NBER WORKING PAPER SERIES

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WHAT DO WE KNOW? WHAT CAN BE DONE?

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Working Paper 25942
http://www.nber.org/papers/w25942

NATIONAL BUREAU OF ECONOMIC RESEARCH
1050 Massachusetts Avenue
Cambridge, MA 02138
June 2019

The authors thank the Editors, two referees, Will Carrington, David Ellwood, Larry Katz, David Weil and other participants at the Conference for helpful comments. The views expressed herein are those of the authors and do not necessarily reflect the views of the National Bureau of Economic Research.

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NBER Working Paper No. 25942
June 2019
JEL No. J23,J38

ABSTRACT

We summarize recent research on the wage and employment effects of minimum wage laws in the U.S. and infer from non-U.S. studies of hours laws the likely effects of unchanging U.S. hours laws. Minimum wages in the U.S. have increasingly become a province of state governments, with the effective minimum wage now closely related to a state's wage near the lower end of its wage distribution. Original estimates demonstrate how the 45-year failure to increase the exempt earnings level for salaried workers under U.S. hours laws 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 U.S. have produced impacts in the directions predicted by economic theory, but that these effects have been quite small.

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I. Regulating Wages and Hours

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 U.S. 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/penalty pay on weekly hours per worker above some level for non-exempt workers in covered industries/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. While both 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 sub-federal 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 the Act's passage; and perhaps because of this, there is a remarkable dearth, especially recently, of research on the economic impact of hours regulation in the U.S.

Because of these contrasts, for the minimum wage we summarize and evaluate recent legislative changes, and we synthesize the large number of recent studies that have examined the effects of minimum wage laws on wages and employment in the U.S. In the case of overtime pay, we evaluate the impact of a provision of the regulations that has not changed in 45 years, and we synthesize the likely impact of changing other provisions of the law on employment, hours and wages by examining international evidence.

II. Minimum Wage Laws in the U.S.: What We Know

A. *Minimum Wage Laws Then and Now*

Initially the FLSA of 1938 set the federal minimum wage at $0.25 (roughly $3.50 today, using the Personal Consumption Expenditure deflator), to increase to $0.30 in 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 multi-year installments). With 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. But over the past 30 years, states' decisions to increase their minimum wages have become increasingly important as the federal minimum has been 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, 29 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 that is 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, although 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 have produced a pattern of minimum wages that approximates a national minimum wage indexed to local wage distributions.[1]

B. Evolution of Research Strategies for Studying Effects of the Minimum Wage

The increasing role of the states in determining minimum wage policy has led to greater cross-sectional variation in the minimum and has provided the basis for a new generation of research on its effects. But the new work has not simply adopted the specifications used in earlier generations of research. Having more years of data and more variation than in earlier years has encouraged researchers to be more ambitious in attempts to control for other factors that may influence low-wage labor markets.

At the time of the Minimum Wage Study Commission, 1979-1981, the available literature was based largely on simple aggregate (national) time-series regressions of the teen employment/population ratio on a minimum wage variable and other variables to control for cyclical forces and longer-run trends (Brown *et al*., 1982). What we used to call the New Minimum Wage research introduced two important advances. First, variation in state minimum wages and in the "bite" of the federal minimum wage in high- vs. low-wage states led to estimation based on state-by-year observations. Most of the data came from

[1]A recent proposal by Rep. Terri Sewell (Sewell, 2019) would formalize this by grouping metropolitan and non-metropolitan areas into five "tiers" based on Regional Price Parity data. The federal minimum wage in the highest tier would be about 30% higher than in the lowest tier. Each tier would then be indexed, though from a lower base level than under most competing proposals.

tabulations of Current Population Survey (CPS), which included demographic variables, employment status, wages and state identifiers. A typical study included fixed effects for state and year and a small number of state-by-year variables as controls. Second, based on surveys of samples of employers, Card and Krueger (1994) and Neumark and Wascher (2000) studied the response of New Jersey fast-food restaurants to a 1992 minimum wage increase, comparing 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 x 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 that was not treated. The border-county approach explicitly identifies the county across the border as the comparison group – i.e., 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 *et al.*, 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]

There is also a smaller literature evaluating 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

[2] Abadie *et al.* had 19 years of data prior to treatment and 38 untreated states from which to form a synthetic control group (for California, which was "treated" with an anti-smoking program in 1989).

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 (Dube *et al.*, 2010), 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.

C. 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 *et al.* (2016) report that, during the 2000-2011 period that 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 data from the Current Population Survey. 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 from 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 Table 1. (Appendix Table 1 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.

There is substantially greater variation 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 *et al*. (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 (s.e. = 0.041). They then add linear state-specific trends, mirroring Allegretto *et al.* (2011), which leaves a much smaller and statistically insignificant employment elasticity, -0.074 (s.e. = 0.078). They then consider more flexible polynomials, leading to estimates that approximate those when no state-specific trends are included. Similarly, allowing region x year fixed effects greatly reduces the original estimate of Allegretto *et al.* (2011).

Just when we thought we had discovered a stable pattern of instability, Allegretto *et al*. (2017) report that, when their sample is extended to 1979-2014, 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 it chooses a sub-set 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.

While variations in specification matter, so does the time-period being considered: Neumark *et al*. (2014) report that a model with linear state-specific trends produces an employment-minimum wage elasticity of –0.229 (s.e. = 0.095) with data from 1994-2007, but only –0.074 (s.e. = 0.078) when the sample period is extended to 1990-2011.

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 "un-treated" states that are "similar" in terms of teen employment and/or other related variables. The difference between teen employment in the treated state and a weighted average of the non-treated 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," since 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. Powell (2017) proposes estimating the control-group weights and the "treatment" (i.e., 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. While 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 right-most 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 Gopalan *et al.* (2018) report a larger elasticity (-.026 [s.e. .012]), all of which comes from changes in tradeable-goods industries.[3] Cengiz *et al*. (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 (s.e. = 0.05) as confirming the theory, while -0.06 (s.e. =0.05) sends a much less clear signal. But

[3]Gopalan *et al.* use payroll-like data from Equifax with nearby states as controls. While this data source includes individual-level wage and turnover data, the sample is skewed toward larger and, apparently, multi-location employers, for whom shuffling production across locations would be relatively easy.

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 Freeman (1996, p. 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 *et al.* (2016) and Gittings and 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 *et al.* (2018) find that the entire employment reduction in their sample following minimum wage increases comes from reduced hiring.

D. 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 factors that might affect employment and wages (e.g., 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 was 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, while 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 BLS, and such data were not available for dropouts or heads of single-parent families. Recent work by Clemens and Strain (2018) suggests that the effect on "low-skilled" workers – those under age 16-25 who have not completed high school – may be larger than the effect on teenagers generally. Comparing states which increased their minimum wage by more than a dollar following the Federal minimum wage increases in 2007-09 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 (16-21) adults (roughly -0.16).[4] Interestingly, states with increases below a dollar above the federal minimum show no reductions in employment in either group, consistent with the notion that the minimum "bites" more strongly the higher it is raised.[5]

Second, there is an intense debate among researchers about how actively one should control for unmeasured factors: Such controls provide protection against omitted-variable bias, but at the cost of statistically eliminating minimum-wage variation that is correlated with the controls. It is worth noting that 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-run; but policy should be based on longer-run effects. With 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 *et al*. (2017) [teenagers] and Dube e*t al*. (2010) [restaurants] report cumulated employment effects after three or four years that are not appreciably different from the current-period estimates. Sorkin (2015) argues that this specification cannot recover long-run effects in an environment where capital

[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.

[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 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 *et al.* (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.

adjustment is slow (e.g., 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. Aaronson *et al.* (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, with the ratio 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 quite uncertain.

One relatively uncontroversial implication of the Sorkin and Aaronson *et al*. models is that long-run 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-run effects – at least if, in forming expectations, employers assume that indexing provisions will remain in place. Early evidence suggests that indexing may matter. Brummund and Strain (forthcoming) 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 x 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 2015-6 among states that had previously indexed their minimum wage.

[6]Card and Krueger (1995, updated 2015) found some evidence of entry, but essentially no exit, by one chain (McDonald's) in an earlier (1986-91) period. Exit rates are very low, overall, in their sample, compared to those in Aaronson *et al*.

III. Overtime Laws and FLSA Overtime: What We Know

A. Overtime Provisions: History, Changes, Current Issues and Expected Effects

In the 80 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 40—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, there have been no major changes 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 8 hours, varying the application of this mandate over the years.[7] While 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, very little research on them has been produced in the U.S. 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, i.e., 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, as it is unclear to what extent employers and workers would wish to avail themselves of the opportunity to substitute more time off for additional pay.

The only other policy issue that has seen serious recent debate in the U.S. 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/week in the mid-1970s, has not changed in over 40 years. As

[7] https://shermanhoward.com/wp-content/uploads/2016/02/WHDI-2016-State-By-State-Chart.pdf .

shown in Figure 2, its erosion in real terms is such that, while the limit was nearly 200 percent of median weekly earnings in the U.S. in 1979, today it is barely above 50 percent (based on calculations using the CPS-MORG for the years 1979-2017). As the figure also shows (right vertical axis), the percentage of the work force that is salaried, although varying slightly, has remained between 42 and 46 percent for the past 4 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 very lengthy discussion, issued a rule in 2016 raising the limit to $913/week and indexing it beginning in 2020 to wages at the 40th percentile of the distribution of earnings of full-time salaried workers (https://www.dol.gov/whd/overtime/final2016/). This rule would have extended the overtime provisions of the FLSA to over one-fifth 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 too). For employment the implications of decreasing H_S (the standard workweek), increasing the penalty or extending coverage are clear: Because employers substitute 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 deceasing. It will not be—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 workhours. 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 non-exempt, 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 worktime.

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 40 per week. These are the least ambiguous theoretical predictions about overtime laws: The laws will reduce the hours of those workers who beforehand were not affected by them, either because they were exempt, uncovered or because their hours were below the previous H_S but are 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 (Trejo, 1991, and subsequent work) shows that expansions of overtime laws do reduce wage rates. That is to be expected: With 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 worktime 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.

B. International Evidence on the Effects of Overtime Laws

Since 2006 nine studies examining changes in overtime laws, in seven different countries, have been published. In Table 2 we summarize them, detailing the legislated changes and their impacts on the essential economic outcomes—overtime hours, hours/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 examined by Cahuc and Carcillo (2014), 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.

With the likely conclusion that expansions of overtime provisions decrease total hours through demand-side effects, but that they produce 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 *et al.* (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).

C. 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 10 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. While large numbers of establishment-based covariates are accounted for, the absence of any exogenous shock that might be altering these outcomes means the study cannot, and does not, claim that the relationship is causal. With 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 over 40 years, the hours, wages, etc., 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 non-exempt status. It works out that the salary limit in this group in 2014-2016, 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.[8]

There are two "control groups": Group C1 consists of low-wage white-collar workers, sufficiently near the bottom of the earnings distribution to be non-exempt in both 2014-2016 and 1987-1989, 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 non-exempt in 2014-2016. Group C2 consists of workers sufficiently high up 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 40, the differences:

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

and:

$$D2 = [(T_{2010s} - T_{1980s}) - [(C2_{2010s} - C2_{1980s})]. \quad (1b)$$

Each double-difference measures the change in the outcome over these 27 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. Since these are the products of hours and wage rates, they are themselves affected by overtime laws and are thus

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

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 3-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 just classify workers based on their predicted weekly pay. And with 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 (e.g., lawyers and judges).[9] These restrictions eliminate roughly 1/3 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 40 hours. The double-differences describing Group T – Group C1 —the group that was always non-exempt— show a clear statistically significant increase in hours because of the failure to raise the exempt amount and a positive but statistically insignificant increase

[9]We use the detailed occupation classifications in the variable *occ80* in the 1987-89 sample and in the variable *occ2012* in the 2014-16 sample (Bureau of the Census, 1989; Centers for Disease Control, 2017).

in bunching at 40 hours per week. Examining what would happen if the 50-percent overtime penalty were extended to the treatment group, the elasticity of overtime hours is a statistically significant -0.180.[10]

The second comparison, between Group T and Group C2, yields a large and statistically highly significant double-difference in hours, with an elasticity of overtime hours with respect to imposing the wage penalty of -0.609. The impact on bunching at exactly 40 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 4 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 in 2014-16 to any expected increase in the presumptively non-exempt 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 (Eisenbrey and Kimball, 2016) suggested that 12.5 million workers would have been affected. 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 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 3 million workers would benefit directly if the exempt limit were raised as was proposed by the Obama Administration. This estimate is quite close to the estimate of 4 million workers produced by the Department of Labor (as noted in Weil, 2017).

[10]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.

Performing the same calculations, but based on the Trump Administration's proposed regulation (an unindexed limit of $679/week) yields the prediction that slightly over 1 million salaried workers would become non-exempt.[11] 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 over half of the one million additional workers would no longer be non-exempt.

D. 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 run 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 relative wage advantage compared to workers and others who are not affected, since the decline in total labor input reduces total GDP. With these conclusions about its effects, 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/reducing exemptions—would have small effects in the same directions.

IV. 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 tradable 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 Eccles and Freeman (1982, p.227) called "the professional consensus."

[11] See U.S. Department of Labor, Wage and Hour Division (2019).

As was true nearly 40 years ago, however, we do not have a reliable estimate of the long-run 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 just not very 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.

While 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, very 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; and 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. Indeed, 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 this 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." E.g., 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] And from a policy

[12]Alternatively, workers in areas with low minimum wages may cross the border to look for perhaps scarcer but higher-paying jobs (Brown *et al*., 1982, pp. 491-2).

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 any find tendency for cross-border estimates to be larger than other estimates, suggesting, at least in the short run, that there is little evidence of this sort of response. The concentration of directly affected jobs in retail trade and service industries ("non-tradables") likely limits the opportunities for cost-saving relocation in the longer run. However, there is some evidence of 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 U.S. 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 workhours. Among these are penalties for work on weekends, evening 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.

[13]See discussions in Pennsylvania, http://www2.philly.com/philly/blogs/inq-phillydeals/overtime-labor-employment-trump-wolf-pennsylvania-overtime-20180626.html; Washington, https://lni.us.engagementhq.com/learn-about-eap-exemptions; and Michigan, http://www.legislature.mi.gov/documents/2015-2016/billintroduced/Senate/pdf/2016-SIB-1137.pdf .

Beyond these specific changes in FLSA policy, it is worth noting that the law was structured to apply to labor markets that are much different from today's. Fewer workers have "9 to 5" schedules at fixed workplaces than was true in the 1930s; and an increasing, although still small fraction of the work force even has irregular "gig" jobs (Abraham and Houseman, this volume; Katz and Krueger, this volume). Even greater changes are likely in the future (Weil, this volume). 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.

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Table 1. Elasticities from Recent Studies of the Employment and Wage Impacts of Higher Minimum Wages (Details in Appendix Table 1)

				Teenagers		Restaurant Workers	
Authors (Year) Data†	**Years**	**Areas**	**Controls**	**E***	**W***	**E**	**W***
Neumark *et al.* (2014) CPS	1990-2006	states	synthetic controls	-0.145#			
Neumark *et al* (2014) QCEW	1990-2011	border counties	state & quarter FE			-0.112	
Addison *et al.* (2015) QCEW	1990-2014	counties	state & year FE			-0.067	0.222#
Addison *et al.* (2015) QCEW	1990-2014	counties	county-specific trends			-0.043#	0.171#
Dube *et al.* (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#		
Allegretto *et al.* (2017) CPS	1990-2014	border counties	period x pair FE			0.023	0.209#
Liu *et al.* (2016) QWI	2000-2009	counties	area-specific trends	-0.173#	0.209#		
Totty (2017) QCEW	1990-2010	counties	factor model			-0.013	0.231#
Totty (2017) CPS	1990-2013	states	factor model	-0.040	0.097#		
Brummund-Strain (forthcoming) QCEW	1990-2016	border counties	period &pair FE			-0.153#	.236#

†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 2. Labor-Market Impacts of Overtime Policy Changes, 7 Countries

Study	Country	Policy Change	Effect on: Overtime Hours	Total Hours	Hourly Wage	Earnings
Sánchez (2013)	Chile	2001-05: H^S ↓, 48 to 45		↓	↑	
Cahuc-Carcillo (2014	France	2007: Exempt OT pay from income and some payroll taxes	↑ high-wage workers only	0		
Chemin-Wasmer (2009)	France	1998: 35-hour H^S, different in 3 départements	`	↓ very small	↓ very small	
Kuroda-Yamamoto (2012)	Japan	1980s-1990s: H^S ↓ 48 to 40		↓		0
Kawaguchi *et al* (2017)	Japan	1990s: H^S ↓ 44 to 40		↓ , if prior 44 > H > 40		0
Kawaguchi *et al* (2013)	Korea	2000s: H^S ↓ 44 to 40		H ↓ by 40 minutes, large cut in Saturday work		
Raposo-van Ours (2010)	Portugal	1996: H^S ↓ 44 to 40		H ↓ a lot, if prior 42>H>40	↑	↓ small
Skuterud (2007)	Quebec	1997-2000: H^S ↓ 44 to 40	OT ↓ nearly 1 hour	↓ small	↓ small	
Chen-Wang (2011)	Taiwan	2001: H^S 48/week to 84/biweekly		↓ high-wage workers only		0 males small ↓ females

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

	Difference 2014-2016 – 1987-1989		Double-difference	
Outcome:	**Hours**	**Pr{H=40}**	**Hours**	**Pr{H=40}**
			GROUPS:	
GROUP:			**T – C1**	
C1. Always non-exempt	-0.442	0.070	0.302	0.010
	(0.150)	(0.008)	(0.138)	(0.008)
Elasticity:			-0.180	
			(0.083)	
T. Exempt 2014-2016, non-exempt 1987-1989	-0.140	0.080		
	(0.058)	(0.003)		
			T – C2	
C2. Always exempt	-1.195	0.087	0.834	-0.007
	(0.048)	(0.064)	(0.087)	(0.003)
Elasticity:			-0.609	
			(0.030)	

*Standard errors in parentheses. Estimates 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 CA or NY were classified as: Always non-exempt, if predicted earnings were below $455 in 201-2016 and in 1987-1989 below $223, the same percentile point as $455 was in 2014-2016; exempt 2014-2016, non-exempt 1987-1989, if predicted earnings were above $455 in 2014-2016 and above $223 but below $455 in 1987-1989; and always exempt, if predicted earnings were above $455 in both periods. In CA the cut-off between C1 and T was $720 in 2014 and 2015, $800 in 2016; in NY the cut-off was $600 in 2014, $656 in 2015, and $675 in 2016.

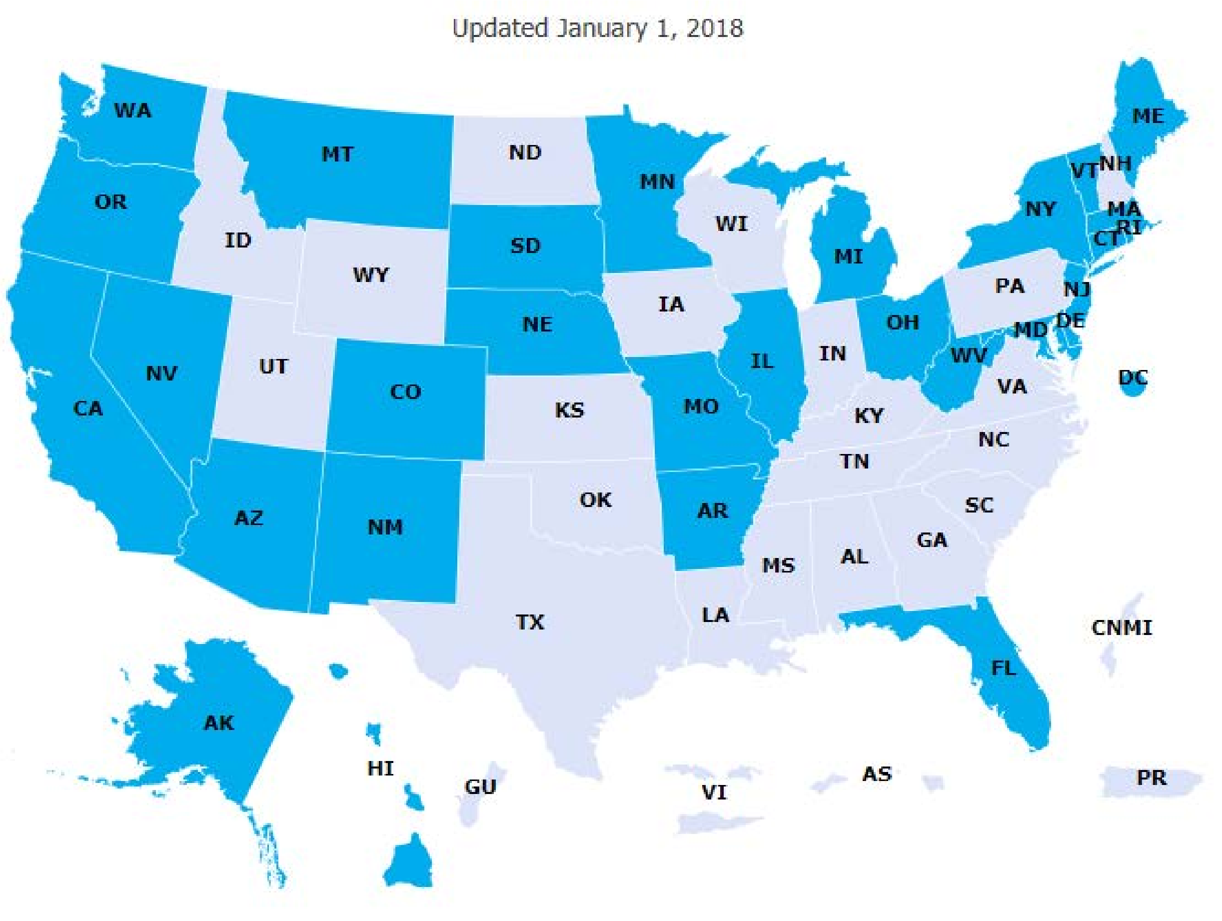

Figure 1. States with Minimum Wages that Exceed the Federal Minimum (in dark blue)

Source: https://www.dol.gov/whd/minwage/america.htm

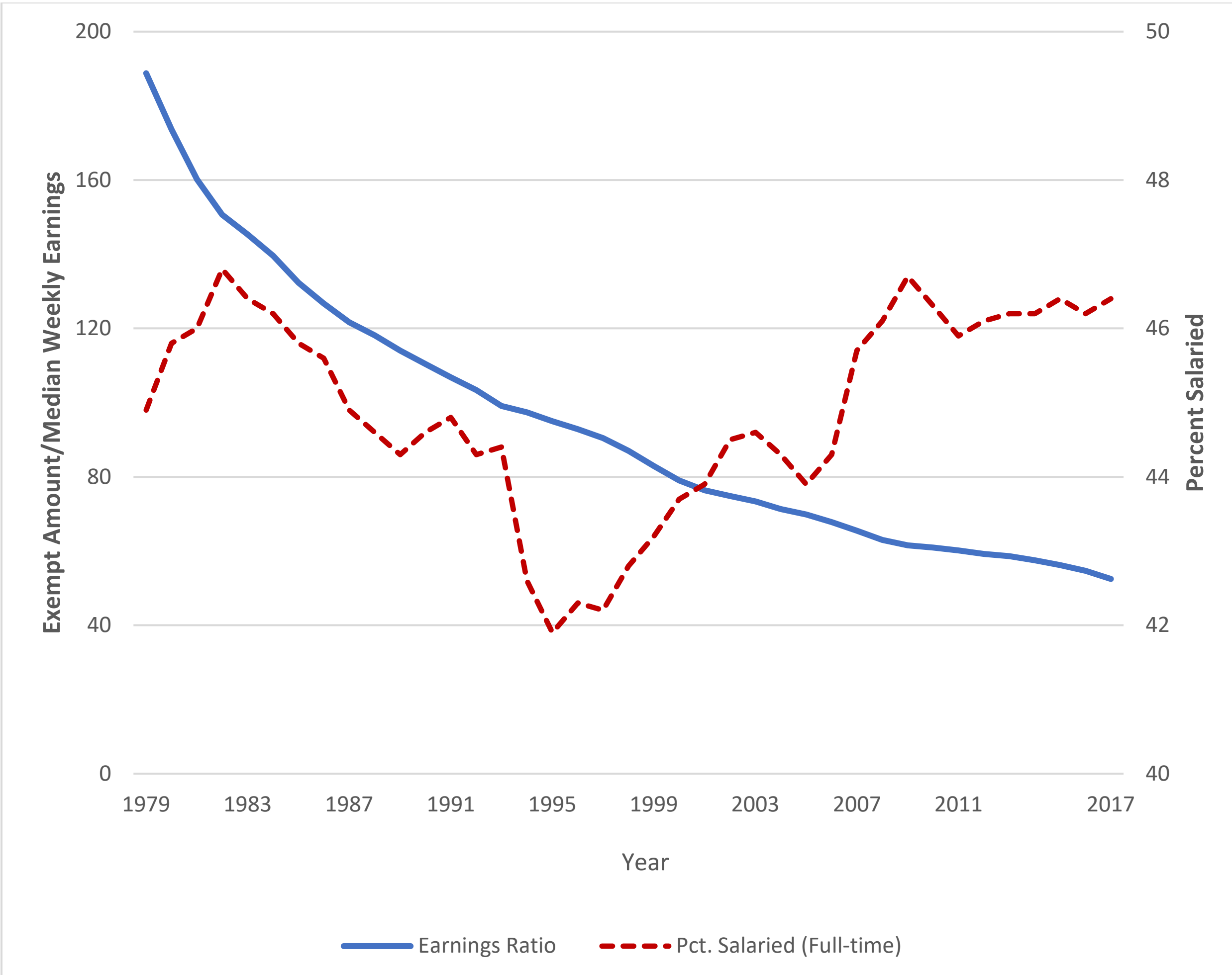

Figure 2. FLSA Overtime Exempt Limit as Percent of Median Earnings, and Percentage Full-time Salaried Workers, 1979-2017

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

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

			Geographic	Control	Employment		Wage*	
Study:	**Data†**	**Years**	**Units**	**Variables**	**β**	**s.e.**	**β**	**s.e.**
Teenagers								
Neumark *et al*. (2014)	CPS	1990-2011	states	state & year FE	-0.165	0.041		
Neumark *et al*. (2014)	CPS	1990-2006	states	synthetic control group	-0.145	0.060		
Dube *et al*. (2016)	QWI	2000-2011	border county pairs	county & quarter FE	-0.173	0.071	0.177	0.036
Dube *et al*. (2016)	QWI	2000-2011	border county pairs	county pair x quarter FE	-0.059	0.084	0.222	0.047
Liu *et al.* (2016)	QWI	2000-2009	counties	county & quarter FE	-0.230	0.067	0.127	0.057
Liu *et al.* (2016)	QWI	2000-2009	counties	Econ area x quarter FE	-0.173	0.047	0.209	0.030
Allegretto *et al*. (2017)	CPS MORG	1979-2014	states	state & year FE	-0.214	0.044	0.266	0.037
Allegretto *et al*. (2017)	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 *et al*. (2014)	QCEW	1990-2011	border county pairs	county & quarter FE	-0.112	0.079		
Neumark *et al*. (2014)	QCEW	1990-2006	counties	synthetic control group	-0.063	0.023		
Addison *et al*. (2015)	QCEW	1990-2014	counties	state & year FE	-0.067	0.042	0.222	0.022
Addison *et al*. (2015)	QCEW	1990-2014	counties	county-specific trends	-0.043	0.023	0.171	0.021
Dube *et al*. (2016)	QWI	2000-2011	border county pairs	county & quarter FE	-0.073	0.042	0.203	0.028
Dube *et al*. (2016)	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 & quarter FE	-0.240	0.075	0.233	0.026
Allegretto *et al*. (2017)	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-Strain (forthcoming)	QCEW	1990-2016	counties	county pair & quarter FE	-0.153	0.078	0.236	0.032
Brummund-Strain (forthcoming)	QCEW	1990-2016	counties	county pair & quarter FE	-0.002	0.051	0.213	0.031

†CPS = Current Population Survey, MORG = Merged Outgoing Rotation Group, QCEW = Quarterly Census of Employment and Wages, QWI = Quarterly Workforce Indicators.
*Wage = Hourly earnings in CPS, weekly earnings in QCEW and QWI.