansion	-5.58*	-7.41**
	(3.20)	(3.16)
Individual controls	No	Yes
Year, age and individual fixed effects	Yes	Yes
Year and age indicators × Treatment	Yes	Yes
State specific linear time trends	Yes	Yes
Observations	10,236	9,950
Number of clusters	5,825	5,710

Notes: This table presents triple-differences estimates of the effect of the ACA Medicaid expansion on full-time work. Individual controls include dummies for being married, divorced, or widowed; an indicator for being enrolled a pension plan from the current job; a set of dummies for self-reported health on a scale of 1-5 from poor to excellent; body mass index; and a fifth-order polynomial in total wealth. Robust standard errors clustered at the level of the individual are in parentheses.

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

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.[22] Columns 1 and 2 of Table 8 presents the estimation results. The result in column 2 shows that Medicaid expansion leads to a decrease in full-time work by 7.4 percentage points for the treatment groups, which is consistent with the main findings.

B. 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 concern about self-selection into Medicaid by manipulating income level through working hours, I constrain the sample based on household income level in the year right before the expansion. The sample is restricted to childless individuals with annual household income equal to or less than \$50 thousand in 2012 .[23] This restriction yields a smaller sample size; nevertheless, it allows me to test the robustness of the main finding.

Table 9. Robustness checks: low income group

Dependent variable: Full-time work	(1)	(2)
Treatment × Expansion × Post2014	-13.2**	-12.5**
	(5.26)	(5.2)
Individual controls	No	Yes
Year and individual fixed effects	Yes	Yes
Year and age indicators × Treatment	Yes	Yes
State specific linear time trends	Yes	Yes
Observations	4,241	4,064
Number of clusters	1,893	1,843

Notes: This table presents triple-differences estimates of the effect of the ACA Medicaid expansion on full-time work. The sample is restricted to low-income (annual household income equal to or less than \$50 thousand in 2012) childless adults, aged 50 to 64 years old . Individual controls include dummies for being married, divorced or widowed; an indicator for being enrolled a pension plan from the current job; a set of dummies for self-reported health on a scale of 1-5 form poor to excellent; body mass index; and a fifth-order polynomial in total wealth. Robust standard errors clustered at the level of the individual are in parentheses.

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

[22] The new equation is as in following :

$$Y_{ist} = \alpha_0 + \beta_0(Treat_{ist} \times Expansion_{st}) + \beta_1 Expansion_{st} + \beta_2 Treat_{ist} + \delta_t \times Treat_{ist} + \delta_a \times Treat_{ist} + \alpha_a + \gamma_t + \mu_i + \beta_3 X_{ist} + \phi_{st} + \varepsilon_{ist}$$

[23] The Federal Poverty Level (FPL) changes based on the number of individuals in the household so does the Medicaid income threshold. I chose annual household income of \$50 thousand as an upper bond of annual household income level because that 138 percent 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.

Table 9 displays the estimate for childless individuals with annual total household income equaled to $50 thousand or less in 2012. The results are consistent with the main findings. It is important to note that the sizes of the estimated effect are relatively higher when the sample is constrained based on current household income level, which might be a sign of self-selection into Medicaid.

C. Alternative control group

Thus far, all the triple-differences regressions have used a control group of individuals who have RHI through their employer or spouses' plan or those who do not have ESHI from any source. The reason for the latter 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.

Table 10. Robustness checks: alternative control group

Dependent variable: Full-time work	(1)	(2)
Treatment $\times$ Expansion $\times$ Post2014	-7.97*	-8.05*
	(4.55)	(4.35)
Individual controls	No	Yes
Year, age and individual fixed effects	Yes	Yes
Year and age indicators $\times$ Treatment	Yes	Yes
State specific linear time trends	Yes	Yes
Observations	3,587	3,529
Number of clusters	2,247	2,217

Notes: This table presents the triple differences estimates of the effect of the ACA 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 a pension plan from the current job; a set of dummies for self-reported health on a scale of 1-5 from poor to excellent; body mass index; and a fifth-order polynomial in total wealth. Robust standard errors clustered at the level of the individual are in parentheses.

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

Applying the triple differences 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 and re-estimate equation (1) to test the robustness of the main results. Column 2 in Table 10 confirms that the main results hold using this alternative control group. Medicaid expansion leads to a fall of 8.05 percentage points in full-time work for treatment groups.

D. 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. In addition, column 2 of Table 5 shows that there is a significant positive change in non-expansion states for the treatment group, which might raise the concern that unobserved factors in non-expansion states are influencing 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.

Table 11. Robustness checks : accounting for the great recession

Dependent Variable: Full-time work	(1)	(2)
Treatment $\times$ Expansion $\times$ Post2014	-7.45*	-8.04**
	(4.15)	(3.98)
Individual controls	No	Yes
Year, age and individual fixed effects	Yes	Yes
Year and age indicators $\times$ Treatment	Yes	Yes
Year, Treatment indicators $\times$ States Indicators	Yes	Yes
Observations	8,269	8,008
Number of clusters	4,682	4,583

Notes: This table presents the triple difference estimates of the effect of the ACA 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 a pension plan from the current job; a set of dummies for self-reported health on a scale of 1-5 from poor to excellent; body mass index; and a fifth-order polynomial in total wealth. Robust standard errors clustered at the level of the individual are in parentheses.

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

The Great Recession did not equally affect all parts of countries or all demographics 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 1-2 in Table 11 show the results are similar to the main results.

F. Hazard models

The Cox proportional hazard model is an alternative to the main estimation with individual fixed effects as it also accounts for unobserved heterogeneity across individuals.

Table 12. Hazard models

Dependent Variables:	Full-time Work		Any Work	
	(1)	(2)	(3)	(4)
Treatment × Expansion × Post2014	1.12*	1.13*	1.26**	1.28**
	(0.07)	(0.071)	(0.15)	(0.148)
Individual controls	No	Yes	No	Yes
Observations	4,063	3,932	5,650	5,452
Number of Clusters	2,339	2,283	3,128	3,042

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: indicator of gender, race, marital status (married, widowed, divorced); indicator for being enrolled a pension plan from the current job; and indicators of self-reported health on scale of 1-5 from poor to excellent; body mass index; 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.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 12 presents estimates of the effect of Medicaid expansion on full-time work (columns 1 and 2) and any work (columns 3 and 4) utilizing 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 hazard estimation are in line with the main finding, with Medicaid expansion hazard of leaving full-time work increase by 13 percent for the treatment group. Similarly, the hazard of leaving any work increases with Medicaid expansion by 28 percent for the treatment group.

VI 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 employment decision of older Americans. The estimation suggests that Medicaid expansion lead to a decrease in full-time work by 8.4 percentage points for treatment groups, of which 77 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 RHI 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). However, my estimate is in contrast to the low or null estimates of early retirement incentives found in recent work 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 to 64 with the ACA 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 would more likely be affected by the expansion. In the case of Aslim, for example, he found a 10 percentage point increase in the probability of retirement of women when he uses the timing of the expansion decisions of states as an instrumental variable for actual Medicaid enrollment. Similarly, Wood (2019) utilizes variation across location, income, and time to identify newly eligible and previously eligible individuals, and finds that 8 percent decrease in labor force participation resulting from Medicaid expansion. In the current paper, I use individuals' health insurance status, education, and income information to isolate those most likely to have coverage affected by 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 heterogeneity in relation to demographic characteristics or health measures due to small sample size limitations.

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APPENDIX

A Data appendix

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

Table A1. 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 The impact of Medicaid expansion at selected quantiles of distribution of household out-of-pocket total medical spending

Table B1 illustrates the change in expenditure at every fifth quantile of the distribution of household out-of-pocket total medical spending of individuals who do not have ESHI associated with Medicaid expansion.

Table B1. The effect of Medicaid expansion on out-of-pocket total medical spending

Quantile	5th	15th	25th	35th	45th	55th	65th	75th	85th	95th
Expansion × Post2014	-599	-557	-545	-529	-511	-464	-386	-365	-324	-275
	(10653)	(8585)	(8043)	(7307)	(6545)	(4827)	(4558)	(5118)	(6691)	(8924)
Expansion	489	493	494	496	498	503	511	514	518	524
	(18170)	(14641)	(13719)	(12462)	(11164)	(8233)	(7774)	(8729)	(11413)	(15220)
Post2014	-53.4	-207	-249	-307	-371	-546	-828	-904	-1056	-1233
	(11072)	(8921)	(8359)	(7593)	(6802)	(5016)	(4736)	(5318)	(6953)	(9274)
Individual Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Standard errors are in parentheses.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

C The impact of employer mandate on working hours

Table C1 illustrate 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 defined as the periods before the employer mandate while year 2016 count as the period after the employer mandate.

Panel A of the Table C1 illustrates the results for full sample; panel B for those who are low-educated, childless and aged 50 to 64.

The difference-in-differences estimate 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.

Table C1. Difference-difference estimate 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 aged 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 employer mandate include year 2010, 2012 and 2014 while after employer mandate include 2016. Robust Standard errors are shown in parentheses.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

D Alternative approaches to calculating standard errors

Table D1 presents p-values for the triple differences coefficient estimate corresponding to alternative approaches to calculate standard errors. The main results cluster at individual level. I cluster standard error by household level and use multilevel clustering at household and states to account for within-household and within-state correlation in full-time work decision.

Table D1. Alternative approaches to calculating standard errors

	(1) Number of Clusters	(2) P-Values
One-Way Clustering		
Household Level	1,882 Households	0.039
*MultiWay Clustering**		
Household Level and State	1,882 Households and 37 states	0.04

Notes: This table presents the p-value for triple-differences estimates of the effect of the ACA Medicaid expansion on full-time work corresponding to a different clustering approach. The triple differences coefficient estimate is -8.4. Regression include indicator for treatment, expansion, post 2014, their interaction and all other covariates used in equation (1).

* The block bootstrap method, 700 block-bootstrapped replications, is applied with multiway clustering to account for over-rejection of the null hypothesis with a small number of clusters (Cameron and Miller, 2015; Donald and Lang, 2007; McCaffrey and Bell, 2006). Multiway clustering was implemented using the *reghdfe* Stata estimator, in which singleton groups are dropped from the regression sample (Correia, 2015).

E Excluding Wisconsin from the sample

Prior to 2014, Wisconsin Medicaid 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 percent of FPL. As a result, Wisconsin is the only non-expansion states without coverage gap.[24] Therefore, I treat Wisconsin as an experimental states so far.

To test the robustness of my findings, I remove Wisconsin from the sample and re-estimate 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 states.

Table E1 presents triple difference estimates that excluding Wisconsin. Columns 1 and 2 of Table E1 estimated on the sample excluding early and late expansion states, while columns 3 and 4 estimated the sample including early and late expansion states. Table E1 confirms that the qualitative results hold using the sample excluding Wisconsin. These findings indicate a 6.87 percentage

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

point decline in full-time work for the sample excluding early and late expansion states and 6.58 percentage point decline in full-time work for the sample including early and late expansion states.

Table E1. Triple differences estimates excluding Wisconsin

	Excluding Early & Late Expansion States		Including Early & Late Expansion States	
Dependent Variable: Full-time work	(1)	(2)	(3)	(4)
Treatment × Expansion × Post2014	-6.87*	-6.87*	-4.65	-6.58**
	(4.05)	(3.9)	(3.2)	(3.16)
Individual controls	No	Yes	No	Yes
Year,age and individual fixed effects	Yes	Yes	Yes	Yes
Year and age indicators × Treatment	Yes	Yes	Yes	Yes
State specific linear time trends	Yes	Yes	Yes	Yes

Notes: This table presents triple-differences estimates of the effect of Medicaid expansion on full-time work using the sample excluding Wisconsin. In columns 1 and 2, the sample is limited to states that expand Medicaid in 2014 under the ACA (no early and late expansion states), while in columns 3 and 4, 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 pension plan from the current job; a set of dummies for self-reported health on a scale of 1-5 from poor to excellent; body mass index; and a fifth-order polynomial in total wealth. Robust standard errors clustered at the level of the individual are in parentheses. The number of observations and clusters are not reported to keep confidential data secure.

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

F 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 F1 presents triple difference estimates of the effect of the ACA Medicaid expansion on full-time work excluding Arizona and Hawaii from the sample. The results are similar to the main findings.

Table F1. Triple differences estimates excluding Hawaii and Arizona

Dependent variable: Full-time work	(1)	(2)
Treatment × Expansion × Post2014	-9.04**	-8.3**
	(4.17)	(3.9)
Individual Controls	No	Yes
Year, age and individuals fixed effects	Yes	Yes
Year and age indicators × Treatment	Yes	Yes
State specific linear time trends	Yes	Yes

Notes: This table presents triple-differences estimates of the effect of the ACA 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; a set of dummies for self-reported health on a scale of 1-5 from poor to excellent; body mass index; and a fifth-order polynomial in total wealth. Robust standard errors clustered at the level of the individual are in parentheses. The number of observations and clusters are not reported to keep confidential data secure.

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

G The effect of Medicaid expansion on labor supply of elderly individuals

Table G1 present the effect of Medicaid expansion on full-time , par-time, any work, and self-employment.

Note that self-employment indicator 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 self-employed or someone else. There are some cases where answers are

Table G1. 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)	(1)	(2)	(1)	(2)	(1)	(2)
Treatment × Expansion × Post2014	-8.22**	-8.4**	1.1	1.9	-7.13*	-6.5 *	1.21	1.84
	(4.06)	(3.93)	(3.57)	(3.65)	(3.91)	(3.78)	(1.84)	(1.92)
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	Yes	Yes	Yes	Yes	Yes	Yes	Yes	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 effect of Medicaid expansion on 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 pension plan from the current job; a set of dummies for self-reported health on a scale of 1-5 from poor to excellent; body mass index; and a fifth-order polynomial in total wealth. Robust standard errors clustered at the level of the individual are in parentheses.

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

missing because respondents refuse to answer or do not know. Therefore, the sample size is smaller when the dependent variable is self-employed.

H The main result with coefficient estimates of individual controls

Table H1 display the triple difference estimates of the effect 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; a set of dummies for self-reported health on a scale of 1-5 from poor to excellent (poor is the reference category); body mass index (BMI); and a fifth-order polynomial in total wealth.

Table H1. Triple differences estimates of the ACA Medicaid expansions on full-time work

Dependent variable: Full-time work	(1)	(2)
Treatment × Expansion × Post2014	-0.082**	-0.084**
	(0.041)	(0.039)
Post2014 × Expansion	0.019	0.025
	(0.0256)	(0.0244)
Treatment × Post2014	0.045	0.113***
	(0.046)	(0.042)
Treatment × Expansion	0.042	0.0354
	(0.045)	(0.038)
Post2014	-0.17***	-0.18***
	(0.068)	(0.06)
Treatment	0.127	-0.005
	(0.081)	(0.084)
Expansion	0.104	0.047
	(0.175)	(0.157)
Married	-	-0.008
		(0.035)
Divorced	-	0.045
		(0.033)
Widowed	-	-0.018
		(0.043)
Pension	-	0.42***
		(0.0242)
Excellent	-	0.037
		(0.028)
Very good	-	0.0411*
		(0.024)
Good	-	0.028
		(0.021)
Fair	-	0.009
		(0.018)
BMI	-	-0.001
		(0.001)
Total Wealth*	-	0.00006
		(0.0017)
(Total Wealth)^2	-	9.81e-07
		(7.48e-7)
(Total Wealth)^3	-	-5.97e-9
		(3.63e-9)
(Total Wealth)^4	-	8.23e-12
		(5.68e-12)
(Total Wealth)^5	-	-3.24e-15
		(2.43e-15)
Year, age and individual fixed effects	Yes	Yes
Year and age indicators × Treatment	Yes	Yes
State specific linear time trends	Yes	Yes
Observations	8,269	8,008
Number of clusters	4,682	4,583

Notes: This table presents triple-differences estimates of the effect of the ACA 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.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$