The Impact of Living Wages on Employers: A Control Group Analysis of the Los Angeles Ordinance

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Abstract

This paper analyzes the impact of the Los Angeles Living Wage Ordinance on employers using two original data sets and a quasi-experimental research design. Relative to a control group of establishments, the starting pay of low-wage workers has risen by $1.70 per hour, paid days off have risen by two days, and employer-paid health benefits have not significantly changed among establishments covered by the living wage ordinance. Living wage establishments have witnessed a sizeable reduction in low-wage worker turnover, a drop in absenteeism, reduced overtime hours, and reduced job training relative to the control group of establishments. The ordinance appears to have had no significant impact on the use of part-time workers, the intensity of supervision, or the tendency of living wage firms to fill vacancies from within.

Thanks to Mark Brenner of the Political Economy Research Institute for assistance in constructing the sample of nonliving wage firms, to Max Neiman for helpful advice on survey technique, to Ted Levine for processing the survey results, and to all the graduate student survey enumerators who worked on the project. Thanks are also due to Carolina Briones of the L.A. Alliance for a New Economy and to David Runsten of the North American Integration and Development Center at UCLA for their collaboration on this project, which included most importantly preparing the data from the living wage employer survey for this analysis. Helpful comments were received from Cecilia Conrad, Michael Reich, David Neumark, an anonymous referee, and from participants in the 2003 Living Wage Research Conference at UC-Riverside. Financial support from the Ford Foundation and from the UC Institute for Labor and Employment is gratefully acknowledged

Living wage ordinances have spread rapidly across U.S. cities since the first successful campaign in Baltimore, Maryland, in 1994. Currently, roughly 110 cities possess living wage ordinances of one type or another (Luce forthcoming). These ordinances typically strive to increase the hourly wages of workers in city service contract firms so that, on a full-time basis, they are equal to or greater than the amount required to bring a family of four above the federal poverty line. In addition, some ordinances mandate or give encouragement to employers to provide health benefits to workers, and some stipulate a minimum number of paid days off for workers per year.

Research on the impact of living wages has, until very recently, taken the form of prospective studies that offer predictions about likely effects on employers, employees, and city budgets before the fact (Pollin and Luce 1998). There have been recent attempts to analyze the actual impact of living wage ordinances on the wages, employment, and poverty levels of workers using the Current Population Survey (Neumark and Adams forthcoming), but, prior to the research presented in this *Symposium*, no systematic effort has been made to analyze the actual impact of living wage ordinances on employers, or to collect information directly from the affected parties themselves in order to better measure effects.

This paper combines an original survey of establishments affected by the Los Angeles living wage ordinance with an original and comparable survey of nonliving wage establishments in Los Angeles to analyze the impact of living wages on employers. The survey of living wage establishments provides us with our very first look at the "before/after" impact of a living wage ordinance on the wages, benefits, and paid days off that employers offer workers, as well as the indirect impact of the ordinance on human

resource outcomes such as turnover, training, absenteeism, and overtime. The control group analysis allows one to discern from simple "before/after" comparisons the true effect of the ordinance on covered firms.

The Los Angeles Living Wage Ordinance

The Los Angeles living wage ordinance was passed in 1997 by the L.A. City Council, and went into effect in May of that year. It was the tenth such ordinance to be passed in the country. At present, perhaps as many as 6,500 workers in 375 firms are directly affected by the wage provision of the ordinance, and thousands more are likely to be affected by the time-off provision.[1] Landscape laborers, janitors, security guards, food service workers, and parking attendants are among the more prominent occupational groups to be affected by the ordinance.

The ordinance covers: (1) companies with a city service contract of $25,000 or more and their subcontractors; (2) companies that receive economic development subsidies (i.e., "business assistance") of $1 million or more in one year, or $100,000 or more annually on an ongoing basis, and their subcontractors; and (3) companies that have a lease with the city, or are granted a license or permit, and their subcontractors.[2] Firms that had a contract or lease at the time the ordinance was passed did not become covered until that contract or lease was renewed.

[1] There are estimated to be roughly 11,000 workers in total at establishments that are affected by the wage provision, and our survey results suggest that roughly sixty percent of workers in these establishments are directly affected by the ordinance. This excludes workers higher up in the wage distribution who received wage increases in order to maintain internal wage norms. It also excludes wage spillovers to workers in the uncovered sector.

[2] Henceforth, for ease of exposition, I refer to all as simply "city service contractors."

Beginning in 1997, covered firms were required to pay their employees who worked on city contracts or on city property a minimum of either $8.50 per hour or $7.25 per hour plus a $1.25 hourly contribution to employee health benefits. The two-tier wage structure was intended to give employers an incentive to provide health insurance to their workers. The living wage levels rise every year, and are indexed to the annual increase in the city employee pension fund. The benefits credit is not indexed. Currently the wage levels stand at $9.78 per hour or $8.53 plus the $1.25 employer contribution to health benefits. Firms must also provide their covered employees with 12 paid days off and 10 unpaid days off per year.

Data and Empirical Methodology

Two original data sets are employed in generating the results of this study. The Survey of Los Angeles Living Wage Employers (SLWE) was conducted as part of a larger project to explore the impact of the Los Angeles living wage ordinance on affected workers. The employer survey was the first stage in an intentional two-part survey design that includes a survey of affected workers at these very same establishments as a second stage. Structured, in-person interviews of roughly 1-2 hours duration were initially conducted with living wage employers at their offices, and then their approval and assistance was solicited in contacting, sampling, and surveying their workers. It is the structured survey information from these employer interviews that is used in the present analysis.

The sampling frame for the SLWE was developed from a database maintained by the City of Los Angeles, and in particular from its list of “priority one” firms – those deemed by the city to employ significant numbers of low-wage workers. A two-stage

stratified cluster approach was used to generate the sample of survey establishments. The "priority one" firms were first stratified into the following industrial groups:

- Airlines
- Airline services, including security screening, baggage handling, and skycaps
- Janitorial
- "Outdoor work," including landscape maintenance, brush clearance, and tree trimming
- Retail and food service
- Security and parking
- Social services
- Transit
- Miscellaneous

Each industrial group was then further subdivided into large firms (>50 workers) and small firms, and a random sample was taken from each stratum. Large firms were over-sampled for cost and clustering reasons. In order to obtain information from a sufficient number of workers in all affected occupations, as well as from union and non-union firms, it was decided that a sample size of around 75 firms was required.

The data on nonliving wage establishments come from the Survey of Diversity in Human Resource Practices (SDHRP) – a survey of roughly 210 establishments in Los Angeles that documents differences in wage and benefits packages, and in human resource outcomes such as turnover, training, and absenteeism, among establishments in narrowly-defined industrial groups. The SDHRP was explicitly designed to mirror the size and sectoral distribution of the "priority one" firms in the SLWE. However, the nonliving wage establishment survey excluded firms in the airline, transit and miscellaneous industry categories that are part of the living wage survey, and included firms in the childcare industry, which are absent from the living wage survey.

In constructing the nonliving wage sample, a sample frame of establishments with a business listing (i.e., phone number, street address, or both) was created from the

targeted set of industries.[3] With the sampling frame established, a two-stage stratified sampling approach was invoked in which establishments were first divided into industry sectors along the lines of the SLWE survey, and then within each sector further divided into large (>50 employees) and small establishments.

The nonliving wage survey questions were patterned directly after those in the living wage survey. Both ask about contemporary conditions and about conditions prior to the living wage ordinance. The timing of the two surveys is roughly comparable – the SLWE began in the fall of 2001 and lasted through the fall of 2002 while the SDRHP took place in the spring and summer of 2002 – so the responses to questions regarding contemporary conditions cover a similar time period.

Despite the similarities, there are a number of differences that might raise concerns of comparability. First, the surveys were conducted by different methods. The living wage survey was conducted in-person, during an on-site visit, whereas the nonliving wage survey was conducted largely by mail, but with significant telephone contact to encourage responses, to answer respondents' questions, and to clarify responses following the receipt of returned surveys. Second, the living wage survey had the official endorsement of the Los Angeles city government, and consequently a much higher response rate (68%) than the nonliving wage survey (23%). How these differences affect the comparability of the results is unclear.

Although both surveys pose similar retrospective questions, because living wage establishments come under the provisions of the ordinance at different times (once their contract is renewed or upon receiving a new contract with the city) there is the issue of

[3] Target industries were defined by four-digit SIC codes. A commercial data product – ReferenceUSA – was used to identify establishments in these target industries.

how to capture a comparable "before" period in the nonliving wage sample. I chose to ask uncovered firms about conditions today versus two years prior, based on preliminary evidence from the living wage survey that the "average" living wage firm came under the ordinance in the middle of the year 2000. However, this "average" disguises much diversity, with some firms coming under the regulations of the ordinance in 1997 and others as late as 2002. Thus, while contemporary information across the two surveys is reasonably comparable, retrospective information is comparable only on average.

This is perhaps acceptable in "before/after" comparisons of variables such as job training that tend to be fairly stable over time and to change only in reaction to major changes in technology or government policies such as living wage mandates. However, for other variables, such as the wage rate, it is probably important for the "before" dates to match as much as possible. In these cases, I offer "before/after" comparative results using, first, the entire sample of living wage establishments and then smaller samples of living wage establishments with "before" dates that are more tightly distributed around the narrow range of "before" dates of the nonliving wage establishments – that is, the spring and summer of 2000.

A final concern about comparability stems from the differing profile of low-wage workers across the two surveys. The living wage survey focussed only on establishments whose workers were affected by the living wage ordinance, whereas the nonliving wage survey, although it sampled in the same industry sectors, was not restricted to establishments with low-wage workers. (Low-wage workers are defined, for the purposes of the nonliving wage survey, as workers who possess wage and health benefits packages that are lower than those stipulated by the living wage). To address this concern, in the

comparative results below I offer findings, first, for the larger sample of nonliving wage firms and then for a smaller sample of these firms all of whom possess low-wage workers.

The empirical approach is that of a "natural or quasi-experiment" in which the living wage establishments are the "treatment" group and the nonliving wage establishments are the "control" group. For purposes of comparison, I use the difference in differences methodology employed by Card and Krueger (1994), who analyzed the impact of the New Jersey minimum wage increase on employment in the fast food industry there, using changing employment in similar fast food establishments in Pennsylvania as the control group.

Specifically, I begin by testing for significant differences in the mean "before/after" changes in particular variables of interest across the living wage and nonliving wage samples by estimating regressions of the following form:

$$(1) \qquad X = B_o + B_1 LW + \varepsilon,$$

where X is the variable of interest (such as change in wages or change in turnover), Bo is an intercept term, LW is a dummy variable indicating coverage by the living wage ordinance, B_1 is the difference in mean differences in X across the living wage and nonliving wage samples, and ε is the error component.

Changes in variables of interest in the nonliving wage sample are assumed to derive from shocks to or cyclical effects in the economy that have similar consequences for living wage and nonliving wage firms.[4] Any change in a variable of interest in the

[4] This assumes the living wage ordinance has no effect on the uncovered sector. Perhaps this a reasonable assumption given that the ordinance is estimated to affect less than 2% of workers in the lowest wage decile of the Los Angeles labor force (Brenner, Wicks-Lim, and Pollin 2002). However, Reich, Hall, and

living wage sample beyond that in the nonliving wage sample is assumed to be due to the effect of the living wage ordinance. Of course, if living wage establishments and nonliving wage establishments differ along important dimensions, cyclical or shock effects might not be similar across the two samples. Thus, I also compare mean changes in variables conditional on establishment size, union status, industrial sector and a host of other features by adding these as controls to the right-hand side of equation (1).[5]

Cyclical or shock effects might also have different impacts on the two sectors if there is nonrandom selection into city service contract work under the living wage ordinance after controlling for these features of establishments. Suppose, for example, that city service contractors may be drawn disproportionately from among high-wage or high-benefits establishments. In this case, cyclical or shock effects observed in the uncovered sector may not be an accurate reflection of what would have happened to firms in the covered sector in the absence of the living wage ordinance.

I am able to shed some light on the possibility of nonrandom selection of firms into city service contract work by exploring the wage and benefits policies of covered and uncovered firms prior to the living wage ordinance. If these two sets of establishments lack substantive differences in wage and benefits policies prior to the living wage, this is

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Jacobs (in this volume) offer evidence of significant wage spillovers to the uncovered sector as a result of a living wage ordinance at the San Francisco Airport.

[5]In some cases, the measure of change is qualitative rather than quantitative. For example, I may only possess a trichotomous measure indicating whether a variable of interest increased, decreased, or remained the same over the period. In these cases, for purposes of comparison with the simple difference in means, I present results from a linear regression analysis using OLS. But, in every case I have also conducted the analysis using an ordered probit technique. Ordinary regression analysis takes the difference between a reported decrease and no change to be the same as that between a reported increase and no change, whereas in fact they are merely a ranking – a decrease is less than no change and an increase is more than no change. Ordered probit techniques correctly consider the differences to be merely a ranking.

suggestive evidence that currently covered firms are essentially a random draw from the larger population of firms.

Note that nonrandom selection into city service contracting, and into city service contracting under the living wage ordinance in particular, also affects the interpretation one can give to my results. I am investigating the impact of the living wage ordinance on current city service contractors, using contemporary and retrospective information from currently covered and uncovered establishments. I know nothing about the human resource policies of firms that exited city service contract work following passage of the living wage ordinance.[6] Thus, my results may give a biased estimate of the impact of the ordinance on city service contractors if establishments that are new to city service contract work after the living wage ordinance are substantively different from those that exited city service contract work following passage of the ordinance.

Suppose, for example, that the introduction of the living wage ordinance enticed establishments with high wage policies to bid on city service contracts, whereas before they never would have done so. My results regarding the impact of the living wage on current city service contract firms will be a negatively biased estimate of the living wage impact on the city service contract sector because the pre-policy wages of firms who exited city service contract following the living wage were inferior to the pre-policy wages of new contractors.

It is perhaps tempting to take the results of this analysis as a reflection of the impact of the living wage on workers. However, an additional form of selection affects

[6] In the original sample of nonliving wage firms, there were three establishments that had at one time done city service contract work. They were removed from the analysis in this study for fear that their "before" wage might have been the living wage. They are of little use to me in this analysis since I am unable to identify when they exited city service contract work.

the extent to which my results offer meaningful evidence on the impact of living wages on workers. Suppose city service contract firms alter hiring policies as a result of the living wage ordinance, selecting, for example, more high-skill labor to replace low-skill labor in reaction to the mandated wage increase (this is the so-called "labor-labor substitution" effect). I possess no information on the changing hiring policies of firms as a result of the living wage. Thus, unless this selection process was unaffected by the ordinance, my results are unlikely to be an unbiased estimate of the impact of the living wage on workers.[7]

Table 1 lists the variables to be used in the analysis. The first four variables following the living wage establishment dummy are used as controls in the analysis, along with a set of seven industry dummies and, in some regressions, three occupation dummies. The "impact" variables follow in the list. Changes in wages, health benefits, and paid days off can be considered "direct impact" variables, as the Los Angeles living wage ordinance attempted to directly alter these employment conditions among city service contractors.

The remaining "impact" variables chart out "indirect effects" of the ordinance, as establishments and workers adjust their behavior to the new wage and benefits conditions. Under this category, I explore the impact of the ordinance on changes in job training, turnover, unscheduled absenteeism, and overtime. In addition, I have analyzed the impact of the living wage ordinance on the usage of part-time workers, the extent of supervision, and the percentage of positions filled from within, but none of these results

[7] Moreover, before we could say anything definitive regarding the impact of the living wage ordinance on workers, we would also have to know more about the wage spillover effects – to workers higher up in the wage distribution and to workers in the uncovered sector.

was statistically significantly different across the living wage and nonliving wage samples.

It is important to note that different impact variables are measured with respect to different reference groups of workers. For example, wage changes are measured with regard to the starting pay of the largest low-wage occupation in the establishment. Benefits have as their reference group workers whose pay was affected by the ordinance (or would otherwise have been affected, in the case of nonliving wage establishments). Absenteeism is measured at the establishment level.

Table 2 gives the basic characteristics of establishments in the living wage (LW) and nonliving wage (NLW) samples. These characteristics are used as control variables in the analysis to come. The starting samples included 70 living wage establishments and 207 nonliving wage establishments. I deleted from consideration any establishment at the Los Angeles airport and any establishment in the airline services industry for fear that "before/after" comparisons would be tainted by the devastating impact of the events of 9/11 on this sector.[8] In addition, any establishment in the nonliving wage sample that was either a current or former city service contractor was deleted from consideration. That left me with a basic working sample of 48 living wage establishments and 184 nonliving wage establishments.

The two samples have roughly similar characteristics, reflecting the fact that the nonliving wage sample was intentionally drawn to mirror the living wage sample. Nonetheless, a few important differences stand out. Living wage establishments are on

[8] A reviewer suggested conducting some preliminary analyses with these firms included in the sample. Living wage establishments at the airport reported significantly greater absenteeism following the living wage, marginally more job training, and a marginally lower tendency to fill positions from within than comparable living wage firms not located at the airport. All are plausibly related to 9/11.

average larger than nonliving wage establishments. However, my focus in the analysis that follows is not on the larger firm with which the city service contractors are associated, but rather on the city service contract work only, as this is where the living wage will have its bite. Thus, I conceive of the city service contract operation as an "independent" establishment, and measure changes in impact variables with regard to that operation alone. The average number of workers per establishment on city service contract work is 62 (with a standard deviation of 157), which is much closer to the average for the nonliving wage sample.

Representation among the eight industry groups is reasonably similar across the two samples except that the living wage sample has no parking establishments and the nonliving wage sample possesses no establishments in the miscellaneous industry category, which includes a grab bag of establishments from amusement park operations to auto upholstery and street sign manufacturing and installation.[9] Parking establishments in the living wage sample were lost with the deletion of firms operating at the airport. The nonliving wage sample was not drawn to include establishments from the miscellaneous industry category. In the analysis that follows, I include establishments from these two industries, but check that the results are robust to their exclusion as well.

In some of the analysis below I work with information on the largest low-wage occupation in the establishment. The four most prominent low-wage occupations in the data are listed at the end of Table 2. Landscape laborers are by far the most commonly reported low-wage occupation, followed by janitors, teacher's aides, and security guards.

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[9] Establishments in the childcare sector in the nonliving wage sample were placed in the "Social Services" industry category.

In several instances, I check for the robustness of findings by limiting the sample to establishments that name one of these four categories as their largest low-wage occupation and replicating the results using detailed occupation controls.

Results

I begin with an analysis of the direct effects of the living wage ordinance, the results of which appear in Table 3. The most important impact a living wage ordinance is likely to have on an establishment is the wage it pays its low-wage workers. The results in row one of Table 3 indicate that starting wages for the largest low-wage occupations have increased far more significantly among living wage establishments ($2.39) than among nonliving wage establishments ($0.73), with a difference of $1.66. The fairly sizeable increase in the wages of low-wage workers at nonliving wage establishments – an increase of roughly ten percent over two years – is probably due in part to the increase in California's minimum wage over this period.[10]

The column four results reveal that the difference in wage increases is not greatly affected by conditioning on establishment size, union status, industry category or the other control variables.[11] There is also surprisingly little change in the results when adjustments are invoked to make the duration of the "before/after" time period more

[10] The California minimum wage increased from $5.75 to $6.75 between the summer of 2000 and the summer of 2002. It rose from $5.75 to $6.25 on January 1, 2001 and then to $6.75 on January 1, 2002.

[11] The full set of results for every "difference with controls" regression is reported in Appendix Table 1. The base industry category is the "miscellaneous" set of establishments from the living wage sample. The negative wage change associated with unionized establishments in the column one results derives entirely from the living wage sector, where the effect is negative and statistically significant in a wage change regression for that sector alone. Perhaps this is because unionized establishments paid higher wages to begin with, but it may also be because unionized establishments are not required to strictly abide by the wage provisions of the living wage ordinance. Thus, unions may use the leverage of living wage ordinances to win other worker demands. Alternatively, unionized employers may use the special exemption for unionized establishments to keep a lid on wage increases.

comparable across the two samples, as shown in column five. For this result, I have eliminated living wage firms that came under the provisions of the ordinance in 1997 and 2002. However, the basic result is robust to even further trimming in the tails of the distribution of effective coverage dates. If I restrict the analysis to living wage establishments that came under the ordinance in 1999 and 2000, the estimated difference in wage changes is $1.73 and is statistically significant. If I further restrict the living wage sample to establishments whose effective coverage date is 2000 – thereby making it strictly comparable to the 2000 "before" date for the nonliving wage firms – the estimated difference is $1.60, with a t-statistic of 4.5.

In column six I restrict the analysis to the four most prevalent low-wage occupational categories – landscape laborers, janitors, teacher's aides, and security guards – to check for the robustness of the earlier findings in an analysis using detailed occupational controls. The estimated difference declines somewhat, but remains strongly statistically significant. This result holds true as I further restrict the number of occupations in the sample. In the end, with only 12 living wage establishments and 39 nonliving wage establishments in the analysis, the estimated difference in wage increases for landscape laborers alone is $1.60, with a t-statistic of 6.

Rows two and three of Table 3 present results for both the current wage and the wage before coverage by the living wage ordinance (for living wage establishments) or two years prior (for nonliving wage establishments). The results suggest that the current difference in wages across the two samples is entirely the result of the living wage ordinance. The current wage difference is strikingly similar to the difference in wage changes over the period, and there is no statistically significant difference in starting

wages for the largest low-wage occupations before the ordinance took effect, which suggests the absence of nonrandom selection into city service contract work.

A unique feature of the data is that all living wage establishments were asked what starting wage they would offer workers in their largest low-wage occupation in the absence of the living wage ordinance. Thus, additional insight into the existence of nonrandom selection can be gleaned by asking whether these counterfactual wages are significantly different from the actual wages that observably similar nonliving wage establishments offer their workers. The results are reported in row five of Table 3. The mean counterfactual starting wage for low-wage occupations in living wage firms is strikingly similar to the mean starting wage currently offered by nonliving wage firms. There is no statistically significant difference between the two, even with controls (column five) and even when this difference is explored within a narrow set of specific occupations (column seven).

In addition to influencing wages, the Los Angeles living wage ordinance gives an incentive for employers engaged in city service contract work to offer health benefits to their employees. The incentive takes the form of a wage minimum that is $1.25 per hour lower for establishments that offer employer-paid health benefits to their workers. The results in row seven of Table 3 suggest that the incentive is insufficient. There is absolutely no change in the incidence of employer-paid health benefits to affected workers among city service contractors as a result of the living wage ordinance, and although there is a very marginal decline in such coverage among nonliving wage establishments, the difference in differences is quantitatively negligible and statistically insignificantly different from zero.

Despite the absence of any significant impact of the living wage ordinance on employer-paid health coverage, the results in row eight suggest that city service contract employers are significantly more likely to offer such coverage than are their nonliving wage counterparts. Moreover, this greater propensity to offer employer-paid health benefits was present before the living wage took effect, as seen in the results of row nine. Thus, city service contractors appear to be a select group regarding employer-paid health benefits, possessing a much higher propensity to pay for health benefits for their workers irrespective of the living wage ordinance.

The final direct effect of the living wage ordinance I consider is the impact on paid days off. The Los Angeles living wage ordinance mandated that city service contractors offer at least twelve paid days off per worker per year. The results in the final rows of Table 3 suggest that the ordinance was responsible for a significant increase in paid days off for employees working on city service contracts. Paid days off for workers who were affected by the ordinance rose by almost three days, while paid days off for low-wage workers in the nonliving wage sector witnessed little change. The difference in differences is around two days off – both with and without controls – and is statistically significantly different from zero. The evidence also suggests that the difference is attributable to the ordinance itself, as there was little difference in mean paid days off between living wage and nonliving wage establishments before the ordinance.

Before turning to the results for the various indirect effects of the living wage ordinance, it is perhaps important to say something about its impact on the most-studied aspect of wage mandates – namely, employment levels. Because the nonliving wage establishment survey did not ask about changes in staffing levels over the period, I am

unable to offer a control group analysis of this important topic. However, the living wage survey did ask about staffing level changes, and, moreover, asked employers what portion of any such changes were attributable to the living wage ordinance. Admittedly, the validity of this exercise rests on the ability of employers to render an accurate assessment of the wage and employment levels in the absence of the ordinance, but the counterfactual wage results above suggest that employers may be surprisingly good at this.

Eighteen percent of the sample of living wage establishments report changes in staffing levels as a result of the living wage ordinance, and all report declines in employment. The loss in employment, factoring in the firms reporting no change in employment, is 1.6 percent of the aggregate labor force of all living wage firms in the sample, and 2.6 percent of the affected labor force (i.e., those who received mandatory wage increases as a result of the ordinance). Using as a benchmark the estimated $1.70 increase in wages attributable to the living wage ordinance (from Table 3), wages rose an average of 25 percent, implying an elasticity of low-wage worker demand of roughly –0.10.[12]

In Table 4 I present the difference in differences results for various indirect effects of the living wage ordinance – changes in human resource outcomes that derive from the behavioral reactions of employers and employees to the impact of the ordinance on wages and benefits. I begin with an analysis of the impact on turnover. Labor turnover

[12] Note that not every "affected" worker received a wage increase as large as this – the $1.70 increase is for beginning workers in the largest low-wage occupations only. Moreover, this estimate assumes that the employment loss figure applies to low-wage workers only, whereas employment loss among these workers may have been less if workers whose wages were unaffected also lost employment, or may have been more if firms increased their employment of more skilled labor in reaction to the living wage mandate.

is costly to firms. There are costs associated with searching for and training new workers, and also reduced productivity and perhaps damaged equipment during the training period.

Note, to begin with, that each turnover variable is measured with respect to a different reference group of workers. The change in turnover variable is measured with respect to affected/low-wage workers, whereas the two contemporary turnover variables are measured at the establishment level and with regard to the largest low-wage occupation respectively. Secondly, in every single case adjustments were required in order to make the worker reference groups comparable across the living wage and nonliving wage samples. This is because the survey questions themselves referred to different worker reference groups in the different surveys. So, for example, in the living wage survey, the change in turnover is measured with respect to affected workers, but in the nonliving wage survey it is measured with respect to the establishment. In the living wage survey, numerical turnover measures are gathered for the entire firm (i.e., including workers, if any, that are part of the firm's operations outside of city service contract work) and for workers affected by the ordinance. In the nonliving wage survey, however, these measures are gathered for the establishment and for the largest low-wage occupation respectively. To further compound matters, the turnover question refers to quits and dismissals in the living wage survey, but incorporates lay-offs as well in the nonliving wage survey.

To make comparable the change in turnover measures across the two samples, I focus only on establishments in both samples that have a high (greater than 60%) share of low-wage workers, and consider the relevant reference group to be affected/low-wage workers. To create greater comparability in the two contemporary measures, I focus, in

the one case, on living wage firms whose city service contract work composes a large (greater than 60%) share of their overall operation, and, in the other case, on living wage firms that possess one and only one affected occupation. Finally, I check the robustness of every result to the exclusion of landscape firms, wherein the seasonality of the work implies that lay-offs may be more common.

The "change in turnover" results appear in row one of Table 4. The mean indicator measures of turnover change suggest that while there has been downward pressure on turnover in both the living wage and nonliving wage sectors, living wage establishments report significantly lower measures of change than do nonliving wage establishments. The difference in change indicators conditional on important establishment characteristics is nearly one-half a point out of a two-point scale. The statistically significantly greater downward pressure on turnover possessed by living wage establishments is also confirmed in an ordered probit estimation, where the coefficient on the living wage dummy is –0.9 with a t-statistic of 2.3.

For a sense of the numerical magnitude of the difference, I present two sets of results on contemporary turnover rates – one measured at the establishment level and the other for the largest low-wage occupation. For the results in row three, I have excluded from the living wage sample any firm whose city service contract employees do not constitute at least 60% of the total employees of the firm.[13] Given that the turnover measure for living wage establishments still incorporates workers outside of city service contracts, and who are therefore likely to be paid a wage lower than the living wage, the difference in turnover rates is astounding. Living wage establishments possess turnover

[13] Both the difference and difference with controls results are largely unchanged (both quantitatively and statistically) if I increase this constraint to 90% of the firm.

rates almost one-third those of nonliving wage establishments. Moreover, the estimated impact on turnover is robust (both quantitatively and statistically) to various changes in the sample of establishments. In column five, for example, only nonliving wage establishments that possess low-wage workers are included in the control group. In results not shown, when landscaping is removed from both samples for fear that the inclusion of lay-offs in the turnover measure for nonliving wage firms may taint the results, the estimated difference with controls is –20 with a t-statistic of 4.

In row five, differences in turnover rates for the largest low-wage occupation are presented. The results suggest that living wage establishments experience turnover rates among low-wage workers that are less than half that of nonliving wage establishments. The order of magnitude of the difference and its statistical significance changes very little when controls are added or when focusing on very specific occupational categories. If landscaping establishments are eliminated from the results in column three, the difference falls to –22.5 but remains statistically significant at the 10% level. When controls are added, however, the difference becomes statistically insignificant.

All else constant, living wage establishments possess turnover rates for their largest low-wage occupations that are 17 percentage points (or 35%) lower than nonliving wage establishments. What accounts for the significantly lower turnover rates among living wage establishments? The prime candidate, of course, is the significantly higher wage these establishments pay their low-wage workers, which itself appears to be due entirely to the living wage ordinance. In unreported results, I add the starting wage of the largest low-wage occupation as an additional control variable to the difference with controls result reported in row five, column four. The estimated coefficient on the living

wage dummy becomes positive, but statistically insignificantly different from zero, and the estimated coefficient on the starting wage variable is –18.67 with a t-statistic of 2.0. The lower turnover rate for low-wage occupations in living wage establishments is entirely accounted for by the higher wage that prevails there.[14]

We can get a sense of the savings in labor costs due to reduced turnover, and compare this with the increased labor costs due to the increased wage, with the following information. The average cost of replacing a low-wage worker (including separation, search, training, and lost productivity while the new worker comes up to speed) is reported to be $807 in the nonliving wage survey. Assuming a drop in turnover of 17 percentage points, the savings in turnover costs amount to $137 per low-wage worker per year. At an extra $1.71 an hour, the extra wage costs per worker amount to $3420 per year (assuming 40 hours of work per week for 50 weeks). Thus, the savings in turnover costs amount to roughly four percent of the increased wage bill per worker, per year.[15]

Unscheduled absenteeism may be viewed as a negative indicator of both employee job satisfaction and labor productivity. Workers who are dissatisfied with their jobs are more apt to miss work, and such absenteeism, especially if unplanned, may harm labor productivity. In row seven of Table 4, results are presented for indicator measures of changes in unscheduled absenteeism. The results reveal downward pressure on unscheduled absenteeism across both the living wage and nonliving wage sectors, but

[14] Recall that city service contractors also possess a greater propensity to offer health benefits to workers, which could be an additional contributing factor to the lower turnover among living wage firms. However, when the Current Health Benefits variable is added to the specification, it is statistically insignificant and the results reported above are unchanged.

[15] This is a rough, and arguably lower bound, estimate of the savings due to decreased turnover. The extra wage costs are calculated for entry-level positions in the largest low-wage occupations, where the living wage impact is likely to be the greatest. The savings from decreased turnover, however, are calculated for all positions in the largest low-wage occupations.

these downward pressures appear to be more significant for living wage establishments. While the simple difference is neither quantitatively or statistically significant, when the difference is made conditional on important establishment characteristics, and when changing absenteeism in living wage establishments is compared only to nonliving wage establishments that possess low-wage workers, the difference becomes quantitatively sizeable and statistically significant.

The results in row nine of Table 4 offer evidence on the living wage impact on the training of new workers in the largest low-wage occupations. Human capital theory posits that workers must pay for job training if the results improve their general human capital skills, and that one way workers may do so is through reduced wages. To the extent wage mandates such as living wages prevent workers from striking a deal with employers to reduce wages temporarily in exchange for job training, these mandates may result in lower on-the-job human capital acquisition for low-skill workers. Wage mandates might also reduce the need for job training if firms substitute towards higher-skill workers as a result.

The first thing to note about these results is that absolutely every living wage establishment reports no change in the hours of training since becoming covered by the living wage ordinance. Thus, absent the nonliving wage control group, we would be tempted to conclude that the living wage has had no affect on job training for low-skill workers. However, when compared to the responses of nonliving wage establishments, the results suggest that the living wage may indeed lower job training for workers entering low-skill occupations. The difference with controls result in column four is quantitatively small, but statistically significant. (Unfortunately, I could not check for the

robustness of these findings in a sample containing more detailed occupational categories because of insufficient variation in the training indicator variable.) The results offer suggestive evidence that job training in living wage firms has not kept pace with increased training in the nonliving wage sector.

The final piece of empirical evidence presented on indirect effects of the living wage on employers regards changes in overtime hours. In row eleven of Table 4 I explore the extent to which overtime hours have changed differentially in the living wage and nonliving wage sectors. The results reveal downward pressure on overtime work in living wage establishments, but increased pressure in nonliving wage establishments. Thus, the living wage ordinance – and most probably, the increased wage mandate itself, which increases the marginal cost of overtime work for employers – has reduced the use of overtime at living wage establishments.

Conclusion

This paper offers an analysis of the impact of the Los Angeles living wage ordinance on employers using a quasi-experimental research design. The results reveal that the ordinance has forced employers to pay starting workers in the largest low-wage occupations on average $1.70 more per hour in wages, and to offer affected workers roughly two additional paid days off. The ordinance has had no effect on the inclination of employers to offer paid health benefits to workers.

As a result of the increased compensation package, establishments have witnessed a reduction in turnover among workers in the largest low-wage occupations of roughly 35%, and a significant reduction in unscheduled absenteeism as well. The wage mandate

has led covered firms to reduce their use of overtime, and to devote fewer hours to training low-skill workers than comparable uncovered firms. The ordinance appears to have had no significant impact on the use of part-time workers, the intensity of supervision, or the propensity to fill vacant jobs from within.

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Table 1
Variable Definitions

LW Firm	=1 if the establishment is covered by the living wage ordinance; 0 otherwise.
Firm Size	Number of employees in the establishment.
For Profit	=1 if the establishment is a for-profit operation; 0 otherwise.
Independent Operation	=1 if the establishment is an independent operation; 0 if a subsidiary.
Unionized	=1 if workers are unionized; 0 otherwise.
Change in Wage	Current starting wage for workers in the largest low-wage occupation minus starting wage before the living wage ordinance/two years prior.
Current Wage	Current starting wage for workers in the largest low-wage occupation.
Wage Before	Starting wage for workers in the largest low-paid occupation before the living wage/two years prior.
Counterfactual Wage	Current starting wage for workers in the largest low-wage occupation if the firm were not covered by the living wage ordinance (or current starting wage for nonliving wage establishments).
Change in Health Benefits	=0 if workers affected by the living wage ordinance/low-wage workers lost employer-paid heath benefits after the ordinance/over the past two years; =1 if no change; =2 if workers newly received employer-paid health benefits.
Current Health Benefits	=1 if workers currently possess employer-paid health benefits; 0 otherwise.
Health Benefits Before	=1 if workers possessed such benefits before the living wage/two years prior.
Change in Paid Days Off	Current number of paid days off (vacation days plus sick days) for workers affected by the living wage/low-wage workers.
Current Paid Days Off	Current number of paid days off for affected/low-wage workers.
Paid Days Off Before	Number of paid days off before the living wage/two years prior.
Change in Turnover	=0 if turnover decreased for affected/low-wage workers; =1 if remained the same; =2 if increased.
Current Firm Turnover	Current turnover rate (per 100 workers per year) for the establishment.
Current Low-Paid Occupation Turnover	Current turnover rate for the largest lowest-wage occupation.

Change in Absenteeism	=0 if unscheduled absenteeism in the establishment decreased; 1 if stayed same; 2 if increased.
Change in Training	=0 if number of hours of training for a new worker in the largest low-wage occupation decreased after living wage/two years prior; 1 if stayed same; 2 if increased.
Hours of Training	Current hours of training for a new worker in the largest low-wage occupation.
Change in Overtime	=0 if average overtime hours for full-time workforce have gone down since the living wage ordinance/two years prior; 1 if stayed same; 2 if increased.

Table 2

Basic Establishment Characteristics

VARIABLE	LW SAMPLE Mean (Std. Dev.)	NLW SAMPLE Mean (Std. Dev.)
Firm Size	147 (200)	46 (137)
For Profit	0.77 (0.42)	0.86 (0.35)
Independent Operation	0.77 (0.42)	0.85 (0.36)
Unionized	0.04 (0.20)	0.03 (0.18)
Landscaping	0.27 (0.45)	0.35 (0.48)
Janitorial	0.08 (0.28)	0.13 (0.33)
Security	0.06 (0.24)	0.08 (0.27)
Parking	--	0.02 (0.15)
Food Service	0.10 (0.31)	0.06 (0.24)
Social Services	0.23 (0.42)	0.25 (0.43)
Retail	0.02 (0.14)	0.12 (0.33)
Miscellaneous	0.23 (0.42)	--
Landscape Laborer	0.25 (0.44)	0.23 (0.42)
Janitor	0.10 (0.31)	0.07 (0.25)
Teacher's Aide	0.02 (0.14)	0.05 (0.22)
Security Guard	0.02 (0.14)	0.03 (0.16)
Number of Observations	**48**	**184**

TABLE 3
DIRECT EFFECTS OF THE LIVING WAGE

Variable	LW Mean (Std.Dev.)	NLW Mean (Std. Dev.)	Difference	Difference With Controls	Difference Adjusted for Timing	Difference with Specific Occupations
Change in Wage	2.39 (0.89)	0.73 (0.50)	1.66** (0.11)	1.70** (0.12)	1.71** (0.15)	1.54** (0.19)
Current Wage	9.14 (0.57)	7.34 (0.61)	1.80** (0.10)	1.71** (0.11)		1.58** (0.16)
Wage Before	6.75 (0.90)	6.61 (0.73)	0.14 (0.14)			
$\frac{N_{LW}}{N_{NLW}}$	47 0	0 111	47 111	47 111	24 111	19 63
Counterfactual Wage	7.32 (0.85)	7.34 (0.61)	-0.02 (0.14)	0.01 (0.16)		0.27 (0.26)
$\frac{N_{LW}}{N_{NLW}}$	43 0	0 111	43 111	43 111		19 63
Change in Health Benefit	1.0 (0.0)	0.985 (0.12)	0.015 (0.010)	0.023 (0.016)		
Current Health Benefits	0.49 (0.51)	0.24 (0.43)	0.25** (0.08)	0.18** (0.08)		
Health Benefits Before	0.44 (0.50)	0.23 (0.42)	0.22** (0.08)			
$\frac{N_{LW}}{N_{NLW}}$	45 0	0 136	45 136	45 136		
Change in Paid Days Off	2.77 (5.15)	0.49 (2.12)	2.28** (0.84)	1.66* (0.92)		
Current Paid Days Off	10.13 (6.78)	7.59 (6.94)	2.54* (1.31)	1.42 (1.23)		
Paid Days Off Before	7.36 (7.67)	7.10 (6.90)	0.26 (1.35)			
$\frac{N_{LW}}{N_{NLW}}$	39 0	0 98	39 98	39 98		

*Statistically significant at the 0.10 level; ** Statistically significant at the 0.05 level. The results in columns four and five control for Firm Size, For Profit, Independent Operation, Unionized, and the eight industry categories in Table 2. The results in column six replace the industry controls with occupation controls for the four occupation categories in Table 2.

TABLE 4
INDIRECT EFFECTS OF THE LIVING WAGE

Variable	LW Mean (Std.Dev.)	NLW Mean (Std. Dev.)	Difference	Difference With Controls	Difference with Low Wage NLW Sample	Difference with Specific Occupations
Change in Turnover	0.71 (0.62)	0.97 (0.52)	-0.26** (0.13)	-0.39** (0.14)		
$\frac{N_{LW}}{N_{NLW}}$	24 / 0	0 / 59	24 / 59	24 / 59		
Current Firm Turnover	10.69 (9.63)	30.30 (41.32)	-19.61** (3.93)	-18.77** (5.11)	-20.78** (5.40)	
$\frac{N_{LW}}{N_{NLW}}$	18 / 0	0 / 160	18 / 160	18 / 160	18 / 130	
Current Low-Paid Occupation Turnover	21.48 (27.73)	48.71 (60.71)	-27.23** (8.02)	-16.97* (9.29)		-27.49** (8.92)
$\frac{N_{LW}}{N_{NLW}}$	23 / 0	0 / 113	23 / 113	23 / 113		16 / 64
Change in Absenteeism	0.95 (0.36)	0.99 (0.45)	-0.04 (0.07)	-0.16** (0.07)	-0.16** (0.08)	
$\frac{N_{LW}}{N_{NLW}}$	47 / 0	0 / 164	47 / 164	47 / 164	46 / 121	
Change in Training	1.00 (0.00)	1.09 (0.34)	-0.09** (0.03)	-0.05* (0.03)		Too little variation
$\frac{N_{LW}}{N_{NLW}}$	46 / 0	0 / 122	46 / 122	46 / 122		18 / 58
Change in Overtime	0.85 (0.36)	1.05 (0.47)	-0.20** (0.06)	-0.21** (0.07)	-0.25** (0.08)	
$\frac{N_{LW}}{N_{NLW}}$	48 / 0	0 / 169	48 / 169	48 / 169	47 / 124	

*Statistically significant at the 0.10 level; ** Statistically significant at the 0.05 level. The results in columns four and five control for Firm Size, For Profit, Independent Operation, Unionized, and the eight industry categories in Table 2. The results in column six replace the industry controls with occupation controls for the four occupation categories in Table 2.

APPENDIX TABLE 1
FULL REGRESSION RESULTS

Variable	Change in Wage	Change in Health Benefits	Change in Paid Days Off	Change in Turnover	Change in Absenteeism	Change in Training	Change in Overtime
LW Firm	1.74** (0.15)	0.02 (0.02)	1.66* (0.92)	-0.39** (0.14)	-0.16** (0.07)	-0.05* (0.03)	-0.21** (0.07)
Firm Size	-0.1E-3 (0.4E-3)	-0.13E-4 (0.23E-4)	0.002 (0.002)	-0.13E-2 (0.12E-2)	-0.68E-3** (0.27E-3)	0.75E-5 (0.95E-4)	-0.5E-3** (0.2E-3)
For Profit	-0.16 (0.20)	0.77E-4 (0.76E-2)	1.33** (0.61)	-0.52* (0.30)	-0.45** (0.13)	0.26** (0.12)	0.10 (0.11)
Independent Operation	-0.08 (0.15)	0.04 (0.03)	0.43 (0.52)	0.11 (0.20)	-0.11 (0.08)	0.11** (0.04)	-0.18** (0.09)
Unionized	-0.76 (0.47)	0.01 (0.02)	1.57 (2.99)	--	-0.08 (0.08)	-0.01 (0.04)	0.21 (0.17)
Landscaping	0.43 (0.33)	0.85E-3 (0.02)	-2.11 (2.01)	-0.55** (0.22)	-0.27** (0.13)	-0.01 (0.02)	0.86E-3 (0.14)
Janitorial	0.27 (0.35)	0.03 (0.02)	-2.97 (1.94)	-0.67** (0.27)	-0.42** (0.16)	-0.06 (0.06)	-0.11 (0.15)
Security	0.60 (0.37)	0.03 (0.02)	-1.96 (2.14)	-0.44 (0.31)	-0.37** (0.15)	0.18 (0.13)	0.09 (0.17)
Parking	0.58* (0.34)	0.03 (0.02)	-3.06 (1.98)	-0.50* (0.26)	-0.38** (0.14)	0.23 (0.21)	-0.06 (0.15)
Food Service	0.58* (0.31)	-0.04 (0.05)	-2.00 (2.25)	-0.58** (0.29)	-0.60** (0.17)	0.09 (0.07)	0.06 (0.20)
Social Services	0.23 (0.37)	0.03 (0.02)	-2.89 (1.97)	-1.25** (0.36)	-0.56** (0.16)	0.32** (0.12)	0.12 (0.15)
Retail	0.35 (0.34)	0.02 (0.02)	-2.68 (1.95)	-0.54* (0.32)	-0.49** (0.15)	0.02 (0.10)	-0.14 (0.19)
Constant	0.58 (0.42)	0.94 (0.05)	1.31 (2.16)	2.03** (0.43)	1.92** (0.22)	0.68** (0.14)	1.12** (0.20)
Adj. R^2	0.60	0.05	0.11	0.06	0.11	0.09	0.04
$\frac{N_{LW}}{N_{NLW}}$	47 / 111	45 / 136	39 / 98	24 / 59	47 / 164	46 / 122	48 / 169

*Statistically significant at the 0.10 level; ** Statistically significant at the 0.05 level