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THE ECONOMIC EFFECTS OF LIVING WAGE LAWS:  
A PROVISIONAL REVIEW

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The Economic Effects of Living Wage Laws: A Provisional Review

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**ABSTRACT**

Nearly 100 cities and local governments in the United States passed living wage laws since the mid-1990s. The central goal of living wages is to reduce poverty, yet they may fail to do so because of disemployment effects. We summarize and critique the existing research on the effects of living wages on wages, employment, and family income, emphasizing common findings, points of disagreement, and important questions for future research. The evidence thus far points to wage increases as well as employment losses for the least-skilled – although there is disagreement about the employment effects – but on net some beneficial distributional effects. The evidence also points to efficiency wage-type effects of living wage laws that may offset some of the adverse impacts on employers.

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## I. Introduction

Campaigns to implement living wages in metropolitan areas across the United States have been pursued with vigor since the nation's first living wage law was passed in Baltimore in 1994. The total number of living wage laws now in effect in the United States—including cities of all sizes, as well as counties and school boards—is near 100 at the time of writing.<sup>1</sup> Living wage laws are now on the books in 10 of the 20 largest cities based on the 2000 Census of Population, and campaigns are under way in additional cities. Brenner (2003) estimates that 40 percent of the population in cities larger than 100,000 resides in cities with living wage laws on the books. In our view, the success of the living wage movement in enacting living wage laws across the nation is one of the most striking developments in public policy in recent history.

Living wage laws have three central features. First, they impose a wage floor that is higher—and often much higher—than traditional federal and state minimum wages. Typical living wage levels as of the beginning of 2002 were \$7.72 (Los Angeles), \$8.83 (Detroit), and \$10.25 (Boston). Second, living wage levels are often explicitly pegged to the wage level needed for a family with one full-time, year-round worker to reach the federal poverty line. And third, coverage by living wage ordinances is generally quite narrow. Frequently, cities impose wage floors only on companies under contract (generally including non-profits) with the city. Other cities also impose the wage floor on companies receiving business assistance from the city, in almost every case in addition to coverage of city contractors. Finally, a much smaller number of cities require that city employees be paid a living wage.<sup>2</sup> Information on the living wage laws covering cities large enough for us to analyze reliably is reported in Table 1.

Like minimum wages, the central goal of living wages is to raise incomes of low-wage workers in order to reduce poverty. For example, the Economic Policy Institute argues that “the living wage is a crucial tool in the effort to end poverty.”<sup>3</sup> Thus, in our view, the key research question with regard to living wages is whether they help low-wage workers and low-income families. This question has been at the core of our research on living wages (Neumark and Adams, 2003 and forthcoming; Adams and

Neumark, forthcoming; Neumark, 2002), as well as other work following up on this research. In this review, therefore, we summarize the existing research on this question, as well as outlining what we view as the most important questions that need to be considered regarding whether living wages achieve their main policy goal.

A more recent but increasingly active area of research on living wages focuses on their microeconomic effects. This research focuses less on what happens to the general population or workforce of cities in which living wages are passed, and more on understanding how covered firms and workers are affected. This research is less relevant to assessing the policy effects of living wages, but more relevant to testing for behavioral responses to living wage laws. Furthermore, the two types of evidence are complementary, and the micro-level evidence can potentially provide important confirmation of more aggregate findings. For example, if we find wages of low-wage workers rising in cities passing living wage laws, we should see evidence of similar effects among covered employers. On the other hand, the converse need not hold. For example, it is conceivable that living wage laws affect too few workers to lead to detectable changes in employment at the city level, yet a closer look at affected firms may reveal employment declines. Such a finding would help confirm theories predicting that living wages reduce employment, while also suggesting that the policy impact at the city level is too small to be detected.

This review is limited in three ways. First, it focuses on research on the economic effects of living wages. There are other interesting issues that researchers have begun to address, including the political and social movement aspects of living wage campaigns (Nissen, 2000; Zabin and Martin, 1999), the incentives of actors in living wage campaigns (Neumark, 2001), and the implementation and administration of living wage laws (Luce, 2003; Sander and Lokey, 1998).

Second, we ignore the earlier wave of “impact” studies that tried to assess the consequences of living wage laws before they were implemented in cities, which was quite a growth industry in the late 1990s. Ultimately, these impact studies reflect educated guesses. These studies have a role to play in helping cities try to develop some understanding of how living wages might impact the unique

jurisdictions with which they are concerned. But from the perspective of public policy research that aims to establish the effects of living wage laws, given that living wages are now on the books in many cities, researchers should instead be turning to more sound social scientific methods of policy evaluation based on treatment and comparison groups.<sup>4</sup>

The third limitation is that the review is provisional. About the only thing growing faster than the number of living wage laws is the volume of research on living wages. This is evidenced by the reference list for this paper. Of the 25 papers on living wages referenced there, only 11 are—by our count—published or due to be published in peer-reviewed outlets, and the first one on economic effects of the living wage was published in 2003. Aside from this limitation regarding the existing work, the review is provisional because our experience with living wages is, to this point, limited to a small number of years for most cities. Clearly, then, our state of knowledge about the economic effects of living wages is far from settled, and there is likely a great deal to be learned in the future relative to what we have learned thus far. As a consequence, while we try to provide a thorough review of what we have learned so far, we also emphasize along the way and in the concluding section what we see as critical questions for future research.<sup>5</sup>

## II. The Effects of Living Wages on Low-Wage Workers and Low-Income Families

As noted above, perhaps the principal rationale for living wages is to help low-income families. Although there is generally no single measure with which the distributional effects of a policy like living wages can be unambiguously assessed, focusing on whether or not living wages help lift families out of poverty strikes us as a reasonable criterion for two reasons. First, to some extent the distributional goals of living wages (like minimum wages) are expressed in terms of fighting poverty. And second, many cities' living wage laws explicitly set a wage designed to ensure that “typical” families (of three or four persons with one earner) earn a wage sufficiently high to reach or exceed the poverty line.

While mandating higher wages for low-wage workers may at first seem a natural way to fight poverty, there are two reasons why such mandates may not help to achieve this goal. First, standard economic theory predicts that a mandated wage floor will discourage the use of low-skilled labor,

operating essentially as a tax on the use of such labor. Thus, whatever wage gains accrue to workers whose employment is unaffected may have to be offset against potential job losses for some workers. Second, mandated wage floors may ineffectively target low-income families (e.g., Burkhauser, et al., 1996). Broadly speaking, low-wage workers in the United States belong to two groups. The first is very young workers who have not yet acquired many labor market skills, but who are likely to escape low-wage work as skills are acquired. The second is low-skilled adults who are likely to remain mired in low-wage work (Carrington and Fallick, 2001), and who—as adults—are much more likely to be in poor families. To the extent that the gains from mandated wage floors accrue to low-wage adults and the employment losses fall on low-wage, non-poor teenagers, mandated wage floors may well reduce poverty. But there is no theoretical reason to believe that this outcome is more likely than the reverse, with concomitant adverse outcomes for low-income families. Thus, while theory predicts employment losses and hence implies that there are likely to be both winners and losers from mandated wage floors, whether living wages have beneficial distributional effects is a purely empirical question.<sup>6</sup>

This ambiguity has the further implication that different types of wage floors may have different distributional effects. Making this concrete, the text below explains that we find positive distributional effects of living wages. Yet other research finds adverse distributional effects of minimum wages (Neumark, et al., 2002). Because the distributional effects depend on both the magnitudes of the wage and employment effects (and other effects), and on their incidence throughout the family income distribution, the gains and losses from living wages may be of quite different magnitudes, and fall at different points in the distribution of family income, than do the gains and losses from minimum wages. This depends in part on the types of workers who are affected by these alternative mandated wage floors. More to the point, evidence regarding the distributional effects of one type of wage floor does not carry over to a quite different type of wage floor.<sup>7</sup>

The preceding considerations have provided the basis for much of our research on living wages. Broadly speaking, this research has focused first on estimating the wage gains and employment losses to

workers—hence identifying the gains and losses to low-wage workers—and second on estimating whether on net living wages succeed or fail in lifting families above the poverty line.

*Our Past Findings on Wage, Employment, and Poverty Effects*

Our research on living wage laws has focused on estimating the effects of these laws on wages and employment of low-skill individuals and on poverty rates among families in the urban areas in which living wages had been implemented, based on comparisons with a control group of cities in which living wages were not implemented (and on comparisons of cities where living wages were raised relative to other cities). Here we describe the most up-to-date evidence we have available, using monthly Current Population Survey (CPS) data that extend from 1996 through 2002.

This research begins by asking whether there is evidence that living wage laws lead to detectable increases in wages at the lower end of the wage or skill distribution. Because living wage are presumed to affect the lowest-skilled workers, our analysis of wage effects focuses on workers in the bottom tenth of the wage distribution in each city-month cell. The model is specified to control for the minimum wage, and to identify the effect of the living wage from increases in the living wage above the minimum wage, which would otherwise be the wage floor. We include some skill-related and other control variables, including education, age, marital status, race, and sex. We also include year and month dummy variables to control for common changes across cities in the sample that could otherwise be confounded with living wage effects, because living wages were typically enacted later in the sample period. City dummy variables are included to control for the possibility that living wage laws were passed in cities that differed systematically from other cities (e.g., with either particularly high or low wages), which would again confound the estimation of their effects. Thus, our specification identifies an effect of the living wage when the dependent variable changes differentially in cities that passed living wage laws (or increased their living wage) relative to cities that did not pass living wage laws—a difference-in-differences research design. Finally, 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, the treatment and control groups are comparable. To relax this assumption we allow differential trends

for cities that do and do not pass living wage laws (and, below, for cities passing different types of living wage laws).<sup>8</sup> The differential trends allow for things like economic conditions that are changing differentially between the treatment and control groups.

Following findings in our past research, we estimate effects contemporaneously, and at six- and 12-month lags. Typically the effects show up at a lag of 12 months, so we highlight these estimates here. Living wage effects may arise with a lag for a couple of reasons. First, in many cities there was an initial period of ambiguity and slack implementation and enforcement between the time when a living wage law was passed and city administrators began to implement the law in a serious way. For example, Sander and Lokey (1998) document a period of about a year after Los Angeles passed its living wage law during which city administrators interpreted exemptions too broadly, did not provide contractors with information about how to comply, and did little follow-up with covered contractors. Second, living wages are only applied as contracts are renewed.<sup>9</sup>

As reported in the first column of Table 2, when we treat all living wages uniformly, the evidence on wages points to an elasticity with respect to the living wage of 0.04,<sup>10</sup> which is not statistically significant at the ten-percent level. However, we have typically found that when we classify living wages as belonging to one of two groups—contractor-only living wage laws and business assistance living wage laws—the effects (on all outcomes) are considerably sharper for the living wage laws with business assistance provisions.<sup>11</sup> We study this question by augmenting the basic specification to allow a distinct effect of each type of living wage law. Once we draw this distinction, the evidence on wages points to a significant impact only of business assistance laws, with an estimated elasticity of 0.07 that is significant at the ten-percent level. In contrast, the estimated impact of contractor-only laws is small and statistically insignificant.<sup>12</sup>

Of course, the potential gains from higher wages may be offset by reduced employment opportunities. We use the same basic empirical framework to study employment, with only two differences. First, we estimate linear probability models for individual employment status. Second, we cannot classify non-working individuals based on their position in the wage distribution. Instead, we

impute wages for everyone and group individuals based on their position in the distribution of imputed wages, or “skills.” For living wages generally, we find an estimated disemployment effect that is significant at the five-percent level at a lag of 12 months for those in the bottom tenth of the imputed wage distribution, paralleling the wage results. The regression coefficient of  $-0.053$  in Table 2 is from a regression of the employment rate on the log of the living wage. Thus, the estimate implies that a one log unit increase in the living wage reduces employment by 5.3 percentage points. Given an employment rate of 43.4 percent in the lowest decile of the imputed wage distribution, this represents a 12 percent employment reduction, or a  $-0.12$  elasticity. When we estimate separate effects of business assistance and contractor-only living wage laws, both estimates are negative, but we find a significant disemployment effect only for business assistance living wage laws, with an elasticity of  $-0.17$ , and a much smaller (and insignificant) estimate for contractor-only laws.<sup>13</sup>

The evidence on wage and employment effects sets the stage for weighing these competing effects, in particular examining the effect of living wage laws on poverty. To examine the impact of living wages on poverty, we estimate linear probability models for the full sample of families from March Annual Demographic Files of the CPS covering 1995 through 2001. The dependent variable is a dummy variable equal to one if a family’s income falls below the federal government’s threshold for poverty, and zero otherwise. The specification includes city and year dummy variables, and differential trends for cities with and without living wage laws (or with different types of living wage laws).

The evidence yields negative point estimates (implying poverty reductions) for both types of living wage laws, but only the estimated effect of business assistance living wage laws is statistically significant (at the ten-percent level). For business assistance living wage laws, the estimated coefficient of  $-0.024$  in Table 2 implies that a one log unit increase in the living wage reduces the poverty rate by 2.4 percent. Relative to an 18.6 percent poverty rate, this represents a 12 percent reduction, or an elasticity of  $-0.12$ . This seems like a large effect, given a wage elasticity for low-wage workers below 0.1. Of course, no one is claiming that living wages can lift families from well below the poverty line to well above it. But living wages may help nudge families over the poverty line, and these average wage effects

are likely manifested as much larger gains concentrated on a possibly quite small number of workers and families. Thus, even coupled with some employment reductions, living wages can lift a detectable number of families above the poverty line. We return to this point later, in the discussion of Brenner's (2003) estimates of the number of workers that experienced wage increases as a result of Boston's living wage law.

It might be viewed as curious that, in Table 2, the estimate for contractor-only living wage laws, although insignificant, is larger. Of course, the offsetting positive wage effects and negative employment effects of business assistance living wage laws imply that these laws need not have a stronger effect on poverty. Yet the larger estimate for contractor-only laws is puzzling in light of the absence of wage or employment effects of these narrower laws. It may in part reflect evidence reported in Neumark (2001) that contractor-only living wage laws boost the wages of unionized municipal employees, presumably because these laws reduce competition from privatization and hence strengthen the bargaining position of these workers.

Although the analysis just described refers to the effects of living wages on poverty, the question of the distributional effects of living wages is more general. We have carried out two related analyses intended to provide a more in-depth analysis of the distributional effects of living wages. First, we use the same difference-in-differences framework to estimate the impact of living wages on the "depth" of poverty among poor families (the percentage by which the family's total income falls short of the poverty threshold). The estimates reveal no statistical evidence of effects of living wage laws, suggesting that the gains from living wages lifting some families above the poverty line are not offset by income declines, on average, among families below the poverty line.

Second, we examine the effects of living wages on the probability that families are below a wider range of family income thresholds—ranging from one-quarter of the official federal poverty threshold to two times the threshold. Living wage laws may have effects above the poverty threshold either because living wages are commonly pegged to an income level for families with one adult worker that is above this threshold (as in, e.g., San Jose, Hartford, and Madison), or because families affected by living wages

have more than one worker. Consistent with the findings on the depth of poverty, we find no significant effects of living wage laws at thresholds well below the poverty line. However, the evidence indicates that living wages—particularly the business assistance variety—reduce the probability that families’ incomes are below three-quarters of the poverty threshold, and the estimated effects are in the same direction and larger (and also significant) at 1.5 times the poverty threshold. Thus, this richer analysis of the effects of living wage laws suggests that in addition to reducing the probability that families are poor, living wages also deliver some gains for families a bit below and a bit above the poverty threshold. On the other hand, the results could reasonably be read as suggesting that living wages do not help the poorest families, which is not entirely surprising because such families are less likely to have workers or may have the lowest wage workers who are most likely to bear the disemployment effects of living wages. An important task for future research, however, is to try to get “inside the black box” of the distributional effects of living wages, to see, for example, whether micro-level evidence confirms that many of the gains from living wages accrue to poor families, and more generally to explore which families (with which types of workers) gain or lose.

Finally, research by Toikka (forthcoming) revisits the issue of the tradeoffs between higher wages, government transfers, and taxes, cautioning that the role of living wages in helping to alleviate poverty is diminished because of the high implicit marginal tax rates faced by low-income families once account is taken of the consequences of payroll taxes, eligibility for social programs such as Medicaid, etc. He uses a sample drawn from the SIPP for seven large cities, attempting to focus on those families with at least one worker whose earnings would be affected by the living wage law in that city, and finds that given the high marginal tax rates faced by low-income families, total income goes up by considerably less than total earnings if we simply assume that workers are swept up to the living wage. However, there are a couple of problems with this analysis. First, Toikka does not have the ability to study workers affected by the living wage per se. Instead, all he can do is look at workers below a given wage floor (and he uses a common wage floor across the seven cities he examines). To the extent that this analysis is intended to go inside the black box and study how living wages might affect poverty, this shortcoming is

serious. As already pointed out, depending on which workers are covered by a particular type of living wage floor, the distributional effects can vary widely. Second, advocates of living wages also argue that living wages will reduce government transfers to low-income families (e.g., Pollin and Luce, 1998, Chapter 5). This suggests that whether it is in some sense “good” if families experience a sharp increase in the share of income coming from earnings, even if total income changes far more modestly, is a philosophical question related to issues of the virtue of self-sufficiency, the dignity of making a living, etc. We are not in a position to take a stand on this question. We do note, however, that Toikka’s same argument could be used to suggest that the benefits from increased skills, education, and educational quality among the disadvantaged would deliver far fewer gains than suggested by the (pre-tax and pre-transfer) wage regression estimates to which we usually appeal. We doubt, though, that he (or many others) would be led from this point to argue that such investments are not very valuable to the individuals who would benefit from higher earnings.<sup>14</sup>

### III. Business Assistance vs. Contractor-Only Living Wage Laws

An important issue that arises in our past work is why we tend to find little evidence of effects of contractor-only living wage laws but stronger effects of business assistance living wage laws. This has turned into a rather contentious question and one that we still regard as unresolved.

We have conjectured that aside from business assistance living wage laws affecting more workers, the two types of laws may have qualitatively different effects. In particular, a few factors could lead to stronger effects of business assistance provisions. In the case of a contractor-only law, it is typically required that employers pay the mandated wage for work done as part of the contract. Assuming that contractors’ employees do some other work in addition to city contracts, employers can mitigate the costs of complying with living wage laws by reallocating their higher skilled or higher seniority (and therefore higher wage) labor to the contract work and their lower wage labor to the non-contract work, or even by reducing wages on non-contract work. Of course the extent to which employers can take these steps depends on the share of their business tied up in city contracts; if contracts are a small component of sales or labor costs, it is conceivable that the effects of a living wage law could be largely dissipated. In

contrast, there may be fewer avenues for mitigating the costs of living wage laws for employers covered under business assistance provisions. For example, an establishment created with the help of business assistance from a city would appear to have no choice but to pay all employees no less than the mandated living wage for all of their work. In addition, although contractors can reduce the share of their business with the city in response to a living wage (and, similarly, cities can shift business to higher wage employers), business assistance recipients may have less leeway as they may have accepted long-term benefits, such as bond financing or tax relief, in return for locating in the jurisdiction. In some cases, at least, relocation costs seem likely to far outweigh costs from shifting business away from city contracts.<sup>15</sup>

Our findings of stronger effects of business assistance living wage laws have met with some skepticism, based on the argument that few workers have actually been affected by the business assistance provisions of living wage laws. This argument has sometimes been stated too extremely, in our view. For example, Brenner, et al. (2002) argued that none of the cities we studied had implemented business assistance provisions of living wage laws during the sample period covered by some of our earlier work. As we have noted in response to this criticism (Adams and Neumark, forthcoming), conversations with the individuals with responsibility for implementation of living wages counter this claim, and the claim is certainly less relevant to our more recent work that extends the sample period through 2002. A related argument against the credibility of our findings of stronger effects of business assistance living wage laws comes from those directly involved with living wage campaigns and living wage implementation, with whom we have interacted in private conversations, at conferences, etc. These “activists,” typically drawing on isolated cases, believe that business assistance living wages laws are, in fact, a very weak tool, modestly increasing the share of workers covered by the contractor provisions of living wage laws, and weakly enforced. All in all, though, there is not at this point a good empirical case against the potentially stronger effects of business assistance living wage laws. Nonetheless, we acknowledge that there is little direct evidence that business assistance living wage laws cover many more workers, or that their economic effects are stronger. Indeed, we regard serious efforts to assess coverage by different

provisions of living wage laws, and to more systematically assess enforcement of these laws, as important areas for future research.

In our view, though, a stronger and more substantive alternative explanation of our findings is that business assistance living wage laws are not fundamentally different or stronger in terms of the underlying economics or the share of workers affected. Rather, business assistance living wage laws may have arisen in cities where a constellation of forces often related to initial living wage campaigns has resulted in stronger living wage laws generally, and perhaps more important stronger implementation and enforcement because of both how the cities oversee the laws and how much community and other groups remain engaged.

To explore this hypothesis more fully, we have recently completed a study that attempts to account in a richer way for the wide variety of living wage laws across cities, and hence to see whether the apparently stronger effects of business assistance living wage laws are in fact reflective of other characteristics of living wage laws that are associated with whether or not they include business assistance provisions. Although we have thus far focused on contractor-only versus business assistance provisions, living wage laws also differ in terms of factors such as: the extent of coverage specified; the employers or workers exempted; enforcement; other provisions such as community hiring or labor peace agreements; and whether living wage laws exist in nearby jurisdictions that might constrain the ability of covered employers to shift business to work not covered by a living wage law, or to make other adjustments, hence effectively amplifying the effects of a given law.

Table 3 summarizes many of these features of living wage laws by city.<sup>16</sup> First, we indicate whether contractor coverage is narrow or broad, relative to most cities, in column (1). For example, Portland's living wage laws applies only to custodial and security workers and parking attendants, and is therefore classified as "narrow," while the laws in Los Angeles and Oakland cover some leaseholders (and in the case of Oakland the port) and are therefore classified as "broad." Next, column (2) classifies business assistance coverage, with three of the cities with business assistance living wage laws coded as having narrow assistance coverage. As an example, San Jose is coded this way because its law only

applies to recipients of direct financial assistance, and excludes forms of assistance that in other cities trigger coverage. One important feature of business assistance living wage laws that was suggested to us by a prominent living wage organizer is whether leaseholders are covered.<sup>17</sup> This is coded in column (3) of the table. Columns (4)-(6) indicate whether the living wage laws include community hiring provisions and labor peace or no strike agreements, and whether the living wage is superceded by collective bargaining agreements.

Column (7) provides a classification of cities based on their enforcement and implementation of the living wage law. This information comes from an unpublished book manuscript by Luce (2003, Table 5.2) that categorizes enforcement/implementation efforts by city based on a survey of living wage laws and city staff, as well as auxiliary information.<sup>18</sup> Based on Luce's discussion of how and why implementation and enforcement differ across cities, we believe that her classification comes very close to capturing the differences—discussed earlier with reference to alternative explanations of the apparently stronger effects of business assistance living wage laws—that arise in part from the dynamics of living wage campaigns and the efforts of community and other groups to remain engaged.

Finally, columns (8) and (9) of Table 3 report whether or not there is a county living wage law or a law in nearby cities (and the first effective year). Both of these characteristics of living wage laws are meant to capture whether a city's living wage law is likely to have its effect strengthened by the presence of nearby living wage laws. Part of the importance of this distinction arises from the nature of the data. In particular, we use CPS data that classify people by where they live, not where they work. Thus, any effect of living wage laws is likely to be amplified when there are nearby living wage laws, simply because more workers in the particular city are in fact covered by a living wage law. But nearby living wage laws may also amplify the effects of any city's living wage law because of the underlying economics, and we suspect this is the more important phenomenon. For example, consider an employer who has business with multiple cities. If a small share of the business is covered by a living wage law, because only one of the cities with which the employer has contracts is covered, then there will be more scope for mitigating the effects of a living wage law. But if nearby cities have living wage laws, more of

the employer's work is likely to be covered by such laws and therefore the employer would have less scope for reducing wages on other work done by its employees, and also less scope for shifting business away from the city that imposes the living wage law and toward cities without living wage laws. Similarly, if covered employers attempt to adjust to living wages by shifting to a more-skilled workforce, there will be less scope for doing this, or the cost will be higher, if similar firms in nearby cities are trying to do the same thing. We would therefore expect less mitigation of the costs of a living wage, and larger adjustments, when there are nearby living wage laws.

Our analysis using the information in Table 3 addresses both a narrow and a broad question. The narrow question is whether the contractor-only versus business assistance distinction is the core distinction, or whether instead these two types of laws are associated with other features of living wage laws that are the ones that really matter. The broad question, and one that we only begin to address with this research, is how the many different features of living wage laws influence the effects these laws have on urban labor markets. In particular, we try to identify those characteristics of living wage laws that are most strongly associated with wage and employment effects.

Two qualifications bear emphasis. First, as we disaggregate living wage laws by their characteristics, we face limited observations on cities with different varieties of living wage laws, making it difficult to draw strong statistical inferences, and instead yielding evidence that is more suggestive. And second, while we noted the importance of accounting for a variety of features of living wage laws and their implementation, there may also be features of living wage campaigns that are not reflected in either the laws that result or the observable features of enforcement, but yet which affect labor market outcomes in their aftermath. These might include, for example, changed norms of behavior affecting wage setting. To some extent, these influences may simply be captured as common influences of all living wage campaigns. But to the extent that they differ across campaigns, they remain an omitted source of variation in our empirical analysis.

The role of heterogeneity in living wage laws is examined in two ways. First, we simply try to gauge how the wage and employment effects of each type of living wage law differ from other laws,

paralleling what we did earlier when considering contractor-only and business assistance living wage laws. Whereas our contractor-only/business assistance results were based on specifications with separate effects of these two types of living wage laws, here, instead, we draw the distinction between each of the characteristics of living wage laws listed in Table 3, and all other laws. These specifications reveal whether—like business assistance living wage laws—some other types of living wage laws have particularly pronounced effects. Second, to see whether these other features can account for the apparently stronger effects of business assistance living wage laws, we estimate models in which we interact dummy variables for these other characteristics of living wage laws with a dummy variable indicating a business assistance law. These specifications ask, more specifically, whether some of these other characteristics of living wage laws actually account for what we have previously interpreted as stronger effects of business assistance living wage laws.<sup>19</sup> A related analysis interacts each characteristic of living wage laws with the separate effects of contractor-only and business assistance living wage laws.

All told, the evidence does not point unambiguously in one direction, and is not statistically overwhelming. However, we read the evidence as pointing towards one alternative to the view that it is the business assistance/contractor-only distinction that is critical. In particular, as Table 3 shows, cities with business assistance living wage laws are considerably more likely to also have a living wage in nearby cities. And the regression estimates suggest that it is this latter distinction that more sharply delineates living wage laws with stronger positive effects on wages and negative effects on employment. Thus, the variation in the effects of different types of living wage laws could have more to do with the concentration of many living wage laws in contiguous or nearby labor markets, rather than with coverage of business assistance recipients. Some may find this source of variation in the strength of living wage effects more palatable, in part because of skepticism about the enforcement and implementation of business assistance living wage laws, and in part because the mechanism by which “overlapping” living wage laws enhance the effect of each living wage law is perhaps more straightforward than the mechanism by which business assistance provisions enhance their effect. Regardless of the precise conclusion one draws on this point, we believe this research points to the potential importance of

accounting for variations in living wage laws that—aside from the level of the mandated wage floor— influence their effects on wages, employment, and other outcomes as well.

In our view, this evidence suggests that developing a better understanding of the effects of different types of living wage laws is a central question for future research on living wages.

Unfortunately, for the near future empirical analyses like ours are constrained by the data. As a consequence (and, indeed, even if this were not the case), other types of evidence, including perhaps comparative case studies of different types of living wage laws, are likely to provide important complementary evidence.

#### IV. The Microeconomics of Living Wages

The evidence discussed thus far is based in large part on our work using the CPS data. The CPS has some disadvantages and some advantages for studying the effects of living wages. Given that the CPS is a household-based survey with virtually no information on employers, it is not possible to identify covered and uncovered workers. Instead, all we can identify (and therefore the only information we exploit in the empirical analysis) is the city in which a worker lives and the type of law prevailing there.<sup>20</sup> Obviously, CPS data are not very useful in exploring the microeconomic effects of living wages at the firm or establishment level (i.e., questions such as whether covered firms engage in substitution away from low-wage labor, how much wages are increased for their lowest-wage workers in their firms, how employment responds, whether turnover falls, etc.). Such questions are best addressed using direct survey data on covered employers and ideally a control group of uncovered or unaffected employers, as has begun to occur in some recent research reviewed in this section.

On the other hand, the CPS data are very useful, and are the best data available at present, for addressing policy questions regarding the effects of living wages on low-wage workers and low-income families, because they permit us to estimate the net effects, including indirect or perhaps “general equilibrium” effects, of such laws on workers and families in the cities where they are passed, relative to other cities. Ultimately, the two types of data and research are complementary and inform each other, with firm-level data allowing researchers to test propositions about the individual-level behavioral

responses of employers and workers to the imposition of living wages and city-level data allowing researchers to assess the policy consequences.<sup>21</sup> With this point in mind, this section turns to evidence from newer research that uses data on employers or workers known to be covered by living wage laws to study the effects of these laws.

This part of our review is limited in two ways. First, it focuses on studies of labor market effects—i.e., effects on employment, wages, turnover, etc. Although there is a small set of before and after studies of contract costs, we do not cover these here. We think it is very difficult to draw inferences from these studies, as the nature of the services provided under contracts may change;<sup>22</sup> another way to state this is that while the findings may be important to cities, it is difficult to use them to draw inferences about labor market effects.<sup>23</sup> In addition, these studies are uniformly missing a control group (perhaps of similar cities without living wage ordinances), to control for other factors that may have been changing contracting behavior over time.

Second, the reader will note that all of this work on living wages that we review is unpublished. This reflects the recent advent of these laws and interest in them. It also suggests, however, that it would be quite easy for us to be unaware of research that has been done but not yet published or submitted for review. We identified the studies reviewed in this section from conference organizers and journal editors, and from our own contacts with researchers in this area. We can only hope, however, that the review is exhaustive.

#### *San Francisco Airport*

Reich, et al. (2003) study the implementation beginning in 1999 of a Quality Standards Program (QSP) at San Francisco International Airport (SFO), as part of the living wage law passed in San Francisco that extended to workers at SFO. In addition to the higher wage, the QSP established recruitment and training standards (including a minimum of 40 hours of training), and occurred simultaneously with unionization of many of the airport's workers that resulted in part from a card check agreement. The data collected by the authors indicate substantial impacts on pay of low-wage workers, extending beyond those bound by the QSP, as well as increases in the provision of health benefits. Not

surprisingly, given the wage and benefit increases, turnover fell rather dramatically. Also not surprisingly, workers reported working harder and employers reported better performance. These latter types of changes plausibly reflect efficiency wage-type effects, but of course that does not imply that they pay for themselves.

More controversial is the authors' claim that employment did not fall. There is, unfortunately, no control group in this study. Their estimates of employment (which come from two different sources) indicate employment gains of about 1,150 employees from 1998 through 2001—from about 7,350 to 8,500. However, a potentially important confounding factor was the opening of a new international terminal during this period, in the fall of 2000. The airport had originally projected an employment increase of 11,000 associated with the new terminal. This may have been overstated, as the authors suggest, and at any rate would not all have occurred immediately after the terminal opened. Nonetheless, the new terminal would almost surely have generated substantial new employment over this period (and the authors do not offer evidence to the contrary). At the same time, air travel declined during 2001 because of the recession, which biases the employment change in the opposite direction. (The September 11 terrorist attacks, by the way, play no role here, because they occurred after the survey establishing the 2001 employment figure.) Referring to the effects of the new international terminal and the recession, the authors note that the exact magnitudes of these offsetting effects are difficult to pin down, but that because “these two effects work in opposite directions ... our conclusion—that employment did not fall because of the QSP—remains intact” (p. 66). This conclusion does not necessarily follow, however, if the two offsetting effects were of very different magnitudes, and it seems plausible to us that the opening of the new international terminal would have had a far greater effect, given that airport traffic at SFO fell by only around ten percent from 2000 to 2001 (Reich, et al., 2003, Table 6.3).<sup>24</sup> Thus, we do not believe that these data provide convincing evidence that employment did not fall as a result of the QSP.<sup>25</sup>

### *Non-Profits*

Reynolds and Vortkamp (forthcoming) conducted a survey of non-profits covered by Detroit's living wage law. They originally targeted the 96 non-profits that were covered by contractor

requirements. In the first stage, a phone survey was administered to the 64 who responded. In total, 63 percent suggested a minimal or minor impact, while 37 percent suggested a significant or major impact.

The survey questions regarding the impact of the living wage clued the interviewees to base their answers on the implied financial changes. After describing the interviewees' responses to these questions, however, the authors write, "Exactly what "significant" or "major" means, however, is unclear from the survey data. The researchers intended these terms to refer to significant cuts in worker hours, employment, or services provided" (p. 9). An unavoidable question is why the researchers did not ask about these outcomes directly, since the survey is their own. Instead, the authors devote considerable effort to arguing that respondents overstated the consequences of the living wage law.<sup>26</sup> In particular, they describe attempts to conduct in-depth interviews of the 26 non-profits that indicated a significant or major impact, although they were able to complete only 15 interviews (and do not explain why the other 11 participated in the first interview but not the second). Based on the in-depth interviews, they suggest that for five of the non-profits interviewed, "the impact was found to be much less serious than the data provided by the phone survey would have suggested" (p. 13), while ten "faced costs to comply with the living wage that were significant enough to merit special consideration" (p. 14).

We see three potential shortcomings of this study. First, the two-stage interviewing procedure, coupled with the admittedly hazy survey questions, cast doubt on the findings. One statistical interpretation of the two-stage procedure is that the interviewers asked some simple questions, identified outliers, and then asked a lot more questions to see if in fact some of the responses for the outliers appeared overstated. One has to wonder whether if the outliers in the other direction were re-interviewed, there would have been a similar process of regression to the mean (with those initially indicating little or no impact subsequently reporting evidence of some adverse impacts). The authors did not explore this issue, but simply assert that they did not re-interview any of those indicating minimal or minor effects because "Further discussion was unlikely to change their self-evaluation or yield new information relevant to the study's core concerns" (p. 13). Second, the survey(s) were not designed to provide simple,

objective measures of the consequences of the living wage law, such as direct questions on employment, hours, wages, turnover, clients served, etc.

Finally, and most important, the survey did not use a control group in its design, to examine how any such changes might have compared to the experiences at similar non-profits not covered by the living wage law. It is true that the survey used employers' perceptions of the impact of living wage laws, and in that sense one could argue that a control group is not needed. But that makes the strong assumption that employers correctly distinguish between the effects of the living wage law and other contemporaneous influences on non-profits. And since the Reynolds and Vortkamp ultimately discount severely the responses the employers provided, it is clear that we would have learned considerably more from objective indicators elicited from a treatment and a control group of non-profits. The next two studies reviewed improve upon the previous micro-level analyses by comparing employers affected by living wage laws to other employers.

#### *Boston's Living Wage*

Brenner (2003) studies the effects of the living wage in Boston on firms with contracts with the city. In particular, he studies the period from 1998, before the living wage was implemented, through the fall of 2001, when the living wage had risen to \$9.11. The main results are based on completed surveys from 66 firms (fewer for some of the variables), out of universe of the 140 firms with contracts that were surveyed. Brenner does not have non-contractor firms to use as a control group. Instead, he distinguishes between those firms that indicate they raised wages to comply with the living wage law (treating them as the treatment group), and those that did not so indicate (the control group). This classification appears largely valid, as the reported share of low-wage workers declined sharply in the treatment group, while scarcely changing in the control group, over the 1998-2001 period; in other words, the difference-in-differences estimate of the effect of the living wage on the wage distribution points to strong wage compression.

Turning to employment, the data indicate that employment grew in both the treatment and control groups, but grew faster in the control group, so that the difference-in-differences estimate indicates that

employment declined in relative terms in the affected firms. Of course these employment figures refer only to the overall workforce, and not necessarily workers directly employed on city contracts; employment change among those workers was not studied.<sup>27</sup> The relative employment decline implied by the estimates is 9.9 percent (that is, employment grew by 7.3 percent in the treatment group, and 17.2 percent in the control group). Over this same period, the percentage deviation between the living wage and the minimum wage grew from zero to 35 percent. Thus, the implied employment elasticity is  $-0.28$ . On the other hand, Brenner also finds that in terms of full-time equivalent (FTE) employment, there is no significant difference between the changes for the treatment and control groups. Aside from the employment effects, Brenner studies the impact of the living wage on turnover and absenteeism, and finds no evidence that living wages reduced either.

Brenner's main conclusion based on these findings is that "we find no evidence that the Boston living wage law has reduced employment for firms covered by its mandate. To the contrary, we found that average employment expanded following the implementation of the living wage law" (p. 32). This conclusion contradicts the difference-in-differences estimate indicating that the living wage caused a decline in employment. It is true that the decline is only relative, but that is precisely the point of the difference-in-differences estimator, as the non-affected firms provide the control group for changes that were otherwise occurring; the estimate implies that employment in the treatment group would have grown more absent the living wage.<sup>28</sup> On the other hand, the results differ between employment and FTE employment. The difference-in-differences estimates indicate that the living wage law caused a decline in the first, but not the second. So taken literally there are fewer jobs as a result of the living wage, but hours increased for some workers. That is not the same, of course, as concluding that there were no disemployment effects. The apparent shift away from part-time and toward full-time work would be expected if there are fixed costs of employment on which employers economize in response to higher wage costs. So we view Brenner's results as most consistent with employment declines, but also pointing to other potentially beneficial offsets for some workers in the form of increased hours of work.

Finally, Brenner uses his data to obtain a rough estimate of the number of workers whose wages were directly affected by the living wage increase in this period, which is interesting to consider in light of the poverty-reducing effects of living wages that we estimate at the city level. His admittedly crude estimate is that 1,000 workers experienced direct positive wage effects (this excludes wage increases from ripple effects, which Brenner suggests are weak). In Boston in 1999, Census figures indicate that there were 17,892 poor families.<sup>29</sup> The wage increase associated with the living wage is quite sharp. As an upper bound, if we assume that the 1,000 families of these workers were lifted out of poverty, then the poverty rate would have fallen by 5.6 percent, implying an elasticity of the poverty rate with respect to the living wage of  $-0.16$ . Of course, not all living wage workers are in poor families, so this is exaggerated. At the same time, most poor families with full-time workers affected by the living wage should be lifted above the poverty line—given that the living wage targets the poverty threshold—unless they have large families. And ripple effects may lift wages of more workers. Finally, if—as Brenner’s data suggest—the living wage induced a shift to full-time employment, then the earnings gains to some workers would be larger than simply those generated by the wage increase, although others may have been hurt by the employment decline. Regardless of the precise magnitude, it is interesting that this elasticity is of the same order of magnitude of the estimates we obtain from the CPS data. What this indicates is that if living wages target poor families well, it does not take huge numbers of affected workers to generate non-trivial changes in poverty.

#### *Covered and Non-Covered Employers in Los Angeles*

Fairris (2003) describes an analysis of two samples: an original survey of establishments affected by the Los Angeles living wage; and a second sample of establishments collected for a quite different purpose, used as a control group. The samples are unfortunately not directly comparable. The author provides a thorough discussion of the differences, and tries to do a few things to account for variations in the ways some of the questions were asked. But two fundamental differences persist; the interviews for the treatment sample were conducted on site, while the control sample was collected by mail with some phone interaction; and the response rate for the control sample is very low (23 percent, versus 68 percent

for the treatment sample). Another limitation is that the survey design does not permit direct measurements of employment changes, although employers in the treatment sample were asked whether staffing levels declined as a result of the living wage ordinance. Fairris is careful, where possible, to distinguish between effects of the living wage, and prior differences between affected and unaffected firms.

The results indicate that wages of low-wage workers were increased as a result of the living wage, not surprisingly. In addition, the wage results suggest that prior to the living wage law wages were similar at the two types of firms, so the effect appears causal. The results for the effects of the living wage on the provision of health insurance and on turnover are potentially more problematic. Regression estimates indicate no change in health insurance provision, and a decline in turnover associated with the living wage (of unknown magnitude, since this is based on a categorical variable for whether turnover rose, fell, or was unchanged). But the results also indicate sharp differences in the provision of health insurance and in turnover prior to the living wage, suggesting that the treatment and control samples may not be sufficiently comparable.

Finally, while the research design does not permit the author to measure employment directly, he does study covered employers' responses regarding employment changes. He finds that 18 percent of covered establishments reported declines in employment stemming from the living wage law, and estimates an overall employment decline in the contractor firms of 1.6 percent. Given an approximate \$1.70 increase in wages for low-wage/affected workers caused by the living wage, Fairris suggests that the estimates imply "an elasticity of low-wage worker demand of roughly  $-0.06$ " (p. 20). This calculation comes from dividing the 1.6 percent employment decline, which is estimated for all workers, by the approximate 27 percent increase in wages for low-wage workers. This calculation, however, compares apples and oranges. The correct ratio to use is the ratio of the percentage employment decline for low-wage, affected workers to their percentage wage increase. Assuming that the employment decline is concentrated among these workers, the elasticity would be larger in absolute value. For example, if low-wage workers were one-quarter of the workforce, the elasticity would be  $-0.24$ .<sup>30</sup>

Although the data in this study are far from ideal, the study is on the right track in trying to use a treatment and control sample to study the microeconomic effects of living wage laws. In our view, the results are quite “neoclassical.” When wages are forced up by living wages, there is (if we take the estimates at face value) a decline in turnover, and also a decline in employment. We have no way of knowing whether the decline in turnover “pays for itself.”

### *Home Care Workers*

Howes (2003) studies the impact of a large increase in wages and provision of health insurance and other benefits to home care workers in San Francisco, resulting from the city’s living wage ordinance, as well as unionization. In particular, she focuses on the In-Home Supportive Services (IHSS) workforce. This industry has some unusual characteristics that may impact some of the results Howes reports. Home care is provided to frail elderly and disabled working-age people in the United States. As the name suggests, it is provided in the home, often as a first stage in the transition to long-term care. The IHSS program is supported by Medicaid, and, according to Howes’ estimates, provides care to about 265,000 individuals in California. Consumers are eligible for an IHSS worker based on having income at or below the poverty line. In her data set, 56 percent of providers were family members; but if a spouse or parent (in the case of a minor) is providing the care, then the cost is entirely financed by the state. Over the period November 1997 to February 2002, wages went from near the state minimum of \$5.00 to \$10.00, and the proportion receiving health, dental, and other benefits went from near zero to include most workers, even those who work part-time.

Howes reports that—based on individual-level data on consumers and workers—turnover (the rate at which consumer-worker matches ended) fell sharply, in response to the increased wage and benefits. The author argues that this reduction in turnover is good for consumers, which seems likely.<sup>31</sup> Howes tries to draw implications from this “experiment” for other providers of home care who face issues of high turnover, labor shortages, etc. However, an unusual feature of the IHSS workforce is that the consumers do not pay the higher wage. Thus, even though turnover fell, and this likely made consumers

better off, there is no basis for concluding that the reduction in turnover paid for itself, or that other providers would find it worthwhile to raise wages and benefits.<sup>32</sup>

### *Summary of Firm-Level Effects*

The studies reviewed in this section try to examine the consequences of living wage laws at the firm level. As the difficulties encountered by these studies illustrate, this is not an easy matter. We simply do not have much longitudinal data on firms in the United States, and when this limitation is combined with the small number of firms covered by living wage laws, the task is even more daunting. Thus, while we have offered some criticisms of the specifics of these studies, we genuinely commend the authors of these first-generation studies of the microeconomic effects of living wages.

There is quite a bit of disparity in the results—partly, we believe, as a consequence of the small and unusual samples with which researchers are forced to contend in studying this topic. Nonetheless, we think a couple of generalizations can be drawn. First, living wage laws do affect wages of low-wage workers, delivering wage gains and to some extent compressing the wage structure.

Second, some of the studies provide evidence of declines in turnover, increases in performance, etc.<sup>33</sup> These are the types of effects that we would expect in light of neoclassical or efficiency wage models of the labor market. It is worth emphasizing, though, that efficiency wage-type effects cannot be used to conclude that living wages pay for themselves. They therefore do not support a conclusion that living wage policies are costless or even bring gains, and therefore that there is a case for policy intervention. If that were the case, we would expect profit-maximizing employers to already engage in this type of wage setting; thus, an important question is whether there is evidence that the types of employers affected by living wage laws do not conform to the profit-maximizing model, or alternatively evidence on whether living wage laws increase or decrease profitability.<sup>34</sup> At the same time, the evidence of efficiency wage-type effects does suggest that the costs of living wages may be mitigated relative to calculations based on assumptions that wages do not affect productivity.<sup>35</sup>

Third, some of the evidence points to employment declines resulting from living wage laws, and in cases where the opposite argument is made, we have argued that the evidence is far from compelling.

Thus, in our view these microeconomic studies provide no basis for refuting the predicted unemployment effects of living wages. At the same time, it is worth noting that some of the evidence of unemployment effects (e.g., Brenner, 2003) comes from living wage laws that cover contractors only.<sup>36</sup> In such a case, the micro-level evidence detects unemployment effects whereas the city-level evidence does not (as the latter evidence does not detect unemployment effects for contractor-only laws). This points to the potential power of the micro-level evidence in testing for behavioral responses to living wage laws, even when the city-level analysis does not reveal net effects of the policy.

#### V. Unanswered Policy Questions

To this point, we have focused on what we have learned about the effects of living wages in achieving their main policy goal of helping low-wage workers and low-income families, and on the microeconomics of living wage laws, as well as unanswered questions about these particular analyses. In our view, it would be rash to conclude that at this point the existing research on living wages leads to concrete policy recommendations regarding living wages. However, by finding some evidence that living wages do have a positive effect on wages at the bottom end of the wage distribution and also appear to lead to modest poverty reductions (despite some employment loss), our research suggests that at least some of the beneficial claims of living wage advocates are borne out in the data. At the same time, some of these findings are contested, and others are insufficiently understood, so that further work will be required to establish more decisively whether and how living wages affect low-wage workers and low-income families.

Moreover, while establishing these effects is in itself a tall order, the policy question is broader still. There are a number of additional questions that are important from the perspective of assessing and understanding living wage policies. We believe that caution is warranted with regard to taking a strong policy stance on living wages until many of these questions are also successfully addressed.

First, as emphasized earlier, there is far more that we do not know than that we do know regarding the microeconomics of employer responses to living wages. Our evidence at the city level, as well as other evidence at the employer level, indicates that—as expected—living wages lead to some

employment declines. But we do not know enough about the micro-level responses to living wage laws, for example whether the employment reductions primarily take the form of existing employers downsizing, or employers relocating outside of the city, and presumably shifting their business to avoid being bound by living wage laws.<sup>37</sup> In addition, because the available data cover either individuals or a relatively small number of employers, these data are unable to answer questions of how specific employers react to living wage laws, including questions such as whether wage spillovers occur across or within firms. A better understanding of employer responses to living wage laws would help policymakers better understand the consequences of living wage laws, including how their effects are distributed across different workers, and give policymakers guidance in trying to minimize any adverse effects of living wage laws.

Second, we need to know more about the institutional, day-to-day workings of living wage laws. Luce (2003) begins to address some of these issues in studying the politics of living wage implementation, and Sander and Lokey (1998) provide a case study of enforcement and compliance problems and solutions in the implementation of Los Angeles' living wage law. But we also need systematic empirical work on enforcement, covering topics such as resources devoted to enforcement, compliance behavior, penalties for non-enforcement, etc. Such information would help to address questions regarding the best ways to implement living wage laws. It would also provide additional information on the effects of living wage laws; for example, evidence that these effects are stronger when enforcement is more vigorous and penalties more severe would bolster any conclusions regarding the causal effects of living wage laws.

Third, aside from the wage, employment, and income tradeoffs on which most of the existing research has focused, there are potentially important "second-round" effects of living wages that require study. Now that the experience with living wage laws in many cities has lengthened, it may be feasible to begin to study some of these questions. For example, because living wages are local, employers may be able to avoid coverage by terminating contracts, grants, abatements, etc., with the city. If this occurs, fewer firms would be left to bid on city contracts, which could lead to less competitive bidding and

therefore higher prices for city services. Aside from this, living wage laws may have an additional adverse consequence if some of the affected recipients of business assistance that subsequently withdraw or reduce services are non-profit organizations providing services to needy individuals and families. In addition, to the extent that higher costs imposed by living wages are absorbed by the cities passing these laws, they may result in higher city taxes or reduced services, with potentially negative consequences for taxpayers and property values. It remains for future empirical research to assess whether these second-round effects of living wages occur, the magnitudes of these effects, on whom they fall, and the extent to which they offset other beneficial effects of living wages.

And finally, a large unknown in thinking about living wages is the likely evolution of living wage campaigns. To date the living wage movement overall has been characterized as largely seeking to spread relatively narrow living wage laws to many cities. But in four cities in the more recent past (New Orleans, Santa Monica, Santa Fe, and San Francisco) legislation or referenda have surfaced for what amount to city-wide minimum wages (or, in the case of Santa Monica, a minimum wage restricted to certain parts of the city and employers above a certain size).<sup>38</sup> Whether living wage campaigns are an attempt to “get the foot in the door” prior to pushing broader city-wide minimum wages, or an end in themselves, is a political question that probably cannot be answered decisively.<sup>39</sup> A second unknown stems from a number of states (including Arizona, Colorado, Florida, Louisiana, Missouri, Oregon, South Carolina, Texas, and Utah) passing laws prohibiting localities in one way or another from implementing city-level minimum wages or living wages in excess of state minimum wages.<sup>40</sup> To date, two states’ pre-emption laws have been challenged, with Louisiana’s law upheld and Missouri’s struck down on a technicality.<sup>41</sup> How this potential struggle between state and local authorities plays out in legislatures and courts over the next few years will likely figure significantly in whether we see growth in city-level living wage laws moving in the direction of city-wide minimum wages. But the key point from our perspective of assessing the policy consequences of living wage laws is that, unfortunately, the growing research on the economic effects of living wages may provide little scope for decisive evidence on the consequences of city-wide minimum wages, as they share elements of both minimum wages (their breadth) and living

wages (their restriction to urban areas), both of which engender employment losses, but have different distributional effects. Thus, if city-wide minimum wages begin to spread, they will require independent evaluation.

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Table 1: Information on Living Wage Laws

City	Initial month law was effective	Living wage requirement as of December 2002 (\$/hr.)	Coverage
Ann Arbor, Michigan	April 2001	8.70	B,C
Baltimore, Maryland	July 1995	8.20	C
Bellingham, Washington	December 2002	10.00	C
Boston, Massachusetts	October 1998	10.25	C
Burlington, Vermont	December 2001	9.90	C,M
Chicago, Illinois	August 1998	9.05	C
Cleveland, Ohio	January 2001	9.20	B,C,M
Dayton, Ohio	April 1998	7.00	M
Denver, Colorado	March 2000	8.70	C
Detroit, Michigan	December 1998	9.05	B,C
Duluth, Minnesota	August 1997	6.50	B
Durham, North Carolina	January 1998	8.45	C,M
Gainesville, Florida	October 2001	8.56	M
Hartford, Connecticut	October 1999	9.96	B,C
Jersey City, New Jersey	July 1996	7.50	C
Los Angeles, California	April 1997	8.17	B,C
Madison, Wisconsin	April 1999	9.05	B,C,M
Milwaukee, Wisconsin	December 1995	7.22	C
Minneapolis, Minnesota	April 1997	9.05	B
New Haven, Connecticut	May 1997	10.86	C
Oakland, California	April 1998	8.72	B,C
Portland, Oregon	July 1996	8.00	C
Rochester, New York	January 2001	8.76	B,C,M
San Antonio, Texas	August 1998	9.27	B
San Francisco, California	September 2000	10.00	C
San Jose, California	December 1998	10.36	B,C,M
Toledo, Ohio	June 2000	9.96	B,C
Tucson, Arizona	October 1999	8.57	C

Note: We list the cities large enough to be identified in our CPS data and included in our analysis, along with the first complete month in which the law was effective. Because most cities adjust their wage requirements based upon federal poverty guidelines, inflation rates, or some other means, we list the wage requirement as of December 2002, the last month included in our sample. Some cities impose a higher living wage if health insurance is not provided; the table reports the lower wage floor. We classify the coverage of laws into the following categories: B (firms receiving some form of business assistance from the city, broadly defined), C (city contractors and subcontractors), and M (municipal employees). We exclude cities that passed a law but do not currently enforce it for a variety of reasons (Buffalo, New York; Omaha, Nebraska; Pittsburgh, Pennsylvania; and St. Louis, Missouri). Neumark and Adams (2003 and forthcoming) provide more detail about each city's law.

Table 2: Estimated Effects of Living Wage Laws

Dependent variable:	Log wages, lowest decile of <u>wage distribution</u>	Employment, lowest decile of predicted <u>wage distribution</u>	Probability that family income <u>below poverty</u>
<u>All living wage laws:</u>			
Log living wage, lagged 12 months	0.040	-0.053**	-0.035**
<u>Business assistance living wage laws:</u>			
Log living wage, lagged 12 months	0.067*	-0.076**	-0.024*
<u>Contractor-only living wage laws:</u>			
Log living wage, lagged 12 months	-0.006	-0.027	-0.038
N	46,374	116,466	142,421

Sources: Adams and Neumark (2003), various tables. The data on labor market outcomes and other worker-related characteristics come from the Current Population Survey (CPS) monthly Outgoing Rotation Group files (ORGs), from January 1996 through December 2002, and the CPS Annual Demographic Files (ADFs), from 1996 through 2002. (The ADFs elicit information on the previous calendar year.) Because SMSAs cannot be identified in the ORG files for a period in 1995, the sample begins in 1996. 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 with the ADFs. Residents of all SMSAs, encompassing all large- and medium-sized cities in the United States, are extracted. However, for the analysis only observations on city-month or city-year cells with 25 or more observations are retained. The correspondence between cities and SMSAs is imperfect, but because many suburban residents work in the city this is not necessarily inappropriate; for expositional ease the text often refers to cities. Since January 1996, the design of the CPS has resulted in the large- and medium-sized metropolitan areas in the sample being self-representing (U.S. Bureau of the Census, 1997); in a small number of cases 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. The data for the first two columns cover 1996-2002, and for the last column covers 1995-2001. The control group is other urban workers; the regressions include controls for city, year, month, minimum wages, and other individual-level controls in the wage and employment specifications, and controls for city, year, and minimum wages in the poverty specification. All specifications also allow differential linear time trends for cities passing or not passing living wage laws, or passing different types of laws. The entries in the first row are from a specification with a single living wage variable, and the entries in the second and third rows are from a specification interacting the living wage variable with dummy variables for the type of living wage. The coefficients for the log wage equation are the from log-log specifications, and hence are elasticities. The coefficients from the employment and poverty regressions measure the change in the share employed or poor in response to a one-unit increase in the log living wage (or a 100-percent increase). ‘\*\*\*’ (‘\*’) superscript indicates estimate is statistically significant at five-percent (ten-percent) level. Reported standard errors are robust to nonindependence (and heteroscedasticity) within city cells, following the suggestions in Bertrand, et al. (2002).

Table 3: Coding of Living Wage Laws

	Contractor coverage (1)	Assistance coverage (2)	Assistance coverage includes tenants/leaseholders (3)	Community hiring (4)	Labor peace (5)	Superseded by collective bargaining (6)	Enforcement / implementation (7)	County (8)	Nearby cities (9)
<u>Contractor only</u>									
Baltimore							Medium		
Boston				Yes			Broad		Yes (1999)
Burlington						Yes	NA		
Chicago							Narrow	Yes (1998)	
Denver							Narrow		
Durham							Narrow		
Jersey City							Narrow	Yes (1999)	
Milwaukee							Narrow	Yes (1997)	
New Haven				Yes		Yes	Medium		Yes (earliest 1999)
Portland	Narrow						Narrow	Yes (1996)	
San Francisco	Broad					Yes	Broad		Yes (many, earliest 1998)
Tucson							Broad	Yes (2002)	
<u>Business assistance plus contractor</u>									
Ann Arbor			Yes				NA	Yes (2001)	Yes (many, earliest 1998)
Cleveland			Yes	Yes			Medium		
Detroit			Yes	Yes			Narrow		Yes (many, earliest 1999)
Hartford		Narrow			Yes		Narrow		Yes (earliest 1997)
Los Angeles	Broad					Yes	Broad	Yes (1999)	Yes (earliest 1996)
Madison						Yes	NA	Yes (1999)	
Oakland	Broad		Yes			Yes	Medium		Yes (many, earliest 1998)
Rochester							NA		
San Jose		Narrow			Yes	Yes	Broad	Yes (1995)	Yes (many, earliest 1998)
Toledo			Yes (narrow)				NA		
<u>Business assistance only</u>									
Duluth				Yes		Yes	Narrow		
Minneapolis				Yes	Yes		Medium		Yes (1997)
San Antonio		Narrow		Yes			Medium	Yes (2001)	

Source: Adams and Neumark (2003, Table 1) and Luce (2003).

## Endnotes

- <sup>1</sup> For accurate and up-to-date information on current living wage laws as well as campaigns that have not yet come to fruition, see the web site of the Employment Policies Institute ([www.epionline.org](http://www.epionline.org)). Researchers may also find the web site of the Employment Policies Foundation (<http://www.livingwageresearch.org/factsheets/adopted.asp>) very useful, as this contains copies of the actual living wage ordinances adopted in most cities.
- <sup>2</sup> Some cities also extend living wage coverage to municipal leaseholders, which might be thought of as a version of business assistance. As Reich (2003) points out, in cases where such provisions cover large municipal airports, they can extend to a large number of workers. Leaseholder living wages are not covered explicitly in this review, although we discuss a study by Reich, et al. (2003) on the effects of living wages at San Francisco International Airport.
- <sup>3</sup> See [www.epinet.org](http://www.epinet.org). The same goes for the minimum wage. For example, Senator Edward Kennedy, a perennial sponsor of legislation to increase the minimum wage, has been quoted as saying “The minimum wage was one of the first—and is still one of the best—anti-poverty programs we have” (quoted in Clymer, 1999, p. 449).
- <sup>4</sup> An exception is for highly unusual living wage proposals such as Santa Monica’s (voted down in 2002). See Sander and Williams (forthcoming) and Pollin (2000) for competing impact studies. Reich (2003) also echoes the call to get beyond prospective impact studies, and “to examine the actual impact of the policies” (p. 9); Brenner (2003) similarly describes impact studies as limited by “lack of information on [sic] actual firm behavior following living wage implementation” (p. 2). Some discussion of one of the most influential impact studies (Pollin and Luce, 1998) is provided in Neumark and Adams (2003).
- <sup>5</sup> Bartik (2002) provides an overview of living wages and the many issues that they raise; he includes only a brief review of some of the earliest evidence on the economic effects of living wages.
- <sup>6</sup> Indeed the distributional effects can be more complicated than simply wage gains offsetting employment losses. While the standard theoretical model predicts overall employment declines, employment may also be redistributed among sub-groups.
- <sup>7</sup> This point has clearly been misunderstood in some of the media coverage of the debate over the effects of living wages. For example, an April 8, 2002, article in *Time*, discussing our poverty findings, attributed to living wage advocates the view of David Neumark as “a conservative minimum-wage basher converted by the success of living wages” (Roston, 2002).
- <sup>8</sup> The specifications and results are only summarized here. For the most up-to-date analysis that includes details regarding the econometric analysis, see Adams and Neumark (forthcoming).
- <sup>9</sup> There are other reasons why employment might respond slowly to mandated wage increases, as employers take time to make the adjustments necessary to use less low-skilled labor. But these adjustment lags should not carry over to wage effects. Because wage responses to living wages also occur with a lag, the reasons offered in the text seem more important.
- <sup>10</sup> The wage equation is a log-log specification, so the elasticity is simply the coefficient on the living wage variable—in this case the 12-month lag.
- <sup>11</sup> Most, but not all living wage laws fall into one of these two categories. We restrict the business assistance category to laws that include these provisions, and include all others in the “contractor-only” group. In a couple of cases, the latter includes cities that extend the living wage to public employees as well as employees of contractors. Finally, we included the two cities with living wage laws only for municipal workers in the contractor-only group (see Table 1), although the conclusions are unchanged if these cities are simply omitted.
- <sup>12</sup> Much of the analysis presented in our earlier papers (Neumark and Adams, 2003 and forthcoming) has recently been subjected to a number of criticisms by Brenner, et al. (2002). We provide a full discussion of their criticisms and our responses to them in Adams and Neumark (2003). However, one particular point relates to wage effects and is quite striking, and therefore bears discussion here. As indicated above, we estimate the wage model for those below a given wage cut-off—in particular below the tenth centile of the wage distribution in each city-month cell. Brenner, et al., argue that this truncated regression model leads to potential bias, something we have always acknowledged and therefore pointed out that the regression must be interpreted as informative about the conditional mean. Brenner, et al., then proceed to argue that a quantile regression approach should be used to avoid this bias. They estimate a quantile regression for the tenth centile, including the same skill-related control variables that we include. The difference in results is pronounced, as the quantile regression yields an estimate very close to zero.

However, this quantile regression does not estimate the effect of living wages on the lowest wage workers, so it is perfectly natural that it fails to detect the positive wage effects that we find. Our regression approach looks at the wage distribution *without* conditioning on skill, picks out those below the tenth centile in

each city-month cell, and then estimates the regression, asking whether living wage laws increase the wages of the lowest wage workers—those below the tenth centile—in the cities where they are passed. In contrast, the quantile regression including the skill-related control variables asks whether living wages shift the wages of those at the tenth centile of this conditional wage distribution. But why would we expect living wages to shift the wages of all workers whose wages are low relative to those with similar characteristics, even among workers with characteristics associated with high wages? Living wage laws impose a wage floor on absolute wages, not relative wages within skill groups. The evidence bears this out. When the data are aggregated up to the city-month level and a regression is estimated for the tenth centile (without control variables), the estimate is very close to the one we reported. And when the quantile regression is estimated without including the skill-related controls, the resulting estimate is in line with our estimates. We see, therefore, that as long as the regression model considers the effect of living wages on low wages in an absolute sense—which is the only sensible question, given the nature of living wage laws—the evidence points to positive wage effects.

<sup>13</sup> We note that, for the most part, wage and employment effects above the tenth centile of the wage or predicted wage distribution are weaker, although we sometimes find hints of evidence of positive wage or employment effects between the tenth and 50<sup>th</sup> centiles, consistent with substitution toward higher-skilled labor.

<sup>14</sup> Of course, increased skills and education would likely generate greater social gains by raising productivity.

<sup>15</sup> Another difference is that contractors may be able to pass through a large share, if not all, of their increased costs to cities, whereas this option does not exist for business assistance recipients who operate in the private sector. This can explain the stronger disemployment effects of business assistance living wage laws but not the stronger wage effects.

<sup>16</sup> Table 2 of Adams and Neumark (2003) gives a more complete description of these and other characteristics of living wage laws.

<sup>17</sup> (Private communication with Jen Kern of ACORN, June 2003.) As an example, if a shopping mall is built with financial assistance that makes the developer and builder covered, then a leaseholder provision implies that the employees of the establishments that eventually occupy the mall will also be covered, which should substantially increase the impact of the living wage law.

<sup>18</sup> Briefly, Luce classifies a city as having “broad” enforcement/implementation when there is at least “one full-time person assigned to oversee ordinance administration, who can answer questions about the law and who can be held accountable for problems” and the “staff actively monitors the ordinance by reviewing payroll records and inspecting worksites” (p. 96). In these cases, according to Luce, staff are engaged in trying to improve the ordinance, creating benchmarks, and making information public. In contrast, she classifies cities without full-time staff, in which it is difficult to find someone to answer questions, and where cities do the minimum to fulfill technical requirements, as having “narrow” enforcement/implementation. She also classifies some intermediate cases as “medium.” This information is missing for a few of the smaller cities, in which case they are dropped from the empirical analysis.

<sup>19</sup> The specifications are explained in Adams and Neumark (2003). In all cases, we consider the additional characteristics of living wage laws one at a time, rather than simultaneously.

<sup>20</sup> Another limitation of the CPS is the relatively small numbers of observations that it yields for smaller cities. This suggests the potential value of trying to study administrative data (such as Unemployment Insurance files) that cover all or most workers. However, such files typically have little or no information on demographic characteristics of workers, no information on the non-employed, and no information on family income.

<sup>21</sup> In our view, this point is often ignored by other researchers studying living wages. For example, Reich (2003) writes that in our work, “although attempting to study the impacts of living wage policies, [we] do not have any direct data on workers or employers covered by living wages” (p. 10).

<sup>22</sup> As Bartik (2002) puts it, “Ideally, we would like to divide a city’s total contracting costs by the real value of services provided in all city contracts” (p. 21). Further, he points out, limiting attention to contracts for which services did not change (as in Niedt, et al., 1999) may entail selection bias, since these may be the contracts whose costs were least affected by living wages.

<sup>23</sup> Pollin (forthcoming) provide the most recent discussion of these contract studies, although he omits a study by the Employment Policies Institute that points to contract cost increases (Employment Policies Institute, 1998) and takes issue with other studies.

<sup>24</sup> Interestingly, the study notes that passenger volume in 2001 fell sharply at SFO, but not at San Jose’s airport, even though the economic downturn was worse in San Jose (and San Jose’s living wage does not apply to its airport workers).

- <sup>25</sup> This discussion is based on the authors' 2003 study. A shorter paper based on this study (Reich, et al., forthcoming) provides fewer details regarding the employment analysis, but reaches the same conclusion.
- <sup>26</sup> They even include a footnote in the table giving these responses suggesting that the numbers overstate the actual significance of the living wage.
- <sup>27</sup> The survey also asked some direct questions about whether staffing levels for city contracts changed, and the results apparently gave no indication of declines in staffing levels due to the city's living wage policy. These responses, however, appear to elicit the interviewee's interpretation of what happened, rather than observed behavior. Furthermore, since the data only establish slower employment growth at affected firms, rather than employment declines, we would not necessarily expect absolute reductions in employment on city contracts.
- <sup>28</sup> Interestingly, the only place where Brenner casts doubt on the difference-in-differences methodology is with respect to these disemployment effects, suggesting that "it is not performed in a setting of independent group assignment ... and that any measured differences between the two groups cannot conclusively be attributed to the 'treatment' " (2003, p. 25). Of course if there was a strong concern about this issue, then none of the findings can be viewed as valid causal estimates.
- <sup>29</sup> See Table DP-1, [censtats.census.gov/data/MA/1602507000.pdf](https://censtats.census.gov/data/MA/1602507000.pdf).
- <sup>30</sup> The precise share of workers for whom the wage effect is estimated is not given. The firms in the living wage sample report an average of 39 percent of workers in four low-wage occupations that Fairris lists, but firms are asked about wages in their largest low-wage occupation. Table 2 indicates that the largest occupation is landscape laborers, who make up an average of 25 percent of employment; using this share rationalizes the calculation leading to the -0.24 elasticity. If we instead use the 39 percent figure (the share in the four low-wage occupations combined), we obtain an elasticity of -0.15.
- <sup>31</sup> As each match is treated as an independent observation, Howes cannot determine for certain whether turnover of prior workers fell, or the composition of the workforce changed to lower turnover workers, but changes in individual behavior are perfectly plausible and indeed it would be surprising if they did not occur.
- <sup>32</sup> A second result that Howes only mentions in passing in this paper, but on which she elaborates in an earlier longer version, is that the wage and benefit increases resulted in a 54 percent rise in the number of IHSS workers and a 47 percent rise in the number of IHSS consumers. This sharp employment increase may well result from the unusual nature of this labor market. First, as Howes describes it, a large share of the IHSS workforce consists of family members, neighbors, friends, etc., who provide home care in return for a wage. Some individuals may have provided these services to families, friends, etc., without compensation. But when the potential remuneration (and benefits) for being an IHSS worker rose so sharply, many of these providers may have turned their status into that of an IHSS home care worker. Second, because of the nature of IHSS, this is a labor market in which demand is unconstrained, as consumers do not pay for the service. Instead, a wage is "posted" and as long as demand is unfulfilled, the number of workers employed is determined by the labor supply curve. Thus, not surprisingly, when the posted wage rose, employment also rose. (That is, this is more akin to an outward shift of the labor demand curve—which traces out the labor supply curve—than a shift along the labor demand curve.) As this discussion makes clear, though, this appears to be a highly idiosyncratic labor market that is of interest in its own right, but probably tells us little about living wages generally.
- <sup>33</sup> We view the Howes (2003) paper as consistent with this decline in turnover, although it is not, technically, a study of firms.
- <sup>34</sup> Indeed one of the findings reported by Brenner (2003) is consistent with this. In particular, he reports that reported turnover costs were much higher in the unaffected high-wage firms in his sample than in the affected low-wage firms (p. 19); that, of course, may explain why the low-wage firms pay low wages.
- <sup>35</sup> The small literature on the effects of living wages on contract costs is meant to address the question of whether city services are actually delivered for less cost after living wage laws are implemented. We have already commented on our skepticism about what one can learn from these studies, without better information on quantity and quality of services provided.
- <sup>36</sup> The evidence in Fairris (2003) comes from contractors, but they are in a city (Los Angeles) in which the living wage law is broader. Because our CPS data do not distinguish among employers, we cannot verify that the disemployment effects of broader living wage laws do not stem in part from employment adjustments among contractors.
- <sup>37</sup> Note that simply relocating does not help, if a business maintains the same contracts with the city. But for some firms, at least, there may be locational advantages that would diminish in importance if the firm were shifting business away from the city. One of us has work in progress that tries to examine relocation decisions of employers in response to living wage laws. Reich (2003) argues that rather than leading to relocation problems,

living wages may serve as local development policies by redistributing income toward households that save little and spend locally.

<sup>38</sup> Earlier city minimum wage proposals were defeated in Houston and Denver. Washington, D.C. has set its minimum at one dollar above the federal minimum since October 1993.

<sup>39</sup> Some living wage advocates are quite open about the goal of a national living wage policy (e.g., Pollin and Luce, 1998, Chapter 6). An argument for more limited policies comes from the perspective that “Public money, our taxpayer money, should not be used to subsidize poverty jobs” (The Western PA Living Wage Campaign, <http://trfn.clpgh.org/alliance/wage.html>, viewed September 23, 2003).

<sup>40</sup> However, these states generally prohibit the broader city minimum wages and not narrower living wages. (This information came from personal communications with Jen Kern of ACORN and Paul Sonn of the Brennan Center for Justice at NYU, September 2003.)

<sup>41</sup> Cities in states with living wage ordinances “derailed” by state laws may provide a better control group with which to estimate the effects of living wage laws (Adams and Neumark, in progress).