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## Living Wages: Protection for or Protection from Low-Wage Workers?

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## Abstract

Living wage laws are touted as anti-poverty measures. Yet they apply to only a small fraction of workers, most commonly covering only employers with city contracts. The apparent contradiction between broad anti-poverty goals and narrow coverage suggests that goals other than poverty reduction may partly underlie living wage campaigns. This paper considers the hypothesis that living wage laws act to maintain or increase rents among unionized municipal workers. 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 for their members. The evidence presented here, from an analysis of CPS data for 1996–2000, indicates that the wages of unionized municipal workers are indeed increased as a result of living wage laws covering contractors.

**KEYWORDS:** living wage laws, municipal workers, unionized municipal employees, municipal unions

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\*The author thanks Scott Adams for collaboration on other research studying living wages. Helpful comments were received from John DiNardo, Paul Lewis, David Levine, Steven Levitt, Thomas MaCurdy, Brian Murphy, Rudy Nothenberg, Robert Pollin, Michael Potepan, Deborah Reed, David Reynolds, Steven Rivkin, Peter Schmidt, and seminar participants at the University of California at Berkeley, the University of California at Santa Cruz, Harvard University, the University of Illinois, the University of Missouri, the University of Washington, the Public Policy Institute of California, RAND, and the Federal Reserve Bank of Kansas City. This research was partially supported by the Michigan Applied Public Policy Research Funds, the Broad Graduate School of Management, and the Public Policy Institute of California (PPIC).

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DAVID NEUMARK\*

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The 1990s witnessed widespread implementation of living wage laws across cities in the United States. At the end of the decade, more than 50 living wage ordinances were in effect (most in cities, but a few with application to counties or school boards), and numerous campaigns for more were under way; currently there are about 100 living wage laws on the books. Living

wages are touted as anti-poverty measures, and 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% of the poverty line for a family of four if health benefits are paid, and 125% without health benefits.

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The data and programs used in this paper will be available from the author for a period of three years following publication.

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 that they cover only employers that are contractors or subcontractors with the city, although some laws broaden coverage to include employers receiving business assistance from the city, and a few cover city employees. Estimates of the percentage of workers directly affected by coverage of contractors are usually quite low (below 1%); the other provisions of living wage laws boost the number of affected workers somewhat, but although there is no hard evidence, the general perception is that these other provisions do not boost coverage a great deal. This raises the question of why—given the 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, which are exceedingly rare.

The hypothesis I consider in this paper is that a principal effect of living wage laws is to benefit unionized municipal employees who might otherwise face competition from low-wage labor employed by city contractors and subcontractors. 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. 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. In other words, living wage laws may act principally to offer unionized municipal workers protection *from* low-wage workers, rather than to offer protection *for* low-wage workers.

Under this rent-seeking hypothesis, there may be a politically powerful constituency for the types of narrow living wage laws that have arisen in the United States. 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 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 has acknowledged the potentially powerful political influence of public sector unions (see also Ehrenberg and Goldstein 1975); as discussed below, unions representing municipal workers are particularly active in advocating for living wage laws. But because unions may have other motivations for supporting living wage laws, the key question on which this paper focuses is whether living wage laws actually appear to increase rents for unionized municipal workers.

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. 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 stemming from living wage laws, 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.

### **Living Wage Laws, Related Research, and the Rent-Seeking Hypothesis**

#### **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 with living wage laws that are sufficiently large to study with the CPS in the sample period used in this paper.<sup>1</sup> The wage levels associated with living wage laws are reported for these cities in the second column of Table 1.<sup>2</sup> In many cases (for example, 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.<sup>3</sup> The range of living wages—as of the end of calendar year 2000—extends from a low of \$6.80 in Milwaukee to a high of \$9.92 in San Jose. To provide some perspective, as of the end of 2000 the federal minimum wage was \$5.15, and the highest state minimum wage was \$6.50 (Oregon). Thus, in many cities the living wage exceeds the prevailing minimum wage by 50% or more.

<sup>1</sup>As explained below, the empirical analysis is restricted to city-month cells (for cities identified in the Current Population Survey) for which there are at least 25 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 months.

<sup>2</sup>The analysis ignores county living wage laws. 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. In the sample period, the only county living wage law that clearly covers a city 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—as was verified in estimates not reported in the tables in which Miami was excluded.

<sup>3</sup>In the empirical analysis reported in this paper, the lower wage with health insurance was used when there was a choice, but estimated effects of living wages are not sensitive to using the alternative higher wage in those cities that have one.

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 (for example, Durham), but sometimes limited to service and perhaps construction contractors (for example, San Jose, Baltimore), and in one case a much narrower set of contractors (Portland). Existing estimates (summarized in Neumark and Adams 2003a) indicate that coverage by contractor provisions of living wage laws is likely very low, on the order of 1% or less of the work force in a city. In contrast, in only three of these cities (Dayton, Minneapolis, and San Antonio) do living wage laws fail to cover contractors.<sup>4</sup> In addition, coverage is extended to city employees in just 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, and so on.

### 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

<sup>4</sup>The living wage law in Minneapolis is classified in Table 1 as covering employers receiving business assistance. 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, it is unclear how wages are regulated over the longer run after work is contracted out. It also would not increase bargaining power of unionized municipal workers who already compete with contracted work, and would not create incentives to reduce contracting out. As a consequence, the Minneapolis law is largely limited to business assistance recipients, and this paper therefore follows existing classifications of this law as covering business assistance recipients only (for example, [www.epionline.org](http://www.epionline.org)).

*Table 1. Living Wage Cities in the Analysis Sample.*

<i>City</i>	<i>Wage Provisions</i>	<i>Coverage</i>
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 subcontractors City employees. City manager directed to use living wage as guidelines for city employee wages if it falls within long-range city financial plan
Dayton	April 1998 (7.00)	
Denver	100% of poverty line for family of four, based on 2,080 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 subcontractors > \$5,000

*Continued*

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.

Table 1. Continued.

City	Wage Provisions	Coverage
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 2,080 annual hours, higher without health benefits: August 2000 (8.84)	Service contractors > \$50,000; assistance > \$100,000

This table covers those cities with living wages for which there were 25 or more observations on workers in at least some months of the CPS Outgoing Rotation Group files for the period 1996 through 2000, conditional on the sample restrictions described in the text. 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 2,000 annual work hours. Effective dates of enacted living wages are shown. When there are alternative living wages, the lower one is reported and used in the analysis (for example, the wage with health benefits). For more complete information on living wages across all cities, see Neumark and Adams (2003b).

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, re-

ducing 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 would a standard, broader wage increase—such as an increase in the minimum wage. To begin, there are two reasons why the scale effect 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).<sup>5</sup> 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 costs, or because some services have to be purchased in quantities that may be largely insensitive to price.<sup>6</sup>

<sup>5</sup>For evidence on efficiency in the private versus public sector, see, for example, Bhattacharyya and Parker (1994), Hollas and Stansell (1994), and Kuoping and Kao (1992).

<sup>6</sup>Ehrenberg and Schwarz (1986) noted 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) was the first paper to estimate these labor demand elasticities, and characterized these elasticities as suggesting that “market forces do not appear to be sufficiently strong to limit the size of real wage

Reductions in labor demand caused by living wage laws might also be moderated to some extent because of the limited coverage of those laws, whereby some of the labor disemployed in the covered sector may shift into the uncovered sector, generating an outward shift in the supply of labor in that sector.<sup>7</sup> The existence of a minimum wage may restrict the ability of uncovered sector wages to fall in response, but in general, some wage decline can be expected, and some resultant moderation of overall employment losses—effects simultaneously reducing the gains and the losses associated with living wage laws. Still, on net, employment will not expand enough in the uncovered sector to offset fully the employment decline in the covered sector.<sup>8</sup>

Despite the reasons to expect some moderation of disemployment effects, the standard theory still predicts that a higher living wage will cause average wages of low-wage or low-skilled workers to increase, and employment (and hours) of workers who would be employed at low wages to fall. Evidence reported in Neumark and Adams (2003a) is broadly consistent with these predictions. In particular, positive wage effects are detected, and negative employment effects appear to arise in response to the wage increases stemming from living wage laws. Neumark and Adams (2003b) considered the effects of these living wage

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increases which state and local government employees may seek in the future” (1973:378). However, in later work Ehrenberg noted that the elasticities are not clearly smaller (in absolute value) than those in the private sector (Ehrenberg and Schwarz 1986:1256–57). Courant et al. (1979) and Freeman (1986) delineated some factors that counter the presumed inelasticity of labor demand for public sector workers.

<sup>7</sup>An 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 deter search in the covered sector.

<sup>8</sup>Formally, 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).

laws on family incomes, rather than workers, specifically asking whether these laws reduce urban poverty, and found some evidence of beneficial effects on net. However, these results did not hold generally. Instead, the effects arose when the living wage laws included business assistance provisions, or when nearby jurisdictions also had living wage laws.<sup>9</sup>

### **Living Wage Laws as Rent-Seeking by Unionized Municipal Workers**

Living wage laws, which were introduced in the mid-1990s and have expanded rapidly since then, are typically touted as anti-poverty measures. The Economic Policy Institute, while noting that other anti-poverty tools are needed, argued that “the living wage is a crucial tool in the effort to end poverty.”<sup>10</sup> And in their treatise advocating living wages, Pollin and Luce argued that “the 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” (1998:1). Yet as noted above, living wage laws generally cover employers with city contracts, and frequently restrict coverage to these employers only, in which case they apply to a very 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 particular economic and political actors to seek passage of living wage laws covering city contractors.

The hypothesis considered in this paper is that living wage laws covering city contractors partly reflect rent-seeking on the part of unionized municipal workers. In particular, 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 for their members. Indeed narrow living wage laws may generate this latter effect without delivering benefits to low-wage workers and low-income families.

In interpreting the earlier evidence coupled with the new evidence reported in this paper, it is important to keep in mind that 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. In addition, 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.<sup>11</sup> Finally, unionized municipal workers can gain from living wage laws even if the contractor provisions of those laws do not result in detectable wage increases for low-wage workers generally. Indeed the gains to unionized municipal 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.

<sup>9</sup>This evidence, and possible explanations, are presented in Adams and Neumark (Forthcoming).

<sup>10</sup>See [www.epinet.org/content.cfm/issueguides\\_livingwage\\_livingwagefaq](http://www.epinet.org/content.cfm/issueguides_livingwage_livingwagefaq).

<sup>11</sup>In this sense, living wage laws may parallel the Davis-Bacon Act and other state-level prevailing wage laws affecting 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.

While the focus of many living wage laws on contractors is consistent with the rent-seeking hypothesis, an alternative view is that narrow living wage laws are the easiest to pass precisely 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 (or contractor-only) living wage laws and then subsequently broadened their coverage. Another related possibility is that narrow living wage laws result from compromises in which advocates seek broader laws, but narrower ones result because of opposition to the broader laws from employer groups or other parties. It would be interesting, although difficult, to try to explore the relationship between the role of municipal unions in particular living wage campaigns and whether these campaigns focused more on contractor provisions of living wage laws, and more generally to try to determine the factors that lead to the passage of different types of living wage laws.

### **The Effects of Living Wage Laws on Wages of Unionized Municipal Workers: Data and Methods**

#### **Data**

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 “metropolitan statistical areas” (MSAs), encompassing all large- and medium-sized cities in the United States, can be identified. Data on these residents are extracted for the empirical analysis. Living wages are assigned to residents of these MSAs based on the major city in the MSA.

This assignment 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 are more likely to work in the city. Also, the correspondence between cities and MSAs is imperfect, but because suburban residents may work in the city, this is not necessarily problematic.<sup>12</sup> 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).<sup>13</sup>

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 MSAs, I consider those working for “local government” to be 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.”<sup>14</sup>

The sample is restricted to those residing in MSAs, 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 hourly wage less than \$1 or greater than \$100. When the wage 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

<sup>12</sup>For ease of exposition, I will often refer to MSAs as “cities.”

<sup>13</sup>The analysis is also restricted to begin in 1996 because MSA codes are unavailable in the ORG files for part of 1995, due to phasing in of a new CPS sample based on the 1990 Census.

<sup>14</sup>The baseline specification (explained below) was also re-estimated 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.

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.<sup>15</sup>

Attention is focused on lower-skilled (and therefore generally lower-wage) unionized municipal workers, 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, much of the analysis focuses on those unionized municipal workers whose predicted wage is below the median of the predicted wage distribution in their city-month cell. The (log) wage regression used to predict (log) wages includes dummy variables for city and for each

unique month in the sample, controls for union status and municipal employment, and a vector of standard wage regression controls.<sup>16</sup> To increase the accuracy of the predicted median in each city-month cell, observations are included in this analysis only for city-month cells with 25 or more workers in total.<sup>17</sup>

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.

### 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. Indexing the data by  $i$  for individuals,  $c$  for cities, and  $m$  for months (where  $m$  identifies each unique month in the sample), the baseline regression estimated for the log wage is of the form

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<sup>15</sup>If living wage laws materially reduce cities' incentives to contract out jobs, then the share of the work force made up by unionized municipal workers might be observed to increase in response to living wage laws. 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, similarly, it would probably not be informative to study changes in actual contracting-out behavior. In any event, there are not very good data available with which to carry out the latter analysis. The International City/County Management Association (ICMA) conducts a survey of "alternative service delivery" in U.S. cities (and counties). But the survey does not permit quantification of the share of work that is contracted out, and has a very low response rate of less than 24% ([www2.icma.org/main/search.asp](http://www2.icma.org/main/search.asp)). In a lengthier working paper version of this study (Neumark 2001), no effects of living wages on the work force share of unionized municipal workers are detected.

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<sup>16</sup>In particular, in addition to the city and month dummy variables, the regression includes dummy variables for workers classified into five categories based on union status and municipal employment, as follows: union and municipal in the occupations potentially affected by living wages (all but teachers, police, and fire, as discussed below); union and municipal in these other occupation categories; union and non-municipal; non-union and municipal; and non-union and non-municipal. Additional controls are age (up to a cubic); education (14 categories); marital status (7 categories); sex; race (4 categories); occupation (16 categories); and industry (23 categories).

<sup>17</sup>Results were very similar when the analysis looked at unionized municipal workers with wages below the median in the corresponding city-month cell.

$$(1) \ln(w_{icm}) = \alpha + \beta \max[\ln(w_{cm}^{lv}), \ln(w_{cm}^{min})] \\ + \gamma \ln(w_{cm}^{min}) + X_{icm} \theta + C_c \delta + M_m \lambda + \varepsilon_{icm}.$$

In this specification,  $w^{min}$  is the higher of the federal or state minimum wage and  $w^{lv}$  is the living wage.<sup>18</sup> It is potentially important 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  $\beta$  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.  $X$  is a vector of control variables (paralleling those used for predicting wages), including age, education, marital status, sex, race, occupation, and industry.  $C$  and  $M$  are vectors of city and month dummy variables, and  $\varepsilon$  is a random error term.

Variations of this specification are discussed as they are introduced in the empirical analysis. For most of the analyses, this regression is estimated for unionized municipal workers with predicted wages below the median in the city-month cell; exceptions are noted along the way. With the standard control variables included in a wage regression or human capital earnings function, the estimates of  $\beta$  for the sample of unionized municipal workers tell us whether living wages result in higher wages net of any shifts in the composition of this work force along dimensions that might be associated with wages, including both skill and the industrial or occupational makeup of the work force.

A recent paper by Bertrand et al. (2002) looked 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 noted the usual result that serial correlation in the error biases the standard errors, and showed both analytically and via a simulation that because of the serial correlation in the treatment in a typical difference-in-differences estimator—in this context in the living wage variable for a city—the standard errors could be seriously understated. 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 present paper, which generally, of course, resulted in higher standard errors.

### **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 more widespread 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

<sup>18</sup>In the few cases of MSAs that straddle states with different minimum wages (for example, Philadelphia), a weighted average of the minimum wages in the two states is used, weighted by the shares of the MSA population in each state (averaged over the months of 1996).

of demand shifts, relative wage constraints, or some other reason. Given 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 important 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 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.

Finally, 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 effects similar to those of contractor living wage laws. According to the rent-seeking hypothesis, they should not.

### Descriptive Information on Workers

Table 2 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 with predicted wages below the median predicted wage in the corresponding city-month cell.<sup>19</sup>

The first row of numbers in the table describes the work force 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 .147, with 29% of union workers employed by municipalities. When attention is restricted to those with predicted wages below the median predicted wage, the unionization rate drops, reflecting either the union wage premium or lower unionization rates among workers with characteristics associated with lower wages. The share of unionized municipal workers among this subset is particularly

<sup>19</sup>In this table, in order to provide the most complete description of the data, I do not restrict the sample to city-month cells with 25 or more observations, as I do in the subsequent tables that move on to the regression analysis. However, none of the qualitative conclusions from Table 2 are changed by imposing this restriction. For the regression analysis, the baseline specification was re-estimated using different cutoffs for the number of observations per city-month cell (no minimum, and cutoffs of 10 and 40 workers per cell). The estimates were similar and the qualitative conclusions were unchanged.

Table 2. Distribution of Workers by Unionization, Municipal Employment, Wages, and Major Occupation.

	Occupational Distribution										Average Wages																		
	All			Below Median			Predicted Wage			All			Below Median			Predicted 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	U/M	U/NM	NU/M NU/NM	U/M	U/NM	NU/M NU/NM											
<b>Work Force Share:</b>	.042	.105	.041	.813	(1)	(2)	(3)	(4)	.873	(5)	(6)	(7)	(8)	.020	.066	.041	.873	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)				
<i>Occupations:</i>																													
Executive,	.055	.052	.145	.161	.032	.030	.064	.078	.020	.066	.041	.873	.020	.066	.041	.873	.020	.066	.041	.873	.020	.066	.041	.873	.020	.066	.041	.873	
Administrative	.093	.078	.121	.121	.055	.029	.064	.044	.055	.029	.064	.044	.055	.029	.064	.044	.055	.029	.064	.044	.055	.029	.064	.044	.055	.029	.064	.044	.055
Professional, excl.	.413	.046	.195	.025	.258	.026	.136	.023	.258	.026	.136	.023	.258	.026	.136	.023	.258	.026	.136	.023	.258	.026	.136	.023	.258	.026	.136	.023	.258
Teachers	.013	.031	.023	.040	.012	.024	.022	.027	.012	.024	.022	.027	.012	.024	.022	.027	.012	.024	.022	.027	.012	.024	.022	.027	.012	.024	.022	.027	.012
Technical	.003	.051	.010	.134	.006	.099	.017	.150	.006	.099	.017	.150	.006	.099	.017	.150	.006	.099	.017	.150	.006	.099	.017	.150	.006	.099	.017	.150	.006
Sales	.127	.148	.217	.157	.291	.163	.313	.193	.291	.163	.313	.193	.291	.163	.313	.193	.291	.163	.313	.193	.291	.163	.313	.193	.291	.163	.313	.193	.291
Clerical	.000	.001	.000	.007	.000	.001	.000	.012	.000	.001	.000	.012	.000	.001	.000	.012	.000	.001	.000	.012	.000	.001	.000	.012	.000	.001	.000	.012	
Private Household	.007	.006	.016	.008	.015	.012	.027	.012	.015	.012	.027	.012	.015	.012	.027	.012	.015	.012	.027	.012	.015	.012	.027	.012	.015	.012	.027	.012	.015
Protective Services,	.007	.006	.016	.008	.015	.012	.027	.012	.015	.012	.027	.012	.015	.012	.027	.012	.015	.012	.027	.012	.015	.012	.027	.012	.015	.012	.027	.012	.015
excl. Police and Fire	.095	.012	.044	.001	.037	.005	.026	.001	.037	.005	.026	.001	.037	.005	.026	.001	.037	.005	.026	.001	.037	.005	.026	.001	.037	.005	.026	.001	.037
Police	.036	.001	.009	.000	.028	.001	.008	.000	.028	.001	.008	.000	.028	.001	.008	.000	.028	.001	.008	.000	.028	.001	.008	.000	.028	.001	.008	.000	.028
Fire	.062	.090	.121	.116	.165	.190	.215	.190	.165	.190	.215	.190	.165	.190	.215	.190	.165	.190	.215	.190	.165	.190	.215	.190	.165	.190	.215	.190	.165
Other Services	.041	.208	.037	.090	.028	.098	.027	.079	.028	.098	.027	.079	.028	.098	.027	.079	.028	.098	.027	.079	.028	.098	.027	.079	.028	.098	.027	.079	.028
Craft	.003	.117	.004	.055	.004	.135	.005	.073	.004	.135	.005	.073	.004	.135	.005	.073	.004	.135	.005	.073	.004	.135	.005	.073	.004	.135	.005	.073	.004
Machine Operator	.036	.080	.033	.031	.046	.061	.041	.038	.046	.061	.041	.038	.046	.061	.041	.038	.046	.061	.041	.038	.046	.061	.041	.038	.046	.061	.041	.038	.046
Transportation	.011	.075	.011	.039	.015	.117	.017	.061	.015	.117	.017	.061	.015	.117	.017	.061	.015	.117	.017	.061	.015	.117	.017	.061	.015	.117	.017	.061	.015
Handler	.006	.005	.013	.014	.009	.008	.019	.021	.009	.008	.019	.021	.009	.008	.019	.021	.009	.008	.019	.021	.009	.008	.019	.021	.009	.008	.019	.021	.009
Farming, Fishery																													

The sample period is 1996–2000. U/M: unionized municipal. U/NM: unionized non-municipal. NU/M: non-unionized municipal. NU/NM: non-unionized non-municipal. Occupational distribution shows the share in each category of workers (for example, unionized municipal) in each listed occupation. CPS sample weights are used. Wage measures (except living wages and minimum wages) are deflated by the average hourly earnings series, and are expressed in 1996:Q1 terms. 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 predicted wage is 193,432. The median predicted wage is constructed for each city-month cell; the construction of the predicted wage is described in the text, in the section “The Effects of Living Wage Laws on Wages of Unionized Municipal Workers: Data and Methods.”

small, representing 2.0% of these lower-skilled 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, it draws attention to the relative scarcity 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, with below-median predicted wages. For this analysis, there are 1,314 observations on individual workers in the control sample of cities never passing living wage laws. In the treatment sample in which contractor living wages are passed, there are 339 observations prior to the implementation of the living wage, and 372 afterward.

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. Among all workers, teachers constitute 41.3% of the unionized municipal work force, by far the largest group. There are also high concentrations of executives, professionals excluding teachers, and police, as well as clerical workers, those in other services, and craft workers. Among lower-skill workers, however, 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 45.6% of unionized municipal workers with below-median predicted wages. This pattern provides another reason—in addition, that is, to the consideration that living wage laws are likely to affect lower-skill workers—for restricting attention to those with lower predicted wages, since the higher-wage unionized municipal workers are heavily representative of occupations (such as teachers and police) for which contracting out is not a factor.

The other columns of Table 2 (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 the wage differentials associated with union status or municipal employment. What is informative, however, is how some of these wages compare with the legislated living wages reported in Table 1. In particular, once attention is restricted to those with below-median predicted 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 suggest the possibility of detecting benefits to these workers from the imposition of living wages, if these benefits exist. In contrast, it seems less plausible that living wage laws would have any detectable effect on higher-skill or higher-wage unionized municipal workers.

### Descriptive Information on Cities

Having reviewed some descriptive information on unionized municipal workers, I turn now to descriptive statistics by city. Panel A of Table 3 provides some general information on wages for cities with any living wage laws, prior to and following the initial implementation of the living wage; cities with living wage laws covering contractors, again before and after the initial implementation of the living wage; and 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 exceeding those in other cities by about 5–6%, and with wages of workers below the 10th centile that were higher by about 3%.

Panel B reports average wages for the group of workers that will be the focus of the analysis, those with below-median predicted wages, and excluding teachers, police, and fire. These figures indicate that living wages were implemented in cities

Table 3. Descriptive Statistics.

Wage Measure	Cities with Any Living Wage Law		Contractor Living Wage Laws		No Living Wage Law
	Pre (1)	Post (2)	Pre (3)	Post (4)	(5)
<b>A. General Wage Measures</b>	N = 45,399	N = 49,007	N = 41,990	N = 42,535	N = 193,063
Average Living Wage	4.59 (.26)	6.88 (.57)	4.60 (.26)	6.80 (.53)	4.59 (.24)
Average Minimum Wage	4.59 (.26)	4.75 (.33)	4.60 (.26)	4.78 (.34)	4.59 (.24)
Average Wage	14.11 (8.75)	14.32 (9.37)	14.28 (8.85)	14.39 (9.49)	13.41 (8.87)
Average Wage, Workers below 10 <sup>th</sup> Centile	5.39 (1.26)	5.45 (1.20)	5.40 (1.27)	5.45 (1.20)	5.23 (1.23)
	N = 7,361	N = 8,500	N = 6,905	N = 7,609	N = 30,616
<b>B. Unionized, Municipal Workers</b>					
Average Wage of Unionized, Municipal Workers—Excluding Teachers, Police, and Fire—with Wage below Median Predicted Wage	9.99 (2.47)	10.09 (2.29)	9.97 (2.52)	10.06 (2.29)	9.56 (2.28)
	N = 362	N = 430	N = 339	N = 372	N = 1,314

See notes to Table 2. Means are reported, with standard deviations reported in parentheses. The sample includes all individuals in city-month cells with at least 25 observations (for all workers). The living wage variable is the higher of the living wage or minimum wage. Wage measures (except living wages and minimum wages) are deflated by the average hourly earnings series, and are expressed in 1996:Q1 terms. "Post" includes months in which the living wage increased, and all months afterward. The median predicted wage is constructed for each city-month cell; the construction of the predicted wage is described in the text, in the section "The Effects of Living Wage Laws on Wages of Unionized Municipal Workers: Data and Methods."

where these workers earned wages that were about 4% higher than wages for their counterparts in other cities. These figures suggest only trivial wage increases 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 3 because different cities are in the "Pre" and "Post" columns for different numbers of months.

### The Effects of Living Wage Laws on Wages of Unionized Municipal Workers: Evidence

#### Contractor Living Wage Laws and Wages of Unionized Municipal Workers

The groundwork having thus been laid, this section reports results from the ex-

PLICIT prediction of the rent-seeking hypothesis regarding contractor living wages, using variants of the specification given by equation (1). The basic results are reported in Table 4, as noted before for unionized municipal workers with below-median predicted wages, in 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 6-month lags and a specification with 12-month lags. These lags allow the effects to take place some time after a living wage is implemented or increased. In earlier work (Neumark and Adams 2003a, 2003b) the effects of living wages on wages, employment, and also family income generally took about one year to appear.

In column (1), the contemporaneous effect of the living wage on the wages of

Table 4. Effects of Contractor Living Wage Laws on the Log Wages of Unionized Municipal Workers Who Are below the Predicted Median Wage.

<i>Specification</i>	<i>(1)</i>	<i>(2)</i>
<b>A. Contemporaneous Specification:</b>		
Log Living Wage	.176 (.082)	.158 (.070)
Log Minimum Wage	-.139 (.182)	—
R <sup>2</sup>	.401	.400
<b>B. 6-Month Lag Specification:</b>		
Log Living Wage	.111 (.086)	.101 (.074)
Log Minimum Wage	-.068 (.172)	—
R <sup>2</sup>	.399	.398
<b>C. 12-Month Lag Specification:</b>		
Log Living Wage	.121 (.053)	.139 (.051)
Log Minimum Wage	.120 (.170)	—
R <sup>2</sup>	.400	.399
<b>D. Including Contemporaneous, 6-Month, and 12-Month Lags of Living Wages and Minimum Wages:</b>		
Living Wage Variables:		
Joint Significance (P-Value)	.020	.006
Sum	.195	.188
(Standard Error)	(.093)	(.077)
Minimum Wage Variables:		
Joint Significance (P-Value)	.220	—
Sum	-.071	—
(Standard Error)	(.216)	
R <sup>2</sup>	.402	.401

See notes to Tables 2 and 3. Regressions are estimated at the individual level for unionized municipal workers, with below-median predicted wages, residing in metropolitan areas. Observations are included only for city-month cells with 25 or more workers in total. There are 2,025 observations in each column. In addition to reported variables, specifications include dummy variables for city and for each unique month in the sample, as well as the following controls: age (up to a cubic); education (14 categories); marital status (7 categories); sex; race (4 categories); residence in the South; occupation (16 categories); and industry (23 categories). Results are reported using the lower wage floor applicable when one was specified (usually in the case where health insurance is provided). The median predicted wage is computed for each city-month cell; the construction of the predicted wage is described in the text, in the section “The Effects of Living Wage Laws on Wages of Unionized Municipal Workers: Data and Methods.” CPS sample weights are used. Standard errors allow for arbitrary serial correlation patterns within cities, by “clustering” on cities only (Bertrand et al. 2002).

unionized municipal workers is large, with an elasticity of .176, and statistically significant at the 5% level. The 6-month lag specification still points to an effect that is positive, although smaller and no longer statistically significant, consistent with some

moderation of the wage effect (although this change could easily be attributable to sampling variation as well). The 12-month lag specification points to a somewhat larger effect, statistically significant at the 5% 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 .195, which is statistically significant at the 5% level. This estimate implies, for example, that implementation of a living wage that exceeds the minimum wage by 30%—which is not uncommon—would raise wages of these workers by approximately 5.9%.

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 .188, and statistically significant at the 5% level with a smaller standard error than in column (1). Thus, the evidence from these specifications is quite clearly consistent with living wages increasing rents for unionized municipal workers.

A number of additional analyses of this relationship are considered next, both to explore the sensitivity of the results and to refine the analysis. Many of these analyses are reported in Table 5, and some only in the text. In all cases, the results come from the specification with the summed contemporaneous and lagged effects, comparable to the last panel of Table 4, and using the more parsimonious specification excluding the minimum wage variable.<sup>20</sup>

Panel A looks at the sensitivity of the results to differences in the specification and sample definitions, including the clas-

sification 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. 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 month 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 was added to the specification. The living wage variable was dropped because all observations were taken prior to the introduction of a living wage, and the time trend itself was not included because the specification already included month dummy variables. The estimated coefficient of the time trend interaction provides a test of differential time trends in the treatment and control groups. 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, with the living wage and time trend variables included 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 5.

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 union-

<sup>20</sup>Excluding the minimum wage had virtually no impact on the estimates reported here, and increased their precision a bit.

Table 5. Alternative Analyses of Estimates of Effects of Contractor Living Wages on Log Wages.

<b>A. Alternative Samples and Specifications, Workers below Median Predicted Wage</b>							
	<i>Only Cities with Living Wages in Sample</i>		<i>Add 18-Month Lag</i>	<i>Equal Weights for Each City-Month Cell</i>	<i>Add Control for Percent Organized</i>	<i>Classification of Cities with Dormant Living Wage Laws</i>	
	<i>Add Time Trend</i>	<i>Period</i>				<i>Exclude</i>	<i>In Control Group</i>
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Living Wage Effect	.211 (.092)	.247 (.096)	.136 (.068)	.254 (.068)	.189 (.078)	.152 (.077)	.155 (.077)
Ever Pass Living Wage $\times$ Time	-.0003 (.001)	—	—	—	—	—	—
N	2,025	711	1,824	2,025	2,025	1,945	2,025
<b>B. Effects for Low-Wage Occupations and Different Ranges of Predicted Wage Distribution</b>							
	<i>All Workers</i>		<i>All Workers</i>			<i>Below Predicted Median</i>	
	<i>6 Occupations with Average Wages Below \$10</i>	$\times$ 1 <sup>st</sup> Quartile	$\times$ 2 <sup>nd</sup> Quartile	$\times$ 3 <sup>rd</sup> Quartile	<i>Main Effect</i>	$\times$ 1 <sup>st</sup> Quartile	<i>Main Effect</i>
Living Wage Effect	.279 (.123)	.165 (.085)	.023 (.053)	.004 (.058)	-.070 (.056)	.119 (.081)	.023 (.051)
N	1,485			5,469			2,025

See notes to Table 4. Unless otherwise specified, specifications include the same variables as in column (2) of Table 4. In each specification, the “living wage effect” is the sum of the estimated coefficients of the contemporaneous, 6-month, and 12-month lags of the living wage variable (corresponding to the “Sum” shown in Table 4). In column (1), Panel A, the time trend counts months, and ranges from 1 to 60. Based on Table 2, column (12), the occupations excluded from column (1), Panel B, are executive/administrative, professional (excluding teachers), technical, sales, clerical, and craft, in addition to teachers, police, and fire; there are of course no private household workers among unionized municipal workers. In Panel B, columns (2)–(7), the specification is augmented by dummy variables for the quartile, as well as the interactions of these with the living wage variables that are shown in the table. The “main” living wage effects reported are for the sums of the contemporaneous, 6-month lag, and 12-month lag of the living wage, and the quartile interactions are for the sums of the interactions between these variables and the quartile dummy variables.

ized municipal workers does not weaken but rather strengthens slightly, and remains statistically significant despite the expected increase in the standard error of the estimate associated with including the time trend interaction. While the time trend coefficient is not statistically significant, its negative sign suggests that contractor living wage laws have tended to be passed in cities where wages of unionized municipal workers are declining relative to the corresponding wages in other cities, 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 interaction included, as living wage laws appear to boost wages of unionized municipal workers relative to a declining trend. Finally, the specification was also expanded to include a quadratic term as well, to allow for a non-linear trend difference; this had no impact on the results.

As yet another way of assessing the robustness of the results with respect to differences between the treatment and control cities, in column (2) I re-estimate the

baseline specification dropping from the sample the cities that never passed a living wage law, so that only the observations on living wage cities in the period prior to their passing a living wage law serve as the controls. The resulting estimate of the effect of the living wage on the wages of unionized municipal workers is larger than the corresponding estimate in Table 4. Of course this specification is less desirable, because it fails to use the later observations in the control group of cities that never pass living wage laws to control for aggregate movements in the wages of these workers, which—as the higher estimate suggests—were in the upward direction.

In column (3) 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. Besides its value in assessing the robustness of the findings, this examination is potentially of substantive interest. Although the evidence that living wages boost wages among lower-skilled unionized municipal workers appears rather unambiguous (Table 4), it is perhaps surprising that the wages of unionized municipal workers seem to adjust rapidly when a living wage law is passed. If some workers are locked into multi-year contracts that are not renegotiated, we would expect wage adjustments to grow as contracts are renewed. It is true that the longer-run effect is larger than the contemporaneous effect, but not by much (for example, .188 versus .158 in column 2 of Table 4). Of course it is possible that the wage effects over the longer term are even larger than those suggested by the estimates in Table 4. While the short sample period available for analysis does not permit this question to be addressed definitively, the evidence reported here suggests that this is not the case. In particular, the estimated impact using an 18-month lag is if anything a bit smaller. The estimate (standard error) for the specification in Table 4, column (2), with the comparable sample for which the 18-month lag can be estimated is .161 (.063), compared with .136 (.068) in Table 5. Thus, the relatively rapid response of wages of unionized mu-

nicipal workers to living wage laws remains somewhat of a puzzle, depending on one's perspective either raising some red flags regarding the interpretation of the evidence, or suggesting that slower wage adjustments owing to multi-year contracts either are not the norm or do little to deter adjustments in response to living wages.

Of course another possibility is that the expectation of living wage laws taking effect in itself increases the bargaining power of unionized municipal workers, since the parties know that higher negotiated wages are less likely, in the future, to be countered by contracting out. To look at this question, I added a 6-month "lead" of the living wage variable to the specification in column (2) of Table 4. The estimated coefficients of the 6-month lead and the contemporaneous living wage variables were each around .10, with standard errors of around .08, and the overall combined effect when including the 6-month lead was larger by .04 than were estimates without the 6-month lead for the same sample. Thus, although the data are not sufficiently precise to pin down whether there is an anticipatory effect, they are consistent with such an effect.

To this point, the data have been weighted by the CPS sampling weights, which make little difference relative to simple unweighted estimates. In either case, though, cities are implicitly weighted by the number of workers in them who are in the sample. If, instead, we want to think about observations stemming from the city level, rather than the level of the individual worker, then we might want to assess the sensitivity of the estimates to reweighting the data so that each city-month cell receives equal weight. This estimate is reported in column (4), and once again, the result is quite similar—if anything slightly larger. As a related analysis of the sensitivity of the results to observations from different cities, 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 .139 to .237, and the t-statistics ranged from 1.80 to 3.49.<sup>21</sup>

Union wage differentials may be related to the strength of union organization, often captured in the percentage organized. While fixed differences across cities would be captured in the city fixed effects, changes in this variable would not. Thus, column (5) adds a control for the percentage organized. This has virtually no effect on the estimate relative to the comparable estimate of .188 in Table 4. However, because the percentage organized may be an outcome that is influenced by living wage laws, the specification excluding this variable seems preferable.

Although 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 law in August 1999, but the city was then 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 have had an impact because of continuing uncertainty, it was not in effect as of the end of the sample period. Finally, in St. Louis a lawsuit 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 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.

That is done in specifications reported in the last two columns of Panel A of Table 5, first by simply excluding these cities from the analysis, and then by instead treating them as if they do not have a living wage, and including them in the control group. As the estimates show, in neither case do the results change much; if anything, the modest declines in the estimated effects of living wage laws might suggest that despite the uncertainty surrounding these laws, they had some effects on wages of unionized municipal workers, which is not entirely surprising given that the impending enactment of a living wage law would be expected to increase bargaining power.

To this point, the results in Table 5 reveal that the positive effect of living wage laws on the wages of unionized municipal workers is very robust with respect to variations in the specification, sample definitions, and classification of treatment and control groups. Panel B of Table 5 reverts to the basic specification and sample, but considers in more detail the identification of the lower-skill or lower-wage unionized municipal workers for whom the effects of living wage laws are identified.

First, rather than use a criterion based on predicted wages, the estimates in column (1) simply select unionized municipal workers in low-paying occupations. In particular, attention is restricted to a subset of affected occupations with average wages below \$10 for non-unionized non-municipal workers (see Table 2). 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—and most likely larger—for them. This is confirmed by the estimated impact of living wages, which is larger than the comparable estimate in Table 4 (.279 versus .188), and statistically significant at the 5% level.<sup>22</sup>

<sup>21</sup>Not surprisingly, the estimates were more sensitive to dropping those living wage cities that contribute more observations to the sample. In particular, the estimate fell the most when San Jose or Detroit was omitted (to .139 or .150, respectively, both significant at the 10% level but not the 5% level), and rose the most when Chicago or Los Angeles was omitted (to .237 and .211, respectively, both significant at the 5% level).

<sup>22</sup>The specification was also augmented to estimate separate effects of living wages for black and

The next specifications, rather than simply using the criterion of the predicted wage being below the median, look in more detail at whether it is, in fact, the lowest-skilled unionized municipal workers whose wages are increased as a result of living wage laws. Columns (2)–(5) report estimates using the full sample of unionized municipal workers, with the specification expanded to identify whether the effects of living wages are stronger in the lower regions of the predicted wage distribution. This is done by adding interactions between the living wage variables and dummy variables for whether the predicted wage is in each of the lower three quartiles of this distribution. (Dummy variables for the quartiles are also included, of course.) These estimates show that the effect of living wage laws arises for the lowest-skilled unionized municipal workers, paralleling the estimates in column (1) that focus on occupation.<sup>23</sup> Finally, columns (6) and (7) report results from a similar exercise re-

stricted to those with predicted wages below the predicted median. Although the differences between the first and second quartile are no longer statistically significant, the point estimates again indicate the strongest effects for the least-skilled unionized municipal workers.

### Effects on Groups That Should Not Gain under the Rent-Seeking Hypothesis

The evidence thus far points to positive effects of living wage laws on wages of unionized municipal workers. The central hypothesis this paper considers—consistent with the evidence just presented—is that contractor 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 the first four columns of Table 6. In particular, specifications similar to those in Table 4 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 alternatives.

Table 6 first presents estimates for unionized municipal workers with below-median predicted 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 wage laws on the wages paid to this group, as the estimated effect is negative rather than positive, and insignificantly different from zero (albeit imprecisely). 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

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white/other workers, not so much because of differences in wage levels by demographic group (for which controls are already included), but to assess whether particular groups might gain from living wages and hence have a greater stake in their passage. The point estimate of the effect of living wages was larger for blacks than for other workers (.233 versus .173), but the difference between the effects for black and other workers was not significant ( $p = .282$ ).

<sup>23</sup>In fact, the main effect, which captures the effect for the top quartile, suggests a decline in response to living wage laws for the most-skilled workers, although this estimate is not statistically significant. This main effect can be viewed as unrelated to living wages, and instead as simply providing another level of differencing by looking at relative changes for lower-skilled unionized municipal workers. As an alternative, it is conceivable that the threat of contracting out increases rather than reduces the bargaining power of these workers both with employers and within the union (if they work in the same union as lower-skilled workers); assuming that some of the highest-skill workers would remain employed by municipalities even in the presence of contracting out, there would be less to moderate their wage demands, both because of the lower share of labor costs their contracts would represent (Marshall's laws) and because of reduced restraint from egalitarian pressures within the union.

Table 6. Estimates of Effects of Contractor Living Wages on the Log Wages of Workers Who Are below the Median Predicted Wage.

	<i>Contractor Living Wage Laws</i>				<i>Non-Contractor Living Wage Laws</i>
	<i>U/M, Teachers, Police, and Fire Only</i> (1)	<i>NU/M</i> (2)	<i>U/NM</i> (3)	<i>NU/NM</i> (4)	<i>U/M, Excluding Teachers, Police, and Fire</i> (5)
Sum of Contemporaneous Value, 6-Month, and 12-Month Lags of Log Living Wage	-.074 (.113)	.011 (.059)	.018 (.033)	.026 (.016)	-.133 (.076)
R <sup>2</sup>	.412	.344	.408	.388	.386
N	848	5,407	9,807	124,478	1,395

See notes to Tables 2, 4, and 5. Unless otherwise specified, specifications include the same variables as in column (2) of Table 4. All estimates are for workers with wages below the predicted median in the city-month cell, paralleling most of the estimates in the previous tables.

included because they represent much smaller work force 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. In contrast to the rent-seeking hypothesis, if cities were passing living wage laws while raising wages of their own lower-wage workers simultaneously, we would see wages of non-unionized municipal workers rising concurrently with living wages,<sup>24</sup> and if living wage laws were propagating 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.

Finally, column (5) returns to the unionized municipal workers in the affected oc-

cupations that were analyzed earlier. However, in this case attention is restricted to the small number of workers covered by non-contractor living wage laws. 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 wage laws and wage increases simultaneously, or if we are simply detecting spillovers from non-contractor living wage laws, 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 rather than positive. However, these estimates should be interpreted cautiously, as they are based on only three cities that have non-contractor living wage laws (Minneapolis-St. Paul, Dayton, and San Antonio, with most of the observations from the first of these cities). Thus, the tests in the other columns of the table, which cover all of the cities with contractor living wage laws, are probably more informative.

The main results reported in Tables 4 and 5 indicated direct effects of living wage laws on wages of unionized municipal work-

<sup>24</sup>Similarly, the results for non-unionized municipal workers imply that the results for contractor living wage laws for unionized municipal workers are not driven by the two cities whose living wage laws cover city employees in addition to contractors (Durham and San Jose).

ers. If the evidence in Table 6 showed similar gains for other groups of workers, this would point to explanations other than rent-seeking, such as the coincidence of rising municipal wages with living wages, or spillovers from living wage laws. However, wage gains do not appear for these other groups, and thus the evidence in this subsection bolsters the case for viewing living wages as enhancing or protecting the rents of unionized municipal workers.

### Union Support for Living Wage Laws

Finally, turning to non-econometric evidence, 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. Of course 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, and so on, 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). Thus, evidence that labor unions representing unionized municipal workers are active in promoting living wages cannot “prove” the rent-seeking hypothesis. However, a finding that these unions were not major players in living wage campaigns would cast some doubt on the hypothesis, so activities by these unions on behalf of living wages can serve as corroborating evidence.<sup>25</sup>

As one method of assessing unions’ involvement with living wage campaigns, I conducted a simple set of Internet searches looking for joint mention of living wage campaigns and labor unions. Specifically,

<sup>25</sup>That is, this involvement might be viewed as a necessary condition for the rent-seeking hypothesis to hold, although hardly a sufficient one.

for each search, using the search engine Google, I entered a combination of (a) the name of each city in the study with a living wage law, and (b) the phrase “living wage.” I then did the searches a second time adding the names of different labor organizations. The labor organizations included were AFL-CIO, AFSCME (the American Federation of State, County, and Municipal Employees), SEIU (the Service Employees International Union), IBEW (the International Brotherhood of Electrical Workers), UFCW (United Food and Commercial Workers Union), and HERE (Hotel Employees and Restaurant Employees).<sup>26</sup>

A relatively high fraction of hits involving living wages and each city also mentioned the AFL-CIO (about 15%) 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, next to 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—AFSCME and SEIU—each of which accounted for between about 5% and 10% of the total hits involving living wages and each city, and about three times more than for any of the other unions.<sup>27</sup> Furthermore, perusal of the materials un-

<sup>26</sup>These were selected based on preliminary examination of searches involving only city names and “living wage.” The acronym for the union was used in all cases except for HERE.

<sup>27</sup>The searches described here were done on April 3, 2001. Of course, the numbers would change from day to day. But a comparison of these numbers with 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, I also performed the searches for Baltimore using Yahoo and Excite. Both of these trials yielded considerably fewer hits (1,120 and 980, respectively) than did the search with Google (2,560). 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.

covered in these searches indicated that when unions were mentioned along with living wage campaigns in various cities, their role was generally described as one of active support.<sup>28</sup> This active support for living wages by unions that organize municipal workers is at least consistent with what would be expected if living wages act to increase rents paid to unionized municipal workers.

### 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 living wage laws generally cover employers with city contracts, and frequently restrict coverage to these employers only, thus applying 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 inducing various economic and political actors to seek passage of narrow living wage laws covering city contractors.

This paper considers the hypothesis that living wage laws partly reflect rent-seeking activity on the part of unionized municipal workers. 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 for their members. Indeed narrow living wage laws may generate this latter effect without delivering benefits to low-wage workers and low-income families.

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 wage laws. In particular, focusing attention on lower-

skilled unionized municipal workers, and on those occupations most likely to be affected, the evidence indicates elasticities of average wages with respect to living wages centered around .18. This finding generally holds up in a variety of analyses. Moreover, 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 the rent-seeking interpretation of the estimated effects of living wage laws on wages of unionized municipal workers. As a final piece of corroborating evidence, the paper presents some material indicating that the main unions involved in organizing municipal workers are heavily involved in living wage campaigns. Of course unions may have other incentives to back initiatives to pass living wage laws. But 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.

Two related qualifications are in order. First, although the empirical results point to evidence consistent with the rent-seeking explanation of narrow living wage laws, it would take a much different type of analysis, focusing on the history of living wage campaigns, the motivations of the actors, and so on—perhaps in combination with this evidence—to assess definitively whether rent-seeking is the predominant explanation for living wage campaigns and in particular for their support by unions that organize city workers. Second, the evidence that unionized municipal workers gain from living wage laws does not imply that living wages cannot offer assistance to low-wage workers or low-income families. Some policies, including living wages, could in principle increase rents of the most interested parties and also help other groups of workers. Thus, even if the strongest possible construction is placed on the evidence for rent-seeking, the findings should

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<sup>28</sup>For more detail, see Neumark (2001).

not be interpreted as condemning living wage laws as nothing but a ploy by which unionized municipal workers protect themselves against competition from lower-wage labor that cities might access through contracting out. However, the evidence does add to the literature on “political economy” interpretations of labor market and other policies (for example, 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, however, 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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