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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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DRAFT

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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 thus have the potential to reduce economic growth. I assess the impact of living wage laws on employment and establishment levels in the cities that pass them. I provide separate estimates for government contractors and other firms that may be indirectly signaled by a change in the local political environment. I use the National Establishment Time-series database to construct a panel dataset that tracks employment and establishment levels for all California jurisdictions. I produce difference-in-difference estimates that indicate that living wage laws have no significant impact on employment or establishment growth. Additionally, I find no evidence that the passage of living wage laws sends a negative "signal" to businesses about a potentially harmful local business climate.

† Postdoctoral Fellow, Institute for Research on Labor and Employment (IRLE). This paper is based on research completed for a dissertation in City and Regional Planning. I wish to thank the members of my committee, Karen Chapple, Michael Reich, Margaret Weir, Mike Teitz, as well as Arindrajit Dube, and Daniel Hartley for helpful comments. This paper was presented at the 2008 ACSP-AESOP conference in Chicago, Illinois and the UC Berkeley PhD Colloquium. Thanks also to the faculty at the UC Berkeley Institute for Research on Labor and Employment (IRLE) for access to the NETS database.

1. 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 either through direct subsidies or by fostering an environment conducive to innovation or, more generally projecting a positive "business climate" within regional and global arenas. Since 1994 however, living wage ordinances—now enacted in 140 local jurisdictions nationwide—have emerged as an alternative response to labor market polarization in urban 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). While 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 impact both direct economic development outcomes (e.g. employment and firm growth) and the city's reputation among business decision makers.

This paper 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 in sub-sectors that are most likely to be directly covered by living wage laws, such as government contractors in low-wage service industries. This paper refers to this analysis as the "direct" impacts. In addition this paper 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 has focused on the overall costs and benefits to individual cities (Fairris 2005; Reich, Hall et al. 2005) or has relied on public data sources with limited sample sizes for all but the largest cities, such as the Current Population Survey (Neumark 2002; Adams and Neumark 2005). A major innovation in this paper is the use of a new, geographically-refined database to measure the employment impacts of an urban policy. The National Establishment Time-Series (NETS) dataset provides time-series data on employment and various business characteristics at the establishment level. In this paper, the NETS is summarized to the city-level to construct a panel dataset that tracks employment levels and establishment counts for all California jurisdictions between 1990 and 2005. Next, this paper 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 CA[1]. This paper 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 (see Rosenbaum and 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 paper finds no evidence that the passage of living wage laws is associated with reduced levels of employment or establishments in directly

[1] See Appendix A for a list of cities with living-wage ordinances in California.

affected industries. In addition, living wage laws do not appear to harm the "business climate" of the cities that pass them, in as far as companies do not avoid living wage cities or relocate to uncovered (suburban) portions of the metropolitan area.

The findings of this paper cast doubt on the broadly held assumption that progressive urban governments face a stark tradeoff between equity and economic growth. In the arena of labor market interventions—such as living wage mandates—this paper 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.

This remainder or this paper is organized as follows. Section two provides background information on living wage movement and discusses the possible ways living wage laws may impact urban labor markets. Section three reviews recent empirical research on the effect of living wage laws on employment and economic growth. Section four presents the methodological steps used to construct a city-level panel dataset from the NETS and estimate direct and indirect living wage impacts. Section five discusses the results. Finally section six concludes and contextualizes the findings within the empirical and theoretical literatures.

2. Background and motivation

2.1. Provenance and substance of living wage laws

In the fourteen years since Baltimore passed the first living wage law, over 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 grass-roots community organizations—often affiliated with a national progressive organizing network such as Association of Community Organizations for Reform Now (ACORN) or the Industrial Areas Foundation (IAF). The coalition-backed organizations that coalesce around a living wage campaign, such as BUILD in Baltimore or LAANE in Los Angeles, put direct pressure on elected officials, testify at public hearings and engage in high-profile public relations efforts to raise the issue of working poverty within local policy discourses.

Just as the political coalitions and local economic conditions of each city are unique, the extent of coverage and 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 three 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. A more broad form of living wage law extends to businesses that receive any form of assistance from the city government. These businesses include those who receive economic development incentives, tax breaks or those that operate in city-owned property. The 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 work below.

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 due to 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 LAANE 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 upon the political economy of San Francisco (as discussed in 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 city-wide minimum wage by a wide margin (2003), then universal health access (2006) and paid family leave in (2006). Without the passage of the living wage ordinance, the subsequent laws were unlikely to even be debated among the city supervisors.

Although these 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. 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? Specifically, do living wage laws shift the balance of employment within metropolitan areas?

2.2. How Living-wage laws alter the urban economy

Economic theory predicts that there are two ways in which firms may react to the living wage laws. This paper refers to these as either *direct* or *indirect* (signaling) effects. Businesses are directly impacted by a newly enacted living wage law if they: (1) are currently located within or considering a move to the enacting city, (2) operate in an industry or business category (e.g. city contractor) that is legally bound by the ordinance, and (3) 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 which may not be 'bound' by a city ordinance may choose to locate elsewhere because the law itself indicates the presence of an anti-business political climate. This political coalition may pass future legislation that directly raises labor costs, 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. (Bartik 2004, 270)

Neumark and Adams (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:

> 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. (Neumark and Adams, 2005, 1-2).

As described below, my research will address 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 Section 3 below.

3. 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 (neoclassical) 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 work 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 -0.1 and -0.3 (Brown, Gilroy et al. 1982; Neumark and Wascher 1992).

This consensus disintegrated in the 1990s as researchers adopted new "quasi-experimental" research designs and studied more recent minimum wage events (Card and Krueger 1995). The now classic paper 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 to control groups in Pennsylvania and New Jersey (firms that already paid $5.05 per hour or higher).

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, Hall and Hsu (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." (Reich, Hall, and Hsu, 1999, 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 uncovered 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 while turnover and absenteeism dropped relative to the control group. Most importantly, he finds no significant difference in employment levels. More recently Dube, Naidu and Reich (2006) 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.

Instead, 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 Current Population Survey (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 dataset. Specifically, Brenner et. al. (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 dataset (CPS) over samples a single case (Los Angeles). The drawback with using broad household surveys such as the CPS is that there are too few cases to accurately distinguish "covered" and "uncovered" workers. Unlike Farris (2005), he cannot specifically identify a worker that

worked for a firm covered by the living wage. Also, due to data constraints Neumark (2002) restricts his analysis to the 1996-2002 period, a relatively short time-period during an economic expansion.

More recently, Neumark and Adams (2005) measure the impact of living-wage laws on the income and employment levels of low-income workers by comparing cities that passed living-wage laws to those in cities that had a failed living-wage campaign. The benefit of this research design (also referred to as a used a regression discontinuity design (RDD)) 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 which may also affect the outcome variables (e.g. employment). Neumark and Adams find a statistically significant negative employment elasticity of -0.15 for lower-skilled workers (Neumark and Adams 2003). However, this study also uses the Current Population Survey, and can be criticized on the same grounds listed above.

There is a discrepancy within the empirical literature on living wage effects. On the one hand, a panel study of the type applied by Neumark (2002) which 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, I introduce 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.

4. Data sources and methodology

To address the gap in the empirical literature between single-city employer surveys and panel studies using national household surveys, I rely on a new, privately produced database to construct a panel dataset of city-level employment for all cities[2] in California. Because the NETS database is relatively new to academic researchers and has not yet been used to estimate the causal effects of urban policies, I begin this section with a detailed description of the NETS database itself. Next, I describe the steps I 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 I use to test for both 'direct' and 'indirect' effects of living-wage laws on employment and establishment levels. Lastly, I introduce my specific identification strategy and present the panel regression equations.

4.1 The National Establishment Time series (NETS) Database

The panel dataset used for this analysis is derived from the National Establishment Time-series (NETS) data. The NETS data 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 Dun and Bradstreet (the DUNS number). The NETS contains

[2]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 (CDPs).

establishment level data on employment, sales (estimated), industry (8-digit SIC), ownership structure, and address for the 1990-2005 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 business are captured by their telephone surveys D&B reviews each states' database of fictitious name filings and business incorporation listings. While 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 County Business Patterns is an annual time-series of employment, number of firms and payroll based on a combination of surveys of firms drawn from the US Census Bureau's Universal Business Establishment List and income tax filings. While both data sources are widely used by economists for minimum wage research (see Dube, Lester et al. 2007), these datasets 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 County Business Patterns), these publicly available sources do not disclose employment figures for detailed industries due to confidentiality constraints. For this reason, it is difficult to identify firms or industries that are likely to be "treated" by a living-wage law that is passed at the urban scale.

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 data contains detailed industry codes as well as information on establishment linkages within firms, I can "drill-down" my 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. While there is no indicator as to what level of government the firm contracts with, this indicator

is especially important for my study of the direct impacts of living wage ordinances in that it would allow a more exact identification of "treated" and "untreated" firms. Lastly, 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).

4.2 Database summary steps

To construct a time-series database of employment and number of firms for each local legal jurisdiction in California (i.e. places) I made several limiting assumptions and recoded information in the NETS. I took these steps to reduce the size of the dataset, to ensure a geographically consistent time-series, and to identify sets of firms that I argue are either "directly" or "indirectly" impacted by living-wage laws. Due to the relative unfamiliarity with the NETS among researchers, I discuss these limiting assumptions and recoding steps in detail here.

Sample limitation:

The full NETS database for California contains over 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, I dropped all records that had between one and four employees. Removing very small establishments, most of which are single proprietors, actually makes the final dataset more comparable to other publically available data sources which do not count self-employed persons.[3] It is important to note that I only dropped records for which employment never reached 5 employees during the 1990-2005 period. If a firm had 4

[3] Neumark, Zhang, and Wall (2005) chart employment totals by firm size for the NETS, compared to the CBP in 2002. The only category for which the NETS differed significantly was the smallest category (1-4).

employees for many years and then grew to 6 in the final period, this firm would be included in my dataset.

Geocoding:

To analyze the impact of living wage laws passed at the urban scale, I need 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 US Postal Service. Unfortunately, the US 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 I geocoded all establishments in the NETS.

After removing very small establishments, the resulting database was geocoded using ArcGIS software based on the latitude and longitude listed in the NETS database. While 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 of the state. These records either indicate firms who 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 (<1%), I simply dropped these records. Once geocoded, I joined to each record the official US Census place FIPS

code based on the spatial relationship between the location of each establishment relative to the boundary of each place.[4] In the resulting database, all establishments have a unique Census place FIPS code, which I use 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 I made separate aggregations for specific industries. I based the industry selections on the three-digit Standard Industrial Classification (SIC) codes that best corresponded to the sectors most likely to be covered by either contactor-only or business assistance living-wage laws. Not surprisingly, these industries include low-wage service jobs. The specific industries chosen are: 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, I calculate employment and establishment counts for the overall retail sector (which includes eating and drinking places), and a "total" category which captures all firms regardless of industry sectors. In the results below, I report estimates of employment effects for three industry categories: (1) 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 and Krueger 1994; Dube, Lester et al. 2007), (2) retail, the sector most likely impacted by laws that cover firms operating on city-owned land such as airports, and (3) all other low-wage service-sectors combined.

[4] 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.

Government contractors

One key contribution of this paper 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 to using publicly available household-based surveys such as the CPS. The government contractor variable is simply a Y/N 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 I further limit the universe of government contractors to those 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 firms that I select and summarize to the city-level is what I refer to as "headquarters". As argued in section 2 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, I select all firms that are categorized as headquarters by D&B (estcat=Headquarters) which also have at least one affiliated establishment with the same headquarters duns number (i.e. kids>0).

Living wage information

Once the place-level time-series NETS database was constructed, I added an indicator variable to each city in California that successfully passed a living wage. Data on living wage laws, including the date of passage was obtained from the Living Wage Resource Center website maintained by ACORN.[5] The panel dataset contains observations for each jurisdiction for each year between 1990 and 2005 (i.e. the unique id for each record is the city, year.[6] I coded each city's living wage variable (LWevent) 0 for all years prior to passage and 1 for all years in which the living wage law was binding for a full year (see Appendix A for a list of Cities in California with living-wage laws and the dates of passage). Cities that never passed a living wage during the sample period are coded 0 for all years. Lastly, I created a dummy variable called LWcity which is coded 1 for cities that ever passed a living-wage law and 0 for all other cities. As discussed in section 2, the exact scope and coverage of each living-wage law is unique. For the purposes of this study however, I treat all contractor-only and business-assistance living wage laws equally.[7]

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. I gathered demographic information from the 2000 Census of Population and Housing Summary File 3 including the following variables: total population, the share of each city' population that is

[5] See http://www.acron.org/livingwage.
[6] For example the id '440001990' corresponds to the place FIPS code for Los Angeles (44000) and the year 1990).
[7] I do not include San Francisco's 2004 citywide minimum wage as a "living wage" law in the traditional sense and begin 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 (2006) for a detailed analysis of the employment impacts of San Francisco's minimum wage law.

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, I added a variable that measures the strength of Democratic voting in each city, 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 is taken from University of California's Institute for Governmental Studies (IGS) Statewide Database.[8]

5. Identification strategy

To measure the impact of living wage laws on the economic development prospects for the cities that enact them, I follow a methodology used commonly in the literature on state and federal minimum wage impacts. However, as discussed in section two, I expand this approach 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 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 (OLS) 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

[8] See http://swdb.igs.berkeley.edu/data.html

treatment group (i.e. states that raised the minimum wage) compared to 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. This first step measures the degree to which minimum wages actually result in higher wages for workers, and consequently, imposes 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 data does not contain information of wages paid to employees. Therefore, I cannot directly test the wage impacts of living-wage laws. While this is a weakness in my 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, Hall 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 percent (Reich, Hall et al. 2005, 1), indicating a significant coverage within the universe of

covered firms. Therefore, while I cannot directly measure the degree to which living-wage laws increase earnings using the NETS data, the fact that I can conduct a more focused analysis on government contractors in low-wage sectors indicates that I am likely to capture firms that were actually "treated" by the living wage.

The baseline model that I use to generate difference-in-difference estimates of the effect of living-wage laws among cities in California is given in equation one.

(1) $$\ln E_{it} = \beta LW_{it} + \delta_t + \lambda_i + \mu_{it}$$

The dependent variable, $\ln E_{it}$, is the natural log of either employment or number of establishments in city *i* in year *t*. For all specifications, I generate estimates 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. δ_t and λ_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, β 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, β

cannot be interpreted strictly as an elasticity[9], rather it is a semi-elasticity, which is the percent change in employment associated with a unit change in the independent variable. In this case it is the percent 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

I structure the empirical analysis in two dimensions. First, I divide the industries and subsets of firms into those that I expect are either directly or indirectly impacted by living wage laws. I define the direct sectors as: 1) government contractors in low-wage service sectors; 2) all government contractors; and 3) all firms in low-wage service sectors. The subset of industries or firms that I argue may be indirectly impacted 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, I also run a secondary specification summarized by equation 2 below.

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

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 above. 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

[9] The term elasticity is defined as the percent change in the outcome variable associated with a percent change in the independent variable.

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 *residentiary* 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.)

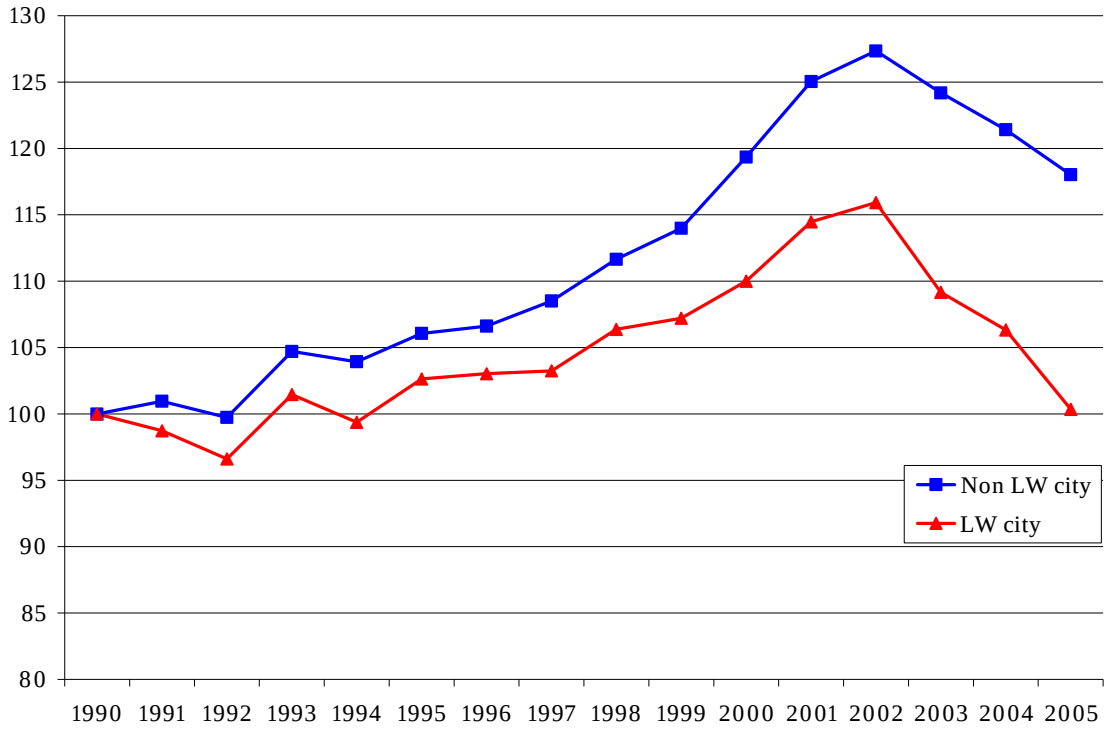
Despite the relative "stickiness" of many low-wage service industries, there is still some degree of geographical mobility at the metropolitan scale, within which consumers and workers can move at relatively low costs. For example, retailers or certain restaurant chains may bypass the central urban jurisdiction (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 conscribed 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 that I use to calculate the difference-in-difference estimates of living wage impacts. The panel regressions based on equations 1 and 2 above produce an estimate of β by comparing employment or establishment counts in cities that pass living wage laws (before and after the year of passage) to 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 to 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.

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



This indicates that using all cities 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, I run the main analysis 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 are the living wage cities themselves, with β 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 and Rubin (1983), I will describe this technique 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 pre-treatment 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, I predict living wage status for each city in the NETS database using a probit model estimated on the following independent variables: percent non-Hispanic white, percent Latino, percent African American, total population, median household income in1999, percent of government workers, percent poverty, employment growth rate (1990-98), population per square mile, percent unemployed, and the percent registered democrats in 1998.[10] Then I run a weighted version of equations 1 and 2 for each subset or industry under consideration. The weighted version is given by multiplying the left hand side of each

[10] Statistical output of the probit model is available from the Author upon request.

specification by $(\hat{p}_i / 1 - \hat{p}_i)$ where $\hat{p}_i$ is the predicted probability city *i* being a living-wage city.

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 1 gives the mean values for each variable used in the probit model for both living wage and non-living wage cities.

Table 1 Demographic and political differences between living wage and non living wage cities in California.

	Unweighted			Propensity Score Weighted		
	(1)	(2)	(3)	(4)	(5)	(6)
Variable	**Living wage City (T)**	**Non-living wage city (C)**	**Difference (T-C)**	**Living wage City (T)**	**Non-living wage city (C)**	**Difference (T-C)**
% Non-Hispanic White	0.471	0.607	-0.136*** *0.016*	0.471	0.432	0.039** *0.018*
% Latino	0.302	0.267	0.035** *0.015*	0.302	0.389	-0.087*** *0.018*
% African American	0.086	0.029	0.057*** *0.004*	0.086	0.068	0.018** *0.007*
Total population	374,048	23,050	350,998*** *7099*	374,048	114,955	259,093*** *48,045*
Median income, 1999	46,798	49,235	-2437* *1498*	46,798	47,511	-713 *618*
% of government workers	0.150	0.168	-0.018*** *0.005*	0.150	0.150	0.000 *0.003*
% poverty	0.139	0.143	-0.004 *0.006*	0.139	0.135	0.005 *0.004*
Emp. growth rate, 1990-98	0.160	0.439	-0.279*** *0.123*	0.160	0.148	0.012 *0.011*
Population per square mile	7,092	2,940	4,152*** *197*	7,092	7,085	7.569 *300*
% unemployed	0.066	0.085	-0.020 *0.004*	0.066	0.067	-0.001 *0.002*
% registered democrats, 1998	0.729	0.539	0.189*** *0.009*	0.729	0.699	0.029*** *0.007*
% registered democrats, 2000	0.738	0.547	0.191*** *0.009*	0.738	0.708	0.030*** *0.007*

Notes: Standard errors of the difference are in italics below mean. 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), % of government workers, % poverty, Ln(Employment growth rate, 1990-98), ln(Population per square mile), % unemployed, % registered democrats 1998.

As indicated in the table, 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 1 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.

6. Results

Overall, I find no evidence that living wage laws have a significant negative impact on the level of employment or 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 I find no negative effects 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 2 summarizes the results of equation 1 specified for the industries or subsets of firms that I claim are directly impacted by living-wage laws. The top panel of Table 2 contains estimates of β 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: (1) living-wage cities only, (2) all cities/places in California, and (3) a propensity score weighted sample of all cities/places in California. For government contractors in low-wage service sectors (row 1) the difference-in-difference estimate is positive (0.223 for employment and 0.109 for establishments) and significant at the 10% level when the sample is limited to only living-wage cities. Although I find a positive

effect, I would not claim that living wage raise employment among covered firms as this sample only compares living wage cities to each other and therefore has a limited sample size.

Table 2 Direct impact of living wage laws on employment and the number of establishments in California cities.

	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	
Industry/Subset	**Estimate**	**SE**	**LB**	**UB**	**Estimate**	**SE**	**LB**	**UB**	**Estimate**	**SE**	**LB**	**UB**
Ln Employment												
Government contractors in low wage Sectors	0.223*	(0.110)	0.039	0.407	-0.068	(0.085)	-0.210	0.074	0.044	(0.090)	-0.106	0.195
All government contractors	0.171	(0.160)	-0.097	0.439	-0.195*	(0.105)	-0.371	-0.019	-0.002	(0.101)	-0.172	0.167
Low wage services	-0.008	(0.029)	-0.057	0.041	-0.065**	(0.018)	-0.096	-0.035	0.036	(0.021)	0.002	0.070
Total employment	0.033	(0.029)	-0.016	0.082	-0.076*	(0.018)	-0.106	-0.046	0.018	(0.019)	-0.014	0.049
Ln Establishments												
Government contractors in low wage Sectors	0.109*	(0.052)	0.022	0.197	-0.029	(0.018)	-0.059	0.000	0.014	(0.031)	-0.038	0.065
All government contractors	0.050	(0.044)	-0.024	0.123	-0.007	(0.019)	-0.038	0.025	0.026	(0.026)	-0.017	0.069
Low wage services	-0.019	(0.012)	-0.038	0.001	-0.033**	(0.009)	-0.049	-0.018	-0.010	(0.009)	-0.026	0.005
Total employment	-0.002	(0.011)	-0.020	0.015	-0.019*	(0.006)	-0.029	-0.009	0.003	(0.007)	-0.008	0.014
N=	(104, 136, 304, 304)				(1896, 4456, 16896)				(1896, 4456, 16896)			
No of jurisdictions in sample	19				886				886			

Notes: 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 SICs 70, 72, 73, 75, 79. Restaurants (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% Confidence intervals in columns 3 and 4.

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, I find a negative and significant impact on overall employment (across all industries), but the point-estimate for low-wage government contractors (-0.068) 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.

While 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 (0.044), which is a more realistic figure than the estimate reported in column 1. This point estimate for β can be interpreted as a semi-elasticity, thus indicating that passing a living wage ordinance is associated with a 4.4 percent increase in employment in government contractors operating in low-wage service sectors. Again, I use caution interpreting this figure as it is not statistically significant from zero. Further as indicated by the confidence interval (columns 11 and 12) the true living wage impact may range from -0.10 to 0.19. Therefore, in the most conservative interpretation, this finding rules out direct disemployment effects larger than -0.10. Overall, my preferred specification is the propensity-score weighted results in

columns 9 as they effectively controls for structural differences between living 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, we plot estimates of β using a distributed lag structure of equation 1. Specifically, we calculate β coefficients on values of the living wage indicator variable three years prior to and three years after the year of passage. So if a given city passed a law in 1999 then $LW_{i,\, t(1999)} = 1$, then the three year lead variable $LW(t\text{-}3)_{i,t(1996)} = 1$ in 1996. Similarly, the three 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 dataset 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, that 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 flat, but in centered on zero. Similarly, the timepath 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

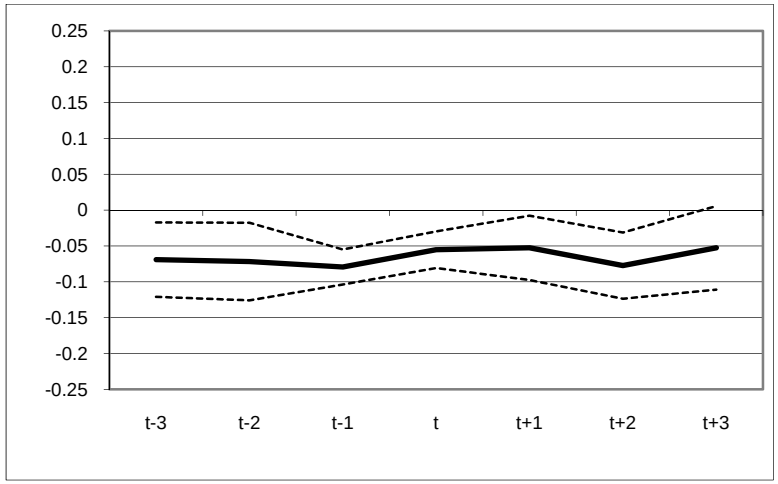
propensity to pass a living wage law, the trend line keeps the same shape but is shifted vertically. The trend in the living wage coefficients for low-wage service sector is actually positive in the post-passage period, which indicates that a pronounced negative impact is unlikely.

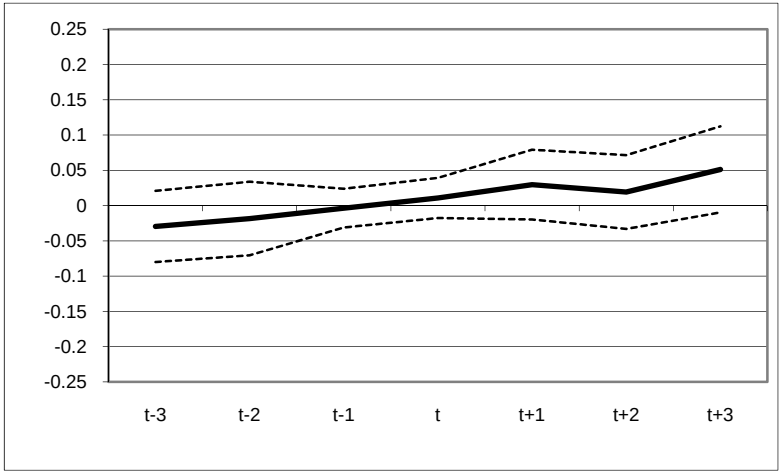
Figure 2 Time-path of living wage employment effects, unweighted and weighted samples.

ALL CALIFORNIA CITIES (UNWEIGHTED)

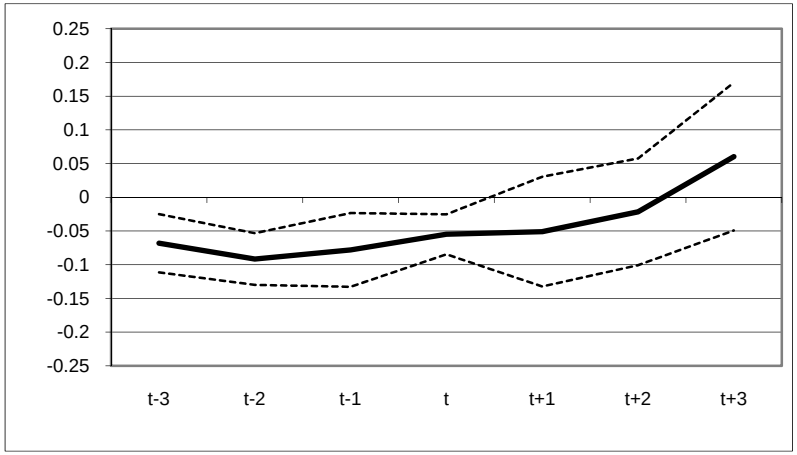
ALL CALIFORNIA CITIES (PROPENSITY WEIGHTED)

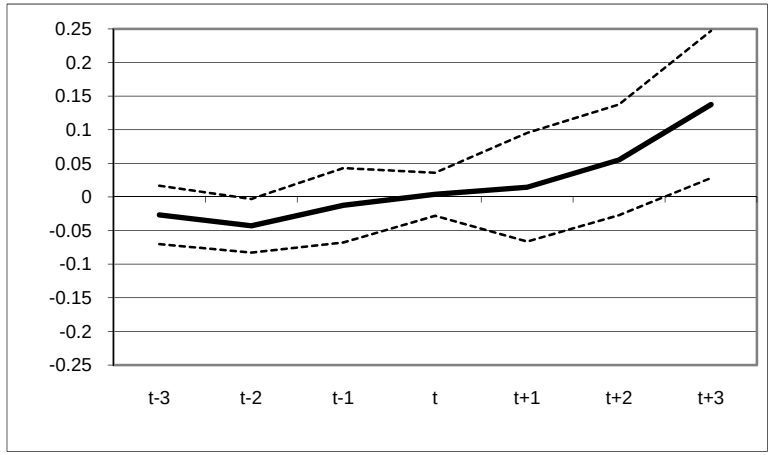
A. TOTAL EMPLOYMENT





A. LOW-WAGE SERVICE SECTORS





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, I also asses whether passing such laws harms a city's business climate by inducing non-covered 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 3.

Table 3 Indirect impact of living wage laws on employment and the number of establishments in California cities.

	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	
Industry/Subset	**Estimate**	**SE**	**LB**	**UB**	**Estimate**	**SE**	**LB**	**UB**	**Estimate**	**SE**	**LB**	**UB**
Ln Employment												
Headquarters	0.012	(0.031)	-0.040	0.065	0.012	(0.026)	-0.032	0.056	0.025	(0.025)	-0.017	0.068
Retail	0.000	(0.015)	-0.025	0.025	-0.020	(0.015)	-0.044	0.005	-0.014	(0.013)	-0.035	0.008
Restaurants	0.023	(0.024)	-0.017	0.063	-0.022	(0.018)	-0.052	0.008	-0.003	(0.019)	-0.034	0.028
Ln Establishments												
Headquarters	-0.018	(0.013)	-0.039	0.004	0.019	(0.011)	0.000	0.037	0.022*	(0.011)	0.004	0.039
Retail	0.009	(0.013)	-0.013	0.031	0.001	(0.009)	-0.014	0.017	-0.006	(0.009)	-0.022	0.009
Restaurants	0.009	(0.020)	-0.026	0.043	-0.041*	(0.016)	-0.067	-0.014	-0.028*	(0.016)	-0.055	-0.001
ln City Share of MSA Employment												
Headquarters	0.017	(0.033)	-0.038	0.072	0.029	(0.032)	-0.025	0.083	0.040	(0.028)	-0.007	0.088
Retail	0.023	(0.015)	-0.002	0.049	0.004	(0.013)	-0.018	0.026	-0.003	(0.013)	-0.024	0.018
Restaurants	0.023	(0.023)	-0.016	0.062	-0.002	(0.017)	-0.030	0.026	-0.007	(0.018)	-0.036	0.023
ln City Share of MSA Establishments												
Headquarters	-0.013	(0.020)	-0.046	0.021	0.078*	(0.023)	0.039	0.117	0.053*	(0.018)	0.023	0.084
Retail	0.023	(0.011)	0.004	0.042	0.012	(0.009)	-0.002	0.027	0.003	(0.009)	-0.011	0.018
Restaurants	0.020	(0.019)	-0.011	0.051	-0.012	(0.015)	-0.037	0.013	-0.016	(0.015)	-0.041	0.008
N=		(304, 304, 304)				(14176, 15664, 13280)				(14157, 15580 13226)		
No of jurisdictions in sample		19				886				886		

Notes: 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 (SIC 58). The category called headquarters is the sum of employment or count of records that are coded by D&B as headquarters that have at least one "child" record associated with their DUNS 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% Confidence intervals in columns 3 and 4.

As indicated in Table 3, 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. When I focus 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 0.012 to 0.025, 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 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 3).

While the retail and restaurant industries were included in the summary category of "all low-wage services" analyzed in the direct analysis, I break out these two industries in the indirect section because, (1) they are the largest employers of low-wage workers in most cities, and (2) 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 3 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 (0.009 to -0.006). For restaurants, however, I find a small negative and significant effect on the number of establishments when comparing living wage cities to all other cities in California (-0.041). This effect is reduced to -0.028 when the sample is adjusted using the propensity score, yet it is still barely significant at the 10 percent level. For both of these sectors however, I find no effect on the city's share of employment or establishment growth at the metropolitan level.

7. Conclusion

Ultimately, the findings of this paper confirm the general conclusions of individual case evaluations such as Fairris (2005) and Reich, Hall and Jacobs (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 upon 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—I find slight positive effects associated with passing living wage laws. However, due to the large standard error, I 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.

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 redistributional policy agenda in an era of federal retrenchment and economic globalization. This paper suggests that cities have more latitude in their ability to counter the dominant 'business-climate'/neoliberal policy 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, my findings based on a sample of cities in California indicate that the equity-growth tradeoff is not a hard and

fast rule. This research does not imply that all cities have the ability to regulate their way back to the labor market outcomes of the Golden Age (1940s-1970s). It simply underscores that the process is more complicated than is suggested by various viewpoints in the theoretical literature.

APPENDIX A- List of Living Wage Cities in California

City Name	Mandated Living-wage	Living-wage w/o Health Benefits	Date of passage
Berkeley CA and Marina	$ 11.39	$ 13.28	June, 2000
Fairfax CA	$ 13.00	$ 14.75	August, 2002
Hayward CA	$ 9.71	$ 11.20	April, 1999
Los Angeles CA	$ 9.39	$ 10.64	March, 1997
Oakland CA	$ 10.07	$ 11.58	March, 1998
Oxnard CA	$ 12.88	$ 12.88	July, 2002
Pasadena CA	$ 9.16	$ 10.73	September, 1998
Port Hueneme CA	$ 9.00	$ 11.50	October, 2003
Richmond CA	$ 11.42	$ 12.92	October, 2001
Sacramento CA	$ 9.67	$ 11.17	December, 2003
San Fernando CA	$ 8.50		April, 2000
San Francisco CA	$ 10.77	$ 10.77	August, 2000
San Jose CA	$ 12.27	$ 13.52	November, 1998
Santa Barbara CA*	$ 12.00	$ 14.00	March, 2006
Santa Cruz CA	$ 12.43	$ 13.56	October, 2000
Santa Monica CA*	$ 12.10	$ 12.10	March, 2005
Sebastopol CA	$ 11.70	$ 13.20	December, 2003
Sonoma CA	$ 11.70	$ 13.20	July, 2004
Ventura CA	$ 9.75	$ 12.50	May, 2006
Watsonville CA	$ 12.43	$ 13.56	September, 2002
West Hollywood CA	$ 8.67	$ 9.92	October, 1997

*****not used in empirical analysis because law went into effect after the end of the sample period (2005).
Source: Living Wage Resource Center, Association of Community Organizations for Reform Now (ACORN). Accessed March, 2008.

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