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Estimating the employment effect of the minimum wage through variation in compliance: Evidence from five US states

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ABSTRACT

The implications of a binding minimum wage law on employment have been the subject of a lively and ongoing debate. Estimation of employment effects may be hindered by the non-random manner in which minimum wage laws are created. To overcome this, we explore the employment implications of the minimum wage in the US restaurant industry through an approach that exploits variation in compliance, as opposed to legislation. In the five US states without state minimum wages, violations of the US federal minimum wage are shown to be associated with decreased employment in the restaurant industry in the time period around the federal minimum wage increases of 2007 through 2009. The most robust specification shows an elasticity of employment with respect to unpaid wages of -0.233. Robustness checks use earlier time periods to show results do not reflect seasonal trends, vary the group of industries used as controls, and only use 2007 to show estimates are not confounded by a unique effect of the Great Recession on the restaurant industry.

1. Introduction

Understanding labor market implications of minimum wage laws has long been of interest to economists. Despite numerous methodologies being developed, results from past empirical papers have been mixed (see, for example, (Giotis and Mylonas, 2022)). We build on this literature by providing novel methodology to estimate employment effects by exploiting quasi-random variation in compliance of the minimum wage, rather than exploiting the creation or adjustment of minimum wage laws. As background, the United States federal minimum wage was first mandated in 1938 at $0.25 per hour. Over the years, it has steadily increased, but only in response to legislative action; it is not indexed to any measure of inflation. Employees subject to the Fair Labor Standards Act (FLSA) must receive the minimum wage.[1] This includes most employees but there are a few notable exceptions.[2] Our work exploits the nature of the restaurant industry, which is covered by the FLSA.

The minimum wage had risen to $5.15 per hour before July 24, 2007, when it increased to $5.85 per hour. The minimum wage was increased to $6.55 per hour on July 24, 2008 and to $7.25 per hour on July 24, 2009, and it has remained at this level since. Workers who rely on tips may be paid less than the minimum wage provided that they earn enough in tips to compensate. Many states mandate a higher minimum wage and some index it to inflation, but we restrict our analysis to the states that lack any state-mandated minimum wage.

This paper provides a novel method to estimate the effect of minimum wage laws by exploiting variation in compliance. Using data on U.S. federal minimum wage violations that are ex-post identified and corrected, the implications of minimum wage compliance on employment are identified. The key to the approach is the use of quasi-random variation in minimum wage compliance that results from restaurants in the United States relying on tipping to reach the minimum wage. The method also exploits the timing of U.S. federal minimum wage increases.

Past approaches to estimating employment effects of minimum wage laws have primarily relied on difference-in-differences designs and panel data methods. Variation in compliance provides an alternative method to estimate the effect of a minimum wage on the number of employees a firm hires. Our strategy centers on estimating changes in employment by firms that are caught violating minimum wage laws. If they hire fewer (or more) employees when violating

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[1] United States Code. Title 29, Chapter 8, Section 203.

[2] A few businesses are exempt from the FLSA based on size. Additionally, some agricultural workers, babysitters, newspaper delivery workers, apprentices, and movie theater employees are excluded from FLSA protections. Wages for railroad workers and truck drivers are governed by other laws. FLSA provisions other than the minimum wage do not apply to some workers, such as supervisory workers and salespeople who work on commission.

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the minimum wage, then we can conclude that minimum wage laws do affect hiring decisions. Mathematically, the goal is to estimate a regression equation with a measure of employment as the outcome and a measure of minimum wage violations as the regressor of interest. The coefficient for violations indicates whether firms that violate the minimum wage hire fewer (or more) employees. Noncompliance with the minimum wage is functionally equivalent to no minimum wage, providing an intuitive link between a minimum wage law, noncompliance, and enforcement. It is difficult to attain consistent estimates for the coefficients in an equation as described because firm owners choose to violate the minimum wage jointly with their choice of the number of employees to hire. To avoid this pitfall, we exploit the nature of the restaurant industry and the timing of federal minimum wage increases.

This balance of this paper proceeds with an overview of relevant research in Section 2 followed by a description of our data and empirical strategy in Section 3. We then present our results in Section 4. Estimates show a negative elasticity of minimum wage violations with employment. Thus, we posit that the presence of a minimum wage (functionally equivalent to compliance) is associated with greater employment. Robustness checks are also presented in Section 4. We conclude in Section 5.

2. Literature review

The theoretical relationship between the minimum wage and employment depends on whether employers have market power.[3] The empirical relationship has been estimated in a variety of settings and with numerous methodologies. These approaches have resulted in a wide variety of point estimates and conclusions. Card and Krueger (1993) sparked the literature by using a difference-in-differences design to show an increase in employment in New Jersey relative to Pennsylvania using time periods before and after a minimum wage increase in New Jersey. Meer and West (2016) use three separate state panels of administrative employment data to conclude that minimum wages cause a reduction in employment, however they show the effect is dynamic and that minimum wages cause firms to slowly transition to a new equilibrium with lower employment. A short list of many other papers includes Dube et al. (2010), who show no adverse effects of the minimum wage on restaurant employment in contiguous counties in states with minimum wage differences; Wang et al. (2019) who show heterogeneous (both positive and negative) effects of the minimum wage on employment across U.S. states using a structural panel model inspired by Dube et al.; and Powell (2022) who uses the synthetic control method on state-level data for fifty states and the District of Columbia to show minimum wages reduce employment among 16–19 year olds. Giotis and Mylonas (2022) survey the literature with a meta-analysis that shows no significant correlation between the minimum wage and employment, although they find hints of a negative relationship, indicating the need for further analysis, and showing the potential contribution of our paper.

Caselli et al. (2023) use a large sample of manufacturing firms in Italy and find that a minimum wage reduces monoposony power that may have some negative employment effects, which they balance with an estimate of the optimal minimum wage. Regarding monopsony power, Deb et al. (2022) find that monopoly power of firms (reducing output to charge high prices and hiring less labor) accounts for about 75% of wage stagnation, and monopsony power (being the sole buyer of a specific type of labor leading to lower wages) accounts for the other 25%.

This paper provides a novel insight into the employment effects of the minimum wage by evaluating the employment effects of compliance/enforcement. This effect likely differs compared to the general equilibrium effect of an economy shifting to an equilibrium with a new (higher) minimum wage. With a quantification of this effect of compliance/enforcement, officials can judge whether enforcement actions cost less than the benefits. In addition, we provide further insight into the debate on whether employers have market power in the labor market.

As we do in this paper, some researchers have used data on minimum wage violations. In May 2017, David Cooper and Teresa Kroeger of The Economic Policy Institute (EPI) published an article detailing trends in minimum wage violations by using CPS data with self-reported hourly wages to measure violations (Cooper and Kroeger, 2019). Interestingly the EPI also published “Why America Needs a $15 Minimum Wage”, mostly justifying the need by an appeal to wage inequality and the cost of living. Kim (2021) used data from the 2008 Survey of Income and Program Participation to document the prevalence of minimum wage violations and show that union membership and employment-based health insurance coverage significantly reduce the odds of an individual experiencing violations. This paper contributes with a novel estimation technique that exploits known violations of the minimum wage to estimate employment effects of the minimum wage.

3. Empirical strategy

This section describes our data and econometric approach to show employment effects of the minimum wage.

3.1. Data

Data are used from two publicly available sources. The first is Wage and Hour Compliance Action Data which are made publicly available by the Wage and Hour Division (WHD) of the US Department of Labor. The dataset contains all concluded compliance actions since the 2005 fiscal year. The dataset includes whether any minimum wage violations were found and the amount of backwages that were paid, as well as the civil monetary penalties assessed.

The second data source is the Longitudinal Employer-Household Dynamics (LEHD), specifically the quarterly workforce indicators (QWI). These provide data at the industry-county-quarter level on employment. The variable used for employment is `emptotal`, a count of people employed in a firm at any time during the quarter. This variable is gathered at the NAICS 3-digit industry level. Total county employment is also gathered. Employment variables used in this paper are for firms with private ownership (public employment should not be susceptible to minimum wage violations).

Our data on violations are a comprehensive dataset on cases of federal minimum wage violations that were caught. While this is an excellent resource, there are two limitations. Firstly, state governments also enforce state minimum wages. Secondly, there are certainly minimum wage violations that are not caught and remain undetected. The first concern is resolved by restricting analysis to the five states that do not have state minimum wage laws, such that all enforcement is at the federal level. These states are Alabama, Louisiana, Mississippi, South Carolina, and Tennessee. When violations happen, some time elapses before they are caught and penalties are imposed. For this reason, we consider that the employment effects of a minimum wage violation are not confounded by the penalty. The second concern, measurement error, is overcome by the use of an instrumental variable approach that is described in what follows.

Violations are aggregated to the industry-county-quarter level, with industries defined by 3-digit NAICS codes. The variable for backwages paid due to violations in a quarter is used as the measure of violations, and transformed into logarithms for regression analysis. The transformation $\ln(x+1)$ is used in regressions due to the presence of industry-county-quarter observations with 0 violations.

The “treated” industry is NAICS code 722, “food services and drinking places”. Many restaurant employees in the United States are paid a

[3] Research regarding the presence of employer market power is discussed in Card’s (2022) review article.

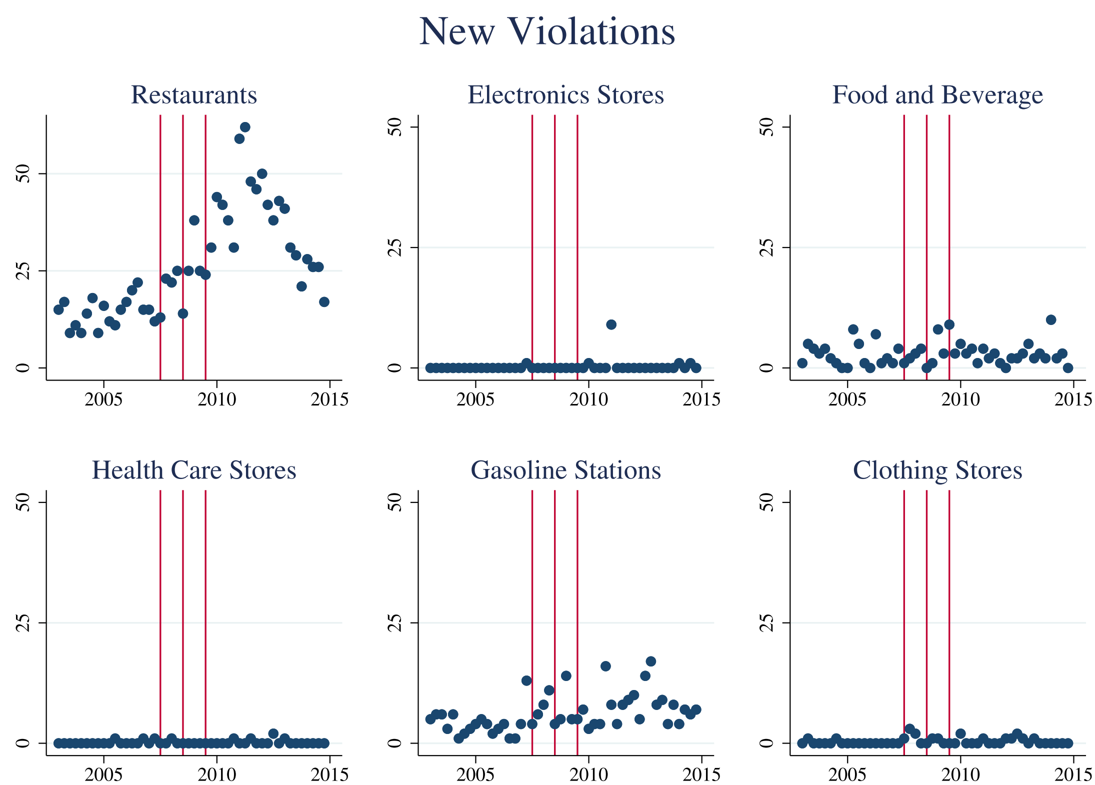

Fig. 1. New violations by industry over time.
Notes: The vertical axis plots the number of new minimum wage violations that started in the quarter that were later penalized by the Department of Labor. Industries are defined by 3-digit NAICS codes. Restaurants is 722, electronics stores is 443, food and beverage stores is 445, health and personal care stores is 446, gasoline stations is 447, and clothing stores is 448. Data are restricted to Alabama, Louisiana, Mississippi, South Carolina, and Tennessee.

baseline wage that is lower than the minimum wage, as permitted by the FLSA, because tips from customers are a large portion of employee compensation. If an employee does not receive sufficient tips to reach the minimum wage, then employers are required to provide sufficient compensation to reach the minimum wage. This provides a setting in which minimum wage violations are frequent and more likely to be exogenous.

We use five other 3-digit NAICS code industries as a "control" group, discussed and justified in what follows. Fig. 1 shows the number of new violations in a quarter for each industry as recorded in the WHD data. Two facts are clear: (1) restaurants are much more prone to minimum wage violations and (2) minimum wage violations in restaurants became more frequent when the Federal government increased the minimum wage. Although violations became more common during the Great Recession and its aftermath, we only observe an increase in restaurants, which are a large category of employers paying the minimum wage, or even paying less and relying on tips to compensate. There is no corresponding increase in violations in other low-paying sectors, like gas stations, which is why we believe the result is due to the minimum wage law, not the accompanying economic conditions.

Summary statistics are shown in Table 1. Data are restricted to the 3rd quarters of 2007, 2008, and 2009 to coincide with the time period used in analysis. Columns 1 and 2 of Table 1 respectively restrict to counties with none, or some, restaurants violating the minimum wage. Counties with violations had a much larger average decrease in restaurant employment compared to counties with no violations, −81.39 compared to −14.35. However, this should be evaluated in the context that counties with some restaurants violating the minimum wage had higher average employment levels, 8357 compared to 1338. In counties with non-zero violations among restaurants, the average county underpaid employees by $2116. Columns 3 and 4 show summary statistics for the other industries used in analysis. A major difference compared to restaurants are the sample sizes when splitting by counties with zero, or some, violations. For other industries, 192 county-industry observations have non-zero violations while 4028 had no violations. For restaurants, 220 counties had violations while only 817 had no violations. As Fig. 1 shows graphically, these sample sizes also show that restaurants are much more susceptible to minimum wage violations. For other industries, when there were no minimum wage violations employment increased by 3.20 employees

Table 1
Summary statistics.

	Restaurants		Other industries	
	No violations	Violations ≥ 1	No violations	Violations ≥ 1
Employment	1,338.2	8,357.0	391.0	1,213.2
	(2,645.5)	(10,147.0)	(789.4)	(1,873.0)
Δ Employment	−14.35	−81.39	3.20	−5.93
	(110.86)	(325.99)	(36.01)	(109.33)
Value of Violations	0	2,115.79	0	259.58
	.	(4,039.80)	.	(637.84)
Δ Value of Violations	−1.28	140.05	−1.37	13.94
	(25.91)	(2,113.93)	(32.69)	(240.76)
N	817	220	4,028	192

Notes: The unit of observation is the county-industry-time period. Data are restricted to the 3rd quarters of 2007, 2008, and 2009 and the states of Alabama, Louisiana, Mississippi, South Carolina, and Tennessee. "Other" industries are NAICS codes of 443, 445, 446, 447, and 448, being respectively electronics stores, food and beverage stores, health and personal care stores, gasoline stations, and clothing stores. Employment is emptotal in the LEHD QWI data. Value of violations is the dollar value of minimum wage violations.

on average between the second and third quarters. For observations with non-zero minimum wage violations, employment declined by 5.93 on average. As with restaurants, observations with non-zero violations are observations with greater average employment, 1213 compared to 391. Nevertheless, summary statistics provide suggestive evidence that violating the minimum wage is associated with firms reducing employment.

We also evaluated trends and seasonality in employment prior to the time period used in main analysis,2007–2009, to evaluate if heterogeneous trends or economic shocks may be a potential source of bias. Restricted to the counties used in estimation, appendix figure A1 plots total employment at the industry-quarter level for 2004 through 2006. Appendix figure A2 plots the average across counties of the natural log of industry-county employment over the same period. The quarters of the year used in main estimation are the second and third quarters. Visually, the restaurant industry's trends and seasonality regarding the second and third quarters do not appear to be anomalous compared to the control industries.

3.2. Baseline empirical model

The method exploits the timing of the federal minimum wage increases which induced heterogeneity in changes of minimum wage compliance across industries, with restaurants having less compliance. Importantly, we argue that this heterogeneity is due to inattention and the reliance of the restaurant industry on tipping. It follows that this heterogeneity is uncorrelated with unobserved determinants of employment.

Our approach is related to that of Anderson and Hsiao (1981) Arellano and Bond (1991). We begin by mathematically describing the setting in its most basic form and then derive the equation of estimation. In the discussion, we explain the drawbacks of other approaches in support of ours. In so doing, we also explain robustness checks to address potential concerns with our approach.

The equation of estimation is derived while considering the data-generating process for the observed data on violations. Let $VioWHD_{ikt}$ denote the WHD measure of violations, with Vio_{ikt} representing the true unobserved quantity of violations. Subscript i denotes the county, k the industry, and t the quarter.

$$\ln(VioWHD_{ikt}) = \delta_0 + \delta_1 \ln(Vio_{ikt}) + \tilde{a_{ik}} + \tilde{c}_t + v_{ikt} \tag{1}$$

The $\tilde{a_{ik}}$ and $\tilde{c}_t$ terms are county-industry and time fixed effects.[4] There is likely no measurement error in the enforcement data, however enforcement is imperfect. The error term, v_{ikt}, represents idiosyncratic predictors of enforcement that are unrelated to actual violations or an innate propensity for enforcement due to the time period or the county-industry.

Employment in an industry is assumed to be autocorrelated and depend on the number of firms that are contemporaneously violating the minimum wage. In the equation below, Emp_{ikt} is employment in industry k of county i in quarter t.

$$\ln(Emp_{ikt}) = \beta_0 + \beta_1 \ln(Vio_{ikt}) + \beta_2 \ln(Emp_{ikt-1}) + a_{ik} + c_t + \varepsilon_{ikt}$$

OLS is known to be biased in the presence of fixed effects with a lagged dependent variable (Nickell, 1981). We proceed by differencing to remove the county-industry fixed effect, a_{ik}.

$$\Delta \ln(Emp_{ikt}) = \beta_1 \Delta \ln(Vio_{ikt}) + \beta_2 \Delta \ln(Emp_{ikt-1}) + c_t + \Delta\varepsilon_{ikt} \tag{2}$$

The variable $\Delta \ln(VioWHD_{ikt})$ is the measured value of ΔVio_{ikt}. Eq. (1) is differenced, rearranged, and plugged into Eq. (2). This results in,

$$\Delta \ln(Emp_{ikt}) = \beta_1 \left(\frac{1}{\delta_1} \Delta \ln(VioWHD_{ikt}) - \frac{1}{\delta_1}\tilde{c}_t - \frac{1}{\delta_1}\Delta v_{ikt} \right) + \beta_2 \Delta \ln(Emp_{ikt-1}) + c_t + \Delta\varepsilon_{ikt}$$

$$\Delta \ln(Emp_{ikt}) = \beta_1^* \Delta \ln(VioWHD_{ikt}) + \beta_2 \Delta \ln(Emp_{ikt-1}) + c_t' + \Delta\varepsilon_{ikt} - \beta_1^* \Delta v_{ikt} \tag{3}$$

Eq. (3) is the equation of estimation. It uses $c_t' := c_t - \beta_1^* \tilde{c}_t$ and, more importantly, $\beta_1^* := \frac{\beta_1}{\delta_1}$. The term β_1^* represents the change in log employment that is associated with one more violation that is caught. The parameter β_1 may not be identified, however β_1^* is still of interest. It is logical that $\delta_1 \in (0,1)$. The division by δ_1 represents "inflating" the parameter by the change in the number of total violations that is associated with one more detected violation. Essentially, β_1^* represents the change in employment that is associated with a violation that is caught jointly with the change in employment that is a result of all uncaught violations that are correlated with the existence of a violation that is detected.

The measured value of violations is possibly correlated with both errors, first, ε, denoting unobserved determinants of employment, and second, v, denoting that the data on violations are not comprehensive due to imperfect enforcement. To overcome this, a binary variable for the industry having a NAICS code of 722, $1[k = 722]$, is used as an instrument for $\Delta \ln(VioWHD_{ikt})$. The instrument is valid if being in the restaurant rather than any control industry is uncorrelated with the change in idiosyncratic determinants of employment and any change in idiosyncratic enforcement. Validity and robustness checks are discussed in what follows. The variable $\Delta \ln(Emp_{ikt-1})$ is also instrumented (also discussed in what follows).

Before expounding on the IV strategy, we explain two restrictions that we impose on the data. First, only the quarters just after the federal minimum wage increases are used (the 3rd quarters of 2007, 2008, and 2009). If variation in violations were to occur only due to random variation in propensity of customers to tip, then it is more plausible that estimates are consistent. We restrict to the 3rd quarters of 2007, 2008, and 2009 because these are the time periods just after the federal minimum wage increases. When going from the second to the third quarter in these years, there is likely a large increase in minimum wage violations in the restaurant industry because tipping behavior presumably does not change, however, the threshold to be reached by tipping increases. In the estimation that follows, the restriction to these specific time periods creates a stronger first stage relationship between being in the restaurant industry and minimum wage violations.

As a second restriction, we separate industries into "treated" and "control" groups. The treated industry is defined by the 3-digit NAICS code of 722, restaurants. The control industries are defined by NAICS codes of 443, 445, 446, 447, and 448, being respectively electronics stores, food and beverage stores, health and personal care stores, gasoline stations, and clothing stores. The control group is chosen to comprise industries with minimum wage labor for which tipping is rare. In contrast, the restaurant industry is susceptible to minimum wage violations due to the reliance on tipping to reach the minimum wage. The claim is that between the second and third quarters of 2007, 2008, and 2009, new violations in the restaurant industry arose due to inattention, which is assumed to be orthogonal to the regression error term. Robustness checks vary the set of control industries.

NAICS codes beginning with 44 are "Retail Trade". The initial idea was to use all 3-digit NAICS codes that begin with 44 as a control group. Three of these were excluded. NAICS code 442, "Furniture and Home Furnishings Stores", was excluded due to the time period of analysis coinciding with a housing market crash. NAICS code 444, "Building

[4] In Eq. (1), δ_1 is the change in detected violations that is associated with one more true violation. δ_1 is the same constant for all counties. To ensure that this assumption is plausible, the data are restricted to counties that have some form of enforcement in the WHD data. The only county omitted is Cameron Parish, Louisiana. Results are robust to the inclusion of that county.

Material and Garden Equipment and Supplies Dealers", was excluded for two reasons. First, the industry is connected to the housing industry. Second, there are 0 minimum wage violations in firms with NAICS codes of 444 recorded in the WHD data for the counties and time periods of analysis, suggesting that this industry may not frequently hire minimum wage labor and be an appropriate control. NAICS code 441, "Motor Vehicle and Parts Dealers", was excluded due to the time period of analysis coinciding with an automotive industry crisis, although results are significant even while including this industry in the control group.

Many other approaches to form a control group may be considered but have more drawbacks compared to our approach of using industries that do not rely on tips. Using unaffected states (with a state minimum wage above the federal level) as controls is problematic because other states have different minimum wages set by state law, but even more problematic is that they also have different levels of enforcement because both state and federal authorities are involved. Our data only capture federal enforcement. Such a control would be unsuitable because it relies on different standards, and different enforcement levels that are not perfectly measured. Comparing high-wage and low-wage counties within treated states is unsuitable because the regional fixed effect used in the regression equation would be collinear with the treatment of being in a low-wage county. Exploiting variation in the number of inspectors could be considered, however, this has a drawback because inspectors are likely not randomly assigned to regions, and their assignments may be a response to minimum wage violations. Thus, it would be difficult to argue the number of inspectors is a valid instrument.

3.3. Instrument validity and robustness

Minimum wage violations are caught and punished ex post. It is plausible that the US government does not differ across industries in changes to enforcement of the minimum wage between the second and third quarters of 2007, 2008, and 2009. Thus, $1[k = 722]$ is plausibly uncorrelated with Δv_{ikt}. Fig. 1 shows that minimum wage violations trended weakly upward in the restaurant industry even before the time of the minimum wage increases, possibly causing concern that the government's enforcement of labor regulations was changing over time specifically in the restaurant industry. As a robustness check, we created a version of Fig. 1 using overtime wage violations. There was no trend prior to 2007. This is consistent with a setting in which government enforcement was not trending over time differently across industries, but that minimum wage violations naturally trended over time in the restaurant industry. Such a trend in violations does not invalidate our approach.

The assumption that being in the restaurant industry is uncorrelated with changes in unobserved determinants of employment, $\Delta\varepsilon_{ikt}$, is more tenuous. If the restaurant industry has seasonal trends in employment that differ from seasonality in the control industries, then $1[k = 722]$ may be correlated with $\Delta\varepsilon_{ikt}$. There is a potential bias if going from the second to third quarter in NAICS 722 is associated with a seasonal change in employment, and no other industry experiences this seasonality. If this is the case, then it would be expected to observe a strong reduced form effect in years for which there is no change in the federal minimum wage, however seasonality persists. As a robustness check, OLS, IV, first stage, and reduced form estimates are reported using the years 2005 and 2006, and results suggest this is not a concern.[5]

A related concern is that the Great Recession began in late 2007. The Great Recession may have more strongly reduced employment in the restaurant industry compared to the control industries, and this effect would invalidate the instrumental variable approach. As a robustness check the specification of interest is estimated while restricting to 2007 (the second and third quarters of which were prior to the onset of the recession). The point estimate for the coefficient of interest is comparable, however the standard error is naturally larger due to the reduced sample size.

There is a secondary concern, that $\Delta \ln(Emp_{ikt-1})$ is correlated with $\Delta\varepsilon_{ikt}$ due to the correlation of $\ln(Emp_{ikt-1})$ with ε_{ikt-1}. This concern is overcome by using the idea of Anderson and Hsiao (1981) to use $\ln(Emp_{ikt-2})$, or even earlier lags, to instrument $\Delta \ln(Emp_{ikt-1})$. The inclusion of a lagged dependent variable is motivated by the fact that fixed effects only capture time-invariant effects of employment, but contracts or other sources of "sticky" employment will not be captured with fixed effects alone. Adding complication to a model using this econometric approach makes results more sensitive to specification. Although we consider the method valid, we also estimate a model without a lagged dependent variable for parsimony. We obtain a coefficient that is even larger in absolute value when doing so.

As described in the discussion of summary statistics, observations with minimum wage violations had higher baseline employment. The final specification adds other controls, notably $\ln(TotalEmp_{it-2})$, the total employment across all industries in the county 2 time periods previous. This controls for the ex ante status of the overall labor market in the county. A county fixed effect is also added to the estimation of Eq. (3) to provide additional robustness.

There is also a natural concern that the choice of control industries may drive results. As robustness checks, the final specification of IV estimation is replicated while iteratively leaving out one control industry, and also while only using each control industry separately. There may be concerns of endogeneity in the first stage equation. However, in IV regressions, it is not important that estimates from the first stage represent a causal effect. It is only critical that the instrument is uncorrelated with the error term in the second-stage equation. The instrument is designed to satisfy this condition by relying on industries that employ workers of similar skill levels and similar wages, but without reliance on tipping.

Due to the average number of employees being meaningfully different between observations with and without violations, the final specification of IV analysis controls for log employment two time periods before the minimum wage change as well as county fixed effects. Results become stronger when including the control for baseline employment.

4. Results of the regression analysis

In this section we present the results of the regression analysis based on the estimation of the empirical model illustrated in Section 3. The estimates of the IV regressions are presented in Table 2. The IV results in columns 4 through 8 of Table 2 show that industries with greater values of minimum wage violations have lower employment. Because the dependent and independent variables are logged, the coefficient is approximately interpreted as an elasticity. Thus, it reflects the approximate percentage change in employment that results from a given percentage change in enforcement actions. In percentage terms, fewer enforcement actions are linked to higher employment. The point estimate varies by specification, however, the elasticity is close to -0.2 in all IV estimations and is significant by conventional criteria. Standard errors are clustered at the county level in all specifications.

This result implies that adhering to the minimum wage causes firms to increase employment, and is consistent with a setting of monopsony power of employers. Table 1 shows that county-industry-quarter observations that had violations also had greater average employment than observations without violations. Column (7) of Table 2 includes a control for the second lag of log employment in the industry and also a control for the second lag of overall employment

[5] Enforcement data begin in 2005, preventing use of even earlier time periods when replicating results. Also, after 2009 there may be dynamic effects of the minimum wage increase and changes in violations, thus these time periods are not used for a robustness check.

Table 2
Main results.

	Change in log employment for the industry										
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	OLS	OLS	OLS	IV	IV	IV	IV	IV	First Stages		Reduced Form
Δ Log Violations	−0.001	−0.001	−0.001	−0.236	−0.166	−0.151	−0.233	−0.233			
	(0.001)	(0.001)	(0.001)	(0.089)	(0.071)	(0.071)	(0.100)	(0.100)			
1[NAICS = 722]									0.096	0.029	−0.015
									(0.027)	(0.005)	(0.005)
Lag Δ Industry Log Employment		−0.121	−0.126		−0.120	−0.047	0.227	0.198			
		(0.025)	(0.026)		(0.028)	(0.091)	(0.159)	(0.190)			
2nd Lag of Log Industry Employment			−0.002				−0.004	−0.004	0.019	−0.050	−0.078
			(0.002)				(0.003)	(0.003)	(0.040)	(0.019)	(0.030)
3rd Lag of Log Industry Employment									0.030	−0.076	0.074
									(0.061)	(0.025)	(0.029)
4th Lag of Log Industry Employment									−0.059	0.129	0.005
									(0.054)	(0.021)	(0.019)
2nd Lag of Log Total Employment			0.002				0.012	−0.038	−0.168	−0.083	−0.014
			(0.001)				(0.004)	(0.066)	(0.189)	(0.038)	(0.046)
N	5,257										

Standard errors clustered at the county level in parentheses.
Notes:
1. Estimated equations are variations of equation (3), which is our basic equation.
2. Data are restricted to the 3rd quarters of 2007, 2008, and 2009 for Alabama, Louisiana, Mississippi, South Carolina, and Tennessee. The unit of observation is an industry-county-quarter.
3. Time FE are included in all specifications.
4. Columns (4) and (5) use a binary variable for being in NAICS 722 as an instrument for the change in violations between quarters 2 and 3.
Column (6) uses the same NAICS 722 binary IV, and also instruments the lag change in log employment for the industry while adding as instruments the second, third, and fourth lags of log industry employment in levels.
6. Column (7) only uses the third and fourth lags as instruments, and includes the second lag as a control directly, in addition to adding the control for the second lag of log total county employment.
7. Column (8) adds a county fixed effect.
8. Column (9) is the first stage for Δ Log Violations using the specification in column (8).
9. Column (10) is the first stage for Lag Δ Industry Log Employment using the specification in column (8).
10. Column (11) is the reduced form for the IV estimation associated with column (8).

in the county. Compared to the specification of column (6) without these controls, the coefficient is even more negative, being −0.233 in column (7) and −0.151 in column (6). Column (8), the most robust specification, includes county fixed effects. The coefficient of interest is unchanged. This is the preferred specification, and it gives an estimated approximate elasticity of employment with respect to unpaid wages of −0.233.

To put our estimate in the context of other literature, Card and Krueger (1993) found that an 18.82% increase in the minimum wage (from \$4.25 to \$5.05) was associated with a 13% increase in employment, resulting in an elasticity of 0.69. Powell's (2022) estimate using a synthetic control approach was −0.178. Meer and West create a variety of estimates of the elasticity, with most numbers around −0.04. Wang et al. (2019), show heterogeneity across groups of states. Their group 1 includes four of the states we use in our analysis, Alabama, Louisiana, Mississippi, and South Carolina. In that group, they find an elasticity of around 0.5. Our preferred estimate of −0.233 has a magnitude that is not out of the ordinary in the context of these other results. Note that the sign of our estimate is "flipped". That is, a negative elasticity of violations with employment (which we show) is in the same spirit as a positive elasticity of the minimum wage with employment. Thus, our point estimate has the same "sign" as that of Wang et al. (2019) when using similar states, however, is smaller in magnitude.

An explanation for why our estimates differ is that we address a different mechanism whereby the minimum wage affects employment. We provide insight beyond the estimates of prior work in that we quantify the effect of compliance/enforcement on employment, which likely differs from the general equilibrium effect of raising the minimum wage on employment. Relying on tips, which are not controlled by the employer but may require the employer to take compliance action, offers a source of plausibly exogenous variation in the employer's need to pay the minimum wage. By focusing on something that requires additional compliance actions based on presumably as good as random tipping behavior, an analysis of the effects of enforcement is presented in this paper. This stands in contrast to other papers about the minimum wage, which rely on the response, or lack thereof, to changes in the law.

As we use an instrumental variable approach, if there are heterogeneous effects our estimate is not of a population average treatment effect, but rather an average treatment effect for the subpopulation in which violations increase as a result of the instrument. We thus estimate the elasticity of minimum wage violations on employment for the type of industry, that, if the industry hypothetically became the restaurant industry, would have an increase in minimum wage violations. This is not natural to think about. To attempt to attain a more natural interpretation, we posit that the restaurant industry is prone to minimum wage violations due to inattention and randomness in tipping. Thus, a better way to think about our estimate is that it represents the average treatment effect for the type of industry that, if there were reliance on tipping to reach the minimum wage, and inattention regarding tipping, there would be more minimum wage violations. We estimate the elasticity of employment with respect to minimum wage violations that are a result of inattention, rather than deliberate.

Results from estimating the reduced form regression are shown in the final column of Table 2. The restaurant industry had a greater decrease in employment between quarters 2 and 3 of the years 2007, 2008, and 2009 compared to the control industries. The coefficient is −0.015 and is highly significant. There is a concern that this is a result of seasonality in the restaurant industry. Results from replicating estimation while using years 2005 and 2006 are shown in Table 3. The first stage is much weaker. Importantly, the final column shows a reduced form effect that is insignificant and closer to 0 compared to the reduced form effect estimated when using the years 2007, 2008, and 2009. The coefficient is −0.009 and the standard error is 0.007.

Seasonality could invalidate the method if restaurant employment changes around the summer months. The purpose of the previous exercise is to demonstrate that this is not the case, and our instrument is indeed valid, critically by estimating the reduced form regression

Table 3
Time robustness check.

	Change in log employment for the industry										
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	OLS	OLS	OLS	IV	IV	IV	IV	IV	First Stages		Reduced Form
Δ Log Violations	−0.003	−0.003	−0.003	−0.736	−0.160	−0.529	−0.237	−0.087			
	(0.002)	(0.002)	(0.002)	(1.979)	(0.453)	(0.613)	(0.466)	(0.319)			
1[NAICS = 722]									0.001	0.028	−0.009
									(0.031)	(0.009)	(0.007)
Lag Δ Industry Log Employment		−0.136	−0.160		−0.169	0.036	0.254	0.387			
		(0.036)	(0.038)		(0.044)	(0.289)	(0.337)	(0.276)			
2nd Lag of Log Industry Employment			−0.000				−0.004	−0.007	0.013	−0.071	−0.088
			(0.002)				(0.007)	(0.006)	(0.053)	(0.031)	(0.028)
3rd Lag of Log Industry Employment									0.107	−0.026	0.033
									(0.115)	(0.041)	(0.033)
4th Lag of Log Industry Employment									−0.111	0.103	0.055
									(0.108)	(0.036)	(0.026)
2nd Lag of Log Total Employment			0.002				0.006	−0.233	0.310	0.012	−0.264
			(0.001)				(0.009)	(0.138)	(0.180)	(0.099)	(0.095)
N	3,524										

Standard errors clustered at the county level in parentheses.
Notes: Identical to Table 2 however using years 2005 and 2006.

Table 4
Industry robustness check.

	Leaving out the industry	Only using the industry
443 - Electronics Stores	−0.081	−0.173
	(0.074)	(0.174)
445 - Food and Beverage Stores	−0.276	0.049
	(0.145)	(0.135)
446 - Health and Personal Care Stores	−0.214	−0.413
	(0.095)	(0.574)
447 - Gasoline Stations	−0.316	−0.104
	(0.137)	(0.206)
448 - Clothing Stores	−0.329	−0.075
	(0.153)	(0.155)

Notes: Coefficients are for the change in log violations using the specification in column (8) of Table 2 with the restriction on the control industries as shown in the column headings.

using data from 2005 and 2006 and comparing that point estimate to 0. If results were driven by bias due to seasonality, the reduced form would show an effect when estimated using data from 2005–2006. We compare the reduced form coefficient to zero when discussing Table 3, and acknowledge that it is still negative (but importantly, insignificant). The 2SLS estimates from 2005–2006 are in fact often negative, but imprecise. We would not expect the IV estimates to be zero because, if the instrument is valid, the model estimates the causal effect of minimum wage violations on employment, which we believe is a negative relationship. Due to the weaker first stage, we intuitively expect noise in the 2SLS estimates. We describe the mathematical underpinnings of these ideas in the appendix.

Nevertheless, the concern may linger because the reduced form coefficient from Table 3 is negative (but insignificant and closer to 0 than the analogous coefficient in Table 2). As additional robustness, the reduced form was estimated without controls separately for 2007–2009 and 2005–2006. The coefficient for 2007–2009 was −0.016 with a standard error of 0.003. For 2005–2006 the values were respectively 0.005 and 0.004. The lack of a reduced form effect during 2005 and 2006 suggests that seasonality is not confounding results.

To confirm that the Great Recession does not drive results, the specification in column (8) of Table 2 (the most robust and preferred specification) was re-estimated while only using data from 2007. The point estimate was −0.277 and the standard error was 0.180. The p-value is 0.123. Due to the reduced sample size, a statistically insignificant effect is not surprising. The fact that the point estimate is even larger in absolute value compared to primary results suggests that, if anything, the Great Recession attenuates estimates.

Table 4 shows results while iteratively excluding, or only using, each industry from the pool of control industries, and estimating the specification shown in column (8) of Table 2. There is a spread of point estimates from 0.049 to -0.413. Most results are close to -0.233, the posited effect based on estimates from Table 2. We note that, when only using NAICS 445 as a control, the point estimate is positive, 0.049, but insignificant. It may be that food and beverage stores and not a valid comparison group. When estimating using all industries except NAICS 445 as controls, the point estimate is -.276 (larger in absolute value compared to our preferred estimate, −0.233) and the p-value is 0.056. Thus, if the use of NAICS 445 as a comparison group induces bias, this appears to, at worst, result in conservative estimates.

At the suggestion of a referee, we replicated estimation while using long differences of 2 time periods for the regressor of interest and outcome, we also did this for long differences of 3 time periods. Point estimates for the IV estimate of the effect of violations on employment were negative and of comparable magnitude to those shown in Table 2, however imprecise.

5. Conclusion

This paper provides new estimates for the employment effect of a minimum wage by exploiting quasi-random variation in compliance of the minimum wage in the restaurant industry. To eliminate confounding effects, we adopt an IV approach that focuses on the restaurant industry and restrict to states that lack a state minimum wage, providing consistent point estimates. This industry-specific focus narrows the analysis, but provides a setting in which minimum wage violations are likely due to the prevalence of tipping, which presumably does not change when the federal minimum wage increases. If employers have market power in the labor market, they can exploit workers by paying lower wages and employing fewer persons. In such a setting, a minimum wage law can increase employment as well as wages. The link we exploit is that violations are equivalent to firms behaving as though there is no minimum wage. This is the connection between the minimum wage law, noncompliance, and enforcement. By excluding states with state-level enforcement, we avoid measurement error concerns.

Although results are specific to five states and the time periods when the federal minimum wage changed, they strongly suggest that employers have market power over low-skilled labor, and that adherence to the minimum wage is linked to increased employment. Despite the narrow focus on the restaurant industry, low-skilled labor may be susceptible to this type of exploitation in general, and raising the minimum wage need not cause employment to decline. The general policy-relevant implication is that minimum wage laws, and enforcement of such laws, likely have a positive impact on the economic well-being of low-skilled workers. Possible applications of this implication take several levels. At the most narrow level of the restaurant industry, mandatory gratuity, or requiring that employers pay workers a baseline wage that is no less than the minimum wage, could be considered. More generally, improving enforcement of the minimum wage may benefit low-skilled workers in all industries. Even more generally, if one is willing to make an assumption regarding external validity to other regions, this provides support for the creation of a minimum wage in regions for which there is no such law, however, a law is being considered.

There are several directions in which this research can be expanded, but which rely on other data sources or identification strategies. For example, by gathering enforcement data at the state or municipality level the analysis could be extended to include additional regions and researchers could evaluate if there is geographical heterogeneity in results. Another, and perhaps more challenging extension, is to expand the analysis to additional industries. Our current identification strategy relies on tipping, which is more common in restaurants than in other sectors, and allows for an identification strategy. Developing an identification strategy that applies generally to other industries would likely be of interest.

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CRediT authorship contribution statement

Michael Gmeiner: Econometrics work and some of the writing. **Robert Gmeiner:** Writing and economic analysis.

Declaration of competing interest

The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

Data availability

Data are publicly available at sources referenced in the paper.

Appendix

In Table 3 we show results from a robustness check in which we replicate estimates using data from 2005 and 2006, a time period for which the first stage is weaker. The point estimates for the 2SLS estimates are negative. One may wonder if the estimates should be centered on 0 because there is a weak first stage, and wonder if the negative point estimates indicate seasonality or some source of bias. If the instrument is valid, then the 2SLS estimator is a consistent estimator of the parameter of interest, which we posit is negative. This is true even if the first stage is weak. The negative point estimates are not a cause for concern. We describe this mathematically below in the context of a generic 2SLS estimation with an endogenous regressor, x, an outcome, y, and an instrument, z (see Figs. A.1 and A.2).

The second stage is,

$$y_i = \beta_0 + \beta_1 x_i + u_i$$

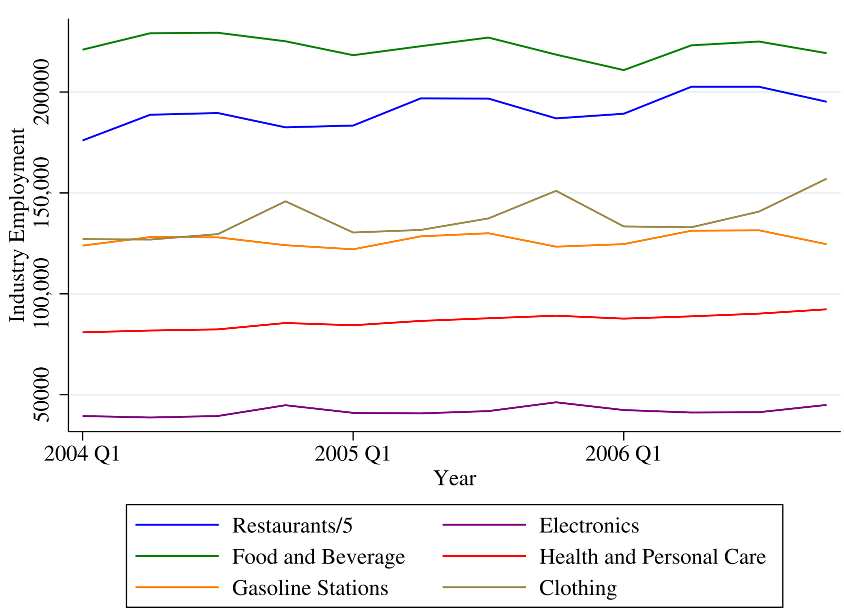

Fig. A.1. Total industry employment by time.
Notes: Restricted to the counties used in primary estimation. The employment measure in levels is summed at the industry-quarter level.

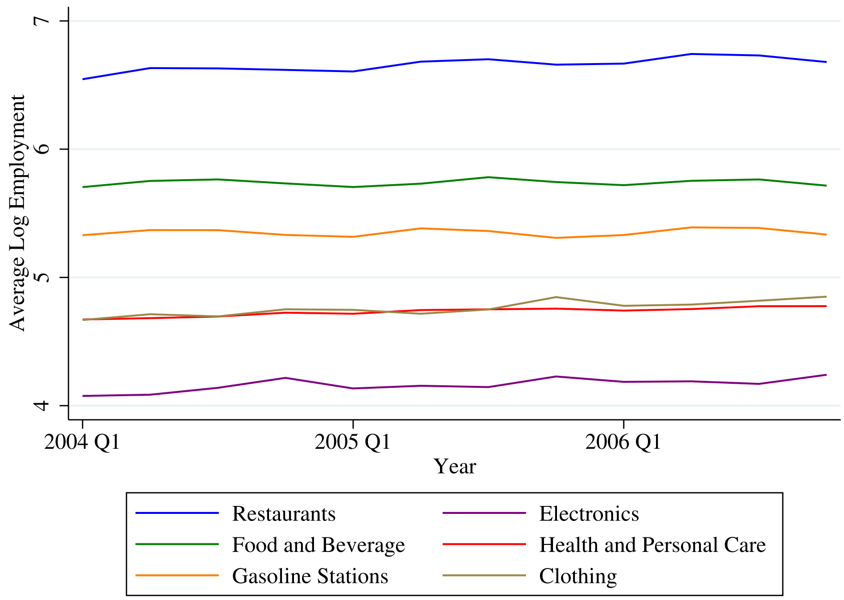

Fig. A.2. Average log employment over time.
Notes: Restricted to the counties used in primary estimation. The natural log of the employment measure is averaged at the industry-quarter level.

The first stage is,

$$x_i = \delta_0 + \delta_1 z_i + \varepsilon_i$$

The reduced form equation is

$$y_i = \alpha_0 + \alpha_1 z_i + \eta_i$$

where $\alpha_1 = \beta_1\delta_1$ and $\eta_i = u_i + \beta_1\varepsilon_i$.

If there is a weak first stage (e.g., $\delta_1 \approx 0$ in the case of no minimum wage change in 2005 and 2006), but the instrument is valid, then the reduced form asymptotically estimates,

$$\alpha_1 = \beta_1\delta_1 \approx \beta_1(0) = 0$$

This is what we show in Table 3 and in the robustness check in-text that estimates the reduced form without controls.

Even in this case with a weak first stage, 2SLS asymptotically estimates

$$\frac{\alpha}{\delta_1} = \frac{\beta_1\delta_1}{\delta_1} = \beta_1 < 0$$

Where the inequality is true if the treatment effect is negative, which we posit is the case in our context. We stress that even in the presence of a weak first stage this is true. 2SLS estimation is known to be noisy in finite samples. This is what we show in Table 3. Point estimates from 2SLS estimation when using 2005 and 2006 (with a weak first stage) are still negative. If the 2SLS estimate using 2005 and 2006 data were centered on 0, that implies the 2SLS estimate has bias on the order of $-\beta$, and would be an indication of seasonality or some other source of bias. If there is seasonality that causes the instrument to be invalid, then being in the restaurant industry is correlated with the outcome for a reason other than minimum wage violations (or our controls). In this case, $Cov(z_i, u_i) \neq 0$, and thus $Cov(z_i, \eta_i) \neq 0$. In such a case, given that $\alpha_1 \approx 0$ (due to the weak first stage), the reduced form asymptotically estimates,

$$\alpha_1 + \frac{Cov(z_i, \eta_i)}{Var(z_i)} \approx 0 + \frac{Cov(z_i, \eta_i)}{Var(z_i)} \neq 0$$

The estimated reduced form coefficient is not significantly different from 0 when using 2005 and 2006 data, justifying instrument validity.

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