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ABSTRACT

We analyze the price pass-through effect of the minimum wage and use the results to provide insight into the competitive structure of low-wage labor markets. Using monthly price series, we find that the pass-through effect is entirely concentrated on the month that the minimum wage change goes into effect, and is much smaller than what the canonical literature has found. We then discuss why our results differ from that literature, noting the impact of series interpolation in generating most of the previous results. We then use the variation in the size of the minimum wage change to evaluate the competitive nature of low-wage labor markets. Finally, we exploit the rich variation in minimum wage policy of the last 10–15 years—including the rise of state- and city-level minimum wage changes and the increased use of indexation—to investigate how the extent of price pass-through varies by policy context. This paper contributes to the literature by clarifying our understanding of the dynamics and magnitude of the pass-through effect and enriching the discussion of how different policies may shape the effect that minimum wage hikes have on prices.

JEL Classification Codes: J3, J48, J11

Key Words: Minimum wage, pass-through effect, monopsony, public policy

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In recent years, partly due to inaction among lawmakers to raise the federal minimum wage, states and cities have increasingly passed their own minimum wage laws. These state and city laws promoted a renaissance in the study of the employment effect of minimum wage hikes for two main reasons. First, they created greater numbers of minimum wage changes to be studied using then-standard techniques. Second, by increasing geographical variation in minimum wage policy, state and city lawmakers created the opportunity to employ "natural experiments" whereby the employment statistics in a state that increased its minimum wage could be compared to those in surrounding states that did not increase their minimum wage. Because of this renaissance, two sides of the minimum wage research developed. One side found that, contrary to the previously accepted belief, some minimum wage hikes led to either no decline in employment or a slight increase in employment (e.g., Card and Krueger 1994, 1995; Dube, Lester, and Reich 2010). A second side continued to find evidence supporting the claim that minimum wage hikes did reduce employment (e.g., Neumark 2001; Neumark and Wascher 2002, 2007, 2008).[1] A comprehensive overview of this research can be found in Belman and Wolfson (2014).

An additional important, although less-studied, question addresses the impact such hikes have on output prices, that is, the "pass-through" effect. Early studies include Wessels (1980) and Card and Krueger (1995). The most influential of these studies, however, has been a series of papers by Daniel Aaronson and coauthors. Aaronson (2001), MacDonald and Aaronson (2006), Aaronson and French (2007), and Aaronson, French, and MacDonald (2008) find evidence for the claim that minimum wage hikes increase output prices and that the size of this pass-through suggests that the increased cost associated with a minimum wage hike is

[1] Explanations for small negative or positive employment effects included the existence of various market frictions arising from imperfect competition or search (e.g., Bhaskar and To 1999; Lang and Khan 1998).

completely passed along to consumers.[2] Aaronson and coauthors used their findings to argue that low-wage labor markets are highly competitive and, by implication, that minimum wage hikes necessarily lower employment. This literature on pass-though, then, is important both in itself and because it sheds indirect light on the ongoing debate over the employment effect of minimum wage hikes.

This paper contributes to the literature on price pass-through by presenting more accurate estimates of the pass-through effect than found in the previous literature, and by using these results to give insight into the competitive structure of low-wage labor markets. In particular, we find that the size of the pass-through effect is much smaller than previously reported, and that the characteristics of pass-through are more consistent with a model of the labor market based on some degree of market power on the demand side than they are with perfect competition. Additionally, we exploit the rich variation in minimum wage policy—the rise of state- and city-level minimum wages, as well as the increased use of indexation of the minimum wage to the CPI in areas such as Florida, Washington, Ohio, and San Francisco—to investigate how the extent of pass-through varies by policy context. For instance, we find that the size of the pass-through effect is smaller when the minimum wage is indexed to inflation and does not vary significantly depending on whether the minimum wage change happens at the federal or state level.

LITERATURE REVIEW AND CONTRIBUTION TO THE LITERATURE

Previous empirical studies have concluded that minimum wage hikes produce substantial price pass-through effects. The oft-cited study by Aaronson (2001) estimated the magnitude of

[2] The studies cited above are for the United States. Lemos (2008) provides a survey of the literature.

the pass-through using metropolitan-area food away from home (FAFH) CPI data between 1978 and 1995. In the base specification (p. 162), which included only monthly and yearly controls, the cumulative wage-price elasticity from three months before up to three months after a minimum wage hike was estimated at about 0.07, meaning that a 10 percent increase in the minimum wage is associated with a 0.7 percent increase in FAFH prices. Aaronson, French, and MacDonald (2008) used microlevel restaurant price data for the period 1995–1997, during which two changes to the federal minimum wage were implemented, to generate a wage-price elasticity of, again, about 0.07.[3] Though the empirical literature is somewhat limited outside of these two formative works (see Lemos [2008] for a review), other studies have found similar results in other countries and other cases.[4]

The magnitude of the pass-through has been presented as being consistent with what models of a perfectly competitive labor market would predict about the size of the pass-through. Based on the assumption that demand elasticities of fast-food, labor share, and capital-labor elasticity took on standard values found in the literature, Aaronson and French (2007) and Aaronson, French, and MacDonald (2008) estimated that in a perfectly competitive industry, a 10 percent increase in the minimum wage would lead to approximately a 0.7 percent increase in output prices, which was exactly what they had found in their empirical work.[5] They concluded,

[3] Behind this average price increase was substantial variation: prices for some restaurant items grew faster than this average, while prices for other items grew slower than the average, and some prices even fell after a minimum wage hike. The price increase was also higher in limited-service restaurants than it was in full-service restaurants.

[4] Other studies include Fougère, Gautier, Bihan (2010), who studied France; Lemos (2006), who studied Brazil; and Wadsworth (2010) and Draca, Machin, and Van Reenen (2011) who both studied the U.K. Another national-level study that focuses on the prices of a few restaurant items (burgers, chicken, pizza) is Basker and Khan (2013).

[5] Although the overall thrust of the existing empirical literature on minimum wage hike pass-though is to support the claim that labor markets for restaurants are best characterized by competition, the evidence is not unambiguous. For instance, Aaronson and French (2007, p. 696) write after their analysis of BLS micro price data for restaurants, “Given that some restaurants do not increase their prices after minimum wage hikes, but restaurants that do raise their prices usually do by more than 0.7 percent, it is difficult to compare the observed price response to the competitive prediction.”

therefore, that their estimates of pass-through supported the claim that low-wage labor markets are best characterized as perfectly competitive. If low-wage labor markets are perfectly competitive, then an increase in the minimum wage increases the marginal cost of labor, which leads, in turn, to higher production costs, higher prices, and, importantly, lower employment. This work on the pass-through therefore speaks to the on-going controversy about the competitive structure of low-wage labor markets and thus about the employment impact of a minimum wage increase.

Policy and academic work has frequently cited the above studies by Aaronson and co-authors as the authoritative studies on minimum wages and pass-through.[6] However, these studies deserve to be updated for a couple of reasons.

First, these studies rely on data from no later than 1997, but since that time we have seen an increase in the variation of minimum wage policy across several dimensions.[7] For instance, since 1997 we have seen a profusion of state and city minimum wage laws whose effect we cannot assume are identical to federal minimum wage hikes. Further, some states and cities have implemented laws that provide for scheduled increases in their minimum wage often indexed to some measure of price inflation. In this way, these new policies differ from the majority of minimum wages investigated by Aaronson and coauthors, which were often large, one-shot increases implemented with relatively little warning to businesses. Again, we cannot presume these new types of minimum wage hikes affect prices, or more generally the economy, in the same way minimum wage changes implemented before 1997 did. Indeed, one contribution of our

[6] Most of the later pass-through literature cites this paper as the canonical example, as well as much of the rest of the literature on the effects of the minimum wage such as Dube et al. (2010) and MaCurdy (2015).

[7] The use of data from this period continues up to present studies, as seen in MaCurdy (2015), who uses data from 1996, and from a single federal minimum wage increase, to draw conclusions about all minimum wages.

study is to present a comparative analysis of different types of minimum wage policies within a common data and econometric setting.

Table 1 details the differences between the minimum wages considered by Aaronson and coauthors with those we consider in this study. The table shows that state-level minimum wage increases are much more common—and federal-level increases much less common—after 1998. Other variations in policy such as indexed, city minimum wages, or perpetually scheduled minimum wage increases were absent or nearly absent from the period considered by the previous studies.

Second, we use the data differently than Aaronson (2001) did in order to extract greater insight into the process of pass-through. For instance, we treat monthly and bimonthly price series separately (instead of combining them, as did Aaronson [2001]) to better reveal the dynamics of pass-through pricing. Furthermore, by embracing the complicating factor of multiple-state metropolitan areas (instead of avoiding it as did Aaronson [2001]), we are able to more accurately measure the impact of different types of minimum wage increases, and thereby are able to shed additional light on the nature of competition in low-wage labor markets.

Finally, by using data after 1997 we are able to use CPI data that are less affected by various biases (such as substitution bias) that was not available to Aaronson (2001). This will again permit us to generate more accurate estimates of the extent of pass-through.

Looking ahead to the results, our first main finding is that wage-price elasticities are notably lower than reported in previous work: we find prices grow by 0.36 percent for every 10 percent increase in the minimum wage, which is almost half of the previously accepted 0.7 percent.[8] Second, we find that pass-through is primarily concentrated on the month that the

[8] This 0.036 elasticity is similar to what was found by Card and Krueger (1995, p. 54) in their study of a single minimum wage increase in New Jersey.

minimum wage hike goes into effect, with no appreciable impact on the month before or after. This finding contradicts most of the previous research. Third, we argue that estimated pass-through is consistent with market power on the demand-side of low-wage labor markets (e.g., monopsony or monopsonistic competition), which sheds light on one of the more contentious issues in the debates over the employment impact of minimum wage hikes. If low-wage labor markets are not perfectly competitive, no guarantee exists that a minimum wage hike will lead to lower employment. Fourth, we find that not all minimum wage hikes are the same. For instance, small, scheduled minimum wage hikes have smaller impacts on prices than large, one-time minimum wage hikes. Yet we find no significant differences between state- and federal-level minimum wage increases, even though we might expect business flight to have a larger impact in the case of state-level minimum wage changes.

DATA AND DATA TRANSFORMATIONS

The dependent variable in this study is the change in the log of food away from home CPI (FAFH CPI), a price index generated by the Bureau of Labor Statistics (BLS) for select U.S. metropolitan areas. FAFH includes food purchased and consumed outside of the home, and for the most part includes items sold at full- and limited-service restaurants.[9] These data are available on the BLS website. We include in our analysis all metropolitan areas that have either monthly or bimonthly FAFH data for at least part of the period of our study, 1978–2015, which gives us 28 series.[10]

[9] Additionally, FAFH includes ready-to-eat food purchased at motels and restaurants, food provided at employer and school sites, along with food purchased at vending machines and from mobile vendors. See BLS (YEAR? Chapter 17). For conciseness, we will refer in the text to "restaurants" when we talk about the group of sites selling food away from home.

[10] Using the major city within the area to identify them, the metropolitan areas included in our study are: Anchorage (bimonthly, until 1986), Atlanta (bimonthly, full time period), Baltimore (bimonthly, until 1995), Boston

We begin our analysis in 1978 because that is the year Aaronson (2001) started his analysis. The minimum wage increase in 1978 was also the first one after the implementation of changes in the Fair Labor Standards Act that directly affected the restaurant industry (for instance, a restructured tip credit process and a repeal of the partial exemption of restaurant employees from overtime rules), along with the expansion of the minimum wage to all covered, nonexempt employees. Thus, 1978 was the first year in which minimum wage changes would affect all minimum wage workers regardless of occupational status or industry, giving our estimates more consistency than if we relied on earlier data where different minimum wages affected different subsets of workers.[11]

One characteristic of the CPI data requires comment. In January 1999, the BLS switched to a geometric mean formula when they calculated CPI price indexes. This switch was prompted by arguments that the BLS's method for calculating the CPI before 1999 produced an upward bias to the CPI and its subcomponents. The new geometric mean formula could mimic consumers' substitution between the products they buy in response to changes in relative prices, something the previously used Laspeyres formula did not do.[12] If the CPI was biased upward before 1999, then any study of the size of the pass-through that uses pre-1999 CPI data, such as Aaronson (2001), generates estimates of the pass-through that are potentially biased upward. Our study, which uses data for 1978–2015, is able to use the more accurate geometric mean-based

(bimonthly, full period), Buffalo (bimonthly, until 1986), Chicago (monthly, full period), Cincinnati (bimonthly, until 1986), Cleveland (bimonthly, full period), Baltimore/Washington D.C. (bimonthly, since 1995), Washington D.C. (bimonthly, until 1995), Dallas (bimonthly, full period), Denver (bimonthly, until 1986), Detroit (monthly until 1986, then bimonthly for rest of period), Honolulu (bimonthly, until 1986), Houston (bimonthly, full period), Kansas City (bimonthly, until 1986), Los Angeles (monthly, full time period), Miami (bimonthly, full period), Milwaukee (bimonthly, until 1986), Minneapolis (bimonthly, until 1986), New York City (monthly, full period), Philadelphia (monthly until 1997, then bimonthly for rest of period), Pittsburgh (bimonthly, until 1997), Portland (bimonthly, until 1986), San Diego (bimonthly, until 1986), San Francisco (monthly between 1987 and 1997, bimonthly for the rest of the series), Seattle (bimonthly until 1986 and then from 1997 for the rest of the period), St. Louis (bimonthly until 1997).

[11] See, for instance, http://www.dol.gov/whd/minwage/coverage.htm (accessed June 21, 2016).

[12] Dalton, Greenlees, and Stewart (1998) provide an overview of this change.

CPI for the second half of the period and therefore is able to generate more accurate estimates of pass-through.

The main independent variable of interest in our regression is the change in (binding) minimum wage rates. Our data on minimum wages come from various issues of the *Monthly Labor Review*, state Department of Labor reports, and, for San Francisco, San Jose, Oakland, Berkeley, Washington, D.C., and Prince George's and Montgomery counties, city and county ordinances. As indicated in Table 2, the years 1978–2015 saw 11 federal minimum wage increases, 126 binding state minimum wage increases, and 23 city minimum wage increases. Table 2 reports the month and year of passage for all of these increases.

We also include, in most of our regressions, control variables such as month, year, and a metropolitan area fixed-effects. One additional control is "CPI-All" (Urban Consumers), included to take into account various unknown determinants of FAFH CPI inflation).[13] The inclusion of the latter control variable might rob some of the influence from minimum wage changes as this control variable is affected by inflation in the FAFH sector. As will be seen, however, this does not seem to be a problem, as when CPI-All is included in our regressions it has virtually no effect on our main coefficients of interest.

The BLS generates FAFH CPI for multistate metropolitan areas by using prices from restaurants located in more than one state. For example, in the case of the New York-Northern New Jersey-Long Island metropolitan area, the FAFH CPI is constructed from prices taken from a sample of restaurants located in four states: New York, Pennsylvania, New Jersey, and Connecticut. Therefore, the FAFH CPI for this single multistate metropolitan area is potentially affected by minimum wage hikes implemented by four different states. Table 3 provides

[13] Published by the BLS and available at www.bls.gov.

information about the metropolitan areas in our sample that include territory from more than a single state.

The existence of multistate metropolitan areas provides a benefit to this study. We are able to include in our data set many more state minimum wage changes than would have been the case if, say, the New York metropolitan area only included territory from New York State alone. But we to transform a single-state minimum wage increase affecting only restaurants in one portion of in a multistate metropolitan area into a variable measuring its impact on average FAFH prices in the full metropolitan area. We will assume that a 10 percent state minimum wage hike that affects only 20 percent of the restaurants in a metropolitan area (that is, those restaurants in that state) will have an impact on prices equal to a 2 percent (10 percent × 20 percent) minimum wage hike for the whole metropolitan area. We will, then, define the "restaurant-weighted state minimum wage change" (RSMW) as,

$$(1) \qquad \Delta_t log(mw_{it}^*) = \sum_s \lambda_{ist} * \Delta_t log(mw_{ist})$$

where i is the metropolitan area, s is the state, t is the month, λ_{ist} is the proportion of restaurants from state s in month t in metropolitan area i, and mw_{st} is the minimum wage change in state s in time t.[14]

When a metropolitan area includes only a single state, λ_{ist} will equal 1 and the RSMW for any minimum wage will simply be the change in the associated state minimum wage. The

[14] For example, consider the District of Columbia in 2009. That series is composed partly of counties in Maryland, Virginia, and West Virginia. Factoring in the number of restaurant establishments in each of these subsamples of counties as a percent of the total establishments in those counties gives the following weight to apply to each state's minimum wage in order to construct the District of Columbia minimum wage variable: D.C. (0.164), Maryland (0.344), Virginia (0.471), West Virginia (0.020). Thus, if Maryland increased its minimum wage in January 2009 by 10 percent, this would be a full metropolitan area equivalent minimum wage change of 3.44% (=10% × 0.344). We tentatively propose, in this case, that a 10percent increase in the minimum wage in Maryland would have the same impact on prices in the wider District of Columbia metropolitan area as would a 3.44 percent increase in the federal minimum wage. We believe that this is the best way of addressing this complication in the price series data. As a check to our strategy, we ran our main regressions with a subsample of series that only contain data from a single state (such as San Francisco, Los Angeles, Atlanta, and Detroit). The coefficients in these regression results do not differ substantially from the ones based on the full sample.

number of restaurant establishments in the various state subsections of multistate metropolitan areas comes from County Business Patterns, while information about the particular towns and cities included in each state subsection of a metropolitan area comes from the definitions of these metropolitan areas provided by the Office of Management and Budget.[15]

An additional noteworthy characteristic of our data is that some of the price series are available monthly while other price series are only available bimonthly. (The same holds true for the data used on Aaronson [2001] and related studies.) Table 4 breaks down the total number of binding minimum wage hikes in our sample by whether the affected price series reports monthly or bimonthly observations.

As can be seen, the monthly price series has connected with them a range of federal and state minimum wage increases, but the number of monthly observations is much less than the number of observations we have for the bimonthly data. Good reason exists, then, to use the information included in the bimonthly data in this study as it permits us to take into account a far wider range of minimum wage increases. Yet, the bimonthly data is not granular enough to permit a consideration of details about the dynamic (here, monthly) impact of the pricing process set in motion by a minimum wage hike.

Our data set and approach can be summarized as follows. We estimate price pass-through due to the minimum wage by using the food away from home price index for 28 cities between 1978 and 2015. In the regressions, we also include each city's CPI-All as a control variable. Since some city data is in fact composed of information from multiple states, we incorporate

[15] The BLS's *Handbook on Methods*, Chapter 17, describes in general terms the way that they select outlets to use as their source of prices. The BLS attempts to select these outlets so they reflect where people are buying their food. We use the regional distribution of restaurant establishments as a proxy for the regional distribution of restaurant purchases. This is an imperfect proxy as regional differences in restaurant sizes and regional differences in average consumer restaurant bills might lead the distribution of restaurant purchases to vary from the regional distribution of restaurant establishments. We also used population weights in place of restaurant establishment weights, but the results we got from using population weights did not different much from what we reported in the text.

additional minimum wage changes into our analysis. We apply a weighting scheme to our minimum wage change variable that draws on County Business Pattern data on the number of restaurant establishments in each city's sample area. We use series that are reported both monthly and bimonthly. In the following section, we discuss our empirical model and present preliminary results using monthly data.

ESTIMATES OF PASS-THROUGH WITH MONTHLY DATA

Our two initial tasks are to 1) estimate the extent of pass-through and 2) discover *when* this pass-through occurs (i.e., either only contemporaneously with the imposition of the minimum wage hike or also in the months before and/or after the hike is imposed). We can accomplish both these tasks simultaneously if we limit ourselves to monthly price series only. As Allegretto and Reich (2015) note as well, the bimonthly price series are not granular enough to reveal the detailed monthly dynamics of the pass-through process and so we temporarily set the bimonthly series aside. The downside of this approach is that we are only able to consider the impact of 82 of the 354 minimum wage hikes appearing in our full sample (see Table 4) and limit ourselves to using less than half the total observations that we have available.

The subsample used in this section comes from the three metropolitan areas (New York, Chicago, and Los Angeles) that have monthly data for the entire period and from three additional metropolitan areas (San Francisco, Philadelphia, and Detroit) that have monthly data for some subset of the period 1978–2015. Monthly observations were reported for San Francisco between 1986 and 1998, for Philadelphia before 1998, and for Detroit before 1987. We do not use the bimonthly data from these metropolitan areas from outside these years. Together, these metropolitan areas account for only about 20 percent of all federal-level minimum wage increases and about 30 percent of all state-level minimum wage increases in our sample.

We estimate the equation below, which has Food Away from Home (FAFH) inflation as the dependent variable and, as independent variables, the weighted log difference in the minimum wage *mw** (defined in Equation [1]), overall metropolitan area CPI inflation, along with metropolitan area, month, and year fixed effects as independent variables:

$$(2) \qquad \Delta \log(FAFH)_{it} = \alpha + \sum_{t=-4}^{4} \beta_t \times \Delta\log(mw_{it}^{*}) + \theta \times \log(cityCPI)_{it} + c_i + \epsilon_{it}$$

This regression includes leads and lags of four months as we want to capture the impact of a minimum wage hike on prices in the months both preceding and following the month on which a minimum wage hike is implemented. City-level fixed effects (c_i) absorb time-invariant unobserved heterogeneity in FAFH inflation between different cities, and city-level inflation is included as a control. Controls for month and year are included as well.

Table 5 reports our findings. As we go from regression 1 to regression 3, we add month and year dummies along with the metropolitan area's overall CPI as controls. Regression 3 is used as the basis for the discussion below.

In regression 3 the contemporary elasticity is 0.039, a value that is statistically significant at the 99 percent confidence level. We also get a statistically significant negative coefficient four months before the minimum wage is imposed, but no other coefficients achieve statistical significance in either regression 2 or 3.[16] According to the monthly data, then, a minimum wage hike leads to a price increase *only* in the month it is imposed. In that month, a 10 percent increase

[16] The finding that only a single lead or lag in regressions 2 or 3 achieves statistical significance is evidence against the potential claim of endogeneity—i.e., that minimum wage policy is partly a response to inflation. Because the dependent variable is the percentage change in FAFH prices, a potential endogeneity problem reflects the idea that minimum wage hikes occur during periods of *escalating* inflation. The fact that the majority of coefficients for the leads and lags are not statistically significant from zero indicates that this sort of endogeneity is not an issue in our regressions.

in the minimum wage is associated with a 0.39 percent increase in the FAFH CPI. We also find that prices also grow slower four months ahead of a minimum wage hike, as indicated by the statistically significant (p-value of 0.015) coefficient of -0.014 for T-4. When we take into consideration the net effect on prices over the 9-month period centered on the minimum wage hike, we find a 10 percent increase in the minimum wage leads to a net increase in FAFH CPI of 0.25 percent.[17]

These findings are different from what Aaronson (2001) reported. For instance, he reports statistically significant price increases in the month before and the month after a minimum wage hike is imposed whereas we find no such effect in those months. Aaronson also reports a much larger pass-through effect than we do: he finds that in the 9 months surrounding a minimum wage hike a 10 percent increase boosts prices by 0.67 percent.[18] Our finding of 0.25 percent is less than half of what Aaronson found. We will defer further comment on these differences until we discover what our full sample (including both monthly and bimonthly data) says about these differences.

We have one interesting finding in common with Aaronson (2001): we both find a statistically significant negative coefficient four months in advance of a minimum wage hike. The elasticities we find are nearly identical, −0.014 for us and −0.013 for Aaronson.[19] That prices grow slower in advance of a minimum wage is hard to square with a perfectly competitive setting, in which businesses only respond to actual changes in costs. Further, that an anticipated increase in future costs might lead to a moderating of price increases ahead of this increase is quite interesting and we can only speculate about the mechanism behind this behavior. If this

[17] However, note that this effect is not statistically significant.

[18] Aaronson (2001, Table 4, regression 2)

[19] Aaronson (2001,Table 4, regression 2). Aaronson has little to say about this statistically significant coefficient.

finding—of slower growth in prices in advance of a minimum wage increase—is confirmed by regressions using our full sample, one implication might be that studies of the impact of the minimum wage (either on prices or even on employment) that limit their focus to a couple of months before and after the minimum wage hike might be missing part of the response they are trying to measure. This was one of the major claims made by Allegretto and Reich (2015) as well, in their recent discussion of the price pass-through literature.

USING INTERPOLATED DATA

We now join our monthly and bimonthly series to create a larger single data set. By combining these two types of data, we expand the number of minimum wage changes we account for from 82 to 354. The first step is transforming, through a process of interpolation, the underlying bimonthly data into monthly series before that data is log-transformed and joined with the log-transformed values of the monthly series. The combination of data increases the number of observations from 1,852 to 8,124.[20]

In much of the econometric literature, interpolation involves creating monthly data from quarterly data or creating quarterly data from yearly data (Gordon and Krenn 2010).

[20] The 8,124 observations include 1852 monthly observations, 3,136 bimonthly observations, and 3,136 interpolated "observations." (Technically, the latter are not observations as they have been partly generated from our bimonthly data.) The degrees of freedom used to calculate standard error in regressions using this data will be less than the number of observations. In general, the degrees of freedom are equal to the number of independent pieces of information that goes into the estimation of a parameter. Some of our interpolated data are not independent, as they have been generated from a linear combination of the bimonthly data on either side of it and, so, such interpolated data do not add independent information. However, *some* of our interpolated data might be seen as adding new information. For instance, when we generate a monthly observation for January by interpolating bimonthly FAFH data for December and February, in some cases we add to this observation new information, for instance that a minimum wage hike occurred in January. Arguably, the latter type of interpolated data does add some new information, and so it might be seen to add an additional degree of freedom to our regression procedures. Yet, this new information is embedded in some not-new information (the interpolated part). We take the conservative approach by assuming that none of the interpolated data contribute degrees of freedom to our estimates of standard errors. So, for instance, if a regression uses the largest data set (8,124 observations) we will use 4,988 (=1852 + 3136) as the starting point for our determination of the degrees of freedom for the standard errors for the coefficients for these regressions.

Interpolation often involves using related higher frequency data to inform the process (e.g., Chow and Lin [1971]). In our study, the frequency change is much smaller (from bimonthly to monthly), and we transform the data in a setting in which no related higher frequency data exists. Therefore, we interpolate by simply averaging the neighboring bimonthly data and, where appropriate, splicing information about the minimum wage hikes that occurred (contemporaneously, with leads or with lags) onto the interpolated monthly series.

Any interpolation process creates something akin to measurement error in the resulting interpolated data points. In our case, by interpolating values for some metropolitan areas for FAFH CPI and City CPI-All, we must treat the dependent variable and one independent variable as if they were measured with error. This raises the possibility that both the coefficients and standard errors produced by regressions using this data are biased. The precise nature of these biases will depend, of course, on the nature of the measurement error and the particular estimation technique used. We will consider each in turn.

Interpolation will likely generate "pseudo-measurement" errors for FAFH CPI that are positive both for the month preceding a minimum wage hike (T − 1) and for the month following such hikes (T + 1). Interpolation will also likely generate pseudo-measurement errors that are negative for the month of a minimum wage hike. The argument that the pseudo-measurement errors have these signs (on average) is simple. First, we assume that the impact of minimum wages on prices in a metropolitan area is unrelated to whether the BLS collects monthly or bimonthly FAFH CPI data for that metropolitan area. If that is the case, we can use the results of our monthly regressions above to say that in metropolitan areas that collect bimonthly data, minimum wage hikes lead to increases in prices on the month of the hike but not in the month before or after.

The upper half of Figure 1 portrays a stylized pattern of FAFH prices when a minimum wage hike is imposed in a particular metropolitan area. In this figure, we presume prices grow smoothly except for in the month of the minimum wage hike (on month 0), when it jumps up due to the minimum wage. We identify four of the actual prices as a, b, c, and d. But suppose that the BLS collects data on a bimonthly basis in the metropolitan area, and does so on month −2, month 0, month +2 and so on. That is, the price data collected includes a and c (but does not include b). The data for b must be estimated from the known data a and c. If we linearly interpolate between a and c (indicated by the plus sign) we can see our interpolated value for b, the price level at month −1, to exceeds the actual data point b. As a result of this, the growth rate in FAFH prices from month −2 to month −1 generated from this interpolated data will be larger than it really is while that from month −1 to month 0 will be smaller than it really is. If, on the other hand, we have bimonthly data for months −1 and +1, then the interpolated data point for month 0 will be lower than it really is, and as a result the growth rate of FAFH prices from −1 to 0 will be lower than it really is and from 0 to +1 the growth rate of prices will be higher than it really is. If we have a mix of the two types of bimonthly data, and generate a monthly series for the growth of FAFH prices, then this will tend to create, in regressions that use this interpolated data, upward biases for the coefficients for T − 1 and T + 1 and a downward bias for T = 0. Interpolation, when prices do jump on the month of a minimum wage increase, shifts the apparent price increases away from the month in which it was imposed onto both the month before and the month after. The same shifting, for the same reason, will occur from T − 4 to T − 3 because of the positive coefficient for T − 4 in the monthly regressions above.[21]

[21] Pseudo-measurement errors might also be correlated with our monthly dummies because of predictable seasonal movements of prices. If prices typically grow rapidly in, say, April and we interpolate between February and April CPI data points then the interpolated value for March will tend to be greater than it really is as will the resulting value for the grow rate of prices in March. Similarly, the growth rate of prices between March and April,

We now turn to the second issue: the impact of the interaction between the particular data we use in this study and the particular estimation technique we use. We gain insight into the consequences of interpolating the bimonthly data by, again, making use of our monthly data. We note that how restaurants respond to minimum wage hikes should not depend on whether the BLS generates monthly or bimonthly FAFH CPI series for their metropolitan area. This suggests the following experiment: for the metropolitan areas that do have monthly data, we can simulate what the data would have been if it actually had been collected bimonthly and then use this data to run our regressions. We can then compare the regression results generated from this simulated bimonthly data with the results produced by the true monthly data. The differences we discover in this experiment using fabricated bimonthly data should be transferable to metropolitan areas for which we have only bimonthly data.

We then return to the six series for which we have full monthly data, deleting half of each city's FAFH and CPI-All observations, and then linearly interpolating each series to create observations to replace those we deleted. For half of the series we delete the December/February/April/… FAFH price index observations, and for the other half we delete the January/March/May/... observations. We then logged and first-differenced each of the fabricated bimonthly (with interpolation) series to obtain our measure of inflation, and estimated a regression model based on Equation (2).

Regression 4 in Table 6 reports the result of using the fabricated bimonthly (with interpolation) data. As predicted above, interpolation spreads out the contemporaneous impact of the minimum wage hike to the month preceding and the month following the hike. As we move from regression 3 (from Table 5) to regression 4, the contemporaneous impact falls from 0.039

using the interpolated data, will be downward biased. If this seasonal issue does occur, our monthly coefficients might be systematically biased. But this additional factor does not affect the estimated coefficients for the variables of interest to us in this study and, so we ignore it here.

to 0.021 while the coefficients for T − 1 and T + 1 rise (and achieve significance or near-significance). The sum of the coefficients for T − 1 to T + 1 is identical in regressions 3 and 4. Once we get to the sum of T − 4 to T + 4, that for regression 4 does exceed that for regression 3 but this increase is due mostly to what happened for T + 4. In most, but not all, cases the standard errors fell but the magnitude of these changes were not large enough to (alone) cause estimated coefficients to achieve significance.[22]

In summary, interpolation in the context of this study tends to reduce the estimated contemporaneous price increase, shifts some of the contemporaneous impact to the months before and after the minimum wage hike, and should be assumed to reduce standard errors. Still, when interpreted carefully, a regression using some interpolated data does provide useful information about the total effect of minimum wage hikes on the FAFH CPI.

Although we cannot say for sure what caused Aaronson (2001) to find statistically significant increases in prices in month before and after minimum wage hikes, the above discussion about the impact of interpolation suggests that Aaronson's results were at least partly (and maybe fully) due to his use of interpolated bimonthly data for the majority of the series he used.

For comparison, regression 5 in Table 6 presents the results using data coming only from those metropolitan areas for which the BLS generates bimonthly price data. No monthly data were used. The regressions were generating from series using bimonthly (with interpolation) data. For some cities, the BLS releases their FAFH price index on a January/March/May/… cycle, while others follow the alternate cycle of December/February/April/…. In order to estimate elasticities using these series, we linearly interpolated the original FAFH price index as

[22] The reason why not all standard errors fall is because we use Huber-White robust standard errors, which (by correcting for arbitrary forms of heteroscedasticity) may end up increasing or decreasing standard errors. When Huber-White standard errors are not used, all standard errors due to interpolation are lower than the baseline case.

well as the city CPI-All. This new series, now made up of a combination of the actual bimonthly data and data interpolated between the bimonthly data, was logged and first-differenced to construct the measure of FAFH inflation that serves as our dependent variable.

The results seen in regression 5 are very similar to those seen in regression 4, but with greater significance on certain coefficients possibly due to the higher number of observations used to estimate regression 5. One difference seen is that the slowdown in the price increase (ahead of the minimum wage hike) shifted forward one month to $T - 3$. The various sums of coefficients are very similar to those found in regressions 3 and 4.

The results of regression 5 are exactly what one would expect if the true underlying monthly data (if it existed) were just like that which generated the results in regression 3. When properly interpreted, the results of regressions using interpolated data give insight into the impact of minimum wage hikes on prices. We turn next to combining monthly and bimonthly (with interpolation) data to consider the impact of minimum wage hikes along with other issues relevant to policy design.

MAIN RESULTS: HOW DO PRICES RESPOND? ARE THE RESULTS CONSISTENT WITH PERFECTLY COMPETITIVE LOW-WAGE LABOR MARKETS?

We now pool together monthly and bimonthly (interpolated) data for the 1978–2015 period. Table 7 presents the results. We focus on the results of regression 7, which includes City CPI-All as a control.

According to regression 7, a 10 percent increase in the minimum wage boosts prices by 0.45 percent in the three months centered on the month the hike is imposed. However, based on the discussion in the previous section, we can say that regression 7 likely overstates the size of the price increases on the month before and after the minimum wage hike is imposed and understates the size of the price increase on the month the hike is actually imposed, though the sum of these coefficients likely does indicate the full impact of these three months. The sum of the coefficients [T − 1, T + 1] in this regression, 0.045, is almost identical to that found in regression 3 (which used only monthly data).[23]

As before, we also find minimum wage hikes lead restaurants to moderate their price increases 3 to 4 months ahead of the hike. In regression 7, the coefficients for T − 3 and T − 4 are both negative and statistically significant. A portion of the price decline assigned to T − 3 in this regression is likely due to a shifting of price increases occurring in T − 4 by the process of interpolation. The sum of the coefficients for these two months is 0.015, which is identical the sum of coefficients of the same two months in regressions 3 and 5.

The *total* effect of minimum wage hikes in the nine months centered on the month the hike is imposed is 0.036, a number close to that seen in regression 5 but somewhat larger than seen in regression 3. So, considering the full period over which a minimum wage affects prices, we find that a 10 percent increase in the minimum wage leads to a 0.36 percent net increase in prices. That is, if a $10.00 item experienced this average price increase, it would become a $10.04 item.

The size of the price increase (and so the implied welfare loss to consumers) we find is lower than previously reported: Aaronson (2001) reports a 10 percent increase in the minimum wage causes a net 0.67 percent increase in the nine months centered on the month the minimum wage hike is imposed.[24] We find a price increase for the same period close to half of that

[23] Although the interpolation process generates standard errors that are biased downwards (as discussed above), the p-values for most of these coefficients in regression 7 are so small that it is hard to believe that the reported statistical significance was due simply to interpolation.

[24] Aaronson (2001, Table 4, regression 2, p. 162).

reported by Aaronson (0.36 percent vs. 0.67 percent), and so our findings suggest a lower welfare loss to consumers following a minimum wage hike.

The importance of our findings goes beyond finding a reduced welfare impact on consumers when a minimum wage hike is imposed. Building on a set of reasonable assumptions about the operation of restaurants in a hypothetical perfectly competitive market, Aaronson and French (2007) argue that restaurants in perfectly competitive markets will fully pass through any increase in the minimum wage and that the full pass-through elasticity will be equal to approximately 0.07. Since they find, in various regressions, elasticities near 0.07, they conclude that low-wage restaurant labor markets are best characterized as perfectly competitive. The implication of being in a perfectly competitive market is that any minimum wage increase will reduce employment.

However, we get results *inconsistent* with highly competitive low-wage labor markets in the restaurant industry: our elasticity of 0.036 for the nine months centered on the month of a minimum wage hike and of 0.043 for the much narrower period of $[T - 1, T + 1]$ fall short of the 0.07 Anderson and French (2009) argue is consistent with perfect competition. However, our finding that the pass-through falls short of that implied by perfect competition does not provide positive support for any particular alternative structure of low-wage labor markets. In the next section, we consider whether the data we have provide positive support for one alternative labor market structure, monopsonistic competition.

MONOPSONISTIC COMPETITION IN LOW-WAGE LABOR MARKETS: THEORY AND EVIDENCE

Monopsonistic competition has been offered in recent years as an alternative model for *some* labor markets.[25] Most notably, Card and Krueger (1995) proposed that monopsony-like conditions in low-wage labor markets might explain their finding that minimum wages increased employment. Since then, Burdett and Mortensen (1998), Bhaskar and To (1999), Bhaskar, Manning, and To (2002) have proposed different causes for imperfect competition on the buyer-side of labor markets, and developed formal models that drew out the potential consequences of monopsonistic competition. All of these formal models of monopsonistic competition, however, generate results that are consistent with Stigler's (1946) observation of the impact of a minimum wage when businesses have market power in labor markets: the impact of a minimum wage on employment (and so on output prices) is context dependent. More narrowly, Stigler pointed out that when employers had power over wages, a small rise in a minimum wage generates increased employment (and, implied by this, increased output and reduced prices) while a large increase in the minimum wage reduces employment (and, by implication, reduces output and raises prices).

This is seen in the standard model of monopsony in the labor market. The monopsonist has market power and, therefore, faces an upward-sloping labor supply curve. To attract more workers, the monopsonist needs to increase the wage, which necessitates increasing the wages of those already hired. This implies the marginal cost of labor for the monopsonist is greater than the wage, and so the marginal cost of labor curve is upward sloping and rises faster than the labor supply curve.

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[25] Few argue that pure monopsony in labor markets has been found outside of a few unusual labor markets (for instance, in the market for professional baseball players in the United States before the ending of the reserve clause). Many economists, however, persist in using the term *monopsony* as shorthand for monopsonistic competition. Bhaskar, Manning, and To (2002) review the empirical work associated with monopsonistic competition, while Staiger, Spetz, and Phibbs (2010) show strong evidence of monopsonistic competition in the nursing labor market.

In Figure 2A, the equilibrium wage for the monopsonist, in the absence of a minimum wage, is at W_m while employment stands at L_m. This equilibrium wage is below what it would have been in a perfectly competitive setting, W_{pc}.

Figure 2B shows the impact of a "small" minimum wage increase. Suppose, just for the sake of convenience, that initially the minimum wage stood at W_m. Next, suppose that a new minimum wage is implemented and the size of the increase is small. The new minimum wage is established at W_{smw}, which stands above W_m but below W_x, where labor supply equals labor demand. The marginal cost of labor now includes the horizontal solid line starting at W_{smw}. The new marginal cost curve will induce the monopsonist to expand employment up to L_{smw} as each worker below that level of employment will now have a marginal cost below his/her value of marginal product (given by the labor demand curve). As drawn, the small increase in the minimum wage will increase employment (that is, $L_{smw} > L_m$). In turn, this increased employment will (given plausible assumptions) lead to higher output (at least in the short-run) and, so, will lower prices.

Figure 2C shows the impact of a "large" increase in the minimum wage. With a large increase, the minimum wage pushes the wage from W_m to above W_x , and employment falls as $L_{lmw} < L_m$. Under reasonable assumptions, this decline in employment is associated with a decline in output and an increase in prices.

This context-dependent nature of the impact of minimum wage hikes on employment, output, and prices within monopsony (or monopsonistic competition) contrasts starkly with the prediction of a model of perfect competition. In perfect competition, an increase in the minimum wage—no matter what its size—will lead to a price hike that fully passes along the higher labor costs onto consumers and will cause lower employment and output. Further, the perfectly competitive labor market model gives no reason to suppose that the wage-price elasticity would vary systematically with the size of a minimum wage change: the wage-price elasticity associated with a small minimum wage increase should not systematically differ from the wage-price elasticity associated with a large minimum wage increase.

Based on this second observation—that the effects of the minimum wage in a perfectly competitive labor market should not vary depending on the size of the increase—we implement a rough test of the claim that low-wage labor markets in the restaurant industry are best characterized this way by seeing whether small increases in minimum wages have a different effect on FAFH prices than large minimum wage increases. We separate the minimum wage changes in our sample into two groups, small and large increases depending whether the minimum wage change is below or above the average minimum wage increase in our sample, 6.8 pecrent. We cannot be sure, of course, that this average is close to W_x in our diagram.

Table 8 (regression 8) presents a regression based on these two types of minimum wage changes, small and large. The standard controls from regression 7 are used in this regression as well.

As can be seen, for the small minimum wage hikes a single coefficient achieves statistical significance, that for [T − 4], and this coefficient is negative. The sum of coefficients for the months immediately surrounding the small minimum wage increase, [T − 1,T + 1], is also negative although statistically insignificant. The sum of coefficients for the full nine-month period surrounding small minimum wage hike, [T − 4,T + 4], is negative and statistically significant. In contrast to the small increases, the coefficients for large minimum wage hikes are statistically significant and positive for all of T, [T − 1,T + 1], and [T − 4,T + 4], with elasticities that closely match the results reported for the full data set in Table 7.This finding—that small minimum wage hikes fail to increase prices and, indeed, appear to cause prices to fall—is inconsistent with the perfectly competitive model. On the other hand, these findings are consistent with a model of monopsony or monopsonistic competition.[26] This finding, however, should be viewed with some degree of caution because of the effect that interpolation has on standard errors, thus possibly causing us to reject null hypotheses more often than is warranted. In summary, while regression 7 provides evidence against perfect competition in low-wage labor markets, regression 8 provides evidence that such labor markets are either monopsonistic or monopsonistically competitive.

POLICY CONTEXTS MATTER, SOMETIMES

Minimum wage policies differ along many dimensions. Consider the competitive context. Most previous studies have either assumed or neglected to explore whether federal, state, and local minimum wage hikes all have equal effects on prices and employment. Most national level studies treat all minimum wages—city, state, or federal—as if they had the same impact on prices, as measured by elasticities. State- or city-level studies similarly assume that their results can be generalized to other minimum wage hikes. But the equivalency of federal, state, and city minimum wage hikes must be tested and not merely assumed. The most obvious potential difference between federal, state, and local minimum wage hike is the competitive context. For instance, we might treat a *federal* minimum wage hike, as far as the restaurant industry goes, as if it was implemented in a closed economy: cross-national trade and capital mobility relevant to the restaurant industry is relatively unimportant. On the other extreme, we might treat a *city* minimum wage increase as if it occurred in an open economy: the movement of restaurants and

[26] While our results are consistent with either monopsony or monopsonistic competition, we follow Bhaskar and To (1999), who argue that the latter is a more realistic model of unskilled labor markets.

customers across the city boundary to or from a neighboring area could be large enough to affect the magnitude of the price increase seen in the area implementing a minimum wage hike.[27]

A second dimension is timing. A law could provide for a single, large increase in the minimum wage or it could provide for a series of smaller, annual increases with no ending date. The former was common for most of the history of the minimum wage, while the latter are becoming increasingly common at the state and local level. The impact on prices and employment of these two laws *might* be different. The latter type of law—implementing a perpetual series of possibly small annual increases—permits more long-term planning by businesses and that, in turn, might lead to different consequences for prices, employment, and output. In addition, as indicated above, a small increase in a minimum wage appears less likely to generate higher prices, and by implication lower employment, than a larger increase—suggesting that indexation might be an effective means of reducing pass-through and other effects.

In this section, we first consider whether the competitive context matters for the level of pass-through. We then consider whether timing has systematic effects on the level of pass-through.

COMPETITIVE CONTEXT: FEDERAL VS. STATE VS. CITY MINIMUM WAGE HIKES

A minimum wage hike might induce cross-border movement of restaurants. If the cost of capital mobility is low, some restaurants might exit the area increasing the minimum wage as

[27] Restaurant meals are much closer to a pure service then they are to a tradable good. While home delivery of meals can cross borders (city, state, or even international) much like a good, the delivery area is typically quite small. Similarly, customers can, and do, travel many miles for restaurant meals (perhaps, again, crossing borders), but typically the distance travelled is far shorter than a good would be if shipped across a border. Our discussion of the impact of a minimum wage hike on prices is therefore only relevant to an industry like the near-pure-service restaurant industry, and not necessarily relevant for minimum wage hikes that affect goods-producing industries.

they seek higher profits in an area that has not increased its minimum wage. Similarly, if the cost of transportation for consumers is low, and restaurant prices grew as a response to a minimum wage hike, some consumers might seek now-relatively cheaper restaurant meals outside the area that had boosted its minimum wage.[28]

The joint effect of these two processes on prices will be ambiguous. When capital mobility cost and transportation cost (relevant to consumers) are both low, the exit of restaurants should shift the supply curve for restaurant meals upward (that is, further than caused by the minimum wage hike alone), while the now-available relatively lower-cost restaurant meals in other areas should cause the demand for restaurant meals (in the area that imposed the new, higher minimum wage) to become more elastic than previously was the case. We cannot say, then, that the price rise following a minimum wage hike will be larger or smaller when the costs of capital mobility and transportation are low. We cannot know a *priori* the net effect of the consequences of cross-border movement of businesses and of customers; it is an empirical matter.

The costs of capital mobility and consumer transportation should be highest in the case of a federal minimum wage hike. The average restaurant or consumer in the United States will likely perceive the cost of moving to another country, seeking to open a new restaurant or seeking a relatively cheaper restaurant meal, as being prohibitively high. On the other extreme, some restaurants operating in a city that has increased its minimum wage might possibly believe the cost of moving outside the city is low enough to make such a move reasonable. Similarly, some consumers who normally buy a restaurant meal within the city raising its minimum wage

[28] Cross-border movement of labor is also possible but we believe such movement would have only a small effect, if any, on output prices following a minimum wage hike as the minimum wage would keep an influx of workers from pushing down wages.

might seek lower-cost meals outside the city. The state case might be between the federal and the city case.

Regression 9 in Table 9 shows the results of separating federal, state, and city minimum wage hikes. If the combined effects of capital mobility and transportation costs systematically vary between federal, state, and city minimum wages, we might see different elasticities for the three different types of minimum wages.

This regression fails to provide evidence that federal and state minimum wage hikes have differing effects on restaurant prices. While the effect, for all periods from T to [T − 4,T + 4], of federal minimum wage hikes on prices is larger than that of state hikes, none of these differences achieve statistical significance (according to F-tests). Noteworthy, however, is that the estimated effect of state minimum wages on prices is smaller than the federal impact as this is contrary to what would be the case if businesses fled states that imposed minimum wage hikes.[29] Further, regression 9 reveals that although the total effect of a federal minimum wage hike on prices over the period [T − 4, T + 4] is positive, this effect is not statistically significant due to the statistically significant negative effect on prices three and four months ahead of the federal minimum wage hike. We do not see such negative effects on prices in the case of state minimum wage hikes.

The results of regression 9 suggest that city minimum wage hikes differ from both federal and state hikes. The total impact on prices of a city hike over [T − 4,T + 4] is not only much larger than that seen in the case of federal and state hikes (0.086 vs. 0.033 and 0.025), the

[29] A possible criticism of this analysis is that for some minimum wage increases we will not be able to capture evidence of business flight because the affected series samples from several states. Thus, for the New York City price series, a firm affected by a minimum wage increase in New York may move to a part of New Jersey that is still sampled in the New York City price series. Thus, no effect would be registered in the New York City series. To address this criticism, as a robustness check we ran a second regression that restricted our sample to series that only contain samples from a single state. The results of this second regression did not differ much from that reported above.

positive impact on prices is more spread out over the months surrounding the month on which it was implemented. Whereas the federal minimum wage hike is associated with negative effects on prices in $T - 3$ and $T - 4$, a city hike is associated with positive effects on these months. While we can't be sure what the exact causes are for these higher elasticities for city minimum wage hikes, they are consistent with the exit of businesses from a city that has implemented a minimum wage hike. However, we are hesitant to draw out too much from these city results because they are dominated by minimum wage hikes implemented in San Francisco which, as we discuss below, is a special case and might require somewhat that we use a somewhat different methodology to discover the true impact of city minimum wage hikes on output prices. We will hold off, therefore, on any firm statements about whether city minimum wage hikes truly have greater impact on prices, and also about whether these results are or are not consistent with the exit of restaurants from cities implementing minimum wage hikes.

One tentative conclusion does seem appropriate: the above results suggest that it might be wrong to presume that federal, state, and city minimum wage hikes all have the same effect on prices. Both the size of the effect and how price increases and decreases are distributed over time might differ between federal, state, and city minimum wage hikes.

SPECIAL CASE: CITY MINIMUM WAGE HIKES

The results of regression 9 indicate that city-level minimum wage hikes are different cases than federal or state hikes and that perhaps we need to use a different approach to study city minimum wage hikes. In this section, we outline an approach that compares price changes in those cities that experienced a minimum wage change to a reference group of cities that did not experience a minimum wage increase in that same month.

Two series in our sample implemented their own minimum wage laws: Washington, D.C., and San Francisco. We have good reason to believe that San Francisco represents a unique case that requires special treatment. San Francisco is the only city that has indexed its minimum wage increases to yearly increases in the *local*—i.e., city—CPI, making wage-price elasticities especially difficult to estimate because of the potential two-way influence between minimum wage hikes and city inflation. Furthermore, a strong housing market, a robust tourism industry, and the rise of Silicon Valley have all led to unusually high rates of increase in the cost of living and in restaurant prices in particular in the San Francisco area. On top of that, in 2008 San Francisco implemented a health care ordinance that directly increased the costs of the restaurant industry, and this policy possibly had its own effect on restaurant prices in the city by further increasing labor costs. One could also argue that Washington, D.C., is also unique for its tourism industry, presence of a large group of young professional workers and public officials, and overall strong demand in the restaurant industry.

For these reasons, we adopt an "event study" approach where we compare FAFH inflation in these two cities in the month of a minimum wage increase to the average FAFH inflation in all other cities that did not see a minimum wage increase in that month. The "events" include all the months of minimum wage increases in both cities plus the months in which there was a change in costs in San Francisco due to the health care ordnance (the first increase was in April 2008, with subsequent increases in January of each year—these increases are thus added on top of the yearly increases in the minimum wage). Previous studies of citywide mandates have used a similar approach where it is convenient to compare a single case to a plausible reference group (Allegretto and Reich 2015; Dube, Naidu, and Reich 2007; Colla et al. 2014). The pass-through effects are modeled in Equation (3) below, with dummy variables mw_change_{it}

indicating the month *t* that a minimum wage affects Washington, D.C., or San Francisco, and dummy variables $mw\_reference_{it}$ indicating cities *i* that, in that same month *t*, did not experience a minimum wage increase. We include leads and lags of four months for consistency with the results reported in other tables.

(3) $$\Delta \log(FAFH)_{it} = \alpha + \sum_{t=-4}^{4} \delta_t \times mw\ change_{S.F. or\ D.C.,t} + \sum_{t=-4}^{4} \gamma_t \times mw\ reference_{it} + \theta \times \log(cityCPI)_{it} + c_i + \epsilon_{it}$$

The overall effect of the minimum wage change in a particular city can then be calculated by subtracting γ_t from δ_t for each *t*. The results are reported in regression 10.

The cumulative T − 1 through T + 1 coefficient is 0.0028 for San Francisco (p-value of 0.0624) and 0.0014 for Washington, D.C., though the latter is not significant (p-value of 0.2085). Since the cumulative coefficient for the reference group is −0.0006, this implies an overall effect for San Francisco of about 0.0031, or a 0.31 percent increase in FAFH prices relative to cities that did not see a minimum wage increase. If we compare the cumulative T − 4 through T + 4 effects instead, the effect for San Francisco and Washington, D.C., after accounting for the behavior of the reference group, rises very slightly to 0.32 percent and 0.28 percent, respectively. This suggests that there are no significant increases in the average price level in either city that occur more than 1 or 2 months outside the month of a minimum wage hike (for example, the T − 4 and T − 3 coefficients for San Francisco indicate a 0.09 percent and −0.01 percent change in the average price level, respectively).

While our results appear to be consistent with the main wage-price elasticities reported in Table 7, recall that those elasticities are based on a 10 percent increase in the minimum wage. Because of indexation, increases in San Francisco’s minimum wage have recently been much less than 10 percent (the large initial increase in January 2004 is the exception). In other words, this coefficient suggests a slightly larger pass-through effect than what was found in the main results. The lack of significance for the case of Washington, D.C., at even an alpha of 10 percent suggests that our findings for San Francisco are more a reflection of the unique aspects of the Bay Area than necessarily a response of prices to a city-level minimum wage hike.[30]

In sum, the event study approach adopted in this section suggests that the pass-through effect for city-level minimum wage hikes might vary by the city imposing the hike, and that the growth of prices even in San Francisco is not obviously greater than that seen in the case of federal or state minimum wage hikes. The slightly higher and statistically significant elasticities found for San Francisco likely have more to do with the uniqueness of the Bay Area described above. Further, by singling out the one city that has indexed its minimum wage changes to changes in the city CPI, we address concerns that indexation could lead to artificially high price increases due to a back-and-forth effect between higher labor costs passing through to higher prices, leading to higher labor costs, and so on. The lack of any sustained increases in the average price level further out (i.e., beyond one or two months before and after a minimum wage increase) suggests that indexation of the minimum wage to the local CPI did not lead to any sustained inflationary effects in San Francisco.

INDEXED VS. SCHEDULED VS. ONE-SHOT

The different competitive context discussed above—the costs associated with capital mobility and consumer transportation—does not exhaust the potentially relevant differences that

[30] Some of the coefficients outside of the $T - 1$ to $T + 1$ range are also significant, but in all of these cases, we argue that they are not economically significant. For example, the coefficient for $T - 3$ in the Washington, D.C., case is significant, but after accounting for the inflation in the reference cases for that month, the measured impact is minor.

might shape the effect of a minimum wage policy on prices or even employment. We now turn to the second dimension of recent minimum wage policy that might affect its impact: timing.

Some minimum wage laws have provided for one-shot increases, where at some future date the minimum wage is increased and the law provides for no further increases. Other minimum wage laws have provided for a series of increases, perhaps occurring for a few years, beyond which there are no additional increases. Federal minimum wage laws have been of this sort. In recent years, however, state and city minimum wage laws have provided for a different process: perpetual increases that do not end, and are (after an initial set increase) tied to some cost-of-living index.

We now take advantage of the variation in minimum wage policy caused by indexation to compare that approach to the traditional minimum wage hike—or the other popular approach of scheduling that hike across several years (the strategy adopted in most of the federal minimum wage changes, for example). Since minimum wage increases are usually not voted on or announced more than a few months before the proposed increase is planned to go into effect, more predictable changes (due to scheduling or indexation) may allow business owners to better prepare for and take account of increases in labor costs. Also, more moderate changes (due to indexation, which—after the initial large increase—generally results in smaller changes in the minimum wage) could also allow firms to more easily absorb the increase in costs. Reflecting on the previous findings that two low minimum wage changes are not the same as one high one, moderation along this dimension could temper the contemporary pass-through effect. At any rate, since the competitive model would clearly not predict any difference in wage-price elasticities across different kinds of policies, any evidence of difference may suggest the presence of noncompetitive elements.

The results are reported in regression 11 in Table 11, where we compare the cases of indexed minimum wages, excluding the indexed minimum wage changes San Francisco (for the reasons discussed above), with "one shot" cases in which the minimum wage increases a single time, as well as scheduled cases in which the minimum wage increase is spread out over a number of years. For both scheduled and one-shot cases, the sum of the $T - 1$ through $T + 1$ coefficients is significant and much higher than the indexed case. For the indexed case, the sum of the coefficients is not significant. An F-test of a comparison of the equality of coefficients across the indexed and scheduled cases provides evidence to support the rejection of the null hypothesis that the coefficients are the same. These results are consistent with our finding earlier that moderate minimum wage changes do not lead to significant increases in FAFH prices, and they provide additional evidence that indexation—if only by mandating regular, small increases in the minimum wage—may temper the pass-through effect.

Taken together, these results imply that the minimization of pass-through (and presumably employment and output) effects can be achieved through moderate increases in the minimum wage that are imposed at the state or federal level. The results from the city-level minimum wage increases suggest that cross-border competition or other factors make it more difficult for firms to adjust to changes in labor costs and at the same time make other areas of the state (or outside the city) more attractive for these same reasons, thereby causing business flight. Our results also suggest that recent attempts to regionalize minimum wage policy or schedule increases toward some upper wage limit (such as $15) without any attachment to a cost of living index may lead to larger economic effects.

SUMMARY

There are several findings in this paper. First, the impact of minimum wage hikes on output prices (more precisely, on the FAFH CPI) is substantially smaller than previously reported. Whereas the commonly accepted elasticity of prices to minimum wage changes is 0.07, we find a value almost half of that, 0.036. Importantly, the value we found, 0.036, falls far short of what would be expected if low-wage labor markets are perfectly competitive. Second, increases in prices following minimum wage hikes generally occur in the month the minimum wage hike is implemented (and not in the month before or the month after). Previous research has reported notable increases in prices the month before and the month after, but we present evidence that such a finding was likely an artifact of interpolation.

Third, the effects of federal, state, and city minimum wages on prices are not necessarily the same: the size of the effect, along with when the price effect occurs, can potentially change for these different types of minimum wage policies. Fourth, small minimum wage hikes do not lead to higher prices, and they might actually lead to lower prices. On the other hand, large minimum wage hikes have clear positive effects on output prices. Such a finding about the different effect of small and of large minimum wage hikes is consistent with the claim that low-wage labor markets are monopsonistically competitive. If such labor markets are indeed monopsonistically competitive, then small increases in minimum wages might lead to increased employment. Our study of restaurant pricing, then, indirectly addresses one of the more contentious issues associated with the employment impact of minimum wage hikes. Fifth, we find no evidence suggesting that exit of restaurants fleeing state minimum wage hikes is large enough to affect output prices

Finally, we find evidence that the particulars of a minimum wage policy (indexed, one-shot, scheduled) might affect how price changes occur within the relevant area. These results can be used to design future minimum wage policies that best temper the pass-through effect.

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Table 1 Characteristics of Minimum Wage Changes Considered in This Study

Characteristic	1978–1997 (Same as Aaronson et al.)	1998–2015	1978–2015 (Total)
Federal	8	3	11
State	25	101	126
City	1	22	23
Indexed	0	43	43
One or two in series of increases[a]	20	25	45
Perpetually scheduled	0	21	21

[a] Four or fewer consecutive yearly minimum wage increases.
[b] More than four consecutive yearly minimum wage increases (e.g., Connecticut 1999–2004; see Table 2).

Table 2 City-, State-, and Federal-Level Minimum Wage Changes Affecting Cities in Our Sample, 1977–2015

Political unit passing minimum wage Increase	Month/year of increase[a]
Federal (11 total, leading to 193 binding minimum wage increases)	1/1978, 1/1979, 1/1980, 1/1981, 4/1990, 4/1991, 10/1996, 9/1997, 8/2007, 8/2008, 8/2009
State (131 total binding minimum wage increases)	Alaska (1978, 1979, 1980, 1981)[b]
	Massachusetts (7/1986, 7/1987, 7/1988, 1/1996, 1/1997, 1/2000, 1/2001, 1/2007, 1/2008, 1/2015)
	New Hampshire (1/1987, 1/1988, 1/1989, 1/1990, 1/1991, 9/2007, 9/2008)
	Connecticut (10/1987, 10/1988, 1/1999, 1/2000, 1/2001, 1/2002, 1/2003, 1/2004, 1/2006, 1/2007, 1/2009, 1/2010, 1/2014, 1/2015)
	Maine (1/2002, 1/2003, 10/2004, 10/2005, 10/2006, 10/2007, 10/2008, 10/2009)
	Wisconsin (7/1989, 6/2005, 6/2006)
	Illinois (1/2004, 1/2005, 7/2007, 7/2008, 7/2009, 7/2010)
	Ohio[c] (1/2007, 1/2008, 1/2009, 1/2011 1/2012, 1/2013, 1/2014, 1/2015)
	West Virginia (7/2006, 7/2007, 7/2008, 1/2015)
	Maryland (1/2007, 1/2015)
	Michigan (10/2006, 7/2007, 7/2008, 9/2014)
	California (7/1988, 3/1997, 3/1998, 1/2001, 1/2002, 1/2007, 1/2008, 7/2014)
	Florida[d] (2/2005, 1/2006, 1/2007, 1/2008, 1/2009, 6/2011, 1/2012, 1/2013, 1/2014, 1/2015)
	New Jersey (4/1992, 10/2005, 10/2006, 1/2014, 1/2015)
	New York (1/2005, 1/2006, 1/2007, 1/2014, 1/2015)
	Pennsylvania (2/1989, 1/2007, 7/2007)
	Delaware (4/1996, 1/1997, 5/1999, 10/2000, 1/2007, 1/2008, 6/2014)
	Washington (1/1989, 1/1990, 1/1999, 1/2000, 1/2001, 1/2002, 1/2003, 1/2004, 1/2005, 1/2006, 1/2007, 1/2008, 1/2009, 1/2011, 1/2012, 1/2013, 1/2014, 1/2015)
City/county	Washington, D.C. (10/1993, 1/2005, 1/2006, 8/2008, 8/2009, 7/2014)
	San Francisco[e] (1/2004, 1/2005, 1/2006, 1/2007, 1/2008, 1/2009, 1/2010[f], 1/2011, 1/2012, 1/2013, 1/2014, 1/2015)
	San Jose (3/2013, 1/2014, 1/2015)
	Oakland (3/2015)
	Berkeley (10/2014)

[a] In some cases, the effective month t of the minimum wage change is shifted to the following month t + 1 because the wage change did not go into effect until later in month t. We used a cutoff date of the 24th day of the month: any minimum wage change that occurred on or after that day was assumed to affect prices beginning the following month.

[b] During these years, Alaska set its minimum wage at $0.50 higher than the federal minimum wage.

[c] Starting in 2007, Ohio indexed its minimum wage to the national CPI.

[d] Starting in 2005, Florida indexed its minimum wage to the South's regional CPI.

[e] San Francisco indexes its minimum wage to the city's CPI.

[f] While the minimum wage did not increase in San Francisco this year, there was a change to labor costs due to the Health Care Security Ordinance (an employer spending mandate) that went into effect starting April 2008 (July 2008 for businesses with 20–49 employees), requiring employers to pay at an hourly rate per employee. For more information on the ordinance, see https://www.wageworks.com/media/179290/2903-SFHCSO-Compliance-Alert.pdf (accessed June 29, 2016). The change in labor costs resulting from this act has been factored into all relevant years.

Table 3 Series with Sample Areas in Multiple States

Series for the FAFH price index	Sample areas used for restaurant weights
Boston	Massachusetts, New Hampshire, Maine (starting in 1998), Connecticut (starting in 1998)
Chicago-Gary-Kenosha	Illinois, Indiana, Wisconsin
Baltimore-Washington, D.C.	Washington, D.C., Maryland, Virginia, West Virginia
New York City-Northern New Jersey-Long Island	New York, New Jersey, Connecticut, Pennsylvania (starting in 1998)
Philadelphia-Wilmington-Atlantic City	Pennsylvania, New Jersey, Delaware (starting in 1998), Maryland (starting in 1998)

NOTE: For the individual counties and towns covered each area, see the sources below. Restaurant establishment data (according to the individual county and town information) found using the County Business Patterns Census Database: http://censtats.census.gov/cgi-bin/cbpnaic/cbpsect.pl (accessed June 29, 2016).
SOURCE: "Metropolitan Areas and Components, 1998" (published through the U.S. Census), http://www.census.gov/population/metro/files/lists/historical/93mfips.txt; 1993 edition: http://www.census.gov/population/metro/files/lists/historical/83mfips.txt (accessed June 29, 2016).

Table 4 Minimum Wage Hikes by Series Periodicity

Periodicity	Observations	Minimum wage hikes			
		Federal	State	Local	Total
Monthly	1,852	40	42	0	82
Bimonthly	3,136	150	101	21	272
Both	4,988	190	143	21	354

NOTE: As noted in the text, most CPI data are reported bimonthly, either on January/March/May/etc. cycles or February/April/June/etc. cycles.
SOURCE: Bureau of Labor Statistics and *Monthly Labor Review* reports (various years).

Table 5 Estimates of Pass-Through Using Monthly Data Dependent Variable: FAFH Inflation

	(1)	(2)	(3)
Minimum wage change			
T − 4	−0.004	−0.014*	−0.014*
	(0.005)	(0.006)	(0.006)
T − 3	0.006	0.000	0.000
	(0.007)	(0.007)	(0.007)
T − 2	0.012	0.003	0.001
	(0.010)	(0.009)	(0.009)
T − 1	0.008	−0.002	−0.001
	(0.005)	(0.005)	(0.005)
T	0.052**	0.039**	0.039**
	(0.010)	(0.010)	(0.010)
T + 1	0.022**	0.008	0.008
	(0.008)	(0.008)	(0.008)
T + 2	0.012	−0.002	−0.002
	(0.007)	(0.006)	(0.006)
T + 3	0.012	−0.002	−0.004
	(0.007)	(0.006)	(0.006)
T + 4	0.010	−0.002	−0.002
	(0.006)	(0.005)	(0.005)
[T − 1,T + 1]	0.081**	0.044**	0.046**
[T − 3,T + 3]	0.121**	0.043	0.041
[T − 4,T + 4]	0.127**	0.027	0.025
City CPI-All	—	—	0.113**
			(0.031)
City fixed effects	Yes	Yes	Yes
Month, year controls	No	Yes	Yes
Observations	1,852	1,852	1,852
Cities	6	6	6
R^2	0.043	0.162	0.170
Adj. R^2	0.036	0.133	0.141

NOTE: * $p < 0.05$, ** $p < 0.01$. Regressions use monthly data from Los Angeles, Chicago, and New York between 1978 and 2015, as well as San Francisco (1987–1997), Detroit (through 1986), and Philadelphia through 1997. The T − 4 coefficient indicates the partial effect of the minimum wage change on FAFH inflation four months prior to the date of the minimum wage change. Standard errors corrected for arbitrary forms of heteroscedasticity are reported in parentheses.
SOURCE: Bureau of Labor Statistics, *Monthly Labor Review* reports (various years).

Table 6 Illustrating the Effect of Interpolation Dependent Variable: FAFH Inflation

	(4)	(5)
Minimum wage change		
T − 4	−0.012 **	−0.007
	(0.004)	(0.003)
T − 3	0.001	−0.008 **
	(0.004)	(0.003)
T − 2	0.005	−0.003
	(0.005)	(0.003)
T − 1	0.010	0.013 **
	(0.006)	(0.004)
T	0.021 **	0.017 **
	(0.007)	(0.005)
T + 1	0.015 *	0.015 **
	(0.007)	(0.005)
T + 2	−0.003	0.002
	(0.005)	(0.004)
T + 3	−0.006	0.006
	(0.004)	(0.004)
T + 4	0.005	0.004
	(0.006)	(0.003)
[T − 1, T + 1]	0.046 **	0.045 **
[T − 3, T + 3]	0.043 *	0.042 **
[T − 4, T + 4]	0.036	0.039 **
City CPI-All	0.084 **	0.132 **
	(0.031)	(0.020)
City fixed effects	Yes	Yes
Month, year controls	Yes	Yes
Observations	1,851	6,272
Metropolitan areas	6	25
R^2	0.285	0.189
Adj. R^2	0.260	0.178

NOTE: * $p < 0.05$, ** $p < 0.01$. Regression 4 uses monthly data from Los Angeles, Chicago, and New York between 1978 and 2015, as well as San Francisco (1987–1997), Detroit (through 1986), and Philadelphia through 1997, which has been interpolated. Regression 5 uses interpolated data from all series for which bimonthly data exist and are meant to be shown for the similarities to the regression 4 results.
The T − 4 coefficient indicates the partial effect of the minimum wage change on FAFH inflation four months prior to the date of the minimum wage change. Standard errors corrected for arbitrary forms of heteroscedasticity are reported in parentheses. See Note 20 for degrees of freedom adjustment made to correct for the use of interpolated data.
SOURCE: Bureau of Labor Statistics, *Monthly Labor Review* reports (various years).

Table 7 Estimate of Pass-Through, Full Data Set Dependent variable: FAFH inflation

	(6)	(7)
Minimum wage change		
T − 4	−0.010 **	−0.009 **
	(0.003)	(0.003)
T − 3	−0.005 *	−0.006 *
	(0.003)	(0.003)
T − 2	0.000	−0.002
	(0.003)	(0.003)
T − 1	0.010 **	0.010 **
	(0.003)	(0.003)
T	0.022 **	0.023 **
	(0.005)	(0.005)
T + 1	0.013 **	0.013 **
	(0.004)	(0.004)
T + 2	0.001	0.001
	(0.003)	(0.003)
T + 3	0.004	0.003
	(0.003)	(0.003)
T + 4	0.002	0.002
	(0.003)	(0.003)
[T − 1, T + 1]	0.044 **	0.045 **
[T − 3, T + 3]	0.043 **	0.043 **
[T − 4, T + 4]	0.035 **	0.036 **
City CPI-All	—	0.130 **
		(0.017)
City fixed effects	Yes	Yes
Month, year controls	Yes	Yes
Observations	8,124	8,124
Metropolitan areas	28	28
R^2	0.170	0.180
Adj. R^2	0.161	0.171

NOTE: * p < 0.05, ** p < 0.01. Regressions 6 and 7 use the full data set (i.e., pooled monthly data with the bimonthly, interpolated, data).

The T− 4 coefficient indicates the partial effect of the minimum wage change on FAFH inflation four months prior to the date of the minimum wage change. Standard errors corrected for arbitrary forms of heteroscedasticity are reported in parentheses. See Note 20 for degrees of freedom adjustment made to correct for use of interpolated data.

SOURCE: Bureau of Labor Statistics, *Monthly Labor Review* reports (various years).

Table 8 Estimate of Pass-Through, Full Data Set
Dependent Variable: FAFH Inflation

	(8)	
Minimum wage change	Small	Large
T − 4	−(0.035) *	− (0.007) **
	(0.011)	(0.003)
T − 3	−0.011	−0.006 *
	(0.011)	(0.003)
T − 2	−0.002	−0.003
	(0.011)	(0.003)
T − 1	−0.011	0.011 **
	(0.010)	(0.003)
T	0.013	0.023 **
	(0.013)	(0.005)
T + 1	−0.005	0.014 **
	(0.011)	(0.004)
T + 2	−0.002	0.001
	(0.011)	(0.003)
T + 3	−0.015	0.005
	(0.011)	(0.003)
T + 4	−0.001	0.002
	(0.010)	(0.003)
[T − 1, T + 1]	−0.003	0.048 **
[T − 3, T + 3]	−0.033	0.045 **
[T − 4, T + 4]	−0.069 *	0.040 **
City CPI-All	0.132**	
	(0.017)	
City fixed effects	Yes	
Month, year controls	Yes	
Observations		8,124
Metropolitan areas		28
R^2		0.178
Adj. R^2		0.172

NOTE: * p < 0.05, ** p < 0.01. Regression 8 uses the full data set (i.e., the monthly data pooled with the bimonthly interpolated data).
The T − 4 coefficient indicates the partial effect of the minimum wage change on FAFH inflation four months prior to the date of the minimum wage change. Standard errors corrected for arbitrary forms of heteroscedasticity are reported in parentheses. See Note 20 for degrees of freedom adjustment made to correct for use of interpolated data.
SOURCE: Bureau of Labor Statistics, *Monthly Labor Review* reports (various years).

Table 9 Pass-Through Effects by Policy Context Dependent Variable: FAFH Inflation

	(9)		
Minimum wage change	Federal	State	City
T − 4	−0.014**	−0.001	0.008*
	(0.004)	(0.003)	(0.003)
T − 3	−0.008*	−0.003	0.008*
	(0.004)	(0.003)	(0.003)
T − 2	−0.001	−0.006	0.009
	(0.005)	(0.004)	(0.005)
T − 1	0.011*	0.005	0.004
	(0.005)	(0.003)	(0.006)
T	0.023**	0.022**	0.012
	(0.006)	(0.006)	(0.008)
T + 1	0.014**	0.010	0.014*
	(0.005)	(0.005)	(0.007)
T + 2	0.000	0.000	0.019*
	(0.004)	(0.004)	(0.009)
T + 3	0.005	−0.002	0.016
	(0.004)	(0.004)	(0.009)
T + 4	0.002	0.004	−0.004
	(0.004)	(0.003)	(0.007)
[T − 1, T + 1]	0.048**	0.036*	0.030*
[T − 3, T + 3]	0.044**	0.026**	0.082**
[T − 4, T + 4]	0.033	0.025**	0.086**
City CPI-All		0.128**	
		(0.017)	
City fixed effects		Yes	
Month, year controls		Yes	
Observations		8,124	
Cities		28	
R^2		0.181	
Adj. R^2		0.170	

NOTE: * $p < 0.05$, ** $p < 0.01$. Regression 9 uses the pooled monthly and bimonthly (interpolated) data. The T − 4 coefficient indicates the partial effect of the minimum wage change on FAFH inflation four months prior to the date of the minimum wage change. Standard errors corrected for arbitrary forms of heteroscedasticity are reported in parentheses. See Note 20 for degrees of freedom adjustment made to correct for use of interpolated data.
SOURCE: Bureau of Labor Statistics, *Monthly Labor Review* reports (various years).

Table 10 Pass-Through Effects by Policy Context Dependent Variable: FAFH Inflation

	(10)[a]		
Minimum wage change	Washington, D.C.	San Francisco	Reference group
T − 4	−0.0017**	0.0009	0.0002
	(0.0007)	(0.0006)	(0.0002)
T − 3	0.0007	-0.0001	0.0001
	(0.0005)	(0.0004)	(0.0002)
T - 2	0.0006	−0.0005	0.0001
	(0.0007)	(0.0005)	(0.0002)
T − 1	0.0004	−0.0001	−0.0003
	(0.0006)	(0.0004)	(0.0002)
T	0.0000	0.0015	−0.0004*
	(0.0007)	(0.0009)	(0.0002)
T + 1	0.009	0.0014	0.0001
	(0.0009)	(0.0009)	(0.0002)
T + 2	0.0023**	0.0004	0.0003
	(0.0008)	(0.0009)	(0.0002)
T + 3	−0.0001	0.0001	0.0001
	(0.0005)	(0.0009)	(0.0002)
T + 4	0.0000	0.0001	0.0001
	(0.0009)	(0.0008)	(0.0002)
[T − 1, T + 1]	0.0014	0.0028	−0.0006
[T − 4, T + 4]	0.0032	0.0036	0.0004
City CPI-All		0.124**	
		(0.0172)	
City fixed effects		Yes	
Month, year controls		Yes	
Observations		8,124	
Cities		28	
R^2		0.172	
Adj. R^2		0.162	

NOTE: * $p < 0.05$, ** $p < 0.01$. Regression 10 uses the pooled monthly and bimonthly (interpolated) data.
[a] Coefficients are based on dummy variables, and therefore do not measure wage-price elasticities. See text (the section titled "Special Case: City Minimum Wage Hikes" on p. 31) for details.
The T − 4 coefficient indicates the partial effect of the minimum wage change on FAFH inflation four months prior to the date of the minimum wage change. Standard errors corrected for arbitrary forms of heteroscedasticity are reported in parentheses. See Note 20 for degrees of freedom adjustment made to correct for use of interpolated data.
SOURCE: Bureau of Labor Statistics, *Monthly Labor Review* reports (various years).

Table 11 Pass-Through Effects by Policy Context Dependent Variable: FAFH Inflation

		(11)	
Minimum wage change	Indexed	Scheduled	One-shot
T − 4	0.006*	−0.014**	−0.002
	(0.003)	(0.004)	(0.003)
T − 3	0.001	−0.008	−0.004
	(0.004)	(0.004)	(0.003)
T − 2	−0.001	0.000	−0.006
	(0.005)	(0.005)	(0.005)
T − 1	0.008	0.012*	0.003
	(0.005)	(0.005)	(0.003)
T	0.011*	0.024**	0.025**
	(0.005)	(0.007)	(0.007)
T + 1	0.001	0.015**	0.014**
	(0.007)	(0.006)	(0.005)
T + 2	0.003	0.001	0.001
	(0.007)	(0.004)	(0.004)
T + 3	0.010*	0.005	−0.004
	(0.004)	(0.004)	(0.004)
T + 4	0.005	0.002	0.001
	(0.004)	(0.004)	(0.004)
[T −1, T + 1]	0.020*	0.051**	0.040**
[T − 4, T + 4]	0.044**	0.037	0.023*
City CPI-All		0.128**	
		(0.017)	
City fixed effects		Yes	
Month, year controls		Yes	
Observations		8,124	
Cities		28	
R^2		0.181	

NOTE: * p < 0.05, ** p < 0.01. Regression 11 uses the pooled monthly and bimonthly (interpolated) data. The T − 4 coefficient indicates the partial effect of the minimum wage change on FAFH inflation four months prior to the date of the minimum wage change. Standard errors corrected for arbitrary forms of heteroscedasticity are reported in parentheses. See Note 20 for degrees of freedom adjustment made to correct for use of interpolated data.
SOURCE: Bureau of Labor Statistics, *Monthly Labor Review* reports (various years).

Table 12 Tests of the Equality of Coefficients across Policy Contexts

	Small vs. large	Indexed vs. scheduled	Indexed vs. one-shot	Federal vs. state	Federal vs. state (robustness check, see notes)	S.F. vs. reference group	D.C. vs. reference group
Regression	8	11	11	9	9	10	10
p-value (equality of contemporaneous coefficients)	0.4863	0.1478	0.1071	0.9350	0.9203	0.0414	0.7256
p-value (equality of T − 1 through T + 1 coefficients)	0.0149	0.0432	0.0925	0.3838	0.1894	0.0227	0.0942

NOTE: This table reports p-values for F-tests of the equality of coefficients across different subsamples of the data. For example, the p-value of 0.0149 reported in the "Small vs. large" column indicates that when a test of the equality of the coefficients in that regression is conducted, Pr(> F) = 0.0149 and thus equality can be rejected at the 0.05 level of confidence.
In the last column, the results are from an unreported regression on a subsample of our data that includes series whose samples are only taken from a single state (unlike, say, Boston or New York City whose samples include restaurants in Connecticut and Philadelphia respectively). See Note 29.
SOURCE: Authors' analysis of results from Tables 8–11.

Figure 1 Interpolation and a Stylized Minimum Wage Hike

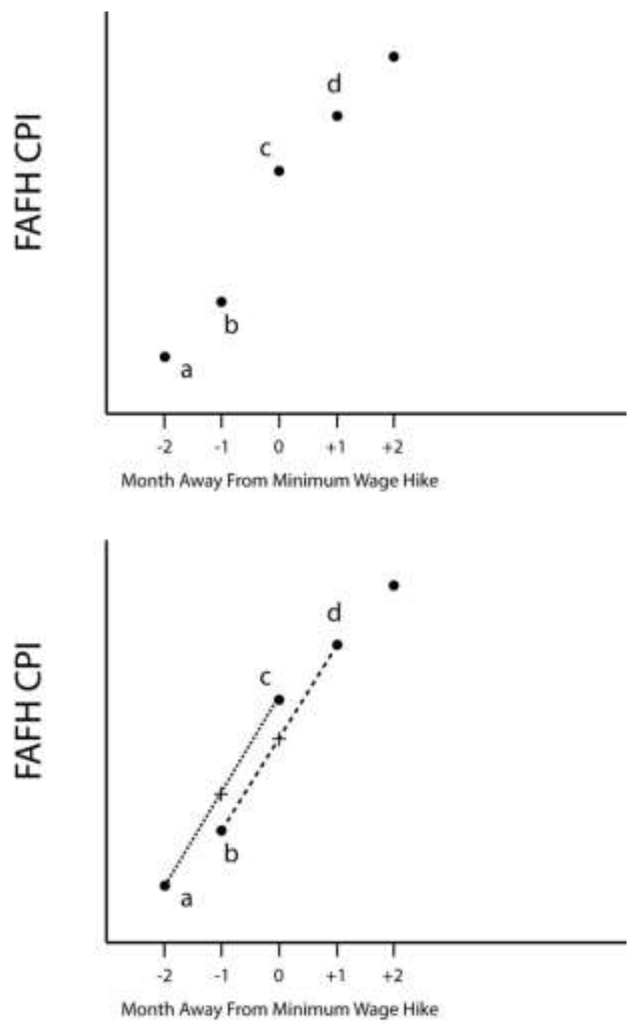

Figure 2 Impact of Minimum Wage Increase in Monopsonistic Competition