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Labor Market and Outreach Effects of Medicaid Expansion Under the Affordable Care Act

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DEDICATION

To my guardian angels, Mehmet Aslım, Miyesser and Kemal Binli, for being the wind beneath my wings.

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Abstract

The Affordable Care Act's (ACA) Medicaid expansion is unique in terms of expanding coverage to adults without dependent children ("childless adults") and increasing community-based outreach to raise awareness about coverage options. This dissertation explores the labor market and outreach effects of the ACA's Medicaid expansion on childless adults and parents, respectively.

First chapter of the dissertation investigates the pre/post labor market implications of the ACA's Medicaid expansion for a population near the income eligibility cutoff. Using an arguably exogenous variation at this cutoff, I find that Medicaid enrollment increases for childless adults. This leads to an employment transition from full-time ($\geq$35 Hrs) to part-time employment ($<$35 Hrs) after the expansion. The employment transition is mainly driven by the increase in employment for working less than 20 hours. These findings support the presence of employment lock – individuals who are employed primarily to retain health benefits. Replication of existing studies that used difference-in-differences (DD) models with expansion states as the treatment yield no employment effects. The treatment group in these models, however, is large and heterogeneous.

In the second chapter, I assess the effect of the ACA's Medicaid expansion on the retirement decision of low-income adults aged 55 to 64 years. This chapter also focuses on childless adults, a

group that gained access to Medicaid coverage after the ACA. Using an instrumental variables (IV) model that exploits both the expansion decision of states and timing, I find that the probability of retirement increases by 14.8 percentage points for childless adults with Medicaid. The probability of retirement increases by 13.4 and 16.1 percentage points for men and women, respectively.

In the last chapter, I show the effect of information on Medicaid enrollment of previously-eligible parents ("woodwork effect"). Previous studies that analyze the changes in Medicaid take-up often ignore potential outreach effects. After controlling for the change in income eligibility limits, I find that woodwork effects are stronger in hard-to-reach communities that consist of low-educated, Hispanic, and non-white parent groups. In addition, woodwork effects increase enrollment in non-expansion states, particularly in states that have high search volume of Medicaid. Overall, the findings support the presence of information spillovers under the ACA's Medicaid expansion.

Chapter 1

Does Medicaid Expansion Affect Employment Transitions?

1.1. Introduction

The Affordable Care Act (ACA), also known as Obamacare, was passed into law by President Barack Obama on March 23, 2010. The ACA enacted major provisions on private and public health insurance to improve the health care status quo in the United States. In order to increase the quality and affordability of health insurance among low-income adults, the ACA proposed a nationwide expansion of the income eligibility limits to 138% of the federal poverty level (FPL). In 2012, however, the Supreme Court found this provision to be unconstitutional, and allowed states to opt-out of the program. Although the Supreme Court's decision was viewed as a major block to the ACA's goal on reducing the uninsured rate, most of the adults were able to gain coverage with the ACA's Medicaid expansion in 2014 (Frean, Gruber, and Sommers, 2017).

The literature has provided an extensive set of studies that investigated the impact of earlier

Medicaid expansions on health outcomes[1] and fiscal measures[2]. An important phenomena that gained more attention in recent years is the "employment lock", i.e., individuals working primarily to secure private health insurance.[3] A Medicaid-induced income effect or "windfall" may affect the labor market behavior of individuals by making the job search and/or reduction in working hours less costly.[4] In fact, the Congressional Budget Office estimated a reduction in the net total of hours worked by around 1.5% to 2% from 2017 to 2024 due to the ACA's Medicaid expansion (CBO, 2014).

This paper provides a novel contribution to the literature by investigating all possible employment transitions resulting from the ACA's Medicaid expansion for a population near the eligibility cutoff. The impact of the ACA's Medicaid expansion on employment, observed as a discontinuity at the cutoff, is captured by exploiting the changes in state eligibility rules for non-elderly adults without dependent children ("childless adults"), a group that gained access to Medicaid coverage after the ACA.[5] The ACA's Medicaid expansion is unique in terms of outreach efforts that involve mass marketing campaigns. Previously eligible adults may come out of the woodwork due to increased outreach, which is referred as the "woodwork effect" or "welcome-mat effect" (Sommers and Epstein, 2011, Frean, Gruber, and Sommers, 2017, Aslim,

[1]Piper, Ray, and Griffin (1990) found that Medicaid coverage in Tennessee did not improve the use of prenatal care, and there was no significant effect on birth outcomes including birth weight and neonatal mortality. Sommers, Baicker, and Epstein (2012) found a strong evidence of a reduction in mortality, improved self-assessed health, and access to care in three expansion states (see, also, **?**). Using a randomized experiment, Baicker et al. (2013) showed an improvement on health care utilization and self-assessed health after the public coverage expansion in Oregon.

[2]Fiscal pressures during recessionary periods and limited political clout among beneficiaries lead to some states to forgo Medicaid expansion (Coughlin et al., 1994 and Hoadley, Cunningham, and McHugh, 2004).

[3]Using the loss of public coverage in Tennessee as a natural experiment, Garthwaite, Gross, and Notowidigdo (2014) found an increase in employment that implies the presence of employment lock. Gooptu et al. (2016), however, found no employment lock under the ACA's Medicaid expansion. In addition, some of the studies investigated employment lock with respect to retirement effects under the ACA (Gustman, Steinmeier, and Tabatabai, 2016, Levy, Buchmueller, and Nikpay, 2016, Aslim, 2018).

[4]This outcome has been predicted by the economic theory of the allocation of time. Using a simple model of leisure, home production, and work, Gronau (1977) showed that increases in income increases leisure, reduces work in the market, and has no effect on home production.

[5]Following nine states have expanded eligibility for childless adults in 2013: Arizona (AZ), Colorado (CO), Connecticut (CT), Delaware (DE), District of Columbia (DC), Hawaii (HI), Minnesota (MN), New York (NY), and Vermont (VT).

2017). Parents and children are excluded in this study due to prior eligibility and "woodwork effects" that may bias the estimates on Medicaid enrollment and labor market outcomes.

A number of studies have explored the employment effects of the ACA's Medicaid expansion. These studies have found little to no effect on employment (see, for example, Gooptu et al., 2016, Leung and Mas, 2016, Kaestner et al., 2017), while contradicting to the findings of Dague, DeLeire, and Leininger (2017), Garthwaite, Gross, and Notowidigdo (2014), and Kim (2016) for Wisconsin (WI), Tennessee (TN), and Connecticut (CT), respectively.[6] The estimates for WI and CT suggested a reduction in employment by 12 percent resulting from the public insurance expansion, and an increase in employment by 6 percent in TN after a loss of public insurance coverage.[7] Duggan, Goda, and Jackson (2017), on the other hand, investigated the effects of multiple provisions of the ACA on labor market outcomes. The results indicated that middle-income individuals who are qualified for private insurance subsidies reduced labor supply.

Table 1.1 provides a comprehensive list of the recent studies and compares the findings with respect to earlier Medicaid expansions for different adult groups.[8] Although a couple of studies have investigated the expansions in 1970s, I restrict the sample of studies to post-2010 to focus on the impact of recent Medicaid expansions on employment.[9] Table 1.1 shows that employment effects of Medicaid expansion are mixed among adult groups with different signs and magnitudes. In particular, studies that used differences-in-differences (DD) models with expansion states as the treatment group found no employment effects (or limited effects) after the ACA's Medicaid expansion.

This paper distinguishes from prior studies on multiple aspects. First of all, I introduce

[6] Gooptu et al. (2016) also found no transition between part-time and full-time employment.

[7] Tuzemen and Nakajima (2014) supported the findings on employment lock using a general equilibrium model that accounts for worker and firm heterogeneity in labor markets.

[8] I only select empirical studies that investigate the impact of Medicaid expansions on adults. The studies related to the Children's Health Insurance Program (CHIP) are not in the scope of this paper.

[9] Some of the earlier studies have focused on potential changes in work incentives when there is an increase in Medicaid income thresholds (Yelowitz, 1995 and Meyer and Rosenbaum, 2001).

an alternative quasi-experimental design that exploits an arguably exogenous variation at the eligibility cutoff. The model incorporates the dichotomous treatment (i.e., Medicaid eligibility) as a deterministic function of the relative distance to the income eligibility cutoff, which is centered around 138% FPL.[10] It has important gains with respect to internal validity by comparing adults with similar observable characteristics around the income eligibility cutoff. This procedure is shown to yield credible results as it is in a randomized experiment (see, for example, Battistin and Rettore, 2008, Lee, 2008, Lee and Lemieux, 2010). Estimating the difference in discontinuities between pre- and post-2014, mainly to eliminate contempraneous shocks, yields the policy effect. This model is coined as "difference-in-discontinuities" in the paper.[11] This is the first study to use this approach to explore the employment effects of the ACA's Medicaid expansion on childless adults.

Secondly, the data set used in this study is from the Current Population Survey (CPS) between January 2010 and July 2016 with more years of data in the post-2014 period than the existing studies. Leung and Mas (2016) used the CPS between January 2010 and July 2015. Gooptu et al. (2016) used the same data up to March 2015. The largest years of data used among these studies are up to May 2016 by Kaestner et al. (2017). In addition, the existing studies have constructed simulated eligibility using a single data source that has information on both household income and outcome variables (see, for example, Cutler and Gruber, 1996, Gross and Notowidigdo, 2011, Sabik et al., 2017). A major contribution of this study is the construction of simulated eligibility using two data sets, March CPS and basic monthly CPS – the former has information on household income and the latter on labor market outcomes for each month. This overcomes the issue of using March CPS as the sole data source, which does not capture the monthly variation in eligibility

[10] The eligibility thresholds for childless adults living in DC and WI are 215% FPL and 100% FPL, respectively.

[11] Grembi, Nannicini, and Troiano (2016) used a similar empirical strategy to examine the impact of fiscal rules on taxes and budget deficits. The difference-in-discontinuities model is also used in the health economics literature (see, for example, Chay, Kim, and Swaminathan, 2010, Hu, Decker, and Chou, 2017).

limits and outcome variables.[12]

Using a difference-in-discontinuities model, I find a statistically significant jump in Medicaid enrollment at the income eligibility cutoff. This leads to an employment transition from full-time (≥35 Hrs) to part-time employment (<35 Hrs) after the ACA's Medicaid expansion. The employment transition is mainly driven by the increase in employment for working less than 20 hours. Subgroups are heterogeneous with respect to their response to employment. The estimates on employment transitions are robust to the inclusion of early expansion states, increased bandwidths, and different functional forms of the running variable. Falsification checks show no employment effects on non-expansion states. All of these findings imply the presence of employment lock prior to the expansion.

I replicate existing studies that used difference-in-differences (DD) models with expansion states as the treatment group. I use various samples that are not only comparable to the samples used in this study, but also to the existing studies. The estimates of DD model suggest no employment effects after the ACA's Medicaid expansion. Large and heterogeneous treatment groups may jeopardize the effect of ACA's Medicaid expansion on employment. The use of simulated eligibility measure, however, reduces these concerns by including a population that is more likely to be affected by the expansion in 2014.

In what follows, Section 2 provides a brief background information about the Medicaid program. Section 3 introduces the data, sample, and variables. The empirical strategy is discussed in Section 4. Section 5 presents the results. Section 6 concludes with a discussion.

[12]Indiana and Louisiana, for example, have increased their income eligibility limits on February 2015 and July 2016, respectively. In March CPS, all of the changes in eligibility limits are assumed to happen in March. If people respond immediately after the expansion, this would lead to an underestimation of the effect.

1.2. Background Information on the Medicaid Expansion

1.2.1. Information on Expansion States and Eligibility

Children, pregnant women, and low-income parents had access to Medicaid prior to the ACA.[13] Medicaid also covered elderly and disabled adults who receive supplemental security income (SSI). Non-elderly adults without dependent children ("childless adults"), however, were not covered by Medicaid due to the strict eligibility categories prior to 2010. The ACA, however, successfully eliminated categorical eligibility by allowing childless adults to access to care via Medicaid. Several states have expanded Medicaid to childless adults before the enactment of the ACA. Sommers, Baicker, and Epstein (2012) used the coverage expansions in Arizona (November 2001), Maine (October 2002), and New York (September 2001), and showed that the expansions increase access to care and reduce mortality for childless adults between 35 to 64 years of age.[14]

In the post-ACA period, states had the option to fully subsidize low-cost health insurance plans to adults with incomes below 138% FPL, and partially subsidize those between 138 - 400% FPL.[15] The Supreme Court, however, ruled out the reform to be an obligation for states (Supreme Court of the United States, 2012). Figure 1.1 depicts the expansion profile of states. As of July 2016, there were 32 expansion states and 19 non-expansion states. Wisconsin (WI) have a unique case among non-expansion states – coverage is fully subsidized to adults with incomes below 100% FPL under the BadgerCare program. In the following analysis, WI is treated as an expansion state due to the high income threshold. On the other hand, there are major differences in state characteristics among expansion states and non-expansion states (see Figure 1.1 for the geographic

[13] Medicaid eligibility thresholds for children and pregnant women, excluding CHIP, have varied between 100 - 133% FPL, whereas the cutoff have always been much lower for parents in non-expansion states (see Table 2.2).

[14] The authors, however, cautioned the readers about the external validity of their estimates because of limited sample of three expansion states.

[15] The subsidies above the eligibility threshold are not uniform. Low cost-sharing plans are available for those with incomes between 138 - 250% FPL.

variation in expansion and non-expansion states). It is crucial to account for these differences in order to disentangle the effect of the ACA's Medicaid expansion on employment. This important issue is further discussed in Section 3.3.

Table 2.2 summarizes the effective expansion date and the corresponding income eligibility levels for both childless adults and parents in each state. As evident from the effective dates, not all expansion states have expanded at the same time. In fact, seven states have expanded Medicaid after the main expansion on January 2014.[16] In addition, nine states have provided health coverage to low-income childless adults in 2013.[17] I have excluded these nine states in the benchmark analysis to eliminate concerns on woodwork effects that may confound the estimates on Medicaid enrollment and employment.[18] The adults who were previously eligible might take-up Medicaid after the information spillover under the ACA's Medicaid expansion (Sommers and Epstein, 2011; Sonier, Boudreaux, and Blewett, 2013).[19] However, I probe the robustness of the estimates to the inclusion of early expansion states.

Overall, the sample used in the analysis is composed of 23 expansion states with 138% FPL eligibility for childless adults, one (expansion) state with 100% FPL eligibility for childless adults, and 18 non-expansion states. An important issue regarding eligibility is whether individuals take up coverage or not. Next section discusses this issue thoroughly using administrative data.

[16]Three recently adopting states are Alaska (AK), Montana (MT), and Louisiana (LA). The effective implementation of the policy in AK is September 1, 2015. MT's and LA's effective implementation dates are January 1, 2016 and July 1, 2016, respectively.

[17]Nine early expansion states are: AZ, CO, CT, DE, DC, HI, MN, NY, and VT.

[18]Leung and Mas (2016) excluded 13 states that had limited benefits in 2013, and their DD estimates are robust to the exclusion.

[19]These concerns could be addressed if the researcher have access to information on health insurance prior to 2014. The current data set has limitations in observing Medicaid enrollment, and woodwork effects are not in the scope of this paper.

1.2.2. Medicaid Enrollment

It is important to explore the changes in Medicaid enrollment in order to understand the effect of the reform on labor market outcomes. If eligible adults do not take-up Medicaid, it is not possible to attribute the changes in labor supply to the ACA's Medicaid expansion. The CBO has predicted the increase in Medicaid and CHIP enrollment for newly eligible individuals to be 13 million in 2016 with an increase to 16 million after 2019 (CBO, 2015).[20] Prior to these estimations on the effect of the ACA's Medicaid expansion, the consensus in the literature with respect to the participation of new eligible individuals was more than 10 million (Sommers et al., 2012a). These estimates suggested a relatively high marginal take-up after the ACA's Medicaid expansion.[21] Studies that simulated the effect of Medicaid expansion showed a reduction in the number of under-insured people by 70 percent and the number of uninsured by 20 million after the enactment of the ACA (Schoen et al., 2011, Parente and Feldman, 2013).

According to the Centers for Medicare & Medicaid Services (CMS), more than 73 million enrolled for Medicaid and CHIP in July 2016 with a 28.72% enrollment growth rate relative to the average of July - September 2013.[22] There is an upward trend in the take-up rate of Medicaid after 2014, which could be explained by increased outreach under the ACA (Frean, Gruber, and Sommers, 2017). The earlier studies, on the other hand, showed modest take-up rates of Medicaid prior to the ACA.[23] Using the CPS from 2007 to 2009, for example, Sommers and

[20]Figure A1 shows the total enrollment growth rate of Medicaid and CHIP. Medicaid enrollment is higher in expansion states relative to non-expansion states. The recent bending of the curve could be due to a possible convergence to steady-state.

[21]It is important to distinguish between marginal take-up and average take-up under the ACA's Medicaid expansion. Marginal take-up refers to those who are newly eligible, whereas average take-up refers to all eligible individuals.

[22]The numbers of enrolled are 51,557,834 and 21,909,593 in expansion states and non-expansion states, respectively. The average enrollment in July - September 2013 and the enrollment in July 2015 exclude Connecticut and Maine. The data could be accessed online through http://www.medicaid.gov.

[23]In previous studies, the estimates of average Medicaid take-up among adults ranged between 52% to 81.3% prior to the ACA (Sommers et al., 2012a).

Epstein (2010a) found the average take-up Medicaid rate to be 61.7%. Using the 2009 American Community Survey (ACS), Kenney et al. (2012) found the average Medicaid take-up rate to be 67%. Although the national averages were relatively low prior to the ACA, Massachusetts have managed to increase Medicaid average take-up rate to 80% after 2006 (Sommers and Epstein, 2010a). Massachusetts, however, reformed health care in 2006, which fully took place in mid-2007, and has been viewed as a for model the ACA. These findings clearly suggest that health care reforms that implement public coverage expansions are associated with increased (average and marginal) take-up rates.

I use administrative data from the Centers for Medicare & Medicaid Services (CMS) through the Medicaid Budget and Expenditure System (MBES) to analyze the average and marginal take-up rate of Medicaid in the post-2014 period. Using the percent changes from January 2014, Figure B2 shows that the average take-up rate is smoother than the marginal take-up rate. Newly eligible enrollees are not only childless adults, but also those who were not eligible with the previous income thresholds. In MBES data, it is not possible to distinguish between parents and childless adults. Using the same data, Kaiser Family Foundation found the newly eligible enrollment for Medicaid to be around 12 million in 2016. I further support this analysis by using a national survey, the Annual Social and Economic Supplement (ASEC) of the CPS, to test whether there is a discontinuity in Medicaid enrollment at the income eligibility cutoff for childless adults. These findings are presented in Section 3.5.

1.3. Data, Sample, and Variables

1.3.1. Data and Sample

The main data set used in the analysis comes from the basic monthly Current Population Survey (CPS). Supplemental data sets on Medicaid enrollment and eligibility rules that vary by state and year are obtained from the Kaiser Family Foundation and the Centers for Medicare & Medicaid Services. The CPS monthly data contain all of the relevant variables on household demographics and labor market outcomes. There is a multistage stratification for the sample households, where a household is interviewed by 4 months consecutively, then followed by a 8 months break, and finally they are interviewed for another 4 months. Most importantly, quick release of the data allows researchers to analyze immediate impacts of a policy change.[24]

The sample period is from 2010 to July 2016 with more years of data after the expansion in 2014 compared to the studies given in Table 1.1. As discussed earlier, the sample includes 24 expansion states and 18 non-expansion states. The remaining nine early expansion states are dropped due to the confounding effects on Medicaid enrollment. Since the group of interest is childless adults, I restrict the sample to those who do not have an own (and/or related) children under the age of 18 living in the household. This reduces the sample size by 57.8 percent from 1,263,469 to 533,808 observations. Childless adults who are below 26 years of age could remain on parent's coverage through the ACA's dependent coverage mandate[25], and those who are above 64 years of age are qualified for Medicare. In addition, those who are in the armed forces are eligible for HMO-type military health-care plans – TRICARE. In order to mitigate the potential bias

[24] An alternative data set is the American Community Survey (ACS), but the main limitation is the lagged release of the data. As of the writing of this paper, the ACS only had one year of data for the post-expansion period.

[25] Antwi, Moriya, and Simon (2013), on one hand, found an evidence supporting employment lock that results from high take-up rates of parental coverage. Bailey and Chorniy (2016), on the other hand, found no evidence of job lock, measured as job mobility, for young adults.

resulting from dependent's coverage, Medicare, and TRICARE, the sample is restricted to non-institutionalized civilized adults who are aged 27 to 64.[26] The sample size reduces from 533,808 to 315,522 observations, which is a 40.9 percent reduction. These are the two major restrictions applied to the sample, which are common in existing studies (see, for example, Leung and Mas, 2016).

The household size is restricted to less than seven in order to prevent issues regarding multiple families. The sample size reduces by 2,835 observations. The sample restrictions defined above are also applied to the ASEC supplement ("March CPS") when used for the simulation on eligibility. The employment measures are constructed using the information on working hours. Note that 5 percent of the sample have varying working hours, which are coded as "hours vary". For childless adults who are working part-time and have varying working hours, I impute the weighted average of those who work less than 20 hours and 20-34 hours. This weighted average is calculated to be 22.76 hours for childless adults who work part-time.

1.3.2. Eligibility Simulation

The most important component of simulated eligibility is household income given that eligibility is a function of income.[27] In order to simulate eligibility, I exploit the information on household income given in March CPS. I use the sample between 2011 to 2013 in the March CPS, excluding nine months before the ACA's Medicaid expansion, to avoid any anticipated changes in household income.[28] The increase in income threshold to 138% FPL could create incentives to manipulate household income to become eligible for Medicaid. In addition, the motivation behind using a

[26]Since an individual can remain on parent's insurance plan until December 31 of the year he/she turns 26, it is more accurate to have a sample starting from the age 27.

[27]When applying for Medicaid, step child, adopted child, foster child, brother, sister, niece, nephew, and grandchild could be included in the household counting under the tax filer's rules. In addition, non-relatives who live for an entire year in the tax filer's house could be claimed as tax dependents. Those who are claimed as tax dependents are included in the tax filer's household counting.

[28]The remaining sample restrictions are the same as those applied to the basic monthly CPS (see Section 1.3.1).

sample after 2010 is to avoid any confounding effects of the Great Recession.[29] The specification used for the data generating process is defined as follows:

$$y_{it_ms} = \gamma_0 + X_i^{'}\gamma_1 + \tau_{t_m} + \tau_{t_m}^2 + \xi_s + \tau_{t_m} * \xi_s + \epsilon_{it_ms} \tag{1.1}$$

where y is household income for individual i at time t_m (year and month) in state s.[30] X includes cell blocks on age, race, gender, marital status, educational attainment, and household size.[31] Linear time trend is captured by τ_{t_m} and $\tau_{t_m}^2$ is trend-squared.[32] State fixed effects are ξ_s, and $\tau_{t_m} * \xi_s$ is an interaction that captures state-specific linear trends. The error term is ϵ. The coefficients obtained from March CPS are used in the basic monthly CPS to get $\hat{y}_{it_ms}$, which is denoted as the simulated household income ($SHHI_{it_ms}$).

In order to determine eligibility for Medicaid, poverty thresholds provided in Table A1 are used.[33] Thus, the formula used to calculate simulated eligibility has the following first step:

$$P_{it_ms} = \frac{SHHI_{it_ms}}{FPL_{ts}} \times 100 \tag{1.2}$$

where FPL_{ts} is the federal poverty level (FPL) that varies by year t and state s. The variables in Equation (1.2) also vary by household size (h). For simplicity in notation, h is suppressed hereinafter. P_{ihs} is income as a percent of FPL for individual i at time t_m (year and month) in state s. The second step is constructed as follows:

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[29]Based on the NBER Recession Indicators for the United States, the Great Recession spans the period between December 2007 and June 2009 (see `https://fred.stlouisfed.org/series/USREC`). Due to slow recovery and high unemployment rates in 2010, I start the sample period as early as 2011.

[30]Note that household income could be negative due to accumulated debt.

[31]When simulating Medicaid eligibility, Golberstein and Gonzales (2015) use cells on age, sex, marital status, number of children, race, and educational attainment.

[32]Note that linear time trend is defined for year-month pairs: $\tau_{t_m} = \{1, \ldots, 79\}$.

[33]For example, 100% FPL in 2015 for a single household is \$11,770 and 138% of FPL is \$16,242 (see Table A1).

$$E_{it_m s} = I\{P_{it_m s} \leq 138\} \tag{1.3}$$

where $E_{it_m s}$ is eligibility and $I\{.\}$ is an indicator variable taking the value 1 if $P_{it_m s} \leq 138$ and 0 otherwise.[34] This simulated eligibility measure accounts for both individual- and state-level differences in household income by using state-specific rules for eligibility. A similar approach is used by Dave et al. (2015) to capture the heterogeneity in the distribution of income using a state-specific sample. Cutler and Gruber (1996) construct simulated eligibility using each cell of observable characteristics for a nationally drawn sample and use it as an instrument for actual eligibility.[35] Pohl (2014) uses a similar simulated eligibility measure as a proxy for actual eligibility rather than using the IV method due to concerns on inconsistent estimates. A major contribution of this study is the use of two different data sets, March CPS and basic monthly CPS, to construct simulated eligibility. This overcomes the issue of using March CPS as the sole data source, which does not capture monthly variation in the changes in income thresholds and the outcome variables.

The running variable, $d_{it_m s}$, is constructed by centering $P_{it_m s}$ around zero. This is defined as follows:

$$d_{it_m s} = P_{it_m s} - R_s, \tag{1.4}$$

where R_s is the state eligibility rule, which is 100% FPL for WI and 138% FPL for the remaining expansion states. Although the sample used for the simulations reduces the possibility of systematic manipulation, I probe the continuity of the running variable using the density test proposed by McCrary (2008). Any non-random sorting around the cutoff biases the

[34]The indicator function is $I\{P_{it_m s} \leq 100\}$ for WI.

[35]There are many studies following the approach suggested by this study. Gross and Notowidigdo (2011) use information on state, year, household income, number of children, and gender to construct Medicaid eligibility. Sabik et al. (2017) use state, year, household income, and family size to construct Medicaid eligibiliy.

estimates. Figure 1.2 clearly shows no evidence of systematic manipulation around the cutoff prior to the policy.[36] The preferred bandwidth is ±4% FPL, which is the largest bandwidth that has smooth covariates around the cutoff. There is a tradeoff between sample size and covariate smoothness: as bandwidth increases covariates are less smooth around the eligibility cutoff. The preferred bandwidth in this paper is selected based on covariate smoothness and the largest sample that is possible. I probe the robustness of the estimates to bandwidth selection.

Since self-reported income is a noisy measure and I impute household income ("$SHHI$") using Equation (1.1), the running variable ($d_{it_m s}$) could be confounded by measurement error. In order to reduce concerns on measurement error, I first employ a graphical analysis on percent Medicaid enrollment around the percent income threshold and second I estimate the jump in Medicaid enrollment around a ±4% FPL and ±8% FPL bandwidth. If eligibility is assigned incorrectly due to measurement error, there would be no evidence of a jump in Medicaid enrollment. Figure 1.3 shows the average Medicaid enrollment within equally-sized bins. The vertical line corresponds to 138% FPL. The figure shows that Medicaid enrollment converges to zero as income increases. It is clear that Medicaid enrollment is higher below the income eligibility cutoff. The estimates on Medicaid enrollment also show a positive and statistically significant jump at the income eligibility cutoff (see Table 1.4). When preferred bandwidth is used in column (1), Medicaid enrollment increases by 27.5 percentage points. The findings are robust to increasing bandwidths. A detailed discussion on bandwidth selection is provided under robustness checks.

1.3.3. Descriptive Statistics

Table 1.3 provides descriptive statistics on outcome and control variables in expansion states for both eligible and non-eligible adults defined by Equation (1.3). The sample is stratified by pre-

[36]There is also no evidence of discontinuity when the sample is not restricted to pre- and post-period.

and post-2014 period to observe possible trends in outcomes. Panel A shows the mean of the outcome variables including the employment measures. The total number of observations used in the study can be calculated by adding the observations in each column. Given the bandwidth, full sample size is captured by those who are in the labor force (N=4,140), and the remaining outcome variables are sub-samples of those who participate in the labor force. The base group for labor force participation includes adults who are not in the labor force. The variable on employed captures the share of employed relative to unemployed (N=3,111). Part-time employment categories (<20 Hrs, 20-34 Hrs, and <35 Hrs) are relative to full-time employment (≥35 Hrs), and the variables capture the working population around the cutoff (N=2,796). Note that the Bureau of Labor Statistics (BLS) defines part-time employment as working less than 34 hours per week.

Before the policy change, part-time employment (<35 Hrs) is relatively higher for non-eligible adults. With the ACA's Medicaid expansion, there is a significant change in employment, where eligible adults reduce working hours by transitioning into part-time employment (<35 Hrs) and non-eligible adults increase full-time employment (≥ 35 Hrs) relative to part-time employment (<35 Hrs). After all the employment transitions, part-time employment (<35 Hrs) increases by 1 percentage point at the cutoff for eligible adults. The change in part-time employment (<35 Hrs) is driven from the increase in employment of 20 working hours for eligible adults that leads to a 2.9 percentage points difference relative to non-eligible adults. The decline in the employment category for 20-34 hours of work also suggests that the dominating effect is the increase in employment for the bottom portion of working hours – working less than 20 hours of work. Both labor force participation and the share of employed increase for eligible and non-eligible adults with the latter being greater in magnitude. Overall, eligible adults have higher part-time employment (both <35 Hrs and <20 Hrs), and lower labor force participation and share of employed relative to non-eligible adults. All of these changes support the presence of employment lock prior to the ACA's Medicaid expansion.

Panel B introduces the control variables used in the benchmark model, which include both individual- and state-level characteristics. Demographic characteristics are comparable among eligible and non-eligible adults in the pre- and post-2014 period, respectively. The base group for the variables on education is having a college education or more. Since educational attainment is positively correlated with income, non-eligible adults have higher educational attainment than eligible adults. All of the composition changes among eligible and non-eligible adults in the post-period follow a similar pattern. When the average of a control variable increases (or decreases) after the expansion for eligible adults, it also increases (or decreases) for non-eligible adults with the exception of the category on separated adults.

After differencing out the composition changes in the pre- and post-2014 period, no statistically significant jumps are observed around the income eligibility threshold. This is discussed further under covariate smoothness test in Section 1.4.1. In addition, the changes in state unemployment rate capture the spillover effects of the Great Recession. The state unemployment rate is obtained by the U.S. Bureau of Labor Statistics and it is seasonally adjusted. On the other hand, state GDP is a percent change from preceding quarters, which is measured in chained dollars. The data on state GDP are publicly available through the U.S. Bureau of Economic Analysis. Note that state time-varying effects are crucial in terms of capturing macro-level differences in expansion and non-expansion states.

The visual representation of discontinuities resulting from the policy change is illustrated in Figure 1.4.[37] The discontinuity plots are centered around zero and the running variable represents the percent FPL relative to the cutoff. The scatter plot represents the mean of the outcome variable within equally-sized bins and the fitted lines are local linear regressions using triangular

[37] Discontinuity plots are commonly used to support RD models to visually identify the policy effect (see, for example, Carpenter and Dobkin, 2009, Card, Dobkin, and Maestas, 2009).

kernel on both sides of the cutoff. The comparison of discontinuities is made for expansion states in pre- and post-period of the policy. The test on discontinuities for non-expansion states is conducted later as a falsification test. For the discontinuity plots, I focus on the transition between part-time employment (<35 Hrs) and full-time employment (≥35 Hrs). The main policy effect is captured by taking the difference in discontinuities.

The findings of discontinuity plots are similar to those shown in Table 1.3. Relative comparison of the discontinuities suggests that part-time employment (<35 Hrs) increases relative to full-time employment (≥35 Hrs) for those who are eligible for Medicaid after the ACA's Medicaid expansion. Moreover, this increase is mainly driven from the increase in the bottom portion of working hours, which is working less than 20 hours.[38]

In the next section, I introduce the benchmark model that incorporates both simulated eligibility and the variables introduced in Panels A and B of Table 1.3.

1.4. Methods

In this section, I present an RD model that takes the treatment as a deterministic function of the covariate given the upward trend in the Medicaid take-up rate, which is further tested using Medicaid enrollment in March CPS.[39] The ignorability or unconfoundedness assumption holds by design. This specification is preferred due to its strong foundation on internal validity by comparing similar populations near the cutoff.[40] A standard RD model is defined as follows:

[38]The discontinuity plot for 20-34 working hours does not show any significant increase for those below the cutoff. This plot is available upon request.

[39]This paper does not use March CPS as the sole data source due to its limitations on constructing simulated eligilibity (see Section 1.3.2). Note also that the questions on health insurance in March CPS are redesigned at the time of the ACA's Medicaid expansion and hence could bias the estimates on enrollment when used in a fuzzy RD design.

[40]See Imbens and Lemieux (2008) for a detailed discussion on the use of RD models in economics.

$$y_{it_m s} = \beta_0 + \beta_1 E_{it_m s} + g(d) + \beta_2 E_{it_m s} * g(d) + X'_{it_m}\beta_3 + \delta_m + \gamma_t + \xi_s + [\psi_{t_m s}] + v_{it_m s} \quad (1.5)$$

where y is labor market outcomes (and Medicaid enrollment) for individual i at year and survey month t_m in state s. $E_{it_m s}$ is the simulated eligibility defined by Equation (1.3). The running variable, denoted as d, is obtained through Equation (1.4). The center of d corresponds to 138% FPL, which is normalized to zero. The functional form of d is captured by $g(d)$ - linear, quadratic and cubic functions. The robustness of the estimates with respect to the functional form of d is tested in the results section. X is composed of control variables including age, age-squared, race, gender, marital status, and educational attainment. The period effects, defined as month and year effects, are δ_m and γ_t, respectively. State fixed effects are defined as ξ_s, and $\psi_{t_m s}$ is state timing-varying effects including the state unemployment rate and GDP growth rate.[41] The error term is $v_{it_m s}$.

A standard RD model could show the discontinuity in expansion states after the policy change. A possible discontinuity in both non-expansion states and expansion states in the pre-2014 period could be solely treated as falsification checks. The visual illustrations of discontinuities, however, in the expansion states show significant jumps in the pre-2014 period for some of the outcome variables (see Figure 1.4).[42] Thus, taking the difference in discontinuities accounts for possible contemporaneous shocks that may vary by household income. Thus, Equation (1.5) is modified by including "*Post*" interactions to difference out those possible contemporaneous shocks. This model can be written as follows:

[41] Note that the time dimension of $\psi_{t_m s}$ accounts for the monthly and quarterly variation in the unemployment rate and GDP growth rate, respectively.

[42] These jumps are not due to any selection around the cutoff since this is checked in the paper through McCrary's manipulation test and covariate smoothness test.

$$
\begin{aligned}
y_{it_ms} =& \alpha_0 + \alpha_1 E_{it_ms} + g(d) + \alpha_2 E_{it_ms} * Post_{t_ms} + \alpha_3 g(d) * Post_{t_ms} \\
&+ \alpha_4 E_{it_ms} * g(d) * Post_{t_ms} + X_i^{'} \alpha_5 + \delta_m + \gamma_t + \xi_s + [\psi_{t_ms}] + v_{it_ms}
\end{aligned} \tag{1.6}
$$

where $Post$ varies by state (s), year and month (t_m), and takes the value 1 after the expansion date and 0 otherwise (see Table 2.2). The rest of the variables is the same as those discussed in Equation (1.5). This is the benchmark model of the paper with β_2, denoted as $Eligibility * Post$ in the tables, being the coefficient of interest. This coefficient captures the change in labor market outcomes at the eligibility cutoff before and after 2014. Since differencing out eliminates contemporaneous shocks, any change at the cutoff is attributed to the ACA's Medicaid expansion. This discontinuity is expected to be statistically insignificant for non-expansion states since they are not (directly) affected from the reform. For the benchmark model, the preferred bandwidth is the largest that passes the covariate smoothness test, which is $\pm 4\%$ FPL. The relationship between the potential outcomes and the running variable has to be smooth to interpret any resulting discontinuity as the average treatment effect. The standard errors are bootstrapped with 400 replications and clustered by state.[43]

In this paper, the validity of model assumptions and the robustness of the estimates are tested using the following approaches: i) density test for the running variable d (discussed in Section 1.3.2); ii) covariate smoothness test around the eligibility cutoff; iii) testing the robustness of the estimates to different bandwidths; iv) changing the functional form of the running variable; and v) including early expansion states in the analysis.

[43]The findings are robust to increasing bootstrap replications. In addition, I exclude significance at 10 percent to avoid any issues on sensitivity.

1.4.1. Covariate Smoothness Test

It is crucial to test whether the population composition is similar around the eligibility cutoff for a given bandwidth. Non-random sorting of a certain group near the cutoff may bias the estimates on eligibility. The main motivation here is to show that there is no selection on either side of the eligibility cutoff with respect to observable characteristics given in Table 1.3. If observable characteristics are smoothly distributed around the eligibility cutoff, this would imply that unobservable characteristics are also smoothly distributed around the eligibility cutoff. In this case, the concerns on internal validity with respect to omitted variables would be reduced and hence any discontinuity could be interpreted as the causal effect of Medicaid expansion.

In order to test for this formally, I run the regression model given in Equation (1.6) by changing the outcome variables as the control variables. Since the policy is not effective in non-expansion states, I examine the eligibility profile of demographic characteristics only in expansion states. The findings, presented in Table 1.5, show no evidence of a discrete jump at the eligibility cutoff. Thus, the preferred bandwidth is the largest bandwidth that passes the covariate smoothness test. It is crucial to note that the increase in bandwidth still yields statistically significant employment effects but there is a tradeoff in comparing similar individuals. This implies that sample size increases as bandwidth gets wider but at the cost of failing the smoothness test for various covariates. In cases of non-random sorting, it is not possible to claim that the sole factor causing the jump is the ACA's Medicaid expansion.

1.5. Results

This section provides the findings from the benchmark and subgroup analysis, covariate smoothness test, robustness and falsification checks, and the DD model to replicate existing studies.

1.5.1. Benchmark Analysis in Expansion States

In this section, I focus on the estimates from Equation (1.6) for expansion states using Tables 1.4 and 1.6. In Table 1.6, each panel denotes a separate regression for the given outcome variable.[44] The estimates show no effect of the ACA's Medicaid expansion on labor force participation, the probability of being employed, and employment for 20-34 working hours. The main effect, however, is observed as an employment transition from full-time ($\geq$35 Hrs) to part-time employment (<35 Hrs). In column (3), there is an increase in part-time employment (<35 Hrs) by 13.7 percentage points relative to full-time employment. It is also evident that employment (<20 Hrs) increases (9.4 percentage points) after the ACA's Medicaid expansion.

The findings imply that the increase in part-time employment is mainly driven from the transition between full-time employment and employment with less than 20 working hours. This is consistent with the priori that adults who are experiencing employment lock will respond to incentives created by the ACA's Medicaid expansion. The main incentive is the Medicaid-induced income effect that makes both the job search and/or employment transitions less costly.[45] For the remainder of the paper, I only focus on the most inclusive regression that has control variables, year and month effects, state fixed effects, and state-time varying effects.

1.5.2. Subgroup Analysis in Expansion States

In this section, I explore the heterogeneity of employment effects across subgroups in expansion states. Female and low-educated (HS or less) adults increase their employment (<20 Hrs) by 14

[44] As discussed earlier, part-time employment is a subgroup of adults who are employed, and those who are employed are a subgroup of adults who are in the labor force. The total number of observations for each outcome are denoted in Section 1.3.3.

[45] In addition, I do not find an increase in self-employment after the expansion. The findings on self-employment are available upon request.

and 21.1 percentage points, respectively. For adults who are married once, there is a relatively stronger effect on part-time employment (<35 Hrs) with a statistically significant increase by 19.6 percentage points.[46] The increase in part-time employment (≥35 Hrs) is again driven by the increase in employment for working less than 20 hours (11.8 percentage points). This finding is consistent with the benchmark analysis that shows a statistically significant increase in employment for working less than 20 hours. This implies that subgroups respond to the ACA's Medicaid expansion by reducing working hours.

There is no evidence of a transition from employment to unemployment across subgroups. In fact, the probability of being employed increases by 10.7 percentage points relative to the probability of being unemployed for adults who are married once. There is also no evidence of a discontinuity in labor force participation and employment for 20-34 working hours across subgroups. There are no effects observed for males, high-educated adults (more than HS), and never married adults. Additionally, there is no evidence of an employment effect for different age groups (27-49 vs. 50-64). Overall, the employment effects are concentrated among females, low-educated adults (HS or less), and adults who are married once.

1.5.3. Robustness and Falsification Checks

The first robustness check is on including early expansion states in the sample. Since the analysis excludes relatively large states, I test whether the main finding prevails. The inclusion of early expansion states increases the total number of expansion states to 33 and the non-expansion states remain the same. It is observed that part-time employment (<34 Hrs) increases by 14.3 percentage points relative to full-time employment (≥35 Hrs), where the main transition is for working less than 20 hours. These findings are consistent with the benchmark analysis. In addition, the

[46] The group on married once includes married, divorced, windowed, and separated adults.

estimates imply that the effect of ACA's Medicaid expansion on employment is not sensitive to the exclusion of early expansion states.

Another important robustness check is on the selection of bandwidths. The initial selection of bandwidth relies on covariate smoothness, where I choose the largest bandwidth possible. As discussed earlier, there is a tradeoff between sample size and covariate smoothness as the bandwidth gets wider. It is more likely to have a selection around the cutoff that would fail the test on covariate smoothness. For the studies that use RD models, it is common to double the preferred bandwidth as a robustness check (see, for example, Black, Galdo, and Smith, 2007, Schmieder, Von Wachter, and Bender, 2012). In this case, doubling the bandwidth yields $\pm 8\%$ FPL around the zero-threshold. As expected, variance gets smaller with the increased bandwidth and the results on employment (<34 Hrs and <20 Hrs) are highly significant and still have a positive sign.[47]

The final robustness check is on the functional form of the running variable, which is defined as $g(d)$ in Equation (1.6). I provide the estimates on employment for quadratic and cubic running variables. In either case, the estimates suggest a transition from full-time employment to part-time employment, which is consistent with the benchmark analysis. It is important to note that the estimates get smaller as the order of the polynomial increases. Gelman and Imbens (2017) showed that high-order polynomial in an RD setup could be misleading due to poor properties on inference, especially on estimating confidence intervals. If the researcher is confident about the functional form, which is rarely the case, then using a high-order polynomial could be a reasonable method.

[47] I also check the robustness of the estimates by changing the preferred bandwidth by $\pm 1\%$ FPL until I have $\pm 10\%$ FPL around the eligibility cutoff. The employment effects for these different bandwidths are fairly robust but I again caution the readers about covariate smoothness. Note that the estimates are available upon request.

1.5.4. Difference-in-Differences

The benchmark findings on employment transition, particularly the increase in part-time employment, contradict to the studies that use the same data source with a different identification method (see, for example, Gooptu et al., 2016, Leung and Mas, 2016, Kaestner et al., 2017). In order to replicate existing studies, I estimate the following DD model:

$$y_{imts} = \theta_0 + \theta_1 Expansion + \theta_2 Expansion * Post + X_i^{'}\theta_3 + \delta_m + \gamma_t + \xi_s + [\psi_{mts}] + v_{imts} \quad (1.7)$$

where $Expansion$ is a dummy variable taking the value 1 if a state is an expansion state and 0 otherwise. The remaining variables are the same as those discussed for Equation (1.6). In this model, θ_2 is an intent-to-treat estimate that shows the changes in labor market outcomes in expansion states after 2014. Robust standard errors are clustered by state, and individual-level weights provided by the CPS are used in the regression.

First, I restrict the sample to ±4% FPL and ±8% FPL to make the estimates from DD model comparable to the benchmark analysis. Second, I use the full sample to compare the estimates with those in the existing studies. The full sample, however, is larger than the sample in existing studies because there are more years of data (2010-July 2016) used in the analysis (see Table 1.1 for detailed comparison).[48]

Table 1.9 presents the findings from the DD model. Columns (1) to (3) show no statistically significant effect on employment measures. The first two columns show that using the same sample as the benchmark analysis does not yield similar estimates. The only statistically significant

[48]There are also minor differences in the sample with respect to the exclusion of states, especially compared to Kaestner et al. (2017).

estimate is for labor force participation that has a positive sign. Although the benchmark analysis show a positive sign for labor force participation, the estimates are statistically insignificant. It is likely that the significance in the DD model is driven from the large and heterogeneous treatment group, where many of the ineligible adults are treated as eligible.

For the full sample, the estimates on employment are similar to the studies in the literature. The probability of being employed in Column (3) increases by 0.2 percentage points and Leung and Mas (2016) found this increase to be 0.5 percentage points. Again in Column (3), part-time employment (<35 Hrs) decreases by 0.4 percentage points relative to full-time employment (≥35 Hrs). Kaestner et al. (2017) found an increase in full-time employment, defined as working more than 30 hours, by 1 percentage point for low-educated childless adults. Using the CPS up to March 2015, Gooptu et al. (2016) show no transition between part-time and full-time employment.[49]

The findings imply that, using a similar DD model, increasing the sample size is not an improvement upon the previous findings. The main limitation of the DD model, however, is not incorporating eligibility and including a population that do not benefit from Medicaid. The benchmark findings improve upon this regard by using an identification method that captures eligibility. It is important to note that childless adults who are really close to the eligibility cutoff share similar characteristics and any changes in the outcomes reflect the effect of Medicaid expansion. There is, however, a trade off between running into measurement error problems and using a large and heterogeneous treatment group.

[49]The probability of transitioning from full-time to part-time employment increases by 0.3 percentage points. This increase, however, is not statistically different from zero.

1.6. Discussion

The labor market implications of Medicaid expansions have taken considerable attention in recent years. There have been studies investigating the labor market implications of Medicaid expansions, mainly the employment lock effect, prior to the Affordable Care Act (ACA). Many of these studies focused on a single state expansion (or a small cluster of states) that had limited external validity. There have been, however, a few studies that investigated the employment effects of the ACA's Medicaid expansion with a relatively large set of expansion states. Using a difference-in-differences (DD) model with expansion states as the treatment group, these studies found no employment effects after 2014 (see, for example, Gooptu et al., 2016 and Leung and Mas, 2016).

This paper provides an alternative quasi-experimental approach for a population near the eligibility cutoff to identify the pre/post employment effects of the ACA's Medicaid expansion. The data used in the study come from the monthly Current Population Survey (CPS) for the years 2010 through July 2016. This study uses more years of data in the post-2014 period than the existing studies to investigate the employment effects of the ACA's Medicaid expansion. A common practice in the literature is to construct a simulated eligibility measure using a single data source. I construct simulated eligibility using both March CPS and basic monthly CPS. This is an improvement upon using March CPS as the sole data source that has limitations in capturing the monthly variation in eligibility limits and outcome variables. The internal validity of the model is tested with respect to contemporaneous shocks and non-random sorting around the eligibility cutoff. There is no evidence of a selection or a manipulation prior to the expansion.

Using an arguably exogenous variation at the eligibility cutoff, I find that Medicaid enrollment increases for adults without dependent children ("childless adults"). There is also a strong evidence of an employment transition, defined as moving from full-time ($\geq$35 Hrs) to part-time employment (<35 Hrs), after the expansion. The employment transition is found to be driven from the increase in employment for working less than 20 hours. This finding is consistent with the priori that those who are primarily employed to secure private health insurance ("employment lock") will respond to the Medicaid-induced income effect. The employment transition is found to be heterogeneous across subgroups with a main effect on females, low-educated adults (HS or less), and adults who are married once. Falsification checks show no effect on non-expansion states and Medicare-eligible adult groups. In addition, the estimates are robust to the inclusion of early expansion states, increasing bandwidths, and varying functional forms of the running variable.

When the difference-in-differences (DD) model is used to replicate existing studies, there are no employment effects after the expansion. The estimates are similar to those found in the literature. The main limitation of the DD model, however, is the large and heterogeneous treatment group that includes adults who are ineligible for Medicaid.

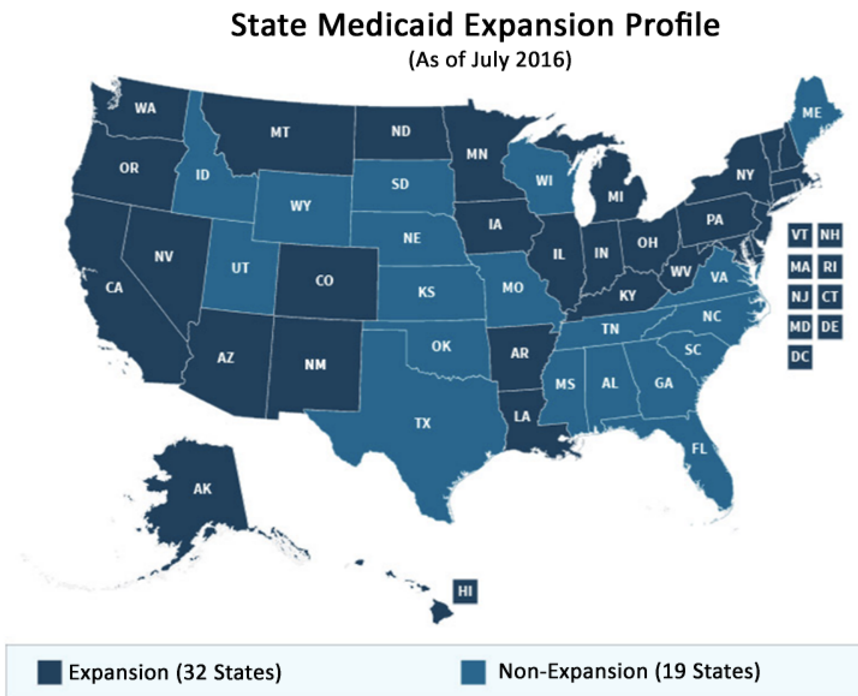

Figure 1.1: State Medicaid Expansion Profile

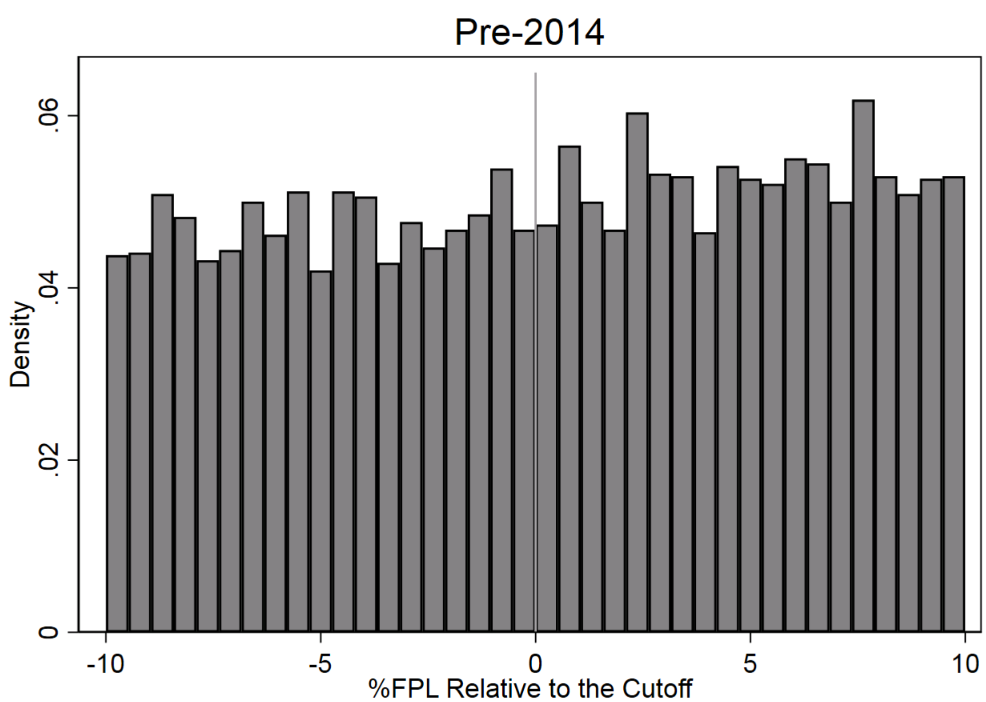

Figure 1.2: Density Test for Systematic Manipulation

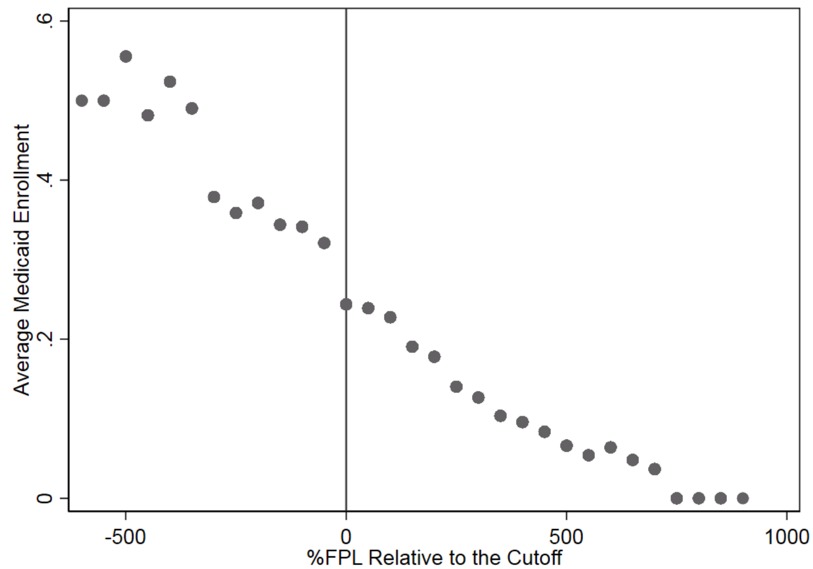

Figure 1.3: Medicaid Enrollment Around the Eligibility Cutoff

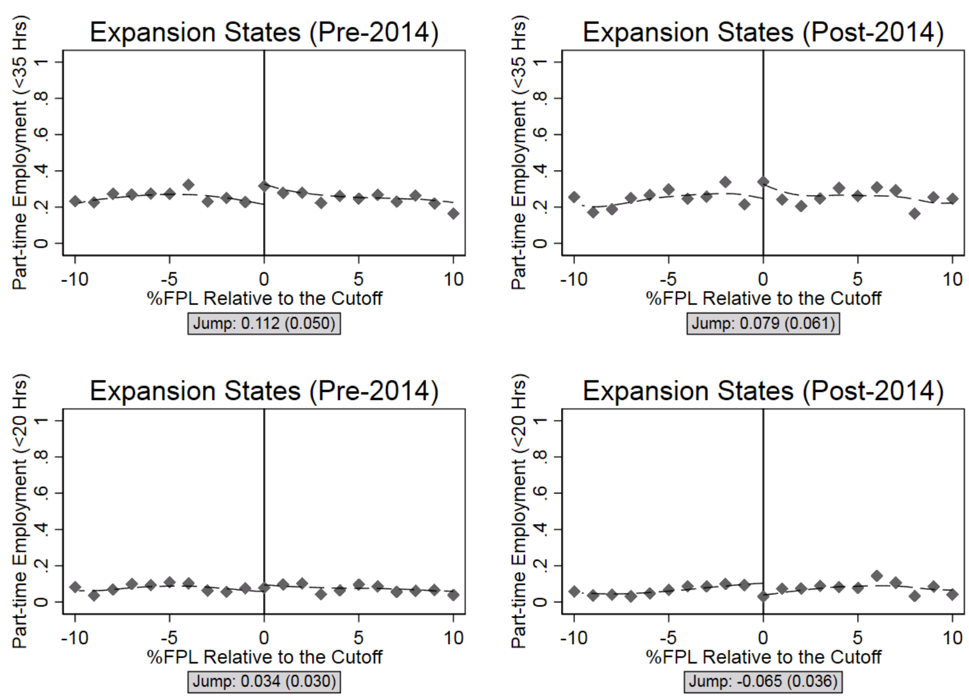

Figure 1.4: Discontinuity Plots

Table 1.1: Recent Studies on the Employment Effects of Medicaid Expansion(s)

Studies After 2010	ACA's Medicaid Expansion[†]	Adult Group[‡]	State(s) of Eligibility[††]	Data Source	Method	Employment Effect
Strumpf (2011)	No	Single women	14 states	March CPS, 1963-1975	DD, DDD models	No effect
Decker and Selck (2012)	No	Single mother	20 states	March CPS, 1966-1974	OLS	No effect
Baicker et al. (2014)	No	Low-income adults (not categorically eligible)	Oregon (limited expansion)	Oregon Health Insurance Experiment, 2008-2009	IV model	No effect
Heim and Lurie (2014)	No	Taxpayers	Massachusetts	Tax returns data (IRS), 1990-2010	DD model, synthetic control	4.7 percent (–)
Garthwaite, Gross, and Notowidigdo (2014)	No[§]	Childless adults	Tennessee[§§] (disenrollment)	March CPS, 2000-2007	DD, DDD models	6 percent (+)
Niu (2014)	No	Adults (eligible)	Massachusetts	Monthly CPS (& March CPS), 1995-2011	DD model	8.4 percent (+)
Pohl (2014)	Yes (uses simulation)	Single mothers (extension on childless adults)	All states	MEPS (restricted), 1996-2009	Multinomial logit model* (for employment choice)	5 - 6 percent (+)
Dave et al. (2015)	No	Unmarried pregnant women	All states	March CPS, 1985-1996	OLS, negative binomial models	13 percent (–)
Gooptu et al. (2016)	Yes	Childless adults	Expansion states**	Monthly CPS, 2005 - March 2015	DD model	No effect
Leung and Mas (2016)	Yes	Childless adults	21 states	Monthly CPS (& ACS), 2010 - July 2015	DD model	No effect
Kim (2016)	Yes	Childless adults	Connecticut	ACS-IPUMS, 2008 - 2013	DDD model, (using an IV approach)	12 - 14 percent (–)
Kaestner et al. (2017)	Yes	Childless adults, parents	9 or 13 states (depending on prior expansion)	Monthly CPS (& March CPS, ACS), 2010 - May 2016	DD model, synthetic control	1.8 percent*** (+)
Dague, DeLeire, and Leininger (2017)	No	Childless adults	Wisconsin (enrollment cap)	State administrative files, 2005-2011	RD, DD models	12 percent (–)

Notes: [†]This column shows whether a study examines the ACA's Medicaid expansion or an earlier expansion. [‡]The studies on the Children's Health Insurance Program (CHIP) are excluded. [††]This column distinguishes between studies that use a single expansion state versus multiple expansion states. [§]The study includes a discussion for the ACA period by using a predicted measure. [§§]This paper focuses on the effect of losing public coverage rather than gaining access. *The paper includes a three-step estimation for the choice of employment, wages, and preference parameters. **The number of expansion states is not denoted in the paper. ***The employment estimates are statistically insignificant when the ACS is used.

Table 1.2: Medicaid Profile Across States
(As of July 2016)

States	Status of the Medicaid Expansion	Effective Date of Expansion†	Income Eligibility	
			Adults with Children	Childless Adults
Alabama	Not Expanding	-	18%	0%
Alaska	Expanded	9/1/2015	138%	138%
Arizona*	Expanded	1/1/2014	138%	138%
Arkansas*	Expanded	1/1/2014	138%	138%
California	Expanded	1/1/2014	138%	138%
Colorado	Expanded	1/1/2014	138%	138%
Connecticut	Expanded	1/1/2014	201%	138%
Delaware	Expanded	1/1/2014	138%	138%
District of Columbia	Expanded	1/1/2014	221%	215%
Florida	Not Expanding	-	34%	0%
Georgia	Not Expanding	-	34%	0%
Hawaii	Expanded	1/1/2014	138%	138%
Idaho	Not Expanding	-	26%	0%
Illinois	Expanded	1/1/2014	138%	138%
Indiana*	Expended	2/1/2015	138%	138%
Iowa*	Expanded	1/1/2014	138%	138%
Kansas	Not Expanding	-	38%	0%
Kentucky	Expanded	1/1/2014	138%	138%
Louisiana	Expanded	7/1/2016	138%	138%
Maine	Not Expanding	-	105%	0%
Maryland	Expanded	1/1/2014	138%	138%
Massachusetts	Expanded	1/1/2014	138%	138%
Michigan*	Expanded	4/1/2014	138%	138%
Minnesota	Expanded	1/1/2014	138%	138%
Mississippi	Not Expanding	-	27%	0%
Missouri	Not Expanding	-	22%	0%
Montana*	Expanded	1/1/2016	138%	138%
Nebraska	Not Expanding	-	54%	0%
Nevada	Expanded	1/1/2014	138%	138%
New Hampshire*	Expanded	8/15/2014	138%	138%
New Jersey	Expanded	1/1/2014	138%	138%
New Mexico	Expanded	1/1/2014	138%	138%
New York	Expanded	1/1/2014	138%	138%
North Carolina	Not Expanding	-	44%	0%
North Dakota	Expanded	1/1/2014	138%	138%
Ohio	Expanded	1/1/2014	138%	138%
Oklahoma	Not Expanding	-	44%	0%

Table 2: Medicaid Profile Across States (Continued)
(As of July 2016)

States	Status of the Medicaid Expansion	Effective Date of Expansion†	Income Eligibility: Adults with Children	Income Eligibility: Childless Adults
Oregon	Expanded	1/1/2014	138%	138%
Pennsylvania*	Expanded	1/1/2015	138%	138%
Rhode Island	Expanded	1/1/2014	138%	138%
South Carolina	Not Expanding	-	67%	0%
South Dakota	Not Expanding	-	52%	0%
Tennessee	Not Expanding	-	101%	0%
Texas	Not Expanding	-	18%	0%
Utah	Not Expanding	-	45%	0%
Vermont	Expanded	1/1/2014	138%	138%
Virginia	Not Expanding	-	44%	0%
Washington	Expanded	1/1/2014	138%	138%
West Virginia	Expanded	1/1/2014	138%	138%
Wisconsin	Not Expanding	-	100%	100%
Wyoming	Not Expanding	-	57%	0%

Notes: This table is constructed by the author using the information on Medicaid expansion profile provided by the Henry J. Kaiser Family Foundation.
†There are nine early expansion states: AZ, CO, CT, DE, DC, HI, MN, NY, and VT. In the analysis, WI is considered as an expansion state due to the eligibility limit of 100% FPL.
*These states have approved Section 1115 waivers for expanding coverage. This waiver allows states to be flexible in terms of federal Medicaid requirements and using federal funds.

Table 1.3: Descriptive Statistics, Monthly CPS 2010-July 2016

	Eligible Adults		Non-Eligible Adults	
	Pre-2014	Post-2014	Pre-2014	Post-2014
Panel A: Outcome Variables				
Labor Force Participation	53.7%	57.4%	53.4%	58.3%
N	1,223	801	1,335	781
Employed	86.2%	88.2%	86.1%	90.8%
N	669	460	752	1,230
Part-time (PT) Employment (<20 Hrs)	7.3%	8.6%	7.9%	5.7%
PT Employment (20-34 Hrs)	18.8%	17.6 %	20.7%	19.5%
PT Employment (<35 Hrs)	26.0%	26.2%	28.6%	25.2%
N	581	414	658	1,143
Panel B: Control Variables				
Female	55.0%	49.2%	51.4%	49.2%
Age	48.1	49.7	48.4	49.0
Married	3.3%	4.4%	3.4%	4.9%
Divorced	35.6%	37.8%	33.4%	37.6%
Widowed	11.4%	9.3%	12.0%	8.7%
Separated	7.5 %	8.9%	7.7%	6.6%
White	56.5%	66.0%	57.6%	64.0%
African-American	36.7%	26.5%	37.4%	28.3%
Asian	1.3%	2.4%	1.1%	3.3%
Less than High School (HS)	15.0%	17.6%	14.2%	18.4%
HS Dropout	35.8%	28.8%	38.7%	31.0%
HS Grad	48.5%	52.3%	43.1%	49.1%
State Unemployment Rate	8.44%	3.66%	8.52%	3.78%
State GDP (% change)	0.40%	0.36%	0.40%	0.39%
N	1,223	801	1,335	781

Notes: The sample is restricted to ±4% FPL around the elgibility cutoff. Nine early expansion states are excluded from the analysis. Eligible and non-eligible adults are determined using the simulated eligibility measure. Individual-level weights are used to calculate the sample means. See Section 1.3.3 for the base group of outcome variables.

Table 1.4: The Effect of Medicaid Expansion on Enrollment:
Difference-in-Discontinuities Design, March CPS 2011-2013

	Expansion States		Non-Expansion States	
	(1)	(2)	(3)	(4)
Medicaid Enrollment				
$Eligibility * Post$	0.275**	0.188**	-0.118	0.149
	(0.136)	(0.093)	(0.168)	(0.105)
Bandwidth	±4% FPL	±8% FPL	±4% FPL	±8% FPL
N	436	923	432	887

Notes: All of the specifications include control variables, state fixed effects, period effects (year and month dummies), and state time-varying effects. See Table 1.3 for a complete list of control variables. State time-varying effects include both state unemployment rate and state GDP (% change). Standard errors in parentheses are bootstrapped with 400 replications and clustered by state. Significance levels are: ***0.01 and **0.05.

Table 1.5: Covariate Smoothness Test, Monthly CPS 2010-July 2016

	Female (1)	Age (2)	Married (3)	Divorced (4)	Widowed (5)	Separated (6)	White (7)	African-American (8)	Asian (9)	Less than HS (10)	HS Dropout (11)	HS Grad (12)
$Eligibility * Post$	0.020	1.489	-0.039	0.074	-0.048	-0.012	-0.040	0.057	0.005	-0.003	-0.014	-0.007
	(0.051)	(1.242)	(0.027)	(0.051)	(0.028)	(0.024)	(0.042)	(0.039)	(0.013)	(0.033)	(0.046)	(0.049)
Constant	0.539***	49.51***	0.003	0.411***	0.140***	0.028	0.620***	0.270***	0.028**	0.130***	0.165***	0.709***
	(0.038)	(0.831)	(0.016)	(0.037)	(0.026)	(0.018)	(0.035)	(0.031)	(0.011)	(0.026)	(0.033)	(0.034)

Notes: N =4,140 for each column. The sample is restricted to ±4% FPL around the elgibility cutoff. The regressions exclude covariates and state time-varying effects and include the remaining terms defined in Equation(1.6). Constant shows the predicted value for childless adults who are about to be eligible at 138% FPL given that d = FPL - 138. Standard errors in parentheses are bootstrapped with 400 replications and clustered by state. Significance levels are: ***0.01 and **0.05.

Table 1.6: Labor Market Outcomes: Difference-in-Discontinuities Design, Monthly CPS 2010-July 2016

	Expansion States			Non-Expansion States		
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Labor Force Participation						
Eligibility ∗ Post	0.014	0.029	0.032	-0.046	-0.031	-0.032
	(0.050)	(0.047)	(0.047)	(0.060)	(0.058)	(0.058)
	[4,140]	[4,140]	[4,140]	[4,079]	[4,079]	[4,079]
Panel B: Employed						
Eligibility ∗ Post	0.009	0.001	-0.001	0.085	0.090	0.094
	(0.046)	(0.046)	(0.046)	(0.049)	(0.048)	(0.049)
	[2,325]	[2,325]	[2,325]	[2,463]	[2,463]	[2,463]
Panel C: PT Employment (<20 Hrs)						
Eligibility ∗ Post	0.087**	0.098**	0.094**	0.059	0.054	0.053
	(0.041)	(0.041)	(0.041)	(0.037)	(0.038)	(0.038)
	[2,056]	[2,056]	[2,056]	[2,229]	[2,229]	[2,229]
Panel D: PT Employment (20-34 Hrs)						
Eligibility ∗ Post	0.048	0.048	0.044	0.023	0.012	0.013
	(0.053)	(0.054)	(0.054)	(0.066)	(0.066)	(0.066)
	[2,056]	[2,056]	[2,056]	[2,229]	[2,229]	[2,229]
Panel E: PT Employment (<35 Hrs)						
Eligibility ∗ Post	0.136**	0.146**	0.137**	0.082	0.065	0.066
	(0.063)	(0.064)	(0.064)	(0.070)	(0.070)	(0.070)
	[2,056]	[2,056]	[2,056]	[2,229]	[2,229]	[2,229]
Controls	N	Y	Y	N	Y	Y
State Time-Varying Effects	N	N	Y	N	N	Y

Notes: The sample is restricted to ±4% FPL around the elgibility cutoff. All of the specifications include state fixed effects and period effects (year and month dummies). See Table 1.3 for a complete list of control variables. State time-varying effects include both state unemployment rate and state GDP (% change). The definition of part-time (PT) employment is taken from the U.S. Bureau of Labor Statistics. Standard errors in parentheses are bootstrapped with 400 replications and clustered by state. The number of observations are given in brackets. Significance levels are: ***0.01 and **0.05.

Table 1.7: Subgroup Analysis in Expansion States, Monthly CPS 2010-July 2016

	Female (1)	Male (2)	HS or less (3)	More than HS (4)	Age (27-49) (5)	Age (50-64) (6)	Never Married (7)	Married Once (8)
Panel A: Labor Force Participation								
$Eligibility * Post$	0.050	0.018	-0.008	0.068	0.002	0.028	-0.033	0.086
	(0.069)	(0.070)	(0.099)	(0.065)	(0.072)	(0.069)	(0.078)	(0.068)
	[2,139]	[2,001]	[1,948]	[2,192]	[1,806]	[2,334]	[1,652]	[2,488]
Panel B: Employed								
$Eligibility * Post$	0.002	0.001	-0.054	-0.055	-0.058	0.073	-0.124	0.107**
	(0.061)	(0.062)	(0.078)	(0.050)	(0.066)	(0.053)	(0.070)	(0.054)
	[1,120]	[1,205]	[948]	[1,377]	[1,199]	[1,126]	[977]	[1,348]
Panel C: PT Employment (<20 Hrs)								
$Eligibility * Post$	0.140**	0.035	0.211**	0.031	0.076	0.110	0.057	0.118**
	(0.058)	(0.064)	(0.084)	(0.045)	(0.060)	(0.064)	(0.060)	(0.056)
	[1,009]	[1,047]	[817]	[1,239]	[1,023]	[1,033]	[853]	[1,203]
Panel D: PT Employment (20-34 Hrs)								
$Eligibility * Post$	0.029	0.046	-0.088	0.123	0.073	0.049	-0.010	0.078
	(0.082)	(0.079)	(0.109)	(0.077)	(0.081)	(0.086)	(0.093)	(0.076)
	[1,009]	[1,047]	[817]	[1,239]	[1,023]	[1,033]	[853]	[1,203]
Panel E: PT Employment (<35 Hrs)								
$Eligibility * Post$	0.168	0.081	0.124	0.154	0.149	0.159	0.047	0.196**
	(0.091)	(0.094)	(0.123)	(0.082)	(0.096)	(0.098)	(0.101)	(0.091)
	[1,009]	[1,047]	[817]	[1,239]	[1,023]	[1,033]	[853]	[1,203]

Notes: The sample is restricted to ±4% FPL around the elgibility cutoff in expansion states. All of the specifications include control variables (excluding the subgroup), state fixed effects, period effects (year and month dummies), and state time-varying effects. See Table 1.3 for a complete list of control variables. State time-varying effects include both state unemployment rate and state GDP (% change). Standard errors in parentheses are bootstrapped with 400 replications and clustered by state. The number of observations are given in brackets. Significance levels are: ***0.01 and **0.05.

Table 1.8: Robustness and Falsification Checks, Monthly CPS 2010-July 2016

	Include Early Expansion States		Double Bandwidth (±8% FPL)		Quadratic Running Variable		Cubic Running Variable	
	Expansion States	Non-Expansion States	Expansion States	Non-Expansion States	Expansion States	Non-Expansion States	Expansion States	Non-Expansion States
Panel A: Labor Force Participation								
Eligibility ∗ Post	0.010	-0.032	0.032	-0.052	0.033	-0.017	0.033	-0.012
	(0.047)	(0.064)	(0.033)	(0.040)	(0.039)	(0.043)	(0.035)	(0.038)
	[5,042]	[4,079]	[8,319]	[8,136]	[4,140]	[4,079]	[4,140]	[4,079]
Panel B: Employed								
Eligibility ∗ Post	-0.037	0.094	-0.003	0.032	-0.005	0.063	-0.006	0.055
	(0.039)	(0.048)	(0.030)	(0.034)	(0.035)	(0.034)	(0.032)	(0.030)
	[2,805]	[2,463]	[4,689]	[4,903]	[2,325]	[2,463]	[2,325]	[2,463]
Panel C: PT Employment (<20 Hrs)								
Eligibility ∗ Post	0.081**	0.053	0.073***	0.030	0.081**	0.042	0.074**	0.038
	(0.036)	(0.041)	(0.028)	(0.032)	(0.032)	(0.030)	(0.029)	(0.027)
	[2,476]	[2,229]	[4,140]	[4,416]	[2,056]	[2,229]	[2,056]	[2,229]
Panel D: PT Employment (20-34 Hrs)								
Eligibility ∗ Post	0.062	0.013	0.040	-0.014	0.042	-0.015	0.040	-0.023
	(0.050)	(0.066)	(0.042)	(0.048)	(0.046)	(0.049)	(0.042)	(0.044)
	[2,476]	[2,229]	[4,140]	[4,416]	[2,056]	[2,229]	[2,056]	[2,229]
Panel E: PT Employment (<35 Hrs)								
Eligibility ∗ Post	0.143**	0.066	0.113**	(0.016)	0.123**	0.027	0.115**	0.015
	(0.057)	(0.072)	(0.048)	(0.054)	(0.049)	(0.055)	(0.046)	(0.048)
	[2,476]	[2,229]	[4,140]	[4,416]	[2,056]	[2,229]	[2,056]	[2,229]

Notes: All of the specifications include control variables, state fixed effects, period effects (year and month dummies), and state time-varying effects. See Table 1.3 for a complete list of control variables. State time-varying effects include both state unemployment rate and state GDP (% change). Standard errors in parentheses are bootstrapped with 400 replications and clustered by state. The number of observations are given in brackets. Significance levels are: ***0.01 and **0.05.

Table 1.9: The Effect of Medicaid Expansion on Labor Market Outcomes: Difference-in-Differences (DD) Model, Monthly CPS 2010-July 2016

	(1)	(2)	(3)
Panel A: Labor Force Participation			
$Expansion * Post$	0.078**	0.052	0.007**
	(0.039)	(0.028)	(0.003)
	[8,219]	[16,455]	[2,653,224]
Panel B: Employed			
$Expansion * Post$	-0.003	0.014	0.002
	(0.029)	(0.018)	(0.002)
	[4,788]	[9,592]	[1,977,933]
Panel C: PT Employment (<20 Hrs)			
$Expansion * Post$	0.011	0.020	-0.002
	(0.018)	(0.014)	(0.001)
	[4,285]	[8,556]	[1,858,595]
Panel D: PT Employment (20-34 Hrs)			
$Expansion * Post$	0.048	0.030	-0.002
	(0.039)	(0.033)	(0.003)
	[4,285]	[8,556]	[1,858,595]
Panel E: PT Employment (<35 Hrs)			
$Expansion * Post$	0.059	0.050	-0.004
	(0.037)	(0.037)	(0.004)
	[4,285]	[8,556]	[1,858,595]
Sample	±4% FPL	±8% FPL	Full Sample

Notes: All of the specifications include control variables, state fixed effects, period effects (year and month dummies), and state time-varying effects. See Table 1.3 for a complete list of control variables. State time-varying effects include both state unemployment rate and state GDP (% change). Standard errors in parentheses are clustered by state and observations are weighted using the individual-level weights in the CPS. The number of observations are given in brackets. Significance levels are: ***0.01 and **0.05.

Chapter 2

The Evidence on Early Retirement After the Affordable Care Act's Medicaid Expansion

2.1. Introduction

The Affordable Care Act's (ACA) Medicaid expansion is a comprehensive health reform for the low-income population in the United States. Although the expansion was planned to be nationwide, the Supreme Court decision in 2012 made it optional for states to expand coverage to low-income adults. In 2014, more than half of the states expanded health insurance coverage to individuals who are below 138% of the federal poverty level (FPL), which is about $22,107 for an household size of two in 2016.[1] The ACA's Medicaid expansion most directly targeted adults without dependent children ("childless adults") that had no prior eligibility. Starting with the early expansions in 2010, the uninsured rate reached to a record low in 2015 (Sommers, Kenney, and Epstein, 2014, Cohen, Martinez, and Zammitti, 2016). Given the availability of Medicaid as

[1] As of January 2018, there are 33 expansion states and 18 non-expansion states.

an alternative to employer-sponsored health insurance (ESHI), understanding the extent that the ACA's Medicaid expansion affects labor supply has become crucial in the literature.[2]

A question that follows from the premise that the ACA's Medicaid expansion affects labor supply is whether the availability of Medicaid affects the retirement decision of workers. The ESHI does not necessarily come with retiree benefits and hence the workers might experience a certain degree of job lock, where they primarily work to secure private health insurance. Most of the job lock studies focus on job switch/turnover as an outcome variable (see Madrian, 1994b, Bailey and Chorniy, 2016).[3] Since the decision to retire is highly associated with the availability of health insurance (see Section 2.2 for a detailed literature review), early retirement, defined as leaving the labor force before the age of 65, could be viewed as a specific type of job lock. This may be an important concern if workers are no longer productive in specific jobs.[4] Reallocation of these workers in the market with a better worker-employer match or even leaving the labor force (temporarily or permanent) may improve total output in the long-run. There is a vast literature showing a positive relationship between retiree health insurance (RHI) and early retirement, but there are only a few studies focusing on the relationship between the ACA's Medicaid expansion and early retirement. The ACA's Medicaid expansion increases the pool of eligible workers that could substitute Medicaid for ESHI in order to retire early. This study takes a novel approach to investigate the effect of Medicaid enrollment on the retirement decision of childless adults aged 55 to 64 years.

The data set used in this study is the American Community Survey (ACS) from 2009 to 2016. The retirement outcomes are not only analyzed for all childless adults but also for men and

[2] There are a number of studies that investigate the relationship between labor market outcomes and the ACA's Medicaid expansion (Kaestner et al., 2017, Duggan, Goda, and Jackson, 2017, Aslim, 2016, Gooptu et al., 2016, Kim, 2016, Leung and Mas, 2016).

[3] Bailey and Chorniy (2016), for example, investigated the presence of job lock using the ACA's dependent coverage mandate as a natural experiment.

[4] A worker might experience productive loss during a tenure due to various reasons including medical problems.

women. In all specifications, the OLS estimates suggest an increase in the probability of retirement for childless adults who enroll for Medicaid, which range between 0.5 to 0.7 percentage points. These estimates should be interpreted with caution due to reverse causality and endogeneity. The theoretical framework in Section 2.3 shows that adults with higher medical expenses have more incentive to reduce working hours (or increase leisure) to qualify for Medicaid. This finding suggests that retirement may affect Medicaid enrollment. On the other hand, unobserved health outcomes are correlated with both Medicaid enrollment and retirement, which introduces another channel of bias. The variables on health outcomes in the ACS are related to disability. Thus, it is not possible to control for a variety of health outcomes.

In order to disentangle the causal effect of Medicaid on early retirement, I instrument Medicaid enrollment with an interaction term that exploits states' decision to expand and the corresponding timing of expansion (see Table 2.1 for both the expansion decision and timing). The instrument could be used as an intent-to-treat (ITT) estimate of a difference-in-differences (DD) model that captures the changes in the probability of retirement in expansion states. The ITT estimates imply that the probability of retirement increases in expansion states after the time of expansion. It is crucial to note that the only channel that expansion states can affect retirement is through Medicaid enrollment, which supports the exclusion restriction of the instrument used in this study. Since the purpose of ACA's Medicaid expansion is to increase coverage among low-income adults, there is no evidence on policy endogeneity with respect to retirement.

The first stage results show that Medicaid enrollment increases by 4 percentage points in expansion states after the ACA's Medicaid expansion. The F-statistics ranges between 28.96 and 39.23 across specifications, which implies that the instrument is not weak according to traditional approaches (Stock and Yogo, 2005). I find that the probability of early retirement increases by 14.8 percentage points for all childless adults who are enrolled for Medicaid. For men and women, the increase in the probability of early retirement is 13.4 and 16.1 percentage points, respectively. These estimates are much larger than the estimates obtained from the OLS regression.

The main findings from the IV regression differ from those found in Gustman, Steinmeier, and Tabatabai (2016) and Levy, Buchmueller, and Nikpay (2016) that show no effect on early retirement. Using the Current Population Survey (CPS), Levy, Buchmueller, and Nikpay (2016) estimate the trends in retirement before and after expansion for all adults aged 50 to 64 years. When all adults are pooled together in the sample, it is likely to underestimate the effect of Medicaid on retirement due to the fact that parents have access to Medicaid in both expansion and non-expansion states pre- and post-2014. This study distinguishes from Levy, Buchmueller, and Nikpay (2016) by not only focusing on childless adults but also identifying those who are enrolled for Medicaid. Gustman, Steinmeier, and Tabatabai (2016), on the other hand, focus on the effect of ACA's enactment in 2010 on early retirement. Their sample period ends in 2014 and hence the study does not capture the effect of the ACA's Medicaid expansion on early retirement.[5] Since most of the coverage gains under the ACA happened with Medicaid expansion, I focus particularly on the period after 2014 (Frean, Gruber, and Sommers, 2017).

The paper is organized as follows: Section 2.2 provides a detailed summary of the literature, Section 2.3 introduces a theoretical framework on leisure and health insurance, Section 2.4 provides a background information on expansion and non-expansion states, Section 3.3 describes the data and Section 2.6 describes the empirical methodology, Section 3.5 presents the results and Section 3.6 concludes.

[5] The data used in this study come from the Health and Retirement Survey (HRS).

2.2. Prior Research on Health Insurance and Early Retirement

This section provides a detailed analysis of the literature on health insurance and early retirement. Most of the previous studies focus on the availability of retiree health insurance (RHI) and its impact on early retirement. In order to make these studies comparable, I analyze them with respect to the choice of data set. There are a number of studies that use the Survey of Income and Program Participation (SIPP) and the Current Population Survey (CPS). A common data set is the Health and Retirement Survey (HRS). There are also a few studies that either use administrative data or confidential data from consulting companies. Since the current ACA studies are comparable to mine, I leave the analysis of those to the end.

Madrian (1994a) provides an earlier evidence on the issue using the 1987 National Medical Expenditure Survey (NMES) with two modules of the Survey of Income and Program Participation (SIPP) for the panels between 1984 and 1986.[6] Using age at retirement as the outcome variable, the paper finds that individuals with RHI retire 5 to 16 months earlier than those without the health insurance benefits.[7] The paper further shows sizable reductions in labor force participation in the case of a health reform towards universal coverage. Karoly and Rogowski (1994) also use the SIPP to investigate the effect of continuation of the employer-provided health insurance ("continuation coverege") on the retirement decision. As different from Madrian (1994a), Karoly and Rogowski (1994) extend the analysis to the 1988 panel of SIPP. The findings, however, are consistent in showing a positive effect on the probability of retirement.

There are a number of studies that use the Health and Retirement Survey (HRS) as the main data source to have a dynamic retirement framework. Marton, Woodbury, and Wolfe (2007) use

[6]The two modules of SIPP are on education, work history, and job characteristics.
[7]The paper addresses censoring problems related to the outcome variable and selection into jobs.

the three waves of the HRS between 1992-1996, and find a 55 percent increase in the probability of retirement for employees with retiree health benefits.[8] They also find heterogeneous effects for men with full-time employed wife and unmarried men. Using the same data period from the HRS, Rogowski and Karoly (2000) find an increase in the probability of retirement by 68 percent for those who have access to RHI.[9] Both studies define retirement as a transition from full-time employment to being retired in the following wave(s). The difference in the estimates, however, is argued to be driven by the differential coding of the existing control variables and/or the sample size of the last wave.

In a study that uses a discrete-time hazard model, Marton and Woodbury (2013) test for the effect of delayed payment contracts in the form of retiree health benefits on the retirement decision of workers in different age groups. When given retiree health benefits, the study shows that workers at the ages of 50 and 51 are less likely to retire than those at the ages of 60 and 61. Robinson and Clark (2010), on the other hand, use a Cox proportional hazard model to analyze the impact of RHI on the decision to separate from employment. Using the eight waves of the HRS between 1992-2006, the findings suggest an increase in the likelihood of job seperation by 21.2 percent for individuals with access to RHI than those without any access. Kapur and Rogowski (2011) also use the eight waves of the HRS to study the retirement decision of women with respect to the availability of RHI.[10] The availability of RHI increases the probability of retirement by 3 and 4.8 percentage points for women in dual-earner couples and single women, respectively.

Strumpf (2010) uses the HRS to not only show the impact of RHI on early retirement but also to analyze the changes in health care utilization, medical costs, and health outcomes. Using

[8]The authors are conservative with respect to the causal interpretation of their estimates.

[9]Marton, Woodbury, and Wolfe (2007) used the final release of the HRS data for wave three whereas Rogowski and Karoly (2000) used the alpha release of the wave three data with less observations.

[10]Retirement is defined as a transition from full-time employment in the baseline year to retirement at the next survey date.

a probit model, she finds an increase in the probability of early retirement by 37 percent when workers have access to RHI. Shoven and Slavov (2014), on the other hand, exploit a multinomial logit to model the retirement decision as a transition from full-time employment to part-time employment ("partial retirement") or leaving the labor force. The sample is restricted to public sector workers who have access to group health coverage before qualifying for Medicare. The paper shows an increase in the probability of leaving full-time employment by 38 and 26 percent for workers in the age groups 55-59 and 60-64, respectively. The findings also suggest that workers in the former age group are more likely to transition into part-time employment whereas those in the latter age group are more likely to leave the labor force. Blau and Gilleskie (2001) also use a multinomial logit model that incorporates employment transitions. As different from prior studies, the model accounts for a possible correlation between the unobservable factors affecting health insurance and the unobservable factors affecting employment decisions. The results indicate an increase in the exit rate from employment by 2 percentage points when individuals share the cost of RHI with the firm, and the increase gets larger as the firm pays all of the insurance costs.

Although reduce form models are common in the literature, some studies use the HRS to estimate a dynamic stochastic model of retirement. Blau and Gilleskie (2006) simulate the retirement decision for multiple scenarios including the availability of RHI for those who do not have any coverage, having a universal health coverage, and increasing the age limit under Medicare. The authors show differential effects for men and women, where non-employment increases for the former (3.1 percentage points) and decreases for the latter (1.8 percentage points) with the inclusion of RHI. Blau and Gilleskie (2008), on the other hand, show an increase in non-employment by 3.6 percentage points after the provision of retiree health benefits for men who had employer-sponsored health insurance with no retiree health benefits. It is crucial to note that both Blau and Gilleskie (2006) and Blau and Gilleskie (2008) do not model savings decision of individuals. Accounting for savings behavior in the model, French and Jones (2011) find that individuals with RHI retire half a year earlier than those who try to secure health insurance by working.

There are a few studies that use the Current Population Survey (CPS) to investigate the relationship between health insurance and early retirement. Gruber and Madrian (1996) exploit the mandates that allow individuals to purchase group health insurance from their employers. The study uses the Merged Outgoing Rotation Group (MORG) of the CPS for the years between 1980 and 1990. Using a probit model, they find an increase in early retirement by 5.4 percent with respect to an increase in continuation coverage by 1 year. Gruber and Madrian (1995) also find a positive relationship between the probability of retirement and continuation coverage such that the hazard ratio increases by 32.4 percent with 1 year of continuation coverage. Boyle and Lahey (2010), on the other hand, use the health insurance expansion of the U.S. Department of Veterans Affairs as a natural experiment to analyze the labor supply of older veterans. The data set used in the study is the March CPS between 1992 and 2002. The authors show a 3.3 percent decrease in the probability of employment after the health expansion by utilizing a difference-in-differences (DD) approach. The findings also suggest a 8.4 percent increase in part-time employment.[11]

Nyce et al. (2013) exploit an employee-level data from the clients of a benefits consulting firm, Tower Watson, for the years 2005 through 2009. The data set provides employee records of 54 firms and information on the size of the employer contribution towards health coverage. The findings imply that the probability of not being employed (defined as "turnover") increases by 36 percent at age 62 for workers with subsidized coverage. The increase in the probability of turnover is 49 percent and 38 percent when ages are 63 and 64, respectively. No effects are found for those who do not meet the eligibility criteria based on years of service for coverage contribution.

[11] The effect on self-employment is negative but it is not statistically different from zero.

Similar to Shoven and Slavov (2014), Fitzpatrick (2014) analyzes the retirement decision of public sector workers given the availability of RHI, but uses a different data set. The author specifically focuses on public school teachers using an administrative data from Illinois Public Schools (IPS) for the school years between 1970-1971 and 1991-1992. The employee needs to have 8 years of tenure to qualify for retirement benefits. Using eligibility for RHI as the treatment, the author shows that eligible workers retire 2 years early.[12]

All of the studies discussed above show a positive relationship between RHI (or continuation coverage) and early retirement. These studies focus on RHI due to the limitation in health insurance alternatives for workers who want to retire early. An alternative health insurance option is made available under the public expansion program of the ACA, which is known as the ACA's Medicaid expansion. Although Medicaid is viewed as an alternative for RHI, its effect on early retirement has not been discovered widely in the literature. As far as I know, there are only two studies investigating the aforementioned relationship. Using the basic monthly CPS, Levy, Buchmueller, and Nikpay (2016) stratify the sample into expansion and non-expansion states and try to capture a possible jump in January 2014. The findings indicate no effect on the probability of retirement and part-time employment after the ACA's Medicaid expansion. As discussed by Aslim (2016), large and heterogeneous treatment groups included in expansion states may jeopardize the estimates for labor market outcomes.

Gustman, Steinmeier, and Tabatabai (2016) use the HRS to investigate the effect of the ACA on early retirement for the period between 2010 and 2014. The study does not find any impact on early retirement resulting from the ACA. Note that the model does not capture the Medicaid expansion in the post-2014 period. Earlier studies show an evidence of reduced working hours with respect to Medicaid expansions (Garthwaite, Gross, and Notowidigdo, 2014, Aslim, 2016,

[12]One limitation of the study is on external validity because the author drops public schools in large cities.

Dague, DeLeire, and Leininger, 2017). In fact, Frean, Gruber, and Sommers (2017) found that 30 percent of the ACA's impact on coverage came after 2014. If job lock is strong, then the provisions between 2010 and 2014 might not be effective in incentivising detachment from the labor force.[13]

2.3. Theoretical Framework

This section introduces a simple static model for a representative household to investigate the relationship between early retirement, modeled as hours of leisure (L), and health insurance (I) that captures the availability of Medicaid. In order to construct the basis of the model, I follow French and Jones (2011) closely. The model, however, differentiates with respect to the construction of health and health insurance variables and the household characteristics. French and Jones (2011) include households above the age of 64 to show the impact of Medicare on labor supply.[14] The main group of interest here is households between the ages of 55 and 64 who are qualified for Medicaid under the Affordable Care Act.

The objective of a representative household is to maximize utility that consists of consumption, C, and L,

$$U(C, L), \tag{2.1}$$

where U is strictly concave in both goods. The first constraint faced by the individual is a time constraint

$$T = L + N + H, \tag{2.2}$$

[13]For adults aged 51 to 56 years, the effective provisions of the ACA within those periods are the changes regarding private health insurance (e.g., eliminating pre-existing conditions), the introduction of health insurance exchanges, and couple of early expansions.

[14]Using a dynamic model, they also simulate the effect of employer-provided health insurance on labor force participation rates.

where T is the total time available and N is the hours allotted to work. It is reasonable to exclude fixed costs resulting from employment and labor market reentry because having both decisions exogenous in the model do not change the outcome of the analysis. The loss of leisure due to time spent sick is captured by H. The production of health depends on health insurance (I) via access to medical care and all other factors (X) including gender, age, and education. This allows us to define sick days as $H = H(I, X)$. The second constraint in the model is a budget constraint

$$Y = C + M, \tag{2.3}$$

where household income, Y, is a function of all government transfers/benefits (Social Security, financial aid etc.), fringe benefits including pensions, spouse's income, asset income, wages, and hours worked. Without loss of generality, I assume that wage is fixed and does not vary with working hours and health. The household income is defined as $Y = Y(A, L)$, where A is all taxable income/benefits that do not vary with L. Medical expenses, on the other hand, are a function of health and health insurance, $M = M(H, I)$. For the health insurance (I), I consider the availability of Medicaid for the household. The likelihood of having Medicaid is defined as a probability function that is conditional on leisure:

$$I = Prob(\text{Medicaid} = 1|L) = \Phi(L). \tag{2.4}$$

This aim here is to capture the positive relationship between leisure and the likelihood of having Medicaid. Since the main focus of the study is on individuals who enroll for Medicaid, I do not exploit the availability of RHI. In addition, most of the low-income adults have limited access to health insurance benefits offered by full-time jobs.[15]

[15] In a dynamic framework, however, it would be interesting to investigate the employment outcomes of individuals who choose between RHI and Medicaid.

The utility maximization problem could be defined as an unconstrained optimization with L being the choice variable. The first order condition to the maximization problem yields the following marginal rate of substitution (MRS)

$$\frac{U_L}{U_C} = \frac{\frac{\partial Y}{\partial L} - \frac{\partial M}{\partial H}\frac{\partial H}{\partial I}\frac{\partial I}{\partial L} - \frac{\partial M}{\partial I}\frac{\partial I}{\partial L}}{\frac{\partial H}{\partial I}\frac{\partial I}{\partial L}}, \tag{2.5}$$

where U_L and U_C represent marginal utilities with respect to L and C. The signs of the partial derivatives are

$$\underbrace{\frac{\partial Y}{\partial L}}_{<0} - \underbrace{\frac{\partial M}{\partial H}\frac{\partial H}{\partial I}\overbrace{\frac{\partial I}{\partial L}}^{\phi(L)}}_{<0} - \underbrace{\frac{\partial M}{\partial I}\overbrace{\frac{\partial I}{\partial L}}^{\phi(L)}}_{<0} \text{ and } \underbrace{\frac{\partial H}{\partial I}\overbrace{\frac{\partial I}{\partial L}}^{\phi(L)}}_{<0}, \tag{2.6}$$

where $\phi(L)$ is the probability distribution of Medicaid that varies with L. Given the signs, two scenarios emerge depending on the magnitude of $\frac{\partial Y}{\partial L}$:

i) If income loss due to leisure is large in magnitude relative to medical expenses, there is going be less leisure and more consumption at the equilibrium, which implies that $U_L > U_C$. The individual is less likely to retire due to the relative magnitude of income loss (e.g., younger adults).

ii) If income loss due to leisure is small in magnitude relative to medical expenses, the equilibrium choice of leisure is going to be higher than consumption, which implies that $U_L < U_C$. The individual is more likely to retire because medical expenses outweigh income loss due to leisure (e.g., older adults).

These implications are consistent with the priori that individuals between the ages of 55 and 64 are more likely to retire early due to the decrease in the opportunity cost of leisure with respect to medical expenses. This also raises concerns about adults self-selecting into Medicaid by manipulating income. On the other hand, younger adults tend to have better health and their loss of income due to leisure is likely to outweigh medical expenses. This model, however, does not capture any savings decisions and/or family considerations that could give more insights about the retirement decision vis-à-vis the changes in income.

2.4. Selection of Expansion and Non-Expansion States

The timing of Medicaid expansion is crucial in order to disentangle the casual effect of Medicaid on early retirement. Although most of the expansion states increased their eligibility limits to 138% FPL in January 2014, some states had a head start in expanding Medicaid coverage for childless adults before January 2014 ("early expansion states"). There are also a number of states that expanded Medicaid coverage after January 2014 ("late expansion states"). Early expansion states differ not only in their timing of expansion but also in terms of the coverage benefits provided for childless adults. Column (3) in Table 2.1, on one hand, includes the list of early expansion states that provided full coverage for eligible childless. Column (1), on the other hand, lists the states that provided limited coverage before the ACA's Medicaid expansion. The mandatory benefits of Medicaid include inpatient and outpatient hospital services, nursing facility services, laboratory and X-ray services, and many more.[16]

There are 13 states that provided limited coverage for adults, mainly to access primary care services, before the expansion in 2014 (see Table 2.1 for a complete list of states). California, an expansion state, provided limited coverage for adults under the Medicaid Coverage Expansion (MCE) and the Health Care Coverage Initiative (HCCI) before January 2014 (Alker et al., 2013). Utah, a non-expansion state, signed the 1115 Primary Care Network (PCN) Demonstration Waiver in December 2011 that provided limited coverage of primary care services for childless adults. Ten of these states with limited benefits fully expanded Medicaid in 2014

[16]See full list of mandatory benefits at `http://Medicaid.gov`.

(labeled as “E” in column (1)) or after 2014 (labeled as “LE” in column (1)), and the remaining three states opted-out of the ACA’s Medicaid expansion in 2014 (labeled as “NE” in column (1)).

In column (2) of Table 2.1, there are two states with closed enrollment that provided full coverage for eligible adults before January 2014. In 2000, Arizona expanded Medicaid coverage for childless adults below 100% FPL. In May 2011, over 200,000 childless adults enrolled for the program. On July 8, 2011, Arizona decided to freeze enrollment to redesign Medicaid program to reduce costs. The Centers for Medicare and Medicaid Services (CMS) approved Arizona’s new Section 1115 waiver on October 21, 2011. Arizona expanded Medicaid for childless below 138% FPL in 2014. Colorado, on the other hand, extended Medicaid coverage to jobless childless adults who are below 10% FPL in May 2012. The state capped the enrollment to 10,000 adults. Similar to the case in Arizona, Colorado fully expanded Medicaid to 138% FPL in 2014.

Due to limited benefits and/or limited enrollment, I include expansion (E) and non-expansion states (NE) in columns (1) and (2) in the main analysis. The early expansion states in column (3) and late expansion states in column (4) are excluded from the analysis to capture the main effect of the ACA’s Medicaid expansion in January 2014. I probe the robustness of the estimates to the inclusion of early and late expansion states. A consistent selection of states in the main analysis is very important for the studies on the ACA’s Medicaid expansion. Levy, Buchmueller, and Nikpay (2016), for example, exclude California, Massachusetts, and Arizona (a closed enrollment state) in the main analysis by assuming that three states have provided full benefits before January 2014. On the other hand, they include Colorado (a closed enrollment state) and the remaining limit benefit states in the main analysis, which contradicts the initial exclusion of three states.

Table 2.1: State Medicaid Expansion Profile for Childless Adults

(1) States with Limited Benefits (Before January 2014)	(2) States with Closed Enrollment (Before January 2014)	(3) Early Expansion States (Before January 2014)	(4) Expansion States (E) (January 2014)	(5) Late Expansion States (LE) (After January 2014)	(6) Non-Expansion States (NE) (As of December 2016)
California (E)	Arizona (E)	Connecticut	Arizona	Alaska	Alabama
Iowa (E)	Colorado (E)	Delaware	Arkansas	Indiana	Florida
Maine (NE)		Hawaii	California	Louisiana	Georgia
Maryland (E)		Minnesota	Colorado	Michigan	Idaho
Massachusetts (E)		New York	Illinois	Montana	Kansas
Michigan (LE)		Vermont	Iowa	New Hampshire	Maine
New Jersey (E)		District of Columbia	Kentucky	Pennsylvania	Mississippi
New Mexico (E)			Maryland		Missouri
Oklahoma (NE)			Massachusetts		Nebraska
Oregon (E)			Nevada		North Carolina
Utah (NE)			New Jersey		Oklahoma
Washington (E)			New Mexico		South Carolina
Wisconsin* (E)			North Dakota		South Dakota
			Ohio		Tennessee
			Oregon		Texas
			Rhode Island		Utah
			Washington		Virginia
			West Virginia		Wyoming
			Wisconsin*		
$n = 13$ states	$n = 2$ states	$n = 7$ states	$n = 19$ states	$n = 7$ states	$n = 18$ states

Notes: Columns (3), (4), (5), and (6) are mutually exclusive (51 states in total). (E) indicates an expansion state, (NE) indicates a non-expansion state, (LE) indicates a late expansion state. The main analysis includes (E) and (NE) states in columns (1) and (2) because of limited benefits compared to full Medicaid and/or limited enrollment. *Although Wisconsin opted-out of the ACA's Medicaid expansion, childless adults below 100% FPL are eligible for Medicaid. In the analysis, Wisconsin is treated as an expansion state due to the high eligibility limit. Arizona closed enrollment on July 8, 2011. Colorado closed enrollment on May 1, 2012.

Source: Kaiser Family Foundation, `https://www.kff.org/medicaid/report/annual-updates-on-eligibility-rules-enrollment-and/`, retrieved February 21, 2018.

2.5. Data

The data set used in the study is the American Community Survey (ACS) for the years between 2009 and 2016. The ACS provides annual information on health insurance, labor market outcomes and demographic characteristics before and after the ACA's Medicaid expansion. As far as I know, this is the first study to use the ACS to investigate the relationship between health insurance and early retirement. There are pros and cons of using the ACS compared to March CPS and HRS for early retirement studies. The ACS data include geographic identifiers that allow a researcher to distinguish between expansion and non-expansion states whereas the publicly available HRS do not include geographic identifiers. In this study, geographic identifiers are used to create an instrument for Medicaid. In addition, the number of observations for childless adults in the ACS is higher than both March CPS and HRS. One limitation of the ACS (and March CPS) is the absence of longitudinal waves, where one can only track labor market outcomes of different individuals over time.[17] The Survey of Income and Program Participation (SIPP) could be viewed as an alternative data set but the latest release is for 2014 as of the writing of this paper.

Since this study focuses on the early retirement decision of childless adults, I restrict the sample to adults without own or related children under the age of 18. The sample is also limited to adults between the ages of 55 and 64.[18] In order to be consistent with the previous studies, I choose the lower limit of age to be 55. In addition, the model proposed in Section 2.3 suggests that the higher valuation of income loss at equilibrium might disincetivize a possible reduction in labor supply for younger adults. I later test whether the findings are sensitive to the changes in the lower limit of age. The main outcome variable, retirement (*Retired*), is defined as an indicator

[17] A minor limitation is that the ACS data are released annually, which do not capture the monthly variation in policy variables. This limitation also exists for March CPS, also known as the Annual Social and Economic Supplement (ASEC), since the data do not vary by months.

[18] Note that adults are eligible for Medicare past age 64 and hence could confound the estimates on Medicaid.

variable taking the value 1 if a person has retirement income in past 12 months and 0 otherwise. An important control variable in the model that affects the retirement decision of an individual is the work status of a spouse.[19] I define the work status of a spouse (*Working Spouse*) as 1 if a spouse is employed and 0 otherwise. I also control for different levels of educational attainment with an omitted category of having college education or more.

Table 2.2: Descriptive Characteristics of Childless Adults Aged 55-64 Years, ACS 2009-2016.

	All ($N = 1,487,639$)	Without Medicaid ($N = 1,332,304$)	With Medicaid ($N = 155,335$)
Outcome Variable			
Retired	0.098	0.094	0.130
Control Variables			
Age	59.32	59.32	59.34
Female	0.513	0.511	0.536
White	0.797	0.813	0.663
African-American	0.109	0.098	0.202
Asian	0.046	0.045	0.052
Hispanic	0.107	0.099	0.165
Married	0.641	0.676	0.48
Widowed	0.054	0.049	0.093
Divorced	0.187	0.174	0.297
Separated	0.024	0.020	0.057
Less than HS	0.055	0.043	0.154
HS Dropout	0.066	0.054	0.162
HS Grad	0.238	0.234	0.264
Working Spouse	0.276	0.282	0.226

Notes: ACS individual-level weights are used in computing means.

The outcome variables and individual characteristics are summarized in Table 2.2. I stratify the sample into childless adults with Medicaid and without Medicaid to gain more insights about individuals who enroll for a means-tested program. Although sample size decreases significantly, the changes in demographic characteristics and retirement are consistent with the priori. Based on

[19] There are studies in the literature that control for either the employment of spouse or the wage (Kapur and Rogowski, 2011, Marton, Woodbury, and Wolfe, 2007).

the summary statistics, those with Medicaid are more likely to retire early. As for the demographic characteristics, those with Medicaid are more likely to be African-American and Hispanic. They are also more likely to be divorced and have relatively lower educational attainment than adults without Medicaid. Since the ACA's Medicaid expansion targets low-income childless adults, the sample composition here is a relatively good representation of the Medicaid population.

2.6. Methods

The empirical approach used in this study exploits the decision to expand and the timing of expansion given in Table 2.3 to estimate the effect of Medicaid enrollment on early retirement. The following model is used to analyze the changes in labor market outcomes:

$$y_{ist} = \beta_0 + \beta_1 Medicaid_{ist} + X'_{ist}\beta_3 + \delta_{1t} + \sigma_{1s} + \epsilon_{ist}. \tag{2.7}$$

The dependent variable, y_{ist}, is an indicator variable on retirement for individual i in state s at time t (year). $Medicaid$ takes the value 1 if the childless adult is enrolled for Medicaid and 0 otherwise. The vector of individual characteristics given in Table 2.2 are denoted as X_{ist}. The year and state fixed effects are defined as δ_{1t} and σ_{1s}, respectively. Standard errors are clustered at the state-level to account for any serial correlation within states of similar characteristics. The individual-level weights provided by the ACS are used to obtain the estimates.

As the theory suggests in Section 2.3, those who are in need of medical care could reduce working hours to qualify for Medicaid. This implies the presence of reverse causality. In addition, there are disability measures in the ACS, but they are limited in capturing the demand for medical care.[20] A possible remedy is to instrument for Medicaid using the timing of expansions in order

[20]The estimates are not sensitive to the exclusion of disabled adults. These findings are available upon request.

to have an exogenous variation in the variable of interest. The instrument that I construct is an interaction variable that consists of *Expansion*, a binary variable on whether a state is an expansion state or a non-expansion state, and *Post*, another binary variable that takes the value 1 after 2014 and 0 before 2014. When early and late expansion states are included in the analysis, the timing of expansion and the variable on *Post* changes accordingly. The instrument is assumed to have a positive relationship with Medicaid enrollment due to the relative increase in the pool of eligible adults in expansion states.

The only channel that expansion states can affect retirement is through Medicaid enrollment and hence the exclusion restriction is satisfied. Given the instrument, the first stage can be described as a difference-in-differences (DD) model:

$$Medicaid_{ist} = \alpha_0 + \alpha_1 Expansion_s * Post_t + X_{ist}^{'} \alpha_3 + \delta_{2t} + \sigma_{2s} + \epsilon_{ist}, \tag{2.8}$$

where α_1 could be interpreted as the casual effect of the policy change on Medicaid enrollment relying on the assumption that expansion states and non-expansions do not trend differentially in the absence of the ACA's Medicaid expansion. Since the purpose of the ACA's Medicaid expansion is to reduce the rate of uninsured, it is assumed that the policy is exogenous with respect to retirement. In addition to exclusion restriction and policy exogeneity, I assume that there are no spillovers resulting from the treatment that changes the outcome of other childless adults, which is referred as the stable unit treatment value assumption (or "SUTVA"). The estimates provided in the regression tables include both the first stage findings and the corresponding two-stage least squares (2SLS) results.[21]

[21] The estimates are obtained using the `ivregress` command in Stata 15.

2.7. Results

2.7.1. Main Results

Some of the previous studies restrict the sample to only men and do not capture possible heterogeneity in outcomes with respect to gender (Rogowski and Karoly, 2000, Marton, Woodbury, and Wolfe, 2007, Boyle and Lahey, 2010). I analyze the changes in outcomes for three different samples: all childless adults, men, and women. All of these samples include childless adults aging from 55 to 64 years in expansion and non-expansion states (referred as the "main sample"). Early and late expansion states are excluded from the analysis. Later, in Section 2.7.2, these states are included for the purpose of robustness checks. The tables include the estimates from the OLS regression, the first stage of the 2SLS regression (denoted as "First Stage"), and the second stage of the 2SLS regression (denoted as "IV"). I denote the F-statistics on omitted instrument ($Expansion * Post$) to show the strength of the first stage results. As mentioned earlier, all of the specifications include control variables, state fixed effects, and year fixed effects.

The first stage estimates are fairly consistent across samples (see Table 2.3). Medicaid enrollment increases by 4 percentage points in expansion states after 2014. The F-statistics on excluded instrument ranges between 28.96 and 39.23 for columns (1), (5) and (9). This finding implies that, given the traditional approaches, the instrument is not weak and the strength of the first stage is good (Stock and Yogo, 2005). The IV estimates show that Medicaid increases early retirement by 14.8 percentage points. For men and women, the increase in early retirement is 13.4 and 16.1 percentage points, respectively. For the OLS regressions, the estimates are much smaller than the IV estimates, which range between approximately 1 percentage point and 2 percentage points. These findings support previous studies that show a positive relationship between health insurance and early retirement.

A model that is widely used in the ACA literature is the difference-in-differences (DD) model that uses expansion states as the treatment group (see, for example, Kaestner et al., 2017, Gooptu et al., 2016). This model provides intent-to-treat (ITT) estimates since all childless adults in expansion states are assumed to receive the treatment (*i.e.* enrolling for Medicaid). In columns (2), (6), and (10), I test whether the findings on early retirement prevail using ITT estimates. I use the first stage regression given in Equation (2.8) by changing the dependent variable to retirement. As expected, the ITT estimates are much smaller than the estimates found in the main analysis. The impact of expansion on early retirement ranges between 0.5 to 0.7 percentage points. It is very convenient to use DD models that provide ITT estimates, but the estimates should be interpreted with caution. There is a lot of heterogeneity in the treatment group by construction that is likely to confound the potential effect. In Section 2.7.2, I further test the robustness of the IV estimates and the ITT estimates.

2.7.2. Robustness Checks

Since the main analysis excludes early expansion states (7 states) and late expansion states (7 states), I test whether the estimates on Medicaid are robust to the inclusion of these 14 states (see Table 2.4). Note that *Post* was initially taking the value 1 after 2014 and 0 otherwise. Now the indicator variable changes with the timing of early expansion and late expansion instead of changing with the 2014 threshold. Table 2.4 shows that the estimates are fairly robust the inclusion of these states. The F-statistics on excluded instrument varies between 24.38 and 28.18, which is still relatively strong. The OLS estimates are still smaller than the IV estimates. There is a 14 percentage points increase in the probability of early retirement for childless adults who enroll for Medicaid. In columns (8) and (12), the probability of early retirement increases by 11.8 and 15.8 percentage points for men and women, respectively. Although the statistical significance is weakened in column (8) compared to the one in Table 2.3, the estimates are still positive and comparable. For the ITT estimates in columns (2), (6), and (10), there is an increase in the probability of retirement in the expansion states after 2014, but the estimate for men is no longer statistically significant. This implies that the ITT estimates are more sensitive to the selection of states.

Next robust check is on changing the lower limit of the age for childless adults. The initial sample includes childless adults aged 55 to 64 years. This age restriction for the lower limit is the same as the previous studies that investigated the effect of RHI on early retirement (Gruber and Madrian, 1996, Rogowski and Karoly, 2000, Boyle and Lahey, 2010, Shoven and Slavov, 2014, Fitzpatrick, 2014). On the other hand, there are some retirement studies that use 50-51 as the lower limit for age (Strumpf, 2010, Robinson and Clark, 2010, Levy, Buchmueller, and Nikpay, 2016). Thus, I test the robustness of the findings in Table 2.3 with respect to the changes in age restriction. Table 2.5 contains the estimates on the relationship between Medicaid enrollment and early retirement for childless adults aged 50 to 64 years. The estimates are still fairly robust to the changes in the sample. The findings show that the effect on early retirement for women becomes smaller in magnitude, which also decreases the estimates for the main sample. This finding is intuitive because the inclusion of younger adults will dampen the effect of Medicaid on the decision to retire. The ITT estimates are also sensitive to the changes in age restriction. Although the expansion affects retirement positively, now the estimate for women is not statistically different from zero. Overall, this analysis suggests that using expansion states as a treatment group could be problematic given that it is sensitive to sample selection.

Table 2.3: The Effect of Medicaid on Early Retirement for Childless Adults Aged 55-64 Years

	All				Men				Women			
	Medicaid	Early Retirement			Medicaid	Early Retirement			Medicaid	Early Retirement		
	First Stage	ITT	OLS	IV	First Stage	ITT	OLS	IV	First Stage	ITT	OLS	IV
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Medicaid			0.014***	0.148**			0.020***	0.134**			0.009***	0.161**
			(0.003)	(0.062)			(0.003)	(0.065)			(0.003)	(0.067)
Expansion*Post	0.040***	0.006**			0.040***	0.005*			0.041***	0.007**		
	(0.007)	(0.003)			(0.007)	(0.003)			(0.007)	(0.003)		
F-statistics on excluded instrument	35.27				28.96				39.23			
N	1,487,639				717,122				770,517			
State Fixed Effects	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Year Fixed Effects	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y

Notes: All of the specifications include control variables denoted in Table 2.2. Standard errors clustered by state are in parentheses.*Statistical significance at the 10 percent level. **Statistical significance at the 5 percent level. ***Statistical significance at the 1 percent level.

Table 2.4: The Effect of Medicaid on Early Retirement for Childless Adults Aged 55-64 Years: Including Early Expansion States and Late Expansion States

	All				Men				Women			
	Medicaid	Early Retirement			Medicaid	Early Retirement			Medicaid	Early Retirement		
	First Stage	ITT	OLS	IV	First Stage	ITT	OLS	IV	First Stage	ITT	OLS	IV
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Medicaid			0.013***	0.139**			0.018***	0.118*			0.009***	0.158**
			(0.002)	(0.065)			(0.002)	(0.071)			(0.002)	(0.069)
Expansion*Post	0.030***	0.004*			0.030***	0.003			0.030***	0.005**		
	(0.006)	(0.002)			(0.006)	(0.002)			(0.006)	(0.002)		
F-statistics on excluded instrument	27.08				24.38				28.18			
N	1,927,885				931,292				996,593			
State Fixed Effects	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Year Fixed Effects	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y

Notes: All of the specifications include control variables denoted in Table 2.2. Standard errors clustered by state are in parentheses. *Statistical significance at the 10 percent level. **Statistical significance at the 5 percent level. ***Statistical significance at the 1 percent level.

Table 2.5: The Effect of Medicaid on Early Retirement for Childless Adults Aged 50-64 Years

	All				Men				Women			
	Medicaid	Early Retirement			Medicaid	Early Retirement			Medicaid	Early Retirement		
	First Stage	ITT	OLS	IV	First Stage	ITT	OLS	IV	First Stage	ITT	OLS	IV
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Medicaid			0.015***	0.126**			0.019***	0.151***			0.045***	0.103*
			(0.003)	(0.050)			(0.003)	(0.052)			(0.006)	(0.059)
Expansion*Post	0.042***	0.005**			0.039***	0.006**			0.041***	0.005		
	(0.007)	(0.002)			(0.007)	(0.002)			(0.007)	(0.003)		
F-statistics on excluded instrument	41.60				33.17				49.73			
N	2,201,978				1,094,622				1,107,356			
State Fixed Effects	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Year Fixed Effects	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y

Notes: All of the specifications include control variables denoted in Table 2.2. Standard errors clustered by state are in parentheses. *Statistical significance at the 10 percent level. **Statistical significance at the 5 percent level. ***Statistical significance at the 1 percent level.

2.8. Conclusion

There has been an increasing amount of studies investigating the relationship between health insurance and labor market outcomes. There are various studies that show the presence of job lock in the United States. Policy makers in certain states are responding to the unforeseen effects of the ACA's Medicaid expansion. Kentucky recently adopted work requirements for Medicaid eligibility. As of January 2018, there are more states waiting to get approval from the CMS for their work requirement waiver. The effect of work requirements on enrollment and labor market outcomes still remain as an empirical question. Since the nature of the Medicaid program is changing by adopting certain welfare rules, it is crucial to understand the spillover effects under the ACA. I investigate the retirement effects of the ACA's Medicaid expansion on childless adults aged 55 to 64 years.

This study finds an evidence of an increase in the probability of early retirement for childless adults who have access to Medicaid. First stage results imply that Medicaid enrollment increases in expansion states after 2014. Both men and women are affected by the ACA's Medicaid expansion in terms of retirement. The estimates are robust to the changes made to the sample with respect to the selection of expansion states and age restriction. Overall, these findings imply the presence of work disincentives under the ACA's Medicaid expansion.

Chapter 3

Woodwork Effects and Medicaid Expansion: Evidence from the Affordable Care Act

3.1. Introduction

Medicaid is a public health insurance program for low-income populations that dates back to the authorization of Title XIX of the Social Security Act in 1965.[1] The Affordable Care Act (ACA) proposed major changes to the Medicaid program including the standardization of means testing with higher income eligibility limits. The program's purpose is not elusive, and yet it had many road blocks on spreading coverage to adults below the federal poverty level (FPL). In 2012, the Supreme Court found the ACA's Medicaid expansion to be unconstitutional and allowed states to opt out of the program. The decision left states with varying income eligibility limits for parents and adults without children.[2] The uninsured rate under the ACA's Medicaid expansion, however, reached a historic low in 2016 for low-income parents from 22.17% in 2010 to 12.59% in 2016 (see

[1]Medicaid and Medicare were signed into law by President Lyndon B. Johnson on June 30, 1965.
[2]Most expansion states covered adults with incomes below 138% FPL.

Figure 3.1 in Section 3.2).

Previous studies exploit earlier expansions and eligibility rules to investigate the changes in Medicaid enrollment among low-income parents and children. Cutler and Gruber (1996) show that Medicaid expansions crowd-out private insurance, later supported by Gruber and Simon (2008) for the 1996-2002 period.[3] Using the change in income eligibility limits between 1996 and 2007, Hamersma and Kim (2013) find an increase in Medicaid enrollment for parents. Current studies also exploit eligibility rules under the ACA to investigate the effect of Medicaid expansion on enrollment (see, for example, Sommers and Epstein, 2010b, Sommers, Swartz, and Epstein, 2011, Aslim, 2016, Kaestner et al., 2017).[4] The ACA's Medicaid expansion, however, differs from earlier expansions with respect to eligibility rules and the extent of outreach.

Under the ACA's Medicaid expansion, outreach efforts are supported by new marketplaces that ease the application process with simplified and data driven technologies. States use "navigators" to increase Medicaid enrollment among hard-to-reach communities including minorities (Hispanic or non-white adults) and immigrants with language barriers. Navigators (or certified application counselors) are individuals or organizations that not only assist consumers throughout the enrollment process, but also contribute to outreach by raising awareness about the Marketplace. Aizer (2003) finds that bilingual enrollment assistance increases Medicaid enrollment among Hispanic and Asian communities in California.

Since outreach is limited in earlier expansions, the role of information in fostering enrollment has not been investigated in previous studies. Sommers et al. (2012b) argue that high take-up rates in Massachusetts after the 2006 health reform is a combined outcome of raising awareness about coverage options and outreach efforts. The information effect via outreach combined with the

[3]There is a vast literature on the crowd-out effects of earlier expansions for low-income children (Dubay and Kenney, 1996, Yazici and Kaestner, 2000, Card and Shore-Sheppard, 2004, Ham and Shore-Sheppard, 2005).

[4]McMorrow et al. (2015), for example, show that the ACA's Medicaid expansion did not lower the number of uninsured among young adults in the short-run.

change in eligibility limits may increase enrollment among previously-eligible adults that do not take-up Mediciad, which is referred as the "woodwork" or "welcome-mat" effect (see, for example, Sonier, Boudreaux, and Blewett, 2013). There are a few studies that analyze woodwork effects under the ACA's Medicaid expansion. Sommers, Kenney, and Epstein (2014) show an increase in Medicaid enrollment among previously-eligible parents in Connecticut. Frean, Gruber, and Sommers (2017) show that 30% of coverage gains after the first year of ACA's Medicaid expansion is due to woodwork effects.[5]

Woodwork effects are often disregarded in studies that explore the changes in Medicaid take-up. This study contributes to the literature by formally analyzing the components of woodwork effects, mainly the information component that is affected by outreach under the ACA's Medicaid expansion. I use a novel approach to investigate the effect of information on Medicaid enrollment of previously-eligible parents. Using data from the American Community Survey (ACS) for the years 2009-2016, I exploit the ACA's Medicaid expansion as a natural experiment to capture the changes in outreach. After controlling for the change in income eligibility limits, woodwork effects capture the effect of information on Medicaid enrollment in the pre- and post-2014 period. In addition, I use Google trends data to determine the search volume of Medicaid in expansion and non-expansion states.

The change in income eligibility limits is referred as the "threshold effect". Similar to Hamersma and Kim (2013), the threshold effect is modeled as a continuous measure that captures the intensive margin of the expansion than an indicator variable for expansion states. Previous eligibility of parents is computed using a simulated eligibility measure (Currie and Gruber, 1995, Cutler and Gruber, 1996). This paper differentiates from Frean, Gruber, and Sommers (2017) on two aspects: i) construction of simulated eligibility and ii) decomposition of

[5]The authors emphasize that large woodwork effects in their study might be due to measurement errors in Medicaid eligibility.

woodwork effects. Frean, Gruber, and Sommers (2017) use income cells to calculate the proportion of eligible adults at the PUMA level[6], whereas I use cells on demographic characteristics to calculate the proportion of eligible adults at the state level, similar to Currie and Gruber (1995).[7] Most importantly, I disentangle the information component of woodwork effects, which has not been explored before.

I find that woodwork effects increase Medicaid enrollment after the ACA's Medicaid expansion in 2014. Woodwork effects peak in 2015 with a 65.4% increase in Medicaid enrollment. I do not find any effect prior to the expansion. I capture heterogeneous woodwork effects after stratifying the sample by education (low-educated or high-educated), ethnicity (Hispanic or Non-Hispanic), and race (white or non-white).[8] The average marginal effects are larger for targeted communities (minorities and low-educated parents) than non-targeted communities. Low-educated parents increase Medicaid enrollment by 46%. For all states, the largest effect is on Hispanic parents (83.9%) when compared to the mean in the pre-ACA period.[9] Non-white parents, on the other hand, increase Medicaid enrollment by 47.1% vis-à-vis woodwork effects. On average, woodwork effects are larger in non-expansion states than expansion states. This evidence implies the presence of information spillovers under the ACA's Medicaid expansion.

To provide further evidence on the presence of information spillovers, I analyze woodwork effects based on the search volume of Medicaid using Google trends data. Woodwork effects are dominant in states with high search volume than states with low search volume. In all states with high search volume, Medicaid enrollment increases by 51.4%. Non-expansion states experience an increase in Medicaid enrollment by 79.8% and 37.4% beyond the pre-ACA mean when search

[6] PUMAs are public use microdata areas within states that have at least 100,000 individuals.

[7] See also Golberstein and Gonzales (2015) for a recent application of simulated eligibility that use cells on demographic characteristics.

[8] See also Garfield et al. (2014) for an analysis of the coverage gap by sociodemographic factors.

[9] Recent estimates on the uninsured rate released by the Kaiser Family Foundation also show a relatively large effect on Hispanic populations (Foutz et al., 2017). The data used for the analysis are from the National Health Interview Survey.

volume is high and low, respectively. This finding is an important evidence towards information spillovers in non-expansion states. I do not find a dominating effect of high search volume over low search volume in expansion states. Woodwork effects in expansion states are likely to be driven by community-based outreach than the information spillover on the Internet. In addition, the search volume of the Marketplace (HealthCare.gov) exhibits seasonality due to open enrollment periods. Although the largest spike occurs in 2014 after the ACA's Medicaid expansion, search volume of the Marketplace after 2016 is not negligible.

The remaining part of the paper is structured as follows. Section 3.2 provides background information on outreach efforts and Medicaid enrollment. Section 3.3 describes the data used in the study. Section 3.4 introduces the empirical methodology. The findings of the paper are discussed in Section 3.5. Section 3.6 concludes the analysis.

3.2. Outreach Efforts and Medicaid Enrollment

States had limited outreach efforts to increase Medicaid enrollment prior to the ACA. On April 2010, the U.S. General Accounting Office (GAO) released a report that outlines outreach activities for both Medicaid and States Children's Health Insurance Program (SCHIP) in 10 states. The report shows that most of the states do not use internet for outreach purposes and only a small proportion of states use media ads to advertise the program. According to state officials, the most common outreach strategy is to use toll-free hotlines.[10] States also benefit from earlier campaigns that had significant impact on Medicaid enrollment. For example, Oregon's Medicaid flyer campaign in schools or Maryland's enrollment assistance. Since the success of Medicaid expansion vis-à-vis enrollment is strongly linked to outreach efforts, mass

[10] The funds available for Medicaid and SCHIP are mixed across states and hence outreach strategies differ across programs.

marketing campaigns become mainstream. In addition to mass marketing campaigns, the introduction of health insurance exchanges, improved enrollment assistance via websites, and using existing data on Supplemental Nutrition Assistance Program (SNAP) to determine eligibility contribute to outreach efforts under the ACA.

Many of the marketing campaigns use a wide range of platforms including TV, radio, social media, sports events, community partner programs, and faith communities that foster Medicaid enrollment. In addition, churches, grocery stores, beauty and barber shops are used for community-based outreach. In Maryland, for example, state officials provide information about Medicaid enrollment at Safeway and Giant stores. Using local organizations, states focus on spreading coverage to hard-to-reach communities including minorities and immigrants with language barriers. The Centers of Medicare and Medicaid Services (CMS) release Medicaid fact sheets in multiple languages for both expansion and non-expansion states to increase targeted outreach (see Appendix, Figure B1). Gates, Stephens, and Artiga (2014), on the other hand, emphasize the importance of text messages for targeted outreach since more than 90 percent of American adults owned a cell phone in 2013.[11]

If previously-eligible individuals enroll for Medicaid due to increased outreach, this is referred as the "woodwork effect" or "welcome-mat effect" (see, for example, Sommers and Epstein, 2011, Sonier, Boudreaux, and Blewett, 2013, Frean, Gruber, and Sommers, 2017).[12] It is possible to estimate the effect of information on Medicaid enrollment since parents are eligible for Medicaid prior to the ACA's Medicaid expansion. Previous studies on the ACA's Medicaid expansion focus on adults without dependent children ("childless adults") that had no access to coverage before 2014 due to the confounding effects of information on parents' Medicaid enrollment (see Aslim, 2016, Leung and Mas, 2016, Dague, DeLeire, and Leininger, 2017). This study distinguishes from

[11] For example, the first free nationwide text messaging service for pregnant women is called "Text4baby".

[12] The terms, information effect and woodwork effect, are used interchangeably in the paper.

previous studies not only by focusing on parents, but also by estimating the effect of outreach on Medicaid enrollment.

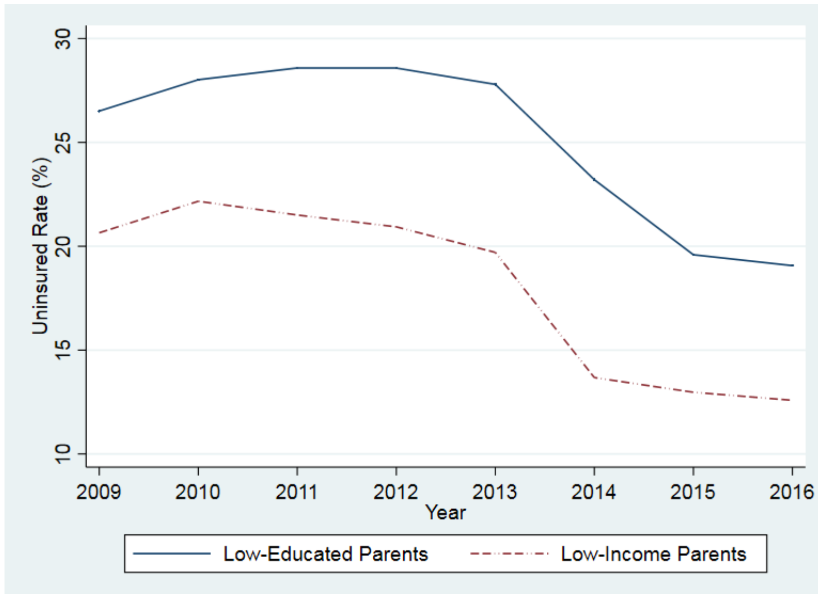

Figure 3.1: Uninsured Rate for Parents, ACS 2009-2016

Using the 2009-2016 American Community Survey (ACS), I define two groups of parents, low-educated and low-income, who are more likely to be affected by the ACA's Medicaid expansion (see Figure 3.1). Low education is defined as having less than or equal to 12 years of education. Low income, on the other hand, is defined as having an income less than or equal to 200% FPL. For both groups, there is a sharp decline in the uninsured rate after 2013. The decline in the uninsured rate coincides with the period of the ACA's Medicaid expansion. In this paper, I exploit woodwork effects that may explain the decline in the uninsured rate after 2013. The enrollment data also have a break point in 2013 (see Appendix, Figure B2). Low-educated parents and targeted minorities (Hispanic and/or non-white) have higher Medicaid enrollment than their counterparts.

A similar pattern is also captured by Google trends data. Figure 3.2 shows the search volume of

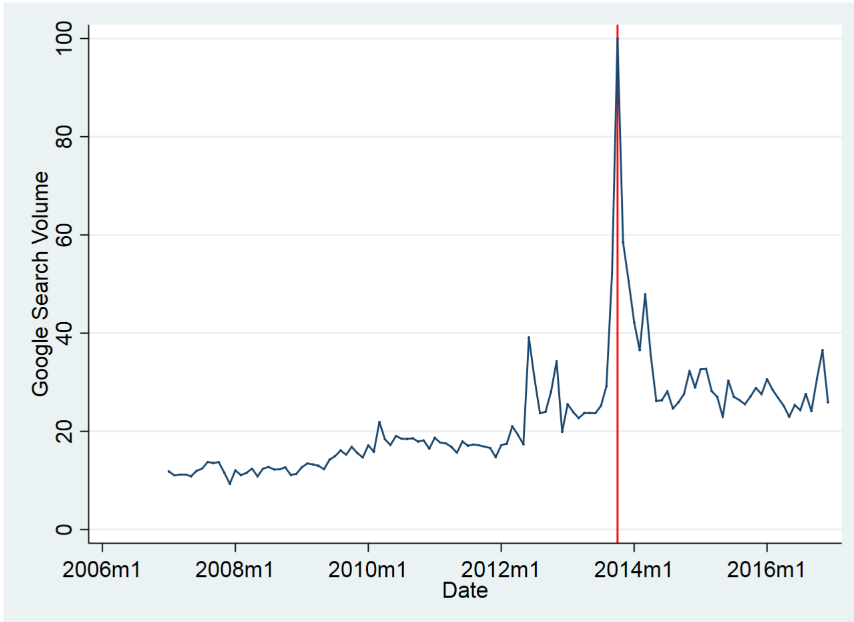

Figure 3.2: Medicaid Search Volume, Google Trends 2007-2016

multiple keywords related to Medicaid. The keywords are: “Medicaid”, “Obamacare”, “Affordable Care Act”, “ACA”, “Medicaid parent’s insurance”.[13] The findings suggest that individuals have started seeking for information prior to the ACA’s Medicaid expansion. Open enrollment period had a lot of information spillover vis-à-vis outreach efforts. The 2014 open enrollment started on October 1, 2013, which also corresponds to the peak point of search volume data. The enactment of ACA in 2010 and ex-ante provisions on Medicaid do not have significant effects on the search volume of Medicaid relative to the ACA’s Medicaid expansion.

3.3. Data

The main data set used in the analysis is the American Community Survey (ACS) for the years 2009 through 2016. The ACS is conducted by the U.S. Census Bureau on a monthly basis by randomly sampling addresses in each state. The ACS is a replacement of the long-form sample

[13]The data show mutually exclusive searches of keywords.

for which the response is required by law.[14] The questionnaire is answered by approximately 250,000 housing units each month.[15] The ACS provides information on health insurance coverage, household income, and demographic characteristics by state and year.[16] Similar information is also provided by the March Current Population Survey (March CPS), but the survey had a change in health insurance questions during the period of ACA's Medicaid expansion, which may jeopardize the estimates on Medicaid enrollment. In addition, the sample of parents in March CPS is much smaller than the ACS.

The sample is restricted to parents using the survey question about the presence of children. Parents who are in the armed forces are excluded from the analysis due to the possibility of enrolling for TRICARE. I also exclude parents aged 27 to 64 years from the analysis due to the confounding effects of the ACA's dependent coverage mandate and Medicare on Medicaid enrollment. Late expansion states (AK, MI, NH, PA, IN, MT, and LA) are dropped from the analysis to avoid any information spillovers that could bias the estimates. I probe the robustness of the estimates to the inclusion of late expansion states.[17]

The information on income eligibility limits for parents, defined as a percent of the federal poverty level (FPL), is obtained from the reports released by the Kaiser Family Foundation (KFF). All income eligibility limits are released for a family of three and hence per capita income adjustments has to be made for family sizes greater than three (see Section 3.3.1 for a detailed discussion). In addition, state rules on eligibility differs for working and jobless parents. The eligibility measure, introduced in next section, accounts for the differences in income thresholds vis-á-vis the employment status of parents.

[14]The long-form sample was conducted once every 10 years.

[15]The households can complete the questionnaire either online or as a hard copy.

[16]Information on survey months is not available in the annual files.

[17]I do not exclude early expansion states (CT, DE, HI, MN, NY, VT, and DC) from the analysis because the early expansion mainly targeted adults without dependent children ("childless adults").

I use Google trends data on keywords related to Medicaid to supplement the analysis on information effects (see Section 3.2 for the list of keywords). It is a monthly data covering the periods between January 2007 and December 2016. The data capture the relative search volume of a keyword by month and state. It is also possible to retrieve the joint search volume of multiple keywords. The data points on search volume are adjusted by the total searches in that region and the period it spans. Repeated searches from an individual over a short period of time are excluded from the data. Google trends data do not provide information on family structure and hence it is not possible to restrict the sample to parents. In order to capture some of the searches from parents, I include a keyword on Medicaid coverage for parents.

3.3.1. Construction of Key Variables

There are two key variables in the model, one that captures the proportion of eligible parents ("simulated eligibility"), and the other that captures the information effect under the ACA's Medicaid expansion. I use these variables to understand the changes in Medicaid enrollment between 2009 and 2016.

Since income eligibility limits for parents are calculated based on a family of three, I use per capita (family) income and state poverty guidelines to determine percent FPL for parents with different family sizes.[18] Using percent FPL of parents and state eligibility rules for each year (138% FPL for most states in 2014), I construct an indicator variable on eligibility for each parent.[19] As will be discussed later in results section, the findings are robust when eligibility is defined only for a family of three.

Previous studies use simulated eligibility as an instrument for actual Medicaid eligibility to

[18] Poverty guidelines are obtained from the U.S. Department of Health & Human Services, which vary by household size, year, and state.

[19] If percent FPL for a parent is less than the state eligibility rule, the measure on eligibility (E) takes the value 1 and 0 otherwise ($E = I\{FPL_{parents} \leq 138\}$).

address concerns on endogeneity (Currie and Gruber, 1995, Cutler and Gruber, 1996, Gruber and Simon, 2008, Gross and Notowidigdo, 2011). This study uses simulated eligibility to measure the effect of outreach on Medicaid enrollment. If previously-eligible parents increase Medicaid enrollment in post-2014 relative to pre-2014, this would imply the presence of woodwork effects, where eligible parents take-up Medicaid after increased outreach. I use the following steps to construct simulated eligibility. First, I randomly select 50 percent of the observations using the ACS sample for the years 2009-2016 ($N = 1,458,728$). Next, I define cells on state, year, age, sex (female or male), marital status (married or non-married), race (white or non-white), ethnicity (Hispanic or Non-Hispanic), number of children ($\leq$ 2 or $>$ 2 children), and education ($\leq$ 12 or $>$ 12 years). Finally, I calculate the proportion of parents eligible for Medicaid using the combination of cells and previously assigned eligibility.[20]

Figure 3.3 depicts the changes in simulated eligibility in expansion and non-expansion states for the pre- and post-2014 period.[21] The median simulated eligibility is approximately the same in non-expansion states for both periods. Since the main effect is in expansion states, there is an increase in the median simulated eligibility after 2014. The upper and lower adjacent values, however, are close to each other in expansion states. There are also cases where the proportion of eligible parents is zero. Since the aim is to have a sample of previously-eligible parents, I exclude those with zero simulated eligibility. The main analysis includes parents with positive simulated eligibility. Later, I check the sensitivity of the estimates to the exclusion of outliers by considering a sample that has simulated eligibility: i) greater than zero and less than the upper adjacent value (Q_3 + 1.5 IQR)[22] or ii) between the 25th and 75th percentile.

[20]I lose 0.19 percent of the sample (5,656 observations out of 2,911,591) due to insufficient number of observations in specific cell combinations.

[21]I suppress outliers on the graph for the sake of comparison.

[22]Third quartile is defined as Q_3, and the interquartile range (IQR) is the difference between the third and first quartiles.

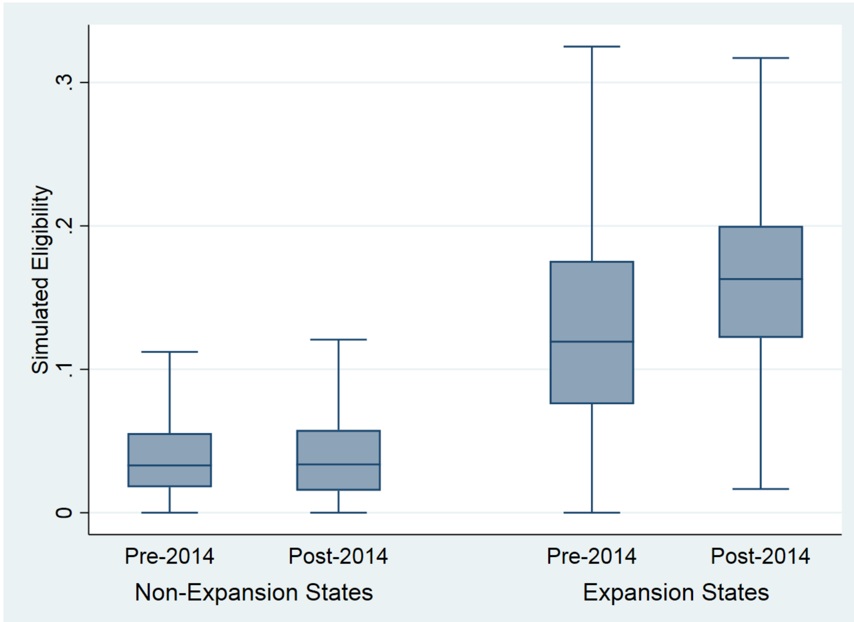

Figure 3.3: Simulated Eligibility in Expansion and Non-Expansion States

Another key variable, ΔLimit, is a continuous measure that captures the intensive margin of Medicaid expansion by taking the difference in income eligibility limits between 2013 and 2014. Since income eligibility limits differ based on employment status, I distinguish between working and jobless parents when taking the difference for each state. Hence, ΔLimit varies not only by state, but also by the employment status of parents. In expansion states, the average change in income eligibility limits is around 27 percent, which corresponds to a $5,424 change in FPL for a family of three in 2014. From 2013 to 2014, Arkansas, for example, experienced the largest change in income eligibility limits by 125 percent for jobless parents.

Table 3.1 provides summary statistics for key variables used in the model. The average Medicaid enrollment rate increases in all states after the ACA's Medicaid expansion. Although the increase is mainly driven by parents in expansion states, there is also an increase in Medicaid enrollment in non-expansion states. Given that the average change in eligibility limits is negative

Table 3.1: Summary Statistics for Key Variables, ACS 2009-2016

	All States		Expansion States		Non-Expansion States	
	Pre-2014	Post-2014	Pre-2014	Post-2014	Pre-2014	Post-2014
Medicaid Enrollment	0.104	0.140	0.117	0.170	0.081	0.092
	(0.305)	(0.347)	(0.322)	(0.376)	(0.273)	(0.289)
ΔLimit	0.126	0.143	0.217	0.243	-0.022	-0.021
	(0.349)	(0.359)	(0.396)	(0.405)	(0.172)	(0.168)
Simulated Eligibility	0.109	0.126	0.143	0.171	0.053	0.051
	(0.089)	(0.089)	(0.088)	(0.076)	(0.054)	(0.048)
N	1,469,797	885,561	907,352	549,719	562,445	335,842

Notes: All specifications include state and year fixed effects and control variables. The sample includes parents with positive simulated eligibility (see text for details). Late expansion states (AK, NH, PA, IN, and MT) are excluded from the analysis. Standard errors are clustered by state and shown in parenthesis. Significance levels are: ***0.01, **0.05, and *0.10.

in non-expansion states, an increase in Medicaid enrollment implies the presence of an information spillover. Since the sample is restricted to parents with positive simulated eligibility, the change in Medicaid enrollment in expansion states, while controlling for the change in income eligibility limits, could be attributed to increased outreach under the ACA's Medicaid expansion. The pattern in average simulated eligibility is consistent with the pattern depicted by Figure 3.3. For the sample of all states, average simulated eligibility increases in the post-2014 period, which is mainly driven by the change in income eligibility limits between 2013 and 2014.

3.4. Methods

The empirical strategy is to identify the effect of information ("woodwork effect") on Medicaid enrollment by exploiting outreach efforts under the ACA's Medicaid expansion. Given the sample on parents with positive simulated eligibility, the following model yields the woodwork effect:

$$m_{i(j)st} = \alpha_0 + \alpha_1 \Delta Limit_{i(j)s} + \alpha_2 \Delta Limit_{i(j)s} * Post_t + X^{'}_{i(j)st}\Gamma + \xi_s + \gamma_t + \epsilon_{i(j)st}, \tag{3.1}$$

where m is Medicaid enrollment for parent i with employment status $i(j)$ in state s and year t. $\Delta Limit_{i(j)s}$ is the change in income eligibility limits between 2013 and 2014. Since income eligibility limits differ for working and jobless adults, the measure that I create varies by $i(j)$. After controlling for $\Delta Limit_{i(j)s}$, the difference in Medicaid enrollment for eligible parents between the pre- and post-2014 period is attributed to woodwork effects, which is captured by $\Delta Limit_{i(j)s} * Post_t$. The indicator variable $Post_t$ takes the value 1 for the years 2014 through 2016 and 0 otherwise. The proportion of eligible adults in both periods are determined using simulated eligibility (see Section 3.3.1 for details). $X_{i(j)st}$ is a vector of individual characteristics including age, gender, marital status, race, ethnicity, number of children, and educational attainment. State and year fixed effects are denoted by ξ_s and γ_t, respectively. Standard errors are clustered at the state level.

The main assumption of the benchmark model is that there are no information shocks affecting Medicaid enrollment prior to 2014. In order to verify this assumption, I estimate the following equation:

$$m_{i(j)st} = \beta_0 + \beta_1 \Delta Limit_{i(j)s} + \sum_{t=2010}^{2016} \lambda_t (\Delta Limit_{i(j)s} * Year_t) + X'_{i(j)st}\beta_2 + \xi_s + \gamma_t + \epsilon_{i(j)st}, \tag{3.2}$$

The key variable of interest is $\Delta Limit_{js} * Year_t$, which yields the pattern of woodwork effects between 2010 and 2016. If the ACA's Medicaid expansion creates woodwork effects, this is expected to be observed in the post-2014 period. If the pre-existing trend of Medicaid enrollment do not exhibit any breaks, then the estimates for $\Delta Limit_{js} * Year_t$ from 2010 to 2013 should not be statistically different from zero. This implies that only the estimates for $\sum_{t=2014}^{2016} (\Delta Limit_{i(j)s} * Year_t)$ are expected to capture statistically significant changes in Medicaid enrollment.

Since minorities[23] are targeted under the ACA's Medicaid expansion, I analyze the changes in Medicaid enrollment vis-á-vis woodwork effects by education (low-educated or high-educated), ethnicity (Hispanic or Non-Hispanic), and race (White or Non-White), and use it as a comparison to all parents in the sample. If woodwork effects are heterogeneous across parent groups, this analysis would show the differences in Medicaid enrollment between targeted and non-targeted parent groups. In addition, I support the analysis on woodwork effects by stratifying the sample to states with high and low search volume of Medicaid on the Internet. I use Google trends data to identify the states that had higher search volume of Medicaid prior to the ACA's Medicaid expansion in 2014. High (low) search volume is defined as having a search volume greater (less) than the mean for all states. As a robustness check, I also use the mean search volume prior to the enactment of ACA in 2010.

3.5. Results

3.5.1. Trends in Medicaid Enrollment

Before presenting the main findings, I first check whether there are any pre-existing trends in Medicaid enrollment. The benchmark model assumes that in the absence of the ACA's Medicaid expansion there are no information shocks affecting Medicaid enrollment. This implies that the time trend (λ_t) has to be the same for all states in the pre-2014 period. As discussed earlier, I exploit the change in income eligibility limits from 2013 to 2014 for a sample of parents with positive simulated eligibility. If woodwork effects exist, there should be a break in the trend of Medicaid enrollment after 2013. Table 3.2 presents the time pattern of the changes in Medicaid enrollment of parents that varies by education, ethnicity, and race.

[23] Minority groups are defined in terms of race, ethnicity, gender, or religion. In this study, I particularly focus on Hispanic and/or Non-White parents.

The results indicate that $\Delta Limit$ increases Medicaid enrollment after 2013 for parents with positive simulated eligibility. This is a supportive evidence on woodwork effects since there are no effects on Medicaid enrollment prior to the ACA's Medicaid expansion. This finding is intuitive given the fact that outreach efforts were limited in the pre-2014 period. The increase in Medicaid enrollment is larger for targeted communities (minorities and low-educated parents) than non-targeted communities. The average marginal effect peaks in 2015 and slightly declines after for all parents. This is consistent with the Medicaid enrollment pattern observed in Figure B2. In 2014, Medicaid enrollment increases for all parents by 4 percentage points. This corresponds to a 38.5% increase in Medicaid enrollment relative to the mean in pre-2014. The increase in Medicaid enrollment almost doubles in 2015 with an increase of 65.4% relative to the mean in pre-2014.

Table 3.2: Trends in Medicaid Enrollment

		Education		Ethnicity		Race	
	All Parents	Low-Educated Parents	High-Educated Parents	Hispanic Parents	Non-Hispanic Parents	White Parents	Non-White Parents
ΔLimit*2010	0.001	-0.002	0.003	0.006	0.001	0.001	0.001
	(0.004)	(0.008)	(0.003)	(0.010)	(0.004)	(0.005)	(0.008)
ΔLimit*2011	-0.003	-0.009	-0.0002	0.014	-0.004	-0.003	-0.001
	(0.005)	(0.007)	(0.004)	(0.010)	(0.005)	(0.004)	(0.010)
ΔLimit*2012	0.002	-0.005	0.004	-0.001	0.002	0.002	-0.002
	(0.005)	(0.009)	(0.004)	(0.013)	(0.005)	(0.005)	(0.008)
ΔLimit*2013	0.005	-0.001	0.009**	0.005	0.005	0.007	-0.007
	(0.006)	(0.008)	(0.004)	(0.010)	(0.005)	(0.005)	(0.010)
ΔLimit*2014	0.040***	0.047***	0.034***	0.087***	0.036***	0.039***	0.046***
	(0.009)	(0.014)	(0.007)	(0.023)	(0.008)	(0.008)	(0.018)
ΔLimit*2015	0.068***	0.092***	0.051***	0.134***	0.063***	0.065***	0.089***
	(0.015)	(0.022)	(0.010)	(0.043)	(0.011)	(0.013)	(0.026)
ΔLimit*2016	0.067***	0.091***	0.050***	0.134***	0.061***	0.065***	0.081***
	(0.015)	(0.023)	(0.010)	(0.042)	(0.011)	(0.012)	(0.028)
N	2,355,358	825,032	1,530,326	311,892	2,043,466	1,849,363	505,995

Notes: All specifications include state and year fixed effects and control variables. The sample includes parents with positive simulated eligibility (see text for details). Late expansion states (AK, MI, NH, PA, IN, MT, and LA) are excluded from the analysis. Coefficients are average marginal effects. Robust standard errors in parentheses are clustered by state. Significance levels are: ***0.01, **0.05, and *0.10.

Although the increase in Medicaid enrollment is statistically significant in 2013 for high-educated parents, the effect is very small in magnitude compared to the post-2014 period.

Medicaid enrollment of high-educated parents increases by 14.5% and 54.8% in 2013 and 2014, respectively. The increase in Medicaid enrollment is largest for Hispanic parents with a 63.5% increase in 2014. In terms of education and race, the woodwork effect increases Medicaid enrollment of low-educated (27%)and non-white (29.3%) parents, but the percent increase in larger for high-educated (54.8%) and non-white parents (29.3%) in 2014. The average marginal effect is higher for socially disadvantaged groups, but the percent change is lower for low-educated and non-white parents due to the high mean value of Medicaid enrollment in the post-2014 period.

3.5.2. Medicaid Enrollment and Woodwork Effects

This subsection complements the analysis above by introducing the benchmark findings on woodwork effects. I analyze woodwork effects separately for expansion and non-expansion states, and compare the findings to all states. I present the estimates for two key variables in the model, ΔLimit and ΔLimit*Post, where the former captures the change in the pool of eligible parents resulting from the change in income eligibility limits and the latter captures woodwork effects by comparing the changes in Medicaid enrollment in the pre- and post-2014 period for parents with positive simulated eligibility. The reason for using positive simulated eligibility is to include parents who are likely to be eligible in the post-2014 period, but do not take-up Medicaid.

Table 3.3 presents the findings on the changes in Medicaid enrollment vis-à-vis woodwork effects in all states. I stratify the sample by education (low-educated or high-educated), ethnicity (Hispanic or Non-Hispanic), and race (white or non-white) to capture the heterogeneous effect of information on targeted communities. ΔLimit is the intensive margin of Medicaid expansion and it affects the proportion of eligible adults. I refer to the effect of ΔLimit on Medicaid enrollment as the "threshold effect" in 2014. After controlling for the threshold effect, the changes in the pre- and post-2014 period shows the effect of information on Medicaid enrollment. Thus, woodwork effects would correspond to the information effect under the ACA's Medicaid expansion.

Table 3.3: Medicaid Enrollment and Woodwork Effects, All States

		Education		Ethnicity		Race	
	All Parents	Low-Educated Parents	High-Educated Parents	Hispanic Parents	Non-Hispanic Parents	White Parents	Non-White Parents
ΔLimit	0.545***	0.681***	0.447***	0.521***	0.534***	0.510***	0.705***
	(0.074)	(0.096)	(0.021)	(0.133)	(0.068)	(0.065)	(0.118)
ΔLimit*Post	0.058***	0.080***	0.042***	0.115***	0.053***	0.055***	0.074***
	(0.011)	(0.017)	(0.008)	(0.033)	(0.009)	(0.010)	(0.020)
N	2,355,358	825,032	1,530,326	311,892	2,043,466	1,849,363	505,995

Notes: All specifications include state and year fixed effects and control variables. The sample includes parents with positive simulated eligibility (see text for details). Late expansion states (AK, MI, NH, PA, IN, MT, and LA) are excluded from the analysis. Coefficients are average marginal effects. Robust standard errors in parentheses are clustered by state. Significance levels are: ***0.01, **0.05, and *0.10.

First, I focus on the analysis for all states. Woodwork effects increase Medicaid enrollment by 5.8 percentage points for all parents. This corresponds to a 55.8% increase in Medicaid enrollment relative to the mean in the pre-2014 period. Woodwork effects are stronger in targeted groups (minorities and low-educated parents), but the percent changes vary depending on the pre-2014 mean. The increase in Medicaid enrollment is 46% and 53% for low-educated and high-educated parents, respectively. A group that is mainly targeted through outreach under the ACA's Medicaid expansion is Hispanic parents (See Figure B1 for Medicaid Fact Sheets in Spanish). In all parent groups, the largest change in Medicaid enrollment is for Hispanic parents. Woodwork effects increase Medicaid enrollment by 11.5 and 5.2 percentage points for Hispanic and non-Hispanic parents, which corresponds to a 83.9% and 54.6% increase in Medicaid enrollment, respectively. In terms of race, the estimate for non-White parents is larger than White parents, but percentage-wise it is the opposite. On one hand, the increase in Medicaid enrollment is 63.2% for White parents. On the other hand, the increase in Medicaid enrollment is 47.1% for non-White parents.

In expansion states, there is a similar pattern across targeted parent groups. Woodwork effects

Table 3.4: Medicaid Enrollment and Woodwork Effects, Expansion States

		Education		Ethnicity		Race	
	All Parents	Low-Educated Parents	High-Educated Parents	Hispanic Parents	Non-Hispanic Parents	White Parents	Non-White Parents
ΔLimit	0.666***	0.840***	0.558***	0.645*	0.652***	0.627***	0.868**
	(0.181)	(0.234)	(0.143)	(0.328)	(0.157)	(0.152)	(0.339)
ΔLimit*Post	0.042***	0.053***	0.033***	0.071***	0.041***	0.044***	0.047***
	(0.007)	(0.009)	(0.005)	(0.018)	(0.006)	(0.006)	(0.009)
N	1,457,071	503,402	953,669	208,333	1,248,738	1,123,346	333,725

Notes: All specifications include state and year fixed effects and control variables. The sample includes parents with positive simulated eligibility (see text for details). There are 26 expansion states in the sample. Late expansion states (AK, MI, NH, PA, IN, MT, and LA) are excluded from the analysis. Coefficients are average marginal effects. Robust standard errors in parentheses are clustered by state. Significance levels are: ***0.01, **0.05, and *0.10.

have a large impact on socioeconomically disadvantaged groups. Low-educated parents increase their Medicaid enrollment by 26.8% relative to the mean in the pre-2014 period. The effect of information is still very high for Hispanic parents such that Medicaid enrollment increases by 43.3%. For non-white parents, the increase in Medicaid enrollment is 27.2%. When all parents are taken into account, the change in Medicaid enrollment is computed as 35.9%. These findings suggest that previously-eligible parents in expansion states are more inclined to take-up Medicaid after the ACA's Medicaid expansion.

Table 3.5: Medicaid Enrollment and Woodwork Effects, Non-Expansion States

		Education		Ethnicity		Race	
	All Parents	Low-Educated Parents	High-Educated Parents	Hispanic Parents	Non-Hispanic Parents	White Parents	Non-White Parents
ΔLimit	0.519***	0.637***	0.397***	0.534***	0.508**	0.476***	0.691***
	(0.066)	(0.088)	(0.048)	(0.110)	(0.065)	(0.057)	(0.099)
ΔLimit*Post	0.045***	0.070***	0.027**	0.010	0.048***	0.043***	0.041**
	(0.011)	(0.011)	(0.011)	(0.012)	(0.012)	(0.011)	(0.016)
N	898,287	321,630	576,657	103,559	794,728	726,017	172,270

Notes: All specifications include state and year fixed effects and control variables. The sample includes parents with positive simulated eligibility (see text for details). There are 18 non-expansion states in the sample. Exclusion of late expansion states (AK, MI, NH, PA, IN, MT, and LA) do not affect the analysis for non-expansion states. Coefficients are average marginal effects. Robust standard errors in parentheses are clustered by state. Significance levels are: ***0.01, **0.05, and *0.10.

In order to strengthen the findings on woodwork effects, I focus on non-expansion states that had limited changes in income eligibility limits (see Table 3.5). In fact, the average change in income eligibility limits is around -0.22 from 2013 to 2014. Although there is a negative shock in non-expansion states with respect to ΔLimit, there is still an increase in Medicaid enrollment.

All parents increase Medicaid enrollment by 55.5%, despite the fact that average ΔLimit is negative. This finding implies that woodwork effects are stronger in non-expansion states than expansion states for all parents. The average marginal effects are still high for socioeconomically disadvantaged groups. I find that low-educated parents increase their Medicaid enrollment by 51.1%. Contrary to the finding in expansion states, I do not observe a statistical significant change in Medicaid enrollment for Hispanic parents in non-expansion states. This finding is very interesting because a group that is mainly targeted in expansion states through outreach is not affected in non-expansion states. Non-Hispanic parents, however, increase Medicaid enrollment by 59.3%. For non-white parents, the increase in Medicaid enrollment is estimated to be 37%. These findings altogether imply that previously-eligible parents in non-expansion states are affected by the information spillover under the ACA's Medicaid expansion.

3.5.3. More Evidence on Woodwork Effects

I provide additional analysis using Google trends data to support the findings on woodwork effects. Since the Internet provides easy access to information, it is exploited under the ACA's Medicaid expansion for outreach purposes.[24] The Google trends data vary by month, year, and state. I aggregate the data by state and year to be able to merge it to the ACS. The distribution of the aggregated data is provided in Figure B3. In the pre-2014 period, the mean search volume of Medicaid is 22.46. In the post-2014 period, however, the mean search volume of Medicaid increases to 28.86. The increase in search volume after the ACA's Medicaid expansion implies that woodwork effects are not only driven by community-based outreach, but also through the Internet that creates information spillovers between states.

Table 3.6 shows how Medicaid enrollment of all parents change in states with high and low

[24]The Current Population Survey (CPS) shows that 75 percent of households have access to the Internet at home in 2012.

search volume of Medicaid. If search volume in a state is at least 22.46, the pre-2014 mean, it is defined as high search volume, and below that threshold is defined as low search volume. The findings imply that woodwork effects increase Medicaid enrollment in all states, but more so in non-expansion states with high search volume. Medicaid enrollment in non-expansion states increase by 6.7 percentage points. This corresponds to an increase in Medicaid enrollment by 79.8% beyond the mean in the pre-2014 period. The increase in Medicaid enrollment is 37.4% in non-expansion states with low search volume. Since expansion states have community-based outreach, there is no information spillover as in the case of non-expansion states. The estimates imply that community-based outreach dampens the effect of search volume. On the other hand, when expansion and non-expansion states are combined, woodwork effects are dominant in states with high search volume (51.4% increase) than states with low search volume (44.1% increase).

Table 3.6: Medicaid Enrollment and Woodwork Effects, Search Volume (Pre-2014)

	All States		Expansion States		Non-Expansion States	
ΔLimit	0.509***	0.619***	0.411***	1.145*	0.573***	0.411***
	(0.069)	(0.187)	(0.102)	(0.544)	(0.101)	(0.050)
ΔLimit*Post	0.055***	0.056***	0.030**	0.053***	0.067***	0.034***
	(0.013)	(0.012)	(0.011)	(0.011)	(0.015)	(0.008)
High Search Volume	Y	N	Y	N	Y	N
Low Search Volume	N	Y	N	Y	N	Y
N	1,095,530	1,259,282	383,433	1,073,638	712,097	186,190

Notes: All specifications include state and year fixed effects and control variables. High and low search volumes are based on the pre-2014 mean value. The sample includes parents with positive simulated eligibility (see text for details). Late expansion states (AK, MI, NH, PA, IN, MT, and LA) are excluded from the analysis. Coefficients are average marginal effects. Robust standard errors in parentheses are clustered by state. Significance levels are: ***0.01, **0.05, and *0.10.

In addition, I analyze the search volume of "HealthCare.gov", the new health insurance exchange (or the Marketplace) under the ACA (see Appendix, Figure B4). Since the website has launched on October 1, 2013, I do not include data prior to 2013. There is seasonality in search volume due to open enrollment periods. The spike in search volume is between October and March each year. The year of ACA's Medicaid expansion, 2014, has had the highest search volume of the Marketplace. The spike in 2017 is also higher than the preceding years, 2016 and 2015. This spike in search volume is in late 2016, after the U.S. presidential election and before the last day of enrollment for plans that start in January 2017. Since the election campaigns in 2016 focused largely on the ACA, it might have triggered woodwork effects. It would be interesting to explore the relationship between elections and woodwork effects in the future.

3.5.4. Robustness Checks

In this subsection I investigate the robustness of woodwork effects to alternative specifications. The main analysis excludes late expansion states (AK, MI, NH, PA, IN, MT, and LA) due to information spillovers between states in 2014. In addition, state eligibility limits provided by the Kaiser Family Foundation are for family of three. I use per capita family income to assign eligibility to parents with different family sizes. In order to check whether the estimates are sensitive to the selection of states and family size, I include late expansion states in the analysis and keep parents with family of three in the benchmark sample that excludes late expansion states (see Table 3.7). The inclusion of late expansion states in the specification for parents with family of three does not alter the findings.[25]

Table 3.7 shows that woodwork effects are fairly robust across specifications. Medicaid enrollment increases by 45.3% and 50.9% when late expansion states are included and family size is restricted to three, respectively. These estimates are comparable to the benchmark estimate that shows a 55.8% increase in Medicaid enrollment. Woodwork effects are stronger in non-expansion states than expansion states for all parents. This finding on woodwork effects is consistent with the analysis above. The inclusion of late expansion states does not affect non-expansion states, and hence the estimates are the same as the benchmark estimates.

[25]The estimates for this case are available from the author upon request.

Table 3.7: Medicaid Enrollment and Woodwork Effects, Inclusion of Late Expansion States and Family of Three

	All States		Expansion States		Non-Expansion States	
ΔLimit	0.575***	0.555***	0.680***	0.675***	0.519***	0.526***
	(0.069)	(0.075)	(0.116)	(0.173)	(0.066)	(0.071)
ΔLimit*Post	0.053***	0.059***	0.041***	0.043***	0.045***	0.050***
	(0.010)	(0.011)	(0.007)	(0.006)	(0.011)	(0.015)
Late Expansion States	Y	N	Y	N	Y	N
Family of Three	N	Y	N	Y	N	Y
N	2,652,334	703,249	1,754,047	428,515	898,287	274,734

Notes: All specifications include state and year fixed effects and control variables. The sample includes parents with positive simulated eligibility (see text for details). Late expansion states are AK, MI, NH, PA, IN, MT, and LA. Coefficients are average marginal effects. Robust standard errors in parentheses are clustered by state. Significance levels are: ***0.01, **0.05, and *0.10.

The second set of robustness checks are on the selection of samples based on simulated eligibility. Since the proportion of eligible adults changes in the post-2014 period, I select samples that are comparable in terms of simulated eligibility in both pre- and post-2014. I use Figure 3.3 provided in Section 3.3.1 to select samples that overlap vis-à-vis simulated eligibility. Sample 1 includes parents with simulated eligibility greater than zero and less than the upper adjacent value. The upper adjacent value is obtained by Q_3 + 1.5 IQR, where Q_3 is the third quartile and interquartile range (IQR) is the difference between the third and first quartiles. Sample 2 includes a smaller of parents with simulated eligibility in between the first quartile and third quartile. Using both samples show whether the estimates on woodwork effects are sensitive to different proportions of simulated eligibility.

Table 3.8 presents findings based on different samples of simulated eligibility. The average marginal effects are close in magnitude to the benchmark estimates, but the percentage changes vary depending on the pre-2014 mean of Medicaid enrollment. The increase in Medicaid enrollment in both samples ranges between 64% and 71% for all states. The percent changes are higher relative to the benchmark analysis given the exclusion of outliers that lower mean simulated eligibility and Medicaid enrollment. The findings are robust across specifications such

Table 3.8: Medicaid Enrollment and Woodwork Effects, Different Samples of Simulated Eligibility

	All States		Expansion States		Non-Expansion States	
ΔLimit	0.547***	0.519***	0.667***	0.728***	0.528***	0.538***
	(0.075)	(0.068)	(0.184)	(0.238)	(0.081)	(0.105)
ΔLimit*Post	0.056***	0.053***	0.041***	0.045***	0.051**	0.049***
	(0.012)	(0.007)	(0.006)	(0.006)	(0.018)	(0.022)
Sample 1†	Y	N	Y	N	Y	N
Sample 2‡	N	Y	N	Y	N	Y
N	2,299,827	1,186,502	1,415,713	786,466	843,001	577,735

Notes: All specifications include state and year fixed effects and control variables. †Sample 1 includes parents with simulated eligibility greater than zero and less than the upper limit of box plot (Q3 + 1.5 IQR). ‡Sample 2 includes parents with simulated eligibility in between the first and third quartile (Q1 and Q3). Late expansion states (AK, MI, NH, PA, IN, MT, and LA) are excluded from the analysis. Coefficients are average marginal effects. Robust standard errors in parentheses are clustered by state. Significance levels are: ***0.01, **0.05, and *0.10.

that Medicaid enrollment is higher in non-expansion states (64-68%) than expansion states (41-50%).

The final robustness check is on the selection of high and low search volume states. The benchmark analysis uses the period between 2009 and 2014 to determine the search volume of Medicaid. Figure 3.2 in Section 3.2 shows spikes in the search volume of Medicaid between 2012 and 2013. Increased outreach during the open enrollment period in 2013 is likely to be driving the search volume of Medicaid. I test whether using a more stable period in terms of outreach, the pre-ACA period (2007-2009), alter the results on Medicaid enrollment. I determine low and high search volume states using the mean search volume in the pre-ACA period. States with search volume greater or equal to 12.81 are coded as high search volume states and those below 12.81 are coded as low search volume states.

The findings on Table 3.9 show that the estimates are not sensitive to the selection of states based on the search volume of Medicaid in the pre-2010 period. The largest impact is in non-expansion states with high search volume of Medicaid. The increase in Medicaid enrollment is 79.8% in non-expansion states with high search volume of Medicaid. Increased outreach in

Table 3.9: Medicaid Enrollment and Woodwork Effects, Search Volume (Pre-2010)

	All States		Expansion States		Non-Expansion States	
ΔLimit	0.519***	0.595***	0.453***	1.075*	0.571***	0.415***
	(0.073)	(0.175)	(0.124)	(0.506)	(0.100)	(0.052)
ΔLimit*Post	0.057***	0.056***	0.030**	0.054***	0.067***	0.034***
	(0.013)	(0.014)	(0.010)	(0.012)	(0.015)	(0.008)
High Search Volume	Y	N	Y	N	Y	N
Low Search Volume	N	Y	N	Y	N	Y
N	1,185,186	1,170,172	484,685	972,386	700,501	197,786

Notes: All specifications include state and year fixed effects and control variables. High and low search volumes are based on the pre-2010 mean value. The sample includes parents with positive simulated eligibility (see text for details). Late expansion states (AK, MI, NH, PA, IN, MT, and LA) are excluded from the analysis. Coefficients are average marginal effects. Robust standard errors in parentheses are clustered by state. Significance levels are: ***0.01, **0.05, and *0.10.

expansion states dampens the effect of search volume on Medicaid enrollment such that the increase in Medicaid enrollment is higher in low search volume states than high search volume states. The overall finding for all states, however, suggests that woodwork effects are stronger in high search volume states, where Medicaid enrollment increases by 51.8%.

3.6. Conclusion

This study analyzes one of the most debated provisions of the ACA, the Medicaid expansion in 2014, vis-à-vis outreach efforts. In 2013, the Centers for Medicare & Medicaid Services (CMS) awarded $67 million to fund navigator programs. Navigators are individuals or organizations that educate the public about the Marketplace and provide in-person enrollment assistance. Navigators are part of the outreach efforts under the ACA's Medicaid expansion. Since outreach is increased with the expansion in 2014, there may be information spillovers about the program across states. This study uses a novel approach to understand the effect of information on Medicaid enrollment of previously-eligible adults ("woodwork effect"). I exploit the ACA's Medicaid expansion as a natural experiment to investigate whether the increase in outreach after 2013 increases the enrollment among parents who were eligible before the expansion, but did not take-up Medicaid. If changes in eligibility limits are controlled in the model ("threshold effect"), any possible woodwork effects will be driven from the spread of information through outreach.

This paper shows that woodwork effects are present in all states, but heterogeneous among parents with different education levels (low-education or high-education), ethnicity (Hispanic or Non-Hispanic) and race (white or non-white). Woodwork effects are stronger in targeted populations including low-educated, Hispanic, and non-white parents. In expansion states, Hispanic parents experience the largest increase in Medicaid enrollment by 43.3%. In addition, parents in non-expansion states increase Medicaid enrollment after the ACA's Medicaid expansion. For all parents, the increase in Medicaid enrollment is 35.9% and 55.5% in expansion and non-expansion states, respectively.

In order to provide more evidence on woodwork effects, I use Google trends data to stratify the sample with respect to the search volume of Medicaid. Woodwork effects are higher in magnitude for all states with high search volume of Medicaid. In states with high and low search volume, Medicaid enrollment increases by 51.4% and 44.1%, respectively, beyond the mean in the pre-2014 period. On one hand, community-based outreach dampens the effect of search volume in expansion states. On the other hand, non-expansion states with high search volume experience strong woodwork effects. Overall, these findings imply that outreach not only increase Medicaid enrollment among hard-to-reach communities, but also have spillover effects in non-expansion states.

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Appendix A

First appendix chapter

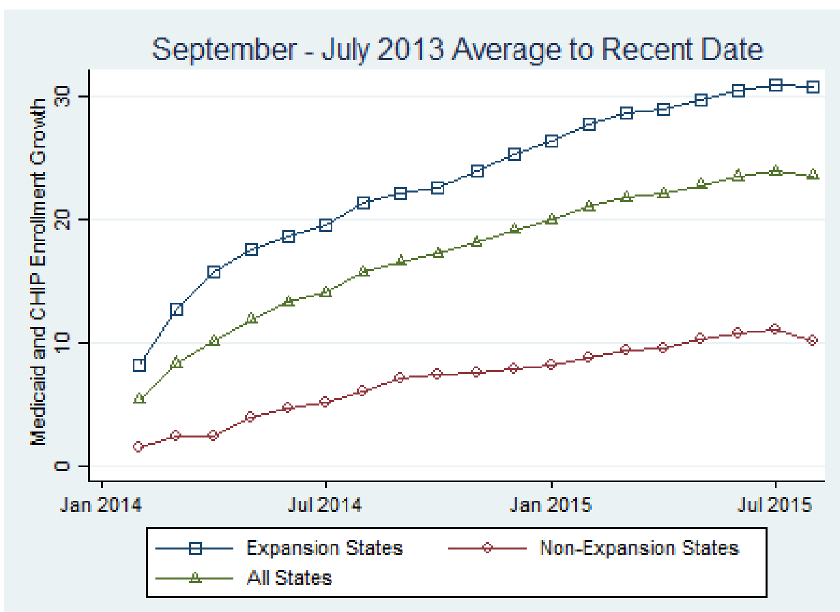


Figure A1: Medicaid and CHIP Enrollment Growth Rate

Notes: The graph is constructed by the author using the Medicaid enrollment data from the Centers for Medicare & Medicaid Services (CMS).

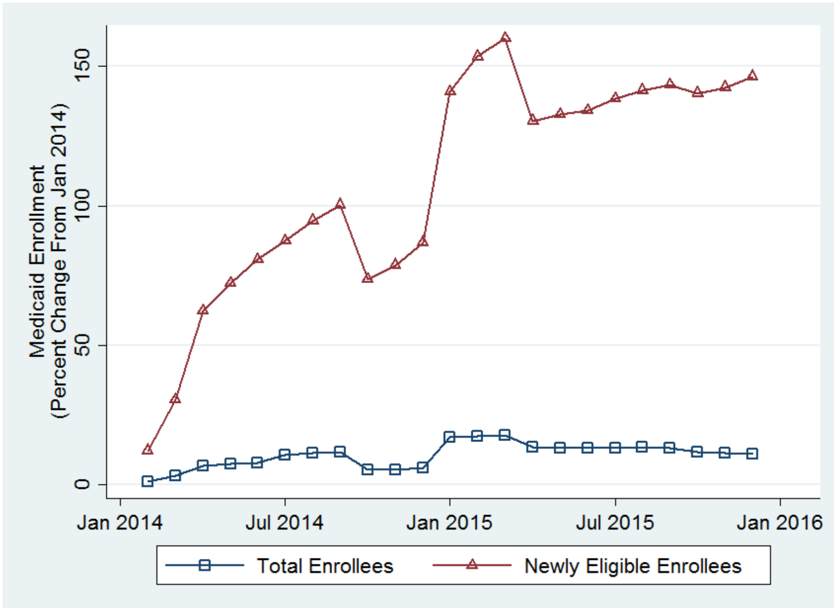


Figure A2: Medicaid Enrollment Growth Rate

Notes: The graph is constructed by the author using the Medicaid enrollment data from the Centers for Medicare & Medicaid Services (CMS).

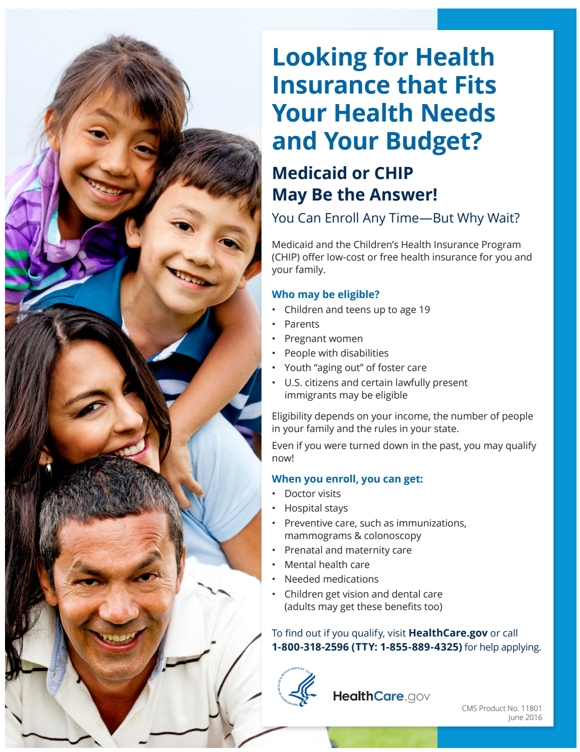
Table A1: Guidelines on the Federal Poverty Level (FPL)

	100% FPL						
Household Size	2010 FPL	2011 FPL	2012 FPL	2013 FPL	2014 FPL	2015 FPL	2016 FPL
1	$10,830	$10,890	$11,170	$11,490	$11,670	$11,770	$11,880
2	$14,570	$14,710	$15,130	$15,510	$15,730	$15,930	$16,020
3	$18,310	$18,530	$19,090	$19,530	$19,790	$20,090	$20,160
4	$22,050	$22,350	$23,050	$23,550	$23,850	$24,250	$24,300
5	$25,790	$26,170	$27,010	$27,570	$27,910	$28,410	$28,440
6	$29,530	$29,990	$30,970	$31,590	$31,970	$32,570	$32,580

Notes: Hawaii and Alaska have different guidelines for the poverty thresholds and those are taken into account.
Source: U.S. Department of Health & Human Services.

Appendix B

Second appendix chapter



English Flyer

Figure B1: Medicaid Fact Sheets

Source: Centers of Medicare & Medicaid Services (CMS)

Spanish Flyer

Figure B1: Medicaid Fact Sheets

Source: Centers of Medicare & Medicaid Services (CMS)

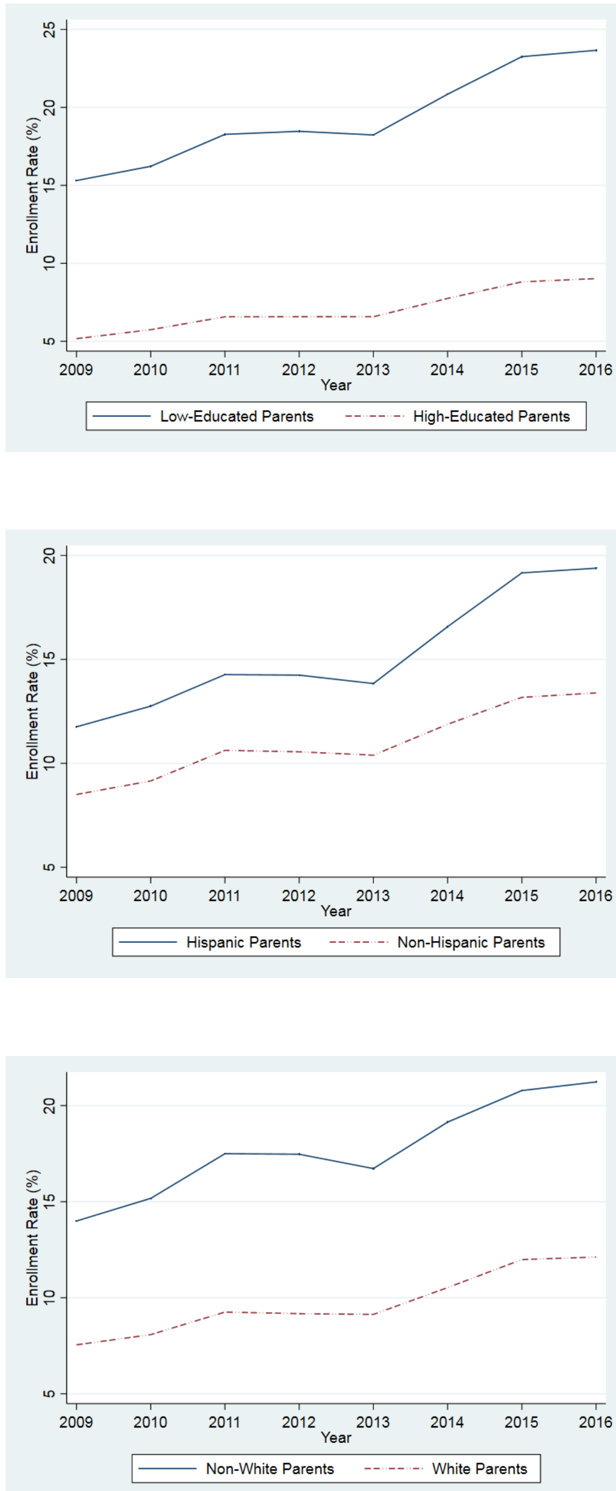


Figure B2: Medicaid Enrollment by Education, Ethnicity, and Race, ACS 2009-2016

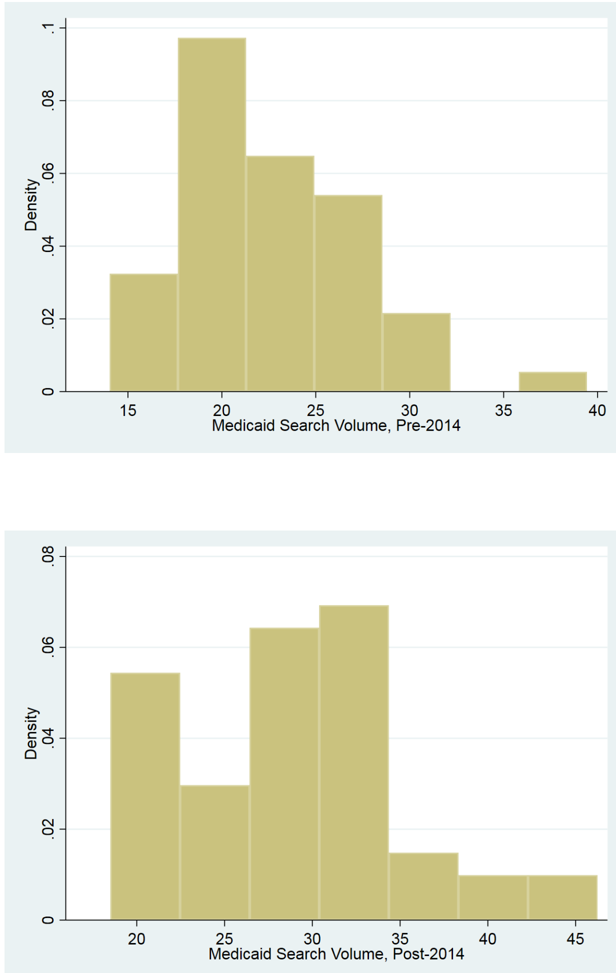


Figure B3: Medicaid Search Volume in the Pre-2014 and Post-2014 Period

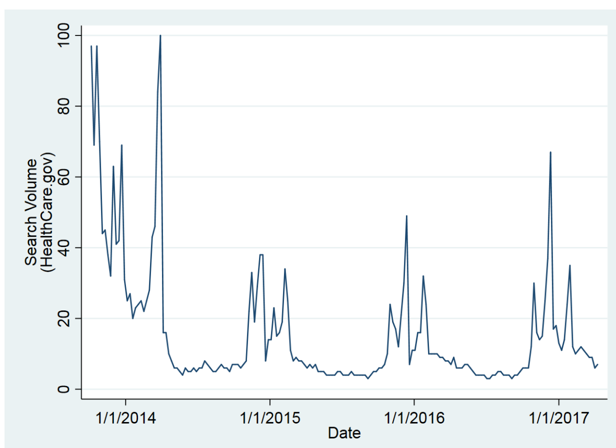


Figure B4: Search volume of HealthCare.gov

Curriculum Vitae

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Education

2014–Present	Ph.D., Economics, Lehigh University, PA (Expected completion May 2018) Dissertation Title: "Labor Market and Outreach Effects of Medicaid Expansion under the Affordable Care Act" Dissertation Committee: Shin-Yi Chou (Chair), Muzhe Yang, Seth Richards-Shubik, Karen Smith Conway
2013–2014	M.A., Economics, University of New Hampshire, NH
2011–2013	M.A., Economics, Bilkent University, Ankara, Turkey
2007–2011	B.A., Economics, Bilkent University, Ankara, Turkey

Fields

Primary Fields:	Health Economics, Applied Econometrics
Secondary Fields:	Labor Economics, Public Finance

Publications

Journal Articles, Peer-Reviewed:

1. Erkmen Giray Aslim and Bilin Neyapti, "Optimal Fiscal Decentralization: Redistribution and Welfare Implications", *Economic Modelling*, 61: 224-234, 2017.

Articles Under Review

1. Erkmen Giray Aslim and Bilin Neyapti, "Fiscal Decentralization, Political Polarization and Fiscal Efficiency"

2. Erkmen Giray Aslim, Irina Panovska, and Anil Tas, "Does Maternity Leave Affect Labor Force Participation and Productivity?"

3. Erkmen Giray Aslim, "The Evidence on Early Retirement After the Affordable Care Act's Medicaid Expansion"

Working Papers

1. Erkmen Giray Aslim, "Does Medicaid Expansion Affect Employment Transitions?" (job market paper)

2. Erkmen Giray Aslim, "Woodwork Effects and Medicaid Enrollment: Evidence from the Affordable Care Act"

3. Erkmen Giray Aslim, "Self-Assessed Health: Longitudinal Evidence from Albania"

Papers in Progress

1. Erkmen Giray Aslim, "Mental Health Care in the United States: Evidence from the Affordable Care Act Young Adult Mandate"

Teaching Experience

2016-2017 Adjunct Professor

Statistical Methods, Spring 2018, Lehigh University
Applied Microeconomic Analysis, Fall 2017, Lehigh University
Principles of Economics (Online), Summer 2017 (4.59/5), Lehigh University
Economics, Student Scholars Institute, Summer 2017, Lehigh University
Economics, Student Scholars Institute, Summer 2016, Lehigh University

2011-2017 Teaching Assistant

Principles of Economics, Fall 2014 (4.37/5), Spring 2015 (4.56/5), Fall 2015 (4.87/5), Spring 2017 (4.93/5), Lehigh University
Money, Banking and Financial Markets, Spring 2016 (4.58/5), Lehigh University
Principles of Macroeconomics, Fall 2013, University of New Hampshire
Principles of Microeconomics, Spring 2014, University of New Hampshire
Principles of Macroeconomics, Spring 2013, Bilkent University
Principles of Microeconomics, Fall 2012, Bilkent University
Macroeconomic Theory I, Fall 2011, Bilkent University

Research Experience

2012–2016 Research Assistant

Lehigh University (Summer 2016, Summer 2015)
University of New Hampshire (Spring 2014)
Bilkent University (Spring 2013, Fall 2012)

Working Experience

Winter 2009	Intern Deloitte, Tax Department, Ankara, Turkey
Summer 2008	Sales Manager Picture Perfect LLC., Wisconsin Dells, WI

Awards And Honors

2017	Teaching Assistant of the Year Award, Lehigh University
2017	Graduate Student Senate Award, Lehigh University
2016	Warren-York Fellowship, Lehigh University
2016	Teaching Assistant of the Year Honorable Mention, Lehigh University
2016	College of Business and Economics Leadership Award, Lehigh University
2010–2011	Merit Scholarship (Highest ranked GPA), Bilkent University

Professional Activities

Organized Conferences (Presenter)

1. Symposium on the Affordable Care Act at the 44th Annual Eastern Economic Association Meetings,: “The Evidence on Early Retirement After the Affordable Care Act’s Medicaid Expansion”, Boston, USA, March 2018.
2. iHEA World Congress: “Does Medicaid Expansion Affect Employment Transitions?”, Boston, USA, July 2017.
3. 43rd Annual Eastern Economic Association Meetings CSWEP Session: “Does Medicaid Expansion Affect Employment Transitions?”, New York, USA, February 2017.
4. 6th Biennial Conference of the American Society of Health Economists: “The Medicaid Expansion and Labor Market Activity: Evidence from the Affordable Care Act”, Philadelphia, USA, June 2016.
5. 42nd Annual Eastern Economic Association Meetings: “The Medicaid Expansion and Labor Market Activity: Evidence from the Affordable Care Act”, Washington DC, USA, February 2016.

Organized Conferences (Chair/Discussant)

1. 44th Annual Eastern Economic Association Meetings, Boston, USA, March 2018
   Discussant: “The Affordable Care Act and Retirement: Effects of Changes in Medicaid and Non-Group Coverage” by Reagan Baughman.
2. 43rd Annual Eastern Economic Association Meetings, New York, USA, February 2017
   Chair - Session: “Conflicts, Household and Child Welfare”
3. 42nd Annual Eastern Economic Association Meetings, Washington DC, USA, February 2016
   Chair - Session: “Higher Education and Its Return”
   Discussant: “The Path to a Bachelor’s Degree: The Effect of Starting at a Community College” by Jessica Scheld.

Invited Presentations

1. Rice University (Baker Institute for Public Policy), “Does Medicaid Expansion Affect Employment Transitions?”, March 2018.
2. University of Pennsylvania (Perelman School of Medicine), “Does Medicaid Expansion Affect Employment Transitions?”, February 2018.
3. Auburn University at Montgomery (Department of Economics), “Does Medicaid Expansion Affect Employment Transitions?”, February 2018.

4. BAU International at Washington D.C. (Department of Economics), “Does Medicaid Expansion Affect Employment Transitions?”, January 2018.
5. Bilkent University (Department of Economics), “Does Medicaid Expansion Affect Employment Transitions?”, December 2016.
6. Lehigh University (Department of Economics), “Mental Health Care in the United States: Evidence from the Affordable Care Act Young Adult Mandate”, May 2016.
7. Lehigh University (Department of Economics), “The Medicaid Expansion and Labor Market Activity: Evidence from the Affordable Care Act”, May 2015.
8. University of New Hampshire (Department of Economics), “Self-Assessed Health: Longitudinal Evidence from Albania”, June 2014.

INVITED CONFERENCES (ATTENDEE)

1. NBER Summer Institute 2017: Health Economics Session

SKILLS

Computer:	MATLAB, Mathematica, SAS, Stata, Eviews and LaTeX
Languages:	Turkish (Native), English (Fluent), German (Beginner)

SERVICE ACTIVITIES

UNIVERSITY

2016-2017	President, Turkish Students Club, Lehigh University
2015-2016	Treasurer, Turkish Students Club, Lehigh University
2015-2016	Graduate Advisor, United States Association for Energy Economics (USAEE) Lehigh Chapter

COMMUNITY

2009-2011	Participant, Baskent Rotaract Club, Ankara, Turkey

PEER-REVIEWING ACTIVITY: PROFESSIONAL JOURNAL

Journal of Labor Research, *Financial Innovation*

PROFESSIONAL SOCIETIES AND AFFILIATIONS

American Economic Association
International Health Economics Association
American Society of Health Economists
Eastern Economic Association

References

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