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ABSTRACT

Minimum Wage Channels of Adjustment*

The economic impact of the 2007-2009 increases in the federal minimum wage (MW) is analyzed using a sample of quick-service restaurants in Georgia and Alabama. Store-level biweekly payroll records for individual employees are used, allowing us to precisely measure the MW compliance cost for each restaurant. We examine a broad range of adjustment channels in addition to employment, including hours, prices, turnover, training, performance standards, and non-labor costs. Exploiting variation in the cost impact of the MW across restaurants, we find no significant effect of the MW increases on employment or hours over the three years. Cost increases were instead absorbed through other channels of adjustment, including higher prices, lower profit margins, wage compression, reduced turnover, and higher performance standards. These findings are compared with MW predictions from competitive, monopsony, and institutional/behavioral models; the latter appears to fit best in the short run.

JEL Classification: J20, J30

Keywords: minimum wages, employment, labor market adjustments, labor market theories

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I. Introduction

The minimum wage (MW) 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 U.S. values. What makes the MW so controversial? Part of the explanation is that it stands as a marker in ongoing debates over the relative size and merits of free markets versus government regulation. We have little to say about this debate, but contribute on two other fronts – MW empirical evidence and its implications for theory. Despite decades of research, pinning-down the labor market effects of MW has proven elusive and controversy continues over the most appropriate model of labor markets for explaining and evaluating these effects.

We hope to provide useful insights on both fronts. To do so, we shift MW analysis to a focus on what we call "channels of adjustment" (CoA). The CoA idea can be broadly likened to the "transmission mechanism" in monetary theory or "tax incidence" in public finance. That is, a binding minimum wage adds to the price of labor and CoA examines the channels through which this cost increase changes the behavior of firms, with impacts on workers, consumers, owners, and other agents. Alternative models of labor markets emphasize different mixes and strengths of adjustment channels.

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%). In this paper we briefly discuss MW predictions from three alternative models – competitive, monopsony, and institutional/behavioral, and compare them with evidence from our data set. Among the CoA identified 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

[1] An October 2011 electronic search of the *EconLit* data base on the subject "minimum wage," limited to journal articles since January 2000 and North America, brings up 147 research studies (plus 21 more for living wage).

exogenous variation across the restaurants in the "bite" of the MW to identify causal effects on employment and hours worked. Although not problem free, these data provide advances in at least three respects. First, our measure of the MW payroll "gap" (or 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 wage bill 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.

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 briefly discuss theory, with emphasis on alternative approaches and adjustment channels, along with prior literature most relevant to the paper. We then describe data sources, survey instruments, estimation strategies, and evidence on employment and non-employment adjustment channels from the MW. In the concluding section we summarize the implications of the CoA evidence with respect to alternative labor market models.

II. Adjustment Channels: Theory

Prior studies have presented labor market models and shown predicted MW effects (for surveys, see Card and Krueger 1995; Brown 1999). In what follows, we (1) link the predicted effects to underlying channels of adjustment, (2) expand the traditional focus from employment effects to a broader array of adjustment margins, and (3) compare predicted CoA across three different labor market models – competitive, monopsony, and institutional/behavioral.

A. Competitive

The competitive model provides the standard base-line for evaluating the MW. The model comes in

different versions, starting with the simple textbook model and then extended to include human capital, worker heterogeneity, job search, and other such additions (see Card and Krueger, 1995, Table 11.1). Widely agreed-upon core components, however, are a negatively sloped labor demand curve and a wage rate that clears the market and is parametric to individual agents.

Starting from a competitive equilibrium, a minimum wage is predicted to move firms up their labor demand curves and decrease employment, the adjustment channel most examined in the literature. The magnitude of the employment reduction depends on the elasticity of labor demand and, with heterogeneous labor, the proportion of a firm's workforce affected by the MW. Demand elasticity is determined through scale and substitution effects, as seen in the Marshall-Hicks laws of derived demand, with ease of substitution in production by firms and in consumption among households increasing with time. Changes in the MW operate at both the firm and market levels. In the case of a nationwide MW, large numbers of restaurants will be affected, albeit by different amounts. Hence, akin to tax incidence some portion of the cost increase is likely passed forward to consumers and backward to suppliers of labor, capital, and other inputs.

With fixed employment costs and worker heterogeneity, firms may reduce work hours but maintain headcount. Employment/hours adjustment may also be affected by wage-related changes in employee work effort and productivity (as in an efficiency wage model), although this extension to the model may take it outside of what is typically considered "competitive."[2] Employment/hours adjustment may not occur, however, if firms do not comply with the mandate or, alternatively, workers undo the loss of jobs á la private bargaining and the Coase theorem (Ippolito 2003).

Numerous other CoA predictions follow from versions of the competitive model. If the MW reduces profitability below the normal level, the number of businesses in affected markets should shrink over the long run until normal returns are restored. Other adjustment channels include: downward pressure on

[2] The degree to which the textbook model can be "stretched" and remain competitive in the theoretical sense is not well established. We try to strike a balance and admit extensions that do not conflict with a downward sloping labor demand curve and (approximately) parametric wage. By these criteria, heterogeneous labor and wage dispersion due to imperfect information are consistent with the competitive model, but an efficiency wage effect is not because the firm sets the wage (as in monopsony) and the labor demand curve may not be entirely downward sloping.

wages in the uncovered sector (where such exists), reduced general on-the-job training, reduction in benefits and other perquisites, labor-labor substitution (e.g., shift from less-skilled toward more skilled workers), reduced turnover and more selective hiring, and greater ease in filling vacancies. With homogeneous labor, the internal wage structure remains a point (all paid MW); with heterogeneous labor the lower part of the internal wage distribution is cut off but the part above MW remains the same absent behavioral-type relativities. The competitive model assumes firms minimize cost and hence in the textbook model no room exists for managers to improve operational efficiency or reduce slack. In a more nuanced version where managerial time and attention are scarce resources, one can interpret "reducing slack" as a cost minimizing redirection of managerial resources in response to a relative price change (Stigler 1966).

B. Monopsony

In recent years the major rival model for examining the MW is some variant of the monopsony model. First developed by Robinson (1933), it now comes in several versions – typically being either "classic" (structural) or "new" (dynamic) monopsony. The former is predicated on one or only a few employers in a labor market and the latter on market frictions related to hiring, turnover, search, and mobility costs on the supply side (Manning 2003). Although the particulars differ, core components of monopsony models are an upward sloping labor supply curve facing firms and some discretion in wage-setting made possible by a "thinness" in the market (due, say, to spatial location, firm-specific skills, etc.).

As applied to MW channels of adjustment, the sharpest difference between the competitive and monopsony models is prediction of employment effects. For MW increases that push below-competitive wages toward competitive levels, classic and new monopsony models predict an employment and/or hours rise rather than fall. MW increases above competitive levels decrease employment, just as in the competitive model. These results are qualified but not reversed with models of oligopsony and monopsonistic competition (Bhaskar and To 1999; Manning 2003).

Monopsony models provide other differences in predicted CoA as compared to the competitive model. The rise in employment, for example, expands industry output (until the MW equals the

competitive wage) and product price should fall (product market structure matters here – see Bhaskar and To 1999). In classic monopsony profits fall and the firm(s) may exit in the long-run; in new monopsony the profit effect may be offset by savings from decreased turnover (Card and Krueger 1995). Unlike the competitive model, expenditures on general training may increase because the employer can capture some of the return. In structural monopsony with homogeneous labor, the MW effect on the internal wage structure mirrors the competitive model (a higher wage paid to all) while in dynamic monopsony the firms's wage distribution narrows (by eliminating differential supply prices for workers in the low end of the distribution) (Booth and Zoega 2008). Although not an issue concerning CoA *per se*, the monopsony model suggests that at least within a range an increase in the MW can enhance labor market efficiency.

C. Institutional/Behavioral.

The institutional model of labor markets was the dominant paradigm for evaluating the MW into the 1950s and provided the principal theoretical rationale behind the passage of the Fair Labor Standards Act (the nation's first federal MW) in 1938 (Craypo 1997). In the last half-century, however, it has gradually faded from consideration in the labor economics literature, partly due to lack of formalization but also because several of the key ideas have been absorbed into neoclassical-based models such as efficiency wage theory and search models. Since a behavioral/social model of the human agent (e.g., bounded rationality, relativities) is a core component of the institutional paradigm, it draws on complementary concepts and models in the field of behavioral economics (Kaufman 1999; Thaler 2000).

Past and present writings in the institutional tradition emphasize several central ideas: rejection of a well-defined downward sloping labor demand curve; labor markets that are imperfectly competitive, institutionally segmented, socially embedded, and prone to excess supply; and the importance of technological and psycho-social factors in firm-level production systems and internal labor markets (ILMs) as determinants of cost and productivity (Taylor and Pierson 1957; Arrowsmith et al. 2003; Brosnan 2003; Kaufman 1988, 2010; Osterman 2011). Regarding efficiency and welfare, the institutional model is similar to the monopsony model in that a MW may improve efficiency and welfare over a moderate range; the CoA sources of welfare gain, however, are not identical.

A key institutional position is that a moderate MW boost may have, particularly in the short-run, an approximately zero or small positive employment effect (Lester 1946, 1960). The reason hinges less on a monopsony upward sloping supply argument and more on characteristics of the firm's labor demand curve and accompanying production/ILM system. The institutional short-run labor demand relationship is a thick band rather than a line; accordingly, a moderate rise in the MW may lead to no employment/hours decline if the negative cost effect is stymied or counter-balanced by (1) indivisibilities and complementarities in production that prevent marginal employment adjustments (e.g., a team form of production, per Alchian and Demsetz 1972), (2) positive effects of higher productivity from enhanced work effort; (3) tighter labor standards and greater managerial/operational "tightening-up" in production and ILMs, and (4) increased sales as a higher MW leads to expanded consumer spending (e.g., a Keynesian income effect). Even if employment declines, this may be socially beneficial. A MW may force out of business inefficient and sometimes exploitative firms (Arrowsmith et al. 2003; Kaufman 2010).

The institutional model addresses other CoA. For example, it posits that the typical firm's production function features constant-to-declining marginal costs and falling unit fixed costs up to the technical full capacity level, implying the firm's output is typically demand-constrained (rather than cost constrained). The first-line response of firm owners to a MW increase, therefore, is not to reduce output and lay off workers (the competitive response) but search for ways to absorb the cost impact by increasing volume through expanded sales from marketing, improved service, and general economic expansion (Lester 1946). At the same time, MW cost impacts can partially be offset by reducing organizational slack and improving operational efficiency. Managers maximize utility but satisfice with respect to profit to the degree that principle-agent constraints permit and profits exceed a threshold "survival" level. The costs that are not absorbed through these methods are passed on through a cost/price mark-up rule, albeit with volume maintenance being a crucial constraint.

Human-related determinants of productivity and cost are particularly stressed; hence, in response to the MW it is more likely that headcount and hours will be maintained (e.g., "take-aways" hurt morale and

engender retaliation). Higher productivity is achieved through tighter human resource practices (e.g., scheduling), increased performance standards and work effort, and enhanced customer service. Costs of turnover plus a surplus of job seekers in typical years provide firms with some monopsony-like power. Thus, in reaction to a MW increase, firms may increase on-the-job training. More so than others, the institutional model predicts that even with homogeneous labor a higher MW leads to a "ripple effect" in the internal wage structure as managers raise the pay of above-MW employees to maintain morale and relativities – perhaps still allowing some internal compression given mobility costs for senior workers.

III. Empirical Evidence

For space reasons we do not survey the voluminous empirical literature on minimum wage laws, apart from a brief overview of important issues bearing on estimation of MW employment effects. The early minimum wage literature consisted primarily of national time-series studies examining the employment responses with respect to changes in federal MW laws. These typically found negative but low 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; Card and Krueger, 1995; Neumark and Wascher, 2008).

Newer studies rely heavily 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 than high-wage markets. The second is substantial growth in the number of states with state minima exceeding the federal minimum, at least prior to the 2007-2009 increases. Thus, introduction of federal MW increases should not impact (or barely impact) markets with high state minima while having larger impacts on states without binding state mandates.

Two broad groups of cross-section, quasi-experimental studies have emerged. The first typically uses the Current Population Survey (CPS) or other national household survey data to examine how employment effects vary with differential exposure to state and Federal minimum wage 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 from -0.2 to -0.3 (Neumark and Wascher, 1992, 2007; Burkhauser *et al.*, 2000; Sabia, 2009; see Orrenius and Zavodny, 2007, for an exception).

The second group of cross-sectional studies, of which our work is an example, is based on the differential impact of the policy change on samples of establishments, most often in fast-food or retail industries where one might expect the largest MW impact. This "quasi-experimental" approach uses variants of difference-in-difference estimates to evaluate the minimum wage effects. Best known is the work by Card and Krueger (1994), who uncovered small positive or insignificant employment effects in a sample of fast-food restaurants in New Jersey where the minimum wage was raised through state law, relative to the stores in the nearby Pennsylvania where the minimum wage did not change. A number of studies have followed in this tradition, typically finding small and insignificant employment effects from minimum wages. For instance, Dube *et al.* (2007) adopt a similar econometric approach to investigate the economic 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.

Among the concerns with the nation-wide MW studies have been inadequate controls of unobserved heterogeneity in employment growth across states and the presence of spatial autocorrelation with inconsistent standard errors and, in the establishment studies, a lack of external validity due to a limited geographic focus and insufficient lag times to capture the full minimum wage effects. Recent studies have attempted to address these limitations. Addison *et al.* (forthcoming) examine county-level employment at the restaurant-and-bar sector from 1990-2006 and incorporate trends in this sector's employment to account for unobserved heterogeneity in employment growth; the finding is no support for a negative employment effect. Dube *et al.* (2010) use minimum wage policy discontinuities at state borders to identify the effects of MW on earnings and employment in restaurant and other low-wage sectors within contiguous county pairs between 1990 and 2006. Their findings also reveal the presence of spurious negative employment effects due to heterogeneity in employment trends. A recent examination of the 2007-2009 MW increases by Addison et al. (2011) uses three national data sets and finds little evidence for MW employment effects or any "recession multiplier" of such effects.

In a paper more closely related to our study, Giuliano (forthcoming) uses data from individual employee personnel records of 700 stores of a nation-wide retail chain, from January 1996 to July 1998 to exploit geographic variation in state minimum wages and the employment impact of federal minimum wage increases.[3] Consistent with theory linking higher wages to fewer vacancies, she does not find evidence for an overall negative employment effect but does find differential employment responses for types of workers (teenagers versus adults) and across geographic areas (high- versus low-income areas).

In short, despite an extensive body of empirical work of increasingly high quality, there is still considerable disagreement over the sign and strength of the MW employment effect. Interestingly, this was also the state of the debate a half-century ago. In the late 1960s Peterson and Stewart (1969) did a major review of thirty years of "pre-regression" MW empirical studies and observed: "there is still no consensus as to their employment and other effects" (p. i). Engaged in the debate were institutional economists (e.g., Richard Lester 1960) claiming an approximate zero effect and an emerging group of neoclassical economists (including Peterson and Stewart) who claimed a negative effect.

IV. 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 comes from restaurants' confidential bi-weekly electronic payroll records on individual employees, collected by the authors for the period January 2007 through December 2009. Because the QSR sector has a sizable 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 negative effects on employment and/or total hours worked, these effects should show up among the most-affected businesses in our survey. Other channels of adjustment in response to the minimum wage 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

[3] Advantages of our data are the presence of information on hours of individual workers and that our businesses are franchisor rather than company owned. A national company may respond to a federal MW increase by adjusting its cost-sharing formulae with its stores, potentially diluting the differential geographic employment adjustments.

and a sample of managers. Below we describe the surveys and provide descriptive data.

A. Payroll Data

The restaurants in our sample are part of a national fast-food chain and are operated by three franchise owners who agreed to release payroll data for our study under condition of strict confidentiality.[4] Although our sample is non-random, we believe it is 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 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-large cities. The spatial differences provide 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 (kitchen staff, assistant manager, etc.), 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 (uniforms, meal discounts) and are omitted from what follows.

We have complete payroll records for all establishments over 72 pay-periods (36 months), commencing in January 2007. 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.[5]

Descriptive statistics for payroll records data are presented in Table 1. The sample contains approximately 64,000 individual-level observations (24 bi-weekly pay periods each year). The average hourly wage for the first year is $6.27, increasing to $6.67 and $7.15 during the latter two years (January-

[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.
[5] 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.

to December, with the MW increase July 1).[6] Regular hours worked remain stable across the study period. Overtime work declines; the share of employees with overtime decreases from 13% in 2007 to 9% in 2009 and average overtime falls from 0.74 to 0.45 hours.[7]

Although limited in its geographic focus, the data set used in this study possesses several advantages. First, hours and wages are measured at the individual worker level rather than being establishment-level averages. These data allow us to construct precise measures of each restaurant's compliance cost from the minimum wage, referred to as *GAP*, since we know each worker's wage at the time of the MW increase. The use of average restaurant and/or starting salary data from restaurants, as in prior QSR studies, does not allow one to calculate an exact measure of compliance cost. Data based on averages also limit the ability to explore important aspects of the minimum wage impact, such as wage compression or other changes in the wage distribution. A second advantage is the presence of regular and overtime hours worked, which are rarely available in other data sets. Finally, payroll data should be highly accurate because they are collected for tax-reporting reasons, decreasing concerns about measurement error due to imperfect information or poor recall. The payroll data were recorded prior to and independently from our survey and thus are not influenced by the research question.

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. In addition, population estimates from the U.S. Census Bureau are used to compute annual population density at the county level.

[6] 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.

[7] 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).

B. Manager and Employee Surveys

In order 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.[8] The manager survey response rate was 81% (66 of the 81 managers) and employee response rate 62%. (1,649 of 2,640 returned and answered at least one question).

The manager survey was structured as follows. In the first section, managers 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 minimum wage increase and document their opinions on both positive and negative aspects of the mandate. In the second section, we examine each cost-saving technique in more detail. We designed a comprehensive list of possible cost-saving responses to the MW increase. The list was partially based on alternative theoretical models but mainly on our 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 a separate survey given to employees. The majority of hourly employees are women and 64% are African-Americans. Relatively few workers (8%) are Hispanic. 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.[9]

About 15% of our restaurants (12 of 81) are located in the Atlanta metro area, where wages and cost of living are higher than elsewhere. Based on the payroll records, Atlanta-area restaurants have fewer employees per establishment, but these employees work longer hours, are more likely to be full time, and earn higher wages. For instance, in June 2009 the average hourly wage for workers in the Atlanta sub-sample is $8.03, compared to $6.94 for hourly employees elsewhere. Atlanta-area employees also display

[8] The questionnaires are available from the authors on request.

[9] Although many economists are opposed to raising the minimum wage (e.g., Neumark and Wascher 2008), the employees in this sample of restaurants are strongly supportive. The employee survey asked respondents to state if they would vote 'Yes' or 'No' to raise the MW from $6.55 (2008) to $7.25 (2009). Nine out of ten marked 'Yes'.

demographic differences (based on the worker survey), being 45% Hispanic and having stronger work attachment (49% consider the position "permanent" compared to 31% elsewhere).

V. Estimation Strategy

In this section, we describe our measure of the restaurant specific compliance costs of the MW, followed by discussion of the estimation strategy for identifying employment and hour responses.

A. Measuring MW Cost Compliance

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 employment and hours worked, we construct a measure, *GAP*, of the compliance cost from the mandate. Although other studies have used similar measures (Card and Krueger, 1994; Katz and Krueger, 1992; Dube et al., 2007; Giuliano, forthcoming), our measure has advantages due to the availability of individual-level data on wages and hours worked.

Specifically, GAP_{jt} is constructed using data from individual worker *i* at restaurant *j* at time *t* (pay period). It measures 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). Specifically, the *GAP* for restaurant *j* at time *t* is defined as:

$$GAP_{jt} = 1 + [(\Sigma MW_{ijt} h_{ijt-1} - \Sigma W_{ijt-1} h_{ijt-1}) / \Sigma W_{ijt-1} h_{ijt-1}],$$

where the bracketed term is summed over workers i for whom $W_{ijt-1} < MW_t$ (i.e., those for whom MW_t is binding) while set at 0 for workers for whom $W_{ijt-1} \geq MW_t$. The numerator in the brackets is the change in the wage bill between time periods t-1 and t if hours remain the same, while the denominator is the original wage bill. *GAP* is calculated by simply summing up each employee's additional earnings (wages times hours) required for compliance with the higher MW. That is, GAP_{jt} measures the change in the wage bill required for compliance with the MW, holding hours constant. Adding 1 to the bracketed term converts the measure from a proportion to a wage ratio (say 1.15 or 15%). We use the natural log of *GAP* (e.g., ln(1.15) = .140) in order to estimate a double log model and employment elasticities with respect to the MW *GAP*. The ln*GAP* (say .140) is a "proportion" 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).

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. The relative cost-impact of the minimum wage grows each year as a larger share of workers are affected and require larger increases to reach compliance, from 2.6% of payroll as a result of the July 2007 MW, to 4.6% in 2008, and to 6.8% in 2009.

The gap measure used here differs from those in previous 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 new 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 the precise compliance cost since we do not know how many workers are impacted by the minimum wage 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 problems, Dube *et al*. (2007) define their measure as a share of workers affected by the city-wide MW increase (their "wage gap" variable is called "treatment intensity"). Although they account for the *quantity* dimension of the MW (i.e., the number of workers whose wages are raised), not accounting for the *price* dimension (i.e., by how much wages must be raised for each worker) may generate an imprecise measure of the cost shock from a minimum wage mandate. Similarly, using establishment averages of wages and hours would 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 (how many workers require wage increases) and cost (by how much these workers' wages must increase).

B. Regression Model: Identification of MW Employment and Hours Effects

The empirical analysis of MW impacts on employment and hours uses establishment-level bi-weekly

averages, created from the payroll data on individuals. Variable definitions, as well as data sources, are provided in Table 4.

Our empirical strategy relies on the following reduced-form employment equations:

(1a) $\ln(E_{jct}) = \alpha_0 + \psi \ln(GAP_{jct})\cdot MW_t + \alpha_1 \ln(GAP_{jct}) + \alpha_2 MW_t + \lambda \Delta \ln Sales_{jct-2} + \gamma Z_{ct} + \mu_c + \varepsilon_{jct}$

(1b) $\ln(h_{jct}) = \alpha_0 + \psi \ln(GAP_{jct})\cdot MW_t + \alpha_1 \ln(GAP_{jct}) + \alpha_2 MW_t + \lambda \Delta \ln Sales_{jct-2} + \gamma Z_{ct} + \mu_c + \varepsilon_{jct}$

where the outcome variable is either $\ln(E_{jct})$, the log of the average employment in store *j*, county *c*, during period *t* (bi-weekly), or the log of aggregate hours h_{jct}, the sum of regular plus overtime hours. MW_t is the time treatment period set to 1 for the 6 months *after* each MW increase (i.e., August through January in each year).

The key variable is the interaction term $\ln GAP \cdot MW$. Its coefficient ψ measures the impact of the cost increase from the minimum wage mandate on establishment employment (or 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.[10]. Based on standard models, competitive theory predicts $\psi < 0$, monopsony $\psi > 0$, and institutionalist/behavioral $\psi \approx 0$.

The regression also includes $\ln GAP_{jct}$, which controls for differences in employment (hours) levels between highly- and lowly-impacted restaurants prior to the MW mandate. $\Delta \ln Sales_{jct-2}$ is the monthly change in log sales, included to capture demand shocks not captured by fixed effects and other controls. It is lagged two periods to avoid simultaneity between concurrent employment and sales (results are insensitive to use of shorter or longer lags). The sales variable is not an ideal measure of output changes since it reflects a mix of prices, product mix, and transactions. The vector Z_{ct} includes time-varying county-level characteristics reflecting local labor market supply and demand factors; specifically, county-level population density and total private sector employment minus employment in the *Accommodations and Food Services* and *Retail* sectors. Vector μ_c represents state, county, and establishment owner time-

[10] In the MW literature, estimated teen elasticities with respect to the MW are far smaller than are estimates of labor demand elasticities in the larger literature (Hamermesh 1993). This is not surprising given that a small proportion of 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. Our measure of ψ is conceptually closer to a true elasticity, being based on employment (hours) changes with respect to the increase in required labor costs.

invariant fixed effects. Composite error term $\varepsilon_{jct} = \varepsilon_{jt} + \varepsilon_{ct}$.

A few econometric issues warrant mention. The error term is likely to be correlated for each establishment over time; failure to control for this serial correlation leads to inconsistent standard errors (Bertrand *et al.*, 2004). Therefore, all standard errors are clustered on individual restaurants. Possibility of heterogeneous trends across counties over time is another potential source of bias. Controlling for time-varying county-level supply and demand shifters (due to the business cycle) mitigates this concern.

VI. Findings from Payroll Records

In this section we report our main findings of the impact of the 2007-2009 federal minimum wage increases on employment and hours from restaurant payroll records. Graphical descriptive evidence is first presented, followed by estimation results.

A. Descriptive Evidence

Cumulative averages across all establishments in our sample would conceal variation due to differences in pre-minimum wage levels and the size of *GAP* (compliance costs). Thus, we group establishments with broadly similar anticipated minimum wage impacts. Establishments in the sample are divided into three groups – least, middle, and most affected based on the size of *GAP* in March-May 2007, prior to the initial July 2007 MW increase. The "least" affected group represents the lowest 25 percentiles of *GAP* and includes all zero *GAP* stores (no compliance costs from the first MW increase). The "middle" 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 shows plots of bi-weekly establishment-level averages in observed (rather than mandated) hourly earnings, employment, average regular hours, and overtime hours for the least, middle, and most affected groups of restaurants over the three year period. 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 minimum wage 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.

Owners confirm that the size and extent of performance based raises were significantly reduced as they sought to contain the MW cost impact. Thus, our data show that in the short-run MW cost increases are partly offset by slower wage growth for other workers. Evidence from Card and Krueger (1995, p. 164) and our interviews suggest that in future years part of this compression will be reversed as managers hold steady or provide small raises for workers at or close to the minimum, while giving more substantial raises to relatively senior workers.[11] In short, we observe that the three federal minimum wage hikes had substantial exogenous effects on average hourly earnings and that the intensity of these treatments differed substantially across restaurants. Estimates of arguably causal employment and hours effects will be based on outcome differences between restaurants with different compliance costs (a varying *GAP*).

Panels B-D of Figure 1 show differences in outcomes between the least, middle, and most affected restaurant groups. Panel B presents average employment. Despite jumps in the average hourly earnings observed in Panel A, we do not observe corresponding reductions in employment that correspond with the size of compliance costs. In short, overall patterns appear similar among the three groups. Although we can see a small decline in employment after the July 2008 hike, overall employment is 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 regular hours per worker across the three groups of establishments. Although regular hours fluctuate substantially on a pay-period basis (in part because the number of days varies

[11] All owners felt conflicted by the desire to give performance increases to maintain morale and keep experienced workers, but to limit them to preserve shrinking profit margins. One owner said the profit pinch, along with the low probability of turnover in a slack economy, led him to give zero performance increases; another stated "compression caught up to us" and reluctantly gave limited performance increases in 2009 even though his labor cost share reached an all-time high. Both agreed their internal wage structure would partially-to-completely rebound over time if business and profits return to normal.

slightly across pay periods), there is no apparent trend in average hours worked over the study period, nor are there apparent differences in trend among the three groups.

Finally, panel D shows the average overtime hours worked. Although the average number of overtime hours is very small (one hour on average), there is a significant difference in overtime hours among the three groups of establishments. In the two more affected groups, overtime work is practically non-existent, while it plays a modest role for the least affected group, where regular bi-weekly hours and full-time work are far more substantial. Trends in overtime hours appear similar across store groups.

Graphical analysis demonstrates that despite substantial differences in the increase in the average hourly wage brought about by the Federal minimum wage increases from 2007 to 2009, there are few evident differences in changes in employment or hours among restaurants most and least affected by wage increases. Were minimum wage shocks producing large employment or hours adjustments, we would expect these to be evident from the raw differences between the most and least affected restaurants. Smaller effects, however, might be masked by other changes co-varying with the size of MW employment effects. Thus, we next explore effects of the MW increases on employment and hours using a regression framework that accounts for covariates.

B. Regression Evidence

As a first step, we estimate models examining employment in the six months after relative to six months before each of the three MW increases, with no differentiation among restaurants based on compliance costs (*GAP*). The change in employment is shown by the coefficients on the treatment dummy *MW*, as seen in Table 5 for each of the three years.

We first estimate this relationship absent covariates (the left panel of Table 5), which largely mimics the earlier visual evidence. There is a small but statistically significant decline in employment of about 3 percent six months after the second minimum wage increase, but no significant relationship is found for the first or third minimum wage hikes. Next we add county, owner, and restaurant fixed effects to establish sensitivity of our estimates to these variables. The coefficient on the MW treatment variable is robust to the inclusion of store fixed effects (columns 3-6), as well as county and owner fixed effects

(columns 7-12). Adding store-fixed effects sharply increases R^2 values, from effectively zero to about .9. Notably, the magnitudes of the estimated employment effects are small and imprecise, indicating modest employment declines of 1% to 3% following the 2007 and 2008 MW increases and a small increase (1%) following the 2009 increase. This exercise demonstrates that time-invariant controls for unobserved county, owner and store-level factors are important determinants of the levels of employment in our sample, but they do not sweep out the "raw" employment change correlations with the minimum wage.

In Tables 6A-6C we present our principal results based on a specification that accounts for store-specific MW compliance costs, as measured by *GAP*. Estimates of φ, the coefficient on ln*GAP·MW* (shown in line 1 of Tables 6A-6C), provide estimates of the employment (or hours) treatment effect of the minimum wage mandate. Table 6A provides results based on the 2007 MW increase, 6B the 2008 increase, and 6C the 2009 increase. Our precise measure of MW compliance costs does not change the finding in other recent studies that MW employment effects are variable, on average small, and clustered around zero. Specifically, there is no statistically significant evidence that stores that experience greater MW compliance cost shocks reduce employment relative to restaurants little affected by MW. Estimates are positive and insignificant for the 2007 increase, with compliance cost employment elasticities of .14 to .19 (and .27 for hours). For the 2008 increase, we obtain negative but insignificant elasticity estimates of about -.16 to -.28 for employment, but +.16 for hours. The final minimum wage increase in July 2009 is associated with an apparent gain in employment for stores most affected, estimates of φ being about .45 for employment and .3 for hours. As mentioned previously, these elasticities are with respect to the required compliance costs and should be substantially larger than the teen elasticities commonly estimated in the MW literature.

In results not shown, we compare the Table 6 estimates using our precise *GAP* measure of dollar compliance costs with estimates from identical specifications, except that we replace *GAP* with a less precise treatment variable measuring the share of affected workers, similar to that used in Dube *et al*. (2007). Both the sign and size of the estimated effects are similar for the first and second MW increases. For the July 2009 MW increase, we continue to obtain positive estimates, but they are lower in magnitude

than estimates shown in Table 6C. The failure to find significant negative employment effects in prior studies using a similar methodology does not appear to have resulted because of imprecise measurement of compliance costs.

In further results (not shown), we examine "long-run" MW employment and hours effects based solely on information before and after all three MW increases. For each restaurant we include two observations, the employment (hours) in January 2007 and in December 2009.[12] The ln*GAP* measure here represents the cumulative *GAP* (compliance costs) from the three MW increases, while the MW dummy is set to 1 for 2009 observations. Measuring the MW effect by the coefficients on ln*GAP·MW*, we obtain negative (and sometimes large) coefficients, but statistically insignificant in both the employment and hours equations. These cumulative estimates are consistent with the evidence seen in Tables 6A-6C.

Brief mention can be made of the coefficients on control variables in Tables 6A-6C. The change in log sales (lagged two periods) has a positive but weak impact on employment. In work not shown, principal results are insensitive to the length of the lag on sales (at least one lag is necessary to avoid simultaneity between sales and employment growth). Other controls, including private sector employment at the country level and population density, are not statistically significant specifications with county fixed effects. We also control for ln*GAP*, as measured in 6A-6C by the initial compliance costs prior to the first MW increase (similar results are obtained if we use the year specific compliance costs), and include a MW dummy (line 3) to account for differences in employment in the months prior to and following the MW increase that are independent of the compliance costs. Neither ln*GAP* nor MW is statistically significant.

Our finding of an approximately zero MW employment and hours effect supports the institutional/behavioral model and seemingly contradicts predictions from both standard competitive ($\psi < 0$) and monopsony models ($\psi > 0$). The fact that our sample spans three years and covers three MW increases provides further discriminating insight. The competitive model's predicted negative employment effect might not show up in the span of several months because of adjustment costs and

[12] With one observation per restaurant before, and one after, there is no concern about serial correlation in errors.

other delays; over a span of three years, however, we would expect negative employment effects to be discernible. We cannot rule out the possibility that had our data extended longer, we eventually might see employment effects through attrition of stores and capital-labor substitution. The pattern of regression coefficients is also at odds with the monopsony model. The model predicts a *positive* MW employment adjustment for the first round (assuming the mandate moves the wage from a sub-competitive level toward a competitive level) but thereafter a negative effect should start to emerge as the MW crosses the competitive threshold and moves firms up their labor demand schedules. We do not find such a pattern; in fact, the most positive (but insignificant) estimates are for the last MW round.

C. Additional Robustness Checks

In addition to the specification checks already noted, we report below on additional robustness checks. We find little basis for modifying previously shown results.

Using the *GAP* variable to measure the impact of the MW on total labor costs is likely to understate the wage costs (and overstate estimates of φ) *if* wages of workers above the minimum are substantially affected. This "ripple effect" may reflect managers' decisions to preserve tenure-wage hierarchy within the store. Capturing the ripple effect is challenging because regular performance-based increases occur along with MW-induced wage increases and we cannot directly observe what raises would have occurred absent the MW. One way to address this issue is to identify workers for whom the MW is non-binding prior to each July increase and then examine how their wages change over the course of the year, assuming *all* the increase was due to MW. Including this "ripple" effect in the GAP variable creates an upper bound for the estimated total cost-impact of minimum wage on labor cost.[13] A related alternative is to examine the change in the total wage bill using *actual* wage changes but keeping the hours fixed; this expanded definition of *GAP* likewise overstates the policy impact of the MW.

Using either measure of an expanded *GAP* including spillovers, we do not see substantial changes in estimates of φ or in other coefficients (results not shown). Separately, we find the sizes of estimated

[13] The "ripple" effect is defined similarly to the GAP variable: it measures the percent increase in the total wage bill due to wage increases only for those employees whose earnings in January through May in a given year are already above the upcoming July minimum rate, keeping hours worked fixed.

spillovers, while overstated, are smaller than the mandatory compliance costs of the MW. Moreover, the two wage effects move differently over time. While the MW compliance costs rise with each year's increase, pay increases for employees above the new minimum weaken.

Our preferred estimates of MW employment and hour effects included fixed effects for location (county) and franchise owner. Exclusion of these controls had little effect on estimates. Nor did exclusion from the sample of the Atlanta-area restaurants substantively affect results.[14]

II. Alternative Channels of Adjustment

The finding of generally small and insignificant impacts of the minimum wage on employment and hours in this study (and others) challenges the predictions of the competitive and monopsony models – at least if considered in relatively standard form. But if employment and hours do not adjust to absorb the cost increase, then what does? We address this question by exploring a wide range of additional channels of adjustment. Evidence is obtained primarily from manager surveys but also the payroll data, establishment owners' records, and interviews. Some of these CoA have received attention in earlier MW studies; others are largely undocumented.

A. Costs and Prices

The competitive and institutional models predict an increase in the output price in response to a binding minimum wage while the monopsony model predicts a decrease in price. As reported previously, using the payroll data we calculate that the direct MW-induced increase in compliance costs (i.e. raising wages below MW just to the minimum, absent any other pay increases) was 2.6%, 4.6%, and 6.8% for the three years. We add to this the estimated increase in employer payroll taxes (OASDI-HI plus federal and state UI, or about 13% of payroll).[15] If there were constant returns-to-scale in production and full pass

[14] One other robustness check requires mention. In March, 2008, the corporate office of this chain issued a requirement for all franchised restaurants to stay open for an additional hour Sunday through Wednesday and for an additional two to three hours Thursday through Saturday. After negotiation with the franchise owners, in August the mandate for Thursday night was removed and twenty-nine stores were exempted from most other requirements. Regression analysis (not shown) distinguishing between exempt and non-exempt stores leaves our reported results unaffected. It is worth noting that the 2008 MW increase is the only one of the three years for which we obtain negative (but insignificant) estimates of MW employment effects. The mandate helps explain the negative employment effects seen in Table 5 across all restaurants, but need not bias coefficients in Table 6B based on differences in compliance costs.

[15] Owners emphasized that discussions about MW mostly ignore the compliance costs from higher payroll taxes.

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 of costs, but use a guesstimate of 24% based on data from one of the franchise owners showing that non-managerial payroll was 18% to 20% of total sales.[16] Taking each year's MW compliance costs (inclusive of payroll taxes) and labor's share, the cumulative three-year increase in total costs due directly 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. However, restaurants faced other sources of labor costs, including performance increases and MW spillovers, and large changes in non-labor costs.

We calculate an extended measure of labor costs for our restaurants taking into account wage increases to MW workers beyond the new minimum, spillovers to above-MW workers, plus any performance increases. We do this by examining the total increase in the wage bill (holding constant hours) over the three year period, 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, we assume that 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 larger 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.[17]

To what extent were restaurant owners able to offset these higher labor and non-labor costs through increases in menu prices? We cannot measure price changes precisely, but did obtain price hikes from the business owners for the 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

[16] Labor's share of total costs should exceed its share of sales. 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.

[17] 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 (ceteris paribus) for stores to handle the 2009 MW hike.

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 only source of cost increase, it seems likely that most-to-all could have been passed through to consumers.

For this sample of QSRs, higher prices rather than cuts in employment and hours seems to be the most important CoA for higher MW cost. We also find evidence, however, of second-order but non-trivial adjustments through a variety of other channels.

B. Internal Wage Structure

Increases in the minimum wage may be partially offset by awarding smaller-than-normal (or zero) pay raises to workers with wages above the minimum, leading to internal wage compression. Alternatively, managers may choose to preserve the wage hierarchy. Our data reveal the former. When we compare the size of the average wage increases among workers for whom the MW was and was not binding, we find that workers at the high end of the wage distribution receive smaller absolute (and relative) pay increases. Hence, the MW raised the mean of the internal pay distribution but reduced dispersion. Figure 2 shows changes in wage distributions between low and high-wage workers, before (March-May) and after (August-December).[18] Wage distributions shift significantly to the right for low-wage workers, with a high frequency of workers receiving the minimum. For higher-wage workers, the distribution barely shifts; thus, over the three MW rounds the higher-paid, more senior workers experienced a relative decline in pay position. Dube *et al.* (2007) report a similar finding. Managers reported that their more senior/experienced workers were typically the most reliable and high-performing and the threat of turnover and poor morale placed limits on the degree of compression. As earlier indicated (footnote 11), owners expected – given macroeconomic improvement and absence of further MW increases -- to gradually reverse this compression in future years by giving more senior workers catch-up increases.

[18] For the period before each MW increase, we wanted months as close as possible to July 1, but excluded June in case MW wage hikes occurred prior to July. For the post-MW period, we excluded July to provide time for restaurant compliance and measured wages through the end of the year to allow sufficient time for spillovers.

C. Turnover

Labor turnover at all ranks is costly to firms. If higher pay rates reduce labor turnover and vacancies, firms can partially offset MW costs by savings in hiring, training, and separation. Higher wages, per the dynamic monopsony model, may also reduce vacancies and lead to a higher level of employment and hours. Empirical evidence on the MW/job attachment link is limited, but generally finds lower turnover resulting from the MW (e.g., Dube et al., 2007; Reich *et al*., 2005; and Fairris and Reich, 2005 for living wage ordinances).

A common way to examine turnover is to apply duration analysis, although our data structure limits what we can do.[19] The completed length of employment is unobserved; we do not observe entry except for recent employees and do not observe exit for those who continue beyond December 2009. Left censoring is addressed by considering only "new hires" — workers who enter the sample *after* mid-January 2007 (i.e., after the first pay period). Using this definition, we observe a high turnover rate, as is common in the fast-food sector: the median "survival" in the sample is 14 weeks (7 pay periods) and roughly 70% of all new hires are not in the sample by the end of December 2009. In addition, we find that low- and high-wage establishments do not have any significant difference in survival spells, suggesting that wage differentials across stores reflect local labor market conditions.

Alternatively, we can evaluate employee turnover by measuring payroll attrition rates, the share of the total workforce observed in pay period *t* but not observed in period *t+1*. By this measure, if all workers are present in two consecutive pay-periods (not counting new hires), the turnover rate is zero. Figure 3 plots the average shares of workers missing from consecutive payrolls for the entire study period. Turnover defined by this measure is high—approximately 8%—but there is a downward trend, particularly pronounced after the second MW increase in July 2008.[20] Over the entire three year period, turnover (attrition) rates fell from roughly 10% to 5%. Rates are not stable across months, with spikes in summer and in December-January.

[19] The "failure event" in our case is falling out of the sample. If a worker is not observed for two or more months and then returns to work, the payroll records count the person as two distinct workers.

[20] 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.

The causal relationship between MW and the decline in turnover is difficult to isolate because our sample period includes peak and trough of a business cycle. The raw data show little downward trend in turnover until the second half of 2008 when the economy seriously slumped. In results not shown, we examined the relationship between store-level probability of attrition and the compliance cost of the MW. We used specifications that mimicked those seen in Tables 6A-6C, but with restaurant turnover (attrition) as the dependent variable. As expected, restaurants with low wage levels have significantly higher levels of turnover than do higher wage restaurants (as reflected in a positive coefficient on ln*GAP*). Based on coefficients on ln*GAP·MW*, which measures how turnover varies with MW compliance costs, we did not find significant turnover effects following the 2007 and 2009 increases, but did find a significant decline in turnover following the 2008 increase.

Whatever the impact of the MW on turnover, it is clear from the manager survey that store managers regard it as important. When asked to comment in an open-ended question about the most positive aspect of the higher MW, managers emphasized lower turnover, along with higher morale, greater worker effort, and more and better job applicants. 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. An owner said on turnover, "We hate to go outside." Although we find a weak and sometimes zero statistical relationship between the MW and turnover rates, some portion of the MW cost effect is arguably offset by reduced training, hiring, and separation costs (Arrowsmith et al. 2003).

D. Operational and Human Resource Efficiencies

We next explore the importance of operational and human resource (HR) efficiencies with data collected from the manager surveys and interviews. Although these data do not permit formal analysis, insights are provided on the "black box" of internal operations generally absent in the MW literature.

As indicated earlier, managers were asked to estimate how much of the MW labor cost increase they could offset by implementing various operational efficiencies. The responses averaged 23%. 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 the MW 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). The survey question was designed so cost-savings from projected price increases and employment/hours changes were explicitly excluded.

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. Figures 4A-4C provide 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 first with efficiencies in HR practices. We identified several such channels. Most of the 66 managers (90%) planned to increase performance standards and, as seen in Figure 4A, among those who provide ratings on its cost savings about two-thirds rank it as "very important." Higher performance standards include things such as requiring a better attendance and on-time record, faster and more proficient performance of job duties, taking on additional tasks, and faster termination of poor-performers. 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. Only 10% of the managers planned to reduce training, contra the hypothesis of the competitive model. The reason given in interviews is that greater operational efficiency requires greater worker 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 are consistent with 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. A significantly greater proportion (60%) planned to reduce work hours, although 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 the 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 (a half-person cannot run a cash register); and team spirit and a cooperative employee attitude are the most important factors for successful operations and lay-offs or cuts in hours undermine these.[21] With regard to the labor input, it appears effort is the short-run continuous adjustment variable in the production function and employment is at least 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 exogenous cost) moves the stores past these targets and owners place pressure on managers to squeeze out some of the added cost through tighter operations. With regard to the MW increase, the number one response was to gain more productivity out of the workforce, such as from cross-training, multi-tasking, and tighter work schedules. As indicated in Figure 4B, boosting morale was also a major CoA (cited by 92%). Managers said one important way to work on this margin is to cast the

[21] 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.

MW cost shock as a "challenge to the store" and use it to energize employees to improve productivity for the sake of the "team." The managers deliberately worked to create interdependent utility functions among their crew to create productive synergies, team spirit, and self-enforcement of high work norms through enhanced peer monitoring. The opposite side of this coin, we were told, is that the greater emphasis 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. This is one reason why managers 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 potential CoA in the operations and productivity area is new capital equipment. We did not include this option on the manager survey because owners make the 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 described earlier 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 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 typically 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/behavior movement to a higher isoquant (generating more output from given managerial inputs by energizing effort/attention to unexploited areas of cost-saving).

Third, owners said the "quality" of the manager is the single most important determinant of unit success (given location, etc.) and 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 two competing hypotheses.

E. 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 (Figure 4C) 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.[22]

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 an employee check more often 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.

F. Profit

The final CoA is profit. In effect, profit is the residual CoA that results from the movement in all the other revenue and cost channels earlier discussed. It is crucial, however, as profit – and profit potential – are fundamental to maintaining and growing companies and their base of jobs and hours.

Prior evidence on the profit effect of MW is scarce; several studies, however, find zero or small negative effects (Card and Krueger 1995; Draca, Machin and Van Reenan 2011)**.** Due to confidentiality

[22] Previously, customers received their food at the counter and then went to the condiment bar, where they could take as much ketchup, sugar, and other supplies as desired. A plan in progress at the time of our interviews was to keep the condiment bar, but first ask customers at the counter how many packets of ketchup (etc.) they wanted and include these with the order in the hope the net effect would provide a reduction in cost.

concerns, none of the owners for this study would share data on annual profit. We did obtain, however, data on profit *change* for approximately three-fourths of the stores spanning the three MW increases. Unfortunately, the profit data cover a fiscal year (e.g., starting October 1) so disentangling the MW effect is difficult; hence, for this and other reasons our evidence is best considered suggestive.

Annual profit change 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). Thus, FY 2007 includes only 3 months of the July 1, 2007 MW increase, while FY 2010 begins 3 months following the July 1, 2009 increase and reflects the wage base following the three MW increases, along with all other cost changes.

We earlier showed that the "bite" of the MW on store labor cost 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 diminished 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 it 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., rising 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).

The conclusion we draw from our sample and time period is that (1) the MW by itself has a negligible to small effect on profits, 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) the three 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.[23]

VIII. 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 modest sample size of restaurants, the qualitative and subjective nature of the manager data, and anecdotal reports from personal interviews, on balance we believe the data set makes possible better-measured answers to some old questions and rarely-measured answers to new or under-explored questions.

Second, we recast analysis of the minimum wage into a broad "channels of adjustment" framework, moving well beyond the conventional emphasis on employment/hours effects. The effect of MW on the internal operation of firms, in particular, has been left as a mostly unexamined black box (but see Arrowsmith et al. 2003). We believe the CoA framework usefully focuses attention on the many margins potentially affected by MW and the various economic agents who bear the cost.

Third, the employment effect of MW has been a point of controversy for decades. 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 recent studies, that the measured employment impact is variable across establishments, but overall not statistically distinguishable from zero. The same absence of a significant negative effect is found for employee hours, even when examined over a three-year period.

Fourth, our study finds evidence that the cost of MW is passed along and absorbed through a wide-range of CoA. The quantitatively most important is increases in product price to consumers. Other CoA

[23] 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.

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 the other CoA, but in economic hard times these 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 found in the literature. Some argue that such estimates are a statistical artifact from data mismeasurement or flawed procedures, while others appeal to structural or dynamic forms of monopsony in labor markets. 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 and hours cuts as a relatively costly and perhaps counter-productive option, regarding them as a last-resort. A zero (or very small) employment effect, from this perspective, is compatible with economic theory as long as the theory posits multiple CoA with differential costs.

Sixth, our study has sought to identify alternative CoA in three labor market models – competitive, monopsony, and institutional/behavioral – and evaluate which model's predictions best align with empirical findings. This comparison is challenging because the models come in different versions; after extensions and qualifications they start to shade into one another; and in some cases the models yield similar CoA predictions. Our first-line judgments are (1) all three models capture important elements of truth regarding the MW and particular CoA and (2) the different permutations of each model and limitations of data constrain ability to differentiate among them. Given this caveat, we find that certain predicted effects of both the standard competitive and monopsony models are not supported by observed CoA in this study. Examples include employment and hours (both), training (competitive), and prices

(monopsony). The evidence is ambiguous in other areas, such as managerial "tightening-up" and changes in the internal wage structure. Judged as a whole, the CoA evidence tends to best cluster around the institutional/behavioral model and, in particular, its emphasis on human-related adjustments and internal operations. However, this conclusion has to be duly qualified with recognition that the institutional model is also least formalized and tightly specified, giving it perhaps greater facility to "fit the data." Nonetheless, we conclude that Richard Lester – neo-institutional labor economist of the 1950s and strongest proponent of a zero MW employment effect – may in hindsight have had more of the story right than neoclassical price theorists were for many years willing to consider.

Although the short- to medium-run employment effects predicted by the competitive model do not show up in our data, we suspect that the competitive model provides guidance for the longer run. To the extent that impacted businesses cannot fully offset or pass through MW cost increases, as was the case following the third year (2009) MW increase, there remains the question of business survival, expansion of existing franchises, and entry over time. We do not observe establishments in our sample beyond the third year or observe how the MW affects entry; thus, we cannot rule out negative employment effects over the long run. Recent work on establishment level productivity (Syverson 2011) shows large differences in productivity even within narrowly defined markets and finds that long run survival is strongly related to productivity. It would not be surprising, therefore, if a higher MW led to the gradual demise of the least-efficient/profitable segment of establishments in an industry.

Seventh, and finally, while our study has not sought to make a welfare case for or against the minimum wage, it does offer useful insights. A competitive model with few CoA yields a relatively negative verdict on the efficiency effects of a MW while an imperfect/behavioral market model with numerous internal CoA is likely to yield a less negative or even positive assessment. Getting the model right is important, therefore, for policy evaluation of minimum wages and other labor laws.

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Figure 1. Average hourly earnings, employment and hours, January 2007 – December 2009

Notes: Shown are establishment averages for hourly earnings, employment, regular hours and overtime hours for three groups of stores by pay-period (bi-monthly). "Least" affected stores include those where *GAP* is in the bottom 25 percentiles, "Middle" the 25th–75th percentiles, and "Most" percentiles 75 and over. The zigzag pattern of hours is due in part to variability in the number of days in bi-monthly payroll periods. Vertical lines mark the MW increases; horizontal lines in Panel A show the MW mandates.

Figure 2. Distribution of hourly earnings: High and low-wage workers before (March-May) and after (August-December) the 2007, 2008, and 2009 minimum wage increases

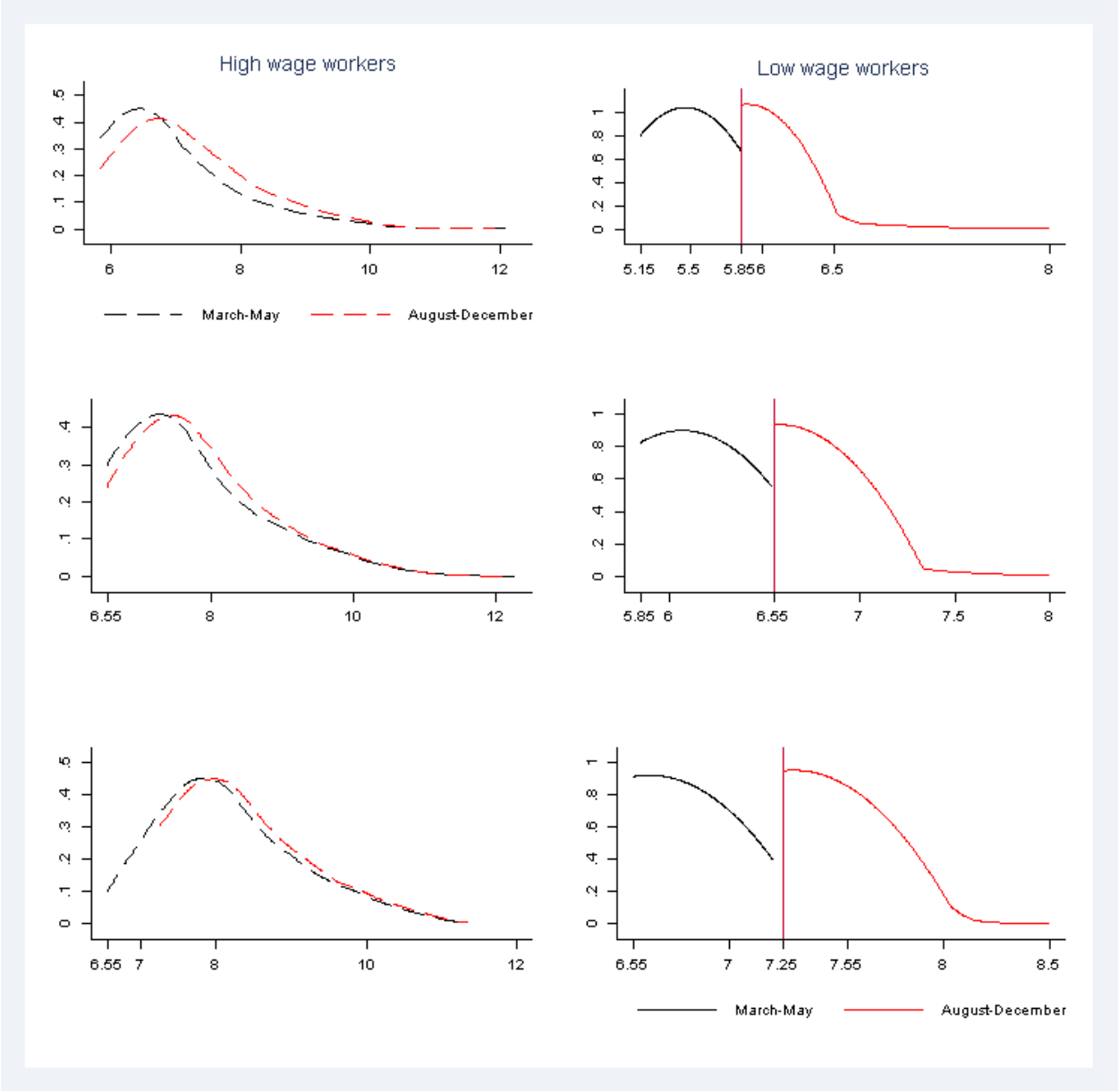


Figure 3. Average turnover of workforce, January, 2007-December, 2009

Figure 4A. Reaction to higher labor cost due to MW increase:
Human resource practices and cost-savings

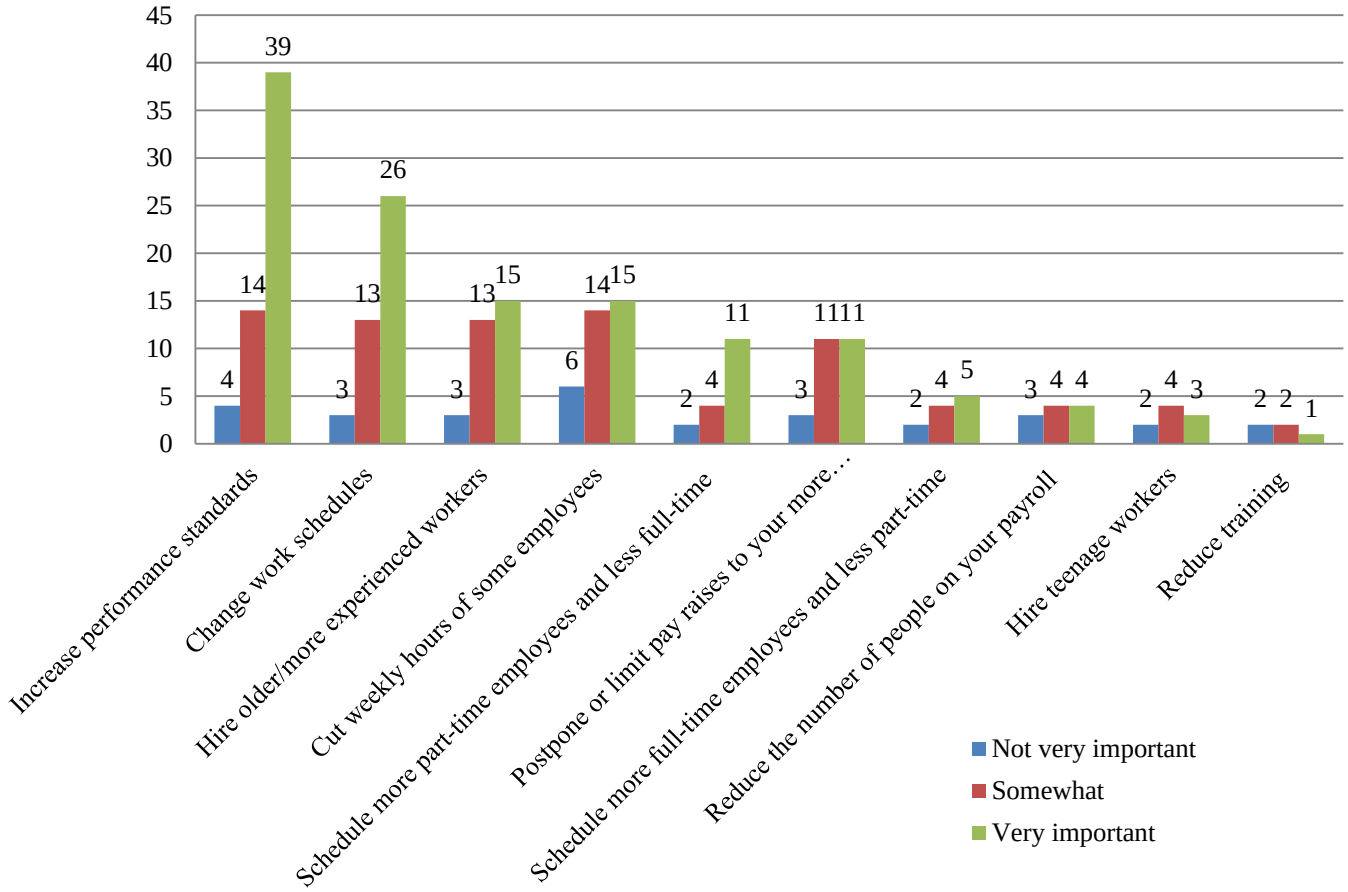


Figure 4B. Reaction to higher labor cost due to MW increase:
Operational efficiency and productivity

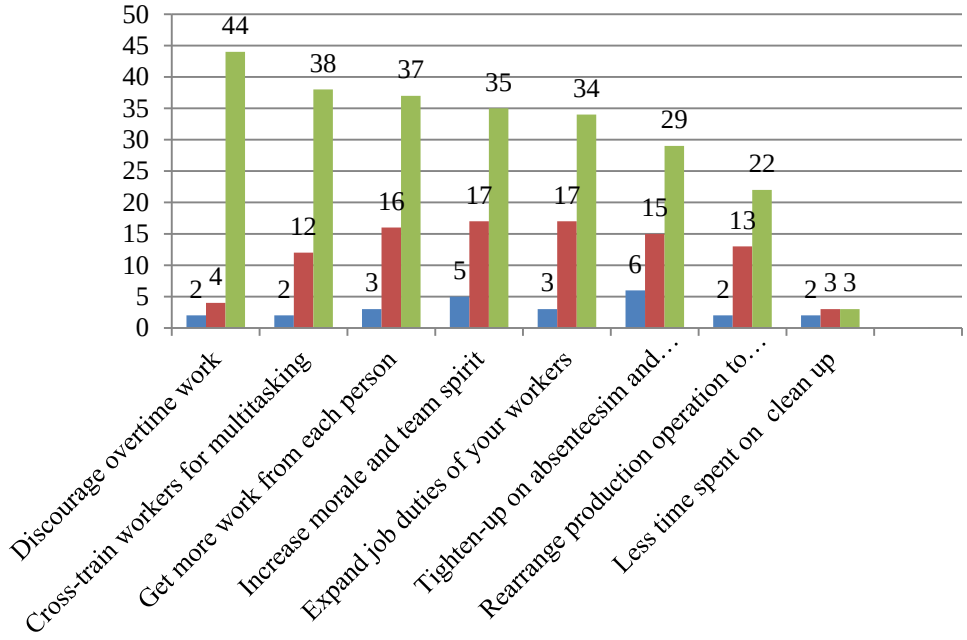
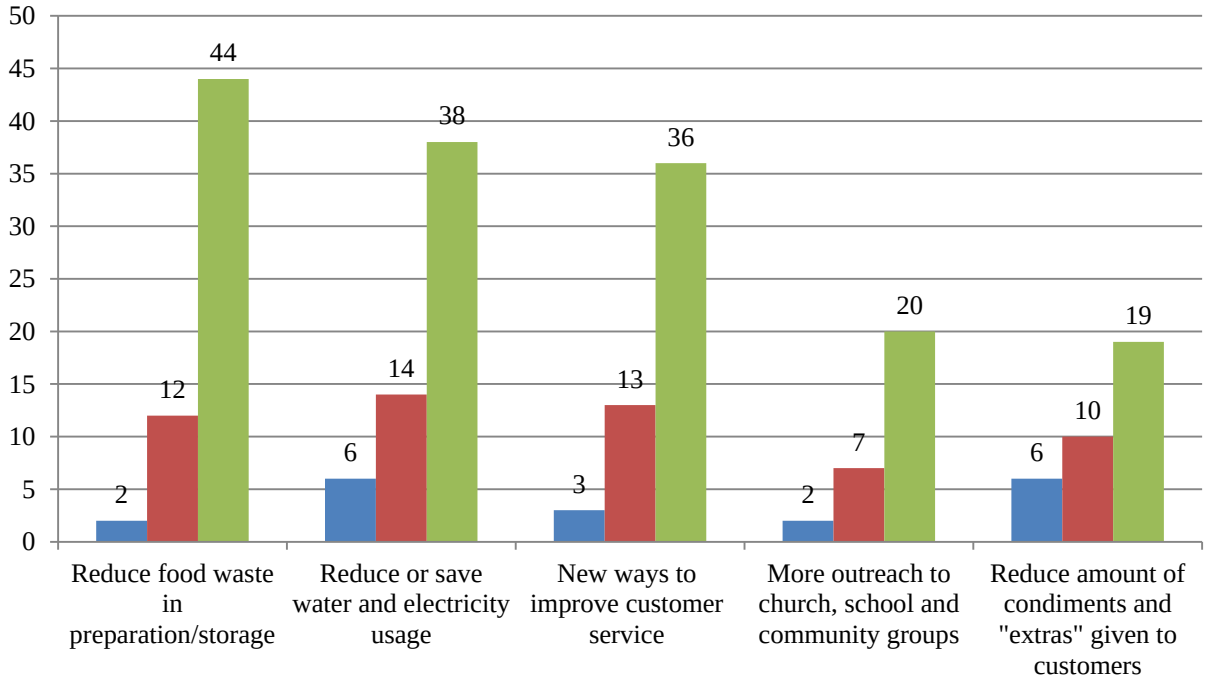


Figure 4C. Reaction to higher labor cost due to MW increase:
Non-labor expenses/customer services and cost-savings



Notes: Figures 4A-C show distribution of answers to Question 7 in the Manager Survey. Each manager was asked the following question: "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.

Table 1. Summary statistics: Individual-level payroll records

Year	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 work (dummy)	63,718	0.12	0.33
	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 work (dummy)	65,290	0.10	0.30
	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 work (dummy)	63,975	0.07	0.25
	Overtime hours	63,972	0.45	2.76

Table 2. Employee descriptive 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	0.38
Divorced/widowed	1,451	0.054	0.226
Living with partner	1,451	0.085	0.28
No. of children under 18	1,625	0.958	1.3
Age	1,628	28.194	10.719
School in Fall (=1)	1,623	0.34	0.474
Level of schooling			
Some high school	1,611	0.273	0.446
High school grad/GED	1,611	0.475	0.5
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

Table 2 (continued). Employee descriptive characteristics from the employee surveys

Variable	N	Mean	St. dev.
Wage in June 2009	1,555	6.987	1.416
Average hours per week	1,568	29.510	8.309
Tenure at store (in months)	1,571	26.812	39.992
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	0.44
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 (mean GAP) as a percentage of payroll

Year	Mean	St. dev.	Min	Max
2007	2.598	2.083	0	8.150
2008	4.640	2.628	0	9.157
2009	6.805	2.638	0.115	10.639

Means are calculated for the same 81 restaurants each year. See text for description of *GAP*. The total increases in payroll, holding hours constant and inclusive of the *GAP*, were 6.0%, 6.6%, and 7.9% following the 2007, 2008, and 2009 MW increases.

Table 4: Variable Definitions and Sources

Variable Name	Definition	Source
Employment	Total number of workers with positive hours, in logs	author calculation; electronic payroll records
Hours	Total number of hours worked, regular plus overtime, in logs	author calculation; electronic payroll records
lnGAP	Percentage increase in the store's wage bill, holding hours constant, resulting from increasing wage of workers up to the new MW for workers whose wage in March-May, averaged for each store in March-May of each year. This captures increases intended to comply with the MW that occur prior to July 1. As defined in text, $GAP_{jt} = 1 + [(\Sigma h_{ijt-1} MW_{ijt} - \Sigma h_{ijt-1} W_{ijt-1}) / \Sigma h_{ijt-1} W_{jt-1}]$, summed over workers i for whom $W_{ijt1} <$ MWt and set to 0 for workers for whom $W_{ijt1} \geq MW_t$	author calculation; electronic payroll records
MWt	Binary variable equal to 1 in August, 2007-January, 2008; August, 2008-Janry, 2009; and August, 2009-December, 2009, respectively	author calculation; electronic payroll records
GA	Binary variable equal to 1 if establishment is in Georgia	
ΔlnSales	Change in log of monthly sales, lagged periods, calculated from bi-monthly information on percentage sales growth	Restaurant owners confidential files
Pop Density	County-level population density; persons per square mile	Census Bureau population estimates; available from http://www.census.gov/popest/counties/counties.html
ln Priv Emp	County-level number of employees in private establishments, minus employment in *Accommodations and Food Services and Retail* (NAICS 722) and *Retail* (NAICS 44-45) sectors	BLS, Quarterly Census of Employment and Wages (QCEW); available from http://www.bls.gov/cew/home.htm
County FE	County fixed effects; binary variable equal to 1 if a restaurant is located in a given county	author calculation based on GSP mapping of store locations
Owner FE	Owner fixed effects; binary variable equal to 1 if a restaurant belongs to one of the three franchise owners	author calculation; electronic payroll records

Table 5. Employment level changes during MW increase periods, with and without county, owner and store fixed effects

	Jan'07-Jan'08	Jan'08-Jan'09	Jan'09-Dec'09	Jan'07-Jan'08	Jan'08-Jan'09	Jan'09-Dec'09	Jan'07-Jan'08	Jan'08-Jan'09	Jan'09-Dec'09	Jan'07-Jan'08	Jan'08-Jan'09	Jan'09-Dec'09
MW	-0.018	-0.032*	0.006	-0.007	-0.033*	0.006	-0.006	-0.033*	0.006	-0.006	-0.033*	0.006
	(0.012)	(0.008)	(0.007)	(0.009)	(0.008)	(0.007)	(0.009)	(0.008)	(0.007)	(0.009)	(0.008)	(0.007)
Store FE	No	No	No	Yes	Yes	Yes	No	No	No	No	No	No
County FE	No	No	No	No	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Owner FE	No	No	No	No	No	No	No	No	No	Yes	Yes	Yes
Obs.	2,029	2,104	1,940	2,029	2,104	1,940	2,029	2,104	1,940	2,029	2,104	1,940
R-squared	0.00	0.00	0.00	0.88	0.91	0.94	0.74	0.79	0.82	0.74	0.79	0.82

***Notes*:** Robust standard errors are clustered on establishment. The dependent variable is store-level log of bi-weekly employment. MW is a dummy variable equal to 1 in the six (or five) months following the July MW increase; that is, August 2007-January 2008, August 2008-January 2009, and August 2009-December, 2009.

Table 6A. Pooled OLS, reduced-form model for employment and aggregate hours, 1st MW increase: January 2007 - January 2008

	(1) lnEmp	(2) lnEmp	(3) lnEmp	(4) lnHours
$\ln GAP_1 \cdot MW_1$	0.194	0.193	0.139	0.265
	(0.503)	(0.502)	(0.490)	(0.334)
lnGAP	1.888	1.889	1.918	0.225
	(1.993)	(1.995)	(1.992)	(1.489)
MW_1	-0.018	-0.018	-0.016	-0.012
	(0.019)	(0.019)	(0.018)	(0.012)
Δ lnSales	0.023	0.024	0.018	0.187*
	(0.034)	(0.034)	(0.034)	(0.052)
County FE	Yes	Yes	Yes	Yes
Owner FE	Yes	Yes	Yes	Yes
Observations	1,685	1,685	1,685	1,843
R-squared	0.75	0.75	0.75	0.64

Notes. Column 2 adds ln*PrivEmp* to (1). Column 3 adds *PopDensity* to (1). Robust standard errors clustered on establishments are in parentheses. MW_1 is a dummy variable equal to 1 in August 2007 - January 2008. Dependent variable in columns 1-4 is the log of average bi-weekly employment; the dependent variable in the last column is the log of total hours (regular + overtime) hours. In line 2, lnGAP is the initial $\ln GAP_1$ prior to the first MW increase. ΔlnSales is lagged two periods. * designates significance at the .05 level.

Table 6B. Pooled OLS, determinants of employment and aggregate hours,
2nd MW increase: January 2008 - January 2009

	(1) lnEmp	(2) lnEmp	(3) lnEmp	(4) lnHours
$lnGAP_2 \cdot MW_2$	-0.202	-0.155	-0.280	0.159
	(0.405)	(0.405)	(0.400)	(0.210)
lnGAP	0.123	0.110	0.143	-1.176
	(1.978)	(1.981)	(1.976)	(1.203)
MW_2	-0.017	-0.015	-0.011	-0.042*
	(0.022)	(0.021)	(0.021)	(0.010)
Δ lnSales	0.124*	0.118*	0.111*	0.125*
	(0.038)	(0.038)	(0.038)	(0.026)
County FE	Yes	Yes	Yes	Yes
Owner FE	Yes	Yes	Yes	Yes
Observations	2,054	2,054	2,054	2,054
R-squared	0.79	0.79	0.79	0.75

Notes. Column 2 adds ln*PrivEmp* to (1). Column 3 adds *PopDensity* to (1). Robust standard errors clustered on establishments are in parentheses. MW_2 is a dummy variable equal to 1 in August 2008 - January 2009. Dependent variable in columns 1-4 is the log of average bi-weekly employment; the dependent variable in the last column is the log of total hours (regular + overtime) hours. In line 2, lnGAP is the initial $lnGAP_1$ prior to the first MW increase. ΔlnSales is lagged two periods. * designates significance at the .05 level.

Table 6C. Pooled OLS, determinants of employment and aggregate hours,
3rd MW increase: January 2009 - December 2009

	(1) lnEmp	(2) lnEmp	(3) lnEmp	(4) lnHours
$lnGAP_3 \cdot MW_3$	0.454	0.456	0.454	0.312
	(0.397)	(0.396)	(0.397)	(0.277)
lnGAP	-0.610	-0.610	-0.610	-1.051
	(1.570)	(1.571)	(1.570)	(1.192)
MW_3	-0.024	-0.027	-0.024	-0.016
	(0.027)	(0.027)	(0.027)	(0.019)
Δ lnSales	0.076*	0.074*	0.076*	0.175*
	(0.036)	(0.035)	(0.036)	(0.025)
			(0.000)	
County FE	Yes	Yes	Yes	Yes
Owner FE	Yes	Yes	Yes	Yes
Observations	1,892	1,892	1,892	1,892
R-squared	0.82	0.82	0.82	0.76

Notes. Column 2 adds ln*PrivEmp* to (1). Column 3 adds *PopDensity* to (1). Robust standard errors clustered on establishments are in parentheses. MW_3 is a dummy variable equal to 1 in August 2009 - December 2009. Dependent variable in columns 1-4 is the log of average bi-weekly employment; the dependent variable in the last column is the log of total hours (regular + overtime) hours. In line 2, lnGAP is the initial $lnGAP_1$ prior to the first MW increase. ΔlnSales is lagged two periods. * designates significance at the .05 level.