The Impact of Public Policy on Nonstandard Work Arrangements

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Abstract

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The social compact between workers, firms, and governing bodies has been typified by the balancing of market efficiency, social fairness, and bargaining power. This balancing act has produced the minimum wage, overtime pay, protections from discrimination in hiring and firing, unemployment insurance, social security, and so much more. Yet, whether it is through an explicit terminology of employment or an implicit organizational structure, the classification of who is an employee remains a critical component of who has access to the benefits of this bargaining effort. The criteria of this classification has changed over time with expansions in the Fair Labor Standards Act, the common law agency test, the economic reality test and the ABC test (Shimabukuro, 2021). This dissertation explores the ways in which public policy impacts the less-conventional forms of work in the United States. The chapters of this dissertation draw on prior work relating to the minimum wage, employer supplied health insurance, nonstandard work arrangements, monopsonistic competition, and tax evasion to examine the impacts of public programs and government regulation of the labor market on self-employment and multiple jobholding.

"A learning experience is one of those things that says, 'You know that thing you just did? Don't do that.'"

Douglas Adams

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Finally, thank you to Caitlin Hand. I'd have certainly gone insane writing this if not for your constant support and willingness to fight to break me out of the cycle of long work days and restless nights. I would not have made it without you.

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The works presented here are dedicated to my parents Andy and Lisa Glasner. They have been, and always will be, my greatest heroes. Not a day goes by that I do not see the impact they have had on my life, and this work is a small step toward repaying their time and effort . . .

Executive Summary

As North (1991) notes, "institutions are the rules of the game in a society," and the rules we have made to govern labor in the United States carry with them a set of expected and unexpected consequences. When debating the introduction of the Fair Labor Standards Act (FLSA), divisions arose not only across but within political parties, with variation in geographies of work playing a crucial role (Fleck, 2002).[1] Labor policy is often about both the policy and who has access to it. The question of access has systematically favored employee-employer relationships in industries dominated by men, and in particular white men. (Derenoncourt and Montialoux, 2021; Hyman, 2018). This has been to the detriment of many, so much so that an extension of the minimum wage to previously uncovered sectors of the economy was among the 10 demands by the Civil Rights Movement in the March on Washington for Jobs and Freedom in 1963. Specifically, the 9th demand read "[We demand] a broadened Fair Labor Standards Act to include all areas of employment that are presently excluded" (Derenoncourt and Montialoux, 2021). Those workers with access to the FLSA are also more likely to have access to unemployment insurance, social security, employer supplied health insurance, retirement support, tax withholding, paid sick-leave, and more (Harris and Krueger, 2015).

This classification of who is an employee and who qualifies for any given policy is a direct result of how firms organize and how policymakers define employment relationships within a firm. From the perspective of Coase (1937) and Williamson (1975), the decision of employee classification and inclusion in the firm is one of efficiency, and not social appropriateness. This comes from a transaction cost perspective of organizations. The fact that we have a class of workers excluded from these social policies is a result of the type of work being done and the manner in which it is performed, and not the workers themselves. Yet, the linking between access to labor market policies, employment classification, and demographic characteristics raises questions about the positivist perspective of classification (Hyman, 2018; Derenoncourt and Montialoux, 2021; Harris and Krueger, 2015).

Due to the nature of labor policy design in the U.S., a free-rider problem has formed where firms in the online gig economy do not need to recognize those supplying goods and services on their marketplaces as workers (Sundararajan, 2016). These firms are not penalized for failing to supply health insurance or other benefit packages, as the platform participants are not employees. The institutional setting of the U.S. creates an environment where the classification of a worker as an employee increases the costs to internalize workers. This institutional setting results in certain business activities on the margin moving

[1] One of the more common explanations of the divisions in the democratic party at the time of passing the FLSA was regarding the distributive effects of the minimum wage provision. It was feared that a national minimum wage would undercut the low-wage industries in the South, making them noncompetitive.

from internalized processes to market activities, leading some firms outside the online gig economy to also free ride when ever possible (Gramm and Schnell, 2001; Weil, 2014; Hyman, 2018).

In this dissertation, I examine the effect of two distinct policies implemented by the U.S. on nonstandard work arrangements. In particular, I identify the effect of the minimum wage on self-employment, independent contracting, the online gig economy, and multiple jobholding. I also address the effect of Medicaid expansion resulting from the Affordable Care Act on self-employment. These examinations tie directly into an interrogation of what the consequences of the "rules of the game" are, and speak to ways of improving our self-imposed constraints.

All three chapters are centered around what effect the policies in question have on the realized labor allocation of workers. To do this, I take advantage of both administrative and survey data sources including Nonemployer Statistics, the American Community Survey, the Current Population Survey, and the Survey of Income and Program Participation. Each offers a distinct window into how individuals interact with the labor market. Recent work highlights where these data sources disagree. Across the various works and data sources we find different samples, definitions, and methodologies, and when used together it is unclear just how many people are participating in self-employment (Abraham et al., 2018b; Abraham et al., 2021). The scope of nonstandard work arrangements and self-employment is broad and captures a wide variety of work (Bracha and Burke, 2016; Jackson, Looney, and Ramnath, 2017a; Abraham et al., 2018b; Abraham, Hershbein, and Houseman, 2021). This breadth of work is rapidly changing (Sundararajan, 2016; Farrell and Greig, 2016; Hyman, 2018), which has left both policymakers and worker advocates with a lack of clarity on what steps need to be taken to support these workers.

The inconsistency in data sources and fast pace of innovation have left policymakers ill-equipped and under-prepared to legislate on new organizational structures (Harris and Krueger, 2015). This dissertation focuses on two policies in their broadest sense, the minimum wage and Medicaid, and explores their effects in the context of the freshly forming literature on self-employment in the age of the online gig economy and nonstandard work arrangements. Chapter 1 and 3 explore the effect of the minimum wage. Chapter 1 focuses on the creation of a covered and uncovered labor market for the administration of a minimum wage, with the uncovered labor market being measured by nonemployer establishments.[2] Chapter 3 focuses instead on the labor-leisure trade-off centered around workers and their willingness to take up multiple jobs at once.

In chapter 1, I use nonemployer statistics to study the effect of the minimum wage on uncovered workers. Nonemployer establishments are businesses that

[2]Nonemployer establishments are businesses that do not have paid employees, primarily composed of the unincorporated self-employed and independent contractors. A more detailed definition can be found in the data section of this paper.

do not have paid employees, and are primarily unincorporated self-employed individuals, including independent contractors. I find evidence that when minimum wages increase, nonemployer establishments in general are largely unaffected. This result does not hold for the online gig economy. For a 10% increase in the minimum wage, the count of nonemployer establishments classified as transportation and warehousing services, the industry subset which includes Uber and Lyft, increases by 3.9%. This positive relationship is driven by counties with low labor market concentration and an active online gig economy, as measured by the presence of Uber. The findings in chapter 1 support the conclusion that transition between work that is covered and uncovered by the minimum wage has become easier, and spillover across the two markets is more likely in analyses post-2012 and in urban areas.

Similar to chapter 1, chapter 2 focuses on the willingness of workers to take up self-employment. Instead of using the minimum wage as a treatment, chapter 2 leverages the expansion in Medicaid taken up by states following the passage of the Affordable Care Act. While chapter 1 explores the dynamics of a policy which creates two distinct markets, and tests for spillover, chapter 2 highlights an access problem. This expansion in Medicaid coverage had the greatest impact on non-elderly low-income adults without children younger than 18 (Leung and Mas, 2016), and offers an example of a means-tested program defined by an individual's modified adjusted gross income (MAGI). For most employees, both the individual and their employer report their income to the IRS, but for the self-employed, no secondary report is required. This structure places the self-employed in a powerful position when it comes to means-tested programs. The self-employed are known to manipulate their reported income in reference to means-tested programs (Andreoni, Erard, and Feinstein, 1998; Saez, 2010; Chetty et al., 2012; Chetty, Friedman, and Saez, 2013), and this chapter explores the effect in reference to two alternative mechanisms, job lock and employment lock.

Job lock has some similarity to the mechanism explored in chapter 1. In chapter 2, I define job lock as the tendency for workers to feel they cannot leave a job due to the loss in benefits incurred by leaving. Changes in compensation packages by alternative employment or work opportunities can overcome the job-lock effect, leading to a change in work arrangement (Gruber and Madrian, 1994; Monheit and Cooper, 1994; Holtz-Eakin, Penrod, and Rosen, 1996; Buchmueller and Valletta, 1999; Anderson, 1997; Wellington, 2001; Heim and Lurie, 2010). In the context of Medicaid expansion, an increased availability of non-employer-sponsored health insurance may offer individuals the flexibility to pursue work arrangements which do not include health benefits (Lee, 2019). Job lock can then be viewed as a similar phenomenon as the low-barrier hypothesis proposed in chapter 1. Job lock is contrasted with employment lock, the tendency for workers to remain employed exclusively for access to, or to be able to afford, health insurance. In this case, we would not see a transition between a covered and uncovered sector as discussed in chapter 1 and the job lock effect of chapter 2, but instead an income effect. Individual workers who receive access to Medicaid may find that their ideal labor-leisure bundle,

with access to a means-tested program added to the calculation, involves less work than before the expansion (Garthwaite, Gross, and Notowidigdo, 2014; Dague, DeLeire, and Leininger, 2017).

In chapter 2 I find evidence of a negative effect of Medicaid expansion on reported earnings and participation in self-employment as measured by nonemployer establishments. States which expanded Medicaid had ~2.5% fewer nonemployer establishments, and a reduction in total declared receipts of 1.49%, in comparison to states which did not expand Medicaid. Using data from before Medicaid expanded, I estimate the effect to be a $10-billion reduction in declared earnings by the self-employed and just under 350,000 fewer declared nonemployer establishments among states which expanded Medicaid. These initial results appear to indicate an employment lock effect following Medicaid expansion, but using data on Uber, an informational reporting platform, I find evidence that the reduction in declared self-employment is significantly manipulated. While I am unable to estimate the exact extent to which evasion is occurring, I find evidence that without an informational reporting platform, transportation and warehousing services respond in a similar way to other industries. Once the work is done on an informational reporting platform, the negative effect of Medicaid expansion on participation in, and earnings from, nonemployer establishments disappears.

Chapter 3 further explores the dynamics of labor-leisure allocations, but with the added complication of a potential hours constraint introduced by employers in response to minimum wage increases. Using data from the Survey of Income and Program Participation, I test for changes in the rate of multiple jobholding and in hours allocations across jobs for respondents in states who experience minimum wage increases. Two of the most prominent motivations for holding multiple jobs are experiencing hours constraints and household income shortages. Minimum wage policies offer the potential to increase the returns to low-wage work by increasing the price floor of labor, but they also carry a risk of job loss (Neumark and Shirley, 2021), hours reductions, and longer search times (Caliendo et al., 2017; Jardim et al., 2017; Jardim et al., 2018), producing uncertain effects on total earnings. Together, the minimum wage offers potential for income and substitution effects, and it is theoretically ambiguous which will be dominant when an hours constraint is introduced.

Chapter 3 reports little effect between the minimum wage and multiple jobholding. While the rate of multiple jobholding is statistically insignificant, the increase in the uptake of new multiple jobholding is statistically significant. A $1 increase in the minimum wage is shown to increase the probability of taking up multiple jobs over a 12 month period post treatment by 0.6% among workers who make on average less than $19 per hour. I find no evidence of a statistically significant effect of the minimum wage level on hours worked or earnings across primary or secondary sources of income. Minimum wage increases do not appear to be an effective tool to alleviate a workers need or tendency to take up multiple jobs.

Chapter 1

The Minimum Wage, Self-Employment, and the Online Gig Economy

1.1 Abstract

This paper estimates the impact of the minimum wage on work exempted from minimum wages, including the self-employed, independent contractors, and workers in the online gig economy. Using two-way fixed effect models, recently updated difference-in-difference methods, and a generalized synthetic control method, I find that increases in the minimum wage have little impact on traditional self-employment, but do impact work done in the online gig economy. For a 10% increase in the minimum wage, the number of workers classified as transportation and warehousing services, and who are exempted from the minimum wage, increases by 3.9%. This positive relationship is driven by counties with low labor market concentration and an active online gig economy, as measured by the presence of Uber.

1.2 Introduction

While a robust literature exists on the effects of minimum wages on work, little distinction has been made between employees and the self-employed. The bulk of the literature has either focused solely on employees or treated participation in the labor market as a single criterion, under-exploring the transition between work that is covered and uncovered by the minimum wage. This inconsistency in the inclusion of uncovered workers in minimum wage analyses, those workers who are exempted from the minimum wage, is a rarely discussed design decision. I find that traditional self-employment is largely unaffected by the minimum wage in aggregate, but substantial effects are present among workers in the online gig economy. Increases in the minimum wage are shown to push individuals to take up uncovered work with low barriers to entry and exit among counties with low levels of labor market concentration. This is result appears to be urban workers seeking additional sources of income by driving for Uber to compensate for lost hours

in their covered jobs or longer search times for work. This result adds to the literature on the minimum wage and growing literature on the online gig economy. This paper also supports the inclusion of self-employment as an increasingly important element of the minimum wage literature as the uncovered labor market develops into a more prominent source of income.

Previous work has identified that nonstandard employment and work which is uncovered across a range of policies is impacted by the regulation of the standard labor market (Gramm and Schnell, 2001; Weil, 2014). Blau (1987) and Bruce and Mohsin (2006) have both looked at the effect of the minimum wage on self-employment, and estimated a negative relationship between higher minimum wages and participating in traditional self-employment. These papers did little to differentiate between types of self-employment; they were written before the expansion in the availability of low-barrier independent contracting opportunities offered by the online gig economy. Nonetheless, following their conclusions, changes to the minimum wage may impact both hiring and job seeking in the uncovered labor market. Given the increasing attention to protections for nonstandard work arrangements and the online gig economy (Berg, 2015; Harris and Krueger, 2015; Srnicek, 2017; Schor and Attwood-Charles, 2017), an evaluation of how the uncovered labor market interacts with our current labor policies, including the minimum wage, is necessary.

The aggregate effect of the minimum wage has seen a wide range of estimates, with a tendency toward small negative effects and null results (Neumark and Wascher, 2007; Belman and Wolfson, 2014a; Wolfson and Belman, 2019). Yet findings of negative employment effects continue to be challenged with findings of no significant employment losses in aggregate (Dube, Lester, and Reich, 2010; Lester, 2011; Caliendo et al., 2018a; Cengiz et al., 2019; Dube, 2019b). In parallel, work exploring how employment effects vary across competitive or noncompetitive labor markets find substantial variation in both the direction and size of effects across types of work and regions (Bhaskar, Manning, and To, 2002; Alan, 2011; Dube et al., 2018; Sokolova and Sorensen, 2021; Caldwell and Oehlsen, 2018; Pörtner and Hassairi, 2018; Azar et al., 2019). These results suggest that the effect of the minimum wage on an uncovered labor market will vary depending on the local labor market characteristics, and specifically the availability of other sources of work. While the literature on competitive and noncompetitive labor markets focuses on the geographic concentration of work, this paper asks instead about the overlapping policy environments of different classifications of work.

Using data on nonemployer establishments,[1] I test the degree to which the minimum wage impacts participation in the uncovered labor market, and the average receipts participants take in. Using data on covered employment from the publicly available County Business Patterns data, I construct an approximate Herfindahl-Hirschman Index (HHI) to measure county level labor

[1] Nonemployer establishments are businesses that do not have paid employees, primarily composed of the unincorporated self-employed and independent contractors. A more detailed definition can be found in the data section of this paper.

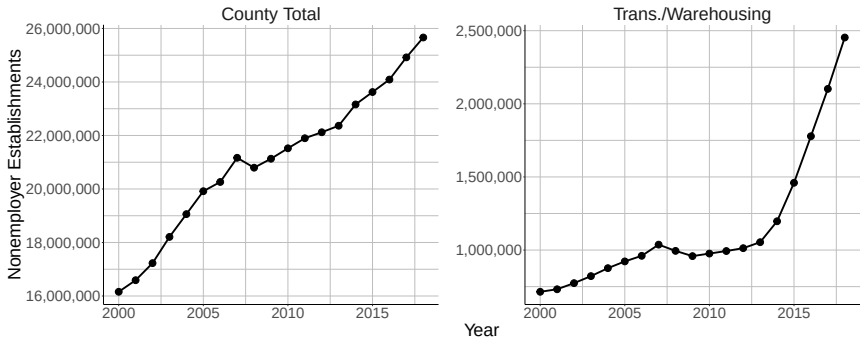
market concentration and test for differences in the effect of the minimum wage. I then test how the effect varies when a low-barrier uncovered labor market is active, indicated by Uber deployment at the county level.

As can be seen in Figure 1.1, while aggregate nonemployer establishments have been growing consistently since 2000, transportation and warehousing services have increased exponentially since 2013.[2] This rapid growth is attributed to the introduction of firms like Uber and Lyft Sundararajan (2016).

I test how the minimum wage relates to the uncovered market by using traditional two-way fixed effect models, the DIDM estimator introduced in De Chaisemartin and D'Haultfœuille (2020b) and De Chaisemartin and D'Haultfœuille (2020a), the group-time difference-in-difference method outlined by Callaway and Sant'Anna (2020), and a generalized synthetic control method.

Trends in Nonemployer Establishments

FIGURE 1.1: Trends in Nonemployer Establishments



These figures show the trend in Nonemployer establishments for the aggregated total of all nonemployer establishments on the left and for transportation and warehousing services (NAICS 48-49) on the right.

I find that for a 10% increase in the minimum wage, between 2010 and 2018, ~1 additional nonemployer establishment was declared for every 1000 members of the counties labor force. A 10% increase in the minimum wage was also shown to increase the average receipts of nonemployer establishments by ~0.5%. I find no significant effect on the count of nonemployer establishments across all NAICS industries. In aggregate, I find little evidence that the minimum wage has a substantial impact on traditional self-employment. however, when I restrict the analysis to transportation and warehousing services, a 10% increase in the minimum wage resulted in a ~3.9% increase in the count of nonemployer establishments, ~0.7 additional transportation and warehousing nonemployer establishments for every 1000 members of the labor force, and an insignificant effect on the average receipts of those nonemployer establishments. I find evidence that this positive relationship is a recent development; I attribute this to the expansion in low-barrier uncovered

[2]Figures A.1 and A.2 show the trend in nonemployer establishments per member of the labor force and the average receipts of nonemployer establishments.

work opportunities, including the online gig economy. I also find evidence of heterogeneous effects across labor market concentration, where counties with low levels of labor market concentration drive the positive effect, while highly concentrated labor markets exhibit no significant relationship between the minimum wage and the number of nonemployer establishments.

The recency of the positive effect of the minimum wage, its dependence on labor market characteristics, and the dependence on the online gig economy, support the conclusion that transition between work that is covered and uncovered by the minimum wage has become easier, and spillover across the two markets is increasingly likely. Policies which operate on the same, or similar, classification systems as the minimum wage are likely to see greater spillover between the uncovered and covered labor market. It is unreasonable to assume that if a policy is not intended to cover all forms of work that it will not still have impacts on the uncovered marketplace.

1.3 Literature Review

The increased attention to the minimum wage both within the U.S. at the federal, state, and local level and internationally,[3] points to the need for a comprehensive understanding of the effects of minimum wage policies. Work addressing this need includes summaries of the aggregate effects of the minimum wage (Brown, Gilroy, and Kohen, 1982; Wascher, 2007; Neumark and Wascher, 2007; Neumark, Wascher, Wascher, et al., 2008; Belman and Wolfson, 2014a; Card and Krueger, 2015; Wolfson and Belman, 2019) as well as explorations of alternative models of the minimum wage (Bhaskar, Manning, and To, 2002; Alan, 2011; Dube et al., 2018; Sokolova and Sorensen, 2021; Caldwell and Oehlsen, 2018; Pörtner and Hassairi, 2018; Azar et al., 2019) and the unintended effects of expansion of new types of work arrangements or industries (Derenoncourt and Montialoux, 2021).

Due to the nature of uncovered work, it is often excluded without concern for biasing results of analyses of the minimum wage,[4] but this ignores systematic differences between the covered and uncovered labor market. Minimum wage coverage serves as a strong predictor for access to employer supplied health insurance, disability insurance, paid sick leave, retirement benefits, unemployment insurance, and more (Harris and Krueger, 2015; Hyman, 2018), creating substantial differences between the experience of participating in the covered and the uncovered labor market.

[3] This can be seen in the recent vote by the U.S. House of Representatives for a $15 minimum wage and the introduction of the minimum wage in Germany in 2015.

[4] Common data sources for minimum wage analyses which include uncovered workers are the Current Population Survey, the American Community Survey, the Quarterly Census of Employment and Wages, Longitudinal Employer-Household Dynamics, and County Business Patterns. One of the primary sources of data on exclusively covered employees are state level unemployment insurance data sets.

While the study of the aggregate effect of the minimum wage has largely focused on shifts in employment on the extensive margin Belman and Wolfson (2014b), Belman and Wolfson (2014a), and Wolfson and Belman (2019), exploration of effects on the number of hours worked, take home pay, and heterogeneous effects by worker experience add insight into alternative treatment effects (Jardim et al., 2017; Jardim et al., 2018; Bossler and Gerner, 2016). A full loss of employment is not the only avenue by which the minimum wage may affect the uncovered labor market, as hours reductions or increased search times may be just as important as job loss in driving workers to enter the uncovered labor market.

A growing interest into the regulation of the uncovered labor market, and in particular independent contracting and the online gig economy,[5] has coincided with the "Fight for 15"'s impact on federal, state, and local minimum wages. Given the increasing attention to issues of fissuring[6] and the misclassification of workers as independent contractors (Weil, 2014), as well as to labor market protections for nonstandard work arrangements and participants in the online gig economy (Berg, 2015; Harris and Krueger, 2015; Srnicek, 2017; Schor and Attwood-Charles, 2017), assessment of the minimum wage that differentiates between the covered and uncovered market is overdue. Inconsistency in the inclusion or exclusion of uncovered workers in the analysis of minimum wage policies is little discussed, yet may present substantial hurdles to the assessment of aggregate effects of the minimum wage. Policy movements to reevaluate the minimum wage will benefit from a reevaluation of how the uncovered labor market interacts with current labor policies.

The uncovered labor market itself is a diverse set of work arrangements. One of the most talked about subsets of the uncovered labor market today is the "online gig economy," which is composed of numerous low-barrier marketplaces, including, but not restricted to, Uber, Lyft, Airbnb, TaskRabbit, Etsy, and Upwork. The online gig economy is a relatively small share of the total uncovered labor market, which includes all independent contractors, the self-employed, spot-market workers, and other nonstandard work arrangements (U.S. Department of Labor, 2016; Katz and Krueger, 2016; Current Population Survey Staff, 2018; Katz and Krueger, 2019). The relative size of the online gig economy can make changes difficult. The legal categorization of these work arrangements within the Fair Labor Standards Act (FLSA), that they are exempt from all aspects of the FLSA, do serve a purpose, though, in addressing the applicability and ease of policy enforcement across types of work (Harris and Krueger, 2015). Policy tools like the minimum wage are not designed for

[5]For example, California has passed AB 5 with the intent to reduce misclassification of workers. New York City introduced a minimum pay rate for drivers on Uber and Lyft. Seattle introduced legislation requiring premium payment for food delivery and transportation gig workers in relation to the hazards of operating amid the COVID-19 civil emergency. The passing of the CARES act included Pandemic Unemployment Assistance (PUA) which supported workers who otherwise would not have had access to unemployment benefits.

[6]Fissuring is defined as the process by which employers and firms externalize a greater share of organizational responsibilities. They do this through shifting responsibilities to subcontractors, third party companies, and misclassification (Weil, 2014).

the self-employed, workers who are operating simultaneously under multiple firms at a single point in time (e.g. an individual driving for both Uber and Lyft simultaneously), or for workers whose hours are prohibitively difficult to track.

Recently emerging hybrid organizational structures walk the line between uncovered and covered workers as well as traditional and alternative work arrangements (Simon, 1991; Malone, Yates, and Benjamin, 1994; Sundararajan, 2016). These hybrid organizations – most recently online gig firms, but grounded in a long-run trend beginning with sub-contracting firms in the post war period and accelerating more recently (Hyman, 2018) – have further reduced the barriers for transition into independent contracting and platform self-employment.

These platforms and labor markets are often easier to enter and exit than their traditional counterparts. Driving for Uber is often easier than becoming a driver for Yellow Cab in New York City, and Airbnb makes it easier to rent out a room than trying to build your own website and attract guests. The taxi and limousine industry in particular has outpaced other industries as a source of self-employment (Abraham et al., 2018a). As a result, it is possible that any effect on the traditional uncovered labor market may differ in size and sign when compared to this subset of the uncovered labor market. Workers in the online gig economy can differ in how they view their labor and how much labor they supply. The online gig economy also appears to be changing the demographics of who works and how, with increases in female participation and a reduction in the share of nonwhite workers in taxi drivers, and an increased likelihood of combining earnings from self-employment and wage and salary work (Abraham et al., 2018a).

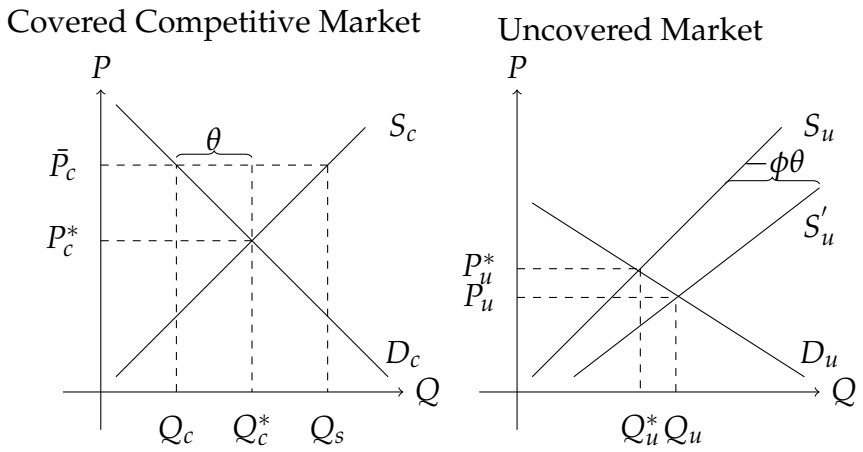
1.4 Theoretical Model

To help frame the effects of a minimum wage on the covered and uncovered labor market, I use the model of the minimum wage outlined by Welch (1974) and Gramlich, Flanagan, and Wachter (1976). This model was premised on the industry-level exclusion of some forms of work from the FLSA, and the expansion of the FLSA to include retail workers, public schools, nursing homes, laundries, and other industries. In this case, I apply the covered and uncovered model of the minimum wage to differences in work arrangements as opposed to the industry of work. This is following the assumption that the two markets are not economically independent.

In the case shown in Figure 1.2, the wage in the covered and uncovered markets before a binding minimum wage is introduced is P_c^* and P_u^* respectively.[7]

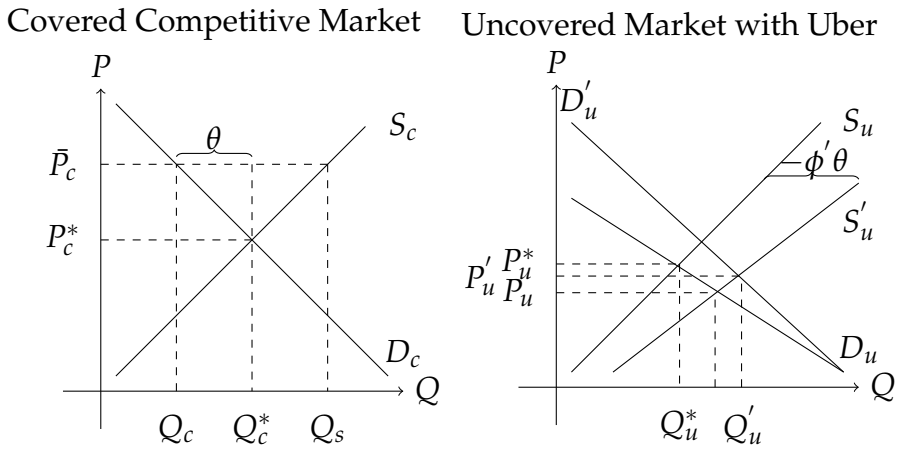
[7]Without any differences in job characteristics, $P_c^* = P_u^*$. As noted by Bracha and Burke (2016), independent contractors tend to earn higher wages than their covered counterparts. This higher wage could be a compensation for the lack of access to non-monetary benefits of the FLSA and other sources of policy support (Harris and Krueger, 2015; Hyman, 2018). For the purposes of this example, I make no assumptions about the relative size of P_c^* or P_u^*.

FIGURE 1.2: The Competitive Model of the Minimum Wage



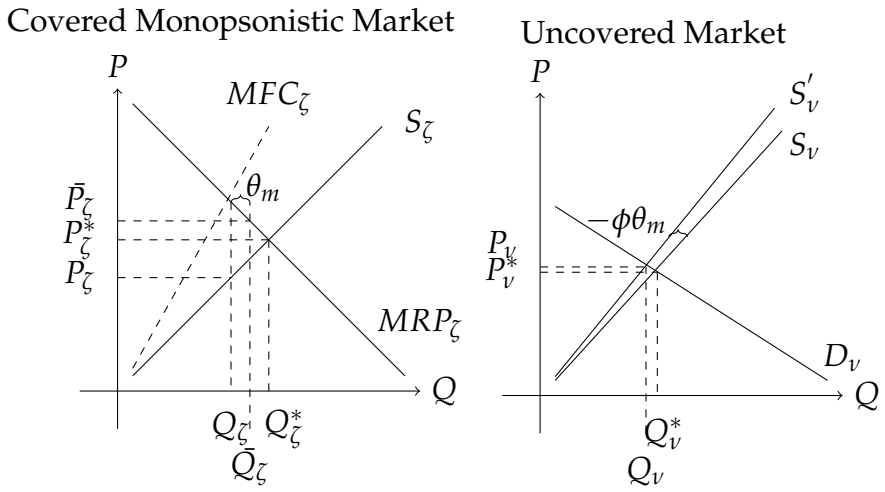
An illustration of the competitive model of the minimum wage in the covered, on the left, and uncovered, on the right, labor market.

FIGURE 1.3: The Competitive Model of the Minimum Wage with Uber



An illustration of the competitive model of the minimum wage in the covered, on the left, and uncovered, on the right, labor market after the introduction of the online gig economy.

FIGURE 1.4: The Monopsonistic Model of the Minimum Wage



An illustration of the monopsonistic model of the minimum wage in the covered, on the left, and uncovered, on the right, labor market.

If the minimum wage is set at level $\bar{P}_c$, such that $\bar{P}_c > P^*_c$, the quantity of labor purchased in the covered labor market falls from Q^*_c to Q_c. This is a change in the quantity of labor purchased of size $\theta = Q^*_c - Q_c$, and results in a difference between the supplied covered labor and purchased covered labor of $Q_s - Q_c$.

A share of the displaced labor, θ, would be willing and able to transition between the covered and uncovered sector, so long as the wage received is greater than their reservation wage, conditional on job characteristics. This share of labor, referred to in this model as ϕ, defines how much displaced labor is interested in, and capable of, transitioning between the two labor markets. The amount of labor that is capable of transitioning into the uncovered market as a result of the minimum wage is $-\phi\theta$, and this depends on both worker and job characteristics, and ϕ is assumed to be independent of the level of the minimum wage. As the uncovered wage is bid down, the movement into the uncovered market falls. This results in an uncovered wage $p_u < p^*$, and the uncovered quantity of labor $Q_u > Q^*_u$. The movement across markets of size $-\phi\theta$ is under the assumption that the total supply of labor across the covered and uncovered market is equal to the sum of Q^*_c and Q^*_u, and that the search for work both within the covered and uncovered sector, and across the sectors, is costless. It should also be noted that an individual worker's labor can be divisible across markets, as individuals may split hours of work across both.

The introduction of the online gig economy, through a firm like Uber, would yield an increased demand for uncovered labor. Firms participating in the online gig economy do not just increase demand for uncovered labor, they also reduce barriers to entry into the uncovered labor market Abraham et al. (2018a). Uber, Lyft, Airbnb, TaskRabbit, and others all have an incentive to make it easy for workers to participate in their marketplace. This would

appear in the model as an $\phi^{'} > \phi$. Figure 1.3 demonstrates how the increased demand for uncovered labor, and reduced barriers to transition into uncovered work, can further increase the difference between Q_u and Q_u^*, resulting in an even greater expansion in uncovered work such that $Q_u^{'} > Q_u > Q_u^*$. The effect on the price of uncovered labor becomes uncertain, as it may be (1) positive with a large enough increase in the demand for uncovered labor, (2) negative but with $P^* > P_u^{'} > P_u$, or (3) with a significant increase in ϕ, $S_u^{'}$ pivots down enough to reduce $P_u^{'} < P_u$. While each of these indicates an increase in the quantity of uncovered labor, the balance between the increased demand for uncovered work and the reduced barriers to transition into the uncovered market make $P_u^{'}$ uncertain.

Alternatively, a monopsonistic model of the minimum wage, as shown in Figure 1.4, highlights the possibility for an increase in the quantity of labor in the covered market. In the monopsonistic model, Q_ζ is the quantity of covered labor purchased by the monopsonist at P_ζ.[8] If a minimum wage were implemented at a level between P_ζ and the intercept of MFC_ζ and MRP_ζ, then Q_ζ would increase, in the case of Figure 1.4, to $\bar{Q}_\zeta$. This increased return to covered work, and the firms capacity to purchase it, pulls labor from the uncovered market. This reduces Q_ν^* to Q_ν. If the minimum wage is binding and greater than $MFC_\zeta = MRP_\zeta$, then a similar result to Figure 1.2 would hold.

1.5 Data

I use Nonemployer Statistics (NES) data from 2000 to 2018, provided by the Census Bureau, to estimate the size and composition of the uncovered labor market. The NES collects annual data on nonemployer establishments and reports the count of establishments by geographic level and industry. Most nonemployer establishments are self-employed individuals running small unincorporated businesses, which includes independent contractors.[9] This analysis uses the aggregate of all NAICS industries, as well as industry-level data at the two-digit NAICS code level for transportation and warehousing services. [10]

[8] Q_ζ is determined by the intercept of the Marginal Factor Cost (MFC) and the Marginal Revenue Product (MRP), with P_ζ following from $S_\zeta(Q_\zeta)$.

[9] Each establishment is defined as a business that has no paid employees, has annual business receipts of 1,000 dollars or more (1 dollar or more in the construction industry), and is subject to federal income taxes. This income restriction means that this analysis omits any shift in Uber drivers or other workers earning less than 1,000 dollars.

[10] I create a balanced panel of counties throughout the sample. When using the aggregated data for the total count of nonemployer establishments in each county, the panel is balanced for each county. Industry subsets may include fewer counties, as subsets are censored due to confidentiality concerns when few nonemployer establishments are active, resulting in missing years. Counties which have no nonemployer establishments in a given industry code are not included in the data, and can therefore be assumed to have zero in a given county-industry-year. Those counties that have less than 3 establishments, but are non-zero in a given year are censored for confidentiality concerns. While it would be possible to estimate

Since the NES is a count of establishments at a geographic level, I am unable to directly measure an individual's intensity of engagement in this type of work, but the publicly available NES does include data on the total receipts taken in by establishments. Using the count of establishments and the total receipts taken in, a measure of the average receipts per establishment can be made.[11] The NES methodology for reporting the receipts of nonemployer establishments was revised in 2009, with new cutoffs intended to filter out likely employers and better depict the nonemployer business universe. As a result, average receipts data is not directly comparable before and after 2009, and this analysis splits the sample before and after this change.[12]

Previous work on nonstandard work arrangements has leveraged the Current Population Survey's Contingent Worker Supplement (CWS) due to its identification of contingent workers, independent contractors, on-call workers, temporary help agency workers, and workers provided by a contract firm. Here I focus on the NES data as it is less restrictive in the set of workers it captures and allows for county-level geographic information.[13] If the primary use of the online gig economy is to act as an income smoothing mechanism, or to cover transitory periods and adverse economic shocks, then it is likely that the NES will reflect effects which are unobservable in surveys which only capture primary sources of income, such as the CWS. Data which only capture primary sources of income are likely to under estimate nonstandard work given findings that the online gig economy is linked to part-time income smoothing mechanisms (Brainard, 2016; Farrell and Greig, 2016; Hall and Krueger, 2018; Katz and Krueger, 2019; Abraham et al., 2018a).

I use publicly available County Business Patterns (CBP) data from 2000 to 2018 to construct a measure of the covered labor market concentration in each county. The CBP includes counts of the number of establishments in the covered labor market and the number of employees working at these establishments. While the publicly available data does not allow for a linking of employees directly to covered establishments, it does identify employment counts in firm size categories. I use this to construct a Herfindahl-Hirschman Index (HHI).[14] The HHI is a measure of labor market concentration, and can

the number of establishments in censored counties, no estimate can be made for the total receipts taken in. As a result, these censored units are dropped from the analysis as well as any structural zeros. The results of this analysis are not sensitive to these decisions. As a result, a balanced panel of counties used in the analysis will vary in the number of counties by industry specification.

[11] This analysis uses publicly available NES data, but recent work has also been done using individual level data linked with the same individuals' wage and salary earnings (Abraham et al., 2018a). It would be worthwhile replicating this analysis with the linked data in future work.

[12] This change in reporting can be seen in Figure A.2.

[13] The restrictive nature of the CWS, specifically the focus on primary sources of income, is one explanation for why the percent of workers in alternative work arrangements and the uncovered marketplace has seemed to remain stable since 1995, which is in contrast to administrative data sources like NES (Abraham et al., 2018b; Katz and Krueger, 2019).

[14] The Herfindahl-Hirschman Index is calculated by squaring the market share of each firm competing in the market and then summing the resulting numbers. The HHI will approach

offer insights into the distribution of labor market power across the US (Rinz et al., 2018). This allows for the testing of how the effect of the minimum wage may vary across more or less concentrated counties.

Due to the censored nature of the CBP data, the reported covered establishment counts are broken into groups based on number of employees.[15] I assume that each establishment in the employee group has the minimum number of employees, and use this to create a measure of labor market concentration in each county for each year.[16] By assuming that each firm employs the minimum number of employees in their group, I estimate the number of employees for each of these firms and can calculate the HHI across each group. I then sum each group HHI to get the county HHI. With the full range of HHI calculated, I then compute 100 quantiles and use the HHI quantile as a measure of labor market concentration.[17].

One of the difficulties of identifying a relationship between the minimum wage and the uncovered labor market within NES data is the annual nature of the NES, and the non-uniform nature of minimum wage policy implementation, with deployment times varying throughout the year. I code the minimum wage of a county as the highest minimum wage active on December 31st in a county each year. This is inclusive of federal, state, and local minimum wages.[18] Minimum wage data at the federal, state, and local level is compiled from Vaghul and Zipperer (2016) and UC Berkeley Labor Center (2018).

The geographic and time varying rollout of Uber makes it possible to construct a treatment for a homogeneous, or nearly homogeneous, market for uncovered

zero with lower levels of labor market concentration, and has a maximum of 10,000. The U.S. Department of Justice uses the HHI, and specifically changes in the HHI, as a flag for problematic firms and mergers.

[15]The CBP breaks the establishments up into groups of firms that have 1 to 4 employees, 5 to 9, 10 to 19, 20 to 49, 50 to 99, 100 to 249, 250 to 499, 500 to 999, 1,000 to 1,499, 1,500 to 2,499, 2,500 to 4,999, and firms greater than 5,000 employees.

[16]I have repeated the analysis using both the minimum, midpoint, and maximum of the range for each group and found the results were consistent. The midpoint and maximum values for the largest establishment category were set at 10,000 and 15,000 employees respectively . Due to the lack of data for the exact midpoint and maximum values for the largest establishment category, the minimum method is favored in this analysis. One risk of the lower-bound method is that it underestimates the amount of labor among a few large firms. This calculation would result in an underestimate of the labor market concentration, and an overestimate of the market competition among low-concentration zones. Since the use of the midpoint and maximum method did not significantly impact my results, I am unconcerned by this issue. Beginning in 2017, the process of censorship extends to any cell with fewer than three establishments. I extend any value from 2016 to both 2017 and 2018. Results are consistent in a sample excluding 2017 and 2018.

[17]Results are robust to the use of raw HHI, as well as the logged HHI, but quantiles are my preferred measure to account for the right tailed nature of the data. This can be seen in Figure A.6

[18]The results of this analysis were robust to a coding of the minimum wage on January 1st and the average across months. The variation in minimum wage policy implementation may produce underestimates of the effect of the minimum wage. If the minimum wage increases in December, then the covered market may not respond completely before the new year begins.

labor, which varies in deployment timing and location. This market has relatively low-barriers to entry and exit and is composed of a labor force which is more similar to the general population than previous taxi industries or the self-employed more broadly (Hall and Krueger, 2018; Abraham et al., 2018a). Uber deployed across the United States in a series of waves starting in 2011 in San Francisco. It then spread nationally and internationally over the following years. Figure A.5 shows this deployment strategy in action at the county level within the U.S. This initial expansion in locations was not random, as Uber sought to operate in markets which would produce high initial uptake of their service, but over time the deployment strategy grew less dependent on local market characteristics. As Uber's head of global expansion said in 2014, "At this point we go so quickly, I wouldn't say that it particularly matters" in response to questions about how Uber selects locations of operation. He went on to say, "If we're not there now, we'll be there in a week" (Huet, 2014). Summary statistics across deployment zones at the county level can be found in Table A.1. While the deployment clearly favored large urban centers as the first deployment locations, these areas had little difference in the prevalence of nonemployer establishments as a share of the labor force.

For the purposes of identifying the effect of Uber, the date of operation of Uber in a given county is used to create an indicator for a homogeneous uncovered labor market.[19] By linking Uber deployment locations to FIPS state-county codes the presence or absence of Uber's marketplace is established for a given year. This coding for the treatment of Uber is expanded to include the core-based statistical areas (CBSAs) in which a county is a member.[20] Linking Uber deployment to the CBSA level rather than exclusively to the county level will not only identify the effect of Uber deploying in a given county, but also capture the effect among counties that are economically linked. This reduces bias as a result of individuals commuting to areas where Uber is operational, as nonemployers will be recognized in counties where they file their taxes and not strictly where driving occurs. This is consistent with the coding of Uber treatment in Abraham et al. (2018a).[21]

Annual county labor force estimates are included using the Bureau of Labor Statistics Local Area Unemployment Statistics. Annual county population estimates are also included, using the Annual Estimates of the Resident Population data from the Census Bureau. These population estimates are further used to create a measure of the population density within a county, coded as the number of people per square kilometer. Summary statistics on the uncovered market, HHI, real minimum wage, and labor force can be found in Table 1.1.

[19] This deployment data was supplied by Uber upon request.

[20] CBSAs are defined by the Census Bureau as a geographic area which "consist of the county or counties or equivalent entities associated with at least one core (urbanized area or urban cluster) of at least 10,000 population, plus adjacent counties having a high degree of social and economic integration with the core as measured through commuting ties with the counties associated with the core" (US Census Bureau, 2010).

[21] I also perform an analysis at the combined statistical area level rather than county level, to account for this exact issue. These results are shown in Table A.5.

TABLE 1.1: Descriptive Statistics of All Nonemployer Establishments, 2000-2018

Statistic	Mean	St. Dev.	Min	Median	Max
Nonemp. Estab. (NE)	7,025	25,976	23	1,710	1,107,080
NE/Labor Force	0.140	0.037	0.008	0.134	0.560
Receipts ($1000s)§	359,441	1,460,626	229	71,292	58,688,161
Average Receipts§	43,711	9,144	7,890	42,533	174,030
Minimum Wage§	6.65	1.24	5.15	7.25	15.69
HHI	812.8	259.1	199.8	750.4	7,307.7
Labor Force	49,980	158,808	232	12,648	5,095,504

§Inflation adjusted to 2016 dollars.

1.6 Methodology

The primary empirical challenge in this analysis is to create a reasonable counterfactual of how many nonemployer establishments would operate in a county-year in the absence of some change to the minimum wage, and the receipts taken in by these establishments. By exploiting state and county level variation in the minimum wage from 2000 to 2018, I estimate the effect of the minimum wage on the number of nonemployer establishments, the number of nonemployer establishments per member of the labor market, and the average receipts taken in by nonemployer establishments. I also test how these effects differ by local labor market concentration, intended to proxy the labor market "competitiveness" of different counties, and the deployment timing and location of Uber.

The extensive marginal effect of the minimum wage on the uncovered labor market can be directly measured using the number of nonemployer establishments active in a county in a given year, defined as E_{cit}, in county c, industry i, and year t. In the case of the aggregate of all nonemployer establishments, the industry subscript can be dropped, and the analysis is performed at the county level. In addition to measuring the direct effect on the count of nonemployer establishments, I estimate the effect of the minimum wage on the number of nonemployer establishments in a given industry per member of the labor force in a county defined as:

Industry Level	**All Nonemployer Establishments**
$e_{cit} = \frac{E_{cit}}{L_{ct}}$	$e_{ct} = \frac{E_{ct}}{L_{ct}}$

L_{ct} is county c's total labor force estimate in year t. This creates a proxy measure for the likelihood that an individual will engage in a particular uncovered industry in a given county-year, given that they are in the labor

force.[22] This measure offers an alternative perspective to the raw count of nonemployer establishments as it estimates a measure of the prevalence of work in the uncovered labor market relative to jobholders and job seekers at the given geographic level. Unfortunately, given the level of the data, no clear comparison can be made between a single nonemployer establishment and a covered job held by a labor market participant.[23]

Using the receipts data included in the NES, I calculate the average receipts of the nonemployer establishments in the county within a given industry. Treating R_{cit} as the total receipts taken in by nonemployer establishments in county c, industry i, and year t, the average receipts are defined as:

Industry Level	**All Nonemployer Establishments**
$r_{cit} = \frac{R_{cit}}{E_{cit}}$	$r_{ct} = \frac{R_{ct}}{E_{ct}}$

Figure 1.5 describes both the log of the number of nonemployer establishments and the log of the average receipts of nonemployer establishments relationship with the real minimum wage, adjusted to 2016 dollars,[24] split between counties which have Uber active and those which do not. A positive relationship can be seen between the number of nonemployer establishments and the real minimum wage when looking at the county total. This positive association holds for counties with and without Uber. The effect on the log of average receipt appears to be somewhat muted. In counties without Uber active, the relationship appears slightly negative, and in counties with Uber active it is slightly positive, but neither seems substantial. The linear relationships depict a situation where higher minimum wages are associated with a higher count of nonemployer establishments and no substantial impact on the average receipts of nonemployer establishments in aggregate.

The right side of Figure 1.5 describes the same relationships, but for the subset of transportation and warehousing services. This industry is chosen as it represents a set of uncovered workers likely to be engaging in a low-barrier marketplace for uncovered work after the expansion of Uber and Lyft. For counties with and without Uber active, the count of nonemployer establishments is positively associated with the minimum wage, but the relationship appears greater among Uber active counties. The impact on the average receipts of transportation and warehousing services indicates a

[22] It is possible for an individual tax filer to be responsible for multiple nonemployer establishments if a self-employed individual operates multiple distinct businesses.

[23] This measure may also capture an impact on aggregate covered employment and labor force participation. If minimum wages result in significant reductions in the labor force, and no change in nonemployer establishments, then the minimum wage would appear to increase the prevalence of uncovered work through a reduction in covered employment.

[24] This adjustment is done using the CPI inflation calculator which uses the Consumer Price Index for All Urban Consumers (CPI-U) U.S. city average series for all items, not seasonally adjusted. The adjustment compares the value of a dollar in each December of that year without reference to a given county's characteristics.

FIGURE 1.5: Bivariate Relationship between the Minimum Wage and Nonemployer Establishments

These figures show both the number of nonemployer establishments (top) and average receipts of nonemployer establishments (bottom) relationship with the real minimum wage. Figures on the left are for aggregate of all industries, and figures on the right are for transportation and warehousing services. Green points are county-year observations where Uber is active, and black points are county-year observations where Uber is not active. The fitted green line is the linear relationship among counties with Uber active, and the red line is the linear relationship among counties where it is not.

negative relationship, and the negative relationship is greater among counties where Uber is active. However, Figure 1.5 may not be free of bias, as the observed positive relationship is vulnerable both to the timing and location of minimum wage increases as well as Uber expansion. Figure 1.5 does not control for the timing of minimum wage increases, timing of Uber deployment, or local county characteristics.[25]

This analysis begins to address these biases with a two-way fixed effect model, at the county-year level, on a balanced panel of counties to identify if changes in the minimum wage have an impact on the uncovered labor market. A total of 2,996 counties are used across 16 years, from 2000 to 2006 and 2010 to 2018. The primary advantage of the two-way fixed effect model is the ability to include interaction effects between the HHI quantile, Uber being active, and the minimum wage. The most basic specification is described in equation (1.1). I repeat this equation for all of my dependent variables, but use Y_{ct} to represent all six.[26]

$$Y_{ct} = \beta_0 + \beta_1 Log(M_{ct}) + \beta_2 \mathrm{U}_{ct} + \beta_3 \mathrm{HHI}_{ct} + \beta_4 \text{Pop-Density}_{ct} + \alpha_c + \tau_t + \epsilon_{ct} \tag{1.1}$$

Equation (1.1) is estimated using OLS with clustered standard errors at the county level, and regressions are weighted by the counties average population across the sample.[27] Similar to previous work on the minimum wage, these models control for time invariant geographic characteristics, and year fixed effects. M_{ct} identifies the real minimum wage at the county level in 2016 dollars.[28] U_{ct} identifies if Uber is active at any time in county c in year t. HHI_{ct} is county c's HHI quantile in year t, where a lower quantile means a less concentrated labor market, and is then thought to be a more competitive county. I also include a control for the counties population density, Pop-Density_{ct}, to account for differences in the structure of the uncovered labor market across more or less dense areas.

The time-invariant geographic characteristics, α_c, are preferred at the county

[25]The relationship shown in Figure 1.5 is excluding outlier levels of the minimum wage resulting from large local changes. The largest real minimum wage in the data are in King County, WA in 2014 and Alameda, CA in 2016. The figures are robust to the exclusion of all real minimum wage values greater than $12.00. Figure A.3 includes these outlier values.

[26]The six dependent variables of this analysis are $\log(E_{ct})$, $\log(E_{cit})$, e_{ct}, e_{cit}, $\log(r_{ct})$, and $\log(r_{cit})$, divided between the aggregate of all nonemployer establishments and transportation and warehousing services.

[27]Weighting by the county population or observed labor force as opposed to county average across the sample skews the estimate toward a recency bias due to demographic trends. An alternative specification without population weights is presented in Table A.6. The results presented in this paper are also robust to clustering at the state level instead of the county level.

[28]The two-way fixed effect estimates were also consistent with the nominal minimum wage.

level given the inclusion of local minimum wage changes, local Uber treatment, and local labor force estimates.[29] These time-invariant factors may influence the nature of uncovered labor markets which form in a county and their long-run behavior, and may have led Uber to select some counties over others in the timing of deployment. Year fixed effects, τ_t, control for shocks which occurred nationally. When utilizing τ_t the analysis is limited to testing the effect of state and local minimum wage changes.

Equation (1.1) presents the average effect of state and local minimum wage changes on the chosen dependent variable, conditional on county fixed effects, year fixed effects, the listed controls, and weighted by the county's population. Following this initial model I introduce interaction effects with U_{ct}, HHI_{ct}, and Pop-Density_{ct} to estimate the effect of local labor market concentration and the availability of the online gig economy on the relationship between the minimum wage and the uncovered labor market.

1.6.1 Alternatives to the Two-Way Fixed Effect Models

Previous work demonstrates that two-way fixed effect models may misestimate the counterfactual employment levels in minimum wage analyses specifically (Allegretto et al., 2017; Cengiz et al., 2019; Callaway and Sant'Anna, 2020), and a growing literature demonstrates the weaknesses of two-way fixed effect models, including the use of negative weights, failure to validate parallel trends, and a nonconformity with event study designs (Borusyak and Jaravel, 2017; Sun and Abraham, 2020a; Goodman-Bacon, 2018; Imai, Kim, and Wang, 2018; Athey and Imbens, 2018; Callaway and Sant'Anna, 2020; De Chaisemartin and D'Haultfœuille, 2020b; De Chaisemartin and D'Haultfœuille, 2020a). As such, I test three alternative designs to estimate the average treatment effect of the minimum wage on the uncovered labor market.

The first alternative specification used comes from De Chaisemartin and D'Haultfœuille (2020b), who demonstrate that two-way fixed effect models can produce negative weights, resulting in biased average treatment effect estimates. This method estimates the treatment effect on a given outcome using group level panel data with non-binary treatments, and will be referred to as the DIDM estimate. The DIDM estimate requires stable treatment values, and estimates dynamic effects relative to the first period where treatment changes. In order to make a comparable design using the DIDM framework, I redefine the treatment as the log of the nominal minimum wage as opposed to the log of the real minimum wage, which would induce a "change in treatment" simply by adjusting for inflation.[30] This is because in a staggered

[29] Additional analysis is performed at the Combined Statistical Area (CSA) level. These are included in Tables A.5 and A.6.

[30] When generating the effect estimate, a stable treatment status is required as well as a broadening of the treatment groups. To do this, I round each nominal minimum wage to the nearest 15 cents. I also specify the re-categorized treatment bins to collect values which may not have many comparable units. This code can be found in the replication files.

adoption design a steady control group needs to be identifiable in each period when a treatment, a minimum wage increase, occurs.[31]

When applying the DIDM estimate to this problem, I split the sample between observations from 2000 to 2006 and 2010 to 2018. I do this for three primary reasons. First, this excludes increases in the federal minimum wage in 2007, 2008, and 2009, which would remove any "never treated units" from the sample. Second, this acts as a divider between measures of the average receipts of nonemployer establishments as a result of the new maximum and minimum receipt cutoffs introduced in 2009 by the Census Bureau. Third, splitting the data allows for a reexamining of the treatment effect of the minimum wage on units that may have been treated very early. The state of the labor market for nonemployer establishments may be systematically different in 2018 in comparison to 2000, and the DIDM method would be unable to detect that without changing the sample time frame. For consistency, I also split the two-way fixed effect estimates up across the same two sub samples.

In addition to the DIDM, I utilize the methodology outlined by Callaway and Sant'Anna (2020). This method does not support non-binary treatments, and will be referred to as the Callaway and Sant'Anna Method (CSDID) in tables of results. This method will not differentiate between varying sizes in the change to the minimum wage, and so I favor the DIDM estimate for aggregate effect estimates. The CSDID also differs in its treatment of control variables and conditional parallel trends. When including controls within the DIDM method, the "first-difference of the outcome is replaced by residuals from regressions of the first-difference of the outcome on the first-differences of the controls and time fixed effects"[32] In contrast to this, the CSDID incorporates time invariant covariates to construct conditional parallel trends. It is not required that covariates are included, but this does offer an alternative method of addressing the parallel trends assumption between the two.

The final design intended as a robustness check is a generalized synthetic control method (GSCM). This addition is following the work of Dube and Zipperer (2015) and Powell (2017) in the application of synthetic control designs for the analysis of minimum wage policies across many treated units with varying treatment size, Gobillon and Magnac (2016) which outlines a method for the application of synthetic control designs for regional policy evaluation, and the GSCM outlined by Bai (2009) and Xu (2017). I apply both the GSCM and the CSDID to the a binary treatment defined as the adoption of a local minimum wage increase at the county or metropolitan level.[33]

[31] To avoid a conflation between the transition from the real minimum wage to the unadjusted minimum wage and a change in methodology, I also tested the used the unadjusted minimum wage in equation (1.1) and found no significant difference from the inflation adjusted results.

[32] This quote comes directly from the help file for the did_multiplegt function.

[33] This GSCM matches in the pre-treatment period on the minimum wage, population density, HHI quantile, and county population. The CSDID performs conditional parallel trends using a constant and the year that Uber becomes active in a county.

Table A.2 identifies which counties adopted local minimum wage changes in this data, when they adopted the local minimum wage, and what combined statistical area the county is in.

1.7 Results

Using equation (1.1) as well as the difference-in-difference estimator from De Chaisemartin and D'Haultfœuille (2020b), I estimate the effect of the minimum wage on the two split samples of the data, from 2000 to 2006 and 2010 to 2018. The first section of Table 1.2 identifies the effect of the minimum wage on all nonemployer establishments, inclusive of all NAICS industry codes. I favor the effect estimates from the DIDM and support the parallel trends assumption by including placebo and dynamic treatment estimates, which can be seen in Figures A.11 and A.12. By including these dynamic treatment effects, the values shown for the DIDM methodology in Tables 1.2, 1.3, A.5, and A.6 can be interpreted as the cumulative effect of the minimum wage up to five years after treatment occurs. I do not find evidence of a violation of parallel trends, but I do find evidence in support of lingering treatment effects.

I find no significant relationship between an increase in the nominal minimum wage and any of the three dependent variables in the 2000 to 2006 sample period. This is in contrast to the 2010 to 2018 sample, where a 10% increase in the minimum wage results in an additional 0.928 nonemployer establishment per 1000 members of the labor force, and 0.477% increase in the average receipts taken in by nonemployer establishments. Both of these effects are significant at the 95% confidence level and are conditional on the counties HHI quantile, population density, and total population. The effect on the logged count of nonemployer establishments was positive, but insignificant.

The two-way fixed effect results from equation (1.1) are included, but the two methods disagree on the size of the coefficients and precision of the estimates. While none of the dependent variables were significant using the DIDM for the 2000 to 2006 sample, the two-way fixed effect method finds that the minimum wage is negatively related to the average receipts of nonemployer establishments. This is in contrast to the statistically insignificant results found using DIDM. For the 2010 to 2018 sample, I find similar results across both methods, but the two-way fixed effect model appears to under estimate the effect, and it is not as precisely estimated.

The second section of Table 1.2 is on a subset of nonemployer establishments, transportation and warehousing services. This subset is intended to illustrate the effect of the minimum wage on an industry which has been heavily influenced by the online gig economy in the 2010 to 2018 sample. Similar to the aggregate of all nonemployer establishments, I find that an increase in the minimum wage has no significant impact on the uncovered labor market from 2000 to 2006. From 2010 to 2018, a 10% increase in the minimum wage increases the count of nonemployer establishments classified as transportation

TABLE 1.2: Treatment Effect Estimates

All Industries		*Dependent variable:*		
Method	Treatment	Log(Estab.)	Estab./Labor Force	Log(Avg. Receipts)
Two-Way FE	Real	-0.0459	-0.0012	-0.0901***
2000 to 2006	Log(Min. Wage)	(0.0319)	(0.0039)	(0.0211)
Two-Way FE	Real	0.0133	0.00679*	0.0265
2010 to 2018	Log(Min. Wage)	(0.0243)	(0.00399)	(0.0172)
DIDM†	Nominal	-1.8148	-0.08895	-2.9884
2000 to 2006	Log(Min. Wage)	(3.9575)	(1.10968)	(5.8444)
DIDM†	Nominal	0.0154	0.00928**	0.0477**
2010 to 2018	Log(Min. Wage)	(0.0362)	(0.00373)	(0.0192)

Transport. and Ware.		*Dependent variable:*		
Method	Treatment	Log(Estab.)	Estab./Labor Force	Log(Avg. Receipts)
Two-Way FE	Real	-0.0005	0.00018	-0.0660***
2000 to 2006	Log(Min. Wage)	(0.0608)	(0.00045)	(0.0219)
Two-Way FE	Real	0.7947***	0.01331***	-0.3020***
2010 to 2018	Log(Min. Wage)	(0.1305)	(0.00256)	(0.0786)
DIDM†	Nominal	-0.7830	0.00550	-9.7920
2000 to 2006	Log(Min. Wage)	(6.3276)	(0.09529)	(47.3025)
DIDM†	Nominal	0.3896**	0.00719**	-0.0872
2010 to 2018	Log(Min. Wage)	(0.1931)	(0.00293)	(0.0917)

All Except Transport. and Ware.		*Dependent variable:*		
Method	Treatment	Log(Estab.)	Estab./Labor Force	Log(Avg. Receipts)
DIDM†	Nominal	-1.9679	-.09452	-2.7322
2000 to 2006	Log(Min. Wage)	(8.3639)	(1.11978)	(7.8621)
DIDM†	Nominal	-0.0191	0.00210	0.0600***
2010 to 2018	Log(Min. Wage)	(0.0260)	(0.00253)	(0.0199)

*p<0.1; **p<0.05; ***p<0.01

This table uses the data at the county level.
†The De Chaisemartin and D'Haultfœuille (2020b) method identifies treatment switchers as those whose nominal minimum wage increases in comparison to the previous period. The increase is treated as an increase in the level of a continuous treatment, and accounts for the difference in size of the first increase. It does not capture subsequent increases in the switching status. To account for the lack in subsequent increases, both the pre-2007 sample and the post-2009 sample include dynamic treatment effects for the maximum duration, when also accounting for pre-treatment periods, following De Chaisemartin and D'Haultfœuille (2020a), and the value presented is the cumulative effect for the specified post-treatment periods. The DIDM estimate is run with 1000 bootstraps and is performed conditional on the counties HHI, population density, and total population. This exact coding can be found in the replication code.

and warehousing services by 3.896%. A 10% increase in the minimum wage also results in 0.719 additional nonemployer establishments per 1000 members of the labor force. The effects on the count of nonemployer establishments and the number of nonemployer establishments per member of the labor force are both significant at the 95% confidence level, but I find no significant impact on the average receipts of nonemployer establishments classified as transportation and warehousing services.

The comparison of the number of nonemployer establishments per member of the labor force is additive, unlike the log of nonemployer establishments, and the 0.719 additional transportation and warehousing nonemployer establishments per 1000 members of the labor force are included in the 0.928 additional nonemployer establishments in aggregate per 1000 members of the labor force. I rerun the analysis on the aggregate of all nonemployer establishments except transportation and warehousing services, and include the results in the third section of Table 1.2. When transportation and warehousing services are excluded, the positive effect on the number of nonemployer establishments per member of the labor market is reduced by ~0.00719 and is now insignificant. The positive effect on the average receipts of nonemployer establishments remains, however increasing to 0.6% for a 10% increase in the minimum wage and is now significant at the 99% confidence level.

These results highlight a discrepancy between transportation and warehousing services and nonemployer establishments in general. The positive relationship between the minimum wage and the number of nonemployer establishments is centered on transportation and warehousing services, and only in the 2010 to 2018 sample. The parameter θ is not dependent on any given industry and should be viewed as a market level effect. This leaves ϕ as the primary explanation for the difference between the observed effect among transportation and warehousing services and the aggregate of all other industries, but the implied surplus in covered labor should be equivalent. The successful transition into the uncovered market is likely in large part due to the increased ease of entry into this type of work, but this does not mean that θ does not exist among markets where we do not see substantial transition. Rather these results seem to indicate that minimum wage increases are linked to a displacement of covered labor, and some of that labor is able to transition into these new types of work with low barriers to entry and exit, although in general, displaced labor has been unable or unwilling to enter into the traditional uncovered market.

These results also seem to indicate that when a minimum wage increase occurs, the primary relationship between the covered and aggregate uncovered market is a slight increase in the average receipts of nonemployer establishments. This could reflect an increase in the demand for uncovered labor following the minimum wage increase with a contraction in the supply of uncovered labor, resulting in a higher price and no substantial change in the number of workers. This may best be described as the prices in the uncovered market following the covered market. However, this trend is overcome by the expansion in low-barrier uncovered work. As minimum wages increase,

an increasing number of workers participate in the uncovered labor market via the online gig economy. What remains to be seen is if the increase in engagement in uncovered online gig work is consistent across labor market concentration, and if the expansion in Uber really is the primary factor for the positive effect among transportation and warehousing services.

1.7.1 The Impact of Labor Market Concentration and the Online Gig Economy

One of the limitations of De Chaisemartin and D'Haultfœuille (2020b)'s estimator is an inability to test for interaction effects on the primary treatment. As a result, I explore the relationship between the real minimum wage and *i*) county labor market concentration and *ii*) the online gig economy, using the two-way fixed effect model. Tables A.3 and A.4 show the extension of equation (1.1) after including interaction effects between the minimum wage, HHI quantile, Uber being active, and population density for the 2010 to 2018 sample. These tables use a Poisson pseudo-maximum likelihood regressions with two-way fixed effects (Correia, Guimarães, and Zylkin, 2020) in addition to two-way fixed effect regressions using OLS. As shown in Table 1.2 and discussed in the methods section, the two-way fixed effect model is a less effective estimator of the size and significance of the relationship between the minimum wage and the uncovered labor market, but it does capture the direction of the effects consistently. The results shown in Tables A.3 and A.4 are used to inform the relationship between the real minimum wage and both the labor market concentration of counties and the online gig economy, but the exact size of the effects are not used to inform the average treatment effect of each relationship.

The marginal effect of the minimum wage on the logged number of nonemployer establishments and average receipts are presented in Figure 1.6. This figure demonstrates that the positive relationship between the minimum wage and the number of nonemployer establishments classified as transportation and warehousing services is driven by the least concentrated counties, in which Uber is active. These counties are also where the majority of the labor force is located as of 2018. Among transportation and warehousing services, the significant positive effect of the minimum wage on the count of nonemployer establishments occurs in the counties where Uber is active and where the HHI quantile is ~50 or less. This is also the set of counties which experience a significant negative relationship between the minimum wage and the average receipts of nonemployer establishments, though that effect is statistically significant until the ~25^{th} quantile. The least concentrated counties, with a low barrier uncovered market, exhibit the predictions from Figures 1.3, with a significant transition into the uncovered market through a low barrier marketplace.

Across quantiles which are statistically significant, with Uber active, a 10% increase in the minimum wage would result in 9.04% more nonemployer establishments classified as transportation and warehousing services, and a

3.25% reduction in average receipts. Counties where Uber is not active show no significant difference at the 95% confidence level in the log of nonemployer establishments across labor market concentration, but they do exhibit a significant positive effect for counties with an HHI quantile less than 10. For a 10% increase in the minimum wage, the average receipts increase by ~3.15%, in direct contrast to the effect observed in counties with Uber active. The increase in receipts without a significant increase in the quantity of workers in non-Uber counties, with low labor market concentration, falls outside of the theoretical results in Figures 1.2, 1.3, and 1.4. This could indicate that the demand for uncovered transportation and warehousing services has increased for reasons other than Uber, without a significant increase quantity of nonemployer establishments, similar to the results found across all nonemployer establishments.

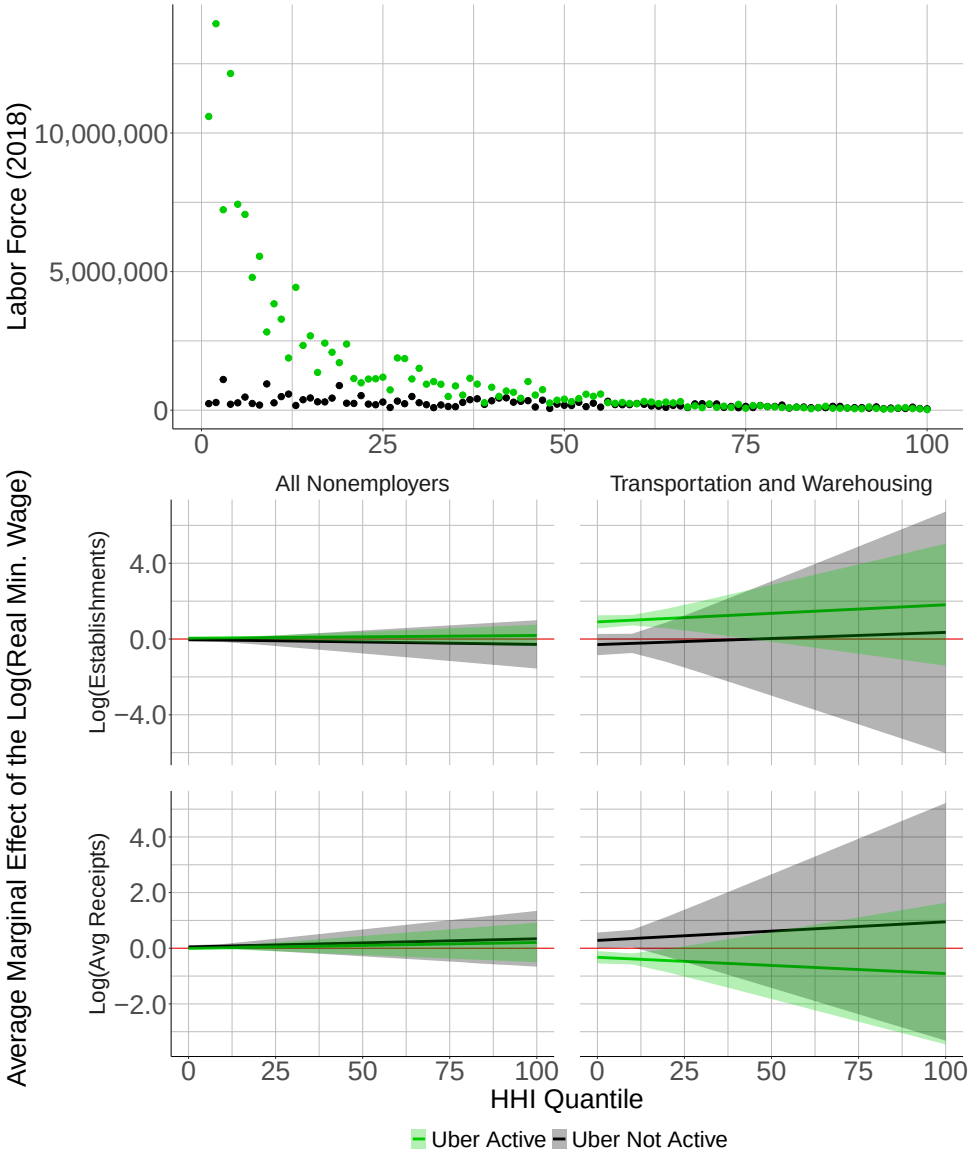
While transportation and warehousing services appear significantly responsive to treatment, the aggregate of all nonemployer establishments appear less so. The deployment of a platform taxi service like Uber is expected to have a smaller impact on construction than it would on transportation, but the clear increase in effect within these industries is telling. The presence of a low-barrier marketplace increases the observed effect substantially. While small, a 10% increase in the minimum wage is expected to result in ~0.5% increase in the number of nonemployer establishments, but only in counties with an HHI quantile of 10 or less and where Uber is active. The effect on the average receipts of nonemployer establishments is only briefly statistically significant at the 95% level for counties with an HHI quantile of 10 where Uber is not active, showing an increase in average receipts of 0.6% for a 10% increase in the minimum wage.

In total, these findings show an increase in the number of workers engaging in uncovered work in more concentrated counties where the online gig economy is active and able to take advantage of large consumer networks in low concentration urban settings. This effect represents a fraction of the broader uncovered market, and aggregate effects are small, but present. The increase in the supply of labor in the uncovered market does significantly impact the average receipts taken by nonemployer establishments classified as transportation and warehousing services, but heterogeneous effects were shown among more or less concentrated labor markets. The lack of a low barrier marketplace also indicates a "return to normal" as transportation and warehousing services exhibit a similar relationship to nonemployers in general. This supports the conclusion that Uber, and other forms of platform work, are able to effectively take up the slack from excess labor supply in the covered labor market resulting from minimum wage increases, and that some slack does exist even if it is not able to break into traditional self-employment.

1.7.2 Local Minimum Wage Increases

Using the DIDM, CSDID, and GSCM, and defining the adoption of local minimum wage increases at the county or metropolitan level as the treatment,

FIGURE 1.6: Marginal Effect of the Minimum Wage by Uber Activity and Labor Market Concentration



These figures illustrate the marginal effect of the log(real minimum wage) on the log(nonemployer establishments) and the log(average receipts) for both transportation and warehousing services and all nonemployer establishments. These results come from Tables A.3 and A.4 and highlight how the effect of the minimum wage varies by labor market concentration. Lower HHI quantiles are less concentrated labor markets.

I find similar results to those already identified among low concentration counties.[34] Neither the CSDID or GSCM methods account for differences in the size of local minimum wage changes, and none of the three account for the process of increasing local minimum wages in the following years. Within the GSCM, matching on the pre-treatment trends of counties which adopt local minimum wage increases offers a more comprehensive accounting of the parallel trends assumption than the two-way fixed effect model alone. Within the CSDID, the doubly-robust difference-in-difference estimate resolves a number of the fundamental issues of variation in treatment timing and treatment effect heterogeneity that arise with two-way fixed effect methods. The CSDID also accounts for the parallel trends assumption after conditioning on covariates. These results are intended as a robustness check on the aggregate effect estimates already discussed, due to concerns regarding the generalizability of local minimum wage changes to state and federal changes.

Table 1.3 includes the effect estimates from the DIDM, CSDID, and GSCM for the introduction of a local minimum wage into a county. The three methods return consistent signs for the relationship between the minimum wage and each of the three dependent variables. While the precision of each estimate does vary across methods, the general conclusions remain the same as those derived from Table 1.2. Increases in the minimum wage have a significant positive relationship to engagement in transportation and warehousing services, and a negative relationship to the average receipts taken in by these nonemployer establishments. It is also worth noting that the effect estimates using the GSCM are substantially larger than for either the DIDM or CSDID.

As shown in Table A.2, the majority of local minimum wage increases occur in low concentration counties and predominantly metropolitan or micropolitan areas. The results on local minimum wage increases then are most similar to the expected results among low concentration counties as shown in Figure 1.6. While the effect estimates are similar, the two-way fixed effect estimates make no explicit test of the parallel trends assumption, unlike the DIDM, CSDID, and GSCM. Figures A.14 and A.13 demonstrate the event study for both transportation and warehousing services and all nonemployer establishments, and show no consistent significant violation of parallel trends, but some evidence of leading terms or spurious significant variation from zero.

Figure A.16 shows the average effect of the treatment on the treated (ATT) for transportation and warehousing services and all nonemployer establishments. It also allows for an assessment of the parallel trends assumption, with no significant differences between the treatment and control units in the pre-period. A significant increase in the number of transportation and warehousing services follows the introduction of a local minimum wage change. I also find a significant reduction in the average receipts at the 95% level. When estimating the effect for all nonemployer establishments, I find similar results, but to a smaller degree, again showing that the aggregate uncovered labor market

[34] The average change in the real minimum wage in the first year of implementation of a local minimum wage in my sample is $1.60. Local minimum wage increases at the county level are shown in Table A.2.

TABLE 1.3: Treatment Effects: Local Minimum Wages

NAICS: All Industries		*Dependent variable:*		
Method	Treatment	Log(Estab.)	Estab./Labor Force	Log(Avg. Receipts)
DIDM †	Min. Wage (Local)	0.0155**	0.00247	-0.0074
		(0.0063)	(0.00157)	(0.0052)
CSDID ‡	Min. Wage (Local)	0.01607	0.00029	0.00045
		(0.01032)	(0.00181)	(0.00754)
GSCM §	Min. Wage (Local)	0.1709***	0.0197***	-0.0969***
		(0.0187)	(0.0035)	(0.013)
NAICS: Transport. and Ware.		*Dependent variable:*		
Method	Treatment	Log(Estab.)	Estab./Labor Force	Log(Avg. Receipts)
DIDM †	Min. Wage (Local)	0.2834***	0.0058***	-0.1291***
		(0.0638)	(0.0011)	(0.0367)
CSDID ‡	Min. Wage (Local)	0.2801***	0.00424***	-0.0954*
		(0.0688)	(0.00120)	(0.0578)
GSCM §	Min. Wage (Local)	0.8989***	0.00890***	-0.3586***
		(0.0813)	(0.00100)	(0.0793)

*p<0.1; **p<0.05; ***p<0.01

This table uses the data at the county level. In this table, treatment switchers are those counties which introduce a local minimum wage that is greater than the state and federal minimum wage at the time of introduction. All three methods presented are run with 1000 bootstraps, but include slightly different control and matching structures.

†DIDM does not capture subsequent increases in the switching status. To account for the omission of subsequent increases, I include dynamic treatment effects for the five years following the introduction of a local minimum wage while also accounting for five pre-treatment years. These dynamic effects follow the method outlined by De Chaisemartin and D'Haultfœuille (2020a), and the value presented is the cumulative effect for the specified post-treatment periods. The DIDM method is also controlling for county HHI, population density, and total population. Effect estimates are weighted by the county's average population.

‡CSDID presents the average treatment effect of an aggregated event study across all local minimum wages, as shown in Figures A.13 and A.14. This method conditions on the average county HHI quantile, county population density in the first year of the sample, and the year that Uber first becomes active in the county. Effect estimates are weighted by the counties average population.

§The GSCM used here presents the average difference between the treated units and each of their synthetic controls after matching on the county HHI quantile, population density, and minimum wage in the pre-period. This process also uses cross-validation to select the optimal number of factors, nonparametric inference, and is weighted by the county's average population.

is less responsive than transportation and warehousing services examined alone.

1.8 Conclusion

The minimum wage remains an important component of the policies governing low-wage labor in the U.S. Though it is intended as a tool for addressing a minimum standard of living, conclusions on the aggregate effects of the minimum wage remain elusive. The presence of uncovered work – work that falls outside the scope of federal, state, and local legislation – adds to the uncertainty. The division between the uncovered and covered labor market is in part driven by structural differences in work, but also the rules we set and the programs we design.

This analysis intended to address the degree to which changes in the minimum wage impact the propensity of workers to engage in the uncovered labor market. I find that (1) in aggregate, increase in the minimum wage have a limited impact on traditional uncovered work arrangements; (2) low-barrier marketplaces and the development of the online gig economy appear to have created a niche of uncovered labor which is significantly responsive to the minimum wage; and (3) the concentration of local labor markets is an important component in determining where the minimum wage is likely to displace labor from the covered to uncovered sector.

I find a positive relationship between the minimum wage and engagement in uncovered work among counties with low levels of labor market concentration and a prevalence of low-barrier uncovered work arrangements, paired with negative effects on the average receipts of establishments in less concentrated counties. Among transportation and warehousing services, I find that for a 10% increase in the minimum wage, the number of nonemployer establishments classified as transportation and warehousing services increases by 3.9% in the period from 2010 to 2018. This positive effect disappears when I consider the stock of all nonemployer establishments, as transportation and warehousing services, and the online gig economy in general, make up a small share of total stock of uncovered labor. Where minimum wages do displace labor from the covered market into the uncovered market, the average receipts of nonemployer establishments fall. I do not find significant effects of the minimum wage on the uncovered market among highly concentrated counties, where monopsonistic or oligopsonistic competition is most likely.

With the identification of significant effects on uncovered labor, and differences in this effect across geography, questions are raised regarding the aggregate effect of minimum wage policies on the labor market. For those studies which utilize data solely on covered work, any negative relationship between minimum wages and the quantity of labor may be overestimated, as the transition of workers between the uncovered and covered market may be classified as exit from the labor market. These studies are also unlikely to capture movement on the intensive margin of the uncovered labor market, masking

what may be important shifts in hours' allocation between the uncovered and covered labor market. Studies which rely on data sources that capture both the uncovered and covered labor market, but fail to distinguish between the two, may underestimate negative consequences of transition. Workers leaving the covered market to take up uncovered work may be losing access to a substantial number of policy protections and fringe benefits. Without properly accounting for this shift, the assessment of aggregate welfare effects will be positively biased. Both of these are of particular concern in concentrated metropolitan regions with access to the online gig economy.

Through organizational restructuring and technological change, portions of the uncovered labor market are growing more similar to traditional work arrangements, and similar types of work exist on either side of divisions recognized or created by policy. The online gig economy has challenged the boundaries of older legislation, leaving policymakers struggling to manage these new work arrangements. The continued development of work arrangements which walk the line of labor protections and regulations creates opportunities for research on the effects of labor policy, but it also impacts the lived experiences of individuals within our communities. This analysis hopes to add to the evidence from which policy makers can draw when creating and improving legislation. With the growing interest in addressing minimum wages within the online gig economy, specifically policy changes mandating minimum wages for drivers on platforms in New York City and Seattle, understanding these dynamics is crucial.

While this analysis is unable to address the effect on aggregate welfare, the assessment of welfare effects is a next step in this research. This paper also highlights the necessity of assessing how other policies which operate on a similar division in the labor market may interact with the uncovered market in general and the online gig economy, and how this effect varies across space in the U.S. These policies include the Affordable Care Act, disability insurance, retirement benefits, paid sick leave, unemployment insurance, tax withholding, and more recently the CARES act.

Chapter 2

Medicaid Expansion's Effect on Self-Employment Reporting

2.1 Abstract

This paper tests whether the expansion of Medicaid following the Affordable Care Act impacted reported self-employment. Using administrative tax data and national surveys, I find evidence of a negative effect of Medicaid expansion on unincorporated self-employment and nonemployer establishments. I estimate that states which expanded Medicaid see a reduction in the number of nonemployer establishments of ~2.5%, and a reduction in total declared receipts of 1.49%. Using data from 2013, before Medicaid expanded, this was equivalent to a $10-billion reduction in declared earnings by the self-employed and just under 350,000 fewer declared nonemployer establishments among states which expanded Medicaid. Using data on Uber, an informational reporting platform, I find evidence that the reduction in declared self-employment is significantly manipulated in reference to the means-tested Medicaid expansion.

2.2 Introduction

The Patient Protection and Affordable Care Act (ACA) passed by Congress in 2010 introduced incentives for the expansion of Medicaid. Over this same period, the prevalence of alternative work arrangements (AWAs) and nonstandard work broadly are thought to have increased as a supplemental source of income (Current Population Survey Staff, 2018; Abraham et al., 2018b; Katz and Krueger, 2019). This paper tests if the expansion of Medicaid impacted reported self-employment through the American Community Survey, the Current Population Survey, and through administrative tax filings of nonemployer establishments.[1]

[1] The majority of nonemployer establishments are self-employed individuals running small unincorporated businesses, which includes independent contractors. Each establishment is defined as a business that has no paid employees, has annual business receipts of 1,000 dollars or more (1 dollar or more in the construction industry), and is subject to federal income taxes.

I find significant negative effects of Medicaid on the number of declared self-employed and the reported receipts of these firms in administrative tax filing data. The expansion in Medicaid is related to a reduction in reported nonemployer establishments and the total declared receipts among expansion states. This significant negative effect is consistent across NAICS industry classification except for transportation and warehousing services. I find evidence in support of the conclusion that where Uber is active, the reduction in self-employment switches sign, and Medicaid expansion results in an increase in declared nonemployer establishments. This result adds to the literature on the interaction between health insurance and the labor market, means-tested program tax evasion, and the growing literature on the online gig economy.

Previous tests of the effect of Medicaid on self-employment have found no significant effect of Medicaid on self-employment, or small increases in the self-employment as a result of reductions in employment lock (Gooptu et al., 2016; Heim and Yang, 2017; Lee, 2019; Lee and Winters, 2020). These previous results rely on survey data, and it has been shown that measurements of the self-employed differ substantially between survey and administrative data sources (Abraham et al., 2018b). One of the hypothesized effects of the ACA was a reduction in job lock and an increase in the prevalence of work without employer-sponsored health insurance (ESI) (Blumberg, Corlette, and Lucia, 2014). Job lock is defined here as the tendency for workers to feel they cannot leave a job due to the loss in benefits incurred by leaving. These benefits can range from paternal leave to retirement programs, and in the case of the ACA, ESI. Alternatively, employment lock, the tendency for workers to remain employed exclusively for access to, or to afford, health insurance may have led to a reduction in engagement in the labor market once more individuals had access to Medicaid.

Beyond the literature on labor market outcomes, Medicaid expansion also has the potential to induce tax evasion responses due to the means-tested nature of the program. This would lead individuals to evade on taxes through extensive or intensive margins (Andreoni, Erard, and Feinstein, 1998; Chetty et al., 2012; Chetty, Friedman, and Saez, 2013) and could substantially impact the comparability of survey and administrative data sources.

Using data on nonemployer establishments and the unincorporated self-employed, I test the degree to which Medicaid expansion impacts measures of self-employment across survey and administrative data sources. I also assess the degree to which Medicaid expansion interacts with the online gig economy in comparison with traditional independent contracting and self-employment. Firms like Uber and Airbnb offer lower friction marketplaces and attract substantially different types of workers than their traditional counterparts (Abraham et al., 2018b). Firms like Uber and Airbnb also act as informational reporters, which may influence a given worker's willingness to evade on their taxes. To test for differences between the self-employed, independent

The count of nonemployer establishments, and the receipts taken in by them, are published in the Nonemployer Statistics (NES) data.

contracting broadly, and the online gig economy, data on where and when Uber was in operation in the U.S. is employed to identify workers who may be engaged in the online gig economy.

Following the passage of the expansion of Medicaid, a new group of individuals gained access to publicly provided health benefits. Simultaneously, the nature of work within the US was changing and the prevalence of nonstandard work increased as a supplemental source of income (Current Population Survey Staff, 2018; Abraham et al., 2018b; Katz and Krueger, 2019). I find evidence of a negative effect of Medicaid expansion on unincorporated self-employment and nonemployer establishments, but also evidence of tax evasion among the traditional self-employed. The results of this analysis contributes to the literature on Medicaid's labor market impacts (Gooptu et al., 2016; Heim and Yang, 2017; Lee, 2019; Lee and Winters, 2020), means-tested programs (Andreoni, Erard, and Feinstein, 1998; Saez, 2010; Chetty et al., 2012; Chetty, Friedman, and Saez, 2013), and the growing interaction between the online gig economy and public policy (Harris and Krueger, 2015; Hyman, 2018).

2.3 Literature Review

The ACA intended to improve access to affordable health insurance through the use of state insurance exchanges, expanded dependent coverage on health plans, and subsidies for the purchase of health insurance on exchanges (David, Melinda, and Rachel, 2015). In addition, the federal government funded the expansion of Medicaid to all individuals below 138 percent of the federal poverty line. After the supreme court ruled against the mandated expansion of Medicaid in National Federation of Independent Business (NFIB) v. Sebelius, expansion became optional. The states which expanded coverage can be seen in Figure 2.1.

The expansion of Medicaid meant that all individuals in states that expanded with incomes below 138 percent of the federal poverty line became eligible. This expansion in coverage had the greatest impact on non-elderly low-income adults without children younger than 18 (Leung and Mas, 2016). In 2016, 138% of the poverty line translated to $22,108, and the median earnings among the unincorporated self-employed as a primary job was $30,510 (Christnacht, Smith, and Chenevert, 2018). To qualify for Medicaid, an individual needed to have a modified adjusted gross income (MAGI) below the annual limit as reported to their states Medicaid agency. Importantly, this is not directly tied to an individual's annual tax filing, but the perceived likelihood of an audit may increase if individual's reported one income to their states Medicaid office and another to the IRS. It is unclear how this impacted the discriminant index formula used by the IRS.

Due to Medicaid expansions' disproportionate impact by demographic characteristics (i.e., non-elderly low-income adults without children younger than 18), and the disproportionate distribution of self-employment across similar

characteristics (Kogut, Luse, and Short, 2016), Medicaid expansion may have affected self-employment to a greater degree than the labor market more broadly. By December 2017, Medicaid enrollment had increased by 14,098,890 people among expansion states (Centers for Medicare & Medicaid Services, 2017). Estimates of the number of self-employed show that 7% of workers earn income solely from self-employment and an additional 6% earn income through a mixture of employment and self-employment (Jackson, Looney, and Ramnath, 2017b). These estimates would imply that roughly 1.8 million of the additional Medicaid participants would be engaged in self-employment in some capacity, with no change in labor market behavior.[2]

In the US, ESI has been the dominant form of health insurance since the early 20th century (Currie and Madrian, 1999), in part due to ESI's exemption from income taxes. Compensation packages which bundle ESI and monetary compensation can expand an individual worker's budget constraints in comparison to a fully taxable monetary package. This allows firms to create more attractive offers for workers at a lower cost, increasing labor recruitment and retention (Woodbury and Huang, 1991; Gruber and Poterba, 1994; Gentry and Peress, 1994). The expansion in Medicaid is an expansion in the availability of non-ESI, which may have impacted individuals on the margin in their decision of where to supply their labor.

The availability of non-ESI predated the expansion in Medicaid though. In fact, the private market has acted as a mechanism for catching those who may not have access to ESI for a wide range of reasons, and this has resulted in selection bias. Those workers who receive insurance through employers are less likely to purchase private insurance, and their dependents with access via shared family plans are also not pressured into the private market. Since health and productivity are positively correlated, the non-employed, and those without access to ESI, are likely to have a higher average cost of health insurance (Johnson and Lambrinos, 1985; Baldwin and Johnson, 1994; Baldwin and Johnson, 2000; Jones, Latreille, and Sloane, 2006; Jones, 2008). This relationship raises the cost of insurance in the private market on average, and this difference is increased by differences in bargaining power and pooling of risk across employees in firms (Service, 1988).

The self-employed have traditionally been unable to access ESI unless they are linked to a shared family plan, or if they are also an employee for a firm which offers ESI and qualify. This means many self-employed individuals actively purchased insurance in the private market, or received their health insurance through a family plan or government program, including Medicaid. For individuals choosing where to allocate their labor, the higher cost of health insurance on the private market could be a deterrent from entering self-employment or leaving work arrangements which offer ESI. How great of

[2]This estimate is an upper bound given the results of Jackson, Looney, and Ramnath (2017b) as it assumes that every additional enrollment is by a member of the labor market. While almost certainly untrue, given that the expansion in Medicaid primarily impacted non-elderly low-income adults without children younger than 18, it is possible as a conservative measure of the relative impact of Medicaid expansion.

a deterrent this is depends on an individual's preferences for health insurance and the differentials in the price and quality of insurance between markets.

Empirical tests of the effect of ESI and Medicaid on labor force participation have been difficult given the endogenous nature of employment matching markets. A number of scholars have used spousal insurance coverage to try and identify job lock effects (Gruber and Madrian, 1994; Monheit and Cooper, 1994; Holtz-Eakin, Penrod, and Rosen, 1996; Buchmueller and Valletta, 1999; Anderson, 1997; Wellington, 2001; Heim and Lurie, 2010), the tendency for workers to feel they cannot leave a job due to the loss in benefits incurred by leaving, in this case ESI. Effect estimates from this literature appear to be sensitive to the data source and method used, but broadly identifies evidence that the job lock effects exists. Workers do have a tendency to value employer supplied benefits, including healthcare, and the price differential between the insurance marketplaces as well as the tax exempt status may prevent movements of labor.

Similarly, previous work has gone toward the study of age-based coverage effects, but found mixed results (DeCicca, 2007; Akosa Antwi, Moriya, and Simon, 2013; Heim, Lurie, and Simon, 2015; Depew, 2015; Bailey, 2017; Heim, Lurie, and Simon, 2018). Both spousal and age-based coverage studies often do little to differentiate between the types of work which do not offer ESI. Instead it is treated as a binary based on coverage. While the self-employed have not been the focus of this research, these findings could generalize assuming that the self-employed are not significantly different in their valuation of health insurance then the labor market broadly. This may be an unreasonable assumption though, as the self-employed have been found to have a higher risk tolerance (Chell, Harworth, and Brearley, 1991; Cramer et al., 2002; Caliendo, Fossen, and Kritikos, 2009; Caliendo, Fossen, and Kritikos, 2014), which may result in them systematically having a lower valuation of health insurance. This would suggest that those most interested in self-employment would not be held back by uncertainty in their source of insurance, and job lock effects may be muted among the self-employed.

An alternative to the job lock hypothesis is employment lock. Employment lock is the tendency for workers to remain employed exclusively for access to, or to afford, health insurance. While job lock is focused on the transition between work arrangements, employment lock is focused on the withdraw of individuals from the labor market. Individuals who are attached to a source of employment exclusively for access to or to afford health insurance are likely not in need of that employment as a primary source of income. Health insurance is certainly not the only expense of a household. In households with multiple earners, one may stay attached to a source of ESI with better coverage than may be attainable in the private market, but leave to take up household labor or leisure if an alternative is available. Workers with multiple sources of income may also reduce hours to, or leave, a work arrangement if they no longer need to pay for health insurance.

There have been mixed results when studying the effects of Medicaid expansion on the labor market. Quasi-experimental research on the effects of Medicaid on labor supply have identified significant negative effects, implying an employment lock effect (Garthwaite, Gross, and Notowidigdo, 2014; Dague, DeLeire, and Leininger, 2017). Garthwaite, Gross, and Notowidigdo (2014) use the disenrollment of Tennessee residents in 2005 to estimate the extensive marginal effect in labor supply. They find an increase in employment among individuals working at least 20 hours a week and who receive ESI. Dague, DeLeire, and Leininger (2017) study "childless adults" in the context of an enrollment cap in Wisconsin by comparing enrollees to those on the waitlist. They find that enrollment in public insurance led to a ~5% reduction in employment. These results differ from the conclusions of Baicker et al. (2014), who use a group of uninsured low-income adults in Oregon that were selected by lottery for the chance to apply for Medicaid. This experiment found no significant effects of being enrolled in Medicaid on labor market outcomes.

Similarly, Leung and Mas (2016) used data from the American Community Survey (ACS) and Current Population Survey (CPS) to test how the state-based expansion of Medicaid impacted employment. While they observed an increase in coverage of 3.0%, they found no significant effect on employment among "childless adults." Duggan, Goda, and Jackson (2017) also use the expansion in Medicaid to test for impacts on labor market outcomes. Duggan, Goda, and Jackson (2017) compares preexisting population shares of uninsured individuals, by income groups, interacted with each state's Medicaid expansion status. They also found no significant effect of Medicaid expansion on labor market outcomes in aggregate, but this was due to the offset of labor force participation reductions among high potential exchange enrollment areas and increases in labor force participation among high potential Medicaid enrollment areas.

The literature on the effects of Medicaid expansion on labor force participation includes a subset with a focus on self-employment. This subset in the literature is the most relevant to this analysis. While self-employment is a broad classification of work, multiple efforts have been made to address how Medicaid may impact a large group of self-insurers. Similar to the studies outlined above, the focus has been on the identification of job lock and employment lock effects. Gooptu et al. (2016), Heim and Yang (2017), and Lee and Winters (2020) all find that Medicaid expansion has no significant effect on the number of self-employed or the probability of self-employment. They find little evidence of either a job lock or employment lock effect. Lee (2019) find a positive relationship between Medicaid expansion and engagement in self-employment.

Unfortunately, all four of these studies rely on survey data, specifically the ACS and CPS. Both the ACS and CPS focus on primary sources of income, and fail to capture supplemental sources of income. As has already been mentioned, survey and administrative data sources differ in their estimates of self-employment (Abraham et al., 2018b). While surveys and administrative

data sources differ in methodology, they also have the potential to capture differing effects of Medicaid expansion.

A means-tested public health insurance program may produce a similar response among the self-employed as other means-tested programs. Specifically, the tendency for the self-employment to manipulation reported earnings to maximize their benefits from a program (Andreoni, Erard, and Feinstein, 1998; Saez, 2010; Chetty et al., 2012; Chetty, Friedman, and Saez, 2013). Chetty, Friedman, and Saez (2013) show the ways in which self-employment earnings are reported in a way to maximize the Earned Income Tax Credit (EITC) refund. This is supported by the work of Saez (2010) which identified that self-employed tax files tend to report income at the kink in the EITC schedule which maximizes tax refunds.

The manipulation of reported earnings from self-employment is not unexpected given that wage earnings are double reported by employers and employees to the IRS, but self-employment has no secondary report. We can imagine then that self-employed workers may have a tendency to not only experience job lock or employment lock, but may also participate in tax evasion to gain access to Medicaid. Effects of tax evasion appear larger on the extensive margin in general, which would imply that any effect of Medicaid expansion on self-employment would likely be best seen on the number of workers declaring self-employment earnings, as opposed to the dollar amount declared (Piketty and Saez, 2012). This would imply that the number of self-employed would likely be lower among states where Medicaid expands, but not due to a real response.

This effect would not be seen, or be significantly smaller, among the online gig economy. This is because platforms act as information reporters for the IRS, increasing the risk to evasion. Information reporting is the practice of having employers report wages and salaries of their employees to the IRS, and the IRS compares these reports to individual tax filings. While many platforms in the online gig economy take a strong stance against calling their drivers and hosts employees, many still reports worker earnings to the IRS. The act of information reporting has been shown to be a significant deterrent to evasion, contributing to the 99% reported net income as a percentage of true net income among wage and salary earners. This is substantially greater than the 43% of true net income reported among nonfarm proprietors (Slemrod and Bakija, 2017).[3] If information reporting among the online gig economy is as effective at reducing evasion, then the traditional self-employed would have access to a response which gig workers might not.

2.4 Data

Following the previous work on the effect of Medicaid expansion on the labor market I use both the Current Population Survey (CPS) and the American

[3]Nonfarm proprietor income accounted for 35% of the overall tax gap in 2001.

Community Survey (ACS).[4] Both the ACS and CPS are nationally representative surveys with information on demographics, health, and labor. I use the 1% sample of U.S. households from the ACS from 2001 to 2017, and the CPS Annual Social and Economic Supplement (ASEC) sample from 2001-2017. I also include Nonemployer Statistics (NES) which collect annual data on nonemployer establishments and report the count of establishments by geographic level and industry.[5]

Both Abraham et al. (2018b) and Katz and Krueger (2019) discuss the advantages and disadvantages of using survey and administrative data sources when studying the self-employed, and specifically independent contractors. One of the difference between the ACS/CPS and NES is their assessment of primary and secondary sources of income. The ACS and CPS supplements focus on primary sources of income and will only identify respondents as self-employed if their main source of income is their business. The NES captures changes in supplemental sources of income, as it is a count of reported earnings regardless of if the earnings are from a primary source. The publicly available NES does not link nonemployer establishments at the individual level though.

Unfortunately, the data on nonemployer establishments does not include a differentiation between types of self-employment, unlike the ACS and CPS, but the majority of the NES is composed of unincorporated self-employed (Mishel and Wolfe, 2018).[6] To create a comparable sample among the ACS and CPS, I identify the unincorporated self-employed using the worker classification tag. This analysis does not subset either survey sample by demographic information, such as age, gender, or family composition, because no equivalent subset can be made out of the NES. I use NES data from 2001 to 2018 and create a balanced panel of states and counties throughout the sample.[7]

[4]These works include Leung and Mas (2016), Gooptu et al. (2016), Heim and Yang (2017), Lee (2019), and Lee and Winters (2020).

[5]The NES is composed of self-employed individuals running small unincorporated businesses. Each establishment is defined as a business that has no paid employees, has annual business receipts of 1,000 dollars or more (1 dollar or more in the construction industry), and is subject to federal income taxes.

[6]The NES is the count of nonemployer establishments, but it is important to consider that any individual filer may be responsible for multiple nonemployer establishments. This means that if a single person files income across multiple separate sources of self-employed income, they will appear in the data multiple times at the aggregate level. The NES may also include the incorporated self-employed, but this is a substantially smaller share of the data.

[7]When using the aggregated data for the total count of nonemployer establishments in each state or county, the panel is balanced for each county. Industry subsets may include fewer counties as they are censored due to confidentiality concerns when few nonemployer establishments are active. Counties which have no nonemployer establishments in a given industry code are not included in the data, and can therefore be assumed to have zero in a given county-industry-year. Those counties that have fewer than 3 establishments, but are non-zero, in a given year are censored for confidentiality concerns. While it would be possible to estimate the number of establishments in censored counties, no estimate can be made for the total receipts taken in. As a result, these censored units are dropped from the analysis as well as any structural zeros. The results of this analysis are not sensitive to these decisions.

As noted by Piketty and Saez (2012), effects of tax evasion appear larger on the extensive margin as opposed to the intensive margin. Following that result, the number of nonemployer establishments per member of the labor force are more likely to show a significant response to the expansion in Medicaid. One of the benefits of the NES is the inclusion of information on the total receipts taken in by all nonemployers at a given geographic level, which allows for the construction of a quasi-measure of the intensive margin. This data on the receipts of nonemployer establishments has two primary limitations. The receipts are not published at the individual nonemployer establishment level, preventing a direct measure of the intensive margin, and in 2009 the methodology for reporting the receipts of nonemployer establishments was revised. New cutoffs intended to filter out likely employers were implemented to better depict the nonemployer business universe. As a result, average receipts data is not directly comparable before and after 2009. This analysis does not include data before 2009 when estimating the effect of Medicaid expansion on the receipts of nonemployer establishments.

Using the ACS and CPS, I create balanced panels at the state level of participation in unincorporated self-employment as the primary source of income. These two panels are intended to be a comparable comparison to the state level NES. As shown in Figure 2.2, both the ACS and CPS appear to capture a stable level of respondents who identify unincorporated self-employment as their primary source of income, and a slight decline in the share of people who claim self-employment as a primary source of income. The NES on the other hand has seen a steady increase in the number of nonemployer establishments, and the number of nonemployer establishments per person. Differences between the trends of the survey and administrative data can be attributed to both the difference in definition, and the difference in context as the NES is administrative data built on reported tax fillings, which may have a more direct impact on a filer's willingness to disclose earnings.

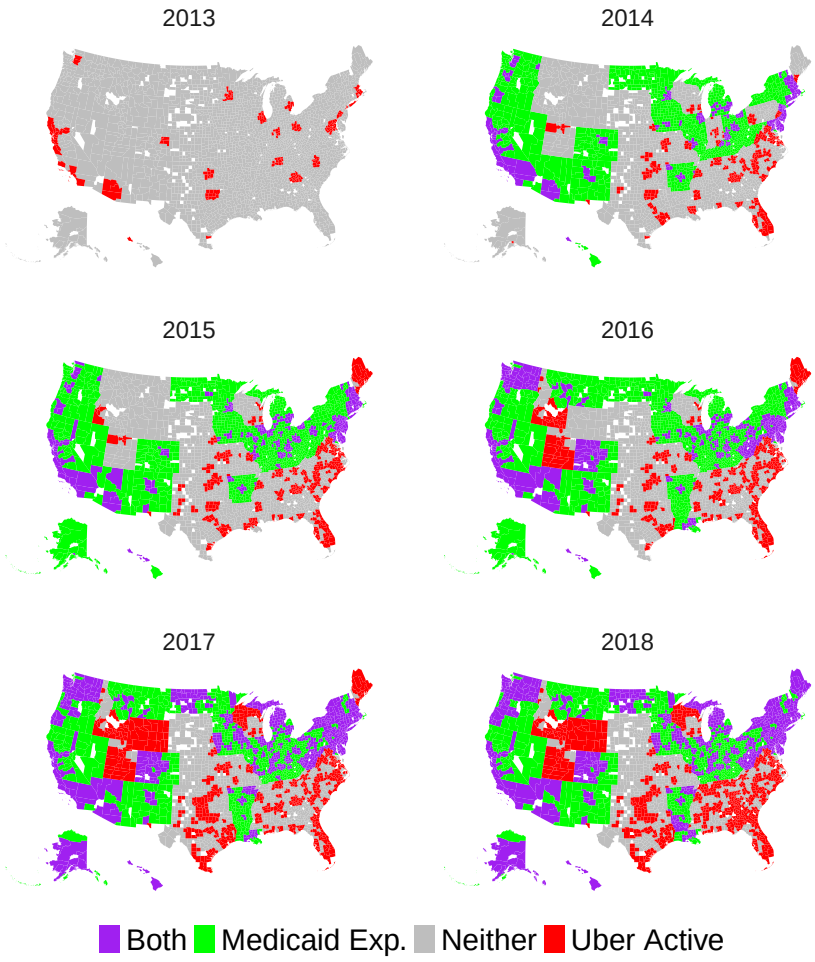
As outlined in the literature review, tax evasion may bias the comparison of effects between the survey sources of data and the administrative.[8] As a result, data on the geographic and time varying rollout of Uber is used to construct an indicator for likely workers in the online gig economy.[9] The addition of platform moderated work reduces the likelihood of tax evasion among Uber drivers. Using Uber drivers in comparison to the general transportation and warehousing industry, as well as a the unincorporated self-employed broadly, can help differentiate job lock, employment lock, and tax evasion. Uber deployed across the United States in a series of waves starting in 2011 in

A balanced panel of counties used in the analysis will vary in the number of counties by industry specification.

[8]While the evidence has already been made clear on the risk of evasion as a biasing factor for administrative tax data, evasion also can occur among the ACS and CPS on the intensive margin (Hurst, Li, and Pugsley, 2014).

[9]Lyft and other transportation services also expanded at the same time as Uber and in a similar geographic rollout. The indicator for when and where Uber is active will also be capturing these other platforms, and potentially underestimating the effect as it compares to "gig" treated counties.

FIGURE 2.1: Medicaid and Uber Expansion Map



The figures above depict the counties in which Medicaid expansion occurs from 2014 to 2017. Grey counties are areas which do not experience a Medicaid expansion, but green counties are where the expansion does occur. Red counties and purple counties show where Uber is active in a given year, split by if Medicaid expansion has or has not occurred in that state. White counties are counties which either do not appear in every year of the panel or are structural zeros and are dropped from the analysis.

San Francisco. It then spread nationally and internationally over the following years. Figure 2.1 shows this deployment strategy in action at the county level within the U.S. in relation to the expansion of Medicaid. This expansion in locations was not random, but over time the deployment strategy grew less dependent on local market characteristics.[10] By linking Uber deployment locations to FIPS state-county codes as defined in the NES, the presence or absence of Uber's marketplace is established for a given year. This data is only merged with the NES county panel though, as no comparable panel can be formed with the publicly available ACS or CPS data.

The treatment of Uber is expanded to include the core-based statistical areas (CBSAs) in which a county is a member.[11] This is done to capture the effect of commuting to work, where drivers may commute to counties or zones which have Uber active, but file their earnings from an address where Uber is not. Nonemployers are recognized in counties where they file their taxes and not strictly where driving occurs. Annual county labor force estimates are included using the Bureau of Labor Statistics Local Area Unemployment Statistics. Annual county population estimates are also included using the Annual Estimates of the Resident Population data from the Census Bureau.

2.5 Methodology

I begin this analysis with a comparison between administrative and survey sources of data. This comparison is valuable to illustrate differences in the methodology of data collection, the portion of the self-employed measured, and the differences in incentives facing respondents. This will also serve to ground the results of this analysis in the context of previous work on Medicaid expansion and self-employment. Within the ACS and CPS, I identify if an individual's primary source of income was through unincorporated self-employment at the time of being surveyed. This creates a proxy for the number of individuals who would be classified as nonemployer establishments within both the ACS and CPS. I create two dependent variables, the log of the number of unincorporated self-employed within the ACS and CPS, and the log of the count of nonemployer establishments in the NES.

Using the receipts data included in the NES, I construct a measure of the average receipts of the nonemployer establishments at the county level. Treating R_{it} as the total receipts taken in by nonemployer establishments in county i and year t, the average receipts of nonemployer establishments are defined as:

[10] For the purposes of identifying the effect of Uber, the date of operation of Uber in a given county is used to create an indicator for a homogeneous exempt labor market. This deployment data was supplied by Uber upon request.

[11] CBSAs are defined by the Census Bureau as a geographic area which "consist of the county or counties or equivalent entities associated with at least one core (urbanized area or urban cluster) of at least 10,000 population, plus adjacent counties having a high degree of social and economic integration with the core as measured through commuting ties with the counties associated with the core" (US Census Bureau, 2010).

$$r_{cit} = \frac{R_{it}}{E_{it}}$$

where E_{it} is the number of nonemployer establishments in state or county i and year t. If individuals respond truthfully and accurately on the ACS and CPS and do not participate in tax evasion, then any difference between the two may represent a difference in behavior between individuals treating self-employment as their primary source of income, or as a secondary or tertiary source of income. Alternative explanations for differences between the ACS/CPS and the NES include individuals failing to correctly identify themselves or household members as independent contractors or gig workers (Abraham et al., 2018b), strategic answering to avoid disclosure Hurst, Li, and Pugsley (2014), and the unincorporated self-employed not being the only type of work arrangement captured by the NES.

This analysis leverages a two-way fixed effect model, similar in specification to that used by Leung and Mas (2016), as shown by equation (2.1), where M_{it} is a dummy variable identifying if the state, s, or county, c, experienced Medicaid expansion in 2014, with both geographic levels labeled as i. I estimate this equation for the logged number of nonemployer establishments or unincorporated self-employed, the average receipts of establishments, and the total receipts of nonemployer establishments, but will use Y_{it} to represent all three.

$$Y_{it} = \beta_0 + \beta_1 M_{it} + \alpha_i + \tau_t + \mu_{it} \tag{2.1}$$

Two-way fixed effect models include control for time invariant geographic characteristics, α_i, where i denotes the geographic unit (e.g., state, county). When making comparisons between the ACS, CPS, and NES, the analysis is performed at the state level. Alternative specifications are used with additional controls and interactions, including an analysis of the Uber interaction effect with Medicaid expansion and controls for population density of counties. When including information on the deployment of Uber, the analysis is performed at the county level given the inclusion of county level Uber treatment. Year fixed effects, τ_t, control for shocks which occurred nationally. When utilizing τ_t the analysis is controlling for federal policy changes which are uniform across all states and counties, which is necessary given the deployment of the ACA nationally.

Equation (2.1) offers a conservative test for the effect of Medicaid expansion on self-employment, but the two-way fixed effect estimator is heavily reliant on the parallel trends assumption: that states which expanded Medicaid following the passage of the ACA would have continued on a similar path as those which did not expand Medicaid, and deviated from the path as a result of this treatment. The take up of expansion was not random though, and was at minimum politically linked, and this raises concerns about the validity of the parallel trends assumption.

Further, the literature on difference-in-differences methods has highlighted multiple weaknesses among the two-way fixed effect approach, including the use of negative weights, failure to validate parallel trends, and a nonconformity with event study designs (Borusyak and Jaravel, 2017; Sun and Abraham, 2020a; Goodman-Bacon, 2018; Imai, Kim, and Wang, 2018; Athey and Imbens, 2018; Callaway and Sant'Anna, 2020; De Chaisemartin and D'Haultfœuille, 2020b; De Chaisemartin and D'Haultfœuille, 2020a). These weaknesses are often specific to designs with heterogeneous treatment timing, and as such any use of the two-way fixed effect method is restricted to states which do not receive Medicaid expansion by 2018 and states which expanded Medicaid in 2014.

Given the identified weaknesses of the two-way fixed effect approach, I also leverage three alternative methodologies. These include a generalized synthetic control method (GSCM) (Xu, 2017), and two specific improvements on the difference-in-differences design from Callaway and Sant'Anna (2020) and De Chaisemartin and D'Haultfœuille (2020b), referred to as CSDID and DIDM in this paper respectively.

I use a GSCM (Abadie, Diamond, and Hainmueller, 2010; Abadie, Diamond, and Hainmueller, 2015) following the methodology of Bai (2009), Gobillon and Magnac (2016), and Xu (2017). GSCMs rely on an approach similar to the difference-in-differences estimator, but with a weighted control group to create a minimized difference control group. This weighted control group may better meet the parallel trends assumption as it approximates the pretreatment trends. Xu (2017)'s methodology generates a counterfactual for each treated unit from the control group by estimating a linear, interactive two-way fixed effect model. This method then allows for the estimation of average treatment effects from comparisons between each treated unit and it's specific synthetic control.

When constructing the GSCM for this analysis I utilize an interactive fixed effects estimator designed to estimate the treatment effects in regional policy evaluation using an Expectation-Maximization (EM) algorithm (Gobillon and Magnac, 2016).[12] The matching process in the pretreatment period for the state-level analysis is on the dependent variable and the state's population. When using county-level data in the NES, matching is done using the dependent variable of interest, the size of the counties labor force, the county population, the county population density, the county unemployment rate, and a measure for how long Uber has been active in the county.

While the GSCM method helps address the issue of parallel trends, two-way fixed effect approaches have been challenged on a variety of issues as discussed earlier. I use the difference-in-differences design outlined by Callaway and Sant'Anna (2020) to estimate the average treatment on the treated. This is done by estimating group-time average treatment effects after matching on time-invariant observables. It can then create dynamic treatment effects similar to an event study, but without the risk of negative weights. This

[12]This is done using the publicly available R package gsynth, maintained by Xu (2017).

methodology is functional even in situations with heterogeneous treatment timing, multiple periods, treatment effect heterogeneity, and conditional parallel trends. Similar to the CSDID, I also include the difference-in-differences estimator from De Chaisemartin and D'Haultfœuille (2020b) and De Chaisemartin and D'Haultfœuille (2020a). The DIDM also improves on the weaknesses of the two-way fixed effect approach, but differs from the CSDID in its conditioning on observables. The DIDM instead estimates the treatment effect on the residual after conditioning on confounding factors.

Where these alternatives to the two-way fixed effect approach are lacking, is the ability to include interaction effects with the treatment. When exploring the impact of Uber on the relationship between Medicaid expansion and self-employment, all three of the alternative methods are unable to directly estimate the interaction effect. This portion of the analysis then must rely on the two-way fixed effect approach among only the 2014 sample. This avoids the use of heterogeneous treatment timing.

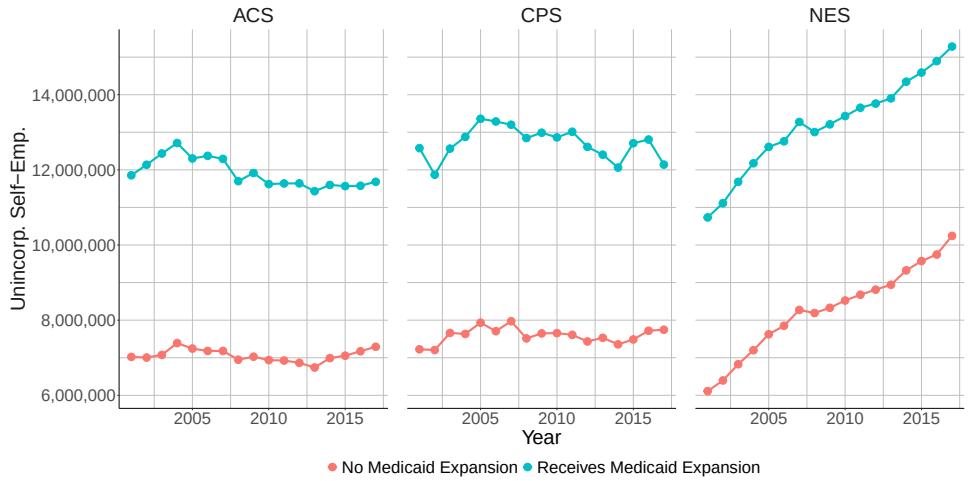
2.6 Results

Before introducing the two-way fixed effect approach, I assess the trends in unincorporated self-employment across states which expanded Medicaid by 2018, and those which did not. Figure 2.2 compares the trends in the number of unincorporated self-employed and nonemployer establishments in states which do and do not receive an expansion in Medicaid following the passage of the ACA. This figure also highlights the divergence in trends between declared self-employment earnings for tax purposes and survey responses regarding self-employment as a primary source of income. These trends are similar to those reported by Abraham et al. (2018b).

What can be seen in Figure 2.2 is a stability in the number of reported unincorporated self-employed among survey respondents in the ACS and CPS. The NES on the other hand shows increases in the count of nonemployer establishments. Across both the Medicaid expansion and non-Medicaid expansion states, an increase in nonemployer establishments occurs in 2014. This is a time when multiple aspects of the ACA go into effect, and an expansion in the availability of the online gig economy occurs across a number of different platforms. While the survey and administrative data sources differ in size and trend, no clear violation in parallel trends is seen at the aggregate level between those states which do and do not expand Medicaid.

Table 2.1 includes the comparison of results at the state level across the ACS, CPS, and NES. I find a consistent negative effect of Medicaid expansion on the number of unincorporated self-employed and declared nonemployer establishments. The level of significance, and estimate size, across all four methods varies though. In previous work done on the effect of Medicaid on self-employment at the state level, null results were generally reported. A key difference between the state-level results in Table 2.1 and those done by previous scholars working with the ACS and CPS is I fail to create sub-samples

FIGURE 2.2: Comparison in Trends Across States which do and do not Expand Medicaid



This figure plots the count of nonemployer establishments or unincorporated self-employed across the three data sets of interest, the ACS, CPS, and NES. The blue line shows the total count among states that took up Medicaid expansion by 2018. The red line shows states which had not expanded Medicaid by 2018.

of the most likely impacted by Medicaid expansion do to the inability to create a comparable sample in the NES. I also leverage new methods for assessing difference-in-differences designs. Specifically, the CSDID and DIDM will report an estimate of the effect of Medicaid on the logged total unincorporated self-employed without bias from the two-way fixed effect design.

TABLE 2.1: Estimates of the Effect of Medicaid Expansion Across Data Sources

Data	NES	ACS	CPS
	Dependent variable:		
	Log(Nonemp. Estab.)	Log(Unincor. Self-Employed)	
Medicaid Expansion (Two-Way)	-0.0602**	-0.0424	-0.0197
	(0.02724)	(0.0286)	(0.0325)
Medicaid Expansion (Generalized Synthetic Control)	-0.0171	-0.0092	0.0343
	(0.0137)	(0.0192)	(0.0337)
Medicaid Expansion (Callaway and Sant'Anna (2020))	-0.0240**	-0.0472**	-0.0098
	(0.0114)	(0.0208)	(0.0374)
Medicaid Expansion (De Chaisemartin and D'Haultfœuille (2020b))	-.0173	-.0397**	-.0033
	(.0105)	(.0144)	(.0294)

*p<0.1; **p<0.05; ***p<0.01

The two-way fixed effect results use the sample of states which had not expanded Medicaid by 2018 and states which expanded Medicaid in 2014.
The Generalized Synthetic Control Method used here matches counties on the dependent variable of interest and the state population. These results include both unit and time fixed effects, use nonparametric inference, and use an EM algorithm.
The Callaway and Sant'Anna (2020) method creates conditional parallel trends using time invariant characteristics. As such, I condition on the population in the first year of the sample.
All reported standard errors are clustered at the state level and results are weighted by state population.

The results from Table 2.1 point to a general reduction in self-employment among expansion states, but the state panel appears too noisy to come to a definitive conclusion on effect size or significance. The general negative effect across data sources and methodologies points to three explanations: (1) individuals are less likely to take up self-employment when they have access to health insurance, (2) in order to meet the income threshold of Medicaid expansion, individuals take up and declare fewer self-employment sources of income, and (3) individuals are less likely to report self-employed income (primary, secondary, or supplemental) as a result of the income threshold of Medicaid expansion. This failure to report self-employed income as an evasion tactic would clearly be seen in tax filings, but it may also be found among national surveys.[13]

Explanation (1) falls in line with the employment lock hypothesis and disemployment effects of publicly supplied health insurance (Garthwaite, Gross, and Notowidigdo, 2014; Dague, DeLeire, and Leininger, 2017). Explanation (3) falls in line with the tax evasion literature among the self-employed (Andreoni, Erard, and Feinstein, 1998; Saez, 2010; Chetty et al., 2012; Chetty, Friedman, and Saez, 2013). Explanation (2) falls between both, where individuals do face a disincentive to work so that they can gain access to an income threshold policy, but rather than evading taxes, they simply withdraw from work. As shown in Figures 2.2 and B.1, the effect of Medicaid expansion does not appear to be a reduction in the number of individuals with declared nonemployer income, but instead fewer new individuals declaring self-employment income in expansion states in comparison to non-expansion states.

To help differentiate between these possible explanations, I utilize the NES county level data on nonemployer establishments, the receipts taken in by these establishments, and data on Uber deployment by county over time. Table 2.2 highlights the effect of Medicaid expansion on nonemployer establishments across all four methodologies. With the improved panel, from state to county level, and information on the receipts taken in by nonemployer establishments, a clearer picture takes form. Across the four methods, both of the more recently developed difference-in-differences models perform similarly, and produce smaller effect estimates than either the TWFE or the GSCM.

The CSDID and DIDM methods estimate that Medicaid expansion resulted in ~2.5% fewer nonemployer establishments among expansion states. This is paired with an insignificant effect on the average receipts of nonemployer establishments and a general reduction in the total reported receipts of ~1.49%, though this effect is only significant at the 10% level. These effect estimates are in the range of estimates from Table 2.1, and given the critiques of TWFE methods, I favor the effect estimates from the CSDID and DIDM methods.

[13] As Hurst, Li, and Pugsley (2014) note, survey data sources are not immune to evasion tendencies, as measured on the intensive margin. Individuals self-identifying as self-employed have a tendency to under report earnings by 25%. In this case, I am exclusively comparing the count of unincorporated self-employed to the number of nonemployer establishments, and not the reported earnings.

TABLE 2.2: Estimates of the Effect of Medicaid Expansion, Nonemployer Statistics at the County Level

	Dependent variable:		
	Log(Nonemp. Estab.)	Log(Average Receipts)	Log(Total Receipts)
Medicaid Expansion (Two-Way)	-0.0872***	0.0509***	-0.0363***
	(0.0199)	(0.0162)	(0.0123)
Medicaid Expansion (GSCM)§	-0.0548***	0.0351*	-0.0466***
	(0.0186)	(0.0187)	(0.0178)
Medicaid Expansion (CSDID)‡	-0.0250**	0.0101	-0.0148*
	(0.0118)	(0.0080)	(0.0088)
Medicaid Expansion (DIDM)†	-0.0252***	0.0116	-0.0136*
	(0.0095)	(0.0073)	(0.0073)

*p<0.1; **p<0.05; ***p<0.01

The two-way fixed effect results use the sample of states which had not expanded Medicaid by 2018 and states which expanded Medicaid in 2014. These results are weighted by county population.
§The Generalized Synthetic Control Method used here matches counties on the dependent variable of interest, the size of the counties labor force, the county population, the county population density, the county unemployment rate, and a measure for how long Uber has been active in the county. These results include both unit and time fixed effects, are weighted by population, use nonparametric inference, and use an EM algorithm.
‡The Callaway and Sant'Anna (2020) method creates conditional parallel trends using time invariant characteristics. As such, I condition on the year in which Uber begins operation in a given county. These results are weighted by county population.
†The De Chaisemartin and D'Haultfœuille (2020b) method estimates the treatment effect on the residual after conditioning on the population density of counties and how long Uber has been active in a county. These results are weighted by county population.
All reported standard errors are clustered at the state level.

I also validate the parallel trends assumption using a dynamic event study design across the treatment groups. This is shown in Figure 2.3, and I find no significant evidence of a violation in parallel trends in the four years before treatment.

While the treatment in this case is access to Medicaid expansion, it is important to note that this expansion takes place in the context of the ACA broadly. Multiple aspects of the ACA were thought to potentially increases the amount of self-employment taken up, including state insurance exchanges, expanded dependent coverage on health plans, and subsidies for the purchase of health insurance on exchanges (David, Melinda, and Rachel, 2015). Given the continued increase in declared nonemployer establishments, and the anticipated effects of the ACA at the federal level on self-employment, these results suggest that the expansion in Medicaid reduced the growth in self-employment among expansion states.

It is possible then, that this reduction in growth would be unevenly distributed across types of self-employment. Those jobs which are easier to enter and exit would be the most likely to be hit by any swing in incentive. If the expansion in Medicaid reduced the incentive for workers to take up self-employment as a primary or secondary source of income, then that effect should be greatest among the nonemployer establishments with low capital constraints. Using the NES industry classification, I estimate the effect of Medicaid expansion across a series of two-digit NAICS classifications. These results can be seen in

FIGURE 2.3: Dynamic Event Study using Callaway and Sant'Anna (2020)

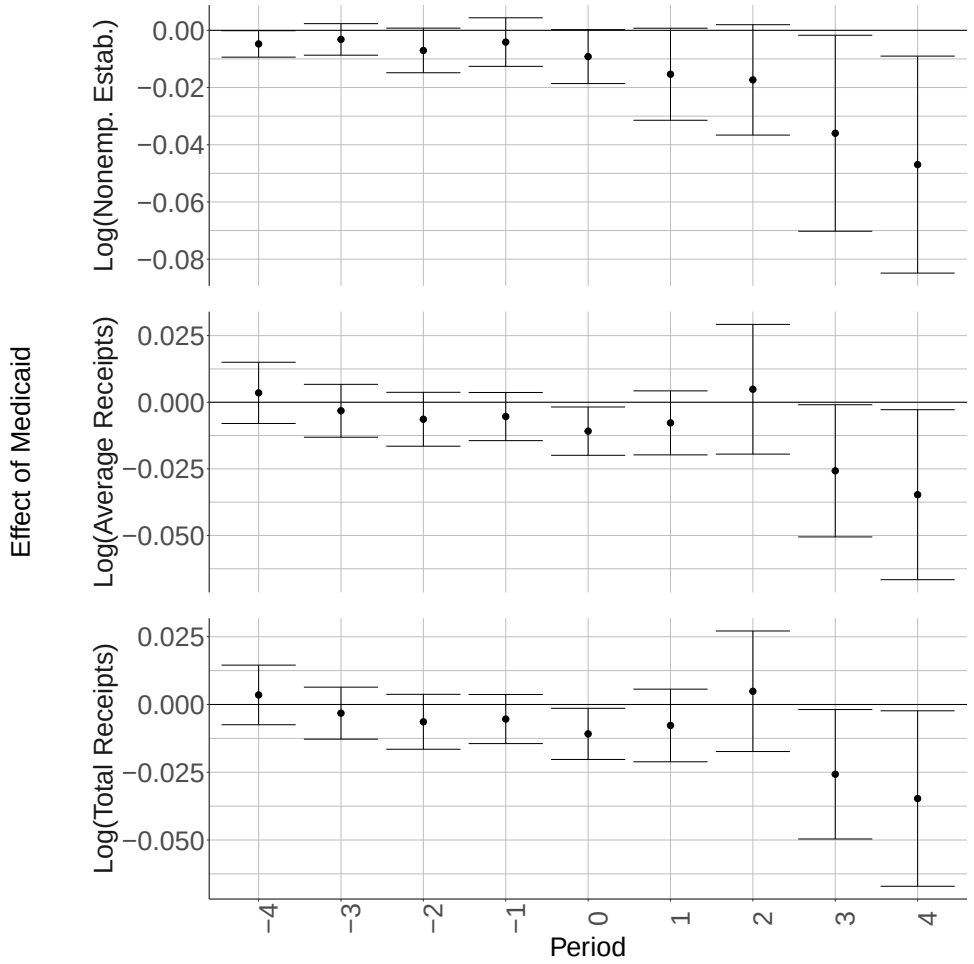
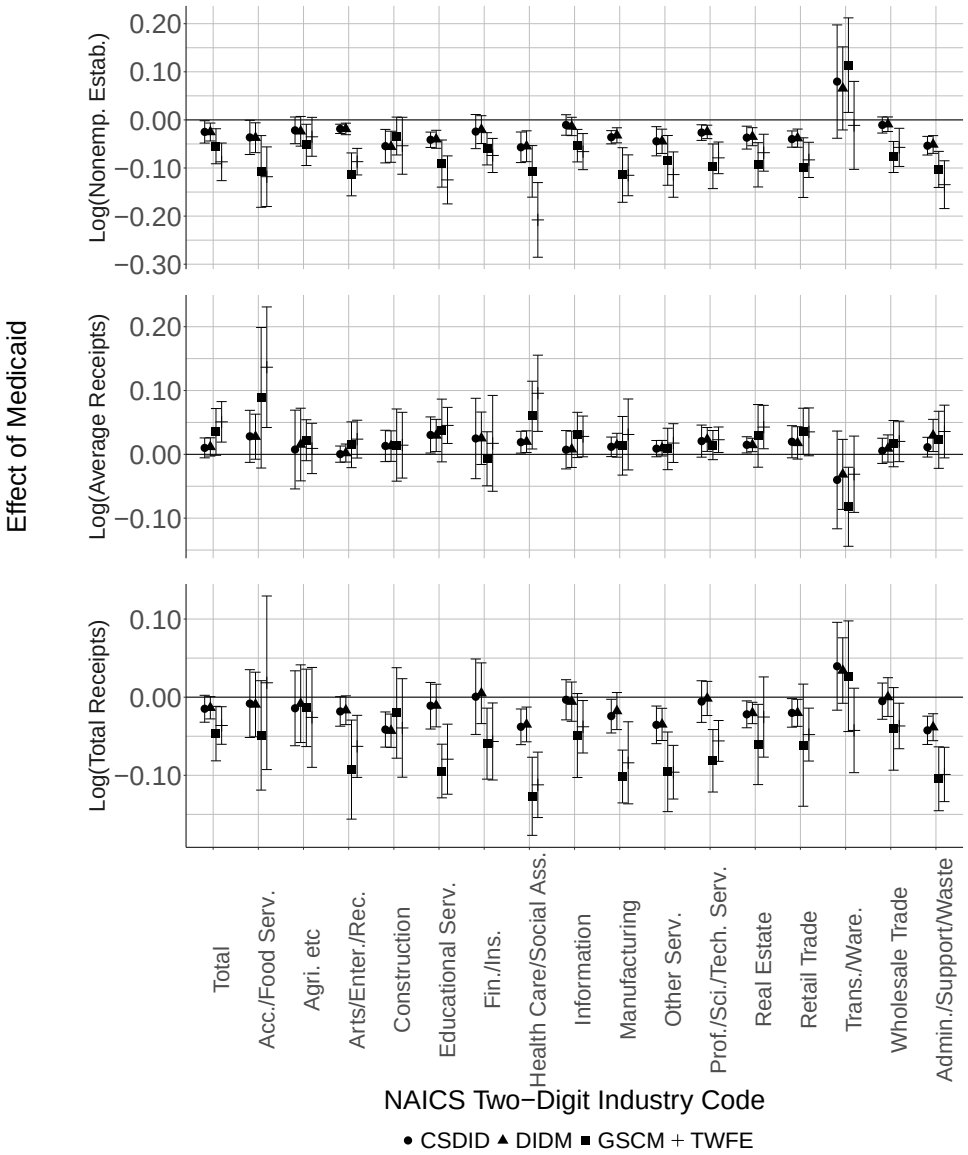


Figure 2.4.

FIGURE 2.4: NAICS Industry Comparison



This figure shows the estimated effect of Medicaid expansion across the three primary dependent variables in the NES, broken out by NAICS industry subsets.

The effect of Medicaid expansion appears relatively consistent across industries in Figure 2.4, with one exception, transportation and warehousing services. Transportation and Warehousing services appear to not only deviate in effect size but also direction for all three of the dependent variables of interest. Occurring during a similar time frame as the expansion in Medicaid, Uber increased in availability across the US, as well as many other sources of online gig work. Uber acts as an information reporter to the IRS of worker earnings, discouraging tax evasion as a response to Medicaid expansion among Uber drivers. The expansion in the amount of transportation and warehousing services, whose earnings are now coming from an information reporting

platform, may be an explanation for this deviation from the trend. Using nonemployer establishment data on transportation and warehousing services and where/when Uber is active in reference to the expansion in Medicaid can allow for a test of the true reduction in self-employment among transportation and warehousing services.

The use of Uber in this way is particularly salient given that online gig work should be one of the more responsive types of self-employment to a job lock or employment lock effect from Medicaid expansion due to the low barriers to entry and exit. Table 2.3 uses an Uber interaction with Medicaid expansion to identify the degree to which the effects shown in Figure 2.4 are a result of Uber's expansion, Medicaid's expansion, or the interaction of the two.

TABLE 2.3: The Effect of Medicaid Expansion and the Availability of Uber

	Dependent variable:		
All Nonemployers	Log(Nonemp. Estab.)	Log(Average Receipts)	Log(Total Receipts)
Medicaid	-0.104^{***}	0.053^{***}	-0.051^{**}
	(0.024)	(0.018)	(0.022)
Uber Active	0.052^{***}	-0.032^{***}	0.019
	(0.012)	(0.007)	(0.013)
Medicaid∗Uber Active	0.049^{***}	-0.015	0.034^{**}
	(0.017)	(0.012)	(0.014)
Pop. Density	0.00000^{**}	0.00000	0.00000^{***}
	(0.00000)	(0.00000)	(0.00000)
Observations	48,456	48,456	48,456
R^2	0.999	0.922	0.998
Adjusted R^2	0.998	0.917	0.998
Transportation and Warehousing			
Medicaid	-0.179^{***}	0.051^{**}	-0.128^{***}
	(0.064)	(0.024)	(0.047)
Uber Active	0.152^{***}	-0.061^{***}	0.092^{***}
	(0.044)	(0.019)	(0.028)
Medicaid∗Uber Active	0.358^{***}	-0.162^{***}	0.195^{***}
	(0.074)	(0.042)	(0.038)
Pop. Density	0.00001	0.00000	0.00001^{***}
	(0.00001)	(0.00000)	(0.00000)
Observations	45,378	45,378	45,378
R^2	0.990	0.844	0.991
Adjusted R^2	0.989	0.835	0.990

*p<0.1; **p<0.05; ***p<0.01

These results are from the two-way fixed effect on the sample of states which had not expanded Medicaid by 2018 and states which expanded Medicaid in 2014.

Without the presence of Uber, transportation and warehousing services appear to behave in a similar way as other industries, with a negative effect on the number of nonemployer establishments, a positive effect on the average receipts of nonemployer establishments, and a negative effect on the total reported receipts. Once Uber is active though, the general effects are

overwhelmed. This is seen most clearly among the subset of transpiration and warehousing services at the bottom half of Table 2.3. Without Uber being active, Medicaid expansion was estimated to reduce the number of transportation and warehousing services by ~16.4%. Once active, and controlling for the Uber effect independent of Medicaid expansion, the number transportation and warehousing services increases by ~19.6%.

Viewing these results on transportation and warehousing services in the context of the aggregate effect outlined earlier, it appears likely that a substantial amount of tax evasion is occurring. Before introducing the secondary reporter, the explanation for the negative effect in the number of nonemployer establishments, without tax evasion, was a reduction in the number of new nonemployer establishments. This effect would be expected to persist among transportation and warehousing services, but the results of Table 2.3 indicate that is not the case. In fact, it appears that the job lock hypothesis best explains the growth in transportation and warehousing services conditional on when and where Uber was in operation.

In total, I find that the expansion in Medicaid resulted in a reduction in the reported receipts of all nonemployer establishments by ~1.49%. In 2013, among expansion states, the total declared receipts were ~$674 billion.[14] This results in a loss of nearly $10-billion dollars of declared income, either through evasion, real exit, or prevented entrance into self-employment.

The number of declared nonemployer establishments also fell by ~2.5% among expansion states. Relative to the 2013 total count of nonemployer establishments in the sample, 13,857,171, this would be a reduction of 346,429 nonemployer establishments. This reduction appears to have been distributed across industries with the exception of transportation and warehousing services. Following the analysis of Uber's effect on nonemployer establishments in relation to Medicaid expansion, we have reason to believe that a significant portion of the ~2.5% reduction in declared self-employment is actually tax evasion. One lesson from the Uber interaction is that self-employment in the online gig economy offers a more reliably taxable form of self-employment thanks to the presence of an information reporting platform. The expansion in the online gig economy may be making substantial contributions to reduce evasion through self-employment generally in addition to income-threshold policies.

2.7 Conclusion

Following the passage of the Affordable Care Act and the expansion of Medicaid to individuals below 138% of the federal poverty level, a new group of individuals gained access to publicly provided health benefits. This access has often been associated with both job lock and employment lock effects on the supply of labor. Simultaneously, the nature of work within the US was

[14] This value comes from the NES county sample used for this analysis. Counties which were censored or dropped to balance the panel are not included in this valuation.

changing and the prevalence of alternative work arrangements and nonstandard work increased as a supplemental source of income (Current Population Survey Staff, 2018; Abraham et al., 2018b; Katz and Krueger, 2019). This paper tested if the expansion of Medicaid to individuals below 138% of the federal poverty level impacted self-employment in a broad sense, but also how it varies across traditional and new forms of work.

Using data on the self-employed from Nonemployer Statistics (NES), the American Community Survey (ACS), and the Current Population Survey (CPS), I find evidence of a negative effect of Medicaid expansion on unincorporated self-employment and nonemployer establishments. Using county level administrative tax filings data, NES, I estimate that states which expanded Medicaid had ~2.5% fewer nonemployer establishments, and a reduction in total declared receipts of 1.49%. Using data from 2013, before Medicaid expanded, this would be roughly equivalent to a $10-billion reduction in declared earnings by the self-employed and just under 350,000 fewer declared nonemployer establishments among states which expanded Medicaid.

By using data on the expansion of the online gig economy, as measured by the geographic and time varying rollout of Uber, and leveraging the fact that Uber practices information reporting to the IRS, I show that the negative effect among administrative data is likely significantly biased by tax evasion. This result falls in line with previous work on means-tested programs (Andreoni, Erard, and Feinstein, 1998; Saez, 2010; Chetty et al., 2012; Chetty, Friedman, and Saez, 2013). Among transportation and warehousing services, I find that Medicaid expansion results in an increase in total receipts and declared nonemployer establishments, when Uber is active. This is after accounting for the effect of Uber on its own.

This analysis fits among previous work on the effect of Medicaid expansion on the labor market, and self-employment in particular. These results also indicate that increasing the number of non-monetary means-tested programs increases the incentive to evade, even in situations where income reporting is filled with a state agency as appose to the IRS. This result also adds to the literature on the interaction between the online gig economy, self-employment, and public policy, as it demonstrates a potential benefit of platform work in its capacity to reduce tax evasion.

Chapter 3

Multiple Jobholding and the Minimum Wage

3.1 Abstract

This chapter tests whether the increase in state minimum wage policies from 2013 to 2016 impacted multiple jobholding and the balance of work hours between jobs. Using the Survey of Income and Program Participation, I find minor evidence of an effect of the minimum wage on the multiple jobholding status. A $1 increase in the minimum wage is shown to increase the probability of transitioning into multiple jobholding over a 12 month period post treatment by 0.6%. I do not find evidence of a significant effect on hours worked or earnings across primary or secondary sources of income. In total, minimum wage policies appear to have little impact on multiple jobholding. These findings highlight that minimum wage policies are not an effective way to reduce the necessity of multiple jobholding among low-wage workers.

3.2 Introduction

Starting in 2012, attention to local policy change in support of workers took off (Meyerson, 2014; Dube and Lindner, 2021). Due to a stagnating federal minimum wage and underwhelming state minimum wage policies among high cost-of-living cities, workers, advocates, and policy makers shifted their attention to local minimum wage policies (Rapoport, 2016). This movement has broadly been referred to as the "Fight for $15." A secondary concern of the "Fight for $15" effort was multiple jobholding (Migoya, 2013). When workers find themselves making too little, or unable to receive enough working hours at a single job, they may seek out additional sources of income. The issue comes from the additional costs of managing two positions. While rising minimum wages may increase the returns to low-wage work, it is theoretically ambiguous what effect a higher minimum wage will have on multiple jobholding. I find little evidence of an effect of minimum wages on the rate of multiple jobholding in aggregate.

Previous work on multiple job holding has tended to focus on the motivations of workers who opt to work in more than one job (Smith Conway and Kimmel,

1998; Kimmel and Powell, 1999; Kimmel and Conway, 2001; Renna and Oaxaca, 2006; Dickey, Watson, and Zangelidis, 2011; Panos, Pouliakas, and Zangelidis, 2014). Among the motivations identified in this literature, hours constraints and household income are both shown to be consistent factors. Demographic characteristics and industry of work have also been major factors. Women, single adults, and service sector employees have a greater tendency to take up more than one job at a time (Allen, 1998; Kimmel and Powell, 1999; Averett, 2001; Amuedo-Dorantes and Kimmel, 2009). Minimum wage policies are also associated with increased hours constraints, impacts on earnings, and varied effects by demographic characteristic and sector of the economy (Clemens and Wither, 2014; Jardim et al., 2017; Jardim et al., 2018; Caliendo et al., 2018a).

The most direct test of the relationship between the minimum wage and multiple job holding comes from an analysis of the national minimum wage in the United Kingdom. Robinson and Wadsworth (2007) found little impact of the minimum wage on the holding of a second job after the national minimum wage was introduced. This analysis was on a slightly different treatment though, introducing a national minimum wage instead of increasing an already existing state minimum wage. This change also occurred in a different institutional environment, with a substantially different social safety net and set of available supports than are available for low-wage workers in the United States. Further, it utilizes a two-way fixed effect approach with potentially heterogeneous treatment effects. As such, the research question bears re-examining with an updated methodology and in an alternative institutional environment.

Using the 2014 Survey of Income and Program Participation (SIPP), I assess the degree to which increasing the minimum wage at the federal, state, or local level impacts multiple job holding. This analysis is done through the use of an updated difference-in-differences methodology intended to address the flaws of two-way fixed effect designs. Given the previous literature, and a brief discussion of theory, I hypothesize that increases in the minimum wage will induce greater hours restrictions among individual workers, which will increase the take up multiple jobholding as a compensatory measure. This hypothesis assumes that the increase in the minimum wage will rarely be large enough to induce low-wage workers to leave their secondary job, but it will be large enough to induce hours constraints from employers.

I find that minimum wage increases do not have a statistically significant impact on the aggregate multiple job holder rate, but it does produce a significant increase in the take up of multiple jobholding in the 12 months that follow a minimum wage increase. A $1 increase in the minimum wage is shown to increase the probability of taking up multiple jobs over a 12 month period post treatment by 0.6%. This increase in the take up of multiple jobs is not shown in the aggregate rate of multiple jobholding. I find no evidence of a statistically significant effect of the minimum wage level on hours worked or earnings across primary or secondary sources of income. These results imply a null effect of the minimum wage on low-wage workers.

While minimum wage increases may represent an increasing price floor in the labor market, they do not appear to be an effective tool to alleviate a workers need, or tendency, to take up multiple jobs. While the "Fight for $15" movement has made strides to support minimum wage increases across the United States, it appears multiple jobholding is not an area which has been impacted.

3.3 Literature Review

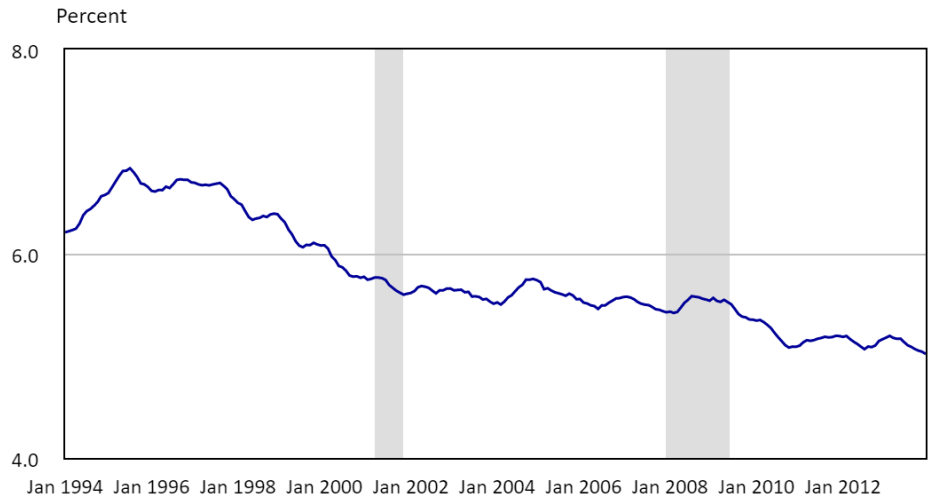
Multiple jobholding in the United States has been a strategy used by a minority of workers, with a steady proportion between 4.5% and 6.2% of total employment since 1970. Since the mid-90s though, the United States has seen a ~20% reduction in multiple jobholders when measured by the Current Population Survey (CPS) (Hipple, 2010). This rate captures workers who had more than one job or business in the survey week.[1] While the share of the labor force engaged in multiple jobholding had remained somewhat stable between the 1970s, and the most recent peak in the mid-90s, it is unclear why exactly the decline observed in the CPS is occurring. Whether the decline is even occurring is also challenged by alternative data sources. Recent work has demonstrated an increase in the proportion of employment made up of multiple jobholders, as measured by the Longitudinal Employer-Household Dynamics (LEHD) data (Bailey and Spletzer, 2020).

Changes in the trends of multiple jobholding may mean changes in motivation, circumstance, or both among workers. Previous work has noted that multiple jobholding moves in a procyclical way, with increased multiple jobholding in times of job growth, and falling in economic downturns (Stinson Jr, 1986; Stinson Jr, 1990; Stinson Jr, 1997; Kimmel and Powell, 1999; Amuedo-Dorantes and Kimmel, 2005; Bailey and Spletzer, 2020). The United States labor force also had more workers combining a full-time job with a part-time job in comparison to Canadian workers throughout the 90s (Kimmel and Powell, 1999), but the decline in multiple jobholding throughout the 2000s appears to be a reduction in the tendency for workers to take up a part-time job in addition to their full-time position (Lalé, 2016). With increased availability of monthly data on multiple jobholders, this conventional finding has been questioned (Hipple, 2010). Multiple jobholding trends remain reliant on both workers' willingness to take up multiple jobs and firms' desire to hire more workers.

Why an individual takes up more than one work arrangement at a given time can be dependent on nonpecuniary aspects of the second job (Kimmel and Conway, 2001; Dickey, Watson, and Zangelidis, 2011; Panos, Pouliakas, and Zangelidis, 2014), heterogeneity in job characteristics (Smith Conway and Kimmel, 1998; Dickey, Watson, and Zangelidis, 2011), hours constraints

[1] Alternative definitions may be just as important though. For example, multiple job holding may better be defined as "a worker who, at any point during a year, worked simultaneously at their main job and at least on other job for longer than 1 week" (Beckhusen, 2019).

FIGURE 3.1: Multiple Jobholding Rate, January 1994 - December 2013



Hover over chart to view data.
Note: Shaded areas represent recessions, as determined by the National Bureau of Economic Research.
Source: U.S. Bureau of Labor Statistics.

This figure shows the multiple jobholding rate using a 3-month centered moving average, seasonally adjusted, from January 1994 to December 2013. These data comes from the Current Population Survey and a multiple jobholder is defined as an individual who held more than one job during the reference week of the survey and who usually receives a wage or salary from his or her primary job (*Multiple jobholding over the past two decades*).

(Smith Conway and Kimmel, 1998; Kimmel and Conway, 2001; Renna and Oaxaca, 2006), and financial difficulties or increased financial commitments (Allen, 1998; Dickey, Watson, and Zangelidis, 2011). When considering who multiple jobholders are within the United States, it has been shown that women, never-married individuals, young persons, and service workers have increased in their take up of multiple jobs throughout the 1990s (Averett, 2001).

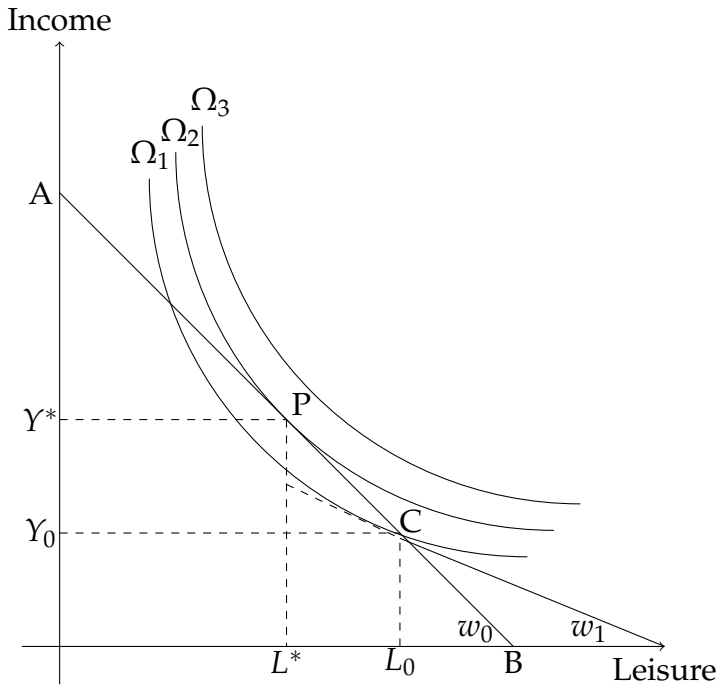
Motivations for multiple jobholding can largely be separated by preference mechanisms and necessity mechanisms. Workers who seek out multiple jobs to gain a greater variety of work experience, mediate transitions across jobs, or acquire more skills, are likely to experience multiple jobholding differently than workers who need to seek out additional employment opportunities. This need could be a result of increased financial burden, such as starting a family or supporting a household, an unexpected financial shock, job loss, or an inability to acquire the necessary hours in their primary source of employment.

We can expect that these motivations are not equally distributed across the income spectrum. Lower-income individuals, or those with limited savings, are more susceptible to financial shocks. If individuals find themselves having to acquire an additional job, they may be faced with additional commuting times, unstable scheduling, work stresses, and household costs including the care of dependents. If an individual worker was previously not seeking out an additional job, and only did so due to a financial shock or burden, or newly instituted hours constraints, then multiple job holding would be a sign of welfare loss, even if earnings increased.

For those workers who experience hours restrictions as a motivation for multiple jobholding, policies which are likely to reduce hours, or prevent workers from acquiring more work, may induce multiple jobholding. Minimum wage policies are a likely candidate to do just that. While the minimum wage literature is dense, and has found mixed results with regards to the aggregate effect on employment (Dube, 2019a; Neumark and Shirley, 2021), recent work pertaining to the effect on hours and scheduling indicates a negative effect of minimum wage policies on hours worked (Couch and Wittenburg, 2001; Sabia, 2009; Caliendo et al., 2018b; Jardim et al., 2018). As minimum wages increase, workers will have an increased incentive to take up multiple jobholding as a compensatory measure for hours constraints.

Jardim et al. (2018) helped demonstrate the negative hours effect in the case of the Seattle minimum wage, but it also showed an increased difficulty for new entrants to find work. This burden on job search may negatively impact a workers ability to take up a secondary source of income following a minimum wage increase. While hours constraints at the individual level may lead workers to seek a second job, when hours constraints are systemic across a type of work (i.e. low wage labor) workers may be unable to pick up a second job. The movement into and out of multiple jobholding may be impacted by minimum wage policies as workers move into and out of

FIGURE 3.2: Labor-Leisure Trade-off



An illustration of the Labor-Leisure trade-off as a motivation for multiple jobholding when an hours constraint is present.

second jobs frequently with significant changes in hours across jobs (Paxson and Sicherman, 1996).

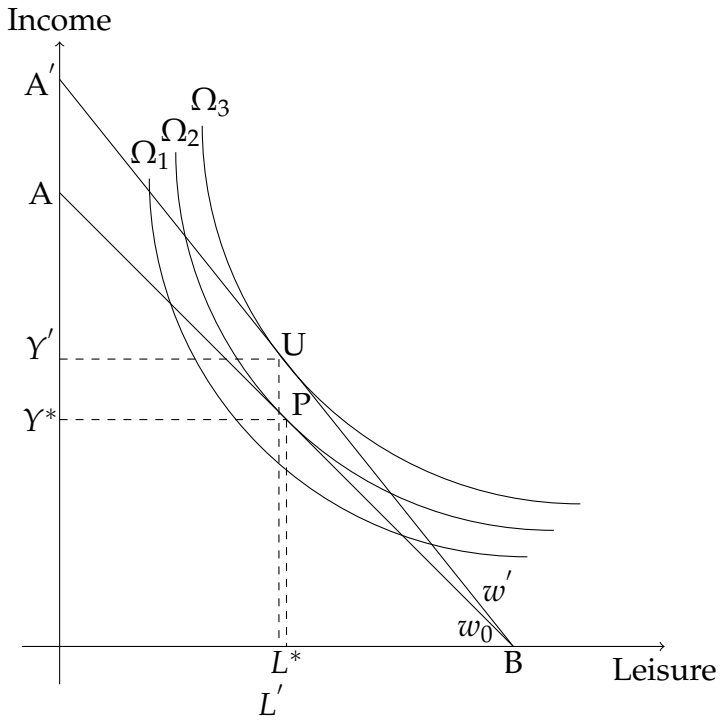
3.4 Theoretical Model

Working from the theoretical model outlined by Shishko and Rostker (1976), the potential income and substitution effects as a result of a minimum wage become clear. Shishko and Rostker (1976) begin by identifying the trade-off between labor and leisure, and motivate multiple jobholding as a means of acquiring more working hours to satisfy an income goal. This approach to motivating multiple jobholding is illustrated in Figure 3.2 and falls in line with an hours constraint motivation for multiple jobholding.

In Figure 3.2, depicts a representative set of indifference curves for an agent combining income and leisure, with point A representing the maximum amount of income which can be generated, and B the maximum amount of leisure. The slope of the line AB, w_0, is the negative wage rate for this agent. By setting the wage rate equal to the marginal rate of substitution between income and leisure, the agent selects bundle P, the tangency between Ω_2 and the wage line.

If the agent is restricted to L_0, then they would be seeking an additional $L^* - L_0$ hours, but would be unable to due to their hours constraint. This would force the individual to point C on indifference curve Ω_1. In this case,

FIGURE 3.3: Labor-Leisure Trade-off with a Minimum Wage



An illustration of the Labor-Leisure trade-off for a minimum wage worker after a minimum wage increase.

so long as an additional source of income has a wage rate greater than the marginal rate of substitution at point C, w_1, they will accept the job and the additional hours. If they can set the hours themselves, they will work until they reach point P.[2]

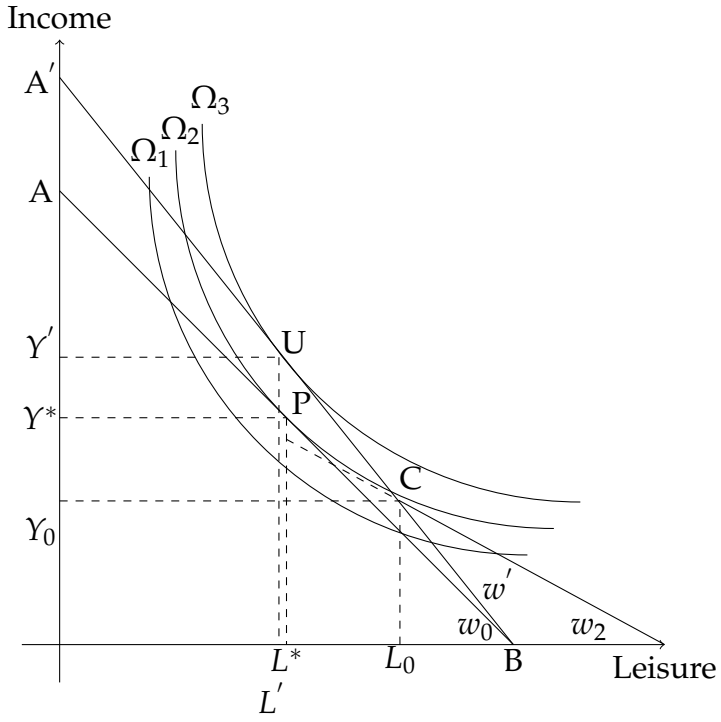
This situation creates a range of suitable wages for which the agent will take on a secondary source of income, $w_1 < w < w_0$, and presumably if the agent had access to a wage greater than w_0, it would be accepted and become the primary job, and this change may induce a new equilibrium amount of hours worked. This initial framework highlights how an hours constraint may induce an individual to seek out a second job, but what happens if a minimum-wage worker sees both an increase in their primary wage, w_0, and a new hours constraint?

For a minimum-wage worker, who sees their wage increase from w_0 to $w^{'}$, we can imagine a circumstance like Figure 3.3 where the worker responds by increasing the number of hours they work and reducing leisure. Thanks to the increased wage rate, the worker will reach a higher indifference curve, Ω_3, and experience a welfare improvement.

This analysis is assuming no new hours constraint is imposed though. If the minimum wage policy is introduced, increasing the agent's wage from w_0 to $w^{'}$, but it carries with it an hours constraint, restricting the agent to L_0, then

[2]This is assuming no additional costs to securing, or operating in, the job.

FIGURE 3.4: Labor-Leisure Trade-off with a Minimum Wage and Hours Constraint



An illustration of the Labor-Leisure trade-off for a minimum wage worker after a minimum wage increase, which induces an hours constraint.

welfare implications are unclear. It is possible to end on a lower, higher, or the same indifference curve as before depending on the shift in wage rate and the size of the hours constraint. In the case of Figure 3.4, the agent has ended on the same indifference curve as before, Ω_2, earning less income and experiencing more leisure than they had before a minimum wage or hours constraint were introduced.

It is also worth considering that since $w^{'}$ is the new minimum wage, the agent should be willing to take any form of work which is not exempted from the minimum wage as it will lay between the marginal rate of substitution of income and leisure and the minimum wage. While the range of acceptable wages for this agent is $w_2 < w < w^{'}$, $w^{'}$ is the wage floor. This is only the case though because point C fell to the right of point U. If instead the income effect of the minimum wage was substantial enough that point C fell to the left of point U, then the agent would have no incentive to take up a secondary source of income.

3.5 Data

In order to test the effect of minimum wage increases on multiple jobholding, I use the 2014 Survey of Income and Program Participation (SIPP). The SIPP is a household-based panel survey, with each panel being a nationally representative sample interviewed over a multi-year period of roughly four years. The SIPP covers a wide range of topics, including detailed information on up to seven jobs held by an individual throughout the full sample. This information includes when a job was held, types of employment, work characteristics, reasons for not holding a job, and if an individual was searching for work. These data allows for a study of the dynamics of employment at the individual level over time and across policy changes. The data is collected over the course of four waves in 2014, 2015, 2016, and 2017 and each wave collects data on the previous calendar year.

For employment and earnings data in the 2014 SIPP, the data is collected at the employment spell level as opposed to the person-month format.[3] A majority of employment or non-employment information will not change then. Respondents' wage/salary earnings within a given job spell are recorded in reverse chronological order and allow the respondent to report up to two changes in pay rate over a job spell and two changes in the number of hours worked. These changes may capture pay raises, changes in part-time status, shifts in weekly hours, and policy changes.[4] Unlike previous versions of the SIPP, data are collected on up to seven jobs instead of just two.

The regular measurement and coding of employment data, specifically the monthly earnings and hours worked by respondents across multiple jobs, makes the SIPP ideally suited to test the effects of policies on multiple jobholding. One negative aspect of the SIPP is a significant share of respondents become nonresponsive across survey waves. Both the treatment group, those who receive at least one minimum wage increase, and control group, those who do not experience a minimum wage increase, of this analysis vary in size from sample wave one to four. I restrict the sample to those individuals who are present for all four waves of the SIPP to maintain a balanced panel and avoid a need for implementation into missing periods.[5] For the purposes

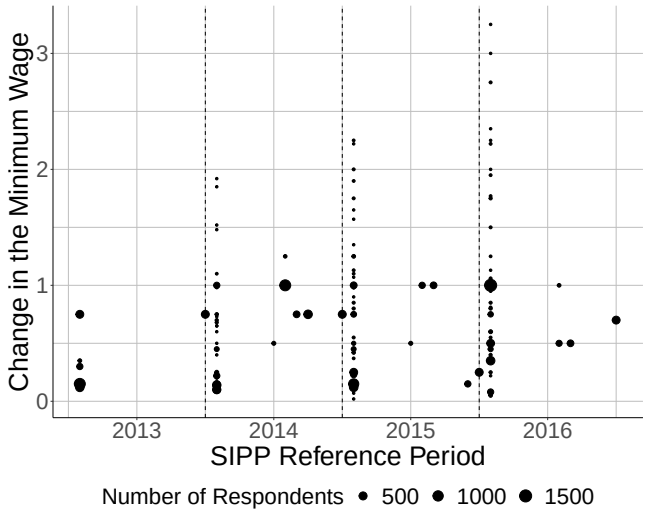
[3]While employment is coded at the spell level it is then translated to the person-month format when actually applied to the data.

[4]Commissions, tips, overtime, and bonuses are all collected at the job spell level if they are recorded every month, but if they are not received every month, they are collected as monthly amounts. This is particularly important among workers who are in heavy tipping industries and may also be exposed to minimum wage changes.

[5]By balancing to individuals who are present across all four waves, these results are best viewed as an analysis of stable workers. SIPP classifies three types of nonresponse. Type A household nonresponse is when "the interviewer finds an eligible household address, but obtains no interviews." Type D household nonresponse, where "original sample members move to an unknown address or an address where the respondent is unable to be interviewed." Type Z Person-level Nonresponse occurs when "a sample person is part of the household on the date of the interview but refuses to answer, or is not available for the interview and a proxy interview is not obtained" (Bureau, 2016). Nonrespondents were estimated to have had a higher median annual earned income and total income by $9,390 and $3,670

of this analysis, I subset the balanced panel to only individuals whose average wage across the whole panel is less than $19 to focus on only those respondents who are most likely to be impacted by the minimum wage.[6]

FIGURE 3.5: Minimum Wage Changes by Timing and Size



This figure plots the timing of minimum wage increases across the four survey waves of the SIPP. It also identifies the size of a minimum wage change (in dollars) and how many respondents are exposed to a given minimum wage increase. This information, as well as the number of never-treated units, can be found in table 3.2

An additional benefit of the SIPP in comparison to many administrative data sources on individual employment is the inclusion of demographic and socioeconomic characteristics. I include the age, sex, and family size of respondents as potential controls. Each of these variables may offer a reason to be both exposed to the minimum wage and take up multiple jobholding.

The treatment for a minimum wage is defined by the state-level minimum wage. Local minimum wages are not included as they could not be identified with the publicly available SIPP given SIPP's restrictions on geographic identifiers. Changes in the minimum wage are coded at the state-month level, with the month that a change occurs being the first period of treatment. Across the sample multiple minimum wages occur at differing times during the year and have uneven impact on the number of respondents exposed. Figure 3.5 shows at what point in a given survey window a minimum wage occurs, and the size of the minimum wage increase in dollars. This same information can be found in Table 3.2.

To estimate the effect of increasing the minimum wage on multiple jobholding, I use a set of nine dependent variables (as listed by the first nine rows of

respectively. Household net worth among nonrespondent households were $16,950 lower though, implying that the balanced panel used here may overestimate household net worth while underestimating household earnings and income (Bureau, 2020). Since this analysis is targeting low-wage workers, I am unconcerned by this.

[6]This calculation of the average wage is done excluding months where an individual is outside the labor force rather than treating those months as a wage of $0.

table 3.1). I measure engagement in multiple job holding with an indicator variable for if workers are multiple jobholders in the SIPP. If a worker reports earnings from more than one job in a given month, I classify them as a multiple jobholder. It is possible this measure will capture transitions in work from one job to another and so I also include two other indicator variables for entrance into and exit from multiple jobholding.[7] It is not enough to just capture if respondents are working multiple jobs. Among the mechanisms outlined in the literature on motivations for multiple jobholding are hours constraints and financial need. I include measures of aggregate hours worked and earnings across all jobs per month, hours worked and earnings from a respondents primary job, and hours worked and earnings from the total of all non-primary sources of income.[8] Across these nine measures, I expect to be able to estimate how minimum wage policies not only impact multiple jobholding, but how that impact is distributed across primary and secondary sources of income.

TABLE 3.1: Descriptive Statistics

Statistic	Mean	St. Dev.	Min	Median	Max
Multiple Jobholding	0.057	0.232	0	0	1
Multiple Jobholding, Enter	0.004	0.063	0	0	1
Multiple Jobholding, Exit	0.003	0.059	0	0	1
Hours Worked (Month)	106.78	82.16	0	128	800§
Earnings (Month)	1,275.71	1,453.63	0	980	130,000
Hours (Primary)	102.47	76.56	0	128	236
Earnings (Primary)	1,229.12	1,392.75	0	924	130,000
Hours (Secondary)	4.31	21.95	0	0	600§
Earnings (Secondary)	46.59	331.45	0	0	23,080
Minimum Wage	7.73	0.68	7.25	7.25	11.50
Female	1.539	0.499	1	2	2
Age	44.37	17.1	15	44	90
Family Size	1.05	0.23	1	1	4

§This respondent reported working 200 hours in their primary job and an additional 600 hours across secondary sources of income, implying at least some overlap of these sources of income. This may reflect multiple on-call positions throughout the entire month.

3.6 Methodology

To estimate these effects requires a method designed to account for staggered treatment in a panel data set with the potential for heterogeneous treatment

[7]Multiple jobholding status also requires that the worker was either working multiple jobs in the month previously, or in the month that follows. This is to avoid classifying a single month job transition as a multiple jobholder.

[8]A workers primary job is identified as the job they spend the most hours working per month. In the event of a tie, the job they have held the longest is defined as the primary job.

effects, as the size of minimum wage increases differ, and the impact of minimum wage policies may vary.[9]

As minimum wage increases do vary in size and timing, using a two-way fixed effect estimator will yield a biased estimate (Sun and Abraham, 2020b; Goodman-Bacon, 2021; Athey and Imbens, 2021; Callaway and Sant'Anna, 2020; De Chaisemartin and D'Haultfœuille, 2020b). I leverage the estimator proposed by De Chaisemartin and D'Haultfœuille (2020a) to resolve this issue and preserve the difference-in-differences design. Before introducing De Chaisemartin and D'Haultfœuille (2020a)'s estimator, I briefly explore why the two-way fixed effect estimate is inappropriate.

The simplest difference-in-differences involves a group which receives some treatment and an alternate group which does not receive the treatment at that time, and had not received it previously, called a 2x2 DD by Goodman-Bacon (2021). In this simplest of designs, the treatment is not rolled out or staggered and the two groups are entirely disjoint from one another in terms of treatment. The issue with two-way fixed effect estimates comes up when considering treatments that are staggered and treatments which have heterogeneous treatment effects across units. (Sun and Abraham, 2020b; Goodman-Bacon, 2021)

The validity of the two-way fixed effect estimator ends up with a complicated set of requirements. Firstly, a new variance-weighted parallel trends assumption is introduced for each treatment group. The two-way fixed effect estimator is also shown to attribute larger weights to treatments in the middle of the data than early or late treatment groups. Even under the best of circumstances, a design with variation in treatment timing and heterogeneous treatment effects approaches the variance weighted average treatment on the treated, which is itself a product of panel length and estimator selection. (Cunningham, 2021a)

Many of the issues of the two-way fixed effect estimator are resolved by preventing already treated units from being involved in control groups. This motivates the group-time approaches of Callaway and Sant'Anna (2020) and Sun and Abraham (2020b), and the similar framing in De Chaisemartin and D'Haultfœuille (2020b) where they focus on "DID estimators comparing a group switching treatment between $t-1$ and t to a control group whose treatment does not change between $t-1$ and t, and with the same treatment as the switching group in $t-1$."(De Chaisemartin and D'Haultfœuille, 2020a).

Sun and Abraham (2020b) begins in a similar fashion to Goodman-Bacon (2021), as it addresses the issues with the two-way fixed effect estimator, with a focus on event study designs. Sun and Abraham (2020b) find that estimates' lead and lag terms are contaminated by other leads and lags except when we impose a number of additional assumptions. These include a restriction on heterogeneity, parallel trends, and limited anticipation of the treatment

[9] Different individual respondents may be more or less likely to experience the effects of minimum wage policies due to job characteristics and personal circumstances.

(Cunningham, 2021b). The contamination of these estimates are so great that tests for pre-trends can offer both false positive and false negative conclusions. After identifying these issues, Sun and Abraham (2020b) offer an alternative estimator using weights and group-time identities to calculate their cohort average treatment effect on the treatment group, called the CATT. This result ends up as a subset of the estimator proposed by Callaway and Sant'Anna (2020).

TABLE 3.2: Minimum Wage Increase's and Group Sizes Across SIPP Waves and Months

Treatment Group	2014	2015	2016	2017
January	$0.25	$0.20	$0.28	$0.69
	(1,904)	(2,020)	(2,530)	(2,107)
February	-	-	-	-
March	-	-	-	-
April	-	-	-	-
May	-	-	-	-
June	-	$0.50	$0.50	-
		(23)	(23)	
July	-	$1.00	$1.00	$0.53
		(759)	(118)	(117)
August	-	$0.75	$1.00	$0.50
		(160)	(159)	(161)
September	-	$0.75	-	-
		(409)		
October	-	-	-	-
November	-	-	$0.15	-
			(178)	
December	$0.75	$0.75	$0.25	$0.70
	(300)	(304)	(303)	(300)
Untreated	$0	$0	$0	$0
	(8,536)	(6,783)	(7,139)	(7,763)
Total	10,440	10,440	10,440	10,440
Never Treated	5,895	-	-	-

This table identifies the average minimum wage increase in each month and year covered by the 2014 SIPP, conditional on a minimum wage increase occurring. In parentheses beneath each average minimum wage is the count of respondents in that treatment group.

Similar to Sun and Abraham (2020b), Callaway and Sant'Anna (2020) estimate the group-time average treatment on the treated (ATT) from a set of unique ATT for each treated cohort of units, where cohorts are defined as receiving

treatment at the same time. Each treatment group receives an independent ATT defined as a comparison against units which have not yet received treatment or never do. Tables 3.2 and 3.3 highlight the distinctions between group treatment timing and the set of respondents that do not receive a minimum wage increase in the sample. The group-time treatment approach is helpful as it prevents treated units from ever being used as a "control," unlike in the two-way fixed effect model. Callaway and Sant'Anna (2020) extend from Abadie (2005) in the use of a propensity score to allow for a conditional parallel trends estimation. Through the use of a dynamic aggregation of treatment group ATT, an effective event study can also be constructed showing treatment effects in the post-treatment periods. This method resolves the event study issue raised, and also resolved by, Sun and Abraham (2020b).

The method proposed by De Chaisemartin and D'Haultfœuille (2020a) is an expansion on De Chaisemartin and D'Haultfœuille (2020b) and takes inspiration from the ideas proposed by Callaway and Sant'Anna (2020) and Sun and Abraham (2020b). This method is intended to be used in panel data where treatment groups are exposed to different doses of a common treatment at staggered times.[10] This methodology estimates the instantaneous and dynamic treatment effects through the use of a dynamic linear model with group specific and time-invariant effects in periods following treatment. These estimates are robust to heterogeneous effects from treatments which increase, decrease, and/or are non-binary. Controls can be included in this method as changes in the dependent variable are replaced with residuals, with respect to the selected controls. These are found using regressions of the change in the outcome on the change in the controls and time fixed effects. This allows for the estimate of the effect of the minimum wage on multiple jobholding in the first month a minimum wage increase occurs and in the months that follow.

Due to the structure of minimum wage policies, the method currently best suited to manage a dosage treatment with a dynamic post-treatment period is the De Chaisemartin and D'Haultfœuille (2020a) estimator. I create four estimates using this method across all nine dependent variables to better capture the effect of the minimum wage on multiple jobholding. I treat the minimum wage both as a binary indication of a minimum wage increase and as a binned continuous treatment.[11] To account for the potential violation of parallel trends and dynamic treatment effects in the months following a minimum wage increase, I include a 12 month post-treatment period and four pre-treatment placebo months. Effect estimates reported in the results section are then the aggregated difference between treated and control units after 12 months.

[10] Note, treatment groups are not necessarily equivalent to the group-time cohorts identified by Callaway and Sant'Anna (2020) and Sun and Abraham (2020b).

[11] The continuous treatment of the minimum wage is binned from \$7 to \$12 with jumps of 20 cents between each. This means that a minimum wage greater than or equal to \$7.00 and less than \$7.20 is assigned the binned minimum wage value of \$7.00. A treatment in this case is then any minimum wage increase that pushes the minimum wage to be greater than or equal to \$7.20.

For the purposes of this analysis I am including all four estimates, but I prefer the estimates when treating the minimum wage as a binned continuous treatment. The reason for including the alternative estimation is because it resembles the treatment design of alternative solutions to the two-way fixed effect problem, including Callaway and Sant'Anna (2020) and Sun and Abraham (2020b). These alternatives prove troublesome in this context though. As seen in Table 3.2, the minimum wage is increased multiple times for multiple groups throughout the panel. Each of these increases could be of different sizes and result in different nominal levels of the minimum wage. If instead of estimating off of the continuous minimum wage I used the first treatment period, as required by Callaway and Sant'Anna (2020) and Sun and Abraham (2020b), my treatment variable is of the form shown in Table 3.3. Not only do I lose variation, but 1,904 observations are treated in the first month of the sample. These units would offer no functional variation for either the Callaway and Sant'Anna (2020) or Sun and Abraham (2020b) estimator.

TABLE 3.3: Treatment Group Size Across Waves

Treatment Group Month of Panel	Count	Average Minimum Wage Post-Treatment	Average Increase
1	1,904	\$7.76	\$0.25
13	430	\$8.836	\$0.46
18	23	\$7.756	\$0.50
19	750	\$9.006	\$1.00
20	160	\$8.006	\$0.75
21	406	\$8.156	\$0.75
25	720	\$7.95	\$0.55
31	108	\$8.25	\$1.00
35	1	\$7.65	\$0.15
37	43	\$8.35	\$1.05
Never Treated	5,895	\$7.35	\$0

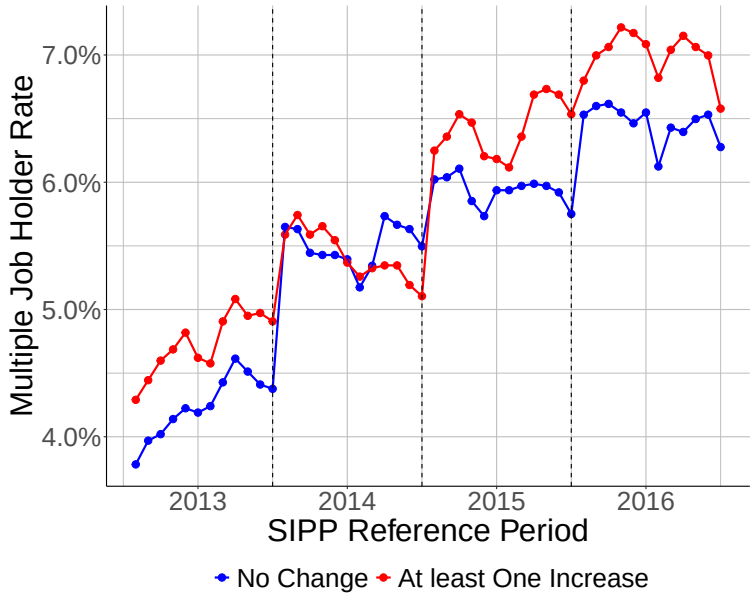
This table identifies the first minimum wage increase each respondent receives between 2013 and 2016. When using the minimum wage increase treatment, these months would be coded as the treatment month for each group. This table illustrates the missing variation when using the minimum wage increase indicator variable as the treatment, as would be required by Callaway and Sant'Anna (2020) and Sun and Abraham (2020b).

3.7 Results

Figure 3.6 plots the multiple jobholder rate among respondents who receive at least one minimum wage increase between 2013 and 2016 and those who do not. Surprisingly, we can see a reported increase in multiple jobholding across both groups in contrast to the measures reported by the CPS, who estimated a somewhat stable level of multiple jobholding, with a range of 4.7% and

5.2% from 2013 to 2016 (U.S. Bureau of Labor Statistics, 2021). It is possible this is a factor of subsetting the SIPP to workers who earned less than $19 on average when engaging in paid work. One pattern that becomes clear is the bump in multiple jobholding at the start of each year. Each year begins the new wave of data in the SIPP and it appears that some degree of updating is occurring with respect to employment. The data used for this plot is balanced at the individual-month level, so this is not a selection effect across waves. This implies that some data in the monthly installments may be unreliable for this analysis, and a 12-month window is necessary after any minimum wage increase occurs to insure that this yearly update is captured.[12]

FIGURE 3.6: Trends in Multiple Jobholding by Treatment Status



This figure plots the survey weighted multiple jobholder rate between individuals who do and do not experience a minimum wage increase from 2013 to 2016. Figure C.1 shows the count of SIPP respondents and their entrance into or out of multiple jobholding.

As shown in Table 3.4, I treat the minimum wage both as a binary indication of a minimum wage increase and as a binned continuous treatment. The estimates in this table show the aggregate difference across a 12 month post-treatment period. The results from this table are shown in their event study format in Figures C.3 through C.10.

The two treatments used in Table 3.4 are similar, but they do capture slightly different group treatment effects. When estimating the treatment effect of a minimum wage with the binned treatment, I am estimating the effect of increasing the minimum wage by $1. This method allows for treated units which

[12]This could be explained by recency bias as a result of the 2014 SIPP's utilization of an event history calendar to develop spell and month level data (Kempf-Leonard, 2004; Bureau, 2016).

TABLE 3.4: De Chaisemartin and D'Haultfœuille (2020a) Dynamic Estimates

Min Wage – Treatment	Binary Unconditioned	Binned Unconditioned	Binary Conditioned	Binned Conditioned
Multiple Jobholding	0.0914***	0.0197	0.0916***	0.0199
	(0.0301)	(0.0423)	(0.0341)	(0.0343)
Multiple Jobholding, Enter	0.0393***	0.0059***	0.0396***	0.0061***
	(0.0126)	(0.0020)	(0.0104)	(0.0023)
Multiple Jobholding, Exit	-0.0129	-0.0023	-0.0128	-0.0027
	(0.0107)	(0.0095)	(0.0150)	(0.0081)
Hours Worked (Month)	17.02	-2.06	16.90*	-2.57
	(11.89)	(4.15)	(9.34)	(5.84)
Earnings (Month)	412.51**	-8.68	411.87**	3.32
	(160.22)	(99.60)	(185.59)	(87.73)
Hours (Primary)	14.65	-1.63	14.53	-2.16
	(10.92)	(7.96)	(9.46)	(7.53)
Earnings (Primary)	333.92**	12.04	333.31*	23.87
	(158.61)	(63.17)	(202.09)	(68.16)
Hours (Secondary)	2.37	-0.43	2.37	-0.41
	(2.29)	(2.85)	(3.42)	(2.75)
Earnings (Secondary)	78.59	-20.72	78.56	-20.55
	(53.45)	(24.10)	(58.63)	(32.30)

*p<0.1; **p<0.05; ***p<0.01

The significance level on the De Chaisemartin and D'Haultfœuille (2020b) results use the covariances between all the pairs of instantaneous and dynamic effects, and between all the pairs of placebos. The dependent variables on the left side of the table are broken into groups. The total group are for all jobs reported by the individual respondent. The primary group is only for the identified primary source of work. The other group captures the sum of all reported non-primary sources of work.

receive multiple minimum wage increases across the panel to be compared so long as a control unit can be identified.[13]

This approach differs from the treatment of a minimum wage increase, which is an indicator variable for if a unit receives a minimum wage increase at any point during the panel, and when the increase occurs. Once treated, a unit cannot be re-evaluated for subsequent minimum wage increases. As shown in Table 3.3, the minimum wage increase treatment also misses out on a substantial amount of variation in the minimum wage. These distinctions can be seen to have a real impact on the estimated effect of a minimum wage on the multiple jobholder rate across the two treatments. When considering a minimum wage increase, the probability of a respondent holding multiple jobs in the same month increases by 9.1% over a 12 month period. This result stems from a significant increase in the take-up of multiple jobs, as opposed to a significant reduction in individuals leaving multiple jobs. The probability that a respondent reports starting a new job, while already working in their current one, and maintaining both for at least one addition month, in the 12 months that follow a minimum wage increase rises by 3.9%.

Using the minimum wage increase treatment also identifies a significant increase in earnings in aggregate. This is a result of an increase in earnings

[13]Control units are defined as respondents with a binned minimum wage equal to the treated units binned minimum wage at time $t-1$. If they have a minimum wage exactly equal to each other, than the binned minimum wage will not be used and instead it is a comparison of the nominal minimum wages.

among primary sources of income, but no significant effect is found for secondary sources of income. The first minimum wage increase in the panel boosts earnings by ~$412 over the course of 12 months, or ~$34.33 per month. Once conditioned on the age, sex, and family size of the respondent, the number of hours worked are also marginally significant at the 10% level, implying an additional 16.9 hours worked across the 12 month period after the first minimum wage increase in the panel.

These results differ from the binned minimum wage treatment, which does not show a statistically significant increase in the aggregate multiple job holder rate, but it does show an increase in the take up of multiple jobholding in the 12 months that follow a minimum wage increase. A $1 increase in the minimum wage is shown to increase the probability of taking up multiple jobs over a 12 month period post treatment by 0.6%.[14] I find no evidence of a statistically significant effect of increasing the minimum wage on hours worked or earnings across primary or secondary sources of income.

It is possible that the difference in results here is being driven by a selection effect in who is being treated, since the binary treatment is unable to capture those 1,904 respondents treated in the first period, which represent 18.2% of my sample and 41.9% of my treated sample. It could also be that the effect of repeated minimum wage increases declines. So estimating the repeated increases of the minimum wage is driving down the effect estimate as captured by the first increase. Across all four years, only 120 respondents experience a single minimum wage increase, while 4,425 respondents experienced more than one. As such, the results from the binned minimum wage treatment are better suited to capture the true effect of increases in the minimum wage between 2013 and 2016, with the understanding that it should be interpreted as a minimum wage increase in the context of a wave of increases.

3.8 Conclusion

The expansion in efforts to support state and local minimum wages throughout the 2010s began with the "Fight for $15" movement. As efforts to support living wages, and decrease the financial burden on low-wage workers grew, attention to multiple jobholding increased as well. Efforts to ensure a minimum standard of living cannot leave multiple jobholding as a blind spot, but minimum wages do not present a clear solution.

The reasons for individuals to take up multiple jobs at once are varied and require different policy solutions than simply raising the price floor of labor, and some require no solution at all. For workers pressured into multiple jobholding by hours constraints and financial need, a minimum wage could be a blessing or a curse. The findings in this chapter fall in line with those from Robinson and Wadsworth (2007), who found little impact of the minimum

[14]The average increase in the minimum wage in the sample is ~$0.46, conditional on an increase having occurred.

wage on the holding of a second job after the national minimum wage was introduced.

I use the 2014 Survey of Income and Program Participation to explore the effect of the minimum wage on multiple jobholding. Due to substantial noise in the outcome, I cannot say definitively if minimum wages do or do not impact the aggregate rate of multiple jobholding. They do result in a small significant increase in the take up of multiple jobholding in the 12 months that follow a minimum wage increase, but this is lost in the noise of the aggregate effect. A $1 increase in the minimum wage increases the probability of taking up multiple jobs over a 12 month period by 0.6%. I find no evidence of a statistically significant effect of the minimum wage on hours worked or earnings across either primary or secondary sources of income.

These results contribute to the growing literature on the effects of the minimum wage on outcomes other than aggregate employment. While policy makers may be interested in estimates of the aggregate effect of minimum wage increases, it is also important to understand what aspects of the labor market are not impacted. Modest, frequent, minimum wage increases are unlikely to significantly increase, or reduce, multiple jobholding.

This analysis could be expanded by exploring how alternative policies may better support multiple jobholders. Alternative policies worth studying in the future include stable scheduling legislation, paid sick leave, and universal pre-K. These policies offer a more stable work schedule from week-to-week, a guarantee to take time off for medical needs, and reduced hours constraints due to household needs respectively.

Appendix A

Appendix for Chapter 1

A.1 Tables

Uber Active Year	Counties	Nonemp. Estab.	Nonemp. Estab./ Labor Force	Total Receipts	Average Receipts	HHI	Labor Force
Not Active by 2018	1,448	1,160	0.14	47,225	41,191	797	8,503
2012	123	17,076	0.15	834,068	46,031	639	133,063
2013	74	11,056	0.14	474,086	45,782	656	76,498
2014	412	5,778	0.14	255,071	41,968	670	47,612
2015	151	4,658	0.13	187,846	39,905	675	31,568
2016	249	2,078	0.14	89,303	40,483	753	14,035
2017	384	1,538	0.14	65,580	40,589	765	10,792
2018	167	1,583	0.15	57,323	37,888	807	9,500

TABLE A.1: This table presents the median values for each of the listed variables in the year before Uber is active in that county. For counties in which Uber is not active by 2018, the median is calculated across the full panel.

TABLE A.2: Local Minimum Wage Increases

st - cty	Combined Statistical Area	Year	HHI Quantile	Uber Year
35-49	Albuquerque-Santa Fe-Las Vegas, NM	2004	38	2015
6-75	San Jose-San Francisco-Oakland, CA	2005	3	2012
35-1	Albuquerque-Santa Fe-Las Vegas, NM	2007	18	2014
6-1	San Jose-San Francisco-Oakland, CA	2014	7	2012
6-85	San Jose-San Francisco-Oakland, CA	2014	1	2012
24-31	Washington-Baltimore-Arlington, DC-MD-VA-WV-PA	2014	4	2012
24-33	Washington-Baltimore-Arlington, DC-MD-VA-WV-PA	2014	13	2012
53-33	Seattle-Tacoma, WA	2014	2	2012
6-13	San Jose-San Francisco-Oakland, CA	2015	11	2012
6-73	Not in a CSA	2015	5	2012
17-31	Chicago-Naperville, IL-IN-WI	2015	1	2012
19-103	Cedar Rapids-Iowa City, IA	2015	3	2014
21-111	Louisville/Jefferson County-Elizabethtown-Madison, KY-IN	2015	6	2014
35-13	El Paso-Las Cruces, TX-NM	2015	40	2015
21-67	Lexington-Fayette-Richmond-Frankfort, KY	2016	7	2014
23-5	Portland-Lewiston-South Portland, ME	2016	11	2014
6-67	Sacramento-Roseville, CA	2017	10	2013
23-19	Not in a CSA	2017	36	2015
53-53	Seattle-Tacoma, WA	2017	10	2014

TABLE A.3: Interacted Two-Way Fixed Effect Models, 2010 to 2018

All Industries	*Dependent variables:*			
	Estab. (TW FE Poisson)	Log(Estab.) (TW FE OLS)	Estab./Labor Force (TW FE OLS)	Log(Avg. Receipts) (TW FE OLS)
Log(Min. Wage(MW))	-0.1422**	-0.1125**	-0.0046	0.0421
	(0.0621)	(0.0571)	(0.0074)	(0.0302)
MW∗UA	0.1912***	0.1827***	0.0133	-0.0697**
	(0.0578)	(0.0523)	(0.0100)	(0.0269)
MW∗HHI	0.0036***	0.0032***	6.55e-05	-0.0004
	(0.0011)	(0.0010)	(0.0001)	(0.0006)
MW∗PD	1.18e-06***	1.12e-06***	4.94e-08	5.19e-08
	(4.00e-07)	(3.71e-07)	(4.33e-08)	(2.37e-07)
MW∗UA∗HHI	-0.0071***	-0.0069***	-0.0006***	0.0018***
	(0.0012)	(0.0011)	(0.0002)	(0.0006)
MW∗UA∗PD	-1.49e-06***	-1.49e-06***	-8.71e-08**	2.54e-07
	(3.80e-07)	(3.34e-07)	(4.37e-08)	(1.77e-07)
MW∗HHI∗PD	-1.15e-07	-7.83e-08	1.50e-08	4.53e-08
	(1.01e-07)	(9.08e-08)	(1.45e-08)	(7.27e-08)
MW∗UA∗HHI∗PD	1.64e-07*	1.47e-07*	7.03e-09	-3.55e-08
	(9.45e-08)	(8.22e-08)	(1.49e-08)	(6.96e-08)
Uber Active (UA)	0.0321***	0.0344**	0.0021***	-0.0135***
	(0.0045)	(0.0040)	(0.0006)	(0.0026)
HHI Quantile (HHI)	3.19e-05	3.12e-05	2.87e-05***	-0.0003***
	(9.32e-05)	(8.27e-05)	(9.28e-06)	(0.0001)
Pop. Density (PD)	7.21e-06***	8.24e-06***	3.40e-07**	-1.69e-06**
	(1.62e-06)	(1.64e-06)	(1.57e-07)	(6.88e-07)
UA∗HHI	-0.0009***	-0.0010***	-0.0001***	0.0005***
	(0.0001)	(0.0001)	(0.00002)	(0.0001)
UA∗PD	-1.60e-07***	-1.80e-07***	-1.16e-08***	6.02e-08***
	(2.93e-08)	(2.92e-08)	(3.32e-09)	(1.55e-08)
HHI∗PD	-9.58e-09	-7.22e-09	-2.47e-10	1.61e-08*
	(9.13e-09)	(8.99e-09)	(1.36e-09)	(9.23e-09)
UA∗HHI∗PD	2.43e-08***	2.58e-08***	4.59e-09***	-1.23e-08***
	(7.18e-09)	(6.31e-09)	(8.69e-10)	(4.18e-09)
County FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Observations	26,964	26,964	26,964	26,964
R^2 *Within*	-	0.224	0.092	0.039
R^2	0.999	0.999	0.978	0.967
Adjusted R^2	-	0.999	0.975	0.963

*p<0.1; **p<0.05; ***p<0.01

The minimum wage used is the demeaned inflation adjusted minimum wage in 2016 dollars. Standard errors are clustered at the county level and the two-way fixed effect results using OLS are weighted by the counties average population from 2000 to 2018.

Uber Active (UA) is a dummy variable for if a county has Uber active at any point in the year.

HHI Quantile (HHI) is the quantile from 0 to 100 of the counties HHI using the CBP estimate of the covered labor market concentration, with lower quantiles meaning less labor market concentration.

Pop. Density (PD) is an estimate of the county level population density, measured as the number of people per square kilometer.

TABLE A.4: Interacted Two-Way Fixed Effect Models, 2010 to 2018

	Dependent variables:			
Trans./Ware.	Estab. (TW FE Poisson)	Log(Estab.) (TW FE OLS)	Estab./Labor Force (TW FE OLS)	Log(Avg. Receipts) (TW FE OLS)
Log(Min. Wage(MW))	-0.8582**	-0.5146*	-0.0035	0.3499**
	(0.3812)	(0.2862)	(0.0035)	(0.1423)
MW∗UA	1.6680	1.5674***	0.0186***	-0.7725***
	(0.3306)	(0.2574)	(0.0042)	(0.1616)
MW∗HHI	0.0247***	0.0181***	0.0001**	-0.0101***
	(0.0063)	(0.0050)	(5.84e-05)	(0.0026)
MW∗PD	2.11e-06*	2.96e-06**	3.02e-08	-9.17e-07
	(1.21e-06)	(1.34e-06)	(3.98e-08)	(7.87e-07)
MW∗UA∗HHI	-0.0395	-0.0386***	-0.0005***	0.0142***
	(0.0062)	(0.0053)	(0.0001)	(0.0036)
MW∗UA∗PD	-3.79e-06***	-4.96e-06***	-2.94e-08	2.23e-06***
	(1.19e-06)	(1.25e-06)	(4.19e-08)	(6.98e-07)
MW∗HHI∗PD	-7.00e-08	-1.56e-07	7.67e-09	2.26e-07
	(5.67e-07)	(4.46e-07)	(7.90e-09)	(3.02e-07)
MW∗UA∗HHI∗PD	4.29e-07	5.53e-07	-1.75e-09	-3.60e-07
	(5.39e-07)	(3.91e-07)	(8.38e-09)	(3.10e-07)
Uber Active (UA)	0.1773***	0.1971***	0.0010***	-0.0850***
	(0.0266)	(0.0205)	(0.0003)	(0.0130)
HHI Quantile (HHI)	0.0011**	0.0006	-1.44e-06	3.93e-05
	(0.0004)	(0.0004)	(5.85e-06)	(0.0003)
UA∗HHI	-0.0081***	-0.0076***	8.38e-05***	0.0030***
	(0.0005)	(0.0005)	(6.80e-06)	(0.0003)
Pop. Density (PD)	1.91e-05***	2.78e-05***	2.78e-07**	-7.63e-06***
	(3.82e-06)	(5.11e-06)	(1.15e-07)	(2.30e-06)
UA∗PD	-5.11e-07***	-6.48e-07***	-6.27e-09***	2.25e-07***
	(6.95e-08)	(8.12e-08)	(2.07e-09)	(5.16e-08)
HHI∗PD	-6.84e-08*	-1.00e-08	3.13e-10	7.99e-09
	(3.94e-08)	(4.47e-08)	(9.42e-10)	(2.59e-08)
UA∗HHI∗PD	1.33e-07***	1.28e-07***	2.02e-09***	-5.72e-08***
	(3.61e-08)	(2.48e-08)	(4.19e-10)	(1.62e-08)
County FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Observations	25,182	25,182	25,182	25,182
R^2 *Within*	-	0.350	0.325	0.160
R^2	0.991	0.994	0.902	0.898
Adjusted R^2	-	0.993	0.889	0.886

*p<0.1; **p<0.05; ***p<0.01

The minimum wage used is the demeaned inflation adjusted minimum wage in 2016 dollars. Standard errors are clustered at the county level and the two-way fixed effect results using OLS are weighted by the counties average population from 2000 to 2018.

Uber Active (UA) is a dummy variable for if a county has Uber active at any point in the year.

HHI Quantile (HHI) is the quantile from 0 to 100 of the counties HHI using the CBP estimate of the covered labor market concentration, with lower quantiles meaning less labor market concentration.

Pop. Density (PD) is an estimate of the county level population density, measured as the number of people per square kilometer.

TABLE A.5: Cumulative Treatment Effects: Combined Statistical Area Level

All Industries		*Dependent variable:*		
Method	Treatment	Log(Estab.)	Estab./Labor Force	Log(Avg. Receipts)
DIDM†	Nominal	0.3300	0.0777	-0.4547*
2000 to 2006	Log(Min. Wage)	(0.1938)	(0.0474)	(0.2653)
DIDM†	Nominal	0.0521	0.0143**	0.0101
2010 to 2018	Log(Min. Wage)	(0.0411)	(0.0070)	(0.0291)
DIDM†	Min. Wage	-0.0070	0.0015	-0.0035
	(Local)	(0.0114)	(0.0020)	(0.0165)
Transport. and Ware.		*Dependent variable:*		
Method	Treatment	Log(Estab.)	Estab./Labor Force	Log(Avg. Receipts)
DIDM†	Nominal	-0.3994	-0.0001	0.2929
2000 to 2006	Log(Min. Wage)	(0.2227)	(0.0010)	(0.4824)
DIDM†	Nominal	0.7294***	0.0137***	-0.1459
2010 to 2018	Log(Min. Wage)	(0.2566)	(0.0038)	(0.1763)
DIDM†	Min. Wage	0.0289	0.0019	0.0036
	(Local)	(0.0836)	(0.0012)	(0.0479)
All Except Transport. and Ware.		*Dependent variable:*		
Method	Treatment	Log(Estab.)	Estab./Labor Force	Log(Avg. Receipts)
DIDM†	Nominal	0.3604	0.0778	-0.4942
2000 to 2006	Log(Min. Wage)	(0.2170)	(0.0478)	(0.3217)
DIDM†	Nominal	-0.0163	0.0037	0.0422
2010 to 2018	Log(Min. Wage)	(0.0483)	(0.0052)	(0.0258)
DIDM†	Min. Wage	-0.0199	-0.0004	0.0079
	(Local)	(0.0087)	(0.0018)	(0.0187)

*p<0.1; **p<0.05; ***p<0.01

This table uses the data at the Combined Statistical Area level in accordance with the counties identified as CSAs in 2017. The group of counties is constant across the whole panel. The DIDM estimate is run with 1000 bootstraps and is performed conditional on the CSAs HHI, population density, and total population. Effect estimates are weighted by the average population of the CSA within the sample. †The De Chaisemartin and D'Haultfœuille (2020b) method identifies treatment switchers as those whose nominal minimum wage increases in comparison to the previous period. The increase is treated as an increase in the level of a continuous treatment, and accounts for the difference in size of the first increase. It does not capture subsequent increases in the switching status. To account for the lack in subsequent increases, both the pre-2007 sample and the post-2009 sample include dynamic treatment effects for the maximum duration, when also accounting for pre-treatment periods, following De Chaisemartin and D'Haultfœuille (2020a), and the value presented is the cumulative effect for the specified post-treatment periods. This exact coding can be found in replication files.

TABLE A.6: Cumulative Treatment Effects: Combined Statistical Area Level Without Population Weights

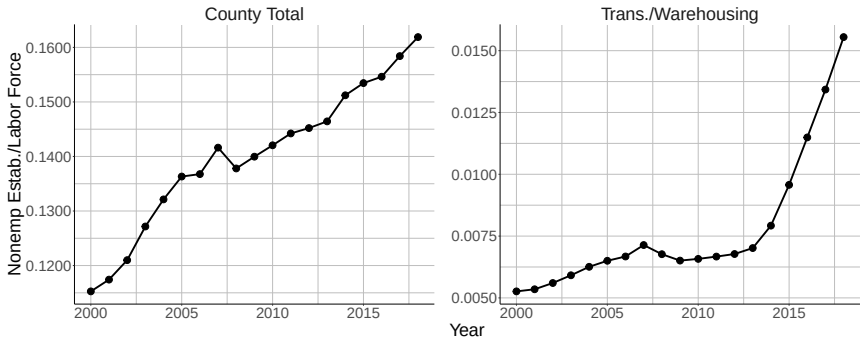
All Industries		*Dependent variable:*		
Method	Treatment	Log(Estab.)	Estab./Labor Force	Log(Avg. Receipts)
DIDM†	Nominal	-0.0209	0.0050	-0.1359**
2000 to 2006	Log(Min. Wage)	(0.0500)	(0.0106)	(0.0648)
DIDM†	Nominal	0.0034	0.0010	0.0270
2010 to 2018	Log(Min. Wage)	(0.0333)	(0.0050)	(0.0344)
DIDM†	Min. Wage	0.0063	0.0018	0.0074
	(Local)	(0.0116)	(0.0016)	(0.0145)
Transport. and Ware.		*Dependent variable:*		
Method	Treatment	Log(Estab.)	Estab./Labor Force	Log(Avg. Receipts)
DIDM†	Nominal	-0.1893	-0.0008	-0.2074
2000 to 2006	Log(Min. Wage)	(0.1076)	(0.0005)	(0.1881)
DIDM†	Nominal	0.3450**	0.0038**	-0.1604
2010 to 2018	Log(Min. Wage)	(0.1486)	(0.0015)	(0.1193)
DIDM†	Min. Wage	0.1135*	0.0017	-0.0310
	(Local)	(0.0614)	(0.0011)	(0.0442)
All Except Transport. and Ware.		*Dependent variable:*		
Method	Treatment	Log(Estab.)	Estab./Labor Force	Log(Avg. Receipts)
DIDM†	Nominal	-0.0136	0.0058	-0.1362*
2000 to 2006	Log(Min. Wage)	(0.0550)	(0.0113)	(0.0805)
DIDM†	Nominal	-0.0203	-0.0016	0.0502
2010 to 2018	Log(Min. Wage)	(0.0349)	(0.0043)	(0.0325)
DIDM†	Min. Wage	-0.0059	0.0002	0.0190
	(Local)	(0.0107)	(0.0014)	(0.0174)

*p<0.1; **p<0.05; ***p<0.01

This table uses the data at the Combined Statistical Area level in accordance with the counties identified as CSAs in 2017. The group of counties is constant across the whole panel. The DIDM estimate is run with 1000 bootstraps and is performed conditional on the CSAs HHI, population density, and total population.
†The De Chaisemartin and D'Haultfœuille (2020b) method identifies treatment switchers as those whose nominal minimum wage increases in comparison to the previous period. The increase is treated as an increase in the level of a continuous treatment, and accounts for the difference in size of the first increase. It does not capture subsequent increases in the switching status. To account for the lack in subsequent increases, both the pre-2007 sample and the post-2009 sample include dynamic treatment effects for the maximum duration, when also accounting for pre-treatment periods, following De Chaisemartin and D'Haultfœuille (2020a), and the value presented is the cumulative effect for the specified post-treatment periods. This exact coding can be found in replication files.

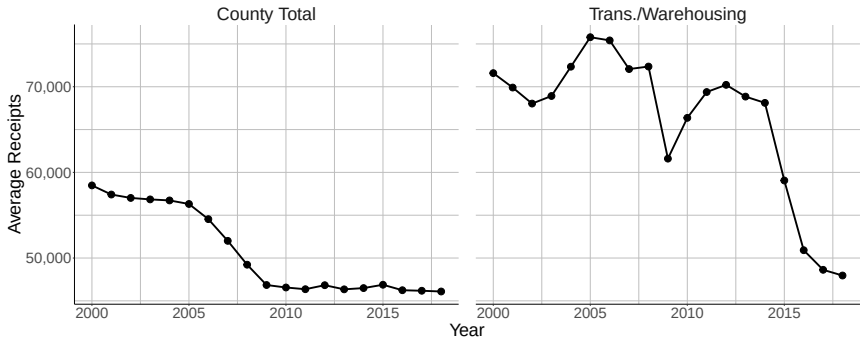
A.2 Figures

FIGURE A.1: Trends in Nonemployer Establishments



These figures show the trend in nonemployer establishments for the aggregated total of all nonemployer establishments on the left and for transportation and warehousing services (NAICS 48-49) on the right.

FIGURE A.2: Trends in Nonemployer Establishments



These figures show the trend in the average receipts of nonemployer establishments for the aggregated total of all nonemployer establishments on the left and for transportation and warehousing services (NAICS 48-49) on the right.

FIGURE A.3: Bivariate Relationship between the Minimum Wage and Nonemployer Establishments with Outliers

These figures show both the number of nonemployer establishments (top) and average receipts of nonemployer establishments (bottom) relationship with the real minimum wage. Figures on the left are for aggregate of all industries, and figures on the right are for transportation and warehousing services. Green points are county-year observations where Uber is active, and black points are county-year observations where Uber is not active. The fitted green line is the linear relationship among counties with Uber active, and the red line is the linear relationship among counties where it is not.

FIGURE A.4: Uber Expansion Map

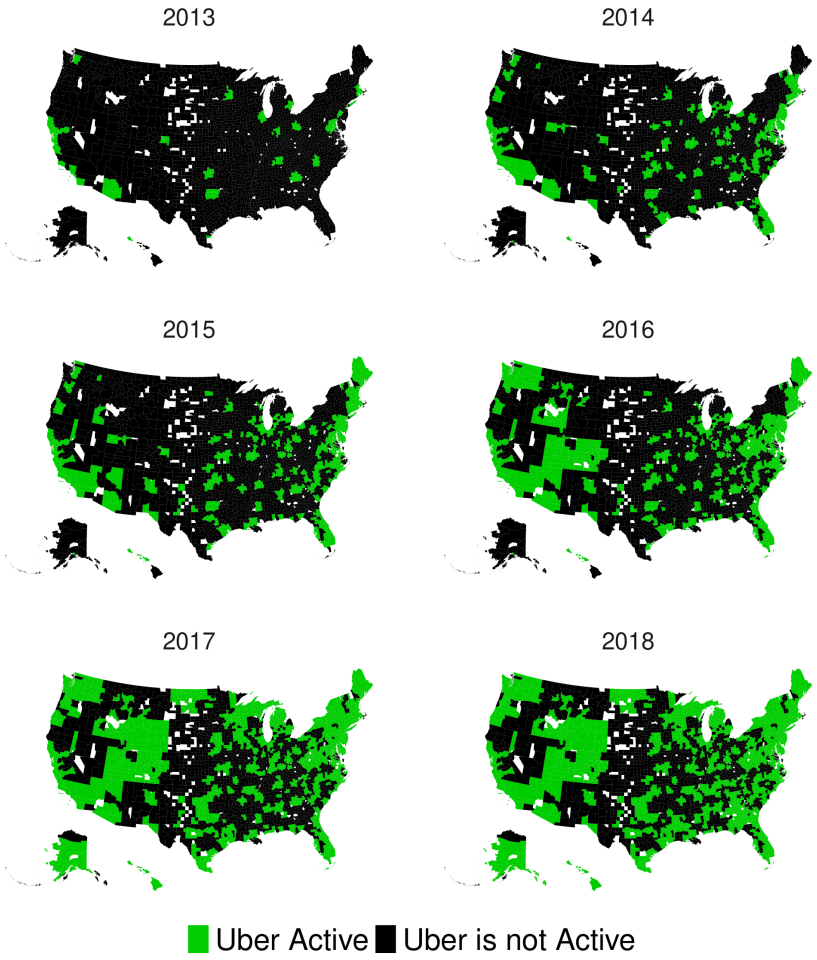
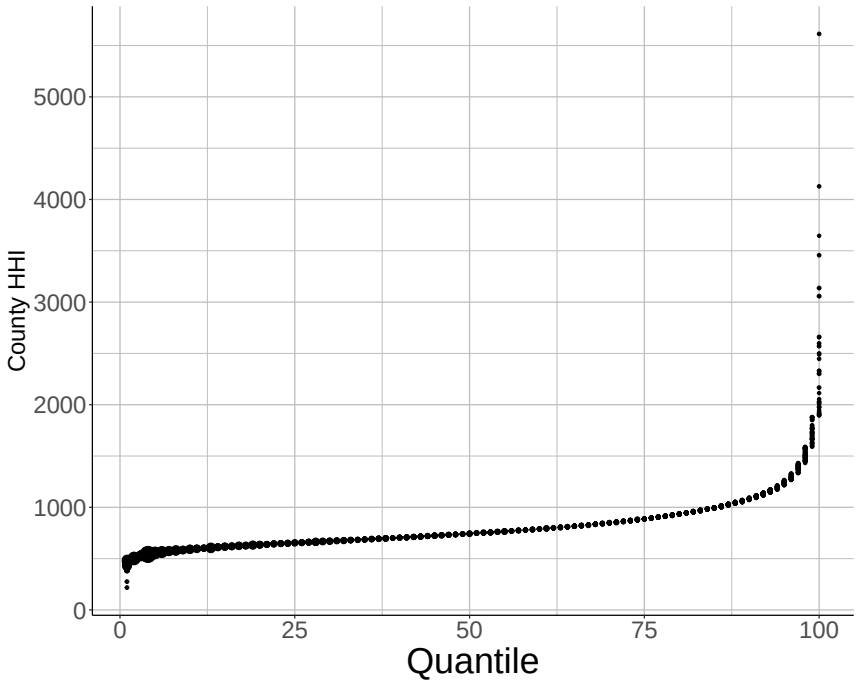


FIGURE A.5: The figures above depict the counties in which Uber is operating from 2013-2018. Black counties are areas without Uber, green counties are where Uber is active, and white counties are counties which are structural zeros and are dropped from the analysis. White counties are not in the balanced panel, but all black and green counties are.

FIGURE A.6: Quantile Measure of the Herfindahl-Hirschman Index (HHI)



This figure shows the distribution of HHI values as they are binned into 100 quantiles. The bulk of the quantile trend is linear with some extreme low HHI scores falling into the first quantile and extreme high falling into the last quantile. This highlights the advantage to using the quantile based measure of HHI as appose to the raw continuous value of HHI when including a linear interaction between HHI and the change in the minimum wage.

FIGURE A.7: Herfindahl-Hirschman Index (HHI) and County Labor Force

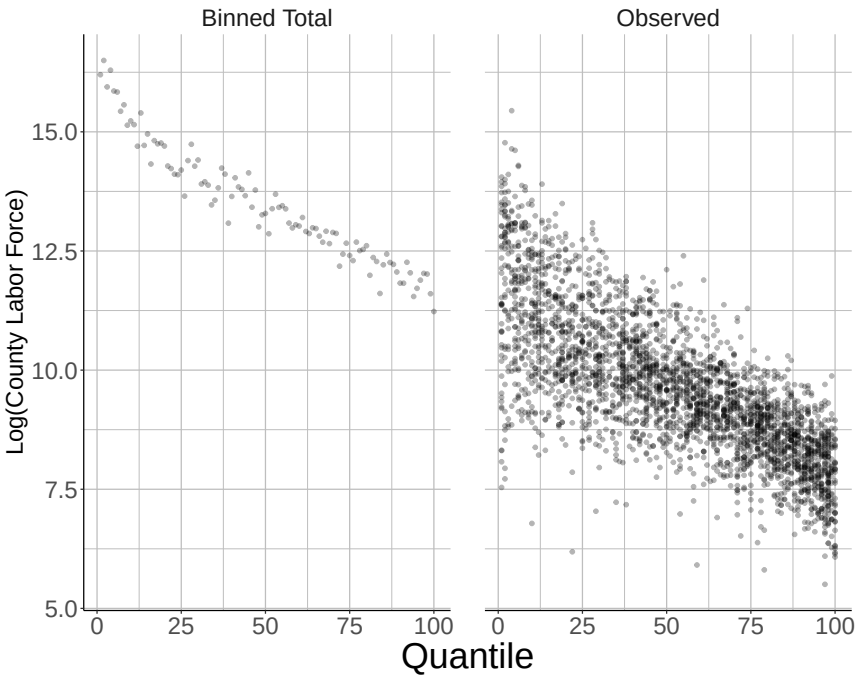


FIGURE A.8: This figure plots the relationship between the log of the county labor force and the HHI quantile for both the binned sum of the labor force and the observed labor force in each county in 2018.

FIGURE A.9: Geographic Distribution of HHI Quantile

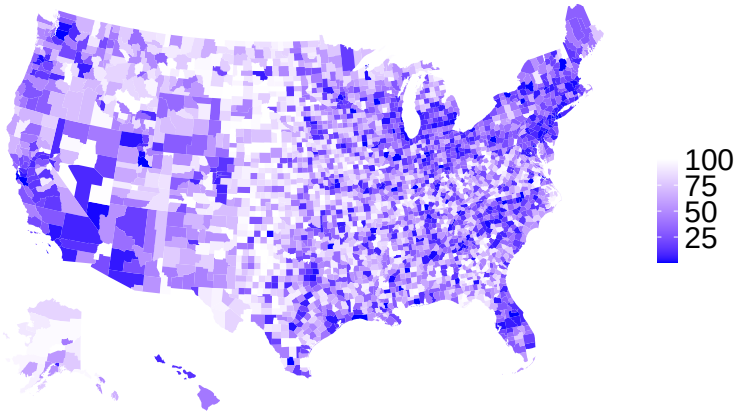


FIGURE A.10: Geographic Distribution of Log(Labor Force)

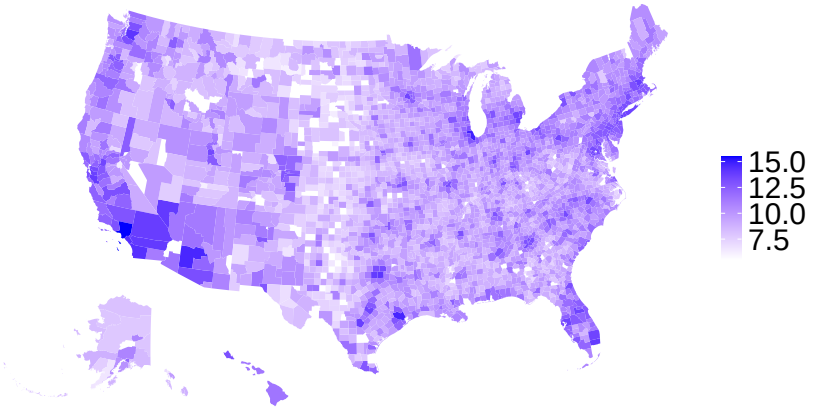
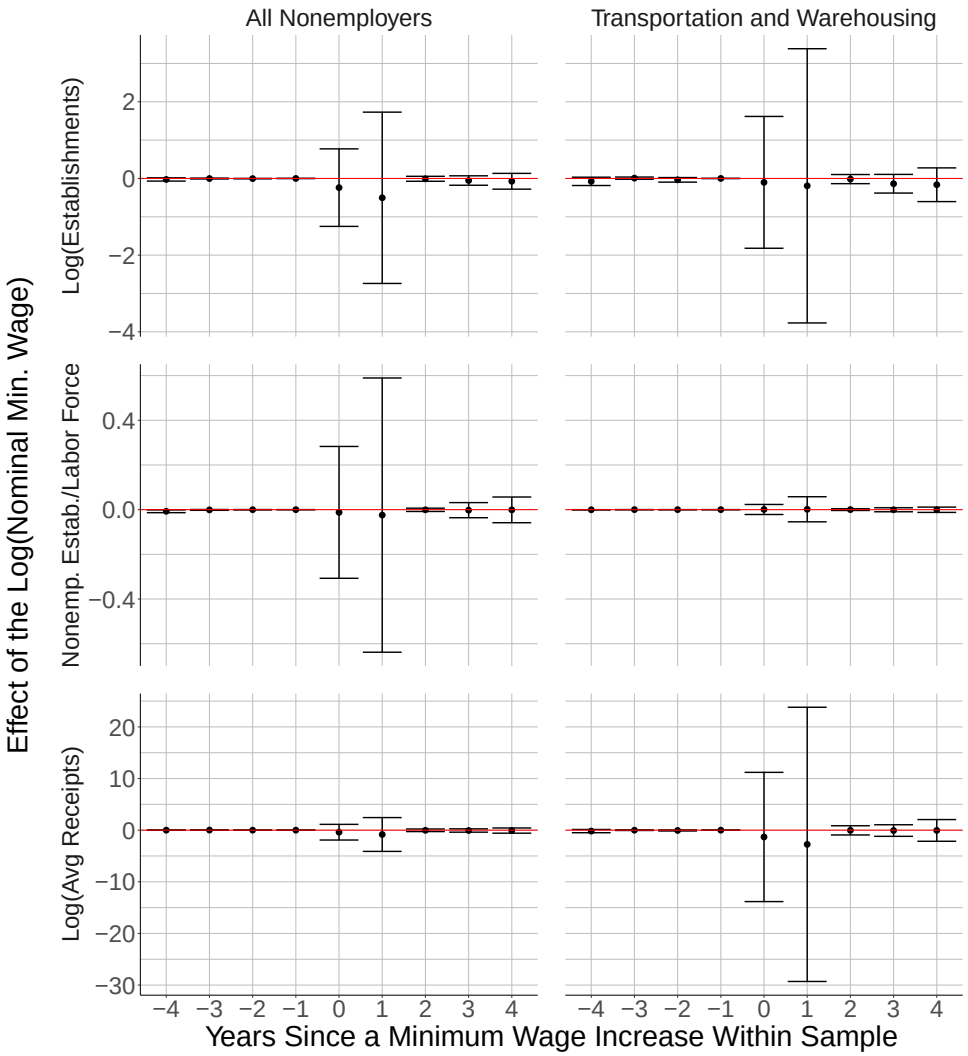
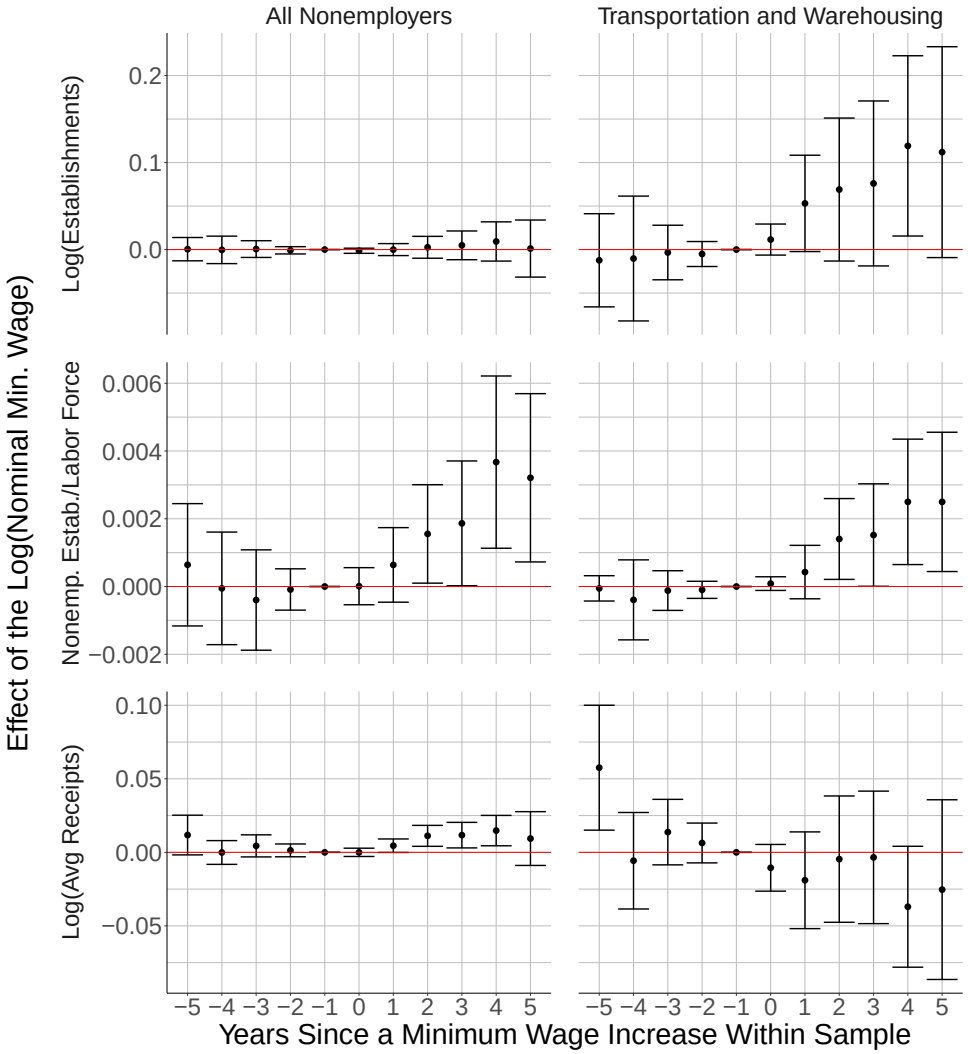


FIGURE A.11: Event Study for De Chaisemartin and D'Haultfœuille (2020b):



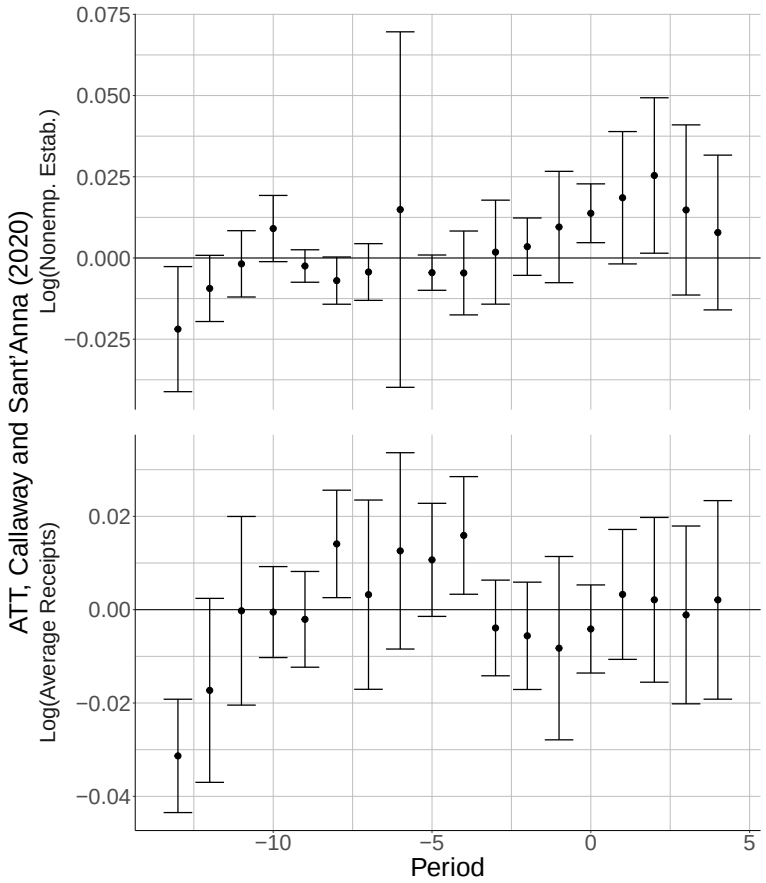
These figures show the event study for the De Chaisemartin and D'Haultfœuille (2020b) methodology for the sample from 2000 to 2006. Bars show a 95% confidence interval around the point estimate.

FIGURE A.12: Event Study for De Chaisemartin and D'Haultfœuille (2020b):



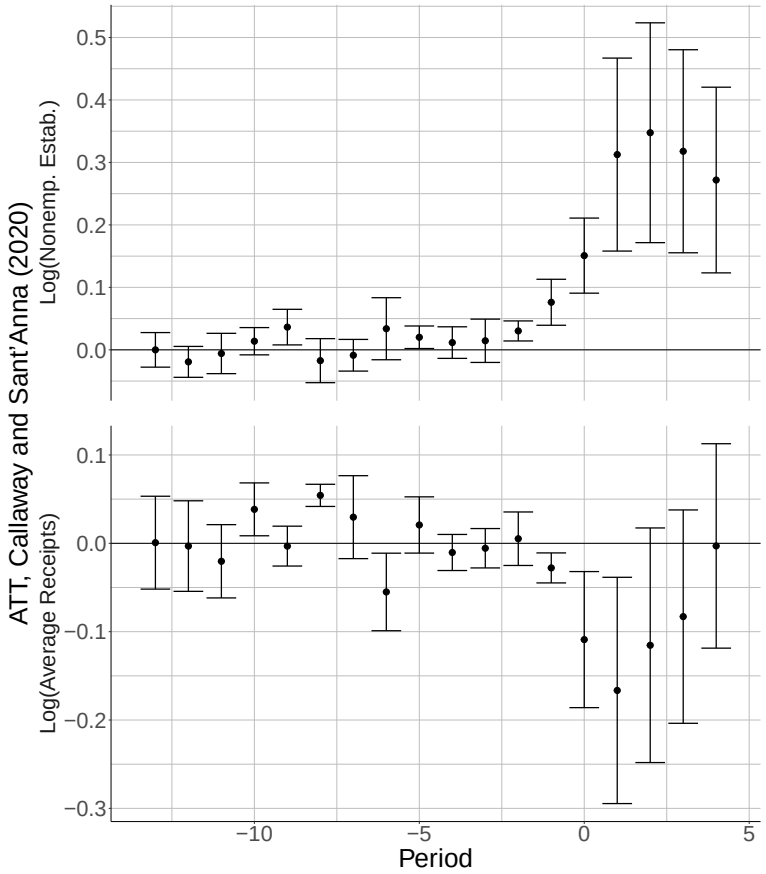
These figures show the event study for the De Chaisemartin and D'Haultfœuille (2020b) methodology for the sample from 2010 to 2018. Bars show a 95% confidence interval around the point estimate.

FIGURE A.13: Event Study Using Callaway and Sant'Anna (2020): All Nonemployer Establishments



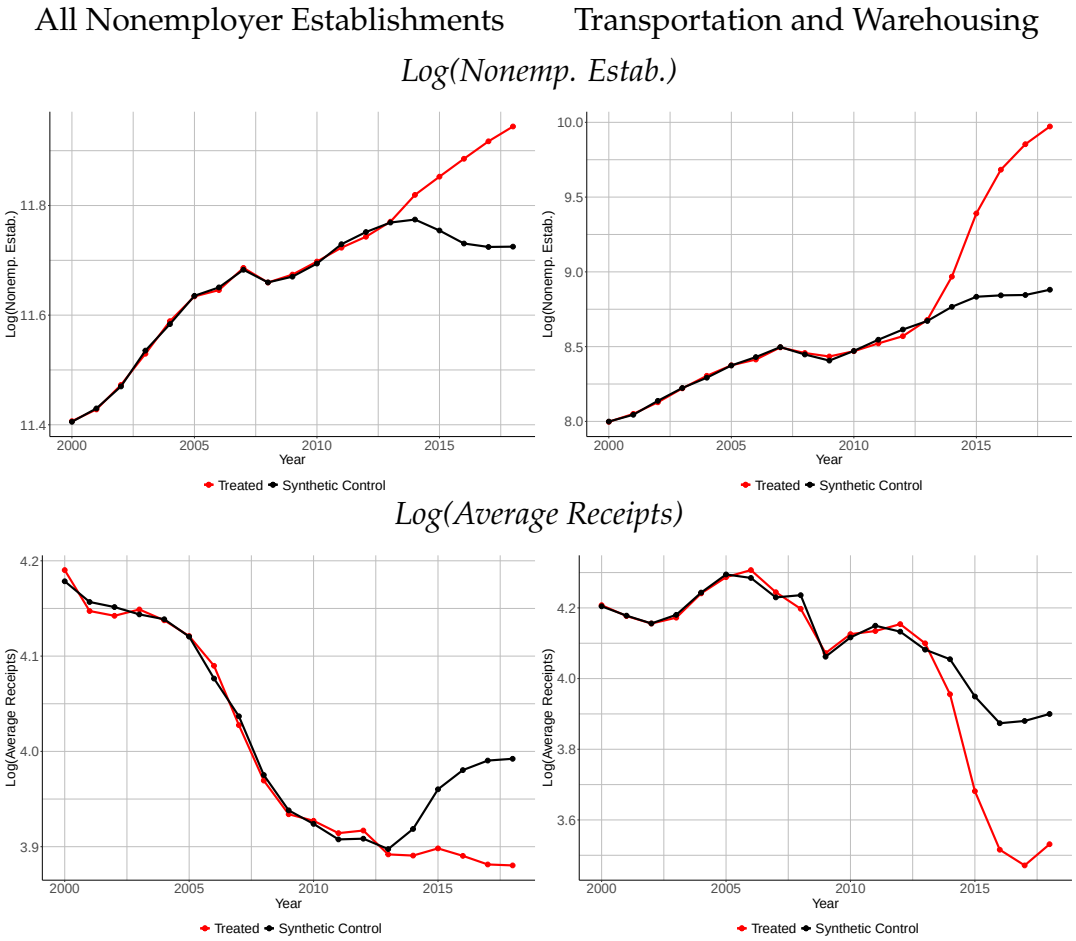
This figure shows the event study for the Callaway and Sant'Anna (2020) methodology for the treatment of a local minimum wage.

FIGURE A.14: Event Study Using Callaway and Sant'Anna (2020): Transportation and Warehousing



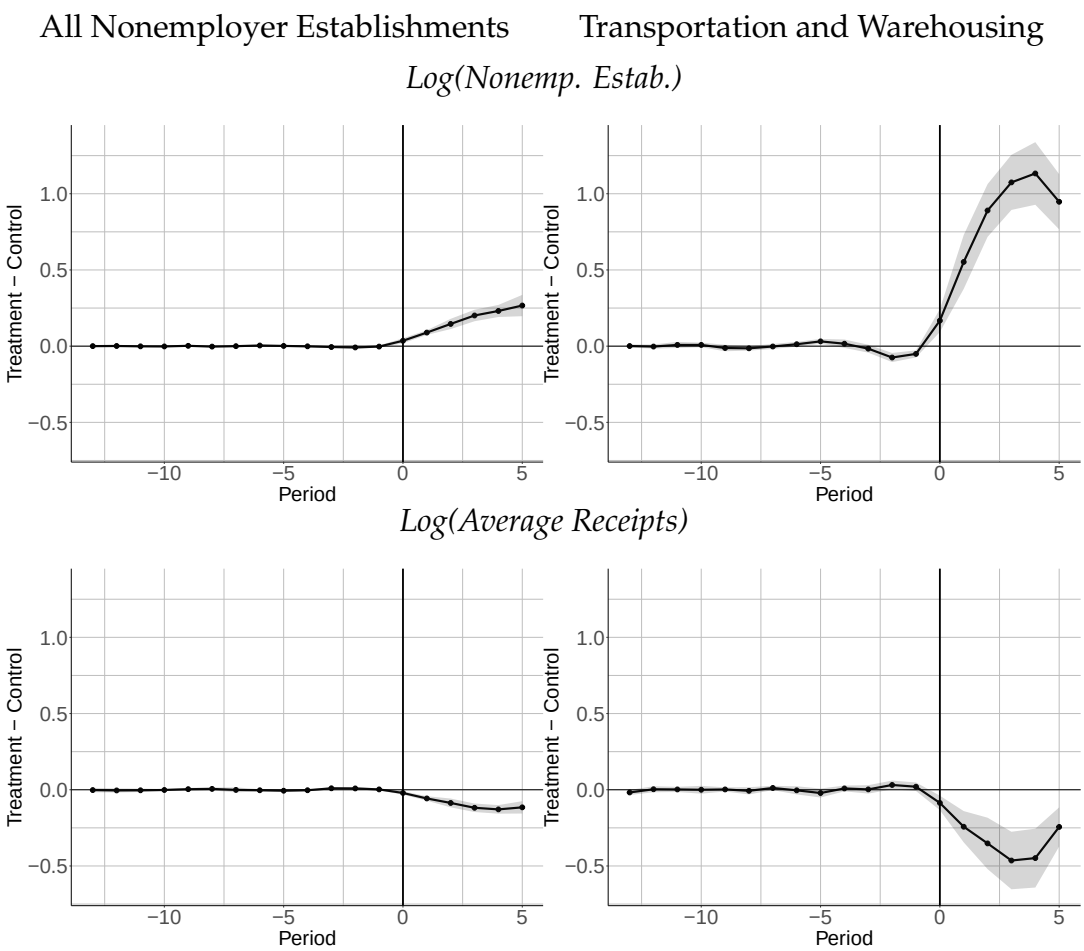
This figure shows the event study for the Callaway and Sant'Anna (2020) methodology for the treatment of a local minimum wage.

FIGURE A.15: Synthetic Control Counterfactual Plots:



These figures illustrate the counterfactual paths for the logged count of nonemployer establishments and the logged average receipts of nonemployer establishments for both transportation and warehousing services and all nonemployer establishments.

FIGURE A.16: Synthetic Control Results:

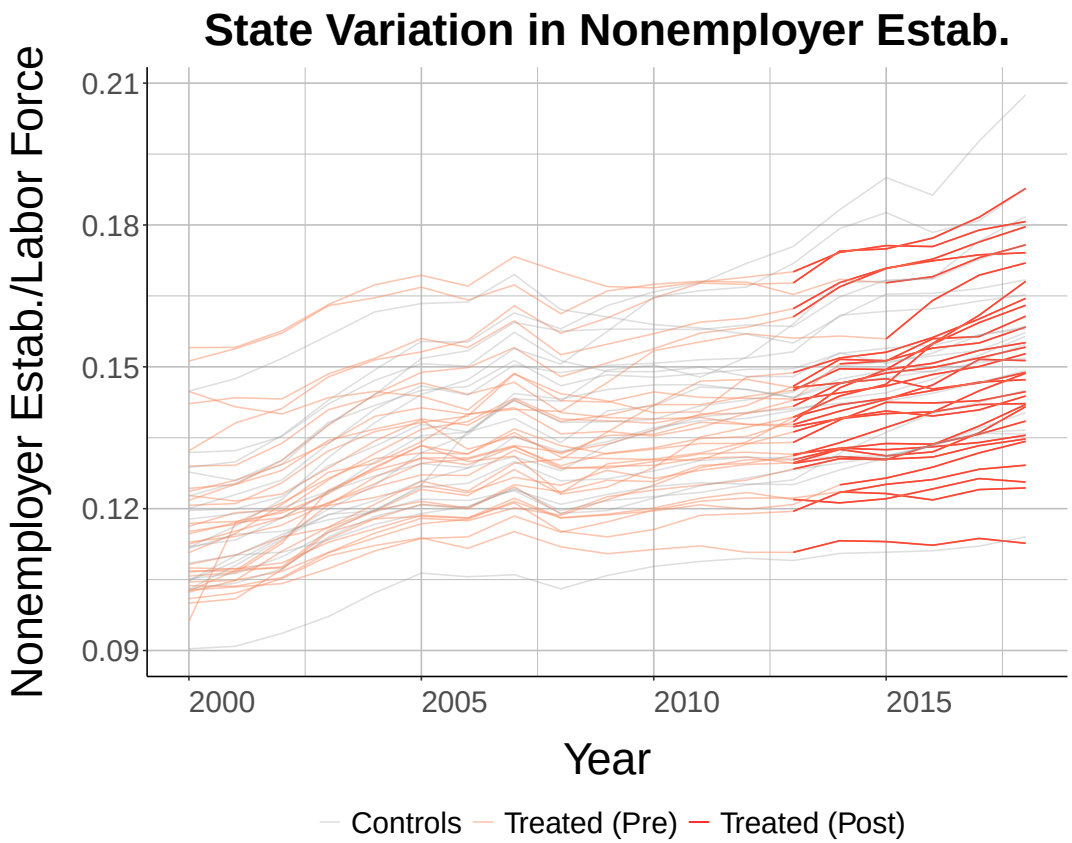


These figures illustrate the average effect of the treatment on the treated (ATT) for the logged count of nonemployer establishments and the logged average receipts of nonemployer establishments for both transportation and warehousing services and all nonemployer establishments.

Appendix B

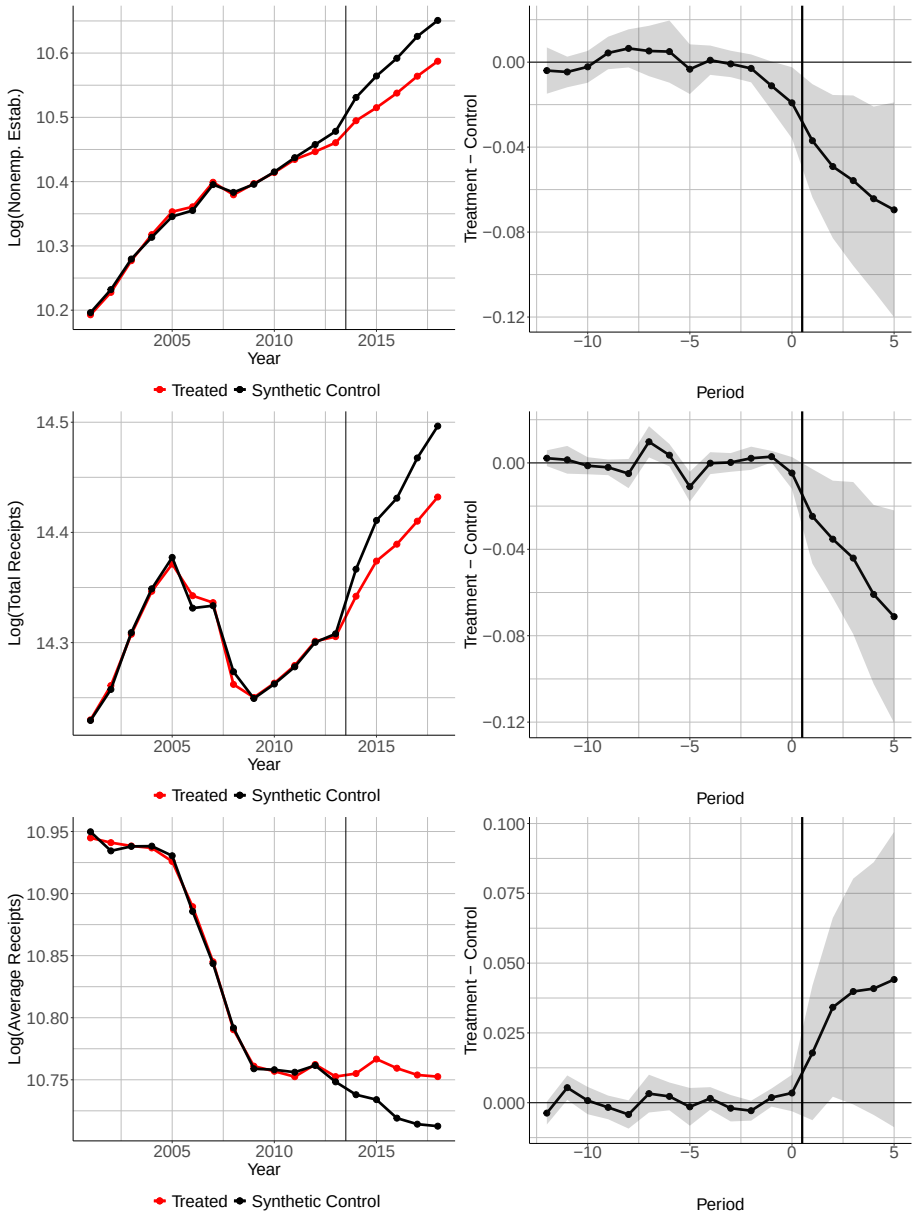
Appendix for Chapter 2

FIGURE B.1: State Trends in Nonemployer Establishment by Treatment Status



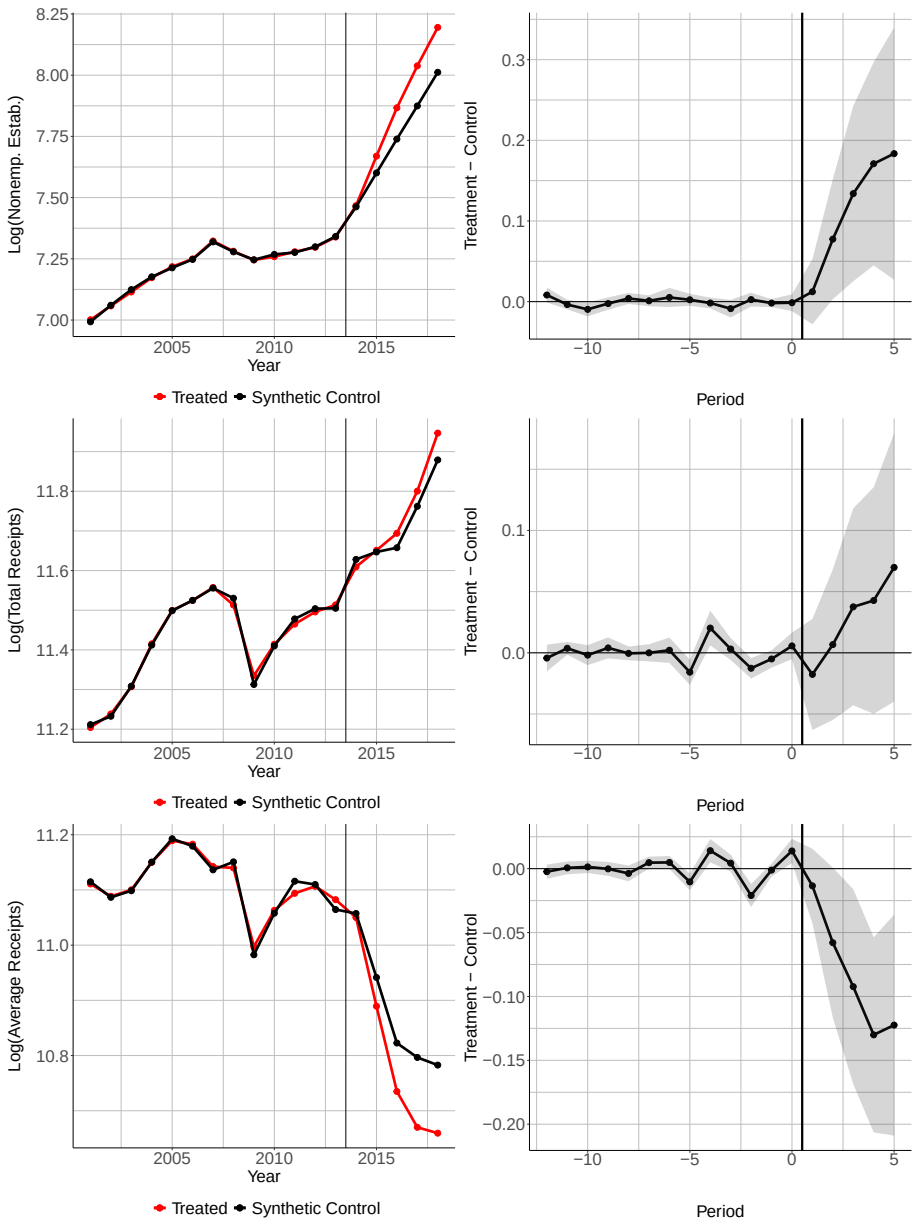
This figure shows the change in the number of nonemployer establishments across the length of the panel at the state level, with colored differentiation by treatment status.

FIGURE B.2: All Nonemployer Establishments, Synthetic Control Results:



This figure shows the treated and counterfactual group averages from 2001 to 2018 across the three dependent variables for all nonemployer establishments.

FIGURE B.3: Transportation and Warehousing Services, Synthetic Control Results:



This figure shows the treated and counterfactual group averages from 2001 to 2018 across the three dependent variables for transportation and warehousing services.

Appendix C

Appendix for Chapter 3

FIGURE C.1: Count of Respondents with Multiple Jobs by Treatment Status

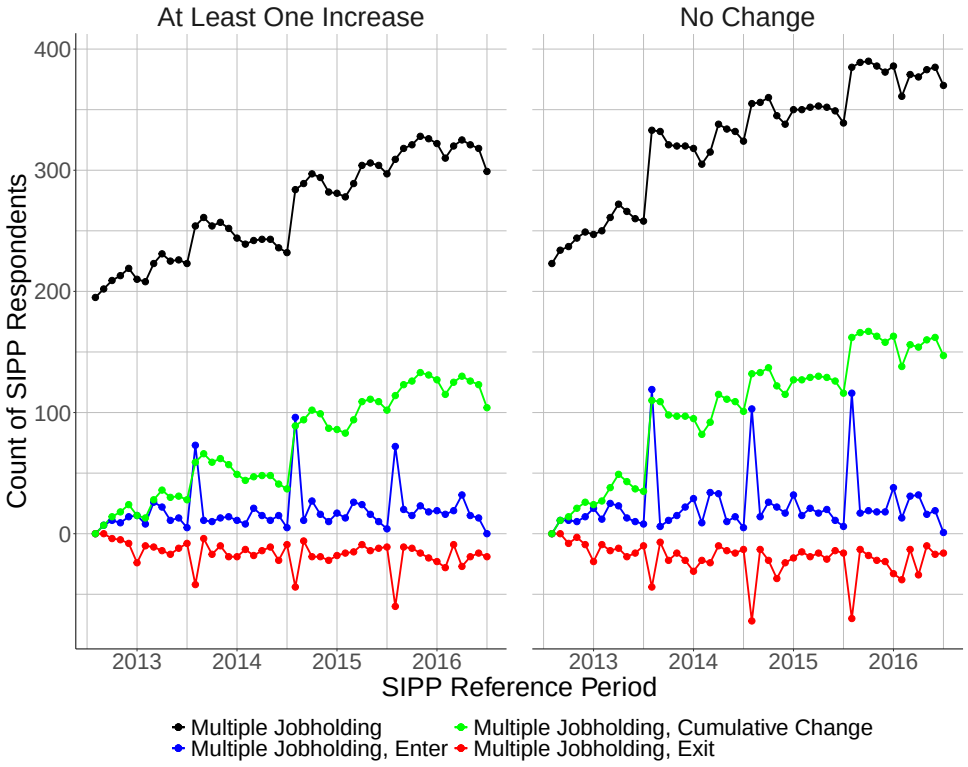


FIGURE C.2: Average Minimum Wage

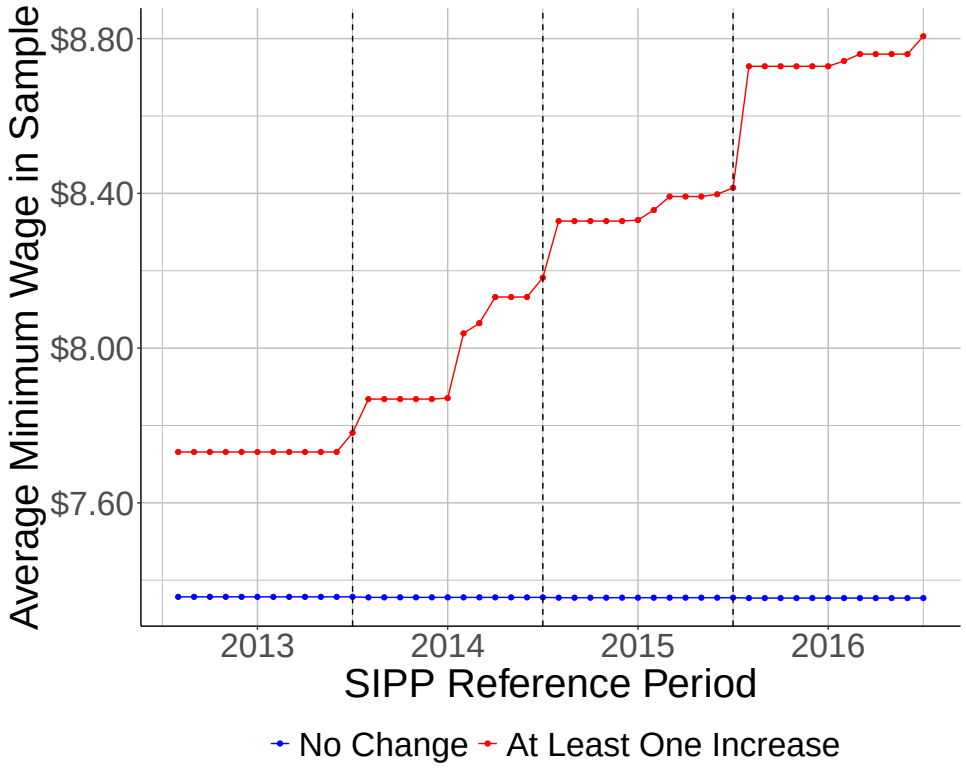
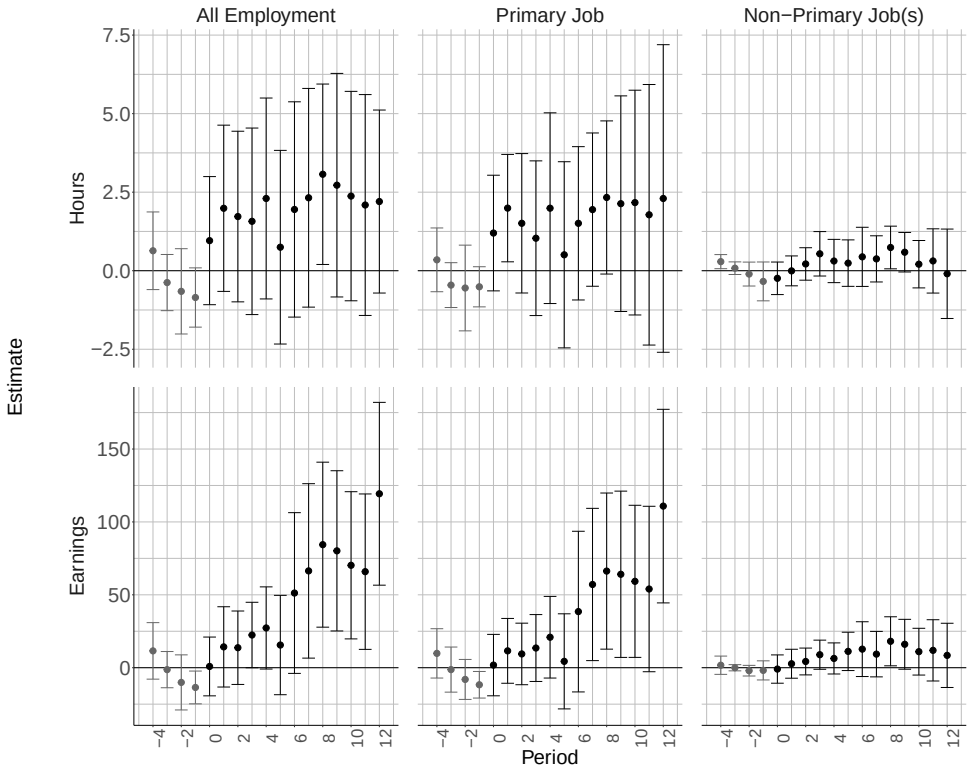
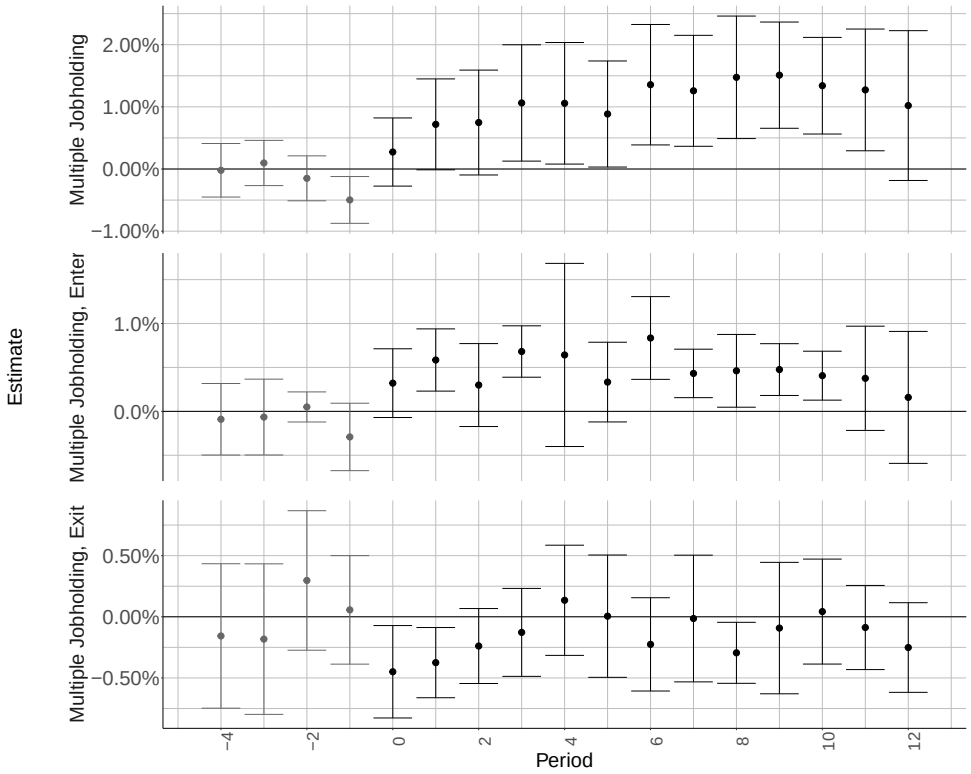


FIGURE C.3: Event Study for the Minimum Wage Increase's Effect on Hours and Earnings, Unconditional



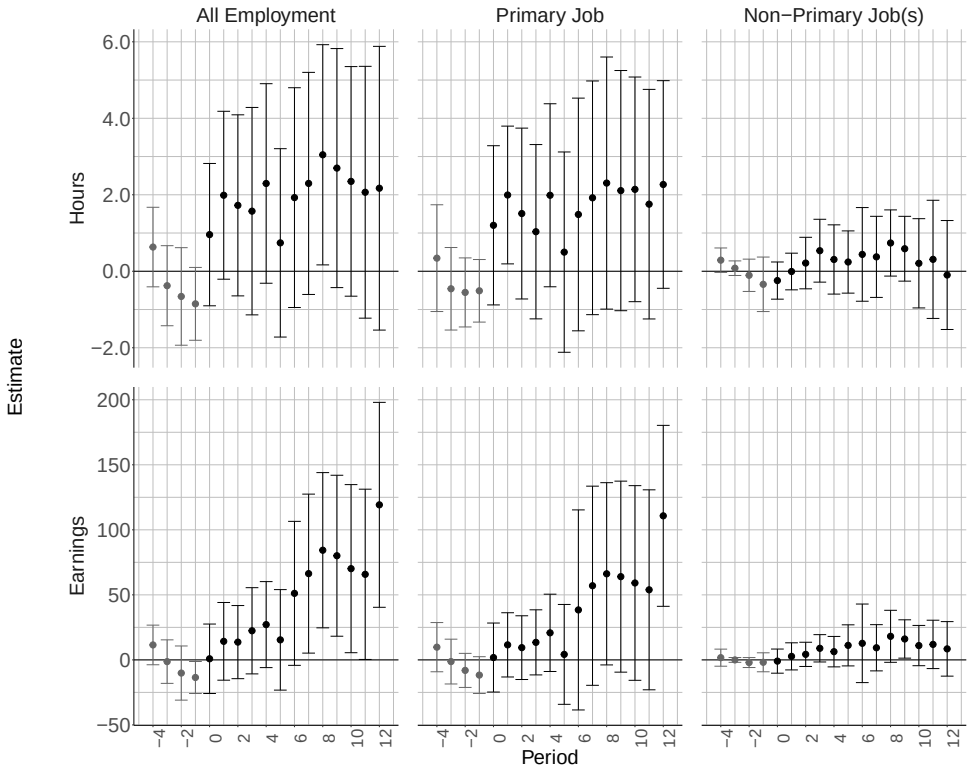
This event study is created using De Chaisemartin and D'Haultfœuille (2020a)'s estimator for dynamic treatment effects over a 12-month period with 4 months of placebo effect estimates before treatment occurs.

FIGURE C.4: Event Study for the Minimum Wage Increase's Effect on Multiple Jobholder Status, Unconditional



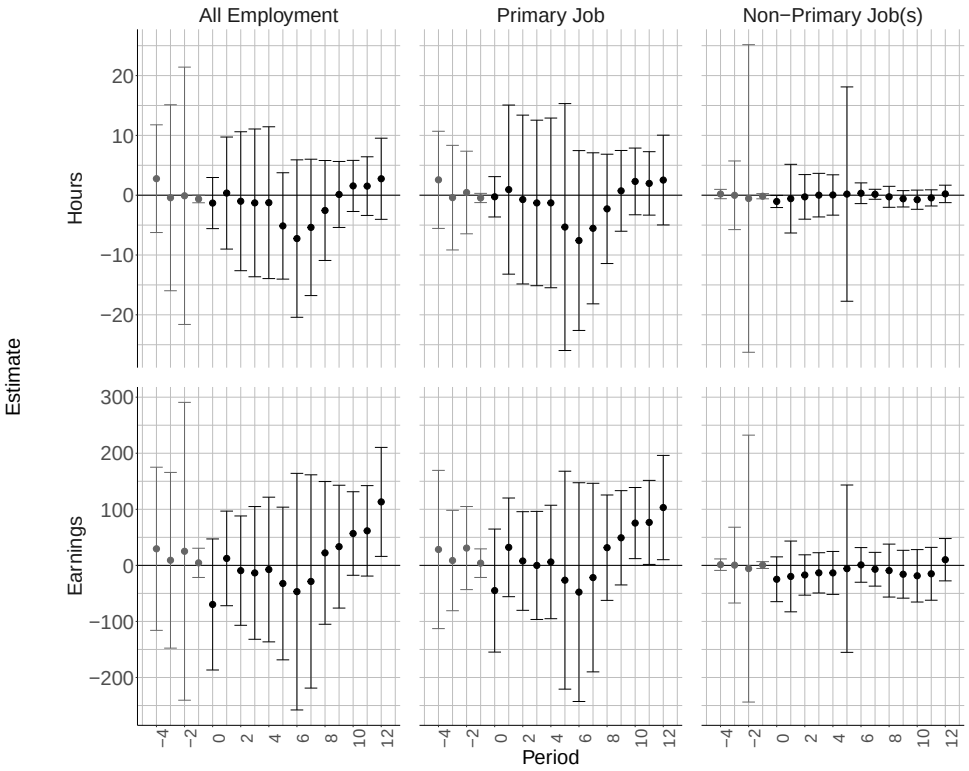
This event study is created using De Chaisemartin and D'Haultfœuille (2020a)'s estimator for dynamic treatment effects over a 12-month period with 4 months of placebo effect estimates before treatment occurs.

FIGURE C.5: Event Study for the Minimum Wage Increase's Effect on Hours and Earnings, Conditional



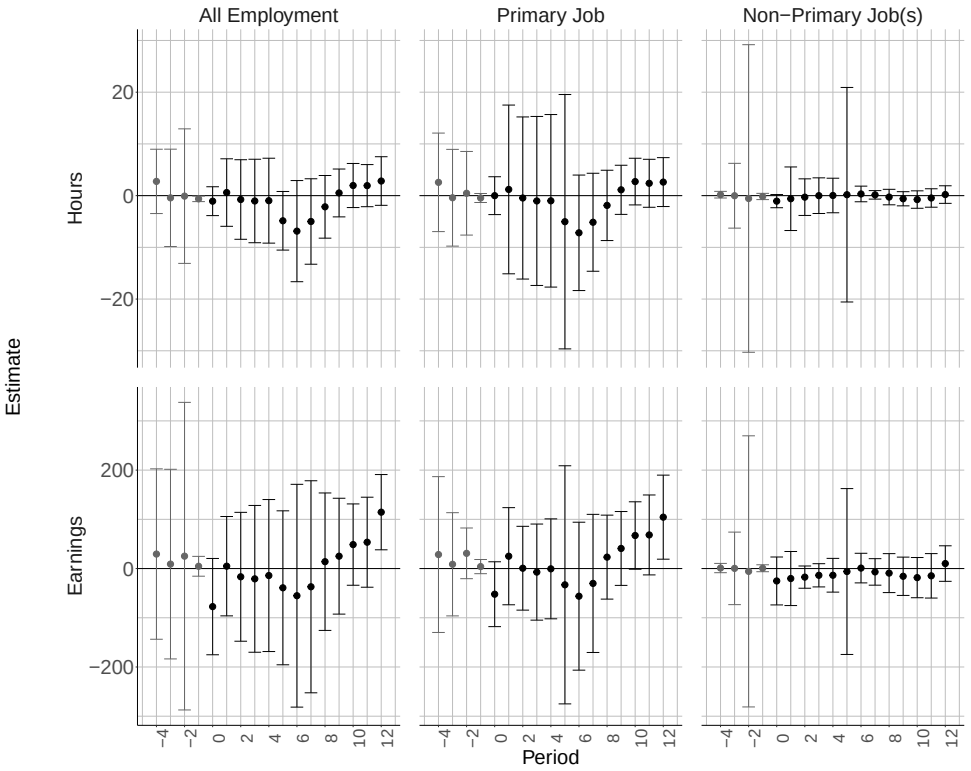
This event study is created using De Chaisemartin and D'Haultfœuille (2020a)'s estimator for dynamic treatment effects over a 12-month period with 4 months of placebo effect estimates before treatment occurs.

FIGURE C.6: Event Study for the Minimum Wage's Effect on Hours and Earnings, Conditional



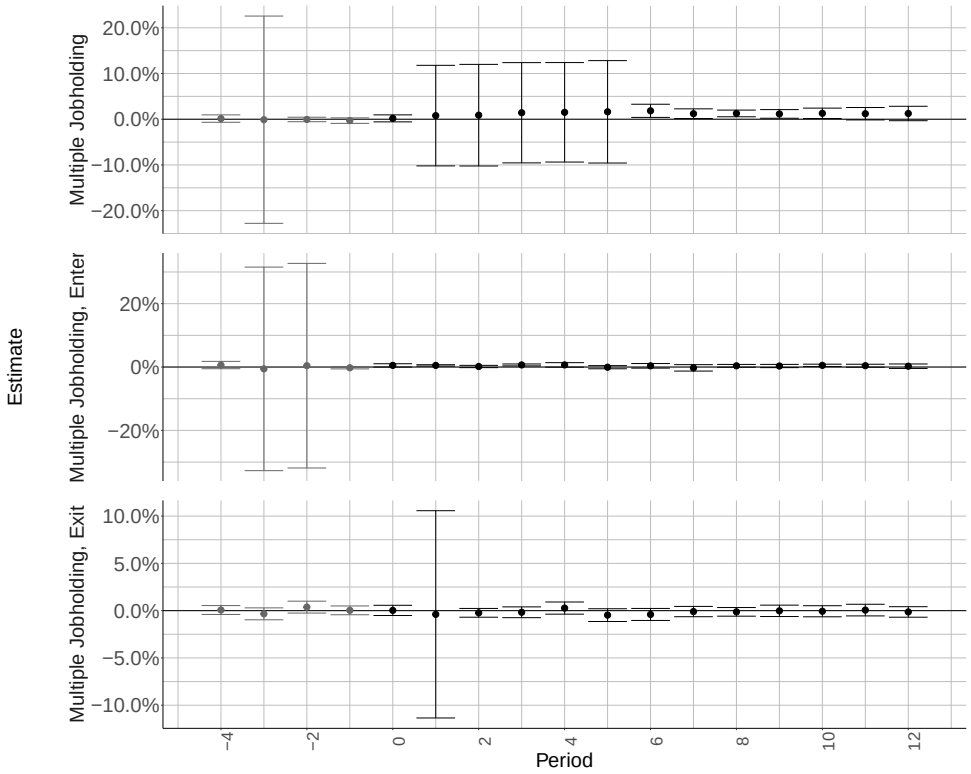
This event study is created using De Chaisemartin and D'Haultfœuille (2020a)'s estimator for dynamic treatment effects over a 12-month period with 4 months of placebo effect estimates before treatment occurs.

FIGURE C.7: Event Study for the Minimum Wage's Effect on Hours and Earnings, Unconditional



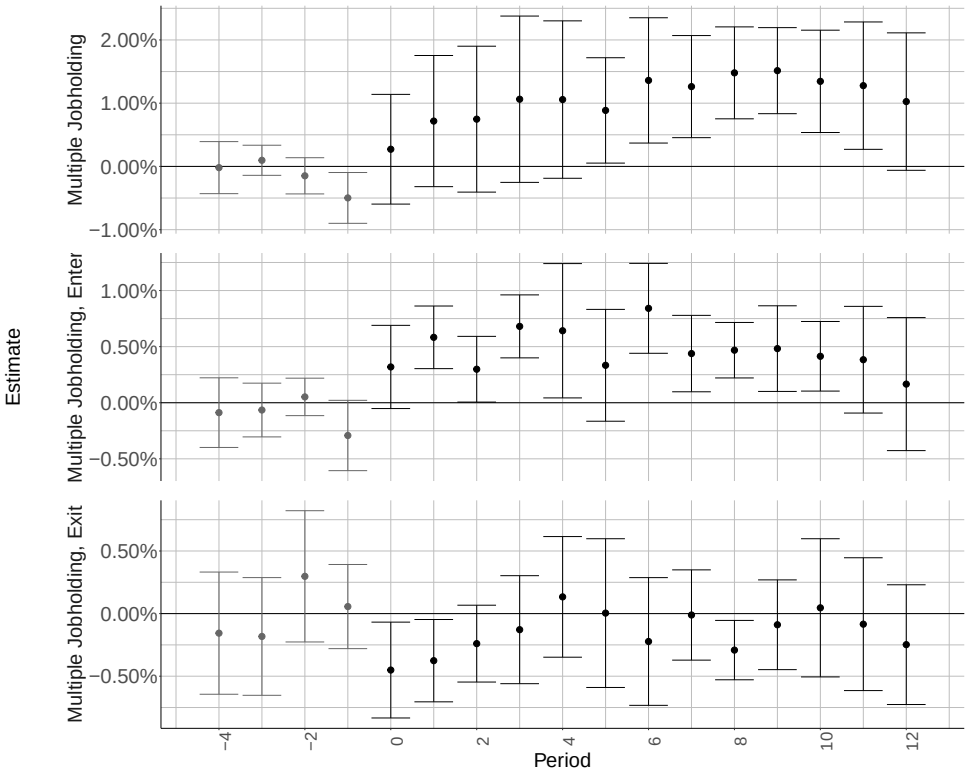
This event study is created using De Chaisemartin and D'Haultfœuille (2020a)'s estimator for dynamic treatment effects over a 12-month period with 4 months of placebo effect estimates before treatment occurs.

FIGURE C.8: Event Study for the Minimum Wage's Effect on Multiple Jobholder Status, Unconditional



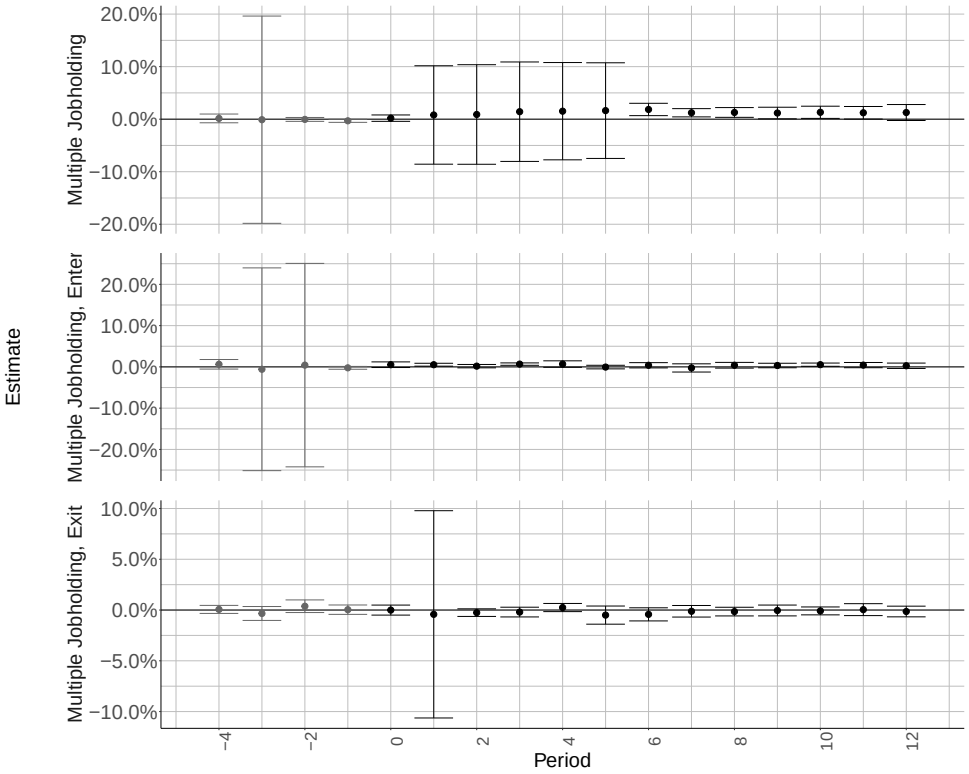
This event study is created using De Chaisemartin and D'Haultfœuille (2020a)'s estimator for dynamic treatment effects over a 12-month period with 4 months of placebo effect estimates before treatment occurs.

FIGURE C.9: Event Study for the Minimum Wage Increase's Effect on Multiple Jobholder Status, Conditional



This event study is created using De Chaisemartin and D'Haultfœuille (2020a)'s estimator for dynamic treatment effects over a 12-month period with 4 months of placebo effect estimates before treatment occurs.

FIGURE C.10: Event Study for the Minimum Wage's Effect on Multiple Jobholder Status, Conditional



This event study is created using De Chaisemartin and D'Haultfœuille (2020a)'s estimator for dynamic treatment effects over a 12-month period with 4 months of placebo effect estimates before treatment occurs.

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