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Abstract

In times of economic crisis, state and local governments increasingly turn to economic development incentive programs to jumpstart job growth. However, recent efforts to attach minimum labor standards to incentives are seen as a threat to traditional practice. Critics of such “business assistance living wage” laws assert that they may reduce employment or redevelopment activity. This paper measures the actual impact of living wage laws using a time-series analysis of employment and business establishment trends in large U.S. cities that have enacted such laws and finds no evidence that ordinances mandating stronger labor standards affect aggregate economic development outcomes.

Keywords

economic development, business incentives, living wages

Introduction

The Great Recession that began in December 2007 and ended officially eighteen months later resulted in the net loss of eight million jobs. During the current recovery period, employment growth has been slow, leading some economists to describe the current period of persistently high unemployment as the “new normal.” Planners and policy makers at all levels of government have responded to this crisis by placing renewed emphasis on job creation activities. At the national scale, the political pressure to promote job growth has led to calls for relaxing environmental regulations and weakening labor standards. The current economic crisis is also influencing the practice of economic development at the state and local levels by simultaneously increasing the demand for planners to produce results (i.e., job creation) while reducing public financial support for economic development programs.¹ For example, despite falling revenues, usage of economic development incentives increased from 72 to 95 percent of local governments from 2004 to 2009 (Warner and Zheng 2011). Collectively, the economic crisis and ongoing fiscal stress have refocused policy makers’ attention on the classic trade-off in economic development between growth, on one hand, and standards that promote sustainable and equitable development on the other (Fitzgerald and Green Leigh 2002, 11–12). For some, calls to reform the development process and efforts to pursue an equity agenda are seen in today’s climate as second-order concerns relative to the jobs crisis. As part of this ongoing planning debate, this paper provides an empirical assessment of the impact of efforts to strengthen labor

standards on publicly supported job growth and business development plans at the local level. Specifically, this article tests whether cities that have attached living wage standards to their traditional economic development incentive practices have experienced less economic growth as measured by job creation and business establishment start-ups.

While a variety of scholarship has examined the growth-equity tradeoff at the city level generally (Peterson 1981; Fainstein 2001; Savitch and Kantor 2002) and the issue of labor standards specifically (Dube and Naidu 2007; Lester 2011), several factors differentiate this paper from previous work. First, this article uses a unique, private-sector database that contains an extensive time series of observations (1990–2008) at the establishment level to make detailed before and after calculations of the impact of business-assistance living wage laws on changes in employment and total business establishments. Second, using a set of thirty-one large and economically diverse urban jurisdictions across the country, this article estimates living wage impacts by comparing outcomes in cities that have passed (and enforce) business assistance living wage laws to those that attempted but failed

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to pass such provisions. This quasi-experimental research design—adopted by both labor economists and, increasingly, planning scholars (Rephann and Isserman 1994; Khattak and Rodriguez 2005)—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 measured impacts. Ultimately, after considering both direct and indirect effects on the broad set of industries and firm types that are most likely to be covered by business assistance living wage laws, this paper finds no evidence that such laws reduce employment or business growth over the short or long term.

The remainder of this article is organized as follows: The next section situates business assistance living wage laws within broader efforts to both reform traditional economic development planning and strengthen labor standards in urban labor markets. The third section provides a theoretical discussion of the ways in which business assistance living wage laws may impact aggregate economic activity at the city level and reviews the recent empirical literature on the subject. The fourth section outlines the research design and data sources used to conduct the analysis. It also describes the empirical strategy for measuring the impact of business assistance living wage laws on the outcome variables of interest. The fifth section presents the results. The final, sixth section discusses the implications for planners and policy makers that are currently debating local policy interventions that, like business assistance living wages, seek to maintain or improve job quality in conjunction with, rather than opposed to, increasing job quantity.

Labor Standards in the Context of Economic Development Planning

Before examining their impact on employment and business attraction, it is crucial to understand the context within which business assistance living wage laws emerged within the practice of economic development. Such a contextualization frames the movement to pass business assistance living wage laws within wider economic development discourses and offers an assessment of their continued use and impact on the field. Efforts to include labor standards in economic development policies are rooted in two distinct but related trends in planning practice. First, labor standards of the type considered in this paper—business assistance living wage laws—are a continuation of a long history of reforms to the “traditional” economic development practice of incentive use. Second, business assistance living wage laws arose from the broader social movement for living wages, which encompasses a variety of progressive interests and groups that have pressed for and enacted over 140 local ordinances that raise wages for various groups of covered workers (Pollin and Luce 1998; Luce 2004).

The oldest and still dominant form of practice in economic development is often referred to as the traditional, or *business attraction* model, and is exemplified by policies—such as

direct subsidies, tax exemptions, and targeted infrastructure improvements—that allocate public funds to private businesses or developers as incentives to locate within a given jurisdiction. Collectively state and local subsidies amount to more than \$50 billion per year (Peters and Fisher 2004), a figure that is likely higher in the aftermath of the Great Recession. The logic behind such policies stems from the idea that businesses are increasingly footloose and have greater choice in where to relocate or expand (Bartik 2005). As Rubin (1988) describes, traditional economic development practice is dominated by a focus on the interests of mobile businesses and private developers, and the resulting policies are oriented toward their needs.

As Fitzgerald and Green-Leigh (2002) describe, there has been a series of challenges to the traditional mode of practice, which directly criticize the use of incentives and offer specific policy reforms. The first phase of reform of the traditional, “smokestack chasing” model stemmed from academics documenting the dominance of local land owners and large, vested interests in the incentive-making process, which led to a lack of oversight and accountability and the promotion of redevelopment projects that had questionable job creation outcomes. The series of critiques of local economic development policy (Mollenkopf 1983; Logan and Molotch 1987; Rast 1999) as wasteful corporate welfare, or as Timothy Bartik so eloquently called “legalized bribery of the rich” (Bartik 2005, 140), led to calls by academics and civic reformers to reform incentive use by requiring greater transparency and accountability.

While the effort to reform incentive use expanded in the 1980s and 1990s, and included fairly widespread adoption of “clawback” agreements (Warner and Zheng 2011) that legally bind a company to repay any upfront subsidy if job creation or investment commitments are not met, there has been little in the way of a broad-based political movement to pressure local elected officials to enact stricter reforms or ensure that job opportunities are targeted to low-income populations. Until recently, incentive reform efforts have been led largely by academics and nonprofit watchdog groups such as Good Jobs First (see Leroy 2007), and waged from within the field itself, rather than at the ballot box or through direct legislative campaigns.²

However, business assistance living wage requirements have nearly all been enacted after extended grassroots political campaigns that resulted in new legislation or, in some cases, a ballot initiative. Thus, business assistance living wage laws must be also understood as an extension of the broader living wage movement. This movement is a direct response to rising inequality in the labor market and stagnant wages growth in low-wage service sector jobs. Since the late 1970s, urban labor markets have been characterized by stark increases in wage inequality resulting from the loss of family sustaining, often unionized manufacturing jobs, and the growth of low-wage service sector positions in industries such as retail, food services, and accommodation that were less mobile but more difficult to unionize (Appelbaum,

Bernhardt, and Murnane 2003; Bernhardt et al. 2008; Carre and Tilly 2008). The rise of working poverty in cities—particularly in the 1990s—led some political leaders, community organizations, and labor unions to seek creative ways to raise wages and labor standards for low-wage workers.

To address the problem of declining job quality, grassroots political coalitions in hundreds of cities across the United States push for living wage ordinances. The original and most common form of living wage ordinance requires businesses receiving city contracts to pay a wage level that ensures workers will earn an income above the poverty level. The living wage movement, which began in Baltimore in 1994, spread rapidly, and currently more than 140 local jurisdictions have some form of living wage law. What began as an effort to ensure that public funds would not be used to exacerbate the problem of low wages evolved into a broader set of urban policies that presented a clear alternative to the business attraction model of economic development. In some cities, such as Los Angeles, San Francisco, Pittsburgh, and most recently New York, living wage advocates sought to extend the living wage law from city contractors to any firms that receive public dollars through economic development subsidies. Thus, business assistance living wage laws were a natural extension of the living wage concept that taxpayer dollars should not be used to support employers that create very-low-wage jobs.

However, unlike the basic “contractor-only” living wage laws, these business assistance living wage laws directly challenge the logic of local economic development policies—which seek to lower the cost burden for private business—by placing additional requirements on firms that engage in development agreements with the public sector. For this reason, some business leaders and pro-business politicians have criticized labor standards for raising the cost of doing business. Specifically, living wage opponents claim that raising wages would lead to job losses since employers who are directly affected by the law would hire fewer workers. In addition, living wage laws are generally identified as an “anti-business” signal to other firms who may not even receive local subsidies (Bartik 2004) but nonetheless would choose not to locate in the enacting city.

While this debate continues, what is certain is that business assistance living wage laws and similar ordinances in large U.S. cities continue to be highly controversial. In 2006, for example, the debate over Chicago’s proposed “big-box” living wage law drew national media attention and resulted in the first-ever veto from Mayor Richard M. Daley. In addition, as New York City debates whether or not to pass a business-assistance living wage law—that would require businesses receiving \$1 million or more in economic development incentives to pay at least \$10 per hour plus \$1.50 toward health care (or \$11.50 without)—a highly visible public debate is taking place between the Bloomberg administration and the NYC Living Wage coalition. Although he supported a living wage for home health care workers on city contracts, Mayor Bloomberg compared extending the living

wage to private businesses in engaged city-funded redevelopment projects to Soviet central planning³ (see Taylor 2011). Thus the concern among elected officials and policy makers is over how business assistance laws will reshape the current, dominant form of economic development practice. An examination of how these laws impact urban economic development will provide context to this ongoing debate. Given the desire for both creating jobs and raising the quality of jobs, research is needed to fully assess the impact of living wage laws that are tied to business attraction policies. This article analyzes whether or not business assistance living wage laws reduce jobs and economic development activity in the cities that choose to pass them.

Detecting Effects of Business Assistance Living Wage Laws

How Business Assistance Living Wage May Impact Urban Economic Development

A critical aspect in the public debate over business assistance living wage laws is the degree to which they will actually impact the local economy. On one hand, many living wage proponents argue that business assistance clauses will not cause significant job losses. This argument is supported by research that indicates that higher minimum and living wages lead to efficiency gains for firms through reduced turnover (see Dube, Lester, and Reich 2011). In addition, increasing wages for the lowest paid workers also has a simulative effect on local economies, as low-income households typically spend more of their dollars locally. In addition to the potential positive, or offsetting, impacts of living wage laws, some researchers suggest that given their narrow scope business assistance living wage laws typically impact only a small number of firms that receive direct subsidies, and only a fraction of these firms employ workers below the mandated wage. In this view, business assistance laws function as a lower bound that serves to prevent localities from subsidizing low-wage jobs, but they don’t themselves represent a drastic reshaping of existing local labor practices.

On the other hand, living wage opponents suggest that these laws may “kill the deal,” arguing that forcing businesses to increase wages above the level usually offered could lead to the loss of all the jobs, not just low-wage positions, created by the subsidized firm. The opposition to business assistance living wage laws is very strong and is based on a set of academic and consultant reports that show negative impacts on employment and business development.⁴ Another argument is that even if deals do move forward despite higher mandated wages for low-wage positions, employers would cut back on staffing levels or substitute toward higher wage, higher skilled labor resulting in fewer people being employed. Thus, opponents predict fewer jobs overall would be created if business assistance provisions were to be enacted.

Given these competing interpretations and multiple paths of causation, it is important to distinguish the myriad ways that business assistance living wage laws could impact urban economies. This article divides such potential effects into two groups, direct and indirect effects. On the direct side, one can distinguish between a narrow set of establishments who are actually subjected to the higher wage provision—as they have sought and accepted financial assistance from a local government and operate businesses that hire a significant number of low-wage workers. Examples of “directly affected” businesses include retailers or food service operators who are part of a larger, publicly subsidized urban redevelopment project subject to the living wage requirement (e.g., Los Angeles’s Staples Center development). It should be noted that although such service sector jobs can be viewed as relatively place-bound, it is still possible that because of higher wage standards, employers would hire fewer workers in the face of higher wages. Alternatively, workers at low-wage assembly plants or back-office processing centers that are targets for local economic development incentives may receive *direct* wage increases as a result of the law.⁵ Beyond the narrow group that receive tax abatements or grants, other low-wage employers may also feel the impact of higher wage rates through a *direct spillover* effect.⁶ Such spillover effects may 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.⁷ According to the textbook, neoclassical economic viewpoint, 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.

In addition, living wage laws may indirectly impact the overall level of economic development activity in a city in two indirect ways. First, the passage of business assistance provisions may send a strong antibusiness signal to employers seeking to locate in the enacting city or to 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 (Bartik 2004). Even firms that may not seek economic development subsidies, but nonetheless hire a significant portion of low-wage workers, may view the passage of 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.

Beyond the signaling or “business climate” effect, business assistance provisions may also impact the practice of economic development policy making 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 on average, such as manufacturing, research and development, or biotechnology. As a result of this shift, the number of economic development “deals” may decline because (1) the chances of landing such high-value targets are lower and (2) higher wage industries may require fewer workers because of high productivity. Ultimately, while labor advocates who oppose public subsidies for low-wage industries may laud this indirect policy impact, it may end up reducing the total *quantity* of jobs created through economic development programs.

Previous Empirical Work

The empirical literature on the impact of living wage ordinances on employment primarily focuses on detecting the direct effects, and only rarely distinguishes between laws that explicitly apply to business assistance provisions. Two types of studies characterize this literature: individual case studies of a single city before and after passage of a given law—which tend to find no effect—and time-series models using panel data from a group of living wage and non–living wage cities—which tend to find significant negative impacts. In this sense, the living wage literature mirrors the tension between cases studies and panel studies in the broader economics literature on the impact of federal and state minimum wage changes (Neumark and Wascher 1992; Card and Krueger 1994; Dube, Lester, and Reich 2011).⁸

While cases studies of living wage impacts have the benefit of clearly identifying covered firms and therefore accurately measuring direct effects, the results of studies that compare a single case to a control group do not 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 have the benefit of greater external validity. For example, Neumark (2002) examines the state minimum wage increases and the impact of urban living wage laws on wages, employment, and poverty rates using a panel of large cities that passed ordinances 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.

Critiques of Neumark’s original study (2002) center on the choice of data set. Specifically, Brenner, Wicks-Lim, and Pollin (2002, 48) claim that Neumark’s wage results are vastly overstated given the fact that most living wage laws cover only a small fraction of workers; his choice of data set (Current Population Survey) can only identify metropolitan

areas, not individual cities; and that the CPS oversamples a single case (Los Angeles). The drawback with using broad household surveys such as the CPS is that there are too few cases to accurately distinguish “covered” and “uncovered” workers. Unlike the detailed cases studies, such as Fairris (2005), he cannot specifically identify a worker that worked for a firm covered by the living wage. Also, because of data constraints, Neumark restricts his analysis to the 1996–2002 period, a relatively short time period during an economic expansion.

More recently, Adams and Neumark (2005) compare the impact of living wage laws on the income and employment levels of low-income workers in cities that passed living wage laws and cities that had a failed living wage campaign. Using the failed cases as a control sample adds the benefit of holding 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). They find a statistically significant negative employment elasticity of -0.15 for lower-skilled workers. However, the authors find significant impacts only for those cities that have business assistance provisions, which they argue have the potential to cover most, if not all, low-wage workers in a given city. To date, this is the only quantitative study that distinguishes results for business assistance living wage laws.

Brenner, Wicks-Lim, and Pollin (2008) and others suggest that Adams’ and Neumark’s study is deeply flawed. First, Brenner, Wicks-Lim, and Pollin argue that business assistance laws directly impact only a small fraction of workers in each city with a living wage ordinance. Second, they argue that using the CPS to identify city-level effects is highly problematic because of small sample sizes at the urban scale and the inaccurate assumption that policy changes at the city level will impact workers throughout a metropolitan area (i.e., which is the scale that the CPS represents). Seeking to address these data-quality concerns and yet maintain a panel study research design, Lester (2011) uses the National Establishment Time Series (NETS)—the same data set used for this study—to measure the impact of living wage laws on employment and the number of business establishments in California. Although, there were too few cases to parse effects for business assistance from contractor-only living wage laws, his findings contradict those of Adams and Neumark (2005). In California, 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 (2008) argue that in some cases the business assistance provisions have been applied to only a very small number of firms, which could not produce a direct impact that is measurable by data sources like the CPS. As Luce (2004) points out, there is wide variation in the degree to

which living wage laws are enforced at the local level. In some cases, business assistance provisions have been simply ignored by economic development officials. In other cases, the negotiations ahead of passage significantly watered down the measure such that incentive thresholds are set so high that *no* firms are likely to be covered upon passage.⁹

Overall, there is a tension in the empirical literature on living wage effects. In general, panel studies of the type applied by Neumark (2002) and Adams and Neumark (2005) that include all or a large sample of living wage cities is preferable to comparing employment before and after passage within a single city. Panel studies—with the exception of Lester (2011)—find a negative impact on employment. On the other hand, case studies, including the detailed surveys performed by Fairris (2005) and others, 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.

The research design proposed in the following section combines the best of both approaches in the literature. First, this paper conducts 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. Next, a time-series quantitative analysis is used to estimate a generalizable impact assessment of business assistance living wage laws on urban economic development.

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. However, this simple direct comparison is extremely limited because of the problem of “endogeneity,” or the fact that cities choosing to pass business assistance living wage laws may be commonly affected by an exogenous trend correlated with employment changes. In one example, cities could be growing slower or faster as a group because of long-term trends such as deindustrialization or suburbanization. Therefore, the true effect of business assistance requirements could be confounded by some common trend.

To overcome the endogeneity problem, one needs to identify an appropriate control group of cities that did not pass business assistance living wage laws to compare to the treatment group. In constructing this control group of non-treated cities, one would ideally want to 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.¹⁰ Such quasi-experimental designs are commonplace in the field of labor economics in general (Ashenfelter and Card 1985; Lee 1999), and the minimum wage literature specifically (see Card and Kruger 1994; Dube, Lester, and Reich 2011), but have also been used widely in planning and economic development. For example, Repmann and Isserman (1994) used a quasi-experimental matching procedure to test the impact of new highway investments on county economic development outcomes. In addition, Khattak and Rodriguez (2005) used a quasi-experimental framework to examine differences in travel behavior among residents of traditional and new-urbanist designed suburbs in North Carolina.

The benefit of this type of research design is that one is comparing outcomes between treatment and control groups that are in all other respects very similar, except for the difference in the policy (i.e., the treatment). Thus, the estimated effect of the policy—derived as a difference-in-difference estimate—is unbiased. As noted above, Adams and Neumark (2005) overcome this problem 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. They refer to their control group as “failed or de-railed campaigns.”

This study adopts the same conceptual research design as Adams and Neumark (2005). Specifically, it compares outcomes for a treatment group including sixteen large, urban jurisdictions that have passed business assistance living wage laws to an equally 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 in that many 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. Implicit in this design is the assumption that the existence of a living wage law campaign indicates that control cities have a similar set of labor advocates and progressive actors who have raised the issue of a living wage in the political spectrum. Similarly, 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. Among all local governments in the United States, this group of cities is significantly different in that they tend to be larger, older, cities located on the West Coast or in the industrialized Northeast and Midwest.

While this research design does a good job of controlling for confounding differences between the treatment and control groups, it does not rule out all possibility of endogeneity. For this reason, this study tests for structural differences between the treatment and control groups and uses an identification strategy that allows for city-specific trends.

Case Selection Methodology

A key difference between this study and that of Adams and Neumark (2005) is in the choice of sample. Specifically, this study conducts a systematic qualitative assessment of the set of cities in the United States that have passed business assistance living wage laws so as to narrow down the treatment group to exclude cases for which the living wage has not been enforced or have thresholds that are too high to have an impact. Below, the research steps and inclusion criteria used for choosing both the treatment and control group are described. See Appendix A for details on each city’s living wage law for the final set of cities in the treatment group.

The first step in the case selection methodology was to determine the universe of all local jurisdictions that have passed or considered living wage laws to apply to businesses receiving any sort of financial assistance, including tax abatements, grants, direct infrastructure improvements, or below-market loans. This universe was determined by searching databases (as of April 30, 2010) maintained by the Employment Policies Institute (EmPI)¹¹ and Living Wage Resource Center.¹² These websites contain basic information on the type of living wage passed, the coverage thresholds, mandated wage levels, and the date of passage. Crucially, the EmPI’s website also contained listings for cities that rejected living wage laws either through a failed ballot initiative or council vote, a veto, or a repeal. This universe consisted of fifty cities, with thirty listed as successfully enacting a law and twenty with failed living wage campaigns.

Next, small cities with a population less than sixty thousand were excluded. This step was taken because small cities tend to engage in fewer economic developments “deals” for which the living wage would apply, and also, given the costs of acquiring NETS data, attention was placed on areas with larger potential impacts.

For the remaining cases in the universe, a deeper analysis was undertaken of the details of each city’s law to ultimately determine whether each city should be assigned to the treatment group, the control group, or dropped from the study altogether. Given the critiques of Brenner, Wicks-Lim, and Pollin (2008) and others, the goal in this process was to ensure that cities in the treatment group have laws that are likely to have either a direct or indirect effect on the local economy.

To ascertain the status and effectiveness of the laws and determine whether to exclude a city, this analysis consisted of three components. First, written copies of each city’s ordinance were obtained through web searches of city legislation. Each ordinance typically lists the exact coverage threshold, the types of financial assistance that qualified under the law, and any exclusions or loopholes. For example, in Boston, Massachusetts, the mayor is able to exempt any covered firm from the living wage at his or her discretion. St. Louis, Missouri, has an extremely high threshold for coverage—\$20

million—which essentially renders the business assistance provision toothless. These cities were thus excluded from the treatment group. The second step in the 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. City staff were contacted at the agency or department listed as responsible for enforcing the living wage or monitoring performance. In addition, phone calls were placed to each city's agency in charge of business attraction. From this work, Eugene, Oregon, and Meriden, Connecticut, were dropped from consideration based on staff indicating that the living wage has never been used or enforced. However, the limited success in reaching knowledgeable staff over the phone led to a last strategy. Secondary sources including local newspaper listings and performance reports by local advocacy groups or foundations were analyzed to look for direct evidence of an economic development incentive deal entered into with an employer for which the living wage would apply. This analysis resulted in a final list of fifteen treatment cities.

These cities meet one or more of the following criteria: (1) assistance level thresholds lower than \$1 million, (2) direct evidence of enforcement from primary and/or secondary sources, or (3) evidence of strong enforcement campaigns and ongoing organizing activity after passage of the living wage (e.g., Los Angeles).

Finally, the list of control cities was selected from an initial universe of twenty cities listed in the EmPI database that rejected a living wage ordinance. This list was narrowed to sixteen cities to produce a balanced sample. While Chicago, Illinois, did not reject an explicit business assistance form of living wage, it was included since the City Council passed a living wage law applying to “big-box” retailers in 2006 that was immediately vetoed by the Mayor.¹³ Similar research steps as above were conducted on the proposed control 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). Finally, several cities were dropped to attempt to maintain a broad regional balance across the treatment and control samples. The resulting list of treatment and control cities is listed in Table 1 below.

Data Sources, Industry Selection, and Identification Strategy

Another key innovation in this research design is the choice of primary data source used to measure outcome variables. This study uses the National Establishment Time Series (NETS) database as the primary data source to construct a city-level panel data set using annual observations. Background information on the NETS itself and the steps used to aggregate observations from the establishment level to the city level are

Table 1. List of Treatment and Control Cities

Treatment Cities	Control Cities
Ann Arbor, Michigan	Albuquerque, New Mexico
Berkeley, California	Chicago, Illinois
Cambridge, Massachusetts	Dallas, Texas
Cleveland, Ohio	Durham, North Carolina
Duluth, Minnesota	Eugene, Oregon
Hartford, Connecticut	Indianapolis, Indiana
Los Angeles, California	Knoxville, Tennessee
Minneapolis, Minnesota	Lansing, Michigan
Oakland, California	Nashville, Tennessee
Philadelphia, Pennsylvania	New York, New York
Richmond, California	Omaha, Nebraska
San Antonio, Texas	Oxnard, California
San Francisco, California	Pittsburgh, Pennsylvania
San Jose, California	Providence, Rhode Island
Santa Fe, New Mexico	South Bend, Indiana
	St. Louis, Missouri

Source: Author's analysis.

described in Appendix B. In addition, the specific industry sectors and firm types used are described in detail.

Choice of Industry and Establishment Type

The primary objective of this paper is to test for the various ways a business assistance living wage law could impact fundamental measures of economic activity in the cities that choose to pass them. Given the richness of the NETS database—which includes a variable that identifies an establishment’s primary industry in the Standard Industrial Classification (SIC) system at the eight-digit level—this study can produce outcome variables allowing one to test the hypothesis that business assistance provisions reduce jobs through both direct and/or indirect ways. Specifically, employment and establishment count variables were calculated for fourteen separate industry sectors and firm types, organized into three broad categories. The first category, which arguably best approximates the set of employers most likely to be *directly* affected by the living wage, consists of low-wage service sector industries. This category includes the following five outcome variables:

- *Broad low-wage services:* This group includes establishment in the following two-digit SIC industries: 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), miscellaneous equipment rental and leasing (735), personnel supply services (736), guard services (738101), automobile parking (752), automotive repair shops (753), carwashes (7542), commercial sports (794), 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.
- *Hotels:* All establishments in SIC 701, hotels and motels.

The main logic behind including the set of industries in the broad low-wage service sector consists of two criteria. Since a major way firms become covered by business assistance provision is through participation in large-scale urban redevelopment projects that often include public subsidies for retail, sports complexes, and general purpose commercial space, this effect captures the broad set of service industries that are both low-wage and may be likely targets for inclusion in redevelopment projects. Since this category groups industries at the two-digit level, it is possible that some higher wage industries may be combined with low-wage ones, hence this category is also modified using the more refined industry data to capture only the low-wage industries from within the broad category. Finally, this analysis also includes the three largest employers of low-wage workers in most urban economies: retail, restaurants, and hotels. While these industries are often targets for local business attraction and urban redevelopment projects, they also represent the group of employers who are potentially most affected by *direct spillover* effects of higher wages.

The second major category of outcome variables comprises of what this article terms “common economic development targets,” which can be thought of as capturing both direct and indirect effects. While this category is not necessarily low-wage, compared to the national average, it includes those industries that are often targets of business attraction efforts. Specifically, employment and business establishment totals by city are calculated for the following groups:

- *Manufacturing:* Since most cities in the United States have experienced some form of deindustrialization and industrial job losses, manufacturing establishments have long been the target of local economic development initiatives. This variable includes all establishments in SIC 20 through SIC 39.
- *Nondurable manufacturing:* This subset of manufacturing industries tends to be less capital intensive, less unionized, and therefore has the potential to pay lower wages. Thus at least some portion of

this sector may be affected by the living wage mandate. Thus, SICs 20–29 are broken out as a separate outcome variable.

- *Back office:* Since economic development deals have also focused on the highly mobile activities of corporate services, such as call-centers and credit processing services, an outcome variable that attempts to capture this activity was created. 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:* Distribution centers are also targets for local economic development, especially for jurisdictions that have former industrial land in need of redevelopment, or large, undeveloped tracts. This industry includes establishments in SICs 50 and 51.
- *Finance Insurance and Real Estate (FIRE):* As one of the higher wage service industries that has actually grown over the past two decades, and generally undergone a process of reurbanization (i.e., tends to cluster in dense urban areas), this variable is included as a falsification test for our model. To the extent that the analysis detects strong negative trends in this sector associated with the passage of a living wage law, this would indicate a potential area of bias in the sample.

Lastly, two outcome variables are generated that are defined not by an establishment’s primary SIC code, but rather their place in the firm structure. Specifically employment and establishment counts are measured for headquarters—establishments that are the headquarters of a firm with at least two other establishments at different locations—as well as branch plants, which are nonheadquarters establishments in firms with multiple establishments. Thus these two variables do not include single-location firms (i.e., small businesses). These variables capture those establishment types that may be more susceptible to the indirect or signaling effects described above. As a summary measure, results for total private sector employment and establishments are also included.

Identification Strategy

To measure the impact of living wage laws on employment and establishments for the industry groups described above, this paper uses the NETS based panel data set to estimate the following regression.

$$\ln(E_{it}^k) = \alpha + \sum_{l=t-2}^{t+2} [\beta_l] * LW_{il(1)} + \ln(POP_{it}) + \delta_1 + \gamma_t + \tau_{it} + \varepsilon_{it} \quad (1)$$

The dependent variable $\ln(E_{it}^k)$ 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 way for each of the thirteen industry groups or establishment types (k) described above such as retail or manufacturing or headquarters. Equation (1) predicts $\ln(E_{it}^k)$ 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. Thus LW_{it} is 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 impact 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 (see Ashenfelter and Card 1985). The logic for including the lead term is also critical for the study of living wage laws in that it controls for the potential impact of employers or developers anticipating the passage of the law during the period in which labor advocates are engaged in campaigning for the law itself. As discussed above, the presence of an organized pro-labor coalition may have an indirect signaling effect on employers. Thus the inclusion of the lead term will ensure that any detected indirect effects are attributable to actual passage, rather than the presence of a campaign itself. Similarly, the inclusion of lag terms of LW (e.g., post passage) 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. Thus, for policy implications, this is the primary coefficient of interest.

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 any 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. A city-specific time trend, τ_{it} , is included that controls for differential trends in the outcome variable across the group of cities that vary over the entire time period (1990–2008). This is critical for the set of cities in the sample that are drawn from various regions of the United States. For example, if some cities are facing long-term declines in manufacturing employment and others are located in growing industrial regions, we want to isolate the impact of living wage passage by removing the overall (time-varying) trend from

each city. To test that adding a city-specific trend potentially overcontrolled for differences among cities, and that the trend itself might be capturing some variation in the outcome variable that is attributable to the true living wage impact, equation (1) was re-estimated with a time trend for each group (i.e., the treatment and control groups as a whole), as well as for each broad census region (e.g., West, South, Northeast). Changing the scale of the time trend made no substantive difference in the results and, as such, are not reported here. Finally, equation (1) includes a constant term (a) and a random error term ε_{it} .

In the results discussed below, this paper only presents 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. Specifically, the coefficients reported can be interpreted as the semielasticity of employment (or establishments) in response to changing living wage status, in other words, the percentage change in the outcome variable that one can expect from passing a business assistance living wage law.

Main Findings

Sample Diagnostics

It is important to explore and understand any potential areas of bias due to the specific sample of 31 treatment and control cities. Table 2 compares the mean values for a variety of demographic and economic variables and notes the key differences that are statistically significant.

From the standpoint of this research design, it is reassuring to see that there are no significant differences between the treatment and control samples for the pretreatment period¹⁴ annual employment growth rates, with treatment cities growing only 0.8 percent slower than the control. Similarly, the two groups are 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. This is likely a reflection of two phenomena. First, 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 earn 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. While income inequality seems to be higher in the treatment group, 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. The second factor in explaining these differences stems from the regional balance of the samples. While an attempt was made to produce balance in selected

Table 2. Comparative Statistics between Treatment and Control Groups

Variable	Treatment Group	Control Group	Difference of Means (T-C)	p-Value
Total population	665,149	1,000,709	-335,560	0.559
% African American	0.188	0.222	-0.034	0.535
% Hispanic	0.221	0.174	0.047	0.485
% Non-Hispanic White	0.453	0.541	-0.088	0.227
% with bachelor's degree or higher	0.349	0.265	0.083	0.107
% foreign born	0.202	0.135	0.066	0.123
% poverty	0.179	0.178	0.002	0.929
% unemployed	0.075	0.074	0.001	0.939
Median household income (1999\$)	41,003	35,943	5,060	0.133
Median rent (1999\$)	700	578	122	0.028**
Median housing value (1999\$)	203,460	111,131	92,329	0.012**
% of households in top US income quintile	0.210	0.152	0.058	0.045**
% employed in FIRE or professional/technical services	0.201	0.181	0.019	0.191
% employed in manufacturing	0.100	0.116	-0.016	0.385
% renters	0.524	0.491	0.032	0.406
Housing vacancy rate	0.052	0.074	-0.022	0.063*
Average annual growth rate, 1990-1997	0.023	0.025	-0.003	0.769
Average 3-year growth rate, 1990-1997	0.071	0.066	0.005	0.777

Sources: U.S. Census Data, 2000 obtained from the State of the Nation's Cities Data System. NETS: author's analysis. FIRE = finance, insurance, and real estate industries.

* $p < .1$, ** $p < .05$, *** $p < .01$.

control cases, we are still left with a treatment sample that is overweighted toward California (seven cases in the West). While these differences are not enough for one to conclude that the samples are systematically biased, it does provide a motivation for including the type of city-specific trend controls discussed in the previous section.

Employment Effects

The main analysis consists of living wage impacts for thirteen distinct outcome variables. Collectively, these dependent variables comprise a comprehensive examination of both the potential direct and indirect impacts of business assistance living wage laws. Table 3 presents the estimated effects of living wage passage, expressed as the percentage change in employment that can be expected after passage, allowing up to two years for lagged effects. Impacts are presented for overall private sector employment, and three subsets of industry sectors or employment types. It is critical to recall that these impacts exclude very small establishments (i.e., with less than four employees). However, since the universe of potentially affected firms includes only those that either directly receive economic development incentives or are tenants in subsidized redevelopment projects, this is a relatively minor concern.

As indicated in Table 3, none of the thirteen outcome variables show a negative and statistically significant

long-term impact of passing a business assistance living wage in either a direct or indirect sense. For total private sector employment—the most fundamental outcome for local policy makers—the estimated long-term effect is -1.6 percent. However, this effect is not statistically different from zero. Based on the range of possible effects, we can rule out any negative impact greater than -4.5 percent. It is also important to note that by this same logic, this estimate cannot rule out a positive impact of less than 1.3 percent.

For the category of industry sectors titled “direct low-wage sectors” this analysis finds that none of the measured impacts are significantly negative. These sectors are often part of urban redevelopment projects. They are also the most likely to be impacted through a potential spillover impact on the low-wage labor market. However, we can strongly rule out such direct impacts as the estimates are very close to zero, and can rule out impacts greater than -3.8 percent for retail, and -3.6 and -3.3 percent for the broad and narrowly defined groupings of low-wage service industries. For restaurants, the largest employer of minimum-wage workers of any industry, the long-term impact is slightly positive. As a result, we can rule out a negative impact of more than -2.4 percent. While not directly comparable because of differences in data sources, this measured impact is much lower than the estimates reported for low-wage workers in previous studies, such as Adams and Neumark (2005). While the estimates for hotels are positive and insignificant—suggesting

Table 3. Summary of Business Assistance Living Wage Employment Effects, by Industry

Industry/Subsector	Pre-trend ^a	Immediate Effect ^b	Long-Term Effect ^c	90% Confidence Interval on Long-term Effect ^d	
				LB	UB
Total private sector	-0.012 (0.011)	0.020 (0.016)	-0.014 (0.013)	-0.035	0.007
Low-wage services					
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)	-0.050	0.042
Narrow low-wage services	0.053* (0.030)	0.023 (0.032)	0.024 (0.035)	-0.034	0.082
Hotels	0.166** (0.083)	0.006 (0.062)	0.023 (0.069)	-0.092	0.138
Restaurants	-0.002 (0.024)	-0.019 (0.032)	0.024 (0.019)	-0.007	0.056
Common economic development targets					
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)	-0.020 (0.034)	-0.076	0.036
Wholesale	-0.032 (0.032)	-0.002 (0.042)	0.032 (0.030)	-0.018	0.083
Back office activity	-0.063 (0.093)	0.023 (0.077)	-0.026 (0.065)	-0.135	0.082
Establishment types					
Headquarters	-0.018 (0.025)	0.037 (0.035)	0.010 (0.022)	-0.027	0.046
Branch plants	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. 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. FIRE = finance, insurance, and real estate industries.

^aThis column lists the coefficient on the 2-year lead of low-wage (LW) treatment.

^bThis column lists the contemporaneous effect.

^cThis column lists the long-term impact of LW treatment.

^dThe final two columns list the lower (LB) and upper bound (UB) of the estimate implied by the standard error.

* $p < .1$, ** $p < .05$, *** $p < .01$.

little or no direct impact—the ranges are large, leading to a large bound of estimated impacts. However, since these ranges are centered around zero, from a statistical point of view, a large positive effect is just as likely as a strong negative effect. Therefore, one cannot conclude that living wage laws actually harm growth in these sectors.

For the industries that are often targets of business attraction activities, the results are similar. For nondurable manufacturing, the long-term point estimate is very close to zero,

but given the larger ranges, one cannot rule out larger negative impacts. However, such potentially large effects are really a function of the statistical reality of the small sample size, rather than a real measured effect. Since the results are approximately centered on zero, or near zero, it is reasonable to conclude that business assistance laws likely have no meaningful impact on employment in industries that are often targets of economic development. This finding casts doubt on the idea that attaching labor standards to economic

development incentives result in fewer deals and reduced targeting efforts on the part of local government.

Lastly, this analysis shows no employment effect for headquarters or branch plants. This finding further discredits the so-called “business climate” thesis, in that the business types which are arguably more mobile than local small businesses do not appear to be fleeing living wage cities or changing their location decisions in response to the change in the local political environment.

Effects on Establishments

To provide an additional measure of economic development activity, results for the number of business establishments in each outcome category are also presented. Even if business assistance laws do not impact 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 falls because of negative signaling effects, or because fewer businesses are attracted through local development initiatives. Table 4 presents the results in a parallel manner to Table 3, with the dependent variable changed to establishment counts, rather than employment.

The results presented in Table 4 indicate that there is no discernible (i.e., statistically significant) impact on the number of business establishments across any of the thirteen dependent variables tests. For the private sector overall, the long-term impact of passing a business assistance living wage is very close to zero (-0.3 percent). Based on the ranges reported, we can rule out negative impacts larger than -1.9 percent. For the set of direct low-wage service sector industries, the results are quite similar, with point estimates near zero measured with enough confidence to rule out negative effects of more than -3.3 percent for the hotel sector. The results for the remainder of the outcome variables are remarkably consistent, and compared to the employment estimates, are more precisely measured.

Conclusion and Policy Implications

The concept of linking publicly financed economic development programs to higher labor standards and quality jobs has been around since the 1980s and was a core component of the equity planning movement. However, it was not until the late 1990s when economic development reform efforts merged with the grassroots political power of the living wage movement that cities began to pass business assistance living wage laws. These laws are fundamentally an attempt to change the dominant practice of economic development and to address the problem of working poverty in urban areas. 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. In many ways, business assistance living wage movement can be thought of as part of larger equity planning agenda being advanced by a variety of social actors and at a variety of spatial scales (Clark and Christopherson 2009; Pastor, Benner, and Matsuoka 2009). However, business assistance living wage laws are particularly controversial, and critics argue that an improvement in job quality comes at the expense of a reduction in the quantity of jobs—claims that are increasingly salient at a time of high unemployment. 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 negative impacts on employment (Adams and Neumark 2005). Yet this research has been questioned on the grounds that the data source could not detect urban-level impacts, and that they do not control adequately for whether cities actually enforce their business assistance provisions. Using a more robust data set than the previous research and conducting the background archival research into each treatment city’s law, this study finds no evidence of negative employment effects from business assistance living wage laws. This research design is conceptually identical to that of Adams and Neumark, yet it can rule out the negative impacts of the scope that they reported.

One caveat is important here: while this data set does allow for the detailed consideration of direct and indirect effects across a wide array of potential industries, it cannot be used to measure the impact on local wages. Therefore, this study cannot show that increases in workers’ wages were a direct result of the application of a business assistance living wage. A finding such as this would be crucial in evaluating the impact of such laws on the main problems it attempts to address, such as poverty and inequality. However, many other studies in the living wage literature have shown that workers and their families do receive wage increases. For this reason, it is still 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.

The results of this study should be viewed by planners and practitioners alongside a broader set of findings that indicates that raising labor standards does not conflict with growth goals. In fact, the results presented here on business assistance living wage laws are consistent with recent research on the economic impact of minimum wage laws (Dube, Lester, and Reich 2011). Collectively, this general finding that labor standards such as the minimum and living wage do not result in the type of negative economic

Table 4. Summary of Business Assistance Living Wage Establishment Effects, by Industry

Industry/Subsector	Pre-trend ^a	Immediate Effect ^b	Long-Term Effect ^c	90% Confidence Interval on Long-Term Effect ^d	
				LB	UB
Total private sector	-0.010 (0.007)	0.001 (0.008)	0.001 (0.006)	-0.009	0.010
Low-wage services					
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
Hotels	0.052 (0.036)	-0.010 (0.034)	0.036 (0.029)	-0.012	0.084
Restaurants	-0.023* (0.013)	-0.006 (0.012)	0.0126 (0.012)	-0.007	0.032
Common economic development targets					
Manufacturing	0.005 (0.013)	0.002 (0.013)	0.004 (0.014)	-0.019	0.026
Nondurable 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)	-0.020	0.029
Wholesale	-0.006 (0.015)	-0.004 (0.018)	-0.004 (0.015)	-0.028	0.021
Back office activity	0.005 (0.031)	0.034 (0.031)	0.029 (0.022)	-0.008	0.066
Establishment Types					
Headquarters	0.0002 (0.011)	-0.005 (0.010)	-0.003 (0.011)	-0.022	0.016
Branch plants	-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. Robust standard errors are clustered at the city level and are listed in parenthesis under each coefficient. N = 608, 15 controls and 15 treatment cities. FIRE = finance, insurance, and real estate industries.

^aThis column lists the coefficient on the 2-year lead of low-wage (LW) treatment.

^bThis column lists the contemporaneous effect.

^cThis column lists the long-term impact of LW treatment.

^dThe final two columns list the lower (LB) and upper bound (UB) of the estimate implied by the standard error.

* $p < .1$, ** $p < .05$, *** $p < .01$.

impact predicted by either orthodox economic theory or pro-business critics offers policy makers a strong alternative interpretation.

The addition of accurate information on the impact of business assistance laws is critical at this time, as the current economic crisis has increased pressure on local leaders to create jobs. Increasingly, local governments are

being asked by businesses both within their jurisdiction and without to lower labor standards in exchange for investment. The findings of this study suggest that such calls to lower labor standards in exchange for jobs are not grounded in fact. Ultimately, living wage ordinances are one tool that a city can use to create jobs of greater quality.

Appendix A

Description of Living Wage Laws in Treatment Cities

City	Passage Date	Description
Ann Arbor, Michigan	3/5/2001	The legislation applies to employers holding city service contracts valued at \$10,000+. Companies with fewer than five employees and nonprofits with fewer than 10 employees are exempt. In 2009, living wages were \$11.71/hour if the company provides health care insurance or \$13.06/hour if no insurance is provided.
Berkeley, California	6/1/2000	The ordinance applies to municipal workers, employers who are awarded city contracts, businesses receiving financial assistance, nonprofit organizations, and municipal lease holders. Living wages in 2010 were established at \$12.41/hour with health benefits or \$14.47/hour if no insurance was provided.
Cambridge, Massachusetts	5/9/1999	The ordinance applies to municipal employees, city contractors, and subcontractors who have contracts exceeding \$10,000, businesses who have received at least \$10,000 in financial assistance. In 2009, the living wage was \$13.69/hour.
Cleveland, Ohio	6/19/2000	The ordinance applies to employers of 20+ employees (50+ for nonprofit organizations) receiving at least \$75,000 in financial assistance from the city, tenants of recipients of financial assistance, and companies holding a contract with the city worth \$25,000+. The ordinance also applies to subcontractors of companies who receive assistance or city contracts. Living wages in 2009 were \$11.71/hour when health insurance is provided and \$13.06/hour if health care is not provided.
Duluth, Minnesota	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, Connecticut	10/12/1999	The ordinance applies to service contracts of \$50,000+, development projects with \$100,000 of city assistance, and real estate developments costing more than \$25,000 on city-owned land. In 2009, the living wage was \$11.66/hour if health insurance is provided and \$17.78/hour if no insurance is provided.
Los Angeles, California	5/5/1997	The ordinance applies to employers who are awarded assistance of \$1,000,000+ in one year or service contracts of \$25,000+, and also applies to subcontractors and employers with public leases/licenses. Living wages were established at \$10.30/hour with health insurance and \$11.55/hour with no insurance. The living wage is subject to annual CPI adjustments.
Minneapolis, Minnesota	11/4/2005	The ordinance applies to employers with service contracts/subcontracts of \$100,000+. Employers must attempt to create one living wage job for every \$25,000 that they receive. In 2009, living wages were established at \$11.66/hour (110% of federal poverty rate) with health insurance or \$13.78/hour (130% of federal poverty rate) without insurance.
Oakland, California	4/1/1998	The ordinance applies to employers awarded \$100,000+ assistance; city contractors receiving \$25,000+; and leaseholders of recipients of assistance who occupy property that is improved through the assistance and employ 20+ people. Living wages in 2009 were set at \$9.13/hour with health insurance or \$10.50/hour if no insurance is provided.
Philadelphia, Pennsylvania	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% 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 members.
Richmond, California	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. The living wage was \$11.42/hour if employer paid \$1.50/hour or more in health benefits, or \$12.92/hour without insurance at the time of the law's adoption.
San Antonio, Texas	7/1/1998	The ordinance applies to businesses receiving tax abatements requiring they pay 70% of employees in new jobs \$9.27/hour, \$10.13/hour to 70% of durable goods workers, and \$8.25/hour to municipal employees. In addition, businesses may be eligible for tax abatement if they fill 25% of new jobs with economically disadvantaged individuals.

(continued)

Appendix A (continued)

City	Passage Date	Description
San Francisco, California	11/1/2000	The ordinance applies to employers who are awarded city contracts, businesses receiving financial assistance, nonprofit organizations, and municipal lease holders at the San Francisco International Airport. In 2002, wages were set at \$10.00/hour and expected to increase by 2.5% each following year.
San Jose, California	6/8/1999	The ordinance applies to employers who are awarded a service/labor contract of \$20,000+ or assistance of \$100,000. In 2005, living wages were set at \$11.61/hour for employers who provide health insurance and \$12.86/hour when no insurance is provided.
Santa Fe, New Mexico	2/27/2002	The ordinance applies to full-time municipal employees, city contractors who have contracts worth over \$30,000 and over 10 employees, recipients of financial assistance worth \$25,000+, and businesses requiring a license from the city. The living wage was \$10.50/hour in 2009.

Appendix B

Description of the National Establishment Time Series (NETS) Data Set and Panel Construction Steps

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 nineteen 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 establishment level data on employment, sales (estimated), industry (eight-digit SIC), ownership structure, and address for 1990–2008. Most importantly, the NETS tracks establishment moves over time, which allows us to accurately account of total employment in each local jurisdiction in each year.

Unlike household surveys, such as the Current Population Survey (CPS), D&B attempts to capture the entire universe of establishments operating in a given year. Once D&B assigns a DUNS number to an establishment, they contact that establishment each year by telephone to update information on their location, ownership structure, industry, employment and sales figures. To ensure that new businesses are captured by their telephone surveys, D&B reviews each 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.

Ultimately, the NETS database does a reasonably good job in capturing the level of economic activity 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. It is valuable to use NETS for a study of living wage impacts as it offers consistent long-term information on employment and the number of establishments at the local level, as opposed to county, metropolitan, or state levels. In addition, NETS offers detailed industry information on each record that allows us to focus on the specific low-wage industry groups that are most likely affected by business assistance provisions. In addition, the NETS can be used to measure impacts on industries that are often targets for local business attraction strategies even if they are not low-wage industries in particular.

The first limiting step in this analysis was on establishment size. Specifically, only the 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 on the one hand to reduce the cost of the data purchase, and on the other, because of comparability with other data sources. Thus it is important to note that the findings in this paper are limited to employment totals by city in establishments with five or more employees. This limiting step is not likely to have a major impact on this research as (1) very small firms do not typically receive local financial assistance and (2) they make up a small portion of overall employment in each city.¹⁵

As indicated above, the NETS is a dynamic database in that it tracks each establishment's location over time. While most establishments do not move, approximately 14 percent of the NETS records in the sample have moved at some point in time. However, 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 and was located there until 2000, when it moved to Boston, its current geographical

Appendix B (continued)

identifiers would reflect a location in Boston. However, if one wants to make an accurate employment total for New York in 1994–2000, one would want to count this firm in New York at this time. Since the NETS contains information not only on current geographic location but on the origin, time, and destination of each establishment move, it is possible to overcome this problem. Specifically, to build the city-level database, this paper combines 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. To match zip codes to the political jurisdictions, a geographical association was used based on the population-weighted centroid of each zip code in 2000.¹⁶ While zip code boundaries shift over time, and 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 this sample of large core urban counties in the NETS. In the overall sample of more than 1 million establishments from the counties that contained the treatment and control cities, 95.1 percent of records were matched to a city (i.e., census place) using this method. The remaining 4.9 percent were geocoded based on their reported current latitude and longitude in the NETS database.¹⁷

Once each establishment was assigned to a city for each year that it was in existence, the database was then aggregated 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 the analysis of living wage impacts.

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Notes

- An example of the fiscal crisis reducing resources for economic development is seen in California Governor Jerry Brown's decision to eliminate funding for all Redevelopment Authorities in the state and reclaim unspent tax revenue.

- Clearly alternative economic development models—which included incentive reforms and a call for greater equity in the development process in general—were key aspects of the platform of successful progressive regimes in the 1980s in cities like Cleveland and Chicago, as Fitzgerald and Green-Leigh (2002) describe. However, the current push for business assistance living wage laws—although similar in spirit—are better understood as efforts to change practice from the outside and not as stemming from a single, cohesive progressive coalition that seeks to dominate local politics.
- Specifically, Mayor Bloomberg is quoted as saying, “We’ve got to attract jobs in this city, and the marketplace is going to set the compensation. . . . The last time people tried to set rates, basically, was in the Soviet Union, and that didn’t work out very well. I don’t think we want to go in that direction.”
- A more detailed discussion of the empirical literature is discussed in a later section. In addition, the New York Economic Development Commission (NYEDC)—the agency that runs the City’s economic development incentive programs, contracted with Charles River and Associates for a large, year-long evaluation of the impact of the proposed business assistance living wage in NYC. This report is based largely on the work of Adams and Neumark (2005) reviewed here and was subject to widespread criticism by living wage advocates. However, this report articulated the major potential negative impacts that a business assistance living wage law may have on urban employment.
- 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 2000 jobs. In addition, the City of San Antonio also attracted an auto parts supplier in 2005 that agreed to raise wages from \$10 to \$11.03 per hour in exchange for a ten-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.)
- 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.
- Such responses to wage floors are consistent with the monopsonistic model of the labor market (see Manning 2003).
- Niedt et al. (1999) produced one of the earliest detailed case studies in their report on the impact of Baltimore’s landmark living wage law passed in 1994. They found that the living wage did not significantly increase contract costs and that employment remained the same at covered firms. However, this study did not compare employment changes at covered firms to a control group. In his study of Los Angeles, Fairris

- (2005) conducted two independent surveys of firms and workers that were covered and uncovered by the city's ordinance that applied to city contractors. 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. In another case study, Reich, Hall, and Jacobs (2005) 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 but had no discernible disemployment effect.
9. 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.
 10. 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 (Card and Krueger 1994; Dube, Lester, and Reich 2010) as well as regression discontinuity approaches (see DiNardo and Lee 2004).
 11. Employment Policies Institute website (<http://www.epionline.com> [accessed April 30, 2010]).
 12. The Living Wage Resource Center was a website maintained by the now defunct Association of Community Organizations for Reform Now (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. The database maintained by the Employment Policies Institute (EmPI), a think tank opposed to minimum wage and living wage policies, was extremely comprehensive in its overall universe and the organization has a vested interest in tracking all cities that considered passing a living wage law of any sort. Collectively, these two organizations maintained the best available listing of living wage laws in effect as well as failed and/or ongoing campaigns.
 13. Throughout the late 1990s and early 2000s, Chicago used Tax Increment Financing (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.
 14. Note that the earliest living wage law in the sample occurred in 1998.
 15. 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.
 16. We obtained the zip-to-place match from the MABLE/Geocorr2K: Geographic Correspondence Engine v 1.3.3 (August 2010), published by the Missouri Census Data Center available at <http://mcdc2.missouri.edu/websas/geocorr2k.html>.
 17. 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.

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Bio

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