The pass-through of minimum wages into US retail prices: evidence from supermarket scanner data*

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[Preliminary and incomplete]

Abstract

We study the impact of 160 increases in state-level minimum wages on the dynamics of prices in local grocery stores. We find that the impact of raising minimum wages on prices is small, significant, and occurs mostly in the months following minimum wage legislation and preceding implementation. Our estimates of the minimum wage elasticity of grocery prices lie between 0.03 and 0.05, and are consistent with a full pass-through of cost increases. Our results suggest that the costs of minimum wage increases are borne by consumers, and in the case of grocery stores disproportionally by the poorest households. We also find that minimum wage hikes significantly raise the probability of price increases, consistent with the predictions of state-specific price setting models. We calibrate a simple menu cost model to match the long run features of our data, and find that the model can fit the short-run response to minimum wage increases.

Keywords: Minimum wages, inflation, retail prices, price dynamics, price pass through, menu cost models

JEL: E31, J23, J38, L11, L81

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1 Introduction

Minimum wage increases are very popular with voters. In an opinion poll conducted in 2015, 75% of the respondents were in favor of rising the US federal minimum wage to $12.50 (NELP, 2015). Support for minimum wage policies has also risen among economists. For instance, in 2006 a group of more than 650 economists signed a statement issued by the Economic Policy Institute that supported a raise of the federal minimum wage[1]. The increasing support reflects that a large body of research did not find the negative employment effects of moderate minimum wage hikes that are predicted by basic economic theory. However, if not low-wage employment, who is it that bears the cost of these minimum wage increases? This central question is still not answered in a satisfactory way. The limited evidence of negative effects on employment suggests that it is either firms—in the form of lower profits—or consumers—in the form of higher prices (MaCurdy, 2015; Harasztosi and Lindner, 2015).

Our paper helps to better understand the redistributive consequences of minimum wages by analyzing the response of grocery store prices to increases in local minimum wages in the US. We construct state-level price indices using high-frequency point-of-sale scanner data. These data allow us to study the dynamics of adjustment in more detail than previously possible, and separately identify significant effects at legislation and implementation of minimum wage increases. We find that the overall minimum wage elasticity of prices in grocery stores is about 0.03 on average, and 0.09 in right-to-work-states. This effect is consistent with a full, or even more than full, pass-trough of increases in grocery stores' cost into consumer prices. Households with low incomes are disproportionally affected by this response, mostly because they spend a relatively high share of their expenditures at grocery stores. Moreover, we find that pass-through starts the month that minimum wage legislation gets passed. By the time that minimum wages increases become effective, pass-through is essentially complete. Firms seem to anticipate changes in cost, and react swiftly. We also find that price increases become much more frequent, and price decreases slightly less frequent. These qualitative features are strikingly in line with the predictions of dynamic state-dependent pricing models. We calibrate such a model to match the long run moments of our price data, and show that the calibrated model predicts the short run response we estimated in the first part.

Our paper contributes to a small literature studying the effects of minimum wage increases on prices. While there is a large literature on the employment effects of minimum wages, the literature on the price effects is restricted to restaurants (Card and Krueger, 1995; Aaronson, 2001; Aaronson and French, 2007; Aaronson et al., 2008; Lemos, 2008;

[1]add citation

Fougère et al., 2010; Allegretto and Reich, 2015). Depending on the time period, location and type of restaurant, these papers find minimum wage elasticities of prices in the range of 0.04 to 0.1. Prices in grocery stores or retail more generally have not been studied so far. However, the pass-through in the groceries sector is important to judge the real redistributive effects of minimum wage increases. First, one in four grocery store employees is employed at the local minimum wage at the end of our sample in 2011. Second, households spend about 10% of their expenditure at grocery stores, compared to, for instance, 5% at restaurants.[2] Poor households spend a particularly sizeable fraction of their household income at grocery stores. For instance, the expenditure share on food at home is 13.2% for households belonging to the two lowest income deciles.[3]Third, research on the product demand elasticity of food shows that consumers can not easily avoid price increases in grocery stores by substituting to other products. Restaurants, for instance, have a higher product demand elasticity than grocery stores (Andreyeva et al., 2010). Economic theory suggests that a lower product demand elasticity magnifies the pass-through of costs into selling prices.[4] Finally, papers studying the price effects of minimum wages in restaurants are faced with the challenge of unobserved adjustments in the quality of services. In contrast, products in grocery stores are very standardized, both across regions and over time. This implies that our evidence is less susceptible to unobserved quality adjustments in the products sold.

We also contribute to the literature studying the pass-through of cost shocks into retail prices. So far, this literature has focused on the pass-through of shocks to the costs of purchases of intermediates. Eichenbaum et al. (2011) study the pass-through of product replacement cost into prices, and find that pass-through is complete but somewhat delayed. Nakamura and Zerom (2010) use variation in the market price of commodity coffee and find that the pass-through into wholesale prices is about one third, and that the increase of wholesale prices is completely passed through to consumers. The timing and magnitude of the empirical estimates are generally consistent with the predictions of state-dependent pricing models (see Klenow and Malin, 2010). In contrast, the pass-through of labor cost into retail prices has not been studied in detail at the firm level. Available evidence is mostly based on the observation that prices changes are more frequent in industries with higher labor intensity of production (Klenow and Malin, 2010) and on qualitative surveys (Bertola et al., 2012; Druant et al., 2012; Loupias and Sevestre,

[2]In the 2014 Consumer Expenditure Survey an average US household spends about 10% of for items typically sold at grocery stores (food at home, Housekeeping supplies, personal care products and services) and about 5% at restaurants (food away from home), see Table 16.

[3]

[4]The estimates presented in Andreyeva et al. (2010) suggest that the product demand elasticity of "food away from home" is 0.81, and thus higher than the elasticity of all other categories of food and beverage considered in the study. The average elasticity for the category "food at home" is 0.59.

2013). In these surveys, managers tend to respond that wages play at most a limited role in firms' price setting decisions. These statements are at odds with any pricing model we are aware of, in which there is a tight link between wages, marginal cost and prices. Minimum wages are a very salient cost shock and thus provide an opportunity to test whether state-dependent pricing models can fit the price response to labor cost shocks.

The remainder of this paper is organized as follows. The next section presents the minimum wage and price data used in the empirical analysis. Section 3 describes our empirical approach. Our main results and their robustness are presented in Section 4. Section 5 discusses what our price results imply concerning the costs and benefits of minimum wage hikes. Section 6 evaluates these results through the lense of a benchmark menu cost model. Section 7 summarizes our results and concludes.

2 Data

2.1 Minimum Wages

We study minimum wage increases during the period from January 2001 to December 2011. Minimum wages in the US are set by federal, state, and local governments. Although city and county level minimum wages have become more common recently, until 2013 only San Francisco, CA, and Santa Fe, NM, had local minimum wage ordinances. We thus focus on variation in the minimum wage on the state level. We define the binding minimum wage as the maximum of the state and federal minimum wage. At the beginning of our sample in January 2001, the federal minimum wage is binding in 40 states. In January 2007 the federal minimum wage is binding in only 21 states. In January 2011 after three federal minimum wage increases, it is binding in 33 states.

In the 41 states that are covered by our price data, there were 160 increases in the binding minimum wage in total from 2001 to 2011. Table 1 lists important features of those hikes.[5] It shows that 60 of 160 (37.5%) of all minimum wage increases resulted from federal minimum wage hikes. All increases due to federal minimum wage legislation follow from the "Fair Minimum Wages Act" of 2007, which increased federal minimum wages for the first time since 1997. The act was passed after votes in January and May 2007, and increased minimum wages in three steps: from \$5.15 to \$5.85 per hour in July 2007, to \$6.55 in July 2008, and to \$7.25 in July 2009. Those three hikes affected 16, 20 and 24 states that had not set higher state minimum wages at the time. The remaining 100 hikes were increases of states minimum wages above the federal one. Of those, 62 followed from legislative action, five were the result of ballot initiatives, and 33 resulted

[5] Table A.1 in the appendix provides further characteristics of the hikes.

from indexation. All approved ballots proposed a one time increase two months after the popular vote, followed by automatic inflation adjustments starting 12 months after the first increase. Indexation is practiced in 10 states in our sample by 2011.[6] Most of these states introduced indexation starting in 2008 after ballots held in November 2006.[7] In those states minimum wages are pegged to the national development of prices, usually the CPI for urban consumers.

Figure 1 illustrates how the 160 minimum wage events are distributed across US states. It shows that all states in our sample experience at least 2 minimum wage hikes. The maximum number of hikes per state is 9, and the average number is 3.9 (see Table 1). Table 1 also shows that more than half of all events in our sample period occur between 2006 and 2008 and that the size of the minimum wage increases varies substantially: the average raise is 8.8%, but 37 of the 160 hikes exceed 12%. Most small increases follow from indexation or federal minimum wages slightly exceeding the ones set in prior state legislation.

[Figure 1 about here.]

We collected the month of enactment for all non-indexation minimum wage increases in our data from media sources. In most cases, final votes by the legislative and executive signatures occur in the same month. In some cases, enactment was preceded by a series of negotiations, legislative votes and executive vetoes, in which case we use the final executive signature to date the month of enactment. The most important piece of legislation in our sample was the "Fair Minimum Wages Act" of 2007. This act was passed in slightly different versions with bipartisan support in both houses of congress in January 2007. After a conference committee added tax-cuts for small businesses to the bill, it was passed and signed by president Bush in May 2007. Since the passage of the actual minimum wage part of the bill seemed highly likely already in January, it is not obvious how the month of enactment should be dated.[8] We use January in our baseline and our results largely remain unchanged if we use May instead.

Most legislative minimum wage increases are carried out in steps over several years. Typically, legislation is passed three to four months before the first raise, with further increases following in steps of 12 months after the first one. On average, there is a gap of 15.5 months between legislation and the implementation of the hike. As Table 1 shows, these hikes are almost exclusively implemented either in January or July.

[6] States with indexation in our sample are Arizona, Colorado, Florida, Missouri, Montana, Nevada, Ohio, Oregon, Vermont and Washington.

[7] The exceptions are Florida, Vermont (both began indexation in 2007), Oregon (beginning in 2004) and Washington (beginning in 1999).

[8] See for instance http://www.nytimes.com/2007/05/25/washington/25wage.html. The discussion was mostly about the extent of tax breaks, support for higher minimum wages was bipartisan.

[Table 1 about here.]

2.2 The minimum wage in the grocery sector

The extent to which we can expect minimum wages to affect prices in grocery stores depends on the importance of minimum wage labor for the variable cost of grocery stores. In this section we aim to assess this importance. To do so, we decompose the share of minimum wage labor in variable cost as into (1) the share of labor cost in variable cost, and (2) the share of minimum wage labor in labor cost.

To estimate the first quantity, we divide labor costs by the sum of purchases of intermediates and labor costs of grocery stores as provided by the Annual Retail Trade Survey conducted by the census bureau. From 2001 to 2011 the share of labor cost ranges from 15.8 to 16.6%. The numbers are of roughly the same magnitudes if we consider shares of total cost instead of our definition of variable costs, since other operating expenses are quantitatively rather unimportant.

The second quantity is estimated using the CPS outgoing rotation group. We define employees at or below the minimum wage as those with hourly earnings of less than 110% of the locally binding minimum wage.[9] We then calculate the share of those workers in employment, hours and earnings at grocery stores for each month and state. Finally, we average those monthly shares across states and for three different time periods.

The results are reported in the columns termed "atorlt" in Table 2. From 2001 to 2005, minimum wage labor amounts to roughly 12% of grocery store employment, 9% of hours worked and 5% of labor cost. The share of minimum wage labor in employment increases to 19% between 2006 and 2008 and 26% between 2009 and 2011 as a consequence of the rise in the binding minimum wage over time. For hours, the share goes up to 15% between 2006 and 2008 and 20% between 2009 and 2011. The share in labor cost increases to 8% between 2006 and 2008 and 12% between 2009 and 2011. For comparison, Table 2 also shows the importance of minimum wages in retail trade as a whole (excluding grocery stores), for wholesale trade, for restaurants and for all other private sector industries. The comparison reveals that minimum wages are less important for other retail trade firms and for wholesalers than they are in grocery stores. In contrast, the prevalence of the minimum wage is two to three times higher in the restaurant industry than in grocery stores, with a share of 46% in employment, 39% in hours and 23% in labor cost between 2009 and 2011.

[Table 2 about here.]

[9] Full-time students, disabled workers, and workers under the age of 20 are covered by exceptions in some minimum wage laws. Typically these workers are subject to a different minimum wage rate that is proportional to the standard one.

These estimates may underestimate the dependence of each industry on minimum wage labor. The reason are possible "ripple effects". Ripple effects occur if minimum wages also affect earnings of workers earning above minimum wages. This could happen, for instance, if firms want to keep relative wages within a firm constant. Recent research, discussed in more detail in section 5, has provided evidence that ripple effects could be important (Dube et al., 2015; Autor et al., 2016). To provide an upper bound on the share of affected employment, hours and labor cost in the presence of substantial ripple effects, the table thus also provides the shares for workers earning less than 175% of the local minimum wage. For the three time periods, 59 to 69% of grocery store employment, 51 to 63% of grocery store hours, and 34 to 44% of grocery store labor cost accrue to workers earning within 175% of the minimum wage.

Multiplying the labor cost share of minimum wages with the labor share in variable cost of grocery stores, our calculations suggest that between 0.7% (no ripple effects) and 5% (very high ripple effects) of grocery stores' variable cost is affected by increases in minimum wages 2001 to 2005. Between 2006 and 2008 the corresponding numbers range from 1.3 to 6% and 2009 to 2011 from 2 to 7%. These estimates provide a benchmark for our estimates of the extent of cost pass-through into prices of grocery stores.

2.3 Prices

We use scanner data provided by market research firm Symphony IRI to track grocery prices in different US states. The dataset is described in detail in Bronnenberg et al. (2008). It contains weekly prices and quantities for 31 product categories sold at grocery stores and drug stores between January 2001 to December 2011. On average, the sample covers 1916 stores and 60600 unique products over this period. Stores are located in 41 states and belong to one of about 90 retailers.[10] Most of the product categories in the data are packaged food products (frozen pizza, cereals, etc.) or non-alcoholic beverages (soda, coffee, milk, etc.). The data also includes a variety of personal care products (e.g., deodorants and shampoo), housekeeping supplies (such as detergents and paper towels), as well as alcoholic beverages (beer and some flavored alcoholic beverages) and tobacco.

We calculate the average price of product i in grocery store g and week w from quantities and revenues. Stores report total revenue (TR) and total sold quantities (TQ) at the level of unique product codes (UPC) for each week:

$$P_{igw} = \frac{TR_{igw}}{TQ_{igw}} .$$

[10]Figure 11 in the appendix provides an illustration of the regional distribution of the store-year observations across US states in our dataset.

Since prices rarely change within weeks, this price will correspond to the price charged during the whole week in most cases.

Many price adjustments in our data are temporary sales. Those temporary price changes make price series more volatile but are not of primary interest to us, since we are analyzing the response to a permanent shock. We filter out temporary changes by applying a sales filter suggested by and described in more detail in Kehoe and Midrigan (2015). This algorithm uses a moving window modal price to determine a "regular price" at any point in time. As expected, the use of regular prices increases the precision of our estimates. As we show, however, it does not affect the amin qualitative conclusions if we use unfiltered price series.

Using the regular price series for each UPC, we construct state-specific grocery price indices. Our procedure largely follows Stroebel and Vavra (2015), who construct state-level price indices using the same dataset. After calculating weekly regular prices, we calculate average monthly prices and construct a geometric index of month to month price changes for each category of products for each state:

$$I_{ct} = \prod_i \left(\frac{P_{igt}}{P_{igt-1}} \right)^{\omega_{igy(t)}} .$$

We omit the state substript for readability. In our baseline index, the weight $\omega_{igy(t)}$ is the share of product i sold at grocery store in total revenue of category c in a state during the calendar year of month t. It is common in the price index literature to use lagged quantitiy weights. However, many products in our sample appear and disappear again in short time spans and using lagged weights would substantially limit the number of products used in construction of our index. We use concurrent weights as a result. In a second step, we aggregate across different categories to create state-level price indices:

$$I_t = \prod_c I_{ct}^{\omega_{cy(t)}} .$$

Again, the weight $\omega_{cy(t)}$ is the share of category c in total revenue in state s during the calendar year of month t. Finally, we obtain inflation rates by taking the logarithm of the index:

$$\pi_t = \log I_t .$$

Note that this approach does not take into account changes in the price level due to the introduction of new products, or due to reappearance of products at a new price after a stockout, a problem shared by most price indices.[11]

[11] As a result, our index may mismeasure the price level to some extent. Moreover, the prices of new products at introduction may be more flexible than the prices of existing products, and this may lead

Table 3 reports features of price adjustment for the regular prices of unique products that our index is based on. To construct these measures, we first calculate the frequency and size of price changes for each product in each store separately. For frequencies, we count changes and divide them by the number of observations for which we also observe a lagged price. We also calculate the standard deviation of the logarithm of prices within each state for each unique product. We then construct expenditure weighted means and medians[12] for each category for the periods 2001 to 2006 and 2007 to 2011. Finally, we take expenditure weighted means over all 31 broad product categories. To summarize inflation rates, we take the weighted mean or median of our state-level inflation rates for the same periods.

[Table 3 about here.]

Regular prices change with a median monthly frequency of 10.3% from 2001 to 2006 and 12.2% from 2007 to 2011. This implies a median duration of a price spell of 9.2 and 7.7 months, respectively.[13] The mean based measures are slightly higher for both frequencies, which is due to the fact that many short-lived products have no price changes at all. The median size of a price change is about 11.4% during the first, and 10.5% during the second half of the sample. The median share of price increases in price changes is about 57% during the first half of the sample and 60% during the latter half. Price increases are smaller than price decreases, with the median size of an increase being 10.5% and the median size of a decrease 14.6% from 2001 to 2006 and slightly lower at 10% and 13.2% from 2007 to 2011. The median log standard deviation of prices of identical products within a state in a given month is 15% and varies little between the two periods. Finally, monthly inflation rates are much lower during the first half of the sample (0.07% to 0.08%) compared with the second half (0.15 to 0.16%). This corresponds to annual inflation rates of 1% in the first and 1.8% in the second half of the sample.[14] Those numbers are in line with what other researchers have found for other retail price datasets.[15]

downward bias when estimating the short run response of inflation to minimum wage increases.

[12]The weighted median is the frequency/size/standard deviation that is higher or equal than the frequency/size/standard deviation of products accounting for 50% of expenditures.

[13]The implied duration is the median duration implied by the corresponding frequency under the assumption of a geometric distribution of price changes. It is computed as $-1/log_2(1 - freq)$.

[14]Stroebel and Vavra (2015) construct similar indices based on the same data and also find that national inflation rates are much lower than CPI inflation from the beginning of the data until 2006 or 2007.

[15]Using CPI data from 1998 to 2005, Nakamura and Steinsson (2008) find a median frequency of regular price changes of 8.5% for retail overall, and of 10.5% for processed food. The fraction of regular price increases for processed food in their data is 65%, and the median size of a change is 13%. Midrigan (2011) uses scanner data covering the period from 1989 to 1997 and finds a monthly frequency of regular price changes of about 12.6% and a median size of a regular price change of about 9%.

3 Empirical specification

We estimate the impact of minimum wage increases on inflation using a panel fixed effects model. Our identification strategy is based on the idea that deviations from longer run state inflation rates in states that did not experience minimum wage hikes form a useful counterfactual for states that did. Many papers studying the effects of minimum wages in the US apply variants of this identification strategy (see Allegretto et al., 2015). Furthermore, given the high frequency nature of our price data, we apply an estimation strategy allows us to study when exactly differences between treated and untreated states emerge. We obtain our baseline estimates from variants of the following flexible linear model:

$$\pi_{s,t} = \delta_s + \gamma_t + \sum_{r=-k}^{k} \beta_r \Delta mw_{s,t-r} + \gamma X_{s,t} + \epsilon_{s,t} \tag{1}$$

In this specification, β_r measures the elasticity of inflation with respect to a minimum wage increase that went into effect r months ago, or that will go into effect r months into the future in case r is negative. This specification is similar to the one used in Aaronson (2001) to study the effect of minimum wages on prices in restaurants. In our baseline estimation we control for time δ_t and state fixed effects δ_s in the inflation rate, as well as state-level unemployment and house price growth. We include the two latter control variables to absorb variation in grocery prices that is due to business cycles or the boom and bust in house prices[16]. Because the number of supermarkets and products used to construct price indices varies widely between states, we account for the heteroskedastic error structure by weighting observations with the inverse variance of a state's inflation time series in our baseline specification. None of our results critically depend on weighting, or the inclusion of controls beyond time fixed effects, but the results tend to be more precisely estimated. We present results with alternative sets of fixed effects, controls and weights below.

We conduct our analysis both around the dates of implementation of minimum wage hikes, as well as around the dates minimum wage legislation was passed. To separately identify effects at implementation and legislation, we estimate the following model:

$$\pi_{s,t} = \delta_s + \gamma_t + \sum_{r=-k}^{k} \beta_r \Delta mw_{s,t-r} + \sum_{r=-k}^{k} \alpha_r \Delta leg_{s,t-r} + \gamma X_{s,t} + \epsilon_{s,t} \tag{2}$$

Here $\Delta leg_{s,t-r}$ denotes the change in legislation at time $t - r$ that will affect the binding

[16]See Stroebel and Vavra (2015) for a discussion of the relationship between real estate and retail prices.

minimum wage in state s at some time in the future. Most legislative packages schedule several minimum wage increases at once. In this case, $\Delta leg_{s,t-r}$ measures the increase from the current minimum wage to the final minimum wage set in the new law. Hence, increases measured by Δleg will be larger on average, but spaced further apart than changes in the current minimum wage Δmw.

Because both the price level and minimum wages are non-stationary, we prefer estimating equation 1 in first differences—i.e. inflation rates and changes in the minimum wage. However, the estimates are best illustrated as the effect of minimum wages on the price level. To do so, we focus on cumulative sums of β_r and α_r coefficients in the presentation of our results. In most figures and tables, we present sequences of the cumulative sum $E_R = \sum_{r=-k}^{R} \beta_r$, and corresponding inference for this sum. Furthermore, we calculate the sum of coeffcients for several intervals to summarize the timing of the response.

An important choice in our estimation is the choice of k, the length of the window in which we identify the effects of hikes. The constraint that we face here arises from the fact that minimum wage hikes often occur in regular intervals within states, in some cases within 12 months. The average length between two subsequent minimum wage hikes is 15.5 months (see Table 1). This implies that several observations are, for instance, 8 months after and 4 months before the next minimum wage hike. In principle, we can nevertheless disentangle the effects of separate events because many states do not have minimum wage increases before 2005 and after 2009, and because some states have more infrequent minimum wage hikes only infrequently. However, our estimation strategy will not work for large k, as the collinearity of the leads and lags becomes increasingly prevalent, the larger k. We settle on estimating the effect with $k = 9$ in our baseline estimation, which suffices to show the short run impact of minimum wage increases on prices.

There are two main concerns with our estimation and identification strategy. The first pertains to reverse causality.[17] It is conceivable that states with higher inflation rates have more frequent and higher nominal minimum wage increases to keep the real minimum wage at a certain level. In this case inflation would cause minimum wage increases rather than the other way around. To cope with this concern, our specification controls for state fixed effects, which take differences in trend inflation between states into account. Moreover, our strategy relies on estimating the minmum wage effects in a

[17]A special case are minimum wage increases following from indexation. Those increases mechanically follow from inflation. However, because no state-level CPI indices exist, all states that practice indexation have pegged their minimum wage to national in inflation rates, usually the CPI for urban consumers. Nationwide changes in inflation, such as those reflected in the national CPI, are, however absorbed by time fixed effects in our specification. We thus keep minimum wage increases that follow from indexation in the sample.

very short time window immediately before and after a hike or a change in legislation. Due to the high frequency of our price data and the flexible estimation model, we can closely examine the timing of the effect such that remaining differences in inflation trends between states with and without hikes would be easily detected. Indeed the timing of effects in our results is not consistent with higher inflation rates causing minimum wage increases, as we observe a large part of the minimum wage effects in the months that immediately follow the passage of minimum wage legislaton.

A second concern relates to the fact that inflation in grocery stores follows a seasonal pattern: it is highest in January, declines in the following months, jumps up in July and then declines again. Incidentally, minimum wages follow a very similar seasonal pattern. Because many hikes are part of legislative packages that set multiple minimum wage hikes in 12-month intervals for several years, most of the hikes occur either in July (for federal minimum wage hikes) or in January (most state-level hikes). It is thus critical to control for seasonality. We include time fixed effects in our baseline specification to control for seasonal patterns. In robustness checks we allow for differences in seasonality between states by including separate calendar month fixed effects for each state. The results do not differ substantially.

4 The effect of minimum wage increases on prices

4.1 Main results

Figure 2 presents our main results. It illustrates the results from our main specification by providing the estimated cumulative effects of the minimum wage on prices, as discussed in the last section. In these and subsequent figures, we also provide the 90% confidence intervals for the sum of coefficients. The figures shows that we find a significant price response to minimum wage hikes both in the months immediately following passage of legislation as well as around implementation of minimum wage increases. We turn to these results in more detail in turn.

Table 5 provide the results of the regression model of equation 1. It shows that states with minimum wage hikes have higher inflation than states without a hike in the months surrounding the hike. Interestingly, this excess inflation begins about 6 months *before* the minimum wage increase actually takes place. As we show below, this evidence does not speak for divergence in trends between affected and non-affected states caused by something other than the minimum wage. Rather, the results are consistent with anticipatory effects of the minimum wage hikes. The estimated elasticity of prices with respect to the minimum wage hike is small but statistically significant. In the period from 6 months previous to 6 months after an increase, we find a price elasticity of 0.036% for every one percent increase in the minimum wage. The remaining columns in the table show that this elasticity declines somewhat if we account more flexibly for potential differences in inflation trends between states. In particular, while including state-specific seasonality does not alter the results, the estimates are somewhat smaller when we include either linear or quadratic state trends or if we include event-window fixed effects, i.e. if we add dummies accounting for changes in trend inflation in a window of plus/minus 24 months around each minimum wage increase.

In Table 5, we study the price response to enactment of minimum wage legislation. The table and the corresponding subfigure in 2 show that prices between states with and without minimum wage hikes diverge immediately after passage of minimum wage laws. Prior to legislation of the hike, they evolve in parallel. Our preferred estimate of the elasticity is calculated as the sum of coefficients in the 6 months following the passage of minimum wage legislation. In these 6 months prices increase by 0.017% for every one percent increase of the minimum wage in the future. After 6 months, prices move in parallel once again. The clear lack of differences in pre-legislation trends apparent in Figure 2 also speaks against concerns that our results may be driven by reverse causality.

[Figure 2 about here.]

While many minimum wage increases are implemented long after passage of legislation—especially in the case of step-wise minimum wage increases—, most legislation is followed by an increase within 9 months. Our estimates of effects at legislation and implementation may thus partly capture the same movement of prices. To assess this possibility, we estimate the effects at legislation and implementation jointly using the regression model of equation 2. Table 6 presents the results. Our qualitative conclusions remain the same with this specification. States with and without hikes do not differ in their price developments until legislation is passed. Immediately after legislation, prices start to respond, with another significant increase in the months around implementation.

We obtain our baseline numbers for the overall elasticity of the price level to minimum wages from the joint estimation. Our preferred estimate of this elasticity sums the coefficients in the 6 months following legislation with the coefficients from 6 months prior to 6 months after implementation of minimum wage increases. For our baseline estimate, this elasticity amounts to 0.042. However, the magnitude depends on the exact specification of trends absorbed around the implementation of minimum wage increases and ranges from 0.028 at the lower end to 0.042 at the upper end. These estimates suggest that the average minimum wage hike in our sample, raising the minimum wage by 8.9%, increased prices in US supermarkets between 0.25% and 0.37% over a span of 18 months around legislation and implementation. This corresponds to two to three months of inflation at the average monthly rate of 0.13%. This is a small effect, but as we will elaborate in more detail further on, the magnitudes of the estimates are consistent with at least a full pass-through of increases in grocery stores' variable cost.

These results are robust to various re-specifications and sensitvity checks. We find results similar to our baseline estimates if we include state-calendar month fixed effects, which control more restrictively for seasonality (column 2 of Table 6); if we add linear or quadratic state-specific time trends (columns 5 and 6); of if we control for event window fixed effects (column 7). We also generally find qualitatively similar elasticity estimates in the important specification checks reported in Table 7. These re-specifications include omitting the weights (column 1), using the average number of monthly price observations to weight states (column 2), omitting the controls (column 3), adding division-time fixed effects (column 4), and using a price series that does not correct for temporary price changes (i.e. sales, column 5). Note, however, that the results are markedly less precisely estimated in the last specification, which highlights that the focus on regular prices is important for the precision of our estimates.

[Table 4 about here.]

[Table 5 about here.]

[Table 6 about here.]

[Table 7 about here.]

In addition to our estimates of effects of minimum wages on inflation and the price level, we also estimate the effects on the ratio of the number of price increases and price decreases and the ratio of the absolute log sizes of price increases and price decreases. Figure 3 shows the coefficients estimated from specification 1 with the ratio of price increases to decreases as the dependent variable. Starting in the month of minimum wage legislation, the ratio of price increases to decreases goes up significantly and stays elevated for about 6 months. The coefficient point estimates are higher for several month around implementation as well, although these coefficients are individually not significantly different from zero. Figure 4 shows the evolution of the ratio of the absolute log sizes of price increases and price decreases around minimum wage legislation and increases. Also in this case, we see that the ratio is significantly elevated immediately after legislation, and preceding implementation of minimum wage increases. It is noteworthy that the increase in the price level goes through both an increase in the frequency of price increases, as well as an increase in the size of increases. This is evidence in favor of state-dependent models of of staggered price adjustment and evidence against time-dependent models (e.g. Calvo pricing).

[Figure 3 about here.]

[Figure 4 about here.]

4.2 Effect heterogeneity

4.2.1 Lead time from legislation to implementation

How do the price effects differ between minimum wage hikes that were announced long before enactment and hikes announced shortly before? To study this, we first split minimum wage legislation into bills that are followed by a first increase within 3 months and those that have more time between legislation and the first increase. Second, we split minimum wage increases into those that are preceded by legislation within the three prior months, and the remainder with more than 3 months in between increase and legislation. Figure 5 illustrates the timing of the price response for these groups of hikes. We find a significant response to legislation irrespective of the time to the first hike. The point estimates for legislation immediately followed by implementation of a first hike is larger, however the differences are not significant. Even more strikingly, we find that minimum wage increases preceded by legislation shortly before cause a very rapid increase in prices around implementation, i.e. for minimum wages with legislated shortly before their implementation, the price effect is concentrated around the time of implementation. For increases that happen longer after legislation, we find a much more gradual increase in prices. The figure provides further suggestive evidence that it is the minimum wage that causes the price increases prior to the implementation identified by our main regression model.

[Figure 5 about here.]

4.2.2 Right-to-work versus no-right-to-work states

Right-To-Work (RTW) laws state that workers cannot be forced to join a union or pay union dues even if their firm is unionized. 21 states in the US, and 17 in our sample, have such laws. Right to work laws can be seen as a proxy for loose labor market regulations, low unionization rates, and potentially low initial wages. Furthermore, in all of these states, during almost all times, the federal minimum wage is the binding one. Another striking difference, and the main motivation to analyze the effect in those states separately, is that Addison et al. (2009) find that increases in the minimum wage have an effect on grocery store labor earnings/cost only in right to work states. However, the elasticity in these states is substantial. They argue that in most other states, grocery stores are heavily unionized, and pay wages above the minimum wage.

[Table 8 about here.]

We estimated separate elasticities for RTW and non-RTW states by interacting the leads and lags of minimum wage implementation and legislation with a dummy for RTW states and its negation. The results are illustrated in Table 8. We find that the effects in RTW states are substantially larger both at legislation and implementation. The overall elasticity of the price level is 0.08 in our baseline specification. In non-RTW states, we estimate an elasticity of 0.024 in the baseline specification. In specifications that are more restrictive on trends, we find smaller elasticities that eventually become insignificant for non-RTW states. However, the individual coefficient in the month of legislation remains significant in non-RTW states even for those specifications.

4.2.3 High- versus low-income products and cheap versus expensive stores

Next we analyze differences in the development of cost of living of different income groups and differences between more expensive and cheaper stores. Potential differences are relevant to judge the redistributive consequences of minimum wage hikes. To study this question, we estimate equation 1 using price indices for products consumed by poorer and richer households, and explore the price reponses to minimum wages separately for cheap and expensive stores.

To construct price indices for low-, medium- and high-income households, we use the panel of households accompanying the IRI data set. This panel allows us to calculate yearly expenditures for each UPC by household income. We pool households in three brackets of yearly income: less then \$25,000, between \$25,000 and \$74,999 and more than \$75,000. We then use expenditures per UPC for each bracket as weights to compute an analogous index to our baseline case. Since households in the panel are located in just two states, we pool all of them and use the same expenditure weights in all states. These 3 indices cover a somewhat different sample of products. Many products are sold to none or only a few households in our panel.[18] Moreover, we cannot match private label products from the household panel to the store panel. The income-specific indices are thus best understood as describing the price development of popular branded products.

To determine which stores are more expensive than others, we follow a procedure used in Coibion et al. (2015). We first calculate the median price in each state and in

[18] There are two potential reasons for this. First, our sample is much smaller. Second, some products may not be sold in the locations of panel households.

each month for each product that is sold by at least 3 stores. We then calculate the log deviation of each price from the state-month median price of the product. Finally, we calculate the average log deviation for each store. Stores whose average deviation is below a state's median are "cheap", and the remaining stores are "expensive". We then calculate our baseline index separately for expensive and cheap stores using our baseline expenditure weights.

We then estimate the same regressions as for our baseline price index. The results are shown in Table 9. Our estimates suggest that expensive stores increase their prices more strongly than cheap stores, but those differences are not statistically significant. Interestingly, the timing of adjustment is different between the two. Cheap stores react more strongly following enactment of legislation, whereas expensive stores react more strongly in the months leading up to implementation. Overall, the minimum wage elasticity is 0.05 in expensive stores and 0.033 in cheap stores.

[Table 9 about here.]

Columns 3–5 of Table 9 show the price effects of minimum wages for products consumed by different income categories. The tables shows that there are no significant differences between price indices with expenditure weights for different income groups. The point estimates are highest for low income groups, and slightly lower for medium and high income groups. In general, the minimum wage elasticity of prices in the smaller sample of products used to construct these indices is smaller, especially around legislation. This suggests that the price response may be higher for less popular products that we don't observe in our household panel. If increases in demand due to increasing salaries of low income households were to play a major role in firm's decisionmaking, we would expect that the low income index would respond more strongly than the high income index. The lack of differences between the three indices thus provides suggestive evidence against the importance of demand effects in the price response to minimum wage increases.

4.3 Robustness of baseline results

4.3.1 Alternative treatment of multiple events

Our analysis has to cope with the fact that all states have multiple minimum wage hikes in the sample period. There is no generally accepted method for conducting event studies in the case of multiple events (Lafortune et al., 2016; Sandler and Sandler, 2014).

In this section, we show that our results are robust to using an alternative approach. This approach treats each of the multiple events in a state as independent. Since legislative packages often foresee stepwise increases in minimum wages, we focus on legislative packages here as these are more likely to be independent events. If a particular state s has E_s events in total, we then create E_s copies of the state panel, indexed by $n = 1, \ldots, E_s$ with an event occurring in period $t^{\star}_{ns}$. Florida, for instance, has two legislative changes in the minimum wage in our sample period, one that was passed in mid-2004 and another one passed in 2007. Consequently, we create two copies of Florida's time series, each containing two minimum wage hikes. The resulting panel data set used for estimation has thus three dimensions: one which is balanced (states) and two that are unbalanced (periods and number of events per state). Because the construction of the event sample implies that some observations are duplicated, we use clustered standard errors on the state level to take into account this duplication.

Using the three-dimensional panel, the regression model is a straightforward extension our baseline model. Our estimation equation specifies the impact of an event n in state s and period t on the inflation rate π_{nst} as:

$$\pi_{ns,t} = \delta_{ns} + \gamma_t + \sum_{r=-k}^{k} \alpha_r \Delta leg_{ns,t-r} + \gamma X_{ns,t} + \epsilon_{ns,t} \tag{3}$$

The results are presented in Figure 6. They confirm our baseline results both qualitatively and quantitatively. In particular, we find an immediate reaction of prices to minimum wage legislation, with price inflation remaining elevated for several months immediately succeding the decision to increase the minimum wage.

[Figure 6 about here.]

4.3.2 Placebo tests

We also conduct two placebo tests, illustrated in Figure 7. In the first placebo, we randomly match the inflation series and control series with the minimum wage series of a random state. The match is drawn with replacement from a uniform distribution including the correct match. Our baseline elasticity estimate of 0.035 lies within the 99th percentile of the estimated elasticities. This test illustrates that our estimates do not seem to be the result of (or substantially biased by) structural breaks in inflation series that are correlated with temporal patterns in minimum wage increases beyond what is captured by the time fixed-effects included in our estimation. It also validates our statistical inference, since our the probability of obtaining an estimate as big as ours in absence of a real relationship is less than 0.01.

[Figure 7 about here.]

4.3.3 Do firms pay attention to minimum wage legislation?

The timing of our results suggests that employers of minimum wage workers closely follow the enactment of minimum wage legislation. For the period after 2004, we assess the attention paid to passage of new minimum wage laws more generally by analyzing Google Trends search data. In particular, we use the the relative frequency of search for "minimum+wage+$statename_s$" in month t to measure the attention paid to local minimum wages in state s. Note that we do not measure search requests originating in different states but in the whole US for different search terms. Google Trends provides the relative frequency of search terms over time as a normalized number between 0 (minimum) and 1 (maximum). We estimate the following regression using this data:

$$\log search_{s,t} = \delta_s + \gamma_t + \sum_{r=-k}^{k} \beta_r incr_{s,t-r} \sum_{r=-k}^{k} \alpha_r legis_{s,t-r} + \epsilon_{s,t} \tag{4}$$

$incr_{s,t-r}$ and $legis_{s,t-r}$ are dummy variables indicating a minimum wage increase and passage of minimum wage legislation repectively.

[Figure 8 about here.]

The results of this regression are illustrated in Figure 8. Both around an increase and around the date of legislation, interest in minimum wages goes up, by about 30% immediately after legislation is passed, and by up to 50% in the months around implementation of minimum wages. There is no elevated interest in minimum wages before in the months before legislation is passed.

Just as our results on prices suggest, agents seem not to pay attention to the legislative process itself, but get informed once a law is passed. It is unclear if these patterns generalize to management of grocery stores, however they are consistent with anticipation of future cost increases from the date of legislation onward. The patterns also validate our data and choices in dating minimum wage legislation. We interpret legislation dates as a measure of when firms become aware of pending minimum wage increases, and the results of this section are consistent with this interpretation.

5 Cost and benefits of minimum wage increases

Up to this point we have focused on the response of prices to minimum wage increases. We now turn to analyzing the pass-through of the cost increase that goes along with raising minimum wages. This allows us to assess directly whether it is firms who pay for minimum wages, or the population of consumers. We then go on to study the distribution of cost between consumers of different income groups. Given that we do not observe wages and hours for the firms in our sample, these calculations rely on some strong assumptions and are thus best considered as back-of-the envelope calculations that put our empirical estimates in perspective.

5.1 Cost pass-through

For retailers, increases in minimum wages are primarily an increase to their variable cost. We define variable cost as the sum of labor cost LC and the cost of purchases of intermediates IC. Assuming a minimum wage hike increases labor cost from LC to LC', we can express the change in variable cost as

$$\Delta VC = \frac{LC' + IC}{LC + IC} - 1 = \left(\frac{LC'}{LC} - 1\right)\left(\frac{LC}{LC + IC}\right). \tag{5}$$

We now predict the increase in grocery stores' variable cost for each minimum wage increase in our sample. We use data from the Retail Trade Survey provided by the BLS to compute the labor share in grocery stores' variable cost. We then construct a measure of the increase in grocery stores' labor cost for each minimum wage hike using the initial distribution of hours and wages observed in the CPS, together with several assumptions on the response of hours and wages to a minimum wage increase. In particular, we will alter wages observed for grocery store workers in the CPS to construct a counterfactual joint distribution of wages and hours.

In constructing this counterfactual distribution, we first assume that employment and hours worked are not affected by minimum wage increases. This assumption entails that firms do not respond to the minimum wage by reducing employment or by adjusting other aspects of the employment relationship (such as fringe benefits). It also implies that consumers do not respond to the higher prices by consuming less, because such behavior would directly contradict the assertion of no employment effects. Even though this assumption is strong, we think that it can be justified by the large literature that finds no or very limited short run employment effects of minimum wage increases (see e.g. Allegretto et al. (2015)). As pointed out by MaCurdy (2015) the assumption of unchanged employment can alternatively be seen as taking the best case scenario held by minimum wage proponents. He calculates the redistributive effects of minimum wages using input-output matrices based on this assumption.

Second, we have to rely on assumptions on the minimum wage elasticity of wages along the wage distribution. Dube et al. (2015) use payroll data from an US retailer and show that this retailer increased wages well above the new minimum wage in response to a 1996/1997 increase in the federal minimum wage. In particular, the retailer grants a gradually declining raise to workers earning an initial wage of up to 130% of the initial minimum wage. Addison et al. (2009) study the impact of a bigger set of minimum wage increases on earnings in grocery stores. They find a minimum wage elasticity of aggregate earnings of about 0.5 in Right-To-Work states and no significant elasticity in other states. Given shares of minimum wage workers in those states, it is hard to rationalize an elasticity of this magnitude without substantial ripple effects. Autor et al. (2016) study the impact of minimum wage increases on wage inequality, and find significant ripple effects up to the 20th percentile and no ripple effects above the 30th percentile of the local wage distribution, even though much less than 20% of employees earn wages close enough to the minimum wage to be directly affected. On average over time and states, the 20th percentile of states' wage distribution corresponds to 138% and the 30th percentile to 162% of local minimum wages.

Our assumptions on the minimum wage elasticity of wages follow the gradient of wage increases observed by Dube et al. (2015):

$$\frac{wage_i'}{wage_i} - 1 = \begin{cases} \left(\frac{mw'}{mw} - 1\right), & \text{if } \frac{wage_i}{mw} \leq 1.1 \\ \left(\frac{mw'}{mw} - 1\right) \frac{R - \frac{wage_i}{mw}}{R - 1.1}, & \text{if } 1.1 < \frac{wage_i}{mw} \leq R \\ 0 & \text{if } \frac{wage_i}{mw} > R \end{cases} \tag{6}$$

We assume that worker is new wage $wage_i'$ is a function of her old wage and the distance of the old wage to the old minimum wage. If the old wage is within 1.1 times the old minimum wage, she receives a wage increase equal to the increase in the minimum wage. The wage increase declines linearly for workers between 1.1 and R times the old minimum wage. Workers with an initial wage above R times the old minimum wage receive no wage increase. For R, we use one of two values. Our "low R" value is 1.3, consistent with the corporate policy of the retailer studied in Dube et al. (2015). Our upper bound, "high R", value is 1.75. We choose this value as follows. We calculate the ratio of the 30th percentile of state hourly wages—the lowest percentile where there is *no* significant effect in Autor et al. (2016)—to local minimum wages. We then average this measure over all states. Depending on the time period, we get values between 1.65 and 1.75. We pick $R = 1.75$ as an upper bound for ripple effects that can be supported by the available literature. In this "high R" scenario, we also incorporate that prices of wholesalers increase slightly because of the minimum wage. The reason is shown in Table 2. In the case of large ripple effects, wholesalers costs are affected by minimum wages. In the "low R" scenario, however, we do not incorporate price effects arising from higher wholesale prices.

We apply formula 6 to predict increases in earnings associated with each minimum wage increase. We then plug into equation 5 and calculate:

$$\Delta VC_{s,t} = \left(\frac{LC_{y(t)}^{RTS}}{LC_{y(t)}^{RTS} + IC_{y(t)}^{RTS}} \right) \left(\frac{\sum_{\tau=t-12}^{t-3} \sum_i \omega_i \cdot wage'_{i,\tau} \cdot hours_{i,\tau}}{\sum_{\tau=t-12}^{t-3} \sum_i \omega_i \cdot wage_{i,\tau} \cdot hours_{i,\tau}} - 1 \right). \tag{7}$$

[Table 10 about here.]

We present a summary of our calculated cost increase measures in Table 10. We do find that cost increases in Right-To-Work states are substantially larger than in non-Right-To-Work states, however, scaled to elasticities by dividing through the associated minimum wage increases there is no substantial difference. Furthermore, even for our "high R" assumption, we still calculate earnings elasticities substantially below the 0.5 estimated by Addison et al. (2009).

We estimate our baseline regression using predicted cost increases as an independent variable instead of minimum wage increases. In particular, we estimate

$$\pi_{s,t} = \delta_s + \gamma_t + \sum_{r=-k}^{k} \beta_r \Delta VC_{s,t-r} + \sum_{r=-k}^{k} \alpha_r \Delta VC_{s,t-r}^{leg} + \gamma X_{s,t} + \epsilon_{s,t} \tag{8}$$

Here $\Delta VC_{s,t}$ is the predicted cost increase associated with a minimum wage increase in state s in month t, and $\Delta VC_{s,t}^{legisl}$ is the sum of predicted cost increases associated with all minimum wage increases in a legislative package enacted in state s in month t. When no minimum wage increases are implemented or legislated, the corresponding variables are zero. The coefficients β_r in this regression quantify the pass-through of the predicted variable cost increase to consumers. $\sum_{r=-k}^{k} \beta_r = 1$ would indicate a complete pass-through within k months. Our predictions are statistics are constructed from a finite number of CPS observations and are thus affected by measurement error. We follow Autor et al. (2016) and use minimum wage increases as an instrumental variable for cost increases to deal with this issue.

[Table 11 about here.]

[Table 12 about here.]

The results are shown in Tables 11 (for high R) and 12 (for low R). We present results for our baseline regressions as well as those controlling for linear and quadratic time trends and event window fixed effects. Just as in our minimum wage regressions, we find that prices respond significantly immediately after legislation is enacted, and in the 6 months before and after imlementation of minimum wage increases. Furthermore, the TSLS estimates are consistently larger than the OLS estimates, suggesting there is indeed measurement error in our cost increase predictions. Our TSLS estimates assuming a high R and large ripple effects are generally slightly larger than, but close to one. In particular, while we can reject the hypothesis that there is zero pass through, we cannot reject the null hypothesis that pass-through is equal to one in all but one specfication. When we run the same regressions assuming a low R and small ripple effects our TSLS point estimates are generally substantially larger than one and depending on the specification lie between 2.1 and 3.7. In most specifications, the increase in prices is systematically larger than the predicted increase in cost assuming $R = 1.3$ and the null hypothesis of full pass-through is rejected at the 10% level in most specifications. We interpret our results for high and low values of R as reasonable bounds on the true pass-through. Based on these bounds, the price response is consistent with full pass-through and inconsistent with less than full pass-through.

5.2 Distribution of cost across households

The results of the previous section suggest that it is largely consumers who pay for minimum wage increases. We now go into more detail and study how the welfare cost of the price increases that result from minimum wage hikes are distributed accross households with different incomes. We will then contrast this with the (predicted) income gains from increases in the minimum wage. It is important to point out that this is a partial analysis: we do not consider the welfare cost of potential price increases in other sectors, such as restaurants. Furthermore, our calculations as far as benefits go are still conditional on the assumptions embodied in equation (6). In particular, we do not account for potential reactions of consumers to the higher prices such as adjumstments in their consumption baskets or changes in their expenditures on food at home. As such, our results provide estimates of the first-order impacts of the price increases on households.

We calculate the equivalent variation of a hypothetical increase in all binding minimum wages in the US in May 2011 by 10%. The equivalent variation of an increase in minimum wages consists of a direct effect of increasing household incomes through higher wages, and a indirect negative effect of increasing prices. Formally, for a household of income group j it can be calculated as

$$\Delta U_j = \Delta Y_j - \sum_i E_{ij} \Delta P_i. \tag{9}$$

We use the May 2011 CPS[19] and apply formula (6) with $R = 1.75$ to compute the mean annualized Dollar increase in household income for households of different income

[19] In addition to information on wages and hours, the May CPS covers information on annual Household Income.

brackets, Y_j . Moreover, we employ the Consumer Expenditure Survey and add expenditures for Food At Home, Housekeeping Supplies and Personal Care Product and Services to obtain a measure of mean grocery store expenditure for the same income brackets. The resulting expenditure shares on products sold in grocery stores are presented in Table 16 in the appendix. Finally, according to our baseline price elasticity, the expected price increase of a 10% increase of all binding minimum wages is 0.36%. Since our results in section 4.2.3 suggest that this elasticity does not vary between different baskets, we use the same elasticity for all income groups.

[Figure 9 about here.]

[Table 13 about here.]

Figure 9 and Table 13 illustrate the disparate welfare impact of the minimum wage across the income distribution. Panels (a) and (b) in Figure 9 illustrate the effect of the equivalent variation of the price increases going along with a minimum wage increase. There are two noteworthy results of our calculations. First, in USD terms, the biggest negative impact of minimum wage hikes accrues to rich households' welfare, because rich households have the highest expenditures for products sold at grocery stores. Relative to annual household income, however, the biggest negative impact is for very poor households, because those households spend the biggest *share* of their income at grocery stores. The USD cost of increasing minimum wages is about \$45 for households earning above \$150,000, \$25 for households earning between \$50,000 and \$70,000 and about \$14 for households earning below \$10,000. Relative to household income, this corresponds to a welfare loss of about 0.02% for the richest, 0.04% for middle-income households and 0.23% for the poorest households.

These numbers may seem small or even economically insignificant at first. However, rather than discussing gains in terms of USD or annual household income, one may analyze the magnitude of the welfare effect relative to the stated goal of most minimum wage legislation: raising the salaries of low-income workers. Panels (c) and (d) of Figure 9 illustrate the annualized gain in incomes (the length of the full bar) and gains net of price increases (the dark grey area) for all household income brackets, using equation 6 with $R = 1.75$ to calculate the wage gains. In USD terms, the biggest benificiaries are households with earnings between \$20,000 and \$80,000, who gain around \$320-\$340 in annual income. The poorest households gain roughly \$125 and the richest households \$167. Households with low to medium income profit the most because these households work more hours than the poorest households and because they are more likely to have one or two wage earners earning close to the minimum wage. However, in terms of household incomes, poor households gain the most and rich households gain relatively little. The poorest households gain about 2%, households in the middle of the income distribution about 0.6%, and the richest households less than 0.1%.

Our calculations, however, also imply that price increases in grocery stores eat up a non-negligible part of the wage increases arising from the minimum wage. For the poorest households, the equivalent variation of price increases undoes about 12% of the income increase due to higher wages. For middle income households, this fraction is 6% to 8% and for the richest households it is almost 27%. Hence, while the welfare losses due to price increases in grocery stores are not big, the income gains undone because of the price effects of the minimum wage are not insignificant.

6 Minimum wage increases and pricing frictions

An interesting feature of our results is the drawn out nature of the response, as well as the fact that pass-through seems to be mostly complete by the time minimum wage increases go into effect. This points to the presence of significant pricing frictions. In this section, we show that the timing of the response is consistent with a pricing model with frictions. Such models fall into one of two categories: state-dependent and time-dependent pricing models. Time-dependent pricing models such as Calvo (1983) or (cite Taylor) would predict that the frequency of price adjustment does not react to cost shocks, and that prices increase through larger than usual price increases alone. State-dependent or menu cost pricing models would predict that adjustment works both through frequencies and size, which is what we find in the data.

We thus compare our empirical results with the predictions of a partial equilibrium menu cost model. We calibrate the model to match the long run features of price adjustment in our data. We then compare the response to a minimum wage shock predicted by the model to our empirical estimates. The purpose of this exercise is to obtain a benchmark, based on an established theory, to assess the magnitude and timing of our empirical results. Indeed we find that the model can match timing and magnitude of the price response rather well.

6.1 A benchmark menu cost model

In our model, there is a continuum of supermarkets selling an imperfectly substitutable single product. The demand for each firm's product is given by

$$Q_{it} = \left(\frac{P_{it}}{P_t}\right)^{-\theta}, \tag{10}$$

with $P_t = \left(\int_0^1 P_{it}^{1-\theta}\right)^{\frac{1}{1-\theta}}$ being the CES price index. Supermarkets face a constant nominal marginal cost,

$$MC_{it} = \frac{W_t^{\alpha} C_t^{1-\alpha}}{A_{it}} . \tag{11}$$

W_t is the nominal wage of low skilled labor that may be affected by a minimum wage. C_t is the nominal cost of all other variable inputs, e.g. merchandise, or higher skilled labor. Idiosyncratic productivity A_{it} follows a log-normal process, $\log A_{it} = \rho \log A_{it-1} + \varepsilon_{it}$, with $\varepsilon \sim N(0, \sigma)$.

Let O_t be an index of all other goods and services sold in the economy. The aggregate price level of our economy is a geometric index $Z_t = P_t^{\omega} O_t^{1-\omega}$ of supermarket prices P_t and other prices O_t. O_t grows at the constant exogenous rate π^O, while supermarket prices are determined endogenously. The aggregate price level thus grows at the endogenous inflation rate $1 + \pi_t^Z = (1 + \pi_t^P)^{\omega}(1 + \pi^O)^{1-\omega}$.

We denote variables in terms of the current aggregate price level with a tilde. A supermarket's real profits are given by

$$\tilde{\Pi}_{it} = \left(\frac{\tilde{P}_{it}}{\tilde{P}_t}\right)^{-\theta} \left(\tilde{P}_{it} - \frac{1}{A_{it}} \tilde{W}_t^{\alpha} \tilde{C}^{1-\alpha}\right) . \tag{12}$$

Nominal costs of other inputs C_t grow at the rate of aggregate prices and the real cost is constant at $\tilde{C}$. The wage low skilled workers earn is the maximum of the nominal market wage W_t^L and an exogenous minimum wage W_t^M. The nominal market wage grows at the rate of aggregate prices, so the real market wage is constant at $\tilde{W}^L$. Nominal minimum wages are constant, unless they change exogenously, and the real minimum wage $\tilde{W}_t^M$ will decline at the rate of inflation. Hence, real wages paid to low skilled labor follow one of two regimes. After an increase in the minimum wage, real wages increase. They will then slowly decline back to the original real market wage, and be constant after. We normalize both $\tilde{C}$ and $\tilde{W}^L$ to 1.

Minimum wage increases are announced k periods in advance. Those announcements happen with zero probability and firms will not take the possibility of an increase into account before it is announced. We also assume that minimum wage increases are at least k periods apart, so that a supermarket will only need to keep track of one future increase. The size of every minimum wage increases is $W_{t+1}/W_t = \Delta$. As a result, all information on future minimum wages can be summarized in one variable, $S_t \in \{0, 1, \ldots, k\}$, which takes as values either the periods to the next minimum wage hike or zero if no future hike has been announced.

To change its price, a supermarket has to invest a small amount of output K at cost Z_t. This makes profit maximization a dynamic problem. A supermarket enters a period with real price P_{it-1}/Z_t. Depending on current and expected productivity draws, aggregate prices and minimum wages, it may be desireable to pay KZ_t and update to another price, or alternatively, stay with the price inherited from the last period. Anticipating the solution of the supermarket's problem, the optimal strategy in such models is to update to a new "desired" price when the inherited price is far from this desired price, and stay with the inherited price if it is close.

Formally, the value of updating is given by

$$\begin{aligned} V^{upd}\left(\tilde{P}_{it-1}, A_{it}, \tilde{P}_t, \tilde{W}_t, S_t\right) = \max_{\tilde{P}_{it}} \Pi\left(\tilde{P}_{it}, A_{it}, \tilde{P}_t, \tilde{W}_t\right) - K \\ + \beta \mathbb{E}_t V\left(\tilde{P}_{it}, A_{it+1}, \tilde{P}_{t+1}, \tilde{W}_{t+1}, S_{t+1}\right) . \end{aligned} \tag{13}$$

The value of staying with the past period's price is

$$\begin{aligned} V^{noupd}\left(\tilde{P}_{it-1}, A_{it}, \tilde{P}_t, \tilde{W}_t, S_t\right) = \Pi\left(\frac{P_{it-1}}{1+\pi_t^Z}, A_{it}, \tilde{P}_t, \tilde{W}_t\right) \\ + \beta \mathbb{E}_t V\left(\frac{P_{it-1}}{1+\pi_t^Z}, A_{it+1}, \tilde{P}_{t+1}, \tilde{W}_{t+1}, S_{t+1}\right) . \end{aligned} \tag{14}$$

Expectations are conditional on the current state and the law of motions of the state variables are given by

$$\log A_{it} = \rho \log A_{it-1} + \varepsilon_{it} , \tag{15}$$

$$\tilde{P}_{t+1} = \tilde{P}_t \frac{1+\pi_{t+1}^P}{1+\pi_{t+1}^Z} , \tag{16}$$

$$\tilde{W}_{t+1} = \tilde{W}_t \frac{\Delta}{1+\pi_{t+1}^Z} \text{ if } S_t = 1, \text{ else } \tilde{W}_{t+1} = \tilde{W}_t \frac{1}{1+\pi_{t+1}^Z} , \tag{17}$$

$$S_{t+1} = \max\{S_t - 1, 0\} . \tag{18}$$

The recursive problem of the supermarket is to maximize

$$\begin{aligned} & V\left(\tilde{P}_{it-1}, A_{it}, \tilde{P}_t, \tilde{W}_t, S_t\right) = \\ & \max\left\{V^{noupd}\left(\tilde{P}_{it-1}, A_{it}, \tilde{P}_t, \tilde{W}_t, S_t\right), V^{upd}\left(\tilde{P}_{it-1}, A_{it}, \tilde{P}_t, \tilde{W}_t, S_t\right)\right\} \end{aligned} \tag{19}$$

We solve this recursive problem using Value Function Iteration. A challenge in solving models of this type is that the optimal policy of individual supermarkets depends on current and expected future aggregate prices. Aggregate prices in turn depend on the optimal policy and the joint distribution of lagged prices P_{it-1} and productivity A_{it}. This distribution would be a state variable in the individual supermarket's full problem, which is not computationally feasible. We follow the approach of Krusell and Smith (1998) and approximate the law of motion of aggregate prices as a function of a small set of aggregate variables. In particular, we restrict firms to use a log-linear function of lagged aggregate prices, the current minimum wage and the value of the current signal to predict current inflation. After converging, this prediction turns out to be very precise, explaining upward of 99% of the variation in aggregate price inflation. Similar methods derived from Krusell and Smith's approach have been applied to menu cost models e.g. by Nakamura and Steinsson (2010) and Midrigan (2011). We describe the algorithm in more detail in the appendix.

[Table 14 about here.]

The model's solution depends on the 10 parameters listed in Table 14. We set some parameters to values commonly used in the literature, and calibrate the rest to fit important features of regular price adjustment in the IRI price data. The monthly discount factor is set to $\beta = 0.96^{1/12}$, corresponding to an annualized real interest rate of 0.042. We set the exogenous price growth of other goods in the CPI to $\pi_O = 0.0013$, the 2001 to 2011 average rate of inflation at grocery stores in the IRI data. Following Nakamura and Steinsson (2008), we use an elasticity of substitution of $\theta = 4$ to get realistic values for supermarket markups. Minimum wage hikes are announced $k = 4$ periods in advance, which is the average for hikes with anticipation periods less than 6 months. The size of minimum wage increases is set to 10.7%, the average size for hikes with short anticipation periods. The remaining parameters K, ρ and σ^2 are chosen to match the median frequency of price changes, the share of price changes that are decreases, and the size of price changes in the IRI data from 2001 to 2006. We set menu costs to $K = 0.035$, $\rho = 0.66$ and $\sigma = 0.06$. Those values are similar to those used in the calibration of similar models elsewhere (e.g. Nakamura and Steinsson (2008) or Gopinath and Itskhoki (2010)).

The central parameter governing the long run pass-through of minimum wage hikes into prices is α, the elasticity of marginal cost with respect to minimum wages. Consider the price setting decisions of firms operating in a static setting without menu costs. Facing the same demand and cost functions as in our dynamic model, firms would set their price to be

$$P_i = \frac{\theta}{\theta - 1} \frac{W^\alpha C^{1-\alpha}}{A_i}.$$

Since the distribution of A_i is independent of W, the minimum wage elasticity of the price level is equal to the minimum wage elasticity of marginal cost, α. In the menu cost model pass-through will not be exactly equal to α, but for the chosen parameter values it will be very close to it. The small difference arises because inflation will erode the real value of nominal minimum wage increases in the months between implementation and the completion of desired pass-through. This effect is larger, the longer pass-through is delayed by pricing frictions.

With Cobb-Douglas production and competitive labor markets, α would correspond to the share of minimum wages in total variable cost.[20] We use $\alpha = 0.01$, as a lower bound value consistent with our calculation in section 2 as our baseline value for α. We also use a higher value for α that fits the magnitude of pass-through we estimate.

6.2 Results

The results are illustrated in Figure 10. The illustrates the results of two experiments in our calibrated model. First, we show the response to a minimum wage shock that is known 2 months in advance, and second, the results to a minimum wage shock that is known 8 months in advance. We compare the two experiments to our estimates of the response to minimum wage increases with short and long lead times in figure 5. The model can account for the features of the empirical results in a number of ways. First, the fit for the increases with short lead times is near perfect. The model predicts a very quick pass-through, consistent with the data. For the response to an increase with a longer lead time, we find that the model predicts that firms start to increase prices quite in advance. However, the initial response of the model is much smaller than in the data. The cause for this that our menu cost model features strong strategic complementarity which leads to a backloading of the price response. We aim at exploring several different specifications of pricing frictions in future versions of this paper.

[Figure 10 about here.]

7 Conclusion

Does a rise in the minimum wage lead to an increase in the prices of grocery stores? To our knowledge, this question has not been addressed so far. We achieve the necessary power to identify the supposedly small effect by examining the impact of 160 minimum wage hikes in US states in the 2001–2011 period on state-level grocery price indices constructed from scanner data. Using a flexible, non-parametric event study design, we use the month-on-month price inflation in a state without a minimum wage hike to construct a counterfactual inflation rate in states with a minimum wage hike at a given point in time.

[20] Cost minimization implies that $\frac{WL}{WL+CX} = \frac{1}{1+\frac{1-\alpha}{\alpha}} = \alpha$.

Our estimates show that minimum wage hikes lead to an increase in grocery store prices in the month of their implementation and the months immediately preceding it. We estimate that a 10% rise in the minimum wage increases prices in supermarkets by 0.3%. The estimated price elasticity is thus about two to three times smaller than the price elasticity of restaurants in the US (Aaronson, 2001; Aaronson et al., 2008; Allegretto and Reich, 2015; Lemos, 2008) and it is consistent with at least a full pass-through of the increase in costs into retail prices. Furthermore, a minimum wage hike increases the probability of price adjustments by 3 percentage points and raises the probability of price increases by 4 percentage points.

To interpret these results, we compare our estimates to the response of retail prices to minimum wage shocks in a simple menu cost model. While the model performs well in predicting the timing of the price response to the cost shock, it predicts a substantially lower pass-through. We suggest that this disconnect may be caused by ripple effects of minimum wages, demand effects, or different production technologies compared to our simple Cobb-Douglas baseline.

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Appendix

A Further Tables

[Table 15 about here.]

[Table 16 about here.]

B Further Figures

[Figure 11 about here.]

C Determining regular prices

TBA

D Solving the Menu Cost Model

We first restate the recursive problem in terms of lagged prices and current inflation rates. Denoting variables in terms of the lagged price level Z_{t-1} with a tilde, we get that

$$V\left(\tilde{P}_{it-1}, A_{it}, \tilde{P}_{t-1}, \tilde{W}_t, \pi_t^P, S_t\right) = \tag{20}$$

$$\max\left\{V^{upd}\left(\tilde{P}_{it-1}, A_{it}, \tilde{P}_{t-1}, \tilde{W}_t, \pi_t^P, S_t\right), V^{noupd}\left(\tilde{P}_{it-1}, A_{it}, \tilde{P}_{t-1}, \tilde{W}_t, \pi_t^P, S_t\right)\right\} \tag{21}$$

. The two possible values V^{upd} and V^{noupd} are given by

$$V^{upd}\left(\tilde{P}_{it-1}, A_{it}, \tilde{P}_{t-1}, \tilde{W}_t, \pi_t^P, S_t\right) = \max_{\tilde{P}_{it}^*} \Pi\left(\frac{\tilde{P}_{it}^*}{1+\pi_t^Z}, A_{it}, \tilde{P}_{t-1}\frac{1+\pi_t^P}{1+\pi_t^Z}, \frac{\tilde{W}_t}{1+\pi_t^Z}, \pi_t^P\right) \tag{22}$$

$$+\beta\mathbb{E}V\left(\frac{P_{it}^*}{1+\pi_t^Z}, A_{it+1}, \tilde{P}_{t-1}\frac{1+\pi_t^P}{1+\pi_t^Z}, \tilde{W}_t\frac{1+\mathbb{1}(S_t=1)\Delta}{1+\pi_t^z}, \pi_{t+1}^P, \min(S_t-1, 0)\right) - \tilde{K} \tag{23}$$

and

$$V^{noupd}\left(\tilde{P}_{it-1}, A_{it}, \tilde{P}_{t-1}, \tilde{W}_t, \pi_t^P, S_t\right) = \Pi\left(\frac{\tilde{P}_{it-1}}{1+\pi_t^Z}, A_{it}, \tilde{P}_{t-1}\frac{1+\pi_t^P}{1+\pi_t^Z}, \frac{\tilde{W}_t}{1+\pi_t^Z}, \pi_t^P\right) \tag{24}$$

$$+\beta\mathbb{E}V\left(\frac{P_{it-1}}{1+\pi_t^Z}, A_{it+1}, \tilde{P}_{t-1}\frac{1+\pi_t^P}{1+\pi_t^Z}, \tilde{W}_t\frac{1+\mathbb{1}(S_t=1)\Delta}{1+\pi_t^z}, \pi_{t+1}^P, \min(S_t-1, 0)\right) \tag{25}$$

. All state variables except for π_t^P are determined independently of the supermarket's policy, but π_t^P depends both on the individual firms optimal policy and the current distribution of firms over the state space. We follow Krusell and Smith (1998) and let individual supermarkets predict aggregate inflation at the time of their decision as a function of lagged and current aggregate variables.

We implement the Krusell and Smith algorithm the following way. We assume supermarkets predict *current* inflation based on the lagged supermarket to aggregate price ratio $\tilde{P}_{t-1}$, the current wage in terms of lagged aggregate prices $\tilde{W}_t$, and information on future mimimum wage increases S_t. We use a log-linear prediction rule:

$$\log(1 + \pi_t^P) = \Gamma\left(S_t, \tilde{W}_t, \tilde{P}_{t-1}\right) \tag{26}$$

$$= constant + \sum_{s \in S} \gamma_s \mathbb{1}(S_t = s) + \phi_P \log \tilde{P}_{t-1} + \phi_W \log \tilde{W}_t \tag{27}$$

This prediction rule turns out to work very well, yielding an R^2 upward of 0.99 after convergence. Using the definition of the aggregate price level, we can also use it to predict π_t^Z. This reduces the supermarket's dynamic problem to 5 state variables, $\tilde{P}_{it-1}$, A_{it}, $\tilde{P}_{t-1}$, $\tilde{W}_t$ and S_t. A solution to this problem, and an equilibrium of this economy, is a fixed point of V, a corresponding policy, and a prediction rule Γ that is consistent with the policy and closely approximates the true path of aggregate prices.

We solve the recursive problem using value function iteration on a discrete grid. We approximate the autoregressive process governing productivity by a Markov chain using a procedure suggested by Tauchen (1986). We specify a logarithmic evenly spaced grid of points for the remaining continous variables $\tilde{P}_{it-1}$, $\tilde{P}_{t-1}$ and $\tilde{W}_t$. We use a dense grid points for individual state variables $\tilde{P}_{it-1}$ and A_{it} with 300 and 30 grid points respectively. This high number of grid points is necessary to produce smooth aggregates. We use less grid points for aggregate variables, 30 for $\tilde{P}_{t-1}$ and 20 for W_t. Adding further grid points does not substantially improve precision.

We then apply the following computational algorithm. (1) We provide an initial parametrization for Γ. (2) We solve the recursive problem on the grid for the current Γ. (3) We use the policy function to simulate the model for a continuum of firms and to obtain time series for aggregate variables. (4) We obtain a new prediction function Γ' by fitting the data obtained from the simulation using OLS. (5) If the Γ' is close to the previous guess Γ, stop. Otherwise we update Γ and proceed from step (2).

This is a computationally expensive procedure. The menu cost friction introduces multiple kinks in the value function, which make faster and more precise interpolation-based techniques unstable. Due to the 3 continous state variables, the number of points in the state space increases quickly.

Figures

Figure 1: Regional distribution of minimum wage hikes in our sample

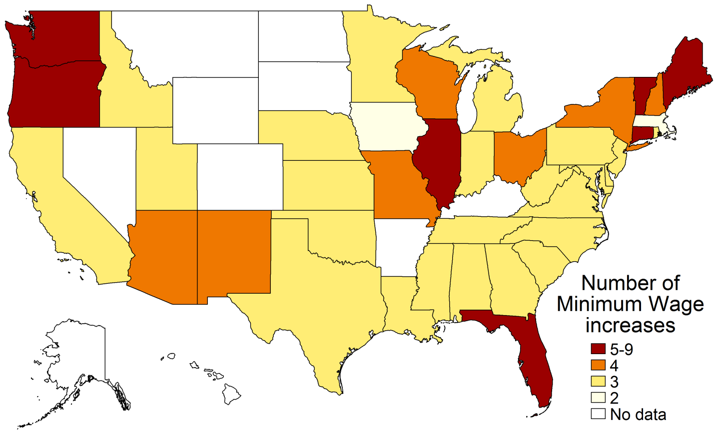
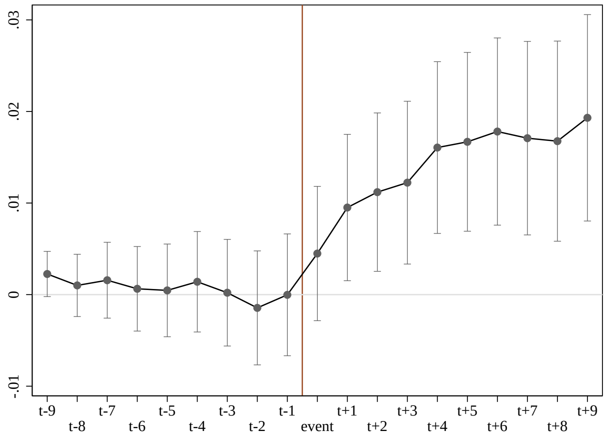
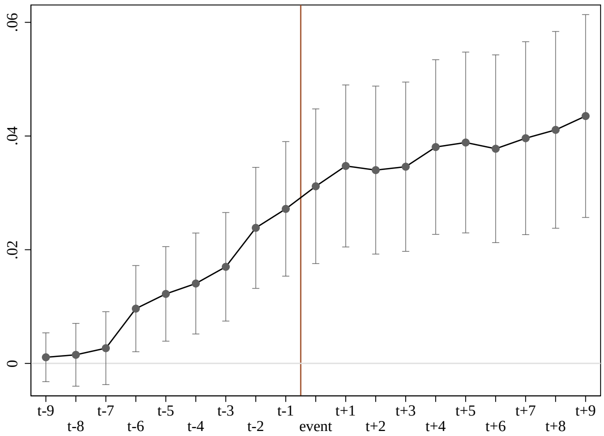


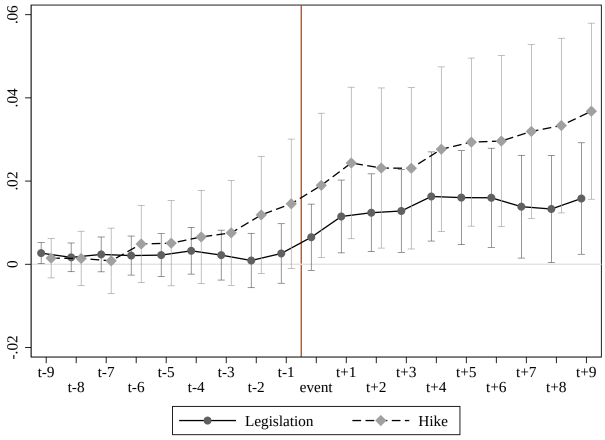
Figure 2: Cumulative coefficient plots of baseline results



(a) Effects around legislation



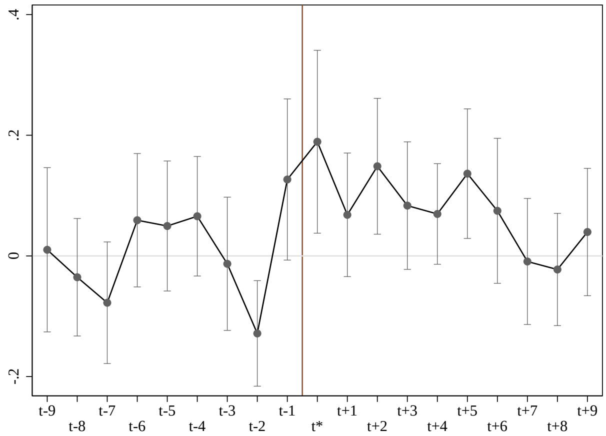
(b) Effects around implementation



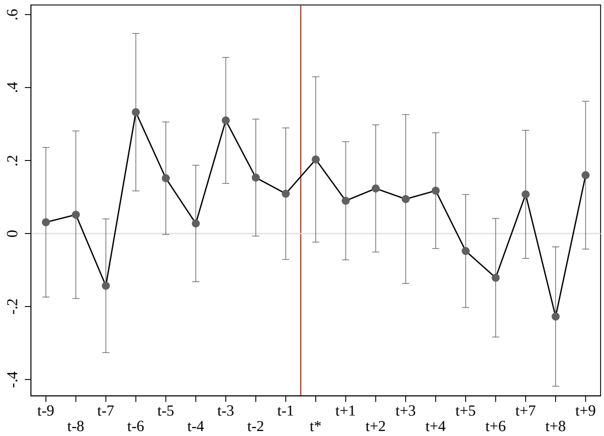
(c) Joint estimation of effects around legislation and implementation

Baseline controls are time and state FE, local unemp. rate and house price growth. Figures show cumulative coefficients E_R and corresponding 90% confidence intervals based on robust SE. Dependent variable is $\pi_{s,t}$.

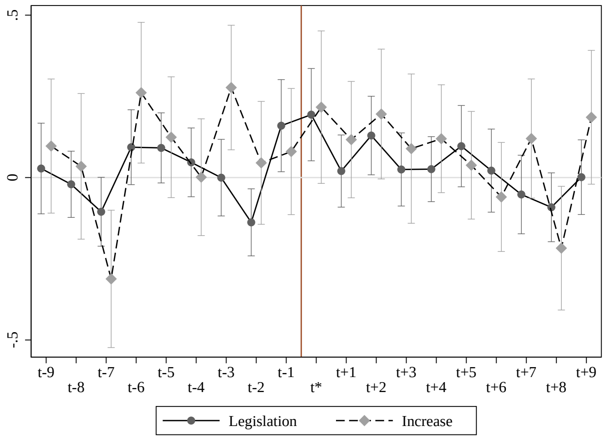
Figure 3: Effects on ratio of price increases to decreases



(a) Effects around legislation



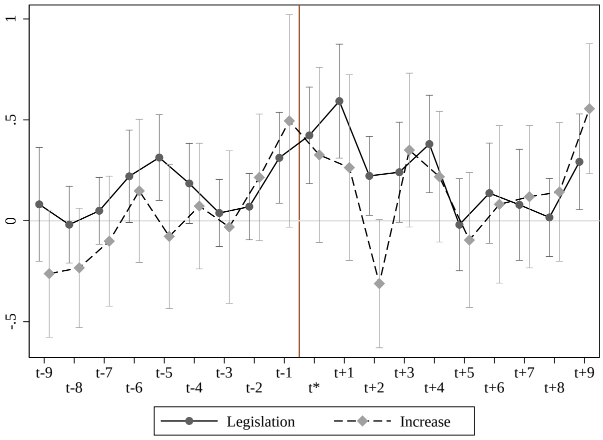
(b) Effects around implementation



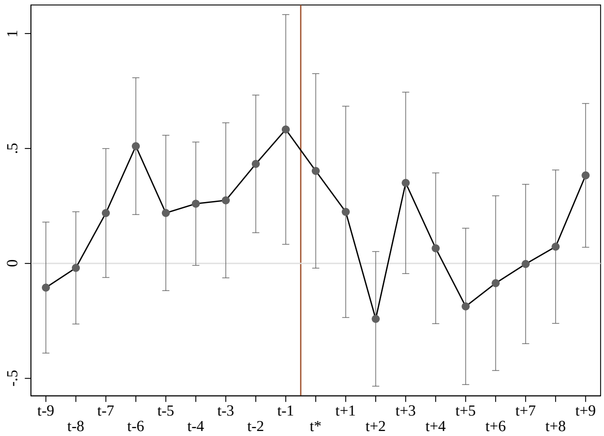
(c) Joint estimation of effects around legislation and implementation

Baseline controls are time and state FE, local unemp. rate and house price growth. Figures show coefficients and corresponding 90% confidence intervals based on robust SE. Dependent variable is the ratio of the price increases and decreases.

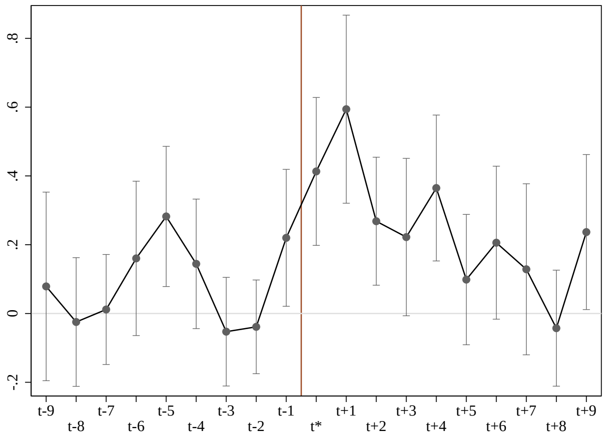
Figure 4: Ratio of size of increases to size of decreases



(a) Effects around legislation



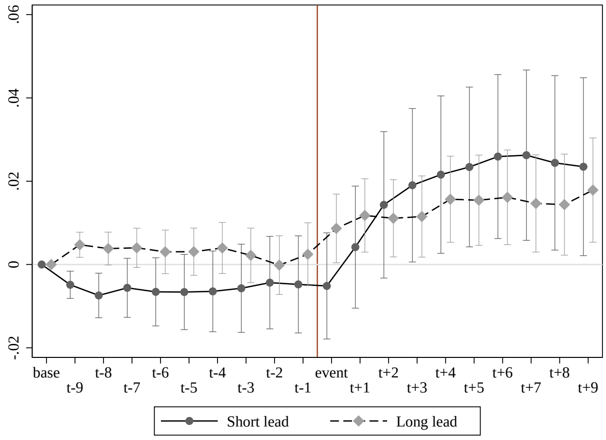
(b) Effects around implementation



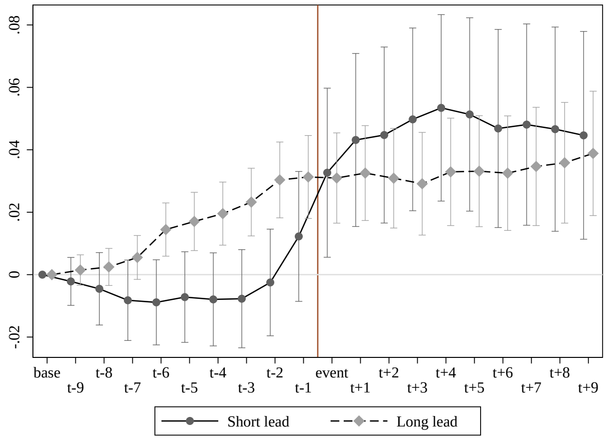
(c) Joint estimation of effects around legislation and implementation

Baseline controls are time and state FE, local unemp. rate and house price growth. Figures show cumulative coefficients and corresponding 90% confidence intervals based on robust SE. Dependent variable is the ratio of the size of price increases relative to decreases.

Figure 5: Short vs. longer lead time from legislation to implementation



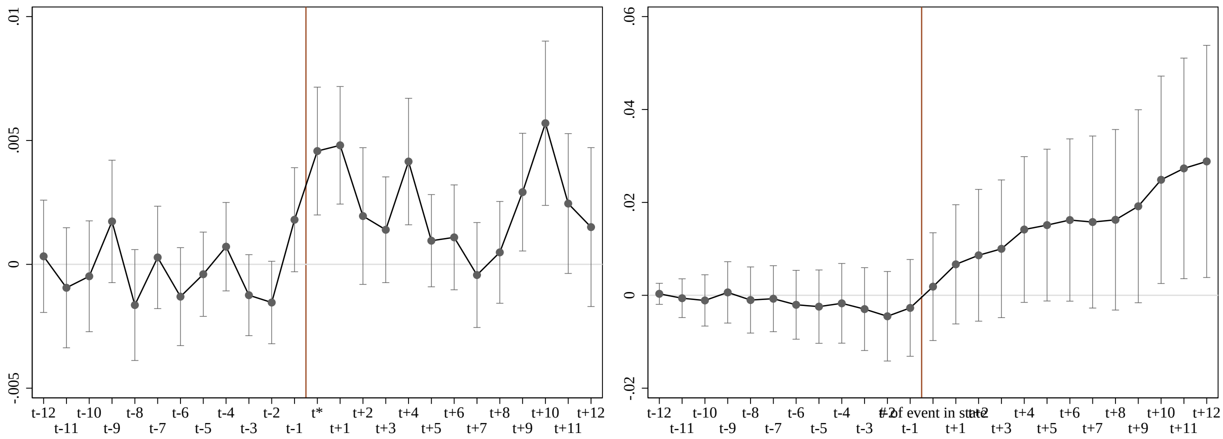
(a) Effects around legislation for hikes with long and short leads



(b) Effects around implementation for hikes with long and short leads

Baseline controls are time and state FE, local unemp. rate and house price growth. Figures show cumulative coefficients E_R and corresponding 90% confidence intervals based on robust SE. Dependent variable is $\pi_{s,t}$. Long: lead ¿ 3 months, average lead 15 months. Short: lead $\leq$ 3 months, average lead 2 months.

Figure 6: Estimated prie effects of the minimum wage in an event-state panel

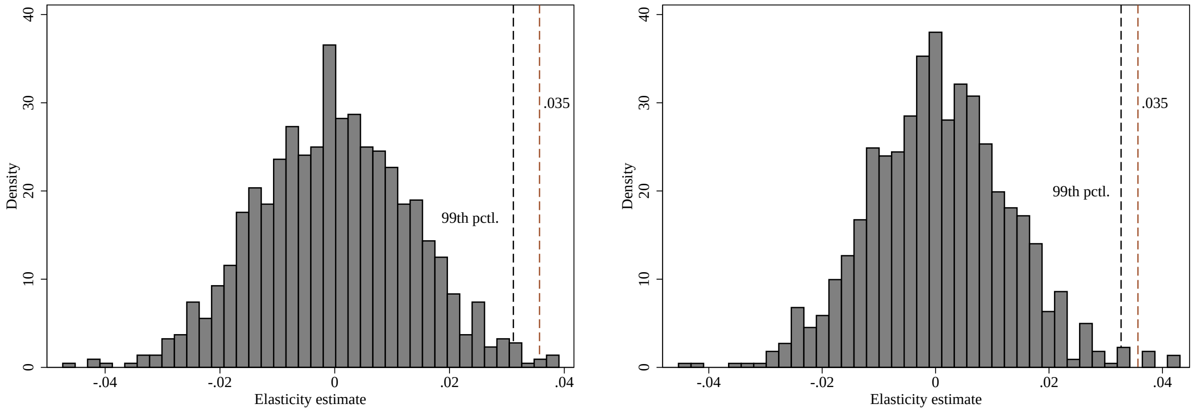


(a) Effect on the inflation rate

(b) Effect on the price level

The figure presents estimates of the event-state panel model of equation 3. The dependent variable is the monthly inflation rate. The panel on the right presents the estimates of α_r and the left panel their cumulative sum over the 24 month panel. Each panel also shows corresponding 90% confidence intervals based on cluster-robust SE, clustered on the state level. The controls included are time and state FE, local unemp. rate and house price growth.

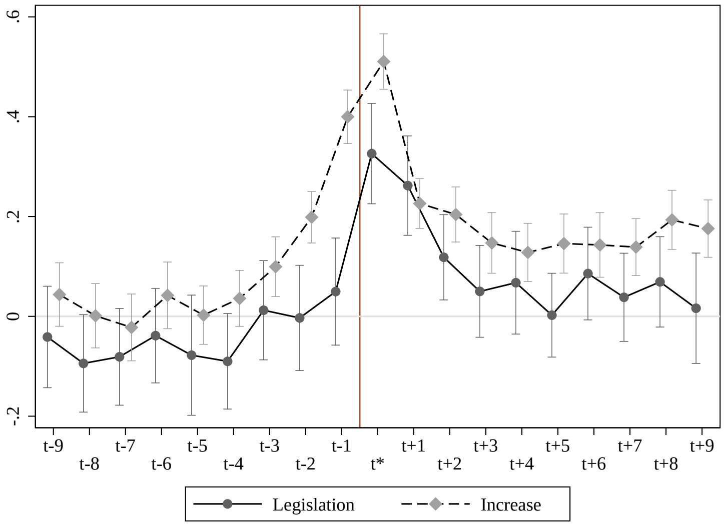
Figure 7: Placebo tests

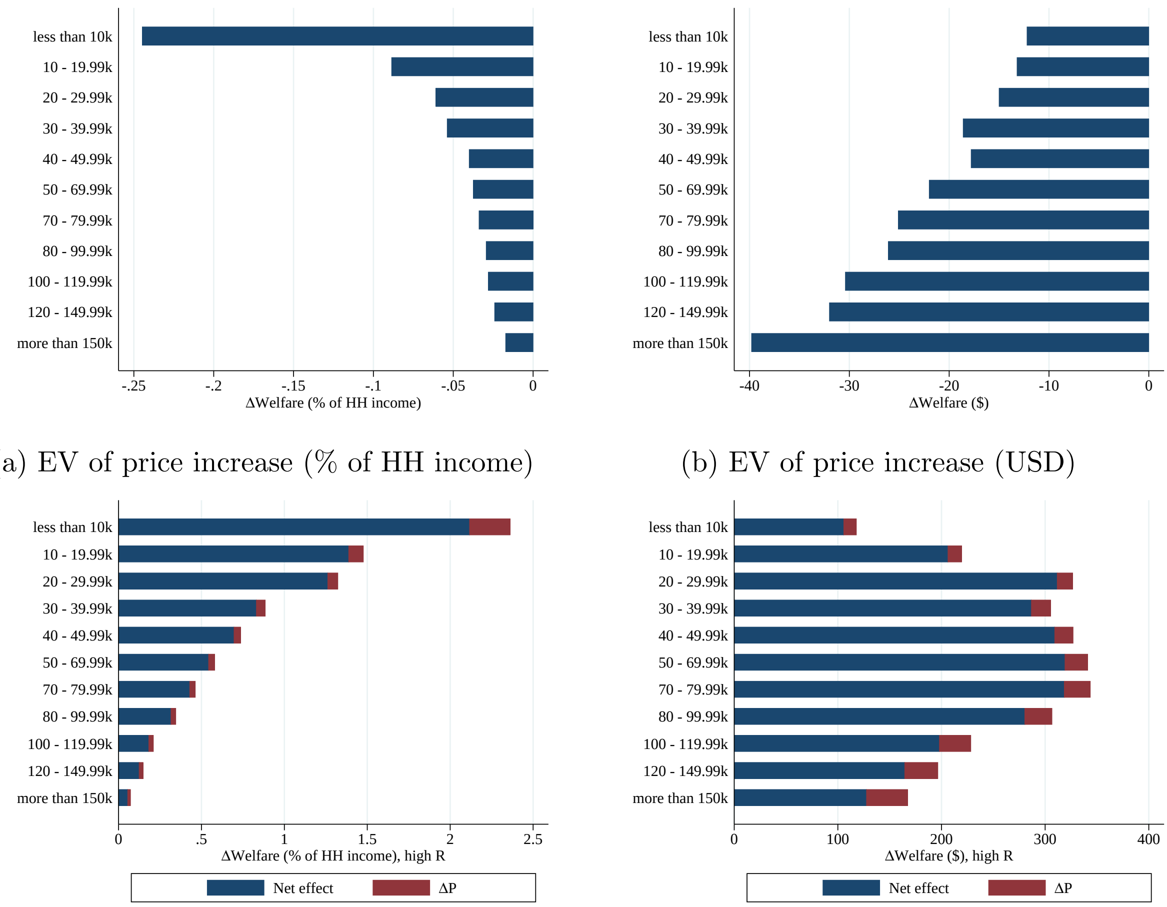


(a) State placebo (b) Time placebo

Histograms show distribution of elasticity estimates after 1000 repetitions. Placebo (a) matches state-level price series with random state-level minimum wage series (draws are with replacement and include the correct match). Placebo (b) matches correct states, but shifts the minimum wage series by a random amount of months. The minimum shift is the negative and the maximum shift the positive sample length. Observations that are shifted out of the sample period get reattached at the start/end.

Figure 8: Frequency of Google search for minimum wages

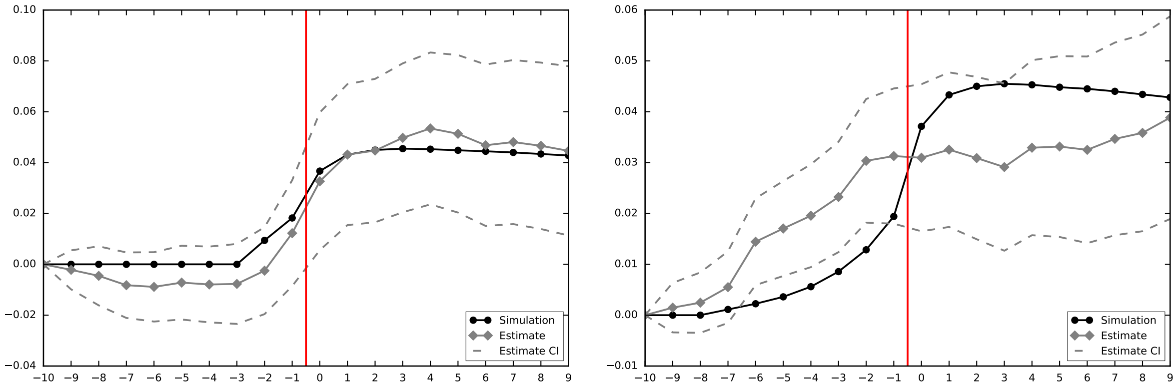




(a) EV of price increase (% of HH income) (b) EV of price increase (USD)

(c) Wage increase net of EV (% of HH income) (d) Wage increase net of EV (USD)

Figure 9: Equivalent variation of a 10% minimum wage increase

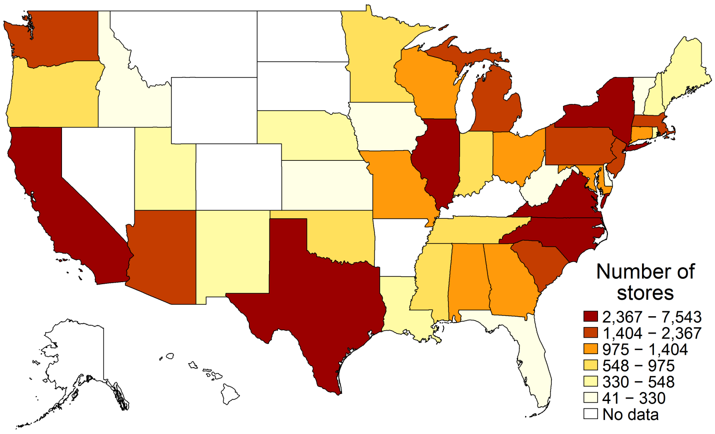


(a) 2 months lead time

(b) 8 months lead time

Figure 10: Model and data response to a minimum wage shocks of different lead time

Figure 11: Regional distribution of stores in IRI data across the US



Tables

Table 1: Descriptive statistics for minimum wage increases and legislation

	Hikes	Legislation
Size	0.0885	0.239
Distance to last	15.54	29.15
Distance to legislation	15.65	8.885
Federal	0.375	0.426
Indexation	0.206	
2001 - 2005	0.163	0.246
2006 - 2008	0.562	0.754
2009 - 2011	0.275	0
January	0.438	0.459
July	0.450	0.0492
Events per state	3.902	1.488
N	160	61

Table 2: Statistics on Minimum Wage employment in grocery stores and other retail

	employment		hours		earnings	
	atorlt	ripple	atorlt	ripple	atorlt	ripple
			2001 - 2005			
Grocery Stores	12.2	58.8	9.2	51.2	4.5	33.5
Other Retail	7.7	46.4	6.0	39.9	2.3	22.1
Wholesale Trade	3.3	22.3	2.9	20.1	0.8	9.3
Restaurants	31.5	76.3	26.4	70.0	12.8	49.2
Other Private	4.5	25.9	3.8	23.1	1.0	10.2
			2006 - 2008			
Grocery Stores	18.5	62.7	14.5	55.6	7.6	37.4
Other Retail	10.6	49.2	7.9	42.7	3.2	24.0
Wholesale Trade	3.4	22.5	3.0	20.6	0.8	9.4
Restaurants	37.0	78.4	31.4	72.7	16.5	51.9
Other Private	5.2	26.6	4.3	23.9	1.2	10.6
			2009 - 2011			
Grocery Stores	26.0	69.8	20.3	62.7	11.6	44.0
Other Retail	16.0	57.9	12.2	51.2	5.4	30.4
Wholesale Trade	4.5	29.3	3.9	27.0	1.2	13.1
Restaurants	45.6	84.3	38.6	79.3	22.5	60.2
Other Private	7.1	33.1	5.9	29.8	1.8	13.7

atorlt: $\leq$ 110% of local MW. ripple: $\leq$ 175% of local MW. All numbers are % of total for sector and are first calculated for each state and half-year and then averaged over all states and half-years for a period.

	2001-2006		2007-2011	
	Mean	Median	Mean	Median
Frequency of price change	0.117	0.103	0.132	0.122
Implied median duration	8.037	9.200	7.064	7.686
Frequency of price increase	0.067	0.060	0.078	0.074
Frequency of price decrease	0.050	0.040	0.054	0.043
Share of price increases in changes	0.605	0.576	0.623	0.602
Size of price change	0.154	0.114	0.144	0.105
Size of price increase	0.147	0.105	0.140	0.100
Size of price decrease	0.184	0.146	0.166	0.132
SD log Price	0.152	0.154	0.150	0.151
Monthly inflation	0.0007	0.0008	0.0016	0.0015

Table 3: Features of regular prices

Table 4: Effects at implementation

	(1) impl	(2) seasonal	(3) notrend	(4) censustrend	(5) ltrend	(6) tpoly	(7) eventwindow
Incr t-9 to t-7	0.003	0.004	0.001	0.003	0.002	0.001	0.002
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
Incr t-6 to t-1	0.025***	0.023***	0.022***	0.025***	0.023***	0.020***	0.022***
	(0.005)	(0.006)	(0.006)	(0.006)	(0.005)	(0.005)	(0.006)
Incr t to t+5	0.012**	0.014**	0.009	0.011**	0.009	0.006	0.003
	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.007)
Incr t+6 to t+9	0.005	0.005	0.003	0.004	0.003	0.002	-0.002
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.005)
Elasticity estimate	0.036***	0.037***	0.031***	0.036***	0.032***	0.026***	0.025**
	(0.009)	(0.008)	(0.009)	(0.009)	(0.009)	(0.009)	(0.011)
Full sum	0.044***	0.047***	0.036***	0.043***	0.038***	0.029**	0.024*
	(0.011)	(0.011)	(0.011)	(0.011)	(0.011)	(0.011)	(0.014)
N	5330	5330	5330	5330	5330	5330	5330

Baseline controls are time and state FE, local unemp. rate and house price growth. Table shows sum of regression coefficients for indicated months with robust SE in parenthesis. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. (1) Estimates effect at implementation. (2) Includes state specific calendar month dummies. (3) No state level inflation trends. (4) Census division inflation trend. (5) Controls for linear time trend. (6) Controls for quadratic time trend. (7) Controls for a fixed effect around each increase +/- 36 months.

Table 5: Effects at legislation

	(1) legisl	(2) seasonal	(3) notrend	(4) censustrend	(5) ltrend	(6) tpoly	(7) eventwindow
Legisl t-9 to t-7	0.002	-0.000	0.001	0.001	0.001	-0.000	-0.001
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Legisl t-6 to t-1	-0.002	-0.001	-0.003	-0.002	-0.002	-0.005	-0.003
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.004)
Legisl t to t+5	0.017***	0.016***	0.015***	0.016***	0.016***	0.013***	0.014***
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.004)	(0.004)
Legisl t+6 to t+9	0.003	0.004	0.002	0.002	0.002	0.000	-0.000
	(0.002)	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)
Elasticity estimate	0.017***	0.016***	0.015***	0.016***	0.016***	0.013***	0.014***
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.004)	(0.004)
Full sum	0.019***	0.019***	0.014**	0.018***	0.018***	0.009	0.009
	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.009)
N	5330	5330	5330	5330	5330	5330	5330

Baseline controls are time and state FE, local unemp. rate and house price growth. Table shows sum of regression coefficients for indicated months with robust SE in parenthesis. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. (1) Estimates effect at legislation. (2) Includes state specific calendar month dummies. (3) No state level inflation trends. (4) Census division inflation trend. (5) Controls for linear time trend. (6) Controls for quadratic time trend. (7) Controls for a fixed effect around each increase +/- 36 months.

Table 6: Joint estimation of effects at legislation and implementation

	(1) base	(2) seasonal	(3) notrend	(4) censustrend	(5) ltrend	(6) tpoly	(7) eventwindow
Legisl t-9 to t-7	0.002	0.001	0.001	0.002	0.002	0.000	-0.001
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Legisl t-6 to t-1	0.000	0.000	-0.002	-0.000	-0.000	-0.003	-0.002
	(0.003)	(0.004)	(0.003)	(0.003)	(0.003)	(0.004)	(0.004)
Legisl t to t+5	0.013***	0.014***	0.012***	0.013***	0.013***	0.011**	0.011**
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.005)
Legisl t+6 to t+9	-0.000	0.001	-0.001	-0.001	-0.000	-0.001	-0.001
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Incr t-9 to t-7	0.001	0.003	0.000	0.001	0.001	0.000	0.000
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
Incr t-6 to t-1	0.014**	0.012*	0.012*	0.014**	0.013*	0.010	0.012
	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.008)
Incr t to t+5	0.015**	0.016**	0.012*	0.015**	0.012*	0.009	0.005
	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.008)
Incr t+6 to t+9	0.007*	0.008**	0.006	0.007	0.005	0.004	0.000
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.005)	(0.005)
Elasticity estimate	0.042***	0.041***	0.036***	0.042***	0.038***	0.030***	0.028**
	(0.010)	(0.009)	(0.010)	(0.010)	(0.010)	(0.010)	(0.013)
Full sum	0.053***	0.054***	0.041***	0.051***	0.046***	0.030**	0.024
	(0.013)	(0.012)	(0.013)	(0.013)	(0.013)	(0.014)	(0.019)
N	5330	5330	5330	5330	5330	5330	5330

Baseline controls are time and state FE, local unemp. rate and house price growth. Table shows sum of regression coefficients for indicated months with robust SE in parenthesis. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. (1) Joint estimation of effect at legislation and implementation. (2) Includes state specific calendar month dummies. (3) No state level inflation trends. (4) Census division inflation trend. (5) Controls for linear time trend. (6) Controls for quadratic time trend. (7) Controls for a fixed effect around each increase +/- 36 months.

Table 7: Further robustness checks

	(1) noweight	(2) obsweights	(3) noweightsnocontrols	(4) divisiontime	(5) piPrMeanWd
Legisl t-9 to t-7	0.002	0.004	0.001	-0.001	0.004
	(0.003)	(0.003)	(0.003)	(0.003)	(0.005)
Legisl t-6 to t-1	-0.000	-0.000	-0.001	0.001	0.008
	(0.004)	(0.004)	(0.004)	(0.004)	(0.008)
Legisl t to t+5	0.012**	0.015***	0.012**	0.006	0.018**
	(0.005)	(0.005)	(0.005)	(0.006)	(0.009)
Legisl t+6 to t+9	-0.001	-0.003	-0.001	-0.001	0.001
	(0.004)	(0.003)	(0.004)	(0.004)	(0.007)
Incr t-9 to t-7	0.001	0.002	0.001	-0.002	-0.003
	(0.005)	(0.005)	(0.005)	(0.005)	(0.009)
Incr t-6 to t-1	0.013*	0.009	0.012	0.003	0.008
	(0.007)	(0.008)	(0.008)	(0.008)	(0.014)
Incr t to t+5	0.011	0.018***	0.009	0.012	0.004
	(0.008)	(0.006)	(0.008)	(0.008)	(0.013)
Incr t+6 to t+9	0.009*	0.005	0.009*	0.000	0.008
	(0.005)	(0.005)	(0.005)	(0.005)	(0.009)
Elasticity estimate	0.036***	0.042***	0.033***	0.022*	0.030
	(0.011)	(0.010)	(0.011)	(0.012)	(0.019)
Full sum	0.048***	0.049***	0.043***	0.019	0.047*
	(0.014)	(0.014)	(0.014)	(0.016)	(0.024)
N	5330	5330	5330	5330	5330

Baseline controls are time and state FE, local unemp. rate and house price growth. Table shows sum of regression coefficients for indicated months with robust SE in parenthesis. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. (1) No weights. (2) Uses average number of monthly price observations to weight states. (3) No weights, no controls except time FE. (4) Census-division-time fixed effect. (5) Does not correct for temporary price changes.

Table 8: Separate elasticities for Right-To-Work states

	(1) base (RTW)	(1) base (NRTW)	(2) ltrend (RTW)	(2) ltrend (NRTW)	(3) poly (RTW)	(3) poly (NRTW)	(4) eventwindow (RTW)	(4) eventwindow (NRTW)
Legisl t-9 to t-7	0.006*	-0.001	0.006*	-0.001	0.004	-0.002	0.004	-0.004
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.004)	(0.004)	(0.004)
Legisl t-6 to t-1	-0.004	-0.000	-0.005	-0.001	-0.008	-0.003	-0.006	-0.003
	(0.005)	(0.004)	(0.005)	(0.004)	(0.005)	(0.004)	(0.007)	(0.005)
Legisl t to t+5	0.016**	0.009*	0.015**	0.009*	0.013*	0.007	0.012	0.007
	(0.006)	(0.005)	(0.006)	(0.005)	(0.007)	(0.005)	(0.008)	(0.006)
Legisl t+6 to t+9	0.006	-0.003	0.006	-0.003	0.005	-0.003	0.003	-0.004
	(0.005)	(0.004)	(0.005)	(0.003)	(0.005)	(0.004)	(0.005)	(0.004)
Incr t-9 to t-7	0.006	-0.000	0.006	-0.001	0.005	-0.001	0.008	-0.002
	(0.009)	(0.005)	(0.009)	(0.005)	(0.009)	(0.005)	(0.009)	(0.006)
Incr t-6 to t-1	0.042***	0.001	0.041***	0.000	0.037***	-0.001	0.054***	-0.006
	(0.011)	(0.008)	(0.011)	(0.008)	(0.011)	(0.009)	(0.015)	(0.010)
Incr t to t+5	0.023*	0.014*	0.019	0.012	0.014	0.009	0.017	-0.001
	(0.012)	(0.007)	(0.012)	(0.007)	(0.013)	(0.008)	(0.015)	(0.009)
Incr t+6 to t+9	0.004	0.010*	0.002	0.008	-0.001	0.006	-0.003	0.000
	(0.008)	(0.005)	(0.008)	(0.005)	(0.008)	(0.005)	(0.009)	(0.006)
Elasticity estimate	0.080***	0.024**	0.075***	0.021*	0.064***	0.014	0.083***	-0.000
	(0.015)	(0.011)	(0.016)	(0.011)	(0.017)	(0.012)	(0.023)	(0.015)
Full sum	0.098***	0.030**	0.090***	0.025*	0.068***	0.012	0.089***	-0.013
	(0.019)	(0.014)	(0.019)	(0.014)	(0.022)	(0.017)	(0.034)	(0.023)
N	5330	5330	5330	5330	5330	5330	5330	5330

Coefficients for RTW and non-RTW states are estimated jointly. Baseline controls are time and state FE, local unemp. rate and house price growth. Table shows sum of regression coefficients for indicated months with robust SE in parenthesis. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.(1) Baseline with separate elasticities for RTW and non-RTW states. (2) Separate elasticities for RTW and non-RTW states, including linear time trend. (3) Baseline with separate elasticities for RTW and non-RTW states, including quadratic time trend. (4) Separate elasticities for RTW and non-RTW states, including eventwindow FE.

Table 9: Baseline estimates for (1) price indices with income specific quantity weights and (2) cheap and expensive stores

	(1) exp	(2) cheap	(3) low	(4) med	(5) hi
Legisl t-9 to t-7	0.002	0.001	0.003	0.003	0.003
	(0.003)	(0.003)	(0.002)	(0.002)	(0.002)
Legisl t-6 to t-1	0.002	0.005	0.002	0.002	0.002
	(0.004)	(0.004)	(0.003)	(0.003)	(0.003)
Legisl t to t+5	0.013***	0.019***	0.007*	0.007*	0.006
	(0.005)	(0.005)	(0.004)	(0.004)	(0.004)
Legisl t+6 to t+9	-0.003	0.004	0.002	0.003	0.003
	(0.004)	(0.004)	(0.003)	(0.003)	(0.003)
Incr t-9 to t-7	0.003	-0.003	0.000	-0.001	-0.001
	(0.006)	(0.005)	(0.004)	(0.004)	(0.004)
Incr t-6 to t-1	0.020**	0.004	0.018***	0.016***	0.017***
	(0.008)	(0.007)	(0.006)	(0.006)	(0.006)
Incr t to t+5	0.018**	0.011	0.007	0.005	0.004
	(0.008)	(0.007)	(0.006)	(0.006)	(0.006)
Incr t+6 to t+9	0.005	0.012***	0.003	0.004	0.006
	(0.005)	(0.004)	(0.004)	(0.004)	(0.004)
Elasticity estimate	0.050***	0.033***	0.032***	0.028***	0.027***
	(0.011)	(0.011)	(0.009)	(0.009)	(0.008)
Full sum	0.060***	0.052***	0.042***	0.039***	0.039***
	(0.015)	(0.014)	(0.011)	(0.011)	(0.011)
N	4813	4813	5330	5330	5330

Baseline controls are time and state FE, local unemp. rate and house price growth. Table shows sum of regression coefficients for indicated months with robust SE in parenthesis. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. (1) Inflation in expensive stores. (2) Inflation in cheap stores. (3) Inflation using low income expenditure weights. (4) Inflation using medium income expenditure weights. (5) Inflation using high income expenditure weights.

Table 10: Summary statistics for cost increase predictions

	cost elasticity				costincr			
	small	small only groc	big	big only groc	small	small only groc	big	big only groc
				2001 - 2005				
Overall	0.019	0.017	0.038	0.034	0.001	0.001	0.003	0.003
Right-To-Work	0.01	0.01	0.031	0.03	0.002	0.002	0.006	0.006
non Right-To-Work	0.019	0.018	0.038	0.035	0.001	0.001	0.003	0.003
				2006 - 2008				
Overall	0.018	0.018	0.039	0.037	0.002	0.002	0.004	0.004
Right-To-Work	0.016	0.015	0.037	0.036	0.002	0.002	0.005	0.005
non Right-To-Work	0.02	0.02	0.04	0.037	0.002	0.002	0.003	0.003
				2009 - 2011				
Overall	0.03	0.029	0.05	0.047	0.002	0.002	0.004	0.004
Right-To-Work	0.033	0.032	0.055	0.052	0.003	0.003	0.006	0.005
non Right-To-Work	0.026	0.025	0.045	0.042	0.001	0.001	0.003	0.002

Summary statistics for cost increase predictions for all minimum wage increases in different time periods. Small corresponds to small ripple effects (R=1.3), big corresponds to big ripple effects (R=1.75). Baseline measures include assumed full price pass-through in the wholesale sector. Elasticities are scaled by size of minimum wage increase.

Table 11: Pass-through estimates for high ripple effects (R=1.75)

	(1) hi OLS	(2) hi TSLS	(3) hi OLS ltrend	(4) hi TSLS ltrend	(5) hi OLS poly	(6) hi TSLS poly	(7) hi OLS window	(8) hi TSLS window
Legisl t-9 to t-7	0.128*	0.092	0.118*	0.082	0.068	0.023	0.114	-0.014
	(0.066)	(0.084)	(0.066)	(0.084)	(0.069)	(0.088)	(0.074)	(0.093)
Legisl t-6 to t-1	0.014	0.056	-0.007	0.035	-0.097	-0.081	0.158	-0.020
	(0.100)	(0.109)	(0.100)	(0.109)	(0.104)	(0.114)	(0.144)	(0.159)
Legisl t to t+5	0.335***	0.492***	0.324***	0.482***	0.261**	0.393***	0.474***	0.413**
	(0.119)	(0.137)	(0.120)	(0.137)	(0.123)	(0.138)	(0.154)	(0.168)
Legisl t+6 to t+9	0.054	0.016	0.060	0.021	0.025	-0.023	0.020	-0.065
	(0.091)	(0.094)	(0.091)	(0.094)	(0.090)	(0.093)	(0.096)	(0.095)
Incr t-9 to t-7	0.103	0.038	0.092	0.025	0.064	0.008	0.049	-0.037
	(0.160)	(0.165)	(0.161)	(0.167)	(0.161)	(0.165)	(0.170)	(0.170)
Incr t-6 to t-1	0.624***	0.524**	0.575***	0.469**	0.484**	0.385*	0.605**	0.342
	(0.201)	(0.225)	(0.201)	(0.224)	(0.204)	(0.222)	(0.247)	(0.269)
Incr t to t+5	0.278	0.590***	0.168	0.482**	0.086	0.363	0.088	0.141
	(0.185)	(0.222)	(0.189)	(0.231)	(0.195)	(0.231)	(0.244)	(0.285)
Incr t+6 to t+9	0.276**	0.274*	0.202	0.202	0.168	0.130	0.152	-0.026
	(0.136)	(0.155)	(0.140)	(0.160)	(0.143)	(0.162)	(0.174)	(0.194)
Elasticity estimate	1.237***	1.606***	1.067***	1.433***	0.830***	1.141***	1.168***	0.895*
	(0.284)	(0.340)	(0.289)	(0.354)	(0.306)	(0.359)	(0.424)	(0.495)
Elast.-1	0.237	0.606*	0.067	0.433	-0.170	0.141	0.168	-0.105
	(0.284)	(0.340)	(0.289)	(0.354)	(0.306)	(0.359)	(0.424)	(0.495)
Full sum	1.813***	2.082***	1.532***	1.798***	1.058**	1.197**	1.661**	0.733
	(0.375)	(0.457)	(0.388)	(0.487)	(0.434)	(0.515)	(0.669)	(0.781)
N	5330	5330	5330	5330	5330	5330	5330	5330

Baseline controls are time and state FE, local unemp. rate and house price growth. Table shows sum of regression coefficients for indicated months with robust SE in parenthesis. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. (1) OLS baseline using cost change due to minimum wage increase as independent variable. (2) TSLS using minimum wage changes as instruments for cost increase. (3) and (4) control for linear time trend. (5) and (6) control for cubic polynomial of time variable. (7) and (8) control for a fixed effect around each increase +/- 36 months.

Table 12: Pass-through estimates for high ripple effects (R=1.3)

	(1) lo OLS	(2) lo TSLS	(3) lo OLS ltrend	(4) lo TSLS ltrend	(5) lo OLS poly	(6) lo TSLS poly	(7) lo OLS window	(8) lo TSLS window
Legisl t-9 to t-7	0.235**	0.215	0.214*	0.194	0.127	0.058	0.255**	-0.012
	(0.113)	(0.180)	(0.114)	(0.181)	(0.120)	(0.188)	(0.128)	(0.201)
Legisl t-6 to t-1	0.122	0.200	0.078	0.150	-0.075	-0.119	0.479*	0.033
	(0.181)	(0.230)	(0.181)	(0.231)	(0.191)	(0.240)	(0.268)	(0.362)
Legisl t to t+5	0.616***	1.149***	0.589***	1.116***	0.474**	0.902***	0.939***	0.951***
	(0.212)	(0.287)	(0.213)	(0.288)	(0.221)	(0.290)	(0.281)	(0.367)
Legisl t+6 to t+9	0.118	0.090	0.124	0.088	0.053	-0.019	0.054	-0.123
	(0.158)	(0.192)	(0.158)	(0.191)	(0.158)	(0.190)	(0.171)	(0.199)
Incr t-9 to t-7	-0.058	0.103	-0.088	0.073	-0.145	0.026	-0.146	-0.071
	(0.296)	(0.357)	(0.295)	(0.360)	(0.298)	(0.357)	(0.311)	(0.367)
Incr t-6 to t-1	1.036***	1.194**	0.932***	1.075**	0.772**	0.867*	1.061**	0.767
	(0.360)	(0.474)	(0.361)	(0.474)	(0.370)	(0.469)	(0.451)	(0.577)
Incr t to t+5	0.501	1.335***	0.285	1.114**	0.172	0.836	0.390	0.368
	(0.331)	(0.491)	(0.337)	(0.514)	(0.351)	(0.514)	(0.438)	(0.655)
Incr t+6 to t+9	0.463*	0.605*	0.316	0.458	0.278	0.296	0.416	-0.030
	(0.250)	(0.347)	(0.258)	(0.360)	(0.264)	(0.364)	(0.317)	(0.452)
Elasticity estimate	2.153***	3.678***	1.807***	3.304***	1.418**	2.605***	2.390***	2.086*
	(0.518)	(0.776)	(0.525)	(0.816)	(0.569)	(0.824)	(0.786)	(1.182)
Elast.-1	1.153**	2.678***	0.807	2.304***	0.418	1.605*	1.390*	1.086
	(0.518)	(0.776)	(0.525)	(0.816)	(0.569)	(0.824)	(0.786)	(1.182)
Full sum	3.033***	4.891***	2.450***	4.268***	1.655**	2.848**	3.448***	1.883
	(0.690)	(1.072)	(0.707)	(1.157)	(0.816)	(1.205)	(1.246)	(1.912)
N	5330	5330	5330	5330	5330	5330	5330	5330

Baseline controls are time and state FE, local unemp. rate and house price growth. Table shows sum of regression coefficients for indicated months with robust SE in parenthesis. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. (1) OLS baseline using cost change due to minimum wage increase as independent variable. (2) TSLS using minimum wage changes as instruments for cost increase. (3) and (4) control for linear time trend. (5) and (6) control for cubic polynomial of time variable. (7) and (8) control for a fixed effect around each increase +/- 36 months.

Table 13: Welfare effects of a 10% minimum wage increase

	ΔP (\$)	ΔP (%)	ΔInc (%) lowR	ΔInc (%) highR	ΔP to ΔInc lowR	ΔP to ΔInc highR
less than 10k	−12.2	−0.24	1.6	2.36	−15.3	−10.4
10 - 19.99k	−13.2	−0.09	0.88	1.48	−10.0	−6.0
20 - 29.99k	−15.0	−0.06	0.71	1.32	−8.5	−4.6
30 - 39.99k	−18.6	−0.05	0.51	0.88	−10.5	−6.1
40 - 49.99k	−17.8	−0.04	0.37	0.74	−11.0	−5.4
50 - 69.99k	−22.0	−0.04	0.28	0.58	−13.3	−6.5
70 - 79.99k	−25.1	−0.03	0.22	0.46	−15.1	−7.3
80 - 99.99k	−26.1	−0.03	0.16	0.35	−17.9	−8.5
100 - 119.99k	−30.4	−0.03	0.1	0.21	−28.1	−13.3
120 - 149.99k	−32.0	−0.02	0.07	0.15	−33.5	−16.3
more than 150k	−39.8	−0.02	0.04	0.07	−42.6	−23.8

Welfare impact is calculated as the Equivalent Variation of increasing all binding MWs by 10%, assuming ripple effects up to 175% of the old MW and a MW elasticity of prices of 0.048. % indicates numbers in % of a bracket's mean total household income.

Parameter	Symbol	Value	Source
Discount factor	β	$0.96^{1/12}$	Nakamura and Steinsson (2008)
Inflation trend	π_O	0.0013	Average in IRI data
Supermarket share in CPI	ω	0.098	Share of food at home, pers. care products, housekeeping supply in CEX
Demand elasticity	θ	4	Nakamura and Steinsson (2008)
Persistence of prod.	ρ	0.66	Chosen jointly to match price adjustment in IRI data
Std. Dev. of prod.	σ	0.06	
Menu cost	K	0.035	
Anticipation length	k	4	Avg. announcement for short announcement hikes
Minimum wage increase	Δ	1.088	Average size of minimum wage hike
Cost sensitivity to minimum wage shock	α	0.01 - 0.03	Cost share of minimum wages

Table 14: Parameter values

Table A.1: Characteristics of minimum wage hikes

	Level of minimum wage		
	Federal	**State**	**Total**
Mechanism of adjustment			
Ballot	0	5	5
Indexation	0	33	33
Legislation	60	62	122
Total	60	100	160
Announcement			
1–3 months	0	9	9
4–11 months	16	27	43
12–23 months	20	25	45
more than 24 months	24	6	30
Total	60	67	127
Events per state			
2 events	0	2	2
3 events	17	7	24
4 or more events	3	12	15
Total	20	21	41
Year of event			
2002–2005	0	26	26
2006–2008	36	54	90
2009–2011	24	20	44
Total	60	100	160
Size of minimum wage hike			
0–6%	9	54	63
6–12%	35	25	60
more than 12%	16	21	37
Total	60	100	160
Month of event			
January	0	70	70
July	60	12	72
Other	0	18	18
Total	60	100	160
Months since last event			
First event	16	25	41
1–11 months	4	9	13
12 months	32	53	85
More than 12 months	8	13	21
Total	60	100	160

	All BEA	All CEX	First quintile	Second quintile	Third quintile	Fourth quintile	Fifth quintile
			Goods sold at Grocery Stores				
2001 - 2005	0.099	0.102	0.143	0.127	0.111	0.101	0.081
2006 - 2008	0.097	0.097	0.131	0.116	0.104	0.097	0.079
2009 - 2011	0.099	0.102	0.14	0.122	0.108	0.099	0.085
			Goods sold at Restaurants				
2001 - 2005	0.049	0.056	0.056	0.053	0.056	0.059	0.057
2006 - 2008	0.049	0.054	0.05	0.052	0.055	0.057	0.054
2009 - 2011	0.049	0.053	0.049	0.049	0.051	0.054	0.054

Shares are computed using the Consumer Expenditure Survey. Grocery stores includes expenditure for goods typically bought at grocery stores: Food at home, houskeeping supplies and personal care products and services. Restaurants includes Food away from home. The grocery share computed using BEA data allows for a more precise definition of products included.

Table 16: Expenditure shares for different income quintiles