

LIVING WAGES: PROTECTION FOR OR PROTECTION FROM LOW-WAGE WORKERS?

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Abstract

Living wage laws, which were introduced in the mid-1990s and have expanded rapidly since then, are typically touted as anti-poverty measures. Yet they frequently restrict coverage to employers with city contracts, and in such cases apply to a small fraction of workers. This apparent contradiction leads to the question of whether there are alternative motivations for various economic and political actors to seek passage of living wage laws. This paper considers the hypothesis that unions representing municipal employees work for the implementation of living wage laws to maintain or increase rents. By raising the wages that city contractors would have to pay, living wage laws may reduce the incentives for cities to contract out work that would otherwise be done by unionized municipal employees, hence increasing the bargaining power of municipal unions and leading to higher wages. The empirical analysis leads to evidence that the wages of unionized municipal workers are increased as a result of living wages, consistent with the rent-seeking hypothesis.

I. Introduction

The 1990s have witnessed widespread implementation of living wage laws across cities in the United States. There are currently more than 50 living wage ordinances in effect in the United States (most in cities, but a few applied to counties or school boards), and numerous campaigns for more are under way. Living wages are touted as anti-poverty measures. The Economic Policy Institute, while noting that other anti-poverty tools are needed, argues that “the living wage is a crucial tool in the effort to end poverty.”¹ And in their treatise advocating living wages, Pollin and Luce argue that “[T]he basic premise of the living wage movement is simple: that anyone in this country who works for a living should not have to raise a family in poverty” (1998, p. 1). Reflecting this anti-poverty goal, existing living wage ordinances often mandate that covered employers must pay their workers a wage sufficient to lift a family above the poverty level. For example, the Detroit living wage is set to 100 percent of the poverty line for a family of four if health benefits are paid, and 125 percent without health benefits. Naturally such goals lead to relatively high living wages, with the mandated wage floor exceeding \$8 or \$9 in many cities.

However, rather than mandate higher wages for all workers—as a minimum wage does, except for minor coverage exclusions—a curious feature of living wage laws is their narrow coverage. In particular, the common feature of most of these laws is their coverage of employers that are contractors or subcontractors with the city. Furthermore, while some laws entail broader coverage including employers receiving business assistance from the city, and a few cover city employees, living wage laws frequently cover *only* contractors or subcontractors. Estimates of the percentage of workers directly affected by coverage of contractors—from a series of city-specific consulting reports—are often quite low (below one percent), while the percentage of workers affected by business assistance provisions of living wage laws may be considerably higher (see Neumark and Adams, forthcoming (a)). This raises the question of why—given the

¹See www.epinet.org/Issueguides/livingwage/livingwagefaq.html.

stated anti-poverty objectives—living wage laws are focused on raising wage floors for so few workers, as opposed to creating more general wage floors at the local level.²

One possibility is that living wage laws serve other interests. In particular, this paper explores the hypothesis that unions representing municipal workers pursue living wage laws to offer higher-paid unionized municipal workers protection *from* low-wage workers, rather than to offer protection *for* low-wage workers. By raising the wages that city contractors must pay, living wage laws may reduce the incentives for cities to contract out work that would otherwise be done by unionized municipal employees.³ It is natural to consider such restrictions as potentially enhancing union bargaining power, and hence in particular protecting or increasing rents for unionized municipal workers. Indeed some evidence reported below suggests that unions representing municipal workers are particularly active in advocating living wage laws, and resolutions from the 1999 AFL-CIO convention note that lifting the wage floor via living wages and other mechanisms “enhances bargaining power and security for all workers.”⁴

This last quote succinctly captures the political economy puzzle posed by living wage laws. If the goal is to increase security for all workers—consistent with a broad anti-poverty agenda—why not push for broad measures? Or is a principal effect of living wage laws to benefit unionized municipal employees who might otherwise face competition from low-wage labor employed by city contractors and subcontractors, so that there is a potentially powerful constituency for narrow living wage laws? Earlier work suggests that public sector unions use their political power to increase demand for public services, hence shifting out the demand for their labor (Courant, et al., 1979; Freeman, 1986). The argument here is related, pointing to a

²Local minimum wage proposals are rare. One was approved in New Orleans in 2001, while proposals were defeated in Houston in 1997 and Denver in 1996. Santa Monica recently approved a living wage that covers city contractors, but also has features like a minimum wage, applying to businesses above a certain size in a restricted geographic area.

³In this sense, living wage laws may parallel the Davis-Bacon Act and other state-level prevailing wage laws impacting public construction projects. See Kessler and Katz (2001) for an analysis of prevailing wage laws. The less well-known 1965 Service Contract Act regulates wages paid by contractors providing services to the federal government.

⁴See www.aflcio.org/convention99/res1_1.htm.

different avenue—raising the price of the labor with which these workers compete—to achieve the goal of shifting out the labor demand curve. In addition, this earlier work acknowledges the potentially powerful political influence of public sector unions (see also Ehrenberg and Goldstein, 1975).

Of course, establishing evidence that living wage laws benefit unionized municipal workers does not necessarily imply that unionized municipal workers seek to establish living wage laws for their own benefit, to enhance their own rents. Other possible reasons unions may work on behalf of living wage laws are discussed below, although these hypotheses do not necessarily predict gains for unionized municipal workers. Empirical analysis can be informative as to the observed consequences of a particular behavior or policy change, but at best can only establish the consistency of the evidence with particular motives. However, to further distinguish the rent-seeking hypothesis from other possible explanations of changes in wages of unionized municipal workers, the effects of living wage laws on the wages of other groups of workers that would be expected to gain under alternative hypotheses, but not under the rent-seeking hypothesis, are also estimated and contrasted with the effects of living wage laws on unionized municipal workers.

II. Background on Living Wage Laws and Related Research

Living Wage Laws

Perhaps the defining feature of living wage laws is the high wage floors they set. Table 1 provides information on living wage laws for the largest cities in which these laws have been implemented. Specifically, Table 1 covers the 19 cities that are sufficiently large to study with the CPS.⁵ The wage levels associated with living wage laws are reported for these cities in the second column of Table 1.⁶ In many

⁵As explained below, the empirical analysis is restricted to city-quarter cells (for cities identified in the Current Population Survey) for which there are at least 100 observations in the Outgoing Rotation Group files. The 19 cities in Table 1 are those that have living wage laws and meet this criterion for at least some quarters.

⁶The analysis ignores county living wages, which are currently on the books in 14 counties. In many cases the counties covered are small, and in general county living wage laws have not attracted a great deal of attention, perhaps because the number of workers covered may be quite low. In the analysis in this paper, county living wage laws are only relevant if they cover workers in cities included in the data set but classified as not having living wage laws. The only county living wage law that clearly covers a city

cases (e.g., Hartford and Minneapolis) these wages are pegged to the poverty level for a family of a specified size. In addition, as already noted the required wage is sometimes higher if health insurance is not provided.⁷ The range of living wages—as of the end of calendar year 2000—is from a low of \$6.80 in Milwaukee to a high of \$9.92 in San Jose. To provide some perspective, the current federal minimum wage (as of the end of 2000) is \$5.15, and the highest state minimum wage is \$6.50 (Oregon). Thus, in many cities the living wage exceeds the prevailing minimum wage by 50 percent or more.

The other distinguishing feature of living wage laws is their coverage. As the last column of Table 1 shows, coverage is far from universal. The common element of most living wage laws is coverage of city contractors—usually general (e.g., Durham), but sometimes limited to service and perhaps construction contractors (e.g., San Jose, Baltimore), and in one case a much narrower set of contractors (Portland). In contrast, in only three of these cities (Dayton, Minneapolis, and San Antonio) do living wage laws fail to cover contractors. In addition, coverage is extended to city employees in only three of the cities in Table 1 (Dayton, Durham, and San Jose). On the other hand, in nine of the cities coverage is also extended to employers receiving some form of “business assistance”—in the form of subsidies, tax breaks, financial assistance, etc.⁸

included in those studied is in Miami-Dade County. In general, this problem should bias any estimated effects of city living wage laws toward zero, as the control group may actually include some individuals subject to living wages. Thus, the effects of living wage laws that are reported in this paper could be slightly understated. But given the large number of cities in the control group, this problem should be negligible, which was verified in estimates not reported in the tables in which Miami was excluded.

⁷In the empirical analysis reported in this paper, the lower wage with health insurance (if there is one) was used, but the qualitative conclusions were not sensitive to using the alternative higher wage.

⁸The living wage law in Minneapolis is classified in Table 1 as covering employers receiving business assistance. Interestingly, making the link between living wage laws and the deterrence of contracting out explicit, the Minneapolis law also states that “Work presently being performed by City employees may not be contracted out unless the contractors pay employees performing that work a living wage or the current City wage and benefits, whichever is higher.” Because this does not apply to all contractors but only those doing newly-privatized work—making it unclear how wages are regulated over the longer-run after work is contracted out—this law should be weaker, and is therefore not classified as generally covering contractors. However, the results were not qualitatively affected by this classification.

The Effects of Living Wage Laws—Predictions of Standard Theory, and Evidence

Standard economic theory predicts that there will be winners and losers from living wage laws. On the one hand, the higher wages mandated by living wage laws may make some workers better off. On the other hand, in the standard labor market model a government-mandated increase in the wage floor induces two types of effects, both of which reduce use of low-wage labor. The substitution effect occurs as employers substitute away from now-more-expensive low-wage labor, toward higher-wage labor or other inputs. The scale effect occurs because this substitution away from low-skilled labor and toward other inputs raises costs of production and hence prices, reducing demand for the product and therefore the overall scale of operation of the employer. The identities of the winners and losers, and the magnitudes of their gains and losses, therefore depend on the strengths of these effects and their incidence.

There are some unique features of living wage ordinances that may lead to smaller reductions in labor demand than a standard, broader wage increase—such as an increase in the minimum wage. To begin, there are two reasons why the scale effects may be moderated. First, the scale effect ultimately stems from cost increases caused by the substitution induced by the living wage. The conclusion that costs must increase is based on the assumption that employers were minimizing costs in the first place, which implies that the input choices (conditional on output) after the imposition of the minimum wage requirement must be higher cost, or they would have been chosen initially. However, it is conceivable that government contracting is done in a less competitive environment in which pressures to minimize costs are mitigated, in which case employers may find ways to offset the increased labor costs for low-wage labor by reducing costs in other dimensions. This idea has its origins in the X-inefficiency theory of Leibenstein (1978).⁹ Second, the extent to which price increases reduce demand depends on the elasticity of demand for the product. Because the city is the purchaser of goods and services from contractors, this demand curve may be highly inelastic over some range, either because the city finds it possible to raise taxes to cover higher

⁹For evidence on efficiency in the private vs. public sector, see, e.g., Bhattacharyya and Parker (1994), Hollas and Stansell (1994), and Kuo-Ping and Kao (1992).

costs, or because some services have to be purchased in quantities that may be largely insensitive to price.¹⁰

Aside from these reasons to expect moderated scale effects, living wage laws have limited coverage. This implies that some of the labor disemployed in the covered sector may shift into the uncovered sector, generating an outward shift in the supply of labor to the uncovered sector,¹¹ although the existence of a minimum wage may restrict the ability of wages to fall in this latter sector. In general, though, shifts to the uncovered sector have the effect of moderating overall employment losses, although also driving down wages, reducing both the gains and the losses associated with living wage laws. However, employment will not expand enough in the uncovered sector to offset fully the employment decline in the covered sector.¹²

Despite these considerations, the standard theory nonetheless still predicts that a higher living wage will cause average wages of low-wage workers to increase, and employment (and hours) of workers who would be employed at low wages to fall. Evidence reported in Neumark and Adams (forthcoming (a)) is broadly consistent with these predictions. In particular, positive wage effects are detected, although only in the cities in which living wage laws apply to employers receiving business assistance, rather than in the cities with narrower laws restricted to contractors, and negative employment effects appear to arise in response to the wage increases stemming from the broader living wage laws. Neumark and Adams (forthcoming (b)) consider the effects of these living wage laws on family incomes, rather than workers,

¹⁰Ehrenberg and Schwarz (1986) note that a perception that labor demand for public sector workers is wage inelastic—because of an absence of market constraints—led to the view that limitations should be placed on collective bargaining rights of these workers (Wellington and Winter, 1971), and indeed many states impose such restrictions. Ehrenberg (1973) is the first paper to estimate these labor demand elasticities, and characterizes these elasticities as suggesting that “market forces do not appear to be sufficiently strong to limit the size of real wage increases which state and local government employees may seek in the future” (1973, p. 378). However, in later work he notes that the elasticities are not clearly smaller (in absolute value) than those in the private sector (Ehrenberg and Schwarz, 1986, pp. 1256-7). Courant, et al. (1979) and Freeman (1986) delineate some factors that counter the presumed inelasticity of labor demand for public sector workers.

¹¹An exception is when workers leave the uncovered sector to “queue” for covered-sector jobs in sufficient numbers (Mincer, 1976). However, this requires—among other conditions—that work in the uncovered sector deters search in the covered sector.

¹²Formally, this is because when the supply of labor shifts out in the uncovered sector, the decline in wages leads some workers to choose non-employment (or reduced hours).

specifically asking whether these laws reduce urban poverty, and find some evidence of beneficial effects on net of the broader business assistance living wage laws.

There are two points to keep in mind in interpreting this earlier evidence coupled with the new evidence reported in this paper. First, there is not necessarily any contradiction between finding some beneficial effects for low-wage workers and low-income families, on the one hand, and gains for unionized city employees, on the other. Living wage laws could benefit low-wage workers or low-income families if the employment and hours responses to wage increases are sufficiently moderate, and the distribution of gains and losses across families generates some benefits for poor families. Rather, the rent-seeking perspective is meant to help clarify the political economy of living wage laws, asking whether there is evidence of gains for other groups that, among other things, suggests alternative motivations for the passage of these laws.

The second point to keep in mind in synthesizing this paper with the earlier evidence is that it is the possibility or “threat” of higher wage floors for contractors that is likely to generate benefits for unionized city workers. Thus, contractor living wage laws are the focus of this paper. Because contractor-only living wage laws do not appear to be associated with benefits for low-wage workers or low-income families, evidence that coverage of contractors by living wage laws benefits unionized city workers would tend to cast living wage laws limited to restrictions on wages paid by city contractors in a particularly negative light. Furthermore, the evidence that contractor-only laws do not result in detectable wage increases for low-wage workers does not imply that unionized municipal workers cannot gain from them. Indeed the gains to the latter workers would come about because contracting out is deterred, so higher wages paid to nonunion contractor employees need not be realized in order for the rent-seeking hypothesis to predict gains for unionized municipal workers.

Implicit in this argument is that the focus of many living wage laws on contractors is consistent with the rent-seeking hypothesis, because it is this particular feature of these laws that is likely to help unionized municipal employees. An alternative view is that contractor-only laws are the easiest to pass

because they are more limited, and hence serve as the initial goal of living wage campaigns to be followed later by broader laws. However, while this cannot be ruled out, there appear to be no cases of cities that first passed contractor-only living wage laws and that subsequently broadened their coverage. Another related possibility is that narrow contractor-only laws result from compromises in which advocates seek broader laws, but narrower ones result because of opposition from employer groups or other parties to the broader laws. It would be interesting, although difficult, to try to explore the relationship between the role of unions in particular living wage campaigns and whether these campaigns focused more on contractor-only laws, and more generally to try to determine the factors that lead to the passage of different types of living wage laws.

III. Union Support for Living Wages

The central evidence considered in this paper is the gains that accrue to unionized municipal workers from the implementation of living wage laws. However, if living wage laws partly reflect rent-seeking on the part of municipal unions, we would expect organizations representing unionized municipal workers to be involved in political efforts to pass living wage laws. This section describes some evidence of such activity.

As one method of assessing the involvement of unions with living wage campaigns, a simple set of Internet searches was conducted looking for joint mention of living wage campaigns and labor unions. This evidence is summarized in Table 2. Column (1) simply reports the number of hits for “living wage” and the name of each of the 19 cities listed in Table 1. Subsequently, names of various labor organizations were added to the list, beginning with the AFL-CIO, and then, based on preliminary searches of the first set of hits in column (1), specific unions that were mentioned often. As shown in columns (2)-(7), a relatively high fraction of hits involving living wages also mentioned the AFL-CIO or a specific labor union. In the absence of information on city contracts—which is difficult to come by—it is not entirely clear which unions might have the most vested interest in living wage laws. Interestingly, however, aside from the AFL-CIO, which is an umbrella organization, the largest shares of hits are associated with the two unions that play a

prominent role in organizing local government workers: the American Federation of State, County, and Municipal Employees (AFSCME), and the Service Employees International Union (SEIU).¹³

In and of itself, of course, the evidence in Table 2 says nothing about support for living wages on the part of unions. However, a casual perusal of the materials uncovered in the search documented in Table 2 indicates strong support. A sampling of quotes from these documents is provided in Table 3. They clearly document the active advocacy of labor unions in support of living wage campaigns. While this is not a random sample of quotes, statements paralleling those in Table 3 were plentiful, and in a broader and random sample of the Internet sites documented in Table 2, no statements by unions in opposition to living wage laws were uncovered.

The evidence presented in Tables 2 and 3 does not prove that unions back living wage laws as part of a rent-seeking strategy. Unions may have other incentives to back initiatives to pass living wage laws, such as preferences for less inequality in the wage structure, or for reductions in poverty or the incidence of low-wage work generally. Alternatively, union support for living wage campaigns may provide publicity, contacts, etc., that prove useful in future organizing drives, or in transforming the public image of unions from one of narrow self-interest to one with broader social goals (Nissen, 2000). Nonetheless, although it is probably not possible to discern the exact motives underlying union support for living wages, the evidence of this support at least suggests that the rent-seeking hypothesis may be plausible. With that in mind, the next section turns to the central analysis of this paper, exploring the consequences of living wage laws for unionized municipal employees.

IV. The Effects of Living Wage Laws on Wages of Unionized Municipal Employees: Data and Methods

Data

¹³Of course the numbers would change from day to day. But checking these numbers versus those obtained five months earlier revealed little qualitative change in the pattern. In addition, different search engines may yield different results. To see whether the qualitative conclusions were sensitive, Yahoo and Excite were also used to do the searches for Baltimore. Both had considerably fewer hits (1120 and 980 respectively). But the percentages accounted for by the various unions revealed similar patterns, with AFSCME and SEIU overrepresented by a factor of at least three relative to other individual unions.

The data used come from the Current Population Survey Outgoing Rotation Group (ORG) files extending from January 1996 through December 2000. The ORG files include approximately 13,000 households per month. In these files, residents of all “Standard metropolitan statistical areas” (SMSAs), encompassing all large- and medium-sized cities in the U.S., can be identified. Data on these residents are extracted for the empirical analysis. This assignment of living wages poses a couple of limitations. First, assignment of people to a metropolitan area based on where they live, rather than where they work, is appropriate to the extent that we are interested—as a policy matter—in how a living wage law affects residents of a city. However, classifying people based on where they work might better reveal direct effects of living wage laws, especially insofar as employees of firms covered by living wage laws seem more likely to work in the city. Also, the correspondence between cities and SMSAs is imperfect, but because suburban residents may work in the city, this is not necessarily problematic.¹⁴ Since January 1996, the design of the CPS has resulted in the large- and medium-sized metropolitan areas in the sample being self-representing (Bureau of the Census, 1997).¹⁵

Central to the analysis in this paper is the classification of workers by union status and municipal employment. Municipal workers are identified from the “class of worker” variable in the CPS, which refers to the primary job; having restricted the sample to those living in SMSAs, those working for “local government” are considered municipal employees, although some may work for other units of government below the state level. Union status is based on whether the individual reports being a “member of a labor union or an employee association similar to a union.”¹⁶

Considering first the individual data, the sample is restricted to those residing in SMSAs, aged 16-70. Observations with allocated values for the important variables are omitted. Only those working for a wage are included, dropping those with a computed wage less than \$1 or greater than \$100. When the wage

¹⁴For ease of exposition, SMSAs are often referred to as “cities.”

¹⁵The analysis is also restricted to begin in 1996 because for part of 1995 SMSA codes are unavailable in the ORG files, due to phasing in of a new CPS sample based on the 1990 Census.

¹⁶All of the empirical results were examined using a broader definition of union status including as well the small share of workers who are not union members but are covered by union contracts. The results were very similar and the conclusions unaffected.

had to be constructed from weekly earnings, usual weekly hours at the main job is used as the denominator. For those responding “hours vary” for this question, the hourly wage cannot be constructed and the observations are dropped.

Dependent Variable and Hypothesis to be Tested

Under the rent-seeking hypothesis, living wages reduce the ability of cities negotiating with unionized municipal workers to threaten to contract out work (or reduce the credibility of such threats). This would increase the bargaining power of unions, and hence result in higher wages. Thus, the empirical implication of the rent-seeking hypothesis is that living wage laws boost the wages of unionized municipal workers.¹⁷

Because of the relatively small number of unionized municipal workers, the empirical analysis of wages is done with city-level data constructed by quarter.¹⁸ Attention is focused on those unionized municipal workers who earn lower wages, as they are more likely to face competition from lower-wage non-union labor hired by city contractors, and, conversely, to benefit from raising the wage floor for this labor. In particular, the dependent variable used is the average wage of below-median wage (relative to their city-quarter) unionized municipal workers.

The hypothesis that living wages protect unionized municipal workers from lower-wage workers stems from the application of most living wage laws to city contractors and subcontractors. As Table 1 showed, there are a few cities for which living wages do not cover contractors. Consequently, most of the empirical analysis focuses on the subset of cities with contractor living wage laws.

¹⁷If living wage laws actually reduce the extent to which cities contract out jobs, by reducing the incentives to do so, then increases in this share in response to living wage laws might be observed. However, this is not a strong prediction of the rent-seeking hypothesis, because the threat need not be carried out (at least, not often) in order to affect bargaining. For this reason, it would probably not be informative to study changes in actual contracting out behavior. Aside from this consideration, data are not yet available with which to carry out such an analysis. The International City/County Management Association (ICMA) conducts a survey of “alternative service delivery” in U.S. cities (and counties), but the last survey was done in 1997, prior to most living wage laws, and the next survey will not be done until 2002. In a lengthier working paper version of this study (Neumark, 2001), no effects of living wages on the workforce share of unionized municipal workers is detected.

¹⁸The results from the living wage studies by Neumark and Adams (forthcoming (a) and (b)), which are based on data aggregated at the monthly level, are robust to aggregating the data quarterly.

Empirical Specification

The hypothesis will be tested using a straightforward difference-in-differences framework. In this framework, the effect of living wages—the treatment—is identified from how changes over time in cities implementing (or raising) living wages differ from changes over the same time period in cities without (or not raising) living wages. Using data for city-quarter cells indexed by city c in quarter q , the baseline regression estimated for the average wage measure (denoted w) is of the form:

$$(1) \quad w_{cqt} = \alpha + \beta \max[\ln(w_{cqt}^{liv}), \ln(w_{cqt}^{min})] + \gamma \ln(w_{cqt}^{min}) + C_c \delta + Q_q \lambda + \epsilon_{cqt} .$$

In this specification, w^{min} is the higher of the federal or state minimum wage and w^{liv} is the living wage.¹⁹ It is essential to control for minimum wages, because many cities with living wages are in states with high minimum wages, and the goal is to estimate the independent effects of living wages. The living wage variable that multiplies β is specified as the maximum of the (log of the) living wage and the minimum wage. In the sample period, living wages—when they exist—always exceed minimum wages, so this variable imposes the minimum as the wage floor in the absence of a living wage, although modifications of this specification are also considered.²⁰ C and Q are vectors of city and quarter dummy variables, with the latter containing a unique dummy variable for each quarter in the sample, and ϵ is a random error term. Variations of this specification are discussed as they are introduced in the empirical analysis.

A recent paper by Bertrand, et al. (2002) looks at the impact of serial correlation in the error term (and the data) across observations on the same unit (in this case, cities) on standard difference-in-differences estimators. It notes the usual result that serial correlation in the error biases the standard errors, and shows both analytically and via a simulation that because of the serial correlation in the treatment in a

¹⁹In the few cases of SMSAs that straddle states with different minimum wages (e.g., Philadelphia), a weighted average of the minimum wages in the two states is used, weighted by the shares of the SMSA population in each state (averaged over the months of 1996). The living wage and minimum wage variables were averaged over the months in each quarter.

²⁰Specifications were also estimated introducing a dummy variable for whether a living wage was in place, allowing a discrete intercept shift associated with living wages generally. This led to less precise estimates of the linear effect of living wages, but qualitatively similar conclusions.

typical difference-in-differences estimator—in this case in the living wage variable for a city—the standard errors could be seriously understated. Kezdi (2001) shows that unbiased estimates of the standard errors allowing an arbitrary serial correlation pattern in the error can be obtained easily, by “clustering” the data by city (rather than by city and month, as would be usual in this case). However, the resulting standard errors are conservative (if anything too large), because no structure is imposed on the serial correlation. This estimator was used for all of the results reported in the paper. It generally resulted in slightly higher standard errors, although occasionally they were smaller. Regardless, the qualitative conclusions were not affected by using these standard errors, rather the OLS standard errors; at most, some results significant at the 5-percent level using OLS standard errors are instead significant at the 10-percent level.

Distinguishing the Rent-Seeking Hypothesis from Other Hypotheses

The rent-seeking hypothesis asserts that living wage laws benefit unionized municipal workers by reducing the threat of contracting out and hence strengthening the hand of these workers in bargaining. However, a positive impact of living wage laws on unionized municipal workers could stem from other sources. For example, an apparent impact of living wage laws on wages of these workers could stem from changes in wages for workers in the wage range of unionized municipal workers that coincide with the passage of living wage laws. These changes could arise if cities simultaneously pass living wage laws and increase wages paid to their own workers, perhaps because of budgetary surpluses, or because cities that pass living wage laws (which typically do not cover city employees) are also sympathetic to their workers. Alternatively, if living wage laws have general effects on low-wage workers, there may be spillover effects onto the wages of unionized municipal workers, whether because of demand shifts, relative wage constraints, or some other reason. Given these possible alternative sources of an empirical association between living wage laws and wages of unionized municipal workers, it is critical to try to isolate the impact of living wage laws via the rent-seeking channel. A number of analyses are carried out to do this.

First, a large share of unionized municipal workers are teachers, police, and firefighters. Because of the inability of municipalities to contract out for services provided by these occupations, unionized

municipal workers in these occupations seem unlikely to require living wage laws to be protected from competition from lower-wage, typically non-union labor. Conversely, living wage laws should not lead to higher wages for unionized municipal employees in these occupations. Thus, most of the analysis is restricted to occupations other than these three. More importantly from the perspective of distinguishing among the competing hypotheses, the effects of living wage laws on wages of unionized municipal workers in these occupations are also estimated. Evidence that living wage laws boost wages of unionized municipal workers in other occupations, but not for teachers, police, and fire, would bolster the rent-seeking hypothesis.

Second, paralleling this strategy, the effects of living wage laws are also estimated for other groups of workers whose wages should not be affected by living wage laws under the rent-seeking hypothesis, but might nonetheless be affected by living wage laws under the alternative hypotheses described above. The three groups of workers considered are: unionized non-municipal workers; non-unionized non-municipal workers; and non-unionized non-municipal workers. If the effects for unionized municipal employees just discussed are attributable to the rent-seeking channel, then similar effects should *not* appear for these other groups of workers, whereas evidence of similar effects for these other workers would point to alternative explanations.

Third, the rent-seeking hypothesis focuses on an explicit mechanism for raising the wages of unionized municipal workers, namely the reduction of the threat from contracting out. In this view, living wage laws that do not apply to contractors should not raise the wages of these workers. Thus, while there are few such laws, further evidence isolating the rent-seeking hypothesis can be obtained by asking whether non-contractor living wage laws have similar impacts to those of contractor living wage laws. According to the rent-seeking hypothesis, they should not.

Finally, to address the alternative spillover hypothesis directly, the impact of living wage laws on wages of unionized municipal workers is estimated for contractor-only living wage laws exclusively, i.e., only those laws that cover contractors but do not cover either employers receiving business assistance or

city employees. This evidence distinguishes the rent-seeking hypothesis from spillovers because previous research (Neumark and Adams, forthcoming (a)) finds that contractor-only living wage laws do not affect wages of workers generally, even among the lowest-wage or lowest-skilled individuals. Rather, it is only the broader living wage laws (those covering employers that receive business assistance) that boost wages at the bottom of the distribution. Thus, it would be difficult to explain an effect of contractor-only laws on unionized municipal workers stemming from spillovers, while under the rent-seeking hypothesis such an effect is expected even in the absence of a detectable effect on general wages of the less-skilled.

Descriptive Information on Workers

Table 4 provides some descriptive information on workers classified by municipal employment and union status, detailing their occupational distribution and average wages. In addition, because living wage laws are likely to affect lower-wage workers, these descriptive statistics are presented for those workers earning below the median wage in the corresponding city-quarter cell. The first row of numbers in the table describes the workforce share of workers classified by union status and municipal employment. Looking first at all workers, in columns (1)-(4), the overall unionization rate in these data is .145, with 29 percent of union workers employed by municipalities. When attention is restricted to those earning below the median wage, the unionization rate drops, reflecting the union wage premium. The share of unionized municipal workers among this subset is particularly small, representing 1.9 percent of these lower-wage workers.

This low share highlights the inherent difficulty of using the CPS data to study a group of workers as narrow as unionized workers working for municipalities, in particular highlighting the relatively small number of workers from whom the effects of living wage laws on unionized municipal workers can be identified. For example, consider the analysis of wage effects on unionized municipal workers in affected occupations, earning below-median wages. For this analysis, there are 1,075 observations on individual workers in the control sample of cities never passing living wage laws. In the treatment sample in which living wages are passed, there are 306 observations prior to the implementation of the living wage, and 353 afterwards.

The remaining rows of columns (1)-(8) provide information on the occupational breakdown of workers based on union status and municipal employment. Each column reports the distribution of workers in that column. Looking first at all workers, the heaviest concentration of unionized municipal workers is among teachers, accounting for 41.3 percent of these workers. There are also high concentrations among executives, professionals excluding teachers, and police, as well as clerical workers, those in other services, and craft workers. Among lower-wage workers, though, as shown in columns (5)-(8), unionized municipal workers are much more concentrated among clerical workers and workers in other services; together these two occupations constitute 49.2 percent of unionized municipal workers earning below-median wages. Aside from the likelihood that living wage laws affect lower-wage workers, this provides another motivation for restricting attention to those earning below-median wages, as the higher-wage unionized municipal workers are heavily representative of occupations for which contracting out is not a factor—such as teachers and police, as noted earlier.

The other columns of Table 4 (columns (9)-(16)) report descriptive information on wages by union status, municipal employment, and occupation. There are no real surprises here in terms of the occupational wage differentials, or those associated with union status or municipal employment. What is informative, though, is a comparison of some of these wages with the legislated living wages reported in Table 1. In particular, once attention is restricted to those earning below-median wages, average wages for most occupations appear to be in the range where living wages might pose a binding constraint for a reasonable fraction of non-unionized non-municipal workers. At the same time, the wage floors imposed by living wages are close enough to the wages of unionized municipal workers to believe that it might be possible to detect benefits to these workers from the imposition of living wages, if these benefits exist. In contrast, the average wages of unionized municipal workers overall are sufficiently high that it seems less plausible that living wage laws would have any detectable effect on the higher-wage members of this set of workers.

Descriptive Information on Cities

Having provided some descriptive information on unionized municipal workers, Table 5 reports descriptive statistics for the city-level data that are used in the empirical analysis. The sample is restricted to city-quarter cells in which there are at least 100 observations, in an attempt to increase the accuracy of the estimates; even so, these cells sometimes contain very few unionized municipal workers.²¹

Panel A provides some general information on wages for cities with any living wage laws, prior to and following the initial implementation of the living wage, for the same breakdown focusing only on living wage laws covering contractors, and for cities with no living wage laws in the sample period. The wage figures are deflated by the average hourly earnings series, with the first quarter of 1996 used as the base. These figures suggest that living wages (either in total, or only those covering contractors) were implemented in cities with wages that were higher by about five percent, and wages of workers below the 10th centile that were higher by about three percent.

Panel B reports average wages for the group of workers that will be the focus of the analysis, those earning below-median wages, and excluding teachers, police, and fire. These figures indicate that living wages were implemented in cities in which these workers earned only slightly higher wages (about one percent), again suggesting similarity of the treatment and control groups. These figures fail to reveal any wage increase for these workers following the implementation of a living wage. Of course this need not hold in the regression analysis, which accounts for common changes over time and city-specific differences in these average wages, which can affect the estimates in Table 5 because different cities are in the “Pre” and “Post” columns for different numbers of quarters.

V. The Effects of Living Wage Laws on Wages of Unionized Municipal Employees: Evidence

Basic Results

Having laid the groundwork, this section reports results from the explicit test of the rent-seeking hypothesis regarding living wages, using variants of the specification given by equation (1). The basic results are reported in Table 6, as noted before for below-median wage unionized municipal workers, in

²¹All of the analyses in the paper were repeated using cutoffs for the number of observations per cell of 50, 75, 125, and 150. The estimates were similar and the qualitative conclusions were unchanged.

occupations excluding teachers, police, and fire. In the first three panels of column (1) three separate specifications are estimated, using the contemporaneous living wage variable (and minimum wage variable), followed by a specification with 2-quarter lags, and a specification with 4-quarter lags. These alternative lags allow the effects to take place some time after a living wage is implemented or increased. For example, in earlier work (Neumark and Adams, forthcoming (a) and (b)) the general effects of living wages on wages, employment, and also family income took about one year to appear.

In column (1), the contemporaneous effect of the living wage on the wages of unionized municipal workers is large, with an elasticity of .137, and statistically significant at the 10-percent level. The 2-quarter lag specification still points to a positive effect, although smaller and no longer statistically significant, consistent with some moderation of the wage effect (although easily attributable to sampling variation as well). The 4-quarter lag specification points to a somewhat larger effect, statistically significant at the 10-percent level. In contrast, the minimum wage effects are always statistically insignificant and imprecisely estimated.

Given the generally persistent effects of living wages, the last panel includes simultaneously the contemporaneous and two lagged living wage variables (and the corresponding minimum wage variables). It reports their overall statistical significance, and, most important, the estimated summed effect and its standard error. This specification points to a relatively large positive effect of living wages on the wages of unionized municipal workers, with an elasticity of .169, which is statistically significant at the 5-percent level. This estimate implies, for example, that implementation of a living wage that exceeds the minimum wage by 30 percent—which is not uncommon—would raise wages of these workers by approximately 5.1 percent.

Because the impact of the minimum wage is small and statistically insignificant, in column (2) it is dropped to obtain more precise estimates of the living wage effect. The estimated impact of the living wage is very similar and is more precise. For example, in the last panel the elasticity is .174, and statistically significant with a smaller standard error than in column (1). Thus, the evidence from these specifications is

quite clearly consistent with the rent-seeking hypothesis regarding unionized municipal workers and living wages.

Sensitivity Analysis

Table 7 turns to a variety of sensitivity analyses of this finding, in all cases focusing only on the specification with the contemporaneous and two lagged effects, comparable to the last panel of Table 6, and using the more parsimonious specification excluding the minimum wage variable.²² Panel A looks at the sensitivity of the results to differences in the specification, sample definitions, and classification of treatment and control groups. First, the difference-in-differences strategy is predicated on the assumption that absent the living wage, and aside from differences captured in the other control variables (including city dummy variables), the treatment and control groups are comparable. In discussing the descriptive statistics for cities in Table 5, it was noted that there were only small differences between wages of unionized municipal workers in cities that never imposed a living wage, and in cities that imposed a living wage, in the period prior to doing so. Of course fixed differences between the treatment and control groups would be captured in the city dummy variables regardless. A potentially more troublesome difference is one in the time pattern of changes. As the specification only includes year and quarter dummy variables assumed to have the same effects across all observations, a difference in time trends between treatment and control groups would tend to be attributed to the effects of living wages, with the direction of the bias unknown a priori. To test for different time trends, the sample was restricted to include only the control group and the pre-living wage treatment group. An interaction between a time trend and a dummy variable for cities later implementing living wages were added to the specification. The living wage variable was dropped because all observations are taken prior to the introduction of a living wage, and the time trend itself is not included because the specification already includes quarter dummy variables. The estimated coefficient of the time trend interaction provides a test of differential time trends in the treatment and

²²Excluding the minimum wage had virtually no impact on the estimates reported here, and increased their precision.

control group. In all cases, this estimated coefficient was small and not significantly different from zero, which bolsters the validity of the research design.

Taking this one step further, the specification was also estimated for the whole sample period, including the living wage and time trend variables, to see whether the estimated wage effect persisted in this more flexible specification allowing different time trends in the treatment and control groups. Results are reported in Panel A, column (1), of Table 7. The estimated time trend in cities passing living wages is negative but not statistically significant. Moreover, the estimated impact of living wages on wages of unionized municipal workers does not weaken but rather strengthens, and remains statistically significant despite the expected increase in the standard error of the estimate associated with including the time trend interaction. While not significant, the negative sign suggests that contractor living wage laws have tended to be passed in cities where wages of unionized municipal workers were declining relatively more, which might be expected if these laws are intended to prop up or increase wages earned by these workers. This would also explain why the estimated impact of living wage laws is larger with the time trend included, as living wage laws appear to boost wages of unionized municipal workers relative to a declining trend. Finally, column (2) expands the specification by including a quadratic term as well, to allow for a non-linear trend; this has no additional impact on the results.

Column (3) restricts attention to a subset of affected occupations with the lowest average wages, specifically wages below \$8.25 for non-unionized non-municipal workers (see Table 4). Unless for some reason these particular occupations are not open to competition from city contractors, it would be expected that the positive wage impact of living wage laws would be present for this group of occupations, and most likely larger. This is confirmed by the estimated impact of living wages, which is larger than the comparable estimate in Table 6 (.222 vs. .174), and statistically significant at the 5-percent level. In column (4) another lag of the living wage variable is added, to see whether there is any evidence that the

positive effect of living wages weakens over time. The estimated impact using the 6-quarter lag is essentially unchanged.²³

Because the data are grouped by city-quarter cell, all of the estimates reported to this point are weighted by the number of observations used in each cell. This yields a more representative regression estimate, as a larger city-quarter cell represents more individuals, and is econometrically correct as it should yield efficient estimates. To gauge the sensitivity of the estimation to this weighting, though, column (5) reports unweighted estimates, in which the standard error rises somewhat and the estimated effect falls, with the combined result that the overall impact becomes statistically insignificant. The increased standard error is not surprising given the loss of efficiency, but given that the key right-hand-side variables are policy variables and not estimated from the data, the sensitivity of the coefficient estimates points to some possible non-robustness of the results to putting less weight on larger cities (rather than a greater influence of measurement error in the unweighted regressions). Taking this issue one step further, in results not reported in the table, the wage effects were re-estimated dropping cities with contractor living wage laws one at a time. Looked at in this way, the finding of a positive impact of living wages on wages of unionized municipal workers was very robust, as the estimated impact corresponding to the combined contemporaneous and lagged effects ranged from .124 to .209, and the t-statistics ranged from 2.26 to 3.19.

While Table 1 described living wage laws that have been passed by cities, some developments have inhibited the implementation of these laws in a few locations. In particular, Buffalo enacted its laws in August 1999, but the city is currently being sued for not yet implementing the law. In Omaha, the living wage ordinance was enacted in April 2000. It was subsequently repealed by the City Council, after which the mayor vetoed the repeal and the City Council overrode the veto. Thus, while the law may be having an impact because of continuing uncertainty, it is not currently in effect. Finally, in St. Louis a law suit based on a state law barring living wage ordinances led to a ruling that the living wage law passed in July 2000 was unenforceable, while leaving open the ability to write a new law. While none of these cities should

²³The estimate (standard error) for the specification in Table 6, column (2), with the comparable sample for which the 6-quarter lag can be estimated is .177 (.070).

have much impact on the estimates, since they all passed living wage laws late in the sample period, it is worth checking that the results are not sensitive to alternative ways of classifying them. This is done in the last two columns of Panel A of Table 7, first simply excluding these cities from the analysis, and then instead treating them as if there is no living wage, and including them in the control group. As the estimates show, in neither case do the results change more than negligibly.²⁴

Panel B of Table 7 reverts to the basic specification and sample, but considers alternative cut-offs for “lower-wage” unionized municipal workers, substituting centiles ranging from the 30th to the 90th for the median. The results indicate that at the extremes the positive impact on wages becomes small and insignificant, but that evidence of this effect emerges as long as the range extends through the middle part of the wage distribution.

Because in the analysis of wages of unionized municipal workers the dependent variable must fall below a certain level (the city-quarter median in most of the analyses), there is potential bias from endogenous selection. In particular, some fraction of workers whose wages are raised by a living wage law may be lifted above the median centile, biasing downward any positive effect of the living wage. As the results ultimately point to a positive effect of living wages on below-median wages, the evidence would likely only be stronger in the absence of this bias. Nonetheless, the estimates should be interpreted carefully as simply measuring the effect on the average wage of workers whose wages are below the specified cut-off, rather than measuring a population regression function.

An alternative that avoids this problem is to use the predicted wage distribution, rather than the actual wage distribution. The cost of this is that using the predicted wage distribution probably results in the inclusion in this lower range of more potentially unaffected workers, thus also biasing any positive effects downward. Panel C of Table 7 reports results substituting wage cut-offs based on predicted wages rather than actual wages. The estimated positive impacts are still present, although in this case when workers higher up in the (predicted) wage distribution are included. This suggests that there is downward

²⁴Another issue regarding the definition of the living wage variable concerns the city-quarter cells in which a living wage was in effect for only a fraction of the months in the quarter. The specification was re-estimated dropping the two cells for which this was the case, with effectively no impact on the estimates.

bias in the estimated wage impacts when actual wages are used, indicating that the problem of workers whose wages are lifted above the median is empirically more important. A second alternative is to analyze lower-wage occupations without a wage cut-off, although given that there are numerous high-wage workers in even the lowest-wage occupations, this is more likely to include unaffected workers and hence obscure the living wage effect. Thus, the wage equation was also estimated for the lower-wage occupations considered in column (3) of Table 7, without the median wage cut-off. The estimated wage effects were still positive, but about half as large (.105) and not statistically significant.

Overall, the positive impact of living wage laws on the wages of unionized municipal workers persists in most of the sensitivity analyses reported in Table 7. Nonetheless, the combined evidence in Panels B and C points to some fragility or perhaps “narrowness” of the inference that living wage laws boost wages of unionized municipal workers, and it remains an open question why the lowest-wage among these workers appear not to benefit from living wage laws. Unfortunately, there is not a sufficiently large data set to support the highly disaggregated analyses that might shed further light on this question.

Distinguishing Among Alternative Hypotheses

To this point, the preponderance of the evidence points to positive effects of living wage laws on wages of unionized municipal employees. The central hypothesis that this paper pursues—consistent with both the active involvement in living wage campaigns of unions that organize municipal workers, and with the evidence just presented—is that living wage laws protect or increase the rents earned by unionized municipal workers.

As discussed above, to distinguish this explanation of the effects of living wage laws on wages of unionized municipal workers from other hypotheses, it is useful to estimate the effects of living wage laws on other groups of workers. This analysis is reported in Table 8. In particular, similar specifications to those in Table 6 are reported, but for various groups of workers (discussed earlier) whose wages should *not* be affected by living wage laws under the rent-seeking hypothesis, but whose wages *would* be increased under some of the alternative hypotheses.

Panel A of Table 8 first presents estimates for unionized municipal workers earning below-median wages, as before, but now looking exclusively at teachers, police, and fire. Workers in these occupations were excluded earlier because they seem unlikely to face competition from lower-wage non-union labor, and therefore contractor living wage laws should not increase their wages via higher rents. The estimates indicate no effect of living wages on the wages paid to this group, as the estimated coefficients are negative rather than positive, and insignificantly different from zero (albeit imprecise). Columns (2)-(4) turn instead to the three other groups of workers classified by union status and municipal employment. In this case teachers, police, and fire are included because they represent much smaller workforce shares. For none of the three groups—unionized non-municipal workers, non-unionized municipal workers, or non-unionized non-municipal workers—is there evidence that living wage laws boost wages. In these regressions the estimates are relatively precise, and insignificantly different from zero.²⁵ In contrast to the rent-seeking hypothesis, if cities were passing living wages while raising wages of their own lower-wage workers simultaneously, we would see wages of non-unionized municipal workers rising concurrently with living wages, and if living wage laws were generating general wage increases that spilled over to unionized municipal workers, we should see evidence that living wage laws are also associated with increased wages of non-unionized non-municipal workers.

Column (5) of Panel A returns to the unionized municipal workers in the affected occupations that were analyzed earlier. However, in this case attention is restricted to the few living wage laws that do *not* cover city contractors. In these instances, we would not expect to see any positive effect on the wages of unionized municipal workers under the rent-seeking hypothesis, whereas if cities implement living wages and wage increases simultaneously, or if we are simply detecting spillovers, a positive wage effect should still appear. Consistent with the analysis of other groups of workers whose wages should not be affected under the rent-seeking view of living wage laws, the evidence points to no effect of non-contractor living wage laws on wages of unionized municipal workers, as the estimated living wage coefficient is negative,

²⁵The evidence for non-unionized non-municipal workers does not contradict that reported in Neumark and Adams (forthcoming (a)), which reports positive wage effects for workers only in the lowest decile of the wage distribution.

small, and statistically insignificant. However, these estimates should be interpreted cautiously, as they are based on only three cities that have non-contractor living wage laws.²⁶

Finally, Panel B of Table 8 (column (6)) turns to what might be considered more direct evidence on the rent-seeking vs. spillover hypotheses, estimating the impact of living wage laws on wages of unionized municipal workers exclusively for contractor-only living wage laws. Recall that this distinguishes the rent-seeking hypothesis from spillovers because previous research finds that contractor-only living wage laws do not affect wages generally, even among the lowest-wage or lowest-skilled individuals, so that spillovers would not explain an impact of contractor-only living wage laws on wages of unionized municipal workers. The evidence reported in the last column of the table indicates that contractor-only living wage laws have as strong an effect on wages of unionized municipal workers as do living wage laws (with contractor coverage) generally, as the estimate of .156 is statistically significant, and only a shade smaller than the overall estimates in Table 6.

This last evidence also addresses to some extent the issue of union motives in working for the passage of living wage laws. As noted earlier, a more benign view of union support for living wages than the rent-seeking hypothesis is that unions push for living wages to benefit low-wage workers generally.²⁷ The evidence in the last column of Table 8, though, coupled with the earlier evidence on general effects of living wages on low-wage, low-skill workers, implies that the most common type of living wage law—namely narrow, contractor-only laws—delivers benefits *only* to unionized municipal workers.²⁸ While this does not disprove the proposition that union support for living wage laws is predicated on a belief that

²⁶Another possibility is that the results for contractor living wage laws for unionized municipal workers are driven by the two cities that also cover city employees (Durham and San Jose), in which case the mechanism would be direct wage increases, rather than the extraction of higher rents via raising the price of non-union contractor labor. Of course, the results in column (2) of Table 8 (Panel A) for non-unionized municipal workers suggest that this is not the case. In addition, when these two cities were dropped and the specifications in Table 6 re-estimated, the effects of living wage laws on wages of unionized municipal workers were stronger, not weaker.

²⁷This is certainly consistent with the quote from David Newby reported in Table 3.

²⁸In results not reported in the table, the specifications in columns (1)-(4) of Panel A were estimated including only contractor-only laws in the treatment group, as in Panel B. Like in the results reported in the table, in no case did the evidence point to positive wage effects for any of these alternative groups of workers.

even contractor-only living wages deliver benefits to all workers, it does establish that there is not an empirical basis for this belief.

V. Conclusion

Living wage laws, which were introduced in the mid-1990s and have expanded rapidly since then, are typically touted as anti-poverty measures. Yet they generally cover employers with city contracts, and frequently restrict coverage to these employers only, in which case they apply to a small fraction of workers. Because the anti-poverty goals would appear to call for broader wage floors, a natural question is whether there are alternative motivations for various economic and political actors to seek passage of living wage laws covering city contractors.

This paper considers the hypothesis that unions representing municipal employees work for the implementation of living wage laws as a rent-seeking activity. In particular, the hypothesis is that by raising the wages that city contractors would have to pay, living wage laws may reduce the incentives for cities to contract out work that would otherwise be done by unionized municipal employees, hence increasing the bargaining power of municipal unions and leading to higher wages (and perhaps also a higher employment share of unionized municipal workers). Indeed narrow living wage laws may generate this latter effect without delivering benefits to low-wage workers and low-income families.

Labor unions representing municipal workers are, in fact, very active in the movement to pass living wage laws. But this provides only prima facie evidence in favor of the rent-seeking hypothesis. The main contribution of the paper is an empirical analysis of the effects of living wage laws on unionized municipal workers.

The evidence indicates that wages of unionized municipal workers are increased as a result of contractor living wages. In particular, focusing attention on those unionized municipal workers in the lower to middle part of the wage distribution in their local labor market, and on those occupations most likely to be affected, the evidence indicates elasticities of average wages with respect to living wages centered around .15 to .17. This finding generally holds up in a variety of sensitivity analyses. On the other hand,

comparisons of estimated effects for unionized municipal workers who—under the rent-seeking hypothesis—should be affected by living wages, with estimated effects for alternative groups of workers that should not experience any impact under this hypothesis, uniformly indicate positive effects only for the former, making more plausible a causal interpretation of the estimated impacts of living wage laws on unionized municipal workers based on rent-seeking behavior.

The evidence that unionized municipal workers gain from living wage laws does not imply that living wages offer no assistance to low-wage workers or low-income families. Indeed, there is evidence that living wage laws help to achieve these latter goals, although not when they are narrowly restricted to cover only city contractors, but instead when they extend to employers receiving business assistance from the city (Neumark and Adams, forthcoming (a)). Thus, this evidence should not be interpreted as condemning living wage laws as nothing but a ploy for unionized municipal workers to protect themselves against competition from lower-wage labor that cities might access through contracting out. However, it does add to the literature on “political economy” interpretations of labor market and other policies (e.g., Brock and Magee, 1978; Goldin, 1994, Fishback and Kantor, 1998). Moreover, it may help in understanding the evolution of living wage laws and, in particular, the narrow coverage restrictions they frequently entail that appear to undermine the anti-poverty effects of living wages while still delivering benefits to unionized municipal workers.

Finally, though, the evidence does suggest that one narrow group that is not the overt intended beneficiary of living wage laws exerts political pressure on behalf of these laws and gains from them. This, in turn, makes it more plausible that alternative policies intended to achieve the goal of reducing urban poverty may be more effective, as living wage laws may result more from considerations of self-interest of narrow but politically-powerful groups of workers than from consideration of the optimal way of achieving this goal.

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Table 1: Living Wage Cities in Analysis Sample

<u>City</u>	<u>Wage provisions</u>	<u>Coverage</u>
Baltimore	Passed in December 1994 but wage requirements were as follows: July 1995 (6.10) July 1996 (6.60) July 1997 (7.10) July 1998 (7.70) July 1999 (7.90)	Construction and service contracts > \$5000
Boston	September 1998 (8.23) July 1999 (8.35) July 2000 (8.53)	Contractors > \$100,000; subcontractors > \$25,000
Buffalo	January 2000 (6.22)	Contractors and subcontractors with at least 10 employees, contracts > \$50,000. Non-profits may be exempted.
Chicago	July 1998 (7.60)	Contractors and sub contractors
Dayton	April 1998 (7.00)	City employees. City manager directed to use living wage as guidelines for city employee wages if it falls within long-range city financial plan
Denver	100% of poverty line for family of four, based on 2080 annual hours: March 2000 (8.20)	Service contractors and subcontractors ≥ \$2,000. Specified as covering parking lot attendants, security guards, childcare workers, clerical support workers
Detroit	100% of poverty line for family of four with health benefits, 125% without: December 1998 (8.23) March 1999 (8.35) March 2000 (8.53)	Contractors, subcontractors, and financial assistance recipients > \$50,000
Durham	January 1998 (7.55)	Contractors, city employees
Hartford	110% of poverty line for family of four: September 1999 (9.19) March 2000 (9.38)	Contractors > \$50,000; commercial development projects receiving subsidies > \$100,000
Los Angeles	Indexed annually for inflation. Initial wage set to 7.25 with health benefits, 8.50 without: April 1997 (7.25) June 1998 (7.37) June 1999 (7.49) June 2000 (7.69)	Service contractors > \$25,000; assistance > \$100,000 or \$1 million lump sum
Milwaukee	Set to 100% of poverty level for family of three on March 1 of each year, based on 2080 annual hours: December 1996 (6.05) March 1996 (6.24) March 1997 (6.41) March 1998 (6.56) March 1999 (6.67) March 2000 (6.80)	Contractors and sub contractors > \$5,000

Table 1 (continued)

<u>City</u>	<u>Wage provisions</u>	<u>Coverage</u>
Minneapolis	100% of poverty level for family of four with health benefits, 110% without: April 1997 (8.03) March 1998 (8.23) March 1999 (8.35) March 2000 (8.53)	Assistance > \$25,000, as of December 1998; > \$100,000 initially
Oakland	Initially set to 8.00 with health benefits and 9.25 without, upwardly adjusted by prior December 31 to December 31 change in the Bay Area CPI: April 1998 (8.00) April 1999 (8.15) April 2000 (8.35)	Contractors > \$25,000; assistance > \$100,000
Omaha	June 2000 (8.19)	Contractors and financial assistance recipients with 10 or more employees, contracts or subsidies > \$75,000
Portland	July 1996 (7.00) July 1998 (7.50) July 1999 (8.00)	Custodial, security, and parking attendant contracts
San Antonio	9.27 to 70% of service employees in new jobs, 10.13 to 70% of durable goods workers: August 1998 (9.27)	Businesses receiving tax breaks
San Francisco	Initially set to 9.00 without benefits, rising to 10.00 in 12-18 months, plus 1.25 without health benefits: November 2000 (9.00)	Service contractors > \$25,000 (\$50,000 for non-profits); airport leaseholders; home healthcare workers
San Jose	9.50 with health benefits; 10.75 without. Reset each February to the new poverty level for a family of three and adjusted upward for higher San Jose cost of living – approximately a 45% premium: December 1998 (9.50) March 1999 (9.68) March 2000 (9.92)	Service contractors > \$20,000; assistance > \$100,000 (excludes trainees and workers under 18); city employees
St. Louis	130% of poverty level for family of three, based on 2080 annual hours, higher without health benefits: August 2000 (8.84)	Service contractors > \$50,000; assistance > \$100,000

HHS poverty guidelines were used for computing wages based on percentages of poverty threshold. Unless otherwise noted, living wages indexed to the poverty line are based on 2000 annual work hours. Effective dates of enacted living wages are shown. When there are alternative living wages, the lower one is reported (e.g., the wage with health benefits). For more complete information on living wages across all cities, see Neumark and Adams (forthcoming (b)). This table covers those cities with living wages for which there were 100 or more observations on workers in at least some quarters of the CPS Outgoing Rotation Group files for the period 1996 through 2000, conditional on the sample restrictions described in the text.

Table 2: Evidence on Union Involvement in Living Wage Campaigns Based on Internet Search Hits

	Total hits by city (name of city + “living wage”)	Percentage of total hits from column (1)					
		(1) + AFL-CIO	(1) + AFSCME	(1) + SEIU	(1) + IBEW	(1) + UFCW	(1) + HERE
City:	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Baltimore	2560	15.9	6.2	5.2	1.3	1.6	1.8
Boston	5290	16.0	4.3	5.1	1.4	1.5	1.7
Buffalo	1300	13.2	5.8	5.0	1.5	2.2	2.2
Chicago	6140	16.9	4.5	6.3	1.4	1.7	1.7
Dayton	715	15.5	6.4	7.1	1.7	1.3	0.8
Denver	1760	12.3	4.6	5.5	1.0	1.4	1.1
Detroit	2980	16.2	5.6	6.8	2.0	2.2	1.9
Durham	676	12.3	5.8	5.3	0.7	2.2	0.9
Hartford	835	16.9	7.1	10.1	1.2	2.0	3.1
Los Angeles	6650	19.7	4.4	6.9	1.2	1.7	2.8
Milwaukee	1680	20.5	9.0	8.5	1.8	2.9	2.5
Minneapolis	2350	12.3	4.7	3.9	0.9	1.4	1.6
Oakland	2890	13.1	4.2	5.9	1.1	1.8	1.8
Omaha	316	11.7	4.1	4.1	1.3	3.5	1.6
Portland	2680	10.5	4.3	4.4	1.1	1.5	0.8
San Antonio	1370	11.9	3.0	3.1	0.8	1.2	1.0
San Francisco	5960	15.8	4.1	6.1	1.0	1.3	1.7
San Jose	1600	19.0	6.3	10.1	1.7	2.8	2.1
St. Louis	1560	16.2	5.3	5.3	1.9	2.2	1.5

The Google search engine was used to compile these figures, on April 3, 2001. Figures in columns (2)-(7) were based on adding the specific union to the search specification. Union abbreviations are as follows: AFSCME: American Federation of State, County, and Municipal Employees. IBEW: International Brotherhood of Electrical Workers. SEIU: Service Employees International Union. UFCW: United Food and Commercial Workers Union. HERE: Hotel Employees and Restaurant Employees. The acronym for the union was used in all cases except for HERE.

Table 3: Sampling of Quotes Documenting Union Involvement in Living Wage Campaigns

In 1994, after more than a year of coalition building, lobbying and negotiating, AFSCME and the community grassroots organization BUILD (Baltimoreans United in Leadership Development) convinced the city council to pass an ordinance requiring companies that have contracts with the city to pay workers \$6.10 an hour (rising to \$7.70 this year).

[“Transforming Low Pay into a *Living Wage*,” AFL-CIO website, www.aflcio.org/articles/am_at_work/corp_transforming.htm]

A campaign has started in Greensboro to pass a living wage ordinance. The Triad-Central Labor Body is leading the effort. The Triad CLB is a coalition of all area unions affiliated with the AFL-CIO. The CLB is in the early stages of pulling together information and reaching out to local community activists and organizations.

[“Living Wage Rage Comes to Greensboro,” Pete Castelli, United Needleworkers, Industrial, and Textile Employees, www.ibiblio.org/prism/may98/living.html]

David Newby, State AFL-CIO President, explained why the labor movement was leading the charge for a living wage even though most union members are earning more than the campaign's goal of 110 percent of the poverty level. “The fundamental purpose and goal of unions is to raise the standard of living and the quality of life for all working people. Our basic value is a fair day's wage for a fair day's work,” said Newby.

[“Living Wage Campaigns: National Trend, Local Focus,” Living Wage Reporter, Oct./Nov. 1997, [www.solidarity.com/LivingWage/Oct.%201997.htm#Living Wage Campaigns: National Trend, Local Focus](http://www.solidarity.com/LivingWage/Oct.%201997.htm#Living%20Wage%20Campaigns%3A%20National%20Trend%2C%20Local%20Focus)]

Things are looking up in New Orleans, where an August court date has finally been set to determine the constitutionality of a Louisiana law prohibiting enactment of local living wage ordinances. The law was passed after ACORN and SEIU Local 100 collected enough signatures to put the living wage on the ballot in New Orleans back in 1996.

[“Living Wage Campaigns Rage on in ACORN Cities and States,” ACORN Report, July 1999, www.acorn.org/acorn-reports/acornrep.livingwage.content.html]

The Los Angeles living wage initiative has been led by the Los Angeles Alliance for a New Economy (LAANE). LAANE is a non-profit organization created by the Hotel Employees and Restaurant Employees Union (HERE) Local 11 in 1993 with the goal of creating a more favorable climate for organizing low-wage workers in Los Angeles' second largest sector, the tourism industry.

[“Living Wage Campaigns in the Economic Policy Arena: Four Case Studies from California,” Carol Zabin and Isaac Martin, Center for Labor Research and Education, Institute of Industrial Relations, UC Berkeley, June 1999, www.phoenixfund.org/livingwage.htm]

Table 4: Distribution of Workers by Unionization, Municipal Employment, Wages, and Major Occupation

	Occupational distribution								Average wages							
	All				Below median wage				All				Below median wage			
	U,M	U,NM	NU,M	NU,NM	U,M	U,NM	NU,M	NU,NM	U,M	U,NM	NU,M	NU,NM	U,M	U,NM	NU,M	NU,NM
Workforce share:	.042	.103	.043	.813	.019	.064	.040	.877								
Occupations:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
Executive, administrative	.055	.052	.145	.161	.028	.026	.057	.073	20.09	18.72	18.39	18.82	10.66	10.08	8.53	8.95
Professional, excl. teachers	.093	.078	.121	.121	.053	.026	.059	.041	20.26	20.81	17.42	20.19	10.46	9.91	8.45	8.71
Teachers	.413	.046	.195	.025	.230	.023	.128	.023	19.87	19.51	14.18	13.82	10.28	9.61	8.05	7.63
Technical	.013	.031	.023	.040	.012	.022	.021	.025	16.19	21.19	12.48	15.06	10.80	10.17	8.34	8.92
Sales	.003	.051	.010	.134	.007	.101	.017	.150	13.01	10.82	8.25	12.03	8.84	7.82	6.80	6.85
Clerical	.127	.148	.217	.157	.313	.165	.324	.194	11.85	13.58	9.47	10.30	9.45	9.61	7.68	8.10
Private household	.000	.001	.000	.007	.000	.002	.000	.012	...	6.49	...	6.67	...	6.49	...	5.88
Protective services, excl. police and fire	.007	.006	.016	.008	.018	.013	.027	.011	12.13	10.92	7.92	8.37	9.20	8.11	6.69	6.94
Police	.095	.012	.044	.001	.035	.003	.022	.001	19.16	17.64	14.17	16.64	11.18	10.42	8.61	9.82
Fire	.036	.001	.009	.000	.023	.001	.007	.000	16.43	15.27	12.80	12.98	9.38	9.03	7.63	7.49
Other services	.062	.090	.121	.116	.179	.200	.228	.194	10.62	10.67	7.33	6.70	8.86	8.01	6.59	6.00
Craft	.041	.208	.037	.090	.024	.093	.026	.078	16.72	17.58	13.24	12.19	10.69	9.78	8.23	8.27
Machine operator	.003	.117	.004	.055	.004	.139	.005	.075	14.62	13.16	10.77	8.49	10.20	8.76	6.97	7.27
Transportation	.036	.080	.033	.031	.051	.061	.041	.039	13.56	15.15	9.89	9.64	10.29	9.67	7.97	7.71
Handler	.011	.075	.011	.039	.016	.120	.018	.062	13.83	11.85	8.22	7.54	10.07	7.92	7.03	6.75
Farming, fishery	.006	.005	.013	.014	.009	.009	.020	.022	13.04	11.12	8.65	7.21	9.76	8.46	6.76	6.44

U,M: unionized, municipal. U,NM: unionized, non-municipal. NU,M: non-unionized, municipal. NU,NM: non-unionized, non-municipal. Occupational distribution shows share in each category of workers (e.g., unionized, municipal) in each listed occupation. Sample-weighted estimated proportions are reported. Wage measures (except living wages and minimum wages) are deflated by the average hourly earnings series, and are expressed in 1996:Q1 terms. Medians are estimated by city-quarter cell, using sample weights. The sample is restricted to individuals residing in metropolitan areas. The overall sample size is 375,483. The sample size for observations below the (weighted) median is 183,315.

Table 5: Descriptive Statistics

	Cities with any living wage law		Contractor living wage laws		No living wage law
	Pre (1)	Post (2)	Pre (3)	Post (4)	(5)
A. General wage measures	N=175	N=176	N=156	N=145	N=822
Average living wage	4.82 (.40)	7.67 (.70)	4.83 (.40)	7.60 (.68)	5.02 (.37)
Average minimum wage	4.82 (.40)	5.35 (.40)	4.83 (.40)	5.38 (.41)	5.02 (.37)
Average wage	11.88 (1.35)	11.97 (1.43)	11.94 (1.30)	11.93 (1.38)	11.32 (1.45)
Average wage, workers below 10 th centile	5.70 (.58)	5.75 (.66)	5.71 (.57)	5.71 (.63)	5.53 (.52)
B. Unionized, municipal workers					
Average wage of unionized, municipal workers—excluding teachers, police, and fire—with wage below median	9.31 (1.68) N=118	9.26 (1.38) N=131	9.26 (1.68) N=108	9.15 (1.35) N=113	9.16 (1.77) N=434

Data used are means computed over city-quarter cells. Standard deviations are reported in parentheses. Sample includes all city-quarter cells with at least 100 observations (for all workers). Living wage variable is higher of living wage or minimum wage, averaged over the quarter. Wage measures (except living wages and minimum wages) are deflated by the average hourly earnings series, and are expressed in 1996:Q1 terms. Centiles of wage distribution are calculated for each city-quarter cell. Estimates are weighted, with the wage variables weighted by the number of corresponding observations in the cell. “Post” includes quarters in which the living wage increased, and all quarters afterwards. Sample sizes (number of city-quarter cells) in top row apply to all rows except for wage figures for unionized workers (in the bottom row of Panel B), where sample sizes can be smaller if there are no workers satisfying the indicated criteria; separate sample sizes are reported for these cells.

Table 6: Effects of Contractor Living Wage Laws on Average Wages of
Below-Median Wage Unionized Municipal Workers

	(1)	(2)
A. Contemporaneous specification:		
Living wage	.137 (.073)	.136 (.063)
Minimum wage	-.012 (.205)	...
R ²	.491	.491
B. 2-quarter lag specification:		
Living wage	.088 (.080)	.077 (.072)
Minimum wage	-.090 (.187)	...
R ²	.488	.488
C. 4-quarter lag specification:		
Living wage	.123 (.061)	.140 (.053)
Minimum wage	.120 (.141)	...
R ²	.491	.491
D. Including contemporaneous, 2-quarter, and 4-quarter lags of living wages and minimum wages:		
Living wage variables		
Joint significance (p-value)	.015	.001
Sum	.169	.174
(standard error)	(.083)	(.069)
Minimum wage variables		
Joint significance (p-value)	.336	...
Sum	.017	...
(standard error)	(.201)	
R ²	.497	.495

See notes to Table 5. There are 655 observations in each column. In addition to reported variables, specifications include dummy variables for city and for each unique quarter in the sample. Results are reported using the wage floor applicable when health insurance is provided. Estimates are weighted by the number of observations in the cell used to construct the wage measure. Standard errors allow for arbitrary serial correlation pattern within cities.

Table 7: Sensitivity Analysis of Estimates of Effects of Contractor Living Wages on Average Wages

A. Alternative samples and specifications, below-median wage workers								
	Full sample	Full sample	Occupations w/ average wages below \$8.25	Add 6-quarter lag	Unweighted	Classification of cities with dormant living wage laws		
	(1)	(2)	(3)	(4)	(5)	Exclude	In control group	
						(6)	(7)	
Sum of contemporaneous, 2-quarter, and 4-quarters lags of living wage	.255 (.111)	.244 (.115)	.222 (.072)	.172 (.077)	.093 (.088)	.171 (.071)	.163 (.070)	
Ever pass living wage \times time	-.003 (.003)	-.009 (.010)	
Ever pass living wage \times time ²0003 (.0005)	
N	655	655	608	584	655	620	655	
B. Alternative centile cutoffs (wages below __ centile)								
	30th	40th	50th	60th	70th	80th	90th	
Sum of contemporaneous, 2-quarter, and 4-quarters lags of living wage	.019 (.171)	.120 (.094)	.174 (.069)	.119 (.055)	.122 (.045)	.069 (.056)	-.009 (.087)	
N	339	498	655	755	823	864	900	
C. Alternative centile cutoffs for predicted wages (predicted wages below __ centile)								
	30th	40th	50th	60th	70th	80th	90th	
Sum of contemporaneous, 2-quarter, and 4-quarters lags of living wage	.049 (.143)	.103 (.103)	.128 (.083)	.151 (.088)	.149 (.067)	.077 (.039)	.134 (.050)	
N	454	562	650	721	765	821	869	

See notes to Table 6. In columns (1) and (2), Panel A, the time trend counts quarters, and ranges from 1 to 20. “Ever pass living wage” refers to the sample period; no cities passed living wages prior to the sample period. Estimates of the specification in column (1) of Table 6 were also insensitive to including the time trend interaction variables. Based on Table 4, column (16), the occupations excluded from column (3), Panel A, include executives/administrative, professional (excluding teachers), technical, and craft, in addition to teachers, police, and fire. The wage regression used to predict wages for Panel C includes controls for education, age (up to a cubic), race, sex, marital status, and year and interview month. Unless otherwise noted, specifications include contemporaneous, 2-quarter, and 4-quarter lags of living wage.

Table 8: Alternative Estimates of Effects of Contractor Living Wages on Average Wages

	A. "Non-treatment" groups				B. Narrower treatment group	
	Contractor living wage laws			Non-contractor living wage laws	Contractor-only living wage laws	
	U/M, teachers, police, and fire only	NU/M	U/NM	NU/NM	U/M, excluding teachers, police, and fire	UM, excluding teachers, police, and fire
	(1)	(2)	(3)	(4)	(5)	(6)
Sum of contemporaneous, 2-quarter, and 4-quarters lags of living wage	-.197 (.150)	-.011 (.067)	-.040 (.051)	.020 (.029)	-.013 (.096)	.156 (.054)
R ²	.458	.391	.566	.858	.453	.450
N	388	1018	1023	1123	462	558

See notes to Tables 6 and 7. All estimates are for below-median wage workers in city-quarter cell, paralleling most of the estimates in the previous tables. Abbreviations are as follows: U/M = unionized municipal; U/NM = unionized non-municipal; NU/M = non-unionized municipal; and NU/NM = non-unionized non-municipal.