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Abstract
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Keywords
minimum wages

THE ECONOMIC EFFECTS OF A CITYWIDE MINIMUM WAGE

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This paper presents the first study of the economic effects of a citywide minimum wage—San Francisco's adoption of an indexed minimum wage, set at $8.50 in 2004 and $9.14 by 2007. Compared to earlier benchmark studies by Card and Krueger and by Neumark and Wascher, this study surveys table-service as well as fast-food restaurants, includes more control groups, and collects data for more outcomes. The authors find that the policy increased worker pay and compressed wage inequality, but did not create any detectable employment loss among affected restaurants. The authors also find smaller amounts of measurement error than characterized the earlier studies, and so they can reject previous negative employment estimates with greater confidence. Fast-food and table-service restaurants responded differently to the policy, with a small price increase and substantial increases in job tenure and in the proportion of full-time workers among fast-food restaurants, but not among table-service restaurants.

In November 2003 San Francisco voters passed a ballot proposition to enact a minimum wage covering *all* employers in the city. The new standard set a minimum wage of $8.50 per hour—an increase of over 26% above the California minimum wage of $6.75—and an annual adjustment for cost-of-living increases (reaching $9.14 in 2007). This standard, which first became effective in late February 2004, constituted the highest minimum wage in the United States and the first implemented municipal minimum wage in a major city (excluding the District of Columbia). In a prospective study of this policy, Reich and Laitinen (2003) estimated that about 54,000 workers, amounting to 10.6% of the city's work force, would receive wage increases, either directly or indirectly, if such a policy were adopted, and that the increased wage costs on average would amount to about 1% of business operating costs.

Simple trends from county-level QCEW administrative data comparing San Francisco with nearby Alameda County suggest that the policy did increase pay, and moreover did so without affecting employment. Figures 1 and 2 present these comparisons for restaurants, the industry with the greatest proportion and absolute number of minimum wage workers. In the years prior to the enactment of the San Francisco policy, restaurant pay and employment in these two geographic areas exhibited quite similar trends. After the policy was

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The authors will supply the data used in this study to others upon request. Contact them at Institute for Research on Labor and Employment, University of California, Berkeley, CA 94720-5555.

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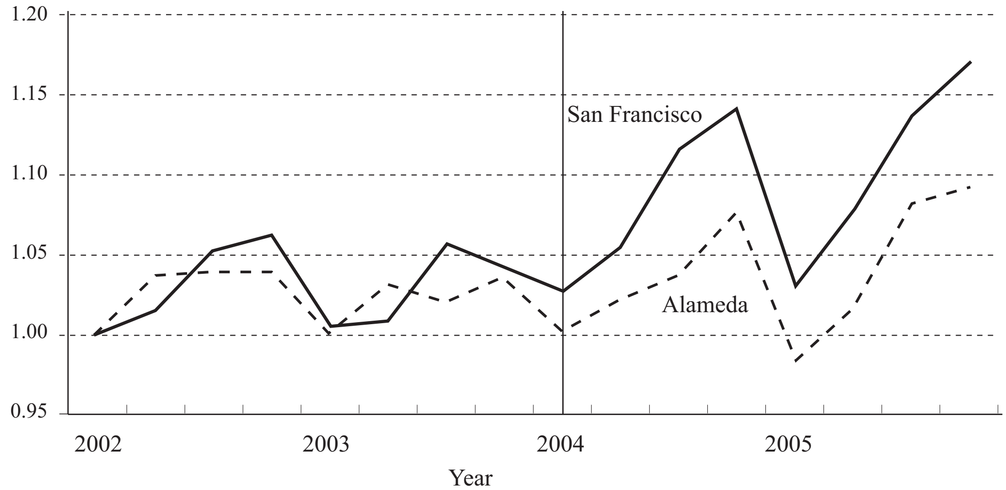
Figure 1. Average Earnings for Restaurant Workers (2004 $) - QCEW. (Indexed to 2002 Q1 level)



implemented, restaurant pay increased in San Francisco relative to Alameda County, while relative employment trends did not change. Of course, these simple comparisons are only suggestive, as they could be affected by other changes, such as changes in the size or type of restaurant in the two areas.

In this paper, we identify the effects of the San Francisco wage policy, drawing on data from a commissioned panel survey of restaurants. A first wave was fielded just prior to the implementation of the new policy, with re-interviews of the same restaurants nine to ten months later. The sample includes small restaurants that were exempt from the policy in its first year, restaurants that did not have any workers paid below $8.50, and restaurants from the neighboring East Bay region that were not covered by the policy.

The principal outcomes we examine are average hourly pay, the distribution of pay, total employment, and part-time and full-time employment. Other outcomes we examine are menu prices, employee tenure, health insurance coverage, proportion of workers who receive tips, and employer compliance with the law.

In addition to the findings themselves, this paper adds in at least seven ways to existing understanding of minimum wage effects. First, by providing the first study of how a *city-wide* minimum wage policy operates, we shed light on whether employment effects of city policies differ from those of state minimum wage laws. Second, our measure of the policy treatment incorporates the fraction of a firm's work force affected by the policy, which we argue is more pertinent than the "wage gap" measure used by Card and Krueger (1994) and others. Third, some features of the policy—notably its delayed application to very small employers—and the geography of the San Francisco Bay Area allow us to have better control groups for identifying minimum wage effects than are typical in the literature. Fourth, we collected data on a greater range of outcomes than previous studies have examined, permitting us to investigate the effects of the policy on work hours, wage compression, employee tenure, and health insurance coverage. Fifth, we are able to investigate whether the experiences of fast-food restaurants differed from those of table-service restaurants. Sixth, we examine other factors that may affect employment response, such as inelasticity of demand (restaurants that are located in tourist areas) and noncompliance with the

Figure 2. Total Restaurant Employment - QCEW.
(Indexed to 2002 Q1 Level)

law (restaurants that hire large numbers of immigrants). Finally, our sample contains much less measurement error, and substantially narrower confidence intervals for our employment elasticities, than are found in previous studies.

Existing Research on Minimum Wages and Theoretical Predictions

An extensive literature has discussed the effects of minimum wages at both the national and state levels (Brown 1999; Lee 1999). Methodologically, our study is most closely related to Card and Krueger (1994), which surveyed fast-food restaurants in New Jersey and Pennsylvania to evaluate the employment effects of a minimum wage law passed in New Jersey. The controversy surrounding their findings of negligible or mildly positive employment effects and the subsequent set of studies it spawned are well known (Neumark and Wascher 2000; Card and Krueger 2000). Since critics of Card and Krueger's study raised questions about their data, such as actual hours worked and measurement error, we pay close attention here to these issues.

The recent minimum wage literature draws on two models of the labor market. The standard competitive model predicts that a minimum wage will have measurable negative employment effects to the extent that (a) the policy is binding and (b) input substitution possibilities are present and/or (c) product demand is price-elastic. In the absence of (b) and (c), a minimum wage could still increase prices but not reduce employment substantially. A minimum wage increase can also be offset by compensating differentials for benefits, such as reductions in health insurance coverage, or by changing the component of income that takes the form of tips relative to employer-paid compensation. These compensating offsets may mitigate any negative employment effects.

Non-competitive labor market models recently have received increased attention in the minimum wage literature, in part because of the positive employment effects reported in Card and Krueger (1994) and their dynamic monopsony explanation (Card and Krueger 1995). In a typical dynamic monopsony model, the presence of labor market frictions from costly matching and search implies that the wage elasticities of recruitment and quits are not infinite, and employers have some wage-setting power (Manning 2003). This model generates an equilibrium in which optimizing firms choose a wage rate that also produces vacancies and positive employee quit rates. Since there are other monopsony models in the literature, we will refer to this class of models as recruitment-retention models to emphasize the centrality of these two factors in the story. For high employee-turnover industries such as fast-food restaurants, the recruitment-retention model predicts that minimum wage increases can cause a fall in quit rates or vacancies or both. For some equilibrium configurations, employment may increase.

Although these two models generate somewhat differing predictions, they are not mutually exclusive. We collected data on a number of the outcomes predicted by both models so that we could compare our findings with each. To determine wages, we asked each firm to report the number of workers paid an hourly wage in five bins: under $6.75, $6.75–$8.49, $8.50–$9.49, $9.50–$10.99, and $11 or higher. We collected hours for the average full-time and part-time worker, and the number of workers in each category, as well as typical employee tenure. We also collected data on employee health coverage, tips, and the price of the most popular menu item.

Sampling and Identification Strategies

The presence in the San Francisco Bay metropolitan area of several major central urban areas besides San Francisco itself (such as the cluster of older cities in the East Bay) provides a quasi-experimental context for testing predictions about the economic effects of a policy that is specific to San Francisco. Since our treatment and control samples are all part of the same San Francisco-Oakland-Fremont Metropolitan Statistical Area, they are likely to provide closer comparisons than cross-state studies.

Sampling Strategy

Our establishment sample is drawn from fast-food and table-service restaurants in San Francisco and the East Bay.[1] The surveys were administered to each establishment's owner or manager by an independent San Francisco surveying firm via telephone, using Computer Aided Telephone Interviewing (CATI) and a sample drawn from a Dun and Bradstreet list of all restaurants in the Bay Area. The surveying firm, which has a substantial group of survey professionals and is very experienced in collecting data from business establishments, completed the first wave of interviews in a small time window. In order to minimize responder bias, the survey instrument did not refer anywhere to the minimum wage policy and all responses were treated confidentially.[2] We included both fast-food and table-service restaurants in order to test for heterogeneities in responses by types of restaurant.[3]

Focusing only on restaurants provides two advantages. First, by comparing trends in the same industry, we eliminate inter-industry growth differentials that are unrelated to the minimum wage increase. Second, restaurants are the most intensive users of minimum wage labor.[4] A large part of the minimum wage impact literature has focused on this industry (or subsets, such as fast-food establishments) partly for this reason.

The sample for the first wave consisted of 254 restaurants in San Francisco and 100 restaurants in the East Bay. The San Francisco sample is larger because we want to be able to compare restaurants within the city by initial firm size (businesses with fewer than 10 employees were exempt from the policy in the first year). Since we did not need such a comparison for the East Bay, we did not collect data from small East Bay restaurants.

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[1]Figures 1 and 2 support the choice of these two areas for closer comparisons. The East Bay here refers to the 510 telephone area code, which includes much of Alameda County and a few areas to its north, in western Contra Costa County. This area consists primarily of older cities, such as Oakland, Hayward, and Berkeley.

[2]A study prepared for the Golden Gate Restaurant Association, the local industry group, uses data that were collected without following many of these standard sampling methods. Moreover, the study contains no control groups. (Sims 2005.)

[3]These correspond to the NAICS labels of limited-service and full-service restaurants, respectively.

[4]Restaurants also account for a substantial proportion of all low-wage workers. Prior to the enactment of the minimum wage, 15.5% of the low-wage (under $7.75 per hour) San Francisco work force was employed in restaurants. The comparable figure among the largest 50 cities in the United States was 15.7%. (Computed by the authors from the 2000 Census, PUMS dataset.)

Prior to sampling, we chose the firm size categories—4 to 8 current workers for small restaurants and 14 to 35 current workers for midsize restaurants—to balance competing concerns. Including in our sample restaurants that are close to the cutoff (say at 11 current employees) may cause us to observe a decline in employment if many of those establishments adopt a strategy of evading the mandate by shedding workers. However, this effect is not the labor demand effect referred to or estimated in the literature. Businesses with 14 workers are much less likely to reduce their work force to 9 just to evade the mandate. Similarly, we chose a cutoff of smaller restaurants at 8 workers to permit observation of employment growth, as well as decline, among restaurants not subject to the mandate.[5] We also truncated the initial size of "mid-size" establishments at 35 current employees, as restaurants of much greater size (say 50 or 100 workers) may be different types of enterprises operating in markets with distinct dynamics.

To summarize, the sampling frame included small San Francisco restaurants, midsize San Francisco restaurants, and midsize East Bay restaurants. The treatment group consists of those midsize San Francisco restaurants that had to raise the pay of at least one employee. Since small East Bay restaurants would not be directly comparable to the treatment group, and since the time available to conduct the survey was limited, we chose to leave out this group. One implication of this sampling frame, which will be further discussed in the next section, is that controlling for both size and region in the regressions will effectively draw from the midsize San Francisco restaurants, comparing those that had to raise wages versus those that did not.

The first wave of our study was conducted during January and most of February 2004, ending just before the new minimum wage went into effect but after the November 2003 vote that made the policy law.[6] Although firms might have adjusted employment in anticipation of the law, in a high-turnover market such as the one for restaurant labor there is little incentive to reduce the labor force through attrition before the policy is implemented.[7] As we show below, evidence from administrative payroll data also suggests that anticipation effects were minimal.

The second wave of the survey was conducted in November and December 2004. Restaurants were re-surveyed for the second wave and surveys were completed for 301 of the original 354 restaurants in wave 1. The respondents in the second wave were similar to non-respondents in terms of employment and wages reported in wave 1. East Bay restaurants were somewhat more likely to respond (95%) than San Francisco restaurants (85%). Among the restaurants that did not respond in the second wave, 15 were confirmed to have closed; the rest (38) were all in operation, but either refused (18) or were unable (20) to complete the survey in wave 2.

Our sample design includes restaurants with either 4 to 8 workers or 14 to 35 workers in San Francisco, and only those with 14 to 35 workers in the East Bay. We constructed sampling weights based on firm-size categories and our sampling design to make the sample representative of the surveyed population. The weights reproduce the size and regional distribution of firms conditional on passing the screen. The derivation of the weights is provided in Appendix B of Dube et al. (2006).

Identification Strategy

Our identification strategy uses the policy change as the exogenous variation, and con-

[5]According to California administrative data, in 2004 restaurants with fewer than ten employees constituted about 60% of restaurant establishments and about 17% of restaurant workers in both San Francisco and the East Bay.

[6]The response rate was 38% for the first wave, comparable to the rates in other similar surveys among a much broader range of establishments (for example, 42% in Reich and Laitinen 2003). As the Appendix shows, the respondents and non-respondents were quite similar by both size and region. Anecdotal reports from the surveying firm indicated that non-respondent restaurants did not differ by neighborhood, ethnicity, or chain versus independent operation.

[7]As we discuss below, we checked this point by comparing results from the early and later completed cases and found no differences.

siders covered firms that are economically affected as the "treatment group." Specifically, a restaurant is considered to be in the "treatment group" if (1) it employed at least one worker whose hourly wage was below \$8.50 in wave 1, and (2) it was covered by the law—that is, it was located in San Francisco and employed at least ten workers in wave 1.[8]

Since our sample includes both small and midsize restaurants in San Francisco, and midsize restaurants in East Bay, we can control for differential trends by restaurant size and region. The first part of our analysis compares the average change in outcome variables between the treatment group and the three control samples—small San Francisco restaurants, the unaffected midsize San Francisco restaurants that were already paying above minimum wages, and midsize East Bay restaurants. We report simple difference-in-difference tests using the percentage point change in the outcome variables (y) in the treatment group as compared to the various control groups:

$$E(y_{i,t+1} - y_{i,t} | Treatment_i = 1) - E(y_{i,t+1} - y_{i,t} | Treatment_i = 0,\ Control_{ij} = 1)$$

Control groups j denote (1) all controls, (2) only small SF controls, (3) only unaffected midsize SF controls that pay above the minimum wage, and (4) only midsize East Bay controls.

We also provide regression estimates in which we regress the growth rates of various outcome variables on the "treatment intensity." The "treatment intensity" is a continuous measure defined as

$$\text{Treatment Intensity}_{i,t} = \begin{cases} \text{\% Workers in Wave 1 under \$8.50 if SF \& Midsize,} \\ 0 \text{ otherwise} \end{cases}$$

The treatment intensity equals the proportion of workers earning under \$8.50 if the restaurant is covered by the law (that is, if it is in San Francisco and employs at least 10 workers) and 0 otherwise. This measure captures the intensity of the policy's effect on a restaurant, as any putative effect will clearly be greater in a restaurant with, say, 90% of the work force affected by the law than in a restaurant with 10% affected.[9]

Our regression specifications are as follows:

$$(1)\quad y_{i,t+1} - y_{i,t} = b_0 + b_1 \cdot \text{TreatmentIntensity}_{i,t} + e_{i,t+1}$$

$$(2)\quad y_{i,t+1} - y_{i,t} = b_0 + b_1 \cdot \text{TreatmentIntensity}_{i,t} + b_2 \cdot MS_{i,t} + e_{i,t+1}$$

$$(3)\quad y_{i,t+1} - y_{i,t} = b_0 + b_1 \cdot \text{TreatmentIntensity}_{i,t} + b_3 \cdot SF_{i,t} + e_{i,t+1}$$

$$(4)\quad y_{i,t+1} - y_{i,t} = b_0 + b_1 \cdot \text{TreatmentIntensity}_{i,t} + b_2 \cdot MS_{i,t} + b_3 \cdot SF_{i,t} + e_{i,t+1}$$

Here $y_{i,t}$ is the outcome variable (such as the natural log of employment) at firm i at time t. MS_{it} is a dummy variable equal to 1 if firm i is in the 14–35 employees category at initial time t; SF_i is a dummy variable equal to 1 if firm i is located in San Francisco. The treatment coefficient is thus b_1—the change in outcome in affected restaurants (that is, covered San Francisco restaurants with some workers below the new minimum), net of change due to firm size and region. A $b_1 < 0$ implies a negative effect of the minimum wage treatment on the dependent variable. The coefficient b_1 can be interpreted as the change in outcome y resulting from the policy in a

[8] One concern is that some of these "midsize" firms may reduce employment to escape coverage. As previously explained, our sample excludes establishments with between 10 and 13 workers in wave 1 to mitigate this problem; and we did not find any such size category changes in our sample.

[9] This "treatment intensity" differs from the Card and Krueger (1994) "wage gap" measure; their measure equals the difference between the starting wage at a firm and the new minimum wage, provided the new minimum is higher. Given a substantial mass point at the minimum wage, most of the variation in increased wage costs between firms is likely to be on the quantity dimension (that is, how many workers were earning less than the new minimum wage) as opposed to the price dimension (that is, the wage gap). For this reason, the fraction of the work force under the new minimum represents a better measurement of treatment.

Table 1. Average Wage and Proportion under $8.50: Descriptive Statistics and Simple Difference-in-Difference.

	Average Wage				*Percent under $8.50*			
Sample	*Wave 1*	*Wave 2*	*Percent Change*	*Dif.-in-Dif.*	*Wave 1*	*Wave 2*	*Percent Change*	*Dif.-in-Dif.*
Treatment Group	10.22	11.01	7.74**		49.72	5.00	–89.94**	
	(0.25)	(0.19)	(2.67)		(2.98)	(2.21)	(4.48)	
All Controls	9.78	10.09	3.17**	4.57*	43.34	38.11	–12.07**	–77.88**
	(0.12)	(0.12)	(1.24)	(2.60)	(2.26)	(2.03)	(5.01)	(6.72)
Alternative Control Samples								
Small S.F.	10.44	10.91	4.50	3.24	33.44	18.90	–43.48**	–46.46**
	(0.26)	(0.24)	(2.37)	(2.58)	(0.15)	(0.34)	(9.11)	(10.16)
Unaffected:								
Midsize S.F.	11.06	11.01	–0.45	8.19**	0.00	2.83	n/a	n/a
	(0.21)	(0.20)	(2.08)	(3.40)	(0.00)	(2.02)		
Midsize East Bay	9.13	9.40	2.96*	4.79*	58.84	59.68	1.43	–91.37**
	(0.14)	(0.17)	(1.72)	(2.88)	(3.02)	(2.98)	(5.20)	(6.88)

*Statistically significant at the .10 level; **at the .05 level; ***at the .01 level.

restaurant with 100% of its workers initially under the new minimum of $8.50 (that is, $\text{TREATMENT}_{i,t} = 1$). All the regressions (except the wage distribution ones) are estimated with OLS.[10] The regressions estimating the effect of the treatment intensity on the four categories of pay are estimated using Seemingly Unrelated Regression, with a cross-equation constraint that the treatment intensity coefficients sum to zero.

Given our sampling frame (small and midsize San Francisco restaurants and midsize East Bay restaurants, but no small East Bay restaurants), adding size and location controls is *numerically identical* to limiting our control samples. That is, specification (2) uses San Francisco controls only, specification (3) uses midsize controls only, and specification (4) uses midsize San Francisco controls only; thus, specification (4) identifies the treatment coefficient by comparing treatment group restaurants to San Francisco midsize restaurants that were already paying $8.50 or above in wave 1. To highlight the control samples used implicitly by these regression specifications, we label the specifications in the tables as "San Francisco Only," "Unaffected Midsize Only," and "SF and Midsize Only." We also report the results from only comparing "low-wage" firms—those that had at least some workers earning below $8.50 in wave 1.

To check formally the consistency of the estimates across specifications, we run Wald tests of the stability of the coefficients across specifications (or subsamples). Finally, we also test for the heterogeneity of effects by type of restaurants by limiting both the treatment and control groups to fast-food or table-service restaurants only.

In order to compare our employment results to results reported in the existing literature, we present point estimates and 90% confidence intervals for elasticities for each specification. Care must be taken in comparing our implied elasticities with those in the literature. Our "minimum wage elasticity" is defined as

$$\beta = \frac{\%\Delta \text{ Employment in Subpopulation}}{\%\Delta \text{ Minimum Wage}}.$$

We compute this "minimum wage elasticity"—the most common type of elasticity

[10]Our standard errors are heteroskedasticity-corrected and so are labeled as robust standard errors in the tables. Since our data span only two periods, we do not have Bertrand et al. (2004)–type autocorrelation issues.

Table 2. Treatment Intensity and Average Wage: Regression Estimates.

	(1) *Full Sample*	(2) *Fast-Food Only*	(3) *Full Service Only*	(4) *S.F. Only*	(5) *Unaffected Midsize Only*	(6) *S.F. and Midsize Only*	(7) *Low-Wage Only*
Treatment	1.416***	1.968**	0.912	1.385**	1.606***	2.237***	1.152**
	(0.503)	(0.820)	(0.658)	(0.554)	(0.512)	(0.591)	(0.525)
N	265	123	142	181	195	111	173
R-Squared	0.02	0.02	0.02	0.04	0.03	0.15	0.06

Notes: Robust standard errors in parentheses. The columns represent different subsamples of restaurants. "S.F." refers to the San Francisco subsample; "Low-Wage Only" refers to the subsample of firms with some workers earning less than $8.50 per hour in wave 1.

*Statistically significant at the .10 level; **at the .05 level; ***at the .01 level.

reported and used in the literature[11]—for a variety of specifications.

The magnitude of the implied elasticity depends crucially on the definition of the subpopulation.[12] In our case, the subpopulation consists of the set of restaurants affected by the policy—the affected midsize San Francisco restaurants. We estimate this elasticity β as equal to the regression coefficient b_1 times the percentage of the midsize San Francisco work force that was earning under the new minimum wage of $8.50 (23%), divided by the percentage increase in the minimum wage (26%).[13] We report both the point estimate of the elasticity and the confidence interval of this statistic as implied by our regressions.

Typically, in the minimum wage literature, the subpopulation consists of teenagers, fast-food workers, or some other high-impact group. For example, using payroll data, Neumark and Wascher (2000) found an elasticity of –0.22, implying that a 10% increase in the minimum wage lowered employment by 2.2% among fast-food workers. In contrast, Card and Krueger (1994) found a positive elasticity of 0.34, which rose to 0.82 when weighted by initial employment. In the literature using *household* data, some ranges of estimated elasticities found for youth aged 16–19 are –0.11 to –0.22 (Neumark and Wascher 1994), –0.37 to –0.87 (Burkhauser et al. 2000), and 0.05 to 0.10 (Card and Krueger 1995).

When comparing minimum wage elasticities, it is critical to ensure that the minimum wage coverage rates are similar in the different populations. Given the industry from which our data are taken, our elasticity estimates are comparable to those from the fast-food industry studies. The teenage population is also comparable to our population in this sense, as 25% of all teenage workers are directly affected by a typical minimum wage increase (Brown 1999), nearly identical to the percentage of San Francisco midsize restaurant workers who received a pay raise in our sample (23%).[14]

Effects on Pay and the Distribution of Pay

In this section, we report our estimates of the impact of the wage policy on the level and distribution of restaurant workers' pay.

[11]This is *not* the same as labor demand elasticity for minimum wage workers, which would be defined as

$$\eta = \frac{\%\Delta \text{ Employment for Minimum Wage Workers}}{\%\Delta \text{ Wage for Minimum Wage Workers}}$$

[12]Brown (1999) presents a thorough discussion of this issue and the various estimates in the literature.

[13]In San Francisco, 22% of midsize table-service restaurant workers and 23% of midsize fast-food workers in our sample earned below the new minimum wage. When computing the elasticities separately for these two industry segments, we used the 22% and 23% figures, respectively.

[14]The same coverage rate is a necessary but not sufficient condition for true comparability, as the composition of workers may change in each of these categories as well (for example, more skilled workers may join restaurants, or more skilled teenagers may enter the labor force).

Table 3. Wage Distributions in Waves 1 and 2.
(percentages)

	Treatment Group		*All Control Groups*	
Wage	*Wave 1*	*Wave 2*	*Wave 1*	*Wave 2*
$11 per hour or more	22.63	25.00	14.30	16.26
$9.50 to 10.99	15.39	21.18	15.36	15.30
$8.50 to 9.49	10.22	49.46	24.12	26.00
$6.75 to 8.50	51.68	4.35	46.18	42.37
Under $6.75	0.08	0.00	0.05	0.07

Table 1 provides the changes in average wage and the proportion of workers earning under $8.50 per hour for the treatment group and the various control groups. As shown in the table, the average wage rose from $10.22 to $11.01 at restaurants in the treatment group, while in other restaurants it rose from $9.78 to $10.09. Comparing the three control groups, we find that wages also grew in small San Francisco restaurants, though substantially less so than in the treatment group.

The fraction of workers in the treatment group receiving a wage below $8.50 declined from 50% to 5% between waves 1 and 2. In contrast, among firms in the control group overall, that fraction remained relatively stable, declining only from 43% to 38%. The difference-in-difference estimate for the proportion of workers earning below $8.50 per hour shows that the pay raise at the bottom in the treatment group was statistically significant and large vis-à-vis all the control groups. This result demonstrates clearly that the minimum wage policy in San Francisco had a strong impact in raising pay at the bottom, as compared to restaurants not affected by the policy.

As Table 2 shows, the treatment coefficient for average wage is statistically significant at the 1% level for our baseline specification (1), and it is statistically significant across the three control samples (specifications 4–6), as well as when examined at low-wage firms only (specification 7).[15] The average wage increase is higher among treated firms in fast-food restaurants only (specification 2) than among table-service restaurants (specification 3).

Besides raising average wages, the policy also clearly reduced the *dispersion* of wages among restaurant workers. This finding is presented in Tables 3 and 4. As the regression estimates in Table 4 show, the statistically significant reduction in employment in the under $8.50 per hour category is overwhelmingly compensated by a statistically significant increase in the $8.50–9.49 per hour category. In most of the specifications, there is no statistically significant increase in the proportions of workers in higher wage categories. These results indicate a strong compression in the within-firm wage distribution for restaurants affected by the minimum wage policy.[16]

Effects on Employment and Full-Time Equivalent Employment

In this section we report our findings using two different employment measures—headcount and full-time equivalent employment. We use the term *employment* specifically to refer to the headcount number of workers. Our survey asked firms to report the number of full-time and part-time workers, as well as the average number of hours per week worked by each. Using this information, we constructed a measure of employment hours, a finer measure of employment than has

[15]In this and all subsequent tables with regression estimates, we report Huber/White robust standard errors, which are consistent in the presence of heteroskedasticity.

[16]Overall wage compression effects can also be measured by comparing changes in the variance of log wages. In the treatment group, the variance declined from 0.108 to 0.079; in the control sample, it rose slightly from 0.076 to 0.082.

Table 4. Treatment Intensity and Wage Distribution: Regression Estimates.

Wage	*(1)* *Full Sample*	*(2)* *Fast-Food Only*	*(3)* *Table-Service Only*	*(4)* *S.F. Only*	*(5)* *Unaffected Midsize Only*	*(6)* *S.F. and Midsize Only*	*(7)* *Low-Wage Only*
$11 or More	0.065	0.148	–0.012	0.073	0.070	0.130*	0.080
	(0.88)	(0.130)	(0.112)	(0.090)	(0.076)	(0.074)	(0.157)
$9.50–10.99	0.061	0.019	0.112	0.040	0.075	0.060	–0.039
	(0.101)	(0.168)	(0.123)	(0.108)	(0.079)	(0.080)	(0.132)
$8.50–9.49	0.718***	0.661***	0.755***	0.610***	0.803***	0.780***	0.330*
	(0.146)	(0.247)	(0.171)	(0.147)	(0.131)	(0.119)	(0.173)
Under $8.50	–0.843***	–0.829***	–0.855***	–0.723***	–0.948***	–0.970***	–0.372
	(0.170)	(0.293)	(0.193)	(0.126)	(0.159)	(0.052)	(0.249)
N	265	123	142	181	195	111	138

Notes: Robust standard errors in parentheses. The columns represent different subsamples of restaurants. "S.F. "refers to the San Francisco subsample. "Low Wage Only" refers to the subsample of firms with some workers earning lower than $8.50 per hour in wave 1. Results in each column are estimated using Seemingly Unrelated Regressions, with a constraint that the treatment intensity coefficients sum to zero.

*Statistically significant at the .10 level; **at the .05 level; ***at the .01 level.

been used in previous studies, most of which have relied on headcount or have imputed full-time equivalent employment based on partial information. We compute full-time equivalent employment as the total number of hours worked per week divided by 40.[17]

Table 5 provides the differences in means between the treated group and the various control groups, for both employment and full-time equivalent employment. The upper panel of Table 5 reports these results for the balanced panel—all the restaurants that responded to both waves of the survey. The lower panel takes into account the businesses that closed or relocated and thus did not respond to the second wave of the survey.

Considering first the balanced panel, the treatment group exhibits an increase in employment (2.79%); this growth exceeds that in the control group overall (1.10%), although it is below the employment growth in other midsize SF controls only (5.91%). For full-time equivalent employment, the treatment group registered a 4.81% growth, while the growth for other control groups ranged between 0.06% and 3.81%. Overall, total hours of work grew more in the treated sample than in any of the other control groups, although the differences were not statistically significant as shown by the standard errors of the difference-in-difference estimate.

The lower panel of Table 5 again estimates the change in employment and full-time equivalent employment, but it now also includes the 15 restaurants that were closed between waves 1 and 2.[18] We continue to find that the growth in employment and full-time equivalent employment was larger in the treatment group; all the control samples registered negative growth. Even when we factor in the closed restaurants, we find that overall growth remained positive for the treatment sample.[19] This finding is consistent with the pattern of business closures in Table 6: closure incidence was lower in the treatment group than in the control groups.[20]

[17]In a later section we report our results on the proportion of full-time workers, and hours worked by full-time and part-time workers.

[18]Their employment (or full-time equivalent employment) was set to zero for wave 2.

[19]However, the margin of difference falls within the margin of statistical error.

[20]To examine new entrants, we collected data on the number of new restaurant permits issued in 2003 and 2004. New permits increased by 39.5% in San Francisco and decreased by 39.7% in Oakland (the only East Bay city for which data were available). These permit statistics exclude permits that were issued solely because of a change in ownership of an existing restaurant.

Table 5. Employment and Full-Time Equivalent Employment: Descriptive Statistics and Simple Difference-in-Difference.

	Employment				*Full-Time Equivalent Employment*			
Sample Description	*Wave 1*	*Wave 2*	*Percent Change*	*Dif.-in-Dif.*	*Wave 1*	*Wave 2*	*Percent Change*	*Dif.-in-Dif.*
Balanced Panel Excluding Business Closures								
Treatment Group	20.42	20.99	2.79		16.23	17.01	4.81	
	(0.68)	(1.46)	(5.79)		(0.74)	(1.29)	(6.34)	
All Controls	15.5	15.67	1.10	1.69	11.18	11.41	2.06	2.75
	(0.27)	(0.43)	(2.24)	(6.21)	(0.29)	(0.35)	(2.62)	(6.95)
Alternative Control Samples:								
Small S.F.	6.14	6.3	2.61	0.19	4.73	4.91	3.81	1.00
	(0.15)	(0.31)	(4.04)	(7.09)	(0.18)	(0.26)	(4.52)	(7.89)
Unaffected:								
Midsize S.F.	20.63	21.85	5.91	–3.12	15.97	15.98	0.06	4.74
	(0.61)	(1.59)	(7.54)	(9.53)	(0.67)	(1.00)	(6.16)	(8.93)
Midsize East Bay	20.43	20.34	–0.44	3.23	14.02	14.34	2.28	2.52
	(0.48)	(0.70)	(2.49)	(6.33)	(0.49)	(0.58)	(3.41)	(7.31)
Including Business Closures								
Treatment Group	20.24	20.28	0.20		16.12	16.43	1.92	
	(0.67)	(1.49)	(5.94)		(0.72)	(1.31)	(6.56)	
All Controls	15.4	14.88	–3.38	3.57	11.11	10.82	–2.61	4.53
	(0.27)	(0.47)	(2.62)	(6.49)	(0.28)	(0.37)	(2.99)	(7.20)
Alternative Control Samples:								
Small S.F.	6.15	5.84	–5.04	5.24	4.69	4.53	–3.41	5.33
	(0.15)	(0.34)	(4.95)	(7.75)	(0.18)	(0.26)	(5.09)	(8.32)
Unaffected:								
Midsize S.F.	20.64	20.54	–0.48	0.68	15.83	15.01	–5.18	7.10
	(0.60)	(1.62)	(7.78)	(9.80)	(0.67)	(1.00)	(6.39)	(9.18)
Midsize East Bay	20.45	19.66	–3.86	4.06	14.1	13.87	–1.63	3.55
	(0.48)	(0.78)	(3.13)	(6.74)	(0.49)	(0.58)	(4.00)	(7.70)

*Statistically significant at the .10 level; **at the .05 level; ***at the .01 level.

Next, we estimate the effect of the (continuous) treatment intensity measure on growth in employment and full-time equivalent employment in the balanced panel. Table 7 presents the coefficients on the treatment variable in a first-differences model. The dependent variables are differences in natural log of employment and natural log of full-time equivalent employment. For employment, the estimated coefficient (with standard error) is 0.041 (.085) in the full sample (specification 1). Comparing all our alternative control samples (specifications 1, 4, 5, 6), we find the coefficients to vary between 0.026 (considering only unaffected midsize SF restaurants) and 0.044 (considering only San Francisco restaurants). The results are very similar to those from the unbalanced panel, which we do not report here.

For full-time equivalent employment, the treatment coefficient estimate (standard error) is 0.039 (0.097) for the full sample. Across alternative control samples, the coefficients range between 0.033 (considering midsize restaurants only) and 0.135 (considering unaffected midsize San Francisco restaurants only). Again, the hypothesis that the coefficient for the baseline specification is the same as the coefficients for alternative specifications cannot be rejected. This finding suggests that the absence of a statistically significant employment effect is not driven by the choice of control samples.[21]

Turning next to the confidence intervals, we present the bounds on the implied elasticity estimates. With a 26% increase in the minimum wage, the coefficient from our baseline specification (using the full sample) implies a (90% level) lower-bound full-time equivalent employment elasticity of –0.10, a point elasticity estimate of 0.03, and an upper-bound elasticity estimate of 0.17. Comparing all our alternative control samples (specifications 1, 4, 5, 6), we find a greatest upper bound of 0.31 (at the 90% confidence level) and a smallest lower bound of –0.12.

Table 8 compares these point employment elasticity estimate and confidence intervals with others found in recent studies. The bounds around our estimate rule out *both* the Card and Krueger (1994) elasticity estimate (0.81 weighted by initial employment and 0.34 unweighted) and the Neumark and Wascher (2000) elasticity estimate of –0.21. Our baseline confidence interval just barely contains the point estimate obtained by Card and Krueger (2000) using ES-202 data, which was 0.17 using the full 14-county Pennsylvania sample.[22] Our point estimate (0.03) is similar to their elasticity using the original seven counties only (0.05). However, our estimates are more precise, as our confidence interval for the elasticity has a spread of 0.27, as compared to 0.86 in Card and Krueger (1994), 0.56 in Neumark and Wascher (2000), and 0.42 in Card and Krueger (2000).[23]

Table 6. Business Closure Rates.

Sample Description	*Mean*	*Std. Error*
Treatment Group	3.09	(2.16)
All Controls	4.57	(1.33)
Alternative Subsamples		
S.F. Only	5.86	(1.94)
Unaffected Midsize Only	3.58	(1.50)
Minimum Wage Only	3.53	(1.74)

Similarly, the elasticities from the teenage worker studies in Burkhauser et al. (2000) of –0.37 or –0.87 are well outside our confidence interval. This comparison is meaningful since, as noted previously, the minimum wage coverage rates among midsize San Francisco restaurants and teenage workers are quite similar.

Fast-Food versus Table-Service Restaurants

Next, we test for heterogeneity in employment responses by considering fast-food and table-service restaurants separately (see columns 3 and 4 in Table 7). For employment, the coefficient (with standard error) for fast-food restaurants is 0.013 (0.078), and that for table-service restaurants is 0.094 (.139). For full-time equivalent employment, the fast-food coefficient (and SE) is 0.048 (0.106) and the table-service coefficient (and SE) is 0.049 (0.150). After we factor in hours, the responses among table-service and fast-food restaurants seem identical, small, and statistically indistinguishable from zero.

Employment Response in Tourist Areas

The demand for restaurant meals by tourists may be relatively less elastic, leading to a smaller disemployment effect in restaurants serving tourists than in other restaurants. Therefore, we test for differences in the impact of the policy on restaurants that are likely to attract many tourists. Since we do not directly observe the actual composition of customers for each restaurant, we use restaurants' location in the city to predict it. We calculate the ratio of hotel workers to the overall work force in each San Francisco zip code using Zip Code Business Patterns

[21]Anticipated employment changes do not seem to drive our results. Restricting the sample to those interviews completed in January produces a full-time equivalent employment treatment coefficient of 0.015 (0.126) as opposed to 0.039 (0.097) in the full sample (standard errors in parentheses).

[22]Our calculations are based on Card and Krueger's (2000) estimate of a New Jersey employment growth differential of 0.032, a standard error of 0.024, and a 18.82% increase in the minimum.

[23]These comparisons are appropriate because the industries are the same, the definition of elasticity is the same, and the magnitude of the wage change is similar. Card and Krueger and Neumark and Wascher reported OLS standard errors. Since the OLS standard errors in our data are smaller than the robust standard errors, our estimates are even more precise, relative to these previous studies, than the table indicates.

Table 7. Treatment Intensity and ln(Employment) and ln(Full-Time Equivalent Employment): Regression Estimates.

Statistic	*(1)* *Full Sample*	*(2)* *Fast-Food Only*	*(3)* *Table-Service Only*	*(4)* *S.F. Only*	*(5)* *Unaffected Midsize Only*	*(6)* *S.F. and Midsize Only*	*(7)* *Low-Wage Only*
Ln(Employment)							
Treatment	0.041	0.013	0.094	0.044	0.035	0.026	0.029
	(0.085)	(0.078)	(0.139)	(0.095)	(0.088)	(0.109)	(0.085)
N	301	137	164	210	216	125	178
R-Squared	0.03	0.01	0.06	0.19	0.00	0.00	0.03
Specification Test P-Value		0.76	0.40	0.92	0.84	0.80	0.74
Elasticity	0.04	0.01	0.08	0.04	0.03	0.02	0.03
Lower Bound	–0.09	–0.10	–0.12	–0.10	–0.10	–0.09	–0.10
Upper Bound	0.16	0.12	0.28	0.17	0.16	0.12	0.15
Ln(Full-Time Equivalent Employment)							
Treatment	0.039	0.048	0.049	0.077	0.033	0.135	0.016
	(0.097)	(0.106)	(0.150)	(0.108)	(0.101)	(0.136)	(0.100)
N	295	135	160	204	216	125	176
R-Squared	0.05	0.03	0.07	0.20	0.02	0.01	0.06
Specification Test P-Value		0.93	0.89	0.38	0.87	0.19	0.58
Elasticity	0.03	0.04	0.04	0.07	0.03	0.12	0.01
Lower Bound	–0.10	–0.11	–0.17	–0.09	–0.12	–0.08	–0.13
Upper Bound	0.17	0.19	0.26	0.22	0.17	0.31	0.16

Notes: Robust standard errors in parentheses. The columns represent different subsamples of restaurants. "S.F." refers to the San Francisco subsample. "Low Wage Only" refers to the subsample of firms with some workers earning less than $8.50 per hour in wave 1.

*Statistically significant at the .10 level; **at the .05 level; ***at the .01 level.

data, and then consider the subsample in which this ratio is greater than the median, 2.7%, under the assumption that these areas are likely to have heavier tourist traffic than other areas.

These results are presented in Table 9. The treatment effect for the "high-tourism" subsample is 0.042 (0.134) for employment and 0.187 (0.154) for full-time equivalent employment; neither of these is statistically significant. For the subsample of San Francisco restaurants in "low-tourism" zip codes, the effects are 0.057 (0.128) for employment and –0.042 (0.154) for full-time equivalent employment; again, neither is statistically significant. As Table 9 shows, the test of coefficient stability is not rejected, with p-values of 0.887 for employment and 0.838 for full-time equivalent employment. Thus, we do not detect statistically distinct effects on employment among restaurants that are located in a tourist district.

Employer Compliance with Minimum Wage Laws

It is possible that non-compliance with the new wage standard is responsible for the lack of employment effects found above. On the other hand, we already have found that the law had a noticeable effect on wages. Even if many restaurants were non-complying, the policy did raise their wage and was, in that sense, successful. To examine this issue further, we identified restaurants that were likely to have higher concentrations of immigrant workers, since non-compliance is usually more of a concern in these establishments

Table 8. Comparisons of Employment Elasticities and 90% Confidence Intervals.

Study	*Elasticity Estimate*	*Standard Error*	*Lower Bound*	*Upper Bound*	*Spread*
Dube, Naidu, and Reich: Specification 1	0.03	0.08	–0.10	0.17	0.27
Card and Krueger (1994)—Weighted	0.81	0.26	0.38	1.24	0.86
Card and Krueger (1994)—Unweighted	0.34	0.26	–0.09	0.77	0.86
Neumark and Wascher (2000)	–0.21	0.17	–0.49	0.07	0.56
Card and Krueger (2000)—7 PA Counties	0.05	0.14	–0.19	0.28	0.47
Card and Krueger (2000)—14 PA Counties	0.17	0.13	–0.04	0.38	0.42

Note: Robust standard errors in first row, OLS standard errors in remaining rows.

than in others. In San Francisco, these restaurants are located in the Chinatown and Mission District neighborhoods.[24]

Our wage data (presented in more detail in Dube et al. 2006) indicate that the fraction of workers paid under $8.50 fell at the same rate in the Chinatown/Mission sample of treated firms (falling from 49% to 5%) as in the rest of the treatment group (falling from 55% to 6%). If reported truthfully, wages increased substantially in these neighborhoods. However, the wage reports may not be truthful. To assess that possibility, we devise an indirect test. In the competitive model, if restaurants in Chinatown/Mission are less compliant with the new standard than are restaurants elsewhere in the city, they should exhibit less negative or more positive employment effects than the rest of the treatment group. To conduct this test, we split the San Francisco sample accordingly and estimate the treatment effect.

The results, shown in Table 9, indicate that the point estimate of treatment on full-time equivalent employment for Chinatown/Mission is –0.162 (0.211), while the point estimate for the rest of the city is 0.131 (0.126). Neither treatment coefficient is statistically significant. However, the effect is negative for the Chinatown/Mission sample and positive for the others, contrary to the hypothesis that firms hiring immigrant workers do not raise wages or reduce employment. We conclude that lack of policy compliance is unlikely to explain the lack of negative employment response predicted by the competitive model.

In summary, and consistent with some of the recent literature on state-level changes in the minimum wage, our employment estimates are small, positive, and not statistically significant.

Data Quality Tests for the Employment Results

Outliers

In a sample with several hundred restaurants, one may worry about the impact of outlying observations on the estimates. Neumark and Wascher (2000) reported that omitting just a few of the outliers in Card and Krueger's data changed a statistically significant positive employment effect into one that is smaller and no longer significant. To address the outlier issue, we experimented with omitting the largest outliers. The results, reported in Dube et al. (2006), indicate that omitting the outliers has minimal impact on our elasticity estimates.

The unimportance of outliers in our data can also be seen in kernel density estimates of the change in ln(Full-Time Equivalent Employment) and change in ln(Employment), shown in Dube et al. (2006), Figures 1 and 2. These graphs provide the kernel densities for both the treatment and control groups. The means and distributions of these kernel densities are remarkably similar, whether or

[24]According to the 2000 Census, 23% of the residents in the four zip codes corresponding to Chinatown and the Mission District were non-citizens, as compared to 14% in the rest of San Francisco. Chinatown and the Mission District together comprise about 17% of residents in the city and 22% of the San Francisco restaurants in our sample.

Table 9. High Tourism and High Immigrant Worker Subsamples: Regression Estimates.

Sample (within San Francisco only)	*High Tourism Sample*	*Low Tourism Sample*	*High Migrant Sample*	*Low Migrant Sample*
ln(Employment)	0.042 (0.134)	0.057 (0.128)	–0.079 (0.202)	0.069 (0.101)
Probability Value: Specification Test (Ho: High = Low)	0.887		0.870	
ln(Full-Time Equivalent Employment)	0.187 (0.154)	–0.042 (0.154)	–0.162 (0.211)	0.131 (0.126)
Probability Value: Specification Test (Ho: High = Low)	0.838		0.610	

Robust standard errors in parentheses.
*Statistically significant at the .10 level; **at the .05 level; ***at the .01 level.

not the left or right outliers are excluded.

Measurement Error

Neumark and Wascher (2000) also raised concerns regarding measurement error in Card and Krueger's data. Persistence in establishment size will generate a strongly positive correlation between employment in wave 1 and employment in wave 2. If, however, the data contain substantial measurement error, one would expect to find a weaker correlation of employment in waves 1 and 2, and a greater negative correlation between the change in employment and employment in wave 1. (Of course, such a negative relationship might also reflect mean reversion in the actual employment levels.) In the data collected by Card and Krueger (1994), the correlations between levels, and between changes and wave 1, were 0.52 and –0.58, respectively. In Neumark and Wascher's payroll data, these correlations were 0.81 and –0.22, respectively. Neumark and Wascher therefore argued that Card and Krueger's data contained severe measurement error problems, while the payroll data did not.

There are two different issues here. First, although measurement error in the dependent variable does not by itself bias the estimated treatment coefficient or its standard error, it may be indicative of data quality problems. If the survey measures of the outcome variables contain considerable error, the right-hand-side variables may also be measured with considerable error, which can then generate attenuation bias in the estimate. As a result, any greater indication of measurement error might warrant more skepticism concerning data quality and the nature of the findings. Second, in our case, the treatment effect itself is a function of initial employment, a result of the firm size threshold in the minimum wage law's initial coverage. Consequently, measurement error could bias our treatment coefficients downward. Such bias is due to the positive covariance between *measured* employment ($E = E + \varepsilon$) and measurement error (ε); the larger the observed employment, the greater is the extent of measurement error, and the greater the likelihood of observing a spuriously low employment growth. Since firm size in our treatment group is larger than firm size in the overall control group, our estimates of the *relative* employment growth of the treatment group may be affected by such spuriously downward bias.

Table 10 presents the correlations for our employment and full-time equivalent employment measures. As the table shows, the positive correlation between full-time equivalent employment in wave 1 and wave 2 is relatively strong, at 0.72. There is, however, little negative correlation between wave 1 full-time equivalent employment and the change in full-time equivalent employment (–0.11), especially compared to the relationship found in previous studies.[25] These indications of

[25]Dube et al. (2006), Figures 3 and 4, present scatter plots of the full-time equivalent employment wave

Table 10. Measurement Error.

	Employment	*ln(Emp)*	*Full-Time Equivalent Employment*	*ln (Full-Time Equivalent Employment)*	*Card/ Krueger (1994) (Full-Time Equivalent Employment)*	*Neumark/ Wascher (2000) (Full-Time Equivalent Employment)*
Correlation (wave 1, wave 2)	0.72	0.87	0.72	0.79	0.52	0.81
Correlation (wave 1, wave 2 – wave 1)	0.02	0.02	–0.11	–0.16	–0.58	–0.22

lower measurement error of employment give us more confidence in our data quality and our results, especially regarding the range of employment elasticities suggested by our regression estimates.

In Dube et al. (2006), Figure 5, we present a locally weighted regression of change in full-time equivalent employment as a function of full-time equivalent employment in wave 1, separately for the treatment and control groups. The figure shows nonparametrically that for virtually all levels of initial full-time equivalent employment, treated restaurants experienced employment growth equal to or greater than that among the control restaurants. The control group's regression function shows a decline in growth with firm size, indicating that the inclusion of smaller restaurants (not covered by the minimum wage) in the control group would bias us toward finding a disemployment effect when none exists—either due to measurement error or due to actual mean reversion.

Finally, Table 11 provides another approach to checking whether employment effects differ between the treatment and control groups. The table reports the percentage of restaurants reporting full-time equivalent employment loss, no change in full-time equivalent employment, and full-time equivalent employment gain. The distributions are remarkably similar across all the groups, but reveal a slightly higher incidence of full-time equivalent employment gains for the treated firms. Although not reported here, headcount employment exhibits the same pattern.

Of course, we cannot rule out all types of measurement error. But these findings suggest that our data are well-behaved, in the sense of having a limited amount of discernible measurement error. The measurement error criticisms leveled against the Card and Krueger study apply much less to our study and support greater confidence in our findings.[26]

Comparisons with Administrative Data

In this section we compare the employment findings from our survey data findings with the QCEW administrative data we previously presented. As Figure 2 shows, restaurant employment grew by similar amounts in San Francisco and Alameda during 2002 and 2003: restaurant employment growth in 2002 and 2003 averaged 5.5% in San Francisco and 4.2% in Alameda. After the implementation of the minimum wage ordinance in 2004, restaurant employment grew substantially faster in San Francisco (2.1%) than in Alameda (–0.27%). This pattern is especially striking since the QCEW data show that overall private sector employment growth in San Francisco lagged behind that in Alameda over this period.

1 and 2 employment and difference (in natural logs) for our data. These plots provide a visual counterpart to Table 10.

[26]Why would our data be better-behaved? The most likely explanation is that our sample included a fuller range of restaurants, not only by size and type of service, but also including non-chain businesses. Chains often require each of their establishments to maintain identical policies on all matters, which can create clustering and increase the number of observations that are situated off the demand curve.

Table 11. Percent Distribution of Restaurants, by Direction of Change in Full-Time Equivalent Employment.

Sample Description	*Loss*	*No Change*	*Gain*
Treatment	25.0	50.0	25.0
All Controls	25.5	50.6	23.8
S.F. Small	25.3	51.3	23.4
S.F. Midsize	25.4	50.7	23.9
East Bay	25.8	50.0	24.2

These industry-level data do not allow us to discern employment growth for firms in the "treatment group," relative to firms that were already paying above the new minimum wage. Still, overall San Francisco restaurant employment growth in 2004 was 3.81% in our survey data, compared to 2.1% in the QCEW; and –0.39% in our East Bay sample, as compared to –0.27% in the QCEW. This similarity in trends between our sample data and the QCEW data is reassuring with respect to both the representativeness of our data and the reliability of our findings.

Findings for Other Outcomes

We present in Table 12 the descriptive statistics for our other outcome variables—for treated and control restaurants in each wave. These outcome variables include price, tenure, turnover, incidence of full-time work, health insurance coverage, and proportion of workers receiving tips. Below we report the results for the treatment effect on these outcome measures.

Effect on Prices

The survey instrument asked respondents in the first wave for the name and price of the most popular entrée. Price refers to dinner price if the restaurant served dinner, lunch price if it served lunch only, or a general price if there was no distinction. In the second wave, respondents were asked about the price of this same entrée. In case this entrée was no longer served, respondents were asked for (a) the current price and (b) the price in January 2004 (the interview period for the first wave) of the currently most popular dish.

Unlike employment, which the firm chooses, the price may be determined at the level of the product market. For this reason, we begin by comparing relative price changes between San Francisco and East Bay. Table 13 shows that San Francisco restaurants in the sample increased their prices by roughly 2.8%, relative to their counterparts in the East Bay. However, this difference was not statistically significant. Considering fast-food restaurants only, we do find a statistically significant 6.2% rise in prices in San Francisco as compared to the East Bay. To put these findings in perspective, consider that just fewer than 50% of workers in these restaurants were initially paid under $8.50, that the minimum wage increase was 26%, and that labor costs in fast-food restaurants account for 35% of operating costs. Multiplying these three numbers together implies a cost pressure on prices of 4.4%.

In the first panel of Table 14, we investigate whether the price change was proportional to the cost pressures implied by the extent of treatment in an individual restaurant, that is, proportional to the fraction of workers earning under $8.50 initially in the covered restaurants. As the table shows, we find no evidence of such a relationship. Moreover, the treatment variable is not correlated with price growth among either fast-food or table-service restaurants. The coefficients are small and not statistically significant.

Taken together, these results suggest that the price adjustment occurred primarily at the geographical market level (as opposed to the individual restaurant level), and primarily among fast-food restaurants. This pattern would be expected if product markets are competitive, and if the elasticity of substitution between restaurants within broad categories (such as all fast-food restaurants) is high. In other words, prices at fast-food restaurants increased more in *locations* affected by the minimum wage than in unaffected locations, but no similar difference is apparent across affected and unaffected restaurants within the treatment location.[27] Moreover, the price increase (6.2% in Table 13) is comparable to

[27] Card and Krueger (1995) reported similar findings.

Table 12. Other Outcomes: Descriptive Statistics and Simple Difference in Difference.

	Treatment		*Small S.F.*		*Unaffected Midsize S.F.*		*Midsize East Bay*	
Variable	*Wave 1*	*Wave 2*	*Wave 1*	*Wave 2*	*Wave 1*	*Wave 2*	*Wave 1*	*Wave 2*
Price	13.65	13.75	8.72	8.94	12.56	12.68	10.17	10.17
	(0.88)	(0.92)	(0.59)	(0.61)	(1.35)	(1.37)	(0.69)	(0.72)
Separation Rate	0.28	0.29	0.28	0.24	0.27	0.32	0.31	0.29
(annual)	(0.05)	(0.04)	(0.05)	(0.04)	(0.03)	(0.05)	(0.03)	(0.02)
Tenure	14.83	18.5	17.21	21.39	16	16.08	13.69	13.55
(months)	(1.84)	(2.26)	(1.74)	(2.94)	(1.57)	(1.52)	(1.19)	(1.18)
Average Hours—	20.86	21.45	22.26	21.26	21.05	20.83	20.91	21.17
Part-Time	(0.65)	(0.67)	(0.71)	(0.63)	(0.61)	(0.71)	(0.62)	(0.58)
Average Hours—	40.43	39.68	37.8	39.34	39.45	38.74	39.74	39.07
Full-Time	(0.78)	(0.58)	(1.16)	(0.3)	(0.47)	(0.33)	(0.78)	(0.4)
Percent	0.58	0.58	0.56	0.57	0.56	0.5	0.38	0.41
Full-Time	(0.03)	(0.04)	(0.03)	(0.04)	(0.03)	(0.03)	(0.03)	(0.03)
Health Insurance	0.13	0.22	0.16	0.16	0.25	0.2	0.12	0.16
Coverage Rate	(0.03)	(0.08)	(0.04)	(0.04)	(0.04)	(0.03)	(0.02)	(0.04)
Percent Tipped	0.4	0.67	0.3	0.49	0.46	0.58	0.23	0.37
Workers	(0.06)	(0.06)	(0.05)	(0.05)	(0.05)	(0.05)	(0.04)	(0.04)

what would be predicted based on the cost pressures on prices among San Francisco fast-food restaurants.

Tenure and Turnover

An empirical regularity in labor economics is the positive association among lower wages, voluntary quits, and shorter tenure more generally (Brown 1999). More recently, Reich, Hall, and Jacobs (2005) found that turnover among San Francisco airport security screeners fell by 80% when their pay rose from the minimum wage of $5.75 to $10 per hour. (The data for their study were all gathered before the 9/11 attacks.) Card and Krueger (1995) suggested that a dynamic monopsony model applies to fast-food restaurant chains, which exhibit short employee tenure and high rates of voluntary quits. The monopsony model helps explain their finding of a positive employment impact of minimum wage increases.

To study such effects, we included in the survey instrument a question that asked the respondent how long the firm's "typical worker" had been working at the restaurant. The results, shown in the second panel of Table 14, indicate a substantial positive treatment effect of 5.01 months on the tenure of the "typical worker," with significance at the 10% level. Interestingly, this effect was concentrated in fast-food restaurants. The regression coefficient of 6.69 months in the fast-food sample is statistically significant at the 10% level. Since approximately half of the workers in the treated fast-food restaurants were earning under $8.50 in wave 1, this coefficient implies an increase in the tenure of the typical worker in these establishments of roughly three and a half months.[28] The choice of control sample affects these findings somewhat, with the smallest increase being documented when we restrict the sample to all San Francisco restaurants only (specification 4).

Surprisingly, as shown in the third panel of Table 14, we do not detect a reduction in the overall separation rate, defined as the number of workers who stopped working over the past year as a proportion of the current

[28]The finding of greater job stabilization mirrors results from recent studies of living wage policies (Fairris and Reich 2005).

Table 13. Price Growth:
San Francisco–East Bay Differential.

Sample	*Full Sample*	*Fast-Food Only*	*Table-Service Only*
ln(Price)	0.028	0.062**	0.018
	(0.020)	(0.028)	(0.030)

*Statistically significant at the .10 level; **at the .05 level; ***at the .01 level.

work force.[29] This result held even among fast-food restaurants, which did report a rise in the tenure of their "typical worker." It appears, therefore, that the change in the separation rate may have been duration-dependent, *leading to increased heterogeneity in tenure*. In other words, the combination of a fall in the separation rate among workers with initially higher tenure with an increase in the separation rate among the rest is consistent with a rise in the tenure of the "typical worker." Increased heterogeneity may thus reflect an increased segmentation of the work force—a core work force that experiences increased stabilization, and a peripheral work force that perhaps is composed of more temporary workers.

Full-Time Work

The fourth panel of Table 14 shows an increase in the proportion of full-time jobs at fast-food restaurants; this increase was statistically significant at the 10% level. In contrast, we do not detect a significant change in the proportion of full-time workers in table-service restaurants. Approximately 50% of the workers in the treated fast-food restaurants earned below $8.50 in wave 1. Consequently, the coefficient of 0.118 in the fast-food regression equation implies roughly a 6 percentage point increase in the incidence of full-time work in these restaurants due to the policy. Increased full-time work could reflect a move toward workers with more commitment to the job, which is consistent with the finding of increases in job tenure.

[29]Separations result from quits as well as layoffs. We emphasize the quit interpretation here.

Health Insurance

As the fifth panel of Table 14 indicates, we do not detect any compensating differential for the increased wages through reduced health insurance. The treatment effect on health insurance coverage is typically positive and not statistically significant. The coefficient for fast-food restaurants is also positive and not statistically significant, while the coefficient for table-service restaurants is close to zero. This absence of a compensating differential is inconsistent with the competitive labor market model.

Proportion of Workers Receiving Tips

The San Francisco policy applies to tipped and non-tipped workers equally. There is no tip credit, which allows employers in many states (but not California) to count tips as wages in complying with minimum wage standards. In the survey instrument, we asked respondents to report the proportion of their "near minimum wage workers"—those earning below $9.50—that receive tips and the proportion that do not receive tips. We can therefore discern whether the wage mandate generated any change in the tip status of workers in this pay range.

Our regression estimates, provided in the final panel of Table 14, show a substantial and statistically significant rise in the proportion of tipped workers in table-service restaurants. The coefficient of 0.337 in column (3), multiplied by .46 (the proportion of workers in treated table-service restaurants who earned below $8.50 in wave 1), implies that the wage mandate increased the tipped proportion of the "near minimum wage" work force in the treated table-service restaurants by 15.55 percentage points. Since the initial proportion of tipped workers was 37%, the wage mandate increased the proportion tipped by 42% (15.55/37) over its initial level. This increase in the tipped proportion of the work force may result from a compensating differential. In the absence of a "tip credit," wages went up for serving staff in many restaurants. Restaurants likely increased the pool of workers who received some tip income (cooks, dishwashers, and so on) as a way to partly offset the increased

Table 14. Treatment Intensity and Various Outcomes: Regression Estimates.

Statistic	(1) Full Sample	(2) Fast-Food Only	(3) Table-Service Only	(4) S.F. Only	(5) Unaffected Midsize Only	(6) S.F. and Midsize Only	(7) Low-Wage Only
Ln(Price)	0.006	–0.003	0.011	–0.014	0.024	0.020	0.024
	(0.060)	(0.131)	(0.048)	(0.064)	(0.062)	(0.089)	(0.062)
N	274	125	149	193	193	112	127
R-Squared	0.03	0.02	0.02	0.05	0.03	0.01	0.05
Tenure (Months)	5.013*	6.691*	2.150	2.361	7.467***	7.162**	7.005**
	(2.726)	(4.039)	(3.866)	(3.349)	(2.758)	(3.475)	(3.010)
N	293	134	159	205	211	123	134
R-Squared	0.00	0.01	0.01	0.01	0.01	0.01	0.02
Separation Rate (Annual)	0.107	0.026	0.179	0.104	0.091	0.028	0.152
	(0.100)	(0.164)	(0.127)	(0.112)	(0.101)	(0.116)	(0.107)
N	271	120	151	192	190	111	161
R-Squared	0.00	0.00	0.01	0.00	0.00	0.00	0.01
Percent Full-Time	–0.024	0.118*	–0.161	0.011	–0.025	–0.025	–0.061
	(0.069)	(0.068)	(0.100)	(0.079)	(0.070)	(0.070)	(0.074)
N	298	137	161	208	214	214	178
R-Squared	0.02	0.04	0.01	0.00	0.05	0.05	0.05
Health Insurance Coverage	0.193	0.518	–0.020	0.243	0.181	0.304	0.184
	(0.200)	(0.501)	(0.088)	(0.204)	(0.206)	(0.220)	(0.201)
N	264	111	153	187	187	110	157
R-Squared	0.00	0.02	0.00	0.01	0.01	0.02	0.01
Fraction Tipped	0.267*	0.068	0.337*	0.262	0.284*	0.326*	0.189
	(0.154)	(0.252)	(0.190)	(0.168)	(0.157)	(0.173)	(0.162)
N	296	137	159	206	214	124	178
R-Squared	0.05	0.02	0.1	0.04	0.05	0.06	0.07

Notes: Robust standard errors in parentheses. The columns represent different subsamples of restaurants. "S.F." refers to the San Francisco subsample. "Low Wage Only" refers to the subsample of firms with some workers earning less than $8.50 per hour in wave 1.

*Statistically significant at the .10 level; **at the .05 level; ***at the .01 level.

income of the wait staff. Providing some tip income to cooks, for example, would mean they would require a lower base wage.[30] The muted increase in the average income in table-service restaurants is also consistent with this finding, which would be predicted by a competitive model.

Summary and Conclusions

We find that the San Francisco wage floor policy increased pay significantly at affected restaurants and compressed the wage distribution among restaurant workers. The policy increased average pay by twice as much among fast-food restaurants as among table-service restaurants. We do not detect any increased rate of business closure or employment loss among treated restaurants; this finding is robust across a variety of alternative specifications and control subsamples.

Our employment findings imply elasticity estimates and confidence intervals that rule out some of the larger negative and positive effects in the recent literature, including Card and Krueger (1994), Card and Krueger (2000), and Neumark and Wascher (2000). Moreover, our confidence intervals are considerably narrower. Our investigation

[30]Our data do not permit examination of pay trends by occupational category.

into possible outlier effects or measurement error in our data revealed far less of these than have some previous studies, suggesting that minimum wage effects can be detected with considerably more precision than some have suggested.

Changes in menu prices, proportion of full-time work, and employee tenure differed significantly between fast-food and table-service restaurants. At fast-food restaurants, but not at table-service restaurants, prices, employment of full-time workers, and employee job tenure all increased significantly. These findings, taken together with the greater wage effects among fast-food restaurants, suggest that the policy improved job quality and worker attachment in this restaurant segment and reduced the gaps in job quality between the two types of restaurants. We did find that table-service restaurants increased their use of tipped workers, a finding consistent with a change in either job quality or skill intensity in that sector.

Regarding other potential avenues for employment effects, we also found that restaurants in tourist areas did not experience outcomes different from those not in tourist areas. Moreover, with respect to policy compliance and employment effects, restaurants in immigrant-intensive areas were no different from restaurants elsewhere in San Francisco.

Our results provide support for each of the labor market models discussed earlier in this paper. The combination of no negative employment effect and the presence of a small positive price effect is consistent with a competitive model, in which small price increases do not significantly reduce product demand. The absence of any reduction in health insurance coverage among affected restaurants is not consistent with the compensating differences predictions of a competitive model. At the same time, the increase in the proportion of tipped workers in table-service restaurants suggests a compensating difference that spreads tips among more workers. The increases in job quality and in job attachment among fast-food restaurants are consistent with a recruitment-retention model, in which wage floors induce higher-quality jobs and more experienced workers in affected restaurants.

Can San Francisco's experience be generalized to other cities? In composition, San Francisco's low-wage industries are similar to those in other major U.S. cities, suggesting that the policy effects are not likely to be artifacts of any peculiarities of the city's labor market. The United States appears to be moving toward a more decentralized system of wage determination, with greater variation in wage floors among both states and cities. Further work will be needed to improve our understanding of the effects of such local wage mandates, spillover effects across cities, and possible heterogeneity in responses to such mandates.

Appendix
Sample Response Rates

Response Rates by Location		
East Bay	40%	
San Francisco	37%	
Total	38%	
Employment Levels of Respondents and Non-Respondents, by Firm Size and Location		
	Respondents[a]	*Non-Respondents*[b]
Midsize East Bay	20.6%	20.6%
Midsize San Francisco	20.3	20.4
Small San Francisco	5.8	5.5

[a]Respondent employment is based on actual survey data.

[b]Non-respondent employment is based on Dun and Bradstreet employment data, conditional on size screens (the size screens are "small"—between 4 and 8—and "midsized"—between 14 and 35).

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