Health Insurance and the Supply of Entrepreneurs: Evidence from the ACA Medicaid Expansion

Kyung Min Lee*

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Abstract

This paper examines whether the expansion of Medicaid under the Affordable Care Act increases entrepreneurship, measured as self-employment. Using the 2003–2017 Current Population Survey and focusing on childless adults in low-income households, and applying difference-in-differences, propensity score weighting, and instrumental variable (IV) methods, I find that expanding Medicaid eligibility raises the self-employment rate by 14 to 22 percent. The effects are driven by the individuals without spousal employer sponsored coverage. IV estimates indicate that covered individuals have more than two times higher probability to become self-employed. Findings suggest that limited access to health insurance is a barrier to entrepreneurship.

* World Bank; Schar School of Policy and Government at George Mason University. Email: klee12@worldbank.org
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1. Introduction

Does limited access to health insurance reduce entrepreneurship? While most Americans purchase insurance through their own or their spouse's employer, the self-employed tend to be excluded from group insurance and therefore had a much higher rate of uninsured than employees prior to the Affordable Care Act (ACA) (Perry and Rosen 2004).[1] The health insurance barriers faced by the self-employed may distort individual occupational choices concerning self-employment (e.g., Fairlie, Kapur, and Gates 2011, Holtz-Eakin, Penrod, and Rosen 1996).[2] Therefore, a reform improving access to health insurance may affect the supply for the self-employed.

In this paper, I study the 2014 ACA Medicaid eligibility expansion to examine whether improving access to health insurance increases the supply of entrepreneurs as measured by self-employment. The ACA expands Medicaid coverage to all individuals in low-income households, with the biggest expansion among childless adults who had been previously ineligible. A total of 34 states adopted the Medicaid expansion between 2014 and 2017. The target population of the ACA Medicaid expansion is markedly large compared to other health reforms, with the proportion under the eligibility threshold making up about 24 percent of nonelderly citizens in the U.S. (Finegold et al. 2015). Frean, Gruber, and Sommers (2017) suggested that Medicaid expansion explains about 60 percent of the reduction in the rate of the uninsured resulting from the ACA. Stephens et al. (2013) projected that approximately 21.3 million low-income adults would be newly covered by Medicaid under the eligibility expansion. This large target population provides a useful case to study the relationship between health insurance and self-employment decisions.

Theoretically, the effects of free access to health insurance on self-employment are ambiguous. On the one hand, access raises the relative attractiveness of self-employment for both current wage earners and labor force entrants.[3] On the other hand, access to free health insurance

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[1] Even if they are allowed to purchase non-group insurance, individuals still face higher premiums because of adverse selection (Blumberg and Nichols 2004; Gruber 2011).

[2] The argument is similar to that of "job lock," whereby employer-sponsored insurance reduces worker mobility (e.g., Madrian 1994; Gruber and Madrian 2002). In a survey of over 50 empirical studies, Gruber and Madrian (2002) found suggestive evidence that health insurance coverage can distort workers' labor market choices, including labor supply, retirement, and job mobility, but they did not study self-employment decisions.

[3] This study examines both wage earners and labor entrants. Although most of the studies in this literature only focus on wage earners (e.g., Fairlie, Kapur, and Gates 2011), improving access to public health insurance for the self-employed reduces risks associated with starting a new business, regardless of previous jobs. Besides, because wage earners may face higher transition costs (e.g., other benefits in addition to health insurance) than labor force entrants, immediate effects may be more substantial for the non-employed. For example, Hombert et al. (2014) found that

may have a negative income effect that reduces labor supply, including self-employment. If income effects are dominant, the self-employment rate may fall as some self-employed individuals reduce their working hours or even stop working.

Exploiting the geographic and time variations created by state policy implementation, I compare self-employment outcomes between expansion and non-expansion states, before and after the states' adoption of the ACA Medicaid expansion. I focus on low-income childless adults—the group experiencing the largest expansion in Medicaid eligibility—and use 2003-2017 data from the Annual Social and Economic Supplement (ASEC) of the Current Population Survey (CPS). Besides the probability of self-employment, I also analyze self-employment entry and exit, which may be useful in reflecting potential negative income effects. Exploiting the sample rotation design of the CPS, I link individuals across years and create two-year panels with a large number of observations to capture self-employment transitions.

I estimate both intent-to-treat and local average treatment effects of the ACA Medicaid expansion. I use a difference-in-differences approach to estimate the intent-to-treat effects. To address state heterogeneity (e.g., different political parties or health insurance markets), I include state fixed effects in my specifications. Different business cycles at the state level may drive self-employment decisions, so my specifications include time fixed effects as well as state unemployment rates. To further address potential heterogeneity in individuals across states, I extend the difference-in-differences framework with propensity score weighting.

To examine whether the channel for self-employment effects is improved access to health insurance, I examine variation in health insurance demand factors, including access to spouses' employer-sponsored insurance (ESI), which reflects alternative access to health insurance. If a barrier to becoming self-employed is associated with limited access to insurance, the effects will be larger for those without spousal coverage.

I also apply an instrumental variable approach to estimate local average treatment effects.[4] Although the ACA Medicaid expansion could have affected all individuals meeting the eligibility

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unemployment insurance for the unemployed starting a new business in France increased the monthly rate of new firm creation by 25 percent.

[4] Finkelstein et al. (2012) and Baicker et al. (2013) examined the Oregon health insurance experiment, using lottery winning as an instrument to examine the local average treatment effects of Medicaid on health care utilization, financial burden, health outcomes, labor market outcomes and social program participation, but not on self-employment.

criteria, not everyone actually enrolled in Medicaid ("compliers").[5] Previous studies focused on the intent-to-treat effect of the increase in access to health insurance; however, the effects on compliers have received much less attention. This makes it difficult to discuss the average causal effects for the treated population. To address this issue, I use the states' adoption of the ACA Medicaid expansion as an instrument to estimate a causal relationship between Medicaid take-up and self-employment. I also apply propensity score weighting to the instrumental variable approach in order to balance covariates.

The principle alternative interpretation for self-employment effects of the Medicaid expansion would be increased consumer demand. Self-employment could rise if Medicaid expansion has positive fiscal effects for state governments and/or if Medicaid enrollees consume more local goods. In this case, however, self-employment would rise not only in the expanded Medicaid-eligible population but also in non-eligible groups. Moreover, demand could also increase among wage earners for incumbent businesses to produce more goods. To examine this alternative story, I conduct placebo tests by estimating the same specifications for non-eligible groups far above the income threshold (more than 400% of the federal poverty line) and for private sector employment in both difference-in-differences and instrumental variable approaches.

There are three main findings. First, the ACA Medicaid expansion is estimated to raise self-employment. In my difference-in-differences analysis, I find that states that expanded Medicaid eligibility experienced a 1.1 to 1.7 percentage point increase in the self-employment rate among low-income childless adults relative to states that did not expand Medicaid eligibility. These are approximately 14 to 22 percent increases from the unconditional mean of the self-employment rate (7.6 percent). My estimates are consistent with previous studies on the large self-employment effects of Medicare (Fairlie, Kapur, and Gates 2011) and the State Child Health Insurance Program (SCHIP) (Olds 2016). Estimates on self-employment transitions also indicate that the entry rate significantly increased while the exit rate remained steady. The latter suggests that there are no large negative income effects of health insurance access on self-employment.

Second, the evidence supports that the mechanism underlying the self-employment effect was through a reduction in the likelihood of being uninsured when starting a new business. Permitting the Medicaid effect to vary with access to spousal employer-sponsored insurance, I find

[5] The availability of Medicaid could also affect individuals above the income threshold because it acts as potential insurance, such as in a case where a negative health event drives income below the threshold.

that, relative to married childless adults with such access, those without access to alternative health insurance experience a large and significant increase in the propensity of self-employment. These results suggest that a channel of the effects is a decrease in the likelihood of being uninsured for the self-employed by improving access to public health insurance.

Third, Medicaid enrollment engenders a higher propensity of being self-employed. The estimates from the first stage in the instrumental variable approach suggest that the expansion led to a significant increase in Medicaid enrollment in the sample by 10 to 15 percentage points, so only a fraction of the newly eligible population enrolled in Medicaid. The second-stage estimates imply that childless adults newly covered by Medicaid due to the increase in eligibility have about an 11 percentage point higher propensity of being self-employed than those without Medicaid.

Concerning the possibility of an overall demand increase accounting for the self-employment effects, I find no evidence of growth in total employment. Furthermore, the placebo tests using middle- or high-income childless adults show that the magnitudes of the estimated effects of the Medicaid expansion are close to zero and statistically insignificant. Finally, the estimates are also robust to several sensitivity analyses, including different estimations (e.g., synthetic control group methods), different sample restriction (e.g., childless adults with low education), and different treatment and control groups.

My study contributes to the broad literature on health insurance and entrepreneurship by providing new evidence from the ACA Medicaid expansion.[6] Most related to my research are studies that examined the eligibility increase in public health insurance options, which provide mixed results on the self-employment effects of public health insurance. Fairlie, Kapur, and Gates (2011) exploited the eligibility threshold for Medicare at age 65 within a regression discontinuity framework and found that eligibility for Medicare increased the rate of self-employment by those over age 65 by 14 percent from the sample mean of 24.6 percent. This study estimates effects only for older workers around age 65, while the Medicaid expansion applies to all age groups. Olds

[6] There is a broad literature on health insurance and entrepreneurship. Early empirical studies used cross-sectional variation created by ESI and access to spousal coverage. These studies found mixed results (Bruce, Holtz-Eakin, and Quinn 2000, Holtz-Eakin, Penrod, and Rosen 1996, Wellington 2001, Zissimopoulos and Karoly 2007). A major concern is that ESI and spousal coverage are endogenously determined. To address endogeneity issues, recent studies have exploited exogenous variations created by health care reforms or regulations, including health care reforms in the non-group insurance market (Becker and Tüzemen 2014; DeCicca 2010; Heim and Lurie 2014a; Heim and Lurie 2014b; Heim and Lurie 2017; Niu 2014) and tax reforms that reduce the cost of non-group health insurance for the self-employed (Gumus and Regan 2014; Heim and Lurie 2010; Velamuri 2012).

(2016) found that the SCHIP raised the self-employment rate among parents by about 15 percent. He argued that although the SCHIP did not cover parents, it eased their worry about health insurance coverage for their children, thus allowing them to take the risk of starting a business. Bailey and Dave (2019) found a smaller effect of about a 3-4 percent increase in the self-employment rate among older adults under the ACA.

Conversely, other studies found limited evidence on the effects of public insurance on self-employment. In her analysis of Medicaid coverage for young children from 1986 to 1992, Dolan (2015) found that the expansion of Medicaid eligibility increased the likelihood of being self-employed among fathers, but not mothers. Boyle and Lahey (2010) examined the eligibility expansions of the U.S. Department of Veterans Affairs (VA) health system in 2006. They found that after the reform, veterans with a university or more education were more likely to be self-employed compared to nonveterans, whereas veterans with a high school or less education were more likely to be not working compared to nonveterans. Finally, Bailey (2017) examined mandated dependent coverage under the ACA. He found no evidence of the dependent coverage mandate leading to an increase in the self-employment rate among young adults (19 to 25 years old), except for those who were disabled.

Considering that the effects on self-employment differ depending on the target population, reflecting the varied economic environments and demands for health insurance, it is valuable to study different groups. My study provides evidence on previously understudied low-income childless adults across all age groups. In addition, because the extent of the eligibility changes in the ACA Medicaid expansion is significantly larger for childless adults previously excluded from traditional state Medicaid programs, my study provides evidence for clarifying the relationship between health insurance and self-employment with a larger population. My identification strategies are different from previous studies in that I estimate the effects of being covered by Medicaid, in addition to the effects of increased eligibility.

My study also contributes to the literature on the effects of the ACA Medicaid eligibility expansion on labor supply. I provide evidence that Medicaid increases the supply of the self-employed by reducing distortion in the occupational choice between wage earners and self-employment. There is a debate on the unexpected consequences of Medicaid on labor market outcomes. Some studies have investigated state-specific Medicaid programs that increase or decrease eligibility for low-income childless adults (Baicker et al. 2013; Dague, DeLeire, and

Leininger 2017; DeLeire 2018; Garthwaite, Gross, and Notowidigdo 2014). Other empirical studies have directly examined the effects of the ACA Medicaid expansion on labor supply (Duggan, Goda, and Jackson 2019; Gooptu et al. 2016; Kaestner et al. 2017; Leung and Mas 2016). However, to the best of my knowledge, self-employment outcomes have not been studied in this literature, except for Duggan, Goda, and Jackson (2019).

Although Duggan, Goda, and Jackson (2019) examined self-employment as one of their labor market outcomes, it was not their main focus. Using the American Community Survey (ACS), they estimated the effects of the Medicaid expansion using a difference-in-differences analysis with an additional geographic variation of the pre-ACA share of the uninsured with income eligible for the ACA Medicaid or health insurance exchanges at the level of the Public Use Microdata Area (PUMA). They also decomposed the income level into two groups: one for the uninsured with income eligible for ACA Medicaid and the other for the uninsured with income at the market exchange. In their second specification with the decomposition, they found a positive effect on the self-employment rate, but the standard error was large and the estimate was statistically insignificant.[7]

My research differs from Duggan, Goda, and Jackson (2019) in several ways. Unlike their sample, which included all civilians aged 26-64, my study sample is restricted to childless adults aged 26-64 and low- or median-income levels. Childless adults became newly eligible for Medicaid because of the ACA Medicaid expansion. However, parents were eligible for Medicaid before the ACA, and the eligible income levels were heterogeneous across states. As a result, the self-employment effects on parents are likely to be smaller than on childless adults. Because of different specifications, it is difficult to directly compare the estimates, but pooling together those affected by the policy (childless adults) with those unaffected (parents) could result in smaller coefficients and larger standard errors. Moreover, the aggregated uninsured rate (at the PUMA level) is likely to be a noisy indicator of individual demand for health insurance.

Instead of geographic variation in the aggregated uninsured rate, my specification exploits the variation coming from spousal coverage, available in the CPS-ASEC. This allows me to

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[7] Following the interpretation in Duggan, Goda, and Jackson (2019), the estimate for the expansion states in their specification is the sum of two coefficients: $(\text{Post} \times M^{*}) + (\text{Expansion} \times \text{Post} \times M^{*}) = 0.0019 + 0.0140 = 0.0159$, where M^{*} is the pre-ACA share of the uninsured with income eligible for Medicaid. This indicates an increase in the self-employment rate in a PUMA with $M^{*} = 1$ relative to $M^{*} = 0$. In other words, a 10 percentage point increase in M^{*} is associated with a 0.16 percentage point increase in the self-employment rate.

examine individual access to alternative health insurance instead of aggregate-level variation. I also examine not just self-employment levels but transitions, including self-employment entry and exit. Compared to wage earners, self-employed individuals have more flexibility to manipulate in reporting their incomes, which suggests that the self-employed may change income levels to get Medicaid. The self-employment entry reduces this concern because it captures those who have newly become self-employed.

There are some caveats in this research. First, entrepreneurship is proxied by self-employment. Of course, the self-employed are heterogeneous in terms of their activities and the types of business they create. Nevertheless, self-employment has been used as a working definition (or proxy) for entrepreneurs in entrepreneurship studies (Parker 2009) because self-employment captures some aspects of entrepreneurship in terms of risk-taking and occupational choices. In addition, because the literature on health insurance and entrepreneurship uses self-employment to measure entrepreneurial activities, using self-employment also allows the comparison of my estimates with those from previous studies. Finally, because the types of businesses low-income people tend to be small and unsophisticated (Balkin 1989), self-employment is a relevant measure to capture entrepreneurship in low-income households.

Second, reported income is used to restrict the sample to focus on relevant groups. However, incomes are endogenously determined with labor supply. Self-reported income in the CPS is different from the modified adjusted gross income determining income eligibility for Medicaid. In addition, while the CPS provides information on annual income, an individual can join Medicaid anytime based on monthly income. To address these issues, I use a wider range of income levels to restrict the baseline sample and compare the estimates from the baseline with those from a sample based on the exact income threshold. Similar to Kaestner et al. (2017), I also check the robustness of my estimates by selecting a sample based on education level, presumably determined prior to the ACA Medicaid expansion.

Lastly, the decisions by states to expand Medicaid are not randomly determined. Although I use propensity score weighting in my first stage, there could still be remaining differences in unobserved characteristics between expansion and non-expansion states. Considering this potential bias, the IV approach in this study should be interpreted as effects on compliers rather than causal effects. The IV estimates capture whether those newly covered by Medicaid have much higher tendencies to become self-employed.

Subsequent sections of this paper proceed as follows. Section 2 describes Medicaid eligibility expansion under the ACA and provides the research design. Sections 3 and 4 describe the data and methods. Section 5 discusses the results, and Section 6 is a conclusion.

2. Medicaid Expansion under the Affordable Care Act

The initial ACA rendition mandated Medicaid expansion to all individuals in families with incomes below 138 percent of the federal poverty line (FPL) and subsidies to all individuals with incomes between 100 and 400 percent of the FPL, starting in 2014. However, a 2012 Supreme Court ruling made this mandate optional to individual states. On January 1, 2014, 25 states adopted this Medicaid expansion, and 7 states followed suit between 2014 and 2017, while 19 states opted out. In non-expansion states, individuals with incomes between 100 and 400 percent of the FPL still receive subsidies, but those with incomes below 100 percent of the FPL do not receive Medicaid or subsidies. This creates a huge coverage gap for low-income households. I provide a list of states with their status regarding Medicaid expansion under the ACA in Table 1.[8]

The target population of the Medicaid expansion is noticeably larger than other policy components of the ACA. The income level corresponding to 138 percent of the FPL was $12,060 for a single family and $24,600 for a family of four in 2017.[9] According to the U.S. Department of Health and Human Services, about 16 percent of the population is below the FPL, and 8 percent of the nonelderly population is between 100 and 138 percent of the FPL.[10] Therefore, the eligibility threshold of the ACA Medicaid expansion covers about 24 percent of nonelderly citizens in the U.S.

Income eligibility for Medicaid is determined based on modified adjusted gross income (MAGI).[11] The MAGI is computed by adding certain deductions to taxable income or the adjusted gross income (AGI).[12] This implies that actual income may be larger than 138 percent of the FPL,

[8] The information of the state-level Medicaid expansion is from the Kaiser Family Foundation's the Status of State Action on the Medicaid Expansion Decision, which is available at the following link: https://www.kff.org/health-reform/state-indicator/state-activity-around-expanding-medicaid-under-the-affordable-care-act/?currentTimeframe=0&sortModel=%7B%22colId%22:%22Location%22,%22sort%22:%22asc%22%7D
[9] See the Federal Register notice of the 2017 poverty guidelines: http://www.kff.org/uninsured/issue-brief/the-coverage-gap-uninsured-poor-adults-in-states-that-do-not-expand-medicaid/view/footnotes/#footnote-201034-1
[10] See Figure 4. Distribution by Income: QHP-Eligible Uninsured vs. General Nonelderly Population: https://aspe.hhs.gov/basic-report/health-insurance-marketplace-uninsured-populations-eligible-enroll-2016
[11] For a methodology to calculate MAGI, see CMS (2012).
[12] Examples of the types of deductions added in MAGI include student loan interest, one-half of self-employment tax, qualified tuition expenses, tuition and fees deductions, passive loss or passive income, IRA contributions, taxable

which is based on MAGI. Moreover, because net self-employment income is computed by subtracting business expenses from business income, business income could be much larger than 138 percent of the FPL for the self-employed compared to wage earners, depending on business expenses.

The Supreme Court decision resulted in exogenous geographical and time variations by states that adopted the Medicaid expansion between 2014 and 2017. The increase in eligibility was the largest for childless adults because they were previously excluded from most states' Medicaid programs. Figure 1 shows differences in the average eligibility threshold for childless adults between expansion and non-expansion states from 2011 to 2017. Before the implementation of the ACA, childless adults were ineligible for federally funded Medicaid in most states, except for some expansion states that made partial or full expansions before 2014. The average eligibility threshold of expansion states was about 30 percent of the FPL. The eligibility threshold jumped to about 120 percent of the FPL in 2014 and then increased to 138 percent as late expansion states adopted the eligibility under the ACA. By contrast, the average eligibility was zero percent of the FPL in non-expansion states before 2014. There was a slight increase in the eligibility threshold in 2014 since Wisconsin increased state-level eligibility for childless adults up to 100 percent of the FPL without adopting ACA Medicaid. Although there were some differences between expansion and non-expansion states, this figure indicates that the largest change in the eligibility threshold occurred in 2014.

Figure 2 highlights the geographic variation used in this study to create treatment and control groups. Using the status of the Medicaid expansion, I create the treatment (expansion states) and control (non-expansion states) groups. I exclude Alaska and Hawaii from the analysis due to different guidelines for the FPL compared to the 48 contiguous states and the District of Columbia (DC). I also exclude 8 prior expansion states that fully or partially expanded eligibility for childless adults (Arizona, Colorado, Connecticut, Delaware, District of Columbia, Minnesota, New York, and Vermont) before 2014 from the treatment group. This is because full prior expansion states increased their eligibility thresholds higher than 138 percent of the FPL, and most partial prior expansion states increased their eligibility thresholds to close to 100 percent of the FPL. Finally, I exclude Wisconsin from the control group because it increased state-level eligibility to childless

social security payments, the exclusion for income from U.S. savings bonds, rental losses, any overall loss from a publicly traded partnership, etc.

adults with income below 100 percent of the FPL. After excluding these states, the treatment group consists of 22 states, while the control group includes 18 states. To counter the possibility that prior expansions may influence the estimated effects, I also estimate effects by including states that made full prior expansion as a robustness check.

3. Empirical Methods

3.1 Difference-in-differences

The baseline estimation approach is a difference-in-differences (DID) analysis that compares expansion states to non-expansion states before and after the states' adoption of the ACA Medicaid expansion. As mentioned earlier, the treatment group includes 22 states that expanded Medicaid between 2014 and 2017, whereas the control group consists of 18 states that did not expand Medicaid. The pre-period is 2003 to 2013, and the post-period is 2014 to 2017. The sample consists of nondisabled childless adults aged between the ages of 26 and 64. I estimate the following DID specification:

$$(1)\quad Y_{ist} = \alpha_0 + \beta(Expansion_s \times Post_{st}) + X_{ist}\gamma + \delta Unemp_{st} + \mu_s + \tau_t + \varepsilon_{ist}.$$

Y_{ist} represents the outcome variables by individual i in state s in time t. Outcome variables include self-employment level, entry, and exit. $Expansion_s$ is an indicator variable for expansion states. $Post_{st}$ is an indicator variable for post periods, which is defined based on each state's ACA Medicaid expansion year. X_{ist} is the set of demographic and human capital variables. $Unemp_{st}$ indicates the unemployment rate for state s in time t, which controls states' business cycles that may affect labor market outcomes differently. The model includes state fixed effects (μ_s) and time fixed effects (τ_t), which remove time-invariant state-specific heterogeneity and contemporaneous shocks, respectively. Given that the focus of this study is the differences in conditional expected values between expansion and non-expansion states across time, I use linear probability models to estimate equation (1). Because of the different timing of the Medicaid expansions, I cluster standard errors at the state-year level.[13] As a robustness check, I also estimate these equations with Logit and Probit models.

The key identifying assumption of the DID specification is a common trend assumption that in the absence of the Medicaid expansion, the trend of the rate of self-employment in expansion states would be the same as the trend in non-expansion states. If this assumption were true, the coefficient of the interaction term (β) would capture the impact of the Medicaid expansion on outcome variables.

To check whether this identifying assumption is plausible, I conduct an event study. I estimate a new specification that interacts treatment states with time fixed effects in the following form:

(2) $Y_{ist} = \alpha_0 + \sum_{t=2003}^{2017} \beta_t (Expansion_s \times Year_t) + X_{ist}\gamma + \delta Unemp_{st} + \mu_s + \tau_t + \varepsilon_{ist}.$

In this specification, $\sum_{t=2003}^{2017} \beta_t$ captures changes in outcomes in the expansion states across time periods, relative to 2013. If the estimated differences between expansion and non-expansion states before 2014 ($\sum_{t=2003}^{2012} \beta_t$) are close to zero and statistically insignificant, the parallel trend assumption could be justified. The estimated effects from 2014 to 2017 ($\sum_{t=2014}^{2017} \beta_t$) differentiate short-term effects from long-term effects.

To examine the mechanism of the effects, I further exploit the variation of demand factors for health insurance within a DID framework. Because individuals have different level of demand for health insurance, the effects of the Medicaid expansion may be heterogenous depending on access to health insurance. I permit the difference-in-differences estimator to vary according to whether there is access to a spouse's ESI, which is an alternative health insurance. Using information from either spouse, here my sample is restricted to married couples. The specification of the model is as follows:

[13] The bootstrapped standard errors clustered at the state-year level provide similar sizes of standard errors without bootstrapping. I also check the sensitivity of standard errors with the state-level cluster because many states adopted the Medicaid expansion at the same time on January 1, 2014. Although I do not report this in this paper, the standard errors become larger, but the most estimated effects are still statistically significant at the 5 or 10 percent level.

(3) $Y_{ist} = \alpha_0 + \beta_1 Expansion_s \times Post_{st} + \beta_2 Expansion_s \times Post_{st} \times NoSESI_{ist} + X_{ist}\gamma + \delta Unemp_{st} + \mu_s + \tau_t + \varepsilon_{ist}$,

where $NoSESI_{ist}$ is an indicator for no access to spousal employer-sponsored insurance. β_1 provides the effects on married couples with access to spousal coverage. β_2 captures how much larger the effect is for those without access to spousal coverage relative to those with access. In addition to a relative effect, I also estimate $\beta_1 + \beta_2$ and test $\beta_1 + \beta_2 \neq 0$ to estimate the effects on married couples without access to alternative health insurance.

Those with spousal coverage are less likely to experience a higher chance of being uninsured when they become self-employed because they have access to alternative health insurance. Therefore, they are less likely to worry about access to group insurance when they make an occupational choice between self-employment and being a wage earner. If the Medicaid expansion increases access to health insurance by reducing the likelihood of being uninsured for the self-employed, a positive and significant β_2 would be expected.

3.2 Propensity Score Weighting

One concern in the DID analysis is that the differences in characteristics between treatment and control groups may affect the trends of outcome variables if these characteristics are associated with outcome variables (Abadie 2005; Imbens and Wooldridge 2009). Additionally, repeated cross-sectional data (such as CPS or ACS) include a random sample in each time period, which may lead to changes in compositions of samples over time (Abadie 2005; Blundell and Costa Dias 2009; Stuart et al. 2014).

To adjust unbalanced observable characteristics and composition changes resulting from repeated cross-section data over time, I apply multiple group propensity score weights to a parametric DID specification, as in Stuart et al. (2014).[14] I define four groups based on Medicaid expansion status as well as pre- and post-periods: Group 1 (expansion states in pre-period), Group 2 (expansion states in post-period), Group 3 (non-expansion states in pre-period), and Group 4 (non-expansion states in post-period). Group 1 is my baseline group for constructing relative

[14] Abadie (2005) originally proposed a similar approach using a propensity score weighing with a semiparametric difference-in-differences model. Blundell and Costa Dias (2008) also suggested a similar approach using a group propensity score matching with a parametric difference-in-differences model.

weights. I use multinomial logistic regression to compute propensity scores for the four groups based on observed characteristics, including age, gender, race/ethnicity, marital status, citizenship, education, and veteran status. Then, I construct weights with the following equation:

(4) $w_i = p_1(X_i)/p_g(X_i)$,

where w_i is the weight for an individual i and $p_g(X_i)$ is the propensity score for an individual i in Group g, for g=1-4. The weight is proportional to the probability of being in Group 1 (the expansion states in pre-periods) relative to the probability of being in the observed Group g. Individuals in Group 1 receive an equal weight of 1, while those in Groups 2 to 4 receive proportional weights.

Applying the weights defined in equation (3) to the baseline model in equation (1), I estimate the propensity score-weighted difference-in-differences (PSW-DID). The treatment effect of this model is specified as follows:

(5) $$\begin{aligned} Effect = {} & (w_i E[Y_{ist}|X_{ist} = x, E_s = 1, P_t = 1] - w_i E[Y_{ist}|X_{ist} = x, E_s = 0, P_t = 1]) \\ & -(E[Y_{ist}|X_{ist} = x, E_s = 1, P_t = 0] - w_i E[Y_{ist}|X_{ist} = x, E_s = 0, P_t = 0]) \end{aligned}$$

The third term is estimated from Group 1. The other three terms are estimated by weighting Group 2 (the first term), 3 (the fourth term), and 4 (the second term). This approach is similar to the inverse probability of treatment weighting (IPTW) in that the observations are weighted based on observable characteristics. However, in this specification, weights are constructed to reflect not only treatment status but time periods.

3.3 Instrumental Variables

Both DID and PSW-DID identify the intent-to-treat (ITT) effects of being eligible for Medicaid through the states' adoption of the ACA Medicaid expansion. Even though the ITT estimates provide the net effects of expanding the eligibility of Medicaid to low-income childless adults, regardless of whether they opted for Medicaid coverage, it is also very important to understand the outcomes of those who enrolled in Medicaid ("compliers").

The relationship between Medicaid coverage and self-employment outcomes can be specified as in the following equation:

$$(6)\quad Y_{ist} = \alpha_0 + \pi Medicaid_{ist} + X_{ist}\gamma + \delta Unemp_{st} + \mu_s + \tau_t + \varepsilon_{ist},$$

where $Medicaid_{ist}$ is an indicator of being covered by Medicaid. Other variables are the same as in the previous specification. The coefficient of $Medicaid_{ist}$ captures the average difference in outcomes between individuals with and without Medicaid coverage.

However, because Medicaid coverage is endogenously determined, directly estimating equation (6) with the ordinary least square (OLS) provides a biased estimate of the Medicaid coverage. To get bias-corrected estimates, I apply an instrumental variable (IV) approach using the ACA Medicaid expansion as an instrument.[15] I estimate equation (6) using two-stage least squares (2SLS) with the following first-stage equation:

$$(7)\quad Medicaid_{ist} = \alpha_0 + \theta(Expansion_s \times Post_{st}) + X_{ist}\gamma + \delta Unemp_{st} + \mu_s + \tau_t + \varepsilon_{ist}.$$

In this equation, the instrumental variable is the interaction term ($Expansion_s \times Post_{st}$), which indicates the ACA Medicaid expansion status. Following Angrist and Pischke (2009), I estimate both first and second equations with a linear probability model and examine whether the F-statistic of the first stage is larger than 10 to check for the possibility of a weak instrument. The first stage in equation (7) is a standard DID specification. As in equation (1), the first-stage equation relies on a common trend assumption. Therefore, I also apply the PSW to the IV (PSW-IV) estimation, which reduces potential bias in the first-stage equation.

The IV approach estimates a local average treatment effect (LATE) for compliers, which is the ratio of the reduced-form estimate in equation (1) to the first stage estimate in equation (7). Considering that the Medicaid expansion is not randomly assigned, I interpret the IV estimates as the effects on compliers rather than causal effects. In this case, compliers are a subgroup of childless adults who are newly covered by Medicaid because of the Medicaid expansion. The IV

[15] Baicker et al. (2013) and Finkelstein et al. (2012) applied an instrumental variable approach to estimate the effects on compliers in their analyses of Oregon's Medicaid expansion.

estimates will capture whether those newly covered by Medicaid have much higher tendencies to become self-employed because of new Medicaid coverage.

The assumption of the IV approach is that the Medicaid expansion affects self-employment outcomes only through being covered by Medicaid (exclusion restriction). There are two potential concerns with this assumption. First, there could be an alternative interpretation of whether demand shock from the Medicaid expansion may affect self-employment decisions. If the Medicaid expansion brings federal money to the expansion states and creates more business opportunities, the expansion may affect self-employment outcomes regardless of Medicaid coverage, therefore violating the exclusion restriction. To deal with this concern, I conduct a placebo test on individuals with 300 percent or more of the FPL and a propensity for being wage earners. If the Medicaid expansion creates more business opportunities, high-income groups and incumbent businesses also respond to such opportunities.

Second, health insurance market exchanges started in both expansion and non-expansion states. Particularly, in the non-expansion states, individuals with income between 100 and 138 percent of the FPL are eligible for subsidies. The subsidies for health insurance my affect both health insurance coverage and self-employment outcomes, which violates the exclusion restriction and may lead to an underestimation bias in the estimated LATE. Nevertheless, my IV estimate still provides the lower bound for the effects on compliers. However, to explicitly address this concern, I further restrict my sample to those with below 100 percent of the FPL and compare the IV estimates.

Another technical assumption is that the Medicaid expansion only positively affects the coverage of Medicaid (monotonicity). In other words, the likelihood of being covered by Medicaid under the Medicaid expansion would be at least equal to or greater than the likelihood under no Medicaid expansion. By this assumption, there are no defiers who drop out of the Medicaid coverage because of the Medicaid expansion. For childless adults, this assumption is reasonable in that they were previously ineligible for Medicaid. The Medicaid expansion is expected to have either positive or no effects on the Medicaid coverage for childless adults, but no negative effects.

4. Data

I use data from the Annual Social and Economic (ASEC) Supplement of the Current Population Survey (CPS).[16] The CPS is a nationally representative survey of U.S. households on a monthly basis. It provides detailed demographic and labor force information from the previous week. The ASEC provides additional information on work experience, income, migration, health insurance, and household members' health conditions, as well as receipt of noncash benefits in the previous calendar year. Since the CPS ASEC provides information for the previous year, I use the data between 2004 and 2018 for the analysis between 2003 and 2017.

Detailed demographic variables are available in the CPS ASEC. For this study, I use age, gender (female), race/ethnicity (Hispanic, non-Hispanic white, non-Hispanic black, non-Hispanic Asian, and non-Hispanic other), immigration status (foreign born status), marital status (married, divorced or separated, widowed, and never married), veteran status, and education (less than high school, high school, some college, university, and more than university). In addition to demographic variables, I also use family income and the official poverty rate to construct the percentage of the federal poverty line (FPL) to define samples, which is discussed in the sample selection in detail. The information on different types of health insurance coverage in the previous calendar year are available in the CPS ASEC.[17] Using that information, I construct health insurance variables including uninsured (no insurance coverage in the last year), employer sponsored insurance (ESI), spousal ESI, non-group insurance, and Medicaid.

The main outcome variables are self-employment activities available in the CPS ASEC. The self-employed are defined as respondents who indicated self-employment as the longest job

[16] The CPS ASEC data comes the Integrated Public Use Microdata Series (IPUMS) CPS (Flood, King, Ruggles, and Warren 2017). The IPUMS-CPS "harmonized" variables in CPS to make feasible cross-time comparisons.

[17] Unlike the American Community Survey (ACS), which asks about the coverage of health insurance at the point of interview, the CPS ASEC traditionally asks about coverage at any time in the previous calendar year. In the CPS ASEC 2014 (for the insurance coverage in 2013), the health insurance questionnaires were redesigned to improve the health insurance coverage measures, add the point of the interview, collect new information on the ACA, and gather information on changes in coverage (SHADAC 2014; SHADAC 2016). The main change relevant to this study was adding the point of the interview. The redesigned questionnaires first ask about the current coverage at the time of the interview and then ask about the coverage at any time between January of the previous year and the interview month, which implies a slight increase in the reference periods. Comparing the traditional questionnaire to the redesigned questionnaire in CPS ASEC 2014, Brault et al. (2014) found that the redesign reduces the estimate of the uninsured in 2013 compared to the previous CPS ASEC questionnaire, but reduction was not large enough to get the same estimate in other federal surveys like ACS. It should be noted that the redesign affects only the responses about insurance coverage. It does not affect the reduced form estimates on self-employment outcomes. Although the results are not reported in this paper, I conduct an event study and found that it does not affect the estimates on the Medicaid coverage in 2013 differentially between expansion and non-expansion states.

held in the previous calendar year. This definition includes both full- and part-time as well as incorporated and unincorporated self-employment. I acknowledge that self-employment is not identical to entrepreneurship, but I consider it as a common path for low-income individuals to become an entrepreneur in that self-employment captures the essential aspects of entrepreneurship, such as risk-taking aspects. Both the economics and business literature often use self-employment to capture entrepreneurial activities. Given previous studies that used self-employment to study entrepreneurship lock, using self-employment is useful to compare my estimates with other studies. To understand the types of self-employment under the Medicaid expansion, I further examine self-employment with positive business income, full- versus part-time self-employment, and incorporated versus unincorporated self-employment at the end of the analysis.

In addition to static self-employment variables, the unique rotation sampling design of the CPS permits investigation of self-employment flows. The CPS includes a total of eight cohorts of household samples. A new cohort of households is included each month, while the oldest cohort is excluded from the CPS sample. These households are interviewed for four months, excluded from the survey for eight months, and then re-interviewed for four months. Exploiting this rotation feature, I link individuals across two years in the CPS ASEC, which creates two-year longitudinal data. To link individuals, I use person identifiers in the IPUMS CPS, which excludes Hispanics and the State Children's Health Insurance Program (SCHIP) oversamples of the ASEC.[18] I further validate the same individuals by using age, gender, and race. Theoretically, at the best, half of the CPS ASEC samples (four among eight cohorts) can be linked across two years. After excluding oversamples and completing the validation process, about 35 percent of individuals under the CPS ASEC are linked.

Using this longitudinal sample, I create one-year self-employment transition variables instead of estimating fixed-effect models because the study time period (2003-2017) is much longer than a two-year panel. At the same time, key policy changes happened in 2014 in most states, which implies that fixed-effect models will be estimated based on variations between 2013 and 2017. For each individual, I construct self-employment entry and exit variables by comparing individual’s self-employment status between two years. Then, I further decompose self-employment entry into self-employment entry from wage earners versus non-employed. I make

[18] Drew et al. (2014) describes details on how IPUMS links individuals using CPS micro data.

the same decomposition for self-employment exit to wage earners versus non-employed. Because I constructed self-employment transition variables based on only part of the sample, they cannot be directly comparable to self-employment levels. Nevertheless, transition variables provide a unique opportunity to separately examine the income effect of the Medicaid expansion (e.g., entry and exit). The decompositions of transition variables also allow investigation of whether the self-employed come from either wage earners or labor entrants.

I restrict the sample to nondisabled, childless adults aged between 26 and 64 to reduce potential bias from access to alternative health insurance. Disabled individuals are eligible for Medicaid. Those aged 65 or older are eligible for Medicare. The ACA-mandated dependent coverage allows young adults to be covered by parents' employer-sponsored insurance (ESI) until they reach age 26. Since childless adults were previously excluded from the Medicaid-eligible groups in most states, they are less likely to be influenced by the states' Medicaid programs before the ACA Medicaid expansion.

I further restrict the sample based on household incomes. Given that the Medicaid expansion made all people under 138 percent of the FPL eligible for insurance, low-income individuals are more likely to be influenced by the expansion. I use three low-income samples—those below 300, 138, or 100 percent of the FPL. My baseline sample is childless adults with income below 300 percent of the FPL, and then I compare estimates from the baseline sample to those from 138 and 100 percent of the FPL.

There are several potential issues in selecting a sample based on incomes. One concern is that some individuals may adjust their incomes to below 138 percent of the FPL in order to be eligible for Medicaid. The self-employed have, particularly, a greater ability to modify their incomes compared to wage earners. Another concern is that although non-expansion states do not increase the eligibility of Medicaid for childless adults, they provide subsidies to those with incomes between 100 and 138 percent of the FPL. Because of the subsidies, some childless adults in non-expansion states may purchase health insurance in the exchanges and become self-employed, which may reduce the differences in self-employment between expansion and non-expansion states. In addition, some self-employed in non-expansion states may increase their income to above 100 percent of the FPL to get subsidies. In this case, restricting the sample based on 100 percent of the FPL may increase the magnitude of the effects.

In Figure 3, I provide the distributions of the FPL for those with incomes below 300 percent of the FPL for expansion and non-expansion states before and after the Medicaid expansion. To examine changes in the distributions before and after the Medicaid expansion, I overlap both pre- and post-expansion histograms with thresholds for 100 and 138 percent groups in each sub-figure. The size of each bin is 6 percent of the FPL. The first two distributions based on the entire sample show a high proportion of childless adults close to 0 percent of the FPL because the sample includes the non-employed. Before the Medicaid expansion, the share of 0 to 6 percent of the FPL is about 0.06 for both expansion and non-expansion states. However, after the Medicaid expansion, the share increased, and the change is somewhat larger for non-expansion states. Aside from this, there is no significant change in distribution after the Medicaid expansion in both expansion and non-expansion states.

Although the self-employed have a greater ability to adjust their income, the share of the self-employed is much lower than wage earners. Because of this, the distribution of the entire sample may not show income adjustment by the self-employed. In the next four sub-figures, I examine the same income distributions conditional on wage earners or the self-employed. For the wage earners, the distribution in the post-expansion period is slightly bumpy, but the general patterns are similar to the pre-expansion period. I do not find a significant difference between expansion and non-expansion states. On the other hand, for the self-employed, the income distributions in the post-expansion period appear differently between expansion and non-expansion states. The share of 0 to 6 percent of the FPL increases in non-expansion states, while it decreases in expansion states. In addition, the post-expansion distributions show some suggestive evidence of bunching below 138 percent of the FPL in the expansion states, but not in the non-expansion states. However, I do not find bunching right above 100 percent of the FPL in non-expansion states.

These distributions suggest that income adjustment is of less concern for wage earners, but it may be a concern for the self-employed. Some self-employed in the expansion states may adjust their incomes below 138 percent of the FPL and become eligible for the Medicaid. Although the distributions do not suggest that some self-employed in non-expansion states also adjust their income above the 100 percent of the FPL, this does not indicate no subsidy effects. The baseline sample based on 300 percent of the FPL does not separate subsidy effects, but it partially addresses the issues of income adjustment because a larger income boundary considers the self-employed who are close to the thresholds and have adjusted their incomes as incumbents.[19] As a result, the baseline estimates provide the lower bound of the Medicaid expansion effects.

Although using the sample with income below 300 percent of the FPL partially addresses selection problems and measurement issues, it does not completely exclude subsidy effects available for those with income below 400 percent of the FPL. The assumption in my empirical method is that subsidy effects for those with income between 138 and 400 percent of the FPL are similar between expansion and non-expansion states, and the difference between expansion and non-expansion states offsets the subsidy effects. In addition, estimates based on the sample below 100 or 138 percent of the FPL exclude those who are eligible for subsidies. Because of the reasons mentioned above, using three different low-income samples is important to check potential biases.

Some may consider exploiting income thresholds in an identification strategy instead of restricting samples. However, publicly available surveys make it challenging to explicitly use income to estimate effects. The information on household income in the survey data may differ from the income applied to Medicaid eligibility for several reasons. First, because household income in the publicly available surveys (including the CPS ASEC in this study) is self-reported by respondents, it is subject to measurement errors. Even if there are extremely low measurement errors, there may be a significant time gap between income and eligibility. Surveys in general measure annual income, but Medicaid eligibility is determined based on monthly income. This implies that some people with annual incomes above the eligibility threshold may be eligible for Medicaid in certain months in the same year. Moreover, because eligibility is determined based on modified adjusted gross income (MAGI), not actual income, the income levels of the self-employed could be larger than the eligibility threshold.

Figure 4 provides scatter plots of the Medicaid coverage by the FPL with quadratic fitted lines for expansion and non-expansion states. In the pre-expansion period, the share of Medicaid coverage increases up to about 80 percent of the FPL and then gradually decreases as income increases in both expansion and non-expansion states. In the post-expansion period, the share jumps only in the expansion states, while the pattern of the share by the FPL is the same in the non-expansion states. The quadratic fitted lines make a parallel shift in the expansion state, which

[19] Analysis of self-employment transition does not suffer from this issue because it captures only people who newly become self-employed. However, because of the small number of transitions, the estimates based on income below 138% of the FPL may be inaccurate (with larger standard errors).

indicates that Medicaid coverage rises for childless adults not just below the 138 percent of the FPL but also above that threshold, as discussed. Also, the quadratic functions make a smooth transition across the threshold.

Figure 5 presents the share of the self-employed by the FPL. Compared to Figure 5, this plot is less clear in the jump of the self-employed share in expansion states. Although the share of the self-employed sees a decrease for those below 50 percent of the FPL, the share rises as income increases in the expansion states relative to non-expansion states, which suggest that the Medicaid expansion may have a positive effect on self-employment. The quadratic functions again make a smooth transition across 138 percent of the FPL. Based on these figures, I use income to define the sample instead of using it to identify the effects.

Using income to select the sample may be still problematic in that labor supply and income are endogenously determined. I check the robustness of the estimates with an alternative sample with education level of high school or less because education levels were predetermined for most adults in the 26 to 64 age group before the eligibility expansions. This approach also has disadvantages. Although education is positively correlated with income, it does not completely determine income. Using education to restrict the sample, therefore, not only includes ineligible groups (e.g., middle- or high-income childless adults with high school or less) but also excludes eligible groups (e.g., low-income childless adults with education more than high school). As a result, the estimated effects could be inaccurate. Nevertheless, comparing estimates between different sample restrictions helps demonstrate the robustness of estimates.

5. Results

5.1 Descriptive Statistics

Table 2 provides the descriptive statistics of demographics and health insurance status for nondisabled childless adults aged 26 to 64 with household incomes below 300 percent of the FPL. The first column provides the descriptive statistics for the full sample. The second and third columns show descriptive statistics for the self-employed and wage earners. The remaining two columns show descriptive statistics by the status of expansion in the pre-expansion period.

The upper panel provides demographic characteristics, including age, gender, race/ethnicity, marital status, foreign-born status, veteran status, and education. Although my sample is restricted to low-income households, the characteristics of the self-employed are similar to those found in the self-employment literature.[20] Compared to wage earners, the self-employed are more likely to be older, white, married, and male. They also tend to have higher education levels, but their family income is lower than wage earners' by 4,100 dollars on average. In the pre-expansion period, demographic characteristics are on average very similar between expansion and non-expansion states, except for race/ethnicity, foreign born, and never married. While the proportion of Hispanics is very similar, the expansion states include more non-Hispanic whites and Asians while the non-expansion states include more non-Hispanic blacks. The expansion states also have a larger proportion of foreign-born or never-married individuals. The state heterogeneity in demographic characteristics may drive self-employment outcomes differently in expansion and non-expansion states. I therefore control all the demographic characteristics in the empirical specifications.

The lower panel shows descriptive statistics on health insurance coverage. The uninsured rate is high among low-income childless adults, who make up approximately 40 percent of the sample. About half of the sample is covered by private insurance, among which ESI coverage is still the main source of insurance coverage (37 percent).[21] By comparison, the uninsured rate is much higher among the self-employed (49 percent) than wage earners (37 percent). Notably, the coverage rate of ESI is significantly lower among the self-employed than wage earners, and the majority of the ESI coverage is explained by spousal ESI. Non-group insurance is the main source of insurance coverage for the self-employed. Nearly 25 percent of the self-employed are covered by it. The coverage by Medicaid is the same between the self-employed and wage earners. Together, the comparison suggests that the self-employed may experience limited access to private health insurance. Compared to non-expansion states, expansion states tend to have lower rates of uninsured and higher rates of Medicaid coverage, which suggests that expansion states may have had more generous public insurance programs before the Medicaid expansion. The rate of private insurance coverage is very similar between expansion and non-expansion states. Since the Medicaid expansions affect health insurance outcomes, I do not adjust health insurance status

[20] See Parker (2009) for a literature review of the determinants of self-employment.

[21] The coverage rate of the ESI for low-income childless adults aged 26-64 (37 percent) is lower than that for all individuals aged 26-64 (65 percent), but it should be emphasized that low-income childless adults include the larger proportion of non-employed individuals. When the sample is restricted to wage earners, the coverage rate of the ESI for low-income childless adults increases to 48 percent. Furthermore, the rate of the uninsured is large for low-income childless adults (40 percent), which reflects limited access to health insurance for this group. These numbers together suggest that the risk of being uninsured is an issue for the low-income childless adults.

when I estimate the effects of the ACA Medicaid expansion on self-employment outcomes. Instead, I directly estimate the effect of Medicaid coverage on self-employment in the instrumental variable approach.

Table 3 presents the descriptive statistics of self-employment and other employment statuses. The first column provides the descriptive statistics for the full sample. The remaining four columns show descriptive statistics by the expansion status and the pre- and post-expansion periods.

The upper panel includes self-employment levels and flows. About 8 percent of low-income childless adults are self-employed. Most of the self-employed among low-income childless adults take it as a main income source. More than two thirds of the self-employed are full-time, while less than one third is part-time. Among the self-employed, the share of unincorporated self-employment (84 percent) is much higher than that of incorporated self-employment (16 percent). This suggests that unincorporated self-employment is the major type of self-employment for low-income households. The entry rate into self-employment is slightly lower than the exit rate on average. The self-employment entry from wage earners is more than two times larger than the entry from labor entrants.

In the pre-expansion period, self-employment levels and flows are qualitatively similar in expansion and non-expansion states. However, in the post-expansion period, the self-employment level increases to 7.9 percent in expansion states and decreases to 7.2 percent in non-expansion states. The changes in self-employment rate are statistically significantly different between expansion and non-expansion states. Similar changes are found in the full-time, part-time, and unincorporated self-employment rates, but not in incorporated ones. When looking at self-employment flows, the increase in the entry rate is larger in expansion states, whereas the rise of the exit rate is larger in non-expansion states. In sum, these descriptive statistics suggest that the ACA Medicaid expansion might lead to higher self-employment activity.

The lower panel in Table 3 depicts the comparison of labor market characteristics between expansion and non-expansion states in terms of other employment-related variables, including the share of labor force participation, wage earners (in the private sector), and full-time workers as well as the average hours worked. The labor force participation rate is 61 percent, among which about 78 percent are employed as wage earners in the private sector. As with self-employment, both expansion states and non-expansion states have, on average, similar employment characteristics in the pre-expansion period. Although the shares of labor force participation and wage earners slightly increase in the expansion states, magnitudes of changes are insignificantly different in the post-expansion time period. These descriptive statistics are consistent with previous research that found no significant effects of ACA Medicaid expansions on labor supply.[22]

One concern in the descriptive statistics is that the self-employment rate increased in expansion states while it declined in non-expansion states. This may result from different trends between expansion and non-expansion states, which invalidates the common trend assumption for difference-in-differences analyses. In Appendix Figure A2, I check the trends in average self-employment, entry, and exit rates for the low-income childless adult sample. The pre-trends of the average self-employment rates in both expansion and non-expansion states are quite similar from 2003 to 2013. Then, the self-employment rate increased only in expansion states in 2014. These trends suggest that the decrease in the average self-employment rate in non-expansion states in the post-expansion period is not driven by different trends. In addition to a visual inspection of trends, I also conduct event studies to complete a formal statistical test for the common trend assumption, which is explained in detail in later sections.

5.2 Effects on Self-employment Levels

I provide the estimated effects of the ACA Medicaid expansion in Table 4. The first and second columns provide the results of the propensity to become self-employed from the DID and PSW-DID, respectively. The last two columns show the results of the likelihood of being a wage earner. The upper, middle, and lower panels show the results from the samples of childless adults with incomes below 300 (baseline), 138, and 100 percent of the FPL, respectively.

The estimated effect from the baseline DID specification indicates that the self-employment rate in the expansion states statistically significantly increased by 1.1 percentage points, relative to non-expansion states across time. This is an approximately 14 percent increase from the unconditional mean of the self-employment rate (7.6 percent). While the DID estimates are adjusted to individual characteristics, some of which were different between expansion and non-expansion states, controlling for these characteristics may not be enough if they differentially

[22] The different composition of industries and occupations might also lead to differential outcomes. I check this possibility by computing the share of 2-digit industries and 2-digit occupations in Appendix Figure A1. Like other labor market variables, the distribution of industries and occupations was very similar between expansion and non-expansion states.

affect the trends of the propensities of being self-employed. The PSW-DID specification addresses this issue by balancing observable characteristics for treatment and control states before and after adopting the ACA Medicaid expansion. The result from the PSW-DID shows that the Medicaid expansion increased the rate of self-employment by 0.8 percentage points, which is about a 10 percent increase from the unconditional mean. The estimate from the PSW-DID is slightly lower, but it is not statistically different from the DID. These results indicate that the estimate from the DID is not driven by differences of observable covariates in treatment and control states.

Panel B shows the results for the sample below 138 percent of the FPL. The patterns of the estimated effects in the sample below 138 percent of the FPL are similar to the sample below 300 percent of the FPL. In states that adopted the ACA Medicaid expansion, the self-employment rate significantly increased by 1.6 and 1.2 percentage points in DID and PSW-DID, respectively. The different sizes of the estimates may result from the fact that the sample below 138 percent of the FPL includes more relevant population. However, as explained in the methods section, the sample below 138 percent of the FPL may also capture the self-employed who adjusted household incomes to become eligible for Medicaid, which could lead to some overestimation of the effects.

Panel C presents the estimated effects from the sample with income below 100 percent of the FPL. One remaining concern in both the 300 and 138 percent of the FPL groups is that individuals with household income between the 100 and 138 percent of the FPL can purchase non-group health insurance in the exchanges and receive premium tax credits in non-expansion states. This may result in increasing in the self-employment rate in non-expansion states and lead to a downward bias in the previous results. However, if the self-employed adjust their incomes above 100 percent of the FPL, it can also lower the estimates from the sample with incomes below 100 percent of the FPL. The results in Panel C show that the estimated effects slightly increase, but the magnitudes of the estimates are quite similar between 100 and 138 percent of the FPL.

Overall, the analysis of self-employment levels suggests that the ACA Medicaid expansion raised the self-employment rate among childless adults in low-income households. The magnitudes of ITT estimates are consistent with previous studies on Medicare (Fairlie, Kapur, and Gates 2011) and the SCHIP (Olds 2016). Also, findings on the employment rate are consistent with recent empirical studies that found the ACA Medicaid expansion did not have significant effects on labor supply (Duggan, Goda, and Jackson 2019; Gooptu et al. 2016; Kaestner et al. 2017; Leung and Mas 2016). The results also suggest that potential biases are of less concern in the estimated effects. I further examine these issues in the upcoming section on the instrumental variable approach.

Alternative explanations for any self-employment effects may be demand shock or different trends of economic recovery. A traditional Medicaid program is jointly funded by the federal and state governments, but under the ACA Medicaid expansion, the federal government provides funding to cover 100 percent of the costs for individuals newly enrolled in Medicaid in the expansion states. This federal funding may raise consumption demand and create more business opportunities in expansion states than in non-expansion states. The time period in this study also coincides with the financial crisis of 2007–2008 and subsequent economic recovery from it. Demand shock or business cycles may contribute to the labor market outcomes (including self-employment) between expansion and non-expansion states. In other words, if higher demand or better economic conditions in expansion states have raised the self-employment rate, they would have also increased the share of wage earners because incumbents would also take advantage of these opportunities.

I estimate the effects on the share of wage earners in columns (3) and (4) of Table 4. To address an issue of different business cycles between expansion and non-expansion states, all specifications control for state-level unemployment rates. If the expansion states experience exogeneous demand shock or face much faster economic recovery than the non-expansion states, it is expected there will be an increase in not just self-employment but also wage earners. However, all the estimates on the share of wage earners from both DID and PSW-DID with different samples (FPL>300% or FPL>400%) are close to zero and statistically insignificant across different samples based on income thresholds. These findings suggest that the influence from demand shock or the business cycle are of less concern.

Since the Medicaid expansion is one of many components in the ACA, other changes related to access to health insurance in expansion states may drive estimated effects. In Table 5, I conduct falsification tests with high-income childless adults (above 300 or 400 percent of the FPL) not affected by the Medicaid expansion. The results show that the Medicaid eligibility expansion did not make noticeable changes in self-employment outcomes among high-income people. The magnitudes of estimates are close to zero and statistically insignificant. I conclude that there is no evidence to ascertain that changes in self-employment in low-income individuals are not driven by other influential changes in expansion states.

The key identifying assumption for all my specifications is that the self-employment rate (or other self-employment outcomes) in expansion states would evolve as in non-expansion states in the absence of the eligibility increase in Medicaid under the ACA. In order to check the validity of this assumption, I conduct an event study and plot the coefficients of the interaction terms between the treatment indicator and year dummies in equation (2) with a 95 percent confidence interval in Figure 6. Each coefficient is estimated relative to 2013, a year before the implementation of the Medicaid expansion. The event study estimates of self-employment levels before 2014 are close to zero and statistically insignificant at 95 percent of the confidence interval. There are no specific upward or downward pre-trends. The estimate significantly jumps in 2014 and stays in 2015. Although the estimate drops in 2016, it recovers to over 0.1 in 2017. Together, these results suggest the plausibility of the parallel trend assumption and support the validity of my baseline estimates.

5.3 Analysis of the Mechanism: Access to Spousal Coverage

Although the previous analysis provides supportive evidence on the self-employment effects of the Medicaid expansion, it does not answer whether the mechanism of these effects is to improve access to health insurance for the self-employed, therefore reducing the likelihood of being uninsured when becoming self-employed. Particularly, the insignificant reduction in the share of wage earners can be somewhat puzzling. If the expansion of Medicaid eligibility reduces entry barrier and many workers may leave their jobs to start businesses, the proportion of wage earners is expected to decrease.

To understand the mechanism of the effects, I estimate a DID specification by permitting the DID estimator to vary with spousal ESI. The sample is restricted to married childless adults. Table 6 provides the results by specifications with different demand factors. The estimate of the DID (β_1) shows the estimated impact on married couples with access to spousal ESI. The estimate of the triple interaction (β_2) provides the additional effect of no access to alternative health insurance. The linear combination of these two estimates ($\beta_1 + \beta_2$) indicates the effects on married couples without access to spousal coverage.

In column (1), the Medicaid expansion insignificantly affects those with spousal coverage, but it has a significant additional effect on those without access to spousal ESI. In other words, relative to those with spousal coverage, childless adults without access to alternative health insurance experience a 2.1 percentage point increase in the propensity of being self-employed in expansion states after states adopted the ACA Medicaid expansion. The effect on married childless adults without access to spousal coverage ($\beta_1 + \beta_2$) is similar to the additional effect (β_2), and the effect is statistically significant. These results suggest that the main effect of the Medicaid expansion is driven by the group with limited access to health insurance, and its mechanism is reducing the probability of being uninsured for the self-employed.

The estimates on the share of wage earners in column (2) show that the Medicaid expansion insignificantly increases in the share of wage earners among those with spousal coverage. However, relative to those with spousal coverage, childless adults without access to alternative health insurance experience a 2.9 percentage point decrease in the propensity of being wage earners in expansion states after states adopted the ACA Medicaid expansion. This result is consistent with the hypothesis that latent entrepreneurs may not leave their jobs because of limited access to health insurance. The test of $\beta_1 + \beta_2$ is still negative, but it becomes statistically insignificant. However, it should be noted that the absolute magnitude of $\beta_1 + \beta_2$ is similar to that of β_1. These results suggest that the decrease in the share of wage earners due to transitions into self-employment may be counterbalanced with the increase in the share of wage earners from labor entrants, which could be the reason why I do not find a significant reduction in the share of wage earners in the baseline estimates.

5.4 Effects on Compliers

Since not all eligible individuals enroll in Medicaid, the baseline estimates capture the ITT effects of being eligible for Medicaid through the states' adoption of the ACA Medicaid expansion. Previous studies found that the estimated effects of the Medicaid expansion on Medicaid coverage range from 2 to 15 percentage points (Couremanche et al. 2017; Duggan, Goda, and Jackson 2019; Frean, Gruber, and Sommers 2017; Kaestner et al. 2017; Leung and Mas 2016; Simon, Soni, and Cawley 2017; Wherry and Miller 2016). Even in the randomized control study of the Oregon health insurance experiment, about 30 percent of eligible people enrolled in Medicaid (Baicker et al. 2013; Finkelstein et al. 2012). The IV approach estimates the LATE of being covered by Medicaid ("compliers").

Table 7 provides estimates from OLS, IV, and PSW-IV. The simple OLS estimate shows that Medicaid coverage is negatively associated with the propensity of being self-employed.

However, this OLS estimate suffers from selection bias. If the self-employed are more ambitious individuals who start their businesses regardless of health insurance, then they are less likely to take up Medicaid. Conversely, the IV estimate captures effects for compliers who do not start their businesses only because of limited access to health insurance. Therefore, these latent entrepreneurs are very sensitive to obtaining health insurance. The IV estimate indicates that being covered by Medicaid due to increased Medicaid eligibility led to an 11 percentage point higher propensity of being self-employed. The PSW-IV estimate is slightly smaller (8 percentage points), but it is still positive and statistically significant. Both IV and PSW-IV estimates suggest that the ACA Medicaid expansion leads to a significant increase in both the propensities of taking up Medicaid and becoming self-employed for latent entrepreneurs.

It is also interesting to see that unlike DID estimates, the size of the IV estimates is similar regardless of sample changes based on different income cutoffs (FPL<300%, FPL<138%, and FPL<100%), which reflects the fact that the IV approach captures effects on compliers across different samples. The main concern in the IV estimates based on 300 or 138 percent of the FPL is that non-expansion states provide subsidies to those with income between 100 and 138 percent of the FPL. If a subsidy significantly improve access to health insurance, those who are eligible for it may choose to become self-employed, which violates the exclusion restriction assumption. However, the magnitude of the IV estimate from the sample based on income below 100 percent of the FPL is qualitatively the same as the estimates from other samples. Considering that the sample based on income below 100 percent of the FPL excludes those eligible for subsidies in non-expansion states, these results justify the assumption of the exclusion restriction, and the IV estimates correctly capture complier effects. Similarly, these results also suggest that the ITT estimates in the sample below 138 or 100 percent of the FPL are less likely to suffer from income adjustment.

Looking at the employment rate, the OLS estimate shows a much larger negative relationship. However, the IV and PSW-IV indicate that being newly covered by Medicaid under the expansion does not have significant effects on the probability of being a wage earner across the sample based on different income thresholds. These estimates are consistent with the ITT estimates, which suggest that the Medicaid expansion does not have significant effects on employment. The F-statistics for the first-stage estimates are over 50, which suggests that the indicator for the ACA Medicaid expansion is not a weak instrument.

It requires some caution to interpret the IV results. Considering that the Medicaid expansion is not randomly assigned across states, I interpret IV estimates as effects on compliers rather than causality. The compliers are the latent entrepreneurs who become self-employed only because of new access to health insurance through the Medicaid expansion. Therefore, these compliers are very sensitive to the new Medicaid coverage. The first stage estimate on Medicaid shows that the eligibility expansion raised the rate of Medicaid coverage by about 10 percentage points. Since I include longer post-expansion periods, the size of this estimate is slightly larger than previous studies on the ACA Medicaid expansion (2 to 7 percentage points). Nevertheless, the Medicaid take-up rate is relatively small given the large number of eligible childless adults. The low take-up rate makes the size of the LATE about 10 times larger than the ITT effects. Considering the unconditional mean of the self-employment rate, the IV estimates suggest that those newly covered by Medicaid under the ACA have a double or higher likelihood of becoming self-employed.

5.5 Effects on Self-employment Transitions

To examine whether there is an income effect, I separately examine self-employment entry and exit. I conduct the same DID and PSW-DID analysis with self-employment transition variables. Here the sample is restricted to only those with income below 300 percent of the FPL. This is because the longitudinal sample is already less than 40 percent of the previous sample and the self-employment transition has relatively fewer events. The decompositions of self-employment entry or exit make the cases much smaller, especially when transition events are divided by expansion status and pre- and post-time periods, thus resulting in inaccurate estimates with larger standard errors when the sample is further restricted using 138 or 100 percent of the FPL.

Table 8 provides the DID and PSW-DID estimates on self-employment entry. The first two columns show the self-employment entry, and the next four columns presents decompositions, including the transitions to self-employment from wage earners or the non-employed (or labor entrants). The estimates indicate that the self-employment entry rate increased by about 10 percentage points in expansion states after the Medicaid expansion. The increase in the self-employment rate was statistically significant, and its magnitude is similar to the estimates on the self-employment level.

Self-employment entry is further decomposed into entry from wage earners and the non-employed. The DID estimates show that about half of the self-employment entry comes from wage earners and the other half from the non-employed. Both estimates are statistically significant at the 10 percent significance level. In the PSW-DID estimates, the magnitudes slightly decrease and the entry from wage earners becomes insignificant, but the effect sizes are the same between entry from wage earners and the non-employed. The results suggest that, as expected, access to health insurance is an important dimension for not only wage earners but also labor entrants. Considering that wage earners face higher transition costs than labor entrants, similar effect sizes imply a large portion of latent entrepreneurs among wage earners.

Table 9 presents the estimated effects on self-employment exit. In contrast to entry, the Medicaid expansion does not have a large effect on self-employment exit. Both DID and PSW-DID estimates are close to zero and statistically insignificant. Since self-employment exit includes both transitions to wage earners and non-employed status, it may raise a concern that the estimates do not capture negative income effects. To investigate whether the Medicaid expansion increases the share of individuals exiting from the labor market, the effects on self-employment exit are further decomposed into the exit to wage earners and the non-employed in columns (3)-(6). All the decomposed effects are also close to zero and statistically insignificant. The negative income effects are negligible.

The key identifying assumption for all my specifications is that self-employment entry or exit in expansion states would evolve similar to non-expansion states in the absence of the Medicaid expansion. In order to check the validity of this assumption, I apply event study approaches to self-employment entry and exit. In Figure 7, the coefficients of the interaction terms between the treatment indicator and year dummies are provided with 95 percent confidence intervals. Each coefficient is estimated relative to 2013, a year before the implementation of the Medicaid expansion. The estimates of self-employment entry and exit before 2014 are close to zero and statistically insignificant at 95 percent of the confidence interval, except for self-employment exit in 2003. There are no specific upward or downward pre-trends. These results suggest the plausibility of the parallel trend assumption and support the validity of my baseline estimates.

Entry rate significantly increases in 2014 and 2015, decreases in 2016, and becomes close zero in 2017. This is an expected pattern. The new eligibility for Medicaid incentivizes latent entrepreneurs to start new businesses by reducing the likelihood of being uninsured, but the effect may not be instantaneous because it may take time to operationalize business ideas. After all latent entrepreneurs respond to the Medicaid expansion, the self-employment entry rate returns to the previous rate so that the difference between expansion and non-expansion states disappear. Conversely, the increased self-employment level remains unless new self-employed individuals have higher failure rates.

Analyzing self-employment transitions also allow to examine the channel in more detail. Restricting the sample to the married individuals, I estimate the DID specification with an interaction between the DID estimator and no access to spousal ESI. Table 10 provides the DID estimator (β_1), the triple interaction (β_2), and the test for the linear combination of these two estimates ($\beta_1 + \beta_2$). The first three columns show the results on self-employment entry, while the last three columns present the results on self-employment exit.

Column (1) shows that the Medicaid expansion insignificantly increases the self-employment entry rate among the married childless adults with spousal coverage. However, the effects on those without spousal coverage ($\beta_1 + \beta_2$) is larger and statistically significant. That is, the self-employment entry rate increases about 1.7 percentage point among married childless adults without alternative insurance after the ACA Medicaid expansion. When this effect is decomposed into entry from wage earners (column 2) and non-employed (column 3), the transition from wage earners into the self-employed significantly increases by approximately 1.3 percentage point, which about 73 percent of the increase in self-employment entry. The transition from the non-employed to the self-employed insignificantly increases by less than 0.5 percentage point. Looking at the self-employment exit, the changes were very small and insignificant.

Overall, the ACA Medicaid expansion raised self-employment entry but not self-employment exit. The negative income effects were not large enough to make the self-employed stop working. These results of self-employment transitions suggest that changes in the self-employment rate is mainly driven by new self-employment entrants, and negative income effects are of less concern.

5.6 Business Characteristics

Who are the newly self-employed individuals? Considering that Medicaid beneficiaries are low-income households, business characteristics may be different from middle- or high-income groups. One may be concerned that the Medicaid expansion may bring bad entrepreneurs to the market. Although challenging without detailed information on businesses, I check the quality of businesses using the limited information in the CPS ASEC.

Table 11 provides some estimates on business characteristic-related variables. The DID specification is applied to all business characteristics. In the upper panel, the sample is restricted to those with incomes above 300 percent of the FPL. The results show that the ACA Medicaid expansion increased self-employment with positive business incomes by 1 percentage point. Given the baseline DID estimate on self-employment (1.1 percentage points), more than 90 percent of newly self-employed people make positive incomes. To capture different types of self-employment activities, I examine full-time versus part-time as well as incorporated versus unincorporated self-employment. The results indicate that both full-time and part-time self-employment in expansion states increase after Medicaid expansion. The increase is about 0.6 percentage points for both full- and part-time self-employment, and both estimates are statistically significant. This result suggests that about half of the new self-employed individuals are serious and committed to their business activities.

The results on incorporated versus unincorporated self-employment show that the Medicaid expansion does not significantly change the rate of incorporated self-employment, whereas it significantly raises the rate of unincorporated self-employment by 1.2 percentage points. In general, incorporated self-employment tends to be more successful than unincorporated self-employment. However, since entrepreneurs tend to incorporate their businesses after they have grown, the rate of incorporation is low among newly self-employed individuals (e.g., Evans and Jovanovic 1989). In addition, unincorporated self-employment is the major form of business among low-income households. Larger effects on unincorporated self-employment are consistent with my expectation that Medicaid expansion increases the number of newly self-employed individuals among low-income households. From this perspective, I do not interpret this result as an indication of bad entrepreneurs.

In the lower panel, the sample is further restricted to the self-employed. In columns (6), (7), and (8), the results provide the estimates on business income (intensive margin), the rate of uninsured, and the health status of the self-employed. The estimates indicate that the Medicaid expansion increases business income by approximately 7 percent, but it is statistically insignificant. The expansion significantly reduces the uninsured rate by about 6 percentage points (13 percent from the unconditional mean), whereas it does not change the health status of the self-employed. It may require more time to see positive health outcomes from access to health insurance.

The previous findings on self-employment exit show no significant negative income effects on the extensive margin of self-employment supply, but there might be adjustment in the intensive margin of incumbent self-employment. However, the results in columns (9) and (10) show a statistically insignificant increase in weeks worked and a statistically insignificant decline in hours worked among the self-employed. The results suggest that adjustment in the intensive margin of the incumbent self-employed reducing hours worked is of less concern. Together, I find no evidence that the Medicaid expansion incentivizes bad entrepreneurs to start businesses.

5.7 Robustness Check

I conduct several robustness checks of the main results with different models, thresholds, and samples. I provide the results of the robustness checks in Appendix C.

First, I estimate the DID and PSW-DID specifications with Logit and Probit models to check whether the estimates are robust to nonlinear models. The results are provided in Table C1. To compare with the linear probability model (LPM), I compute the average marginal effects for both nonlinear models. The overall patterns and magnitudes of the estimated effects from Logit and Probit models are very similar to the LPM estimates in previous tables. The effects on self-employment level and entry are positive and statistically significant, whereas the effects on self-employment exit are close to zero and statistically insignificant. These results show that my estimates are robust to different linear and nonlinear models.

Second, I estimate the DID and PSW-DID specifications using the sample with education level of high school or less in Table C2. Kaestner et al. (2017) suggested that the robustness of a sample would increase by defining a sample comprising individuals with low education levels rather than income levels because education levels tended to be determined before the ACA Medicaid expansion and individuals with education level of high school or less are more likely to be covered by Medicaid. Conversely, Simon et al. (2017) claimed that low education is not a strong predictor for low income. Selecting samples based on education levels also includes high-income people with low education levels, which may result in noise in the estimates. My results for the sample with low education individuals show slightly smaller estimates than the sample below 300 percent of the FPL. Also, standard errors in the low-education childless adult sample become slightly larger. As Simon et al. (2017) suggested, this could be because selecting samples with low education levels leads to the inclusion of ineligible populations. Nevertheless, the patterns of estimates are similar in that they show significant and positive increases in self-employment level and entry but insignificant change in self-employment exit. These results suggest that my estimates are robust to a sample based on education.

Third, I control for different health insurance market conditions in Table C3. The DID and PSW-DID specifications include state fixed effects that control time-invariant unobserved state heterogeneities. However, they do not address time-varying state heterogeneities, especially insurance market conditions. I use individual and small group market Herfindahl-Hirschman Indexes (HHI) as proxies to control time-varying insurance market conditions. For example, some states with few insurance companies having high market shares may have a limited number of insurance options and higher insurance premiums on average. Since the HHI indexes from the Kaiser Family Foundation are available for only 2011–2016, the study time period is further restricted. In most specifications, both the individual and small group market HHI are negatively associated with self-employment outcomes. That is, states with low insurance market competition tend to have lower self-employment outcomes, which is consistent with the entrepreneurship lock hypothesis. Even after controlling for insurance market conditions, though, the estimated effects are similar to previous results. These results imply that state fixed effects already control most state heterogeneities and that my estimates are robust to remaining heterogeneities.

Fourth, I check the robustness of my estimates using different treatment and control groups, as shown in Table C4. In Panel A, I estimate the effects by excluding late expansion states since these states may have had different economic or political reasons to adopt the ACA Medicaid expansion. This exclusion slightly increases my estimates, but they are qualitatively the same. In Panel B, I include Wisconsin in the control group. Wisconsin increased the state's own eligibility threshold for Medicaid up to 100 percent of the FPL for all adults, but it did not adopt the ACA Medicaid expansion. In Panel C, I include prior expansion states in the treatment group. Most prior expansion states partially increased their eligibility before 2014 and additionally increased it to 138 percent of the FPL in 2014. In Panel D, I include both Wisconsin and prior expansion states. The results show that the baseline estimates are robust even after including Wisconsin and/or late expansion states.

Finally, I apply a synthetic control group method (SCGM) as an alternative approach. Like DID and PSW-DID, the SCGM estimates the ITT effects.[23] Table C5 provides the estimated effects of the Medicaid expansion on self-employment levels and transitions. Since the permutation test is used for SCGM, the p-value of the root mean square prediction error (RMSPE) ratio for the expansion states is reported in brackets. The results of the SCGM indicate that the self-employment rate statistically significantly increased in treatment states after the Medicaid expansion. The magnitude of the estimate is 0.95 percentage point, which is similar to the DID and PSW-DID estimates. The SCGM estimate on self-employment entry is positive and two times larger than the estimate on self-employment exit, but it is statistically insignificant. Similar to the event study, the difference of self-employment outcomes between the treatment and synthetic control groups for each year is provided with placebo estimates for using each non-expansion state as a treatment group in Figure C1. The pre-trends for all self-employment outcomes are fairly close to zero. When looking at the optimal weights to create a synthetic control group in Table C6, both the included states and weights are somewhat different across all self-employment outcomes. In general, the SCGM seems slightly more sensitive in estimating a small incidence of self-employment transitions. This may be because the SCGM uses aggregated information at the state and year levels, which reduces the sample size.

6. Discussion and Conclusion

In this study, I examine the effects of the ACA Medicaid expansion on the supply of entrepreneurs measured by self-employment decisions. With both a pooled cross-section and a linked two-year panel of the CPS ASEC, I apply multiple credible identification strategies—including difference-in-differences, propensity score weighting, event study, and instrumental variable approaches. I find evidence that the self-employment rate significantly increased in the states that adopted the ACA Medicaid expansion. The effect of the ACA Medicaid expansion is much larger for those without alternative access to health insurance (e.g., spousal ESI coverage). The childless adults who are newly covered by Medicaid due to the expansion have a much higher propensity to become self-employed. Finally, I find that the increase in self-employment is driven by self-employment entry and there is little evidence of the increase in the rate of self-employment

[23] The detailed description of the SCGM is provided in Appendix B.

exits, which imply that substitution effects dominated income effects. These findings together suggest that limited access to health insurance may be a barrier to entrepreneurship.

This study provides some policy implications for health insurance and entrepreneurs. My findings are consistent with a hypothesis that the tight connection between ESI and the workplace may distort the supply of entrepreneurs (Fairlie, Kapur, and Gates 2011; Olds 2016). However, the estimated effects of the Medicaid expansion in this study differ from studies that found limited evidence of the entrepreneurship effects in other aspects of the ACA, such as the dependent coverage mandate (Bailey 2017) or health insurance marketplace (Heim and Yang 2017).

The different findings may be explained by differences in the target populations and the sizes of policies. First, the Medicaid expansion has a much larger target population with a higher demand for health insurance compared to the dependent coverage mandate for young adults below age 26. Because young adults have better health conditions and lower demand for health insurance than older ones, they are less likely to face entrepreneurship lock. Second, the effect of the ACA Medicaid expansion may be larger than the health insurance exchange because it directly provides free health insurance. The main beneficiaries in the health insurance marketplace are middle-income groups, and the amount of subsidy decrease as income increases. Self-employed individuals already receive tax deductions of 100 percent for their health insurance under the 1986 Tax Reform Act. Thus, the size of the exchanges may not be large enough to allow individuals to become self-employed. My findings suggest that focusing on populations with higher demands for health insurance and providing direct access to health insurance may be important in designing health insurance policies for entrepreneurs.

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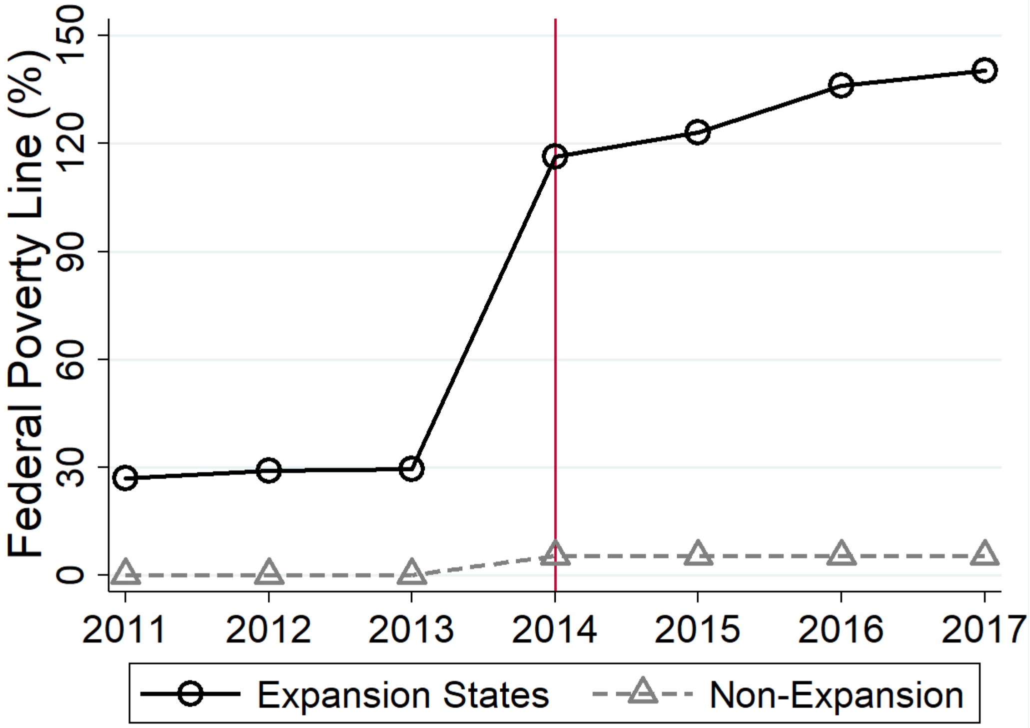
Table 1. Status of States by ACA Medicaid Expansion and Prior Expansion on Childless Adults

No ACA Medicaid Expansion	ACA Medicaid Expansion	
	Prior Expansion	No Prior Expansion
Alabama, Florida, Georgia, Idaho, Kansas, Mississippi, Missouri, Nebraska, North Carolina, Oklahoma, South Carolina, South Dakota, Tennessee, Texas, Utah, Virginia, Wisconsin[†], Wyoming	Arizona[ǂ], Colorado, Connecticut, Delaware, District of Columbia[⁋], Hawaii, Minnesota, New York, Vermont[⁋]	Alaska[§], Arkansas[ǂ], California, Illinois, Indiana[§ǂ], Iowa[ǂ], Kentucky, Louisiana[§], Maryland, Massachusetts[*], Michigan[§ǂ], Montana[§ǂ], Nevada, New Hampshire[§ǂ], New Jersey, New Mexico, North Dakota, Ohio, Oregon, Pennsylvania[§], Rhode Island, Washington, West Virginia

Note: [†] Wisconsin did not adopt the ACA Medicaid expansion but increased Medicaid eligibility to childless adults up to 100% FPL in 2014. [⁋] indicates states that made full expansions before 2014. [§] identifies states that adopted the Medicaid expansion after January 1, 2014: Michigan (4/1/2014), New Hampshire (8/15/2014), Pennsylvania (1/1/2015), Indiana (2/1/2015), Alaska (9/1/2015), Montana (1/1/2016), and Louisiana (7/1/2016). [ǂ] specifies states that have approved Section 1115 waivers for the Medicaid expansion: Arizona, Arkansas, Indiana, Iowa, Michigan, Montana, and New Hampshire. [*] Under the MassHealth Medicaid waiver, parents and childless adults up to 133% of the FPL were covered in Massachusetts.

Figure 1. Medicaid Eligibility for Childless Adults by ACA Medicaid Expansion, 2011–2017



Notes: Data is from the Kaiser Family Foundation, "Medicaid Income Eligibility Limits for Other Non-Disabled Adults, 2011–2018." Income eligibility limits for coverage that provides full Medicaid benefits (federal matching funds). Waiver programs or fully state-funded programs are not included. Arizona, Colorado, Connecticut, Delaware, District of Columbia, Hawaii, Minnesota, New York, and Vermont fully or partially adopted the ACA Medicaid expansion between 2011 and 2013. Michigan, New Hampshire, Pennsylvania, Indiana, Alaska, Montana, and Louisiana expanded Medicaid between 2014 and 2016. Wisconsin did not adopt the ACA Medicaid expansion but expanded Medicaid eligibility to childless adults up to 100% FPL.

Figure 2. States That Adopted ACA Medicaid Expansion Between 2014 and 2017

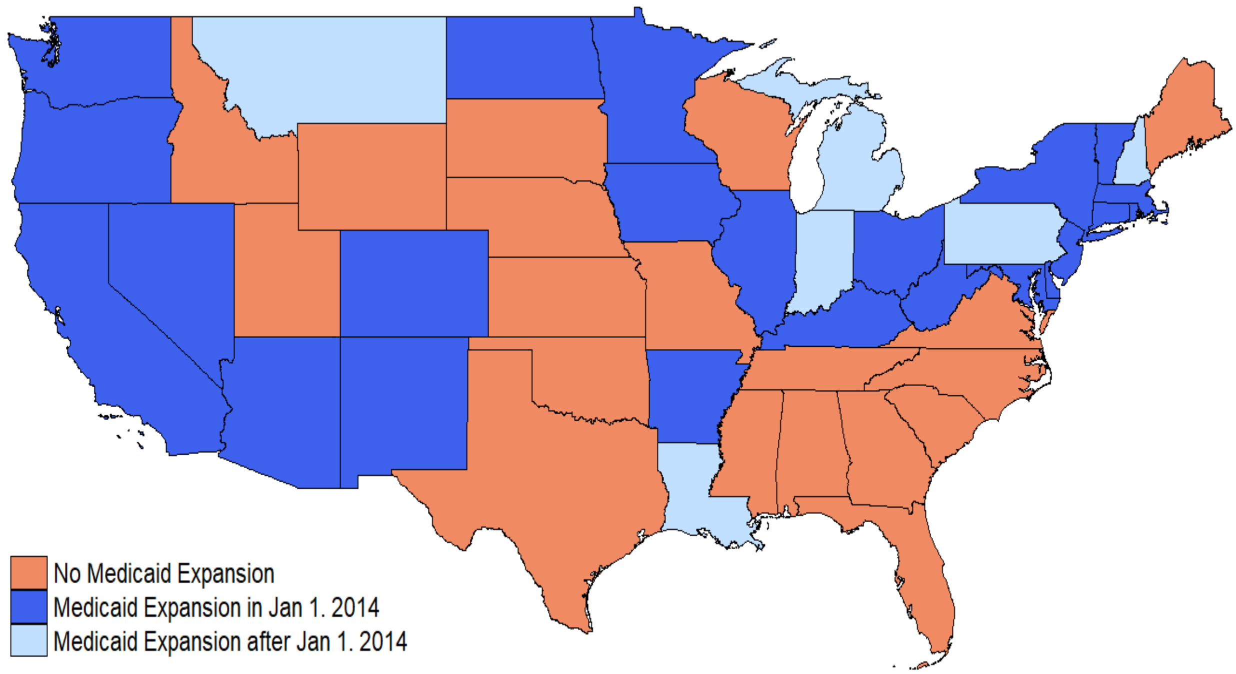
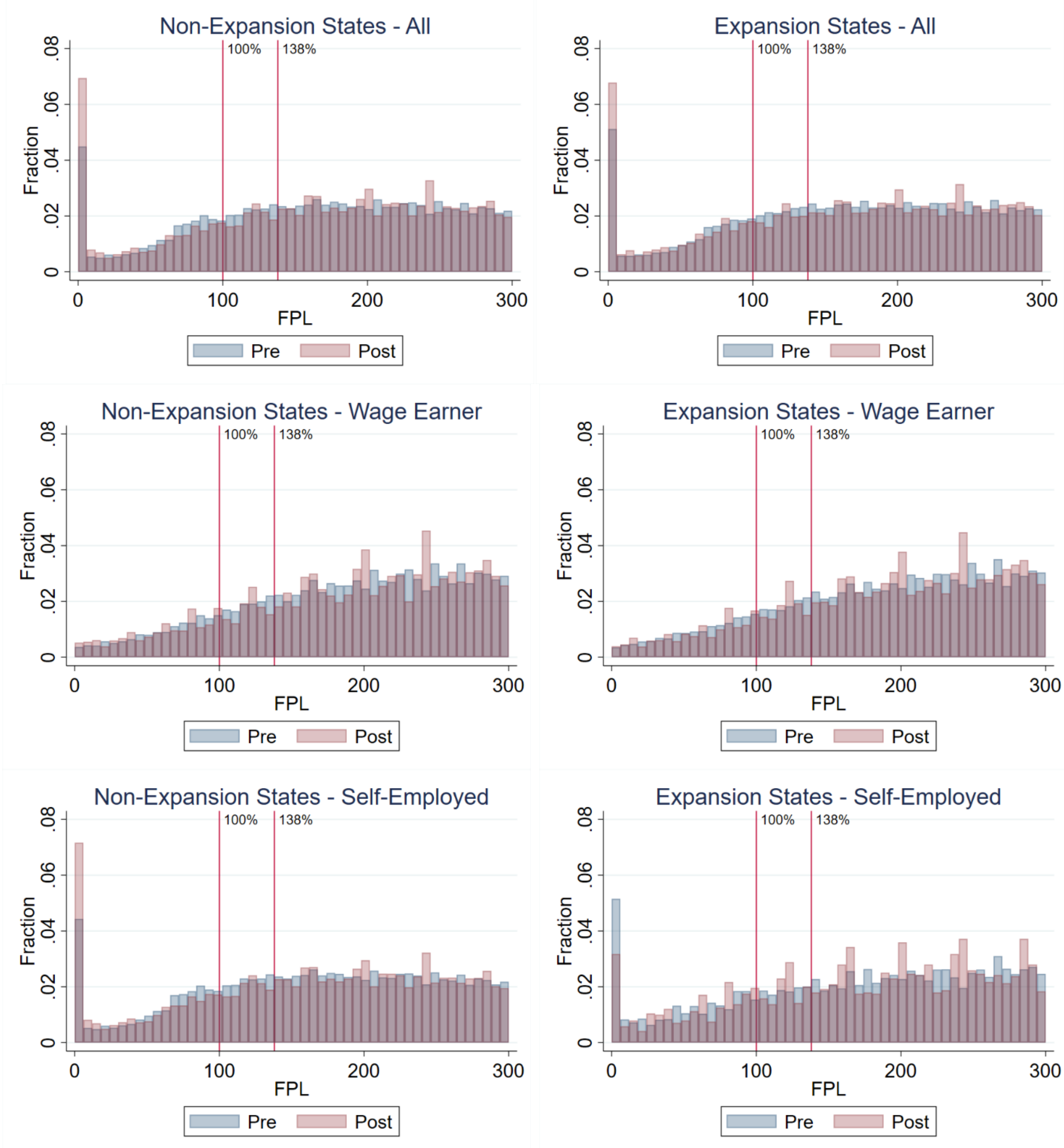
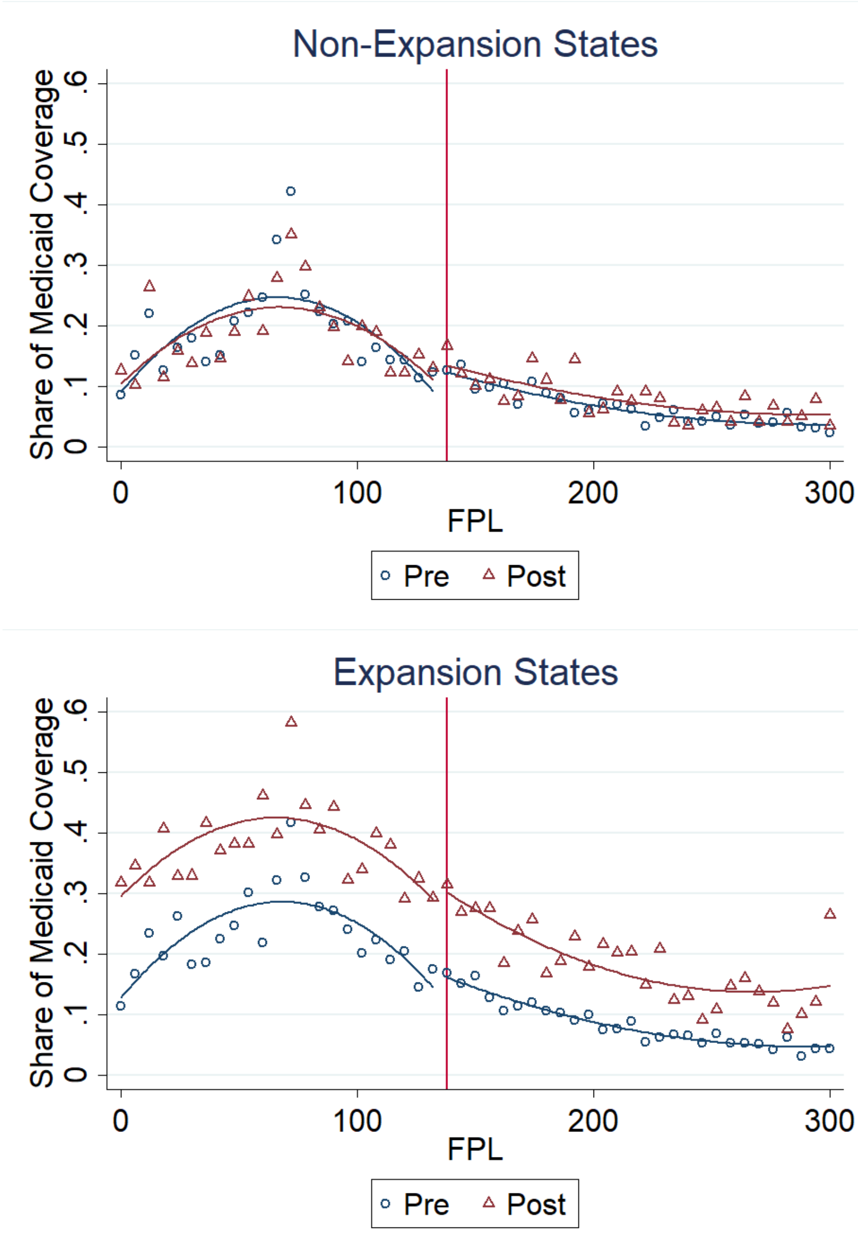


Figure 3. Histograms of Federal Poverty Level (FPL), Low-income Childless Adults Sample



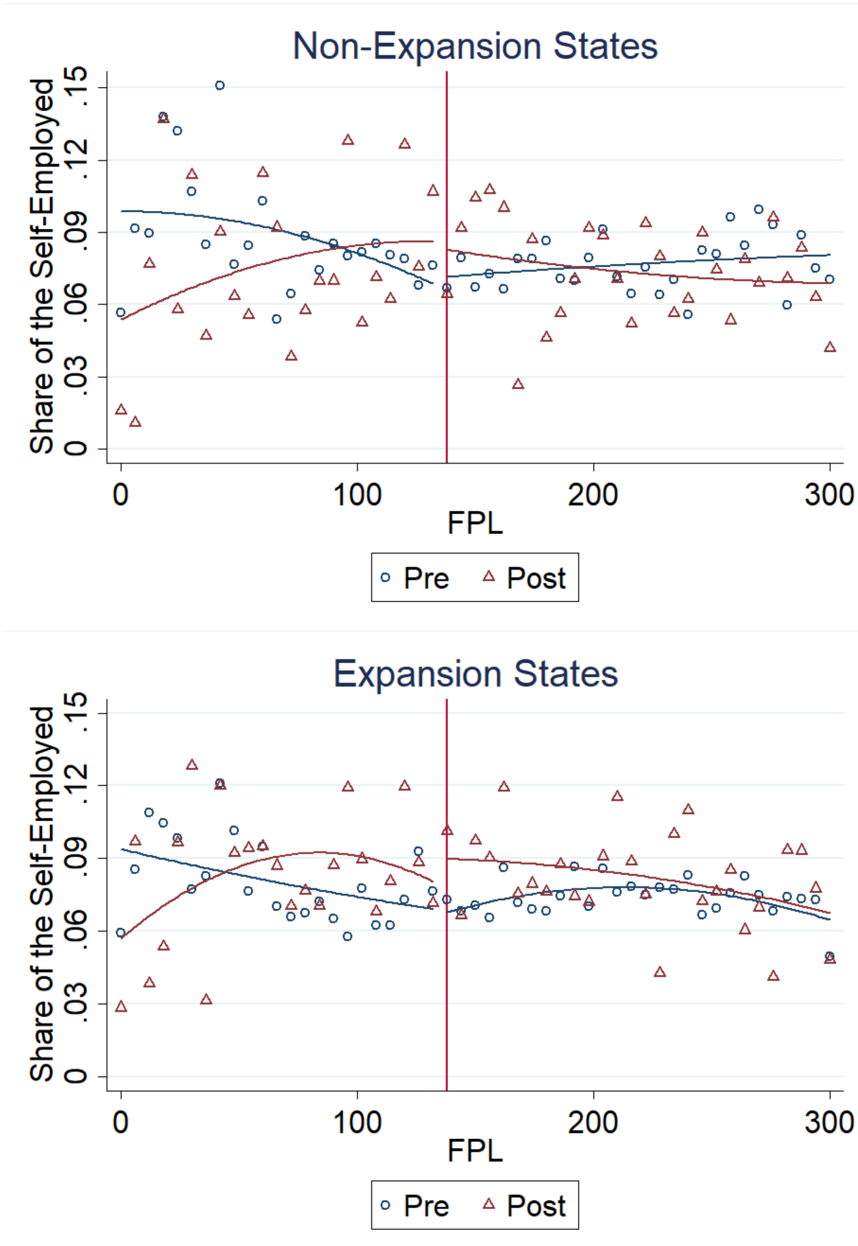
Notes: Sample is restricted to nondisabled childless adults aged 26-64 with income below 300 percent of the FPL.

Figure 4. Medicaid Coverage by Federal Poverty Line (FPL), Low-income Childless Adults Sample



Notes: Sample is restricted to nondisabled childless adults aged 26-64 with income below 300 percent of the FPL.

Figure 5. Share of the Self-employed by Federal Poverty Line (FPL), Low-income Childless Adults Sample



Notes: Sample is restricted to nondisabled childless adults aged 26-64 with income below 300 percent of the FPL.

Figure 6. Event Study Analysis of the Self-employment Level, Low-income Childless Adult Sample

Figure 7. Event Study Analysis, Low-income Sample

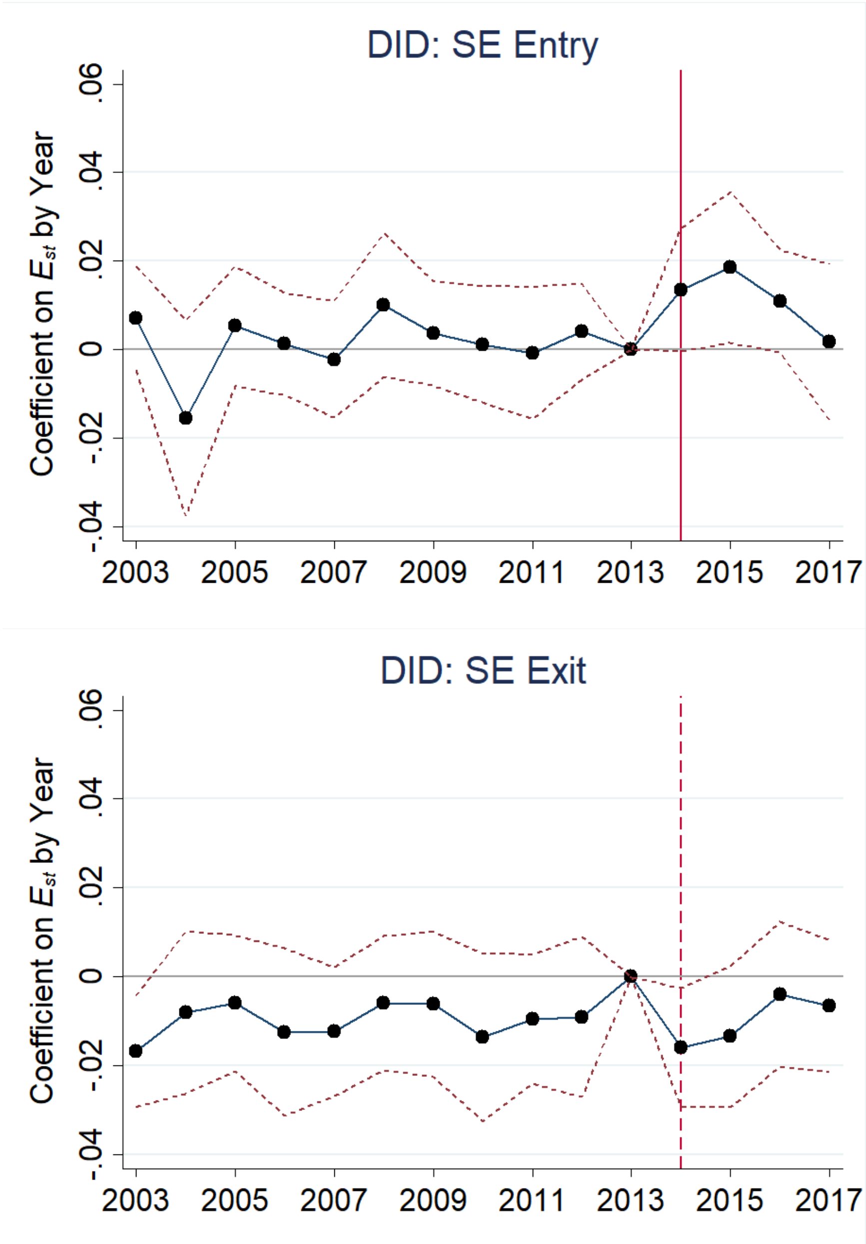


Table 2. Descriptive Statistics of Demographics and Health Insurance, Low-income Childless Adults Sample, CPS ASEC 2003–2016

	(1)	(2)	(3)	(4)	(5)
				Pre-expansion	
	All	Self-employed	Wage Earner	Expansion	No Expansion
Demographics					
Age	47.172	48.530	44.100	47.150	47.206
Female	0.501	0.343	0.470	0.502	0.496
Hispanic	0.131	0.111	0.163	0.123	0.133
Non-Hispanic White	0.683	0.784	0.652	0.716	0.659
Non-Hispanic Black	0.137	0.061	0.136	0.105	0.174
Non-Hispanic Asian	0.032	0.031	0.032	0.040	0.018
Non-Hispanic Other	0.017	0.014	0.016	0.016	0.017
Foreign Born	0.166	0.173	0.194	0.176	0.144
Married	0.367	0.420	0.297	0.357	0.388
Divorced or Separated	0.260	0.274	0.281	0.258	0.273
Widowed	0.051	0.036	0.039	0.050	0.054
Never Married	0.322	0.270	0.383	0.335	0.285
Veteran	0.080	0.084	0.067	0.083	0.092
< High School	0.181	0.132	0.172	0.180	0.208
High School	0.392	0.357	0.403	0.395	0.390
Some College	0.262	0.285	0.277	0.258	0.255
University	0.119	0.165	0.114	0.119	0.109
> University	0.046	0.061	0.035	0.048	0.038
Family Income	24,321	23,877	27,947	23,590	23,560
Insurance					
Uninsured	0.397	0.489	0.372	0.405	0.451
Private Insurance	0.468	0.445	0.562	0.462	0.443
ESI	0.364	0.198	0.483	0.373	0.362
Spouse ESI	0.132	0.141	0.115	0.137	0.128
Non-group Insurance	0.104	0.247	0.079	0.089	0.081
Medicaid	0.135	0.066	0.066	0.132	0.106
Observations	153,931	11,908	72,813	65,596	51,212

Notes: Sample is restricted to nondisabled childless adults aged 26-64 with income below 300 percent of the FPL. The estimates are calculated by using weights provided by the U.S. Census. For transition variables including SE Entry, SE Exit, and SW to SE, the sample is restricted to individuals with labor force information for the last year. The sample size for transition variables in CPS ASEC 2003-2017 is 57,134, which is about 37 percent of the entire sample of CPS ASEC.

Table 3. Descriptive Statistics of Employment and Insurance, Low-income Childless Adults Sample, CPS ASEC 2003–2016

	(1)	(2)	(3)	(4)	(5)
		Pre-expansion		Post-expansion	
	All	Expansion	No Expansion	Expansion	No Expansion
Self-employment					
Self-employed (SE)	0.076	0.075	0.078	0.079	0.072
Full-time SE	0.052	0.051	0.053	0.052	0.050
Part-time SE	0.024	0.024	0.025	0.027	0.022
Incorporated SE	0.012	0.011	0.013	0.012	0.014
Unincorporated SE	0.064	0.064	0.065	0.067	0.058
SE Entry	0.028	0.025	0.029	0.036	0.031
Wage Earner (WE) to SE	0.020	0.017	0.021	0.025	0.023
Non-employed (NE) to SE	0.008	0.008	0.009	0.011	0.007
SE Exit	0.031	0.030	0.030	0.034	0.036
SE to WE	0.017	0.016	0.016	0.021	0.020
SE to NE	0.014	0.013	0.014	0.013	0.016
Other Employment					
Labor Force	0.610	0.599	0.616	0.626	0.621
Wage Earner	0.473	0.469	0.472	0.484	0.483
Full Time	0.474	0.453	0.494	0.473	0.495
Usual Working Hours	22.955	22.256	23.483	23.326	23.668
Observations	153,931	65,596	51,212	20,629	16,494

Notes: Sample is restricted to nondisabled childless adults aged 26-64 with income below 300 percent of the FPL. The estimates are calculated by using weights provided by the U.S. Census. For transition variables including SE Entry, SE Exit, and SW to SE, the sample is restricted to individuals with labor force information for the last year. The sample size for transition variables in CPS ASEC 2002-2016 is 57,134, which is about 37 percent of the entire sample of CPS ASEC.

Table 4. Difference-in-differences Estimates for the Intent-to-treat Effects of Medicaid Expansion on Self-employment, Low-income Childless Adults Sample

	(1)	(2)	(3)	(4)
	Self-employed		Wage Earner	
	DID	PSW-DID	DID	PSW-DID
Panel A: FPL<300%				
Expansion × Post	0.0112***	0.0081***	-0.0016	-0.0036
	(0.0037)	(0.0030)	(0.0059)	(0.0056)
Panel B: FPL<138%				
Expansion × Post	0.0160**	0.0128**	0.0025	0.0023
	(0.0063)	(0.0051)	(0.0095)	(0.0086)
Panel C: FPL<100%				
Expansion × Post	0.0167**	0.0162***	-0.0094	-0.0102
	(0.0076)	(0.0062)	(0.0109)	(0.0096)
Individual Characteristics	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Unemployment Rate	Yes	Yes	Yes	Yes

Notes: Sample is restricted to nondisabled childless adults aged 26-64. Estimates are calculated using CPS ASEC weights. Individual characteristics include age, sex, race, education, marital status, foreign-born status, and citizenship status. Standard errors clustered at the state-year level are provided in parentheses. The numbers of observations for the FPL<300% and FPL<138% full samples are 156,182 and 57,432, respectively.

Table 5. Falsification Test for Self-employment and Wage Earner, High-income Childless Adult Sample

	(1)	(2)	(3)	(4)
	Self-employed		Wage Earner	
	DID	PSW-DID	DID	PSW-DID
Panel A: FPL>300%				
Expansion × Post	-0.0027	-0.0028	-0.0045	-0.0030
	(0.0026)	(0.0024)	(0.0048)	(0.0040)
Panel B: FPL>400%				
Expansion × Post	-0.0033	-0.0014	-0.0056	-0.0062
	(0.0030)	(0.0026)	(0.0055)	(0.0046)
Individual Characteristics	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Unemployment Rate	Yes	Yes	Yes	Yes

Notes: Sample is restricted to nondisabled childless adults aged 26-64. Estimates are calculated using CPS ASEC weights. Individual characteristics include age, sex, race, education, marital status, foreign-born status, and citizenship status. Standard errors clustered at the state-year level are provided in parentheses. The numbers of observations for the FPL>300% and FPL>400% full samples are 288,814 and 227,781, respectively.

Table 6. Difference-in-differences Interacted with No Access to Spousal Coverage, Low-income Married Childless Adult Sample

	(1) Self-employed	(2) Wage Earner
Panel A: 300% FPL		
Expansion × Post (β_1)	0.0017	0.0166
	(0.0092)	(0.0174)
Expansion × Post × NoSESI (β_2)	0.0206**	-0.0287*
	(0.0092)	(0.0163)
Test: $\beta_1 + \beta_2 \neq 0$	0.0223***	-0.0122
[p-value]	[0.0003]	[0.8696]
Controls	Yes	Yes
State FE	Yes	Yes
Year FE	Yes	Yes
Unemp. Rate	Yes	Yes

Notes: Sample is restricted to nondisabled childless adults aged 26-64. Estimates are calculated using CPS ASEC weights. Individual characteristics include age, sex, race, education, marital status, foreign-born status, and citizenship status. Standard errors clustered at the state-year level are provided in parentheses. The number of observations for the FPL<300% married sample is 58,839.

Table 7. Instrumental Variable Estimates for the Local Average Treatment Effects of Medicaid Expansion on Self-employment, Low-income Childless Adult Sample

	(1)	(2)	(3)	(4)	(5)	(6)
	Self-employed			Wage Earner		
	OLS	IV	PSW-IV	OLS	IV	PSW-IV
Panel A: FPL<300%						
Medicaid	-0.0387***	0.1139***	0.0775***	-0.2752***	-0.0053	-0.0295
	(0.0020)	(0.0385)	(0.0287)	(0.0070)	(0.0600)	(0.0522)
1st Stage		0.0987***	0.1064***		0.0987***	0.1064***
		(0.0069)	(0.0065)		(0.0069)	(0.0065)
F-statistics		104.1	134.9		104.1	134.9
Panel B: FPL<138%						
Medicaid	-0.0524***	0.1148**	0.0841**	-0.1767***	0.0156	0.0134
	(0.0029)	(0.0456)	(0.0335)	(0.0065)	(0.0685)	(0.0551)
1st Stage		0.1408***	0.1566***		0.1408***	0.1566***
		(0.0114)	(0.0108)		(0.0114)	(0.0108)
F-statistics		76.96	105.3		76.96	105.3
Panel C: FPL<100%						
Medicaid	-0.0541***	0.1135**	0.1004**	-0.1364***	-0.0669	-0.0642
	(0.0032)	(0.0524)	(0.0404)	(0.0063)	(0.0724)	(0.0589)
1st Stage		0.1499***	0.1626***		0.1499***	0.1626***
		(0.0138)	(0.0127)		(0.0138)	(0.0127)
F-statistics		58.69	82.52		58.69	82.52
Characteristics	Yes	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Unemp. Rate	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Sample is restricted to nondisabled childless adults aged 26-64. Estimates are calculated using CPS ASEC weights. Individual characteristics include age, sex, race, education, marital status, foreign-born status, and citizenship status. Standard errors clustered at the state-year level are provided in parentheses. The numbers of observations for the FPL<300% and FPL<138% full samples are 156,257 and 57,478, respectively.

Table 8. Difference-in-differences Estimates for the Intent-to-treat Effects of Medicaid Expansion on Self-employment Entry, Low-income Childless Adult Sample

	(1)	(2)	(3)	(4)	(5)	(6)
	SE Entry		WE to SE		NE to SE	
	DID	PSW-DID	DID	PSW-DID	DID	PSW-DID
Panel A: FPL<300%						
Expansion × Post	0.0097**	0.0082**	0.0048*	0.0041	0.0049*	0.0041**
	(0.0039)	(0.0035)	(0.0029)	(0.0027)	(0.0025)	(0.0020)
Characteristics	Yes	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Unemp. Rate	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Sample is restricted to nondisabled childless adults aged 26-64. Estimates are calculated using CPS ASEC weights. Individual characteristics include age, sex, race, education, marital status, foreign-born status, and citizenship status. Standard errors clustered at the state-year level are provided in parentheses. The numbers of observations for the FPL<300% and FPL<138% transition samples are 57,134 and 20,079, respectively.

Table 9. Difference-in-differences Estimates for the Intent-to-treat Effects of Medicaid Expansion on Self-employment Exit, Low-income Childless Adult Sample

	(1)	(2)	(3)	(4)	(5)	(6)
	SE Exit		SE to WE		SE to NE	
	DID	PSW-DID	DID	PSW-DID	DID	PSW-DID
Panel A: FPL<300%						
Expansion × Post	-0.0015	0.0008	0.0015	0.0020	-0.0030	-0.0012
	(0.0036)	(0.0034)	(0.0029)	(0.0025)	(0.0023)	(0.0021)
Characteristics	Yes	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Unemp. Rate	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Sample is restricted to nondisabled childless adults aged 26-64. Estimates are calculated using CPS ASEC weights. Individual characteristics include age, sex, race, education, marital status, foreign-born status, and citizenship status. Standard errors clustered at the state-year level are provided in parentheses. The numbers of observations for the FPL<300% and FPL<138% transition samples are 57,134 and 20,079, respectively.

Table 10. Difference-in-differences Estimates for the Intent-to-treat Effects of Medicaid Expansion on Self-employment Transitions, Low-income Childless Adult Sample

	(1)	(2)	(3)	(4)	(5)	(6)
		Entry			Exit	
	SE Entry	WE to SE	NE to SE	SE Exit	SE to WE	SE to NE
Panel A: 300% FPL						
Expansion × Post	0.0052	0.0105	-0.0053	0.0003	0.0021	-0.0018
	(0.0079)	(0.0075)	(0.0037)	(0.0111)	(0.0080)	(0.0076)
Expansion × Post	0.0120	0.0021	0.0100*	0.0038	0.0040	-0.0002
× NoSESI (β_2)	(0.0084)	(0.0078)	(0.0057)	(0.0118)	(0.0083)	(0.0075)
Test: $\beta_1 + \beta_2 \neq 0$	0.0173***	0.0126***	0.0047	0.0041	0.0061	-0.0020
[p-value]	[0.0056]	[0.0051]	[0.2001]	[0.3052]	[0.1439]	[0.6467]
Controls	Yes	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Unemp. Rate	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Sample is restricted to nondisabled childless adults aged 26-64. Estimates are calculated using CPS ASEC weights. Individual characteristics include age, sex, race, education, marital status, foreign-born status, and citizenship status. Standard errors clustered at the state-year level are provided in parentheses. The numbers of observations for the FPL<300% married transition samples is 23,496.

Table 11. Business Characteristics, Low-income Childless Adult Sample

	(1) Biz Income (Positive)	(2) Full-time SE	(3) Part-time SE	(4) Inc. SE	(5) Uninc. SE
Panel A: All					
Expansion × Post	0.0102***	0.0055*	0.0057**	-0.0004	0.0116***
	(0.0033)	(0.0033)	(0.0023)	(0.0016)	(0.0033)
	(6) Log of Biz Income (Amount)	**(7) Uninsured**	**(8) Unhealthy**	**(9) Weeks Worked**	**(10) Hours Worked**
Panel B: Self-Employed					
Expansion × Post	0.0727	-0.0626**	0.0043	0.1574	-0.3954
	(0.1993)	(0.0271)	(0.0147)	(0.6450)	(0.8103)
Characteristics	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Unemployment Rate	Yes	Yes	Yes	Yes	Yes

Notes: Sample is restricted to nondisabled childless adults aged 26-64. Estimates are calculated using CPS ASEC weights. Individual characteristics include age, sex, race, education, marital status, foreign-born status, and citizenship status. Standard errors clustered at the state-year level are provided in parentheses. The numbers of observation for the FPL<300% is 156,257.

Appendix A

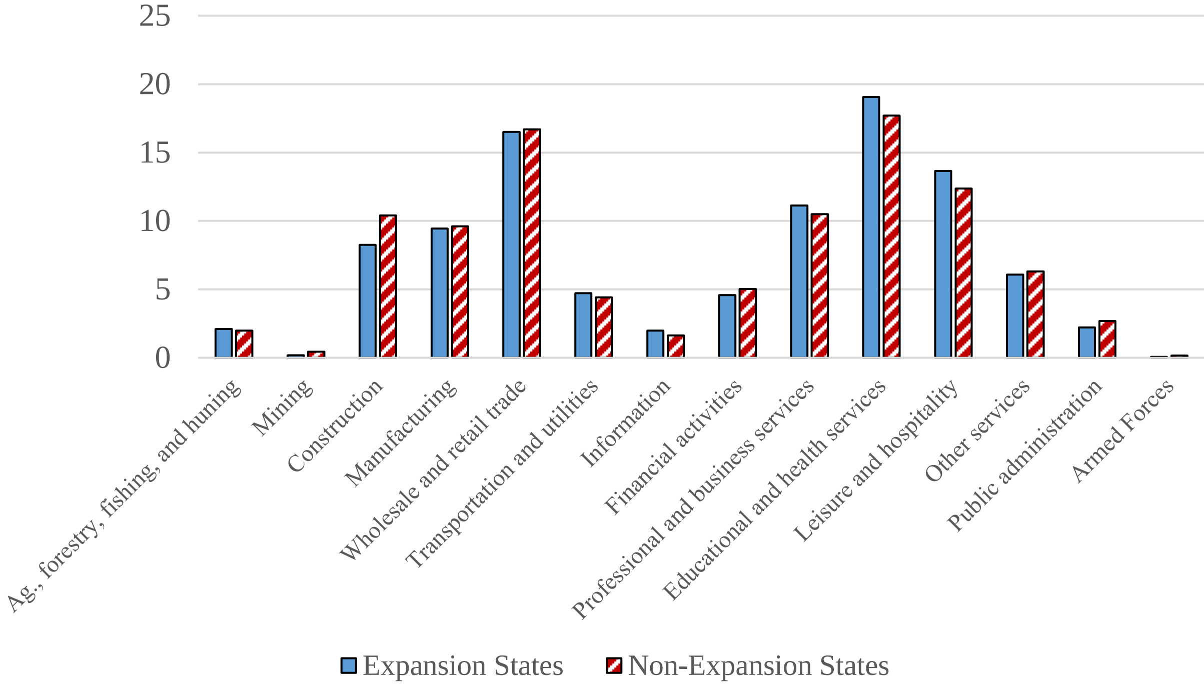
Table A1. Annual Income Thresholds of the ACA Medicaid Eligibility for Childless Adults in 2016

Size of Family Unit	138% of the FPL for Childless Adults (No Related Children under 18 Years)
One person (unrelated individual)	
Under 65 years	16,996
65 years and over	15,669
Two people	
Householder under 65 years	21,877
Householder 65 years and over	19,746
Three people	25,555
Four people	33,697
Five people	40,637
Six people	46,739
Seven people	53,780
Eight people	60,149
Nine people or more	72,353

Notes: These numbers are computed based on the official federal poverty line available in the following link: https://www.census.gov/data/tables/time-series/demo/income-poverty/historical-poverty-thresholds.html

Figure A1. Share of 2-digit Industries and 2-digit Occupations by Expansion and Non-expansion States, CPS ASEC 2007–2013

Panel A. Share of 2-digit Industries



Panel B. Share of 2-digit Occupations

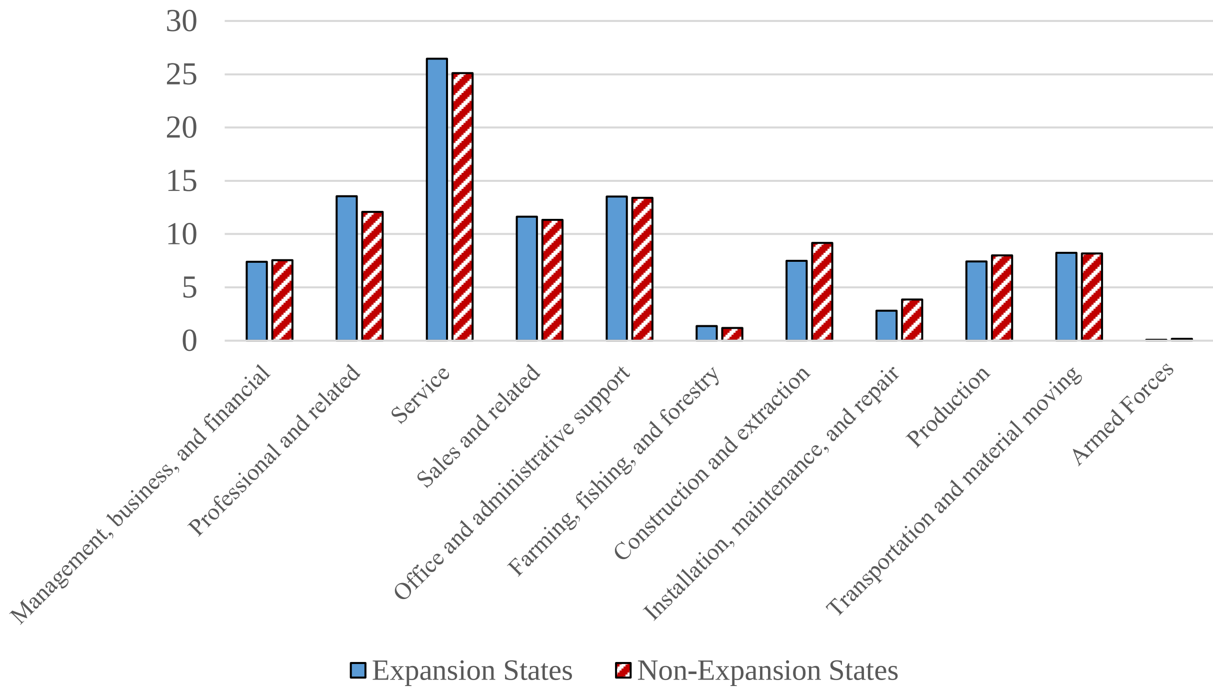


Figure A2. Trends of the Share of Self-employment, Self-employment Entry, and Self-employment Exit, Low-income Childless Adult Sample (FPL < 300%)

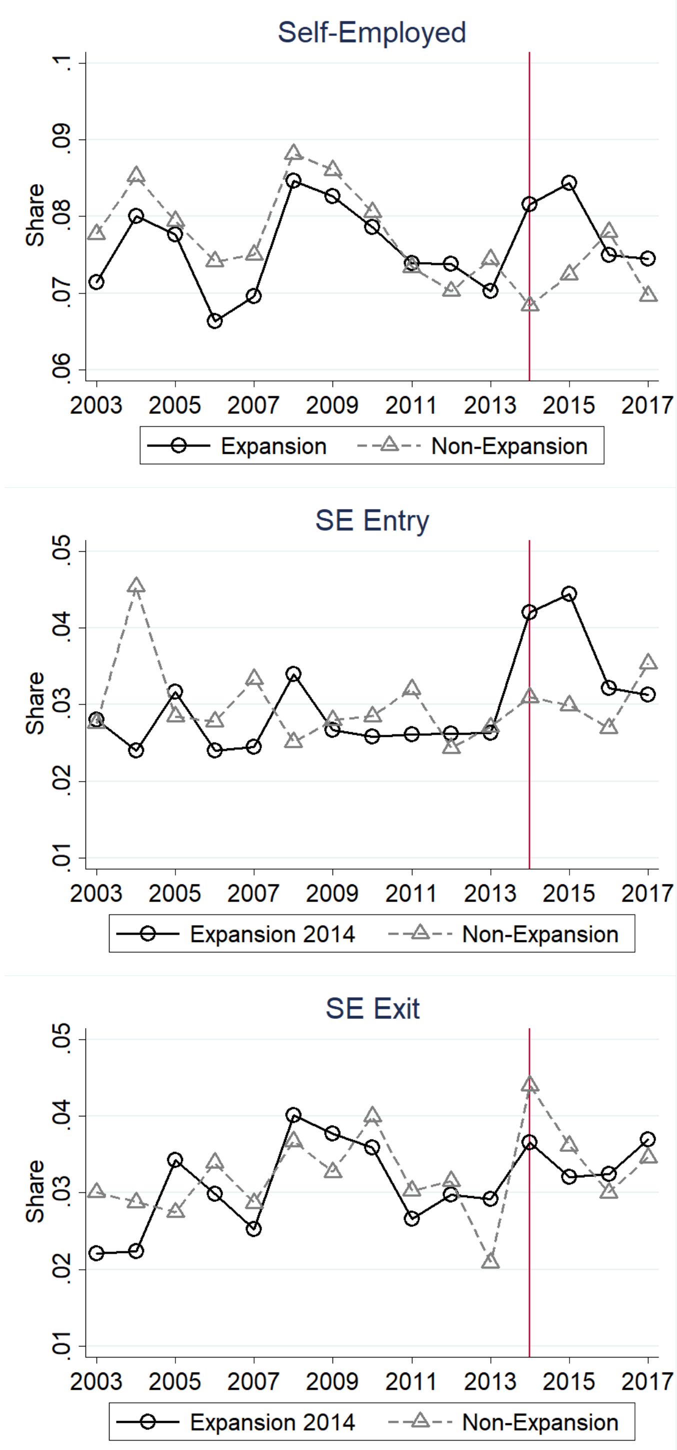


Figure A3. Event Study Analysis with Propensity Score Weighting, Low-income Childless Adult Sample (FPL < 300%)

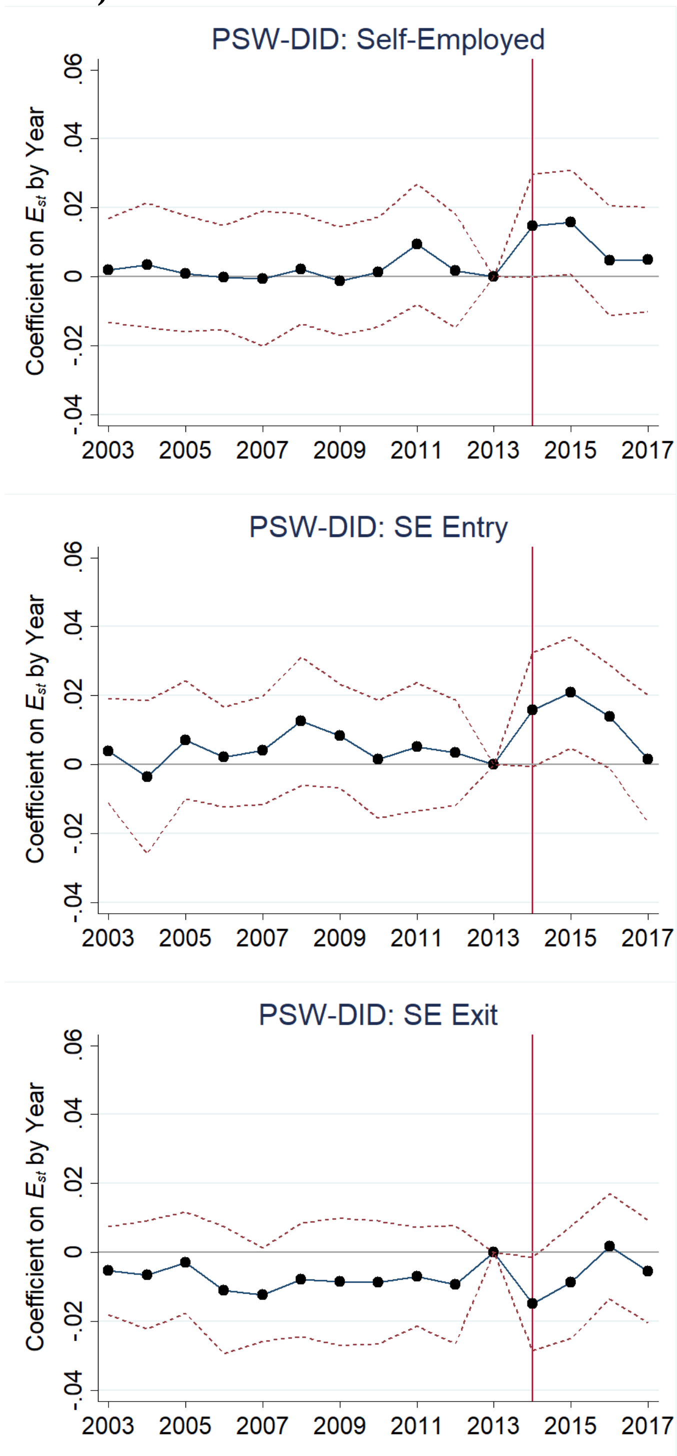


Table A2. Event Study Analysis, Low-income Sample (<300% FPL)

	(1)	(2)	(3)	(4)	(5)	(6)
	Self-employed		SE Entry		SE Exit	
	DID	PSW- DID	DID	PSW- DID	DID	PSW- DID
Expansion × 2003	0.0018	0.0023	0.0070	0.0042	-0.0175***	-0.0064
	(0.0085)	(0.0076)	(0.0060)	(0.0078)	(0.0064)	(0.0066)
Expansion × 2004	0.0003	0.0036	-0.0157	-0.0034	-0.0081	-0.0065
	(0.0102)	(0.0091)	(0.0113)	(0.0113)	(0.0093)	(0.0080)
Expansion × 2005	0.0024	-0.0001	0.0050	0.0072	-0.0054	-0.0022
	(0.0091)	(0.0087)	(0.0070)	(0.0089)	(0.0076)	(0.0073)
Expansion × 2006	-0.0000	0.0001	0.0015	0.0026	-0.0124	-0.0108
	(0.0089)	(0.0076)	(0.0060)	(0.0076)	(0.0095)	(0.0093)
Expansion × 2007	-0.0006	-0.0020	-0.0026	0.0037	-0.0119	-0.0117*
	(0.0113)	(0.0102)	(0.0066)	(0.0080)	(0.0073)	(0.0069)
Expansion × 2008	0.0036	0.0021	0.0098	0.0128	-0.0078	-0.0087
	(0.0092)	(0.0080)	(0.0083)	(0.0097)	(0.0072)	(0.0082)
Expansion × 2009	0.0034	-0.0007	0.0035	0.0086	-0.0065	-0.0093
	(0.0086)	(0.0080)	(0.0060)	(0.0078)	(0.0081)	(0.0092)
Expansion × 2010	0.0059	0.0023	0.0010	0.0016	-0.0133	-0.0085
	(0.0092)	(0.0080)	(0.0068)	(0.0088)	(0.0095)	(0.0091)
Expansion × 2011	0.0081	0.0092	-0.0009	0.0051	-0.0096	-0.0067
	(0.0099)	(0.0089)	(0.0075)	(0.0095)	(0.0073)	(0.0073)
Expansion × 2012	0.0079	0.0014	0.0038	0.0037	-0.0095	-0.0100
	(0.0090)	(0.0082)	(0.0055)	(0.0079)	(0.0091)	(0.0086)
Expansion × 2014	0.0186**	0.0145*	0.0111	0.0146*	-0.0147**	-0.0135**
	(0.0088)	(0.0075)	(0.0075)	(0.0086)	(0.0068)	(0.0068)
Expansion × 2015	0.0177*	0.0149**	0.0175**	0.0203**	-0.0130	-0.0083
	(0.0100)	(0.0076)	(0.0086)	(0.0082)	(0.0080)	(0.0083)
Expansion × 2016	0.0059	0.0044	0.0112*	0.0148*	-0.0042	0.0012
	(0.0102)	(0.0082)	(0.0059)	(0.0079)	(0.0081)	(0.0075)
Expansion × 2017	0.0118	0.0062	0.0026	0.0037	-0.0076	-0.0068
	(0.0090)	(0.0075)	(0.0089)	(0.0095)	(0.0077)	(0.0075)
Characteristics	Yes	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Unemp. Rate	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Sample is restricted to nondisabled childless adults aged 26-64. Estimates are calculated using CPS ASEC weights. Individual characteristics include age, sex, race, education, marital status, foreign-born status, and citizenship status. Standard errors clustered at the state-year level are provided in parentheses. The numbers of observations for the FPL<300% full and transition samples are 156,257 and 57,166, respectively.

Appendix B. Synthetic Control Group Method

As an alternative approach to estimate the effects of the Medicaid expansion on self-employment, I use the synthetic control group method (SCGM) invented by Abadie, Diamond, and Hainmueller (2010). The synthetic control method weights outcome measures from control groups before the policy intervention to construct a counterfactual outcome measure for the treated group in the absence of the treatment effects. Then, it estimates the causal effect by using the differences between the treated and synthetic control groups after the implementation of the policy. The main advantage of using the synthetic control method is to allow the effects of unobserved characteristics on the outcome to vary across time, which addresses concerns about potential bias due to unobserved heterogeneity across states.

Following the notation used in Abadie, Diamond, and Hainmueller (2010), I specify the observed outcome can be specified as follows:

$$Y_{jt} = Y_{jt}^{N} + \alpha_{jt} D_{jt}. \tag{B.1}$$

Y_{jt} is the observed outcome at group j and time t. Y_{jt}^{N} is the unobserved outcome in the absence of the Medicaid expansion. α_{jt} is the effect of the Medicaid expansion for group j and time t. D_{jt} is an indicator variable for the treated group in the post intervention, which is equal to one if $j = 1$ and $t > T_0$ and zero otherwise. Because of the indicator variable, only the treated group in the post-expansion period can have the effect of the intervention.

In order to estimate the effect of the Medicaid expansion (α_{1t}), I need both Y_{1t} and Y_{1t}^{N}. Since Y_{1t} is the observed outcome, I estimate the treatment-free outcome variable Y_{jt}^{N} with following specification:

$$Y_{jt}^{N} = \delta_t + \lambda_t \mu_j + \theta_t Z_j + \varepsilon_{jt}. \tag{B.2}$$

δ_t are time effects; μ_j are time-invariant unobserved variables with time-varying coefficients λ_t; Z_j are the observed time-invariant covariates with time-varying coefficients θ_t; and ε_{jt} are unobserved transitory shocks at the group across time. There is an assumption of linearity between pre-treated covariates and post-untreated outcomes.

By weighting covariates among control groups, the SCGM constructs a synthetic control group that produces an approximation of covariates of the treated group in pre-intervention time periods, which is the linear combination of observed outcomes in the control groups: $\widehat{Y_{1t}^{N}} =$

$\sum_{j=2}^{J+1} w_j^* Y_{jt}$. As in Abadie, Diamond, and Hainmueller (2010), I select weights that minimize the root mean square prediction error (RMSPE) in the pre-intervention time period, which is specified as

$$RMSPE = \sqrt{\frac{\sum_t^{T_0}(Y_{1t} - \sum_{j=2}^{J+1} w_j^* Y_{jt})^2}{T_0 - t + 1}}, \tag{B.3}$$

where w_j^* is the optimal weights that minimizes RMSPE. t is the beginning and T_0 is the end of the time periods. $T_0 - t + 1$ computes the number of periods. The RMSPE measures the difference between observed outcomes of the treatment group and synthetic control estimates. If the synthetic counterparts are not close to the observed outcomes of the treatment group, the value of RMSPE increases.

Then, the effect of the Medicaid expansion for the treated groups can be estimated by subtracting counterfactual outcomes from the observed outcomes as follows:

$$\widehat{\alpha_{1t}} = Y_{1t} - \widehat{Y_{1t}^N} = Y_{1t} - \sum_{j=2}^{J+1} w_j^* Y_{jt}. \tag{B.4}$$

I aggregate data up to the state level with the weights from the CPS. This process creates 24 treatment states and 21 control states, excluding late expansion states and the District of Columbia. For the case of multiple treatment groups, Abadie, Diamond, and Hainmueller (2010) suggested aggregating all treatment groups into a single treatment group. I create a new single treatment group by aggregating the 24 treatment states.

For inference of the treatment effects, I use a permutation test as suggested in Abadie et al. (2015). Using the RMSPE equation with the optimal weight structure, I first compute both pre- and post-intervention RMSPE as well as the RMSPE Ratio (=$RMSPE_{pre}/RMSPE_{post}$) for my treatment group. I also run placebo estimates by changing treatment status and compute RMSPE ratios for all placebo estimates. After I rank these RMSPE ratios of treatment and placebo estimates, I calculate p-values by using the percentile of the rank of the RMSPE for treatment estimate. Since I have one treatment and 19 placebo estimates, the smallest p-value would be 0.05. Considering the small number of states, I take a conservative perspective and consider a significant treatment effect only if the RMSPE ratio of treatment estimates is the first rank of a distribution of RMSPE ratios.

Appendix C. Robustness Check

Table C1. Difference-in-differences: Logit and Probit Models, Low-income Childless Adult Sample (<300% FPL)

	(1)	(2)	(3)	(4)	(5)	(6)
	Self-employed		SE Entry		SE Exit	
	DID	PSW- DID	DID	PSW- DID	DID	PSW- DID
Panel A: Logit						
Expansion × Post	0.0115***	0.0081***	0.0090***	0.0083**	-0.0014	0.0009
	(0.0037)	(0.0030)	(0.0034)	(0.0033)	(0.0034)	(0.0033)
Panel B: Probit						
Expansion × Post	0.0116***	0.0084***	0.0094***	0.0082**	-0.0015	0.0009
	(0.0037)	(0.0030)	(0.0034)	(0.0033)	(0.0034)	(0.0032)
Characteristics	Yes	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Unemp. Rate	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Sample is restricted to nondisabled childless adults aged 26-64. Estimates are calculated using CPS ASEC weights. Individual characteristics include age, sex, race, education, marital status, foreign-born status, and citizenship status. Standard errors clustered at the state-year level are provided in parentheses. The numbers of observations for the FPL<300% full and transition samples are 156,257 and 57,166, respectively.

Table C2. Difference-in-differences, Low-education Childless Adult Sample (High School or Less)

	(1)	(2)	(3)	(4)	(5)	(6)
	Self-employed		SE Entry		SE Exit	
	DID	PSW- DID	DID	PSW- DID	DID	PSW- DID
Panel A: Low Education						
Expansion × Post	0.0067**	0.0053*	0.0087**	0.0064*	0.0008	0.0008
	(0.0034)	(0.0030)	(0.0040)	(0.0034)	(0.0036)	(0.0030)
Characteristics	Yes	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Unemp. Rate	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Sample is restricted to nondisabled childless adults aged 26-64. Estimates are calculated using CPS ASEC weights. Individual characteristics include age, sex, race, education, marital status, foreign-born status, and citizenship status. Standard errors clustered at the state-year level are provided in parentheses. The numbers of observations for the low-education full and transition samples are 183,762 and 70,300, respectively.

Table C3. Difference-in-differences: Control Herfindahl-Hirschman Index (HHI), Low-income Childless Adult Sample (<300% of the FPL)

	(1)	(2)	(3)	(4)	(5)	(6)
	Self-employed		SE Entry		SE Exit	
	DID	PSW- DID	DID	PSW- DID	DID	PSW- DID
Panel A: Individual and Small Group Insurance Market Competition						
Expansion × Post	0.0118**	0.0073*	0.0097**	0.0085**	-0.0054	-0.0013
	(0.0052)	(0.0040)	(0.0043)	(0.0042)	(0.0047)	(0.0044)
Individual	-0.0001	0.0003	-0.0046***	-0.0026*	-0.0020	-0.0008
Market HHI/1,000	(0.0014)	(0.0014)	(0.0017)	(0.0013)	(0.0020)	(0.0017)
Small Group	-0.0003	-0.0019	-0.0027	-0.0058*	-0.0013	-0.0020
Market HHI/1,000	(0.0033)	(0.0030)	(0.0031)	(0.0033)	(0.0032)	(0.0030)
Characteristics	Yes	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Unemp. Rate	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Sample is restricted to nondisabled childless adults aged 26-64. Because of data limitations, the sample period is restricted to 2011-2016. The Herfindahl-Hirschman Index (HHI) for the health insurance market is from the Kaiser Family Foundation. Estimates are calculated using CPS ASEC weights. Individual characteristics include age, sex, race, education, marital status, foreign-born status, and citizenship status. Standard errors clustered at the state-year level are provided in parentheses. The numbers of observations for the FPL<300% full and transition samples with HHI information are 60,075 and 21,466, respectively.

Table C4. Difference-in-differences: Different Treatment and Control Groups, Low-income Childless Adult Sample

	(1)	(2)	(3)	(4)	(5)	(6)
	Self-employed		SE Entry		SE Exit	
	DID	PSW- DID	DID	PSW- DID	DID	PSW- DID
Panel A: Excluding Late Expansion States						
Expansion × Post	0.0114***	0.0085***	0.0104**	0.0084**	-0.0023	0.0002
	(0.0040)	(0.0032)	(0.0043)	(0.0038)	(0.0040)	(0.0037)
Panel B: Including Wisconsin in Control Group						
Expansion × Post	0.0116***	0.0083***	0.0085**	0.0070**	-0.0006	0.0014
	(0.0036)	(0.0029)	(0.0039)	(0.0035)	(0.0036)	(0.0033)
Panel C: Including Prior Expansion States in Treatment Group						
Expansion × Post	0.0108***	0.0077***	0.0081**	0.0068**	-0.0002	0.0020
	(0.0034)	(0.0029)	(0.0037)	(0.0034)	(0.0035)	(0.0033)
Panel D: Including Both Wisconsin and Prior Expansion States						
Expansion × Post	0.0111***	0.0080***	0.0070*	0.0058*	0.0006	0.0023
	(0.0034)	(0.0028)	(0.0037)	(0.0033)	(0.0035)	(0.0032)
Characteristics	Yes	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Unemp. Rate	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Sample is restricted to nondisabled childless adults aged 26-64. Estimates are calculated using CPS ASEC weights. Individual characteristics include age, sex, race, education, marital status, foreign-born status, and citizenship status. Standard errors clustered at the state-year level are provided in parentheses. The numbers of observations for Panels A, B, C, and D full samples are 141,933, 159,344, 185,205, and 188,292, respectively. The numbers of observations for Panels A, B, C, and D transition samples are 51,724, 58,338, 67,736, and 68,908, respectively.

Table C5. Synthetic Control Group Method: Average Treatment Effects of Medicaid, Low-income Childless Adult Sample (<300% of the FPL)

	(1) Self-employed	(2) SE Entry	(3) SE Exit
Panel A: FPL<300%			
Treatment Effect	0.0095*	0.0038	0.0015
P-value	[0.0526]	[0.8421]	[0.7895]
RMSPE	0.003	0.005	0.011

Notes: Sample is restricted to nondisabled childless adults aged 26-64. Weights are chosen based on individual characteristics that include age, sex, race, education, marital status, foreign-born status, and citizenship status. P-values of the permutation tests are provided in brackets. The number of observations is 285 at the state-year level.

Table C6. Optimal Weights for the Synthetic Control Group

States	Self-employed	SE Entry	SE Exit
Alabama	0.097	0.005	0
Florida	0.217	0.455	0.305
Georgia	0.071	0.107	0.018
Idaho	0	0.032	0
Kansas	0	0	0.038
Maine	0	0.006	0
Mississippi	0	0	0
Missouri	0	0	0.29
Nebraska	0	0.018	0
North Carolina	0.152	0.038	0.025
Oklahoma	0.011	0.009	0.027
South Carolina	0	0	0
South Dakota	0.023	0.063	0.004
Tennessee	0	0.118	0
Texas	0.231	0.147	0.167
Utah	0.052	0	0.016
Virginia	0.145	0	0.11
Wyoming	0	0.001	0

Figure C1. Treatment Effects 2003–2017, Low-income Childless Adult Sample