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THE EMPLOYMENT AND REDISTRIBUTIVE EFFECTS OF REDUCING OR ELIMINATING MINIMUM WAGE TIP CREDITS

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ABSTRACT

Recent policy debate on minimum wages has focused not only on raising the minimum wage, but on eliminating the tip credit for restaurant workers. We use data on past variation in tip credits—or minimum wages for restaurant workers—to provide evidence on the potential impacts of eliminating (or reducing) the tip credit. Our evidence points to higher tipped minimum wages (smaller tip credits) reducing jobs among tipped restaurant workers, without earnings effects on those who remain employed sufficiently large to raise total earnings in this sector. And most of our evidence provides no indication that higher tipped minimum wages would be well targeted to poor or low-income families or reduce the likelihood of being poor or very low income.

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INTRODUCTION

Minimum wage tip credits allow employers to pay workers a guaranteed hourly wage, often referred to as the cash wage, which is less than the statutory minimum wage as long as tips bring the worker up to the minimum wage; if tips leave the employee short of the minimum wage, employers have to make up the difference. The current U.S. federal minimum wage is $7.25 for non-tipped workers, while the required hourly minimum wage for tipped workers is $2.13; equivalently, the tip credit is 70.6 percent.

Table 1 below shows the policy variation for 2019. The table displays the regular minimum wage prevailing in the state (the higher of the state or federal minimum wage), the prevailing tipped minimum wage, and it groups states based on their comparison to the federal policy. For example, the first group (Panel A), consists of 15 states where the federal regular minimum wage and tipped minimum wage bind, and the fourth group (Panel D), consists of 21 states where both the state minimum wage and tipped minimum wage are higher than the federal regular and tipped minimum wage, but there are tip credits. The last group (Panel E) consists of 7 states where the state minimum wage exceeds the federal minimum wage, and there is no tip credit, so the tipped minimum wage is the same as the (higher) state minimum wage.

[Table 1 here]

In this paper, we present evidence on the effects of minimum wage tip credits, motivated in part by recent policy initiatives to couple elimination of the tip credit with increases in the minimum wage—most notably the *Raise the Wage Act of 2021* (H.R. 603).[1] To provide information on the potential impacts of eliminating (or substantially reducing) the tip credit, we present evidence on the effects of variation in tip credits on earnings, employment, and family income relative to needs (the share of

[1] See https://www.congress.gov/bill/117th-congress/house-bill/603/text.

families in extreme poverty, poverty, and near-poverty).[2] We use data from the Quarterly Census of Employment and Wages (QCEW), at the state level, for full-service and limited-service restaurants, from 1990-2019 (also looking at more recent data for some analyses). And we use data from the Current Population Survey Monthly Outgoing Rotation Group (ORG) files and March Annual Social and Economic Supplement (ASEC) files, covering the same years.

Predictions for the employment effects of the minimum wage in the restaurant industry are ambiguous. The competitive model's prediction that a higher minimum wage reduces low-skill employment may not hold with monopsony (Stigler, 1946). And indeed some recent evidence suggests that in labor markets with a high degree of monopsony power, higher minimum wages do not reduce low-skilled employment (Azar et al., 2019; Munguía Corella, 2020). Monopsony-type behavior may be particularly relevant in the restaurant sector. Wessels (1997) develops a model for the restaurant industry in which workers receive both a cash wage and tips. Tips are shared among workers, and thus the average tip received by a worker is inversely related to the number of workers employed by the restaurant. In order to hire more workers, the restaurant must offset the decline in average tip income by increasing the cash wage paid to all of its existing employees. This pay structure leads to a gap between the wage and the marginal cost of labor similar to that in the textbook monopsony model, so that an increase in the minimum wage will, over some range, lead to an increase in employment, but an increase in the minimum wage above the maximum of that range will lead to a decrease in employment.

Wessels (1997) presents some evidence consistent with this model, which indicates that as the tipped minimum wage rises, it first increases and then decreases employment (as a share of sales). He also presents analysis from the extension of the minimum wage to the restaurant sector in 1966 ($1,

[2] "Income-to-needs" is the ratio of family income to the poverty threshold for that family (which depends on number of people and their ages). A family with an income-to-needs ratio of 1 is at the poverty line (a family with income-to-needs below 1 is poor). Families with income-to-needs below one-half of the poverty line are commonly referred to as being in "extreme poverty," while the "near-poverty" threshold is 1.5 times the poverty line.

with a 50 percent tip credit). Descriptive evidence indicates that this change increased both wages and employment the most in the South, possibly consistent with a positive effect of the minimum wage on employment. However, this latter analysis does not allow for a test of the "bend" in the employment response to the tipped minimum wage. Neither of these analyses is a strong test of the monopsony model, in the sense of tying the differences in employment effects to labor market concentration. Thus, we view the contribution of Wessels (1997) as primarily the theoretical insight about tipping and the marginal cost of labor.

Other research studies the effect of the tipped minimum wage, although without reference to the change in the employment response as the minimum wage increases. Wessels (1993) estimates adverse effects of tipped minimum wages on employment and hours of tipped restaurant workers, as well as adverse effects on earnings. Even & Macpherson (2014) estimate a positive earnings effect and a negative employment effect.

Our analysis of employment (and earnings) effects is closest to Even & Macpherson's. We find some evidence that smaller tip credits, equivalent to higher tipped minimum wages, increase earnings of tipped restaurant workers, although the evidence is not strong and the effect is modest. We find evidence of negative effects on employment, with employment elasticities around −.08. Reflecting the fact that the negative employment elasticity exceeds the positive elasticity for earnings per worker, we find that the estimated effect on total earnings is negative (although indistinguishable from zero). This evidence suggests that tipped restaurant workers do not gain, on average, from increases in the tipped minimum wage, and may even lose.

We then turn to a distributional analysis, which could, in principle, point to gains depending on how the effects vary across workers in different parts of the distribution of family income-to-needs. Our distributional analysis has two components. The first is a simulation along the lines of other studies of the potential redistributive effects of minimum wages that simply consider the impact of raising statutory minimum wages, without considering behavioral responses. To do this, we compare the distribution of family income-to-needs among tipped workers earning less than the statutory minimum wage to the distribution among other low-wage workers. We find that restaurant workers earning tipped wages in states where the federal minimum wage binds are in families that are either in similar positions or a bit higher in the income-to-needs distribution compared to other low-wage workers in those same states, depending on how we define other low-wage workers. This evidence suggests that higher tipped minimum wages are unlikely to have much beneficial distributional effect. We examine this more closely by comparing two simulated policy changes. In one, we eliminate the tip credit in the federal minimum wage and thereby raising the tipped minimum wage to the federal minimum wage. In the second, we implement a comparable general minimum wage increase, which raises the wage bill the same amount while preserving the tipped minimum wage. We find that the latter policy has the advantage of raising earnings for more workers and is better targeted towards reaching more poor and low-income families.

The second component is an analysis of the impact of tipped minimum wage variation on the proportion extremely poor, poor, or near-poor. Consistent with the evidence from the earnings and employment analysis, as well as the simulation analysis (absent behavioral responses), we find that neither eliminating the tipped minimum wage nor increasing it are likely to deliver redistributional benefits. However, some of these conclusions are more fragile with respect to whether we focus on more recent data only or use a longer sample period; the latter evidence, which does not point to distributional benefits of higher tipped minimum wages, is more robust and reliable.

DATA

Our different analyses use several data sources. Our data on tipped minimum wages come from a dataset provided by William Even & David Macpherson, which extends through 2020, and includes the tipped minimum wage by state and month, as well as regular minimum wages. Our earnings and employment analysis uses data from the Quarterly Census of Employment and Wages (QCEW). We use data at the state level, broken out into full-service and limited-service restaurants.[3] We interpret the data for full-service restaurants as capturing tipped restaurant workers, and the data for limited-service restaurants as capturing non-tipped restaurant workers, although we realize this classification does not apply uniformly to each worker in the two sectors. We use data on the limited-service restaurant sector to capture changes or shocks to the restaurant industry for which we want to control when estimating the effects of tipped minimum wages (on the full-service sector).

For the first part of our distributional analysis where we present descriptive evidence comparing restaurant workers to other low-wage workers and simulate the distributional effects of alternative minimum wage policy changes, we use Current Population Survey (CPS) data, combining Monthly Outgoing Rotation Group (ORG) files with March Annual Social and Economic Supplement (ASEC) files taken from IPUMS (Flood et al., 2020). The former provides information on hourly wages and allows us to identify tipped workers for the distributional analysis;[4] the latter provides the income information needed to determine the income-to-needs ratio of workers' families.[5] We use all data we can match

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[3] We do this based on 6-digit NAICS codes. In data prior to 2011, NAICS code 722110 identifies the full-service restaurant industry and NAICS code 722211 identifies the limited-service restaurant industry. In data after 2011, NAICS code 722511 identifies the full-service restaurant industry and NAICS code 722513 identifies the limited-service restaurant industry.

[4] The ORG data identify whether a respondent receives tips, commission, or overtime pay, and we use industry and occupation restrictions to better isolate tipped restaurant workers. The 2019 CBO minimum wage study identified other occupations (and some industries within them) as tipped (like hairdressers and massage therapists), but our sense is that most of these are self-employed, earn more than the minimum wage, or tips/commissions are not a large share of their pay. We hence focus on restaurant workers.

[5] The wage measures we use are hourly wages paid by employers. The family income data used to compute income-to-needs include government transfers, but not the EITC, and are pre-tax. For constructing income-to-needs, we use the family income variable generated by IPUMS to match the official poverty statistics (see https://cps.ipums.org/cps/poverty_notes.shtml).

between the March ASEC and March–June ORG files.[6] We pool all years from 2010 through 2019.[7] We use hourly wage data reported directly in the CPS ORG files, whenever possible, to measure the base rate of pay for hourly workers—and the cash wage for tipped workers.[8] Because hourly wages may be estimated poorly in some cases, for our analysis we focus only on those reporting hourly wages.[9] We construct the income-to-needs ratio using family income from the ASEC files divided by the reported poverty threshold for the family size.

In our standard panel data analysis measuring the distributional effects from changes in the tipped minimum wage and the regular minimum wage, we aggregate the CPS microdata to a state-by-year panel dataset using the 1990–2019 ASEC data. Aside from the minimum wage policy variation, we construct other controls used in this analysis. We take state GDP data from the Bureau of Economic Analysis.[10] We use these data to construct an annual state GDP growth rate to control for the pace of economic growth. We construct variables to measure the generosity of state EITCs, which could affect hours worked and therefore family income, using the percent supplement to the federal EITC for 0, 1, 2,

[6] Some CPS respondents in the April-June files can be matched when they are in the outgoing rotation group in that month. In the matching of ORG and ASEC files, the number of ORG observations that did not get matched to ASEC is 17,624 out of 270,673 – or 6.5 percent of the sample. We estimated a probit model for a successful match, on CPS basic variables such as age and dummy variables for household head, female, black, nonwhite, marital status, restaurant industry, waiter or bartender occupation, high school degree, bachelor's degree, master's degree or higher, whether a person receives overtime or tips or commission, and whether hourly wage was imputed. Additionally, we included flags for whether usual hours worked, hours worked last week, hourly wage, or earnings per week were missing. We reweighted observations by 1 over the predicted probability of a match from the probit model. (This had no impact on the substantive conclusions.)
[7] We use the more recent data for the simulation to best capture current data.
[8] The survey question asks about the hourly rate of pay on the main job excluding overtime, tips, and commissions.
[9] We do not report a standard panel analysis estimating tipped minimum wage effects on earnings or employment specific to tipped restaurant workers as identified in the CPS data, because the samples by state (by year or quarter) can be very small. We only use this identification of tipped workers in the part of our distributional analysis where we pool across many years to provide descriptive evidence.
[10] The BEA cautions that the data are not strictly comparable before and after 1997, owing to the change from SIC to NAICS industry definitions (see https://www.bea.gov/cautionary-note-about-annual-gdp-state-discontinuity).

and 3 or more children.[11] Data on this EITC policy variation comes from the Tax Policy Center.[12] We use the CPS data to construct other controls, including: the unemployment rate of 25–69 year-olds; the shares married, female, high school degree (of household head), bachelor's degree (of household head), masters or higher (of household head), black, nonwhite, and Hispanic; average family size and number of children;[13] and average age and average age squared.

EARNINGS AND EMPLOYMENT ANALYSIS

This analysis uses data from 1990–2019. Thus, we begin with a more complete depiction of the variation in minimum wage tip credits across states and over time. Since 1990, the number of states that eliminated tip credits has increased from 2 to 7. Over this period, the averages of both state minimum wages and state tipped minimum wages (the cash minimum wage required for tipped workers) have been increasing. Figure 1 provides a complete picture, showing the regular minimum wage and tipped minimum wage by year for each state. States where the tip credit was eliminated show the two minimum wages as equal.

[Figure 1 here]

Our goal is to estimate the effects of increases in the tipped minimum wage (or alternatively, holding the regular minimum wage fixed, decreases in the tip credit) on earnings and employment of tipped restaurant workers, in the QCEW data. Our dependent variables include average weekly wages,[14] employment, and total earnings.[15] Our analysis of the QCEW differs from Even & Macpherson (2014),

[11] The federal variation is subsumed in the year fixed effects included in the model, although in analyses that differentiate effects for families or individuals with different numbers of children, one would want to account for the variation in federal EITC policy with number of children. The percent supplement varies with number of children only for Wisconsin.
[12] See https://www.taxpolicycenter.org/statistics/state-eitc-percentage-federal-eitc. This source is missing information for 2018, so we filled this in by checking state websites when the Tax Policy Center data indicated a change from 2017 to 2019. The state sources used are available upon request.
[13] These are constructed using the same definitions of families used to generate the income-to-needs ratio.
[14] This is average weekly earnings, but the QCEW documentation refers to wages.
[15] Average weekly earnings is computed for those with earnings only. Employment is defined based on any earnings in the quarter. Total earnings is the total captured in the data, and hence will reflect employment effects.

apart from the period studied, in that we do not use CPS controls in our model that they include. Rather, we rely on controls constructed from data found only in the QCEW; we include controls for average weekly wages, total quarterly earnings, or average employment for the entire private sector by state.[16]

Clearly the challenge is to identify the effect of changes in the tipped minimum wage, net of other influences on earnings and employment. There are two potential concerns we address. First, we want to isolate the effect of variation in the tipped minimum wage from the effect of variation in the regular minimum wage. The regular minimum wage may not affect tipped workers directly, but it may affect them indirectly. For example, if the regular minimum wage rises in isolation, employers might shift more workers into tipped work. Or the cash wage may increase to cover the gap between the tipped and regular minimum wage. Finally, labor demand changes for the non-tipped workers that are induced by the regular minimum wage may affect the demand for tipped workers. In addition, the QCEW breakdown into full-service and limited-service restaurants does not provide an absolutely clean split on the basis of whether workers are tipped or not. We address this issue by controlling for the regular minimum wage.

Second, we want to isolate the effect of variation in the tipped minimum wage from other shocks or influences on restaurant industry employment. This issue (in general, and not just with respect to the restaurant sector) has occupied multiple papers and exchanges on the minimum wage in recent years (Allegretto, Dube, & Reich, 2011; Allegretto et al., 2017; Baskaya & Rubinstein, 2015; Clemens & Wither, 2019; Dube, Lester, & Reich, 2010; Liu, Hyclak, & Regmi, 2016; Neumark, Salas, & Wascher, 2014a and 2014b; and Neumark & Wascher, 2017). In the present context, we believe we have a particularly compelling strategy—estimating the effects of tipped minimum wages in the full-service

[16] They include controls for demographic variables and the prime-age unemployment rate. We think omitting these controls is appropriate (and even those we include may be superfluous), given that we focus on the difference between the effects on the full- and limited-service sectors, as explained below. In addition, this way our analysis is more similar to other analyses of restaurant or other low-wage sector employment, such as Dube, Lester, & Reich (2010).

sector relative to the limited-service sector. It seems highly plausible that shocks to the restaurant sector are common to these two sectors. This is, of course, an identifying assumption. But it seems far more plausible than assumptions made in other research on minimum wages to try to isolate minimum wage variation from variation in other shocks to the workers studied—such as assuming that shocks to the same Census division (clusters of 5–6 states) are common and estimating models with interactions between period and dummy variables for these Census divisions to absorb these shocks (e.g., Allegretto, Dube, & Reich, 2011).

Nonetheless, our approach can be couched in the same econometric framework, to highlight the similarity in the underlying idea. In particular, our analysis uses a panel data approach with state-by-quarter data.

We estimate, for each sector j, the regression:

(1) $E^j_{st} = \alpha^j + \beta^j \cdot TMW_{st} + \gamma^j \cdot MW_{st} + D_s\lambda^j + D_t\theta^j + \varepsilon^j_{st}$.[17]

In equation (1), E^j_{st} is log employment (or earnings) in sector (full/limited, j = F, L), state s, period t. D_s represents state dummies, and D_t quarter dummies. MW_{st} is the log of the minimum wage, and TMW_{st} the log of the tipped minimum wage.

We could interpret equation (1), for j = F or L, as simply standard minimum wage-employment regressions for each of the two sectors. But we might think there are shocks to restaurant employment that could be correlated with changes in either minimum wage. That implies that there is the potential for omitted interactions $D_s \cdot D_t$ in the equation for each sector. For either sector considered separately—and of course we are interested in the full-service sector—we cannot identify the minimum wage effects if we include the interactions $D_s \cdot D_t$. However, if we assume these have the same coefficient in the

[17] While there is some discussion of whether to regress the log of employment on the log minimum wage, or the level of employment on a minimum wage relative to an average wage, that discussion typically arises in regressions for employment *rates* (see Neumark & Yen, 2022). In the present context, the employment (and earnings) measures are totals from the QCEW data, so the log transformation is important to study the relative vs. absolute changes in outcomes, as the absolute outcomes would be dominated by variation in the large states.

equation for each sector—in other words, that the shocks are the same in the two sectors (and recall the dependent variable is defined in logs, so the relative effects have to be the same)—then the model for each sector is:

(2) $E^j_{st} = \alpha^j + \beta^j \cdot TMW_{st} + \gamma^j \cdot MW_{st} + D_s\lambda^j + D_t\theta^j + D_s \cdot D_t\psi + \varepsilon^j_{st}$.

In this case, we can think of the data as a panel on state-by-quarter-by-year observations, with two observations with the same fixed effect for each state-quarter-year observation. We can then difference the model, obtaining:

(3) $(E^F_{st} - E^L_{st}) = (\alpha^F - \alpha^L) + (\beta^F - \beta^L) \cdot TMW_{st} + (\gamma^F - \gamma^L) \cdot MW_{st} + D_s{}^j \cdot (\lambda^F - \lambda^L) + D_t \cdot (\theta^F - \theta^L) + (\varepsilon^F_{st} - \varepsilon^L_{st})$.

That is, with different coefficients on all variables for the two sectors, except for the interactions $D_s \cdot D_t$, all of the other variables still appear in the model. We can then interpret the coefficient on TMW as the relative effect of the tipped minimum wage on the full-service sector. If we are willing to assume $\beta^L = 0$, then the coefficient on TMW measures the absolute effect. But since the tipped minimum wage could shift demand toward the limited-service sector, by raising costs and prices in the full-service sector, it is better not to make this latter assumption.

The upshot of this discussion, then, is that when we estimate the model for the difference between the two sectors, we have allowed common shocks, by period (quarter) to the two restaurant sectors in each state. This parallels the kind of spatial heterogeneity control advocated, for example, by Allegretto, Dube, & Reich (2011), although we would argue that the approach is far more defensible in this case.[18]

In our view, the estimated effects of minimum wages on differences in outcomes between the full-service and limited-service sectors provide the most compelling evidence on the effects of tipped minimum wages. However, if the limited-service sector is affected by tipped minimum wages, then one

[18] See the critique in Neumark, Salas, & Wascher (2014a) of using this in the context of the Census division-by-period interactions used by Allegretto, Dube, & Reich.

may also be interested in the estimated effects for the full-service sector in isolation.[19]

The results are reported in Table 2. In Panel A we report results for the full sample period. In Panel B we restrict attention to more recent data (a period that also corresponds to our distributional analysis below). Estimates are weighted by state private-sector employment.[20] We start, in columns (1)–(3), with the estimated effects on average weekly wages. As shown in columns (1) and (2), for the full period there is a positive estimated effect of the tipped minimum wage only for the full-service sector, whereas for the more recent period there is a positive significant coefficient for both sectors, although larger (and more strongly significant) for the full-service sector. The estimated effect of the regular minimum wage is much larger for the limited-service sector, with elasticities in the .24 to .28 range, quite consistent with other estimates using the QCEW data.

[Table 2 here]

For reasons explained above, the estimates in column (3), for the relative effect on the full-service sector, are more defensible than those in column (1) as causal estimates. Here, we find that the tipped minimum wage raises earnings in the full-service sector relative to the limited-service sector for the full sample period, while the regular minimum wage has the opposite effect, which makes sense. For the more recent data, we do not find a positive effect on average weekly wages in the full-service sector (and the estimate is near zero). Keep in mind, however, that the QCEW data in columns (1)–(3) do not

[19] One spillover may be via costs, as a higher tipped minimum wage increases the costs of the full-service relative to the limited-service sector, thus shifting demand to the latter sector, leading to overstatement of disemployment effects in the full-service sector. The same might happen if employment reductions in the full-service sector shift out labor supply to the limited-service sector, which (as long as the minimum wage does not bind) would increase employment in the latter.

[20] We report unweighted results in Appendix Table A1. (All appendices are available at the end of this article as it appears in JPAM online. Go to the publisher's website and use the search engine to locate the article at http://onlinelibrary.wiley.com.) Qualitatively, the findings are very similar. The unweighted data can be thought of as representative of states, answering the question: "On average, what happened to states when the tipped (or regular) minimum wage changed?" The weighted data are representative of workers, reflecting to a much greater extent the experiences of the largest states, hence answering the question: "On average, what happened to workers?" A state policymaker – at least one from a representative state – might be most interested in the answer to the first question, whereas a national policymaker might be most interested in the answer to the second.

measure average hourly wages, but rather average weekly wages (earnings), and hence can reflect declines in hours worked.

Columns (4)–(6) present the estimates for employment. Looking at the sectors separately, most of the estimates are statistically insignificant, with the exception of the negative effect of the tipped minimum wage for the full-service sector, for the full sample period. The point estimates indicate that the tipped minimum wage reduces employment in the full-service sector, while the regular minimum wage reduces employment in the limited-service sector—both consistent with conventional disemployment effects of the minimum wage (specific to that sector), although the estimate for the more recent data is near zero.

The relative estimates in column (6), which uses the limited-service sector as a control, point to disemployment effects in the full-service sector. The full-period estimates in Panel A imply an elasticity of −.08 (significant at the 5 percent level), and the recent-period estimates in Panel B imply an elasticity of −.07 (not statistically significant).[21]

The estimates are often qualitatively similar to those in Even & Macpherson (2014). They study two sample periods: 1990:Q1–2011:Q4, and 1994:Q1–2007:Q3 (the latter to avoid recessions). In the specification for the full service vs. limited service differences, they find a positive elasticity of average weekly wages with respect to the tipped minimum wage of .034–.056, depending on the sample period and specification (including or not including state-specific trends). And they find an employment elasticity ranging from −.038 to −.079. The estimates for the shorter sample period are significant only at the 10 percent level. Thus, the disemployment effects we estimate are a little larger, and the key substantive difference is that in our more recent data (2011–2019), we do not find evidence of a

[21] We also estimated the models in Table 2 including state-by-calendar quarter interactions, to allow for different seasonality by state. The estimates were virtually the same (results available upon request).

positive average weekly wages effect.[22]

Columns (7)–(9) present estimates for total quarterly earnings, which will reflect effects on both average earnings and on employment. For the full sample period, we do not find an effect on earnings in the full-service sector, although we do (at the 10 percent significance level) for the more recent period. Now, however, the estimated effects on earnings for the full- vs. limited-service sectors are negative (not significant) for both time spans of the data, suggesting that employment declines if anything lower total earnings relative to average weekly wages (the latter are estimated over workers).

Recent econometric research has developed a better understanding of potential pitfalls of using restricted panel data estimators (like the two-way fixed effects estimator with dummy variable treatment), when in fact the underlying data-generating process is richer.[23] In the context of continuous treatment models, like ours, it seems that the most important check to is see whether there are pre-trends or dynamic treatment effects that change over time, which would imply that a richer model might be needed. To examine this question, we estimated what we regard as the core models—the estimates corresponding to Panel A of Table 2, columns (6) and (9), which show results for full minus limited service restaurants, for employment and for total earnings. Rather than just including the contemporaneous minimum wage variable, we estimate models (for a slightly shorter sample period) with two years of leads and lags (the contemporaneous effect plus 7 lags). Figure 2 report the estimates of these coefficients. The figure indicates that the far-more disaggregated estimates are, not surprisingly, much less precise. But there are two key things to notice. First, there is no evidence of leading effects such as differential pre-trends associated with subsequent minimum wage variation. And

[22] Although they include some other controls, these appear not to matter much, as we might expect, especially, for the full service minus limited service specifications.

[23] These issues were broached less formally, in earlier empirical research, by, e.g., Korenman & Neumark (1991) and – in the context of minimum wage effects – by Meer & West (2016). For a discussion of the growing number of formal econometric treatments, see, e.g., Callaway, Goodman-Bacon, & Sant'Anna (2021) and Wooldridge (2021, and the many references therein).

second, there is no obvious indication of treatment effects that evolve systematically (such as getting larger) over time.[24]

[Figure 2 here]

Thus, the evidence is most consistent with adverse employment effects from raising the tipped minimum wage, with an employment elasticity of around −.07 to −.08. Using the data for the more recent period, the elasticity is smaller (−.07) and not significant; but the estimate for the full period (−.08) is statistically significant. Perhaps more surprisingly, the evidence does not point to strong positive effects on average weekly wages. Only the estimate for the full period is much different from zero and statistically significant, but the elasticity is small (.04). And the estimated effects on total earnings are negative and insignificant. We suspect that the weak effects on average earnings reflect declines in hours worked, although another conjecture is that tips decline. Additionally, the negative total earnings effect is consistent with declines in employment (and hours).[25]

A potential caveat is that the QCEW data may not adequately capture tips. Indeed, the BEA, in constructing the National Income and Product Accounts, adjusts wage and salary data from the QCEW for the misreporting of wages, including tips. In 2017 data, the total adjustment was an increase of 1.18 percent.[26] This seems to be a relatively minor adjustment, although given that the adjustment is based on data from a broader set of jobs than just restaurant workers (like barbers and taxi drivers), and tips

[24] Figures corresponding to many of the other estimates in Table 2, available upon request, are qualitatively similar. Note that the contemporaneous and lagged coefficient estimates should be accumulated to provide a comparison with the estimates in Table 2.

[25] We also conducted a synthetic control analysis of a few cases of large and isolated increases in the tipped minimum wage. This is described in Appendix B. (All appendices are available at the end of this article as it appears in JPAM online. Go to the publisher's website and use the search engine to locate the article at http://onlinelibrary.wiley.com.) Overall, we find the synthetic control analyses broadly consistent with the preceding panel data analyses, although the synthetic control analysis is less informative statistically, likely as a consequence of considering separate analyses of single tipped minimum wage changes in one state at a time.

[26] See NIPA Handbook, December 2020, https://www.bea.gov/resources/methodologies/nipa-handbook/pdf/all-chapters.pdf, Chapter 10, Table 10.2, and https://www.bea.gov/sites/default/files/methodologies/IOmanual_092906.pdf, 5-6 to 5-7. At the end of the day, the QCEW is supposed to include tips (https://www.bls.gov/cew/overview.htm).

are likely a larger share of compensation for full-service restaurant workers, this is likely an under-adjustment for restaurant workers. In addition, the earnings gains from tipped minimum wages would be understated only if the under-reporting of tip income increases when the tipped minimum wage is raised. There is no obvious reason to expect this kind of change. Indeed, there is anecdotal evidence that increases in tipped minimum wages led some restaurant owners to try to reduce the use of tips (Cohen, 2015), which might imply less under-reporting after increases in the tipped minimum wage; this would suggest even less of a positive earnings effect than what we find.[27] Of course, if tipping were actually reduced when the tipped minimum wage rises, this would also moderate the earnings effect.

DISTRIBUTIONAL ANALYSIS

Our distributional analysis proceeds in two parts. First, we present a static simulation analysis assuming no behavioral responses. Then we estimate the effects of changes in the tipped minimum wage on poverty and related metrics of the distribution of income.

Static simulation

We focus on states and years where the federal minimum wage binds, which includes 36 states at the start of 2010, declining to 21 states by the end of 2019. We find it more informative to restrict to these states (and years) to isolate the effects of tip credits. If instead we combined states with higher vs. lower minimum wages, it would be difficult to know whether any variation in family income (relative to needs) that we document between tipped and other low-wage workers comes from tip credits or differences in minimum wages.

When we use data we can match between the March ASEC and March–June ORG files, and pool

[27] On the other hand, another possibility raised by a reviewer is that when the tipped minimum wage rises, employees reduce tip reports because a lower tip amount is needed to meet the minimum wage requirement. This would presumably be driven by employers' interest in more over-reporting of tips prior to the increase in the tipped minimum wage, so that smaller wage payments would have been needed to meet the minimum wage requirement. This kind of change could lead to downward bias in the estimated earnings effect of increases in the tipped minimum wage.

all years from 2010 through 2019, we obtain 2,214 observations for tipped restaurant workers and 249,332 observations for all other workers. The latter sample size is reduced based on other restrictions imposed in the analyses we report below.

We begin by showing information on the distributions of hourly wages for tipped restaurant workers (measured without tips) and other hourly workers.[28] The histograms for wages are shown in the top panel of Figure 3, with a maximum wage of $40.[29] In both cases, we can see a spike for tipped minimum wage restaurant workers at the federal tipped minimum wage (recall that some states where the federal minimum wage binds have a higher tipped minimum wage), and we can see a spike for other hourly workers at the federal minimum wage.[30] The figure shows, as we would expect, lower cash wages for tipped restaurant workers.[31] This result also holds if we compute estimated effective hourly wages from total earnings including tips.[32]

[Figure 3 here]

The distributions shown in Figure 3 do not control for other characteristics of workers, and the "other workers" category may include many workers who are higher-skilled than restaurant workers.

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[28] Our comparable hourly workers do not include non-waiter or non-bartender occupations that report working in the restaurant industry and receiving tips, overtime, and commission, to avoid misclassification issues.

[29] Because we are interested in the wage histograms in the distributions relative to the minimum, we do not adjust wages for inflation to be comparable across years. This would have no impact on the question of where different workers are in the family income-to-needs distribution. For the final simulation we do, this could have a minor impact on the calculations because the implied increases in earnings that we calculate come from different years. But it should not materially affect the key comparison we do between two alternative minimum wage policies (which we verified).

[30] For tipped workers the wages are base wages, net of tips.

[31] There is no explicit lower minimum wage for commissioned workers, but our best understanding is that commissions can count towards minimum wages. See, e.g., https://www.workplacefairness.org/minimum-wage#9 and https://smallbusiness.chron.com/rights-commissiononly-paid-workers-44625.html. Many websites providing this kind of information say the same thing, although we have not found explicit federal guidance. Regardless, when we looked at the hourly wage distribution for hourly non-restaurant workers who earn tips, commissions, or overtime (we cannot break out those who earn the latter), there is little evidence of hourly wages below the federal minimum – nothing as pronounced as for tipped restaurant workers in Figure 3.

[32] This is shown in Figure A1 in Appendix A. (All appendices are available at the end of this article as it appears in JPAM online. Go to the publisher's website and use the search engine to locate the article at http://onlinelibrary.wiley.com.)

And they do not include tips. This is reflected in the much greater mass in the right tail of the distribution of wages for other hourly workers. Because of this, below we restrict attention to comparisons between tipped and other workers with more similar wage distributions.

First, though, we compare the distributions of family income-to-needs for these two groups of workers. The income data include tips, and incomes of other family members. These are reported in the bottom panel of Figure 3. As that panel shows, it appears that tipped workers have lower values of family income-to-needs, including, for example, a greater share at or below the poverty line.

However, this conclusion from Figure 3 could be very misleading because of the far greater representation of high-wage (and hence likely higher-skilled) workers in the "other" group. Hence, we next restrict comparisons to other hourly workers who earn lower wages. These lower-wage, non-tipped hourly workers are more relevant to comparing the distributional effects of eliminating (or reducing) tip credits vs. general increases in the minimum wage. Thus, we next compare tipped workers to workers with wages at or below the federal minimum wage.[33] When doing this, the number of "comparison" other hourly workers drops substantially, from about 145,000 to about 5,400.

The histograms for hourly wages are shown in the top panel of Figure 4. We now show the data only up to $15, since the sample is restricted to low-wage other hourly workers, and, as the top panel of Figure 3 showed, there are relatively few restaurant workers with higher hourly wages. The top panel of Figure 4 shows, not surprisingly, that almost all non-tipped hourly workers earning less than or equal to the federal minimum wage in fact earn exactly that minimum wage.[34]

[Figure 4 here]

Again, we next compare the distributions of family income-to-needs for the two groups of

[33] Recall that we restrict to states and years in which the federal minimum wage ($7.25) binds.

[34] Figures A2.a and A3.a show similar graphs with different comparison workers – first those within 10 percent of the minimum wage, and second for non-tipped restaurant workers. In both cases, because we do not cap the comparison workers at the federal minimum wage, the spike is less pronounced; but it is still apparent. All three versions of the figure show a large share of tipped restaurant workers with lower wages.

workers in the top panel of Figure 4. These are reported in the bottom panel of the figure. The evidence differs from that in the bottom panel of Figure 3. We now see that other hourly workers are more likely to be in poor or extremely poor (family income below one-half the poverty line) families than tipped workers. Moreover, the higher incomes-to-needs of tipped workers is not concentrated only near the poverty line, but up to more than three times the poverty line. To draw some more precise conclusions, based on the numbers underlying the figure, 18.2 percent of tipped restaurant workers are classified as poor, compared to 21.7 percent of other hourly workers who earn the federal minimum or less, a 3.5 percentage point difference. In addition, 7.0 percent of tipped restaurant workers are classified as extremely poor compared to 9.7 percent for the comparison group, a 2.7 percentage point difference.[35]

The evidence of higher family incomes among tipped restaurant workers may arise because of the sharp restriction of other hourly workers to those earning at or below the federal minimum wage. Thus, we next adopt a more middle-ground comparison, comparing tipped workers to workers with wages at or below 125 percent of the federal minimum wage. In this case, the number of comparison other hourly workers increases about five-fold, to over 26,500.

The histograms for wages are shown in the top panel of Figure 5. We again show the data only up to $15, since the sample is restricted to low-wage other hourly workers, and, as the top panel of Figure 3 showed, there are relatively few restaurant workers with higher hourly wages. The top panel of Figure 5 differs from the top panel of Figure 4 in including observations on other hourly workers earning above $7.25.

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[35] Figures A2.b and A3.b show these figures for the same two alternative comparison groups. The first of these looks very similar to the bottom panel of Figure 4. (Specifically, the percent poor (extremely poor) for non-restaurant workers near the minimum wage is 21.6 percent (9.0 percent). For tipped restaurant workers the percentages are 18.2 percent (7.0 percent).) However, the second, using non-tipped restaurant workers, indicates that tipped restaurant workers have slightly lower income-to-needs than non-tipped restaurant workers. (Specifically, the percent poor (extremely poor) for non-tipped restaurant workers near the minimum wage is 16.3 percent (6.1 percent). For tipped restaurant workers the percentages are, again, 18.2 percent (7.0 percent).) But it would seem difficult to rationalize raising the tipped minimum wage just to address this shortfall, when other non-tipped workers are in lower-income families than tipped restaurant workers, as the bottom panel of Figure 4 shows.

[Figure 5 here]

We next, as before, compare the distributions of family income-to-needs for these two groups of workers. These are reported in the bottom panel of Figure 5. The evidence in Figure 5 now indicates fairly similar distributions of family income-to-needs for tipped restaurant workers and other hourly workers. There are small differences in the proportions in each income-to-needs category, but the differences are small. For example, 18.2 percent of tipped restaurant workers are poor, vs. 18.6 percent of other low-wage workers up to 125 percent of the federal minimum wage.[36] What this evidence indicates, in comparison to Figure 4, is that tipped restaurant workers are fairly low-wage but are more comparable to workers earning up to 125 percent of the minimum wage than to minimum wage workers.[37]

Finally, we provide evidence on the relationship between minimum wage policy and the distribution of income-to-needs by presenting a calculation that parallels one used often in the research literature (see, e.g., Lundstrom, 2017). In particular, we simulate the distributional effects of a change in minimum wage policy by applying the change in policy to all affected workers. Assuming no other behavioral changes (i.e., declines in employment or hours, or changes in other workers' wages), the change in minimum wage policy generates an overall change in the wage bill paid to workers, which we then allocate to different parts of the family income-to-needs distribution based on wages of workers and their family income-to-needs. What we do differently from prior research like this is that we directly compare a policy of eliminating the federal minimum wage tip credit to an equivalent policy that

[36] The extreme poor percentage is comparable; 7.0 percent of tipped restaurant workers are extremely poor compared to 7.4 percent of other hourly workers who make 125 percent of the minimum wage or less.

[37] We also did calculations similar to the last two (in Figure 5), but using estimated hourly wages for non-hourly workers. This only increased the number of comparison workers, as we cannot compute an hourly wage net of tips for non-hourly restaurant workers. This computation may be somewhat unreliable, and hence we do not emphasize the findings for these samples (results available upon request). In addition, we redid the analyses in Figures 3-5 dropping observations with imputed earnings (as discussed, e.g., in Hirsch & Shumacher, 2004), and the findings were not substantially different (results available upon request).

preserves the tipped minimum wage but raises the general minimum wage enough to create the same overall increase in the wage bill. We evaluate which policy is more effective at increasing incomes of workers in the lower end of the family income-to-needs distribution.

First, we do this calculation for eliminating the tip credit, so that the minimum wage for restaurant workers in all the states and years we study is increased to the $7.25 federal minimum wage. To estimate the number of hours to which to apply the wage increase, we use hours usually worked per week from the ORG files and weeks worked last year from the ASEC files.[38] We use the ORG earnings weight to calculate total benefits (i.e., the total wage bill increase). But since we use four monthly ORG files, we divide these weights by 4.[39] This calculation is applied to all tipped restaurant workers (but of course yields a non-zero estimate only for those who earn less than the regular federal minimum wage). The implied increase in the wage bill (for wages excluding tips) is $18.3 billion.

We then do an alternative calculation where we maintain the tipped minimum wages as they are but instead raise minimum wages for all other workers who are paid $7 or more.[40,41] We find that an increase in the minimum wage from $7.25 to $8.15—using data on hours and weeks in the same way—delivers the same approximate $18.4 billion increase in the wage bill.[42]

Finally, we compare the distribution of these two ways of increasing the wage bill across ranges

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[38] When usual hours worked per week was missing, we used hours worked last week from the ORG files, and if that was also missing, we use usual weekly hours worked last year from the ASEC files. The first method provided hours for almost all observations, and all methods combined provided hours for all but a handful of observations. The latter are discarded.

[39] These earnings weights in each month are intended to make the sample representative of the U.S. population. This affects the calculated benefit amount. But if we did not rescale by four, the distributional calculation (i.e., the share going to each income-to-needs range) would be the same.

[40] To be clearer, we preserve the federal or state tipped minimum wage that prevails. We do this because the federal law, at least, does not specify the tipped minimum wage as a percentage of the regular minimum wage.

[41] There may be some paid lower wages because they are not covered by the law. If their wages were, however, increased owing to a minimum wage hike, we would be understating the gains to the group of other hourly workers.

[42] We arrive at the $8.15 minimum wage by adjusting it until we match the total benefit. This works because the total benefit is monotonically increasing in the minimum wage change. In fact, the benefit in the second case was $16.624 billion. This is closest we came to $16.676 billion using 1 penny increments in the minimum wage.

of the distribution of family income-to-needs. The results are reported in Table 3. The table shows that the general increase in the minimum wage does more to increase incomes of the lowest-income workers. The share of benefits going to those in extreme poverty, for example, is 4.9 percent from the general minimum wage increase, compared to 3.5 percent for the elimination of the tip credit. Similarly, the total percentage going to those in poor families is 18.4 percent for the general minimum wage increase, vs. 14.8 percent for the elimination of the tip credit. On the other hand, the elimination of the tip credit distributes somewhat more income to those between the poverty line and three times the poverty line. Note also that far more workers benefit from the general minimum wage increase.

[Table 3 here]

Thus, for the same overall increase in labor costs and assuming no employment effects (or other behavioral responses) from increased minimum wages or tipped minimum wages, a general minimum wage increase, as compared to elimination of the tip credit, does more to increase incomes of workers in the lowest-income families, and spreads the benefits to more workers.

Panel data analysis

Our panel data analysis of distributional effects is simpler than our analysis of earnings and employment, because we are not estimating relative effects on two sectors. Rather, we estimate standard panel data models, although we do consider the effects of including state-specific linear time trends (partially following Dube, 2019).[43] We also incorporate the other controls he incorporates; these controls were discussed above. Finally, we estimate models with both levels and logs of the minimum wage variable and the dependent variables.

In Table 4 we report estimates for regressions of the proportions of individuals in families that

[43] In our view, these are problematic, for reasons discussed in, for example, Meer & West (2016). However, we want to report on the sensitivity of the results. We do not introduce the Census division x period interactions that Dube does, for reasons discussed at length in Neumark, Salas, & Wascher (2014a).

are in extreme poverty, in poverty, or in near poverty. We also report results for all individuals aged 16+.[44] The results are weighted by population.

[Table 4 here]

In Panels A–C, for the full period from 1990–2019, the most striking finding is that a higher tipped minimum wage is never associated with lower extreme poverty, lower poverty, or lower near poverty. Nearly every estimate is positive, with one exception out of a total of 12 estimates, and in that one case (Panel B, column (2)) the estimate is essentially zero. In fact, several of the positive estimates are significant for both the level and log specifications, and with or without state-specific trends. A higher minimum wage can increase poverty because of the employment effects, especially if these happen to be concentrated among workers in families with lower income-to-needs.[45] Thus, there is no evidence in these estimates that a higher tipped minimum wage reduces the incidence of poverty or very low income—in fact, the evidence is more suggestive that higher tipped minimum wages are associated with higher incidence of very low income, particularly for near-poverty (income-to-needs below 1.5 times the poverty line).

Although not our focus, we also report the effects of regular minimum wages. These kinds of estimates have been reported in numerous other papers, with many studies finding no clear relationship between minimum wages and poverty, and the literature more generally reporting ambiguous or weak statistical conclusions.[46] For the full period, the estimated effects of regular minimum wages are always negative (in both tables), and sometimes significant, pointing to declines in the incidence of low income

[44] Results were very similar for ages 16-70.
[45] We report unweighted estimates in Appendix Table A2. The estimates are quite similar to the weighted estimates (*all* are positive). (All appendices are available at the end of this article as it appears in JPAM online. Go to the publisher's website and use the search engine to locate the article at http://onlinelibrary.wiley.com.)
[46] For papers finding no significant relationship, see, e.g., Neumark & Wascher (2001), Sabia & Burkhauser (2010), and Sabia & Nielsen (2015). Neumark (2016) finds that a variety of estimates point to poverty reductions, but the relationship is not statistically significant. Neumark, Schweitzer, & Wascher (2004) provide some evidence of adverse distributional effects. Dube (2019) reports strong poverty reduction effects, and Addison & Blackburn (1999) and DeFina (2008) find poverty reduction effects, but only for subgroups (very narrow in the case of Addison & Blackburn – teenagers and junior high school dropouts).

with elasticities ranging to as large as –.30 (but more commonly in the range of approximately −.1 to −.2).

For the recent period, from 2010–2019, however, the results are different, and quite fragile. The estimates in Panels D–F of Table 4 always point to reductions in the incidence of poverty or low income from higher tipped minimum wages, with negative estimates. For the proportions poor and nearly poor, some of the estimates are statistically significant, but only for specifications in levels. The evidence of reductions in extreme poverty is stronger, with negative and significant results across specifications, with one large elasticity (−.37 in column (3)). However, in the recent period, the estimates do not indicate any statistically significant evidence that regular minimum wages reduce the incidence of low income, and the estimates in columns (2) and (4) imply that higher regular minimum wages increase extreme poverty.[47,48]

Thus, the only way to support an inference that tipped minimum wage increases would have beneficial distributional effects is to focus *only* on the recent data (and *only* on the weighted estimates). That is clearly a fragile conclusion. Moreover, if one wanted to embrace the recent evidence on tipped minimum wages (from the weighted data), one would also have to conclude that the distributional effects of regular minimum wages are to increase extreme poverty (unless one discarded the models with state-specific linear trends), with no clear effects on the proportions poor or near-poor.

CONCLUSIONS AND DISCUSSION

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[47] The unweighted estimates of the effects of regular minimum wages for the longer period, in Appendix Table A2, are closer to zero, vary in sign, and are never significant. And for the shorter, more recent period, the estimates do not indicate any statistically significant evidence that regular minimum wages reduce the incidence of low income; and the estimates with state linear trends point to increases in extreme poverty. (All appendices are available at the end of this article as it appears in JPAM online. Go to the publisher’s website and use the search engine to locate the article at http://onlinelibrary.wiley.com.)

[48] Figure 6 reports results with leads and lags for a key specification using the CPS data – in particular, the estimates corresponding to Table 4, Panel B, column (4) – for poverty. These graphs indicate that the more disaggregated estimates are, not surprisingly, much less precise; but there is no obvious indication of pre-trends or evolving treatment effects. Additional figures leading to the same conclusion, for other specifications, are available upon request.

Recent policy debate on minimum wages has focused not only on raising the minimum wage, but on eliminating the tip credit for restaurant workers. We use data on past variation in tip credits—or minimum wages for restaurant workers—to provide evidence on the potential impacts of eliminating (or substantially reducing) the tip credit. We present evidence on the effects of variation in tip credits on earnings, employment, and family income relative to needs.

Our evidence on employment and earnings is most consistent with adverse employment effects from raising the tipped minimum wage, with an employment elasticity of around −.08. Moreover, the evidence does not point to strong positive effects on average weekly wages, and the estimated effects on total earnings are negative (albeit not statistically significant). These results are quite robust to the alternative analyses we do.

With regard to effects on the incidence of extreme poverty, poverty, and near-poverty, the evidence is a bit less robust. Using the longer sample period (1990–2019), we find that tipped minimum wages do not deliver benefits to poor or low-income workers and may have adverse consequences, while regular minimum wages provide some benefits. A static simulation analysis leads to similar conclusions. For much more recent data, these results are sometimes flipped, although the estimates using the recent data only are fragile, and interestingly the same specifications that suggest possible beneficial distributional effects of tipped minimum wages also sometimes point to quite strong adverse distributional effects of regular minimum wages. However, the latter evidence is statistically significant only for the specifications with state-specific linear trends, which might be particularly hard to disentangle from policy effects in a short panel.

What do we make of the conflicting evidence? The argument that more data is always good suggests that the estimates for the full period are most reliable. In this case, we would conclude that higher tipped minimum wages do not help reduce poverty or the incidence of low income, and may even increase them slightly, whereas there is some evidence that regular minimum wages can reduce poverty or the incidence of low income. This conclusion is also consistent with our distributional simulation analysis pointing to the greater effectiveness of regular minimum wages at delivering some benefits to low-income families.

However, the argument that more data is always better is perhaps more appropriate to empirical analyses focusing on estimating a parameter that is fairly stable. As emphasized in Neumark & Wascher (2008), the effects of minimum wages on poverty and the income distribution depend on many factors, such as other distributional policies, the wage distribution, how the wage distribution varies across the income distribution, and the incidence of the effects of minimum wages. Thus, the perspective of estimating a stable parameter may be inappropriate, and more recent data may be more informative about the likely effects of near-term policy changes. In this case, however, we reach less clear conclusions.

Only a carefully "curated" selection of the results—using the weighted data for the recent period only—could make the case for benefits from raising the tipped minimum wage. This same curation, however, could lead to the conclusion that higher regular minimum wages increase extreme poverty. We suspect that neither of these conclusions is reliable, and clearly any argument that higher tipped minimum wages would have beneficial distributional effects rests on precarious evidence.

With the longer time span included, there is no case for distributional benefits from raising the tipped minimum wage. And even for the shorter period, our static distributional analysis suggests tipped minimum wages are not well targeted to those in poor families. Finally, our evidence is quite clear and unambiguous in pointing to higher tipped minimum wages (smaller tip credits) reducing jobs among tipped restaurant workers, without enough of an increase in earnings of those who remain employed to offset the job loss.

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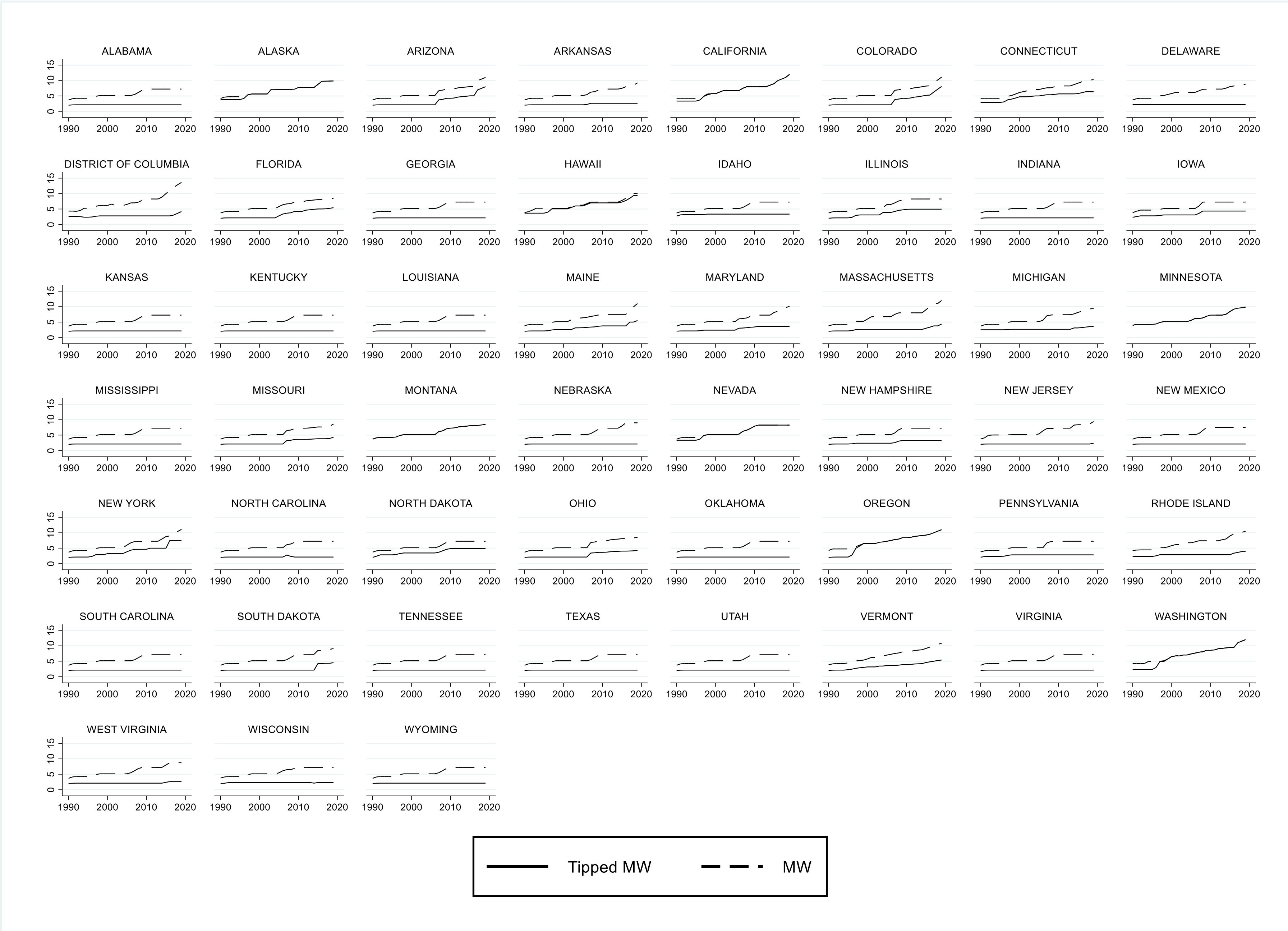
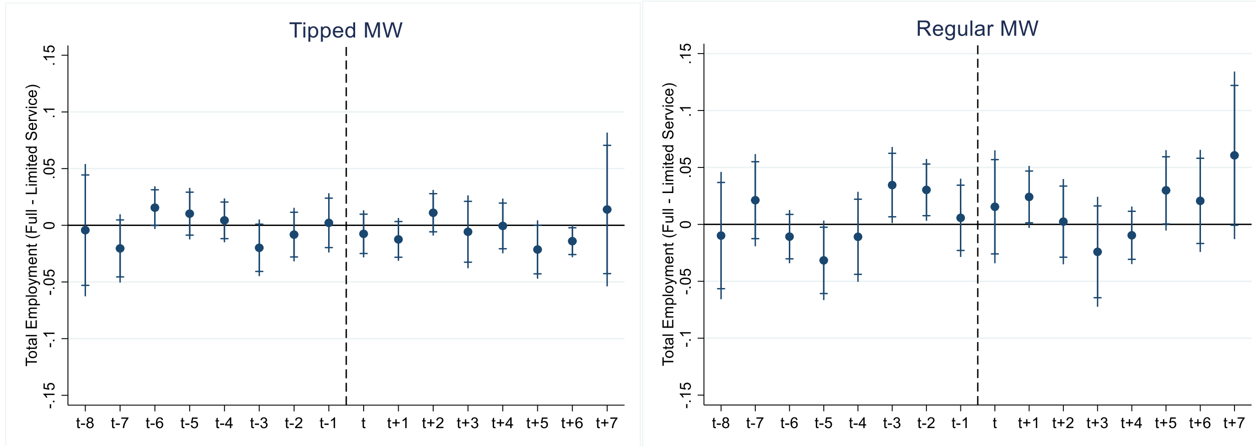


Figure 1. Federal and State Regular and Tipped Minimum Wages by State, 1990–2019

A. Log Employment, Full–Limited (Table 2, Panel A, Column (6))



B. Log Total Earnings, Full–Limited (Table 2, Panel A, Column (9))

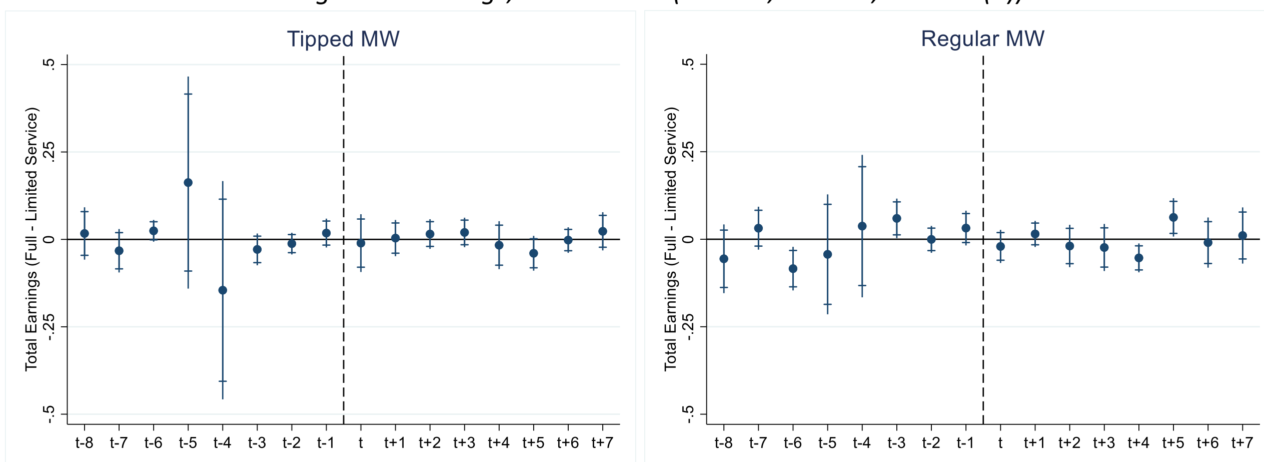
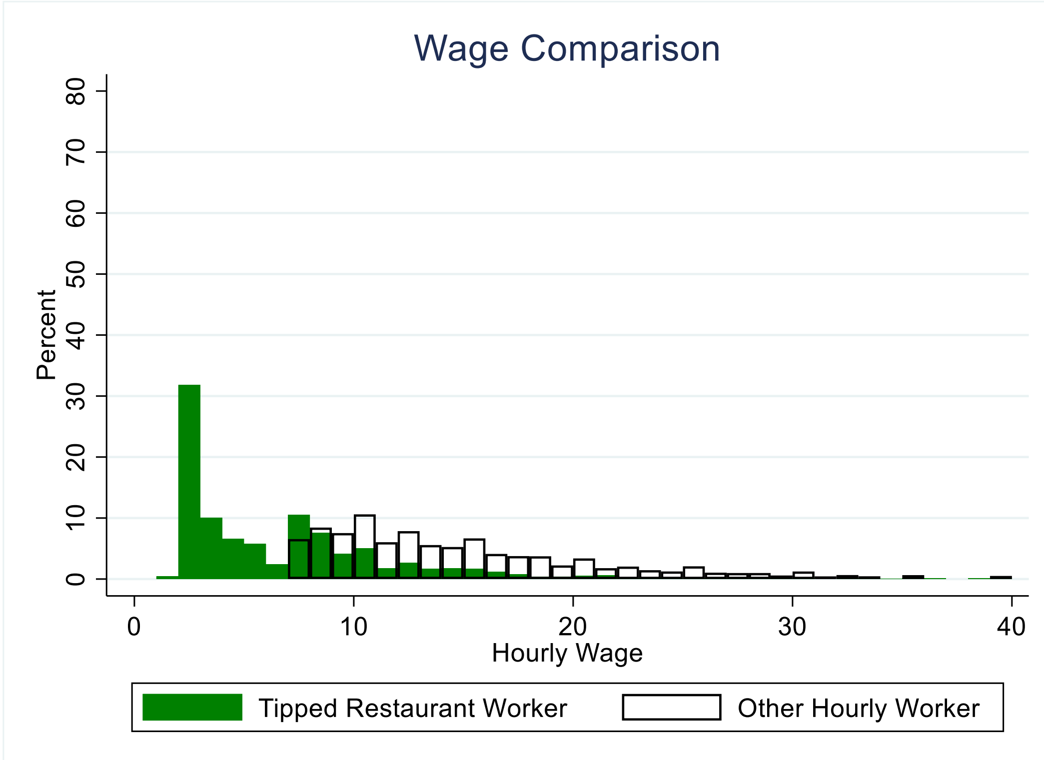


Figure 2. Leads and Lags for Estimated Effects Using QCEW Data

Notes. The figures report estimates (and 95 percent confidence intervals) for estimates from 1991:Q4–2017:Q4 of the baseline model expanded to the minimum wage variable with 8 leading quarters (t−8 to t−1) and 7 lagging quarters (t+1 to t+7). The contemporaneous and lagged coefficient estimates should be compared with Table 2 based on accumulating these estimates.



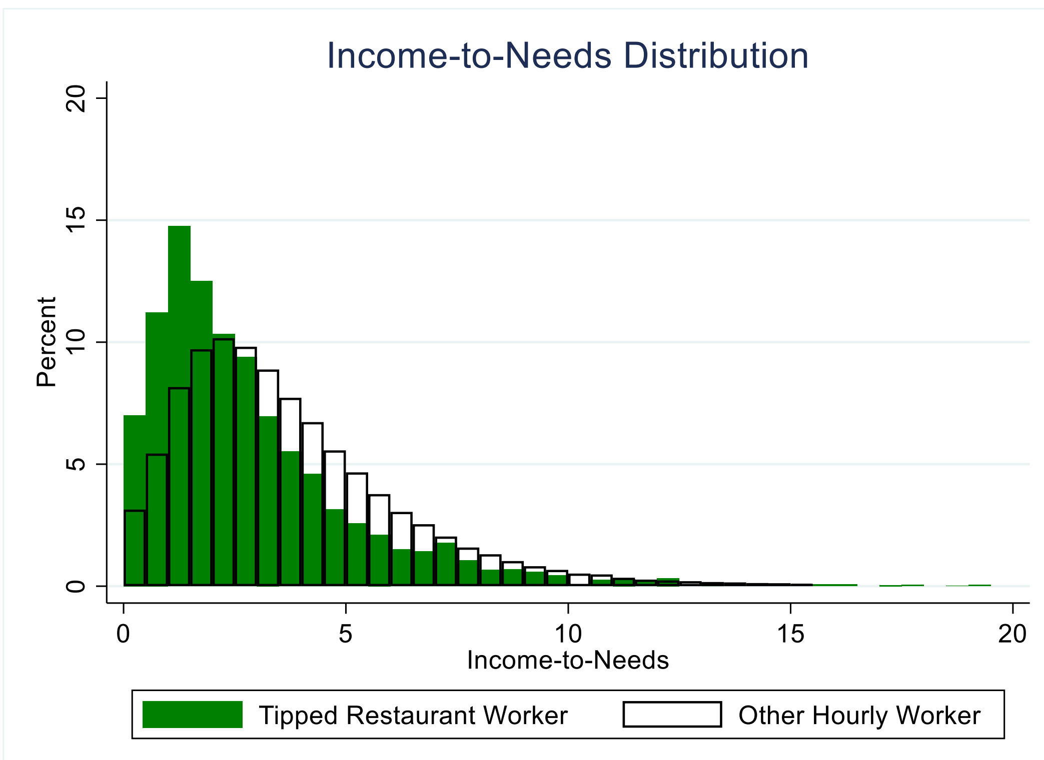


Figure 3. Wage Distributions and Family Income-to-Needs Distributions of Tipped Restaurant Workers and All Other Hourly Workers

Notes. There are 2,214 observations on tipped restaurant workers, and 144,786 observations on other hourly workers.

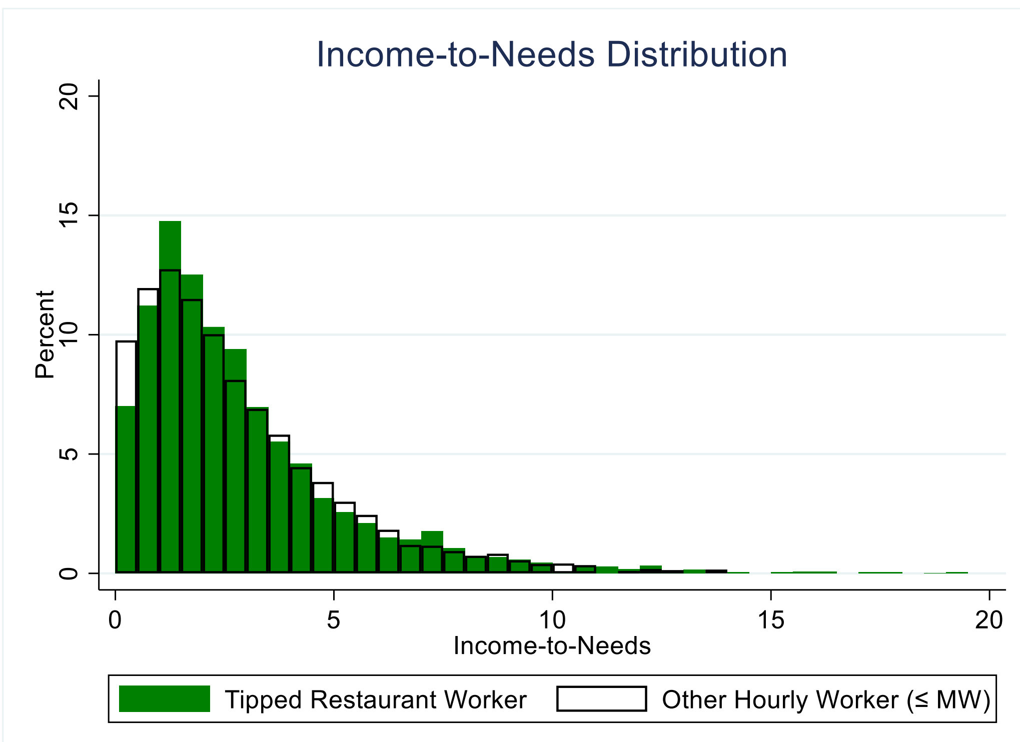
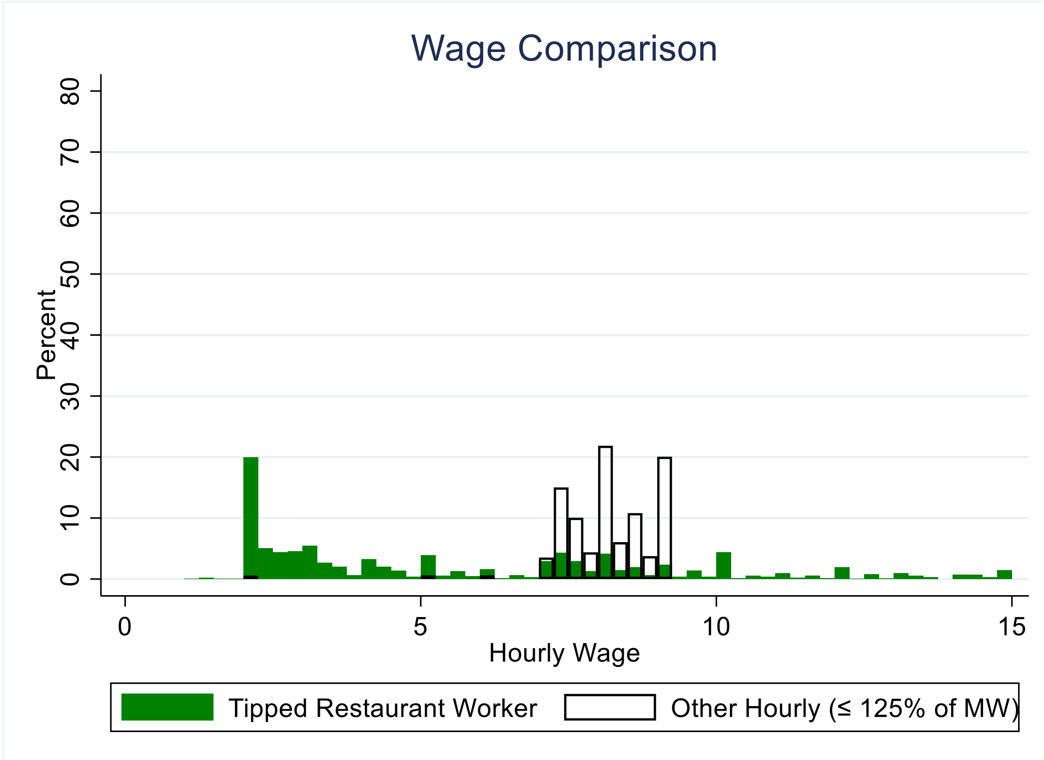


Figure 4. Wage Distributions and Family Income-to-Needs Distributions of Tipped Restaurant Workers and Other Hourly Workers Earning Less Than or Equal to the Federal Minimum Wage

Notes. There are 2,214 observations on tipped restaurant workers, and 5,434 observations on other hourly workers earning ≤ $7.25.



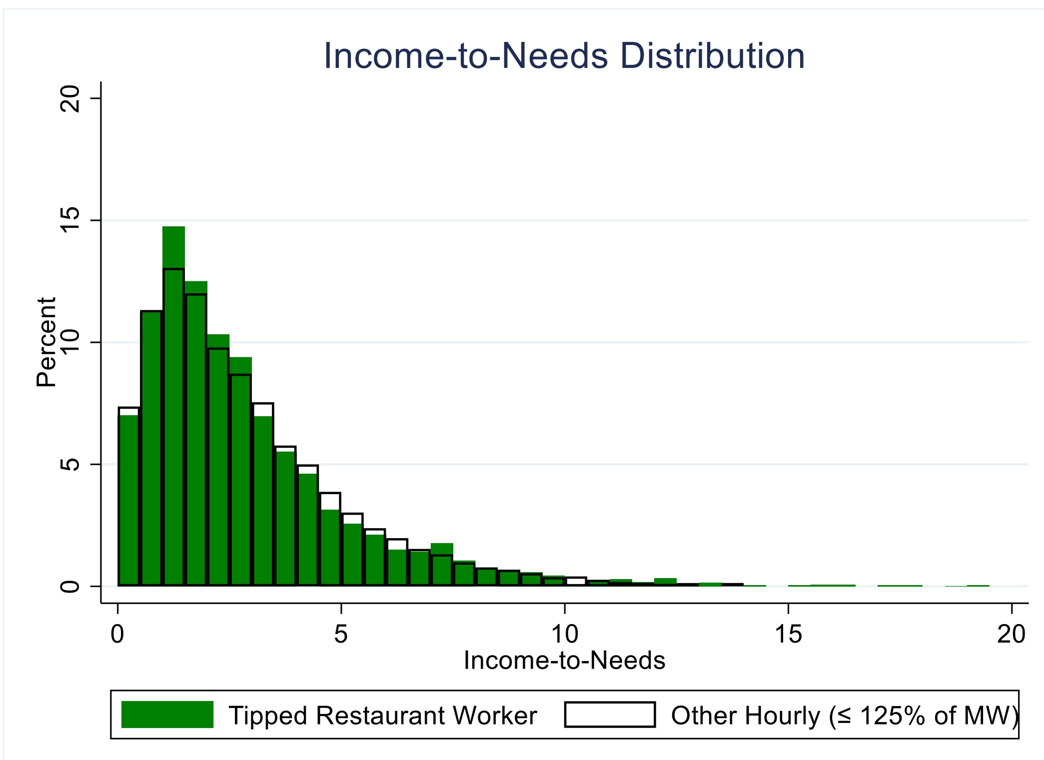


Figure 5. Wage Distributions and Family Income-to-Needs Distributions of Tipped Restaurant Workers and Other Hourly Workers Earning Less Than or Equal to 125 Percent of the Federal Minimum Wage

Notes. There are 2,214 observations on tipped restaurant workers, and 26,501 observations on other hourly workers earning ≤ $7.25 x 1.25.

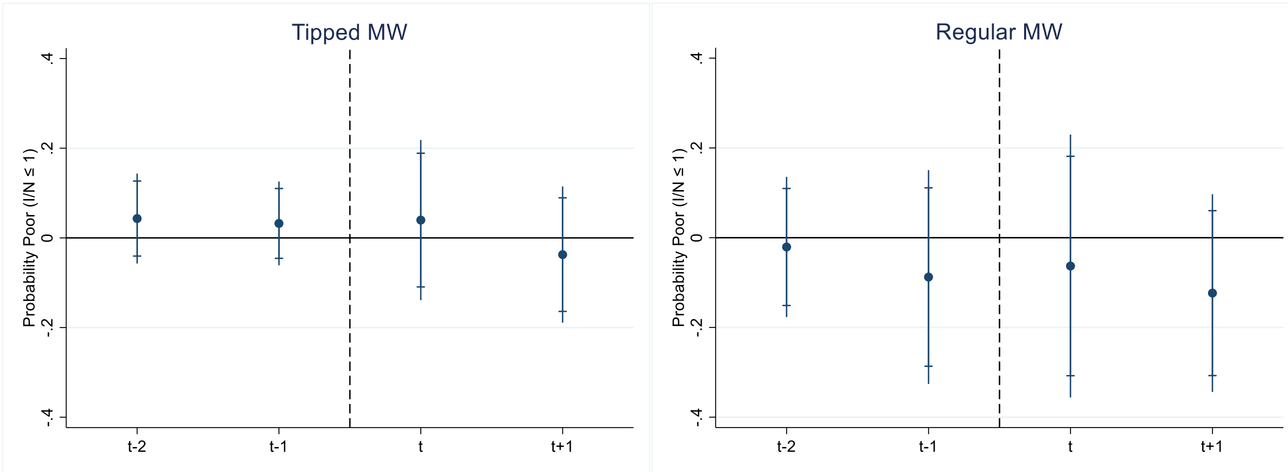


Figure 6. Leads and Lags for Estimated Effects Using CPS Data, Proportion Poor (Table 4, Panel B, Column (4))

Table 1. State minimum wages and tip credits (2019)

State	MW	Tipped MW
A. States with federal regular MW ($7.25) and federal tipped MW ($2.13) (15)		
ALABAMA, GEORGIA, INDIANA, KANSAS, KENTUCKY, LOUISIANA, MISSISSIPPI, NORTH CAROLINA, OKLAHOMA, SOUTH CAROLINA, TENNESSEE, TEXAS, UTAH, VIRGINIA, WYOMING	7.25	2.13
B. States with federal regular MW but higher tipped MW (6)		
IDAHO	7.25	3.35
IOWA	7.25	4.35
NORTH DAKOTA	7.25	4.86
NEW HAMPSHIRE	7.25	3.26
PENNSYLVANIA	7.25	2.83
WISCONSIN	7.25	2.33
C. States with MW higher than federal, federal tipped MW (2)		
NEBRASKA	9.00	2.13
NEW MEXICO	7.50	2.13
D. States with MW higher than federal, higher tipped MW than federal (21)		
ARIZONA	11.00	8.00
ARKANSAS	9.25	2.63
COLORADO	11.10	8.08
CONNECTICUT	10.33	6.38
DELAWARE	8.88	2.23
DISTRICT OF COLUMBIA	13.63	4.17
FLORIDA	8.46	5.44
HAWAII	10.10	9.35
ILLINOIS	8.25	4.95
MAINE	11.00	5.50
MARYLAND	10.10	3.63
MASSACHUSETTS	12.00	4.35
MICHIGAN	9.42	3.58
MISSOURI	8.60	4.30
NEW JERSEY	9.43	2.38
NEW YORK	11.10	7.50
OHIO	8.55	4.30
RHODE ISLAND	10.50	3.89
SOUTH DAKOTA	9.10	4.55
VERMONT	10.78	5.39
WEST VIRGINIA	8.75	2.62
E. States with MW higher than federal, no tip credit (7)		
ALASKA	9.89	9.89
CALIFORNIA	12.00	12.00
MINNESOTA	9.86	9.86
MONTANA	8.50	8.50
NEVADA	8.25	8.25
OREGON	11.00	11.00
WASHINGTON	12.00	12.00

Notes. MW is calculated as average monthly minimum wage over the year. States are in alphabetical order within each panel. There are some difference in minimum wages, in some states, based mainly on employer size but also some other features. For example, as of January 2022, California, Maryland, and New Jersey had slightly lower minimum wages for employers with 25 employees or fewer (California), fewer than 15 employees (Maryland), or fewer than 6 (New Jersey). However, New Jersey and Maryland appear to have the same tipped minimum wage regardless of employer size. Other differences include a lower minimum wage if health insurance is provided (Nevada), a lower minimum wage for secondary school students (New Mexico), and a lower minimum wage for firms below a sales threshold or for workers under age 18 (Minnesota). We do not incorporate such information since we are not using data on individual firms, nor do we know this information; and most of our analysis does not focus on specific workers. We also ignore city minimum wages (although see Neumark & Yen, 2022, for evidence on these), because the data we use are not at the city level. Regardless, with a couple of exceptions (San Francisco, Santa Fe, and Albuquerque), most city minimum wages were implemented only in the mid 2010's or later.

Table 2. QCEW estimates of effects of tipped minimum wages on earnings and employment

	Full-Service	Limited-Service	Full – Limited	Full-Service	Limited-Service	Full – Limited	Full-Service	Limited-Service	Full – Limited
	Log average weekly wages	Log average weekly wages	Log average weekly wages	Log employment	Log employment	Log employment	Log total earnings	Log total quarterly earnings	Log total quarterly earnings
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
A. 1990–2019									
Tipped MW	0.062***	0.019	0.044**	-0.064*	0.015	-0.079**	-0.004	0.031	-0.036
	[0.014]	[0.020]	[0.019]	[0.037]	[0.046]	[0.032]	[0.045]	[0.044]	[0.034]
MW	0.094***	0.244***	-0.149***	0.040	-0.075	0.115***	0.099	0.133**	-0.034
	[0.021]	[0.046]	[0.041]	[0.052]	[0.062]	[0.041]	[0.066]	[0.061]	[0.045]
B. 2010–2019									
Tipped MW	0.088***	0.079*	0.009	-0.053	0.011	-0.065	0.087*	0.170***	-0.083
	[0.031]	[0.043]	[0.044]	[0.051]	[0.034]	[0.058]	[0.052]	[0.060]	[0.065]
MW	0.110**	0.284***	-0.175***	0.071	-0.009	0.080	0.132*	0.208**	-0.075
	[0.043]	[0.061]	[0.059]	[0.051]	[0.053]	[0.082]	[0.067]	[0.097]	[0.100]

Notes. The dependent variables and minimum wage variables are measured in logs. The models also include state and quarter fixed effects. Estimates are weighted by state private-sector employment. Columns (1)–(3) include controls for log average private-sector weekly wages. Columns (4)–(6) include controls for log private-sector employment. Columns (7)–(9) include controls for log total private-sector quarterly earnings by state. The constants are not reported. There are 6,120 observations in Panel A and 2,040 observations in Panel B. Standard errors are clustered at the state level. *** $p < .01$; ** $p < .05$; * $p < .1$.

Table 3. Simulated distributional effects of alternative minimum wage policy changes

Family Income to Poverty Ratio	Total Benefits (Cumulative, 2010–2019)	Total Benefits (%)	Average Beneficiaries per Year	Avg. Benefits/ Person	Average Hours/ Person
A. Eliminate Tip Credit					
(0, 0.5)	$642,454,942	3.5%	21,115	$3,043	26.6
(0.5, 1.0)	$2,062,930,398	11.3%	35,758	$5,769	29.4
(1.0–1.5)	$3,318,946,128	18.1%	53,840	$6,164	31.8
(1.5–2.0)	$2,636,861,802	14.4%	44,609	$5,911	30.8
(2.0–3.0)	$3,953,457,204	21.6%	68,491	$5,772	31.6
3.0 or higher	$5,689,558,615	31.1%	112,433	$5,060	28.7
Total	$18,304,209,089	100.0%	336,247	$5,444	30.0
B. Increase MW to $8.15, Preserve Tipped MW					
(0, 0.5)	$906,463,199	4.9%	204,770	$443	27.6
(0.5, 1.0)	$2,490,347,844	13.5%	391,885	$635	29.5
(1.0–1.5)	$2,903,779,672	15.7%	439,925	$660	31.0
(1.5–2.0)	$2,398,051,618	13.0%	385,060	$623	29.7
(2.0–3.0)	$3,534,259,358	19.2%	586,717	$602	28.3
3.0 or higher	$6,207,848,747	33.7%	1,204,473	$515	26.0
Total	$18,440,750,438	100.0%	3,212,830	$574	28.1

Notes. This table shows the results of static simulations that apply the minimum wage policy changes indicated to all affected workers, and assume no other behavioral responses. We compute the share of the increased wage bill going to workers in different regions of the distribution of family income to needs (the poverty line). See the text for additional details.

Table 4. Estimated distributional effects of changes in tipped and regular minimum wages

	Age 16+	Age 16+	Age 16+	Age 16+
1990–2019	(1)	(2)	(3)	(4)
A. Proportion extremely poor (I/N ≤ .5)				
Tipped MW	0.001	0.000	0.064	0.104*
	[0.001]	[0.001]	[0.048]	[0.058]
MW	-0.001*	-0.001*	-0.264**	-0.297***
	[0.001]	[0.001]	[0.099]	[0.103]
B. Proportion poor (I/N ≤ 1)				
Tipped MW	0.001	-0.001	0.069**	0.053
	[0.001]	[0.002]	[0.030]	[0.042]
MW	-0.002	-0.003	-0.181*	-0.220**
	[0.001]	[0.001]	[0.092]	[0.094]
C. Proportion nearly poor (I/N ≤ 1.5)				
Tipped MW	0.002**	0.001	0.065***	0.058*
	[0.001]	[0.002]	[0.020]	[0.031]
MW	-0.002	-0.004**	-0.094*	-0.141**
	[0.002]	[0.002]	[0.051]	[0.056]
2010–2019				
D. Proportion extremely poor (I/N ≤ .5)				
Tipped MW	-0.003***	-0.001	-0.365***	-0.144
	[0.001]	[0.001]	[0.121]	[0.103]
MW	0.001	0.003**	0.195	0.467**
	[0.001]	[0.001]	[0.182]	[0.196]
E. Proportion poor (I/N ≤ 1)				
Tipped MW	-0.004*	-0.003*	-0.159	-0.103
	[0.002]	[0.002]	[0.104]	[0.092]
MW	0.001	-0.000	0.034	-0.084
	[0.002]	[0.002]	[0.140]	[0.125]
F. Proportion nearly poor (I/N ≤ 1.5)				
Tipped MW	-0.004	-0.004*	-0.103	-0.067
	[0.003]	[0.002]	[0.073]	[0.069]
MW	0.002	-0.001	0.034	-0.099
	[0.003]	[0.003]	[0.101]	[0.094]
For all panels				
Levels or logs	Levels	Levels	Logs	Logs
State-specific trends	No	Yes	No	Yes

Notes. The dependent variables and minimum wage variables are measured in either levels or logs when specified in the table. We use a one-year lag for the March minimum wage, because poverty is measured from family income in the past twelve months. The model includes the following controls: State unemployment rate for 25–69 year-olds, log state GDP, log state GDP x 1997 flag, EITC (using the percent supplement to the federal EITC for 0, 1, 2, and 3 or more children), share married, share female, share high school degree, share bachelor's degree, and share master's degree or higher (or household head), average age, average age^2, share Black, share nonwhite, share Hispanic, average family size, and average number of children. Estimates are weighted by state population. *** Standard errors are clustered by state. There are 1,530 observations for 1990–2019 and 510 observations for 2010–2019. *** $p < .01$; ** $p < .05$; * $p < .1$.