The Pass-through of Minimum Wages into US Retail Prices: Evidence from Supermarket Scanner Data[a]

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Abstract

This paper estimates the pass-through of minimum wage increases into the prices of US grocery and drug stores. We use high-frequency scanner data and leverage a large number of state-level increases in minimum wages between 2001 and 2012. We find that a 10% minimum wage hike translates into a 0.36% increase in the prices of grocery products. This magnitude is consistent with a full pass-through of cost increases into consumer prices. We show that price adjustments occur mostly in the three months following the passage of minimum wage legislation rather than after implementation, suggesting that pricing of groceries is forward-looking.

Keywords: Minimum wages, inflation, retail prices, price dynamics, price pass-through

JEL: E31, J23, J38, L11, L81

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1 Introduction

In recent years, a number of US states and municipalities have increased their minimum wage, in a context of low wage growth and stagnation of the federal minimum wage. Similarly, several European countries have introduced a national minimum wage (e.g., Germany) or hiked their minimum wage (e.g., the United Kingdom). A large body of research in economics shows that moderate increases in the minimum wage have no or limited dis-employment effects (see e.g., Card and Krueger, 1994; Belman and Wolfson, 2014; Cengiz et al., 2019), suggesting that such a policy can raise nominal incomes of low-wage workers. However, there is much less evidence on how changes in the minimum wages affect consumer prices (see Lemos (2008) for a literature review) and therefore *real* incomes. In principle, it is possible that nominal wage increases for low-wage workers may be partly offset by increases in the prices of the goods and services consumed by the poorest households. To assess the economic impact of minimum wage changes on real incomes, it is thus central to understand the pass-through of minimum wage increases into prices.

In this paper, we study the pass-through of minimum wages increases into prices in the US. We exploit a large number of changes in the minimum wage between 2001 and 2012 and leverage scanner-level data from weekly price observations of 2,500 distinct grocery and drug stores. We make three main contributions. First, we provide new evidence on how minimum wages affect prices in the grocery sector, which had not been previously studied in the literature.[1] The grocery sector is especially important because the share of minimum wage labor costs in groceries' marginal cost is sizable, and because groceries make up a large share of consumer expenditure, up to 15% for low-income households. Second, we take advantage of the high frequency of scanner data to study the dynamics of the price response over time. Since minimum wage laws are usually passed several months before implementation and typically set a schedule of increases rather than one-off hikes,

[1] In this article, we use "grocery sector" for grocery and drug stores. Likewise, we sometimes use "grocery stores" for grocery stores *and* drug stores.

firms may increase prices in anticipation of higher future minimum wages. We use a newly collected dataset with legislation dates for every minimum wage increase in our sample period and we find strong evidence for anticipation effects. Third, we use a large consumer panel data linked to the store-level information to investigate how the price response varies across household income groups. This allows us to better understand the implications of minimum wage changes for real incomes.

Our main finding is that there is a full pass-through of minimum wage increases into grocery prices. Our main research design compares monthly price movements across states exploiting time variation in state-level minimum wage hikes. We supplement this approach by using a second identification strategy that exploits within-state variation in the bite of the hikes. We find that a 10% minimum wage hike translates into a 0.36% increase in grocery prices. Importantly, there is no statistically significant difference between the average price elasticity of 0.036 and our estimate of the minimum wage elasticity of groceries' costs, which suggests a full pass-through of minimum wage cost increases into prices. We do not find evidence that the demand for grocery products changes, nor do we find evidence that stores reduce employment. Taken together, these results suggest that consumers, rather than firm-owners or workers, bear the bulk of the burden of minimum wage increases in the grocery sector.

Another important finding of this paper, with implications for macroeconomic models, is that price adjustments occur mostly in the three months following the passage of a minimum wage legislation, rather than after implementation. In other words, grocery and drug stores appear to be forward-looking in their pricing decisions. Using Google Trends data, we show that the legislation of minimum wage increases represents a very salient event in the public. Based on flexible event study regressions tracking prices around the months in which minimum wage hikes are legislated, we find that grocery stores respond to future cost increases by increasing prices months before the minimum wage is actually implemented. This type of forward-looking behavior of firms is qualitatively consistent with the predictions of purely rational models, where firms think about the future as well as the present (i.e. they are not myopic). The rise in prices occurs mostly through an increase in the frequency of price changes.

Last, we quantify the welfare consequences of minimum wage hikes after accounting for our estimated pass-through of minimum wages into prices. We estimate that the price effects of minimum wage increases are similar for goods usually consumed by low-income and high-income households. Low-income households are nevertheless disproportionately affected by the rise in grocery prices since a larger share of their expenditures is on groceries. The rise in grocery store prices following a \$1 minimum wage increase reduces real income by about \$19 a year for households earning less than \$10,000 a year, and by about \$63 a year for those earning more than \$150,000. The price increases in grocery stores offset only a relatively small part of the gains of minimum wage hikes. Minimum wage policies thus remain a redistributive tool even after accounting for price effects in grocery stores.

This paper contributes to a body of work in labor economics and macroeconomics.

First, this paper provides novel insights into the redistributive effects of minimum wages and into the price effects of minimum wages in low-wage sectors. A small literature studies the product market effects of minimum wage increases. This literature has focused on restaurants (see e.g., Aaronson, 2001; Allegretto and Reich, 2018).[2] Our contribution to this literature is to study the impact of minimum wage changes in a new sector, the grocery sector. This sector employs a high and rising share of workers at or just above the minimum wage: we document that the share of workers earning below 110% of the minimum wage was 12% in 2001-2005; it was up to 25% in 2010-2012. We also document the existence of large spikes around the minimum wage in this sector. Almost 50% of workers earned below 130% of the minimum wage in 2010-2012.[3] Therefore, the effect of minimum wage hikes is potentially large. Moreover, groceries are an important component of households' cost of living, particularly for poor households. Groceries make

[2] Outside of the US, Fougère et al. (2010) analyze the response of restaurant prices to an increase in the French minimum wage. Harasztosi and Lindner (2019) analyze the price response of a large minimum wage increase in Hungary in the manufacturing sector.

[3] See Appendix Table H.1 and Figure B.1.

up 11% of household expenditures, two to three times more than spending on restaurant meals, depending on household income (see Appendix Table B.2).

We break new ground in documenting the price response in the retail sector thanks to the availability of high-quality scanner-level data. The use of this data enables us to overcome certain shortcomings in studies of the price effects of minimum wages. These limitations include classical measurement error (Card and Krueger, 1994; Aaronson, 2001), the use of city-level CPI data that are only available in the largest US metro areas (Aaronson, 2001; Aaronson and French, 2007; Aaronson et al., 2008), and the fact that price and wage changes in restaurants may not be well measured due to tipping and quality changes (e.g., size of portions served). These concerns do not apply to retail scanner data, as products in grocery stores are very standardized and retail workers are not tipped. Compared to official BLS price indexes, our micro-data allow us to compute price changes by income group (as well as price changes conditional on non-zero price adjustments).[4]

Most closely related to our work are the contemporary papers by Leung (2020) and Ganapati and Weaver (2017), who also study the pass-through of minimum wage changes into retail prices. These papers focus on a different period (2006–2015 and 2005–2015, respectively, vs. 2001–2012 in our study), are based on another dataset (the Nielsen data), and use different identification strategies. Ganapati and Weaver (2017) finds a zero pass-through of minimum wage increases into prices, and Leung (2020) more than a full pass-through. In Appendix K, we reconcile our findings with these two studies. The main substantial difference between our work and these two studies is that we document the forward-looking pricing decision of grocery stores, by studying the effect of minimum wage legislation (before implementation) on subsequent price changes. Two other distinctive features of our work are that we study in detail whether our results are consistent with full pass-through of prices into costs, and that we quantify the extent to which the price

[4]The official BLS indexes, although less detailed than micro-data, also have a number of strengths to study the effect of minimum wage changes: they are weighted at all levels of aggregation, rely on case-by-case adjustments for item turnover, and the BLS has established procedures for dealing with missing price observations. See Section 2.1 below for a comparison of the price indices constructed using our data and the official BLS price indices.

increases in grocery stores affect the redistributive effects of minimum wage policies.

Second, our paper contributes to the macroeconomic literature on price-setting. We provide causal evidence of the effect of a rise in labor costs on retail price inflation. This adds to the macro literature that has mainly focused on the effects of rising wholesale costs on pricing decisions.[5] Our detailed micro-data allow us to document a price response to a future cost shock at the time it becomes known and several months before it actually occurs. Because minimum wage changes can be seen as a shock to grocery store activities which is plausibly exogenous, these shocks can help identify the effect of movements in costs on prices. Our results highlights the role of expectations in the propagation of shocks. Forward-looking price setting is a central prediction of state-dependent models (i.e. menu cost models), as well as time-dependent models with nominal frictions. These latter models include the Calvo (1983) model of staggered price setting and models with adjustment costs such as Rotemberg (1982). In the macroeconomics literature, these models have been used as a microeconomic foundation for the New Keynesian Phillips Curve (see, e.g., Galí, 2015).

Finally, we contribute to the research on price rigidity in retail chains. We provide evidence that chains try to maintain uniform prices across grocery stores in the US. We we find that, within interregional chains, a minimum wage hike in one state affects prices in stores within the same chain located in another state. These results suggest that minimum wage hikes can affect consumer welfare in other states. Consistently, we find that grocery prices are more responsive to local minimum wage hikes in regional chains than in national chains. This is consistent with Dellavigna and Gentzkow (2019) who document uniform pricing decisions in the retail sector in response to local economic shocks in general, and to Leung (2020) who documents this behavior in the case of local minimum wage hikes.

The remainder of this paper is organized as follows. The next section details the

[5] For instance, Eichenbaum et al. (2011) find that pass-through is complete but somewhat delayed. Nakamura and Zerom (2010), using variation in the market price of commodity coffee, find that the pass-through into wholesale prices is about one third, but that the increase of wholesale prices is completely passed through to consumers by retail stores.

data we use. Section 3 describes our main identification strategy. Section 4 contains our estimates of the retail price elasticity with respect to the minimum wage, discusses robustness checks and analyzes the heterogeneity of this price response along several dimensions. Section 5 characterizes the anatomy of the price response. Section 6 studies the magnitude of the price pass-through elasticity. Section 7 concludes.

2 Data description

We combine several data sets to conduct our empirical analysis. We begin by describing the construction of our key dependent and explanatory variables before detailing the other data sources we use.

2.1 Retail price data: IRI data

Retail scanner data. Our empirical analysis is based on scanner data provided by the market research firm Symphony IRI. The dataset is described in detail in Bronnenberg et al. (2008) and Kruger and Pagni (2013). It contains weekly prices and quantities for 31 product categories sold at grocery and drug stores between January 2001 and December 2012. The estimation sample covers 2,493 distinct grocery and drug stores and contains their zip code location.[6] It provides information on an average of 60,600 products over this period. Products are identified by Unique Product Codes (UPC). As an example, a 12oz can and a 20oz bottle of Coca Cola Classic are treated as different products in our data. Stores are located in 530 counties, 41 states and belong to one of about 90 retail chains. The data covers 17% of US counties which are home to about 29% of the US population.[7] Most of the included product categories are packaged food products (frozen pizza, cereals, etc.) or beverages (soda, milk, etc.). The data also includes personal care products (deodorants, shampoo, etc.), housekeeping supplies (detergents, paper towels, etc.), alcoholic beverages (beer and some flavored alcoholic beverages) and tobacco.

[6]Grocery stores make up three-quarters of the stores' sample. Drug stores make up one fourth.
[7]Figure A.1 in the appendix shows the regional distribution of stores.

Our key dependent variable is the monthly store-level price inflation, defined as follows:

$$\pi_{jt} = \log I_{jt} \text{ with } I_{jt} = \prod_{c} I_{cjt}^{\omega_{cjy(t)}} \quad (1)$$

where π_{jt} is the inflation rate in store j in month t; I_{jt} is a single Laspeyres price index at the store level that aggregates price changes across product categories c; the weight $\omega_{cjy(t)}$ is the share of product category c in total revenue in store j and month t. We detail in Appendix A how we constructed store-level price indices.

There are several reasons why we use store-level indices instead of more disaggregated product level price data. First, wages are paid at the store and not at the product level, and we thus think that stores are the natural unit of analysis. Second, it is useful to weight products by their importance for stores and consumers, and store-level price indices are a natural way to do so. Third, entry and exit are much less of a concern at the store level than at the product level. Especially low-volume products are frequently introduced and discontinued, and may also go unsold for extended time periods due to stock-outs, seasonality or low demand. This results in frequent gaps in products' price series. By contrast, our panel at the store level is much more balanced. A fully balanced panel is obtained when we conduct our analyses at the state-level, rather than at the store level. We show in section 4.2 and Appendix C that our main results are robust to changing the level of analysis from the store to the state level.

Our approach closely follows methods used in previous articles on retail price movements (see, e.g., Coibion et al., 2015). We show in appendix A that the features of our price index are in line with what other researchers have documented for the IRI Symphony data, and other retail price datasets. We also show that our price index correlates well with inflation measures provided by the Bureau of Labor Statistics (see Appendix Figure A.2). Importantly, we apply a filter suggested by Kehoe and Midrigan (2015) to remove temporary price fluctuations (i.e. sales). The algorithm uses a moving window modal price to determine a "regular price" at any point in time. There are two reasons why we remove sales from our price series: first, we are interested in capturing the *permanent* effect of minimum wages on prices; second, we are interested in studying the *dynamics* of the price response – something that turns out to be empirically infeasible in our demanding specification when sales are inclduded in the price series, because of the multicollinearity issue it introduces.[8] We discuss the two reasons of this choice in more detail in Section 4.2, and show how incorporating sales affects our results.

Consumer panel data. In addition to the retail scanner data, IRI provides a consumer panel dataset with shopping data for about 5,000 households in two local markets: Eau Claire, Wisconsin and Pittsfield, Massachusetts. In general, the shopping data also covers purchases at grocery and drug stores that are not covered by the IRI price data. The panel contains details about household demographic characteristics (e.g., race and education) and most importantly for us, pre-tax household income (in relatively detailed brackets). This is the data we use in section 4.3 to study whether prices of goods consumed by low-income households increase more than prices of goods of high-income households.

2.2 A new minimum wage database

We construct a minimum wage database of federal and state-level minimum wage increases by pulling together data from the Tax Policy Center, the US Department of Labor, and state departments of labor. For each state, we collect the legally binding rate, i.e. the maximum of federal and state rates.[9]

The novelty of our database is that in addition to the implementation dates of minimum wage hikes, we collect information on the time that each minimum wage law is passed, derived from legislative records and media sources. Since most minimum wage

[8]The multicollinearity issue arises because sales lead to a very strong seasonal pattern in month-on-month inflation rates. Including sales impairs our ability to separate seasonal movements in prices from the effects of minimum wages which are often implemented step by step in intervals of 12 months. This makes our specification in monthly first-differences rather sensitive to specification choices

[9]We focus on state-level minimum wage changes in our paper, and not on city or county-level changes, because from 2001 to 2012, only San Francisco, CA, and Santa Fe, NM, had local minimum wage ordinances.

increases are known in advance, firms potentially have ample time to act in anticipation.

In some cases, passage of legislation is preceded by a series of votes and negotiations; in this case, we try to assess from media sources at which point in the process a minimum wage increase became certain. A good example is the "Fair Minimum Wages Act of 2007" that raised the federal minimum wage from \$5.15 an hour to \$7.25 in three steps in July 2007, 2008 and 2009. The act was passed in slightly different versions in January 2007. After a conference committee added tax-cuts for small businesses to the bill, the final version was passed and signed by President Bush in May 2007. Since the passage of the actual minimum wage part of the bill seemed certain already in January, we use January as the month of legislation in our baseline.[10]

An important assumption of our approach is that the legislation dates represent points in time when future minimum wage increases become more salient. We use Google Trends data to assess the plausibility of this assumption. Google trends is available from 2004 onward. We use the search volume for the term "minimum+wage+*statename*" over a month to measure interest in the local minimum wage of a given state.[11] We then estimate the following simple regression using this data:

$$\log search_{s,t} = \delta_s + \gamma_t + \sum_{r=-k}^{k} \beta_r imp_{s,t-r} + \sum_{r=-k}^{k} \alpha_r leg_{s,t-r} + \epsilon_{s,t}. \quad (2)$$

$imp_{s,t-r}$ and $leg_{s,t-r}$ are dummy variables indicating implementation of a higher minimum wage and passage of minimum wage legislation in state s in period $t-r$. The results of this regression are presented in Figure 1a. Both around implementation and around the date of legislation, interest in minimum wages goes up substantially, by about 30% immediately after legislation is passed. There is no elevated interest in minimum wages in the months before legislation is passed. Three months after passage of legislation, search volume is back at the baseline value. These results show that the passage of minimum

[10] We present results using only state-level legislation to show that our conclusions hold more generally and are not driven by this single important event.

[11] Note that we do not measure search requests originating from different states, but from the US as a whole for different search terms.

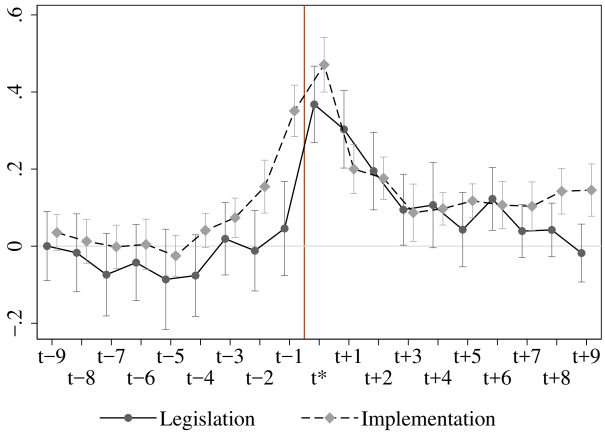
wage legislation is a salient event and that the public takes notice of pending minimum wage increases when they are written in law. The results also validate our coding choices in the collection of legislation dates.

The primary explanatory variables in our analysis are changes in the implemented minimum wage and changes in the "legislated minimum wage." Figure 1b shows how we measure the "legislated minimum wage". It is the highest future binding minimum wage set in current law. The legislated minimum wage increases to the highest future minimum wage at the time the law is passed.

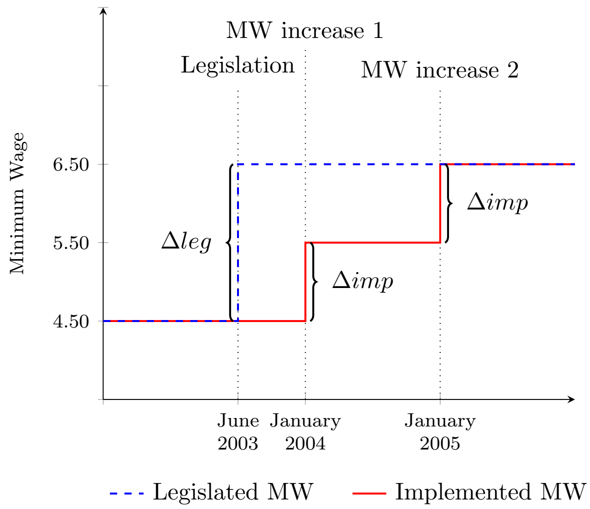
We leverage 166 changes in the implemented minimum wage and 62 changes in the legislated minimum wage. This allows us to exploit variation in minimum wages across states, time and size of hikes. Figure 2 shows the distribution of changes in the implemented and legislated minimum wage over states and time. States in our sample experience between 2 and 11 hikes. Most of the events in our sample occur between 2006 and 2009. The average increase in the binding minimum wage amounts to 8.2% (see Appendix Table B.1). Changes in the legislated minimum wage are larger on average (20%), since they usually encompass several steps. The average interval between passage of legislation and implementation of a first hike is 9 months. Hence, even the first increase in a package is often known long before it is implemented. Moreover, 36% of all increases in the implemented minimum wage and 42% of increases in the legislated minimum wage result from changes at the federal level. 24% of all increases in the implemented minimum wage result from indexation. Minimum wages in states with indexation are pegged to the national development of prices and exhibit small annual increases. We do not assign legislation dates to increases following from indexation.[12]

[12]Indexation is practiced in 10 states at the end of our sample period. The following states in our sample have indexation: Arizona, Colorado, Florida, Missouri, Montana, Nevada, Ohio, Oregon, Vermont, and Washington. Most of these states introduced indexation starting in 2008 after ballots held in November 2006. The exceptions are Florida, Vermont (both began indexation in 2007), Oregon (beginning in 2004) and Washington (beginning in 1999).

Figure 1: Explanatory variables: Changes in implemented and legislated minimum wage



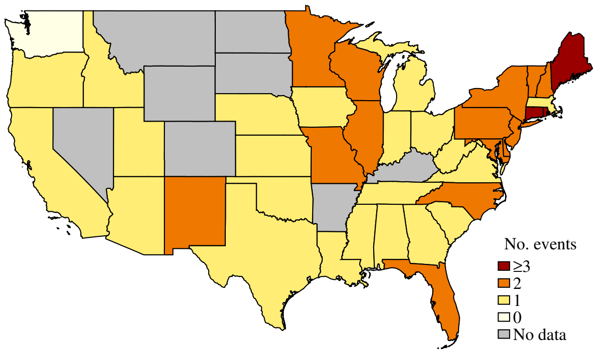
(a) Google search volume for "minimum wage *statename*" around legislation and implementation of minimum wage increases



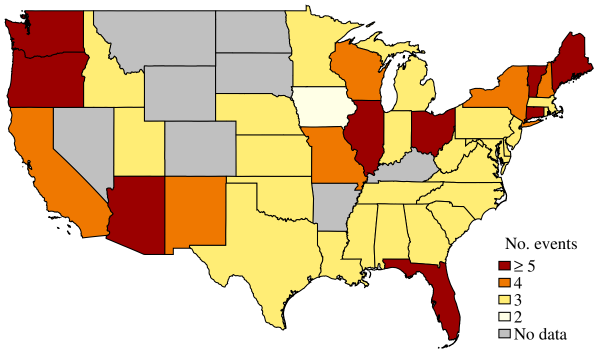
(b) Example for the measurement of explanatory variables

Notes: Panel (a) shows the log change in monthly Google search volume for the search term "Minimum wage+*statename*" around changes in minimum wage legislation and implementation of higher minimum wages in state *statename*. The coefficients are estimated from equation 2. The effects are relative to state and time fixed effects. Note that the search terms differ between states, but measured search volume is for United States as a whole. Panel (b) illustrates the measurement of changes in the legislated and implemented minimum wage based on an hypothetical minimum wage increase in two steps. In June 2003, legislation is passed that will increase the minimum wage in from an initial value of $4.50 to $6.50. The law schedules an increase to 5.50 in January 2004, and to 6.50 in January 2005. Our measure of the legislated minimum wage is equal to 4.50 before June 2003. It increases to 6.50 when the legislation is passed in June 2003. Before June 2003 and after January 2005 the legislated minimum wage is equal to the implemented minimum wage.

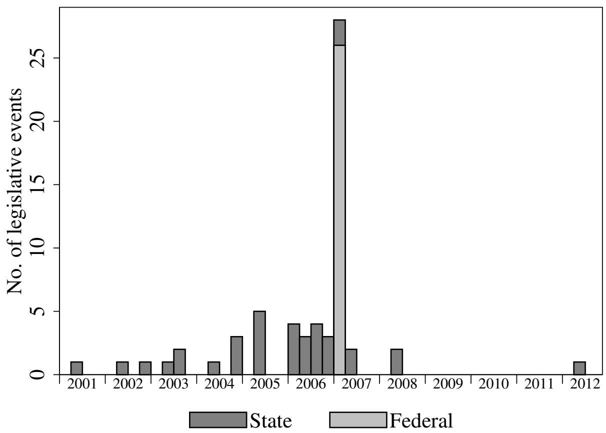
Figure 2: Distribution of minimum wage hikes and legislative events over time and states



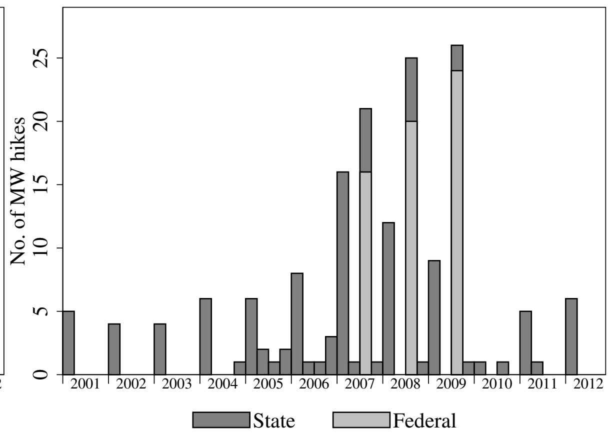
(a) Legislative events by state



(b) Minimum wage increases by state



(c) Legislative events over time



(d) Minimum wage increases over time

Notes: The figure illustrates the distribution of changes in the implemented minimum wage and changes in the legislated minimum wage over time and states. Overall, we observe 166 increases in the implemented minimum wage and 62 legislative events from 2001 to 2012. 60 changes in the implemented minimum wage and 26 changes in the legislated minimum wage follow from federal minimum wage policy. The remainder follows from state-level policies.

2.3 Other data sources

We rely on several other data sources in our empirical analyses, and detail them in the relevant sections: data on employment and wages come from the Bureau of Labor Statistics (BLS) Quarterly Census of Employment and Wages (QCEW) files (see section 6.2) and the Current Population Survey (CPS) (see Figure B.1 and Appendix B); data on house prices are quarterly state-level series from the Federal Housing Finance Agency interpolated to monthly frequency using monthly division-level indices based on the Denton method; data on the share of labor costs and wholesale costs in grocery stores come from the BLS Annual Retail Trade Survey Consumption data (see section 6.2); consumption data come from the Consumer Expenditure Survey (CES) (section 6.2); and wholesale data from the annual BEA input-ouput tables (see section 6.2).

3 Main identification strategy

We estimate the price response to minimum wage increases by relating month-on-month store-level inflation rates to increases in the binding minimum wage and passage of minimum wage legislation at the state level. The identification strategy is based on the idea that, conditional on a set of controls and fixed effects, inflation in stores in states that did not experience a minimum wage hike or new legislation is a useful counterfactual for stores in states that did. Many papers studying the effects of minimum wages in the US apply variants of this identification strategy (see Allegretto et al., 2017). The high frequency of our price data allows us to estimate detailed temporal patterns of the effects before and after an event. We use a flexible first-differenced specification to capture the dynamics of the price response over time, as, e.g., proposed by Meer and West (2016) in the minimum wage context and similar to the specification commonly used in the international economics literature to study the pass-through of exchange rate variation (for example Gopinath et al., 2010):

$$\pi_{j,t} = \delta_j + \gamma_t + \sum_{r=-k}^{k} \beta_r \Delta imp_{s(j),t-r} + \sum_{r=-k}^{k} \alpha_r \Delta leg_{s(j),t-r} + \psi X_{j,t} + \epsilon_{j,t} \tag{3}$$

In this model, $\pi_{j,t}$ is the month-on-month inflation rate in grocery store j and calendar month t. The main exogenous variables of interest are the change in the logarithm of implemented and legislated minimum wages in the state $s(j)$ in which store j is located, which we denote $\Delta imp_{s(j),t}$ and $\Delta leg_{s(j),t}$, respectively. The coefficients β_r and α_r measure the elasticity of inflation with respect to minimum wage increases or legislation r months ago, or r months in the future in case r is negative. In our baseline estimation we control for time fixed effects γ_t and store fixed effects δ_j. Because our estimation is in first differences, the latter account for *trends* in stores' price levels.

The vector of controls $X_{j,t}$ includes the county-level unemployment rate and state-level house price growth. We include these control variables to absorb variation in grocery prices that is due to business cycles or the boom and bust in house prices (see Stroebel and Vavra, 2019). Yet, we show below that our results are very similar if we omit these controls.

We start by estimating the effects at legislation and implementation separately by omitting all terms related to either $\Delta imp_{s(j),t}$ or $\Delta leg_{s(j),t}$. However, in our preferred specification, we jointly estimate effects at legislation and implementation of minimum wage increases. The reason is the the separate estimates may capture the same variation in prices since legislation is often passed in the 9 months preceding implementation. We cluster our standard errors at the state level. The resulting standard errors are conservative and substantially larger than the standard errors that we would get if we clustered at the store level, for example.

While our estimates of equation 3 are in first differences, the estimates are best illustrated as the effect of minimum wages on the price level. We thus construct cumulative sums of β_r and α_r coefficients in the presentation of our results. We normalize the effect to zero in a baseline period two months before an event, and calculate the cumulative effect as $E_R = \sum_{r=-1}^{R} \beta_r$. We also summarize pre-event coefficients in a similar way. To be consistent with the normalization we calculate them as $P_R = -\sum_{r=2}^{-R-1} \beta_{-r}$. Our baseline measure of overall elasticities is E_4 and thus includes effects one month before to 4 months after an event.[13] We report E_4 separately for implementation of minimum wages and passage of legislation, as well as the sum of both.

An important choice in our estimation is the number of estimated lag and lead coefficients k. One constraint here is that minimum wage hikes generally occur in regular intervals, often within 12 months (see Table B.1). This implies that the event dummies become collinear if k gets larger.[14] A second constraint is that the store panel is not balanced. The more leads and lags we include, the more likely it is that changes in the underlying store sample may affect our estimates. In our baseline estimation, we settle on estimating the effect with $k = 9$. This is sufficient to show the short run impact of minimum wage increases on prices.[15]

The central concern with our estimation and identification strategy is the possibility of reverse causality. States with higher inflation rates could have more frequent and higher nominal minimum wage increases to avoid reductions in the real minimum wage. In this case inflation would cause minimum wage increases, rather than the other way around.[16] Although we view it as unlikely that legislators consider changes in state-level grocery price inflation within the few months relevant for our empirical analyses, we deal with this concern in our estimation in several ways. First, our main specification includes store fixed effects, which absorb differences in trend inflation between states. Second, due

[13] In principle, we could report E_k and include all lag coefficients. However, coefficients beyond 4 months out are typically close to zero and insignificant. In most specifications E_k is not significantly different from E_4 but substantially less precise.

[14] This implies that some observations lie, for instance, 8 months after the last and 4 months before the next minimum wage hike. In principle, we can disentangle the effects of the two events in such cases because many states do not have minimum wage increases before 2005 and after 2009, and because some states increase minimum wages only infrequently. However, our estimation strategy will not work in practice for large k, as the leads and lags become increasingly collinear.

[15] We present results for longer or shorter windows in robustness checks in Appendix Table B.6.

[16] A special case are minimum wage increases following from indexation. All states that practice indexation peg their minimum wage to national inflation rates. Changes in national inflation are absorbed by time fixed effects in our specification.

to the high frequency of our price data and the flexible estimation model, we can closely examine the timing of the effect, and any remaining differences in inflation trends around a minimum wage event would be easily detected in our pre-event coefficients. Third, we present estimates that only use variation due to changes in the federal minimum wage (see section 4.2). We view it as unlikely that federal lawmakers take into account regional inflation differences when setting the federal minimum wage.

4 The price response to minimum wage increases

4.1 Main results

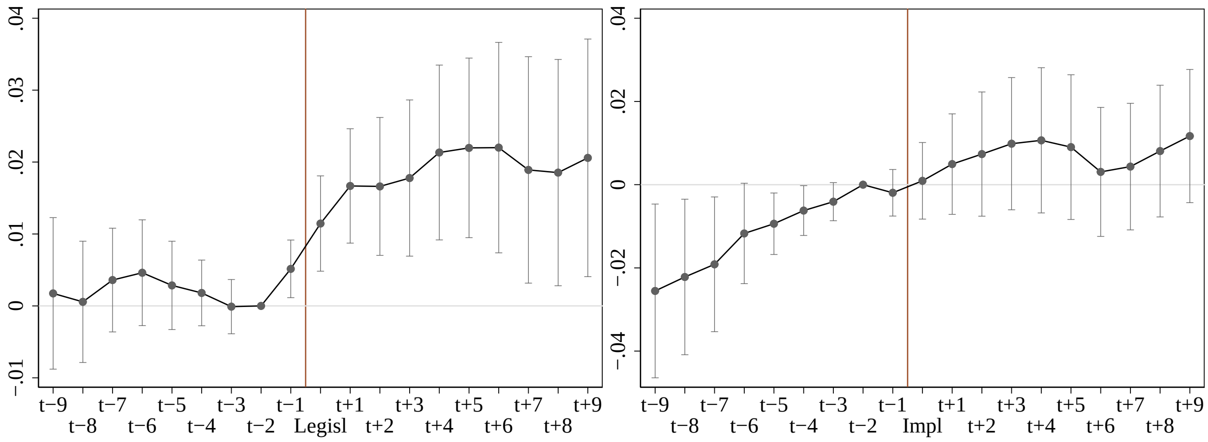
We start by using our main regression model (equation (3)) to estimate the effects of minimum wages on grocery prices at legislation and implementation separately. The dependent variable is the store-level month-on-month inflation rate. Figure 3a presents the estimated price effects at legislation. Reassuringly, the figure provides no evidence for significant movement in grocery prices in the months leading up to passage of minimum wage legislation. In the month that immediately precedes legislation, however, grocery stores start to increase prices. Prices continue to rise for 3 months. Overall, we estimate that the price elasticity of minimum wages at legislation of the hike is 0.021 and statistically highly significant.[17]

Figure 3b presents the results at the time of implementation of minimum wage increases. Our baseline estimate for the elasticity at implementation is comparable in size to the one for legislation. The figure points to a gradual increase in prices in the months leading up to implementation of a minimum wage increase. We show in section 5.2 that these significant pre-trends are driven by minimum wage events that are known long before implementation. They thus capture the effects at legislation for these events shown in Figure 3a. Hence, by the time the minimum wage has actually risen to the level set in

[17] We present our estimates of effects at legislation and implementation separately in Appendix Table B.4, as well as robustness checks that include division- and chain-time fixed effects.

the new legislation, the price adjustment appears to be more or less complete. We return to this evidence for forward-looking price setting of grocery stores in section 5.2.

Figure 3: Cumulative minimum wage elasticities of prices from separate estimation



(a) At legislation

(b) At implementation

Notes: The figures present the cumulative minimum wage elasticity of prices at grocery stores. Effects at legislation and implementation are estimated separately. The estimated coefficients are summed up to cumulative elasticities E_R as described in section 3. The figures also show 90% confidence intervals of these sums based on SE clustered at the state level.

Column 1 of Table 1 presents the results of our preferred specification that jointly estimates the effects at legislation and implementation.[18] This specification accounts for the fact that the price effects at legislation and preceding implementation may reflect the same variation in prices. Again, the sum of pre-event coefficients ($\sum$ Pre-event) is close to zero and not statistically significant, thus validating our empirical strategy. Our preferred estimate of the price elasticity of minimum wages sums up all coefficients in the five months that follow legislation and implementation ($E_4^{leg} + E_4^{imp}$). This elasticity amounts to 0.036 and is statistically significant at the 5% level. It suggests that the average minimum wage increase in our sample—which we estimate to be +20%[19]—raises prices in grocery stores by 0.72% over three months at the time when legislation is passed. In this example, inflation would more than double during these 3 months relative to the sample average rate of 0.13%.

[18] See the corresponding graphs in Appendix Figure B.2.

[19] See Appendix Table B.1.

4.2 Robustness tests

We first show that these baseline estimates survive an extensive set of robustness checks.

Alternative specifications of our main empirical strategy. We present alternative specifications of our main empirical strategy for the joint estimation in Table 1. Column 2 shows that the estimated effects are similar if we weight each store by the number of products used to construct the stores' price index. Column 3 shows that none of our results depend on the inclusion of controls beyond time fixed effects. The inclusion of controls tend to improve the precision of the estimates. In Column 4, we remove store fixed effects which account for differential trends in stores' price levels in our baseline specification. Controlling for such trends might attenuate estimates of the minimum wage effects if the minimum wage affects the growth rate rather than the level of prices (Meer and West, 2016). Reassuringly, the estimated elasticities in Column 4 are very similar to our baseline results.[20] Column 5 shows that our baseline estimate is robust to the inclusion of state-calendar month fixed effects, which control more restrictively for possible differences in the seasonality of prices increases across states. In Column 6, we winsorize the inflation rates below the 1st and above the 99th percentile of the distribution to show that our results are not driven by outliers. Finally, columns 7 and 8 add census division-time and chain-time fixed effects, respectively. These fixed effects capture changing trend inflation within regions and grocery chains. They also largely control for possible effects of minimum wages on wholesale prices, as these would likely affect stores that are geographically close or belong to the same chain similarly. In those two cases, the price elasticities become indeed smaller, possibly because the fixed effects absorb increases in wholesale costs following minimum wage hikes (see section 6.3 for a discussion).

[20]We find similar overall price elasticities to the (implemented and legislated) minimum wage if we use a version of equation 3 in long first-differences (see Table B.5). Moreover, specifications that are differenced over longer time periods yield larger price elasticities. The incremental increase comes to an end after 9 months. These findings suggest that minimum wages temporarily affect the growth rate of prices, which supports our focus on inflation rates in the months around changes in minimum wages.

Table 1: Main results and robustness checks

	(1) Baseline	(2) Weighted	(3) No Controls	(4) No Store FE	(5) Seasonal	(6) Winsorized	(7) Div.-time	(8) Chain-time	(9) No Sales Filter
E_0^{leg}	0.011***	0.007**	0.011***	0.011***	0.009**	0.009***	0.013***	0.007*	0.018***
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.002)	(0.002)	(0.004)	(0.006)
E_2^{leg}	0.015***	0.013**	0.015***	0.016***	0.015***	0.013***	0.019***	0.011**	0.026***
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.004)	(0.004)	(0.005)	(0.009)
E_4^{leg}	0.019***	0.017***	0.019***	0.021***	0.019***	0.017***	0.020***	0.013**	0.031***
	(0.006)	(0.006)	(0.007)	(0.007)	(0.006)	(0.005)	(0.005)	(0.006)	(0.009)
E_0^{imp}	0.002	0.008	0.002	0.001	0.002	0.003	-0.003	-0.007	-0.004
	(0.006)	(0.007)	(0.006)	(0.007)	(0.006)	(0.006)	(0.006)	(0.006)	(0.008)
E_2^{imp}	0.012	0.016	0.012	0.011	0.011	0.012	0.000	-0.001	0.013
	(0.011)	(0.011)	(0.011)	(0.012)	(0.011)	(0.011)	(0.007)	(0.007)	(0.009)
E_4^{imp}	0.016	0.023*	0.017	0.016	0.018	0.015	0.006	0.002	0.022*
	(0.013)	(0.013)	(0.013)	(0.014)	(0.013)	(0.012)	(0.009)	(0.009)	(0.011)
Estimation Summary									
$E_4^{leg}+E_4^{imp}$	0.036**	0.040***	0.036**	0.036**	0.037**	0.033**	0.026**	0.016	0.053***
	(0.014)	(0.015)	(0.014)	(0.016)	(0.014)	(0.013)	(0.011)	(0.011)	(0.015)
$\sum$ All	0.046*	0.057***	0.046*	0.046	0.045*	0.040*	0.033	0.020	0.041
	(0.024)	(0.020)	(0.024)	(0.028)	(0.024)	(0.021)	(0.024)	(0.018)	(0.026)
$\sum$ Pre-event	0.010	0.016	0.010	0.008	0.008	0.004	-0.006	0.004	-0.004
	(0.016)	(0.013)	(0.016)	(0.018)	(0.016)	(0.014)	(0.019)	(0.013)	(0.018)
N	191,568	191,568	191,641	191,568	191,568	191,568	191,568	181,816	191,568
Controls	YES	YES	NO	YES	NO	YES	YES	YES	YES
Time FE	YES	YES	YES	YES	YES	YES	YES	YES	YES
Store FE	YES	YES	YES	NO	YES	YES	YES	YES	YES
Weights	NO	Obs	NO	NO	NO	NO	NO	NO	NO
Seasonality	NO	NO	NO	NO	YES	NO	NO	NO	NO
Div. time FE	NO	NO	NO	NO	NO	NO	YES	NO	NO
Chain time FE	NO	NO	NO	NO	NO	NO	NO	YES	NO

Notes: The dependent variable is the store-level inflation rate. Baseline controls are the unemployment rate and house price growth. The table lists cumulative elasticities E_R, R months after legislation or implementation. Column 1 is the result of joint estimation of effects at implementation and legislation in our preferred specification. (2) weights observation by the number of products (UPC) used to construct the store-level price index. (3) does not contain any control variables. (4) does not control for store fixed effects. (5) accounts for state-specific calendar month fixed effects. (6) uses a winsorized outcome (98% winsorization). (7) includes division-time FE, (8) chain-time FE. ((9) does not correct for temporary price changes. $\sum$ All is the sum of all lead and lag coefficients. $\sum$ Pre-event is the sum of all coefficients up to $t-2$. SE are clustered at the state level. * p < 0.1, ** p < 0.05, *** p < 0.01.

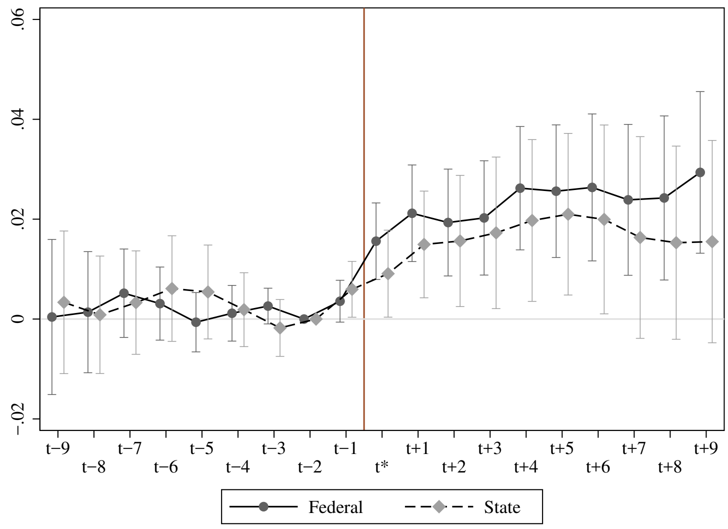
Including sales. Column 9 of Table 1 shows that the price elasticity is larger (0.053) if we use price indices that are not adjusted for temporary price changes. The reason, as we show in section 5.2, is that grocery stores reduce the frequency and the size of sales in the months around the legislation of minimum wage increases. By construction, however, sales represent a temporary deviation variation in prices. These results thus do not necessarily imply that our preferred estimate understates the permanent effect of minimum wages on prices. This would require that higher minimum wages decrease the frequency and size of sales permanently. To check the influence of sales on the price elasticity in the longer term, we present specifications in price levels (cf. column 4 and 6 of Table K.1) and long first-differences (cf. columns 4–7 of Table B.5) using price series that include sales. Both of these specifications are more robust to the large monthly price fluctuations caused by sales than our baseline model. The estimations suggest that our omission of sales does not lead to a downward bias in the estimated minimum wage elasticity of prices. Indeed, our preferred price elasticity of 0.036 is close to the elasticity in column 6 of Table K.1 estimated with prices that include sales.

Robustness to other specification choices. Appendix Table B.6 contains further robustness checks. Our results are robust to using only stores that we observe throughout the whole sample period (and hence are not driven by stores' entry or exit); to controlling for county level trends in the inflation rate; to changing the event window to $k = \pm 6$ or $k = \pm 12$ months; to excluding the Great Recession by focusing on the 2001–2007 period only; and if we only look at the effects of the first minimum wage hike in each state in our sample period, which represents an alternative method to address the fact that all states are treated multiple times in the sample period.

Our results are also robust to changing the level of analysis from the store to the state level (see Appendix C). Advantages of the state-level estimation are that the state panel is balanced and that the estimation can be extended to a longer panel without missing leads and lags due to store entry and exit. Reassuringly, the state-level estimates confirm our baseline estimates. Moreover, we find no evidence for differential trends in state-level prices between states with and without hike in the 15 months leading up to minimum wage legislation. These results speak against the concern that price inflation is the cause for minimum wage hikes rather than vice versa in the short estimation window relevant for our analyses.

Addressing reverse causality. Figure 4 presents a further robustness check that speaks against reverse causality. In particular, we estimate the separate effects for federal and state-level hikes by augmenting our baseline model with separate sets of leads and lags for events following from state and following from federal legislation. The response to new minimum wage legislation is similar in both magnitude and timing for federal and state-level minimum wage changes. While changes in state-level minimum wages could potentially be a response to local price increases, it is arguably very unlikely that price developments in particular states cause adjustments in the federal minimum wage.

Figure 4: Cumulative minimum wage elasticities of prices around federal- and state-level minimum wage legislation



Notes: The figure presents the cumulative minimum wage elasticity of prices at grocery stores around federal and state-level minimum wage legislation. The estimated coefficients are summed up to cumulative elasticities E_R as described in section 3. The figures also present 90% confidence intervals of these sums based on SE clustered at the state level.

Testing inference and specification. We also conduct a placebo test to test our inference and our regression framework. In particular, we repeatedly match all stores of a state with the minimum wage series of a random state. The match is drawn without replacement from a uniform distribution including the correct match. For each trial, we estimate the cumulative elasticity in the five months after legislation and implementation, $E_4^{leg} + E_4^{imp}$, using equation 3. We present the distribution of 1,000 estimated elasticities in Appendix Figure B.6. Our price elasticity estimate of 0.036 at legislation and implementation lies above *all* the placebo estimates. Furthermore, the placebo estimates are centered around zero. The permutation test suggests that our results are not driven by misspecification or structural breaks in the inflation series that correlate with temporal patterns of minimum wage increases. Moreover, the results suggest that our statistical inference is quite conservative.

Price effects by bindingness of the minimum wage. We show that, as expected, price effects are larger where the earnings effects of the minimum wage are largest. We present two pieces of evidence that this is the case.[21]

First, using our main empirical strategy, we run separate regressions for stores located in "right-to-work" states and all other stores. Right-to-work laws prohibit mandatory union membership for workers in unionized firms, and weaken the position of unions. As a result, wages in grocery stores tend to be lower in those states and substantially more responsive to minimum wage hikes (see Addison et al. (2009), and our own estimates of average earnings elasticities with respect to the minimum wage in Table 4). Consistent with expectations, the price effects of minimum wage hikes at legislation and implementation are substantially larger in right-to-work states (see Appendix Figure D.1 and Table D.1).

Second, we show that our baseline results are robust to an alternative identification strategy that exploits within-state variation in wages. The idea is that a statewide mini-

[21] The full details of our approaches are presented in Appendix D.

mum wage hike affects stores that pay low wages more than stores that pay higher wages. While we cannot observe stores' wages, we can exploit the large geographic variation in average wages of grocery stores across counties within a state. We find that stores in higher wage counties exhibit significantly *lower* inflation than stores in the same state in low wage counties, in the quarter legislation is passed (see Appendix Table D.2).

4.3 Distributional consequences

This section studies the distributional consequences of the price effects of minimum wages in grocery stores. We conduct three analyzes. First, we assess whether grocery prices increase more in cheap compared to expensive stores. Second, we study whether there are differences in the price development of products that differ by their consumers' income. Third, we present estimates of the annual dollar value of price hikes following an increase in the minimum wage by income brackets. [22]

Price effects by store expensiveness. Columns 1–4 of Table 2 present estimates of our joint regression model when splitting the sample into cheap and expensive stores in two ways. We reduce the length of the estimation window to 6 months before and after an event in order to reduce the number of coefficients estimated from these smaller samples. We use a procedure implemented by Coibion et al. (2015) to calculate expensiveness relative to other stores in a state (columns 1 and 2) and county (columns 3 and 4), respectively.[23] While the estimated effects tend to be larger for cheap stores, the difference in the response of the two groups of stores are not statistically significant. This is a first

[22]We have also estimated the price effects separately by product category (see Appendix Figure B.3). We find that the price responses are largest for products for household products (such as laundry detergent, paper towels and facial tissues), alcoholic beverages and certain types of food (such as mayonnaise, yogurt and tomato sauce) – potentially because the demand for those products is less elastic.

[23]We first calculate the mean price during a year for each product and store. For each product, we then calculate the mean price in a county. We then calculate the deviation of each store from this price and aggregate deviations over all products sold in each store, weighted by the dollar revenue of the product. We only use products that are sold in at least 3 stores in a county and drop counties with less than 3 stores. Finally, we label stores that are on average more expensive than other stores in a county as expensive, and the remaining stores as cheap. The results are very similar if we measure expensiveness relative to other stores in a state rather than a county.

piece of evidence that speaks against the fact that the price effects of minimum wage in the grocery sector fall disproportionately on poor households.

Elasticities of income-specific price indices. Do price elasticities differ for products consumed by low- vs. high- income households? Taking advantage of the IRI consumer panel dataset, we construct separate price indices for low-, medium- and high-income households, and run our baseline regression for each index separately.[24] We find that the elasticties for the products consumed by the three types of household are almost identical: 0.030, 0.028 and 0.027 for low-, medium- and high-income households respectively (see Table E.5). This suggests that stores increase product prices across the board. They are also very close in magnitude to our baseline estimate. This is a second piece of evidence that speaks against the fact that the price effects of minimum wage in the grocery sector fall disproportionately on poor households. They also speak against demand shifts as a cause of the price response, a point we discuss in more details in section 6.4.

Magnitude of price hikes across the income distribution. To put the magnitudes of the price hikes along the household income distribution in perspective, we use the IRI consumer panel dataset to estimate the Equivalent Variation of the grocery price caused by a 20% minimum wage hike—which corresponds to the average legislated increase in the minimum wage in our sample (see Table B.1), and to a \$1.24 minimum wage increase between 2001 and 2012. The Equivalent Variation is a first order approximation to the welfare cost of a price change, measured in US dollars. It assumes that households maximize utility and abstracts from second order effects reflecting the response to changes in relative prices.

A first-order approximation of the equivalent variation of a price change in the grocery sector j can be written as: $EV_j = E_{hj}\Delta P_j$, where E_{hj} denotes the mean household expenditure for goods sold in sector j for households in income bracket h.[25] We divide

[24]We present the full details of our analysis in Appendix E.

[25]An alternative interpretation of our EV measure is the cost for consumers to maintain consuming the same basket of goods after an x% price change. Our first-order approximation ignores some second-order

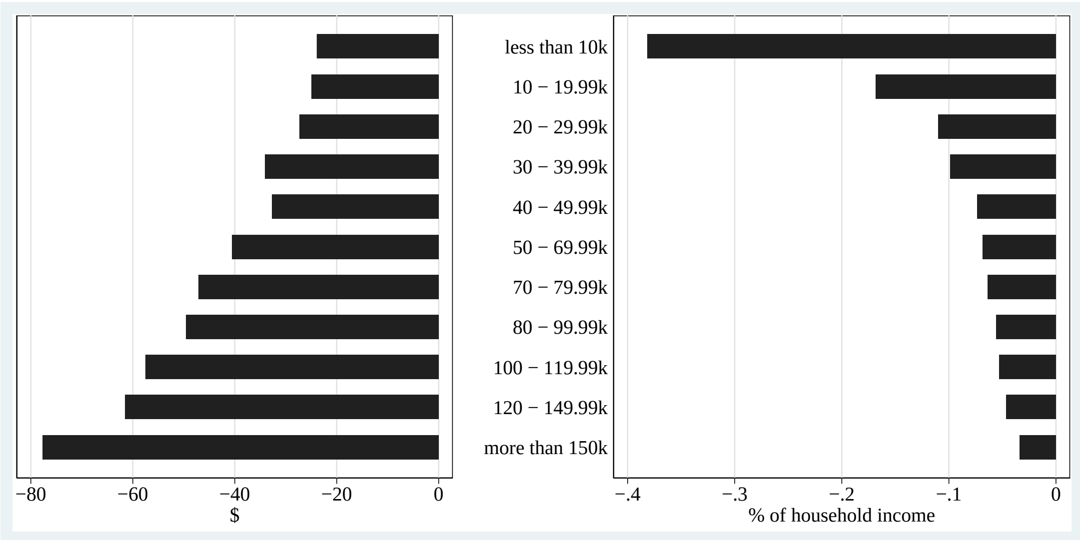
Table 2: Price effects of minimum wage by store characteristics

	(1) Expensive (state)	(2) Cheap (state)	(3) Expensive (county)	(4) Cheap (county)	(5) Regional chain	(6) Interregional chain
E_0^{leg}	0.007**	0.012***	0.005	0.013***	0.015***	0.007*
	(0.003)	(0.004)	(0.005)	(0.004)	(0.005)	(0.003)
E_2^{leg}	0.010**	0.016***	0.011	0.016***	0.018**	0.013**
	(0.005)	(0.006)	(0.007)	(0.006)	(0.008)	(0.005)
E_4^{leg}	0.013**	0.020***	0.014*	0.020***	0.022**	0.014**
	(0.006)	(0.007)	(0.007)	(0.007)	(0.009)	(0.007)
E_0^{imp}	-0.001	0.003	0.003	0.003	0.011	-0.007
	(0.008)	(0.006)	(0.009)	(0.006)	(0.007)	(0.006)
E_2^{imp}	0.002	0.015	0.006	0.016	0.020	0.002
	(0.016)	(0.010)	(0.017)	(0.010)	(0.013)	(0.010)
E_4^{imp}	0.008	0.019*	0.010	0.021*	0.030**	0.002
	(0.018)	(0.011)	(0.021)	(0.011)	(0.014)	(0.012)
Estimation Summary						
E_4^{leg}+E_4^{imp}	0.021	0.039***	0.024	0.042***	0.052***	0.017
	(0.019)	(0.014)	(0.022)	(0.014)	(0.017)	(0.014)
$\sum$ All	0.031	0.041*	0.037	0.049**	0.060**	0.013
	(0.024)	(0.022)	(0.026)	(0.023)	(0.029)	(0.021)
$\sum$ Pre-event	0.017	0.008	0.022	0.014	0.012	0.006
	(0.013)	(0.017)	(0.016)	(0.019)	(0.019)	(0.017)
N	47668	146374	30583	119234	111175	82867
Controls	YES	YES	YES	YES	YES	YES
Time FE	YES	YES	YES	YES	YES	YES
Store FE	YES	YES	YES	YES	YES	YES

Notes: The table presents cumulative minimum wage elasticities of prices at grocery and drug stores along several heterogeneity dimensions. The dependent variable is the store-level inflation rate. Baseline controls are the unemployment rate and house price growth. The effects shown in the columns are based on the joint estimation (equation 3), estimated separately for each sample indicated in the column header. Columns 1–4 differentiate stores by their price level relative to other nearby stores in the state (columns 1 and 2) and county (columns 3 and 4), as desribed in the text. Columns 5 and 6 split chains into "interregional" and "regional" chains, as described in the text. The table lists cumulative elasticities E_R, R months after legislation or implementation. $\sum$ All is the sum of all lead and lag coefficients. $\sum$ Pre-event is the sum of all coefficients up to $t-2$. SE are clustered at the state level. * p $<$ 0.1, ** p $<$ 0.05, *** p $<$ 0.01.

the household income distribution into 11 household income brackets (increments of $10K from 0 to $100K and coarser categories above). We estimate the mean household expenditure for each of these categories using the expenditure data by income bracket provided in the Consumer Expenditure Survey (CES).[26] ΔP_j denotes the price change in sector j. Since we do not find differences in the price response of products consumed by different household income groups, we use our baseline elasticity (estimated jointly at legislation and implementation) of 0.036 for all household categories.

Figure 5: Equivalent Variation of price increase



Notes: The figure illustrate the Equivalent Variation (EV) of increasing all binding minimum wages in the US by 20%. See section 4.3 and Appendix E for a detailed description of the calculations involved. Figure 5 shows the EV for each income bracket in US dollars (left) and relative to mean household incomes (right).

Figure 5 presents the costs of price increases caused by minimum wage hikes, measured in US dollars and relative to household incomes. The dollar value of costs is increasing in household incomes. Since groceries are not an inferior good, this is to be expected. For households with incomes below $10,000, the annual costs amounts to $24 (or, equivalently, to $19 a year for a $1 minimum wage increase). The costs increase up to $78 for households

terms that capture substitution to other products.

[26] We include expenditures for the CES categories Food at Home, Personal Care Products and Services, Household Supplies and Alcoholic Beverages as groceries.

with incomes above \$150,000 (or, equivalently, up to \$63 a year for a \$1 minimum wage increase). Expressing the costs as a percentage of annual household incomes reveals the regressive impact of the price response in grocery stores. The costs make up 0.4% of annual income for households in the poorest bracket, and less than one tenth of that, i.e. 0.03% for households in the richest bracket. The numbers underlying Figure 5 are available in Appendix Table E.3a. We present a full welfare analysis of grocery price increases (and, as an additional exercise grocery and restaurants' price increases taken together) in Appendix E.

5 The anatomy of the price response

This section discusses a number of facts about the effect of the minimum wage on grocery store prices that are of interest to the macroeconomic literature on price setting.

5.1 Uniform pricing of grocery chains

We look at the heterogeneity of the price increase across regional and interregional chains to investigate whether grocery stores apply uniform pricing in response to minimum wage hikes. Regional chains are chains with stores in less than 3 distinct states on average. Interregional chains are those with stores in 3 or more states. We find that prices increase by 5.2% in response to a minimum wage hike in regional chains (Table 2, col. 5). While we find a statistically significant price response in interregional chains around legislation, the estimated sum of the price effects is smaller in interregional chains compared to regional chains (col. 6). This latter finding is consistent with Dellavigna and Gentzkow (2019), who find that US retail chains maintain pricing across stores as uniform as possible, making prices less likely to respond to local economic shocks.

Another implication of uniform pricing within grocery chains is that a minimum wage hike in a specific state may affect prices in stores within the same chain located in another state. We augment our baseline regression model with variables that would capture such spillovers across states within chains (see Table B.7). We find little evidence for spillovers if we estimate the regression using our baseline sample that includes all stores. This is different, however, if we restrict the sample to stores that belong to interregional chains. In these chains, we observe a disproportionate price increase in the quarter of the announcement of the minimum wage increase even in the stores of the chain that did *not* experience the specific minimum wage hike. The estimated spillover effects amount to roughly half of the direct (i.e. within-state) minimum wage effect on prices. There is also some evidence that price spillovers occur at implementation. These results suggest that the price effects of the minimum wage may cause (small) welfare losses for individuals that live in states where the minimum wage does not increase.[27]

5.2 Firms' forward-looking pricing decisions

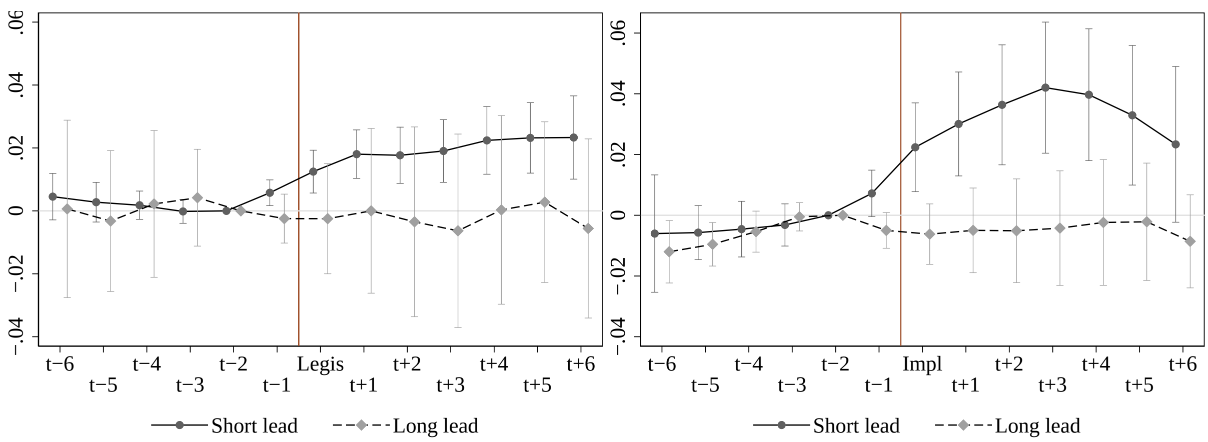
One striking result from our baseline regressions (see section 4.1) is that grocery stores appear to anticipate future cost increases by increasing their prices as soon as the minimum wage hike is announced (i.e. before the hike is implemented). In this section, we provide more details on this result and discuss how it relates to the macroeconomic literature on pricing behavior.

We first establish that retail stores seem to anticipate future cost increases by temporarily raising their markups between announcement and implementation. Using a similar methodology to study the dynamics of the wage response as for prices, we show that the earnings effect of the minimum wage hike is concentrated in the quarter when the hike is implemented. The price response at legislation thus reflects an anticipation of future wage increases, rather than premature compliance with future minimum wage laws (see Appendix Table F.1). Forward-lookingness in pricing decisions is consistent with price-

[27] One might be concerned that these results suggest an issue with our empirical strategy, namely that our control group of stores in states without minimum wage hikes may be partially treated. Reassuringly, however, the implied downward bias in our baseline specification is quantitatively small. We find no spillovers for our baseline sample that includes all stores (column 2 of Table B.7). Moreover, the bias is limited even among stores in interregional chains where the spillovers occur as can be seen by, e.g., comparing the estimated coefficient on $\Delta leg_{s(j),t+0}$ in columns 3 & 4 of Table B.7.

setting models with adjustment frictions, in which firms rationally consider the future as well as the present. These models include time-dependent models in which prices can only be changed in certain periods (see, e.g., Calvo, 1983) and state-dependent models (i.e. menu cost models), in which firms can adjust prices at a cost. Time-dependent models with a low probability of price change can feature a substantial degree of anticipation. In menu cost models, the speed of the price adjustment is more complex to predict[28] and the bulk of adjustment tends to happen close to implementation (see, e.g., Karadi and Reiff, 2019; Hobijn et al., 2006).

Figure 6: Firms' forward-looking pricing decisions: cumulative minimum wage elasticities



(a) Events at different timing, legislation (b) Events at different timing, implementation

Notes: Panel (a) shows the effects at legislation for legislation that is followed by implementation of a first increase in less than a year ("short lead") and legislation that is implemented further in the future ("long lead"). Panel (b) shows the effects at implementation for increases that are preceded by legislation within less than half a year ("short lead") and those whose legislation lies further in the past ("long lead"). The estimated coefficients are summed up to cumulative elasticities E_R as described in section 3. The figures also show 90% confidence intervals of these sums based on SE clustered at the state level.

To further illustrate these anticipation effects, we look at events with different lead times between legislation and implementation of higher minimum wages. In panel (a) of Figure 6, we split minimum wage laws into those that are followed by a first increase in the minimum wage within less than a year and those with longer time between legislation

[28]The speed of the price adjustment depends on many parameters: the minimum wage increase; the menu cost; and other product-level shocks (see, e.g., Karadi and Reiff, 2019)

and implementation.[29] The figure provides evidence that prices respond at legislation when implementation happens shortly after legislation, but not when implementation is at least a year out. In panel (b) of Figure 6, we split minimum wage laws into those that are followed by an increase in the minimum wage within less than 6 months and those with longer time between legislation and implementation. The figure shows that prices rise at implementation only when there is a short lag (less than 6 months) between legislation and implementation. In contrast, there are no price effects around implementation in the case of minimum wage hikes that are known long in advance. Rather, there is some evidence for an increase in prices in the months longer before the hike. If stores have enough time to anticipate the increase in cost, they appear to increase prices before their labor costs actually increase. Both sets of results are consistent with the predictions of price-setting models with frictions, that adjustment should be quicker for increases that become known shortly before they are implemented.

Finally, Appendix Figure B.5 provides clear evidence in favor of an anticipatory pricing behavior by showing that prices increase 6 (2) months before implementation for events that were legislated exactly 6 (2) months before they are implemented.[30]

Next, Table 3 investigates the channels through which US grocery stores make their forward-looking pricing decisions. The table provides five main insights. First, grocery stores increase the frequency with which they adjust regular (i.e., sales-filtered) prices as a response to minimum wage increases (column 1). The increase in the frequency of price changes happens primarily through an increase in the frequency of price increases (column 2). The point estimates imply that a 10% minimum wage hike raises the weekly frequency of regular price increases by 0.014–0.038 percentage points in the quarters around legislation and implementation, roughly 1.5–3.5% relative to the mean of the frequency.[31] Second, there is no increase in the absolute size of price changes overall

[29]There are 50 legislative events with a "short" and 12 with a "long" lead time between legislation and the first hike. Increases resulting from indexation are excluded from this analysis.

[30]The story is different for hikes legislated 4 months before implementation: there is only a small, if any, price effect at the time of legislation (i.e. at month $t = -4$), but prices increase quite strongly after implementation.

[31]We compute frequencies of price adjustments at the weekly level for each product. We then aggregate

(column 4). These first two results are consistent with menu cost models but not with time-dependent models (see, e.g., Nakamura and Steinsson, 2008; Nakamura et al., 2018). Third, Columns 5 and 6 show that firms increase the size of increases and reduce the size of decreases in regular prices around legislation. Fourth, grocery stores reduce the frequency and the size of sales around legislation (columns 7 and 8). Relative to the mean of the outcome, the effects on sales are smaller than the effects on the frequency of regular price changes. Finally, we find no statistically significant evidence that the pass-through of products whose prices are frequently adjusted occurs closer to the implementation date than for prices with long duration (see Appendix Figure B.4), as would be predicted by standard menu-cost models. Rather, the price effects at legislation appear to be driven both by goods with stickier and less sticky prices.

across products using expenditure weights. The quarterly data represent an average over the weekly frequencies. For instance, the mean of 0.0204 in column 1 means that 2.04% of the regular prices are changed in an average week of a quarter. This implies that regular prices remain unchanged for 49 weeks in the estimation sample. See Appendix A for more details.

Table 3: Effects of minimum wage increases on frequency and size of price changes

	(1) Freq. change	(2) Freq. increase	(3) Freq. decrease	(4) Size change	(5) Size increase	(6) Size decrease	(7) Freq. sales	(8) Size sales
$\Delta leg_{s(j),t-1}$	0.0004	0.0010	-0.0006	0.0001	0.0001	0.0001	-0.0167	-0.0259***
	(0.0013)	(0.0008)	(0.0009)	(0.0006)	(0.0006)	(0.0008)	(0.0119)	(0.0076)
$\Delta leg_{s(j),t+0}$	0.0004	0.0014*	-0.0011	-0.0001	0.0007**	-0.0009**	-0.0187*	-0.0141
	(0.0011)	(0.0008)	(0.0007)	(0.0003)	(0.0003)	(0.0004)	(0.0102)	(0.0099)
$\Delta leg_{s(j),t+1}$	0.0008	0.0013	-0.0005	-0.0004	0.0000	-0.0008**	-0.0210**	-0.0200**
	(0.0014)	(0.0010)	(0.0007)	(0.0004)	(0.0005)	(0.0004)	(0.0102)	(0.0096)
$\Delta imp_{s(j),t-1}$	0.0033***	0.0030**	0.0002	0.0002	0.0006	-0.0003	0.0050	-0.0000
	(0.0011)	(0.0012)	(0.0009)	(0.0006)	(0.0007)	(0.0009)	(0.0186)	(0.0126)
$\Delta imp_{s(j),t+0}$	0.0035*	0.0038**	-0.0003	0.0000	0.0009	-0.0008	0.0092	0.0099
	(0.0020)	(0.0019)	(0.0013)	(0.0007)	(0.0006)	(0.0012)	(0.0201)	(0.0171)
$\Delta imp_{s(j),t+1}$	0.0005	0.0016	-0.0012	-0.0006	-0.0006	-0.0006	0.0091	-0.0143
	(0.0018)	(0.0013)	(0.0012)	(0.0008)	(0.0008)	(0.0009)	(0.0187)	(0.0136)
Observations	75278	75278	75278	75278	75278	75278	75278	75256
Controls	YES	YES	YES	YES	YES	YES	YES	YES
Time FE	YES	YES	YES	YES	YES	YES	YES	YES
Store FE	YES	YES	YES	YES	YES	YES	YES	YES
Mean Dependent Variable	0.0204	0.0116	0.0087	0.0064	0.0067	0.0060	0.3440	0.1534

Notes: The table presents estimates of minimum wage effects on the frequency and size of price changes and sales. The estimates are derived from quarterly-level estimations of our joint regression model (equation 3) at the store level. $\Delta imp_{s(j),t}$ and $\Delta leg_{s(j),t}$ denote the percent change in the logarithm of implemented and legislated minimum wages, respectively, in quarter t and state $s(j)$ in which store j is located. The dependent variable in column 1 is the frequency of price increases in regular (i.e. sales-adjusted) prices, computed as the count of price changes between weeks of months at the product level and aggregated to the store level weighting each product equally. Similarly, the dependent variables in columns 2–6 are the frequency of price decreases (column 2), the size of price *increases* in sales-filtered prices (column 3), the size of price *decreases* in sales-filtered prices, the frequency of sales according to the sales filter by Kehoe and Midrigan (2015) (column 5) and the size of sales according to the sales filter. Baseline controls are the unemployment rate and house price growth. SE are clustered at the state level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

6 The magnitude of the price pass-through

6.1 Benchmark model of the minimum wage elasticity of groceries' labor costs

In this section, we estimate the impact of minimum wage increases on grocery stores' cost with the aim of quantifying the degree of cost pass-through. We first clarify the assumptions required to estimate the impact of minimum wages on marginal cost.

We describe a general theoretical framework from which we derive our estimation procedure in Appendix G. We assume that grocery stores provide retail services using a production technology $F(L, X)$. F is homogeneous to some degree—including the possibility of non-constant returns to scale. X denotes the quantity of purchased merchandise. L is a composite input defined by a linear homogeneous aggregator over N different types of labor inputs $L_1, L_2, \ldots, L_N$ with wages $w_1, w_2, \ldots w_N$. The wages of these different types of workers may be affected by minimum wages differently[32]. An important implication of these assumptions is that the composition of worker types does not vary with the scale of the firm. Finally, we assume competitive labor markets[33].

Under these assumptions, the minimum wage elasticity of marginal cost at constant output equals:

$$\frac{\partial MC}{\partial MW}\frac{MW}{MC} = \frac{\overline{W}L}{C} \cdot \frac{\partial \overline{W}}{\partial MW}\frac{MW}{\overline{W}} \tag{4}$$

C denotes the total variable cost of a grocery store, and $\overline{W}$ denotes the average wage the

[32] We thus allow for the fact that workers earning wages above the minimum wage may also benefit from minimum wage hikes to some degree (Dube et al., 2015; Autor et al., 2016). For instance, table H.1 in the appendix illustrates that our results on the minimum wage share of groceries' costs would depend substantially on the wage cutoff we use to define minimum wage workers.

[33] We make this assumption because our evidence for positive price effects and no employment effects of minimum wages is generally inconsistent with monopsonistic labor markets (Aaronson et al., 2008). Monopsonistic labor markets have been brought forward as an explanation why minimum wages have limited effects on employment (Card and Krueger, 1995; Stigler, 1946). Our assumptions and our results are compatible with small or no disemployment effects if low-skilled labor is difficult to substitute with other factors—at least in the short run—and full price pass-through has small or no effects on sectoral output.

store pays. We can estimate this elasticity as the product of two factors: (i) the labor share in costs and (ii) the minimum wage elasticity of the average wage $\overline{W}$.[34] We provide an empirical calibration for those two factors in the following section. Note that these assumptions are only necessary to interpret our estimates as marginal cost pass-through. Alternatively our estimates can be interpreted—without any required assumptions—as average cost pass-through.

6.2 Empirical calibration

Labor share in costs. We estimate that the labor cost share of grocery stores is 16%, using the 2007 and 2012 BLS Annual Retail Trade Surveys. This estimate corresponds to the labor cost share in variable cost —which is the one that matters for price setting in the short run (see Appendix Table B.3). Labor costs include salaries, fringe benefits and commission expenses. Variable costs include labor costs, costs of goods sold and some smaller items such as transport and packaging costs.[35] We also note that the most important factor in grocery store costs is the cost of goods sold (83%).

Minimum wage elasticity of the average wage. We estimate the minimum wage elasticity of average earnings of grocery store workers using quarterly county-level data from the Quarterly Census of Employment and Wages (QCEW). QCEW employment and wage measures are the ones reported by employers in their Unemployment Insurance contributions. The QCEW files cover more than 95% of US jobs. We calculate average earnings as the ratio of total earnings of grocery store workers and grocery store employment. We assume that the elasticity of average earnings is equal to the elasticity of the

[34]The fact that the price response is related to the labor share is well-known in the literature (see, e.g., Hamermesh, 1993; Aaronson and French, 2007; Cahuc et al., 2014; Leung, 2020).

[35]Variable costs differ from total costs. In addition to variable costs, total costs include building and equipment costs (such as rents, utilities, depreciation and purchases of equipment), purchased services (such as maintenance, advertisement, etc.) and other operating expenses (such as taxes). Note that our estimate of labor cost share in variable cost does not include purchased services in the denominator. These services make up about 2% of total costs and include some tasks that are likely done by low-skilled workers, for example maintenance work. These costs may depend on minimum wages as well, but it is hard to determine to which extent.

average wage.[36] We restrict the data to the states and time period included in our price regressions. We then estimate standard state-level two-way fixed effects regressions that are often used to estimate minimum wage effects on employment in the US (see Allegretto et al., 2017, for a critical assessment):

$$\log \overline{W}_{c,q} = \gamma_c + \delta_q + \beta \log MW_{c(s),q} + Controls_{c,q} + \epsilon_{c,q} \tag{5}$$

Table 4 shows that we find significant positive effects of minimum wages on average earnings. This is also true if we control for state-specific linear time trends—an important sensitivity check for the two-way fixed effects model in the minimum wage context (Allegretto et al., 2017). Moreover, as we show in appendix H, the elasticity of earnings in grocery stores increases with the bindingness of a minimum wage hike.

Our baseline estimate for the labor cost elasticity in grocery stores is 11%. This is in line with what other papers have found, only slightly smaller than our estimate for the accommodation and food service industry, and larger than for retail trade as a whole (see columns 3–6 of Table 4).[37]

6.3 Implied cost pass-through rates

The combined estimates of the labor cost share and the minimum wage elasticities of the average wage allow us to compute pass-through rates using equation 4. Our baseline point estimate for the elasticity of cost is $0.16 \cdot 0.11 = 0.018$. We compute pass-through rates by dividing the price elasticity by the estimated cost elasticity. The results are shown in Table 5.[38] Our estimate for pass-through based on our baseline specification

[36]The two will be equal if there are no negative effects on employment and hours of low-wage workers. In the case of negative employment effects, the earnings elasticity will underestimate the wage elasticity. However, we do not find evidence for negative employment effects (see Panel B of Table 4, consistent with Addison et al. (2009)'s estimates who also use county-level QCEW data for the period 1990–2005). We also do not find evidence of a negative effect on the number of establishments (see Panel C of Table 4).

[37]Our baseline labor cost elasticities are somewhat smaller than the elasticities for the US retail sector estimated in Sabia (2009) using CPS wage data. They are larger than those estimated in Addison et al. (2009) for the 1990–2005 period. Our estimates are similar to those reported in Leung (2020) and Ganapati and Weaver (2017), who also use QCEW data for a similar time period.

[38]Full details for the calculations made in this section are available in Appendix I.

Table 4: Earnings and employment elasticities to the minimum wage in grocery stores, retail, and restaurants

	Grocery stores		Retail trade		Acc. and food services	
	(1) Baseline	(2) Trend	(3) Baseline	(4) Trend	(5) Baseline	(6) Trend
Panel A: Dep. variable: Labor cost per worker						
log MW	0.108**	0.083***	0.048*	0.038	0.151***	0.147***
	(0.043)	(0.027)	(0.026)	(0.024)	(0.024)	(0.025)
N	80,722	80,759	124,000	124,000	98,056	98,080
Only Right-To-Work states						
log MW	0.165***	0.159***	0.064	0.096	0.246***	0.238***
	(0.056)	(0.050)	(0.070)	(0.063)	(0.062)	(0.070)
N	40,385	40,385	71,583	71,583	56,322	56,322
Panel B: Dep. variable: Employment						
log MW	-0.010	0.089**	-0.002	-0.003	-0.042	-0.046*
	(0.048)	(0.036)	(0.027)	(0.017)	(0.033)	(0.027)
N	80,722	80,759	124,000	124,000	98,056	98,080
Panel C: Dep. variable: Number of establishments						
log MW	-4.30	-1.66	46.57	6.06	-25.51	4.29
	(3.98)	(3.96)	(36.85)	(14.22)	(24.58)	(14.37)
N	114,000	114,000	125,000	125,000	118,000	118,000
Controls	YES	YES	YES	YES	YES	YES
County FE	YES	YES	YES	YES	YES	YES
Time FE	YES	YES	YES	YES	YES	YES
State linear trends	NO	YES	NO	YES	NO	YES

Notes: The table shows elasticities to state-level minimum wages in the 2001–2012 period by industry, estimated using county-level panel data for 41 states used in our price regressions. The data are based on the QCEW. Retail trade corresponds to NAICS codes 44–45, grocery stores to NAICS code 4451, and accommodation and food services to NAICS code 72. The outcome in panel A is log average earnings by industry. The outcome in Panel B is the log employment in an industry, computed as the average employment in the three months in the respective quarter. The controls are the log of county population and the log of total employment in private industries per county. Standard errors are clustered at the state level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

(joint estimation at legislation and implementation, panel A) amounts to 2.026. This number may seem large, but given the large standard errors on the insignificant elasticity estimate at implementation, we cannot reject the hypothesis that pass-through is equal to 1—the p-value on this test is 0.485. The pass-through ratios based on estimates including division-time (1.492, p-value: 0.553) and chain-time fixed effects (0.836, p-value: 0.748) are closer to and also not significantly different from one.

Table 5: Implied cost pass-through for various specifications

	(1) Baseline	(2) Division-time FE	(3) Chain-time FE
	A. Pass-through at legislation and implementation		
Implied cost pass-through	2.026	1.492	0.836
p-value PT=1	0.485	0.553	0.748
	B. Pass-through at legislation and implementation (incl. predicted effects on COGS)		
Implied cost pass-through	0.968	0.784	0.516
p-value PT=1	0.867	0.127	0.003

Notes: The table illustrates the implied cost pass-through. Pass-through at legislation and implementation is the minimum wage elasticity of prices 5 months after legislation E_4^{leg} plus the same elasticity at implementation E_4^{imp} relative to the estimated elasticity of marginal cost. p-values for a test of full pass-through are computed using standard errors for the pass-through ratio calculated using the Delta method.

However, the pass-through rates in panel A do not take into account that minimum wages increases may also increase the cost of goods sold (COGS) in grocery stores. Wholesale prices, in turn, may increase if minimum wage workers are employed in the production of grocery products. Due to the high share of COGS in grocery stores' cost—as shown in Appendix Table B.3, COGS make up about 83% of cost—even a minor increase in wholesale prices could matter for retail prices. Moreover, retail stores have been shown to be very responsive to changes in COGS (Eichenbaum et al., 2011; Nakamura and Zerom, 2010).

We cannot test directly whether minimum wages affect COGS as our data does not include wholesale cost. However, we can calculate an upper bound for this effect using input-output tables by assuming full pass-through of increases in labor costs into prices all along the production chain for each of the sectors producing groceries, similar as in MaCurdy (2015). Assuming that all workers that earn 110% (130%) of the minimum wage are affected by the minimum wage, we predict that a 10% increase in the minimum wage would increase the prices of COGS by 0.016% (0.024%) (see Appendix Table I.1). Hence, under the assumption of full-pass-through, price increases for COGS may indeed affect the marginal costs of grocery stores in a comparable magnitude as the direct effect through higher labor costs in grocery stores themselves.

Importantly, due to our DiD design, the estimated minimum wage elasticities of grocery prices only reflect the effects of higher prices for COGS to the extent that these occur locally. If wholesale groceries are highly tradable, price increases in COGS would affect all stores and pass-through would be absorbed in time fixed effects. We study the origin of groceries sold in different states. Using grocery wholesale-to-retail flows from the Commodity Flow Survey, we find that the majority of groceries sold in a state are delivered by wholesalers located in the same state or census division. As a consequence, it is likely that our estimates partly capture pass-through of increases in COGS.

Panel B of Table 5 thus shows pass-through rates that take into account effects of minimum wage increases on COGS. To calculate these pass-through rates, we assume that the major part of the price effect occurs in the state in which the minimum wage occurs and that the price pass-through along the production chain is the same as in the retail sector. The estimate for pass-through based on our baseline specification falls to 0.97 when we incorporate possible wholesale cost increases. The implied cost pass-through rate is significantly lower than 1 if we incorporate effects on COGS and control for chain-time fixed effects. Note, however, that the division-time and chain-time effects in columns 2 and 3 likely absorb at least part of the price effects of COGS already, so that incorporating this additional cost effect leads to a lower bound on the pass-through rate. For this reason, the estimated pass-through rates in columns 2 and 3 of panel B are possibly biased downward.

6.4 Demand increases: a discussion

So far, we have treated minimum wage increases as a cost shock to grocery stores. However, minimum wages also raise the incomes of low-wage workers, which may affect the demand for groceries. This demand may in turn also elicit a response of grocery prices. This view has been advocated in Leung (2020) and Alonso (2016), who find a positive impact of minimum wages on real grocery store revenues. In contrast, Aaronson et al. (2012) find no evidence for an impact of minimum wages on consumption of non-durables and services in households with minimum wage earners.

Our results also suggest that minimum wages have limited effects on grocery consumption. Appendix Table J.1 shows that we find no effect of minimum wages on quantities sold at or on revenues of grocery stores, neither at legislation nor at implementation. Note, however, that the estimates are not very precise, precluding us from strong conclusions on the magnitude of the effects.

Even if minimum wages affected the grocery demand for households with low-wage workers, there are a priori good reasons to be skeptical that minimum wage hikes lead to a substantial shift in market demand that would have a quantitatively important effect on prices. To see this, note that the role of demand in the price response to minimum wage increases is determined by three factors: First, minimum wages need to have a substantial effect on local *aggregate* incomes. Second, the market demand for groceries has to be responsive to changes in aggregate incomes. Third, grocery stores' prices have to be responsive to changes in local demand.

We expect small effects of minimum wages along on at least the first two dimensions. First, Dube (2019) shows that minimum wages increase incomes of low-income families with an elasticity of up to 0.5 after two years. He finds effects on incomes up to the 15th percentile of family incomes. Yet, in 2011, these families account for less than 2% of total incomes. The elasticity of aggregate incomes to the minimum wage would thus be at the order of $0.5 \cdot 0.02 = 0.01$. Second, the magnitude of the shift in individual demand associated with increasing income depends on the income elasticity of grocery demand. Products sold in grocery stores are typically necessities with income elasticities below one (see, e.g., Banks et al., 1997; Lewbel and Pendakur, 2009; Okrent and Alston, 2012). Any shift in individual demand is thus likely to be smaller than the underlying increase in income. Third, several estimates of grocery stores' supply curve suggest that prices are rather unresponsive to *temporary* changes in demand in the short run, even in the face of very large demand shifts (Chevalier et al., 2003; Gagnon and Lopez-Salido, 2014; Cavallo et al., 2014). More relevant to the study here, Stroebel and Vavra (2019) estimate that the elasticity of retail prices to a *permanent* house-price-induced demand shocks is on the order of 0.1–0.2. This is a relatively sizable effect on prices.[39] However, even taking the upper bound of that third elasticity, along with the upper bounds of the first two elasticities, lead us to estimate that the demand-side price elasticity of minimum wage increases in our study is at most $0.01 * 1 * 0.2 = 0.002$. This is 18 times smaller than our baseline price elasticity of 0.036. We conclude that it is unlikely that our baseline price elasticity is driven by demand side effects.

Finally, we note that the timing of our price response is not consistent with a demand-side effect on prices. Quantity responses would likely occur at the time the minimum wages is implemented and wages of workers actually increase (see Appendix F). The price increases, however, largely happens around legislation, and thus on average several months before household incomes increase.

7 Conclusion

In this paper, we study the effects of minimum wage increases on prices in grocery stores. We use scanner data to analyze the response to 166 minimum wage increases and 62 legislative events in the US from 2001 to 2012.

Our findings can be summarized by three key results. First, the minimum wage elasticity of prices is about 0.036. This means that the average minimum wage increase

[39]Stroebel and Vavra (2019) also document that the price response is mainly driven by markups rather than marginal costs, a result that does not align with ours.

from 2001 to 2012 (+20%) raises prices by 0.72%, i.e. that inflation more than doubles around the minimum wage hike. Our results are consistent with a full pass-through of cost increases to consumers. Second, we find that the response to minimum wage increases happens around the time of passage of legislation, rather than at the time of implementation of hikes. This result suggests that grocery stores set their prices in a forward-looking manner. The price increase occurs mostly through an increase in the frequency of price increases, consistent with menu cost models. Third, we show that prices rise as much for low-, medium- and high-income households. Since groceries make a larger share of low-income households' budgets, low-income households are hit the most by price increases. For households with income below $10,000, the annual costs associated with a $1 minimum wage increase is $19. Overall, consumers rather than firms seem to bear the cost of minimum wage increases in the retail sector.

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