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Creating Good Jobs in Our Communities: How Higher Wage Standards Affect Economic Development and Employment

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How Higher Wage Standards Affect Economic Development and Employment

T. William Lester and Ken Jacobs November 2010



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How Higher Wage Standards Affect Economic Development and Employment

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November 2010

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Executive summary

From sports arenas to high-tech manufacturing zones and from commercial office buildings to big-box retail, local governments spend billions of dollars every year to entice private businesses to invest in their communities and create jobs. Yet these public funds often help create jobs that pay poverty-level wages with no basic benefits.

Cities across the country are working to gain greater control over these projects and help create quality jobs by attaching wage standards to their economic development subsidies. Communities are linking labor standards to public development projects in various ways, including community benefits agreements and prevailing wage laws. But the most common and comprehensive policies are business assistance living wage laws, which require businesses receiving public subsidies to pay workers wages above the poverty level.

These economic development wage standards have successfully raised pay for covered workers. Yet opponents of these standards argue that such laws prevent businesses from creating jobs and thus help some workers at the expense of employing more workers. Some business leaders and developers also claim that adding labor standards to economic development projects will scare away potential investors by sending an “antibusiness” signal.

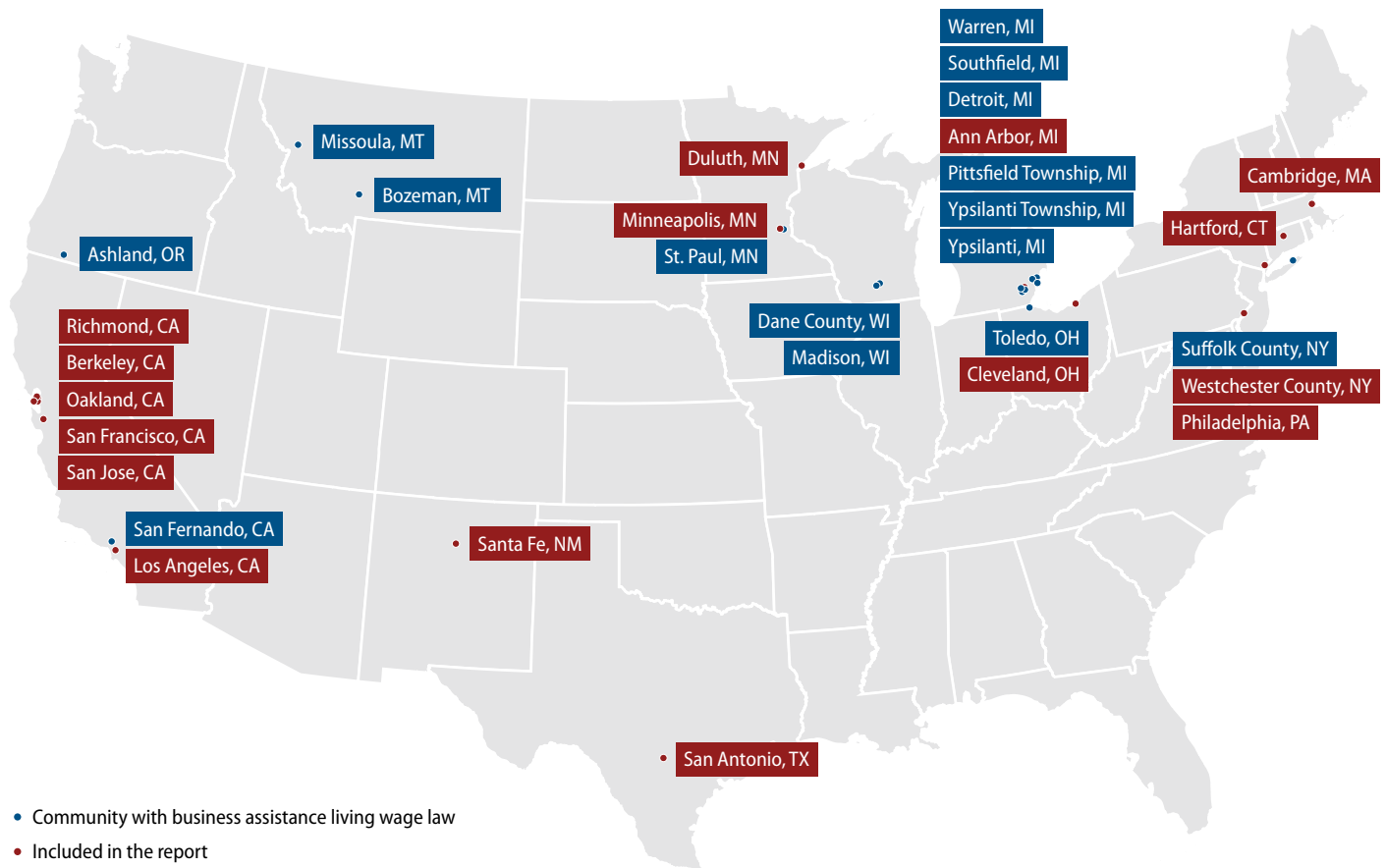
This report examines these claims and finds that economic development wage standards have no negative effect on citywide employment levels. This casts serious doubt on arguments that standards dampen municipalities’ ability to use subsidies to attract new businesses or create negative business climates where all firms avoid investment.

The study finds that the 15 cities effectively implementing business assistance living wage laws—Ann Arbor, Berkeley, Cambridge, Cleveland, Duluth, Hartford, Los Angeles, Minneapolis, Oakland, Philadelphia, Richmond, San Antonio, San Francisco, San Jose, and Santa Fe—had the same levels of employment growth

overall as a comparable group of control cities. The study also finds that these laws do not harm low-wage workers. Employment in the low-wage industries most likely affected by the living wage laws was unaffected by the change.

The study is the most methodologically sound, quantitative study conducted to date on business assistance wage standards. It uses the best available data that tracks employment by establishment and establishment movements over time in order to make accurate accounts of employment change at the city level. The study carefully selects cities that have effectively implemented business assistance living wage laws and ensures a controlled comparison that minimizes the effects of

Communities with business assistance living wage laws



unobservable variables by comparing 15 living wage cities to 16 cities with similar attributes where advocates lodged unsuccessful campaigns to pass such ordinances.

This study provides a strong test of the economic impact of wage standards because business assistance living wage laws are the type of economic development wage standard likely to have the most widespread effect on employment. Other types of economic development wage standards, such as community benefits agreements and prevailing wage laws, either affect far fewer projects or are more closely tied to market wages, and are thus even less likely to have any effect on employment.

This report—like the groundbreaking studies that established that minimum wage laws do not kill jobs as opponents maintained—brings academically sound, empirical research to bear on a debate that for too long has been relatively uninformed by quality, comparative evidence on the laws' actual effects.

The evidence demonstrates that raising job standards does not reduce the number of jobs in a city. This means that job growth does not have to come at the expense of job quality. Local government leaders can therefore ensure that taxpayer dollars do not subsidize poverty wages by supporting economic development wage standards and feel confident that their local business climate will not be affected.

Introduction

State and local leaders enact a wide variety of economic development policies to encourage private businesses to locate, invest, and ultimately create jobs for local residents. This business attraction model is exemplified by policies—such as direct subsidies, tax exemptions, and targeted infrastructure improvements—that allocate public funds to private businesses or developers. Conservative estimates indicate that state and local governments spend more than \$50 billion every year on this type of activity.¹ The logic behind such policies stems from the idea that businesses are relatively mobile and may choose to relocate or expand in low-cost areas. Yet these publicly funded projects have sometimes resulted in jobs that pay low wages and provide no benefits.

Stark increases in overall labor market inequality have led some policymakers and labor advocates to challenge the dominant business attraction strategy. Data from the past two decades suggests there is a fractured link between employment growth and raising local citizens' overall well-being. Many now view chasing jobs at all costs to be a questionable policy.

Even during the job-rich growth of the 1990s a significant portion of new jobs paid low wages and typically lacked benefits and career ladders. This trend continued in the 2000s and has led to falling real wages for most workers, increases in working poverty, and rising income inequality. Average wage growth for the bottom 80 percent of workers grew by only 0.6 percent between 2001 and 2007 while wages for those in the top quintile rose by 5.3 percent.²

Labor advocates, religious and community leaders, and elected officials have pushed for and passed local wage standard ordinances to address the problem of declining job quality. The push to link labor standards to public development projects has occurred through various forms, including community benefits agreements as well as prevailing wage and living wage laws.

A community benefits agreement is a project-based contract signed by community groups and a developer that requires the employers participating in the project to adhere to a negotiated set of wage standards and provide specific amenities on a particular project. CBAs are a growing phenomenon but so far have only affected a relatively small number of completed projects.

Prevailing wage laws require that covered businesses pay their employees wages at or above the typical wages in a certain industry, and thus not undercut the existing market wage structure. Prevailing wage laws have been used frequently on government contracts but only very recently have begun to be applied to a broad range of jobs created by government-supported economic development.

The most common and comprehensive economic development wage standards are business assistance living wage laws, which require businesses participating in projects receiving public subsidies to pay workers wages above the poverty level.

The living wage movement began in Baltimore in 1994 and more than 140 local jurisdictions now have some form of living wage law. The movement originally focused on ensuring that government contractors did not pay poverty wages but evolved into a broader set of urban policies that presented a clear alternative to the business attraction model of economic development. Living wage advocates in some cities have extended the basic form of living wage law to firms that receive public dollars through economic development subsidies.

These “business assistance” living wage laws directly challenge the logic of local economic development policies by placing additional requirements on firms that engage in development agreements with the public sector. Some business leaders and politicians have criticized wage standards for raising the cost of doing business. These opponents claim that raising wages would lead to job losses since employers would walk away from development deals. They also often identify economic development wage standards as an “antibusiness” signal to other firms who may not receive local subsidies but would nonetheless choose not to locate in the city.³

What is certain is that economic development wage standards in large U.S. cities continue to be highly controversial. The debate over Chicago’s proposed “big-box” living wage law in 2006, for example, drew national media attention and resulted in Mayor Richard M. Daley’s first-ever veto. New York City is currently debating whether to adopt a citywide economic development wage standard and Pittsburgh recently extended a prevailing wage law to cover workers at firms that receive financial assistance. The current debates are critical at this time, not only

because several major cities are considering business assistance living wage laws but also because the current economic crisis—with its near double-digit unemployment—increases the pressure on elected officials to increase the number of jobs, regardless of their quality.⁴

Given the public’s desire for both creating jobs and raising the quality of jobs, this report assesses the question of whether or not business assistance living wage laws reduce jobs and economic development activity in the cities that choose to pass them.

We examine business assistance living wage laws because they are the most widespread form of economic development wage standards, which means they provide a large enough sample of cities and affected employers to allow for rigorous quantitative analysis. They also allow for more consistent comparison across cities than community benefits agreements, which tend to be unique to each deal. And living wages have been subject to previous academic study, providing a useful basis of comparison.

This study provides a hard test of the economic impact of wage standards because business assistance living wage laws are the type of economic development wage standard likely to have the most widespread effect on employment. Other types of economic development wage standards, such as community benefits agreements and prevailing wage laws, either affect far fewer projects or are more closely tied to market wages and are thus likely to have less influence on employment or business climate.

This study uses a unique, private-sector database that contains an extensive time series of observations from 1990 to 2008 to make detailed before and after calculations of how living wage laws change employment and total business establishments at the city level. We estimate these changes among a set of 31 large and economically diverse urban jurisdictions by comparing outcomes in cities that have passed (and enforce) business assistance provisions to those that attempted, but failed to pass such provisions. This research design—adopted by other living wage researchers and used widely in labor economics and policy analysis—has the benefit of controlling for underlying institutional and structural differences between cities with and without business assistance living wage laws that have the potential to confound results.

The study considers the broad set of industries and firm types most likely to be covered by business assistance living wage laws and finds no evidence that such laws reduce employment or business growth over the short or long term.

How business assistance living wage standards may affect urban economic development

Many living wage proponents argue that business assistance clauses will not cause significant job losses. Research indicates that higher minimum and living wages lead to efficiency gains for firms through reduced turnover.⁵ Increasing wages for the lowest-paid workers also stimulates local economies, as low-income households typically spend more of their dollars locally.

In addition, some researchers point out that business assistance living wage laws typically apply to only a small number of firms that receive direct subsidies, and only a fraction of these firms employ workers below the mandated wage. Business assistance laws function from this perspective as a lower bound that serves to prevent localities from subsidizing low-wage jobs, but don't represent a drastic reshaping of existing local labor practices and thus could not have a significant effect on employment.

Living wage opponents suggest, on the other hand, that these laws could “kill deals.” If local governments force subsidized businesses to increase wages above the level usually offered, these firms will choose not to enter into development agreements, leading to the loss of all the jobs, not just the low-wage positions. Another argument is that, even if deals do move forward, employers would cut back on staffing levels or substitute toward higher-wage, higher-skilled labor, resulting in fewer people employed. Still another argument is that these laws create a poor business climate. Opponents predict fewer jobs created overall if government enacts business assistance provisions.

These competing interpretations and multiple paths of causation make it important to distinguish the myriad ways that business assistance living wage laws could affect urban economies. We divide such potential effects into three groups—direct, direct spillover, and indirect effects—each of which may have one or more potential consequences.

Direct effects

Wage standards directly affect a narrow set of establishments that participate in subsidized development projects and operate businesses that hire a significant number of low-wage workers. Examples of “directly affected” businesses include retailers or food service operators that are part of a larger, publicly subsidized urban redevelopment project that is subject to the living wage requirement—such as Los Angeles’s Staples Center sports arena development. Workers at low-wage assembly plants or back-office processing centers that are often targets for local economic development incentives may also receive direct wage increases as a result of the law.⁶ Measuring only these direct consequences is nearly impossible through quantitative analysis because of the limited number of deals affected by such laws and the type of data available.

Direct spillover effects

Other low-wage employers may experience changes from higher wage rates through a direct spillover effect.⁷ Such direct spillover effects accrue if the mandated wage increases at covered firms result in an overall increase in wage standards in the local economy that forces other low-wage employers to raise wages as a competitive response.⁸ The textbook, neoclassical economic viewpoint explains that this increase in wages would result in a reduced labor demand. This is the same theoretical interpretation that some researchers apply to the minimum wage debates.

Indirect effects

Living wage laws may also indirectly affect the overall level of economic development activity in a city. The passage of business assistance provisions may send a strong antibusiness signal to employers seeking to locate in the enacting city or existing businesses considering local expansion. Some researchers argue that the indirect effect of living wage laws—particularly business assistance provisions since they theoretically could affect a much broader set of firms than contractor-only laws—may actually outweigh any observable direct effect on the local business climate.⁹ Even firms that may not seek economic development subsidies, but nonetheless hire a significant portion of low-wage workers, may view a strong living wage law as a proxy for broader political shifts at the local

scale toward a more pro-labor stance and therefore reduce their investments in the local jurisdiction.

Business assistance provisions may also shift the practice of economic development policymaking itself and thereby reduce aggregate employment levels. If business assistance clauses are effective in acting as a minimum standard, or floor, for the type of job quality expected from economic development incentive projects, then city officials may shift their business attraction strategy away from industries that provide a larger number of lower-paid positions to sectors that pay higher wages such as manufacturing, research and development, or biotechnology. The number of economic development “deals” may decline because the chances of landing such high-value targets are lower, and higher wage industries may require fewer workers due to high productivity. Labor advocates who oppose public subsidies for low-wage industries may laud these indirect effects but policy changes may end up reducing the total number of jobs created through economic development programs.

Previous studies on wage standards

The empirical literature on how living wage ordinances affect employment primarily focuses on detecting the direct and direct spillover effects and only rarely distinguishes whether the laws explicitly apply to business assistance provisions. Two types of studies characterize this literature: individual case studies of single cities before and after passage of a given law—which tend to find no employment effect—and quantitative studies from a group of living wage and nonliving wage cities over a period of time—which tend to find significant negative consequences. The living wage literature mirrors the tension between case studies and panel studies in the broader economics literature on federal and state minimum wage changes.¹⁰

One of the earliest detailed case studies on Baltimore’s landmark 1994 contractor-only living wage law found that the living wage did not significantly increase contract costs and that employment remained the same at covered firms.¹¹ Yet this study did not compare employment changes at covered firms to a control group. Researchers in a study of Los Angeles conducted two independent surveys of firms and workers that were covered and uncovered by the city’s ordinance that applied to city contractors.¹² The study found that wages in covered firms increased while turnover and absenteeism dropped relative to the control group, and there was no significant difference in employment levels.¹³ Another case study showed that San Francisco’s living wage law that applied to workers at the SFO International Airport resulted in direct wage increases for nearly 10,000 workers but had no discernable effect on employment.⁴

These empirical case studies do not focus explicitly on business assistance provisions but they provide valuable insights into the laws’ potential effects. And studies of city-level minimum wage provisions provide a further sense of the likely impact of living wage laws that extend beyond city contractors. Minimum wage ordinances cover all private-sector establishments, not just those that receive financial aid from the city. A study of Santa Fe’s minimum wage law in 2003 found

only marginal cost increases for businesses and no significant effect on employment.¹⁵ In San Francisco and Alameda County, researchers surveyed restaurants before and after San Francisco's citywide minimum wage took effect in 2004.¹⁶ They found a significant wage increase, a reduction in labor turnover, and no negative affect on employment.

Living wage case studies have the benefit of clearly identifying covered firms and therefore accurately measuring direct effects but the results of studies that compare a single case to a control group don't allow us to generalize about the greater effects of living wage ordinances. Research designs that use observations from all or many living wage cities and make comparisons across a large number of controls generally have greater external validity—that is, they are more validly generalizable to other communities.

David Neumark, an economist at the University of California, Irvine, who is frequently cited by opponents of living wage laws, examines how state minimum wage increases and city living wage laws affect wages, employment, and poverty rates using a panel of large cities that passed ordinances between 1996 and 2002.¹⁷ Based on data from the Current Population Survey, Neumark's research finds large wage increases and reductions in family poverty associated with the timing of living wage laws. But it also finds significant disemployment effects for younger, lower-skilled workers.

Robert Pollin, economics professor at the University of Massachusetts, Amherst, and head of the university's Political Economics Research Institute, and his colleagues at PERI, Jeannette Wicks-Lim and Mark Brenner, who have extensively studied living wage laws, critique Neumark's wage results as being vastly overstated given the fact that most living wage laws cover only a small fraction of workers, and that his dataset only identifies metropolitan areas rather than individual cities and weights Los Angeles too heavily.¹⁸ The drawback of using broad household surveys, such as the CPS, is that there are too few cases to accurately distinguish "covered" and "uncovered" workers. Neumark cannot specifically identify a worker employed at a firm covered by the living wage.¹⁹ Neumark also restricts his analysis to the 1996-2002 period due to data constraints, which is a relatively short time period during an economic expansion.

Summary of previous living wage studies

Individual case studies: This research has found no negative employment effects. Studies have been successful at identifying covered firms but are viewed by some as less generalizable and have not explicitly addressed business assistance laws.

Multiple-case, panel studies: This research has generally, although not always, found that living wage standards reduce employment and that business assistance laws are more harmful than contractor-only laws. Yet they are weak at identifying covered firms and most have not used appropriate datasets for examining cities.

Scott Adams, an economist at the University of Wisconsin, Milwaukee, and David Neumark more recently compare low-wage workers' income and employment levels in cities that passed living wage laws and cities that had a failed living wage campaign.²⁰ Using the failed cases as a control sample to attempt to hold constant the local political or institutional factors that fuel living wage campaigns—such as union density—may also affect the outcome variables such as employment. The study finds a statistically significant negative employment effect for lower-skilled workers—but only for those cities that have business assistance provisions, which they argue have the potential to affect most, if not all, low-wage workers in a given city. This is the only quantitative study that distinguishes results for business assistance living wage laws.

Brenner, Wicks-Lim, and Pollin, the University of Massachusetts economists, and others suggest that the latest Adams and Neumark living wage study is also deeply flawed.²¹ These authors argue that business assistance laws only directly affect a small fraction of workers in each city with a living wage ordinance. They also argue that using the CPS to identify city-level effects is highly problematic due to small sample sizes at the urban scale and the inaccurate assumption that policy changes at the city level will affect workers throughout a metropolitan area.

T. William Lester, a University of North Carolina professor and co-author of this study, seeks to address these data-quality concerns by using the National Establishment Time Series—the same dataset used for this study—to measure how living wage laws affect employment and the number of business establishments in California. The findings contradict Adams and Neumark, although there were too few cases to parse effects for business assistance from contractor-only living wage laws.²² The study concludes that living wage laws had no negative impact on government contractors or low-wage service industries that might be indirectly affected by the living wage.

Panel studies of business assistance living wage laws are also criticized for treating all laws equally. Brenner, Wicks-Lim, and Pollin argue that governments have, in some case, only applied the standards to a very small number of firms, which could not produce a direct impact that is measurable by data sources like the CPS.²³ There is wide variation in the degree to which living wage laws are enforced at the local level.²⁴ Economic development officials have simply ignored business assistance provisions in some cases. And negotiations ahead of passage significantly watered down the measure in other cases such that incentive thresholds were set so high that no firms were likely to be covered upon passage.²⁵

There is a tension in the empirical literature on living wage effects overall. Panel studies of the type applied by Adams and Neumark, which include all or a large sample of living wage cities, are preferable generally to comparing employment before and after passage within a single city.²⁶ Previous panel studies—with the exception of Lester—find a negative impact on employment, though this research has generally used inappropriate data and failed to properly select cities to study.²⁷ Individual case studies, including studies with detailed surveys, generally find no disemployment effect and make a more convincing case for measuring outcomes among firms and workers who are covered by the living wage.²⁸ But their limited scope makes it difficult to generalize the findings more broadly.

The research design proposed in the following section combines the best of both approaches in the literature. We conduct a front-end qualitative assessment of nearly all the business assistance living wage laws in the United States to construct an accurate treatment group consisting of large urban areas that have living wage laws that are binding and/or likely to be enforced. We then conduct a time-series quantitative analysis to estimate a generalizable assessment of how business assistance living wage laws will affect urban economic development. And we use a more appropriate dataset than previous research.

Research design and case selection methodology

Conceptual approach

The simplest way of measuring the effect of business assistance living wage laws is to gather information on the total number of jobs and business establishments for jurisdictions that have business assistance requirements for several years before and after each law went into effect. Yet this simple direct comparison is extremely limited due to the problem of “endogeneity”—the fact that cities that choose to pass business assistance living wage laws may experience other trends that are correlated with employment changes. Cities could be growing slower or faster as a group due to long-term trends such as deindustrialization or suburbanization, for example, masking the true effect of business assistance requirements.

To overcome the endogeneity problem, we need to identify an appropriate control group of cities without business assistance living wage laws to compare to our treatment group. This group of nontreated cities would ideally control for all relevant factors that may influence employment or establishment growth. Short of randomization, economists often look for natural experiments to analyze policy changes.²⁹ The benefit of this type of research design is that it compares outcomes between treatment and control groups that are in all other respects very similar, except for the difference in the policy. The estimated effect of the policy is therefore unbiased. Adams and Neumark attempt to control for endogeneity by comparing living wage cities to cities that experienced living wage campaigns, but either failed to pass a living wage or had had their law vetoed or struck down by the courts.³⁰ They refer to their control group as “failed or de-railed campaigns.”

We adopt the same conceptual research design in this report as Adams and Neumark.³¹ We compare outcomes for a treatment group that includes 15 large, urban jurisdictions that have passed business assistance living wage laws to a comparably sized set of cities that failed to pass business assistance provisions. This choice of control group minimizes differences in unobservable, confounding variables because these cities have similar institutional settings with regard to labor

regulation—many of the cities either have basic, contractor-only living wage laws or have undergone significant campaigns to pass stronger business assistance provisions, but did not ultimately enact them.

We assume that the existence of a living wage law campaign indicates that control cities have a similar set of labor advocates and progressive actors that have raised the issue of a living wage in the political spectrum. Both treatment and control cities are drawn from the overall set of cities in the United States that have at least proposed a living wage law. This group of cities is significantly different than other local U.S. governments in that they tend to be larger, older cities located on the West Coast or in the industrialized Northeast and Midwest.

This design does a good job of controlling for confounding differences between the treatment and control groups but it does not rule out all possibility of endogeneity. We therefore test for structural differences between the treatment and control groups cities to ensure that they are truly comparable. We also add controls to allow for city-specific trends to further address concerns about endogeneity.

Case selection methodology

A key difference between our study and that of Adams and Neumark is our sample choice.³² We conduct a systematic qualitative assessment of the set of U.S. cities that have passed business assistance living wage laws to narrow down the treatment group to exclude where the living wage has not been enforced or thresholds are too high to have an effect.

The first step in our case selection methodology was to determine the universe of all local jurisdictions that have passed or considered living wage laws that apply to businesses receiving any sort of financial assistance, including tax abatements, grants, direct infrastructure improvements, or below-market loans. We determined this universe by searching databases maintained by the Employment Policies Institute and Living Wage Resource Center.³³ These websites contain basic information on the type of living wage passed, coverage thresholds, mandated wage levels, and date of passage. EmPI's website also contained listings for cities that rejected living wage laws either through a failed ballot initiative or

What makes this study better than previous research

Careful screening of treatment cities to exclude cases where business assistance laws have weak enforcement or significant loopholes.

Better data that captures only the city where the law applies and allows for analysis of the industries most likely to be affected by business assistance living wage laws.

council vote, a veto, or a repeal. This universe consisted of 50 cities, with 30 listed as successfully enacting a law and 20 as having failed living wage campaigns.

We excluded small cities with fewer than 60,000 people because small cities tend to engage in fewer economic development “deals” for which the living wage would apply, and because we wanted to focus on cities that would have significant employment volume given the high cost of acquiring NETS data.

We then undertook a deeper analysis of each city’s law to determine whether it should be assigned to the treatment group, the control group, or dropped from the study altogether. Our goal in this process was to take the critiques of Brenner, Wicks-Lim, and Pollin and others into account by ensuring that cities in the treatment group have laws that directly or indirectly affect the local economy.³⁴

Our analysis to ascertain the status and effectiveness of the laws and determine whether to exclude the city consisted of three components. First, we obtained written copies of each city’s ordinance through web searches of city legislation. Each ordinance typically lists the exact coverage threshold, the types of financial assistance that qualify under the law, and any exclusions or loopholes.

The second step in our analysis was to make phone calls to the cities that were indicated as having enacted a business assistance living wage ordinance to determine if the law had ever been enforced. We called city staff at the agency or department listed as responsible for enforcing the living wage or monitoring performance. We also called each city’s agency in charge of business attraction.

Our limited success in reaching knowledgeable staff led us to our third step. We scanned secondary sources including local newspaper listings and performance reports by local advocacy groups or foundations to look for direct evidence of an economic development incentive deal entered into with an employer where the living wage would apply. This allowed us to finalize a list of 15 treatment cities.

These cities have one or more of the following criteria: assistance level thresholds of \$1 million or less; direct evidence of enforcement from primary and/or secondary sources; and evidence of strong enforcement campaigns and ongoing organizing activity after passage of the living wage.

Finally, we began the process of selecting the control cities with the list of 20 cities in the EmPI database that rejected a living wage ordinance and narrowed the list of cities to 16 to produce a balanced sample.³⁵ We conducted similar research

steps on the proposed control cities as we did on the treatment cities to ensure that a law was not eventually passed after the most recent update to the EmPI database—as was the case with Philadelphia, Pennsylvania. And we dropped several cities to attempt to maintain a broad regional balance across the treatment and control samples. Only one treatment case came from Texas, for example, and we thus felt it was not necessary to have both Houston and Dallas in the control. The resulting list of treatment and control cities is listed in Table 1.

We took extensive efforts to ensure that our treatment and control cities are comparable but it is possible that they may still differ in important ways. Table 2 compares the average values for a variety of demographic and economic variables.

It is reassuring to see that there are no significant differences between the treatment and control samples for the pretreatment period annual employment growth rates.³⁶ Treatment cities grow only 0.2 percent slower than the control. The two groups are also quite similar in terms of poverty and unemployment rates and racial and ethnic composition.

The only areas for which the groups differ significantly are on measures of household income and housing costs. The group of living wage treatment cities has clearly experienced significant growth at the upper end of the income spectrum, which results in higher levels of income inequality. The only distributional variable—the proportion of a city’s households that earns above the 80th percentile nationally—bears this out. This upper income growth likely adds to housing pressure as measured by the significantly higher median rental rates and housing values. Income inequality seems to be higher in the treatment group but it is unclear that inequality itself would lead to lower job growth in the industries that are likely to be affected by living wage provisions.

Regional balance of the samples also explains some of these differences. We attempted to produce balance in selected control cases but we are still left with a treatment sample that is overweighted toward California (seven cases in the West). These differences are not enough to conclude that the samples are systematically biased but it does provide a motivation for including the type of city-specific trend controls discussed later.

TABLE 1
List of treatment and control cities

Treatment cities	Control cities
Ann Arbor, MI	Albuquerque, NM
Berkeley, CA	Chicago, IL
Cambridge, MA	Dallas, TX
Cleveland, OH	Durham, NC
Duluth, MN	Eugene, OR
Hartford, CT	Indianapolis, IN
Los Angeles, CA	Knoxville, TN
Minneapolis, MN	Lansing, MI
Oakland, CA	Nashville, TN
Philadelphia, PA	New York, NY
Richmond, CA	Omaha, NE
San Antonio, TX	Oxnard, CA
San Francisco, CA	Pittsburgh, PA
San Jose, CA	Providence, RI
Santa Fe, NM	South Bend, IN
	St. Louis, MO

Source: Author’s analysis.

TABLE 2
Comparative statistics between control and treatment group

Variable	Treatment group (mean)	Control group (mean)
Total population	665,149	1,000,709
% African American	18.8%	22.2%
% Hispanic	22.1%	17.4%
% Non-Hispanic White	45.3%	54.1%
% With BA or higher	34.9%	26.5%
% Foreign born	20.2%	13.5%
% Poverty	17.9%	17.8%
% Unemployed	7.5%	7.4%
Median household income	\$41,003	\$35,943
Median rent	\$700	\$578
Median housing value	\$203,460	\$111,131
% Of households in top US income quintile	21.0%	15.2%
% Employed in FIRE or professional/tech. services	20.1%	18.1%
% Employed in manufacturing	10.0%	11.6%
% Renters	52.4%	49.1%
Housing vacancy rate	5.2%	7.4%
Average annual growth rate 1990-1997	2.3%	2.5%
Average three-year growth rate 1990-1997	7.1%	6.6%
Total number of cities	15	16

Frequency by region

Northeast	3	3
Midwest	4	6
South	1	4
West	7	3

Source: US Census Data, 2000 obtained from the State of the Nation's Cities.

Database construction

Another key innovation in our research design is the primary data source used to measure the outcome variables. We use the National Establishment Time Series database as our primary data source to construct a city-level panel data set using annual observations.

Background on the National Establishment Time Series dataset

The NETS data is a proprietary database developed by Dr. Donald Walls of Walls and Associates in conjunction with the Dun and Bradstreet 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. NETS is different from the typical D&B files that are sold to business and credit issuing entities in that it is a longitudinal database created by taking 19 annual snapshots of the D&B file and linking establishments across years using a unique identifier assigned by Dun and Bradstreet. This identifier is called the DUNS number. NETS contains establishment-level data on employment; estimated sales; industry, as tracked by the eight-digit Standard Industrial Classification code; ownership structure; and address for 1990-2008. NETS tracks establishment moves over time, which allows us to accurately account of total employment in each local jurisdiction in each year.

NETS is unlike household surveys such as the Current Population Survey in that it 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.³⁷

The NETS database does a reasonably good job in capturing the level of economic activity and in measuring employment levels. A careful academic review of the NETS file argues that D&B has “an economic incentive” to ensure that its infor-

mation is up-to-date and accurate, and that it covers all existing establishments.³⁸ It is valuable to use NETS for a study of the living wage because it offers consistent long-term information on employment and the number of establishments at the local level rather than the county, metropolitan, or state level. NETS also offers detailed industry information on each record, which allows us to focus on the specific low-wage industry groups that are most likely to be affected by business assistance provisions, but also measure industries that are often targets for local business attraction strategies even if they are not low-wage industries in particular.

Using the National Establishment Time Series

The first limiting step in our analysis was on establishment size. We only use NETS records for establishments that had more than four employees at any point in their life cycle between 1990 and 2008. This limiting step was done to reduce the cost of our data purchase and to maintain comparability with other data sources. This limiting step is not likely to have a major effect on this research since very small firms do not typically receive local financial assistance, and they make up a small portion of overall employment in each city.³⁹

NETS is a dynamic database in that it tracks each establishment's location overtime. Most establishments do not move but approximately 14 percent of the NETS records in our sample have changed location at some point in time. The address information listed in the NETS is only for a firm's current location, so if a given establishment started in New York in 1994 but moved to Boston in 2000, its current geographical identifiers would reflect a location in Boston. But we would want to count this firm in New York in order to make an accurate employment total for New York in 1994-2000. We are able to overcome this problem since NETS contains information not only on current geographic location but also on the origin, time, and destination of each establishment move. We build our city-level database by combining the information on the origin zip code and current zip code of each establishment to construct a set of variables that track the zip code location of each establishment in each year.⁴⁰

Once each establishment was assigned to a city for each year that it was in existence, we then aggregated the NETS database to the city level by summing employment and the number of establishments in each city for various industry sectors and firm types of interest to our analysis.

Measuring employment and establishments

The primary objective of this report is to test for the various ways a business assistance living wage law could affect fundamental measures of economic activity in the cities that choose to pass them. The richness of the NETS database enabled us to produce outcome variables to test the hypothesis that business assistance provisions reduce jobs through direct, direct spillover, and indirect means. We calculate employment and establishment count variables for 14 separate industry sectors and firm types, organized into three broad categories. The first category, which we argue best approximates the set of employers most likely to be affected by the living wage through direct or direct spillover effects, consists of low-wage service sector industries. We calculate five outcome variables for this category: broad low-wage services; narrow low-wage services; retail; restaurants; and hotels.

The first variable in this category, broad low-wage services, captures a broad set of low-wage industries likely to be affected by large-scale urban redevelopment projects. However, this variable is of a broad cross section of industries and may be combining some higher-wage industries with low-wage ones. We therefore also break down this variable using the more refined industry data to produce a variable that captures only the low-wage industries from within the broad category, such as building security and parking services. We furthermore include the three largest employers of low-wage workers in most urban economies: retail; restaurants; and hotels. These industries are often targets for local business attraction and urban redevelopment projects and represent the group of employers who are potentially most affected by direct spillover effects of higher wages.

The second major category of outcome variables comprise what we term “common economic development targets,” which can be thought of as capturing both direct and indirect effects. Workers in this category are not necessarily low-wage but it includes those industries that are often targets of business attraction efforts. We define employment and business establishment totals by city for the following groups: manufacturing; nondurable manufacturing; back-office; wholesale; big-box retail; and finance insurance and real estate.

Most U.S. cities have experienced some form of deindustrialization and industrial job losses, and manufacturing establishments have long been the target of local economic development initiatives. Nondurable manufacturing industries tend to be less capital intensive and less unionized, and therefore have the potential to pay lower wages. At least some portion of this sector may be affected by the

living wage mandate. Economic development deals have also focused on the highly mobile back office activities of corporate services such as call centers and credit processing services, and so we construct an outcome variable that attempts to capture this activity. Wholesale distribution centers are also targets for local economic development, especially for jurisdictions that have former industrial land in need of redevelopment or that have large, undeveloped tracts. We include so-called big-box retail stores because they are often targets of local development deals, especially in jurisdictions heavily dependent on local sales taxes. Finally, we include finance insurance and real estate as an additional test because we would not expect that this high-wage industry would be affected by a living wage law.

We also generate two outcome variables that are defined by an establishment's place in the firm structure. We measure establishments that are the headquarters of a firm that has at least two other establishments at different locations—as well as branch plants, which are nonheadquarter establishments in firms with multiple establishments. These two variables do not include single location firms or small businesses. We characterize these establishment types as those that may be more susceptible to the indirect or signaling effects described above. Decisions about where to locate them are based to some degree on the region's business climate, and they typically provide jobs above the living wage threshold although they are somewhat less likely to be targets for development subsidies.

We also provide results for total private-sector employment and establishments as a summary measure.

Using the database to measure the effects of wage standards

We use our panel data set to measure how passing a business assistance living wage law affects a city's level of employment and its total number of establishments. These are the basic outcomes of economic development. We conduct this analysis by using a panel regression model that is now standard in the empirical literature on the economics of minimum and living wage increases.

The first step is to gather data on the timing of each treatment city's passage of a business assistance living wage. We measure changes in employment in the years after passage relative to changes in employment in the years leading up to passage. We then compare this difference in employment change to the same employment changes in the control sample. This technique is referred to as “difference-in-differences.”

We also use statistical techniques to control for confounding factors such as the fact that different cities passed laws at different times and that there are significant differences between the cities in terms of economic structure, historical growth patterns, size, and demographics. Our model includes a control for population size based on the Census' annual estimates as well as controls for each year in the panel. It also includes dummy variables for each year in the controls for macro-economic effects that are common to all cities in the analysis. The U.S. economy was in recession in 2001, for example, and most local economies experienced job losses. Failing to control for such effects could lead us to erroneously conclude that living wage laws passed in 2000 resulted in significant job losses, which were in fact caused by a cyclical trend that was unrelated to passage.

We also include controls for each city itself and controls for city-specific linear trends. We include these controls to adjust for any idiosyncratic differences between the cities both within the treatment sample itself and between the treatment and control samples. For instance, Santa Fe is included in the treatment group based on its passage of a citywide minimum wage that includes all firms. Yet the entire Southwest region of the United States grew at a faster rate than other areas of the country for the full panel period of 1990-2008. Failing to control for these regional differences in growth could lead us to understate the living wage effects.

The study finally measures the effect of passing a business assistance living wage law over a four-year period, including estimates for two years prior and two years after passage. This allows us to control for any prepassage spike or fall in the outcome variable and also allows us to examine if the living wage has any delayed effect. If the impact on a city's business climate is real, it may take several years to have a detectable influence of overall employment or employment in a specific low-wage industry.

Main findings

Employment effects

The study examines how living wage standards affect 14 distinct employment variables: total citywide; broad low-wage services; narrow low-wage services; retail; restaurants; hotels; manufacturing; nondurable manufacturing; back-office; wholesale; big-box retail; finance insurance and real estate; headquarters; and branch plants.

Together these provide a comprehensive examination of the potential combined direct, direct spillover, and indirect effects that business assistance living wage laws can have on local employment. Figure 1 presents these 14 variables as the possible range of employment change expected after passage, allowing up to two years for lagged effects. None of the 14 outcome variables show a statistically significant negative consequence of passing a business assistance living wage standard. Statistically significant outcomes would mean that we are 90 percent confident that the estimate is different from zero. But this is not the case for any of the variables, which means we can conclude that there is no employment effect. (More detailed results are presented in the technical appendix.)⁴¹

Our estimates indicate that passage of a business assistance living wage law has no measurable effect on citywide employment. Employment levels are unaffected in low-wage industries as is employment in industries likely to be targets of economic development subsidies and in firms that are sensitive to the perceived business climate of a city. This suggests that business assistance living wage laws are unlikely to have direct, direct spillover, or indirect effects on employment levels. These findings discredit the primary arguments used by opponents of business assistance living wage laws that these laws are harmful to employment in direct and indirect ways.

It is important to note that the results are based on nearly 20 years of data—a timeframe that contained years of recessions and expansions—which suggests that business assistance living wage laws are unlikely to have an effect on employment levels even during hard economic times.

These results are also quite robust. For example, the inclusion or exclusion of any particular city from the treatment group has no meaningful effect on the results.

Figure 1 visually represents the 90 percent confidence interval of our point estimates. Any number line in Figure 1 that includes zero in the shaded area indicates that the estimated effect is not different than zero. This means that there is no estimated employment effect, which is the case for all the variables tested. The estimated impact of employment in low-wage industries—the sectors where we can expect the living wage to have the largest bite—bears some additional discussion. Our estimates for the five low-wage sectors we measure are all nearly zero, or slightly positive. These results strongly contrast with the findings of Adams and Neumark, who find significant negative employment effects for low-wage workers overall. For retail and restaurants our estimates are precise enough to reject the point estimates of their study.⁴²

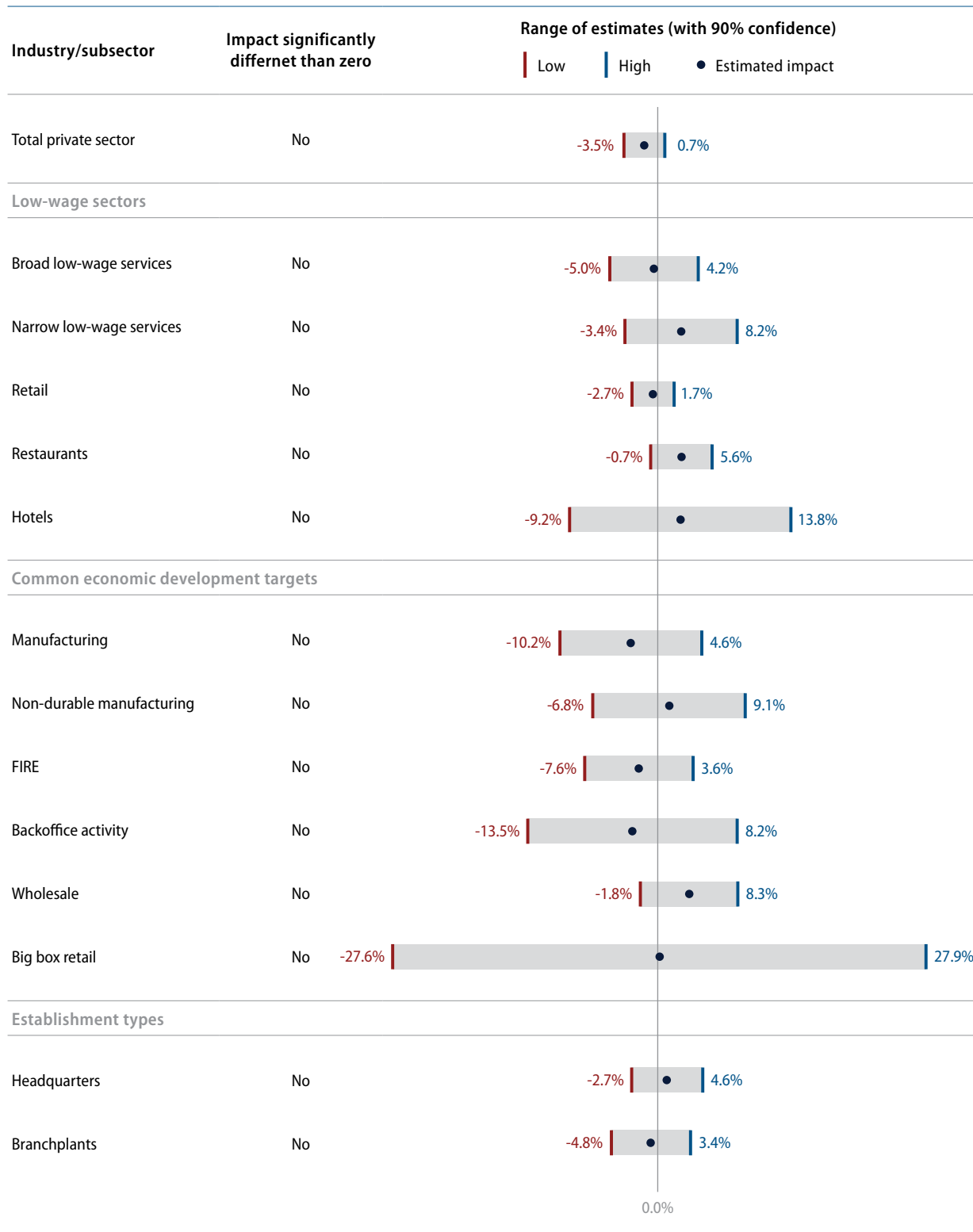
Effects on establishments

We also present our results for the number of business establishments in each outcome category to provide an additional measure of economic development activity. Even if business assistance laws do not affect aggregate employment levels in these sectors in a detectable manner, it is still possible that the overall number of businesses established in a living wage city would decrease due to negative signaling effects or because fewer businesses are attracted through local development initiatives.

Figure 2 presents the results in a parallel manner to Figure 1, with the dependent variable changed to establishment counts, rather than employment. Figure 2 visually represents the 90 percent confidence interval of our point estimates. Any number line that includes zero in the shaded area indicates that the estimated effect is not different than zero—meaning there is no employment effect, which is the case for all the variables tested, with the exception of the number of non-durable manufacturing establishments, which are estimated to slightly increase because of a business assistance living wage law.

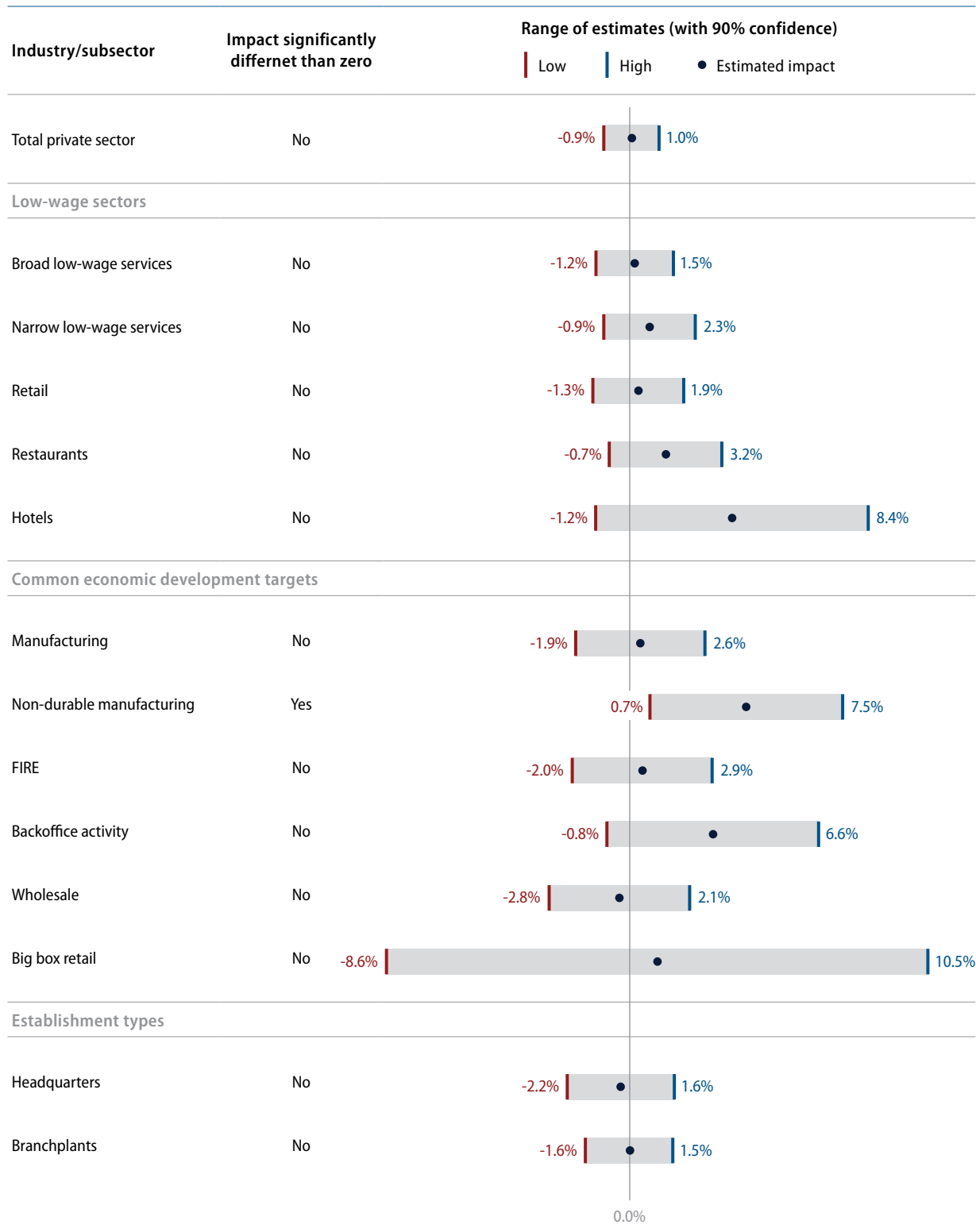
The results presented in Figure 2 indicate that none of the 14 variables show any discernable—or statistically significant—negative effect on the number of business establishments. These results provide additional confirmation that the passage of a business assistance living wage law is unlikely to have a harmful effect citywide or in any particular industry.

FIGURE 1
Living wage employment impacts



Note: All specifications include controls for the natural log of population, city linear trends, and city and year fixed effects.
 N: 465, 16 controls and 15 treatment cities.

FIGURE 2
Living wage establishment impacts



Note: All specifications include controls for the natural log of population, city linear trends, and city and year fixed effects.
 N: 465, 16 controls and 15 treatment cities.

Conclusion and policy implications

Business assistance living wage laws are promoted as a way to maximize a city or county's economic development subsidies by supporting the creation of family-supporting jobs. Critics argue that an improvement in job quality comes at the expense of a reduction in the quantity of jobs. This study presents strong evidence that these claims are unfounded.

Previous empirical research on the impact of business assistance living wage laws has detected significant decreases in employment.⁴³ Yet experts have questioned this past research on the grounds that the data sources could not detect urban-level impacts and that they did not adequately control for whether cities actually enforce their business assistance provisions. This study uses a more robust dataset than the previous research and includes background archival research into each treatment city's law, and we find no evidence of negative employment effects from business assistance living wage laws. Our research design is conceptually identical to that of Adams and Neumark, yet we can rule out negative consequences of the scope they report.

One caveat is important here. Our dataset does allow for the detailed consideration of direct and indirect effects across a wide array of potential industries but we cannot use it to measure the effect on local wages. We cannot show that workers directly received wage increases due solely to the application of a business assistance living wage. This finding would be crucial in evaluating how effective living wage laws are on the main problems they attempt to address, such as poverty and inequality. Yet many other studies in the living wage literature have shown that workers and their families do receive wage increases.⁴⁴ It is important to consider these findings in conjunction with the type of detailed case studies that can gather direct observations of wages and employment at covered firms.

Our results—which indicate no significant impact on economic development outcomes—are far from an extreme finding. In fact, it is consistent with recent research on the economic impact of minimum wage laws.⁴⁵ These general findings that labor

standards such as the minimum and living wage do not result in the type of negative economic consequences predicted by either orthodox economic theory or critics of the laws stand to offer a strong alternative interpretation for policymakers.

Accurate information on business assistance laws is critical at this time, as the current economic crisis has increased pressure on local leaders to create jobs. Local governments are increasingly being asked by businesses to lower labor standards in exchange for investment. This study suggests that such calls to lower labor standards in exchange for jobs are not based in fact.

Economic development wage standards are one tool that a city can use to create jobs of greater quality. We have compared two sets of cities in order to assess the effectiveness of such laws—those with enforced business assistance living wage laws and those without—and found that there is no loss in the number of jobs due to the living wage requirement. It appears that, even during hard times, economic development wage standards are an effective tool for increasing wages in a city without sacrificing the number of jobs.

Technical appendix

Employment and establishment variables listed by Standard Industry Classification codes

Broad low-wage services: Personal Services (72), Business Services (73), Automotive Repair, Services and Parking (75), Miscellaneous Repair Services (76), and Amusement and Recreation Services (79).

Narrow low-wage services: Miscellaneous Personal Services (729), Mailing, Reproduction, Stenographic (733), Services to Buildings (734), Misc. Equipment Rental & Leasing (735), Personnel Supply Services (736), Guard services (738101), Automobile Parking (752), Automotive Repair Shops (753), Carwashes (7542), Commercial Sports (794), and Misc. Amusement, Recreation Services (799).

Retail: All establishments in SIC 51–59, with the exception of SIC 58, Eating and Drinking Places.

Restaurants: SIC 58, Eating and Drinking Places, including cafeterias.

Hotels: All establishments in SIC 701, Hotels and Motels.

Manufacturing: We include all establishments in SIC 20 through SIC 39 in this group.

Nondurable manufacturing: This variable includes establishments in SICs 20–29.

Back office: This variable includes establishments in the following SIC codes: Adjustment and collection services (7322), Direct mail advertising services (7331), Photocopying and duplicating services (7334), Computer and Data Processing Services (737), and Telephone services (738910).

Wholesale: This industry includes establishments in SICs 50 and 51.

Big-box retail: We approximate the big-box category by only including retail establishments that are branches of firms with at least 10 other locations and with sales volume in the top 75 percent of the other retailers in the city.

Finance insurance and real estate: This industry includes establishments in SICs 60–67.

Identification strategy

We use our panel dataset to estimate the following regression that measures how living wage laws effect employment and establishments for the industry groups described above.

$$\text{Ln}(E_{it}^k) = a + \sum_{l=t-2}^{l=t+2} (\beta_l * LW_{it(l)}) + \ln(\rho\rho_{it}) + \delta_i + \gamma_t + \tau_{it} + \epsilon_{it}$$

The dependent variable $\text{Ln}(E_{it}^k)$ is the natural log of the outcome variable (either employment or the count of establishments) in city i in year t . The model is estimated separately in the same for each of 14 industry groups or establishment types (k) such as retail or manufacturing or headquarters. Equation (1) predicts $\text{Ln}(E_{it}^k)$ as a function of a living wage indicator variable LW_{it} , which is coded 1 for each year that a business assistance living wage provision is in effect for an entire year in a given city. $LW_{(it)}$ is therefore zero for all years in the control sample and 1 for all years beginning in the calendar year after passage for the treatment group. The set of coefficients (β_l) that measure the effects of living wage passage are entered in distributed lag structure beginning two years before the living wage and continuing two years postpassage. The inclusion of lead terms on the LW variable captures what is happening to the outcome variable just before the law takes effect. This is important and has become a standard procedure in panel studies of causal effects because a spike (or dip) in employment just before the treatment can result in an erroneous treatment effect.⁴⁶ The inclusion of lag terms of LW (for example, postpassage) similarly accounts for long-term effects. The coefficient on the final lagged term ($\beta_{l=t+2}$) represents the cumulative effect not only in the second year after passage but in all years in the sample after passage. This is therefore the primary coefficient of interest for policy implications.

For controls, Equation (1) includes a term that measures the natural log of each city’s annual population, fixed effects for each city δ_i —which control for and idiosyncratic differences between cities that do not vary with time—as well as year fixed effects, γ_t , which adjusts for common time effects such as changes in the macroeconomic environment. We also include a city-specific time trend, τ_{it} , that controls for differential trends in the outcome variable across the group of cities that vary over the entire time period. This is critical for the set of cities in our sample, which are drawn from various regions of the United States. If some cities are facing long-term declines in manufacturing employment and others are located in growing industrial regions, for example, we want to isolate the impact of living wage passage by removing the overall (time-varying) trend from each city. We also estimated Equation (1) with a time trend for each group (the treatment and control groups as a whole), as well as regional (for example, West, South, Northeast) trends to test that adding a city-specific trend potentially over controlled for differences between each city, and that the trend itself might be capturing some variation in the outcome variable that is attributable to the true living wage impact. Changing the scale of the time trend made no substantive difference in the results and, as such, is not reported here. Equation (1) also includes a constant term (a) and a random error term ε_{it} .

We only present results for $(\beta_l = t-2)$ two years prior to and $(\beta_l = t+2)$ two years after the passage of the living wage. The lagged term can be interpreted as the “long-term” impact of passing a business assistance living wage. The coefficients reported can be interpreted as the semielasticity of employment (or establishments) in response to changing living wage status. In other words, the percent change in the outcome variable that one can expect from passing a business assistance living wage law.

TABLE A
Results of employment regression

Industry/subsector	(1)	(2)	(3)	90% confidence interval on long-term effect	
	Pre-trend	Immediate effect	Long-term effect	Lower bound	Upper bound
Total private sector	-0.012 (0.011)	0.02 (0.016)	-0.014 (0.013)	-0.035	0.007
Retail	-0.023 (0.014)	-0.008 (0.017)	-0.005 (0.013)	-0.027	0.017
Broad low-wage services	0.025 (0.024)	0.012 (0.037)	-0.004 (0.028)	-0.05	0.042
Narrow low-wage services	0.053* (0.030)	0.023 (0.032)	0.024 (0.035)	-0.034	0.082
Wholesale	-0.032 (0.032)	-0.002 (0.042)	0.032 (0.030)	-0.018	0.083
Restaurants	-0.002 (0.024)	-0.019 (0.032)	0.024 (0.019)	-0.007	0.056
Manufacturing	0.008 (0.041)	-0.046 (0.050)	-0.028 (0.044)	-0.102	0.046
Non-durable manufacturing	0.062 (0.045)	-0.119* (0.070)	0.012 (0.047)	-0.068	0.091
FIRE	-0.007 (0.033)	0.071** (0.035)	-0.02 (0.034)	-0.076	0.036
Hotels	0.166** (0.083)	0.006 (0.062)	0.023 (0.069)	-0.092	0.138
Backoffice activity	-0.063 (0.093)	0.023 (0.077)	-0.026 (0.065)	-0.135	0.082
Big box retail	0.046 (0.082)	0.075 (0.118)	0.002 (0.166)	-0.276	0.279
Headquarters	-0.018 (0.025)	0.037 (0.035)	0.01 (0.022)	-0.027	0.046
Branchplants	0.001 (0.023)	0.011 (0.031)	-0.007 (0.025)	-0.048	0.034

Note: All specifications include controls for the natural log of population, city linear trends, and city and year fixed effects.

Column (1) lists the coefficient on the 2-year lead of LW treatment, Column (2) lists the contemporaneous effect, and Column(3) lists the long-term impact of LW treatment. Robust standard errors are clustered at the city level and are listed in parenthesis under each coefficient.

N: 465, 16 controls and 15 treatment cities.

*significant at .1 level, ** significant at .05 level, *** significant at .01 level

TABLE B
Results of establishments regression

Industry/subsector	(1)	(2)	(3)	90% confidence interval on long-term effect	
	Pre-trend	Immediate effect	Long-term effect	Lower bound	Upper bound
Total private sector	-0.01 (0.007)	0.001 (0.008)	0.001 (0.006)	-0.009	0.01
Retail	-0.021* (0.011)	-0.005 (0.010)	0.003 (0.010)	-0.013	0.019
Broad low-wage services	-0.003 (0.009)	0.002 (0.008)	0.002 (0.008)	-0.012	0.015
Narrow low-wage services	-0.012 (0.012)	-0.002 (0.012)	0.007 (0.010)	-0.009	0.023
Wholesale	-0.006 (0.015)	-0.004 (0.018)	-0.004 (0.015)	-0.028	0.021
Restaurants	-0.023* (0.013)	-0.006 (0.012)	0.0126 (0.012)	-0.007	0.032
Manufacturing	0.005 (0.013)	0.002 (0.013)	0.004 (0.014)	-0.019	0.026
Non-durable manufacturing	0.005 (0.014)	0.015 (0.027)	0.041** (0.020)	0.007	0.075
FIRE	-0.012 (0.012)	0.005 (0.017)	0.00432 (0.015)	-0.02	0.029
Hotels	0.052 (0.036)	-0.01 (0.034)	0.036 (0.029)	-0.012	0.084
Backoffice activity	0.005 (0.031)	0.034 (0.031)	0.029 (0.022)	-0.008	0.066
Big box retail	-0.022 (0.063)	0.169** (0.080)	0.0096 (0.057)	-0.086	0.105
Headquarters	0 (0.011)	-0.005 (0.010)	-0.003 (0.011)	-0.022	0.016
Branchplants	-0.021 (0.014)	0.002 (0.014)	-0.0003 (0.009)	-0.016	0.015

Note: All specifications include controls for the natural log of population, city linear trends, and city and year fixed effects.

Column (1) lists the coefficient on the 2-year lead of LW treatment, Column (2) lists the contemporaneous effect, and Column(3) lists the long-term impact of LW treatment. Robust standard errors are clustered at the city level and are listed in parenthesis under each coefficient.

N: 465, 16 controls and 15 treatment cities.

*significant at .1 level, ** significant at .05 level, *** significant at .01 level

TABLE C
Description of living wage laws: Treatment sample

City	Passage date	Description
Ann Arbor, MI	3/5/2001	The legislation applies to employers holding city service contracts valued at \$10,000 or more. Companies with fewer than five employees and nonprofits with fewer than 10 employees are exempt. The living wage was \$11.71/hour in 2009 if the company provided health care insurance or \$13.06/hour if it provided no insurance.
Berkeley, CA	6/1/2000	The ordinance applies to municipal workers, employers who are awarded city contracts, businesses receiving financial assistance, nonprofit organizations, and municipal leaseholders. The living wage in 2010 is \$12.41/hour with health benefits or \$14.47/hour if no insurance is provided.
Cambridge, MA	5/9/1999	The ordinance applies to municipal employees, city contractors and subcontractors who have contracts worth more than \$10,000, and businesses who have received at least \$10,000 in financial assistance. The living wage was \$13.69/hour in 2009.
Cleveland, OH	6/19/2000	The ordinance applies to companies with 20 or more employees and nonprofits with 50 or more employees that receive at least \$75,000 in financial assistance from the city, as well as tenants of recipients of financial assistance, and companies holding a contract with the city worth \$25,000 or more. The ordinance also applies to subcontractors of companies who receive assistance or city contracts. The living wage in 2009 was \$11.71/hour when health insurance was provided and \$13.06/hour if health care was not provided.
Duluth, MN	7/14/1997	The legislation applies to employers and subcontractors who receive at least \$25,000 of financial assistance in the form of business loans or grants, enterprise zone credits, tax increment financing, industrial land write-downs, and lease abatements.
Hartford, CT	10/12/1999	The ordinance applies to service contracts of \$50,000 or more, development projects with \$100,000 or more in city assistance, and real estate developments costing more than \$25,000 on city-owned land. The living wage was \$11.66/hour in 2009 if health insurance was provided and \$17.78/hour if no insurance was provided.
Los Angeles, CA	5/5/1997	The ordinance applies to employers who are awarded assistance of \$1,000,000 or more in one year or service contracts of \$25,000 or more. It also applies to subcontractors and employers with public leases or licenses. The living wage is \$10.30/hour with health insurance and \$11.55/hour with no insurance in 2010. The living wage is subject to annual cost of living adjustments.
Minneapolis, MN	11/4/2005	The ordinance applies to employers with service contracts or subcontracts of \$100,000 or more. Employers must attempt to create one living wage job for every \$25,000 that they receive. The living wage in 2009 was \$11.66/hr (110 percent of the federal poverty rate) with health insurance, or 13.78/hr (130 percent of federal poverty rate) without insurance.
Oakland, CA	4/1/1998	The ordinance applies to employers awarded \$100,000 or more in assistance, city contractors receiving \$25,000 or more, and leaseholders of recipients of assistance who occupy property that is improved through the assistance and employ 20 or more people. The living wage in 2009 was \$9.13/hour with health insurance or \$10.50/hour if no insurance is provided.
Philadelphia, PA	5/26/2005	The ordinance applies to city contractors with contracts worth more than \$10,000 and recipients of city financial aid in excess of \$100,000, as well as lessees of city property. It sets the living wage at 150 percent of the federal minimum wage. It includes a clause on health benefits, which states that an employer must provide health insurance if it provides benefits to some full-time employees elsewhere in the firm. The ordinance mandates a living wage advisory commission to oversee enforcement, of which businesses may represent no more than 4/9 of the members.
Richmond, CA	10/1/2001	The ordinance applies to all city contractors with a contract worth more than \$25,000, and recipients of any local economic development aid of \$100,000 or more. It also applies to lessees of public property that employ 25 full-time employees or more and generate \$350,000 or more in annual gross receipts. And it includes subcontractors of contractors, economic development recipients, and lessees. The living wage was \$11.42/hour if employer paid at least \$1.50/hour in health benefits, or \$12.92/hour without insurance at the time of the law's adoption.
San Antonio, TX	7/1/1998	The ordinance applies to businesses receiving tax abatements requiring they pay 70 percent of employees in new jobs \$9.27/hour, and 70 percent of durable goods workers \$10.13/hour. Businesses may be eligible for tax abatement if they fill 25 percent of new jobs with economically disadvantaged individuals.
San Francisco, CA	11/1/2000	The ordinance applies to employers who are awarded city contracts, businesses receiving financial assistance, nonprofit organizations, and municipal leaseholders at the San Francisco International Airport. It set wages at \$10.00/hour in 2002 with 2.5 percent increases expected annually.
San Jose, CA	6/8/1999	The ordinance applies to employers who are awarded a service or labor contract of \$20,000 or more, or assistance of \$100,000 or more. The living wage was \$11.61/hour in 2005 for employers who provided health insurance and \$12.86/hour when employers provided no insurance.
Santa Fe, NM	2/27/2002	The ordinance applies to full-time municipal employees, city contractors who have contracts worth more than \$30,000 and that have more than 10 employees, recipients of financial assistance worth \$25,000 or more, and businesses requiring a license from the city. The living wage was \$10.50/hour in 2009.

Endnotes

- 1 Alan Peters and Peter Fisher, "The Failures of Economic Development Incentives," *Journal of the American Planning Association* 70 (1) (2004).
- 2 Author's reanalysis of CPS wage data presented in: Lawrence Mishel, Jared Bernstein, and Heidi Shierholz, *The State of Working America 2008/2009* (Ithaca: Cornell University Press, 2009), Table 3.5.
- 3 Timothy J. Bartik, "Thinking About Local Living Wage Requirements," *Urban Affairs Review* 40 (2) (2004): 269–299.
- 4 For example, for the first time a significant proportion of North Carolina's state-level economic development incentives have gone to companies that pay wages below the county's average wage. See: David Bracken, "State settles for luring low-wage jobs," *News & Observer*, September 19, 2010, available at <http://www.newsobserver.com/2010/09/19/690381/incentive-deals-settle-for-low.html>.
- 5 Arindrajit Dube, T. William Lester, and Michael Reich, "Minimum Wage Effects Across State Borders: Estimates Using Contiguous Counties," *Review of Economics and Statistics* 92 (4) (2010).
- 6 For example, the City of San Antonio used financial incentives in 2005 to attract World Savings Bank to expand its mortgage processing facility in the area, creating approximately 2,000 jobs. In addition, the City of San Antonio also attracted an auto parts supplier in 2005, which agreed to raise wages from \$10 per hour to \$11.03 per hour in exchange for a 10-year tax abatement. See: David Hendricks, "Enticements for Toyota suppliers should pay dividends for S.A.," *San Antonio Express-News*, July 9, 2005; Elizabeth Allen, "World Savings banking on tax breaks," *San Antonio Express-News*, November 17, 2005.
- 7 While many economists refer to spillover effects as an indirect rather than direct policy outcome, we use the term "direct spillover" to distinguish between those effects that result in clear upward wage pressure on firms—either through a mandated wage floor (narrow direct) or through competitive effects (direct spillover)—from those that have indirect effects on the local political context for economic development.
- 8 Such responses to wage floors are consistent with the monopsonistic model of the labor market. See: Alan Manning, *Monopsony in Motion* (Princeton, NJ: Princeton University Press, 2003).
- 9 Bartik, "Thinking About Local Living Wage Requirements."
- 10 David Card and Alan B. Krueger, "Minimum Wages and Employment: A Case Study of the Fast-Food Industry in New Jersey and Pennsylvania," *The American Economic Review* 84 (4) (1994): 772–793; Dube, Lester, and Reich, "Minimum Wage Effects Across State Borders"; David Neumark and William Wascher, "Employment Effects of Minimum and Subminimum Wages: Panel Data on State Minimum Wage Laws," *Industrial and Labor Relations Review* 46 (1) (1992): 55–81.
- 11 Christopher Niedt and others, "The Effects of the Living Wage in Baltimore." Working Paper 119 (Economic Policy Institute, 1999).
- 12 In its most basic form, the difference-in-differences, or DD method simply calculates employment changes before and after passage of a law in both the treatment and control groups and then subtracts the employment difference in the treatment group from the difference in the control.
- 13 David Fairris, "The Impact of Living Wages on Employers: A Control Group Analysis of the Los Angeles Ordinance," *Industrial Relations* 44 (1) (2005): 84–105. Card and Krueger's method included in: Card and Krueger, "Minimum Wages and Employment."
- 14 Michael Reich, Peter Hall, and Ken Jacobs, "Living Wage Policies at the San Francisco Airport: Impacts on Workers and Businesses," *Industrial Relations* 44 (1) (2005): 106–138.
- 15 Robert Pollin and Jeannette Wicks-Lim, "Comments on Aaron Yelowitz, 'Santa Fe's Living Wage Ordinance and the Labor Market.'" Working Paper WP108 (Political Economy Research Institute, 2005).
- 16 Arindrajit Dube, Suresh Naidu, and Michael Reich, "The Economic Impacts of a Citywide Minimum Wage." Working Paper 111-05 (Institute of Industrial Relations, 2006).
- 17 David Neumark, "How Living Wage Laws Affect Low-Wage Workers and Low-Income Families" (San Francisco: Public Policy Institute of California, 2002).
- 18 Mark D. Brenner, Jeannette Wicks-Lim, and Robert Pollin, "Measuring the Impact of Living Wage Laws: A Critical Appraisal of David Neumark's 'How Living Wage Laws Affect Low-Wage Workers and Low-Income Families.'" Working Paper 43 (Political Economy Research Institute, 2002).
- 19 David Fairris, "The Impact of Living Wages on Employers: A Control Group Analysis of the Los Angeles Ordinance," *Industrial Relations* 44 (1) (2005): 84–105.
- 20 Scott Adams and David Neumark, "The Effects of Living Wage Laws: Evidence from Failed and Derailed Living Wage Campaigns." Working Paper 11342 (National Bureau of Economic Research, 2005).
- 21 Mark D. Brenner, Jeannette Wicks-Lim, and Robert Pollin, "Detecting the Effects of Living Wage Laws." In Robert Pollin and others, eds., *A Measure of Fairness: The Economics of Living Wages and Minimum Wages in the United States* (Ithaca: ILR Press, 2008).
- 22 Adams and Neumark, "The Effects of Living Wage Laws."
- 23 Brenner, Wicks-Lim, and Pollin, "Detecting the Effects of Living Wage Laws."
- 24 Stephanie Luce, *Fighting for a Living Wage* (Ithaca: Cornell University Press, 2004).
- 25 For example, St. Louis technically has a business assistance law but it only applies when a firm receives at least \$20 million in local incentives. As discussed below, in this paper we treat St. Louis as a control city since its business assistance provision is weak.
- 26 Neumark, "How Living Wage Laws Affect Low-Wage Workers and Low-Income Families"; Adams and Neumark, "The Effects of Living Wage Laws."
- 27 T. William Lester, "The Impact of Living Wage Laws on Urban Economic Development Patterns and the Local Business Climate: Evidence from California Cities," *Economic Development Quarterly* (forthcoming).

- 28 Fairris, "The Impact of Living Wages on Employers."
- 29 Examples of such natural experiments in labor economics include spatial discontinuities such as comparing employment on either side of a state line after a minimum wage increase (see: Card and Krueger, "Minimum Wages and Employment"; Dube, Lester, and Reich, "Wage Effects Across State Borders") as well as regression discontinuity approaches (see: John DiNardo and David S. Lee, "Economic Impacts of New Unionization on Private Sector Employers: 1984-2001," *The Quarterly Journal of Economics* 119 (4) (2004): 1383-1441).
- 30 Adams and Neumark, "The Effects of Living Wage Laws."
- 31 Ibid.
- 32 Ibid.
- 33 "Employment Policies Institute," available at <http://www.epionline.org> (last accessed April 30, 2010). The Living Wage Resource Center was a website maintained by the now-defunct Association of Community Organizations for Reform Now, or ACORN, which was very active in supporting and passing living wage laws across the country. This web-based listing contained information on living wage type, wage level, and date of passage, and was accessed in 2008 during the author's dissertation research, which is published as Lester, "The Impact of Living Wage Laws on Urban Economic Development Patterns and the Local Business Climate: Evidence from California Cities," (forthcoming).
- 34 Brenner, Wicks-Lim, and Pollin, "Detecting the Effects of Living Wage Laws."
- 35 Chicago, Illinois, did not reject an explicit business assistance form of living wage but we included it since the City Council passed a living wage law that applied to "big-box" retailers in 2006 that the mayor immediately vetoed. Throughout the late 1990s and early 2000s, Chicago used Tax Increment Financing, or TIF, to help bring in a host of big-box retailers including Target and Home Depot. Although this legislation was not tied to the receipt of financial assistance, since it targeted a segment of the retail market that had become accustomed to receiving development assistance, it would have been functionally equivalent to a business assistance provision. In addition, during the ongoing debates over the merits of the law, opponents frequently aired the negative "business climate" argument, which suggests that the law would have had a similar indirect effect.
- 36 Note that the earliest living wage law in our sample occurred in 1998.
- 37 To ensure that new businesses are captured by their telephone surveys, D&B reviews each state's 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.
- 38 David Neumark, Junfu Zhang, and Brandon Wall, "Employment Dynamics and Business Relocation: New Evidence from the National Establishment Time Series." Working Paper W11647 (National Bureau of Economic Research, 2005).
- 39 Since NETS has a higher capture rate for very small firms, including self-employed persons, it is less comparable with other publicly available data sources such as the QCEW or County Business Patterns. Previous research indicates that for establishments with five or more employees there is a high correlation between employment measurements in NETS and other sources.
- 40 To match zip codes to the political jurisdictions we used a geographical association based on the population-weighted centroid of each zip code in 2000. We obtained the zip-to-place match from the MABLE/Geocorr2K: Geographic Correspondence Engine v1.3.3 (August 2010), published by the Missouri Census Data Center, available at <http://mcdc2.missouri.edu/websas/geocorr2k.html>. While we understand that zip code boundaries shift over time, and that new zip codes are created that would perhaps not be recognized by the 2000 Census, this turned out not to be a significant issue for our sample of large core urban counties in the NETS. In our sample of more than 1 million establishments from the counties that contained our treatment and control cities, 95.1 percent of records were matched to a city (i.e. census place) using this method. For the remaining 4.9 percent we geocoded each record based on their reported current latitude and longitude in the NETS database. To be fair, among this group of geocoded records (4.9 percent) we are not able to capture the effect of moves since the latitude and longitude information is only available for the last year the establishment was active in the database. However, of this group only 9 percent ever moved, resulting in an overall capture rate of firm moves of 99.9 percent for the entire sample.
- 41 Please note that the variable for big box retail, due to how it was narrowly defined, doesn't allow us to include all the treatment and control cities in the analysis. This leads the standard errors to be much bigger than all the other estimates.
- 42 Adams and Neumark, "The Effects of Living Wage Laws."
- 43 Ibid.
- 44 Michael Reich, Peter Hall, and Ken Jacobs, "Living Wage Policies at the San Francisco Airport: Impacts on Workers and Businesses," *Industrial Relations* 44 (1) (2005); David Fairris and others, "Examining the Evidence: The Impact of the Los Angeles Living Wage Ordinance on Workers and Businesses" (Los Angeles: Los Angeles Alliance for a New Economy, 2005); Mark Brenner, "The Economic Impact of the Boston Living Wage Ordinance," *Industrial Relations* 44 (1) (2005).
- 45 Dube, Lester, and Reich, "Wage Effects Across State Borders"; Card and Krueger, "Minimum Wages and Employment."
- 46 Orley Ashenfelter and David Card, "Using the Longitudinal Structure of Earnings to Estimate the Effect of Training Programs," *The Review of Economics and Statistics* 67 (4) (1985): 648-660.

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