ESSAYS ON THE EMPLOYMENT OF LOW-INCOME WORKERS

A Dissertation

by

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ABSTRACT

This dissertation introduces three essays about the employment of low-income workers, a group whose employment has large implications for economic growth, tax revenue, and government spending on means-tested transfer programs. The first two essays in this dissertation analyze the causal impact of two of these transfer programs, the Earned Income Tax Credit and Medicaid, on employment decisions. The third essay provides a descriptive analysis of the job mobility of low-wage workers.

The first essay "Employment Effects of the Earned Income Tax Credit for Childless Adults" analyzes the employment and labor supply effects of state expansions of the Earned Income Tax Credit for childless adults, a group who has been at the center of policy discussions. The Earned Income Tax Credit was designed to assist low-income households while also encouraging work. I find that state expansions of the Earned Income Tax Credit led to increases in employment and labor force participation for childless women and declines for men. Employment increases were largest for women age 25-34 and employment declines were largest for older men, age 55-64.

The second essay "Public Insurance and Retirement Decisions", joint work with Laura Dague and Marguerite Burns, analyzes the effect of state Medicaid expansions on retirement decisions. In the last two decades, many states have extended public insurance by eliminating categorical eligibility requirements for Medicaid such as disability or responsibility for a dependent child. The availability of public health insurance can affect retirement-related decisions for adults who value these benefits highly. We find that public insurance access led to delays in retirement and Social Security claims until age 65, the age at which Medicare, public health insurance for all elderly, becomes available.

The third and final essay "Occupational and Industry Mobility of Low-Wage Worker", joint work with Jonathan Meer, studies the occupation and industry mobility of low-wage workers. We find that low-wage workers switch both occupation and industry frequently. Occupation switches are more common than industry switches, but the majority of mobility is explained by simultaneous switching of both industry and occupation.

DEDICATION

To Cydney, Cameron, and Brooke who give my life meaning and joy.

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1. INTRODUCTION

Understanding the impacts of public policy is important to estimate the benefits of past, present, and future policy changes. To this end, sections 2 and and 3 analyze the causal impact of two US transfer programs, the Earned Income Tax Credit and Medicaid, on the employment decisions of childless adults. Childless adults have historically been ineligible for most government assistance in the US and have only recently become the subject of policy talks aimed at helping low-income workers. Section 4 provides a descriptive analysis of the occupation and industry mobility of low-wage workers.

Section 2 studies the casual effects of state expansions of the Earned Income Tax Credit on the employment, labor force participation, and work hours of childless adults. I find that state EITC expansions caused employment to increase by 1.22pp, labor force participation to increase by 0.06pp, and work hours to increase by 1.2% for childless women. Conversely, EITC expansions caused employment and labor force participation to decrease for childless men. Men's work hours were not affected. These results highlight both the intended and unintended consequences of policies designed to assist low-income workers. Baseline employment levels, age, and household structure were strongly correlated with employment effects. Employment gains were concentrated in younger childless women living with family, while employment losses were concentrated in older men living with family.

Section 3 studies the casual effects of Medicaid expansions on the retirement decisions of older adults. In the United States, eligibility for health insurance is strongly tied to labor market participation. In the last two decades, many states have expanded their Medicaid programs to reach adults at higher income levels and eliminated categorical eligibility requirements such as disability or responsibility for a dependent child. The increased availability of public health insurance changes this dynamic, particularly for the population of older workers whose eligibility for health insurance is strongly tied to labor market participation. Medicaid expansions caused adults without dependents, especially women, to delay retirement and Social Security claiming until age 65, the threshold for Medicare eligibility.These findings show Medicaid is an important bridge for insurance access for the near-elderly population, and can actually lead to increases in labor supply, which could offset some of the costs of program implementation through increased tax revenue and reductions in transfer program spending.

Section 4 gives a descriptive analysis of the occupational and industry mobility of low-wage workers, a group that is the frequent target of state and federal welfare reforms. Understanding worker mobility is important for studies that use occupation or industry to restrict to policy-affected populations or to calculate labor market concentration. We find that workers switch occupations and industries frequently. In fact, in a given year, almost 18% of low-wage workers switched occupation, and about 13% switched both occupation and industry. Based on these findings, future research should look at worker mobility before using occupation or industry to define labor markets.

2. THE EARNED INCOME TAX CREDIT AND EMPLOYMENT FOR CHILDLESS ADULTS

2.1 Introduction

Childless adults have increasingly been the center of policy talks aimed at helping low-income workers. In 2014, many states chose to expand Medicaid to low-income childless adults through the Affordable Care Act. More recently, federal and state representatives have had discussions about expanding the Earned Income Tax Credit (EITC) for childless adults by increasing income eligibility limits, raising the maximum credit generosity, and lowering the eligibility age.

Recent estimates suggest that the EITC lifted 8.9 million Americans out of poverty in 2017 (Beltrán, 2019). The EITC is designed to encourage recipients to work by increasing in generosity as work earnings increase. After reaching a maximum, the credit plateaus over a range of earnings and then gradually phases out until it reaches zero. Childless adults make up over 54% of adults earning less than the poverty level, but receive less than 5% of EITC payments.[1] Because federal EITC payments are so low for childless adults, they are the only group of workers who whose net income after taxes pushes them below the poverty line.[2] The maximum EITC for adults with dependents is 6.6-12.4 times the size of the EITC for adults without dependents.

Previous literature has documented large increases in the employment of other groups (mostly single mothers) from EITC expansions (Eissa and Liebman, 1996; Meyer, 2002; Meyer and Rosenbaum, 2001; Bastian, 2017a; Leigh, 2010; Wilson, 2018). The EITC has resulted in a number of significant economic and social benefits for eligible groups, such as reductions in recidivism (Agan and Makowsky, 2018), improved mother and infant health (Hoynes et al., 2015; Evans and Garthwaite, 2014; Markowitz et al., 2017), and boosts in

[1] I calculate the share of low-income adults that are childless using all 18-64 year old adults in the 2018 CPS ASEC.

[2] All workers are subject to Medicare and Social Security taxes.

educational achievement and attainment (Bastian and Michelmore, 2018; Michelmore, 2013; Chetty et al., 2011). Additionally, Bastian and Jones (2018) show that the EITC is also a very cost-effective policy with a self-financing rate of about 83% when considering increases in tax revenue and reductions in transfer program participation. There are many reasons to think that childless adult employment would be responsive to EITC increases, despite their credit being substantially lower than that of households with dependents. Childless adults have very different housing needs, purchase different product bundles, and do not need to find childcare. They are also categorically ineligible for most transfer programs such as WIC (Supplementary Nutrition for Women, Infants, and Children), SNAP (Supplementary Nutrition Assistance Program), and TANF (Temporary Assistance for Needy Families), and Medicaid.

To my knowledge, no other paper has looked at the direct effects of the EITC on the employment of childless adults. I use a difference-in-differences approach with changes in state EITCs over 1994-2018, the Current Population Survey (CPS), and the American Community Survey (ACS) to estimate the causal effect of EITC expansions on childless adult employment. 29 states implemented their own EITC over this time span.

Childless women increased their employment by 1.22 percentage points (pp) with an increase of $100 in the maximum state EITC. Women between 25-34 and/or those living with family are the primary drivers of these positive effects (2.0-3.5pp) Childless male employment declined by 1.25pp and this decline was concentrated in older childless men or men living with their parents. Positive employment effects are strongly associated with low baseline employment levels. and the nature of their unemployment. Those who are unemployed because they are entering or re-entering the labor force are much more likely to increase employment than those who are unemployed because they recently lost or left their job.

Hours worked increases by 1.28% for all women, and these effects are mostly driven by older women age 55-64 (2.29%). Mens' hours do not significantly change conditional on being employed. The only margin men are affected on, as a whole, appears to be the extensive margin of employment.

Declines in male employment might at first seem inconsistent with theory of the incentives of the EITC. But one issue this paper has to contend with is that lots of groups are eligible, who each have varying levels of EITC incentives. This, combined with different combinations of reservation wages and employer-employee match preferences, make this theory even less straightforward. Groups entering the labor force due to the EITC, could crowd out the employment of other groups through less available job openings or lower wages through increases in labor market supply, without offsetting increases in labor market demand.

To supplement my state EITC analysis, I also look at the special case 2015 District of Columbia EITC (DC EITC) expansion, the largest state expansion of the EITC to date for childless adults. In addition to its size, another important feature of this DC EITC expansion was that it solely affected eligibility for childless adults, which is not true of previous state expansions. The DC EITC expansion also extended the maximum earnings eligibility level from about \$15k to \$25k, and also raised the maximum credit from 40% (\$200) to 100% (\$500) of the Federal EITC. The DC EITC expansion led to a 5.78pp increase in employment and a 2.5pp increase in the labor force participation of childless women. Mens' employment and labor force participation declined 1.48pp and 3.71pp respectively. I found that employment effects in DC were consistent with magnitudes found in other state EITC expansions.

2.2 Background on the EITC for Childless Adults

The Earned Income Tax Credit was first introduced in 1975 for households with at least one dependent. In 1992, the federal EITC became more generous for families with two or more children relative to one-child families. Over 1993-1996, the generosity of the EITC dramatically increased, and remained stable until 2009, when the EITC expanded for families with three or more children relative to families with two or less children. The federal EITC for childless adults was introduced in 1994, and its generosity has not been expanded since, except for annual inflation adjustments. Childless adults are eligible to claim the EITC if they have sufficiently low-income and they (or their spouse) are age 25-64. Figure B.1 shows the EITC schedule for single adults with varying numbers of dependents. Both EITC generosity and reach increases substantially with the number of dependents claimed by tax filers. To supplement the federal EITC, a number of states have introduced their own EITCs, most of which are available to childless adults. Most states calculate their EITC as a fraction of the federal EITC. Figure B.4 shows how state EITC generosity has evolved over time for each state. Only 6 states had an EITC in 1994, growing to 29 states in 2018 with generosity ranging from 3%-100% of the federal EITC.

In 2016, the Speaker of the House of Representatives, Paul Ryan, and President Barack Obama proposed nearly identical plans to expand the EITC for households with dependents. Both plans would have lowered the eligibility age from 25 to 21, expanded the income eligibility range, and almost doubled the maximum credit amount to about $1000 (Marr et al., 2017). Despite the similarities in policy parameters between the two plans and bipartisan support for expanding the EITC for childless adults, the plans stalled because of disagreements on how to fund it.[3] EITC expansion talk has continued with the Cost-of-Living Refund Act (COLRA) of 2019 (Sen. Sherrod Brown, Rep. Ro Khanna), the LIFT Act (Sen. Kamala Harris), and the EITC Modernization Act. These Acts include provisions such as raising the maximum eligible income limit, increasing the maximum EITC generosity, making low-income students or at-home caregivers eligible, and lowering the minimum eligibility age from 25.

Some states have already implemented EITC-type expansions for childless adults. In 2006, New York introduced a non-custodial parent tax credit available to adults who have a child, but are not a primary caregiver of their children. In 2015, D.C. was the first region to

[3]President Obama proposed paying for the plan by raising taxes on high income workers and reducing tax benefits for corporations. Conversely, Speaker Ryan proposed cuts to other transfer programs such as “Social Service Block Grant, the Fresh Fruits and Vegetables Program, the Economic Development Administration, and the Farmers’ Market Nutrition Program. It also would reduce fraud in the Additional Child Tax Credit by requiring the use of Social Security numbers." (Matthews, 2015)

expand the EITC exclusively for childless adults (40% to 100% of the federal EITC), while leaving it unchanged for families.

The definition of "childless adult" encompasses a wide range of adults with different fertility histories and household structures. Some examples are adults who have never had children, empty nesters (parents with grown children no longer considered dependents), or non-custodial parents. A 2018 CPS report estimates that 27% of all children had a parent who lived in another household, and at least half of these had some sort of child support agreement in place; many of whom also live below the poverty level (Grall, 2020). In my analysis looking at household structure and earnings, I find that the majority of low-income childless adults are primary earners in their households. Single childless adults most commonly live alone or with a few family members, rather than with unrelated roommates. Married childless couple households typically do not have other household members.

Most of the academic literature about the EITC focuses on single mothers and finds large positive effects on the extensive margin of employment (employed or not) (Eissa and Liebman, 1996; Meyer, 2002; Meyer and Rosenbaum, 2001; Bastian, 2017a; Leigh, 2010; Wilson, 2018). Intensive margin effects are more nuanced and depend on the freedom that workers have to adjust their hours and earnings. Saez (2010) and Chetty et al. (2013) show that self-employed workers respond to EITC incentives strongly on the intensive margin.

Research looking at the EITC and childless adults is limited. Miller et al. (2018) have studied the effect of an EITC-like payment (up to $2000) to low-income childless adults in New York City. Employment effects were positive (with the largest effects found in women (2.3-4.6pp) and disadvantaged men (0.6-5.8pp). Results on other outcomes are promising as well. They find increases in after-tax incomes, tax filing rates, and child support payments. But during this time (2015-2017), New York implemented several large minimum wage increases. Neumark and Wascher (2011) show that EITC expansions coupled with minimum wage increases enhance employment effects for eligible groups, but can have adverse effects for ineligible groups. Therefore, the experiment effects might overstate the benefits of the

EITC, by exacerbating the difference between the treatment and control groups.

This paper adds to the literature by looking at both extensive and intensive margin employment effects for childless adults, a group that faces substantially different opportunity costs with regards to their time, purchases, and housing than parents, who have been the main focus of previous EITC research.

2.3 Data

My main sample comes from a harmonized version of the Annual Social and Economic Supplement of the Current Population Survey (CPS ASEC) from IPUMS covering the years 1994-2018 (Flood et al., 2020).[4] I limit my sample to all childless adults age 18-64 with no post-high school education.[5]

In Figure B.2 I show that the share of childless women with earnings in the range of EITC eligibility steadily declines with age. 22% of childless women are eligible at 25 and this number drops to less than 14% by the time they reach 64. In Figure B.3, I show that childless men's eligibility by age follows more of a U-shape; 19% of childless men are eligible which declines to less than 8% at 57 and slightly increases to over 13% between 57 and 64. I define earnings as individual wage earnings for single adults and individual plus spouse wage earnings for married adults.

My treatment variation comes from differences in EITC generosity across and within states overtime. I define my main treatment variable as the maximum state EITC available to childless adults in a given state-year.

To assess the impact of the District of Columbia EITC expansion (DC expansion) for childless adults, I use data from a harmonized version of the American Community Survey (2010-2017) from IPUMS (Ruggles et al., 2020) to estimate the effects of the District of Columbia EITC expansion in 2015.

[4]My sample begins in 1994 because this is when both federal and state EITCs became available to childless adults.

[5]I also estimated the effects for childless adults with at least some post high school education, and found no evidence of employment effects.

Variation in state EITCs over time can be seen in Figure B.4. State EITCs in 2018 ranged from 3% (Louisiana) to 100% (DC) of the federal EITC, with a median of 20% of the federal EITC (approximately $100). The mean childless state EITC between 1994-2018 is $77, conditional on a state having an EITC. Table A.1 shows descriptive statistics for childless adults for men and women separately. The most notable differences between men and women are marriage rates, employment levels, work hours, earnings, and wages. The sample of women is also slightly older than the male sample, which is not surprising given that children are more likely to be living with a single mother than a single father, and single parents are excluded from my sample.

2.4 Empirical Strategy

I estimate the employment effects of the EITC for childless adults using a difference-in-differences approach, where treatment variation comes from changes in state EITCs over the years 1994-2018. I use variation in state EITCs rather than the 1994 federal expansion for childless adults for two reasons. First, the treatment and control groups are clearly defined. States that implemented an EITC are in the treatment group, and states that do not are in the control group, whereas with the federal expansion it is unclear which group would be a good counterfactual for childless adults. Second, the federal EITC expansion occurred during a significant economic boom and at the same time as the mid-1990s welfare reform. This makes it near impossible to rule out that employment effects are entirely driven by the EITC expansion. By using only variation in state EITC expansions, the identification assumption is that in the absence of state EITC expansions, changes in employment would have evolved similarly for expansion and non-expansion states. It's possible to calculate a predicted EITC value that is individual-specific based on their previous years earnings. However, estimates using this method will be biased because predicted EITC values are based on individual income, which is endogenous. Maximum state EITC, on the other hand, is not related to individual income and will capture both the occurrence and magnitude of expansions.

The generalized difference-in-differences framework can be summarized in regression form as follows:

$$Y_{i,s,t} = \beta_0 + \beta_1 StateEITC_{s,t} + X_{i,s,t}\beta_2 + \delta_s + \gamma_t + \varepsilon_{i,s,t}$$

In this equation i represents individuals, s represents state, and t represents year. The variable Y includes the following employment outcomes: whether workers were employed, log of hours worked per week (zeros are dropped), whether workers were in the labor force, and whether they were unemployed because they left their job. $StateEITC$ is the maximum state EITC for a state-year measured in \$100s. The vector X contains individual controls for marital status, and age and race fixed effects, and state-level economic factors such as the log of the real effective minimum wage, state-year indicators for whether pre-1996 welfare reforms were introduced, and state-year indicators for whether a state had expanded Medicaid through the Affordable Care Act. All regressions, unless otherwise indicated, are weighted using CPS ASEC population weights provided by IPUMS.

2.5 Results

In this section, I present my main results, and discuss reasons for differences across age, gender, household structure, and marital status.

2.5.1 Main Results

My main results are found in Tables A.2. For childless women with no post-high school education, I find that increasing the generosity of the maximum state EITC by \$100 increases employment by 1.55 percentage points (2.3% increase over a baseline employment rate of 65%). Effects are largest for younger childless women age 25-34 (3.53pp). Childless women over the age of 45-64 do not appear to change their employment following state EITC expansions. Aggregate employment effects for childless men are negative (-1.28pp). These declines are concentrated in older childless men age 55-64 (-2.93pp). Effects for other men's age groups are negative, but close to zero in magnitude.

The effect of the EITC on hours worked is insignificant for both men and women, but this is not surprising given that incentives to change hours worked depend on the design of the phase-in and phase-out rates and the length of the earnings range where the EITC reaches its maximum. My identification is based on the maximum state EITC amounts, so I will miss some of these nuanced changes in hours worked. I could have split the sample into earnings bins defined by distinct portions of the EITC schedule (phase-in, maximum, phase-out), but this introduces an endogenous income variable that is potentially correlated with the error term and hours worked. Rather than following the latter strategy, I plot earnings distributions for men and women in Figures B.5 and B.6 to see whether earnings are grouped around certain portions of the EITC schedule. Earnings are not distinctly grouped enough to be able to predict whether we should see an aggregate increase or decrease in earnings.

I also look at labor force participation rates and job leaving. In Table A.3 I show that for young childless women (age 25-34), labor force participation increased by a significant 2.82pp while job leaving declined by -0.018pp (not significant). These effects get smaller in magnitude by age. I find effects in the opposite direction for most groups of men. Men's labor force participation declined by 1.15pp (not significant) and rates of job leaving increased by 0.011pp (not significant).

By looking at effects in labor force participation and job leaving, I find that employment effects are driven from multiple sources such as the entry/exit of new workers into the labor force and increases/declines in job exits. Women's employment and labor force participation increased, and their rates of job leaving decreased after state EITC expansions. The opposite pattern arose for men. Men were less likely to be employed, and more likely to leave their jobs or drop out of the labor force. How entry and exit occurs matters for how policies like the Earned Income Tax Credit are evaluated.

2.5.2 Differences by Sex

Treatment should be affecting marginal workers. Workers on the margin can be defined as those who would find employment if their expected wage from employment exceeds their reservation wage, or conversely, would leave their jobs if their wage dropped below their reservation wage. Whether workers are on the margin of employment is not directly observable, so as a proxy I look at workers who report being unemployed. I consider this group on the margin of employment given that they are not employed but actively looking for a job.

Before looking at unemployed workers, I first look at baseline employment levels. Low baseline employment levels can signals which groups have the highest potential employment capacity. I find that mens' employment (74.9%) was approximately 10 percentage points higher than women's employment (65.6%).

Looking at the employment of marginal workers (unemployed), I find stark differences in "reasons for being unemployed" between childless men and women. In Table A.4, I find that men are much more likely to be unemployed because they lost their job (70.6% for vs. 53.2% for women), whereas women are much more likely to be unemployed because they are entering or re-entering the labor force (33.95% for women vs. 21.38% for men). Therefore, female marginal workers are more likely to be entering the labor force and willingly searching for jobs, whereas male marginal workers are more likely to be searching because of an involuntary job loss.

Why men experience job loss or lower employment and labor force participation in this setting is up for debate. One possibility is that wages are driven down below men's new reservation wage by the entry of new workers, causing men to leave their jobs. Alternatively, employers might be substituting male workers for female workers. Neumark and Wascher (2011) and Groves (2016) show evidence that welfare reform and EITC expansions caused male employment to be crowded out by single mothers entering the labor force because of these policy changes. This type of substitution could come from either job firings, declines in hiring, or voluntary leaving. Given that single mothers are the most likely to be claiming the EITC, it's possible that employers use gender as an imperfect signal of who is eligible for a wage subsidy (EITC), and hence would expect a lower wage offer.

One last reason for why mens' employment declines, not unrelated to the previous reason, is that men's reservation wages are simply higher than women's. This would make them more likely to become unemployed if wages decline. This is difficult to test empirically in the EITC setting, but existing research suggests that men do in fact have higher reservation wages than women (Brown et al., 2011; Caliendo et al., 2014).

For those that stay employed, in Table A.6, I show that wages significantly declined for older males (10% for every $100 increase in the maximum state EITC), the same group that I see declines in employment, labor force participation, and job tenure. These findings are line with firms either substituting away from older male workers to younger female workers or men leaving jobs as a result of declining wages.

2.5.3 Differences by Household Arrangements

In addition to considering differences between men and women, I also look at results by household arrangements. I expect results to differ by household arrangements because it is related to the opportunity cost of getting a job. For example, caregivers of elderly parents or children would have a higher opportunity cost of employment than adults who are not any type of caregiver.

Given the limited amount of research about the EITC and childless adults, I give a descriptive view of household arrangements and then show that employment results vary significantly by household structure.

To describe household arrangements of childless adults I first limit the sample to adults (25-64) earning in the federal EITC eligible range (less than $15k for single adults and $21k for couples that are married and filing taxes jointly), and then I look at household rosters and incomes to determine whether they are the largest contributor to earnings in the household.

As shown in Figure B.7, the majority of eligible single childless adults live with zero non-family household members, meaning that most are living either by themselves or with other family members. Going a step further, I break down living arrangements by the number of family members they live with. In Figure B.8 you can see that most childless adults in my sample live either alone or with just a few other family members. When looking at relationships to the householder, childless adults were most likely to be either a child of the householder or the householder themselves (Figure B.9). For those living with zero non-family members, 29% were living with either a mother or father (13% live with both).

Given that a significant share of childless adults are living with a parents, I show whether childless adults are receiving a majority of financial support from other household members or vice versa using household earnings rankings. I find that 62% of low-income childless adults, conditional on having wage earnings less than $15k/year, were still the highest earners in their household. For those who are a child of the householder, 41% are the highest earners in the household. Although childless adults living with parents are less likely to be the primary earners than if they were householders, there is suggestive evidence that a significant number of parents might be depending on their low-income childless children for financial support or care.

In summary, I find that the typical single childless adult lives alone or in a small household with just a few family members (most frequently a parent). Married childless adults, on the other hand, almost exclusively live in two-person households with their spouse. As far as household earnings goes, there is a substantial amount of heterogeneity in whether childless adults are primary or secondary earners in the household.

Next, I show how employment results differ by household arrangement. I organize single childless adults into three mutually exclusive household arrangements: living with your parents, living with other family than your parents, and living with non-family. Results by household arrangement can be seen in Table A.8. I find that the largest employment changes are concentrated in the group of childless adults living with family, particularly those living with their parents. For those living with their parents, childless women's employment increases by 3.08pp, while men's employment declined by 2.76pp. Hours worked per week did not differ significantly across household arrangement. Combining these findings with results by age suggests that EITC expansions are causing employment increases for younger women living with family and employment decreases for older men living with family. Those living exclusively with non-family do not have strong employment responses to EITC expansions.

The fact that the same household arrangement produces opposite employment effects for older men and younger women, suggests that there are other household-level characteristics that interact with EITC expansions that this study cannot account for.

2.5.4 Differences by Marital Status

In this section, I show how results differ by marital status. Employment responses for married and unmarried groups might differ for a couple reasons. First, married dual-earning couples face a "marriage penalty" in EITC reciept relative to married single-earner couples or non-married single adults. The "marriage penalty" occurs when a married couple's EITC is lower as joint-filers than if they had filed their taxes separately (as non-married single filers). Second, the magnitude of the "marriage penalty" was reduced in 2001 at the federal level by extending the EITC maximum earnings eligible range. In other words, the maximum EITC for married couples began to phase out at higher earnings levels than that of single adults.

Results by marital status can be seen in Table A.9. I find that on average, unmarried childless women are almost twice as likely to increase employment relative to married childless women (1.56pp vs. 0.8pp), however I cannot rule out these estimates are different from zero at the 5% significance level. Employment effects are negative and insignificant for both married and unmarried men. Hours worked increased by about 1.2% for both unmarried and married women, but this increase is not significant. Unmarried childless mens' employment declined by about 1.25pp (significant at the 10% level). Married mens' employment declines by 1.0pp (not significant). Similar to the results by age, mens' hours worked does not change following EITC expansions. These results suggest that marital status is not nearly as strong of predictor of employment effects as sex or household arrangements for childless adults.

2.6 Exploring Non-EITC Explanations For Employment Effects

The potential pitfalls of using a difference-in-differences strategy is that employment changes or trends may be correlated with changes in EITC expansions. In the next sections I show that the employment effects I find cannot be explained by state economic conditions, welfare reforms, forward-looking future mothers, or childless adults claiming other adults' children in the same household.

2.6.1 State Economic Conditions

Given that I find such large employment effects for single childless adults, there might be a potential concern that my estimates are biased upward by economic expansions or tax increases that are correlated with the timing of EITC expansions (Bastian and Jones, 2018; Bastian, 2017b; Leigh, 2010). To address this concern, I implement a triple difference approach (DDD), where in addition to using variation in state EITCs across states and time, I use variation within state-years between eligible and ineligible groups, where eligibility is defined by age. Childless adults are only able to receive the EITC if they (or their spouse) were age 25-64 sometime during the tax year. Therefore I call childless adults "Eligible" if they (or their spouse) are age 25-64, and "Ineligible" if otherwise. I restrict my sample to all childless adults with no more than a high school degree and age 18-64. I only include those with no more than a high school degree because college students older than 18 can be claimed as dependents on their parent's tax return, which makes them ineligible to claim the EITC for themselves.

If my results hold, then this provides strong evidence that state EITC expansions are driving my results, rather than state-specific factors. The underlying assumption being made here is that state-specific factors such as economic expansions and changing tax rates should affect eligible and ineligible childless adults similarly. The triple difference (difference-in-difference-in-differences) specification I estimate is:

$$\begin{aligned} Y_{i,s,t} = \beta_1 StateEITC_{s,t} \cdot Eligible_i + X_{i,s,t}\beta_2 + \delta_s + \gamma_t + \alpha_{st} \\ + Eligible_i \cdot \delta_s + Eligible_i \cdot \gamma_t + \varepsilon_{i,s,t} \end{aligned}$$

In this regression, i represents individuals, s represents state, and t represents year. X contains controls for marital status, age and race fixed effects, and state-year level variables such as the log of the real effective minimum wage, an indicator for pre-1996 TANF waivers, and and indicator for Medicaid expansion through the Affordable Care Act. $Eligible$ is an indicator for an individual or their spouse being age 25 or older, which is interacted with $StateEITC$, the maximum state EITC for childless adults in \$100s of 2017 dollars. β_1 is the coefficient of interest and measures the effect of expanding the state EITC by \$100 on employment for eligible workers (25 or older) relative to ineligible workers (younger than 25). I don't include an $Eligible$ dummy because age fixed effects eliminates the need for this control. I also do not include $StateEITC$ because I include state-by-year fixed effects and $StateEITC$ does not vary within state-years.

In Table A.10, I find large positive employment effects for childless women. Expanding the maximum state EITC by \$100 led to a 1.67 pp increase in employment for eligible childless women relative to ineligible childless women. I also find that eligible childless women increase hours worked per week by 0.646 on average. For childless men, employment declines and work hours increase relative to ineligible childless men, but these differences are small and insignificant.

An alternative way to do this analysis to run the same regressions as I do for childless adults age 25-64 for ineligible childless adults age 18-24. These results can be seen in Table A.11. Employment and hours effects are small and insignificant for this group, suggesting that the labor supply changes I find for eligible childless adults cannot be explained by state level factors that are common to both eligible and ineligible childless adults.

The triple difference approach and placebo analysis on ineligible groups, both confirm that state-level economics shocks correlated with EITC expansions cannot explain the employment effects I find.

2.6.2 State Employment Trends

To supplement my difference-in-difference employment results, I also conducted a state-year panel event study to test whether my results appear to be driven by pre-existing trends in employment that are correlated with state EITC expansions. I estimate the following specification and plot the coefficients separately for men and women:

$$Y_{s,t+j} = \beta_0 + \beta_1 StateEITC_{s,t} + \delta_s + \gamma_t + \beta_2 Y_{\{s,j=-1\}} + X_{s,t}\beta_3 + \varepsilon_{s,t}$$

for $j \in [-5, 5]$. Each j represents a different regression, s represents state and t represents year. Including $Y_{\{s,j=-1\}}$ controls for state employment in the year prior to EITC expansion ($j = -1$), which means that β_1, the coefficient of interest, is interpreted as the effect of a \$100 increase in the maximum state EITC relative to the year before expansion. The employment results for women are shown in Figures B.10 and B.11 and employment results for men are shown in Figures B.12 and B.13. Labor force participation results look very similar to employment effects, and are available upon request.

Based on these results I do not find significant cause for concern that pre-existing trends are driving the increase in women's employment or labor force participation. When the older males are included in the sample, I show that male employment is declining slightly prior to expansion. These declines are not statistically significant from zero, but I would caution readers to interpret the results for male employment with the caveat that declines in male employment might potentially be driven by other state-level factors than EITC expansions.

2.6.3 Welfare Reform in the 1990s

Some recent papers have expressed concern with the EITC employment literature using variation in federal expansions occurring in the 1990s (Looney and Manoli, 2016; Mead, 2014). In short, the concern with these large EITC expansions is that many states are also reforming welfare programs between 1992-1996, and then in 1996, the Personal Responsibility and Work Opportunity Reconciliation Act of 1996 (PRWORA) was signed into law replacing the Aid to Families with Dependent Children (AFDC) program with the Temporary Assistance for Needy Families program (TANF). TANF's objective was to make welfare temporary and require participants to enter the work force. States were also given more discretion on designing participation rules and how to spend TANF dollars. The Center on Budget and Policy Priorities show that direct cash payments to welfare recipients has declined dramatically since the implementation of TANF. In 2017, only about 23% of the block grants were used for cash assistance (Reich et al., 2017). Therefore it's possible that employment effects cannot be solely attributed to EITC changes during this time period because of welfare reforms (Kleven, 2019). Using state expansions alleviates this concern, especially since most of the state expansions occurred after PRWORA.

2.6.4 Forward-looking future single mothers

Expanding the EITC for childless adults might not have the intended effect on employment if effects due to childless women preparing to collect the much larger "single mother EITC" by working now for when they have their own child in future months. Under this alternative scenario, my interpretation of the estimates, as the employment effects of expanding the EITC for childless adults would be incorrect.

I follow two strategies to investigate whether the interpretation of my estimates is correct. First I calculate what share of single childless women are expecting to have a child during the following year to see what share of my sample would be affected by a misinterpretation of estimates. Second, I look at whether EITC expansions affected the total number of childless women.

I use the panel nature of the CPS combined with National Vital Statistics estimates to calculate the share of single women who intend to have a child in the following year. For a subset of the interviewees in the CPS, individuals can be linked for up to 2 consecutive March interviews (e.g. March 2017 and March 2018 can be linked) [6]. Based on this linked sample, I find that about 2.3% of single childless adults in the range of household income eligibility for a "single mother EITC" (less than about $40000/year) will have a child in the following year. Hymowitz (2014) using National Vital Statistics data show that intended births in the US (as a share of all live births) have stayed constant at about 40% since the early 1990s for never married women. A back of the envelope calculation suggests that only

[6] For information on CPS linkages see Rivera Drew et al. (2014)

about 0.92% (0.023 * 0.4) of single childless women expecting the EITC had a planned birth the following year. Given this is such small proportion of single childless women, it seems unlikely this group is driving the effects.

I also look at whether state EITC expansions affect the number of childless women in the US. I do this based on the reasoning that childless women who want to take advantage of the more generous single mother EITC will subsequently have children, causing the number of childless women to fall. To do this, I first construct a state-year panel and regress the log of the number of single childless women in a given state-year on the maximum state EITC in \$100s.

$$Log(NumChildlessWomen)_{s,t} = \theta_0 + \theta_1 StateEITC_{s,t} + X_{s,t}\theta_2 + \delta_s + \gamma_t + \varepsilon_{s,t}$$

This regression includes state and year fixed effects and the same state level controls used in my analysis of employment effects. I find that for every \$100 increase in the maximum state EITC, the number of childless women decreases by about 0.02% (p-value = 0.907), a very small change that is statistically insignificant.

Given that the share of single childless women expecting a child in my sample was only 0.92%, and the number of single childless women does not significantly decline after treatment, there is little evidence to reject the assumption that childless women are responding to expansions of the EITC for childless adults.

2.6.5 Childless Adults Claiming Others' Dependents

Given that most of the state EITC expansions affect the generosity of the EITC for adults with and without dependents, there is the possibility that households are optimizing EITC claiming by having the adult or whose income the maximizes household EITC benefits, claim the children in the household. This type of claiming behavior would cause a misinterpretation of my results, in the same way as the previous subsection because individuals would be responding to the EITC incentives for households with dependents. In Table A.7, I show results for the sample of household that do not have any children present. Dropping childless adults living in households with children reduces the sample size by about 16% for both men and women. I find that these are results are comparable with the main results I found in Table A.2. This suggest that strategic claiming in the household is not driving the main results.

2.7 The 2015 District of Columbia EITC Expansion Solely for Childless Adults

Prior to 2015, most state EITC expansions were characterized by increases in EITC generosity for federal EITC eligible adults with or without dependents. The 2015 District of Columbia expansion of the EITC (DC EITC expansion) differed in a few distinct ways from that of previous state EITC expansions. First, the DC EITC expansion was specific to childless adults. Second, DC EITC generosity increased from 40% (maximum $\approx$ \$210) to 100% (maximum $\approx$ \$520) of the federal EITC. Third, the annual earnings eligibility range increased from about \$15k to \$24k, which also increased the number of potential eligible childless EITC claimants. Muhammad (2019) finds that the number of childless EITC claimants increased by 12,490 between, while the number of claimants with dependents stayed relatively fixed. They also found that 76% of new childless claimants were earning in the new range of income eligibility (\$15k-\$24k), and the rest were earning in the Federal EITC eligible range (\$1-\$15k). The 24% of new claimants that were not earning above \$15k could have come from two sources, new workers, or those who did not claim the EITC in previous years because the opportunity cost to apply was too high relative to the benefit. Previous research has shown that claiming increases with EITC amounts (Blumenthal et al., 2005). If the 2015 DC EITC expansion only caused claiming to increase rather than increases in new workers, I should find little to no employment effects.

For the analysis of the the EITC expansion in DC, I used a harmonized version of the American Community Survey for years 2010-2017 (Ruggles et al., 2020) with a difference-in-difference empirical strategy to estimate the effects of the DC expansion without a simultaneous expansion for households with dependents and an extension of the EITC to higher incomes. The advantage of using the ACS is that it has a much larger sample size than the

CPS, but it only is available starting in 2010, after many of the EITC expansions had already occurred. This is the reason why the ACS is not used as my primary source of data for employment effects of state EITC expansions. I restrict the sample to childless adults age 25-64 with no more than a high school degree.

In my difference-in-differences framework, I define childless adults in DC as the treatment group, and childless adults in states that never expanded the EITC as the control group. The identifying assumption is that childless adults in never expanding states provide a good counterfactual for childless adults in DC, in the absence of a 2015 EITC expansion. I show evidence of this assumption by showing trends in employment between DC and non-EITC states before and after the 2015 DC EITC expansion. In Figures B.15 and B.16, it appears that employment between DC and non-EITC states trended parallel for both men and women until the 2015 expansion. Employment is elevated in the post expansion period for DC women. For men, employment levels in 2015 and 2017 are elevated, but 2016 is not.

To estimate changes in employment, hours worked, and labor force participation. I estimate the following regression equation:

$$Y_{i,s,t} = \beta_0 + \beta_1 DCexpansion_{s,t} + X_{i,s,t}\beta_2 + \delta_s + \gamma_t + \varepsilon_{i,s,t}$$

where X is a vector including age and race fixed effects, and controls for marital status, education, ACA expansion status, and the log of real effective minimum wage. $DCexpansion$ is an indicator for being in DC during the treatment period (2015-2017). δ_s and γ_t are state and year fixed effects, respectively and Y includes employment outcomes such as employed, labor force participation, and log usual hours worked. [7]

Results for men and women are shown in Tables A.12 and A.14. The DC expansion of the EITC increased women's employment by 5.78pp and decreased men's employment by

[7]Usual hours worked in the ACS differs slightly from the hours worked variable I use in the CPS. CPS respondents report hours worked the week before their March interview, whereas ACS respondents are interviewed on a rolling basis throughout the entire year. This means that if employment effects are different in March relative to other months, estimates for effects of EITC expansions on hours worked would be different between the CPS and ACS.

2.59pp. Increases in employment were driven by younger childless women and decreases in employment were driven by older men. This result is consistent with the results I found using variation in other state EITC expansions with CPS data. Hours worked increased by 13.6% for women aged 35-44 (no significant change for other female age groups), and decreased for older men (2.6-5.6%). Changes in labor force participation do not exactly mirror employment effects. Women's labor force participation increases less than employment (in percentage points), while men's labor force participation declines more than employment (in percentage points). This suggests that the DC EITC expansion caused women to, both, enter the labor force and increase their employment attachment (declining job exit rates). Men on the other hand exited the labor force or stayed employed, but saw work hours cut by employers or voluntarily worked less hours. This is consistent with findings from Wilson (2018) that show that increased job attachment is an important mechanism for employment effects of the EITC.

The previous approach showed the aggregate effect of the DC expansion on employment. To make estimates more directly comparable with results using other state expansions in the CPS, I replace $DCExpansion$ with a measure of the maximum DC EITC in $100s of 2017 dollars. These results are shown in Tables A.13 and A.14. I find that women's employment increased for all childless women by 0.065pp, which is about half the magnitude of the estimate I found in the CPS analysis. The differences in these estimates might be explained by the fact that the DC EITC expansion reached a higher range of income levels and was already relatively generous compared to other states' EITC programs. However, it could also have been that childless adult employment was already high in 2015, so there was less room for employment growth. In Figure B.14, you can see that employment in DC was about 10 percentage points higher than in non-EITC states. It's likely both of these explanations are at play, but data restrictions prevent me from unambiguously stating what the main mechanisms are, which leaves room for future research.

In summary, I find large increases (decreases) in employment, hours worked and labor force participation for women (men), following the expansion of the DC EITC to childless adults. The ACS dataset was ideal given the large annual sample available across all states. However, I would caution readers to take the DC results with a grain a salt given that the DC sample is still quite small after conditioning on adults having no more than a high degree. There were about 1,687 observations across 4 years, in the DC post-period, which is small enough to cause figures and tables to have estimates that are quite noisy.

2.8 Discussion

The effects of the Earned Income Tax Credit for childless adults has been understudied, especially considering that the EITC has been available to this group since 1994. Looking forward, future changes to state and federal EITCs for childless adults could take many forms, including but not limited to, expanding eligibility beyond age 25-64, extending the range of income eligibility, or raising the maximum credit.

Overall, I find that the EITC significantly increased employment and labor force participation for childless women, particularly younger childless women, while employment for older men significantly declined. The 2015 District of Columbia EITC expansion, the first to implement a childless adult specific EITC expansion, increased the employment of women by 5.78pp and decreased the employment of men by 2.59pp, a result that is consistent with findings from studying other state expansions of the EITC.

Marital status was not a strong predictor of employment effects, however, household arrangement was. Single childless women living with family members increased employment, while single childless men decreased employment. These results suggest that other household dynamics beyond household arrangement differentially affect the role that the EITC plays on employment for men and women. I also found that the direction and magnitude of employment effects were correlated with a few key statistics. First, groups with lower levels of baseline employment saw larger increases in employment. This was true for earlier expansions as well as the DC expansion. Second, positive employment effects were largest for groups that reported being unemployed because they are entering the labor force (rather than because they lost their job). These correlations suggest that the effectiveness of EITC expansions for childless adults, at least in terms of finding pro-work effects, depends strongly on the baseline characteristics of marginal workers. States looking to expand EITCs further should at least consider employment levels, and reasons for unemployment, all of which are related to these marginal workers. Federal and state policymakers would be wise to also carefully weigh the costs and benefits of policies, like the EITC, that are designed to encourage work. While employment and labor force participation can increase for some groups, there is always the possibility of unintended consequences for other groups. For this reason, future researchers should continue to study how effects of the EITC differ between different types of individuals and households on a variety of outcomes.

3. PUBLIC HEALTH INSURANCE AND RETIREMENT DECISIONS

3.1 Introduction

Understanding how and why health affects retirement decisions is key to the dynamics of labor markets and highly salient to public policy. Health is a major determinant of labor force participation (Currie and Madrian, 1999; Gruber and Madrian, 2002; Prinz et al., 2018). Poor health can make workers less productive and take time away from work, as formalized in the Grossman (1972) model; while financial risk associated with health shocks might make labor market attachment more crucial to sustain financial well-being. Institutional features of an economy such as the availability of public pensions, health insurance, and disability insurance can have important interactions with both work decisions and health status (French and Jones, 2011), and the long-run financial stability of these institutions depends critically on decisions about when and how people retire.

In the United States, eligibility for health insurance is strongly tied to labor market participation, which can lead to job lock (Madrian, 1994). Public health insurance programs weaken this tie by providing an opportunity for workers to change jobs or retire without concern over losing their benefits, but have historically had exclusive eligibility criteria that limit their relevance for the non-elderly population. In the last two decades, many states have expanded their Medicaid programs to reach adults at higher income levels and eliminated categorical eligibility requirements such as disability or responsibility for a dependent child.

The increased availability of public health insurance alters the net benefits of work with the potential to cause shifts into or out of the labor force, particularly in the population of older workers. Older workers, who face worsening health and increasing medical costs and will value health insurance much more highly than younger workers, making job lock more likely if they face the same insurance premiums. Health insurance is also an important input into health, and could lead to increased labor force attachment, if public insurance sufficiently boosts worker health. Additionally, for a worker on the margin of retirement, if the value of leisure is greater than the marginal product of labor they should retire; but they may be unwilling to do so if their job offers insurance that they cannot obtain elsewhere (at a similar price). Older workers without employer-sponsored health insurance will see a shift in their non-labor income proportional to their valuation of public insurance, which could lead to shifts into or out of the labor force.

The purpose of this paper is to examine whether the availability of public health insurance prior to retirement age affects early retirement. Specifically, we examine the behavioral effects of increased Medicaid availability in older adulthood on retirement decisions, including retirements status, work hours, and Social Security claiming, an important determinant of financial security in retirement. We use the natural experiments created by state decisions to expand Medicaid to estimate the effect of expanded public health insurance coverage on retirement decisions in the nationally representative Current Population Survey Annual Social and Economic Supplement (CPS). We combine the CPS with detailed data we collected on the numerous state expansions of public health insurance that took place from 1996 – 2013. The CPS is widely used to measure labor market activity and serves as an important benchmark.

We find that public insurance access led to large declines in uninsurance rates and delays in retirement for older adults without dependents. Following Medicaid expansions, older adults, especially women, became more likely to delay retirement and Social Security claiming until age 65. At age 65, both workers and retirees can enroll in Medicare, the public insurance program for the elderly, without restriction. Our findings show Medicaid is a potentially important bridge for insurance access for the near-elderly population. In fact, expansions to Medicaid can lead to increases in labor supply which can potentially offset some of the costs of implementation through increased tax revenue and reductions in Supplemental Security Income (SSI) use.

3.2 Background

Two streams of research support the hypothesis that Medicaid expansions may influence retirement decisions, Social Security claiming and retirement income. First, recent studies indicate that Medicaid expansions to non-elderly adults without dependent children may cause larger labor supply responses among those close to retirement age (Dague et al., 2017; Garthwaite et al., 2014). Second, a large empirical literature on health insurance and retirement generally finds that the availability of health insurance that is independent of employment (e.g., Medicare or retiree health insurance) hastens retirement. Medicaid, like retiree insurance, can bridge the time between leaving work and eligibility for Medicare. This relationship between health insurance and retirement may be particularly salient for Medicaid-eligible adults who have low-incomes and report poor health relative to their privately insured peers. There are two recent studies that consider the effects of Medicaid expansions on retirement-related decisions. Levy et al. (2016) study the impact of the Affordable Care Act (ACA) and find no differences in retirement or part-time work coincident with the 2014 implementation of ACA or across states that did and did not expand Medicaid in 2014. Aslim (2019) and Wood (2019) also study the impact of the ACA Medicaid expansions, but find increases in early retirement for subgroups of women and adult couples.

We extend available research on Medicaid and retirement in several directions. The research design takes advantage of Medicaid expansions from 1996–2013, enabling us to isolate the effects of Medicaid expansions from the ACA-related changes in the private health care market in 2014. Our original dataset on Medicaid coverage characteristics for all states and years from 1996-2013 allows us to define Medicaid coverage according to income limitations, coverage generosity, and enrollment caps in addition to the binary expansion or non-expansion formulation. These more nuanced measures of Medicaid expansions may better predict retirement-related outcomes to the extent that program characteristics affect the perceived value of Medicaid coverage among prospective beneficiaries. Additionally, we assess a broad set of retirement-related outcome measures including overall labor supply, the decision to retire, and Social Security income.

3.3 Data

We use two data sources to study these questions: 1) a dataset we constructed that includes detailed information on state Medicaid programs for non-elderly, non-disabled adults from 1996-2013, the Medicaid Waiver Dataset (MWD) and the Current Population Survey Annual Social and Economic Supplement (CPS) from survey years 1997-2013.

The Medicaid Waiver Dataset characterizes adult Medicaid coverage in each state and the District of Columbia from 1996 - 2013 including income eligibility limits, enrollment caps, and coverage characteristics. We constructed the MWD through a systematic review of state and federal Medicaid documents, research publications, and onsite data collection at the Centers for Medicare and Medicaid Services. The Medicaid Waiver Dataset is made publicly available by (Burns et al., 2016) [1].

The Annual Social and Economic Supplement to the Current Population Survey is the largest national survey that contains both labor market and health insurance measures during the time period of interest. We specifically use the harmonized CPS data from IPUMS (Flood et al., 2020). In the CPS, we use the 1997-2013 surveys (1996-2012 reference years) and match to reference year expansions that happened prior to July 1.

The sample for our first empirical strategy uses the near-elderly civilian population (ages 50-64) of adults without dependent children. Our second empirical strategy uses the same population, but restricts age to 55-75. Some analyses specifically include various subgroups defined by educational status or health status, which are defined when introduced. We make no restrictions on income, which is endogenous. We also don't restrict the sample based on group quarter residence. The CPS sample includes only non-institutional group quarters (no nursing homes). In some cases, we include as controls annual state level unemployment rates from the Bureau of Labor Statistics.

Outcome variables of interest include health insurance coverage, probability of reporting

[1] See the reference page for the website link

being retired, probability of reporting being newly retired, Social Security income amounts, and having positive Social Security income. We define individuals as being retired if they responded being retired as the reason for not being in the labor force during the previous year. Individuals in our sample are considered newly retired if they reported being retired as the reason for being engaged in the labor force for only part of the previous year. Table A.15 reports selected population characteristics including outcome variables for each dataset by states' change in their Medicaid expansion status during the period. Data are unweighted.

3.4 Empirical Strategy

We take two methodological approaches. First, we use a difference-in-differences design in order to estimate the effect of Medicaid coverage expansions for adults without dependent children on retirement decisions, Social Security claiming and retirement income. In particular, we compare our outcome variables in states that changed Medicaid coverage for adults relative to those that did not, before and after the change occurred. The key identifying assumption behind the difference-in-differences analysis is that of parallel trends. We assume states that did not expand (or had not yet expanded) Medicaid would have had similar trends in the outcome variables, conditional on observables included in the model, as those that did, so that those states and years provide a good counterfactual. During the 1996-2013 time period, 26 states expanded or contracted adult Medicaid coverage for adults without dependent children (i.e., childless adults) at least once. These Medicaid policy changes are distributed across the entire study period. The generalized difference-in-differences specification we estimate is:

$$Y_{i,s,t} = \beta_0 + \beta_1 Expansion_{s,t} + X_{i,s,t}\beta_2 + \delta_s + \gamma_t + \varepsilon_{i,s,t}$$

In this individual level regression equation, i represents individuals, s represents states, and t represents years. Expansion is an indicator for whether or not the individual lived in a state that had a Medicaid expansion in a particular year; Y represents the outcome under consideration. Characteristics that vary at the individual-state-year level are represented by X_{ist}, δ_s are state fixed effects, and θ_t are year fixed effects.

Second, we implement a difference in discontinuities version of the estimator in which those 65-75 (Medicare eligible) are compared to the 55-64 age group (not Medicare eligible). This approach is motivated in Figure B.18, which pools data from all study years and shows uninsured rates by age for individuals who reside in states that always, changed, or never had Medicaid expansion in place. While the level of uninsurance is highly differentiated depending on whether a state has a Medicaid expansion in place up to age 65, at age 65 health insurance status is uniform across states. The need for public health insurance through Medicaid as a primary source of coverage disappears at 65 because of the availability of Medicare, motivating the difference in discontinuities design. [2] This method compares the size of the drop in the outcome variables at age 65 across state-years that did and did not have Medicaid expansions in place. Medicare eligibility does not vary by state or year during this period because it is a federal program. While the generosity of Medicaid coverage for the elderly can vary by state due to income and asset counting rules and 209(b) status, Medicaid eligibility for the elderly was not changing at the same times that the expansions to the non-elderly populations were occurring, so those differences can be accounted for through fixed effects. The specification we estimate is:

$$
\begin{aligned}
Y_{i,s,t} = \theta_0 + \theta_1 Expansion_{s,t} + \theta_2 Expansion_{s,t} \cdot MedicareAge_i \\
+\theta_3 MedicareAge_i + X_{i,s,t}\theta_4 + \delta_s + \gamma_t + \varepsilon_{i,s,t}
\end{aligned}
$$

The model is estimated using local linear regression with flexible age controls, specifically allowing for different relationships between age and the outcome variables below and above 65 in X. Age controls were recentered around age 65 by subtracting 65 from individual-level reported ages. $Expansion$ is an indicator for whether states have imple-

[2]We do not use a triple differences approach because we might expect that labor market trends in the near elderly and elderly populations are not responsive to the same external phenomena (and therefore would not satisfy the necessary parallel trends assumption). In the difference-in-discontinuities, we do not need parallel trends for these two groups.

mented Medicaid expansions and $MedicareAge$ is an indicator for age 65 or older. The coefficient θ_3 gives the average size of the gap in the outcome measure at age 65, which we call the “baseline discontinuity". The coefficient of interest, θ_2 then measures the additional effect of Medicaid expansion on the gap, which we call the “difference-in-discontinuity". This empirical strategy requires the assumption of strict exogeneity of the Medicaid expansions to the outcome of interest, in addition to exogeneity of the age threshold, and assumes that in absence of the expansions, there would be no difference in the drop in uninsurance at age 65 conditional on the variables in the model.

We also examine the sensitivity of estimates to the inclusion and exclusion of various control variables and treatment states and subsamples such as controls for demographics, lagged state unemployment rates, changes in unemployment rates, and state-specific linear time trends.

3.5 Results

3.5.1 Main Results

We first study uninsurance to obtain a sense of the “first stage” effects of the expansions. While this should not be interpreted as a strict IV first stage, since the exclusion restriction may not hold, we think it is useful to have a benchmark for the relative size of these expansions. These results are shown in Tables A.16 and A.17 and Figure B.19. The generalized DD in our preferred specifications (including demographic and unemployment controls) suggests a 1-2 percentage point decrease in uninsurance as a result of the average early expansion to childless adults, which is approximately 1/3 the size of the expansions associated with the Affordable Care Act as estimated by Courtemanche et al. (2017). The difference in discontinuity estimates suggest a baseline discontinuity in uninsurance at age 65 relative to the near elderly of 11 percentage points, which is reduced by 3 percentage points if a state had a Medicaid expansion. The differences across methods may mean that there is heterogeneity by age in the effects; for adults close to the Medicare eligibility threshold, Medicaid eligibility may serve as an important bridge to coverage. These results also suggest that looking at the broad population estimate from the generalized DD may be misleading for considering marginal retirement incentives. For these reasons, we focus on the difference in discontinuity results for the retirement outcomes.

Results for retirement outcomes are reported in Tables A.18 and A.19 and Figure B.20 Difference-in-differences results for the retirement outcomes are null for all specifications. The difference-in-discontinuity results suggest a baseline increase in retirement of 8 percentage points at age 65 in the CPS, and the difference in discontinuities across expansion and non-expansion states is positive and statistically significant in both samples, suggesting that Medicaid expansion exposure increases the likelihood of retiring at 65 (rather than prior to 65). In Table A.20, general labor supply results indicate a reduction in net hours worked of roughly two hours per week in the CPS at 65. Difference-in-discontinuity coefficients in Table A.21 are not statistically different from zero, suggesting no additional relationship between labor supply and Medicaid expansion. In Table A.23, for reporting being newly retired, there is no statistically significant age 65 discontinuity overall, but there are differences across Medicaid expansion and non-expansion states. Difference-in-difference results in Table A.22 suggest a small increase in the probability of reporting being newly retired at 65 in expansion states. If Medicaid increased the likelihood of early retirement, we would expect a reduction in the gap at 65 for each of these outcomes, implying a difference in discontinuities coefficient of the opposite sign than the baseline discontinuity coefficient. However, we find that for retired and newly retired outcomes, the discontinuity at 65 is larger in expansion versus non-expansion states.

We next turn to Social Security (SS) related outcomes. For these specifications we restrict the sample to 62 or older because this is the only group eligible to claim SS. Tables A.24 and A.25 show results for the probability of claiming SS and Tables A.26 and A.27 and Figure B.21 show results for the amount of SS claimed. Difference-in-differences results for the probability of claiming and amount claimed from Social Security are null. There is a baseline discontinuity in Social Security claiming at age 65 of 9.8 percentage points with a difference across Medicaid expansion and non-expansion states of an additional 1.2 percentage points, suggesting an increase in the gap in Social Security claiming at age 65. These results are echoed by the total amount of Security Income an individual receives (we still keep individuals receiving zero dollars from Social Security). There is a baseline discontinuity in SS Income of about $870 at age 65. This discontinuity increases by about $400 for expansion states.

If Medicaid increased the likelihood of Retirement or SS claiming, we would expect a reduction in the gap at 65 (negative difference in discontinuities coefficient), therefore we interpret these results as near retiree age adults delaying retirement until age 65 following Medicaid expansion.

Lastly, we find that results for health insurance coverage and retirement outcomes are robust to the inclusion of controls for demographics, unemployment controls, state-specific linear time trends across all specifications and state-by-year fixed effects in the difference-in-discontinuity specifications.[3].

3.5.2 Alternative Treatment Variation: Coverage Generosity

Many states not only categorically expanded Medicaid to childless adults over the sample period, but also changed maximum earnings eligibility thresholds. These changes are illustrated in Figure B.17. Over 2001-2013, the number of states with income thresholds below 100% FPL declined and the number of states with thresholds above 100% FPL is increased.

Our uninsured and retirement estimates remain consistent using maximum earnings eligibility thresholds as our treatment rather than using a binary indicator for whether Medicaid expansion was in effect. Maximum earnings thresholds are coded as the state-year average maximum earnings threshold as a percent of the Federal Poverty Level (FPL). We divide the maximum income threshold by 100, so that estimates can be interpreted as the effect of

[3]We do not use state-by-year fixed effects in the difference-in-differences specification because Expansion does not vary within state-years for the age 50-64 population

raising income threshold by 100% of the FPL on outcome discontinuities at age 65. In our sample, maximum earnings thresholds ranged from 10% FPL (Colorado) to 400% FPL (Tennessee). The most frequent cap levels were 100% FPL (27% of expansion periods), 200% FPL (27% of expansion periods), and 133% FPL (18% of expansion periods).

Difference-in-differences specifications show a decline of 0.7 percentage points in uninsurance and null results for retirement outcomes in Table A.30. Table A.31 shows that a 100% FPL increase in state Medicaid earnings thresholds closed the gap in uninsurance at age 65 by 1.71 percentage points. The discontinuity in retirement increased by 0.44 percentage points in expansion states over a baseline discontinuity of 8 percentage points in non-expansion states. SS claiming results were comparable to retirement results. Expansion led to an increase of $223 over a baseline discontinuity of $923 in non-expansion states. These results suggest that intensive margin changes in income thresholds for states that have already expanded Medicaid to low-income adults also have large implications for labor supply and retirement outcomes.

3.5.3 Crowd-out of Private Insurance

One issue this paper has to contend with is whether Medicaid expansions crowd-out private insurance. In other words, once public insurance is an option, a subset of the population that is already insured through employers or the private market, might drop their private coverage and enroll in Medicaid. To test this, we show our difference-in-differences and difference-in-discontinuity specifications with private insurance as an outcome in Tables A.28 and A.29. The difference-in-difference estimates show small insignificant increases in private insurance from Medicaid expansion for the 50-64 year old population. The difference in discontinuity estimates show that the gap in private insurance at 65 grew mildly after expansions. If Medicaid was crowding out private insurance we'd expect the gap in private insurance at age 65 to get smaller. Across both methodologies we do not find evidence of private insurance crowd-out for the age 50-64 population from Medicaid expansions.

3.6 Discussion

Current projections of the economic effects of health care reform are subject to considerable uncertainty. Broad labor market trends suggest that challenges for public policy on labor and health will only continue to grow Buchmueller and Valletta (2017). This uncertainty is partly a function of the small number of studies that quantify the impact of Medicaid expansions on labor market outcomes including employment, earnings, and particularly, retirement. These projections are nonetheless a key decision-aide in federal policy-making in the domain of health and social welfare. With a focus on retirement outcomes, we provide new and rigorously estimated potential inputs to applied economic models that seek to capture the economic effects of Medicaid expansions beyond the health care sector. Our study's extensive observation period (1996-2013) allows us to consider the consequences of Medicaid expansions when implemented in relative isolation (i.e., before the Affordable Care Act).

Overall, our estimates suggest that Medicaid expansion can be an important source of health insurance coverage for older adults, reducing the gap in uninsurance at age 65 for the near elderly by 3 percentage points. Our results for retirement and labor supply do not suggest that Medicaid encourages early retirement or Social Security claiming; rather, we find that states with a Medicaid expansion program have a larger discontinuity in retirement and Social Security claiming at age 65, which suggests that it may instead allow individuals to maintain labor force ties until they reach age 65. Further work to test the robustness of these results, examine mechanisms, and tie expansions to within-person retirement decisions should be performed.

4. OCCUPATION AND INDUSTRY MOBILITY OF LOW-WAGE WORKERS

4.1 Introduction

Low-wage workers as shown in the previous sections are the frequent target of state and federal welfare reforms. In this paper, we look at the occupational and industry mobility of low-wage workers. This paper contributes to a large literature concerned with understanding worker mobility, employee-employer matching, and markets for low-wage labor. Low-wage worker mobility across occupation and industry has strong implications for studies that either condition on these dimensions to restrict to policy-affected populations or use them to define labor markets.

The data we use use for our analysis comes from the Survey of Income and Program Participation (SIPP) 2001, 2004, and 2008 panels. The SIPP tracks monthly employment, income, demographics, and program participation of individuals within households for up to 5 years. Because of its monthly frequency, panel length, and large sample size, the SIPP is an ideal dataset to track US job mobility.

We find that occupation switches are more slightly common than industry switches. Our results suggest that workers switching industries are most often changing occupations as well. Switches occur over short time periods and are not limited to just a small subset of low-wage workers. We find that almost 13% of low-wage workers changed occupation and industry at least once within a year. The share of workers switching occupation and industry is 25% over 2 years, and 29% over 3 years. Occupation and industry mobility is negatively correlated with wage levels and job tenure. Surprisingly, demographics such as race and sex are not strong predictors of occupation or industry switching after conditioning on wage levels. Overall, our results suggest that occupation and industry are not fixed characteristics of low-wage workers.

4.2 Background

4.2.1 Job Mobility

There is a long literature that documents job mobility. Some of the baseline work in this literature includes Topel and Ward (1992), Rosenfeld (1992), Neal (1999), (Neal, 1995), Flinn (1986), and Loprest (1992). This literature shows substantial job mobility across young workers. They also find that job switching is one of the leading contributors to wage growth which suggests that human capital is industry and occupation specific rather than firm specific.

The existing literature specifically looking at mobility across occupation and industry focuses on working age males in the US. Moscarini and Thomsson (2007) finds that about 3.5% of US male workers over 1979-2006 changed occupation between two consecutive months. Several studies also suggest that occupational mobility in the US follows a pro-cyclical pattern (Moscarini and Thomsson, 2007; Kambourov and Manovskii, 2008). Outside of the US context, Groes et al. (2015) use Danish administrative data for all workers to find that occupational mobility follows a U-shape across the wage distribution, while industry mobility follows an L-shape.

We contribute to this literature by measuring US occupational and industry mobility in the US using monthly panels with worker coverage of up 6 years.

4.2.2 Labor Market Concentration, Occupations, and Industries

The literature concerned with understanding the labor market power of firms commonly defines labor markets as the interaction of an occupation or industry code with a geography and time dimension. Recent examples include Azar et al. (2018) who use occupation-commuting zone-quarter, Benmelech et al. (2018) who use industry-county-year, and Rinz (2018) who uses industry-commuting zone-year to define distinct labor markets for the purpose of calculating firm concentration. Justification for this type of market definition is generally based on the strong assumption that search frictions make occupation or industry a relatively fixed characteristic for most workers (Manning, 2003).

Low-wage work is a particularly interesting context to understand this phenomenon, given that many low-wage jobs do not require extensive training or prerequisites. Despite many low-wage jobs having different tasks (such as cleaning, food preparation, loading/unloading, etc.), training for these jobs is relatively short before workers can perform their jobs mostly unsupervised. If low-wage workers move freely across occupation and industry then all firms that hire low-wage labor would be competing for the same workers, then common measures of market power such as the four-firm concentration or the Herfindahl-Herschman Index (HHI) will be sensitive to how labor market shares are calculated. The Federal Trade Commission labels market concentration in the following ways: a marketplace with HHI less than 1500 is a competitive market, a marketplace with an HHI between 1500 to 2500 is moderately concentrated marketplace, and a marketplace with HHI greater than 2500 to is a highly concentrated marketplace (FTC, 2015). Equations for calculating the HHI under different labor market definitions are as follows:

$$HHI_a = \sum_{i=1}^{N} (\frac{Ea_i}{Ea} \cdot 100)^2 \tag{4.1}$$

$$HHI_b = \sum_{i=1}^{N} (\frac{Eb_i}{Eb} \cdot 100)^2 \tag{4.2}$$

$$HHI_{ab} = \sum_{i=1}^{N} (\frac{Ea_i + Eb_i}{Ea + Eb} \cdot 100)^2 \tag{4.3}$$

The first two equations above show what labor market concentration would be in occupation defined labor markets. The third equation shows what labor market concentration would be in a market defined as all firms that hire low-wage labor.

4.2.3 Demonstrating the Sensitivity of HHI to Labor Market Definitions

To illustrate how market concentration measures like the HHI are sensitive to these different market definitions, there are two fictional labor market scenarios shown below. In both scenarios, HHI_a is the concentration measure of a labor market defined by occupation a, HHI_b is the concentration measure of a labor market defined by occupation b, and HHI_{ab} is the concentration measure of a labor market defined by all firms that hire low-labor labor. For simplicity, we assume that (1) firms only hire low-wage labor; and (2) workers can only work one occupation at a single firm at any given time[1].

In the first scenario, we consider an isolated region with 2 firms (Firm 1 and Firm 2) that employ low-wage labor across two occupations (a or b):

- There are 36 employees divided across 2 firms and 2 occupations
- Firm 1 has 5 occupation a employees and 7 occupation b employees (12 employees total)
- Firm 2 has 9 occupation a employees and 15 occupation b employees (24 employees total)

Then the market concentration measures would be calculated as follows:

$$HHI_a = (\frac{5}{14} \cdot 100)^2 + (\frac{9}{14} \cdot 100)^2 \approx 5408$$

$$HHI_b = (\frac{7}{22} \cdot 100)^2 + (\frac{15}{22} \cdot 100)^2 \approx 5661$$

$$HHI_{ab} = (\frac{12}{36} \cdot 100)^2 + (\frac{24}{36} \cdot 100)^2 \approx 5555$$

In this first scenario, the values of HHI would lead us to conclude that labor markets are highly concentrated regardless of which labor market definition is used. Our second scenario, illustrates that this is not always the case. In this scenario we consider an isolated region with 4 firms (Firms 1, 2, 3, and 4) that employ low-wage labor across two occupations (a or b):

- There are 50 employees divided across 4 firms and 2 occupations

[1] The same exercise could be done for labor markets by industry or a combination of industry and occupation.

- Firms 1 and 2 each have 8 employees in occupation a and 0 employees in occupation b (8 employees each)
- Firm 3 has 7 employees in occupation a and 18 employees in occupation b (25 employees)
- Firm 4 has 7 employees in occupation a and 2 employees in occupation b (9 employees)

Then the market concentration measures would be calculated as follows:

$$HHI_a = (\frac{8}{30} \cdot 100)^2 + (\frac{8}{30} \cdot 100)^2 + (\frac{7}{30} \cdot 100)^2 + (\frac{7}{30} \cdot 100)^2 \approx 2511$$

$$HHI_b = (\frac{0}{20} \cdot 100)^2 + (\frac{0}{20} \cdot 100)^2 + (\frac{18}{20} \cdot 100)^2 + (\frac{2}{20} \cdot 100)^2 \approx 8200$$

$$HHI_{ab} = (\frac{8}{50} \cdot 100)^2 + (\frac{8}{50} \cdot 100)^2 + (\frac{25}{50} \cdot 100)^2 + (\frac{9}{50} \cdot 100)^2 \approx 3028$$

Under the assumption that occupation defines labor markets, we would conclude that labor market a is moderately concentrated and labor market b is highly concentrated. However, under the alternative assumption that labor markets are defined by all firms that hire low-skill labor in the region, we would come to a different conclusion, that the low-wage market in this region is only moderately concentrated. What hopefully becomes obvious from these examples is that the latter HHI definition, HHI_{ab}, is a weighted average of HHI_a and HHI_b, where the weights depend on the number of firms, firm size and how labor is distributed across firms and occupations or industries. So depending on how the weights are distributed, researchers could potentially come to different conclusions about how concentrated labor markets are.

4.3 Data

The data we use for our descriptive analysis of occupational and industry mobility comes from the 2001, 2004, and 2008 Survey of Income and Program Participation (SIPP), which overlap to cover the calendar years 2001-2011. The SIPP tracks monthly employment, income, demographics, and program participation of individuals within households for up to 6 years. Because of the monthly frequency and length of the panel relative to the CPS monthly panels, this data is ideal to track US job mobility. The PSID and NLSY panels follow individuals over a longer period, but have sample sizes that are too small to accurately characterize the mobility of low-wage workers, a subset of the US workforce. The SIPP offers many useful variables, but for this study we restrict our attention to occupation and industry codes, job start and end dates, birthdates, and basic demographics such race, age, or sex.

4.4 Methodology

4.4.1 Defining Low-Wage Workers

We define individuals in the sample as low-wage workers based on the following criterion:

1. Age 15-64

2. First observed wage in the sample is two-third of state median wage

The "two-thirds" rule is a common method used to define low-wage workers (Bernstein and Gittleman, 2003; Boushey et al., 2007; Gautié and Schmitt, 2010; Albelda and Carr, 2014). We use state median wages from the Bureau of Labor Statistics OES survey rather than calculate them directly from the SIPP survey to ensure that we are using the most accurate median wage estimates. The OES produces state employment wage estimates for over 800 occupations. We specifically use the state median wages reported for all workers across all occupations. [2] BLS does not report state median wages for all occupations in the year 2000, so we use median wages for 2001 to define low-wage work for individuals' wages that were first observed in 2000.

[2] Bureau of Labor Statistics, U.S. Department of Labor, Occupational Employment Statistics, [2/18/2020] [www.bls.gov/oes/].

4.4.2 Defining Occupation and Industry Switches

Occupation and industry codes are provided by the US Census Bureau. We define an occupation/industry switch as a change in occupation/industry code from one month to the next for a given individual. For the most conservative approach we do not define a switch as adding a second job in a different occupation, if at least one of their jobs in the previous month is the same occupation code as their second job. Examples of scenarios that we define as a switch are shown in Table A.32.

4.4.3 Summary Statistics: SIPP Low-Wage Workers

Summary statistics are shown in Table A.33. The average age of low-wage workers in our sample is 31, and the average wage is $7.56 in 2013 dollars. The unconditional probability of an occupation change in a given month is higher than the unconditional probability of an industry change (0.0213 vs. 0.0176). The average number of occupation and industry switches for all low-wage individuals is 0.777 and 0.641, respectively.

4.5 Occupation and Industry Mobility Estimates

Our estimates suggest that occupational switches are more common than industry switches. We find that the unconditional probability of occupation and industry switches for low-wage workers in any given month was 2.13% and 1.76% respectively. We also look at differences in unconditional switching probabilities across several demographics. These differences by demographics can be seen in Figures B.22 (sex), B.23 (race), B.24 (wage level), and B.25 (marital status). First, we find that occupation and industry mobility is negatively correlated with wage levels. Those earning less than $8/hr switch occupation more than half as much as those earning $15/hr or more. Surprisingly, occupation and industry switches are relatively uniform across race. White and black switch occupations and industries at about the same rate. Men switch occupations slightly more often than women, but these differences in very small in magnitude (0.01-0.02 percentage points). Lastly, we find that marriage appears to be correlated with lower levels of occupation and industry changes.

What we take away from these unconditional switching probabilities is that fixed worker characteristics like race or sex are not strongly correlated with mobility, but more fluid worker characteristics like wages and marital status are.

4.5.1 Cumulative Occupation and Industry Mobility Overtime

Tables A.34 and A.35 show that the share of low-wage workers that switched occupation or industry one, two, or three times for the first observable year, two years, and three years for low-wage individuals. We find that almost 18% of low-wage workers switched occupation at least once within the first year of sample entry, 33.9% switched at least once within two years of sample entry, and 38.2% switched at least once within three years of sample entry. Switching probabilities by industry followed a similar pattern, 14.5% of low-wage workers switched industry at least once within the first year of sample entry, 28% switched at least once within two years of sample entry, and 31.6% switched at least once within three years of sample entry.

In Table A.36 we show the share of low-wage workers that switched both occupation and industry codes one, two, or three times within one to three years of sample entry. Neal (1995) refers to these type of simultaneous switches as complex switches. We find that about 13% switched occupation and industry within the first year of sample entry, 25% switched occupation and industry within the first two years, and 29% switched within the first three years. If we compare the shares that switch industry or occupation with the shares that switch occupation and industry, we can see that occupation and industry switches are most likely to occur simultaneously.

4.5.2 Job Mobility by Usual Hours Worked

In Figure B.26, we show that occupation and industry mobility follows a U-shape by usual hours worked. Switches are most common for those working less than 10 hours a week or more than 40 hours a week. This cross-section view gives us an interesting look at a possible mechanism for individuals wanting to switch jobs, getting too few hours or working too many hours. Switching probabilities are lowest between 20-30 hours a week.

4.5.3 Job Mobility by Job Tenure

In Figure B.27, we show the frequency of occupation and industry switches for low-wage workers by job tenure. In this figure, we only include the first observed jobs in the sample period, so that the figure is not weighted towards a small set of individuals that that frequently switch jobs over short intervals. Job tenure is defined as the number of months, you've been at a job based on the difference between SIPP reference dates and self-reported start or end dates (if applicable).

We find that occupation and industry switches are most common within the first few months of starting a job. Within the first month of job tenures, occupation switches and industry switch probabilities were approximately 2.4 percentage points. These probabilities drop drastically the first 3 months and then begin to slightly increase the remainder of job tenure.

4.6 Discussion

In this paper we find that low-wage workers switch both occupation and industry at relatively high frequencies and these switches are not specific to small subsets of workers, but rather low-wage workers as a whole. Collectively, our results suggest that low-wage workers are not defined by occupation or industry, which calls into question a frequently used method of defining labor markets on these same dimensions. We provided two fictional scenarios to illustrate how market concentration measures are sensitive to labor market definitions, but future research should also empirically test how alternative definitions of labor markets affect existing concentration measures.

5. SUMMARY

Low-income workers are a vital input into economic growth. Economic theory predicts that workers of all types, not just low-income workers, will enter the labor force if the marginal benefit of labor exceeds the marginal benefit of leisure; and exit the labor force if the opposite is true. Programs such as the Earned Income Tax Credit, Medicaid, and other means-tested transfer programs that use earnings and employment status as eligibility criteria can have a tremendous impact on these marginal benefit calculations. Marginal benefits of labor can come in many forms. Benefits like wages or salary are the first that come to mind, but there are also many non-pecuniary benefits of jobs that workers might value highly such as health insurance, paid leave, work environment, schedule flexibility, and co-worker relationships.

This dissertation showed that the Earned Income Tax Credit changed whether the marginal benefit of labor exceeded the marginal benefit of leisure for younger childless women and older childless men, especially those living with family members. Medicaid expansions, on the other hand, affected the labor-leisure tradeoff for older adults just under age 65, while having little to no effects on other age groups. These findings reinforce that individual worker characteristics and circumstances are a crucial element of the direction and magnitude of labor supply responses to policy changes. These characteristics are also predictive of the likelihood that workers will switch jobs, occupations, or industries. However, worker characteristics like these are hidden in aggregate employment statistics for broadly defined groups. Future researchers and policy makers should be cognizant of this fact when designing and implementing large scale transfer programs intended to help low-income workers and their households.

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APPENDIX A

TABLES

Table A.1: Summary Statistics: CPS Childless Adults Age 25-64

	Male	Female	Total
Employed	0.683	0.569	0.631
	(0.465)	(0.495)	(0.482)
Hours/wk	40.89	36.86	39.26
	(12.35)	(11.79)	(12.29)
Earnings/wk	796.1	594.9	710.3
	(485.1)	(378.5)	(453.9)
Hourly Wage	1.488	1.099	1.314
	(5.414)	(4.126)	(4.882)
State Effective MW	7.710	7.667	7.690
	(0.833)	(0.823)	(0.829)
State Unemployment Rate	0.0622	0.0617	0.0620
	(0.0213)	(0.0208)	(0.0211)
Married	0.448	0.569	0.502
	(0.497)	(0.495)	(0.500)
White	0.775	0.773	0.774
	(0.417)	(0.419)	(0.418)
Age	44.82	49.22	46.79
	(12.03)	(11.18)	(11.86)
Live w/ Parent	0.162	0.0879	0.129
	(0.369)	(0.283)	(0.335)
State EITC %	5.963	5.732	5.859
	(12.54)	(12.36)	(12.46)
Max State EITC	29.60	28.43	29.08
	(62.58)	(61.62)	(62.16)
Max Federal EITC	497.1	496.8	497.0
	(8.121)	(7.974)	(8.057)
Observations	409347		

The CPS sample covers years 1995-2018 and is restricted to childless adults age 25-64 with no more than a high school degree. All variables reported in dollars are adjusted to 2017 dollars using annual CPI-U.

Table A.2: Employment Effects of State EITCs: Age 25-64

	Women				Men			
	(1) Employed	(2) Employed	(3) Log(Hours)	(4) Log(Hours)	(5) Employed	(6) Employed	(7) Log(Hours)	(8) Log(Hours)
State EITC ($100s)	0.0122*		0.0122**		-0.0125		0.0008	
	(0.0071)		(0.0057)		(0.0077)		(0.0075)	
State EITC ($100s) x 25-34		0.0351***		0.0038		-0.0108		-0.0018
		(0.0097)		(0.0107)		(0.0119)		(0.0126)
State EITC ($100s) x 35-44		0.0159		0.0246		-0.0014		-0.0001
		(0.0125)		(0.0179)		(0.0103)		(0.0089)
State EITC ($100s) x 45-54		0.0108		0.0042		-0.0091*		0.0059
		(0.0170)		(0.0079)		(0.0053)		(0.0087)
State EITC ($100s) x 55-64		0.0020		0.0185		-0.0264**		0.0029
		(0.0100)		(0.0124)		(0.0115)		(0.0102)
Observations	177155	177155	97338	97338	216495	216495	142684	142684

Standard errors in parentheses

* $p < .1$, ** $p < .05$, *** $p < .01$

The CPS sample covers years 1995-2018 and is restricted to childless adults age 25-64 with no more than a high school degree. All standard errors are clustered at the state level.

Table A.3: Labor Force Participation and Job Leaving Effects of State EITCs: Age 25-64

	Women				Men			
	(1) LFP	(2) LFP	(3) Left Job	(4) Left Job	(5) LFP	(6) LFP	(7) Left Job	(8) Left Job
State EITC ($100s)	0.0069		-0.0011**		-0.0115		0.0011	
	(0.0060)		(0.0005)		(0.0087)		(0.0006)	
State EITC ($100s) x 25-34		0.0282**		-0.0018		-0.0092		0.0013
		(0.0111)		(0.0019)		(0.0111)		(0.0020)
State EITC ($100s) x 35-44		0.0126		-0.0020		0.0003		0.0012
		(0.0126)		(0.0021)		(0.0108)		(0.0008)
State EITC ($100s) x 45-54		0.0023		-0.0008		-0.0142**		0.0015
		(0.0123)		(0.0007)		(0.0059)		(0.0009)
State EITC ($100s) x 55-64		-0.0004		-0.0008*		-0.0203		0.0002
		(0.0105)		(0.0005)		(0.0134)		(0.0008)
Constant	0.7011***		0.0021		0.7702***		0.0189**	
	(0.0621)		(0.0061)		(0.0533)		(0.0074)	
Observations	177092	177092	177155	177155	215937	215937	216495	216495

Standard errors in parentheses

* $p < .1$, ** $p < .05$, *** $p < .01$

The CPS sample covers years 1995-2018 and is restricted to childless adults age 25-64 with no more than a high school degree. All standard errors are clustered at the state level.

Table A.4: Marginal Workers: Reason for Being Unemployed (By Sex)

	Male	Female	Total
Unemployed-Lost Job	0.739	0.596	0.696
	(0.439)	(0.491)	(0.460)
Unemployed-Left Job	0.0704	0.106	0.0812
	(0.256)	(0.308)	(0.273)
Labor Force Re-entrant	0.175	0.266	0.203
	(0.380)	(0.442)	(0.402)
Labor Force New Entrant	0.0149	0.0315	0.0200
	(0.121)	(0.175)	(0.140)
Observations	21553		

The CPS sample covers years 1995-2018 and is restricted to childless adults age 25-64 with no more than a high school degree.

Table A.5: Marginal Workers: Reason for Being Unemployed (By Household Arrangement)

	Parents	Family-No Parents	Non-Family	Total
Unemployed-Lost Job	0.596	0.658	0.688	0.654
	(0.491)	(0.474)	(0.463)	(0.476)
Unemployed-Left Job	0.0951	0.110	0.0916	0.0968
	(0.293)	(0.313)	(0.288)	(0.296)
Labor Force Re-entrant	0.268	0.212	0.210	0.228
	(0.443)	(0.409)	(0.407)	(0.419)
Labor Force New Entrant	0.0409	0.0190	0.0108	0.0217
	(0.198)	(0.137)	(0.103)	(0.146)
Observations	25878			

The CPS sample covers years 1995-2018 and is restricted to childless adults age 25-64 with no more than a high school degree.

Table A.6: Wage Effects of State EITCs: Age 25-64

	Women		Men	
	(1)	(2)	(3)	(4)
	log(wage)	log(wage)	log(wage)	log(wage)
State EITC (\$100s)	-0.0122		-0.0199	
	(0.0138)		(0.0204)	
State EITC (\$100s) x 25-34		-0.0213		0.0005
		(0.0226)		(0.0297)
State EITC (\$100s) x 35-44		-0.0118		0.0153
		(0.0318)		(0.0239)
State EITC (\$100s) x 45-54		-0.0252		-0.0374
		(0.0352)		(0.0233)
State EITC (\$100s) x 55-64		0.0132		-0.0995**
		(0.0268)		(0.0421)
Observations	13978	13978	18758	18758

Standard errors in parentheses

* $p < .1$, ** $p < .05$, *** $p < .01$

The CPS sample covers years 1995-2018 and is restricted to childless adults age 25-64 with no more than a high school degree. All standard errors are clustered at the state level.

Table A.7: Employment Effects of State EITCs: No Child Households Age 25-64

	Women				Men			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Employed	Employed	Log(Hours)	Log(Hours)	Employed	Employed	Log(Hours)	Log(Hours)
State EITC ($100s)	0.0115		0.0146**		-0.0128**		0.0021	
	(0.0076)		(0.0057)		(0.0061)		(0.0076)	
State EITC ($100s) x 25-34		0.0353***		0.0074		-0.0080		-0.0018
		(0.0088)		(0.0117)		(0.0094)		(0.0125)
State EITC ($100s) x 35-44		0.0226		0.0293		-0.0022		0.0037
		(0.0162)		(0.0197)		(0.0096)		(0.0106)
State EITC ($100s) x 45-54		0.0087		0.0025		-0.0080		0.0071
		(0.0167)		(0.0082)		(0.0057)		(0.0090)
State EITC ($100s) x 55-64		0.0015		0.0229*		-0.0293***		0.0023
		(0.0104)		(0.0131)		(0.0099)		(0.0105)
Observations	159320	159320	88373	88373	192856	192856	127104	127104

Standard errors in parentheses

* $p < .1$, ** $p < .05$, *** $p < .01$

The CPS sample covers years 1995-2018 and is restricted to childless adults age 25-64 with no more than a high school degree. All standard errors are clustered at the state level.

Table A.8: Employment Effects of State EITCs for Childless Adults By Household Arrangement

	Women		Men	
	(1)	(2)	(3)	(4)
	Employed	Log(Hours)	Employed	Log(Hours)
State EITC ($100s) x Parents	0.0308**	0.0115	-0.0276**	-0.0070
	(0.0140)	(0.0129)	(0.0120)	(0.0134)
State EITC ($100s) x Family-No Parents	0.0202**	0.0076	-0.0027	-0.0047
	(0.0098)	(0.0125)	(0.0087)	(0.0097)
State EITC ($100s) x Non-Family	0.0082	0.0125	-0.0107	-0.0044
	(0.0142)	(0.0176)	(0.0084)	(0.0079)
Observations	75891	42276	119166	75136

Standard errors in parentheses

* $p < .1$, ** $p < .05$, *** $p < .01$

The CPS sample covers years 1995-2018 and is restricted to childless adults age 25-64 with no more than a high school degree. All standard errors are clustered at the state level.

Table A.9: Employment Effects of State EITCs for Childless Adults By Marital Status

	Women		Men	
	(1)	(2)	(3)	(4)
	Employed	Log(Hours)	Employed	Log(Hours)
State EITC ($100s) x Unmarried	0.0156*	0.0123	-0.0125*	-0.0055
	(0.0086)	(0.0091)	(0.0070)	(0.0081)
State EITC ($100s) x Married	0.0083	0.0122	-0.0100	0.0096
	(0.0084)	(0.0118)	(0.0100)	(0.0080)
Observations	177155	97338	216495	142684

Standard errors in parentheses

* $p < .1$, ** $p < .05$, *** $p < .01$

The CPS sample covers years 1995-2018 and is restricted to childless adults age 25-64 with no more than a high school degree. All standard errors are clustered at the state level.

Table A.10: Triple Difference: Use Younger Categorically Ineligible Childless Adults as Third Difference

	Women 18-64		Women 18-34		Men 18-64		Men 18-34	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Employed	Log(Hours)	Employed	Log(Hours)	Employed	Log(Hours)	Employed	Log(Hours)
State EITC (\$100s) x Age>24	0.0127	0.0280***	0.0249**	0.0209*	-0.0060	0.0044	-0.0023	-0.0023
	(0.0086)	(0.0088)	(0.0118)	(0.0116)	(0.0070)	(0.0100)	(0.0075)	(0.0097)
Observations	248931	133525	92664	50412	323090	201393	157250	96858

Standard errors in parentheses

* $p < .1$, ** $p < .05$, *** $p < .01$

The CPS sample covers years 1995-2018 and is restricted to childless adults age 18-64 with no more than a high school degree. All standard errors are clustered at the state level.

Table A.11: Placebo Employment Effects of State EITCs: 18-24 year olds

	Women				Men			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Employed	Employed	Log(Hours)	Log(Hours)	Employed	Employed	Log(Hours)	Log(Hours)
State EITC (\$100s)	0.0027		-0.0085		0.0031		-0.0130	
	(0.0075)		(0.0130)		(0.0065)		(0.0124)	
State EITC (\$100s) x 18-21		0.0063		-0.0224		0.0100		-0.0232
		(0.0079)		(0.0175)		(0.0067)		(0.0139)
State EITC (\$100s) x 22-24		-0.0090		0.0201		-0.0126		-0.0045
		(0.0167)		(0.0212)		(0.0124)		(0.0156)
Observations	62468	62468	31435	31435	93885	93885	50925	50925

Standard errors in parentheses

* $p < .1$, ** $p < .05$, *** $p < .01$

The CPS sample covers years 1995-2018 and is restricted to childless adults age 18-24 with no more than a high school degree. All standard errors are clustered at the state level.

Table A.12: Employment and Hours Worked: 2015 DC Expansion

	Women				Men			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Employed	Employed	Log(Hours)	Log(Hours)	Employed	Employed	Log(Hours)	Log(Hours)
DC Expansion	0.0578***		0.0119		-0.0259		-0.0148	
	(0.0097)		(0.0098)		(0.0193)		(0.0121)	
DC Expansion x 25-34		0.1477***		-0.0029		0.0074		-0.0578***
		(0.0094)		(0.0107)		(0.0213)		(0.0131)
DC Expansion x 35-44		0.0285**		0.1367***		-0.0279		-0.0082
		(0.0110)		(0.0111)		(0.0184)		(0.0128)
DC Expansion x 45-54		0.0810***		-0.0170		-0.0560***		0.0217
		(0.0110)		(0.0113)		(0.0200)		(0.0129)
DC Expansion x 55-64		-0.0062		-0.0124		-0.0266		0.0134
		(0.0095)		(0.0118)		(0.0188)		(0.0117)
Observations	630754	630754	371331	371331	834228	834228	547705	547705

Standard errors in parentheses

* $p < .1$, ** $p < .05$, *** $p < .01$

The ACS sample covers 2010-2017 and is restricted to childless adults age 25-64 with no more than a high school degree. All standard errors are clustered at the state level.

Table A.13: Employment and Hours Worked: 2015 DC Expansion (DC EITC in $100s)

	Women				Men			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Employed	Employed	Log(Hours)	Log(Hours)	Employed	Employed	Log(Hours)	Log(Hours)
DC EITC ($100s)	0.0065*		-0.0043		-0.0085		0.0054	
	(0.0033)		(0.0034)		(0.0066)		(0.0040)	
DC EITC ($100s) x 25-34		0.0196***		-0.0249***		0.0021		0.0050
		(0.0031)		(0.0038)		(0.0071)		(0.0047)
DC EITC ($100s) x 35-44		-0.0052		0.0195***		-0.0267***		-0.0047
		(0.0037)		(0.0037)		(0.0064)		(0.0040)
DC EITC ($100s) x 45-54		0.0232***		-0.0086**		-0.0032		0.0011
		(0.0037)		(0.0038)		(0.0071)		(0.0042)
DC EITC ($100s) x 55-64		-0.0090***		0.0081**		-0.0101		0.0286***
		(0.0031)		(0.0039)		(0.0063)		(0.0038)
Observations	630754	630754	371331	371331	834228	834228	547705	547705

Standard errors in parentheses

* $p < .1$, ** $p < .05$, *** $p < .01$

The ACS sample covers 2010-2017 and is restricted to childless adults age 25-64 with no more than a high school degree. All standard errors are clustered at the state level.

Table A.14: Labor Force Participation: 2015 DC Expansion

	Women		Men	
	(1) LFP	(2) LFP	(3) LFP	(4) LFP
DC Expansion	0.0250***		-0.0371***	
	(0.0068)		(0.0074)	
DC Expansion x 25-34		0.0512***		-0.0335***
		(0.0073)		(0.0082)
DC Expansion x 35-44		0.0186**		-0.0005
		(0.0078)		(0.0084)
DC Expansion x 45-54		0.0413***		-0.0605***
		(0.0070)		(0.0071)
DC Expansion x 55-64		-0.0010		-0.0428***
		(0.0079)		(0.0069)

	Women		Men	
	(1) LFP	(2) LFP	(3) LFP	(4) LFP
DC EITC ($100s)	0.0068***		-0.0083***	
	(0.0022)		(0.0025)	
DC EITC ($100s) x 25-34		0.0030		-0.0153***
		(0.0029)		(0.0029)
DC EITC ($100s) x 35-44		0.0009		-0.0116***
		(0.0026)		(0.0028)
DC EITC ($100s) x 45-54		0.0193***		0.0001
		(0.0021)		(0.0026)
DC EITC ($100s) x 55-64		0.0018		-0.0045**
		(0.0024)		(0.0020)
Observations	630754	630754	834228	834228

Standard errors in parentheses

* $p < .1$, ** $p < .05$, *** $p < .01$

The ACS sample covers 2010-2017 and is restricted to childless adults age 25-64 with no more than a high school degree. All standard errors are clustered at the state level.

Table A.15: Summary Statistics: CPS Adults Age 50-64

	Always	Never	Changed	Total
Female	0.554	0.546	0.550	0.549
	(0.497)	(0.498)	(0.498)	(0.498)
Age	56.79	56.81	56.73	56.77
	(4.246)	(4.364)	(4.303)	(4.315)
White	0.841	0.875	0.880	0.873
	(0.366)	(0.331)	(0.325)	(0.333)
Hispanic	0.119	0.103	0.0942	0.100
	(0.324)	(0.304)	(0.292)	(0.301)
Married	0.678	0.712	0.679	0.690
	(0.467)	(0.453)	(0.467)	(0.463)
Low Ed	0.557	0.577	0.563	0.567
	(0.497)	(0.494)	(0.496)	(0.496)
Uninsured	0.122	0.136	0.117	0.124
	(0.327)	(0.343)	(0.321)	(0.329)
Medicaid	0.0833	0.0630	0.0646	0.0667
	(0.276)	(0.243)	(0.246)	(0.250)
Priv Ins	0.783	0.767	0.795	0.784
	(0.412)	(0.423)	(0.403)	(0.411)
Hours/wk	39.81	40.26	39.81	39.96
	(12.47)	(12.78)	(12.16)	(12.41)
Retired	0.125	0.126	0.121	0.123
	(0.331)	(0.332)	(0.326)	(0.329)
New Retired	0.0291	0.0278	0.0326	0.0305
	(0.168)	(0.164)	(0.178)	(0.172)
SS Inc	939.8	1126.9	1106.8	1089.5
	(2710.4)	(2981.4)	(3187.8)	(3056.8)
SS Inc 0	0.135	0.165	0.152	0.154
	(0.342)	(0.371)	(0.359)	(0.361)
Observations	15585			

The CPS sample covers years 1996-2012 and is restricted to childless adults age 50-64.

Table A.16: Difference-in-Differences Results: Uninsured

	(1) Uninsured	(2) Uninsured	(3) Uninsured	(4) Uninsured
Expansion Exposure	-0.0059	-0.0063	-0.0100***	-0.0008
	(0.0041)	(0.0042)	(0.0031)	(0.0034)
Observations	405854	405854	405854	405854
Specification	Baseline	Demographics	Add UE Controls	State Trends

Standard errors in parentheses

* $p < .1$, ** $p < .05$, *** $p < .01$

The CPS sample covers years 1996-2012 and is restricted to childless adults age 50-64. All standard errors are clustered at the state level.

Table A.17: Difference-in-Discontinuity Results: Uninsured

	(1) Uninsured	(2) Uninsured	(3) Uninsured	(4) Uninsured
Baseline Discontinuity	-0.1103***	-0.1113***	-0.1113***	-0.1111***
	(0.0019)	(0.0019)	(0.0019)	(0.0019)
Difference in Discontinuities	0.0299***	0.0297***	0.0297***	0.0296***
	(0.0018)	(0.0018)	(0.0018)	(0.0018)
Observations	419727	419727	419727	419727
Specification	Baseline	Demographics	UE Controls	State-Year FEs

Standard errors in parentheses

* $p < .1$, ** $p < .05$, *** $p < .01$

The CPS sample covers years 1996-2012 and is restricted to childless adults age 55-75. All standard errors are clustered at the state level.

Table A.18: Difference-in-Differences Results: Retired

	(1) Retired	(2) Retired	(3) Retired	(4) Retired
Expansion Exposure	-0.0016	-0.0020	-0.0017	-0.0054*
	(0.0030)	(0.0032)	(0.0030)	(0.0032)
Observations	405854	405854	405854	405854
Specification	Baseline	Demographics	Add UE Controls	State Trends

Standard errors in parentheses

* $p < .1$, ** $p < .05$, *** $p < .01$

The CPS sample covers years 1996-2012 and is restricted to childless adults age 50-64. All standard errors are clustered at the state level.

Table A.19: Difference-in-Discontinuity Results: Retired

	(1) Retired	(2) Retired	(3) Retired	(4) Retired
Baseline Discontinuity	0.0794***	0.0798***	0.0798***	0.0802***
	(0.0034)	(0.0033)	(0.0033)	(0.0033)
Difference in Discontinuities	0.0089**	0.0085**	0.0084**	0.0080**
	(0.0035)	(0.0035)	(0.0035)	(0.0035)
Observations	419727	419727	419727	419727
Specification	Baseline	Demographics	UE Controls	State-Year FEs

Standard errors in parentheses

* $p < .1$, ** $p < .05$, *** $p < .01$

The CPS sample covers years 1996-2012 and is restricted to childless adults age 55-75. All standard errors are clustered at the state level.

Table A.20: Difference-in-Differences Results: Hours Worked Per Week

	(1) Hours/wk	(2) Hours/wk	(3) Hours/wk	(4) Hours/wk
Expansion Exposure	0.0549	0.0511	0.1595	0.1160
	(0.2079)	(0.1961)	(0.1359)	(0.1999)
Observations	287135	287135	287135	287135
Specification	Baseline	Demographics	Add UE Controls	State Trends

Standard errors in parentheses

* $p < .1$, ** $p < .05$, *** $p < .01$

The CPS sample covers years 1996-2012 and is restricted to childless adults age 50-64. All standard errors are clustered at the state level.

Table A.21: Difference-in-Discontinuity Results: Hours Worked Per Week

	(1) Hours/wk	(2) Hours/wk	(3) Hours/wk	(4) Hours/wk
Baseline Discontinuity	-1.8050***	-1.8875***	-1.8897***	-1.9060***
	(0.1518)	(0.1488)	(0.1487)	(0.1488)
Difference in Discontinuities	0.1434	0.1318	0.1344	0.1110
	(0.1707)	(0.1680)	(0.1679)	(0.1682)
Observations	202620	202620	202620	202620
Specification	Baseline	Demographics	UE Controls	State-Year FEs

Standard errors in parentheses

* $p < .1$, ** $p < .05$, *** $p < .01$

The CPS sample covers years 1996-2012 and is restricted to childless adults age 55-75. All standard errors are clustered at the state level.

Table A.22: Difference-in-Differences Results: Newly Retired

	(1) New Retired	(2) New Retired	(3) New Retired	(4) New Retired
Expansion Exposure	0.0007	0.0007	0.0006	0.0004
	(0.0013)	(0.0013)	(0.0012)	(0.0013)
Observations	405854	405854	405854	405854
Specification	Baseline	Demographics	Add UE Controls	State Trends

Standard errors in parentheses

* $p < .1$, ** $p < .05$, *** $p < .01$

The CPS sample covers years 1996-2012 and is restricted to childless adults age 50-64. All standard errors are clustered at the state level.

Table A.23: Difference-in-Discontinuity Results: Newly Retired

	(1) New Retired	(2) New Retired	(3) New Retired	(4) New Retired
Baseline Discontinuity	-0.0001	0.0001	0.0001	0.0000
	(0.0016)	(0.0016)	(0.0016)	(0.0016)
Difference in Discontinuities	0.0039**	0.0041**	0.0041**	0.0042**
	(0.0017)	(0.0017)	(0.0017)	(0.0017)
Observations	419727	419727	419727	419727
Specification	Baseline	Demographics	UE Controls	State-Year FEs

Standard errors in parentheses

* $p < .1$, ** $p < .05$, *** $p < .01$

The CPS sample covers years 1996-2012 and is restricted to childless adults age 55-75. All standard errors are clustered at the state level.

Table A.24: Difference-in-Differences Results: Positive Social Security Income

	(1) SS Inc >0	(2) SS Inc >0	(3) SS Inc >0	(4) SS Inc >0
Expansion Exposure	0.0066	0.0074	0.0031	-0.0042
	(0.0090)	(0.0088)	(0.0082)	(0.0115)
Observations	70596	70596	70596	70596
Specification	Baseline	Demographics	Add UE Controls	State Trends

Standard errors in parentheses

* $p < .1$, ** $p < .05$, *** $p < .01$

The CPS sample covers years 1996-2012 and is restricted to childless adults age 50-64. All standard errors are clustered at the state level.

Table A.25: Difference-in-Discontinuity Results: Positive Social Security Income

	(1)	(2)	(3)	(4)
	SS Inc >0	SS Inc >0	SS Inc >0	SS Inc >0
Baseline Discontinuity	0.0982***	0.0985***	0.0985***	0.0980***
	(0.0081)	(0.0080)	(0.0080)	(0.0080)
Difference in Discontinuities	0.0109*	0.0118*	0.0118*	0.0114*
	(0.0065)	(0.0065)	(0.0065)	(0.0065)
Observations	110124	110124	110124	110124
Specification	Baseline	Demographics	UE Controls	State-Year FEs

Standard errors in parentheses

* $p < .1$, ** $p < .05$, *** $p < .01$

The CPS sample covers years 1996-2012 and is restricted to childless adults age 55-75. All standard errors are clustered at the state level.

Table A.26: Difference-in-Differences Results: Social Security Income

	(1)	(2)	(3)	(4)
	SS Inc	SS Inc	SS Inc	SS Inc
Expansion Exposure	-47.7516	-27.6784	-84.5164	-52.0350
	(127.5600)	(122.9993)	(113.0617)	(133.0773)
Observations	70596	70596	70596	70596
Specification	Baseline	Demographics	Add UE Controls	State Trends

Standard errors in parentheses

* $p < .1$, ** $p < .05$, *** $p < .01$

The CPS sample covers years 1996-2012 and is restricted to childless adults age 50-64. All standard errors are clustered at the state level.

Table A.27: Difference-in-Discontinuity Results: Social Security Income

	(1)	(2)	(3)	(4)
	SS Inc	SS Inc	SS Inc	SS Inc
Baseline Discontinuity	908.7948***	869.5419***	871.1746***	865.7207***
	(107.2047)	(106.2386)	(106.2318)	(106.6737)
Difference in Discontinuities	378.2967***	405.3462***	405.4107***	401.1617***
	(95.0508)	(93.8192)	(93.8049)	(94.2040)
Observations	110124	110124	110124	110124
Specification	Baseline	Demographics	UE Controls	State-Year FEs

Standard errors in parentheses

* $p < .1$, ** $p < .05$, *** $p < .01$

The CPS sample covers years 1996-2012 and is restricted to childless adults age 55-75. All standard errors are clustered at the state level.

Table A.28: Difference-in-Differences Results: Private Insurance

	(1)	(2)	(3)	(4)
	Priv Ins	Priv Ins	Priv Ins	Priv Ins
Expansion Exposure	0.0039	0.0031	0.0066	0.0014
	(0.0037)	(0.0041)	(0.0040)	(0.0037)
Observations	405854	405854	405854	405854
Specification	Baseline	Demographics	Add UE Controls	State Trends

Standard errors in parentheses
* $p < .1$, ** $p < .05$, *** $p < .01$

The CPS sample covers years 1996-2012 and is restricted to childless adults age 50-64. All standard errors are clustered at the state level.

Table A.29: Difference-in-Discontinuity Results: Private Insurance

	(1)	(2)	(3)	(4)
	Priv Ins	Priv Ins	Priv Ins	Priv Ins
Baseline Discontinuity	-0.0457***	-0.0415***	-0.0415***	-0.0420***
	(0.0033)	(0.0031)	(0.0031)	(0.0031)
Difference in Discontinuities	-0.0058*	-0.0054*	-0.0054*	-0.0046
	(0.0034)	(0.0033)	(0.0033)	(0.0033)
Observations	419727	419727	419727	419727
Specification	Baseline	Demographics	UE Controls	State-Year FEs

Standard errors in parentheses
* $p < .1$, ** $p < .05$, *** $p < .01$

The CPS sample covers years 1996-2012 and is restricted to childless adults age 55-75. All standard errors are clustered at the state level.

Table A.30: Difference-in-Differences Results Using Maximum Income Thresholds as Medicaid Expansion Measure

	(1)	(2)	(3)	(4)
	Uninsured	Retired	Hours/wk	SS Inc
100% FPL Threshold	-0.0070***	-0.0007	0.0414	-19.9480
	(0.0023)	(0.0013)	(0.0692)	(49.8066)
Observations	405854	405854	287135	70596
Specification	Add UE Controls	Add UE Controls	Add UE Controls	Add UE Controls

Standard errors in parentheses
* $p < .1$, ** $p < .05$, *** $p < .01$

The CPS sample covers years 1996-2012 and is restricted to childless adults age 50-64. All standard errors are clustered at the state level.

Table A.31: Difference-in-Discontinuity Results Using Maximum Income Thresholds as Medicaid Expansion Measure

	(1) Uninsured	(2) Retired	(3) Hours/wk	(4) SS Inc
Baseline Discontinuity	-0.1092***	0.0811***	-1.8643***	923.8537***
	(0.0019)	(0.0033)	(0.1461)	(106.3801)
Difference-in-Discontinuity (100% FPL)	0.0171***	0.0044**	0.0177	223.5075***
	(0.0010)	(0.0020)	(0.0976)	(56.3662)
Observations	419727	419727	202620	110124
Specification	UE Controls	UE Controls	UE Controls	UE Controls

Standard errors in parentheses

* $p < .1$, ** $p < .05$, *** $p < .01$

The CPS sample covers years 1996-2012 and is restricted to childless adults age 55-75. All standard errors are clustered at the state level.

Table A.32: Examples of Occupation Switches

Individual	Month 1 Occupation(s)	Month 2 Occupation(s)	Occupation Change?
Joanna	Cashier	Cashier	No
Jace	Salesperson & Cashier	Salesperson	No
Jody	Maid	Cashier	Yes
Judith	Cook	Cook & Waitress	No
John	Salesperson	Waiter & Cook	Yes

Table A.33: Summary Statistics: SIPP Low-Wage Subsample

	Male	Female	Total
Occupation Change	0.0227	0.0202	0.0213
	(0.149)	(0.141)	(0.144)
Industry Change	0.0187	0.0167	0.0176
	(0.136)	(0.128)	(0.131)
Number of Occupation Changes	0.819	0.746	0.777
	(1.073)	(1.014)	(1.040)
Number of Industry Changes	0.675	0.617	0.641
	(0.985)	(0.932)	(0.955)
Age as of last birthday	30.05	32.76	31.61
	(13.21)	(13.76)	(13.59)
White	0.785	0.767	0.775
	(0.411)	(0.423)	(0.418)
Married	0.294	0.361	0.332
	(0.456)	(0.480)	(0.471)
Wage-Job 1 (2013 dollars)	8.245	7.057	7.562
	(7.772)	(6.551)	(7.119)
Usual Hours- Job 1	23.31	20.05	21.43
	(20.15)	(18.66)	(19.37)
Observations	2033049		

The sample includes workers age 15-64, whose first observed wage is less than or equal to 2/3 of the state median wage in the corresponding wage year.

Table A.34: Share of Sample that Switches Occupation

Time Length	1 switch	2 switches	3+ switches
1 Year	0.156	0.023	0.004
2 Years	0.237	0.082	0.020
3 Years	0.246	0.101	0.035

The sample includes workers age 15-64, whose first observed wage is less than or equal to 2/3 of the state median wage in the first observed year. 1 year refers to the first 12 months observed for an individual in the sample, 2 years refers to the first 24 months, and 3 years refers to the first 36 months.

Table A.35: Share of Sample that Switches Industry

Time Length	1 switch	2 switches	3+ switches
1 Year	0.125	0.017	0.003
2 Years	0.205	0.061	0.014
3 Years	0.211	0.081	0.024

The sample includes workers age 15-64, whose first observed wage is less than or equal to 2/3 of the state median wage in the first observed year. 1 year refers to the first 12 months observed for an individual in the sample, 2 years refers to the first 24 months, and 3 years refers to the first 36 months.

Table A.36: Share of Sample that Switches Occupation and Industry

Time Length	1 switch	2 switches	3+ switches
1 Year	0.112	0.013	0.000
2 Years	0.189	0.051	0.011
3 Years	0.197	0.069	0.018

The sample includes workers age 15-64, whose first observed wage is less than or equal to 2/3 of the state median wage in the first observed year. 1 year refers to the first 12 months observed for an individual in the sample, 2 years refers to the first 24 months, and 3 years refers to the first 36 months.

APPENDIX B

FIGURES

Figure B.1: Federal EITC For Single Adults By Number of Dependents: 2018 Tax Year

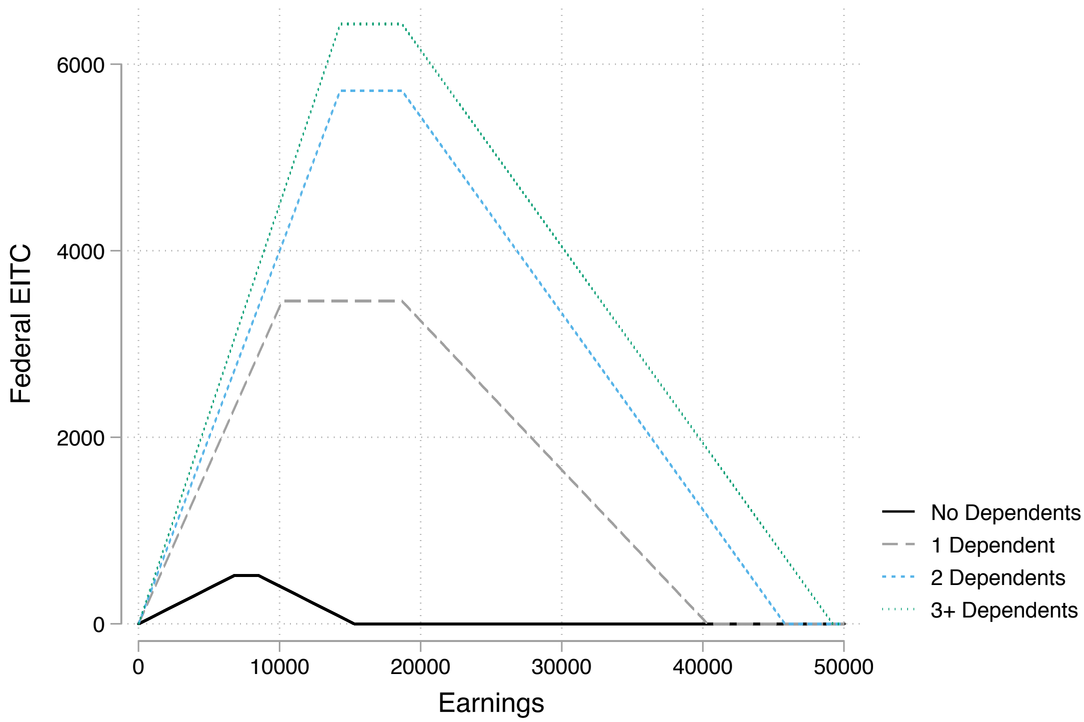


Figure B.2: Share of Childless Women Earning Less than EITC Earning Threshold

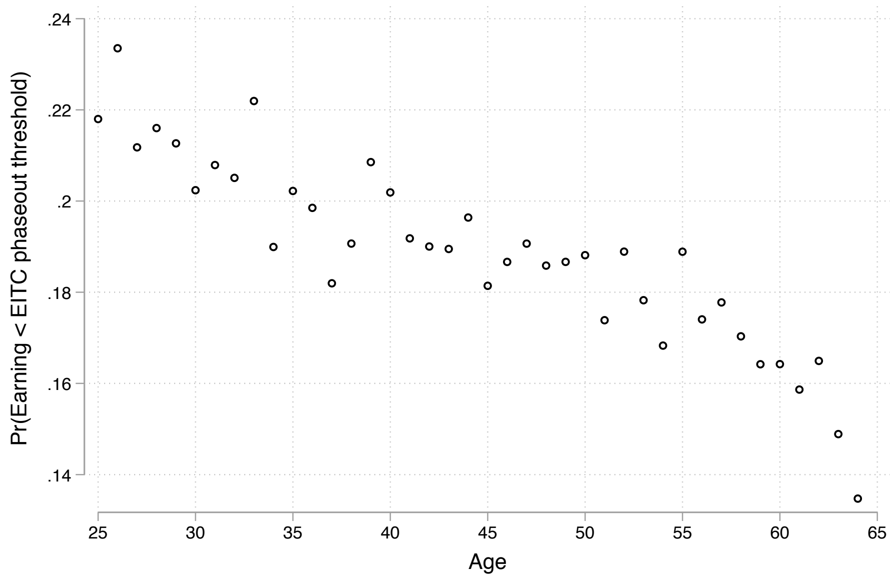


Figure B.3: Share of Childless Men Earning Less than EITC Earning Threshold

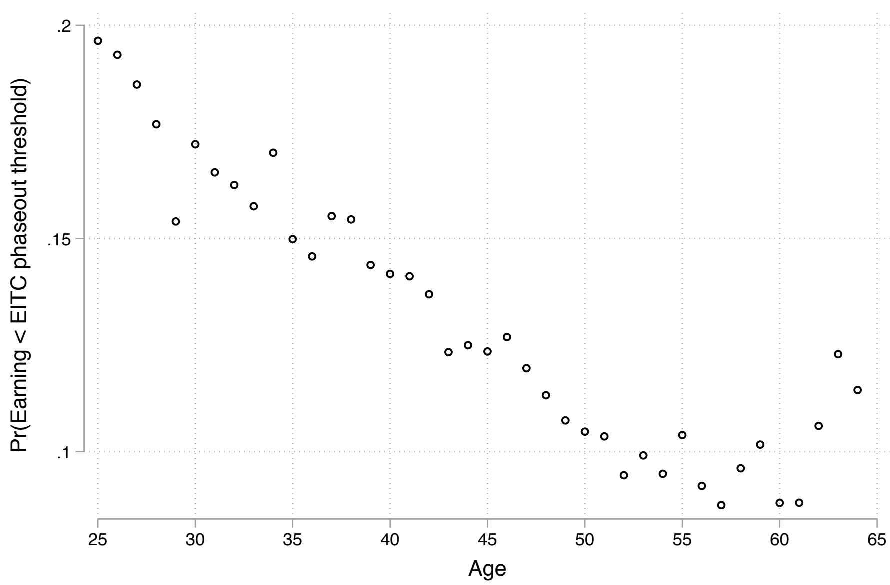


Figure B.4: State EITCs as a Percentage of the Federal EITC for Childless Adults

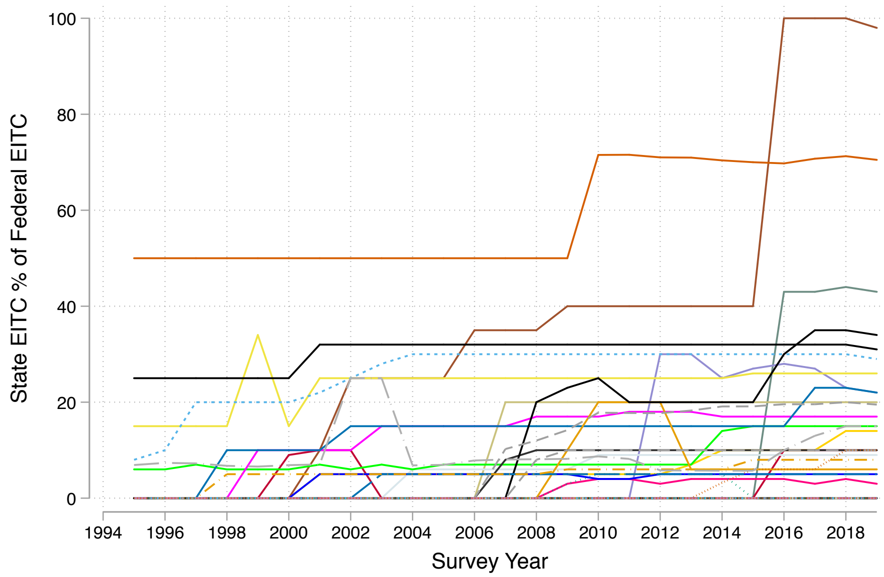
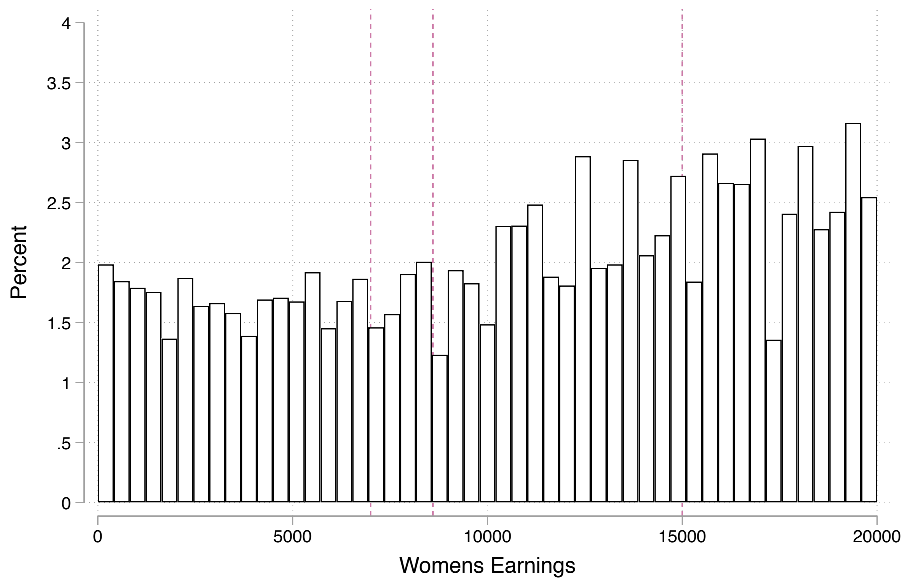
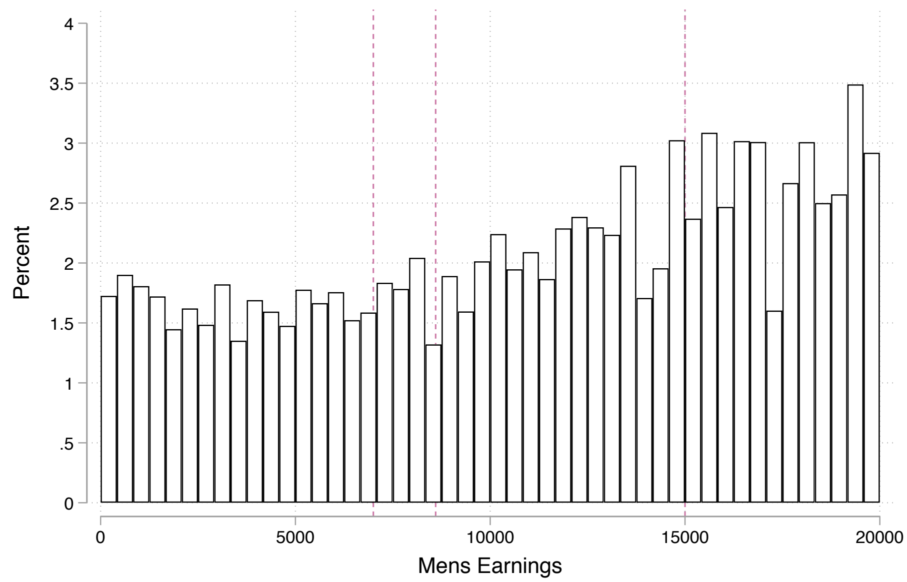


Figure B.5: Distribution of Women's Earnings



The EITC phases-in up to $7000, maxes out between $7000-$8600, and phases out until it reaches $0 at $15000. The vertical denoted lines mark this approximate thresholds.

Figure B.6: Distribution of Men's Earnings



The EITC phases-in up to \$7000, maxes out between \$7000-\$8600, and phases out until it reaches \$0 at \$15000. The vertical denoted lines mark this approximate thresholds.

Figure B.7: Number of Non-family Members in Household

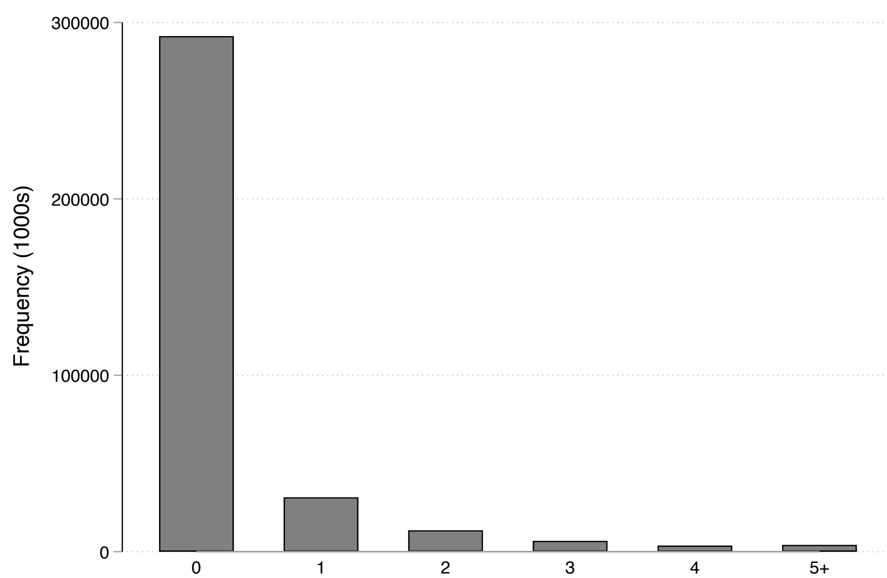


Figure B.8: Number of Family Members Present in Household

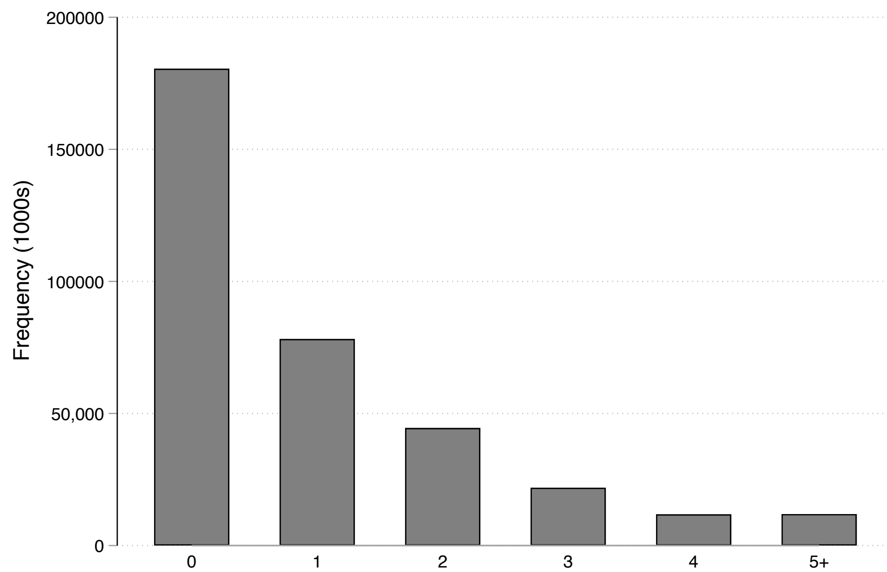


Figure B.9: Frequency of Relationship to Householder

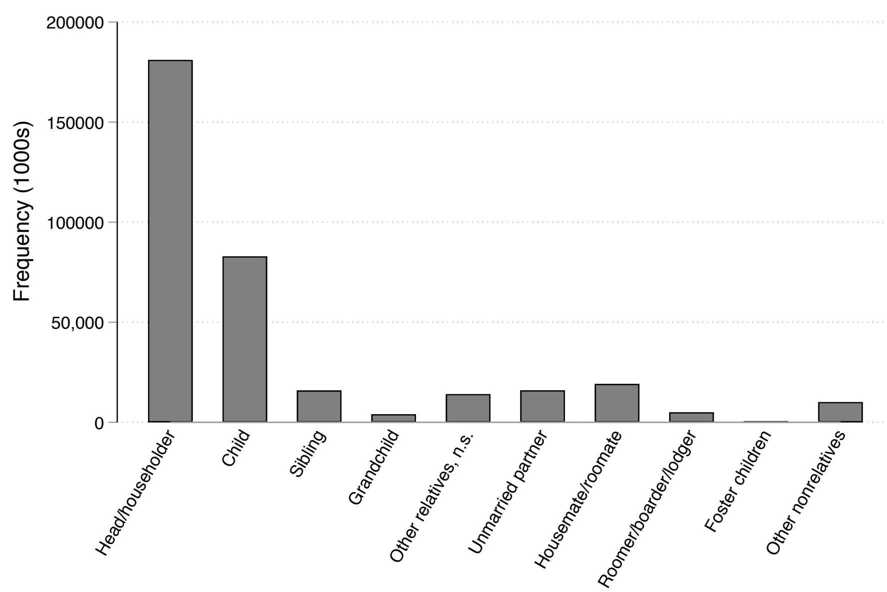


Figure B.10: Employment and State EITC Event Study: Women 25-64

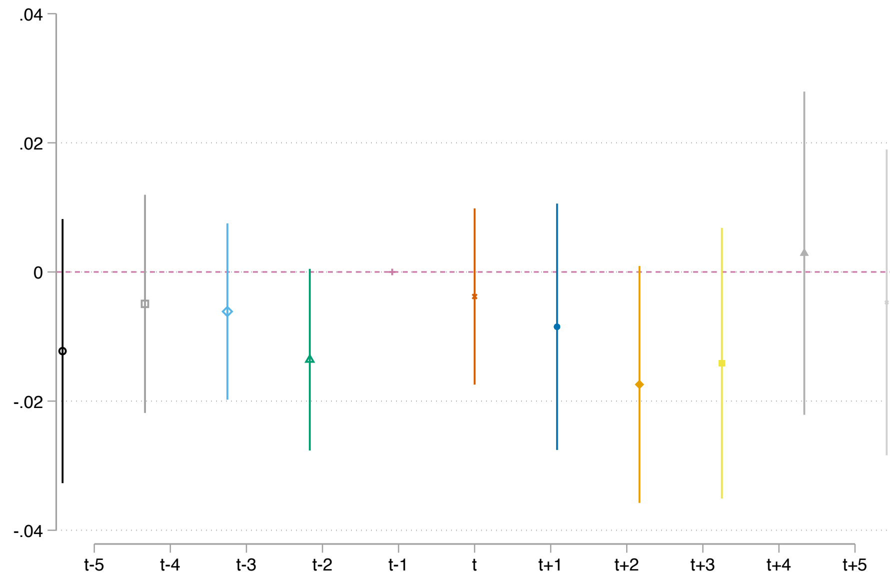


Figure B.11: Employment and State EITC Event Study: Women 25-34

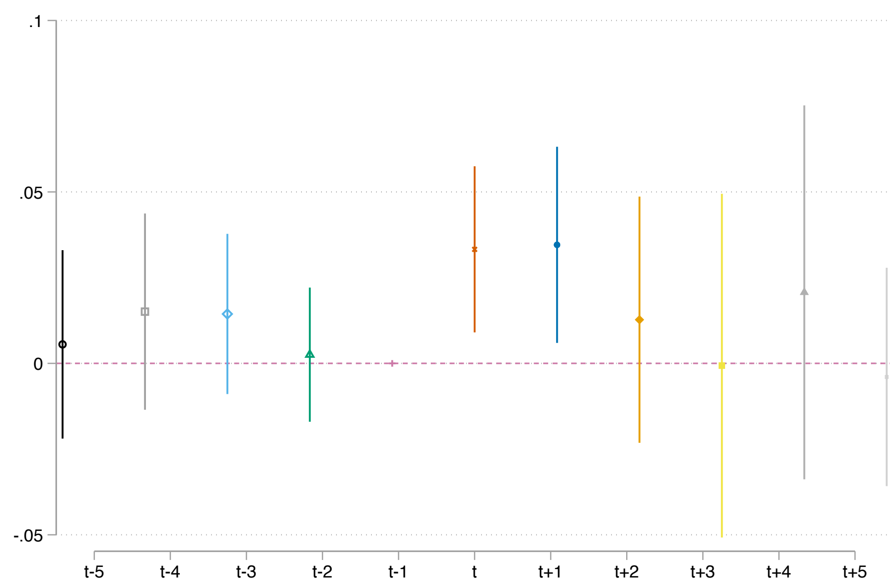


Figure B.12: Employment and State EITC Event Study: Men 25-64

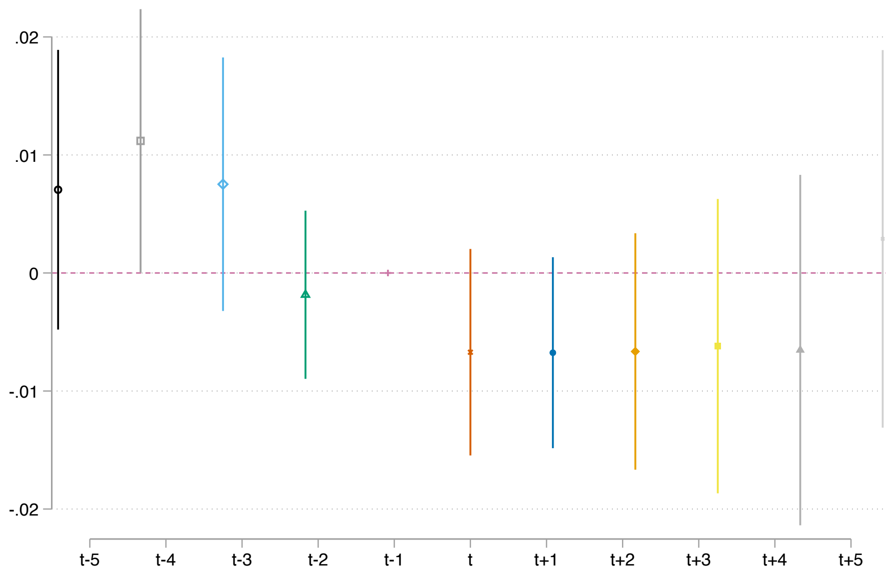


Figure B.13: Employment and State EITC Event Study: Men 25-34

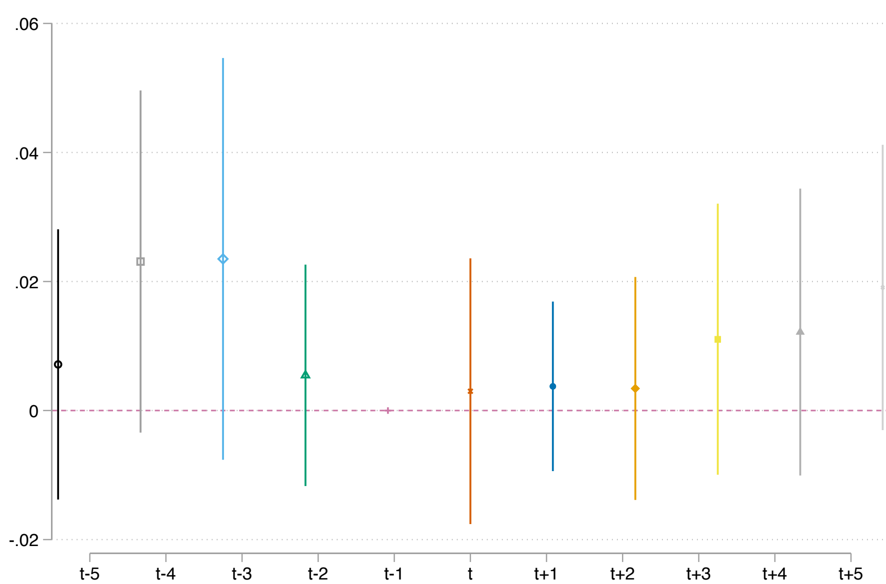


Figure B.14: DC and Non-EITC States Employment Trends: All Childless Adults

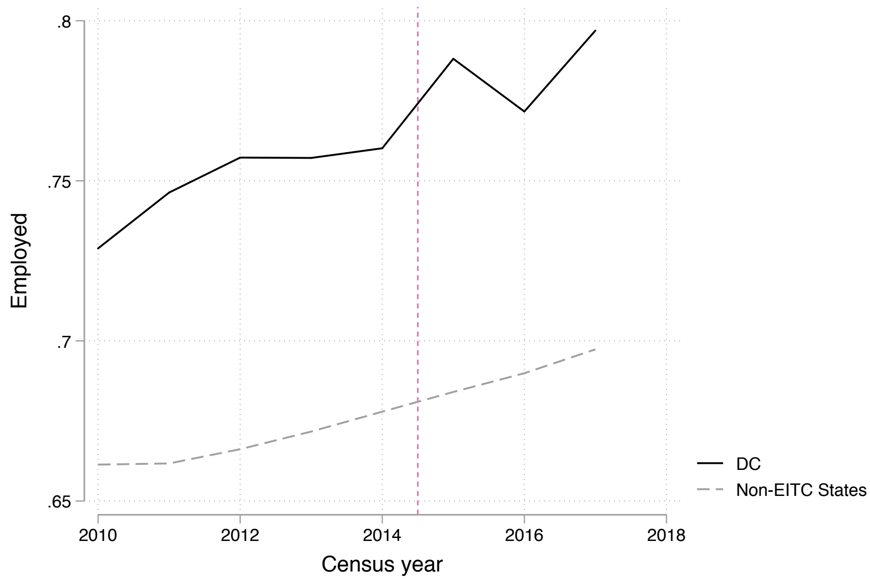


Figure B.15: DC and Non-EITC States Employment Trends: Childless Women

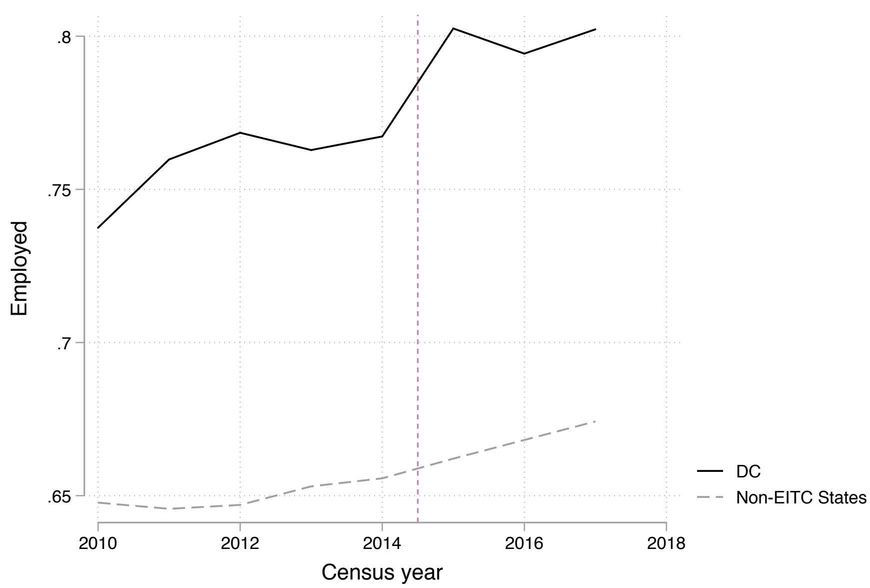


Figure B.16: DC and Non-EITC States Employment Trends: Childless Men

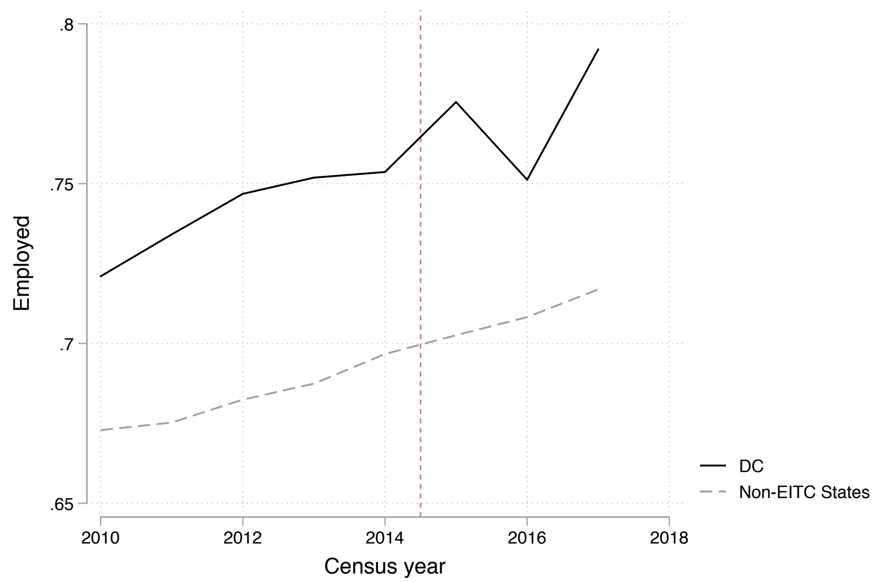


Figure B.17: Medicaid Income Eligibility Thresholds Overtime

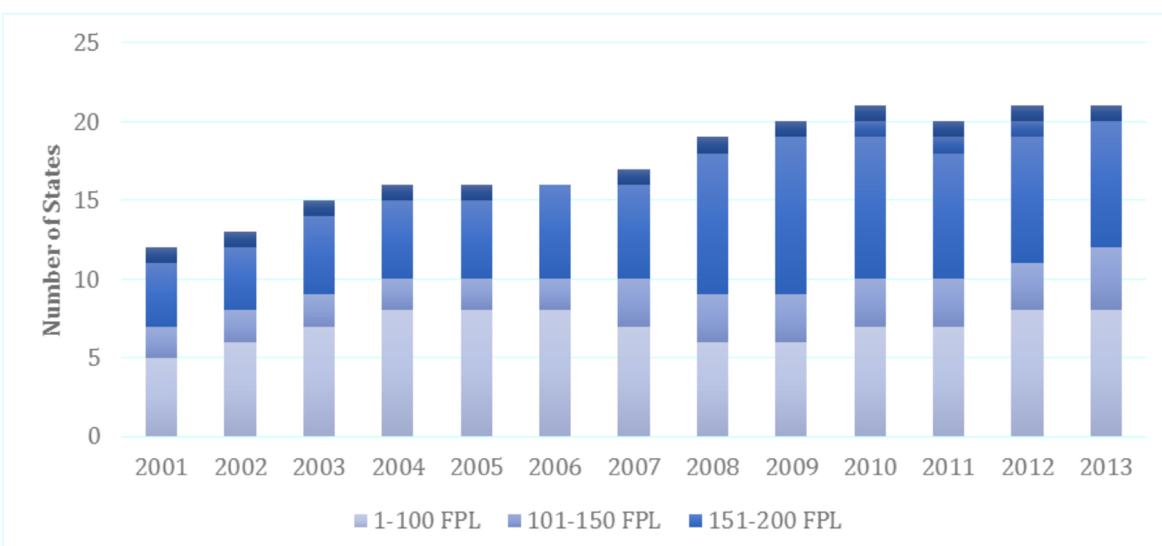


Figure B.18: Uninsured Rates By Age and Medicaid Expansion Status

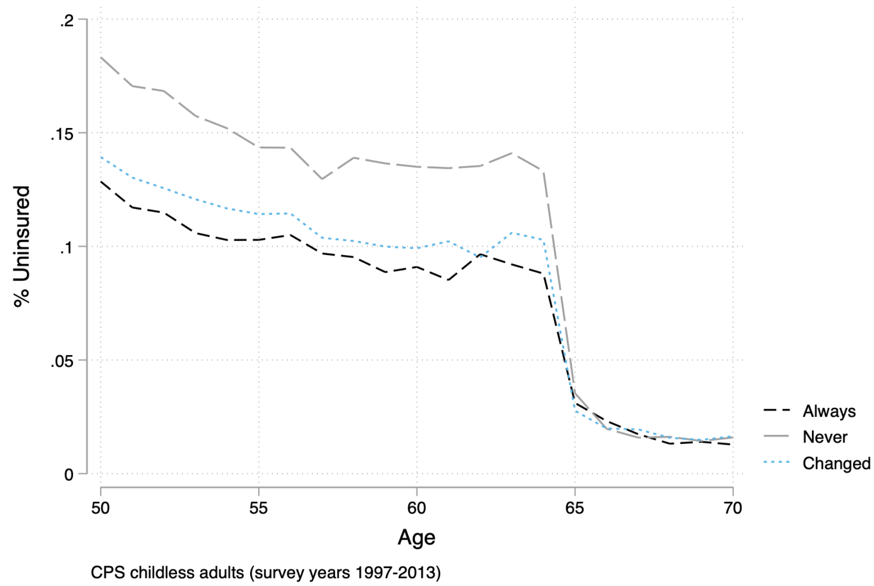


Figure B.19: Difference-in-Discontinuity: Uninsured

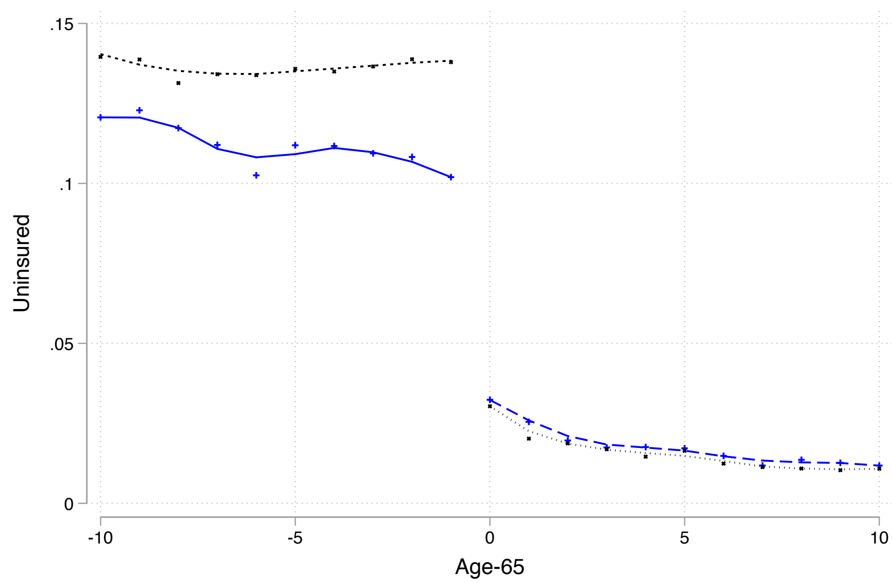


Figure B.20: Difference-in-Discontinuity: Retired

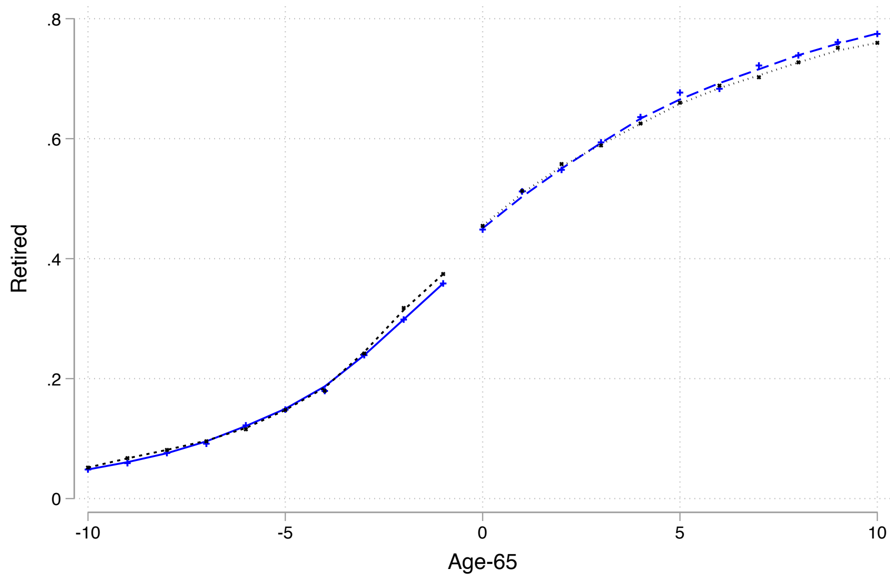


Figure B.21: Difference-in-Discontinuity: Income from Social Security

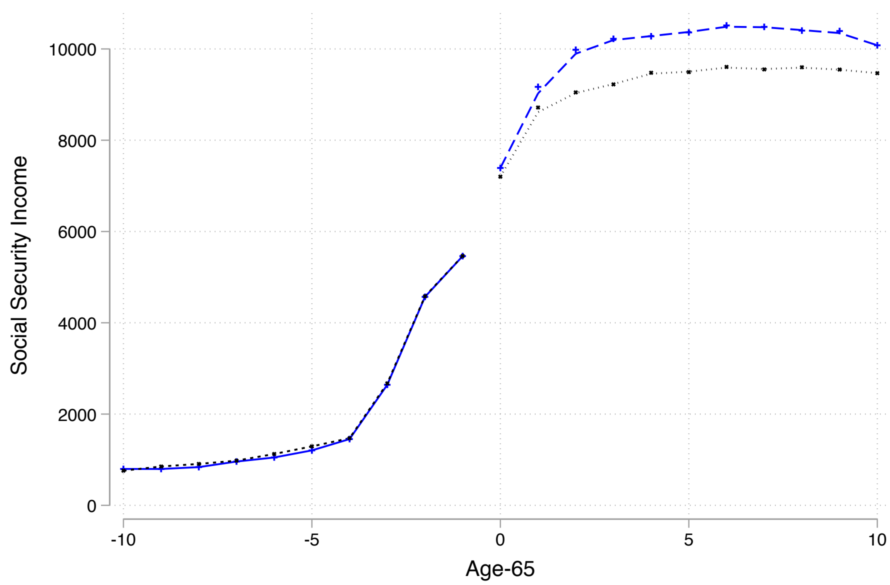


Figure B.22: Occupation and Industry Mobility by Sex

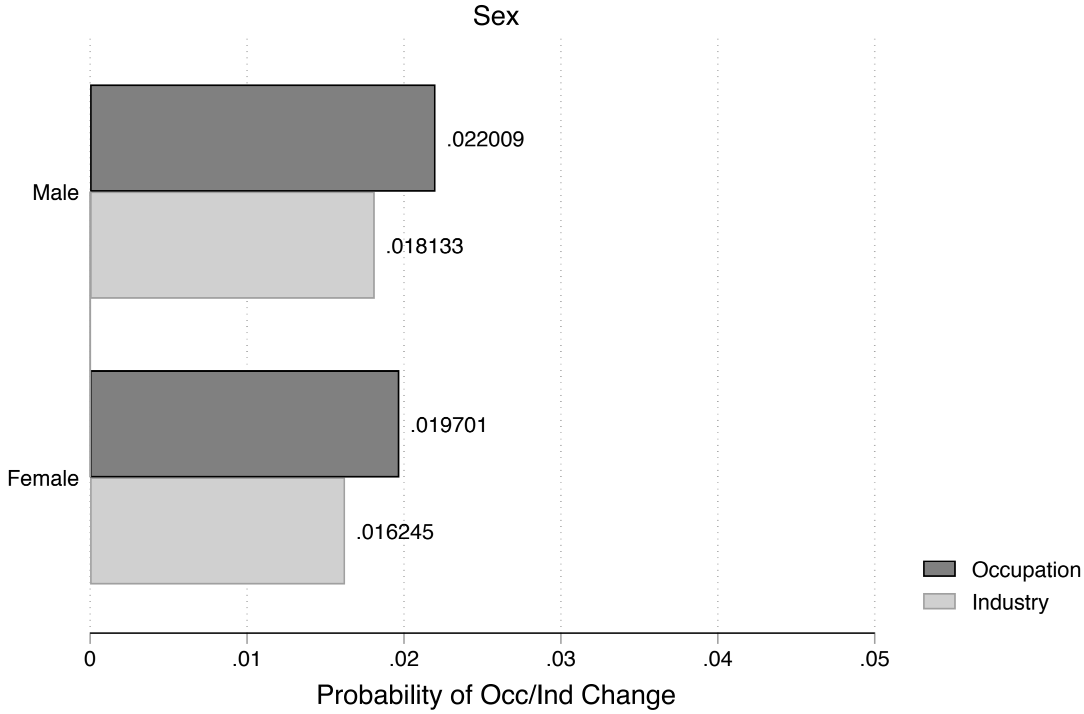


Figure B.23: Occupation and Industry Mobility by Race

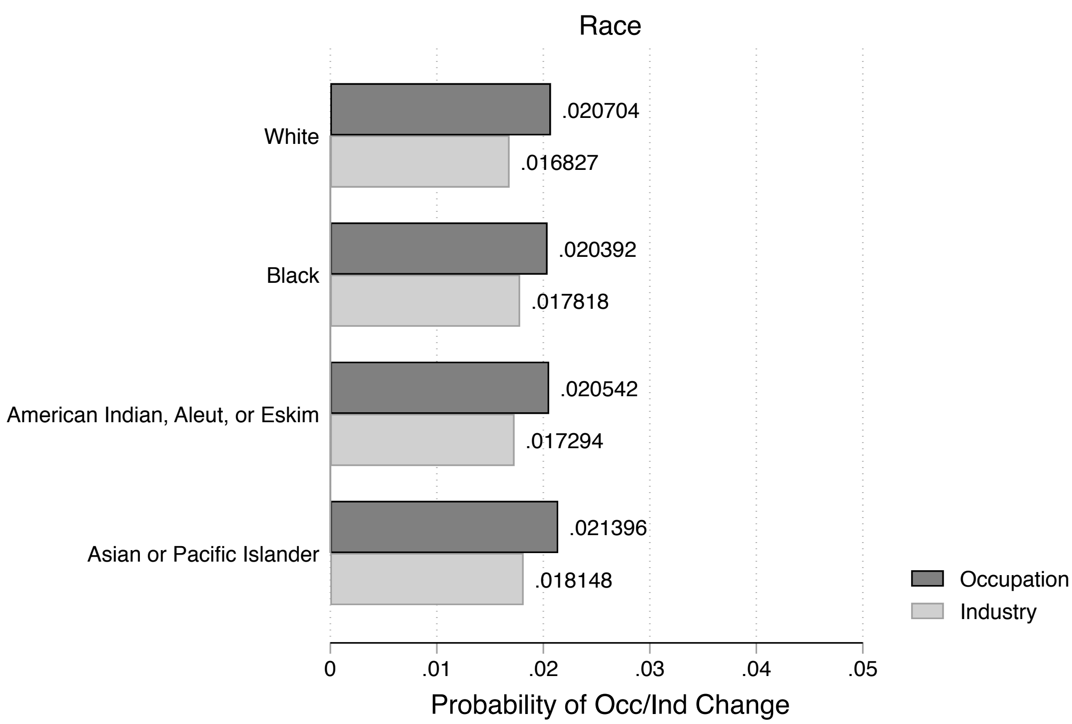


Figure B.24: Occupation and Industry Mobility by Wage Level

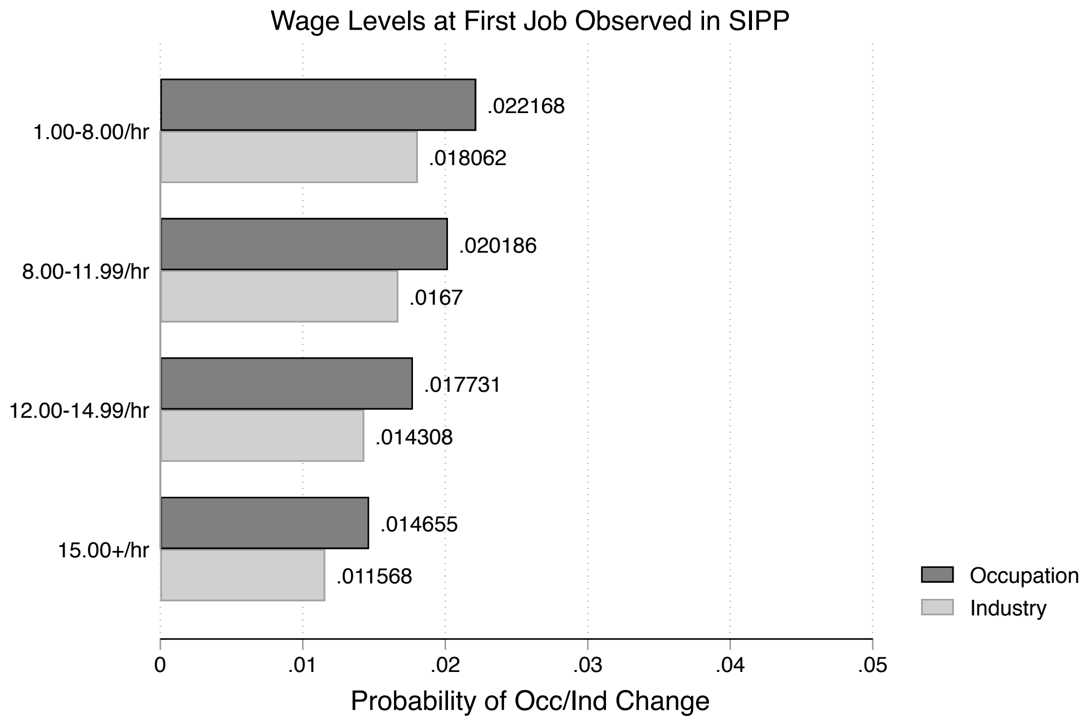


Figure B.25: Occupation and Industry Mobility by Marital Status

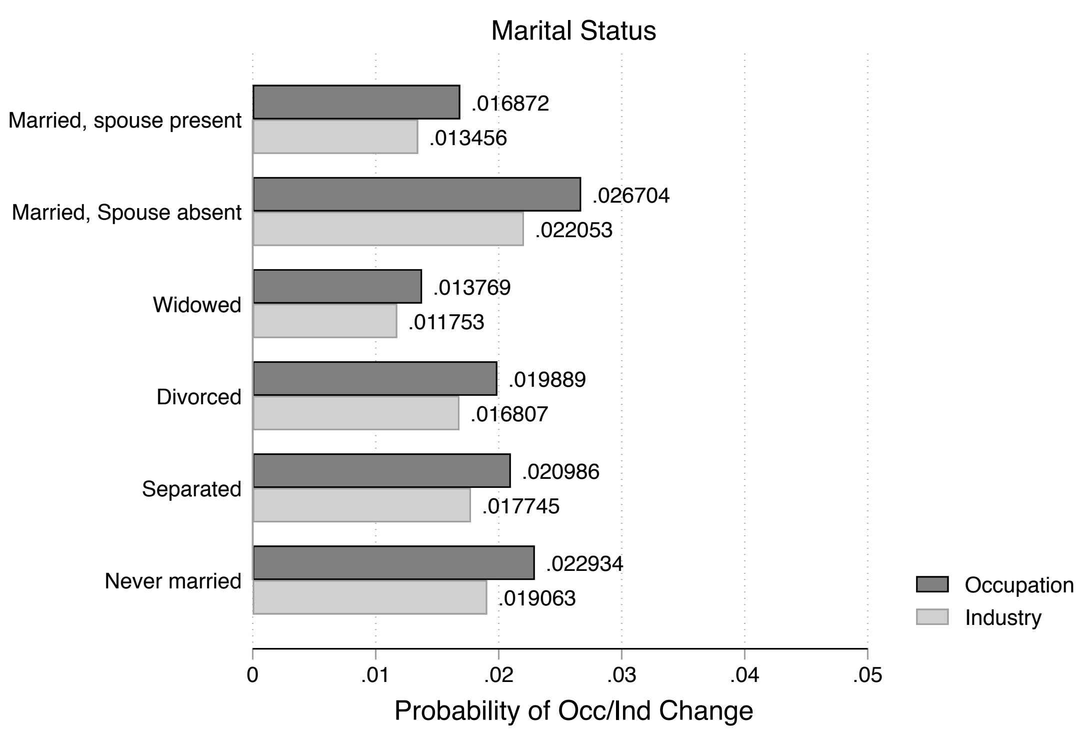


Figure B.26: Occupation and Industry Mobility by Usual Hours Worked

Figure B.27: Occupation and Industry Mobility by Job Length