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The Dissipation of Minimum Wage Gains for Workers Through Labor-Labor Substitution

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Abstract

This paper utilizes an original employer-employee matched data set on city contract firms following the Los Angeles Living Wage Ordinance to explore the extent of labor-labor substitution resulting from a minimum wage. We are able to test for substitution on both observable and unobservable skills and to measure the extent to which such substitution dissipates the benefits of a wage minimum for workers. The results suggest substitution on observable skills such as education, age at time of hire, and prior formal training, and on unobservable skills as well. The latter is evidenced by the finding that the “before” wages of workers who are new to city contract work following the ordinance are significantly higher, conditional on observable skills, than the “before” wages of city contract workers who preceded the ordinance. We estimate that the intended wage gain for workers is dissipated by roughly 40% through labor-labor substitution.

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Economic theory suggests that firms will replace low-skill workers with high-skill workers if the two are substitutes in production and the wages of low-skill workers increase, as happens when a minimum wage is enacted. Empirically testing the labor-labor substitution hypothesis is of interest not just as a confirmation of economic theory, but also because it has implications for the wage gains workers receive from a wage minimum. If low-skill workers are replaced by high-skill workers following enactment of a wage minimum, the benefits of the regulation for workers may be dissipated as the wage gain for new hires is less than for members of the original workforce.

Empirical evidence on the impact of wage minimums suggests that the complete dissipation of wage rents through labor substitution is unlikely. It is now well established empirically that there exists a “spike” in the wage distribution around the relevant minimum wage, suggesting that low-skill workers’ wages are indeed affected by wage mandates.¹ Moreover, job queues exist for minimum wage jobs, suggesting that economic rents are earned by low-wage workers in these positions. And, finally, the empirical literature on the employment effects of wage minimums has found little substantive employment loss among affected occupations as the result of government wage mandates (Brown 1999; Card and Krueger 1995). This suggests that there is little labor-labor substitution across broad skill/occupational categories – e.g., adults for teenagers, workers with some college experience for high-school graduates, or backhoe operators for ditch diggers – as a result of minimum wages.

While the complete dissipation of wage gains through labor substitution across occupations seems ruled out by this evidence, the *partial* dissipation of rents from *within-occupation* substitution is not. Abowd and Kilingsworth (1981) have noted that labor-labor substitution may take place at a more disaggregate level, across more detailed

¹ Studies have also shown that this clustering of wages is not offset by variation in non-wage benefits such as employer provided health insurance, paid vacations, or bonuses. See, for example, Wessels (1980) and Alpert (1986), and Sicilian and Grossberg (1993).

worker skill sets but within broad occupational and skill categories. Neumark and Wascher (1996) find evidence that minimum wages lead firms to substitute away from younger and towards older, more experienced teenagers. Labor-labor substitution of this type may result from more minor changes in firms' skill demands, from employer efforts to dissipate worker rents by requiring greater work effort or less absenteeism from workers (and by seeking workers that better match these new employer goals), or from pure labor supply effects that are independent of firms' intentions to alter hiring standards. The latter may operate entirely through worker replacement following voluntary quits, and may be the unintended result of firms hiring from a more highly-skilled applicant pool which itself is brought on by the higher wage mandate.

Economists and policy makers are typically concerned with the issue of labor-labor substitution because of the negative employment effects it implies for workers who are the intended beneficiaries of minimum wage policies. Given evidence that job loss among low-skill workers is minimal following a wage mandate, some have become sanguine about the positive consequences of such policies. However, this evidence is entirely consistent with labor-labor substitution within skill/occupational categories of the sort described above, where low-skill workers are essentially being substituted for other low-skill workers. Under this scenario, while employment loss for low-skill workers might be minimal, the wage benefits for workers nonetheless may be significantly dissipated if higher-wage, low-skill workers replace lower-wage, low-skill workers following a mandated minimum wage.

This paper utilizes a unique employer-employee matched data set to explore the extent of labor-labor substitution and dissipation of wage gains for workers resulting from a significant wage mandate – a living wage ordinance – among city contract firms in Los Angeles. The data offer demographic and human capital characteristics of a sample of stayer and joiner workers at affected city contract establishments, which allows

us to shed light on the extent of substitution along observable skill dimensions. We also know the wages of these workers prior to the wage mandate, which allows us to measure the extent of substitution along the dimension of unobservable skills, assuming before wages proxy workers' overall skills set. Exploring unobservable skill differences for workers hired before and after the wage mandate is important since the substitution that interests us is perhaps less likely to occur across observed dimensions of skill such as schooling or experience.

The survey data also offer information on city contract establishments. In addition to numerous establishment characteristics, the survey asked employers about their intentions to change hiring standards in response to the living wage, thereby allowing us to separate out the demand-side (i.e., employer-initiated) component of the overall substitution response from the supply-side (i.e., changing applicant pool) component. Finally, estimates of the average wage increase for stayers and joiners are used to derive a measure of the overall benefit to affected city contract workers both with and without the observed substitution. This provides us with an estimate of the dissipation of wage benefits for workers due to labor-labor substitution.

The outline of the paper is as follows: We begin with a brief discussion of the Los Angeles Living Wage Ordinance. This is followed by a discussion of the data and empirical methodology. The results of the analysis of labor-labor substitution on observable and unobservable skills are then discussed. We conclude with an estimate of the wage gain for workers resulting from the living wage ordinance, accounting for the impact that labor-labor substitution has had on the dissipation of worker rents. The results suggest minor substitution on observable skills such as schooling, experience, or prior formal training, but rather significant substitution on unobservable skills, as evidenced by the finding that "before" wages of joiners are significantly higher than those of stayers.

The intended benefits to affected workers are thus dissipated rather significantly by labor-labor substitution.

The Los Angeles Living Wage Ordinance

The Los Angeles Living Wage Ordinance (LWO) was passed in 1997 by the Los Angeles City Council, and went into effect in May of that year. It was the tenth such ordinance to be passed in the country, in what has become a movement that currently encompasses over 120 localities. Living wage ordinances typically strive to increase the hourly wages of workers in city 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 give encouragement to employers to provide health benefits to workers, and some stipulate a minimum number of paid days off for workers per year. The Los Angeles ordinance is the third largest in the country (behind New York and San Francisco). Employers are covered in Los Angeles if they lease land from the city, including, most importantly, businesses at the Los Angeles and Ontario International Airports.

It is estimated that as many as 8,000 workers in 150 firms were directly affected by the wage provision of the ordinance (Fairris, Runsten, Briones, and Goodheart 2005).² While the Los Angeles ordinance is among the largest in the country, it directly affects perhaps two percent of the low-wage labor force in the area. Thus, one must be careful in generalizing from the results of this analysis about the impact of a wage mandate that covers a significant portion of the low-wage workforce. Landscape laborers, janitors, security guards, food service workers, and parking attendants are among the more prominent occupational groups affected by the ordinance.

² 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.

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.³ 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. Finally, unionized firms are exempt from the provisions of the ordinance.

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 incentive comes largely in the form of tax savings, as employers pay taxes on the wage component of labor costs but not on the value of the fringe benefits package. 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 \$10.03 per hour or \$8.78 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.⁴

Data

This paper utilizes original employer-employee matched data. The Survey of Los Angeles Living Wage Employers (SLWE) was the first stage in an intentional two-part

³ Henceforth, for ease of exposition, we refer to all as “city contractor firms.”

⁴ At the time of enactment, the stipulated living wage was 70% above the state minimum wage. Today, it stands at roughly 50% above the state minimum.

survey design that included a survey of affected workers at these very same establishments as a second stage. The survey response rate for the employer survey was 84 percent. Structured, in-person interviews of roughly 1-2 hours were initially conducted with 82 living wage employers at their offices, and then approval and assistance was solicited in contacting, sampling, and surveying their workers. The SLWE began in the fall of 2001 and was largely completed by the fall of 2002. Retrospective questions were asked about conditions prior to the onset of living wage coverage for each firm.

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
- Janitorial
- Landscaping
- Retail and food service
- Security and parking
- Social services
- Transit
- Miscellaneous

Each industrial group was then further stratified by firm size: large firms, with 50 or more workers, and small firms with less than 50 workers. Large firms were over-sampled for cost and clustering reasons. Weights were developed to render the results of statistical analyses representative of the population of living wage firms.

In the second stage of the project, 320 workers from 65 living wage establishments responded to surveys from the winter of 2002 to the summer of 2003. This represents a response rate of 81 percent. Worker surveys were typically administered away from the place of employment, and lasted from 45 minutes to 1.5 hours. The target population of

the Survey of Living Wage Workers (SLWW) was workers directly affected by the living wage ordinance – specifically, workers in jobs wherein the wages for their jobs were mandated to increase as a result of the ordinance. The sampling design in this case was intended to have two stages of stratification, with the establishments functioning as a cluster of workers. For this purpose, information on the population of affected workers was requested from the sampled establishments via lists that included name, occupation and hiring date. The first level of intended stratification was based on occupational groups. However, since not every establishment supplied complete information as requested, the stratification at this stage was possible in only 11 establishments.

The second level of intended stratification was by date of hire, in order to capture both workers who were at the firm before the living wage ordinance took effect (stayers) and workers who were hired afterwards (joiners). Because information on hiring date was not provided by all establishments, the stratification at this level was applied to only eight of the establishments that were first stratified by occupations. In five other establishments, the only stratification applied was the one on date of hire. No stratification was applied in the 49 establishments in which neither the occupation or hiring date of the workers was supplied. In these cases, workers in the sample were selected randomly. Sampling weights were developed for the worker survey to render statistical results representative of the population of workers affected by the living wage ordinance.⁵ The establishment and worker survey data were merged to form the matched employer-employee data set.

⁵ The weights take into consideration the stratification within establishments as well as the stratification between establishments. In other words, each worker's weight is the inverse of their probability of selection, which is the product of the probability of the establishment being selected among all establishments in the same industry-size stratum and the probability of the worker being selected among all workers in the stratum within the establishment. The whole establishment was a stratum for those cases where no stratification was applied.

One of the major challenges posed by our data in adequately testing the labor-labor substitution hypothesis is that we know nothing about the leavers – i.e., those workers who left city contract work between the time their firm became covered by the living wage ordinance and the time of survey. A proper test of the hypothesis would compare joiners with leavers rather than stayers. However, to the extent firms actively seek to alter the skills set of workers, it seems reasonable to assume that leavers would be drawn disproportionately from among the lower-skill workers and stayers from among the higher-skill workers, which leads to an underestimate of labor-labor substitution effects in our results. It is less clear what to expect of the skill set of workers who voluntarily quit following the ordinance. Those who quit due to bad matches are perhaps more likely to be lower paid and lower skilled, but voluntary exits might also be high among more highly-skilled workers because their outside wage offers are greater.

Lacking information on leavers also means that our analysis of the effects of the living wage ordinance is restricted to the workforce of the city contract sector only, and does not cover the Los Angeles low-wage labor market as a whole. The employment conditions of leavers following city contract work, and thus the extent of worker benefits dissipation beyond the city contract sector, are not known.

Thus far, nothing has been said about the entry of new firms into the city contract sector following the living wage ordinance. New firms did indeed enter city contract work following the ordinance and they are part of our sample, as are the workers in these firms. The labor-labor substitution hypothesis is perhaps most appropriately applied to firms that held a contract with the city prior to passage of the living wage ordinance, as it is in these firms where true “substitution” towards more high-skill labor may take place. However, new firms face the same mandated wage, and so similar incentives exist in these firms regarding the employment of high-skill workers. Labor-labor substitution may take place through labor turnover in older city contract firms, as high-skill labor is

substituted for low-skill labor, or through firm turnover, as firms utilizing high-skill labor are substituted for firms utilizing low-skill labor.

In order to estimate the total dissipation of wage benefits for workers in city contract work, an assessment of the observable and unobservable skills of workers in these new firms is necessary. However, because the dynamics of substitution are not the same for old and new firms, we analyze them separately in the analysis that follows. Our analysis thus contains two types of firms – “old” and “new” – and three types of workers – “stayers,” “joiners,” and “new joiners.” New joiners are not only new to city contract work, they are also overwhelmingly (more than 90%) new employees in the firms that employ them.

Table 1 lists the variables from the employer and worker surveys to be used in the analysis. Several variables warrant discussion. *Hiring standards change* is a dummy variable equaling one if the establishment changed hiring standards due to the LWO. The hiring standards referred to in this question are greater skills, experience, education, English language skills, or workers who are more responsible. This variable is used to parse out demand (i.e., employer intended) and supply (i.e., changing applicant pool) effects when substitution occurs.

Another 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. The variable *counterfactual wage* captures these responses. We can use this variable, in combination with an original control group sample of firms outside the city contract sector (see Fairris 2005), to test for nonrandom selection of new firms into city contract work following passage of the ordinance and whether old firms represent a nonrandom draw from the larger population of firms. Controlling for a host of establishment variables, we find no evidence of selection of either form with regard to firm wage policies. However, city contract employers are

significantly more likely to be unionized and to offer health benefits to workers than firms outside the city contract sector.

In the analysis to follow, we control for *occupation* using very detailed occupational categories. Among the more prominent occupations are child care provider, landscape laborer, janitor, security guard, parking attendant, cashier, and counselor for youth activities. The detailed occupations come from ten major occupational groups: childcare workers, cleaning service workers, landscape workers, parking service workers, restaurant workers, retail workers, social service workers, security workers, airline service workers, supervisors, and miscellaneous occupations. These categories follow closely the industry where the job is performed.

Table 2 gives the basic descriptive statistics of the establishments from the SLWE dataset. The total number of useable establishments in the dataset is 52, following deletion of firms with missing responses to pertinent information. The descriptive statistics are also broken out by firm type – old and new – in the table. There are major differences worth noting between old and new establishments in the sample. Old establishments are grouped in the social service (25%) and miscellaneous (30%) industries, whereas new establishments are highly concentrated in the retail and food (52%) industry. The average number of employees per establishment is 388 in the old establishments and 119 in the new establishments. (This difference is not explained by differences in industry mix across the two establishment types.) Only 4% of the new establishments in the sample were unionized when interviews were conducted. In contrast, 19% of the old establishments have workers who are represented by unions. Also, new establishments are much more likely to be located at one of the two airports in the sample.

Table 3 gives the descriptive statistics for variables from the SLWW dataset. The total number of useable observations in the data is 264, after deletion of workers with

missing responses to pertinent information. The descriptive statistics are also presented by worker type: stayers, joiners, and new joiners. There are a number of interesting differences across the worker groups. While it is perhaps not very surprising that stayers are currently older than both groups of joiners, it is interesting that there are differences in the age and experience of workers at the time of hire. Stayers were hired at an average age of 37, whereas joiners were 35 and new joiners were 32 when first hired. Relatedly, stayers were hired with an average experience of 17 years, whereas joiners and new joiners had 16 and 12 years of experience, respectively, when first hired. The average education level is very similar across the three worker groups (completion of high school), but the proportion of joiners who have completed a formal (technical skills) training program is much higher – 18% – than that of stayers (12%) or new joiners (11%). Particularly interesting are the different percentages of females in the three worker groups. The percentage of females is lower for joiners as compared to stayers, but much higher for new joiners as compared to stayers.

One of the characteristics that distinguishes workers in the living wage sector from a sample of similar workers from the more general population of firms is the high degree of unionization (Fairris 2005). This fact is suggested in the numbers showing significantly less unionization among both joiners and new joiners prior to joining the city contract sector. Interestingly, however, union density among stayers was also fairly low prior to coverage by the living wage ordinance. Following passage of the ordinance several very large employers underwent successful union organizing drives. Thus, it is not that a large number of living wage establishments are unionized, but rather that the larger establishments are unionized. Unionized establishments possess 67% of the overall workforce of sampled establishments.

The occupational mix is similar across the three types of workers with a few important exceptions. New joiners are disproportionately in the janitorial (22%) and retail

and sales (25%) occupations. Only 16% of the stayers and 9% of joiners work as janitorial services workers, while 1% of the stayers and none of the joiners are in the retail and sales occupations. Another contrast occurs in the airport services occupation, where joiners (32%) and stayers (39%) have a significant representation, but where new joiners have none.

A final important distinction between the groups occurs in regards to health insurance provision by the employer. Workers in new firms are less likely to obtain health insurance through the employer than are workers – stayers or joiners – in old firms. The percentage of new joiners obtaining employer-provided health benefits is only 24%, whereas almost the double the percentage of stayers and joiners receive such benefits.

For the 264 workers in the sample, we possess wage histories dating back, in some cases, to the previous five jobs. From this information, we can retrieve the hourly wage paid in the job held immediately before the living wage, the most recent date on which the worker was paid this wage, and a host of variables associated with the job, including, for example, whether it was unionized and whether it offered employer-provided health benefits. Thus, it is possible to identify for each worker the wage earned immediately before they became covered by the LWO. For both sets of joiners, this “before” information applies to previous employers, whereas for stayers it represents information at their current living wage employer prior to the onset of the living wage. In combination with “after” wage observations, it is possible to calculate the raise experienced by each worker. In the case of stayers, the after wage observation occurs when a worker’s firm became subject to the provisions of the LWO, whereas for joiners and new joiners it occurs when these workers joined the city contract sector.

In addition, we are able to build a longer panel of hourly wage observations for 254 workers in the sample, as more than half of the sampled workers offered three or more historical wage observations and ten percent offered five or more distinct wage

observations. We use this information in a robustness check on the analysis of labor-labor substitution on unobserved skills. Immediately before wage observations are typically farther back in time for stayers than they are for joiners or new joiners, and so, even with time controls, we may be concerned with the integrity of the findings on immediately before wages. Utilizing the longer panel of wage histories allows us to create greater uniformity across these worker categories in the timing of before wage observations.⁶

Finally, the employer and worker survey results are matched, making it possible to explore labor-labor substitution tendencies within establishments. On average, there are five worker observations for each establishment in the sample. Focusing on the old living wage establishments only – those involved in city contract work prior to the onset of the ordinance – the working sample is 231 workers in the observable skills analysis and 190 workers in the unobservable skills analysis. When new firms are added to the mix, the sample sizes grow to 264 and 220, respectively.

Empirical Methodology

We evaluate the effects of the living wage ordinance on both the observable and unobservable skills of new hires. We begin by comparing the observable, skill-related characteristics of joiners and stayers in establishments having engaged in city contract work prior to passage of the ordinance – i.e., old firms. Next, the observable characteristics of workers in establishments that are new to city contract work are introduced into the analysis. We begin with a comparison of skill-related characteristics

⁶ We are able to accurately attribute only a limited number of worker and firm characteristics to these historical wage observations. The age, sex, and race and ethnicity of the worker at the time of the survey may be imputed without error. However, the timing of acquired schooling is not available in the survey, and so the current level of worker education is attached to each historical wage observation. The union status and health benefits provision variables are assigned to previous jobs based on responses concerning the job immediately preceding coverage by the living wage ordinance.

of old and new workers, but then proceed to break out differences across stayers, joiners, and new joiners.

A comparison of workers based exclusively on observable skills may produce an incomplete picture of labor-labor substitution. There remains the issue of skill-related characteristics that are unobservable to the researcher but yet observable to participants in the hiring decision. To explore differences in unobservable skills, we rely on a comparison of wages received prior to the living wage ordinance. This exercise assumes that differences in conditional before wages capture unobserved heterogeneity in workers' skill sets. For this analysis as well, we begin with a comparison of stayers and joiners in old city contract firms, and follow this with a comparison of workers in old and new firms.

Observable Skills Substitution

The first phase consists in the estimation of differences in the conditional mean of observable characteristics between joiners and stayers. The focus in this analysis is on *experience at hire, hire age, years of schooling, currently enrolled, native English speaker* and *prior formal training*. We focus, here, on a comparison of skills at time of hire, and capture the positive returns to tenure for stayers in the analysis of unobservable skill differences. In addition, we explore substitution towards a more male-dominated workforce by including *female* in this analysis as well.⁷ Employers dissipate minimum wage gains for workers when they replace workers that are discriminated against in the labor market with those that are not.

⁷ We pursued this analysis only after observing preliminary results suggesting that joiners are statistically more likely to be male as compared to stayers. Further analysis revealed no statistically significant difference across the stayer/joiner populations based on either race or ethnicity, except that joiners are far more likely than stayers to be Latino. We ascribe the latter finding to demographic changes in the southern California labor force, due largely to the increased immigration of low-skilled workers from Latin America in recent years.

The empirical approach is to perform a series of OLS regressions, the general specification of which is:

$$(1) \quad y_i = \alpha + \gamma_J \text{joiner}_i + \beta_1' X_i + \beta_2' Z_{k[i]} + \varepsilon_{ik},$$

where y_i is the observable characteristic of worker i and joiner_i is a dummy variable indicating whether worker i is a joiner. For notational purposes, the control variables are divided into two groups – those that are attributable to individuals, X_i , and those that are employer based, Z_k . The error term is indexed by both i and k , suggesting possible interdependence between individuals working for the same establishment. All regressions are estimated utilizing a correction for this possible clustering effect. The indicator of labor-labor substitution is given by the coefficient γ_J , which reflects the mean conditional difference in observable characteristics between joiners and stayers.

To compare the characteristics of new joiners with those of workers (stayers and joiners combined) in established city contract firms, the specification of the regression is:

$$(2) \quad y_i = \alpha + \gamma_N \text{old establishment}_i + \beta_1' X_i + \beta_2' Z_{k[i]} + \varepsilon_{ik}.$$

The coefficient γ_N measures the difference in the conditional mean of the relevant observable characteristic between old establishment workers and new joiners. For the three-way comparison between stayers, joiners, and new joiners, the specification is:

$$(3) \quad y_i = \alpha + \gamma_S \text{stayer}_i + \gamma_J \text{joiner}_i + \beta_1' X_i + \beta_2' Z_{k[i]} + \varepsilon_{ik}$$

The coefficients γ_S and γ_J measure the difference between stayers and new joiners, and joiners and new joiners, respectively.⁸

A set of categorical variables indicating the occupation of workers is included in all regressions to insure that the comparison of observable characteristics is restricted to variation within occupations. A host of worker demographic characteristics also appear as

⁸ To avoid cumbersome notation, the subscript J is always used to identify coefficients that measure differences in conditional means between the workers in the base (stayers or new joiners depending on the regression) and joiners. The specific base that is used is clearly indicated in the text to avoid confusion.

independent variables, including race and ethnicity categories, *part-time job*, and *health insurance*, in addition to the other observable, skill-related and gender characteristics of workers. The following employer characteristics are also included in the set of control variables: *union status*, *number of employees*, *affected worker ratio*, *airport* and *subsidiary*.

The basic regression results provide a picture of labor-labor substitution taking place within occupational categories and across industries and firms. However, we can obtain a more detailed picture of within-firm substitution by invoking firm fixed effects, which is made possible by the use of worker-firm matched data. To avoid collinearity in this case, the employer characteristics particular to each establishment are excluded. In this case, the coefficient γ_j gives us the conditional mean difference in observable characteristics between joiners and stayers within establishments.

For the comparison between new joiners and workers in old establishments, a different strategy is adopted. Firm fixed effects are not appropriate in this context since new firms have neither stayers nor joiners and old firms do not have new joiners. The contrast between workers in this case is done across establishments but within industry by adding a set of *industry* categorical variables to the right-hand side of the regression specification.

Unobservable Skills Substitution

As previously mentioned, “before” wage comparisons are used to uncover differences in skills between joiners and stayers that are unobservable to the researcher. Human capital theory suggests that, all else constant, a higher wage is indicative of greater skills. However, there is ample empirical evidence of unexplained wage differences across similarly skilled workers, presumably owing to differences in establishment human resource policies (Groschen 1991; Gibbons and Katz 1992; Abowd, Kramarz, and

Margolis 1999). In the end, we are unable to clearly distinguish wage differences that reflect unobservable skill differences from wage differences due to unobserved establishment pay policies. (Recall, however, that we are able to control for both union status and employer-provided health benefits on previous jobs.) This is an important matter for future research. Note that this inability in no way compromises our estimate of the dissipation of minimum wage gains for workers through worker replacement.

To detect differences in unobservable skills between stayers and joiners we utilize a wage regression of the following form:

$$(4) \quad w_{it} = \beta_0 + \gamma_j \text{joiner}_i + \beta_1' X_{it} + \beta_2' Z_{k[i]} + \delta_i m_{it} + \varepsilon_{ik},$$

where w_{it} is the log “before” wage of individual i at time t , X_{it} is a vector of worker characteristics, and $Z_{k[i]}$ is a vector of establishment-based control variables. The disturbance term has similar properties as the disturbance term in (1) above. The index t is used here with more liberty than is customary in the literature. It indicates that the observations are dated – i.e., not all wages are observed in the same year – but it does not imply that there is a time series component to the analysis. The difference in the conditional mean wages of joiners and stayers – our measure of unobserved skill differences – is captured by the coefficient γ_j . For a within-establishment measure of substitution on unobservable skills, establishment fixed effects are added to the regression, thereby converting the coefficient γ_j into the before wage difference between stayers and joiners within establishments. This is the most meaningful test of labor-labor substitution based on unobservable skills.

To eliminate the effect of observable skills and other individual characteristics on wages, we control for *experience at hire*, *hire age*, *female*, *years of schooling*, *native English speaker*, and race and ethnicity variables. To remove important differences in establishment characteristics, controls are added for *health benefits* and *union* as well.

The city contract sector is significantly more likely to offer employer-paid health benefits and to be unionized.

Differences in the timing of wage observations pose a challenge. Not only are immediately before wages observed at different points in time, but these time periods are not uniform across stayers and joiners. The before wages of stayers accord with the period just before their city contract establishment became subject to the LWO. Firms become subject to the ordinance once their contract with the city is renewed. While the LWO took effect in 1997, a majority of firms were not affected by the ordinance until after 1999. The before wage observations of joiners, however, typically come from the period immediately prior to the worker joining the living wage sector – that is, after (and sometimes long after) the city contract establishment was affected by the ordinance. Thus, the stayers tend to be overrepresented in the early years and joiners in the later years.

Controlling for time in this analysis is especially important because most individuals in the sample are low-wage workers and the California minimum wage increased several times over this period. Any observed difference in wages between stayers and joiners might be an artifact of the different minimum wage regimes affecting these workers. We address this challenge by including a set of categorical variables – m_{it} – indicating minimum wage periods. An indicator variable in this set equals one if the wage observation corresponds to minimum wage period l and equals zero otherwise. This is equivalent to re-dating the wages using a calendar based on minimum wage regimes instead of regular years.

New establishments are introduced at a second stage of the analysis. The new/old establishment comparison follows from a regression of the following form:

$$(5) \quad w_{it} = \beta_0 + \gamma_o \text{old establishment}_i + \beta_1' X_{it} + \beta_2' Z_{k[i]} + \delta_l m_{it} + \varepsilon_{i[k]}.$$

Because establishment fixed effects are inappropriate in this specification, to obtain a more refined measure of unobservable differences across new and old establishment workforces, industry categorical variables are used instead.

The comparison between joiners, stayers and new joiners is achieved using the following regression:

$$(6) \quad w_{it} = \beta_0 + \gamma_J \text{joiner}_i + \gamma_S \text{stayer}_i + \beta'_1 X_{it} + \beta'_2 Z_{k[i]t} + \delta_t m_{it} + \varepsilon_{ik}$$

Here, there are two wage comparisons to consider: joiner relative to new joiner, γ_J , and stayer relative to new joiner, γ_S .

Wage Gains Estimation and Measure of Dissipation

To estimate the wage gains for workers from the LWO, we use an approach that is similar to the methodology used in the unobservables analysis above, but with a few changes in specification and data.⁹ Our strategy consists in estimating:

$$(7) \quad w_{it} = \alpha + \gamma_{JB} \text{joiner}_i 1\{t_w \leq t^*\} + \gamma_{JA} \text{joiner}_i 1\{t_w > t^*\} + \gamma_{NB} \text{newjoiner}_i 1\{t_w \leq t^*\} \\ + \gamma_{NA} \text{newjoiner}_i 1\{t_w > t^*\} + \phi_S 1\{t_w > t^*\} + \beta'_1 X_{it} + \beta'_2 Z_{k[i]t} + \varepsilon_{ij},$$

where w_{it} is the immediately *before wage* and *after wage* observations, t_w is the time in month and year when the wage is reported, t^* is the time in month and year when the worker became affected by the LWO, and $1\{\cdot\}$ denotes an indicator function.

The advantage of this specification is that the calculation of the wage gain for each group of workers is easily obtained from the coefficients of the indicator functions. For example, an estimate of the wage gain for stayers attributable to the LWO is:

$$E[w_{it} | t > t^*, \text{joiner} = 0, \text{newjoiner} = 0, W_i] - E[w_{it} | t < t^*, \text{joiner} = 0, \text{newjoiner} = 0, W_i] \\ = \phi_S,$$

where W_i is the vector $[X_{it}, Z_{k[i]t}]$. Similarly, the wage gain for joiners can be estimated as:

$$E[w_{it} | t > t^*, joiner = 1, newjoiner = 0, W_i] - E[w_{it} | t < t^*, joiner = 1, newjoiner = 0, W_i] \\ = \gamma_{JA} + \phi_S - \gamma_{JB}$$

and for new joiners as:

$$E[w_{it} | t > t^*, joiner = 0, newjoiner = 1, W_i] - E[w_{it} | t < t^*, joiner = 0, newjoiner = 1, W_i] \\ = \gamma_{NA} + \phi_S - \gamma_{NB}.$$

We know the city contract sector stands out among similar employers in the region by possessing a higher union density and greater provision of employer-paid health benefits, both of which could be factors in the estimated wage gains of joiners and new joiners especially. Some of the estimated difference in wage gains for workers might also be related to differences in the length of elapsed tenure between the before and after period of recorded wages. Finally, we want to make wage gain comparisons across worker groups both within occupations and within similar establishments. Thus, to render the wage gains comparable, we control for union status and *health benefits* both before and after, and, in addition, *tenure change*, *occupation*, and three establishment characteristics – *number of employees*, *affected worker ratio*, and *subsidiary*.

Another source of the estimated difference in wage gains might be related to the fact that joiners and new joiners come under provision of the LWO later than stayers. One way to free the wage gain estimate of this temporal effect is to control for *minimum wage* period, just as we did for the unobservables analysis. However, that approach is not appropriate in this case because simple minimum wage controls would also condition for annual increases in the living wage instead of relegating these increases to the coefficient ϕ_S . Thus, instead, we use a set of interactives of the *minimum wage* variables with the indicator functions – $1\{.\}$ – that identify wages before and after.

⁹ The LWO benefited workers primarily through wage gains as opposed to nonmonetary benefits such as additional health insurance. The only additional established benefit for workers is two additional paid days off (Fairris 2005).

To estimate the dissipation of wage gains, we simply total the wage gains for all three categories of workers – stayers, joiners, and new joiners – resulting from the LWO and compare this total to a counterfactual wage gain which assumes that no labor substitution had taken place. The counterfactual wage gain is derived by attributing to every affected worker the wage gain received by stayers. We ignore the dissipation of wage gains resulting from employment loss in this calculation. Survey estimates put this loss of employment at less than 1 percent (see Fairris et al., 2005).

Results

Observable Skills Substitution

We begin with an analysis of differences in observable, skill- and gender-related characteristics between stayers and joiners in old establishments. All, with the exception of *female*, may be plausibly related to differences in skills acquired through labor market experience, schooling, and formal training programs, or due to the possession of English language abilities. *Female* may capture important differences in workers' choice of working conditions or labor force attachment; it may also reflect the increased opportunity for labor market discrimination that minimum wage mandates confer on firms.

The results are reported in Table 4.¹⁰ The two characteristics that stand out in the column one results, which do not control for firm fixed effects, are formal training and gender. Joiners are more likely to have had prior formal training and to be male. The likelihood of having received prior formal training is eight percentage points higher for joiners; this is a considerable magnitude given that only 10% of stayers report having

¹⁰ The full set of results for select regressions is reported in Appendix Table 1.

received prior formal training. While 56% of the stayers are female, only 43% of joiners with similar characteristics are likely to be female.

Although we have controlled for a host of worker and establishment characteristics in generating these findings, stayers and joiners are compared in this analysis across firms and industries. The results may therefore reflect differences in job growth and hiring across firms rather than the effect of the wage mandate. In column two of Table 4, we exploit the worker-firm matched aspect of the data to test whether these results are robust to controls for firm fixed effects. These results represent the within-firm conditional differences in mean hiring characteristics, and are the results most consistent with the labor-labor substitution hypothesis. The estimated differences regarding formal training and gender are robust to the addition of firm fixed effects. Indeed, the statistical significance of the formal training result is substantively enhanced by the introduction of firm controls. None of the other skill-related characteristics become statistically different across stayers and joiners with the introduction of firm fixed effects.

Are these results suggestive of labor-labor substitution? There is clearly a competing interpretation of the finding on gender differences – namely, discrimination in hiring practices made possible by the wage minimum. However, an alternative interpretation can be given to the formal training result as well. Theory suggests that a minimum wage may reduce job training if, given the wage constraint, low-wage workers are unable to accept a temporarily lower training wage in order to pay for the acquisition of skills on the job. The formal training result may merely indicate that firms are now seeking workers that already possess the skills that can no longer be imparted through in-house training programs. More skilled workers are not being utilized; it's just that the training of workers no longer takes place within firms. Joiners pay for and receive elsewhere the general human capital skills that stayers paid for and received within firms.

Information is available for shedding some light on this alternative explanation. The worker survey contains a question regarding the existence of job training **after** being hired (*received training*). Regression results reveal no statistical difference in the likelihood of initial training for stayers and joiners, controlling for an array of worker characteristics and firm fixed effects. Moreover, adding this training variable to the column two specification of the prior formal training regression does not alter the substantive finding that joiners are significantly more likely than stayers to have received formal training prior to taking their jobs. Although we know nothing about differences in the *amount* of job training workers receive after being hired, these results offers at least suggestive evidence that joiners are indeed more highly trained than stayers.

In the theoretical formulation of the labor-labor substitution hypothesis, both demand- and supply-side factors play a role. The minimum wage mandate gives firms an incentive to demand more high-skill labor at the same time that it fosters an increased supply of high-skill applicants for minimum wage jobs. Given supply-side effects, it is possible for labor-labor substitution to occur even without firms consciously changing hiring standards; a mere random draw from a higher-skill applicant pool will result in an increased use of high-skill labor.¹¹

We are able to explore this issue in greater depth with the worker-firm matched data because the establishment survey asked firms about their intentions to change hiring standards as a result of the living wage. This variable records the intention of firms to seek more skilled workers, measured along a variety of dimensions including greater schooling, experience, and level of responsibility. In column three of Table 4, we present the results of specifications in which the firm-level *hiring standards change* variable is interacted with the joiner variable in the observable skills regressions.

¹¹ Holzer, Katz, and Krueger (1991) offer evidence that the applicant pool increases with a wage minimum. There is no research of which we are aware that explores the skills of new applicants.

The estimated coefficient on the joiner variable captures the extent to which the supply-side effect is driving the earlier results, and the estimated coefficient on the joiner/hiring standards change interaction term picks up the demand-side effect.¹² The results suggest that it is the supply-side effect – or changing applicant pool – that accounts for the earlier findings. The difference between stayers and joiners in gender composition is statistically unrelated to firms' decisions to change hiring standards. And compared to stayers, joiners are more likely to have had prior formal training in firms where hiring standards **did not** change; where hiring standards changed, firms were less likely to hire new workers with prior formal training.

While we are hesitant to reach strong conclusions from a subjective response variable regarding establishment intentions and the simple interactive specification we have employed to parse out demand and supply effects, the results are at least suggestive. Joiners possess more prior formal training and are more male because of changes in the applicant pool, or, in the case of the gender result, because of the non-productivity related criteria used by employers when they draw from that pool. To the extent living wage firms benefit because their joiners possess greater skills, this appears to have been a consequence of the wage mandate that was unintended by firms. Older establishments hire more men following the LWO either because discrimination is less costly with a minimum wage or, absent discriminatory preferences, because more men apply for city contract jobs now that these jobs pay a living wage, and a random draw from a more male dominated applicant pool yields more men.

We now turn to an analysis of the new firms that entered city contract work following passage of the living wage ordinance. Because, as noted above, the workforces of these new firms have been built largely from scratch, they may possess even greater flexibility

¹² The coefficient on the stand-alone hiring standards change variable measures the difference in observable skills between stayers in firms that intended to change hiring standards and stayers in firms that did not.

in hiring high-skill labor than older city contract employers. Thus, we begin this analysis with the following question: Are the observable characteristics of workers in new firms significantly different from those of workers in old city contract firms? More interesting though, assuming that stayers represent the workforces of older city contract firms prior to the living wage ordinance, is the following question: Are the observable characteristics of new-firm workforces markedly different from those of stayers, and perhaps even joiners, in old city contract firms?

Table 5 gives the regression results for the seven observable characteristics of interest in a comparison of both old and new workforces, and then stayers, joiners, and new joiners separately. We focus on the column two and four results which control for industry.¹³ The column two results suggest that new-firm workforces are less experienced and yet older at the time of hire, more female, and possess more years of education than the workforces of old city contract firms. It is difficult to know how to trade off almost two years less experience for an additional year and a third of education and the additional maturity of judgment attributable to being almost four years older at the time of hire, but the combination is at least suggestive of a more highly-skilled workforce in new firms as compared to old.

The column four results reveal that new joiners possess these attributes in almost equal measure relative to both stayers and joiners in old firms. While the workforces of new firms possess more prior formal training than stayers, and slightly less than joiners, neither of these differences is statistically significant. Thus, new firms tend to select on a different set of observable skills as compared to old firms. The priority given to hiring females following passage of the ordinance is also different from the previous findings.

¹³ Controlling for firm fixed effects is inappropriate in this case since the indicator variable – old/new – is itself firm (as opposed to worker) based.

One concern we might have with these results is that, although industry is controlled for in the regression specifications, we know from Table 3 that the mix of industries is substantively different in new versus old firms. Most importantly, there are no new firms in either the miscellaneous or airline service industry categories. If old firms in these industries are removed from consideration, the results change in two respects: New-firm workers are no longer statistically significantly less experienced at the time of hire, but they now possess statistically significantly more prior formal training than old-firm workers.¹⁴ If these results are to be privileged, we conclude that there is evidence of selection towards higher-skill workers in new firms. New-firm workers are older at the time of hire, and possess both more education and more prior formal training than old-firm workers.

Unobservable Skills Substitution

Worker characteristics that are unobservable to the researcher and yet observable to firms may also form the basis for labor-labor substitution. Indeed, even if unobservable to firms, workers with higher unobservable skills will be drawn to the applicant pool of living wage firms following the mandated wage increase and perhaps take part in a process of substitution that is unintended by firms. Strength, mental agility, diligent work habits, intensity of labor effort, and being at ease with customers are among the many worker characteristics that are unobserved in the data we possess. Do firms substitute, intentionally or otherwise, towards workers with higher unobserved skills as a result of a government-imposed wage minimum?

We tackle this question by comparing the “before” wages of stayers and joiners, holding constant the other, presumably observable worker characteristics from above. For

¹⁴ New joiners are statistically significantly more likely to have had prior formal training than stayers, but not more so than joiners.

stayers, the before wage refers to the wage these workers received in the city contract sector immediately before the living wage ordinance took effect, whereas for joiners, it is the wage workers received in the job immediately preceding their joining the city contract sector. Worker characteristics such as whether the worker was a union member or possessed employer-provided health benefits refer to the before wage observation. The occupational controls refer to the present job in the city contract sector.

Table 6 gives the results of this analysis for old city contract firms. In column one, we see that the before wages of joiners are roughly 14% above those of stayers, all else constant. Interestingly, the only worker characteristics that are statistically significant in this regression are hire age, union status, and gender: workers receive a 0.69% premium for every year of age upon hire, union members receive roughly 11% more in wages compared to nonunion workers, and women receive roughly 6% less than men. There is no significant compensating differential for the absence of employer-provided health benefits, and no significant return to either schooling or experience. Little or no return to human capital characteristics is a common feature of secondary labor market jobs.

In column two, we control for firm fixed effects, and thereby explore the extent of within-firm labor-labor substitution on unobservable skills. These results offer the clearest indication of labor-labor substitution at the firm level since the column one findings may be due to a variety of across-firm features such as disproportionate hiring by “high-wage firms.” The column two finding suggests that within-firm substitution is, if anything, stronger than indicated by the earlier result. Joiners possessed wages before joining the city contract sector that were 15% higher than the before wages of stayers.

The finding that joiners’ before wages are significantly higher than those of stayers is all the more striking given that, absent the living wage, we might expect exactly the reverse. Andersson, Holzer, and Lane (2005) find that earnings growth is greater for job changers than for job stayers in low-wage labor markets. Thus, for workers who are

similar both at time of hire and in after wages, as is the case here, we would expect the before wages of joiners to be **less than** the before wages of stayers in the absence of the LWO.

Is labor-labor substitution on unobservable skills greater for firms that changed hiring standards as a result of the LWO? Recall that we found no evidence of greater substitution on observable skills for these firms as compared to firms that did not change hiring standards in the analysis above. Indeed, quite the opposite was the case. The interactive results of column three, Table 6 offer similar conclusions for unobservable skills substitution. Firms that claim to have changed hiring standards do engage in positive selection on unobservable skills, but to a statistically significantly lesser extent than firms that report no change in hiring standards. Once again, then, the evidence suggests that labor-labor substitution emerges in the presence of rather passive substitution intentions on the part of firms, and is therefore largely due to high-wage workers seeking jobs in the now high-wage city contract sector.

Labor-labor substitution on unobservable skills may also take place through the entry of new establishments into the city contract sector following passage of the ordinance. The first two columns of Table 7 give results comparing the before wages of workers in new firms with those of workers in old firms. The results are similar: old establishment workers possess before wages that are marginally less than those of new establishment workers, but the differences are not statistically significant. When the results are broken out – in columns three and four – by stayers, joiners, and new joiners, the nature of the earlier findings becomes clear. Stayers in old firms possess before wages that are 11% below those of new joiners in new firms, whereas the before wages of joiners and new joiners are not statistically significantly different. Selection on unobservable skills is similar, at least in value, for joiners in old firms as for new joiners in new firms.

One concern we have with the unobservables analysis is that, on average, the before wages of stayers are observed at an earlier point in time than the before wages of joiners or new joiners, thereby giving rise to a possible positive bias in the before wage comparisons. Differences in the timing of wage observations have been accounted for through the introduction of minimum wage categorical variables, but these may not be entirely satisfactory. This concern is addressed in a number of ways. First, we trim the sample to include stayers, joiners, and new joiners within a narrower range of before wage observations – 1996-2001. Second, we exploit the longer panel of wage data to compare not immediately before wages but before wage profiles, which contain many before wage observations distributed more evenly over time for stayers, joiners, and new joiners.¹⁵ In neither approach is the basic statistical significance of earlier findings substantively altered. Moreover, regarding quantitative impact, the most extreme change occurs in the trimming exercise for the stayer/joiner comparison, where the estimated coefficient falls from 0.15 to only 0.12.

A second concern with this analysis, as with the analysis of observables, regards the absence of information on leavers. If stayers are more skilled than leavers, our results comparing joiners with stayers are an underestimate of the extent of labor-labor substitution due to the LWO. However, if stayers are less skilled, our results cannot be taken as convincing evidence of such substitution. Even in the absence of information on leavers, it may be possible to shed some light on this matter by asking where, in the intra-firm, intra-occupational wage distribution, joiners enter. If joiners fill the positions that leavers vacate, then their after wages, relative to others in the same firm and same occupation, will reflect the position of leavers in the before wage distribution.

¹⁵ This procedure brings with it some risk of bias due to measurement error because worker characteristics are not accurately recorded for all before wage observations.

In order for this exercise to be valid, stayers must not have been promoted into the jobs of leavers, and, despite the enactment of a wage minimum, there must exist significant intra-firm, intra-occupational wage dispersion. We address the first condition by interacting the joiner variable in the after wage analysis with the percentage of vacant jobs filled from within – a question asked in the establishment survey. Regarding the second condition, an analysis of variance exercise reveals that 25% of the variation in after log wages remains even after controlling for occupation and firm fixed effects. When the joiner variable is added to this basic regression specification, along with its interaction with the propensity towards internal promotion and other worker and establishment control variables, we find no evidence that joiners earn higher after wages than stayers under any circumstances, regardless of the propensity towards internal promotion.¹⁶ This result is robust to a number of changes in specifications and samples, including an after wage regression in which each establishment is constrained to have at least two stayers and two joiners in the same occupation (wherein the sample size drops to 113 workers). These results offer at least suggestive evidence that leavers did not exit from the upper tail of the intra-firm, intra-occupational wage distribution of old firms.

Wage Gain

We have found evidence to suggest that labor-labor substitution takes place on both observable and unobservable worker skills among city contract firms as a result of the living wage ordinance. What does this imply about the dissipation of worker wage gains from the ordinance? Table 8 reports the conditional mean raise experienced by the three groups of workers as estimated from the wage gain analysis. The raise is measured as the difference in wages immediately before and after the LWO affected each worker.

¹⁶ The control variables include all of those utilized in the before wage analysis above, except that current age and current experience are substituted for age at hire and experience at hire, as well as a stand alone variable capturing internal promotion intensity.

Controls are invoked for a host of worker and establishment characteristics, excluding observable skill-related variables and gender. Most importantly, because we wish to estimate the impact of the ordinance on workers' wages, the effects of changing union status and employer-paid health benefits are netted out of the estimated wage increases.¹⁷ Other variables control for the time span between before and after wage observations (*tenure change*), the timing of the raise (*MW periods*), the establishment type (*number of employees, affected worker ratio, and subsidiary*), and the occupations and industries in which workers reside.

The findings reveal that the wage gain resulting from the LWO is greater for stayers than for joiners, but only marginally greater for stayers as compared to new joiners. The small wage gain for joiners is influenced, in part, by a select few joiners who possessed very high before wages, and who experienced an absolute pay cut in joining the city contract sector. These workers may have been suffering from recent job displacement and may have found superior employment alternatives outside the city contract sector soon after the survey period, or they may have joined the city contract sector to escape bad working conditions in their previous jobs.

The large wage gain for new joiners may seem surprising given earlier results suggesting that their before wages, conditional on worker characteristics, resemble those of joiners, and further analysis revealing that their conditional after wages also resemble those of joiners. However, recall that the wage gain analysis does not condition on human capital and gender characteristics. The somewhat lower average before wage of new joiners relative to joiners stems from the fact that new joiners are more female, while the

¹⁷ We also capture, and net out, the wage impact of changing union status in the city contract sector for those stayers who were part of successful union organizing drives during this period. One could argue that any wage increases owing to unionization for these workers were part of the benefits of the LWO, since the ordinance contributed, at least in part, to the success of the organizing drives. In this sense, we have underestimated the wage increase for stayers in this analysis.

somewhat higher average after wage is explained by the fact that new joiners are older at the time of hire (see Table 5). Both female and hire age are important determinants of wages in this low-wage labor market.

Finally, we utilize these results to estimate the wage gain for workers resulting from the LWO, both with and without labor-labor substitution.¹⁸ The intended benefits of the ordinance, assuming leavers resemble stayers, can be estimated by applying the average wage increase for stayers to the entire post-LWO affected workforce in city contract establishments. This number may then be compared to the actual average wage increase with labor-labor substitution: the (population-weighted) sum of the average wage increases for the three groups. Using this method, our estimate is that living wage gains for workers were dissipated by 41 percent through the impact of labor-labor substitution.

Conclusion

This paper utilizes an original employer-employee matched data set on city contract establishments following the Los Angeles Living Wage Ordinance to explore the extent of labor-labor substitution resulting from a wage mandate. We test for substitution on both observable and unobservable skills and measure the extent to which such substitution dissipates the benefits of a wage minimum for workers. The results suggest selection on observable skills such as years of schooling, prior formal training, and age at time of hire. We also find evidence of significant substitution towards workers with greater unobservable skills, as evidenced by the finding that the “before” wages of workers new to city contract work following the ordinance are significantly higher the “before” wages of city contract workers preceding the ordinance. The intended wage

¹⁸ Here, we ignore raises for workers in city contract firms whose before wages were higher than the living wage mandate. However, research reveals that this vertical spillover is not insignificant (Fairris et al. 2005). In fact, in addition to the roughly 8,000 workers whose wages were directly affected by the ordinance, another 2,000 received wage increases from employers in order to maintain relative wage norms.

gains for workers are thus dissipated significantly. We estimate that living wage gains for workers were dissipated by roughly 40 percent through labor-labor substitution.

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TABLE 1 VARIABLE DEFINITIONS

EMPLOYER SURVEY	
Old Establishment	= 1 if establishment had a contract or lease with city before becoming subject to the LWO; 0 otherwise
Number of Employees	Number of employees at the establishment
Affected Worker Ratio	Ratio of affected workers to total on LA city contract
Union Status	= 1 if city contract workers are unionized; 0 otherwise
Union Changes	= 1 if became unionized following LWO; 0 if no change in union status
Counterfactual Wage	Starting wage of the largest low-wage occupation that the establishment would have paid in the absence of the LWO.
Subsidiary	= 1 if subsidiary or a division of a larger corporation; 0 if independent firm
Airport	= 1 if workers located at LAX or ONT airports; 0 otherwise
Hiring Standards Change	= 1 if hiring standards changed due to LWO; 0 otherwise
Promotion from within	Percent of positions beyond entry-level that are typically filled through promotion from within the firm
Industry	8 industry categorical variables

WORKER SURVEY

Stayer	= 1 if working for the city contract operation of a former contractor before the establishment was affected by the LWO; 0 otherwise.
Joiner	= 1 if worker joined the city contract operation of a former contractor after the establishment was affected by the LWO; 0 otherwise.
New Joiner	= 1 if worker joined the city contract operation of a new contractor; 0 otherwise.
Age	Years of age.
Hire Age	Age when worker was hired by LW establishment.
Experience	Years of labor market experience after age 16.
Experience at Hire	Experience when worker was hired by LW establishment.
Female	= 1 if female; 0 male.
Years of Schooling	Years of schooling completed.
Currently Enrolled	= 1 if currently enrolled in school; 0 otherwise.
Native English Speaker	= 1 if English is first language; 0 otherwise.
Received Training	= 1 if received any training when started current job; 0 otherwise.
Prior Formal Training	= 1 if completed any training before starting current job; 0 otherwise.
Latino	= 1 if Latino/a; 0 otherwise.
Black	= 1 if African American or black; 0 otherwise.
White	= 1 if White; 0 otherwise.
Asian	= 1 if Asian; 0 otherwise.
Union Member Before	= 1 if worker was a union member before LW for stayer or at last job for joiners and new joiners; 0 otherwise.
Health Insurance	= 1 if the employer provides health insurance for the worker; 0 otherwise.
Health Insurance Before	= 1 if employer provided health insurance before LW for stayer or at last job for joiners and new joiners; 0 otherwise.
Part-Time Job Before Wage	= 1 if hours per week is less than 35; 0 otherwise.
After Wage	Wage earned by stayers before their employer became subject to the LWO, or by joiners before being hired by a LW establishment.
Recent Wage	Wage earned by stayers immediately after their employer became subject to the LWO, or by joiners immediately after being hired by a LW establishment.
Health Benefits	Wage earned by workers at the time of the interview.
Union	= 1 if the wage observation is accompanied by the existence of employer-provided health benefits, and 0 otherwise.
Tenure Change	= 1 if the wage observation is from a unionized employer, and 0 otherwise.
Occupation	Years of tenure after the LW establishment was affected by the LWO or hired by a LW establishment.
LW Establishment	47 occupational categorical variables.
Minimum Wage	37 living wage establishment variables.
	7 minimum wage period variables.

TABLE 2 DESCRIPTIVE STATISTICS FROM EMPLOYER SURVEY.

	<i>Whole Sample</i>	<i>Old Establishment</i>	<i>New Establishment</i>
Airline Service	.04 (.02)	.06 (.03)	0
Landscape Maintenance	.09 (.03)	.08 (.04)	.11 (.08)
Janitorial	.11 (.05)	.09 (.05)	.16 (.15)
Miscellaneous	.23 (.07)	.30 (.08)	0
Retail and Food	.19 (.08)	.10 (.07)	.52 (.21)
Security and Parking	.10 (.05)	.12 (.06)	.04 (.04)
Social Service	.23 (.08)	.25 (.09)	.18 (.16)
Number of Employees	328.27 (165.51)	387.57 (211.04)	118.77 (65.32)
Affected Worker Ratio	.66 (.04)	.65 (.04)	.70 (.08)
Union Status	.16 (.05)	.19 (.06)	.04 (.04)
Union Changes	.03 (.02)	.03 (.02)	.04 (.04)
Counterfactual Wage ^{a)}	7.80 (.23)	7.57 (.14)	8.77 (.72)
Subsidiary	.78 (.07)	.77 (.07)	.79 (.16)
Airport	.30 (.08)	.25 (.07)	.48 (.22)
Hiring Standard Change	.26 (.08)	.32 (.09)	.05 (.06)
Promotion from within ^{b)}	43.42 (7.09)	48.62 (7.90)	24.55 (12.77)
Number of sampled workers	5.04 [8.03]	5.05 [8.08]	5.04 [8.23]
Number of observations	52	44	8

Standard errors are in parentheses. Standard deviations are in brackets.

a) The sample sizes for the variable counterfactual wage is 37 and 7 for old and new establishments respectively.

b) The sample sizes for the variable promotion from within is 36 and 5 for old and new establishments.

TABLE 3 DESCRIPTIVE STATISTICS FROM WORKER SURVEY.

	<i>Whole Sample</i>	<i>Stayer</i>	<i>Joiner</i>	<i>New Joiner</i>
Age	38.81 (.93)	42.26 (1.38)	36.64 (1.69)	33.95 (1.56)
Hire Age	35.34 (.85)	36.68 (1.51)	34.91 (1.65)	32.07 (1.43)
Experience at Hire	16.09 (.91)	17.37 (1.99)	15.98 (1.33)	12.00 (1.31)
Tenure	3.46 (.36)	5.57 (.34)	1.73 (.13)	1.88 (.23)
Female	.57 (.05)	.57 (.06)	.49 (.05)	.84 (.10)
Years of Schooling	12.16 (.33)	12.04 (.48)	12.09 (.37)	12.82 (.55)
Currently Enrolled	.19 (.03)	.15 (.04)	.18 (.05)	.38 (.08)
Native English Speaker	.40 (.06)	.36 (.06)	.39 (.08)	.48 (.12)
Prior Formal Training	.15 (.04)	.12 (.04)	.18 (.06)	.11 (.10)
Received Training	.85 (.04)	.85 (.04)	.85 (.06)	.80 (.12)
Log-Wage Before	1.97 ^a (.03)	1.81 ^a (.03)	2.10 ^a (.04)	2.04 ^a (.03)
Log-Wage After	2.20 ^b (.02)	2.22 ^b (.02)	2.17 ^b (.03)	2.20 ^b (.01)
Recent Log-Wage	2.23 (.02)	2.25 ^b (.02)	2.20 ^b (.03)	2.25 ^b (.02)
Latino	.46 (.06)	.42 (.09)	.49 (.09)	.52 (.09)
Black	.31 (.06)	.31 (.07)	.37 (.08)	.14 (.04)
White	.08 (.03)	.10 (.04)	.04 (.03)	.15 (.07)
Asian	.14 (.03)	.16 (.05)	.10 (.05)	.19 (.07)
Childcare Workers	.03 (.02)	.03 (.02)	.02 (.02)	.10 (.10)
Cleaning Service Workers	.14 (.06)	.16 (.08)	.09 (.06)	.22 (.08)
Landscape Workers	.01 (4.51e-3)	.01 (4.58e-3)	3.97e-3 (3.21e-3)	.03 (.03)
Parking Service Workers	.07 (.04)	.04 (.03)	.11 (.07)	0
Restaurant Workers	.05 (.03)	.04 (.03)	.06 (.04)	.06 (.05)
Retail Workers	.03 (.02)	.01 (.01)	0	.25 (.08)
Social Service Workers	.01 (.01)	.02 (.01)	.01 (.01)	0
Security Service Workers	.04 (.03)	.02 (.02)	.06 (.05)	0
Miscellaneous Occupations	.28 (.08)	.25 (.10)	.31 (.10)	.30 (.13)
Airport Service Workers	.31 (.13)	.39 (.15)	.32 (.15)	0
Supervisors	.03 (.01)	.05 (.02)	.01 (.01)	.04 (.04)
Union Status	.67 (.10)	.69 (.11)	.69 (.12)	.51 (.28)
Union Member Before	.11 (.03)	.11 (.04)	.07 (.02)	.25 (.09)
Health Insurance	.43 (.11)	.45 (.14)	.46 (.13)	.24 (.16)
Health Insurance Before	.34 (.06)	.38 (.10)	.33 (.06)	.21 (.06)
Part-Time Job	.28 (.06)	.16 (.05)	.30 (.08)	.53 (.17)
Number of observations	264	120	111	33

Standard errors are in parentheses. a) Sample size is 211 due to missing wages before. b) Sample size is 252 due to missing wages.

TABLE 4: OBSERVABLE CHARACTERISTICS REGRESSIONS. OLD ESTABLISHMENTS

	(1)	(2)	(3)
Experience at Hire			
Joiner	.07 (.62)	.14 (.80)	.14 (.75)
Joiner x HS Change	--	--	-.15 (.99)
HS Change	--	--	5.59 (1.94)
Hire Age			
Joiner	-.22 (.49)	-.05 (.65)	.08 (.64)
Joiner x HS Change	--	--	-.72 (1.09)
HS Change	--	--	-7.88 (1.89)
Female			
Joiner	-.13 (.05)	-.14 (.06)	-.16 (.06)
Joiner x HS Change	--	--	.06 (.23)
HS Change	--	--	.15 (.18)
Years of Schooling			
Joiner	-.37 (.51)	-.05 (.54)	.07 (.63)
Joiner x HS Change	--	--	-.42 (1.12)
HS Change	--	--	3.55 (.89)
Currently Enrolled			
Joiner	.03 (.04)	-.04 (.06)	-8.82e-5 (.07)
Joiner x HS Change	--	--	-.21 (.18)
HS Change	--	--	.03 (.14)
Native English Speaker			
Joiner	4.97e-3 (.05)	-.07 (.05)	-.07 (.05)
Joiner x HS Change	--	--	-.12 (.21)
HS Change	--	--	-.16 (.14)
Prior Formal Training			
Joiner	.08 (.06)	.11 (.06)	.16 (.07)
Joiner x HS Change	--	--	-.37 (.13)
HS Change	--	--	.45 (.15)
LW Establishment	No	Yes	Yes
Number of Observations	231	231	222

Control variables include: union status, health insurance, number of employees, affected worker ratio, airport, part-time job, subsidiary, race, and 41 occupations, in addition to the other observable characteristics. The number of LW Establishments in regressions (2) is 35. 8 observations were excluded in (3) because the information on changes in hiring standards by their LW employer was missing. For this reason and the fact that one of the two additional control variables are collinear with one of the LW Establishments, the number of these Establishments in (3) is 33.

TABLE 5: OBSERVABLE CHARACTERISTICS REGRESSIONS. OLD AND NEW ESTABLISHMENTS.

		(1)	(2)	(3)	(4)
Experience at Hire					
	Old Establishment	.90 (1.02)	1.79 (1.05)	--	--
	Stayer	--	--	.81 (1.03)	1.69 (1.01)
	Joiners	--	--	.99 (1.09)	1.87 (1.17)
Hire Age					
	Old Establishment	-1.78 (1.23)	-3.60 (1.28)	--	--
	Stayer	--	--	-1.58 (1.21)	-3.39 (1.25)
	Joiners	--	--	-1.97 (1.25)	-3.78 (1.31)
Female					
	Old Establishment	-.38 (.10)	-.43 (.11)	--	--
	Stayer	--	--	-.30 (.10)	-.50 (.22)
	Joiners	--	--	-.43 (.10)	-.46 (.21)
Years of Schooling					
	Old Establishment	-.89 (.47)	-1.32 (.54)	--	--
	Stayer	--	--	-.73 (.59)	-1.16 (.66)
	Joiners	--	--	-1.04 (.45)	-1.45 (.55)
Currently Enrolled					
	Old Establishment	.15 (.11)	.05 (.11)	--	--
	Stayer	--	--	.12 (.10)	.04 (.10)
	Joiners	--	--	.17 (.12)	.07 (.13)
Native English Speaker					
	Old Establishment	.04 (.06)	-.05 (.07)	--	--
	Stayer	--	--	.05 (.06)	-.05 (.07)
	Joiners	--	--	.04 (.06)	-.05 (.07)
Prior Formal Training					
	Old Establishment	-.05 (.05)	-.01 (.07)	--	--
	Stayer	--	--	-.09 (.06)	-.06 (.07)
	Joiners	--	--	-.01 (.05)	.03 (.07)
Industry		No	Yes	No	Yes
Number of Observations		264	264	264	264

Control variables include: union status, health insurance, number of employees, affected worker ratio, airport, part-time job, subsidiary, race, and 45 occupations, in addition to the other observable characteristics. The number of industries in regressions (2) and (4) is 6.

TABLE 6: UNOBSERVABLE CHARACTERISTICS REGRESSIONS. OLD ESTABLISHMENTS

	(1)	(2)	(3)
Joiner	.14 (.05)	.15 (.07)	.19 (.07)
Joiner x HS Change	--	--	-.27 (.09)
HS Change	--	--	-.03 (.11)
LW Establishment	No	Yes	Yes
Number of Observations	190	190	182

Control variables include: female, experience at hire, age at hire, years of schooling, native english speaker, health benefits, union, 3 races and ethnicities, 37 occupations and 7 minimum wage periods. The number of LW Establishments in regressions (2) and (3) is 30 and 29 respectively.

TABLE 7: UNOBSERVABLE CHARACTERISTICS REGRESSIONS. OLD AND NEW ESTABLISHMENTS.

	(1)	(2)	(3)	(4)
Old Establishment	-.001 (.03)	-.01 (.03)	--	--
Stayer	--	--	-.09 (.04)	-.11 (.05)
Joiners	--	--	.05 (.03)	.03 (.03)
Industry	No	Yes	No	Yes
Number of Observations	221	221	221	221

Control variables include: female, experience at hire, age at experience, years of schooling, native English speaker, health benefits, union, 3 races, 45 occupations and 7 minimum wage periods. The number of industries in regressions (2) and (4) is 6.

TABLE 8: WAGE GAIN REGRESSION RESULTS

	<i>Before LW</i>	<i>LW</i>	<i>LW Raise</i>	<i>Wage Increase (%)</i>	<i>N</i>
Stayer	6.29	7.19	.90	14.31%	97
Joiner	6.96	7.04	.08	1.09%	94
New Joiner	6.62	7.51	.89	13.44%	29

Control variables include: tenure change, union status, health insurance, number of employees, affected worker ratio, subsidiary, 43 occupations, 6 industries and 11 minimum wage periods.

Appendix Table 1: Full Regression Results. Observable and Unobservable Characteristics.

Independent Variables	Table 4		Table 5		Table 6	Table 7
	Prior Formal Training	Female	Prior Formal Training	Female	(2) Log-Wage	(4) Log Wage
Joiner	.11 (.06)	-.14 (.06)	.03 (.07)	-.48 (.11)	.15 (.07)	-.03 (.03)
Stayer	--	--	-.06 (.07)	-.35 (.10)	--	-.11 (.05)
Experience at Hire	-3.09e-3 (.01)	-.01 (.01)	-2.42e-3 (4.69e-3)	-.01 (.01)	-1.48e-3 (3.74e-3)	-1.57e-4 (3.21e-3)
Hire Age	1.04e-3 (.01)	-2.43e-2 (.01)	-7.53e-4 (3.96e-3)	1.12e-3 (.01)	.01 (.3.68e-3)	.01 (2.99e-3)
Female	-.08 (.05)	--	-.10 (.05)	--	-.04 (.03)	-.08 (.03)
Years of Schooling	0.01 (.01)	-.03 (.01)	.01 (.01)	-.03 (.01)	-5.06e-6 (.01)	-9.86e-4 (.01)
Currently Enrolled	.10 (.05)	.24 (.13)	.10 (.04)	.21 (.09)	--	--
Native English Speaker	-.04 (.07)	.10 (.12)	-.08 (.06)	.13 (.09)	-.02 (.06)	-.02 (.04)
Prior Formal Training	--	-.19 (.12)	--	-.22 (.07)	--	--
Black	.03 (.05)	.04 (.17)	.01 (.05)	.03 (.13)	-.04 (.11)	-.02 (.07)
White	.12 (.10)	.11 (.17)	.11 (.08)	.13 (.11)	-.10 (.06)	-.09 (.05)
Asian	.14 (.06)	-.13 (.15)	.06 (.05)	-.05 (.14)	-.17 (.06)	-.15 (.05)
Union Status	.45 (.31)	-.15 (.60)	.16 (.07)	.12 (.07)	.09 (.08)	.11 (.05)
Health Insurance	.03 (.07)	-.22 (.11)	.02 (.05)	-.03 (.08)	.06 (.03)	.06 (.04)
Airport	.06 (.11)	.50 (.61)	-.08 (.07)	.36 (.19)	--	--
Part-Time Job	-.09 (.06)	.12 (.07)	-.06 (.05)	.12 (.07)	--	--
Subsidiary	.14 (.14)	1.00 (.28)	-.10 (.08)	-.20 (.10)	--	--
Constant	1.07 (.30)	.44 (.71)	-.23 (.17)	1.44 (.26)	1.81 (.14)	2.21 (.14)
Number of Industries	--	--	6	6	--	6
Number of Occupations	35	39	46	46	36	45
Number of LW Establishments	38	34	--	--	30	--
Number of MW periods	--	--	--	--	7	7
R ²	.680	.592	.0599	.551	.643	.605
Number of Observations	231	231	264	264	190	221

Appendix Table 2: Full Regression. Wage gain.

Independent Variables	Log-Wage
Wage Increase for Stayers	.09 (.05)
Before Wage Difference Joiner-Stayer	.11 (.04)
After Wage Difference Joiner-Stayer	.05 (.05)
Before Wage Difference New Joiner-Stayer	.04 (.05)
After Wage Difference New Joiner-Stayer	-.03 (.04)
Tenure Change	.01 (.01)
Union Status	.08 (.03)
Health Insurance	.04 (.02)
Constant	2.01 (.23)
Number of Employees	2.30e-6 (4.92e-6)
Affected Worker Ratio	-.08 (.04)
Subsidiary	.02 (.08)
Number of Industries	6
Number of MW periods	7
Number of Occupations	43
LR chi2	392.74
Number of Observations	392

Standard errors are in parentheses.