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DO LIVING WAGE ORDINANCES REDUCE URBAN POVERTY?

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ABSTRACT

Many cities in the United States have recently passed living wage ordinances. These ordinances typically mandate that businesses under contract with the city or, in some cases, receiving assistance from the city, must pay their workers a wage sufficient to support a family financially. To date, there has been no empirical analysis of the actual effects of living wages on the expected beneficiaries—low-wage workers and their families. In this paper, we estimate the effects of city living wage ordinances on the wages and hours of workers in cities that have adopted such legislation. We also look at the effects of the ordinances on employment and poverty rates in these cities.

Our findings indicate that living wage ordinances boost wages of low-wage workers. The estimated elasticities are small, however, which seems consistent with the fact that living wages have limited coverage, and may also have limited compliance and enforcement. In addition to the wage effects, we find weak negative hours effects of living wage ordinances on low-wage workers, and strong negative employment effects. Finally, our estimates of the effects of living wages on poverty rates indicate that living wage ordinances may help to achieve modest reductions in urban poverty.

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I. Introduction

Since December 1994, many cities in the United States have passed living wage ordinances. These ordinances typically mandate that businesses under contract with the city or, in some cases, receiving assistance from the city, must pay their workers a wage sufficient to support a family financially. Baltimore was the first city to pass such legislation, and at least 26 cities and a number of other counties and school districts have followed suit. Living wages are increasing in popularity, "...at the rate of a new ordinance a month," according to a *New York Times* article published on November 19, 1999. Given this, an empirical investigation of the effects of living wages on low-income workers is in order, to evaluate the claims of beneficial effects made by advocates of these ordinances.

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

Another feature of living wage ordinances is that they are not flexible regarding family size, even though poverty levels vary dramatically depending on the number of children and adults in a household. As an example, consider a city that sets its wage floor to the federal poverty threshold for a family of four with two children, which was \$16,530 in 1998. This implies an hourly wage of \$8.27 for a typical worker that logs 2,000 hours in a year.¹ The poverty level for a single adult, however, is only \$8,480. The ordinances do

¹The wage floor in this example is about average compared to the actual ordinances, which range from a low of \$6.22 an hour to a high of \$10.00 an hour. See Table 1 (discussed below) for detailed information on ordinances that have been adopted thus far.

not allow companies to pay single adults a lower wage. Similarly, the ordinances do not take account of the income of other family members. Thus, for example, if two adults are working for a covered contractor or grantee, both would receive the minimum wage, placing their incomes at well over two times the poverty level.

Coverage by living wage ordinances is far from universal. Some cities only impose wage floors on companies under contract with the city (e.g., Milwaukee and Boston). Others also impose the wage on companies receiving business assistance from the city (e.g., Detroit and Oakland). Still, other cities also impose the requirement on themselves and pay city employees a living wage (e.g., San Jose). This lack of universal coverage limits the applicability of what is known about the effects of traditional minimum wages—which have near-universal coverage—to the effects of living wage laws. Thus, a separate study of living wages is necessary. To date, there has been no analysis of the actual effects of living wages on the expected beneficiaries, low-wage workers and their families. In this paper, we estimate the effects of city living wage ordinances on the wages and hours of workers in cities that have adopted legislation. We also look at the effects of the ordinances on employment rates and poverty rates in these cities. Given the fact that an increasing number of cities have passed living wage laws recently, and, according to the Employment Policies Institute,² campaigns for such legislation are under way in at least seventy other cities, it is critical to analyze the effects of these laws on low-wage workers and poor families. Only then can policy-makers, employer organizations, labor unions, and voters make informed judgments regarding the merits of this policy innovation.

II. Theoretical Predictions

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

²The Employment Policies Institute maintains a comprehensive listing of current and planned living wage initiatives on their web page (www.epionline.org).

Nonetheless, the existing work on standard minimum wages provides a useful “road map” for analyzing the consequences of living wage ordinances.

Living wage ordinances are sure to be binding for some employers with grants or contracts with their city. However, quantitative measurement of the fraction of the workforce of employers with either contracts or grants that is likely to be bound by the minimum wage requirement, and the extent to which their current wages fall short of the required wage, is likely to prove a difficult task.³ Nonetheless, proceeding from the point of view that, qualitatively, some employers will face higher costs for some workers, standard economic analysis makes some predictions regarding the effects of these ordinances.

Whether in the for-profit or non-profit sector, employers are assumed to minimize the costs of production, which depends in perhaps complex ways on the relative prices of the different inputs used to produce the particular good or service in question, as well as the technology for producing this good or service. Even if we know relatively little about the specific constraints and choices facing an employer, theory predicts that a government-mandated increase in the price of one of its inputs—in this case the price of low-skilled labor, which we conceptualize as labor that would be paid a wage below the minimum living wage requirement in the absence of the living wage ordinance—leads to two sets of effects. The first set of effects occurs as employers substitute away from the now-more-expensive input, and toward other inputs. For example, depending on the good or service under consideration, employers may employ fewer low-skilled workers (or, more precisely, use fewer low-skilled hours), and more high-skilled labor. Alternatively, they may mechanize some tasks previously performed by low-skilled labor, substituting toward capital. Even if we cannot predict the inputs towards which employers will substitute, we can nonetheless say that there will be substitution effects away from low-skilled labor.

The second set of effects occurs because this substitution away from low-skilled labor and towards other inputs raises costs of production and results in scale effects. This follows from the assumption that employers were minimizing costs in the first place, which implies that the input choices (conditional on

³For an ambitious effort in the context of a proposed living wage ordinance in San Francisco, see Alunan, et al. (1999).

output) after the imposition of the minimum wage requirement must be higher cost, or they would have been chosen initially. When costs rise, though, the price charged for the good or service will rise. In a private market, this will reduce demand for the product and, hence, use of all its variable inputs. These scale effects differ from substitution effects in that they entail an overall scaling back of the employer's outputs and inputs, whereas substitution effects concern changing the mix of inputs. However, *both* effects reduce employment of low-skilled labor.⁴

However, this is one of many instances where we have to consider some unique features of living wage ordinances; in particular, the city is a purchaser of goods and services from contractors (and possibly also grantees). Thus, it is not necessary that its demand curve for particular services slopes downward, or at least not appreciably over some range, either because the city finds it possible to raise taxes to cover higher costs, or because some services have to be purchased in quantities that may be largely insensitive to price (such as snow plowing). Of course, this is unlikely to hold in practice, because a city government has some limits on its ability to raise taxes. In addition, living wage ordinances do not apply only to the labor done in fulfillment of city contracts. Rather, city contractors or grantees are likely to have to pay higher wages to workers who are producing goods and services sold on the private market as well, where the law of demand surely applies. The bottom line is that despite the fact that we are considering to a large extent purchases of services by the city, scale effects are still likely to exist, although perhaps tempered compared to the private market. Thus, the effects on employers are likely to entail lower output, higher output price, reduced employment of low-skilled labor, and ambiguous changes in the use of other inputs, although increased use of these inputs seems relatively more likely, to the extent that scale effects are moderated.

The responses considered to this point, however, are “first-round” effects. Unlike an increase in the national minimum wage, which covers nearly all employers and leaves them essentially no choice but to pay the higher wage, employers affected by living wage ordinances may in some cases find it more profitable to terminate contracts, grants, abatements, etc., with the city. This is more likely to occur, of course, when the

⁴In contrast, substitution and scale effects may have opposite effects on the use of other inputs such as high-skilled labor.

costs imposed by the minimum wage requirement would be greater. In particular, firms are more likely to take this step the greater their reliance on low-skilled labor, *ceteris paribus*.

These “second-round” responses have a couple of negative implications. First, those firms most likely to “select out” of city business are those employing the highest shares of low-skilled labor—precisely the workers whom these ordinances are intended to help in the first place. Second, as some firms terminate contracting with the city, fewer firms are left to bid on city contracts, which may—if the number of remaining firms becomes sufficiently small—lead to less competitive bidding and therefore higher prices for city services. Thus, although working out the precise effects of these second-round responses is complex, it appears most likely that they reinforce the first-round effects by raising prices of city-purchased services even more, and reducing employment of low-skilled workers among covered employers.⁵

We have already explained why economic theory predicts that employment of low-skilled labor by employers covered by living wage ordinances falls as wages are forced up. This implies that there are winners and losers among the set of low-skilled workers employed by covered employers. Those who remain employed earn a higher wage; it was this higher wage that spurred the employment reductions. However, other low-skilled workers are no longer employed at these firms, or work fewer hours. What is likely to happen to these workers, and how are their economic fortunes likely to change?

It is once again instructive to draw insights from the standard minimum wage literature. In recent years in the U.S., the fraction of employers covered by minimum wage laws is well over 90%, so that coverage is widely regarded as near-universal. In this case, workers disemployed as a result of a minimum wage increase do not find alternative employment, and instead become non-employed.⁶ However, an earlier minimum wage literature considered the situation in a period when minimum wage coverage was less universal, so that there was a sizable uncovered sector in which workers disemployed by minimum wages

⁵On the other hand, the decision of some employers to select out of city contracts and grants may increase private-sector competition and lower prices there.

⁶Whether or not they are *unemployed* depends on whether they continue to seek work, as the definition of unemployment includes both being available for work *and* looking for work. Because the decision to look for work may depend on a variety of factors, analyses of minimum wages—and the analysis of the living wage ordinances in this paper—focus on employment vs. non-employment.

might seek alternative employment (e.g., Mincer, 1976). In this situation, some of the labor disemployed in the covered sector is likely to shift into the uncovered sector; this is an outward shift in the supply of labor to the uncovered sector. By definition, in that sector wages are free to adjust in response to the greater number of workers seeking employment, so the result is twofold. First, wages in the uncovered sector fall for *all* low-skilled workers in that sector.⁷ Second, this results in lower costs of production, leading to lower output prices and higher employment and output. However, employment will not expand enough in the uncovered sector to offset fully the employment decline in the covered sector.⁸

Are there any differences when we consider living wage ordinances? As we see it, there are two. First, as a purely quantitative matter, we suspect that even under broad definitions of coverage by living wage ordinances, a relatively small fraction of workers is likely to be covered. As a consequence, although possible, we regard it as relatively unlikely that there would be substantial lowering of wages of low-skilled workers in the uncovered private sector, although this may occur in a subset of industries or occupations in which workers disemployed from the covered sector happen to be highly concentrated. Second, one substantive difference from the traditional model is that the sector of the economy that is not covered by the living wage ordinance nonetheless is covered by the federal (and state) minimum wage. This introduces a wage floor in the sector that is not covered by the living wage ordinance, which may restrict the ability of wages to fall in this sector. If this wage floor becomes binding, it will restrict the extent to which employment can expand in the uncovered sector, thus exacerbating the overall disemployment effects of the ordinance, while moderating any wage declines.

We have seen to this point that among the low-skilled workers who are the intended beneficiaries of living wage ordinances, there are likely to be winners and losers. The biggest winners are those whose wages are forced up by the minimum wage requirement, and who retain their jobs (and hours) with covered

⁷An exception is when workers leave the uncovered sector to “queue” for covered-sector jobs in sufficient numbers (Mincer, 1976). However, this requires—among other conditions—that work in the uncovered sector deters search in the covered sector.

⁸Formally, this is because when the supply of labor shifts out in the uncovered sector, the decline in wages leads some workers to choose non-employment (or reduced hours).

employers. The biggest losers are those who lose their jobs with covered employers, and end up working at lower wage jobs in the uncovered sector, or perhaps end up non-employed. There are some additional possible winners and losers. First, as low-skilled workers disemployed from the covered sector shift to the uncovered sector, wages there may be bid down somewhat, although we suggested that the magnitudes involved are likely to be small. Second, high-skilled workers could gain or lose, depending on whether low- and high-skilled labor are “substitutes” or “complements” in production. That is, when low-skilled labor shifts to the uncovered sector, this could either decrease (the substitutes case) or increase (the complements case) the productivity of high-skilled labor in that sector.⁹

To summarize, standard economic analysis predicts that the effects of living wage ordinances on workers varies substantially across subgroups of workers. In particular, it yields the following set of predictions. First, wages should rise for some low-skilled workers employed by covered employers. Second, there should be some disemployment or negative hours effects for other low-skilled workers originally employed by covered employers. Third, there may be lower wages for low-skilled workers generally. Finally, predictions for the effects on wages of high-skilled workers are ambiguous.

III. Empirical Evidence from the Minimum Wage Literature

Before turning to new empirical evidence on the effects of such ordinances, we briefly review the available evidence on the effects of standard minimum wages. This review is instructive as to the potential for such ordinances to achieve their policy goal of reducing urban poverty.

The Employment Effects of Minimum Wages

Labor economists have written innumerable papers testing the prediction that minimum wages reduce employment of low-skilled workers. Earlier studies used time-series data to study the effects of changes in the national minimum wage.¹⁰ The consensus view from these “first generation” studies was that the

⁹Combined with this effect, we noted in discussing economic consequences for employers that the net impact of scale and substitution effects for these workers in the covered sector was ambiguous, although an increase in employment and hence wages is probably more likely. Since there is no binding wage floor for high-skilled workers in either sector, we would expect labor to flow across these two sectors so as to equalize wages.

¹⁰Brown, et al. (1983) is a good example of such a study.

elasticity of employment of low-skilled (young) workers with respect to minimum wages was most likely between -0.1 and -0.2 (Brown, et al., 1982).

More recent studies have used panel data covering multiple states over a period of years to study the effects of changes in minimum wages at the state level (e.g., Neumark and Wascher, 1992; Card and Krueger, 1994). Evidence from these “second generation” studies has spurred considerable controversy regarding whether or not minimum wages actually reduce employment of low-skilled workers, with some labor economists arguing that the new evidence shows that the predictions of the standard model are wrong, and that minimum wages do not reduce and may even increase employment (Card and Krueger, 1995). On the other hand, much recent evidence using similar sorts of data confirms the prediction that minimum wages reduce employment of low-skilled workers, and concludes, paralleling the earlier time-series evidence, that the elasticity of employment of low-skilled workers with respect to the minimum wage is in the -0.1 to -0.2 range, with estimates for teenagers—who have often been the focus of minimum wage research—closer to -0.1 (Neumark and Wascher, 1996 and forthcoming). As further evidence, a leading economics journal recently published a survey of economists’ views of the best estimates of various economic parameters. Results of this survey, which was conducted in 1996—after most of the recent research on minimum wages was well-known to economists—indicated that the median “best estimate” of the minimum wage elasticity for teenagers was -0.1 , while the mean estimate was -0.21 (Fuchs, et al., 1998). Thus, although there may be some outlying perspectives, economists’ views of the effects of the minimum wage are centered in the range of the earlier and many of the more-recent studies.

Minimum Wages and Low-Wage Workers

When we turn from such estimates to asking whether minimum wages raise incomes of low-wage workers, it is often assumed that an elasticity as small as -0.1 or -0.2 implies that raising minimum wages entails minor disemployment effects and hence is sound public policy.

However, the argument that “small” minimum wage effects imply that minimum wages raise incomes of low-wage workers is flawed. One problem with using a -0.1 or -0.2 elasticity to make this argument is

that such estimates are taken from studies of the employment effects of minimum wages for entire age groups and are not equivalent to—as some have asserted—the elasticity of demand for minimum wage workers. An estimate of the effect of a minimum wage increase on total employment in any particular age group is really the effect on the low-wage individuals in the group for whom the new minimum wage raises wages, averaged over all workers in this age category; as high-wage workers are for the most part unaffected by changes in the minimum wage, the aggregate elasticity will likely understate the employment effect on the affected workers.¹¹

Another consideration is that there may be wage increases for workers a bit above the minimum wage, whether stemming from relative demand shifts to slightly higher-wage workers, or relative wage constraints faced by employers (Gramlich, 1976; Grossman, 1983). Such effects are potentially quite important in assessing the consequences of minimum wages for low-wage workers (and low-income families), since in the U.S., because of the relatively low level of the minimum wage historically, many workers earning above the minimum would nonetheless be considered low-wage workers, and a sizable proportion of them are in poor and near-poor families. On the other hand, the focus on employment effects ignores hours effects, which could be more widespread than disemployment effects but equally damaging to earnings. Finally, the conventional elasticity uses the legislated minimum wage increase as the denominator, whereas the wage increases received by workers below the new minimum are typically smaller, since many of these workers earn wages above the old minimum initially. Reducing the denominator in the elasticity also increases its absolute magnitude.

Recent research has considered the effects of minimum wages on employment, hours, wages, and ultimately labor income of workers at different points in the wage distribution (Neumark, et al., 1999). This research indicates that workers initially earning near the minimum wage are adversely affected by minimum wage increases, while, not surprisingly, higher-wage workers are little affected. Although wages of low-wage workers increase, their hours and employment decline, and the combined effect of these changes is a

¹¹See Neumark and Wascher (1997) for a thorough discussion of this point.

decline in earned income.

Minimum Wages and Low-Income Families

While there are few poor or low-income families with high-wage workers, there are many high-income families with low-wage workers (such as teenagers). For example, while one-third of workers likely to be affected by the 1990 increase in the federal minimum wage were in poor or near-poor families (defined as those with family incomes up to 1.5 times the poverty line based on their family's size), roughly another one-third were in families with incomes exceeding three times the poverty line (Burkhauser, et al., 1996). If the job loss from a minimum wage increase is concentrated among teenagers in relatively affluent families, while, conversely, the wage gains from the legislated increase are concentrated among single-parent heads of households, then it is considerably more likely that a minimum wage increase would help poor or low-income families. On the other hand, the opposite scenario is also possible, with the job loss concentrated among low-wage workers in low-income families, in which case poor families would be especially hurt by minimum wage increases.¹²

Theoretical economic analysis offers no sharp predictions as to whether minimum wages will benefit poor or low-income families; this is purely an empirical question. Recent research indicates that past experience with minimum wage increases in the U.S. is at odds with the prediction that raising the minimum will help lift families out of poverty. Rather, raising the minimum wage does not reduce the proportion of families living in poverty, and if anything instead increases it, thus *raising* the poverty rate (Neumark, et al., 1998).

In summary, the evidence from standard minimum wages indicates that minimum wage hikes fail to accomplish their principal policy goal of raising incomes of poor or low-income families. This raises a caution flag for those who claim that living wage ordinances are likely to help reduce urban poverty.

¹²This ambiguity in whether minimum wages particularly help poor or low-income families is also apparent in many living wage ordinances. As noted above, the minimum wage requirements set by these laws frequently impose a wage floor pegged to the federal poverty level for a family with a given number of children, *without regard to the income earned by other family members*. Thus, there will no doubt be at least some beneficiaries in families whose incomes are well above the poverty line.

However, as pointed out numerous times already, these results for minimum wages do not necessarily generalize to living wage ordinances, which differ from standard minimum wage increases in potentially important ways. In the next section, therefore, we turn to empirical evidence on the effects of living wage ordinances.

IV. Empirical Evidence on the Effects of Living Wage Ordinances

Existing Research

Because of the recentness with which living wage laws have spread in popularity, little empirical research has been conducted on their effects. Most importantly, no one has attempted an empirical analysis of the actual effects of living wage laws on low-wage workers and their families.

The best-known work on living wages is the book by Pollin and Luce (1998, hereafter PL). Although the primary purpose of their book was to advocate living wages as a viable poverty-fighting tool, it is a useful starting point for research on the subject. First, PL argue that living wage ordinances will deliver a higher standard of living for low-wage families. Second, they posit that such legislation will reduce government subsidy payments to working families. To support these two claims, they perform a calculation based on a typical Los Angeles family of four with a single wage earner that experiences a wage gain equal to the change from the California minimum wage to the Los Angeles living wage. In this calculation, the family's disposable income increases by \$2,500 per year, and as a result of the higher income, the value of the food stamps and Medicaid that they receive falls, and their earned income tax credit becomes smaller.¹³

Additionally, they argue that these benefits will occur with relatively small added labor costs to firms.¹⁴

¹³In addition to these benefits for families, PL also use interviews with three Los Angeles employers that paid employees relatively high wages before the living wage ordinance was passed, to show that paying a high wage to workers may be beneficial to firms. Specifically, the interviews suggest that worker turnover and absenteeism is lower because the firms' workers are happier receiving higher wages than their counterparts at other firms. This evidence is purely anecdotal, however. These firms are arguably making a profit-maximizing decision to pay their workers higher wages, and thus their experience does not necessarily predict what would happen to other firms if external legislation mandated raising wages.

¹⁴To measure the added labor costs per firm, PL use CPS Outgoing Rotation Group files to determine the percentage of workers in an industry that would be affected by a living wage (i.e., those earning below the Los Angeles living wage). They then use employer-level data to estimate the average number of employees per firm in the same industries. Merging this information allows for the calculation of a per firm average number of workers that would be affected by a living wage increase, and, thus, the increase in labor

There are several problems with this work. Foremost among these is that the calculations are hypothetical, and done in the absence of any evidence based on data before and after the passage of living wage ordinances. Most importantly, PL do not attempt to estimate whether there are unemployment effects or hours reductions from living wages; if either results from a living wage increase, then some families may suffer potentially sizable income declines.¹⁵ In addition, their calculations are based on a typical Los Angeles family, but they admit that only 42% of those earning at or below the Los Angeles living wage are the single wage earner in a family. Moreover, the average family size for these workers is 2.1, indicating that on average people are not supporting a family of four on living wages. Thus, their estimates of disposable income gains cannot be read as expected effects of living wages on families of four with a single worker earning a low wage. These same facts imply that their estimate of the reduction in benefits that would be received from the government for workers affected by the living wage is wrong, as these benefits are conditional on the number of dependents and income of other workers in the family. In short, PL's work cannot be viewed as reliable empirical evidence on the effects of living wages on low-income families.¹⁶

Despite the fact that PL's work cannot serve as a basis for evaluating the impact of living wages, its calculations have been used to evaluate ordinances in New Orleans, Miami-Dade County, and Detroit (Reynolds, 1999). Not surprisingly, given the assumptions, these studies reach similar conclusions. For example, for Detroit Reynolds argues that the costs to employers operating under a city contract would increase by only 5% to 9% of the cost of the contract. For those receiving financial assistance as part of the Empowerment Zones program or the Industrial Facilities Tax Exemption, the added costs would be under 1% of the firm's annual budget. While the costs are small, Reynolds asserts that there will be a financial benefit

costs per firm.

¹⁵PL cite only Card and Krueger's work specifically in concluding that living wages have no employment effects, and they also state, "Numerous other studies, examining the detailed changes in specific labor markets throughout the country due to an increase in the minimum wage, have produced results similar to those in Card and Krueger's analysis of New Jersey and Pennsylvania" (p. 41). Given that much recent evidence contradicts this, however, the possibility that workers will lose their jobs or face hours reductions as a result of living wage ordinances cannot be dismissed.

¹⁶In partial acknowledgment of this, PL call for further research on living wages repeatedly in their book.

accrued by about 2,300 Detroit workers who will each see annual income gains for their families of between \$1,300 and \$4,400 annually. Reynolds claims that the benefits outweigh the costs, although the basis for this claim is unclear.

There have also been attempts to predict the loss of jobs that will result from living wages. For instance, two studies use existing estimates from the minimum wage literature and apply them to living wages. Tolley, et al. (1999) report that over 1,300 jobs will be lost in Chicago from the city's living wage ordinance.¹⁷ Recall the point made earlier, though, that empirical estimates from minimum wage studies may not carry over to living wages. The Employment Policies Institute (1999) estimates that if all of California adopted a living wage, there would be over 600,000 lost jobs and \$8.3 billion in lost income. These calculations assume that every firm in California would be subject to a living wage law despite the fact that no such state-level laws exist (or are even in the planning stages, to the best of our knowledge). Also, no current city and county ordinances cover all workers. Finally, their calculation of income lost due to a living wage assumes that workers who are laid off will not find jobs elsewhere; we suggested earlier that employment in the uncovered sector is a likely outcome of realistic living wage laws for at least some of these workers.

In addition to the specific shortcomings of each of the studies mentioned thus far, the unifying problem is that they all fail to attempt to study what has actually happened in localities where living wages have been adopted. Thus, they are not particularly useful in assessing the impact of living wages on low-income families.

Only two studies look at living wage effects after adoption of legislation, focusing on the contracting side. Both focus on the experiences of Baltimore, the first city to adopt a living wage. Weisbrot and Sforza-Roderick (1996), who review costs of and bidding for city contracts via an analysis of 23 matched pairs of pre-living wage and post-living wage contracts in Baltimore, conclude that the real cost of city contracts

¹⁷They also estimate that the cost to the city would be near \$20 million per year, including enforcement costs of \$4.2 million. The latter figure comes from the Office of Management and the Budget, but actual figures reported by Los Angeles and Baltimore suggest enforcement costs well under \$1 million (Reynolds, 1999).

actually declined as a result of living wage ordinances, thus apparently debunking a central argument of living wage opponents.¹⁸ Also, there was a small decline in bids per contract, but this was not statistically significant. The estimated costs of monitoring and enforcement were small as well.¹⁹

Niedt, et al. (1999) conducted a second study of the effects of the Baltimore living wage ordinance and arrive at similar conclusions to those of Weisbrot and Sforza-Roderick (1996) in regard to cost increases for cities and the number of bids per contract. They also suggest that there has been a financial gain for a small amount of workers, but they do not quantify what this gain was or apply any estimation technique to arrive at this conclusion. Based on interviews with workers, they argue that there has been no reduction in employment. Once again, however, no attempt is made to estimate a direct effect of the living wage on low-wage workers or low-income families.²⁰

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

Our Approach

Our ultimate goal in studying the effects of living wage ordinances is to determine whether or not there is evidence that these ordinances help poor or low-income families. However, we are also interested in examining other effects of these ordinances, to assist in understanding how they affect low-wage workers as well as their families.

¹⁸Although they provide no evidence of the impact on the wages of workers or the incomes of families, they do state that employment levels did not change as a result of the living wage ordinance. They base the claim solely on interviews with 31 firms conducted shortly after the passage of the legislation.

¹⁹A critique of this study by the Employment Policies Institute (1998) questions these results. Among the many problems cited, it is claimed that one of the 23 contracts matched by Weisbrot and Sforza-Roderick was just an extension of a pre-existing contract and not subject to the living wage law. Additionally, many contracts considered as post-living wage contracts actually started before the law went into effect. Finally, bid information was overstated. The study claims that correcting all of these errors reverses the findings of the study.

²⁰Moreover, none of the existing studies of living wage ordinances appear in peer-reviewed journals.

To obtain evidence on these issues, data will be used from a set of cities, some of which passed living wage ordinances in the 1990s, and some of which did not. To infer the wage, hours, employment, and income effects of living wage ordinances, changes in these outcomes for workers and families in cities passing these ordinances will be compared with changes in similar periods for workers and families in cities not passing such ordinances. The latter serve as a control group that is essential to draw any reliable causal conclusions. Otherwise, there is a greater likelihood that passage of living wage laws in various cities is spuriously correlated with other changes (such as overall economic activity) that may influence these outcomes. When a control group of workers in cities that did not pass such laws is used in the analysis, only the *relative change* associated with living wage laws is causally attributed to such laws. Aside from the identification of “treatment” and “control” groups of workers, because living wage ordinances are not randomly assigned the statistical analysis will also control for some other sources of city-specific economic changes.

Living Wage Laws

To begin, we used multiple sources including personal communications with municipalities to assemble information on living wage ordinances. Although few laws were passed prior to 1996, most came into effect in 1996 or after. For this reason, and because cities cannot be identified in our central data set for a period in 1995, we restrict much of the analysis to 1996 and after.²¹ Table 1 lists information on living wage laws in all cities, including the wage floors and their effective dates, information on who is covered by these laws, and other details. Not all of these are used in our empirical analysis, as some of the smaller municipalities cannot be identified in our data. These include: Cambridge; Duluth; Hayward; Jersey City; Madison; New Haven; Pasadena; Somerville; West Hollywood; and Ypsilanti. We assume that the populations of these municipalities are sufficiently small so as not to contaminate the control group

²¹Specifically, for part of 1995 SMSA codes are unavailable for the outgoing rotation groups due to phasing in of a new CPS sample based on the 1990 Census.

appreciably.²²

Initially, we simply look at the effects for all workers, individuals, or families within a city. This provides a broad view of the effects of living wage laws, which precludes testing some predictions of the theory, such as differential effects for covered and uncovered workers. On the other hand, it offers two advantages relative to attempts to isolate effects for covered workers. First, it avoids issues of the endogeneity of the decision or outcome regarding work in the covered sector. Second, although not explicitly stated, we presume that the goals of advocates of living wage laws are to help low-income workers or families generally in a particular city, rather than to help subgroups of these at the possible expense of other subgroups. Thus, our more sweeping approach may say less about some of the underlying economics, but more about the success of the policy. Nonetheless, for some specifications we also attempt to identify those workers more likely to be covered by living wage ordinances in cities, and to estimate the different effects of living wages for covered and uncovered workers.

Data

The data used come from the Current Population Survey Outgoing Rotation Group (ORG) files extending from January 1996 through October 1999 and the Current Population Survey Annual Demographic Files (ADFs) from 1996 through 1999. The ORG files provide data on wages, employment, hours, etc., for individuals, and it is possible to combine data on individuals in the same family. The ORG files include approximately 13,000 households per month, or about 570,000 observations overall on individuals. In these files, residents of all “Standard” metropolitan statistical areas (SMSAs), encompassing all large- and medium-sized cities in the U.S., can be identified. We extract data on these residents for our empirical analysis. Ideally, we would like to know where they work rather than where they live, but such information is not available. Also, the correspondence between cities and SMSAs is imperfect, but because suburban

²²Some living wage ordinances specify two different wage floors, one (lower) applicable when health insurance is provided, and another when it is not. We always report results using the wage floor applicable when health insurance is provided. The estimates were very similar when we re-estimated the models using the higher wage floor.

residents may work in the city, this is not necessarily inappropriate.²³ The variables constructed from the ORGs are listed and described in Table 2. Their uses in the empirical analyses are described below. Since January 1996, the design of the CPS has resulted in the large and medium-sized metropolitan areas in the sample being self-representing (Bureau of the Census, 1997).²⁴ This is yet another reason for only using information from this month on.

For several reasons, most of our analysis uses the ORGs, rather than the ADFs. First, as Table 1 shows, there is variation in the months in which living wage ordinances pass. If we primarily used the ADFs, we would restrict ourselves to a single “reading” per year and would lose a lot of variation in living wages across observations. Second, the ADFs would give us fewer observations overall. Whereas the ORGs have information on wages only for one-fourth of the sample, because we get these data in each month the ORGs provide a sample three times as large. Third, the ADFs are released slowly, while the monthly ORG files are released quickly. For example, the March 1999 files were not released until October 1999. In addition, the March files cover the previous year, so not until the March 2000 data are released would we be able to study the living wage ordinances put into place in 1999. Finally, as discussed below, for some of the analyses of arguments proffered by advocates of living wage ordinances, the monthly files provide more appropriate data.

Undeniably, however, the ADFs do have some advantages over the ORGs when analyzing the effects of living wages on poverty. First, the ADFs allow for a more accurate measurement of family income because non-earned family income information is included. Thus, we can more accurately determine the percentage of families in poverty for SMSAs in a given year. Second, with the ADFs we are able to match families and their income information from 1995 to city living wage information. This is because family income information in 1995 is reported in the 1996 ADF, which falls after the sample redesign that rendered SMSAs self-representing. For these reasons, we supplement the estimates of living wages on poverty that

²³For expositional ease, we often refer to cities rather than SMSAs.

²⁴In a small number of cases, though, outlying counties are excluded from the CPS sampling frame for an SMSA, in which case the data are representative of the remainder of the SMSA.

use the ORGs with estimates that use the ADFs.

Wage Effects

We begin the analysis by asking whether there is evidence that living wage laws succeed in boosting wages of low-wage workers. If they do not, of course, then it is unlikely that any positive (or negative) effects will flow from them. This may seem like a trivial question, with the answer certain to be in the affirmative, but indeed there is no research documenting the extent of compliance with these laws.²⁵ In contrast, compliance with standard minimum wage laws has been studied and documented (Ashenfelter and Smith, 1979), as have the effects of minimum wages on the wage distribution.

To study this question, we estimate a wage equation for various ranges of the wage distribution in SMSAs.²⁶ Specifically, we look at workers that fall below the 10th percentile, between the 10th and 25th percentile, between the 25th and 50th percentile, and between the 50th and 75th percentile of their city's wage distribution in a particular month.²⁷ Pooling data across months, we estimate the following regression for each percentile range

$$(1) \quad \ln(w_{ijst}^p) = \alpha + X_{ijst}\beta + \omega \cdot \ln(w_{jst}^{\min}) + \gamma \cdot \max[\ln(w_{jst}^{\text{liv}}) - \ln(w_{jst}^{\min}), 0] + \delta_Y Y_t + \delta_M M_s + \delta_C C_j + \epsilon_{ijst} ,$$

where w^p is the hourly wage for individuals in the specified range (p) of the wage distribution, X is a vector of individual characteristics,²⁸ w^{\min} is the higher of the federal or state minimum wage, and w^{liv} is the living

²⁵One reason compliance may be an issue is a lag between initial passage of an ordinance and the adoption and dissemination of guidelines to contractors and others. For example, Detroit's ordinance passed via a referendum in the fall of 1998, but as far as we have been able to ascertain, the city still has not decided on details regarding its implementation.

²⁶For the estimation of wage effects, as well as hours and employment effects, we restrict our sample to workers with an hourly wage greater than one dollar and less than or equal to 100 dollars. Also, we limit the sample to those between the ages of 16 and 70 years of age, inclusive.

²⁷When cities have very few observations for a given month, determining whether a worker falls in a particular range of the wage distribution is impossible or unreliable. We therefore restrict our sample to only workers in larger cities (i.e., those SMSAs with more than 25 observations in every month). A complete list of cities that we include in our analysis of the ORG files is included in Appendix Table A1.

²⁸These include dummy variables for gender, race, educational attainment, and marital status.

wage.²⁹ The subscripts $i, j, s,$ and t denote individual, city, month, and year. $Y, M,$ and C are vectors of year, month, and city (SMSA) dummy variables. ϵ_{it} is a random error term.³⁰

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

The living wage variable that multiplies δ measures the percentage gap between the living wage and the minimum wage; if there is no living wage (or if the living wage were below the minimum wage, which does not occur in our sample), this variable is set to zero. If living wages boost the wages of low-wage workers, we would expect to find positive estimates of δ when we are looking at workers in relatively low ranges of the wage distribution.³¹ Finally, we also estimate specifications in which we lag w^{\min} and w^{liv} by six or 12 months, to allow for a slower, adaptive response to changes in minimum wages and living wages.³²

²⁹In the few cases of SMSAs that straddle states with different minimum wages (Charlotte, Philadelphia, Portland, and Providence), we use a weighted average of the minimum wages in the two states, weighting by the shares of the SMSA population in each state.

³⁰We also estimated the wage equations using the specified wage percentiles for the city (i.e., 10th, 25th, 50th, and 75th) as dependent variables, rather than the individual-level data on individuals in these ranges. In these estimations we weighted because we have different numbers of observations per SMSA-month cell, and we expect estimates to be less precise in cells with fewer observations. In the standard case where the data are means over individuals in a cell, this weighting entails multiplying the observations by $(N_{jst})^{1/2}$. Because the variance of the sample mean for a cell is proportional to $1/N_{jst}$, under the assumption that the true variance of the dependent variable is constant across cells (i.e., state-month observations), this transformation results in homoscedastic errors. When our dependent variable is a percentile for a cell rather than a mean, a closely related but slightly different assumption is needed. The variance of the p th percentile is

$$\frac{p(1-p)}{N_{jst} f^2(p)}$$

where $f(p)$ is the density evaluated at p (Mood, et al., 1974). Thus, as long as the density is the same across cells, the same weighting is appropriate. Regardless, the results were qualitatively very similar to those reported in the paper using individual-level data.

³¹Among the SMSAs with a living wage effective in a particular month, the living wage was below the 10th percentile in 13.3% of cases, between the 10th and 25th percentile in 65.7% of cases, between the 25th and 50th percentile in 19.2% of cases, and between the 50th and 75th percentile in 1.7% of cases.

³²While independence across cities and months in our sample can be assumed, it is highly unlikely that observations on individuals within a given city-month cell are independent. Because of this, the standard

Table 3 reports estimates of this equation. All coefficient estimates and standard errors are multiplied by 100. Looking first at minimum wages, Table 3 reveals that the estimated wage effects are quite sharp initially for both workers below the 10th percentile and workers between the 10th and 25th percentiles, with elasticities of .14 and .15, respectively. No significant effects show up at higher ranges of the wage distribution. The effects at the lower percentiles dissipate over time. Six months following a minimum wage increase, the estimated elasticities for workers below the 10th percentile and between the 10th and 25th percentiles have fallen to .09 and .06, respectively, with neither estimate statistically significant.³³ After 12 months, the estimated elasticities are nearly zero for all ranges of the wage distribution. This dissipation of the minimum wage effects is consistent with results reported in Neumark, et al. (1999) using a quite different empirical framework, who suggest that this occurs as nominal wages catch up for other workers. This replication of those results for minimum wages helps to establish the validity of our data set. However, the minimum wage effects are not central here, so in the remaining analyses of wage effects, we focus mainly on the impact of living wages.

Table 3 reveals no contemporaneous effects of living wages for the 0th-10th percentile range and the 10th-25th range. Six months after a living wage increase, the estimated effect for the 0th-10th percentile range is positive, but small and not statistically significant. At a lag of one year, however, we find more strongly significant effects for the 0th-10th percentile range, with an elasticity of .08. The lagged effect is not unreasonable. Compliance may well be weaker or slower for living wages than for minimum wages. Moreover, living wage laws are new for most cities in our sample, and implementation of the laws may therefore be a rather drawn-out process, or cities may only enforce compliance as contracts are renewed (as happened, for example, in Baltimore and San Jose). In addition, the rather small elasticity (compared to contemporaneous minimum wage effects) is not surprising, since coverage is much more restricted. Finally,

errors that would be obtained from estimating equation (1)'s parameters by ordinary least squares will be incorrect. We therefore estimate robust standard errors that relax the assumption of independence (and homoscedasticity) within city-month cells. Corrected standard errors are reported in all tables.

³³Curiously, in the six-month lag specification there is a negative estimated effect for workers between the 50th and 75th percentile, almost significant at the 10% level. However, this does not show up for the contemporaneous or 12-month lag specification.

there is a smaller positive impact for workers in the 10th-25th percentile range, significant at the 10% level. As we might expect, though, there is never any evidence of wage effects in the higher percentile ranges. In general, these data detect moderate wage-increasing effects of living wage ordinances, especially about one year after implementation.^{34,35}

As further verification that we are truly detecting effects of living wage ordinances, we next attempt to estimate wage effects for covered and uncovered workers. As discussed earlier, coverage of living wages is not universal. Moreover, theory makes no definitive predictions on what the expected direction of the effects of living wages on workers in the uncovered sector will be. Nonetheless, we would expect positive effects to be stronger for covered workers, and indeed regard no effect as the most likely outcome for uncovered workers.

Using the limited information we have on workers and the scope of city ordinances, we attempted to identify those individuals most likely to work for a company under contract with the city, and therefore covered by their city's living wage legislation. For workers in cities where businesses receiving financial assistance from the city are covered, virtually any non-government worker may work for a company that is subject to the legislation. Therefore, we characterize all private sector workers as being "covered" in these cities. Table 4 details our best attempt at identifying all potential covered workers.

Next, in equation (1) we replace the living wage variable with a pair of interaction terms—a dummy variable indicating that a worker is covered and a dummy variable indicating non-coverage, each multiplied by the living wage variable. These reveal the respective effects of living wages on covered and uncovered workers. We also add a series of dummy variables representing the worker subgroups that are covered by living wages. Since our estimated definition of coverage differs somewhat by city, we had to add separate

³⁴We experimented with lags of different lengths. This relationship is relatively robust, with estimated effects strengthening through about one year as the lag is lengthened.

³⁵As mentioned earlier, some cities' living wage laws cover only workers at companies under contract with the city, while other cities' laws extend coverage to workers at companies receiving business assistance from the city. We estimated separate effects for these two types of living wage laws. Although the lagged effects on low-wage workers of both types of legislation are positive, we found stronger wage effects in cities where those receiving business assistance are subject to the laws. This is sensible given the fact that many more workers are expected to be subject to the legislation in these cities.

dummy variables for each group. These dummy variables pick up wage differences between the types of workers that are covered and the types that are not covered, which are separate from any living wage effect. Estimates obtained should be interpreted with caution, however. Some living wage ordinances are not explicit about what types of workers are covered. For many localities, we had to make strong assumptions concerning the types of industries in which covered individuals work. Table 4 shows that we chose the broadest definitions of potential coverage, so as not to exclude those that are potentially affected. Actual coverage rates should be much lower than those we report. Nonetheless, we believe we have distinguished between workers more and less likely to be covered.

With the potential problems of our approach noted, the top panel of Table 5 reports the results. For workers below the 10th percentile of their city's wage distribution, those that are identified as covered by legislation appear to experience positive wage effects. These are significant at the 5% level when living wages are lagged by 12 months. For those between the 10th and 25th percentiles of the wage distribution, positive living wage effects that are significant at the 10% level are observed when living wages are lagged by 12 months. Wald tests of the equality of coefficients reveal that the differential effects of legislation on covered and uncovered workers are not statistically significant, however. Essentially no differing effects are detected at higher levels of the wage distribution. Overall, despite the imprecision of the estimates, the results are most consistent with those workers more likely to be covered by living wage ordinances receiving the bulk of the wage gains.

Next, we exclude from the sample completely those cities with living wage legislation that applies to all firms receiving business assistance from the city. Given the difficulty of predicting the types of workers that would be directly affected by the legislation in these cities, the coverage rates in our sample are most likely too high. Thus, we redo the above approach, estimating effects of living wage laws on workers in cities where only contractors are subject to the legislation. The bottom panel of Table 5 reports the results. Although no effects are statistically significant at the 10% level, the point estimates are similar to those estimated using the entire sample, with if anything sharper differences between the effects on covered and

uncovered workers. Thus, the evidence is broadly consistent with low-wage covered workers receiving most of the wage gains.

Effects on Poverty-Level Earnings

As suggested in the introduction, living wage laws are designed to enable a person to earn enough to lift his or her family out of poverty. Our next analysis describes evidence on whether living wage ordinances are likely to achieve this goal. We consider two types of hypothetical families: families with one parent and two children; and families with two parents and two children. We then ask whether an individual's earnings (assuming a full year of work) are below the poverty line for each of these two types of families, defining variables P^1 and P^2 that are, respectively, dummy variables denoting whether a worker's earnings would place him or her below the poverty line for one- and two-parent, two-child families.³⁶ We estimate regressions of the form

$$(2) \quad P_{ijst}^k = \alpha + X_{ijst}\beta + \omega \cdot \ln(w_{jst}^{\min}) + \gamma \cdot \max[\ln(w_{jst}^{\text{liv}}) - \ln(w_{jst}^{\min}), 0] + \delta_Y Y_t + \delta_M M_s + \delta_C C_j + \epsilon_{ijst}, \quad k=1,2.$$

In this regression, estimates of ω that are less than zero indicate that living wages increase the probability that an individual's earnings would be sufficient to lift his or her family out of poverty, and similarly for γ and minimum wages. What this regression is telling us, essentially, is whether changes in wages induced by living wages push earnings over particular thresholds relevant to the policy debate. We do not contend that this is the most meaningful analysis of living wages, but it does parallel the calculations used by advocates of living wages. Here we ignore employment effects, as the calculations are done only for those with a wage, and ignore hours effects, except for hours variation across hourly workers.

Estimates of equation (2) are reported in Table 6. We find no significant effects of living wages in providing income sufficient to lift families out of poverty, for either one- or two-parent families. In contrast, we do find evidence from the specifications with contemporaneous effects or six-month lags that minimum

³⁶Following Census Bureau recommendations, we used the CPI to inflate 1998 poverty lines for 1999.

wage increases could accomplish this goal, with the effects significant at the 5% level. However, once we look a year after the minimum wage increase, the estimated effects are no longer significant, consistent with the dissipation of wage effects reported in Table 3.³⁷

Of course, these estimates relate to “hypothetical” families. We could find different answers once we look at actual families, and at earnings of all family members. In addition, these estimates ignore employment effects, because they apply to workers only. Finally, these estimates do not allow us to discern the effects of living wage ordinances on hours. We therefore next turn to evidence on hours and employment effects, before concluding with evidence on whether living wage ordinances increase the proportion of families whose actual earnings or income exceed the poverty line for that specific family.

Hours and Employment Effects

The specifications we use to study effects of living wage ordinances on hours and employment parallel closely the wage specifications in Table 3. In particular, we define the same ranges of percentiles of the wage distribution, and estimate hours and employment equations for individuals in these ranges. The only complication, however, is that we want to include non-workers in these calculations, but need to impute a wage percentile for them. We do this in a simple fashion with well-known limitations, imputing wages for everyone in the sample, and using percentiles of the imputed wage distribution for everyone.³⁸

Results are reported for hours in Table 7 and for employment in Table 8. Turning first to the minimum wage, the shorter-term results are consistent with initial hours and employment reductions for the lowest-wage workers (although not significant), and increases in hours and employment for those between the 10th and 25th percentiles, consistent with substitution towards slightly higher-wage workers; this parallels findings reported by Neumark and Wascher (1996). With respect to living wage ordinances, in Table 7 we find essentially no evidence of contemporaneous effects of living wages on hours for those at or

³⁷The results were nearly identical using as dependent variables the percentage of families in the city-month cell below the poverty line. Also, when we allowed differential effects of living wages for covered and uncovered "families," all the estimated impacts were similarly statistically insignificant.

³⁸If we used actual wages for workers and imputed wages for non-workers, we would rarely have non-workers in the extreme percentiles of the wage distribution.

below the 10th percentile of the wage distribution. But the point estimates at lags of six months and 12 months are negative, with the latter significant at the 10% level. The employment results in Table 8 are a bit stronger, with strongly significant negative effects at a 12-month lag for the lowest-wage workers; the estimates imply that a 10% increase in the living wage lowers the employment rate by .9 percentage point. There are no significant effects of living wages at the higher percentiles. The evidence of possible employment and hours reductions among low-skill workers stemming from living wages will likely work against the positive effects on wages reported earlier, when we look at effects on poverty.

Effects on Poverty

Finally, we turn to evidence on the effects of living wage laws on the proportion of families in poverty using both the ORGs and the ADFs. For the analysis using the ORGs, we match up all individuals in the same family in the CPS files, compute monthly family earnings, multiply by 12 to obtain an estimate of annual earnings, and ask whether this is above or below the poverty line for that family. Note that the resulting definition of poverty does not correspond to the “official” definition, because we use data on earnings only, and not unearned income, transfers, etc.³⁹ Thus, the results speak to the ability of families to earn their way out of poverty, which in some ways may be a more interesting policy question. We then compute the proportion of families whose earnings are below the poverty line in each SMSA-month cell, and estimate the following equation

$$(3) \quad P_{jst} = \alpha + \beta \cdot \ln(w_{jst}^{\min}) + \gamma \cdot \max[\ln(w_{jst}^{\text{liv}}) - \ln(w_{jst}^{\min}), 0] + \delta_Y Y_t + \delta_M M_s + \delta_C C_j + \epsilon_{jst} .$$

Note that the level of observation is now at the SMSA-month level, as the dependent variable P_{jst} is the poverty rate in city j , month s , and year t . Thus, we estimate the equation by least squares, weighted by the

³⁹We exclude families with members aged 65 years or older. This is because those who are at least 65 are more likely to receive income separate from earnings (e.g., Social Security). Thus, determining whether their family's income falls below the poverty threshold is problematic. When we leave these families in the sample, however, there is no substantial change in the estimated effects of living wages on poverty.

square root of the cell size to ensure homoscedastic errors (see footnote 30).

The estimates of this equation are reported in Table 9, and can be summarized briefly and succinctly. The analysis using the ORGs provides no evidence that living wage ordinances reduce poverty. The estimates for the contemporaneous and six-month lag specification are near zero and insignificant. The estimate for the 12-month lag specification is where we would expect to find effects, if there are any, since it was in this specification that we found positive wage effects, but also negative hours and especially employment effects. Of course, these effects are offsetting with respect to family earnings. The estimated net impact is actually positive, consistent with a slight increase in poverty, but the estimate is not statistically significant.

Note that the failure to find significant effects of living wages in the ORGs, whether positive or negative, is not because the data are uninformative. First, we are able to detect statistically significant impacts of living wages on wages and employment. Second, the standard errors on the estimated effects of living wages in Table 9 are not large. For example, in the 12-month lag specification, the standard error implies that if a 25% increase in the living wage produced a change in the poverty rate of .73 percentage point, or .0073, the effect would be statistically significant. Thus, we read the evidence as indicating no sizable effect of living wage laws on the proportion of families with below poverty-level earnings.

Next, we turn our attention to estimates of living wages on the percentage of families in poverty using the ADFs. In order to provide a basis for comparison between the ORG results and the ADF results, we first reestimate the poverty effects reported in Table 9 excluding the 1999 ORG files, since the latest available ADF is for 1999, which contains information on 1998 income. Estimates from the 1996-1998 ORGs appear in column (1) of Table 10. The estimated effects of living wages on the percentage of families in poverty are negative, but they are not statistically significant, still suggesting little effect of the legislation on poverty, although more consistent with beneficial effects than the corresponding estimates in Table 9 using 1999 data.

We next estimate poverty effects using the 1997-1999 ADFs, which contain information on family

earnings and incomes from 1996-1998. We do this for the same SMSAs included in our analysis of the ORGs. Together, this analysis is the closest correspondence we can achieve with our ORG data. As with the ORGs, we first construct an annual family earnings measure by summing individual annual earnings of the members of the family. Next, we determine the percentage of families in an SMSA whose annual earnings fall short of the federal poverty threshold. Column (2) presents the estimates of the effects of minimum wages and living wages on poverty.⁴⁰ The effects of the January living wage—corresponding to the 12-month lag—on the percentage of families in poverty in the SMSA is negative and stronger, but still not significant. In order to obtain estimates using more information, we next expand the SMSAs used in the analysis beyond those used in the ORG analysis. In particular, because the ADF is an annual file, there is a much larger set of SMSAs for which we have at least 25 observations. Column (3) reports estimates for this larger data set, which are not substantially different from the column (2) results; indeed the standard errors are no smaller, in part perhaps because experiences are more diverse across some of the smaller SMSAs that are picked up in column (3), and in part because these additional SMSAs receive little weight. Finally, in column (4) we use the largest sample, adding data from 1995 (for which identifying SMSAs in the ADF is not problematic, unlike the ORGs). Again the results are similar. Overall, while these estimates using family earnings from the ADFs do not indicate statistically significant beneficial effects of living wages in the 12-month lag specification on which we focus, they do indicate stronger effects, in this direction, than those obtained using the ORGs.

Finally, in the remainder of the table we turn to the most useful purpose of the ADFs—analyzing total family income. If fighting poverty is the goal of living wage estimates, these estimates are perhaps more appropriate than the estimates obtained using just total family earnings. Not only do they take into account

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

both the gains in family earnings that result from living wages as wages of family members increase and the declines in family earnings that result when employment or hours are reduced by the legislation, but they also take into account differences in transfer income or other income received as a result of the changing wages, hours, or employment status of family members. Paralleling the earlier columns, column (5) begins with an analysis of living wage legislation and family income from 1996 through 1998, for the SMSAs we studied using the ORGs. In this case, in the 12-month lag specification for which we detected wage, hours, and employment effects in the ORGs, the results indicate that the effect of the living wage on the percentage of families in poverty in an SMSA is negative and nearly significant at the 10% level; the coefficient estimate of -3.50 implies that a 25% increase in the living wage reduces the poverty rate by .88 percentage point, or .0088. Column (6) reports results from expanding to the larger set of SMSAs, and column (7) from further expanding the data set to include 1995. The estimated negative effect of living wage ordinances on poverty rates is robust; more importantly, it is slightly larger in these latter columns, and is statistically significant at the 10% level.⁴¹

These results obtained using the total family income information from the ADFs stand in contrast to the results for minimum wages; in Table 10, the signs of the estimates are consistent with minimum wages increasing poverty one year after they rise; this result holds more strongly in research using a much larger span of years (Neumark, et al., 1998). There is some evidence, however, that living wages are at least modestly successful at reducing urban poverty in the cities that have adopted such legislation. The contrast with minimum wage results may be explained by less elastic response of labor demand by cities than by general employers, or by other differences between living wages and minimum wages discussed in Section II. The stronger results for family income than for family earnings are consistent with the following explanation. Living wage laws are successful at boosting the wages of some workers in low-income families. For those who remain employed without hours reductions, any reductions in transfer income that result from higher earnings may be sufficiently small that total income rises by more than earnings. For those whose hours are

⁴¹This evidence is slightly stronger if we do not exclude families with members aged 65 or over.

reduced or whose employment is ended, transfer income may soften the blow to earnings, permitting some families to avoid falling into poverty. Thus, while theory makes no predictions regarding the effects of living wages or minimum wages on poverty, it appears that—in contrast to minimum wages—living wages may help to achieve the goal of reducing urban poverty.

V. Conclusions

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

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

Our findings suggest that there are no contemporaneous effects of living wage ordinances on the wages of low-wage workers. There are, however, lagged effects on wages that are positive and significant. The estimated elasticities are relatively small, which seems consistent with the fact that living wages have limited coverage, and may also have limited compliance and enforcement. In addition to the wage effects, we estimate that the contemporaneous effects on hours worked are not significantly different from zero, and

the lagged effects are negative but only marginally significant for low-wage workers. We estimate lagged effects on the employment rates of low-wage workers, however, that are negative and strongly significant. Finally, our estimates of the effects of living wages on city poverty rates indicate that living wage ordinances may result in modest reductions in urban poverty.

Living wages have only been in existence for a short time, however, and as yet in a small number of cities. While the wage gains, employment declines, and poverty rate declines that we estimate as effects of living wage ordinances are striking, more work will need to be done to evaluate whether these hold in a larger sample of cities that have adopted such legislation over a longer period of time.

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Table 1 - Summary of living wage information

City	Date enacted (wage requirement)	Coverage
Baltimore	Passed in December 1994 but wage requirements were as follows: July 1995 (6.10) July 1996 (6.60) July 1997 (7.10) July 1998 (7.70) July 1999 (7.90)	Construction and service contracts > \$5000
Boston	September 1998 (8.23)	Contractors > \$100,000; subcontractors > \$25,000 (> 25 employees)
Buffalo	Passed in July 1999 but does not appear to be in effect yet (6.22 starting in 2000); therefore not coded as a living wage in our data	Contractors and subcontractors > \$50,000 (> 10 employees)
Cambridge	September 1998 (8.23)	City employees, contractors and subcontractors > \$10,000; recipients of business assistance > \$10,000
Chicago	July 1998 (7.60)	Contractors and subcontractors > 25 employees
Dayton	April 1998 (7.00)	City employees
Detroit	November 1998 (100% of poverty line with health benefits; 125% without)	Contractors, subcontractors, and financial assistance recipients > \$50,000
Duluth	July 1997 (6.50 with health benefits; 7.25 without)	Recipients of grants, low interest loans, or direct aid > \$25,000; 10% of employees exempted
Durham	January 1998 (7.55)	Contractors and city employees
Hayward (CA)	April 1999 (8.00 with health benefits; 9.25 without; adjusted annually on April 1 for cost of living in Bay Area)	City employees; contractors and subcontractors > \$25,000 – maintenance, custodial, landscaping, laundry, temporary, pest, security, and social service
Jersey City	June 1996 (7.50 with health benefits)	Contractors – clerical, food service, janitorial, and unarmed security
Los Angeles	April 1997 (7.39 with health benefits; 8.64 without benefits; indexed annually for inflation)	Service contractors > \$25,000; assistance > \$100,000 or \$1 million lump sum
Madison	March 1999 (100% of poverty level for family of four in 1999; 105% in 2000; 110% in 2001)	Service contractors and subcontractors > \$5,000; assistance > \$100,000; non-union city employees
Milwaukee	November 1995 (set to poverty level for family of three on March 1 of each year; assumes 2,080 annual hours)	Contractors and subcontractors > \$5,000
Minneapolis	March 1997 (100 % of poverty level for family of four with health benefits; 110% without benefits)	Assistance > \$25,000, as of December 1998; > \$100,000 initially
New Haven	April 1997 (7.96; revised every 5 years)	Service contractors

Table 1 (continued)

City	Date enacted (wage requirement)	Coverage
Oakland	April 1998 (initially set to 8.00 with health benefits and 9.25 without; upwardly adjusted by prior December 31 to December 31 change in the Bay Area CPI)	Contractors > \$25,000; assistance > \$100,000
Pasadena	September 1998 (7.25 with health benefits; 8.50 without)	Contractors > \$25,000; city employees
Portland	July 1996 (7.00) July 1998 (7.50) July 1999 (8.00)	Custodial, security, and parking attendant contracts
San Antonio	July 1998 (9.27 to 70% of service employees in new jobs; 10.13 to 70% of durable workers)	Businesses receiving tax breaks
San Jose	November 1998 (9.50 with health benefits; 10.75 without; reset each February to the new poverty level for a family of three and adjusted upward for higher San Jose cost of living – currently a 45.2% premium)	Service contractors > \$20,000; assistance > \$100,000 (excludes trainees and workers under 18); city employees.
Somerville (MA)	May 1999 (8.35 with health benefits)	City employees, contractors and subcontractors
St. Paul	September 1998 (100% of poverty level for family of four with health benefits; 110% without benefits)	Recipients of assistance > \$100,000
Tucson	September 1999 (8.00 with health benefits; 9.00 without health benefits)	Contractors; recipients of economic development assistance > \$100,000 annually
West Hollywood	October 1997 (initially, 7.25 with health benefits; 8.50 without; adjusted annually as the City Employees Retirement System benefits are adjusted)	Service contractors > \$25,000 and entering into a contract of at least 3 months
Ypsilanti (MI)	May 1999 (8.50 with health benefits; 10.00 without)	Contractors > \$5,000 (> 10 employees); non-profits receiving > \$10,000 in assistance

Note: Some cities are listed in some sources as having living wage ordinances, but instead have prevailing wage laws (e.g., New York, Gary, and Memphis). Other cities, like Des Moines, have an average wage goal policy, rather than a living wage. In addition to the cities in the table, the Employment Policies Institute lists at least nine counties and one school district that have adopted similar living wage ordinances. At least 70 other cities (as well as a number of counties and school districts) have campaigns underway to enact similar living wage provisions. Among these are Cincinnati, Cleveland, Dallas, Denver, Houston, Philadelphia, San Francisco, Seattle, and Washington. Much of the information for this table was obtained through correspondences with city governments. Some data, however, was obtained through information made publicly available by the Employment Policies Institute (www.epionline.org) and the Association of Community Organizations for Reform Now (www.acorn.org). The consistency of information provided by these two organizations and the city governments gives us confidence in the accuracy and completeness of the above table.

Table 2 - Variables used in the analysis of the effects of living wages

Variable	Definition/construction
Hourly wage	earnings per hour for hourly workers; usual weekly earnings/usual hours at main job per week for everyone else
Poverty threshold	the yearly income determined by the U.S. Census Bureau below which a family with a set number of adults and children are in poverty
Hours worked	the usual hours that one works at his main job per week
Employment	dummy variable set equal to one if individual currently has a job; set to zero otherwise
Annual earned income for individuals	hourly wage \times usual hours at main job \times 52
Annual earned income for a family	sum of the annual earned income of all family members in the CPS
Minimum wage	the minimum wage effective on the first of the month in the state in which the SMSA is located (weighted average of minimums if SMSA straddles states)
Living wage	the living wage effective in an SMSA
Year dummy variables	separate dummy variables for each year from 1996 to 1999
Month dummy variables	separate dummy variables for each calendar month (11)
SMSA dummy variables	separate dummy variables for each SMSA

Table 3 – Contemporaneous and lagged effects on log wages of workers in various percentile ranges of the wage distributions of SMSAs

Percentile range of SMSA's wage distribution	Below 10th	10th – 25th	25th – 50th	50th – 75th
Specification 1:				
Minimum wage (ω)	14.00 (5.81)	14.89 (4.64)	2.34 (4.79)	-1.55 (4.96)
Living wage (γ)	-1.55 (2.24)	-1.69 (1.73)	-2.42 (1.60)	-1.57 (1.73)
Specification 2:				
Minimum wage 6 months ago	8.80 (6.01)	5.83 (4.94)	-5.71 (5.18)	-8.20 (5.07)
Living wage 6 months ago	1.70 (2.58)	0.43 (1.89)	-0.79 (1.71)	0.10 (1.88)
Specification 3:				
Minimum wage 12 months ago	3.12 (6.79)	1.36 (5.47)	-1.59 (5.39)	-1.10 (5.38)
Living wage 12 months ago	8.40 (2.72)	3.78 (2.22)	-1.57 (2.12)	-1.35 (2.39)
Sample size	32,344	41,620	69,173	69,900

Note: Reported are the estimated effects of minimum wages and living wages effective in an SMSA on the log wages of workers in various percentile ranges of SMSAs. Robust standard errors are reported in parentheses. All estimates are multiplied by 100. The sample includes information on workers in 63 SMSAs across 46 months (January 1996 - October 1999). SMSAs are included if there are more than 25 observations for the SMSA in each month of the sample. Thus, the sample excludes individuals in small SMSAs and those not living in SMSAs.

Table 4 – Summary of the construction of “covered” worker variable

	Coverage specified in legislation	Private industries classified as covered in our sample	Public sector workers classified as covered	Prop. covered
<u>Cities where only contractors are subject to living wage law:</u>				
Baltimore	Construction and service contracts > \$5000	Construction, transportation (excluding U.S. Postal workers), communications, utilities and sanitary services, custodial, protective service, parking, certain professional and social services	...	0.14
Boston	Contractors > \$100,000; subcontractors > \$25,000	Same as Baltimore	...	0.17
Chicago	Contractors and subcontractors	Same as Baltimore	...	0.14
Durham	Contractors and city employees	Same as Baltimore	City employees	0.22
Milwaukee	Contractors and subcontractors > \$5,000	Same as Baltimore	...	0.13
Portland	Custodial, security, and parking attendant contracts	Custodial, protective service, parking	...	0.01
<u>Cities where those receiving business assistance are also subject to living wage law:</u>				
Detroit	Contractors, subcontractors, and financial assistance recipients > \$50,000	All	...	0.89
Los Angeles	Service contractors > \$25,000; assistance > \$100,000 or \$1 million lump sum	All	...	0.87
Minneapolis	Assistance > \$25,000, as of December 1998; > \$100,000 initially	All	...	0.86
Oakland	Contractors > \$25,000; assistance > \$100,000	All	...	0.82
San Antonio	Businesses receiving tax breaks	All	...	0.80
San Jose	Service contractors > \$20,000; assistance > \$100,000 (excludes trainees and workers under 18); city employees	All (excluding workers under 18)	City employees	0.95

Note: The “Coverage specified in legislation” column repeats information from Table 1. Three-digit industry codes in the CPS were used to identify non-public sector workers that were most likely subject to living wage legislation. “Certain professional and social services” include health services, libraries, educational services, job training, child care, family care, residential care, miscellaneous social services, museums, architectural and surveying, accounting and auditing, research and testing, management and public relations, and miscellaneous professional and related services. City workers were those that are classified as local government workers in the CPS. Only information for those cities that are large enough to make our sample cut for the wage analyses are included in this table.

Table 5 – Contemporaneous and lagged living wage effects on log wages of covered sector and uncovered sector workers in various percentile ranges of the wage distributions of SMSAs

Percentile range of SMSA's wage distribution	Below 10th	10th – 25th	25th – 50th	50th – 75th
<u>Entire Sample:</u>				
Contemporaneous effects:				
Uncovered workers	-2.53 (2.94)	-0.73 (1.97)	-1.55 (1.76)	-1.67 (1.91)
Covered workers	-0.72 (2.60)	-2.24 (1.93)	-2.96 (1.78)	-1.51 (1.83)
6 month lagged effects:				
Uncovered workers	-0.52 (3.58)	0.17 (2.18)	-0.71 (1.87)	-0.62 (2.08)
Covered workers	3.10 (2.95)	0.55 (2.16)	-0.74 (1.95)	0.67 (2.04)
12 month lagged effects:				
Uncovered workers	3.71 (4.20)	3.16 (2.80)	0.04 (2.23)	-1.02 (2.32)
Covered workers	10.69 (2.96)	4.17 (2.43)	-2.54 (2.51)	-1.62 (2.83)
<u>Sample excluding cities receiving business assistance:</u>				
Contemporaneous effects:				
Uncovered workers	-0.31 (3.51)	-0.19 (2.49)	-1.35 (2.31)	-1.73 (2.61)
Covered workers	3.40 (6.13)	-1.26 (3.29)	-4.29 (2.89)	-1.92 (2.94)
6 month lagged effects:				
Uncovered workers	0.09 (4.12)	0.25 (2.71)	-2.16 (2.43)	-1.35 (2.75)
Covered workers	5.60 (6.41)	-1.96 (3.60)	-5.31 (3.27)	-1.06 (3.00)
12 month lagged effects:				
Uncovered workers	2.80 (4.92)	2.53 (3.46)	-0.36 (2.85)	-0.70 (2.84)
Covered workers	10.47 (6.83)	0.53 (4.65)	-4.80 (3.62)	-1.16 (3.35)

Note: Contemporaneous effects, and effects lagged by 6 and 12 months, are estimated from separate specifications, as in Table 3. See Table 3 notes for further details.

Table 6 – Contemporaneous and lagged effects on the probability that a worker’s earnings are below particular poverty lines

Assumed family size of wage earners	Single parent, 2 children	2 parents, 2 children
Specification 1:		
Minimum wage	-5.39 (2.32)	-2.04 (2.65)
Living wage	0.87 (0.84)	0.97 (0.97)
Specification 2:		
Minimum wage 6 months ago	-6.16 (2.44)	-1.26 (2.82)
Living wage 6 months ago	0.53 (0.87)	0.93 (1.06)
Specification 3:		
Minimum wage 12 months ago	-0.22 (0.26)	0.41 (3.06)
Living wage 12 months ago	-0.65 (1.06)	-0.05 (1.24)

Note: Reported are the estimated effects of minimum wages and living wages effective in an SMSA on the probability that a worker’s earnings are below the specified poverty line, if each wage earner was the only source of income in a family, using linear probability models. Poverty thresholds are imputed for 1999 using the CPI. Robust standard errors are reported in parentheses. All estimates are multiplied by 100. The sample includes information on workers in 63 SMSAs across 46 months (January 1996 - October 1999). The sample size is 272,024. See Table 3 notes for further details.

Table 7 – Contemporaneous and lagged effects on hours worked by individuals in various ranges of the imputed wage distribution of SMSAs

Percentile range of SMSA's imputed wage distribution	Below 10th	10th – 25th	25th – 50th	50th – 75th
Specification 1:				
Minimum wage	-0.65 (2.55)	2.92 (2.48)	1.24 (1.90)	1.13 (1.77)
Living wage	-0.45 (0.93)	-0.44 (0.94)	0.58 (0.69)	0.80 (0.64)
Specification 2:				
Minimum wage 6 months ago	-0.68 (2.37)	5.52 (2.48)	0.12 (1.95)	-0.28 (1.80)
Living wage 6 months ago	-1.25 (0.98)	-0.75 (1.01)	0.91 (0.72)	0.20 (0.70)
Specification 3:				
Minimum wage 12 months ago	2.02 (2.58)	-1.70 (2.62)	3.46 (2.08)	-1.39 (1.86)
Living wage 12 months ago	-2.22 (1.20)	-0.97 (1.17)	0.07 (0.92)	0.22 (0.87)
Sample size	51,071	73,454	122,361	123,253

Note: Reported are the estimated effects of the minimum wage and living wage effective in an SMSA on the weekly hours of individuals in the range of an SMSA's imputed wage distribution specified at the top of each column. The wage distribution is imputed using basic respondent characteristics. Robust standard errors are reported in parentheses. All estimates are multiplied by 100. The sample includes information on workers in 85 SMSAs across 46 months (Jan. 1996 - October 1999). See Table 3 notes for further details.

Table 8 – Contemporaneous and lagged effects on the probability of employment in various ranges of the imputed wage distributions of SMSAs

Percentile range of SMSA's imputed wage distribution	Below 10th	10th – 25th	25th – 50th	50th - 75th
Specification 1:				
Minimum wage	-4.08 (7.20)	4.99 (6.08)	5.09 (4.44)	2.48 (3.90)
Living wage	0.74 (2.68)	-2.10 (2.35)	0.60 (1.54)	1.71 (1.41)
Specification 2:				
Minimum wage 6 months ago	-6.49 (7.17)	12.51 (6.20)	-2.29 (4.62)	-0.13 (3.96)
Living wage 6 months ago	-2.16 (2.90)	-1.52 (2.51)	2.10 (1.58)	0.71 (1.55)
Specification 3:				
Minimum wage 12 months ago	6.35 (7.63)	-5.84 (6.36)	5.37 (4.78)	-0.66 (4.24)
Living wage 12 months ago	-9.25 (3.60)	-1.87 (2.83)	0.82 (2.10)	0.65 (1.90)
Sample size	51,071	73,454	122,361	123,253

Note: Reported are the estimated effects of the minimum wage and living wage effective in an SMSA on the employment of individuals in the range of an SMSA's imputed wage distribution specified at the top of each column, using linear probability models. See Table 3 and Table 6 notes for further details.

Table 9 – Contemporaneous and lagged effects on the percentage of families with earnings below the poverty line

Specification 1:

Minimum wage	-2.55 (3.38)
Living wage	-1.03 (1.19)

Specification 2:

Minimum wage 6 months ago	-5.72 (3.29)
Living wage 6 months ago	0.04 (1.27)

Specification 3:

Minimum wage 12 months ago	-0.46 (3.54)
Living wage 12 months ago	0.44 (1.48)

Note: Reported are the estimated effects of minimum wages and living wages effective in an SMSA on the percentage of families with poverty-level earnings. Poverty thresholds are imputed for 1999 using the CPI. Standard errors are in parentheses. The level of observation is at the SMSA-month. The regressions are estimated by least squares, weighted by the square root of the cell size. There are 2,300 observations (53 SMSAs across 46 months, Jan. 1996 – Oct. 1999). See Table 3 notes for further details.

Table 10 – Contemporaneous and lagged effects on the percentage of families with earnings or income below the poverty line

	Effects on the percentage of families with total earnings below the poverty line in an SMSA-month (for ORGs) or SMSA-year (for ADFs)				Effects on the percentage of families with total income below the poverty line in an SMSA-year (for ADFs)			
	(1)	(3)	(3)	(4)	(5)	(6)	(7)	
Specification 1:								
Minimum wage (December of prior year for ADFs)	-0.17 (3.85)	-2.41 (7.18)	-4.86 (6.71)	-4.07 (5.79)	-9.22 (6.62)	-8.42 (5.85)	-6.99 (5.05)	
Living wage (December of prior year for ADFs)	-1.57 (1.54)	1.11 (1.68)	0.68 (1.76)	0.96 (1.63)	1.12 (1.54)	0.12 (1.54)	0.37 (1.42)	
Specification 2:								
Minimum wage 6 months ago (June of prior year for ADFs)	-5.06 (4.04)	4.59 (5.95)	0.86 (5.39)	1.36 (4.94)	-1.50 (5.55)	-0.90 (4.73)	-0.08 (4.31)	
Living wage 6 months ago (June of prior year for ADFs)	-1.08 (1.77)	-1.04 (2.19)	-0.94 (2.31)	-1.08 (2.08)	0.71 (2.04)	-0.12 (2.02)	-0.46 (1.82)	
Specification 3:								
Minimum wage 12 months ago (January of prior year for ADFs)	2.40 (4.45)	10.76 (8.50)	6.14 (7.43)	3.23 (6.49)	3.89 (7.91)	4.79 (6.51)	3.24 (5.65)	
Living wage 12 months ago (January of prior year for ADFs)	-1.48 (1.98)	-2.74 (2.31)	-2.64 (2.44)	-2.84 (2.37)	-3.50 (2.15)	-3.72 (2.14)	-3.64 (2.07)	
Data set	ORG	ADF	ADF	ADF	ADF	ADF	ADF	
Sample restrictions	>=25 per cell	SMSAs from ORGs	>=25 per cell	>=25 per cell	SMSAs from ORGs	>=25 per cell	>=25 per cell	
Years that sample covers	96-98	96-98	96-98	95-98	96-98	96-98	95-98	
Number of observations	1908	159	654	872	159	654	872	

Note: Reported are the estimated effects of minimum wages and living wages effective in an SMSA on the percentage of families with poverty-level earnings or income. Standard errors are in parentheses. The level of observation is at the SMSA-month for Outgoing Rotation Group (ORG) samples and at the SMSA-year for Annual Demographic File (ADF) samples. The regressions are estimated by least squares, weighted by the square root of the cell size. Given that the ADF surveys are conducted in March and information on family earnings and income refer to the prior calendar year, the applicable contemporaneous and lagged minimum and living wages are noted in parentheses in the left-hand column. The ADF regressions include year dummy variables instead of month dummy variables. See Table 3 and Table 9 notes for further details.

Appendix Table A1 – SMSAs used in the various samples

Akron	Cincinnati*+	Kansas City*+	Oklahoma City*+	St. Louis*+
Albany	Cleveland*+	Las Vegas*+	Omaha*+	Salt Lake City*+
Albuquerque*	Columbus*+	Little Rock	Orange County (CA)	San Antonio*+
Allentown	Dallas *+	Los Angeles*+	Orlando*+	San Diego*+
Anchorage*	Dayton	Louisville*	Philadelphia*+	San Francisco*+
Atlanta*+	Denver*+	Miami*+	Phoenix*+	San Jose*+
Austin	Detroit*+	Middlesex (NJ)*	Pittsburgh*+	Seattle*+
Baltimore*+	Fargo	Milwaukee*+	Portland (OR)*+	Springfield (MA)
Bergen-Passaic*+	Fort Lauderdale*+	Minneapolis*+	Portsmouth (NH)	Tampa*+
Birmingham	Fort Worth*+	Monmouth (NJ)*	Providence*+	Tucson
Boise*+	Grand Rapids*+	Nashville*	Provo	Tulsa
Boston*+	Greenville (SC)*	Nassau*+	Raleigh-Durham*+	Washington*+
Buffalo*+	Hartford*	New Orleans*+	Reno	West Palm Beach
Burlington (VT)*	Honolulu*+	New York*+	Richmond	Wichita
Charleston (WV)	Houston*+	Newark*+	Riverside*+	Wilmington*
Charlotte*+	Indianapolis*	Norfolk	Rochester*+	Youngstown*
Chicago*+	Jacksonville+	Oakland*+	Sacramento+	Winston-Salem*+

Note: The SMSAs listed appear in at least the sample that is used to estimate hours and employment effects. An asterisk (*) denotes that the city is in the sample that is used to estimate the wage effects as well. A plus (+) denotes that the city is in the sample that is used to estimate the poverty effects in Table 9, and in columns (1), (2), and (5) of Table 10; in the other columns the set of SMSAs is much larger than that listed in this table.