

Wages and Hours Laws: What Do We Know? What Can Be Done?



CHARLES C. BROWN AND DANIEL S. HAMERMESH

We summarize recent research on the wage and employment effects of minimum wage laws in the United States and infer from non-U.S. studies of hours laws the likely effects of unchanging U.S. hours laws. The effective minimum wage, increasingly a province of state government, is now closely related to the lower end of a state's wage distribution. Original estimates demonstrate how the forty-five-year failure to increase the exempt earnings level for salaried workers has raised hours of lower-earning salaried workers and reduced their weekly earnings. The overall conclusion from the literature and the original work is that wages and hours laws in the United States have produced impacts in the directions predicted by economic theory, but that these effects have been quite small.

Keywords: minimum wages, overtime, employment, hours, labor regulation

In most markets, we concern ourselves with two dimensions—price and quantity. In labor markets too we concentrate on price (broadly, compensation per hour, of which the hourly wage is the largest component); but in considering quantity we examine both its incidence—the number of employees—and its intensity—hours per employee. In the United States, the Fair Labor Standards Act (FLSA) of 1938 has regulated the wage rate by setting a minimum on what can be paid in covered employment and has mandated premium or penalty pay on

weekly hours per worker above some level for nonexempt workers in covered industries and firms.

Here we review recent policy developments and try to synthesize what we know about the economic effects of these two major methods by which we regulate labor markets. Although wages and hours are regulated under the same law, policy developments and research on the law's impacts could not be more different between the two areas. The federal minimum wage has been raised numerous times; and many

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subfederal jurisdictions impose their own wage minima that, where they exceed the federal minimum, supersede it. Perhaps because of this variation, a huge literature examining the effects of minimum wages on the U.S. labor market has arisen and has continued to burgeon. A fair conclusion is that American labor economists have spilled more ink per federal budgetary dollar on this topic than on any other labor-related policy. The opposite is the case for regulating hours. The essential parameters of hours regulation have not changed since passage of the act; and perhaps because of this, the dearth of research on the economic impact of hours regulation in the United States, especially recently, is remarkable.

Because of these contrasts, for the minimum wage we summarize and evaluate recent legislative changes and synthesize the large number of recent studies that have examined the effects of minimum wage laws on wages and employment in the United States. In the case of overtime pay, we evaluate the impact of a provision of the regulations that has not changed in forty-five years, and we synthesize the likely impact of changing other provisions of the law on employment, hours, and wages by examining international evidence.

MINIMUM WAGE LAWS IN THE UNITED STATES: WHAT WE KNOW

The FLSA initially set the federal minimum wage at \$0.25 (roughly \$3.50 today, using the personal consumption expenditure deflator), to increase to \$0.30 the following year; coverage was limited to workers engaged in or producing goods for interstate commerce. Since then, the nominal minimum has been increased nine times (often in multiyear installments). Given periodic nominal adjustments, the impact of the law has followed a saw-toothed pattern, increasing discretely when a higher minimum wage was mandated and then eroding gradually until the next hike. These adjustments have become less frequent over time—roughly twice a decade in the 1960s and 1970s, once a decade since. Coverage of the law was also expanded, most notably to include workers in construction and large retail trade and service employment in 1961 and 1966 (U.S. Department of Labor 2018).

State legislation can matter in two ways: by extending coverage to small employers who are exempted from the federal law and by requiring a higher minimum than the federal law for existing covered employers. State-level coverage became less important as federal coverage expanded through the 1970s. Over the past thirty years, however, states' decisions to increase their minimum wages have become increasingly important given that the federal minimum has changed less frequently. For example, in 2010 (after the 2007 federal increases had become fully effective) only one-third of the workforce was in states with state minima that exceeded the federal \$7.25. By 2016, with the federal minimum still at \$7.25, that fraction had risen to nearly two-thirds. As of 2018, twenty-nine states, shown shaded dark in figure 1, had minimum wages above \$7.25.

States that have raised their minimum wages above the federal minimum have tended to be high-wage states, and the result has been a minimum wage much more closely (though still imperfectly) aligned with local wages. A simple way of summarizing this relationship is to regress the logarithm of the minimum wage in each state (the higher of the federal or state minimum) on the logarithm of the wage rate at the 25th percentile in the state (from the Occupational Employment Survey). For 2010, this regression yields an elasticity of 0.28, $R^2 = 0.27$. Only six years later, the combination of federal gridlock and state activism had raised the elasticity to 0.98, $R^2 = 0.54$. The pattern is similar, though a bit less dramatic, using the median wage rather than the wage at the 25th percentile as the measure of local market wages.

Supporters of the minimum wage often argue that a skillfully set minimum raises wages at minimal cost to employment, but that further increases threaten unacceptable employment losses (Castillo-Freeman and Freeman 1992; Krueger 2015). It is difficult to imagine that the point at which the wage gain–employment loss trade-off becomes too steep is the same in all states. It seems likely that an ideal minimum wage would vary geographically, in line with wages at some relevant percentile of the local wage distributions. As states seem to have overcome the fear that a higher minimum wage will drive business to other states, they

paring outcomes there to those of nearby employers in Pennsylvania who were bound only by an unchanging federal minimum—a cross-border approach.

The 2010s have seen significant further developments of both new approaches. As the state-by-year panels became richer—more years and more state-level variation—researchers have been able to control for state-specific trends. And as more states have increased their minimum wages, researchers have extended the cross-border estimation strategy to exploit the large number of experiments provided by employers in adjacent counties in different states facing different minimum wages. In some cases, these studies follow the state-by-year panels in adding area- (typically, county-) specific time trends as controls; others adopt an alternative specification in which each border-county pair in each time period has its own fixed effect. These studies relate differences in wages or employment in border-county pairs to differences in minimum wages across the border.

In the experimental paradigm, each of these approaches can be thought of as comparing outcomes in a treated state or county affected by a minimum wage increase to those in a control area. The border-county approach explicitly identifies the county across the border as the comparison group—that is, as the basis for inferring what would have happened in the treated county but for the higher minimum wage there. But geographic proximity need not be a reliable indicator of underlying similarity. “Synthetic” control groups (Abadie, Diamond, and Hainmueller 2010) have provided an alternative strategy for identifying what would have happened in a state or county absent a change in the minimum wage.²

A smaller literature evaluates the effects of local minimum wage laws. We do not focus on these here, for two reasons. First, it is challenging enough to do justice to the large and very diverse literature on state and federal minimum wages; comparisons for local legislation would require a separate study. Second, given

the evidence that the estimated effects of individual state-level changes are quite dispersed, as Arindrajit Dube, William Lester, and Michael Reich (2010) have shown, it is not clear that we have enough local ordinances to have any confidence that results from the small number of early adopters would generalize to other cities.

Recent Evidence on the Effects of Minimum Wage Laws on Wages and Employment

Studies of the effects of the minimum wage on employment have generally focused either on teenagers or on workers in the restaurant industry. This focus is largely due to the relatively large share of minimum wage workers in both groups. For example, Dube, Lester, and Reich (2016) report that, during the 2000 to 2011 period they study, 30 percent of teenagers and 23 percent of restaurant workers earned within 10 percent of the minimum wage in effect in their states. Given that a minority of workers are directly affected by the minimum wage even in these relatively minimum wage intensive groups, the elasticity of the average wage with respect to the minimum wage will be much less than one, and the elasticity of employment will be much less than a conventionally estimated elasticity of labor demand (Neumark 2019).

Studies of teenagers have traditionally relied on CPS data. A recent addition to our data arsenal, the Quarterly Workforce Indicators (QWI), matches information about payroll and employer industry from Unemployment Insurance records to a limited set of demographic variables, primarily taken from Social Security records. Researchers who focus on low-wage industries have tended to rely on the Quarterly Census of Employment and Wages (QCEW), which provides data on payroll and employment (but not worker demographics) from essentially all employers. The QWI and QCEW both provide data by county, which are often used to study adjacent counties in states with different minimum wages. The wage measure is weekly earnings and so also captures any changes in hours worked per week.

An overview of recent work is presented in

2. Alberto Abadie and his colleagues had nineteen years of data prior to treatment and thirty-eight untreated states from which to form a synthetic control group (for California, which was treated with an antismoking program in 1989).

Table 1. Elasticities from Recent Studies of the Employment and Wage Impacts of Higher Minimum Wages

Authors	Year Data†	Years	Areas	Controls	Teenagers		Restaurant Workers	
					E*	W*	E*	W*
Neumark, Salas, and Wascher	2014 CPS	1990–2006	States	Synthetic controls	-0.145#			
—	QCEW	1990–2011	Border counties	State and quarter FE			-0.112	
Addison, Blackburn, and Cotti	2015 QCEW	1990–2014	Counties	State and year FE			-0.067	0.222#
—	QCEW	1990–2014	Counties	County-specific trends			-0.043#	0.171#
Dube, Lester, and Reich	2016 QWI	2000–2011	Border counties	Period x pair FE	-0.059	0.222#	-0.022	0.207#
Allegretto et al.	2017 CPS	1979–2014	States	State-specific trends	-0.062	0.228#		
—	CPS	1990–2014	Border counties	Period x pair FE			0.023	0.209#
Liu, Hyclak, and Regmi	2016 QWI	2000–2009	Counties	Area-specific trends	-0.173#	0.209#		
Totty	2017 QCEW	1990–2010	Counties	Factor model			-0.013	0.231#
—	CPS	1990–2013	States	Factor model	-0.040	0.097#		
Brummund and Strain	2019 QCEW	1990–2016	Border counties	Period and pair FE			-0.153#	.236#

Source: Authors' compilation.

Note: For detail, see table A1.

†CPS = Current Population Survey, QCEW = quarterly census of employment and wages, QWI = quarterly workforce indicators.

*E = employment; W = hourly earnings in CPS, weekly earnings in QCEW and QWI.

#Significantly different from zero at the 90 percent level of confidence.

table 1. (Table A1 presents a less condensed summary, with more specifications and with standard errors for each estimate.) A robust finding is that minimum wage laws raise the wages of teenagers, with an elasticity of about 0.20. This is roughly in line with a naïve model in which the wages of teenagers who initially earn less than the minimum are raised up to that level, and better-paid teens are unaffected.

Variation is substantially greater in the estimated effects on employment, which is largely due to the different strategies used to control for the effects of determinants of employment that are not explicitly included in the analysis. The estimates are also often sensitive to the choice of sample period. Thus, for example, Neumark, Ian Salas, and Wascher (2014) begin with what they call a standard panel data model—fixed effects for year and state—and estimate an employment elasticity of -0.165 ($SE = 0.041$). They then add linear state-specific trends, mirroring Sylvia Allegretto, Dube, and Reich (2011), which leaves a much smaller and statistically insignificant employment elasticity, -0.074 ($SE = 0.078$). They then consider more flexible polynomials, leading to estimates that approximate those when no state-specific trends are included. Similarly, allowing region by year fixed effects greatly reduces the original Allegretto, Dube, and Reich estimate.

Just when we thought we had discovered a stable pattern of instability, Allegretto and her colleagues (2017) report that when their sample is extended to 1979 through 2014 estimates without state-specific time trends remain negative and significant. But including state-specific trends—whether linear or a higher-order polynomial—greatly reduces the estimated impacts and leaves them statistically insignificant. They then attempt to let the data decide the appropriate set of control variables, using a LASSO procedure. The optimal specification produces estimates similar to those using state-specific trends, but chooses a subset of linear trends and one set of region-period fixed effects. Perhaps this is optimal for prediction, but it certainly does not allow any understanding of what economic factors the chosen set of controls might represent.

Although variations in specification matter, so does the period being considered: Neumark,

Salas, and Wascher (2014) report that a model with linear state-specific trends produces an employment–minimum wage elasticity of -0.229 ($SE = 0.095$) with data from 1994 to 2007, but only -0.074 ($SE = 0.078$) when the sample period is extended to 1990 to 2011.

The sensitivity of the estimates to the introduction of these additional control variables has led researchers to consider an alternative strategy—synthetic controls. For a state experiencing a minimum wage increase in year t (that is, treated in t), the procedure selects a set of untreated states that are similar in terms of teen employment or other related variables. The difference between teen employment in the treated state and a weighted average of the nontreated states (the synthetic control) is an estimate of the effect of treatment.

When analyzing the minimum wage, the set of untreated states is constantly changing, and identifying untreated states requires a relatively short memory. (Operationally, in these studies *untreated* means not treated recently, given that even currently untreated states might have seen changes in minimum wages in the past.) It is therefore perhaps not surprising that the results of studies using these synthetic (data-driven) controls vary greatly depending on the criteria for choosing the controls. David Powell (2017) proposes estimating the control group weights and the treatment (that is, minimum wage) effects simultaneously, making it “unnecessary to make the distinction between ever-treated and never-treated units.” The resulting employment–minimum wage elasticity, -0.45 , is larger than the typical estimate, but the 95 percent confidence interval extends almost up to 0. Although his method allows for inclusion of traditional time-varying controls, Powell does not include them on grounds that they are potentially affected by the minimum wage.

The rightmost columns of table 1 show estimates of the effect of the minimum wage on employment in the restaurant industry. Once again, minimum wage increases raise wages, again with an elasticity of about 0.20. The employment elasticities are somewhat less varied than those for teenagers, although in specifications with county-specific trends, matched border-pairs, or synthetic control groups the estimates are often not significantly different

from zero. In a broader sample of low-wage industries, Radhakrishnan Gopalan and his colleagues (2018) report a larger elasticity (-0.026 [SE 0.012]), all of which comes from changes in tradeable goods industries.³ Doruk Cengiz and his colleagues (2019) report a broadly similar pattern.

What one makes of the wealth of estimates in table 1 may depend on why one is interested in them. If the goal is to test predictions of standard labor demand theory, we might view an estimated elasticity of -0.12 (SE = 0.05) as confirming the theory, while -0.06 (SE = 0.05) sends a much less clear signal. But from a policy perspective, the two estimates are both “small”—small enough that the earnings gains caused by a minimum wage increase are only partially offset by employment losses. It is this perspective that led Richard Freeman (1996, 639) to describe the minimum wage as “a risky but potentially ‘profitable’ investment in redistribution.”

A related literature focuses on the effects of minimum wages on job transitions. Both Dube, Lester, and Reich (2016) and Kaj Gittings and Ian Schmutte (2016) report that accession and separation rates fall (for teenagers and restaurant workers) in response to higher minimum wages. A decline in separations is not predicted by frictionless models, but is consistent with search models with employed workers less likely to encounter a better opportunity and so less likely to leave jobs voluntarily; Gopalan and his colleagues (2018) find that the entire employment reduction in their sample following minimum wage increases comes from reduced hiring.

Conclusions and Concerns About Minimum Wage Effects

For both teenagers and workers in the restaurant industry, the employment effects of the minimum wage are often but certainly not always estimated to be negative. In general, studies that control more aggressively for other fac-

tors that might affect employment and wages (such as by including state-specific trends) tend to find smaller effects of minimum wages on employment, while the effects on wages tend to be more robust. As a rule, proportional reductions in employment tend to be smaller than (weekly) wage gains, more clearly in the studies that attempt to control for more unmeasured factors. The sensitivity of the estimates to choices about specification is noted in an earlier survey (Belman and Wolfson 2014). It remains despite the fact that newer studies have more data and often better-detailed empirical strategies (Neumark 2019).

The bottom line that employment effects are fairly small comes with three important caveats. First, although focusing on groups with relatively high concentrations of low-wage workers, such as teenagers, makes sense from a statistical point of view, impacts on workers who are likely to be members of low-income families—or on the income of poor households—would be of greater policy interest. The focus on teenagers arises partly from historical accident. The early time-series studies relied on employment data tabulated from the CPS and published by the Bureau of Labor Statistics (BLS), and such data were not available for dropouts or heads of single-parent families. Recent work by Jeffrey Clemens and Michael Strain (2018) suggests that the effect on “low-skilled” workers—those ages sixteen to twenty-five who have not completed high school—may be larger than the effect on teenagers generally. Comparing states that increased their minimum wage by more than a dollar following the federal minimum wage increases in 2007 through 2009 to those that remained at the federal minimum, they find much larger employment elasticities for low-skilled workers (on the order of -0.40) than for young (sixteen through twenty-one) adults (roughly -0.16).⁴ Interestingly, states with increases below a dollar above the federal minimum show no reductions in

3. Gopalan and his colleagues use payroll-like data from Equifax with nearby states as controls. Although this data source includes individual-level wage and turnover data, the sample is skewed toward larger and, apparently, multilocation employers, for whom shuffling production across locations would be relatively easy.

4. Clemens and Strain do not report elasticities, and their estimates for young adults differ between those based on the American Community Survey and the CPS. The text reports our attempt to construct traditional elasticities with respect to the minimum wage, averaged over the two data sets.

employment in either group, consistent with the notion that the minimum “bites” more strongly the higher it is raised.⁵

Second, the debate among researchers about how actively one should control for unmeasured factors is intense. Such controls provide protection against omitted-variable bias, but at the cost of statistically eliminating minimum wage variation that is correlated with the controls. The additional controls do not lead to imprecise minimum wage estimates—typically, the standard error of the minimum wage variable is reduced. Progress on this front is likely to depend on the ability to identify what economic forces such variables as state-specific trends actually represent.

Finally, the impacts discussed so far are all short term; but policy should be based on longer-term effects. Given longer data series for each state and more states altering their minimum wages, researchers have used distributed-lag specifications to try to tease out the long-run effects of a higher minimum wage. Both Allegretto and her colleagues (2017), on teenagers, and Dube, Lester, and Reich (2010), on restaurants, report cumulated employment effects after three or four years that are not appreciably different from the current-period estimates. Isaac Sorkin (2015) argues that this specification cannot recover long-term effects in an environment where capital adjustment is slow (for example, in the model the capital-labor ratio is fixed, so adjustment comes from entry and exit). Indeed, the effects of a saw-toothed increase are small, and the cumulative effects on employment decline rather than increase as time passes since a minimum wage was increased. Daniel Aaronson, Eric French, and Sorkin (2018) find that both exit and entry of limited service restaurants (especially chains) rise

following an increase in the minimum wage, with somewhat more exit than entry, while employment at restaurants remaining in business stays flat, consistent with Sorkin’s model.⁶ In their calibrated model, the long-run effects are two to five times larger than the short-run estimates, and the ratio is sensitive to estimates of minimum wage labor’s share of costs and to ex ante substitution possibilities between capital and labor. Given that they find much less evidence of entry and exit effects in other low-wage industries, the ratio of long- to short-run effects in low-wage labor markets remains uncertain.

One relatively uncontroversial implication of the Sorkin and Aaronson, French, and Sorkin models is that long-term responses to increases in minimum wages depend on employers’ expectations about future increases. In the absence of data on these expectations, the experience of states that have indexed their minimum wage may help identify long-term effects—at least if, in forming expectations, employers assume that indexing provisions will remain in place. Early evidence suggests that indexing may matter. Peter Brummund and Strain (2019) allow the effect of the minimum wage on employment to differ in states that have indexed their minimum wage compared to those that have not. In their preferred specifications (with county and period fixed effects or using border-county pairs) the effect of an indexed minimum wage is about three times the effect of that in non-indexed states; but when border-pair multiplied by time-effects are allowed, there is no effect of either indexed or unindexed minimum wages. Clemens and Strain (2018) do not find further employment losses in 2015 and 2016 among states that had previously indexed their minimum wage.

5. Studying low-wage workers directly is difficult, because wages change from job to job and, of course, are changed by the minimum wage. Clemens and Michael Wither (2019) focus on workers paid less than \$7.50 before the federal minimum wage increased from \$6.55 to \$7.25 and find very large employment losses in states where the federal increase was fully binding—from a relatively small increase. Cengiz and his colleagues (2019) report that minimum wage increases reduce employment below the new minimum but increase employment at and above the new minimum, for no net loss—and that this applies to workers who had been employed prior to the increase. The conflicting messages of these two studies are difficult to reconcile.

6. Card and Krueger (2015) find some evidence of entry, but essentially no exit, by one chain—McDonald’s—between 1986 and 1991. Exit rates are very low, overall, in their sample, relative to those in Aaronson, French, and Sorkin.

OVERTIME LAWS AND FLSA OVERTIME: WHAT WE KNOW

In the eighty years since the FLSA was enacted, the specification of its crucial parameters regulating hours—a penalty rate of 50 percent extra wages on hours beyond the standard weekly hours (H_s) of forty—has not changed. The only changes have been extensions of coverage to additional industries and firms, all of which were complete by the mid-1970s, and alterations in the weekly earnings above which salaried workers are presumed exempt from the law. Similarly, no major changes have been effected in state overtime laws. Indeed, most states simply extend the FLSA's provisions to some otherwise uncovered workers. Only Alaska, California, Colorado, and Nevada have mandated overtime penalties on daily work schedules beyond eight hours, varying the application of this mandate over the years.⁷ Although this additional requirement does alter labor-market outcomes (Hamermesh and Trejo 2000), it too has not changed in the last two decades. In short, because of their remarkable constancy, U.S. overtime laws do not provide as fertile a field for evaluating policy as the regulation of wages. Not surprisingly, therefore, little research on them has been produced in the United States in the last decade.

This absence does not mean that overtime regulations have been neglected in debates over labor-market policy. A repeated topic has been the substitution of comp time for overtime pay, that is, allowing employers to offer workers time off in lieu of the 1.5 times their wage rate that they would otherwise receive for overtime hours worked. This proposal regularly resurfaces in Republican-controlled Congresses, as it did in 2017 (115th Congress, H.R. 1180), passing the House of Representatives but stalling in the Senate. Evaluating its potential impacts is extremely difficult because the extent to which employers and workers would wish to

avail themselves of the opportunity to substitute more time off for additional pay is unclear.

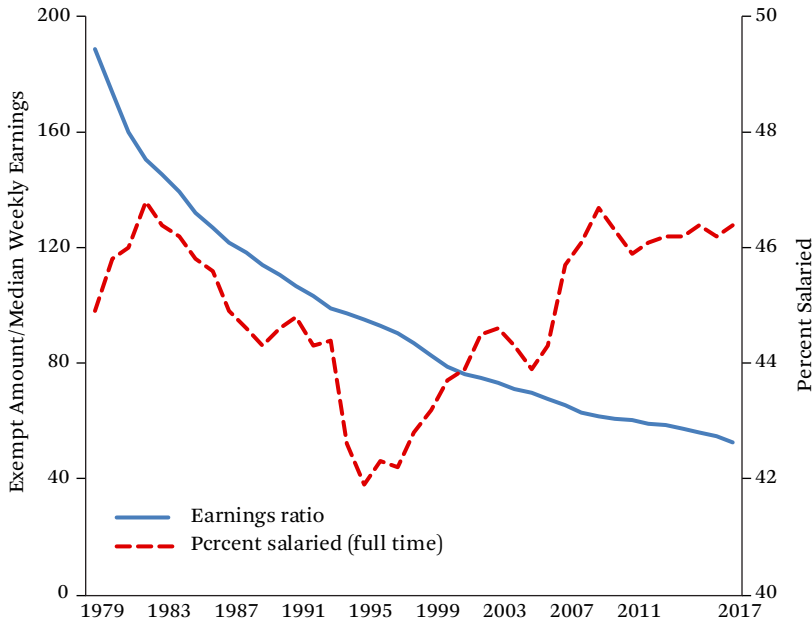
The only other policy issue that has seen serious recent debate in the United States is the dollar amount above which white-collar (salaried) workers can be considered presumptively exempt from the FLSA's overtime provisions. This limit, set at \$455 per week in the mid-1970s, has not changed in more than forty years. As shown in figure 2, its erosion in real terms is such that, though the limit was nearly 200 percent of median weekly earnings in the United States in 1979, today it is barely above 50 percent (based on calculations using the CPS-MORG for the years from 1979 to 2017). As the figure also shows (right vertical axis), the percentage of the workforce that is salaried, although varying slightly, has remained between 42 and 46 percent for the past four decades. The rough constancy of this percentage of workers means that the dollar limit has become decreasingly relevant for nearly half the U.S. workforce.

To extend overtime protection to more salaried workers, the Obama administration, after lengthy discussion, issued a rule in 2016 raising the limit to \$913 per week and indexing it beginning in 2020 to wages at the 40th percentile of the distribution of earnings of full-time salaried workers.⁸ This rule would have extended the overtime provisions of the FLSA to more than 20 percent of all full-time salaried workers, as many as an additional 6 percent of all American workers. An injunction was issued before the rule became effective, and it was struck down by district and appellate courts in 2017.

The economic outcomes that might be affected by changes in the provisions of overtime laws are employment, hours per worker, overtime hours per worker, and the hourly wage rate (and thus total earnings). For employment, the implications of decreasing H_s (the standard workweek), increasing the penalty, or extending coverage are clear: because employers sub-

7. Wage and Hour Defense Institute, "State-by-State Wage and Hour Law Summary," January 1, 2018, <https://shermanhoward.com/wp-content/uploads/2016/02/WHDI-2016-State-By-State-Chart.pdf>, accessed June 30, 2019.

8. Wage and Hour Division, Department of Labor, "Defining and Delimiting the Exemptions for Executive, Administrative, Professional, Outside Sales and Computer Employees," Federal Registrar, May 23, 2016, <https://www.federalregister.gov/documents/2016/05/23/2016-11754/defining-and-delimiting-the-exemptions-for-executive-administrative-professional-outside-sales-and>, accessed June 30, 2019.

Figure 2. FLSA Overtime-Exempt Limit

Source: Authors' calculations based on CPS-MORG weekly earnings.

stitute workers for hours when the price of the latter increases, which it will with all these changes, more binding overtime laws are a job creator. The issue, of course, is how many jobs, and whether any induced increase in employment would be large enough to prevent total labor input from decreasing. It will not be that total worker hours will decrease because of the scale effect induced by the higher price of labor at the margin (Hamermesh 1993).

Hours per worker overall will decrease with the greater stringency of overtime laws, but the changes depend on where the worker would be in the distribution of work hours. Those working less than H_s when coverage is extended or the penalty rises, and those whose hours are below any new, lower H_s , will be unaffected or might even see their hours increase as employers shift away from the now more expensive workers who had put in more than H_s hours per week. Lowering H_s makes hours of workers who were already covered and nonexempt, whose hours exceed the previous H_s , more expensive. It produces scale effects on employers' demand for them but does not affect the price of a marginal hour of their work time.

Those workers who become covered or

whose hours are newly partly subject to a penalty will see their weekly hours reduced—those hours have become more expensive at the margin, generating both substitution and scale effects. Moreover, their hours will be more likely to be at the corner solution of forty per week. These are the least ambiguous theoretical predictions about overtime laws: the laws will reduce the hours of those workers who were not affected by them, either because they were exempt or uncovered, or because their hours were below the previous H_s but above the new H_s . Even though their total hours will fall, their hours that are paid as overtime hours will rise.

This discussion assumes that these changes do not alter hourly wage rates. The evidence shows that expansions of overtime laws do reduce wage rates (Trejo 1991). That is to be expected. Given a supply of labor to firms and the market that is not perfectly inelastic, offering some workers a higher return on the marginal hour of their work time induces them to supply more effort. That enables employers to offer lower straight-time pay per hour. A strong prediction is that hourly wage rates will decrease as overtime provisions become more stringent. Coupled with receipt of overtime pay for more

hours, whether the cut is enough to reduce total earnings is unclear. But because of it, and because of the induced cuts in weekly hours, we can be sure that earnings will rise by less than the product of 50 percent of any previous hourly wage and previous hours exceeding the new H_s .

International Evidence on the Effects of Overtime Laws

Since 2006, nine studies examining changes in overtime laws in seven different countries have been published. We summarize them in table 2, detailing the legislated changes and their impacts on the essential economic outcomes—overtime hours, hours per worker, the hourly wage rate, and earnings. No study presents estimates of effects on employment, and none offers evidence on all outcomes; but on each of the other outcomes we have evidence from at least two studies. Except for the French tax waiver that Pierre Cahuc and Stéphane Carcillo (2014) examined, all the research summarized in the table deals with changes that extended overtime protection by reducing H_s .

The most frequently studied outcome in this recent research has been the impact on total hours. The results are unanimous and consistent with the theory: reducing H_s , thus subjecting more weekly hours to overtime penalties, leads employers to cut total worker hours. The reductions in hours are concentrated among those workers whose hours had been below the previous H_s but above its new lower level. The somewhat sparser evidence shows that overtime hours decrease as overtime laws become more stringent.

The direct evidence on changes in wage rates and earnings from these foreign legislative changes is ambiguous—in some of the cases wage rates rise, in others they fall. The impacts on total earnings are varied, but typically tiny. Given the decreases in hours, the minute effects on earnings suggest that declines in wage rates eat up much of the impact of broader applications of overtime laws.

Given the likely conclusion that expansions of overtime provisions decrease total hours through demand-side effects, but that they pro-

duce at worst small decreases in weekly earnings, the question is whether workers benefit from this trade-off—from sacrificing a bit of income to obtain a reduction in their work time. Comparing life satisfaction of affected workers in Japan and Korea before and after legislated decreases in H_s , Hamermesh, Daiji Kawaguchi, and Jungmin Lee (2017) show that they were happier after being forced to accept this trade-off, which changes in Japanese and Korean hours legislation induced employers to make.

Measuring the Impact of the Erosion of the FLSA Exempt Level

No doubt because of the absence of major changes in the law, only one scholarly economic study of U.S. overtime laws has been published in the past ten years (Barkume 2010). Using detailed information on the quasi-fixed costs of labor from the BLS National Compensation Survey, the study finds that lower hourly wages are associated with more use of overtime in a plant. Although large numbers of establishment-based covariates are accounted for, the absence of any exogenous shock that might be altering these outcomes means that the study cannot, and does not, claim that the relationship is causal. The current catchphrase in applied microeconomics being “causality über alles,” this makes it difficult to assess whether any changes in the law’s parameters would affect outcomes.

Despite the absence of legislated changes, the law’s economic effects may have changed. Because the nominal exempt level for salaried workers has been fixed at \$455 for more than forty years, the hours, wages, and other conditions of some workers who would have been affected by the FLSA overtime provisions are no longer affected because of its erosion in real terms. This is the treatment group, Group T, defining treatment as removal of presumptive nonexempt status. It works out that the salary limit in this group for 2014 through 2016, the recent period that we use in our empirical example, would have been almost exactly at the 40th percentile of full-time salaried workers’ earnings had the exempt limit not eroded.⁹

Of the two control groups, Group C1 consists

9. In the recent period, two states—California and New York—have set exempt limits above the federal level. We account for these by classifying workers whose weekly earnings in 2014, 2015, or 2016 were between these

Table 2. Labor-Market Impacts of Overtime Policy Changes, Seven Countries

Study	Country	Policy Change	Effect on			
			Overtime Hours	Total Hours	Hourly Wage	Earnings
Sánchez 2013	Chile	2001–2005: $H_s \downarrow$, 48 to 45		\downarrow	\uparrow	
Cahuc and Carcillo 2014	France	2007: Exempt OT pay from income and some payroll taxes	\uparrow high-wage workers only	0		
Chemin and Wasmer 2009	France	1998: 35-hour H_s , different in 3 <i>départements</i>		\downarrow very small	\downarrow very small	
Kuroda and Yamamoto 2012	Japan	1980s–1990s: $H_s \downarrow$ 48 to 40		\downarrow		0
Kawaguchi, Naito, and Yokoyama 2017	Japan	1990s: $H_s \downarrow$ 44 to 40		\downarrow , if prior 44 > H > 40		0
Kawaguchi, Lee, and Hamermesh 2013	Korea	2000s: $H_s \downarrow$ 44 to 40		H \downarrow by 40 minutes, large cut in Saturday work		
Raposo and van Ours 2010	Portugal	1996: $H_s \downarrow$ 44 to 40		H \downarrow a lot, if prior 42 > H > 40	\uparrow	\downarrow small
Skuterud 2007	Quebec	1997–2000: $H_s \downarrow$ 44 to 40	OT \downarrow nearly 1 hour	\downarrow small	\downarrow small	
Chen and Wang 2013	Taiwan	2001: H_s 48/week to 84/biweekly		\downarrow high-wage workers only		0 males small \downarrow females

Source: Authors' compilation.

of low-wage white-collar workers, sufficiently near the bottom of the earnings distribution to be nonexempt both from 2014 to 2016 and from 1987 to 1989, the earlier period that we use here. Only full-time salaried workers in the lowest 8 percent of the earnings distributions of such workers remained nonexempt in 2014 through 2016. Group C2 consists of workers sufficiently high in the distribution of salaried workers' earnings to be exempt today and to have been exempt in the past. These are in the top 60 percent of the distributions of these workers' earnings.

We chose to examine these two periods because both were times of near full employment (roughly 5 percent in both periods), and because the earlier period included the first three such years for which CPS-MORG data are available to provide information on method of pay, usual weekly earnings, actual weekly hours, and detailed demographics. The outcomes of interest are the double-differences in weekly hours and the probability that weekly hours equal forty: the differences

$$D1 = [(T_{2010s} - T_{1980s}) - [(C1_{2010s} - C1_{1980s})]], \quad (1)$$

and

$$D2 = [(T_{2010s} - T_{1980s}) - [(C2_{2010s} - C2_{1980s})]]. \quad (2)$$

Each double-difference measures the change in the outcome over these twenty-seven years in the treatment group relative to the change in a control group.

In classifying workers into these two groups we cannot use their actual weekly earnings. Because these are the products of hours and wage rates, they are affected by overtime laws and are thus endogenous to the shock whose impact we are measuring. We thus estimate a first-stage earnings regression over all full-time salaried workers, using as covariates all the demographic information in the CPS—marital status, gender, and their interaction; presence

of young children; and indicators of state of residence, years of schooling, and age. Also included are vectors of indicators of three-digit occupation and industry; but because of the endogeneity problem, weekly hours of work are excluded. We then classify full-time salaried workers into one of the three groups—T, C1, and C2—using the worker's predicted earnings.

Because of the duties test in determining whether a salaried worker is exempt from the overtime provisions, even if the worker's earnings are below the exempt limit, we cannot simply classify workers based on their predicted weekly pay. Also, the test being idiosyncratically applied in individual cases by employers, we cannot be sure which workers in the treatment or control groups might be eligible for overtime pay. The best we can do without national firm-based micro data that includes employers' classifications of their workers by overtime eligibility, data that do not exist, is to exclude from the samples workers whose employers are likely to claim they are exempt because of the duties test. We thus eliminate all those CPS respondents whose occupational classification is as manager, supervisor, or one of several occupations in which the employee clearly supervises others (such as lawyers and judges).¹⁰ These restrictions eliminate roughly one-third of salaried workers from the samples, very few from (the low-wage employees in) C1, nearly half from (the higher-paid employees in) C2.

Table 3 presents single- and double-differences in weekly hours (actual hours worked in the CPS reference week) and the probability of working exactly forty hours. The double-differences describing Group T and Group C1—which was always nonexempt—show a clear statistically significant increase in hours because of the failure to raise the exempt amount and a positive but statistically insignificant increase in bunching at forty hours per week. Examining what would happen if the 50 percent overtime penalty were extended to the

higher amounts and \$455 as in C1, those above these levels but below the 40th percentile of earnings as in the treatment group.

10. We use the detailed occupation classifications in the variable *occ80* in the 1987–1989 sample and in the variable *occ2012* in the 2014–2016 sample (U.S. Bureau of the Census 1989; CDC 2017).

Table 3. Effect on Hours of Lowering Real Overtime-Exempt Weekly Earnings, 1987–1989 to 2014–2016

Outcome	Difference 2014–2016 over 1987–1989		Double-Difference	
	Hours	Pr{H = 40}	Hours	Pr{H = 40}
Group			T – C1	
C1. Always nonexempt Elasticity	–0.442 (0.150)	0.070 (0.008)	0.302 (0.138)	0.010 (0.008)
T. Exempt 2014 to 2016, non- exempt 1987 to 1989	–0.140 (0.058)	0.080 (0.003)	–0.180 (0.083)	
			T – C2	
C2. Always exempt Elasticity	–1.195 (0.048)	0.087 (0.064)	0.834 (0.087)	–0.007 (0.003)

Source: Authors' compilation based on CPS-MORG data.

Note: Standard errors in parentheses. Estimates are based on comparisons of weekly earnings predicted from a densely specified earnings regression based on CPS-MORG data for these years. Salaried workers in occupations where the duties test was unlikely to render the worker exempt and in states other than California or New York were classified as follows: always nonexempt, if predicted earnings were below \$455 from 2014 to 2016 and from 1987 to 1989 below \$223, the same percentile point as \$455 was from 2014 to 2016; exempt from 2014 to 2016, nonexempt from 1987 to 1989, if predicted earnings were above \$455 from 2014 to 2016 and above \$223 but below \$455 from 1987 to 1989; and always exempt, if predicted earnings were above \$455 in both periods. In California, the cut-off between C1 and T was \$720 in 2014 and 2015, \$800 in 2016; in New York, the cut-off was \$600 in 2014, \$656 in 2015, and \$675 in 2016.

treatment group, the elasticity of overtime hours is a statistically significant -0.180 .¹¹

The second comparison, between Group T and Group C2, yields a large and statistically highly significant double-difference in hours, showing an elasticity of overtime hours with respect to imposing the wage penalty of -0.609 . The impact on bunching at exactly forty hours is essentially zero, albeit statistically significantly negative. Averaging the double-differences, the best conclusion is that failing to allow the presumptively exempt limit on salaried workers' overtime to increase over the past four decades has raised some salaried workers' work time by about one-half hour above what it otherwise would have been.

This exercise does not allow estimating what the impact on hourly wage rates would have been had the limit been increased. But the literature suggests that any decline in wage rates

would have been too small for earnings received on (the reduced) overtime hours not to have risen. It also does not account for possible anticipatory responses by employers from 2014 to 2016 to any expected increase in the presumptively nonexempt salary level that briefly would have become effective in December 2016 and that was formally proposed in 2015. If such responses did occur, however, they simply mean that the absolute values of the estimated elasticities in table 3 are biased toward zero.

We can conclude that increasing the exempt limit would have raised some salaried workers' earnings and reduced their weekly hours. One exercise suggested that 12.5 million workers would have been affected (Eisenbrey and Kimball 2016). Using our estimates, workers who could possibly have been affected if the increase in the exempt amount had been allowed to remain in effect are those in the treatment

11. The elasticity is calculated as the double-difference change in hours shown in the table divided by the average of overtime hours in the two groups, all divided by 0.5.

group T. Taking the size of this group relative to the number of salaried workers in 2016, and using this ratio along with the fraction of all employees who are salaried, yields a prediction that around three million workers would benefit directly if the exempt limit were raised as proposed by the Obama administration. This estimate is quite close to the estimate of four million workers produced by the Department of Labor (as noted in Weil 2017).

Performing the same calculations, but based on the Trump administration's proposed regulation (an unindexed limit of \$679 per week) yields the prediction that slightly more than one million salaried workers would become nonexempt (U.S. Department of Labor, Wage and Hour Division 2019). This number would erode over time due to the absence of indexation under the proposed regulation. Indeed, assuming 3 percent annual growth in earnings in this part of the distribution, by 2025 more than half of the one million additional workers would no longer be nonexempt.

Conclusions About the FLSA's Overtime Provisions

The FLSA's overtime provisions currently have only small effects on labor-market outcomes. They do reduce employers' demand for overtime hours, and they reduce weekly hours of work slightly. The law probably spreads employment among a few more labor-force participants, although total labor input—hours per worker times employment—probably decreases because hours drop more than employment increases. Of course, in the long term it has no impact on unemployment rates. Earnings of affected workers are probably very slightly above what they would otherwise be, even though their hourly wage rates are probably reduced. In the context of general equilibrium, however, this is a wage advantage relative to workers and others who are not affected given that the decline in total labor input reduces total gross domestic product. We can also infer that small changes that would apply the FLSA overtime provisions more broadly—by lowering standard hours, raising the overtime penalty or expanding coverage, or reducing exemptions—would have small effects in the same directions.

WHAT MIGHT BE DONE?

When the federal minimum wage was first adopted, it covered employers engaged in interstate commerce. As such, it had the capacity to change the distribution of economic activity between high- and low-wage areas, because it focused on the tradeable sector. As the economy and minimum wage legislation have co-evolved, the law's impact is now more concentrated on locally consumed goods and services. Perhaps because of this, most studies find modest effects on the employment of low-wage groups—in line, *déjà vu* all over again, with what Mary Eccles and Freeman (1982, 227) called “the professional consensus.” As was true nearly forty years ago, however, we do not have a reliable estimate of the long-term effects of minimum wages.

The controversy over recent proposals to increase the minimum wage to \$15 an hour has implicitly raised the question of the level at which minimum wages begin to have more serious negative effects on employment. The available evidence is not helpful in answering this question. First, states in which the minimum wage has come closest to this level have been high-wage states. Second, as the minimum wage is increased, both the fraction of workers affected and the average increase that affected workers receive increases. Third, most recent studies have backtracked from measuring the impact of the minimum wage based on these two factors—in effect, assuming that the effect of raising the minimum wage from, say, \$10 to \$11 is the same in Washington as it would be in Alabama.

Although the question of the “right” level of the minimum wage remains controversial, the evolution of recent minimum wage policy has taken what seem like two constructive directions. The flurry of state legislation has produced a set of minimum wages that are, roughly, *de facto* indexed to local wages in a cross-sectional sense; and twelve states have explicitly indexed the level of their legislated minimum wage over time. Whatever the right level of the minimum wage, it ought to vary with local wages. Apart from historical accident, it is hard to see why indexing makes sense for Social Security, federal income tax brackets, and the estate tax but not for the minimum wage. In-

deed, even at the federal level indexing seems like a sensible idea, and it might have the important political advantage of reducing the frequency and severity of legislative fights over changing the minimum.

The mobility of firms, workers, and consumers across state borders raises potentially important concerns for a decentralized minimum wage policy. At an empirical level, estimates that rely on cross-border designs may be biased if the control counties are out of control. For example, if employment there rises as employers move to avoid higher minimum wages in adjacent jurisdictions, the difference between treatment and controls may exaggerate the true effect on treated jurisdictions.¹² From a policy perspective, such migration is one more source of elasticity to the demand for low-wage labor in each state and one more source of job loss from an aggressive minimum wage policy. We do not find any tendency for cross-border estimates to be larger than other estimates, suggesting, at least in the short term, that little evidence indicates this sort of response. The concentration of directly affected jobs in retail trade and service industries (nontradeables) likely limits the opportunities for cost-saving relocation in the longer run. However, some evidence does indicate this sort of relocation in tradeable goods industries.

Recent history suggests that major changes in the FLSA are not likely to occur any time soon. Perhaps in response to this absence of mobility, and as with states' responses to the lack of federal action on minimum wages, a number of states are now considering joining California and New York and changing their laws to bring more salaried workers into non-exempt status.¹³ If federal rigidity continues, it is likely that the number of such states will expand.

Even with expansion of overtime regulations at the state level, and even if the applicability of FLSA overtime regulations were to be expanded, the effects on labor-market outcomes—wages, earnings, and, of particular interest, hours and employment—would be small. If we are interested in spreading work among more people and removing the United States from its current position as the international champion among wealthy countries in annual work time per worker, minor tinkering with current overtime laws will do little. We might borrow from some of the panoply of European mandates that alter the amount and timing of work hours. Among these are penalties for work on weekends, evenings, and nights and limits on annual overtime hours, while lengthening the accounting period for overtime beyond the current single week. If our goal is to spread work and make for a more relaxed society, these changes will help but their effects will also be small.

Beyond these specific changes in FLSA policy, the law was structured to apply to labor markets that are much different from today's. Fewer workers have nine-to-five schedules at fixed workplaces than they did in the 1930s; and an increasing though still small fraction of the workforce even has irregular gig jobs (Abraham and Houseman 2019; Katz and Krueger 2019). Even greater changes are likely in the future (Weil 2019). These considerations will make it worthwhile for policy analysts to go beyond the kind of narrow but important recommendations that we have presented based on our analyses of existing wage-employment-hours structures to think more broadly about how and even whether wage and hours policy fits into a labor market that is hugely different from what was contemplated when the FLSA was enacted in 1938.

12. Alternatively, workers in areas with low minimum wages may cross the border to look for perhaps scarcer but higher-paying jobs (Brown, Gilroy, and Kohen 1982, 491–92).

13. See discussions in Pennsylvania (Joseph DiStefano, "Pa. Proposal Would Boost Overtime for Half a Million Workers," *Philadelphia Inquirer*, June 26, 2018, <http://www2.philly.com/philly/blogs/inq-phillydeals/overtime-labor-employment-trump-wolf-pennsylvania-overtime-20180626.html>, accessed June 30, 2019); Washington (Washington State Department of Labor and Industries, "Learn About EAP Exemptions," <https://ini.us.engagementhq.com/learn-about-eap-exemptions>, accessed June 30, 2019); and Michigan (Michigan Legislature, Senate Bill no. 1137, October 20, 2016, <http://www.legislature.mi.gov/documents/2015-2016/bill-introduced/Senate/pdf/2016-SIB-1137.pdf>, accessed June 30, 2019).

Table A1. Elasticities from Recent Studies of the Employment and Wage Impact of Higher Minimum Wages

Study	Data [†]	Years	Geographic Units	Control Variables	Employment		Wage*		
					β	SE	β	SE	
Teenagers									
Neumark, Salas, and Wascher 2014	CPS	1990-2011	States	State and year FE	-0.165	0.041			
—	CPS	1990-2006	States	Synthetic control group	-0.145	0.060			
Dube, Lester, and Reich 2016	QWI	2000-2011	Border county pairs	County and quarter FE	-0.173	0.071	0.177	0.036	
—	QWI	2000-2011	Border county pairs	County pair x quarter FE	-0.059	0.084	0.222	0.047	
Liu, Hyclak, and Regmi 2016	QWI	2000-2009	Counties	County and quarter FE	-0.230	0.067	0.127	0.057	
—	QWI	2000-2009	Counties	Economic area x quarter FE	-0.173	0.047	0.209	0.030	
Allegretto et al. 2017	CPS MORG	1979-2014	States	State and year FE	-0.214	0.044	0.266	0.037	
—	CPS MORG	1979-2014	States	State-specific trends	-0.062	0.041	0.228	0.020	
Totty 2017	QWI	2000-2011	Counties	Factor model	-0.036	0.017	0.308	0.018	
Restaurant workers									
Neumark, Salas, and Wascher 2014	QCEW	1990-2011	Border county pairs	County and quarter FE	-0.112	0.079			
—	QCEW	1990-2006	Counties	Synthetic control group	-0.063	0.023			
Addison, Blackburn, and Cotti 2015	QCEW	1990-2014	Counties	State and year FE	-0.067	0.042	0.222	0.022	
—	QCEW	1990-2014	Counties	County-specific trends	-0.043	0.023	0.171	0.021	
Dube, Lester, and Reich 2016	QWI	2000-2011	Border county pairs	County and quarter FE	-0.073	0.042	0.203	0.028	
—	QWI	2000-2011	Border county pairs	County pair x quarter FE	-0.022	0.091	0.207	0.059	
Allegretto et al. 2017	QCEW	1990-2014	Counties	County and quarter FE	-0.240	0.075	0.233	0.026	
—	QCEW	1990-2014	Border county pairs	Period x pair fixed FE	0.023	0.069	0.209	0.033	
Totty 2017	QCEW	1990-2010	Counties	Factor model	-0.023	0.019	0.148	0.026	
Brummund and Strain 2019	QCEW	1990-2016	Counties	County pair and quarter FE	-0.153	0.078	0.236	0.032	
—	QCEW	1990-2016	Counties	County pair and quarter FE	-0.002	0.051	0.213	0.031	

Source: Authors' compilation.

[†]CPS = Current Population Survey, MORG = Merged Outgoing Rotation Groups, QCEW = Quarterly Census of Employment and Wages, QWI = Quarterly Workforce Indicators.

*Wage = Hourly earnings in CPS, weekly earnings in QCEW and QWI.

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