Minimum Wage Channels of Adjustment

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Abstract: The effects of minimum wage increases in 2007-2009 are analyzed using a sample of restaurants from Georgia/Alabama. Store-level payroll records provide precise measures of compliance costs. Examined are multiple adjustment channels. Exploiting variation in compliance costs across restaurants, we find employment and hours responses to be variable and in most cases statistically insignificant. Channels of adjustment to wage increases and to changes in non-labor costs include prices, profits, wage compression, turnover, and performance standards.

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Introduction

The minimum wage (MW) and its effect on employment is the most researched and debated policy issue in American labor economics (Hamermesh 2009).[1] At first glance, this seems odd. The U.S. minimum wage is low by international standards, currently covers a tiny percentage of the workforce, and in real terms is not high compared to historic values. Economists remain fascinated with the minimum wage, however, partly because controversy continues on its empirical effects and, also, because the subject provides an opportunity to test some of the field's most basic models and propositions. We contribute to this debate by shifting analysis to what we call MW "channels of adjustment" (CoA). The CoA approach examines MW empirical effects along multiple dimensions, with an eye toward adjustment channels in which competing models can yield different predictions.

The event examined is the three-step increase in the U.S. minimum wage, from $5.15 to $5.85 in July 2007, to $6.55 in July 2008, and to $7.25 in July 2009, an increase of $2.10 (41%). Among the CoA examined are employment, hours, prices, profits, training, work effort, human resource practices, operational efficiencies, and internal wage structure. The empirical evidence comes from a unique multi-part data set collected for quick-service restaurants located in Georgia and Alabama and owned by three franchisees. Our investigation starts with the most intensively examined adjustment channel, changes in employment and hours. We rely on large exogenous variation across the restaurants in the "bite" of the MW to identify causal effects on employment and hours. Although the data set is neither large nor problem free, it nonetheless provides advances in several respects. First, our measure of the MW payroll "gap" (the treatment) is calculated from *individual* worker payroll data provided by the franchisees, an improvement over store-level averages or industry-level aggregates used in other studies (Card and Krueger, 1994; Dube *et al.*, 2007), allowing us to measure precisely each restaurant's compliance costs resulting from each MW hike. Second, the data are from store-level electronic payroll records and thus relatively free of measurement error. Third, the data extend over three years, containing information before and after each of the three MW increases, and thus reflect both short- and medium-run adjustments.

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[1] A July 2013 electronic search of the *EconLit* data base on the subject "minimum wage," limited to journal articles since January 2000 and North America, brings up 191 research studies (plus 28 more for living wage).

Information on other channels of adjustment comes from data provided by franchise owners, a separately administered written survey of restaurant managers, and qualitative/anecdotal data collected from field-level interviews with restaurant owners and managers. We also construct a statistical profile of workforce characteristics based on a survey of individual employees. In what follows, we describe data sources, survey instruments, estimation strategies, evidence on employment and non-employment adjustment channels, and implications for MW analyses. Given space constraints, we focus throughout on empirical analysis and only at the end, and then very briefly, consider theoretical implications.

Empirical Analysis: Setting the Stage

For space reasons the voluminous literature on the MW is not surveyed, apart from a broad overview of issues related to estimation of MW employment effects. The early MW literature consisted primarily of national time-series studies examining employment responses to changes in the federal MW. Such studies typically found negative but small teenage employment elasticities with respect to the MW, on the order of -0.1 to -0.3 with more recent studies indicating the lower elasticities (for surveys, see Brown et al. 1982; Brown 1999; Card and Krueger 1995; Neumark and Wascher 2008).

More recent studies rely on cross-sectional as well as time variation in the MW. Cross-section variation stems from two principal sources. One is the difference in prevailing wages across markets; hence MW has greater "bite" in low-wage markets. The second is growth in the number of states with state minima exceeding the federal minimum. Thus, introduction of federal MW increases should not impact (or barely impact) markets with high state minima, while having larger impacts on other states.

Two broad groups of cross-section, quasi-experimental studies have emerged. The first uses the Current Population Survey (CPS) or other national survey data to examine how employment responses vary with exposure to state and Federal MW laws. Applying panel techniques to estimate employment effects for teenagers or low-skilled workers, these studies typically (but not always) obtain evidence of adverse employment effects, with employment elasticities on the order of -0.2 to -0.3 (Neumark and Wascher 2007; Burkhauser *et al.* 2000; Sabia 2009; see Orrenius and Zavodny 2007, for an exception).

Recent nationwide MW studies focus on controls for unobserved heterogeneity in employment

growth and the presence of spatial correlation across states (Allegretto et al. 2011) and state borders (Dube et al. 2010). Addison et al. (2012) examine county-level employment in the restaurant-and-bar sector from 1990-2006 and include trends in employment. These studies find minimal MW employment effects, concluding that negative effects found in prior studies result from heterogeneity in employment trends. Addison et al. (forthcoming) examine the 2007-2009 MW increases using three national data sets and find little evidence for employment effects or a "recession multiplier" of such effects. Neumark et al. (2013) sharply question results from these studies, arguing they are sensitive to specification and that in avoiding one potential set of problems, researchers are discarding much of the information that can provide valid identification of MW effects. Allegretto et al. (2013) respond to these criticisms.

A second group of cross-section studies, of which our work is an example, uses either a unique data source with special advantages and/or data that permit study of MW differences in nearby markets. Best known is Card and Krueger (1994), who uncovered small positive or insignificant employment effects in a sample of fast-food restaurants in New Jersey where the MW was raised through state law, relative to the stores in nearby Pennsylvania where the MW did not change. Studies following in this tradition, typically have found small and insignificant employment effects from the MW. For instance, Dube *et al.* (2007) investigate the effects of a citywide minimum wage in San Francisco, relative to the neighboring Alameda County, and do not detect any significant employment loss attributable to the mandate.

In a paper more closely related to our study, Giuliano (2013) uses 1996-1998 individual employee personnel records from 700 stores of a nationwide retail chain, exploiting geographic variation in state minimum wages and the employment impact of federal minimum wage increases.[2] Consistent with dynamic monopsony theories linking higher wages to fewer vacancies, she finds little evidence of negative employment effects, but does find differential employment responses for types of workers (teenagers versus adults) and across geographic areas (high versus low-income areas). While the MW had little effect on adults, it increased teen labor force participation and attracted more qualified teens,

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[2] In contrast to our study, Giuliano (2013) has a large nationwide sample that is company rather than franchisor owned. She does not have information on individual hours worked.

increasing their employment at the chain's stores.[3]

Despite an extensive body of empirical work of increasingly high quality, there is still considerable disagreement over the sign and strength of MW employment effects. Although we do not "solve" the puzzle, we move the literature forward through a focus on multiple channels of adjustment.

Data and Sample Description

We use complementary data sets for a sample of 81 Quick-Service restaurants (QSRs) in Georgia and Alabama. The primary data used to investigate employment effects come from restaurants' biweekly electronic payroll records on individual employees, collected by the authors for the period January 2007 through December 2009. Because the QSR sector has a low-wage workforce and neither Georgia nor Alabama has a binding state MW law, these restaurants are good candidates for investigating the effects of the three-step federal minimum wage increases. If MW laws have substantive and relatively uniform effects on employment, these should be evident among the most-affected businesses in our survey. Other MW channels of adjustment are explored qualitatively using our survey of store managers, which in turn is supplemented by data obtained from confidential employee surveys and information from semi-structured interviews with store owners and a sample of managers.

Payroll Data

The restaurants in our sample belong to a national fast-food chain and are operated by three franchise owners who agreed to provide payroll data for our study under condition of strict confidentiality.[4] Although our sample is non-random, it is likely to be representative since the products offered at fast-food restaurants are uniform and employees' skill sets are highly similar. Sampled establishments display considerable geographic and city size variation: 20 of our 81 restaurants are located in 12 Eastern Alabama counties close to the Georgia border and the rest are located in 23 Georgia counties scattered across Central and Southern Georgia; likewise, some restaurants are in small rural communities or along interstate highways while others are in medium or large size cities. The spatial differences provide

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[3] A similar result is in Ahn et al. (2011). The authors develop a search model with endogenous labor supply. They find evidence that employment for teens from more-privileged families pushes out those less privileged.
[4] We refer to "owners" throughout the paper; however, one of the three was the chief operating officer rather than owner. Attempts to gain data from other franchise owners and the national chain were unsuccessful.

variation in the expected impact of the minimum wage across stores and time periods.

Electronic payroll data provide the following information for each worker for two pay periods per month: restaurant I.D., individual worker I.D., job title, regular hours worked and regular pay, overtime hours and overtime pay, and total pay. The straight-time wage rate was reported directly or calculated by dividing regular pay by regular hours. Payroll data for managers were not available and they are excluded from our analysis. Fringe benefits for hourly employees were close-to-nil and are not analyzed.

We have complete payroll records for establishments over three years (72 "biweekly" pay periods), commencing in January 2007.[5] Six stores enter our sample later (one store opened in May 2007 and one in January 2008; four more stores were acquired by the owner in May 2007). None of the stores went out of business during the study period. Also provided were data on monthly percentage changes in sales.[6]

Pay setting and employment at restaurants follows a two-tier process. Owners establish the basic parameters of pay policy for all stores as a group, principally by giving each store manager a constraint on total labor cost as a percent of sales. Each manager then works out a starting wage and wage hierarchy that stays within the target while fitting in with local labor market conditions. Owners, as a first principle, prefer to enforce the target while letting managers search out their own solutions for hiring, scheduling, turnover, filling vacancies, and morale problems. Nonetheless, owners allow case-by-case deviation from the target if warranted by the local labor market.

Descriptive statistics for payroll records data are presented in Table 1. The sample contains about 64,000 individual-level observations on biweekly pay. The average hourly wage for the first year is $6.28, increasing to $6.68 and $7.15 during the latter two years (January-to December, with MW increases on July 1).[7] Regular hours worked are stable across the study period. Overtime work declined; the share of employees with overtime decreasing from 12% in 2007 to 7% in 2009 and average overtime falls from

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[5] Although we refer to the payroll records as biweekly, there were two per month with slight variation in the number of days, but averaging just over 15.
[6] We requested data on the monthly *level* of sales but for confidentiality reasons two owners provided only *changes* in sales. Monthly percent changes in sales are transformed into log points to make them additive over time.
[7] None of the stores utilized the youth or training minimum wage, which sets a lower minimum wage for new employees under age 21 for the first 60 to 90 days of employment.

0.74 to 0.45 hours.[8] About 15% of our restaurants (12 of 81) are located in the Atlanta metro area, where wages and cost of living are higher. These restaurants have fewer employees per establishment, but they have higher wages, longer hours, and are more likely to be full time.[9]

Although limited in its geographic focus, our data set possesses several advantages. First, hours and wages are measured at the individual rather than establishment level. The data allow us to construct precise measures of each restaurant's compliance cost from the MW, referred to as *GAP*, since we know each worker's wage at the time of the MW increase. A second advantage is the presence of information on regular and overtime hours worked. Finally, payroll data should be accurate because they are collected for tax-reporting reasons. The payroll data were recorded prior to and independent of our research.

Payroll data are supplemented with county-level data from the *Quarterly Census of Employment and Wages* (QCEW), produced by the Bureau of Labor Statistics (BLS). Measures from the QCEW are used to control for business and labor market fluctuations at the local level. We extract 2007-2009 data on total employment (as well as number of establishments and wages) for all industries and then separately for *Accommodation and Food Services* (NAICS sector 72) and *Retail Trade* (NAICS 44-45). We also compile data at the 3-digit (*Food Service and Drinking Places*, NAICS 722) and 4-digit (*Limited-service Eating Places*, NAICS 7222) levels. Population estimates from the U.S. Census Bureau are used to compute annual population density at the county level.

Manager and Employee Surveys

To examine a broader range of firm behavior, we use data collected from written surveys of managers and a survey of employees (not seen by employers). Questionnaires were administered in mid-July through early-August 2009.[10] The manager survey response rate was 81% (66 of the 81 managers) and employee response rate 62% (1,649 of 2,640 returned surveys that answered at least one question).

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[8] A decline in overtime hours can result from a business downturn or as a response to minimum wages. If average hourly earnings and hours are determined jointly in an implicit contract, then a MW mandate raising the straight-time wage would reduce use of overtime hours in order to (roughly) maintain average hourly earnings. For theory and evidence, see Trejo (1991) and Barkume (2010).

[9] In June 2009, prior to the July 1 MW increase from $6.55 to $7.25, the mean hourly wage for workers in the Atlanta sub-sample was $8.03, compared to $6.94 for hourly employees elsewhere.

[10] The questionnaires are available from the authors on request. The employee surveys were anonymous and cannot be matched to individual payroll records.

Managers first were asked a series of open-ended questions about cost-saving strategies in different areas of business operation, including human resource (HR) practices, operational efficiency, non-labor costs, and customer service. The goal was to let managers express their own views about the MW mandate. For a second section of the survey, we designed a comprehensive list of possible cost-saving responses to the MW. The list was based partly on alternative theoretical models and partly on face-to-face discussions with managers and franchisee owners. The goal was to document which internal adjustments used by managers might be most effective in mitigating cost increases from the MW.

A portrait of employee demographics can be seen in Table 2, based on the separate survey given to employees (they can be linked to their store but, for confidentiality concerns, not to their individual payroll records). Hourly employees are disproportionately female (66%) and African-American (64%). Relatively few are Hispanic (8%). The average age is 28 and only 23% are teenagers. Respondents report low family incomes; 38% with annual family income less than $10,000 and an additional 26% with income between $10,000 and $20,000. Other worker attributes can be seen in the table.[11]

Minimum Wage Compliance Costs and Descriptive Evidence from Payroll Records

In this section, we describe our measure of restaurants' MW compliance costs, followed by presentation of descriptive evidence relating these costs to changes in wages, employment, and hours.

Measuring MW Compliance Costs (GAP)

Our identification strategy uses the minimum wage policy change and its effect on the establishment-level wage bill as a source of exogenous variation. In order to identify the effect of MW increases on wages, employment, and hours worked, we construct the variable *GAP*, a measure of MW compliance costs. Although other studies have used related measures (Card and Krueger, 1994; Katz and Krueger, 1992; Dube et al., 2007; Giuliano, 2013; plus earlier studies cited in Brown et al., 1982), our measure has advantages due to the availability of store payroll data on individual workers' wages and hours.

Specifically, GAP_{jt} is constructed using data from individual worker i at restaurant j at pay period t.

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[11] Although economists are often opposed to raising the minimum wage, these employees are strongly supportive. The employee survey asked respondents if they would have voted 'Yes' or 'No' to raise the MW from $6.55 (2008) to $7.25 (2009). Nine out of ten marked 'Yes'.

It measures a restaurant's compliance costs by the log change in unit *j's* wage bill resulting from a MW increase, assuming individual workers' hours *h* stay fixed between periods *t-1* and *t* (before and after the minimum wage increase). The *GAP* for restaurant *j* at time *t* is defined as:

$$\text{GAP}_{jt} = 1 + [(\Sigma \text{MW}_{ijt}h_{ijt-1} - \Sigma W_{ijt-1}h_{ijt-1}) / \Sigma W_{ijt-1}h_{ijt-1}],$$

where the bracketed term is summed over *i* workers for whom $W_{ijt-1} < \text{MW}_t$ (i.e., those for whom MW_t is binding) while set at 0 for workers for whom $W_{ijt-1} \geq \text{MW}_t$. The numerator in the bracketed term is the change in the wage bill between time periods t-1 and t if hours remain the same, while the denominator is the initial wage bill. *GAP* is calculated by summing up each employee's additional earnings (wages times hours) required for MW compliance. Adding 1 to the bracketed term converts the measure from a proportion to a wage ratio (say 1.10 or 10%). The natural log of *GAP* is a "proportion" (e.g., ln(1.10) = 0.095) based on an intermediate base between the initial wage and subsequent minimum wage. If restaurant *j* in period *t-1* were paying all employees above the new MW effective in period *t*, then *GAP* = 1 (and ln*GAP* = 0). We use ln*GAP* in double-log models to estimate employment and hours elasticities with respect to *GAP* and, separately, to instrument the wage and obtain wage elasticity estimates.

Table 3 presents summary statistics for *GAP* expressed as a percentage (by subtracting 1 and multiplying by 100), defined at the establishment level as an average over March-May of each year prior to the July 1 minimum wage increases. As larger shares of workers were affected each year, the compliance cost of the MW grew, from 2.6% of payroll as a result of the July 2007 MW, to 4.6% in 2008, and to 6.8% in 2009. Total payroll increases realized, holding hours constant, were 6.0%, 6.6%, and 7.9%.

Our gap measure is similar but not identical to those in prior studies. In their seminal paper, Card and Krueger (1994) define *GAP* as a proportional increase in the store's initial *starting* wage necessary to meet the mandated rate. According to their definition, *GAP* reduces to the wage ratio of the new minimum wage rate and the wage for a new employee several months before the increase (MW_t/W_{t-1}). This variable provides imperfect information on compliance cost since one does not know how many workers are impacted by the MW or by how much, nor does one know the work hours over which employees will be awarded the higher wage. Rather, Card and Krueger's "wage gap" is a proxy for a

relatively high- or low-cost store location. Recognizing such issues, Dube *et al*. (2007) define their measure as a share of workers affected by the MW increase, as seen previously in Card (1992) (we include this measure in Table 3). Although the share measure accounts for the *quantity* dimension of the MW (i.e., the number of workers whose wages must rise), not accounting for the *price* dimension (i.e., by how much wages must increase) may generate an imprecise measure of the cost shock from a MW mandate. Similarly, using establishment-level averages of wages and hours may provide a noisy measure since workers with wages well above the new MW would effectively "cancel out" affected workers (i.e., businesses with equivalent average wages will have different compliance costs). Since our *GAP* measure is based on individual data we are able to capture both quantity and cost per employee.

Descriptive Evidence on Wages, Employment, and Hours Worked

Evidence on how wages, employment, and biweekly hours varied with respect to the minimum wages is visually summarized in Figures 1 and 2. Figure 1 shows how wages, employment, and hours varied over the three year period among groups of restaurants least affected, moderately affected, and most affected by MW increases. Figure 2 shows the size of long-run (early 2007 to late 2009) individual restaurant changes in wages, employment, and hours with respect to their MW compliance costs. Both sets of figures illustrate clear-cut systematic effects of the MW mandates on wages, coupled with responses in employment and work hours that are variable and far less systematic.

Average employment changes across all establishments would mask variation associated with differences in pre-MW wage levels. In Figure 1, establishments are divided into three groups – those least, moderately, and most affected by the MW increases based on the size of *GAP* in March-May 2007, prior to the initial July 2007 increase. The "least" affected group represents the lowest 25 percentiles of *GAP* and includes all zero *GAP* stores (no compliance costs from the 2007 MW). The "moderately" affected group has *GAP* values between the 25th and 75th percentiles of *GAP*, while the "most" affected group has *GAP* compliance costs in the 75th and over percentiles.

Figure 1 provides plots of biweekly establishment-level averages in observed hourly earnings and employment over the three year period, plus average daily regular and overtime hours. In panel A, there

are three distinct jumps in average hourly earnings in July of each year, coinciding with the three MW hikes. The least affected group of stores is largely unaffected since the average wage there was slightly above $7 during the whole study period. Importantly, there is no evidence of pre-adjustments in hourly wages prior to MW increases (confirmed by owners). Small increases around January-February in 2007 and 2008 are attributed to performance-based raises for a portion of the workforce. The final MW increase in July 2009 has the strongest effect across all three groups. The three federal MW hikes had substantial exogenous effects on average hourly earnings and the intensity of these treatments differed substantially across restaurants. Estimates of arguably causal employment and hours effects can be based on outcome differences between restaurants facing different compliance costs.

Panels B-D of Figure 1 show differences in outcomes between the least, middle, and most affected restaurants. Panel B presents average employment. Despite jumps in the average hourly earnings observed in Panel A, we do not observe reductions in employment that correspond with the size of compliance costs. Overall patterns appear roughly similar among the three groups. Although we can see a small decline in employment after the July 2008 hike, employment is fairly stable both within and across the three groups of establishments facing varying cost "shocks." The least affected stores have fewer employees on average due to greater use of full-time workers among Atlanta-area establishments. Some seasonal fluctuations in the average employment are also evident; there are systematic increases in employment (and turnover) twice a year (in June-July and December-January), which owners attribute to long vacation leaves (some of which show up as turnover in our data) and voluntary turnover.

Panel C shows average daily regular hours per worker across the three groups of establishments.[12] Although regular hours fluctuate on a pay-period basis, there is no apparent trend in average hours worked over the study period, nor are there apparent differences in trend among the three groups. Panel D shows overtime hours. Although average daily overtime is minimal (a small fraction of an hour), there are differences among the three groups of restaurants. In the two most affected groups, overtime is practically non-existent, but is higher on average in the least affected group where there are more full-time workers.

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[12] Average daily hours are shown to account for slight differences in length of the twice a month payroll periods.

There is a decline over time in overtime hours, due primarily or in part to weak business conditions.

Figure 2 provides panels showing three-year "long-run" changes in wages, employment, and total hours (regular plus overtime) with respect to the cumulative three-year compliance costs (the sum of ln*GAP*). As seen in the figures, restaurants' three-year compliance costs varied from close to zero to about 25%, with a sizable share of the restaurants having wage bill mandates between about 13 and 23 log points. In each panel, a nonparametric curve (using smoothed local regressions) is fitted to the array of restaurant data points.[13] As expected, the wage-GAP relationship is upward sloping with a tight fit between changes in ln*W* and ln*GAP*. As seen previously in Table 3, increases in restaurants' total wage bills exceeded legally required increases, but not by much.

In contrast to the pattern on wages, panels B and C of Figure 2 indicate little systematic relationship between MW compliance costs and the changes in employment and total hours. Rather than sloping downward with respect to costs, changes in employment average close to zero across the range of compliance costs. For total hours (panel C), the businesses with the highest compliance costs (about 18+ log points), one sees a corresponding decline in hours, but throughout the rest of the distribution (gaps below 18 log points) one sees more of a positive than a negative hours-wage relationship.

The graphical analysis shows that despite substantial differences in wage gains due to the 3-step MW increases, there are rather limited differences in the employment or hours responses between those restaurants most and least affected by the mandates. Below we examine MW effects on employment and hours within a formal regression framework.

Estimating MW Employment and Hours Effects From Payroll Records

The empirical analysis of MW effects on employment and total work hours uses biweekly establishment averages created from the payroll data. We initially estimate the following reduced-form equations, estimated separately for each of the three MW increases, with MW effects based on restaurant-specific compliance costs of the MW (i.e., ln*GAP*). We then estimate "structural" equations in which log wages are instrumented by the ln*GAP* prior to each MW increase. Using a similar approach, we then

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[13] The curves are fitted from locally weighted regressions using the "lowess" command in Stata (Cleveland 1979). Figures showing untransformed rather than log values of wages, employment, and hours are highly similar.

provide estimates of MW employment and hours effects over the full three year period.[14]

(1a) $\ln E_{jct} = \alpha + \psi \ln GAP_{jt} \cdot MW_t + \tau \ln GAP_{jt} + \delta MW_t + \zeta \Delta \ln Sales_{jt-1} + \gamma Z_{ct} + \theta F_j + \varepsilon_{jt}$

(1b) $\ln h_{jct} = \alpha + \psi \ln GAP_{jt} \cdot MW_t + \tau \ln GAP_{jt} + \delta MW_t + \zeta \Delta \ln Sales_{jt-1} + \gamma Z_{ct} + \theta F_j + \varepsilon_{jt}$

The outcome variables are $\ln E_{jt}$, the log of the average employment in store j during period t (biweekly),

and the log of aggregate hours h_{jt}, the sum of regular plus overtime hours. MW_t is the time treatment

period set to 1 for the 5-6 months *after* each MW increase (i.e., August–January in 2007 and 2008;

August–December in 2009). We provide estimates with and without store fixed effects; the fixed effects

F_j absorb any impacts specific to the business owner and county location, as well as the standalone store-

level measure, $\ln GAP_{jt}$. The error term is ε_{jt}.

The key variable is the interaction term $\ln GAP \cdot MW$, whose coefficient ψ measures the impact of the

cost increase from the MW mandate on establishment employment (hours) averaged over the months

following the increase. The parameter ψ provides a measure of the employment (hours) elasticity with

respect to the exogenous required wage change. We next estimate a structural model that includes the

predicted wage; $\ln GAP$ proves to be a strong instrument (F test>100). Based on standard models,

competitive theory predicts $\psi < 0$, while monopsony and institutionalist/behavioral theories allow for

values of ψ that are negative, near zero, or positive (e.g., Manning 2003; Kaufman 2010).[15]

Regressions without store FE include $\ln GAP_{jt}$ entered separately, which controls for differences in

employment (hours) levels between high- and low-impacted restaurants prior to the MW mandate.

$\Delta \ln Sales_{jt-1}$ is the lagged monthly change in log sales, included to reflect demand shocks (reflected in

prices, product mix, and transactions) not captured by other controls. The lag enables us to exclude the

current month and avoid simultaneity between concurrent employment and sales. The vector Z_{ct} includes

time-varying county-level characteristics reflecting labor market supply and demand factors; specifically,

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[14] This approach is similar to that used by Card (1992), whose state-level analysis presents both reduced form employment estimates, based on each state's fraction of teens affected by the MW, and structural estimates, in which the fraction affected by MW is used to instrument wage change.

[15] Elasticity estimates in the MW literature are far smaller than are elasticity estimates in the larger labor demand literature (Hamermesh 1993). This is not surprising given that a small proportion of workers (including teens) are affected by any given MW increase and, even among those affected, compliance costs are low for those whose current wage is close to the mandated wage. Estimates of ψ from our structural model can be interpreted as wage elasticities; those from the reduced-form model approximate elasticities to the extent that wage increases mimic legally required increases. Thus, elasticity estimates here are potentially larger than in much of the MW literature.

county-level population density and total private sector employment minus employment in the *Accommodations and Food Services* and *Retail* sectors.

A few econometric issues warrant mention. The error term is likely to be correlated for each restaurant over time; hence, all standard errors are clustered on individual stores. Apart from clustering, some degree of independence in employment, pay, and price setting across each owner's stores mitigates concern about franchise-specific effects that would overstate the precision of our estimates (Donald and Lang 2007). The possibility of heterogeneous trends across stores is a potential source of bias if correlated with ln*GAP*, a concern mitigated by controls for time-varying county-level demand-supply shifters.

Regression Evidence

The models estimating MW effects on wages, employment, and hours for each of the three MW increases are shown in Table 4. Average wages, employment, and hours are measured in each biweekly pay period for the six months after (five months after for 2009), with the MW_t treatment set to 1, and for the six months before each of the three increases ($MW_t = 0$). Store-specific MW compliance costs are accounted for by *GAP*. Estimates of φ, the coefficient on ln*GAP·MW* are shown in line 1 of the tables and provide estimates of the wage, employment, and hours *GAP* elasticities. Panel A of Table 4 provides results based on the 2007 MW increase, panel B the 2008 increase, and panel C the 2009 increase. Each outcome equation is shown first with no controls, next with controls other than store FE, then with store FE, and then using IV. Because lnGAP is such a strong predictor of store-specific wage changes, the structural IV results, which are our preferred results, are highly similar to OLS. As one might expect based on descriptive evidence seen in Figures 1 and 2, we find minimal evidence for negative MW employment and hours effects. In 2007 and 2008, elasticity estimates are small, mixed in sign, and never statistically significant, while in 2009, we obtain positive elasticity estimates.

Focusing first on the 2007 MW increase, wage elasticities with respect to *GAP* are close to one and precisely estimated, the estimate with store FE being 1.1. The employment and hours elasticities with respect to *GAP* are close to zero and not precisely estimated. The employment point estimate for the OLS model with store FE is 0.10, while the IV estimate using the predicted wage is 0.09, each with standard

errors close to 0.5. The hours point estimate using OLS and store FE is -0.19 and the IV estimate is -0.16; neither is close to statistical significance.

For the 2008 MW increase, wage elasticities are a precisely estimated 1.1, but the employment and hours estimates are again small and imprecisely estimated. The OLS and IV employment elasticity estimates with store FE are -0.29 and -0.26, while the corresponding total hours estimates are 0.15 and 0.13. Given the large standard errors and the flipping of signs between 2007 and 2008, we do not attach weight to finding different signs for employment and total hours, an outcome that could occur were there meaningful changes in hours per worker.

In 2009, the wage-*GAP* elasticity is 0.9. In contrast to 2007 and 2008, we find more substantive and significant employment/hours elasticity estimates. Both set of estimates are positive, however, approximately 0.7 for employment and 0.5 for total hours. As mentioned previously (fn. 15), these elasticities are with respect to the required compliance costs or, using IV, the wage, and thus are expected to be larger in magnitude than are teen elasticities common in the literature.

In Table 5, we examine "long-run" MW employment and hours effects based solely on information before and after the three MW increases. Our priors are that within-store employment and hour adjustments can occur quickly. However, those believing such adjustments require considerable time might place greater weight on the long-run results.[16] In keeping with the approach used in Table 4, we include two observations for each restaurant, average employment (and total hours) in January-March 2007 and Oct-December 2009, and then include store fixed effects. The ln*GAP* measure represents the cumulative *GAP* (compliance costs) from the three MW increases, while the dummy *MW* is set to 1 for end-period observations. The coefficient on ln*GAP·MW* provides the elasticity estimates; ln*GAP* is absorbed by store FE. The wage and gap elasticity estimates with store FE shown in Table 5 are identical to those obtained using a simple difference model with one observation per restaurant.

As seen in Table 5, the three-year employment and hours elasticity estimates are positive, but not

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[16] In the concluding section, we briefly discuss long-run adjustments involving restaurant closures and entry. Walter Wessels has suggested to us that it is difficult a priori to determine the timing of employment responses to a multi-part MW increase. Firms can evaluate the present value of future MW increases while changes in staffing can either lead or lag wage changes, making longer-run estimates preferable.

precisely estimated. Our preferred employment elasticity, from the structural IV model, is 0.39 (the OLS estimate is 0.42). The IV hours elasticity estimate is 0.28, marginally significant at the .10 level (the OLS estimate is 0.29). Given the descriptive evidence seen in Figure 2, coupled with results shown in Table 4 for the annual MW increases, these long-run results are not surprising.

The pattern seen in Figure 2 between the three-year change in employment and GAP (or wages) suggests that the relationship may be nonlinear , consistent with a monopsony explanation. We estimate an added nonlinear specification in which we add a squared ln*GAP·MW* interaction term. When we do so, we consistently get a positive coefficient on ln*GAP·MW* and a negative coefficient on the squared term, for both the employment and hours equations. For example, coefficients (all significant) for these two terms from the full specification with store fixed effects are 2.15 and -7.68 in the employment equation, and 1.88 and -6.87 in the hours equation. Apart from the squared term, the specifications are identical to Table 5, columns (6) and (10).

Despite well measured data and a tight correspondence between MW compliance costs and observed wage increases, we find no compelling evidence for negative employment or hours effects for these quick service restaurants in response to the three MW increases during 2007-2009. Although the weight of the evidence suggests somewhat positive rather than negative MW effects, both the sample size and statistical precision of the estimates are too low to allow us to draw such a strong conclusion, absent strong priors favoring monopsony and/or institutional models of the labor market. Whatever the average response to the MW, it is clear from our results that there is considerable store-specific heterogeneity (in shocks and/or behavior), making it difficult to observe systematic responses to minimum wage mandates.[17]

Alternative Channels of Adjustment

The finding of generally small and insignificant impacts of the minimum wage on employment in this and other studies challenges the predictions of standard economic models. But if adjustments in employment and hours are minimal, what changes do occur? We address this question by exploring a

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[17] Neumark, Salas, and Wascher (2013) cite our earlier long-run estimates (absent store FE) and characterize their imprecision as the product of "uninformative data." We lean toward the alternative interpretation that the data are informative, illustrating that wage-employment relationships involve multiple channels of adjustment and that there exists greater flexibility and heterogeneity among establishments than suggested by textbook theory.

wide range of channels of adjustment (CoA). Evidence is obtained from alternative sources. In addition to use of the payroll data and other information from the franchise owners, we rely on information from manager surveys and personal interviews. Some of these CoA have received attention in earlier MW studies; others are largely undocumented.

Wages, Non-labor Costs, and Prices: Insight from Back-of-the-Envelope Calculations

As reported previously, calculations from the payroll data show that the direct MW cost of compliance (i.e. raising wages just to the minimum, absent other pay increases) was 2.6%, 4.6%, and 6.8% of total payroll (hours constant) for the three years. We add to this the estimated increase in employer payroll taxes.[18] If there were constant returns-to-scale in production and full pass through, the increase in price due to higher wages should be proportional to the share of labor in total factor cost. We do not have a direct measure of labor's share, but use a guesstimate of 24% based on data from a franchise owner showing that non-managerial payroll was 18% to 20% of total sales.[19] Taking each year's MW compliance costs (including payroll taxes) and labor's share, the cumulative three-year increase in total costs directly due to MW compliance is an estimated 3.9%.

If MW compliance was the only increase in costs over three years, it might be readily handled through price and non-price channels of adjustments. Restaurants faced other sources of labor costs, however, including pay increases for workers above the MW, and substantial changes in non-labor costs. We calculate an extended measure of labor costs for our restaurants taking into account MW compliance costs plus additional increases in pay. We do this by examining the total increase in the wage bill (holding hours constant) over the three years, including estimated payroll tax increases. A cumulative 25.0% increase in total labor costs boosted total costs (labor plus non-labor) by an estimated 5.7%.

Although important, increases in wages were not the principal source of cost increases during this period. For the approximately 76% share of costs due to non-labor inputs and managers' compensation,

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[18] OASDI-HI plus federal and state UI costs are about 13% of payroll. Owners were surprisingly vocal about the burden of higher payroll taxes necessitated by the MW.

[19] Labor's share of total costs exceeds its share of sales. Aaronson et al. (2008, p 695) provide evidence that limited-service restaurants have a 30-35% share of labor to total costs. Our estimate of the price pass-through needed to offset cost increases is insensitive to the assumed labor's share once we account for non-labor cost.

we assume these rose at the same rate as the BLS Producer Price Index for "finished consumer foods" during 2007-2009. On a percentage basis, these costs increased roughly half as fast as did labor costs, but they had a larger impact on total costs given their large share. Taking the weighted average of the labor and non-labor costs, we calculate that average per unit costs rose 15.4% during the three-year period.[20]

To what extent were restaurant owners able to offset these higher labor and non-labor costs through increases in menu prices? We cannot measure overall price changes, but did obtain price hikes from the owners for the single most popular menu item (homogeneous among the stores), a combo meal made up of a sandwich, small fries, and a drink. The price increase for this item over the three years, averaged across all restaurants (each given an equal weight) was 10.9%. Although less than the estimated 15.4% increase necessary for full pass through, a ballpark estimate is that about 2/3 of total cost increases were offset by higher prices. Had the MW been the sole source of increasing costs, and had the economic downturn in 2009 not so seriously affected sales, it seems likely that the restaurants could have passed most-to-all of the higher labor cost through to consumers.[21] Of course, such an extensive pass-through of costs is most likely when labor and non-labor cost changes are similar across competitors.[22]

For this sample of QSRs, higher prices rather than cuts in employment and hours seems to be the more important CoA from higher wages due to the MW. That said, we also find evidence of second-order but non-trivial adjustments through a variety of other channels.

Compression and the Internal Wage Structure

Cost increases following minimum wage hikes can be mitigated by awarding smaller-than-normal pay raises to workers with wages above the minimum and by permitting the internal wage structure to

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[20] Franchised stores buy all food and related supplies from corporate-designated wholesalers. An owner provided us with data on the annual percentage price change for a typical food supply "basket." This yields a cost estimate highly similar to that using the PPI. Whereas MW compliance costs were low in 2007 but climbed in 2008 and 2009, non-labor costs exhibited the opposite pattern (many input prices fell in the 2008-2009 recession), making it easier (all else the same) for stores to handle the 2009 MW hike.

[21] Addison et al. (forthcoming) provide evidence that teenagers in the food service and drinking sector were most affected by the 2007-2009 MW increases in those states with high unemployment rates.

[22] Our evidence on prices is consistent with Aaronson et al. (2008), who provide comprehensive nationwide evidence on price changes among restaurants in response to the two-step MW increase in 1996-1997 from $4.25 to $4.75 to $5.15. Prices increase more for limited-service than for other restaurants and vary geographically as expected based on state minimum wages and market wage levels. They interpret their evidence as largely inconsistent with monopsony while supportive of the competitive model.

become more compressed. Although managers stated that they desired to preserve wage differences between the less experienced and more senior workers, it became increasingly difficult to do so over the three years. As seen previously in Table 3, required compliance costs for the three MW increases were 2.6%, 4.6%, and 6.8% of payroll in 2007, 2008, and 2009. Overall payroll increases (holding hours constant) following the MW increases were 6.0%, 6.6%, and 7.9%, implying that pay increases beyond those legally required declined from roughly 3 to 2 to 1 percent of payroll over the three years.

Although we cannot determine the extent to which wages across the distribution would have increased absent the MW mandates, evidence that the internal pay distribution became more compressed is strong, as seen in prior literature (e.g., Dube *et al.* 2007). Average within-store pay dispersion (measured by the CV of dollar wages) was 0.156 in March-May 2007, but steadily decreased following the three July MW increases, to 0.139, 0.102, and 0.070 in October-December 2007, 2008, and 2009. Increased compression is also observed among workers for whom the MW was not binding, with the CV for non-affected workers being 0.141, 0.125, and 0.104 following the three increases.

Owners confirm that the size and extent of performance-based raises were reduced as they sought to contain increasing costs during a period of weak demand. When we compare wage increases among workers for whom the MW was and was not binding, we find that workers above the MW received far smaller relative (and absolute) pay increases. Average pay increases among affected workers were 22, 39, and 62 cents in 2007, 2008, and 2009, whereas workers whose wages were at or above the legally-required level received average increases of 16, 22, and 21 cents during those same years.

Insight regarding pay compression also is gained by measuring the share of workers with pay at or near the MW. Prior to the July 2007 MW increase from $5.15 to $5.85, 41% of workers had wages at or within 50 cents of the MW (i.e., a wage between $5.15 and $5.65 in May 2007). By October 2009 following the third MW increase, those with wages at or near the minimum (a wage between $7.25 and $7.75) had increased to 89%, with 35% paid exactly $7.25. In short, following the third MW increase, most workers were paid close to the MW.

We also examine pay compression by asking how wages among non-affected workers (those with

wages above the minimum) varied with respect to store compliance costs. Recall that our first-stage wage regressions (see Table 4, column 3) produced precisely-estimated store-level wage-GAP elasticities close to 1.0 for each of the years. When we estimate this same model, but use as our dependent variable the store-level wage among those at or above each MW (rather than for all workers), we obtain wage-GAP elasticities that are positive (i.e., consistent with MW spillovers) but close to zero and insignificant – 0.026 (0.208), 0.063 (0.099), and 0.125 (0.076) for the three years (standard errors in parentheses).

Evidence from these restaurants clearly shows that cost increases following MW increases were constrained by limiting pay increases for higher wage workers and thus allowing store pay distributions to become increasingly compressed. Pay increases above the minimum were largely unrelated to MW compliance costs, however, suggesting that non-labor cost increases and weak demand during the recession played an important role. Evidence from Card and Krueger (1995, p. 164) and our interviews suggest that in future years extreme pay compression is likely to be mitigated as managers provide smaller raises for workers at or close to the minimum and more substantial raises to more senior workers.

Separations and Turnover

Labor turnover is costly to firms. If higher pay reduces turnover, firms can partially offset MW costs by savings in hiring and training (Arrowsmith et al. 2003). Higher wages, per the dynamic monopsony model (Manning 2003), may also reduce vacancies and lead to a higher level of employment and hours. Empirical evidence on the MW and job attachment link generally finds lower turnover from the MW (e.g., Dube et al. 2007; Reich *et al*. 2005; plus Fairris and Reich 2005 for living wage ordinances). Using data on employment flows, Gittings and Schmutte (2013) show that MW employment effects depend in part on turnover, the MW decreasing employment in low turnover sectors and increasing employment in high turnover sectors.[23] They argue that turnover is a cost channel for firms and that reduced turnover from MW lowers the marginal cost of employment.

Below we first examine how store-level turnover differs with respect to MW compliance costs. We then turn to an analysis of individual worker employment duration.

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[23] Data from the BLS's Job Openings and Labor Turnover Survey (JOLTS) show that quit rates in "Accommodation and Food Services" are the highest among any of the very broad industries for which they publish results.

Store-level analysis: separations, hiring and turnover

We provide formal as well as descriptive analysis on employee separation rates – the share of the total workforce observed in pay period *t* but not in period *t+1*. By this measure, if all workers (other than new hires) are present in two consecutive pay-periods, the separation rate is zero. Such separation rates are high—approximately 8% each pay period—but there is a downward trend, particularly pronounced after the second MW increase in July 2008. Over the three year period, separation rates fell from roughly 10% to 5%. Rates are not stable across months, with spikes in summer and in December-January. We provide a similar measure for new hires per pay period, and a measure of turnover that is simply the sum of the separation and hires divided by two times employment from the previous payroll period.

In Figure 3, these rates are shown separately for restaurants that are least, moderately, and most affected by the MW increases (based on the cumulative GAP). As expected, the separations, hiring, and turnover rates closely track each other over time. Moreover, it is difficult to discern any obvious differences in levels or patterns between these three groups of restaurants.

More formally, in work not shown, we estimated separation rate regressions that mimic the approach seen in Table 3 for employment and hours. The dependent variable is the share of the workforce observed in pay period *t* but not observed in period *t+1*. In addition to store fixed effects (which absorb the lnGAP measure), we include a dummy for the post-MW period and the interaction of the post-MW period with compliance costs (ln*GAP·MW*), with the coefficient on this interaction term identifying the effect of the MW mandate on separations. A second specification adds two variables – the change in store sales (lagged) and change in county private employment. We obtain the expected negative coefficients of MW compliance costs (which closely predict wage increases) for the 2008 and 2009 increases (the former but not the latter being significant), but obtain positive and insignificant coefficients for 2007. Although the 2008 and 2009 results support standard theory, we are reluctant to give these results substantial weight given the far less plausible positive coefficients in 2007.

Using the exact same setup, we also estimated hiring and turnover rate equations. None of the ln*GAP·MW* coefficients was statistically significant. The turnover coefficients had the same signs as in

the separation equations for all three years, while the hiring coefficient signs matched the separation

equations in two of the three years (2008 was the exception). Given that these estimates were largely

uninformative, we do not show the results.

We also examine "long-run" (three year) changes in store separation rates with respect to the

cumulative store compliance costs. This relationship is shown in Figure 4. Although we would expect

separations to decline across stores with respect to the GAP, what we see is a relatively flat relationship

throughout much of the distribution, with declines evident only among those restaurants most impacted

by the MW. Taking the pattern seen in the local regression curves at face value, the suggestion is that

only among the lowest-paid stores – those most affected by the MW – does one see reductions in

separations (and turnover more generally). Drawing such a conclusion requires that we largely ignore the

large variability in separations across stores. Equivalent figures for hiring and separations are highly

similar to that seen for separations.

Individual worker analysis of employment duration

Although we do not observe a strong relationship between store-level turnover and compliance costs,

our data allow analysis at the individual rather than store level. The structure of the data, however, limits

our ability to provide a fully-satisfactory duration analysis.[24] The completed length of an employment

spell is unobserved; we do not observe entry for those present in the initial January 2007 pay period, and

do not observe exit for those continuing beyond December 2009. We do not know the length of very short

spells (less than two weeks) since we only observe such workers' hours and pay for a biweekly payroll

period. Left censoring (inability to observe the point of entry for each worker) can be addressed by

considering only "new hires" — workers who enter the sample *after* mid-January 2007. Descriptive

evidence on employment duration confirms that turnover is high, as expected. For this sub-sample of

workers, we find that the median "survival" in the sample is 14 weeks (7 pay periods). Roughly 70% of

all new hires beginning mid-January 2007 are not in the sample at the end of December 2009.

Keeping the censoring issues in mind, we examine how individual worker's employment duration is

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[24] The "failure event" is falling off the payroll. Workers present for only one biweekly payroll are omitted.

affected by the compliance costs due to the MW increase. We construct individual-worker "gaps" by calculating the percent increase in individual wages needed to comply with each year's July 1 MW increase, based on observed wages two months earlier. Unaffected workers – those whose wages were already at or above the new required minimum –-have a zero gap. Instead of collapsing the data set to create single-spell observation for each worker, the data set contains multiple spells per worker (i.e., the number of pay-periods on the payroll) to take advantage of time-varying covariates.

The set-up for the duration analysis resembles our earlier analysis for employment and hours (Table 3). Separate models are estimated for each of the three years. Here, the "outcome" for each worker equals zero for all pay-periods except the final one, which equals one if a worker is not on the next payroll. Survival time is censored for those who do not leave the payroll. Independent variables include the worker-level "gap" as calculated in each year, a binary variable for the six months after each July MW increase, and their interaction. The estimated coefficient on the interaction term tells us how the risk of a separation (due mostly to quits, but in some cases a layoff or long leave period) varies with the exogenous shock of a MW increase, conditional on being on a payroll a certain amount of time. We also control for store fixed effects and worker tenure (the baseline hazard rate).

Estimates are derived applying a discrete logistic model.[25] As expected, survival time decreases with time spent on the payroll (a higher baseline hazard rate) for each of the three periods.[26] However, we do not find a strong relationship between MW increases and separations in our data, consistent with the weak store-level evidence provided previously. We find little evidence of individual employment duration being affected by the first two MW increases (signs are mixed and insignificant). Stronger evidence is found for 2009, a year when the job market substantially contracted. A worker requiring an 8% (0.079 log point) wage increase to reach \$7.25 in July has a 4.8% lower probability of separation in August (a statistically significant -0.61 coefficient times 0.079 = -0.048 or, equivalently, exp(-0.048) = 0.95 lower

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[25] If we assume transition events (in and out of employment) can occur at any point in time, we would have a continuous time duration process. Because of the structure of our payroll data, we observe "events" at discrete intervals (biweekly), making the discrete time duration process arguably more appropriate for the analysis.

[26] Baseline hazard rate is assumed to have logistic functional form, but results are unaffected when we assume a non-parametric baseline hazard rate and include interval-specific dummy variables.

hazard rate).

In contrast to the weak relationship found here between store compliance costs and separations, clear-cut effects have been found elsewhere using comprehensive national data. For example, a recent paper by Dube et al. (2013) examines county-level data on both employment flows and stocks. They examine contiguous county pairs across state borders using data for the restaurant industry and, separately, for teenagers. In both sets of analyses, they find that a higher MW leads to substantially lower separations and hires, but has little effect on employment levels in their preferred specification. A Canadian study by Brochu and Green (forthcoming) provides similar evidence using household data, concluding that higher minimum wages lead to lower hiring rates and separation rates, the latter heavily influenced by lower layoff rates in the first six months of employment. They conclude that jobs in a higher wage regime are more stable but harder to get.[27]

The relationship between the MW and separation/turnover rates in our sample is admittedly weak. Given the downward trend in turnover evident among both high and low wage restaurants over this period, coupled with the strong evidence of a turnover effect seen in prior studies, it seems likely that some portion of the added costs from MW increases were offset by reduced training, hiring, and separation costs. Two of the franchise owners estimated turnover cost at $300-$400 per employee – factoring in approximately 40 hours of non-revenue producing training over six months. It is clear from the manager survey that store managers regard turnover as important. When asked to comment in an open-ended question about positive aspects of a higher MW, managers emphasized lower turnover, along with higher morale, greater worker effort, and more and better job applicants.

Operational and Human Resource Efficiencies

We next explore operational and human resource (HR) efficiencies with data collected from manager surveys and interviews. Although these data do not readily lend themselves to formal analysis, insights are gained by peering into the "black box" of internal operations. That said, managers' statements regarding what is important or what they are likely to do need not fully align with actual responses.

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[27] We thank a referee who pointed us to these two studies.

Managers were asked to estimate the share of the (then upcoming) 2009 MW labor cost increase they could offset by implementing various operational efficiencies, after excluding price and employment and hours adjustments. Their responses averaged 23%, with no significant difference in responses among the least and most affected stores (p-value = 0.468 based on the 2009 GAP). Although conjectural and perhaps subject to upward bias (from over-optimism, etc.), this estimate has some reliability because the managers (1) had already worked out as part of their business plans the likely increase in their wage bill (the ratio's denominator) and (2) had two rounds of experience with operational belt-tightening from the 2007 and 2008 MW increases from which to project an estimate for 2009 (the numerator).

We next sought to identify the specific sources of cost savings. Based on input from manager interviews and several pre-tests of the survey instrument, we developed a list of 23 potential CoA in the following areas: human resource practices, operational efficiency and productivity, non-labor costs, and customer service. Despite careful wording, some items inevitably have a degree of overlap.

Managers were asked whether they currently use or plan to use in the *next three months* each of these 23 cost-saving measures in order to *offset MW cost*. Those who answered yes then rated its contribution to cost savings on a scale from 1 to 5, with 5 the most cost-effective. Of the total 81 stores, 66 managers responded to the survey, all of whom answered the yes/no question. Among those who answered yes, all but a small number provided ratings on importance. In the text below, we state the proportion of the managers who answered yes. Table 6 provides the distribution of answers among those who rated its importance. We collapse the five categories into three (ratings 1-2, 3, and 4-5), labeling them "not very important," "somewhat important," and "very important."

We start with efficiencies in HR practices. We identified several such channels. Most of the 66 managers (90%) planned to increase performance standards and, among those who provide ratings on its cost savings, about two-thirds rank it as "very important" (Table 6, panel A). Higher performance standards include things such as requiring a better attendance and on-time record, faster and more proficient performance of job duties, and taking on additional tasks. Managers said in interviews that part of effective HR practice is to directly communicate to employees the quid pro quo of higher performance for higher

wages. Another cost-saving measure is adjusting employee work schedules to more tightly match beginning and ending times with customer demand, thus gaining a fuller utilization of each employee hour. Regarding labor-labor substitution, most managers did not consider changing the part-time/full-time mix. They did express interest in hiring more experienced and older workers but fewer teenagers, and there is a positive correlation between the size of the cost shock from the 2009 MW increase and managers' reports to use this cost-saving strategy.[28] Only a few managers planned to reduce training, the reason given in interviews being that greater operational efficiency requires more training (the two are complements). Consistent with evidence from the payroll data, a number of managers (40%) state they would delay or limit pay raises/bonuses for more experienced employees.

The manager surveys support the evidence from payroll records that reducing employment is not a principal CoA. Only 23% of managers planned to decrease their workforce to offset the higher cost and only 12% rated this strategy as "somewhat" or "very important" for cost saving. In fact, there is a significant negative correlation between the size of the 2009 MW impact on costs and managers' plans to reduce their workforce. In contrast to our findings of largely unchanged hours worked from the payroll data, a significantly greater proportion (60%) planned to reduce work hours. Judged from the written responses, this seems to substantially overlap with the question on change in work schedules. An example cited is letting a person on the night crew leave at 11:00 instead of midnight.

We probed in interviews for reasons behind the small-to-zero employment effect and several factors were cited: speedy customer service is a "must-do" and reduced staffing threatens it; the production process features indivisibilities that preclude marginal labor adjustments; and team spirit and a cooperative employee attitude are the most important factors for successful operations and layoffs or cuts

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[28] We measured all the responses in Table 6 separately for restaurants least, middle, and most affected by MW compliance costs. In most cases, we could not see a clear-cut pattern between the responses and the *GAP* group. We also tested more formally for labor-labor substitution using information in the employee surveys, which identify the store but not employee. Using data on age and high school completion, coupled with their reported tenure, we matched these to stores' MW compliance costs. Simple difference-in-difference models were used, with outcome variables being age and high school completion. The independent variables included a binary variable equal to 1 if a worker was hired *after* each of the three MW increases, the store's ln*GAP* for that year, and the interaction between the two. Coefficients on the interaction terms in the two sets of regressions were positive in all years for education and for two of the three years for age, but none was significant.

in hours undermine these.[29] With regard to labor input, worker effort appears to be a short-run continuous adjustment factor in the production function, with employment partially discontinuous.

We next transition to various forms of operating efficiencies. The owners keep detailed records on daily and weekly indicators of costs, sales, and payroll, and establish targets that managers are expected to meet. A rise in the MW (or other costs) moves stores past these targets and create pressure on managers to squeeze out costs through tighter operations. With regard to the MW, the most common response was to gain greater productivity from the workforce through, say, cross-training, multi-tasking, and tighter work schedules. As indicated in Table 6, panel B, boosting morale was also a major CoA (cited by 92%). Interestingly, increasing performance standards and enhancing workers' morale and team spirit are the two strategies that are strongly and positively correlated with the degree to which a given store is affected by the 2009 MW increase (analysis not shown). The managers work to create interdependent utility functions among their crew to create productive synergies, team spirit, and self-enforcement of high work norms through peer monitoring. Reliance on team production makes it crucial to treat employees not as disposable "hired hands" but as valued crew members who managers strive to respect, support, and treat fairly. Managers stated that they try to avoid overt cuts in hours and headcount, an exception being use of the MW increase as an opportunity to weed out particularly low-performing employees. Another CoA in the operations and productivity area is new capital equipment. We did not include this option on the manager survey because owners make capital investment decision. Owners indicated a steady if slow process of capital-labor substitution, constrained in the short run by cash-flow constraints and the relatively simple nature of the technology of fast-food production.

We note for later discussion that there is both a neoclassical and institutional/behavioral perspective on managerial "tightening-up." In the competitive and monopsony models, firms are assumed to continuously minimize cost so no slack (underutilized resources) exists. Owners and managers told us that since fast-food is highly competitive they vigilantly monitor costs and after two previous MW increases felt like they had pretty well squeezed out the "fat." Yet when questioned on planned

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[29] One owner predicted to us in the first interview (before data analysis) that we would find a zero employment effect, citing reasons such as those just given.

adjustments to the third MW hike they cited a number of actions to further improve efficiency.

Three observations from the interviews are apropos. The first is that the managers are overloaded with daily operation issues and work long weekly hours (often 50-55) and, hence, cannot devote the time to actively address important but longer-run or secondary operational issues. A MW hike thus acts as a catalyst or shock that forces managers to step out of the daily routine and think about where extra savings can occur. Second, as a practical matter it is difficult to distinguish such adjustments as a neoclassical-like movement along an isoquant (substituting a given stock of managerial time from one activity to another) versus an institutional/behavioral movement to a higher isoquant (generating more output from given managerial inputs by energizing effort/attention to unexploited areas of cost-saving). If isoquants shift out in response to MW or other shocks, it is plausible that over time they gradually shift in as shocks (old and new) becomes less salient. Third, owners said the "quality" of the manager is the single most important determinant of unit success (given location, etc.), but managerial quality is scarce and heterogeneous. What is typically referred to as "slack" in part reflects differences in managerial quality, along with other unmeasured factors that account for productivity differences across establishments (Syverson 2011). All in all, we feel unable to disentangle these competing hypotheses.

Non-labor Costs and Customer Service

Two other distinct CoA are savings on non-labor inputs and customer service improvements. Interviews indicated that utility costs, insurance, food costs, food wastage, size of drinks, and condiment supply all received attention. The survey responses (table 6, panel C) indicate an overwhelming majority of managers plan to economize on electricity and water usage and reduce food waste (e.g., by more careful scheduling of deliveries and tighter inventory control). One area of focus was cost control at the condiment bar.

Another CoA is aimed at increasing sales through improved service. Part of raising performance standards is more "smiling faces" at the counter and drive-in window; another element of customer service is having employees more often check that bathrooms, dining tables, and parking lots are clean. Marketing strategies, such as meal discounts, new menu items, and raffles are implemented to maintain or increase volume. A number of the managers interviewed said they also planned to offset higher cost by

building volume with more special events, for example birthday parties and events for local churches, youth groups, sports teams, and retirement homes.

Profit

Profit is the residual that remains following movement in all other revenue and cost channels earlier discussed. Prior evidence on the MW profit effects is scarce; several studies, however, find zero or small negative effects (Card and Krueger 1995; Draca et al. 2011)**.** Due to confidentiality concerns, none of the owners would share data on annual profit. We did obtain, however, data on the average profit *changes* among approximately three-fourths of the stores spanning the three MW increases, based on franchise-wide and not store-specific results. The data cover a fiscal year (e.g., starting October 1) so disentangling the MW effect is difficult. For this and other reasons our evidence is best considered suggestive.

Annual profit changes at these stores averaged 6.9% (FY 2006), 20.0% (FY 2007), 5.9% (FY 2008), 0.5% (FY 2009), and -20.0% (FY 2010). FY 2007 includes 3 months of the 2007 MW increase, while FY 2010 begins 3 months following the 2009 increase and reflects the wage base following all three increases, plus other cost changes. We earlier showed that the "bite" of the MW on store labor costs increased over the three rounds; these data show that profit growth correspondingly slowed in each round from FY 2007 through FY 2010. For each MW hike, the businesses were able to keep revenue ahead of costs (profits grew each year through FY 2009) through various CoA, but their ability to do so lessened over time.

Owners indicated in interviews that if the *only* cost increases over 2007-2009 were from the MW they could have mostly-to-completely offset them through savings in other CoA. Profit growth declined over this period, however, as increases in labor cost were compounded by increases in other cost areas (e.g., food and operating costs) and a substantial slowdown in sales starting in 2008 due to the global economic crisis. FY 2010 was, in the words of one owner, a "perfect storm" for profit (-20%) since the base of labor cost rose the most in 2009, commodity prices spurted upward in 2010, and local economy activity and restaurant sales remained anemic. It is impossible for us to decompose the weight due to each

factor, but the owners agreed in interviews that the decline in sales volume much dominated the MW as a contributor to lower profit growth (roughly estimated as 10-to-1).[30]

The conclusion we draw from our sample and time period is that (1) the MW by itself had a small effect on profit, employment, and growth but (2) these companies are struggling with multiple sources of cost increase in a climate of significantly deteriorated sales and, hence (3) these factors together have posed growing profit and (in some cases) survival problems, with potentially negative consequences for additional business formation and employment growth in the medium-to-long run.[31]

Summary and Conclusion

Since empirical study of the minimum wage has a near century-long history, one must question whether an additional study breaks new ground. We believe this study does so along several dimensions.

First, our data set is innovative. It captures three rounds of MW increases, contains accurately measured establishment-level pay and employment data, supplements it with data from manager surveys on a wide range of rarely-captured aspects of internal firm operations, and is rounded out with information from field-level interviews. Although shortcomings are present – for example, a small sample size of restaurants resulting in imprecise estimates, the qualitative and subjective nature of the manager data, and anecdotal reports from personal interviews – on balance we believe the analysis adds valuable insights to some old questions while addressing several new or under-explored ones.

Second, we recast analysis of the minimum wage into a broad "channels of adjustment" framework, moving beyond the conventional emphasis on employment effects. The effect of MW on the internal operation of firms has been left as a mostly unexamined black box (but see Arrowsmith et al. 2003). We believe the CoA framework usefully broadens attention to the many margins potentially affected by MW, the manner in which employers and markets adjust these margins, and the resulting incidence of costs.

Third, the employment effect of MW continues to be a major point of controversy in the literature

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[30] Lester (1946) found a similar result regarding employer sensitivity to sales versus wage changes.

[31] The owner with the largest number of stores indicated in 2010 that for the first time in the company's history (40+ years) no new units were planned or under construction. A second owner was forced to close two stores in 2011 and in 2013 sold all the stores after three years of cumulative loss and lack of cash flow to pay for needed store renovations and corporate-mandated marketing programs.

and for public policy. Our analysis identifies the employment and hours effects based on differences across restaurants and over time in the compliance costs resulting from the three MW mandates. We find, in line with other industry-specific studies, that the measured employment and hours impacts are highly variable across establishments and in many or most cases not statistically distinguishable from zero. Although we had hoped to have a larger sample of businesses, we regard the observed variability in this sample as an informative indicator of the extent and importance of heterogeneity in economic relations.

Fourth, our study finds evidence that the cost of MW is passed along and absorbed through a wide-range of CoA. One of the more important channels is the increase in product prices, an adjustment made easier because the MW affects local competitors in a similar manner. Although standard theory predicts higher prices cause lower output and employment, it is difficult to discern such responses in our data. Other CoA include operational "tightening-up," higher employee performance standards and work effort, new marketing programs to expand sales, and compression of the internal wage structure. Interview evidence suggests that in good economic times restaurants can mostly and perhaps completely maintain profit margins by utilizing these other CoA, but in economic hard times they are insufficient and a higher MW takes a bite out of profit, particularly at marginal/low-volume stores.

Fifth, this study offers a new explanation for the small and insignificant MW employment effects frequently found in the literature. Common explanations are that "failed" (non-negative) estimates are a statistical artifact from data mismeasurement or flawed estimation procedures; also, appeal is made to structural or dynamic forms of monopsony. Our study suggests an additional three-part explanation. The first – empirically important for the firms in this study but not a factor much noted in the MW literature – is that even large increases in the MW may be modest as compared to other cost increases that business owners must routinely offset or absorb, thus leading to a lower MW elasticity (per the Marshall-Hicks condition on the size of labor's cost share). The second is that a MW cost increase flows through more adjustment channels than economists have typically considered. And the third is that managers regard employment cuts as a relatively costly and perhaps counter-productive option, regarding them as a last-resort. So viewed, a zero or very small employment effect is compatible with economic theory as long as

the theory posits multiple CoA with differential costs.

For space and focus reasons we have refrained from discussion of how our empirical results bear on alternative models of labor markets. We end with brief thoughts on this matter. Since Card and Krueger (1995), debate over the minimum wage has focused primarily on the merits of a competitive versus monopsony model of labor markets. Yet going back in time, an institutional/behavioral model also contended for attention (Lester 1964; Kaufman 2010, 2012), as indicated by Card and Krueger who dedicate their book to 1950s neo-institutional labor economist Richard Lester. All three models come in different permutations (e.g., search version of the competitive model, structural versus dynamic versions of monopsony) and are further modified and interpreted by individual users, making comparison and empirical discrimination difficult. Given these caveats, our first-line judgment is that all three models capture important elements of truth regarding the MW and adjustment effects. Since all models are abstractions and therefore capture only a slice of reality, this conclusion is not surprising.

The competitive model appears to face the most anomalies. Restaurants in our sample are not wage-takers but have some modest, wage-setting ability which managers use in setting the entry wage and internal wage structure.[32] Further, our study does not find evidence of clear-cut employment losses – even over three years and a 41% increase in the MW. Possibly over a still longer time span, or with a larger sample of restaurants, a negative effect might appear. Discussion with owners and evidence outside our study period suggest that negative effects may manifest through reduced store openings and increased risk of store closings. Given this important qualification, the message from our study, along with other results in the literature, is that employment effects in the short-to-medium run are small, perhaps near zero in many settings, and certainly smaller than expected based solely on the competitive model. It would be surprising (at least to us) were an important reason for this result not the behavioral dimensions of wage setting and human resource management (e.g., discretionary effort, equity concerns, management

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[32] An earlier study of Atlanta fast food restaurants (Young and Kaufman 1997) identified 24 pairs (or triplets or quadruplets) of restaurants located within two blocks of each other – often near the same street intersection. The managers were asked the wage paid for a standardized entry-level new hire (working the lunch shift Monday-Friday, high school degree, no experience). Using only observations above the minimum wage (to avoid truncation bias), high-to-low restaurant wages varied by an average 5% (equivalent to $.36 relative to the current $7.25 MW), suggesting even at this finely standardized level restaurant managers have some flexibility in their pay policy.

heterogeneity) that the standard competitive and monopsony models largely ignore.

Our suggestive evidence of a nonlinear employment effect, with employment and hours increases in response to small wage increases and declines with respect to large increases, better matches a monopsony than competitive model. The monopsony model, however, at least in its structural version, predicts that prices should fall if the MW leads to higher employment and output, yet we find clear-cut evidence of price increases, as in the broader literature (Aaronson et al. 2008). With respect to prices, the competitive model performs better. The overall behavior of turnover and separation rates does not clearly match a dynamic monopsony story, the exception being restaurants "most affected" by the MW.

The institutional-behavioral model also faces challenges. Like the monopsony model(s), but for different reasons (the monopsony model emphasizes an upward sloping supply curve, the institutional-behavioral model changes the demand curve from a well-defined line to a band), it finds support from the small-to-nil employment effect and possible nonlinear dynamic pattern (vertical movement through the band, then up the band). Price increases and the human-behavioral effects also accord with this model. However, an institutional-behavioral model also predicts, at least relative to the other models, that the internal wage structure is relatively inflexible due to fairness and morale reasons, yet our evidence finds that the MW led to significant internal compression. Also, the model's hypothesis that organizational slack allows managers to absorb some of the MW cost increase faces the challenge of explaining its persistence in firms even after several rounds of tightening-up.

As stated above, a variety of models/approaches appear to offer insight into the labor market effects of minimum wages. Yet we also conclude that in hindsight Richard Lester, the strongest proponent of an institutional-behavioral explanation for a (near) zero employment effect, probably captured more of the reality of minimum wages than neoclassical price theorists have been willing to accommodate. Such a conclusion suggests that a broad CoA framework, as applied in this study, may usefully inform not only professional thinking and ongoing public policy debate on the minimum wage, but on other labor market issues as well.

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Figure 1. Average Hourly Earnings, Employment, and Hours, January 2007 – December 2009

Notes: Shown are establishment averages for biweekly hourly earnings, biweekly employment, and regular and overtime daily hours for three groups of stores by biweekly pay period. "Least" affected stores include those where *GAP* is in the bottom 25 percentiles, "Middle" the 25th–75th percentiles, and "Most" percentiles 75 and over. Vertical lines mark MW increase dates for increases to $5.85, $6.55, and $7.25 in July 2007, 2008, and 2009.

Figure 2: Three-year Log Changes in Wages, Employment, & Hours with Respect to MW GAP
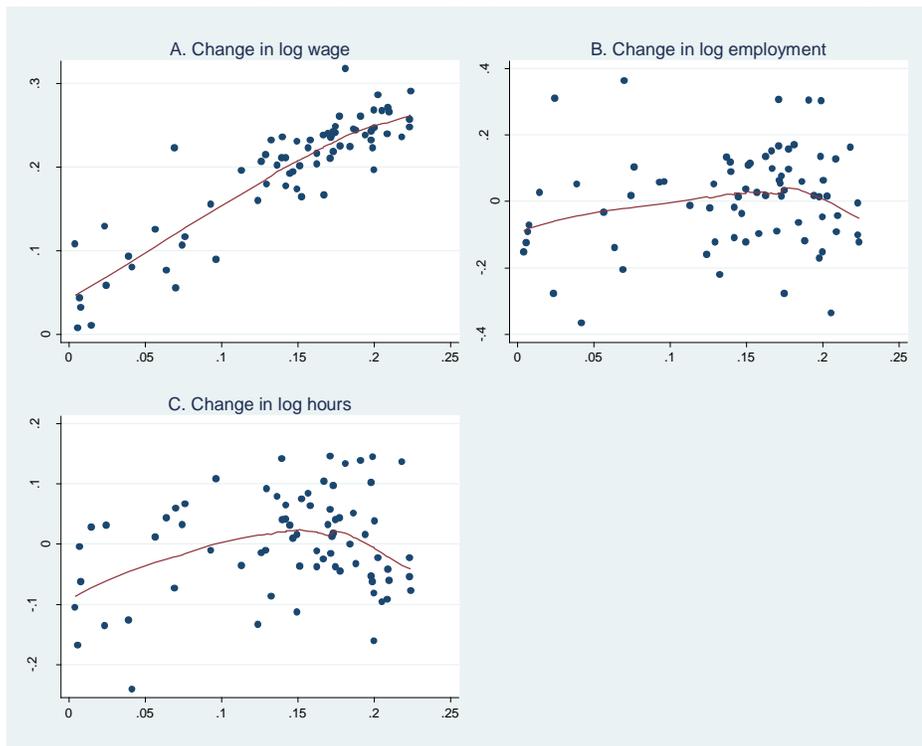
Notes: The vertical axis measures log changes from Mar-May 2007 to Oct-Dec 2009. The horizontal axis measures MW compliance costs using the cumulative three-year ln*GAP*.

**Figure 3: Average Store Separation, Hiring, and Turnover Rates, 2007–2009,
by Store Groups Compliance Cost of MW**

A.Separations

B.Hiring

C.Turnover

Least Middle Most
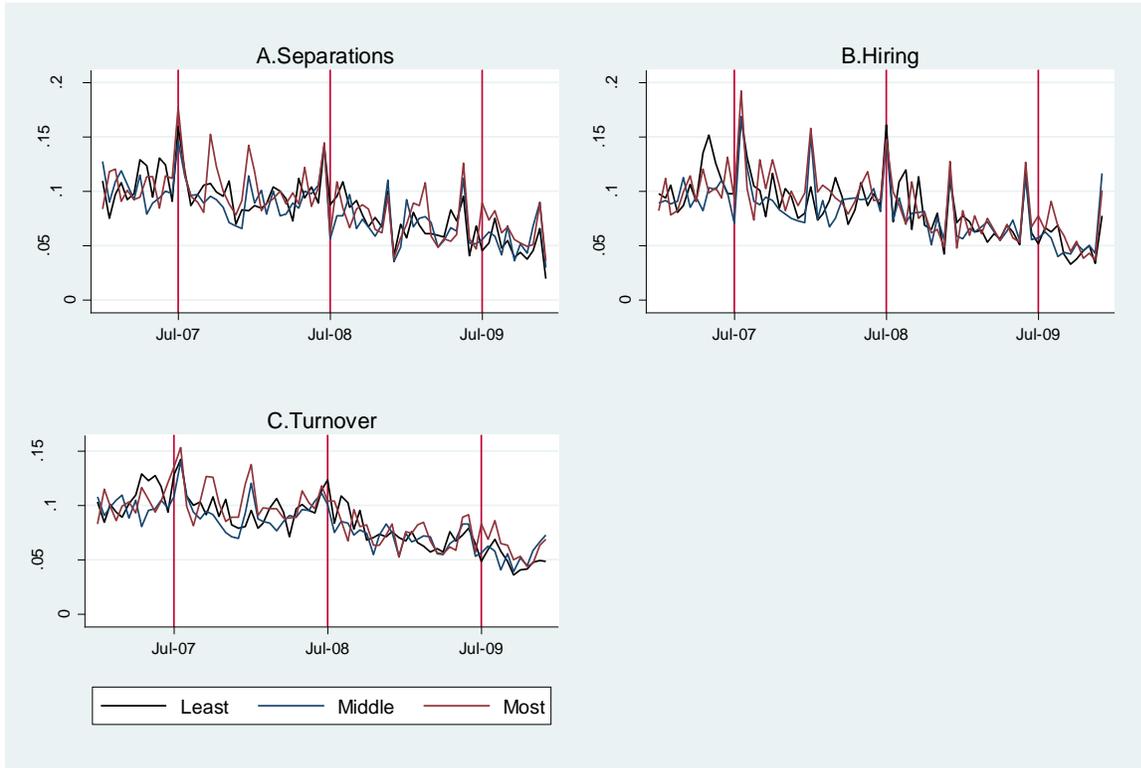
Notes: Separation and hire rates are defined in the text. The turnover rate is the sum of separations and hires divided by two times employment. "Least" affected stores include those where *GAP* is in the bottom 25 percentiles, "Middle" the 25th–75th percentiles, and "Most" percentiles 75 and over. Vertical lines mark MW increase dates.

**Figure 4: Three-year Changes in Store-Level Separation Rates
with Respect to Cumulative MW Compliance Costs**
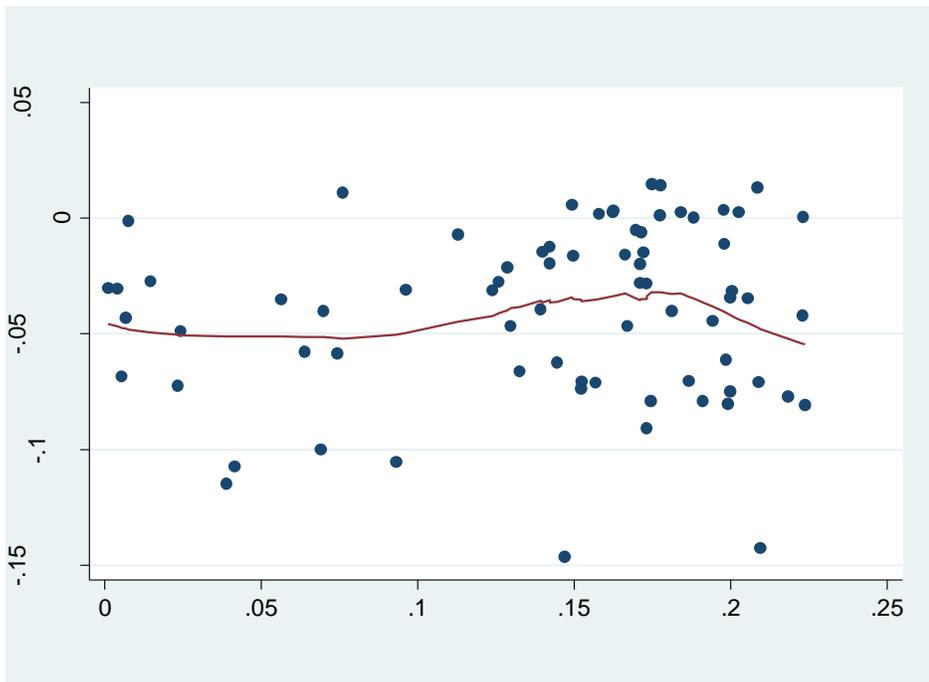
Notes: The vertical axis measures the change in store separation rates between Mar-May 2007 and Oct-Dec 2009. The horizontal axis measures MW compliance costs using the cumulative three-year ln*GAP*.

Table 1. Summary Statistics: Individual-Level Biweekly Payroll Records

Payroll record	N	Mean	St. dev.
2007:			
Hourly wage rate	63,164	6.28	0.95
Regular hours	63,716	49.18	22.58
Overtime hours	63,716	0.74	3.38
2008:			
Hourly wage rate	64,764	6.68	0.84
Regular hours	65,291	49.27	22.30
Overtime hours	65,290	0.63	3.15
2009:			
Hourly wage rate	63,484	7.15	0.69
Regular hours	63,972	49.18	21.67
Overtime hours	63,972	0.45	2.76

Table 2. Worker Characteristics from the Employee Surveys

Variable	N	Mean	St. dev.
Gender (female=1)	1,649	0.657	0.475
Race			
White	1,595	0.207	0.405
Hispanic	1,595	0.082	0.275
Black	1,595	0.644	0.479
Asian	1,595	0.053	0.225
Other	1,595	0.014	0.117
Marital status			
Single	1,451	0.686	0.464
Married	1,451	0.175	0.380
Divorced/widowed	1,451	0.054	0.226
Living with partner	1,451	0.085	0.280
No. of children under 18	1,625	0.958	1.300
Age	1,628	28.194	10.719
School in Fall (=1)	1,623	0.340	0.474
Level of schooling			
Some high school	1,611	0.273	0.446
High school grad/GED	1,611	0.475	0.500
Some college	1,611	0.220	0.414
College graduate	1,611	0.032	0.175
Health insurance (=1)	1,618	0.406	0.491
Country of origin			
U.S.	1,551	0.917	0.276
Mexico	1,551	0.050	0.219
Other	1,551	0.033	0.178
Wage in June 2009	1,555	6.987	1.416
Average hours per week	1,568	29.510	8.309
Tenure at store (months)	1,571	26.812	39.992

Table 2 (continued). Worker Characteristics from the Employee Surveys

Variable	N	Mean	St. dev.
No other jobs	1,592	0.832	0.374
Total annual family income			
Less than 10,000	1,541	0.382	0.486
10-20,000	1,541	0.263	0.440
20-50,000	1,541	0.276	0.283
>50,000	1,541	0.079	0.115
Vote "yes" for MW (=1)	1,607	0.912	0.283

Table 3: MW Compliance Cost, Share Affected, and Payroll Increase

	2007	2008	2009
Compliance cost (mean GAP) as % of payroll:			
Mean	2.6	4.6	6.8
S.D.	2.1	2.6	2.6
Min	0.0	0.0	0.1
Max	8.2	9.2	10.6
Share of workers affected by MW increases (as %):			
Mean	49.2	71.5	82.2
S.D.	29.2	26.0	20.3
Min	0	0	6.6
Max	93.5	97.9	100
Total increase in payroll (%)	6.0	6.6	7.9

Notes: Means are calculated for the same 81 restaurants in 2008 and 2009, and 79 in 2007. Excluded in 2007 was a store that opened in January 2008 and another that opened in May 2007 with unusually high employment during the opening period. See the text for description of *GAP*. The payroll increase measures hold total work hours constant.

Table 4: Wage, Employment and Aggregate Hours Effects of the 2007, 2008, and 2009 MW Increases

	ln(Wage)			ln(Employment)				ln(Total Hours)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Description	OLS	OLS	OLS	OLS	OLS	OLS	IV	OLS	OLS	OLS	IV
	No FE	No FE	Store FE	No FE	No FE	Store FE	Store FE	No FE	No FE	Store FE	Store FE
2007:											
lnGAP'07 x MW'07	0.968***	1.102***	1.148***	1.034*	0.446	0.099	0.087	0.606	0.298	-0.187	-0.163
	(0.149)	(0.144)	(0.105)	(0.590)	(0.618)	(0.500)	(0.422)	(0.423)	(0.429)	(0.307)	(0.261)
lnGAP'07	-3.860***	-3.502***		3.589**	1.921			1.976**	0.570		
	(0.380)	(0.336)		(1.637)	(1.484)			(0.875)	(0.900)		
MW'07 (Aug07-Jan08)	0.021***	0.015***	0.012***	-0.043**	-0.033	-0.011	-0.012	-0.019	-0.017	0.004	0.006
	(0.006)	(0.005)	(0.004)	(0.021)	(0.020)	(0.017)	(0.021)	(0.016)	(0.016)	(0.011)	(0.014)
Δln(sales), lagged 1 month		0.017**	0.010***		0.014	0.044	0.043		0.183***	0.189***	0.191***
		(0.007)	(0.003)		(0.047)	(0.045)	(0.043)		(0.053)	(0.053)	(0.051)
County pop density (log)		0.021**	0.043		0.020	-0.772	-0.776		0.024	-1.578***	-1.571***
		(0.010)	(0.114)		(0.039)	(0.679)	(0.668)		(0.033)	(0.540)	(0.518)
County other private emp (log)		0.005	0.018		-0.112**	0.044	0.043		-0.088**	-0.155	-0.152
		(0.011)	(0.034)		(0.043)	(0.155)	(0.150)		(0.034)	(0.116)	(0.113)
Constant	1.925***	1.748***	1.303	3.394***	4.490***	8.155*	8.043*	7.352***	8.164***	19.277***	19.490***
	(0.015)	(0.069)	(0.814)	(0.053)	(0.296)	(4.643)	(4.440)	(0.032)	(0.212)	(3.876)	(3.732)
Observations	2,013	1,843	1,843	2,013	1,843	1,843	1,843	2,013	1,843	1,843	1,843
R-squared	0.624	0.706	0.975	0.106	0.272	0.890	0.889	0.074	0.262	0.840	0.839
2008:											
lnGAP'08 x MW'08	1.117***	1.105***	1.115***	-0.268	-0.192	-0.292	-0.262	0.165	0.177	0.147	0.132
	(0.053)	(0.052)	(0.052)	(0.344)	(0.350)	(0.348)	(0.302)	(0.175)	(0.181)	(0.177)	(0.156)
lnGAP'08	-2.788***	-2.634***		5.654***	3.946***			2.763***	1.279		
	(0.191)	(0.147)		(1.241)	(1.242)			(0.879)	(0.909)		
MW'08 (Aug08-Jan09)	0.002	0.001	0.002	-0.020	-0.017	-0.011	-0.010	-0.046***	-0.043***	-0.039***	-0.040***
	(0.003)	(0.003)	(0.003)	(0.019)	(0.019)	(0.018)	(0.018)	(0.009)	(0.009)	(0.009)	(0.009)
Δln(sales), lagged 1 month		-0.030***	-0.025***		0.043	0.024	0.018		0.141***	0.121***	0.124***
		(0.008)	(0.005)		(0.050)	(0.031)	(0.031)		(0.037)	(0.025)	(0.024)
County pop density (log)		0.019**	0.068		-0.038	-1.791***	-1.773***		-0.032	-0.848**	-0.857**
		(0.008)	(0.134)		(0.058)	(0.668)	(0.659)		(0.042)	(0.363)	(0.352)
County other private emp (log)		-0.007	0.014		-0.052	0.046	0.049		-0.037	0.061	0.059
		(0.008)	(0.020)		(0.063)	(0.075)	(0.071)		(0.045)	(0.082)	(0.080)
Constant	2.011***	1.969***	1.245	3.225***	4.059***	14.654***	14.980***	7.284***	7.912***	12.148***	11.983***
	(0.011)	(0.043)	(0.917)	(0.071)	(0.394)	(4.553)	(4.542)	(0.052)	(0.276)	(2.912)	(2.868)
Observations	2,104	2,100	2,100	2,104	2,100	2,100	2,100	2,104	2,100	2,100	2,100
R-squared	0.784	0.821	0.952	0.243	0.354	0.922	0.923	0.156	0.291	0.912	0.912

Table 4: Wage, Employment and Aggregate Hours Effects of the 2007, 2008, and 2009 MW Increases (continued)

Description	ln(Wage)			ln(Employment)				ln(Total Hours)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	OLS	OLS	OLS	OLS	OLS	OLS	IV	OLS	OLS	OLS	IV
							Store FE				
	No FE	No FE	Store FE	No FE	No FE	Store FE	FE	No FE	No FE	Store FE	Store FE
2009:											
lnGAP'09 x MW'09	0.859***	0.871***	0.856***	0.671**	0.570*	0.652**	0.762**	0.417**	0.352*	0.405**	0.473**
	(0.028)	(0.029)	(0.031)	(0.307)	(0.331)	(0.307)	(0.359)	(0.201)	(0.206)	(0.197)	(0.227)
lnGAP'09	-1.921***	-1.778***		6.951***	5.215***			3.461***	1.929**		
	(0.105)	(0.104)		(1.231)	(1.160)			(0.853)	(0.865)		
MW'09 (Aug09-Dec09)	0.014***	0.014***	0.013***	-0.038*	-0.036	-0.041*	-0.051*	-0.025*	-0.022	-0.024	-0.031*
	(0.002)	(0.002)	(0.002)	(0.022)	(0.022)	(0.022)	(0.027)	(0.015)	(0.015)	(0.016)	(0.018)
Δln(sales), lagged 1 month		-0.030***	-0.025***		-0.010	-0.012	0.007		0.167***	0.178***	0.190***
		(0.006)	(0.005)		(0.036)	(0.032)	(0.034)		(0.027)	(0.025)	(0.026)
County pop density (log)		0.003	0.021*		0.010	0.159	0.376		-0.014	0.031	-0.018
		(0.004)	(0.012)		(0.051)	(0.151)	(0.242)		(0.039)	(0.066)	(0.103)
County other private emp (log)		0.003	-0.033**		-0.086	-0.148	-0.123		-0.050	-0.060	-0.045
		(0.004)	(0.014)		(0.053)	(0.185)	(0.178)		(0.039)	(0.080)	(0.075)
Constant	2.070***	2.007***	2.119***	2.993***	3.935***	4.092***	0.978	7.142***	7.836***	7.713***	6.959***
	(0.008)	(0.027)	(0.078)	(0.094)	(0.334)	(1.037)	(0.827)	(0.066)	(0.248)	(0.444)	(0.456)
Observations	1,940	1,940	1,940	1,940	1,940	1,940	1,940	1,940	1,940	1,940	1,940
R-squared	0.836	0.849	0.915	0.399	0.478	0.940	0.936	0.234	0.352	0.910	0.907

Notes: Robust standard errors in parentheses, clustered at the store level. *** p<0.01, ** p<0.05, * p<0.1

Table 5: Three-Year Wage, Employment and Aggregate Hour Changes

	ln(Wage)			ln(Employment)				ln(Total Hours)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Description	OLS	OLS	OLS	OLS	OLS	OLS Store FE	IV Store FE	OLS	OLS	OLS	IV
	No FE	No FE	Store FE	No FE	No FE	FE	FE	No FE	No FE	Store FE	Store FE
Cumulative lnGAP x Oct-Dec '09	1.051***	1.052***	1.059***	0.580	0.584	0.415	0.392	0.295	0.297	0.288	0.277*
	(0.101)	(0.090)	(0.081)	(0.758)	(0.695)	(0.355)	(0.328)	(0.519)	(0.453)	(0.176)	(0.167)
Cumulative lnGAP	-1.399***	-1.344***		1.715***	1.290***			0.674**	0.303		
	(0.084)	(0.082)		(0.452)	(0.435)			(0.320)	(0.313)		
Oct-Dec '09	0.045***	0.044***	0.048**	-0.077	-0.083	-0.045	-0.063	-0.042	-0.048	-0.081**	-0.096**
	(0.017)	(0.015)	(0.018)	(0.123)	(0.109)	(0.067)	(0.081)	(0.087)	(0.075)	(0.035)	(0.042)
County pop density (log)		0.008*	0.084		0.019	-1.363	-1.396		0.019	-0.091	-0.115
		(0.004)	(0.232)		(0.036)	(1.062)	(1.031)		(0.026)	(0.588)	(0.571)
County other private emp (log)		0.001	0.063		-0.068	-0.170	-0.195		-0.060**	-0.384***	-0.398***
		(0.005)	(0.046)		(0.042)	(0.224)	(0.222)		(0.027)	(0.088)	(0.086)
Constant	2.016***	1.955***	0.539	3.237***	3.879***	14.230*	14.018*	7.298***	7.851***	12.160***	11.972***
	(0.014)	(0.039)	(1.260)	(0.073)	(0.295)	(7.792)	(7.705)	(0.054)	(0.186)	(3.782)	(3.716)
Observations	146	146	146	146	146	146	146	146	146	146	146
R-squared	0.946	0.952	0.983	0.269	0.315	0.920	0.917	0.117	0.206	0.941	0.940

Notes: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Robust standard errors in parentheses. Included are two observations for each restaurant, one with Mar-May 2007 values and the other with Oct-Dec 2009 values. Estimated MW effects identical to a change model with 1 observation per store. In addition to stores not opened in early 2007, two stores were excluded due to large outlier residuals, high leverage, and extreme changes in employment (one positive and the other negative).

Table 6: Importance of Cost Saving Strategies in Response to MW Increases, Manager Surveys

	Very important	Somewhat important	Not very important
A. Human resource practices and cost savings:			
Increase performance standards	39	14	4
Change work schedules	26	13	3
Hire older/more experienced workers	15	13	3
Cut weekly hours of some employees	15	14	6
Schedule more part-time employees and less full-time	11	4	2
Postpone or limit pay raises to your more experienced workers	11	11	3
Schedule more full-time employees and less part-time	5	4	2
Reduce the number of people on your payroll	4	4	3
Hire teenage workers	3	4	2
Reduce training	1	2	2
B. Operational efficiency and productivity cost savings:			
Discourage overtime work	44	4	2
Cross-train workers for multitasking	38	12	2
Get more work from each person	37	16	3
Increase morale and team spirit	35	17	5
Expand job duties of your workers	34	17	3
Tighten-up on absenteeism and discipline	29	15	6
Rearrange production operation to be more efficient	22	13	2
Less time spent on clean up	3	3	2
C. Non-labor expenses, customer services and cost savings:			
Reduce food waste in preparation/storage	44	12	2
Reduce or save water and electricity usage	38	14	6
New ways to improve customer service	36	13	3
More outreach to church, school and community groups	20	7	2
Reduce amount of condiments and "extras" given to customers	19	10	6

Notes: Table 6 provides distribution of answers to the following question in the Manager Survey: "Other research studies of the minimum wage have identified the following list of items as BUSINESS ADJUSTMENTS you might possible make in order to OFFSET the payroll cost increase associated with the higher minimum wage. Which of the following are you planning to do in the next 1-3 months OR have done already in the last month (please check YES or NO)? If your answer is YES, please rate the impact of your action for cost-saving on the scale 1 to 5 (1=least important; 5=very important). Please circle one number from 1 to 5." The responses were collapsed as follows: "Not very important" if a respondent answered 1 or 2; "Somewhat" if 3; and "Very important" if 4 or 5. Shown are responses for those who answered YES to the "planning to do" or "have done" question.