

## **Do Living Wage Ordinances Reduce Urban Poverty?**

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**ABSTRACT:** Living wage ordinances typically mandate that businesses under contract with a city or, in some cases, receiving assistance from a city, must pay their workers a wage sufficient to support a family financially. We estimate the effects of these ordinances on wages, hours, and employment in cities that have adopted such legislation. We then examine the effects of these laws on poverty. Our findings indicate that living wage ordinances boost wages of low-wage workers. Moreover, we find a moderate negative employment effect. Finally, some of the evidence suggests that living wages achieve modest reductions in urban poverty.

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## **I. Introduction**

Since December 1994, many cities in the United States have passed living wage ordinances. These ordinances typically mandate that businesses under contract with the city, or in some cases receiving assistance from the city, must pay their workers a wage sufficient to support a family financially. Baltimore was the first city to pass such legislation, and nearly 40 cities have followed suit. Given the increasing popularity of this policy innovation, an empirical investigation of the effects of living wages is in order, to evaluate the claims of beneficial effects made by advocates of these ordinances, and the claims of adverse effects issued by their opponents.<sup>1</sup>

The feature common to all living wage ordinances is a minimum wage requirement that is higher—and often much higher—than the traditional minimum wages set by state and federal legislation. These wage requirements are typically linked to definitions of family poverty. Many ordinances explicitly peg a wage to the level needed for a family to reach the federal poverty line (for example, Milwaukee, San Jose, and St. Paul). Thus, when the federal government defines new poverty lines each year, the living wages in these cities increase. Other localities set an initial wage that is increased annually to take into account increases in the cost of living (for example, Los Angeles and Oakland). Although these latter ordinances may not explicitly state the basis for setting the initial wage, poverty is undoubtedly an underlying factor.

Another feature of living wage ordinances is that they are not flexible regarding family size, even though poverty levels vary dramatically depending on the number of children and adults in a household. Similarly, the ordinances do not take account of the income of other family members; for example, if two adults are working for a covered contractor or grantee, both

would receive the living wage, placing their incomes well above the poverty level.<sup>2</sup> Both of these considerations suggest that living wage laws may do a poor job of targeting needy families, a criticism also leveled at minimum wages. However, living wage laws may affect a substantially different set of workers than do minimum wages, implying that the effects of living wages on poverty require separate study.

Finally, coverage by living wage ordinances is far from universal. Some cities only impose wage floors on companies under contract with the city (for example, Milwaukee and Boston); this is the most common specification of coverage. Others also impose the wage on companies receiving business assistance from the city (for example, Detroit and Oakland); in almost every case, this is added to coverage of city contractors. Still, other cities also impose the requirement on themselves and pay city employees a living wage (for example, San Jose), although this is less common. This lack of universal coverage also limits the applicability of what is known about the effects of traditional minimum wages—which have near-universal coverage—to the effects of living wage laws.

To date, there has been no analysis of the actual effects of living wages on the expected beneficiaries—low-wage workers and their families. In this paper, we estimate the effects of city living wage ordinances on wages, hours, and employment in cities that have adopted legislation. Most importantly, we look at the effects of these ordinances on poverty. Given that an increasing number of cities have passed living wage laws recently, and that campaigns for such legislation are under way in numerous other cities, it is critical to analyze the effects of these laws on low-wage workers and poor families.<sup>3</sup> Only then can policy-makers, employer organizations, labor unions, and voters make informed judgments regarding the merits of this policy innovation.

Of course a finding that living wage laws reduce poverty would not imply that these laws increase economic welfare overall (or vice versa). Surely someone pays for the higher wages induced by living wage laws, and interpersonal comparisons leading to overall welfare calculations are problematic if not insoluble. In addition, living wage laws, like all tax and transfer schemes, generally entail some inefficiencies that may reduce welfare relative to the most efficient such scheme. However, it seems rather clear that policy-makers and the public regard the poverty rate as an important metric, and living wages as a viable means of attempting to reduce it. Thus, the effect of living wage laws on urban poverty is an important policy issue. If living wage laws fail to reduce urban poverty, the principal argument of living wage advocates would be undermined. But if they achieve this goal, then considerations of potential costs of living wages and comparisons with other possible alternatives would become quite important.

## **II. Living Wages and Minimum Wages**

Although city living wage ordinances have received little attention from academic researchers, the effects of standard federal or state minimum wages have been studied extensively, both theoretically and empirically. However, there are important reasons why the effects of living wage ordinances may be quite different. As a result, original research on living wage ordinances is needed to draw reliable conclusions. Nonetheless, the existing work on standard minimum wages provides a useful “road map” for analyzing the consequences of living wage ordinances.

### ***A. Employment Effects***

The theoretical predictions regarding the employment effects of minimum wages are well known. Substitution effects act to reduce employment of low-skilled labor, as employers switch

from now-more-expensive low-skilled labor toward other inputs. In addition, assuming firms were minimizing costs initially, the changed input mix raises costs and therefore prices, reducing the overall scale of output as well as input use (“scale effects”), which also acts to reduce employment of low-skilled labor. The consensus view from earlier time-series studies was that the elasticity of employment of low-skilled (or young) workers with respect to minimum wages was most likely between  $-0.1$  and  $-0.2$  (Brown, Gilroy, and Kohen 1982). More recent studies have spurred considerable controversy regarding whether or not minimum wages actually reduce employment of low-skilled workers. Although this is not the place to review this evidence (see Neumark and Wascher 1996 for a thorough review), a leading economics journal recently published a survey of economists’ views of the best estimates of various economic parameters. Results of this survey—which was conducted in 1996, after most of the recent research on minimum wages was well known to economists—indicated that the median “best estimate” of the minimum wage elasticity for teenagers was  $-0.10$ , while the mean was  $-0.21$  (Fuchs, Krueger, and Poterba 1998). Thus, although there may be some outlying perspectives, economists’ views of the effects of the minimum wage are centered on the earlier consensus range.

Living wage ordinances are sure to be binding for some employers with grants or contracts with their city, although quantitative measurement of the fraction of the covered workforce is extremely difficult.<sup>4</sup> Nonetheless, assuming that some employers will face higher costs for some workers, the predictions of the effects of minimum wages ought to carry over at least qualitatively to the effects of living wages. However, there are at least three unique features of living wages that are likely to weaken their effects, relative to standard minimum wages.

First, the city is a purchaser of goods and services from contractors (and possibly also

grantees). Thus, it is not necessary that its demand curve for particular services slopes downward, or at least not appreciably over some range. This could be the case either because the city finds it possible to raise taxes to cover higher costs (thus largely allowing contractors to pass through the increased labor costs), or because some services have to be purchased in quantities that may be largely insensitive to price (such as snow plowing). On the other hand, a city government has some limits on its ability to raise taxes. In addition, city contractors or recipients of assistance may pay higher wages to workers who are producing goods and services sold on the private market as well (perhaps the same workers who do some covered work and some uncovered work, or different workers working for the same employer). The responses to wage increases for this work done for the private sector are more likely to be subject to the law of demand.

Second, because living wage laws specify wage levels that must be paid without reference to skill levels of workers, employers who do some work covered by these laws and some work that is not covered may reallocate their higher-skill and higher-wage labor to the former and their lower-skill and lower-wage labor to the latter in order to comply. This may still entail some inefficiencies, but could moderate any cost-increasing effects of living wages.

Third, even under broad definitions of coverage by living wage ordinances, only a fraction of the workforce is likely to be covered, in contrast to the near-universal coverage of minimum wage laws. In this situation, some of the labor disemployed in the covered sector is likely to shift into the uncovered sector. Because wages in that sector can adjust downward in response to the outward supply shift, wages may fall for *all* low-skilled workers in that sector, although the state or federal minimum still provides a wage floor that can constrain this response.

However, given the wage decline, employment will not expand enough in the uncovered sector to offset fully the employment decline in the covered sector.<sup>5</sup>

Thus, although the qualitative prediction that higher living wages will adversely affect employment (or hours) of low-skilled workers still stands, there are reasons to expect the effects to be more moderate than those of minimum wages. Possibly offsetting this in practice, though, is the considerably higher wage floor set by many living wage ordinances.<sup>6</sup>

### ***B. Distributional Effects***

Among the low-skilled workers affected by living wage ordinances, there are likely to be winners and losers. Gains accrue to those whose wages are forced up by the wage requirement, and who retain their jobs (and hours) with covered employers. Workers whose employment prospects worsen, who perhaps end up working at lower-wage jobs in the uncovered sector or non-employed, lose from living wages. There are some additional possible winners and losers. First, as low-skilled workers disemployed from the covered sector shift to the uncovered sector, wages there may be bid down somewhat, although we suspect that the magnitudes involved are likely to be small. Second, the changes in employment and in the allocation of low-skilled workers across the covered and uncovered sectors could shift relative demands for higher-skilled workers, with the direction of the effects depending, among other things, on whether low- and high-skilled labor are substitutes or complements in production. In addition, there may be wage increases for higher-skill workers attributable to “ripple” effects stemming from increases in the mandated wage floor (Gramlich 1976; Grossman 1983).

Because we have no a priori basis for predicting the full range of effects on workers, or whether the workers that lose or benefit are likely to be in higher-income or lower-income

families, there are no theoretical predictions for the effects of living wages on poverty. In the minimum wage literature, earlier research speculated that minimum wages would do little to reduce poverty (without addressing the question directly). In particular, it noted that while there are few poor or low-income families with high-wage workers, there are many high-income families with low-wage workers (such as teenagers).<sup>7</sup> If the job loss tends to be concentrated among low-wage workers in low-income families, and the wage gains occur more among low-wage workers in higher-income families, minimum wage increases could hurt poor families or lead to more poor families. In fact, recent research using past experiences with minimum wage increases in the U.S. finds that a minimum wage does not reduce the proportion of families living in poverty, and if anything instead increases it (Neumark, Schweitzer, and Wascher 1998).

This ambiguity in whether minimum wages help poor or low-income families is also apparent in many living wage ordinances. As noted above, the wage requirements set by these laws frequently impose a wage floor pegged to the federal poverty level for a family with a given number of children, without regard to the income earned by other family members. Thus, there are likely to be at least some beneficiaries in families whose incomes are well above the poverty line. Nonetheless, because different workers may work in jobs covered by living wage ordinances compared with jobs affected by minimum wages, the distributional effects of living wage laws could easily differ, and there is no a priori reason to rule out living wages being more beneficial than are minimum wages in reducing poverty.

### **III. Existing Research on the Effects of Living Wage Ordinances**

Because living wage ordinances have been enacted only recently, little empirical research has been conducted on their effects. Most importantly, no one has attempted a systematic

empirical evaluation of the actual (observed) effects of living wage laws on low-wage workers and their families.

The best-known work on living wages is the book by Pollin and Luce (1998, hereafter PL). Although the primary purpose of this book was to advocate living wages as a viable poverty-fighting tool, it is a useful starting point for research on the subject. PL argue that living wage ordinances will deliver a higher standard of living for low-income families. They also posit that such legislation will reduce government subsidy/transfer payments to working families.<sup>8</sup> This work is problematic. First, PL's calculations regarding improved living standards and reduced subsidy/transfer payments are based on a family of four with a single wage earner, despite the facts (given in their book) that (i) only 42 percent of those earning at or below the Los Angeles living wage are the single wage earner in a family, and (ii) the average family size for these workers is 2.1, indicating that on average people potentially affected by living wages are not supporting a family of four. Second, PL do not attempt to estimate whether there are disemployment effects or hours reductions from living wages, but instead assume no such effects. If either results from a living wage increase, then some families may suffer potentially sizable income declines. On the other hand, it is no surprise that calculations based on raising wages of low-wage workers while assuming no employment or hours reductions will look beneficial to low-wage workers and to some extent to low-income families. In short, PL's work cannot be viewed as reliable empirical evidence on the effects of living wages on low-income families. Despite this, calculations paralleling PL's have been used to evaluate proposed ordinances in other cities (for example, Reynolds 1999). Not surprisingly, given the assumptions, these evaluations reach similar conclusions.

Other studies have attempted to predict the loss of jobs that will result from living wages. For instance, two studies use existing estimates from the minimum wage literature and apply them to living wages. Tolley, Bernstein, and Lesage (1999) projected that over 1,300 jobs would be lost in Chicago from the city's living wage ordinance. As noted earlier, though, empirical estimates from minimum wage studies may not carry over to living wages. The Employment Policies Institute (1999) estimates that if all of California adopted a living wage, there would be over 600,000 lost jobs and \$8.3 billion in lost income. This calculation assumes that every firm in California would be subject to a living wage law despite the fact that no such state-level laws exist (or are even in the planning stages, to the best of our knowledge). Also, no current ordinance covers all workers. Finally, this calculation assumes that workers who are laid off will not find jobs elsewhere, again ignoring the limited coverage of living wage laws, which implies the existence of a substantial uncovered sector. Like PL, these studies (and other similar ones) do not attempt to study what has actually happened in localities where living wages have been adopted.

There are two studies that look at living wage effects following the adoption of legislation, although focusing on the costs of city contracts (Weisbrot and Sforza-Roderick 1996; Niedt et al. 1999). These studies of small numbers of contracts in Baltimore conclude that the real cost of city contracts actually declined as a result of living wage ordinances, thus apparently debunking a central argument of living wage opponents. However, a critique by the Employment Policies Institute (1998) of the study conducted by Weisbrot and Sforza-Roderick (1996) questions these results.<sup>9</sup> In addition, because these studies focus on one city, there is no “control sample” of cities in which living wages did not increase with which to compare the cost changes

for Baltimore.

This brief review emphasizes the need for considerably more analysis of the effects of living wage ordinances on workers and families, studying the actual experiences of cities where living wages have been enacted.<sup>10</sup> Proponents of the living wage make strong claims that poverty will be reduced, and opponents make strong claims that some low-wage workers will lose their jobs as a result of living wages, making poverty increases more likely. Empirical evidence is required to resolve these questions.

#### **IV. Data on Living Wage Laws and Labor Market Outcomes**

##### ***A. Living Wage Laws***

We used multiple sources including personal communications with municipalities to assemble information on living wage ordinances. Although a few laws were passed prior to 1996, most came into effect in 1996 or after. For this reason, and because cities cannot be identified in our data set for a period in 1995, we restrict much of the analysis to 1996 and after.<sup>11</sup> Table 1 lists information on living wage laws in all cities, including the wage floors and their effective dates, information on who is covered by these laws, and other details.<sup>12</sup> Not all of these are used in our empirical analysis, as some of the smaller municipalities cannot be identified in our data.<sup>13</sup> Because the analysis focuses on larger metropolitan areas, the failure to identify these municipalities does not contaminate the control group.<sup>14</sup>

##### ***B. Labor Market Data***

The data on labor market outcomes and other worker-related characteristics come from the CPS Outgoing Rotation Group (ORG) files extending from January 1996 through December 2000 and the CPS Annual Demographic Files (ADFs) from 1996 through 2000. The ORG files

provide data on wages, employment, hours, etc., for individuals. In these files, residents of all SMSAs, encompassing all large- and medium-sized cities in the U.S., can be identified. We extract data on these residents for our empirical analysis. In some respects, we would like to know where people work rather than where they live, but such information is not available, and employees of firms covered by living wage laws need not work in the SMSA. Also, the correspondence between cities and SMSAs is imperfect, but because many suburban residents may work in the city, this is not necessarily inappropriate.<sup>15</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>16</sup> This is yet another reason for using only information from this month on.

For several reasons, most of our analysis uses the ORGs, rather than the ADFs. First, as Table 1 shows, there is variation in the months in which living wage ordinances pass. If we primarily used the ADFs, we would restrict ourselves to a single “reading” per year and would lose some variation in living wages across observations. Second, the ADFs would give us fewer observations overall. The ORGs have information on wages only for one-fourth of the sample, but because we get these data in each month, the ORGs provide a sample three times as large. Third, the ADFs are released slowly, while the monthly ORG files are released quickly. For example, the March 2000 ADF was not released until the fall of 2000. In addition, the March files cover the previous year, so not until the March 2001 data were released would we have been able to study the living wage ordinances put into place in 2000. However, the ADFs provide the data set of choice when analyzing the effects of living wages on poverty, because the measures of income in the ADFs correspond to those used in defining poor families, by including non-earned

family income and transfer payments, and by using an annual basis to measure income.<sup>17</sup> We therefore use the ADFs for the estimates of the effects of living wages on poverty. The variables constructed from the ORGs and the ADFs, as well as policy variables and other variables used in the regression analysis, are listed and described in Table 2. Their uses in the empirical analysis are described below.

## **V. Research Design**

\_\_\_\_\_To infer the wage, hours, employment, and poverty effects of living wage ordinances, changes in outcomes for workers and families in cities passing these ordinances are studied, and compared with changes in outcomes in similar periods for workers and families in a control group of cities not passing such ordinances. By looking at changes in outcomes, spurious correlations of living wage laws with unmeasured city-specific factors are avoided. By using cities that do not pass living wage laws as a control group, spurious correlations with changes in outcomes that are common to all cities are avoided. That is, because living wage ordinances are not randomly assigned across either space or time, the research design accounts for the possible correlation of living wage laws with unmeasured influences on labor market outcomes that vary across the cities or years in the sample.

We begin the analysis by asking whether there is evidence that living wage laws succeed in boosting wages of low-wage workers. If they do not, of course, then it is unlikely that any positive (or negative) effects will flow from them. This may seem like a trivial question, with the answer certain to be in the affirmative, but indeed there is no research documenting the extent of compliance with these laws.<sup>18</sup> In contrast, compliance with standard minimum wage laws has been studied and documented (Ashenfelter and Smith 1979), as have the effects of minimum

wages on the wage distribution. A detailed discussion of the specification used to analyze the effects of living wage laws on wages, and of issues that arise in drawing causal inferences from estimates of this specification, is used to provide a more in-depth discussion of the research design that we apply to all of the outcomes that we study.

We estimate a wage equation for various ranges of the wage distribution in SMSAs.<sup>19</sup> Specifically, we look at workers that fall below the 10th percentile, between the 10th and 25th percentiles, between the 25th and 50th percentiles, and between the 50th and 75th percentiles of their city's wage distribution in a particular month. Pooling data across months, we estimate the following regression for each percentile range

$$(1) \quad \ln(w_{ijst}^p) = \alpha + X_{ijst}\omega + \beta \cdot \ln(w_{jst}^{\min}) + \gamma \cdot \max[\ln(w_{jst}^{\text{liv}}), \ln(w_{jst}^{\min})] + \delta_Y Y_t + \delta_M M_s + \delta_C C_j + \epsilon_{ijst} ,$$

where  $w^p$  is the hourly wage for individuals in the specified range ( $p$ ) of the wage distribution,  $X$  is a vector of individual characteristics,<sup>20</sup>  $w^{\min}$  is the higher of the federal or state minimum wage, and  $w^{\text{liv}}$  is the living wage.<sup>21</sup> The subscripts  $i$ ,  $j$ ,  $s$ , and  $t$  denote individual, city, month, and year.  $Y$ ,  $M$ , and  $C$  are vectors of year, month, and city (SMSA) dummy variables.<sup>22</sup>  $\epsilon$  is a random error term.<sup>23</sup> When cities have very few observations for a given month, determining whether a worker falls in a particular range of the wage distribution is impossible or unreliable. We therefore restrict our sample to workers in city-month cells with at least 25 observations. All SMSAs identified in the CPS and meeting the sample size restrictions are included in the analysis, and hence all of those without living wages but meeting these criteria are used as controls. Each table reports the number of cities used in the analysis.

It is essential to control for minimum wages, because many cities with living wages are in states with high minimum wages, and we want to estimate the independent effects of living wages. In addition, we have strong expectations that we should see positive wage effects for the lowest-wage workers stemming from minimum wages, so this serves as a check on the validity of the data.

The living wage variable that multiplies  $\gamma$  is specified as the maximum of the (logs of the) living wage and the minimum wage. This imposes the minimum as the wage floor in the absence of a living wage.<sup>24</sup> As such, there is no “qualitative” difference associated with the existence of living wages, aside from imposing a higher wage floor. However, because living wages may have different effects from minimum wages, the coefficient is allowed to differ from that of the standard minimum wage floor. If living wages boost the wages of low-wage workers, we would expect to find positive estimates of  $\gamma$  when we are looking at workers in relatively low ranges of the wage distribution.<sup>25</sup> Finally, we also estimate specifications in which we lag  $w^{\min}$  and  $w^{\text{liv}}$  by six or 12 months, to allow for slower, adaptive responses to changes in minimum wages and living wages.<sup>26</sup>

Equation (1) uses a difference-in-differences strategy to identify the effects of living wages. 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. This same strategy is used in analyzing the other outcomes (hours, employment, and poverty) considered in this paper. 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. While fixed differences between the treatment and control groups are captured in the city dummy variables, potentially more troublesome is a difference in the time pattern of changes stemming, for example, from a different prior trend in a dependent variable in the treatment and control groups. As the specification only includes year and month dummy variables assumed to have the same effects across all observations, such a difference in the time pattern would tend to be incorrectly attributed to the effects of living wages.

To test for different time trends, the sample was restricted to include only the control group and pre-living wage observations on the treatment group. Specifications for each dependent variable were then estimated adding—in addition to the control variables each one includes—a time trend and an interaction between this time trend and a dummy variable for cities later implementing living wages; the living wage variable was dropped because all observations are taken prior to the introduction of a living wage. The estimated coefficient of the time trend interaction provides a test of differential time trends in the treatment and control groups for the dependent variable in question.<sup>27</sup> In all cases, this estimated coefficient was small and not significantly different from zero, which bolsters the validity of the research design.<sup>28</sup>

## **VI. Empirical Results**

### ***A. Wage Effects***

Table 3 reports estimates of the wage equation. Looking first at minimum wages, the estimated wage effects are quite sharp initially for workers below the 10th percentile. As all coefficient estimates (and standard errors) are multiplied by 100, the estimate implies an elasticity of 0.23. The positive impact between the 10th and 25th percentiles is marginally

significant, and no effects show up at higher ranges of the wage distribution. The effects at the lower percentiles dissipate over time. Six months following a minimum wage increase, the estimated elasticity for workers below the 10th percentile falls to 0.16, and after 12 months the estimated elasticity declines to 0.09 and is not statistically significant. At higher ranges of the wage distribution, all of the estimated minimum wage effects are indistinguishable from zero. This dissipation of the minimum wage effects for low-wage workers is consistent with results reported in Neumark, Schweitzer, and Wascher (1999) using a quite different empirical framework. They suggest that this occurs as nominal wages catch up for other workers. Our replication of those results for minimum wages helps to establish the validity of our data set. However, the minimum wage effects are not central here, so in the remaining analyses of wage effects we focus on the impact of living wages.

Table 3 reveals no contemporaneous effects of living wages for the 0th-10th percentile range and the 10th-25th percentile range. Six months after a living wage increase, the estimated effect for the 0th-10th percentile range is positive, but small and not statistically significant. At a lag of one year, however, we find more strongly significant effects in this range, with an elasticity of 0.07. A lagged effect is not unreasonable. Compliance may well be weaker or slower for living wages than for minimum wages. Moreover, living wage laws are new for most cities in our sample, and implementation of the laws may therefore be a rather drawn-out process, or cities may only enforce compliance as contracts are renewed (as happened, for example, in Baltimore and San Jose). In addition, the smaller elasticity (compared to contemporaneous minimum wage effects) is not surprising, since coverage is much more restricted. There is no evidence of a positive impact for workers in the 10th-25th percentile range, or in the higher

percentile ranges. The fact that we find no wage effects in higher parts of the wage distribution is evidence against some forms of spurious relationships; that is, one could think of the combined evidence for the different percentile ranges as providing a difference-in-difference-in-differences estimate for low-wage relative to high-wage workers. In general, then, these data detect wage-increasing effects of living wage ordinances for the lowest-wage workers, especially about one year after implementation.<sup>29</sup>

Because in the wage analysis the dependent variable is based on wages falling below a certain level (for the 0th-10th percentile range), there is potential bias from endogenous selection. In particular, focusing on the results for the lowest decile of the wage distribution, some fraction of workers whose wages are raised by a living wage law may be lifted above the 10th percentile cut-off, biasing downward any positive effect of the living wage. As the results ultimately point to a positive effect of living wages on this wage measure, this suggests that the results would only be stronger in the absence of this bias. Note, though, that even if all affected workers have their wage increased to a point above the 10th centile, the average wage of those at or below the 10th centile increases; as low-wage workers are “cleared out” from below the 10th centile, the 10th centile increases, and the bottom 10th of the wage distribution is therefore made up of higher-wage workers on average.<sup>30,31</sup> 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 (or within the specified range), rather than measuring a population regression function.<sup>32</sup>

A natural question is why the minimum wage effects dissipate over time, while the living wage effects do not. Of course given the lags with which living wages increase wages (which, as

argued above, is a reasonable expectation), this apparent difference could be solely the result of failure to include longer lags of living wages. Although we do not have a long panel available, we added lags of 18 and 24 months to check this. This resulted in if anything slightly stronger positive wage effects of living wages, so the difference seems real. The simplest explanation for this is that the slow process of implementing and enforcing compliance with living wage laws means that the growing effects of these influences may offset any diminution of effects paralleling those that arise with minimum wages. This is especially likely to be true in a short panel that to a large extent captures the beginnings of living wage legislation; data some years down the road should be able to provide more decisive evidence on this question. It is also possible that because many living wage laws are indexed, employers expect the wage constraint to keep pace with inflation, and hence respond differently than to a minimum wage increase.

We conduct one additional empirical analysis to try to gauge whether we are truly detecting effects of living wage ordinances, or whether instead the effects we find are spurious. In particular, we attempt to estimate wage effects for workers more likely and less likely to be covered by living wage laws. As discussed earlier, coverage of living wages is not universal. Although theory makes no definitive predictions regarding the direction of the effect of living wages on wages of workers in the uncovered sector (see Mincer 1976), we would expect positive effects to be stronger for covered workers, and indeed regard no effect or a negative effect as the most likely outcome for uncovered workers.

Using the limited information we have on workers and the scope of city ordinances, we first attempted to identify those individuals who could potentially work for a company under contract with the city, and are therefore potentially covered by their city's living wage legislation.

For workers in cities where businesses receiving financial assistance from the city are covered, virtually any non-government worker may work for a company that is subject to the legislation. Therefore, we characterize all private sector workers as being “potentially covered” in these cities. Table 4 details our best attempt at identifying all potentially covered workers, or those who work in the “covered sector.”

Next, in equation (1) we replace the living wage variable with a pair of interaction terms—a dummy variable indicating that a worker is potentially covered and a dummy variable indicating non-coverage, each multiplied by the living wage variable. These are intended to reveal the respective effects of living wages on potentially covered and uncovered workers. We also add a series of dummy variables representing the worker subgroups that may be covered by living wages. Since our estimated definition of potential coverage differs somewhat by city, we had to add separate dummy variables for each group. These dummy variables pick up wage differences between the types of workers that are more likely to be covered, and those that are not, which are separate from any living wage effect but might otherwise be reflected in the interactions with the living wage. The estimates obtained should be interpreted with caution. Some living wage ordinances are not explicit about what types of workers are covered. For many localities, we had to make strong assumptions concerning the types of industries in which covered individuals work. Table 4 shows that we chose the broadest definitions of potential coverage, so as not to exclude those that are potentially affected. At best, we identify those workers who could in principle be covered; actual coverage rates should be much lower than those we report. Nonetheless, we suspect that we have probably distinguished between workers more and less likely to be covered.

Table 5 reports the results. For workers below the 10th percentile of their city's wage distribution, those that are identified as potentially covered by legislation appear to experience positive wage effects. These are significant at the 5 percent level when living wages are lagged by 12 months, with an elasticity of 0.11. There are also positive, smaller effects apparent for the contemporaneous and six-month lag, although only the latter is statistically significant. For those unlikely to be covered the evidence points to a negative wage impact if anything, although the estimate falls to zero after 12 months have passed. Wald tests of the equality of coefficients for the 12-month lag specification reveal that the differential effects of legislation on covered and uncovered workers are statistically significant. Overall, although we reiterate the qualification that we only crudely distinguish between covered and uncovered workers, the results are consistent with those workers more likely to be covered by living wage ordinances receiving the bulk of the wage gains, bolstering the case that the effects of living wage laws that we estimate are real; certainly the reverse finding, with stronger wage effects for workers less likely to be covered, would cast doubt on a causal interpretation of the positive overall wage effects reported in Table 3.

### ***B. Employment and Hours Effects***

The specifications we use to study effects of living wage ordinances on employment and hours parallel closely the wage specifications in Table 3. In particular, we define the same ranges of percentiles of the wage distribution, and estimate hours and employment equations for individuals in these ranges. The only complication, however, is that we want to include non-workers in these calculations, but need to impute a wage percentile for them. We do this in a simple fashion with well-known limitations, imputing wages for everyone in the sample, and

using percentiles of the imputed wage distribution for our cut-offs.<sup>33</sup>

Results are reported for employment in Table 6. Turning first to the minimum wage, there is no clear evidence of either positive or negative effects.<sup>34</sup> The employment analysis for living wages points to negative effects in all three specifications for the lowest-wage workers. The effect is statistically significant in the 12-month lag specification, precisely where the significant positive wage effect showed up. The estimates imply that a 10 percent increase in the living wage lowers the probability of employment by 0.0056.<sup>35</sup> Since 44 percent of those in this imputed wage category are employed (last row of table), this estimated effect represents an elasticity of  $-0.13$  ( $5.6/44$ ), evaluated at the mean. Thus, the estimated disemployment effect appears moderate, although account should be taken of the fact that living wage laws do not cover many workers. There is some evidence of positive employment effects for workers in the highest percentiles of the wage distribution, which could be attributable to substitution. The evidence of employment reductions among low-skilled workers stemming from living wages will work against the positive effects on wages reported earlier, when we look at effects on poverty. In looking at hours, we found no evidence of sizable or statistically significant effects of living wages for those at the lower end of the predicted wage distribution, although the point estimates were negative.<sup>36</sup> Thus, only for employment is there evidence of adverse effects of living wages.

### ***C. Poverty Effects***

Finally, the central question in evaluating the policy effectiveness of living wage laws is whether they help families escape poverty. We consider two related questions. First, as suggested in the introduction, living wage laws are designed to help families earn their way out of poverty. Thus, we first ask whether living wage laws increase the probability that families'

earnings exceed the poverty line. Note that the resulting definition of poverty does not correspond to the “official” definition, because we use data on earnings only, and not unearned income, transfers, etc.<sup>37</sup> Nonetheless, the impact of living wages on the ability of families to earn their way out of poverty is an important policy question, as policies that accomplish such goals via earnings rather than transfers tend to attract more political support (for example, the EITC vs. AFDC). Following that, we turn to a parallel analysis using total family income. If fighting poverty is the goal of living wages, these estimates are more appropriate than the estimates obtained using just total family earnings. Not only do they take into account both the gains in family earnings that result from living wages as wages of family members increase, and the declines in family earnings that result when employment or hours are reduced by the legislation, but they also take into account differences in transfer income or other income received as a result of the changing wages, hours, or employment status of family members. As noted earlier, for these analyses we use the (March) ADF files of the CPS.<sup>38</sup>

In both cases, we compute whether a family’s earnings or income are below the poverty line (denoted by  $P$ ), and estimate the following equation

$$(2) \quad P_{ijst} = \alpha + \beta \cdot \ln(w_{jst}^{\min}) + \gamma \cdot \max[\ln(w_{jst}^{\text{liv}}), \ln(w_{jst}^{\min})] + \delta_Y Y_t + \delta_M M_s + \delta_C C_j + \epsilon_{ijst} .$$

We begin, in Table 7, using the 1997-2000 ADFs, which contain information on family earnings and incomes from 1996-1999. This corresponds as closely as possible to the years covered by the ORG data.<sup>39</sup> However, because the ADF covers all individuals in the sample in March, rather than one-fourth (as in the ORGs), there is a larger set of SMSAs for which we have at least 25 observations.

Column (1) presents the estimates of the effects of living wages and minimum wages on poverty based on earnings alone.<sup>40</sup> The estimated living wage effects are consistent with living wages reducing poverty, as the estimates for the contemporaneous, six-month, and 12-month lag specifications are negative and significant. The estimates are stronger the longer the lag, consistent with the pattern of estimated wage effects (although the negative employment effects also strengthened with the lag length). In column (2) we use the larger sample, adding data from 1995 (for which identifying SMSAs in the ADF is not problematic, unlike the ORGs). This results in the estimated coefficients falling in absolute value, while remaining negative and generally significant.

Columns (3) and (4) turn to the analysis of living wage legislation and poverty based on total family income. In both columns, the estimated effects of living wages on the probability that a family is poor are always negative, but statistically significant (at the 5 percent or 10 percent level) only in the 12-month lag specification.<sup>41</sup> The estimates for the effects of living wages using total income to classify families as poor indicate that a 10 percent increase in the living wage reduces the probability that a family lives in poverty by 0.0033 to 0.0039. The implied elasticity is about  $-0.19$ .

It is worth considering whether the estimated effects on poverty are plausible, given the magnitudes of the wage effects noted earlier indicating that a similar 10 percent increase in the living wage boosts average wages of low-wage workers by only 0.7 percentage point. Of course no one is claiming that living wages lift a family from well below the poverty line to well above it. But living wages may help nudge some families over the poverty line, especially when we recognize that this estimated wage impact is an average effect, whereas the more likely scenario

is larger gains concentrated on fewer workers and families. For example, if the 0.7 percentage point average wage increase is concentrated on 10 percent of low-wage workers (1 percent of workers overall), then the implied wage increase for them is 7 percent. If we consider a worker earning the federal minimum, this translates into an earnings increase of \$720 over the course of the year for a full-time worker; for a higher-wage worker the annual earnings increase would of course be larger. If the families of one-third of the affected workers (0.33 percent of all families) are initially poor and are lifted above poverty, then the reduction of poverty would approximately equal the magnitudes implied by the poverty estimates noted in the previous paragraph; while one third may seem high, earnings gains of \$800 or \$900 or more are not out of the question. Thus, even coupled with some employment reductions, if a fair amount of the gains from living wages go to low-income families (and even more so if the losses fall more heavily on other families), it is possible that living wages on net lift a detectable number of families above the poverty line.

The results for minimum wages differ to some extent. In Table 7, columns (1)-(4), the signs of the estimates are consistent with minimum wages initially reducing poverty. But while there is some evidence that minimum wages initially appear to reduce the probability that families are poor, the longer-term (after one year) effects on poverty are smaller and always insignificant.

The difference-in-differences strategy used to identify the effects of living wages on poverty is intended to avoid evidence based on a spurious relationship with other changes occurring in cities passing living wage laws, by using a control sample of cities that did not pass such laws. Nonetheless, state-level policy changes affecting lower-income families may affect

labor market outcomes for low-wage workers and poor families and may be correlated with the passage of living wage laws; in particular, changes in benefits associated with welfare reform and the introduction of TANF could impact the same workers we are studying, and also be correlated with changes in living wages. To address the possibility that state-level changes represent confounding influences in our estimates of living wage effects, we augment equation (2) to use only within-state variation in living wage laws to identify the effects of living wages. The augmented equation is:

$$(3) \quad P_{ijst} = \alpha + \beta \cdot \ln(w_{jst}^{\min}) + \gamma \cdot \max[\ln(w_{jst}^{\text{liv}}) \times LW_{jst}, \ln(w_{jst}^{\min})] + \\ \theta \cdot \max[\ln(w_{jst}^{\text{liv}}), \ln(w_{jst}^{\min})] + \delta_Y Y_t + \delta_M M_s + \delta_C C_j + \epsilon_{ijst} .$$

Equation (3) embodies two changes. First, we assign the living wage to all cities in the state.<sup>42</sup> If no city has a living wage,  $w^{\text{liv}}$  is zero. For almost all states, at most one city in the state has a living wage, in which case all cities in the state get assigned that living wage. In those states where multiple cities (among those included in the analysis) have a living wage, a weighted average based on city size is used for observations in the state.<sup>43</sup> Second, the living wage variable is entered directly, and interacted with a dummy variable for the city (or cities) in which the living wage is actually imposed ( $LW_{jst}$ ), which is set to one for every month in which the city's living wage law is in effect, and zero otherwise (and always for cities without a living wage). This specification allows  $\theta$  to pick up any state-level changes correlated with living wage changes, while  $\gamma$  captures the differential changes in the city in which the living wage is actually implemented. The latter is a more plausible estimate of the causal effect, and corresponds to a difference-in-difference-in-differences estimator using other cities in the same state as the control

sample. This identifies the effects of living wages from the differences in outcomes between cities that have adopted living wage laws and cities in the same state that have not, and hence avoids spurious correlations with state-level policy changes. The results are reported in columns (5) and (6) of Table 7, with column (5) using total family earnings, and column (6) using total family income. The estimates are very similar to the comparable estimates in columns (2) and (4), although the 12-month lag effect on poverty using total income is statistically significant only at the 10 percent level.

Overall, then, there is some evidence suggesting that living wages may be modestly successful at reducing urban poverty in the cities that have adopted such legislation.<sup>44</sup> At the very least, the results provide no evidence and indeed sometimes reject the view that living wages increase urban poverty. Thus, while theory makes no predictions regarding the effects of living wages on poverty, we view the evidence as suggesting that it is more likely that living wages reduce urban poverty than the opposite.

## **VII. Conclusions**

Living wage ordinances mandate wage floors that are typically much higher than the wage floors set by state and federal minimum wage legislation. These ordinances are frequently tied to the federal government's definition of poverty. While traditional minimum wage legislation is nearly universal in coverage, living wages apply to a subset of firms. Only businesses under contract to provide services to the city and, in some cases, firms receiving assistance from the city for the purpose of economic development or job creation, or city employees, are subject to the requirements of these ordinances. Thus, theoretical predictions of the effects of traditional minimum wage laws and the extensive empirical literature that tests

these predictions can only serve as a rough guide to studying the effects of living wages. Their unique features require separate empirical examination.

To date, there has been no attempt to estimate the actual impact that living wage ordinances have had on their expected beneficiaries—low-wage workers and low-income families in the cities where these ordinances have been enacted. In this paper, we present evidence on the effects of these city ordinances on wages, employment, hours, and poverty. This is done by comparing the changes in these outcomes for workers in cities that have adopted living wages to changes for workers in cities that have not adopted them.

Our findings point to positive and significant effects of living wage ordinances on the wages of low-wage workers. In addition to the wage effects, we find moderate negative effects on the employment rates of low-skilled workers. Finally, our estimates provides some evidence that living wage ordinances result in modest reductions in the likelihood that urban families live in poverty.<sup>45</sup>

Living wages have only been in existence for a short time, however, and as yet in a limited number of cities. More work will need to be done to evaluate whether the evidence we find holds up in a larger sample of cities that have adopted such legislation over a longer period of time, and whether alternative research strategies confirm these findings.

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## Endnotes

1. See, for example, <http://www.afscme.org/livingwage/>, for the claim that living wages reduce poverty, and [http://epionline.org/study\\_epi\\_livingwage\\_08-2000.pdf](http://epionline.org/study_epi_livingwage_08-2000.pdf) (p. 29), for the opposite claim.
2. Of course, we would not want a higher minimum for workers from needier families, as this would reduce relative demand for these workers. This points to the potential advantages of income-support policies based on family income (rather than individual income or wages), such as the Earned Income Tax Credit (see, for example, Neumark and Wascher 2001).
3. The Employment Policies Institute maintains a comprehensive listing of current living wage laws and ongoing living wage campaigns on their web page ([www.epionline.org](http://www.epionline.org)).
4. For an ambitious effort in the context of a proposed living wage ordinance in San Francisco, see Alunan et al. (1999).
5. Mincer (1976) models minimum wage effects in a covered and uncovered sector.
6. There are also some possible “second-round” effects of living wage laws. Employers affected by living wage ordinances may in some cases find it more profitable to terminate contracts, grants, tax abatements, etc., with the city. This may reduce the benefits from living wage laws, because those firms most likely to “select out” of city business are those employing the highest shares of low-skilled labor—precisely the workers whom these ordinances are intended to help. In addition, as some firms terminate contracting with the city, fewer firms are left to bid on city contracts, which may—if the number of remaining firms becomes sufficiently small—lead to less competitive bidding and therefore higher prices for city services. Thus, although working out the precise effects of these second-round responses is difficult, it appears most likely that they

reinforce the first-round disemployment effects. On the other hand, the decision of some employers to select out of city contracts and grants may increase competition in the private sector, and lower prices there.

7. For example, while one-third of workers likely to be affected by the 1990 increase in the federal minimum wage were in poor or near-poor families (defined as those with family incomes up to 1.5 times the poverty line based on their family's size), roughly another one-third were in families with incomes exceeding three times the poverty line (Burkhauser, Couch, and Wittenburg 1996).

8. In addition to these benefits for families, PL also use interviews with three Los Angeles employers that paid employees relatively high wages before the living wage ordinance was passed to argue that paying a high wage to workers may be beneficial to firms. Specifically, the interviews suggest that worker turnover and absenteeism were lower because the firms' workers were happier receiving higher wages than their counterparts at other firms. This evidence is purely anecdotal, however. These firms are arguably making a profit-maximizing decision to pay their workers higher wages, and thus their experience does not necessarily predict what would happen to other firms if external legislation mandated raising wages.

9. Among the many problems cited, it is claimed that one of the 23 contracts matched by Weisbrot and Sforza-Roderick was just an extension of a pre-existing contract and not subject to the living wage law. Additionally, many contracts considered as post-living wage contracts actually started before the law went into effect. The study claims that correcting these and other errors reverses the findings.

10. Moreover, none of the studies reviewed in this section appear in peer-reviewed journals, to

the best of our knowledge.

11. Specifically, for part of 1995 standard metropolitan statistical area (SMSA) codes are unavailable in the Outgoing Rotation Groups of the Current Population Survey (CPS) due to phasing in of a new CPS sample based on the 1990 Census.

12. Some living wage ordinances specify two different wage floors, one (lower) applicable when health insurance is provided, and another when it is not. We always report results using the lower wage floor applicable when health insurance is provided. The estimates were similar when we re-estimated the models using the higher wage floor.

13. These include Berkeley, Cambridge, Corvallis, Hayward, Pasadena, San Fernando, Santa Cruz, Somerville, Warren, West Hollywood, and Ypsilanti. We also cannot identify St. Paul separately in the data, and therefore just apply the Minneapolis living wage (which is identical but came two months later) to the Minneapolis-St. Paul SMSA.

14. The analysis ignores county living wages, which are currently on the books in 14 counties (in California, Florida, Illinois, New Jersey, Oregon, Pennsylvania, Texas, and Wisconsin). 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 included in those we study is in Miami-Dade County; however, excluding Miami from the analysis did not influence the results. 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.

15. For expositional ease, from this point on we often refer to cities rather than SMSAs.

16. In a small number of cases, though, outlying counties are excluded from the CPS sampling frame for an SMSA, in which case the data are representative of the remainder of the SMSA.

17. Using the ORGs to study family earnings (rather than income) is problematic. Because there is no monthly poverty threshold, one can either choose some arbitrary method of interpolating annual thresholds by month or face differences in poverty rates driven by the month of the year from which monthly data are drawn.

In addition, with the ADFs we are able to obtain one earlier year of data. Because family income information in 1995 is reported in the 1996 ADF, for which SMSA codes are available, information on family income and city of residence for 1995 can also be used in the empirical analysis. Although most living wage activity starts up somewhat later, data from 1995 are useful in the case of living wages in a couple of cities, and for the control group. (But we also report results with the ADFs using the data beginning in 1996, for comparability.)

18. One reason compliance may be an issue is a lag between initial passage of an ordinance and the adoption and dissemination of guidelines to contractors and others, as well as the establishment of an apparatus to verify and enforce compliance. Sander and Lokey (1998) provide case study evidence from Los Angeles indicating slow but increasing progress toward compliance.

19. For the estimation of wage effects, we restrict our sample to workers with an hourly wage greater than one dollar and less than or equal to 100 dollars. Also, we limit the sample to those between the ages of 16 and 70 years of age, inclusive. The latter restriction is also applied to the hours and employment analyses.

20. These include controls for age, sex, race, educational attainment, and marital status.

21. In the few cases of SMSAs that straddle states with different minimum wages in some years (Davenport-Quad Cities, Philadelphia, Portland, and Providence), we use a weighted average of the minimum wages in the two states, weighting by the shares of the SMSA population in each state (averaged over the months of 1996).

22. For all of the specifications reported in the paper, we also estimated less restrictive specifications using unique dummy variables for each month in the sample (so, for example, for two years of data we would have 23 dummy variables instead of one year dummy variable and 11 month dummy variables). The estimates were virtually unchanged.

23. We also estimated the wage equations using the specified wage percentiles for the city (10th, 25th, 50th, and 75th) as dependent variables, rather than the individual-level data on individuals in these ranges. Because we have different numbers of observations per SMSA-month cell, and we expect estimates to be less precise in cells with fewer observations, we weighted by multiplying the observations by  $(N_{jst})^{1/2}$ ; when the dependent variable is a percentile for a cell rather than a mean, this is the correct weighting scheme as long as the density is the same across cells (Mood, Graybill, and Boes 1974). The results were very similar to those reported in the paper using individual-level data.

24. We could also specify the living variable as the gap between the living wage and the minimum wage; the coefficient on the living wage variable is insensitive to this transformation.

25. Among the SMSAs with a living wage effective in a particular month, the living wage was below the 10th percentile in 17.5 percent of cases, between the 10th and 25th percentile in 64.3 percent of cases, between the 25th and 50th percentile in 15.9 percent of cases, and between the

50th and 75th percentile in 2.3 percent of cases.

26. While independence across cities and months in our sample is assumed, it is unlikely that observations on individuals within a given city-month cell are independent. Because of this, the standard errors that would be obtained from estimating equation (1)'s parameters by ordinary least squares are incorrect. We therefore estimate robust standard errors that relax the assumption of independence (and homoscedasticity) within city-month cells. Corrected standard errors are reported in all tables.

27. If dummy variables unique to each sample month were included, only the time trend interaction would be identified, and the relevant test would still be whether its coefficient was different from zero.

28. We also took this one step further. In particular, for each set of results we report, we estimated specifications including the entire sample period, retaining the differential time trends for the treatment and control groups. Even though in these cases the effects of the living wage and the time trend for the treatment group may be difficult to separate, because living wages tend to grow over the sample period, the estimated effects of living wages on the various outcomes we consider were generally similar to those reported in the tables that follow, sometimes a bit stronger and sometimes a bit weaker.

29. We experimented with lags of different lengths. This relationship is relatively robust, with estimated effects strengthening through about one year as the lag is lengthened.

30. To see this in a simple example, suppose there are initially 50 workers, with five earning a wage of \$5, 20 earning \$6, and 25 earning \$7, so the 10th centile is \$5. Now let one worker's wage go from \$5 to \$7. In this case, the 10th centile rises to \$6, and the average wage of workers

at or below the 10th centile rises to \$5.10.

31. Indeed it is possible in principle, although unlikely, for the entire estimated wage effect to arise due to a disemployment effect among the lowest-wage workers, in which case any wage gains identified by this empirical strategy would not be viewed as salutary. The analysis of poverty effects that follows, however, speaks directly to the question of income gains, taking account of wage increases, employment losses, and other changes.

32. An alternative is to use the predicted wage distribution, although this would be expected to identify less accurately those workers likely to be affected by a living wage law, since, for example, some workers with low observable skills earn high wages. We verified that the results were qualitatively unchanged using the predicted wage distribution to identify low-wage/low-skilled workers. (The regression used to predict wages included controls for education, age (up to a cubic), race, sex, marital status, number of family members, number of children under age 18, year, and interview month.)

33. We obtain the predicted wage in the same manner described in the previous footnote. We use predicted wages for all respondents, whether or not they work, because if we used actual wages for workers and imputed wages for non-workers we would rarely have non-workers in the extreme percentiles of the wage distribution. Another alternative is to use the actual wage for workers and a predicted wage plus a randomly-generated residual for non-workers. But this would imply asymmetric treatment of the observations based on the dependent variable, with more accurate classification for those holding a job. Finally, trying to infer something about residuals for non-workers based on their decision not to work requires identification of a selection model, which we think is tenuous. However, we would expect the market wages faced

by those who choose not to work to be on average lower than those faced by observationally-equivalent individuals who choose to work (paralleling the standard sample selection problem).

To assess the consequences of this in a simple manner, the estimates were re-calculated reducing the predicted wages of the non-workers by 5 percent and 10 percent. The results were similar.

34. This evidence is not strictly comparable to much existing work estimating employment effects of minimum wages using CPS data, as the data set covers a short sample period and is restricted to metropolitan areas. We have looked into the metropolitan/non-metropolitan distinction, and find more evidence of negative minimum wage effects in non-metropolitan areas, which is not surprising given that they are lower-wage areas.

35. The coefficients in the table are multiplied by 100, so the coefficient estimate of -5.62 implies that a one-unit change in the living wage variable reduces the probability of employment by 0.0562. Since the living wage variable is expressed in natural logs, a one-unit change represents a 100 percent increase; we therefore divide by ten to approximate the effect of a 10 percent increase in the living wage. The same calculation is used in describing the poverty results below.

36. These results are not reported in the tables, but are available from the authors upon request.

37. In this analysis we exclude families with members aged 65 years or older. Because of Social Security, those who are at least 65 are more likely to have substantially greater income than earnings.

38. We treated related subfamilies as distinct families for the analysis reported in the paper. The results were qualitatively similar but a bit weaker statistically when we combined related subfamilies and primary families.

39. The 2001 ADF covering 2000 was not available when this research was conducted. We did confirm, though, that using the ORG data through 1999 yielded the same wage and employment effects as reported in Tables 3, 5, and 6.

40. Since the ADFs contain earnings and income information from the prior calendar year, the estimated effects of the December living wage, the June living wage, and the January living wage, correspond roughly to the effect of the contemporaneous living wage, the living wage six months ago, and the living wage 12 months ago in the ORGs, respectively. The same is true of minimum wages. Estimates were also obtained using a weighted average of the applicable minimum and living wage in the SMSA over the year. As might be expected, the estimated effects were quite close to the estimated effects using the June (mid-year) minimum wage and living wage.

41. We also estimated the model using an 18-month lag specification, which yielded negative and significant effects of living wages on the probability that a family was poor using either measure.

42. For this analysis, individuals in SMSAs with living wages that straddle states (Portland and St. Louis) are assumed to be part of the state where the bulk of the SMSA residents live (Oregon and Missouri, respectively).

43. For these latter states, if we simply dropped the smaller living wage cities in the state, and used the living wage for the largest city for all remaining observations in the state, the results were virtually unaffected.

44. In other work (Neumark and Adams 2001) we explore the effects of different types of living wage laws, and find that positive wage effects and negative employment effects occur primarily

in response to living wage laws that cover employers receiving business assistance from cities—living wage laws that have broader coverage than those covering contractors only. In results not reported in the table, we verified that the anti-poverty effects of living wage laws similarly arise for those laws covering business assistance recipients, and not solely contractors.

45. A recent paper by Bertrand, Duflo, and Mullainathan (2001) looks at the impact on standard difference-in-differences estimators of serial correlation in the error term (and the data) across observations on the same unit (in this case, cities). It finds that, especially in the absence of statistical diagnostic tests, these estimators are likely to lead to biased and often understated standard errors, and hence erroneous findings of statistical significance. 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, e.g., by city and month, as done in the regressions reported in Tables 3, 5, and 6). However, the resulting standard errors are likely conservative (if anything too large), because no structure is imposed on the serial correlation. This estimator was implemented for all of the key specifications underlying the conclusions in this paper. While the standard errors generally rose somewhat, the changes were not dramatic. The results for the business assistance living wage laws—which are the types of laws for which significant effects tend to be found—remained statistically significant at the 5 or 10 percent level. And most of the results based on specifications that did not distinguish between types of living wage laws remained statistically significant at the 5 or 10 percent level (the only exception being the results for poverty based on total income, which are not always strongly significant anyway).