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

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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, that 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%. Many states with higher minimum wages have tip credits, whereas seven do not.

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 how these compare to the federal policy. For example, the entries for Alabama show the federal regular and tipped minimum wages, and the number one (for "Yes") for every other entry indicates that the federal policy binds on all dimensions. In contrast, in Wisconsin the federal regular minimum wage binds, but the tip credit is a bit higher ($2.33), so the last two entries are coded as zeros (for "No"). At the other extreme, in California the state minimum wage in 2019 was $12 and there is no tip credit.

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 poverty, extreme poverty, and near-poverty).[2]

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 did not reduce low-skilled employment (Azar et al., 2019; Munguía Corella, 2020). The monopsony model 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. In this model, 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, with a 50% 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

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[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, etc. 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.

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 and Macpherson (2014) estimate a positive earnings effect and a negative employment effect.

Our analysis of employment (and earnings) effects is closest to Even and 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. We find evidence of negative effects on employment, with employment elasticities centered around −.1. Reflecting the fact that the negative employment elasticity exceeds the positive elasticity for earnings per worker, we find that the effect on total earnings is negative (although often 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 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 first compare 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 also compare the potential redistributive effects of eliminating the tip credit in the federal minimum wage to a broad increase in the federal minimum wage that raises the wage bill by the same amount while leaving the tip credit intact.

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 in the income-to-needs distribution than are other low-wage workers in those same states, or perhaps a bit higher, depending on how we define other low-wage workers. Reflecting this, a simulated policy change of eliminating the tip credit in the federal minimum wage, thereby raising the tipped minimum wage to the federal minimum wage, delivers less income to poor and other low-income families when compared to a general minimum wage increase that raises the wage bill (and hence earnings of low-wage workers) by the same amount while preserving the tipped minimum wage; the latter policy has the added advantage of raising earnings for more workers.

The second component is an analysis of the impact of tipped minimum wage variation on the probability of being 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 unlikely 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 a number of data sources. Our data on tipped minimum wages come from a dataset provided by William Even and 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 in estimating the effects on the full-service sector.

For the first part of our distributional analysis where we present descriptive evidence comparing restaurant workers with other low-wage workers, and simulate the distributional effects of alternative minimum wage policy changes, we use Current Population Survey (CPS data) that combines 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] while the latter provides the income information needed to determine the income-to-needs ratio of the worker's family.[5] We use all data we can match between the March ASEC and March-June ORG files.[6] We pool all years from 2010 through 2019. We use hourly wage data reported directly in the CPS ORG files, whenever possible, to measure the base rate of pay – and the cash wage for tipped workers.[7] Because hourly wages may be estimated

[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 isolated tipped 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).

[6] Some CPS respondents in the April-June files can be matched when they are in the outgoing rotation group in that month.

[7] The survey question asks about the hourly rate of pay on the main job excluding overtime, tips, and commissions.

poorly in some cases, for our analysis we focus only on those reporting hourly wages.[8] We use family income from the ASEC files and construct the income-to-needs ratio from the reported poverty threshold for the family.

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.[9] 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, and 3 or more children.[10] Data on this EITC policy variation comes from the Tax Policy Center.[11] 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

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[8] We do not report analyses of earnings or employment using tipped workers identified from 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 our distributional analyses where we pool across many years (to provide descriptive evidence).

[9] 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).

[10] 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.

[11] 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: https://www2.illinois.gov/rev/programs/EIC/Pages/default.aspx; https://www2.illinois.gov/rev/programs/EIC/Pages/default.aspx; https://www.taxcreditsforworkersandfamilies.org/state/maine/#:~:text=Latest%20Legislative%20Action%3A%20In%20June,eligibility%20from%2025%20to%2018; https://taxnews.ey.com/news/2018-1373-new-jersey-law-raises-2018-income-tax-and-withholding-rates; https://www.taxcreditsforworkersandfamilies.org/state/ohio/#:~:text=Latest%20Legislative%20Action%3A,the%20state's%202019%20transportation%20budget; https://www.taxcreditsforworkersandfamilies.org/tcwf-news/rhode-island-eitc-increases-to-12-5/; and https://dor.sc.gov/communications/sc-earned-income-tax-credit-increases-in-2020.

of children;[12] 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, as depicted in Figure 1. Over this period, the averages of both state minimum wages and state tipped minimum wages (the cash minimum wage required for tipped workers) have both been increasing, as shown in Figure 2. Figure 3 provides a more 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.

Our goal is to estimate the effects of increases in the tipped minimum wage (or alternatively, decreases in the tip credit) on earnings and employment of tipped restaurant workers, in the QCEW data. Our dependent variables include average weekly wages,[13] total earnings, and employment. Our analysis of the QCEW differs from Even and Macpherson (2014), 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.[14]

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

[12] These are constructed using the same definitions of families used to generate the income-to-needs ratio, as discussed above.

[13] This is average weekly earnings, but the QCEW documentation refers to wages.

[14] 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 et al. (2010).

affect them indirectly. 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 either controlling for the regular minimum wage (and, in some analysis reported in Appendix A, using treatments and controls where there is no variation in regular minimum wages).

Second, we want to isolate the effect of variation in the tipped federal 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 et al., 2011 and 2017; Baskaya and Rubinstein, 2015; Clemens and Wither, 2019; Dube et al., 2010; Liu et al., 2016; Neumark et al., 2014a and 2014b; and Neumark and 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 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 et al., 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.[15] For this analysis, define:

E_{jst} = log employment (or earnings) in sector (full/limited) j, state s, period t

F_j = full-service sector dummy, j = F, L

D_s = state dummies

[15] This analysis follows fairly closely Even and Macpherson (2014), but extends the analysis to more recent data.

D_t = quarter dummies

MW_{st} = log minimum wage

TMW_{st} = log tipped minimum wage

We then estimate, for each sector j, the regression:

(1) $E^j_{st} = \alpha^j + \beta^j \cdot TMW_{st} + \gamma^j \cdot MW_{st} + D_s\lambda^j + D_t\theta^j + \varepsilon^j_{st}$.[16]

We could interpret these as simply standard MW-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 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

[16] 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 and Yen, 2021). 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.

as the relative effect of the tipped minimum wage on the full-service sector. Or, if we are willing to assume $\beta^L = 0$, 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 et al. (2011), although we would argue that the approach is far more defensible in this case.[17]

The results are reported in Table 2. Panels A and B report weighted results, and Panels C and D unweighted results (as a robustness check). 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). 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.

For reasons explained above, the estimates in column (3), for the relative effect on the full-service sector, are more defensible 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,

[17] See the critique in Neumark et al. (2014a) of using this in the context of Census division-by-period interactions used by Allegretto et al.

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

Columns (4)-(6) present estimates for total quarterly earnings, which will also reflect employment changes. For the full sample period, we do not find an effect on earnings in the full-service sector, although we do (at the 10% 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 employment declines lower total earnings relative to average weekly wages (the latter are estimated over workers).

Columns (7)-(9) 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 (9) 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% level), and the recent-period estimates in Panel B imply an elasticity of −.07 (not statistically significant).

The estimates in Panels C and D of Table 2 are for the unweighted data, so that every state (by quarter) observation gets equal weight. Qualitatively, the findings are very similar. It is useful to focus on the relative estimates in columns (3), (6), and (9), which are the best causal estimates. In column (3), we find a positive and significant effect on the average weekly wages of workers in the full-service sector, with a small elasticity of .04, but no significant impact in the more recent data. Column (6), in contrast, points to an earnings decline in the more recent data. And column (9) provides stronger evidence that tipped minimum wages reduce relative employment in the full-service sector.[18,19]

Thus, the evidence is most consistent with adverse employment effects from raising the tipped minimum wage, with employment elasticities centered around −.1. Using the weighted data for the more recent period, the elasticity is smaller (−.07) and not significant; but the other three estimates are statistically significant, and range as large as −.13. Perhaps more surprisingly, the evidence does not point to strong positive effects on average weekly wages. Only the estimates for the full period (whether weighted or not) are much different from zero and statistically significant, but the elasticities are small (.04). And the estimated effects on total earnings are negative (albeit significant, at the 10% level, only in Panel D). We suspect that the weak effects on average earnings effects reflect hours declines, although it is possible that tips decline. And we have evidence that the negative total earnings effects reflect employment declines.

In the minimum wage literature, it is increasingly common to report the "own-wage" elasticity, defined as the ratio of the elasticity of employment with respect to the minimum wage to the elasticity of wages with respect to the minimum wage. The interpretation of this ratio is that if it is less than 1 (in absolute value, because the numerator is typically negative), then on net earnings are increased by the minimum wage increase, despite some job loss. Here, for tipped minimum wages the ratio of the

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[18] The estimates are often qualitatively similar to those in Even and Macpherson (2014). 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. 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% 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), there is no evidence of a positive average weekly wages effect.

[19] 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).

employment elasticity to the average wage elasticity is well *above* 1 in absolute value in every case (and there is certainly no evidence that it is less than 1); compare the estimates for the effect of the tipped minimum wage in columns (9) and (3), in each panel. This evidence is consistent with the negative (although generally insignificant) effects on total earnings, which can reflect employment declines as well.[20]

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%.[21] This seems to be a relatively minor adjustment. 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.

Distributional Analysis

Our distributional analysis proceeds in two part. 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 other metrics of the distribution of income.

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[20] 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 A. Overall, the synthetic control analysis points to increases in average weekly earnings in the full-service sector as a result of increases in the tipped minimum wage. The employment estimates tend to point to job loss, although only one estimate is statistically significant. Using the point estimates to compare the elasticities implied by the earnings and employment estimates, we find roughly offsetting elasticities, implying, again, that there is little evidence that raising tipped minimum wages on net benefits tipped workers in the full-service restaurant sector. In sum, then, we find the synthetic control analyses broadly consistent with the panel data analyses, although the synthetic control analysis is less informative statistically, likely as a consequence of considering only a single tipped minimum wage change in one state.

[21] See NIPA Handbook, December 2020, Chapter 10, Table 10.2, https://www.bea.gov/resources/methodologies/nipa-handbook).

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 all years from 2010 through 2019, we obtain the numbers of observations for tipped restaurant workers and all other workers displayed in Panel A of Table 3. The sample sizes for hourly workers are reported in Panel B of Table 3.

We begin by showing information on the distributions of hourly wages for tipped restaurant workers (measured without tips) and other hourly workers, for which the sample sizes are reported in Panel B of Table 3. The histograms for wages are shown in Figure 4A – in the top panel with more detail, with a maximum wage of $15, and in the bottom panel with less detail, with a maximum wage of $50.[22] 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.[23] Both

[22] 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 would likely not materially affect the key comparison we do between two alternative minimum wage policies (which we verified).

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

figures show, as we would expect, lower wages for tipped restaurant workers.[24]

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

Next, 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 Figure 4B; again, we show a figure focused on the lower end of the distribution followed by a more comprehensive one. In Figure 4B, 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 4B 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.

We therefore next compare tipped workers to workers with wages at or below the federal minimum wage, which yields the sample sizes in Panel C of Table 3.[25] As the table shows, the number of "comparison" other hourly workers drops substantially, from about 145,000 to about 5,400.

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[24] 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 4A.

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

The histograms for hourly wages are shown in Figure 5A. We now show the data only up to $15, since the sample is restricted to low-wage other hourly workers, and, as Figure 4A showed, there are relatively few restaurant workers with higher hourly wages. Figure 5A 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.

Again, we next compare the distributions of family income-to-needs for these two groups of workers. These are reported in Figure 5B; as above, we show a figure focused on the lower end of the distribution followed by a more comprehensive one. The evidence in Figure 5B differs from that in Figure 4B. 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, 18.1% of tipped restaurant workers are classified as poor, compared to 21.7% of other hourly workers who earn the federal minimum or less, a 3.6 percentage point difference. In addition, 6.9% of tipped restaurant workers are classified as extremely poor compared to 9.7% for the comparison group, a 2.8 percentage point difference.

However, 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% of the federal minimum wage, which yields the sample sizes in Panel D of Table 3. As the table shows, the number of "comparison" other hourly workers increases about five-fold, to over 26,500.

The histograms for wages are shown in Figure 6A. We again show the data only up to $15, since the sample is restricted to low-wage other hourly workers, and, as Figure 4A showed, there are relatively few restaurant workers with higher hourly wages. Figure 6A differs from Figure 5A in including observations on other hourly workers earning above $7.25.

We next, as before, compare the distributions of family income-to-needs for these two groups of workers. These are reported in Figure 6B. The evidence in Figure 6B 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, 18.1% of tipped restaurant workers are poor, vs. 18.6% of other low-wage workers up to 125% of the federal minimum wage.[26] What this evidence indicates, in comparison to Figure 5B, is that tipped restaurant workers are fairly low-wage, but are more comparable to workers earning up to 125% of the minimum wage than to minimum wage workers.[27]

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. 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 other workers' wages), the change in minimum wage policy

[26] The extreme poor percentage is comparable, with 6.9% of tipped restaurant workers are extremely poor compared to 7.3% of other hourly workers who make 125% of the MW or less.

[27] We also did calculations similar to the last two, 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. Comparing the resulting figure for minimum wage workers to Figure 5B, we still found that minimum wage workers are more likely to have the lowest income-to-needs. And the figure corresponding to Figure 6B looks very similar, because in this case the sample of other workers does not expand that much when we add those for whom we compute hourly wages. Curiously, the sample of comparison workers increased more for the lower-wage cutoff (corresponding to Figure 5B). Given that we would expect higher-earning workers to be less likely to be paid by the hour, this suggests that some of the lowest computed wages are erroneous.

In addition, we redid the analyses in Figures 4A-6B dropping observations with imputed earnings (as discussed, e.g., in Hirsch and Shumacher, 2004). This has virtually no impact in Figures 4A-5B. In Figure 6B, for the comparison with other hourly workers earning less than or equal to 125% of the federal minimum wage, it resulted in slightly stronger evidence that tipped restaurant workers were a bit higher in the income-to-needs distribution (results available upon request). Note, however, that there is not a flag for imputed family earnings in the IPUMS data we use, so we cannot treat the two types of earnings symmetrically.

generates an overall change in the total wages – or wage bill – paid to workers, which we then divide up based on distribution of this increased wage bill to those in different parts of the family income-to-needs distribution.

What we do differently from the research literature is that we directly compare a policy of eliminating the federal minimum wage tip credit to an equivalent policy that 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 part 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. 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.[28] We use the ORG earnings weight to calculate total benefits (i.e., the total wage bill increase). These earnings weights in each month are intended to make the sample representative of the U.S. population. But since we use four monthly ORG files we divide these weights by 4.[29]

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 is $16.676 billion.

We then do an alternative calculation where we maintain the tipped minimum wages as they

[28] 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.
[29] 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.

are, but raise minimum wages for all non-tipped restaurant workers who are paid $7 or more.[30,31] We find that an increase in the minimum wage from $7.25 to $8.14 – using data on hours and weeks in the same way – delivers the same approximate $16.676 billion increase in the wage bill.[32]

Finally, we compare the distribution of these two ways of increasing the wage bill across ranges of the distribution of family income-to-needs. The results are reported in Table 4. 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% from the general minimum wage increase, compared to 3.5% for the elimination of the tip credit. Similarly, the total percentage going to those in poor families is 18.4% for the general minimum wage increase, vs. 14.7% 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.

Thus, for the same overall increase in labor costs and assuming no employment effects of 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

This analysis 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

[30] 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.
[31] 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.
[32] We arrive at the $8.14 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.

we do (partially following Dube, 2019), consider the effects of including state-specific linear time trends.[33] We also incorporate the other controls he incorporates; these controls were discussed above.

In Tables 5A and 5B we report estimates for regressions of the proportion of individuals in families that are in poverty, in extreme poverty, or in near poverty. We also report results for all individuals aged 16+, and for those of "working age" (through age 70). The results are weighted in Table 5A, but not 5B.

Looking first at Table 5A, 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 poverty, lower extreme poverty, or lower near poverty. Nearly every estimate is positive, with two exceptions out of a total of 24 estimates, and in those two cases (Panel A, columns (2) and (6)) the estimates are essentially zero. In fact, several of these positive estimates are significant for both the level and log specifications, and with or without state-specific trends. In the unweighted estimates, in Table 5B, the estimates are quite similar to the weighted estimates (*all* are positive), serving as a robustness check. 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

[33] In our view, these are problematic, for reasons discussed in, for example, Meer and 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 et al. (2014a).

statistical conclusions.[34] 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 with elasticities ranging to as large as –.33 (but more commonly in the −.1 to −.2 range).

For the recent period, from 2010 – 2019, however, the results are different, and quite fragile with respect to both weighting and specification. The estimates in Table 5A always point to reductions in the incidence of poverty or low income from higher tipped minimum wages, with negative estimates. For the probability 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 some large elasticities (e.g., −.37 in column (3)). However, the corresponding unweighted estimates in Table 5B are closer to zero, vary in sign, and are never significant. Moreover, in the recent period, neither the weighted nor the unweighted estimates indicate any statistically significant evidence that regular minimum wages reduce the incidence of low income, and the weighted estimates with state linear trends point to increases in extreme poverty.

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, discarding both the weighted and unweighted estimates from the longer sample period, and the unweighted estimates for the more recent data. Moreover, if one wanted to embrace the recent evidence 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 probability of being poor or

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[34] For papers finding no significant relationship, see, e.g., Neumark and Wascher (2001), Sabia and Burkhauser (2010), and Sabia and Nielsen (2015). Neumark (2016) finds that a variety of estimates point to poverty reductions, but the relationship is not statistically significant. Neumark et al. (2004) provide some evidence of adverse distributional effects. Dube (2019) reports strong poverty reduction effects, and Addison and Blackburn (1999) and DeFina (2008) find poverty reduction effects, but only for subgroups (very narrow in the case of Addison and Blackburn – teenagers and junior high school dropouts).

near-poor.

Conclusions and Discussion

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 (the share of workers in poverty, extreme poverty, and near-poverty).

Our evidence on employment and earnings is most consistent with adverse employment effects from raising the tipped minimum wage, with employment elasticities centered around −.1. Moreover, the evidence does not point to strong positive effects on average weekly wages, and the estimated effects on total earnings are negative (albeit generally not statistically significant). These results are quite robust to the alternative analyses we do.

With regard to effects on the incidence of poverty, extreme 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 with respect to weighting, and interestingly the same – and quite isolated – 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 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. Using this evidence, there is also no conflict or ambiguity regarding weighted vs. unweighted data. 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 and 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 different conclusions using the weighted or unweighted data.

The unweighted data can be thought of as representative of states, answering "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 "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. However, the answer to the second question, based on the weighted data, is driven relatively more by a small number of states and hence the beneficial effects suggested by the more recent, and weighted, data could provide misleading evidence on the effects of a change in federal minimum wage policy.

Only a carefully "curated" selection of the results from this paper – 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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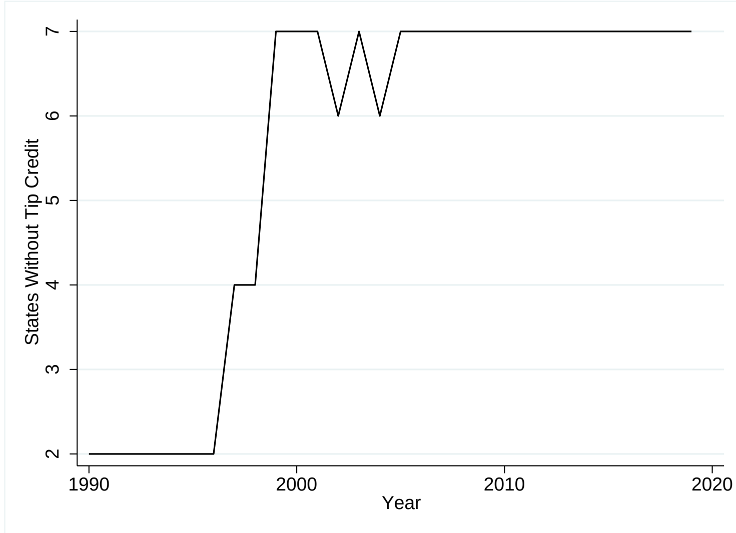
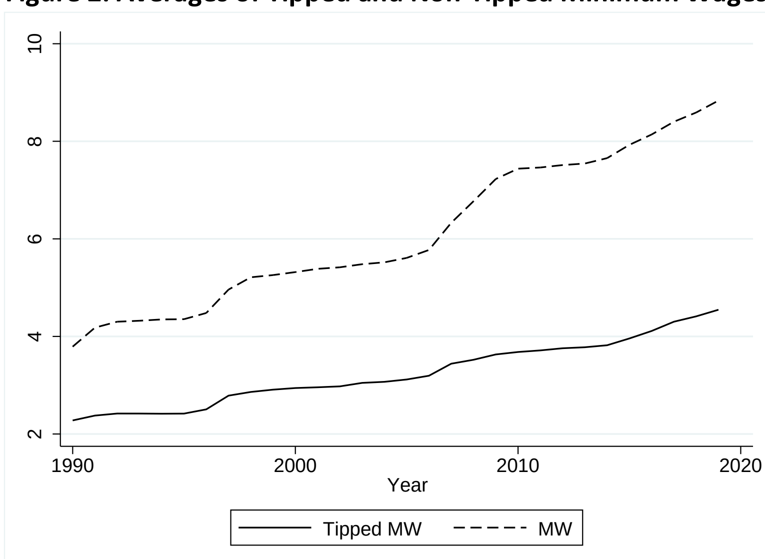
Figure 1: Number of States with No Tip Credit



Note: Washington State changes back and forth between 2002 and 2005.

Figure 2: Averages of Tipped and Non-Tipped Minimum Wages



Note: Washington State changes back and forth between 2002 and 2005.

Figure 3: Federal and State Regular and Tipped Minimum Wages by State, 1990-2019

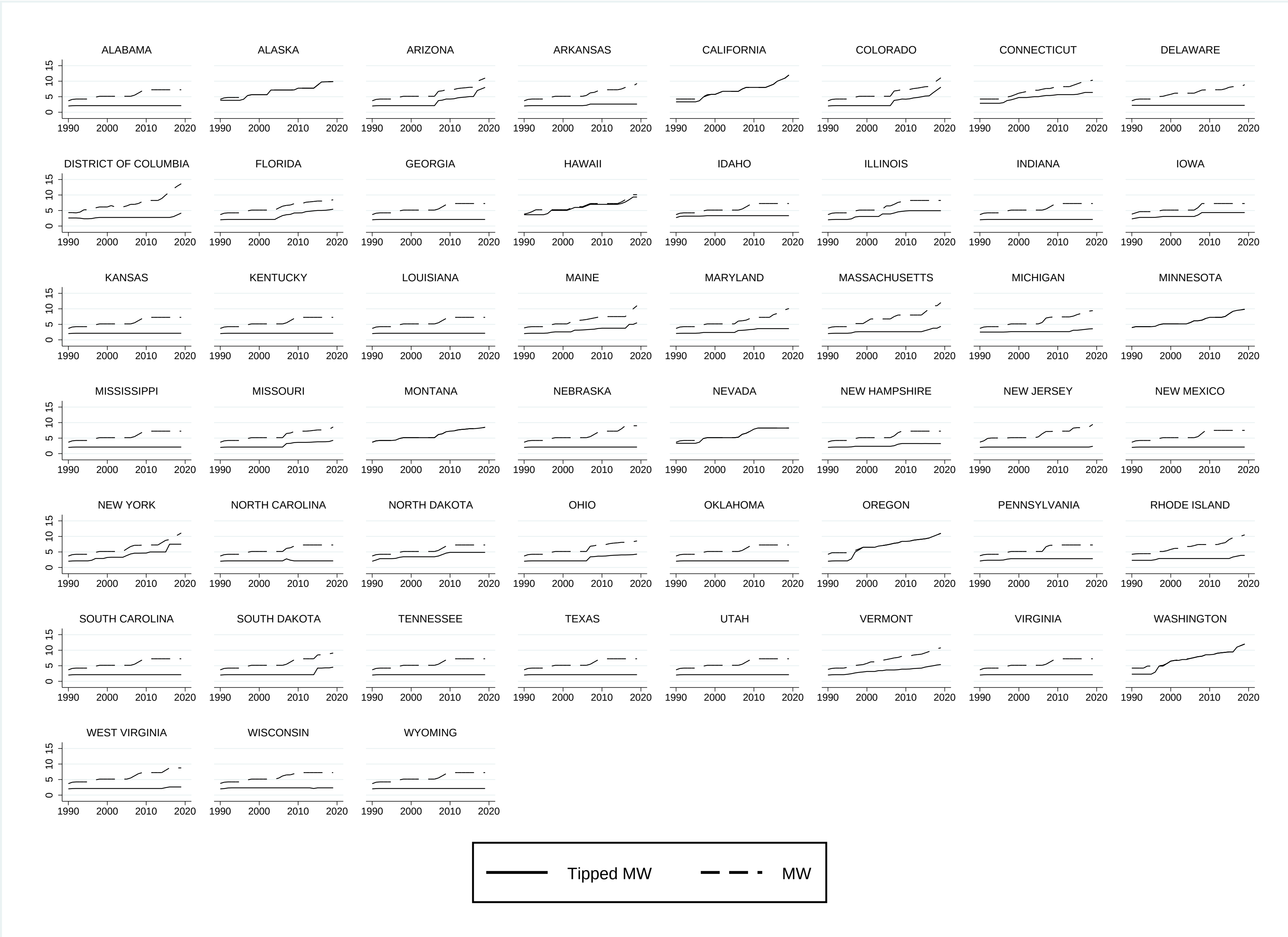
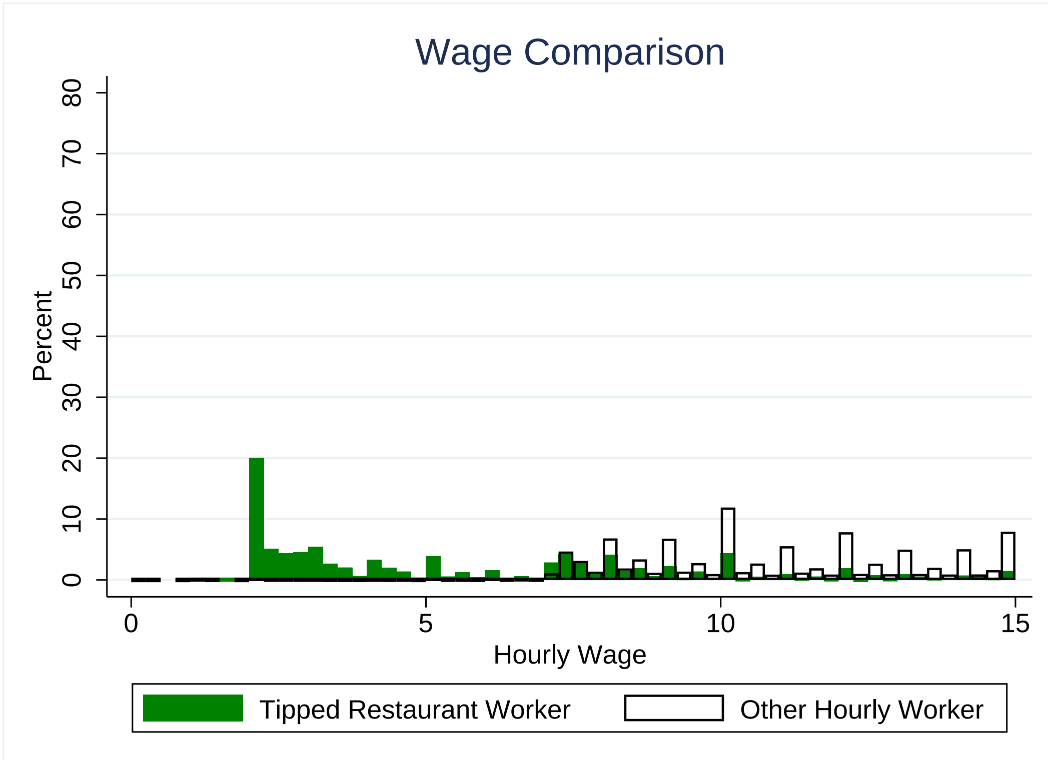


Figure 4A: Wage Distributions of Tipped Restaurant Workers and All Other Hourly Workers



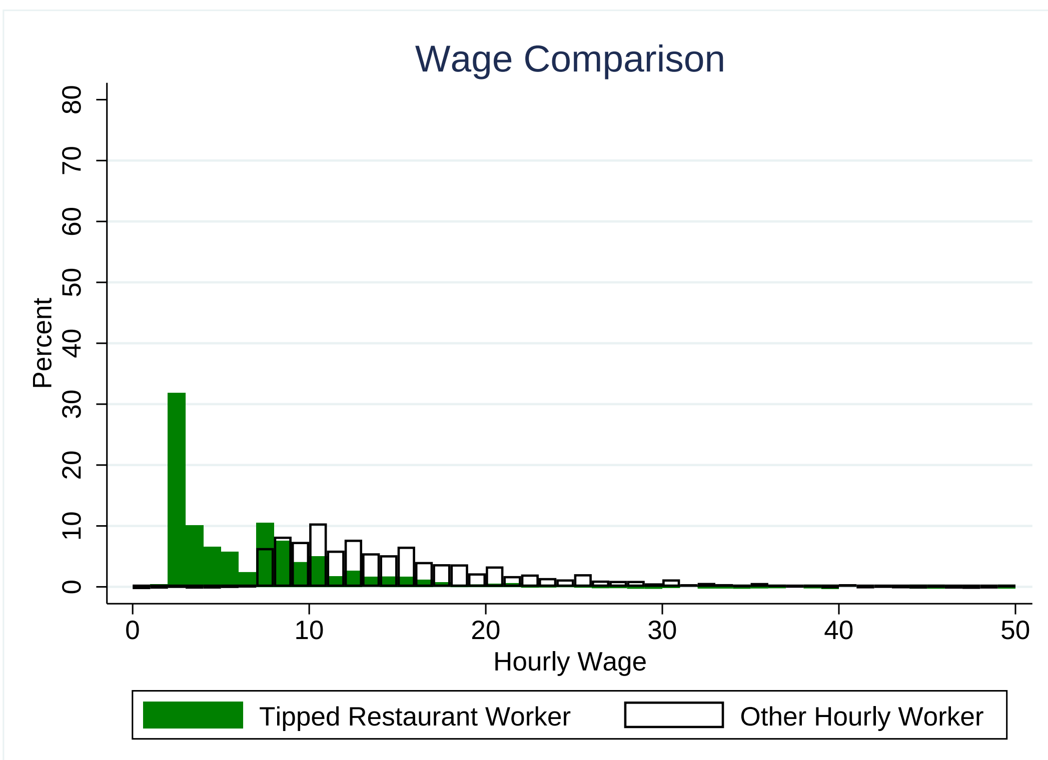


Figure 4B: Family Income-to-Needs Distributions of Tipped Restaurant Workers and All Other Hourly Workers

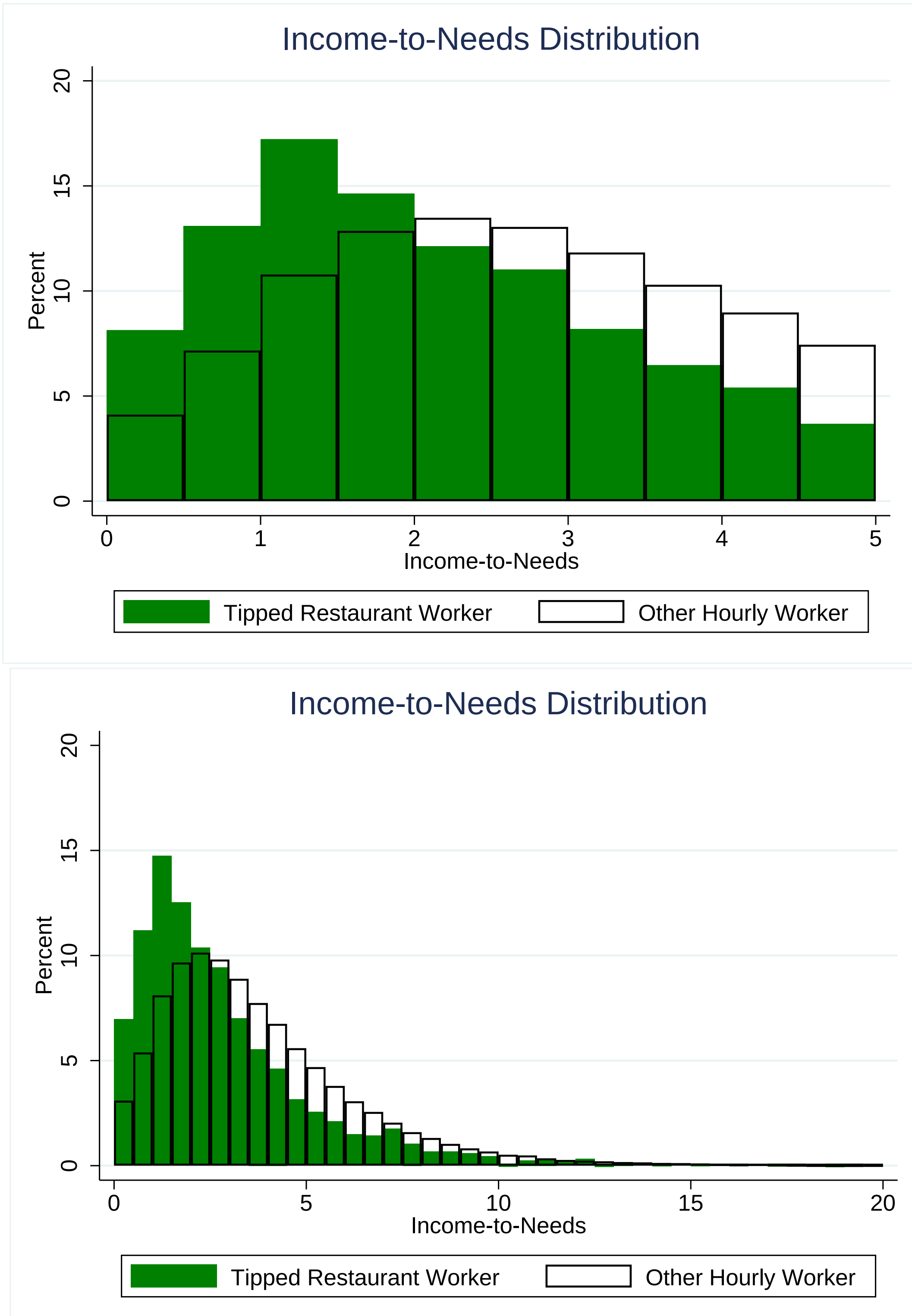


Figure 5A: Wage Distributions of Tipped Restaurant Workers and Other Hourly Workers Earning Less Than or Equal to the Federal Minimum Wage

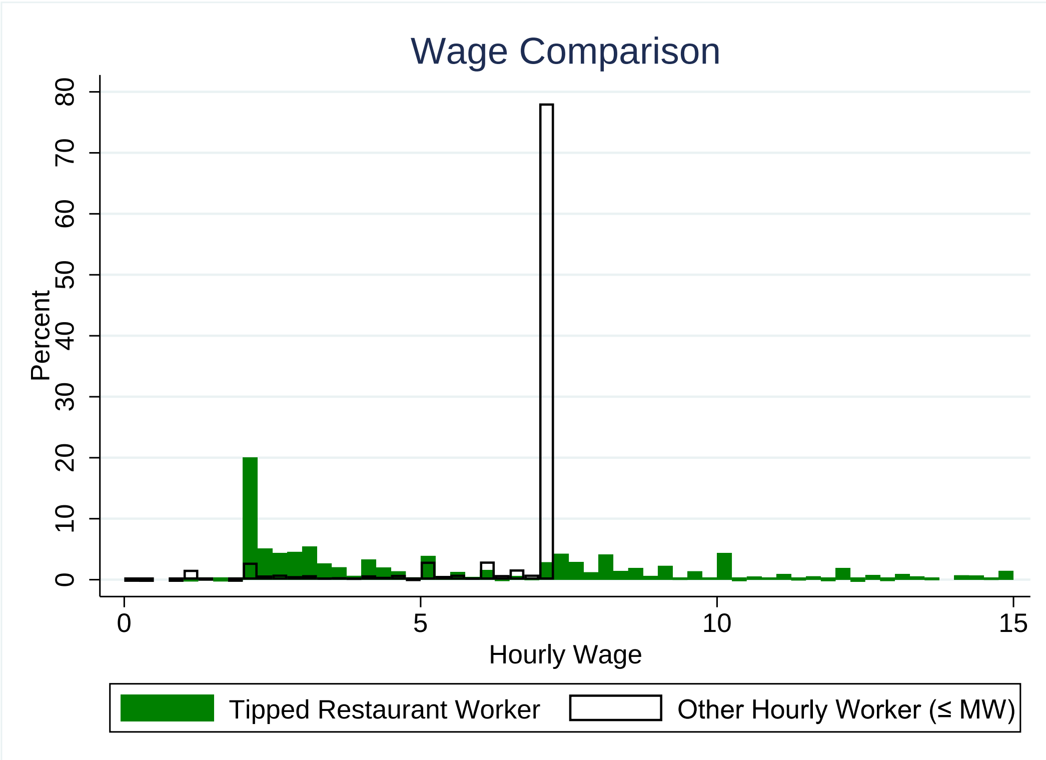


Figure 5B: Family Income-to-Needs Distributions of Tipped Restaurant Workers and Other Hourly Workers Earning Less Than or Equal to the Federal Minimum Wage

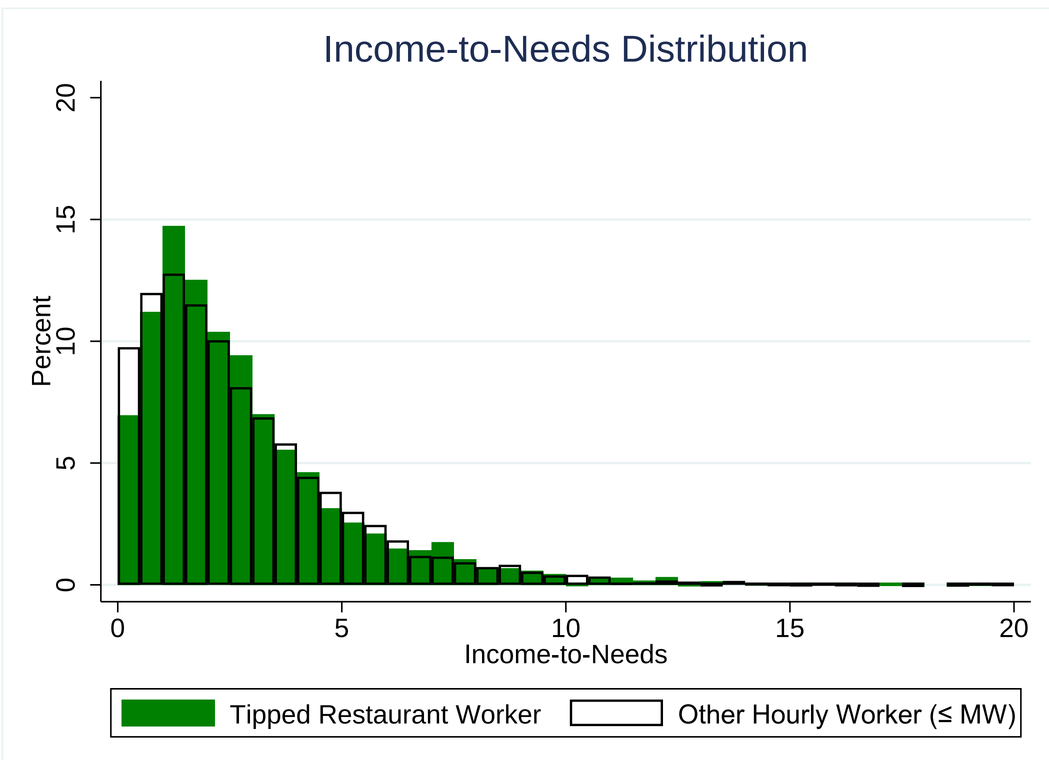


Figure 6A: Wage Distributions of Tipped Restaurant Workers and Other Hourly Workers Earning Less Than or Equal to 125% of the Federal Minimum Wage

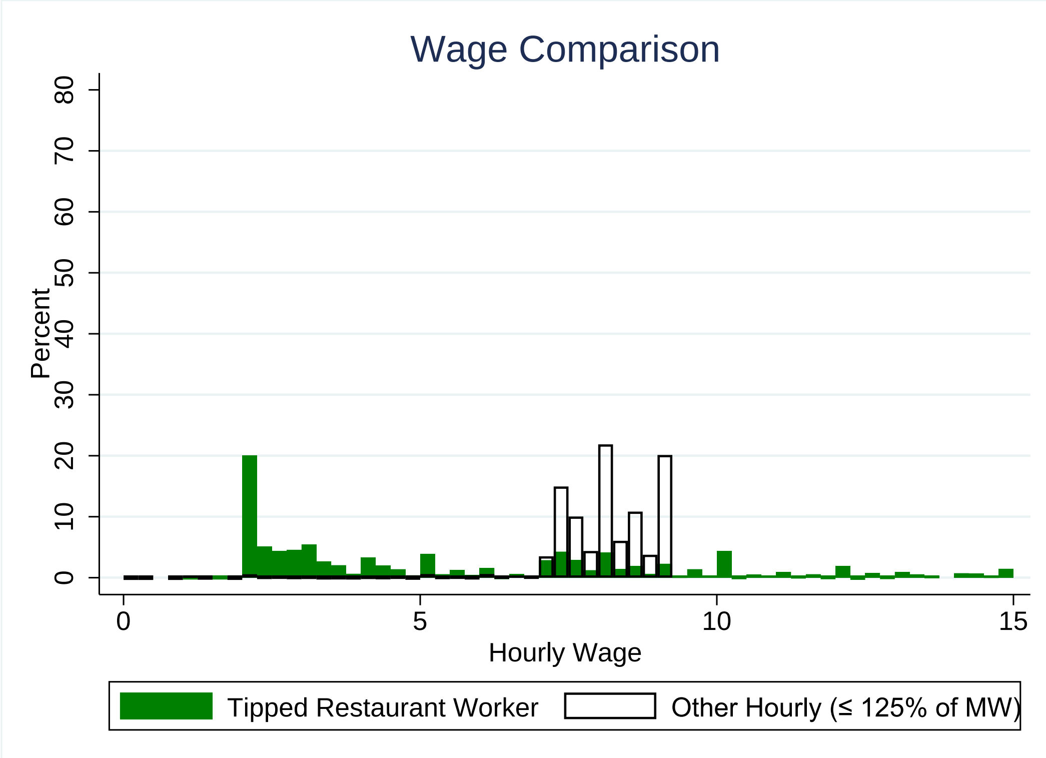
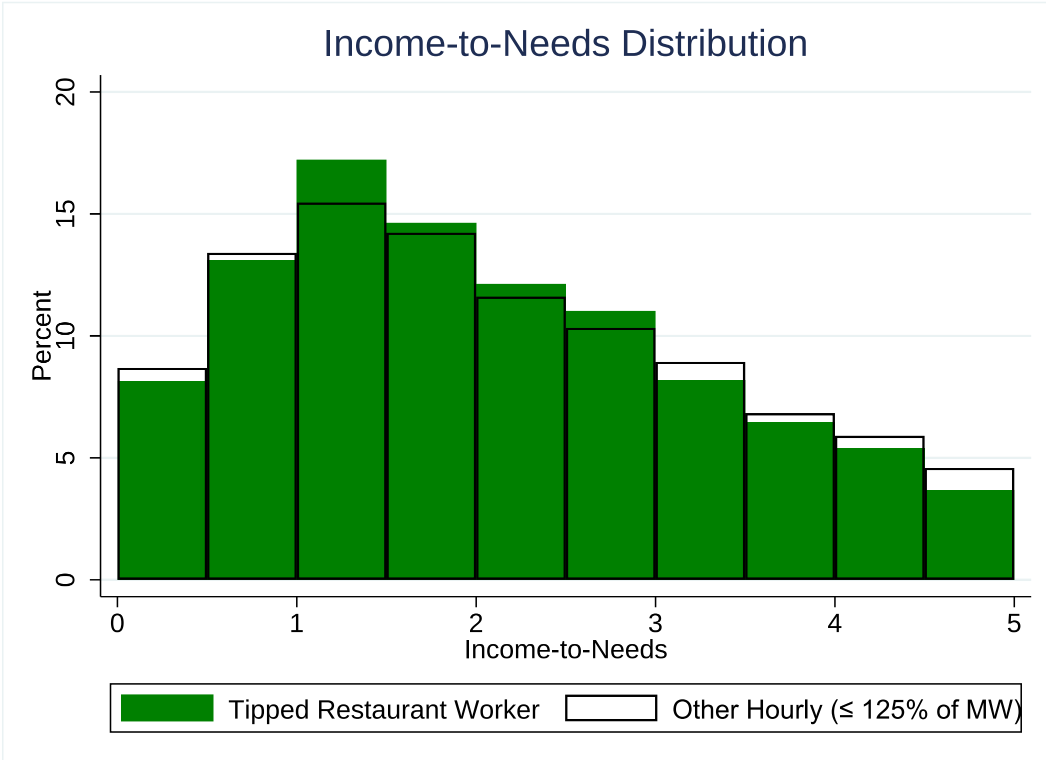


Figure 6B: Family Income-to-Needs Distributions of Tipped Restaurant Workers and Other Hourly Workers Earning Less Than or Equal to 125% of the Federal Minimum Wage



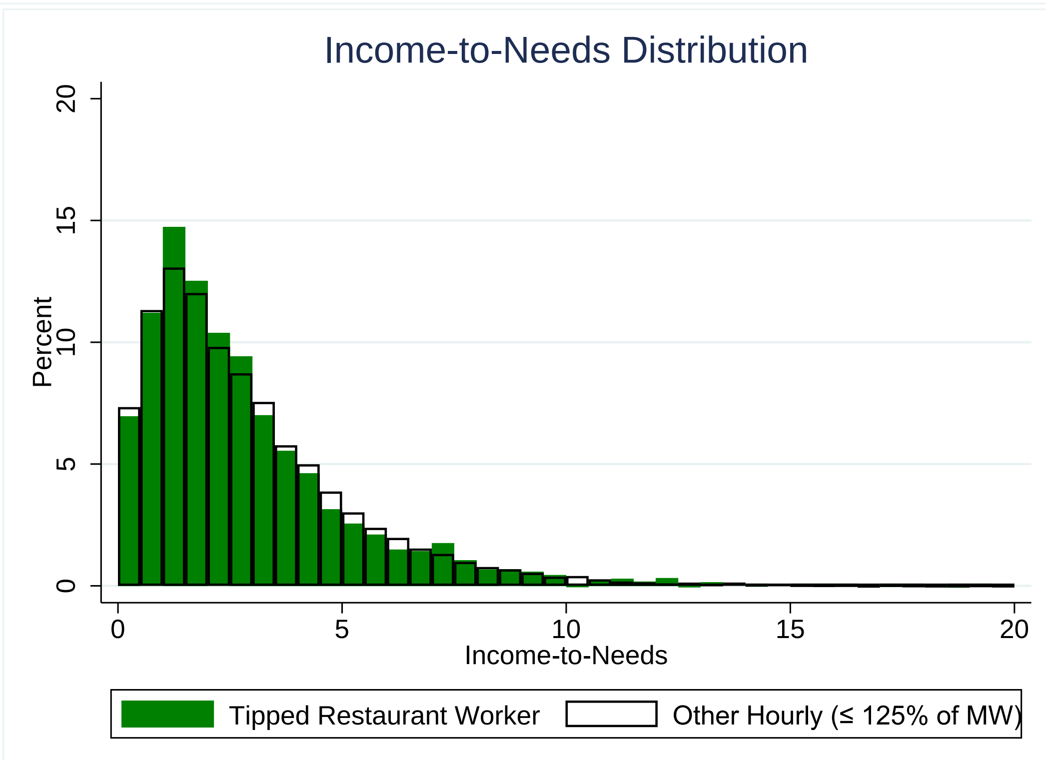


Table 1: State Minimum Wages and Tip Credits (2019)

State	MW	Tipped MW	Federal MW Binds ($7.25)	Some Tip Credit	Federal MW Binds + Some Tip Credit	Federal Tip Credit ($2.13)	Fed MW Binds + Fed Tip Credit
Total			**21**	**44**	**21**	**17**	**15**
ALABAMA	7.25	2.13	1	1	1	1	1
ALASKA	9.89	9.89	0	0	0	0	0
ARIZONA	11.00	8.00	0	1	0	0	0
ARKANSAS	9.25	2.63	0	1	0	0	0
CALIFORNIA	12.00	12.00	0	0	0	0	0
COLORADO	11.10	8.08	0	1	0	0	0
CONNECTICUT	10.33	6.38	0	1	0	0	0
DELAWARE	8.88	2.23	0	1	0	0	0
DISTRICT OF COLUMBIA	13.63	4.17	0	1	0	0	0
FLORIDA	8.46	5.44	0	1	0	0	0
GEORGIA	7.25	2.13	1	1	1	1	1
HAWAII	10.10	9.35	0	1	0	0	0
IDAHO	7.25	3.35	1	1	1	0	0
ILLINOIS	8.25	4.95	0	1	0	0	0
INDIANA	7.25	2.13	1	1	1	1	1
IOWA	7.25	4.35	1	1	1	0	0
KANSAS	7.25	2.13	1	1	1	1	1
KENTUCKY	7.25	2.13	1	1	1	1	1
LOUISIANA	7.25	2.13	1	1	1	1	1
MAINE	11.00	5.50	0	1	0	0	0
MARYLAND	10.10	3.63	0	1	0	0	0
MASSACHUSETTS	12.00	4.35	0	1	0	0	0
MICHIGAN	9.42	3.58	0	1	0	0	0
MINNESOTA	9.86	9.86	0	0	0	0	0
MISSISSIPPI	7.25	2.13	1	1	1	1	1
MISSOURI	8.60	4.30	0	1	0	0	0
MONTANA	8.50	8.50	0	0	0	0	0
NEBRASKA	9.00	2.13	0	1	0	1	0
NEVADA	8.25	8.25	0	0	0	0	0
NEW HAMPSHIRE	7.25	3.26	1	1	1	0	0
NEW JERSEY	9.43	2.38	0	1	0	0	0
NEW MEXICO	7.50	2.13	0	1	0	1	0
NEW YORK	11.10	7.50	0	1	0	0	0
NORTH CAROLINA	7.25	2.13	1	1	1	1	1
NORTH DAKOTA	7.25	4.86	1	1	1	0	0
OHIO	8.55	4.30	0	1	0	0	0
OKLAHOMA	7.25	2.13	1	1	1	1	1
OREGON	11.00	11.00	0	0	0	0	0
PENNSYLVANIA	7.25	2.83	1	1	1	0	0
RHODE ISLAND	10.50	3.89	0	1	0	0	0
SOUTH CAROLINA	7.25	2.13	1	1	1	1	1
SOUTH DAKOTA	9.10	4.55	0	1	0	0	0
TENNESSEE	7.25	2.13	1	1	1	1	1
TEXAS	7.25	2.13	1	1	1	1	1
UTAH	7.25	2.13	1	1	1	1	1
VERMONT	10.78	5.39	0	1	0	0	0
VIRGINIA	7.25	2.13	1	1	1	1	1
WASHINGTON	12.00	12.00	0	0	0	0	0
WEST VIRGINIA	8.75	2.62	0	1	0	0	0
WISCONSIN	7.25	2.33	1	1	1	0	0
WYOMING	7.25	2.13	1	1	1	1	1

Note: MW is calculated as average monthly MW over the year. 1 indicates “yes” and 0 indicates “no.”

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 total earnings	Log total quarterly earnings	Log total quarterly earnings	Log employment	Log employment	Log employment
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Weighted by state private-sector employment									
A. 1990-2019									
Tipped MW	0.062***	0.019	0.044**	-0.004	0.031	-0.036	-0.064*	0.015	-0.079**
	[0.014]	[0.020]	[0.019]	[0.045]	[0.044]	[0.034]	[0.037]	[0.046]	[0.032]
MW	0.094***	0.244***	-0.149***	0.099	0.133**	-0.034	0.040	-0.075	0.115***
	[0.021]	[0.046]	[0.041]	[0.066]	[0.061]	[0.045]	[0.052]	[0.062]	[0.041]
B. 2010-2019									
Tipped MW	0.088***	0.079*	0.009	0.087*	0.170***	-0.083	-0.053	0.011	-0.065
	[0.031]	[0.043]	[0.044]	[0.052]	[0.060]	[0.065]	[0.051]	[0.034]	[0.058]
MW	0.110**	0.284***	-0.175***	0.132*	0.208**	-0.075	0.071	-0.009	0.080
	[0.043]	[0.061]	[0.059]	[0.067]	[0.097]	[0.100]	[0.051]	[0.053]	[0.082]
Unweighted									
C. 1990-2019									
Tipped MW	0.063***	0.018	0.044**	-0.031	0.005	-0.037	-0.099**	-0.019	-0.080**
	[0.015]	[0.020]	[0.019]	[0.047]	[0.041]	[0.041]	[0.044]	[0.039]	[0.033]
MW	0.055	0.172***	-0.117***	0.141*	0.129*	0.012	0.107	-0.018	0.125*
	[0.034]	[0.038]	[0.030]	[0.082]	[0.072]	[0.064]	[0.094]	[0.076]	[0.066]
D. 2010-2019									
Tipped MW	0.094***	0.077**	0.017	-0.032	0.089	-0.122*	-0.128**	0.005	-0.133**
	[0.017]	[0.036]	[0.034]	[0.068]	[0.064]	[0.069]	[0.051]	[0.038]	[0.059]
MW	0.056	0.227***	-0.170***	0.221***	0.162**	0.059	0.174**	-0.054	0.227**
	[0.037]	[0.051]	[0.057]	[0.077]	[0.076]	[0.103]	[0.076]	[0.055]	[0.088]

Note: The dependent variables and minimum wage variables are measured in logs. The models also include state and quarter fixed effects. Col (1) – (3) include controls for log average private-sector weekly wages. Col (4) – (6) include controls for log total private-sector quarterly earnings by state. Col (7) – (9) include controls for log private-sector employment. 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: Sample Sizes, March ASEC Files Linked to March-June ORG files, 2010-2019

Comparison to:	**Observations**
A. All Workers	
Tipped Restaurant Workers	2,214
Other	249,332
B. Hourly Workers (Figures 4A and 4B)	
Tipped Restaurant Workers	2,214
Other hourly workers	144,786
C. Low-Wage Hourly Workers (Figures 5A and 5B)	
Tipped Restaurant Workers	2,214
Other hourly workers earning ≤ $7.25	5,434
D. Low-Wage Hourly Workers (Figures 6A and 6B)	
Tipped Restaurant Workers	2,214
Other hourly workers earning ≤ $7.25 x 1.25	26,501

Note: Tipped restaurant workers report receiving overtime, commission, or tips and are currently working in either the restaurant or drinking place establishment industry and in the waiter/waitress or bartender occupations. Workers who report receiving overtime, commission, or tips and are currently working in either the restaurant or drinking place establishment industry but not in the waiter/waitress or bartender occupations are dropped from the sample to prevent misclassification issues.

Table 4: 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)	$582,319,088	3.5%	38,445	$1,515	28.5
(0.5, 1.0)	$1,867,968,359	11.2%	61,822	$3,022	30.9
(1.0-1.5)	$3,021,461,139	18.1%	81,359	$3,731	32.1
(1.5-2.0)	$2,393,460,872	14.4%	69,139	$3,462	31.8
(2.0-3.0)	$3,615,734,332	21.7%	109,324	$3,309	32.2
3.0 or higher	$5,195,451,314	31.2%	193,538	$2,688	29.7
Total	$16,676,395,104	100.0%	553,626	$3,016	30.9
B. Increase MW to $8.14, Preserve Tipped MW					
(0, 0.5)	$815,576,653	4.9%	187,527	$424	27.0
(0.5, 1.0)	$2,244,875,753	13.5%	359,847	$599	29.0
(1.0-1.5)	$2,622,440,026	15.8%	404,965	$640	30.3
(1.5-2.0)	$2,164,136,958	13.0%	354,580	$617	29.3
(2.0-3.0)	$3,193,925,777	19.2%	540,369	$576	28.1
3.0 or higher	$5,583,210,428	33.6%	1,105,406	$499	25.5
Total	$16,624,165,596	100.0%	2,952,694	$553	27.6

Table 5A: Estimated Distributional Effects of Changes in Tipped and Regular Minimum Wages (Weighted)

	Age 16+	Age 16+	Age 16+	Age 16+	Age 16-70	Age 16-70	Age 16-70	Age 16-70
1990 – 2019	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
A. Probability poor (I/N ≤ 1)								
Tipped MW	0.001	-0.001	0.069**	0.053	0.001	-0.000	0.060*	0.051
	[0.001]	[0.002]	[0.030]	[0.042]	[0.001]	[0.002]	[0.031]	[0.044]
MW	-0.002	-0.003	-0.181*	-0.220**	-0.002	-0.003*	-0.199**	-0.247***
	[0.001]	[0.001]	[0.092]	[0.094]	[0.001]	[0.002]	[0.086]	[0.090]
B. Probability extremely poor (I/N ≤ .5)								
Tipped MW	0.001	0.000	0.064	0.104*	0.001	0.000	0.065	0.092*
	[0.001]	[0.001]	[0.048]	[0.058]	[0.001]	[0.001]	[0.049]	[0.053]
MW	-0.001*	-0.001*	-0.264**	-0.297***	-0.002*	-0.002**	-0.295***	-0.327***
	[0.001]	[0.001]	[0.099]	[0.103]	[0.001]	[0.001]	[0.101]	[0.101]
C. Probability nearly poor (I/N ≤ 1.5)								
Tipped MW	0.002**	0.001	0.065***	0.058**	0.002*	0.001	0.063***	0.064*
	[0.001]	[0.002]	[0.020]	[0.031]	[0.001]	[0.002]	[0.023]	[0.036]
MW	-0.002	-0.004**	-0.094*	-0.141**	-0.002	-0.004**	-0.110**	-0.169***
	[0.002]	[0.002]	[0.051]	[0.056]	[0.002]	[0.002]	[0.051]	[0.060]
2010-2019								
D. Probability poor (I/N ≤ 1)								
Tipped MW	-0.004*	-0.003*	-0.159	-0.103	-0.005*	-0.003	-0.176	-0.095
	[0.002]	[0.002]	[0.104]	[0.092]	[0.003]	[0.002]	[0.119]	[0.111]
MW	0.001	-0.000	0.034	-0.084	0.002	-0.001	0.016	-0.129
	[0.002]	[0.002]	[0.140]	[0.125]	[0.002]	[0.002]	[0.168]	[0.159]
E. Probability extremely poor (I/N ≤ .5)								
Tipped MW	-0.003***	-0.001	-0.365***	-0.144	-0.004***	-0.002**	-0.404***	-0.231*
	[0.001]	[0.001]	[0.121]	[0.103]	[0.001]	[0.001]	[0.127]	[0.123]
MW	0.001	0.003**	0.195	0.467**	0.002	0.003**	0.208	0.424**
	[0.001]	[0.001]	[0.182]	[0.196]	[0.001]	[0.001]	[0.200]	[0.204]
F. Probability nearly poor (I/N ≤ 1.5)								
Tipped MW	-0.004	-0.004*	-0.103	-0.067	-0.005*	-0.005**	-0.116	-0.072
	[0.003]	[0.002]	[0.073]	[0.069]	[0.003]	[0.002]	[0.075]	[0.075]
MW	0.002	-0.001	0.034	-0.099	0.003	-0.001	0.029	-0.134
	[0.003]	[0.003]	[0.101]	[0.094]	[0.003]	[0.003]	[0.101]	[0.109]
For all panels								
Levels or logs	Levels	Levels	Logs	Logs	Levels	Levels	Logs	Logs
State-specific trends	No	Yes	No	Yes	No	Yes	No	Yes

Note: 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 population. *** Standard errors are clustered by state. There are 1,530 observations for 1990-2019 and 510 observations for 2010-2019. *** $p < .01$; ** $p < .05$; * $p < .1$.

Table 5B: Estimated Distributional Effects of Changes in Tipped and Regular Minimum Wages (Unweighted)

	Age 16+	Age 16+	Age 16+	Age 16+	Age 16-70	Age 16-70	Age 16-70	Age 16-70
1990 – 2019	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
A. Probability poor (I/N ≤ 1)								
Tipped MW	0.001	0.001	0.072^{*}	0.075^{*}	0.001	0.001	0.062	0.069
	[0.001]	[0.001]	[0.036]	[0.044]	[0.001]	[0.001]	[0.038]	[0.045]
MW	-0.000	-0.001	-0.083	-0.150^{*}	-0.001	-0.002	-0.109	-0.185^{**}
	[0.001]	[0.001]	[0.074]	[0.076]	[0.001]	[0.001]	[0.080]	[0.081]
B. Probability extremely poor (I/N ≤ .5)								
Tipped MW	0.001	0.000	0.081^{*}	0.100^{*}	0.001^{**}	0.001	0.088^{*}	0.095
	[0.000]	[0.001]	[0.048]	[0.069]	[0.000]	[0.001]	[0.049]	[0.063]
MW	-0.001	-0.001	-0.187^{**}	-0.196^{**}	-0.001^{*}	-0.001^{*}	-0.220^{**}	-0.244^{***}
	[0.001]	[0.001]	[0.082]	[0.087]	[0.001]	[0.001]	[0.085]	[0.086]
C. Probability nearly poor (I/N ≤ 1.5)								
Tipped MW	0.002^{**}	0.002	0.068^{***}	0.075^{**}	0.002^{*}	0.002	0.063^{**}	0.071^{*}
	[0.001]	[0.002]	[0.024]	[0.033]	[0.001]	[0.002]	[0.027]	[0.036]
MW	-0.000	-0.003^{**}	-0.076	-0.145^{***}	-0.001	-0.003^{**}	-0.094	-0.172^{***}
	[0.001]	[0.001]	[0.051]	[0.050]	[0.001]	[0.001]	[0.057]	[0.057]
2010-2019								
D. Probability poor (I/N ≤ 1)								
Tipped MW	-0.001	0.004	-0.062	0.130	-0.001	0.004	-0.065	0.137
	[0.002]	[0.004]	[0.075]	[0.144]	[0.003]	[0.005]	[0.079]	[0.152]
MW	-0.000	-0.004	0.010	-0.211	-0.001	-0.005	-0.038	-0.252
	[0.002]	[0.003]	[0.100]	[0.160]	[0.002]	[0.004]	[0.113]	[0.180]
E. Probability extremely poor (I/N ≤ .5)								
Tipped MW	-0.001	0.000	-0.147	0.030	-0.001	0.000	-0.123	0.027
	[0.001]	[0.002]	[0.111]	[0.181]	[0.002]	[0.002]	[0.108]	[0.177]
MW	-0.000	0.002	0.005	0.266	-0.000	0.001	-0.035	0.199
	[0.001]	[0.002]	[0.163]	[0.196]	[0.001]	[0.002]	[0.176]	[0.197]
F. Probability nearly poor (I/N ≤ 1.5)								
Tipped MW	-0.002	0.003	-0.044	0.067	-0.002	0.003	-0.090	0.059
	[0.003]	[0.005]	[0.066]	[0.113]	[0.003]	[0.005]	[0.055]	[0.112]
MW	0.001	-0.004	0.003	-0.169	0.000	-0.005	-0.006	-0.189
	[0.002]	[0.003]	[0.087]	[0.117]	[0.002]	[0.004]	[0.091]	[0.128]
For all panels								
Level or IHS	Levels	Levels	Logs	Logs	Levels	Levels	Logs	Logs
State-specific trends	No	Yes	No	Yes	No	Yes	No	Yes

Note: See notes to Table 5A. Estimates are unweighted.

Appendix A: Synthetic control analysis

As a complement to our panel data analysis of effects on employment and earnings, we also conducted a case-study approach, studying the impact of some isolated cases of large state increases in the tipped minimum wage, constructing controls using synthetic control methods. There are four states that have large increases in their tipped minimum wages or their tipped minimum wages relative to their regular minimum wages, coupled with periods of stable or nearly stable tipped and regular minimum wages both before and after the policy change. These changes all occurred in 1996:Q4, in Alaska, Nevada, Oregon, and Washington, with Oregon and Washington providing far larger relative increases in the tipped minimum wage. Figure A1 provides enlarged versions of the figures from Figure 3, for these four states (note the vertical scales in the graphs differ).

Table A1 shows that the increase in the tipped minimum wage relative to the regular minimum wage was $.90 in Alaska and Nevada, and $2.60 in Washington. The table also shows that both minimum wages were stable back to 1994:Q1, and for the two quarters after 1996:Q4, in these three states. In Oregon, there was a large change in the tipped minimum wage relative to the minimum wage, and a nearly stable post-treatment period. In particular, in Oregon the tipped minimum wage increased by $2.62 in 1996:Q4, with no change in the regular minimum wage, but the regular minimum wage increased to $5.50 in the next quarter; still, the relative increase in the tipped minimum wage was large even compared to the $5.50 regular minimum – an increase of $1.87.

By the same argument, we present the analysis for each state through 1997:Q2, during which all minimum wages were stable, but we also present results for Washington through 1998:Q4, a period of a minor increase in the state minimum wage ($.25), still leaving an increase in the relative tipped minimum wage of $2.35. Given the magnitudes of the changes, the “experiments” for Oregon and

Washington are most informative about the effects of large increases in tipped minimum wages.[35] For all the analyses, the pre-treatment period starts in 1994:Q1, because for all four states both minimum wages are stable from then until the increases in 1996:Q4.

For the synthetic control analysis, for the employment analysis we define the outcome as the share of restaurant employment (in the full-service or limited-service sector) relative to total employment. Scaling restaurant employment by total employment effectively controls for aggregate economic conditions. For the earnings analysis we use average weekly wages.[36] In the analysis, we match on pre-treatment observations. For average weekly wages, we match on average weekly wages for the two quarters preceding the treatment quarter-year, and then the employment rate and average weekly wages each averaged over the entire pre-treatment time period. For the employment rate, we match on the employment rate for the two quarters preceding the treatment quarter-year, and then, again, the employment rate and average weekly wages each averaged over the entire pre-treatment time period.[37]

The results are reported in Figures A2 and A3 and in Table A2. Figures A2 and A3 show the estimates for each post-treatment quarter. For average weekly wages, in Panel A of Figure A2, for full-service restaurants, there is clear evidence of an increase only for Nevada. However, the relative increases appear larger than what occurred in the limited-service sector (Panel B) for Alaska, Nevada, and Washington, and for Oregon in the last quarter. When we estimate the models for the difference between the two sectors, in Panel C, we see increases in all four states.

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[35] Note that all four states eventually eliminated their lower tipped minimum wages, as also shown in Figure A1. California also eliminated its lower tipped minimum wage, but never provides a period with a large relative tipped minimum wage increase and stable minimum wage policies before and after.
[36] We do not report result for total earnings; as explained above, knowing the effect on average weekly wages and on employment provides the information we need to interpret the evidence on minimum wage effects in these data.
[37] We do not match on the entire pre-treatment history of the dependent variable, because this makes any other covariates irrelevant. See Kaul et al. (2017), whose recommendations we follow.

The employment results are reported in Figure A3. For Alaska, Oregon, and Washington, Panel A shows employment declines in the full-service sector (more clearly for Oregon and Washington). And Panel B shows employment increases for all four states in the limited-service sector. The relative estimates, in Panel C, indicate employment declines in Alaska, Oregon, and Washington.

Table A2 reports the corresponding estimated treatment effects for the full post-treatment period (recall that there are two for Washington). Jumping to Panel C, for the relative estimates on earnings, for all five cases (the four states, including two post-treatment periods for Washington), the estimated earnings effect is positive. It is statistically significant at the 5% level for Nevada, and at the 10% level for the longer post-treatment period for Washington.

For employment, in Panel F, four of the five estimates are negative, but only the estimate for Alaska is statistically significant (at the 10% level). Note that the units are log of the percentage of employment in the sector (on a 0-100 scale), so the magnitudes are elasticities for this percentage or share.

Overall, then, the synthetic control analysis points to increases in average weekly earnings in the full-service sector as a result of increases in the tipped minimum wage. The employment estimates tend to point to job loss, although only one estimate is statistically significant. Using the point estimates to compare the elasticities implied by the earnings and employment estimates, we find roughly offsetting elasticities, implying that there is little evidence that raising tipped minimum wages on net benefits tipped workers in the full-service restaurant sector. In sum, then, we find the synthetic control analyses broadly consistent with the panel data analyses, although the synthetic control analysis is less informative statistically, likely as a consequence of considering only a single tipped minimum wage change in one state.[38]

[38] Powell (forthcoming) has developed a synthetic control estimator for multiple events, with continuous treatment that can occur in any of the donor observations (and applies this to the estimated effect of the minimum wage on teen employment). It cannot be adopted here, to the best of our knowledge, because we effectively have two different treatments.

Figure A1: State Regular and Tipped Minimum Wages (Enlarged Figures for States in Synthetic Control Analysis)

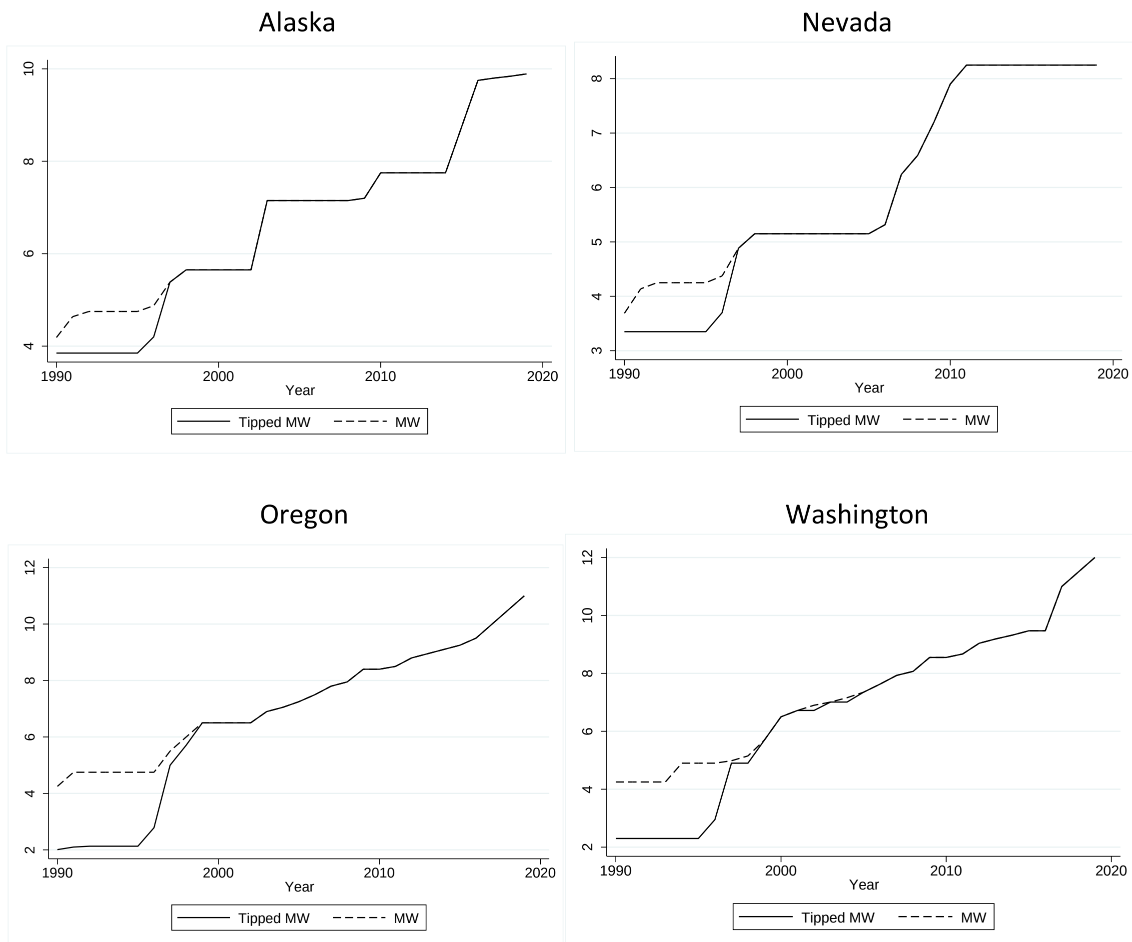
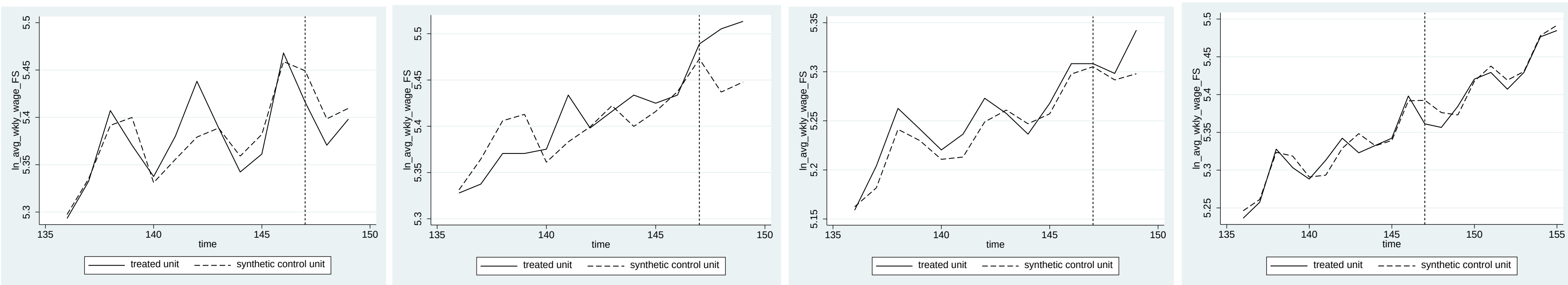


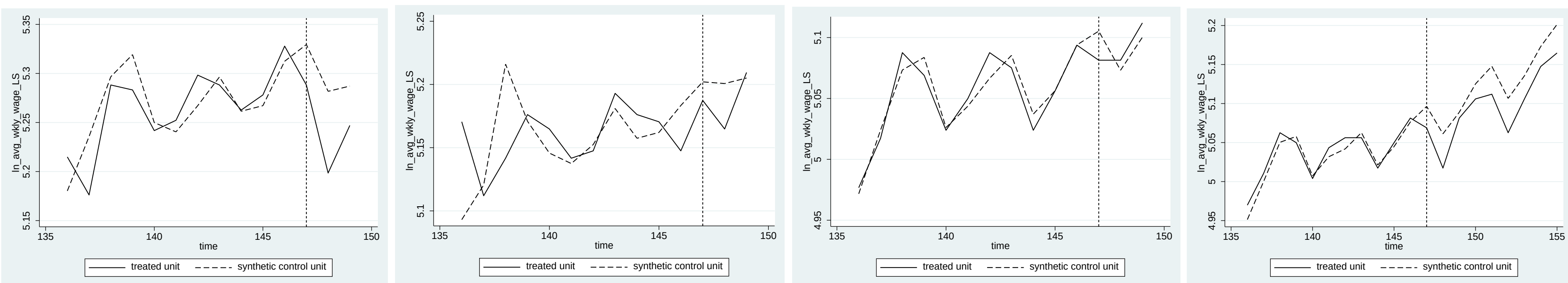
Figure A2: Synthetic Control Graphs for Log Average Weekly Wages (Match On Last Two Quarters + Average Weekly Wages/Employment)

A. Full Service

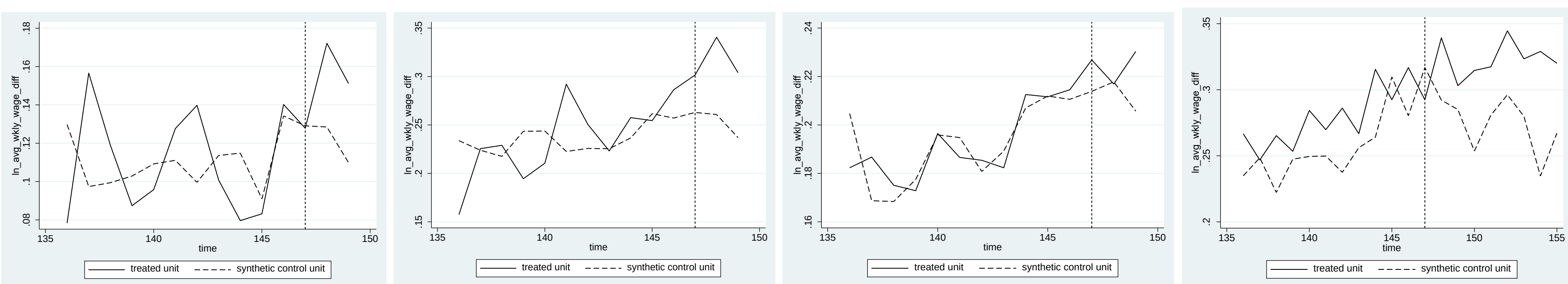
Alaska | Nevada | Oregon | Washington



B. Limited Service



C. Full Service – Limited Service

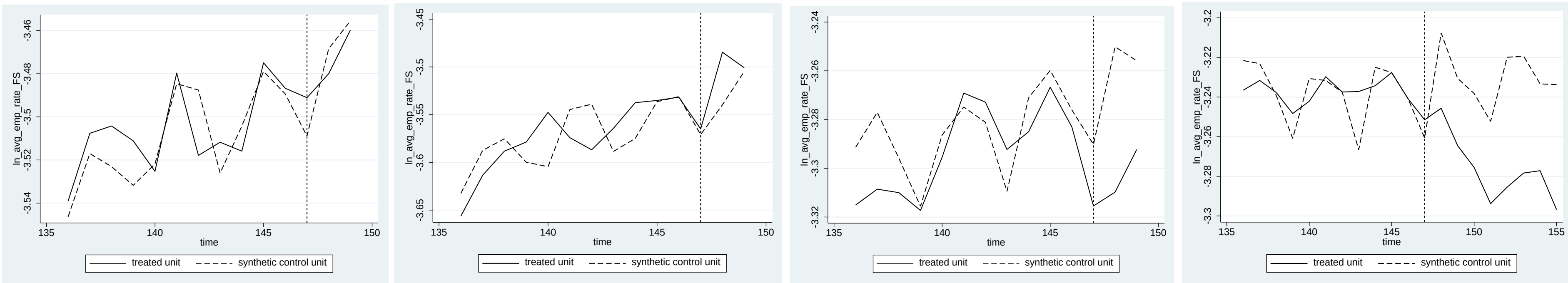


Treatment time 147 corresponds to 1996:Q4.

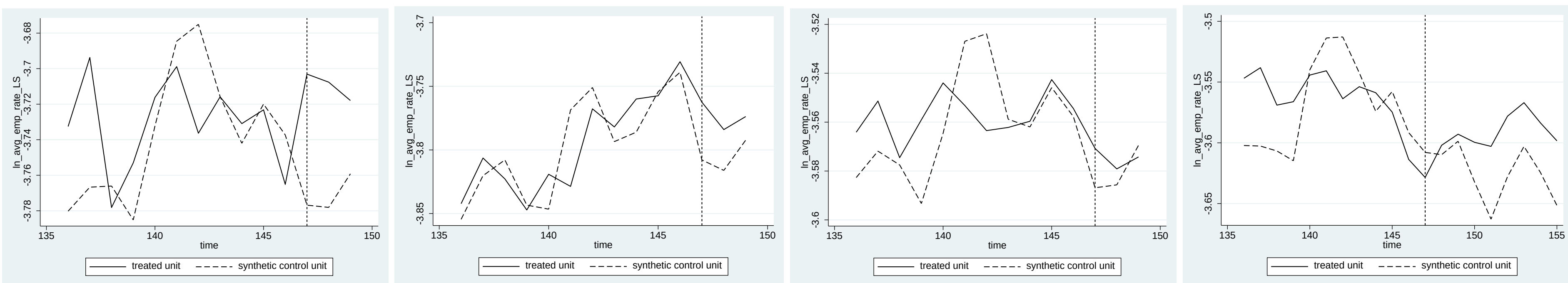
Figure A3: Synthetic Control Graphs for Log Restaurant Employment/Total Employment (Match On Last Two Quarters + Average Weekly Wages/Employment)

A. Full Service

Alaska | Nevada | Oregon | Washington



B. Limited Service



C. Full Service – Limited Service

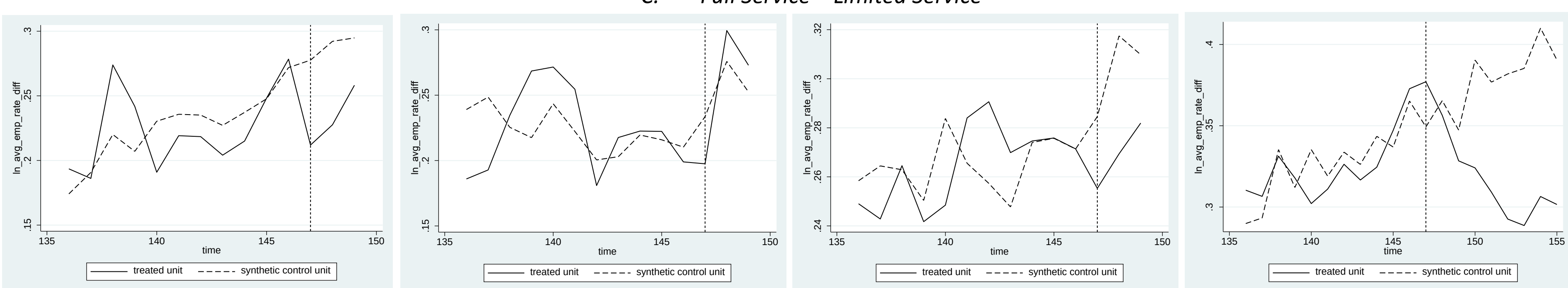


Table A1: Tipped Minimum Wages and Regular Minimum Wages for Treated States, Synthetic Control Analysis

Year	Qtr.	MW	Tipped MW	MW	Tipped MW	MW	Tipped MW	MW	Tipped MW
State		Alaska		Nevada		Washington State		Oregon	
1993	1	4.75	3.85	4.25	3.35	4.25	2.30	4.75	2.13
1993	2	4.75	3.85	4.25	3.35	4.25	2.30	4.75	2.13
1993	3	4.75	3.85	4.25	3.35	4.25	2.30	4.75	2.13
1993	4	4.75	3.85	4.25	3.35	4.25	2.30	4.75	2.13
1994	1	4.75	3.85	4.25	3.35	4.90	2.30	4.75	2.13
1994	2	4.75	3.85	4.25	3.35	4.90	2.30	4.75	2.13
1994	3	4.75	3.85	4.25	3.35	4.90	2.30	4.75	2.13
1994	4	4.75	3.85	4.25	3.35	4.90	2.30	4.75	2.13
1995	1	4.75	3.85	4.25	3.35	4.90	2.30	4.75	2.13
1995	2	4.75	3.85	4.25	3.35	4.90	2.30	4.75	2.13
1995	3	4.75	3.85	4.25	3.35	4.90	2.30	4.75	2.13
1995	4	4.75	3.85	4.25	3.35	4.90	2.30	4.75	2.13
1996	1	4.75	3.85	4.25	3.35	4.90	2.30	4.75	2.13
1996	2	4.75	3.85	4.25	3.35	4.90	2.30	4.75	2.13
1996	3	4.75	3.85	4.25	3.35	4.90	2.30	4.75	2.13
1996	**4**	**5.25**	**5.25**	**4.75**	**4.75**	**4.90**	**4.90**	**4.75**	**4.75**
1997	1	5.25	5.25	4.75	4.75	4.90	4.90	5.50	4.75
1997	2	5.25	5.25	4.75	4.75	4.90	4.90	5.50	4.75
1997	3	5.38	5.38	4.88	4.88	4.98	4.90	5.50	5.00
1997	4	5.65	5.65	5.15	5.15	5.15	4.90	5.50	5.50
1998	1	5.65	5.65	5.15	5.15	5.15	4.90	6.00	5.50
1998	2	5.65	5.65	5.15	5.15	5.15	4.90	6.00	5.50
1998	3	5.65	5.65	5.15	5.15	5.15	4.90	6.00	5.83
1998	4	5.65	5.65	5.15	5.15	5.15	4.90	6.00	6.00

Table A2: Synthetic Control Graphs for Average Weekly Wages (Match On Last Two Quarters + Average Weekly Wages/Employment)

	Alaska	**Nevada**	**Oregon**	**Washington**	**Washington**
Treatment period	1996:Q4-1997:Q2	1996:Q4-1997:Q2	1996:Q4-1997:Q2	1996:Q4-1997:Q2	1996:Q4-1998:Q4
	(1)	(3)	(4)	(5)	(6)
Average weekly wages					
A. Full service					
Estimate	-0.024	0.050**	0.018	-0.013	-0.007
p-value	0.222	0.037	0.296	0.593	0.778
RMSPE	0.023	0.027	0.011	0.012	0.012
B. Limited service					
Estimate	-0.055	-0.015	-0.001	-0.026	-0.030
p-value	0.111	0.296	0.778	0.222	0.111
RMSPE	0.026	0.036	0.011	0.009	0.009
C. Full service – limited service					
Estimate	0.028	0.062**	0.012	0.014	0.042*
p-value	0.148	0.037	0.370	0.444	0.074
RMSPE	0.031	0.038	0.010	0.032	0.032
Restaurant employment/ total employment					
D. Full service					
Estimate (%)	0.001	0.022	-0.040	-0.021	-0.041
p-value	0.963	0.407	0.185	0.444	0.148
RMSPE	0.014	0.000	0.013	0.012	0.012
E. Limited service					
Estimate (%)	0.062*	0.032*	0.006	-0.002	0.029
p-value	0.074	0.074	0.852	0.926	0.370
RMSPE	0.035	0.024	0.0020	0.037	0.037
F. Full service – limited service					
Estimate (%)	-0.056*	0.003	-0.035	-0.000	-0.057
p-value	0.074	0.963	0.296	1.000	0.148
RMSPE	0.026	0.032	0.019	0.015	0.015

Note: There are 26 states in the donor pool. *** $p < .01$; ** $p < .05$; * $p < .1$. We match on the pre-treatment outcome variable for the two quarters preceding the treatment quarter, the average pre-treatment average weekly wages, and the average pre-treatment restaurant employment/total employment. The p-value is calculated from the placebo inference procedure (Abadie et al., 2010).