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# The Impact of Living Wage Laws on Urban Economic Development Patterns and the Local Business Climate: Evidence From California Cities

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## Abstract

Traditional local economic development policies entice private businesses to create high-paying jobs in a given jurisdiction through direct subsidies or by projecting a positive “business climate” within regional and global arenas. Since 1994 however, living wage ordinances have emerged as an alternative response to labor market polarization in urban areas. However, these laws raise labor costs for employers and may thus reduce economic growth. This article advances the empirical literature on living wage impacts through the use of a novel data set—the National Establishment Time Series—to track employment and establishment growth at the city level among directly affected employers (e.g., government contractors), as well as other establishments that may be indirectly signaled by a change in the local political environment. Using panel regression models that account for structural differences between living wage and non-living wage cities, this article finds that living wage laws have no significant impact on employment or establishment growth. Additionally, this article finds no evidence that living wage laws “signal” businesses about a potentially harmful change in the local business climate.

## Keywords

living wage, labor market institutions, economic development impacts, business climate

## Introduction

Faced with a polarized labor market and a competitive international marketplace, city leaders and planners attempt to develop middle-class jobs through a variety of strategies. These strategies typically aim to entice private businesses to create high-paying jobs through direct subsidies, by fostering an environment conducive to innovation, or more generally by projecting a positive “business climate” within regional and global arenas. Although there have been alternative economic development strategies offered by certain progressive regimes since the 1980s, the dominant trajectory of economic development policy in the United States remains focused on business attraction. Although a business attraction stance remains the dominant paradigm, beginning in the mid-1990s there emerged a new set of alternative responses to the problem of labor market inequality. These responses focused attention explicitly on increasing job quality and strengthening labor market institutions at the urban scale. These alternative policies include community benefits agreements, prevailing wage laws, first

source hiring agreements, incentive clawbacks, and, most prominently, living wage laws. Taken together, these “pro-equity” policies provide a direct challenge to the dominant mode of economic development practice at the local level. Although these individual examples of alternative urban policies have not amounted to a shift in the dominant paradigm of urban policy making with regard to labor market interventions or economic development in general, they nonetheless are indicative of a new, highly contested institutional environment at the local scale. Since 1994, living wage ordinances—now enacted in more than 140 local jurisdictions nationwide—have emerged as the most common alternative response to labor market polarization in urban

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areas. Although the coverage and enforcement mechanisms vary across jurisdictions, the broadest type of living wage ordinance mandates that firms with city contracts or receiving business assistance funds must pay their workers an hourly wage significantly higher than the federal minimum wage (e.g., \$9–\$14 per hour). For living wage proponents, the basic rationale for such laws is the idea that public funds should not be used to support labor market practices that promote working poverty. Indeed, the genesis for Baltimore's living wage campaign stemmed from the presence of full time workers on city contracts at local soup kitchens (Niedt, Ruiters, Wise, & Schoenberger, 1999). Although basic social justice claims are at the heart of any campaign, the rapid growth of living wage ordinances was also, in part, a response on the part of local unions to stem the tide of outsourcing of city services. Although urban living wage laws provide raises for low-income workers and residents, by raising costs for employers and taxpayers, these laws run counter to dominant economic development practices and may negatively influence both direct economic development outcomes (e.g., employment and firm growth) and the city's reputation among business decision makers.

This article estimates the impact of living wage laws on the economic development prospects of the cities that pass them. Specifically, it assesses their impact on urban employment and firm growth overall and in areas of the local economy that are most likely to be directly covered by living wage laws, such as government contractors in low-wage service industries. This article refers to this analysis as the "direct" impacts. In addition, this article also provides an indirect test of the "business climate thesis" by measuring the impact of living wage laws on employment in headquarter establishments and other indirectly affected industries, as well as the spatial distribution of firms within the metropolitan area that contains each living wage city.

Previous empirical research on living wage laws focused on the overall costs and benefits to individual cities (Fairris, 2005; Reich, Hall, & Jacobs, 2005) or relied on public data sources with limited sample sizes for all but the largest cities, such as the Current Population Survey (CPS; Adams & Neumark, 2005; Neumark, 2002). A major innovation in this article is the use of a new, geographically refined database to measure the employment impacts of an urban policy. The National Establishment Time Series (NETS) data set provides time series data on employment and various business characteristics at the establishment level. In this article, the NETS is summarized to the city level to construct a panel data set that tracks employment levels and establishment counts for all California jurisdictions between 1990 and 2005.<sup>1</sup> Next, this article uses a methodology that controls for serial and spatial fixed effects to estimate the impact of living wage laws on employment and establishment change within the 19 living wage jurisdictions in California. This article also compares each outcome variable in California's living wage cities (i.e., the treatment sample) to all other jurisdictions in the State (the

control sample) in the years prior to and following the passage of each law to produce so-called difference-in-differences estimates. Finally, a propensity score-weighting procedure (Rosenbaum & Rubin, 1983) is used to adjust for underlying differences in observable demographic and political characteristics between living wage and non-living wage cities. Ultimately, this article finds no conclusive evidence that the passage of living wage laws is associated with reduced levels of employment or establishments in directly affected industries. Although this analysis shows that living wage cities did experience slower growth compared with all other jurisdictions in the state, it also shows that (a) this growth differential occurred long before living wage laws were passed and (b) once one adequately controls for the underlying structural differences between the samples, there is no discernable direct economic impact. In addition, this article shows that living wage laws do not appear to harm the "business climate" of the cities that pass them insofar as companies do not avoid living wage cities or relocate to uncovered (suburban) portions of the metropolitan area.

The findings of this article cast doubt on the broadly held assumption that urban policy makers face a stark trade-off between equity and economic growth. In the arena of labor market interventions—such as living wage mandates—this article suggests that urban actors have some "room to maneuver" in attempting to strengthen labor market institutions without causing immediate harm to the prospects for economic development.

The remainder of this article is organized as follows: The Background and Motivation section provides background information on the living wage movement and discusses the possible ways living wage laws may affect urban labor markets. The Empirical Literature Review section reviews recent empirical research on the effect of living wage laws on employment and economic growth. The Data Sources and Methodology section discusses the important differences in coverage and scope among living wage laws in the sample of living wage cities in California and presents the methodological steps used to construct a city-level panel data set from the NETS and estimate direct and indirect living wage impacts. The Identification Strategy section describes the formal empirical strategy and methodology for detecting any potential impacts. The Results section discusses the results, and the final section concludes and contextualizes the findings within the empirical and theoretical literatures.

## Background and Motivation

### *Provenance and Substance of Living Wage Laws*

In the 16 years since Baltimore passed the first living wage law, more than 140 local jurisdictions have mandated that companies that receive some form of local public dollars pay their workers a wage that is approximately equivalent to the

federal poverty line. The base of support for living wage laws stems from groups with an interest in fighting working poverty in the United States: a problem made worse in large cities where stagnating wages for low-skilled workers are combined with rising living costs. Although there are city-specific differences, living wage coalitions typically consist of public sector labor unions and central labor councils, clergy, and grassroots community organizations—often affiliated with a national progressive organizing network such as the Association of Community Organizations for Reform Now or the Industrial Areas Foundation. The coalition-backed organizations that coalesce around a living wage campaign, such as Baltimoreans United in Leadership Development or the Los Angeles Alliance for a New Economy, put direct pressure on elected officials, write ordinances, testify at public hearings, and engage in high-profile public relations efforts to raise the issue of working poverty within local policy discourses. Thus, for such progressive labor coalitions, the campaign for a given living wage ordinance is one part of broader efforts to raise labor standards for low-income workers. Although some coalitions disband after an initial policy success (e.g., passing a living wage ordinance), others remain intact and engage in the process of monitoring and enforcement (see Luce, 2005) and in some cases support additional policy interventions and/or provide support for local labor-organizing campaigns.

Just as the political coalitions and local economic conditions of each city are unique, the extent of coverage and the level of monitoring and enforcement written into each living wage law also vary from place to place. Despite this variation however, living wage laws typically take on one of two major forms. The initial and most basic form—referred to as contractor-only laws—applies to firms that have contracts with city government to perform services ranging from janitorial work to fleet maintenance to human services. The number of workers who directly receive pay increases from such laws can vary widely and depends on both the degree to which a city hires private or nonprofit service contractors as well as the threshold contract level specified in each law (e.g., contracts more than \$10,000). Estimates on the number of covered workers ranges from 1,500 in Baltimore (Niedt et al., 1999) to more than 12,000 in San Francisco (Reich, Hall, & Hsu, 1999). An alternative form of living wage law extends to private businesses that receive any form of financial assistance from the city government. These businesses include those that receive economic development incentives or tax breaks or those that operate on city-owned property. This form of living wage law has the potential to cover a much larger set of workers, yet the actual extent of coverage is a matter of recent debate (see Adams & Neumark, 2005; Brenner, Wicks-Lim, & Pollin, 2008). The exact extent of coverage of business assistance laws depends on several factors, including the degree of local enforcement, the threshold incentive level for coverage, and whether or not the locality in question offers incentives to low-wage employers in the first place. In most

cases cities will have only one form of living wage law (typically the contractor-only form), but in some instances the city will pass a law that combines both forms into a single ordinance. The exact scope and coverage provisions for the sample of living wage cities in California is described in the Living Wage Coverage and Case Selection section below.<sup>2</sup>

The wage level mandated by each living wage law may be specific to the jurisdiction, but all the ordinances set the minimum compensation for covered employees well in excess of the federal or state minimum wage. Thus, hourly wages for workers in firms for which a living wage is binding exceed the average wage for similar workers in similar occupations, potentially raising wage standards because of competition among firms for workers. In the low-wage sectors of the local labor market, therefore, living wage laws not only help those workers who see direct wage increases, but they may also help raise the wage standards across the sector.

On the political side, the passage of a living wage law sets a precedent within the city for progressive interventions in the local labor market and creates a more hospitable environment for further regulation. In the case of Los Angeles, once the first contractor-only living wage law was passed in 1997, the Living Wage Coalition led by the Los Angeles Alliance for a New Economy continued to have a strong role in its implementation and subsequently fought for its extension to the airport and to large city-sponsored redevelopment projects. The living wage had perhaps the most profound effects on the political economy of San Francisco (Lester, 2009). In 1999, the board of supervisors passed a living wage that applied only to San Francisco International Airport, which was later extended to the entire city in 2000. Within a few years, the city's voters had passed a citywide minimum wage by a wide margin (2003), followed by universal health access (2006) and paid family leave (2006). Without the passage of the living wage ordinance, the subsequent laws were unlikely to even be debated among the city supervisors.

Although living wage laws vary in their coverage and wage guarantees, they nonetheless represent dramatic and important reform of labor market institutions at the urban scale in that they specifically attempt to raise wage and benefits standards for low-income workers by targeting the practices of low-wage employers. Furthermore, as described above, the passage of a living wage law is often indicative of broader institutional changes afoot in some local labor markets. However, there remains a key question for urban policy makers and progressive activists: Do living wage ordinances result in the loss of jobs and businesses within the city limits? Or do living wage laws shift the balance of employment within metropolitan areas?

### *How Living Wage Laws May Alter the Urban Economy*

Economic theory predicts that there are two ways in which firms may react to the living wage laws. This article refers to

these as either *direct* or *indirect* (signaling) effects. Businesses are directly affected by a newly enacted living wage law if they (a) are currently located within or considering a move to the enacting city, (b) operate in an industry or business category (e.g., city contractor) that is legally bound by the ordinance, and (c) pay at least some of their workers a wage that falls below the new mandated minimum.

The second way that a firm's location decision or business strategy may be influenced by the adoption of a living wage law is through an indirect signaling process. Firms that may not be "bound" by a city ordinance may choose to locate elsewhere because the law itself indicates the presence of an antibusiness political climate. The political coalition that pushed for the living wage may pass future legislation that directly raises labor costs or taxes or reduces the overall attractiveness of the area for residents or other businesses (i.e., potential customers). Thus, given that living wage campaigns are controversial and attract the attention of local or national media, the successful passage of a law may "signal" a broad spectrum of businesses, ranging from small locally-oriented proprietors to transnational corporations with "global reach."

In his recent review article on living wage laws, Tim Bartik (2004) argues that

Living wages may, however, indirectly affect the labor market by signaling employers about public attitudes toward business, wages, and the disadvantaged. The symbolism of living wages may affect decisions of employers that living wages do not officially cover. (p. 270)

Adams and Neumark (2005) also recognize the possibility of indirect impacts of living wage laws in that the public campaigning process results in an alteration of local labor market institutions. They write that

the strongest campaigns often entail some degree of organization and mobilization among low-wage workers and their advocates, and heightened public discussion of the plight of low-wage workers. . . . Thus, the momentum for change that begins with a living wage movement might ultimately result in changes in wage setting norms and firm behavior, even if no legislation ultimately results. Wages might be increased in response to public pressure or the greater organizing of low-skill labor. (pp. 1-2)

As described below, this article addresses both the direct and indirect potential causal mechanisms generated by the passage of living wage ordinances. Before turning to the specific methodology, existing empirical research on this topic is reviewed in the Empirical Literature Review section below.

## Empirical Literature Review

Empirical research on the "direct" impacts of living wages has important antecedents in earlier work on the impact of the federal and state minimum wage increases. Orthodox economic theory predicts that because firms are perfectly competitive and therefore "price takers" for labor, an increase in the unit price of labor will force firms to reduce their labor demand, resulting in an aggregate disemployment effect across the economy. Most of the initial empirical works on the impact of minimum wage increases on the employment rate of low-wage workers (e.g., teenagers) were based on national time series data from the 1970s and early 1980s and resulted in a "consensus" elasticity of between  $-1$  and  $-3$  (Brown, Gilroy, & Kohen, 1982; Neumark & Wascher, 1992).

This consensus disintegrated in the 1990s as researchers adopted new "quasi-experimental" research designs and studied more recent minimum wage events. The now classic article in this vein is Card and Krueger's (1994) comparison of fast-food restaurants in New Jersey and eastern Pennsylvania before and after New Jersey raised its minimum wage to \$5.05 per hour in 1992. They found that—contrary to orthodox economic theory—as wages rose for workers in New Jersey, employment actually increased compared with control groups in Pennsylvania and New Jersey (firms that already paid \$5.05 per hour or higher). A more recent study by Dube, Lester, and Reich (2010) generalizes the case study approach by comparing employment changes in all contiguous county pairs that lie along either side of state borders that have a minimum wage differential. This article finds no adverse employment effects associated with raising the minimum wage.

Economists studying the living wage have generally followed the pattern set by the earlier minimum wage research in that they apply either a time series wage regression approach or a quasi-experimental design. An important exception to this are the careful cost-benefit analyses produced prior to or concurrent with the passage of a given law. For example, in an analysis of San Francisco's 1999 living wage law that covered all employees of city contractors as well as home health care workers, Reich et al. (1999) examined city contracts and estimated that "nearly 12,000 low-wage earners working in San Francisco would receive an additional \$50.3 million in wages and \$11.2 million in health benefits each year" (p. 2).

In terms of systematic research on the impact of living wage ordinances on wage levels and employment rates of low-wage workers, two sets of studies stand out. First, Fairris (2005) used a "quasi-experimental" research design by conducting two independent surveys of firms and workers in Los Angeles that were covered and not covered by the ordinance, respectively. Using a similar "difference-in-differences" method as Card and Krueger (1994), Fairris (2005) concludes that wages in covered firms increased whereas turnover and

absenteeism dropped relative to the control group. Most important, he finds no significant difference in employment levels. More recently, Dube, Naidu, and Reich (2007) used a similar survey methodology of firms in San Francisco and Alameda County to measure the impact of the citywide minimum wage that took effect in San Francisco in 2004.

However, Neumark (2002) follows his earlier panel study method used in Neumark and Wascher (1992) to examine the impact of living wage laws on wages, employment, and poverty rates using a panel of large cities that passed an ordinance between 1996 and 2002. Based on data from the CPS, he finds large wage increases and reductions in family poverty associated with the timing of living wage laws. However, he also finds significant disemployment effects for younger, lower-skilled workers.

Neumark's original study (2002) has been criticized on methodological grounds that center on the choice of data set. Specifically, Brenner, Wicks-Lim, and Pollin (2002) claim that Neumark's wage results are vastly overstated given the fact the most living wage laws cover only a small fraction of workers and his choice of data set (CPS) oversamples a single case (Los Angeles). The drawback with using broad household surveys such as the CPS is that there are too few cases for accurately distinguishing "covered" and "uncovered" workers. Unlike Farris (2005), he cannot specifically identify a worker who worked for a firm covered by the living wage. Also, because of data constraints Neumark (2002) restricts his analysis to the 1996-2002 period, a relatively short time period during an economic expansion.

More recently, Adams and Neumark (2005) measure the impact of living wage laws on the income and employment levels of low-income workers by comparing the levels in cities that passed living wage laws with those in cities that have a failed living wage campaign. The benefit of this research design is that using the failed cases as a control sample holds constant the local political or institutional factors that fuel living wage campaigns (e.g., union density) but that may also affect the outcome variables (e.g., employment). Neumark and Adams (2003) find a statistically significant negative employment elasticity of  $-.15$  for lower-skilled workers. Critically however, the authors find significant impacts only for those cities that have the broader "business assistance" forms of living wage laws, which they argue have the potential to cover most, if not all, low-wage workers in a given city. This finding has been challenged on two levels by Brenner et al. (2008). First, these authors argue that business assistance laws only directly affect a very small fraction of workers in each living wage city. Second, they argue that using the CPS to identify city-level effects is highly problematic and can be criticized on the same grounds listed above.

Thus, there is a discrepancy within the empirical literature on living wage effects. On one hand, a panel study of the type applied by Neumark (2002) and Adams and Neumark

(2005) that included all, or a large sample of, cities that passed a living wage law is preferable to comparing employment before and after passage within a single city. On the other hand, the detailed surveys performed by Farris (2005) make a more convincing case for measuring outcomes among firms and workers who are actually covered by the living wage (i.e., they do a better job of identifying a real treatment effect). In the following section, this article introduces a methodology that uses a new data source that allows one to combine the statistical power of panel studies with the more accurate measurements of the single-city case studies.

## Data Sources and Methodology

To address the gap in the empirical literature between single-city employer surveys and panel studies using national household surveys, this article relies on a new, privately produced database to construct a panel data set of city-level employment and establishment counts for all cities<sup>3</sup> in California. This section begins with a detailed description of the specific living wage laws passed in the sample of 19 California cities that make up the treatment group in the empirical analysis. Next, as the NETS database is relatively new to academic researchers and has not yet been used to estimate the causal effects of urban policies, this section presents a detailed description of the NETS database, paying particular attention to the steps used to summarize the NETS establishment-level database to a city-level time series and to define the specific industry groupings and employer classifications that make up the tests for both "direct" and "indirect" effects of living wage laws.

### Living Wage Coverage and Case Selection

The first step in the estimation of the impact of living wage laws on urban economic development is to define the set of cities that passed living wage laws in California, and determine the scope of each city's law and accurately define the time of passage. For this article, data on living wage laws, including the date of passage was obtained primarily from the Association of Community Organizations for Reform Now's Living Wage Resource Center website.<sup>4</sup> This source listed 21 cities in California that passed some type of living wage law and listed the mandated wage rates at the time of passage.<sup>5</sup> Information from the Employment Policies Institute on the type and coverage thresholds of each law was appended and is listed in Table 1.<sup>6</sup>

This list of cities represents the "treatment" group used in the empirical analysis described below. A few key issues are important to note about this sample. First, the period in which living wage laws were passed in California ranges from 1997 through 2004, but most laws were passed between 1998 and 2002, which allows a robust analysis of lagged effects. Second, all of the 19 living wage cities listed in Table 1

**Table 1.** Description of Living Wage Laws in California

City	Mandated living wage (\$)	Living wage without health benefits	Date of passage	Coverage information	Business assistance clause
Berkeley	11.39	13.28	June 2000	Private city contractors >\$25k, nonprofits >\$100k, business assistance >\$100k, all municipal leaseholders	Yes
Fairfax	13.00	14.75	August 2002	City contracts >\$10k	No
Hayward	9.71	11.20	April 1999	City contracts >\$25k	No
Los Angeles	9.39	10.64	March 1997	City contracts >\$25k, business assistance >\$100k annually or \$1m in 1 year	Yes
Oakland	10.07	11.58	March 1998	City contracts >\$25k, business assistance >\$100k, nonprofits, municipal lease holders	Yes
Oxnard	12.88	12.88	July 2002	City contracts >\$25k	No
Pasadena	9.16	10.73	September 1998	City contracts >\$25k	No
Port Hueneme	9.00	11.50	October 2003	City contracts >\$25k, business assistance >\$25k	Yes
Richmond	11.42	12.92	October 2001	City contracts >\$25k and >10 employees, nonprofits >\$100k, business assistance >\$100k, leaseholders with earnings of >\$350k and >25 employees	Yes
Sacramento	9.67	11.17	December 2003	City contracts >\$25k or business receiving >\$100k	Yes
San Fernando	8.50		April 2000	City contracts >\$25k, business assistance >\$25k	Yes
San Francisco <sup>a</sup>	10.77	10.77	August 2000	City contracts >\$25k (private and nonprofits), leaseholders at SFO airport	Yes <sup>b</sup>
San Jose	12.27	13.52	November 1998	City contracts >\$20k, businesses assistance >\$100k per year	Yes
Santa Cruz	12.43	13.56	October 2000	City contracts >\$10k, nonprofits service providers with grants >\$5k	No
Sebastopol	11.70	13.20	December 2003	City contracts >\$10k, business assistance >\$100k	Yes
Sonoma	11.70	13.20	July 2004	City contracts >\$10k	No
Ventura	9.75	12.50	May 2001	City contracts >\$25k, business assistance >\$25k	Yes
Watsonville	12.43	13.56	September 2002	City contracts >\$10k	No
West Hollywood	8.67	9.92	October 1997	City contracts >\$25k or >3 months	No

Note. Living wage information by city was compiled based on information from the Living Wage Resource Center, Association of Community Organizations for Reform Now. Supplementary information was obtained from the Employment Policies Institute's website (<http://www.epionline.org>) and through online searches of individual city's written ordinances.

a. This article does not include San Francisco's 2004 citywide minimum wage as a "living wage" law in the traditional sense and begins coding "LWevent" for San Francisco in 2000 when it passed its first living wage ordinance that applied to all city contractors. See Dube, Naidu, and Reich (2007) for a detailed analysis of the employment impacts of San Francisco's minimum wage law.

b. San Francisco's Minimum Compensation Ordinance does not explicitly include businesses receiving aid; however, the San Francisco Redevelopment Authority has its own living wage policy that mirrors the city's Minimum Compensation Ordinance.

passed living wage laws that cover private businesses contracting with the city, whereas nearly two thirds also include provisions that cover businesses receiving financial assistance or leasing city-owned property. For city contracts, the threshold level for coverage by the living wage is relatively consistent within the sample and ranges from \$10,000 to \$25,000. For business assistance provisions, there is less consistency with some cities including smaller subsidies and all firms that rent city-owned property (e.g., Berkeley and San Francisco) and others only covering large awards. In the analysis that follows, all of the 19 cities are analyzed jointly regardless of whether or not they include business assistance clauses. This joint estimation is justified for the following reasons. First, as other scholars have argued (Brenner et al., 2008), business assistance clauses have only been shown to be directly binding on a very small set of firms. Yet, such clauses may have important indirect effects that cannot be observed through case studies. As argued above, businesses may be signaled about an adverse business climate and locate elsewhere. Additionally, cities that have such clauses may simply not engage in the type of economic development practice that seeks to attract (low-wage) jobs, resulting in a potential negative effect on aggregate employment levels. Last, given the relatively small sample of treatment cities in California, it is not feasible to estimate separate impacts for each type of living wage law.

Last, it is critical to note that California's living wage cities are not a representative sample of all local governments operating in the state. Conversely, this list contains most of the state's large, centralized, urban areas—which generally experienced their most rapid growth periods much earlier in the 20th century—as well as several smaller, liberal-leaning towns and suburbs in the Bay Area and Southern California. This issue is a critical motivating factor for the propensity score-weighting approach described below that controls for such structural differences between the treatment group and the rest of California.

To merge the living wage information contained in Table 1, it is first converted to a panel data set that contains observations for each jurisdiction for each year between 1990 and 2005 (i.e., the unique ID for each record is the city code, year).<sup>7</sup> Next, a living wage indicator variable (LWevent) for each city is coded 0 for all years prior to passage and 1 for all years in which the living wage law was binding for a full year. Thus, in the case of Los Angeles, which passed its law in 1997, the LWevent variable is coded 1 beginning in 1998. Cities that never passed a living wage during the sample period are coded 0 for all years. Last, a dummy variable called LWcity was created, which is coded 1 for cities that ever passed a living wage law and 0 for all other cities. As discussed in the Background and Motivation section, the exact scope and coverage of each living wage law is unique.

### *The National Establishment Time Series Database*

The panel data set used for this analysis is derived from the NETS data. The NETS is a proprietary database developed by Donald Walls, PhD (Walls and Associates), in conjunction with the Dun and Bradstreet (D&B) business listings information service. D&B gathers data each year from extensive phone surveys of businesses for the purposes of establishing credit ratings for businesses of all sizes. Unlike the typical D&B files that are sold to business and credit issuing entities, the NETS is a longitudinal database created by taking 16 annual snapshots of the D&B file and linking establishments across years using a unique identifier assigned by D&B (the Data Universal Numbering System [DUNS] number). The NETS contains establishment level data on employment, sales (estimated), industry (eight-digit Standard Industry Code [SIC]), ownership structure, and address for the 1990 to 2005 period.

Unlike household surveys, such as the CPS, D&B attempts to capture the entire universe of establishments operating in a given year. Once D&B assigns a DUNS number to an establishment, they contact that establishment each year by telephone to update information on their location, ownership structure, industry, employment, and sales figures. To ensure that new businesses are captured by their telephone surveys, D&B reviews each states' database of fictitious name filings and business incorporation listings. Although D&B makes multiple attempts to reach each establishment, there are cases in which a DUNS number appears for several years, then disappears, and then reappears at the same address. In such cases, Walls and Associates imputes employment figures for each missing year based on the previous available records. Ultimately, the NETS database does a reasonably good job in capturing the level of economic activity (i.e., contacting all establishments) and in measuring employment levels. As noted in their careful review of the NETS file, Neumark, Zhang, and Wall (2005) argue that D&B has "an economic incentive" to ensure that its information is up-to-date and accurate and that it covers all existing establishments.

The value of using the NETS for a study of living wage impacts stems from the fact that it offers a consistent long-term (1990-2005) time series of employment and number of establishments at the local, as opposed to county, metropolitan, or state levels. The two major publicly available databases on employment over this time period are the Quarterly Census of Employment and Wages (QCEW) and the County Business Patterns (CBP). The QCEW is based on a census of all firms that file with state unemployment insurance agencies and captures a universe of nearly all establishments with payroll employment (with the exception of railroad industries). The CPB is an annual time series of employment, number of firms, and payroll, based on a combination of

surveys of firms drawn from the U.S. Census Bureau's Universal Business Establishment List and income tax filings. Although both data sources are widely used by economists for minimum wage research (see Dube et al., 2010), these data sets are not geographically refined enough to identify city-level employment or establishment counts in each year. At their finest level of detail—county, in the case of QCEW, and zip codes, in the case of the Zip Code Business Patterns (drawn from the CPB)—these publicly available sources do not disclose employment figures for detailed industries because of confidentiality constraints. For this reason, it is difficult to identify firms or industries that are likely to be “treated” by a living wage law passed at the urban scale.

In comparing the differences between employment figures contained in the NETS versus the QCEW or CBP, Neumark et al. (2005) show that the NETS differs significantly from these sources only for very small establishments (less than 5 employees). This is reasonable given that NETS captures many more self-employed persons (through its canvassing of fictitious name filings) and very small “cottage” firms. For larger establishments, employment estimates from NETS are quite similar to other sources. In terms of capturing employment changes, Neumark et al. also show that given the imputation methods used in the NETS, although the correlation between 1-year employment change in the NETS and the QCEW is somewhat weak, the correlation is much stronger for longer time periods (e.g., 3 years). These two data quality issues motivate both the data limitation steps below, as well as the additional analysis of living wage impacts overtime.

Unlike the QCEW or CBP, the NETS records are at the establishment level with detailed geographic identifiers, including the street address and latitude and longitude. This level of detail allows researchers to summarize employment and other information to any higher geographic summary level. Additionally, because the NETS database contains detailed industry codes as well as information on establishment linkages within firms, one can “drill down” the analysis to sectors and subsets of firms that are more likely to be covered by the living wage or more sensitive to changes in local labor market institutions.

In addition, the NETS file contains an indicator variable (govcont) for firms that have a contract with the government. D&B started to ask this question in 1998. Although there is no indicator as to what level of government the firm contracts with, this indicator is especially important for this study of the direct impacts of living wage ordinances in that it allows a more exact identification of “treated” and “untreated” firms.<sup>8</sup> Last, the unique identifier (DUNS number) for each record contains information on each establishment's place within a corporate structure (e.g., headquarters, branch, sole proprietor).

## Database Summary Steps

The process of constructing a time series database of employment and number of firms for each local legal jurisdiction in California (i.e., places) involved several limiting assumptions and the recoding of information in the NETS. These steps were taken to reduce the size of the data set, to ensure a geographically consistent time series, and to identify sets of firms that are arguably either “directly” or “indirectly” influenced by living wage laws. Because of the relative unfamiliarity with the NETS among researchers, these limiting assumptions and recoding steps are discussed in detail here.

**Sample limitation.** The full NETS database for California contains more than 4 million records, each with several hundred variables. To reduce the size of the California file so that it could be manipulated and summarized to the city level with the computing and software resources available, all records that had between one and four employees were dropped from the analysis. As noted above, removing very small establishments, most of which are single proprietors, actually makes the final data set more comparable with other publically available data sources that do not count self-employed persons.<sup>9</sup> It is important to note that this process only dropped records for which employment never reached five employees during the 1990 to 2005 period. If a firm had four employees for many years and then grew to six in the final period, this firm would be included in the data set.

**Geocoding.** To analyze the impact of living wage laws passed at the urban scale, one needs to construct a time series of employment and the number of establishments at the city level, as opposed to county or metropolitan level, since this is the scale of policy change. The NETS includes address information from each establishment that contains both the city name and a city code assigned by D&B. D&B assigns a unique city code for each city name recognized by the U.S. Postal Service. Unfortunately, the U.S. Postal Service city names do not correspond with actual political jurisdictions. For example, many establishments list their address as being located in the city of “North Hollywood.” However, North Hollywood is part of the City of Los Angeles, and firms located there are subject to the living wage ordinance. To overcome this disjuncture between the D&B city codes and the local jurisdictional boundaries, all establishments in the NETS were geocoded.

After removing very small establishments, the resulting database was geocoded using ArcGIS software based on the latitude and longitude listed in the NETS database. Although all records were successfully geocoded, some records were located outside the state of California. These records also contained address information that indicated they were located outside the state. These records either indicate firms that have moved outside the state or for which only the

headquarters location information is available. Because these records make up such a small share of the total reported employment (<1%), they were simply dropped. Once geocoded, each record was joined to an official U.S. Census place Federal Information Processing Standards (FIPS) code based on the spatial relationship between the location of each establishment relative to the boundary of each place.<sup>10</sup> In the resulting database, all establishments have a unique Census place FIPS code, which was used to aggregate employment and establishment counts up to the place level (i.e., city level).

**Industry selection.** When summarizing the establishment data to the city level, separate aggregations were made for specific targeted industries. The industry selections were based on the two-digit SIC codes that best corresponded to the sectors most likely to be covered by either contractor-only living wage laws. Several criteria were used to generate this industry selection. First, these industries must pay below average wages and be services rather than manufacturing sectors. Second, in many cases the text of a city's living wage law specified the specific service contract functions that are covered by the law. The specific list of industries chosen thus not only approximates the list of activities specified in the legal texts but also includes additional service industries that employ a large proportion of low-wage workers. The list includes eating and drinking places (SIC 58), hotels (SIC 70), personal services (SIC 72), business services (SIC 73), auto repair and parking (SIC 75), miscellaneous repair (SIC 76), and amusement and recreation (SIC 79). In addition, employment and establishment counts are calculated for the overall retail sector (which includes eating and drinking places) and a "total" category that captures all firms regardless of industry sectors. The results below report estimates of employment effects for three industry categories: (a) eating and drinking places, which is broken out by itself to compare with previous research on the employment effects of state-level minimum wage changes (Card & Krueger, 1994; Dube et al., 2010); (b) retail, the sector most likely affected by laws that cover firms business assistance provisions and leaseholders operating on city-owned land, such as airports; and (c) all other low-wage service sectors combined.

**Government contractors.** One key contribution of this article is the use of the NETS to "drill down" to those firms that are most likely to be covered by living wage laws. Starting in 1998, D&B began asking firms if they held contracts with government agencies. Although this variable reduces the panel size from 16 years (1990-2005) to 8 years (1998-2005), selecting out government contractors is a significant improvement in accuracy compared with using publicly available household-based surveys such as the CPS. The government contractor variable is simply a *Yes/No* indicator variable for each establishment record and gives no indication of the level of government a firm contracts with (e.g.,

local, state, or federal). Since living wage laws have no bearing on federal or state contractors, this category is further limited to the universe of government contractors in the low-wage service industries listed above. This would therefore not capture large defense contractors such as Boeing or Lockheed Martin located in Southern California, for example.

**Headquarters identification.** The final category of records that were selected and summarized to the city level are headquarters establishments. As argued in the Background and Motivation section above, living wage laws may have important indirect, signaling effects to firms who are concerned about the business climate in a given jurisdiction. To capture business establishments that have decision making power over where to locate, all firms that are categorized as headquarters by D&B (estcat=Headquarters) that also have at least one affiliated establishment with the same headquarters DUNS number (i.e., kids>0) were included in this category. The inclusion of headquarters in the empirical analysis is important because it offers the simplest test of the so-called business climate thesis, in that such establishments have the largest degree of latitude in choosing their location and are often the subject of interjurisdictional competition. The extent to which these more mobile establishments are negatively signaled by the living wage is a potentially important indirect effect.

**Demographic and political covariates.** The final step in creating the database used for the analysis below is the inclusion of demographic and political characteristics used to develop a propensity score (described below) based on the likelihood of passing a living wage law. As argued in the Living Wage Coverage and Case Selection section, the list of living wage cities in California differs markedly from the average local government in the state across a broad set of fixed structural factors that may influence employment changes. Given that the treatment group is relatively more urbanized and more politically liberal, information on city age, location, and demographics was gathered from the 2000 Census of Population and Housing Summary File 3. Specifically, the variables chosen were total population; the share of each city's population that is African American, Latino, and non-Hispanic White, respectively; median household income (in 1999 dollars); share of residential workforce that is unemployed; share employed by the government; and the share of the population with incomes below the poverty level. In addition to these population characteristics, a variable that measures the strength of Democratic voting in each city was included, as living wage laws tend to be passed by cities that are dominated by politically progressive governing coalitions headed by Democrats. Data on the share of registered voters who are Democrats are taken from University of California's Institute for Governmental Studies Statewide Database.<sup>11</sup>

## Identification Strategy

To measure the impact of living wage laws on the economic development prospects for the cities that enact them, this article follows a methodology commonly used in the literature on state and federal minimum wage impacts. However, as discussed in the Background and Motivation section, this approach is expanded to analyze the *indirect* impacts of living wage laws on the business climate, as well as the *direct* effect on sectors or groups of firms likely to be covered by a living wage law. To estimate the impact of minimum wage increases, researchers typically assemble a panel database of employment and wage levels over a time period that includes observations for several periods before and after one (or many) minimum wage increases. Using a standard ordinary least squares regression on this panel of data with employment as the dependent variable and fixed effects for each state and each time period, the coefficient on the minimum wage variable is interpreted as a “difference-in-difference” estimate. In other words, the minimum wage effect on employment is the difference in employment levels before and after the minimum wage change in the treatment group (i.e., states that raised the minimum wage) compared with the difference in employment in the control group, or states that never raised their minimum wage.

The traditional research design in panel studies of minimum wage effects involves two steps. The first step estimates the effect of minimum wage changes on the hourly wage of a certain group of workers or the average earnings levels for workers in a given industry such as restaurants. The second step estimates the effect of the minimum wage increase on the outcome variable of interest, typically employment. The first step measures the degree to which minimum wages actually result in higher wages for workers and consequently, impose higher costs on firms, potentially forcing them to reduce employment. This first step intends to establish that there was a “treatment” in the first place. If, for example, the minimum wage increased in a (hypothetical) state where all workers already earned wages above the new minimum wage, then increasing the minimum wage would be unlikely to have a direct negative effect on employment. Thus, estimating a wage regression checks for this scenario and indicates how much a given policy change is “binding” on employers.

Unfortunately, the NETS database does not contain information of wages paid to employees. Therefore, this article cannot directly test the wage impacts of living wage laws. Although this is a weakness in the research design, direct survey evidence from individual case studies in Los Angeles, San Francisco, Baltimore, and other cities indicates that workers in covered firms receive significant wage increases (Fairris, 2005; Reich et al., 2005). For example, after San Francisco passed its living wage law, the proportion of workers at the SFO airport earning less than \$10 per hour fell from

55% to 5% (Reich et al., 2005), indicating a significant coverage within the universe of covered firms. Therefore, although one cannot directly measure the degree to which living wage laws increase earnings using the NETS data, the fact that NETS allows a more focused analysis on government contractors in low-wage sectors indicates that this design is more likely to capture firms that were actually “treated” by the living wage, compared with previous panel regression studies.

The baseline model that used to generate difference-in-difference estimates of the effect of living wage laws among cities in California is given in Equation (1).

$$\ln E_{it} = \beta LW_{it} + \delta_t + \lambda_i \mu_{it}.$$

The dependent variable,  $E_{it}$ , is the natural log of either employment or number of establishments in city  $i$  in year  $t$ . For all specifications, estimates are generated for both employment and the total number of establishments to test whether the passage of a living wage law reduces either measure of economic development. The key independent variable,  $LW$ , is a living wage dummy variable coded 1 for all years that a living wage law is in effect for an entire year, 0 otherwise.  $\delta_t$  and  $\lambda_i$  are year and city fixed effects, respectively. Including a fixed effect for each year controls for the overall economic shocks associated with a given year, thereby holding constant broad economic growth patterns that commonly affect all cities in California. The city fixed effect absorbs all idiosyncratic characteristics of each city that do not vary over the time. The coefficient of interest,  $\beta$ , explains changes in the outcome variable within each city that coincide with the timing of the living wage law. Since Equation (1) is not a “log–log” equation,  $\beta$  cannot be interpreted strictly as an elasticity;<sup>12</sup> rather it is a semielasticity, which is the percentage change in employment associated with a unit change in the independent variable. In this case, it is the percentage change in employment or establishments that cities can expect by switching from non–living wage to living wage status.

*Direct versus indirect living wage impacts.* The empirical analysis is structured in two dimensions. First, the industries and subsets of firms used in the analysis are separated into those that are expected to either be directly or indirectly affected by living wage laws. The direct sectors are defined as (a) government contractors in low-wage service sectors, (b) all government contractors, and (c) all firms in low-wage service sectors. The subset of industries or firms that may potentially be indirectly affected by a living wage law include headquarter establishments and firms with significant shares of low-wage workers such as restaurants and retailers that may be signaled by a city’s attempt to regulate the local labor market. For the indirect analysis, this article also estimates a secondary specification summarized by Equation (2):

$$\ln(E_{it} / E_{mt}) = \beta LW_{it} + \delta_i + \lambda_i + \mu_{it}$$

In this specification, the dependent variable  $\ln(E_{it} / E_{mt})$  is natural log of city  $i$ 's share of the total employment (or establishments) in its metropolitan area ( $m$ ). All other terms in this equation are the same as Equation (1). This specification provides an additional test of the impact of living wage laws on a city's business climate, as it measures changes in the level of activity the city captures within the metropolitan context. Living wage laws and similar labor regulations passed at the local level primarily affect service sector industries that largely are geographically constrained in space. As such, a large share of the low-wage workers that such regulation aims to help perform activities—such as cooking restaurant meals or attending parking garages—that must take place face-to-face or in close proximity to final consumers. Therefore, we may not expect that firms in such *residential* industries to be highly mobile in response to changes in local labor costs (i.e., a fast-food restaurant in Los Angeles cannot relocate to Mexico and still serve the same customers).

Despite the relative “stickiness” of many low-wage service industries, there is still some degree of geographical mobility at the metropolitan scale, within which consumers and workers can move at relatively low costs. For example, retailers or certain restaurant chains may bypass the central urban jurisdiction (which are more likely to be living wage cities) and locate at or near the city boundary or in a nearby suburb, while still capturing the same geographically circumscribed market share. The modified dependent variable in Equation (2) will detect changes in a city's competitive position even if its own employment totals remain the same or even rise as it measures the share of total regional activity the city captures.

*Choosing the appropriate control sample for living wage cities in California.* The second dimension of the empirical analysis is to vary the control group used to calculate the difference-in-difference estimates of living wage impacts. The panel regressions based on Equations (1) and (2) produce an estimate of  $\beta$  by comparing employment or establishment counts in cities that pass living wage laws (before and after the year of passage) with those in other jurisdictions in California that did not pass such laws. There is a potential problem in making such a simple comparison because living wage cities may differ from non-living wage on a variety of unobserved characteristics, some of which may be correlated with economic growth. Specifically, the cities and towns that have successfully passed living wage laws in California tend to be older, slower growing, urbanized jurisdictions. These cities, such as Oakland, Los Angeles, and San Francisco, are geographically confined within their metropolitan areas and have experienced many cycles of industrial growth and decline. Not surprisingly, these cities experienced slower economic growth compared with the newer, growing portions of the state or even the outlying exurbs within their metropolitan

areas. As Figure 1 indicates, employment growth was slower among living wage cities throughout the sample period. This pattern is evident both before living wage laws began to spread within the state (1990-1997) and, to a greater extent, in the later period (1998-2005), which spans the dot-com boom and bust.

This indicates that using all non-living wage cities in California as a control sample for Equations (1) and (2) may produce biased estimates that could overstate a disemployment effect if living wage cities were declining relative to others around the time they passed living wage laws.

To test for and adjust for potential biased estimates, the main analysis is estimated for three different control samples. The first sample is limited to just the 19 cities that passed living wage laws at some point during the sample period. In this case the control group consists of the living wage cities themselves, with  $\beta$  being identified by variation in the timing of passage. The second sample uses all 886 places in California. This sample makes no adjustments for bias. The third sample uses all observations in California but applies a propensity score weight to each city's data, based on the likelihood of each city passing a living wage law. Although propensity score-weighting methodologies are not new in the program evaluation literature (see Rosenbaum & Rubin, 1983), this particular technique is described in more detail as it pertains to my particular research design.

As discussed above, living wage cities (the treatment group) and non-living wage cities (the control group) in California are likely to be different from one another on a variety of economic, social, and political variables in addition to their difference in living wage status. To adjust for differences in the pretreatment characteristics between treatment and control groups, one can gather data on all relevant covariates that might influence the likelihood that an individual case is treated and run a probit model that predicts treatment status based on the covariates. The resulting probability scores from the probit (i.e., the predicted values) are then used to weight the observations of the outcome variable. In this case, treatment status is determined by whether or not a given city passed a living wage law. To create the propensity score weights, living wage status is predicted for each city in the NETS database using a probit model estimated on the following independent variables: percentage non-Hispanic White, percentage Latino, percentage African American, total population, median household income in 1999, percentage government workers, percentage poverty, population per square mile, percentage unemployed, and percentage registered Democrats in 1998.<sup>13</sup> Finally, a weighted version of Equations (1) and (2) is estimated for each subset or industry under consideration. The weighted version is given by multiplying the left-hand side of each specification by  $(\hat{p}_i / [1 - \hat{p}_i])$ , where  $\hat{p}_i$  is the predicted probability of city  $i$  being a living wage city.

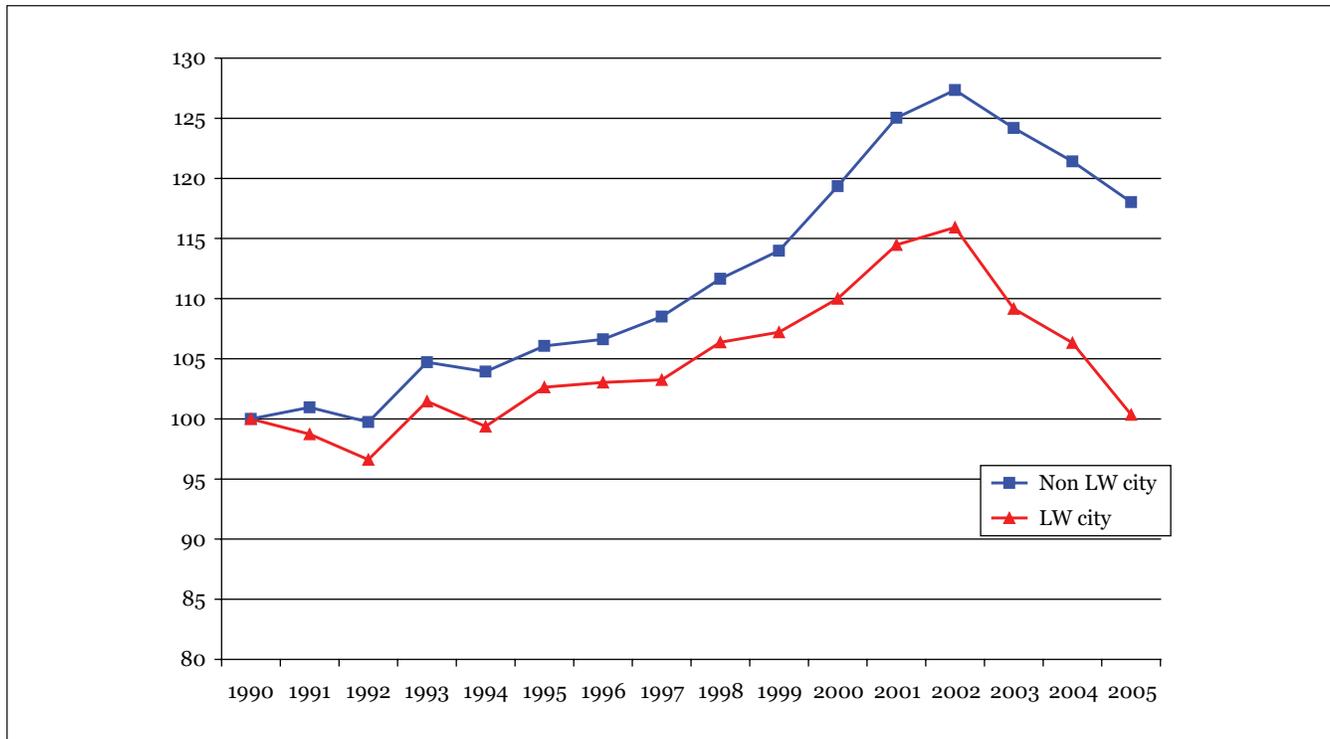


Figure 1. Employment trends in living wage cities and non-living wage cities, 1990-2005

Weighting the difference-in-difference estimates by a propensity score gives greater importance to cities that have similar demographic and political characteristics as the actual set of living wage cities. Table 2 gives the mean values for each variable used in the probit model for both living wage and non-living wage cities.

As indicated in Table 2, living wage cities tend to be significantly larger and denser, have a higher proportion of minority residents, and have slower employment growth in the pre-living wage period (1990-1998). In addition, living wage cities have a significantly higher share of registered democrats. The right-hand columns of Table 2 give the means on each variable after weighting by the propensity score. In almost all variables, the difference between the treatment and control groups is significantly diminished, indicating a successful balancing of these covariates after weighting.

## Results

Overall, this article finds no evidence that living wage laws have a significant negative impact on the level of employment or on establishments in the cities that choose to pass them. Furthermore, passing living wage ordinances is not associated with a negative shock to a city's business climate, as no negative effects are detected for the groups of firms that are likely to be sensitive to changes in the regulatory environment in low-wage labor markets.

## Direct Impacts

Table 3 summarizes the results of Equation (1) specified for the industries or subsets of firms that are directly affected by living wage laws. The top panel of Table 2 contains estimates of  $\beta$  for log employment and the bottom panel lists the impacts on log of establishments. As discussed above, each regression is estimated on three different samples: (a) living wage cities only, (b) all cities/places in California, and (c) a propensity score-weighted sample of all cities/places in California. For government contractors in low-wage service sectors (row 1) the difference-in-differences estimate is positive (.223 for employment and .109 for establishments) and significant at the 10% level when the sample is limited to only living wage cities. Although a positive effect is detected here, it is not warranted to claim that living wage raise employment among covered firms as this sample only compares living wage cities with each other and therefore has a limited sample size.

However, the estimates based on the larger control sample of all California cities (column 5) are negative for all industries and statistically significant for all sectors except low-wage government contractors. It is interesting that the effects are larger and/or more significant for the subsets of firms that are less and less likely to capture establishments for which the living wage is binding. For example, a negative and significant impact is detected on overall employment (across all

**Table 2.** Demographic and Political Differences Between Living Wage and Non-Living Wage Cities in California

Variable	Unweighted			Propensity score weighted		
	1. Living wage city (T)	2. Non-living wage city (C)	3. Difference (T – C)	4. Living wage city (T)	5. Non-living wage city (C)	6. Difference (T – C)
% Non-Hispanic White	0.471	0.607	–0.136*** <i>0.016</i>	0.471	0.432	0.039** <i>0.018</i>
% Latino	0.302	0.267	0.035** <i>0.015</i>	0.302	0.389	–0.087*** <i>0.018</i>
% African American	0.086	0.029	0.057*** <i>0.004</i>	0.086	0.068	0.018** <i>0.007</i>
Total population	374,048	23,050	350,998*** <i>7,099</i>	374,048	114,955	259,093*** <i>48,045</i>
Median income, 1999	46,798	49,235	–2,437* <i>1,498</i>	46,798	47,511	–713 <i>618</i>
% Government workers	0.150	0.168	–0.018*** <i>0.005</i>	0.150	0.150	0.000 <i>0.003</i>
% Poverty	0.139	0.143	–0.004 <i>0.006</i>	0.139	0.135	0.005 <i>0.004</i>
Employment growth rate, 1990-1998	0.160	0.439	–0.279*** <i>0.123</i>	0.160	0.148	0.012 <i>0.011</i>
Population per square mile	7,092	2,940	4,152*** <i>197</i>	7,092	7,085	7.569 <i>300</i>
% Unemployed	0.066	0.085	–0.020 <i>0.004</i>	0.066	0.067	–0.001 <i>0.002</i>
% Registered Democrats, 1998	0.729	0.539	0.189*** <i>0.009</i>	0.729	0.699	0.029*** <i>0.007</i>
% Registered Democrats, 2000	0.738	0.547	0.191*** <i>0.009</i>	0.738	0.708	0.030*** <i>0.007</i>

Note. T = treatment; C = control. Standard errors of the difference are in italics below the mean values. All variables are from 2000, unless otherwise noted. Propensity score weights are the predicted probabilities resulting from a probit model of living wage status. Predictors in the probit model are % non-Hispanic White, % Latino, % African American, ln(total population), ln(median household income), % government workers, % poverty, ln(Employment growth rate, 1990-1998), ln(Population per square mile), % unemployed, % registered Democrats, 1998.

\* $p < .10$ . \*\* $p < .05$ . \*\*\* $p < .01$ .

industries), but the point estimate for low-wage government contractors (–.068) is smaller and less significant than the estimate for total employment (–.078 significant at the 10% level). The estimated effects on establishments in this sample are similar to those for employment but are smaller in magnitude. A strong disemployment effect for the entire economy is not plausible as living wage laws only cover a small share of the firms in any given city.

Although this evidence may appear to suggest a negative impact on direct sectors, after the sample is weighted based on the propensity of each city to pass a living wage, the negative effects disappear. For this sample, the estimate for low-wage government contractors is slightly positive and insignificant (.044), which is a more realistic figure than the estimate reported in column 1. This point estimate for  $\beta$  can be interpreted as a semielasticity, thus indicating that passing a living wage ordinance is associated with a 4.4% increase in

employment in government contractors operating in low-wage service sectors. Again, one should use caution interpreting this figure as it is not statistically significant from zero. Furthermore, as indicated by the confidence interval (columns 11 and 12), the true living wage impact may range from –.10 to .19. Therefore, in the most conservative interpretation, this finding rules out direct disemployment effects larger than –.10. Overall, my preferred specification is the propensity score-weighted results in column 9 as they effectively controls for structural differences between living wage and non-living wage cities.

### Timing of Living Wage Effects

To further illustrate the point that the negative elasticities found in the full sample (column 5 above) are spurious, estimates of  $\beta$  are plotted, using a distributed lag structure of

**Table 3.** Direct Impact of Living Wage Laws on Employment and the Number of Establishments in California Cities

Industry/subset	Living wage cities only				All California cities				Propensity score weighted			
	1	2	3	4	5	6	7	8	9	10	11	12
	90% CI				90% CI				90% CI			
	Estimate	SE	LB	UB	Estimate	SE	LB	UB	Estimate	SE	LB	UB
<b>In(Employment)</b>												
Government contractors in low-wage sectors	.223*	.110	.039	.407	-.068	.085	-.210	.074	.044	.090	-.106	.195
All government contractors	.171	.160	-.097	.439	-.195*	.105	-.371	-.019	-.002	.101	-.172	.167
Low-wage services	-.008	.029	-.057	.041	-.065**	.018	-.096	-.035	.036	.021	.002	.070
Total employment	.033	.029	-.016	.082	-.076*	.018	-.106	-.046	0.018	.019	-.014	.049
<b>In(Establishments)</b>												
Government contractors in low-wage sectors	.109*	.052	.022	.197	-.029	.018	-.059	.000	.014	.031	-.038	.065
All government contractors	.050	.044	-.024	.123	-.007	.019	-.038	.025	.026	.026	-.017	.069
Low-wage services	-.019	.012	-.038	.001	-.033**	.009	-.049	-.018	-.010	.009	-.026	.005
Total employment	-.002	.011	-.020	.015	-.019*	.006	-.029	-.009	.003	.007	-.008	.014
N	104; 136; 304; 304				1,896; 4,456; 16,896				1,896; 4,456; 16,896			
No. of jurisdictions in sample	19				886				886			

Note. CI = confidence interval; LB = lower bound; UB = upper bound. Each row corresponds to an individual regression model with the dependent variable being the natural log (Ln) of employment or establishments in a given industry sector or firm subset. Total is the sum of all types of establishments. Other low-wage service sector includes Standard Industry Codes (SICs) 70, 72, 73, 75, and 79; restaurants is SIC 58. Government contractors are establishments that have a contract with any level of government. Column 1 contains the point estimate of the impact of living wage and approximates a percentage impact on the dependent variable. Column 2 is the robust standard error used to calculate the 90% CIs in columns 3 and 4.

\* $p < .10$ . \*\* $p < .05$ . \*\*\* $p < .01$ .

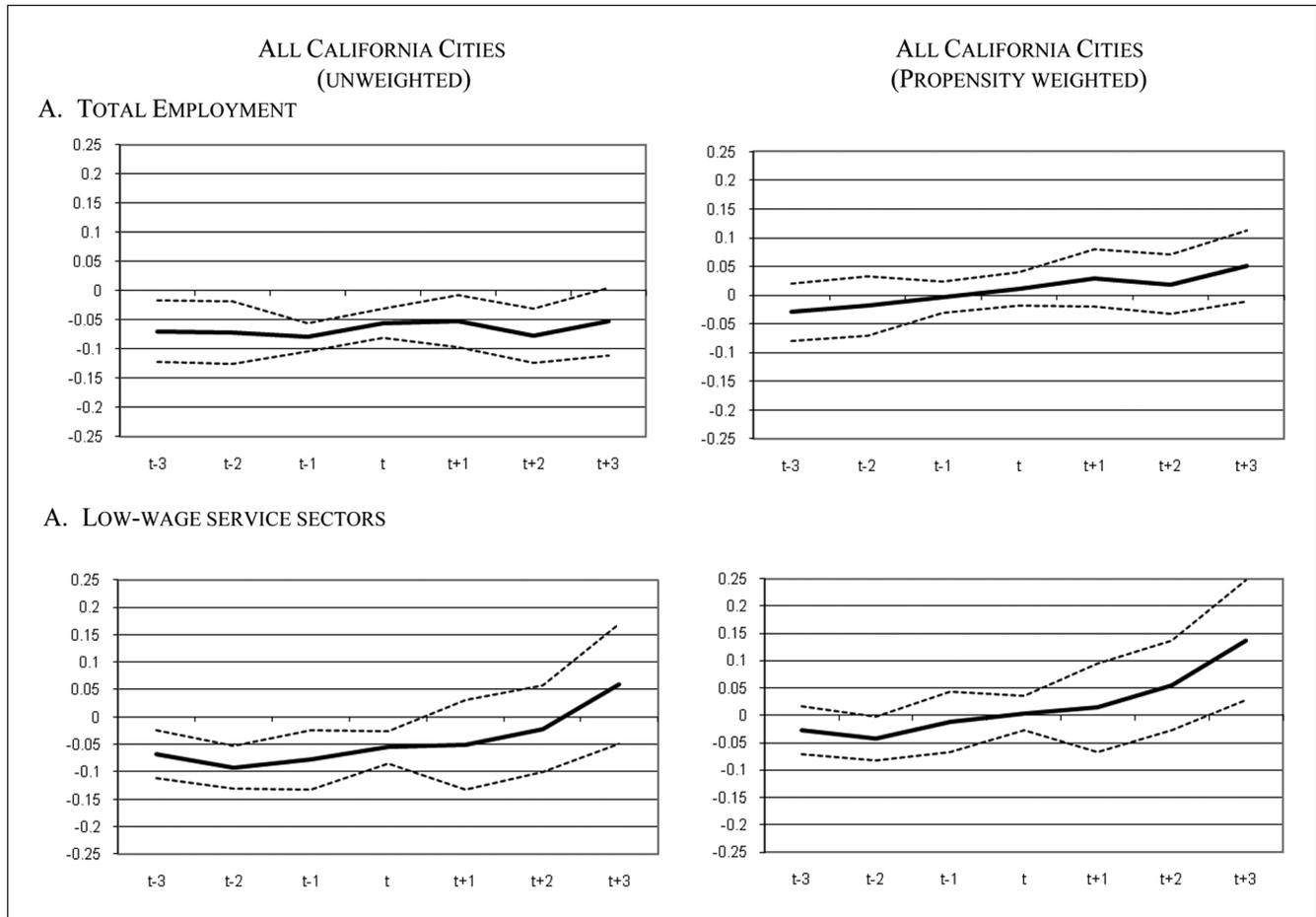
Equation (1). Specifically,  $\beta$  coefficients are calculated on values of the living wage indicator variable 3 years prior to and 3 years after the year of passage. So if a given city passed a law in 1999, then  $LW_{it(1999)} = 1$ , then the 3-year lead variable  $LW(t - 3)_{it(1996)} = 1$  in 1996. Similarly, the 3-year lag of the same city's  $LW$  variable would be coded 1 in 2002. Figure 2 plots the employment effects using this distributed lag analysis for both the unweighted sample of California cities, as well as the propensity score-weighted sample. The effects for low-wage service industries and total employment are listed. Since the government contractor variable only started in 1998, there are not enough years available in the data set to estimate the full set of (seven) coefficients.

As Figure 2 indicates the trend in living wage coefficients for the unweighted sample of total employment (A) is relatively flat and remains negative throughout the period before and after the passage of a living wage law ( $t_0$ ). This is consistent with the trend observed in Figure 1 above, which showed that living wage cities tended to grow more slowly than non-living wage cities in California. However, after weighting by the propensity score, the trend line shifts up and is still

essentially flat and is in all periods in centered on zero (i.e., the error bars lie on either side of the zero line). Similarly, the time path of employment effects in low-wage service sector industries does not indicate a sharp negative drop around the time of passage. After controlling for the propensity to pass a living wage law, the trend line keeps the same shape but is shifted vertically. The trend in the living wage coefficients for low-wage service sector is actually positive in the postpassage period, which indicates that a pronounced negative impact is unlikely.

### Indirect Effects

In addition to measuring the impact of living wage laws on employment in the firms that are most likely to be covered by living wage laws, this article also analyzes whether passing such laws harms a city's business climate by inducing noncovered establishments to relocate outside the city or for new firms to bypass the living wage city all together. The results of the indirect analysis are summarized in Table 4.



**Figure 2.** Time path of living wage employment effects, unweighted and weighted samples

As indicated in Table 4, nearly all specifications and samples indicate that the indirect effect of passing living wage laws on employment or the total count of establishments is very close to zero. Focusing on headquarters—those establishments that are listed as headquarters and have one or more related establishments under them—the point estimates is consistently small, ranging from .012 to .025, and closely centered on zero. This indicates that for firms that have some degree of power to make location decisions, the living wage—as it proxies for more progressive shift in the local business climate—does not result in job loss or relocation. The point estimates for headquarters remain slightly positive and centered on zero for the metropolitan share specifications (listed in the bottom panels of Table 4). The positive and significant coefficients obtained in the propensity-weighted specification are quite small but should be interpreted cautiously. It is unlikely that the living wage laws somehow induce headquarters to move to the enacting jurisdiction; rather, what is more likely is that living wage laws are passed, in some degree as a response to the development pressure and labor market inequality that stem from being a competitive location for mobile capital.

Although the retail and restaurant industries were included in the summary category of “all low-wage services” analyzed in the direct analysis, these two industries are broken out separately in the indirect section because (a) they are the largest employers of low-wage workers in most cities and (b) only a small portion of establishments are likely to be directly covered. In addition, restaurants associations and large retailers tend to be the loudest voices in opposition to living wage laws and similar government attempts to strengthen labor market institutions at the local scale. The results in Table 4 test whether living wage laws send a negative signal to these firms. For retailers the indirect effect is very close to zero for all samples and specifications (.009 to  $-.006$ ). For restaurants, however, a small negative and significant effect is found on the number of establishments when comparing living wage cities with all other cities in California ( $-.041$ ). This effect is reduced to  $-.028$  when the sample is adjusted using the propensity score, yet it is still barely significant at the 10% level. For both of these sectors however, we find no effect on the city’s share of employment or establishment growth at the metropolitan level.

**Table 4.** Indirect Impact of Living Wage Laws on Employment and the Number of Establishments in California Cities

Industry/subset	Living wage cities only				All California cities				Propensity score weighted			
	1	2	3	4	5	6	7	8	9	10	11	12
	Estimate	SE	90% CI		Estimate	SE	90% CI		Estimate	SE	90% CI	
<b>In(Employment)</b>												
Headquarters	.012	.031	-.040	.065	.012	.026	-.032	.056	.025	.025	-.017	.068
Retail	.000	.015	-.025	.025	-.020	.015	-.044	.005	-.014	.013	-.035	.008
Restaurants	.023	.024	-.017	.063	-.022	.018	-.052	.008	-.003	.019	-.034	.028
<b>In(Establishments)</b>												
Headquarters	-.018	.013	-.039	.004	.019	.011	.000	.037	.022*	.011	.004	.039
Retail	.009	.013	-.013	.031	.001	.009	-.014	.017	-.006	.009	-.022	.009
Restaurants	.009	.020	-.026	.043	-.041*	.016	-.067	-.014	-.028*	.016	-.055	-.001
<b>In(City share of MSA employment)</b>												
Headquarters	.017	.033	-.038	.072	.029	.032	-.025	.083	.040	.028	-.007	.088
Retail	.023	.015	-.002	.049	.004	.013	-.018	.026	-.003	.013	-.024	.018
Restaurants	.023	.023	-.016	.062	-.002	.017	-.030	.026	-.007	.018	-.036	.023
<b>In(City share of MSA establishments)</b>												
Headquarters	-.013	.020	-.046	.021	.078*	.023	.039	.117	.053*	.018	.023	.084
Retail	.023	.011	.004	.042	.012	.009	-.002	.027	.003	.009	-.011	.018
Restaurants	.020	.019	-.011	.051	-.012	.015	-.037	.013	-.016	.015	-.041	.008
N		304; 304; 304			14,176; 15,664; 13,280				14,157; 15,580; 13,226			
No. of jurisdictions in sample	19				886				886			

Note. CI = confidence interval; LB = lower bound; UB = upper bound; MSA = Metropolitan Statistical Area. Each row corresponds to an individual regression model with the dependent variable being the natural log (ln) of employment or establishments in a given industry sector or firm subset. Retail is SIC 52-59; restaurants is SIC 58. The category called headquarters is the sum of employment or count of records that are coded by Dun and Bradstreet as headquarters that have at least one "child" record associated with their Data Universal Numbering System ID number. Column 1 contains the point estimate of the impact of living wage and approximates a percentage impact on the dependent variable. Column 2 is the robust standard error used to calculate the 90% CIs in columns 3 and 4.

\* $p < .10$ . \*\* $p < .05$ . \*\*\* $p < .01$ .

## Conclusion

Ultimately, the findings of this article confirm the general conclusions of individual case evaluations such as Fairris (2005) and Reich et al. (1999) that living wage laws do not have large negative impacts on employment. Since the NETS database provides a consistent time series of employment and establishment counts at the city level (the scale at which the laws are enacted) and allows a more accurate identification of covered firms (i.e., government contractors), this research improves on existing panel studies (e.g., Adams and Neumark (2005) and contradicts their finding of a significant disemployment effect. For government contractors in low-wage sectors—where one would expect to find the largest impacts—this article finds slight positive effects associated with passing living wage laws. However, because of the large standard error, one can only rule out negative impacts larger than 10%. In addition to adding additional information to the empirical literature on the direct impact of

living wage laws, my findings also suggest that living wage laws do not significantly harm a city's business climate. With the exception of restaurant establishments, there is little evidence that firms flee cities that pass living wage laws or that economic growth shifts to suburban portions of the metropolitan area.

Although the research was based on an analysis of living wage laws in California—and as such the empirical findings are limited to the confines of the state—the findings are relevant to the broader living wage debate throughout the United States for several reasons. First, California is a large and economically diverse state that contains significant variation in industrial structure and the degree of economic restructuring at the urban scale. For instance, this sample of living wage cases includes cities that have undergone significant industrial decline such as Oakland and Richmond, similar to cities in the so-called rust belt. It also contains examples of growing, high-technology urban economies that generate considerable wealth and income inequality (e.g.,

San Jose and San Francisco). Second, California, like many states in the United States, exhibits divergent growth pattern among local governments characterized by rapidly growing exurban areas surrounding older, slow growth central cities. The research design presented in this article offers other researchers a method of controlling for such structural differences between local governments when attempting to isolate the impact of urban policy changes. Although California does not perfectly mirror the interurban trends across the United States, policy makers may find the results of this article to be more generalizable than an individual case study.

In addition to its implications for the empirical literature on the economic impact of local wage regulation, this research also adds information to broader theoretical debates over the degree to which local government can promote a redistributive policy agenda in an era of federal retrenchment and economic globalization. This article suggests that cities have more latitude in their ability to counter the dominant “business climate” discourse. Contrary to the expectation that if cities strengthen regulations or raise costs for local businesses, then they simultaneously harm their prospects for future economic development, the findings presented here indicate that the equity–growth trade-off is not a hard-and-fast rule. As discussed in the introduction, living wage ordinances—whether they apply to city contracts or financial incentives—are one policy tool among many examples of alternative approaches to economic development. Although the actors and coalitions that push for an help implement these progressive labor market interventions vary in their strength and effectiveness, they nonetheless have presented a policy agenda that stands in stark contrast to the mainstream, business attraction model of economic development at the urban scale. The addition of clear empirical analyses of the impact of alternative economic development policies—such as the living wage—is crucial to these ongoing debates.

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### Notes

1. This research was made possible by the purchase of the NETS database by the Institute for Research on Labor and Employment at University of California, Berkeley. Given the Institute for Research on Labor and Employment’s focus on California and given the cost of acquiring NETS data for the entire United States, the author was limited to the case of California. However, focusing on cases within a single state does have the advantage of minimizing the effect of state policy differences or broad regional growth trends on the empirical estimate of living wage impacts.
2. A third type of living wage law applies to only a subset of firms in the city defined either by their geographic location (e.g., Santa Monica’s 2005 law affecting only its tourist area) or by their industry and size characteristics (e.g., Chicago’s failed 2006 “Big-Box” retail living wage law). This latter type of living wage law is relatively rare; thus they will not be included in the empirical analysis.
3. As described below the unit of analysis in my study is the “place” level, as defined by the U.S. Census. Places include all forms of local government jurisdiction such as cities, townships, villages, and unincorporated county lands, referred to as census-designated places.
4. Information initially gathered from the Living Wage Resource Center, a project of the now defunct organization ACORN (URL now off-line). However this information is still available from the Employment Policies Institute (EmPI) <http://www.epionline.org>.
5. Note that two cities, Santa Barbara and Santa Monica, enacted laws in 2005 or later and are therefore excluded from the analysis since the NETS database only contains employment information through 2005.
6. Information on the exact coverage and scope of each city’s law was double-checked via web searches of the actual municipal codes that were passed.
7. For example, the ID “440001990” corresponds to the place FIPS code for Los Angeles (44000) and the year (1990).
8. In the city-level NETS sample constructed here, the average share of employment made up of government contractors was 5.2% whereas government contractors in low-wage service sector industries had a mean employment share of 1.2% across the sample. These figures are comparable with estimates listed in Pollin, Brenner, Wicks-Lim, and Luce (2008).
9. Neumark et al. (2005) chart employment totals by firm size for the NETS, compared with the CBP in 2002. The only category for which the NETS differed significantly was the smallest category (1-4).
10. The place shapefile used for the spatial join is available from the U.S. Census (<http://www.census.gov/geography>). The specific type of spatial join used matched records that fell within a place boundary to that place. In instances in which a NETS record fell outside a place boundary, it was assigned the nearest place FIPS based on Euclidian distance.
11. See <http://swdb.igs.berkeley.edu/data.html>.

12. The term *elasticity* is defined as the percentage change in the outcome variable associated with a percentage change in the independent variable.
13. Statistical output of the probit model is available from the author on request.

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