ABSTRACT

Title of dissertation: HEALTHCARE PROVIDER AND LOCAL ECONOMY RESPONSES TO PRO-EMPLOYMENT POLICIES

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Understanding the responses of businesses and local economies to pro-employment policies is of critical economic and public policy interest. Better understanding of these policies not only can improve our ability to achieve major social goals, but also can shed light on more fundamental aspects of how local economies work and of how firms respond to incentives. This dissertation focuses on two specific cases of pro-employment policies: payroll subsidies for nursing homes and fiscal stimulus during the Great Recession.

For the first policy, I study the effect of payroll subsidies offered by state Medicaid programs to nursing homes on nursing home employment and wages. I identify the effect of subsidies using within-state, across-nursing home variation in subsidy rates and find that the subsidies were very effective at inducing nursing homes to increase nurse and nursing assistant employment and wages. Subsidy effect estimates are consistent with 100% pass-through to labor, with an implied elasticity of employment to subsidies on top of average Medicaid payments of 4.5. Beyond these baseline findings, I also investigate how the effectiveness of subsidies varies by nursing home market competitiveness and across for-profit and not-for-profit nursing homes. Cumulatively, my findings indicate that nursing home payroll subsidies are substantially more effective than previously thought,

suggesting that revisiting the efficacy of other payroll subsidies using firm-level subsidy variation would be valuable as well.

For the second policy, in work joint with coauthors, I examine the employment effects of the American Recovery and Reinvestment Act of 2009's stimulus expenditure during the Great Recession. We use across-county, within-state variation in stimulus to identify local fiscal multipliers, with a focus on identifying how multipliers vary by how severely a place was affected by the Great Recession. We find that the employment multiplier is more than twice as large in the half of counties most negatively affected by the recession than in the least affected half of counties. These findings demonstrate that the fiscal multiplier varies spatially across local labor markets as a function of exposure to employment reductions. They also imply that an employment-maximizing stimulus package targeted to high excess capacity counties would have created nearly twice as many jobs as were actually created by the American Recovery and Reinvestment Act.

Healthcare Provider and Local Economy Responses to Pro-Employment Policies

by

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Dedication

To my Mom, Dad, and Gammy – without your unwavering support, encouragement, and early instillation of a love for learning, I dare not ask where I would be.

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Contents

List of Tables

List of Figures

I Chapter 1: Introduction

Understanding the responses of firms and local labor markets to various pro-employment policies is of critical economic and public policy interest. These policies can be important levers for achieving a range of social goals, from ameliorating economic downturns to aiding disadvantaged populations. Better characterizing these policies' efficacy not only can improve our ability to achieve major social goals but also can shed light on more fundamental aspects of how local economies work and on how firms respond to incentives. My dissertation focuses on two pro-employment policies, payroll subsidies and fiscal stimulus, and pays particular attention to how best to design and target each. Chapter 2 of this dissertation focuses on the effect of payroll subsidies offered by state Medicaid programs on nursing home employment and wages, and aims to draw both general conclusions about payroll subsidies and conclusions specific to the nursing home policy context where increasing nursing home employment is important for improving care quality. Chapter 3 builds on the work in Chapter 2 and explores how nursing home market competitiveness interacts with nursing home payroll subsidies. Finally, Chapter 4 explores the effect of fiscal stimulus on employment during the Great Recession, with a particular focus on how local fiscal multipliers vary by local economic conditions and what implications this variation has for economic theory.

The focus on nursing home payroll subsidies in Chapters 2 and 3 is driven by a dual interest in their healthcare policy and labor economics implications. Judged purely as a labor policy, the nursing home payroll subsidies studied exhibit a number of features that make them uniquely valuable for informing the broader payroll subsidy literature. Within each state where subsidies were offered, they were designed so that different nursing homes were offered different effective subsidy rates. This within-state, establishment-level heterogeneity in subsidy rates provides a uniquely valuable opportunity for identifying the per dollar effect of payroll subsidies on employment and wages since it relies on an unusually detailed and well-measured source of variation that is orthogonal to any state-wide labor market shocks. The availability of this variation is particularly important since payroll subsidy adoption is often motivated by market-wide economic downturns that can confound estimates of the effect of payroll subsidies that rely on only state-level variation in subsidy adoption. Moreover, the variation I use comes from a set of payroll subsidies that were offered independently from other policy changes. This is a reasonably unique feature of my nursing home payroll subsidies. Most evidence currently available on payroll subsidies comes from the context of subsidies that were part of a broader policy bundle of active labor market programs. While the available evidence does point toward positive employment effects of payroll subsidies in these contexts (see Katz (1998), Neumark (2009), and Card et al. (2018)), it is difficult to fully differentiate between the effect of payroll subsidies and other features of the programs. This problem is not just theoretical and has led in the past to some rather paradoxical findings, such as is the case in Burtless (1985) where payroll subsidies reduced employment within the target population of welfare recipients, apparently by contributing to their stigmatization among employers. Additionally, problems relating to selective policy adoption and different policy changes being bundled with one-another are also prevalent in previous studies of nursing home policies, suggesting that this dissertation offers some broader lessons to that literature as well.

In addition to the above identification-based motives for studying nursing home payroll subsidies, these subsidies also are structured in an economically interesting way that potentially holds lessons for subsidy design in a wider array of contexts. In particular, the subsidies are designed to subsidize only marginal increases in payroll, as only a nursing home's increase in payroll relative to its payroll in some base year was eligible for subsidization. A common concern regarding payroll subsidies without this feature is that they may require the government to spend excessive amounts subsidizing inframarginal payroll, despite that such spending has dubious expected returns in any setting where scale effects are not large. By contrast, the marginal payroll subsidy structure focuses expenditure on maximizing the reduction in firms' marginal cost of raising wages and hiring workers. Although not very common in practice, this substitution effect maximizing approach seems more promising from the perspective of eliciting the largest employer response per dollar of expenditure. Although the nursing home payroll subsidies studied in Chapters 2 and 3 all exhibit some heterogeneity in design across states, they all exhibit this marginal payroll subsidy structure and so better understanding their efficacy is informative about how much promise this subsidy design holds.

Beyond being informative about payroll subsidies generally, nursing home payroll subsidies are also an independently important healthcare policy with substantial health and welfare implications. One in three Americans who reach the age of 65 are expected eventually to require some nursing home care, while at any given time over 1.3 million Americans receive nursing home care (KFF, 2017). Despite the high human stakes, nursing home care quality is often sub par and nursing homes' nurse and nursing assistant staffing levels, the critical input into care quality (Castle and Ferguson, 2010), are generally thought to be inefficiently low (Hackmann, 2018; Harrington et al., 2016). If the marginal payroll subsidy design that typifies nursing home payroll subsidies is indeed very effective at increasing nursing home employment, nursing home payroll subsidies may be highly effective tools for improving nursing home care quality and thereby both the health and quality of life of nursing home residents. Note that in addition to the implications for future policy makers seeking to improve our long-term care system in the face of an aging population, nursing home payroll subsidies have already impacted a large number of people: 25 states offered nursing home payroll subsidies for at least one year between 1996 and 2015, but typically for many more. If anything, this understates the impact of these policies, since while they were offered by half of states, they were offered in a set of states including California, Texas, and Florida that collectively represent a disproportionately large share of the United States population.

As is true of the subsidies' potential human impact, their public finance relevance is also substantial. These nursing home payroll subsidies are offered by state Medicaid programs, the primary financiers of nursing home care in the United States (KFF, 2017). At any given time, just under two-thirds of nursing home residents are on Medicaid (KFF, 2017), with the share of persons receiving long-term nursing care that eventually spend down their assets and take up Medicaid being still larger. Nursing home care constitutes approximately 10% of Medicaid's budget nation-wide (KFF, 2017), or about one-third of Medicaid's nation-wide expenditure on long-term care more generally. To the extent that marginal payroll subsidies represent a subsidy structure that more efficiently induces nursing homes to increase staffing and care quality than can be achieved by less targeted measures, such as general increases in Medicaid payment rates, they may be an important tool for helping Medicaid programs ensure nursing home residents receive high quality care even in the face of tightening budgets or rising care costs.

As a final point, it should be remembered that by the nature of the conditions that necessitate nursing home care, nursing home residents tend to be a vulnerable and overlooked population. Nursing home residents tend to be limited in their ability to advocate for themselves, be that by agitating for policy changes, complaining about care quality to state authorities, or by shopping around for higher quality facilities. This is a natural consequence of the conditions that necessitate nursing home care causing most nursing home residents to have mobility impairments and causing more than three in five nursing home residents to have some kind of cognitive impairment, such as dementia or the aftermath of a stroke (KFF, 2017). Moreover, most nursing home residents rely on Medicaid and have little in terms of assets, complicating their ability to change nursing homes or advocate for themselves even when physically able to do so. Given these limitations, it would seem that maintaining a high quality nursing home care system will depend in large part on broader social interest in doing so, especially given Medicaid's dominant role in the industry. However, although media exposés of appalling nursing home conditions do tend to be published periodically[1], nursing home care and long-term care more generally tends to receive less media attention and public interest than other aspects of the health care system (Miller et al., 2012b). This lack of interest may partly be because long-term care generally tends to viewed as a lower status part of the healthcare system (Miller et al., 2012b), though perhaps also results from a lack of broader public awareness of the large role Medicaid and public policy plays in the nursing home industry. Given these circumstances, it is my hope that research advancing the evidence pool on nursing home policy can help rectify some of the inattention this policy area has received and thereby serve a socially important role.

[1]For some recent examples, see: *"They Want Docile:" How Nursing Homes in the United States Overmedicate People with Dementia* (Human Rights Watch, 2018) or *Sick, Dying and Raped in America's Nursing Homes* (CNN, 2017).

Given these motives, Chapter 2 presents novel evidence about the effects of nursing home payroll subsidies on nursing home staffing and wages by studying a set of subsidies offered by 12 state Medicaid programs between 1996 and 2015, excluding from analysis the 13 states where subsidies were adopted or repealed contemporaneously with other major policy changes. Chapter 2 employs within-state, across-nursing home variation in subsidy rates to identify the effect of the subsidies on employment and wages. My approach contrasts with that taken in a small prior literature relying solely on across-state variation in subsidy adoption. This chapter's results imply that a nursing home receiving the average subsidy of $2.32 per nursing home resident-day increased its direct care worker employment per resident-day by 6.4 percent (11.4 minutes) and increased the average hourly wage paid to its direct care workers by 1.5 percent ($0.31). These figures correspond with an elasticity of employment to subsidies on top of average Medicaid payments of 4.5 and an average wage elasticity of 1.1. Furthermore, these estimates are consistent with 100% pass-through of subsidies to labor. I also find suggestive evidence that for-profit nursing homes are less responsive to the subsidies and that subsidies are less effective per dollar when marginal payroll subsidy rates are set at 100%. These results are robust to extending the empirical approach to compare nursing homes in payroll subsidy states to nursing homes in either synthetic control states or geographically neighboring control states that would have received similar subsidies were they located in treatment states. Taken as a whole, these findings indicate that nursing home payroll subsidies are substantially more effective than previously thought and suggest that revisiting the efficacy of other payroll subsidies using establishment-level subsidy variation would be valuable.

Chapter 3 builds on the evidence in Chapter 2 to help clarify the local market circumstances under which nursing home payroll subsidies are more or less effective. In particular, nursing homes in more concentrated markets may have weaker incentives to provide high quality care owing to softer competition over residents and over high-paying residents in particular. This may then translate to these nursing homes having a weaker motive to take up subsidies and to spend subsidies on improving staffing and care quality when they do take them up. Nursing homes in concentrated markets also may have market power in the labor market for nurses, raising their marginal cost of hiring nurses in response to subsidies and so further reducing their incentives to take up subsidies. Understanding the interplay between market power and subsidy efficacy not only is economically interesting, but also is of substantial policy importance. While only about 30% of nursing homes are located in concentrated markets overall, a much larger share of nursing home geographic markets are concentrated: most nursing homes in most states are in concentrated markets. As such, although concentration might not be a relevant feature of the markets facing most nursing homes, it is an important concern for most state Medicaid policy makers. Ultimately, my analysis in this chapter uncovers suggestive, though not statistically conclusive, evidence that nursing homes in more concentrated markets increase both their employment per resident-day of nurses and their payroll per resident-day by less per subsidy dollar than do nursing homes in competitive markets. These results are suggestive of either lower subsidy take up rates by nursing homes in concentrated markets or of weaker staffing responses when those subsidies are taken up, though neither the precise mechanism nor separate effects of product and labor market power could be identified.

In addition to the nursing home market focused work in Chapters 2 and 3, Chapter 4 of this dissertation studies the employment effects of the American Recovery and Reinvestment Act of 2009's stimulus expenditure during the Great Recession. Note that this Chapter is joint work with Arindrajit Dube, Ethan Kaplan, and Ben Zipperer. Chapter 4 is focused on identifying how the efficacy of stimulus spending varies by local labor market conditions or, more specifically, on how local fiscal multipliers vary by the severity of the impact of the Great Recession on county labor markets. Understanding these employment effects is critical once again from both an economic and a policy perspective. Economically, heterogeneity in local fiscal multipliers driven by variation in the extent of increases in local unemployment would have substantial implications for macroeconomic theory and would recommend models that feature state dependent fiscal multipliers that depend on more than just monetary policy conditions. From a policy perspective, such heterogeneity would stress the importance of efficient targeting of stimulus expenditure toward whichever places are most negatively impacted by a given recession. In turn, that would further suggest that projects to maximize the employment impact of fiscal stimulus would benefit either from preparing some sort of institutional capacity to allow for rapid disbursement of stimulus to needy regions or from greater usage of automatic stabilizers.

Chapter 4 identifies the effect of the American Recovery and Reinvestment Act's stimulus spending on employment using within-state, across-county variation in stimulus expenditure conditional on demographic trend controls and controls for predicted employment and wage bill derived using an industry shift-share measure. These results translate into estimates of an average annualized employment multiplier of just 0.42 job-years per $100,000 spent per county resident, with strong evidence for the employment response being much greater in counties more severely affected by the Great Recession and thus in counties that likely have greater excess capacity. In particular, the employment multiplier is 0.15 in below-median excess capacity counties, while it rises by nearly a factor of seven to 0.98 in above median-counties. These findings demonstrate that the multiplier varies spatially with the depth of the recession across different local labor markets. They also imply that an employment-maximizing stimulus package targeted to high excess capacity counties would have yielded an output multiplier of 1.09, 130% greater than we calculate actually was achieved by the ARRA. Additionally, this chapter uncovers evidence consistent with hysteresis, as positive employment effects of stimulus last many years after initial receipt and into the current expansion, further magnifying the importance of correctly targeting stimulus expenditure. Finally, this chapter also sheds some light on the mechanisms responsible for the spatial heterogeneity in the multiplier, demonstrating that increases in public sector employment caused by stimulus crowd out private sector employment in low excess capacity counties where labor markets are tighter and not in high excess capacity counties where they contain more slack.

II Chapter 2: The Effect of Establishment-Varying Payroll Subsidies on Nursing Home Employment and Wages

II.I Introduction

On any given day, approximately 1.3 million people live and receive care in United States nursing homes. About 62 percent of those people finance their nursing home care using Medicaid and still more are likely to spend down their assets and eventually take up Medicaid.[2] Ensuring that these people receive high-quality care is an ongoing challenge for state Medicaid programs, especially as measuring care quality in nursing homes, and therefore contracting on care quality, is difficult (Castle and Ferguson, 2010). One potential solution to this problem is to subsidize important inputs in the delivery of care quality. In particular, subsidizing nursing home employment of licensed nurses and nursing assistants (collectively, direct care workers[3]) may be a promising approach as nursing homes are highly labor intensive: the industry labor share of income is 88 percent[4] and experts often use nursing home direct care worker to resident staffing ratios as a proxy for care quality (Castle and Ferguson, 2010). Furthermore, experts often regard nursing homes as understaffed, attributing a range of nursing home quality issues to low staffing levels (e.g., Harrington et al. (2016)), and investigative reports suggest that low staffing levels may be partially responsible for overprescription of antipsychotic drugs and elder abuse in nurs-

[2]Resident count and Medicaid usage rate figures per author's calculations using 2015 Medicaid certification inspection survey data.

[3]I use the term direct care worker to refer to licensed nurses and certified nursing assistants engaged in direct care work only, excluding nurses performing administrative duties. I will also use the term nurses and licensed nurses interchangeably to refer to the set of both registered nurses and licensed practical nurses.

[4]This figure is for the Nursing and Residential Care Facilities industry (NAICS 623) in 2016 according to the Bureau of Economic Analysis's *GDP by Industry Value Added* data.

ing homes.[5] More formally, Hackmann (2018), relying on nursing home cost data and estimates from a structural demand estimation, found that nursing homes' nurse staffing levels on average fall 43 percent below the social optimum. As such, understanding the efficacy of nursing home payroll subsidies for increasing nursing home employment is important for the welfare of nursing home residents, a population that will grow in importance as the population ages, as well as for the 1.6 million people employed in the nursing home industry annually.[6]

In order to shed light on this issue, in this chapter, I study the employment and wage effects of a set of payroll subsidies offered by 12 state Medicaid programs[7] to nursing homes between 1996 and 2015. These subsidies were structured to subsidize only incremental increases in direct care worker payroll per day of nursing home resident care relative to the same figure in a base year. Since these subsidies only apply to payroll increases, they may be capable of eliciting large responses from firms while minimizing subsidy expenditures on inframarginal employment. I use previously unexploited establishment-level heterogeneity in subsidy rates coupled with data on the universe of Medicaid and Medicare-certified nursing homes to study the effect of these payroll subsidies on nursing home direct care worker employment per resident-day and average direct care worker hourly wages. A benefit of using this within-state variation to identify the subsidy effects is that it is orthogonal to state-wide nursing home industry and labor market trends that otherwise might confound estimates of the effect of subsidy adoption.

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[5]For examples of recent coverage, see: *"They Want Docile:" How Nursing Homes in the United States Overmedicate People with Dementia* (Human Rights Watch, 2018) and *Sick, Dying and Raped in America's Nursing Homes* (CNN, 2017).

[6]The employment figure is from the Bureau of Labor Statistics' *Current Employment Statistics* and is for the Nursing Care Facilities industry in September, 2017.

[7]The 12 states being Florida, Kansas, Louisiana, Massachusetts, Maine, Montana, North Dakota, Utah, Virginia, Vermont, Washington, and Wyoming. Note that Massachusetts and Montana have two subsidy events each.

As an extension to the above within-state approach, I also employ a within-and-across-state empirical design that compares nursing homes in states with payroll subsidies to nursing homes in control states whose characteristics would have caused them to receive similarly sized payroll subsidies had their state adopted a payroll subsidy. For this across-state approach, I consider a range of possible control states, including geographically neighboring control state pairs and statistically constructed synthetic control states. These extensions to my primary research design relax the assumption that nursing homes in treated states with characteristics causing them to be offered different subsidy amounts are on similar wage and employment per resident-day trends, replacing it with the alternative assumption that nursing homes with those characteristics face similar trends across the treatment and control states.

In contrast to the pre-existing literature on these subsidies that finds rather limited effects on nursing home employment (Foster and Lee, 2015; Feng et al., 2010), evidence from my within-state, across-nursing home research design suggests the employment and wage effects of these subsidies are quite large. Specifically, I estimate that increasing the maximum effective subsidy amount offered to nursing homes by an additional $1 per resident-day causes nursing homes to increase their employment of direct care workers per resident-day by a statistically significant 4.9 minutes, implying an increase in staffing of approximately 6.3 percent at the mean nursing home. At the same time, the effect on the average hourly wage paid to direct care workers is to increase it by 13 cents for each subsidy dollar offered per resident-day, implying an increase in average wage by 1.5 percent at the mean nursing home. Results from the various across-state models are similar to these estimates. Finally, interpreting these payroll subsidies as payment increases relative to state average daily Medicaid nursing home payment rates, I estimate that the elasticity of direct care worker employment per resident-day to subsidies is 4.5 and that the elasticity of average direct care worker hourly wages to subsidies per resident-day is 1.1.

Cumulatively, the evidence I find in this chapter suggests that nursing home payroll subsidies are very effective at increasing nursing home employment and so may be an effective way of addressing concerns related to poor care quality in nursing homes. These results also are informative for the broader payroll subsidy literature, suggesting that attention to detail regarding the structure of payroll subsidy formulas is important and adding to a growing pool of research examining payroll subsidies using firm-level variation in subsidy receipt (Saez et al., 2017).

II.II Literature Review

This research is related to a number of studies in the nursing home policy literature as well as in the broader payroll subsidy literature. The study most closely related to mine is a paper by Foster and Lee (2015) that studies the effect of a set of nursing home payroll subsidies[8] offered by state Medicaid programs between 1999 and 2004. They identify the average effect of subsidy adoption on direct care worker employment per resident-day (direct care worker staffing) at nursing homes in states adopting subsidies using a state-level difference-in-differences research design and nursing home level data. In addition to regressing staffing on the standard state difference-in-differences treatment indicator variables and fixed effects, they also control for a number of time-varying nursing home characteristics and a number of state-level policy environment variables. Using this approach, Foster and Lee find that the average effect of subsidy adoption was to increase nursing

[8] A note on terminology: Foster and Lee, as well as other papers in the nursing home literature, often refer to nursing home payroll subsidies as "wage pass-throughs." Medicaid agencies sometimes use this term for their policies, but also sometimes refer to them as "supplemental payments for direct care workers" or "wage enhancement payments." I will simply refer to all of these as payroll subsidies.

assistant employment per resident-day by nearly 1 percent, with no impact on nurse employment per resident-day. While these effects are small, they do find a larger 2.8 percent effect on the subset of nursing homes with relatively few residents on Medicaid. One concern with Foster and Lee's approach is that their state difference-in-differences research design is vulnerable to underestimating the effect of subsidy adoption if nursing home payroll subsidies were adopted in response to state-wide declining nursing home staffing trends. Policy endogeneity of this sort is likely, as Miller et al. (2012a) document that states are more likely to adopt payroll subsidies during business cycle expansions, when low unemployment makes hiring more difficult for nursing homes and when more favorable fiscal conditions reduce state legislatures' reluctance to increase Medicaid spending.

Foster and Lee's research is limited in a number of additional respects that create further opportunities for improvement. First, Foster and Lee determine when a state Medicaid program offers a payroll subsidy using a survey of state Medicaid offices. When compared to my documentation, developed through extensive archival research,[9] this survey 1) missed a number of payroll subsidies adopted within the survey window; 2) indicated that a number of subsidies were repealed immediately after adoption that I found remained in place; and 3) listed a number of subsidies as being in place for which I found no corresponding payroll subsidies.[10] As a result, Foster and Lee measure their main policy

[9]For details on the policy regime documentation process, please see Appendix II.X.I on the subject.

[10]Examples of these phenomena include the survey missing subsidies offered in Florida and Massachusetts from 2000-2001, finding spurious subsidy repeals in 2003 Wyoming, 2001 Montana, and 2001 Virginia, and listing subsidies as being offered for which I can find no documentary evidence in 2001 Rhode Island and 2002 New York. The difference between my records and the survey results may be due to Medicaid officials remembering to list dates when policies were adopted but forgetting to explicitly list that the policies continued in other years. It may also be due to Medicaid officials responding affirmatively to questions about additional payments for direct care workers when inflation adjustments and small demonstration programs were all that occurred. Additionally, the survey was conducted in 2003 and surveyed Medicaid state offices about their 1996-2002 policies retrospectively. Retrospective surveying of this sort is often viewed as unreliable within the survey methodology literature (Groves et al., 2011) and may be particularly unreliable in the context of Medicaid state offices where institutional memory may be limited by staff turnover.

regime variable with considerable error and likely present attenuated estimates of payroll subsidy employment effects. Foster and Lee's measures of other policy regime changes occurring within their sample also miss some policy changes, suggesting their policy regime controls may permit some residual contamination of their treatment and control groups by confounding policy events. Finally, Foster and Lee's difference-in-differences regressions include controls for a number of time-varying nursing home level characteristics that represent margins on which nursing homes may respond to subsidy adoption and which sometimes enter directly into states' payroll subsidy formulas.[11] Controlling for these nursing home level margins of response and characteristics associated with subsidy rates may attenuate their state-wide estimates of the impact of subsidy adoption.

Many of the limitations affecting Foster and Lee's research also apply to antecedent work in the nursing home literature, such as a similar study of the effect of payroll subsidies on nursing home staffing by Feng et al. (2010). They apply as well to research by Baughman and Smith (2010) that studies the effect of nursing home and home health care industry payroll subsidies on nursing assistant wages from 1996 to 2003. Baughman and Smith estimate using data on nursing assistant wages from the Surveys of Income and Program Participation to estimate that their sample of payroll subsidies caused a 12.2 percent increase in nursing assistant average hourly wages. While large proportional nursing assistant wage responses are not necessarily surprising given the low base wage rate for these workers, the tendency of states to offer subsidies when upward pressure on employment and wages are high would suggest that this estimate may be upwardly biased. Cumulatively, the limitations affecting the nursing home policy literature suggest that ex-

[11] Examples of these characteristics include nursing home resident counts, shares of nursing home residents on Medicaid, and nursing homes' average severity of resident care needs.

isting estimates of the employment effects of nursing home payroll subsidy adoption may be understated while the estimated wage effects may be overstated.

From the broader literature on payroll subsidies, Saez et al. (2017)'s study of the employment and wage effects of a Swedish payroll tax cut is, in some respects, closely related to this research. Similar to this study, Saez et al. estimate the effect of their policy using firm-level variation in subsidy amounts generated by pre-subsidy variation in firm characteristics. In particular, the payroll tax cut they study was targeted at workers under the age of 26 and so Saez et al. compare firms receiving different subsidy windfalls due to variation in their employment of younger workers. Using their approach, Saez et al. find that more heavily subsidized firms, those facing a 2.4 percent greater reduction in average labor costs, exhibited 4.6 percent more employment growth. While these firms did not raise pre-tax wages, neither did they lower them, resulting in 2.4 percent growth in wages net of taxes. Interestingly, Saez et al. find that firms did not restrict this employment and wage growth to just young workers, but rather shared it across all types of workers employed by the firm. Saez et al. further show that when estimating the effect of the policy by comparing labor market wide outcomes for workers just above and below the age 26 threshold, one finds no effect on wages net of taxes and only a 2-3 percentage point increase in employment driven not by hiring but rather by a reduction in the separation rate. Use of firm-level variation in their setting, though for a different reason than the identification one in this study, was necessary to identify the positive effect of the tax cut on wages and to uncover the extent of firm-level employment responses.

Beyond Saez et al.'s recent work, there are many studies examining payroll subsidies in the United States. In many respects, this literature faces challenges similar to those in the nursing home payroll subsidy literature. Payroll tax cut and subsidy effects can be difficult to disentangle from the broader economic conditions that motivate them as well as from the effect of other policy changes that frequently accompany them. One key source of evidence on payroll subsidy efficacy is the active labor market program literature. A meta-analysis by Card et al. (2018) suggests that active labor market programs including payroll subsidies targeted at hiring specific types of workers were more likely to have positive employment effects than other programs. Literature reviews by Katz (1998) and Neumark (2009) on the efficacy of payroll subsidies drawing heavily on variation from active labor market programs also point toward positive effects of payroll subsidies on employment, though small or insignificant employment effects are not uncommon. The estimated subsidy effects also can be quite context dependent. For example, Heaton (2012) finds that payroll subsidies targeted toward hiring disabled veterans have substantial positive employment effects, while Burtless (1985) finds that similar subsidies targeted toward welfare recipients can reduce employment by contributing to the stigmatization of the target population. A related source of evidence may be drawn from the literature on place-based policies where, in a prominent example,[12] Busso et al. (2013) study the effect of the Federal Empowerment Zone Program, which includes geographically targeted payroll subsidies, and find substantial positive effects on employment and wages. However, these payroll subsidy effects cannot be disentangled from the impact of other components of the program, such as from the effect of spending on local development initiatives. Overall, the active labor market program literature points toward payroll subsidies having the expected positive effects but leaves substantial room for improving upon the identification of the subsidy effects and for better separating those effects from other aspects of the programs being studied.

[12] For a broader literature review of place-based policies, please see Neumark and Simpson (2014).

Another useful source of evidence on payroll subsidies may be drawn from the literature on hiring tax credits, a policy which provides a tax subsidy for hiring individual workers but not for wage increases, hours increases, or continued employment of existing workers. Neumark and Grijalva (2017) study a broad sample of recent state hiring credits, many adopted during the Great Recession, using a difference-in-differences style research design. They found hiring credits tend to have positive effects on employment and have stronger positive effects during recessions. However, they also found many examples of state hiring tax credits which had little or no effect beyond increasing employee churn at firms, with null results being more likely when states lacked auditing and enforcement mechanisms for ensuring credits only went to firms that genuinely increased hiring. Meanwhile, Cahuc et al. (2018) provide related evidence, finding substantial positive employment effects for a French hiring credit implemented during the Great Recession that was targeted toward low wage workers at firms with fewer than ten employees. This part of the payroll subsidy literature suggests one should expect to observe positive employment effects for payroll subsidies, but that attention to detail in the design and targeting of the subsidies is necessary.

II.III Subsidy Background and Expected Effects

The nursing home payroll subsidies included in this study vary in design but include a number of common features. Notably, all of the policies I study were structured to subsidize incremental increases in nursing home direct care worker payroll relative to each nursing home's direct care worker payroll in a base year. This structure minimizes subsidy payments for inframarginal employment and creates a potential for strong employment responses by firms. To ensure that nursing homes actually increased payroll relative to the chosen base year, states administered these subsidies through their broader nursing home Medicaid reimbursement systems which had substantial pre-existing auditing capabilities.[13] Many states set very high subsidy rates for incremental payroll increases, so in order to contain costs, states also capped the size of the increase in payroll that they would subsidize. I use the state subsidy formulas, including the subsidy caps, to simulate the maximum subsidy amount per resident-day that nursing homes would have been offered given their observed characteristics. I take these simulated maximum subsidy offers per resident-day, which I term the nursing home's "effective subsidy rate," to summarize the degree to which individual nursing homes within each state are subsidized by their state's policies. This approach is similar to Saez et al.'s (2017) method of calculating subsidy windfalls for individual firms in their setting and carries the same limitation in that it does not allow for separate identification of the effect of variation in maximum subsidy amounts offered and in the marginal subsidy rate offered up to those maximums.

The variation in effective payroll subsidy rates offered to nursing homes within each state is generated by the details of state subsidy formulas. These formulas follow one of three general patterns: allocated payment style subsidy formulas; daily rate style subsidy formulas; and hourly rate style subsidy formulas. Allocated payment subsidy formulas compute for each nursing home a maximum annual payment and subsidize, at a 100 percent rate, increases in total payroll relative to a base year by up to that nursing home-specific annual maximum. Typically, these maximum subsidy amounts are calculated by taking a fixed total amount of state-wide subsidy funding and allocating it to nursing homes in proportion to their share of the state-wide total Medicaid nursing home

[13] These systems were already collecting detailed information on nursing home expenditure by cost category and could adapt their prior auditing procedures to enforcement of payroll subsidy provisions. The existence of these auditing and enforcement mechanisms is, given Neumark and Grijalva's (2017) findings, likely to contribute to the efficacy of the subsidies.

resident population in the base year.[14] For example, under an allocated payment subsidy, a nursing home with 32,000 resident-days per year might be offered a $64,000 annual subsidy, which would lead me to calculate an effective subsidy rate of $2 per resident-day.[15] Such a nursing home would face a 100 percent marginal subsidy rate for increases in direct care worker payroll per resident-day of up to $2 relative to their base year payroll.

Daily rate payroll subsidy formulas take a different approach. These formulas specify a maximum subsidy amount per nursing home Medicaid resident-day for all nursing homes and offer to subsidize incremental increases in nursing homes' direct care worker payroll per Medicaid resident-day at a 100 percent rate up until that amount. These subsidy designs further require that nursing homes match payroll expenditure on Medicaid residents one-for-one with expenditure on non-Medicaid residents.[16] For example, under a daily rate subsidy, a nursing home with two-thirds of its residents on Medicaid might be offered a maximum subsidy of $3 per Medicaid resident-day, resulting in a marginal subsidy rate of 66 percent and an effective subsidy rate of $2 per resident-day due to the matching requirement.

Hourly rate payroll subsidy formulas use an approach similar to daily rate states. These designs subsidize increases in direct care worker wages at a 100 percent marginal

[14]More precisely, this allocation is often a function of each nursing home's "allowed" Medicaid direct care worker payroll expenditure. This amount is usually calculated as the number of the nursing home's residents on Medicaid multiplied by the lesser of the nursing home's actual direct care worker payroll per resident-day and a Medicaid-set maximum billable amount of payroll expenditure per resident-day specific to each nursing home. During this study's time period, maximum billable amounts were often set very low. This reduced the amount of variation in allowed payroll expenditure per resident-day across nursing homes, pushing the measure closer toward just being proportional to Medicaid resident count. When simulating subsidy rates, I do not observe the maximum billable amounts and so just use numbers of nursing home residents on Medicaid rather than allowed Medicaid payroll.

[15]All example subsidy figures, though rounded to convenient whole numbers, are approximately accurate for an average nursing home. For precise figures, please see the summary statistics in Table II.1. Note that the resident-days per year figure is obtained by multiplying the observed resident counts by 365.

[16]This is due to Medicaid requirements that Medicaid and non-Medicaid nursing home residents receive equal care. Grabowski et al. (2008) show that these requirements are, in fact, effective.

rate up to some maximum wage increase but only for the share of a nursing home's workers serving Medicaid residents. Nursing homes are then required to match these wage increases for their other direct care workers. As in the daily rate subsidy states, the hourly rate subsidy states suppose that the share of a nursing home's direct care workers serving Medicaid residents is equal to the share of the nursing home's residents on Medicaid. For example, under an hourly rate subsidy, a nursing home might be offered a $1 subsidy to increases in hourly wages for workers serving Medicaid residents. For a nursing home with two-thirds of its residents on Medicaid and three hours of direct care worker employment per resident-day, this would translate into a 66 percent marginal subsidy to increases in direct care worker wages up to a $1 increase and so an effective subsidy per resident-day of $2.[17]

Since all subsidy formulas reduce the marginal cost of increasing payroll per resident-day, the substitution effect would suggest nursing homes should respond by increasing payroll per resident-day in some fashion, be that by increasing employment or by raising wages. While in principle these subsidies might have some kind of scale effect on nursing home employment, since the subsidies are structured to minimize payments for inframarginal payroll expenditure, it seems likely that the substitution effect will be more important in this setting. Relative to nursing homes in allocated payment subsidy states, nursing homes in daily rate and hourly rate subsidy states may be less likely to take up their full subsidy amounts because they face lower average marginal subsidy rates, but also may respond by more per subsidy dollar spent because of their states' matching requirements. It is worth noting that across all subsidy types, any nursing home whose direct care worker payroll has fallen below its base year payroll faces marginal subsidy rates of

[17]This is calculated by taking the $1 per Medicaid worker hour subsidy, multiplying it by the share of residents on Medicaid (0.66), and then by the number of worker hours per resident-day in the facility (3).

0 until it spends back up to its initial payroll level. This subsidy feature may be expected to generate some polarizing behavior across nursing homes, where nursing homes whose payroll has fallen significantly might ignore their offered subsidy amounts while nursing homes whose payroll has fallen less might exhibit very strong subsidy responses due to the additional spending required of them to begin qualifying for subsidies.

One concern with this characterization of the effective subsidy rates offered to nursing homes is that if nursing homes may freely and quickly adjust their share of residents on Medicaid, then these effective subsidy rate calculations overstate the degree of variation in maximum subsidy amounts offered to different nursing homes in the daily rate and hourly rate subsidy states. This, in turn, would attenuate my estimated subsidy effects. In practice, however, shares of nursing home residents on Medicaid tend to be very sticky. In the short run, legal limitations on ejecting nursing home residents prevents adjustments to nursing home resident pools from happening much faster than the natural rate of nursing home resident churn.[18] In the medium run, state Medicaid bed certification laws prevent nursing homes from increasing their maximum Medicaid resident count without going through a lengthy and potentially unsuccessful regulatory approval process. Nursing homes also may be slowed in adjusting their Medicaid resident shares by the need to make capital investments when retooling to serve different resident populations.[19] Finally, even in the long run, nursing homes may be constrained from adjusting

[18] Note that nursing homes can influence this churn rate to some extent. Hackmann and Pohl (2018) show that high occupancy nursing homes seem to manipulate their Medicaid residents' average length of stay, finding specifically that Medicaid resident average stay lengths are 36 percent lower at nursing homes with occupancy rates close to 100 percent relative to at nursing homes with occupancy rates close to 75 percent. However, nursing homes have little incentive to adjust their share of residents on Medicaid through a length of stay mechanism when nursing homes have spare capacity and Hackmann and Pohl do not find substantial variation in Medicaid resident stay lengths across nursing homes with occupancy rates under 90 percent.

[19] While similar in some respects, Medicaid and Medicare patients typically have some different care needs because Medicare covers only short-term rehabilitation care, while Medicaid covers long-term care. Private pay patients, meanwhile, may have different care needs as well as different expectations for facility quality.

their share of residents on Medicaid by local market conditions. Hackmann (2018) shows that most nursing home residents come from within 15 miles of their nursing home, with just 2 percent traveling more than 30 miles, suggesting that nursing homes in lower wealth areas may struggle to attract non-Medicaid residents. Given this substantial set of frictions slowing adjustments in nursing home resident pool characteristics, it seems reasonable to expect that my calculated effective subsidy rates do not substantially overstate, at least in the short run, the degree of variation in subsidization across nursing homes in daily rate and hourly rate subsidy states.

II.IV Research Design

My primary research design is a within-state empirical approach that uses across-nursing home variation in effective subsidy rates to identify the effect of subsidies on direct care worker wages and employment per resident-day through comparison of differentially subsidized nursing homes within subsidy states. The details of this approach will be described in subsection IV.I below. However, it is worth noting immediately that the critical identifying assumption for this design is that nursing homes offered different effective subsidy rates are on parallel trends in terms of direct care worker wages and employment per resident-day. Given the nature of the state subsidy formulas, this assumption is similar to requiring nursing homes with different shares of residents on Medicaid to be on similar direct care worker wage and employment trends. Though my within-state approach relies on this assumption, it has the benefit of using a source of variation orthogonal to any nursing home industry level shocks that may be generated by the state-wide changes in labor market conditions that Miller et al. (2012a) suggest coincide with nursing home payroll subsidy adoption.

In addition to this within-state approach, I also detail two secondary across-state research designs in sections IV.II-IV.IV. The first is an across-state extension of my within-state empirical approach that compares nursing homes in subsidy states to nursing homes in control states with characteristics that would have caused them to receive similar subsidy amounts had their state adopted a similar payroll subsidy. This across-state extension replaces the parallel trends assumption of my within-state research design with one that allows for differentially subsidized nursing homes to be on different wage and employment trends, provided those trends are common across states to nursing homes with similar characteristics. This alternate research design is useful for establishing robustness to national, regional, and other trends that may differentially affect nursing homes with greater exposure to the Medicaid program. The second design is a traditional state-level difference-in-differences analysis, included for comparability with prior literature despite concerns about the validity of this approach's parallel trends assumption and despite that the bulk of variation in subsidy amounts occurs within-state rather than across-state. Note that results from this approach are similar in size to those found by Foster and Lee (2015) and may be found in Appendix II.XIII.

II.IV.I The Within-State Empirical Approach

My baseline within-state model regresses nursing home level outcomes on the maximum subsidy amount offered to each nursing home conditional on state-by-year fixed effects and nursing home-by-policy event fixed effects. This regression is specified below:

$$(1) \quad Y_{i,t} = \beta_1 * subsidy_{i,t} + \mu_{i,w} + \gamma_{t,s} + \epsilon_{i,t,w}$$

In this equation, i indexes nursing homes, t indexes years, s indexes states, and w indexes policy event specific windows. This regression is estimated on a sample including only observations that fall within these event specific windows, which extend for a two-year symmetric period around subsidy adoption, repeal, or modification.[20] Note that indexing these treatment windows in addition to indexing states is important because Massachusetts and Montana include two subsidy events each.[21] In equation (1), the term $Y_{i,t}$ is a time-varying nursing home level outcome variable, $subsidy_{i,t}$ is the simulated maximum effective subsidy amount offered to nursing home i in year t measured in 2017 dollars per nursing home resident, β_1 is the effect on $Y_{i,t}$ of increasing the maximum effective subsidy amount per resident-day offered to a nursing home by \$1, $\mu_{i,w}$ is a nursing home fixed effect that is allowed to vary across event windows in any state with more than one treatment event, $\gamma_{t,s}$ is a state-by-year fixed effect, and $\epsilon_{i,t,w}$ is an idiosyncratic nursing home level error term which exhibits policy event window level clustering.

I calculate two sets of standard errors and test statistics for these regressions. The first set is based on standard errors that I calculate analytically and cluster at the policy event level, meaning one cluster for each state subsidy formula change. Since subsidy rates are assigned to nursing homes at the policy event level, there is a clear design-based justification for clustering at this level (Abadie et al., 2017). However, because effective subsidy amounts are not constant within states for any policy events and since the policy events I observe are a large share of all events which occur, clustering at this level

[20]Treatment windows were limited to this two-year length in order to allow nursing homes time to respond to the subsidies without compromising identification by extending the windows so far as to include confounding policy changes. Note that in many cases, subsidies are adopted in the middle of a calendar year or adopted and then expanded the next year. In every case, the two-year windows are constructed to take the first calendar year when any subsidies are paid as the first treatment year.

[21]Both states' subsidy event study windows are non-overlapping and separated by a number of years. Appendix II.XII.V shows results are robust to dropping the second event window.

using the standard Liang-Zeger method likely understates the amount of available independent variation within my sample and should produce overly conservative standard errors (Abadie et al., 2017). As such, I produce a second set of test statistics using a randomization inference approach inspired by Fisher (1925) and similar to what is employed by Robbins et al. (2017). Namely, I rank the genuine treatment group's coefficient within a distribution of 1000 placebo coefficients as well as provide the standard deviation of that placebo distribution. These test statistics, being based off the empirical distribution of coefficients observed when randomly assigning control states into treatment using the same type of clustering observed in the actual treatment states, should address the design-based clustering concerns without raising the same over-clustering concerns. As such, they are my preferred test statistics. For more details on this procedure and on why alternatives such as wild cluster bootstrapping are unreliable in this setting, please refer to Appendix II.XII.I.

One reasonable concern with the within-state regression is that estimation of β_1 will be biased by the endogeneity of $subsidy_{i,t}$ to potential nursing home policy responses, owing to nursing home resident counts and Medicaid resident shares entering into the effective subsidy formula. Nursing homes selection into higher subsidy rates may cause estimates of β_1 to be biased if this selection is systematically associated with nursing home employment and wage levels. Additionally, since the formula for effective subsidy amounts divides by resident-day counts in some states, this may generate division bias when examining per resident-day outcomes, thereby mechanically linking subsidy rates and some outcome measures.

In order to address these specific problems with the reduced form regression, I adopt an instrumental variable approach that purges from the variation in subsidy rates any selection effects and that reduces the extent of any mechanical bias. Specifically, I instrument for the simulated nursing home effective subsidy rates $subsidy_{i,t}$ using $subsidy^*_i$, the maximum effective subsidy amount offered to firm i calculated using the actual subsidy formula and its pre-treatment characteristics. The first and second stage instrumental variable regression equations used thus are as follows:

$$\text{(1a)} \quad subsidy_{i,t} = \beta_{stage1} * subsidy^*_i + \mu_{i,w} + \gamma_{t,s} + \eta_{i,t}$$

$$\text{(1b)} \quad Y_{i,t} = \beta_1 * \widehat{subsidy}_{i,t} + \mu_{i,w} + \gamma_{t,s} + \epsilon_{i,t,w}$$

Note that when calculating $subsidy^*_i$, I replace all time-varying nursing home characteristics in the subsidy formula with their average from the period two to four years prior to the policy change. I begin this period two years prior to treatment in order to minimize the risk that any policy anticipation effects enter into the instrument. I then average over a three-year range in order to reduce the influence of any idiosyncratic nursing home annual shocks on the instrument as well as to ensure that nursing homes which happen to have a year of missing data will still have a valid instrument calculated as an average over the remaining available years. The F-statistic associated with this first stage regression is 49.7,[22] easily exceeding the conventional weak instrument test threshold of 10, as might be expected given the stickiness of nursing home resident pool characteristics over relatively short time periods.

[22]This figure is the first stage regression F-statistic conditional on state-by-year and nursing home-by-policy event fixed effects, within the subset of years when subsidies are being offered. Repeating this regression within the full treatment sample increases the F-statistic, since the simulated subsidy and simulated subsidy instrument are both 0 when subsidies are not being offered.

II.IV.II The Within-and-Across-State Empirical Approach

My second research design is an extension of my within-state empirical approach. In this approach, for each treatment state adopting or repealing a payroll subsidy, I select a set of control states. The set of states included in the control group varies by specification, as detailed in subsection IV.IV, but never includes states in which other major policy changes occurred during the subsidy event study window. Within these control states, I use their paired treatment state's subsidy formula to simulate placebo subsidies that are the effective subsidy rates the control states' nursing homes would have been offered had each control state adopted their paired treatment state's payroll subsidy design.[23] I then regress nursing home level outcomes on the simulated effective subsidy rates that would have been offered to nursing homes had all control states adopted subsidies, an interaction between the simulated subsidy rates and a dummy variable that is 1 for states that actually offered subsidies and 0 otherwise, and then a set of state-by-year and nursing home fixed effects. The regression equation used is:

$$Y_{i,t} = \beta_1 * subsidy_{i,t,p} + \beta_2 * treated\_state_{i,p} * subsidy_{i,t,p} + \mu_{i,p} + \gamma_{t,s,p} + \epsilon_{i,t,p} \quad (2)$$

In this equation, i indexes nursing homes, t indexes years, s indexes states, and p indexes the treatment and control state group pairs for each policy event. As before, $Y_{i,t}$ is a time-varying nursing home level outcome variable. The variable $subsidy_{i,t,p}$ is the effective subsidy amount (measured again in 2017 dollars per nursing home resident-day) that would be offered to nursing home i in year t as calculated using the subsidy formula for the treated state in treatment-control pair p. The variable $treated\_state_{i,p}$ is an indicator

[23]When the treatment state uses an allocated payment style subsidy formula, I adjust the statewide total subsidy amount in the control state so that it matches expenditure in the treatment state in per Medicaid resident-day terms.

that is 1 if nursing home i is in a state that genuinely offered subsidies within that pair p and is 0 otherwise. As such, β_1 is the estimated effect of placebo subsidy receipt in the control states and so presumably reflects the effect of any pre-trends associated with subsidy receipt, of any nursing home industry shocks correlated with subsidy receipt occurring at the same time as subsidy adoption, or of mechanical endogeneity between outcomes and treatment generated by the subsidy formulas. The coefficient β_2 on the interaction term is the genuine effect of subsidy receipt in the treatment states, estimated relative to the placebo effect. The term $\mu_{i,p}$ is a nursing home-specific fixed effect that is allowed to vary across treatment windows in treatment states and control states. Finally, $\gamma_{t,s,p}$ is a year fixed effect for each treatment state and for each control state and $\epsilon_{i,t,p}$ is an error term. The regression specified above also suffers from the same problem as the within-state regression with subsidy endogeneity to nursing home characteristics. I address this problem the same way as in the within-state regressions by instrumenting for $subsidy_{i,t,p}$ with a simulated subsidy calculated using only pre-treatment nursing home characteristics. Finally, in each case, I analytically calculate clustered standard errors with one cluster for each treatment event and one cluster for each treatment event's control group.

II.IV.III The Difference-in-Differences Approach

For comparability with prior literature, I also estimate a traditional difference-in-differences specification where nursing home level outcome variables are regressed on a dummy variable that is 1 in states and years where subsidies are in place and 0 otherwise[24] along with nursing home fixed effects and year by treatment control group pair fixed effects. The difference-in-differences regression equation is specified below:

[24]In the case of Montana's second policy event, where an existing payroll subsidy is cut but not fully eliminated, this indicator is 1 prior to the cut and 0 afterward.

$$(3) \quad Y_{i,t} = \beta_1 * policy_{s,t} + \mu_{i,p} + \gamma_{t,p} + \epsilon_{i,t,p}$$

In this equation, i indexes nursing homes, t indexes years, s indexes states, and p indexes the treatment and control state group pairs for each policy event. Similar to as in the prior research designs, $Y_{i,t}$ is an outcome variable varying at the nursing home by year level, $policy_{s,t}$ is an indicator variable that is 1 when a subsidy is being offered in state s in year t and 0 otherwise, $\mu_{i,p}$ is a nursing home fixed effect that is allowed to vary across treatment events, $\gamma_{t,p}$ is a year by treatment-control group pair fixed effect, and $\epsilon_{i,t,p}$ is an error term. Note that calculation of standard errors for this regression will vary by choice of control group and will be discussed when these are introduced below. As expected, this research design yields results attenuated relative to the within-state and within-and-across state research designs, with the full results obtained using this approach being available in Appendix II.XIII.

II.IV.IV Choice of Control States

I estimate each of the two types of across-state regressions on one of three different samples of control states. In the first sample, for each policy event, I pair nursing homes from treatment states with nursing homes in the same years from all non-subsidy states that have no other major policy changes occurring at the same time.[25] This control group has the benefit of helping to control for any national pre-trends, or shocks contemporaneous to subsidy adoption, correlated with the characteristics that cause nursing homes to receive different subsidy amounts in subsidy states.

[25] When several treatment states make policy changes at the same time, control state nursing homes will appear more than once in the sample. However, these duplicate nursing homes have different simulated placebo subsidy amounts depending on which treatment event they are paired with and do not share fixed effects in common.

In the second sample, I pair treatment states with geographically neighboring states that did not undergo other major policy changes of their own. The motivation behind this approach is to better control for any pre-trends or shocks correlated with subsidy receipt that are common among geographically proximal states due to regional labor market or other factors.

Finally, in the third control set, I pair treatment states with a statistically constructed synthetic control state developed in the fashion of Abadie et al.'s (2010) synthetic control method. For the within-and-across-state approach, I construct synthetic control states that match the treatment states as best as possible in terms of a set of conditional correlations between nursing homes' pre-trends in a number of key variables and the instrumented subsidy amounts offered to them, conditioning on state and nursing home fixed effects. For the difference-in-differences case, I construct synthetic controls that as best as possible match the treatment states in terms of average pre-trends across all nursing homes in the state. The motivation for constructing these synthetic control states is to build for each subsidy state a control whose nursing homes are on similar wage, employment, and other trends as their counterpart nursing homes in the paired treatment state, thereby controlling for differences in trends associated with subsidy receipt regardless of their source.

I construct my synthetic control sample according to the following procedure. First, for each policy event, I identify a set of candidate synthetic control donor states that do not have other policy events occurring during the two-year symmetric window around when the treatment state makes its policy change. Second, for each treatment and candidate donor state associated with each policy event, I calculate a set of data moments to match between the two. In particular, for each state and each policy event, I calculate two moments for each of seven variables. The seven variables used are:

- Minutes of direct care worker employment per resident-day
- Minutes of nurse employment per resident-day
- Minutes of nursing assistant employment per resident-day
- Average nursing home direct care worker hourly wage
- Total direct care worker payroll per resident-day
- Total number of nursing home residents
- Share of nursing home residents on Medicaid

These seven variables were chosen because they are either outcomes of interest or, in the case of resident counts and shares of residents on Medicaid, because they are important inputs into the subsidy formulas. The two moments calculated for each variable above in each state and for each policy event are:

- Coefficient $\beta_{Subsidy}$ from the following regression, which is estimated in each state: $X_{i,t} = \beta_0 + \beta_{Subsidy} * subsidy_{i,t} + \mu_i + \eta_t$ where $X_{i,t}$ is one of the seven outcome variables above, $subsidy_{i,t}$ is the simulated subsidy amount offered to nursing home i based on the subsidy formula for the policy event for which moments are being calculated, μ_i is a nursing home fixed effect, and η_t is a year fixed effect. Note that for each state, this regression is estimated within a one-year symmetric window of data centered around the year prior to when the policy event actually occurred. Additionally, in these regressions I instrument for $subsidy_{i,t}$ using $subsidy_i^*$, which is the simulated subsidy amount for nursing home i using only pre-policy event nursing home characteristics, constructed in the same way as in the within-state regressions.

- Coefficient β_{DiD} from the following regression, estimated separately within each state: $X_{i,t} = \beta_0 + \beta_{DiD} * policy_t + \mu_i$ where $X_{i,t}$ is one of the seven outcome variables above, $policy_t$ is a dummy variable that is 1 when payroll subsidies are being offered and 0 otherwise, and where μ_i is a nursing home-specific fixed effect. Note that for each state, this regression is estimated within a one-year symmetric window of data centered around the year prior to when the policy event actually occurred.

Next, I apply the synthetic controls method of Abadie et al. (2010) to the set of seven $\beta_{Subsidy}$ coefficients to develop synthetic control weights that, when applied to the synthetic control donor states, yield a weighted combination of the donor states that as closely as possible matches the genuine treatment state in terms of the distribution of pre-trends across nursing homes receiving varying subsidy rates.[26] Note that since using these $\beta_{Subsidy}$ coefficients as synthetic control moments is a form of matching on conditional correlations, it may intuitively be thought of as related to matching based on the cross products of the instrumented subsidy amounts and the seven different matching variables. In addition to performing this procedure for the $\beta_{Subsidy}$ coefficients, I repeat this procedure using the β_{DiD} coefficients as the synthetic control moments.

Finally, I build the synthetic control states used in the within-and-across-state regressions by applying the state-level synthetic control weights from the $\beta_{Subsidy}$ case as probability weights to each state's set of nursing homes. I do the same for the difference-in-differences regressions but using the weights from the β_{DiD} case. Note that variation in the number of nursing homes across states may cause larger synthetic control donor states to contribute more to their synthetic control than smaller states, thereby causing their con-

[26]When calculating each set of synthetic control weights, rather than using Abadie et al.'s weighting procedure, I weight all moments to be of equal importance so as to ensure the weights I use are constant regardless of my outcome variable.

tribution to the synthetic control state to vary from that required by the synthetic control weights. In order to address this problem, I re-weight each synthetic control donor state's nursing homes so that they constitute an equal share by weight of the final synthetic control state's donor pool. Specifically, I do this by multiplying each synthetic control donor state's weight by the average number of nursing homes in each donor state within its synthetic control divided by its total number of nursing homes. With these treatment event specific synthetic control states constructed, the final dataset used for the difference-in-differences and within-and-across-state specifications consists of the nursing home-year observations from each treatment event and from its paired synthetic control, coupled with the appropriate weights for each synthetic control.

The synthetic control procedure above, in addition to building off of Abadie et al.'s (2010) foundational work on the method, is similar to the approach of Robbins et al. (2017) for constructing synthetic controls with a sample of many treated locations. Like in Robbins et al. (2017), I conduct inference on my synthetic control coefficient estimates using a randomization inference type procedure that compares the genuine estimated synthetic control coefficient to a distribution of 1000 placebo coefficients. I estimate each placebo coefficient by replacing the observations from each treatment event with data from a randomly selected control state, applying the original treatment state's subsidy formula to the new placebo state, constructing for that placebo state a synthetic control comparison group of its own according to the same procedure as above, and then estimating my regressions using the paired placebo and synthetic control sample. I conduct inference in this fashion because, as Robbins et al. (2017) observe, calculating standard errors by clustering at the treatment and synthetic control levels may fail to capture all variation in the estimated coefficient created by the procedure for generating synthetic controls. As such, I report the rank of each estimated coefficient within the distribution of placebo estimates, report the standard deviation of the placebo distribution, and take the estimated coefficients to be statistically significant at the 5 percent level if it falls in the top or bottom 2.5 percent of the placebo coefficient distribution.

II.V Data and Measurement

I gather data on nursing home direct care worker employment per resident-day, resident counts, and shares of residents on Medicaid from a longitudinal dataset of nursing home-year observations spanning the universe of nursing homes certified to accept either Medicare or Medicaid residents. These observations are drawn from a set of annual nursing home surveys conducted by state agencies. Responses are self-reported by nursing homes to inspectors during nursing homes' annual Centers for Medicare and Medicaid Services (CMS) certification inspections and refer to values from a two week reference period prior to inspection. These data also include information on nursing home resident average acuity (a measure of the average severity of resident care needs), nursing home occupancy rates, nursing home addresses, whether or not a nursing home is owned by a for-profit company, and other items. After collection by state agencies, these data are then reported to CMS and stored in their Certification and Survey Provider Enhanced Reporting (CASPER) database, formerly known as the Online Survey Certification and Reporting (OSCAR) database.

I obtained historical copies of CMS's OSCAR/CASPER nursing home survey data from Brown University's publicly available Long Term Care Focus (LTCFocus) database for the years 2000 to 2015 and from the Cowles Research Group for the years 1996-1999. The raw survey data are not processed by CMS in any substantial way and suffer from a number of data quality issues that require correction. These corrections are detailed in Appendix II.XI and consist of a mix of dropping observations reporting implausible staffing levels, dropping duplicate observations from the data, and manually correcting observations reporting clearly erroneous shares of residents on Medicaid.[27]

In addition to these nursing home survey data, I obtained data on nursing home direct care worker payroll and average hourly wages for most free-standing Medicare-certified nursing homes from 1996-2015.[28] These data are drawn from CMS's Healthcare Cost Reporting Information System (HCRIS) database, which is composed of formal cost reports submitted by nursing homes to Medicare. I inflation adjust dollar figures in these data using the Consumer Price Index so that they are all denominated in 2017 dollars and then convert the total payroll figures into payroll per resident-day terms. Unfortunately, the HCRIS wage and payroll data are affected by a high reporting error rate. These errors sometimes generate extreme outliers, such as $10,000 average hourly wages, which greatly affect standard errors and state-specific data moments. Consistent with practice in prior literature (e.g., see Dafny et al., 2016), I trim the top and bottom 5 percent of the payroll per resident-day and average hourly wage data within each state and year, thereby restricting the distribution of these variables to remain within a qualitatively reasonable range.[29] Note

[27]For example, nursing homes reported resident counts by Medicaid, Medicare, and other payer type across three columns on a worksheet. They sometimes transposed the values across these columns, implying implausibly dramatic shifts in nursing home resident pool composition that last for only a single year, which I correct by transposing back to match the nursing home's more general pattern.

[28]The definition of direct care workers used in this payroll data are broader than what I can observe in the Medicaid data. In the Medicaid survey data available to me, I can separately observe employment of nursing assistants, licensed practical nurses, and registered nurses. The payroll data, however, report values for all of these workers bundled together and including medication assistants, certain types of contractors that may be performing some administrative duties, and a small number of other related workers.

[29]I apply a constant level of trimming within each state to maintain a comparable level of trimming in each sample even as the set of control states change. I use the 5 percent trimming threshold based off of the judgment in prior literature that it best eliminates spurious observations from the HCRIS dataset. Results are robust to varying this trimming threshold. In general, reducing the degree of trimming increases both standard errors and point estimates in the within-state wage and payroll regressions.

that the sample of nursing homes with payroll data is 39 percent smaller than the full sample, due primarily to missing observations in the cost report data for nursing homes that do not accept Medicare residents as well as for smaller, often highly Medicare-focused nursing homes that face less comprehensive reporting requirements.

Given these employment and wage data, I construct my estimation sample and simulated payroll subsidy rates as follows. First, for a payroll subsidy policy change to be eligible for inclusion in the treatment group, its payroll subsidy formula must have been specified in a way that permits me to simulate subsidy rates for specific nursing homes.[30] Second, for a subsidy to be eligible for inclusion, it must be the case that no substantial confounding policy changes occurred when the subsidy policy changed, in the year after the change, or in the four years prior to the policy change.[31] While only the two years immediately prior to treatment enter into the symmetric treatment windows used in my primary regressions, the additional years prior to a window's start are kept free of confounding policy events in order to minimize the risk of policies designed to phase in over time affecting nursing homes in my estimation sample.[32] Applying these requirements limits the set of 25 states adopting payroll subsidies of some kind to just 12, with 14 policy events among them.[33] A table listing the set of treated states and some details about each of their policy regimes is available in Appendix II.X.III. For these policy events, simulat-

[30]This requirement notably excludes a subsidy adopted in Texas, where subsidy amounts were calculated using a partially recursive formula that depended on detailed resident medical characteristics and wage rates for particular types of workers. It also excluded a number of states that conditioned payments on employee satisfaction surveys and staff turnover rates.

[31]Please refer to Appendix II.X.II for details on which policy events are considered to be substantial enough to be a confounding event. Broadly, they consist of other major Medicaid policy changes that are likely to disproportionately affect nursing homes with large shares of their residents on Medicaid and so which also would be likely to receive relatively large effective subsidy amounts.

[32]In practice, nearly all treatment events excluded from the sample are excluded due to a confounding event occurring within one year of the change in payroll subsidy policy.

[33]While I use all 14 policy events in my main estimation sample, all primary results are robust to restricting the sample to one event per state. See Appendix II.XII.V for details.

ing the maximum effective subsidy amounts offered to nursing homes is a straightforward matter of applying the state subsidy formulas to the observed nursing home characteristics and then inflation adjusting the subsidy amounts to 2017 dollars. Note that I obtained details on state payroll subsidy formulas and information on the presence of confounding policy events in all 50 states from 1996 to 2015 through extensive archival research, relying mainly on primary source documents produced by state Medicaid agencies. For more details on the process of collecting this policy regime information, please refer to Appendix II.X.I.

With an understanding of the source data and sample now in hand, it is useful to consider some summary statistics for the nursing homes in my estimation sample. Table II.1 presents summary statistics for the full sample of nursing homes in subsidy states within the two-year symmetric windows around each policy event, as well as separately for the subset of nursing homes for which payroll data is available. The average nursing home within the treatment sample offers 197 minutes of direct care worker employment per nursing home resident-day, with that staffing being split between certified nursing assistants (CNAs) and licensed nurses in an approximately 2:1 ratio. The average direct care worker hourly wage paid at these nursing homes was $22.21 per hour, the average resident count was 88, the average share of residents on Medicaid was 62 percent, and the average increase (or decrease) in subsidies offered was $2.32 per resident-day. Nursing homes in the sample with payroll data are similar, though have 13 more residents on average. This similarity in terms of shares of residents on Medicaid and Medicare may be surprising, given the cost report data lose non-Medicare-certified nursing homes. However, the loss of these non-Medicare nursing homes is offset by the loss of many low resident count, very high Medicare share nursing homes not reporting wage and payroll data in the HCRIS dataset. In addition to these within-treatment sample summary statistics, Table II.2 presents summary statistics comparing the full treatment sample of nursing homes to all nursing homes from the same years in other states. The two samples are very similar. Finally, Figure II.1 visually presents the main source of variation used in my within-state empirical approach, graphing the simulated subsidy amounts offered to nursing homes against nursing homes' average shares of residents on Medicaid.[34] In this figure, variation from daily rate states is visible in the form of nursing home observations arrayed on lines, with each line being generated by some state's fixed daily rate subsidy amount being interacted with nursing home's share of residents on Medicaid. Variation from hourly rate and allocated payment rate states is also visible, though is not arrayed on lines in the same way as in daily rate states due to the dependence of subsidy amounts in these states on more than just Medicaid resident shares.

II.VI Results

II.VI.I Within-State Primary Results

My principal findings on the employment effects of subsidies are listed in Table II.3. Table II.3 reports the effect of a $1 increase in effective subsidy amount offered per nursing home resident-day on minutes of direct care worker employment per resident-day (DCW Staffing), nursing assistant employment per resident-day (CNA Staffing), and licensed nurse employment per resident-day (Nurse Staffing).[35] The instrumental variable

[34]When states repeal subsidies, the pre-repeal value of the subsidy is shown. For the second of Montana's two policy events, Montana cut its subsidies but did not entirely repeal them. In this case, the change in subsidies is shown.

[35]The measure of nurse employment per resident-day pools licensed practical nurse and registered nurse employment. I pool these worker types because while the difference in nursing home employment levels and nursing home workplace duties of nursing assistants versus nurses tends to be consistent across states and time, this is much less the case for licensed practical nurses versus registered nurses. Subsidy effects

(IV) regressions indicate that a $1 increase in subsidy per resident-day causes a statistically significant 4.9 minute increase in direct care worker employment per resident-day, composed of a 3.1 minute nursing assistant effect and a 1.8 minute nurse effect. Each of these estimates is statistically significant per the randomization inference procedure, with each representing the largest effect size observed out of a pool of 1000 placebo estimates.

Table II.3 also presents the ordinary least squares (OLS) regression results for each outcome. The OLS estimates are in all cases larger than the IV estimates. To the extent that this difference is meaningful to comment on, it could be suggestive of either greater responsiveness to subsidies by nursing homes selecting into higher subsidy rates, positive selection on staffing into higher subsidy rates, or some sort of mechanical bias generated by the construction of the treatment and outcome variables. Note that while the OLS estimates exhibit standard errors of similar size to the instrumental variable estimates, the OLS estimates of the effect of subsidies on direct care worker and nursing assistant employment per resident-day are not statistically significant in the randomization inference tests.

Additionally, Table II.3 reports the mean effect of the policy across nursing homes, calculated by scaling the simulated subsidy rates offered to nursing homes by the estimated regression coefficients and dividing by the corresponding outcome variables. The mean effect of the subsidies, per the instrumental variable estimates, was to increase nursing home staffing of direct care workers and nursing assistants by approximately 6.4 percent and to increase it for nurses by approximately 7.4 percent, figures several times larger than those estimated in Foster and Lee (2015).

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qualitatively similar to the pooled effects are observed for both worker types, though appear to be stronger for registered nurses.

Table II.4 presents the same regressions as Table II.3, but using average direct care worker hourly wages (Avg DCW Wage) and direct care worker payroll per resident-day (DCW PPR) as outcome variables.[36] The instrumental variable average hourly wages result suggests that for each subsidy dollar offered per resident-day, nursing homes increased the average wage paid to direct care workers by 13 cents, equivalent to a 1.5 percent wage increase at the mean nursing home. This effect is statistically significant and similar to the effect estimated in the OLS case. Note that because these wage estimates are for the average wage paid to all direct care worker nursing home employees, I cannot distinguish between wage increases for a fixed pool of employees and compositional changes in that pool of workers toward employees receiving higher wages. However, the relative homogeneity of the employment effects across nurses and nursing assistants in percentage terms, as reported in Table II.3, suggests changes in workforce composition are unlikely to be the explanation. Meanwhile, the effect of a $1 subsidy per resident-day on direct care worker payroll per resident-day is 1.12 in the instrumental variable estimates but statistically insignificant and very imprecisely estimated, perhaps due to the payroll data sample being smaller and subject to greater measurement error than the staffing data. The ordinary least squares regression yields a larger payroll per resident-day estimate, though it also is not statistically significant in the randomization inference test. Unfortunately, these payroll per resident-day coefficients are estimated with too little precision to determine if nursing homes are induced to spend more or less than one dollar per dollar of subsidy offered.

[36] Results estimating employment effects within the sample of nursing homes for which there is payroll data available may be found in Table II.5. They are larger than the results for the full sample though the nurse and nursing assistant employment effects are similar to one another in relative terms.

Given these subsidy effect estimates, I calculate what they imply in terms of elasticities of nursing home employment, wages, and payroll to subsidies for the mean nursing home. When calculating these elasticities, I calculate percentage changes in subsidy amounts as being made relative to states' average Medicaid payment rates for a day of nursing home resident care.[37] I calculate that the elasticity of direct care worker employment per resident-day to subsidy payments on top of state average Medicaid payment rates is 4.5. The elasticities for licensed nurse employment and nursing assistant employment are remarkably similar, being 4.5 and 4.7 respectively. Finally, the implied elasticity of average direct care worker hourly wages to subsidies is 1.1, while the same elasticity implied by the (statistically insignificant) point-estimate for the effect on payroll per resident-day is 1.5.[38] These employment elasticities in particular are quite large but not implausible relative to the elasticities estimated by Hackmann (2018).[39] In particular, Hackmann estimates that the elasticity of nurse staffing to a nursing home-specific increase in Medicaid payment rates is approximately 1.1, or 0.9 for the elasticity to a universal increase in Medicaid payment rates. Hackmann further estimates that 45 percent of these general rate increases are captured by nursing homes as profits, with the residual 55 percent going

[37]Specifically, for each outcome variable, I calculate these elasticities as follows. First, I take the outcome variable's corresponding instrumental variable coefficient, scale it by the mean subsidy amount offered, and then divide by the mean of the outcome variable across all nursing homes and across all years in the sample. Then, I obtain the final elasticity by dividing this by the mean subsidy amount offered divided by the mean of the state average per resident-day Medicaid payment rate across all nursing homes in all states and years in the sample. The data on average Medicaid per resident-day payment rates for each state were obtained from LTCFocus and are available only from 2000 to 2009. I obtain estimates of the average rates from 1996-1999 and 2010-2015 by projecting the observed 2000 and 2009 average rates backward and forward respectively using the mean annual change in payment rates observed in each state from 2000 to 2009.

[38]The elasticities obtained by re-estimating the primary within-state regressions after summing the simulated subsidy amounts with state average Medicaid per resident-day payment rates and then logging all variables are similar to these and may be found in Appendix II.XII.III.

[39]Note that this is true only for the nurse staffing elasticities. The large and similarly sized elasticities for both nurses and nursing assistants in this study are unique. Both Hackmann and Foster and Lee (2015) estimate very differently sized nurse and nursing assistant effects, with Hackmann finding only a very small nursing assistant elasticity.

toward a mix of employment per resident-day increases and private pay nursing home price reductions. The large magnitude of the elasticities found in this study relative to Hackmann's, at least in the case of nurses, could be explained if the incremental nature of the payroll subsidies made it difficult for nursing homes to divert them into increased profits or reduced prices. Indeed, the relative magnitudes of these employment elasticities as well as the fact that the coefficient for the effect of subsidies on direct care worker payroll per resident-day exceeds one both are suggestive of subsidy pass-through rates of 100% though, once again, the imprecision in my estimate of the payroll coefficient prevents drawing firm conclusions.[40]

All of the subsidy effect estimates reported so far examine the impact of subsidies at the average subsidized nursing home. Potentially, nursing home responses to subsidies may vary by nursing home size. If so, from a nursing home resident welfare perspective, it is likely more important to consider the effect of subsidies on nursing home employment and wages experienced by the average nursing home resident. In order to examine the effects experienced by the average resident, I re-estimate all of the primary within-state regressions while weighting nursing homes by their within-sample mean number of residents and report the instrumental variable results in Table II.6. These results are somewhat larger than in the unweighted case, suggesting that the average nursing home resident experienced similar or greater increases in direct care worker employment per resident-day than occurred at the average nursing home. Additionally, note that for this and all ensuing tables, I report only the instrumental variable subsidy effect estimates. I do this in the in-

[40]Unfortunately, I do not directly observe nursing home profits or private pay prices and so cannot test how either of these respond to subsidies. However, I can test for subsidy effects on certain other margins that nursing homes might adjust in response to subsidies: resident counts, occupancy, share of residents on Medicaid, and nursing home resident average care needs. These effects are discussed in Appendix II.XII.II. Results from these regressions are inconclusive: estimated effects are statistically insignificant but imprecisely estimated.

terest of brevity and because the relationship between the instrumental variable and ordinary least squares results observed in Tables II.3 and II.4 continues essentially unchanged through the ensuing analyses.

II.VI.II Within-State Heterogeneous Effects Results

The incentives for nursing homes to respond to payroll subsidies likely vary by the type of payroll subsidy they are facing. Nursing homes in daily rate subsidy states and hourly rate subsidy states are required to match subsidy dollars received for their Medicaid residents with additional expenditure on non-Medicaid residents. Nursing homes in these states thus may be expected to exhibit larger responses to subsidies than nursing homes in allocated payment subsidy states provided that these matching requirements do not deter nursing homes from taking up the subsidies. However, if the matching requirements do reduce take-up, then allocated payment type subsidies might elicit greater responses than the hourly rate and daily rate subsidies. I test if nursing homes exhibit different responses to allocated payment style subsidies by re-estimating my primary regressions after interacting the simulated subsidy amount variable with an indicator variable that is 1 if a nursing home is in an allocated payment style subsidy state and 0 otherwise. For the instrumental variable regressions, in the first stage regressions for simulated subsidies and the simulated subsidy interaction term, I include both the subsidy instrument and the instrument interacted with the indicator variable. In terms of the relative prevalence of different subsidy types, note that approximately 39 percent of nursing homes in the treatment sample are from allocated payment style subsidy states, with 11 percent being from hourly rate style subsidy states and the remaining 50% being from daily rate style subsidy states.[41]

I report the results from these subsidy type interaction regressions in Table II.7. The estimated effects for the daily rate and hourly rate style states are substantially larger than in the pooled regressions, with the effect of a $1 increase in subsidies on direct care worker employment per resident-day rising to 8.0 minutes and the effect on average direct care worker hourly wages rising to 23 cents. The interaction terms, meanwhile, all suggest that allocated payment style subsidies are markedly less effective than daily rate and hourly rate style subsidies, with the effect of allocated payment subsidies on direct care worker employment per resident-day being a statistically significant 5.3 minutes lower per subsidy dollar. The point estimate of the average hourly wages effect is also 16 cents lower, though this interaction is not statistically significant. These results imply that the effect of allocated payment style subsidies was to increase direct care worker employment per resident-day by only 2.7 minutes per subsidy dollar per resident-day and to increase average direct care worker hourly wages by only 7 cents per subsidy dollar per resident-day. Given the large magnitude of these interaction terms, I test to see if the allocated payment style subsidy effects, obtained by summing the interaction terms with the baseline effects, retain their statistical significance. All of these effects lose their significance at the five percent level in the randomization inference tests, though the subsidy effect on direct care worker employment does retain its significance at the ten percent level with a ranking at the 97th percentile of the placebo distribution.

[41]Note that this distribution of nursing homes across subsidy types reflects why I do not present results separating effects by daily rate and hourly rate types. Separately estimating the effect of hourly rate and daily rate style subsidies is severely hampered by low statistical power resulting from there being relatively few hourly rate states and relatively few nursing homes in those states. For more details on which states adopted which type of subsidy, please refer to Appendix II.X.III.

The interaction results in Table II.7 suggest that daily rate and hourly rate subsidy states' expenditure matching requirements elicit large subsidy responses from nursing homes. They further suggest that subsidy take up was high in daily rate and hourly rate style subsidy states. However, these results should be interpreted with some caution. First, the smaller effect estimates for allocated payment style subsidies might partially result from greater measurement error in my simulated subsidy amounts for nursing homes in these states. Allocated payment state subsidy amounts depend on nursing home characteristics during a base year, while I observe nursing home characteristics only for the two-week reporting window before a nursing home survey. To the extent that nursing home characteristics during survey windows vary from their annual average characteristics, this introduces a source of measurement error that may partially attenuate subsidy effect estimates. Note that this type of measurement error does not affect daily rate or hourly rate subsidy states, as subsidy amounts offered to nursing homes in these states depend only on their characteristics at the time of the offer. Second, to the extent that states select into subsidy types based on their Medicaid programs' ability to administer and enforce subsidy requirements, subsidy effectiveness may vary across subsidy types due to policy regime characteristics unrelated to the subsidy formulas themselves. While I do not expect either of these sources of bias to explain the entire reduction in subsidy effectiveness for allocated payment style subsidy states, one should keep in mind that the actual reduction may be overstated.

In addition to heterogeneity by subsidy characteristics, nursing homes' incentives to respond to payroll subsidies may also vary by nursing home characteristics. In particular, if not-for-profit homes have a greater altruistic motive to increase spending on nursing home staffing and care quality, for-profit nursing homes may be less responsive to subsidies than not-for-profit nursing homes.[42]

Similar to how I test for differences by subsidy design type, I test if for-profit nursing homes exhibit a smaller response to payroll subsidies by re-estimating my primary regressions with the addition of an interaction between the simulated subsidy amount variable and an indicator variable that is 1 if a nursing home is a for-profit nursing home and 0 otherwise. Note that two-thirds of nursing homes in the treatment sample are for-profit nursing homes. Table II.8 presents the results of these regressions. Coefficients on the interaction term between effective subsidy amounts offered and for-profit status are generally negative across all outcome variables, suggesting that for-profit nursing homes may not respond to payroll subsidies as aggressively as not-for-profit nursing homes. However, none of the estimates from these instrumental variable regressions are statistically significant per the randomization inference tests and so these results should only be taken as suggestive.

II.VI.III Within-State Assumption Validation Tests

The core assumption undergirding the within-state empirical analysis is that the effective payroll subsidy amounts offered to nursing homes are orthogonal to pre-trends in direct care worker wages and employment. I explore this assumption in Table II.9, where I report the estimates from five regressions estimated on different time periods for each category of direct care worker employment per resident-day and on average direct care worker hourly wages. Each row reports the result of a regression of the listed outcome

[42] A long literature has examined whether for-profit status affects hospital care quality and other margins of behavior, including Duggan (2002), and finds mixed results but with some evidence for differences. The literature examining this question for nursing homes is less developed.

variable on the instrumented subsidy amount offered within a one-year symmetric window centered around the adoption of a policy change or, depending on the row, around a different year before or after the change. Although it is possible to use data centered one year prior to the real policy change for nursing homes in all states, due to data time-span limitations, it is not possible to do so for two years prior in Utah and it is not possible to do so for three years prior in an additional five states. Differences between the three and two years prior coefficients thus should be interpreted with caution due to variation in sample composition across these regressions.

The estimates from Table II.9 suggest that there was at most a modest negative association between pre-trends in employment per resident-day and eventual subsidy receipt. Perhaps the most extreme pre-trend observed is for the effect of subsidies on direct care worker employment per resident-day estimated one year prior to the subsidies actually being offered is -1.9 minutes. Even in this case, the pre-trend effect is reasonably small relative to the magnitude of the primary employment effects and the pre-trends estimated for two and three years prior to subsidy adoption tend to be more mixed in size and sign. Overall, the evidence is suggestive of the treatment effects on employment being at most modestly attenuated by selection into subsidy adoption on negative employment per resident-day trends at the type of nursing homes eventually receiving higher subsidies. While this may raise some concerns that the contemporaneous treatment effects on employment may reflect some form of mean reversion, the contemporaneous effects are more than double the size of the one year prior pre-trends in absolute value and either persist or grow in the year after first adoption of treatment, a finding inconsistent with mean reversion. Meanwhile, the estimates for average direct care worker hourly wages also suggest that there is at most modest selection into subsidy adoption on positive wage trends at the type of nursing homes eventually receiving higher subsidies.

In addition to these formal tests, we can also visually inspect for the presence of pre-trends at any part of the distribution of effective subsidy amounts by nonparametrically regressing different outcome pre-trends on the change in subsidies offered to nursing homes. It is important to use the change in subsidy amounts in order to distinguish between cases where subsidies are repealed and where they are established, since the pre-trends of greatest concern are of opposite sign across these two cases. Figure II.2 displays the results of nonparametrically regressing the change in minutes of direct care worker employment per resident-day, calculated from two years to one year prior to treatment, on the change in subsidy amount experienced by each nursing home.[43] Note that the gray bands in this plot represent 95 percent confidence intervals, calculated without any use of clustering, while the dashed line is the kernel density of the distribution of changes in subsidy amounts offered.

Figure II.2 suggests that for nursing homes facing either eventual increases or decreases in subsidization, direct care worker employment per resident-day pre-trends were reasonably stable, if somewhat decreasing, in the magnitude of eventual subsidy change. The most dramatic differences in pre-trends were experienced for nursing homes in relatively sparse portions of the distribution of changes in subsidy amounts. Considering that there are more nursing homes in states adopting payroll subsidies than in states cutting them, these results cumulatively suggest that employment pre-trends are working against

[43]The nursing homes receiving the top 5 percent largest subsidy changes are trimmed from the figure, due to the sparseness of their distribution preventing reasonably precise nonparametric estimation. This is also true of Figure II.3.

the within-state approach finding a positive subsidy effect on employment, with the most severe pre-trend violations being contained to a relatively small number of nursing homes.

Figure II.3 presents similar results as in Figure II.2, but uses one-year changes in nursing home average direct care worker hourly wages as the outcome variable. The results from this figure qualitatively echo those in the employment per resident-day case, though pre-trends appear to be more decreasing in the dense part of the positive future subsidy change distribution here than in the employment case. This figure lends itself to similar conclusions as above but with stronger evidence suggesting that the estimated subsidy effect on wages may be an underestimate.

My nonparametric pre-trend checks suggest that the parallel trends assumption for nursing homes receiving different ultimate subsidy amounts is most violated for nursing homes qualifying for small subsidy amounts and so likely with very low shares of their residents on Medicaid. In order to test if these nursing homes are disproportionately driving my estimated effects, I adopt two approaches. First, I re-estimate my primary regressions, but drop the bottom 5 percent and then 10 percent of nursing homes by subsidy amounts offered as well as separately by shares of residents on Medicaid. Dropping these less subsidized and less likely to be subsidized nursing homes does not substantially affect the estimated employment and wage effects. Tables presenting these results may be found in Appendix II.XII.IV.

As a second approach, I re-estimate my primary within-state regressions using a locally linear semiparametric estimator that identifies the effect of subsidies using only variation in subsidy amounts among similarly subsidized nursing homes. Figure II.4 presents the results of regressing minutes of direct care worker employment per resident-day on simulated subsidy amounts using Epanechnikov kernel weights within one standard deviation bandwidths at varying points along the distribution of the mean simulated subsidy amounts offered to nursing home while subsidies were in place.[44] Within each one standard deviation bandwidth, the regression uses state-by-year and nursing home-by-policy event fixed effects as before. The dashed lines represent 95 percent confidence intervals, calculated using a policy event clustered bootstrap procedure, and the dotted line is the kernel density of the changes in subsidy amounts offered.

The results in Figure II.4 suggest that even when estimating without the influence of nursing homes with relatively extreme subsidization levels, there still are positive and reasonably stable effects of subsidies on employment per resident-day within the densest part of the change in subsidy distribution.[45] This suggests that the primary effects are not driven solely by comparison of subsidized nursing homes to the barely subsidized nursing homes that may have experienced different employment pre-trends. Figure II.5 presents the same results as in Figure II.4 but using average direct care worker hourly wages as the outcome variable. The estimated wage effects exhibit a pattern very similar to the employment effects and so do not appear to be driven disproportionately by nursing homes receiving very large or very small subsidy offers.

Another possible concern with the within-state estimates of subsidy effects on employment is that, instead of identifying increases in employment per resident-day that vary in size across nursing homes receiving different subsidy amounts, they might just reflect employment reallocation across nursing homes within subsidy states. That is, reductions in employment at low subsidy nursing homes and increases at high subsidy ones could

[44]Similar to the prior nonparametric case, I trim the top 5 percent of observations by subsidy amount offered due to estimates being extremely imprecise for these points.

[45]I do not use the instrumental variable approach in this semiparametric setting for econometric reasons but would note that replacing all uses of simulated subsidies here with the fitted values from the full sample's instrumental variable first stage regression does not yield qualitatively different results.

cause the within-state estimator to find positive employment effects of subsidies, even if there is no market-wide response.

In order to test the employment reallocation explanation, I check if its hypothesized employment reductions at low subsidy nursing homes actually occurred. I conduct this test by locally linearly estimating the difference-in-differences employment regression using Epanechnikov kernel weights within one standard deviation bandwidths along the distribution of the mean simulated subsidy amounts offered to nursing home while subsidies were in place and using the same fixed effects as in the fully parametric difference-in-differences regression. Figure II.6 presents these semiparametric difference-in-differences results using minutes of direct care worker employment per resident-day as the outcome variable and trimming, due to sparseness of observations, the top 5 percent of nursing homes by the absolute subsidy amount offered. Although the estimates are imprecise, the point estimates suggest that only a very small number of nursing homes receiving essentially no subsidies whatsoever exhibited staffing declines relative to the control states. These locally linear estimates thus find too few firms with negative subsidy adoption effects to be consistent with the hypothesis that the within-state estimates are driven solely by employment shifting across nursing homes.

II.VI.IV Within-and-Across-State Results

In this section, I relax the within-state empirical approach's assumption that nursing homes receiving different subsidy amounts were on similar trends in direct care worker employment and hourly wages. I do so by presenting the results from my within-and-across-state regressions, which allow for differential trends between variably subsidized nursing homes provided those trends are held in common across similar nursing homes in the treatment and control states. Tables II.10, II.11, and II.12 give results from regressions of direct care worker employment and wage outcomes on the simulated subsidy rates offered to nursing homes in treatment states relative to the similarly calculated placebo subsidy rates offered in control states conditional on state-by-year and nursing home-by-policy event fixed effects. In these tables, the "Placebo Effect" coefficients report the effect of the placebo subsidies in the control group, while the "Subsidy Effect" coefficients report the additional effect of subsidies in treatment states on top of the placebo subsidy effects.[46]

Table II.10 presents within-and-across-state results for the sample using all unconfounded states in the control group. The subsidy effect estimates here are very similar to the original within-state subsidy effect estimates, while the placebo subsidies are estimated to have almost no effect. Overall, these results suggest that the within-state instrumental variable subsidy effect estimates are reasonably reliable and not contaminated by differential pre-trends associated with subsidy receipt of a type common across all states at the time of subsidy adoption.

Table II.11 presents within-and-across-state results using the geographic neighbors control group sample. These results point toward similar though smaller direct care worker employment effects than in the within-state analysis case, while the subsidy effect on wages is estimated to be larger. The placebo effects remain small. This set of results suggest that the original within-state estimates of subsidy effects are not contaminated by differential pre-trends associated with subsidy receipt of a sort common to both treatment states and their geographic neighbors.

[46]As a complement to these findings, I conduct pre-trend validation tests for each of the within-and-across-state specifications. I do these tests in a fashion analogous to those done for my within-state regressions. The results from these tests are supportive of the different within-and-across state control groups helping to eliminate some of the variation in pre-trends observed across nursing homes in the within-state sample. For more details, please refer to Appendix II.XIV.

Finally, Table II.12 presents within-and-across-state results for the synthetic control sample. Here, as in the geographic neighbors case, estimates of subsidy effects on employment per resident-day are somewhat smaller than in the within-state analysis, with some placebo effects being observed for nursing assistant staffing in particular. The wage effects, however, remain essentially unchanged. These results once again speak favorably of the within-state instrumental variable subsidy effect estimates, suggesting that those results are not being driven by differential trends in employment and wages correlated with subsidy receipt of a sort that can be controlled for through my synthetic controls procedure.

The evidence resulting from this within-and-across-state analysis, coupled with the evidence from the within-state validation tests, bolsters the credibility of the within-state analysis's claim to identification. The within-state validation tests suggest that any differential pre-trends correlated with subsidy receipt in treatment states are at most modest in size and likely working against the main employment results. While modest pre-trends could be interpreted as signaling an impending larger trend break, the synthetic controls within-and-across-state analysis shows that this did not occur in other states exhibiting similar pre-trends. Meanwhile, the geographic neighbors and all other states analyses indicate that, for the within-state analysis's estimated subsidy effects to be a spurious result of differential pre-trends across nursing homes, these pre-trends must be unique to the treatment states and not shared either nationally or by nearby states. Finally, although these tests cannot rule out within-state effects being driven by employment and wage shocks that are correlated with subsidy receipt, contemporaneous to subsidy receipt, and unique to the treatment states, my effort to purge confounding policy events from the treatment sample through archival research on state policy environments eliminates the most likely causes of such confounding shocks. Cumulatively, this evidence strongly points toward the credibility of my within-state subsidy effect estimates.

II.VII Concluding Discussion

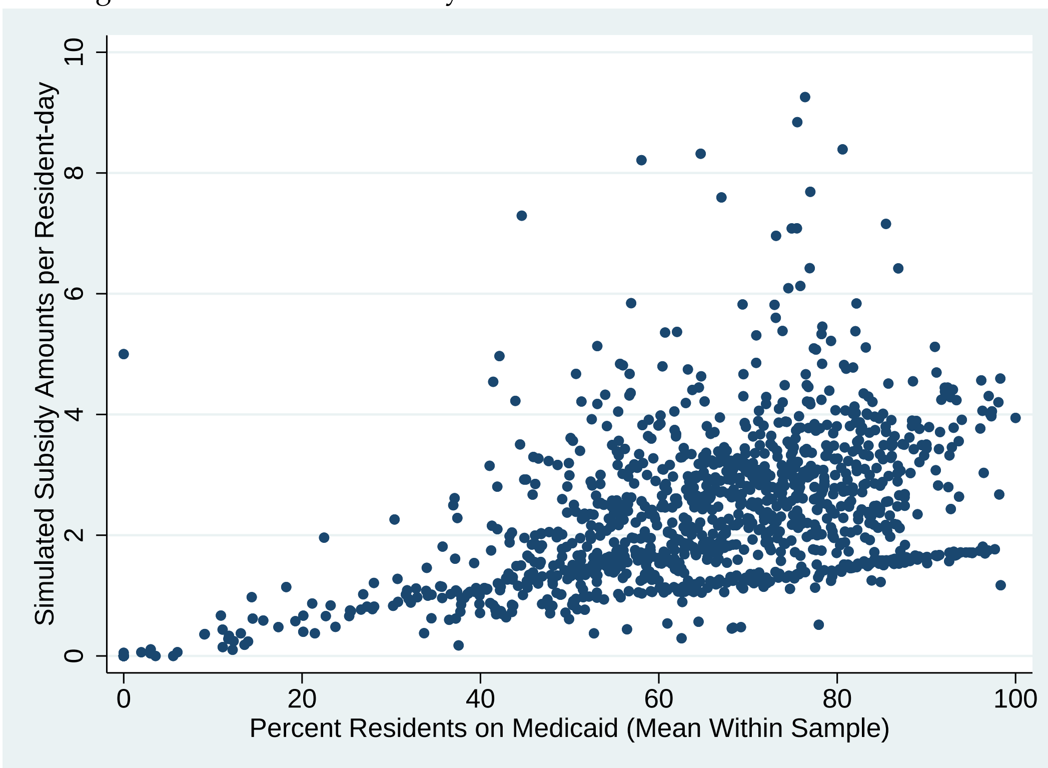
My findings from this chapter demonstrate that the establishment of nursing home payroll subsidies by state Medicaid programs have substantial positive effects on direct care worker employment and hourly wages at subsidized nursing homes. My primary empirical approach uses a within-state across-firm identification strategy and finds that for every $1 increase in effective payroll subsidy per resident-day, there is a 4.9 minute increase in direct care worker employment per resident-day and a 13 cent increase in average direct care worker hourly wages. These effects correspond to a 6.4 percent increase in direct care worker employment and a 1.5 percent increase in average direct care worker hourly wages at the mean nursing home. Furthermore, taking payroll subsidies as increases relative to state average Medicaid reimbursement rates, I find that the elasticity of nursing home employment per resident-day to Medicaid subsidy payments is 4.5 and the corresponding direct care worker average wage elasticity is 1.1. These estimates are reasonably large relative to similar parameters found in prior research (Foster and Lee, 2015; Hackmann, 2018) and are consistent with 100% pass-through of the subsidies to labor, pointing toward incremental nursing home payroll subsidies being powerful incentives for affecting nursing home behavior. Finally, in terms of implications for nursing home residents, the estimated subsidy effects appear to be stronger when weighting nursing homes by their resident counts, suggesting the average nursing home resident experienced a larger change in staffing than what occurred at the average nursing home.

These primary within-state results prove robust to a wide range of validation tests. Within the treatment states, pre-trends in nursing home direct care worker employment and wages are not substantially associated with eventual subsidy amounts offered. The small associations that do exist suggest that, if anything, the estimated subsidy effects may be attenuated by pre-trends. These within-state results are not being driven by nursing homes receiving outlier subsidy amounts or by nursing homes with very low shares of residents on Medicaid, two subsets of nursing homes that appear to exhibit the greatest differential pre-trends. Evidence from the within-and-across-state results suggest that the observed subsidy effects are not an artifact of confounding shocks contemporaneous to subsidy adoption or pre-trends correlated with subsidy receipt that are common across either subsidy states and all other control states, subsidy states and their geographic neighbors, or subsidy states and their paired synthetic control states. As a whole, this evidence bolsters the credibility of the within-state instrumental variable payroll subsidy effect estimates.

A number of possible avenues for future research extend from this work. While high nursing home direct care worker employment to resident ratios are an important input in the delivery of high quality nursing home care, I do not have data on resident-level outcomes and so cannot directly investigate the health effects of these subsidies. Better understanding of these health effects would be valuable both for better quantifying the ultimate welfare impact of these subsidies and for shedding light on whether nursing homes respond to payroll subsidies by substituting additional labor for other inputs. Additional research using resident-level health data might be able to shed greater light on the implications of these subsidies for nursing home resident welfare. Next, most nursing home payroll subsidies enacted since 2005 have been part of a broader nursing home policy reform package. While my research points toward strong positive effects of nursing home payroll subsidies on employment and wages when adopted as a policy on their own, understanding the degree to which payroll subsidies do or do not complement pay-for-performance programs, minimum staffing requirement increases, and other policies with which they are often bundled would be of substantial value. Finally, better understanding of the market-level circumstances that influence payroll subsidy efficacy would be valuable for understanding where subsidies could be most beneficially adopted and for developing a richer understanding of the incentives facing nursing homes themselves. This last avenue for further research will be examined in greater depth in the subsequent chapter.

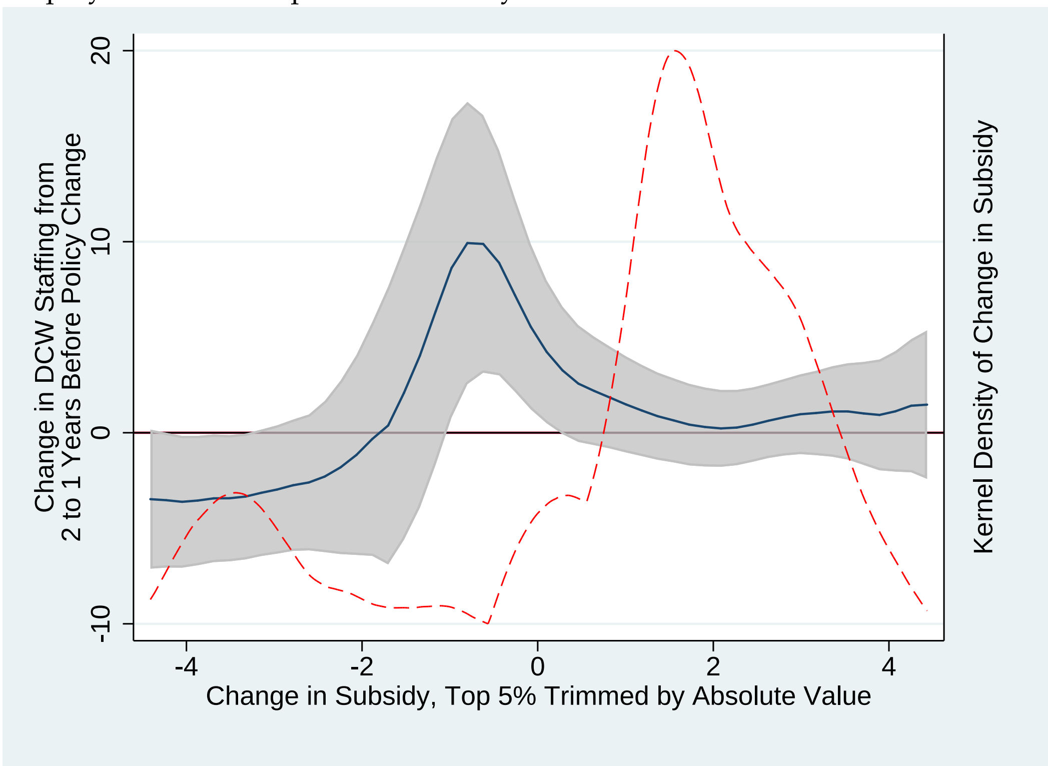
II.VIII Figures

Figure II.1: Simulated Subsidy Amounts Offered vs. Medicaid Resident Shares



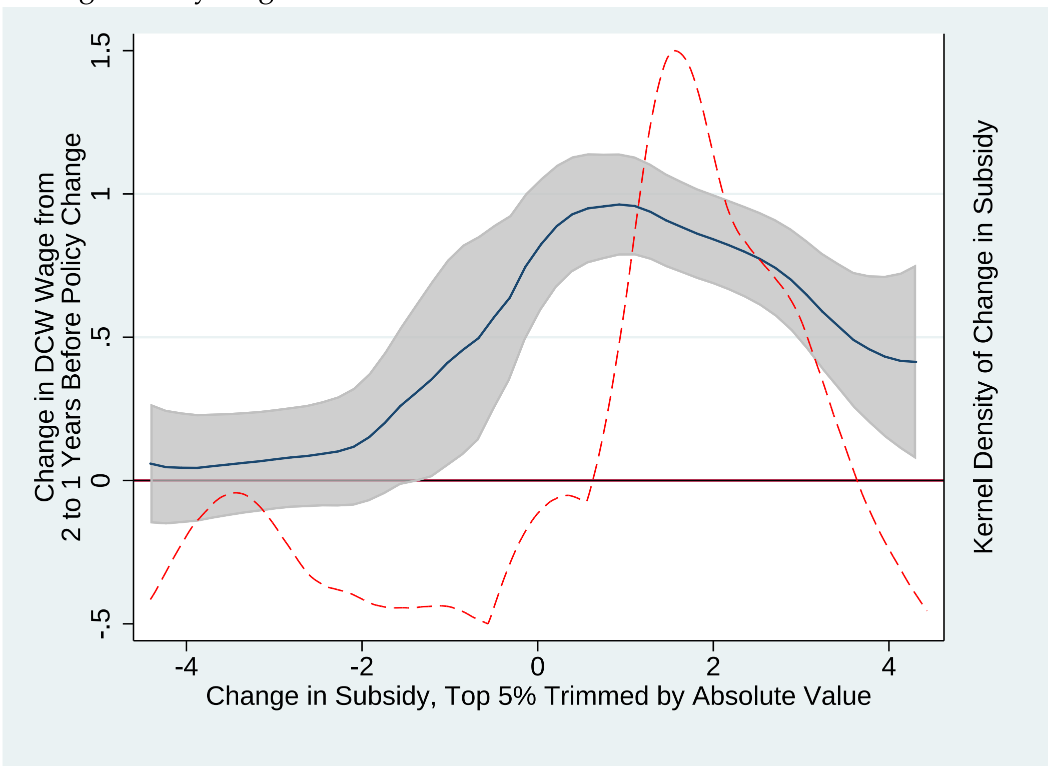
This figure graphs nursing homes' shares of residents on Medicaid against the simulated subsidy amounts per resident-day offered to them when subsidy policies are in place. Medicaid resident shares are within-sample averages. For Montana's second policy event, where a payroll subsidy is cut but not fully eliminated, subsidy amounts presented are nursing home specific average changes in subsidy amounts. A small number of nursing homes receiving subsidy amount changes greater than $10 per resident-day are trimmed. Observations shown are a 40 percent random sample of nursing homes, selected to avoid cluttering the graph.

Figure II.2: Nonparametric Effect of Subsidies on Pre-policy Change in Direct Care Worker Employment Minutes per Resident-Day



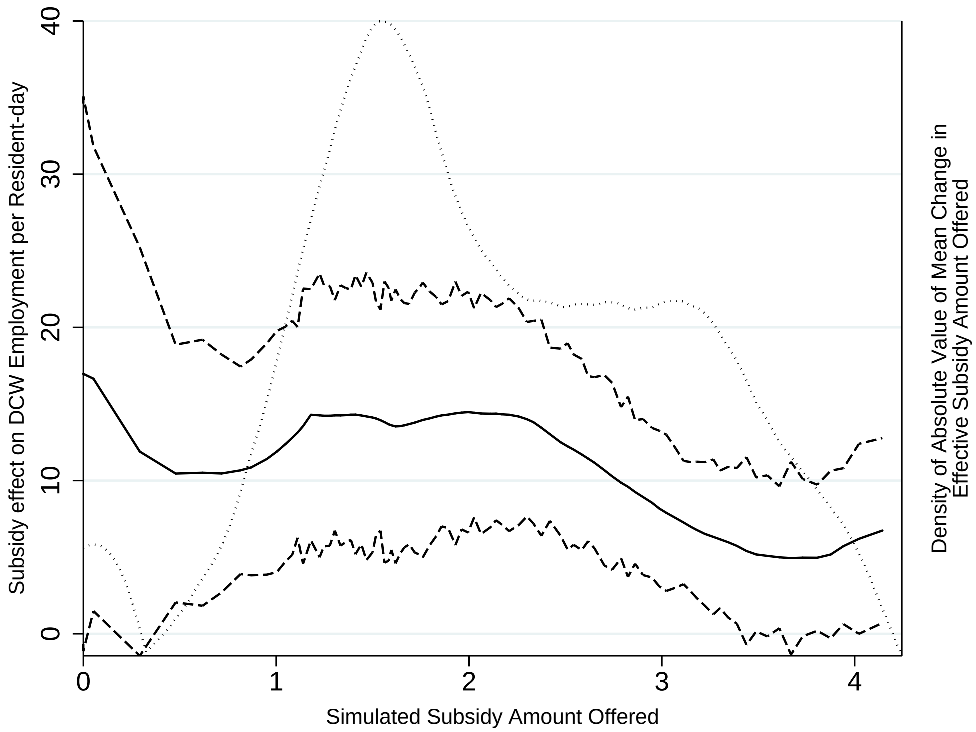
This figure presents nonparametric estimates of the effect of the change in effective subsidy amount offered to each nursing home on the change in minutes of direct care worker employment per resident-day experienced by each nursing home from two years prior to the change in subsidies to the year immediately prior to the change in subsidies. Changes in subsidies are used to distinguish between cases where subsidies are adopted and where they are eliminated. The gray bands are confidence intervals, estimated without clustering adjustments. The dashed line is the kernel density of the eventual change in subsidy amounts offered to nursing homes, with differences in its height representing relative differences in the number of nursing homes. Negative changes reflect nursing homes in states which cut their payroll subsidies. These results are shown for a sample of nursing homes where the top 5 percent by magnitude of eventual subsidy change are trimmed, due to sparseness among these nursing homes preventing estimation of nonparametric estimates with meaningful confidence intervals. Nonparametric estimates here are produced using an Epanechnikov kernel and a locally linear estimator.

Figure II.3: Nonparametric Effect of Subsidies on Pre-policy Change in Direct Care Worker Average Hourly Wages



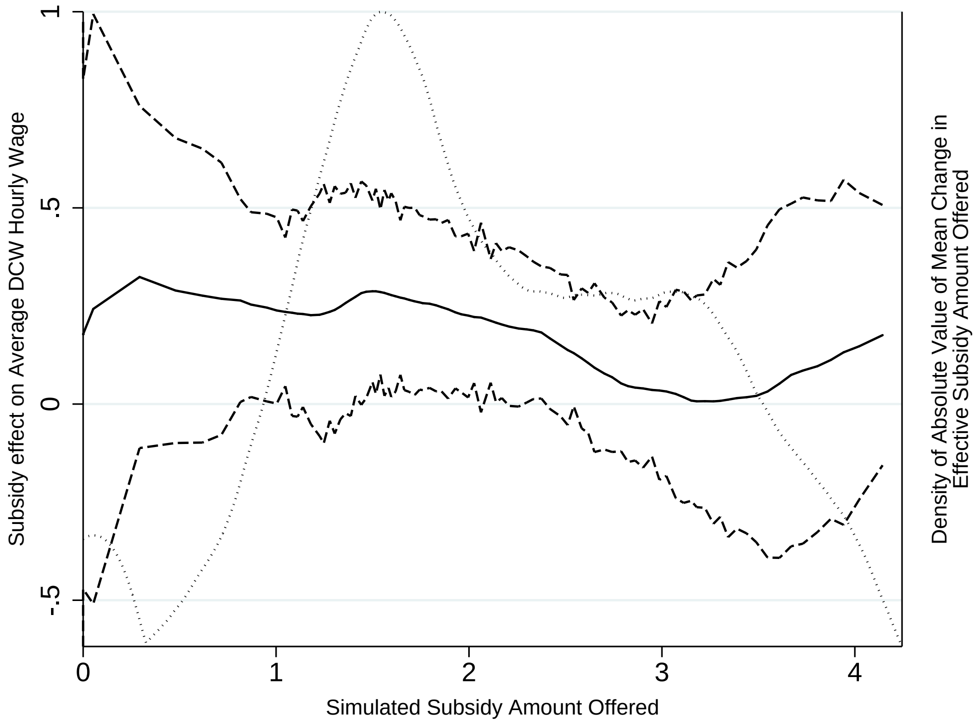
This figure presents nonparametric estimates of the effect of the change in effective subsidy amount offered to each nursing home on the change in average direct care worker (DCW) hourly wages offered by each nursing home from two years prior to the change in subsidies to the year immediately prior to the change in subsidies. Changes in subsidies are used to distinguish between cases where subsidies are adopted and where they are eliminated. The gray bands are confidence intervals, estimated without clustering adjustments. The dashed line is the kernel density of the eventual change in subsidy amounts offered to nursing homes, with differences in its height representing relative differences in the number of nursing homes. Negative changes reflect nursing homes in states which cut their payroll subsidies. These results are shown for a sample of nursing homes where the top 5 percent by magnitude of eventual subsidy change are trimmed, due to sparseness among these nursing homes preventing estimation of nonparametric estimates with meaningful confidence intervals. Nonparametric estimates here are produced using an Epanechnikov kernel and a locally linear estimator.

Figure II.4: Locally Linear Regression of Direct Care Worker Employment on Subsidies per Resident-Day, Estimated in Bandwidths along Subsidy Amount Offered



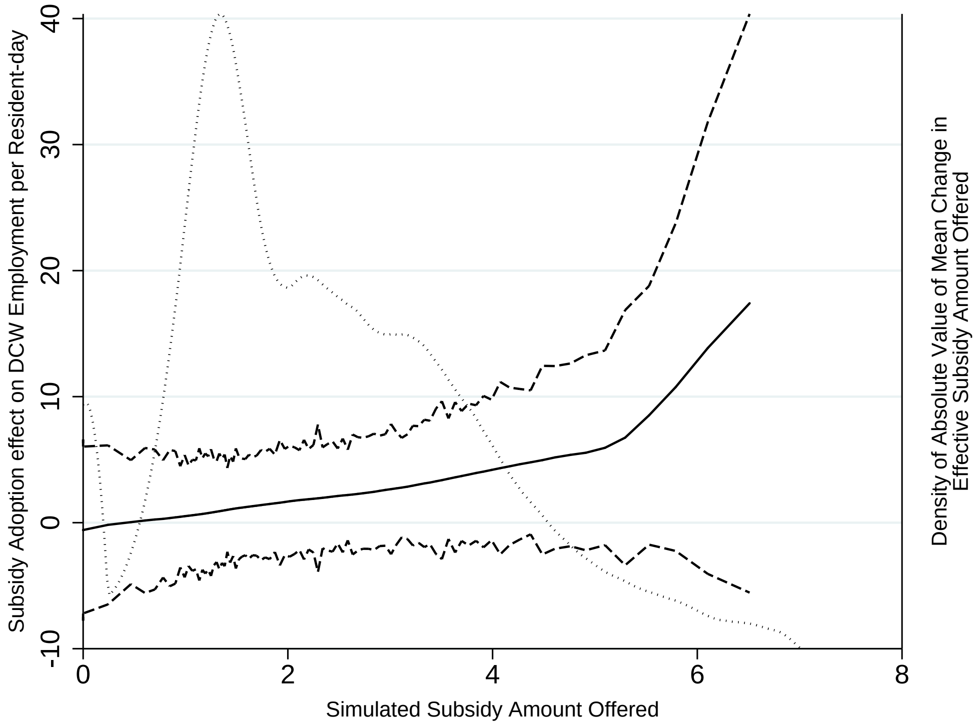
This figure presents locally linear estimates from the regression of direct care worker (DCW) minutes per resident-day on simulated subsidy amounts conditional on state-by-year and nursing home-by-policy event fixed effects. These regressions are estimated within two-year symmetric treatment windows around each policy change. The line represents the results from estimating this regression using Epanechnikov kernel weights in one standard deviation bandwidths along the mean simulated subsidy amount offered to each nursing home when it is being offered subsidies. For Montana's second policy event, where a payroll subsidy is cut but not fully eliminated, subsidy amounts used are nursing home specific average changes in subsidy amounts. The dashed lines are 95 percent confidence intervals calculated using a policy event clustered bootstrap procedure. The dotted line is the kernel density of the absolute value in the change in subsidy amounts offered to nursing homes, with differences in its height representing relative differences in the number of nursing homes. The top 5 percent of nursing homes by change simulated subsidy amount are trimmed from the graph, due to their sparseness preventing estimation with a meaningful level of precision.

Figure II.5: Locally Linear Regression of Direct Care Worker Average Hourly Wages on Subsidies per Resident-Day, Estimated in Bandwidths along Subsidy Amount Offered



This figure presents locally linear estimates from the regression of direct care worker (DCW) average hourly wages on simulated subsidy amounts conditional on state-by-year and nursing home-by-policy event fixed effects. These regressions are estimated within two-year symmetric treatment windows centered around each policy change. The line represents the results from estimating this regression using Epanechnikov kernel weights in one standard deviation bandwidths along the mean simulated subsidy amount offered to each nursing home when it is being offered subsidies. For Montana's second policy event, where a payroll subsidy is cut but not fully eliminated, subsidy amounts used are nursing home specific average changes in subsidy amounts. The dashed lines are 95 percent confidence intervals calculated using a policy event clustered bootstrap procedure. The dotted line is the kernel density of the absolute value in the change in subsidy amounts offered to nursing homes, with differences in its height representing relative differences in the number of nursing homes. The top 5 percent of nursing homes by change simulated subsidy amount are trimmed from the graph, due to their sparseness preventing estimation with a meaningful level of precision.

Figure II.6: Locally Linear Difference-in-Differences Estimates of the Effect of Subsidy Adoption on Direct Care Worker Employment per Resident-Day, Estimated in Bandwidths along Eventual Subsidy Amount Offered within Sample of Subsidy States vs. All Control States



This figure presents locally linear estimates from the regression of minutes of direct care worker (DCW) employment per resident-day on an indicator variable that is 1 when subsidies are being offered and 0 otherwise, on treatment and control group-specific year fixed effects, and on nursing home-by-policy event fixed effects. The sample includes all treatment states and all unconfounded control states. The line represents the results from estimating this regression using Epanechnikov kernel weights in one standard deviation bandwidths along the mean simulated subsidy amount offered to each nursing home when it is being offered subsidies. For Montana's second policy event, where a payroll subsidy is cut but not fully eliminated, subsidy amounts used are nursing home specific average changes in subsidy amounts. The dashed lines are 95 percent confidence intervals calculated using a policy event clustered bootstrap procedure. The dotted line is the kernel density of the absolute value in the change in subsidy amounts offered to nursing homes, with differences in its height representing relative differences in the number of nursing homes. The top 5 percent of nursing homes by change simulated subsidy amount are trimmed from the graph, due to their sparseness preventing estimation with a meaningful level of precision.

II.IX Tables

Table II.1: Summary Statistics for Full Treatment Sample vs. Subset with Payroll Data

	Full Subsidy Sample			Payroll Sample Subset		
	Mean	Median	Std Dev	Mean	Median	Std Dev
DCW Staffing Minutes	196.85	185.58	62.36	186.53	183.19	41.29
CNA Staffing Minutes	127.42	122.70	37.92	124.06	121.26	32.47
Nurse Staffing Minutes	69.43	61.20	39.23	62.48	61.08	17.04
DCW Average Hourly Wage	22.21	22.40	4.99	22.13	22.34	4.98
Share of Residents on Medicaid	62.46	66.26	23.08	66.49	67.89	15.77
Share of Residents on Medicare	12.62	8.11	17.80	10.90	9.41	7.74
Share of Residents on Private	24.92	20.97	18.15	22.61	20.17	14.12
Resident Count	88.37	86.00	47.74	101.27	100.00	44.34
Percent Occupancy	86.47	90.78	13.05	87.78	91.67	11.55
Absolute Value of Change in Subsidies	2.32	2.08	1.51	2.48	2.29	1.34
Observations	12259			7207		

This table lists means, medians, and standard deviations for key variables within two samples. The sample on the left is the set of all nursing home observations from subsidy states within a two-year symmetric window around each policy change used in estimation. Note that this sample is slightly larger than the within-state estimation sample, since it includes nursing homes observed an insufficient number of times for instrument construction. The sample on the right is the same, but limited to the Medicare-certified nursing homes for which payroll and wage data are available. Staffing variables are denominated in minutes per resident-day of either direct care worker (DCW), certified nursing assistant (CNA), or licensed nurse employment. Direct care worker average hourly wages are denominated in 2017 dollars per hour. Pct Medicaid, Pct Medicare, and Pct Private refer to percentage shares of nursing home residents funded by each source, with Pct Private including all non-Medicaid and non-Medicare residents. Absolute Value of Change in Subsidy refers to the absolute value of the changes in effective subsidy amount per resident-day offered to nursing homes before and after a policy event. Absolute values of changes are used here to simplify comparison between subsidy adoption states and subsidy repeal states. The observation count is the number of nursing home-year observations.

Table II.2: Summary Statistics for Full Treatment Sample vs. All Other States

	Full Subsidy Sample			All Other States		
	Mean	Median	Std Dev	Mean	Median	Std Dev
DCW Staffing Minutes	196.85	185.58	62.36	197.88	187.20	64.33
CNA Staffing Minutes	127.42	122.70	37.92	128.35	123.96	41.36
Nurse Staffing Minutes	69.43	61.20	39.23	69.53	61.68	37.68
DCW Average Hourly Wage	22.21	22.40	4.99	21.36	20.95	4.51
Share of Residents on Medicaid	62.46	66.26	23.08	62.64	67.13	23.18
Share of Residents on Medicare	12.62	8.11	17.80	13.28	9.26	16.84
Share of Residents on Private	24.92	20.97	18.15	24.08	20.00	18.20
Resident Count	88.37	86.00	47.74	92.12	83.00	59.57
Percent Occupancy	86.47	90.78	13.05	84.61	89.16	14.41
Observations	12259			184368		

This table lists means, medians, and standard deviations for key variables within two samples. The sample on the left is the set of all nursing home observations from subsidy states within a two-year symmetric window around each policy change used in estimation. Note that this sample is slightly larger than the within-state estimation sample, since it includes nursing homes observed an insufficient number of times for instrument construction. The sample on the right is the set of all nursing home observations from all other states in the same years. Staffing variables are denominated in minutes per resident-day of either direct care worker (DCW), certified nursing assistant (CNA), or licensed nurse employment. Direct care worker average hourly wages are denominated in 2017 dollars per hour. Pct Medicaid, Pct Medicare, and Pct Private refer to percentage shares of nursing home residents funded by each source, with Pct Private including all non-Medicaid and non-Medicare residents. The observation count is the number of nursing home-year observations.

Table II.3: Response of Direct Care Worker Employment Minutes per Resident-Day by Worker Type to Subsidies per Resident-Day

	DCW Staffing	CNA Staffing	Nurse Staffing
OLS			
Subsidy	7.317*	4.749*	2.567*
	(1.872)	(1.376)	(0.700)
Fisher Rank	0.916	0.851	0.983*
Fisher SD	[1.796]	[1.306]	[0.525]
Mean Effect	9.420%	9.251%	10.471%
IV			
Subsidy	4.942*	3.126*	1.814
	(2.198)	(1.219)	(1.028)
Fisher Rank	1.000*	1.000*	1.000*
Fisher SD	[1.137]	[0.889]	[0.479]
Mean Effect	6.362%	6.089%	7.400%
Clusters	14	14	14
Obs	11685	11685	11685

All estimates are coefficients from independent regressions of minutes of direct care worker (DCW), certified nursing assistant (CNA), and licensed nurse employment per resident-day on simulated subsidy amounts offered conditional on state-by-year fixed effects and nursing home-by-policy event fixed effects. These regressions are estimated within two-year symmetric windows around each policy change. Simulated subsidies are the simulated maximum subsidy amount a nursing home could receive in terms of dollars per resident-day given its observed characteristics, which is instrumented for in the IV case using the amount simulated using the same formula but a fixed set of pre-subsidy adoption characteristics. Standard errors in parentheses are calculated analytically and clustered at the policy event level. Significance levels implied by these standard errors are denoted by either * ($p < 0.05$) or +($p<0.1$) and placed on the coefficient. The listed "Fisher Rank" is the percentile rank of the listed coefficient within a distribution of 1000 placebo coefficients, each calculated on a randomly selected placebo sample. Rankings in the extreme 2.5 percent and 5 percent tails of the placebo effect distribution are denoted with a * and a + respectively. The "Fisher SD" is the standard deviation of the distribution of placebo coefficients. The "Mean Effect" is the mean of the effect of subsidies across all subsidized nursing homes, calculated by scaling the simulated subsidy rates offered to nursing homes by the estimated regression coefficients and dividing by the corresponding outcome variables. The cluster count is the number of policy events in the sample, while the observation count is the number of nursing home-year observations in the sample.

Table II.4: Response of Direct Care Worker Average Hourly Wages and Payroll per Resident-Day to Subsidies per Resident-Day

	Avg DCW Wage	DCW PPR
OLS		
Subsidy	0.111*	2.588*
	(0.048)	(0.802)
Fisher Rank	0.979*	0.901
Fisher SD	[0.055]	[0.663]
Mean Effect	1.261%	5.331%
IV		
Subsidy	0.133*	1.116
	(0.052)	(1.180)
Fisher Rank	0.982*	0.925
Fisher SD	[0.064]	[0.701]
Mean Effect	1.508%	2.298%
Clusters	14	14
Obs	7719	7785

All estimates are coefficients from independent regressions of direct care worker (DCW) average hourly wages and direct care worker payroll per resident-day on simulated subsidy amounts offered conditional on state-by-year fixed effects and nursing home-by-policy event fixed effects. For more on the construction of the sample, instrument, mean effects, and test statistics, please refer to the notes attached to Table II.3.

Table II.5: Response of Direct Care Worker Employment per Resident-Day to Subsidies per Resident-Day in Sample With Valid Payroll Data for All Variables

	DCW Staffing	CNA Staffing	Nurse Staffing	Avg DCW Wage	DCW PPR
Subsidy	7.979+	5.396*	2.582	0.064	0.908
	(3.865)	(2.219)	(1.840)	(0.053)	(1.194)
Fisher Rank	1.000*	1.000*	0.999*	0.890	0.924
Fisher SD	[1.584]	[1.329]	[0.627]	[0.066]	[0.710]
Clusters	14	14	14	14	14
Obs	7036	7036	7036	7036	7036

All estimates are coefficients from independent instrumental variable regressions estimated within the subset of nursing homes that have valid observations for both their average direct care worker hourly wage and their direct care worker payroll per resident-day. Note that this sample is smaller than the samples used in the main wage and payroll regressions, since those samples require nursing homes have valid observations for only the outcome variable being examined. Outcome variables include direct care worker (DCW), certified nursing assistant (CNA), and licensed nurse minutes per resident-day. These are regressed on simulated subsidy amounts offered conditional on state-by-year fixed effects and nursing home-by-policy event fixed effects. All results reported are from instrumental variable regressions; For more on the construction of the sample, instrument, and test statistics, please refer to the notes attached to Table II.3.

Table II.6: Resident Count Weighted Response of Direct Care Worker Employment, Wages, and Payroll per Resident-Day to Subsidies per Resident-Day

	DCW Staffing	CNA Staffing	Nurse Staffing	Avg DCW Wage	DCW PPR
Subsidy	6.544+	3.769*	2.774+	0.157*	1.613
	(3.084)	(1.738)	(1.402)	(0.055)	(1.164)
Fisher Rank	1.000*	1.000*	1.000*	0.968+	0.944
Fisher SD	[1.780]	[1.261]	[0.668]	[0.079]	[0.780]
Clusters	14	14	14	14	14
Obs	11685	11685	11685	7719	7785

All estimates are coefficients from independent instrumental variable regressions where nursing homes are weighted by their mean number of residents within each policy event's two-year symmetric treatment window. Outcome variables include direct care worker (DCW), certified nursing assistant (CNA), and licensed nurse minutes per resident-day as well as direct care worker average hourly wages and direct care worker payroll per resident-day. These are regressed on simulated subsidy amounts offered conditional on state-by-year fixed effects and nursing home-by-policy event fixed effects. All results reported are from instrumental variable regressions. For more on the construction of the sample, instrument, and test statistics, please refer to the notes attached to Table II.3.

Table II.7: Heterogeneous Effects of Subsidies per Resident-Day by Subsidy Type on Direct Care Worker Employment, Wages, and Payroll per Resident-Day: Allocated Payment Type Subsidies vs All Other Subsidy Types

	DCW Staffing	CNA Staffing	Nurse Staffing	Avg DCW Wage	DCW PPR
Subsidy	7.957*	4.770*	3.186+	0.231*	2.988*
	(3.304)	(1.730)	(1.617)	(0.091)	(1.314)
Fisher Rank	0.999*	1.000*	0.998*	0.961+	0.967+
Fisher SD	[2.773]	[1.993]	[1.085]	[0.137]	[1.793]
Subsidy X Allocated Type	−5.289	−2.851	−2.438	−0.161	−3.036+
	(3.549)	(1.885)	(1.766)	(0.108)	(1.630)
Fisher Rank	0.007*	0.016*	0.004*	0.123	0.026+
Fisher SD	[2.986]	[2.209]	[1.176]	[0.158]	[1.963]
Clusters	14	14	14	14	14
Obs	11682	11682	11682	7719	7785

All estimates labeled "Subsidy" are coefficients from independent instrumental variable regressions of minutes of direct care worker (DCW), certified nursing assistant (CNA), and licensed nurse employment per resident-day or of either average direct care worker hourly wages or direct care worker payroll per resident-day on simulated subsidy amounts offered conditional on state-by-year fixed effects and nursing home-by-policy event fixed effects. All estimates labeled "Subsidy X Allocated" are coefficients from the interaction in those regressions between subsidy amounts offered and an indicator variable that is 1 in allocated payment type subsidy states and 0 otherwise. All results reported are from instrumental variable regressions. Note that one first stage regression is estimated for each of the subsidy and the subsidy interaction term, with each first stage regression including both the main subsidy instrument and the main subsidy instrument interacted with the allocated payment indicator variable. For more on the construction of the sample, instrument, and test statistics, please refer to the notes attached to Table II.3.

Table II.8: Heterogeneous Effects of Subsidies per Resident-Day by Nursing Home For-Profit Status on Direct Care Worker Employment, Wages, and Payroll per Resident-Day

	DCW Staffing	CNA Staffing	Nurse Staffing	Avg DCW Wage	DCW PPR
Subsidy	5.896*	3.853*	2.042+	0.153*	1.778
	(2.164)	(1.176)	(1.015)	(0.069)	(1.284)
Fisher Rank	1.000*	0.999*	0.999*	0.985*	0.898
Fisher SD	[1.235]	[0.913]	[0.497]	[0.064]	[0.686]
Subsidy X For Profit	−1.254*	−0.922+	−0.332	−0.024	−0.795
	(0.470)	(0.446)	(0.190)	(0.037)	(0.674)
Fisher Rank	0.357	0.456	0.275	0.658	0.190
Fisher SD	[1.063]	[0.775]	[0.445]	[0.057]	[1.088]
Clusters	14	14	14	14	14
Obs	11682	11682	11682	7719	7785

All estimates labeled "Subsidy" are coefficients from independent instrumental variable regressions of minutes of direct care worker, certified nursing assistant, and licensed nurse employment per resident-day or of either average direct care worker (DCW) hourly wages or direct care worker payroll per resident-day on simulated subsidy amounts offered conditional on state-by-year fixed effects and nursing home-by-policy event fixed effects. All estimates labeled "Subsidy X For-Profit" are coefficients from the interaction in those regressions between subsidy amounts offered and an indicator variable that is 1 for-profit nursing homes and 0 otherwise. All results reported are from instrumental variable regressions. Note that one first stage regression is estimated for each of the subsidy and the subsidy interaction term, with each first stage regression including both the main subsidy instrument and the main subsidy instrument interacted with the for-profit indicator variable. For more on the construction of the sample, instrument, and test statistics, please refer to the notes attached to Table II.3.

Table II.9: Response of Direct Care Worker Employment, Wages, and Payroll per Resident-Day to Subsidies per Resident-Day, Estimated within 1 Year Symmetric Windows Centered Around Varying Years Relative to Subsidy Adoption or Repeal

	DCW Staffing	CNA Staffing	Nurse Staffing	Avg DCW Wage	DCW PPR
3 Years Before					
Subsidy	−0.194	−0.640	0.446*	0.120+	2.575*
	(1.316)	(1.283)	(0.085)	(0.058)	(0.713)
Clusters	8	8	8	8	8
Obs	3034	3034	3034	1956	1994
2 Years Before					
Subsidy	−0.043	0.304	−0.348	0.064	0.475
	(1.832)	(0.963)	(1.039)	(0.068)	(0.912)
Clusters	13	13	13	10	10
Obs	4114	4114	4114	2136	2230
1 Year Before					
Subsidy	−1.924	−1.786	−0.137	0.024	0.734
	(2.863)	(1.824)	(1.164)	(0.047)	(0.743)
Clusters	14	14	14	14	14
Obs	4882	4882	4882	2980	2986
Contemporaneous					
Subsidy	5.228	3.822+	1.404	0.128*	1.059
	(3.123)	(1.813)	(1.393)	(0.047)	(1.144)
Clusters	14	14	14	14	14
Obs	5578	5578	5578	3448	3484
1 Year After					
Subsidy	1.368	−0.145	1.513	−0.002	0.977
	(1.822)	(1.016)	(0.890)	(0.038)	(0.882)
Clusters	14	14	14	14	14
Obs	5166	5166	5166	3306	3348

All estimates are coefficients from independent instrumental variable regressions of direct care worker (DCW), certified nursing assistant (CNA), or nurse minutes per resident-day or of average direct care worker hourly wages on simulated subsidy amounts offered conditional on state-by-year fixed effects and nursing home-by-policy event fixed effects. These regressions are estimated within a one-year symmetric window, with the window being centered around the actual policy change in the "Contemporaneous" panel and being centered the specified number of years before or after the genuine policy change in the other panels. Note that the results and sample sizes here for the contemporaneous results vary from the primary results, due to restriction to a one-year window here rather than to a two-year window. Standard errors in parentheses are calculated analytically and clustered at the policy event level. Significance levels implied by these standard errors are denoted by either * ($p < 0.05$) or +($p < 0.1$) and placed on the coefficient. All results reported are from instrumental variable regressions. For more on the construction of the simulated subsidy and instrument variables, please refer to the notes attached to Table II.3.

Table II.10: Response of Direct Care Worker Employment, Wages, and Payroll per Resident-Day to Subsidies per Resident-Day in Subsidy States, Relative to the Effect of Placebo Subsidies in All Unconfounded Control States

	DCW Staffing	CNA Staffing	Nurse Staffing	Avg DCW Wage	DCW PPR
Placebo Effect	−0.029	−0.013	−0.016	−0.007	0.163
	(0.524)	(0.380)	(0.161)	(0.013)	(0.180)
Subsidy Effect	4.963*	3.129*	1.832+	0.139*	0.950
	(2.220)	(1.254)	(1.021)	(0.053)	(1.172)
Clusters	28	28	28	28	28
Obs	316998	316998	316998	202550	203992

All estimates are coefficients from independent instrumental variable regressions estimated in a sample including all subsidy states and all unconfounded control states. Outcome variables include direct care worker (DCW), certified nursing assistant (CNA), and licensed nurse minutes per resident-day as well as direct care worker average hourly wages and direct care worker payroll per resident-day. All regressions regress one of those outcomes on the simulated effective subsidy amount that was or would have been offered to each nursing home, an interaction term between this variable and a dummy variable that is 1 in treatment states and 0 in control states, state-by-year-by-policy event fixed effects, and nursing home fixed effects. All regressions are estimated within two-year symmetric windows around each policy event. The "Placebo Effect" coefficient reports the effect of the placebo subsidies within the control group, while the "Subsidy Effect" coefficient reports the interaction term that reflects the effect of subsidies in the treatment group over and above the placebo effect in the control group. Standard errors in parentheses are calculated analytically and clustered at the policy event by treatment vs. control group level. Significance levels implied by these standard errors are denoted by either * ($p < 0.05$) or +($p<0.1$) and placed on the coefficient. The cluster count is the number of treatment policy events and paired control groups in the sample, while the observation count is the number of nursing home-year observations in the sample. All results reported are from instrumental variable regressions.

Table II.11: Response of Direct Care Worker Employment, Wages, and Payroll per Resident-Day to Subsidies per Resident-Day in Subsidy States, Relative to the Effect of Placebo Subsidies in Geographically Neighboring Control States

	DCW Staffing	CNA Staffing	Nurse Staffing	Avg DCW Wage	DCW PPR
Placebo Effect	0.099	0.383	−0.284	−0.076+	−0.218
	(1.162)	(0.886)	(0.420)	(0.045)	(0.387)
Subsidy Effect	3.507+	1.897	1.609+	0.229*	1.173
	(1.992)	(1.254)	(0.876)	(0.070)	(0.914)
Clusters	40	40	40	40	40
Obs	38840	38840	38840	26295	26450

All estimates are coefficients from independent instrumental variable regressions estimated in a sample including all subsidy states and their paired geographic neighbor states. Outcome variables include direct care worker (DCW), certified nursing assistant (CNA), and licensed nurse minutes per resident-day as well as direct care worker average hourly wages and direct care worker payroll per resident-day. The "Placebo Effect" coefficient reports the effect of the placebo subsidies within the control group, while the "Subsidy Effect" coefficient reports the interaction term that reflects the effect of subsidies in the treatment group over and above the placebo effect in the control group. Standard errors are calculated analytically and clustered at the treatment state-neighbor pair level, resulting in one cluster per geographic neighbor. For more on the regression specification or on the construction of the sample, instrument, and test statistics, please refer to the notes attached to Table II.10.

Table II.12: Response of Direct Care Worker Employment, Wages, and Payroll per Resident-Day to Subsidies per Resident-Day in Subsidy States, Relative to the Effect of Placebo Subsidies in Synthetic Control States

	DCW Staffing	CNA Staffing	Nurse Staffing	Avg DCW Wage	DCW PPR
Placebo Effect	1.334	1.171	0.163	−0.019	0.863*
	(1.238)	(0.851)	(0.470)	(0.048)	(0.419)
Fisher Rank	0.978*	0.974+	0.918	0.583	0.972+
Fisher SD	[0.986]	[0.756]	[0.394]	[0.065]	[0.645]
Subsidy Effect	3.600	1.946	1.653	0.152*	0.250
	(2.487)	(1.467)	(1.113)	(0.070)	(1.232)
Fisher Rank	0.983*	0.952+	0.983*	0.934	0.521
Fisher SD	[1.495]	[1.158]	[0.620]	[0.082]	[0.966]
Clusters	28	28	28	28	28
Obs	76584	76584	76584	46947	47303

All estimates are coefficients from independent instrumental variable regressions estimated in a sample including all subsidy states and their synthetic control states. Outcome variables include direct care worker (DCW), certified nursing assistant (CNA), and licensed nurse minutes per resident-day as well as direct care worker average hourly wages and direct care worker payroll per resident-day. The "Placebo Effect" coefficient reports the effect of the placebo subsidies within the control group, while the "Subsidy Effect" coefficient reports the interaction term that reflects the effect of subsidies in the treatment group over and above the placebo effect in the control group. The listed "Fisher Rank" is the percentile rank of the listed coefficient within a distribution of 1000 such coefficients, each calculated on a randomly constructed placebo sample. Rankings in the extreme 2.5 percent and 5 percent tails subsidy of the placebo effect distribution are denoted with a * and a + respectively. The "Fisher SD" is the standard deviation of the distribution of placebo coefficients. For more on the regression specification or on the construction of the sample, instrument, and test statistics, please refer to the notes attached to Table II.10.

II.X Policy Appendix

II.X.I Details On Collection of Policy Information

I compiled information about state Medicaid nursing home policy regimes from 1994-2017 from across a number of sources.[47] As no sources in the academic literature had previously documented information about the size and design of nursing home payroll subsidies, I obtained that information through a careful review of historical primary source documents coupled with efforts to reach out to Medicaid state offices. Information about other major reforms was developed in a similar way. The procedure used for collecting this information was as follows.

My preferred type of resources to consult for information about state payroll subsidy formulas and other policies were the Medicaid policy and reimbursement manuals published by state Medicaid programs. These manuals were used to specify the details of state nursing home policies and state Medicaid nursing home payment formulas. If a payroll subsidy-like policy was offered at some point, manuals published at the time often, though not always, would describe them in great detail. While most states do not make historical copies of these manuals readily available online, I was able to obtain copies of these manuals for many years through the Internet Archive (archive.org), which hosts historical copies of state Medicaid programs' websites, and through reaching out to state Medicaid agencies and requesting them. While manuals were usually not available for all years, they often would contain information useful for inferring policy regime information in years for which I could not obtain a manual. In particular, manuals often gave dates when specific

[47] In addition to information on nursing home Medicaid policies, I also obtained some information on broader changes in long-term care policy, such as Medicaid of home and community based care.

provisions were updated or first implemented and sometimes would preserve repealed or expired policies in the manual text alongside a note specifying their last date in effect.

When state policy and reimbursement manuals were not available, in many cases I was able to obtain comparable information from archives of the provider letters and emails sent by state Medicaid offices to nursing homes.[48] These letters often would mention when major policy changes were occurring and would describe them in sufficient detail for my purposes. In addition to the reimbursement manuals and provider letters, archival copies of state Medicaid programs' websites[49] were useful in a number of states where the websites were used for directly posting information about new policies. In some states, details of nursing home reimbursement and other policies were specified either in legislation or as part of the state administrative code. In these states, archival copies of the relevant portions of law and code[50] were useful in identifying major policy changes. Finally, in some states, I was able to obtain copies of Medicaid state plan amendments,[51] which occasionally would list when major nursing home policy reforms occurred. Since states varied in terms of how they tended to document their Medicaid policies, I consulted all of the above sources for as many years as possible. The above process generally yielded good coverage of state policy environments, though information gaps are relatively more likely to remain in the 1990s and in a number of typically very small states that outsourced administration of their Medicaid nursing home reimbursement system to private accounting firms.

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[48] These letters often were archived on state Medicaid programs' own websites. When not available there, the Internet Archive (archive.org) once again proved valuable.

[49] Typically, these copies were available through the Internet Archive (archive.org).

[50] These were available usually through either state secretary of state websites, state law libraries, or through the Internet Archive (archive.org).

[51] These are available directly from state sources for some years, federal sources for others, and from the Internet Archive (archive.org) copies of state and federal websites for still other years.

In addition to these governmental primary source documents, I relied on some past work by other researchers and government agencies. Surveys conducted by the North Carolina Division of Family Services (NCDFS 1999; 2000), surveys by the Paraprofessional Healthcare Institute (Harmuth and Dyson 2004a; 2004b; 2005; 2006; 2009), and a Government Accountability Office report on nursing home quality initiatives (GAO 2000) provided some information on when various policy changes occurred as well as some details about payroll subsidy formulas. Data collection efforts by Brown University's Long Term Care Focus project (LTCFocus) and a number of papers in the existing literature (Grabowski et al., 2004; Feng et al., 2010; Miller et al., 2012a; Foster and Lee, 2015) were useful for obtaining information about Medicaid average payment rates and about when various policies were in place[52], though these sources did not include details about payroll subsidy formulas. I also obtained information about state nursing home minimum staffing requirements through correspondence with the University of California, San Francisco's Dr. Charlene Harrington and through reference to a number of papers on the subject (Harrington, 2005a,b, 2010; Park and Stearns, 2009; Lin, 2014). Information from all of the above sources were valuable for helping flag times and states where policies were in place, allowing me to better calibrate the reliability of my primary source document based procedure. Where the information from these sources contradicted primary source documents, and where these contradictions could not be resolved through further more detailed examination of as many primary source materials as possible, I used whatever was listed in the original governmental sources.

[52]In particular, LTCFocus offers information on when where nursing home bed taxes are in place, on adoption of case mix reimbursement, on the share of Medicaid long term care spending going to nursing home care versus home and community based care, on adoption of bed hold policies, and some information on adoption of wage pass-throughs.

Finally, when the above procedure left gaps in my state policy records, I reached out to state Medicaid offices via phone and email. The conversations that ensued often were helpful for clarifying details about how payroll subsidies were implemented and about policy changes occurring within years for which I could find no clear records otherwise.

II.X.II Details On Defining Confounding Policy Events

In addition to adoption of payroll subsidies, state Medicaid programs have experimented with a wide array of policy changes. By my tally, over 100 major non-payroll subsidy policy changes have occurred across all states between 1996 and 2015. Often, payroll subsidies are adopted in conjunction with these other changes, as was the case in over ten states between 1996 and 2015. I exclude from my sample any policy event where one of these other major policy events occurred in the same year as, the year after, or the four years prior to the treatment event. This time window was chosen as other policy changes occurring within it would either directly contaminate the two-year symmetric study window around a state's treatment event or would occur close enough to its start that policies operating at a lag may have an effect within the window. States are also excluded from entering into control groups in the across-state analyses when they have major policy changes occurring within their corresponding treatment state's two-year symmetric study window.

Not all policy changes need be considered a major confounding policy event. Rather, policies only need to be excluded if they are likely to substantially affect nursing homes in a way that is correlated with the subsidy amounts offered to nursing homes. This suggests that policies disproportionately affecting nursing homes with large shares of residents on Medicaid deserve particular scrutiny. As such, I consider the following types of policy changes to be confounding:

- Adoption of substantial Medicaid nursing home pay-for-performance programs
- Changes to nursing home minimum staffing requirements
- Large changes in Medicaid nursing home reimbursement rates not associated with payroll subsidy changes, namely: 20 percent increases or 10 percent decreases in Medicaid daily payment rates (corresponding roughly with the 1 percent tails of the reimbursement rate change distribution)[53]
- Substantial changes in the structure of the Medicaid reimbursement system (e.g., changing from retrospective to prospective reimbursement)
- Adoption of a payroll subsidy not included within the treatment sample (e.g., because the subsidy formula cannot be simulated without access to confidential data)
- Adoption of some other type of labor incentive policy (e.g., adoption of payments that are a function of employee turnover rates or employee satisfaction surveys)

I take these policy changes to be major confounding events for a variety of reasons. First, all but the minimum staffing requirements directly have a disproportionate effect on nursing homes with larger shares of their residents on Medicaid. Although minimum staffing requirements bind on all nursing homes, they too may be expected to bind disproportionately on high Medicaid share nursing homes since high Medicaid nursing homes tend to have lower staffing ratios. Second, many of these policies generate incentives similar to payroll subsidies and so would be particularly difficult to distinguish from them. Finally, I restrict to "large" and "substantial" policy changes in a number of cases in order to distinguish major policy events from the kind of year-to-year minor policy changes that were

[53]Note that data on these changes are available only in the 2000s.

otherwise pervasive during this time period.[54] This requirement is also useful for screening out symbolic policies, e.g. policies offering public recognition and a token prize for high performance on various quality metrics, and pilot policies where only a few nursing homes are affected in a given year.[55]

A final issue relates to how I treat data for time periods where I have no policy information available. As a general rule, I take time periods to be confounded unless evidence strongly suggests otherwise. Data from prior to when I have any state policy regime information available is thus assumed to be unacceptable for study. When I am missing information on a time period between two periods for which I do have information, I consider this time period to be free of confounding policy events only if two conditions are met. First, the information available around the documentation gap must be sufficiently detailed as to show that the state was keeping high-quality records of policy changes around that time, suggesting that the absence of records is relatively more likely to correspond with an absence of policy changes than with the disappearance of disorganized, poorly kept records. Second, comparison of the state policy regime before and after the information gap must reveal no major differences in policy and must not contain anything else suggesting policy changes occurred between regime observations. Unless these two conditions are met, I assume data from policy information gaps are confounded and unsuitable for study.

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[54]Similarly, I also exclude policy changes that may be impactful in the long run but not within the timescale of my analysis. I placed in this category policies intending to limit demand for nursing homes by encouraging expanded usage of substitute long-term care services, such as home and community-based care, as well as policies intending to limit supply for nursing homes by placing further restrictions on certifying new nursing home beds.

[55]Adopting a stricter definition that classifies minor changes to state case mix adjustment systems and other types of very small programs as confounding events shrinks the treatment sample by four policy events. This attenuates the estimated employment and wage effects in the main within-state regressions, but the difference in results obtained using the two samples is not statistically significant.

II.X.III Details of State Specific Nursing Home Payroll Subsidies

The table below lists each subsidy policy event used in this study. The table lists for each state what type of subsidy they adopted (allocated, daily, or hourly) and when the policy change occurred, listing subsidy adoption and repeal dates separately. Subsidy start and repeal years are listed in terms of calendar years. Note that in most cases, the policy change occurred in the middle of the year, when most states transition between fiscal years. Start and repeal years are listed as "Not in Sample" if a confounding event prevents their inclusion. A repeal year is listed as "NA" if the subsidy either was not repealed or, when repealed, the subsidy amounts offered to each nursing home were added as a flat sum to their per Medicaid resident daily reimbursement rates. The table also lists the mean subsidies per resident-day as the average across all nursing home observations in the two years after subsidy adoption or as the average over the two years before repeal. Averages over two years are listed both because some states – in particular, Massachusetts – adjusted their rates in the second year of being offered and because nursing home-specific effective subsidy rates could vary from year to year as nursing home characteristics changed. Note that Massachusetts and Montana both have two policy events each: Massachusetts has an adoption and a repeal, while Montana has an adoption and a later reduction. Finally, the table also lists the number of nursing homes observed in each state at any point within the treatment sample.

State	Subsidy Type	Start Year	Repeal Year	Mean Subsidy / Resident-Day	Nursing Home Count
FL	Daily	2000	Not in Sample	$1.80	742
KS	Daily	1999	NA	$1.45	408
LA	Daily	1999	Not in Sample	$1.36	360
MA	Allocated	2000	2008	$2.85; $3.34 to 0	548
ME	Allocated	Not in Sample	2011	$5.22 to 0	108
MT	Allocated	1999	2011	$2.73; $3.87 to $2.59	103
ND	Hourly	2001	NA	$3.80	87
UT	Daily	1998	NA	$2.82	87
VA	Hourly	1999	NA	$1.71	280
VT	Allocated	1999	NA	$8.62	46
WA	Daily	2008	NA	$1.06	239
WY	Hourly	2000	NA	$2.72	40

II.XI Data Cleaning Appendix

The raw Medicaid and Medicare certification inspection survey data suffer from a number of problems that require correction, despite some pre-processing already having been done to the data for the years 2000-2015 by the LTCFocus project. First, while the certification inspections generating these data are in principle conducted annually, occasionally state agencies fail to conduct annual inspections on schedule or entirely. When nursing homes have their surveys conducted off-schedule (e.g., as a result of one survey being conducted in January and the next in December of the same year), the LTCFocus data address this by assigning to each year the survey conducted closest to that year's midpoint. I replicate this same correction procedure within my version of the same data from 1996-1999.

Second, the survey data were subject to a number of data entry and reporting errors that I correct through manual review. These errors potentially are a result of the data collection process involving complicated forms and multiple stages of manual data entry and re-entry. One such issue is that data on resident counts by payer status (Medicaid, Medicare, and all others) sometimes were entered into the wrong column, resulting in errors like entries for Medicaid and Medicare residents being transposed. For example, one can observe nursing homes with hundreds of residents reporting 70-20-10 Medicaid-Medicare-Other resident splits nearly every year for a decade that then, for a single year in the middle of the decade, report a 20-70-10 split. When this type of problem clearly presents itself, I manually correct it by transposing back the data to match the nursing home's typical resident share split.

A number of other problems affecting the data on resident counts by payer type occasionally produce obviously incorrect values, but are harder to precisely diagnose. For example, sometimes nursing homes report exactly even splits of residents by payer status across the categories of residents the nursing home admits (e.g., 33-33-33, or 50-0-50). When these exactly even splits are reported only for a small number of years and represent vary large deviations from the nursing home's usual resident allocation, I judge the exactly even splits as likely to be spurious data. In these and similar cases, I keep the nursing home's resident count the same and reallocate residents across payer categories to match the mean of the nursing home's resident allocations observed in the years surrounding the problematic data point or data points.

A final set of problems that I correct through manual review is the presence of observations from nursing homes surveyed in the midst of opening or closing. These nursing homes tend to report employment totals consistent with their eventual or former resident population but extremely low resident counts, resulting in very unlikely staffing ratios. I address this problem in part by dropping nursing home observations with two or fewer residents, but I also manually drop these nursing home observations when the nursing homes have greater than two residents and appear to report erroneous data.

Although I correct the above data issues by way of manual review, I did not review every single data point in my dataset. Rather, I reviewed all data for any nursing home with more than 5 observations and an observation meeting one of the following criteria:

- its initial observation in the dataset had a resident count an order of magnitude different from its immediately ensuing data
- its share of residents on Medicaid changed by 40 percentage points or more in a single year
- its number of total residents, Medicare residents, or Medicaid residents deviated from its median number in some year by a factor of 4 or more
- its share of residents on Medicaid (or its actual resident total) was reported as 0, although the median value was 40 percent (or 50 residents) or more

Note that within the estimation sample, only 415 nursing home year observations were manually corrected. Re-estimating the within-state employment per resident-day and direct care worker wage results after dropping all manually edited data points yields essentially identical point estimates as to when the corrections are included. Re-estimating with all observations but without corrections attenuates point estimates and expands standard errors.

In addition to these issues corrected through manual review, I also dropped certain observations according to some more general trimming rules. The motive for these trimming rules is that a variety of data entry errors, and in particular misplaced decimal point errors, were fairly common in the data collection process, resulting in some wildly inaccurate resident count and employment figures. I drop nursing home observations if they had registered nurse employment to resident-day ratios equal to 0 or exceeding 3.2 hours per resident-day, licensed practical nurse ratios less than 0.05 hours per resident-day or exceeding 4 hours per resident-day, or nursing assistant ratios less than 0.5 hours per resident-day or exceeding 6 hours per resident-day. These trimming thresholds were suggested by the Cowles Research Group, which calibrated them based off of interviews with nurses and other subject area experts. I additionally drop nursing homes that report more than 8 hours of total direct care worker hours per nursing home bed, a cleaning standard used in the LTCFocus data. Finally, I drop the small number of additional nursing homes reporting internally inconsistent observations, such as observations where the number of nursing home residents reported exceeds the number of beds in the facility.

An additional concern with these data are that some nursing homes appear to have duplicate observations in the sample, listing precisely identical resident counts, shares of residents on Medicaid and Medicare, and employee staffing ratios by employee type in consecutive years. In these cases, I randomly select one of the duplicate observations and drop the others.

A final concern with these data relates to the difficulty in linking nursing homes over time. This process is complicated by states occasionally adjusting nursing home provider numbers as well as due to provider numbers sometimes changing in response to nursing home changes in formal ownership, certification status, and the like. In order to address this, for the years 2000 to 2015, I use a panel identifier constructed by LTCFocus to track specific facilities over time. I then link between the 1996-1999 sample and the 2000-2015 sample using the reported facility provider numbers in 1999 and 2000. I then address the relatively small number of observations that fail to match between 1999 and 2000 or between years in the 1996-1999 sample using a matching procedure that links nursing homes using nursing home names and addresses.

II.XII Within-State Empirical Approach Appendix

II.XII.I Standard Errors

The primary test statistics I use in the within-state regressions are calculated using a randomization inference procedure inspired by Fisher (1925). Details on the method used for doing so are as follows. For each policy event, I randomly select a control state that, at the same time, had no other major policy events occurring. I assign that control state to replace the genuine treatment state for that policy event within my sample. Then, I calculate the effective subsidy amount that would have been received by each nursing home in the control state had it adopted the treatment state's subsidy formula.[56] I then conduct my regression of interest on this randomly constructed placebo sample and obtain a regression coefficient. I then repeat this procedure 1000 times to obtain a distribution of such coefficients. Given this distribution, I report its standard deviation and the percentile rank of the genuine treatment group's coefficient within it, taking any observations appearing

[56] When the treatment state uses an allocated payment style subsidy formula, I adjust the statewide total subsidy amount in the control state to match expenditure in the treatment state in per Medicaid resident-day terms.

in the top or bottom 2.5 percent of the effect distribution as being statistically significant at the 5 percent level.

The randomization inference approach above should address the design-based justification for clustering standard errors while avoiding the pitfalls of other approaches that produce undersized test statistics. Note that, strictly speaking, this randomization inference procedure does not test coefficients against the null hypothesis of subsidies having zero effect unless the mean effect of placebo subsidies in the control states is zero. As such, to the extent that the subsidy formulas generate any mechanical bias linking outcomes with the subsidy amount variables, these randomization inference test statistics test the observed subsidy effects less any mechanical bias against a null hypothesis of zero and so should be viewed as more conservative than other methods. However, note that the instrumental variable placebo effect estimates from the within-and-across state analysis using all control states are close to 0, suggesting that this procedure does in effect test against a null hypothesis of zero in at least the instrumental variable case.

In addition to the randomization inference test statistics, I present secondary test statistics based off of standard errors calculated analytically and clustered at the policy event level. Given that I have only 14 policy events, I likely have too few clusters for analytic clustering to provide reliable standard error estimates (Cameron et al., 2008). However, standard solutions like wild cluster bootstrapping perform poorly when clusters are few in number and very heterogeneous in size (Ibragimov and Müller, 2016), as is the case in my setting where clusters include states with wildly varying nursing home counts like Maine, Massachusetts, Montana, and Florida. Since wild cluster bootstrap-based test statistics turn out to be generally similar to those obtained using analytic clustering in my setting but carry the disadvantage of being more complicated without necessarily being superior, I only present the analytic standard errors as the comparison alternative to my preferred randomization inference-based approach. Note that, as a fourth alternative, I could calculate p-values according to the method proposed by Ibragimov and Müller (2016), which is valid even for small numbers of clusters of heterogeneous size. For their method, test statistics are obtained for each regression by estimating it separately within each cluster and then t-testing the set of coefficients from each cluster specific regression. This approach, however, seems likely to produce undersized test statistics in my setting since it does not allow for taking much advantage of the available independent within-state variation in treatment.

II.XII.II Estimates of Other Nursing Home Responses to Payroll Subsidies

In addition to effects on employment and wages, one may also be interested in the effect of payroll subsidies on other possible nursing home responses. Table II.13 presents within-state estimates of the effect of payroll subsidy receipt on nursing home resident counts, nursing home occupancy rates, an index of the severity of nursing home residents' care needs (Resident Acuity), and the share of nursing home residents on Medicaid. Per the randomization inference tests, no statistically significant subsidy effects on any of these outcomes are found at the 5 percent level in either the instrumental variable or ordinary least squares regressions. The point estimates and analytically clustered standard errors, however, are suggestive in the instrumental variable regressions of negative subsidy effects on resident counts, occupancy rates, and shares of residents on Medicaid coupled with increases in average resident care needs. The ordinary least squares regressions show still more negative effects on resident counts and occupancy rates, similar effects on resident care needs, and a similarly sized but opposite signed positive effect on nursing home shares of residents on Medicaid.

While the positive point estimates of subsidy effects on resident acuity are sensible in that they imply nursing homes take on residents with greater care needs as the cost of providing that care falls, the effects on resident counts and resident Medicaid shares are counterintuitive. While it is worth stressing that none of these effects are statistically significant, they perhaps deserve some comment nonetheless. First, negative effects on resident counts and positive effects on Medicaid shares in the ordinary least squares estimates may be a result of mechanical endogeneity between these outcomes and the subsidy amount offered, since subsidy amounts per resident-day are generally increasing in the share of residents on Medicaid (generating multiplication bias) and decreasing in nursing home resident counts (generating division bias). The fact that switching to the instrumental variable strategy shifts these estimates away from the expected direction of the mechanical bias would seem to support this hypothesis. Furthermore, the fact that the randomization inference method finds no statistically significant OLS effect on resident count, despite producing reasonably tight standard deviations, suggests that the distribution of placebo estimates is not centered with mean 0, consistent with the resident count placebo effect estimate distribution having been shifted by the proposed mechanical bias as well.

The instrumental variable estimate of a negative subsidy effect on Medicaid shares cannot be explained by this same mechanical bias, though may be due instead to non-classical measurement error. As nursing homes approach the 100 percent of residents on Medicaid upper bound, the distribution of Medicaid resident share shocks they face becomes censored on the right since no shock can push them above a 100 percent Medicaid share. These nursing homes, due to state subsidy formulas that offer effective subsidy amounts increasing in a nursing home's share of residents on Medicaid, also face higher effective subsidy amounts. As a result, there is a correlation between even instrumented subsidy amounts and negative Medicaid resident share shocks. This could, in turn, explain the negative coefficient estimate. The estimated effects on resident counts, occupancy, and Medicaid resident shares thus should be regarded as potentially spurious.[57]

II.XII.III Log-Log Within-State Estimates

In the main body of this research, I calculate the elasticities of employment and wages to subsidies given the subsidy effect estimates and the characteristics of the average nursing home. An alternative approach is to obtain them by re-estimating the within-state approach's primary regressions after log transforming the subsidy and outcome variables. As in the main body of the text, the log transformed subsidy variable and subsidy instrument are summed with state-wide sample-average Medicaid per resident-day payment rates prior to log transformation. The results from adopting this alternative approach are reported in Table II.14. These elasticities are similar to those calculated in the main text.

II.XII.IV Robustness to Dropping Low Medicaid Share and Low Subsidy Rate Nursing Homes

One concern about the within-state research design is that nursing homes with very low shares of residents on Medicaid or nursing homes otherwise receiving very low subsidy amounts may be on different trends in wages and employment than other nursing homes. In order to see if these potential differential pre-trends bias the within-state subsidy effect estimates, I re-estimate the primary within-state results dropping these nursing

[57] Additionally, regressing pre-treatment trends in shares of residents on Medicaid on eventual changes in subsidy amounts offered yields rather substantial negative pre-trends, further suggesting against interpreting these effects as unbiased.

homes. Table II.5 presents these results dropping the bottom 5 percent of nursing homes in terms of the absolute value of the mean change in subsidy amounts they faced. This 5 percent threshold falls at nursing homes receiving less than a 24.3 cent change in subsidy amount per resident-day. Table II.16 is similar but sets the threshold at the bottom 10 percent, or nursing homes receiving a change in subsidy amount per resident-day of less than 84.7 cents. Table II.17 gives these results for models estimated after dropping the bottom 5 percent of nursing homes in terms of within-treatment-sample average share of residents on Medicaid, so all nursing homes with 1.2 percent or fewer residents on Medicaid. Finally, Table II.18 drops all nursing homes with average share of residents on Medicaid below the 10 percent threshold, amounting to nursing homes with average Medicaid resident shares under 32.5 percent.

Across these four different cases, there is remarkably little variation in the estimated effect of subsidies on employment per resident-day and on average direct care worker hourly wages. While subsidy employment effect estimates attenuate somewhat when dropping the bottom ten percent of nursing homes by amount of subsidies received, dropping the bottom ten percent of nursing homes by share of residents on Medicaid increases point estimates. Cumulatively, this evidence is not consistent with differential pre-trends or other unusual behavior among less subsidized or among low Medicaid resident share nursing homes driving the primary within-state results.

II.XII.V Robustness to Restricting to One Policy Event per State

One concern with the within-state results is that inclusion of states with more than one policy event may yield biased results, potentially due to the characteristics used in the simulated subsidy instrument for the second policy event including nursing home observations from the first policy event's treatment period. To allay concerns that inclusion of these second policy events is biasing the primary results in some way, I re-estimate the within-state results dropping the second policy events occurring in Montana and Massachusetts, the only states with more than one policy event in the treatment sample. These results are in Table II.19 and are not substantially different from the full sample results: point estimates are essentially the same and carry similar randomization inference based test statistics.

II.XIII Difference-in-Differences Results Appendix

Prior literature relied on state difference-in-differences research designs to estimate the effect of payroll subsidy adoption on average nursing home staffing ratios. My state difference-in-differences estimates of the wage and employment effects of subsidy adoption using the set of all unconfounded control states as the control group are listed in Table II.20. These results are uniformly statistically insignificant, though point estimates suggest that the effect of subsidy adoption is to increase average employment of direct care workers across all nursing homes by 1.7 minutes per resident-day, an effect driven almost entirely by a staffing increase among nursing assistants. While statistical insignificance due to reduced precision in this setting should not be surprising — the difference-in-differences regression greatly reduces the amount of identifying variation used and so should have less power — this point estimate is almost an order of magnitude smaller than the 11.4 minute per resident-day average effect implied by the main within-state results.[58] While

[58] 11.4 minutes being approximately equal to the average subsidy amount offered, $2.32, times the effect of subsidies on direct care worker staffing, 4.9 minutes per resident-day per subsidy dollar.

also statistically insignificant, the point estimate for the effect on average direct care worker hourly wages is 22 cents, a figure closer to the implied average effect of 31 cents from the within-state analysis.[59] The comparable results using the geographic neighbor state approach are listed in Table II.21 and are similar in terms of wage results though smaller in terms of employment per resident-day results. Finally, the results from the difference-in-differences estimates using the synthetic controls approach are available in Table II.22 and are very similar to the results from the all unconfounded control states case.

The small magnitude of the difference-in-differences employment results, coupled with the relatively full sized wage results, lend themselves to a number of interpretations. One possibility is that the expected selection into treatment on negative employment per resident-day trends and positive hourly wage trends severely biases the estimated effects of subsidy adoption, even within geographically paired states and among states matched on pre-trends exhibited immediately prior to treatment. A second interpretation is that the difference-in-differences estimates may be attenuated by measurement error, generated by their inability to capture variation in the degree of subsidization across states adopting differently sized subsidies and within the few states which phased in their full subsidy payments over two years. A third interpretation is that these difference-in-differences estimates represent the genuine state average subsidy effects and that the within-state subsidy estimates yield positive employment effects only because they are picking up reallocation of workers across nursing homes within states. For this hypothesis to be true, nurse and nursing assistant labor, even in relatively large states like Florida and Massachusetts, must be supplied inelastically. The reasonably large positive difference-in-differences wage effects observed are consistent with this explanation, since market level wage increases cou-

[59] 31 cents being approximately equal to the average subsidy amount offered, \$2.32, times the effect of subsidies on the average direct care worker hourly wages, 13 cents per subsidy dollar.

pled with variation in subsidies across firms could be the mechanism through which this reallocation occurs. However, this hypothesis is not consistent with the semiparametric difference-in-differences evidence presented in Figure II.6 showing that employment does not decline at less subsidized nursing homes.

In order to provide additional evidence about which of these explanations is most likely, I conduct pre-trend tests here analogous to those used in the within-state pre-trend tests. Here, I conduct the difference-in-differences regressions within one-year symmetric treatment windows, varying the center of those windows through a range of years from three years prior to subsidy adoption to one year after subsidy adoption. The results from these regressions for the all states control group case are in Table II.23. These results point toward the presence of positive pre-trends in staffing and wages immediately prior to subsidy adoption, but with large negative pre-trends in the years still prior to those. While the estimated coefficients generally are not statistically significant at the 5 percent level, the pre-trend magnitudes are large relative to the main difference-in-differences estimates.

Table II.24 presents similar results to those in Table II.23, except for the geographic neighbors case, with the results being qualitatively similar to those found in the all states case. Note that in both cases, subsidy adoption effects tend to be larger in the year after subsidy adoption than in the year of subsidy adoption, potentially reflecting that the subsidies are more likely to be completely phased in during the second year relative to the first. Finally, Table II.25 presents these same results for the synthetic control sample. Relative to the other results, the synthetic controls procedure successfully helped to shrink the magnitude of pre-trend differences in the time period immediately prior to subsidy adoption. This is to be expected, as these are the pre-trends used in constructing the synthetic control states. However, even given the small immediate pre-trends, there appears to remain substantial negative pre-trend differences in the treatment windows centered two and three years prior to the actual adoption of subsidies.

Cumulatively, the evidence from these tests suggest that the difference-in-differences estimates of subsidy efficacy are not trustworthy. It appears that the twin problems of low statistical power and selection into subsidy adoption on employment and wage trends are very difficult to surmount using state-level policy variation. While these difference-in-differences estimates could be read to suggest that the within-state estimates are spurious and driven solely by shifting in employment across nursing homes, there is little evidence per Figure II.6 of there being reductions in employment at the nursing homes from which employment supposedly would have shifted. Overall, these results comport with expectations that the difference-in-differences research design may be a poor tool for use in this setting.

II.XIV Within-and-Across-State Pre-trends Appendix

In order to further test the validity of the subsidy effect estimates from the within-and-across-state empirical approach, I estimate pre-trend tests for these regressions similar to those used in the within-state case. In particular, within a one-year symmetric window of data centered at varying years before and after the policy change, I regress direct care worker employment per resident-day, average hourly wages, and payroll per resident-day on instrumented subsidy amounts offered and on instrumented placebo subsidy amounts offered, conditional on state-by-year and nursing home-by-policy event fixed effects.

Results for the all unconfounded control states case are in Table II.26. These exhibit subsidy effect pre-trends reasonably similar to those in the within-state results. Table II.27 presents these results for the sample using the paired geographic neighbors control group.

The estimated pre-trends in subsidy effects, while not necessarily small in magnitude, are statistically insignificant and inconsistent across time periods. Furthermore, note that the geographic neighbors control group seems to have had some success in suppressing pre-trend differences, reducing the magnitude of employment per resident-day pre-trends in the years just prior to subsidy adoption, though at the apparent cost of larger wage pre-trends. Finally, Table II.28 presents these results using the synthetic control group, yielding estimates similar to the results in the all unconfounded controls case. Cumulatively, this evidence suggests that the within-and-across-state approaches contribute to identification by partially suppressing the influence of pre-trend differences in employment and wages across nursing homes, as well as by controlling for any shocks correlated with subsidies contemporaneous to subsidy adoption and common between the treatment and control groups. However, these approaches do not completely purge the sample of the influence of all possible pre-trend differences across subsidized nursing homes, as pre-trend correlations with future subsidy receipt retain non-zero magnitudes in many cases.

II.XV Appendix Tables

Table II.13: Response of Resident Count, Occupancy, Resident Acuity, and Share of Residents on Medicaid to Subsidies per Resident-Day

	Residents	Occupancy	Resident Acuity	Pct Medicaid
OLS				
Subsidy	−2.024*	−1.726*	0.051*	1.124+
	(0.768)	(0.609)	(0.018)	(0.571)
Fisher Rank	0.448	0.483	0.922	0.948
Fisher SD	[0.567]	[0.567]	[0.018]	[0.377]
Mean Effect	−7.759%	−4.932%	1.160%	4.282%
IV				
Subsidy	−1.090	−0.876	0.046*	−1.116*
	(0.700)	(0.689)	(0.013)	(0.415)
Fisher Rank	0.083	0.348	0.950+	0.444
Fisher SD	[0.350]	[0.487]	[0.020]	[0.387]
Mean Effect	−4.179%	−2.502%	1.031%	−4.251%
Clusters	14	14	14	14
Obs	11685	11685	11685	11683

All estimates are coefficients from independent regressions of nursing home resident counts, nursing home occupancy rates, nursing home resident acuity, and the share of nursing home residents on Medicaid on simulated subsidy amounts offered conditional on state-by-year fixed effects and nursing home-by-policy event fixed effects. These regressions are estimated within two-year symmetric windows around each policy change. Simulated subsidies are the simulated maximum amount a nursing home could receive in terms of dollars per resident-day given its observed characteristics, which is instrumented for in the IV case using the amount simulated using the same formula but a fixed set of pre-subsidy adoption characteristics. Standard errors in parentheses are calculated analytically and clustered at the policy event level. Significance levels implied by these standard errors are denoted by either * (p <0.05) or +(p<0.1) and placed on the coefficient. The listed "Fisher Rank" is the percentile rank of the listed coefficient within a distribution of 1000 placebo coefficients, each calculated on a randomly selected placebo sample. Rankings in the extreme 2.5 percent and 5 percent tails subsidy of the placebo effect distribution are denoted with a * and a + respectively. The "Fisher SD" is the standard deviation of the distribution of placebo coefficients. The "Mean Effect" is the mean of the effect of subsidies across all subsidized nursing homes, calculated by scaling the simulated subsidy rates offered to nursing homes by the estimated regression coefficients and dividing by the corresponding outcome variables. The cluster count is the number of policy events in the sample, while the observation count is the number of nursing home-year observations in the sample.

Table II.14: Elasticity of Direct Care Worker Employment Minutes per Resident-Day, Average Hourly Wages, and Payroll per Resident-Day to Subsidies per Resident-Day

	DCW Staffing	CNA Staffing	Nurse Staffing	DCW Avg Wage	DCW PPR
OLS					
Subsidy	6.197*	6.251*	5.770*	1.334*	3.698*
	(1.596)	(1.822)	(1.614)	(0.452)	(1.360)
Fisher Rank	0.890	0.816	0.969+	0.996*	0.799
Fisher SD	[1.582]	[1.944]	[1.161]	[0.466]	[1.109]
IV					
Subsidy	4.140+	3.990+	4.187+	1.659*	2.756
	(1.996)	(1.917)	(2.316)	(0.562)	(2.365)
Fisher Rank	1.000*	1.000*	1.000*	0.994*	0.931
Fisher SD	[0.993]	[1.233]	[0.930]	[0.552]	[1.172]
Clusters	14	14	14	14	14
Obs	11685	11685	11685	7719	7785

All estimates are coefficients from independent regressions of log nursing home employment per resident-day by worker type, log direct care worker (DCW) average hourly wage, or log direct care worker payroll per resident-day on log simulated subsidy amounts offered conditional on state-by-year fixed effects and nursing home-by-policy event fixed effects. Prior to logging the simulated subsidy amounts, they are summed with their state's average daily Medicaid payment rate from within the sample, in order to allow for interpretation of the subsidies as a payment increase relative to the baseline Medicaid payments. These regressions are estimated within two-year symmetric windows around each policy change. Simulated subsidies are the simulated maximum amount a nursing home could receive in terms of dollars per resident-day given its observed characteristics, which is instrumented for in the IV case using the amount simulated using the same formula but a fixed set of pre-subsidy adoption characteristics. Standard errors in parentheses are calculated analytically and clustered at the policy event level. Significance levels implied by these standard errors are denoted by either * ($p < 0.05$) or +($p<0.1$) and placed on the coefficient. The listed "Fisher Rank" is the percentile rank of the listed coefficient within a distribution of 1000 placebo coefficients, each calculated on a randomly selected placebo sample. Rankings in the extreme 2.5 percent and 5 percent tails subsidy of the placebo effect distribution are denoted with a * and a + respectively. The "Fisher SD" is the standard deviation of the distribution of placebo coefficients. The "Mean Effect" is the mean of the effect of subsidies across all subsidized nursing homes, calculated by scaling the simulated subsidy rates offered to nursing homes by the estimated regression coefficients and dividing by the corresponding outcome variables. The cluster count is the number of policy events in the sample, while the observation count is the number of nursing home-year observations in the sample.

Table II.15: Response of Direct Care Worker Employment, Wages, and Payroll per Resident-Day to Subsidies per Resident-Day in Sample Excluding Bottom 5 percent of Nursing Homes by Subsidy Amount

	DCW Staffing	CNA Staffing	Nurse Staffing	Avg DCW Wage	DCW PPR
Subsidy	5.252+	3.398+	1.853	0.103	0.947
	(2.883)	(1.822)	(1.154)	(0.059)	(1.121)
Fisher Rank	0.999*	1.000*	0.996*	0.934	0.877
Fisher SD	[1.457]	[1.168]	[0.558]	[0.066]	[0.748]
Clusters	14	14	14	14	14
Obs	11150	11150	11150	7617	7744

All estimates are coefficients from independent instrumental variable regressions where the bottom 5 percent of nursing homes by the absolute value of the change in subsidization are dropped (i.e., dropping nursing homes with less than a 24.3 cent change in subsidy per resident-day). Outcome variables include direct care worker (DCW), certified nursing assistant (CNA), and licensed nurse minutes per resident-day as well as direct care worker average hourly wages and direct care worker payroll per resident-day. These are regressed on simulated subsidy amounts offered conditional on state-by-year fixed effects and nursing home-by-policy event fixed effects. These regressions are estimated within two-year symmetric windows around each policy change. Simulated subsidies are the simulated maximum amount a nursing home could receive in terms of dollars per resident-day given its observed characteristics, which is instrumented for using the amount simulated using the same formula but a fixed set of pre-subsidy adoption characteristics. Standard errors in parentheses are calculated analytically and clustered at the policy event level. Significance levels implied by these standard errors are denoted by either * ($p < 0.05$) or +($p<0.1$) and placed on the coefficient. The listed "Fisher Rank" is the percentile rank of the listed coefficient within a distribution of 1000 placebo coefficients, each calculated on a randomly selected placebo sample. Rankings in the extreme 2.5 percent and 5 percent tails subsidy of the placebo effect distribution are denoted with a * and a + respectively. The "Fisher SD" is the standard deviation of the distribution of placebo coefficients. The cluster count is the number of policy events in the sample, while the observation count is the number of nursing home-year observations in the sample.

Table II.16: Response of Direct Care Worker Employment, Wages, and Payroll per Resident-Day to Subsidies per Resident-Day in Sample Excluding Bottom 10 percent of Nursing Homes by Subsidy Amount

	DCW Staffing	CNA Staffing	Nurse Staffing	Avg DCW Wage	DCW PPR
Subsidy	3.884*	2.323*	1.560	0.090	0.703
	(1.743)	(1.019)	(0.933)	(0.057)	(1.043)
Fisher Rank	0.999*	0.995*	0.998*	0.890	0.807
Fisher SD	[1.540]	[1.250]	[0.574]	[0.072]	[0.761]
Clusters	14	14	14	14	14
Obs	10606	10606	10606	7313	7475

All estimates are coefficients from independent instrumental variable regressions where the bottom 10 percent of nursing homes by the absolute value of the change in subsidization are dropped (i.e., dropping nursing homes with less than a 84.7 cent change in subsidy per resident-day). Outcome variables include direct care worker (DCW), certified nursing assistant (CNA), and licensed nurse minutes per resident-day as well as direct care worker average hourly wages and direct care worker payroll per resident-day. These are regressed on simulated subsidy amounts offered conditional on state-by-year fixed effects and nursing home-by-policy event fixed effects. These regressions are estimated within two-year symmetric windows around each policy change. Simulated subsidies are the simulated maximum amount a nursing home could receive in terms of dollars per resident-day given its observed characteristics, which is instrumented for using the amount simulated using the same formula but a fixed set of pre-subsidy adoption characteristics. Standard errors in parentheses are calculated analytically and clustered at the policy event level. Significance levels implied by these standard errors are denoted by either * ($p < 0.05$) or +($p<0.1$) and placed on the coefficient. The listed "Fisher Rank" is the percentile rank of the listed coefficient within a distribution of 1000 placebo coefficients, each calculated on a randomly selected placebo sample. Rankings in the extreme 2.5 percent and 5 percent tails subsidy of the placebo effect distribution are denoted with a * and a + respectively. The "Fisher SD" is the standard deviation of the distribution of placebo coefficients. The cluster count is the number of policy events in the sample, while the observation count is the number of nursing home-year observations in the sample.

Table II.17: Response of Direct Care Worker Employment, Wages, and Payroll per Resident-Day to Subsidies per Resident-Day in Sample Excluding Bottom 5 percent of Nursing Homes by Share of Residents on Medicaid

	DCW Staffing	CNA Staffing	Nurse Staffing	Avg DCW Wage	DCW PPR
Subsidy	5.364+	3.322+	2.041	0.102+	1.042
	(2.911)	(1.679)	(1.305)	(0.051)	(1.168)
Fisher Rank	1.000*	1.000*	0.997*	0.953+	0.906
Fisher SD	[1.368]	[1.081]	[0.553]	[0.062]	[0.689]
Clusters	14	14	14	14	14
Obs	11186	11186	11186	7646	7760

All estimates are coefficients from independent instrumental variable regressions where the bottom 5 percent of nursing homes by their own sample average share of residents on Medicaid are dropped (i.e., dropping nursing homes with less than 1.2 percent of residents on Medicaid). Outcome variables include direct care worker (DCW), certified nursing assistant (CNA), and licensed nurse minutes per resident-day as well as direct care worker average hourly wages and direct care worker payroll per resident-day. These are regressed on simulated subsidy amounts offered conditional on state-by-year fixed effects and nursing home-by-policy event fixed effects. These regressions are estimated within two-year symmetric windows around each policy change. Simulated subsidies are the simulated maximum amount a nursing home could receive in terms of dollars per resident-day given its observed characteristics, which is instrumented for using the amount simulated using the same formula but a fixed set of pre-subsidy adoption characteristics. Standard errors in parentheses are calculated analytically and clustered at the policy event level. Significance levels implied by these standard errors are denoted by either * ($p < 0.05$) or +($p<0.1$) and placed on the coefficient. The listed "Fisher Rank" is the percentile rank of the listed coefficient within a distribution of 1000 placebo coefficients, each calculated on a randomly selected placebo sample. Rankings in the extreme 2.5 percent and 5 percent tails subsidy of the placebo effect distribution are denoted with a * and a + respectively. The "Fisher SD" is the standard deviation of the distribution of placebo coefficients. The cluster count is the number of policy events in the sample, while the observation count is the number of nursing home-year observations in the sample.

Table II.18: Response of Direct Care Worker Employment, Wages, and Payroll per Resident-Day to Subsidies per Resident-Day in Sample Excluding Bottom 10 percent of Nursing Homes by Share of Residents on Medicaid

	DCW Staffing	CNA Staffing	Nurse Staffing	Avg DCW Wage	DCW PPR
Subsidy	5.976+	3.939+	2.036	0.130+	1.024
	(3.018)	(1.940)	(1.220)	(0.065)	(1.198)
Fisher Rank	1.000*	1.000*	1.000*	0.938	0.904
Fisher SD	[1.591]	[1.264]	[0.590]	[0.080]	[0.671]
Clusters	14	14	14	14	14
Obs	10744	10744	10744	7486	7635

All estimates are coefficients from independent instrumental variable regressions where the bottom 10 percent of nursing homes by their own sample average share of residents on Medicaid are dropped (i.e., dropping nursing homes with less than 32.5 percent of residents on Medicaid). Outcome variables include direct care worker (DCW), certified nursing assistant (CNA), and licensed nurse minutes per resident-day as well as direct care worker average hourly wages and direct care worker payroll per resident-day. These are regressed on simulated subsidy amounts offered conditional on state-by-year fixed effects and nursing home-by-policy event fixed effects. These regressions are estimated within two-year symmetric windows around each policy change. Simulated subsidies are the simulated maximum amount a nursing home could receive in terms of dollars per resident-day given its observed characteristics, which is instrumented for using the amount simulated using the same formula but a fixed set of pre-subsidy adoption characteristics. Standard errors in parentheses are calculated analytically and clustered at the policy event level. Significance levels implied by these standard errors are denoted by either * ($p < 0.05$) or +($p<0.1$) and placed on the coefficient. The listed "Fisher Rank" is the percentile rank of the listed coefficient within a distribution of 1000 placebo coefficients, each calculated on a randomly selected placebo sample. Rankings in the extreme 2.5 percent and 5 percent tails subsidy of the placebo effect distribution are denoted with a * and a + respectively. The "Fisher SD" is the standard deviation of the distribution of placebo coefficients. The cluster count is the number of policy events in the sample, while the observation count is the number of nursing home-year observations in the sample.

Table II.19: Response of Direct Care Worker Employment, Wages, and Payroll per Resident-Day to Subsidies per Resident-Day in Sample Including Only One Policy Event per State

	DCW Staffing	CNA Staffing	Nurse Staffing	Avg DCW Wage	DCW PPR
Subsidy	4.892	3.116+	1.774	0.135+	0.952
	(2.942)	(1.600)	(1.379)	(0.068)	(1.540)
Fisher Rank	1.000*	1.000*	1.000*	0.983*	0.926
Fisher SD	[1.136]	[0.906]	[0.483]	[0.063]	[0.646]
Clusters	12	12	12	12	12
Obs	9756	9756	9756	6167	6225

All estimates are coefficients from independent instrumental variable regressions where the second policy event in multi-policy event states is dropped (i.e., the second event in Massachusetts and Montana). Outcome variables include direct care worker (DCW), certified nursing assistant (CNA), and licensed nurse minutes per resident-day as well as direct care worker average hourly wages and direct care worker payroll per resident-day. These are regressed on simulated subsidy amounts offered conditional on state-by-year fixed effects and nursing home-by-policy event fixed effects. These regressions are estimated within two-year symmetric windows around each policy change. Simulated subsidies are the simulated maximum amount a nursing home could receive in terms of dollars per resident-day given its observed characteristics, which is instrumented for using the amount simulated using the same formula but a fixed set of pre-subsidy adoption characteristics. Standard errors in parentheses are calculated analytically and clustered at the policy event level. Significance levels implied by these standard errors are denoted by either * ($p < 0.05$) or +($p<0.1$) and placed on the coefficient. The listed "Fisher Rank" is the percentile rank of the listed coefficient within a distribution of 1000 placebo coefficients, each calculated on a randomly selected placebo sample. Rankings in the extreme 2.5 percent and 5 percent tails subsidy of the placebo effect distribution are denoted with a * and a + respectively. The "Fisher SD" is the standard deviation of the distribution of placebo coefficients. The cluster count is the number of policy events in the sample, while the observation count is the number of nursing home-year observations in the sample.

Table II.20: Difference-in-Differences Effect of Subsidy Adoption on Direct Care Worker Employment, Wages, and Payroll per Resident-Day in Subsidy States Relative to in All Control States

	DCW Staffing	CNA Staffing	Nurse Staffing	Avg DCW Wage	DCW PPR
Subsidy Adoption	1.651	1.594	0.057	0.222	0.650
	(1.213)	(1.227)	(0.326)	(0.134)	(0.933)
Clusters	28	28	28	28	28
Obs	326852	326852	326852	207280	208096

All estimates are coefficients from independent instrumental variable regressions of minutes of direct care worker (DCW), certified nursing assistant (CNA), and licensed nurse employment per resident-day or of the average direct care worker hourly wages on an indicator variable that is 1 in states and years where subsidies are being offered and 0 otherwise. These regressions are conditional on nursing home-by-policy event fixed effects and year fixed effects shared between each pair of treatment and control states. The regressions are estimated within a two-year symmetric window around each policy change using the set of all unconfounded control states as the control group for each policy event. Standard errors in parentheses are calculated analytically and clustered at the policy event by treatment vs. control group level. Significance levels implied by these standard errors are denoted by either * ($p < 0.05$) or +($p<0.1$). The cluster count is the number of treatment policy events and paired control groups in the sample, while the observation count is the number of nursing home-year observations in the sample.

Table II.21: Difference-in-Differences Effect of Subsidy Adoption on Direct Care Worker Employment, Wages, and Payroll per Resident-Day in Subsidy States Relative to in Geographically Neighboring Control States

	DCW Staffing	CNA Staffing	Nurse Staffing	Avg DCW Wage	DCW PPR
Subsidy Adoption	0.359	0.332	0.027	0.210	0.466
	(0.661)	(0.610)	(0.245)	(0.133)	(0.753)
Clusters	40	40	40	40	40
Obs	39789	39789	39789	26731	26856

All estimates are coefficients from independent instrumental variable regressions of minutes of direct care worker (DCW), certified nursing assistant (CNA), and licensed nurse employment per resident-day or of the average direct care worker hourly wages on an indicator variable that is 1 in states and years where subsidies are being offered and 0 otherwise. These regressions are conditional on nursing home-by-policy event fixed effects and year fixed effects shared within each pair of treatment and control states. The regressions are estimated within a two-year symmetric window around each policy change using paired geographically neighboring states as the control group for each policy event. Standard errors in parentheses are calculated analytically and clustered at the treatment state-neighbor pair level, resulting in one cluster per geographic neighbor. Significance levels implied by these standard errors are denoted by either * ($p < 0.05$) or +($p<0.1$). The cluster count is the number of treatment policy events and paired control groups in the sample, while the observation count is the number of nursing home-year observations in the sample.

Table II.22: Difference-in-Differences Effect of Subsidy Adoption on Direct Care Worker Employment, Wages, and Payroll per Resident-Day in Subsidy States Relative to in Synthetic Control States

	DCW Staffing	CNA Staffing	Nurse Staffing	Avg DCW Wage	DCW PPR
Subsidy Adoption	1.493	1.826+	−0.333	0.315*	1.694+
	(1.002)	(0.894)	(0.502)	(0.116)	(0.910)
Fisher Rank	0.851	0.886	0.403	0.995*	0.935
Fisher SD	[1.420]	[1.357]	[0.462]	[0.118]	[1.155]
Clusters	28	28	28	28	28
Obs	49115	49115	49115	31497	31649

All estimates are coefficients from independent instrumental variable regressions of minutes of direct care worker (DCW), certified nursing assistant (CNA), and licensed nurse employment per resident-day or of the average direct care worker hourly wages on an indicator variable that is 1 in states and years where subsidies are being offered and 0 otherwise. These regressions are conditional on nursing home-by-policy event fixed effects and year fixed effects shared between each pair of treatment and synthetic control states. The regressions are estimated within a two-year symmetric window around each policy change using the set of all unconfounded control states as the control group for each policy event. Standard errors in parentheses are calculated analytically and clustered at the policy event by treatment vs. synthetic control group level. Significance levels implied by these standard errors are denoted by either * ($p < 0.05$) or +($p<0.1$). The listed "Fisher Rank" is the percentile rank of the listed coefficient within a distribution of 1000 placebo coefficients, each calculated on a randomly selected placebo sample. Rankings in the extreme 2.5 percent and 5 percent tails subsidy of the placebo effect distribution are denoted with a * and a + respectively. The "Fisher SD" is the standard deviation of the distribution of placebo coefficients. The cluster count is the number of treatment policy events and paired control groups in the sample, while the observation count is the number of nursing home-year observations in the sample.

Table II.23: Difference-in-Differences Effect of Subsidy Adoption on Direct Care Worker Employment, Wages, and Payroll per Resident-Day in Subsidy States Relative to in All Control States, Estimated within 1 Year Symmetric Windows Centered Around Varying Years Relative to Subsidy Adoption or Repeal

	DCW Staffing	CNA Staffing	Nurse Staffing	Avg DCW Wage	DCW PPR
3 Years Before					
Subsidy Adoption	−3.364*	−2.173*	−1.190*	−0.295*	−1.521*
	(1.219)	(0.896)	(0.454)	(0.075)	(0.567)
Clusters	16	16	16	16	16
Obs	69608	69608	69608	43210	43376
2 Years Before					
Subsidy Adoption	−1.860+	−1.183+	−0.677	0.019	−0.973+
	(0.965)	(0.655)	(0.527)	(0.114)	(0.541)
Clusters	26	26	26	26	26
Obs	101664	101664	101664	59774	59946
1 Year Before					
Subsidy Adoption	1.501	0.694	0.806*	0.075	−0.823
	(0.937)	(0.779)	(0.248)	(0.129)	(0.920)
Clusters	28	28	28	28	28
Obs	126888	126888	126888	75362	75766
Contemporaneous					
Subsidy Adoption	0.255	0.351	−0.096	0.165	0.444
	(1.128)	(0.968)	(0.365)	(0.126)	(0.953)
Clusters	28	28	28	28	28
Obs	145638	145638	145638	84426	84866
1 Year After					
Subsidy Adoption	0.778	0.928	−0.150	0.086	1.014
	(0.792)	(0.752)	(0.349)	(0.081)	(0.675)
Clusters	28	28	28	28	28
Obs	133394	133394	133394	79890	80408

All estimates are coefficients from independent instrumental variable regressions of minutes of direct care worker (DCW), certified nursing assistant (CNA), and licensed nurse employment per resident-day or of the average direct care worker hourly wages on an indicator variable that is 1 in states and years where subsidies are being offered and 0 otherwise. These regressions are conditional on nursing home-by-policy event fixed effects and year fixed effects shared between each pair of treatment and control states. The regressions are estimated within a one-year symmetric window centered at the time specified in the panel relative to when the actual policy change occurred. The full sample included here includes as controls all unconfounded control states. Standard errors in parentheses are calculated analytically and clustered at the policy event by treatment vs. control group level. Significance levels implied by these standard errors are denoted by either * (p <0.05) or +(p<0.1). The cluster count is the number of treatment policy events and paired control groups in the sample, while the observation count is the number of nursing home-year observations in the sample.

Table II.24: Difference-in-Differences Effect of Subsidy Adoption on Direct Care Worker Employment, Wages, and Payroll per Resident-Day in Subsidy States Relative to in Geographically Neighboring Control States, Estimated within 1 Year Symmetric Windows Centered Around Varying Years Relative to Subsidy Adoption or Repeal

	DCW Staffing	CNA Staffing	Nurse Staffing	Avg DCW Wage	DCW PPR
3 Years Before					
Subsidy Adoption	−4.423*	−3.561*	−0.861	−0.680*	−3.981+
	(1.584)	(1.321)	(0.570)	(0.298)	(2.000)
Clusters	23	23	23	23	23
Obs	8824	8824	8824	5570	5652
2 Years Before					
Subsidy Adoption	−2.053+	−1.553+	−0.500	0.030	−1.685
	(1.100)	(0.832)	(0.419)	(0.055)	(1.062)
Clusters	35	35	35	34	34
Obs	12802	12802	12802	7820	7882
1 Year Before					
Subsidy Adoption	0.703	0.171	0.531+	0.166	1.378
	(0.978)	(0.768)	(0.305)	(0.157)	(1.279)
Clusters	40	40	40	40	40
Obs	16040	16040	16040	9954	9912
Contemporaneous					
Subsidy Adoption	−1.153	−0.663	−0.490	0.135	−0.497
	(0.823)	(0.603)	(0.416)	(0.163)	(0.831)
Clusters	40	40	40	40	40
Obs	18228	18228	18228	11458	11530
1 Year After					
Subsidy Adoption	1.281	0.680	0.601	−0.007	1.064
	(0.862)	(0.783)	(0.429)	(0.127)	(0.717)
Clusters	40	40	40	40	40
Obs	16744	16744	16744	10924	11078

All estimates are coefficients from independent instrumental variable regressions of minutes of direct care worker (DCW), certified nursing assistant (CNA), and licensed nurse employment per resident-day or of the average direct care worker hourly wages on an indicator variable that is 1 in states and years where subsidies are being offered and 0 otherwise. These regressions are conditional on nursing home-by-policy event fixed effects and year fixed effects shared between each pair of treatment and control states. The regressions are estimated within a one-year symmetric window centered at the time specified in the panel relative to when the actual policy change occurred. The full sample included here includes only subsidy states and their geographic neighbors. Standard errors in parentheses are calculated analytically and clustered at the treatment state-neighbor pair level, resulting in one cluster per geographic neighbor. Significance levels implied by these standard errors are denoted by either * ($p < 0.05$) or +($p<0.1$). The cluster count is the number of treatment policy events and paired control groups in the sample, while the observation count is the number of nursing home-year observations in the sample.

Table II.25: Difference-in-Differences Effect of Subsidy Adoption on Direct Care Worker Employment, Wages, and Payroll per Resident-Day in Subsidy States Relative to in Synthetic Control States, Estimated within 1 Year Symmetric Windows Centered Around Varying Years Relative to Subsidy Adoption or Repeal

	DCW Staffing	CNA Staffing	Nurse Staffing	Avg DCW Wage	DCW PPR
3 Years Before					
Subsidy Adoption	−3.923*	−3.181*	−0.741*	−0.606*	−4.837*
	(1.279)	(0.983)	(0.333)	(0.203)	(2.025)
Clusters	16	16	16	16	16
Obs	9972	9972	9972	6140	6228
2 Years Before					
Subsidy Adoption	−0.993	−0.368	−0.624	0.128	−2.116*
	(1.113)	(0.639)	(0.591)	(0.140)	(0.906)
Clusters	26	26	26	26	26
Obs	15536	15536	15536	8992	9036
1 Year Before					
Subsidy Adoption	0.245	0.176	0.067	0.044	0.014
	(0.144)	(0.122)	(0.081)	(0.053)	(0.428)
Clusters	28	28	28	28	28
Obs	18928	18928	18928	11278	11332
Contemporaneous					
Subsidy Adoption	0.098	0.344	−0.246	0.274*	1.189
	(1.033)	(0.811)	(0.471)	(0.101)	(0.976)
Clusters	28	28	28	28	28
Obs	21996	21996	21996	13078	13142
1 Year After					
Subsidy Adoption	1.067	1.315+	−0.248	0.032	0.546
	(0.687)	(0.760)	(0.341)	(0.124)	(0.830)
Clusters	28	28	28	28	28
Obs	20214	20214	20214	12346	12488

All estimates are coefficients from independent instrumental variable regressions of minutes of direct care worker (DCW), certified nursing assistant (CNA), and licensed nurse employment per resident-day or of the average direct care worker hourly wages on an indicator variable that is 1 in states and years where subsidies are being offered and 0 otherwise. These regressions are conditional on nursing home-by-policy event fixed effects and year fixed effects shared between each pair of treatment and synthetic control states. The regressions are estimated within a one-year symmetric window centered at the time specified in the panel relative to when the actual policy change occurred. The sample included here consists of just treatment states and their synthetic control states. Standard errors in parentheses are calculated analytically and clustered at the policy event by treatment vs. control group level. Significance levels implied by these standard errors are denoted by either * ($p < 0.05$) or +($p<0.1$). The cluster count is the number of treatment policy events and paired control groups in the sample, while the observation count is the number of nursing home-year observations in the sample.

Table II.26: Response of Direct Care Worker Employment, Wages, and Payroll per Resident-Day to Subsidies per Resident-Day in Subsidy States, Relative to the Effect of Placebo Subsidies in All Control States, Estimated within 1 Year Symmetric Windows Centered Around Varying Years Relative to Subsidy Adoption or Repeal

	DCW Staffing	CNA Staffing	Nurse Staffing	Avg DCW Wage	DCW PPR
3 Years Before					
Placebo Effect	0.152	0.078	0.073	0.031	0.093
	(0.251)	(0.161)	(0.107)	(0.022)	(0.118)
Subsidy Effect	−0.447	−0.797	0.350+	0.097	2.150*
	(1.360)	(1.351)	(0.167)	(0.063)	(0.884)
Clusters	16	16	16	16	16
Obs	68778	68778	68778	42724	42972
2 Years Before					
Placebo Effect	0.047	0.193*	−0.146	0.003	0.164
	(0.216)	(0.089)	(0.169)	(0.010)	(0.214)
Subsidy Effect	0.111	0.183	−0.072	0.047	0.856
	(1.966)	(1.053)	(1.101)	(0.064)	(0.661)
Clusters	26	26	26	26	26
Obs	100708	100708	100708	59242	59494
1 Year Before					
Placebo Effect	−0.850	−0.494	−0.356	−0.002	0.038
	(0.673)	(0.448)	(0.237)	(0.017)	(0.256)
Subsidy Effect	−1.612	−1.498	−0.113	0.026	0.299
	(2.885)	(1.874)	(1.148)	(0.055)	(0.778)
Clusters	28	28	28	28	28
Obs	125580	125580	125580	74660	75172
Contemporaneous					
Placebo Effect	0.104	0.121	−0.016	0.001	0.159
	(0.389)	(0.262)	(0.156)	(0.018)	(0.238)
Subsidy Effect	5.622+	4.064*	1.556	0.137*	0.705
	(2.976)	(1.825)	(1.248)	(0.053)	(1.151)
Clusters	28	28	28	28	28
Obs	140704	140704	140704	82272	83006
1 Year After					
Placebo Effect	0.340	0.181	0.159*	−0.002	0.005
	(0.204)	(0.185)	(0.067)	(0.008)	(0.179)
Subsidy Effect	0.556	−0.453	1.009	−0.042	1.053
	(2.290)	(1.239)	(1.146)	(0.028)	(0.850)
Clusters	28	28	28	28	28
Obs	129098	129098	129098	77732	78426

All estimates are coefficients from independent instrumental variable regressions. Outcome variables include direct care worker (DCW), certified nursing assistant (CNA), and licensed nurse minutes per resident-day as well as direct care worker average hourly wages and direct care worker payroll per resident-day. All regressions regress one of those outcomes on the simulated effective subsidy amount that was or would have been offered to each nursing home, an interaction term between this variable and a dummy variable that is 1 in treatment states and 0 in control states, state-by-year-by-policy-event fixed effects, and nursing home fixed effects. The regressions are estimated within a one-year symmetric window centered at the time specified in the panel relative to when the actual policy change occurred. The sample here includes all subsidy states and all unconfounded control states. The “Placebo Effect” coefficient reports the effect of the placebo subsidies within the control group, while the “Subsidy Effect” coefficient reports the interaction term that reflects the effect of subsidies in the treatment group over and above the placebo effect in the control group. Standard errors in parentheses are calculated analytically and clustered at the policy event by treatment vs. control group level. Significance levels implied by these standard errors are denoted by either * ($p < 0.05$) or +($p<0.1$) and placed on the coefficient. The cluster count is the number of treatment policy events and paired control groups in the sample, while the observation count is the number of nursing home-year observations in the sample.

Table II.27: Response of Direct Care Worker Employment, Wages, and Payroll per Resident-Day to Subsidies per Resident-Day in Subsidy States, Relative to the Effect of Placebo Subsidies in Geographically Neighboring Control States, Estimated within 1 Year Symmetric Windows Centered Around Varying Years Relative to Subsidy Adoption or Repeal

	DCW Staffing	CNA Staffing	Nurse Staffing	Avg DCW Wage	DCW PPR
3 Years Before					
Placebo Effect	−1.217	−0.808	−0.410	0.132	0.805
	(2.089)	(1.495)	(0.839)	(0.207)	(1.155)
Subsidy Effect	0.874	0.106	0.769	−0.022	0.663
	(2.234)	(1.703)	(0.849)	(0.221)	(1.429)
Clusters	23	23	23	23	23
Obs	8772	8772	8772	5546	5632
2 Years Before					
Placebo Effect	−0.786	0.152	−0.938	0.205+	1.814+
	(1.190)	(0.976)	(0.605)	(0.116)	(0.972)
Subsidy Effect	0.472	0.081	0.391	−0.193	−0.650
	(1.725)	(1.213)	(0.932)	(0.139)	(1.284)
Clusters	35	35	35	34	34
Obs	12742	12742	12742	7790	7858
1 Year Before					
Placebo Effect	−1.079	−1.202	0.123	−0.119+	−0.542
	(1.436)	(0.981)	(0.555)	(0.063)	(0.369)
Subsidy Effect	0.221	0.466	−0.244	0.153	0.717
	(2.200)	(1.535)	(0.892)	(0.091)	(0.955)
Clusters	40	40	40	40	40
Obs	15970	15970	15970	9910	9888
Contemporaneous					
Placebo Effect	0.376	0.852	−0.476	−0.074+	0.171
	(1.092)	(0.668)	(0.506)	(0.042)	(0.322)
Subsidy Effect	4.124+	2.312	1.810+	0.227*	0.840
	(2.305)	(1.465)	(0.946)	(0.069)	(1.329)
Clusters	40	40	40	40	40
Obs	17806	17806	17806	11258	11350
1 Year After					
Placebo Effect	0.268	0.535	−0.266	0.052+	−0.178
	(1.011)	(0.813)	(0.397)	(0.027)	(0.493)
Subsidy Effect	−1.018	−1.537	0.518	−0.079+	0.785
	(1.876)	(1.260)	(0.907)	(0.041)	(1.018)
Clusters	40	40	40	40	40
Obs	16358	16358	16358	10722	10894

All estimates are coefficients from independent instrumental variable regressions. Outcome variables include direct care worker (DCW), certified nursing assistant (CNA), and licensed nurse minutes per resident-day as well as direct care worker average hourly wages and direct care worker payroll per resident-day. All regressions regress one of those outcomes on the simulated effective subsidy amount that was or would have been offered to each nursing home, an interaction term between this variable and a dummy variable that is 1 in treatment states and 0 in control states, state-by-year-by-policy-event fixed effects, and nursing home fixed effects. The regressions are estimated within a one-year symmetric window centered at the time specified in the panel relative to when the actual policy change occurred. The sample here includes all subsidy states and their paired geographic neighbor states. The "Placebo Effect" coefficient reports the effect of the placebo subsidies within the control group, while the "Subsidy Effect" coefficient reports the interaction term that reflects the effect of subsidies in the treatment group over and above the placebo effect in the control group. Standard errors in parentheses are calculated analytically and clustered at the treatment state-neighbor pair level, resulting in one cluster per geographic neighbor. Significance levels implied by these standard errors are denoted by either * (p <0.05) or +(p<0.1) and placed on the coefficient. The cluster count is the number of treatment policy events and paired control groups in the sample, while the observation count is the number of nursing home-year observations in the sample.

Table II.28: Response of Direct Care Worker Employment, Wages, and Payroll per Resident-Day to Subsidies per Resident-Day in Subsidy States, Relative to the Effect of Placebo Subsidies in Synthetic Control States, Estimated within 1 Year Symmetric Windows Centered Around Varying Years Relative to Subsidy Adoption or Repeal

	DCW Staffing	CNA Staffing	Nurse Staffing	Avg DCW Wage	DCW PPR
3 Years Before					
Placebo Effect	−1.396	−1.969*	0.573+	0.104	−0.363
	(0.814)	(0.621)	(0.317)	(0.066)	(0.247)
Subsidy Effect	1.102	1.251	−0.149	0.024	2.606*
	(1.565)	(1.479)	(0.341)	(0.089)	(0.910)
Clusters	16	16	16	16	16
Obs	21396	21396	21396	11556	11686
2 Years Before					
Placebo Effect	−1.394+	−0.397	−0.996*	0.061	−0.009
	(0.771)	(0.482)	(0.396)	(0.109)	(0.738)
Subsidy Effect	1.551	0.772	0.778	−0.011	1.028
	(2.101)	(1.154)	(1.158)	(0.126)	(0.967)
Clusters	26	26	26	26	26
Obs	25624	25624	25624	14430	14524
1 Year Before					
Placebo Effect	−0.908	−0.971	0.063	0.039	0.381
	(2.152)	(1.488)	(0.685)	(0.042)	(0.583)
Subsidy Effect	−1.554	−1.021	−0.533	−0.015	−0.043
	(3.536)	(2.351)	(1.316)	(0.066)	(0.938)
Clusters	28	28	28	28	28
Obs	30580	30580	30580	17398	17510
Contemporaneous					
Placebo Effect	1.238	1.192	0.046	−0.023	1.333*
	(2.063)	(1.376)	(0.864)	(0.031)	(0.554)
Subsidy Effect	4.488	2.992	1.494	0.161*	−0.468
	(3.600)	(2.271)	(1.510)	(0.059)	(1.255)
Clusters	28	28	28	28	28
Obs	35072	35072	35072	19560	19710
1 Year After					
Placebo Effect	1.217	1.063*	0.154	0.003	−0.930
	(0.741)	(0.491)	(0.383)	(0.054)	(0.575)
Subsidy Effect	−0.321	−1.334	1.014	−0.047	1.988+
	(2.398)	(1.320)	(1.206)	(0.060)	(1.010)
Clusters	28	28	28	28	28
Obs	32140	32140	32140	18520	18628

All estimates are coefficients from independent instrumental variable regressions. Outcome variables include direct care worker (DCW), certified nursing assistant (CNA), and licensed nurse minutes per resident-day as well as direct care worker average hourly wages and direct care worker payroll per resident-day. All regressions regress one of those outcomes on the simulated effective subsidy amount that was or would have been offered to each nursing home, an interaction term between this variable and a dummy variable that is 1 in treatment states and 0 in control states, state-by-year-by-policy-event fixed effects, and nursing home fixed effects. The regressions are estimated within a one-year symmetric window centered at the time specified in the panel relative to when the actual policy change occurred. The sample here includes all subsidy states and their synthetic control states. The "Placebo Effect" coefficient reports the effect of the placebo subsidies within the control group, while the "Subsidy Effect" coefficient reports the interaction term that reflects the effect of subsidies in the treatment group over and above the placebo effect in the control group. Standard errors in parentheses are calculated analytically and clustered at the policy event by treatment vs. control group level. Significance levels implied by these standard errors are denoted by either * ($p < 0.05$) or +($p<0.1$) and placed on the coefficient. The cluster count is the number of treatment policy events and paired control groups in the sample, while the observation count is the number of nursing home-year observations in the sample.

III Chapter 3: Market Structure and the Efficacy of Nursing Home Payroll Subsidies

III.I Introduction

In the preceding chapter, I found that nursing home payroll subsidies were, on average, a very effective tool for increasing nursing home employment and wages. Chapter 2 further found evidence suggestive of non-profit nursing homes exhibiting greater responses to subsidies and evidence suggesting that subsidies are more effective when offered with marginal subsidy rates less than 100%. This chapter builds on Chapter 2's work investigating heterogeneity in the efficacy of payroll subsidies and examines another potentially important source of heterogeneity in the efficacy of payroll subsidies: local variation in the degree of competition faced by nursing homes. Contributing evidence on how market concentration affects nursing homes' response to payroll subsidies is of importance not only because of rising general interest in the effects of market power, but also because it adds to an existing literature on market power in the labor market for nurses (Staiger et al., 2010; Prager and Schmitt, 2019). Moreover, understanding how market concentration affects subsidy efficacy is of substantial practical importance for policy makers. While only about 30% of nursing homes are located in concentrated markets, most nursing home markets are concentrated. Furthermore, nursing homes are distributed across states so that in most states the average nursing home is in a concentrated market, meaning that taking the effect of market concentration into account should be of importance to most policymakers.

Market concentration has the potential to affect nursing home responses to payroll subsidies through both a product market power and a labor market power channel. The product market channel relates to how competition affects nursing homes' incentives to offer high quality care and are largely rooted in competition between nursing homes for residents.[60] This competition occurs on two margins: resident payer mix and occupancy. For the first, nursing homes compete to attract private pay (non-Medicaid) residents, who typically are charged a rate substantially higher than the rate nursing homes receive from Medicaid. Since Medicaid payment rates are set for nursing homes by state Medicaid programs, private pay residents are the only residents for whom nursing homes have discretion in setting prices. Nursing homes thus may compete for these residents both on care quality and on price.[61] Similarly, nursing homes may compete for private insurance and Medicare short stay residents, both of whom pay higher rates than Medicaid residents, though doing so may require some repositioning of services offered since, although similar in many respects, short stay and long stay residents do not have identical care needs. Competition for these resident types also is done in large part on the basis of care quality, since nursing homes do not have discretion in setting Medicare payment rates, though nursing homes should have discretion in negotiating with private insurers.

The second component of product market competition in this setting is that nursing homes compete to attract residents of all kinds in order to fill their beds and achieve high occupancy rates, allowing them to spread their fixed costs over a wider pool of residents.

[60] In some states, Medicaid also generates some financial incentives to provide care quality through adoption of pay-for-performance programs. Programs that condition a large share of payments on performance measures beyond just conditioning payments on homes meeting some minimal standards, however, are fairly rare and a rather recent development.

[61] Nursing home discretion in setting private pay prices is partially limited by Medicaid regulations as well. When dealing with customers who will eventually take up Medicaid, nursing homes have an incentive to bargain with them over some total asset transfer, allow them to shift all other assets to family members or charitable causes, and then bill the resident the agreed upon amount immediately on admission. This would allow the nursing home to immediately shift the resident on to Medicaid and thereby start receiving Medicaid payments as well. State Medicaid programs impose a number of rules in order to prevent this, including imposing maximum daily private pay rates that must be respected if a resident is to be eventually transitioned on to Medicaid without delay.

To the extent that this competition is largely over Medicaid residents, nursing homes cannot compete on price and so must compete on service quality or other amenities. In any case, whether competition is over private pay residents or residents more generally, competition between nursing homes should be based in large part on care quality, with market concentration reducing competition and weakening the incentive to provide high quality care.

If a lack of competition substantially softens nursing homes' incentive to provide high quality care, then nursing homes in noncompetitive product markets may be reluctant to take up subsidies when marginal subsidy rates are less than 100%.[62] Nursing homes in noncompetitive markets that take up subsidies also may be more adept at diverting subsidies away from labor. Cumulatively, this points toward the expected effect of product market concentration being to reduce the efficacy of payroll subsidies in general. This would also imply that the substantial effects of payroll subsidies on employment found in Chapter 2 are being driven by even larger subsidy effects among the approximately 70% of nursing homes that are located in competitive markets.

While these competitive effects are possible, it is worth noting that competitive incentives to provide high quality nursing home care may well be weak even in competitive markets. There are a variety of potential reasons for this, including information asymmetries regarding nursing home quality which may be difficult for consumers to resolve if a nursing home must be chosen on a short deadline. Additionally, nursing homes historically have faced, and sometimes still face, severe regulatory constraints on expanding their bed counts. These regulations, and in particular Certificate of Need (CON) laws, were particularly common in the 1980s and 1990s, with research by Gertler (1989) and Ching et al.

[62]Even when marginal subsidy rates statutorily are set at 100%, if a nursing home was planning to reduce its payroll relative to its base year payroll, it does not face an effective marginal subsidy rate of 100%.

(2015) suggesting that these regulations rationed large shares of Medicaid eligible persons out of nursing homes. When these circumstances generate excess demand for nursing home care, nursing home competition should be less fierce since nursing homes cannot expand and compete to attract the excess demand. If these forces reduce competitive pressure for nursing homes even in the presence of potential competitors, there may be little practical difference in the incentives faced by nursing homes in more and less concentrated markets, leading in turn to little practical difference in the effect of subsidies across markets. Indeed, evidence drawn from structural models of the nursing home industry suggest that product market competition has surprisingly little effect on nursing home care quality and staffing levels in general (Lin, 2015; Hackmann, 2018), suggesting that the product market competition channel may be weak in practice.

The second channel through which market concentration may affect nursing homes' response to payroll subsidies is the labor market power channel. Nursing homes with fewer product market competitors necessarily also face less competition for workers from other nursing homes. This effect presumably is negligible in the market for relatively unspecialized workers. Nursing assistants, for example, generally only require a high school degree and face relatively minimal occupational licensing requirements. Nursing homes often compete with generic retail employers for these workers and so likely face abundant competition for these workers in most markets. While nursing homes may still have some dynamic monopsony power over these workers due to a variety of frictions generating imperfect competition, the degree of monopsony power nursing homes have in the market for nursing assistants seems unlikely to vary dramatically with the number of other nursing homes in their vicinity. However, for more specialized workers like registered nurses and licensed practical nurses, the local pool of potential employers is much more limited and nursing home market concentration might non-trivially reduce their number of potential employers. Moreover, nursing home product market concentration is most likely to occur in relatively low population areas where the number of hospitals and other healthcare providers is likely to be lower, meaning that nursing home market concentration may be associated with greater monopsony power over specialized healthcare workers even if it is not the primary cause of it. Given that there is substantial evidence to suggest that hospitals in concentrated markets have monopsony power in the market for nurses and pharmacists (Staiger et al., 2010; Prager and Schmitt, 2019), it is reasonable to expect that nursing homes in more concentrated markets may have monopsony power over nurses as well.

Regardless of the source, nursing home monopsony power may affect nursing home responses to payroll subsidies in a number of ways. First, monopsonistic nursing homes seeking to hire in response to subsidies must raise wages and so face rising average costs of labor that dampen the degree to which subsidies can increase nursing home staffing. Unless increasing wages also elicits substantially more effort from nursing homes' existing employees, the net effect should be to dampen the degree to which subsidies enable the nursing home to improve care quality, potentially making subsidy uptake less desirable and so reducing payroll subsidy efficacy. Second, if nursing homes have varying amounts of monopsony power over different types of workers, monopsonistic nursing homes should have a greater incentive to spend subsidies hiring those workers over which they have least monopsony power, since hiring more such workers should not necessitate wage increases. Given that nursing homes likely have more monopsony power over nurses than nursing assistants, this suggests that labor market power should lead nursing homes to spend their subsidies disproportionately on nursing assistants relative to nursing homes homes in more competitive markets.

In order to shed empirical light on the above hypothesized effects of nursing home competition on payroll subsidy efficacy, in this chapter, I extend the within-state, across-nursing home research design from Chapter 2 to include an interaction term between subsidies and a binary indicator variable flagging when nursing homes are located in a concentrated product market. I estimate that nursing homes located in concentrated markets exhibit smaller increases in licensed nurse employment per resident-day and direct care worker payroll per resident-day in response to subsidies than do nursing homes located in competitive markets. The differential effect of subsidies on nursing assistant employment per resident-day in concentrated markets, meanwhile, is more ambiguous. These effects are consistent with both or either of the product market competition and labor market monopsony channels dampening nursing homes' incentives to respond to subsidies. These findings require caution in their interpretation, however, as they do not exhibit a robust pattern of statistical significance across all specifications and so should only be viewed as suggestive, rather than as statistically conclusive.

III.II Research Design

This chapter's empirical approach closely follows the within-state approach from Chapter 2, which regresses outcomes on subsidies conditional on state-by-year and nursing home by policy event fixed effects. In this chapter's first specification, an interaction term between subsidies and a dummy variable indicating whether or not a nursing home is in a concentrated market is added to the baseline Chapter 2 regression. In this chapter's second specification, a year-by-concentrated market status fixed effect is also added. These regressions are specified below:

$$(1) \quad Y_{i,t} = \beta_1 * subsidy_{i,t} + \beta_2 * subsidy_{i,t} * concentrated_i + \mu_{i,w} + \gamma_{t,s} + \epsilon_{i,t,w}$$

$$(2) \quad Y_{i,t} = \beta_1 * subsidy_{i,t} + \beta_2 * subsidy_{i,t} * concentrated_i + concentratedFE_{i,t}$$
$$+\mu_{i,w} + \gamma_{t,s} + \epsilon_{i,t,w}$$

In these equations, as also in Chapter 2, i indexes nursing homes, t indexes years, s indexes states, and w indexes the two year, symmetric policy event specific windows in which the regressions are estimated. The term $Y_{i,t}$ is a time-varying nursing home level outcome variable, $subsidy_{i,t}$ is the simulated maximum effective subsidy amount offered to nursing home i in year t measured in 2017 dollars per nursing home resident, and $concentrated_i$ is a binary indicator variable for whether a nursing home is in a concentrated product market. The coefficient β_1 is the effect on $Y_{i,t}$ of increasing the maximum effective subsidy amount per resident-day offered to a nursing home in an unconcentrated market by \$1, the coefficient β_2 is the differential effect of subsidies on $Y_{i,t}$ in concentrated markets relative to in unconcentrated markets, $concentratedFE_{i,t}$ is a year by concentrated market dummy fixed effect, $\mu_{i,w}$ is a nursing home-specific fixed effect that is allowed to vary across event windows in any state with more than one treatment event, $\gamma_{t,s}$ is a state-by-year fixed effect, and $\epsilon_{i,t,w}$ is an idiosyncratic nursing home level error term which exhibits policy event window level clustering. Note that for conducting statistical inference, I calculate standard errors using policy event clustered Liang-Zeger standard errors.

As in Chapter 2, I address the endogeneity of subsidies amounts offered to the subsidy policy itself by instrumenting for the simulated nursing home effective subsidy rates $subsidy_{i,t}$ using $subsidy_i^*$, the maximum effective subsidy amount offered to firm i calculated using the actual subsidy formula and its pre-treatment characteristics. I calculate

$subsidy_i^*$ and employ the instrument the same way as in Chapter 2, but with a few modifications. First, I add an interaction between $subsidy_i^*$ and $concentrated_i$ to the right hand side of the first stage regression for $subsidy_{i,t}$ and, in the case of specification (2), also add the additional fixed effects in the first stage regression. Second, I must conduct an additional first stage regression for the interaction term $subsidy_{i,t} * concentrated_i$, employing the same right hand side variables as in the $subsidy_{i,t}$ first stage regression.

Given the inclusion of the new interaction term, it is worth considering what the identifying assumptions in specifications (1) and (2) for the parameter of interest, β_2, actually are and how stringent they might be. In order for specification (1) to successfully identify the difference in subsidy efficacy between the concentrated and unconcentrated market samples, it must either be the case that nursing homes with characteristics qualifying them for different subsidy amounts be on parallel trends across all nursing homes or that any pre-trends associated with subsidy receipt among the concentrated market nursing homes be the same as those associated with subsidy receipt among the unconcentrated market nursing homes. It also must be the case that no national or state-specific shocks to outcomes of interest occur that disproportionately affect nursing homes in either concentrated or unconcentrated markets.[63]

The identifying assumptions for specification (2) are similar to those for specification (1), but slightly weaker. Specification (2)'s inclusion of year-by-market concentration status fixed effects allows for annual, nationwide shocks that disproportionately affect nursing homes in either concentrated or unconcentrated markets. However, any state-specific shocks to nursing homes in these different types of markets remain problematic. Both of these specifications contrast with a potential third approach that would include state-by-

[63]This would not be required if nursing homes in concentrated and unconcentrated markets received the same subsidy amounts on average, but Table III.3 shows that this is not the case.

year-by-concentrated market status fixed effects. This third approach would effectively be equivalent to estimating the effect of subsidies separately within each of the samples of concentrated and unconcentrated nursing homes and then comparing the estimates afterwards. This third specification would likely have superior identification in that it would allow for state-specific shocks to nursing homes in concentrated or unconcentrated markets. However, power considerations in this setting severely limit its practical application.[64] Given this constraint, specification (2) represents the research design with the least demanding identifying assumptions and so is my preferred specification.

As a final caution regarding identification, it is important to observe that the conditions outlined above are necessary for identifying the difference in the causal effect of subsidies received by nursing homes in concentrated and unconcentrated markets. These conditions are not sufficient, however, for guaranteeing that that difference in causal effects of subsidies is itself a causal effect of differences in market structure. That interpretation requires that whether or not a market is concentrated be exogenous to any other local market characteristics that may affect the efficacy of subsidies. To some extent, this orthogonality assumption is strengthened by the fact that Medicaid agencies in many states used Certificate of Need laws to systematically block nursing home entry into a range of markets for decades. This ensures that nursing home market structure within my sample, especially in the late 1990s and early 2000s, bears a closer resemblance to the market structure dictated by conditions 10-20 years prior to observation than would be the case in other industries. Despite this, the exogeneity assumption here remains quite strong, since many factors po-

[64]Results using this specification are generally uninformative. The standard errors obtained generally are very large, even relative to the baseline subsidy effects in Chapter 2. This is not surprising given that the state-by-year-by-concentration status fixed effects eliminate large quantities of useful variation from the concentrated market subsample and virtually all useful variation in this sample for some states, resulting in a severely underpowered research design that delivers very imprecise estimates.

tentially affecting both market structure and subsidy efficacy may well remain reasonably constant even over the span of decades. For example, nursing home product markets tend to be more competitive in densely populated urban areas, where the efficacy of subsidies also may vary due to differences in labor supply elasticities or consumer characteristics.[65] Overall, this market-level identifying assumption is very strong and suggests caution in the interpretation of my results.

III.III Data and Measurement

This chapter relies on the same sample of nursing home payroll subsidies and the same nursing home data detailed in the second chapter of this dissertation. For more on the construction of the sample, on how subsidy amounts offered to each nursing home are calculated, or on the source of the nursing home staffing and other data, please refer to Chapter 2. One major measurement issue that is not addressed in Chapter 2, however, is this chapter's approach for measuring nursing home market concentration, which is detailed below.

My preferred approach for measuring market concentration begins by defining a nursing home's relevant product market as the set of all nursing homes, regardless of any differences in specialization or type of residents served, within 15 miles of its location. I measure nursing home locations using geographic coordinates when available in the OSCAR/CASPR nursing home data, but when not available I assign nursing homes to their zip code's geographic centroid. I opted to use a relatively small geographic area for defining a nursing home's product market in order to account for the marked impor-

[65] Despite this possibility, subsidy efficacy does not vary much between the bottom quartile of nursing homes by county population relative to all other nursing homes. This suggests that any differential effects of subsidies in concentrated markets likely do not merely reflect differences in local population size.

tance of geographic differentiation in nursing home markets which results from proximity to home and loved ones being a very important amenity for most nursing home residents. My choice of a 15 mile radius in particular was influenced by calculations by Hackmann (2018). Using nursing home resident population data from the Minimum Data Set from 2000 to 2002, Hackmann found that the median nursing home resident traveled less than 5 miles from their home to reach their nursing home, while the 90th percentile resident traveled less than 15 miles. This suggests that nursing homes located within 15 miles of one another likely are competitive with each other for all or most potential residents located between them and a reasonable amount of residents located on their extremes. In order to facilitate robustness checks later in this chapter, I also calculate market definitions based off of a 30-mile radius around each nursing home as well as simply taking a nursing home's county as its relevant product market. I note, however, that a 30-mile radius seems overly expansive relative to the actual distribution of resident distances traveled, while using counties as a market definition might yield misleading results for nursing homes located near county borders.

Given these product market definitions, I then calculate nursing home market concentration using a Herfindahl-Hirschman Index (HHI) that is computed by summing the squares of 100 times the market shares of each nursing home in a given market. In my preferred definition of market concentration, I define each nursing home's market share as its total number of residents divided by the total number of nursing home residents in its market, but for robustness purposes also calculate HHIs based off of nursing homes' shares of more lucrative non-Medicaid residents. In my preferred approach to calculating HHIs, I also adjust for the presence of nursing homes owned by firms with multiple establishments in a given market by treating these nursing homes as one entity, summing their market shares prior to squaring. Once again, for robustness purposes, I also calculate HHIs where I treat all nursing home establishments as independent. On balance, however, it seems preferable to treat nursing homes owned by the same firm as a single unit since coordination with respect to staffing decisions, at least for scarcer types of workers like registered nurses, seems likely given nursing home administrators' perennial complaints about nursing shortages. Note that identification of which nursing homes belong to the same parent company was not an entirely trivial task and was based off of data on parent company names reported by nursing homes in select years. Please refer to the Multi-Establishment Firm Identification Appendix for more details on how this problem was solved.

With these Herfindahl-Hirschman Index measures of market concentration in hand, I constructed time invariant market concentration measures by calculating nursing home-specific over time HHI means within the treatment sample. I then constructed binary indicators for a nursing home being in a concentrated market by defining any market with an HHI exceeding 2500 as concentrated. I chose this particular threshold to correspond with the Department of Justice Antitrust Division's HHI threshold for a market being "moderately concentrated". This threshold also approximately corresponds with a rule of thumb for defining nursing home markets with five or fewer nursing homes as concentrated, a rule of thumb likely based on Bresnahan and Reiss (1991)'s influential work that found that market conduct does not tend to vary substantially with the number of market participants once the market includes five competitors. While the HHI and firm count based measures correspond closely, I prefer the HHI based measure since it more naturally handles cases where markets include some very small nursing homes by appropriately down-weighting these nursing homes' competitive contribution.

In order to illustrate how some of these different market concentration measures vary in practice, consider Table III.1. Table III.1 shows the share of nursing homes within the treatment sample, as well as within the universe of nursing homes observed from 1996-2015, that reside in concentrated markets per various definitions. All market concentrations are calculated with multi-establishment firm adjustments and using total resident count based market shares, but different concentration rates are presented using a 15-mile, 30-mile and county-based market definition. Within the treatment sample, about 30% of nursing homes are in concentrated markets according to either the 15-mile or county-based market definition, while only 11% are in concentrated markets per the 30-mile definition. The shares of states and counties for which the mean nursing home resides in a concentrated market are also calculated and shown using the 15-mile and county market definitions. While most nursing homes do not reside in concentrated markets, the average nursing home in 60 to 80% of states and counties is in a concentrated market, with the precise share depending on one's preferred market concentration definition.

In addition to Table III.1, Table III.2 summarizes the distribution of HHI market concentration measures experienced by nursing homes in the treatment sample and in the universe of nursing homes using my preferred 15-mile market definition and the county-based market definition variant. The distributions observed for these two approaches are quite similar. The HHI distributions for market concentration variants that drop the multi-establishment firm adjustments or that adopt a non-Medicaid resident based market shares are very similar to these. The only substantial differences in the distribution of market concentrations across definitions are obtained when comparing the 30-mile market definition to the 15-mile and county definitions, with market concentration measures being lower at every quartile of the market concentration distribution for the 30-mile market definition.

In order to better understand the differences between nursing homes in concentrated and unconcentrated markets, Table III.3 presents summary statistics for nursing homes in concentrated and unconcentrated markets (per my preferred 15-mile market concentration definition) on nursing homes' staffing ratios, average hourly direct care worker wages, shares of residents on Medicaid, resident counts, occupancy rates, and subsidy offers received. These summary statistics suggest that the largest difference between nursing homes in concentrated and unconcentrated markets is in terms of size: the mean resident count for nursing homes in concentrated markets was 68.6 compared to 96.6 in unconcentrated markets. The next largest differences were in terms of subsidies received and nurse staffing ratios, with nurse staffing ratios being 10 minutes per resident-day lower and largest subsidy offers ever received being 60 cents higher in concentrated markets than in unconcentrated markets. Beyond this, nursing assistant staffing levels, shares of residents on Medicaid, and occupancy rates are qualitatively similar across the two groups.[66] While there is a $4.50 difference in average direct care worker hourly wages across the two sets of nursing homes, it is difficult to say to what extent this reflects differences in wages and cost of living between concentrated and unconcentrated markets as opposed to differences in employee composition at nursing homes in concentrated and unconcentrated markets. Overall, these summary statistics point toward lower staffing ratios and smaller nursing homes in concentrated areas, as well as higher average degrees of subsidization.

[66] Strictly speaking, the difference in means across the concentrated and unconcentrated market samples for each variable shown in Table III.3 is statistically significant per a Welch's t-test, with the exception of the difference in mean nursing assistant staffing levels. However, it seems likely that many of the smaller differences observed are not economically significant.

III.IV Results

This chapter's main results may be found in Table III.4, which presents instrumental variable estimates of β_1, the effect of subsidies in unconcentrated markets ("Subsidy Baseline" in the table), and β_2, the differential effect of subsidies in concentrated markets ("Subsidy Interaction" in the table), using my preferred 15-mile market definition with multi-establishment firm adjustments and total resident count based market shares. Results are also estimated using a county-based market definition and are presented using both specifications (1) and (2) for each market definition. The outcome variables examined include direct care worker staffing in minutes per resident-day, nurse staffing, nursing assistant staffing, the percentage share of total staffing minutes provided by nurses, the average hourly wage paid by the nursing home to its direct care workers, and the nursing home's direct care worker payroll expenditure per resident-day. The percentage share of total staffing minutes provided by licensed nurses is added here as an outcome of interest, given that the prediction from the monopsony power channel that nursing homes in concentrated markets may exhibit greater responses for nursing assistants than for nurses.

The results from Table III.4 on the differential effect of subsidies in concentrated markets exhibit some features of interest, but do also not present a robust pattern of statistical significance. What follows will be an interpretation of what the estimated coefficients suggest if taken at face value, but it should be noted that since most coefficients are not statistically significant at the 5% or even 10% level, these findings cannot be taken as conclusive.

Focusing at first on the results from specification (1) using my preferred 15-mile market definition, available in the first column of Table III.4, the results point toward the differential effect of subsidies in concentrated markets on overall staffing being small and positive. This small positive effect is itself composed of a negative differential effect on nurse staffing and a positive differential effect on nursing assistant staffing. These staffing effects are sufficiently small to have little net impact on the share of staffing minutes provided by nurses. These staffing effects are coupled with a (potentially compositional) negative differential effect on average hourly wages and a large, negative differential effect on direct care worker payroll per resident-day, both of which are statistically significant at the 10% level.

Moving to the second column of Table III.4, when year-by-concentrated market fixed effects are added to these regressions, effect sizes grow and become markedly more negative, with the concentrated market differential effect of subsidies on direct care worker staffing per resident day becoming a (statistically insignificant) -1.7 minutes. This effect is driven in large part by a remarkably large -1.4 minute differential effect on nurse employment per resident-day, an effect that is statistically significant at the 5% level and large enough to nullify nearly two-thirds of the positive effect of subsidies on nurse staffing observed in unconcentrated markets. Meanwhile, the nursing assistant differential effect in this specification has become small and negative, the net differential effect on the share of staffing minutes provided by nurses has grown markedly negative, the (once again, potentially compositional) differential effect on average hourly wages has grown to -14 cents and has achieved statistical significance at the 5% level, and finally the differential effect on payroll per resident has become a statistically significant at the 10 percent level -1.11 dollars relative to an unconcentrated market effect of 1.60 dollars.

These two pools of 15-mile market definition differential subsidy effects are, to some extent, consistent with the expected effects of market power on subsidy efficacy. The negative differential effects of subsidies on payroll per resident-day are suggestive of lower subsidy take up or larger rates of subsidy diversion away from labor in more concentrated markets, as might be expected if nursing homes face weaker incentives to compete on care quality in those areas. The weaker effects on nurse staffing that occur in specification (2) without an offsetting increase in nursing assistant staffing are also consistent with nursing homes in concentrated markets having weaker incentives to spend subsidies on care quality, seeing as nurses are an impactful, high skill input into care quality. The fact that the differential effect on nurse employment per resident-day is markedly more negative than is observed for nursing assistants is also consistent with expectations that nursing homes' greater monopsony power over nurses will cause their reduced response to be driven largely by reduced hiring of nurses rather than of nursing assistants. Also of note is the difference between the results for specifications (1) and (2). The specification including year-by-concentrated market fixed effects finds markedly more negative effects across the board and suggests the presence of either some sort of nationwide positive staffing shocks occurring in more concentrated markets following subsidy adoption or the presence of some kind of competitive spillover in these areas that raises staffing for all affected nursing homes. This difference raises some doubts about the quality of my research design for estimating these interaction terms in specification (1), and these doubts will receive further scrutiny when validation tests for the primary regressions are conducted in the next section.

Table III.4 also presents, in the table's third and fourth columns, differential subsidy effect results from models employing a market concentration measure that uses the county-based market definition. When using this market definition, the differential subsidy effects from the specification that includes no additional fixed effects are all negatively signed and relatively small in magnitude. Switching to specification (2), which adds year-by-concentrated market fixed effects, larger and more negative staffing differential effects are uncovered, though the differential effects on average hourly wages and payroll per resident-day remain small. One interesting deviation in specification (2) from the comparable 15-mile market results is that the differential effect of subsidies in concentrated markets nurse and nursing assistant are approximately the same in absolute terms. Note that while no absolute wedge exists between the two, the implied proportional effect on nurse staffing is much larger since nursing homes have lower baseline nurse staffing levels. Overall, the results for the county-based market definition models are not markedly different in terms of interpretation than those from the 15-mile market definition case and once again stress the apparent importance of the year-by-concentrated market fixed effects.

III.V Validation and Robustness Tests

III.V.I Robustness

One concern with this chapter's primary results is that they might not be robust to changes in market concentration measurement that should otherwise be innocuous. In order to demonstrate robustness to these measurement decisions, this section presents tables comparing estimates of the differential effect of subsidies in concentrated markets per the preferred 15-mile market concentration definition and the county-based market concentration definition to estimates obtaining using using alternative market concentration definitions.

The most dramatic difference in results relative to the primary findings are obtained when switching to a 30-mile radius market definition. Table III.5 compares results estimated using the preferred 15-mile market definition to those obtained using the 30-mile definition. In specifications including only nursing home and state-by-year fixed effects, the differential effect of subsidies in concentrated markets on various staffing measures is found to be about the same for both market definitions, though due to differences in baseline staffing levels by worker type this nets out to a more negative differential effect of subsides on the share of staffing minutes provided by nurses in the 30-mile case. The largest difference across the two definitions for specification (1), the no additional fixed effects case, is in the differential effect of subsidies on average hourly wages and direct care worker payroll per resident day. Each of these parameters is larger and more negative in the 30-mile case than in the 15-mile market case, achieving magnitudes larger than the baseline positive subsidy effects. Taken literally, these findings suggest that subsidy receipt reduces average wages and payroll expenditure in markets that are concentrated per the 30-mile definition.

These estimates are difficult to reconcile with the more modest staffing effects and raise questions about whether the approximately 10% of nursing homes in concentrated 30-mile markets exhibit some sort of severe wage and payroll parallel trends assumption violations. Note that the effects estimated using the 30-mile market definition when including year-by-concentrated market fixed effects do not exhibit these remarkable wage and payroll differential effects, and in fact show markedly more positive point estimates across all coefficients than appear in the corresponding 15-mile regressions. Given that the results from the 30-mile market definition vary so severely depending on the set of fixed effects included and deliver implausible estimates in some specifications the 30-mile market definition approach is unreliable. This may be because its results are overly sensitive to dynamics involving very small numbers of nursing homes owing to how few nursing homes actually qualify as residing in a concentrated market according to this definition.[67]

Results from estimates using the other market concentration definitions are in greater concordance with the primary results. Table III.6 compares results estimated using the multi-establishment firm adjusted 15-mile market definition to those estimated similarly, but treating all establishments as independently owned. Table III.7 makes the same comparisons, but using the county market definition. For each market definition and specification, there is a strong degree of correspondence across the multi-establishment firm adjusted and independent establishment results. The only substantial differences are between the staffing results for the two approaches using the 15-mile market definition, where the overall differential effect on staffing in concentrated markets is more positive using the independent establishment definition due to more positive nursing assistant staffing differential effects and less negative nurse staffing differential effects. Note, however, that these differences in the staffing differential subsidy effects do not appear to translate into marked differences in average wage and payroll differential effects. Table III.7 also exhibits minimal differences in the differential effects estimated using the county multi-establishment firm adjusted and independent establishment market concentration definitions. Cumulatively, I read these results to suggest that the primary findings in Table III.4 are reasonably robust to whether or not multi-establishment firm adjustments are used.

Tables III.8 and III.9 present results equivalent to those in Table III.4, but comparing cases where the 15-mile and county market definitions are calculated using total resident count market shares versus using total non-Medicaid resident count market shares. If

[67]Note that this problem is exacerbated when studying wage and payroll outcomes, given wage and payroll data are not available for all nursing homes.

nursing home competition over non-Medicaid residents is fierce but nursing homes can reliably fill any excess capacity by taking in Medicaid residents as needed, these two market definitions could yield substantially different findings. This, however, does not prove to be the case. There are no substantial differences in the differential effects of subsidies in concentrated markets across the various specifications in the 15-mile market definition case. The same is largely true when the exercise is repeated using the county market definition case, with the exception of differences occurring when looking specifically at the payroll per resident results. The direct care worker payroll per resident differential subsidy effects in concentrated markets are markedly more negative in the non-Medicaid resident HHI case than in the total resident HHI case for the county market definition, yielding results closer to what are otherwise found in the 15-mile market HHI case for these same regressions.

The broad pattern found within these robustness tests is that the results obtained using my preferred 15-mile market definition and my preferred approach to measuring market concentration are generally not affected much by small adjustments to measurement. The measurement choice with the largest impact on results is the decision not to use the 30-mile market definition. This alternative market definition shrinks the pool of nursing homes labeled as residing in concentrated markets by two-thirds and seems to deliver estimates that are relatively unstable and implausible in some respects, suggesting that not using the 30-mile definition may be for the best.

III.V.II Validation

One of the key identifying assumptions for the differential effect of subsidies in more concentrated markets is that any trend differences between nursing homes qualifying for different subsidy amounts in concentrated markets must be the same as the trend differences observed between nursing homes qualifying for different subsidy amounts in unconcentrated markets. In order to better test the validity of this assumption, I regressed the change in all outcomes of interest used in Table III.4 from two years prior to each policy change to one year prior to each policy change on the change in the instrumented subsidy offers received by each nursing home and an interaction between that change and the concentrated market indicator. The extent to which the coefficients on the pre-trend interaction terms substantially vary from zero will be informative about the degree to which the parallel trends assumption described above appears to be violated in the years prior to each policy event.

Table III.10 presents these pre-trend robustness test results for all of the specifications tested in Table III.4. Focusing at first on the 15-mile market definition, the differential pre-trend associations with subsidies in concentrated markets are, though not statistically significant in general, large and positive for all types of direct care worker staffing. The concentrated market payroll differential effect is also large and positive relative to the baseline subsidy effects in the specification including year-by-concentrated market fixed effects. While these pre-trend tests do not exhibit a robust pattern of statistical significance indicating systematic assumption test failures in the 15-mile market definition case, the large magnitude of the results raises some concerns. Given that the primary effects in Table III.4 also do not exhibit a robust pattern of statistical significance, large magnitude pre-trend violations cast further doubt on their reliability. While the implied pre-trends are positive and therefore work against the finding that subsidies have weaker effects in concentrated areas, the pre-trend violations implied are large enough that they would imply highly implausible effect sizes if literally subtracted from the effects in Table III.4. Table III.10 also presents comparable pre-trend estimates for the county market definition, yielding similar results with the exception of smaller and more negative payroll pre-trend differential effects, prompting broadly similar concerns as to in the 15-mile market case. Cumulatively, the presence of associations between subsidies and pre-trends in various outcomes of interest that differ for nursing homes in concentrated and competitive markets provide some additional reason to doubt this chapter's primary results, though the pre-trend violations are insufficiently severe and insufficiently precisely estimated to conclude that the primary results are entirely uninformative.

In addition to the validation tests above for the primary results, I also repeat the pre-trend test exercise using different market definitions and different market concentration measures. Table III.11 presents pre-trend test results comparing the 15-mile and 30-mile market definitions. As suspected from the robustness test results, the 30-mile market definition results exhibit larger pre-trend violations than occur when using the 15-mile definition, further contributing to the sense of unreliability surrounding the 30-mile market definition results. Table III.12 presents pre-trend associations from the regressions using the 15-mile market multi-establishment firm adjusted and unadjusted market concentration measures. There is little in terms of systematic pre-trend effect differences between the two definitions. Much the same is true for the same comparisons made using the county-based market definition, as may be observed in Table III.13. Comparison of pre-trend associations with subsidies across the total resident count based and non-Medicaid resident count based market concentration measures are presented in Table III.14 for the 15-mile market definition and in Table III.15 for the county-based market definition. The results also do not exhibit substantially different pre-trend associations with the differential effect of subsidies in concentrated markets across the different market concentration measures. Cumulatively, this pool of validation test results suggest that differences across market structures in terms of how subsidies are associated with pre-trends in outcomes are largely not affected by how market concentration is measured, with the exception of there being more severe pre-trend violations when the 30-mile market definition is used.

III.VI Concluding Discussion

This chapter's primary results are suggestive of nursing homes in concentrated markets responding to marginal payroll subsidies with smaller increases in direct care worker payroll per resident-day and smaller increases in nurse staffing per resident-day than occur at subsidized nursing homes in more competitive markets. These results are consistent with a lack of product market competition dampening incentives to provide high quality care and thereby tamping down nursing home incentives to take up subsidies and spend them on hiring more workers per resident-day. The results are also consistent with nursing home monopsony power dampening incentives to spend subsidies on hiring workers over which nursing homes have greater monopsony power. Finally, the results are generally robust to varying the precise method used for measuring market concentration and for defining a nursing home's product market.

The potential conclusions to be drawn from this chapter's findings must be interpreted with caution. While the signs of estimated effects are consistent with the market power explanations given above and while some findings are statistically significant, I did not find a broad and robust pattern of statistical significance across specifications and different outcome measures. As such, the primary findings should be viewed as statistically inconclusive on this subject. Moreover, the validation tests indicate that subsidy receipt potentially may be associated with pre-trends in employment and payroll expenditure within concentrated markets in a fashion different from the association observed in unconcentrated markets. While these validation tests also do not yield a broad and robust pattern of statistically significant pre-trend assumption violations, some of the tests do fail in a statistically significant fashion and, even when not statistically significant, the coefficients on the pre-trend tests are large in magnitude. While this in and of itself does not imply that my research design is insufficient for addressing whether subsidy efficacy varies by market concentration, in the context of the estimated differential effects of subsidies in concentrated markets themselves being imprecisely estimated and not entirely reliable, this research design uncertainty is a cause for further caution. As a final note of caution, to the extent that my estimates indicate some difference in subsidy efficacy by market concentration, it should be noted that I cannot causally attribute that difference to market concentration itself, as I cannot rule out that some third market characteristic causes both variation in market concentration and in subsidy efficacy.

The results from this chapter point to a need for further research in settings better tailored to identifying heterogeneity in nursing home responses to subsidies by market concentration. The results suggest, but do not prove, that there may be non-trivial reductions in subsidy efficacy for nursing homes in concentrated markets. If confirmed, this heterogeneity would be of substantial policy significance given that nearly a third of nursing homes are in concentrated markets. Moreover, nursing home policy is in large part set by state Medicaid programs, and the distribution of nursing homes across states is such that in most states, the average nursing home is in a concentrated market. More precisely estimated differences in subsidy efficacy by market concentration would also be independently interesting and potentially very informative about which types of competition, if any, are very influential upon nursing home behavior.

III.VII Tables

Table III.1: Share of Nursing Homes, Counties, and States in Concentrated Markets by Market Definition

	Treatment Sample	Universe
	Mean	Mean
Concentrated 15-Mile Market	0.29	0.30
Concentrated 30-Mile Market	0.11	0.06
Concentrated County Market	0.30	0.33
Concentrated 15-Mile Market (County Averages)	0.74	0.67
Concentrated 15-Mile Market (State Averages)	0.70	0.58
Concentrated County Market (County Averages)	0.79	0.76
Concentrated County Market (State Averages)	0.64	0.56
Observations	12259	281795

This table lists the share of nursing homes in the payroll subsidy treatment sample and overall 1996-2015 universe of nursing homes which reside in a concentrated market for various market definitions. In each case, a market is taken to be concentrated if the multi-establishment firm adjusted Herfindahl-Hirschman Index for the market, calculating market shares in terms of nursing homes' shares of market-wide resident counts, exceeds 2500. The different market definitions explored include defining markets as the set of all nursing homes within a 15 or 30 mile radius around a given nursing home as well as the set of all nursing homes within a given nursing home's county. This table also presents the share of counties and states for whom the county or state's mean nursing home is in a concentrated market based on the 15-mile or county market definition.

Table III.2: Distribution of Nursing Home, County Average, and State Average Market Concentrations by Market Definition

	Treatment Sample				Universe			
	Mean	25th Pct	Median	75th Pct	Mean	25th Pct	Median	75th Pct
15-Mile Market HHI	2292.62	402.28	980.89	3034.39	2157.31	389.34	1218.92	2805.18
15-Mile Market HHI (County Averages)	5460.77	2372.03	5005.43	10000.00	4541.92	2050.83	3561.50	6070.87
15-Mile Market HHI (State Averages)	3680.26	2189.27	3375.20	6130.17	2864.17	1523.92	2710.88	3550.17
County Market HHI	2261.87	425.34	993.35	3314.82	2302.81	488.01	1387.91	3267.22
County Market HHI (County Averages)	5931.44	3016.93	5168.68	10000.00	5348.96	2572.19	5004.59	10000.00
County Market HHI (State Averages)	3413.79	1869.87	3211.39	5662.62	2913.62	1594.29	2671.92	4099.11
Observations	12259				281795			

This table gives the mean, 25th percentile, median, and 75th percentile market concentration for nursing homes in the treatment sample and in the overall 1996-2015 universe of nursing homes based on 15-mile radius and county-based market definitions. All market concentrations are presented in terms of multi-establishment firm adjusted Herfindahl-Hirschman Indices calculated using nursing homes' shares of market wide resident counts as the market share concept. This table also presents the same concentration moments but for the distribution of county-level and state-level market concentration averages.

Table III.3: Nursing Home Summary Statistics by 15-mile Market Concentration Status

	Unconcentrated (15-Mile)			Concentrated (15-Mile)		
	Mean	Median	SD	Mean	Median	SD
DCW Staffing Minutes	199.78	188.72	61.20	189.84	176.82	64.54
Nurse Staffing Minutes	72.21	64.03	39.03	62.77	54.40	38.90
CNA Staffing Minutes	127.57	123.36	36.10	127.07	120.66	41.96
DCW Average Hourly Wage	23.22	23.64	4.78	18.70	18.62	4.04
Share of Residents on Medicaid	62.12	66.67	24.04	63.26	64.76	20.60
Resident Count	96.60	97.00	47.65	68.62	58.00	41.81
Percent Occupancy	87.25	91.43	12.52	84.59	88.59	14.08
Largest Subsidy Offer Received	2.90	2.79	1.85	3.50	3.01	2.31
Observations	8651			3608		

This table lists means, medians, and standard deviations for key variables within two samples. The sample on the left is the set of all nursing home observations from the treatment sample in an unconcentrated market, per the 15-mile radius market definition with multi-establishment firm adjustments and using resident count based market shares. The sample on the right is the set of all treatment sample nursing homes from concentrated markets, per the same market definition. The key variables examined included staffing variables denominated in minutes per resident-day of either direct care worker (DCW), certified nursing assistant (CNA), or licensed nurse employment. They also include direct care worker average hourly wages denominated in 2017 dollars per hour, the percentage share of nursing home residents on Medicaid, the total number of residents in a nursing home, nursing home occupancy rates, and the largest subsidy offer in dollars per resident-day ever received by a given nursing home.

Table III.4: Effect of Subsidies on Staffing, Wages, and Payroll by Market Concentration per the 15-mile and County Market Definitions

	15 Mile HHI		County HHI	
	State X Year FE	SY FE + Year X Concentrated FE	State X Year FE	SY FE + Year X Concentrated FE
DCW Min				
Subsidy Baseline	4.800+	5.105*	5.008*	5.237*
	(2.349)	(2.319)	(2.187)	(2.162)
Subsidy*Concentrated	0.270	−1.665	−0.317	−1.687
	(1.123)	(1.411)	(0.658)	(1.235)
Nurse Min				
Subsidy Baseline	1.899+	2.148*	1.862+	1.945+
	(1.035)	(0.990)	(1.006)	(0.992)
Subsidy*Concentrated	−0.291	−1.394*	−0.283	−0.865+
	(0.361)	(0.481)	(0.316)	(0.456)
CNA Min				
Subsidy Baseline	2.899+	2.955*	3.144*	3.292*
	(1.350)	(1.356)	(1.234)	(1.242)
Subsidy*Concentrated	0.561	−0.270	−0.035	−0.822
	(0.933)	(1.065)	(0.581)	(1.039)
Pct Nurse				
Subsidy Baseline	0.032	0.094	0.026	0.038
	(0.114)	(0.076)	(0.108)	(0.096)
Subsidy*Concentrated	−0.044	−0.247+	−0.040	−0.129
	(0.132)	(0.131)	(0.151)	(0.162)
Avg Wage				
Subsidy Baseline	0.168*	0.184*	0.146*	0.152*
	(0.049)	(0.051)	(0.052)	(0.052)
Subsidy*Concentrated	−0.080+	−0.139*	−0.045	−0.066
	(0.039)	(0.050)	(0.032)	(0.049)
DCW PPR				
Subsidy Baseline	1.450	1.600	1.156	1.164
	(1.110)	(1.067)	(1.172)	(1.169)
Subsidy*Concentrated	−0.664+	−1.114+	−0.130	−0.210
	(0.349)	(0.625)	(0.193)	(0.373)

This table gives estimates of the instrumented effect of subsidies by market concentration on a number of outcomes. The "Subsidy Baseline" coefficients refer to the effect of receiving a one dollar per resident-day subsidy offer on nursing homes in unconcentrated markets, while the "Subsidy Interaction" coefficient gives the differential effect of subsidy receipt on nursing homes in concentrated markets. Note that in each specification, one first stage regression is estimated for each of the subsidy and the subsidy interaction term, with each first stage regression including both the main subsidy instrument and the main subsidy instrument interacted with the concentrated market indicator variable. The two super columns vary the market concentration definition being used. Each takes a market as concentrated if its Herfindahl-Hirschman Index exceeds 2500, calculating the HHI using shares of market-wide resident counts as market shares and assigning residents for nursing homes with the same owner to a single entity. The super columns vary, however, in terms of market definition, using a 15-mile radius around each nursing home as the market definition in one and each nursing home's county as the market definition in the other. Within each super column, the left column reports results from the instrumental variables regression including only state-by-year and nursing home-by-policy event fixed effects, while the column on the right adds a market concentration status-by-year fixed effect. Finally, note that outcome variables used include direct care worker (DCW), certified nursing assistant (CNA), and licensed nurse staffing, each denominated in minutes of labor per resident-day, as well the percentage share of staffing minutes provided by registered nurses, the average hourly wage paid to direct care workers, and average direct care worker payroll expenditure per resident-day.

Table III.5: Effect of Subsidies on Staffing, Wages, and Payroll by Market Concentration per the 15-mile and 30-mile Market Definitions

	15 Mile HHI		30 Mile HHI	
	State X Year FE	SY FE + Year X Concentrated FE	State X Year FE	SY FE + Year X Concentrated FE
DCW Min				
Subsidy Baseline	4.800+	5.105*	4.806+	4.753+
	(2.349)	(2.319)	(2.292)	(2.369)
Subsidy*Concentrated	0.270	−1.665	0.727	1.060
	(1.123)	(1.411)	(0.757)	(1.664)
Nurse Min				
Subsidy Baseline	1.899+	2.148*	1.813	1.807
	(1.035)	(0.990)	(1.062)	(1.088)
Subsidy*Concentrated	−0.291	−1.394*	−0.237	−0.174
	(0.361)	(0.481)	(0.429)	(0.737)
CNA Min				
Subsidy Baseline	2.899+	2.955*	2.992*	2.945*
	(1.350)	(1.356)	(1.267)	(1.314)
Subsidy*Concentrated	0.561	−0.270	0.964	1.234
	(0.933)	(1.065)	(0.547)	(1.094)
Pct Nurse				
Subsidy Baseline	0.032	0.094	0.034	0.044
	(0.114)	(0.076)	(0.104)	(0.098)
Subsidy*Concentrated	−0.044	−0.247+	−0.137	−0.268
	(0.132)	(0.131)	(0.173)	(0.215)
Avg Wage				
Subsidy Baseline	0.168*	0.184*	0.151*	0.146*
	(0.049)	(0.051)	(0.052)	(0.053)
Subsidy*Concentrated	−0.080+	−0.139*	−0.176*	−0.095
	(0.039)	(0.050)	(0.051)	(0.065)
DCW PPR				
Subsidy Baseline	1.450	1.600	1.377	1.289
	(1.110)	(1.067)	(1.178)	(1.167)
Subsidy*Concentrated	−0.664+	−1.114+	−1.803*	−0.381
	(0.349)	(0.625)	(0.654)	(0.763)

This table gives estimates of the instrumented effect of subsidies by market concentration on a number of outcomes. The “Subsidy Baseline” coefficients refer to the effect of receiving a one dollar per resident-day subsidy offer on nursing homes in unconcentrated markets, while the “Subsidy Interaction” coefficient gives the differential effect of subsidy receipt on nursing homes in concentrated markets. The two super columns vary the market concentration definition being used. Each takes a market as concentrated if its Herfindahl-Hirschman Index exceeds 2500, calculating the HHI using shares of market-wide resident counts as market shares and assigning residents for nursing homes with the same owner to a single entity. The super columns vary, however, in terms of market definition, using a 15-mile radius around each nursing home as the market definition in one and using a 30-mile radius in the other. Within each super column, the left column reports results from the instrumental variables regression including only state-by-year and nursing home-by-policy event fixed effects, while the column on the right adds a market concentration status-by-year fixed effect. For further details on how the instrumental variable regressions are estimated and on the outcome variables, please see the notes to Table III.4.

Table III.6: Effect of Subsidies on Staffing, Wages, and Payroll by Market Concentration per the 15-mile Multi-Establishment Firm Adjusted and Unadjusted Market Definitions

	15 Mile Firm HHI		15 Mile Estab HHI	
	State X Year FE	SY FE + Year X Concentrated FE	State X Year FE	SY FE + Year X Concentrated FE
DCW Min				
Subsidy Baseline	4.800+	5.105*	4.522+	4.597+
	(2.349)	(2.319)	(2.313)	(2.290)
Subsidy*Concentrated	0.270	−1.665	1.027+	0.048
	(1.123)	(1.411)	(0.483)	(0.884)
Nurse Min				
Subsidy Baseline	1.899+	2.148*	1.754	1.946+
	(1.035)	(0.990)	(1.045)	(0.992)
Subsidy*Concentrated	−0.291	−1.394*	0.063	−0.716
	(0.361)	(0.481)	(0.258)	(0.488)
CNA Min				
Subsidy Baseline	2.899+	2.955*	2.767+	2.650+
	(1.350)	(1.356)	(1.308)	(1.323)
Subsidy*Concentrated	0.561	−0.270	0.964	0.765
	(0.933)	(1.065)	(0.572)	(0.620)
Pct Nurse				
Subsidy Baseline	0.032	0.094	0.022	0.087
	(0.114)	(0.076)	(0.110)	(0.077)
Subsidy*Concentrated	−0.044	−0.247+	−0.021	−0.239
	(0.132)	(0.131)	(0.127)	(0.143)
Avg Wage				
Subsidy Baseline	0.168*	0.184*	0.158*	0.171*
	(0.049)	(0.051)	(0.048)	(0.050)
Subsidy*Concentrated	−0.080+	−0.139*	−0.059+	−0.113+
	(0.039)	(0.050)	(0.032)	(0.056)
DCW PPR				
Subsidy Baseline	1.450	1.600	1.493	1.755
	(1.110)	(1.067)	(1.153)	(1.086)
Subsidy*Concentrated	−0.664+	−1.114+	−0.751	−1.042
	(0.349)	(0.625)	(0.477)	(0.636)

This table gives estimates of the instrumented effect of subsidies by market concentration on a number of outcomes. The "Subsidy Baseline" coefficients refer to the effect of receiving a one dollar per resident-day subsidy offer on nursing homes in unconcentrated markets, while the "Subsidy Interaction" coefficient gives the differential effect of subsidy receipt on nursing homes in concentrated markets. The two super columns vary the market concentration definition being used. Each takes a market, defined as the set of all nursing homes within a 15-mile radius around a given nursing home, as concentrated if its Herfindahl-Hirschman Index exceeds 2500, calculating the HHI using shares of market-wide resident counts as market shares. The super columns vary, however, in that in the Firm-based HHI case, market shares for nursing homes owned by the same firm are summed prior to squaring, while in the Establishment-based HHI case all nursing home establishments are assumed to operate independently. Within each super column, the left column reports results from the instrumental variables regression including only state-by-year and nursing home-by-policy event fixed effects, while the column on the right adds a market concentration status-by-year fixed effect. For further details on how the instrumental variable regressions are estimated and on the outcome variables, please see the notes to Table III.4.

Table III.7: Effect of Subsidies on Staffing, Wages, and Payroll by Market Concentration per the County Multi-Establishment Firm Adjusted and Unadjusted Market Definitions

	County Firm HHI		County Estab HHI	
	State X Year FE	SY FE + Year X Concentrated FE	State X Year FE	SY FE + Year X Concentrated FE
DCW Min				
Subsidy Baseline	5.008*	5.237*	5.057*	5.278*
	(2.187)	(2.162)	(2.199)	(2.178)
Subsidy*Concentrated	−0.317	−1.687	−0.488	−1.763
	(0.658)	(1.235)	(0.617)	(1.261)
Nurse Min				
Subsidy Baseline	1.862+	1.945+	1.863+	1.966+
	(1.006)	(0.992)	(1.007)	(0.999)
Subsidy*Concentrated	−0.283	−0.865+	−0.289	−1.013*
	(0.316)	(0.456)	(0.309)	(0.429)
CNA Min				
Subsidy Baseline	3.144*	3.292*	3.193*	3.310*
	(1.234)	(1.242)	(1.244)	(1.250)
Subsidy*Concentrated	−0.035	−0.822	−0.199	−0.750
	(0.581)	(1.039)	(0.529)	(1.055)
Pct Nurse				
Subsidy Baseline	0.026	0.038	0.015	0.037
	(0.108)	(0.096)	(0.107)	(0.096)
Subsidy*Concentrated	−0.040	−0.129	−0.004	−0.176
	(0.151)	(0.162)	(0.146)	(0.140)
Avg Wage				
Subsidy Baseline	0.146*	0.152*	0.151*	0.155*
	(0.052)	(0.052)	(0.053)	(0.054)
Subsidy*Concentrated	−0.045	−0.066	−0.063+	−0.059
	(0.032)	(0.049)	(0.032)	(0.052)
DCW PPR				
Subsidy Baseline	1.156	1.164	1.218	1.207
	(1.172)	(1.169)	(1.208)	(1.202)
Subsidy*Concentrated	−0.130	−0.210	−0.328	−0.127
	(0.193)	(0.373)	(0.206)	(0.417)

This table gives estimates of the instrumented effect of subsidies by market concentration on a number of outcomes. The "Subsidy Baseline" coefficients refer to the effect of receiving a one dollar per resident-day subsidy offer on nursing homes in unconcentrated markets, while the "Subsidy Interaction" coefficient gives the differential effect of subsidy receipt on nursing homes in concentrated markets. The two super columns vary the market concentration definition being used. Each takes a market, defined as the set of all nursing homes within a county, as concentrated if its Herfindahl-Hirschman Index exceeds 2500, calculating the HHI using shares of market-wide resident counts as market shares. The super columns vary, however, in that in the Firm-based HHI case, market shares for nursing homes owned by the same firm are summed prior to squaring, while in the Establishment-based HHI case all nursing home establishments are assumed to operate independently. Within each super column, the left column reports results from the instrumental variables regression including only state-by-year and nursing home-by-policy event fixed effects, while the column on the right adds a market concentration status-by-year fixed effect. For further details on how the instrumental variable regressions are estimated and on the outcome variables, please see the notes to Table III.4.

Table III.8: Effect of Subsidies on Staffing, Wages, and Payroll by Market Concentration per the 15-mile Market Definition with Resident Count HHIs vs Non-Medicaid Resident Count HHIs

	15 Mile Resident HHI		15 Mile Non-Medicaid Resident HHI	
	State X Year FE	SY FE + Year X Concentrated FE	State X Year FE	SY FE + Year X Concentrated FE
DCW Min				
Subsidy Baseline	4.800+	5.105*	4.884+	4.989+
	(2.349)	(2.319)	(2.362)	(2.360)
Subsidy*Concentrated	0.270	−1.665	0.068	−1.164
	(1.123)	(1.411)	(1.128)	(1.552)
Nurse Min				
Subsidy Baseline	1.899+	2.148*	1.923+	2.084+
	(1.035)	(0.990)	(1.045)	(1.005)
Subsidy*Concentrated	−0.291	−1.394*	−0.340	−1.173*
	(0.361)	(0.481)	(0.382)	(0.519)
CNA Min				
Subsidy Baseline	2.899+	2.955*	2.959*	2.904+
	(1.350)	(1.356)	(1.354)	(1.383)
Subsidy*Concentrated	0.561	−0.270	0.408	0.008
	(0.933)	(1.065)	(0.904)	(1.141)
Pct Nurse				
Subsidy Baseline	0.032	0.094	0.023	0.081
	(0.114)	(0.076)	(0.120)	(0.080)
Subsidy*Concentrated	−0.044	−0.247+	−0.021	−0.221
	(0.132)	(0.131)	(0.134)	(0.126)
Avg Wage				
Subsidy Baseline	0.168*	0.184*	0.171*	0.183*
	(0.049)	(0.051)	(0.051)	(0.050)
Subsidy*Concentrated	−0.080+	−0.139*	−0.085+	−0.128*
	(0.039)	(0.050)	(0.043)	(0.050)
DCW PPR				
Subsidy Baseline	1.450	1.600	1.466	1.670
	(1.110)	(1.067)	(1.098)	(1.026)
Subsidy*Concentrated	−0.664+	−1.114+	−0.683+	−1.291*
	(0.349)	(0.625)	(0.331)	(0.561)

This table gives estimates of the instrumented effect of subsidies by market concentration on a number of outcomes. The "Subsidy Baseline" coefficients refer to the effect of receiving a one dollar per resident-day subsidy offer on nursing homes in unconcentrated markets, while the "Subsidy Interaction" coefficient gives the differential effect of subsidy receipt on nursing homes in concentrated markets. The two super columns vary the market concentration definition being used. Each takes a market, defined as the set of all nursing homes within a 15-mile radius around a given nursing home, as concentrated if its Herfindahl-Hirschman Index exceeds 2500, adjusting the HHI for multi-establishment firms by summing the market shares of nursing homes owned by the same firm prior to squaring the market shares. The super columns vary, however, in that in one case, market shares are calculated in terms of shares of market-wide nursing home resident counts, while in the other, they are calculated in terms of shares of market-wide nursing home non-Medicaid resident counts. Within each super column, the left column reports results from the instrumental variables regression including only state-by-year and nursing home-by-policy event fixed effects, while the column on the right adds a market concentration status-by-year fixed effect. For further details on how the instrumental variable regressions are estimated and on the outcome variables, please see the notes to Table III.4.

Table III.9: Effect of Subsidies on Staffing, Wages, and Payroll by Market Concentration per the County Market Definition with Resident Count HHIs vs Non-Medicaid Resident Count HHIs

	County Resident HHI		County Non-Medicaid Resident HHI	
	State X Year FE	SY FE + Year X Concentrated FE	State X Year FE	SY FE + Year X Concentrated FE
DCW Min				
Subsidy Baseline	5.008*	5.237*	4.895*	5.162*
	(2.187)	(2.162)	(2.238)	(2.240)
Subsidy*Concentrated	−0.317	−1.687	0.052	−1.372
	(0.658)	(1.235)	(0.618)	(1.147)
Nurse Min				
Subsidy Baseline	1.862+	1.945+	1.880+	2.010+
	(1.006)	(0.992)	(1.022)	(1.001)
Subsidy*Concentrated	−0.283	−0.865+	−0.295	−1.021+
	(0.316)	(0.456)	(0.311)	(0.496)
CNA Min				
Subsidy Baseline	3.144*	3.292*	3.014*	3.150*
	(1.234)	(1.242)	(1.256)	(1.288)
Subsidy*Concentrated	−0.035	−0.822	0.346	−0.351
	(0.581)	(1.039)	(0.536)	(0.803)
Pct Nurse				
Subsidy Baseline	0.026	0.038	0.029	0.057
	(0.108)	(0.096)	(0.113)	(0.094)
Subsidy*Concentrated	−0.040	−0.129	−0.044	−0.178
	(0.151)	(0.162)	(0.143)	(0.139)
Avg Wage				
Subsidy Baseline	0.146*	0.152*	0.160*	0.166*
	(0.052)	(0.052)	(0.047)	(0.048)
Subsidy*Concentrated	−0.045	−0.066	−0.069*	−0.089+
	(0.032)	(0.049)	(0.028)	(0.047)
DCW PPR				
Subsidy Baseline	1.156	1.164	1.528	1.606
	(1.172)	(1.169)	(1.014)	(0.994)
Subsidy*Concentrated	−0.130	−0.210	−1.002*	−1.266*
	(0.193)	(0.373)	(0.415)	(0.411)

This table gives estimates of the instrumented effect of subsidies by market concentration on a number of outcomes. The "Subsidy Baseline" coefficients refer to the effect of receiving a one dollar per resident-day subsidy offer on nursing homes in unconcentrated markets, while the "Subsidy Interaction" coefficient gives the differential effect of subsidy receipt on nursing homes in concentrated markets. The two super columns vary the market concentration definition being used. Each takes a market, defined as the set of all nursing homes within a county, as concentrated if its Herfindahl-Hirschman Index exceeds 2500, adjusting the HHI for multi-establishment firms by summing the market shares of nursing homes owned by the same firm prior to squaring the market shares. The super columns vary, however, in that in one case, market shares are calculated in terms of shares of market-wide nursing home resident counts, while in the other, they are calculated in terms of shares of market-wide nursing home non-Medicaid resident counts. Within each super column, the left column reports results from the instrumental variables regression including only state-by-year and nursing home-by-policy event fixed effects, while the column on the right adds a market concentration status-by-year fixed effect. For further details on how the instrumental variable regressions are estimated and on the outcome variables, please see the notes to Table III.4.

Table III.10: Association Between Subsidy Receipt and Pre-Trends in Staffing, Wages, and Payroll by Market Concentration per the 15-mile and County Market Definitions

	15 Mile HHI		County HHI	
	State X Year FE	SY FE + Year X Concentrated FE	State X Year FE	SY FE + Year X Concentrated FE
DCW Min				
Pre-Trend Baseline	−3.131	−3.598	−2.436	−3.049
	(2.914)	(3.331)	(2.962)	(3.113)
Pre-Trend*Concentrated	1.726	4.314	0.067	3.862
	(1.127)	(3.275)	(1.414)	(2.461)
Nurse Min				
Pre-Trend Baseline	−0.841	−0.906	−0.582	−0.665
	(1.071)	(1.239)	(1.134)	(1.184)
Pre-Trend*Concentrated	0.970*	1.687	0.482	1.330
	(0.419)	(1.700)	(0.650)	(1.602)
CNA Min				
Pre-Trend Baseline	−2.289	−2.692	−1.853	−2.384
	(1.983)	(2.221)	(1.940)	(2.040)
Pre-Trend*Concentrated	0.755	2.628	−0.415	2.532+
	(0.903)	(1.838)	(0.900)	(1.277)
Pct Nurse				
Pre-Trend Baseline	0.259	0.360+	0.282	0.388+
	(0.201)	(0.202)	(0.180)	(0.183)
Pre-Trend*Concentrated	0.132	−0.199	0.109	−0.359*
	(0.157)	(0.165)	(0.157)	(0.154)
Avg Wage				
Pre-Trend Baseline	0.019	0.023	0.029	0.023
	(0.061)	(0.057)	(0.053)	(0.052)
Pre-Trend*Concentrated	0.013	0.053	−0.019	0.026
	(0.071)	(0.135)	(0.077)	(0.129)
DCW PPR				
Pre-Trend Baseline	0.502	0.068	0.625	0.557
	(0.927)	(0.895)	(0.850)	(0.812)
Pre-Trend*Concentrated	−0.315	1.916*	−1.041	−0.065
	(0.590)	(0.834)	(0.648)	(1.100)

This table gives estimates of the instrumented effect of eventual subsidy receipt by market concentration on pre-trends in all major outcomes considered in the primary analysis section of this chapter. Pre-trends are calculated using the change in outcomes from two years prior to each policy event to one year prior to each policy event. The subsidy measure used is the immediate one-year change in instrumented subsidy offers experienced by each nursing home. The "Subsidy Baseline" coefficients refer to the association between pre-trends and experiencing a one dollar per resident-day subsidy offer increase on nursing homes in unconcentrated markets, while the "Subsidy Interaction" coefficient gives the differential effect of increased subsidy receipt on nursing homes in concentrated markets. Note that in each specification, one first stage regression is estimated for each of the subsidy and the subsidy interaction term, with each first stage regression including both the main subsidy instrument and the main subsidy instrument interacted with the concentrated market indicator variable. The two super columns vary the market concentration definition being used. Each takes a market as concentrated if its Herfindahl-Hirschman Index exceeds 2500, calculating the HHI using shares of market-wide resident counts as market shares and assigning residents for nursing homes with the same owner to a single entity. The super columns vary, however, in terms of market definition, using a 15-mile radius around each nursing home as the market definition in one and each nursing home's county as the market definition in the other. Within each super column, the left column reports results from the instrumental variables regression including only state-by-year and nursing home-by-policy event fixed effects, while the column on the right adds a market concentration status-by-year fixed effect.

Table III.11: Association Between Subsidy Receipt and Pre-Trends in Staffing, Wages, and Payroll by Market Concentration per the 15-mile and 30-mile Market Definitions

	15 Mile HHI		30 Mile HHI	
	State X Year FE	SY FE + Year X Concentrated FE	State X Year FE	SY FE + Year X Concentrated FE
DCW Min				
Pre-Trend Baseline	−3.131	−3.598	−2.590	−3.106
	(2.914)	(3.331)	(2.887)	(2.900)
Pre-Trend*Concentrated	1.726	4.314	1.329	7.878*
	(1.127)	(3.275)	(1.240)	(2.353)
Nurse Min				
Pre-Trend Baseline	−0.841	−0.906	−0.548	−0.794
	(1.071)	(1.239)	(1.140)	(1.061)
Pre-Trend*Concentrated	0.970*	1.687	0.827	3.577*
	(0.419)	(1.700)	(0.505)	(1.158)
CNA Min				
Pre-Trend Baseline	−2.289	−2.692	−2.042	−2.312
	(1.983)	(2.221)	(1.897)	(1.950)
Pre-Trend*Concentrated	0.755	2.628	0.502	4.303*
	(0.903)	(1.838)	(0.871)	(1.895)
Pct Nurse				
Pre-Trend Baseline	0.259	0.360+	0.307+	0.325+
	(0.201)	(0.202)	(0.169)	(0.176)
Pre-Trend*Concentrated	0.132	−0.199	0.050	−0.215
	(0.157)	(0.165)	(0.183)	(0.222)
Avg Wage				
Pre-Trend Baseline	0.019	0.023	0.032	0.019
	(0.061)	(0.057)	(0.052)	(0.055)
Pre-Trend*Concentrated	0.013	0.053	−0.133*	0.087
	(0.071)	(0.135)	(0.053)	(0.225)
DCW PPR				
Pre-Trend Baseline	0.502	0.068	0.512	0.339
	(0.927)	(0.895)	(0.798)	(0.836)
Pre-Trend*Concentrated	−0.315	1.916*	−1.346	1.509*
	(0.590)	(0.834)	(1.156)	(0.588)

This table gives estimates of the instrumented effect of eventual subsidy receipt by market concentration on pre-trends in all major outcomes considered in the primary analysis section of this chapter. The "Subsidy Baseline" coefficients refer to the association between pre-trends and experiencing a one dollar per resident-day subsidy offer increase on nursing homes in unconcentrated markets, while the "Subsidy Interaction" coefficient gives the differential effect of increased subsidy receipt on nursing homes in concentrated markets. For further details on how the instrumental variable regressions are estimated and on construction of the pre-trends and subsidy measure used here, please see the notes to Table III.10. The two super columns vary the market concentration definition being used. Each takes a market as concentrated if its Herfindahl-Hirschman Index exceeds 2500, calculating the HHI using shares of market-wide resident counts as market shares and assigning residents for nursing homes with the same owner to a single entity. The super columns vary, however, in terms of market definition, using a 15-mile radius around each nursing home as the market definition in one and a 30-mile radius in the other. Within each super column, the left column reports results from the instrumental variables regression including only state-by-year and nursing home-by-policy event fixed effects, while the column on the right adds a market concentration status-by-year fixed effect.

Table III.12: Association Between Subsidy Receipt and Pre-Trends in Staffing, Wages, and Payroll by Market Concentration per the 15-mile Multi-Establishment Firm Adjusted and Unadjusted Market Definitions

	15 Mile Firm HHI		15 Mile Estab HHI	
	State X Year FE	SY FE + Year X Concentrated FE	State X Year FE	SY FE + Year X Concentrated FE
DCW Min				
Pre-Trend Baseline	−3.131	−3.598	−2.873	−2.905
	(2.914)	(3.331)	(2.933)	(3.156)
Pre-Trend*Concentrated	1.726	4.314	1.236	2.388
	(1.127)	(3.275)	(0.992)	(2.595)
Nurse Min				
Pre-Trend Baseline	−0.841	−0.906	−0.699	−0.545
	(1.071)	(1.239)	(1.170)	(1.262)
Pre-Trend*Concentrated	0.970*	1.687	0.702+	0.364
	(0.419)	(1.700)	(0.378)	(1.099)
CNA Min				
Pre-Trend Baseline	−2.289	−2.692	−2.173	−2.359
	(1.983)	(2.221)	(1.924)	(2.065)
Pre-Trend*Concentrated	0.755	2.628	0.533	2.024
	(0.903)	(1.838)	(0.786)	(1.672)
Pct Nurse				
Pre-Trend Baseline	0.259	0.360+	0.266	0.364+
	(0.201)	(0.202)	(0.186)	(0.195)
Pre-Trend*Concentrated	0.132	−0.199	0.128	−0.350*
	(0.157)	(0.165)	(0.150)	(0.155)
Avg Wage				
Pre-Trend Baseline	0.019	0.023	0.010	0.007
	(0.061)	(0.057)	(0.061)	(0.064)
Pre-Trend*Concentrated	0.013	0.053	0.036	0.077
	(0.071)	(0.135)	(0.060)	(0.125)
DCW PPR				
Pre-Trend Baseline	0.502	0.068	0.394	0.050
	(0.927)	(0.895)	(0.872)	(0.883)
Pre-Trend*Concentrated	−0.315	1.916*	−0.103	1.806*
	(0.590)	(0.834)	(0.452)	(0.545)

This table gives estimates of the instrumented effect of eventual subsidy receipt by market concentration on pre-trends in all major outcomes considered in the primary analysis section of this chapter. The "Subsidy Baseline" coefficients refer to the association between pre-trends and experiencing a one dollar per resident-day subsidy offer increase on nursing homes in unconcentrated markets, while the "Subsidy Interaction" coefficient gives the differential effect of increased subsidy receipt on nursing homes in concentrated markets. For further details on how the instrumental variable regressions are estimated and on construction of the pre-trends and subsidy measure used here, please see the notes to Table III.10. The two super columns vary the market concentration definition being used. Each takes a market, defined as the set of all nursing homes within a 15-mile radius around a given nursing home, as concentrated if its Herfindahl-Hirschman Index exceeds 2500, calculating the HHI using shares of market-wide resident counts as market shares. The super columns vary, however, in that in the Firm-based HHI case, market shares for nursing homes owned by the same firm are summed prior to squaring, while in the Establishment-based HHI case all nursing home establishments are assumed to operate independently. Within each super column, the left column reports results from the instrumental variables regression including only state-by-year and nursing home-by-policy event fixed effects, while the column on the right adds a market concentration status-by-year fixed effect.

Table III.13: Association Between Subsidy Receipt and Pre-Trends in Staffing, Wages, and Payroll by Market Concentration per the County Multi-Establishment Firm Adjusted and Unadjusted Market Definitions

	County Firm HHI		County Estab HHI	
	State X Year FE	SY FE + Year X Concentrated FE	State X Year FE	SY FE + Year X Concentrated FE
DCW Min				
Pre-Trend Baseline	−2.436	−3.049	−2.455	−2.979
	(2.962)	(3.113)	(2.963)	(3.086)
Pre-Trend*Concentrated	0.067	3.862	0.134	3.572
	(1.414)	(2.461)	(1.411)	(2.489)
Nurse Min				
Pre-Trend Baseline	−0.582	−0.665	−0.605	−0.642
	(1.134)	(1.184)	(1.159)	(1.190)
Pre-Trend*Concentrated	0.482	1.330	0.565	1.042
	(0.650)	(1.602)	(0.678)	(1.551)
CNA Min				
Pre-Trend Baseline	−1.853	−2.384	−1.850	−2.337
	(1.940)	(2.040)	(1.909)	(2.007)
Pre-Trend*Concentrated	−0.415	2.532+	−0.431	2.530+
	(0.900)	(1.277)	(0.840)	(1.367)
Pct Nurse				
Pre-Trend Baseline	0.282	0.388+	0.274	0.382*
	(0.180)	(0.183)	(0.161)	(0.175)
Pre-Trend*Concentrated	0.109	−0.359*	0.135	−0.438*
	(0.157)	(0.154)	(0.110)	(0.163)
Avg Wage				
Pre-Trend Baseline	0.029	0.023	0.028	0.020
	(0.053)	(0.052)	(0.053)	(0.053)
Pre-Trend*Concentrated	−0.019	0.026	−0.014	0.001
	(0.077)	(0.129)	(0.081)	(0.103)
DCW PPR				
Pre-Trend Baseline	0.625	0.557	0.591	0.564
	(0.850)	(0.812)	(0.842)	(0.816)
Pre-Trend*Concentrated	−1.041	−0.065	−0.912	−0.312
	(0.648)	(1.100)	(0.649)	(1.078)

This table gives estimates of the instrumented effect of eventual subsidy receipt by market concentration on pre-trends in all major outcomes considered in the primary analysis section of this chapter. The "Subsidy Baseline" coefficients refer to the association between pre-trends and experiencing a one dollar per resident-day subsidy offer increase on nursing homes in unconcentrated markets, while the "Subsidy Interaction" coefficient gives the differential effect of increased subsidy receipt on nursing homes in concentrated markets. For further details on how the instrumental variable regressions are estimated and on construction of the pre-trends and subsidy measure used here, please see the notes to Table III.10. The two super columns vary the market concentration definition being used. Each takes a market, defined as the set of all nursing homes within a county, as concentrated if its Herfindahl-Hirschman Index exceeds 2500, calculating the HHI using shares of market-wide resident counts as market shares. The super columns vary, however, in that in the Firm-based HHI case, market shares for nursing homes owned by the same firm are summed prior to squaring, while in the Establishment-based HHI case all nursing home establishments are assumed to operate independently. Within each super column, the left column reports results from the instrumental variables regression including only state-by-year and nursing home-by-policy event fixed effects, while the column on the right adds a market concentration status-by-year fixed effect.

Table III.14: Association Between Subsidy Receipt and Pre-Trends in Staffing, Wages, and Payroll by Market Concentration per the 15-mile Market Definition with Resident Count HHIs vs Non-Medicaid Resident Count HHIs

	15 Mile Resident HHI		15 Mile Non-Medicaid Resident HHI	
	State X Year FE	SY FE + Year X Concentrated FE	State X Year FE	SY FE + Year X Concentrated FE
DCW Min				
Pre-Trend Baseline	−3.131	−3.598	−2.985	−3.554
	(2.914)	(3.331)	(2.920)	(3.245)
Pre-Trend*Concentrated	1.726	4.314	1.354	4.313
	(1.127)	(3.275)	(1.031)	(2.875)
Nurse Min				
Pre-Trend Baseline	−0.841	−0.906	−0.809	−0.941
	(1.071)	(1.239)	(1.080)	(1.199)
Pre-Trend*Concentrated	0.970*	1.687	0.879*	1.815
	(0.419)	(1.700)	(0.398)	(1.567)
CNA Min				
Pre-Trend Baseline	−2.289	−2.692	−2.175	−2.613
	(1.983)	(2.221)	(1.976)	(2.164)
Pre-Trend*Concentrated	0.755	2.628	0.474	2.498
	(0.903)	(1.838)	(0.799)	(1.579)
Pct Nurse				
Pre-Trend Baseline	0.259	0.360+	0.248	0.344
	(0.201)	(0.202)	(0.201)	(0.197)
Pre-Trend*Concentrated	0.132	−0.199	0.157	−0.173
	(0.157)	(0.165)	(0.153)	(0.150)
Avg Wage				
Pre-Trend Baseline	0.019	0.023	0.026	0.021
	(0.061)	(0.057)	(0.057)	(0.058)
Pre-Trend*Concentrated	0.013	0.053	−0.003	0.067
	(0.071)	(0.135)	(0.063)	(0.134)
DCW PPR				
Pre-Trend Baseline	0.502	0.068	0.494	0.070
	(0.927)	(0.895)	(0.927)	(0.901)
Pre-Trend*Concentrated	−0.315	1.916*	−0.298	1.928*
	(0.590)	(0.834)	(0.594)	(0.840)

This table gives estimates of the instrumented effect of eventual subsidy receipt by market concentration on pre-trends in all major outcomes considered in the primary analysis section of this chapter. The "Subsidy Baseline" coefficients refer to the association between pre-trends and experiencing a one dollar per resident-day subsidy offer increase on nursing homes in unconcentrated markets, while the "Subsidy Interaction" coefficient gives the differential effect of increased subsidy receipt on nursing homes in concentrated markets. For further details on how the instrumental variable regressions are estimated and on construction of the pre-trends and subsidy measure used here, please see the notes to Table III.10. The two super columns vary the market concentration definition being used. Each takes a market, defined as the set of all nursing homes within a 15-mile radius around a given nursing home, as concentrated if its Herfindahl-Hirschman Index exceeds 2500, adjusting the HHI for multi-establishment firms by summing the market shares of nursing homes owned by the same firm prior to squaring the market shares. The super columns vary, however, in that in one case, market shares are calculated in terms of shares of market-wide nursing home resident counts, while in the other, they are calculated in terms of shares of market-wide nursing home non-Medicaid resident counts. Within each super column, the left column reports results from the instrumental variables regression including only state-by-year and nursing home-by-policy event fixed effects, while the column on the right adds a market concentration status-by-year fixed effect.

Table III.15: Association Between Subsidy Receipt and Pre-Trends in Staffing, Wages, and Payroll by Market Concentration per the County Market Definition with Resident Count HHIs vs Non-Medicaid Resident Count HHIs

	County Resident HHI		County Non-Medicaid Resident HHI	
	State X Year FE	SY FE + Year X Concentrated FE	State X Year FE	SY FE + Year X Concentrated FE
DCW Min				
Pre-Trend Baseline	−2.436	−3.049	−2.274	−2.824
	(2.962)	(3.113)	(3.028)	(3.281)
Pre-Trend*Concentrated	0.067	3.862	−0.419	2.815
	(1.414)	(2.461)	(1.270)	(2.844)
Nurse Min				
Pre-Trend Baseline	−0.582	−0.665	−0.563	−0.639
	(1.134)	(1.184)	(1.153)	(1.250)
Pre-Trend*Concentrated	0.482	1.330	0.364	1.092
	(0.650)	(1.602)	(0.633)	(1.777)
CNA Min				
Pre-Trend Baseline	−1.853	−2.384	−1.711	−2.185
	(1.940)	(2.040)	(1.984)	(2.135)
Pre-Trend*Concentrated	−0.415	2.532+	−0.784	1.724
	(0.900)	(1.277)	(0.741)	(1.404)
Pct Nurse				
Pre-Trend Baseline	0.282	0.388+	0.294	0.391+
	(0.180)	(0.183)	(0.190)	(0.204)
Pre-Trend*Concentrated	0.109	−0.359*	0.057	−0.344
	(0.157)	(0.154)	(0.168)	(0.235)
Avg Wage				
Pre-Trend Baseline	0.029	0.023	0.037	0.032
	(0.053)	(0.052)	(0.055)	(0.060)
Pre-Trend*Concentrated	−0.019	0.026	−0.036	−0.003
	(0.077)	(0.129)	(0.081)	(0.142)
DCW PPR				
Pre-Trend Baseline	0.625	0.557	0.641	0.623
	(0.850)	(0.812)	(0.864)	(0.813)
Pre-Trend*Concentrated	−1.041	−0.065	−0.873	0.017
	(0.648)	(1.100)	(0.523)	(0.783)

This table gives estimates of the instrumented effect of eventual subsidy receipt by market concentration on pre-trends in all major outcomes considered in the primary analysis section of this chapter. The "Subsidy Baseline" coefficients refer to the association between pre-trends and experiencing a one dollar per resident-day subsidy offer increase on nursing homes in unconcentrated markets, while the "Subsidy Interaction" coefficient gives the differential effect of increased subsidy receipt on nursing homes in concentrated markets. For further details on how the instrumental variable regressions are estimated and on construction of the pre-trends and subsidy measure used here, please see the notes to Table III.10. The two super columns vary the market concentration definition being used. Each takes a market, defined as the set of all nursing homes within a county, as concentrated if its Herfindahl-Hirschman Index exceeds 2500, adjusting the HHI for multi-establishment firms by summing the market shares of nursing homes owned by the same firm prior to squaring the market shares. The super columns vary, however, in that in one case, market shares are calculated in terms of shares of market-wide nursing home resident counts, while in the other, they are calculated in terms of shares of market-wide nursing home non-Medicaid resident counts. Within each super column, the left column reports results from the instrumental variables regression including only state-by-year and nursing home-by-policy event fixed effects, while the column on the right adds a market concentration status-by-year fixed effect.

III.VIII Multi-Establishment Firm Identification Appendix

The primary measure of market concentration used in this chapter assigns market shares for nursing homes owned by the same firm to a single entity, thereby adjusting market shares to account for ownership by multi-establishment firms. The process of identifying which nursing homes were owned by the same entity was non-trivial. The OSCAR/CASPR data reports, for each year, whether a given nursing home is part of a nursing home chain or if instead it is independently owned. The OSCAR/CASPR data also elicits for nursing homes that are not independently owned the name of the firm that owns it, though this information is only available for me in the years 1996-1999. Although the data includes these firm owner names, it does not include numerical identifiers for owner firms. This is a non-trivial complication since nursing homes often report their owners' names inconsistently. Beyond just typographic errors and the like, nursing homes also vary in terms of whether they report their parent company's full name, in their use of various abbreviations, and in whether they report their parent company's name or the name of the division in their parent company that they most directly interact with.

In order to address these difficulties and identify which nursing homes are owned by the same parent companies, I adopted the following procedure. First, I took the set of all nursing home parent company names reported in the 1996-1999 data, converted them to lower case, stripped them of punctuation, and stemmed them (e.g., converted words like "walks", "walking", and "walked" to "walk"). I took any nursing homes reporting the same parent company name after this simple cleaning procedure to be owned by the same parent company. Next, I calculated the string distance between each unique, cleaned nursing home parent company name and every other unique, cleaned parent company name using the Jaro-Winkler string distance metric, using a penalty parameter of 0.1. Using this set of string distances between each pair of parent company names, I adopted the following algorithm to assign nursing homes to parent companies and applied it to my list of nursing home parent company names:

For a given seed parent company name under consideration, begin by forming a list of candidate parent company names which may refer to the same parent company as the seed parent company name. This candidate list must contain every parent company name with a Jaro-Winkler string distance of less than or equal to 0.3 from the seed name under starting consideration. For each parent company name on the candidate list, construct for it its own sub-list of other parent company names that also may refer to it, using the same string distance of 0.3 or less criterion. Next, for each parent company name that appears on either the initial candidate list or on one of the sub-lists, calculate how many lists (including both the sub-lists and the seed name's list) it appears on. Then, conclude that any parent company name appearing on 80% or more of the lists (again, including both the sub-lists and the initial seed name's list) refers to the same parent company as the seed name being examined. Assign all nursing homes that report being owned by either the seed parent company name or by one of the matched parent company names a single, unique owner identifier number and remove their reported parent company names from the pool of unassigned names. Finally, iterate this procedure on the now shrunken list of unassigned names until all nursing homes are assigned to unique parent company owner identifiers.

The above procedure is intended to address a number of practical difficulties. Since I have no information about the total number of nursing home parent companies in existence in a given year, I must deduce how many unique parent companies exist based on patterns in the parent company name data. The sub-list appearance procedure is intended to help do that by linking nursing home parent company names only when a given pool of names frequently match with one another. This helps ensure that parent company names that, for idiosyncratic reasons, happen to match just one parent company name in a pool but not the others will not be assigned to the pool, while still allowing parent company names that match most of the pool to fail to match some of the names in the pool. This procedure also helps reduce the degree to which the results of the algorithm depend on the order in which different parent company names are examined. For an example of why these properties are valuable, consider the case of a nursing home owned by the hypothetical "Jekyll Corporation" that reports as its parent company name "Jekyll North West", the division of its parent company with which the nursing home most directly interacts.[68] Taking "Jekyll North West" as the seed parent company name, I might form an immediate match candidate list consisting of "Jekyll Inc", "Jekyll North East", "Jekyll South West", "Jekl South West", "Jekyll Northwet", and "Hyde North West". The sub-list correspondence procedure will help ensure that "Jekyll South East" enters the final match pool as it will match with most of the other "Jekyll" names, despite not matching very well with the seed name "Jekyll North West". The sub-list procedure will also help screen out "Hyde North West" as it will be a poor match for most Jekyll names despite being a reasonably good match for "Jekyll North West".

There are a few other issues associated with this procedure worth considering. First, I apply the above algorithm to the set of all parent company names from 1996-1999, rather than going separately year by year. This is done so as to allow a larger pool of parent company names to be used, which in turn should help improve the reliability of the sub-list

[68]Within the actual pool of nursing home parent company names, nursing homes do appear to report parent company division names rather frequently.

procedure described above. Spot checks indicate that nursing homes are usually successfully matched to the same parent company firm ID over time, with exceptions appearing to represent either actual changes in nursing home ownership or a name changes by the nursing home's owner. Second, in order to address the years 2000 to 2015, I carry forward the 1999 nursing home parent companies linkages, changing them only to relabel nursing homes as independent if they report no longer being owned by a nursing home chain. Nursing homes who enter after 1999 or enter into parent company ownership after 1999 are labeled as independent. While this procedure is reasonable for the bulk of my sample – most of my payroll subsidy variation occurs in the late 1990s and early 2000s – there may be substantial measurement error in my assignment of nursing homes to owners the in late 2000s and 2010s. As a final comment, it is worth noting that my procedure does assign a number of nursing homes to parent companies that do not appear to own any other nursing homes. While it is potentially true that this is due to a failure by my algorithm to match nursing homes to the correct owners, it is also worth noting that many nursing homes are part of multi-establishment firms that genuinely only own one nursing home, but which do own other healthcare businesses such as hospitals or assisted living facilities.

IV Chapter 4: Excess Capacity and Heterogeneity in the Fiscal Multiplier: Evidence from the Obama Stimulus Package

This chapter is coauthored with Arindrajit Dube, Ethan Kaplan, and Ben Zipperer.

IV.I Introduction

> We do not have a good measure of the effects of fiscal policy in a recession because the methods that we use to estimate the effects of fiscal policy—both those using the observed outcomes following different policies in aggregate data and those studying counterfactuals in fitted model economies—almost entirely ignore the state of the economy and estimate "the" government multiplier, which is presumably a weighted average of the one we care about—the multiplier in a recession—and one we care less about—the multiplier in an expansion. Notable exceptions to this general claim suggest this difference is potentially large. Our lack of knowledge stems significantly from the focus on linear dynamics: vector autoregressions and linearized (or close-to-linear) dynamic stochastic general equilibrium (DSGE) models. Our lack of knowledge also reflects a lack of data: deep recessions are few and nonlinearities hard to measure" – Parker (2011).

This chapter shifts focus away from the payroll subsidies examined in the prior two chapters in order to study the effect of fiscal stimulus on employment, aiming in particular estimate the fiscal multiplier as a function of employment conditions. We do so using spatial variation in stimulus expenditure generated by the American Recovery and Reinvestment Act of 2009 during the Great Recession along with variation in the county-level impact of the recession itself. While there is already a large body of research aiming to estimate fiscal multipliers, the approach laid out in this chapter will help address the gaps in our knowledge laid out by Parker (2011) as our use of spatial variation will enable us to much more credibly identify the multiplier and the degree to which it varies with the state of the economy.

There is considerable variation in existing empirical estimates of fiscal multipliers. Some are in the 0-0.5 range (Barro and Redlick, 2011; Conley and Dupor, 2013), others are near to 1 (Ramey, 2011), and yet others are well above 1 (Blanchard and Perotti, 2002). Theoretical estimates exhibit similar variation, from near zero (Baxter and King, 1993) to well over 1 and sometimes even over 2 (Chodorow-Reich, 2017; Christiano et al., 2011; Woodford, 2011). Part of the reason for the disagreement, as Parker (2011) points out, may be due to heterogeneity in the multiplier as a function of the degree of slack in the economy. For macroeconomic stabilization purposes, the figure of greatest interest is the multiplier during a recession when there is a great deal of excess capacity in the economy. Unfortunately, estimation of the multiplier as a function of excess capacity has been elusive. The reasons for this are three-fold. First, at a country level, identification must exclusively rely on time series variation. However, the timing of expenditures is correlated with the business cycle itself and thus expenditures are confounded by the state of the economy. Second, there is a limited sample size from which to perform statistical inference. Some recent efforts have attempted to use a narrative approach in order to estimate fiscal multipliers (Romer and Romer, 2010). However, this approach is plagued by small sample sizes, which is particularly problematic when investigating heterogeneous effects of fiscal stimulus by the amount of slack in the economy. Third, when excess capacity is high, interest rates tend to be low and fiscal expenditure more effective. Thus, the interest rate confounds the estimate of the heterogeneity in the multiplier. This is particularly important when excess capacity is quite large since during such times the interest rate on government debt is likely to be close to the zero lower bound on nominal interest rates, and a large recent literature argues that multipliers are much higher at the zero lower bound and when policy interest rates are sticky downwards (Christiano et al., 2011; Eggertsson et al., 2003; Eggertsson, 2011; Woodford, 2011).

Beginning with Auerbach and Gorodnichenko (2012), a burgeoning literature has attempted to estimate heterogeneity in the multiplier using time series variation (Auerbach and Gorodnichenko, 2012; Baum et al., 2012; Clemens and Miran, 2012; Fazzari et al., 2015; Ramey, 2011; Ramey and Zubairy, 2018; Mittnik and Semmler, 2012; Semmler and Semmler, 2013). However, it has focused on the difference in the multiplier at the zero lower bound in nominal interest rates. One recent paper (Ramey and Zubairy, 2018) also uses time series variation and employs vector autoregression methods to estimate the differential multiplier in recessions versus expansions. They find no differential due to high unemployment and only limited evidence for a difference in multipliers caused by the zero lower bound. Their multiplier estimates lie between 0.4 and 0.8 throughout recessions and booms and during periods of high interest rates as well as low interest rates. However, their time series based estimation is not well identified. Moreover, such estimates are not robust to timing misspecification or to alternative choices of method for constructing impulse response functions (Ramey and Zubairy, 2018).

An alternative approach to multiplier estimation using national time series is estimation using intra-national variation in fiscal expenditures over time (Chodorow-Reich et al., 2012; Chodorow-Reich, 2017; Conley and Dupor, 2013; Feyrer and Sacerdote, 2011; Moretti, 2010; Nakamura and Steinsson, 2014; Serrato and Wingender, 2016). There is even a small set of papers estimating local multipliers using variation in American Recovery and Reinvestment Act spending across local areas (Chodorow-Reich et al., 2012; Conley and Dupor, 2013; Feyrer and Sacerdote, 2011).

This chapter uses cross-county variation in expenditure during the American Recovery and Reinvestment Act to estimate fiscal multipliers. Further, we semi-parametrically estimate the multiplier as a non-parametric function of the degree of counties' excess capacity, measured by the magnitude of the negative employment shock each county received as a consequence of the Great Recession. We show that our estimates of the fiscal multiplier satisfy time placebos and are robust to inclusion of Bartik controls for evolution of employment based upon the sectoral composition of local employment and national trends in employment by sector. Moreover, Boone et al. (2014) show that allocation of funds in ARRA was not correlated with the unemployment rate.[69] Our paper is closest in topic to Ramey and Zubairy (2018) in that we estimate the heterogeneity in the multiplier as a function of excess capacity, partialing out interest rate effects. It is closest to Feyrer and Sacerdote (2011) in terms of methods, though they do not estimate differential multipliers by excess capacity. In contrast to Ramey and Zubairy (2018), we find a seven-fold increase in the fiscal multiplier for above-median excess capacity counties compared to below-median excess capacity counties. One recent paper, Michaillat (2014), shows that when unemployment is higher, public sector employment crowds out fewer private matches and thus the multiplier is countercyclical. Our methods are unable to distinguish whether the multiplier is higher in high excess capacity areas because in those areas there is more idle capital (Keynes, 2018), whether there is less crowdout of matching efficiency from employment programs (Michaillat, 2014), or whether in areas of high

[69] These results are at the Congressional District level but they hold at the county level as well.

excess capacity, unemployment is also greater and consumption multipliers are therefore higher (Gross et al., 2016). It is also with noting that, as with the rest of the local multiplier literature, our use of spatial variation comes at a cost. We do not directly estimate a national fiscal multiplier and, because expenditures are financed through federal rather than through local taxes, what we are estimating is more akin to a transfer multiplier in an open economy as opposed to a national fiscal multiplier.

In addition to providing an estimate of the multiplier as a function of excess capacity, our paper also adds to the literature in a number of other ways. First, we provide more evidence on the dynamics of expenditure, computing non-parametric impulse responses similar to what is estimated in the Vector Autoregression literature (Blanchard and Perotti, 2002; Ramey, 2011; Ramey and Zubairy, 2018). Allowing more non-parametric estimation of impulse responses to fiscal expenditure is beneficial because it allows for arbitrary non-linearities in the time path of the effect of expenditure. This is a particularly important contribution given Ramey and Zubairy (2018)'s demonstration of the sensitivity of the multiplier to the method of constructing impulse response functions in non-linear models such as the VAR models pioneered by Auerbach and Gorodnichenko (2012) to estimate heterogeneity in the multiplier over the business cycle.

Second, we point out that, given the heterogeneity which we find in the fiscal multiplier, our estimates reflect an average multiplier which is averaged over each dollar spent. We separate out the economic and political aspects of the multiplier by computing the multiplier for a politically unconstrained government which optimally targets federal dollars to the highest multiplier areas. In other words, in addition to estimating the actual multiplier, we compute what the multiplier would have been had the stimulus dollars been optimally spent based upon the information that the government would have had access to at the time.

IV.II Research Design

To estimate the effects of fiscal stimulus we regress county-level quarterly employment and earnings on a quarterly measure of county-level stimulus and additional controls. We primarily use fixed effect methods. To explore heterogeneity of the effects of stimulus across counties of varying excess capacity, we estimate over split samples as well as estimate using the semiparametric smooth coefficient estimator proposed by Li et al. (2002). To explore heterogeneity of the multiplier in the extent of stimulus, we add a quadratic term in the amount of stimulus. We also re-estimate our results, focusing on not just total employment and earnings but also on employment and earnings by industry.

Our primary regressions are estimated at the county-by-quarter level on a sample stretching from 2006Q1 to 2016Q3. Letting i, s, and t denote, respectively, county, state, and quarterly indices, the following specification:

$$Y_{it} = \alpha + \sum_{k=-8}^{8} \beta_k S_{it-k} + \gamma B_{it} + F_{st} + tD_{it}^{'}\Delta + \epsilon_{it} \tag{IV.1}$$

regresses quarterly employment or earnings per capita outcomes Y_{it} on eight lags and leads in stimulus per capita S_{it}. We then report the the contemporaneous effects of stimulus β_0 summed with two, four, six, and eight quarters of lagged stimulus effects. We also report the eight quarter summed leads on stimulus. Controls in this specification include a Bartik shift-share control for predicted employment based upon industrial shares of employment in county i in 2008 Quarter 1, state-time-specific fixed effects F_{st} and demographic controls which vary over time and across counties denoted by D_{it}. Demographic controls all consist of cross-sectional controls interacted with linear time trends. These controls include time trends interacted with 2000 Census estimates of the share of county population that is black, Hispanic, urban, and in poverty. They also include county median income, average amount of home purchase loans in 2006, and total HMDA loans per capita in 2006. In some specifications, the fixed effects are at the state-by-time level or at just the pure time level: F_t. Note that in the specifications including county and state-time fixed effects, identification of stimulus effects hinges on the allocation of stimulus within states being exogenous to trends in employment and wage bill conditional on the demographic-linked trends and on industrial composition-predicted employment and wage bill. Additionally, identification in these specifications requires that stimulus spending has no cross-county spillover effects. Given that these spillover effects should be expected to some degree, our local fiscal multiplier estimates should be viewed more as lower bounds since cross-county spillovers should bias our estimates of stimulus effects toward zero.

In order to assess how the effect of stimulus varies by excess capacity, we simply re-estimate the specification in equation IV.1 above, but separately among counties with above median and below median excess capacity. Note that our measure of excess capacity (E_i), the construction of which is discussed further in the next section, is based on pre-period industry shares and is constant for each county. Additionally, we also investigate heterogeneity in the multiplier using a more flexible non-linear interaction between stimulus and excess capacity, or semi-parametric smooth coefficient model:

$$Y_{it} = g(E_i) + (S_{it}, \mathbf{Z}_{it})'\mathbf{h}(E_i) + \epsilon_{it} \tag{IV.2}$$

where the scalar g and vector $\mathbf{h}$ are unspecified functions of excess capacity. We estimate equation IV.2 at each excess capacity percentile e^p by linear regressions of Y_{it} on S_{it}

and $\mathbf{Z}_{it}$ for observations whose population-weighted kernel-based distance is near e^p, as suggested by Li et al. (2002). We county-cluster bootstrap these estimates to conduct inference. Note that with this approach, including lags and leads on the effect of stimulus is infeasible due to sample size constraints, so we are limited only to estimating contemporaneous stimulus effects here.

IV.III Data and Measurement

Our primary data sources are the Quarterly Census of Employment and Wages (QCEW) from the Bureau of Labor Statistics (BLS) [70] and the recipient-reported American Recovery and Reinvestment Act stimulus award data from recovery.gov.[71] Additionally, we use a variety of other demographic data as control variables from multiple sources that we detail below.

IV.III.I Outcomes

The key dependent variables are employment and earnings (wage bill) per capita. We construct our main employment measure, employment per capita, by dividing county-level employment reported at the quarterly level from the QCEW and dividing it be intercensal population estimates from the Bureau of the Census for the population aged 15-64.[72]. Quarterly earnings data are taken directly from the county-level BLS QCEW. We

[70] https://www.bls.gov/cew/

[71] This data was formerly available at http://www.recovery.gov/FAQ/Pages/DownloadCenter.aspx; a portion of it is currently available at https://www.nber.org/data/ARRA/.

[72] The definition of employment per capita used by BLS divides aggregate employment in the CPS by the 16+ population. Our definition differs from this definition in a few respects. First, we use the QCEW rather than the CPS. The QCEW is based on unemployment insurance records reported to state governments by firms and then transmitted to the U.S. Census. It differs from the CPS in that it is a census, not a sample. Thus, we have accurate measures of employment by county in each quarter. However, though it contains 98% of jobs, it does not contain the self-employed. Additionally, at the county level, we use July intercensal estimates of population for people aged 15-64 (http://www.census.gov/popest/) rather than 16+. Our measures of employment are thus smaller than those used by BLS to construct national employment per capita. However, our

divide the aggregate wage bill in a county-quarter by the size of the population aged 15-64. We also then divide by 100,000 so that wage bill impacts can be interpreted as impacts upon per capita wages per $100,000. These data are not seasonally adjusted. To calculate quarterly population levels we use the annual July 1st intercensal estimates published by the US Census Bureau[73] as our third quarter population estimates and interpolate estimates among quarters assuming a quarterly geometric growth rate.

IV.III.II Treatment

For our treatment variable, we use the amount of stimulus funds per capita spent in a county in a quarter. We construct this variable using recipient-reported stimulus award data that we downloaded from www.recovery.gov. These data are a panel of individual contracts, grants, and loans reported quarterly beginning in 2009q4 through 2013q3, though we only use data on contracts and grants.[74] Award data are also reported for a single 2009q1-2009q2 period, which we we assign to 2009q2.Recipient-reported data is available for prime awardee recipients and sub-recipients who receive more than $25,000. Prime awardees report the overall award amount and sub-recipients report subawards. We construct our dataset using prime awards and their award amount. We then add in subrecipients and their subawards.

Prime awards report their expenditure-to-date on a quarterly basis. We use this data to construct prime awards' expenditure per quarter. Subawards do not report expenditure-to-date, instead reporting only when the subaward is active. We assume that subawards

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measures of population are larger. Overall, our employment per capita measure is smaller than the national measure constructed by BLS.

[73] http://www.census.gov/popest/

[74] We exclude data on loans out of concern that their inclusion would overstate the quantity of stimulus flowing to different regions, depending on the relative interest rates between public and private loans and on whether recipients were credit constrained.

are spent at the same rate as their prime awards over the period in which the subaward is active. Specifically, in any quarter when a subaward is active, we assume the share of the subaward's total value spent in that quarter is equal to the its prime award's expenditure that quarter divided by the total prime award expenditure that occurs while the subaward is active.[75]

A reasonably small number of prime awards report nonmonotonicities in their cumulative amount spent over time, such as when they report cumulative expenditure levels of 0 in their final reporting periods. In cases like these where awards report cumulative expenditure levels lower than the prior cumulative expenditure level for up to two consecutive quarters, we correct these nonmonotonicities by replacing those observations with data linearly interpolated based on the two quarters surrounding the block of nonmonotonic cumulative expenditure levels. When expenditure levels fall in an award's final reporting period, we use the value from the prior period. Only a small share of the data exhibits nonmonotonicities of any kind. In the rare cases that these corrective procedures fail to produce monotonically increasing cumulative expenditure data, we drop the data from our sample.

For a given award in the recipient-reported data, funds awarded and place of performance zip code are available for prime recipients and subrecipients. We assign zip codes to counties using the MABLE/Geocorr2K Census 2000 zip code-to-county crosswalk.[76] In the rare cases where place-of-performance zip code data is not reported or is reported with

[75]Not all prime award expenditure is reported as spent prior to the end of our stimulus award sample. In cases where a subaward is active up through the last reporting period for a prime award that does not report expenditure of the entire award, we divide by total prime award expenditure during the subaward active period summed with the outstanding unspent funds.

[76]When a zip code in multiple counties, we allocate awards based on population shares using MABLE/Geocorr2K Census 2000 population allocation factors.

error, we use an award's reported city and state to assign it to a county. When this data is also not available, we use the award recipient's zip code.

We divide our stimulus measure by the size of the population aged 15-64 per the Census. We then also divide by $100,000. Thus, our estimates of the impact of stimulus can be interpreted as the impact of receiving an additional $100,000 of expenditure per capita in a county.[77]

IV.III.III Excess Capacity

For our measure of county excess capacity, we compute a county's excess capacity as the largest observed 1-year or 2-year reduction in industry shift-share predicted employment in the county from 2006 to 2008. We then multiply our measure by -1 for reporting convenience. We restrict ourselves to 1-year and 2-year reductions in order to account for possible variation in the degree to which employment is seasonal across counties. In practice, however, this seasonality adjustment is not very impactful on the construction of the excess capacity measure. Intuitively, this measure ranks counties by the size of the reduction in the employment to population ratio that their industry-composition suggests they should have received, prior to passage of the American Recovery and Reinvestment Act.

IV.III.IV Controls

Because our identification using panel data relies on county fixed effects, we do not use lagged outcomes as controls for fear of biasing our OLS estimates. Instead, we use as controls predicted employment and earnings, using pre-period county-level industry shares and contemporaneous national level employment and earnings to predict actual

[77] Since we divided the dependent and independent variables by the same measure of population, any measurement error in the county-level intercensal population estimates produced by the Census introduces negative bias into our estimated multiplier.

employment and earnings in the manner of Bartik (1991). Specifically, we first calculate county-level averages over the years 2006 and 2007 employment (earnings) shares of national employment (earnings) at the three-digit NAICS level. Then we multiply these county-NAICS shares by contemporaneous national three-digit NAICS employment. We sum the resulting county-NAICS series over NAICS categories to form a single, time-varying predicted employment (earnings) series for each county.

In addition to Bartik-predicted outcomes and geographic and time dummies, we also employ a variety of pre-period demographic controls in the hope of increasing the precision of our estimates and also to account for potential selection bias remaining in stimulus assignment. In particular, we focus on 2000 Census county-level estimates of the share of county populations that are black, Hispanic, urban, and in poverty, as well as county-level median income.[78] Because of the central role of housing wealth in the most recent recession, we also use two county-level housing variables derived from the loan origination reported under the Home Mortgage Disclosure Act (HMDA): the 2006 average value of home purchase loans and the 2006 total of all HMDA loans divided by county population.[79] We then interact these demographic controls with a time trend and include them in our county fixed effects regressions.

IV.IV Results

We begin by showing the amount of stimulus spent over time. Figure IV.1 shows the amount spent nationally over time. The majority of the funds were spent by 2012. However, even at the end of our data set, 13% of them had not been allocated. We see in Figure IV.2 that the amount spent was relatively randomly distributed across the United

[78] http://www.icpsr.umich.edu/icpsrweb/ICPSR/studies/13402/ascii

[79] http://www.ffiec.gov/hmda/hmdaproducts.htm

States. Looking at the shaded map of amount spent by county, we see no obvious spatial patterns of expenditure.

IV.IV.I Own-county Multipliers

We present our baseline estimates of the contemporaneous own-county impact of stimulus in Table IV.1. These estimates are broken down into two super-columns. The left super-column contains estimates of the impact of stimulus on own-county employment and the right super-column contains estimates of stimulus on the own-county wage bill. Across the various rows, we show the sum of the contemporaneous effect of stimulus and either 2, 4, 6, or 8 lags of stimulus. Finally, in the bottom row, we show the sum of all 8 leads on stimulus. Within each super column, we show four separate specifications with progressively more stringent sets of controls and fixed effects. All columns contain county fixed effects. The first column additionally contains quarter-by-year (henceforth time) fixed effects. The second column puts in a set of controls which vary across counties and over time: a Bartik predicted outcome variable [80] percent black, hispanic, urban, and under poverty, median income and 2006 average home purchase price for loans and 2006 total HMDA loans per capita. The third column drops demographic and economic covariates but replaces time fixed effects with state-by-time fixed effects. The final column re-adds the demographic and economic controls to the model with state-by-time and county fixed effects.

We use our estimates with the most stringent set of controls as our main estimates. We believe that they are the best identified. They generally also have the greatest precision.

[80] When the outcome variable is the employment to population ratio, then the Bartik prediction is for the employment to population ratio; when the outcome variable is the wage, then we compute a Bartik predicted wage. The Bartik controls are computed using county-level employment and wage bill from the QCEW, averaged over the 2006-2007 time period. Predictions are made using industrial composition at the three-digit NAICS level.

Our results do vary some across specification. However, they are all of the same sign and, with a single exception among the six-quarter aggregated and eight-quarter aggregated effects, are all significant at the 5% level. Our benchmark estimate of the effect of an additional $100,000 of stimulus expenditure per capita on employment is that the additional expenditure increases employment per capita by 0.424 percentage points. Since ARRA was $787 billion and the U.S. population in 2011 (when the median ARRA dollar was spent) was 311 million, this amounts to $2500 per person or 2.5% of $100,000 per person. Thus, assuming that the multiplier for all ARRA expenditure is the same as for ARRA's contract and grant expenditure, our estimates imply that ARRA expenditures raised employment per capita by 1.07 percentage points.

We also estimate the impact of stimulus on wage bill. The 8-lag time-aggregated wage bill effect is 0.172, though just barely fails to achieve statistical significance at the 5% level. This point estimate implies that an extra $100,000 of expenditure per capita raises the per capita wage bill by $17,200. If we divide this by the employment multiplier, we find that if wages for employed people did not rise, the average person employed by the stimulus would have made $40,566. The marginal jobs created by ARRA thus were likely lower than median paying jobs, but not exceptionally low paying. Overall, our time-aggregated results are consistent with a positive and significant multiplier. It is also worth noting that across all specifications, the eight-quarter time-aggregated leads fail to achieve statistical significance, suggestive of stimulus expenditure not being preceded by substantial differences in employment or wage bill shocks.

The sign of our estimates is informative and useful in better understanding the channels through which stimulus worked. It is important to note that we are estimating the impact of expenditures holding fixed tax payments, as long as tax payments are not correlated with stimulus expenditures. We empirically estimate, though do not report, estimates controlling for deciles of tax payments per capita, and find that including these controls does not qualitatively affect our results. Thus, we find that our estimates do not reflect differences in future potential tax burden across counties, but rather reflect differences in expenditures. Unlike traditional national estimates, our local estimates therefore effectively hold expected future tax payments constant and net wealth increases are at least weakly larger in the areas which receive stimulus. Since, in the baseline macro model, an increase in wealth should reduce rather than increase labor supply, the fact that we estimate positive coefficients indicates that the multiplier is not likely through the wealth-labor supply channel. The leading alternative channel is the Keynesian demand-side channel.

IV.IV.II Heterogeneity by Excess Capacity

In Table IV.2 and Figures IV.3, IV.4, and IV.5we present estimates of heterogeneity by the degree of excess capacity. Recent macroeconomic theory (Christiano et al., 2011; Woodford, 2011; Eggertsson, 2011) suggests strong heterogeneity in the multiplier when the interest rate reaches the zero lower bound. However, others have suggested that government expenditures may vary also by the degree of slack or excess capacity in the economy (Keynes, 2018; Parker, 2011). There are many reasons as to why the multiplier may vary with the degree of excess capacity. In counties with greater slack, employment does not necessarily rely upon large capital investments. In addition, in areas with high unemployment, consumers have may be more liquidity constrained and have a higher propensity to consume out of money spent.

Our parametric estimates of the impact of stimulus funds in counties with higher and lower excess capacity are in Table IV.2. Once again, we separate out the presentation of our estimates into two super columns. In the left super column, we present employment multipliers and, on the right, wage bill multipliers. Each super column contains three columns. These columns show pooled estimates (identical to the estimates in Table IV.1), estimates from below-median excess capacity counties, and estimates from above-median excess capacity counties. All results presented here are estimated using the all controls benchmark specification (column 4 in Table IV.1). Note that a visualization of these results that plots the impulse-response of employment and wage bill to stimulus is available in Figure IV.3.

We find very strong differences between low and high excess capacity counties in their estimated multipliers. Our cumulative lag estimates for both wage bill and employment in low excess areas are substantially smaller and statistically insignificant. The stimulative effect of an extra $100,000 per capita of government expenditure in below-median excess capacity counties is to raise employment per capita by a statistically insignificant 0.148 over two years. Turning to above median-excess capacity counties, we find a substantially larger employment multiplier. The high-excess capacity multiplier is almost 7 times the size of the low-excess capacity multiplier. An additional $100,000 of expenditure per capita in high excess capacity counties yields 0.981 extra jobs per person. ARRA overall is thus estimated to have increased employment in high excess capacity areas by 3.17 percentage points over two and a quarter years (or 1.41 per year). It is important to point out that these cross-sectional multipliers are identified off of differential changes across counties with high versus low excess capacity. Since interest rates were the same in high and low excess capacity areas (and in fact, policy rates were at the zero lower bound for most of the period), our estimates are not confounded by differential multipliers at the zero lower bound, as is common with time series estimates (Ramey and Zubairy, 2018). In addition to the ability to estimate using variation unconfounded by the state of the economy, the ability to unconfound state-contingent multipliers from interest-rate contingent multipliers is one of the greatest benefits of our cross-county panel estimation strategy.

The wage bill multipliers exhibit similar patterns to what is observed for the employment multipliers. The low excess capacity wage bill coefficient implies an additional expenditure of $100,000 per person yields a statistically insignificant additional $4,800 per person in wage income. Strikingly, the wage bill effect in high excess capacity counties is nearly 10 times larger than the effect in low excess capacity counties. Specifically, in high excess capacity counties, an additional expenditure of $100,000 per person yields an addition $40,600 wage bill per person, though this effect is only statistically significant at the 10% level. This point estimates implies that ARRA generated an additional $1,400 per person in high excess capacity counties. Note that, as in the pooled case, the aggregated lead effects of stimulus on employment and wage bill are once again statistically insignificant in both the low and high excess capacity samples.

Once concern with our main parametric approach is that our main effects are estimated using lag operators. However, in a balanced panel, longer lags are estimated off of a truncated sample of time periods. Thus, dynamic estimates can reflect causal effects of treatment or compositional differences in lag estimation. To address this concern, we also estimate the dynamic impact of stimulus by regressing employment per capita and the wage bill respectively on a set of time dummies interacted with the (time-invariant) average stimulus amount spent in a county. We plot these estimates in Figure IV.5. The results here are consistent with our main estimates. In low excess capacity counties, we see essentially no effect on employment per capita, and only a small effect on wage bill. In high excess capacity counties, by contrast, we see an immediate, large, and persistent increase in employment for counties that received greater stimulus. The effect on wage bill is smaller and less persistent, but also present during the height of stimulus expenditure.

Finally, in Figure IV.4, we present the plots of our semi-parametric estimates of the effect of stimulus by excess capacity on both employment per capita and the wage bill. In these plots, the x-axis gives county excess capacity, while the y-axis the effect of stimulus estimated at a given point in the excess capacity distribution. As we can see from the first panel, the employment multiplier increases weakly monotonically until excess capacity reaches high levels, where estimates become more imprecise. The wage bill multipliers exhibit a similar pattern, though remain more flat at lower excess capacity levels. These results comport with our parametric findings, identifying once again substantial heterogeneity in the multiplier as a function of excess capacity.

IV.IV.III Placebo Tests

There is a risk that our multipliers may not reflect causal effects of stimulus but rather the differential evolution of employment per capita and wages across counties that would have occurred in the absence of stimulus spending. For example, if counties suffering more from the recession received more money but, owing to their steeper reduction in employment, also had larger recoveries, it is possible that ARRA expenditures could merely reflect the depth of the crisis and the subsequent natural recovery. In order to test if this is the case, in Table IV.3, we conduct a set of placebo tests where we regress a number of measures of pre-ARRA changes in employment and wage bill on the total amount of stimulus expenditure taking place in each county. In particular, we use as outcomes the change in employment and wage bill between 2006Q1 and 2007Q1 as well as between 2006Q1 and 2009Q1. We also use the severity in the pre-Great Recession drop in employment, measured as the maximum quarter-to-quarter pre-Great Recession dip (over the period 2006Q1–2007Q4 to 2008Q1-2009Q1). All these regressions control for state fixed effects and we present results both with and without the same cross-sectional controls used in our primary regressions. Note that we estimate on the full set of counties, but regress on our placebo treatment measure and that same measure interacted with a dummy for above median excess capacity.

Out of 20 regressions, we find none with statistically significant coefficients and only one with a coefficient significant at even the 10% level. We thus find that stimulus expenditures in a county was not well predicted by evolution of employment and wages before the passage of ARRA. As such, this evidence indicates that ARRA funding is likely not endogenous to the evolution of employment and wages in a county.

IV.IV.IV Non-Linear Impacts of Stimulus Funds

One possible explanation for the heterogeneity in the multiplier between high excess capacity regions and low excess capacity regions is that more money was spent in high excess capacity regions and that the multiplier increases with the amount spent. In Table IV.4, we show that this does not explain our heterogeneity results. In particular, we regress the employment to population ratio as well as the wage bill on the same fixed effects and controls as used in the benchmark heterogeneous effects tables, but now including a quadratic term in contemporaneous stimulus expenditure and for each lag of stimulus expenditure. We drop the leads on stimulus in this specification, since they do not impact the model much, are not very informative in this context, and prove taxing in terms of degrees of freedom when estimated with quadratic terms. Once again, we break our results into pooled effects, effects in low excess capacity counties, and effects in high excess capacity counties.

The results in Table IV.4 do not support the hypothesis that higher multipliers in high excess capacity areas are driven in some way by increasing returns to stimulus. Within the pooled set of counties and in each subset of counties, the point estimate of the quadratic term in the effect of stimulus on each of employment per capita and wage bill at every level of lag aggregation is negative. However, note that the negative quadratic terms are only statistically significant in the pooled models. These results suggest that, if anything, there may be diminishing returns to stimulus expenditure. However, it is worth noting that the practical impact of this diminishing returns seems small, with substantial differences in the effect occurring only in a relatively small number of outlier counties. Table IV.4 reports the effect of stimulus at each of the 25th and 75th percentiles of the distribution of actual stimulus expenditure within each sample of counties. The nonlinearity in the effect of stimulus implies virtually no difference in stimulus efficacy across this range.

IV.IV.V Sector Specific Multipliers

For further evidence on the mechanism behind variation in our local multipliers by excess capacity, we show how the impact of fiscal expenditure on employment and wages varies by sector of the local economy. Funds allocated to public sector schools, to Medicaid, and to other public programs are likely to increase public employment. In addition, contracts given to manufacturing firms are likely to increase local employment in manufacturing. However, stimulus expenditures may also increase employment in non-tradable sectors which did not receive federal funds through a consumption multiplier. The impact of aggregate stimulus expenditure in the county on industry-specific employment captures both direct contractual effects of stimulus expenditure as well as demand spillovers. In order to shed more light on differential effects by sector, we simply re-estimate our main regressions by industry, using the QCEW broken down by industry. Our sample size drops slightly because in a small number of county-quarters, data on employment by industry data is missing for disclosure reasons. Results from these regressions are available in Table IV.5.

We first break down employment into public sector employment and private sector employment. We estimate effects on all counties, on low excess capacity counties and on high excess capacity counties separately. We find that the public sector employment effect does not substantially vary across low and high excess capacity counties, consistent with the absence of demand spillovers to public sector employment and wages. Meanwhile, the effect for private sector employment is small and negative in low excess capacity counties but positive and much larger in high excess capacity counties. Similar is true of the results for wage bill. Breaking down the private sector employment effects into sub-industries, we find that the negative employment effects in low excess capacity counties are spread across all private industries but goods and construction, while they are spread across all industries in the wage bill case. Point estimates of employment and wage bill multipliers in the high excess capacity counties, meanwhile, are positive for all industries. Note that all of these results require some interpretive caution, as the industry effect estimates are all estimated rather imprecisely and do not achieve statistical significance.

Overall, our industry-specific results are useful for validating our approach and for explicating the channels through which expenditure impacts employment and the wage bill. First, they point toward stimulus crowding out private sector employment in low excess capacity places but not in high excess capacity places. This is consistent with labor market slack being required for public sector employment expansion to not come at the expense of private sector employment and is consistent with the predictions of Michaillat (2014). Second, the fact that the larger high excess capacity multipliers are being driven by the private sector, and especially non-tradable industries in the case of the employment multiplier, is suggestive of the difference in multipliers being driven by demand effects.

IV.IV.VI Output Multipliers and the Effectiveness of an Optimally Allocated Stimulus

In this section, we compute the number of job-years created from the contracts and grants portion of the Obama stimulus bill and then compute the number of job-years that would have been created had the money been spent solely in above median excess capacity counties. We also calculate the output multipliers implied by our employment multipliers.

Our baseline estimates suggest a time-aggregated employment multiplier of 0.424 jobs per $100,000 spent. Since $261 billion were spent on the contracts and grants portion of the American Recovery and Reinvestment Act, the portion of ARRA funding studied in this analysis, that leaves us with 1.107 million job years created. As shown in Section 4.2 and in Boone et al. (2014), the amount of stimulus provided to an area is very weakly correlated with the unemployment rate in the area. Under the assumption that the multiplier is the same in all regions, this allocation would maximize the efficacy of the stimulus. However, in this paper, we have shown that the multiplier in high excess capacity regions is many times greater than the multiplier in low excess capacity areas. This poor targeting of high unemployment areas lowers the average multiplier that we estimate.

We now compute the number of jobs created if the stimulus had been optimally spatially targeted towards high excess capacity counties. We do this using a simple back of the envelope calculation. We assume that there are two multipliers: one for above median excess capacity counties and a separate one for below median excess capacity counties. We also assume that the multipliers do not substantially change with increased expenditures.[81] We do note that even if stimulus had been allocated to only above median excess capacities, excess capacity in the above median counties would still have remained above the median.

We compute the output multiplier, following Chodorow-Reich (2017), by dividing income per worker by cost per job. We use the year 2011 as a benchmark year since the median ARRA dollar was spent in 2011. In 2011, income per worker was $111,400. Our estimated effect on employment per capita per $100,000 of stimulus expenditures per capita was to create 0.424 jobs per capita. This translates into a cost per job of $235,850. Our employment multiplier thus translates to an output multiplier of 0.472. However, if money had solely been allocated to above-median excess capacity counties, the multiplier would have been 0.981 and the cost per job would have been $101,900. This implies that the associated output multiplier would have been 1.09.[82] Thus the multiplier would have been 130% higher – it would have more than doubled.

This computation is revealing for two reasons. First, from a policy perspective, it shows the importance of optimally allocating funds. Of course, optimal allocation of funds might make bill passage more difficult. However, the welfare consequences of allocating funds in a spatially optimal manner are large. Second, this computation makes a methodological point about multiplier estimation. Most macroeconomic theories are divorced

[81] Although the nonlinearity results do point toward eventual diminishing returns to stimulus, the negative nonlinearities are quite weak: moving from the 25th to the 75th percentile of the actual stimulus distribution has virtually no effect on the multiplier. For this reason, we view the assumption that multipliers would not substantially change due to nonlinearities in stimulus expenditure as quite reasonable. Furthermore, optimal reallocation to high excess capacity counties would involve reducing expenditure in at least some very high stimulus areas where returns to marginal stimulus expenditure is negative, counterbalancing some of the diminishing returns in other counties.

[82] By contrast, the implied output multiplier in low excess capacity counties is just 0.164. This figure is on the low end of other measures of multipliers in "good times" (Ramey and Zubairy, 2018).

from the political economy of bill passage. To the degree that the economy has a single multiplier, this is a useful abstraction. In the presence of spatial heterogeneity in the multiplier, estimating national multipliers using national data estimates an average multiplier rather than an employment maximizing multiplier.

IV.V Conclusion

This chapter estimates local employment multipliers and converts them into output multipliers. On average, we find a jobs multiplier of 0.42, meaning that an additional $100,000 of expenditure translates into just 0.42 additional jobs over 2 years. This jobs multiplier yields an equivalent output multiplier of 0.47. However, this average multiplier masks substantial heterogeneity in the multiplier by excess capacity. We both parametrically and semi-parametrically estimate the multipliers as a function of excess capacity. We find large differentials between low and high excess capacity regions. Even during the Great Recession, we find no statistically significant, cumulative impact of public expenditures on overall employment in counties below median excess capacity. The evidence from these counties is consistent with the additional public employment largely crowding out private employment, leading to an employment multiplier of an additional 0.15 jobs for every $100,000 of expenditures. However, for the counties with above median excess capacity, we find a substantially larger contemporaneous employment multiplier of 0.98 jobs per person for every $100,000. This translates into a fiscal multiplier in above median excess capacity areas of about 1.09. We also find that multipliers do decline in the amount spent, but only to a small degree.

The employment effects we find are surprisingly persistent, even through the end of our sample period at the end of 2016. This persistence suggest that the gains we document here may understate the ultimate, long run impact of the policy. These findings of persistent stimulus employment effects are also consistent with the finding of cross-sectional hysteresis documented in Yagan (2019), who finds that the employment losses during the Great Recession were highly persistent. By reducing the severity of employment loss from the Great Recession, the American Recovery and Reinvestment Act likely reduced the extent of labor market scarring in hard-hit counties.

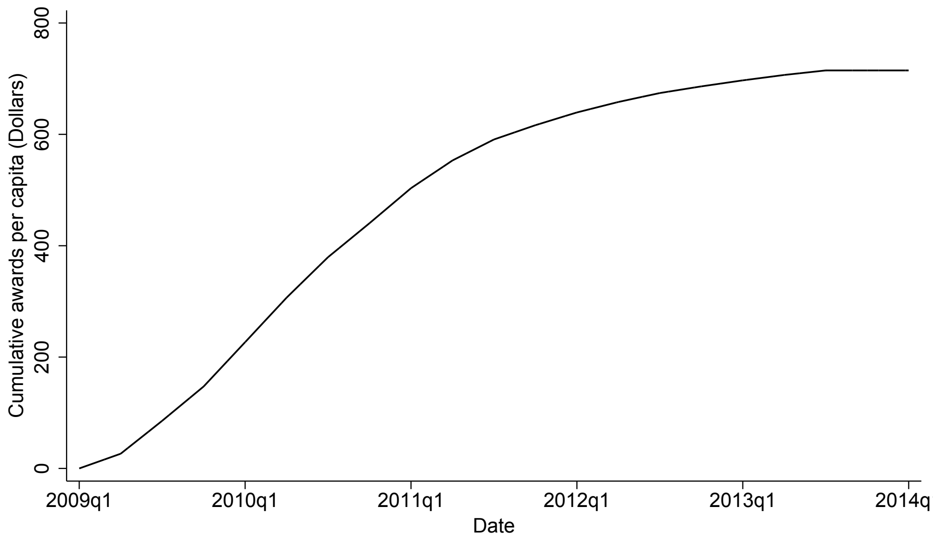
Our estimates are useful for understanding when and where public funds are effective at increasing employment and output and for designing employment maximizing stimulus programs. They also point to the importance of policies that act as automatic stabilizers. The spatial heterogeneity in the multiplier further highlights an important point in testing theories of the multiplier. The aggregate national multiplier is influenced by the political economy of the spatial allocation of funds. Thus, estimates using national data implicitly test a joint economic and political hypothesis. In contrast, by using spatial variation in the multiplier, it is possible to compute a spatially-optimal employment-maximizing multiplier and test an economic hypothesis unconfounded by the political process that targets stimulus to different regions.

We hope that future work will improve upon what we have done by better reconciling local multiplier estimates with national estimates. This reconciliation could be improved in three ways. First, we have focused solely upon labor market impacts of stimulus. However, stimulus could impact capital income as well. Second, our research design cannot capture cross-county spillover effects of stimulus, and so presumably largely identifies effects on various types on non-tradable employment. However, the magnitude of the multiplier would presumably be greater if we could incorporate effects on the tradable sector. There could also be qualitative differences between tradable and non-tradable sector multipliers. Finally, we have estimated our effects during the Great Recession when the nominal interest rate on government debt was at zero. This presumably makes monetary policy relatively ineffective but fiscal policy quite effective. Our estimates would be improved if they could be generalized outside of the ZLB interest rate zone and also if they could incorporate endogenous responses of monetary policy. We hope future empirical work will make progress in these three ways.

Finally, we also would like to see better theoretical explanations for why the fiscal multiplier is increasing in excess capacity. We can imagine three classes of theories focusing on either the labor market, consumption, or production. In the first theory, variation in the degree of public sector crowdout of private sector employment leads the multiplier to be decreasing in the degree of labor market tightness (Michaillat, 2014). In the second, in high unemployment areas, a higher fraction of individuals are liquidity constrained and thus have higher average marginal propensities to consume, generating a larger consumption multiplier and thereby a larger fiscal multiplier. Finally, in the third theory, in higher excess capacity areas, it is easier to hire labor without accompanying capital investments due to slack in the usage of capital. Our empirical results cannot entirely differentiate a labor market tightness effect from a liquidity effect or from an excess capacity effect, though our finding of public sector crowd out of private sector employment in low excess capacity counties provides some suggestive evidence for the presence of a labor market tightness effect. Given this, our paper calls on future theoretical work on the potential impact of excess capacity on the multiplier as well as further empirical work differentiating between the different theories of the countercyclical multiplier.

IV.VI Figures

Figure IV.1: Cumulative Flow of Stimulus Awards



Figures shows per capita cumulative flow of stimulus awards from 2009q1 to 2014q1 in Dollars. County population is the number of residents aged 15-64 in 2008. Timing of stimulus expenditures is adjusted from stimulus recipient reports from www.recovery.gov. Dollars are adjusted for inflation using the quarterly GDP deflator index from FRED, to 2009q3 Dollars.

Figure IV.2: Total Stimulus Awards per Capita

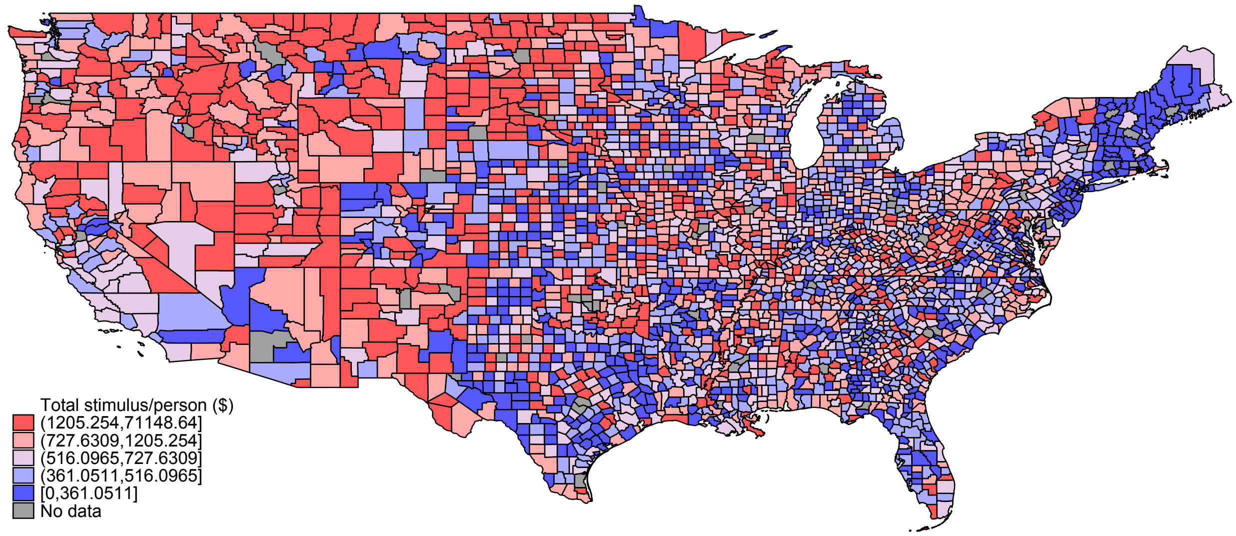
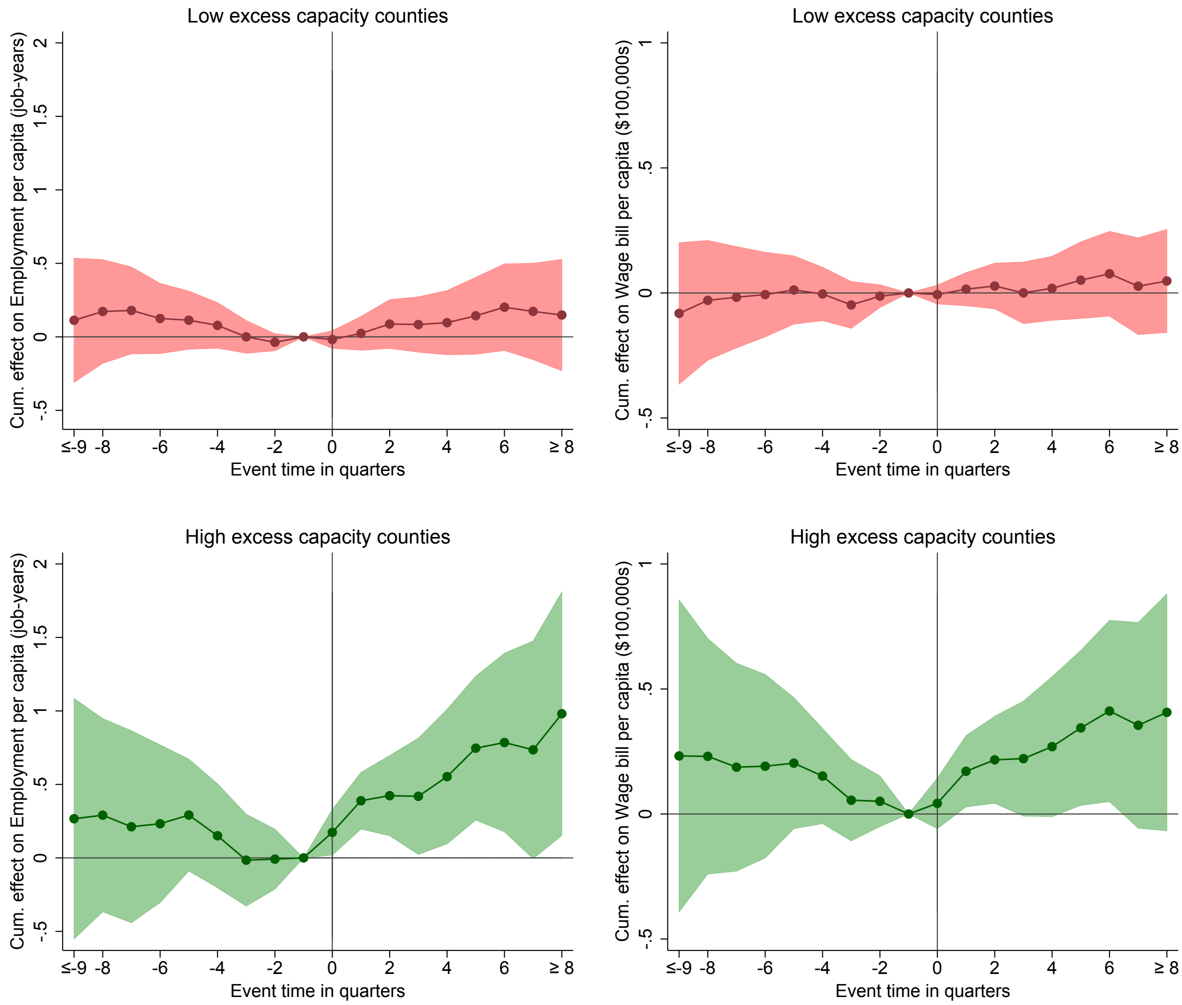


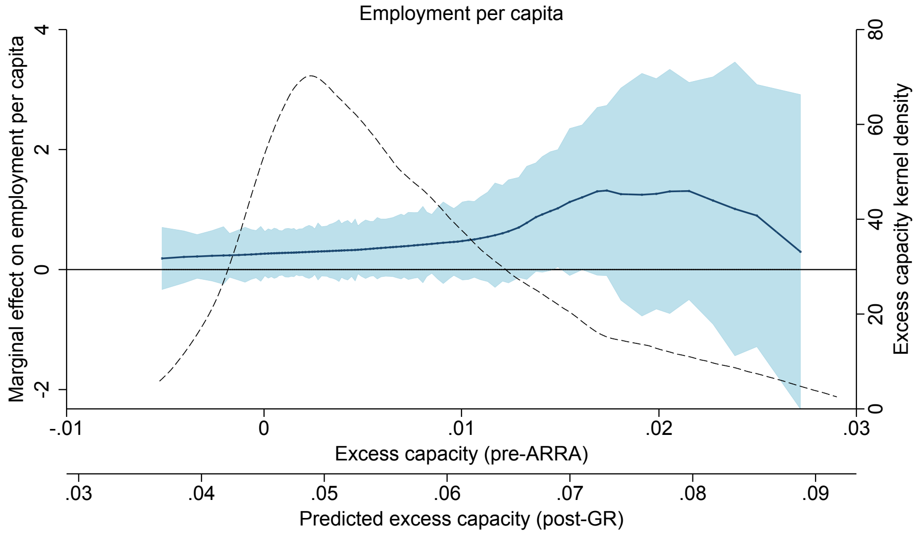
Figure shows total stimulus awards per capita in Dollars, for each county. County population is the number of residents aged 15-64; total county stimulus is divided by this population in 2008. Stimulus expenditures are adjusted from stimulus recipient reports from www.recovery.gov. Dollars are adjusted for inflation using the quarterly GDP deflator index from FRED, to 2009q3 Dollars.

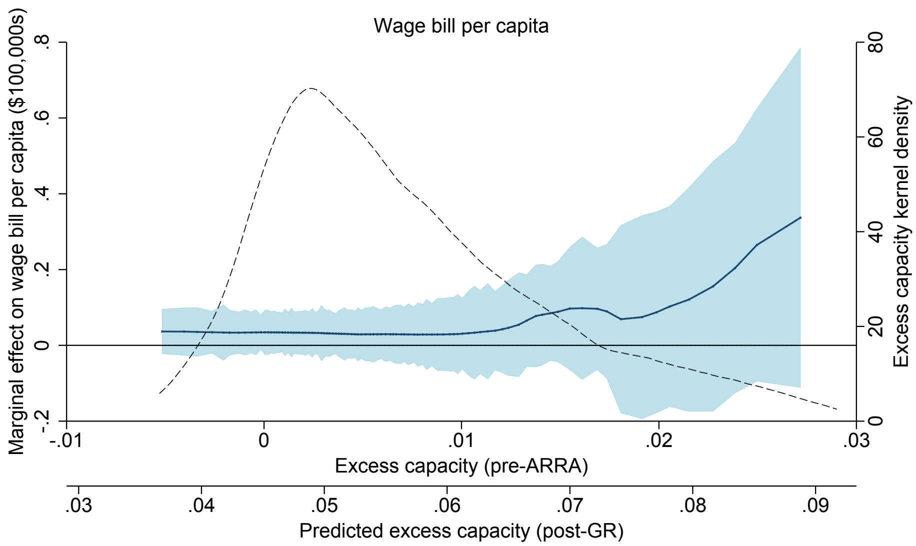
Figure IV.3: Cumulative Response of Employment and Wages to Stimulus, Sample Split by Excess Capacity



Figures show cumulative effects from regressions of own-county employment per capita and own-county wage bill per capita on own-county stimulus, with quarterly lags and leads of treatment. Stimulus expenditures and the wage bill are measured in $100,000 per person. County population is the number of residents aged 15-64. The sample is from 2006Q1 to 2016Q3. Employment and wage bill data come from the QCEW. Timing of stimulus expenditures is adjusted from stimulus recipient reports from www.recovery.gov. Coefficients on lags and leads are (separately) summed cumulatively from event-date -1, where the effect is normalized to 0. The sums of lags include the contemporaneous effect at event-date 0. The vertical reference line indicates 2009q1. The colored line indicates the summed coefficients, while the shaded area is the associated 95% confidence interval. Employment estimates are annualized such that coefficients should be understood as effects on job-years. Regressions control for Bartik predicted employment to population ratio, Bartik predicted wage bill, demographic controls, state-by-time fixed effects and county fixed effects. Bartik predictions are based upon county-level employment and wage bill averages over 2006-2007 at the three-digit NAICS level. Demographic controls are Census 2000 estimates of percents black, hispanic, urban, and under poverty, as well as median income and 2006 average home purchase loans and 2006 total HMDA loans per capita. All demographic controls are interacted with a time trend. Low excess capacity is below and high excess capacity is above the 50th percentile of county excess capacity. Standard errors are clustered at the state level.

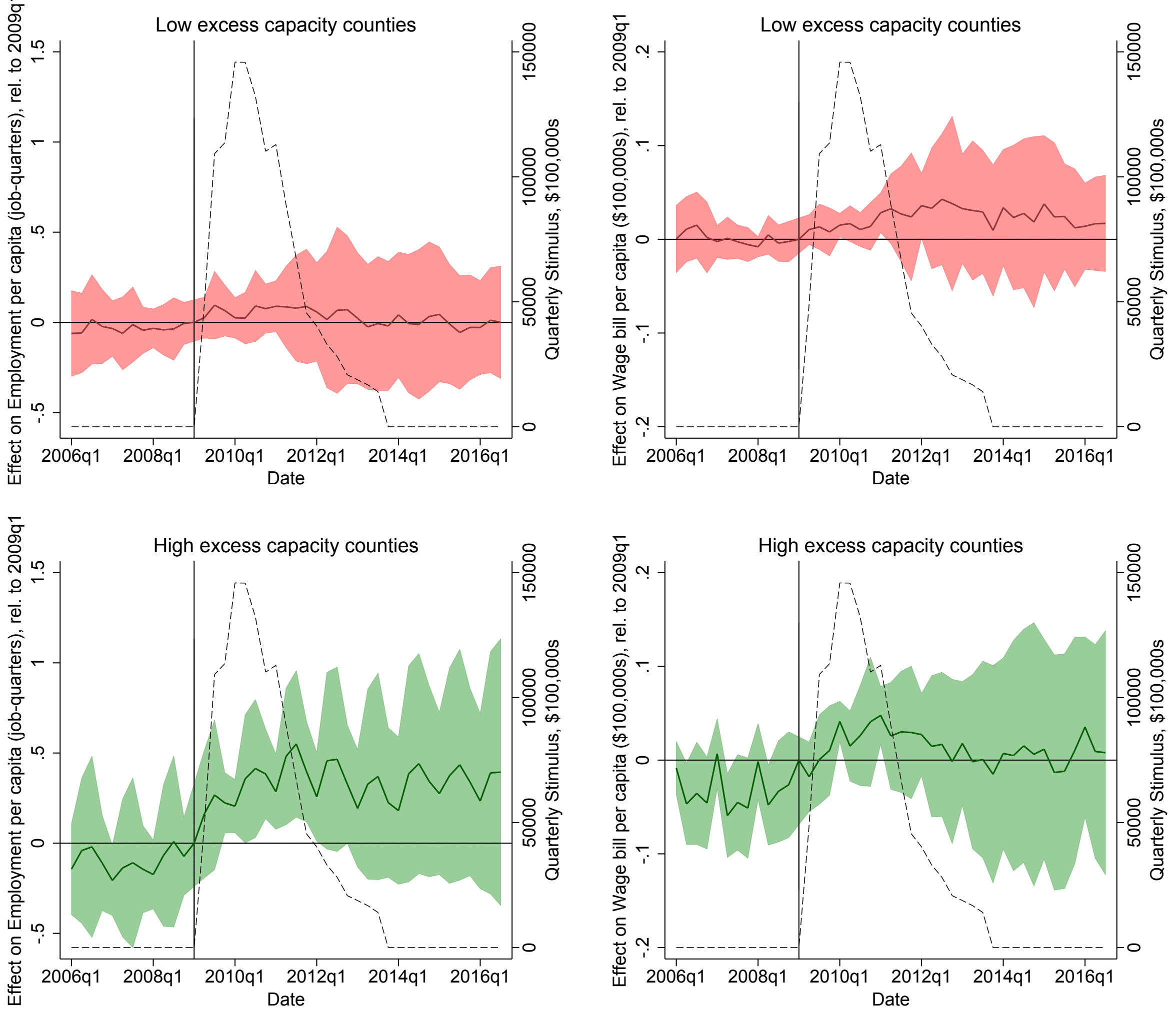
Figure IV.4: Semiparametric Effects of Stimulus by Excess Capacity





Figures show effects from semi-parametric smoothing regressions of own-county employment per capita and own-county wage bill per capita on own-county stimulus, by excess capacity. Stimulus expenditures and the wage bill are measured in $100,000 per person. County population is the number of residents aged 15-64. The sample is from 2006Q1 to 2016Q3. Employment and wage bill data come from the QCEW. Timing of stimulus expenditures is adjusted from stimulus recipient reports from www.recovery.gov. The estimates are from a non-linear interaction model where the linear regression of the outcome on stimulus (plus controls) is estimated at each excess capacity percentile p for observations whose population-weighted kernel-based distance is near p. The colored line indicates the estimated coefficients at each value of excess capacity, while the shaded area is the associated 95% confidence interval. The model is estimated using county excess capacity before the American Recovery and Reinvestment Act was introduced, and these values of excess capacity are indicated on the main x-axis. An additional x-axis is provided which indicates the associated level of excess capacity drawn from the entire Great Recession (GR) period. It shows predicted values from a linear regression of post-GR excess capacity on pre-ARRA excess capacity. The dashed line indicates the density of pre-ARRA excess capacity, estimated using the Epanechnikov kernel function. The figure shows effects only up to the 95th percentile of pre-ARRA excess capacity. Employment estimates are annualized such that coefficients should be understood as effects on job-years. Regressions control for Bartik predicted employment to population ratio, Bartik predicted wage bill, demographic controls, state-by-time fixed effects and county fixed effects. Bartik predictions are based upon county-level employment and wage bill averages over 2006-2007 at the three-digit NAICS level. Demographic controls are Census 2000 estimates of percents black, hispanic, urban, and under poverty, as well as median income and 2006 average home purchase loans and 2006 total HMDA loans per capita. All demographic controls are interacted with a time trend. The estimates are state-cluster bootstrapped to conduct inference.

Figure IV.5: Time-based Effects of Stimulus, Using Time Fixed Effects Interacted with Total Award per Capita, Sample Split by Excess Capacity



Figures show coefficients from regressions of own-county employment per capita and own-county wage bill per capita on own-county aggregate ARRA-stimulus fully interacted with quarterly time dummies. Stimulus expenditures and the wage bill are measured in $100,000 per person. County population is the number of residents aged 15-64. The sample is from 2006Q1 to 2016Q3. Employment and wage bill data come from the QCEW. Timing of stimulus expenditures is adjusted from stimulus recipient reports from www.recovery.gov. Coefficients are interpreted with reference to 2009q1, the omitted time-dummy. The vertical reference line indicates 2009q1. The colored line indicates the coefficients on the stimulus--time-dummy interaction, while the shaded area is the associated 95% confidence interval. The dashed line indicates the total flow of stimulus awards over time, across all counties. Employment estimates are annualized such that coefficients should be understood as effects on job-years. Regressions control for Bartik predicted employment to population ratio, Bartik predicted wage bill, demographic controls, state-by-time fixed effects and county fixed effects. Bartik predictions are based upon county-level employment and wage bill averages over 2006-2007 at the three-digit NAICS level. Demographic controls are Census 2000 estimates of percents black, hispanic, urban, and under poverty, as well as median income and 2006 average home purchase loans and 2006 total HMDA loans per capita. All demographic controls are interacted with a time trend. Low excess capacity is below and high excess capacity is above the 50th percentile of county excess capacity. Standard errors are clustered at the state level.

IV.VII Tables

Table IV.1: Main Effects of Stimulus on Employment and Wage Bill

	Employment per capita				Wage bill per capita			
	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
Time-aggregated lags								
Two quarters	0.314**	0.267**	0.261**	0.206**	0.085	0.078	0.100*	0.082*
	(0.083)	(0.084)	(0.062)	(0.064)	(0.058)	(0.051)	(0.039)	(0.039)
Four quarters	0.404**	0.329**	0.342**	0.259*	0.139^{+}	0.108	0.148**	0.114*
	(0.116)	(0.121)	(0.093)	(0.101)	(0.073)	(0.066)	(0.053)	(0.055)
Six quarters	0.662**	0.557**	0.530**	0.421**	0.260**	0.229*	0.213**	0.184*
	(0.164)	(0.170)	(0.123)	(0.134)	(0.083)	(0.086)	(0.070)	(0.070)
Eight quarters	0.808**	0.655*	0.565**	0.424*	0.357**	0.281*	0.216*	0.172^{+}
	(0.252)	(0.258)	(0.167)	(0.181)	(0.106)	(0.120)	(0.093)	(0.088)
Time-aggregated leads								
Eight quarters	0.333	0.337	0.371^{+}	0.299	−0.115	−0.047	0.045	0.061
	(0.278)	(0.256)	(0.210)	(0.196)	(0.277)	(0.224)	(0.134)	(0.131)
Common time FE	Y	Y			Y	Y		
State X time FE			Y	Y			Y	Y
Controls		Y		Y		Y		Y

Estimates are of own-county employment and wage bill on own-county stimulus expenditures. Stimulus expenditures and the wage bill are measured in $100,000 per person. County population is the number of residents aged 15-64. The sample is from 2006Q1 to 2016Q3. Employment and wage bill data come from the QCEW. Timing of stimulus expenditures is adjusted from stimulus recipient reports from www.recovery.gov. The outcome variable is employment per capita in the four columns on the left and wage bill per capita in the four columns on the right. Regressions are at the quarterly level, but employment estimates are annualized such that coefficients should be understood as effects on job-years. Each column shows sums of coefficients from a single regression. The rows under time-aggregated lags show the sum of contemporaneous results plus subsequent lags. The row under time-aggregated leads shows the sum of coefficients from eight quarter leads. The controls are Bartik predicted employment to population ratio, Bartik predicted wage bill, and demographic controls. Bartik predictions are based upon county-level employment and wage bill averages over 2006-2007 at the three-digit NAICS level. Demographic controls are Census 2000 estimates of percents black, hispanic, urban, and under poverty, as well as median income and 2006 average home purchase loans and 2006 total HMDA loans per capita. All demographic controls are interacted with a time trend. All specifications include county-level fixed effects. Standard errors are clustered at the state level.

Table IV.2: Effects of Stimulus on Employment and Wage Bill, Sample Split by Excess Capacity

	Employment per capita			Wage bill per capita		
	All counties	Low excess	High excess	All counties	Low excess	High excess
Time-aggregated lags						
Two quarters	0.206**	0.087	0.423**	0.082*	0.028	0.217*
	(0.064)	(0.083)	(0.136)	(0.039)	(0.045)	(0.087)
Four quarters	0.259*	0.097	0.553*	0.114*	0.019	0.269^{+}
	(0.101)	(0.109)	(0.228)	(0.055)	(0.064)	(0.139)
Six quarters	0.421**	0.202	0.785*	0.184*	0.077	0.412*
	(0.134)	(0.146)	(0.302)	(0.070)	(0.084)	(0.180)
Eight quarters	0.424*	0.148	0.981*	0.172^{+}	0.048	0.406^{+}
	(0.181)	(0.188)	(0.412)	(0.088)	(0.102)	(0.235)
Time-aggregated leads						
Eight quarters	0.299	0.113	0.266	0.061	−0.082	0.232
	(0.196)	(0.210)	(0.407)	(0.131)	(0.141)	(0.310)

Estimates are of own-county employment and wage bill on own-county stimulus expenditures. Stimulus expenditures and the wage bill are measured in $100,000 per person. County population is the number of residents aged 15-64. The sample is from 2006Q1 to 2016Q3. Employment and wage bill data come from the QCEW. Timing of stimulus expenditures is adjusted from stimulus recipient reports from www.recovery.gov. The outcome variable is employment per capita in the three columns on the left and wage bill per capita in the three columns on the right. Regressions are at the quarterly level, but employment estimates are annualized such that coefficients should be understood as effects on job-years. Each column shows sums of coefficients from a single regression. The rows under time-aggregated lags show the sum of contemporaneous results plus subsequent lags. The row under time-aggregated leads shows the sum of coefficients from eight quarter leads. Regressions control for Bartik predicted employment to population ratio, Bartik predicted wage bill, demographic controls, state-by-time fixed effects and county fixed effects. Bartik predictions are based upon county-level employment and wage bill averages over 2006-2007 at the three-digit NAICS level. Demographic controls are Census 2000 estimates of percents black, hispanic, urban, and under poverty, as well as median income and 2006 average home purchase loans and 2006 total HMDA loans per capita. All demographic controls are interacted with a time trend. Low excess capacity is below and high excess capacity is above the 50th percentile of county excess capacity. Standard errors are clustered at the state level.

Table IV.3: Pre-Trend Validation Tests, Sample Split by Excess Capacity

	No controls		Controls	
	Low excess	High excess	Low excess	High excess
Change in employment				
2006q1–2007q1	0.033	0.018	0.003	0.056
	(0.141)	(0.129)	(0.191)	(0.143)
2006q1–2009q1	0.085	0.212	0.033	0.211
	(0.142)	(0.175)	(0.184)	(0.180)
Change in wage bill				
2006q1–2007q1	−0.002	0.068	−0.006	0.065^{+}
	(0.020)	(0.048)	(0.028)	(0.037)
2006q1–2009q1	0.003	0.007	−0.002	0.006
	(0.022)	(0.030)	(0.031)	(0.026)
GR severity (employment)	−0.052	−0.015	−0.039	0.004
	(0.068)	(0.146)	(0.067)	(0.139)

Estimates are of a variety of trends in pre-ARRA outcome variables on own-county total stimulus expenditure. Stimulus expenditures and the wage bill are measured in $100,000 per person. County population is the number of residents aged 15-64. Employment and wage bill data come from the QCEW. Timing of stimulus expenditures is adjusted from stimulus recipient reports from www.recovery.gov. Outcome variables are listed along the rows. The first four rows' variables are changes in employment per capita or wage bill per capita between 2006q1 and 2007q1, or 2006q1 and 2009q1. The fifth row's variable is a measure of the severity of the Great Recession in a given county before the American Recovery and Reinvestment Act was enacted – it is the largest dip in employment per capita from any quarter in 2006q1-2007q4 to any quarter in 2008q1-2009q1, comparing only between same quarters of the year. Each row per super-column ("No controls" vs "Controls") is a single regression, with "Low excess" and "High excess" specifications estimated simultaneously; each entry is interpretable as coming from a separate (split-sample) regression. Where controls are indicated, regressions include Census 2000 estimates of percents black, hispanic, urban, and under poverty, as well as median income and 2006 average home purchase loans and 2006 total HMDA loans per capita. Low excess capacity is below and high excess capacity is above the 50th percentile of county excess capacity. All regressions control for state fixed effects. Standard errors are clustered at the state level.

Table IV.4: Nonlinear Effects of Stimulus Expenditure, Sample Split by Excess Capacity

	Employment per capita			Wage bill per capita		
	All counties	Low excess	High excess	All counties	Low excess	High excess
Two quarters' cumulative lags						
Linear term	0.366**	0.169	0.668*	0.161*	0.052	0.317*
	(0.081)	(0.120)	(0.255)	(0.065)	(0.062)	(0.148)
Quadratic term	−9.865**	−4.093	−24.247	−5.734*	−1.391	−11.069
	(3.619)	(3.987)	(40.514)	(2.593)	(2.293)	(23.428)
Effect at 25th pct of stimulus	0.365**	0.169	0.665*	0.161*	0.051	0.316*
	(0.081)	(0.120)	(0.251)	(0.065)	(0.062)	(0.146)
Effect at 75th pct of stimulus	0.354**	0.164	0.638**	0.154*	0.050	0.304*
	(0.079)	(0.117)	(0.213)	(0.063)	(0.061)	(0.124)
Four quarters' cumulative lags						
Linear term	0.402**	0.146	0.692*	0.197*	0.027	0.292
	(0.138)	(0.152)	(0.306)	(0.079)	(0.084)	(0.199)
Quadratic term	−11.094+	−3.381	−16.850	−7.408*	−1.195	−5.959
	(5.794)	(6.227)	(56.646)	(3.610)	(3.135)	(34.146)
Effect at 25th pct of stimulus	0.401**	0.145	0.691*	0.196*	0.027	0.291
	(0.138)	(0.152)	(0.302)	(0.078)	(0.084)	(0.196)
Effect at 75th pct of stimulus	0.388**	0.142	0.672*	0.188*	0.026	0.285+
	(0.134)	(0.147)	(0.251)	(0.075)	(0.082)	(0.165)
Six quarters' cumulative lags						
Linear term	0.701**	0.351+	1.212*	0.363**	0.155	0.579*
	(0.169)	(0.195)	(0.464)	(0.106)	(0.097)	(0.237)
Quadratic term	−18.746*	−7.698	−49.228	−12.803*	−4.378	−20.406
	(7.051)	(7.401)	(76.284)	(5.068)	(4.208)	(44.388)
Effect at 25th pct of stimulus	0.700**	0.350+	1.208*	0.362**	0.155	0.577*
	(0.168)	(0.195)	(0.458)	(0.105)	(0.097)	(0.234)
Effect at 75th pct of stimulus	0.679**	0.342+	1.153**	0.347**	0.150	0.554**
	(0.164)	(0.190)	(0.392)	(0.101)	(0.094)	(0.194)
Eight quarters' cumulative lags						
Linear term	0.712**	0.249	1.584**	0.343**	0.114	0.502+
	(0.243)	(0.266)	(0.551)	(0.126)	(0.127)	(0.271)
Quadratic term	−20.450*	−5.945	−71.469	−13.457*	−4.264	−14.036
	(9.808)	(10.596)	(88.433)	(5.926)	(5.369)	(51.624)
Effect at 25th pct of stimulus	0.710**	0.249	1.578**	0.341**	0.113	0.500+
	(0.243)	(0.265)	(0.545)	(0.126)	(0.126)	(0.267)
Effect at 75th pct of stimulus	0.688**	0.242	1.498**	0.326**	0.109	0.485*
	(0.236)	(0.258)	(0.477)	(0.121)	(0.123)	(0.226)

Please see the notes on Table IV.2 for details on sample construction and measurement. This table varies from Table IV.2 in that for each level of time aggregation, the table reports the summed coefficients on both linear and quadratic terms in stimulus expenditure. It also differs in that it does not include leads on stimulus. In addition to coefficients, this table also reports the effect of stimulus at the actual 25th and 75th percentiles of the county stimulus distribution. Regressions control for Bartik predicted employment to population ratio, Bartik predicted wage bill, demographic controls, state-by-time fixed effects and county fixed effects.

Table IV.5: Effects of Stimulus on Employment and Wage Bill by Industrial Sector, Sample Split by Excess Capacity

	Employment per capita			Wage bill per capita		
	All counties	Low excess	High excess	All counties	Low excess	High excess
Overall	0.424*	0.148	0.981*	0.172+	0.048	0.406+
	(0.181)	(0.188)	(0.412)	(0.088)	(0.102)	(0.235)
Public sector	0.182	0.224	0.136+	0.053+	0.072+	0.018
	(0.118)	(0.159)	(0.074)	(0.031)	(0.039)	(0.027)
Private sector	0.222	−0.098	0.792+	0.059	−0.109	0.341
	(0.176)	(0.207)	(0.409)	(0.092)	(0.122)	(0.238)
Tradables	0.073	−0.003	0.284+	0.011	−0.047	0.142
	(0.094)	(0.104)	(0.160)	(0.049)	(0.052)	(0.119)
Non-tradables	0.199	−0.050	0.419	0.081	−0.047	0.122
	(0.140)	(0.171)	(0.341)	(0.056)	(0.085)	(0.167)
Services	0.118	−0.095	0.250	0.043	−0.073	0.056
	(0.105)	(0.126)	(0.313)	(0.050)	(0.070)	(0.175)
Goods	0.097	0.049	0.248+	0.031	−0.004	0.131
	(0.087)	(0.094)	(0.140)	(0.045)	(0.044)	(0.102)
Construction	0.115+	0.010	0.255+	0.016	−0.048	0.126+
	(0.065)	(0.067)	(0.127)	(0.042)	(0.056)	(0.069)

Estimates are of own-county employment and wage bill on own-county stimulus expenditures. Stimulus expenditures and the wage bill are measured in $100,000 per person. County population is the number of residents aged 15-64. The sample is from 2006Q1 to 2016Q3. Employment and wage bill data come from the QCEW. Timing of stimulus expenditures is adjusted from stimulus recipient reports from www.recovery.gov. The outcome variable is employment per capita in the three columns on the left and wage bill per capita in the three columns on the right. Each estimate is the sum of a contemporaneous effect and eight quarterly lags of stimulus. The full specification includes 8 quarterly leads of stimulus which are not reported. Regressions control for Bartik predicted employment to population ratio, Bartik predicted wage bill, demographic controls, state-by-time fixed effects and county fixed effects. Bartik predictions are based upon county-level employment and wage bill averages over 2006-2007 at the three-digit NAICS level. Demographic controls are Census 2000 estimates of percents black, hispanic, urban, and under poverty, as well as median income and 2006 average home purchase loans and 2006 total HMDA loans per capita. All demographic controls are interacted with a time trend. Low excess capacity is below and high excess capacity is above the 50th percentile of county excess capacity. Industries come from NAICS classifications in the QCEW. Standard errors are clustered at the state level.

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