Three Essays on Market Structure Variation and Labor Market Outcomes

by

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To Mommy, Daddy, Sue Anne, Dan, Griffey, and my grandparents. And to Bella and Danny– this is why Aunt Christina has been in college for so long.

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CHAPTER I

The Effects of Hospital Consolidation on Labor Market Outcomes

1.1 Introduction

A well-documented "merger wave" occurred in the hospital sector during the 1990s and early 2000s. These mergers received a lot of attention from economists, policy makers, and antitrust authorities. There now appears to be a new wave on the horizon. From 2009 to 2012, the number of annual hospital mergers more than doubled, from 50 to 105 (New York Times, 2013). Congressman Jim McDermott recently asked the U.S. Government Accountability Office to investigate the effects of hospital mergers as he is worried that hospitals are starting to resemble the consolidated banks that were once "too big to fail."

While hospitals tout increased efficiency and decreasing costs as a reason for the mergers, policy analysts and antitrust authorities worry that the decrease in competition could result in higher prices for consumers.[1] These potential price effects have been studied extensively in the literature. In contrast, in this chapter, I am interested in the labor market consequences of consolidation, where consolidation can be one of two types: hospital mergers or hospital system-joinings. Given the importance of hospitals to local labor markets, the effect of consolidation on labor market outcomes could be significant. According to the American Hospital Association, hospitals employed over 5.4 million people in 2011, making them the second largest source of private sector jobs behind restaurants. In fact, Kaiser Permanente, New York City and Health, and Advocate Health Care are the largest non-government employers for all workers in Los Angeles, New York, and Chicago, respectively. Moreover, hospitals employ specialized labor. Researchers have long argued that nurses, in particular, may be subject to monopsony power (Hurd, 1973; Sullivan, 1989; Staiger et al., 2010)

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[1] For a review of the hospital merger literature, see Vogt and Town (2006).

because some research suggests they are relatively immobile (Kovener et al., 2011; Rosenberg et al., 2011) and because hospital jobs are different in terms of caseloads and tasks from other nursing jobs. Thus, some have argued that hospital mergers could have significant effects on labor markets, especially the market for nurses.

In this chapter, I use data from the American Hospital Association's Annual Survey (AHA) and the National Sample Survey of Registered Nurses (NSSRN) to create a unique panel data set covering the years 1983-2009. I use a difference-in-differences approach and propensity-score weighting to correct for selection bias, and examine changes in employment levels and wages of registered nurses (RNs) and licensed practical nurses (LPNs) before and after a hospital consolidation. I also conduct a "rival" analysis similar to Woolley (1989), Connor and Feldman (1998), and Dafny (2009), where I look for effects on the subsample of hospitals that never merge but are exposed to a merger.

This chapter contributes to the literature in four main ways. First, while there is a large literature on hospital mergers, these studies focus on merger impacts on prices, costs, and patient outcomes (Connor and Feldman, 1998; Ho and Hamilton, 2000; Dranove and Lindrooth, 2003; Town et al., 2006; Dafny, 2009; Harrison, 2010; Hass-Wilson and Garmon, 2011; Hayford, 2011; Gaynor et al, 2012; Patel, 2013). They find mixed results. In fact, very few studies have examined the effects of mergers, or any takeover, on labor market outcomes (Brown and Medoff, 1988; McGuckin and Nguyen, 2001; Li, 2012). To the best of my knowledge, no study of hospital mergers has ever focused directly on the potential employment and wage effects.

Second, I allow estimates of the effects to differ by mergers and system-joinings. Thus far, the hospital consolidation literature has largely ignored the potential effects of system-joinings (Cueller and Gertler (2005) and Melnick and Keeler (2007) are exceptions). System-joinings differ from mergers in that they do not result in a full merging of assets or a shared license. Hospitals that join a system share a common governing owner but continue to operate under separate licenses. These system-joinings can involve as little as simply becoming part of a "brand" or as much as a full change in the governing owners. While wage decisions and union bargaining happen at the hospital level even after joining a system, it is plausible that system-joinings could impact employment through either decreased competition in the labor market or efficiency gains from shared knowledge or facilities.

Third, I contribute to the literature on monopsony power (or buying power) in general, and in the market for nurses specifically (Link and Landon, 1975; Sullivan, 1989; Hirsch and Schumacher, 1995; Staiger et al., 2010; Matsudaira, 2013). To identify potential reasons for hospital consolidation, I examine how wages of registered nurses and licensed practical nurses change after a hospital merger or a system-joining. If, from a labor perspective, decreased competition for labor is the dominant effect of consolidation then wages should decrease as a result of the increased buying power of the hospitals; if efficiency gains are the dominant effect due to consolidation, as the hospitals contend they are, then wages should be unaffected.

Fourth, and finally, the length of the period covered by the data I have gathered allows for a detailed analysis of both the short-term and long-term effects on local labor market outcomes.

I find that RN employment decreases by an average of 16% after a merger and that this effect persists five years after a merger. I find that LPN employment also substantially decreases even more following a merger, falling 24% on average. These results appear to be driven by efficiency gains rather than an increase in buying/monopsony power, as average hospital payroll per worker does not decrease.[2] In addition, the ratio of nurses to admissions either stays the same or decreases. I corroborate that the results are driven by efficiency gains by examining rival hospitals that are exposed to a hospital merger. If hospitals that merge are exploiting an increase in buying power, then hospitals in the same labor market should also be able to exploit the increase in market concentration. While I find some reductions in employment of RNs and LPNs at rival hospitals, these effects do not appear until 12 years and 14 years, repectively, after the merger occurs. These results at long horizons appear to confirm that the employment effects of a merger are driven by efficiency gains and not an increase in monopsony power as they are likely due to unobserved variations in the market that are unrelated to the merger.

Perhaps not surprisingly, the effects of system-joinings on employment levels are much smaller. System-joinings result in a small initial decrease in both RN and LPN employment, but this effect disappears after three years. In fact, the number of LPNs increases in the long-run. I find little to no effect on hospital wages in the period after a system-joining, also implying the initial employment decreases represent efficiency gains rather than increased buying power. Thus, it appears that while joining a hospital system may result in some early efficiency gains from labor reductions, they are much smaller and more temporary than those associated with hospital mergers. Of course, there may be other benefits from system-joinings, such as an increase in profit or financial stability.

The chapter is organized as follows. I discuss the theoretical framework in Section 1.2, and describe the data in Section 1.3. I explain my empirical model and present my results in Section 1.4. Finally, Section 1.5 offers some concluding remarks.

[2] The wage results presented here are only average hospital payroll per worker. That is, in using the AHA data, I do not observe the extent of the true change in nurses' wages. To deal with this limitation, I supplement my wage data with the National Sample Survey of Registered Nurses and the Bureau of Labor Statistics (BLS) Occupational Employment and Wage Estimates. I discuss this data in further detail in Section 1.3.3.

1.2 Theoretical Framework

My theoretical framework identifies the causes of employment effects, if any, following a hospital consolidation. There are two reasons to expect employment effects following consolidation. First, increases in efficiency resulting from consolidation may require that hospitals reduce labor. Second, an increase in monopsony power due to a consolidation can also give rise to reductions in labor. Note that the effects of the two forms of consolidation, mergers and system-joinings, may differ depending on the extent to which they lead to monopsony power in the labor market or give rise to efficiency.

When two hospitals merge, they come together to operate under a shared license. This is a form of horizontal integration. The efficiency benefits from doing so can include eliminating duplicate patient care departments or simply consolidating administrative or legal staff. Interviews with industry insiders indicate that system-joinings may allow for similar forms of efficiency gains depending on the location of the two hospitals. In other words, joining a system can mimic the effects of a merger if two hospitals are located in the same labor market.[3]

Even though system-joining does not result in a shared license, it is plausible that we could see similar employment effects of mergers and system-joinings. For example, two hospitals in the same market that are in the same system may be less likely to compete over workers than two hospitals that are not in the same system. In addition, two hospitals in the same system that are located close to each other may benefit from administrative efficiencies or the ability to allocate patients more efficiently across hospitals.

1.2.1 Increase in Monopsony Power

The consolidation of two hospitals may cause a change in competition in the labor market, especially the market for specialized labor. In particular, a merger may increase the bargaining power of the employer and thus allow them to reduce worker wages. If a hospital is in an imperfectly competitive labor market, it faces an upward-sloping labor supply curve. A merger resulting in increased buying (or monopsony) power on the part of the hospitals would lead the merged hospital to restrict employment below the competitive level and pay wages below the marginal revenue product of the employee (see the Appendix, Section 1.6.1). Thus, we would expect to see decreases in employment and wages at the newly consolidated hospital.

Furthermore, suppose there are labor-market effects at consolidating hospitals, and these effects are due to a decrease in employment competition on the demand side. There should also be employment effects at the hospitals in the same labor market that do not partake in any form of hospital consolidation. This is

[3]The same would be true for potential effects of system-joinings on increased prices if hospitals are in the same product market. In fact the two studies that do examine system-joinings, Cueller and Gertler (2005) and Melnick and Keeler (2007), find higher prices and an increase in market power of system-affiliated hospitals versus non-system hospitals.

because the consolidation affects the competitiveness of the entire labor market, not just for hospitals that consolidate.[4] The other hospitals in the market, particularly those located most closely to the consolidating hospital, should also see an increase in their buying power. The expectation, therefore, is that rival hospitals in the same labor market as the consolidating hospital will also see a decrease in employment and wages if consolidation results in an increase in monopsony power.

Testable Implication 1: If a hospital merger or system-joining results in a decrease in competition in the nursing labor market, then all else equal, average market wage and number of nurses at the consolidating hospital will decrease. In addition, there should be a decrease in the number of employees and wages at non-consolidating local, i.e. rival, hospitals.

1.2.2 Efficiency Gains

Hospital consolidation may also lead to increased efficiencies. When a hospital consolidates, scale economies may be realized. For example, consider a merger between two hospitals in the same local health care market. These hospitals may be able to do one or more of the following: close down an under-utilized department that is present in both hospitals pre-merger; share administrative personnel or legal staff; share technologies that decrease demand for certain types of labor. A hospital that joins a system may also be able to enjoy these efficiencies, but perhaps less so to the extent that having a separate license creates a barrier to horizontal integration.

In the case of an increase in efficiency, we would expect wages to stay the same because nothing has happened in the labor market, so the competitive wage is unaffected.[5] There is also a possibility that wages could increase if the efficiency gains result in the hospital being more profitable. If this increase in profit is associated with an increase in the marginal product of labor (MP_L), then wages may rise as employees' wages are adjusted to this higher MP_L. Alternatively, employees acting collectively, or a labor union, may be able to successfully bargain for a share of the higher profit generated post merger or system-joining.[6]

Testable Implication 2: If a hospital merger or system-joining results in increased efficiencies, the number of employees should decrease or output should increase. If a hospital merger or system-joining results in

[4] E.g. Dafny (2009) finds an increase in prices at rival hospitals.

[5] It is important to note that this framework assumes a flat supply curve of labor. If the supply curve is upward sloping, and an increase in efficiency leads to a decrease in demand for labor, the competitive wage would decrease.

[6] Unfortunately, this is not something I can measure as I do not have data on unionization at the hospital level. This is necessary since nurse bargaining occurs at the hospital level, that is, a union bargains over wages with a specific hospital, even after a hospital has joined a hospital system. For instance if two hospitals join a larger hospital system, say Kaiser, the bargaining over wages does not happen with Kaiser as a whole, it stays at the individual hospital.

increased efficiencies, assuming flat supply, then wages should stay the same. If the source of efficiencies (e.g. a positive change in capital intensity) further enhance MP_L, wages might go up. Finally, rival hospitals should see no change in number of employees or wages.

1.3 Data and Descriptive Statistics

The main dataset used in this chapter is the American Hospital Association's (AHA) Annual Survey of Hospitals (1983-2009). I supplement the wage information in the AHA data with data from the National Sample Survey of Registered Nurses (NSSRN) (1984-2004) and the Bureau of Labor Statistic's Occupational Employment and Wage Estimates (1997-2009).

1.3.1 The American Hospital Association's Annual Survey of Hospitals

The AHA annual survey provides data for 98% of US hospitals, including location, ownership status, employment levels for certain occupations, operating costs, number of beds, admissions, and information on hospital system affiliation. The full sample of hospitals for the years 1983-2009 contains 176,596 hospital-year observations. I restrict the sample to those hospitals that are classified as "general medical and surgical," thus leaving out psychiatric hospitals and other specialty centers that might compete for a different type of employee or have a different propensity to merge or join a system. This reduces the sample to 7,149 hospitals and 143,057 hospital-year observations.

The AHA's annual survey also provides a "summary of changes." Included in this summary of changes are notes concerning hospital mergers and a classification of each merger, i.e. two or more hospitals that merge to form a new hospital or one or more hospitals that merge into an existing hospital. The AHA tracks hospitals that merge as a single entity after mergers, that the two separate hospital share a single ID and appear as one hospital after the merger. The AHA merger data used in Dranove and Lindrooth (2003) was generously provided by the authors. I use the AHA information to expand on those data, which covered 1989-1997, and identify all mergers from 1983-2009.

I use the definition of the AHA when identifying mergers and system-joinings. A hospital merger, as defined by the AHA, is when there is a "full-asset merger" and two separate hospitals come together to operate under a shared license. For example, in 1996, Our Lady of Lourdes Hospital and Lutheran Community Hospital in Norfolk, Nebraska merged to form Faith Regional Health Services. Both of the original hospitals gave up their separate hospital licenses and began to operate under the same new license. Previous research has shown that hospitals located within 0.3 mi of each other are almost three times as likely to merge as hospitals that are further away from each other (Dafny, 2009). This close proximity of merging hospitals makes it easy for a hospital to condense duplicate and unnecessary operations and reallocate resources as needed.

When a hospital joins a system it retains its license although governing ownership may be transferred to a new governing body. It is important to note that a system-joining that results in full ownership transfer must be reported to the Federal Trade Commission (FTC) and is subject to review. No other type of system-joining needs to be reported. Still, they may be subject to antitrust scrutiny under Section 2 of the Sherman Act.[7]

While a hospital merger always implies a complete consolidation of hospital assets, joining a hospital system can be as weak as only adopting the "brand name" of the system or as strong as a full ownership conversion. The latter may be blocked by the FTC. For example, in 2012, the FTC prevented St. Luke's from joining the Promedica Hospital System. Even though it was not a traditional "full-asset merger," St. Luke's intended to turn their entire ownership over to Promedica.

Each type of consolidation may allow for a hospital to combine certain aspects of their operations. Both the acquiring and acquired hospital can benefit from consolidation. Hospitals that either join a system or merge are typically in dire financial straits pre-consolidation and are rescued from financial catastrophe. I show this in the Appendix, Section 1.6.2. I use cost per bed as a proxy for financial distress, and perform a probit analysis showing that hospitals that are targeted for either type of consolidation have a higher cost per bed, statistically significant at the 1% level (Table 1.11). Presumably, and according to industry insiders, the targeting hospital or existing system sees the poorly-run hospital as an opportunity to restructure, refurbish, and turn a profit.

Figure 1.1 shows that there were 637 mergers between 1983 and 2009, with a peak of 65 mergers in 1997. The total number of observations for hospitals that merged at some time in the data period is 14,617. Given that I will examine certain outcomes before and after a merger, I further restrict the analysis to those that I can see at least five years before and five years after merging. This leaves a final sample of at 11,846 hospital-years for most of my analyses of hospitals involved in a merger.

As for system-joinings, Figure 1.1 also shows that there were 5,449 of these between 1986 and 2009, with a peak of 520 system-joinings in 1989. Note that the AHA did not start collecting information on hospital systems until 1985. While some hospitals may have joined a system in 1985, it is unclear from the data if they were in a system in 1984. Therefore, when analyzing hospital systems, I focus on the years 1986-2009. As with hospital mergers, I restrict the analyses to those system-joiners that I can see at least five years before and five years after consolidation. The final sample of hospitals that join a system is 15,270.

[7] This information is from conversations with individuals from the FTC.

For hospitals that consolidate, I sum the data for the years prior to consolidation. For example, if hospitals A and B merge to form hospital C, I attach all observations to hospital 'C' and sum up the data for A and B prior to the merger. I similarly sum within systems in the same labor market (the "Component Economic Area", described immediately below) for all system-joinings.[8] In addition, I deal with multiple mergers and multiple hospital system-joinings (a very small subset of hospitals) by creating duplicate observations for those that consolidate multiple times. For instance, if a hospital merges in both 1992 and 1999, then it is combined with the other hospital and is in the dataset twice, appearing in the pre-merger period in the years leading up to 1992 as well as the years up to 1999.

1.3.2 Market definition

As my focus is on labor outcomes, I define markets using the Bureau of Economic Analysis's Component Economic Area (CEA). Each CEA consists of one or more metropolitan areas that serve as centers of economic activity, known as "nodes," and the surrounding counties that are economically related to each node (Johnson, 1995). Commuting patterns are the main factor used in determining the appropriate relationships among surrounding counties. CEAs, therefore, are meant to include workers' places of work and places of residence. Given that the CEA is based on commuting patterns workers are more likely to be mobile within a CEA than across them. Thus, it makes sense to define the labor market in which a hospital competes as the CEA.

There are 348 CEAs in the US, of very different sizes, with a median population of 286,415 and a mean population of 1.4 million. The number of hospitals in the AHA data in a CEA varies from 1 to 1400. CEA definitions are obtained from the Federal Communications Commission's website. Publicly-available U.S. census data, obtained using the American Fact Finder tool on census.gov, provides information on market population.

The hospital market concentration is measured using the Herfindahl-Hirshman Index (HHI). The HHI is calculated by squaring the market share of each firm competing in the market and then summing the resulting numbers. In this chapter, market share is calculated as the ratio of total beds in a hospital to total beds in the market. This is a commonly-accepted way of calculating hospital market share (Link and Landon (1975) and Kessler and McClellan (2005)).[9] I count hospitals in the same market that are in the same system as one hospital for the purposes of this calculation (see also Dranove, et al. (1992)). Table 1.1 shows that market concentration is slightly higher for the markets where hospitals that merge or join a system are located, but not markedly different from that of other hospitals. By definition, market concentration

[8]Summary statistics for system-joinings, however, are reported at the hospital level, not at the system consolidation level.

[9]I also calculate the HHI based on hospital admissions and it is highly correlated with the HHI based on beds. All results are robust to using this alternative definition of HHI.

also increases after both types of consolidation (Table 1.2), confirming that most system-joinings, like most mergers, tend to occur among hospitals in the same market.

1.3.3 Wage data

In the AHA survey, hospital administrators are asked to report all hospital and facility personnel who are on the payroll at the end of the reporting period, as well as total payroll expenses. Dividing total payroll expenses by total facility personnel yields an average salary, or payroll per worker, for hospital personnel.[10] Average hospital payroll is used as a proxy for nurses' wage at the hospital. Clearly, this measure captures more than nurses' wage. If either the composition of labor changes or the wages of other hospital personnel move in the opposite direction relative to nurses' wage post-consolidation, the wage changes experienced by RN and LPNs will be incorrectly measured. For example, suppose that after a hospital consolidation, the wages of RNs and LPNs decrease, while the wages of other hospital personnel increase. I will only observe the net change in average hospital payroll per worker and will not observe the extent of the true change in RN and LPN wages. To deal with this limitation, I supplement my wage data with the National Sample Survey of Registered Nurses (NSSRN) and the Bureau of Labor Statistics (BLS) Occupational Employment and Wage Estimates.

The NSSRN is a survey administered by the United States Department of Health and Human Services approximately every 4 years to RNs working in the U.S. Information on base salaries, bonuses, county where employed, job responsibilities, education and demographics of RNs is collected. Selected from the licensure lists in each state, the goal is to sample and estimate the characteristics of the registered nurses in the country. Each survey consists of about 1-2% of all RNs in the U.S. While some RNs are surveyed in multiple years, there are no unique identifiers across survey years, so there is no way to match their data over time.

RN hourly wages from this data source are constructed using total yearly salary (including bonuses but not benefits), hours worked per week, and number of weeks worked in a year. All wages are adjusted to 2004 dollars. Unfortunately, detailed demographic information is unavailable in the survey for privacy reasons. Instead, the data are broken down by demographic "group." Specifically, there are 10 age groups (less than 25, 25-29, 30-34, 35-39,...greater than 65). Assuming a minimum working age of 18 and a maximum age of 70, I take age to be the median of each of these groups. There are only two categories for race, namely "white" and "other," and education is stated by type of degree completed. I limit the sample to those nurses who work in a hospital and I remove observations where information is missing on key variables (salary, hours

[10] While the American Hospital Association provides information about the number of physicians employed by the hospital, these numbers are not reported in a uniform way: some hospitals choose to report only those physicians employed by the hospital and others report those that have admitting privileges at the hospital. Therefore, I restrict my analysis to RNs and LPNs.

worked, demographic control variables), leaving 109,575 observations. Summary statistics are presented in Table 1.2. While the sample analyzed is limited to those nurses who work in a hospital, summary statistcs for nurses who work in nursing homes and doctors' offices are also displayed for comparative purposes. This table shows that hospital nurses look quite different than those who work in nursing homes and doctors' offices. They earn a higher hourly wage and tend to be more educated. Additionally, 92% of hospital nurses report being involved in direct patient care while only 9% of nurses working in doctors' offices report the same. Mean comparison tests of hospital RNs with nursing home and doctor's office RNs show that the means of wage, education, race, direct patient care, percent of time spent on patient care, whether the nurse works full-time, and hours worked are all statistically different. Additionally, the data shows that the vast majority of nurses work in a hospital (approximately 80%).

A third source of wage data that I use is the Bureau of Labor Statistics (BLS) Occupational Employment and Wage Estimates. The Occupational Employment and Wage Estimates are put together through a mail survey of non-farm establishments. The sample is derived from the list of establishments maintained by State Workforce Agencies (SWAs) and is selected to provide an accurate representation of establishments from every metropolitan and nonmetropolitan area in every state, across all surveyed industries, and from establishments of different sizes. I have annual wage data at the market level for the years 1997-2009. Not every market is represented each year, so this is an unbalanced panel of market-level wages. Summary statistics for the wage data are presented in Table 1.3. The mean hourly wage of a registered nurse is $24.19 and this wage ranges anywhere from $7.68 to $53.38. The average registered nurse employment in an MSA is about 5,852 RNs.

The average yearly wage from these two data sources is quite a bit higher than the average hospital wage from the AHA. If we assume that a worker works about 2000 hours per year, than the NSSRN and BLS yearly salaries are approximately, $50,000 and $48,000, respectively. This is larger than the average hospital wage in the AHA of $29,000. Thus, the need for the alternative wage analyses and the NSSRN and BLS provide better proxies of the average wage of a registered nurse.

1.3.4 Dependent Variables

Summary statistics for hospitals that merged, those that joined a system, and all other hospitals are presented in Table 1.1. These statistics include all hospital observations, not just those observed five years before and after consolidation. Hospitals are larger and supply many more RNs and LPNs on average than those that do not. This higher observed employment level for hospitals that have merged are aggregated together for the purposes of these analyses. A more comparable mean is the employment to output ratio, which adjusts for the relative size of the hospital. RNs per admission and LPNs per admission are significantly lower for consolidating hospitals than for those that do not merge or join a system. This suggests that consolidating hospitals are allocating their employment differently from those that do not consolidate. In addition, organizational type, i.e. the proportion of not-for-profit, for-profit, and government-owned, are different between hospitals that merge or join a system and hospitals that do not partake in either form of consolidation. Almost 65% of hospitals that merge are not-for-profit hospitals, whereas only 41% of hospitals that join a system and 49% percent of hospitals that do neither are not-for-profit. The opposite is true for system-joinings, where hospitals that do change ownership are more likely to transition to for-profit following the system-joining.

Table 1.4 shows the outcome variables of interest five years before, one year before, one year after, and five years after both a merger and a system-joining. On average, there is a small increase in number of RNs and a moderate decrease in number of LPNs the year after a merger in comparison to the year before although when looking at five years before and after it appears that RNs are in decline before a merger and then increasing after. This is in contrast to system-joinings which are associated with a large decrease in the number of both RNs and LPNs one year after a merger, and these levels increase slightly 5 years after. Admissions decrease very slightly one year after a merger, but increase by almost 6 thousand when looking 5 years after a merger. System-joinings, however, see their decrease in admissions persist 5 years after. Payroll per worker increases a small amount after merging and slightly decreases after system-joining. Organizational type shifts a small amount for mergers indicating that some hospitals go from either being for-profit or government-owned to not-for-profit after system-joining. System-joinings experience a much bigger shuffling in organizational type, reflecting the recent trend of hospitals shifting from not-for-profit to for-profit (Selvam, 2012).

1.4 Empirical Analyses

To estimate the effects of hospital consolidation on employment outcomes, I use a difference-in-differences analysis, one for each type of hospital consolidation. Of course consolidation is not a random event, so I need to correct for selection. I deal with selection in the following ways. First, I conduct a difference-in-differences analysis and include a variety of control variables that are expected to affect the decision to consolidate as well as the outcome variable of interest. Second, I explore the short-term and long-term effects of the mergers and system-joinings both to see if changes immediately follow consolidation and if they persist, while controlling for the same set of factors included in the difference-in-differences estimation. Third, I model the probability of participating in hospital consolidation using observable hospital and market characteristics, and then use propensity score weighting to control for the propensity to consolidate. Fourth, and finally, I examine the effect on rival hospitals: as described in Section 1.2, if the merged hospitals have increased market power in the labor market, rivals should also benefit from the merger.[11] For the rival hospitals, exposure to a merger is more likely to be an exogenous event.

1.4.1 Difference-in-Differences Analysis: Mergers Only

In this section, as in the prior literature, I focus on the effects of hospital mergers only and compare them to hospitals that do not merge (Alexander et al., 1996; Hass-Wilson and Garmon, 2011). In other words, I ignore the possibility of joining systems in these analyses. Taking the hospital-year as the unit of observation, I restrict my sample to observations for hospitals that never merge, along with those that I observe at least five years prior and five years after their merger. I estimate the following difference-in-differences model as my main specification:

$$\ln(Y_{it}) = \theta AfterM_{it} + \beta X_{it} + \mu_i + \tau_t + \epsilon_{it} \tag{1.1}$$

where $AfterM_{it}$ equals one for periods after the merger for hospitals that merge.[12]

In equation (1), $\ln(Y_{it})$ represents the natural log of eight possible outcome variables of interest for hospital i in year t: the number of registered nurses (RNs), the number of licensed practical nurses (LPNs), the sum of the number of RNs and LPNs, the log number of admissions, the ratio of each of these employment outcome to number of admissions, and payroll per worker. From sectoin 2, whether efficiency or monopsony, theta is expected to be negative for the first six outcomes. For payroll per worker, however, if monopsonistic exploitation is present then theta will be negative as well.

I include (consolidated) hospital fixed effects, μ_i and year fixed effects, τ_t. I also include a vector of time-varying hospital and market control variables (X_{it}) that are expected to be related to the propensity to merge. These variables are:

Cost per bed:

This is a measure of financial distress. It is believed that hospitals that are most likely to want to consolidate and that are targeted for consolidation are in poor financial straits prior to merging and/or joining a system. Since I do not have data on hospital revenue, I cannot calculate hospital profit. I can, however, look at the ratio of costs to beds as a proxy for financial distress. Hospitals that are less financially sound should have higher cost per bed as they are using their resources less effectively. As shown in Table 1.1 and in Section 1.3, hospitals are more likely to consolidate with higher cost per

[11]This analysis is limited to mergers because there are not enough hospitals that are exposed to only one system-joining. Almost all markets experience more than one system-joining over the sample period.

[12]Recall that characteristics of merging hospitals are aggregated in the periods before and after the merger. The data, therefore, is at the consolidation-year level.

bed.

Ownership type (i.e. for-profit, not-for-profit, or government):

Hospitals with different ownership types have may have different objective functions (Newhouse, 1970). For-profit hospitals answer to shareholders who are looking to make money while not-for-profit hospitals are required to reinvest their profits in the hospital. I want to make sure, therefore, that I am comparing hospitals with similar incentives.

Current system membership:

Although we see in Section 1.3 that joining a system does not preclude merging and vice versa, we may think that those that choose to do both are different from those that do not.

Market population:

Prior research has shown that merging hospitals are more likely to be in urban areas (Patel, 2013).

Number of hospitals in the same zip code:

Dafny (2009) shows that hospitals that are located closer to each other are more likely to merge. Specifically, most hospitals that merge are located within 0.3 miles (as the crow flies) of each other. Unfortunately, I do not have exact distances among hospitals, but I do know how many hospitals are located within the same zip code. I would expect that having more hospitals within the same zip code makes a hospital more likely to merge.

Table 1.5 column (1) displays the results of estimating equation (1) for the different outcomes of interest. I only show the parameter for $AfterM_{it}$, so each cell of column (1) represents a separate regression. Below each estimate is its standard error clustered at the hospital level. The results show that those hospitals that merge see a 22.6% decrease in number of Registered Nurses (RNs) following a merger, when compared to all other hospitals. This result is even larger when looking at the effect on the number of Licensed Practical Nurses (LPNs) as the employment level falls 28.7% following a merger. These results are significant at the 1% level. The number of admissions falls by 22.5% following a hospital merger, and accordingly, the ratio of RNs to admissions does not change. The ratio of LPNs to admissions does fall, however. The average wage at a hospital also increases in the post-merger period, but the effect is not statistically or economically significant.

Consistent with the theoretical framework in Section 1.2, the wage result is evidence that the decrease in employment is due to gains in efficiency from the merger rather than an increase in monopsony power. Additionally, given that admissions are decreasing following a merger and the number of RNs per admission is staying constant, there is less of a concern that this decrease in employment will negatively affect the quality of care at the hospital.

1.4.2 Difference-in-Differences Analysis: Mergers and System-Joinings

I am also interested in studying system-joinings and directly comparing their effects to mergers. To examine the effects of both mergers and systems, I estimate the following equation:

$$\ln(Y_{it}) = \theta_1 AfterM_{it} + \theta_2 AfterS_{it} + \beta X_{it} + \mu_i + \tau_t + \epsilon_{it} \tag{1.2}$$

where $AfterS_{it}$ is equal to one in the periods after the system-joining. Like before, $AfterM_{it}$ is equal to one in the periods after the merger occurs. Note that because I am interested in the changes for systems as well, the sample is restricted to those hospitals that I observe at least five years prior and five years after their system-joining or merger, and all observations of those hospitals that never merge or join a system. I also include the same set of control variables as in equation (1) and again cluster standard errors at the hospital level.

The second and third columns of Table 1.5, present the results from estimating equation (2). The coefficients for mergers are slightly smaller in magnitude but largely similar, including the decrease in admissions and the zero wage effect. As for systems, we see that the coefficients are markedly different from those for mergers: all the coefficients are either highly insignificant or positive. These results show no effect of system-joinings on employment and a small effect on the ratio of RNs and LPNs to admissions. The positive and significant coefficient on each of these ratios suggests that while the number of RNs may be decreasing, the number available for each patient case the hospital addresses is actually increasing. Again, similar to the results in column (1), these results indicate that the decrease in employment may not result in a decrease in quality of care. A t-test for the difference in coefficients (not shown) of mergers and systems shows statistically significant differences at the 1% level for all coefficients. In other words, merging and system-joining result in very different employment effects.

1.4.3 Event Study

My main specifications will not allow me to see if changes in the outcome variable are slow moving or a sharp change at the time of the event. To examine this in greater detail, I estimate a model of separate treatment effects for years before and after a merger and a system-joining (Jacobson et al., 1993). Specifically, I estimate

$$\ln(Y_{it}) = \delta_{Merge_{it}} + \sum_{i=-5}^{5} M_{it}\theta + \beta X_{it} + \mu_i + \tau_t + \epsilon_{it} \tag{1.3}$$

where M_{it} are a set of dummy variables indicating each hospital's relative timing to a merger, where i ranges from five years before to five years after a hospital merger. The omitted category is the year before the event.

I then estimate a similar equation but, again, examine the effects of both mergers and system-joinings.

$$\ln(Y_{it}) = \delta_1 Merge_{it} + \delta_2 System_{it} + \sum_{i=-5}^{5} M_{it}\theta_i + \sum_{i=-5}^{5} S_{it}\lambda_i + \beta X_{mt} + \mu_i + \tau_t + \epsilon_{it} \tag{1.4}$$

After estimating equation (4), I test whether the coefficients are statistically different between those hospitals that merge and those that do no merge.

The results are presented graphically in Figures 1.2-1.4.[13] As before, all outcomes are measured in logs and these outcomes are depicted on the y-axes. In all figures, the x-axis represents the time relative to the event of merging or system-joining. The 95% confidence intervals are displayed above and below each coefficient line.

Figure 1.2 shows the different employment outcomes in relation to the timing of mergers. We see that for both RNs and LPNs, there is no effect pre-merger, yet a strong negative trend in number of RNs and LPNs after the merger. Both the number of RNs and the number of LPNs are continuing to trend downward even six years post-merger. We also see a large drop in the number of RNs and LPNs immediately at the time of the merger. It is important to keep in mind that these are hospitals that do not close after the merger. While I cannot see if a hospital ultimately closes a few years later, I am able to identify if the hospital closes at the time of the merger. These decreases in employment, therefore, are not the result of an immediate hospital closure.

As in the results in Section 1.4.1, there is a large decrease in admissions and there is no significant change in the ratio of RNs to admissions in the years following a merger. Therefore, while there is a clear decrease in RN employment, output per worker is continuing to rise. We do see a small decrease in the ratio of LPNs to admissions that is statistically significant 3+ years after a merger.

Combined with the employment to output results, the lack of a wage effect after a merger indicates that the decrease in employment is the result of an efficiency gain and not an increase in monopsony power.

Figures 1.3 and 1.4 display the results of the event study regressions where both mergers and systems are included in one specification. Figure 1.3 illustrates the merger coefficients while Figure 1.4 illustrates the system-joining coefficients. The effects of mergers on the number of RNs and LPNs, as displayed in Figure

[13]The numerical tables are available upon request.

1.3, are largely similar to those shown in Figure 1.2. This specification still shows a statistically significant decrease in admissions, and LPNs to admissions in the first couple of years following a merger. All of these results indicate some small efficiency gains following a merger, but they are not as long-term or as large in magnitude as those in Figure 1.2. This is because in Figure 1.2, most of the system-joinings are in the control group, where here they are explicitly accounted for. While there is a clear decrease in the number of RNs and LPNs following a merger there is no change in wages. Again, this result is consistent with post-merger efficiency gains, rather than an increase in monopsony power.

In Figure 1.4 we do not see any obvious jump in employment levels before and after system-joinings. While the results for RNs and LPNs are marginally significant at the 10% level, it is clear that they are very different in magnitude from the effects associated with hospital mergers. For example, the coefficient on RNs one year post-merger is -0.16, significant at the 1% level, whereas the coefficient on RNs for the period one year after system-joining is -0.05, significant at the 10% level. These effects entirely disappear after three years, and in fact, the coefficients become positive when looking 5+ years out from the time of the event. Once again we see no change in wage following a system-joining, continuing to rule out exploitation of market power as a reason for the change in employment levels.

A post-estimation test of the difference in coefficients for each year following a merger and each year following a system-joining shows that the coefficients are statistically different at the 1% level. For example, I can reject the null hypothesis that the difference in coefficients one year post-merger and one-year post system-joining is equal to zero.

1.4.4 Propensity Score Analyses

The difference-in-differences and event study results may be biased if certain pre-existing trends in the outcome variables differ across those hospitals that consolidate and those that do not. One way to mitigate this problem is to use propensity score weighting (Dranove and Lindrooth, 2003). In this section, I model the probability of consolidation based on certain observable hospital and market characteristics:

$$Merge_{it} = \beta_1 Z_{it} + \delta_1 X_{mt} + \epsilon_{it} \tag{1.5}$$

$$System_{it} = \beta_2 Z_{it} + \delta_2 X_{mt} + \epsilon_{it} \tag{1.6}$$

$Merge_{it}$ is an indicator with a value of one for hospitals that merge in time t and $System_{it}$ is an indicator with a value of one for hospitals that join a system in time t. Z_{it} and X_{mt} are a set of hospital and market-level variables that affect both the likelihood of merging and the outcomes. Namely, I weight on the population of the market, cost per bed, whether the hospital is in a system (for the mergers-only regression), the number of hospitals in the same zip code, whether the hospital is not-for-profit or for-profit, and concentration level of the market(HHI).

After re-estimating equations (1) to (4) correcting for selection bias using these weights, we see that the merger results are actually stronger. These results are presented in Table 1.6 and Figure 1.5. The effects of merging on employment are even stronger, with 30% and 36% decreases in the number of RNs and LPNs after a merger, significant at the 1% level. Once again, I find no effect on payroll per worker, further supporting the hypothesis that the reduction in labor is an efficiency effect rather than exploitation of buying power.

The estimations when examining both mergers and system-joinings are presented in Table 1.6 (Equation (2)) and Figures 1.6 and 1.7. Again, there is no effect on system-joinings on the number of RNs or LPNs in a hospital. The change in number of admissions following a system-joining is more negative and statistically significant. The coefficient indicates that admissions decrease by 9% following a system-joining. The effects on mergers after adding in system-joinings are relatively similar to those when estimating only equation (1).

Lastly, there is still no evidence of a wage effect due to hospital consolidation when looking at mergers or mergers and system-joinings.

1.4.5 Additional Analyses

So far the results point to an increase in efficiency post-consolidation and not an increase in buying power, at least with respect to the nurses labor market. Two other ways I check to see if this result holds are(1) explore two other sources of wage data and (2) examine other hospitals in the market that do not merge yet are exposed to a merger ("Rivals Analysis").

1.4.5.1 Additional Wage Data Analyses

As described in section 1.3.3, I only observe the net change in average hospital payroll per worker and do not observe the extent of the true change in RN and LPN wages. To deal with this limitation, I supplement these analyses using the NSSRN and BLS data. Specifically, I examine how the market wage of RNs change as the number of consolidations in a market increases by estimating the following equations:

Mergers Only:

$$\ln(Wage_{kt}) = \theta \# Mergers_{kt} + \beta X_{kt} + \mu_k + \tau_t + \epsilon_{kt} \tag{1.7}$$

Mergers and System-Joinings:

$$\ln(Wage_{kt}) = \theta_1 \# Mergers_{kt} + \theta_2 \# SystemJoinings_{kt} + \beta X_{kt} + \mu_k + \tau_t + \epsilon_{kt} \tag{1.8}$$

where $Wage_{kt}$ is the average log hourly wage of an RN in Component Economic Area (market) k in year t. When using the NSSRN data, I restrict the analysis to only examining the average market wage of RNs working in a hospital. The BLS data does not allow for this type of detailed stratification.

X_{kt} is a set of market control variables. Since the NSSRN provides some demographic information, I control for average age, race, gender and marital status of hospital RNs in the market. μ_k is a CEA-level fixed effect, τ_t is a year fixed effect, and all standard errors are clustered at the CEA.

The results from estimating equations (7) and (8) are presented in table 1.7. Regardless of the data source used, the effect of hospital consolidations on the wages of RNs is very small and highly insignificant. These results are consistent with the wage results obtained with the AHA data, and they continue to indicate that the effects of consolidation on employment are due to an increase in efficiency post-consolidation and not an increase in monopsony/buying power.

1.4.5.2 Rivals Analysis

Finally, I examine employment effects for hospitals that are exposed to a merger in the market but do not merge themselves, i.e. "Rival" hospitals. As discussed in Section 1.2, if the consolidation affects competition for labor, we may expect there to be employment spillover effects at rival hospitals after a merger. Following Woolley (1989), Connor and Feldman (1998), and Dafny (2009), I conduct a rival analysis where I estimate the effects of being exposed to a merger on the employment effects of interest.

Given that there is no evidence of wage effects thus far, the results indicate that the effects are efficiency driven. I can further test this by seeing if there are any employment effects at rival hospitals, i.e. those hospitals that are in the same market as a hospital that merges but do not merge. These hospitals experience the same change in concentration but should experience none of the gains in efficiencies as the merging hospitals. If there is monopsony power in the market, I would expect to see a decrease in number of RNs and/or LPNs at non-merging hospitals following a merger of other hospitals within the market. The key assumption here is that exposure to a merger creates an exogenous change in competition for RN services. I look at hospitals that are exposed to exactly one merger and compare them to hospitals that are not exposed to any mergers. Given that system-joinings are so common, I cannot find enough markets that are exposed to only one hospital that joins a system. Therefore, I conduct the rival analysis for only rivals of merging hospitals.

I perform a difference-in-differences analysis and estimate the following specification using OLS:

$$\ln(Y_{it}) = \alpha_0 + \beta_1 Before_{it} + \beta_2 After_{it} + \lambda X_{it} + \gamma_i + \theta_t + \epsilon_{it} \tag{1.9}$$

where $\ln(Y_{it})$ represents the natural log of two possible outcome variables for hospital i in year t: the number of registered nurses (RNs) and the number of registered nurses plus licensed practical nurses (LPNs) nurses employed by hospital i in year t. $Before_{it}$ is an indicator equal to 1 if the hospital is in the period before "exposure" to a merger and $After_{it}$ is an indicator equal to 1 if the hospital in a period following "exposure" to a merger. Indicators for the amount of years since the merger occurred are alternatively used in place of the aggregated indicator $After_{it}$. X_{it} is vector of the same time-varying hospital and market control variables as used in equation (1). Year fixed-effects (θ_t) are included and the regressions are run with hospital fixed-effects (γ_i) unless otherwise indicated. Standard errors are clustered at the hospital level.

The results of the pre- versus post-merger effects are reported in Table 1.8. Being in the post-merger period does not appear to be associated with a change in the number of RNs or LPNs employed. As expected, being in the pre-merger period is also not associated with any difference in number of RNs or LPNs at a hospital.

I also conduct more of a traditional event study, like that in Section 1.4.3, on rival hospitals and present these results in Tables 6 and 7. Table 1.9 presents a condensed event study where the intervals are in groups of five years (e.g. 1-5 years since merger, 6-10 years since merger,...), and Table 1.20 presents the results of having individual indicators for each year since the merger occurred (e.g. "1 year since merger, 2 years since merger, ...).

The fixed-effects specification in Table 1.9 indicates that being 11-15 years removed from exposure to a merger is associated with having 4.4% fewer nurses and this grows to 5.3% fewer nurses when 16-20 years removed (significant at the 5% level and 10% level, respectively). The further disaggregated indicators (Table 1.10) show that this effect is concentrated in years 12-17, with the largest coefficient coming in year 15. This means that exposure to a merger is not associated with a reduction in RN employment until 12 years after the merger occurred. While it is possible that the effects of an increase in buying power could take 10+ years to occur, it is unlikely. These results at long horizons are probably due to other unobserved variations in the markets, rather than effects directly related to mergers. These results, therefore, appear to confirm that the employment effects of a merger are driven by efficiency gains and not an increase in monopsony power.

The rival event study results are similar for LPN employment levels. The fixed-effects specification in Table 1.9 does not show a significant effect until 16-20 years after exposure to a merger, with a coefficient of -0.08. The further disaggregated indicators (Table 1.10) show that this effect is concentrated to years 14-16, with the largest coefficient coming in year 16. This means that exposure to a merger is not associated with a reduction in LPN employment until 14 years after the merger occurred. Again, these results confirm that the effects of a merger on LPN levels are driven by efficiency gains and not an increase in monopsony power.

1.5 Conclusions

Previous studies of hospital merger effects have focused on prices, costs, and patient outcomes. There is, however, reason to believe that mergers may also affect labor market outcomes as well. First, hospitals may see an increase in efficiency through shared knowledge or facilities. Second, a merger may create a change in labor market competition and lead to an increase in employer monopsony power. The latter has been a concern for the market for nurses due to their perceived relative immobility and specialized labor. Prior studies of monopsony typically look at a change in hospital market concentration on the market wages of nurses. None of these studies, however, exploit mergers as a source of change in competition. In this chapter, I examine the effects of hospital mergers and system-joinings, which have been largely ignored in the literature, on the employment outcomes of Registered Nurses and Licensed Practical Nurses. I find a clear reduction in employment levels following a merger but no change in wages. These results are consistent with efficiency arguments for the mergers and system-joinings and not with increases in buying power. In other words, some of the cost reductions documented in the previous literature (Dranove et al, 2003;Harrison, 2011) take the form of a reduction in the employment of nurses. Given these reductions, it would be interesting to examine other subsets of hospital staffing, to see what workers, if any, are replacing these nurses.

I also examine the effects of hospital system-joinings on the same labor market outcomes. System-joinings are another popular type of hospital consolidation, yet have largely been ignored in the hospital consolidation literature. While I do find some small RN and LPN employment reductions after a system-joining, these results are concentrated in the first three years and disappear completely by year five.

As consolidation is not a random event, I address this selection problem through the use of control variables and propensity score weighting. There may be factors that I do not observe that still affect the probability of consolidating and are correlated with the outcome variables. For instance, profit would be a preferable measure of financial distress than cost per bed, but I do not observe revenues. I also cannot unambiguously rule out monopsony power as a cause for the reductions in RN and LPN employment since I do not observe RN and LPN wages at the hospital level, only average hospital wages across all employees.

Despite these limitations, the results provide the first evidence of employment level effects following a hospital merger and support the previous studies that do not find evidence of monopsony power in the market for nurses (Hansen, 1992; Hirsch and Schumacher, 1995; Hirsch and Schumacher, 2005). Hospital monopsony power has long been thought to be a problem in the nurse labor market. Given that I examine 27 years of data, three different wage sources, and am able to identify a real change in market competition by examining almost the entire of population of hospital consolidations, it is likely that the monopsonistic exploitation is not as prevalent as once believed.

While this study makes no statements on net welfare effects since there is no analysis of price or patient outcomes, it does point to the need for including employment effects in an overall welfare analysis of hospital consolidations. A full welfare analysis of any hospital consolidation should not only include price effects and changes in patient outcomes but also how employment changes and how these effects contribute to patient outcomes. On the other hand, given that I also find a decrease in number of admissions following consolidation as well as no change in the number of RNs per admissions, it may be the case that these employment decreases do not affect quality of care.

Finally, although I do not find employment effects attributable to system-joinings, it does not rule out that some system-joinings may experience similar effects to those of a hospital merger. Since system-joinings cover such a broad range of consolidation, in order to get a more complete picture of how system-joinings affect employment levels in hospitals, it would be necessary to identify those system-joinings that most closely mimic a merger. It is possible that these system-joinings that partake in a greater form of consolidation (i.e. those whose consolidation most closely resembles a merger) see greater employment effects.

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Figure 1.1: Number of Hospital Mergers and System-Joinings: 1983-2009

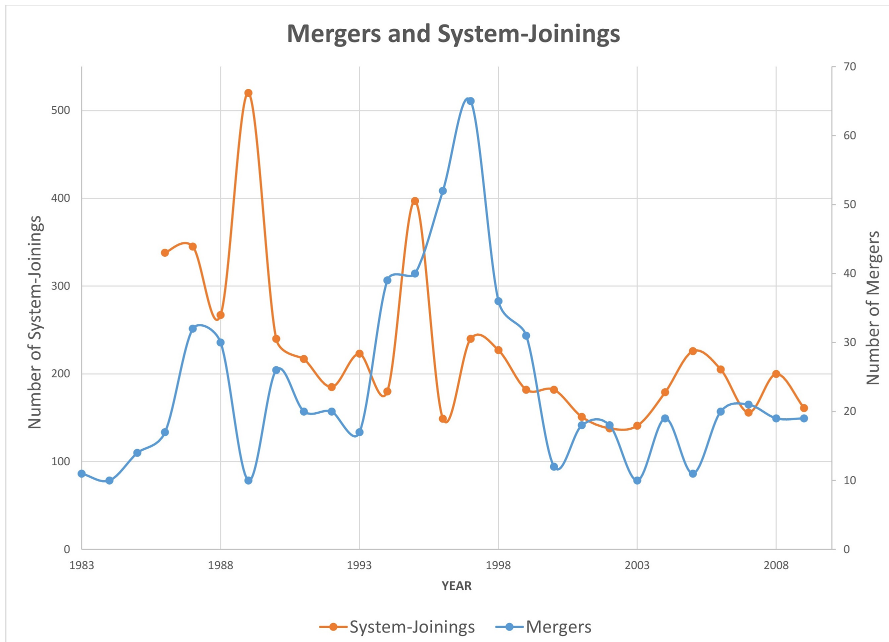


Table 1.1: Summary statistics of all hospitals

	Merging Hospitals	System-Joining Hospitals	Other Hospitals
RNs	503.84	220.51	155.04
LPNs	83.26	35.73	31.67
RNs+LPNs	587.10	256.25	186.71
RNs/1000 Admissions	23.04	33.33	34.35
LPNs/1000 Admissions	5.61	10.67	24.90
RNs+LPNs/1000 Admissions	28.65	31.01	59.26
Payroll per Worker ($000s)	28.08	28.47	22.95
Admissions (000s)	24.92	10.23	8.64
Not for Profit (%)	64.85	41.18	49.38
For-profit (%)	18.20	29.04	11.29
Government (%)	16.83	29.77	39.32
HHI (beds)	995.38	981.95	966.95

Table 1.2: NSSRN Summary Statistics

	Hospital		Nursing Home		Doctor's Office	
	Mean	S.D.	Mean	S.D.	Mean	S.D.
Log wage	3.14	0.39	2.97	0.42	2.99	0.50
Hourly wage	24.99	15.52	21.52	16.57	22.50	16.42
Hours per week	35.86	10.87	35.13	11.58	33.83	11.74
Weeks per year	50.33	5.44	49.94	6.49	49.84	6.32
Age (Median)	39.14	10.47	45.66	11.42	42.98	10.13
Male	0.18	0.38	0.16	0.37	0.23	0.42
Race = White	0.90	0.30	0.92	0.27	0.94	0.24
Married	0.69	0.46	0.69	0.46	0.79	0.40
Widowed/Divorced/Separated	0.14	0.35	0.20	0.40	0.13	0.34
Never married	0.15	0.35	0.10	0.30	0.07	0.25
Any children?	0.90	0.30	0.89	0.31	0.86	0.35
Full time	0.71	0.45	0.68	0.47	0.64	0.48
RN Diploma	0.34	0.47	0.47	0.50	0.41	0.49
Associate's Degree	0.38	0.49	0.38	0.48	0.33	0.47
Bachelor's Degree	0.27	0.45	0.15	0.35	0.25	0.44
Master's Degree	0.00	0.04	0.00	0.04	0.01	0.07
Involved in Direct Patient Care	0.92	0.27	0.03	0.17	0.09	0.29
% time spent on administration	8.40	18.89	19.32	26.91	11.03	19.94
% time spent on consultation	5.16	11.03	7.16	12.76	8.13	13.37
% time spent on patient care	69.67	34.07	38.23	33.96	68.97	31.23
% time spent on supervising	10.70	19.34	26.61	26.20	5.67	13.54
% time spent on research	1.41	6.84	1.62	6.69	1.89	8.62
% time spent on teaching	3.36	10.06	4.59	12.87	1.82	6.63
% time spent on other duties	1.17	8.06	2.23	11.37	2.33	11.68
Household income ¡ 15K	0.01	0.10	0.04	0.18	0.01	0.11
15K ¡ Household income ¡ 25K	0.07	0.26	0.11	0.31	0.05	0.22
25K ¡ Household income ¡ 35K	0.14	0.35	0.17	0.37	0.10	0.30
35K ¡ Household income ¡ 50K	0.25	0.43	0.26	0.44	0.20	0.40
50K ¡ Household income ¡ 75K	0.28	0.45	0.24	0.43	0.27	0.45
75K ¡ Household income ¡ 100K	0.15	0.35	0.12	0.32	0.18	0.38
100K ¡ Household income ¡ 150K	0.08	0.27	0.06	0.23	0.12	0.33
Household income ¿ 150K	0.02	0.15	0.01	0.11	0.06	0.24
Observations	109575		12682		14551	

Table 1.3: Summary Statistics of BLS Wage Data: 1997-2009

	Observations	Mean	S.D	Min	Max
Mean Wage	4270	24.19	5.49	7.68	53.38
Median Wage (50th Percentile)	4270	23.65	5.52	7.21	55.88
Hourly Wage of 10th Percentile	3952	17.81	3.87	5.54	33.88
Hourly Wage of 25th Percentile	3952	20.62	4.52	6.10	45.16
Hourly Wage of 75th Percentile	3951	28.04	6.47	8.34	63.39
Hourly Wage of 90th Percentile	3947	32.57	7.59	10.17	69.19
Total Employment of RNs (by MSA)	3660	5852.21	9514.59	60	99010

Table 1.4: Examining Hospitals Before and After a Merger or System-Joining

	Merging Hospitals				System-Joining Hospitals			
	5 Years Before	1 Year Before	1 Year After	5 Years After	5 Years Before	1 Year Before	1 Year After	5 Years After
RNs	506.65	479.38	482.21	589.76	306.54	328.68	173.30	189.96
LPNs	99.31	78.69	68.30	73.21	56.15	46.56	29.98	29.62
RNs+LPNs	605.96	558.07	550.51	662.97	362.70	375.24	203.27	219.59
Admissions (000s)	24.56	23.93	23.47	30.27	14.49	15.07	8.40	9.23
RNs/1000 Admissions	23.45	23.20	22.16	22.03	25.05	27.95	28.95	27.89
LPNs/1000 Admissions	6.08	5.68	4.78	4.36	8.00	6.73	9.40	8.23
Payroll per Worker($000s)	24.01	28.49	30.00	33.65	23.04	28.23	27.61	31.51
Not for Profit (%)	63.33	62.33	66.53	69.11	66.92	66.15	60.55	38.40
For-profit (%)	18.95	22.43	16.63	15.77	5.67	11.22	21.10	34.35
Government (%)	17.72	15.24	16.83	15.12	27.41	22.63	18.34	27.25
HHI (beds)	683.67	708.24	1129.26	1178.56	949.57	980.78	981.22	937.85

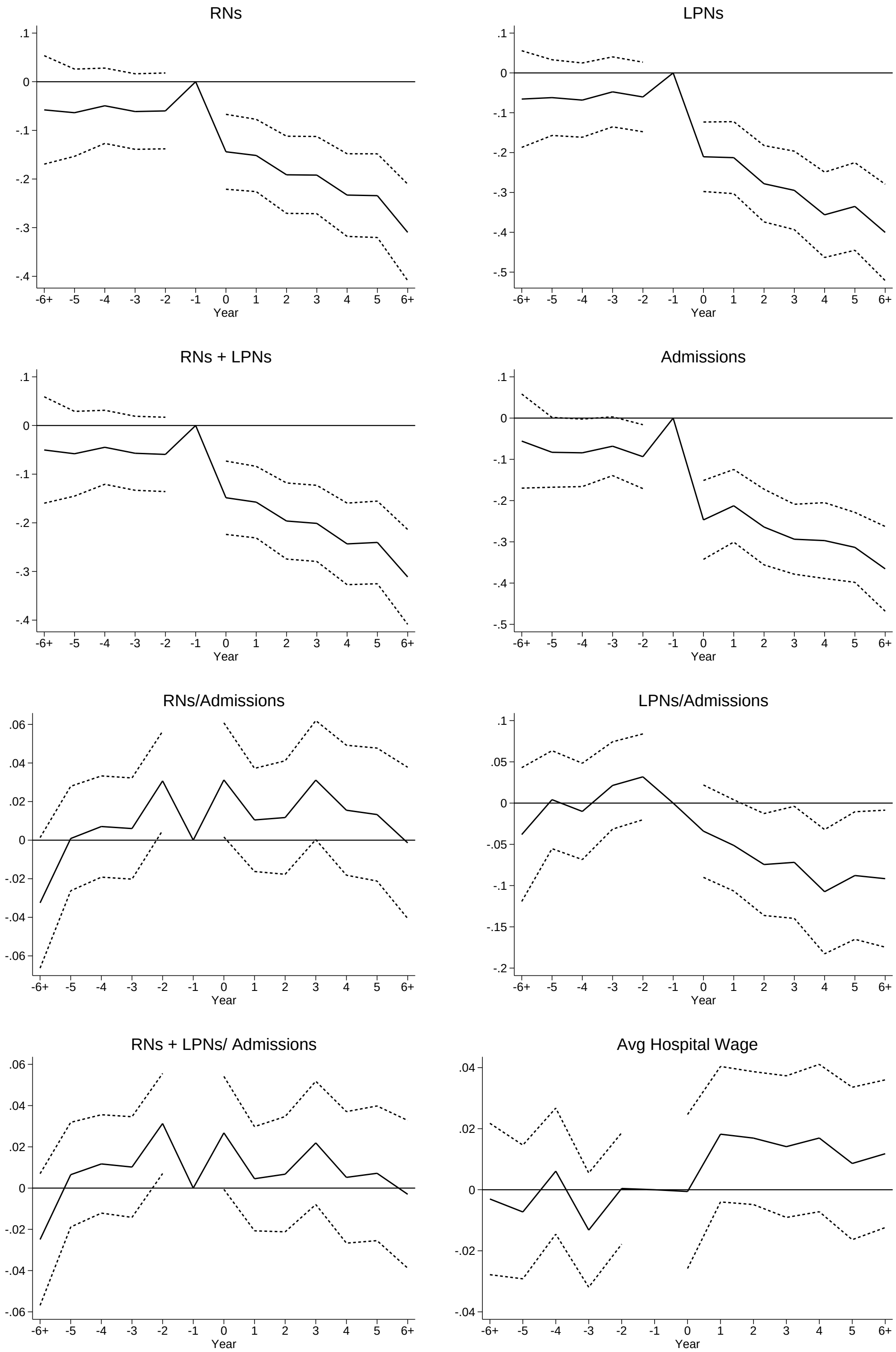
Table 1.5: Effects of Hospital Consolidation on Employment Outcomes: difference-in-differences Estimates

	Mergers	Mergers	Systems
Dependent Variables	Equation (1)	Equation (2)	
RNs	-0.226***	- 0.204***	-0.026
	(0.034)	(0.034)	(0.045)
LPNs	-0.287***	-0.270***	0.047
	(0.045)	(0.045)	(0.061)
RNs+LPNs	-0.233***	-0.212***	-0.007
	(0.034)	(0.034)	(0.044)
Admissions	-0.225***	-0.205***	-0.070
	(0.033)	(0.032)	(0.044)
RNs/Admissions	-0.001	0.000	0.044*
	(0.017)	(0.015)	(0.024)
LPNs/Admissions	-0.063**	-0.066**	0.115**
	(0.033)	(0.031)	(0.055)
RNs+LPNs/Admissions	-0.008	-0.006	0.062***
	(0.016)	(0.014)	(0.023)
Avg Hospital Wage	0.006	0.006	-0.008
	(0.010)	(0.011)	(0.012)

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

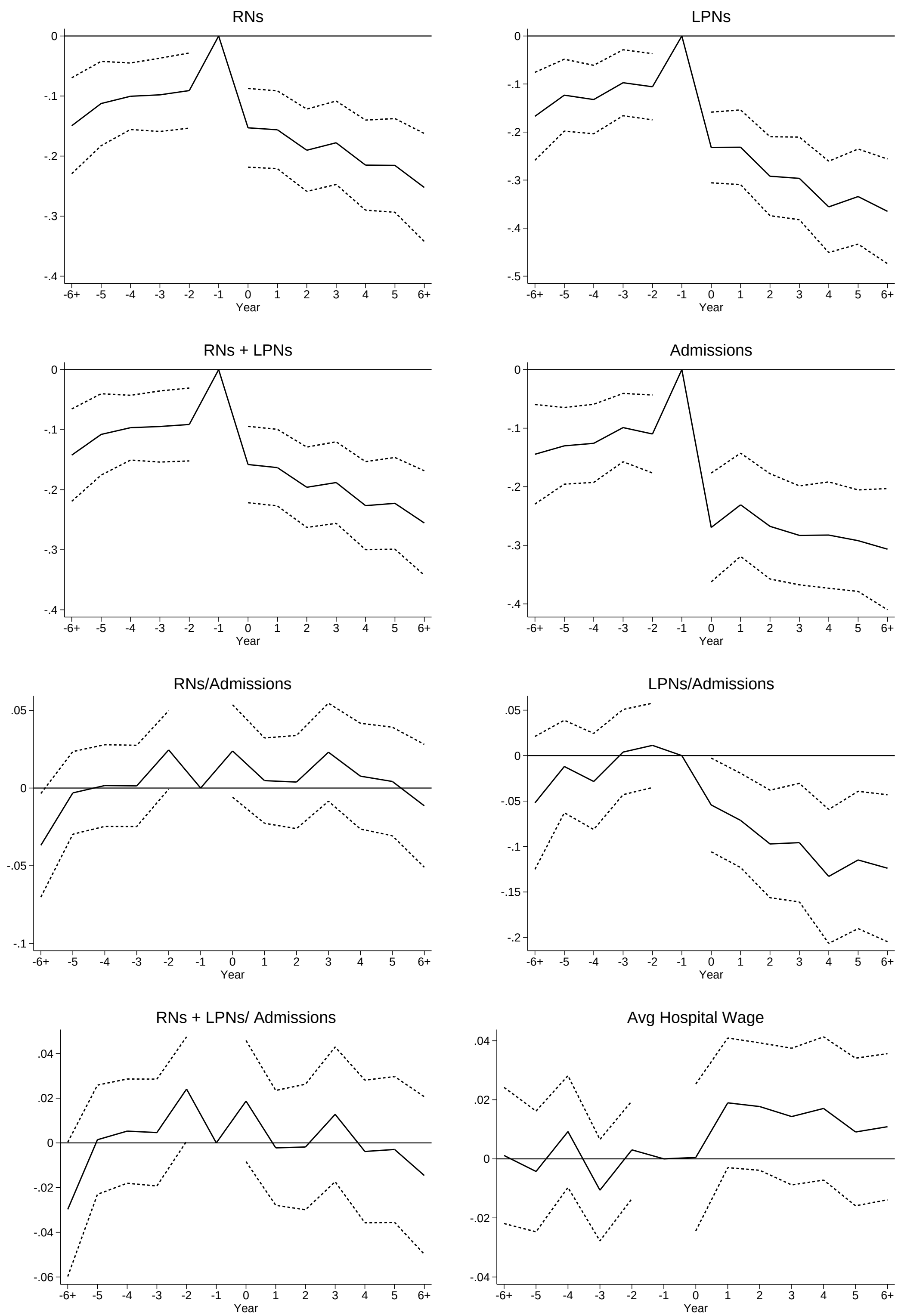
Each row in Column 1 represents a separate regressions, showing the coefficient for $AfterM_{it}$ only. Each row in the next two columns combined also represents a single regression, with coefficients for $AfterM_{it}$ and $AfterS_{it}$ reported in Columns 2 and 3, respectively. All regressions contain the following control variables: cost per bed, the number of hospitals in the same zip code, whether the hospital is not-for-profit or for-profit, market population, and concentration level of the market (HHI). Year fixed-effects are included in all regressions and standard errors are clustered at the hospital level.

Figure 1.2: Event Study Results: Effects of Mergers (Only Merger Effects Estimated)



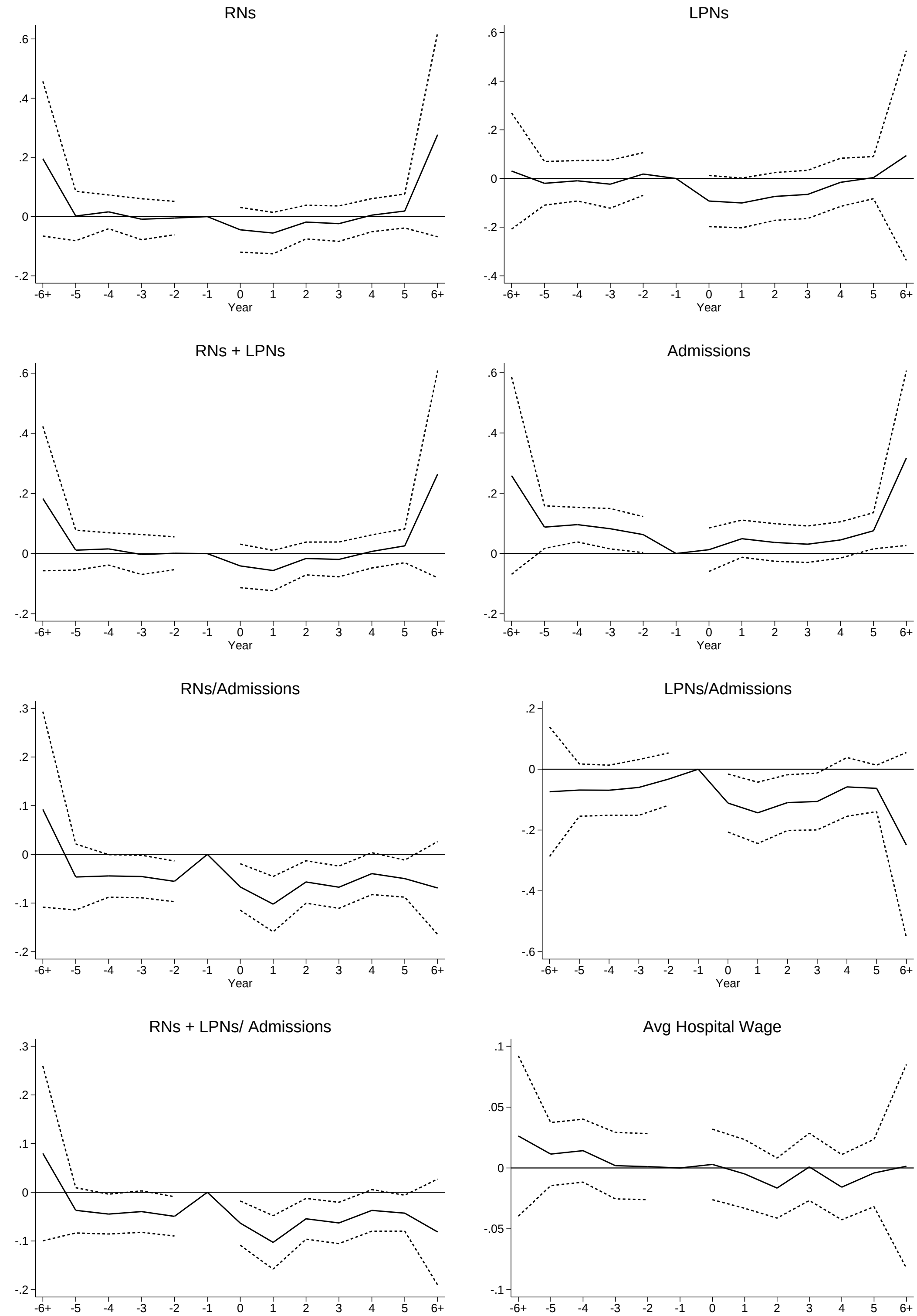
Graphs indicate the estimated effects and 95% confidence intervals for each year surrounding hospital mergers. The year prior to merger is omitted in all regressions. All outcomes are measured in logs.

Figure 1.3: Event Study Results: Effects of Mergers (Mergers and Systems Effects Both Estimated)



Graphs indicate the estimated effects and 95% confidence intervals for each year surrounding a merger or system-joining. The year prior to merger is omitted in all regressions. All outcomes are measured in logs.

Figure 1.4: Event Study Results: Effects of Joining Systems (Mergers and Systems Effects Both Estimated)



Graphs indicate the estimated effects and 95% confidence intervals for each year surrounding a merger or system-joining. The year prior to merger is omitted in all regressions. All outcomes are measured in logs.

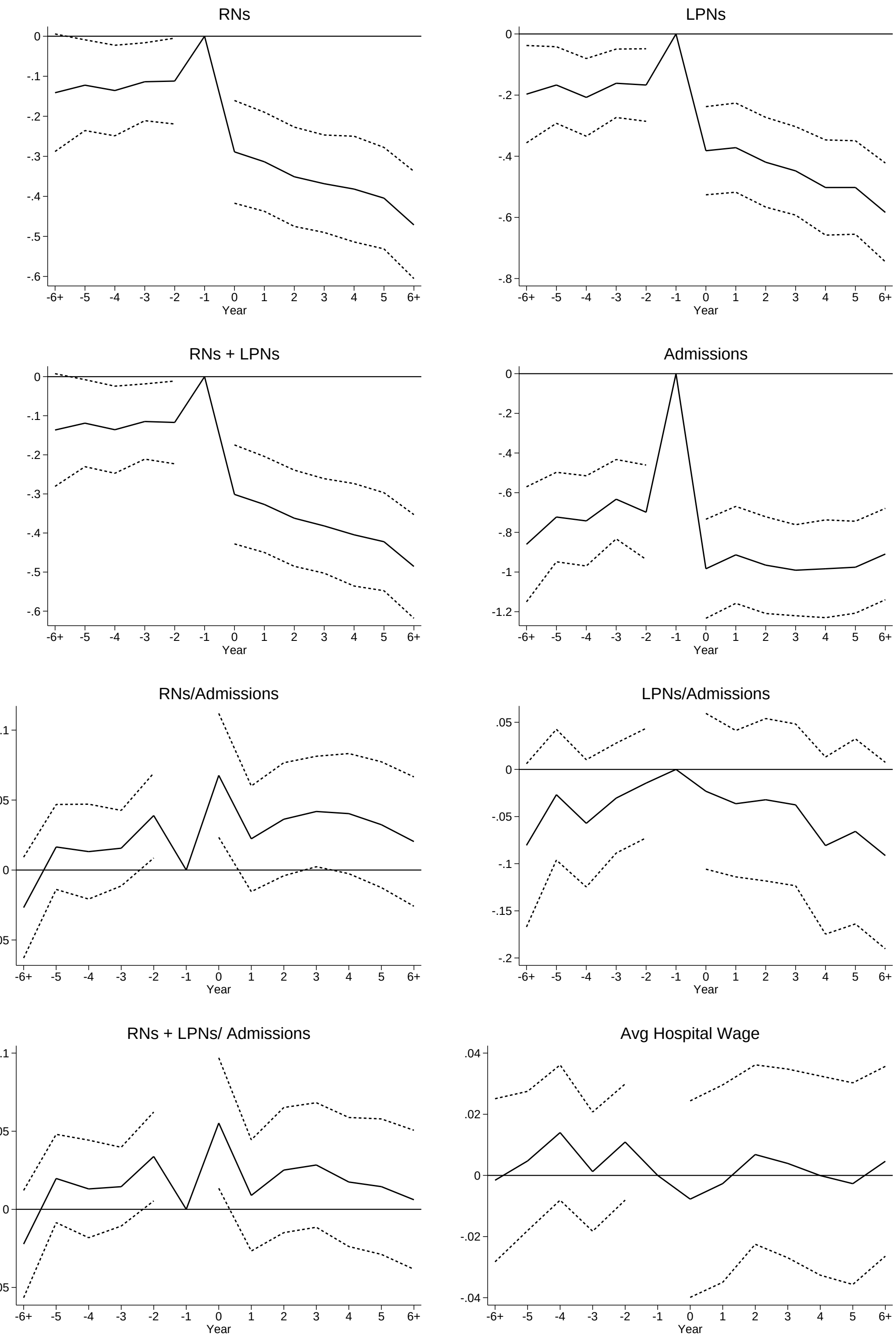
Table 1.6: Effects of Hospital Consolidation on Employment Outcomes: difference-in-differences Estimates, Weighted by Propensity Scores

	Mergers	Mergers	Systems
Dependent Variables	Equation (1)	Equation (2)	
RNs	-0.302***	- 0.300***	-0.041
	(0.040)	(0.041)	(0.050)
LPNs	-0.356***	-0.385***	0.034
	(0.050)	(0.051)	(0.066)
Admissions	-0.556**	-0.558***	-0.09**
	(0.081)	(0.081)	(0.048)
RNs/Admissions	0.008	0.005	0.047**
	(0.016)	(0.016)	(0.023)
LPNs/Admissions	-0.047	-0.081**	0.121**
	(0.034)	(0.034)	(0.055)
Avg Hospital Wage	-0.000	0.009	-0.008
	(0.012)	(0.011)	(0.013)

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

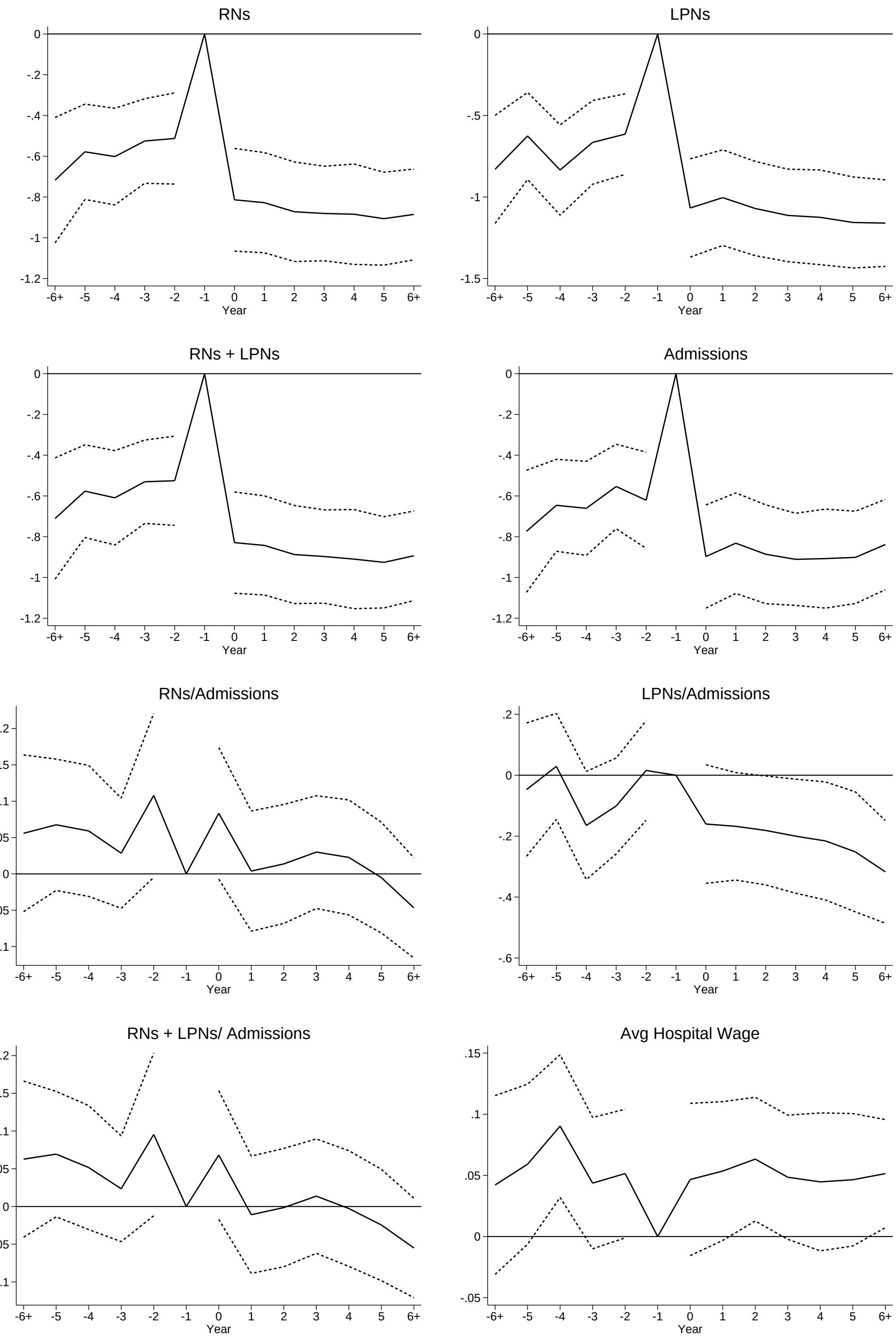
Each row in Column 1 represents a separate regressions, showing the coefficient for $AfterM_{it}$ only. All regressions contain the following control variables: cost per bed, the number of hospitals in the same zip code, whether the hospital is not-for-profit or for-profit, market population, and concentration level of the market (HHI). Year fixed-effects are included in all regressions and standard errors are clustered at the hospital level.

Figure 1.5: Event Study Results: Effects of Mergers (Only Merger Effects Estimated and Weighted by Propensity Scores)



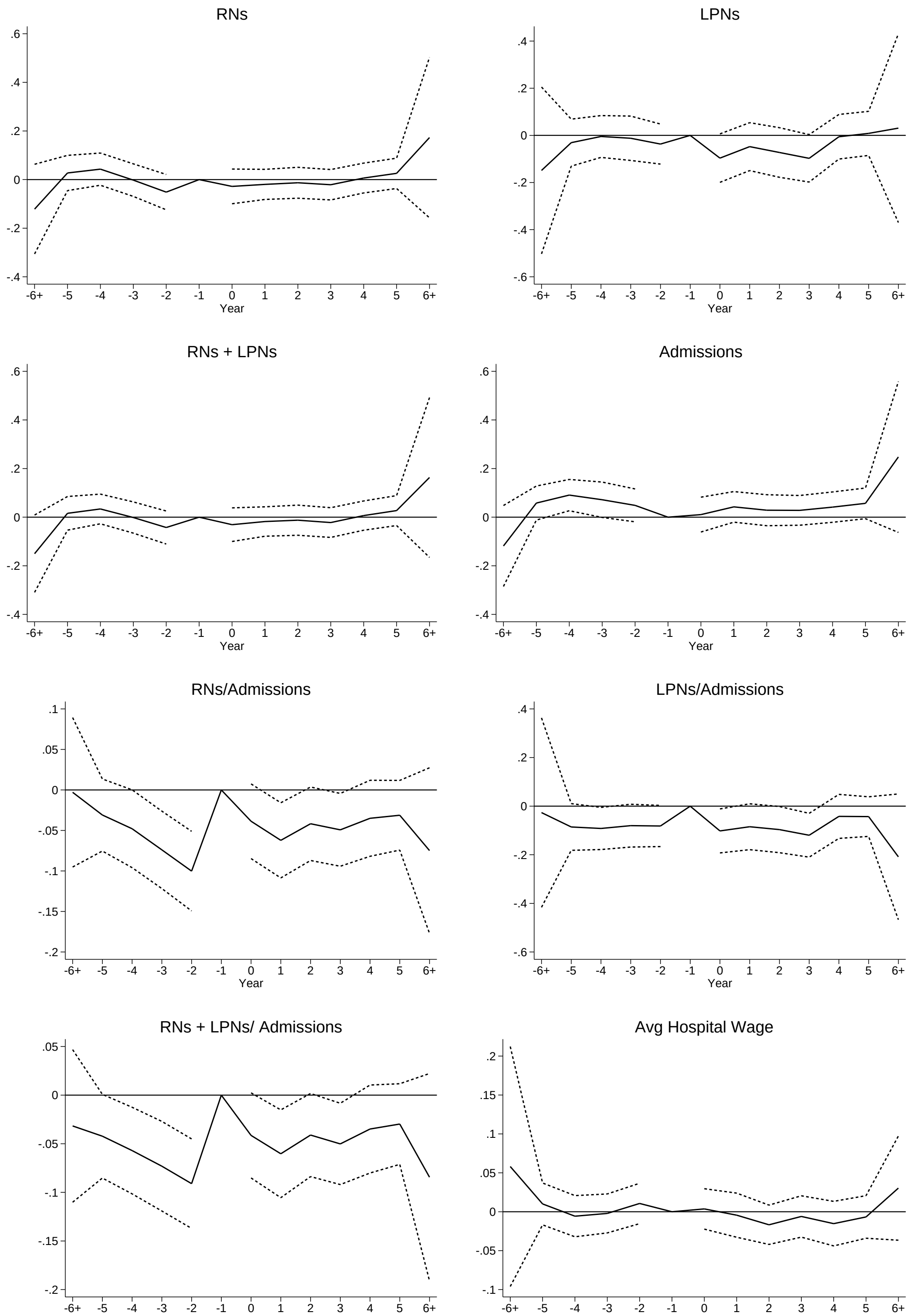
Graphs indicate the estimated effects and 95% confidence intervals for each year surrounding hospital mergers. The year prior to merger is omitted in all regressions. All outcomes are measured in logs.

Figure 1.6: Event Study Results: Effects of Mergers (Mergers and Systems Effects Both Estimated and Weighted by Propensity Scores)



Graphs indicate the estimated effects and 95% confidence intervals for each year surrounding hospital mergers. The year prior to merger is omitted in all regressions. All outcomes are measured in logs.

Figure 1.7: Event Study Results: Effects of System (Mergers and Systems Effects Both Estimated and Weighted by Propensity Scores)



Graphs indicate the estimated effects and 95% confidence intervals for each year surrounding hospital mergers. The year prior to merger is omitted in all regressions. All outcomes are measured in logs.

Table 1.7: Supplemental Wage Data Results using NSSRN and BLS data

	Mergers	Mergers	Systems
	Equation (7)	Equation (8)	
Market Log Wage of RN (NSSRN)	-0.005	-0.006	-0.003
	(0.010)	(0.034)	(0.004)
Market Log Wage of RN (BLS)	0.004	0.005	0.000
	(0.004)	(0.004)	(0.003)

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

The first row is the effect on RN wage using the National Sample Survey of Registered Nurses (NSSRN). The second row is the effect on RN wage using the Bureau of Labor Statistics Occupational Employment and Wage Estimates (BLS).

Table 1.8: Rival Analysis: Employment Effects on Hospitals Exposed to a Merger, Difference-in-Differences Estimates

	(1)	(2)	(3)	(4)
	RNs	RNs	LPNs	LPNs
Pre-merger	-0.0590	-0.0328	-0.0329	-0.0280
	(0.0397)	(0.0180)	(0.0480)	(0.0318)
After-merger	-0.0431	-0.0267	-0.0718	-0.0187
	(0.0268)	(0.0180)	(0.0528)	(0.0301)
Hospital FE		Yes		Yes

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

All regressions contain the following control variables: cost per bed, the number of hospitals in the same zip code, whether the hospital is not-for-profit or for-profit, market population, and concentration level of the market (HHI). Year fixed-effects are included in all regressions and standard errors are clustered at the hospital level.

Table 1.9: Event Study: Rival Analysis, Employment Effects on Hospitals Exposed to a Merger

	(1) RNs	(2) RNs	(3) LPNs	(4) LPNs
Pre-merger	-0.0325	-0.0100	0.0274	-0.0010
	(0.0289)	(0.0142)	(0.0401)	(0.0251)
1 to 5 years since merger	-0.0034	0.0112	0.0261	0.0237
	(0.0230)	(0.0134)	(0.0328)	(0.0216)
6 to 10 years since merger	-0.0101	-0.0096	0.0000	-0.0009
	(0.0268)	(0.0156)	(0.0358)	(0.0266)
11 to 15 years since merger	-0.0389	-0.0440**	-0.0221	-0.0320
	(0.0281)	(0.0159)	(0.0372)	(0.0311)
16 to 20 years since merger	-0.0394	-0.0532*	-0.1737**	-0.0804*
	(0.0364)	(0.0206)	(0.0537)	(0.0384)
Hospital FE		X		X

All regressions contain the following control variables: cost per bed, the number of hospitals in the same zip code, whether the hospital is not-for-profit or for-profit, market population, and concentration level of the market (HHI). Year fixed-effects are included in all regressions and standard errors are clustered at the hospital level.

Table 1.10: Event Study: Rivals Analysis, Employment Effects on Hospitals Exposed to a Merger

Years since merger:	RNs	RNs	LPNs	LPNs
< 0	-0.032	-0.009	0.027	0.001
	(0.029)	(0.014)	(0.040)	(0.025)
1	0.007	0.008	0.023	0.016
	(0.025)	(0.014)	(0.035)	(0.023)
2	0.008	0.020	0.030	0.034
	(0.024)	(0.016)	(0.036)	(0.025)
3	-0.006	0.011	0.012	0.015
	(0.025)	(0.016)	(0.036)	(0.026)
4	-0.022	0.009	0.047	0.038
	(0.026)	(0.016)	(0.036)	(0.026)
5	-0.007	0.010	0.019	0.019
	(0.027)	(0.017)	(0.038)	(0.028)
6	-0.005	0.003	0.019	0.021
	(0.027)	(0.017)	(0.037)	(0.030)
7	-0.008	0.004	-0.005	0.003
	(0.028)	(0.018)	(0.039)	(0.030)
8	-0.009	-0.013	0.005	0.003
	(0.029)	(0.018)	(0.040)	(0.030)
9	-0.018	-0.024	-0.015	-0.020
	(0.030)	(0.018)	(0.040)	(0.032)
10	-0.010	-0.020	-0.009	-0.014
	(0.031)	(0.018)	(0.042)	(0.035)
11	-0.040	-0.028	-0.025	-0.013
	(0.032)	(0.017)	(0.042)	(0.035)
12	-0.075*	-0.037*	-0.002	0.002
	(0.037)	(0.019)	(0.043)	(0.034)
13	-0.059	-0.039*	-0.009	-0.025
	(0.035)	(0.018)	(0.043)	(0.037)
14	0.020	-0.072***	-0.025	-0.084*
	(0.034)	(0.021)	(0.051)	(0.043)
15	0.003	-0.076**	-0.080	-0.108*
	(0.037)	(0.023)	(0.056)	(0.046)
16	-0.022	-0.074**	-0.147*	-0.116*
	(0.039)	(0.025)	(0.060)	(0.049)
17	-0.061	-0.062*	-0.162**	-0.083
	(0.044)	(0.024)	(0.063)	(0.048)
18	-0.038	-0.039	-0.147*	-0.082
	(0.043)	(0.025)	(0.065)	(0.050)
19	-0.021	-0.044	-0.249**	-0.094
	(0.046)	(0.026)	(0.077)	(0.059)
20	-0.059	-0.047	-0.184*	-0.024
	(0.044)	(0.029)	(0.073)	(0.056)
Hospital FE		X		X

All regressions contain the following control variables: cost per bed, the number of hospitals in the same zip code, whether the hospital is not-for-profit or for-profit, market population, and concentration level of the market (HHI). Year fixed-effects are included in all regressions and standard errors are clustered at the hospital level.

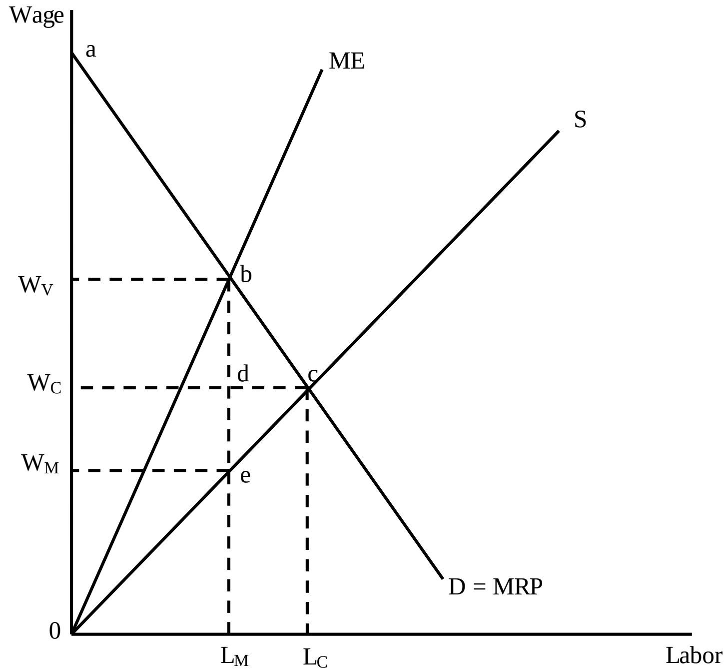
1.6 Appendix

1.6.1 A Simple Model of Monopsony

If a hospital faces an upward sloping supply curve of nurses, it is possible that monopsonistic behavior on the part of hospitals could explain changes in employment and wages. If a hospital has monopsony power, they will behave like the hospital in Figure 1.8.

For the hospital, profit maximization occurs where marginal revenue product (MRP) equals marginal expenditure (ME). Thus the monopsony employment is L_M and the monopsony wage is W_M. Keep in mind the hospital will only hire a nurse when there is value or revenue to be gained from the unit so the hospitals demand for each nurse is their marginal revenue product. Clearly the profit maximizing number of nurses and corresponding wage is less than the amount of nurses that would have been hired under competition, L_C and W_C, respectively. Additionally, it is worth noting that the monopsony wage of W_M is not only below the competitive wage of W_C, but well below the marginal value of labor, W_V. The difference between the marginal value and the wage paid is known as the *monopsonistic exploitation*.

Figure 1.8: Monopsony Labor Market



1.6.2 Characteristics Associated with Consolidation

These results show that a higher cost per bed is associated with an increased probability for both types of hospital consolidations. In addition not-for-profit hospitals and for-profit hospitals are more likely to consolidate than government-owned hospitals. Having another hospital in the same zip code is also associated with a higher probability to consolidate.

Table 1.11: The Effects of Hospital Characteristics on the Probability of Consolidating: Mergers and System-Joinings

	(1) Mergers	(2) System-Joinings
HHI(00s)	-0.0027***	-0.0013*
	(0.0006)	(0.0005)
cost per bed (000s)	0.0231***	0.0286***
	(0.0003)	(0.0002)
System Membership	0.7775***	
	(0.0175)	
Not-For-Profit	0.2768***	0.1673***
	(0.0136)	(0.0118)
For-Profit	0.5036***	0.1346***
	(0.0177)	(0.0168)
Number of Hospitals (in zip code)	0.0556***	0.0253***
	(0.0043)	(0.0043)
Constant	-1.9731***	-1.6396***
	(0.0147)	(0.0128)

Standard errors in parentheses

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

For ease of interpretation, HHI is divided by 100 and cost per bed is divided by 10000.

CHAPTER II

State Regulation and the Mobility of Nurses: An Examination of the Nurse Licensure Compact

2.1 Introduction

Occupational licensing is pervasive in the US and Europe. Nearly 29% of the US workforce requires a federal- or state-granted license to practice or work in their occupation (Kleiner and Krueger 2008; Thornton and Timmons, 2013) and over 800 occupations are licensed by at least one state (Kleiner 2000). While licensing is meant to protect consumers and ensure safety by certifying provider quality, it also may create rents for incumbent members of the occupation.

One mechanism through which licensing could restrict supply is by impeding geographic mobility. Since licensing and certification is primarily at the state level, workers typically must obtain a new license whenever they move across states before they are permitted to work. Similarly, workers typically must obtain separate licenses for each state in which they work, even if their jobs are identical. Such a barrier to mobility may prevent workers from seeking jobs across state lines, misallocating workers geographically and depressing employment and labor force participation. Moreover, licensed professionals may be less likely to move to areas of high demand.

In this chapter, we examine the impact of frictions imposed by licensing requirements on the labor market for nurses, exploiting a unique policy change that made it substantially easier for nurses to work and move across state lines. Nursing is an important occupation to focus on, as the availability of nurses is important for community health, hospital care, and disaster relief. Furthermore, an already acute nursing shortage is expected to increase over the next decade, as more nurses retire and the aging population increases demand for health care services (Buerhaus, Staiger, Auerbach, 2009). Insurance expansions embodied in the Affordable Care Act will only exacerbate this shortage. Removing licensing barriers is one mechanism to

This chapter is co-authored with Kevin Stange.

better utilize the existing supply of trained nurses. The ability for technology to improve health care delivery is also hampered by cross-state licensing barriers for health care workers, which makes telemedicine difficult legally (Sulentic 1991).

Surprisingly, compelling evidence on the impact of licensing on the geographic scope of labor markets is thin. Several studies, dating back a half-century, document a cross-sectional correlation between licensing restrictions and interstate mobility of professionals (Holen, 1965; Pashigian, 1979; Conrad and Dolan,1980; Kleiner, Gay, and Greene, 1982). A challenge with this cross-sectional analysis is that licensure practices may correlate with other unobserved state-level attributes that influence migration. Peterson, Pandya, LeBang (2014) address this problem by exploiting changes in residency training requirements for immigrant physicians within states over time, finding that states that impose more stringent requirements receive fewer immigrant physicians. We add to this literature by examining a recent policy change, the Nurse Licensure Compact (NLC), with a compelling research design that lets us control for several sources of bias that may confound previous estimates.

The NLC was introduced to reduce licensing burdens by permitting registered nurses living in member states to practice across state lines. It also made licensure easier to obtain for nurses moving between member states. Twenty-four states have implemented the NLC since its inception in 2000 and another 5 states currently have NLC legislation pending. We exploit the staggered adoption of the NLC across states and over time to examine whether a reduction in licensure-induced barriers is associated with a greater likelihood of cross-state commuting, longer travel time to work, and greater labor force participation of nurses. We estimate difference-in-differences models, comparing nurses in states adopting the NLC to those in states that do not. While this controls for time-invariant characteristics of states that may correlate with both labor market outcomes and licensure laws, state-specific time-varying factors may still bias estimates of the policy. A unique feature of our setting is that we are able to use non-nurse health workers (who are not affected by the NLC) to construct triple difference models to control for any health care labor market changes that may happen to correlate with NLC adoption. We find that this feature is important, as results from the triple difference models are substantively different than the basic difference-in-differences, suggesting a time-varying source of bias in the latter.

Using data on over 1.5 million nurses and other health care workers from the 1990 and 2000 Census and the 2006-2012 American Community Surveys, we find no effect of NLC adoption on labor force participation. Estimates are precise enough to rule out even small impacts on labor force participation. Thus, it does not appear that cross-state licensing restrictions impact the aggregate labor supply of nurses. However, we find positive effects of NLC adoption on travel time to work, particularly among nurses living in MSAs that encompass multiple states. This suggests that eliminating cross-state licensure restrictions expands the geographic scope of the nurse labor market. Though estimated effects on the likelihood of working across state lines are positive and large for multi-state MSAs, they are not statistically different from zero. This study provides some of the first evidence on the likely effects of nationalizing nurse licensure.

We provide background on nurse licensing and the NLC in the next section. Section 2.3 reviews the literature on occupational regulation. Our methods and data are described in Section 2.4. Results are discussed in Section 2.5 and Section 2.6 concludes.

2.2 The Nurse Licensure and RN Compact

In 1947, New York was the first state to require mandatory licenses for nurses. Today, every state requires a nurse to obtain a license to practice within the state (Benefiel, 2011) Obtaining a license typically means passing a licensing examination and meeting requirements that are set by each state individually. The exams for registered nurses and licensed practical nurses differ from each other and although the exams may differ by state, there has been a trend toward uniformity in recent years (Barnum, 1997). Besides an examination, there are monetary costs associated with obtaining an initial license in a state. For example, in the state of California, the examination registration fee is $200 plus the fee for verification of licensure ($60) plus the application fee of $100. If a nurse is licensed in another state, he/she must pass the California exam and pay all of the same application fees before obtaining a California nursing license.

The Nurse Licensure Compact (NLC) was first passed in 1999 by Utah and Arkansas, and Utah was the first to implement on Jan 1, 2000 along with Maryland, Texas and Wisconsin (This conflicts with the list which says Maryland was first to implement in 1999). This compact allows a nurse licensed in one NLC-member state to practice in other NLC states without obtaining a separate license for the other state. Each state that is a member mutually recognizes other member states. Since 2000, in total, 24 states have implemented the Nurse Licensure Compact and another 5 states currently have NLC legislation pending.

While 12 of the 24 states joined the compact in its first two years of existence, an average of 1-2 states per year have continued to join the compact since. Figure 2.1 displays the roll-out of the compact and how many states were part of the compact in 2000, 2004, and 2008. It is worth noting that each member state has another member state that is adjacent to it except for Rhode Island (although Massachusetts currently has legislation pending).

In order for a state to join the NLC, they must meet four requirements: 1) The bill language drafted by state legislators must mirror that of the "NLC Model Legislation," that is provided by the National Council of State Boards of Nursing; 2) The state legislature must pass the legislation; 3) The state board of nursing must implement the compact[1]; and 4) The state must pay $3000 per year to keep their membership in the NLC active. The NLC applies to registered nurses (RNs), licensed practical (LPNs) and licensed vocational nurses (LVNs) only.[2] To be eligible for a multistate license, a nurse's primary state of residence must be a compact-member state. As long as a nurse declares a compact state as a primary state of residence and the nurse is in good standing, the license automatically becomes a multistate license and the nurse can practice physically or electronically in other compact states. If a nurse works in a compact states but lives (i.e. has a primary state of residence) in a non-compact state, he/she is not eligible for a multistate license.

If a compact-eligible nurse permanently relocates to another compact state, that is, the nurse obtains a new driver's license in another state, changes the state where he/she votes, and/or files federal taxes in another state, the nurse must apply for licensure by endorsement and declare the new state as his/her primary state of residence. This must be completed within 90 days of moving although some states are currently in the process of amending the amount of time a nurse may practice with a license issued by another state. Figure 2.2 provides a flow chart explaining whether a nurse is eligible for a multi-state license.

Advocates of the NLC cite five main benefits. First, The NLC clarifies the authority to practice for many nurses currently engaged in telenursing or interstate practice. Second, the NLC provides greater mobility for nurses. (They cite the U.S. Department of Health and Human Services Health Resources and Services Administration's 2010 report, "Health Licensing Board Report to Congress" as evidence.) Third, the NLC improves access to licensed nurses during a disaster or other times of great need for qualified nursing services. Fourth, the NLC improves access to nursing care. Finally, the NLC enhances discipline and information-sharing among participating states.

2.3 Related Literature on Occupational Licensing

Relative to its prevalence, there is little research on the labor market effects of occupational licensing and restrictions.[3] Prior work on the labor market effects have focused primarily on wages, generally finding that restrictive licensing is associated with higher wages (Thornton and Timmons, 2013)[4]. Using unique data on the dental health of Air Force recruits, Kleiner and Kudrle (2000) find that restrictive licensing increases dental prices and earnings of dentists. Weeden (2002) finds that licensed occupations have higher wages, controlling for a whole host of other individual- and occupation-specific determinants of earnings, such as the

[1]The takes approximately one year from the time the bill is passed.

[2]Advanced Practical Registered Nurses (APRNs) do not fall under the NLC, but instead have their own separate APRN Compact that provides for a multistate license.

[3]Kleiner (2000) and Kleiner (2006) provide an overview of much of the theoretical and empirical literature on occupational regulation.

[4]There is also a very small literature on the effect of licensing on provider and service quality (Kleiner and Kudrle 2000; Angrist and Guryan, 2003) and output markets (Schaumans and Verboven, 2008; Hotz and Xiao, 2011; Kleiner, Marier, Won Park, and Wing 2011; Stange, 2014).

skill and task requirements of the job and education level. Kugler and Sauer (2005) find very large returns to acquiring an occupational license among immigrant physicians in Israel. A unique feature of their study is that they exploit variation in licensing that is driven by a policy that assigns immigrant physicians to different re-training regimes based on their experience. This represents an advance over much of the prior literature, which simply compared licensed with similar unlicensed occupations. More recently, Kleiner and Park (2010) and Kleiner, Marier, Won Park, and Wing (2011) find that changes in occupational regulations for dental hygienists and nurse practitioners, respectively, increase wages for these occupations.

While the evidence of licensing's effect of wages is robust, there is little direct evidence on whether reduced labor mobility is the primary channel. Several studies document a cross-sectional correlation between licensing restrictions and interstate mobility. Fifty years ago, Holen (1965) found that the in-migration of dentists, lawyers, judges, physicians, and surgeons was higher in states that had easier re-licensing. Pashigian (1979) found that the in-migration of lawyers was hampered by restrictive licensing. Kleiner, Gay, and Greene (1982) examined mobility in 14 different occupations as it relates to licensure restrictiveness. They found that states with less restrictive licensing and easier endorsement from other states have higher rates of in-migration. Thus restrictive licensing creates a barrier to mobility, misallocating workers across states. Thornton and Timmons (2013) add to this evidence by showing that the occupational regulation of massage therapist through state licensing appears to reduce the number of massage therapists, while Zapletal (2014) finds no effect of occupational licensing on the number of cosmetologists. Conrad and Dolan (1980) showed that reciprocity rules limit the migration of professions into restrictive states. A challenge with these cross-sectional analyses is that licensure practices may correlate with other state-level attributes that influence migration (beyond the variables controlled for). Peterson, Pandya, LeBang (2014) address this problem by exploiting changes in residency training requirements for immigrant physicians within states over time, finding that states that impose more stringent requirements receive fewer immigrant physicians.

We add to this literature in three ways. First, the nature of the policy change which affected nurses but not other health workers permits us to estimate triple difference models, which control for time-varying sources of demand for health care workers that may happen to correlate with NLC adoption. Second, we study a very recent policy change, whereas most of the literature relies on data from forty years ago, when labor market institutions and structures may have been quite different. Finally, we examine nurses, who are the second largest licensed profession behind teachers (Kleiner 2000) and whose supply and geographic distribution are targets of extensive policy deliberation.

2.4 Empirical Approach

2.4.1 Data and Samples

We analyze nurses and other health care workers surveyed in the public use micro surveys of the 1990 and 2000 U.S. Census and the 2006 to 2012 American Community Survey (ACS).[5] The ACS is an ongoing survey that provides yearly information about communities in the years between the Decennial Censuses. Detailed information is collected about age, sex, race, income, education, where one works, commuting distance, where one lives, as well as occupation. The Census data provides similar information, albeit on a ten-year basis and for much larger samples. While the ACS helps provide additional information in the years when states are joining the NLC, the Census data helps to provide more data points before the advent of the NLC. Both identify Registered Nurses and Licensed Practical Nurses, who are also subject to the NLC, helping to give us a more complete picture in regards to the impact of the NLC.

Our data include all workers in health occupations, including nurses, physicians, medical assistants, home health aids, and several other smaller occupational categories. Non-nurse health care workers are not subject to the Nurse Licensure Compact but work in similar settings and thus are a good control group with which to compare RNs and LPNs. Locational information allow us to identify whether an individual lives and/or works in a compact state as well as whether they live in an MSA that crosses state lines. Figure 2.3 shows the percentages of nurses and other health workers living in a compact state over our sample period. Our sample includes 1.5 million health workers, about one third of which are nurses.

Table 2.1 provides summary statistics on our full sample of nurses and all health workers across all years, separately by compact status and border or non-border crossing MSA residence. Across all states and years, 3-4% of nurses and other health workers commute across state lines to work, with an average travel time of 23 to 24 minutes. Cross-state commuting and travel time are both higher among workers living in Compact states. Unsurprisingly, residents of MSAs that cross state lines are much more likely to commute across state lines to work, with the average rising to 8% for all health workers and 9% for nurses. Approximately 87-88% of the nurses in the sample are currently in the labor force, with 98% of those who are labor force participants currently employed. In addition, there is very little variation in these samples across states that are part of the compact and those that are not nor those areas that are border MSAs and non-border MSAs. These labor force numbers are similar for all health workers as well, with 86-87% (depending on the locational restriction) currently in the labor force. Of those health workers in the labor force, 97% are currently employed.

[5] We also estimate some models using the National Sample Survey of Registered Nurses (NSSRN) for years 1980-2008. This data does not allow us to compare nurses to other health workers, which we find is important. Thus we only present results from analysis using NSSRN in the Appendix, Section 2.7.3 (Table 2.16).

2.4.2 Identification Strategy and Specification

The raw summary statistics suggest that workers, particularly nurses, are more mobile when the NLC is in place. However, this raw correlation is unlikely to provide a good estimate of the causal effect of the policy on worker mobility. Time trends, state characteristics, worker characteristics, or labor market shocks that happen to correlate with the presence of the NLC are likely to bias estimates of its effect. To address these, we exploit the fact that states adopted the NLC at different times and that it only pertained to nurses.

Our first approach is to compare changes in labor force participation and commuting patterns of nurses between states that adopted the NLC with those that did not during the same time period. We begin by estimating simple difference-in-difference models on the sample of registered and licensed practical nurses using regressions of the form:

$$Y_{ist} = \beta_0 + \beta_1 Compact_{st} + \beta_z Z_{st} + \beta_x X_{ist} + \gamma_s + \gamma_t + \epsilon_{ist} \tag{2.1}$$

Our dependent variable, Y_{ist}, is one of three outcomes: 1) an indicator for whether individual i residing in state s during year t works in a different state; 2) the average time individual i residing in state s during year t spends commuting each day; and 3) an indicator for whether individual i residing in state s during year t is in the labor force.[6] $Compact_{st}$ is an indicator for whether state s is a compact state in year t. Aggregate time trends in the prevalence of cross-state commuting and employment are accounted for by year fixed effects γ_t. Location fixed effects control for average differences in commuting and employment prevalence across areas that may be related to the adoption of the NLC. For instance, states that typically have many nurses commuting across the border may have a greater incentive to join the compact. Where necessary, our location fixed effects are either state fixed-effects or Metropolitan Statistical Area (MSA) fixed-effects. In some specifications, we also control for time-varying state Z_{st} and individual X_{ist} characteristics, such as economic conditions and worker demographics that may influence commuting patterns and also happen to correlate with adoption of the compact. The coefficient of interest β_1 is the change in share of nurses who commute across state lines following the adoption of the NLC in their home state. Standard errors are clustered at the state level, to address the possibility that observations within states are not independent.

The simple difference-in-differences specification assumes that outcomes for treatment and control states would trend similarly in the absence of treatment. Labor market trends and shocks could violate this assumption if, for instance, states adopt the NLC in anticipation of growing demand for nurses or as a response to declining supply. The typical approach to ruling out this form of violation is to look for evidence of dif-

[6] We are also in the process of examining the differences between working in a different state versus working in a different state that is a member of the NLC. This analysis is ongoing and the results are available upon request.

ferential trends between Compact and non-Compact states before the former enact the NLC. Unfortunately we do not have enough high frequency data to evaluate pre-trends. However, several features of the NLC naturally facilitate variations on the basic specification to probe the validity of this main identifying assumption. Most importantly, we exploit the fact that only nurses (registered and licensed vocational/practical) are affected by the compact while other health professionals (physicians, medical assistants, etc.) are not to construct a triple difference estimator. We first estimate (1) on the sample of non-nurse health workers and test whether there is any "effect" on these workers when there should not be.[7] We then explicitly use these workers as a control group, and estimate the following model of the form:

$$\begin{aligned} Y_{ist} = \beta_0 + \beta_1 Compact_{st} + \beta_2 Nurse_{ist} + \beta_3 Compact_{st} * Nurse_{ist} \\ + \beta_z Z_{st} + \beta_x X_{ist} + \gamma_s + \gamma_t + \epsilon_{ist} \end{aligned} \tag{2.2}$$

The coefficient on $Compact_{st}$ captures any change in commuting patterns among non-nurse health care workers that are correlated with NLC adoption. The coefficient on the interaction term captures the differential impact on nurses and is our coefficient of interest. Like in equation (2.1) the dependent variable, Y_{ist}, is one of the same three outcomes. This specification controls for any time-varying labor market shocks that similarly affect nurses and other health care workers.

We also explore whether commuting patterns may differ between those who live in MSAs versus those that who live in areas that are not assigned to an MSA. Given that MSAs are centered on an urban area, those not assigned to an MSA are likely in a rural area. Thus, we stratify our data by residents living in an MSA and those not living in an MSA and re-estimate equation (2.1). Lastly, the compact is most useful to (and commuting is most common for) residents that live close to state borders. Thus in some specifications we estimate (1) separately for residents of border and non-border MSAs. If the population of areas at the interior of states grew at a faster rate than those near the border, this would naturally depress the observed commuting rate. [8] As a variant on this, we also estimate a modified version of equation (2.2) that further interacts $Compact_{st} * Nurse_{ist}$ with an indicator for whether the individual i lives in a border MSA $Compact_{st} * Nurse_{ist} * BorderMSA_{st}$, pooling all people who live in MSAs. These results are presented in the Appendix, Section 2.7.2.

Since occupational licensing regimes are not experimentally assigned, there are several remaining threats to identification that confound estimates of the effect of NLC participation. First, it is possible that other policies are adopted simultaneously with the NLC that only impact nurse labor markets (but not other

[7] These results are available upon request.

[8] We are currently working on identifying residents of counties on state borders, as another way of focusing on workers likely to be most impacted by the policy.

health workers). We are not aware of any such policies, but cannot rule this out. Second, our approach takes residency location decisions as exogenous. If the NLC also impacts where nurses choose to live, our estimates may confound true causal effects with changes in the composition of nurses who work in compact states.

2.5 Results

Tables 2.2-2.4 present results for the effect of the NLC on our three outcome variables, with each specification with varying amounts of control variables. We first examine the impact on nurses exclusively. Column (2) includes year fixed-effects only, Column (3) includes state fixed effects, and Column (4) includes year and state FE, which is our basic difference-in-difference estimate. Column (5) adds in our full set of demographic control variables to control for any changes in the composition of nurses. Each of these regressions helps break down how the inclusion of control variables affects our coefficient of interest. In each of these tables, we see that the addition of year FE decreases our coefficient on living in a compact state. This implies that our initial coefficient in column (1) was biased upward due to commuting and labor force participation trends increasing over time. When state FE are included, our coefficient becomes more positive and significant, suggesting that states adopting the NLC have lower commute rates and worse labor markets than states that do not. Finally specification (4) is our main difference-in-differences specification. These results imply that as a state switches from being a non-Compact state to a Compact state, the probability of working in a different state, commuting times, and labor force participation all decrease. Adding demographic controls (specification 5) has little impact on this finding, suggesting that worker composition does not change dramatically when the NLC is adopted. To summarize, the basic difference-in-difference results suggest that NLC adoption is actually associated with lower levels of cross-state commuting, and labor force participation.

The basic difference-in-difference estimates in Tables 2.2-2.4 are biased if nurses are subject to other policy or labor market shocks or trends that coincide with NLC adoption. To address this source of bias, Table 2.5 presents triple difference estimates (equation 2.2) for all three outcomes. The odd columns repeat our final difference-in-difference estimates including full controls (year FE, state FE, and the full set of demographic controls).[9] The even columns present triple difference estimates, using non-nurse health workers as a within-state control group that should be unaffected by NLC adoption. When we expand our identification strategy to include all health workers and look at the interaction between living in a compact state and being a nurse (Column (2)), our coefficient on cross-state commuting becomes positive although it is still statistically and economically insignificant. Specifically, our result indicates that nurses who live in a Compact state are

[9]Note that the rest of the results that we discuss in this section reflect the same specifications discussed in Table 2.5.

0.14% more likely to work in a different state than other health professionals. In addition, this result implies that the coefficient on equation (2.1) is biased downward as those states that join the NLC have shorter commuting patterns in general. The results in columns (3) and (4) also support this assessment, as the coefficient on Compact State is negative in column (3) indicating nurses in compact states commute 0.25 of a minute less than those nurses that do not live in a compact state. Our triple difference results in Column (4), however, show that nurses who live in a compact state commute 1.08 minutes more (significant at the 1% level) than other health workers living in the same state when the NLC is adopted.

Columns (5) and (6) labor force participation probabilities. We may think that the Compact would encourage nurses to more actively search for jobs as the Compact should increase the amount of jobs available to a nurse. Our estimates imply that nurses who live in a compact state are actually less likely to participate in the labor force; however these results are very small in magnitude especially when compared to the mean labor force level.

Tables 2.6 and 2.7 present the results of equations (2.1) and (2.2) for every outcome for the sample of nurses and health workers but restricts the sample to those living in an MSA and those who are not assigned to an MSA, respectively. The results in both tables support the belief that workers in urban areas have different commuting patterns than workers in rural areas. The coefficients in Panel A on both the Compact variable (Column (1)) and the interaction term in Column (2) are negative and insignificant, indicating that the compact has little effect on the probability of nurses working in a different state who live in an MSA. Those not living in an MSA, however, have a positive coefficient on both the Compact variable in Column (1) and the interaction term in Column (2). The magnitude of each coefficient represents about a half % increase in the probability of working in a different state following the introduction of the compact in the nurse's home state (in comparison to other health workers). This magnitude is large in comparison to the mean of the outcome variable which is about 4%.

The results for commuting time follow a slightly different pattern than when examining the probability of working in a different state. That is, nurses who live in a compact state and don't live in an MSA see a smaller effect on commuting time than those nurses who live in an MSA, despite being more likely to work in an entirely different state. To be specific, the triple difference estimate suggests that living in a Compact State increases commuting time by 0.95 minutes following the compact (significant at 95% level) while nurses living in a non-MSA see an increase in commuting time of .3 min (insignificant). This result is intuitive, however, as commuting time is measured in minutes and workers in MSAs are more likely to be driving smaller distances yet hitting more traffic than workers in rural areas who are potentially driving further yet spending less time in the car.

The effects of the compact on labor force participation for MSAs and non-MSAs are similar to those found when not stratifying by location (see Table 2.5). The magnitudes of the coefficient of the Compact variable (Columns (5)) is negative and very small compared to the mean (about 1% in comparison to a mean outcome of 87%). The coefficient on the interaction term (Columns (6)) is almost zero. Both of these results indicate very little, if not, a negative effect on labor force participation that is attributable to the introduction of the Compact.

Tables 2.8 and 2.9 presents the results of estimating equations (2.1) and (2.2) but separates nurses and health workers into those who live in MSAs that cross state lines (Border MSAs, Table 2.8) and those that do not (Non-Border MSAs, Table 2.9). The results of those living in border MSAs vs. those living in non-Border MSAs are different. Those nurses living in non-Border MSAs, i.e. those in less of a position to take advantage of the compact, are less likely to work in a different state following the introduction of the compact in their home state (significant at the 1% level). Nurses, however, living in border MSAs are 1.3% more likely to work in a different state following the introduction of the compact in their home state (statistically insignificant, but a large magnitude given a mean of 7.8%).

The effect of the compact on the commuting time of nurses is positive and statistically significant for nurses living in both border and non-border MSAs. (Columns (3) and (4)). This coefficient on the interaction term in Column (4) implies that the compact has increased commuting time more for nurses who live in a compact state than for other health workers. Furthermore, the effect on commuting time in border MSAs is much larger than for nurses in non-border MSAs (2.32 minutes vs. 0.45 minutes), with the border MSA result equating to an increase that is just under 10% of the mean (27.1 minutes).

Like our other specifications, the compact does not appear to affect labor force participation of nurses living in either border or non-border MSAs. This is again consistent with the result seen when looking at MSAs as a whole. In other words, there is little difference between labor force participation following the introduction of the compact between nurses living in border and non-border MSAs.

2.5.1 Robustness Checks

Since 1990 is relatively far in advance of the introduction of the Nurse Licensure Compact, it may not provide an accurate pre-NLC trend of commuting or labor force participation patterns in the U.S. As a robustness check, we re-estimated equations (2.1) and (2.2) for all of our location stratifications but excluding census year 1990. The results are presented in the Appendix, Section 2.7.1. Generally, the results stay same as the before, specifically for the outcomes regarding working in a different state and labor force participation. When stratifying the data by those workers who live in an MSA, live in a border MSA, and live in a non-border MSA, the results for equation (2.1) do change for commuting times (Column (3) of Tables 2.11, 2.13, and 2.14). These changes, however, are small and statistically and economically insignificant.

As mentioned in Section 2.4, we also estimate a modified version of equation (2.2) and using the following quadruple difference specification for individual i living in state s in year t:

$$Y_{ist} = \beta_0 + \beta_1 Compact_{st} + \beta_2 Nurse_{ist} + \beta_3 BorderMSA_{st} + \beta_4 Compact_{st} * Nurse_{ist} + \beta_5 Compact_{st} * BorderMSA_{st} + \beta_6 Nurse_{ist} * BorderMSA_{ist} + \beta_7 Compact_{st} * Nurse_{ist} * BorderMSA_{st} + \beta_z Z_{st} + \beta_x X_{ist} + \gamma_s + \gamma_t + \epsilon_{ist} \quad (2.3)$$

where $Compact_{st} * Nurse_{ist} * BorderMSA_{st}$ is the interaction of whether nurse i lives in a border MSA in year t and a compact state s in year t. The key identifying assumption is that the NLC should have zero impact on workers in MSAs that do not encompass multiple states. These results are presented in Table 2.15. The coefficient on the interaction term $Compact_{st} * Nurse_{ist} * BorderMSA_{st}$ is positive and statistically significant. The result suggests that Nurses who live in a Compact State and also in a Border MSA work in a different state 2.8% points than those health workers and nurses that do not meet these criteria. This result is very large as the mean % of health workers who work in a different state is less than 3%. On the other hand, when we estimate equation (2.3) for commuting times, the result becomes negative and highly insignificant. We view this analysis as somewhat speculative because there are some MSAs classified as non-border MSAs (because they do not straddle multiple states) that are clearly within commuting distance of another state. This will tend to bias our estimates from this quadruple difference downwards.

2.6 Conclusion

The Nurse Licensure Compact was first introduced in 1999 and allows Registered Nurses and Licensed Practical Nurses with licenses in one NLC-member state to practice in other NLC states without obtaining a separate license for that state. The Compact was created with the intention of providing greater mobility for nurses, clarifying the authority to practice for nurses currently engaged in telenursing or interstate practice, improving access to nursing care in general and during a disaster or other times of great need, and enhancing information-sharing among member NLC states While only three states joined the Compact in its first year, currently 24 states are now members and a few more have pending legislation.

In this chapter, we use data from the American Community Survey and the U.S. Census for years 1990-2012 to estimate the effects of the Compact on commuting patterns and labor force outcomes. In comparison to other health workers who were not affected by the compact, we do find some evidence that the mobility of nurses increases following the adoption of the Compact in the nurses' home state. Specifically, nurses that live in a border MSA (metropolitan areas that cross multiple state lines) see a 1.2 percentage point increase in the probability of living in one state and working in another state following their home state joining the Compact. Similarly, we also find that commuting times of these same nurses increase by approximately 2.32 minutes. We find no effect of the Compact labor force participation of nurses. Thus the reduction of licensing barriers on cross-state mobility appears to widen the geographic reach of the nurse labor market.

While this is the first study to empirically look at the effect of the Nurse Licensure Compact, we recognize that this is a first step in fully identifying the labor market effects of the Nurse Licensure Compact. Our results indicate that the effects are concentrated in Border MSAs, yet there are likely other nurses in non-Border MSAs in the Compact state that are affected that we fail to accurately separate from other non-Border MSA residents (e.g. MSAs that are on the "border" of a state, yet do not cross state lines).

Finally, we test just one of the expected impacts of the Compact. As discussed, the Compact was introduced with the intention of creating other benefits besides increasing nurse mobility. To get a fuller picture of the effect of the Compact, it would be necessary to empirically test the whether access to care increased following the adoption of the Compact in a patient's home state, and how this has affected the prevalence and scope of telenursing throughout the United States.

The implications of our results for licensing and health care policy are three-fold. First, it does appear that state-specific licensing creates labor market frictions that reduce the geographic scope of the nurse labor market. This may be inefficient if it prevents the workforce from being allocated to areas of highest need. Though we focus on nurses, the same may be true of many other licensed professionals, such as lawyers, therapists, physicians and teachers. Nationalized licensing, which the NLC is mimicking, will reduce these geographic barriers and thus increase efficiency. From a health care perspective, our results offer a mixed bag. On one hand, reducing licensing barriers will likely increase the pool of workers from which hospitals draw, perhaps enabling them to better fill specific needs. On the other, it does not appear that the NLC brings new nurses into the labor force, so a reduction in licensing barriers is not a solution to a shortage of nurses in the aggregate.

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Figure 2.1: Nurse Licensure Compact States, 2000 to 2008

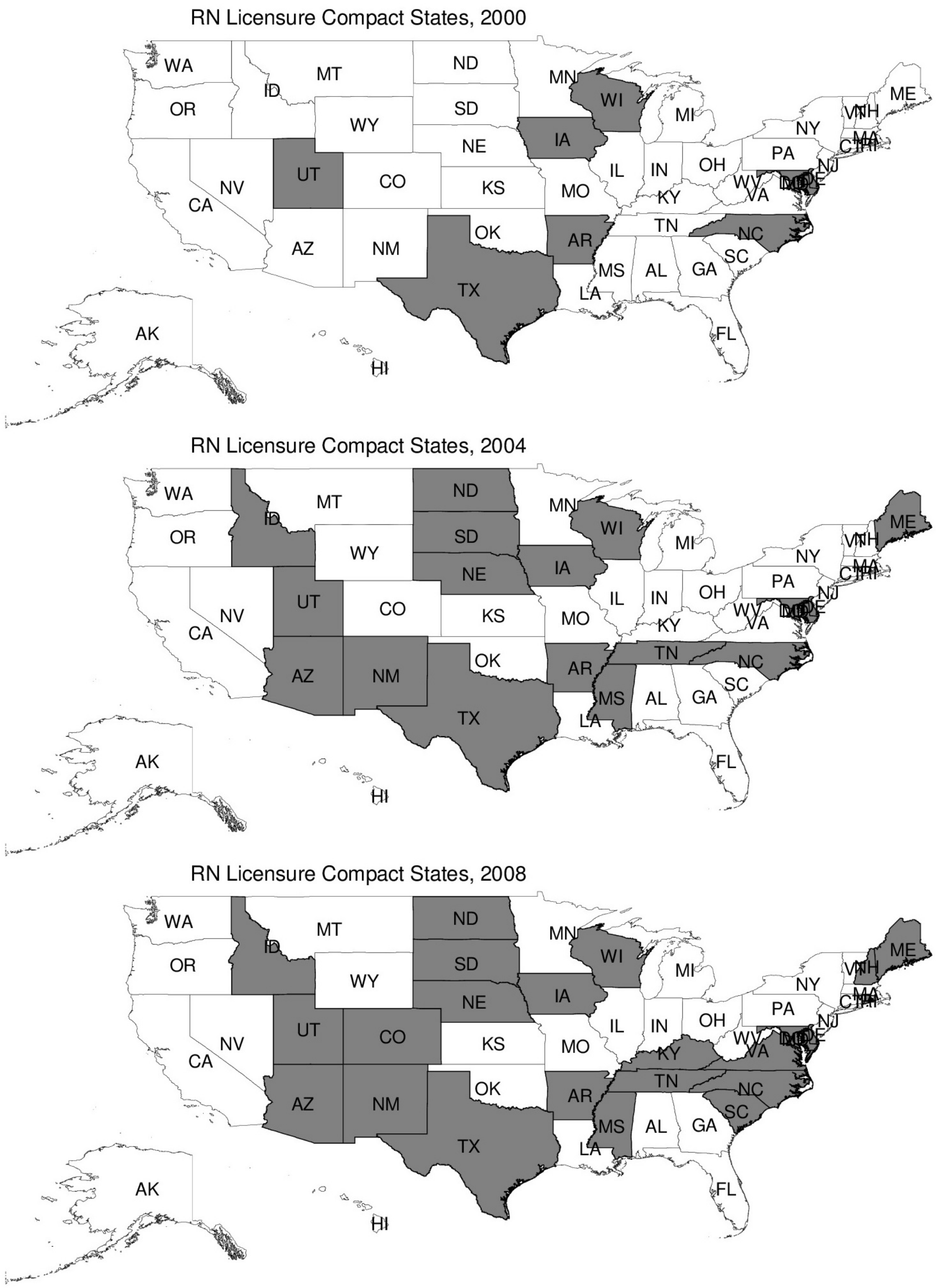

Figure 2.2: Is a Nurse Eligible for A Multi-State License?

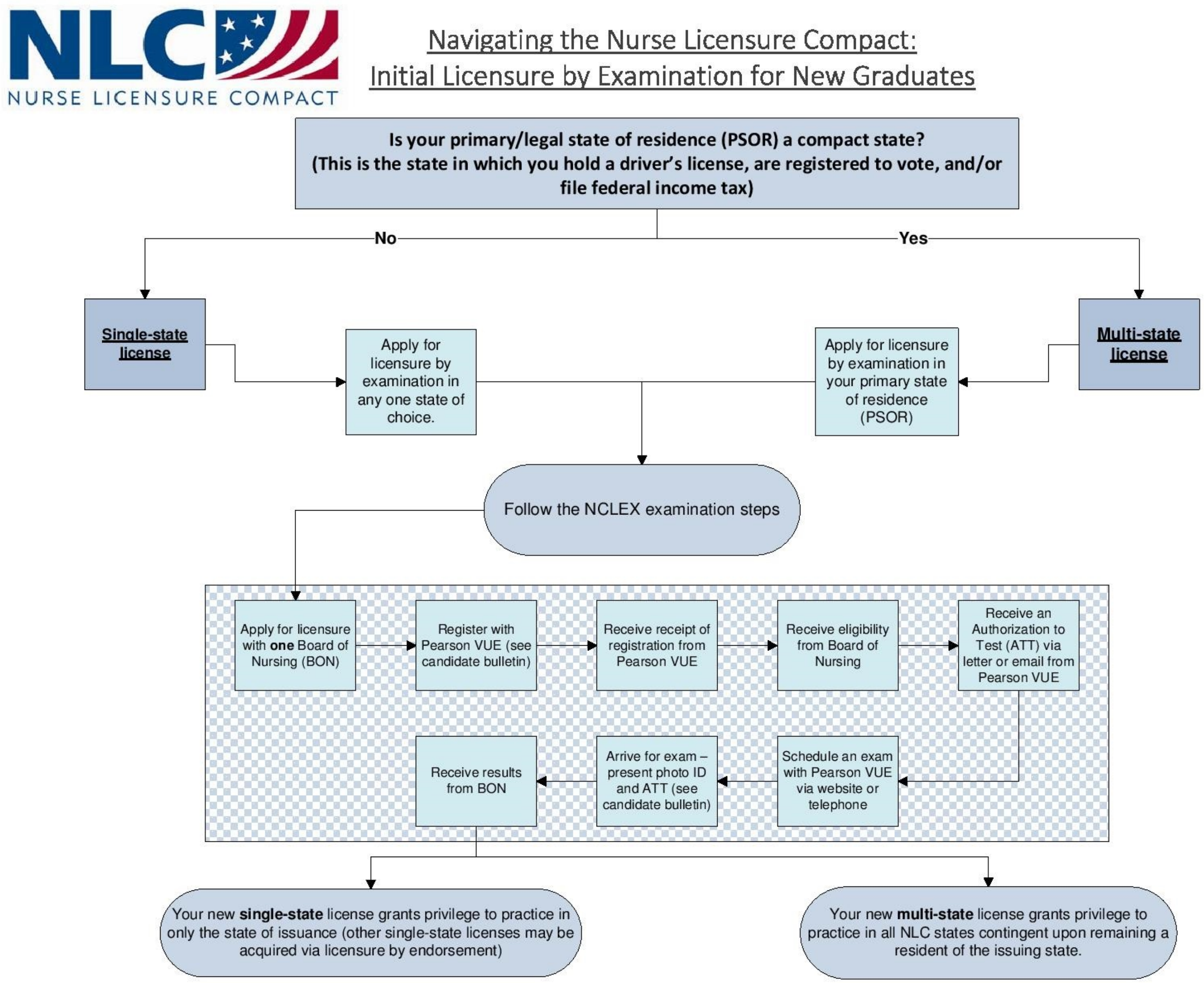

Figure 2.3: Nurses and Other Health Workers Living in A Compact State

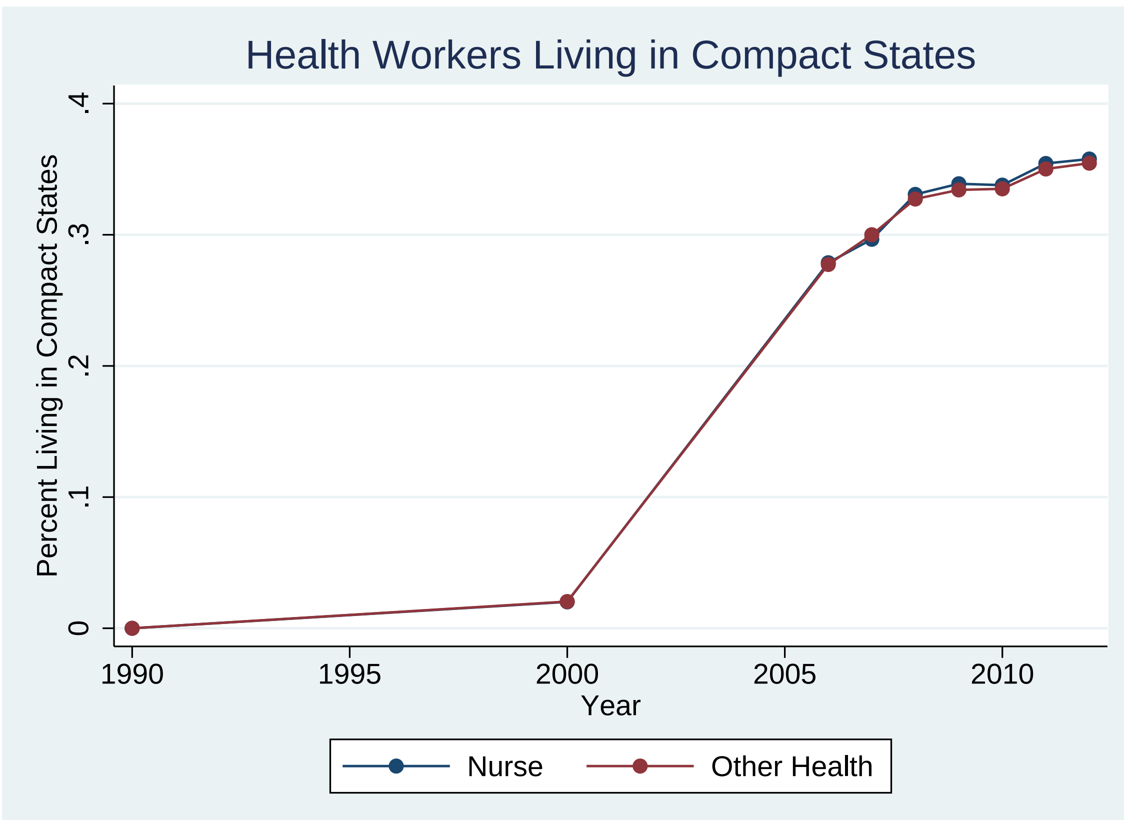

Table 2.1: Summary Statistics of Nurses and All Health Workers

	Nurses only (Census and ACS)					All health workers (Census and ACS)				
	All States	Part of Compact	Never in Compact	Border MSA	Non-Border MSA	All States	Part of Compact	Never in Compact	Border MSA	Non-Border MSA
Share of workers who										
Work in State of Residence	0.97	0.95	0.97	0.91	0.99	0.97	0.96	0.97	0.92	0.99
Work in a Different State	0.03	0.05	0.03	0.09	0.01	0.03	0.04	0.03	0.08	0.01
Employed*	0.98	0.98	0.98	0.98	0.98	0.97	0.97	0.97	0.97	0.97
In the Labor Force	0.87	0.88	0.87	0.88	0.87	0.86	0.87	0.86	0.87	0.86
Average Travel Time to Work	24.13	24.89	23.97	26.47	23.24	23.37	23.56	23.33	27.10	22.64
Individual characteristics										
Female	0.93	0.92	0.93	0.94	0.92	0.81	0.83	0.80	0.79	0.80
Registered nurse						0.30	0.31	0.30	0.30	0.30
Licensed vocational/practical nurse						0.08	0.08	0.08	0.05	0.07
Educational Categories										
High School Degree or Less	0.25	0.22	0.26	0.22	0.24	0.41	0.40	0.41	0.38	0.40
Some College	0.33	0.35	0.33	0.27	0.32	0.20	0.22	0.20	0.16	0.20
College Degree	0.31	0.34	0.30	0.37	0.33	0.21	0.22	0.21	0.24	0.22
Post-College Education	0.11	0.09	0.11	0.14	0.11	0.18	0.15	0.18	0.22	0.18
Race Categories										
White	0.82	0.83	0.82	0.70	0.80	0.79	0.79	0.79	0.66	0.76
Black	0.10	0.11	0.10	0.18	0.11	0.12	0.13	0.11	0.21	0.12
American Indian	0.01	0.01	0.01	0.00	0.00	0.01	0.01	0.01	0.00	0.01
Asian	0.05	0.04	0.06	0.09	0.07	0.05	0.03	0.06	0.08	0.07
Other Race	0.01	0.01	0.01	0.01	0.01	0.02	0.02	0.02	0.03	0.03
Observations	550,643	85,432	465,211	83,690	294,556	1,547,920	248,704	1,299,216	234,967	799,650
Number of states	51	24	27	22	29	51	24	27	22	29

The share of workers who are employed is conditional on being in the labor force.
The share of workers who work in a different state or in their state of residence is conditional on being employed

Table 2.2: Effects of the Nurse Licensure Compact (NLC) on Probability of Working in a Different State

	(1)	(2)	(3)	(4)	(5)
	Works in a Different State				
Compact State Resident	0.0122	0.0118	0.0040*	-0.0016	-0.0015
	(0.0130)	(0.0141)	(0.0020)	(0.0025)	(0.0025)
Constant	0.0319***	0.0291***	0.0341***	0.0284***	0.0272***
	(0.0065)	(0.0058)	(0.0006)	(0.0017)	(0.0082)
Observations	484,534	484,534	484,534	484,534	484,534
R-squared	0.0009	0.0009	0.0365	0.0367	0.0375
Sample	All Nurses	All Nurses	All Nurses	All Nurses	All Nurses
Year FE	No	Yes	No	Yes	Yes
State FE	No	No	Yes	Yes	Yes
Controls	None	None	None	None	Full
Y mean	0.0347	0.0347	0.0347	0.0347	0.0347

Table 2.3: Effects of the Nurse Licensure Compact (NLC) on Log Commuting Time

	(1)	(2)	(3)	(4)	(5)
	Commuting Time				
Compact State Resident	-0.0129	-0.0419	0.0828***	-0.0085	-0.0087
	(0.0283)	(0.0300)	(0.0081)	(0.0077)	(0.0078)
Constant	2.9926***	2.8401***	2.9665***	2.8368***	2.6692***
	(0.0157)	(0.0140)	(0.0022)	(0.0052)	(0.0339)
Observations	480,234	480,234	480,234	480,234	480,234
R-squared	0.0001	0.0049	0.0180	0.0218	0.0266
Sample	All Nurses	All Nurses	All Nurses	All Nurses	All Nurses
Year FE	No	Yes	No	Yes	Yes
State FE	No	No	Yes	Yes	Yes
Controls	None	None	None	None	Full
Y mean	2.9699	2.9699	2.9699	2.9699	2.9699

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

Notes: All regressions in tables 2.2 and 2.3 are weighted using ACS person weights and standard errors are clustered at the State Level. Each regression includes controls for Sex, Race, Education, Age, and Age^2. In tables 2.2 and 2.3, the sample is restricted to workers currently employed.

Table 2.4: Effects of the Nurse Licensure Compact (NLC) on Labor Force Participation

	(1)	(2)	(3)	(4)	(5)
			In Labor Force		
Compact State Resident	0.0056*	-0.0048*	0.0269***	-0.0105**	-0.0116***
	(0.0028)	(0.0027)	(0.0056)	(0.0047)	(0.0038)
Constant	0.8886***	0.8701***	0.8828***	0.8698***	0.3136***
	(0.0018)	(0.0023)	(0.0015)	(0.0017)	(0.0131)
Observations	582,543	582,543	582,543	582,543	582,543
R-squared	0.0001	0.0031	0.0011	0.0038	0.1318
Sample	All Nurses	All Nurses	All Nurses	All Nurses	All Nurses
Year FE	No	Yes	No	Yes	Yes
State FE	No	No	Yes	Yes	Yes
Controls	None	None	None	None	Full
Y mean	0.8708	0.8708	0.8708	0.8708	0.8708

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

Notes: All regressions are weighted using ACS person weights and standard errors are clustered at the State Level. Each regression includes controls for Sex, Race, Education, Age, and Age^2.

Table 2.5: Effects of the NLC on Nurses and Health Workers: No Location Restrictions

	(1)	(2)	(3)	(4)	(5)	(6)
	Works in a Different State		Commuting Time		In Labor Force	
Compact State Resident	-0.0015	-0.0005	-0.0087	-0.0163*	-0.0116***	-0.0083**
	(0.0025)	(0.0023)	(0.0078)	(0.0089)	(0.0038)	(0.0034)
Nurse		0.0060***		0.0560***		0.0242***
		(0.0019)		(0.0126)		(0.0014)
Compact State Resident * Nurse		0.0014		0.0397***		-0.0018
		(0.0034)		(0.0142)		(0.0029)
Male	0.0092***	0.0086***	0.0582***	0.0223***	0.0197***	0.0394***
	(0.0028)	(0.0021)	(0.0093)	(0.0056)	(0.0017)	(0.0017)
White	-0.0056	-0.0040	-0.0364***	-0.0682***	0.0024	0.0105**
	(0.0076)	(0.0050)	(0.0133)	(0.0185)	(0.0036)	(0.0041)
Black	-0.0013	-0.0032	0.0701***	0.0575**	0.0089**	0.0141***
	(0.0079)	(0.0060)	(0.0240)	(0.0231)	(0.0043)	(0.0048)
American Indian	0.0141	0.0055	-0.0197	-0.0836**	-0.0270**	-0.0123
	(0.0087)	(0.0053)	(0.0345)	(0.0324)	(0.0102)	(0.0078)
Other	-0.0013	-0.0029	-0.0097	0.0011	-0.0004	0.0094***
	(0.0046)	(0.0041)	(0.0316)	(0.0379)	(0.0039)	(0.0025)
High School Degree or Less	-0.0022*	0.0000	-0.0294***	-0.0085	-0.0183***	-0.0058***
	(0.0012)	(0.0008)	(0.0062)	(0.0066)	(0.0029)	(0.0014)
College Degree	0.0045	0.0056**	0.0432***	0.0601***	0.0104***	0.0157***
	(0.0027)	(0.0025)	(0.0074)	(0.0072)	(0.0016)	(0.0014)
Post-College Education	0.0091***	0.0084**	0.0744***	0.0203**	0.0167***	0.0461***
	(0.0031)	(0.0032)	(0.0082)	(0.0098)	(0.0021)	(0.0018)
Age	0.0003	0.0005**	0.0084***	0.0154***	0.0320***	0.0269***
	(0.0003)	(0.0002)	(0.0014)	(0.0011)	(0.0006)	(0.0003)
Age^2	-0.0000	-0.0000**	-0.0001***	-0.0002***	-0.0004***	-0.0003***
	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)
Constant	0.0272***	0.0136***	2.6692***	2.4874***	0.3136***	0.3724***
	(0.0082)	(0.0049)	(0.0339)	(0.0269)	(0.0131)	(0.0083)
Observations	484,534	1,263,406	480,234	1,243,432	582,543	1,547,788
R-squared	0.0375	0.0332	0.0266	0.0355	0.1318	0.0845
Sample	All Nurses	All Health	All Nurses	All Health	All Nurses	All Health
Y mean	0.0347	0.0313	2.9699	2.9209	0.8708	0.8620

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

Notes: All regressions have year fixed effects and state fixed effects included. The regressions are weighted using ACS person weights and standard errors are clustered at the State Level. Each regression includes only the controls listed above. The odd numbered columns are the sample of registered nurses and licensed practical nurses ("All Nurses") and the even numbered columns are the sample of health workers including nurses ("All Health"). In columns (1)-(4), the sample is restricted to workers currently employed.

Table 2.6: Effects of the NLC on Nurses and Health Workers Living in MSAs

	(1)	(2)	(3)	(4)	(5)	(6)
	Works in a Different State		Commuting Time		In Labor Force	
Compact State Resident	-0.0078	-0.0057	-0.0072	-0.0194*	-0.0107***	-0.0098***
	(0.0119)	(0.0088)	(0.0096)	(0.0110)	(0.0038)	(0.0033)
Nurse		0.0052***		0.0416***		0.0249***
		(0.0014)		(0.0131)		(0.0015)
Compact State Resident * Nurse		-0.0021		0.0335**		0.0003
		(0.0031)		(0.0152)		(0.0025)
Constant	0.0275***	0.0165***	2.6862***	2.5264***	0.3090***	0.3779***
	(0.0094)	(0.0049)	(0.0349)	(0.0195)	(0.0109)	(0.0067)
Observations	314,830	844,958	312,013	831,428	378,229	1,034,563
R-squared	0.0900	0.0806	0.0517	0.0575	0.1265	0.0800
Sample	All Nurses	All Health	All Nurses	All Health	All Nurses	All Health
Y mean	0.0275	0.0258	2.9898	2.9593	0.8724	0.8635

Table 2.7: Effects of the NLC on Nurses and Health Workers Living in Non-MSAs

	(1)	(2)	(3)	(4)	(5)	(6)
	Works in a Different State		Commuting Time		In Labor Force	
Compact State Resident	0.0052	0.0033	-0.0278*	-0.0258	-0.0099**	0.0009
	(0.0037)	(0.0038)	(0.0149)	(0.0164)	(0.0040)	(0.0052)
Nurse		0.0081***		0.1051***		0.0238***
		(0.0018)		(0.0067)		(0.0022)
Compact State Resident * Nurse		0.0053		0.0103		-0.0050
		(0.0040)		(0.0115)		(0.0035)
Constant	0.0272***	0.0086	2.5723***	2.3045***	0.3220***	0.3597***
	(0.0091)	(0.0059)	(0.0498)	(0.0417)	(0.0157)	(0.0121)
Observations	169,704	418,448	168,221	412,004	204,314	513,225
R-squared	0.0278	0.0243	0.0314	0.0441	0.1451	0.0971
Sample	All Nurses	All Health	All Nurses	All Health	All Nurses	All Health
Y mean	0.0481	0.0425	2.9329	2.8436	0.8679	0.8592

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

Notes: The regressions in Table 2.6 have year fixed effects and MSA fixed effects included. The regressions in Table 2.7 have year fixed effect and state fixed effects included. The regressions are weighted using ACS person weights and standard errors are clustered at the MSA Level. Each regression includes controls for Sex, Race, Education, Age, and Age^2. The odd numbered columns are the sample of registered nurses and licensed practical nurses ("All Nurses") and the even numbered columns are the sample of health workers including nurses ("All Health"). In columns (1)-(4), the sample is restricted to workers currently employed.

Table 2.8: Effects of the NLC on Nurses and Health Workers Living in Border MSAs

	(1)	(2)	(3)	(4)	(5)	(6)
	Works in a Different State		Commuting Time		In Labor Force	
Compact State Resident	-0.0308	-0.0355	-0.0145	-0.0523**	-0.0079	-0.0020
	(0.0474)	(0.0339)	(0.0239)	(0.0221)	(0.0075)	(0.0070)
Nurse		0.0149***		-0.0100		0.0237***
		(0.0048)		(0.0305)		(0.0039)
Compact State Resident * Nurse		0.0128		0.0830**		-0.0057
		(0.0098)		(0.0334)		(0.0047)
Constant	0.0992***	0.0409**	2.9076***	2.6459***	0.3354***	0.3975***
	(0.0332)	(0.0184)	(0.0642)	(0.0254)	(0.0269)	(0.0116)
Observations	69,862	192,233	69,253	189,512	83,685	234,953
R-squared	0.0598	0.0616	0.0407	0.0564	0.1197	0.0770
Sample	All Nurses	All Health	All Nurses	All Health	All Nurses	All Health
Y mean	0.0871	0.0784	3.0783	3.0825	0.8758	0.8668

Table 2.9: Effects of the NLC on Nurses and Health Workers Living in Non-Border MSAs

	(1)	(2)	(3)	(4)	(5)	(6)
	Works in a Different State		Commuting Time		In Labor Force	
Compact State Resident	-0.0006	0.0007	-0.0043	-0.0046	-0.0110**	-0.0107***
	(0.0017)	(0.0011)	(0.0090)	(0.0067)	(0.0046)	(0.0027)
Nurse		0.0017**		0.0596***		0.0254***
		(0.0007)		(0.0040)		(0.0014)
Compact State Resident * Nurse		-0.0025*		0.0150*		0.0014
		(0.0013)		(0.0079)		(0.0027)
Constant	0.0060	0.0081***	2.6205***	2.4901***	0.3009***	0.3707***
	(0.0040)	(0.0026)	(0.0359)	(0.0242)	(0.0122)	(0.0086)
Observations	244,968	652,725	242,760	641,916	294,544	799,610
R-squared	0.0486	0.0413	0.0490	0.0467	0.1285	0.0810
Sample	All Nurses	All Health	All Nurses	All Health	All Nurses	All Health
Y mean	0.0105	0.0103	2.9646	2.9229	0.8715	0.8625

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

Notes: All regressions have year fixed effects and MSA fixed effects included. The regressions are weighted using ACS person weights and standard errors are clustered at the MSA Level. Each regression includes controls for Sex, Race, Education, Age, and Age^2. The odd numbered columns are the sample of registered nurses and licensed practical nurses ("All Nurses") and the even numbered columns are the sample of health workers including nurses ("All Health"). In columns (1)-(4), the sample is restricted to workers currently employed.

2.7 Appendix

2.7.1 Effects of the Nurse Licensure Compact: Years 2000-2012

Table 2.10: Effects of the NLC on Nurses and Health Workers, No Location Restrictions: 2000-2012

	(1)	(2)	(3)	(4)	(5)	(6)
	Works in a Different State		Commuting Time		In Labor Force	
Compact State Resident	-0.0002	0.0011	-0.0066	-0.0171**	-0.0133***	-0.0078*
	(0.0027)	(0.0020)	(0.0080)	(0.0075)	(0.0043)	(0.0043)
Nurse		0.0063***		0.0568***		0.0263***
		(0.0021)		(0.0131)		(0.0015)
Compact State Resident * Nurse		0.0012		0.0401***		-0.0044
		(0.0035)		(0.0144)		(0.0028)
Constant	0.0332***	0.0187***	2.8489***	2.6544***	0.3700***	0.4220***
	(0.0076)	(0.0043)	(0.0355)	(0.0253)	(0.0138)	(0.0081)
Observations	375,059	1,016,980	371,362	998,976	450,916	1,255,987
R-squared	0.0373	0.0328	0.0234	0.0337	0.1292	0.0820
Sample	All Nurses	All Health	All Nurses	All Health	All Nurses	All Health
Y mean	0.0360	0.0317	3.0046	2.9481	0.8720	0.8585

Table 2.11: Effects of the NLC on Nurses and Health Workers Living in MSAs: 2000-2012

	(1)	(2)	(3)	(4)	(5)	(6)
	Works in a Different State		Commuting Time		In Labor Force	
Compact State Resident	-0.0069	-0.0058	0.0026	-0.0161*	-0.0126***	-0.0090**
	(0.0172)	(0.0113)	(0.0111)	(0.0087)	(0.0043)	(0.0040)
Nurse		0.0056***		0.0437***		0.0272***
		(0.0015)		(0.0134)		(0.0016)
Compact State Resident * Nurse		-0.0024		0.0329**		-0.0023
		(0.0033)		(0.0154)		(0.0025)
Constant	0.0343***	0.0238***	2.8571***	2.6848***	0.3620***	0.4265***
	(0.0091)	(0.0058)	(0.0434)	(0.0194)	(0.0117)	(0.0069)
Observations	240,565	671,046	238,161	658,919	289,335	829,750
R-squared	0.0878	0.0786	0.0498	0.0568	0.1246	0.0778
Sample	All Nurses	All Health	All Nurses	All Health	All Nurses	All Health
Y mean	0.0284	0.0257	3.0221	2.9841	0.8724	0.8588

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

Notes: The regressions in Table 2.11 have year fixed effects and MSA fixed effects included. The regressions in Table 2.10 have year fixed effect and state fixed effects included. The regressions are weighted using ACS person weights, standard errors are clustered at the MSA Level, and include controls for Sex, Race, Education, Age, and Age^2. In columns (1)-(4), the sample is restricted to workers currently employed.

Table 2.12: Effects of the NLC on Nurses and Health Workers Living in Non-MSAs: 2000-2012

	(1)	(2)	(3)	(4)	(5)	(6)
	Works in a Different State		Commuting Time		In Labor Force	
Compact State Resident	0.0049	0.0046	-0.0348**	-0.0345*	-0.0076	0.0039
	(0.0041)	(0.0046)	(0.0164)	(0.0186)	(0.0050)	(0.0060)
Nurse		0.0079***		0.1020***		0.0257***
		(0.0019)		(0.0069)		(0.0024)
Compact State Resident * Nurse		0.0056		0.0156		-0.0073*
		(0.0041)		(0.0115)		(0.0037)
Constant	0.0219**	0.0060	2.8337***	2.6197***	0.3884***	0.4172***
	(0.0104)	(0.0069)	(0.0512)	(0.0467)	(0.0163)	(0.0119)
Observations	134,494	345,934	133,201	340,057	161,581	426,237
R-squared	0.0287	0.0249	0.0276	0.0408	0.1413	0.0941
Sample	All Nurses	All Health	All Nurses	All Health	All Nurses	All Health
Y mean	0.0495	0.0433	2.9733	2.8784	0.8711	0.8580

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

Notes: All regressions have year fixed effect and state fixed effects included. The regressions are weighted using ACS person weights, standard errors are clustered at the MSA Level, and include controls for Sex, Race, Education, Age, and Age^2. In columns (1)-(4), the sample is restricted to workers currently employed.

Table 2.13: Effects of the NLC on Nurses and Health Workers Living in Border MSAs: 2000-2012

	(1)	(2)	(3)	(4)	(5)	(6)
	Works in a Different State		Commuting Time		In Labor Force	
Compact State Resident	-0.0208	-0.0305	0.0045	-0.0392*	-0.0110	-0.0012
	(0.0597)	(0.0404)	(0.0319)	(0.0214)	(0.0067)	(0.0075)
Nurse		0.0156***		-0.0092		0.0263***
		(0.0049)		(0.0310)		(0.0045)
Compact State Resident * Nurse		0.0122		0.0825**		-0.0085
		(0.0100)		(0.0338)		(0.0051)
Constant	0.1072***	0.0515**	3.1267***	2.8134***	0.3896***	0.4435***
	(0.0322)	(0.0204)	(0.1033)	(0.0367)	(0.0286)	(0.0122)
Observations	52,027	149,557	51,535	147,328	62,447	185,009
R-squared	0.0555	0.0588	0.0372	0.0550	0.1199	0.0759
Sample	All Nurses	All Health	All Nurses	All Health	All Nurses	All Health
Y mean	0.0898	0.0776	3.1184	3.1167	0.8751	0.8609

Table 2.14: Effects of the NLC on Nurses and Health Workers Living in Non-Border MSAs: 2000-2012

	(1)	(2)	(3)	(4)	(5)	(6)
	Works in a Different State		Commuting Time		In Labor Force	
Compact State Resident	-0.0007	0.0008	-0.0008	-0.0066	-0.0122**	-0.0098***
	(0.0022)	(0.0012)	(0.0094)	(0.0067)	(0.0056)	(0.0035)
Nurse		0.0019***		0.0622***		0.0276***
		(0.0007)		(0.0041)		(0.0015)
Compact State Resident * Nurse		-0.0027**		0.0142*		-0.0013
		(0.0013)		(0.0080)		(0.0027)
Constant	0.0112**	0.0132***	2.7798***	2.6444***	0.3536***	0.4201***
	(0.0045)	(0.0029)	(0.0356)	(0.0230)	(0.0133)	(0.0087)
Observations	188,538	521,489	186,626	511,591	226,888	644,741
R-squared	0.0497	0.0421	0.0471	0.0455	0.1259	0.0784
Sample	All Nurses	All Health	All Nurses	All Health	All Nurses	All Health
Y mean	0.0115	0.0108	2.9955	2.9459	0.8717	0.8582

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

Notes: All regressions in tables 2.13 and 2.14 have year fixed effects and MSA fixed effects included. The regressions are weighted using ACS person weights and standard errors are clustered at the MSA Level. Each regression includes controls for Sex, Race, Education, Age, and Age^2. The odd numbered columns are the sample of registered nurses and licensed practical nurses ("All Nurses") and the even numbered columns are the sample of health workers including nurses ("All Health"). In columns (1)-(4), the sample is restricted to workers currently employed.

2.7.2 Alternative Specification: Difference-in-Difference-in-Difference-in-Differences

Table 2.15: Alternative Specification: Equation 2.3

	(1) Works in a Different State	(2) Commuting Time
Compact State Resident	0.0015	-0.5470*
	(0.0020)	(0.3311)
Nurse	0.0052***	0.6925*
	(0.0014)	(0.3760)
Compact State Resident * Nurse	-0.0061***	0.9558**
	(0.0019)	(0.4058)
Border MSA	0.1005***	-2.4261***
	(0.0261)	(0.4765)
Compact * Border MSA	-0.0371	-0.0818
	(0.0300)	(0.2816)
Compact * Border MSA * Nurse	0.0281***	-0.0085
	(0.0076)	(0.3308)
Constant	-0.0076	5.5887***
	(0.0051)	(0.4394)
Observations	844,958	867,781
R-squared	0.0811	0.0477
Sample	All Health	All Health
Y mean	0.0258	23.6564

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

2.7.3 Effects of the Nurse Licensure Compact: Using Data from the NSSRN

Table 2.16: Summary Statistics and Basic Specification with NSSRN Data

	Nurses only (NSSRN)		
	All states	Part of Compact	Never in Compact
Share of workers who			
work in state of residence	0.95	0.94	0.96
work in different state	0.05	0.06	0.04
Observations	216,411	81,612	105,431
Number of states	51	22	29

Regression Result

	Different State
Lives in a Compact State	-0.0057
	0.0042
Observations	211,463
R-Squared	0.05

CHAPTER III

Have the obesity employment and wage penalties disappeared? Evidence from the NLSY, 1989-2008.

3.1 Introduction

It is no secret that the waistlines of Americans have been expanding. The obesity prevalence rate, defined as the share of people with a Body Mass Index (BMI) of 30 or higher, has increased in the United States from 15% in 1980 to 34.9% in 2012 (Finkelstein et al. 2003; Ogden et al. 2014). Not surprisingly given its large medical and economic costs (Cawley and Meyerhoefer 2012 estimate that 20.6% of U.S. national health expenditures are spent treating obesity-related illnesses), a vast literature has emerged on the causes and consequences of obesity.

Economists, in particular, have devoted a great deal of attention to the labor market consequences of obesity, namely wages and, to a more limited extent, employment. In theory, obesity can negatively affect employment and wages for at least three, non mutually exclusive reasons: first, employers might be reluctant to hire obese workers because these might have lower productivity; second, employers who provide health insurance might be concerned about paying higher health care costs when they employ obese individuals; third, employers might discriminate against obese persons either because they or their customers have a distaste for interacting with them.

The economics literature has documented important labor market penalties for obese individuals in the US, in particular white females, who have been found to suffer a wage penalty around 7.5-9 percent (Cawley 2004; Han, Norton and Stearns 2009). The evidence on the effects of weight on the employment prospects of Americans is much more limited. Han et al. (2009) find that the probability of employment decreases for obese white and Hispanic females. The fact that the negative consequences of weight on labor market outcomes tend to mainly affect white females thus appears to be a consistent finding in the literature (we offer a more detailed discussion of the literature in the next section). The literature is more mixed, however,

This chapter is co-authored with Mario Macis.

when it comes to what explains the findings, with some authors concluding in favor of employer or consumer distaste for obese workers (Cawley 2007; Han et al. 2009) and others pointing to the higher health care costs Bhattacharya and Bundorf 2009).

In this chapter, we use data from the 1989-2008 National Longitudinal Survey of Youth (NLSY), and re-examine the wage and employment effects of obesity. Most of the studies on the labor market consequences of weight have been conducted with NLSY data; however, virtually all of the studies have examined the years before 2000, and all of the studies have typically shown results from pooling all the data together. In this chapter, we extend the data until 2008 and explicitly examine the evolution of the wage penalty over time. Doing so is important for at least two reasons. First, documenting whether the wage penalty has changed or remained stable over time is important in its own right. Second, as we explain below in detail, examining their evolution over time can be used to distinguish between competing explanations for the existence of wage and employment penalties.

Our findings can be summarized as follows. When examining the data as a whole, we find no significant wage penalty for obese individuals. When splitting the data, however, and examining the years before 2000, we do confirm the wage penalty documented in previous studies for obese and overweight white females. In more recent years, however, the penalty seems to have disappeared. Similarly, we confirm a negative effect of being obese on the employment probability of white females but only limited to the years before 2000; after the year 2000, if anything overweight and obese white female have higher employment probabilities compared to females of normal weight.

The disappearance of the wage penalty is inconsistent with explanations for the wage and employment penalties based on healthcare costs. Healthcare costs related to obesity have been rising steadily in the past decades. A 2012 report by the Robert Wood Johnson foundation suggests that the yearly medical cost associated with obesity is between $147 billion and $210 billion and a recent study finds that these costs could grow to as much as $550 billion in the next 15 years (Finkelstein et al. (2012). These costs, contrary to what we find, should have worsened the employment and wage prospects of obese individuals.

The findings are, however, consistent with explanations based on weight-based discrimination against female employees. First, theories in psychology and sociology suggest that discrimination against a certain trait is negatively correlated with the prevalence of that trait among the relevant population (Cawley 2007), and obesity is less prevalent among white females (30.7 percent according to Hedley et al. 2004) than among hispanic females (34.8 percent) or black females (49 percent); this is consistent with obese and overweight white females, but not hispanic or black, suffering employment and wage penalties in the first part of the period we analyzed.

Second, as reported above, the prevalence of obesity has increased over time. In 2012, the total of obese and overweight (i.e., individuals with BMI above 25) in the United States reached 68.5 percent overall; the figure among white females was 63.2 percent (Ogden et al. 2014). Figure 3.1 provides a map of state obesity trends over the past 25 years. When fewer people were obese, employers' distaste for these workers could have led to reduced employment opportunities and lower wages. As the fraction of above-normal weight people increased, employers (who are themselves likely to be obese) might be less inclined to discriminate against fellow obese individuals. In other words, as "overweight" becomes the new "normal", discrimination is reduced or disappears.

Finally, Figure 3.1 also illustrates the non-uniformity of obesity. This lack of a uniform-trend across the U.S. may leads to different biases towards obesity in different areas. We are able to perform a baseline test of this explanation by estimating the wages for obese individuals in regions with relatively higher BMIs and higher percentages of obesity versus obese individuals working in regions with lower BMIs. If this is a form of social discrimination, we would expect those regions with a higher prevalence of obesity to discriminate less. Our results provide evidence of this explanation as we find that obese individuals living in the regions with the a higher percentage of obesity earn higher wages than those obese individuals in other regions.

The chapter proceeds as follows. After discussing the related literature in section 3.2, we describe the data in section 3.3. In section 3.4, we present our empirical methods and our results. In section 3.5 we conclude and offer directions for future research.

3.2 Related Literature

The related literature has focused on documenting the prevalence of obesity among children and adults (Hedley et al. (2004), Ogden et al (2014)), the health care costs attributable to obesity (Finkelstein et al, 2003; Cawley and Meyerhoefer, 2012; Ogden et al., 2014), the labor market consequences of obesity (Register and Williams, 1990; Mitra, 2001; Cawley, 2004; Baum and Ford, 2004, Morris, 2006; Cawley, 2007; Johansoon et al., 2007; Bhattacharya and Bundorf, 2009; Han et al., 2009), and the explanations for these labor market effects (Hedley et al., Bhattacharya and Bundorf, 2009; Han et al., 2009; Ogden, 2014).

Research has shown a consistent wage penalty for obese women in the United States. Most of the literature continually finds a negative correlation between obesity and wages among women and less of a penalty (if any) for obese men. In his seminal paper, Cawley (2004) finds that a difference in weight of 64 pounds is associated with a decrease in wages of 9% for white women. Higher healthcare costs (Bhattacharya and Bundorf, 2009), social discrimination (Mitra, 2001; Baum and Ford, 2004) and lower productivity (Mitra, 2001) are often cited and explored as possible causes for this wage penalty.

Studies that examine the wage penalty for obesity look at years as early as 1979 (Baum and Ford, 2004) and as late the early 2000s (Bhattacharya and Bundorf 2009; Gregory and Ruhm, 2009). These studies also document wage penalties and depending on the study, find that obese females experience a larger wage penalty than obese men. Depending on their specification, they find the wage penalty for obese females to be 1.6-2.6% larger than the penalty for obese males (Baum and Ford, 2004).

Many of the studies try to offer an explanation for this wage penalty. Bhattacharya and Bundorf (2009) observe that among the insured, obese women earn $1.66 per hour less than non-obese women while men earn $1.21 per hour less than non-obese men. Workers without employer-sponsored insurance see no wage penalty. Through an examination of medical expenditure data, they conclude that $583 of the average obese woman's yearly wage penalty can be attributed to higher medical expenditures. Baum and Ford (2004), on the other hand, find that obese individuals with employer-provided health insurance receive less of a wage penalty than those obese individuals without such health insurance. Gregory and Ruhm (2009) pay particular attention to how wages vary within different groups of people, such as white males or black females. When examining wage functions by weight, they find that wage functions peak at much higher levels for minorities, i.e. the highest average wage earned by a minority occurs at a weight that is much higher than it is for white individuals. This suggests racial differences in perception of desired body weight. They also find that white females wages peak well below the clinical cutoff for obesity. Given that health care costs are the same for overweight (non-obese) individuals and normal weight individuals, this finding lends some credence to the belief that this penalty is a form of social discrimination.

Much less research has been done on the consequences of obesity on employment (in comparison to the effect on wages). Han et al. (2009) is the only study to examine employment effects in the United States. The authors use data from the NLSY for the years 1982-1998 and find that the probability of employment decreases with weight among white females. Other studies show some evidence of lower employment of obese individuals in Europe (Morris, 2006; Johansson et al, 2007).

While wage and employment penalties for obese individuals are well documented, all of the aforementioned studies focus on years prior to 2003. Although the rate of obesity has been increasing, it is not obvious from the previous studies that this penalty would increase along with it or remain stable. In fact, if the penalty is a form of social discrimination, as the prevalence of obesity increases, and obesity becomes the new majority, we may even see this penalty decrease. In this chapter, we examine data from 1989-2008, which hopefully gives us a better sense of the current labor market consequences of obesity.

3.3 Data

The National Longitudinal Survey of Youth (NLSY) is a nationally representative sample of 12,686 people aged 14-22 who were first surveyed in 1979. This survey continued annually until 1994 and biennially thereafter. The NLSY contains information on wages, insurance sources, fringe benefits, height, weight, and many other demographic characteristics, and is used among other things to analyze obesity and worker wages. The NLSY provides a good dataset to study our question as it provides an accurate representation of BMI trends over the sample period. Figure 3.2 shows the BMI trends of population of adults in the United States compared with those in the NLSY. As we can see, these follow a very similar path.

Following Cawley's (2004) and Bhattacharya and Bundorf's (2009) lead, we construct a panel data set as follows. First, we use the NLSY for the years 1989-2008 and are able to follow individual respondents across those years. The year 1991 is omitted because weight was not recorded for that survey year. Therefore, the maximum number of times an individual appears in the dataset is twelve. After accounting for attrition throughout the years, we are left with an unbalanced panel of 103,823 observations.

Next, the sample is restricted to exclude pregnant women and observations with missing data for the key variables, namely weight, height, and hourly wage, and those whose height and weight result in a either a negative BMI or a BMI greater than 100. This leaves a final sample size of 95,858.

The key dependent variable, hourly wage, is top and bottom coded at \$1 and \$500 (Cawley, 2004). In order to construct the BMI, we need an individual's height and weight. While weight is recorded every survey year, height was only measured up until 1985. Thus, we take the individuals height in 1985 and apply it to all years in our dataset. While it might be a concern that respondents are still growing after 1985, the youngest individual at that time was 20 years old. We assume that respondents stop growing after that. BMI is then constructed by using the regular BMI formula and we use the standard BMI cutoffs to create indicators for different weight categories: a person with a BMI of less than 25 is considered to be of normal weight, a BMI between 25 and 30 is classified as overweight, and a BMI greater than 30 is classified as obese.[1]

Table 3.1 provides the average wage (in dollars) of each gender and race segmented by weight group. Since our empirical strategy will focus on separating the earlier years from the later years, we present the average wage of each group before 2002 and after 2002. If we look at the entire sample ("All"), the difference between wages of normal weight and obese individuals have narrowed in later years. When we split the sample up into smaller demographic groups, however, we see that this narrowing is likely driven by the large positive wage gap between obese black males and normal weight black males: obese black males make about

[1]BMI is calculated by taking weight (lbs), multiplying by 703, and then dividing by height-squared (measured in inches). Those with a BMI under 18.5 are underweight and a those with a BMI over 35 are morbidly obese.

$4.5 more per hour. These simple tabulations provide little information and point to the need for the more precise empirical estimation detailed in section 3.4.

We include controls for standard demographic variables, i.e. age, age^2, gender, and race. We also include controls for variables that might affect a worker's earning potential. That is, whether there are any children in the household, marital status, educational attainment categories, job tenure, location of residence, number of employees at the workplace, industry category, and occupational category.[2] The presence of any children in the household, coded as a dummy variable, and its interaction with gender are also included in the regressions to account for the idea that children have a greater impact on a female's participation in the workforce in comparison to men. We also include an indicator representing the quartile of a respondent's AFQT score. The AFQT is the armed forces qualification test. There are four components to the score: word knowledge, paragraph comprehension, arithmetic reasoning, and mathematics knowledge. The score is a percentile score and ranges from 0-99. We created four dummy variables to represent scoring in each quartile. Thus, respondents who scored in the 0-25th percentile would receive a 1 for the first score dummy variable, while those scored above the 25th percentile would receive a zero. Summary statistics for these variables are presented in table 3.2.

3.4 Empirical Methods and Results

3.4.1 Wage Penalty

In order to examine how the wage penalty for obese individuals has changed over time we estimate the following equation using Ordinary-Least Squares (OLS):

$$lnwage_{it} = \beta_0 + \beta_1 WeightMeasure_{it} + \beta_x X_{it} + \gamma_i + \gamma_t + \epsilon_{it} \tag{3.1}$$

where $lnwage_{it}$ is the natural log of the hourly wage of individual i in year t, X_{it} is a vector of individual characteristics (age, gender, race, children, marital status, education, industry, occupation), γ_i is a person-specific fixed effect, γ_t is a time fixed-effect, and ϵ_{it} is the orthogonal regression error, which is clustered at the individual level. $WeightMeasure_{it}$ is the weight of i in year t, where we use three different measures of weight (Cawley, 2004): BMI, an individuals weight (controlling for height), and three different BMI category indicators: underweight (BMI under 18.5), overweight (BMI between 30-35), and obese (BMI over 35). Each

[2]The industries are agriculture; forestry and fisheries; mining; construction; manufacturing; transportation, communications, and other public utilities; wholesale trade; retail trade; finance, insurance and real estate; business and repair services; personal services; entertainment and recreation services; and public administration. Occupations are managerial and professional specialty; technical and sales; administrative support; service; farming, forestry, and fishing; precision, production, craft, and repair; and operators, fabricators, and laborers.

wage equation is first estimated using OLS and then re-estimated to include individual fixed-effects. Given the possible reverse causality of weight and wages, that is people who earn less may tend to gain weight (e.g., by say, eating lower-cost, unhealthier food), we assume that lagged weight is uncorrelated with the current wage residual and follow Cawley (2004) and substitute in a 5-year lagged measure of weight. Each regression is run for the full-time employed sample and is then segmented by gender and race. Most importantly, to capture the changing wage penalty over time, we also run two separate regressions for years 1989-2000 and years 2002-2008, in addition to estimating for the full sample period.

For the sake of brevity, we discuss the results of only the female groups since females (specifically white females) are the individuals who have most consistently had a documented wage penalty attributable to obesity. The results for the entire sample, all males, all females, white males, and black males, can be found in the Appendix, Section 3.6.1, tables 3.20-3.25.

In table 3.3, we present the results when looking at the entire sample period of white and black females for years 1989-2008. Columns (1), (2), (4), and (5) do not include individual fixed effects and Columns (2) and (5) replace the weight measure with 5-year lagged weight measures. The OLS estimations indicate a wage penalty for heavier white and black females. In addition, the coefficients on the lagged weight measures are very similar to the non-lagged weight measures, implying that there is no reverse causality, and that current wages have no effect on current weight. When we include fixed effects, however, most of the significance on each coefficient disappears, including in the case of white females (table 3.3, column 3). These results are inconsistent with the previously documented wage penalty for white females.

To more closely examine the wage penalty by year, we estimate the following two equations:

$$lnwage_{it} = \alpha + \beta Before2002_{it} + \lambda BMICategory_{it} + \delta BMICategory_{it} * Before2002 + \rho X_{it} + \gamma_i + \gamma_t + \epsilon_{it} \quad (3.2)$$

$$lnwage_{it} = \alpha + \beta Year + \lambda BMICategory_{it} + \delta BMICategory_{it} * Year + \rho X_{it} + \gamma_i + \epsilon_{it} \quad (3.3)$$

where $BMICategory_{it}$ is one of the three different BMI category indicators: underweight (BMI under 18.5), overweight (BMI between 30-35), and obese (BMI over 35), $Before2002_{it}$ is an indicator if the year is before 2002, $Year_{it}$ is a vector of years, and $BMICategory_{it} * Year$ is an interaction between each BMI category and the year.

Table 3.4 presents the results for equation 3.2 and table 3.5 presents the results for equation 3.3. The results in table 3.4 show that obese white females earn 6.5% less in years before 2002 in comparison to normal weight white females in those years. These results are the same whether looking cross-sectionally (the first column) or within person (the second column). When examining the interaction of obese and year (table 3.5), we see that the log wage of obese white females tends to increase each year, with a large increase in years after 2002 (i.e., years 2004, 2006, and 2008). While it may seem curious that the jump in wages is so large for white females in years after 2002, Figure 3.3 sheds some light on this result. In this figure, we see not only a fairly steady increase in the log wages of obese white females (the green line) but also a decrease in the difference in wages between normal weight (the blue line) and obese white females. In other words, while the difference in the log wage of normal weight and obese white females was quite large in 1989, this difference has substantially decreased in later years, and sharply decreased in year 2004. This convergence of log wages in 2004 accounts for the large positive coefficient on the interaction between obese and the year 2004 (2004* BMIObese). For the rest of the wage and employment analyses, therefore, we will separately examine the years before 2002 and the years 2002 and later.

In table 3.6, we present the results for the years 1989-2000 only. Unlike the results for the the entire sample, the estimates for those years most commonly examined in the obesity literature are consistent with the wage penalty of obese white females that has been documented. Regardless of the measure of weight is used, heavier and obese white females earn a lower wage than their thinner counterparts (coefficients are negative and significant). This result persists when including individual fixed effects. For black females, the wage penalty for obesity becomes insignificant when we include individual fixed-effects.

When we examine the later years of our sample, a previously unexamined time period in the obesity literature, the coefficient on obesity for white females in the fixed-effects regression essentially becomes zero (-0.0009; table 3.7, Column (3)). In addition, the coefficient on obesity for black females in the fixed-effect regression (Column (6)) is also very insignificant. These results suggest that the previously documented wage penalty for females has disappeared in recent years.

3.4.2 Employment Penalty

Next, we examine how the employment penalty has changed over time. We estimate a marginal probit regression for the entire range of years (1989-2008), as well as years before 2000 and the years after 2000 using the full sample of employed and unemployed individuals:

$$Prob(Employment_{it}) = \beta_0 + \beta_1 Underweight_{it} + \beta_2 Overweight_{it} + \beta_3 Obese_{it} + \beta_x X_{it} + \gamma_i + \gamma_t + \epsilon_{it} \quad (3.4)$$

where $Employment_{it}$ is 1 if the individual works at least 1 hour per week, and $Underweight_{it}$, $Overweight_{it}$, and $Obese_{it}$ are the same BMI categorical indicators from the wage regressions. As before, equation (2), is estimated for both the full sample and then stratified by both gender and race.[3]

The results for years 1989-2008 are in table 3.8. Similar to the wage regressions over the entire sample, we find small and statistically insignificant coefficients on the obese indicators for every subsample. In fact, most of these coefficients, including the one for obese white females, are positive although statistically indistinguishable from zero. Worth noting is the employment penalty of those who fall in to the underweight category. This is consistent for each race and gender group and points to two possible reasons: social discrimination against those who appear "scrawny" and"weak," and lower productivity, as those who are clinically underweight are perhaps sicker than those who are at a healthier weight.

The results when we separate the years before 2000 and after 2000 are presented in Tables 3.9 and 3.10. Looking at Column (1) of table 3.9, we see that obese individuals have a lower probability of employment in comparison to "normal" weight individuals. When we disaggregate this sample, obese males and obese females, specifically white males and females, still have a negative (but statistically insignificant) coefficient indicating that they face an employment penalty in comparison to their normal weight counterparts. For the years after 2000 (table 3.10), not only does this penalty disappear, but obese white females, and obese females in general, are more likely to be employed than "normal" weight females/white females. These results are consistent with our wage results in that they imply that the labor market penalty for obesity has disappeared in later years.

3.4.3 Regional Analysis

We next attempt to identify the source of the decreasing wage and employment penalties of obese individuals. If the obesity penalty is due to social discrimination, then the negative effect should decrease as the share of Americans who are overweight increases, either because firms no longer have a choice but to hire people who are overweight, or because being overweight might become the new "normal". In other words, the decrease in the penalty that we see in tables 3.4, and 3.7, is due to a decrease in discrimination stemming from the fact that the average person in the U.S. is now overweight or obese. Thus, we would expect both the wage and employment penalties to be lower for those obese workers living in areas with high BMI. We use the most micro locational data available, "region", to identify which region has the highest average BMI in each year. The NLSY identify four regions: 1.) North East, 2.) North Central, 3.) South, and 4.) West.[4] Figure 3.4 shows the trend of wages of obese individuals in regions with the highest BMI versus

[3]For alternative specifications, see the Appendix, Section 3.6.2, which displays the results when we redefine "employed" to be those who work full-time.

[4]For a list of the states that are part of each region, see the Appendix, Section 3.6.3, table 3.29.

obese individuals in other regions (i.e. those with relatively lower BMI). There is a clear and consistent wage gap between obese individuals in the "High BMI" regions and those in the "Low BMI" regions, and this gap widens in later years.

To test whether wages differ for obese individuals living in different regions of the U.S., we estimate the following two specifications:

$$lnwage_{it} = \alpha + \beta RegionWeight_t + \lambda BMICategory_{it} + \delta BMICategory_{it} * RegionWeight_{it} + \rho X_{it} + \gamma_i + \gamma_t + \epsilon_{it} \quad (3.5)$$

$$ProbEmployment_{it} = \alpha + \beta RegionWeight_{it} + \lambda BMICategory_{it} + \delta BMICategory_{it} * RegionWeight_{it} + \rho X_{it} + \gamma_t + \epsilon_{it} \quad (3.6)$$

where $RegionWeight_{it}$ can be two different measures of the relative weight of a region 1.) and indicator equal to 1 if the individual is living in the region with the highest BMI in year t; or 2.) the percentage of obesity in a given region in year t. The coefficient δ represents the interaction between this regional weight measure and the different BMI Categories. We include a person-specific fixed effect for some of the anlayses as well as region by year fixed effects where appropriate.

Tables 3.11-3.13 display the results when examining individuals living in the region with the highest BMI. We see that for the cross-sectional specifications, obese females overall, as well as white and black females specifically, earn higher wages in the regions with the highest BMI. For instance, obese females in the region with the highest BMI earn about 2.0% higher wages than normal weight individuals in the same region. This result holds when looking within-person. In addition, we see a slightly larger magnitude on this coefficient when looking specifically at white females–the group most discriminated against (columns 5 and 5, table 3.11). Similarly, we see an increase in the probability of employment for females, specifically white obese females, who live in a region with the highest BMI.

Simply looking at the region with the highest BMI does not provide the variation needed to control for the year or region since this variable does not vary over time–that is, the region with the highest BMI tends to stay the same. Therefore, we cannot rule out that some regions pay higher wages. As a result, we estimate equations 3.5 and 3.6 using the percentage of obesity within a region, something that varies every year. This allows us to control for each region and year in the specifications. Tables 3.14-3.16 display these results. The results indicate that as the percentage of obesity in a region increases, the log wages of obese white females increase as well (the cross-section and within-person coefficients are 0.34 and 0.30, respectively, significant at the 10% level). The same is true when examining the probability of employment. We also estimate the same specifications and include region*year fixed-effects (tables 3.17-3.19). The results largely stay the same, although the magnitude on the coefficient for white females when including the individual-level fixed effects is insignificant due to the slight decrease in magnitude.

3.5 Concluding Remarks

In this chapter, we use data from the 1989-2008 National Longitudinal Survey of Youth (NLSY), and re-examine the wage and employment effects of obesity. Most of the studies on the labor market consequences of weight have been conducted with NLSY data; however, virtually all of the studies have examined the years before 2000, and all of the studies have typically shown results from pooling all the data together. In this chapter, we extend the data until 2008 and explicitly examine the evolution of the wage penalty over time. We find that the group of individuals believed to be most discriminated against, obese white females, have seen both their wage penalty and employment penalty decrease over time.

Many papers have put forth competing explanations for this wage penalty, specifically that the wage penalty is driven by higher health care costs for obese individuals versus the penalty as a form of social discrimination. We offer support for the latter explanation by examining the wage penalty across regions. We find that obese individuals who live in regions with an average higher BMI, suffer less of a wage and employment penalty than those individuals in regions with relatively lower BMIs. Furthermore, we find that as the percentage of obesity in a region increases, so does the wage for obese white females. In addition, our OLS result for the earlier years do not support higher health care costs as the driving factor of the penalty since there is an obvious wage penalty for overweight women, yet overweight people do not incur greater medical expenditures (Finkelstein et al., 2003).

There are, of course, limitations with our studies and many avenues for future research. First, BMI is not the best measure of obesity as there is a bias against tall people that are relatively fit. That is, tall people who do not appear overweight to the naked eye may in fact be classified as obese and yet would be less likely to be socially discriminated against. This bias, however, is likely consistent through out the years and would be unlikely to change in the later years.

Second, our region measure is a crude divider of the location of obese individuals. More micro-level data would provide better precision and could help dive further into the discriminations across areas with higher BMIs.[5]

Finally, even though our results appear to support the idea that the penalty is disappearing and that

[5]The NLSY has detailed Geocode data, which would allow us to see the State that the respondent lives in. We are in the process of receiving this restricted data.

this result is consistent with a decline in social discrimination, this does not rule out the health care cost explanation. For instance, if the penalty is limited to jobs that offer insurance, it is possible the penalty has declined because insurance coverage has declined over time. A complete analysis would include an analysis of the wages of jobs that do offer employer-sponsored insurance versus jobs that do not.

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Figure 3.1: U.S. State by State Obesity Trends Over Time

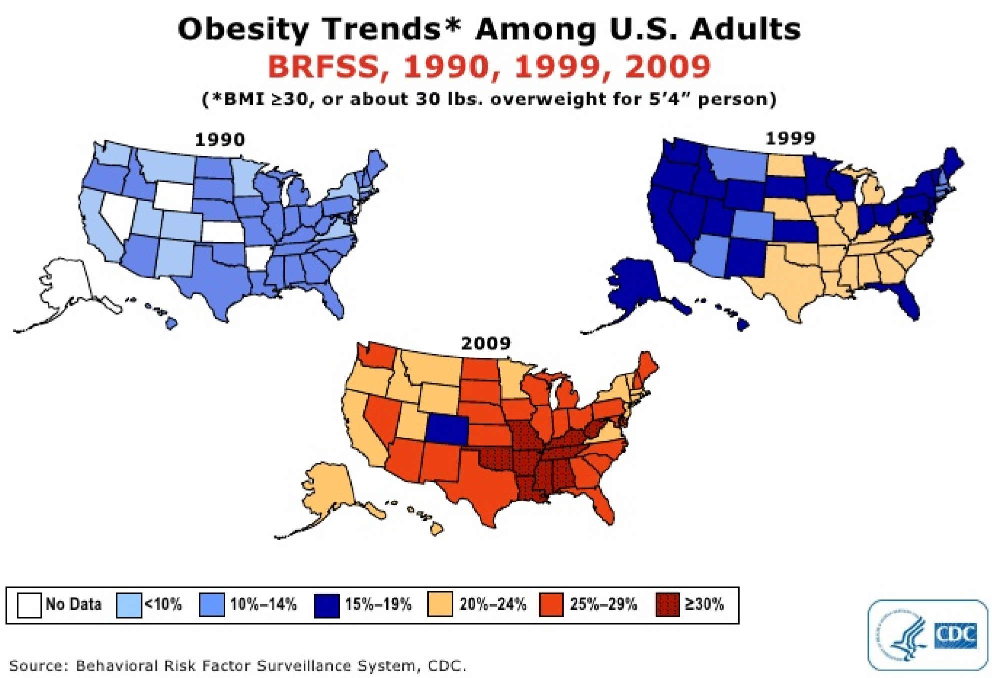

Figure 3.2: BMI Trends: NLSY Statistics Compared with CDC Statistics

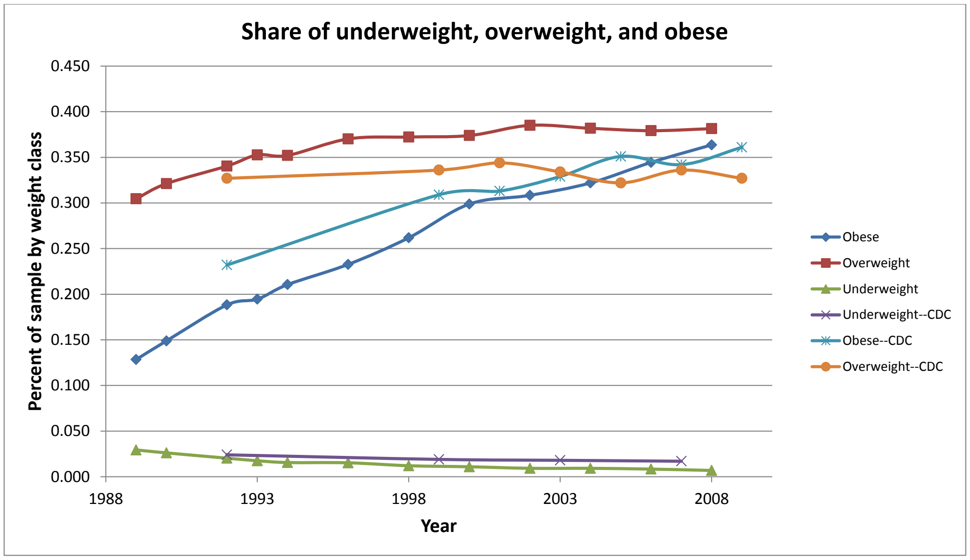

Figure 3.3: Log Wages of White Females Over Time: Separated by Weight Classification

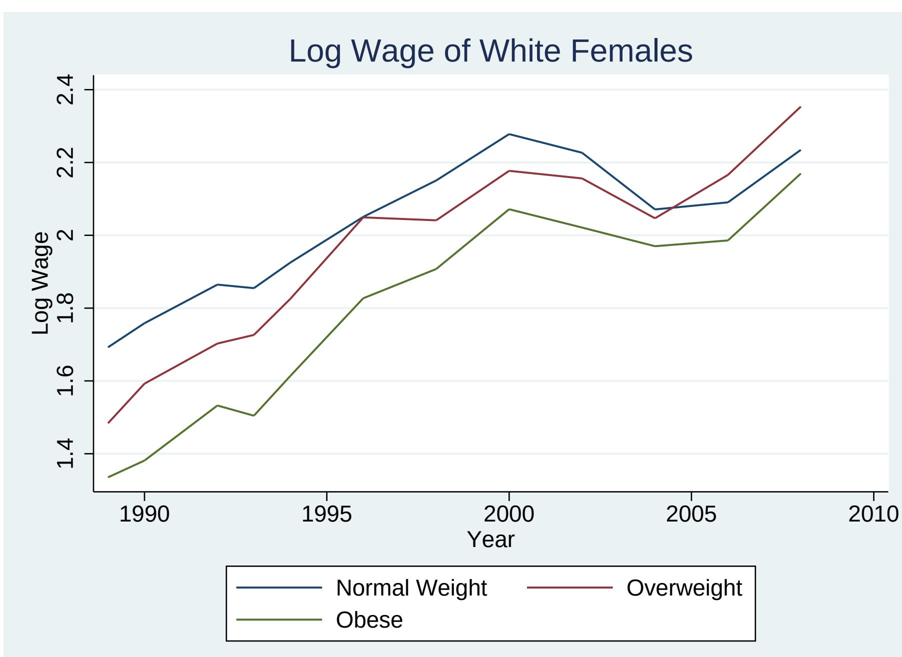

Figure 3.4: Log Wages of Obese Individuals Living in Regions with Relatively High BMI

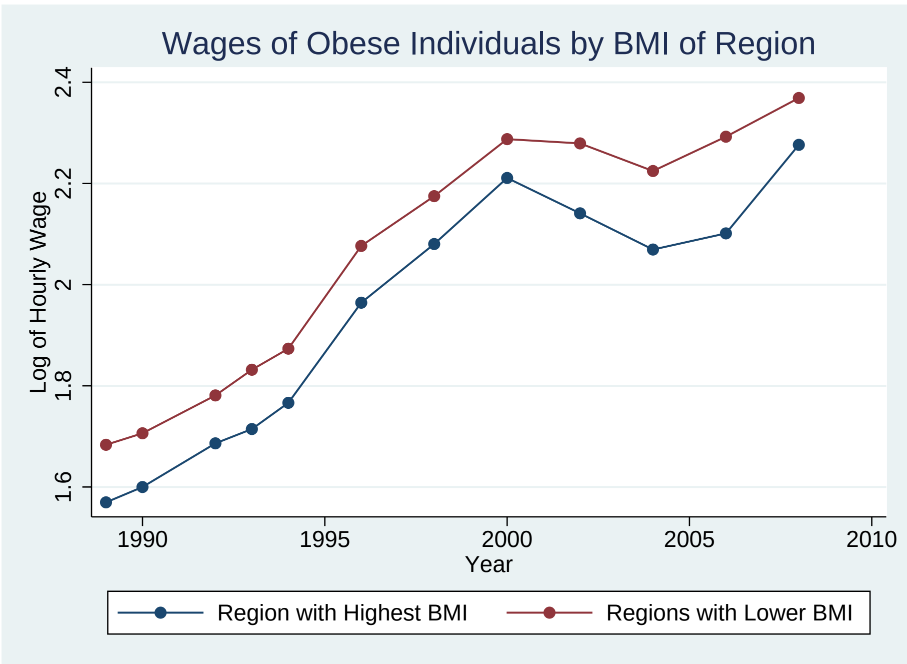

Table 3.1: Average Wage (in dollars) by Gender, Race, and Weight Group

	Before 2002	After 2002		Before 2002	After 2002
All			White Female		
Normal Weight	11.295	16.851	Normal Weight	10.729	15.790
Obese	10.075	15.051	Obese	8.249	12.802
Overweight	11.645	18.704	Overweight	9.645	15.265
Underweight	9.042	10.640	Underweight	10.170	11.992
Female			White Male		
Normal Weight	10.989	14.250	Normal Weight	14.597	24.270
Obese	7.309	11.973	Obese	13.659	19.976
Overweight	7.605	13.309	Overweight	14.751	24.642
Underweight	6.160	15.701	Underweight	9.341	8.315
Male			Black Female		
Normal Weight	11.689	16.316	Normal Weight	7.861	12.360
Obese	12.157	15.901	Obese	7.278	11.291
Overweight	11.900	17.845	Overweight	8.427	12.291
Underweight	21.943		Underweight	5.404	8.053
			Black Male		
			Normal Weight	9.150	10.724
			Obese	10.654	15.363
			Overweight	9.772	16.040
			Underweight	4.009	5.066

Table 3.2: Summary Statistics by Weight Group

	Underweight	Normal Weight	Overweight	Obese
Sample Size	1516	36861	34212	23269
Male	0.153	0.407	0.618	0.478
Age	33.278	34.520	36.372	37.926
Age^2	1144.177	1229.947	1364.212	1478.377
Urban Residence	0.785	0.779	0.773	0.753
AFQT Score $\leq$ 25	0.340	0.333	0.388	0.457
25 $<$ AFQT Score $\leq$ 50	0.315	0.252	0.248	0.242
50 $<$ AFQT Score $\leq$ 75	0.162	0.194	0.169	0.155
75 $<$ AFQT Score $\leq$ 100	0.135	0.181	0.153	0.108
Race Categories				
White	0.766	0.714	0.636	0.562
Black	0.197	0.238	0.303	0.375
Other	0.037	0.048	0.062	0.063
Marital Status				
Married	0.550	0.600	0.624	0.603
Never Married	0.242	0.249	0.234	0.259
Formerly Married	0.208	0.151	0.143	0.138
Education Categories				
Highest Grade $\leq$ Grade 9	0.035	0.025	0.030	0.040
9 $<$ Highest Grade $\leq$ 12	0.530	0.512	0.543	0.574
Highest Grade is 13 +	0.435	0.464	0.427	0.386
Job Characteristics				
Employer size $\leq$ 10	0.172	0.199	0.183	0.162
10 $<$ Employer size $\leq$ 25	0.115	0.113	0.114	0.109
25 $<$ Employer size $\leq$ 50	0.065	0.094	0.095	0.092
50 $<$ Employer size $\leq$ 1000	0.233	0.294	0.318	0.328
Employer size is 1000+	0.084	0.097	0.093	0.087
Job Tenure $\leq$ 1 Year	0.251	0.221	0.198	0.178
1 $<$ Job Tenure $\leq$ 3	0.197	0.215	0.207	0.193
3 $<$ Job Tenure $\leq$ 6	0.144	0.173	0.168	0.159
Job Tenure is 6+ Years	0.143	0.246	0.296	0.316

Table 3.3: Log Wage Regressions of White Females and Black Females 1989-2008

	(1)	(2)	(3)	(4)	(5)	(6)
		White Females			Black Females	
BMI	-0.0087***		-0.0006	-0.0037**		0.0015
	(0.0013)		(0.0020)	(0.0013)		(0.0020)
BMI (lag)		-0.0081***			-0.0045*	
		(0.0018)			(0.0018)	
Weight (in pounds)	-0.0015***		-0.0002	-0.0006**		0.0003
	(0.0002)		(0.0003)	(0.0002)		(0.0003)
Weight (lag)		-0.0014***			-0.0008*	
		(0.0003)			(0.0003)	
Underweight	-0.0106		-0.0199	-0.0353		-0.0335
	(0.0294)		(0.0347)	(0.0609)		(0.0577)
Overweight	-0.0310*		0.0129	0.0170		0.0486*
	(0.0157)		(0.0157)	(0.0192)		(0.0206)
Obese	-0.1194***		-0.0183	-0.0484*		0.0257
	(0.0189)		(0.0241)	(0.0209)		(0.0288)
Underweight (lag)		0.0406			0.0082	
		(0.0388)			(0.0731)	
Overweight (lag)		-0.0564**			-0.0154	
		(0.0214)			(0.0231)	
Obese (lag)		-0.0989***			-0.0565*	
		(0.0252)			(0.0258)	
Observations	24775	12237	24775	10765	5707	10765

Standard errors in parentheses, * ($p < 0.05$), ** ($p < 0.01$), *** ($p < 0.001$)
One of the three measures of weight is used: BMI, weight (controlling for height) or the three indicators
Column (2) (5) use lagged weight measures, Columns (3) (6) include individual fixed-effects
Controls for whether R has children, having children*female, marital status, age, age^2, education, firm size, job tenure, AFQT score, type of residence, industry, occupation, and year dummies are included in all regressions.

Table 3.4: Effect of Obesity on Wages of Females Before 2001

	White Females		Black Females	
Before 2001	-0.0253	0.1453***	-0.0900**	0.0524
	(0.0278)	(0.0242)	(0.0366)	(0.0338)
Underweight	-0.0310	0.0794	0.0759	-0.0166
	(0.0684)	(0.0661)	(0.1138)	(0.0731)
Overweight	0.0158	0.0548*	0.0203	0.0500
	(0.0307)	(0.0280)	(0.0381)	(0.0384)
Obese	-0.0657**	0.0217	-0.0014	0.0539
	(0.0311)	(0.0326)	(0.0382)	(0.0433)
Underweight*Before2001	0.0259	-0.1221*	-0.1244	-0.0247
	(0.0719)	(0.0662)	(0.0890)	(0.0809)
Overweight*Before2001	-0.0562*	-0.0588**	0.0067	0.0048
	(0.0312)	(0.0295)	(0.0399)	(0.0389)
Obese*Before2001	-0.0652**	-0.0654**	-0.0499	-0.0399
	(0.0316)	(0.0297)	(0.0376)	(0.0387)
Constant	-0.1879	-0.3491	-0.5532	-0.7104*
	(0.2296)	(0.2456)	(0.4885)	(0.4078)
Observations	24,775	24,775	10,765	10,765
R-squared	0.3039	0.6104	0.4033	0.6503
Person FE	NO	YES	NO	YES

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

Notes: Interactions with Year and Other BMI Categories are Included but not reported. Each regression includes controls for whether the woman has any children, marital status, age, age^2, education, AFQT score, whether she lives in an urban area, job tenure, employer size, industry and occupation.

Table 3.5: Effect of Obesity on Wages of Females: Year by Year

	White Females		Black Females	
Underweight	0.1236	0.1291	-0.0530	-0.0258
	(0.0946)	(0.1151)	(0.0537)	(0.0702)
Overweight	-0.0920***	-0.0207	0.0062	0.0449
	(0.0284)	(0.0325)	(0.0318)	(0.0351)
Obese	-0.1780***	-0.0726	-0.0680*	0.0311
	(0.0404)	(0.0495)	(0.0354)	(0.0426)
1990*BMIObese	0.0216	-0.0076	0.0050	0.0008
	(0.0478)	(0.0519)	(0.0408)	(0.0418)
1992*BMIObese	0.0702	0.0392	-0.0060	-0.0259
	(0.0487)	(0.0541)	(0.0471)	(0.0475)
1993*BMIObese	-0.0012	-0.0021	0.0288	0.0025
	(0.0514)	(0.0545)	(0.0443)	(0.0456)
1994*BMIObese	-0.0011	-0.0158	-0.0182	-0.0337
	(0.0460)	(0.0485)	(0.0460)	(0.0483)
1996*BMIObese	0.0497	0.0280	-0.0889*	-0.0965*
	(0.0471)	(0.0502)	(0.0494)	(0.0533)
1998*BMIObese	0.0471	0.0453	0.0537	-0.0028
	(0.0501)	(0.0525)	(0.0475)	(0.0503)
2000*BMIObese	0.0364	0.0248	0.0013	-0.0024
	(0.0482)	(0.0512)	(0.0521)	(0.0550)
2002*BMIObese	0.0271	0.0264	0.0343	0.0040
	(0.0502)	(0.0534)	(0.0575)	(0.0596)
2004*BMIObese	0.1903***	0.1716***	0.1529**	0.0829
	(0.0635)	(0.0663)	(0.0716)	(0.0748)
2006*BMIObese	0.1244*	0.0955	-0.0183	-0.0406
	(0.0721)	(0.0744)	(0.0744)	(0.0779)
2008*BMIObese	0.0916*	0.0643	0.0911	0.0458
	(0.0518)	(0.0567)	(0.0562)	(0.0589)
Constant	1.0594***	0.7333	0.6243	0.1972
	(0.2933)	(0.5606)	(0.5296)	(0.7188)
Observations	24,775	24,775	10,765	10,765
R-squared	0.3240	0.6160	0.4233	0.6532
Person FE	NO	YES	NO	YES

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

Notes: Interactions with year and other BMI categories are included but not reported. Each regression includes controls for whether the woman has any children, marital status, age, age^2, education, AFQT score, whether she lives in an urban area, job tenure, employer size, industry and occupation.

Table 3.6: Log Wage Regressions of White Females and Black Females 1989-2000

	(1)	(2)	(3)	(4)	(5)	(6)
	White Females			Black Females		
BMI	-0.0097***		-0.0026	-0.0044**		-0.0027
	(0.0013)		(0.0024)	(0.0014)		(0.0024)
BMI (lag)		-0.0097***			-0.0044*	
		(0.0019)			(0.0022)	
Weight (in pounds)	-0.0016***		-0.0005	-0.0007**		-0.0004
	(0.0002)		(0.0004)	(0.0002)		(0.0004)
Weight (lag)		-0.0016***			-0.0007*	
		(0.0003)			(0.0004)	
Underweight	-0.0071		-0.0384	-0.0480		-0.0001
	(0.0306)		(0.0374)	(0.0545)		(0.0664)
Overweight	-0.0500**		-0.0146	0.0201		0.0286
	(0.0152)		(0.0162)	(0.0201)		(0.0200)
Obese	-0.1414***		-0.0502*	-0.0708***		-0.0394
	(0.0189)		(0.0251)	(0.0208)		(0.0308)
Underweight (lag)		-0.0003			0.0498	
		(0.0430)			(0.0882)	
Overweight (lag)		-0.0750**			0.0127	
		(0.0228)			(0.0250)	
Obese (lag)		-0.1191***			-0.0568	
		(0.0290)			(0.0310)	
Observations	18008	5575	18008	7476	2485	7476

Standard errors in parentheses, * ($p < 0.05$), ** ($p < 0.01$), *** ($p < 0.001$)

One of the three measures of weight is used: BMI, weight (controlling for height) or the three indicators

Column (2) (5) use lagged weight measures, Columns (3) (6) include individual fixed-effects

Controls for whether R has children, having children*female, marital status, age, age^2, education, firm size, job tenure, AFQT score, type of residence, industry, occupation, and year dummies are included in all regressions.

Table 3.7: Log Wage Regressions of White Females and Black Females 2002-2008

	(1)	(2)	(3)	(4)	(5)	(6)
	White Females			Black Females		
BMI	-0.0070***		0.0008	-0.0026		0.0025
	(0.0019)		(0.0040)	(0.0018)		(0.0051)
BMI (lag)		-0.0071**			-0.0045*	
		(0.0022)			(0.0021)	
Weight (in pounds)	-0.0012***		0.0002	-0.0004		0.0004
	(0.0003)		(0.0007)	(0.0003)		(0.0008)
Weight (lag)		-0.0012**			-0.0008*	
		(0.0004)			(0.0004)	
Underweight	-0.0230		0.0992	0.0869		-0.0701
	(0.0699)		(0.1129)	(0.1238)		(0.1262)
Overweight	0.0094		0.0361	0.0201		0.0151
	(0.0301)		(0.0405)	(0.0364)		(0.0516)
Obese	-0.0755*		-0.0009	-0.0066		0.0166
	(0.0303)		(0.0508)	(0.0364)		(0.0769)
Underweight (lag)		0.0833			-0.0238	
		(0.0549)			(0.0769)	
Overweight (lag)		-0.0381			-0.0346	
		(0.0290)			(0.0321)	
Obese (lag)		-0.0878**			-0.0596	
		(0.0314)			(0.0316)	
Observations	6767	6662	6767	3289	3222	3289

Standard errors in parentheses, * ($p < 0.05$), ** ($p < 0.01$), *** ($p < 0.001$)

One of the three measures of weight is used: BMI, weight (controlling for height) or the three indicators

Column (2) (5) use lagged weight measures, Columns (3) (6) include individual fixed-effects

Controls for whether R has children, having children*female, marital status, age, age^2, education, firm size, job tenure, AFQT score, type of residence, industry, occupation, and year dummies are included in all regressions.

Table 3.8: Probability of Employment 1989-2008

	(1) Full Sample	(2) Males	(3) Females	(4) White Males	(5) Black Males	(6) White Females	(7) Black Females
Underweight (d)	-0.0791***	-0.1620*	-0.0652*	-0.1689	-0.1637	-0.0615*	-0.1391*
	(0.0224)	(0.0750)	(0.0254)	(0.0899)	(0.1143)	(0.0278)	(0.0545)
Overweight (d)	0.0117*	0.0015	0.0180*	-0.0042	0.0361**	0.0144	0.0379**
	(0.0049)	(0.0054)	(0.0081)	(0.0060)	(0.0125)	(0.0092)	(0.0128)
Obese (d)	0.0078	0.0014	0.0072	-0.0035	0.0210	0.0022	0.0141
	(0.0060)	(0.0068)	(0.0100)	(0.0076)	(0.0158)	(0.0120)	(0.0163)
Observations	91998	45239	46759	29444	13354	30409	13693

Standard errors in parentheses, * ($p < 0.05$), ** ($p < 0.01$), *** ($p < 0.001$)

(d) for discrete change of dummy variable from 0 to 1

Controls for whether R has children, having children*female, marital status, age, age^2, education

AFQT score, type of residence, and year dummies are included in all regressions.

Table 3.9: Probability of Employment 1989-1998

	(1) Full Sample	(2) Males	(3) Females	(4) White Males	(5) Black Males	(6) White Females	(7) Black Females
Underweight (d)	-0.0528**	-0.1088	-0.0552*	-0.1042	-0.1352	-0.0483	-0.1489**
	(0.0204)	(0.0639)	(0.0259)	(0.0681)	(0.1184)	(0.0279)	(0.0562)
Overweight (d)	0.0050	-0.0010	0.0052	-0.0045	0.0250*	0.0054	0.0226
	(0.0045)	(0.0036)	(0.0090)	(0.0040)	(0.0117)	(0.0103)	(0.0154)
Obese (d)	-0.0070	-0.0083	-0.0169	-0.0118	0.0058	-0.0256	0.0053
	(0.0060)	(0.0056)	(0.0113)	(0.0067)	(0.0163)	(0.0138)	(0.0184)
Observations	57656	28385	29271	18616	8232	19263	8353

Standard errors in parentheses, * ($p < 0.05$), ** ($p < 0.01$), *** ($p < 0.001$)

(d) for discrete change of dummy variable from 0 to 1

Controls for whether R has children, having children*female, marital status, age, age^2, education

AFQT score, type of residence, and year dummies are included in all regressions.

Table 3.10: Probability of Employment: 2000-2008

	(1) Full Sample	(2) Males	(3) Females	(4) White Males	(5) Black Males	(6) White Females	(7) Black Females
Underweight (d)	-0.1151**	-0.2470*	-0.0990*	-0.2591	-0.2145	-0.1013	-0.1185
	(0.0422)	(0.1028)	(0.0471)	(0.1389)	(0.1323)	(0.0520)	(0.0975)
Overweight (d)	0.0282**	0.0103	0.0398**	0.0016	0.0545*	0.0302*	0.0680***
	(0.0088)	(0.0127)	(0.0123)	(0.0144)	(0.0224)	(0.0139)	(0.0199)
Obese (d)	0.0328***	0.0177	0.0372**	0.0110	0.0406	0.0351*	0.0347
	(0.0096)	(0.0139)	(0.0137)	(0.0160)	(0.0239)	(0.0160)	(0.0227)
Observations	34342	16854	17488	10828	5122	11146	5340

Standard errors in parentheses, * ($p < 0.05$), ** ($p < 0.01$), *** ($p < 0.001$)

(d) for discrete change of dummy variable from 0 to 1

Controls for whether R has children, having children*female, marital status, age, age^2, education

AFQT score, type of residence, and year dummies are included in all regressions.

Table 3.11: Effect of Living in the Region with the Highest BMI on Log Wages: Females, Males, and White Females

	Effect on Log Wages					
	Females		Males		White Females	
High BMI Region	0.1641***	0.1409***	0.1459***	0.0377*	0.1670***	0.1591***
	(0.0119)	(0.0220)	(0.0121)	(0.0226)	(0.0140)	(0.0257)
Obese	-0.6653***	-0.4540**	0.1604	0.4720**	-0.7617***	-0.5998**
	(0.2218)	(0.2137)	(0.2021)	(0.1922)	(0.2811)	(0.2691)
RegionBMI*Obese	0.0198**	0.0158**	-0.0069	-0.0166**	0.0228**	0.0208**
	(0.0081)	(0.0078)	(0.0075)	(0.0071)	(0.0103)	(0.0098)
Observations	38405	38405	34954	34954	25317	25317
r2	0.3235	0.6128	0.3965	0.6899	0.3103	0.6068

Standard errors in parentheses
Even numbered columns have person-specific fixed effects included
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3.12: Effect of Living in the Region with the Highest BMI on Log Wages: Black Females, White Males, and Black Males

	Effect on Log Wages					
	Black Females		White Males		Black Males	
High BMI Region	0.1559***	0.0488	0.1558***	0.0685***	0.1411***	0.0109
	(0.0160)	(0.0316)	(0.0136)	(0.0226)	(0.0167)	(0.0313)
Obese	-0.6653***	-0.4540**	0.1604	0.4720**	-0.7617***	-0.5998**
	(0.2549)	(0.2773)	(0.2280)	(0.2113)	(0.2843)	(0.2965)
RegionBMI*Obese	0.0198**	0.0116	-0.0137	-0.0222***	0.0229**	0.0194*
	(0.0095)	(0.0103)	(0.0084)	(0.0077)	(0.0104)	(0.0108))
Observations	10964	10964	27632	27632	11471	11471
r2	0.4176	0.6510	0.3835	0.6787	0.3774	0.6638

Standard errors in parentheses
Even numbered columns have person-specific fixed effects included
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3.13: Effect of living in the Region with the Highest BMI on Employment

	Effect on Probability of Employment					
	Females	Males	White Females	Black Females	White Males	Black Males
High BMI Region	0.0154	-0.2355***	0.0178	0.0714	-0.3166***	-0.0402
	(0.0311)	(0.0402)	(0.0367)	(0.0498)	(0.0506)	(0.0559)
Obese	-2.1607***	-1.0907	-2.4546***	-0.3058	-1.4891	-0.1913
	(0.5188)	(0.7450)	(0.6605)	(0.7498)	(0.9878)	(1.0069)
RegionBMI*Obese	0.0787***	0.0390	0.0884***	0.0130	0.0518	0.0103
	(0.0188)	(0.0266)	(0.0239)	(0.0275)	(0.0350)	(0.0359)
Observations	46759	38283	30409	13693	29444	13354

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3.14: Regional Effects of Obesity on Log Wages (All Females, All Males, White Females)

	Effect on Log Wages					
	Females		Males		White Females	
Percent Obese in Region	3.0772***	2.1489***	2.8368***	1.1432***	3.1396***	2.4507***
	(0.2226)	(0.3442)	(0.2319)	(0.3684)	(0.2626)	(0.4037)
Obese	-0.1878***	-0.0755*	0.0120	0.0881**	-0.2110***	-0.1023**
	(0.0376)	(0.0386)	(0.0349)	(0.0352)	(0.0480)	(0.0491)
Percent Obese*Obese	0.2836*	0.2129	-0.1795	-0.2997**	0.3414*	0.2995*
	(0.1460)	(0.1393)	(0.1367)	(0.1270)	(0.1860)	(0.1764)
Observations	38142	38142	34672	34672	25158	25158
r2	0.3345	0.6142	0.4061	0.6908	0.3210	0.6080

Standard errors in parentheses
Even numbered columns have person-specific fixed effects included
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3.15: Regional Effects of Obesity on Log Wages (Black Females, White Males, Black Males)

	Effect on Log Wages					
	Black Females		White Males		Black Males	
Percent Obese in Region	2.9021***	0.5360	2.9807***	1.4846***	2.8317***	0.8048
	(0.2721)	(0.5247)	(0.2584)	(0.3560)	(0.2919)	(0.4933)
Obese	-0.1148***	-0.0197	0.0360	0.1216***	-0.0362	-0.0387
	(0.0432)	(0.0501)	(0.0409)	(0.0398)	(0.0520)	(0.0552)
Percent Obese*Obese	0.2385	0.1805	-0.3557**	-0.4485***	0.3598*	0.3296*
	(0.1679)	(0.1798)	(0.1578)	(0.1410)	(0.1839)	(0.1868)
Observations	10903	10903	27426	27426	11413	11413
r2	0.4293	0.6529	0.3911	0.6792	0.3921	0.6645

Standard errors in parentheses
Even numbered columns have person-specific fixed effects included
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3.16: Regional Effects of Obesity of Employment

	Effect on Probability of Employment					
	Females	Males	White Females	Black Females	White Males	Black Males
Percent Obese in Region	0.2215	-4.3611***	0.2713	0.9171	-5.0455***	-0.8713
	(0.5722)	(0.7235)	(0.6752)	(0.9182)	(0.8728)	(1.0227)
Obese	-0.3551***	-0.1984	-0.4348***	0.0437	-0.2516	0.0886
	(0.0959)	(0.1357)	(0.1229)	(0.1379)	(0.1802)	(0.1945)
Percent Obese*Obese	1.4278***	0.7055	1.6459***	0.0312	0.7560	0.0385
	(0.3501)	(0.4625)	(0.4469)	(0.5124)	(0.6027)	(0.6281)
Observations	46751	38274	30404	13692	29437	13353

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3.17: Regional Effects of Obesity on Log Wages (All Females, All Males, White Females): Year by Region Fixed Effects

	Effect on Log Wages					
	Females		Males		White Females	
Underweight	0.0032	-0.0215	-0.1181	-0.0437	0.0064	-0.0220
	(0.0271)	(0.0313)	(0.0963)	(0.0836)	(0.0295)	(0.0341)
Overweight	-0.0320**	0.0133	0.0223*	0.0256*	-0.0342**	0.0114
	(0.0138)	(0.0141)	(0.0133)	(0.0137)	(0.0158)	(0.0163)
Obese	-0.1867***	-0.0794**	0.0216	0.0916***	-0.2084***	-0.1048**
	(0.0383)	(0.0392)	(0.0351)	(0.0351)	(0.0487)	(0.0497)
Percent Obese*Obese	0.2759*	0.2110	-0.2179	-0.3146**	0.3260*	0.2864
	(0.1494)	(0.1416)	(0.1378)	(0.1274)	(0.1894)	(0.1787)
Observations	38142	38142	34672	34672	25158	25158
r2	0.3400	0.6201	0.4080	0.6926	0.3273	0.6149

Standard errors in parentheses
Even numbered columns have person-specific fixed effects included
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3.18: Regional Effects of Obesity on Log Wages (Black Females, White Males, Black Males): Year by Region Fixed Effects

	Effect on Log Wages					
	Black Females		White Males		Black Males	
Underweight	-0.0421	-0.0445	-0.1532	-0.0456	-0.0822	-0.0549
	(0.0613)	(0.0545)	(0.1044)	(0.0687)	(0.0964)	(0.1423)
Overweight	0.0169	0.0543^{***}	0.0160	0.0210	0.0366^{**}	0.0287
	(0.0191)	(0.0207)	(0.0154)	(0.0145)	(0.0184)	(0.0201)
Obese	-0.1044^{**}	-0.0131	0.0455	0.1287^{***}	-0.0250	-0.0321
	(0.0436)	(0.0502)	(0.0413)	(0.0397)	(0.0521)	(0.0562)
Percent Obese*Obese	0.2009	0.1624	-0.3944^{**}	-0.4763^{***}	0.3193^{*}	0.3057
	(0.1688)	(0.1783)	(0.1592)	(0.1412)	(0.1842)	(0.1875)
Observations	10903	10903	27426	27426	11413	11413
r2	0.4348	0.6573	0.3933	0.6811	0.3968	0.6683

Standard errors in parentheses
Even numbered columns have person-specific fixed effects included
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3.19: Regional Effects of Obesity of Employment: Year by Region Fixed Effects

	Effect on Probability of Employment					
	Females	Males	White Females	Black Females	White Males	Black Males
Underweight	-0.2405^{***}	-0.7257^{***}	-0.2326^{**}	-0.4530^{***}	-0.8191^{***}	-0.5312
	(0.0825)	(0.2254)	(0.0925)	(0.1462)	(0.2819)	(0.3492)
Overweight	0.0789^{**}	0.0271	0.0659^{*}	0.1512^{***}	-0.0350	0.1749^{***}
	(0.0331)	(0.0406)	(0.0383)	(0.0505)	(0.0503)	(0.0568)
Obese	-0.3945^{***}	-0.2412^{*}	-0.4658^{***}	0.0185	-0.3087^{*}	0.0511
	(0.0939)	(0.1316)	(0.1201)	(0.1357)	(0.1741)	(0.1903)
Percent Obese*Obese	1.5766^{***}	0.8523^{*}	1.7617^{***}	0.1414	0.9434	0.1733
	(0.3427)	(0.4465)	(0.4357)	(0.5004)	(0.5769)	(0.6116)
Observations	46751	38274	30404	13692	29437	13353
r2						

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

3.6 Appendix

3.6.1 Effects of Obesity for other Sub-samples

Table 3.20: Log Wage Regressions of Full Sample, Males, and Females 1989-2008

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
		Full Sample			Males			Females	
BMI	-0.0051***		-0.0007	-0.0026		0.0000	-0.0080***		-0.0005
	(0.0008)		(0.0013)	(0.0014)		(0.0019)	(0.0010)		(0.0016)
BMI (lag)		-0.0047***			-0.0032			-0.0076***	
		(0.0011)			(0.0018)			(0.0014)	
Weight (in pounds)	-0.0007***		-0.0001	-0.0004		0.0000	-0.0013***		-0.0001
	(0.0001)		(0.0002)	(0.0002)		(0.0003)	(0.0002)		(0.0003)
Weight (lag)		-0.0007***			-0.0005			-0.0013***	
		(0.0002)			(0.0003)			(0.0002)	
Underweight	-0.0061		-0.0159	-0.1176		-0.0405	-0.0113		-0.0192
	(0.0268)		(0.0288)	(0.0920)		(0.0637)	(0.0271)		(0.0319)
Overweight	0.0044		0.0193*	0.0251		0.0264*	-0.0297*		0.0141
	(0.0096)		(0.0092)	(0.0133)		(0.0125)	(0.0136)		(0.0136)
Obese	-0.0621***		-0.0031	-0.0319		0.0112	-0.1117***		-0.0166
	(0.0116)		(0.0135)	(0.0167)		(0.0179)	(0.0157)		(0.0201)
Underweight (lag)		0.0570			0.0143			0.0362	
		(0.0331)			(0.1092)			(0.0353)	
Overweight (lag)		-0.0053			0.0129			-0.0526**	
		(0.0123)			(0.0163)			(0.0181)	
Obese (lag)		-0.0593***			-0.0499*			-0.0947***	
		(0.0146)			(0.0208)			(0.0203)	
Observations	77884	38767	77884	40278	19755	40278	37606	19012	37606

Standard errors in parentheses, * ($p < 0.05$), ** ($p < 0.01$), *** ($p < 0.001$)
One of the three measures of weight is used: BMI, weight (controlling for height) or the three indicators
Column (2) (5) (7) use lagged weight measures, Columns (3) (6) and (9) include individual fixed-effects
Controls for whether R has children, having children*female, marital status, age, age^2, education, firm size,
job tenure, AFQT score, type of residence, industry, occupation, and year dummies are included in all regressions.

Table 3.21: Log Wage Regressions of White Males and Black Males 1989-2008

	(1)	(2)	(3)	(4)	(5)	(6)
		White Males			Black Males	
BMI	-0.0041*		-0.0009	0.0047*		0.0049
	(0.0016)		(0.0023)	(0.0022)		(0.0032)
BMI (lag)		-0.0048*			0.0050	
		(0.0021)			(0.0027)	
Weight (in pounds)	-0.0006*		-0.0001	0.0007*		0.0007
	(0.0002)		(0.0003)	(0.0003)		(0.0005)
Weight (lag)		-0.0007*			0.0007	
		(0.0003)			(0.0004)	
Underweight	-0.1355		-0.0520	-0.0621		-0.0328
	(0.1027)		(0.0684)	(0.0908)		(0.1428)
Overweight	0.0225		0.0261	0.0413*		0.0260
	(0.0153)		(0.0145)	(0.0180)		(0.0201)
Obese	-0.0516**		0.0023	0.0671**		0.0518
	(0.0194)		(0.0211)	(0.0234)		(0.0293)
Underweight (lag)		0.0101			-0.0317	
		(0.1184)			(0.0636)	
Overweight (lag)		0.0091			0.0308	
		(0.0188)			(0.0217)	
Obese (lag)		-0.0719**			0.0542	
		(0.0243)			(0.0310)	
Observations	26981	13069	26981	11204	5681	11204

Standard errors in parentheses, * ($p < 0.05$), ** ($p < 0.01$), *** ($p < 0.001$)
One of the three measures of weight is used: BMI, weight (controlling for height) or the three indicators
Column (2) (5) use lagged weight measures, Columns (3) (6) include individual fixed-effects
Controls for whether R has children, having children*female, marital status, age, age^2, education, firm size, job tenure, AFQT score, type of residence, industry, occupation, and year dummies are included in all regressions.

Table 3.22: Log Wage Regressions of Full Sample, Males, and Females 1989-2000

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
		Full Sample			Males			Females	
BMI	-0.0056***		-0.0011	-0.0023		0.0021	-0.0089***		-0.0030
	(0.0009)		(0.0015)	(0.0014)		(0.0023)	(0.0011)		(0.0019)
BMI (lag)		-0.0055***			-0.0029			-0.0090***	
		(0.0012)			(0.0020)			(0.0016)	
Weight (in pounds)	-0.0008***		-0.0001	-0.0003		0.0003	-0.0015***		-0.0005
	(0.0001)		(0.0002)	(0.0002)		(0.0003)	(0.0002)		(0.0003)
Weight (lag)		-0.0008***			-0.0004			-0.0015***	
		(0.0002)			(0.0003)			(0.0003)	
Underweight	-0.0114		-0.0386	-0.1089		-0.0483	-0.0112		-0.0369
	(0.0281)		(0.0322)	(0.0976)		(0.0840)	(0.0282)		(0.0348)
Overweight	-0.0043		0.0044	0.0184		0.0198	-0.0439***		-0.0124
	(0.0093)		(0.0098)	(0.0127)		(0.0136)	(0.0132)		(0.0140)
Obese	-0.0751***		-0.0241	-0.0353*		0.0056	-0.1334***		-0.0551**
	(0.0116)		(0.0145)	(0.0165)		(0.0199)	(0.0155)		(0.0209)
Underweight (lag)		0.0128			0.0053			-0.0017	
		(0.0407)			(0.1377)			(0.0395)	
Overweight (lag)		-0.0109			0.0109			-0.0636***	
		(0.0128)			(0.0168)			(0.0191)	
Obese (lag)		-0.0656***			-0.0425			-0.1163***	
		(0.0162)			(0.0228)			(0.0230)	
Observations	56684	17881	56684	29734	9343	29734	26950	8538	26950

Standard errors in parentheses, * ($p < 0.05$), ** ($p < 0.01$), *** ($p < 0.001$)
One of the three measures of weight is used: BMI, weight (controlling for height) or the three indicators
Column (2) (5) (7) use lagged weight measures, Columns (3) (6) and (9) include individual fixed-effects
Controls for whether R has children, having children*female, marital status, age, age^2, education, firm size,
job tenure, AFQT score, type of residence, industry, occupation, and year dummies are included in all regressions.

Table 3.23: Log Wage Regressions of White Males and Black Males 1989-2000

	(1)	(2)	(3)	(4)	(5)	(6)
	White Males			Black Males		
BMI	-0.0036*		0.0017	0.0040		0.0042
	(0.0017)		(0.0028)	(0.0022)		(0.0037)
BMI (lag)		-0.0048*			0.0068*	
		(0.0023)			(0.0031)	
Weight (in pounds)	-0.0005*		0.0002	0.0006*		0.0005
	(0.0002)		(0.0004)	(0.0003)		(0.0005)
Weight (lag)		-0.0006*			0.0010*	
		(0.0003)			(0.0005)	
Underweight	-0.1285		-0.0616	-0.0554		-0.0610
	(0.1071)		(0.0891)	(0.1154)		(0.2140)
Overweight	0.0160		0.0231	0.0321		-0.0000
	(0.0146)		(0.0159)	(0.0198)		(0.0206)
Obese	-0.0540**		-0.0007	0.0539*		0.0371
	(0.0193)		(0.0234)	(0.0252)		(0.0307)
Underweight (lag)		-0.0044			0.0546	
		(0.1482)			(0.0878)	
Overweight (lag)		0.0023			0.0488	
		(0.0194)			(0.0257)	
Obese (lag)		-0.0647*			0.0718	
		(0.0267)			(0.0368)	
Observations	20066	6231	20066	8115	2637	8115

Standard errors in parentheses, * ($p < 0.05$), ** ($p < 0.01$), *** ($p < 0.001$)
One of the three measures of weight is used: BMI, weight (controlling for height) or the three indicators
Column (2) (5) use lagged weight measures, Columns (3) (6) include individual fixed-effects
Controls for whether R has children, having children*female, marital status, age, age^2, education, firm size, job tenure, AFQT score, type of residence, industry, occupation, and year dummies are included in all regressions.

Table 3.24: Log Wage Regressions of Full Sample, Males, and Females 2002-2008

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
		Full Sample			Males			Females	
BMI	-0.0039**		0.0017	-0.0030		0.0025	-0.0064***		0.0010
	(0.0012)		(0.0027)	(0.0020)		(0.0045)	(0.0015)		(0.0033)
BMI (lag)		-0.0039**			-0.0030			-0.0066***	
		(0.0014)			(0.0022)			(0.0018)	
Weight (in pounds)	-0.0006**		0.0003	-0.0004		0.0004	-0.0011***		0.0002
	(0.0002)		(0.0004)	(0.0003)		(0.0007)	(0.0003)		(0.0006)
Weight (lag)		-0.0006**			-0.0005			-0.0011***	
		(0.0002)			(0.0003)			(0.0003)	
Underweight	-0.0000		0.0703	-0.2495*		-0.1092	-0.0066		0.0909
	(0.0578)		(0.0944)	(0.1223)		(0.2019)	(0.0624)		(0.0996)
Overweight	0.0286		0.0367	0.0431		0.0402	0.0025		0.0335
	(0.0191)		(0.0275)	(0.0274)		(0.0433)	(0.0263)		(0.0356)
Obese	-0.0321		0.0106	-0.0184		0.0135	-0.0701**		0.0037
	(0.0195)		(0.0326)	(0.0289)		(0.0501)	(0.0260)		(0.0441)
Underweight (lag)		0.0973*			0.0010			0.0722	
		(0.0423)			(0.1003)			(0.0490)	
Overweight (lag)		0.0008			0.0169			-0.0406	
		(0.0168)			(0.0226)			(0.0246)	
Obese (lag)		-0.0524**			-0.0517			-0.0829**	
		(0.0186)			(0.0271)			(0.0254)	
Observations	21200	20886	21200	10544	10412	10544	10656	10474	10656

Standard errors in parentheses, * ($p < 0.05$), ** ($p < 0.01$), *** ($p < 0.001$)
One of the three measures of weight is used: BMI, weight (controlling for height) or the three indicators
Column (2) (5) (7) use lagged weight measures, Columns (3) (6) and (9) include individual fixed-effects
Controls for whether R has children, having children*female, marital status, age, age^2, education, firm size, job tenure, AFQT score, type of residence, industry, occupation, and year dummies are included in all regressions.

Table 3.25: Log Wage Regressions of White Males and Black Males 2002-2008

	(1)	(2)	(3)	(4)	(5)	(6)
		White Males			Black Males	
BMI	-0.0046*		0.0028	0.0052		0.0013
	(0.0023)		(0.0054)	(0.0033)		(0.0067)
BMI (lag)		-0.0045			0.0035	
		(0.0025)			(0.0034)	
Weight (in pounds)	-0.0007*		0.0004	0.0007		0.0001
	(0.0003)		(0.0008)	(0.0005)		(0.0010)
Weight (lag)		-0.0007			0.0004	
		(0.0004)			(0.0005)	
Underweight	-0.3209*		-0.2182	-0.0415		0.1237
	(0.1551)		(0.2453)	(0.0789)		(0.0937)
Overweight	0.0380		0.0368	0.0665*		0.0448
	(0.0316)		(0.0508)	(0.0304)		(0.0578)
Obese	-0.0405		0.0037	0.0925**		0.0335
	(0.0333)		(0.0589)	(0.0347)		(0.0760)
Underweight (lag)		0.0043			-0.1408	
		(0.1068)			(0.0829)	
Overweight (lag)		0.0169			0.0082	
		(0.0262)			(0.0293)	
Obese (lag)		-0.0727*			0.0364	
		(0.0318)			(0.0381)	
Observations	6915	6838	6915	3089	3044	3089

Standard errors in parentheses, * ($p < 0.05$), ** ($p < 0.01$), *** ($p < 0.001$)
One of the three measures of weight is used: BMI, weight (controlling for height) or the three indicators
Column (2) (5) use lagged weight measures, Columns (3) (6) include individual fixed-effects
Controls for whether R has children, having children*female, marital status, age, age^2, education, firm size, job tenure, AFQT score, type of residence, industry, occupation, and year dummies are included in all regressions.

3.6.2 Additional Specifications: Probability of Employment

We redefine the definition of "employed" to be those who work full-time, i.e. at least 7 hours per day, and estimate the probability of being employed. The results are presented in table 3.B.1-B.3 and differ in a couple ways to those in Tables 6-8. First, the coefficient on overweight white males is insignificant, and second, the coefficient on obese white females is now insignificant as well. While the point estimate for the coefficient for overweight white males is similar to that in table 3.5, the coefficient for obese white females is much smaller than before. This means that the employment penalty for being heavier is concentrated in "part-time" jobs, or jobs where one works less than 7 hours per day. (Need to change to reflect actual results).

In addition, we split these results for years before 2000 and after 2000 and reestimate to see if the employment probability has changed over time.

Table 3.26: Probability of Full-time Employment 1989-2008

	(1) Full Sample	(2) Males	(3) Females	(4) White Males	(5) Black Males	(6) White Females	(7) Black Females
Underweight (d)	-0.0986***	-0.2496**	-0.0762**	-0.2671**	-0.1858	-0.0755*	-0.1375**
	(0.0266)	(0.0894)	(0.0285)	(0.1018)	(0.1361)	(0.0313)	(0.0519)
Overweight (d)	0.0214***	0.0075	0.0225*	0.0012	0.0441**	0.0164	0.0555***
	(0.0062)	(0.0065)	(0.0106)	(0.0073)	(0.0143)	(0.0124)	(0.0154)
Obese (d)	0.0204**	0.0050	0.0199	-0.0003	0.0268	0.0152	0.0305
	(0.0078)	(0.0084)	(0.0129)	(0.0096)	(0.0184)	(0.0156)	(0.0191)
Observations	91998	45239	46759	29444	13354	30409	13693

Standard errors in parentheses,* ($p < 0.05$), ** ($p < 0.01$), *** ($p < 0.001$)
(d) for discrete change of dummy variable from 0 to 1
Controls for whether R has children, having children*female, marital status, age, age^2, education
AFQT score, type of residence, and year dummies are included in all regressions.

Table 3.27: Probability of Full-time Employment 1989-1998

	(1) Full Sample	(2) Males	(3) Females	(4) White Males	(5) Black Males	(6) White Females	(7) Black Females
Underweight (d)	-0.0674*	-0.2006*	-0.0536	-0.2016*	-0.1699	-0.0481	-0.1400*
	(0.0264)	(0.0906)	(0.0284)	(0.0978)	(0.1441)	(0.0310)	(0.0579)
Overweight (d)	0.0115	0.0050	0.0019	0.0001	0.0376**	-0.0015	0.0363*
	(0.0064)	(0.0055)	(0.0122)	(0.0060)	(0.0144)	(0.0144)	(0.0182)
Obese (d)	-0.0009	-0.0105	-0.0076	-0.0140	0.0030	-0.0161	0.0218
	(0.0085)	(0.0083)	(0.0148)	(0.0095)	(0.0207)	(0.0184)	(0.0212)
Observations	57656	28385	29271	18616	8232	19263	8353

Standard errors in parentheses, * ($p < 0.05$), ** ($p < 0.01$), *** ($p < 0.001$)
(d) for discrete change of dummy variable from 0 to 1
Controls for whether R has children, having children*female, marital status, age, age^2, education
AFQT score, type of residence, and year dummies are included in all regressions.

Table 3.28: Probability of Full-time Employment 2000-2008

	(1) Full Sample	(2) Males	(3) Females	(4) White Males	(5) Black Males	(6) White Females	(7) Black Females
Underweight (d)	-0.1657***	-0.3648**	-0.1474**	-0.4212**	-0.2243	-0.1567**	-0.1435
	(0.0459)	(0.1132)	(0.0494)	(0.1433)	(0.1492)	(0.0540)	(0.0950)
Overweight (d)	0.0412***	0.0141	0.0544***	0.0060	0.0558*	0.0428*	0.0933***
	(0.0101)	(0.0136)	(0.0150)	(0.0154)	(0.0241)	(0.0171)	(0.0245)
Obese (d)	0.0499***	0.0248	0.0533**	0.0174	0.0526*	0.0506*	0.0538*
	(0.0111)	(0.0149)	(0.0168)	(0.0172)	(0.0263)	(0.0198)	(0.0272)
Observations	34342	16854	17488	10828	5122	11146	5340

Standard errors in parentheses, * ($p < 0.05$), ** ($p < 0.01$), *** ($p < 0.001$)
(d) for discrete change of dummy variable from 0 to 1
Controls for whether R has children, having children*female, marital status, age, age^2, education
AFQT score, type of residence, and year dummies are included in all regressions.

3.6.3 NLSY States by Region

Table 3.29: NLSY States by Region

Region 1: 'Northeast'	**Region 2: 'North Central'**
Connecticut	Illinois
Maine	Indiana
Massachusetts	Iowa
New Hampshire	Kansas
New Jersey	Michigan
New York	Minnesota
Pennsylvania	Missouri
Rhode Island	Nebraska
Vermont	North Dakota
	Ohio
	South Dakota
	Wisconsin
Region 3: 'South'	**Region 4: 'West'**
Alabama	Alaska
Arkansas	Arizona
Delaware	California
District of Columbia	Colorado
Florida	Hawaii
Georgia	Idaho
Kentucky	Montana
Louisiana	Nevada
Maryland	New Mexico
Mississippi	Oregon
North Carolina	Utah
Oklahoma	Washington
South Carolina	Wyoming
Tennessee	
Texas	
Virginia	
West Virginia	