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Three Essays in Applied Economics

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by

Matthew Fitzgerald

Committee in charge:

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December 2022

The Dissertation of Matthew Fitzgerald is approved.

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Three Essays in Applied Economics

To my parents Raymond and Mary, to my brother Kevin, and to my wife Danae. Thank you for always believing in me.

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Thank you also to my family for their love and support. I am so grateful to my parents, Ray and Mary Fitzgerald, and my brother, Kevin Fitzgerald, for helping me through all of the ups and downs of graduate school. I owe them more than I can put into words.

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EDUCATION

2022	Ph.D. in Economics (Expected), University of California, Santa Barbara.
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WORKING PAPERS

"The Impact of Unemployment Benefits on Personal Bankruptcy Filings: Evidence from Pandemic Unemployment Benefit Expiration"

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RESEARCH POSITIONS

Summer 2018	Intern at the U.S. Department of Agriculture Economic Research Service
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Permissions and Attributions

The content of Chapter 2 and its appendix material are the result of a collaboration with Molly Schwarz.

Abstract

Three Essays in Applied Economics

by

Matthew Fitzgerald

This dissertation consists of three chapters. In the first chapter I analyze the impact of unemployment insurance on personal bankruptcy filings. In response to the COVID-19 pandemic, the US federal government expanded unemployment benefits and extended coverage to previously ineligible workers through Pandemic Unemployment Assistance (PUA) and Federal Pandemic Unemployment Compensation (FPUC). I study the impact of pandemic era unemployment benefit terminations on personal bankruptcy filings in states that ended these federal programs early. Using nonlinear difference-in-differences, I find that Chapter 13 filings increased between 14-15% on average. These findings are consistent with previous work that documented an increase in employment and a decrease in household financial security associated with the termination of pandemic unemployment insurance programs.

In the second chapter, which is joint work with Molly Schwarz, we investigate the extent to which the provision of Medicaid to previously ineligible, low-income childless adults affects their household composition. Using a staggered adoption difference-in-differences design on an urban sample of individuals with less than a high school degree, we find that 26 to 39 year olds experience a significant 4.2% decline in the number of individuals living in the household, which is due to living with fewer extended family members. At the same time, 26 to 39 year olds experience a relatively smaller decline in the number of rooms (1.8%), leading to a 3.1% reduction in the level of household crowding, as measured by persons per bedroom. These reductions in household crowding are

strongest for Hispanic individuals and those living in areas with above-median housing costs. In comparison, there are no significant impacts on household composition for 40 to 64 year olds as a consequence of the policy.

In the third chapter I explore the estimation and interpretation of the coefficient on a treatment variable in two-way fixed effects regressions with group-by-time fixed effects. Using the decomposition from [1] I show that while this design can be successful in leveraging within group variation to estimate the parameter of interest, in cases with heterogeneous treatment effects it leads to the well-documented weighting issues that arise in the canonical two-way fixed effects regression when there is variation in treatment timing. As opposed to the canonical two-way fixed effects regression, however, when group-by-time fixed effects are included weighting issues can arise when there is variation in treatment timing within groups, across groups, or both. I also show that in this setting it is possible to include groups in which all units share the same treatment sequence or that include only subgroups with constant treatment, and that observations in these groups do not contribute to the estimation of the parameter of interest. In this case, under heterogeneous treatment effects the average treatment effect on the treated cannot be obtained, but rather in certain cases the average treatment effect on the treated for groups that have within group variation in treatment sequences is obtained under the assumption that parallel trends hold in every group. While this is typically still a parameter of interest, including groups in which all units share the same treatment sequence or that include only subgroups with constant treatment incorrectly reduces the size of the standard errors and therefore biases inference.

Contents

Chapter 1

The Impact of Unemployment Benefits on Personal Bankruptcy Filings: Evidence from Pandemic Unemployment Benefit Expiration

1.1 Introduction

The COVID-19 pandemic brought about large levels of unemployment and financial insecurity in the United States. In response to the crisis, the federal government passed the CARES Act at the end of March 2020 which created Pandemic Unemployment Assistance (PUA) and Federal Pandemic Unemployment Compensation (FPUC). These programs both expanded unemployment benefits and extended coverage to previously ineligible individuals. The Census Bureau estimates that PUA and FPUC prevented 4.7 million people from falling into poverty, with the largest effects coming from Black and Hispanic individuals [2]. In addition, the Bureau of Labor Statistics found that compared to people who applied for UI benefits during the pandemic and did not receive them, those that received UI benefits were less likely to have difficulty with household expenses, experience food insecurity, be behind on mortgage or rent payments, and be experiencing symptoms of anxiety or depression [3].

While there were temporary lapses in 2020, these UI programs were authorized to be in place for all states from January through September 6, 2021. However, in May of 2021 concerns that PUA and FPUC were hurting employment recovery resulted in 26 states ending at least one of the programs between June and August, with 18 states ending both in June. The remaining 24 states and the District of Columbia continued the programs until their official expiration in September. I use nonlinear difference-in-differences to estimate the impact of ending these UI programs on personal bankruptcy filings.

The theoretical impact of ending PUA and FPUC on personal bankruptcy filings is ambiguous, as losing UI benefits can have a variety of income effects. If an individual who loses UI benefits does not become employed, they experience an immediate loss of income which could result in the person either filing for bankruptcy or not depending on their level of debt and personal resources. If instead the loss of UI benefits leads to employment, then the change in their income relative to their UI benefits could be positive, negative, or negligible depending on their new wage. While the literature on UI benefits pre-pandemic has typically found that increases in UI benefits lead to longer unemployment durations [4], the context of UI benefit receipt during the pandemic differs from that of standard UI benefit receipt. States that ended PUA and FPUC did so while the pandemic was still ongoing, and fear that working could increase the probability of contracting the virus could have altered the typical employment response. However, recent work on the employment effects resulting from the termination of PUA and FPUC has found increases in employment. [5] follow a group of individuals with a UI deposit in April of 2021 and who were not employed by the end of the month and find that unemployed individuals saw a 4.4 percentage point (20 percent) increase in the probability of having found a job through the first week of August. Similarly, using the Current Population

Survey [6] find that ending the programs led to a 14 percentage point increase in the unemployment-to-employment flow.

While both papers find increases in employment, however, they also find evidence of decreases in financial health. Using the Household Pulse Survey [6] find a two percentage point (five percent) decrease in the share of respondents that report having no difficulty in meeting their expenses in the past seven days. [5] find that the average UI benefits of the individuals in states that ended PUA and FPUC dropped by $278 per week while earnings only rose by $14 per week, and thus only 5% of the loss in UI benefits was offset by earnings.

In my analysis I find only weak evidence that Chapter 7 filings increased, but I find strong evidence that Chapter 13 filings increased. Specifically, across my specifications I find that Chapter 13 filings increased between 14-15%. Since workers typically have to be employed to file for Chapter 13 and ending the pandemic UI benefits should not have had an effect on filings for workers who were employed before benefit termination, my results suggest that the increase was driven by newly employed individuals. I verify this finding by showing that individual filings in common law states increased, which rules out strategic joint filing by married couples with an unemployed spouse as the main driver of the effect. While filing for Chapter 13 allows workers to restructure their debt, it also prevents creditors from garnishing wages. Given the recent concern that wage garnishment could impede economic recovery [7], I investigate whether garnishment drove the increase in Chapter 13 filings. Federal law dictates that weekly income below thirty times the federal minimum wage cannot be garnished, and some states have passed stricter protections. If prevention of wage garnishment was the main mechanism behind the increase in filings, then workers whose wages were not subject to garnishment should not have been affected. I therefore analyze filings for workers whose income is below their state's garnishment cutoff. I find that this group saw a significant increase in filings, and thus I do not find evidence that wage garnishment was the sole driver of the increase in bankruptcy filings that I observe.

Recent work has found that the negative employment effects resulting from the pandemic were worse in urban areas relative to rural areas [8, 9]. I therefore rerun my main analysis on counties in metro areas and counties in non-metro areas separately. I find that my main results are mostly driven by metro areas, indicating that labor markets which faired worse during the pandemic also experienced higher Chapter 13 bankruptcy filings after PUA and FPUC were terminated.

My findings point to increases in employment but decreases in financial stability, which adds to the evidence of such effects found in the previous literature. In addition, my results highlight a new channel of financial instability caused by the end of the pandemic era UI programs. It is important to note that during the period I consider, there were still many protections in place that benefited debtors. For example, student loan payments and interest were frozen through the end of 2021 and the federal government, along with many private lenders, offered special mortgage forbearance periods for home loans. I find that even with these protections, newly employed individuals entering expanding labor markets were experiencing a significant amount of financial distress. Financial distress need not always lead to bankruptcy, as bankruptcy comes at a high cost to future credit and therefore is typically a last resort for debtors. Therefore, while other papers have found a decrease in financial health caused by the termination of PUA and FPUC, I show an increase in this more extreme form of financial insecurity. My results thus underscore the importance of considering metrics other than employment rates when evaluating economic recovery.

My paper contributes to the literature on public assistance programs and personal bankruptcy filing. Large medical bills have been closely associated with bankruptcy filing, and previous work has found that access to health insurance can significantly reduce filings [10, 11, 12, 13, 14] while loss of health insurance can increase filings [15]. Other work has found that Aid to Families with Dependent Children (AFDC) and child support also reduce personal bankruptcy filings [16]. This paper is most closely related to the empirical literature on unemployment insurance and personal bankruptcy filing which has found a negative association between UI benefits and filings [16, 17]. However, these papers look at increases in benefits rather than reductions in benefits that are more common during economic recoveries following recessions. I therefore add to this literature by analyzing this alternative and policy relevant setting.

The rest of the paper is structured as follows. Section 2 gives an overview of personal bankruptcy, the federal UI programs implemented during the pandemic, and previous work on bankruptcy and unemployment insurance. Section 3 describes the data used in my analysis. Section 4 lays out the empirical strategy, Section 5 describes the results, and Section 6 concludes.

1.2 Background

1.2.1 Bankruptcy

Bankruptcy is the legal process through which debtors can discharge debt and/or restructure debt payments. Nearly all consumer bankruptcies fall under either Chapter 7 or Chapter 13. During a Chapter 7 bankruptcy, a debtor's nonexempt assets are liquidated to repay creditors and all remaining debt is discharged. If a debtor's current monthly income is above the state median, in order to qualify for Chapter 7 the debtor must pass a means test which compares their last 5 years of income to their amount of unsecured debt [18].

Chapter 13 bankruptcy, sometimes called a 'wage earner's plan', allows debtors with regular income to create a plan to repay some or all of their debts [19]. To initiate a Chapter 13 bankruptcy, a debtor submits a repayment plan to the court that outlines how the debtor will repay their creditors. In most cases if a debtor's monthly income is less than the state median the plan must be for three years, if it is greater the plan must be for five years. Under their proposed plan, debtors pay off secured debt (ex: mortgage, taxes, auto loans) and partially pay unsecured, non priority debt (ex: medical bills and credit cards). All remaining debts that are part of the bankruptcy filing are then dismissed.

The bankruptcy process was modified during the pandemic beginning in March of 2020. Under the CARES Act (signed into law March 27, 2020), COVID related payments from the federal government were excluded from current monthly income calculations to prevent these payments from making debtors ineligible for filing under either chapter. This was especially impactful for Chapter 13 filers as a filer's disposable income is used to determine their repayment plan. In addition, debtors whose bankruptcy filing was confirmed before March 27, 2020 were allowed to extend their repayment plans up to seven years if they could show that they experienced 'material financial hardship' due to COVID-19. These provisions were set to sunset after one year, but the COVID-19 Bankruptcy Relief Extension Act of 2021 (signed into law on March 27, 2021) extended them for an additional year[1].

Under both forms of bankruptcy, filing automatically stays most collection actions against debtors and their property. This means that creditors are not allowed to initiate or continue lawsuits, contact debtors asking for payments, or garnish wages [18, 19]. However, while both Chapter 7 and Chapter 13 can help debtors who are behind on payments, they differ in their form of relief. Chapter 7 allows debtors to quickly discharge most of their debt, but this form of bankruptcy does not allow them to catch up on payments. This means that debtors cannot avoid foreclosure or the repossession of their assets, as all of their non exempt assets can be sold to pay back creditors. Chapter 7 also remains on a filer's credit for up to 10 years. While Chapter 13 does not allow debtors to discharge most of their debt, the repayment plan allows them to catch up on

[1]The extension allowed debtors whose Chapter 13 bankruptcy filing was confirmed before March 27, 2021 to extend their plans to up to seven years. These provisions were not extended further, and therefore expired on March 27, 2022.

payments such as mortgages and car payments which allows them to keep their property. In addition, a Chapter 13 filing only remains on a filer's credit for up to 7 years.

Figure (1.3) shows the number of Chapter 7 and Chapter 13 filings for the 12 months ending June 30 of every year from 2009 to 2022. Bankruptcy filings peaked during the Great Recession for both chapters, though the impact on Chapter 13 filings was more muted. This would be expected during a period with low employment since unemployment typically makes a debtor ineligible for filing Chapter 13. Figures (1.4) and (1.5) highlight the period just before the pandemic through June of 2022. Bankruptcy filing remained relatively flat for both chapters in the years leading up to the pandemic, but decreased sharply in 2020 and 2021. While Chapter 7 filings have not rebounded, Chapter 13 filings increased from June 30, 2021 to June 30, 2022, the first time since the beginning of the pandemic.

1.2.2 PUA and FPUC

The CARES Act created several temporary UI programs that both expanded UI benefit eligibility and increased weekly payments to UI recipients. Pandemic Unemployment Assistance (PUA) extended unemployment benefits to previously ineligible individuals including the self-employed, independent contractors, gig economy workers, and those not able to telework who were not receiving any paid leave. It also extended benefits to individuals that were unemployed, partially unemployed, or unable to work due to a specific COVID-19 related reason [20]. Federal Pandemic Unemployment Compensation (FPUC) supplemented all UI benefits (including for those claiming benefits through PUA) by providing an additional $600 per week. Under the CARES Act FPUC was authorized through July 25, 2020, however it was temporarily replaced by the Lost Wages Assistance Act which provided a $300 per week supplement to individuals receiving at least $100 in weekly UI benefits through September 6, 2020 [2].

[2]Some states provided additional weekly supplements on top of the federal benefits [21].

Beginning December 26, 2020, the Continued Assistance Act reauthorized FPUC at $300 per week. These benefits were set to expire on March 14, 2021, but both PUA and FPUC were reauthorized by the American Rescue Plan which extended both programs through September 6, 2021. However, 26 states decided to end at least some of the temporary benefits before September citing lowering state unemployment rates, elimination of many industry shutdowns, reopened child care facilities, and worker shortages [20]. Of these states, 18 ended both PUA and FPUC in June of 2021, two states ended both programs in July, four states ended FPUC and not PUA, and Maryland and Indiana attempted to end both programs but were required by court order to continue them [20]. The CARES Act required that states sign agreements with the Department of Labor (DOL) to administer these programs, and the agreements specified that a state needed to provide at least 30 day notice to the DOL before terminating either PUA or FPUC. Table 1.1 shows the date that each state publicly announced it was dropping PUA and FPUC as well as the actual date that the programs were dropped for the states used in my analysis.

1.2.3 Bankruptcy and UI

Filing for bankruptcy has been tightly linked to loss of income [22, 23], and has been shown to significantly increase after job displacement [24]. This is unsurprising given that 59% of Americans live paycheck to paycheck and 44% typically have a credit card balance or struggle to keep up with bills [25]. Unemployment insurance therefore may play a key role in mitigating the prevalence of bankruptcy filing. Previous empirical work examining the interaction between UI benefits and Chapter 7 bankruptcy filings has found a negative association [16, 17]. This work, however, focuses on gaining benefits rather than losing benefits. In addition, since individuals typically need to be employed to file for Chapter 13, these papers do not consider the response of Chapter 13 filing to changes in UI benefits.

Understanding how reductions in UI benefits affect bankruptcy filing is important when considering economic recoveries. Take, for example, the Great Recession. During the Great Recession the federal government implemented the Emergency Unemployment Compensation (EUC) program which provided additional UI benefits to states. EUC was modified multiple times, but for most of the program's duration it provided more benefits to states with higher unemployment rates in order to ease the impact of the recession on households. EUC was phased out beginning in 2012, and was completely ended by January 1, 2014. Understanding the impact of this reduction in benefits could inform the implementation and ultimate termination of future recession era UI programs, as finding the optimal rate of benefit termination during an economic recovery could help policy makers accelerate the return to pre-recession economic conditions. However, while the impact of this phase out is extremely policy relevant, the endogeneity of the program to state level unemployment rates makes it difficult to leverage the reduction in benefits to estimate causal effects[3].

While the pandemic created a unique economic climate, PUA and FPUC benefits were not determined by economic conditions within a state and states that dropped these programs opted out of the same expanded benefits. Thus, given the uniformity of PUA and FPUC across states, this setting provides a novel opportunity to better understand the impact of UI benefit reduction during an economic recovery.

1.3 Data

I construct monthly bankruptcy filings for each county using the Federal Judicial Center's Integrated Database (IDB). The IDB contains all US bankruptcy filings from fiscal year 2008 to present, and I extract data from January through August of 2021 for all Chapter 7 and Chapter 13 filings. The distributions of Chapter 7 and Chapter 13

[3]The appropriate way to control for this endogeneity is an active area of debate in the employment effects literature, as different methods typically lead to significantly different results [26, 27, 28, 29].

monthly filings are shown in Figures (1.6) and (1.7), respectively. The data are heavily skewed with a large number of monthly counts of zero, which supports my use of the Poisson regression that I describe in the next section. PUA and FPUC termination dates come from [20], and Figure (1.8) shows the states used in my analysis. I include states that ended both PUA and FPUC in June of 2021 and the 24 states along with the District of Columbia that kept both programs until the federal expiration in September of 2021.

Labor market and population controls come from the Bureau of Labor Statistics (BLS) and the Census Bureau. I obtain monthly county unemployment rates from the BLS Local Area Unemployment Statistics (LAUS) program, and I create three month lags to account for changes in unemployment due to the termination of PUA and FPUC. I also obtain county level population data from the Census Bureau's Population Estimates Program [30]. The Census provides annual population estimates, and I log-linearly interpolate these to construct monthly estimates.

I create COVID-19 related controls using data from the Oxford COVID-19 Government Response Tracker (OxCGRT) [31]. The OxCGRT contains daily indices on the stringency of state level COVID-19 policies for both vaccinated individuals and unvaccinated individuals. I average these daily indices to obtain monthly indices for each month in my sample. I also include data on the number of new state level COVID-19 cases.

1.4 Empirical Strategy

In order to examine the impact of ending PUA and FPUC on bankruptcy filings, I run nonlinear difference-in-differences regressions. Given that my data are monthly county level counts, I estimate the following Poisson difference-in-differences model:

$$Filings_{ct} = exp\{\beta^{pois} EndBenefits_{ct} + \delta_c + \phi_t + \epsilon_{ct}\} \tag{1.1}$$

where $Filings_{ct}$ is the number of filings in county c in month t. $EndBenefits_{ct}$ is an indicator that takes the value of 1 if county c in month t no longer provides PUA and FPUC. Since all of the states in my sample ended PUA and FPUC between June 12 and June 26, in my main specification I drop the month of June. However, my results are robust to including June as a treated month, though they are slightly attenuated as one would expect given that states still had the programs in place for much of June. The results with June are provided in the appendix.

Let $Y_{c,t}(0)$ denote the untreated potential outcome for county c in period t. In my setting, $Y_{c,t}(0)$ is the potential outcome for county c in period t if PUA and FPUC are both still in place. Then the parallel trends assumption required for a causal interpretation of β^{pois} is:

$$\text{For } t \geq 2, \ \frac{\mathbb{E}[Y_{c,t}(0)]}{\mathbb{E}[Y_{c,t-1}(0)]} \text{ is constant across } c \tag{1.2}$$

In the case of a balanced panel with no covariates, the β^{pois} obtained from (1.1) is the same as that in a regression of the same form as (1.1) only replacing the county fixed effects δ_c with an indicator equal to one if a county is in the treated group and zero otherwise, and the time fixed effects ϕ_t with an indicator equal to one once the treatment has begun and zero otherwise [32]. Let D represent the indicator that an observation is in the treated group and $Post$ represent the indicator that takes the value of one once treatment begins. Then under the parallel trends assumption, we have

$$\begin{aligned}\beta^{pois} &= log(\mathbb{E}[Y|D=1, Post=1]) - log(\mathbb{E}[Y|D=1, Post=0]) \\ &- (log(\mathbb{E}[Y|D=0, Post=1]) - log(\mathbb{E}[Y|D=0, Post=0]))\end{aligned}$$

Note that we can rewrite β^{pois} as

$$\beta^{pois} = log\left(\frac{\mathbb{E}[Y|D=1, Post=1]}{\mathbb{E}[Y|D=1, Post=0]} \cdot \frac{\mathbb{E}[Y|D=0, Post=0]}{\mathbb{E}[Y|D=0, Post=1]}\right) \tag{1.3}$$

Let q denote the last period before treatment and let T denote the last period in the sample. Then there are q pre-treatment periods and $T-q$ post treatment periods. Let n_0 denote the number of units that never receive treatment and let n_1 denote the number of units that become treated in period $q+1$. Using the plug in estimators for the expectations in (1.3) yields

$$\hat{\beta}^{pois} = log\left(\frac{\frac{1}{(t-q)n_1}\sum\limits_{\substack{t>q,\\ D=1}} y_{ct}}{\frac{1}{qn_1}\sum\limits_{\substack{t\leq q,\\ D=1}} y_{ct}} \cdot \frac{\frac{1}{qn_0}\sum\limits_{\substack{t\leq q,\\ D=0}} y_{ct}}{\frac{1}{(t-q)n_0}\sum\limits_{\substack{t>q,\\ D=0}} y_{ct}}\right)$$

Notice that the weights cancel and the ratios are made up of only sums over the outcome counts. Then note that since dropping observations with an outcome of zero (i.e. $y_{ct} = 0$) does not affect the sums and the corresponding weights have canceled, one can calculate $\hat{\beta}^{pois}$ from only the observations with a positive count. Therefore observations for which $y_{ct} = 0$ provide no identifying information in computing $\hat{\beta}^{pois}$.

Compared to a linear regression model which does not require variation within the dependent variable to estimate unit fixed effects, the fixed effects Poisson model requires that at least one y_{ct} differs from zero for a county c in order to estimate the unit fixed effect for county c [33]. Since these observations are not required to estimate $\hat{\beta}^{pois}$, this poses no problem in my empirical design with no covariates. In the case with covariates, however, removing observations that have a count of zero in every period removes identifying information and alters the interpretation of the estimate. Therefore, when I include covariates in the Poisson specification I replace county fixed effects with state fixed effects. With covariates (1.1) becomes

$$Filings_{ct} = exp\{\beta^{pois} EndBenefits_{ct} + \delta_s + \boldsymbol{X'_{ct}}\gamma + \phi_t + \epsilon_{ct}\} \tag{1.4}$$

where δ_s are state fixed effects and $\boldsymbol{X}_{ct}$ are the population, unemployment, and COVID-19 controls described in the previous section. Adding covariates comes at the expense of controlling for time invariant county level factors. However, since all states in my sample have counts greater than zero in each period, I keep all observations in the estimation of $\hat{\beta}^{pois}$ while still controlling for time invariant state level factors. In spite of this drawback, my results are similar across these two specifications.

In order to test the parallel trends assumption, I run event study regressions where I replace $EndBenefits_{ct}$ in (1.1) with $\sum_{t \neq May} \beta_t Treat_{ct}$, where $Treat_{ct}$ is equal to one in period t if county c is in the treated group. In this regression the β_t's capture the log ratio of the number of filings between states that ended PUA and FPUC and those that did not each month, relative to the log ratio in May of 2021.

The event study coefficients for the regression for Chapter 7 filings with no controls are shown in Figure 1.9. While there is not evidence against the parallel trends assumption, this is not robust to the inclusion of controls. Therefore care needs to be taken in interpreting results for Chapter 7 filings. The event study coefficients for the Poisson regression for Chapter 13 filings with no controls are shown in panel (a) of Figure 1.10, and I cannot reject that the two groups of states had parallel trends. Given the short time span that I am considering, the county fixed effects in my regression specification should control for many county level differences. However, I also run an event study including the time varying controls mentioned above. The results are shown in panel (b) of Figure 1.10, and again there is no evidence of a violation of the parallel trends assumption.

1.5 Results

My main results are given in Table 1.3. Row 1 gives the results for Chapter 7 filings. Column 1, the estimate from the Poisson regression with no controls, indicates that ending PUA and FPUC increased the number of monthly Chapter 7 bankruptcy filings by 5.7%[4]. This coefficient is only marginally significant, and as noted in the previous section parallel trends do not hold after adding controls. Therefore this result is suggestive of a small increase in Chapter 7 filings, but provides only weak evidence for such an effect. Row 2 gives the results for Chapter 13 filings. The estimate from the Poisson regression with no controls in column 1 indicates that ending PUA and FPUC increased the number of monthly Chapter 13 bankruptcy filings by 15%. Column 2 adds controls, and slightly reduces the estimate to a 14.2% increase. Both results for Chapter 13 filings are significant at the 1% level, and given the tenuous results for Chapter 7 filings, I focus on Chapter 13 filings for the rest of the paper.

My results indicate that ending PUA and FPUC increased Chapter 13 filings. Since ending these programs did not change the circumstances of the already employed, and those who remained unemployed after the programs ended were in general not eligible to file for Chapter 13 bankruptcy, my results suggest that newly employed workers explain the increase in filings. However, there is one channel through which the effect could be driven by workers who lost unemployment benefits and did not find a job. Married couples are allowed to make a joint bankruptcy filing, and both individuals' incomes are used to determine eligibility for Chapter 13 and the repayment plan. Therefore, it could be the case that the increase in filings that I find is driven by one spouse losing their unemployment benefits, which reduces household income, and then filing for Chapter 13 bankruptcy using the working spouse's income. While spouses can also file individually, the non-filing spouse's debt will not be included in the payment plan and therefore their debt will not be restructured/discharged. This means that if the effect is mostly driven

[4]The exact percentage effect is obtained by computing $exp(\beta) - 1$.

by married couples with a working spouse and an unemployed spouse, ending PUA and FPUC should have had little impact on individual filings.

I observe joint filing status in my data and I use this to test whether my results are driven by married couples with one working and one non-working spouse. I remove joint filings to construct counts of monthly county level individual filings, and I drop common property states as individual filings in these states cover more of the non filing partner's debt[5]. My regression results are given in Table 1.4. The coefficient from the Poisson regression without controls in column 1 indicates a 13.3% increase in individual filings. Adding controls in column 2 yields a 15.4% increase. These results provide evidence that individual filings increased during this period to a similar extent as in the full sample, and thus strategic filings by married couples with one employed partner and one unemployed partner are not driving the results. While I cannot rule out the possibility that this type of filing is occurring, I can conclude that newly employed workers are an important group of new filers after the termination of PUA and FPUC.

Aside from direct changes in income as a result of the end of PUA and FPUC, another factor may have contributed to the rise in personal bankruptcies: wage garnishment. The National Consumer Law Center produced a report in 2021 which argued that weak garnishment exemption laws were leading those struggling to recover financially from the pandemic to "face seizure of wages and essential property due to a wave of debt collector lawsuits" [7]. Newly employed workers in states ending PUA and FPUC may have been subject to wage garnishment from debt collectors. While a few states had protections against various forms of garnishment (only Texas and South Carolina had protection against most forms of wage garnishment in the states that ended PUA and FPUC), there were no pandemic specific federal protections against garnishment during this time period. Since unemployment benefits typically cannot be garnished, a new job may not offer a large financial improvement for workers in debt if wage gains over UI

[5]The common property states in my data are California, Idaho, Nevada, New Mexico, Texas, Washington, and Wisconsin.

benefits are garnished.

I explore wage garnishment as a potential mechanism driving the increase in filings that I observe. Federal law dictates that only the lesser of 25% of one's weekly income or the amount of weekly income that exceeds 30 times the federal minimum wage can be garnished. In other words, an individual working 30 hours a week or less at the federal minimum wage cannot have any of their wages garnished. While all states are subject to this federal minimum, some states have increased their own protections over time by increasing the multiplier from 30, decreasing the percent of income subject to garnishment to lower than 25%, tying the calculation to their own state minimum wage, or some combination of these changes[6]. For state s denote the number of weekly hours used in the calculation as h_s, the minimum wage used in the calculation as MW_s, and the percent of weekly income protected as λ_s. Then the general structure for garnishment across all states can be written as[7]:

$$\text{Weekly Income Subject to Garnishment in State } s \quad \begin{cases} 0 & \text{if } Income \leq MW_s \cdot h_s \\ min\{Income - MW_s \cdot h_s,\ \lambda_s \cdot Income\} & \text{if } Income > MW_s \cdot h_s \end{cases}$$

While I do not directly observe wage garnishment in my data, individuals are required to report their current monthly income with their bankruptcy filing. I therefore create monthly county counts of bankruptcy filings from individuals making less than their applicable state income garnishment cutoff, and who are therefore not subject to any wage garnishment. If garnishment is the main driving factor behind the increase in bankruptcy filings that I observe, then this group should not be affected by the end of

[6]California is currently the only state that not only ties the minimum wage to the state minimum wage, but also mandates that the local minimum wage is used in areas with higher minimum wages than the state's.

[7]California's formula differs in that only 50% of the amount by which an individual's income exceeds $MW_s \cdot h_s \cdot 4.35$ is subject to garnishment if it is less than 25% of their income.

PUA and FPUC.

The results from my regression specifications with and without controls are given in Table 1.5. The estimate from the Poisson regression with no controls, given in Column 1, shows that the impact of ending PUA and FPUC on the filers who were not subject to garnishment was a 21.3% increase in monthly county filings. Column 2 gives the estimate with controls, however, North Dakota, South Dakota, and Wyoming did not have any filings from individuals with incomes below their respective income cutoffs during this period. This specification uses state fixed effects, and when all observations within a state have a zero count for all periods, they do not contribute to the estimation of the coefficient of interest. As stated above, this changes the interpretation of the coefficient, which now gives the impact of ending PUA and FPUC on the filers who were not subject to garnishment in all analysis states except the three that are dropped. This coefficient is not entirely comparable to the coefficient from the other specification, however, the interpretation in this case is similar enough that I still report it for completeness. In the sample without the three states with zero filings, the estimate from the Poisson with controls is a 15.1% increase in monthly county filings. These results provide evidence that while wage garnishment may have led to some individuals filing for bankruptcy, this was not the sole explanation for the increase in filings.

Loss of labor income during the pandemic due to high unemployment levels left many individuals in a vulnerable position. Since I show that neither joint filings from married couples with one unemployed spouse nor wage garnishment can fully explain the increase in Chapter 13 filings, my results indicate that employment driven by the reduction in pandemic UI benefits did not alleviate this condition. Further, while filing for Chapter 7 would discharge debt quickly, this could lead to the liquidation of an individual's assets which leaves no recourse to keep one's home (for homeowners) or car. A newly employed individual can instead file for Chapter 13 and can restructure their debt, allowing them to keep their assets. Thus, given that many newly employed filers could choose between

Chapter 7 and Chapter 13, the increase in Chapter 13 filings shows that keeping these assets was important to filers during this time.

Finally, I investigate whether bankruptcy filing changed differentially in response to ending PUA and FPUC across urban and rural areas. Recent work has found that the employment effects of the pandemic tended to be worse for urban areas [8, 9], and therefore these areas may have experienced a larger change in bankruptcy filings due to the reduction in pandemic UI benefits. I rerun my main analysis separately for metro and non-metro areas as defined by the Office of Management and Budget, and the results are given in Table 1.6. Columns 1 and 2 give the results for metro areas using the Poisson model with and without controls. Both models indicate a greater than 15% increase in bankruptcy filings after the end of PUA and FPUC. Columns 3 and 4 give the results for non-metro areas, and indicate an insignificant 5.2% increase in the model without controls and an insignificant 6.3% increase in the model with controls. Thus my results are mostly driven by metro areas whose labor markets were more impacted by the pandemic.

1.6 Conclusion

In this paper I study the impact of early termination of pandemic era unemployment insurance programs on personal bankruptcy filings. I use nonlinear difference-in-differences and find little evidence that Chapter 7 filings increased, but strong evidence that Chapter 13 filings increased. While individuals typically have to be employed to file for Chapter 13, unemployed individuals with an employed spouse can submit a joint filing using the working spouse's income for the repayment plan. The increase in filings I find could therefore have been the result of unemployed individuals remaining unemployed and filing in response to the drop in UI benefits after the end of PUA and FPUC. I explore this possibility by examining the impact on individual filings in common law states. I find a significant increase in these filings of similar magnitudes to the full sample, indicating that newly employed individuals helped drive the increase in filings. This is consistent with previous literature which documents both an increase in employment and a decrease in household financial health after states ended PUA and FPUC early [5, 6].

Given the recent concern that wage garnishment could impact workers returning to the labor market [7], I also explore wage garnishment as a driving factor for the increase in Chapter 13 filings. The federal government stipulates that those working for 30 hours per week or less at the federal minimum wage are exempt from wage garnishment. While all states are subject to this minimum, some states have created their own laws to increase the amount of income exempt from garnishment. I use these state income cutoffs to examine filings made by individuals not subject to wage garnishment. I find that filings in this category also saw a significant increase, and therefore factors other than wage garnishment helped lead to the overall increase in filings.

Lastly, I estimate the impact of ending PUA and FPUC on metro and non-metro counties separately. I find large and significant results for metro areas and smaller, insignificant results for non-metro areas. Thus my main results are driven by metro areas, whose labor markets have been found to have been more negatively impacted by the pandemic.

One distinctive feature of this time period compared to more typical periods of labor market recoveries was the special circumstances being afforded to many types of debt payments. During my sample period there were protections in place that allowed households to delay some debt payments. For example, federal student loan payments were paused and their interest rates held to 0% beginning in January of 2020 [34]. This relief was in place for the entirety of 2021, and the January announcement of the freeze on payments and interest initially had no end date. It was not until August 6, 2021 that the White House specified an end date of January 31, 2022. In addition, for those facing financial hardship due to the pandemic, the CARES Act allowed for mortgage forbearance of up to 180 days, with the possibility of an additional 180 day extension on all federally backed mortgages. Private mortgage lenders offered various forms of mortgage forbearance as well. The increase in bankruptcy filings occurred in spite of these lingering pandemic protections which are not in place in typical recoveries, which speaks more generally to the importance of considering financial health as well as employment prospects when incentivizing labor market transitions.

1.7 Figures and Tables

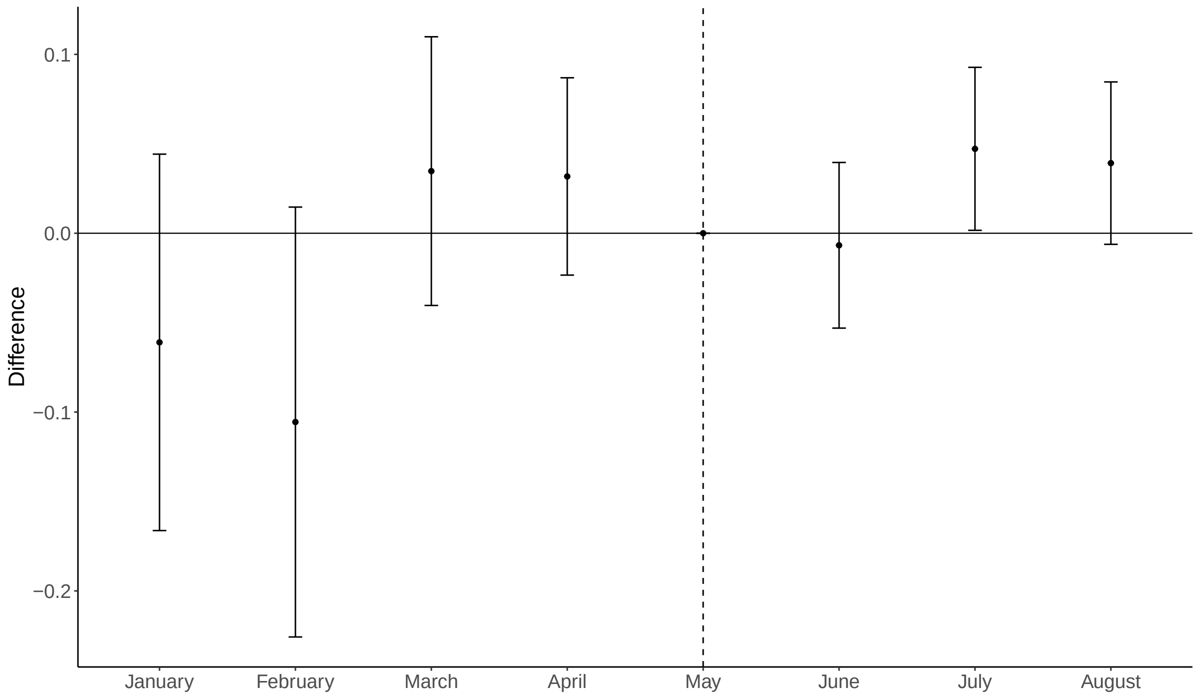
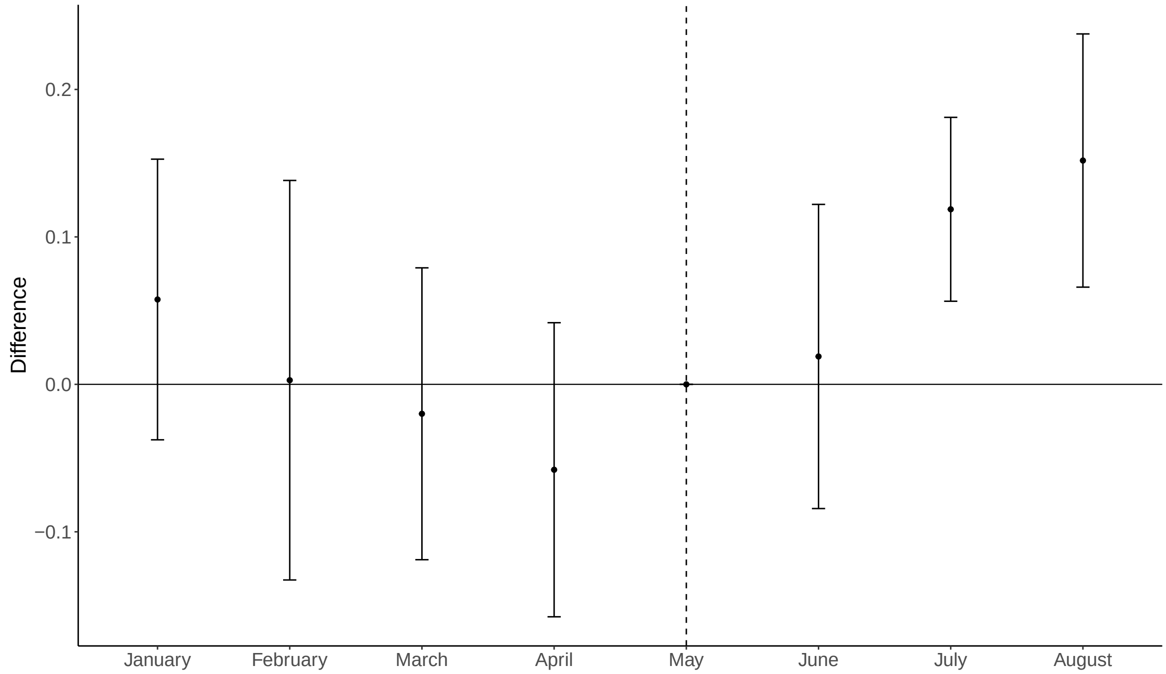


Figure 1.1: Chapter 7 filings event study for the Poisson regression without covariates including June. Standard errors are clustered at the state level.

(a) Poisson



(b) Poisson with covariates

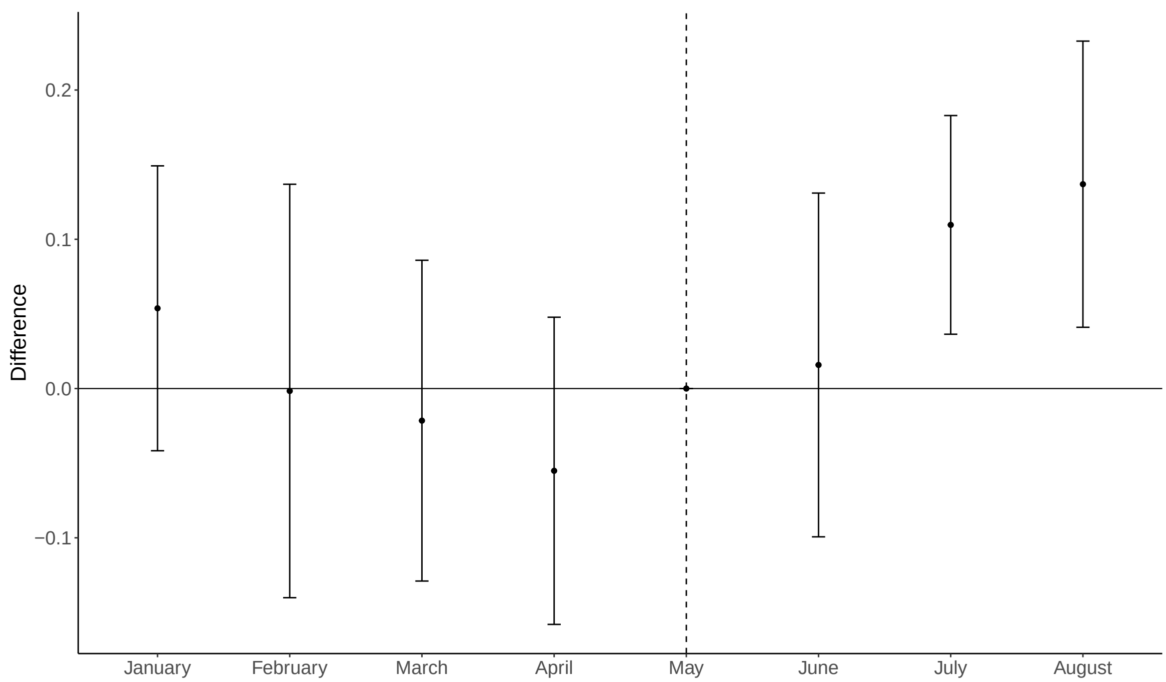


Figure 1.2: Chapter 13 filings event study graphs for the Poisson regression with and without covariates including June. Panel (a) gives the coefficients from the regression without covariates and panel (b) gives the coefficients from the regression with covariates. Standard errors are clustered at the state level in both specifications.

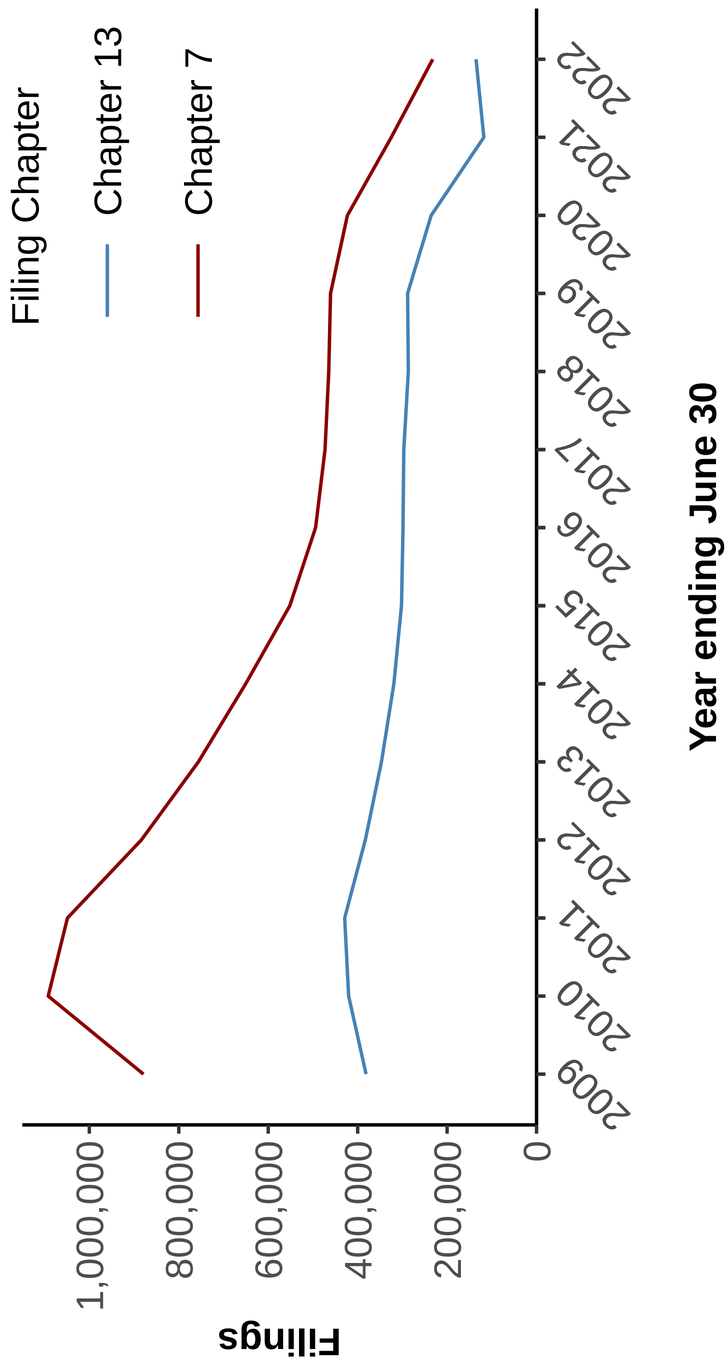


Figure 1.3: Chapter 7 and Chapter 13 filings for the 12 months ending June 30 of each year.

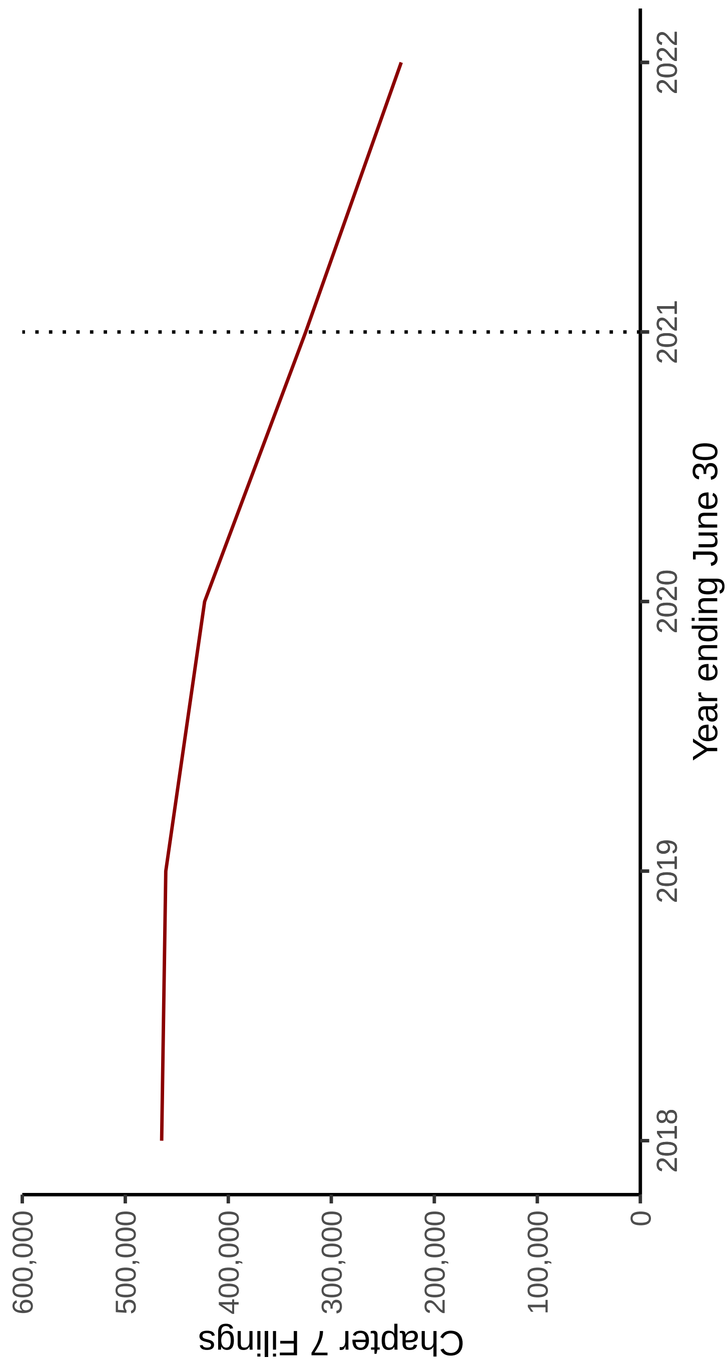


Figure 1.4: Chapter 7 filings for the 12 months ending June 30 of each year.

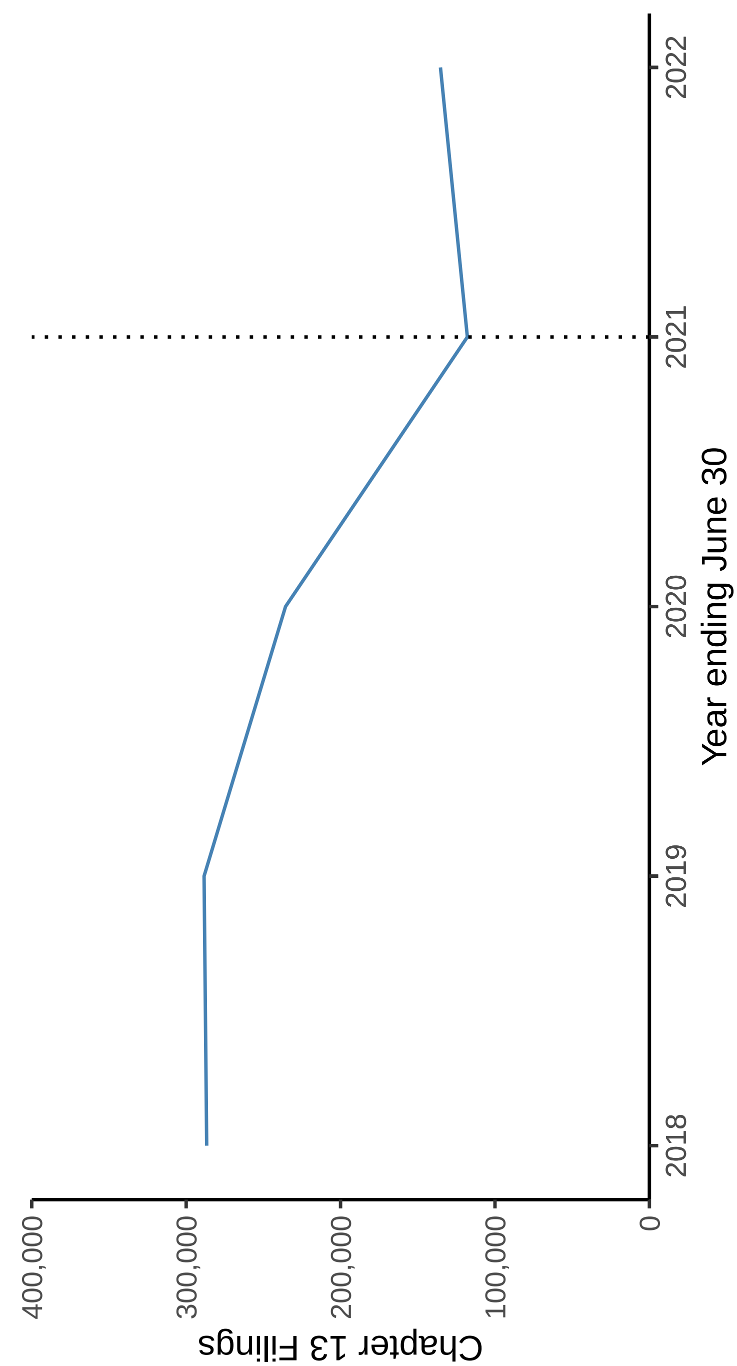


Figure 1.5: Chapter 13 filings for the 12 months ending June 30 of each year.

State	Public Announcement Date	Drop Date
Alabama	May 10, 2021	June 19, 2021
Arkansas	May 7, 2021	June 26, 2021
Georgia	May 13, 2021	June 26, 2021
Idaha	May 11, 2021	June 19, 2021
Iowa	May 11, 2021	June 12, 2021
Mississippi	May 10, 2021	June 12, 2021
Missouri	May 11, 2021	June 12, 2021
Montana	May 4, 2021	June 26, 2021
Nebraska	May 24, 2021	June 19, 2021
New Hampshire	May 18, 2021	June 19, 2021
North Dakota	May 10, 2021	June 19, 2021
Oklahoma	May 17, 2021	June 26, 2021
South Carolina	May 6, 2021	June 26, 2021
South Dakota	May 12, 2021	June 26, 2021
Texas	May 17, 2021	June 26, 2021
Utah	May 12, 2021	June 26, 2021
West Virginia	May 14, 2021	June 16, 2021
Wyoming	May 12, 2021	June 19, 2021

Table 1.1: This table shows the date that each state in the analysis announced that it would be dropping both PUA and FPUC, and the date that both programs were officially dropped.

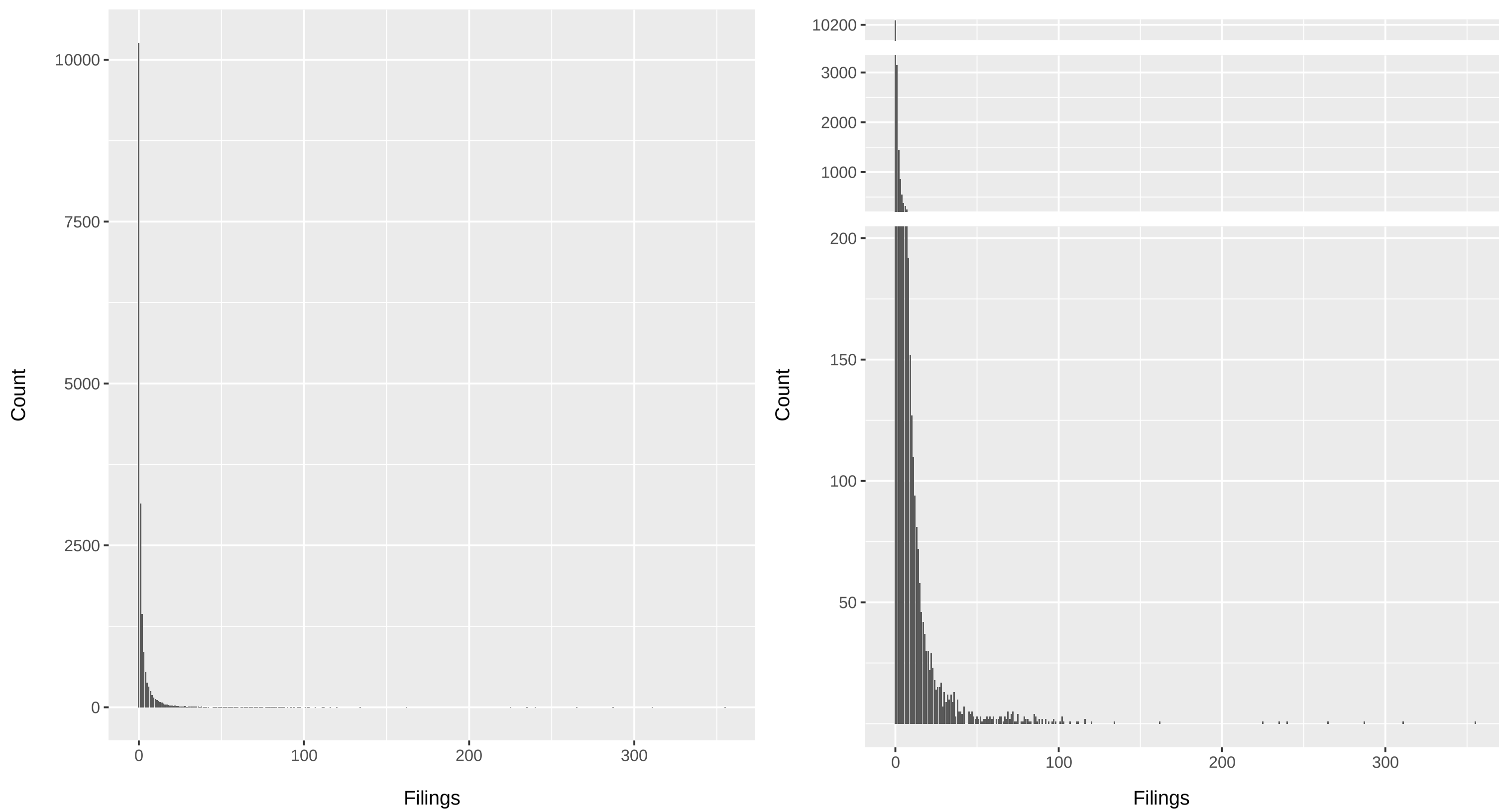


Figure 1.6: Histograms of Chapter 7 filings. The left panel shows the raw histogram, while the right panel breaks the y-axis into three pieces to better display the right tail of the distribution.

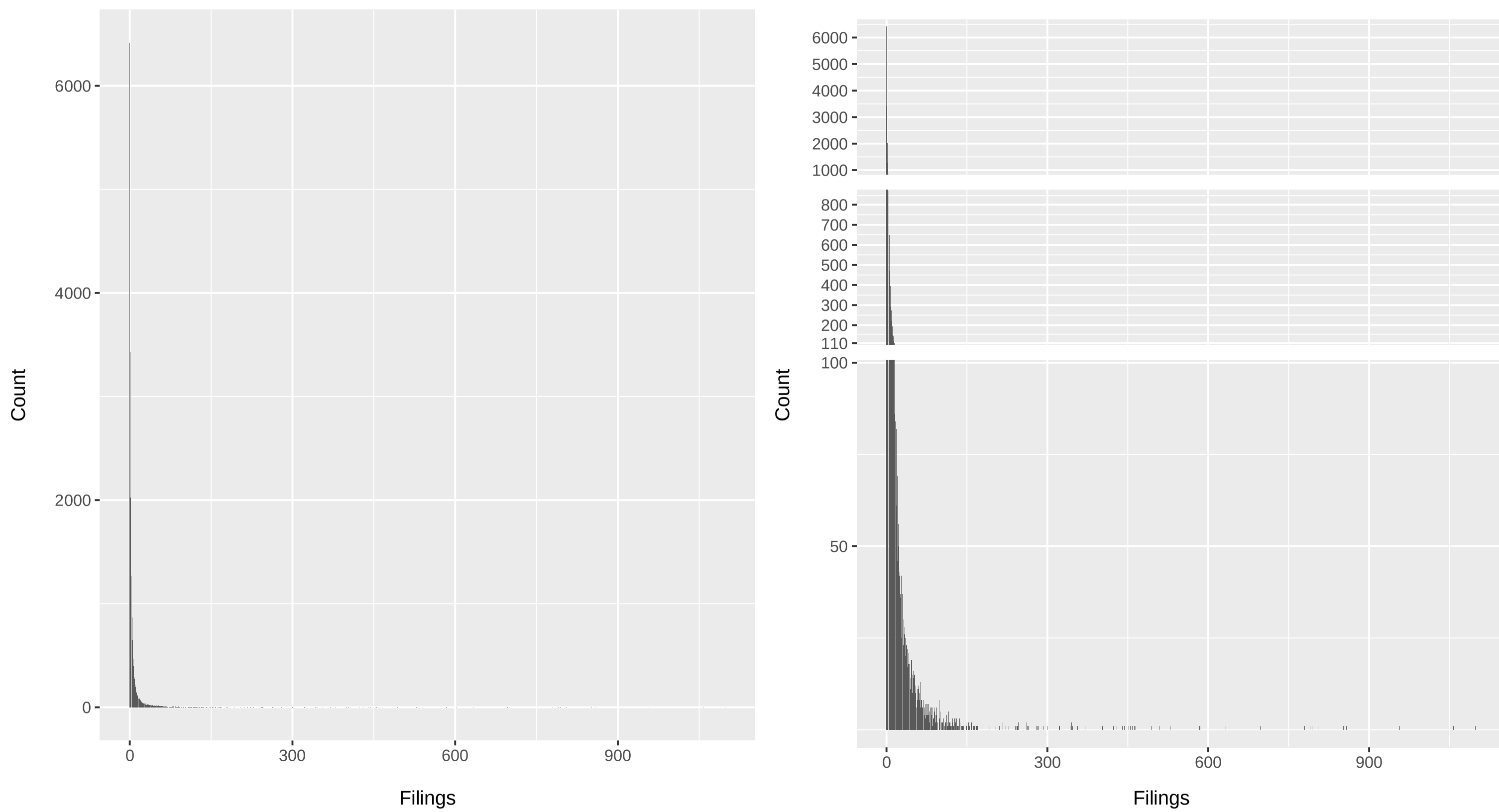


Figure 1.7: Histograms of Chapter 13 filings. The left panel shows the raw histogram, while the right panel breaks the y-axis into three pieces to better display the right tail of the distribution.

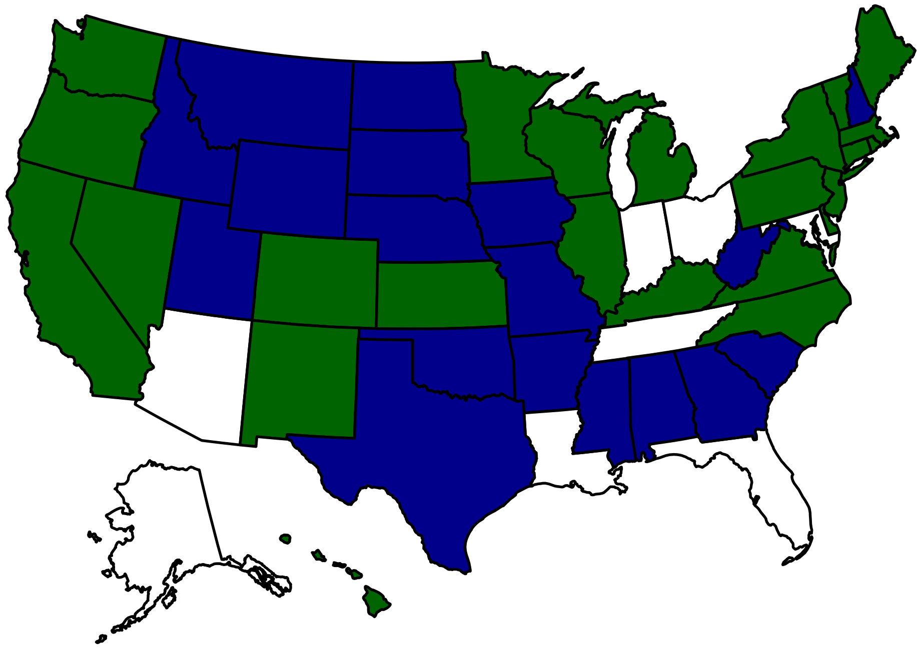


Figure 1.8: States in blue ended both PUA and FPUC in June of 2021 while states in green kept both programs in place until their expiration on September 6, 2021.

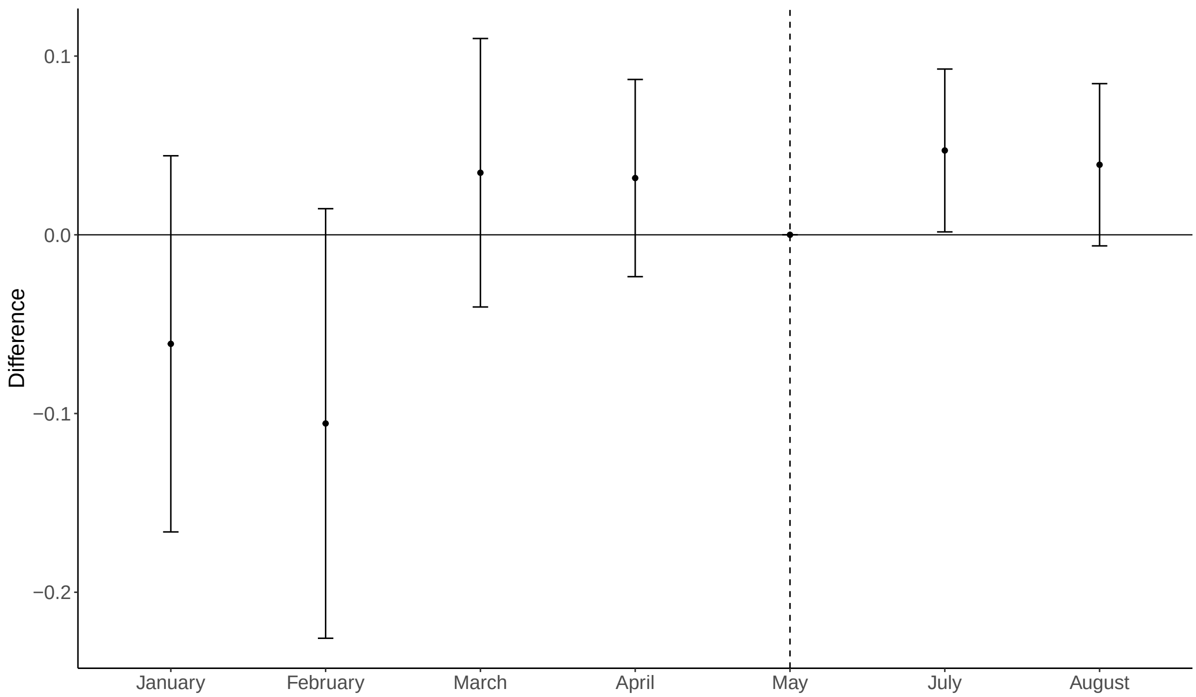
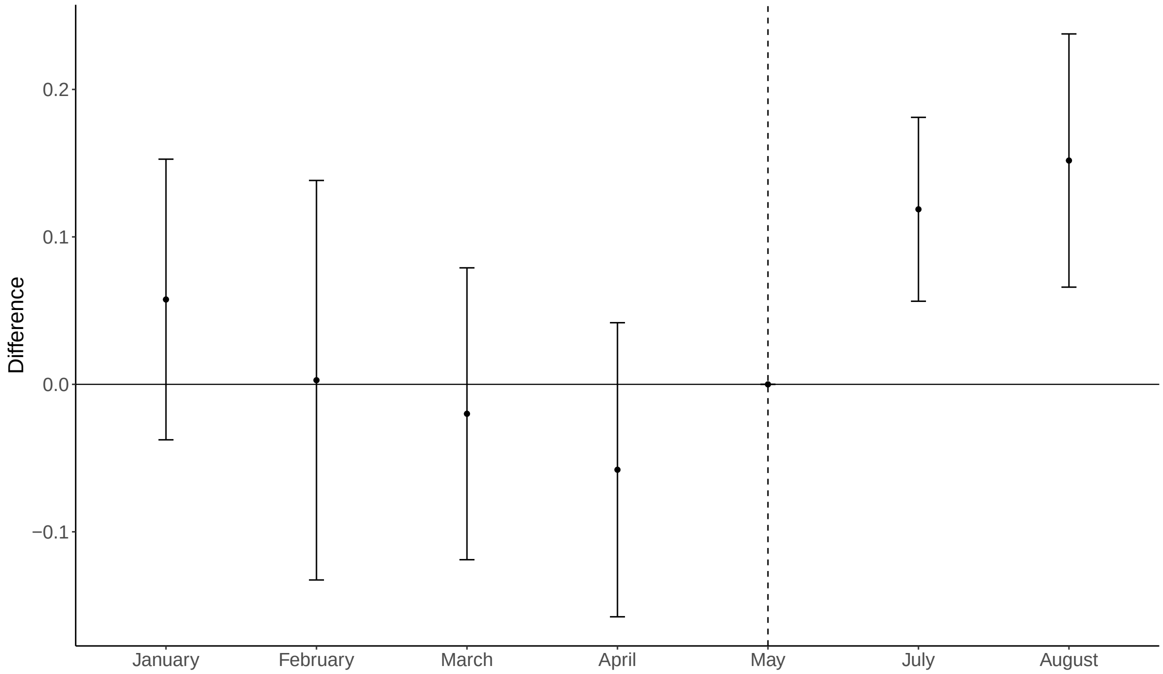


Figure 1.9: Chapter 7 filings event study for the Poisson regression without covariates. Standard errors are clustered at the state level.

(a) Poisson



(b) Poisson with covariates

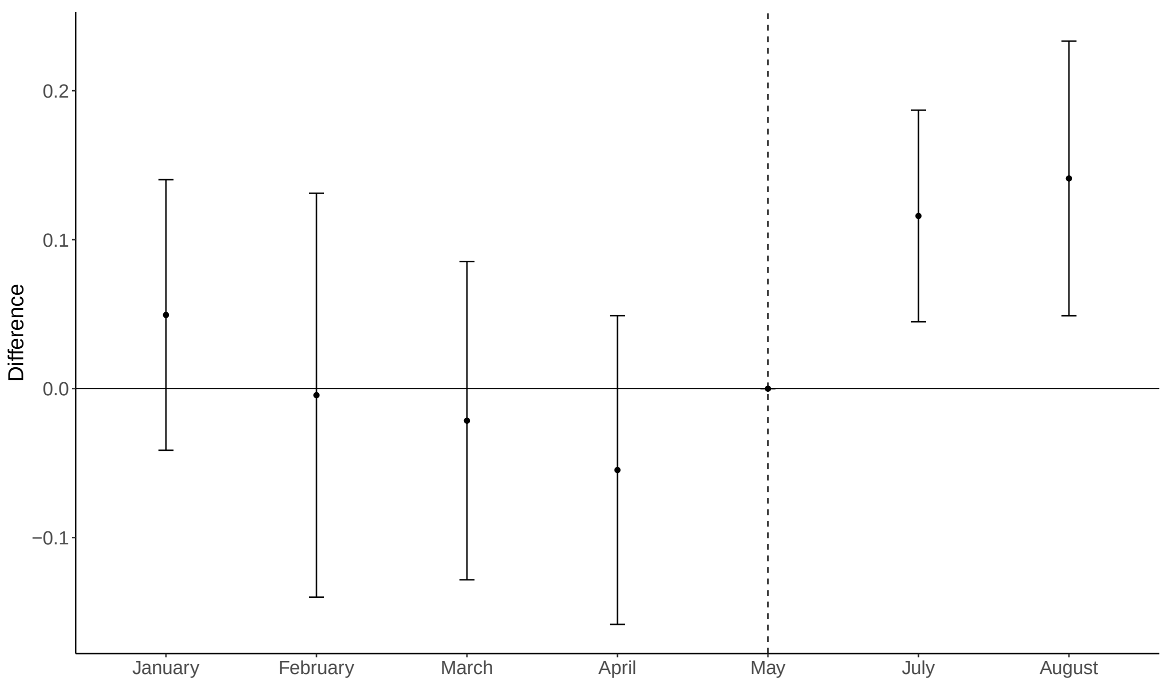
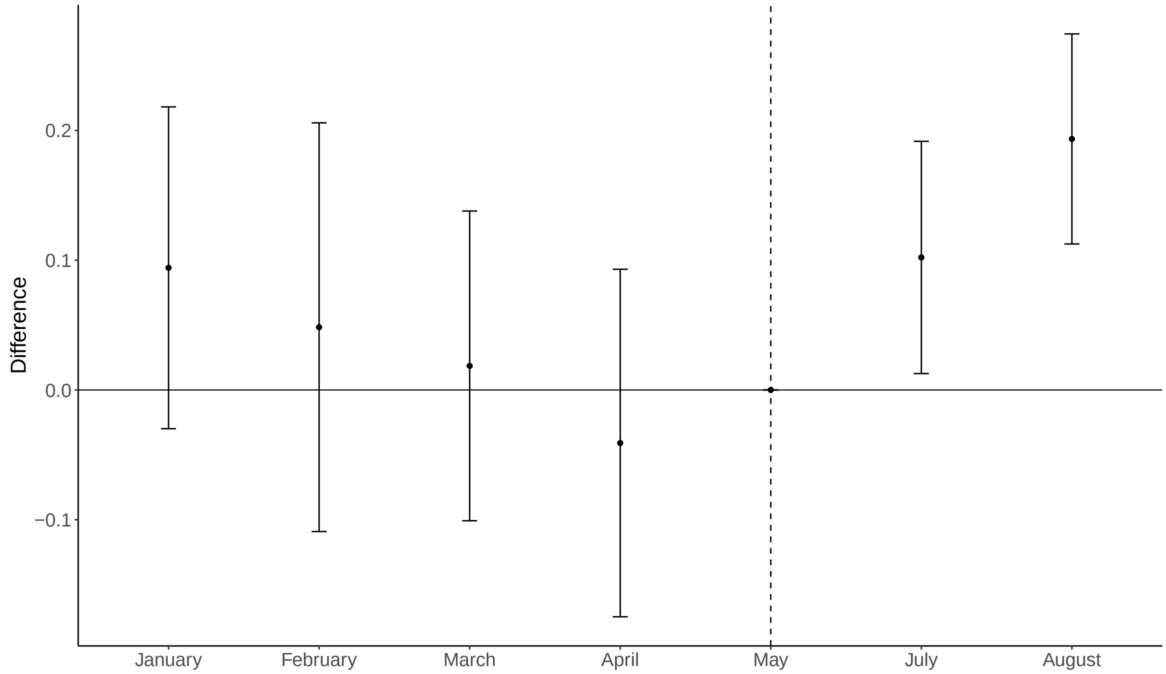


Figure 1.10: Chapter 13 filings event study graphs for the Poisson regression with and without covariates. Panel (a) gives the coefficients from the regression without covariates and panel (b) gives the coefficients from the regression with covariates. Standard errors are clustered at the state level in both specifications.

(a) Poisson



(b) Poisson with covariates

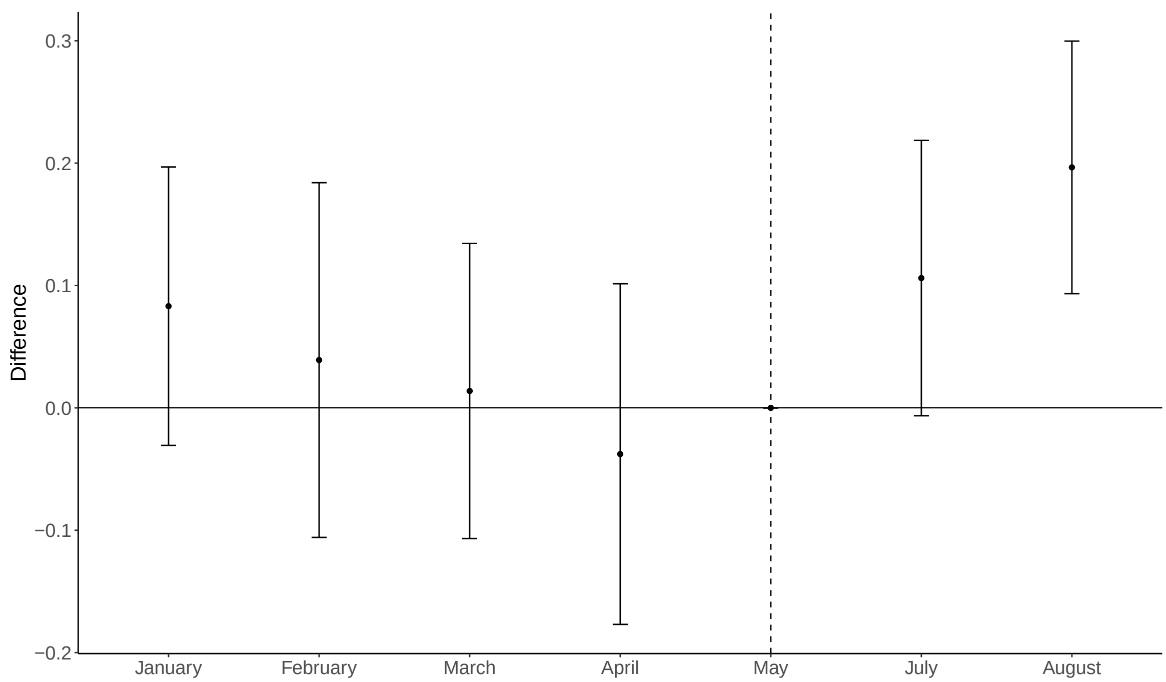
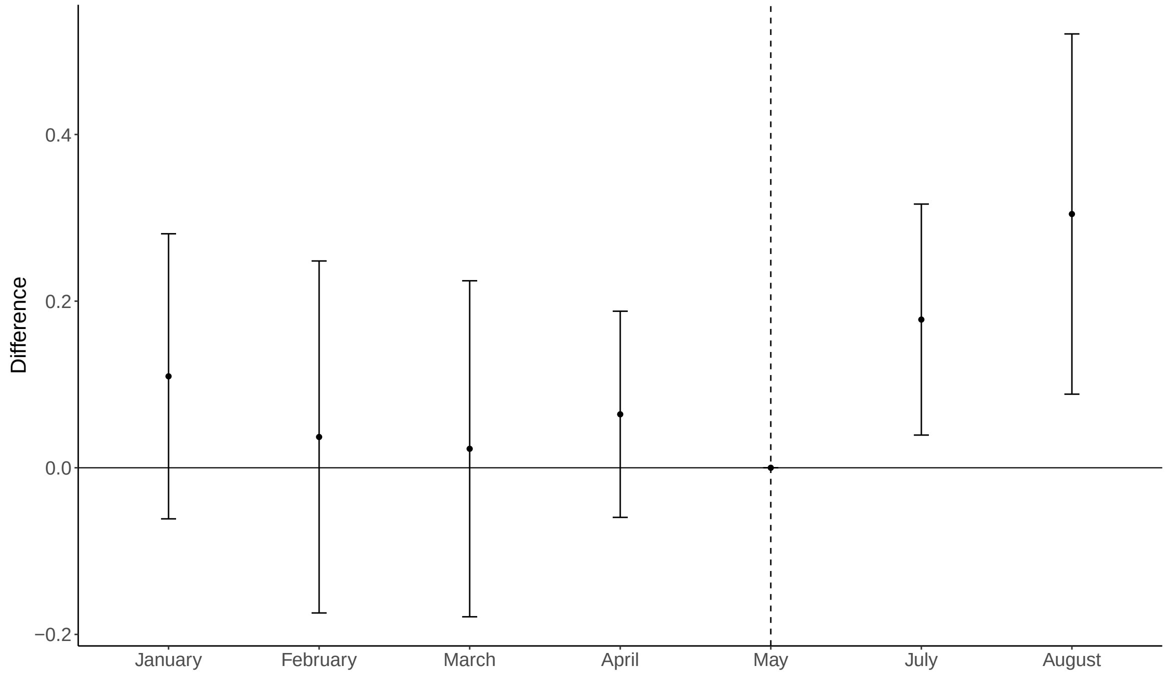


Figure 1.11: Joint filing analysis chapter 13 filings event study graphs for the Poisson regression with and without covariates. Panel (a) gives the coefficients from the regression without covariates and panel (b) gives the coefficients from the regression with covariates. Standard errors are clustered at the state level in both specifications.

(a) Poisson



(b) Poisson with covariates

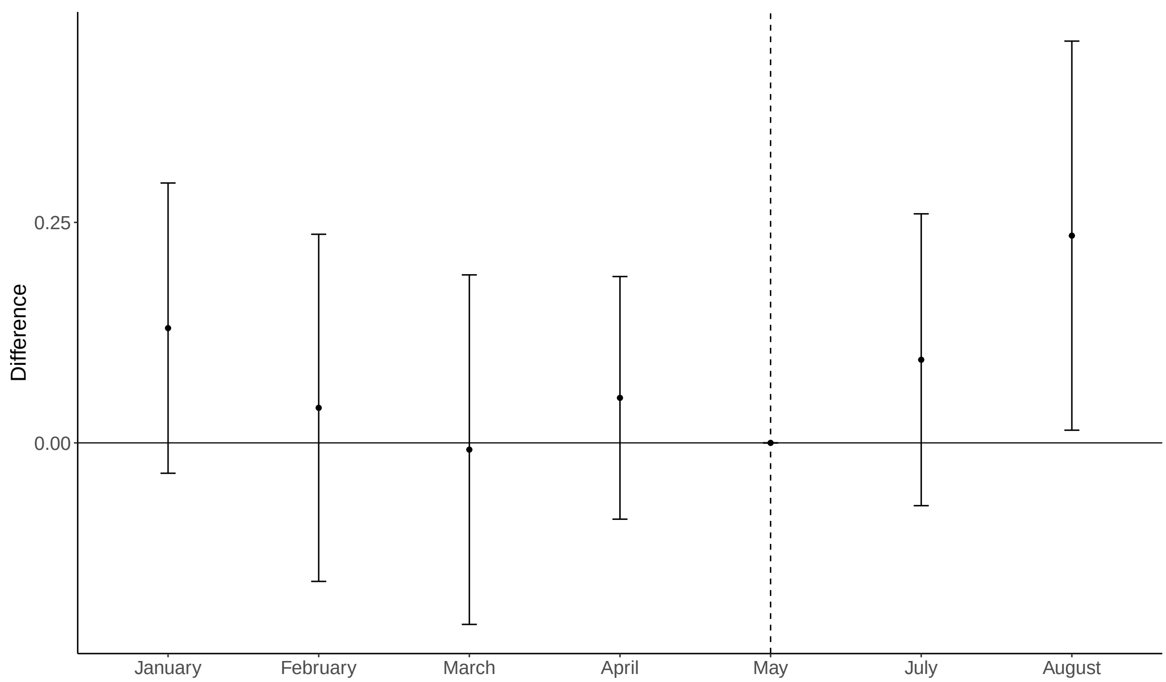
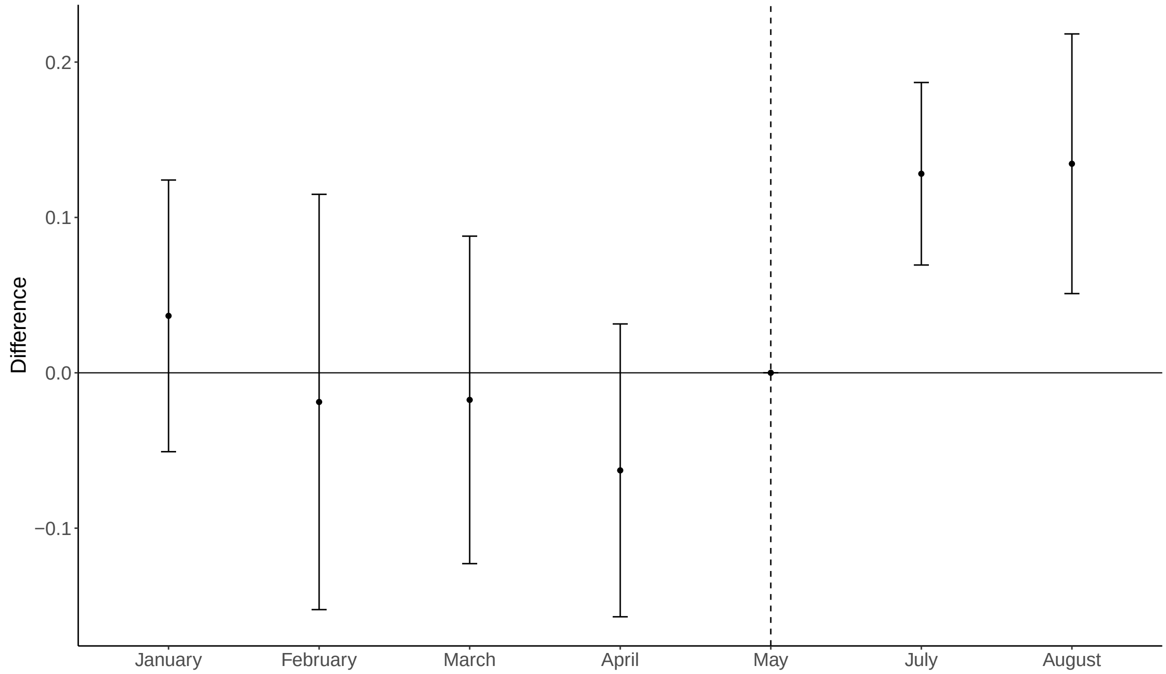


Figure 1.12: Wage garnishment analysis chapter 13 filings event study graphs for the Poisson regression with and without covariates. Panel (a) gives the coefficients from the regression without covariates and panel (b) gives the coefficients from the regression with covariates. Standard errors are clustered at the state level in both specifications.

(a) Poisson



(b) Poisson with covariates

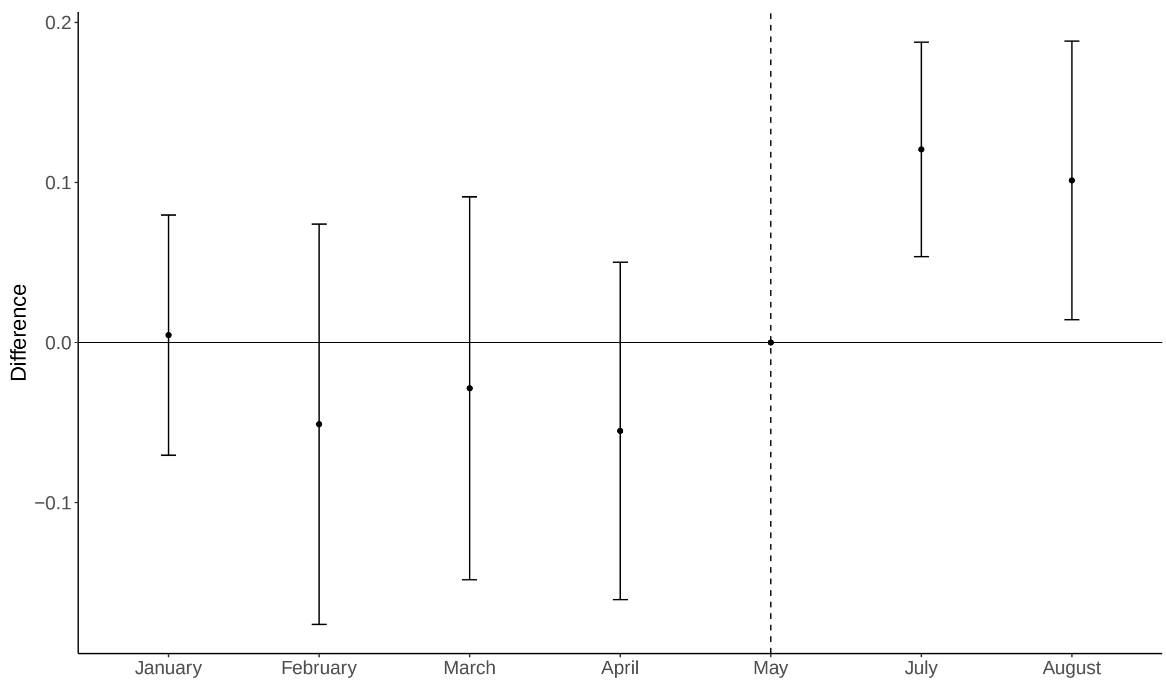
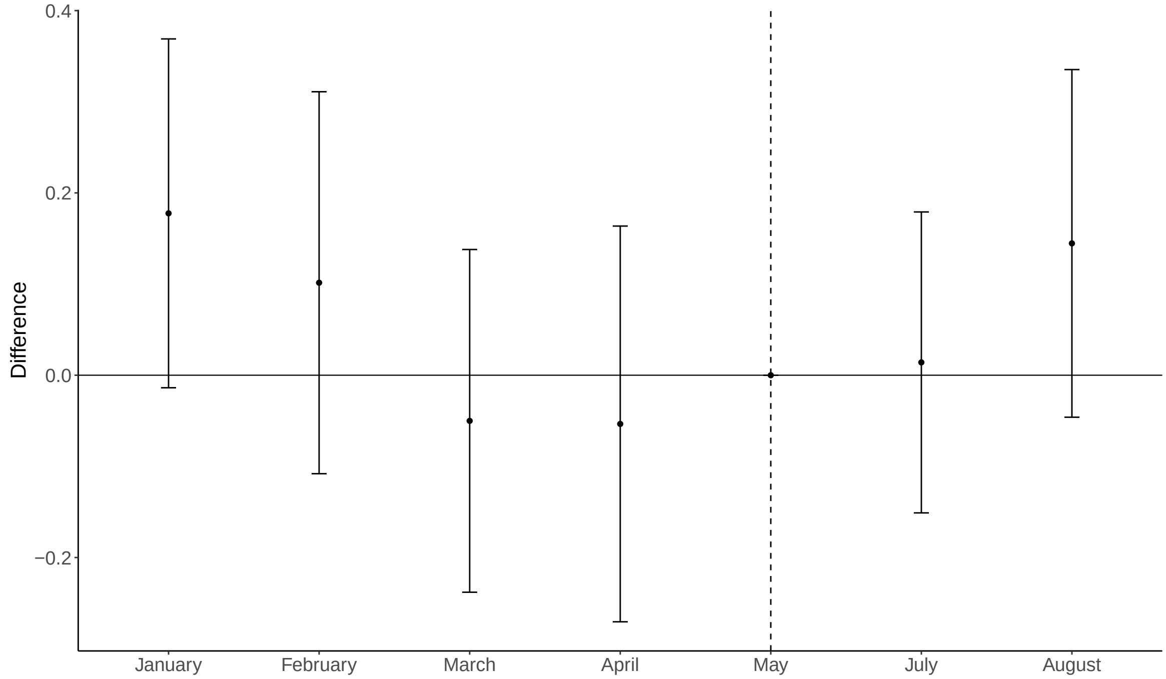


Figure 1.13: Chapter 13 filings in metro areas event study graphs for the Poisson regression with and without covariates. Panel (a) gives the coefficients from the regression without covariates and panel (b) gives the coefficients from the regression with covariates. Standard errors are clustered at the state level in both specifications.

(a) Poisson



(b) Poisson with covariates

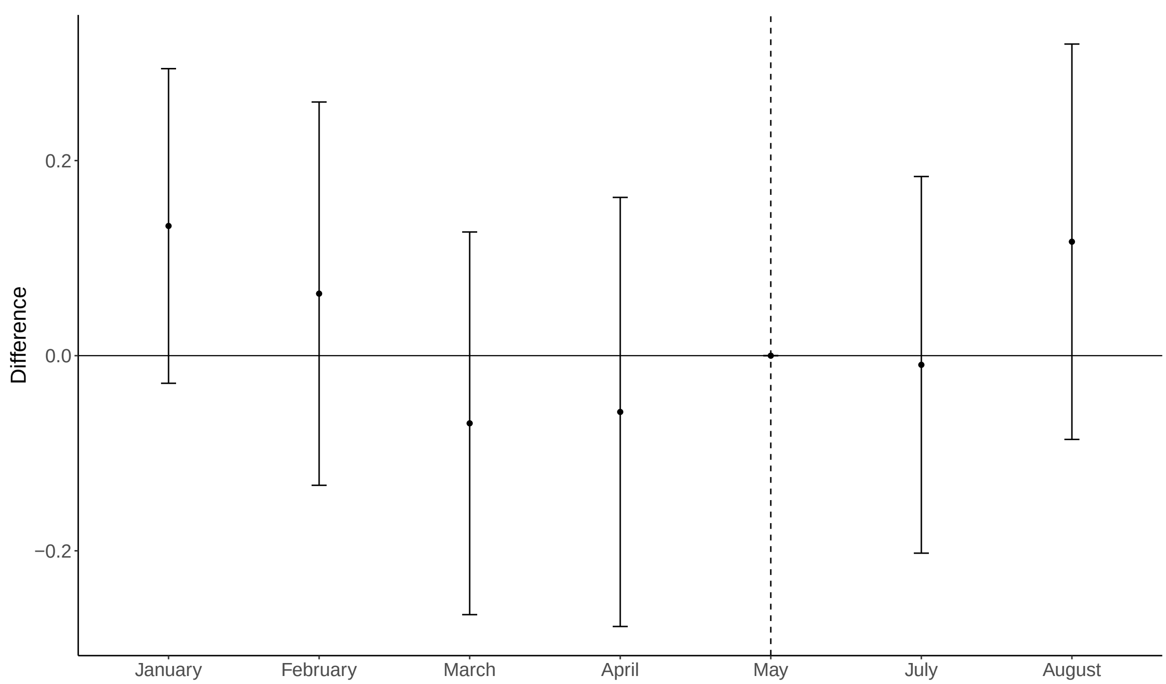


Figure 1.14: Chapter 13 filings in non-metro areas event study graphs for the Poisson regression with and without covariates. Panel (a) gives the coefficients from the regression without covariates and panel (b) gives the coefficients from the regression with covariates. Standard errors are clustered at the state level in both specifications.

	Poisson	Poisson with Controls
Chapter 7	0.037	0.034
	(0.028)	(0.032)
Chapter 13	0.103***	0.089**
	(0.027)	(0.031)
Number of Observations	21,344	21,344
Month Fixed Effects	X	X
County Fixed Effects	X	
State Fixed Effects		X
Clustered Std. Errors	State	State

+ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 1.2: Results for full sample including June. Column 1 gives the Poisson results with no covariates and Column 2 adds covariates. Standard errors are clustered by state in all specifications.

	Poisson	Poisson with Controls
Chapter 7	0.055+	0.048
	(0.030)	(0.036)
Chapter 13	0.140***	0.133***
	(0.029)	(0.030)
Number of Observations	18,676	18,676
Month Fixed Effects	X	X
County Fixed Effects	X	
State Fixed Effects		X
Clustered Std. Errors	State	State

+ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 1.3: Results for full sample. Column 1 gives the Poisson results with no covariates and Column 2 adds covariates. Standard errors are clustered by state in all specifications.

	Poisson	Poisson with Controls
Chapter 13	0.125***	0.143***
	(0.036)	(0.040)
Number of Observations	18,676	18,676
Month Fixed Effects	X	X
County Fixed Effects	X	
State Fixed Effects		X
Clustered Std. Errors	State	State

+ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 1.4: Results for individual filings in common law states. Column 1 gives the Poisson results with no covariates and Column 2 adds covariates. Standard errors are clustered by state in all specifications.

	Poisson	Poisson with Controls
Chapter 13	0.193**	0.141*
	(0.059)	(0.058)
Number of Observations	18,676	17,682
Month Fixed Effects	X	X
County Fixed Effects	X	
State Fixed Effects		X
Clustered Std. Errors	State	State

+ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 1.5: Results for filings with current monthly income below state garnishment income cutoffs. Column 1 gives the Poisson results with no covariates and Column 2 adds covariates. Standard errors are clustered by state in all specifications.

	Poisson (Metro)	Poisson with Controls (Metro)	Poisson (Non-Metro)	Poisson with Controls (Non-Metro)
Chapter 13	0.144***	0.141***	0.051	0.061
	(0.026)	(0.033)	(0.065)	(0.080)
Number of Observations	6,601	6,601	12,075	12,075
Month Fixed Effects	X	X	X	X
County Fixed Effects	X		X	
State Fixed Effects		X		X
Clustered Std. Errors	State	State	State	State

+ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 1.6: Results for individual filings in metro and non-metro areas. Columns 1 and 2 give the results for metro areas and Columns 3 and 4 give the results for non-metro areas. Columns 1 and 3 are results from the Poisson model with no covariates and Columns 2 and 4 are results from the Poisson model with covariates. Standard errors are clustered by state in all specifications.

Chapter 2

The Effect of Affordable Care Act Medicaid Expansions on Household Composition

2.1 Introduction

Political and social interest in housing issues has increased in the United States as growth in rents and home prices has outpaced wage growth in real terms. From 1960 to 2016, the median renter's (homeowner's) income grew by only 5% (50%) while the median rent (home value) rose by 61% (112%) after adjusting for inflation [35]. These trends have led to many individuals spending larger shares of their income on housing. This is concerning since households with burdensome housing costs often have lower levels of liquid savings, making them vulnerable to material hardship such as loss of housing, food insecurity, or the inability to afford utilities or medical care [36, 37].

These housing-burdened households are also susceptible to housing instability and sudden changes in household composition. Financial shocks can lead to individuals being forced out of their current housing situation, such as through an eviction or foreclosure [38]. Additionally, current programs aimed at assisting low-income individuals with accessing affordable and stable housing cannot meet demand. For example, due to funding limitations, approximately three in four eligible households do not receive assistance through federal housing subsidy programs [39, 40]. With few affordable or accessible housing options after a forced move, individuals may end up in crowded or unstable living situations [41]. Since housing security is intimately linked with financial security and current demand for housing assistance is not met by housing related programs, it is plausible that programs broadly improving financial security, such as subsidized health insurance programs, could also help improve housing conditions for low-income individuals.

This paper examines whether the expansion of Medicaid under the Affordable Care Act (ACA) impacts urban household composition for newly eligible childless adults in expansion states. Using a staggered adoption difference-in-differences (DiD) design, we leverage state ACA Medicaid expansions from 2014 to 2019 to identify the effect of expanding Medicaid on housing outcomes. These outcomes include the number of individuals living in the household (household size), the number of rooms in the housing unit (number of rooms), household size divided by number of rooms (persons per room), and household size divided by number of bedrooms (persons per bedroom). The expansion extended coverage to childless adults with incomes at or below 138% of the federal poverty level (FPL), so we restrict our sample to adults with no own minor children in the household and less than a high school degree to target this group. Given the higher prevalence of household crowding in urban areas, we also restrict our sample to individuals in metropolitan statistical areas (MSAs) [42].

We find that the ACA Medicaid expansion has significant impacts on the housing outcomes of younger individuals (aged 26 to 39) but not older individuals (aged 40 to 64). For younger individuals, household size declines by 4.2% and number of rooms declines by 1.8%, leading to a reduction in the level of household crowding (decline of

3.1% for persons per bedroom). For older individuals, there are neither significant nor large effects on household size (0.7%), number of rooms (0.3%), or household crowding (-0.2% for persons per bedroom).

There are several reasons why the younger age group experiences larger effects than the older group. First, this group sees larger gains in health insurance coverage as a result of the expansion. In addition, younger individuals potentially have greater housing mobility than older individuals and may therefore be in a better position to change their housing situation after the expansion. Deeper investigation of the reduction in household size for 26 to 39 year olds indicates that the effect is driven by living with fewer extended family members. Due to the repeated cross-sectional rather than panel nature of the data, we cannot tell who is moving in or out of a housing unit. Though we find no evidence that the probability of moving in the past year changes for younger individuals, we do find that this group is less likely to live in the principal city of an MSA (-15.5%) after the Medicaid expansions. This suggests that only the type of moves (i.e., the neighborhoods to which they move, the size of the households and units to which they move, etc.), not the frequency of moves, is impacted by the expansions. This is consistent with younger individuals moving out of larger households and forming or joining smaller ones. Further heterogeneity analyses within the younger group also point to the reduction in household crowding being concentrated among Hispanic individuals (10.0% reduction in persons per bedroom) and people living in higher housing cost areas (4.0% reduction in persons per bedroom), which aligns with previous housing literature [42].

Our results are supported by a body of literature showing the multifaceted impacts of health insurance coverage on a variety of outcomes. In the case of Medicaid, [43] are able to leverage random assignment in the Oregon Health Insurance Experiment to better understand its various impacts. The authors find that after the first year, the participants randomly offered access to Medicaid have higher health care utilization, lower out-of-pocket medical expenditures (both on average and at all nonzero quantiles of the distribution), lower medical debt, and better self-reported physical and mental health compared to those not chosen in the lottery. Many of these results are replicated for the ACA Medicaid expansions. Medicaid through the ACA is shown to affect medical spending by reducing both average out-of-pocket spending and the probability of having a large out-of-pocket bill.[1] The ACA Medicaid expansions are also shown to reduce debt load, increase access to credit, and improve credit scores, all of which also improve an individual's financial position.[2]. Thus, these financial improvements resulting from access to Medicaid could affect housing outcomes both directly through reductions in out-of-pocket medical spending and debt and indirectly through improved credit and changes in expectations about the financial impact of unexpected health shocks on a household's financial stability.

Our paper makes two key contributions. First, we contribute to the nascent literature on the impact of the ACA Medicaid expansions on housing outcomes [51]. While [51] conducts a county-level analysis of the ACA Medicaid expansions on evictions and eviction filings using a dataset constructed from administrative court records, we are able to complement this analysis by providing evidence on the underlying changes in household composition that may be accompanying reductions in formal evictions. By using the American Community Survey, which has a large amount of individual-level data, we are also able to highlight the heterogeneity in impacts based on age, race, and ethnicity. Additionally, given the recent literature on the possible limitations of using administrative eviction court records in research [52], our results provide support for those found in [51] and underscore the importance of using a variety of data sources to shed light on multifaceted changes in housing outcomes.

Second, we contribute to the recent literature on the impact of non-housing poverty programs on housing outcomes [53, 54]. [53] estimate the impact of ACA marketplace insurance subsidies on rent and mortgage delinquencies. The authors find that eligibility

[1] See [44, 45, 46, 47].
[2] See [10, 48, 12, 49, 50].

for subsidies is associated with close to a 25% decline in the delinquency rate, which implies potentially large decreases in evictions and foreclosures. [54] examine the impact of Earned Income Tax Credit (EITC) changes on housing outcomes for single mothers. The authors find that increases in the EITC reduce both doubling-up, which is defined as one or more adults living with the head of household and partner, and household crowding. We contribute to this literature by showing the first evidence of the impact of Medicaid eligibility on household composition. Our paper helps to strengthen the evidence on the link between financial stability and housing outcomes and further illustrates the importance of understanding how non-housing programs can impact housing for low-income individuals.

The rest of the paper is structured as follows. In Section 2.2, we describe the ACA Medicaid expansions and the novelty of our sample and outcomes. In Section 2.3, we detail our data and sample. In Section 2.4, we describe our empirical strategy. In Section 2.5, we discuss results. In Section 2.6, we conclude.

2.2 Background

2.2.1 Medicaid and the ACA

Medicaid is a public health insurance program in the United States that states and the federal government fund jointly. It was created in 1965 under Title XIX of the Social Security Act to expand access to health care for individuals who lack the resources to secure health insurance or afford medical expenses on their own [55]. For example, these groups include elderly people, disabled people, and low-income earners.

The federal government sets national requirements for Medicaid coverage, and each state implements its own Medicaid program that must adhere to these minimum standards. Since states can choose to cover services or groups beyond the national requirements, there is some variation by state in which services and people are covered. The groups for whom the federal government mandates coverage include "some low-income people, families and children, pregnant women, the elderly, and people with disabilities," according to the U.S. Department of Health & Human Services, though the full list is more extensive [56]. For the purpose of this paper, it is important to note that the federal government did not require states to cover low-income childless adults before the ACA, and therefore most Medicaid programs did not cover this group until the ACA.

Under the ACA, which was signed into law on March 23, 2010, Medicaid was expanded to include almost all adults under age 65 with incomes at or below 138% of the FPL beginning January 1, 2014.[3] [4] However, the 2012 Supreme Court ruling of National Federation of Independent Business v. Sebelius made the Medicaid expansion optional for states. Therefore, adults under 65 with incomes at or below 138% of the FPL were eligible for Medicaid in states that expanded under the ACA but were not eligible in states that opted out.[5]

The 2012 Supreme Court ruling created variation in the timing of Medicaid adoption, which is detailed in Appendix Table A.1. To date, 38 states and DC have expanded Medicaid coverage to childless adults with household income up to 138% of the FPL. From 2013 to 2020, enrollment in Medicaid and the Children's Health Insurance Program (CHIP) increased by 32.3% of the baseline among 49 reporting states [58]. As of November 2020, 72.2 million (or about one in five) individuals in the U.S. were enrolled in Medicaid [59].

Importantly, the federal government covered nearly all the costs of the ACA Medicaid expansion. Though Medicaid is funded jointly by federal and state governments, the

[3] The actual text of the ACA sets the eligibility threshold at 133% of the FPL. However, the ACA also defined a new measure of income called the modified adjusted gross income, or MAGI. This new measure makes the effective limit 138%.

[4] For a family with only one person, the 2021 FPL is $12,880. For a family with two people, the 2021 FPL is $17,420.

[5] Although the ACA Medicaid expansions also impacted the eligibility threshold for low-income parents with dependent children, we only focus on childless adults in this paper. Every state had a Medicaid eligibility threshold for adults with dependent children above 0% of the FPL before 2014, and previous research has shown the effect of the ACA Medicaid expansion on health insurance coverage was much smaller for adults with dependent children compared to childless adults [57].

federal government covered 100% of the cost of newly eligible individuals from 2014 through 2016. Starting in 2017, the rate dropped slightly each year. For 2020 and beyond, the rate levels off at 90%. Typically, the federal government pays about 57% of Medicaid's costs, so the coverage of the expansion is quite high relative to the usual coverage rate [60]. In addition, prior work has found no evidence of the expansion leading to reductions in state spending on other programs, such as "education, corrections, transportation, or public assistance" [61].

2.2.2 Childless adults and housing outcomes

The extension of Medicaid to low-income childless adults is unique, given that subsidized health insurance programs in the U.S have not historically targeted this group [62, 63]. Some of the largest health insurance programs such as Medicare and CHIP target children, elderly people, and disabled people. In all states, there was some amount of Medicaid eligibility for adults with children before the ACA, but eligibility for childless adults was scarcer. It is thus not surprising that childless adults constituted both the majority of uninsured adults prior to the ACA expansion[6] and the majority of adults gaining insurance coverage from 2010 to 2019.[7] We know relatively little about how publicly provided health insurance impacts low-income childless adults since they were previously excluded from Medicaid access in most states. In addition, understanding how access to Medicaid impacts this group is important given the growing interest in placing work requirements on who is able to access Medicaid [65].

Our housing outcomes attempt to assess the overall living situations of households in our sample. Household crowding is associated with a number of negative mental health outcomes including depression, social withdrawal, hostility, and aggressive behavior [66, 67, 68]. It is also associated with increases in infectious diseases [69], which is especially

[6] We estimate that in 2010, childless individuals constituted 65% of all uninsured 26 to 64 year olds.

[7] Using methods similar to [64], we estimate that around 11.4 million adults aged 26 to 64 gained insurance coverage from 2010 to 2015, with 69% of the increase coming from childless adults.

salient in light of the COVID-19 pandemic [70]. Crowding is typically defined in the literature as exceeding a threshold of people to rooms, such as having more household occupants than the number of rooms or more than two occupants per bedroom [71, 72]. While these definitions correlate with poor housing outcomes, each definition is bound by an arbitrary threshold that treats crowding as dichotomous as opposed to continuous [73]. The main benefit of binary measures is their ease of interpretation since households are definitively either crowded or not, while the downside is that they fail to capture many poor housing outcomes that do not fit their exact definition. We briefly look at three common binary measures (persons per room exceeding one, persons per bedroom exceeding two, being in a doubled-up household) in Section 2.5.5 to compare to the literature but find they provide rather imprecise results. We instead focus on the level or degree of household crowding by using the outcomes of persons per room and persons per bedroom and argue that looking at these outcomes helps us to better understand the nuance of an individual's housing situation.

Another strength of our household composition outcomes is that they allow us to capture effects that are often overlooked in housing studies that use more formal housing outcomes. For example, [51] find that the ACA Medicaid expansion reduces both county-level evictions and eviction filings but note that a major limitation of their study is that their data only measure formal evictions. The authors therefore can say nothing about informal evictions, and recent work has found the number of informal evictions to be large. In the Milwaukee Area Renters Study, 48% of all forced moves are informal evictions, and only 24% are formal evictions [74]. Thus, while eviction is an important outcome, more work is needed to assess the impact of the expansion on changes in housing outcomes initiated through informal channels.

Our paper helps to fill this gap, especially through our investigation of changes in household size. Many individuals experiencing housing instability end up living with family members. According to the 2017 American Housing Survey (AHS), if faced with an eviction (foreclosure) 25.6% of renter respondents (32.7% of owner respondents) would move to a family member's home, 8.8% (7.9%) would move to a friend's home, and 5.2% (2.3%) would either have to split up their household or go to a homeless shelter [75]. Given that the majority of the decrease in household size we find for 26 to 39 year olds is driven by individuals living with fewer family members, we believe that we are identifying important changes in household composition that are unobserved when only looking at severe outcomes like formal evictions. Living with family members is not inherently a poor housing outcome, and cultural norms play an important role in determining preferences for household composition. However, the statistics from the AHS show that for many individuals, living with family members can be indicative or predictive of housing instability. Rather than attempting to identify housing instability with formal outcomes, our study is able to capture changes in housing outcomes regardless of whether these changes occur through formal or informal channels. This allows for a more nuanced and well-rounded understanding of housing changes for this group of people.

2.3 Data and sample restrictions

2.3.1 Data sources

Our main source of data is the 2012 to 2019 American Community Survey (ACS), accessed via IPUMS [76]. The ACS is an annual, cross-sectional survey that elicits information from about 3.5 million U.S. households each year. All of the outcome variables, such as household size, number of rooms, persons per room, and persons per bedroom, are created using the ACS' detailed information about household members and housing structure. Additionally, we use the ACS to obtain individual-level demographic characteristics including age, race, Hispanic heritage, sex, education, and presence of difficulties.[8] We

[8]The ACS asks respondents whether they have seven different types of difficulties: cognitive, ambulatory, independent living, self-care, vision, and hearing. We separately control for each type of difficulty.

can also identify the state and MSA in which individuals reside at the time of the survey.

We use data from several sources to control for economic and housing characteristics in our regression analysis. Data from the University of Kentucky Center for Poverty Research's National Welfare Data provide information on state-year characteristics, such as the state unemployment rate, state EITC rate, state minimum wage, and state political characteristics, including the governor's political affiliation and the shares of Democrats in state legislative bodies.[9] We also merge in data from Zillow on the Zillow Home Value Index (ZHVI) to control for MSA-level median and bottom-tier home values and the Zillow Rent Index (ZRI) to control for MSA-level median rental values.[10] Finally, we use Medicaid expansion data from the Kaiser Family Foundation to identify state Medicaid expansion dates as well as Medicaid eligibility criteria.

2.3.2 Sample restrictions

Our sample restrictions focus on creating a targeted group of low-income childless adults that were likely to be newly eligible for Medicaid under the expansions and to experience household crowding. We refine our sample to include only U.S. citizens aged 26 to 64 with no own minor children in the household, no receipt of supplemental security income, and not living in group quarters.[11] We also only include individuals with less than a high school degree. Restricting on education is preferable to restricting on family

[9] State legislature political affiliation data are available for all states except Nebraska. Since the passage of a constitutional amendment in 1935, Nebraska has had a unicameral, nonpartisan state legislature. However, we are able to obtain the political affiliation of Nebraska's legislators through the state's historical Blue Books, public political party endorsements, and news articles.

[10] From Zillow's website, the ZHVI "reflects the typical value for homes in the 35th to 65th percentile range," the bottom-tier measures "the typical value for homes that fall within the 5th to 35th percentile range," and the ZRI is the "mean of rent zestimates between the 40th to 60th percentiles" [77]. More details on our Zillow controls can be found in Appendix A.3.

[11] We exclude individuals under age 26 due to their ability to stay on their parents' health insurance plans as a provision of the ACA, and we exclude those over age 64 due to their Medicare eligibility. We restrict on citizenship because the ACS does not break non-citizenship into different types, and only some non-citizens are eligible for Medicaid. SSI was an existing qualifier for Medicaid. The housing structure for people living in group quarters is not easily comparable to the majority of our sample. Groups quarters, as defined by the U.S. Census Bureau, are any housing units other than a house, apartment, mobile home, or rented room. Thus, nursing homes, college dormitories, correctional facilities, military barracks, rooming houses, etc. are classified as group quarters.

income and poverty status because income is potentially endogenous and educational status is highly correlated with income.[12] We also restrict our sample to individuals living in MSAs because crowding is more prominent in urban areas [42].[13]

To understand the difference between people included in the sample based on these restrictions and those excluded from it, we present summary statistics in Appendix Table A.2 for 2012 to 2013. Individuals in our sample are less likely to have insurance coverage before the expansion and more likely to have family income below the ACA Medicaid eligibility threshold. They are also less likely to be employed and have lower individual income on average. Additionally, they are more likely to live in rented housing and live with their parents. Thus, these sample restrictions target a group of individuals likely to benefit from the ACA Medicaid expansion and to have relatively more mutable housing conditions.

We use 37 states in our analysis, including 24 treatment states and 13 control states. They are listed in Table 2.1 and shown in Figure 2.1. Our treatment states are those that expanded the Medicaid eligibility threshold for childless adults from 0 to 138% of the FPL between January 1, 2014 and December 31, 2019. Our control states are those that had not expanded Medicaid to childless adults by December 31, 2019.[14]

[12]Other papers focusing on the ACA Medicaid expansion also use education, as opposed to income, as a sample restriction. For example, [57] and [78] both study individuals with a high school degree or less to examine employment outcomes, while [79] study the union of individuals who have less than a high school degree and individuals with family income below 138% of the FPL.

[13]In the ACS, the lowest level of geography reported is the Public Use Microdata Area (PUMA). Because of this, there is some match error that arises when IPUMS relates PUMAs to MSAs. This results in the suppression of some MSA codes for MSAs with a high match error. IPUMS only reports MSA codes when the match error is no more than 15%.

[14]The 13 states and DC that do not end up in our final sample are dropped for the following three reasons. 1. To ensure a clean empirical strategy, we do not include in our sample the eights states and DC that expanded Medicaid but had an eligibility threshold above 0% of the FPL for childless adults before January 1, 2014. 2. The restriction of using MSAs forces one treatment state (Montana) and two control states (South Dakota and Wyoming) to fall from our sample due to lack of data. 3. Wisconsin and Utah are dropped from the sample due to their idiosyncratic Medicaid provision, as detailed in Table A.1.

2.4 Empirical strategy

Our main analysis focuses on a staggered adoption DiD specification. To uncover the effect of the ACA Medicaid expansion on the housing outcomes of low-income childless adults, we estimate a regression of the following form:

$$Y_{i,s,m,y} = \beta Medicaid_{s,y} + X'_{i,s,m,y}\gamma + \delta_m + \eta_y + \varepsilon_{i,s,m,y} \tag{2.1}$$

where $Y_{i,s,m,y}$ is an outcome for individual i in state s, MSA m, and year y. The vector $X_{i,s,m,y}$ contains the individual level controls for age, sex, race, Hispanic heritage, and difficulties. The vector also contains state-year characteristics including the state unemployment rate, the state EITC contribution above the federal level, the log of the state minimum wage, the governor's political affiliation, and the share of Democrats in the state's legislative bodies. Additionally, the vector contains the log of the MSA-year ZHVI median and bottom-tier home values and the log of the MSA-year ZRI median rental value. The variable δ_m captures MSA fixed effects. The variable η_y captures calendar year fixed effects.

$Medicaid_{s,y}$ is a continuous variable between zero and one. It takes the value zero for the control states, which do not expand Medicaid to low-income childless adults through the ACA during our sample period. For states that expanded on January 1st of a given year, the variable takes the value one in the expansion year and every year after. For states that expanded for a fractional amount of their initial year, $Medicaid_{s,y}$ expresses the fractional amount of that year with coverage and takes the value one every year after (e.g., Alaska expanded provision on September 1, 2015, so it had coverage for one third of 2015). Given that the ACS surveys households randomly throughout the year, it can be thought of as the probability that Medicaid was expanded in a household's state at the time of the survey. The coefficient β captures the average causal effect of an entire year of Medicaid eligibility on housing outcomes if the identifying assumptions for a DiD analysis hold. It is also important to note that β is the intent to treat effect for Medicaid coverage since take-up is not universal.

To estimate equation 2.1, we employ a weighted least squares (WLS) regression using the square root of the IPUMS person weight and use robust standard errors clustered by state.[15] Research has shown that conducting inference with a small number of clusters can lead to the nominal size of a statistical test differing from the actual size, and 30 clusters is often cited as the threshold below which there is reason for concern. Given that we are close to this threshold, we also conduct inference using the wild cluster bootstrap method with 999 replications and Rademacher weights [80]. Since we have 37 clusters, we consider the wild bootstrap to be a slightly more conservative approach. Reassuringly, the p-values from clustering or using the bootstrap method are typically very similar for our analyses, and we only note differences in significance when applicable.

We estimate all results separately for 26 to 39 year olds and 40 to 64 year olds. The summary statistics comparing the two groups are found in Table 2.2. There are several reasons why we might expect heterogeneous effects by age. First, younger people have lower baseline insurance coverage and thus experience a higher percentage change in insurance coverage. Next, 26 to 39 year olds have a higher baseline level of household crowding, are more likely to have moved in the past year and are more likely to live in rented housing compared to the older group. Finally, [48] show that financial improvements due to the Medicaid expansions are strongest for younger people (44 and under). These reasons inform our decision to estimate the results separately by age group. Our age group cutoff is the same as the one used in [49]. However, given that the age group cutoff is somewhat arbitrary, in Appendix Table A.3 we instead use age groups of 26 to 44 and 45 to 64 as in [48]. This leads to qualitatively similar results for our main analysis.

The main identifying assumption for our empirical strategy is that, in absence of

[15]The person weight indicates how many individuals in the U.S. population are represented by a given person in the sample. Thus, we employ analytical weights, which use the square root of this number as the weight in a WLS regression, to both make the estimates representative of the population and correct for heteroskedasticity.

treatment, trends in the outcomes would evolve in parallel for treatment and control. We test whether pre-treatment trends are parallel with an event study regression of the following form, that is also estimated separately by age group:

$$Y_{i,s,m,y} = \sum_{t,t\neq -1} \beta_t Medicaid_{s,t} + X'_{i,s,m,y}\gamma + \delta_m + \eta_y + \varepsilon_{i,s,m,y} \tag{2.2}$$

where $Medicaid_{s,t}$ is a continuous variable between zero and one that expresses the share of the year t that state s has ACA Medicaid coverage for low-income childless adults. This summation creates a series of coefficients that tracks the dynamic effects of an entire year of Medicaid coverage. By leaving the variable $Medicaid_{s,-1}$ out of the estimation, each coefficient β_t identifies the average effect of treatment in year t, relative to the year before adoption. Since most of the states in our sample adopt between 2014 and 2016, the pre-treatment event study coefficients that can be estimated for a majority of the treatment states are $\widehat{\beta}_{-2}$, $\widehat{\beta}_{-3}$, and $\widehat{\beta}_{-4}$.[16]

To test the parallel trends assumption, we estimate equation 2.2 using the same estimation and inference strategy mentioned above. We plot the coefficients $\widehat{\beta}_{-4}$ through $\widehat{\beta}_{5}$ in Figure 2.2 for the first stage insurance coverage outcomes and in Figures 2.3 and 2.4 for the main housing outcomes. The pre-trends test consists of testing whether ther coefficients $\widehat{\beta}_{-2}$, $\widehat{\beta}_{-3}$, and $\widehat{\beta}_{-4}$ jointly equal zero.[17] There is no strong evidence against parallel pre-trends for any of the outcomes for 26 to 39 year olds. For 40 to 64 year olds, there is evidence that parallel pre-trends are violated for persons per room and persons per bedroom, so we proceed with caution in interpreting these two particular results as causal.

[16] The remaining pre-treatment time periods are pooled into the variable $Medicaid_{s,\leq -5}$, and the estimate $\widehat{\beta}_{\leq -5}$ is not reported in the event study figures.

[17] We do not use ACS data further back than 2012 due to changes in official geographic boundaries. Every ten years geographic boundaries of Public Use Microdata Areas (PUMAs) and MSAs are updated based on the decennial census. The completion of the 2010 census resulted in IPUMS changing which MSAs it reported or suppressed in 2012 due to changes in the correspondence of PUMAs and MSAs, making exact comparison of the geographic areas in our sample before and after 2012 impossible.

In our setting, we require an additional assumption for causal identification. The ACA is a broad policy that includes a bundle of reforms. Most importantly for this analysis, the ACA marketplace and its corresponding subsidies came into effect in 2014 for both treatment and control states. Therefore, the states that adopted Medicaid in 2014 saw both programs begin in the same year. U.S. citizens with family income between 100 and 400% of the FPL are typically eligible for subsidies if they do not have access to affordable coverage through their employer and are not eligible for other forms of public assistance such as Medicaid, Medicare, or CHIP. Thus, marketplace subsidies affect eligible individuals with family income from 138 to 400% of the FPL in treatment states and 100 to 400% of the FPL in control states. Since we do not restrict our sample based on income, there are low-income childless adults in our sample who might be impacted by the subsidies. This means that even if pre-trends are parallel before 2014, the parallel trends assumption would not be met if the introduction of marketplace subsidies impacts the outcomes differentially in expansion and non-expansion states in the absence of Medicaid. For our results to isolate the effect of the Medicaid expansion, we must assume that our housing outcomes would have evolved in parallel for both treatment and control states in response to the introduction of marketplace subsidies.

We use individuals with a college education or more to test whether the introduction of the subsidies differentially impacts treatment versus control states. Only 7% of these individuals have income below 138% of the FPL before 2014 while 30% fall in the 138 to 400% range. Thus, they are plausibly more impacted by the subsidies and less impacted by Medicaid than individuals with less than a high school education. Said differently, they serve as a reasonable group for which we can examine the counterfactual scenario of expanding subsidies and not expanding Medicaid. The results in Table A.4 estimate a variant of equation 2.1 where instead of including the variable $Medicaid_s$, we use a variable that equals one in 2014 for our treatment states and zero otherwise. The regressions also only include data from 2012 to 2014. In this case, the coefficient β

captures whether the change in housing outcomes before and after the introduction of marketplace subsidies is different for our treatment states compared to our control states. These results show there is little evidence that the introduction of marketplace subsidies leads to housing outcomes evolving differently in treatment versus control states.

2.5 Results

In this section, we discuss our results. Given that the outcomes are often of different scales, we present most results as percent changes relative to the pre-2014 outcome mean. When the outcome is a binary variable, the regression model is a linear probability model, so the effect can be interpreted as the change in the probability of the outcome occurring.

2.5.1 Impact on insurance coverage

First, we look at the impact of ACA Medicaid expansions on insurance coverage to gain a sense of the first stage effects. While other papers have quantified these effects, it is important to understand the magnitudes for our specific sample. Looking to Table 2.3, we show the DiD estimates using binary indicators for insurance coverage as the outcomes. In column (1), we look at the impact on any health insurance coverage, and in column (2), we look at the impact on government-assisted health insurance coverage, such as Medicaid.[18] The impacts are positive, large, and significant. The probability of any insurance coverage grows by 28.4% for 26 to 39 year olds and 9.7% for 40 to 64 year olds. The impact in percent terms is much larger for the younger group than the older group due to both a lower baseline insurance coverage and a larger percentage point increase. Looking to column (2), it is clear that most of the people in our sample do not

[18]The ACS asks, " Is this person CURRENTLY covered by any of the following types of health insurance or health coverage plans?" including employer-sponsored, privately purchased, Medicare, Medicaid or other governmental, TRICARE, or Veterans Administration-provided insurance. The first outcome equals one if they answer "yes" for any of the listed options and the second outcome equals one only if they answer "yes" for "Medicaid, Medical Assistance, or any kind of government-assistance plan for those with low incomes or a disability."

have Medicaid or other governmental insurance coverage in the pre-period, though some members of our sample are indeed covered by these types of insurance due to targeted yet imperfect sample restrictions. Government-assisted health insurance coverage grows a great deal as a consequence of the policy—by 86.5% for younger people and 64.9% for older people. The impact on government-assisted coverage is even larger than the effect on any insurance coverage, given a small crowd out of other insurance types and a lower baseline mean. It is important to note that the large increases in government-assisted insurance coverage are also most likely underestimates of the true increase in Medicaid coverage given the well-documented under-reporting of Medicaid coverage in survey data [81] and Medicaid's feature of retroactive coverage.

2.5.2 Main results

We now move to looking at our four main outcomes of interest: the number of individuals living in the household (household size), the number of rooms in the housing unit (number of rooms), household size divided by number of rooms (persons per room), and household size divided by number of bedrooms (persons per bedroom).[19]

Looking at Table 2.4, the estimates in the first two columns show the results for household size and number of rooms, separated into panels by age group. For younger individuals, both household size and number of rooms decrease, though the percent change in household size (-4.2%) is larger than the change in number of rooms (-1.8%). Both results are statistically significant at the 5% level. For older individuals, there are very small, statistically insignificant increases in household size (0.7%) and number of rooms (0.3%). By separating rooms into bedrooms and non-bedrooms in columns (5) and (6) of Table 2.4, it is clear that most of the changes are due to non-bedrooms, which include living spaces such as the living room, kitchen, or family room but not spaces like

[19]Persons per bedroom has a smaller sample size than the other outcomes due to a small number of housing units having zero bedrooms (e.g., studios), thus resulting in a missing value.

hallways or bathrooms.[20] For 26 to 39 year olds, 67% of the reduction in rooms is due to fewer non-bedrooms while the remaining 33% is due to fewer bedrooms.

Next, we look at the results for our household crowding measures, persons per room and persons per bedroom. From the magnitude and sign of the results for household size and number of rooms, it is clear that we should expect persons per room and persons per bedroom to decline for the younger group since the percent decrease in household size (4.2%) exceeds the percent decrease in rooms (1.8%) and bedrooms (1.2%). Looking at columns (3) and (4) of Table 2.4, persons per room declines by 3.6% and persons per bedroom declines by 3.1% for 26 to 39 year olds. The estimate for persons per room is marginally significant with clustered standard errors (cluster p-value 0.094) but loses precision with the bootstrap method (bootstrap p-value of 0.196). The impact on persons per bedroom is significant with a cluster p-value of 0.021 and also maintains marginal significance using the more conservative wild bootstrap with a bootstrap p-value of 0.066. We take these results together as suggestive evidence that household crowding, particularly as measured by persons per bedroom, decreased for younger individuals as a result of the Medicaid expansions. For 40 to 64 year olds, the results for persons per room and persons per bedroom are both economically insignificant with point estimates of -0.5% and -0.2%, respectively, and statistically insignificant as well.

Our main results show that the Medicaid expansions have strong effects on our housing measures for younger individuals but have little impact on older individuals. This is consistent with the results from section 2.5.1, which show that older individuals have smaller gains in health insurance coverage after the Medicaid expansion relative to younger individuals. In addition, younger individuals may be more mobile than older individuals, a point we return to in Section 2.5.3. Given these considerations, the remainder

[20] The exact question from the ACS for "rooms" asks, "How many separate rooms are in this house, apartment, or mobile home? Rooms must be separated by built-in archways or walls that extend out at least 6 inches and go from floor to ceiling. Include bedrooms, kitchens, etc. Exclude bathrooms, porches, balconies, foyers, halls, or unfinished basements." The exact question from the ACS for "bedrooms" asks, "How many of these rooms are bedrooms? Count as bedrooms those rooms you would list if this house, apartment, or mobile home were for sale or rent. If this is an efficiency/studio apartment, print 0."

of the results focus on the 26 to 39 year old group.[21]

2.5.3 Breakdown of household size

We now look deeper into whether the household size effect for 26 to 39 year olds is driven by living with family members versus non-family members. Columns (1) and (2) in Table 2.5 use the number of the individual's family members and non-family members living in the household as the outcome.[22] "Family" has a broad definition in the ACS, so family members can be related to the individual by "blood, marriage/cohabitating partnership, or adoption." The effect is very clearly driven by family members—79% of the household size effect is accounted for by a reduction in the number of family members in the household.

We then break down the composition of the family member effect by the number of immediate and extended family members living in the household. Immediate family members are partners,[23] parents, adult children, and siblings. These results are reported in columns (3) and (4) of Table 2.5. About 18% of the reduction in family members comes from immediate family members, while the remaining 82% comes from extended family members. Due to the way relationships are reported in the ACS, it is difficult to identify exactly which types of extended family members are living in the household. So, the results provide evidence that a reduction in living with extended relatives, such as grandparents, aunts, uncles, in-laws, cousins, nieces, nephews, etc., accounts for most of the effect on household size, though we cannot diagnose whether the effect is being driven by a specific type of extended relative. One way we can break down the family member reduction is through age. The results in Table 2.6 indicate that about 30% of the family member effect is coming from minor family members (aged 17 or younger), 50%

[21]The interested reader can find the companion results for the 40 to 64 year old group in the Appendix.

[22]Note that household size is defined as one plus the sum of the number of family members and the number of non-family members living in the housing unit. This is because the categories of family members and non-family members do not include the individual, who also lives in the household.

[23]In the ACS, this includes marriage and cohabitation for both opposite- and same-sex couples

is coming from adult family members (aged 18 to 64), and 20% is coming from senior family members (aged 65 or older).

Given that the ACS is a repeated cross-section, it is not possible to determine whether younger individuals are moving out of the home where the extended family members live or whether the extended family members are moving out. However, we can look at whether there is a change in the probability of moving after the expansion of Medicaid. Column (1) of Table 2.7 shows that there is a small and insignificant increase in the probability of moving residences in the past year (2.8%). Thus, we find little evidence that the probability or rate of moving in the past year changes due to the policy, even though individuals are on average living in households with fewer housemates and rooms. One way to explain this result is that the types of moves they are making change due to the policy. While the geographic identifiers in the ACS are too coarse to identify changes in neighborhood characteristics for movers, we can look at whether individuals are more or less likely to live in the principal city of an MSA after the expansion of Medicaid.[24] Column (2) of Table 2.7 indicates that 26 to 39 year olds are 15.5% less likely to live in the principal city of an MSA after the expansion, suggesting that the location choices of movers change. While more work needs to be done to identify the mechanisms behind these changes, our results are consistent with individuals experiencing greater freedom in housing choice due to the benefits of the Medicaid expansion.

2.5.4 Heterogeneity in effects

Finally, we investigate whether there is heterogeneity in the main effects by demographic factors and characteristics of the local housing market for 26 to 39 year olds. In Table 2.8, we break down results by whether the individual is White, Black, or Hispanic. In Table 2.9, we break down results by whether the individual lives in an MSA with

[24]The principal city of an MSA is the largest city in the metropolitan area. Additional cities can qualify if they meet population and employment criteria. As an example, the principal city of the Los Angeles-Long Beach-Anaheim, CA is Los Angeles, CA.

above- or below-median housing values. We use the ZHVI to determine above- versus below-median MSAs. Reports using the AHS have found that households with Black or Hispanic householders and households residing in high cost of living areas are more likely to experience household crowding or doubling-up [42, 82].

For 26 to 39 year olds, it is clear that the reduction in household size is largest for Hispanic individuals (9.9%) compared to White (4.8%) or Black (1.8%) individuals. Additionally, Hispanic individuals have large reductions in both crowding measures that are much larger than those for White and Black individuals. They see a 10.6% reduction in persons per room and a 10.0% reduction in persons per bedroom.

For the comparison of above- vs. below-median housing cost MSAs, the results are more nuanced. The above-median housing cost MSAs experience slightly smaller decreases in household size (-2.9%) and number of rooms (-1.1%) compared to the reductions in those outcomes in below-median housing cost MSAs (-6.1% and -2.6%, respectively). In terms of crowding, though, the above-median MSAs experience a larger reduction in persons per room (-4.1%) and persons per bedroom (-4.0%) than in the below-median MSAs (-2.8% and -2.3%, respectively), though only the result for persons per bedroom in above-median MSAs is statistically significant. These results can be explained by the fact that household crowding is a more prevalent issue in MSAs where housing is more expensive.

2.5.5 Robustness checks

We test the robustness of our main results for 26 to 39 year olds to various iterations of the empirical strategy. First, we check whether the results are robust to the inclusion or exclusion of different covariates in Table 2.10. Compared to the main results with all covariates in column (4), the results in columns (1) through (3) show that the results are very stable regardless of which covariates are used.

Next, we check the robustness of the main results for 26 to 39 year olds to different fixed effects in columns (1) and (2) of Table 2.11. Since some MSAs cross state borders, in column (1) we check the robustness of the results to MSA-by-state fixed effects. This means that for the small number of MSAs that cross borders, there is a fixed effect for each MSA-state combination.[25] Additionally, in column (2), we check that our results hold when we remove all cross-border MSAs from the sample. None of the results qualitatively change for either variant.

We check three assorted specification concerns in columns (3) through (5) of Table 2.11. We first address the potential non-randomness of treatment assignment. We follow [83] and restrict the sample to states with a predicted probability of treatment within the range of [0.1,0.9]. The predicted probability is found by estimating a logit regression of a binary indicator for being treated by the end of the sample period as a function of the 2012 state-level covariates included in equation 2.1. The estimates in column (3) show the results are robust to this restriction, indicating that potentially non-random treatment assignment is not substantially affecting these results. The second possible concern is the introduction of Medicaid work requirements. The only state that has implemented a work requirement and actually rescinded Medicaid coverage due to the requirement was Arkansas in 2018 [84]. Column (4) shows that results are robust to dropping Arkansas from the sample. Finally, we use a binary, as opposed to continuous, treatment variable that rounds a state's treatment assignment to the closest year if treatment began after January 1st.[26] Results in column (5) remain consistent with the main analysis.

Next, we investigate the potential issue of negative weights in our empirical strategy. A growing literature has pointed out issues with negative weights biasing the estimated treatment effect when using two-way fixed effects models with variation in treatment timing and heterogeneous treatment effects.[27] In the most severe cases, the presence of

[25]Out of the 211 MSAs in our sample, 19 cross a state border. For example, both the Missouri part and the Illinois part of the St. Louis MSA would receive their own fixed effect with this strategy.

[26]For example, since Alaska adopted on September 1, 2015, its treatment variable is zero for 2015 and one for 2016 in the binary specification.

[27]For a discussion of these issues specifically within staggered adoption designs, see [85].

negative weights could lead the two-way fixed effects estimator to have the opposite sign of the true effect. While many policies featuring staggered adoption are highly staggered with few groups adopting in any one time period (e.g., state minimum wage changes), our scenario features 18 of the 24 treatment states adopting in 2014 and could be described as "barely" staggered. To isolate a scenario free from negative weights and their associated issues, we exclude the 6 later adopting treatment states, re-estimate our main results using a non-staggered DiD design, and compare it to our main results from equation 2.1.[28]

Column (6) of Table 2.11 reports the results only using 2014 adopters. Given that there is no fundamental difference in the direction or magnitude of the results compared to our main analysis, we are not concerned that negative weights due to variation in treatment timing are significantly influencing our results.

Finally, we look at the results for household crowding using binary variables as opposed to the level variables used in our main analysis. We also look at the effect on another related housing measure, doubling-up. These three variables are an indicator for persons per room exceeding one, an indicator for persons per bedroom exceeding two, and an indicator for living in a "doubled-up" household.[29] All three yield insignificant results as seen in Table 2.12. The point estimates for persons per room exceeding one and persons per bedroom exceeding two are -1.0% and -7.7%, respectively. The point estimate for doubling-up is quite small in percent terms (-0.6%). We do not have enough precision to make conclusions about whether there are definitive reductions in the likelihood of experiencing household crowding in the right tail of the distribution or being doubled-up, but we can rule out increases in the three outcomes beyond 20.6%, 13.5%, and 3.0% ,

[28]One reason we use this strategy as opposed to recently proposed ones is because 1. our regression model is at the individual-level as opposed to the group-level and 2. it also includes sample weights. The currently recommended tools that identify the severity of this issue and offer alternative estimators do not perfectly translate to our setting due to these two factors, so we consider this a cleaner option than one where we would need to aggregate our data.

[29]Though doubling-up has several definitions, using the ACS we define a doubled-up household as one that is either a multifamily household or a household where an adult child lives with a parent.

respectively . While our main results show important changes in household composition, these binary measures do not definitively answer whether there were changes after the expansions. As such, our results highlight important changes in household composition that might not be captured by just using these binary measures in isolation to assess crowding and household composition.

2.6 Discussion and Conclusion

This paper investigates the effect of extending Medicaid eligibility to low-income childless adults on household composition. By employing a staggered adoption DiD design on an urban sample of childless adults with less than a high school degree, we find that younger individuals (aged 26 to 39) live with fewer people in the household, live in housing units with slightly fewer rooms, and experience lower levels of household crowding due to this policy, while older individuals (aged 40 to 64) see no sizable impact on housing outcomes. The reduction in household size for younger individuals is mostly driven by living with fewer extended family members, and the reductions in household crowding are concentrated among Hispanic individuals and people living in above-median housing cost MSAs.

We argue that the changes we estimate occur due to reduced out-of-pocket medical spending and improved financial wellbeing. As one simple comparison, previous studies examining the ACA Medicaid expansions such as [45] have estimated a $382 reduction in annual out-of-pocket medical expenditures alone for families during the 2010 to 2015 time period. Given that the average monthly household-level rent payment for renters in our sample is $812 in 2015 dollars, these out-of-pocket savings are significant. In addition, improvements in credit and debt load as a result of Medicaid could also be helping individuals pass credit check requirements, thereby improving access to different housing options that were previously unattainable. While more work is needed to better understand these mechanisms, our results help to highlight their importance for household composition.

Compared to other studies looking at changes in household size due to policy changes, our estimated effect is large. In [54], a $1,000 increase in EITC benefits leads to a 0.6% reduction in household size for their sample of single mothers in the ACS. Comparatively, we find that household size decreases by 4.2% for 26 to 39 year olds as a consequence of the ACA Medicaid expansions. These results have important policy implications. Our findings, combined with those from [53] and [54] indicate that non-housing targeted policies can have large, positive impacts on individuals' housing outcomes. These may be unaccounted for in many cost-benefit analyses, and future studies should consider these often overlooked benefits.

One limitation of our study is the cross-sectional nature of the data. Since we do not have panel data, we cannot definitively determine whether the changes in household composition and crowding are due to the individual or the housemates moving out. If sufficiently detailed panel data that can be used to examine this question become available, it would be a fruitful avenue for future research. In addition, while examination of the impact of healthcare provision on other housing outcomes, such as homelessness, are beyond the scope of this project, further investigation into these topics could greatly improve our understanding of the extent to which the provision of healthcare improves the most severe housing outcomes.

2.7 Figures and Tables

Year	States
2014	AR, CA, IL, IA, KY, MD, MA, MI, NV, NH, NJ, NM, ND, OH, OR, RI, WA, WV
2015	AK, IN, PA
2016	LA
2019	ME, VA
Had not implemented expansion by December 31, 2019	AL, FL, GA, ID, KS, MS, MO, NE, NC, OK, SC, TN, TX

Notes: States in blue are used as the treatment group. States in orange are used as the control group. Of the control states, ID and NE implemented their Medicaid expansions in 2020, and MO and OK have plans to implement their expansions in 2021.

Table 2.1: List of the 37 Treatment and Control States Used in Analysis, by Year of Policy Adoption

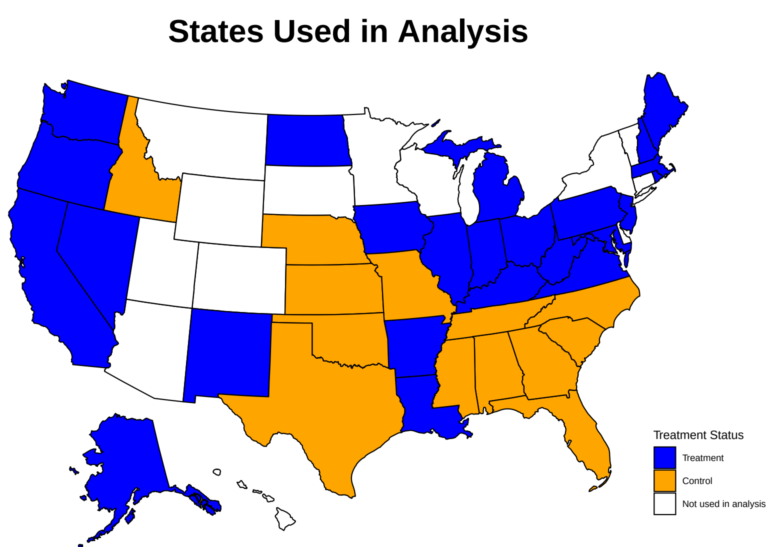
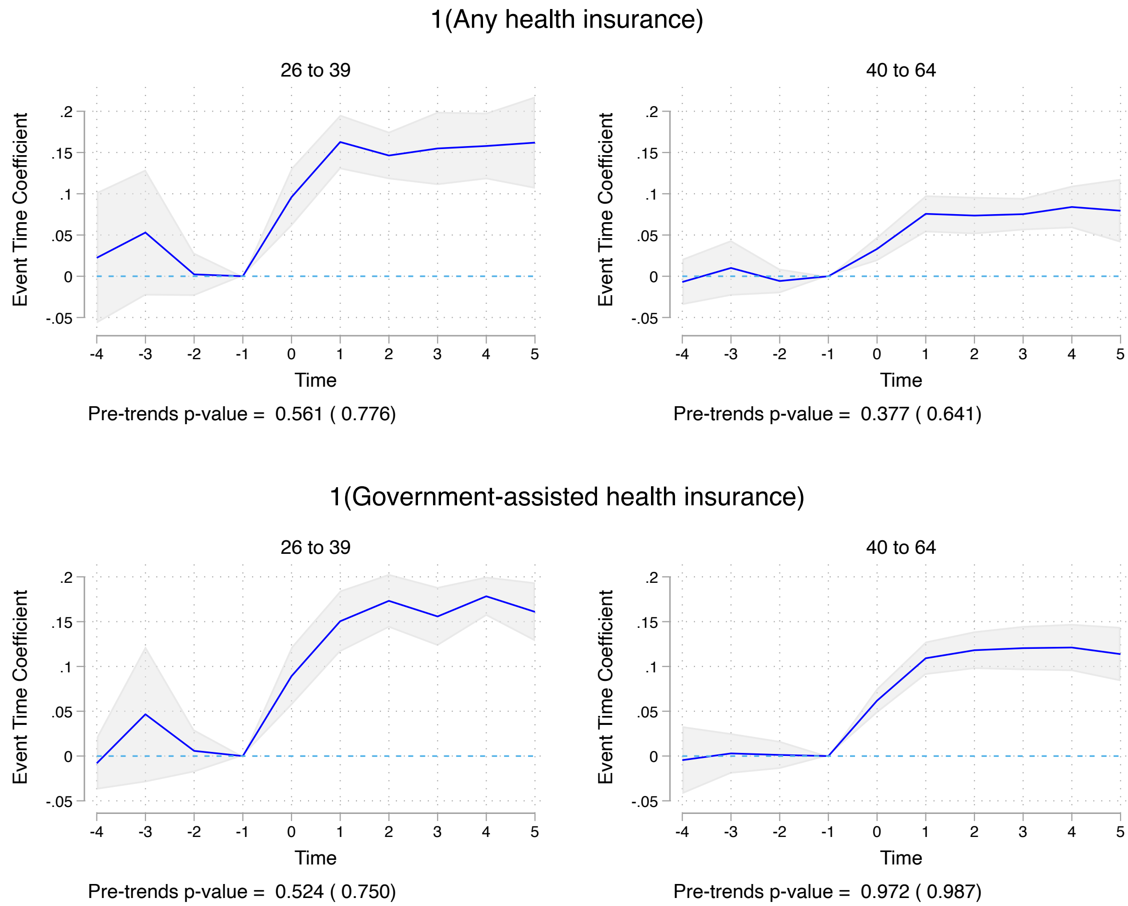


Figure 2.1: Map of the 37 Treatment and Control States Used in Analysis

	26 to 39	40 to 64	P-value of diff.
Any insurance	0.46	0.66	0.00
	(0.50)	(0.47)	
Between 0-138% FPL	0.37	0.32	0.00
	(0.48)	(0.47)	
Household size	3.41	2.86	0.00
	(2.07)	(1.68)	
Number of rooms	5.49	5.55	0.06
	(2.12)	(2.03)	
Persons per room	0.67	0.56	0.00
	(0.57)	(0.38)	
Persons per bedroom	1.31	1.11	0.00
	(0.76)	(0.64)	
Moved in past year	0.21	0.12	0.00
	(0.41)	(0.32)	
Lives in rented housing	0.53	0.38	0.00
	(0.50)	(0.49)	
Lives with parent(s)	0.41	0.10	0.00
	(0.49)	(0.31)	
Has a difficulty	0.15	0.25	0.00
	(0.36)	(0.44)	
Employed	0.54	0.52	0.02
	(0.50)	(0.50)	
Married	0.21	0.55	0.00
	(0.41)	(0.50)	
Female	0.30	0.48	0.00
	(0.46)	(0.50)	
White	0.60	0.64	0.01
	(0.49)	(0.48)	
Black	0.24	0.19	0.00
	(0.43)	(0.39)	
Hispanic	0.28	0.26	0.01
	(0.45)	(0.44)	
Total individual income	17,554.54	22,972.88	0.00
	(25,732.35)	(33,202.60)	
Sample size	12,257	61,259	

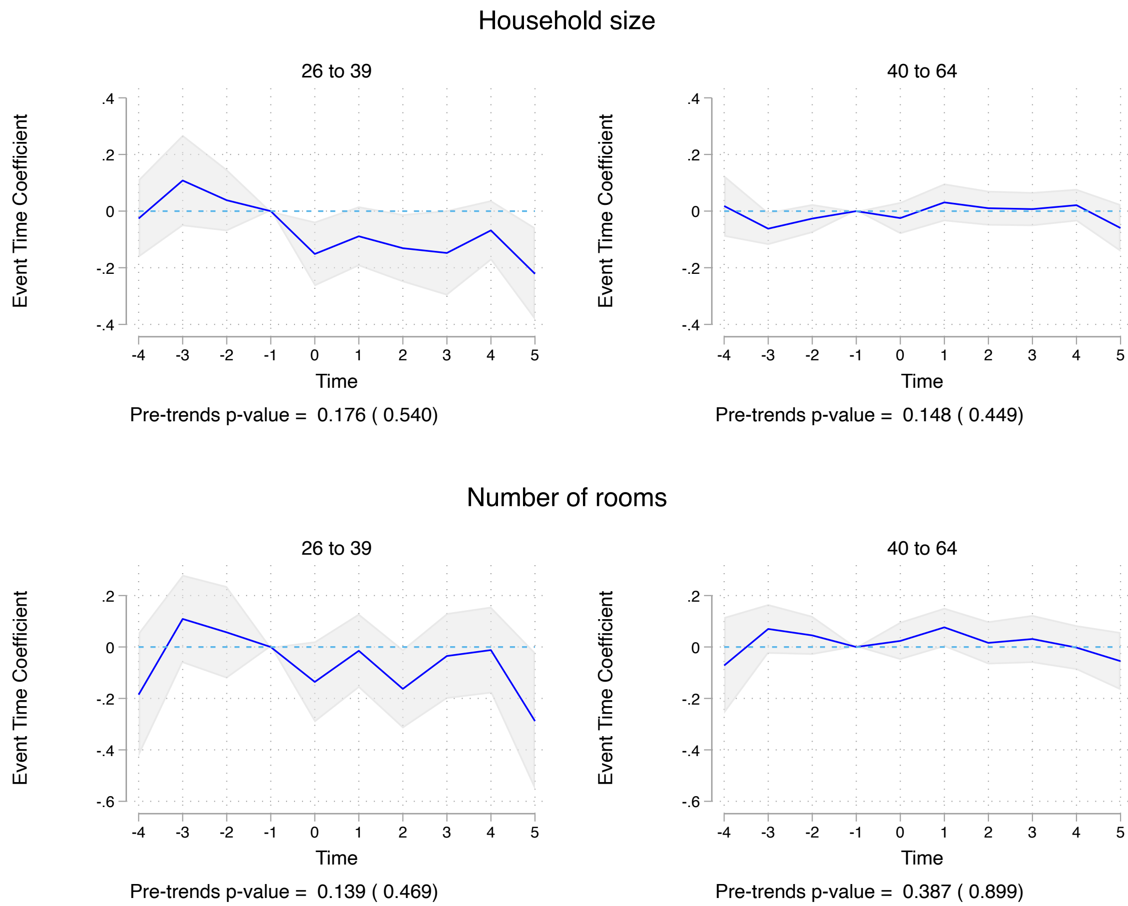
Notes: The individuals in this table include 26 to 64 year olds in the 37 treatment and control states in 2012 and 2013. Means are listed first with standard deviations in parentheses below, which are weighted by the square root of the IPUMS person weight. The p-values for the difference between 26 to 39 year olds and 40 to 64 year olds are found by estimating a WLS regression of the variable of interest on a binary indicator for being in the sample with standard errors clustered by state. Total income is in 2019 dollars.

Table 2.2: Compare 26 to 39 Year Olds to 40 to 64 Year Olds



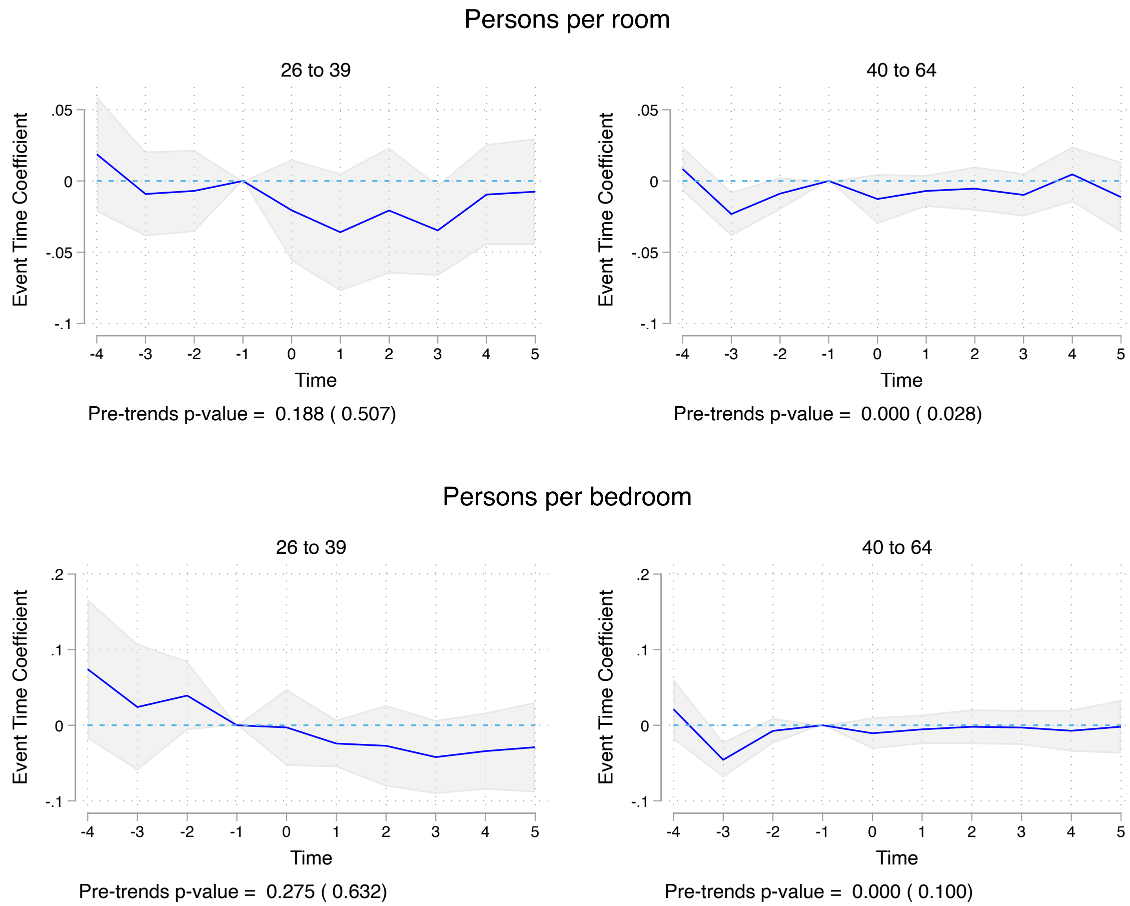
Notes: These graphs plot the coefficient estimates from equation 2.2 in blue. The 95% confidence intervals using robust standard errors clustered by state are in grey. Below each graph, the cluster robust p-value for the test of joint significance for $\widehat{\beta}_{-2}$, $\widehat{\beta}_{-3}$, and $\widehat{\beta}_{-4}$ is listed first and then followed by the wild cluster bootstrap p-value of the same test in parentheses, found using 999 replications and Rademacher weights.

Figure 2.2: Event Study Figures for Any Insurance Coverage and Medicaid Coverage



Notes: These graphs plot the coefficient estimates from equation 2.2 in blue. The 95% confidence intervals using robust standard errors clustered by state are in grey. Below each graph, the cluster robust p-value for the test of joint significance for $\widehat{\beta}_{-2}$, $\widehat{\beta}_{-3}$, and $\widehat{\beta}_{-4}$ is listed first and then followed by the wild cluster bootstrap p-value of the same test in parentheses, found using 999 replications and Rademacher weights.

Figure 2.3: Event Study Figures for Household Size and Number of Rooms



Notes: These graphs plot the coefficient estimates from equation 2.2 in blue. The 95% confidence intervals using robust standard errors clustered by state are in grey. Below each graph, the cluster robust p-value for the test of joint significance for $\widehat{\beta}_{-2}$, $\widehat{\beta}_{-3}$, and $\widehat{\beta}_{-4}$ is listed first and then followed by the wild cluster bootstrap p-value of the same test in parentheses, found using 999 replications and Rademacher weights.

Figure 2.4: Event Study Figures for Persons per Room and Persons per Bedroom

	(1) 1(Any health insurance)	(2) 1(Government-assisted health insurance)
a) 26 to 39 year olds		
Treatment effect	0.131	0.134
	(0.016)	(0.016)
Y mean	0.462	0.155
Cluster p	0.000	0.000
Boot p	0.000	0.000
Clusters	37	37
MSAs	211	211
N	51,537	51,537
b) 40 to 64 year olds		
Treatment effect	0.064	0.098
	(0.009)	(0.010)
Y mean	0.659	0.151
Cluster p	0.000	0.000
Boot p	0.000	0.000
Clusters	37	37
MSAs	211	211
N	241,340	241,340

Notes: The coefficient estimates in the table are found by estimating equation 2.1 with a WLS regression. The weights are the square root of the IPUMS person weight. The estimated coefficient $\hat{\beta}$ is reported as the treatment effect, and the cluster robust standard error is below it in parentheses. Y mean reports the pre-2014 outcome mean. Cluster p reports the p-value found using robust standard errors, clustered by state. Boot p reports the p-value found by using the wild cluster bootstrap method with 999 replications and Rademacher weights. Clusters reports the number of clusters (states) used for inference. MSAs reports the number of MSAs used in the regression. N reports the sample size.

Table 2.3: First Stage Effect on Health Insurance Coverage

	(1) Household size	(2) Number of rooms	(3) Persons per room	(4) Persons per bedroom	(5) *Number of bedrooms*	(6) *Number of other rooms*
a) 26 to 39 year olds						
Treatment effect	-0.143	-0.099	-0.024	-0.041	-0.033	-0.066
	(0.039)	(0.045)	(0.014)	(0.017)	(0.022)	(0.030)
Y mean	3.397	5.494	0.671	1.303	2.736	2.758
Cluster p	0.001	0.036	0.094	0.021	0.154	0.033
Boot p	0.007	0.022	0.196	0.066	0.141	0.026
Clusters	37	37	37	37	37	37
MSAs	211	211	211	211	211	211
N	51,537	51,537	51,537	50,322	51,537	51,537
b) 40 to 64 year olds						
Treatment effect	0.019	0.017	-0.003	-0.002	0.006	0.010
	(0.021)	(0.032)	(0.006)	(0.008)	(0.018)	(0.018)
Y mean	2.850	5.543	0.557	1.102	2.728	2.815
Cluster p	0.369	0.604	0.589	0.792	0.716	0.579
Boot p	0.416	0.729	0.660	0.831	0.811	0.649
Clusters	37	37	37	37	37	37
MSAs	211	211	211	211	211	211
N	241,340	241,340	241,340	236,334	241,340	241,340

Notes: The coefficient estimates in the table are found by estimating equation 2.1 with a WLS regression. The weights are the square root of the IPUMS person weight. The estimated coefficient $\hat{\beta}$ is reported as the treatment effect, and the cluster robust standard error is below it in parentheses. Y mean reports the pre-2014 outcome mean. Cluster p reports the p-value found using robust standard errors, clustered by state. Boot p reports the p-value found by using the wild cluster bootstrap method with 999 replications and Rademacher weights. Clusters reports the number of clusters (states) used for inference. MSAs reports the number of MSAs used in the regression. N reports the sample size.

Table 2.4: Main Results and Breakdown of Number of Rooms

	(1) Number of family members	(2) Number of non-family members	(3) Number of immediate family members	(4) Number of extended family members
Treatment effect	-0.113	-0.030	-0.020	-0.093
	(0.039)	(0.030)	(0.023)	(0.034)
Y mean	1.868	0.529	1.142	0.726
Cluster p	0.007	0.319	0.395	0.010
Boot p	0.037	0.371	0.470	0.028
Clusters	37	37	37	37
MSAs	211	211	211	211
N	51,537	51,537	51,537	51,537

Notes: The coefficient estimates in the table are found by estimating equation 2.1 with a WLS regression. The weights are the square root of the IPUMS person weight. The estimated coefficient $\hat{\beta}$ is reported as the treatment effect, and the cluster robust standard error is below it in parentheses. Y mean reports the pre-2014 outcome mean. Cluster p reports the p-value found using robust standard errors, clustered by state. Boot p reports the p-value found by using the wild cluster bootstrap method with 999 replications and Rademacher weights. Clusters reports the number of clusters (states) used for inference. MSAs reports the number of MSAs used in the regression. N reports the sample size.

Table 2.5: Breakdown of Household Size for 26 to 39 Year Olds

	(1) Number of minor family members	(2) Number of adult family members	(3) Number of senior family members
Treatment effect	-0.033	-0.056	-0.023
	(0.020)	(0.031)	(0.015)
Y mean	0.361	1.304	0.203
Cluster p	0.110	0.077	0.120
Boot p	0.156	0.162	0.184
Clusters	37	37	37
MSAs	211	211	211
N	51,537	51,537	51,537

Notes: The coefficient estimates in the table are found by estimating equation 2.1 with a WLS regression. The weights are the square root of the IPUMS person weight. The estimated coefficient $\hat{\beta}$ is reported as the treatment effect, and the cluster robust standard error is below it in parentheses. Y mean reports the pre-2014 outcome mean. Cluster p reports the p-value found using robust standard errors, clustered by state. Boot p reports the p-value found by using the wild cluster bootstrap method with 999 replications and Rademacher weights. Clusters reports the number of clusters (states) used for inference. MSAs reports the number of MSAs used in the regression. N reports the sample size.

Table 2.6: Breakdown of Age of Family Members for 26 to 39 Year Olds

	(1) 1(Moved within past year)	(2) 1(Live in principal city of MSA)
Treatment effect	0.006	-0.028
	(0.009)	(0.010)
Y mean	0.216	0.181
Cluster p	0.513	0.006
Boot p	0.557	0.007
Clusters	37	37
MSAs	211	211
N	51,537	51,537

Notes: The coefficient estimates in the table are found by estimating equation 2.1 with a WLS regression. The weights are the square root of the IPUMS person weight. The estimated coefficient $\hat{\beta}$ is reported as the treatment effect, and the cluster robust standard error is below it in parentheses. Y mean reports the pre-2014 outcome mean. Cluster p reports the p-value found using robust standard errors, clustered by state. Boot p reports the p-value found by using the wild cluster bootstrap method with 999 replications and Rademacher weights. Clusters reports the number of clusters (states) used for inference. MSAs reports the number of MSAs used in the regression. N reports the sample size.

Table 2.7: Effect of ACA Medicaid Expansion on Moving within the Past Year and Living in the Principal City of an MSA for 26 to 39 Year Olds

	(1) Household size	(2) Number of rooms	(3) Persons per room	(4) Persons per bedroom
a) All 26 to 39 year olds				
Treatment effect	-0.143	-0.099	-0.024	-0.041
	(0.039)	(0.045)	(0.014)	(0.017)
Y mean	3.397	5.494	0.671	1.303
Cluster p	0.001	0.036	0.094	0.021
Boot p	0.007	0.022	0.196	0.066
Clusters	37	37	37	37
MSAs	211	211	211	211
N	51,537	51,537	51,537	50,322
b) White 26 to 39 year olds				
Treatment effect	-0.159	-0.154	-0.021	-0.048
	(0.045)	(0.071)	(0.013)	(0.020)
Y mean	3.334	5.567	0.652	1.273
Cluster p	0.001	0.036	0.114	0.023
Boot p	0.009	0.047	0.189	0.044
Clusters	37	37	37	37
MSAs	211	211	211	211
N	32,183	32,183	32,183	31,520
c) Black 26 to 39 year olds				
Treatment effect	-0.058	0.068	-0.015	-0.018
	(0.064)	(0.108)	(0.016)	(0.020)
Y mean	3.217	5.423	0.626	1.261
Cluster p	0.369	0.537	0.359	0.363
Boot p	0.431	0.604	0.375	0.364
Clusters	34	34	34	34
MSAs	188	188	188	186
N	10,014	10,014	10,014	9,776
d) Hispanic 26 to 39 year olds				
Treatment effect	-0.401	-0.144	-0.090	-0.156
	(0.137)	(0.120)	(0.036)	(0.052)
Y mean	4.039	5.251	0.847	1.566
Cluster p	0.006	0.236	0.019	0.005
Boot p	0.016	0.299	0.092	0.032
Clusters	36	36	36	36
MSAs	191	191	191	191
N	15,077	15,077	15,077	14,579

Notes: The coefficient estimates in the table are found by estimating equation 2.1 with a WLS regression. The weights are the square root of the IPUMS person weight. The estimated coefficient $\hat{\beta}$ is reported as the treatment effect, and the cluster robust standard error is below it in parentheses. Y mean reports the pre-2014 outcome mean. Cluster p reports the p-value found using robust standard errors, clustered by state. Boot p reports the p-value found by using the wild cluster bootstrap method with 999 replications and Rademacher weights. Clusters reports the number of clusters (states) used for inference. MSAs reports the number of MSAs used in the regression. N reports the sample size.

Table 2.8: Heterogeneity by Race and Ethnicity for 26 to 39 Year Olds

	(1) Household size	(2) Number of rooms	(3) Persons per room	(4) Persons per bedroom
a) All 26 to 39 year olds				
Treatment effect	-0.143	-0.099	-0.024	-0.041
	(0.039)	(0.045)	(0.014)	(0.017)
Y mean	3.397	5.494	0.671	1.303
Cluster p	0.001	0.036	0.094	0.021
Boot p	0.007	0.022	0.196	0.066
Clusters	37	37	37	37
MSAs	211	211	211	211
N	51,537	51,537	51,537	50,322
b) 26 to 39 year olds in above-median MSAs				
Treatment effect	-0.104	-0.058	-0.029	-0.054
	(0.060)	(0.074)	(0.019)	(0.019)
Y mean	3.543	5.473	0.713	1.362
Cluster p	0.093	0.436	0.141	0.007
Boot p	0.167	0.487	0.293	0.042
Clusters	31	31	31	31
MSAs	104	104	104	104
N	33,482	33,482	33,482	32,567
c) 26 to 39 year olds in below-median MSAs				
Treatment effect	-0.194	-0.141	-0.017	-0.028
	(0.092)	(0.084)	(0.014)	(0.030)
Y mean	3.159	5.527	0.603	1.207
Cluster p	0.045	0.107	0.242	0.354
Boot p	0.123	0.160	0.297	0.395
Clusters	25	25	25	25
MSAs	104	104	104	104
N	17,988	17,988	17,988	17,689

Notes: The coefficient estimates in the table are found by estimating equation 2.1 with a WLS regression. The weights are the square root of the IPUMS person weight. The estimated coefficient $\hat{\beta}$ is reported as the treatment effect, and the cluster robust standard error is below it in parentheses. Y mean reports the pre-2014 outcome mean. Cluster p reports the p-value found using robust standard errors, clustered by state. Boot p reports the p-value found by using the wild cluster bootstrap method with 999 replications and Rademacher weights. Clusters reports the number of clusters (states) used for inference. MSAs reports the number of MSAs used in the regression. N reports the sample size.

Table 2.9: Heterogeneity by MSA Housing Costs for 26 to 39 Year Olds

	(1)	(2)	(3)	(4)
a) Household size				
Treatment effect	-0.126	-0.116	-0.145	-0.143
	(0.036)	(0.034)	(0.036)	(0.039)
Y mean	3.407	3.407	3.407	3.407
Cluster p	0.001	0.001	0.000	0.001
Boot p	0.006	0.003	0.006	0.007
Clusters	37	37	37	37
MSAs	211	211	211	211
N	51,537	51,537	51,537	51,537
b) Number of rooms				
Treatment effect	-0.095	-0.091	-0.095	-0.099
	(0.035)	(0.035)	(0.042)	(0.045)
Y mean	5.493	5.493	5.493	5.493
Cluster p	0.010	0.014	0.032	0.036
Boot p	0.007	0.010	0.013	0.022
Clusters	37	37	37	37
MSAs	211	211	211	211
N	51,537	51,537	51,537	51,537
c) Persons per room				
Treatment effect	-0.019	-0.018	-0.019	-0.024
	(0.011)	(0.011)	(0.011)	(0.014)
Y mean	0.672	0.672	0.672	0.672
Cluster p	0.078	0.107	0.095	0.094
Boot p	0.159	0.203	0.166	0.196
Clusters	37	37	37	37
MSAs	211	211	211	211
N	51,537	51,537	51,537	51,537
d) Persons per bedroom				
Treatment effect	-0.025	-0.023	-0.037	-0.041
	(0.021)	(0.020)	(0.018)	(0.017)
Y mean	1.308	1.308	1.308	1.308
Cluster p	0.246	0.276	0.049	0.021
Boot p	0.314	0.339	0.102	0.066
Clusters	37	37	37	37
MSAs	211	211	211	211
N	50,322	50,322	50,322	50,322
MSA and year FE	X	X	X	X
Demographics		X	X	X
State-year controls			X	X
Housing controls				X

Notes: The coefficient estimates in the table are found by estimating equation 2.1 with a WLS regression. The weights are the square root of the IPUMS person weight. The estimated coefficient $\hat{\beta}$ is reported as the treatment effect, and the cluster robust standard error is below it in parentheses. Y mean reports the pre-2014 outcome mean. Cluster p reports the p-value found using robust standard errors, clustered by state. Boot p reports the p-value found by using the wild cluster bootstrap method with 999 replications and Rademacher weights. Clusters reports the number of clusters (states) used for inference. MSAs reports the number of MSAs used in the regression. N reports the sample size. Sample is consistent across columns for ease of comparison and includes only the most restrictive sample of observations that have Zillow controls available.

Table 2.10: Check the Inclusion of Different Covariates for 26 to 39 Year Olds

	(1) MSA-by-state FEs	(2) Drop cross-border MSAs	(3) Propensity between 0.1 and 0.9	(4) Drop work requirement state	(5) Binary treatment	(6) Just 2014 adopters
a) Household size						
Treatment effect	-0.140	-0.161	-0.210	-0.147	-0.136	-0.135
	(0.040)	(0.052)	(0.057)	(0.039)	(0.040)	(0.048)
Y mean	3.397	3.441	3.186	3.401	3.397	3.438
Cluster p	0.001	0.004	0.002	0.001	0.002	0.009
Boot p	0.010	0.013	0.028	0.002	0.008	0.024
Clusters	37	36	18	36	37	31
MSAs	231	192	132	209	211	178
N	51,537	41,749	25,414	51,314	51,537	45,825
b) Number of rooms						
Treatment effect	-0.125	-0.195	-0.111	-0.099	-0.094	-0.150
	(0.051)	(0.059)	(0.054)	(0.046)	(0.045)	(0.061)
Y mean	5.494	5.449	5.480	5.490	5.494	5.444
Cluster p	0.019	0.002	0.053	0.037	0.044	0.021
Boot p	0.020	0.004	0.123	0.025	0.030	0.019
Clusters	37	36	18	36	37	31
MSAs	231	192	132	209	211	178
N	51,537	41,749	25,414	51,314	51,537	45,825
c) Persons per room						
Treatment effect	-0.021	-0.022	-0.026	-0.025	-0.022	-0.017
	(0.014)	(0.019)	(0.014)	(0.014)	(0.014)	(0.018)
Y mean	0.671	0.688	0.621	0.672	0.671	0.686
Cluster p	0.152	0.274	0.078	0.083	0.110	0.368
Boot p	0.270	0.439	0.165	0.176	0.216	0.503
Clusters	37	36	18	36	37	31
MSAs	231	192	132	209	211	178
N	51,537	41,749	25,414	51,314	51,537	45,825
d) Persons per bedroom						
Treatment effect	-0.033	-0.020	-0.063	-0.042	-0.039	-0.024
	(0.016)	(0.020)	(0.020)	(0.017)	(0.016)	(0.023)
Y mean	1.303	1.317	1.225	1.304	1.303	1.324
Cluster p	0.049	0.325	0.007	0.019	0.022	0.302
Boot p	0.098	0.391	0.073	0.074	0.060	0.425
Clusters	37	36	18	36	37	31
MSAs	231	192	132	209	211	178
N	50,322	40,716	24,934	50,102	50,322	44,694

Notes: The coefficient estimates in the table are found by estimating equation 2.1 with a WLS regression. The weights are the square root of the IPUMS person weight. The estimated coefficient $\hat{\beta}$ is reported as the treatment effect, and the cluster robust standard error is below it in parentheses. Y mean reports the pre-2014 outcome mean. Cluster p reports the p-value found using robust standard errors, clustered by state. Boot p reports the p-value found by using the wild cluster bootstrap method with 999 replications and Rademacher weights. Clusters reports the number of clusters (states) used for inference. MSAs reports the number of MSAs used in the regression. N reports the sample size.

Table 2.11: Check Variants of Fixed Effects and Other Miscellaneous Specification Checks for 26 to 39 Year Olds

	(1) 1(Persons per room>1)	(2) 1(Persons per bedroom>2)	(3) 1(Doubled-up)
Treatment effect	-0.001	-0.008	-0.004
	(0.007)	(0.007)	(0.011)
Y mean	0.102	0.104	0.678
Cluster p	0.831	0.303	0.726
Boot p	0.844	0.366	0.712
Clusters	37	37	37
MSAs	211	211	211
N	51,537	51,537	51,537

Notes: The coefficient estimates in the table are found by estimating equation 2.1 with a WLS regression. The weights are the square root of the IPUMS person weight. The estimated coefficient $\hat{\beta}$ is reported as the treatment effect, and the cluster robust standard error is below it in parentheses. Y mean reports the pre-2014 outcome mean. Cluster p reports the p-value found using robust standard errors, clustered by state. Boot p reports the p-value found by using the wild cluster bootstrap method with 999 replications and Rademacher weights. Clusters reports the number of clusters (states) used for inference. MSAs reports the number of MSAs used in the regression. N reports the sample size.

Table 2.12: Look at Binary Outcomes of Persons per Room Exceeding One, Persons per Bedroom Exceeding Two, and Doubling-Up for 26 to 39 Year Olds

Chapter 3

Two-way Fixed Effects Regressions with Group-by-Time Fixed Effects Under Heterogeneous Treatment Effects

3.1 Introduction

Many empirical settings that have treatment variation within groups (e.g. regions, states, counties) use linear regression with unit and group-by-time fixed effects to estimate the causal effect of the treatment. This is often done with the stated purpose of exploiting within group variation in order to construct better control groups for treated observations. By limiting the comparison of treated units to untreated units within the same group, researchers attempt to control for unobserved group level shocks that could otherwise bias their estimates of causal effects. While these regressions, which I call GT regressions, are common in empirical literature, to the best of my knowledge the assumptions required to interpret the coefficient on the treatment variable have not been explicitly investigated in previous research.

In this paper I analyze the coefficient on the treatment variable in GT regressions with heterogeneous treatment effects. I show that in the simple setting where each group has one subgroup that never receives treatment, each group has one subgroup that begins untreated and becomes treated, and all treated units receive treatment at the same time during the sample period, the average treatment effect on the treated (ATT) can be obtained under the assumption that parallel trends hold within every group. While this relaxes the parallel trends assumption in the canonical two-way fixed effects (TWFE) regression as trends need only be parallel within groups and not across groups, it comes at the cost of a less directly testable assumption as many settings will not have sufficient observations to test for parallel trends within each group. Take, for example, the Contiguous Border County Pair (CBCP) design with county level data. This design defines county pairs as U.S. counties in different states that share a border, and estimates treatment effects using a GT regression with county and county pair-by-time fixed effects. Researchers using a CBCP design often choose it over a canonical TWFE regression in order to control for local shocks across geographically proximate areas. In this setting, each group has two observations and thus the parallel trends assumption cannot be tested for individual groups. Therefore, in such cases where researchers can choose between the canonical TWFE regression and a GT regression, if researchers choose the GT regression they should make an economic argument as to why controlling for group level shocks is important in their setting and note the potential drawbacks.

Using the decomposition from [1], I show that while GT regressions can be successful in leveraging within group variation, under heterogenous treatment effects the coefficient of interest is subject to similar weighting issues as in the canonical TWFE regression. I further show that the weights from a GT regression can be separated into the product of two terms. The first term is the weight from a TWFE regression on only observations from a unit's corresponding group. This weight causes bias when there is variation in treatment timing within a group. The second term is specific to cases with multiple groups, and causes bias when there is variation in treatment timing across groups. Thus, in settings where within every group there is one subgroup that is never treated and one subgroup that becomes treated, but that treatment occurs at different times for different groups, the first weight from a GT regression reduces to one for all units and thus does not cause bias. However, the variation in treatment timing across groups causes the second weight to differ from one, and thus the coefficient of interest in not an unbiased estimate of the ATT. In this case each group provides a simple difference-in-differences setting and even though within group variation is being leveraged to estimate the coefficient on the treatment variable, the weighted average of the treatment effects in each group leads to bias.

I also consider GT regressions that include groups in which all units have the same treatment sequence and that include groups in which no subgroup experiences a change in treatment status. This is not possible in the canonical TWFE regression, since the canonical TWFE regression is a special case of the GT regression with one group. In a GT regression, including such groups will not cause collinearity between the treatment variable and the fixed effects as long as at least one group has units with different treatment sequences. Observations from groups with no variation in treatment do not contribute to the estimation of the coefficient on the treatment variable, and therefore I refer to them as *irrelevant observations*. I show that when groups with irrelevant observations are included, when all groups that do not contain irrelevant observations have a never treated subgroup and a treated subgroup that never reverts to being untreated, and when there is neither variation in treatment timing within group nor across groups for groups that do not contain irrelevant observations, the coefficient on the treatment variable in a GT regression is an unbiased estimate of the ATT for the groups without irrelevant observations.

While this parameter is still policy relevant, I also show that including groups with irrelevant observations biases clustered standard errors due to finite sample adjustments used in major regression packages. While some TWFE regression packages can handle GT regressions, I test the package from [1] in the case with irrelevant observations under three scenarios: the irrelevant observations are always treated during the sample period, the irrelevant observations are never treated during the sample period, and the irrelevant observations all begin untreated and become treated at the same time during the sample period. In the first two scenarios I show that the standard error for the estimate provided by [1] are influenced by these observations similar to the GT regression. However, in the case in which the irrelevant observations begin untreated and become treated, the estimate itself is changed. My simulation is intentionally simple and therefore more work needs to be done to understand this issue, but the main conclusion is that researchers need to think carefully about their empirical setting and drop irrelevant observations before using either a GT regression or an estimation package provided in the literature.

This paper contributes to the growing literature on TWFE regressions. Much work has been done on the weighting issues that arise in TWFE regressions and potential estimation solutions [86, 1, 87, 88, 89, 90]. I provide an explicit analysis of GT regressions, of which the canonical TWFE regression is a special case. I also show that irrelevant observations can be added to GT regressions with more than one group, and that current TWFE methods do not necessarily correct for the issues that this causes.

I also contribute to the empirical literature that uses GT regressions to identify causal effects. Researchers use a variety of group-by-time fixed effects in GT regressions including census region-by-year [91], state-by-year [92], and county pair-by-year/month [93, 94, 95, 96, 29, 97]. Papers using these designs have used different strategies to argue that they are identifying causal effects, however, these papers often fail to explicitly state either a parallel trends assumption or the specific parallel trends assumption needed to estimate the ATT in their GT regression. I provide researchers using these designs with the identifying assumptions necessary to identify causal effects, the cases when an ATT type parameter can be estimated with a GT regression, and the cases in which weighting issues will lead to a biased estimate of the ATT.

The rest of the paper is structured as follows. Section 2 provides the decomposition result from [1] applied to GT regressions, an explanation of what happens when irrelevant observations are included in GT regressions, and the cases in which an ATT type parameter can be identified. Section 3 discusses the assumptions necessary to identify an ATT type parameter in a GT regression, as well as the potential drawbacks to testing the parallel trends assumption in this setting. Section 4 discusses how group level ATT estimates can be used to construct the full sample ATT estimate, and how different aggregation methods could be used to identify different treatment effects. Section 4 also provides a simulation that shows how TWFE packages can also lead to issues when irrelevant observations are included, and Section 5 concludes.

3.2 Regression with group-by-time fixed effects

3.2.1 Decomposition

I consider models with unit and group-by-time fixed effects of the following form:

$$y_{it} = \beta^{GT} D_{it} + \alpha_i + \gamma_{gt} + \epsilon_{it} \tag{3.1}$$

y_{it} is the outcome for unit i in period t, D_{it} is a binary treatment variable, α_i are unit fixed effects, and γ_{gt} are group-by-time fixed effects. Note that in the case with one group, (3.1) collapses to the canonical TWFE model. Therefore the canonical TWFE model is a special case of a model with group-by-time fixed effects.

Researchers using (3.1) often describe the group-by-time fixed effect as controlling for group level shocks[1]. However, little research has been done on the interpretation of β^{GT}

[1]For example, in minimum wage studies researchers using the CBCP design often cite concern over unobserved local labor market shocks as justification for their identification strategy. For an early

in (3.1), and the assumptions under which it has a causal interpretation.

For clarity I adopt the following notation. A subgroup s within group g is defined as a collection of units within group g that share the same treatment sequence. In settings with a binary treatment, staggered adoption, and county level data where states act as groups (i.e. (3.1) would include county and state-by-time fixed effects), a subgroup would be defined as the collection of counties within a state that entered treatment at the same time. Rewriting (3.1) to reflect this terminology gives:

$$y_{g(s,i),t} = \beta^{GT} D_{g(s,i),t} + \alpha_i + \gamma_{gt} + \epsilon_{g(s,i),t} \tag{3.2}$$

where $x_{g(s,i),t}$ denotes the variable x for unit i, in subgroup s, within group g, in period t. For the remainder of the paper I will refer to subgroups whose units remain untreated for all $t \in \{1, ..., T\}$ as never treated, to subgroups whose units are treated for all $t \in \{1, ..., T\}$ as always treated, and to subgroups whose units begin untreated in period 1 and become treated in some period q such that $1 < q \leq T$ as treated.

Let $Y_{g(s,i),t}(0)$ denote the potential outcome of unit i in period t without treatment and let $Y_{g(s,i),t}(1)$ denote the potential outcome of unit i in period t with treatment. Then let the outcome for observation i in period t be $Y_{g(s,i),t} = Y_{g(s,i),t}(D_{g(s,i),t})$ for all $i \in N$. I consider the case of a balanced panel, and therefore define $N_{g(s)}$ as the number of observations in subgroup s of group g and N_g as the number of observations in group g. Following [1] I define the average value of a variable x for subgroup s in group g in period t as $x_{g(s),t} = \frac{1}{N_{g(s)}} \sum_{i \in g(s)} x_{g(s,i),t}$. I refer to this as the subgroup average. Then for any variable $x_{g(s),t}$ I denote $\bar{x}_{g(s),.} = \frac{1}{T} \sum_{t=1}^{T} x_{g(s),t}$ the time average, $\bar{x}_{\bar{g}(.),t} = \frac{1}{N_g} \sum_{s \in g} N_{g(s)} x_{g(s),t}$ the group average, and $\bar{\bar{x}}_{\bar{g}(.),.} = \frac{1}{T} \frac{1}{N_g} \sum_{t=1}^{T} \sum_{s \in g} N_{g(s)} x_{g(s),t}$ the group-time average.

The decomposition in [1] considers the canonical TWFE regression with unit and time fixed effects. However, in their appendix [1] include an additional decomposition

example, see [93].

for a canonical TWFE regression with covariates which can include group-by-time fixed effects. In this case, the group-by-time fixed effects would be collinear with the time fixed effects included in the canonical TWFE regression and the result would be the model that I consider in this paper. Therefore the decomposition with covariates in [1] becomes the same as my decomposition when the covariates are group-by-time fixed effects. The following decomposition, however, leverages the special structure of (3.1) as to avoid adding collinear fixed effects which eases exposition of the parallel trends assumption later in the paper.

I next list the assumptions on which my results rely. As mentioned previously, the canonical TWFE model is a special case of the model that I consider and thus the following assumptions are the same as those in the main text of [1] but simply generalized to cases with more than one group for balanced panels.

Assumption 1 (Balanced Panel) *For every observation $i \in \{1, ..., N\}$ in the data, we observe* $\{\ Y_{g(s,i),1},\ Y_{g(s,i),2}\ .\ .\ .\ ,\ Y_{g(s,i),T}\ \}$*, which implies* $N_{g(s),t} = N_{g(s),t-1}$.

Assumption 2 (Sharp Design) *In each group $g \in \{1, ..., G\}$, for all $(g(s), t) \in \{g(1), ..., g(S_g)\} \times \{1, ..., T\}$ and $i \in \{1, ..., N_{g(s)}\}$,* $D_{g(s,i),t} = D_{g(s),t}$

Assumption 1 says that our data is a balanced panel and thus the number of observations in each group and in each subgroup remain constant over time. Assumption 2 says that units within the same subgroup have the same treatment sequence. This is called a sharp design in [1].

Assumption 3 (Independent Subgroups) *The vectors* $(Y_{g(s),t}(0), Y_{g(s),t}(1), D_{g(s),t})_{1 \leq t \leq T}$ *are mutually independent.*

Assumption 3 states that potential outcomes and treatments of different subgroups are independent.

Assumption 4 (Strong Exogeneity) *For ever group* $g \in \{1, ..., G\}$ *and for all* $(g(s), t) \in \{g(1), ..., g(S_g)\} \times \{2, ..., T\}$, $E[Y_{g(s),t}(0) - Y_{g(s),t-1}(0) | D_{g(s),1}, ..., D_{g(s),T}] = E[Y_{g(s),t}(0) - Y_{g(s),t-1}(0)]$

Assumption 4 states that shocks impacting a subgroup's $Y_{g(s),t}(0)$ be mean independent of that subgroup's treatment sequence, which eliminates the possibility that a subgroup became treated because of a negative shock.

Assumption 5 (Parallel Trends within Groups) *For every* $g \in \{1, ..., G\}$ *and for* $t \geq 2$, $E[Y_{g(s),t}(0) - Y_{g(s),t-1}(0)]$ *does not vary across* $s \in g$.

Assumption 5 states that the evolution of untreated potential outcomes must be the same for each subgroup within a group. Note that in the canonical setup with one group, parallel trends must hold for all subgroups. When there is more than one group, Assumption 5 requires that parallel trends hold within groups but not across groups.

Let $N_1 = \sum_{g(s,i),t} D_{g(s,i),t}$ denote the number of treated units across all groups. Then following [1] the average treatment effect across all treated units is

$$\Delta^{TR} = \frac{1}{N_1} \sum_{(g(s,i),t):D_{g(s),t}=1} [Y_{g(s,i),t}(1) - Y_{g(s,i),t}(0)]$$

Then denote the ATT as $\delta^{TR} = E[\Delta^{TR}]$, and let

$$\Delta_{g(s),t} = \frac{1}{N_{g(s)}} \sum_{i=1}^{N_{g(s)}} [Y_{g(s,i),t}(1) - Y_{g(s,i),t}(0)]$$

denote the average treatment effect in subgroup s of group g in period t. As in [1], we have

$$\delta^{TR} = \mathbb{E}\Big[\sum_{(g(s),t):D_{g(s),t}=1} \frac{N_{g(s),t}}{N_1} \Delta_{g(s),t} \Big] \tag{3.3}$$

Note that we can rewrite (3.3) as

$$\delta^{TR} = \mathbb{E}\Big[\sum_{g} \sum_{(s,t):s\in g, D_{g(s),t}=1} \frac{N_{g(s),t}}{N_1} \Delta_{g(s),t} \Big] \tag{3.4}$$

Then let $\nu_{g(s),t}$ denote the residual of subgroup s in group g at time t in the regression of $D_{g(s),t}$ on subgroup and group-by-time fixed effects:

$$D_{g(s),t} = \kappa + \alpha_s + \gamma_{gt} + \nu_{g(s),t}$$

Then let

$$w^{GT}_{g(s),t} = \frac{\nu_{g(s),t}}{\sum_g \sum_{(s,t):s\in g, D_{g(s),t}=1} \frac{N_{g(s),t}}{N_1} \nu_{g(s),t}} \tag{3.5}$$

Theorem 1 *Under assumptions 1 to 5, the coefficient from a regression on unit and group-by-time fixed effects is*

$$\beta^{GT} = \mathbb{E}\Big[\sum_{g} \sum_{(s,t):s\in g, D_{g(s),t}=1} \frac{N_{g(s),t}}{N_1} w^{GT}_{g(s),t} \Delta_{g(s),t} \Big]$$

Theorem 1 shows that in general β^{GT} is not equal to δ^{TR} as noted in [1]. However, Theorem 1 has additional implications when considering staggered adoption settings with group-by-time fixed effects. To see this, consider a simple case with three periods and two groups, where each group has two subgroups. Subgroup 1 of both groups never receives treatment. Subgroup 2 in group 1 receives treatment in the third period, while subgroup 2 of group 2 receives the treatment in both periods 2 and 3. Note that within each group we have a simple difference-in-differences setting, and therefore running a standard DID regression on each group individually will yield an unbiased estimate of the ATT for each group so long as parallel trends hold within each group. However, while there is no variation in treatment timing within group, across groups we have a staggered adoption setting. Then the residuals from a regression of the treatment variable on subgroup and group-by-time fixed effects are given by

$$\nu_{g(s),t} = D_{g(s),t} - D_{g(s),.} - D_{\bar{g}(.),t} + D_{\bar{g}(.),.}$$

Then we have

$$\begin{aligned}
w^{GT}_{1(2),3} &= \frac{1/3}{2/9} \\
w^{GT}_{1(2),3} &= \frac{1/6}{2/9} \\
w^{GT}_{1(2),3} &= \frac{1/6}{2/9}
\end{aligned}$$

Thus we have

$$\begin{aligned}
\beta^{GT} &= \frac{1}{2}E[\Delta_{1(2),3}] + \frac{1}{4}E[\Delta_{2(2),2}] + \frac{1}{4}E[\Delta_{2(2),3}] \\
&\neq \frac{1}{3}E[\Delta_{1(2),3}] + \frac{1}{3}E[\Delta_{2(2),2}] + \frac{1}{3}E[\Delta_{2(2),3}] \\
&= \delta^{TR}
\end{aligned}$$

Even though treatment is not staggered within groups, β^{GT} is still not equal to the

ATT due to the treatment being staggered across groups. It is important to note that this is not possible in a canonical TWFE regression setting. To better understand this issue, note that we can rewrite (3.5) for subgroup s in group g in period t as the product of two terms:

$$w^{GT}_{g(s),t} = \underbrace{\frac{\sum_{(s,t):s\in g, D_{g(s),t}=1} \frac{N_{g(s),t}}{N_{1g}} \nu_{g(s),t}}{\sum_g \sum_{(s,t):s\in g, D_{g(s),t}=1} \frac{N_{g(s),t}}{N_1} \nu_{g(s),t}}}_{w^O_{g(s),t}} \cdot \underbrace{\frac{\nu_{g(s),t}}{\sum_{(s,t):s\in g, D_{g(s),t}=1} \frac{N_{g(s),t}}{N_{1g}} \nu_{g(s),t}}}_{w^I_{g(s),t}} \tag{3.6}$$

Note that $w^I_{g(s),t}$ in (3.6) is the weight for subgroup s in period t obtained from a TWFE regression using only observations in group g. Then clearly in the case with one group, $w^O_{g(s),t}$ is equal to one and the weights collapse to those considered in the main text of [1]. In the case with more than one group, the same weighting issues that arise in a TWFE regression will occur within every group in a regression with group-by-time fixed effects. Then the term $w^O_{g(s),t}$ is a second weighting term that arises in the case with group-by-time fixed effects, and it is this weight that led to the difference between β^{GT} and δ^{TR} in the example above. When there is no variation in treatment timing within groups, as shown in [1], $w^I_{g(s),t}$ is equal to one. With only one group, this implies $\beta^{GT} = \delta^{TR}$. With more than one group, variation in treatment timing *across* groups, even in the case when there is no variation in treatment timing *within* groups will cause $w^O_{g(s),t}$ to differ from one and cause β^{GT} to differ from δ^{TR}.

3.2.2 Including irrelevant observations

I now consider the case when one or more groups are added in which all observations within the group have the same treatment sequences or in which no subgroup experiences a change in treatment status. In other words, groups in which all observations are untreated for the entire sample period, all observations are treated for the entire sample period, all observations begin untreated (treated) and become treated (untreated) at the same time, or there is both a subgroup of always treated units and a subgroup of never treated units. As stated above, the canonical TWFE regression is a special case of a regression with unit and group-by-time fixed effects in which there is one group. Therefore, if the sample was one of the cases just mentioned it would be impossible to run the canonical TWFE regression due to collinearity between the treatment variable and the fixed effects. With more than one group, as long as there is one group in which there are subgroups with different treatment sequences it is possible to run the regression with group-by-time fixed effects. Note, however, for a group containing observations that have the same treatment sequences or in which no subgroup experiences a change in treatment status we have that

$$\begin{aligned}\nu_{g(s),t} &= D_{g(s),t} - D_{g(s),.} - D_{\bar{g}(.),t} + D_{\bar{g}(.),.} \\ &= 0\end{aligned}$$

since $D_{g(s),t} = D_{\bar{g}(.),t}$ and $D_{g(s),.} = D_{\bar{g}(.),.}$. To keep things simple, consider the case where we have two groups and two periods. Each group has one subgroup that remains untreated in both periods, and one subgroup that begins untreated and receives a binary treatment in the second period. We therefore have a simple DID setting in each group with neither variation in treatment timing within nor across groups. As discussed above, this will yield an unbiased estimate of the ATT. Now consider if we have data on a third group, but within the third group all units have the same treatment status in both periods. Then we know that $w^{GT}_{3(s),t}$ will be equal to zero for all observations in group 3. Denote $N_{1,(1,2)}$ as the number of treated units in groups 1 and 2, then from (1) we have:

$$\begin{aligned}\beta^{GT} &= \mathbb{E}\Big[\sum_{g}\sum_{(s,t):s\in g, D_{g(s),t}=1}\frac{N_{g(s),t}}{N_1}w^{GT}_{g(s),t}\Delta_{g(s),t}\Big] \\ &= \mathbb{E}\Big[\sum_{g\in\{1,2\}}\sum_{(s,t):s\in g, D_{g(s),t}=1}\frac{N_{g(s),t}}{N_{1,(1,2)}}\Delta_{g(s),t}\Big]\end{aligned}$$

Note that in this case β^{GT} is equal to the ATT in groups 1 and 2, but it is not equal to the ATT in the entire sample. Therefore, when researchers include all of the data in their sample in a TWFE regression with group-by-time fixed effects, they can only estimate the ATT for the collection of units in the groups that have multiple subgroups with variation in treatment sequence. Since observations in groups with no variation in treatment sequence do not contribute to β^{GT}, I call these *irrelevant observations*.

While the interpretation of the parameter of interest in the example above is still an ATT, and would still then potentially be of interest, consider a further problem with estimation in this case. From Lemma (1) we have

$$\hat{\beta}^{GT} = \frac{\sum_t \sum_g \sum_{s \in g} N_{g(s),t} y_{g(s),t} [(D_{g(\bar{s}),t} - \bar{D}_{g(s),.}) - (\bar{D}_{\bar{g}(.),t} - \bar{\bar{D}})]}{\sum_t \sum_g \sum_{s \in g} N_{g(s),t} D_{g(\bar{s}),t} [(D_{g(\bar{s}),t} - \bar{D}_{g(s),.}) - (\bar{D}_{\bar{g}(.),t} - \bar{\bar{D}})]} \tag{3.7}$$

Continuing with the three group example, we have $(D_{3(\bar{s}),t} - \bar{D}_{3(s),.}) - (\bar{D}_{\bar{3}(.),t} - \bar{\bar{D}}) = 0$ and from (3.7) we can see that $\hat{\beta}^{GT}$ will be the same as in the two group case. Again observations from group 3 are "irrelevant" in the sense that they provide no identifying variation in the estimation of $\hat{\beta}^{GT}$. However, consider the standard errors when group 3 is included. If standard errors are clustered by group, as is common in empirical settings, then the cluster-robust estimate of the variance matrix of the OLS estimator is given by:

$$\hat{V}_c(\hat{\beta}) = (\boldsymbol{X'X})^{-1} \Big[\sum_{g=1}^{G} \boldsymbol{X'_g \hat{u}_g \hat{u}'_g X_g} \Big] (\boldsymbol{X'X})^{-1} \tag{3.8}$$

where $\boldsymbol{\hat{u}_g}$ is the vector of residuals for the g^{th} cluster [98]. Most programming languages include finite sample corrections when using clustered standard errors to reduce downward bias in $\hat{V}_c(\hat{\beta})$ resulting from a finite number of clusters [98]. They replace $\boldsymbol{\hat{u}_g}$ with $\sqrt{c}\boldsymbol{\hat{u}_g}$ where c is often given by:

$$c = \frac{G}{G-1} \frac{N-1}{N-K} \tag{3.9}$$

Returning to our example, assume that the researcher clusters their standard errors by group. It is simple to show that when group 3 is added to the sample (3.8) is unchanged. However, while adding group 3 does not change (3.8), it does change (3.9) by adding another cluster (increasing G) and by increasing the number of observations (increasing N). Thus even though adding group 3 to the sample provides no additional identifying variation for the estimation of $\hat{\beta}^{GT}$, it will reduce the size of the standard errors[2].

3.2.3 Combining results

The results above lead to the following Corollaries:

Corollary 1 *Consider the case when all groups have one never treated subgroup and one treated subgroup. If the treated subgroup in every group gets treated in the same period, no unit reverts to being untreated after becoming treated, and Assumptions 1-5 hold, then*

$$\beta^{GT} = \delta^{TR}$$

Corollary 1 says that when treatment is neither staggered within nor across group and every group has observations with variation in treatment sequence, researchers can use (3.1) to get an unbiased estimate of the ATT with the main identifying assumption being that parallel trends hold within each group.

Corollary 2 *Consider the case when there are groups with a never treated subgroup and a treated subgroup, and groups in which either all observations share the same treatment sequence or all observations have no variation in treatment sequence. Let $\delta^{TR}_{U,T}$ be the Average Treatment Effect on the Treated for observations in groups that have one never treated subgroup and one treated subgroup. Then if at least one group of each type is*

[2][98] note that Stata uses this adjustment, and many packages in R also use this adjustment (see, for example, the 'fixest' package). However, SAS uses the simpler adjustment $c = G/(G-1)$, as does Stata when estimating nonlinear models. This simpler adjustment removes the problem of adding additional observations from a cluster with no identifying variation for $\hat{\beta}^{GT}$, however, the problem still persists since c remains a function of the number of clusters.

present in the data, the treated subgroup in every group with a treated and untreated subgroup gets treated in the same period, and if no unit reverts to being untreated after becoming treated in groups with a treated and untreated subgroup, under Assumptions 1-5 we have

$$\beta^{GT} = \delta^{TR}_{U,T}$$

3.3 Assumptions to identify causal parameters

As noted above, many researchers use regressions with group-by-time fixed effects to control for group level unobserved shocks, and therefore argue that their design provides better control units than in the canonical TWFE regression. However, many papers fail to explicitly state the main identifying assumption of this type of regression. Some papers use a parallel trends type argument and show an event study as is common in DID designs, but typically fail to specify how their assumption differs from that of a canonical DID setup with one group [92, 29]. Other papers do not discuss a parallel trends assumption directly, but rather justify their use of group-by-time fixed effects by either arguing that controlling for group level shocks provides the correct control groups [17], or by adding regressors to their specification whose coefficients should be zero if the group-by-time fixed effects appropriately control for group-time shocks [99, 96]. While Theorem 1 shows that, in general, regressions of this form provide a biased estimate of the ATT, in the special cases given by Corollary 1 and Corollary 2, researchers can obtain an unbiased estimate of an ATT type parameter. However, this result relies on a different parallel trends assumption than the canonical DID design. As stated in Assumption 5, Corollaries 1 and 2 rely on parallel trends within each group which aligns with the intuition that models with group-by-time fixed effects use within group comparisons to estimate the ATT.

When groups are large it is straightforward to test this assumption. Researchers can simply use parallel trends tests from the TWFE literature for each group. However, when groups are small testing parallel trends will be problematic since tests will lack statistical power to identify a deviation in trends. Consider CBCP designs. In this case each group has exactly two units, and testing parallel trends will entail testing trends for each pair in the sample. This is clearly not feasible, and thus it will be necessary instead to aggregate the trends tests and use an event study design as is common in the DID literature but replace time fixed effects with group-by-time fixed effects. Note that this does not directly test the underlying assumption, but instead will only detect a deviation in trends when at least one group experiences non-parallel trends that are strong enough for the researcher to detect, which is similar to the extrapolation typically made in a difference-in-differences regression analysis with covariates. Therefore, in many empirical settings using group-by-time fixed effects is not a panacea for controlling for group level shocks, as it may come with a less testable assumption. Researchers using GT regressions with small groups should therefore note this potential drawback and use economic justifications for why this design is necessary if it results in an underpowered or aggregated test of the main identifying assumption.

3.4 Aggregating treatment effects

While Corollaries 1 and 2 give specific cases for when regressions with group-by-time fixed effects can give an unbiased estimate of the ATT, many empirical settings do not align with this specific case. Define δ_g^{TR} as the ATT for group g such that:

$$\delta_g^{TR} = \mathbb{E}\Big[\sum_{(s,t):s\in g, D_{g(s),t}=1} \frac{N_{g(s),t}}{N_{1,g}} \Delta_{g(s),t}\Big] \tag{3.10}$$

Then note that (3.3) can be rewritten as:

$$\delta^{TR} = \sum_{g} \frac{N_{1,g}}{N_1} \delta_g^{TR} \tag{3.11}$$

Thus in more complicated settings researchers can take advantage of (3.11), as they can use alternative methods to estimate the ATT for each group and use these group ATT's to construct estimates of δ^{TR}. Consider the simple example provided above where there was one subgroup that was never treated and one subgroup that switched from untreated to treated within each group with variation in treatment timing across groups. In this situation, researchers can estimate the ATT for each group by running a simple difference-in-differences regression on each group separately and use these estimates to get an estimate of δ^{TR}.

While estimating each group ATT can help researchers construct an aggregate effect of a treatment, [87] note that different forms of aggregation can help highlight treatment effect heterogeneity across different dimensions. This may be of particular interest in the case where the researcher is concerned about dynamic treatment effects. Many packages are available from the TWFE literature that consider other ATT type estimands. Consider the estimand considered in [1] which, in a staggered adoption setting, is the average treatment effect in the first period of treatment across all groups that become treated during the sample period. The authors develop an unbiased estimator for this treatment effect, DID_M, which in staggered adoption settings is robust to dynamic treatment effects. While DID_M is presented as an alternative to the estimate from the canonical TWFE regression, [1] apply it to an empirical example with group-by-time fixed effects by including the group-by-time fixed effects as covariates[3]. Given the issues with including irrelevant observations in GT regressions presented in Section 3.2.2, I test DID_M in a sample that contains a group with irrelevant observations to analyze how the estimator

[3]Specifically, they consider a first difference regression with state-by-year fixed effects.

handles this situation. I generate data from a linear model of the following form:

$$y_{g(s,i),t} = \beta_g D_{g(s,i),t} + \alpha_i + \gamma_{gt} + \epsilon_{g(s,i),t}$$

where $D_{g(s,i),t}$ is a binary treatment variable, $g \in \{1, 2, 3\}$, $t \in \{1, 2\}$, and $\epsilon_{g(s,i),t} \sim N(100, 10)$. To keep things simple I generate data for only three groups. There are heterogenous treatment effects such that:

$$\beta_1 = 10$$

$$\beta_2 = 20$$

$$\beta_3 = 30$$

Each unit i appears in both time periods so we have a balanced panel and each group contains 100 units. In Groups 1 and 2 all units begin untreated in period 1 and both groups have 50 units that become treated in period 2. All units in Group 3 are treated in both periods. I run the GT regression on Groups 1 and 2 only with standard errors clustered by group, and I report the results of this regression in column 1 of Table 3.1. Next I rerun the GT regression adding Group 3 and report the results in column 3 of Table 3.1. Note that, as expected, the coefficient is the same when Group 3 is added as Group 3 provides no identifying information for the coefficient of interest. Also as expected, the standard error is smaller. Next I compute DID_M using did_multiplegt in Stata on Groups 1 and 2 only, and then again adding Group 3 with standard errors clustered by group[4]. Columns 2 and 4 show the results with and without Group 3, respectively, and again the estimate is the same in both estimations. As opposed to the change in standard errors from the GT regression, the standard errors for the estimate of DID_M when Group 3 is added are larger. This is due to the fact that the standard

[4]I use the version of did_multiplegt that was released on October 14, 2022, which as of this writing is the newest version available.

error for DID_M is computed using the block bootstrap and since groups with irrelevant observations are not automatically dropped when using did_multiplegt, they are included in the bootstrap sampling routine. The standard error in the example thus changes due to the units in Group 3 being sampled in the bootstrap, and it shows that estimation of DID_M also suffers from incorrect standard errors when groups are added that have no variation in treatment. Tables 3.2 and 3.3 consider the cases when Group 3 contains only never treated units and units that all begin untreated and then become treated in the second period. Note that while the case where Group 3 contains only never treated units results in the correct estimate but incorrect standard errors as in the case with only always treated units, when Group 3 contains units whose treatment status changes DID_M itself is impacted. The results in Table 3.3 show that in this example including Group 3 leads to a very different estimate compared to when Group 3 is dropped.

While this simulation is quite simple, it highlights the importance for researchers to understand their data and the variation that they are leveraging to estimate their desired effect before estimation. When a GT regression is to be used, it is up to the researcher to first identify groups that include irrelevant observations and drop them from their sample in order to get valid standard errors. Also, given my results with did_multiplegt, it is important for researchers utilizing unit and group-by-time fixed effects to examine their data carefully when using packages for alternative estimators. To the best of my knowledge, the issue with irrelevant observations has not been identified in the literature and therefore other user written packages may also fail to drop these observations before estimation.

3.5 Conclusion

In this paper I consider TWFE regressions with unit and group-by-time fixed effects. Estimation in these GT regressions is typically described as leveraging within group variation to estimate the causal effect of a treatment. Since each group is given its own time fixed effect, this specification is utilized to control for unobserved group level shocks that could bias the estimation of causal effects. I show that while this specification can be used to leverage within group variation to estimate treatment effects, variation in treatment timing across groups, within groups, or both can lead to a biased estimate of the ATT. I also discuss cases in which an ATT can be unbiasedly estimated, as well as the potential difficulty in testing the required parallel trends assumption relative to the canonical TWFE parallel trends assumption in a difference-in-differences setting.

As opposed to TWFE regressions, GT regressions allow researchers to include groups whose units all have the same treatment sequence or whose subgroups have constant treatment sequences. When such groups are included in a GT regression, their observations do not provide any identifying information for the estimation of the coefficient on the treatment variable. However, they reduce the size of clustered standard errors due to finite sample corrections included in many modern regression packages. Further, packages developed to correct for weighting issues in TWFE regressions that can be applied to GT regressions may also have issues when irrelevant observations are included, and therefore researchers should drop these observations before proceeding with any form of estimation.

While my paper helps to clarify important aspects of GT regressions, I do not consider the case with covariates. While published work has used GT regressions without covariates, future research should analyze GT regressions when covariates are included. Adding covariates is a common extension in empirical work and more research is needed to understand how it affects interpretation and estimation in GT regressions.

3.6 Tables

	GT Regression	DID_M	GT Regression	DID_M
	15.87 (4.15)	15.87 (2.93)	15.87 (3.59)	15.87 (3.17)
Number of Observations	400	400	600	600
Groups 1 and 2 Only	X	X		
Groups 1, 2, and 3			X	X
Unit FE's	X	X	X	X
Group-by-Time FE's	X	X	X	X
Clustered Std. Errors	Group	Group	Group	Group

Table 3.1: Results from GT regressions and did_multiplegt. Group 3 in this case contains always treated observations. Column 1 gives the coefficient on the treatment variable in a GT regression on only Groups 1 and 2. Column 3 gives the same coefficient adding Group 3. Columns 2 and 4 give DID_M for only Groups 1 and 2, and for all Groups, respectively. Standard errors are clustered by Group in all specifications.

	GT Regression	DID_M	GT Regression	DID_M
	15.87 (4.15)	15.87 (2.93)	15.87 (3.59)	15.87 (3.17)
Number of Observations	400	400	600	600
Groups 1 and 2 Only	X	X		
Groups 1, 2, and 3			X	X
Unit FE's	X	X	X	X
Group-by-Time FE's	X	X	X	X
Clustered Std. Errors	Group	Group	Group	Group

Table 3.2: Results from GT regressions and did_multiplegt. Group 3 in this case contains never treated observations. Column 1 gives the coefficient on the treatment variable in a GT regression on only Groups 1 and 2. Column 3 gives the same coefficient adding Group 3. Columns 2 and 4 give DID_M for only Groups 1 and 2, and for all Groups, respectively. Standard errors are clustered by Group in all specifications.

	GT Regression	DID_M	GT Regression	DID_M
	15.87 (4.15)	15.87 (2.93)	15.87 (3.59)	-105.48 (64.20)
Number of Observations	400	400	600	600
Groups 1 and 2 Only	X	X		
Groups 1, 2, and 3			X	X
Unit FE's	X	X	X	X
Group-by-Time FE's	X	X	X	X
Clustered Std. Errors	Group	Group	Group	Group

Table 3.3: Results from GT regressions and did_multiplegt. Group 3 in this case contains observations that begin untreated in period 1 and become treated in period 2. Column 1 gives the coefficient on the treatment variable in a GT regression on only Groups 1 and 2. Column 3 gives the same coefficient adding Group 3. Columns 2 and 4 give DID_M for only Groups 1 and 2, and for all Groups, respectively. Standard errors are clustered by Group in all specifications.

3.7 Proofs

Lemma 1 *In a balanced panel where* $\tilde{D}_{g(s,i),t} = (D_{g(s,i),t} - \bar{D}_{g(s,i),.}) - (\bar{D}_{\bar{g}(.,i),t} - \bar{\bar{D}})$ *are the residuals from a regression of a treatment indicator on unit and group-by-time fixed effects, we have:*

$$\hat{\beta}^{GT} = \frac{\sum_t \sum_g \sum_{s \in g} N_{g(s),t} y_{g(s),t}[(D_{g(\bar{s}),t} - \bar{D}_{g(s),.}) - (\bar{D}_{\bar{g}(.),t} - \bar{\bar{D}})]}{\sum_t \sum_g \sum_{s \in g} N_{g(s),t} D_{g(\bar{s}),t}[(D_{g(\bar{s}),t} - \bar{D}_{g(s),.}) - (\bar{D}_{\bar{g}(.),t} - \bar{\bar{D}})]} \tag{3.12}$$

$$\tag{3.13}$$

Proof: By the Frisch Waugh Lovell Theorem we have:

$$\hat{\beta}^{GT} = \frac{\hat{Cov}(y_{it}, \tilde{D}_{g(s,i),t})}{\hat{Var}(\tilde{D}_{g(s,i),t})} \tag{3.14}$$

$$= \frac{\frac{1}{T}\frac{1}{N}\sum_{t=1}^{T}\sum_{i=1}^{N} y_{g(s,i),t}[(D_{g(s,i),t} - \bar{D}_{g(s,i,.)}) - (\bar{D}_{\bar{g}(.,i),t} - \bar{\bar{D}})]}{\hat{Var}(\tilde{D}_{g(s,i),t})} \tag{3.15}$$

where $\bar{D}_{g(s,i),.} = \frac{1}{T}\sum_{t=1}^{T} D_{g(s,i),t}$ *is the time average,* $\bar{D}_{\bar{g}(.,i),t} = \frac{1}{N_g}\sum_{i \in g} D_{g(s,i),t}$ *is the group average, and* $\bar{\bar{D}}_{\bar{g}(.,i),.} = \frac{1}{T}\frac{1}{N_g}\sum_{t=1}^{T}\sum_{i \in g} D_{g(s,i),t}$ *is the group-time average. Note that treatment varies within group, but is constant within subgroup by group. Then adding and subtracting subgroup means to the numerator of equation 3.15 yields:*

$$\frac{1}{T}\frac{1}{N}\sum_{t=1}^{T}\sum_{i=1}^{N} y_{g(s,i),t}[(D_{g(s,i),t} - \bar{D}_{g(s,i,.)}) - (D_{g(\bar{s},i),t} - \bar{D}_{g(\bar{s},i,.)})$$
$$+ (D_{g(\bar{s},i),t} - \bar{D}_{g(\bar{s},i,.)}) - (\bar{D}_{\bar{g}(.,i),t} - \bar{\bar{D}})] \tag{3.16}$$

Then note that since $D_{g(s,i),t}$ *only varies at the subgroup-by-time level within each group, we have that* $D_{g(s,i),t} = D_{g(\bar{s},i),t}$*. Similarly, since* $\bar{D}_{g(s,i,.)}$ *varies only at the subgroup* *level within each group, we have that* $\bar{D}_{g(s,i,.)} = \bar{D}_{g(\bar{s},i,.)}$. *Thus we have*

$$(D_{g(s,i),t} - \bar{D}_{g(s,i,.)}) - (D_{g(\bar{s},i),t} - \bar{D}_{g(\bar{s},i,.)}) = 0$$

and therefore equation 3.15 becomes

$$\frac{\frac{1}{T}\frac{1}{N}\sum_{t=1}^{T}\sum_{i=1}^{N} y_{g(s,i),t}[(D_{g(\bar{s},i),t} - \bar{D}_{g(\bar{s},i,.)}) - (\bar{D}_{\bar{g}(.,i),t} - \bar{\bar{D}})]}{\hat{Var}(\tilde{D})} \tag{3.17}$$

Then note that the first sum in the numerator is:

$$\frac{1}{N}\sum_{i=1}^{N} y_{g(s,i),t}[\underbrace{(D_{g(\bar{s},i),t} - \bar{D}_{g(\bar{s},i,.)})}_{\textit{Varies at subgroup level}} - \underbrace{(\bar{D}_{\bar{g}(.,i),t} - \bar{\bar{D}})}_{\textit{Varies at groupXtime level}}] \tag{3.18}$$

Then we can collect the treatment terms to the subgroup level since all four variables are constant within subgroupXtime. Let $D_{g(s),t}$ *denote the value of* $D_{g(s,i),t}$ *for all units in subgroup* s *and let* $N_{g(s),t}$ *denote the number of units in subgroup* s *in period* t*, then* (3.18) *becomes*

$$\frac{1}{N}\sum_{g}\sum_{s\in g} N_{g(s),t} y_{g(s),t}[D_{g(\bar{s}),t} - \bar{D}_{g(s),.} - (\bar{D}_{\bar{g}(.),t} - \bar{\bar{D}})] \tag{3.19}$$

Then the numerator of (3.17) *becomes*

$$\frac{1}{NT}\sum_{t}\sum_{g}\sum_{s\in g} N_{g(s),t} y_{g(s),t}[D_{g(\bar{s}),t} - \bar{D}_{g(s),.} - (\bar{D}_{\bar{g}(.),t} - \bar{\bar{D}})] \tag{3.20}$$

Then note that

$$\begin{aligned}\hat{Var}(\tilde{D}) &= \frac{1}{NT}\sum_{t}\sum_{i}[(D_{g(s,i),t} - \bar{D}_{g(s,i),.}) - (\bar{D}_{\bar{g}(.,i),t} - \bar{\bar{D}})]^2 \\ &= \frac{1}{NT}\sum_{t}\sum_{i} D_{g(s,i),t}[(D_{g(s,i),t} - \bar{D}_{g(s,i),.}) - (\bar{D}_{\bar{g}(.,i),t} - \bar{\bar{D}})] \end{aligned} \tag{3.21}$$

By the same steps as above replacing Y_{it} with D_{it}, (3.21) becomes

$$= \frac{1}{NT}\sum_t \sum_g \sum_{s\in g} D_{g(s),t} N_{g(s),t}[(D_{g(s),t} - \bar{D}_{g(s),.}) - (\bar{D}_{\bar{g}(.),t} - \bar{\bar{D}})] \tag{3.22}$$

Then dividing (3.20) by (3.22) completes the proof.

■

Corollary 1. *Consider the case when all groups have one never treated subgroup and one treated subgroup. If the treated subgroup in every group gets treated in the same period, no unit reverts to being untreated after becoming treated, and Assumptions 1-5 hold, then*

$$\beta^{GT} = \delta^{TR}$$

Proof: Let $\tau \in \{1, ..., T\}$ be the period in which the treated subgroup in each group becomes treated. Then for all $t \geq \tau$:

$$\begin{aligned} \nu_{g(s),t} &= D_{g(s),t} - D_{g(s),.} - D_{\bar{g}(.),t} + D_{\bar{g}(.),.} \\ &= 1 - \frac{T-\tau+1}{T} - \frac{1}{2} + \frac{T-\tau+1}{2T} \\ &= \frac{1}{2} - \frac{T-\tau+1}{2T} \end{aligned}$$

The first equality follows from a balanced panel and the second from $D_{g(s),t}$ being a binary variable. Then the weights are:

$$
\begin{aligned}
w^{GT}_{g(s),\tau} &= \frac{\frac{1}{2} - \frac{T-\tau+1}{2T}}{\sum_g \sum_{(g(s),t):D_{g(s),t}=1} \frac{N_{g(s),t}}{N_1}\left(\frac{1}{2} - \frac{T-\tau+1}{2T}\right)} \\
&= \frac{1}{\sum_g \sum_{(g(s),t):D_{g(s),t}=1} \frac{N_{g(s),t}}{N_1}} \\
&= \frac{N_1}{\sum_g \sum_{(g(s),t):D_{g(s),t}=1} N_{g(s),t}} \\
&= 1
\end{aligned}
$$

Thus all of the weights are equal to 1 which implies $\beta^{GT} = \delta^{TR}$

■

Lemma 2 *Under assumptions 1 to 5, for each $g \in \{1, ..., G\}$, for all $[g(s), g(s'), t, t'] \in \{g(1), ..., g(S_g)\}^2 \times \{1, ..., T\}^2$*

$$
\begin{aligned}
&E[Y_{g(s),t}|\boldsymbol{D}] - E[Y_{g(s'),t}|\boldsymbol{D}] - (E[Y_{g(s'),t}|\boldsymbol{D}] - E[Y_{g(s'),t'}|\boldsymbol{D}]) \\
&= D_{g(s),t}E[\Delta_{g(s),t}|\boldsymbol{D}] - D_{g(s),t'}E[\Delta_{g(s),t'}|\boldsymbol{D}] - (D_{g(s'),t}E[\Delta_{g(s'),t}|\boldsymbol{D}] - D_{g(s'),t'}E[\Delta_{g(s'),t'}|\boldsymbol{D}])
\end{aligned}
$$

Proof: *For each $g \in \{1, ..., G\}$, for all $[g(s), g(s'), t, t'] \in \{g(1), ..., g(S_g)\}^2 \times \{1, ..., T\}^2$*

$$
\begin{aligned}
E[Y_{g(s),t}|\boldsymbol{D}] &= \mathbb{E}\left[\frac{1}{N_{g(s),t}} \sum_{i=1}^{N_{g(s),t}} Y_{g(s,i),t}|\boldsymbol{D}\right] \\
&= \mathbb{E}\left[\frac{1}{N_{g(s),t}} \sum_{i=1}^{N_{g(s),t}} Y_{g(s,i),t}(0) + D_{g(s,i),t}(Y_{g(s,i),t}(1) - Y_{g(s,i),t}(0))|\boldsymbol{D}\right] \\
&= E[Y_{g(s),t}(0)|\boldsymbol{D}] + D_{g(s),t}E[\Delta_{g(s),t}|\boldsymbol{D}] \\
&= E[Y_{g(s),t}(0)|\boldsymbol{D_{g(s)}}] + D_{g(s),t}E[\Delta_{g(s),t}|\boldsymbol{D}]
\end{aligned}
$$

The first equality holds by the definition of $Y_{g(s),t}$, the second by the definition of $Y_{g(s,i),t}$, the third by Assumption 2, and the fourth by Assumption 3. Therefore we have

$$
\begin{aligned}
&E[Y_{g(s),t}|\boldsymbol{D}] - E[Y_{g(s'),t}|\boldsymbol{D}] - (E[Y_{g(s'),t}|\boldsymbol{D}] - E[Y_{g(s'),t'}|\boldsymbol{D}]) \\
&= E[Y_{g(s),t}(0)|\boldsymbol{D_{g(s)}}] - E[Y_{g(s),t'}(0)|\boldsymbol{D_{g(s)}}] - (E[Y_{g(s'),t}(0)|\boldsymbol{D_{g(s)}}] - E[Y_{g(s'),t'}(0)|\boldsymbol{D_{g(s)}}]) \\
&+ D_{g(s),t}E[\Delta_{g(s),t}|\boldsymbol{D}] - D_{g(s),t'}E[\Delta_{g(s),t'}|\boldsymbol{D}] - (D_{g(s'),t}E[\Delta_{g(s'),t}|\boldsymbol{D}] - D_{g(s'),t'}E[\Delta_{g(s'),t'}|\boldsymbol{D}]) \\
&= E[Y_{g(s),t}(0) - Y_{g(s),t'}(0)] - E[Y_{g(s'),t}(0) - Y_{g(s'),t'}(0)] + D_{g(s),t}E[\Delta_{g(s),t}|\boldsymbol{D}] \\
&- D_{g(s),t'}E[\Delta_{g(s),t'}|\boldsymbol{D}] - (D_{g(s'),t}E[\Delta_{g(s'),t}|\boldsymbol{D}] - D_{g(s'),t'}E[\Delta_{g(s'),t'}|\boldsymbol{D}]) \\
&= D_{g(s),t}E[\Delta_{g(s),t}|\boldsymbol{D}] - D_{g(s),t'}E[\Delta_{g(s),t'}|\boldsymbol{D}] - (D_{g(s'),t}E[\Delta_{g(s'),t}|\boldsymbol{D}] - D_{g(s'),t'}E[\Delta_{g(s'),t'}|\boldsymbol{D}])
\end{aligned}
$$

Where the second equality holds by Assumption 4 and the third holds by Assumption 5.

■

Theorem 1. *Under assumptions 1 to 5, the coefficient from a regression on unit and group-by-time fixed effects is*

$$\beta^{GT} = \mathbb{E}\Big[\sum_{g}\sum_{(s,t):s\in g, D_{g(s),t}=1} \frac{N_{g(s),t}}{N_1} w^{GT}_{g(s),t}\Delta_{g(s),t}\Big]$$

Proof: By Lemma 1 we have

$$\hat{\beta}^{GT} = \frac{\sum_t\sum_g\sum_{s\in g} N_{g(s),t}\epsilon_{g(s),t}Y_{g(s),t}}{\sum_t\sum_g\sum_{s\in g} N_{g(s),t}\epsilon_{g(s),t}D_{g(s),t}}$$

Then we have

$$E[\hat{\beta}^{GT}|\boldsymbol{D}] = \frac{\sum_t\sum_g\sum_{s\in g} N_{g(s),t}\epsilon_{g(s),t}E[Y_{g(s),t}|\boldsymbol{D}]}{\sum_t\sum_g\sum_{s\in g} N_{g(s),t}\epsilon_{g(s),t}D_{g(s),t}} \tag{3.23}$$

Note that for each $g \in \{1, ..., G\}$:

$$\sum_{t=1}^{T} N_{g(s),t}\epsilon_{g(s),t} = 0 \text{ for all } s \in \{1, ..., S_g\} \tag{3.24}$$

$$\sum_{s=1}^{S_g} N_{g(s),t}\epsilon_{g(s),t} = 0 \text{ for all } s \in \{1, ..., T\} \tag{3.25}$$

Then for the numerator in (3.23) we have:

$$\begin{aligned}
&\sum_t \sum_g \sum_{s \in g} N_{g(s),t}\epsilon_{g(s),t} E[Y_{g(s),t}|\boldsymbol{D}] \\
&= \sum_t \sum_g \sum_{s \in g} N_{g(s),t}\epsilon_{g(s),t}(E[Y_{g(s),t}|\boldsymbol{D}] - E[Y_{g(s'),t}|\boldsymbol{D}] - (E[Y_{g(s'),t}|\boldsymbol{D}] - E[Y_{g(s'),t'}|\boldsymbol{D}])) \\
&= \sum_t \sum_g \sum_{s \in g} N_{g(s),t}\epsilon_{g(s),t}(D_{g(s),t}E[\Delta_{g(s),t}|\boldsymbol{D}] - D_{g(s,1)}E[\Delta_{g(s,1)}|\boldsymbol{D}] \\
&\qquad - (D_{g(1),t}E[\Delta_{g(1),t}|\boldsymbol{D}] - D_{g(1,1)}E[\Delta_{g(1,1)}|\boldsymbol{D}])) \\
&= \sum_t \sum_g \sum_{s \in g} N_{g(s),t}\epsilon_{g(s),t} D_{g(s),t} E[\Delta_{g(s),t}|\boldsymbol{D}] \\
&= \sum_g \sum_{g(s,t):D_{g(s,t)=1}} N_{g(s),t}\epsilon_{g(s),t} E[\Delta_{g(s),t}|\boldsymbol{D}]
\end{aligned} \tag{3.26}$$

The first and third equalities hold by (3.24) and (3.25). The second equality holds by Lemma 2 and the fourth by Assumption 2. For the denominator in (3.23) we have:

$$\sum_t \sum_g \sum_{s \in g} N_{g(s),t}\epsilon_{g(s),t} D_{g(s),t} = \sum_g \sum_{g(s,t):D_{g(s,t)=1}} N_{g(s),t}\epsilon_{g(s),t} \tag{3.27}$$

Then plugging (3.26) and (3.27) into (3.23) and applying the Law of Iterated Expectations we have

$$\beta^{GT} = \mathbb{E}\Big[\sum_g \sum_{(s,t):s \in g, D_{g(s),t}=1} \frac{N_{g(s),t}}{N_1} w^{GT}_{g(s),t} \Delta_{_{g(s),t}}\Big]$$

■

Appendix A

Appendix

A.1 Additional Tables and Figures

Table A.1: ACA Medicaid Expansion for Childless Adults by State

State	Has expanded to date?	Date of implementation	Had coverage before January 1, 2014?
Alabama	N	-	-
Alaska	Y	9/1/15	N
Arizona	Y	1/1/14	Y
Arkansas	Y	1/1/14	N
California	Y	1/1/14	N
Colorado	Y	1/1/14	Y
Connecticut	Y	1/1/14	Y
Delaware	Y	1/1/14	Y
D.C.	Y	1/1/14	Y
Florida	N	-	-
Georgia	N	-	-
Hawaii	Y	1/1/14	Y
Idaho	Y	1/1/20	N
Illinois	Y	1/1/14	N
Indiana	Y	2/1/15	N
Iowa	Y	1/1/14	N
Kansas	N	-	-

Continued on next page

State	Has expanded to date?	Date of implementation	Had coverage before January 1, 2014?
Kentucky	Y	1/1/14	N
Louisiana	Y	7/1/16	N
Maine	Y	1/10/19	N
Maryland	Y	1/1/14	N
Massachusetts	Y	1/1/14	N
Michigan	Y	4/1/14	N
Minnesota	Y	1/1/14	Y
Mississippi	N	-	-
Missouri	Y	7/1/21	N
Montana	Y	1/1/16	N
Nebraska	Y	10/1/20	N
Nevada	Y	1/1/14	N
New Hampshire	Y	8/15/14	N
New Jersey	Y	1/1/14	N
New Mexico	Y	1/1/14	N
New York	Y	1/1/14	Y
North Carolina	N	-	-
North Dakota	Y	1/1/14	N
Ohio	Y	1/1/14	N
Oklahoma	Y	7/1/21	N
Oregon	Y	1/1/14	N
Pennsylvania	Y	1/1/15	N
Rhode Island	Y	1/1/14	N
South Carolina	N	-	-
South Dakota	N	-	-
Tennessee	N	-	-
Texas	N	-	-
Utah[a]	Y	1/1/20	N
Vermont	Y	1/1/14	Y
Virginia	Y	1/1/19	N
Washington	Y	1/1/14	N
West Virginia	Y	1/1/14	N
Wisconsin[b]	N	-	-
Wyoming	N	-	-

Notes: All information comes from a compilation of the Kaiser Family Foundation, healthinsurance.org, and [57].

[a] Utah expanded eligibility to adults under 100% of the FPL in April 2019 and expanded fully to 138% of the FPL in January 2020.

[b] Wisconsin did not adopt the ACA expansion, but it does cover adults up to 100% of the FPL with Medicaid.

Table A.2: Comparison of Individuals in Sample to Individuals out of Sample

	26 to 39			40 to 64		
	In sample	Out of sample	P-value of diff.	In sample	Out of sample	P-value of diff.
Any insurance	0.46	0.74	0.00	0.66	0.83	0.00
	(0.50)	(0.44)		(0.47)	(0.38)	
Between 0-138% FPL	0.37	0.24	0.00	0.32	0.18	0.00
	(0.48)	(0.43)		(0.47)	(0.38)	
Household size	3.41	3.44	0.74	2.86	2.89	0.63
	(2.07)	(1.71)		(1.68)	(1.53)	
Number of rooms	5.49	5.67	0.00	5.55	6.38	0.00
	(2.12)	(2.37)		(2.03)	(2.57)	
Persons per room	0.67	0.66	0.60	0.56	0.50	0.00
	(0.57)	(0.40)		(0.38)	(0.34)	
Persons per bedroom	1.31	1.30	0.81	1.11	1.01	0.00
	(0.76)	(0.67)		(0.64)	(0.56)	
Moved in past year	0.21	0.22	0.81	0.12	0.10	0.00
	(0.41)	(0.41)		(0.32)	(0.30)	
Live in rented housing	0.53	0.46	0.00	0.38	0.24	0.00
	(0.50)	(0.50)		(0.49)	(0.43)	
Lives with parent(s)	0.41	0.15	0.00	0.10	0.06	0.00
	(0.49)	(0.36)		(0.31)	(0.23)	
Has a difficulty	0.15	0.07	0.00	0.25	0.14	0.00
	(0.36)	(0.25)		(0.44)	(0.35)	
Employed	0.54	0.75	0.00	0.52	0.70	0.00
	(0.50)	(0.43)		(0.50)	(0.46)	
Married	0.21	0.53	0.00	0.55	0.67	0.00
	(0.41)	(0.50)		(0.50)	(0.47)	
Female	0.30	0.50	0.00	0.48	0.51	0.00
	(0.46)	(0.50)		(0.50)	(0.50)	
White	0.60	0.71	0.00	0.64	0.77	0.00
	(0.49)	(0.46)		(0.48)	(0.42)	
Black	0.24	0.14	0.00	0.19	0.12	0.00
	(0.43)	(0.34)		(0.39)	(0.33)	
Hispanic	0.28	0.21	0.03	0.26	0.13	0.00
	(0.45)	(0.41)		(0.44)	(0.33)	
Total individual income	17,554.54	38,225.69	0.00	22,972.88	51,305.68	0.00
	(25,732.35)	(45,212.01)		(33,202.60)	(66,704.46)	
Sample size	12,257	805,509		61,259	1,691,316	

Notes: The individuals in this table include 26 to 64 year olds in the 37 treatment and control states in 2012 and 2013. Individuals in the columns labeled "In sample" include those with less than a high school degree, no own minor children in the household, and U.S. citizenship who live in an MSA, do not live in group quarters, and do not receive supplemental security income. Individuals in the columns labeled "Out of sample" include all other individuals aged 26 to 64 in the 37 treatment and control states. Means are listed first with standard deviations in parentheses below, which are weighted by the square root of the IPUMS person weight. The p-values for the difference between individuals in the sample and individuals out of the sample are found by estimating a WLS regression of the variable of interest on a binary indicator for being in the sample with standard errors clustered by state. Total income is in 2019 dollars.

Table A.3: Main Results Using Different Age Groups

	(1) Household size	(2) Number of rooms	(3) Persons per room	(4) Persons per bedroom
a) 26 to 44 year olds				
Treatment effect	-0.089	-0.061	-0.017	-0.026
	(0.034)	(0.051)	(0.011)	(0.012)
Y mean	3.269	5.457	0.648	1.266
Cluster p	0.012	0.240	0.142	0.042
Boot p	0.033	0.259	0.272	0.075
Clusters	37	37	37	37
MSAs	211	211	211	211
N	71,479	71,479	71,479	69,767
b) 45 to 64 year olds				
Treatment effect	0.015	0.013	-0.004	-0.004
	(0.023)	(0.034)	(0.006)	(0.008)
Y mean	2.837	5.565	0.553	1.093
Cluster p	0.529	0.709	0.517	0.593
Boot p	0.566	0.788	0.589	0.634
Clusters	37	37	37	37
MSAs	211	211	211	211
N	221,398	221,398	221,398	216,889

Notes: The coefficient estimates in the table are found by estimating equation 2.1 with a WLS regression. The weights are the square root of the IPUMS person weight. The estimated coefficient $\hat{\beta}$ is reported as the treatment effect, and the cluster robust standard error is below it in parentheses. Y mean reports the pre-2014 outcome mean. Cluster p reports the p-value found using robust standard errors, clustered by state. Boot p reports the p-value found by using the wild cluster bootstrap method with 999 replications and Rademacher weights. Clusters reports the number of clusters (states) used for inference. MSAs reports the number of MSAs used in the regression. N reports the sample size.

Table A.4: Comparison of 2014 Marketplace Subsidies between Treatment and Control for College-Educated Individuals

	(1) Household size	(2) Number of rooms	(3) Persons per room	(4) Persons per bedroom
a) 26 to 39 year olds				
Interaction	-0.021	-0.056	0.000	-0.008
	(0.018)	(0.053)	(0.003)	(0.007)
Y mean	2.189	5.481	0.444	0.933
Cluster p	0.264	0.298	0.911	0.249
Boot p	0.289	0.352	0.909	0.264
Clusters	36	36	36	36
MSAs	199	199	199	199
N	163,972	163,972	163,972	161,029
b) 40 to 64 year olds				
Interaction	-0.000	-0.074	0.003	0.001
	(0.013)	(0.036)	(0.002)	(0.004)
Y mean	2.135	6.964	0.340	0.728
Cluster p	0.996	0.046	0.122	0.898
Boot p	0.992	0.099	0.199	0.903
Clusters	36	36	36	36
MSAs	199	199	199	199
N	358,651	358,651	358,651	356,432

Notes: The coefficient estimates in the table are found by estimating a variant of equation 2.1 with a WLS regression. The weights are the square root of the IPUMS person weight. The coefficient on the interaction between being an expansion state and the year being 2014 is listed as “Interaction,” and the cluster robust standard error is below it in parentheses. Y mean reports the pre-2014 outcome mean. Cluster p reports the p-value found using robust standard errors, clustered by state. Boot p reports the p-value found by using the wild cluster bootstrap method with 999 replications and Rademacher weights. Clusters reports the number of clusters (states) used for inference. MSAs reports the number of MSAs used in the regression. N reports the sample size. There are only 36 clusters and 199 MSAs in these regressions because 12 MSAs lack Zillow controls for 2012 through 2014, the years of data used in these regressions.

A.2 Tables for 40 to 64 year olds

Table A.5: Breakdown of Household Size for 40 to 64 Year Olds

	(1) Number of family members	(2) Number of non-family members	(3) Number of immediate family members	(4) Number of extended family members
Treatment effect	0.028	-0.009	0.009	0.019
	(0.019)	(0.014)	(0.012)	(0.013)
Y mean	1.634	0.216	1.089	0.545
Cluster p	0.149	0.533	0.483	0.161
Boot p	0.197	0.565	0.526	0.216
Clusters	37	37	37	37
MSAs	211	211	211	211
N	241,340	241,340	241,340	241,340

Notes: The coefficient estimates in the table are found by estimating equation 2.1 with a WLS regression. The weights are the square root of the IPUMS person weight. The estimated coefficient $\hat{\beta}$ is reported as the treatment effect, and the cluster robust standard error is below it in parentheses. Y mean reports the pre-2014 outcome mean. Cluster p reports the p-value found using robust standard errors, clustered by state. Boot p reports the p-value found by using the wild cluster bootstrap method with 999 replications and Rademacher weights. Clusters reports the number of clusters (states) used for inference. MSAs reports the number of MSAs used in the regression. N reports the sample size.

Table A.6: Breakdown of Age of Family Members for 40 to 64 Year Olds

	(1) Number of minor family members	(2) Number of adult family members	(3) Number of senior family members
Treatment effect	0.012	0.019	-0.002
	(0.011)	(0.016)	(0.006)
Y mean	0.260	1.156	0.218
Cluster p	0.279	0.248	0.690
Boot p	0.328	0.283	0.707
Clusters	37	37	37
MSAs	211	211	211
N	241,340	241,340	241,340

Notes: The coefficient estimates in the table are found by estimating equation 2.1 with a WLS regression. The weights are the square root of the IPUMS person weight. The estimated coefficient $\hat{\beta}$ is reported as the treatment effect, and the cluster robust standard error is below it in parentheses. Y mean reports the pre-2014 outcome mean. Cluster p reports the p-value found using robust standard errors, clustered by state. Boot p reports the p-value found by using the wild cluster bootstrap method with 999 replications and Rademacher weights. Clusters reports the number of clusters (states) used for inference. MSAs reports the number of MSAs used in the regression. N reports the sample size.

Table A.7: Effect of ACA Medicaid Expansion on Moving within the Past Year and Living in the Principal City of an MSA for 40 to 64 Year Olds

	(1) 1(Moved within past year)	(2) 1(Live in principal city of MSA)
Treatment effect	-0.002	-0.012
	(0.004)	(0.009)
Y mean	0.115	0.162
Cluster p	0.623	0.187
Boot p	0.647	0.204
Clusters	37	37
MSAs	211	211
N	241,340	241,340

Notes: The coefficient estimates in the table are found by estimating equation 2.1 with a WLS regression. The weights are the square root of the IPUMS person weight. The estimated coefficient $\hat{\beta}$ is reported as the treatment effect, and the cluster robust standard error is below it in parentheses. Y mean reports the pre-2014 outcome mean. Cluster p reports the p-value found using robust standard errors, clustered by state. Boot p reports the p-value found by using the wild cluster bootstrap method with 999 replications and Rademacher weights. Clusters reports the number of clusters (states) used for inference. MSAs reports the number of MSAs used in the regression. N reports the sample size.

Table A.8: Heterogeneity by Race and Ethnicity for 40 to 64 Year Olds

	(1) Household size	(2) Number of rooms	(3) Persons per room	(4) Persons per bedroom
a) All 40 to 64 year olds				
Treatment effect	0.019	0.017	-0.003	-0.002
	(0.021)	(0.032)	(0.006)	(0.008)
Y mean	2.850	5.543	0.557	1.102
Cluster p	0.369	0.604	0.589	0.792
Boot p	0.416	0.729	0.660	0.831
Clusters	37	37	37	37
MSAs	211	211	211	211
N	241,340	241,340	241,340	236,334
b) White 40 to 64 year olds				
Treatment effect	0.025	-0.035	0.004	0.002
	(0.027)	(0.039)	(0.007)	(0.010)
Y mean	2.772	5.634	0.533	1.068
Cluster p	0.360	0.367	0.584	0.856
Boot p	0.413	0.443	0.623	0.864
Clusters	37	37	37	37
MSAs	211	211	211	211
N	157,279	157,279	157,279	154,640
c) Black 40 to 64 year olds				
Treatment effect	0.049	0.146	-0.017	-0.007
	(0.046)	(0.064)	(0.013)	(0.015)
Y mean	2.597	5.321	0.524	1.069
Cluster p	0.292	0.028	0.174	0.649
Boot p	0.296	0.053	0.263	0.663
Clusters	36	36	36	36
MSAs	202	202	202	201
N	38,525	38,525	38,525	37,483
d) Hispanic 40 to 64 year olds				
Treatment effect	0.050	0.103	-0.008	0.003
	(0.047)	(0.060)	(0.010)	(0.013)
Y mean	3.434	5.296	0.699	1.321
Cluster p	0.295	0.097	0.458	0.808
Boot p	0.405	0.237	0.513	0.767
Clusters	36	36	36	36
MSAs	209	209	209	207
N	66,903	66,903	66,903	64,889

Notes: The coefficient estimates in the table are found by estimating equation 2.1 with a WLS regression. The weights are the square root of the IPUMS person weight. The estimated coefficient $\hat{\beta}$ is reported as the treatment effect, and the cluster robust standard error is below it in parentheses. Y mean reports the pre-2014 outcome mean. Cluster p reports the p-value found using robust standard errors, clustered by state. Boot p reports the p-value found by using the wild cluster bootstrap method with 999 replications and Rademacher weights. Clusters reports the number of clusters (states) used for inference. MSAs reports the number of MSAs used in the regression. N reports the sample size.

Table A.9: Heterogeneity by MSA Housing Costs for 40 to 64 Year Olds

	(1) Household size	(2) Number of rooms	(3) Persons per room	(4) Persons per bedroom
a) All 40 to 64 year olds				
Treatment effect	0.019	0.017	-0.003	-0.002
	(0.021)	(0.032)	(0.006)	(0.008)
Y mean	2.850	5.543	0.557	1.102
Cluster p	0.369	0.604	0.589	0.792
Boot p	0.416	0.729	0.660	0.831
Clusters	37	37	37	37
MSAs	211	211	211	211
N	241,340	241,340	241,340	236,334
b) 40 to 64 year olds in above-median MSAs				
Treatment effect	0.013	0.043	-0.008	-0.008
	(0.022)	(0.045)	(0.007)	(0.008)
Y mean	2.973	5.508	0.589	1.150
Cluster p	0.546	0.342	0.240	0.364
Boot p	0.573	0.485	0.408	0.429
Clusters	31	31	31	31
MSAs	104	104	104	104
N	158,475	158,475	158,475	154,639
c) 40 to 64 year olds in below-median MSAs				
Treatment effect	0.024	-0.039	0.007	0.002
	(0.037)	(0.044)	(0.008)	(0.013)
Y mean	2.639	5.606	0.503	1.021
Cluster p	0.517	0.381	0.357	0.850
Boot p	0.539	0.425	0.396	0.883
Clusters	25	25	25	25
MSAs	104	104	104	104
N	82,511	82,511	82,511	81,346

Notes: The coefficient estimates in the table are found by estimating equation 2.1 with a WLS regression. The weights are the square root of the IPUMS person weight. The estimated coefficient $\hat{\beta}$ is reported as the treatment effect, and the cluster robust standard error is below it in parentheses. Y mean reports the pre-2014 outcome mean. Cluster p reports the p-value found using robust standard errors, clustered by state. Boot p reports the p-value found by using the wild cluster bootstrap method with 999 replications and Rademacher weights. Clusters reports the number of clusters (states) used for inference. MSAs reports the number of MSAs used in the regression. N reports the sample size.

Table A.10: Check the Inclusion of Different Covariates for 40 to 64 Year Olds

	(1)	(2)	(3)	(4)
a) Household size				
Treatment effect	0.020	0.024	0.008	0.019
	(0.026)	(0.024)	(0.021)	(0.021)
Y mean	2.860	2.860	2.860	2.860
Cluster p	0.449	0.315	0.710	0.369
Boot p	0.542	0.412	0.751	0.416
Clusters	37	37	37	37
MSAs	211	211	211	211
N	241,340	241,340	241,340	241,340
b) Number of rooms				
Treatment effect	-0.002	0.003	0.017	0.017
	(0.027)	(0.028)	(0.029)	(0.032)
Y mean	5.547	5.547	5.547	5.547
Cluster p	0.937	0.922	0.545	0.604
Boot p	0.953	0.944	0.675	0.729
Clusters	37	37	37	37
MSAs	211	211	211	211
N	241,340	241,340	241,340	241,340
c) Persons per room				
Treatment effect	0.001	0.002	-0.006	-0.003
	(0.005)	(0.005)	(0.005)	(0.006)
Y mean	0.559	0.559	0.559	0.559
Cluster p	0.809	0.748	0.241	0.589
Boot p	0.814	0.765	0.327	0.660
Clusters	37	37	37	37
MSAs	211	211	211	211
N	241,340	241,340	241,340	241,340
d) Persons per bedroom				
Treatment effect	0.007	0.007	-0.003	-0.002
	(0.007)	(0.007)	(0.007)	(0.008)
Y mean	1.106	1.106	1.106	1.106
Cluster p	0.371	0.346	0.629	0.792
Boot p	0.411	0.384	0.662	0.831
Clusters	37	37	37	37
MSAs	211	211	211	211
N	236,334	236,334	236,334	236,334
MSA and year FE	X	X	X	X
Demographics		X	X	X
State-year controls			X	X
Housing controls				X

Notes: The coefficient estimates in the table are found by estimating equation 2.1 with a WLS regression. The weights are the square root of the IPUMS person weight. The estimated coefficient $\hat{\beta}$ is reported as the treatment effect, and the cluster robust standard error is below it in parentheses. Y mean reports the pre-2014 outcome mean. Cluster p reports the p-value found using robust standard errors, clustered by state. Boot p reports the p-value found by using the wild cluster bootstrap method with 999 replications and Rademacher weights. Clusters reports the number of clusters (states) used for inference. MSAs reports the number of MSAs used in the regression. N reports the sample size.

Table A.11: Check Variants of Fixed Effects and Other Miscellaneous Specification Checks for 40 to 64 Year Olds

	(1) MSA-by-state FEs	(2) Drop cross-border MSAs	(3) Propensity between 0.1 and 0.9	(4) Drop work require-ment state	(5) Binary treatment	(6) Just 2014 adopters
a) Household size						
Treatment effect	0.029	0.050	-0.017	0.017	0.015	0.033
	(0.021)	(0.025)	(0.022)	(0.021)	(0.021)	(0.026)
Y mean	2.850	2.875	2.691	2.852	2.850	2.886
Cluster p	0.161	0.055	0.462	0.421	0.460	0.204
Boot p	0.184	0.085	0.480	0.440	0.513	0.243
Clusters	37	36	18	36	37	31
MSAs	231	192	132	209	211	178
N	241,340	195,119	121,984	240,139	241,340	214,485
b) Number of rooms						
Treatment effect	0.016	0.038	-0.023	0.016	0.021	-0.006
	(0.033)	(0.039)	(0.031)	(0.032)	(0.031)	(0.040)
Y mean	5.543	5.495	5.555	5.543	5.543	5.509
Cluster p	0.633	0.340	0.481	0.619	0.503	0.885
Boot p	0.755	0.514	0.516	0.731	0.627	0.918
Clusters	37	36	18	36	37	31
MSAs	231	192	132	209	211	178
N	241,340	195,119	121,984	240,139	241,340	214,485
c) Persons per room						
Treatment effect	-0.002	-0.003	-0.001	-0.003	-0.005	-0.002
	(0.006)	(0.008)	(0.006)	(0.006)	(0.005)	(0.007)
Y mean	0.557	0.568	0.519	0.558	0.557	0.568
Cluster p	0.790	0.688	0.865	0.554	0.352	0.784
Boot p	0.844	0.774	0.882	0.624	0.455	0.839
Clusters	37	36	18	36	37	31
MSAs	231	192	132	209	211	178
N	241,340	195,119	121,984	240,139	241,340	214,485
d) Persons per bedroom						
Treatment effect	0.002	0.001	-0.007	-0.003	-0.004	0.009
	(0.008)	(0.009)	(0.009)	(0.008)	(0.007)	(0.010)
Y mean	1.102	1.113	1.040	1.103	1.102	1.117
Cluster p	0.824	0.949	0.438	0.716	0.631	0.389
Boot p	0.829	0.953	0.511	0.721	0.681	0.379
Clusters	37	36	18	36	37	31
MSAs	231	192	132	209	211	178
N	236,334	190,914	119,985	235,151	236,334	209,876

Notes: The coefficient estimates in the table are found by estimating equation 2.1 with a WLS regression. The weights are the square root of the IPUMS person weight. The estimated coefficient $\hat{\beta}$ is reported as the treatment effect, and the cluster robust standard error is below it in parentheses. Y mean reports the pre-2014 outcome mean. Cluster p reports the p-value found using robust standard errors, clustered by state. Boot p reports the p-value found by using the wild cluster bootstrap method with 999 replications and Rademacher weights. Clusters reports the number of clusters (states) used for inference. MSAs reports the number of MSAs used in the regression. N reports the sample size.

Table A.12: Look at Binary Outcomes of Persons per Room Exceeding One, Persons per Bedroom Exceeding Two, and Doubling-Up for 40 to 64 Year Olds

	(1) 1(Persons per room>1)	(2) 1(Persons per bedroom>2)	(3) 1(Doubled-up)
Treatment effect	-0.000	-0.001	0.003
	(0.002)	(0.003)	(0.005)
Y mean	0.057	0.063	0.507
Cluster p	0.979	0.579	0.608
Boot p	0.977	0.612	0.653
Clusters	37	37	37
MSAs	211	211	211
N	241,340	241,340	241,340

Notes: The coefficient estimates in the table are found by estimating equation 2.1 with a WLS regression. The weights are the square root of the IPUMS person weight. The estimated coefficient $\hat{\beta}$ is reported as the treatment effect, and the cluster robust standard error is below it in parentheses. Y mean reports the pre-2014 outcome mean. Cluster p reports the p-value found using robust standard errors, clustered by state. Boot p reports the p-value found by using the wild cluster bootstrap method with 999 replications and Rademacher weights. Clusters reports the number of clusters (states) used for inference. MSAs reports the number of MSAs used in the regression. N reports the sample size.

A.3 Description of Zillow controls

Zillow provides the ZHVI median and bottom-tier indices as well as the ZRI index for each month on their website. We link Zillow defined metro areas to Census metro areas using the crosswalk provided by Zillow. One issue for our sample is that beginning in November of 2019, Zillow changed the way it constructs the ZHVI median and bottom-tier values. For example, the middle ZHVI value represented a median value for a region, but Zillow states that the new ZHVI should be interpreted as the “typical” home value for a region.[1] We therefore extrapolate the values of the ZHVI median and bottom-tier index in our data. To do this, for each index we estimate a linear regression model of the missing month (either November or December) on the index value for all previous months (the values for January to October) and MSA fixed effects. We then use this model to predict the values for November and December of 2019. Finally, we annualize

[1] See: `https://www.zillow.com/research/zhvi-user-guide/`.

the data by taking an unweighted average across all months. The Zillow data is missing for certain MSA-month combinations. Given the non-linearities in housing prices within calendar years, we therefore only construct Zillow annual estimates for MSA's that have data for all 12 months to avoid biasing our housing controls. The two tables below show our main results using either no Zillow controls and the unrestricted sample with all 221 possible MSAs (Table A.13) or linearly interpolated Zillow controls that fill in the missing months for MSA-years that had at least 6 months of reported Zillow data (Table A.14). Both tables indicate our results are robust to these variations of Zillow controls and sample.

Table A.13: Main Results and Breakdown of Number of Rooms, Zillow Controls Not Used

	(1) Household size	(2) Number of rooms	(3) Persons per room	(4) Persons per bedroom	(5) *Number of bedrooms*	(6) *Number of other rooms*
a) 26 to 39 year olds						
Treatment effect	-0.120	-0.079	-0.015	-0.035	-0.027	-0.052
	(0.035)	(0.044)	(0.012)	(0.019)	(0.024)	(0.028)
Y mean	3.397	5.494	0.671	1.303	2.736	2.758
Cluster p	0.001	0.080	0.203	0.070	0.260	0.068
Boot p	0.006	0.070	0.291	0.135	0.252	0.061
Clusters	37	37	37	37	37	37
MSAs	221	221	221	221	221	221
N	53,559	53,559	53,559	52,299	53,559	53,559
b) 40 to 64 year olds						
Treatment effect	0.000	0.015	-0.006	-0.005	-0.001	0.016
	(0.020)	(0.030)	(0.004)	(0.006)	(0.018)	(0.016)
Y mean	2.850	5.543	0.557	1.102	2.728	2.815
Cluster p	0.988	0.616	0.149	0.440	0.943	0.312
Boot p	0.985	0.727	0.224	0.485	0.949	0.404
Clusters	37	37	37	37	37	37
MSAs	221	221	221	221	221	221
N	251,986	251,986	251,986	246,803	251,986	251,986

Notes: The coefficient estimates in the table are found by estimating equation 2.1 with a WLS regression. The weights are the square root of the IPUMS person weight. The estimated coefficient $\hat{\beta}$ is reported as the treatment effect, and the cluster robust standard error is below it in parentheses. Y mean reports the pre-2014 outcome mean. Cluster p reports the p-value found using robust standard errors, clustered by state. Boot p reports the p-value found by using the wild cluster bootstrap method with 999 replications and Rademacher weights. Clusters reports the number of clusters (states) used for inference. MSAs reports the number of MSAs used in the regression. N reports the sample size.

Table A.14: Main Results and Breakdown of Number of Rooms, Linearly Interpolated Zillow Controls Used

	(1) Household size	(2) Number of rooms	(3) Persons per room	(4) Persons per bedroom	(5) *Number of bedrooms*	(6) *Number of other rooms*
a) 26 to 39 year olds						
Treatment effect	-0.127	-0.083	-0.020	-0.041	-0.022	-0.062
	(0.038)	(0.048)	(0.014)	(0.017)	(0.023)	(0.031)
Y mean	3.397	5.494	0.671	1.303	2.736	2.758
Cluster p	0.002	0.091	0.161	0.020	0.350	0.051
Boot p	0.007	0.088	0.277	0.059	0.335	0.049
Clusters	37	37	37	37	37	37
MSAs	212	212	212	212	212	212
N	52,927	52,927	52,927	51,682	52,927	52,927
b) 40 to 64 year olds						
Treatment effect	0.017	0.013	-0.003	-0.002	0.006	0.007
	(0.020)	(0.032)	(0.005)	(0.007)	(0.017)	(0.018)
Y mean	2.850	5.543	0.557	1.102	2.728	2.815
Cluster p	0.392	0.677	0.615	0.746	0.724	0.691
Boot p	0.433	0.782	0.679	0.774	0.821	0.749
Clusters	37	37	37	37	37	37
MSAs	212	212	212	212	212	212
N	248,627	248,627	248,627	243,501	248,627	248,627

Notes: The coefficient estimates in the table are found by estimating equation 2.1 with a WLS regression. The weights are the square root of the IPUMS person weight. The estimated coefficient $\hat{\beta}$ is reported as the treatment effect, and the cluster robust standard error is below it in parentheses. Y mean reports the pre-2014 outcome mean. Cluster p reports the p-value found using robust standard errors, clustered by state. Boot p reports the p-value found by using the wild cluster bootstrap method with 999 replications and Rademacher weights. Clusters reports the number of clusters (states) used for inference. MSAs reports the number of MSAs used in the regression. N reports the sample size.

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