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Aaron J. Sojourner
Robert J. Town
David C. Grabowski
Michelle M. Chen

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Impacts of Unionization on Employment, Product Quality and Productivity: Regression Discontinuity Evidence From Nursing Homes

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ABSTRACT

This paper studies the effects of unions in private-sector nursing homes on a broad range of labor, firm, and consumer outcomes. We link national data on nursing home characteristics from the Centers for Medicare and Medicaid Services to records on establishment-level unionization from federal labor agencies, and employ a regression discontinuity design to identify union effects by contrasting outcomes in nursing homes where unions closely won representation elections to outcomes in facilities where unions closely lost such elections. After showing that these two sets of homes are similar leading up to the election, we estimate union effects on staffing levels, care quality, and other outcomes. We find negative effects of unions on staffing levels and no decline in care quality, suggesting positive productivity effects. Consistent with these results, supplementary analysis shows significant increases in wages for some classes of nursing labor. Some evidence suggests that nursing homes in local product markets that were less competitive and had lower union density at the time of election experienced stronger union employment effects. We find no impact of unionization on facility survival. By combining credible identification of union effects, a comprehensive set of outcomes over time with measures of market-level characteristics, this study generates some of the best evidence available on many controversial questions in the economics of unions. Furthermore, it generates evidence from the service sector, which has grown in importance and where evidence on these questions has been thin.

Aaron J. Sojourner
University of Minnesota
Carlson School of Management
321 19th Ave S, 3-300
Minneapolis, MN 55455
asojourn@umn.edu

David C. Grabowski
Harvard University
Department of Health Care Policy
Harvard Medical School
180 Longwood Avenue
Boston, MA 02115
grabowski@med.harvard.edu

Robert J. Town
Health Care Management Department
The Wharton School
University of Pennsylvania
3641 Locust Walk
Philadelphia, PA 19104
and NBER
rtown@wharton.upenn.edu

Michelle M. Chen
Department of Finance and Real Estate
The College of Business Administration
Florida International University
11200 S.W. 8th Street
Miami, Florida 33199
min.chen2@fiu.edu

1 Introduction

Understanding the impact of unionization is a long-standing, controversial, and difficult question in labor economics. Specifically, the literature has long sought to understand the effect of the unionization of an organization’s workers on its wages, employment, output, productivity, and survival. A central challenge in estimating the effect of unions is to identify variation in unionism that is credibly unrelated to firm and employee unobservables. Traditionally, economists used control functions or individual fixed-effects designs to identify the impact of unions on wages [Mellow, 1981]. Identification of effects on employment, output, and profits conventionally came from comparisons across firms in different kinds of markets, though the endogeneity of unionization is difficult to address in these designs [Lewis, 1963; Freeman and Medoff, 1984; Hirsch and Addison, 1986; Hirsch, 2004].

Recent work has relied on a more credible source of variation to identify union effects. DiNardo and Lee [2004] introduced the use of regression discontinuity (RD) design to identify and estimate union effects. National Labor Relations Board (NLRB) rules generate a discontinuity in the probability of unionization as a function of vote share in union certification elections. RD analysis exploits this by comparing outcomes among firms where unions barely won elections to outcomes in firms where unions barely lost. If firms on either side of the threshold are similar prior to the election, systematic differences observed in subsequent years can be credibly understood as effects of unionization.

The current paper applies RD analysis to estimate union effects on a variety of outcomes focusing on a single industry: nursing homes. Building off data assembled for Holmes [2006], we linked records from two federal labor agencies — the NLRB and the Federal Mediation and Conciliation Service (FMCS) between 1978 and 2002 — to health systems data collected by the federal Centers for Medicare and Medicaid Services (CMS) from 1993 to 2008. CMS’s Online Survey, Certification, and Reporting (OSCAR) system provides establishment-level data over time on each nursing home in the U.S. that cares for Medicaid- or Medicare-financed residents. It includes 96% of all U.S. nursing home establishments. The OSCAR

data set contains information on facility characteristics and employment by type of worker. Importantly, OSCAR contains detailed data on quality of care. Unfortunately, the OSCAR data do not contain wage information, so we supplement our analysis with data from the Current Population Survey (CPS). Combining analysis on the effect of unions on employment, output, and quality, this study offers some of the best evidence available on union productivity effects and some of the only evidence from the service sector. To the best of our knowledge, this is the first study of the impact of unionization on product quality in which quality is reliably measured and unionization effects are plausibly and cleanly identified.

There are a number of advantages to focusing empirical analysis on the nursing home industry. First, the activities of nursing home workers and technologies of production are essentially similar across organizations. Thus, we remove an important dimension of organizational heterogeneity that may confound study of an industry that encompasses more heterogeneity or of cross-industry analyses.

Second, we get a deep view into firms, permitting greater insight than a study focused on a few isolated outcomes. Due to the large public role in the finance and regulation of nursing homes, rich detailed establishment-level outcome data exist across a broad range of labor, firm and consumer outcomes. Data are available for almost the entire industry nationally and in a panel across many years. This rich panel also permits powerful falsification tests. We find no evidence of discontinuities across the RD threshold in *pre*-election characteristics of nursing homes, suggesting that the RD identification strategy is valid in this setting.

Third, unions might be expected to have larger effects on firms and on consumers in nursing homes than in many other industries, which should throw any productivity effects into sharp relief. Here, labor is central to the production process. It makes up two-thirds of nursing home costs [Gertler and Waldman, 1992] and is the key input into the quality of patient care [Wunderlich et al., 1996]. Furthermore, the need to provide nursing home care that is proximate to residents' hometowns or families limits the possibility of outsourcing [Helpman and Krugman, 1987] and reduces the elasticity of consumer and labor demand.

Our analysis thus focuses on the impact of unionization in a setting with potentially large effects.

Fourth, recognizing this potential, unions have made nursing homes a strategic organizing priority for more than 20 years [Sojourner et al., 2011]. This bears on both the internal and external validity of the current study. Regarding internal validity, a study of nursing homes offers considerable statistical power compared to what would be obtained in any other single industry. Several hundred unionization elections are available for the regression discontinuity analysis, even after applying filtering criteria designed to minimize bias.

Regarding external validity, our ability to measure union impacts on quality and productivity in nursing homes contributes new insight into an increasingly important sector of the economy. Prior research on union productivity effects has focused almost exclusively on manufacturing, mining or construction. However, health care in particular and the service sector in general are essential settings for understanding unions going forward. For instance, unlike in years past, more Americans and more union members now work in health care and social services than in manufacturing.¹

Fifth, the unusually rich data permit exploration of theoretical predictions about which economic contexts will generate stronger union effects [Hirsch and Addison, 1986; Stewart, 1990; Booth, 1995]. By combining credible identification of union effects with measurement of market characteristics, this paper offers some of the best available evidence on these issues. For each home experiencing an election, we construct measures of the local market in which it operates. We examine how union effects vary depending on firms' power in the local product market. Where economic rents are greater and consumer demand is less elastic, theory unambiguously predicts larger union effects and this is what we find. The empirical evidence available previously has been mixed and plagued by confounding factors.

¹According to the Bureau of Economic Analysis, the number of Americans employed in manufacturing fell by about 40 percent since 1970, from 18 million to 11 million, while the number in health care and social assistance grew by 533%, from 3 million to 16 million. In terms of the incidence of unionization, the number of union members in manufacturing fell 76 percent between 1983 and 2010, from 5.8 to 1.4 million. Meanwhile, the number of union members in health care and social assistance rose by 133 percent, from 1.2 to 1.6 million over the same period [Hirsch and Macpherson, 2011].

Further, by constructing the first national dataset with establishment-level measures of nursing home unionization status, we can measure union share in each market and test whether union effects differ in more highly-unionized versus less-unionized markets, something that has been done before only with weaker designs. We also examine how unionization affects the employment of workers by skill level and how this interacts with the strength of state regulation of nurse staffing levels.

Sixth, this analysis has significant policy relevance due to the ongoing policy debates over health care and labor law reform. Through the Employee Free Choice Act and proposed administrative rule changes [Greenhouse, 2011], federal labor policy debates have recently heated up and focused on reforming the process by which the NLRB certifies unions to represent new bargaining units. The effect of this certification is precisely what the regression discontinuity design measures.

Theory is inconclusive regarding the impact of unionization on many outcomes of interest thus heightening the importance of empirical analysis. For example, the relationship between unionization and employment levels within a firm are theoretically ambiguous [Pencavel, 1991]. If unions raise the price of labor, firms may purchase less and employment may fall. This is a central prediction of conventional views of unions and of right-to-manage models of bargaining over wages. However, if bargaining occurs over both compensation and employment levels, then unions can bind firms to efficient contracts that raise labor compensation without reducing employment [McDonald and Solow, 1981]. Also, in monopsonistic labor markets, unions may also raise wages without decreasing employment [Link and Landon, 1975; Manning, 2003]. Theory and empirical evidence, however, suggest that unions have a negative effect on firm profits [Clark, 1984; Booth, 1995; Lee and Mas, 2009]. The impact of the reduced profitability on long-run firm investment, innovation and survival is still unclear. Freeman and Kleiner [1999] find evidence that unions extract enough profits to slow firms' growth but not so much as to threaten firm survival. We can study effects on growth and survival directly.

Unions may also affect labor productivity and again theory offers potentially countervailing factors but provides little guidance on the overall direction of effects. Unions advocate for some changes that, *ceteris paribus*, reduce productivity, such as rigid job classifications and work rules. However, by helping workers' bargain for public goods and by promoting labor-management cooperation, unions may enhance productivity [Duncan and Stafford, 1980; Freeman and Medoff, 1984]. These factors affect productivity holding the distribution of labor and capital fixed. Unions can also affect the distribution of labor and capital in ways that impact productivity indirectly. To the extent that unions raise wages and compensation, unionized firms have a greater incentive to employ workers who are more productive. Given higher marginal labor costs, firms will want to employ more productive labor. Therefore, union firms may have more productive workers due to personnel selection policies [Pettengill, 1979; Reynolds, 1986; Card, 1996; Hirsch and Schumacher, 1998b] and to larger investment in workers' firm-specific human capital and in liquid capital goods that complement labor [Acemoglu and Pischke, 1999; Dustmann and Schönberg, 2009]. We generate evidence that the overall direction of change is positive and that this is not due to capital-deepening, but cannot distinguish the other possible underlying mechanisms.

Studies employing RD designs led to somewhat different qualitative findings relative to the previous literature.² DiNardo and Lee [2004] study a narrow set of outcomes in the broad U.S. private-sector economy and wider set of outcomes in manufacturing plants. They find that unions have little impact on average wages, employment and productivity as measured by output per worker. Using an RD design to identify the effect of certification on expected profits at publicly-traded firms, Lee and Mas [2009] find a near zero effect at the vote share threshold, though they do find substantial negative effects on average using event-study methods when including firms farther from the threshold. Frandsen [2010] also uses RD to examine the impact of unions on the distribution of wages and finds that unions compress the wage distribution without much affecting average wage.

²This may be due to difference in design or in the estimand, which weighs firms on the margin of unionization more heavily than the average firm [Lee and Lemieux, 2010].

Our estimates of the impact of unionization on nurses and nursing homes are meaningfully different from Dinardo and Lee's estimates from the manufacturing sector. We find that unionization of a nursing home facility leads to significant declines in the mean number of nursing hours per resident day. Specifically, unionization is associated with large decreases in nurse aide (NA) and registered nurse (RN) hours per resident day. These declines in staffing suggest that unionization leads to significant increases in wages for NA and RNs, which is confirmed by analysis of the CPS data. In an important and surprising result, the decline in staffing is not associated with changes in care quality. That is, unionization appears to increase labor productivity measured by both output per nursing hour and quality of care per nursing hour. There is also no evidence of a significant impact on establishment size, occupancy rates, resident case mix, or facility survival.

We also explore whether unions have heterogeneous impacts along three dimensions. First, we find larger effects in more concentrated markets, as predicted by theories of rent-sharing. Second, we find larger effects in less organized markets, consistent with theories emphasizing threat effects and union substitution strategies by nonunion management in more organized markets. Finally, effects vary by the strength of state regulation of high-skill nurse staffing in complex ways consistent with basic theory. In less regulated states, unionization induces homes to reduce staffing levels across all skill levels. In states with tighter restrictions on the ability of homes to reduce higher-skill staffing levels, adjustments are concentrated on the margin of adjusting levels of less-skilled staff.

The remainder of the paper is organized as follows. The next section describes the institutional background of unionization and nursing homes and our data. Section 3 describes our empirical framework, based largely on Lee and Lemieux [2010]. Section 4 focuses on evidence on the validity of our identification strategy. Section 5 presents our principal results. Section 6 concludes.

2 Institutional Setting and Data

Institutional Setting. The nursing home sector is large and growing. In 2009, nursing homes expenditures were approximately \$187B, and the sector employed more than 1.8 million people. Nursing homes provide long-term, custodial and post-inpatient recuperative and rehabilitation services for patients who suffer from significant disabilities that require 24-hour monitoring and care. The combination of an aging population and increasing life expectancy points to increasing demand for long-term nursing home care over the coming decades. The Bureau of Labor Statistics forecasts that employment in this sector will grow by 24 percent over the next decade. There are over 16,000 nursing homes operating in the US with 1.7 million beds which care for 1.4 million patients. The typical nursing home bed is occupied – the average occupancy rate is 82 percent. The dominant payer for nursing home patients is Medicaid with a little over 60 percent share of the patient population. The remaining 40 percent of patients are roughly split between Medicare and private pay/privately insured patients.³ For-profit, not-for-profit and government owned firms all provide nursing home care with for-profit firms accounting for the majority (approximately two-thirds) of the facilities.

Nursing home care is labor intensive and the activities that nursing home employees perform (e.g. assist patients with bathing, toileting, feeding, medication management and moving in and out of bed) are essentially the same across all facilities. Most direct patient care in nursing homes is provided by certified nursing assistants (NAs) who have limited professional training. Nursing homes also employ registered nurses (RNs) and licensed practical nurses (LPNs). RNs typically have two to four years of education at a college, university, or hospital. LPNs have nine months to one year of education, typically in a community college. RNs can provide direct patient care and often oversee LPN and NA staff. These different types of labor are imperfect substitutes for one another. In our data, nursing homes employ, on average, 2.13 NA, 0.77 LPN and 0.59 RN hours per resident day.

³Medicare covers post-acute care services for 100 days after a qualified inpatient stay at a hospital.

Unions have long focused on nursing homes as a ripe pool for their organizing efforts. In 1983, Services Employees International Union (SEIU) President John Sweeney initiated a campaign to organize hundreds of chain-affiliated nursing homes. More recently, Andy Stern (who succeeded Sweeney as SEIU President), vowed to increase organizing efforts directed towards nursing homes. This “Dignity, Rights and Respect” campaign aimed to organize 100 facilities a year. At approximately the same time, the United Food and Commercial Workers International (UFCW) began targeting southern nursing facilities. Dozens of other unions also attempted to organize long-term care facilities during these years. Despite this, unions have not kept pace with the industry’s rapid growth and the unionized share of workers has declined, though at a slower rate than in many other sectors [Sojourner et al., 2011].

Data Sources. Our principal analysis dataset is the first national panel on nursing home characteristics that also includes information on labor relations at each home, including data on union elections, collective bargaining, and unionization status among each home’s employees. Our sample includes all federally-financed, privately-owned nursing homes existing in years between 1991 and 2001. For this population, we have information on labor relations from the late 1970s to the early 2000s and outcome data for 1993 to 2008. We link information from several sources. We began with data used in Holmes [2006] that linked nursing home records from three sources:

- (A) NLRB union certification and decertification elections held between 1976 and 2002
- (B) FMCS intent-to-bargain notices filed between 1983 and 2003
- (C) CMS provider identifiers for nursing homes in the On-line Survey, Certification, and Reporting (OSCAR) system for the years 1991, 1996, or 2001

Using sources (A) and (B), we learn about homes’ paths through labor relations processes. From (A), we observe when union elections occurred, what unions were involved and the election results. These measures enable the regression discontinuity estimation of the effect of NLRB certification. This is conceptually somewhat different from the effect of a union

but it is more closely tied to the data-generating process discussed in the next section. Each relevant NLRB and FMCS record is associated with a home i and at date t . From (B), we can infer the presence of a union contract. This is useful because many newly-certified bargaining units fail to reach initial contract agreements and the union may fade before establishing a toehold within the establishment. We use the FMCS data in a falsification test and observe that certification really does lead to differential increases in unionization, similar to DiNardo and Lee [2004].

Using the matched CMS identifiers from (C), we link an additional CMS data source that provides rich information on nursing home characteristics, the complete OSCAR panel data of facility characteristics from 1993 to 2008. The OSCAR system provides rich establishment-level data from all Medicaid- and Medicare-certified nursing home facilities in the United States (96 percent of all facilities). The OSCAR data include information about nursing homes' compliance with federal regulatory requirements. Following an initial survey, states survey each facility about every 12 months on average and no less often than every 15 months. Following the survey, nursing homes submit facility, resident, and staffing information which are captured in the OSCAR data.

Variables. The share of votes in favor of the union is the forcing variable in the RD analysis. In constructing this variable, care is required to deal with features of the institutional process and their statistical implications. First, some elections involve multiple unions running simultaneously against each other and the no-union option. For any one union to win, it has to receive support from a majority of voters. Therefore, we use the share of total votes received by the union with the highest vote share as the raw share. Second, the fact that the support of the raw vote share depends mechanically on the number of voters can create problems in the context of regression discontinuity analysis. For instance, only elections with an even number of voters can achieve exactly a 50 percent share. To deal with these issues, we follow DiNardo and Lee [2004] in adjusting the raw shares, binning the resulting shares, and using the mid-point value of an election's bin as the vote share measure.

We analyze the impact of union certification on a series of employment, care quality, and other establishment outcomes. Table 1 provides summary statistics. All of these measures have been used extensively in previous economic analyses of the nursing home industry [Cawley et al., 2006; Grabowski et al., 2008; Lu, forthcoming]. In terms of employment outcomes, we examine certified nurse aide hours per resident day (HPRD), licensed practical nurse HPRD, and registered nurse HPRD. All three of these staff types are reported on the federal “Nursing Home Compare” report card website.⁴ Nursing home consumers and their support persons are encouraged to use this information, along with other quality measures provided on the web site, to help select a nursing home.

We focus on the three primary measures of care quality. Two are based on results from the comprehensive mandated government inspections reported in OSCAR: the total number of deficiencies found and an indicator that a severe health deficiency was found. The third is market-based: the percentage of the home’s residents who pay for their care with their own private funds rather than through Medicaid or Medicare. These will be described in greater detail when results are presented.

We analyze other strategic operating margins that unionization might affect and which could be potential confounds in understanding union effects on productivity. These include each home’s total number of beds (scale of production and firm growth), percentage of beds occupied (labor-capital ratio), and acuity of residents’ health conditions. Finally, we study the effect on establishment survival, to see if differential attrition from the sample could drive the results.

Patients generally receive nursing home care near their primary residence or the primary residence of their adult children. This preference implies that markets for nursing home care are geographically local, and therefore there are hundreds of nursing home markets in the US with meaningful variation in the structure of the product market. Theory suggests that the impact of unionization will be largest in less concentrated markets [Abowd and Farber, 1990].

⁴<http://Medicare.gov/nhcompare>

We measure market competitiveness with an Herfindahl-Hirschman Index (HHI) based on nursing homes' shares of total beds in its county. Virtually the whole range of possible HHI values is observed in the data. Basing HHI on a home-specific, 25-mile radius market does not change results.

Theory also predicts that the impact of the unionization of a given establishment may depend upon penetration of unions into the relevant labor market [Booth, 1995; Hirsch and Addison, 1986], though the relationship is complex and the empirical implications ambiguous. We can explore this empirically as we have variation across markets in nursing home union density and can estimate unionization effects as a function of geographic market union penetration. Neither the CMS data nor any other prior national data set distinguishes union from nonunion nursing homes. So, we construct measures of unionization status for each nursing home facility at each point in time from the NLRB and FMCS data [Sojourner et al., 2011]. Union market share is measured as the percentage of beds in the same county that are in unionized homes.

Over the last several decades most states have instituted some form of nurse staffing ratio regulation with significant variation across the states in both the timing of implementation and requirements of the regulations. Most of these regulations specify RN and LPN ratios with significant variation across the states in both the amount of RN/LPN supervision and the direct care staffing requirements [Harrington, 2008]. In so far as these regulations are binding, they may limit the ability of nursing homes to adjust to wage increases by focusing adjustments on the NA staff. For this reason, we examine the role of these state regulations in mitigating the impact of unionization.

Additionally, the length of time a union has had to operate in a nursing home may affect its ability to have effects. Our data allows us to stratify the sample by length of time since certification in order to explore this possibility.

Sample Selection Criteria. The NLRB and FMCS data cover only private (i.e., non-government) nursing homes. Thus, we exclude government-owned facilities (about 8 percent

of total) from the establishment-level analysis. Our resulting sample consists of 22,357 unique licensed facilities; 14,556 were in operation in 1992, and 15,638 facilities in 2002. In our data, 2,088 facilities had at least one certification election between 1978 and 2001. Of these, 1,375 (66 percent) homes had at least one election where a union won. In the other 713 facilities with elections, the elections went against the union.

In the regression discontinuity analysis, we focus on NLRB certification elections that meet the following criteria:

1. *At least 20 individuals voted.* This minimizes the risk that the exact outcome could be perfectly controlled by the company, the union, or workers. This would undermine the quasi-randomization across the vote-share threshold [DiNardo and Lee, 2004; Lee and Lemieux, 2010].⁵
2. *Occurred after at least one inspection report is observed in our OSCAR data for that home.* The OSCAR data start in 1993. The last NLRB election in our data is from 2002. Therefore, all elections we consider occur in 1993-2002. The requirement of at least one pre-election OSCAR observation ensures our ability to use pre-election home characteristics for two important purposes [Lee and Lemieux, 2010]. First, we can test for pre-election discontinuities in this rich set of baseline observable home characteristics. If such discontinuities were evident, this would cast doubt on the validity of the identifying assumptions. As these are the post-election outcomes of interest, it is very useful to see whether there were discontinuities prior to the election. Second, we can include baseline pre-election characteristics as explanatory variables in the analysis of union effects. These are not included to control for selection, but to reduce the variance unexplained influences and increase power, as in analysis of experimental treatment effects.

3. *First such election observed in a home.* Considering multiple elections for the same

⁵Bajari et al. [2011] develop an approach appropriate when the forcing variable is a choice variable.

home raises a number of conceptual issues. Suppose a union got 49 percent of the vote in a 1996 election and 51 percent in a subsequent 1997 election. For an outcome measured in 1998, is this two years away from a 49 percent election or one year away from a 51 percent election or both? To test for discontinuity in baseline characteristics and for inclusion of baseline characteristics as conditioning variables in estimation it is unclear which year’s OSCAR record to use or what dependencies to allow. Focusing on only the first post-OSCAR election in each home sidesteps these thorny issues. We terms this the home’s *focal election*.

Another issue with multiple elections is the possibility that unions or management learn enough through recent elections to manipulate outcome of the election in such a way as to introduce systematic differences across the threshold in unobservables and, thereby, to invalidate the identifying assumption. This concern diminishes as the time between elections extends. We exclude homes that experienced an NLRB election in the five years immediately prior to the focal election in our data.

4. *In a home without evidence of unionized employees.* We do this to clarify interpretation of the “treatment” as a contrast between homes with no unions certified as bargaining agent and any union so certified. We exclude elections in homes that had previously filed notice of intent to bargain with the FMCS, which imply the presence of a union. Because the FMCS records extend back to the late 1970s and we consider elections in the 1993-2002 period, this gives us at least 15 years of history to examine.⁶

5. *Nursing home that was not publicly owned.* Publicly-owned homes have their labor relations regulated through state agencies. Data from the federal NLRB and FMCS

⁶We may mistakenly include some long-time union homes in our sample. Homes that: (1) unionized prior to the start of our NLRB data (late 1970s), (2) whose union and management both consistently failed to comply with the FMCS requirement to file a notice of intent to bargain at each contract expiry, (3) had no NLRB election between the start of our NLRB data and the start of the OSCAR data, and (4) had an NLRB election after its first OSCAR observation. This does not undermine the validity of the design; it only moves the treatment a little closer to Dinardo and Lee’s definition. Because they analyze all elections in any firm, the treatment is newly-certifying an agent to represent an additional bargaining unit in a home that may or may not already contain unionized workers.

do not cover these homes so we would not have accurate measures of the home’s unionization status. This makes our estimates relevant to private-sector nursing homes, including both for-profit and not-for-profit, but silent on their effects in public-sector homes.

Applying criteria 1, 3, 4, and 5 yields a sample of 1,846 elections. Applying criteria 2, restricting the sample to the end of the period, cuts this to 627 elections. This is the analytic dataset we use to estimate the parameters.

3 Identification

The empirical model we take to the data is essentially identical to the DiNardo and Lee [2004] set up, although some differences in data structure require modest adjustments.

Population and timing. We use an unbalanced establishment-level panel. Nursing home facilities are indexed by $i = 1, 2, \dots, I$. We focus on facilities that experience a union certification election on date \tilde{t}_i . Home characteristics may be observed on any date in calendar time \tilde{t} .

Variables. For each home-date, we observe L characteristics of the facility denoted $W_{it} \in \mathbf{R}^L$. For the regression discontinuity analysis, observation dates are measured as the time elapsed between an observation calendar date and the date of the home’s election. The normalized, elapsed time date of observation is defined as $t \equiv \tilde{t} - \tilde{t}_i$. We examine several different outcomes (e.g. employment, quality of care) which we collectively denote Y_{it} .

The threshold-centered union vote share in the election for home i is given by $X_i \in [-0.5, 0.5]$. Union certification is indicated by $D_i \in \{0, 1\}$.

Model. Adapting from Lee and Lemieux [2010], the empirical model is given by the three equation system:

$$Y_{it} = D_i\tau + W_{it}\delta_1 + U_{it} \quad (1)$$

$$D_i = 1[X_i > 0] \quad (2)$$

$$X_i = W_{i0}\delta_2 + V_i \quad (3)$$

Equation 3 specifies that the forcing variable, the observed threshold-centered pro-union vote share (X_i), depends on observed home covariates at the time of the election (W_{i0}) and unobserved influences (V_i). Equation 2 says that, in homes where vote share exceeds the threshold, a union is certified as the collective bargaining agent of workers. In other homes, no union is so certified.

Equation 1 defines the outcome vector. It depends on observable characteristics of the establishment and its market (W_{it}), which can include lagged values of (Y, W) . For post-election observations ($t > 0$), Y_{it} may depend on whether or not a union was certified (D_i). In this way, τ measures union certification effects. Y_{it} also depends on unobserved influences U_{it} .

Identification of τ requires that the mean of unobservable influences on outcomes U_{it} does not shift discontinuously across the threshold. For any t ,

$$\lim_{x \uparrow 0} E(U_t | W, X = x) = \lim_{x \downarrow 0} E(U_t | W, X = x) \quad (4)$$

The validity of this assumption rests on the likelihood that, prior to the election, no systematic difference exists between homes where unions just lose the election ($x \uparrow 0$) and homes where unions just win the election ($x \downarrow 0$). Conditional on having a union election and on the election outcome being close, which side wins is determined by idiosyncratic factors — factors that do not influence later outcomes except through the fact that they affect union certification. Within a point on either side of the threshold, this assumptions almost surely holds. However, as a practical matter, obtaining precise estimates requires using information

from homes with vote shares farther from the threshold, which could introduce bias. For this reason, we provide evidence on the validity and sensitivity of the identification assumption in the Results section.

The probability of union certification is thus discontinuous in X_i . Homes with very small differences in pre-election characteristics (W_{i0}, V_i) but with X_i on opposite sides of the threshold will have different post-election experiences. Above the threshold, $D_i = 1$, implying that a union is certified as the collective bargaining agent for at least a subset of workers in the home. The union and management then have a legal duty to bargain towards a first contract. Below the threshold, no such certification or duty is created. Therefore, observed post-election differences in outcomes between homes on either side of the threshold are interpreted as union (certification) effects.

4 Summary Statistics and Evidence of RD Validity

The top panel of Table 1 presents summary statistics for the distribution of vote shares and outcomes in focal elections. On average, unions won elections by 2.9 percentage points and unions won a majority in 55.8 percent of the elections.⁷ Figure 1 presents the fraction of elections in each vote share bin for the 627 focal elections. The distribution peaks in the first bin above the cut-off.

Test for pre-election discontinuities. The RD design is premised on the assumption that no systematic differences exist in the populations of homes across the threshold before the election. We test this with respect to characteristics observable up to the date of the election. We focus on homes with elections occurring after at least one OSCAR observation is available precisely in order to enable this kind of falsification test. In formal terms, we want to test the joint hypothesis that $\tau_k = 0$ for all k for $t \leq 0$. To facilitate joint hypothesis testing while allowing for possible correlation in errors within home across characteristics (dimensions of

⁷This variable is not raw vote share. It is modified to account for the fact that the support of the raw vote share variable changes mechanically with the number of votes cast [DiNardo and Lee, 2004]. Further, the shares are normalized so that a 50 percent vote share has the value 0.

Y), we use system (or stacked) OLS [Lee and Lemieux, 2010].⁸ Each observed Y_{itk} for $t \leq 0$ is included as an outcome, the 4 parameters for a linear vote share function are interacted with k indicators so each characteristic is allowed to have a separate vote share function with $4K$ parameters.

$$\begin{aligned}
 Y_{it1} &= \beta_{01} + \beta_{11}X_i + \tau_1D_i + \beta_{21}X_iD_i + \epsilon_{it1} \\
 Y_{it2} &= \beta_{02} + \beta_{12}X_i + \tau_2D_i + \beta_{22}X_iD_i + \epsilon_{it2} \\
 \dots &= \dots \\
 Y_{itK} &= \beta_{0K} + \beta_{1K}X_i + \tau_KD_i + \beta_{2K}X_iD_i + \epsilon_{itK}
 \end{aligned} \tag{5}$$

We use only observations from homes that have elections with vote shares close to the threshold, where close is defined as within a given bandwidth (h) of the threshold. Discontinuity estimates for each characteristic ($\hat{\tau}_k$) from this analysis are presented in Table 2. In addition to the nursing home characteristics from OSCAR that are the focus of our analysis, we also include two characteristics of the focal NLRB election in order to provide an even stronger test: the logs of bargaining unit size and number of valid votes cast. Columns correspond to different values of h with the final row presents the p -value from the joint hypothesis test. At each bandwidth, the joint hypothesis is not rejected. There is no evidence of systematic differences across the threshold in nursing home characteristics prior to the election.⁹

Certification and unionization. The data speak directly to the effect of NLRB certification. If certification is the definition of the treatment, then we have a sharp discontinuity design because there is basically a one-to-one mapping between which side of the threshold one falls on and whether the NLRB certifies a union as bargaining agent for the workers

⁸When we shift to estimating post-election effects in the next section, we present estimates based on nonparametric methods. There, joint hypothesis testing is relatively less important and obtaining unbiased estimates more important. Estimated effects based on parametric functional forms will also be presented in the appendix.

⁹Allowing errors to be correlated within home and measure ik across time t rather than within i across kt gives similar results.

in the bargaining unit. Subsequent elections and any unobserved prior elections muddy the waters somewhat, but this interpretation still remains relatively clean. However, certification is not equivalent to unionization.¹⁰

There is evidence that certification raises the probability of unionization. FMCS notices give reliable measures that a union is present, although absence of such notices is not a reliable indicator of union absence. In all private firms, the union and company have a duty to file notices of intent to bargain at least 30 days prior to contract expiry. These are filed only if a union contract is in place.¹¹ In the health care industry uniquely, firms are also required to file notices of intent to bargain for first contracts. These notices are filed subsequent to a union election victory but prior to signing an agreement. In some cases, the parties fail to reach agreement on an initial contract, the union dissipates, and the firm may remain nonunion. To try to separate these notices and isolate those pertaining to existing unions only, we study an indicator of having an FMCS notice filed more than a year post-election.

For each home, we consider whether it had any FMCS notice of intent to bargain filed subsequent to the focal election. Figure 2 presents evidence that homes' likelihood of filing FMCS notices of intent-to-bargain is discontinuous at the threshold. Because non-filing could be due to either failure to reach first contract or filing noncompliance, the post-one-year filing rate is a lower bound on the discontinuity in unionization generated by crossing the certification threshold. Over 40 percent of homes where unions won the focal election

¹⁰Slippage can occur in both directions. On one hand, achieving NLRB certification is no guarantee that a union will take root at a firm. Though the parties have a duty to bargain, they are not legally obligated to reach an agreement and in a sizable minority of cases, they do not [Bronfenbrenner, 2003]. In these cases, support for unionization may wither and the home remain nonunion. This would work against finding a union effect and is analogous to noncompliance with an assignment to treatment. On the other hand, especially in recent years, many unions organize outside the NLRB election process. These homes are not directly relevant to the RD analysis carried out here but are indirectly relevant in two ways. First, if an FMCS notice is subsequently filed in a home that organizes outside the NLRB process, the home is counted as union for the purpose of measuring the market's union density for nearby homes that later experience elections. Secondly, if unions form outside the NLRB process in homes where unions previously lost NLRB elections, this would also diminish the chance of finding a certification effect as it is analogous to noncompliance with an assignment to control.

¹¹Though it may not always be filed when contracts are in place as compliance is not perfect. Further our data may sometimes miss a match even when filed.

had such filings while less than 20 percent of those where unions lost focal election did, presumably where unions formed through subsequent campaigns. If FMCS notices filed in the first post-election year are included, the discontinuity is substantially larger.

The cleanest interpretation of the RD estimate is as the effect of NLRB certification. Because certification raises the probability of unionization and is unlikely to have effects through other channels, the sign of the certification effects in these firms should be the same as the the sign of unionization effects. Our primary results come from signing effects so we use the terms certification and unionization effects interchangeably. However, the magnitude of effects of unions taking root in firms may be larger. An alternative interpretation of the results would be as intent-to-treat effect with noncompliance.¹²

In sum, our analysis of the data indicates that the RD approach is valid as there is no systematic discontinuity in nursing home characteristics prior to the certification election. Further, the data also indicate that, in fact, union certification strongly increases the likelihood that union contracts exist in the future.

5 Estimated Effects of Certification

We now turn to estimating the effect of a union winning NLRB certification across a range of outcomes using the panel of post-election observations. In the post-election sample, the median elapsed time is 4.7 years, with an average of 5.0 years and a standard deviation of 3.3 years. For each outcome k , we estimate the discontinuity τ_k in the expected value of characteristic k across the threshold and interpret this as the effect of certification on that characteristic. For each k , we present estimates from a variety of specifications to assess sensitivity.

Employment and Wages. Table 3 presents estimates of the post-election discontinuity in each characteristic using local linear regression with optimal bandwidth choice grounded in

¹²We refrain from fuzzy RD analysis due to concern about measurement error in the union “treatment” measure.

asymptotic theory [Imbens and Kalyanaramang, 2009]. This approach is attractive because it does not rely on an *ad hoc* choice of bandwidth or assumptions about a particular parametric relationship between vote share and the expectation of the outcome. In order to allow for the possibility of errors correlated within home across observations, we estimate standard errors using a home-clustered bootstrap ($M=200$).

As described in the upper-left corner of Table 3, the estimated effect of union certification on nurse’s aide staffing levels (NA HPRD) is $-.311$ (0.116). This suggests that, in the years following elections, homes where unions just win elections have almost a third of an hour (18.7 minutes) less nurse’s aide staffing per resident day compared to homes where unions just lost elections. The mean level of NA staffing in the post-election sample is 2.12 HPRD (Table 1). Therefore, union certification is estimated to reduce NA staffing levels by about 15 percent ($\approx \frac{0.311}{2.12}$).

As one might do in an experimental setting, we can include additional pre-election conditioning variables to improve precision. Although specification A conditions only on vote share, specification B conditions also on each home’s pre-election mean NA HRPD. Specification C conditions also on the pre-election means for the full vector of other OSCAR characteristics. As in an experiment, we would expect point estimates to remain stable and standard errors to fall. This is largely what we see and provides further evidence of the discontinuity design’s credibility. The estimate for NA staffing is very stable across alternative conditioning sets. The estimate from specification B is $\hat{\tau} = -.360$ (0.118) and from C is $-.300$ ($.118$). Stability across specifications holds for almost all outcomes and subsamples.

For completeness and robustness, Figure 3 presents an analysis for the NA HPRD outcome graphically. Homes are grouped into vote share bins of width 5 percent. Only those with vote shares between 15 percent and 85 percent are included in order to reduce bias from including homes far from the 50 percent threshold. The pre-election observations are considered separately from the post-election observations. The former help test for pre-treatment discontinuity at the threshold, which would arouse suspicion about the identifying

assumption. The latter helps assess the union certification effect. The conditional mean as a function of vote share is somewhat noisy, likely due to relatively small sample sizes. The graph also presents estimates of $\hat{\tau}$ at the threshold using a parametric conditional mean assumption. Piecewise quadratic functions of vote share are allowed on either side of the threshold. Prior to the election, the discontinuity is small and insignificant: -0.049 (0.145). Looking at observations after the election, the discontinuity is large and significant: -0.248 (0.102).¹³

Evidence suggests sizable negative effects of certification on the employment levels for registered nurses (RN) as well. The preferred specification C produces an estimate of -0.211 (0.125). Because the average RN HPRD is only 0.503, this is a very large percentage reduction from certification, on the order of 40 percent fewer RN HPRD in homes where the union just won the election. Although the effect on LPN hours is estimated to be small and not significantly different than zero, the signs are consistently negative.

Taken together, these three direct care staffing level outcomes suggest that unionization leads to reduced staffing level in nursing homes. This presumably occurs because they raise wages and compensation costs through collective bargaining. Faced with higher prices, firms reduce demand. Because the OSCAR does not contain wage data, it is not possible to develop direct evidence on this price mechanism.

To develop additional evidence on this central question, we draw on the Current Population Survey from 2003-2009 to estimate union wage premia for NA, RN, and LPNs in the nursing home industry. If the negative union effects on nurse aide and registered nurse employment are being driven by positive union effects on compensation, we would expect to estimate a positive union wage premium among such workers. Although the unit of analysis, sample, and research design are all different in this exercise compared to the regression

¹³Analogous graphs for the other outcomes are available in Figures A.1 through A.10 in the Web Appendix at <https://sites.google.com/site/aaronsojourner>. Further, estimated effects for each outcome under a range of assumed parametric functional forms and bandwidths are presented in Tables A.1 through A.11 in the Web Appendix. Results are generally stable though the graphs show that many of the conditional mean functions are noisy.

discontinuity approach, it yields interesting, suggestive evidence.

The estimates are remarkably consistent with the employment effects from the regression discontinuity analysis. Unionization is associated with sizable and statistically significant wage premia for nurse aides 14.8 (1.8) percent and registered nurses 8.5 (4.3) percent. This is consistent with negative union effects on employment for these occupations, although this moderate positive RN wage effect alone would not be expected to drive such a large negative RN employment effect. Further, the lack of a wage premium, estimated at 2.4 (5.3) percent, for LPNs is also striking. It is consistent with the lack of an LPN employment effect in the regression discontinuity design.¹⁴

Aside from differential wage effects, federal minimum staffing requirements could also be part of the explanation. Federal law requires all of these homes to have an RN on duty at least 8 hours every day and to have a licensed nurse (RN or LPN) on duty the rest of the time. However, federal law has no requirements regarding NAs. Therefore, any nursing homes employing RNs more than 8 hours per day, when faced with increases in RN wages might substitute towards LPNs, even if LPN wages also increase.

Productivity. What do these results imply about the effect of unions on productivity? If productivity is measured as output (resident-days of care provided) per labor hour, then productivity went up after union certification.¹⁵ The inverse of this is built directly into our staffing level measures, which measure staff hours per resident day. Because the effects on all three types of labor are nonpositive and some are significantly negative, this suggests that productivity, as measured by output per hour of labor, increased.

Given that the nursing home literature generally links staffing levels to care quality, one might expect the decline in staffing levels to have a deleterious impact on the care quality. If

¹⁴Summary statistics by occupation are provided in Table A.12 and more details on the wage premia estimates in Table A.13 of the Web Appendix. Hirsch and Schumacher [1998a] also found a larger union wage premium for nurse aides than for RNs or LPNs in their study of health care occupational premia using the 1973-1994 CPS, though they did not focus exclusively on nursing homes. It also echoes a common finding of larger premia for lower-skilled occupations [Freeman and Medoff, 1984; Blanchflower and Bryson, 2004] and lower-paid workers [Frandsen, 2010].

¹⁵Lacking wage data we cannot measure the effect of unions on productivity measured as output per labor dollar.

so, it may be that unions reduce quality-adjusted productivity. For this reason, it is essential to assess the union impact on quality.¹⁶

Quality. As Table 3 reports, we find no evidence of an impact on average care quality, suggesting that labor productivity increased enough to offset the reduction in staffing levels per resident. We offer evidence from a rich, reliable set of measures of care quality. Under the direction of CMS, state surveyors use 175 consolidated measures encompassing structural, procedural, and outcome measures of quality to assign deficiencies during the regular inspection of nursing homes that are reported in the OSCAR data. As is common in the nursing home literature, we examine both the total number of survey deficiencies and an indicator for the presence of a severe health inspection deficiency. Several alternative remedies could be imposed on facilities that receive a high number of deficiencies. These punishments include civil money penalties, denial of payment for new admissions, state monitoring, temporary management, immediate termination, and other approaches. Beyond their importance as a government oversight mechanism in monitoring nursing homes, deficiencies have long been used as an approximation for nursing home quality and are widely thought to depend on staffing levels [Konetzka et al., 2004]. Finally, the federal government has made information on the number of deficiencies assigned to each certified facility nationwide available to consumers on its “Nursing Home Compare” web site. No significant effects were found on the total number of deficiencies nor on the presence of a severe deficiency, and their signs are opposite suggesting no consistent pattern.

Further, there is no significant effect on homes’ percentage of private-pay residents, those who are likely more responsive to and demanding of care quality. Although the Medicaid program is the dominant payer of nursing home services (accounting for about 50 percent of

¹⁶An obvious empirical strategy one might consider is to estimate the parameters of a production function in an RD framework. We decided not to pursue this possibility. In our context, at a minimum, we would want to specify an empirical quality production technology that allows for a hierarchical organizational structure [Simon and Barnard, 1976; Williamson, 1967; Rosen, 1992] and an additional, correlated technological unobservable beyond unobservables that affect union certification [Olley and Pakes, 1996]. To the best of our knowledge, no one has estimated such a model. Even if estimating such a model were feasible given our data, it is beyond the scope of this paper to develop and estimate such a model.

expenditures and roughly 70 percent of bed-days), private-pay residents are associated with higher profit margins relative to Medicaid residents and are therefore an important signal of facility resources [Mor et al., 2004]. To the extent that nursing homes compete to attract these higher margin clients by offering higher quality care, percentage private-pay can be taken as a market-based proxy for care quality. If unions have negative effects on quality, it would be expected to show up as a negative effect on percentage private pay.

Other strategic margins. If unionization leads homes to shift to a less severe case mix and the work becomes easier, then this might be confounded for a productivity increase. To assess this possibility, we study the effect of union certification on a facility acuity index, which measures the residents physical functioning level by incorporating both an activities of daily living index and the proportion of residents requiring special treatments. There is no evidence of a significant difference in residents' average acuity.

We also examine effects on total number of beds (establishment size or scale of production) and the percentage of beds occupied (measuring operating efficiency), both of which measure potential adjustments to labor-capital ratios. We observe no significant effect of certification on either the total number of beds nor the occupancy rate.¹⁷

Digging even deeper, we estimate union effects on seven specific proxies for care quality that are particularly sensitive to labor quality, that represent potential ways to use (or misuse) technology to substitute away from high quality staffing, or are direct evidence of lower quality care [Cawley et al., 2006].¹⁸ Each has legitimate, medically-appropriate levels, but union effects on the percentage of residents in any of these seven conditions would suggest

¹⁷We also investigated the direct impact of unionization on the number of residents and found no significant effect.

¹⁸Urethral catheterization can lessen the need for staff to assist with toileting, but place the resident at greater risk for urinary tract infection, with other long-term complications including bladder and renal stones, abscesses, and renal failure. Immobility resulting from the use of physical restraints may increase the risk of pressure ulcers, depression, mental and physical deterioration, and mortality. Feeding tubes can result in complications including self-extubation, infections, aspiration, unintended misplacement of the tube, and pain. Overuse and misuse of psychoactive medications may result in mental and physical deterioration. Although many residents are bedfast or chairbound due to medical conditions, bedfast and chairbound residents are at a higher risk of developing pressure ulcers and other complications. Pressure ulcers are areas of the skin and underlying tissues that erode as a result of pressure or friction and/or lack of blood supply.

that union certification and the decline in staffing levels it induces leads to lower quality care. Results are presented in Table 4. We find no consistent evidence of changes in care quality. The only significant effect suggested higher quality – the percent of residents on psychoactive medications is about 4 percent *lower* in homes where unions win elections, off a base of 55 percent.

Market structure and unionization. Turning back to the estimates at the bottom of Table 3, we see no effect of certification on the competitive structure of the local nursing market. We do see a positive effect on union density, which follows almost mechanically, because those homes where unions win elections are categorized as newly union in computing its market’s union density. It is interesting to note the magnitude of the estimate, as it suggests the average unionizing home holds about 10 percent of the beds in its county’s market.

Finally, we do not find evidence that certification affects firm survival. This finding is consistent with prior theoretical and empirical work [Freeman and Kleiner, 1999; DiNardo and Lee, 2004] and consistent with positive productivity effects. Although unions clearly have an incentive to extract rents from the firm, they also trade off any rent extraction against the increased risk that the firm would exit the market. Table 5 presents estimated certification effects on various measures of establishment survival without conditioning on anything more than vote share (A) or conditional on the full vector of pre-election home characteristics (C). The top panel measures the effect on survival to 2005, which 87.2 percent of the sample achieves.¹⁹ We also analyze the effect of certification on surviving at least 5 years post-election or 10 years post election and find no significant effects.

¹⁹We measure nursing home closure as failure to appear in the OSCAR data for three years, following Bowblis [2011]. Given that inspections are mandated at least every 15 months, this is a conservative measure. As our OSCAR panel extends through 2007, we study survival to 2005 as defined by an indicator measuring whether each home has any OSCAR observation in calendar years 2005, 2006 or 2007.

5.1 Effects by market characteristics and time horizon

The impact of unionization may be affected by the antecedents of local labor market union activity, the competitive environment, the regulatory environment and the amount of time the union has been active. Below we report the RD estimates by differences in these market and nursing home level characteristics.

Union density. Unions' ability to negotiate and enforce terms of employment depends on their strength in the local labor market, not just on being certified at a particular home. To assess the relationship between a market's union density and the effect of union certification there, we stratify the sample based on the home's local market union density at the time of the election. We divide the sample into those elections that occurred in homes in markets with below median union density and those in homes with above median union density. Then, in each subsample, we estimate the effect of certification. Results are in presented in Table 6. The negative point estimates on RN and NA staffing are larger in magnitude in less unionized markets (top panel) than in more unionized markets (bottom panel).²⁰ The results on quality look very similar in both types of markets suggesting that productivity may adjust smoothly.

In more highly unionized markets, unions have more power to support higher compensation levels. However, part of this ability comes from reducing competitive pressure on union firms by also raising compensation at nonunion firms in those markets. This occurs directly through raising market-clearing wages and indirectly through threat effects, which induce nonunion managers to try to match union standards in order to preempt employees' interest in unionizing. Therefore, in more unionized markets, there may be less scope for newly organized unions to raise wages. In this scenario, existing unions in more highly organized markets raise standards in nonunion firms, meaning their effects potentially pre-date unionization and would be under-estimated in the RD design. Although in markets where few

²⁰Bootstrap tests for difference in the specification A estimates show that only the difference in RN staffing is significant ($p=0.06$).

employers are organized and wages are low, there may be lots of potential for newly installed unions to change conditions of employment. This effect suggests the opposite possibility, bigger direct effects in less organized markets.

Nursing home market competitiveness. Unions' ability to extract economic rents from firms depend on the existence of rents in the firms. Because firms in less competitive markets might have more rents, we might expect to see bigger union effects in less competitive (higher HHI) markets. For NA and RN HPRD, this pattern is apparent by comparing the top and bottom panels of Table 7. The negative employment effects are negative but statistically insignificant in competitive markets but larger and highly significant in less competitive markets.²¹ For LPNs, the pattern runs in the other direction. None are significant.

The evidence suggests that certification leads to lower care quality in less competitive markets and perhaps higher quality care in more competitive ones. In more competitive markets, the likelihood of a severe deficiency decreases 18.9 (9.6) percent with certification and the point estimate on total deficiency count is negative, also consistent with higher quality of care. Negative employment effects accompanied by positive quality effects suggest positive productivity effects in more competitive markets. In contrast, in less competitive markets, where staff declines are sharper, the evidence suggests negative effects on care quality. Total deficiency counts go up by 2.93 (1.36) and the signs on private pay percentage and severe deficiency results are similarly signed. The effect on productivity in less competitive markets is ambiguous. Certification has significantly worse effects on two of the three quality measures in less competitive markets compared to more competitive markets: total deficiency count ($p=0.02$) and percentage private-pay ($p=0.09$). However, none of the differences in staffing level effects are significant. This finding strengthens the empirical foundation for an earlier literature that suggested more positive union productivity effects in more competitive product markets and more concern about negative productivity effects in less competitive

²¹Ideally, we would like to look at both HHI and union density simultaneously and in a statistical framework that allows for formal measurement of differences. However, given the limited sample size, this does not produce meaningful results.

product markets [Hirsch and Addison, 1986].

Another possible interpretation of this result comes from noting that product market competitiveness is likely highly correlated with labor market competitiveness, suggesting HHI may proxy for monopsony in the labor market. In that case, this finding could be interpreted as supportive of a theoretical result in Section 12.5 of Manning [2003] that union wage mark-ups are likely higher in less competitive labor markets, assuming unions do not strongly prefer greater employment over higher wages.

Staffing regulations. Many states have legislated minimum staffing levels that are more stringent than the national standards. In order to explore the impact of these regulations on the impact of unionization, we classify state/year pairs as either having “strong” or “weak” staffing regulations based on the work of Harrington [2008] and estimate the impact of union certification for each sample. Strong regulatory states place floors on RNs and LPNs per resident and typically do not place as binding limits on the use of NAs. Unionization in the “strong” states should thus be primarily associated with significant declines in NAs as the regulations limit adjustments in RN and LPN staff. The results are presented in Table 8. Consistent with our priors, certification has a much larger (and statistically significant) impact on NAs in strong state-year nursing homes compared to weak state-year nursing homes. In weak regulatory regimes, the point estimates for NA and RN are all negative but imprecisely estimated.

Elapsed time. Union effects might grow over time, as the union becomes entrenched and the contract expands. On the other hand, the union might accelerate changes that would have occurred anyway. This relationship would produce a large effect early, which narrows over time [Freeman and Kleiner, 1990]. To investigate this issue, we split the sample of observations at the median post-election elapsed time (4.6 years) to investigate short-run versus long-run effects. Table 9 presents short-run estimates using only those OSCAR observations from years close to the election in the top panel and long-run estimates from observations in later years in the bottom panel.

No large differences exist between the short and long run effects. The negative NA employment point estimate increases in magnitude and the RN estimate diminishes somewhat. However, these are not statistically different. Looking at the long-run sample, there appears to be a marginally significant increase in total deficiencies. This differs from the result in the short-run and overall sample and could be evidence of a decline in care quality over time driven by greater decreases in RN staffing levels. However, neither of the other care quality measures, the private pay percentage and presence of a severe deficiency, confirms this story.

To summarize, certification is associated with decreased employment of NAs and RNs but not LPNs and there is evidence of corresponding wage increases for NAs and RNs. Although there are significant declines in employment, we do not observe an associated change in the quantity or quality of nursing home care produced, nor do we see evidence that certification affects capital investment or nursing home survival. The effect of certification is enhanced in less dense union markets, in more concentrated markets, and when staffing regulations are more stringent.

6 Conclusion

This paper provides important new evidence on the effects of unions on employment, product quality, and productivity in the increasingly important service sector, where little is known about union effects. Using a regression discontinuity design, we found that unionization of nursing homes led to a significant decline in staffing levels. Using the CPS, we found support for the idea that this decrease in staffing corresponded to higher wages in union facilities. Importantly, the decline in staffing level following unionization did not, on average, harm firm growth or survival, or the quantity or quality of care produced. This suggests that unionization increased labor productivity. We also observed stronger unionization effects in facilities in more concentrated markets and the least unionized markets, although the latter result is also consistent with large but difficult to observe threat effects of unions in highly

unionized markets.

The exact explanation for increased productivity following unionization remains somewhat open. It is consistent with increases in productivity holding workers fixed [Freeman and Medoff, 1984; Perelman, 2011]. Unionized nursing homes may also have a stronger incentive to invest in workers' human capital and provide additional training. In the context of higher union wages, the qualifications of the workers entering the firms should be higher and the rate of staff turnover lower. These factors have been found to relate positively to nursing home care outcomes.

These findings inform debates over proposals to change federal and state regulations governing workers' rights to organize unions. A new flurry of diverse labor law reform proposals is being debated all around the U.S. The Employee Free Choice Act (EFCA) proposed three reforms to make it easier to form and maintain unions: permitting unions to demonstrate with majority support through signed authorization cards rather than elections, strengthen monetary penalties for violations of employees' rights to organize, and ending first contract impasses through arbitration. In the absence of legislative support for EFCA, the NLRB has recently taken administrative action to speed NLRB elections and crack down on alleged violations of employees' right to strike. On the other hand, Wisconsin, Ohio, and other states are debating reforms that would make it harder for public-sector workers to unionize. In nursing homes at least, this study finds that expanding certification on the margin increased nursing home labor productivity, with little effect on firm survival or product quality.

Looking at the nursing home industry, although interesting in its own right, is important primarily because it can give insight into the role of unions in broader parts of the economy. Going forward, unions are likely to survive and expand primarily in nontradeable sectors where the possibility of outsourcing is minimal and in sectors where organized workers can most directly use their political influence to shape the terms of market competition. Nursing homes exhibit both of these qualities. While the present and future of organized labor is in

health care, education, public services, retail and other industries of this kind, much of the literature on union effects has focused elsewhere.

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7 Tables

Table 1: Summary statistics

	Mean	Std. Dev.	Min	Max	Obs.	Mean	Std. Dev.	Min	Max	Obs.
NLRB election characteristics										
Vote share	0.029	0.195	-0.475	0.475	627					
1(vote share > 0.50)	0.558	0.497	0	1	627					
OSCAR nursing home characteristics										
	All observations					Post-election observations only				
NA hours/resident day (HPRD)	2.13	0.763	0	8	7942	2.123	0.718	0	8	5319
RN HPRD	0.558	0.803	0	7.914	7978	0.503	0.736	0	7.914	5332
LPN HPRD	0.773	0.528	0	8	7993	0.778	0.485	0	7.137	5334
Total deficiency count	6.826	6.5	0	81	8045	6.87	5.978	0	81	5353
1(severe deficiency)	0.233	0.423	0	1	8045	0.265	0.442	0	1	5353
Pct. private pay	22.622	17.641	0	100	8045	21.59	16.196	0	100	5353
Acuity index	10.275	1.576	3	24.083	7971	10.22	1.536	3	24.083	5329
Total beds	125.684	79.377	5	977	8045	124.74	73.111	5	976	5353
Pct. beds occupied	84.837	19.272	0.207	100	8045	85.284	16.942	0.556	100	5353
HHI, in county	0.152	0.195	0.003	1	8045	0.159	0.2	0.003	1	5353
Union density, private only	31.102	26.744	0	100	8045	35.446	27.538	0	100	5353
Turnout	86.571	9.07	45.833	100	8031	86.671	9.138	45.833	100	5344
Log(bargaining unit size)	4.497	0.65	3.091	7.142	8031	4.479	0.616	3.091	7.142	5344
Log(number valid votes)	4.347	0.652	2.996	7.066	8045	4.331	0.619	2.996	7.066	5353

Table 2: System OLS test for discontinuity in nursing homes characteristics in pre-election panel

Variables	Vote share within h of 0.50 threshold				
	$h = 0.05$	$h = 0.15$	$h = 0.25$	$h = 0.35$	$h = 0.50$
<i>Staffing</i>					
NA HPRD	-.0584 (0.111)	-.058 (0.182)	-.0987 (0.122)	-.0204 (0.222)	-.137 (0.111)
RN HPRD	-.113 (0.101)	-.117 (0.161)	-.0307 (0.106)	.0529 (0.217)	-.0723 (0.100)
LPN HPRD	-.103 (0.176)	-.0872 (0.231)	.00984 (0.173)	.11 (0.257)	-.0321 (0.151)
<i>Quality</i>					
Total deficiency count	.349 (0.853)	-.264 (1.09)	.518 (0.89)	1.26 (0.798)	1.09 (0.713)
1(severe deficiency)	-.0584 (0.111)	-.058 (0.182)	-.0987 (0.122)	-.0204 (0.222)	-.137 (0.111)
Prct. private pay	2.47 (3.06)	2.72 (3.76)	-.275 (3.00)	.148 (2.58)	-1.78 (2.42)
<i>Other strategic margins</i>					
Acuity index	-.181 (0.276)	-.0348 (0.349)	-.166 (0.259)	-.172 (0.31)	-.388 (.23)*
Number of beds	-8.63 (26.4)	-11 (30.2)	-7.96 (22.6)	2.67 (19)	.499 (16.7)
Prct. beds occupied	.505 (4.7)	1.46 (5.7)	-2.25 (4.35)	-.671 (3.7)	-.162 (3.45)
<i>Market characteristics</i>					
Market HHI - same county	.0125 (0.0301)	.0786 (0.124)	-.00284 (0.043)	.123 (0.205)	.0234 (0.072)
Union density, private only	-3.94 (3.54)	-4.86 (4.6)	-3.81 (3.78)	-5.36 (3.48)	-6.71 (3.08)**
<i>Election characteristics</i>					
Log(NLRB election unit size)	-.184 (0.169)	-.188 (0.235)	-.156 (0.17)	.0313 (0.236)	-.106 (0.142)
Log(Valid votes cast in election)	-.207 (0.167)	-.206 (0.232)	-.168 (0.17)	.0122 (0.239)	-.125 (0.144)
Number of homes observed	131	350	483	576	626
Number of home-dates observed	587	1,517	2,042	2,480	2,688
Number of home-date-outcomes observed	6,986	18,077	24,350	29,574	32,053
Adjusted R ²	.526	.602	.628	.639	.645
Joint-null F -test p -value	.844	.885	.968	.697	.359

Coefficient (within-home-correlation corrected SE). Significance: *: 10% **: 5% ***: 1%.

Each column presents discontinuity estimates from a separate system of first-order linear equations (6).

Only observations from homes with vote share within h of the 0.50 threshold are used.

Table 3: Estimated effects on nursing homes characteristics using post-election panel and local linear estimator

Specification	Estimates			Home-dates obs.		
	1	2	3	1	2	3
<i>Staffing</i>						
NA HPRD	-.311 (.116)***	-.360 (.118)***	-.331 (.114)**	5,319	5,250	5,220
RN HPRD	-.278 (.208)	-.317 (.153)**	-.291 (.140)**	5,332	5,279	5,236
LPN HPRD	-.072 (.106)	-.050 (.068)	-.043 (.073)	5,334	5,302	5,234
<i>Quality</i>						
Total deficiency count	1.326 (.897)	.839 (.837)	1.609 (.870)*	5,353	5,353	5,245
1(severe deficiency)	-.067 (.052)	-.065 (.052)	-.048 (.044)	5,353	5,353	5,245
Prct. private pay	2.633 (3.908)	-.971 (2.220)	-2.023 (2.202)	5,353	5,353	5,245
<i>Other strategic margins</i>						
Acuity index	-.177 (.311)	-.352 (.242)	-.188 (.230)	5,329	5,328	5,221
Number of beds	-24.31 (25.81)	5.151 (4.624)	5.368 (4.728)	5,353	5,353	5,245
Prct. beds occupied	3.143 (4.584)	2.334 (3.851)	2.081 (3.724)	5,353	5,353	5,245
<i>Market characteristics</i>						
Market HHI - same county	.051 (.051)	-.0004 (.008)	.003 (.010)	5,353	5,353	5,245
Union density, private only	5.545 (7.328)	9.688 (4.904)**	10.841 (3.557)***	5,353	5,353	5,245
Specification conditional on home's pre-election mean of:						
This outcome variable	N	Y	Y			
All other outcome variables	N	N	Y			

Coefficient (within-home cluster bootstrap SE). Significance: *: 10% **: 5% ***: 1%.

Each cell presents discontinuity estimate from a separate local linear regression.

Number of home-dates observed varies due to missing outcomes or conditioning values.

Table 4: Estimated effects on nursing homes characteristics using post-election panel and local linear estimator

Specification	Estimates			Home-dates obs.		
	1	2	3	1	2	3
Prct. urethral catheter	-.817 (1.441)	-.933 (.997)	-.917 (.956)	5,353	5,353	5,245
Prct. mobility restrained	.478 (1.578)	-.526 (1.577)	-1.415 (1.584)	5,353	5,353	5,245
Prct. spc. feeding tube	-.580 (1.738)	-.206 (1.043)	.27 (1.006)	5,353	5,353	5,245
Prct. psycho-active meds	-4.841 (2.765)*	-4.667 (2.47)*	-3.131 (2.61)	5,353	5,353	5,245
Prct. skin pressure sores	-1.369 (.97)	-1.673 (1.322)	-.613 (1.121)	5,238	3,484	3,417
Prct. bedfast	.123 (1.052)	-.217 (.977)	.26 (.935)	5,353	5,353	5,245
Prct. mobility chair	.237 (3.568)	.07 (3.485)	1.412 (2.992)	5,353	5,353	5,245

Specification conditional on home's pre-election mean of:

This outcome variable

N

Y

Y

All Table 3 variables

N

N

Y

Coefficient (within-home cluster bootstrap SE). Significance: *: 10% **: 5% ***: 1%.

Each cell presents discontinuity estimate from a separate local linear regression.

Number of home-dates observed varies due to missing outcomes or conditioning values.

Table 5: Estimated effects on survival

	Specification	
	A	C
DV: 1(survives to 2005)		
	.107	.069
	(.082)	(.063)
<i>N</i>	627	611
DV Mean	0.872	
DV: survival over various post-election spans		
1(survived > 5 years)		
	0.033	0.009
	(0.084)	(0.055)
<i>N</i>	480	466
DV Mean	0.915	
1(survived > 10 years)		
	.011	-.066
	(.038)	(.094)
<i>N</i>	102	101
DV Mean	0.873	
Conditional on home's pre-election mean Table 3 variables		
	N	Y

Coefficient/(SE). Significance: *: 10% **: 5% ***: 1%.

Each cell presents discontinuity estimate from a separate local linear regression. Sample sizes differ between A and C due to missing conditioning values. Specification B omitted.

Table 6: Estimated effects by market's union density

Specification	Estimates			Home-dates obs.		
	A	B	C	A	B	C
Home in Less Unionized Nursing Home Markets (below median density) at Election						
NA HPRD	-0.418 (0.185)**	-0.535 (0.196)**	-0.533 (0.219)**	2,723	2,695	2,686
RN HPRD	-0.637 (0.331)*	-0.610 (0.243)**	-0.487 (0.252)*	2,728	2,706	2,693
LPN HPRD	-0.055 (0.156)	-0.056 (0.084)	-0.051 (0.106)	2,730	2,720	2,692
Total deficiency count	.877 (1.295)	1.678 (1.202)	1.974 (1.401)	2,733	2,733	2,695
1(severe deficiency)	-0.045 (0.060)	-0.045 (0.061)	-0.033 (0.065)	2,733	2,733	2,695
Prct. private pay	2.987 (4.768)	-2.029 (3.019)	-4.207 (3.819)	2,733	2,733	2,695
Acuity index	.394 (0.491)	-.134 (0.363)	-.377 (0.432)	2,719	2,719	2,681
Number of beds	-14.887 (29.86)	3.433 (4.536)	6.475 (5.164)	2,733	2,733	2,695
Prct. beds occupied	4.681 (7.638)	6.560 (4.856)	5.784 (5.966)	2,733	2,733	2,695
Market HHI - same county	.065 (0.092)	-0.003 (0.014)	-0.010 (0.020)	2,733	2,733	2,695
Union density, private only	21.408 (8.230)**	20.500 (7.974)**	19.564 (6.004)**	2,733	2,733	2,695
Home in More Unionized Nursing Home Markets (above median density) at Election						
NA HPRD	-0.165 (0.137)	-0.097 (0.137)	-0.111 (0.202)	2,596	2,555	2,534
RN HPRD	.124 (0.132)	.071 (0.155)	-.128 (0.187)	2,604	2,573	2,543
LPN HPRD	-.107 (0.125)	-.128 (0.108)	-.034 (0.137)	2,604	2,582	2,542
Total deficiency count	.508 (1.203)	-.069 (1.264)	1.005 (1.474)	2,620	2,620	2,550
1(severe deficiency)	-0.047 (0.065)	-0.038 (0.067)	-0.012 (0.081)	2,620	2,620	2,550
Prct. private pay	2.098 (4.393)	-.627 (2.882)	-2.154 (6.170)	2,620	2,620	2,550
Acuity index	-.482 (0.382)	-.525 (0.324)	-.128 (0.566)	2,610	2,609	2,540
Number of beds	-6.139 (34.682)	3.734 (8.154)	4.293 (02.030)	2,620	2,620	2,550
Prct. beds occupied	-5.274 (4.867)	-5.259 (4.865)	-3.595 (5.516)	2,620	2,620	2,550
Market HHI - same county	-.026 (0.041)	.003 (0.005)	.007 (0.007)	2,620	2,620	2,540
Union density, private only	-3.588 (8.432)	.593 (6.394)	2.014 (8.390)	2,620	2,620	2,550
Specification conditional on home's pre-election mean of:						
This outcome variable	N	Y	Y			
All other outcome variables	N	N	Y			

Coefficient (within-home cluster bootstrap SE). Significance: *: 10% **: 5% ***: 1%.

Each cell presents discontinuity estimate from a separate local linear regression and each column corresponds to a different set of control variables. Number of home-dates observed varies due to missing outcomes or conditioning values.

Table 7: Estimated effects by product market competitiveness

Specification	Estimates			Home-dates obs.		
	A	B	C	A	B	C
Home in More Competitive Nursing Home Markets (below median HHI) at Election						
NA HPRD	-.237 (0.170)	-.230 (0.153)	-.188 (0.163)	2,513	2,478	2,475
RN HPRD	-.217 (0.219)	-.064 (0.167)	-.116 (0.220)	2,518	2,510	2,480
LPN HPRD	-.247 (0.122)**	-.160 (0.085)*	-.135 (0.102)	2,517	2,503	2,479
Total deficiency count	-1.074 (1.208)	-1.464 (1.193)	-.444 (1.437)	2,531	2,531	2,486
1(severe deficiency)	-.165 (0.077)**	-.187 (0.085)**	-.189 (0.096)**	2,531	2,531	2,486
Prct. private pay	9.453 (5.662)*	.789 (3.261)	-.327 (4.265)	2,531	2,531	2,486
Acuity index	-.344 (0.424)	-.251 (0.384)	-.190 (1.036)	2,521	2,520	2,476
Number of beds	-23.298 (22.68)	6.419 (6.073)	-2.614 (8.040)	2,531	2,531	2,486
Prct. beds occupied	7.826 (4.843)	5.718 (4.940)	6.366 (7.311)	2,531	2,531	2,486
Market HHI - same county	.007 (0.007)	-.0004 (0.002)	-.0003 (0.003)	2,531	2,531	2,486
Union density, private only	.355 (5.509)	-1.098 (3.586)	-1.373 (5.375)	2,531	2,531	2,486
Home in Less Competitive Nursing Home Markets (above median HHI) at Election						
NA HPRD	-.343 (0.156)**	-.416 (0.160)**	-.552 (0.216)**	2,806	2,772	2,745
RN HPRD	-.344 (0.285)	-.560 (0.270)**	-.470 (0.236)**	2,814	2,769	2,756
LPN HPRD	.092 (0.147)	.011 (0.106)	.005 (0.118)	2,814	2,799	2,755
Total deficiency count	3.056 (1.240)**	2.500 (1.306)**	2.927 (1.363)**	2,822	2,822	2,759
1(severe deficiency)	.036 (0.063)	.038 (0.064)	.018 (0.065)	2,822	2,822	2,759
Prct. private pay	-4.090 (5.142)	-4.100 (3.469)	-3.836 (3.293)	2,822	2,822	2,759
Acuity index	-.264 (0.491)	-.739 (0.378)**	-.402 (0.412)	2,808	2,808	2,745
Number of beds	-20.69 (33.933)	1.644 (6.333)	3.534 (6.156)	2,822	2,822	2,759
Prct. beds occupied	-1.883 (6.556)	-.655 (4.935)	-2.013 (5.232)	2,822	2,822	2,759
Market HHI - same county	.044 (0.081)	.003 (0.014)	.002 (0.019)	2,822	2,822	2,759
Union density, private only	8.990 (10.872)	18.033 (7.887)**	16.348 (7.587)**	2,822	2,822	2,759
Specification conditional on home's pre-election mean of:						
This outcome variable	N	Y	Y			
All other outcome variables	N	N	Y			

Coefficient (within-home cluster bootstrap SE). Significance: *: 10% **: 5% ***: 1%. Each cell presents discontinuity estimate from a separate local linear regression and each column corresponds to a different set of control variables. Number of home-dates observed varies due to missing outcomes or conditioning values.

Table 8: Estimated effects by strength of state RN/LPN staffing regulation

Specification	Estimates			Home-dates obs.		
	A	B	C	A	B	C
Observations in State-Years with Strong RN/LPN Staffing Regulation						
NA HPRD	-.352 (.152)**	-.405 (.151)***	-.371 (.151)**	3,585	3,545	3,529
RN HPRD	-.032 (.254)	-.153 (.200)	-.202 (.192)	3,595	3,560	3,541
LPN HPRD	-.089 (.113)	-.108 (.106)	-.095 (.104)	3,595	3,571	3,539
Total deficiency count	.363 (1.000)	.586 (.991)	1.333 (.944)	3,605	3,605	3,544
1(severe deficiency)	-.068 (.054)	-.067 (.055)	-.033 (.060)	3,605	3,605	3,544
Prct. private pay	5.135 (4.382)	.274 (2.846)	-1.825 (2.890)	3,605	3,605	3,544
Acuity index	-.266 (.393)	-.446 (.316)	-.071 (.317)	3,593	3,593	3,532
Number of beds	-23.182 (25.639)	-2.329 (5.630)	-1.274 (3.924)	3,605	3,605	3,544
Prct. beds occupied	6.824 (6.485)	6.190 (4.752)	6.899 (4.784)	3,605	3,605	3,544
Market HHI - same county	.015 (.043)	.003 (.005)	.006 (.007)	3,605	3,605	3,544
Union density, private only	2.971 (7.753)	5.006 (4.113)	6.698 (3.984)*	3,605	3,605	3,544
Observations in State-Years with Weak RN/LPN Staffing Regulation						
NA HPRD	-.184 (.196)	-.183 (.168)	-.255 (.268)	1,734	1,705	1,691
RN HPRD	-.550 (.324)*	-.414 (.199)**	-.375 (.865)	1,737	1,719	1,695
LPN HPRD	.051 (.204)	.045 (.078)	.053 (.283)	1,739	1,731	1,695
Total deficiency count	2.054 (1.584)	.158 (1.748)	.828 (4.270)	1,748	1,748	1,701
1(severe deficiency)	.002 (.079)	.005 (.078)	-.019 (.232)	1,748	1,748	1,701
Prct. private pay	-2.687 (5.021)	-2.194 (3.753)	-3.974 (17.703)	1,748	1,748	1,701
Acuity index	-.400 (.499)	-.473 (.487)	-.498 (.721)	1,736	1,735	1,689
Number of beds	19.657 (47.458)	6.801 (6.706)	3.956 (10.597)	1,748	1,748	1,701
Prct. beds occupied	-7.227 (5.241)	-6.307 (5.027)	-8.562 (11.836)	1,748	1,748	1,701
Market HHI - same county	.095 (.102)	-.009 (.019)	.015 (.105)	1,748	1,748	1,701
Union density, private only	7.996 (13.836)	17.627 (10.088)*	18.429 (13.306)	1,748	1,748	1,701
Specification conditional on home's pre-election mean of:						
This outcome variable	N	Y	Y			
All other outcome variables	N	N	Y			

Coefficient (within-home cluster bootstrap SE). Significance: *: 10% **: 5% ***: 1%.

Each cell presents discontinuity estimate from a separate local linear regression and each column corresponds to a different set of control variables. Number of home-dates observed varies due to missing outcomes or conditioning values.

Table 9: Estimated effects by elapsed time since election

Specification	Estimates			Home-dates obs.		
	A	B	C	A	B	C
Short-run effects: observations less than median elapsed time (4.6 years) past election						
NA HPRD	-.333 (0.163)**	-.350 (0.135)***	-.337 (0.134)**	2,652	2,618	2,600
RN HPRD	-.307 (0.258)	-.282 (0.171)*	-.236 (0.152)	2,659	2,632	2,609
LPN HPRD	-.090 (0.136)	-.061 (0.083)	-.042 (0.075)	2,663	2,651	2,610
Total deficiency count	.593 (1.001)	-.065 (0.876)	.512 (0.896)	2,676	2,676	2,617
1(severe deficiency)	-.078 (0.073)	-.070 (0.074)	-.046 (0.060)	2,676	2,676	2,617
Prct. private pay	-.737 (3.905)	-2.135 (2.237)	-3.341 (2.200)	2,676	2,676	2,617
Acuity index	-.165 (0.359)	-.296 (0.267)	-.044 (0.241)	2,652	2,651	2,593
Number of beds	-18.729 (28.978)	11.296 (4.804)**	9.624 (4.949)*	2,676	2,676	2,617
Prct. beds occupied	1.728 (5.235)	1.164 (3.687)	.210 (3.348)	2,676	2,676	2,617
Market HHI - same county	.046 (0.045)	.007 (0.008)	.011 (0.009)	2,676	2,676	2,617
Union density, private only	8.462 (6.441)	11.058 (4.165)***	12.109 (3.249)***	2,676	2,676	2,617
Long-run effects: observations more than median elapsed time (4.6 years) past election						
NA HPRD	-.270 (0.129)**	-.261 (0.128)**	-.250 (0.149)*	2,667	2,632	2,620
RN HPRD	-.131 (0.170)	-.305 (0.153)**	-.318 (0.129)**	2,673	2,647	2,627
LPN HPRD	-.029 (0.099)	-.049 (0.080)	-.051 (0.073)	2,671	2,651	2,624
Total deficiency count	1.848 (1.103)*	1.593 (1.084)	2.546 (1.182)**	2,677	2,677	2,628
1(severe deficiency)	-.056 (0.053)	-.056 (0.051)	-.033 (0.057)	2,677	2,677	2,628
Prct. private pay	1.885 (4.128)	-1.296 (2.759)	-.176 (2.688)	2,677	2,677	2,628
Acuity index	-.178 (0.330)	-.375 (0.302)	-.269 (0.284)	2,677	2,677	2,628
Number of beds	-29.470 (21.108)	-1.161 (6.805)	2.660 (5.498)	2,677	2,677	2,628
Prct. beds occupied	3.010 (5.515)	2.372 (4.555)	3.411 (4.423)	2,677	2,677	2,628
Market HHI - same county	.053 (0.062)	-.005 (0.011)	-.001 (0.012)	2,677	2,677	2,628
Union density, private only	2.714 (7.974)	8.053 (6.123)	8.570 (5.217)*	2,677	2,677	2,628
Specification conditional on home's pre-election mean of:						
This outcome variable	N	Y	Y			
All other outcome variables	N	N	Y			

Coefficient (within-home cluster bootstrap SE). Significance: *: 10% **: 5% ***: 1%. Each cell presents discontinuity estimate from a separate local linear regression and each column corresponds to a different set of control variables. Number of home-dates observed varies due to missing outcomes or conditioning values.

8 Figures

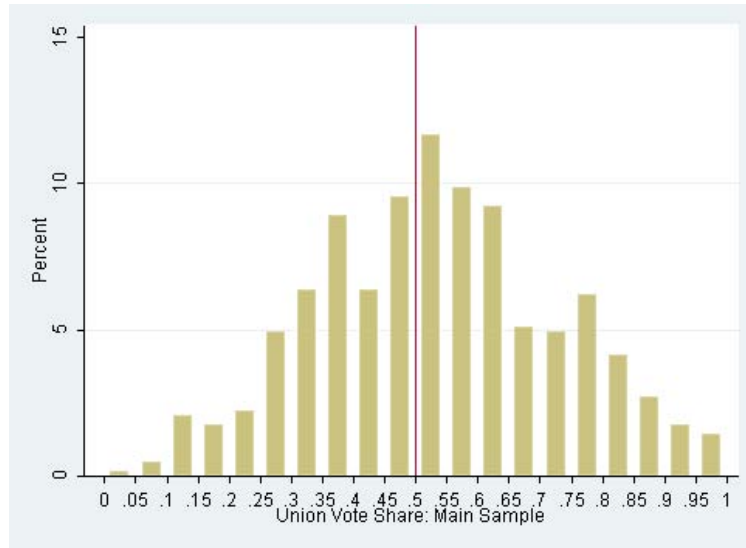


Figure 1: Density of election vote shares in main sample ($N=627$).

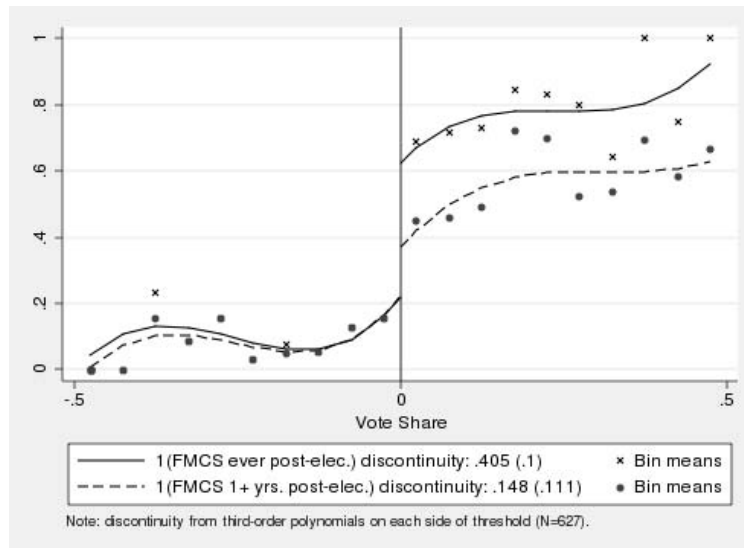


Figure 2: Expectation of indicators for FMCS notice filed anytime subsequent to focal NLRB election and FMCS notice filed at least one year after focal NLRB election by vote share. Means of the indicator variable are presented for each of 20 vote share bins. The discontinuity from a piece-wise polynomial estimated using all homes is also presented.

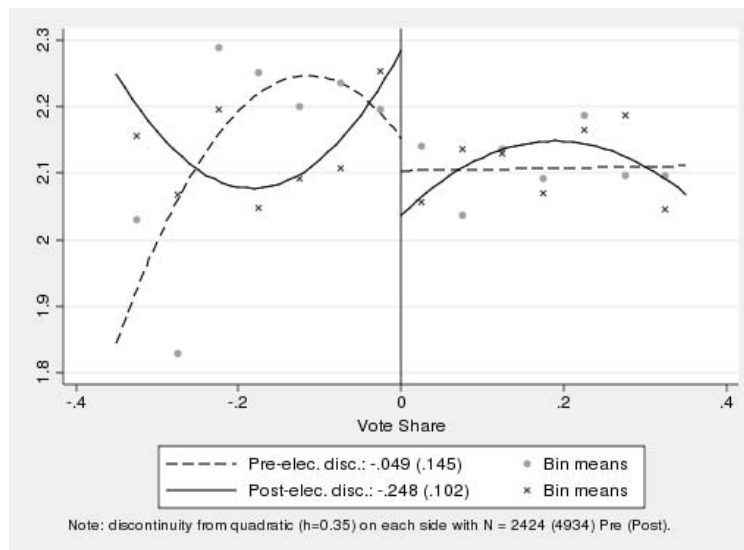


Figure 3: Expected nurse's aide hours/resident day (NA HPRD) by vote share bin in the pre- and post-election periods. The discontinuities from piece-wise second-order polynomial estimated using all homes is also presented. Only homes with vote shares between 15% and 85% are used.