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THE IMPACT OF GOVERNMENT POLICIES ON RETAIL PRICES AND WELFARE

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To my parents and my sister

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ABSTRACT

This dissertation is composed of two chapters studying the impact of government policies on retail prices and welfare. The first chapter estimates the impact of the minimum wage on retail prices using store-level scanner data. I provide empirical evidence that a 10% increase in the minimum wage raises grocery store prices by 0.6%-0.8%, and suggest that the minimum wage not only raises labor costs but also affects product demand, especially in poorer regions. This points to novel channels of heterogeneity in pass-through that have distributional consequences, with key implications for real wage inequality. The second chapter estimates the impact of the Supplemental Nutrition Assistance Program (SNAP, formerly the Food Stamp Program) on retail prices, sales, and household consumption. We develop a theoretical partial equilibrium framework to calculate the local incidence of SNAP benefits for SNAP-eligible products, using our reduced-form estimates as sufficient statistics. We find that producers mostly benefit at the expense of non-SNAP households due to market power. A marginal dollar of SNAP benefits increases producer surplus by about \$0.5, increases SNAP consumer surplus by about \$0.7, and decreases non-SNAP consumer surplus by about \$0.4. If the objective of SNAP is to guarantee a floor of real spending power on food, federal maximum benefits should be increased by about 10% to account for the price response.

CHAPTER 1

MINIMUM WAGE AND REAL WAGE INEQUALITY: EVIDENCE FROM PASS-THROUGH TO RETAIL PRICES

1.1 Abstract

This paper jointly considers the impact of the minimum wage on both labor and product markets using detailed store-level scanner data. I provide empirical evidence that a 10% increase in the minimum wage raises grocery store prices by 0.6%-0.8%, and suggest that the minimum wage not only raises labor costs but also affects product demand, especially in poorer regions. This points to novel channels of heterogeneity in pass-through that have distributional consequences, with key implications for real wage inequality, residential segregation, and future minimum wage increases. I also find that price rigidity within retail chains ameliorates these effects, reducing the pass-through elasticity for retail prices by about 60%.

1.2 Introduction

Minimum wage laws are one of the most frequently used policies to combat poverty around the world. They are a nearly universal policy instrument and applied in around 90% of all countries (ILO, 2006). However, the efficacy of minimum wage laws as an anti-poverty tool has been debated for many decades, beginning with Stigler (1946). While the minimum wage raises wages for low wage workers, less clear are who exactly pays for these increases and how much. There are three main ways through which these higher labor costs are transmitted throughout the economy. First, firms can reduce employment or adjust non-monetary returns to workers (e.g. less fringe benefits such as fewer paid lunch hours and holidays). In this case, low wage workers pay. Second, firms may reduce profits, which means that owners pay. Third, firms may raise prices, hence consumers also pay. The first mechanism has received much of the attention in the empirical literature, but the magnitude of the disemployment effect is still hotly debated.[1] Significantly less studied are the latter two mechanisms of prices and in particular, profits.[2]

This paper studies the impact of the minimum wage on retail prices for a wide range of products. To estimate the minimum wage pass-through elasticity, store and product group specific price indices are constructed using retail scanner data. I apply a standard difference-in-differences approach to exploit a large number of federal, state, and city minimum wage law changes in the US from 2006-2015 as sources of variation, covering both changes during the Great Recession and the subsequent recovery. I show that this standard identification strategy generates estimates that can be interpreted as plausibly causal because stores in different states exhibit no differential pre-trends. In the labor market, I find that a 10% minimum wage hike raises earnings of grocery store workers up to 1.5%. In the product market, I find that the impact of the minimum wage on retail prices is economically large and statistically significant in grocery stores. A 10% hike in the minimum wage raises grocery store prices by around 0.58%. These estimates are economically significant since both the national CPI and grocery store inflation rates are around 2% annually over the sample period.

Pass-through estimates for other store types such as drug and merchandise stores are statistically insignificant. I provide empirical evidence that most grocery chains either adopt regional pricing or operate only in a few states, while most drug and merchandise chains adopt rigid pricing within retail chains across the nation. Among these store types, grocery stores account for about 60% of consumer expenditure while drug and merchandise stores account for 40%. I estimate that within-chain price rigidity attenuates the impact of an increase in *local* minimum wage on retail prices by 58%. These findings suggest that extrapolations from my data about the impact of a rise in the *federal* minimum wage on retail prices can only be

1. One side has shown no significant disemployment effect (e.g. Card and Krueger 1995; Allegretto et al. 2013) while the other side has shown significantly negative employment effects (e.g. Neumark and Wascher 2006).

2. Draca, Machin and Van Reenen (2011) find that the minimum wage reduces firm profitability in the UK.

made using grocery stores, but we cannot rule out the possibility that drug and merchandise stores would also change prices nationally across the entire retail chain in response to *federal* minimum wage hikes.

Furthermore, there is substantial heterogeneity in pass-through elasticities between stores in rich and poor counties. When focusing only on grocery stores in poor counties where the minimum wage is more binding, the estimated pass-through elasticity is larger than predicted by theory if minimum wage hikes are purely labor cost shocks. Interacting the pass-through elasticity with measures of how binding the minimum wage is within counties gives strongly significant coefficients. I propose that demand-induced feedback is one mechanism that can explain the large magnitude in pass-through elasticity as well as the dispersion between rich and poor counties. If the minimum wage generates spillover effects, a large number of workers would experience an increase in income and possibly household credit, lowering demand elasticities and raising prices as stores increase markups. This effect would be particularly strong in regions where the minimum wage is more binding. I derive pass-through formulas to predict the magnitude of the pass-through elasticity if the minimum wage only increased demand and show that theoretical calibrations are consistent with the reduced-form estimates. I also provide suggestive evidence that poorer households reduce their shopping intensities when the minimum wage rises.

The estimated pass-through elasticity passes a series of robustness checks and exhibits significant heterogeneity in ways consistent with theory. For example, the pass-through elasticities are higher for product groups with lower demand elasticities, consistent with changes in optimal markups. In addition, minimum wage pass-through elasticities vary geographically. For example, they are larger in counties with a higher proportion of low wage workers and stores with lower revenue. These results point to novel channels of spatial heterogeneity in pass-through elasticity that have distributional consequences.

My paper contributes to several strands of literature in labor economics on the minimum wage. First, the literature on the price effects of the minimum wage is surveyed in Lemos

(2008), who concludes that most studies have found that a 10% US minimum wage increase raises food prices by no more than 4% and overall prices by no more than 0.4%. The literature has focused mostly on prices of food away from home since minimum wage workers are predominantly hired by restaurants, accounting for close to a third of all minimum wage workers.[3] Table 1.1 shows the share of minimum wage workers in the industries that hire them. I study specifically retail stores for two reasons. First, scanner data for goods sold in retail stores have unparalleled richness, providing information on quantities and prices on a weekly basis for over two million goods sold in over 35,000 stores across the entire US, covering a 10-year panel from 2006-2015 with over 220 minimum wage changes. This overcomes challenges in the previous literature, where prices of goods are sampled from stores and often subject to sampling error. Second, retail stores hire many minimum wage workers, accounting for over 7% of all minimum wage workers, and are a crucial part of the consumption basket, covering around 18% of all expenditure in the CPI as shown in Figure 1.1.[4] Poorer consumer units spend a larger share of their income on retail goods, which further magnifies the distributional effect of increasing retail prices. To my knowledge, there are two contemporaneous papers that also estimate the minimum wage pass-through elasticity using retail scanner data. Renkin, Montialoux and Siegenthaler (2017) find smaller results with an elasticity of around 0.02 with different data, while I find an elasticity of about 0.058 for grocery stores. Ganapati and Weaver (2017) uses the same data but also different methodology and obtain different results. I describe both of their approaches and reconcile the differences in Appendix 1.12.1.

Second, there is a scant but growing literature demonstrating that the minimum wage generates spending responses of considerable magnitude, partly by expanding access to credit

3. Aaronson (2001) uses ACCRA price indices and publicly available BLS data while Aaronson, French and MacDonald (2008) use restricted CPI data on food away from home but only for a short panel from 1995-1997 and 7,500 food items across 1,000 establishments.

4. According to the Consumption Expenditure Survey (CEX) conducted by the BLS (BLS 2015), expenditures on food at home exceeds those on food away from home, taking up around 60% of food expenditures and 10% of the overall consumer basket.

(Kennan 1995; Aaronson, Agarwal and French 2012; Dettling and Hsu 2017). Alonso (2016) estimates a positively significant spending response for non-durables with the same scanner data used in this paper, although he does not focus on the impact on prices. Using a longer sample period that contains rich minimum wage variation and slightly different measures of real spending, I find that the results suggesting a spending response are not very robust, and argue that both labor cost shocks and higher markups could lower real spending. In addition, whether the effect of increased income among minimum wage workers who remained employed may be offset by income losses of workers who become unemployed is an empirical question. To my knowledge, this is the first paper to demonstrate that minimum wage hikes can increase prices through *both* supply and demand.

Third, several recent studies have tried to jointly consider all these mechanisms to understand who pays for the minimum wage (Aaronson and French 2007; Harasztosi and Lindner 2015). My paper also investigates jointly both labor markets and product markets. These two markets do not act in isolation, and their interaction provides useful information about the impact of minimum wages. Specifically, I focus on the impact of the minimum wage on labor costs and the resulting cost pass-through in product markets. Building on cost pass-through formulas derived by Weyl and Fabinger (2013), I am able to pin down the pass-through elasticity under different assumptions about the amount of minimum wage spillovers.[5] I then use the reduced form estimates of pass-through elasticities to recover what the range of spillovers would be to justify these estimates, and find that even the largest spillover estimates in the literature cannot explain the magnitude of the estimated pass-through elasticity. This relates to a vast literature on the drivers of wage inequality in the past four decades, which has often found that declining real minimum wages had major impacts on rising wage inequality (Autor, Manning and Smith 2016, hereafter AMS; Dube 2017).[6] I highlight a mechanism that implies the reduction in real wage inequality caused

5. Minimum wage spillovers are defined as the effect of the minimum wage on the wages of workers earning above the percentiles at which the minimum wage binds.

6. One side of the debate has attributed rising inequality to skill-biased technical change (e.g. Juhn,

by a minimum wage hike could be smaller than the reduction in nominal wage inequality.

Another paper that considers the effects of the minimum wage on real wage inequality is MaCurdy (2015), who uses an input-output model to simulate the distributional impacts of the rise in the federal minimum wage on prices under many strong assumptions, concluding that the minimum wage is more regressive than a typical sales tax. The mechanism he highlights is that low income workers tend to consume a higher share of goods produced by minimum wage labor. In this paper, I directly estimate the distributional effect of the minimum wage on prices empirically by using regional heterogeneity in wages and earnings across counties, which allows me to relax some of the assumptions in MaCurdy (2015). This highlights an additional mechanism that enhances the regressive nature of the minimum wage tax: Regions with more low wage workers experience larger changes in product demand since the minimum wage is more binding and affects a larger share of consumers, leading to a higher minimum wage pass-through elasticity in those places.[7]

Fourth, this paper adds to a growing macroeconomic literature on how large demand shocks can affect retail prices procyclically. For instance, Beraja, Hurst and Ospina (2015) show that regions with larger employment declines experience lower growth in price levels. Stroebel and Vavra (2015) (hereafter SV) study how changes in housing prices affect consumer wealth and consequently retail prices by raising markups.[8] I argue that the minimum wage increases retail prices by the same mechanism, providing further evidence on

Murphy and Pierce 1993, Autor, Katz and Kearney 2008, and Autor and Dorn 2013) while the other side has emphasized institutional factors such as the declining real minimum wage (e.g. DiNardo, Fortin and Lemieux 1996 and Lee 1999). Dustmann, Ludsteck and Schönberg (2009) find evidence for both, with technological change responsible for upper-tail inequality and episodic events such as supply shocks and changes in labor market institutions responsible for lower-tail inequality.

7. One limitation is that as mentioned above, scanner data do not cover the entire consumption basket, so any distributional effect found is driven by heterogeneity across income groups in consumption only for goods sold in covered retail stores. Nevertheless, the data cover a broad range of product groups across several store types. I describe this in further detail below.

8. These papers exploit large, persistent, and unanticipated demand shocks that could potentially shift demand and lower demand elasticities, in contrast to other existing studies that find countercyclical pricing or small price responses such as Chevalier, Kashyap and Rossi (2003), Gagnon and Lopez-Salido (2014), and Cavallo, Cavallo and Rigobon (2014), which exploit predictable seasonal holidays and episodic weather events.

how increased income among consumers may lead to lower household shopping intensities and demand elasticities, generating a procyclical natural markup. I also derive pass-through formulas to shed light on the factors that determine the size of the price response.

Fifth, this paper adds to literature that studies price rigidity within retail chains. DellaVigna and Gentzkow (2017) (hereafter DVG) find that most retail chains in the US implement uniform pricing across stores in the same chain, and show that this dampens the overall response of prices to local economic shocks using a calibration. I provide direct empirical evidence to support this claim and find that this has major policy implications, since the local price response to local minimum wage shocks is completely attenuated for stores in rigid chains.

Sixth, this paper also links to a vast literature on estimating pass-through in international economics, industrial organization, and marketing. Most of the literature has focused on the pass-through of *cost* shocks and consistently shown that cost pass-through is incomplete in most markets due to markup adjustment in imperfectly competitive markets among other factors. I provide suggestive evidence that the minimum wage pass-through to retail prices consists not only of a labor *cost* shock but also a *demand* response, which raises prices further in imperfectly competitive markets. By calibrating the size of the pass-through elasticity when demand increases as a result of a minimum wage hike, I show that the demand response can generate a sizable price effect. I also show suggestive empirical evidence that multi-product retailers price strategically across product groups by raising markups in the most demand inelastic product groups.

This paper is organized as follows. I first describe the data and how I construct the price indices in Section 2.3. Next, I discuss my empirical strategy in Section 2.4. Main results are then presented in Section 2.5. Pass-through formulas are derived in Section 2.6 to shed light on the determinants of the minimum wage pass-through elasticity. Section 1.7 corroborates the theory by providing evidence for consumer response in shopping behavior to minimum wage hikes. Section 1.8 presents results on the heterogeneity of the minimum wage pass-through elasticity across products, along with a discussion of the key policy implications of the results. Concluding remarks are offered in Section 2.7.

1.3 Data and Construction of Price Indices

In this section, I give an overview of the data used for analysis and outline the construction of the price indices. I describe the data further in Appendix Section 1.12.3.

1.3.1 Price Indices

1.3.1.1 Nielsen Retail Scanner

I use the Nielsen Retail Scanner Dataset available through a partnership between the Nielsen Company and the James M. Kilts Center for Marketing at the University of Chicago Booth School of Business.[9] The data consist of weekly pricing, volume, and store merchandising conditions generated by participating retail store point-of-sale systems across the US from 2006-2015. Data are included from approximately 35,000 participating stores and include store types such as drug, grocery, and mass merchandise stores, covering around 53-55% of national sales in food and drug stores and 32% of national sales in mass merchandise stores. The finest location of each store is given at the county level. I only use stores that appear throughout the entire sample period such that store entry and exit do not affect results. Among the stores in the sample in 2006, 84% remain throughout the entire sample period. A huge number of products from all Nielsen-tracked categories are included in the data, with 2.6 million universal product codes (UPCs) in total aggregated into around 1,100 product modules, which are further aggregated up to 125 product groups.

The advantage of using the retail scanner data as opposed to the Nielsen Consumer Panel is that a wider range of goods is observed at higher frequencies and quantities.[10] Scanner

9. Information on access to the retail scanner data as well as the consumer panel data described below is available at http://research.chicagobooth.edu/nielsen/.

10. I also attempted to use the consumer panel, but since only a small number of goods are regularly

price indices are constructed as in Beraja, Hurst and Ospina (2015). I briefly describe the approach they adopt in Appendix 2.10.4 and refer interested readers to their paper for details. I also construct a range of different price indices using alternative methods, which give nearly identical results.

To investigate the behavior of the constructed indices, the scanner price index is compared to the publicly available CPI series. Since the BLS only publishes local price indices for around 20 sample areas in the US, I match the available CPI price indices at the city level with the store price indices constructed by taking a sales-weighted average across stores in each available city. This leaves 16 cities that can be compared to the scanner price indices, and this is done for food, food at home, and food away from home. The indices exhibit a high correlation of around 0.75-0.8. Figure 1.2 shows the different indices for New York City. Plots for other cities are shown in Appendix Figure 1.11. Overall, the Nielsen grocery store price indices track the ones produced by the CPI food indices closely. The average annual inflation rates are around 2%, similar to the national CPI inflation over the sample period.

1.3.2 Nielsen Consumer Panel

The Nielsen Consumer Panel Dataset represents a longitudinal panel of approximately 40,000 to 60,000 US households from 2004 to 2015 who continually provide information to Nielsen about their households and what products they buy, as well as when and where they make purchases. Panelists use in-home scanners to record all their purchases, from any outlet, intended for personal, in-home use. Products include all Nielsen-tracked categories of food and non-food items, across all retail outlets in the US Nielsen samples all states and major markets. Panelists are geographically dispersed and demographically balanced. Each panelist is assigned a projection factor, which enables purchases to be projectable to the entire US. I use this data to construct measures of household expenditure and shopping

purchased by each household, it is difficult to construct a representative regional price index of high frequency by income group. Furthermore, the consumer panel is subject to non-response bias since consumers may not always scan their purchases.

intensities, which include (1) the share of expenditures using coupons (coupon share), (2) the share of expenditures on goods that are on sale (deal share), and (3) the share of expenditures on generic store brands (store brand share).

1.3.3 Minimum Wage Series in the US

I use the state-by-month or state-by-quarter minimum wage data in the US from 2006-2015. These data are made available by Vaghul and Zipperer (2016) and compiled from a wide variety of primary sources.[11] Results are nearly identical when accounting for local minimum wage ordinances at the city or county level. The minimum wage used is the maximum of the federal and state minimum wage, which is commonly known as the state effective minimum wage. I plot the minimum wage over time for all states from 2006-2015 in Figure 1.3. Note that there is quite a lot of within-state variation over the period of interest that is staggered across time for different states, providing useful variation for identification. The lower envelope is the federal minimum wage over time, since some states are consistently bound by federal changes. This implies that there is substantial variation beyond the federal changes due to state minimum wage laws. Furthermore, federal changes also provide identifying variation since some states are forced to comply with federal legislation while states with higher minimum wages are unaffected.

To get a better understanding of the frequency of minimum wage changes in the sample period, Figure 1.4 plots the number of minimum wage changes by year and type, pooling together all states, from 2006-2015. The figure indicates that the sample period contains quite a lot of minimum wage variation that comes in two waves across different phases of the business cycle, with mostly federal changes in the first wave in 2007-2009 and only state changes in the second wave in 2014-2015.[12] There are 4 types of minimum wage changes:

11. Latest version available at https://github.com/equitablegrowth/VZ_historicalminwage/releases.

12. Federal minimum wage changes are defined as those which were binding on states. For example, a state which had a state minimum wage lower than the new federal minimum wage would contribute to a

federal legislation, state legislation, state ballot (where voters decide whether the minimum wage should be increased), and subsequent changes due to indexation. This implies that the minimum wage is often raised automatically since it is linked to price indices, raising potential concerns about reverse causality. However, almost all states (with the exception of Colorado) use the national CPI for indexation, which implies that period fixed effects are a sufficient control. Results are robust to dropping these indexing states. I also report additional characteristics of the minimum wage changes in Appendix Tables 1.17 and 1.18. Minimum wages are often implemented after the announcement of the legislation with some time lag. The average implementation lag is around 4.13 quarters in the first wave and 1.80 quarters in the second wave, which is important for understanding the dynamics of price changes in response to minimum wage hikes.

1.3.4 Labor Market Data

The Quarterly Workforce Indicators (QWI) dataset provides labor market statistics by county, detailed industry, worker demographics, employer age and size. It was first used in the minimum wage literature by Dube, Lester and Reich (2013), which I follow closely to provide empirical evidence on labor market impacts of the minimum wage on retail stores. I use five dependent variables for my analysis: earnings, employment, hires, separations, and turnover.

I also use the ACS and CPS Merged Outgoing Rotation Groups (MORG) data due to the availability of information on hourly wages, which is unfortunately not present in the QWI. Since they are frequently used in the literature, I do not describe the data here.[13]

federal minimum wage change for that state, while states with a state minimum wage already above the new federal minimum wage would not because that federal minimum wage change was not binding for those states. State minimum wage changes are defined as changes not directly caused by a binding rise in the federal minimum wage.

13. Information on the ACS is available at https://usa.ipums.org/usa/, while the CPS MORG data is available at http://www.nber.org/data/morg.html.

1.4 Empirical Strategy

To estimate the impact of the minimum wage on prices, I apply typical panel fixed effects approaches as opposed to a pure event study methodology or synthetic control due to numerous overlapping minimum wage events in my sample. This also allows me to take advantage of variation in magnitudes of minimum wage hikes across events. While both price indices at the state and store level can be constructed, I report store-level regressions because there is more information available on store type and geographic location. In addition, I report results using quarterly price indices because labor market variables are at the quarterly level, and most pass-through literature also uses quarterly variables. In my preferred specification, the log of the scanner price index P_{it} for store i in state s and time period t is regressed on log minimum wage MW_{st} for the store-year-quarter panel with store and period fixed effects to control for unobserved store characteristics and common time trends that affect prices, as shown in equation 2.5:

$$\ln P_{it} = \alpha + \beta \ln MW_{st} + X'_{ct}\gamma + \alpha_i + \alpha_t + \varepsilon_{it} \tag{1.1}$$

Since the level of the price index is not interpretable, only relative changes are relevant. The log-log specification gives the interpretation of β as the elasticity of prices with respect to the minimum wage, which is known as the minimum wage pass-through elasticity. I also include control variables X_{ct} matched to the county c the store is located in, such as log housing price, log county unemployment rate, log county average wages and log county population. These variables have been shown to have impacts on regional prices in SV, Beraja, Hurst and Ospina (2015), and Handbury and Weinstein (2015). Results are robust to the exclusion of control variables. Standard errors are clustered by state to allow for autocorrelation in unobservables within states since the identifying variation is at the state level, following Bertrand, Duflo and Mullainathan (2004).

I compare the contemporaneous effect of the minimum wage estimated with equation 2.5

with the cumulative effect by using a distributed lag model as shown in equation 2.6:

$$\ln P_{it} = \alpha + \sum_{j=-k}^{k} \beta_j \ln MW_{s,t-j} + X'_{ct}\gamma + \alpha_i + \alpha_t + \varepsilon_{it} \tag{1.2}$$

The cumulative effect is obtained by adding together all the coefficients. While the standard cumulative effect includes only the sum of the contemporaneous effect and all the lag coefficients, the lead coefficients are added as well because minimum wage changes are often announced ahead of time and there could be anticipatory changes in prices attributable to the minimum wage change. I also explored the alternative of matching the announcement date as opposed to the implementation date to the minimum wage change. More importantly, these leads provide a very useful falsification test that is common in the literature, since the minimum wage is not expected to have effects on variables of interest many quarters before implementation.

In addition, I also implement a triple differences approach by interacting the log minimum wage with several determinants of the pass-through elasticity, denoted as B_{it}, as shown below in equation 2.7:

$$\ln P_{it} = \alpha + \beta_1 \ln MW_{st} + \beta_2 \ln B_{it} + \beta_3 \ln MW_{st} \times \ln B_{it} + X'_{ct}\gamma + \alpha_i + \alpha_t + \varepsilon_{it} \tag{1.3}$$

For example, B_{it} can be measures of how binding the minimum wage is in each county, such that county-level variation in bindingness can be used in addition to state-level variation in the minimum wage. State-period fixed effects α_{st} can also be used in this case to control for heterogeneous trends at the state level, in which case β_1 will not be identified.

To address the potential issue of heterogeneous trends, I also apply the local controls approach in Dube, Lester and Reich (2010) to analyze labor markets.[14] The local controls

14. One problem with the local controls approach to study product markets is that if consumers travel across state borders to contiguous county stores due to the price rises from a minimum wage increase in their

approach is illustrated in equation 1.4:

$$\ln P_{it} = \alpha + \beta \ln MW_{st} + X_{ct}^{\prime}\gamma + \alpha_i + \alpha_{pt} + \varepsilon_{it} \tag{1.4}$$

α_{pt} are county-pair-period fixed effects, which means that the pass-through elasticity is identified only by the variation in minimum wage between contiguous county pairs.

1.5 Main Results

In this section, I present the main empirical evidence on how the minimum wage impacted retail stores, first through the labor markets, which has implications for the product markets. I give further interpretation of the results and the interaction between these two markets using theory derived in Section 2.6.

1.5.1 Labor Markets

As discussed in Section 2.2, there has been a back and forth debate regarding the choice of control groups used to identify the employment effect of the minimum wage. I first apply standard fixed effects as in equation 2.5 to estimate the minimum wage effects on earnings and employment. Results are presented for four types of retail stores that are present in scanner data that can be matched to 4-digit NAICS industries: health and personal care stores (drug stores), grocery stores, department stores, and discount stores (merchandise stores). Restaurants are also included for comparison. Table 1.2 reports estimates of contemporaneous minimum wage effects from 10 separate regressions, with 5 industries and 2 outcomes. To match with the product market evidence using scanner data, the sample period is chosen to be 2006-2015. A 10% minimum wage hike raises earnings of grocery store workers by around 1.5%, while effects are also present for restaurants as in previous literature and also

state, using contiguous county stores would attenuate the estimated pass-through elasticity due to price rises in those counties as well from substitution.

for merchandise stores. To test for parallel trends, I also added six quarters of leads and four quarter of lags to estimate the cumulative effect, and a subset of those results is shown in Figure 1.5, which indicate that results are not driven by differential pre-trends. Estimates for employment are statistically insignificant.

In addition, to investigate whether the earnings effect is stronger in counties where the minimum wage is more binding, I estimate the effect separately for two samples. I use the Kaitz index, defined as the ratio of the minimum wage to the average wage, in the pre-period, i.e. first quarter of 2006, and define a county as "rich" or "poor" if it has a Kaitz index below or above median respectively. Results shown in Appendix Table 1.20 imply that the earnings effect is indeed stronger in poor counties.[15] I also follow Dube, Lester and Reich (2013) and use a contiguous county sample to estimate the impact of the minimum wage on earnings, employment, hires, separations, and turnover in Appendix Table 1.19. In addition to replicating their results on restaurants, I also find similar results for grocery stores, although the contiguous county-pair period fixed effects attenuates the earnings effect for both grocery and merchandise stores. Overall, these results provide suggestive evidence that the minimum wage increases labor costs in grocery and merchandise stores, providing a range of estimates for the minimum wage impact on labor costs. This effect is also stronger in regions where the minimum wage is more binding. I interpret the implications of these results for product markets in Section 2.6.

1.5.2 Product Markets

I first estimate the contemporaneous effect of the minimum wage on prices using the standard fixed effects approach and store price indices as shown in equation 2.5. Table 1.3 presents the estimated pass-through elasticities for all store types with and without control variables. The point estimates are statistically significant for grocery stores with a pass-through elasticity of 0.058, while the coefficients on the control variables have signs mostly

15. Using triple differences instead implies nearly identical point estimates.

consistent with previous literature. The estimates are not statistically significant for other store types. I explain why the pass-through elasticity is large and significant in grocery stores but not in other store types in Section 1.5.2.2.

I test for differential pre-trends by estimating a distributed lag model for grocery stores as shown in equation 2.6. I choose an event window of 6 quarters before and 4 quarters after a minimum wage hike and plot both the distributed lag coefficients and the cumulative effects. This event window is long enough to show parallel pre-trends, and short enough such that little of the minimum wage variation is dropped from the sample.[16] Given that the mean announcement of a minimum wage hike is around 3.21 quarters before the implementation, Figure 1.6 shows suggestive evidence that prices spike mostly during the announcement of minimum wage hikes, and trended slightly upwards after these events. Renkin, Montialoux and Siegenthaler (2017) also found announcement effects and argue that this is consistent with models of price setting with nominal rigidities. More importantly, there is no evidence that the positively significant results shown earlier are driven by differential pre-trends. Figure 1.7 plots the observations (collapsed into 50 bins) used to estimate the cumulative effect 6 quarters before a minimum wage change and 4 quarters after a change, showing that the slope is initially flat but becomes positive. Although the timing of the price changes is not extremely sharp, this is consistent with the fact that announcement dates vary for each minimum wage hike and the possibility that menu costs lead to gradual adjustment in prices.

To get a clearer picture of the timing, I first run the distributed lag model using the announced minimum wage instead of the implemented minimum wage as in Renkin, Montialoux and Siegenthaler (2017). For announcements that cover multiple minimum wage changes, I take the maximum across all changes as the announced minimum wage. I also drop states which index their minimum wage to the national CPI since there is no exact

16. A lot of the variation is in 2015q1, and the latest available minimum wage data are in 2016q3, which is 6 quarters after 2015q1.

announcement timing for these states. Figure 1.6b confirms that prices rise right after the announcement of minimum wage. To further address concerns of differential pre-trends, I argue that it is more reliable to draw inference from minimum wage changes for which the implementation lag is short as well as use variation outside of the recession. Therefore, I run the distributed lag within the sample period of 2013-2015, since most of these minimum wage changes have much shorter implementation lags as shown in Table 1.18. Figure 1.6c shows sharper timing and parallel trends in the entire pre-period, and a similar cumulative effect of around 0.1.

There is reason to believe that the pass-through elasticity is heterogeneous across counties, since a minimum wage hike should have a larger effect where it is more binding. I employ the triple differences approach shown in equation 2.7 and examine how the pass-through elasticity changes with the Kaitz index, which is fixed to its value in the first quarter of 2006. I report the results with state-period fixed effects in Table 1.4. The interaction coefficient is large and strongly significant. Raising the Kaitz index by 0.1 increases the pass-through elasticity by about 0.0426. This implies that within the Kaitz index distribution, moving a county from the 25th percentile value of around 0.3 to the 75th percentile of around 0.5 raises the pass-through elasticity by about 0.085. Similar to Alonso (2016), I use alternative measures of how binding the minimum wage is in each county: the log average wage, the fraction of households earning below $25,000 annually (which is the closest bracket available in the ACS to the annual income of an average minimum wage worker), and the minimum wage annual income to median household annual income ratio. All of these measures give strongly significant results with consistent magnitudes. These results imply that the minimum wage does have distributional effects by acting as a regressive tax, raising prices by larger magnitudes in poorer regions. I also find that the pass-through elasticity is larger in stores with lower revenue, since smaller stores are more likely to be located in poorer regions. The interaction coefficient is insignificant using a proxy of grocery market concentration, the number of grocery stores per population in each county.[17]

Alternatively, I separate the sample by how binding the minimum wage is for each store using the county-level Kaitz index. Again, I define a store as "rich" or "poor" if it resides in a county with a pre-period Kaitz index below or above median respectively.[18] Summary statistics for these two groups of counties in the pre-period are shown in Table 1.5. On average, rich counties are larger in population by about 4 times, which implies that there are far more grocery stores in rich counties than in poor counties, although the number of stores per capita is actually similar in poor counties. Importantly, both groups of counties are geographically dispersed and located in almost all states, providing sufficient variation in the minimum wage. Figure 1.8 maps out these counties.

I estimate equation 2.5 separately for both groups of counties by store type and show the results in Table 1.6. The estimated pass-through elasticity for grocery stores in poor counties is statistically significant at the 1% level[19] and economically significant. A 10% increase in the minimum wage raises grocery store prices in those counties by 0.84%. This magnitude is slightly larger the pass-through elasticity in restaurants estimated by Aaronson, French and MacDonald (2008) of 0.7%. In Figure 1.9, these results are graphically displayed in a plot of the residualized log price index against the residualized log minimum wage with each store-year-quarter observation collapsed into 50 bins. The slope is steeper for poor counties.[20]

To examine the large pass-through elasticity for grocery stores, I conduct several robustness checks. First, I provide estimates separately in four periods in Table 1.7, since there

17. Theoretical derivations in Section 2.6 show the supply and demand shifts have opposing predictions on how competition affects the pass-through elasticity.

18. Results are similar using the mean across the sample period instead. Results are robust to dividing counties into 4 quartiles rather than 2 quantiles according to their Kaitz index as shown in Appendix Table 1.21.

19. The p-value is 0.0002, while the number of clusters remains high at 41. Even if a Bonferroni adjustment is made for multiple hypothesis testing across 6 independent hypotheses, the result is still statistically significant at the 1% level since $0.01/6 = 0.0017$.

20. I also plot similar figures for drug and merchandise stores in Appendix Figure 1.13.

may be concern that the results are driven by heterogeneous trends during the Great Recession. Indeed, the results are strong in the 2008-2009 recessionary period but potentially even stronger in the 2014-2015 recovery period. These results indicate that the mechanism inducing a high pass-through persists across different phases of the business cycle.

In Table 1.8, I estimate the pass-through elasticity by using minimum wage levels instead of logs,[21] dropping stores with average price indices below the 5th percentile or above the 95th percentile, monthly instead of quarterly observations, states that do not index the minimum wage to the CPI, counties that are interior or contiguous to state borders, accounting for local minimum wage ordinances, weighting observations by store sales or county population, aggregating store price indices to the county level using store sales before weighting the observations by county sales, and adding store-specific time trends. The point estimates remain roughly similar and statistically significant. In Table 1.9, I construct price indices using alternative methods as illustrated in Appendix 2.10.4 and show that my results are robust. I also show additional results using only city and county level minimum wage variation in Appendix 1.12.4.

1.5.2.1 Sales and Results by Product Department

Next, I present results on the response of real and nominal sales to the minimum wage. While the theoretical prediction for the sign of the quantity response is ambiguous due to the interaction between supply and demand, an empirical estimate may provide a useful test of whether demand effects are also at work. Alonso (2016) finds that real sales, defined as sales with prices fixed to a particular time period, increases in response to minimum wage hikes. I present alternative results by defining real sales as nominal sales divided by the store specific price index and using this as a measure of quantities for an additional two years of data.[22] Table 1.10 shows the effect of the minimum wage on nominal and real sales for all store types

21. The coefficient implies that a \$1 increase in minimum wage increases prices by 0.85%, which is in line with a pass-through elasticity of 0.058 since the federal minimum wage is \$7.25.

22. Using sales of goods actually used to construct the price index gives very similar results.

with the sample again segmented by rich and poor counties. While both the nominal and real sales response are positive and higher in poor counties for grocery stores, only the nominal sales response is marginally statistically significant. The nominal and real sales response in poor merchandise stores is large and statistically significant, which is suggestive of demand effects given arguments below in Section 2.6. Overall, these coefficients are slightly smaller than those found in Alonso (2016), and the somewhat large standard errors make it difficult to draw any conclusions from these results about the interaction of supply and demand effects.[23]

Furthermore, I also construct price indices by product department as classified by Nielsen to understand how price and real sales responses differ across product departments as shown in Table 1.11, which presents results for grocery stores in poor counties. [24] Most of the results are driven by food, which generates most of the revenue in grocery stores in the sample, as prices and quantities both have positive responses, although the quantity response is marginally insignificant. There is a large price response in non-food grocery while the quantity response is negative and insignificant, and alcoholic beverages also experience a statistically significant increase in price.

1.5.2.2 Impact of Within-Chain Price Rigidity

To understand why the pass-through elasticity estimates are heterogeneous across store types, I first show the proportion of revenue generated by each of five product departments across store types in Table 2.13. Drug stores earn most of their revenue from health and beauty care products, while both grocery and merchandise stores earn most of their revenue from food, although grocery stores earn a lot more from food at around 77%. Combining

23. Deseasonalizing sales (and prices) gives almost identical results. I attempted two methods, using store-quarter-of-the-year fixed effects and the X-13ARIMA-SEATS Seasonal Adjustment Program available through the Census Bureau, both of which give similar results.

24. I report additional results by store type in Appendix Tables 1.23, 1.24, and 1.25. There is evidence that prices and quantities increased in certain product departments in mass merchandise stores and the results are statistically significant.

these facts with the results in Section 1.5.2.1 provides a potential explanation for why other store types have statistically insignificant pass-through elasticities. If consumers respond to income increases mostly by changing demand for products such as food but not other types of products, then there would be smaller price responses in drug and merchandise stores, both of which do not derive the majority of their revenue from selling food.

The pass-through elasticity estimated from state minimum wage shocks should be affected by the extent to which chains are pricing rigidly. Chains that are located primarily in one state should also exhibit local pricing and react to local shocks, while chains that price rigidly and locate across many states should exhibit national pricing and will not react to local shocks. I follow DVG to measure the extent of price rigidity for each of the retail chains in the data and lay out the details in Appendix Section 1.12.3. I plot the distribution of flexibility measures as well as number of states each chain is in across stores by store type in Figure 2.19. A chain is pricing more rigidly if the flexibility measure is closer to zero. Both drug and merchandise stores belong to a few large chains that price rigidly, while a large amount of grocery stores belong to chains that price flexibly or chains that are located only in a few states. There are over 50 grocery chains while both drug and merchandise stores come from around 5 retail chains each.[25] This implies that most grocery stores are engaging in local pricing while drug and merchandise stores are not.

In Table 1.12, I show that the estimated pass-through elasticity decreases with the number of states the chain is in and increases for more flexible chains by interacting the minimum wage with these two variables in a sample with all store types. This is consistent with the previous finding that pass-through elasticity is small and insignificant in drug and merchandise stores but large and significant in grocery stores. Furthermore, I divide the grocery store sample into stores with local pricing and those without. I define stores as local pricing

25. Data from the Economic Census also show that the grocery store industry is much less concentrated and less dominated by chains than the drug and merchandise store industries. For example, market share of the 4 largest firms is 30.7%, 54.4%, and 73.2% for grocery stores, drug stores, and merchandise stores respectively in 2007.

if it has a flexibility measure above or at the median, or if it belongs to a chain that is located in one or two states. I include chains with two states because almost all such chains earn over 90% of their revenue from one state. The minimum wage pass-through elasticity increases from 0.058 in the full sample to 0.083 in the flexible sample and the 95% confidence interval rules out estimates below 0.03. On the other hand, the estimate in the rigid sample is small, insignificant, and statistically different from the estimate in the flexible sample.[26] This further substantiates the claim that price rigidity, rather than difference in the type of goods sold, completely attenuates the effect of the minimum wage on prices for stores in rigid chains.

Overall, I conclude that a 10% increase in the minimum wage raises prices in grocery stores by about 0.6-0.8% but not in other types of stores because of within-chain price rigidity, and the response is statistically significant and larger in poorer counties. There is no clear evidence for changes in quantities sold. Results are not driven by differential pre-trends and pass a variety of robustness checks. I further interpret the results with theory derived in the next section.

1.6 Theory

To understand the determinants of the minimum wage pass-through elasticity and provide estimates based on theory to compare with the reduced-form empirical estimates, I first derive the *cost* pass-through elasticity under a range of assumptions about the degree of competition in the product market. This framework holds the demand side *fixed* and assumes the minimum wage only raises labor costs. I show that cost pass-through theory cannot fully explain my empirical findings, suggesting that demand side effects are also needed to explain the results. I then derive the *demand* pass-through elasticity under a range of assumptions about the degree of competition in the product market, holding the supply side *fixed* and

26. In addition, all results are robust to adding as a control variable the revenue-weighted average of minimum wages for stores in the same chain in other states.

assuming that the minimum wage only raises household income. I show that the theoretically calibrated magnitude of the demand pass-through elasticity is consistent with my reduced-form estimate. I show further details on the theory in Appendix Section 1.12.6.

1.6.1 *Cost Pass-Through Elasticity*

I first assume that the minimum wage only affects labor costs. I use unit-tax pass-through derivations from Weyl and Fabinger (2013), and convert them to minimum wage pass-through elasticities. I lay out the derivations in Appendix 1.12.5.1, which shows that multiplying the pass-through rate $\frac{dp}{dt}$ by the cost share of minimum wage labor s_{L_1} gives the minimum wage pass-through elasticity $\frac{\partial \ln p}{\partial \ln w_1}$. These formulas, under perfect competition, monopoly, and asymmetric imperfect competition, are presented below in equations 1.5, 1.6, and 1.7, respectively:

Perfect competition

$$\frac{\partial \ln p}{\partial \ln w_1} = \frac{s_{L_1}}{1 + \frac{\varepsilon_D}{\varepsilon_S}} \tag{1.5}$$

Monopoly

$$\frac{\partial \ln p}{\partial \ln w_1} = \frac{s_{L_1}}{1 + \frac{\varepsilon_D - 1}{\varepsilon_S} + \frac{1}{\varepsilon_{ms}}} \tag{1.6}$$

Symmetric imperfect competition

$$\frac{\partial \ln p}{\partial \ln w_1} = \frac{s_{L_1}}{1 + \frac{\theta}{\varepsilon_\theta} + \frac{\varepsilon_D - \theta}{\varepsilon_S} + \frac{\theta}{\varepsilon_{ms}}} \tag{1.7}$$

Under perfect competition, the pass-through elasticity depends only on the cost share, demand elasticity ε_D, and supply elasticity ε_S. If I further assume a constant returns to scale production function, the supply elasticity is infinite and the pass-through elasticity is simply the cost share. Under monopoly, an additional term ε_{ms} determines the pass-through elasticity. $ms \equiv -p'q$ is the marginal consumer surplus, so $\varepsilon_{ms} = ms/ms'q$ depends on the curvature of demand. Under symmetric imperfect competition, the pass-through elasticity also depends on the market-conduct parameter $\theta = \frac{p-mc}{p}\varepsilon_D$, which is the elasticity-adjusted Lerner index and is commonly used to measure the degree of competition. Furthermore, if market conduct varies with quantity, pass-through elasticity would depend on the elasticity of market conduct with respect to quantity. For example, pass-through elasticity is smaller if higher prices create more competitive conduct ($\varepsilon_\theta > 0$).

These formulas have several interesting empirical implications. First, store types that have higher minimum wage labor cost share should have higher pass-through elasticities. Second, product groups with lower elasticities of demand should have higher pass-through elasticities. Third, pass-through elasticity should vary across locations with different degrees of competition, holding store type and product group constant.[27] Fourth, how income relates to the pass-through elasticity is ambiguous. On the one hand, individuals in poorer locations would probably face higher pass-through elasticities because more minimum wage workers live there, but on the other hand, lower income of most customers could possibly raise elasticities of demand, lowering the pass-through elasticity. MaCurdy (2015)'s result that the minimum wage is a regressive tax is driven by the fact that low income workers tend to consume a higher share of goods produced by minimum wage labor. These formulas point to several additional determinants of the pass-through elasticity that drive distributional effects: which type of store a consumer shops in, the demographic characteristics of the consumers that shop there, and the competitive environment the store faces are all important factors to consider.

To obtain some back-of-the-envelope estimates of the pass-through elasticity, I first obtain estimates of the pass-through rate from the existing literature. Besanko, Dubé and Gupta (2005) show that wholesale cost pass-through elasticity is more than 60% for 9 of 11

27. However, many of these economic parameters, such as supply and demand elasticities as well as market conduct, are not easy to estimate, although I attempt to obtain some proxies for them.

product categories, and the median wholesale cost pass-through elasticity for the 11 product categories they investigate is 83%. Based on the Annual Retail Trade Survey conducted by the US Census Bureau, the cost of goods sold as a percentage of sales, or the wholesale cost share, is around 73% for grocery stores, so the pass-through rate is around 80-100%. MaCurdy (2015) assumes complete pass-through for all industries. Leung and Seo (2018) estimates a pass-through rate of about 50% with the same data used in this paper. To obtain an upper bound for the pass-through elasticity, I assume complete pass-through.

I show the theoretical estimates of pass-through elasticity in Table 1.13 for different industries under different assumptions using data from the ACS. [28] First, I assume no spillover effects. In this case, the minimum wage pass-through elasticity equals the minimum wage cost share, which is obtained by multiplying the pass-through rate with the payroll ratio (labor cost share) and the proportion of wages paid to minimum wage workers. The first three industries are the store types available in the scanner data, while I also include restaurants because this industry has been extensively studied in the literature and is useful as a comparison. The estimates I obtain for restaurants are very similar to those obtained by Aaronson, French and MacDonald (2008), who also obtain theoretical estimates in a similar fashion and show that their empirical estimates are almost identical, suggesting complete pass-through and perfect competition in labor markets. There are a few points worth highlighting. First, the labor cost share is relatively low for retail stores at around 10%. Second, this implies that the estimated pass-through elasticity for retail stores is over 5 times smaller than that for restaurants. For instance, the estimated pass-through elasticity is 0.0082 for grocery stores but 0.045 for restaurants.

Second, the minimum wage could raise wages for workers earning above the minimum wage. AMS find spillovers up to approximately the 15th percentile of the wage distribution,

28. I use the ACS rather than the CPS because the ACS contains about 30 times more observations, which allows me to segment the population by the Kaitz index in their county of residence. Results for the full sample are roughly similar using the CPS as shown in Appendix Table 1.26. There is no information on county of residence for some observations in the ACS probably due to confidentiality reasons. I can only use the observations for which the county of residence is known.

which are already smaller than those estimated in previous work such as Lee (1999) (hereafter Lee). However, the minimum wage is never nominally binding above the 10th percentile for their sample period, and never binding above the 6th percentile for my sample period. Previous literature has often accounted for spillovers in an ad hoc manner by including the earnings of workers with wages some percentage above the minimum wage into the minimum wage labor cost share. I derive a reduced form way of incorporating existing estimates of spillover into the pass-through elasticity formula in Appendix 1.12.5.2. The relevant formula is given in equation 1.8:

$$\frac{\partial \ln p}{\partial \ln w_1} = \frac{dp}{dt} \frac{\sum_{i=1}^{n} w_i L_i}{pY} \sum_{i=1}^{n} s_i \varepsilon_{w_i, w_1} \tag{1.8}$$

There are n types of workers, and minimum wage workers are denoted as $i = 1$. The minimum wage pass-through elasticity $\frac{\partial \ln p}{\partial \ln w_1}$ equals the pass-through rate $\frac{dp}{dt}$ multiplied by the payroll ratio $\frac{\sum_{i=1}^{n} w_i L_i}{pY}$ and a weighted sum of the shares of wages earned by workers impacted by the minimum wage s_i, where the weights are given by the spillover elasticities ε_{w_i, w_1} for each type of worker. By representing each worker type by each percentile on the national wage distribution, these spillover elasticities can be obtained from estimates in AMS and Lee. To obtain the share of wages earned by each percentile for each industry, I use the ACS to obtain percentiles in the national wage distribution as opposed to the within-industry distribution because the estimates from AMS and Lee are relevant for the national wage distribution.

Using the procedure described above, I provide evidence suggesting that the minimum wage pass-through elasticity is not driven purely by supply shocks. Even if I use the largest estimates of spillover effects estimated in Lee, the calculated pass-through elasticity is much smaller than the empirical estimate I obtain for grocery stores in poor counties. The smallest lower bound of the 95% confidence interval of my estimates is around 0.035, whereas the largest estimate of cost pass-through elasticity is 0.024. Another important fact that emerges from Table 1.13 is that pass-through elasticities are not that different for poor counties if the minimum wage hike only impacted costs. In theory, it is possible that the minimum wage labor cost share is much higher in poor counties because more of the grocery workers are minimum wage workers. However, the data suggest that this is not the case. This can be easily reconciled if the share of workers in low wage industries is what determines the Kaitz index in each county rather than the within-industry share.[29]

Alternatively, the earnings elasticity provides useful information about spillover effects. I derive an expression for the earnings elasticity in equation 1.9. The derivation is shown in Appendix 1.12.5.3.

$$\varepsilon_{\overline{w},w_1} = \sum_i s_i \varepsilon_{w_i,w_1} \tag{1.9}$$

For simplification and based on previous literature, I assume the hours elasticity ε_{h_i,w_i} is zero.[30] This expression is the weighted sum of shares earned by minimum wage affected workers shown in equation 1.8, providing an alternative way to obtain the pass-through elasticity. I use this formula to interpret the reduced form estimates of earnings elasticity in Section 1.5.1, and I also use further reduced form estimates by Kaitz index in Appendix Table 1.20. The earnings elasticity estimate using standard fixed effects is 0.117 and 0.184 for rich and poor counties respectively, and the 95% upper confidence bounds do not exceed 0.28. Theoretically, together with the fact that the labor cost share is 10% as mentioned above, this implies a pass-through elasticity of 0.028, lower than the 95% lower confidence bound of 0.035 for the reduced form estimate in Section 1.5.2. Therefore, I conclude that cost pass-through theory cannot explain the large magnitude of the minimum wage pass-through elasticity estimate or the dispersion in the estimate between rich and poor counties, suggesting that demand side effects play an important role.

29. One caveat about the method used to derive the pass-through elasticity with spillovers is that it assumes that the spillover effect estimated with the national wage distribution applies to workers in these specific industries. This is required because there are no empirical estimates of the spillover effect within industries.

30. This is a reasonable assumption given the paucity of estimates on the hours elasticity. Dube, Naidu and Reich (2007) shows that the hours elasticity is not statistically significantly different from zero.

1.6.2 Demand Pass-Through Elasticity

I now assume that the minimum wage only affects household income. Let market demand $Q^D(p, I)$ depend on price p and income I, and now assume instead that the minimum wage w_1 has an effect on demand only through income. Under perfect competition, I obtain the demand pass-through elasticity by differentiating the equilibrium condition with respect to the minimum wage:

$$
\begin{aligned}
Q^D(p, I) &= Q^S(p) \\
\frac{d \ln p}{d \ln w_1} &= \frac{\frac{\varepsilon_{Q^D,I}}{-\varepsilon_D}\varepsilon_{I,w_1}}{1 - \frac{\varepsilon_S}{\varepsilon_D}}. \qquad (1.10)
\end{aligned}
$$

I show all steps of the derivations in Appendix Section 1.12.5.4. The pass-through elasticity is now equal to $\frac{1}{1-\frac{\varepsilon_S}{\varepsilon_D}}$, which is 1 minus the cost pass-through rate, multiplied by the income elasticity of demand $\varepsilon_{Q^D,I}$, the inverse demand elasticity, and the income elasticity with respect to the minimum wage ε_{I,w_1}. Likewise, I can derive the demand pass-through formula under symmetric imperfect competition. First, I start from the profit-maximization condition and differentiate it with respect to income. In addition, I allow the demand elasticity to depend on income. I obtain an expression for the quantity response to income:

$$
\begin{aligned}
P(Q, I) + \theta \frac{\partial P(Q, I)}{\partial Q} Q - c'(Q) = 0 \\
\frac{dQ}{dI} = -\frac{\theta \frac{\partial^2 p}{\partial I \partial Q} Q + \frac{\partial p}{\partial I}}{\frac{\partial MR}{\partial Q} - \frac{\partial MC}{\partial Q}}. \qquad (1.11)
\end{aligned}
$$

Next, I use the above expression to obtain the pass-through formula:

$$
\begin{aligned}
\frac{dp}{dw_1} &= \frac{\partial p}{\partial Q}\frac{dQ}{dI}\frac{dI}{dw_1} + \frac{\partial p}{\partial I}\frac{dI}{dw_1} \\
\varepsilon_\rho \equiv \frac{d\ln p}{d\ln w_1} &= \left(1 - \frac{dp}{dt}\right)\frac{\varepsilon_{Q^D,I}}{-\varepsilon_D}\varepsilon_{I,w_1} + \left(\frac{dp}{dt}\right)\frac{\theta}{-\varepsilon_D}\varepsilon_{p',I}\varepsilon_{I,w_1}. \qquad (1.12)
\end{aligned}
$$

There are two terms through which demand side effects could raise prices. First, if the minimum wage increases product demand among consumers due to increased income, a demand shift could raise prices if the pass-through rate is below one. This first term, which I denote as the shift effect, is the same as the term under perfect competition, except the pass-through rate is different because the market structure is different. Second, income could have a direct effect on demand elasticity. I denote this second term as the slope effect. These two terms imply that retailers could raise prices by raising markups even if marginal costs are flat. According to SV, marginal costs of retailers are not very responsive to local demand shocks. Around 75% of costs in a retail store are wholesale costs, which exhibit little geographic variation due to the tradable nature of retail goods as well as restrictions imposed by the Robinson-Patman Act. Almost all remaining costs are retail rents and labor costs, and SV show that these two components do not respond strongly to local demand shocks. Therefore, if the supply elasticity is large, the pass-through rate would be incomplete and hence below one if the market-conduct parameter is positive and demand is log-concave.

In Table 1.14, I theoretically calibrate the magnitude of the minimum wage pass-through elasticity assuming only demand effects using equation 2.34. I focus on the shift effect because the slope effect requires an estimate of the super-elasticity that is rarely estimated in the previous literature. I focus on food-at-home for these estimates since grocery stores derive over 75% of their revenue from food-at-home and estimates of marginal propensity to consume (MPC) for food-at-home are most readily available. As summarized in Hoynes and Schanzenbach (2009), MPC estimates for food-at-home are around 0.03-0.17. I use the lower range of these estimates to calculate the income elasticities of demand for food-at-home, which are around 0.03-0.1 given an expenditure share of 0.1 for food-at-home. The demand elasticity estimate is obtained from Leung and Seo (2018) and consistent with Andreyeva, Long and Brownell (2010). The pass-through rate estimate is also estimated in Leung and Seo (2018) and is slightly higher than those in Besanko, Dubé and Gupta (2005) due to a high estimated curvature of demand. The income elasticity of minimum wage is calculated using derivations in Section 1.12.5.3 and estimates from Dube (2017). The calibrated magnitudes show that the shift term of the demand effect alone can generate pass-through elasticities of about 0.02-0.06. Given differences in the income elasticity of the minimum wage across rich and poor counties, the magnitude of heterogeneity in my reduced-form estimates of pass-through elasticities are also in line with the heterogeneity in these theoretical calibrations. I also provide suggestive evidence for decreases in demand elasticities in Sections 1.7 and 1.8.1. First, I show that poorer households decrease shopping intensities when the minimum wage rises. Second, I find that product groups with lower demand elasticities have higher pass-through elasticities as consistent with the theory.

Overall, there are three mechanisms through which the minimum wage could impact prices and quantities sold in product markets. First, labor cost increases and shifts the supply curve upwards, raising prices and decreasing quantities sold. Second, consumer demand could shift outwards and raise quantities sold. Third, consumer demand elasticities could drop, leading retailers to markup retail prices and decrease quantities sold. The combined effects of these three mechanisms raise retail prices while the impact on quantities sold is ambiguous. The fact that the effect of the minimum wage on real sales in merchandise stores is positively significant, as shown in Table 1.10, is suggestive of demand effects. Since merchandise stores price nationally, quantities will not decrease as much as grocery stores as a result of increased markups, while many of the products sold in merchandise stores, in particular food, are also sold in grocery stores. Out of the three mechanisms described above, only a demand shift can raise quantities sold.

However, it is important to note that demand-induced feedback is only one of several mechanisms that could potentially increase prices beyond the magnitude predicted under complete labor cost pass-through. First, minimum wage increases could also increase the labor costs of manufacturing goods sold in the retail sector. I argue that this mechanism is unlikely to be driving the results for two reasons. First, the share of minimum wage workers in manufacturing is very low based on the CPS at around 0.1%. Second, these goods are tradable such that most are not sold in the states in which they are produced. Another mechanism is that consumers could be substituting to food-at-home as prices for food-away-from-home rises, increasing the demand for groceries and possibly lowering the demand elasticity as well. The results illustrated above are also consistent with this mechanism. Quantifying the magnitude of this mechanism would require estimates of the cross-price elasticity of food-at-home with respect to food-away-from-home. Although there is no clear consensus on the magnitude of this elasticity, Richards and Mancino (2014) estimate a cross-price elasticity of food-at-home with respect to fast food prices at around only 0.06. Given existing estimates of the minimum wage pass-through to restaurant prices are around 0.07, a cross-price elasticity at least an order of magnitude larger would be needed to explain the real sales responses I find.

1.7 Shopping Behavior

To provide evidence that stores raised markups due to lower demand elasticities among households in response to the minimum wage, I investigate the shopping behavior of households in response to the minimum wage using the Nielsen Consumer Panel. As shown below in equation 1.13, I regress shopping outcome Y_{it} for household i at time t on the minimum wage MW_{st} corresponding to the state s that household i lives in. I split the households into 2 quantiles based on their household income in 2006, denoting poorer households with the indicator variable $\mathbb{1}\{HHIncome_i \in LowerQuantile\}$, and interact the indicator with the minimum wage. This provides a measure for whether a household is affected by the minimum wage, since spillovers have effects up to around the 15th percentile of the wage distribution.[31] Control variables X_{it} such as the county level variables used in previous regressions are included along with household size fixed effects as well as household and period fixed effects.

$$\begin{aligned}
\ln Y_{it} = \alpha + \beta_1 \ln MW_{st} + \beta_2 \mathbb{1}\{HHIncome_i \in LowerQuantile\} \\
+ \beta_3 \ln MW_{st} \times \mathbb{1}\{HHIncome_i \in LowerQuantile\} \\
+ X_{it}'\gamma + \alpha_i + \alpha_t + \varepsilon_{it}
\end{aligned} \tag{1.13}$$

Results are shown in Table 1.15 with and without control variables. The coefficient on the interaction between the minimum wage and log expenditure is negative although it is statistically insignificant. A priori, it seems reasonable to believe that expenditures should rise for poorer households but this is theoretically ambiguous as shown in the previous section. Furthermore, store level results do not show strong effects of the minimum wage on nominal sales. However, the coefficient on all 3 measures of shopping intensity are strongly significant, suggesting that compared to richer households, poorer households may not spend more as the minimum wage rises but they become less price sensitive and reduce their shopping intensity. There may be a concern that some of the estimated effects on below median households are positive in magnitude. This could be consistent with poorer households adjusting their shopping behavior due to increased prices. I argue that it is the difference in shopping intensities between the rich and poor households in response to minimum wage changes that suggests demand effects. I show robustness checks following SV in Appendix Table 1.27 by applying state-period fixed effects, interacting household characteristics with the log minimum wage, and adding product department-period fixed effects to a sample with household expenditures at the product department level. The positively significant results for the rich in coupon share and store brand share suggests that since richer households do not experience increased wages from the minimum wage hike but face higher prices, they

31. The data only contain household income and does not contain wage information.

raise their shopping intensities. On the other hand, poorer households might face even larger increases in prices, but their shopping intensities actually do not rise or even decrease because of increased income.

1.8 Heterogeneity and Policy Implications

1.8.1 Product Heterogeneity

To offer further evidence of heterogeneity in pass-through elasticities and support for increased markups, I use the components of the store price indices, which are constructed by product group, and estimate the regression shown in equation 2.5 to obtain pass-through elasticities by product group and store type for both rich and poor counties. These estimates can be used in a second stage regression and regressed on the determinants of the pass-through elasticity such as demand elasticities by product group. As suggested by Lewis and Linzer (2005),[32] I present both OLS results with heteroskedasticity robust errors as well as weighted least squares, where the weight is the inverse standard error of the estimates, in Table 1.16. I first regress the pass-through elasticities on indicators for store type. The store type indicators are positively significant with drug stores as the omitted group, which corroborates with previous evidence. I also include as a regressor the log of the absolute value of the estimated demand elasticities for each product group, which are obtained from Broda and Weinstein (2010). The coefficient is negative and significant in the OLS specification for poor counties, which implies that when demand is more inelastic, the pass-through elasticity is higher. This is consistent with supply side effects as shown by cost pass-through theory as well as demand side effects due to variable markups.[33] Lastly, I add the expenditure share in 2006 accounted for by households with below median household income in each product

32. Lewis and Linzer (2005) argue that for estimated dependent variable models, using OLS with White errors is preferred to the WLS with inverse standard errors as weights in most cases.

33. The standard markup formula has a positive second derivative with respect to the demand elasticity, which implies that for the same percentage drop in demand elasticity, a smaller initial demand elasticity creates a larger percentage increase in price.

group according to the consumer panel. The estimated coefficient is positive but insignificant. There is also very little variation in the expenditure share across product groups, which may explain the insignificant result. I present additional results by constructing store price indices segmented by two product characteristics, and describe these results in Appendix Section 1.12.7.

1.8.2 Policy Implications

Overall, these results have several interesting and important implications. First, while the existing literature has found that the minimum wage reduces nominal wage inequality, the reduction in real wage inequality is less substantial. For example, AMS find that for a 10% increase in the minimum wage, a worker earning the 10th percentile of the national wage distribution experiences around a 1.6% increase in wages relative to the median. Extrapolating earlier findings to the entire consumer basket, with caveats that other products may have smaller responses due to smaller MPCs and rigid pricing in national chains, the increase in real wages would be smaller by 0.3-0.9% in poor counties, which brings the increase in real wages relative to the median down to about 0.7-1.3%. Furthermore, the poor who are not working would only bear the higher costs of living without increases in income.

Second, within-chain price rigidity can substantially lower the impact of local minimum wage shocks on local prices. In stores that belong to chains that practice rigid pricing and locate in states across the US, the estimated pass-through elasticity is indistinguishable from zero. This also attenuates the increase in real wage inequality by negating demand feedbacks. Since grocery stores have an expenditure share of about 60% among the stores in the data,[34] this implies that chain rigidity lowers the pass-through elasticity to about 0.035 from 0.083 using only flexible pricing stores, which is a 58% decrease. However, stores that price nationally are still likely to react to national shocks such as a rise in the federal

34. This share is calculated from the Nielsen Consumer Panel using expenditures on store types available in the Nielsen Retail Scanner.

minimum wage, and extrapolating from the subsample of grocery stores that exhibit local pricing, a 10% rise in the federal minimum wage could raise prices nationally by 0.8%.

Third, these results imply that looking only at the nominal spending response of the minimum wage hike would hugely overstate its benefits for low wage workers, as the response in real spending is around half of the response in nominal spending.

Fourth, increasing residential segregation would magnify the regressive nature of the minimum wage tax, since low wage workers will be more likely to shop at the same stores and experience bigger rises in cost of living. This mechanism should also apply to local goods and services with sufficiently high demand responses to income, and could arise even within counties if shopping locations are strongly segregated by the income of consumers. While there are no good data on the demographic characteristics of customers in each store, I provide suggestive evidence that income segregation does magnify this mechanism in Appendix 1.12.8.

Fifth, there has been a global movement to increase the minimum wage to unprecedented levels in both Europe and the US. These results imply that the pass-through elasticity will become correspondingly larger and each minimum wage hike will have increasing effects on inflation. For example, if the national minimum wage in the US increases from $7.25 to $15, the national Kaitz index would be raised from around 0.3 to 0.6, assuming the average wage only increases slightly. Extrapolating out of sample with the triple difference results above, this implies that the pass-through elasticity would increase from 0.06 to roughly 0.19. The effect of each minimum wage hike is progressively stronger due to the non-linearity in the price response. A further 10% increase in minimum wage would raise grocery store prices by 1.9%

1.9 Conclusion

In this paper, I find evidence that the minimum wage increases prices in grocery stores but not in other store types because of rigid pricing within retail chains. A 10% minimum wage hike raises grocery store prices by about 0.58%. This finding holds across different phases of the business cycle and passes a variety of robustness checks. Furthermore, the pass-through elasticity is stronger in regions where the minimum wage is more binding. I present evidence that the minimum wage increases earnings of grocery store workers, but based on cost pass-through theory, this labor *cost* increase is not large enough to fully explain the rise in prices. I propose that *demand*-induced feedback leads to a larger pass-through elasticity by increasing income, lowering demand elasticities, and increasing retail markups. I support this claim with four pieces of evidence. First, I derive pass-through formulas for calibrations to show that demand effects can account for size of the reduced-form estimate. Second, I find suggestive evidence that poorer households lower shopping intensities when the minimum wage rises, consistent with lower price sensitivities. Third, I provide evidence that multi-product retailers raise prices for more demand inelastic product groups, consistent with retail markups. Fourth, I find that merchandise stores, which do not raise their prices in response to local minimum wage shocks due to within-chain price rigidity, exhibit large nominal and real sales responses to minimum wage increases.

Demand-induced feedback would also create significant dispersion in pass-through elasticity between rich and poor regions, which has important implications for real wage inequality, residential segregation, and future minimum wage increases. My results imply that due to regional heterogeneity in earnings, the reduction in real wage inequality caused by minimum wage hikes is smaller than the reduction in nominal wage inequality. Regional heterogeneity in earnings, combined with residential segregation by different income groups, could lead to income segregation in shopping locations. Thus, increasing residential segregation may lead to larger dispersion in the minimum wage pass-through elasticity between income groups. By using regions where the minimum wage is more binding, I extrapolate that the minimum wage could have significant inflationary impacts if it continues to rise relative to the average wage. While the effects of local minimum wage hikes on prices and real wage inequality are weakened by within-chain price rigidity, a rise in the federal minimum wage could have much larger effects.

As movements to increase the minimum wage to historic levels gain traction around the world, the impact of the minimum wage on the cost of living becomes more crucial. Better data on both prices and quantities of goods and services in other industries and countries are needed to inform the policy debate.

1.10 Tables

Table 1.1: Share of workers paid at or below the minimum wage by industry, top 10

Industry	Proportion of all MW workers hired	Within-industry share of MW workers
Restaurants and other food services	0.296	0.289
Grocery stores	**0.040**	**0.114**
Elementary and secondary schools	0.036	0.032
Other amusement, gambling, and recreation industries	0.027	0.129
Colleges and universities, including junior colleges	0.022	0.049
Department stores and discount stores	**0.022**	**0.073**
Construction	0.020	0.021
Traveler accommodation	0.019	0.108
Private households	0.019	0.194
Child day care services	0.016	0.113

Notes: Pooled data from CPS MORG, 2006-2015. Shares are constructed using CPS sample weights. Almost all workers appear twice by construction of the rotating panel. Industries are classified according to the 2010 Census occupational classification used by the CPS. The second column refers to the share of all MW workers that work in a specific industry, whereas the third column refers to the share of MW workers within the specified industry.

Table 1.2: Minimum wage impact on labor markets by industry

	(1)	(2)	(3)	(4)	(5)
Industry	Drug	Grocery	Department	Merchandise	Restaurant
Earnings	0.0638	0.146***	0.0323	0.157***	0.252***
	(0.0390)	(0.0420)	(0.0466)	(0.0568)	(0.0341)
	115,663	122,081	66,330	115,499	122,698
Employment	0.0880	0.00491	-0.135	-0.245	-0.0755
	(0.0604)	(0.0955)	(0.178)	(0.184)	(0.0551)
	87,483	101,726	35,355	93,439	118,609
County FE	X	X	X	X	X
Year-Quarter FE	X	X	X	X	X

Notes: Data from the QWI, 2006-2015. Coefficients are obtained from 10 separate regressions of the outcomes on the minimum wage under different specifications. Robust standard errors are in parentheses, clustered by state. Number of observations are given below the standard errors. *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Log county population is added as a control variable. Industries include drug stores, grocery stores, department stores, other merchandise stores, and restaurants as classified by 4-digit NAICS (4461, 4451, 4521, 4529, and 7225).

Table 1.3: Effect of minimum wage on prices by store type

	(1)	(2)	(3)	(4)	(5)	(6)
Store Type	Drug		Grocery		Merchandise	
VARIABLES	Log price index					
Log MW	-0.0469	-0.0632	0.0605*	0.0576**	-0.00623	-0.0121
	(0.0555)	(0.0500)	(0.0311)	(0.0255)	(0.0241)	(0.0214)
Log housing price		0.0461**		0.0181		0.0265**
		(0.0194)		(0.0140)		(0.0131)
Log unemployment rate		-0.00828		-0.000742		0.00245
		(0.00858)		(0.00606)		(0.00452)
Log population		0.00427		-0.0776***		-0.0593***
		(0.0315)		(0.0243)		(0.0212)
Log average wage		-0.0111		-0.00676		-0.00342
		(0.00989)		(0.00867)		(0.00742)
Observations	354,064	353,938	287,284	287,122	282,092	282,037
R-squared	0.873	0.875	0.928	0.929	0.895	0.896
Prob > F	0.403	0.000	0.058	0.002	0.797	0.064
Store FE	X	X	X	X	X	X
Year-Quarter FE	X	X	X	X	X	X
Number of units	8853	8852	7183	7180	7054	7054
Number of clusters	48	48	48	48	49	49

Notes: Robust standard errors are in parentheses, clustered by state. *** p<0.01, ** p<0.05, * p<0.1.

Table 1.4: Pass-through elasticity estimates interacted with determinants, grocery stores

	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	Kaitz index	Log AW	Fraction below 25K	MW / median	Log sales	Log est. per cap
Interaction coefficient	0.426***	-0.124***	0.367***	0.438***	-0.0853***	-0.000954
	(0.0617)	(0.0171)	(0.0526)	(0.0579)	(0.00909)	(0.00971)
Observations	287,122	287,122	287,122	287,122	287,122	287,117
R-squared	0.949	0.949	0.949	0.949	0.951	0.948
Prob > F	0.000	0.000	0.000	0.000	0.000	0.000
Number of units	7180	7180	7180	7180	7180	7180
Number of clusters	48	48	48	48	48	48
State-period FE	X	X	X	X	X	X

Notes: Robust standard errors are in parentheses, clustered at the state level. *** p<0.01, ** p<0.05, * p<0.1. The interaction coefficient is obtained from a regression of the log price index on log minimum wage, the variable of interest, and its interaction with the minimum wage, along with control variables as well as store and period fixed effects. Adding state-period fixed effects implies that only within-state-period variation is used, i.e. county variation in the determinants of pass-through. The Kaitz index is defined as the ratio of the minimum wage to the average wage in each county. Sales are constructed from Nielsen retail scanner data. Average wages (AW), the number of establishments per capita (Est. per cap) are obtained at the county level from the QWI and QCEW and matched to store type. Fraction of HHs earning below $25K (Fraction below $25K) and median household income are obtained from the ACS 5-year estimates.

Table 1.5: Summary statistics of rich and poor counties by Kaitz index, 2006 first quarter

Variable	Rich	Poor
	Mean	
Population	204859	55956
Kaitz index	0.32	0.43
Average weekly wage	691.73	521.15
Unemployment rate	5.27	6.01
Grocery stores per 100K population	26.00	24.55
Grocery stores	72.10	16.54
States	49	47
N	1098	1086

Notes: Data from QCEW. A county is defined as rich or poor if it has a Kaitz index below median or above median respectively, relative to all counties. The number of counties are not equal due to ties.

Table 1.6: Effect of minimum wage on prices by store type and Kaitz index

	(1)	(2)	(3)	(4)	(5)	(6)
Store Type	Drug		Grocery		Merchandise	
Counties	Rich	Poor	Rich	Poor	Rich	Poor
VARIABLES	Log price index					
Log MW	-0.0578	-0.101	0.0584**	0.0837***	-0.0123	-0.00306
	(0.0453)	(0.0680)	(0.0270)	(0.0201)	(0.0230)	(0.0198)
Observations	309,002	44,936	246,966	40,156	231,709	50,328
R-squared	0.872	0.894	0.928	0.941	0.894	0.912
Prob > F	0.000	0.038	0.005	0.003	0.119	0.252
Number of units	7730	1124	6177	1004	5799	1261
Number of clusters	48	40	48	41	48	45

Notes: Robust standard errors are in parentheses, clustered at the state level. *** p<0.01, ** p<0.05, * p<0.1. Control variables as well as store and period fixed effects are included. A county is defined as rich or poor if it has a Kaitz index below median or above median respectively, relative to all counties.

Table 1.7: Effect of minimum wage on prices by sample period, grocery stores

	(1)	(2)	(3)	(4)
Sample period	2006-07	2008-09	2010-13	2014-15
VARIABLES	Log price index			
Log MW	0.00697	0.0588***	0.0303	0.0759***
	(0.0111)	(0.0219)	(0.0387)	(0.0230)
Observations	57,390	57,432	114,864	57,436
R-squared	0.862	0.880	0.931	0.963
Prob > F	0.009	0.000	0.073	0.000
Number of units	7179	7179	7179	7180
Number of clusters	48	48	48	48

Notes: Robust standard errors are in parentheses, clustered at the state level. *** p<0.01, ** p<0.05, * p<0.1. Control variables as well as store and period fixed effects are included. All of the federal minimum wage variation is announced in May 2007 and occurs in 2007-2009, along with many state changes during that period. There was very little minimum wage variation from 2010-2013 barring those from states that indexed their minimum wage to the national CPI. A new wave of minimum wage hikes began in 2014-2015 and were all initiated by states.

Table 1.8: Effect of minimum wage on prices for grocery stores, robustness checks

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Specification	Default	Drop outliers	Monthly	No indexing states	Interior counties	Contiguous counties	Substate MW	Store revenue weights	Population weights	County observations	Time trend
VARIABLES	Log price index										
MW level	0.00845**										
	(0.00365)										
Log MW		0.0474**	0.0564**	0.0671**	0.0690**	0.0432***	0.0545**	0.0466*	0.0567**	0.0455*	0.0571***
		(0.0201)	(0.0249)	(0.0299)	(0.0319)	(0.0144)	(0.0241)	(0.0243)	(0.0226)	(0.0237)	(0.0171)
Observations	287,122	258,398	857,886	220,762	188,702	98,420	287,122	287,122	287,122	53,682	287,118
R-squared	0.929	0.939	0.929	0.928	0.927	0.934	0.929	0.928	0.919	0.964	0.994
Prob ¿ F	0.002	0.000	0.005	0.000	0.000	0.000	0.003	0.008	0.018	0.000	0.002
Number of units	7180	6461	7151	5521	4721	2464	7180	7180	7180	1344	7179
Number of clusters	48	48	48	38	44	48	48	48	48	48	48

Notes: Robust standard errors are in parentheses, clustered at the state level. *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Control variables as well as store and period fixed effects are included. MW level refers to using minimum wage levels as the regressor instead of log minimum wage. Dropping outliers refers to dropping stores with average price indices below the 5th percentile or above the 95th percentile. Monthly observations refers to a regression using a monthly price index instead of quarterly. No indexing states drops states that index their minimum wage to the national CPI. Contiguous and interior counties refers to stores in counties that are contiguous to counties in another state across the border and those that are not. Include substate MW matches stores to their local minimum wage (city or county level) if such local ordinances are in place. County observations refers to aggregating the price indices to the county level with store revenue weights and then weighting each county observation by total store revenue in that county. Time trend refers to adding store-specific time trends.

Table 1.9: Effect of minimum wage on prices for grocery stores, alternative price indices

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Method	Alternative weights	Tornqvist	One-stage	Geometric	Fixed	Fixed posted price	County base
VARIABLES	Log price index						
Log MW	0.0576**	0.0526**	0.0566**	0.0576**	0.0595*	0.0567*	0.0506**
	(0.0246)	(0.0228)	(0.0239)	(0.0236)	(0.0317)	(0.0335)	(0.0214)
Observations	295,562	295,562	295,562	295,562	295,562	295,322	55,270
R-squared	0.926	0.920	0.924	0.857	0.874	0.900	0.956
Prob > F	0.015	0.017	0.013	0.020	0.002	0.024	0.002
Number of units	7391	7391	7391	7391	7391	7385	1382
Number of clusters	48	48	48	48	48	48	49

Notes: Robust standard errors are in parentheses, clustered at the state level. *** p<0.01, ** p<0.05, * p<0.1. Control variables as well as store (or county whenever appropriate) and period fixed effects are included. Details on each of the index construction methods used above are shown in Appendix 2.10.4. For the county-level specification, the regression is weighted by county revenue.

Table 1.10: Effect of minimum wage on real and nominal sales by store type and Kaitz index

	(1)	(2)	(3)	(4)	(5)	(6)
Store Type	Drug		Grocery		Merchandise	
Counties	Rich	Poor	Rich	Poor	Rich	Poor
Log real sales	0.0516	0.161	-0.0147	0.0695	0.00473	0.221**
	(0.0707)	(0.134)	(0.0590)	(0.0763)	(0.0425)	(0.0831)
Log sales	-0.00614	0.0591	0.0437	0.153*	-0.00761	0.218**
	(0.0580)	(0.0953)	(0.0551)	(0.0771)	(0.0586)	(0.0946)
Observations	309,002	44,936	246,966	40,156	231,709	50,328
Number of units	7730	1124	6177	1004	5799	1261
Number of clusters	48	40	48	41	48	45

Notes: Coefficients are obtained from 12 separate regressions (by store type and Kaitz index) of the two outcomes, log real sales and log nominal sales, on the minimum wage along with control variables as well as store and period fixed effects. Robust standard errors are in parentheses, clustered at the state level. *** p<0.01, ** p<0.05, * p<0.1. Real sales are defined as nominal sales divided by the store-specific price index. A county is defined as rich or poor if it has a Kaitz index below median or above median respectively, relative to all counties.

Table 1.11: Effect of minimum wage on prices and real sales for grocery stores in poor counties by product department

Product Department	(1) Health & Beauty Care	(2) Food	(3) Non-food Grocery	(4) Alcohol	(5) General merchandise
Log price index	0.0333	0.0575**	0.168***	0.112**	-0.0623
	(0.0277)	(0.0266)	(0.0444)	(0.0529)	(0.0461)
Log real sales	-0.0762	0.111	-0.167	0.350	0.183
	(0.141)	(0.0705)	(0.140)	(0.351)	(0.167)
Observations	40,276	40,236	40,076	39,436	40,236
Number of units	1007	1006	1002	986	1006
Number of clusters	41	41	41	39	41

Notes: Coefficients are obtained from 10 separate regressions of the two outcomes, log price index and log real sales, on the minimum wage along with control variables as well as store and period fixed effects. Robust standard errors are in parentheses, clustered at the state level. *** p<0.01, ** p<0.05, * p<0.1. A county is defined as rich or poor if it has a Kaitz index below median or above median respectively, relative to all counties.

Table 1.12: Effect of chain rigidity on pass-through elasticities

	(1)	(2)	(3)	(4)
Store Type	All		Grocery	
VARIABLES	Log price index			
Log MW	0.0156	-0.0350	0.0828***	0.00318
	(0.0278)	(0.0344)	(0.0259)	(0.0261)
States in chain x Log MW	-0.00140***			
	(0.000443)			
Flexibility measure x Log MW		0.197*		
		(0.110)		
Observations	923,057	889,925	206,644	80,478
R-squared	0.877	0.877	0.931	0.935
Prob > F	0.020	0.065	0.000	0.012
Number of units	23081	22340	5167	2013
Number of clusters	49	49	48	39
Local pricing			X	

Notes: Robust standard errors are in parentheses, clustered at the state level. *** p<0.01, ** p<0.05, * p<0.1. Control variables as well as store and period fixed effects are included. Local pricing refers to stores with flexible pricing or stores in chains located in one or two states.

Table 1.13: Theoretical estimates of minimum wage pass-through elasticity, labor cost effect

Industry	Labor cost share	Weighted share of wages earned by MW affected workers			Spillover adjustment	Pass-through rate	MW pass-through elasticity		
		Poor counties	Rich counties	All			Poor counties	Rich counties	All
Grocery Stores	0.101	0.084	0.073	0.081	None	1	0.0085	0.0074	0.0082
Grocery Stores	0.101	0.162	0.164	0.181	AMS	1	0.0163	0.0165	0.0181
Grocery Stores	0.101	0.210	0.207	0.228	Lee	1	0.0211	0.0208	0.0229
Health Stores	0.120	0.034	0.025	0.028	None	1	0.0041	0.0031	0.0034
Health Stores	0.120	0.101	0.095	0.103	AMS	1	0.0120	0.0114	0.0123
Health Stores	0.120	0.129	0.117	0.128	Lee	1	0.0154	0.0141	0.0153
Department Stores	0.108	0.097	0.069	0.074	None	1	0.0104	0.0074	0.0080
Department Stores	0.108	0.202	0.171	0.188	AMS	1	0.0218	0.0185	0.0202
Department Stores	0.108	0.273	0.222	0.246	Lee	1	0.0294	0.0240	0.0265
Restaurants	0.297	0.181	0.137	0.150	None	1	0.0538	0.0407	0.0447
Restaurants	0.297	0.283	0.253	0.273	AMS	1	0.0841	0.0752	0.0811
Restaurants	0.297	0.340	0.305	0.324	Lee	1	0.1010	0.0905	0.0964

Notes: Pooled data from ACS, 2006-2015. Industries are classified according to the NAICS. Labor cost shares are from 2007 and 2012 SUSB. Shares are constructed using ACS sample person weights. Spillover adjustments are made based on theoretical derivations and using spillover elasticity estimates from Autor, Manning, and Smith (2016) (AMS) and Lee (1999), normalized by the maximum percentile. Pass-through rates are taken from estimates in previous literature. The minimum wage pass-through elasticity is a multiple of the labor cost share, the weighted share of wages earned by MW affected workers, and the pass-through rate as shown by theory.

Table 1.14: Theoretical estimates of minimum wage pass-through elasticity, demand effect

MPC	Income elasticity	Demand	Income elasticity of MW			Pass-through	Demand pass-through elasticity		
	of demand	elasticity	Poor counties	Rich counties	All	rate	Poor counties	Rich counties	All
0.03	0.3	0.709	0.102	0.067	0.0833	0.5	0.0216	0.0142	0.0176
0.065	0.65	0.709	0.102	0.067	0.0833	0.5	0.0468	0.0307	0.0382
0.1	1	0.709	0.102	0.067	0.0833	0.5	0.0719	0.0472	0.0587

Notes: MPC estimates are collected from the previous literature. Income elasticities of demand are calculated from the MPC estimates assuming an expenditure share of 0.1 for food-at-home. Demand elasticity and pass-through rate are estimated in Leung and Seo (2018) and consistent with the previous literature. Income elasticity of minimum wage is calculated from estimates from Dube (2017) and derivations in Section 1.12.5.3.

Table 1.15: Effect of minimum wage on shopping behavior

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
VARIABLES	Log expenditure		Coupon share		Deal share		Store brand share	
Log MW	0.00964	-0.0148	0.00893***	0.00917***	0.00623	0.00850	0.0136**	0.0150**
	(0.0330)	(0.0295)	(0.00236)	(0.00219)	(0.00949)	(0.0109)	(0.00663)	(0.00606)
Log MW x Below median	-0.0212	-0.0181	-0.00705***	-0.00678***	-0.0456***	-0.0440***	-0.00936**	-0.00957**
	(0.0203)	(0.0204)	(0.00161)	(0.00160)	(0.00671)	(0.00659)	(0.00372)	(0.00365)
Observations	1,539,441	1,539,076	1,539,443	1,539,078	1,539,441	1,539,076	1,539,441	1,539,076
R-squared	0.729	0.731	0.767	0.767	0.850	0.850	0.688	0.688
Prob > F	0.571	0.000	0.000	0.000	0.000	0.000	0.008	0.008
Number of units	61437	61431	61437	61431	61437	61431	61437	61431
Number of clusters	49	49	49	49	49	49	49	49
Controls		X		X		X		X

Notes: Robust standard errors are in parentheses, clustered at the state level. *** p<0.01, ** p<0.05, * p<0.1. Observations are weighted by sampling weights. Household and period fixed effects are included. Control variables include log housing price, log county unemployment rate, log county population, log county average wage, and household size fixed effects. Below median refers to an indicator variable for households with a household income below the median in 2006. Coupon share denotes the share of expenditures made using a coupon, deal share denotes the share of expenditures on goods on sale, and store brand share denotes the share of expenditures on goods that are store brand.

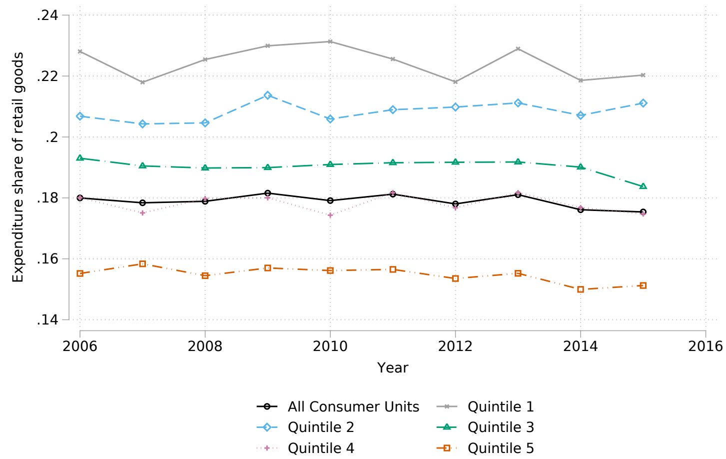
Table 1.16: Determinants of pass-through elasticity

	(1)	(2)	(3)	(4)
Counties	Rich		Poor	
VARIABLES	Pass-through elasticity estimate			
Grocery	0.0948***	0.0765***	0.0615**	0.0728***
	(0.0152)	(0.0102)	(0.0261)	(0.0143)
Merchandise	0.0692***	0.0352***	0.0567***	0.0264**
	(0.0146)	(0.00885)	(0.0218)	(0.0122)
Log demand elasticity	-0.00409	-0.00278	-0.0179**	-0.00673
	(0.00540)	(0.00337)	(0.00769)	(0.00520)
Poor revenue share	0.0251	0.0767	0.0615	0.0101
	(0.140)	(0.0971)	(0.234)	(0.137)
Constant	-0.0571***	-0.0415***	0.00985	-0.00834
	(0.0206)	(0.0110)	(0.0302)	(0.0163)
Observations	283	283	274	274
R-squared	0.157	0.160	0.052	0.089
Prob > F	0.000	0.000	0.003	0.000
OLS Robust	X		X	
WLS		X		X

Notes: Second stage regression where estimated store type by product group pass-through elasticities are regressed on regressors of interest, with drug stores being the omitted group. Demand elasticities by product group are obtained from Broda and Weinstein (2010). Robust standard errors are in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Poor revenue share is the revenue share in 2006 accounted for by households with below median household income in each product group according to the consumer panel. Pass-through elasticities estimated with fewer than 10 states are dropped due to lack of power in the estimate, and results are robust to using other reasonable numbers as thresholds.

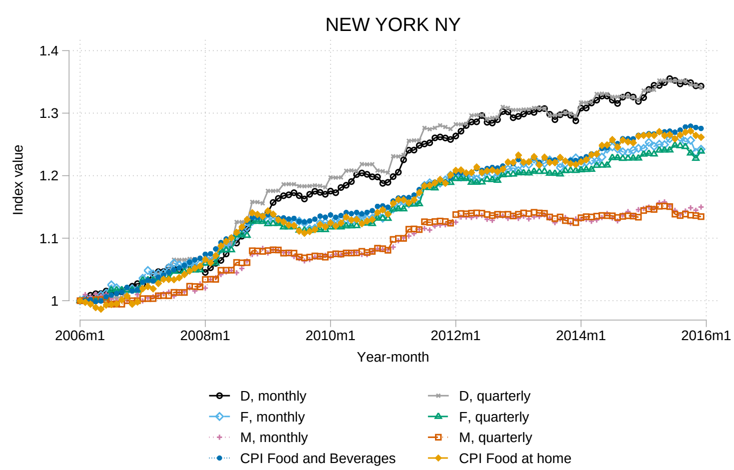
1.11 Figures

Figure 1.1: Expenditure shares on retail goods by income quintile and year



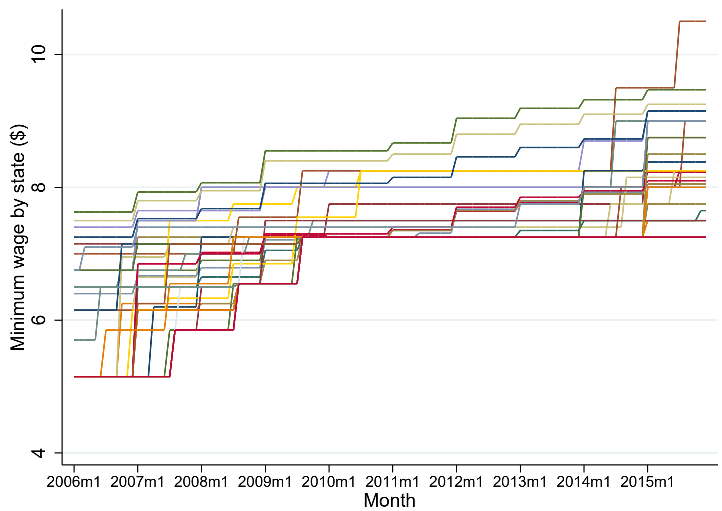
Notes: This figure plots expenditure shares on retail goods by income quintile from 2006-2015, which range from an average of 15.48% for top quintile consumer units to 22.44% for bottom quintile consumer units. Data from the CEX are used and selected categories are all sold by Nielsen retail stores, which include alcoholic beverages, drugs, food at home, toys, audio and visual equipment, medical supplies, household furnishings and equipment, housekeeping supplies, personal care products and services, and tobacco.

Figure 1.2: Comparison of Nielsen price indices with CPI



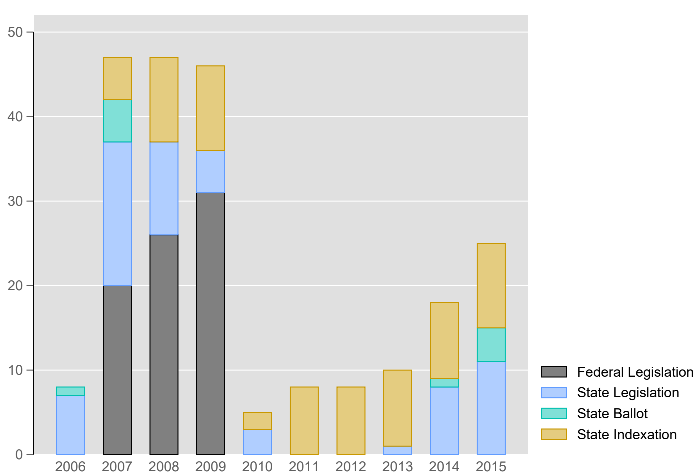
Notes: This figure plots city-level price indices from 2006-2015 constructed using Nielsen retail scanner data against those used by the BLS to construct the CPI. D, F, and M correspond to Nielsen price indices for drug stores, grocery stores, and mass merchandise stores respectively. Nielsen price indices are first constructed at the store level, and aggregated to the city level by taking a sales-weighted average.

Figure 1.3: Minimum wage over time for all states, 2006-2015



Notes: This figure plots the state-effective minimum wage for each state in each month from 2006-2015. States that do not have a state minimum wage are bounded by the federal minimum wage.

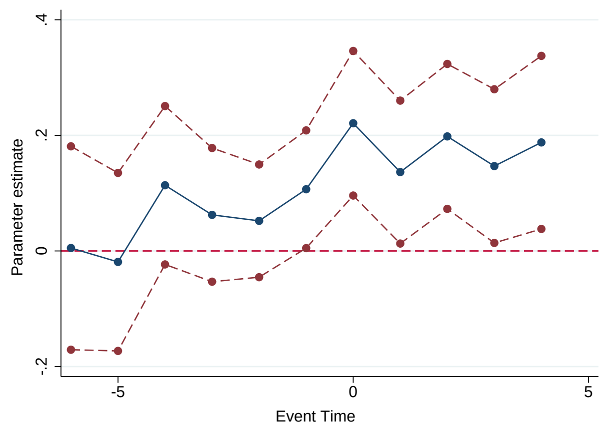
Figure 1.4: Minimum wage changes over time by type for all states



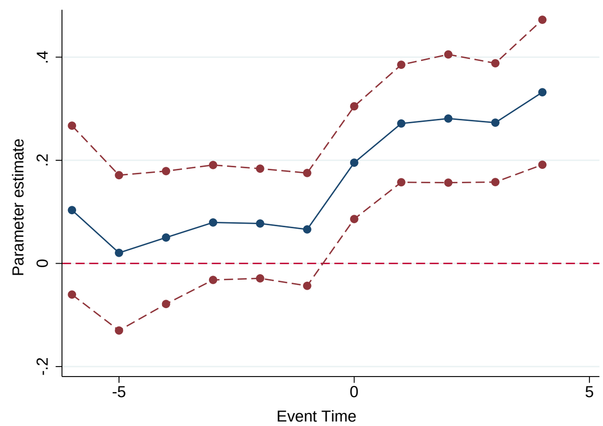
Notes: This figure plots the total number of changes in the minimum wage across states in each year, segmented by the type of change. There are 4 types of minimum wage changes: federal legislation, state legislation, state ballot (where voters decide whether the minimum wage should be increased), and subsequent changes due to indexation to the national CPI (with the exception of Colorado which indexes their minimum wage to the city-level CPI).

Figure 1.5: Cumulative effects of minimum wage on labor markets

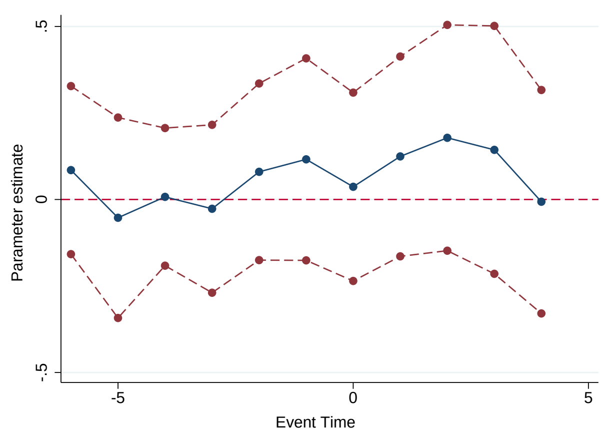
(a) Earnings, grocery stores



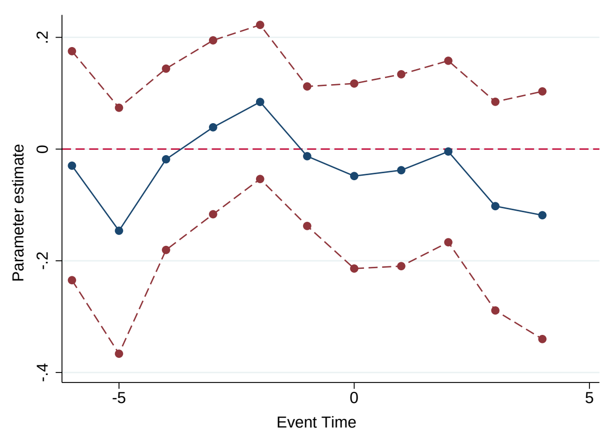
(b) Earnings, restaurants



(c) Employment, grocery stores



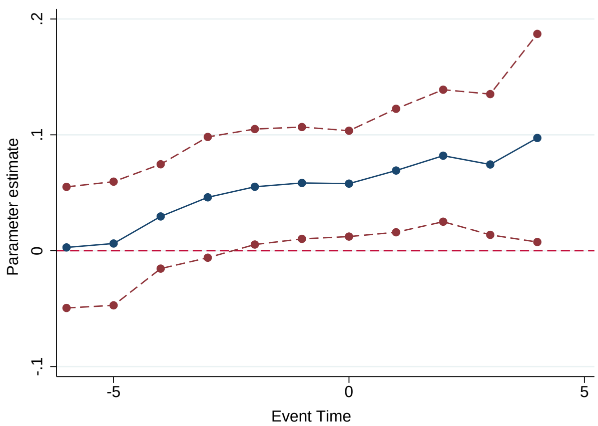
(d) Employment, restaurants



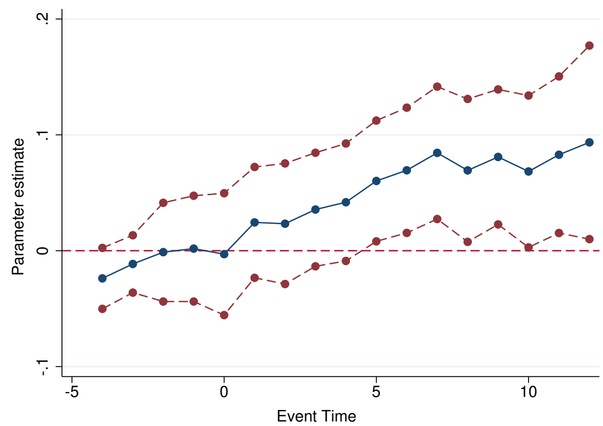
Notes: This figure plots the sum of estimated coefficients for each period, along with the 95% confidence intervals, from regressions using a distributed lag model, where log earnings and log employment are regressed on log minimum wage. County and period fixed effects are included. The event window starts from 6 quarters before a minimum wage change and ends 4 quarters after the change. Regressions are run separately for both grocery stores and restaurants.

Figure 1.6: Cumulative effect of minimum wage on prices for grocery stores

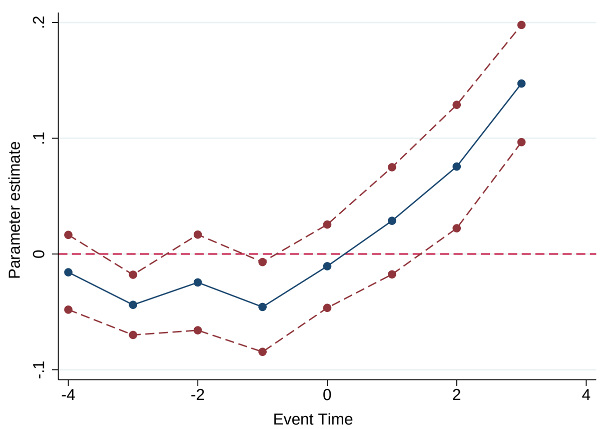
(a) Implemented minimum wage, 2006-2015



(b) Announced minimum wage, 2006-2015



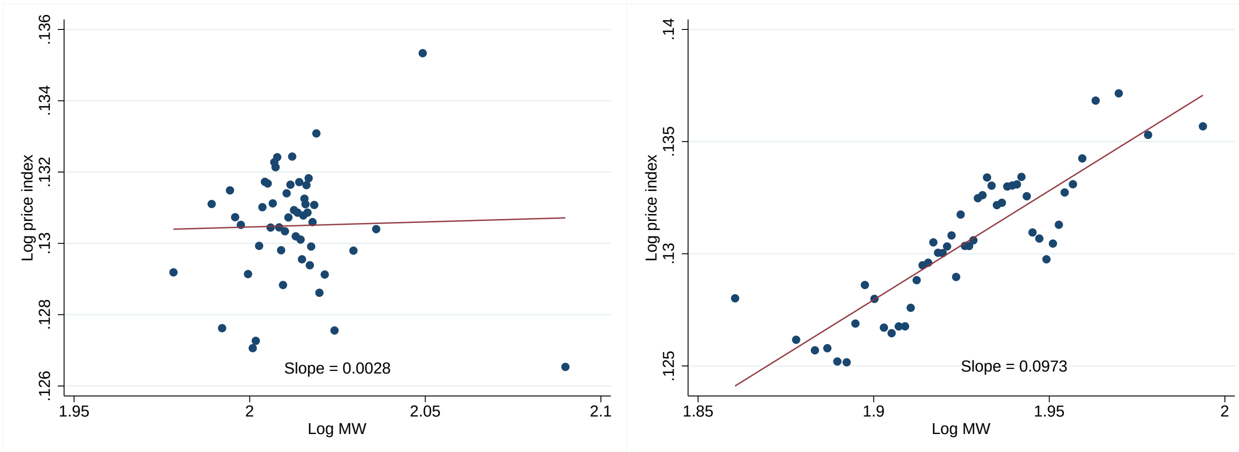
(c) Implemented minimum wage, 2013-2015



Notes: This figure plots the sum of estimated coefficients for each period, along with the 95% confidence intervals, from regressions using a distributed lag model, where log price index is regressed on log minimum wage. Control variables as well as store and period fixed effects are included. In panel (a), the effect of the implemented minimum wage is estimated. The event window starts from 6 quarters before a minimum wage change and ends 4 quarters after the change. Prices rise 3-4 quarters before implementation in the 2006-2015 sample, suggesting announcement effects. Panel (b) confirms this by estimating the effect of the announced minimum wage for states that do not index their minimum wage to the national CPI, with the event window shifted to retain observations. When focusing on 2013-2015 when implementation lags were much shorter, the timing of the effect is sharper. Event windows are chosen to retain 2015q1 minimum wage variation, since the latest minimum wage data are available until 2016q3.

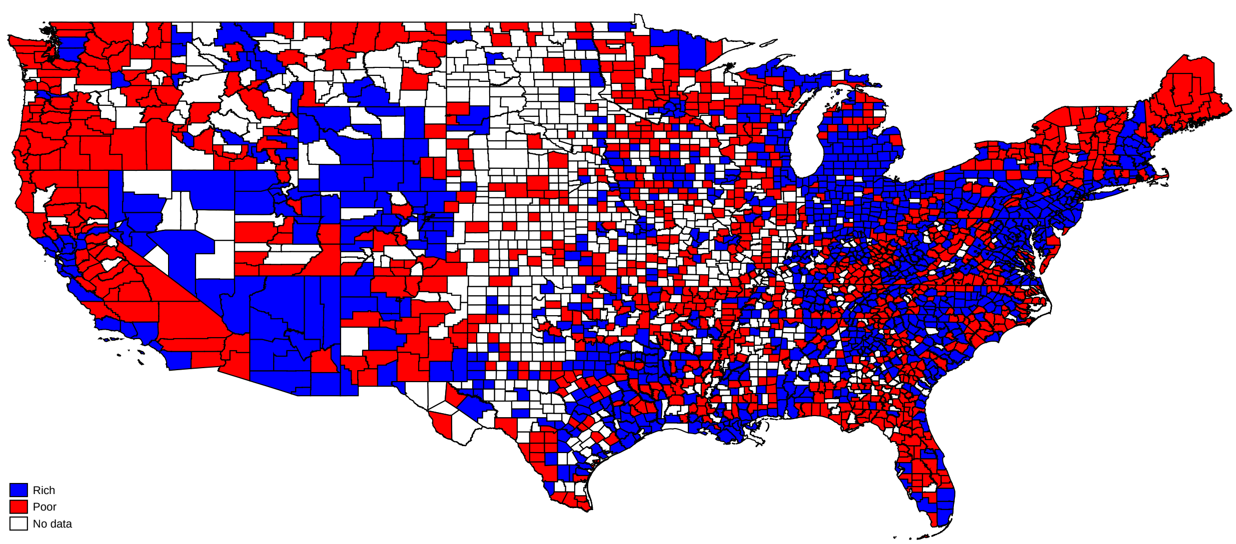
Figure 1.7: Cumulative effect of minimum wage on prices, grocery stores

(a) 6 quarters before minimum wage change (b) 4 quarters after minimum wage change



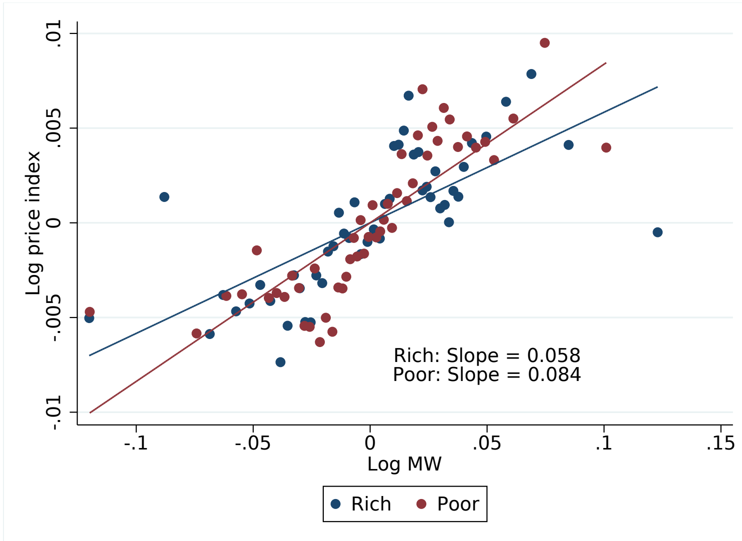
Notes: This figure plots the observations used to estimate the cumulative effect of the minimum wage on prices as shown in Figure 1.6. Two coefficients from the same regression are shown: Panel (a) shows the cumulative effect 6 quarters before a minimum wage change, while Panel (b) shows the cumulative effect 4 quarters after a change. For each store-year-quarter observation, the residualized log minimum wage (6 quarters ahead or 4 quarters after) is calculated and grouped into 50 quantiles. The x-axis displays the mean of the residualized log minimum wage in each quantile. The y-axis shows the mean of the residualized log price index in each quantile. The line of best fit is obtained from the regression using all observations, and its slope is reported on the graph. A county is defined as rich or poor if it has a Kaitz index below median or above median respectively, relative to all counties.

Figure 1.8: Rich and poor counties by Kaitz index, 2006 first quarter



Notes: This figure maps out counties that contain stores present in the Nielsen retail scanner data, denoting them as rich and poor as measured by the Kaitz index in the first quarter of 2006. The Kaitz index is defined as the ratio of the minimum wage to the average wage in each county. A county is defined as rich or poor if it has a Kaitz index below median or above median respectively, relative to all counties.

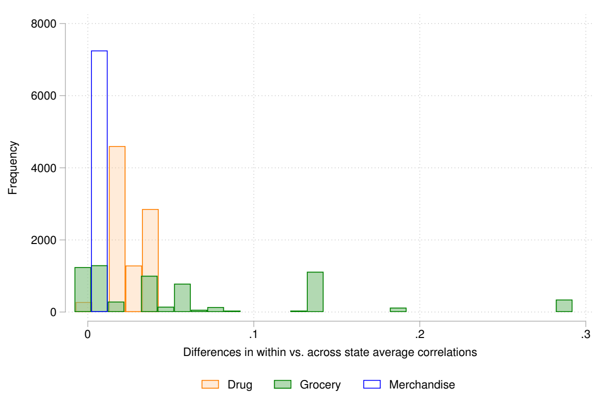
Figure 1.9: Log price index on log minimum wage, grocery stores in rich and poor counties



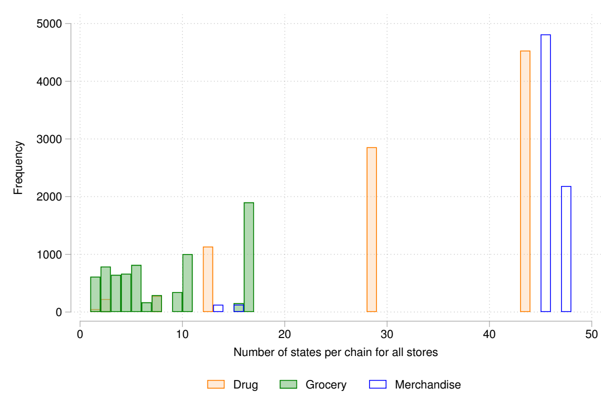
Notes: This figure plots the log price index against the log minimum wage by rich and poor counties as measured by the Kaitz index. Both variables are residualized by regressing on a set of controls, store fixed effects, and period fixed effects. For each store-year-quarter observation, the residualized log minimum wage is calculated and grouped into 50 quantiles. The x-axis displays the mean of the residualized log minimum wage in each quantile. The y-axis shows the mean of the residualized log price index in each quantile. This is done separately for samples containing rich and poor counties, and the line of best fit is obtained from the regression using all observations in each sample, and the slopes are reported on the graph. A county is defined as rich or poor if it has a Kaitz index below median or above median respectively, relative to all counties. The sample is restricted to grocery stores.

Figure 1.10: Distribution of flexibility measures across store types

(a) Flexibility measures



(b) Number of states in each chain for all stores



Notes: This figure plots the chosen flexibility measure, differences in within vs. across state average correlations, as illustrated in Section 1.5.2.2, and also the number of states in each chain for all stores.

1.12 Appendix

1.12.1 Comparisons with Contemporaneous Work

There are several differences between Renkin, Montialoux and Siegenthaler (2017) and my paper. First, the authors use the Symphony IRI scanner dataset which has a smaller amount of stores, with 3,187 grocery stores across 41 states from 2001-2012. However, there are few minimum wage changes between 2001-2005. In contrast, I use the Nielsen Retail Scanner dataset from 2006-2015 which contains 35,000 stores and includes drug, grocery, and mass merchandise stores. The sample period contains a much larger number of minimum wage changes after the recession as shown in Figure 1.4, which could explain why the estimated pass-through elasticity is higher in my paper. As shown in Table 1.7, the estimated pass-through elasticity is slightly larger in the recovery period. Second, they use first differences of the log price index rather than the level as the dependent variable to allow for store-specific time trends. I show in Table 1.8 that my results are robust to adding store-specific time trends. Third, I provide suggestive evidence that a higher pass-through elasticity could be explained by demand effects. Fourth, the availability of other store types in my data enables me to further verify the effect of price rigidity within retail chains on the minimum wage pass-through elasticity.

Another contemporaneous paper is Ganapati and Weaver (2017). In their appendix, they construct price indices in a manner similar to the methodology I use, but with several major alterations and argue that the results in this paper are not robust. First, they aggregate their observations to the county-product-quarter level or county-product-store type-quarter level while I aggregate to the store-product-quarter level. To directly verify their claims, I construct the price index using this alternative method as mentioned in Appendix 2.10.4 and show that the results remain robust to this method as well as a range of alternative price index construction methods in Table 1.9.

In addition, in their preferred specification, instead of constructing a price index which measures the cost of living, Ganapati and Weaver (2017) draw a random 1% sample (or weighted by revenue) of 5,000 goods in dry grocery in grocery stores that appear in more than one state for more than one year, and run regressions with observations at the county-product-quarter level with county-product and product-time fixed effects. They find that the estimated pass-through elasticity is insignificant and point estimates are close to zero. Their approach will likely generate different results from using a price index because in their regression, each county-product-quarter observation selected into the sample is weighted equally, while the price index I construct aggregates products weighted by their quantity sold in the first stage and revenue in the second stage. Their specification estimates the impact of the minimum wage on prices for the average *county-product* in their random or weighted sample while my specification estimates the impact of the minimum wage on the *cost of living* for a consumer buying a consumption bundle sold in an average store. To illustrate that this is the case, I replicate their preferred specification using an unweighted 1% sample of goods in dry grocery in grocery stores. I show in Table 1.28 that the regression coefficients depend strongly on the weighting scheme used. When products are weighted by their total revenue, the point estimate becomes larger but insignificant, and the point estimates become larger and significant when the regression is weighted by county revenue, population, or county-product revenue. In Figure 1.14, I show that these weights are highly unequal and Pareto distributed, which suggests that weighting all counties equally may mask substantial heterogeneity in coefficients across regions. The coefficients are also much larger when only county-product observations that appear in every period are used, since there are a lot of missing values for products that have zero sales in a period. These results are robust across a large number of alternative 1% samples. On the other hand, I show that my regression results are robust to weighting by store revenue, county population, or aggregating to the county level using store-level price indices in Table 1.8.

1.12.2 Construction of Price Indices

Beraja, Hurst and Ospina (2015) adopt a two-stage procedure that is very similar to the one used by the BLS in constructing the CPI, and introduce some improvements enabled by scanner data.[35] A viable alternative would have been to use an exact price index as defined in Diewert (1976) for the CES unit-cost function by applying Sato (1976) and Vartia (1976) weights, which would be theoretically founded. These exact price indices can also account for new product varieties within the CES framework as demonstrated in Feenstra (1994) and implemented in Broda and Weinstein (2010). However, I did not choose this alternative for two reasons. First, I wish to make the indices more comparable to the CPI for easier comparison. Second, theoretically founded indices that account for product turnover require estimation of parameters that are very computationally intensive given the size of the dataset.

In the first stage, the index is constructed at both the monthly and quarterly level for each product group (125 groups) and store. Stores that do not appear throughout the entire sample period are dropped, retaining around 23,500 stores in the sample. Therefore, the results are not affected by store entry and exit. Among the stores that are in the sample in 2006, 84% remain throughout the entire sample period. Although the scanner data are weekly, they are aggregated up to the monthly or quarterly level to decrease missing values and reduce chain drift, as pointed out by Ivancic, Diewert and Fox (2011). Each base observation is a monthly or quarterly unit value for each store-product, i.e. monthly or quarterly revenue divided by the total number of units sold in that period, which is equivalent to a quantity-weighted average price. Products are defined as UPC codes. Alternatively, weekly prices can be sampled from each store-product-period. Only goods that appear consistently across an entire year are included such that around 50% and 70% of all sales are

35. Although alternate price indices released by government agencies do exist, they have limitations that render them less suitable for my analysis, especially due to sampling error. These limitations are outlined in Beraja, Hurst and Ospina (2015). ACCRA price indices used in previous work (Aaronson 2001) that covers a wider range of goods are also problematic as illustrated in Handbury and Weinstein (2015). Therefore, this paper uses price indices constructed from micro data.

used in constructing store-level monthly and quarterly indices respectively.[36] Quantities are directly observed and used as weights, which is a major advantage relative to the CPI, which collects price quotes at the store level but not quantities, so that they use quantities that are lagged 3-4 years and are obtained from the BLS CEX. Quantity weights are updated yearly to avoid chain drift, and the weights (denoted as $q_{i,y}$) are lagged one year to ensure that price changes are not a result of changing consumption patterns in response to current prices or shocks. The CPI weights are updated every two years, which is less frequent than the scanner index. Hence, the CPI is more subject to substitution bias and the basket is less relevant over time. The price index $P^L_{j,t,y}$ at time t and year y for product group j for each store is shown below in equation 2.30:

$$P^L_{j,t,y} = P^L_{j,t-1,y} \times \frac{\sum_{i \in j} p_{i,t} q_{i,y-1}}{\sum_{i \in j} p_{i,t-1} q_{i,y-1}} \tag{1.14}$$

Each unique item is defined by its UPC code. Prices and quantities are observed for each store and UPC pair, which is denoted as product i. By construction, changes in the price index only reflect relative changes in prices for a given bundle and are unaffected by price levels. Therefore, product switching among consumers to more expensive bundles does not change the price index for given price levels.

The second stage is similar to the first stage and aggregates the product group-specific price indices for each store using expenditure shares $s_{j,y-1}$ that are lagged one year and fixed within year. To follow the CPI more closely, a Tornqvist price index can also be constructed using the average expenditure shares between two periods as weights. While I present results using the first method, both methods give almost identical results and are shown in equation 2.31:

36. Prices of goods that did not sell within a given week are not recorded in retail scanner data. Therefore, aggregating up to to the monthly or quarterly level decreases missing values. Furthermore, products that are not bought by consumers are inherently not an important part of the consumer basket.

$$\frac{P_t}{P_{t-1}} = \sum_{j=1}^{N} s_{j,y-1} \left(\frac{P^L_{j,t,y}}{P^L_{j,t-1,y}} \right) \tag{1.15a}$$

$$\frac{P_t}{P_{t-1}} = \prod_{j=1}^{N} \left(\frac{P^L_{j,t,y}}{P^L_{j,t-1,y}} \right)^{\frac{s_{j,t}+s_{j,t-1}}{2}} \tag{1.15b}$$

I also construct a range of price indices using alternative methods. The first index weights each product group using the expenditure shares of only products chosen to construct the price index, i.e. products that satisfy the consistency criterion illustrated above, as opposed to all products in the data. The second index uses the Tornqvist index mentioned above. The third index constructs the price index in one stage instead of two stages. The fourth index uses a weighted geometric average in the first stage similar to SV instead of a weighted arithmetic average. The fifth index uses weights that are fixed over time at the base period to ensure that the results are not driven by shifts in the consumption bundle over time as opposed to actual price changes. To construct such an index, only products that appeared consistently over the entire sample period can be used. The sixth index again uses fixed weights but also base observations that are sampled from the last observable posted price for each store-product-quarter.[37] The seventh index uses base observations that are first aggregated to the county-store type level. All indices are highly correlated and results are robust to using any of the above methods. I show these indices for New York City in Figure 2.17, which are nearly identical across construction methods.

37. In the raw data, the posted price is actually a weekly unit value for Saturday-ending weeks. DVG highlight that this creates a slight aggregation bias but the bias is relatively small for state-level shocks in their calibrations.

1.12.3 Data Details

1.12.3.1 Nielsen Consumer Panel

For each period, I calculate the total expenditures of each household. The advantage of using this measure as opposed to the nominal sales for each store is that demographic information for the household can be observed instead of county-level demographics for the store. Several measures of shopping intensities among households can be constructed following SV. For each good purchased, the household records whether the good is purchased with coupons and if it is on sale. The barcode of the good is also scanned so the brand of the good can be observed. Therefore, I use three measures of shopping intensity: (1) the share of expenditures using coupons (coupon share), (2) the share of expenditures on goods that are on sale (deal share), and (3) the share of expenditures on generic store brands (store brand share).[38]

1.12.3.2 Labor Market Data

The Quarterly Workforce Indicators (QWI) data are the public use aggregation of the Longitudinal Employer-Household Dynamics (LEHD) linked employer-employee microdata, which are collected via a unique federal-state data sharing collaboration and cover 95% of US private sector jobs.[39] The first two outcomes of interest are (1) Earnings: Average monthly earnings of employees who worked on the first day of the reference quarter and (2) Employment: The number of jobs on the last day of the reference quarter. These two variables are consistent with the more commonly used Quarterly Census of Employment and Wages (QCEW) data, which I also use for county-level characteristics such as the number of

38. Results are robust using alternative measures that control for changes in the shopping bundle across product groups, e.g. by demeaning each household-product group-period observation by the mean within each product group-period observation across households, then aggregating over product groups to the household-period level with household-product group-period consumption weights.

39. Information on access to the data is available at http://lehd.ces.census.gov/data/.

establishments.[40] The next three variables are novel worker flow variables, which include (3) Hires: The number of workers who started a new job in the reference quarter, (4) Separations: The number of workers whose job with a given employer ended in the reference quarter, and (5) Turnover: The number of hires and separations as a share of total employment, which is defined as hires plus separations divided by two times employment.

1.12.3.3 Control Variables

Data on housing prices are obtained from the Federal Housing Finance Agency, which produces housing price indices at both the 3-digit zip code level as well as at the state level from 2006-2015. County housing price indices from 2006-2014 are also obtained from CoreLogic through the Fama-Miller Center at the University of Chicago Booth School of Business. Results are presented using 3-digit zip code level housing prices since they are available for the entire sample period, while results are very similar using county level housing prices whenever available. Labor force variables are obtained from the BLS and the Census.

1.12.3.4 Pricing Flexibility Measures

I follow DVG to measure the extent of price rigidity for each of the retail chains in the data. First, I pick the top UPC from each of 12 product modules with high revenue: canned soup, cat food, chocolate, coffee, cookies, carbonated soft drinks, yogurt, orange juice, bleach, toilet tissue, paper towel, and tooth cleaners. Next, I calculate the weekly correlation in prices for each store pair as a similarity measure, first demeaning the price at the store-quarter-product level before calculating the correlations over all weeks and products which are not missing both store pairs. For each chain, I define the flexibility measure as the percentage difference between the average correlation for store pairs within the same state and the average correlation for store pairs across different states. A chain is pricing more rigidly if the flexibility measure is closer to zero. Since there are multiple product modules,

40. Information on access to the data is available at http://www.bls.gov/cew/data.htm.

I take either the mean or median flexibility measure across product modules for each chain. DVG perform the same exercise using an alternative similarity measure.

1.12.4 City and County Level Minimum Wage Variation

Some cities as well as counties began to implement local minimum wage ordinances in 2013-2014. Very few studies have exploited this new wave of minimum wage changes. An exception is Allegretto and Reich (2015), who study the response of restaurant prices to a minimum wage hike in San Jose in 2013 by using neighboring restaurants outside the city boundaries as a control group. I exploit the wide geographic coverage of retail stores in my data by using minimum wage variation from 24 cities or counties, which are denoted as substates. Appendix Figure 1.15 shows the substate minimum wage variation for California and New Mexico, the two states with the most local minimum wage ordinances. In California, San Francisco has continually adopted a higher minimum wage than the state, while San Jose was second in the state to adopt a local minimum wage ordinance, raising minimum wages from \$8 to \$10 in March 2013. 12 other cities soon followed with minimum wage hikes in 2014. I choose the sample to include all stores in substates with their own local minimum wage ordinances, which restricts the sample to 6 states. Results are shown in Appendix Table 1.29 for all store types using 2 different specifications. In the first specification, store and period fixed effects are included. In the second specification, I add state-period fixed effects to control for state-specific time trends and use only within-state minimum wage variation across cities. The sample is further restricted to 17 substates in California and New Mexico. While pass-through elasticity estimates for drug and merchandise stores are shown to be insignificant using state level variation, the point estimates are large and statistically significant when estimated using substate variation. Adding state-period fixed effects attenuates these results, possibly due to the lack of sample size, but pass-through elasticity estimates for grocery stores actually become significant. The point estimate also increases to 0.047 and is only slightly smaller than the estimate of 0.058 using state level variation.

A major concern when using substate minimum wage variation is that there are not enough substates for valid inference. In my results, standard errors are clustered at the level of variation, i.e. by substate. Since the number of clusters is rather small, I also use wild bootstrap for inference. The results for grocery stores using only state-period fixed effects is significant only at the 15% level. Therefore, I caution against over-interpreting these results and emphasize the robustness in the point estimate for grocery stores between using substate and state variation. Until a sufficient amount of substates implement their own local minimum wage ordinances, it remains difficult to draw valid inference from a research design using only substate variation.

1.12.5 Derivations

1.12.5.1 Cost Pass-Through Elasticity Without Spillovers

Denote w_1 and L_1 as the minimum wage and the number of minimum wage workers respectively.

Shepherd's Lemma: $\frac{\partial C}{\partial w_1} = L_1$

$$\frac{dp}{dw_1} = \frac{dp}{dC}\frac{\partial C}{\partial w_1} = \frac{dp}{dC}L_1$$

$$\begin{aligned} \frac{dp}{dw_1}\frac{w_1}{p} &= \frac{dp}{dC}\frac{C}{p}\frac{p}{C}L_1\frac{w_1}{p} \\ &= \frac{dp}{dC}\frac{C}{p}\frac{w_1 L_1}{C} \end{aligned} \tag{1.16}$$

Unit tax: $\frac{\partial C}{\partial t} = Y$

$$\frac{dp}{dt} = \frac{dp}{dC}\frac{\partial C}{\partial t} = \frac{\partial p}{\partial C}Y$$

$$\begin{aligned} \frac{dp}{dC}\frac{C}{p} &= \frac{dp}{dt}\frac{t}{p}\frac{p}{t}\frac{1}{Y}\frac{C}{p} \\ &= \frac{dp}{dt}\frac{t}{p}\frac{C}{tY} \end{aligned} \tag{1.17}$$

Equations 1.16 and 1.17 $\Rightarrow$

$$\begin{aligned} \frac{d\ln p}{d\ln w_1}\frac{C}{w_1 L_1} &= \frac{d\ln p}{d\ln t}\frac{C}{tY} \\ \frac{d\ln p}{d\ln w_1} &= \frac{dp}{dt}\frac{w_1 L_1}{pY} \\ &= \frac{dp}{dt}s_{L_1} \end{aligned}$$

1.12.5.2 Cost Pass-Through Elasticity with Spillovers

There are n types of workers, and for each type there are L_i workers. Let $i = 1$ denote the minimum wage workers. Spillover effects imply that $\Rightarrow \frac{\partial w_i}{\partial w_1} > 0$ for $i > 1$, and assume the minimum wage w_1 exogenously affects wages of other workers, such that the cost function can be written as:

$$C = \sum_{i=1}^{n} w_i(w_1) L_i + rK$$

By Shepherd's Lemma,

$$\frac{\partial C}{\partial w_1} = \sum_{i=1}^{n} L_i \frac{\partial w_i}{\partial w_1}$$

Using the unit-tax pass-through formula,

$$\begin{aligned}
\frac{d\ln p}{d\ln w_1} &= \frac{dp}{dt}\frac{w_1}{pY}\frac{\partial C}{\partial w_1} \\
&= \frac{dp}{dt}\sum_{i=1}^{n}\frac{w_i L_i}{pY}\varepsilon_{w_i,w_1} \\
&= \frac{dp}{dt}\frac{\sum_{i=1}^{n} w_i L_i}{pY}\sum_{i=1}^{n}\frac{w_i L_i}{\sum_{i=1}^{n} w_i L_i}\varepsilon_{w_i,w_1} \\
&= \frac{dp}{dt}\frac{\sum_{i=1}^{n} w_i L_i}{pY}\sum_{i=1}^{n} s_i\varepsilon_{w_i,w_1}
\end{aligned}$$

where $\varepsilon_{w_i,w_1} = \frac{\partial w_i}{\partial w_1}\frac{w_1}{w_i}$ and $s_i = \frac{w_i L_i}{\sum_{i=1}^{n} w_i L_i}$

1.12.5.3 Earnings Elasticity

There are n types of workers, and for each type there are l_i workers. Each of them earn an hourly wage rate of w_i and work h_i hours. Let $i = 1$ denote the minimum wage workers. To simplify the analysis, I assume no employment effects, i.e. $\frac{\partial l_i}{\partial w_1} = 0$. This is supported by the empirical evidence in Section 2.4. Furthermore, spillover effects imply that $\Rightarrow \frac{\partial w_i}{\partial w_1} > 0$ for $i > 1$, and I assume $h_i = h_i(w_i), w_i = w_i(w_1)$.

The average earnings $\overline{wh}$ is given by

$$\overline{wh} = \frac{\sum_i l_i w_i h_i}{\sum_i l_i}$$

The earnings elasticity can then be derived as

$$
\begin{aligned}
\varepsilon_{\overline{wh},w_1} &= \frac{\partial \overline{wh}}{\partial w_1}\frac{w_1}{\overline{\overline{wh}}} = \sum_i \underbrace{\frac{l_i w_i h_i}{\sum_i l_i w_i h_i}}_{s_i}\left(\frac{\partial w_i}{\partial w_1}\frac{w_1}{w_i} + \frac{\partial h_i}{\partial w_1}\frac{w_1}{h_i}\right) \\
&= \sum_i s_i \left(\varepsilon_{w_i,w_1} + \varepsilon_{h_i,w_1}\right) \\
&= \sum_i s_i \varepsilon_{w_i,w_1}\left(1 + \varepsilon_{h_i,w_i}\right)
\end{aligned}
$$

Note that s_i denotes the share of earnings earned by minimum wage workers over the entire wage bill. Without spillover effects, the earnings elasticity is simply given by

$$
\varepsilon_{\overline{w},w_1} = s_1\left(1 + \varepsilon_{h_1,w_1}\right)
$$

These formulas imply that a large hours elasticity and a large spillover effect raises the earnings elasticity.

1.12.5.4 Demand Pass-Through Elasticity

Let market demand $Q^D(p, I)$ depend on price p and income I, and now let us assume instead that the minimum wage w_1 has an effect on demand only through income. Under perfect competition, I can obtain the demand pass-through elasticity by differentiating the equilibrium condition with respect to benefits:

$$
\begin{aligned}
Q^D(p,I) &= Q^S(p) \\
\frac{\partial Q^D(p,I)}{\partial p}\frac{dp}{dw_1} + \frac{\partial Q^D(p,I)}{\partial I}\frac{dI}{dw_1} &= \frac{\partial Q^S(p)}{\partial p}\frac{dp}{dw_1} \\
\rho \equiv \frac{dp}{dw_1} &= \frac{\frac{\partial Q^D(p,I)}{\partial I}\frac{dI}{dw_1}}{\frac{\partial Q^S(p)}{\partial p} - \frac{\partial Q^D(p,I)}{\partial p}} \\
\varepsilon_\rho \equiv \frac{d\ln p}{d\ln w_1} &= \frac{\frac{\varepsilon_{Q^D,I}}{-\varepsilon_D}\varepsilon_{I,w_1}}{1-\frac{\varepsilon_S}{\varepsilon_D}}. \qquad (1.18)
\end{aligned}
$$

Likewise, I can derive the demand pass-through formula under symmetric imperfect competition. First, I start from the profit-maximization condition and differentiate it with respect to income. In addition, I allow the demand elasticity to depend on income. I obtain an expression for the quantity response to income:

$$
\begin{aligned}
P(Q,I) + \theta\frac{\partial P(Q,I)}{\partial Q}Q - c'(Q) &= 0 \\
MR(Q,I) - MC(Q) &= 0 \\
\left(\frac{\partial MR}{\partial Q} - \frac{\partial MC}{\partial Q}\right)\frac{dQ}{dI} + \frac{\partial MR}{\partial I} &= 0 \\
\frac{dQ}{dI} &= -\frac{\frac{\partial MR}{\partial I}}{\frac{\partial MR}{\partial Q} - \frac{\partial MC}{\partial Q}} \\
\frac{dQ}{dI} &= -\frac{\theta\frac{\partial^2 p}{\partial I\partial Q}Q + \frac{\partial p}{\partial I}}{\frac{\partial MR}{\partial Q} - \frac{\partial MC}{\partial Q}}. \qquad (1.19)
\end{aligned}
$$

Next, I use to above expression to obtain the pass-through formula:

$$
\begin{aligned}
\frac{dp}{dw_1} &= \frac{\partial p}{\partial Q}\frac{dQ}{dI}\frac{dI}{dw_1} + \frac{\partial p}{\partial I}\frac{dI}{dw_1} \\
&= \left(1 - \frac{\frac{\partial p}{\partial Q}}{\frac{\partial MR}{\partial Q} - \frac{\partial MC}{\partial Q}}\right)\frac{\partial p}{\partial I}\frac{dI}{dw_1} + \left(- \frac{\frac{\partial p}{\partial Q}}{\frac{\partial MR}{\partial Q} - \frac{\partial MC}{\partial Q}}\theta\frac{\partial^2 p}{\partial b \partial Q}Q\frac{dI}{dw_1}\right) \\
\varepsilon_\rho \equiv \frac{d\ln p}{d\ln w_1} &= \left(1 - \frac{\frac{\partial p}{\partial Q}}{\frac{\partial MR}{\partial Q} - \frac{\partial MC}{\partial Q}}\right)\frac{\varepsilon_{Q^D,I}}{-\varepsilon_D}\varepsilon_{I,w_1} + \frac{\frac{\partial p}{\partial Q}}{\frac{\partial MR}{\partial Q} - \frac{\partial MC}{\partial Q}}\frac{\theta}{-\varepsilon_D}\varepsilon_{p',I}\varepsilon_{I,w_1} \\
&= \left(1 - \frac{dp}{dt}\right)\frac{\varepsilon_{Q^D,I}}{-\varepsilon_D}\varepsilon_{I,w_1} + \left(\frac{dp}{dt}\right)\frac{\theta}{-\varepsilon_D}\varepsilon_{p',I}\varepsilon_{I,w_1}. \qquad (1.20)
\end{aligned}
$$

1.12.6 Theory Details

Future work could consider several extensions of the cost pass-through elasticity formulas. First, it is not clear how the labor market structure would impact the pass-through elasticity. For example, Aaronson and French (2007) point out that the pass-through elasticity is negative under labor market monopsony when the minimum wage lies within a certain range, but could be positive under monopsony outside that range. The pass-through elasticity formula derived only requires Shepherd's Lemma to hold. Second, the fact that sellers are multi-product retailers may imply that pass-through elasticities between products are interdependent, such that cross-price elasticities might be determinants of the pass-through elasticity.

One interesting point to note is that there could be regional heterogeneity in the pass-through rate. To my knowledge, there does not seem to be any empirical evidence documenting this. However, theory suggests that the pass-through rate should be lower in poor regions such that heterogeneity in pass-through rate should not be the explanation for my findings. First, demand elasticities are generally larger in poor regions, which lowers the pass-through rate. Second, market conduct is usually lower in poor regions, which again lowers the pass-through rate. This holds unless the demand curve is very convex. Third, there is no clear prior on how the curvature of demand or supply elasticity should vary across regions.

For the calibration of the cost pass-through elasticity, the labor cost share is obtained from averaging the ratio of total payroll to total receipts in 2007 and 2012 from the Statistics of US Businesses (SUSB). This payroll ratio is not available annually because it is obtained from the economic census, which is conducted every 5 years. The payroll ratio is very stable across the 2 periods. The minimum wage share of labor costs is obtained by taking the ratio of total wages earned by workers earning at or below the minimum wage to total wages earned by all workers in a given year for the chosen industry.

To obtain the spillover elasticity, I normalize the maximum elasticity among all percentiles to 1, and scale the other estimates proportionally. This implicitly assumes that the minimum wage binds at the percentile at which the spillover elasticity is highest, and implies that the relative rate of attenuation in spillover effects as one moves up the wage distribution is used, rather than the absolute magnitudes. If the absolute magnitudes are used without scaling, the shares obtained are even smaller than those obtained without assuming spillover effects. By scaling them up, I obtain an upper bound for what the pass-through elasticity would be if a minimum wage hike is a pure cost shock with spillovers.

To calculate the national wage percentiles, I had to drop observations earning far below the minimum wage due to measurement error. However, I retain these observations in calculating the shares. This implies that the estimate I obtain is likely an upper bound.

1.12.7 Results by Product Characteristics

To test the hypothesis of whether products more likely to be consumed by the poor have higher pass-through elasticities, I first show using the consumer panel that expenditure shares by households with different household income levels vary across certain product characteristics. I select the sample to include only households in the pre-period of 2006. Similar patterns hold for later years. I divide households in the data into four household income quartiles after controlling for household size. Results are similar without controlling for household size. Next, for each product group, I calculate the total expenditure recorded by households on organic and non-organic food products as defined by the USDA, as well as store brand and name brand. I do the same for each household income quartile and divide it by the total to obtain expenditure shares by household income quartile within each product group. In Appendix Table 1.30, I show the average expenditure shares across product groups. Richer households account for a much larger share of expenditures in organic food products than the poor, while poorer households account for a slightly larger share of expenditures in store brand products.

Next, I construct store-level price indices using the same method described in Section 2.10.4, but further divide the sample of products according to the two product characteristics. I then estimate the pass-through elasticity by running a separate regression for each of these characteristics as shown in Appendix Table 1.31. The estimated pass-through elasticity and real sales response is positively significant for non-organic food and similar to previous estimates for food. This is expected since non-organic food accounts for about 99% of food sales in the data. The estimates for organic food are statistically insignificant, consistent with the idea that prices and real sales of products consumed mostly by the rich are unaffected by minimum wage shocks. On the other hand, the estimated pass-through elasticity is slightly smaller for store brand products even though these products are more likely to be consumed by the poor. This may be caused by a higher demand elasticity among those products, offsetting the impact of a likely larger demand response in those products. This may also be driven by consumers switching from store brands to name brands as shown in Section 1.7.

1.12.8 Income Segregation

As mentioned above, income segregation creates dispersion in pass-through elasticity since poor consumers who experience an increase in income from minimum wage hikes tend to go to the same stores while the rich consumers who are unaffected go to other stores. To investigate this claim, I obtain county-level measures of income segregation made available by

Chetty and Hendren (2016), who construct Reardon's rank-order index of income segregation (Reardon 2011) at the county level using census-tract level data. To understand the impact of income segregation across counties, consider a poor county and a rich county. Assume that residential income segregation leads to consumption segregation by income at stores. A poor county that is more segregated is more likely to contain stores with higher pass-through elasticities on average while a rich county that is more segregated is likely to contain stores with lower pass-through elasticities on average. Therefore, the interaction between the log minimum wage, the Kaitz index, and the income segregation measure should be positive. Results are shown in Appendix Table 1.32. The interaction coefficient is positive and statistically significant as expected, providing support for the hypothesis that income segregation generates dispersion in pass-through elasticity.

1.12.9 Tables

Table 1.17: Type and source of minimum wage changes, 2006-2015

	Source		
Type	State	Federal	Total
Ballot	11	0	11
Indexation	71	0	71
Legislation	63	77	140
Total	145	77	222

Notes: Data collected manually from online sources such as federal and state government websites, news articles, and the National Conference of State Legislatures etc. All sources are documented and available upon request.

Table 1.18: Summary statistics for minimum wage changes, 2006-2015

Variable	Mean	Std. Dev.	Median	Min	Max
2006-2010					
MW change (dollars)	0.58	0.33	0.70	-0.03	1.80
MW change (%)	0.10	0.06	0.11	0.00	0.35
Implementation lag (quarters)	4.13	3.76	4.69	0.00	18.57
2011-2013					
MW change (dollars)	0.19	0.10	0.15	0.06	0.37
MW change (%)	0.02	0.01	0.02	0.01	0.05
Implementation lag (quarters)	0.08	0.42	0.00	0.00	2.14
2014-2015					
MW change (dollars)	0.48	0.37	0.42	0.08	1.25
MW change (%)	0.06	0.05	0.05	0.01	0.17
Implementation lag (quarters)	1.80	1.63	1.02	0.24	7.05

Notes: Data collected manually from online sources such as federal and state government websites, news articles, and the National Conference of State Legislatures etc. All sources are documented and available upon request. Implementation lags refers to the number of quarters it took for the minimum wage to be implemented after the announcement of the legislation.

Table 1.19: Minimum wage impact on labor markets by industry, pooled case studies

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Industry	Drug		Grocery		Department		Merchandise		Restaurant	
Earnings	0.00233	-0.0288	0.139***	0.0177	0.0344	-0.0896	0.208**	0.0554	0.280***	0.225***
	(0.0785)	(0.0777)	(0.0449)	(0.0373)	(0.0547)	(0.0738)	(0.0797)	(0.0664)	(0.0292)	(0.0285)
	88,240	83,092	92,362	90,772	50,067	30,984	87,667	82,570	92,302	90,690
Employment	0.0474	-0.150**	-0.0908	-0.0620	-0.143	0.120	-0.130	0.115	-0.133**	-0.0201
	(0.0652)	(0.0626)	(0.0812)	(0.0751)	(0.262)	(0.202)	(0.210)	(0.209)	(0.0627)	(0.0741)
	65,615	50,958	78,025	67,780	27,314	12,430	70,611	57,434	89,881	86,386
Hires	0.215**	0.0602	-0.379**	-0.224**	-0.123	0.1000	-0.277	0.173	-0.370***	-0.199**
	(0.0994)	(0.146)	(0.147)	(0.0926)	(0.268)	(0.217)	(0.180)	(0.197)	(0.0974)	(0.0988)
	58,943	42,966	77,462	66,918	27,361	12,458	69,744	56,396	89,592	85,738
Separations	0.207*	-0.0185	-0.289**	-0.194*	-0.146	0.0174	-0.224	0.166	-0.393***	-0.198**
	(0.112)	(0.142)	(0.135)	(0.101)	(0.271)	(0.205)	(0.180)	(0.180)	(0.0927)	(0.0916)
	58,586	42,530	77,278	66,660	27,312	12,428	69,593	56,164	89,358	85,398
Turnover	0.144*	0.157	-0.253	-0.144	0.00593	-0.0695	-0.124	0.0107	-0.251**	-0.167**
	(0.0803)	(0.123)	(0.167)	(0.0897)	(0.127)	(0.116)	(0.159)	(0.155)	(0.0958)	(0.0821)
	56,278	39,968	76,896	66,122	27,242	12,364	68,998	55,460	89,004	84,740
County FE	X	X	X	X	X	X	X	X	X	X
Year-Quarter FE	X	X	X	X	X	X	X	X	X	X
County pair x period FE		X		X		X		X		X

Notes: Data from the QWI, 2006-2015. Coefficients are obtained from 50 separate regressions of the outcomes on the minimum wage under different specifications. County pair-period FE refers to contiguous county pair-period fixed effects. Robust standard errors are in parentheses, clustered by state. Number of observations are given below the standard errors. *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Log county population is added as a control variable. Industries include drug stores, grocery stores, department stores, other merchandise stores, and restaurants as classified by 4-digit NAICS (4461, 4451, 4521, 4529, and 7225).

Table 1.20: Minimum wage impact on labor markets by industry and Kaitz index

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Industry	Drug		Grocery		Department		Merchandise		Restaurant	
Counties	Rich	Poor	Rich	Poor	Rich	Poor	Rich	Poor	Rich	Poor
Earnings	0.0489	0.0856	0.117**	0.184***	0.0155	0.123**	0.0724	0.265***	0.204***	0.312***
	(0.0376)	(0.0579)	(0.0447)	(0.0487)	(0.0542)	(0.0574)	(0.0514)	(0.0752)	(0.0314)	(0.0361)
	59,866	55,797	61,724	60,357	43,066	23,264	60,307	55,192	61,971	60,727
Employment	0.0437	0.142*	0.0558	-0.0709	-0.138	-0.227	-0.225	-0.264	-0.0910*	-0.0791
	(0.0625)	(0.0811)	(0.104)	(0.123)	(0.131)	(0.338)	(0.158)	(0.254)	(0.0514)	(0.0677)
	52,163	35,320	56,154	45,572	27,707	7,648	53,563	39,876	60,722	57,887
County FE	X	X	X	X	X	X	X	X	X	X
Year-Quarter FE	X	X	X	X	X	X	X	X	X	X

Notes: Data from the QWI, 2006-2015. Coefficients are obtained from 20 separate regressions of the outcomes on the minimum wage by industry and Kaitz index. Robust standard errors are in parentheses, clustered by state. Number of observations are given below the standard errors. *** p<0.01, ** p<0.05, * p<0.1. Log county population is added as a control variable. Industries include drug stores, grocery stores, department stores, other merchandise stores, and restaurants as classified by 4-digit NAICS (4461, 4451, 4521, 4529, and 7225). The Kaitz index is defined as the ratio of the minimum wage to the average wage in each county. A county is defined as rich or poor if it has a Kaitz index below median or above median respectively, relative to all counties.

Table 1.21: Effect of minimum wage on prices by Kaitz index quartile, grocery stores

	(1)	(2)	(3)	(4)
Kaitz quartile	1	2	3	4
VARIABLES	Log price index			
Log MW	0.0553*	0.0645***	0.0778***	0.0828**
	(0.0287)	(0.0221)	(0.0173)	(0.0347)
Observations	196,266	50,700	26,316	13,840
R-squared	0.925	0.938	0.945	0.935
Prob > F	0.039	0.000	0.001	0.004
Number of units	4912	1270	658	346
Number of clusters	45	46	40	27

Notes: Robust standard errors are in parentheses, clustered at the state level. *** p<0.01, ** p<0.05, * p<0.1. Control variables as well as store and period fixed effects are included. Store location is known at the county level. For each county in the sample, I measure the Kaitz index in 2006q1 and calculate the quartile each county is in, which is denoted as the Kaitz quartile.

Table 1.22: Proportion of revenue earned by each product department by store type

Store Type	Product department				
	Health & Beauty Care	Food	Non-Food Grocery	Alcohol	General Merchandise
Drug	0.479	0.216	0.171	0.052	0.083
Food	0.053	0.765	0.094	0.065	0.023
Merchandise	0.219	0.312	0.225	0.008	0.237

Notes: This table lists the proportion of revenue earned by each product department by store type across all stores from 2006-2015.

Table 1.23: Effect of minimum wage on prices and real sales by product department and Kaitz index, drug stores

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Counties	Rich					Poor				
Product Department	Health & Beauty Care	Food	Non-food Grocery	Alcohol	General merchandise	Health & Beauty Care	Food	Non-food Grocery	Alcohol	General merchandise
Log price index	-0.0100	-0.0441	-0.0768	-0.0898	-0.0556*	0.0181	0.0254	-0.293	-0.150**	-0.00437
	(0.0234)	(0.0276)	(0.0763)	(0.0795)	(0.0312)	(0.0304)	(0.0278)	(0.174)	(0.0621)	(0.0504)
Log real sales	-0.0413	-0.0310	0.283**	0.392	-0.107*	0.0130	0.0607	0.486	0.435	-0.0932
	(0.0555)	(0.0624)	(0.128)	(0.351)	(0.0628)	(0.0963)	(0.141)	(0.321)	(0.267)	(0.120)
Observations	309,842	309,802	304,802	102,458	309,642	45,136	45,136	44,536	24,552	45,136
Number of units	7751	7750	7625	2563	7746	1129	1129	1114	614	1129
Number of clusters	48	48	48	36	48	40	40	40	22	40

Notes: Coefficients are obtained from 20 separate regressions of the two outcomes, log price index and log real sales, on the minimum wage along with fixed effects and controls by product department and Kaitz index in drug stores. Robust standard errors are in parentheses, clustered at the state level. *** p<0.01, ** p<0.05, * p<0.1. Real sales are defined as nominal sales divided by the store-specific price index. A county is defined as rich or poor if it has a Kaitz index below median or above median respectively, relative to all counties.

Table 1.24: Effect of minimum wage on prices and real sales by product department and Kaitz index, grocery stores

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Counties	Rich					Poor				
Product Department	Health & Beauty Care	Food	Non-food Grocery	Alcohol	General merchandise	Health & Beauty Care	Food	Non-food Grocery	Alcohol	General merchandise
Log price index	0.00584	0.0459*	0.106**	0.0380	-0.0594**	0.0333	0.0575**	0.168***	0.112**	-0.0623
	(0.0160)	(0.0229)	(0.0433)	(0.0675)	(0.0223)	(0.0277)	(0.0266)	(0.0444)	(0.0529)	(0.0461)
Log real sales	0.0397	0.0131	-0.152	-0.114	0.161*	-0.0762	0.111	-0.167	0.350	0.183
	(0.0945)	(0.0578)	(0.0996)	(0.229)	(0.0833)	(0.141)	(0.0705)	(0.140)	(0.351)	(0.167)
Observations	247,806	248,206	245,766	243,286	247,646	40,276	40,236	40,076	39,436	40,236
Number of units	6198	6208	6147	6085	6194	1007	1006	1002	986	1006
Number of clusters	48	48	48	48	48	41	41	41	39	41

Notes: Coefficients are obtained from 20 separate regressions of the two outcomes, log price index and log real sales, on the minimum wage along with fixed effects and controls by product department and Kaitz index in grocery stores. Robust standard errors are in parentheses, clustered at the state level. *** p<0.01, ** p<0.05, * p<0.1. Real sales are defined as nominal sales divided by the store-specific price index. A county is defined as rich or poor if it has a Kaitz index below median or above median respectively, relative to all counties.

Table 1.25: Effect of minimum wage on prices and real sales by product department and Kaitz index, mass merchandise stores

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Counties			Rich					Poor		
Product Department	Health & Beauty Care	Food	Non-food Grocery	Alcohol	General merchandise	Health & Beauty Care	Food	Non-food Grocery	Alcohol	General merchandise
Log price index	-0.0198**	-0.0117	-0.0226**	0.0974	0.0858	-0.0145	-0.0187	-0.0261**	0.0285	0.116**
	(0.00864)	(0.0140)	(0.0105)	(0.0708)	(0.0540)	(0.00966)	(0.0154)	(0.0117)	(0.0934)	(0.0444)
Log real sales	0.000282	-0.0301	0.00231	-1.511*	-0.0867**	0.147**	0.359**	0.198*	0.645	-0.0212
	(0.0484)	(0.0606)	(0.0575)	(0.831)	(0.0394)	(0.0717)	(0.158)	(0.115)	(0.726)	(0.0691)
Observations	232,189	232,229	230,193	28,476	232,189	50,488	50,488	49,968	6,360	50,488
Number of units	5811	5812	5761	712	5811	1265	1265	1252	159	1265
Number of clusters	48	48	48	36	48	45	45	45	21	45

Notes: Coefficients are obtained from 20 separate regressions of the two outcomes, log price index and log real sales, on the minimum wage along with fixed effects and controls by product department and Kaitz index in mass merchandise stores. Robust standard errors are in parentheses, clustered at the state level. *** p<0.01, ** p<0.05, * p<0.1. Real sales are defined as nominal sales divided by the store-specific price index. A county is defined as rich or poor if it has a Kaitz index below median or above median respectively, relative to all counties.

Table 1.26: Theoretical estimates of minimum wage pass-through elasticity

Industry	Labor cost share	Weighted share of wages earned by MW affected workers	Spillover adjustment	Pass-through rate	MW pass-through elasticity
Grocery Stores	0.1005	0.0461	No	1	0.00463
Grocery Stores	0.1005	0.1284	AMS	1	0.01291
Grocery Stores	0.1005	0.2078	Lee	1	0.02088
Health Stores	0.1198	0.0204	No	1	0.00244
Health Stores	0.1198	0.0563	AMS	1	0.00675
Health Stores	0.1198	0.1004	Lee	1	0.01203
Department Stores	0.1078	0.0309	No	1	0.00333
Department Stores	0.1078	0.1133	AMS	1	0.01222
Department Stores	0.1078	0.2004	Lee	1	0.02160
Restaurants	0.2972	0.1780	No	1	0.05291
Restaurants	0.2972	0.2385	AMS	1	0.07087
Restaurants	0.2972	0.3436	Lee	1	0.10210

Notes: Pooled data from CPS MORG, 2006-2015. Shares are constructed using CPS sample weights. Almost all workers appear twice by construction of the rotating panel. Industries are classified according to the 2010 Census occupational classification used by the CPS. Labor cost shares are from 2007 and 2012 SUSB. Shares are constructed using ACS sample person weights. Spillover adjustments are made based on theoretical derivations and using spillover elasticity estimates from Autor, Manning, and Smith (2016) (AMS) and Lee (1999), normalized by the maximum percentile. Pass-through rates are taken from estimates in previous literature. The minimum wage pass-through elasticity is a multiple of the labor cost share, the weighted share of wages earned by MW affected workers, and the pass-through rate as shown by theory.

Table 1.27: Effect of minimum wage on shopping behavior

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
VARIABLES	Log expenditure			Coupon share			Deal share			Store brand share		
Log MW			-0.0249			0.0112***			0.0104			0.0275***
			(0.0363)			(0.00263)			(0.0132)			(0.00905)
Log MW x Below Median	-0.0176	-0.00669	0.0217	-0.00764***	-0.00643***	-0.00796***	-0.0488***	-0.0414***	-0.0496***	-0.00880**	-0.00865**	-0.0227***
	(0.0202)	(0.0197)	(0.0232)	(0.00170)	(0.00157)	(0.00174)	(0.00737)	(0.00644)	(0.00804)	(0.00364)	(0.00350)	(0.00445)
Observations	1,539,076	1,539,076	3,461,760	1,539,078	1,539,078	3,461,992	1,539,076	1,539,076	3,461,760	1,539,076	1,539,076	3,461,760
R-squared	0.733	1.000	0.707	0.768	1.000	0.584	0.852	1.000	0.703	0.690	1.000	0.445
Prob > F	0.482	0.009	0.020	0.000	0.000	0.000	0.000	0.000	0.000	0.001	0.030	0.000
Number of units	61431	61431	27984	61431	61431	27984	61431	61431	27984	61431	61431	27984
Number of clusters	49	49	49	49	49	49	49	49	49	49	49	49
State-period FE	X			X			X			X		
Additional controls		X			X			X			X	
Product department-period FE			X			X			X			X

Notes: Robust standard errors are in parentheses, clustered at the state level. *** p<0.01, ** p<0.05, * p<0.1. Observations are weighted by sampling weights and product expenditure share when applicable. Control variables, household and period fixed effects are included. Control variables include log housing price, log county unemployment rate, log county population, log county average wage, and household size fixed effects. Below median refers to an indicator variable for households with a household income below the median in 2006. Coupon share denotes the share of expenditures made using a coupon, deal share denotes the share of expenditures on goods on sale, and store brand share denotes the share of expenditures on goods that are store brand. Additional controls are household characteristics interacted with the minimum wage variable. These household characteristics include race, marital status, age, and education of the household head(s). Product department-period FE is applied to the regression that segments household expenditure by product department and uses balanced households.

Table 1.28: Effect of minimum wage on prices, 1% sample of products in dry grocery in grocery stores

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Weights		Product revenue	County revenue	County population	County-product revenue		Product revenue	County revenue	County population	County-product revenue
VARIABLES	Log price									
Log MW	0.00456	0.0240	0.0259**	0.0243**	0.0412*	0.0179	0.0240	0.0532**	0.0444**	0.0528*
	(0.0136)	(0.0208)	(0.0118)	(0.0118)	(0.0232)	(0.0178)	(0.0208)	(0.0198)	(0.0209)	(0.0270)
	[-.0138,.0181]	[-.0232,.0465]	[.00160,.0421]	[-.00160,.0426]	[-.00389,.0701]	[-.0219,.0360]	[-.0318,.0593]	[.000764,.0683]	[-.00509,.0688]	[-.0269,.0827]
Observations	10,573,758	10,573,758	10,573,758	10,561,865	10,573,758	2,306,678	10,573,758	2,306,678	2,306,189	2,306,678
R-squared	0.973	0.975	0.974	0.973	0.984	0.981	0.975	0.984	0.983	0.985
Prob > F	0.739	0.254	0.034	0.045	0.082	0.320	0.254	0.010	0.038	0.056
Number of units	747072	747072	747072	744875	747072	57656	747072	57656	57656	57656
Number of clusters	49	49	49	49	49	49	49	49	49	49
County-Product FE	X	X	X	X	X	X	X	X	X	X
Product-Period FE	X	X	X	X	X	X	X	X	X	X
Balanced						X	X	X	X	X

Notes: Robust standard errors are in parentheses, clustered by state. *** p<0.01, ** p<0.05, * p<0.1. Point estimates and standard errors are for a particular 1% random sample of products. 95% intervals are in brackets, constructed using 100 different random 1% samples from the entire set of products. Weights refer to regression weights using each of the specified variables, which are totals across the sample period. Balanced refers to retaining only county-product observations that appear in every period in the sample.

Table 1.29: Effect of minimum wage on prices by store type, substates only

	(1)	(2)	(3)	(4)	(5)	(6)
Store Type	Drug		Grocery		Merchandise	
VARIABLES	Log price index					
Log MW	0.0935***	0.0599	0.0373	0.0469**	0.0874***	0.0271
	(0.0256)	(0.0377)	(0.0254)	(0.0167)	(0.0218)	(0.0234)
Observations	18,948	14,320	20,880	15,160	7,292	4,360
R-squared	0.808	0.937	0.905	0.923	0.876	0.888
Prob > F	0.018	0.012	0.073	0.000	0.000	0.001
Number of units	474	358	522	379	183	109
Number of clusters	24	17	24	17	16	9
State-period FE		X		X		X

Notes: Robust standard errors are in parentheses, clustered by substate. *** p<0.01, ** p<0.05, * p<0.1. Control variables as well as store and period fixed effects are included.

Table 1.30: Average expenditure shares by product characteristic and income quartile across product groups

Characteristic	Non-organic	Organic	Store Brand	Name Brand
	Average Expenditure share			
Income Quartile				
1	0.258	0.172	0.292	0.241
2	0.235	0.199	0.245	0.230
3	0.243	0.247	0.233	0.247
4	0.265	0.382	0.230	0.282
Number of groups	59	59	105	105

Notes: This table presents average expenditure shares across product groups by product characteristics and household income quartile in 2006. Results are similar for later years in the sample period. Household income quartiles are constructed after controlling for household size fixed effects and results are robust to skipping this correction.

Table 1.31: Effect of minimum wage on prices and real sales for grocery stores by product characteristic

	(1)	(2)	(3)	(4)
Product Characteristic	Non-organic	Organic	Store Brand	Name Brand
Log price index	0.0407**	-0.0307	0.0309	0.0517**
	(0.0172)	(0.0348)	(0.0244)	(0.0210)
Log real sales	0.125**	-0.00555	0.0203	0.0163
	(0.0552)	(0.197)	(0.0644)	(0.0533)
Observations	645,366	645,366	791,178	791,178
Number of units	5379	5379	6595	6595
Number of clusters	48	48	48	48

Notes: Coefficients are obtained from 8 separate regressions of the two outcomes, log price index and log real sales, on the minimum wage along with control variables as well as store and period fixed effects. Robust standard errors are in parentheses, clustered at the state level. *** p<0.01, ** p<0.05, * p<0.1.

Table 1.32: Effect of income segregation on minimum wage pass-through elasticity

	(1)	(2)
VARIABLES	Log price index	
Log MW	-0.00835	
	(0.0607)	
Income segregation	5.105**	4.448**
	(2.037)	(1.678)
Log MW x Income Segregation	-1.129**	-0.707*
	(0.485)	(0.379)
Kaitz Index	-0.297	-0.119
	(0.479)	(0.318)
Log MW x Kaitz Index	0.301*	0.249***
	(0.156)	(0.0825)
Kaitz Index x Income Segregation	-15.80**	-14.02**
	(6.287)	(5.228)
Log MW x Income Segregation x Kaitz Index	3.179**	2.055*
	(1.427)	(1.154)
Observations	287,122	287,122
R-squared	0.932	0.949
Prob > F	0.000	0.000
Number of units	7180	7180
Number of clusters	48	48
State-period FE		X

Notes: Robust standard errors are in parentheses, clustered by state. *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Control variables as well as store and period fixed effects are included. The Kaitz index is defined as the ratio of the minimum wage to the average wage in each county and fixed to the value in 2006q1. The measure of income segregation used is Reardon's rank-order index from Reardon (2011) and constructed at the county level in Chetty and Hendren (2016).

1.12.10 Figures

Figure 1.11: Comparison of Nielsen price indices with CPI

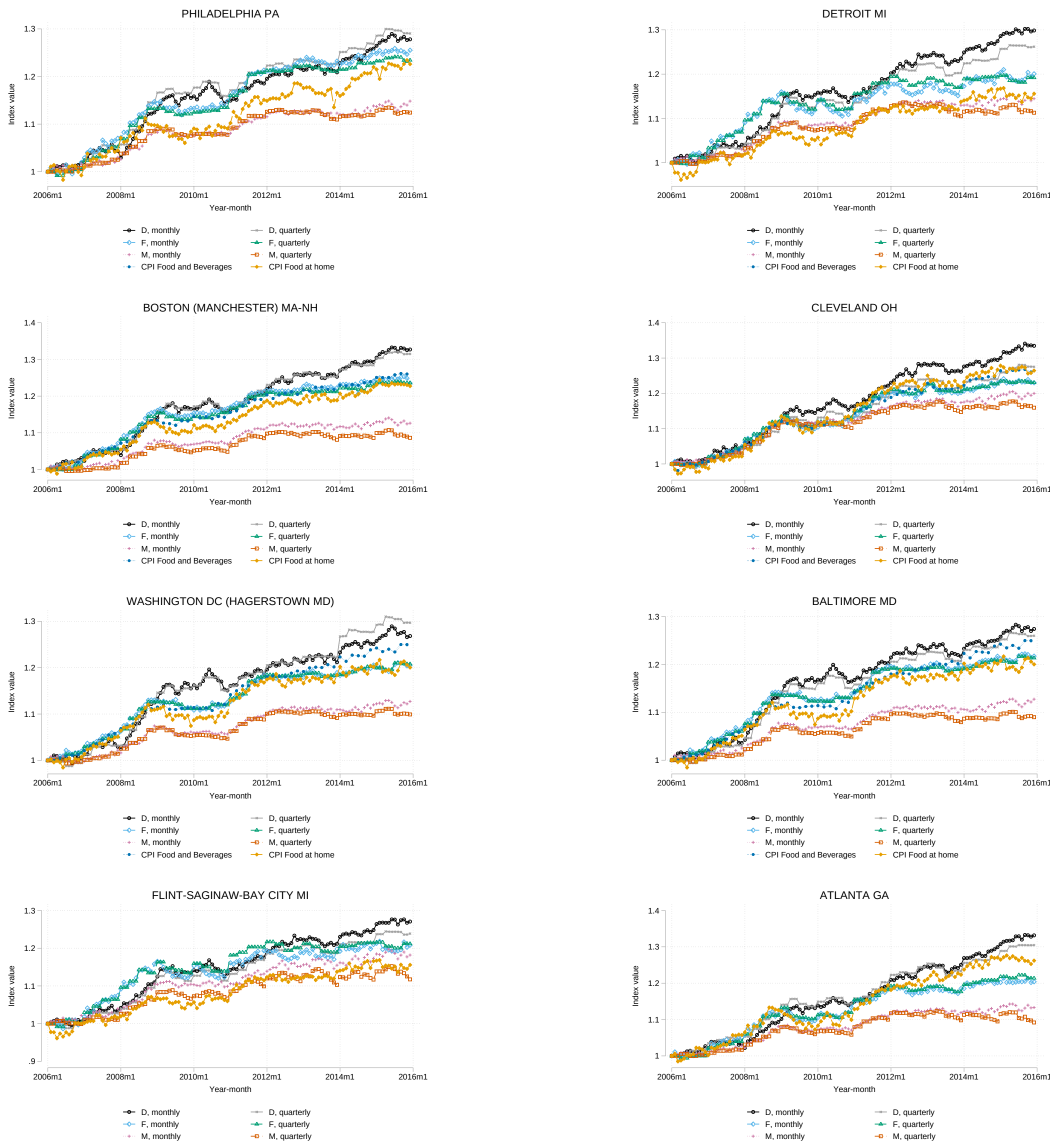
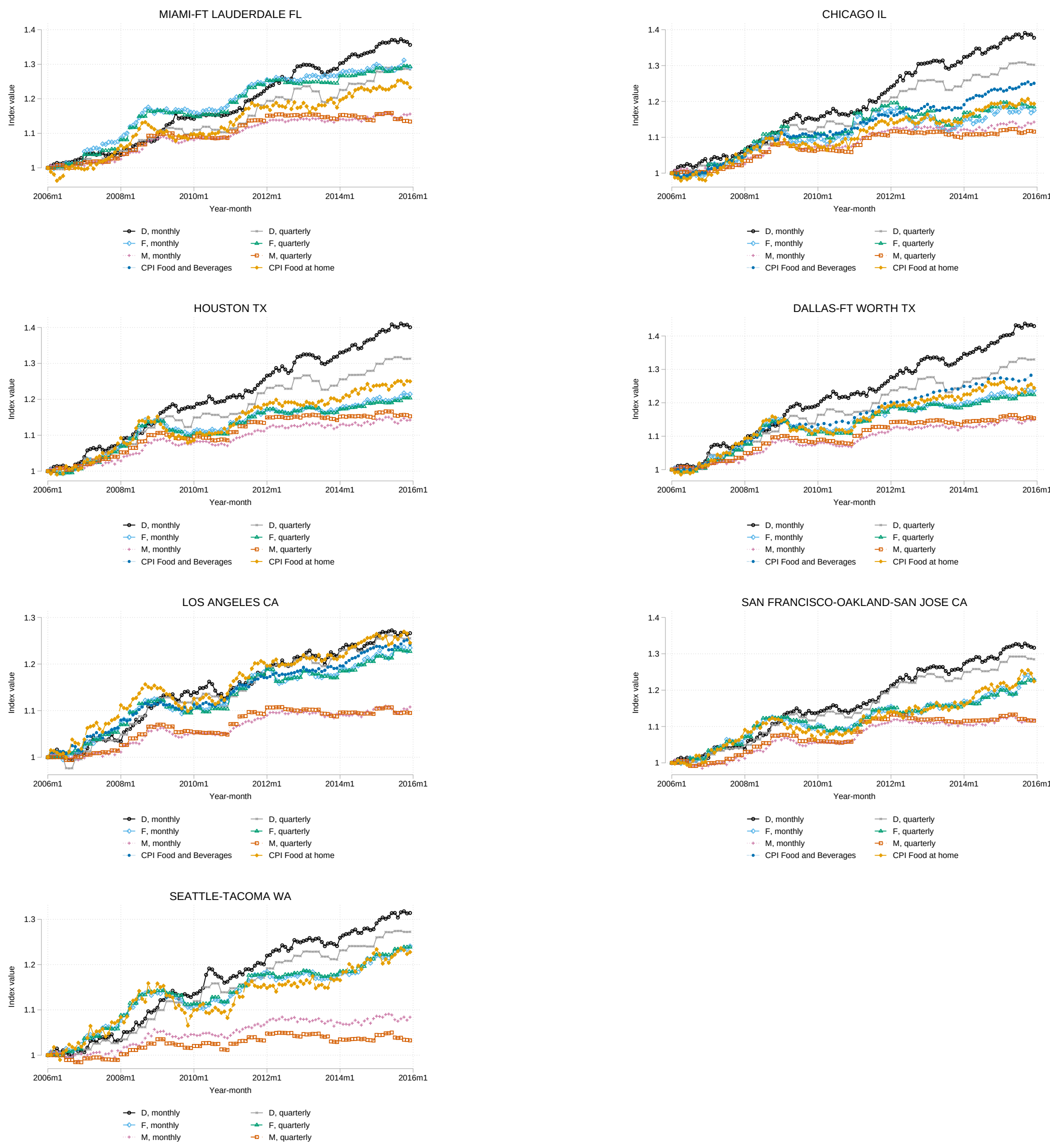
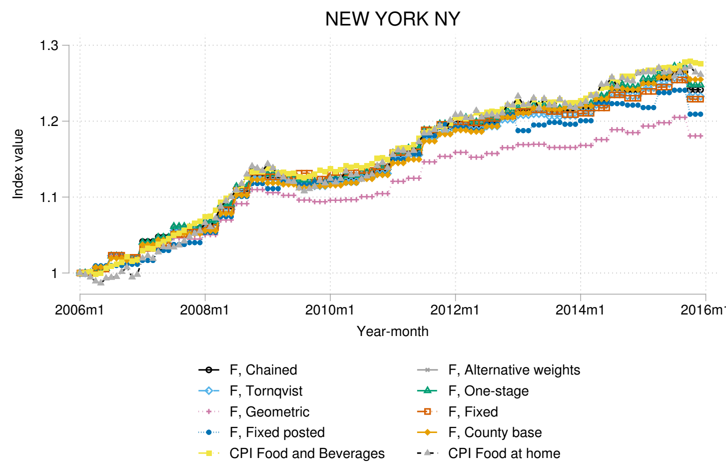


Figure 1.11, continued: Comparison of Nielsen price indices with CPI



Notes: This figure plots city-level price indices from 2006-2015 constructed using Nielsen retail scanner data against those used by the BLS to construct the CPI. D, F, and M correspond to Nielsen price indices for drug stores, grocery stores, and mass merchandise stores respectively. Nielsen price indices are first constructed at the store level, and aggrgated to the city level by taking a sales-weighted average. City-level CPI price indices are publicly available and published by the BLS.

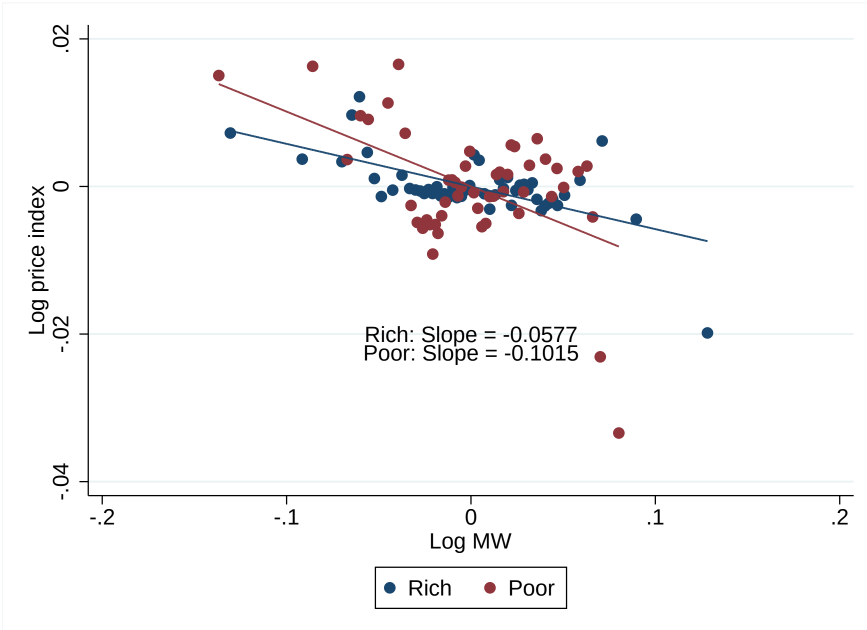
Figure 1.12: Comparison of Nielsen price indices with CPI, grocery stores



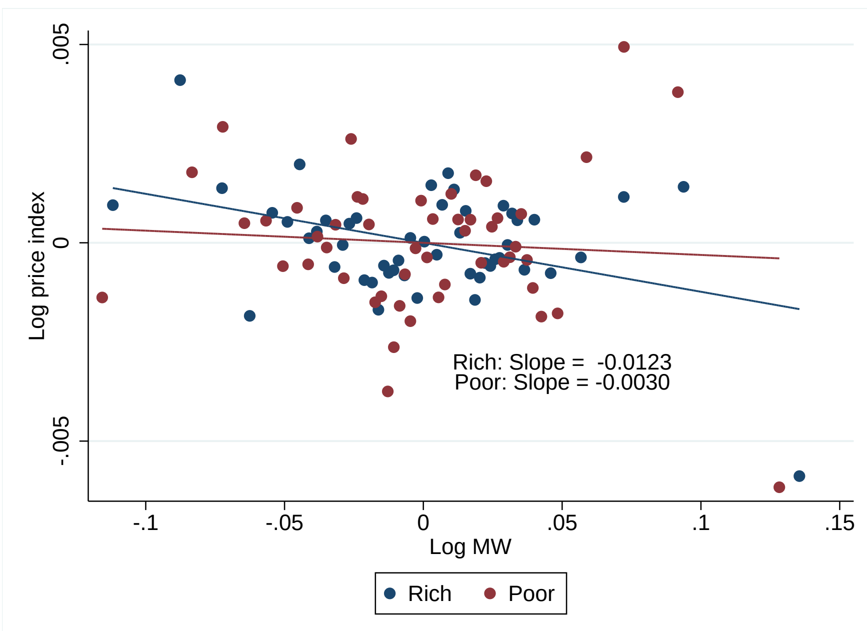
Notes: This figure plots city-level price indices from 2006-2015 constructed using Nielsen retail scanner data with alternative methods against those used by the BLS to construct the CPI. F correspond to Nielsen price indices for drug stores, grocery stores, and mass merchandise stores respectively. Nielsen price indices are first constructed at the store level, and aggregated to the city level by taking a sales-weighted average.

Figure 1.13: Log price index on log minimum wage, drug and merchandise stores in rich and poor counties

(a) Drug

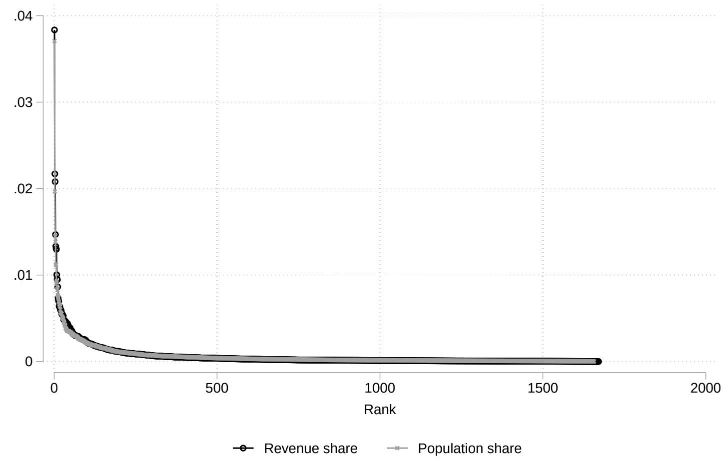


(b) Mass merchandise



Notes: This figure plots the log price index against the log minimum wage by rich and poor counties as measured by the Kaitz index. Both variables are residualized by regressing on a set of controls, store fixed effects, and period fixed effects. For each store-year-quarter observation, the residualized log minimum wage is calculated and grouped into 50 quantiles. The x-axis displays the mean of the residualized log minimum wage in each quantile. The y-axis shows the mean of the residualized log price index in each quantile. This is done separately for samples containing rich and poor counties, and the line of best fit is obtained from the regression using all observations in each sample, and the slopes are reported on the graph. A county is defined as rich or poor if it has a Kaitz index below median or above median respectively, relative to all counties. The sample is restricted to drug and mass merchandise stores.

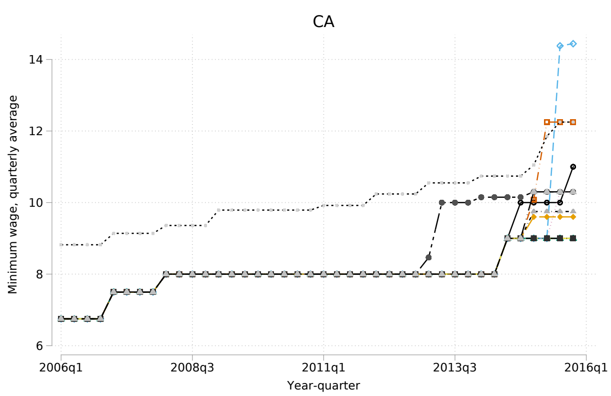
Figure 1.14: County revenue and population shares against their ranks



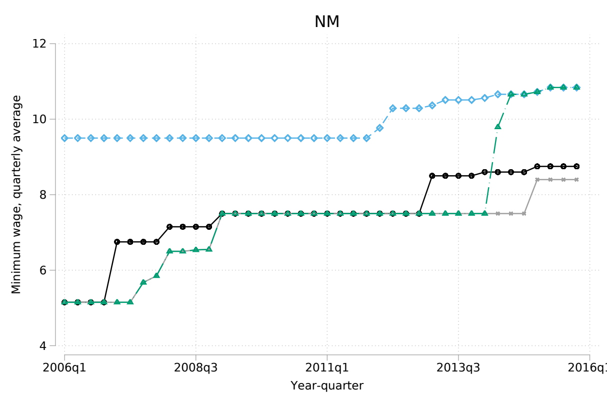
Notes: This figure plots total revenue generated by each county in a 1% random sample of products in dry grocery in grocery stores, as well as the total population in each county in the chosen sample. Both of these variables are Pareto distributed, implying that the shares have a very unequal distribution.

Figure 1.15: Substate minimum wages in California and New Mexico

(a) California



(b) New Mexico



Notes: This figure plots the minimum wages for substates in California and New Mexico with their own local minimum wage ordinances. Substates in California include Berkeley, El Cerrito, Emeryville, Los Angeles County, Mountain View, Oakland, Pal Alto, Richmond, Sacramento, San Diego, San Francisco, San Jose, Santa Clara, and Sunnyvale. Substates in New Mexico include Albuquerque, Las Cruces, Santa Fe, and Santa Fe County.

CHAPTER 2

HOW DO GOVERNMENT TRANSFER PAYMENTS AFFECT RETAIL PRICES AND WELFARE? EVIDENCE FROM SNAP

written jointly with Hee Kwon Seo

2.1 Abstract

The Supplemental Nutrition Assistance Program (SNAP), a food-consumption safety net in the US, accounts for over 10%-15% of food-store expenditures each year. Using national retail and consumer scanner data, we study the impact of large-scale variation in SNAP disbursement on retail prices, sales, and household consumption during the Great Recession. To address potential endogeneity in payments, we construct an instrumental variable induced by state-specific program adjustments. Price responses exhibit long and flat pre-trends, sharp timing, and stronger effects in regions where the recipient share of the population is larger. Estimates of marginal propensities to consume are also in close agreement with the past literature. Our results suggest a 1% increase in SNAP benefits per population leads to a 0.08% increase in grocery store prices. We develop a theoretical partial equilibrium framework to calculate the local incidence of SNAP benefits for SNAP-eligible products and find that producers mostly benefit at the expense of non-SNAP households due to market power. A marginal dollar of SNAP benefits increases producer surplus by about \$0.5, increases SNAP consumer surplus by about \$0.7, and decreases non-SNAP consumer surplus by about \$0.4. If the objective of SNAP is to guarantee a floor of real spending power on food, federal maximum benefits should be increased by about 10% to account for the price response.

2.2 Introduction

As the second-largest means-tested aid program in the US after Medicaid, the Supplemental Nutritional Assistance Program (SNAP, formerly the Food Stamp Program) is a central element of the US safety net, providing about \$60 billion to \$70 billion worth of in-kind electronic benefits for food to more than 40 million low-income Americans annually. Although much attention has focused on the impact of SNAP on participating households who are the intended beneficiaries of the program, less is known about the effect on participating retailers on the supply side. Over 280,000 authorized retailers sell food to SNAP recipients, and SNAP benefits account for over 10%-15% of US food-at-home sales in food stores as shown in Figure 2.1.[1] On one side of the policy debate, emphasis is placed on how SNAP feeds poor starving children, but on the other side, emphasis is placed on how SNAP feeds rich giant corporations. Quantifying how producers respond to changes in SNAP, in particular by changing prices, is essential to uncovering the welfare consequences of the program. Price effects could attenuate the real spending power of government transfer payments and distort the redistributions such transfer programs intend to achieve. These mechanisms of economic incidence have broader implications for other transfer programs such as universal basic income, which are frequently discussed as a way to assure a standard of living for all in a world of robots and artificial intelligence.

We graphically illustrate how economic incidence changes in a simple partial equilibrium framework under different assumptions about the food market in Figure 2.2. For simplicity, we begin with an example in which all consumers receive SNAP benefits. In Figure 2.2a, we assume a perfect competitive market with a flat supply curve. A SNAP-benefit increase shifts the demand curve out from D to D', and prices remain constant while quantities consumed increase from Q to Q'. Assuming no income effects or a parallel shift in demand, we evaluate incidence under the original demand curve. All of the surplus generated goes to

1. For instance, in 2013, Americans spent \$425.2 billion buying food for consumption at home across food stores, whereas SNAP spent \$76.1 billion, or 17.9% of the American total (USDA, 2016, 2017).

the consumer as consumer surplus increases by $PBCP^{S'}$. In Figure 2.2b, we again assume perfect competition but let the supply curve be upward-sloping. Prices now increase from P to P' when SNAP benefits increase. The increase in surplus is now divided between the producer and the consumer, with producer surplus increasing by $P'ABP$ and consumer surplus increasing by $PBCP^{S'}$. In Figure 2.2c, we assume the firm is a monopolist with a constant marginal cost. A SNAP-benefit rise increases prices from P to P' because the firm raises its markup and sells at a more inelastic portion of the demand curve. The increase in surplus is again split between the producer and the consumer, with producer surplus increasing by $P'ABCDEP$ and consumer surplus increasing by $PEBP^{S'}$. Therefore, either an upward-sloping supply or market power would shift a portion of the benefits generated by the transfer from SNAP recipients to producers.

As a federal program, SNAP has experienced little variation across states for most of its history. Changes in SNAP benefits per population are also driven by local economic shocks as both household eligibility for the program and benefits per participant are determined by characteristics such as household income and household size. In this paper, we study the dramatic rise in scale that SNAP underwent during the Great Recession: The program expanded 140% between 2007 and 2014 partly due to the 2008 Farm Bill and the 2009 American Recovery and Reinvestment Act (ARRA). We use national retail and consumer scanner data to estimate the impact of benefit changes on retail prices, sales, and household consumption. To address endogeneity concerns, we develop a novel identification strategy in the spirit of Currie and Gruber (1996) by exploiting state-level variation in SNAP benefits induced only by policy changes, holding the characteristics of participants fixed to the pre-period.

We motivate our empirical strategy by using an event-study approach to show that price effects exhibit long and flat pre-trends as well as sharp timing. A 1% increase in benefits per population causes a large and persistent increase in store-level prices of around 0.08% in grocery stores. This result passes a series of robustness checks. Consistent with theory, this effect is stronger in counties with higher SNAP participation rates and higher grocery market concentration. Increases in sales after controlling for price effects are also larger for stores in counties with higher SNAP participation rates. In addition, given that SNAP is only eligible for food items, we exploit the richness of barcode-level retail scanner data to estimate differential impacts on SNAP-eligible products versus SNAP-ineligible products. Interestingly, we estimate that SNAP has positive price effects of similar magnitude for both groups of products, although the sales effect is much stronger in high-participation counties for SNAP-eligible goods but not SNAP-ineligible goods. This result can be explained by several models of multi-product pricing and suggests that while SNAP generates positive demand shocks for eligible products, retailers are able to raise markups on both eligible and ineligible products. We also estimate small and statistically insignificant effects on other store types, firm dynamics such as retailer entry and exit, and market structure.

Next, we use consumer scanner data to estimate the effect of SNAP on household consumption, which allows us to classify consumption into that by eligible and ineligible households on eligible and ineligible products. We estimate a marginal propensity to consume food (MPCF) out of SNAP for eligible households in line with previous literature of about 0.44, whereas the MPC for ineligible products or ineligible households is statistically insignificant and different from the MPCF out of SNAP. Using across-state variation in issuance schedules, we find the MPCF out of SNAP is larger in weeks of benefit disbursement We also investigate the effect of SNAP on shopping behavior and find small and mixed effects.

We then utilize our reduced-form estimates of the price response and the MPCF out of SNAP as sufficient statistics to measure the local incidence of changes in SNAP benefits and study the welfare consequences of changes in program scale by adapting theoretical tools developed in Weyl and Fabinger (2013). We find that for SNAP-eligible goods, increases in prices enable retailers to capture additional producer surplus due to market power, whereas SNAP consumers still experience a large increase in consumer surplus. On the other hand, non-SNAP consumers suffer a decline in consumer surplus. A marginal dollar of SNAP benefits increases producer surplus by about \$0.5, increases SNAP consumer surplus by about \$0.7, and decreases non-SNAP consumer surplus by about \$0.4, or about \$0.06 per non-SNAP consumer. We also derive pass-through formulas to show that the size of price responses can be predicted using estimates of the MPCF out of SNAP, demand elasticity, and pass-through rate. We find that the magnitude of our reduced-form estimate of price response is consistent with the theoretically calibrated pass-through elasticity. Furthermore, theory predicts the price responses increase with SNAP participation rates and market power, consistent with our results on heterogeneity.

First, this paper contributes to an extensive literature that evaluates the impacts of social programs such as in-kind transfers, especially SNAP. Recent work regarding SNAP (Hoynes and Schanzenbach, 2009; Almond, Hoynes and Schanzenbach, 2010; Hoynes, Schanzenbach and Almond, 2016) exploits the county-level rollout of the program in the 1970s as a quasi-experiment to evaluate the impact of SNAP on consumption, birth weights, and long-run outcomes. Furthermore, a growing number of studies have estimated the MPCF out of SNAP benefits. Hastings and Shapiro (2017) (hereafter HS) use transaction-level data from a large US grocery retailer's operations in five states to estimate an MPCF out of SNAP of 0.5 to 0.6 but an MPCF out of cash of 0.1. Based on these estimates and other evidence, they reject the fungibility of SNAP benefits.[2] This vast literature is summarized in Hoynes and Schanzenbach (2016).

The supply-side responses of retailers has received less attention. Regarding SNAP's impact on prices in particular, a literature exploits state-level variation in SNAP payment across weeks within-month to investigate how consumers exhaust their benefits quickly upon receipt, and whether retail stores take advantage of these predictable cyclical demand shocks to raise profits by increasing prices.[3] Instead of studying cyclical variation within-month, we

2. Other work includes Beatty and Tuttle (2015), who report an MPCF of 0.484 estimated using the Consumer Expenditure Survey data and the matching-on-observables method, comparing food expenditure patterns of SNAP-recipient and non-SNAP recipient households before and after the 2009 ARRA.

3. However, consensus is lacking on whether the within-month price response is a general phenomenon: Hastings and Washington (2010) examine three stores in Nevada between 2006 and 2008 and report sharp

utilize a novel identification strategy to study the impact of a persistent increase in SNAP benefits per population, which has different implications for incidence from the policy they study, which involves staggering benefit issuance over multiple days of the month. Jaravel (2018) also studies the impact of SNAP on prices and product variety. He uses consumer scanner data only and utilizes changes in participation rates as identifying variation and focuses on long-run effects in which product entry could lower prices through competition. By contrast, we use a different identification strategy while focusing on medium-run effects and economic incidence.

Regarding other social programs and their impact on prices, Cunha et al. (2015) study a village-level random experiment in Mexico and find that in-kind transfers of food decrease prices due to increases in supply, whereas equivalently valued cash transfers have a negligible impact on prices. Filmer et al. (2018) analyze a randomized evaluation of a Philippine cash transfer program and show prices of perishable protein-rich foods rose as a result of the transfers, leading to a negative impact on the nutrition of non-beneficiary children. A literature on the price effect of minimum wages also exists.[4] Village-level transfers directly increase food supply, whereas minimum wages decrease product supply by increasing labor costs and could have demand-side effects by increasing the income of consumers. By contrast, as an in-kind transfer funded by the federal government through electronic-benefit transfers (EBT), SNAP mostly shifts the demand curve without directly shifting the supply curve. Similar to Filmer et al. (2018), we find that transfer programs can have unintended consequences on non-recipients through equilibrium effects.

Second, we contribute to a growing literature providing empirical evidence on how demand shocks affect prices. Recent work using retail scanner data suggests price responses to

price responses, but Goldin, Homonoff and Meckel (2016) extend the same empirical strategy to stores across the 48 contiguous states and find little evidence for price responses.

4. A few studies from this literature include Aaronson, French and MacDonald (2008), who provide empirical evidence for complete labor-cost pass-through in restaurants, whereas MaCurdy (2015) calibrates the distributional effect of prices with an input-output model under the assumption of complete pass-through. Leung (2018) suggests that in addition to the cost channel, changes in product demand could further increase prices under variable markups.

demand shocks are strong and significant. Stroebel and Vavra (2015) show the relationship between zip-code-level housing-price shocks and zip-code-level retail price indices is strongly positive. They argue their results are driven by procyclical markups due to decreases in demand elasticities as a result of increases in housing wealth. Beraja, Hurst and Ospina (2015) show a negative relationship between state-level retail prices and state-level unemployment rates. Our paper demonstrates another setting in which demand shocks, in this case driven by government transfers, increase prices. We also derive pass-through formulas to shed light on the factors that determine the size of the price response.

The paper is structured as follows. Section 2.3 discusses the data we use and the institutional details of SNAP payments, and introduces the instrument. Section 2.4 formally lays out the empirical specifications and graphical evidence to support the empirical strategy. Section 2.5 reports the main results. Section 2.6 derives pass-through formulas to shed light on the factors that determine the size of the price response and builds a theoretical partial equilibrium framework to calculate the incidence of changes in SNAP benefits. Section 2.7 concludes.

2.3 Data and Institutional Details

This section gives an overview of the data used for analysis, which include data on SNAP benefits, retail prices, quantities sold, household expenditures, and other auxiliary data. We also explain the formula used by the government to determine SNAP benefits for a given household, which motivates the construction of the instrumental variable (IV).

2.3.1 SNAP Benefits

We use publicly available state-by-month data on SNAP benefits and SNAP participation released by the Department of Agriculture (USDA) Food and Nutrition Service (FNS). County-level benefits are available at the bi-annual level for 33 states, because some states do not report benefits to the FNS at the county-level. County-level participation counts are available for all states at the annual level. We use state-by-month data for our main results to retain the full sample while showing the results are robust to using county-level benefits for the set of available states.

Figure 2.3 plots out the SNAP benefits per population over time from 2006 to 2015, which is our sample period. Substantial variation exists across states, with benefits per population ranging from \$5 to \$30 per month. Large discrete jumps in SNAP benefits per population mainly come from two events, namely, the 2008 Farm Bill and the 2009 ARRA, in which maximum benefits for SNAP households were raised by around 8.5% and 13.6%, respectively. Both of these events increased SNAP benefits at the federal level in October 2008 and April 2009, respectively, through the intensive margin (SNAP benefits per recipient) as shown in Figure 2.4a, with benefits per recipient ranging from \$80 to \$150. However, variation exists in the percentage change of average SNAP benefits per population in each state for these two events, as shown in Figure 2.5, with changes ranging from around 6% to 25%. Furthermore, SNAP participation rates were growing over this period, contributing to the smooth rise in the series over time through the extensive margin (SNAP recipients per population), as shown in Figure 2.4b, with participation rates ranging from 5% to 25%. Other major changes include cost-of-living adjustments (COLA) in 2007 as well as the expiration of the ARRA in 2013. In Figure 2.6a, we plot out the variation across states in changes in SNAP benefits per population across these four events. The Farm Bill and the ARRA contribute large and dispersed changes across states, whereas the 2007 COLA and 2013 ARRA expiration contribute smaller and less dispersed changes.

We also use the SNAP Quality Control (QC) Survey, which contains detailed demographic, economic, and program-eligibility information for a nationally representative sample of approximately 50,000 SNAP households. This dataset is generated from monthly quality-control reviews of SNAP cases that are conducted by state SNAP agencies to assess the accuracy of eligibility determinations and benefit calculations for the state's SNAP caseload.

We also obtain the code used to construct the QC data and apply exact procedures and parameters the FNS uses, which allows us to observe the exact SNAP benefits formula and construct the simulated instrument. We verify that the formula we use generates identical benefits to those in the QC data for every household.[5]

2.3.2 A Simplified Introduction to the SNAP Formula and Instrumentation Strategy

The federal SNAP formula is designed to ensure a floor of spending power on what the government considers sufficient nutrition for households in poverty. Every year, the USDA analyzes microdata collected in June that determines the Consumer Price Index (CPI). Using these data, the USDA prepares a monthly dietary budget called the "Thrifty Food Plan" for a typical four-person household. In October, the USDA updates the SNAP-benefit parameters based on the size of this budget, which becomes the maximum level of benefit a four-person household can receive from SNAP. The annual formula adjusts maximum levels for households with different numbers of family members based on a sliding scale. SNAP may update these parameters in months other than October at the discretion of the Congress. For example, the 2009 ARRA included a provision to add 13.6% to the Thrifty Food Plan budget, increasing the maximum benefit level for a single-member household to $200, and for a household of seven or more members to $150 per person. We show how maximum benefits changed over time in Figure 2.7a and how the maximum benefits per recipient decrease as household size increases in Figure 2.7b.

Because the federal formula's intent is to ensure a floor of spending power on food, the benefit amount falls as a given household's estimated ability to contribute to food spending rises above the floor. The final benefit level of a household is given by the maximum benefit level minus 30% of household "net countable income," which is the household's gross income

5. Due to extremely complex state idiosyncrasies in the implementation of the SSI-CAP program, we do not code in the exact SSI-CAP adjustments for these households, which constitute only 2% of the national sample of participants.

minus expenses considered indispensable. Examples of such expenses include the standard utility allowance (SUA) determined by each state to cover utility expenses, rental costs, and a homeless deduction for living expenses conditional on being homeless (e.g. transportation). Some of the components of the formula, such as the federal maximum benefit level and state-determined SUA, are determined by government policy, whereas other components of the formula, such as household gross income and rental costs, are determined by household characteristics. We describe in detail how changes in the SUA are determined by states in a plausibly exogenous manner in Appendix Section 2.10.2.1.

We first introduce the instrument using a few examples before laying out the details. To construct the instrument, we use the SNAP benefits formula to simulate what a fixed sample of participants would have received throughout the sample period, by allowing only policy parameters to vary over time while holding household economic variables such as household gross income and rental costs fixed, using a sample of about 50,000 SNAP participating households surveyed in the pre-period of 2006 from the SNAP QC data. We then sum over these simulated benefits for each recipient to construct our IV, synthetic benefits per population at the state-level.

Variation in the instrument is almost entirely driven by two policy changes. First, federal maximum benefits were raised and state-specific adjustments to the SUA were made during the Farm Bill. We illustrate these changes in Figure 2.8 by comparing New Hampshire and Wyoming, two states that had the largest difference in percentage changes of the instrument during the Farm Bill. In Figure 2.8a, we show that New Hampshire raised the heating and cooling SUA (HCSUA) by about 29% in October 2008 while Wyoming lowered it by about 23%. These changes led to a much larger percentage increase in simulated benefits per recipient, which we also denote as synthetic benefits per recipient, for New Hampshire as shown in Figure 2.8b. The two states had almost identical synthetic benefits per recipient until they diverged during the Farm Bill.

Second, the ARRA also raised federal maximum benefits, as illustrated in Figure 2.9 by comparing Missouri and New York, two states that had the largest difference in percentage changes of the instrument during the ARRA. In Figure 2.9a, we show the two states had only small changes in the HCSUA during the Farm Bill and the ARRA, with New York raising the HCSUA in February 2009 instead. However, the ARRA led to a 20.26% increase but only a 15.31% increase in the instrument for Missouri and New York respectively, because Missouri has a lower synthetic benefits per recipient, in particular because it has continually had a lower HCSUA relative to New York. Hence, because the ARRA raises federal maximum benefits by the same absolute magnitude for all states, Missouri has a higher percentage change in the instrument because it starts from a smaller base level.

Next, we introduce a simplified approximation of how the government specifies the SNAP-benefits formula to further elucidate the instrument. The approximation is for the sake of isolating intuition; we provide more details in Appendix Section 2.10.1, where we formalize the SNAP formula in its entirety, relying on the government's SNAP QC code and technical documents.

Let $X_{it} = \{N_{it}, S_{it}, I_{it}, R_{it}, U_{it}\}$ represent the set of potential SNAP recipient i's household characteristics observed by the government at time t. Characteristics important to the government include N_{it}, the person's household size; S_{it}, the person's state of residence; I_{it}, the person's gross income minus earned income deduction; R_{it}, the person's rent; and U_{it}, the person's utility expenditure.[6] Let $p_t(X_{it}) = \{b_t, o_t, u_t\}$ represent SNAP's formulaic parameters, which are functions of X_{it}. b_t represents the maximum-benefits formula; u_t, the utility-cost deduction formula; and o_t, the deduction formulae for various basic needs other than housing. An approximation of the SNAP benefits per recipient, denoted by $\tilde{B}_{it}$, is given by

$$\tilde{B}_{it} = b_t(N_{it}) - 0.45\, I_{it} + 0.45\, o_t(X_{it}) + 0.3 R_{it} + 0.3\, u_t(N_{it}, S_{it}, U_{it}). \tag{2.1}$$

6. The government uses other observed characteristics as well, but in this subsection, we do not focus on these characteristics, which include legal child-support expenses, dependent-care expenses, and medical expenses for households with elderly or disabled members; in 2008, deductions based on these expenses comprised only 2.2% of total expense deductions granted by the program.

As described before, b_t decreases in N_{it}. o_t also generally decreases in N_{it}. u_t is determined by the state-determined SUA in which each state sets a uniform utility-deduction standard intended to reflect the average state utility cost per household; these deductions apply uniformly across the state to households with a positive reported utility cost.

The problem with using state-level SNAP benefits per population, denoted as B_{st}, directly in an ordinary least-squares framework to identify effects of SNAP on retail prices is that variables such as I_{it} and R_{it} are endogenously correlated with unobservable economic fundamentals that determine prices.

The instrument we propose in this paper is a simulated benefits' process that relies *only on* pre-recession (2006) program participants, their household characteristics, and changes in formulaic parameters across time and state. It is similar in spirit to the simulated instrument introduced in Currie and Gruber (1996) and used in a series of related work, as well as those employed by Nakamura and Steinsson (2014), Wilson (2012), and Chodorow-Reich et al. (2012). We show that we can recast our instrument in a general Bartik-style shift-share instrument framework in Appendix Section 2.10.3.[7] The initial local shock exposure measure is pre-recession household characteristics while the shocks are policy changes. Our instrument $\tilde{Z}_{st}$ for the actual state-level benefits per population B_{st} can be expressed as,

$$\tilde{Z}_{st} = \frac{1}{n_{s,0}} \sum_{i \in s,0} \left[\tilde{B}_{it} \left(X_{i0}; p_t(X_{i0}) \right) \right]. \tag{2.2}$$

The IV $\tilde{Z}_{st}$ is constructed by summing over simulated benefits per recipient $\tilde{B}_{it}$ to the state-level using sampling weights in the QC data, and divided by $n_{s,0}$, the population of state s in the pre-period of 2006, which is denoted as $t = 0$. The actual state-level benefits per population B_{st} is obtained from the FNS data. Now, given that we use log points instead of levels in our empirical specification to facilitate interpretations in terms of elasticities, the

7. The Bartik instrument is named after Bartik (1991). Recent work on the econometrics of shift-share instruments include Goldsmith-Pinkham, Sorkin and Swift (2018), Borusyak, Hull and Jaravel (2018), Adão, Kolesár and Morales (2018), and Jaeger, Ruist and Stuhler (2018).

percentage changes in simulated benefits per population, which we also denote as synthetic benefits per population, can be approximated as,

$$\Delta \ln \tilde{Z}_{s,t} \approx \frac{\tilde{Z}_{s,t} - \tilde{Z}_{s,t-1}}{\tilde{Z}_{s,t-1}} \tag{2.3}$$

$$= \frac{\bar{\Delta}_s\, b_t(N_{i0}) - 0.45\bar{\Delta}_s\, o_t(X_{i0}) + 0.3\bar{\Delta}_s\, u_t(N_{i0}, S_{i0}, U_{i0})}{\frac{1}{n_{s,0}} \sum_{i \in s,0} \left[b_{t-1}(N_{i0}) - 0.45\, I_{i0} + 0.45\, o_{t-1}(X_{i0}) + 0.3 R_{i0} + 0.3\, u_{t-1}(N_{i0}, S_{i0}, U_{i0})\right]}, \tag{2.4}$$

where $\bar{\Delta}_s$ is the average difference operator.

The above equation shows that changes in the numerator across time are determined only by the interaction of changes in program parameters with fixed pre-recession (2006) household characteristics of program participants, most notably household size, state of residence, and the standard utility allowance. These changes in the numerator are then scaled by simulated state mean benefit levels in the previous period, which again only depend on changing program parameters and fixed pre-recession (2006) household characteristics of program participants. When using this instrument, we include state and time fixed effects. Therefore, our identification relies on the assumption that the interaction of changes in formula parameters, namely maximum benefits and SUA, with pre-recession household characteristics is plausibly exogenous to growth rates of other unobservable economic fundamentals affecting prices and expenditures.

We use the SNAP QC sample of participants in 2006 to construct the simulated instrument and decompose the variation in the instrument into its formulaic components. For both the Farm Bill and the ARRA, we regress state-level percentage changes in synthetic benefits per population on the log of state-level average SNAP household characteristics such as gross income, rent, and household size. We also include the share of participating households with elderly or disabled members and homeless members, because certain restrictions on the benefit amount, such as the shelter cap, do not bind for these households. All these measures are calculated in the pre-period of 2006 to match the sample used to construct the IV. In addition, we include the log of the amount of the SUA before each event, as well as the percentage change in SUA during the Farm Bill because no change occurred in the SUA during the ARRA. We then decompose the variance in the percentage change in the IV into components driven by each variable.[8] The results in Table 2.1 show that almost 80% of the variation in the IV is driven by variation in state-level changes in the SUA during the Farm Bill, whereas over 70% of the variation in the IV is driven by variation in base levels of the SUA during the ARRA. Average gross income and average rent also contribute to variation in the IV. A higher level of gross income, a lower level of rent, and a lower level of SUA in the pre-period leads to a lower initial level of the IV and hence a larger percentage change for the same absolute change in the IV. In Appendix Section 2.10.2.1, we illustrate how changes in SUA depend on changes in energy quantities consumed, as well as prices by fuel type during the Farm Bill and idiosyncratic state policies determined in the pre-period. We argue that after controlling for a vector of variables for state-level energy usage and prices, changes in synthetic benefits are plausibly exogenous to unobservables that affect our outcomes of interest, since residual policy variation should be driven by prediction errors by states in setting the SUA and idiosyncratic state-level differences in methodologies used to set the SUA.

Anticipating discussion of controls, we verify robustness of results to further inclusion of housing prices, unemployment rates, average state wage bills, average income, and average rent of program participants as controls. We find that, among these, housing-price control is necessary for meaningful precision, because the program scale also varied substantially on the extensive margin over this period, and participation rates and retail prices are strongly negatively correlated during this period. Including the housing-price control substantially improves the first stage. Results are robust to the inclusion or exclusion of any other combination of controls.

8. Consider a state-level regression $Y_s = \alpha + \sum_{i=1}^{n} \beta_i X_{i,s} + \varepsilon_s$ with n covariates. Variance can then be decomposed as $var(Y_s) = \sum_{i=1}^{n} cov(Y_s, \beta_i X_{i,s}) + var(\varepsilon_s) = \sum_{i=1}^{n} var(\beta_i X_{i,s}) + \sum_{i \neq j} cov(\beta_i X_{i,s}, \beta_j X_{j,s}) + var(\varepsilon_s)$. We define the covariance share for covariate X_i as $cov(Y_s, \beta_i X_{i,s})/var(Y_s)$.

Assuming changes in the SUA interacted with household characteristics of participating households prior to the Great Recession are plausibly exogenously distributed across states, our strategy allows us to shut down intensive-margin changes in variables affecting the realized benefits, such as I_{it} and R_{it}, that are exposed to influences of local economic conditions. Our strategy also allows us to shut down any extensive-margin channels, that is, the influences of household entry and exit into the program induced by evolving local economic conditions during the Great Recession.

2.3.3 Price Indices

2.3.3.1 Nielsen Retail Scanner

We use the Nielsen Retail Scanner Dataset through a partnership between the Nielsen Company and the James M. Kilts Center for Marketing at the University of Chicago Booth School of Business.[9] The data consists of weekly pricing, volume, and store merchandising conditions generated by participating retail store point-of-sale systems across the US from 2006-2015. Data is included from approximately 35,000 participating stores and include store types such as drug, grocery, and mass merchandise stores, covering around 53%-55% of national sales in food and drug stores and 32% of national sales in mass merchandise stores. The finest location of each store is given at the county level. We only use stores that appear throughout the entire sample period such that store entry and exit do not affect results. Among the stores in the sample in 2006, 84% remain throughout the entire sample period. A huge number of products from all Nielsen-tracked categories are included in the data, with 2.6 million universal product codes (UPCs) in total aggregated into around 1,100 product modules, which are further aggregated up to 125 product groups.

Although alternate price indices released by government agencies do exist, they have limitations that render them less suitable for our analysis, especially due to sampling error.

9. Information on access to the retail scanner data as well as the consumer panel data below is available at http://research.chicagobooth.edu/nielsen/.

These limitations are outlined in Beraja, Hurst and Ospina (2015). Therefore, this paper uses price indices constructed from micro data.

The advantage of using the retail scanner data as opposed to the Nielsen Consumer Panel is that a wider range of goods is observed at higher frequencies and quantities. Scanner price indices are constructed as in Beraja, Hurst and Ospina (2015). We briefly describe the approach they adopt in Appendix Section 2.10.4 and refer interested readers to their paper for details. Leung (2018) also investigates the behavior of the constructed indices and finds grocery store price indices are quite similar to the CPI city-level food-price indices, which are published for around 20 sample areas in the US. We also construct a range of different price indices using alternative methods, which give nearly identical results.

Retail stores need to apply to the FNS to accept SNAP payments. We obtain a panel of SNAP participating retail stores from the FNS and are able to match this panel with Nielsen stores. Our string-matching algorithms match over 90% of Nielsen stores to SNAP participating stores across store types, which implies that almost all stores in our data participate in SNAP. However, we focus on results from grocery stores for several reasons. First, as shown in DellaVigna and Gentzkow (2017) and Leung (2018), both drug and merchandise stores in the data mostly implement national pricing within retail chains, such that the local price response to local policy changes is negligible. We show this is indeed the case for SNAP in Section 2.5.1. Second, grocery stores derive the highest proportion of revenue from food, at around 77%.

2.3.4 Quantities sold and expenditures

Data on expenditures are obtained in two ways. First, we use the total nominal monthly sales for each store, constructed from the retail scanner data. In particular, we construct a measure for quantities using real sales, which are defined as the nominal sales divided by the store price index. Second, we use the Nielsen Consumer Panel Dataset.

2.3.4.1 Nielsen Consumer Panel

The Nielsen Consumer Panel Dataset represents a longitudinal panel of approximately 40,000-60,000 US households who continually provide information to Nielsen about their households and what products they buy, as well as when and where they make purchases. Panelists use in-home scanners to record all their purchases, from any outlet, intended for personal, in-home use. Products include all Nielsen-tracked categories of food and non-food items, across all retail outlets in the US Nielsen samples all states and major markets. Panelists are geographically dispersed and demographically balanced. Each panelist is assigned a projection factor, which enables purchases to be projectable to the entire US.

For each period, we calculate the total expenditures of each household on SNAP-eligible and SNAP-ineligible products. We classify goods as SNAP-eligible based on guidelines published by the FNS. The average SNAP-eligible goods-expenditure share across households is close to 60%. The advantage of using this measure as opposed to the nominal sales for each store is that demographic information for the household can be observed instead of county-level demographics for the store, such that we can use information on household income and household size to classify households as SNAP-eligible.[10] We also construct several measures of shopping intensities among households, following Stroebel and Vavra (2015). For each good purchased, the household records whether the good is purchased with coupons and if it is on sale. The barcode of the good is also scanned so the brand of the good can be observed. Therefore, we use three measures of shopping intensity: (1) the share of expenditures using coupons (coupon share), (2) the share of expenditures on goods that are on sale (deal share), and (3) the share of expenditures on generic store brands (store-brand share).[11]

10. We classify households as SNAP-eligible if their household incomes are 130% of the poverty line or below during both the event periods in 2008-2009.

11. To construct these variables in a way that controls for changing composition of the consumer basket, we follow HS by using the difference between actual and predicted shares for each household, predicting the shares using the behavior of other households consuming the same bundle.

2.3.5 Auxiliary Data

To construct control variables and measures of grocery industry concentration in addition to other measures of interest, we use a variety of auxiliary data sources, which we document in detail in Appendix Section 2.10.5.

2.4 Empirical Strategy

2.4.1 Empirical Specifications

The discussion in Section 2.3 makes it clear that, despite the SNAP formula being set at the federal level, states can introduce idiosyncratic variation to the benefit series. To estimate the impact of SNAP benefits on prices, we first apply typical panel fixed-effects approaches. Our research design exploits variation in magnitudes of benefit increases across states and events, because benefit increases mostly occur at the same time across states due to federal implementation of the Farm Bill and the ARRA. While both price indices at the state and store level can be constructed, we report store-level regressions because more information is available on store type and geographic location, although results are robust at the state level. In addition, we report results using monthly price indices to take advantage of the sharp timing of the events. In our preferred specification, the log of outcome Y_{it} for store i in state s is regressed on state-level log benefits per population B_{st} for the store-year-month panel with store and period fixed effects to control for unobserved store characteristics and common period trends that affect prices, as shown in equation (2.5):

$$\ln Y_{it} = \alpha + \beta \ln B_{st} + X'_{it}\gamma + \alpha_i + \alpha_t + \varepsilon_{it}. \tag{2.5}$$

Y_{it} are outcomes such as the store price index or store real sales. Because the level of the price index is not interpretable, only relative changes are relevant. The log-log specification gives the interpretation of β as the elasticity of prices (or real sales) with respect to SNAP benefits per population. We also include control variables X_{it} matched to the store's location, such as log state housing price, log county unemployment rate, log county average wages and log county population. Stroebel and Vavra (2015), Beraja, Hurst and Ospina (2015), and Handbury and Weinstein (2015) show that these variables have impacts on regional prices. Other controls include average gross income and rent of SNAP recipients in each state, state tobacco taxes, and energy prices and quantities consumed by fuel type. Standard errors are clustered by state to allow for autocorrelation in unobservables within states because the identifying variation is at the state level, following Bertrand, Duflo and Mullainathan (2004).

Second, we use a distributed-lag model to estimate a cumulative impulse response function starting 12 months prior to impact, which allows us to check whether the pre-trends are parallel. If the pre-trend is flat and the impulse response exhibits sharp timing, we can infer that the benefit series, after controls, is largely exogenous to unobservable trends that are determining prices prior to impact, suggesting the unobservable trends may also be parallel after impact. The distributed lag model provides a useful falsification test, and is shown in equation (2.6):

$$\ln Y_{it} = \alpha + \sum_{j=-k}^{k} \beta_j \ln B_{s,t-j} + X_{it}'\gamma + \alpha_i + \alpha_t + \varepsilon_{it}. \tag{2.6}$$

We can obtain the cumulative effect by adding together all the coefficients. Whereas the standard cumulative effect includes only the sum of the contemporaneous effect and all the lag coefficients, the lead coefficients are added as well because benefit changes are often announced ahead of time and anticipatory changes in prices could occur. For example, the Farm Bill was passed four months before its implementation while the ARRA was announced two months before its implementation.

Third, we estimate a triple-difference model at the store level to examine whether the estimated interaction coefficients are consistent with hypotheses predicted by theory. In our triple-difference regression, we interact log benefit per capita measured at the state level with the number of recipients per population measured at the county level in 2006. Intuitively, the interaction coefficient should be positive and statistically significant, because a higher share of recipients means that the recipient population figures more importantly in that county's composition of demand. On the contrary, if the contamination by recessionary forces was severe and if this influence was stronger in counties with higher shares of recipients, as suggested by previous literature, we would expect the negative bias contributed by recessionary forces to attenuate the interaction coefficient between benefits per population and recipient share. This specification is shown in equation (2.7):

$$\ln Y_{it} = \alpha + \beta_1 \ln B_{st} + \beta_2 \ln A_{it} + \beta_3 \ln B_{st} \times \ln A_{it} + X_{it}'\gamma + \alpha_i + \alpha_t + \varepsilon_{it}. \tag{2.7}$$

For example, A_{it} is the SNAP participation ratio in each county in the pre-period of 2006, which allows us to exploit county-level variation in the interaction variables.

Fourth, we implement a quadruple-difference model at the household level. In analyzing the consumer panel data, we are able to exploit the richness of the data to further relax the identifying assumptions. Specifically, we expect a rise in SNAP benefits to have the largest effect on SNAP-eligible goods consumed by SNAP-eligible households. Neoclassical theory predicts that if households are inframarginal in food spending, they will treat benefit increases as cash and non-SNAP spending could rise. Nevertheless, HS find that most households are inframarginal and have a low MPC for SNAP-ineligible goods relative to SNAP-eligible goods, and also argue that mental accounting is another channel that could raise MPC out of SNAP. We test these results by the following specification in equation (2.8). Each observation is a household-product group-period and classified into one of four groups based on whether a household is SNAP-eligible as well as whether the expenditures are on products categorized as SNAP-eligible or not. We regress (log) expenditures of household i

in group g in period t on the (log) benefits per participant in state s that household i resides in period t, and interact this variable with four group indicators T_g. Household-month-of-the-year fixed effects are included to control for household-specific characteristics that may vary seasonally, along with group fixed effects and period fixed effects:

$$\ln Y_{igt} = \alpha + \sum_{g=1}^{4} \beta_g \times T_g \times \ln B_{st} + X_{it}'\gamma + \alpha_{im} + \alpha_g + \alpha_t + \varepsilon_{igt}. \tag{2.8}$$

This specification allows us to provide supporting evidence that the SNAP-benefit shocks are exogenous to unobservable recessionary shocks and other contextual factors, because unobservable shocks that affect spending are unlikely to differentially impact only SNAP-eligible households and SNAP-eligible product groups. The level-level specification, as opposed to the log-log specification, provides an estimate of the MPC of a SNAP household. The more closely in line the estimated coefficient is with past MPC estimates of SNAP benefits on SNAP-eligible products (or ineligible products), the more support this finding lends to the credibility of our overall estimates.

Fifth, we estimate IV regressions for each of the specifications discussed above. We construct the instrument, denoted as synthetic benefits per population, as illustrated in Section 2.3.2. For each of the specifications discussed above, we use the synthetic benefits per population as an IV for the actual benefits per population.

2.4.2 Graphical Evidence

To motivate and substantiate our empirical strategy, we present some graphical evidence that suggests the identifying assumption of parallel trends is satisfied. Figure 2.10 plots the revenue-weighted average grocery store price indices by the quantile of change in residualized log synthetic benefits per population, our IV, for states above and below the median, respectively. To obtain these quantiles, a state-month panel of log synthetic benefits per population is regressed on the set of control variables as well as state and period fixed effects, then the total change in the residualized log synthetic benefits per population during the Farm Bill and the ARRA for each state is used to separate the states into quantiles. The first quantile denotes the 24 states with larger changes in residualized log synthetic benefits per population and the second quantile denotes the 24 states with smaller changes. The figure shows the pre-trends are parallel and the price indices, which are normalized to 1 in January 2006, diverge just around the Farm Bill and become roughly parallel again after the ARRA, implying price growth in states with larger changes in synthetic benefits per population is around 2%-3% larger than states with smaller changes. Likewise, we plot these figures for population-weighted averages of housing prices and unemployment rates in Figure 2.11 to check how observable economic covariates vary across quantiles. Both housing-price and unemployment-rate trends look nearly identical across the two quantiles, implying the variation generated by the IV is largely uncorrelated with observable economic variables.[12]

2.5 Main Results

In this section, we present the main empirical evidence on how SNAP-benefit increases affect retail stores and their consumers. We first focus on results using the retail scanner data and then discuss our findings using the consumer panel data. All results use data from 2007-2010 only to focus on the period around the Farm Bill and the ARRA where substantial variation exists in SNAP-benefit disbursement, although our results are robust to using the entire sample period.

2.5.1 Retail Stores

We estimate the contemporaneous effect of SNAP-benefit increases on prices and real sales as shown in equation (2.5). We first show the estimated price effects for grocery stores in Table 2.2. Without controls, the OLS estimate is statistically insignificant because the

12. In fact, the slight divergence in trends implies the states with smaller changes in the IV have slightly better economic outcomes, creating, if any, a slightly negative bias in our results.

extensive margin is counter-cyclical, generating a negative bias on the coefficient, and the IV is also weak without controls. We argue that without controls, the benefits-per-population series exhibits large and countercyclical variation due to movements in the extensive margin (SNAP participation rate), whereas our IV only exploits policy variation in the intensive margin (benefits per participant) for the baseline sample. Economic variables such as housing prices explains variation in the SNAP participation rate and helps increase the relevance of the IV. When a set of economic variables such as state-level housing prices are included to control for recessionary shocks, the effect of raising SNAP benefits is positive and significant. Results are robust to the further addition of controls such as the state-level tobacco tax, which strongly affected tobacco prices over this period, economic characteristics of SNAP recipients, and energy quantities consumed and prices, all of which affect benefits per recipient as shown in Section 2.3 and Appendix Section 2.10.2.1.

Regarding our IV approach, the first-stage F-test passes the standard thresholds benchmarked in the literature. We also show in Figure 2.12 that the changes in the synthetic-benefit series matches changes in the actual series closely. Using the IV approach further increases the magnitude and precision of the point estimate. The size of the coefficient is larger by almost a factor of four, suggesting that at least the direction of the OLS coefficient is credible and, if anything its magnitude is being somewhat underestimated. A 10% increase in benefits per population increases prices by about 0.8%. Because the average change in benefits was about 15% for the two events, we can infer that prices were raised by 1.2% in each event. Given that national and grocery store inflation was around 2% over this period, these effects are economically substantial. On the other hand, the impact of increased SNAP benefits on real sales is statistically insignificant as shown in Table 2.3. Figure 2.13 plots the observations (collapsed into 50 bins) used in the estimation in a binned scatter plot, which shows the data support a log-log specification with most of the observations lying close to the regression line.

In addition, we conduct a series of robustness checks in Table 2.4 to show the estimated price response is robust to weighting by store revenue or county population, regressing with state-level observations, using county-level benefits, using the full sample from 2006 to 2015, which includes recovery periods in which few changes occur in SNAP-benefit disbursement, removing SNAP disaster payments, and including a set of government transfer policy controls at the state-year level. We also show our results are robust to using alternative price indices in Table 2.5. The construction methods for these indices are illustrated in Appendix Section 2.10.4.

Next, we estimate equation (2.6) and plot the cumulative effects of the benefit increase in Figure 2.14a. We show that pre-trends are parallel over the entire pre-period of 12 months. Prices rise sharply when benefits rise, and continue to rise after the event. We also plot the cumulative effects of an increase in synthetic benefits to understand the IV variation in Figure 2.14b. Likewise, the pre-trends are parallel for most of the pre-period, although prices begin to rise slightly before the event. Because the Farm Bill was announced four months before its implementation, this finding could be consistent with announcement effects found in Agarwal and Qian (2014).

Furthermore, we expect the effect of SNAP-benefit changes will be stronger for stores located in regions with higher SNAP participation rates. We interact county-level SNAP participation per population, which is fixed at its pre-period value in 2006, with log benefits per population as shown in equation (2.7). Results are shown for both OLS and IV specifications in Table 2.6. The interaction coefficient is positive and significant for both prices and real sales. Increasing the SNAP participation ratio from the 10th percentile of 2.8% to the 90th percentile of 13.7% increases the price elasticity by about 0.014 and the real-sales elasticity by about 0.034. The price response to SNAP-benefit changes is also stronger in counties with higher concentration measures, a proxy for market power, as shown in Table 2.7. We use two different county-level measures of concentration in the pre-period: the number of grocery establishments per population and the HHI in the grocery industry, using the number of employees as a measure of firm size. We provide theoretical derivations in Section

2.6 to show that higher market power leads to a stronger subsidy pass-through for many standard parameterizations of demand.

Given that SNAP is only eligible for food items, we also present IV results by product eligibility. Although the effect of SNAP-benefit increases should be strongest in food because SNAP benefits cannot be used to buy goods from other product departments, we have several reasons to expect otherwise.

First, as mentioned above, previous literature investigating whether consumers consider SNAP benefits fungible has estimated the MPCF out of SNAP to be at most 0.6, which is below 1. Hence, SNAP participants might increase their purchases of other items, depending on their MPC for SNAP-ineligible products; given that previous literature has found MPC for ineligible products to be much lower than MPC for food, we expect the effect on SNAP-ineligible goods to be small as well.

Second, previous literature studying the behavior of multi-product retailers argues that firms maximize profits across product categories, such that shocks to one product category may transmit to others. Table 2.8 illustrates that the positive effect on food prices is strongly significant, whereas the effect on food real sales is positive but statistically insignificant. The effect on prices in ineligible products is also positive and statistically significant, with a magnitude similar to eligible products, but the real-sales response is negative and statistically insignificant. These results would be consistent with increased retail markups in other product categories as a result of lower demand elasticities of SNAP consumers for both eligible and ineligible products due to the their increased disposable income.

To explore this mechanism further, we estimate equation (2.7) by product eligibility in grocery stores. Table 2.8 provides further evidence that food is the main product department with a positive demand shock caused by SNAP-benefit hikes. The effects on both prices and real sales become stronger in high-SNAP-participation regions, whereas evidence also suggests the interaction effect for prices is also higher in other product departments but the interaction effect for real sales is negative although statistically insignificant, lending additional support to the hypothesis that to raise profits, multi-product retailers increase markups in other product departments when SNAP benefits increase. We show analogous results sorting by five product departments in Appendix Tables 2.11 and 2.12. We further discuss multi-product pricing in Section 2.6.5 and show our results are consistent with a model of multi-product pricing. In addition, we show in Table 2.14 that results are statistically insignificant and smaller for both drug and merchandise stores. As mentioned above, these store types adopt national chain pricing and have negligible response to local shocks as shown in DellaVigna and Gentzkow (2017) and Leung (2018). I repeat the approach and results in Leung (2018) in Appendix Section 2.10.6.

In Appendix Section 2.10.7, we present results on the effects of SNAP-benefit changes on firm dynamics and market structure in the grocery industry. The estimated effect on entry and exit is positive, but almost all of the estimates are statistically insignificant and economically small, suggesting SNAP-benefit changes had little impact on entry and exit margins and market structure.

2.5.2 Households

To understand how consumption and shopping behavior responds across SNAP-eligible and -ineligible households, as well as how each type of household responds in SNAP-eligible and -ineligible product groups, we estimate equation (2.8) and present the results in Table 2.9. For expenditures, these results show a statistically significant MPC only for the both-eligible group, and the point estimate is positive and large despite somewhat large standard errors. The implied MPC is 0.44, which is close to previous estimates of MPC out of food stamps of 0.5-0.6 from HS. Note that our estimate of MPC also captures price responses whereas the estimate in HS should not, because their variation comes from individuals transitioning into and out of SNAP. Netting out the price response, our estimates of MPC would be slightly smaller. The MPC estimated from the both-eligible group is statistically larger than those from other groups. This finding suggests the state variation we are using is not driven by unobservable recessionary shocks but plausibly exogenous SNAP-benefit shocks that differentially impact SNAP-eligible goods and households. The point estimates from the product- or household-eligible-only groups are quite large although statistically insignificant. This finding is suggestive of both increased prices raising food expenditures of the ineligible households and increased consumption of ineligible products among eligible households.

Furthermore, following Goldin, Homonoff and Meckel (2016), we estimate the response of consumption by week of the month. Whereas they show the average consumption patterns follow state-specific SNAP-benefit-issuance schedules, we show the MPC is also higher in the weeks in which more SNAP benefits are issued to households, by interacting each eligibility indicator with the fraction of SNAP benefits issued in a given week by state. As shown in Table 2.16, the interaction coefficient for the MPC is positive and strongly significant for eligible households in both product groups but is higher for eligible products, and the difference is statistically significant. As suggested by Goldin, Homonoff and Meckel (2016) and Baker, Johnson and Kueng (2017), consumption is complementary for both eligible and ineligible product groups due to fixed shopping-trip costs. Even the percentage increase in consumption during weeks with higher SNAP issuance is positive but marginally insignificant despite the higher base levels of consumption in weeks with higher SNAP issuance. On the other hand, the MPC does not vary with SNAP-issuance schedules for ineligible households.

We also investigate the effect of increased benefits on shopping behavior, using the outcomes described in Section 2.3, which include coupon share, deal share, store brand share. We also include the number of shopping trips taken. In Table 2.17, we show the results are mixed and inconclusive, and the point estimates all imply the response to a 1% increase in benefits is less than 0.001. HS find that coupon share and store-brand share decrease as SNAP benefits increases, but the response is small economically.

We present results during the 2013 ARRA expiration in Tables 2.18 and 2.19. Suggestive but inconclusive results within 2013 show that SNAP-benefit decreases lowered store prices and household consumption, but these results are not robust. One of the difficulties in using this variation is that the average drop in benefits per population was only 8% during this period, with very little dispersion across states relative to the 2008 Farm Bill and the 2009 ARRA, especially for the IV as shown in Figure 2.6b, which leads to a weak first stage.

2.6 Theory

In this section, we first introduce a theoretical partial equilibrium framework to study the local incidence of increased SNAP benefits, first by graphical illustration in Section 2.6.1 and then by derivations based on adaptations of Weyl and Fabinger (2013) in Section 2.6.2 and Section 2.6.3. We then use our reduced-form estimates as sufficient statistics to calculate incidence under a range of assumptions about relevant parameters in Section 2.6.4. In Section 2.6.5, we also derive multi-product versions of these formulas and give explanations for why SNAP-ineligible products also increase in prices.

2.6.1 Graphical Illustration

In this subsection, we introduce a theoretical partial equilibrium framework following Weyl and Fabinger (2013) to study the local incidence of changes in SNAP benefits. First, we illustrate the framework graphically under perfect competition and monopoly.

The setup of the theoretical framework is analogous to that for a consumer unit subsidy by the government given to a specific product in partial equilibrium. We first graphically illustrate the case under perfect competition in Figure 2.15. Consider the market-demand curve D for SNAP-eligible products in the retail sector, which is obtained by the horizontal summation of demand of SNAP recipients D^S and non-SNAP recipients D^{NS}. Equilibrium price P and quantity Q are obtained by the intersection of demand D and supply S. The government then disburses SNAP benefits to recipients, shifting the demand curve of SNAP recipients up to $D^{S'}$. Equilibrium price increases to P' and quantity increases to Q' by the intersection of new market demand D' and supply S. We now evaluate the changes in welfare as a result of SNAP. The increase in price and quantity leads to increased producer surplus of $P'CDP$. Assuming either no income effects or a parallel shift in demand, we evaluate the welfare of SNAP recipients under their old demand curve D^S. SNAP recipients now act as if they face lower prices of $P^{S'}$ and quantity demanded increases to $Q^{S'}$ due to the disbursement of benefits, and hence their consumer surplus increases by $PGFP^{S'}$. On the other hand, non-SNAP consumers now face a higher price P', and their consumer surplus decreases by $P'BEP$. To consider the deadweight loss of the program, we first observe that the loss in non-SNAP consumer surplus $P'BEP$ directly transfers to a gain in producer surplus. The remaining producer surplus $BCDE$ from SNAP consumers is equal in area to $P'AGP$. The cost of SNAP benefits to the government is $P'AFP^{S'}$, which implies the deadweight loss of the program is GAF. This deadweight loss is a result of SNAP consumers buying marginal units they value at less than the marginal cost for producers.

Next, we graphically illustrate the case under monopoly in Figure 2.16. Likewise, the market-demand curve D for SNAP-eligible products is obtained by the horizontal summation of demand of SNAP recipients D^S and non-SNAP recipients D^{NS}. Equilibrium price P and quantity Q are now obtained by the intersection of marginal revenue MR that is obtained from demand D and marginal cost given by supply curve S. We now assume marginal cost is constant based on evidence on retailers by Stroebel and Vavra (2015). If the supply curve is flat under perfect competition, the price response is zero. On the other hand, we illustrate that the price response could be large with market power. The government then disburses SNAP benefits to recipients, shifting the demand curve of SNAP recipients up to $D^{S'}$. Equilibrium price increases to P' and quantity increases to Q' by the intersection of new marginal-revenue curve MR' and supply S. We now evaluate the changes in welfare as a result of SNAP. The increase in price and quantity leads to increased producer surplus of $P'CDEFP$. SNAP recipients now act as if they face lower prices of $P^{S'}$ and quantity demanded increases to $Q^{S'}$ due to the disbursement of benefits, and hence their consumer surplus increases by $PMJP^{S'}$. On the other hand, non-SNAP consumers now face a higher price P', and their consumer surplus decreases by $P'BGP$. To consider the deadweight loss of the program, we first observe that the loss in non-SNAP consumer surplus $P'BGP$ directly transfers to a gain in producer surplus. The remaining producer surplus $BCFG$ from SNAP consumers is equal in area to $P'AMP$. The cost of SNAP benefits to the government is $P'AJP^{S'}$, which implies part of the deadweight loss of the program is MAJ. However, this deadweight loss is offset by the change in producer surplus $FCDE$. To better understand the change in total surplus, note that $FCDE$ is identical in area to $MAKL$ minus $BGHI$. Therefore, the change in total surplus of $FCDE$ minus MAJ is equivalent to $MJKL$ minus $BGHI$. In other words, the change in deadweight loss of the program is given by the decrease in deadweight loss when the monopolist sells more to SNAP consumers and the increase in deadweight loss when the monopolist further restricts output to non-SNAP consumers.

2.6.2 *Pass-through Formulas*

Next, we adapt pass-through formulas derived in Weyl and Fabinger (2013) to analyze the price response to a demand shock. Let market demand for SNAP-eligible goods $Q^D(p,b)$ be a function of prices and SNAP benefits per population and supply $Q^S(p)$ be a function of prices. Market demand is composed of demand by SNAP recipients $Q^{D,S}(p,b)$ and non-SNAP recipients $Q^{D,NS}(p)$. Note the latter does not depend on benefits per population. Beginning with the case of perfect competition, pass-through formulas can be derived from the equilibrium condition as illustrated below.

$$Q^D(p,b) = Q^{D,S}(p,b) + Q^{D,NS}(p) = Q^S(p)$$

$$\varepsilon_\rho \equiv \frac{dp}{db}\frac{b}{p} = \frac{\frac{\varepsilon_{Q^{D,S},b}}{-\varepsilon_D}\frac{pQ^{D,S}}{pQ}}{1-\frac{\varepsilon_S}{\varepsilon_D}}. \tag{2.9}$$

We show all steps of the theoretical derivations in Appendix Section 2.10.8. As mentioned above, these formulas are extensions of unit subsidy pass-through. The unit-subsidy pass-through elasticity can be written as the pass-through rate $\frac{1}{1-\frac{\varepsilon_S}{\varepsilon_D}}$ multiplied by 1, whereas the pass-through elasticity of benefits ε_ρ is written as the pass-through rate multiplied by this new term composed of the MPC out of SNAP in elasticity form $\varepsilon_{Q^{D,S},b}$ divided by the absolute value of the demand elasticity ε_D, which is then multiplied by the proportion of sales accounted for by SNAP recipients. Note the subsidy pass-through rate is 1 minus the cost pass-through rate $\frac{1}{1-\frac{\varepsilon_D}{\varepsilon_S}}$, frequently estimated in the pass-through literature. An incomplete-cost pass-through, that is, a cost pass-through rate below 1 is typically found in the literature, which implies a positive-subsidy pass-through rate below 1.

Likewise, we can derive the pass-through formula under symmetric imperfect competition. First, we start from the profit-maximization condition and differentiate it with respect to the amount of benefits. In addition, we allow the demand elasticity to depend on the amount of benefits. We obtain an expression for the quantity response to benefits:

$$P(Q,b) + \theta \frac{\partial P(Q,b)}{\partial Q} Q - c'(Q) = 0$$

$$\frac{dQ}{db} = -\frac{\theta \frac{\partial^2 p}{\partial b \partial Q} Q + \frac{\partial p}{\partial b}}{\frac{\partial MR}{\partial Q} - \frac{\partial MC}{\partial Q}}. \tag{2.10}$$

Next, we use the above expression to obtain the pass-through formula:

$$\frac{dp}{db} = \frac{\partial p}{\partial Q}\frac{dQ}{db} + \frac{\partial p}{\partial b}$$

$$\varepsilon_\rho \equiv \frac{dp}{db}\frac{b}{p} = \left(1 - \frac{1}{1 + \frac{\theta}{\varepsilon_\theta} + \frac{\varepsilon_D - \theta}{\varepsilon_S} + \frac{\theta}{\varepsilon_{ms}}}\right)\frac{\varepsilon_{Q^{D,S},b}}{-\varepsilon_D}\frac{pQ^{D,S}}{pQ} + \left(\frac{1}{1 + \frac{\theta}{\varepsilon_\theta} + \frac{\varepsilon_D - \theta}{\varepsilon_S} + \frac{\theta}{\varepsilon_{ms}}}\right)\frac{\theta}{-\varepsilon_D}\varepsilon_{p',b}. \tag{2.11}$$

The pass-through elasticity now consists of two main terms. The first term, which we denote as the shift effect, is the same term from the pass-through formula under perfect competition with a different pass-through rate. The second term, which we denote as the slope effect, is 1 minus the subsidy pass-through rate multiplied by the market conduct parameter, the inverse demand elasticity, and the elasticity of the slope of the inverse demand with respect to benefits, which reflects how much the demand elasticity changes as benefits increase. An increase in the slope elasticity increases the pass-through elasticity because a benefit increase now leads to a larger increase in the absolute value of the inverse demand elasticity, which raises markups.

Therefore, extending these pass-through formulas from perfect competition to monopoly and symmetric imperfect competition only varies the pass-through rate and also adds a slope-effect term. Assuming a range of pass-through rates, we can check whether the estimated pass-through elasticity in Section 2.5.1 is comparable to what would be predicted using these formulas by utilizing the MPC in elasticity form estimated in Section 2.5.2 and from HS as well as the demand elasticity for SNAP-eligible products and the proportion of SNAP sales.

Furthermore, as shown in Table 2.7, the price response is stronger in counties with higher measures of concentration, which we regard as proxies for market conduct. This result is consistent with Cabral, Geruso and Mahoney (2018), who provide empirical evidence that the pass-through of a producer subsidy is lower when market conduct is higher. Because the pass-through rate of a consumer subsidy is 1 minus the producer subsidy pass-through rate, the consumer subsidy pass-through is higher when market conduct is higher, implying a larger price response. Cabral, Geruso and Mahoney (2018) also show that theoretically, the pass-through rate is lower when market conduct is higher under the assumption that market conduct is constant for standard parameterizations of demand. Because we also find that market conduct does not respond significantly to benefit changes in Appendix Section 2.10.7, our empirical results are consistent with theoretical derivations.

2.6.3 Incidence

Given the intuition provided by the graphical illustrations, we now derive the changes in consumer and producer surplus of the program. Using the original inverse market-demand curve $P(Q)$, the change in consumer surplus from an increase in SNAP benefits can be written as follows:

$$
\begin{aligned}
CS(p) &= \int_{P(Q(p))}^{\infty} Q^D(x)dx \\
\frac{dCS}{db} &= \left(\frac{\varepsilon_{Q^{D,S},b}}{-\varepsilon_D} - \varepsilon_\rho\right)\frac{pQ^{D,S}}{b} + \left(- \varepsilon_\rho\right)\frac{pQ^{D,NS}}{b}.
\end{aligned} \tag{2.12}
$$

$Q^D(x)$ is the demand function. Starting from the aggregate market demand, one can decompose the change in consumer surplus into that for SNAP consumers and non-SNAP consumers. This expression is the same under different assumptions about the market structure.[13] Under perfect competition, the change in producer surplus can be written as follows:

$$
\begin{aligned}
PS(p) &= \int_0^p Q^S(x)dx \\
\frac{dPS}{db} &= \rho Q = \varepsilon_\rho \frac{pQ}{b}.
\end{aligned} \tag{2.13}
$$

We can extend our results to the case of symmetric imperfect competition following Weyl and Fabinger (2013). $\frac{dCS}{db}$ remains unchanged whereas the change in producer surplus can be rewritten as follows:

13. Note that if we allow the demand curve to shift out from the benefit increase rather than using the original demand curve, $\frac{dCS}{db} = -\rho Q + \int_p^\infty \frac{\partial Q^D(x,b)}{\partial b}dx = -\rho Q + \int_0^Q \frac{\partial P(x,b)}{\partial b}dx$. Hence assuming no income effects by using the original demand curve is equal to approximating the integral $\int_0^Q \frac{\partial P(x,b)}{\partial b}dx$ by $\frac{\partial P}{\partial b}Q$, that is, assuming $\frac{\partial P(x,b)}{\partial b}$ is constant.

$$
\begin{aligned}
PS &= (p-c)Q \\
\frac{dPS}{db} &= \left(\varepsilon_\rho + \frac{p-c}{p}\frac{dQ}{db}\frac{b}{Q}\right)\frac{pQ}{b} \qquad (2.14)\\
&= \left[\varepsilon_\rho + \theta\left(\frac{\varepsilon_{Q,b}}{-\varepsilon_D} - \varepsilon_\rho\right)\right]\frac{pQ}{b}. \qquad (2.15)
\end{aligned}
$$

θ is the elasticity-adjusted market-conduct parameter defined in Weyl and Fabinger (2013) and can be estimated using data on markups. Therefore, the change in producer surplus can be obtained from either equation (2.14) using retail margins from the literature and our estimate of real-sales response or equation (2.15) under different assumptions about the market-conduct parameter.

2.6.4 Calibration

Given these formulas provided by our theoretical framework, we now utilize reduced-form estimates to calibrate the pass-through elasticity, and compare the calibrated elasticity with the reduced-form estimate. We also calculate local incidence. First, consider the market for SNAP-eligible goods. We take estimates of the MPC from Section 2.5.2 and from HS. We multiply their MPC estimate by 0.9, which is approximately the mean of SNAP benefits to food spending in the CEX, to obtain an elasticity. We also obtain demand elasticities by regressing log real sales on log price indices for both SNAP-eligible and -ineligible goods with store and period fixed effects.[14] Estimates of demand elasticities using panel variation may be endogenous, but both DellaVigna and Gentzkow (2017) and Hitsch, Hortacsu and Lin (2017) estimate product-level elasticities using panel variation and find these estimates are similar to those obtained using alternative methods. We follow Hitsch, Hortacsu and Lin

14. Note that this approach estimates the residual demand elasticity for the store as opposed to the market demand elasticity, which is the object of interest. However, the absolute value of the residual demand elasticity is likely an upper bound for the absolute value of the market demand elasticity. Hence, the calibrated shift effect is likely an underestimate.

(2017) and use 3-digit zip-code-period fixed effects, which give similar estimates to just using period or market-period fixed effects. Our estimated demand elasticity for food is similar to those in the literature as summarized by Andreyeva, Long and Brownell (2010). We obtain the proportion of sales accounted for by SNAP recipients from USDA data.

We show our calculations in Table 2.10 under different assumptions about market conduct. First, we compare our estimated pass-through elasticity with the one predicted by equation (2.34) assuming a pass-through rate of 1, which is the magnitude of the shift effect. Our estimated pass-through elasticity is 0.07, which is smaller than the predicted pass-through elasticity of 0.08 under a subsidy pass-through rate of 1, which we denote as the shift magnitude. This result implies the subsidy pass-through rate is below 1, consistent with incomplete-cost pass-through. In addition to markup adjustments due to demand curvature, that is, the shift effect, direct changes in markups due to decreased demand elasticities as income increases could also further raise prices, that is, the slope effect. Alternatively, we also use the MPC estimate in HS, which is larger and has a very small standard error. The shift magnitude is now 0.12, which is again larger than our estimated price response. We discuss how we can obtain an estimate of the pass-through rate of around 0.49 by estimating the curvature of demand in Appendix Section 2.10.9. This pass-through rate is roughly consistent with Besanko, Dubé and Gupta (2005). All these estimates imply a calibrated price response of 0.04 and 0.06, respectively under an MPC elasticity of 0.34 and 0.52. Hence, our reduced-form estimate of the price response of 0.07 is slightly larger than the theoretically calibrated price responses using only the shift effect, which implies a relatively small slope effect. We can also calibrate the quantity response, which is around 0.038 and close to our empirical estimate of 0.061.[15]

15.

$$\frac{dQ}{db} = \frac{\partial Q}{\partial p}\frac{dp}{db} + \frac{\partial Q}{\partial b}$$

$$\frac{dQ}{db}\frac{b}{Q} = \varepsilon_D \varepsilon_\rho + \varepsilon_{Q^{D,S},b}\frac{pQ^{D,S}}{pQ}$$

Plugging in our estimates of demand elasticity, pass-through elasticity, MPC elasticity, and the proportion

Next, we use both MPC estimates to calculate the change in surplus for producers, consumers (both SNAP and non-SNAP) per marginal dollar of SNAP disbursed under different assumptions about market conduct. Given that average retail markups are around 0.3 as shown in Hottman (2016), who shows a significant degree of market power in the retail sector, this markup implies a market-conduct parameter of roughly 0.16 under our demand-elasticity estimate. Focusing on the incidence when market conduct is 0.2, the increase in producer surplus takes up around 50%, whereas the increase for SNAP consumers is around 66%. These increases come at the expense of non-SNAP consumers whose loss is close to 36% of that marginal dollar of SNAP, which is about 6% per non-SNAP consumer. Only the increase in producer surplus varies under different market-conduct parameters, with magnitudes ranging from 0.4 to 0.7. Using the smaller MPC estimate, we find the gain is now smaller for SNAP consumers. Given the large standard errors on our MPC estimate but tight confidence intervals around both the price response and demand elasticities, we prefer the calculations using the larger MPC estimate from HS. Overall, these results combined with intuition from our graphical illustrations imply that as SNAP benefits increase, producers are major beneficiaries because they are able to raise prices to extract additional surplus, although SNAP benefits directly increase SNAP consumer surplus the most.

Several points are worth noting regarding our incidence calculations. First, because the MPCF out of SNAP is not 1, some of the benefits disbursed leads to changes in consumption in other product markets. Hence, the changes in surplus calculated above apply only to the SNAP-eligible goods market, that is, the market for food. Because SNAP is a food-consumption safety-net program, this market is of particular interest for policy. Effects on other product markets could either increase or decrease both consumer and producer surplus, but the same mechanism for incidence should apply. We show in Appendix Table 2.21 the incidence for the ineligible goods market in grocery stores. Because ineligible goods account for only around 25% of revenue in grocery stores, the magnitudes of consumer- and

of SNAP sales, we obtain a calibrated response of 0.038.

producer surplus- changes are smaller. At the calibrated market conduct parameter of 0.5, a marginal dollar of SNAP benefits increases producer surplus by about \$0.1, decreases SNAP consumer surplus by about \$0.01, and decreases non-SNAP consumer surplus by about \$0.15, because grocery stores raise markups on ineligible goods, which we discuss further in the next subsection.

Second, our results take the MPCF out of SNAP as given and is agnostic to whether fungibility is violated. If we assume the MPCF out of SNAP is equal to the MPCF out of cash of around 0.1 as found in previous literature,[16] the calculated changes in surplus would be much smaller and changes in consumption in other product markets become more important. The price response under a subsidy pass-through rate of 1 would be around 0.02, whereas the changes in producer surplus and SNAP consumer surplus are around 0.1 for each marginal dollar of SNAP.

Third, if we account for the distortionary cost of taxation as in Hendren (2017) by using efficient welfare weights, surplus to SNAP consumers could be weighted 1.5-2x more than surplus to non-SNAP consumers who are richer. Likewise, assume producer surplus is redistributed to shareholders of retailers who are richer than SNAP consumers. Implementing these weights would raise the economic efficiency of SNAP-benefit changes.

2.6.5 Multi-product Pricing

To better understand the price and quantity movements seen in Table 2.8, we extend pass-through formulas to a multi-product setting. In the case of perfect competition, we rewrite the equilibrium condition with demand $Q_j^D(p)$ for good j as a function of a vector of prices $p = (p_1, ..., p_n)$ below:

16. HS use changes in gas prices to estimate a MPCF out of cash of no more than 0.1 and earlier literature has also found a range of estimates of about 0.03-0.17 ((Hoynes and Schanzenbach, 2009)).

$$Q_j^D(p,b) = Q_j^{D,S}(p,b) + Q_j^{D,NS}(p,b) = Q_j^S(p)$$

$$\varepsilon_{\rho_j} \equiv \frac{dp_j}{db}\frac{b}{p_j} = \frac{\frac{\varepsilon_{Q_j^{D,S},b}}{-\varepsilon_j^D}\frac{p_j Q_j^{D,S}}{p_j Q_j} + \sum_{i\neq j}\frac{\varepsilon_{j,i}^D - \varepsilon_{j,i}^S}{-\varepsilon_j^D}\varepsilon_{\rho_i}}{1-\frac{\varepsilon_j^S}{\varepsilon_j^D}}. \tag{2.16}$$

In the multi-product extension, the pass-through formula for a particular good contains an additional term: Assuming the supply elasticity is zero with respect to other goods, this term sums over all other goods the ratio of cross-price elasticities to own-price elasticity (the cross-good diversion ratio in elasticity form) multiplied by the pass-through elasticity of other goods. Intuitively, for goods that are complements, a larger increase in price for one good leads to lower demand for the other and hence a lower price, and vice versa for substitutes.

We estimate cross-price elasticities again using panel variation as in the previous subsection. Based on these estimates, an increase in SNAP-eligible-good prices raises demand for SNAP-ineligible goods, with a cross elasticity of 0.23. This finding implies a pass-through elasticity of 0.015 for SNAP-ineligible products, which is too small relative to the reduced-form estimate of 0.08. Given that the existing framework cannot fully rationalize the size of the price response, we provide two additional explanations for the large pass-through elasticity for SNAP-ineligible products. First, because SNAP benefits lead to a rise in disposable income for SNAP consumers, demand elasticities for SNAP-ineligible products could decrease, leading to an increased markup by retailers on ineligible goods through the slope effect. Second, we review past literature on multi-product pricing. These models focus on imperfect competition and markups. We apply several to our context by modeling SNAP benefits as decreasing demand elasticities of SNAP consumers for SNAP-eligible products, and possibly for SNAP-ineligible products as well because the MPCF out of SNAP is below 1.

Chen and Rey (2012) find that loss leading and cross-category pricing can arise when firms with broader product ranges exploit their market power to discriminate between consumers with heterogeneous shopping costs. Their model is driven by asymmetric market power across product departments and firms. A large firm competes with a smaller firm in one product department, but monopolizes another product department. We show in Appendix Section 2.10.10 that when the valuation for products in the competitive department increases, prices in the monopolized department will increase. This mechanism would explain our results if food (SNAP-eligible) were the competitive department and non-food products (SNAP-ineligible) face less competition, which is consistent with anecdotal evidence. Zhou (2014) finds the multi-product search effect can generate both loss leading and cross-category pricing in a multi-product sequential-search model. In his model, a rise in valuation for one good can lead to an increase in price for the other. Given that we find that prices rise but quantities fall for SNAP-ineligible products, this result is highly suggestive of increased markups for SNAP-ineligible products.[17]

2.7 Conclusion

In this paper, we find evidence that large increases in government transfer payments in the form of electronic benefits for food increase retail prices. Applying an instrumental variable approach that uses state-specific program adjustments, we estimate that a 1% increase in SNAP benefits per population raises grocery store prices by about 0.08%. Using this reduced-form estimate as a sufficient statistic in a theoretical partial equilibrium framework, we estimate that a marginal dollar of SNAP benefits increases producer surplus by about \$0.5, increases SNAP consumer surplus by about \$0.7, and decreases non-SNAP consumer surplus

17. Other models include Rhodes (2015), who highlights an alternative mechanism in which a firm's optimal advertising strategy can give rise to loss leading. Johnson (2017) points to a different mechanism that leads to loss leading in which asymmetric multi-product retailers compete for consumers who make unplanned purchases. Thomassen et al. (2017) provide empirical evidence by applying a novel demand model to UK consumer data and find that product categories are complements due to shopping costs. These models have different predictions on whether prices will rise together when one product category is shocked, and we leave a more detailed analysis of the implications of these models to future work.

by about \$0.4, or about \$0.06 per non-SNAP consumer. The price response is larger in regions with higher proportions of SNAP participants and higher market concentration. Furthermore, our estimates of MPCF out of SNAP are around 0.44 and are consistent with those from recent literature.

These findings have several policy implications. First, if the objective of SNAP is to guarantee a floor of *real* spending power on food, federal maximum benefits should be increased by about 10% to account for the price response. Second, enhanced competition in the grocery sector could improve the targeting properties of SNAP by shifting more surplus to SNAP consumers. Third, given that SNAP benefits per population rose by about 30% due to the Farm Bill and the ARRA, our findings suggest expansions in SNAP countered deflationary pressures by contributing to price hikes of 2.4% in grocery stores. Future variation in policy changes in non-recessionary periods would be useful for verifying the external validity of our results. Fourth, we interpret our findings using a partial equilibrium model of incidence. We find that while the intended beneficiaries, SNAP consumers, obtain more consumer surplus, the increased SNAP benefits also benefit producers at the expense of non-SNAP consumers. Fifth, increased SNAP benefits could raise total welfare and reduce deadweight loss due to the expansion of output by producers with market power. Sixth, the fungibility of SNAP benefits would lower the magnitude of the effects on the market for food, and fully capturing incidence would require a study of the effect on other product markets. However, the mechanisms we illustrate apply beyond in-kind transfers to cash transfer programs such as universal basic income. Seventh, our findings suggest residential segregation would shift consumer surplus generated by SNAP away from SNAP consumers, because SNAP consumers would be more likely to shop in stores with higher SNAP participation rates. Segregation in consumption baskets by income group would also generate a similar effect, because SNAP consumers would be more likely to buy goods that non-SNAP consumers do not. In ongoing work, we also study the impact of SNAP on other outcomes of interest such as labor and health.

2.8 Tables

Table 2.1: Decomposing variance in percentage changes in synthetic benefits per population, Farm Bill and ARRA

	(1)		(2)	
VARIABLES	% change in IV, FB	Covariance share	% change in IV, ARRA	Covariance share
Log average gross income	7.321***	0.268	6.412***	-0.0915
	(0.954)		(0.785)	
Log average rent	-0.925**	-0.0459	-1.409***	0.160
	(0.456)		(0.392)	
Log average household size	4.758	-0.0812	2.666	0.0806
	(2.925)		(1.736)	
Share elderly/disabled	6.222***	0.0811	0.479	-0.00265
	(1.960)		(1.442)	
Share homeless	16.63	-0.00388	1.655	-0.00183
	(10.61)		(7.429)	
Log SUA	-1.849***	-0.0520	-2.846***	0.712
	(0.276)		(0.247)	
% change in SUA	0.179***	0.770	-0.000765	0.00323
	(0.0148)		(0.00986)	
Residual		0.0637		0.140
Observations	48		48	
R-squared	0.936		0.860	
Prob > F	0.000		0.000	

Notes: Robust standard errors are in parentheses, clustered by state. *** p<0.01, ** p<0.05, * p<0.1. % change in IV refers to the percentage change in synthetic benefits per population, FB refers to the Farm Bill in 2008m10, ARRA refers to the American Recovery and Reinvestment Act in 2009m4. Covariance share refers to the share of the variance in the outcome explained by each variable. All variables are measured in 2006 except for two variables. Log standard utility allowance (Log SUA) is measured the month before the event of interest, and the percentage change in the standard utility allowance (% change in SUA) is measured at the Farm Bill only because the SUA did not change during the ARRA.

Table 2.2: Effect of SNAP-benefit changes on prices

Specification	(1) OLS	(2) IV	(3) OLS	(4) IV	(5) OLS	(6) IV	(7) OLS	(8) IV	(9) OLS	(10) IV
VARIABLES	Log price index									
Log benefits per population	0.00397	0.232	0.0384*	0.148***	0.0240*	0.118***	0.0235*	0.118***	0.0211**	0.0820***
	(0.0177)	(0.165)	(0.0197)	(0.0267)	(0.0123)	(0.0300)	(0.0127)	(0.0295)	(0.00951)	(0.0234)
Log housing price			0.0681***	0.118***	0.0641***	0.106***	0.0652***	0.107***	0.0474***	0.0752***
			(0.0169)	(0.0190)	(0.0147)	(0.0171)	(0.0151)	(0.0172)	(0.0115)	(0.0124)
Log unemployment rate			0.00146	-0.00802	0.00304	-0.00518	0.00306	-0.00493	0.0000128	-0.00383
			(0.00391)	(0.00619)	(0.00391)	(0.00599)	(0.00381)	(0.00573)	(0.00244)	(0.00355)
Log population			-0.131**	-0.146**	-0.119*	-0.136**	-0.119**	-0.134**	-0.132***	-0.144***
			(0.0652)	(0.0648)	(0.0596)	(0.0627)	(0.0588)	(0.0618)	(0.0383)	(0.0391)
Log average wage			0.0122**	0.00755	0.0118**	0.00830	0.0118**	0.00852	0.00930*	0.00748
			(0.00557)	(0.00559)	(0.00525)	(0.00544)	(0.00537)	(0.00540)	(0.00552)	(0.00555)
Log tobacco tax					0.0134***	0.00741***	0.0135***	0.00740***	0.00826***	0.00519***
					(0.00236)	(0.00268)	(0.00235)	(0.00265)	(0.00158)	(0.00180)
Log SNAP average gross income							-0.00428	0.00382	0.00133	0.00634
							(0.00898)	(0.0139)	(0.00472)	(0.00851)
Log SNAP average rent							-0.00300	-0.00512	-0.00348	-0.00490*
							(0.00333)	(0.00369)	(0.00269)	(0.00291)
Observations	382560	382560	382560	382560	382560	382560	382560	382560	382524	382524
R-squared	0.884	0.771	0.893	0.874	0.897	0.884	0.897	0.884	0.904	0.899
Prob > F	0.824	0.010	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Number of units	7970	7970	7970	7970	7970	7970	7970	7970	7970	7970
Number of clusters	48	48	48	48	48	48	48	48	48	48
First stage F-stat		2.578		17.857		20.655		20.682		20.194
Energy controls									X	X

Notes: Robust standard errors are in parentheses, clustered by state. *** p<0.01, ** p<0.05, * p<0.1. Store and period fixed effects are included. Energy controls refer to annual state-level total energy, electricity, LPG, and natural gas quantities consumed and prices for the residential sector.

Table 2.3: Effect of SNAP-benefit changes on real sales

Specification	(1) OLS	(2) IV	(3) OLS	(4) IV	(5) OLS	(6) IV	(7) OLS	(8) IV	(9) OLS	(10) IV
VARIABLES	Log real sales									
Log benefits per population	-0.0702*	0.0318	0.0386	-0.0661	0.0508	-0.0448	0.0484	-0.0402	0.0617*	0.00597
	(0.0404)	(0.150)	(0.0360)	(0.0866)	(0.0363)	(0.108)	(0.0355)	(0.106)	(0.0324)	(0.109)
Log housing price			0.0707	0.0228	0.0740*	0.0316	0.0749*	0.0357	0.0665	0.0412
			(0.0446)	(0.0518)	(0.0435)	(0.0553)	(0.0439)	(0.0564)	(0.0403)	(0.0566)
Log unemployment rate			-0.144***	-0.135***	-0.146***	-0.137***	-0.146***	-0.139***	-0.141***	-0.137***
			(0.0288)	(0.0298)	(0.0289)	(0.0307)	(0.0292)	(0.0306)	(0.0288)	(0.0297)
Log population			-0.163	-0.149	-0.173	-0.156	-0.176	-0.162	-0.153	-0.142
			(0.157)	(0.165)	(0.153)	(0.166)	(0.153)	(0.165)	(0.117)	(0.126)
Log average wage			-0.00170	0.00271	-0.00138	0.00219	-0.00196	0.00112	0.000677	0.00234
			(0.0392)	(0.0395)	(0.0390)	(0.0394)	(0.0389)	(0.0394)	(0.0395)	(0.0395)
Log tobacco tax					-0.0113**	-0.00514	-0.0111**	-0.00541	-0.00298	-0.000174
					(0.00539)	(0.00777)	(0.00536)	(0.00773)	(0.00608)	(0.00747)
Log SNAP average gross income							-0.0257	-0.0333	-0.0193	-0.0239
							(0.0180)	(0.0236)	(0.0172)	(0.0212)
Log SNAP average rent							0.00302	0.00501	-0.000600	0.000703
							(0.00969)	(0.00959)	(0.00920)	(0.00890)
Observations	382560	382560	382560	382560	382560	382560	382560	382560	382524	382524
R-squared	0.962	0.962	0.963	0.963	0.963	0.963	0.963	0.963	0.963	0.963
Prob > F	0.089	1.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Number of units	7970	7970	7970	7970	7970	7970	7970	7970	7970	7970
Number of clusters	48	48	48	48	48	48	48	48	48	48
First stage F-stat		2.578		17.857		20.655		20.682		20.194
Energy controls									X	X

Notes: Robust standard errors are in parentheses, clustered by state. *** p<0.01, ** p<0.05, * p<0.1. Store and period fixed effects are included. Energy controls refer to annual state-level total energy, electricity, LPG, and natural gas quantities consumed and prices for the residential sector.

Table 2.4: Effect of SNAP-benefit changes on prices, robustness checks

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Log price index						
Log benefits per population	0.0863***	0.0656***	0.0811***	0.0711***	0.0704**	0.0855***	0.101***
	(0.0224)	(0.0206)	(0.0212)	(0.0229)	(0.0323)	(0.0240)	(0.0259)
Observations	382524	382524	2301	307750	836631	382524	382524
R-squared	0.899	0.888	0.963	0.883	0.931	0.901	0.899
Prob > F	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Number of units	7970	7970	48	6429	7153	7970	7970
Number of clusters	48	48	48	1050	48	48	48
First stage F-stat	19.798	43.623	19.874	21.093	21.529	20.753	33.105
Store revenue weights	X						
Population weights		X					
State population weights			X				
County-level benefits				X			
Full sample					X		
Remove disaster payments						X	
Policy controls							X

Notes: Robust standard errors are in parentheses, clustered by state. *** p<0.01, ** p<0.05, * p<0.1. Control variables as well as store and period fixed effects are included. Weights are fixed to the pre-period of 2006m1. Population weights refer to regressing with county-level population weights. State population weights refer to collapsing observations to the state-level and regressing with state population weights. County-level benefits refer to using county-level benefits and county-level synthetic benefits for the set of states that report benefits at the county-level. Full sample refers to using the entire sample from 2006-2015. Removing disaster payments refers to removing any variation in both benefits and participants due to disaster relief events. Policy controls refer to state-year measures of amounts paid out by transfer programs available from the BEA. The transfer amounts are logged and these programs include social security, other retirement and disability insurance transfers, Medicare, Medicaid and other vendor payments, military medical insurance, SSI, EITC, other income maintenance programs such as TANF and WIC, state unemployment insurance, other unemployment insurance, Veterans' benefits, education and training assistance, other government transfers, transfers from nonprofit institutions, and transfers from businesses.

Table 2.5: Effect of of SNAP-benefit changes on prices, alternative price indices

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Method	Default	Alternative weights	Tornqvist	One-stage	Geometric	Fixed	Fixed, posted price
VARIABLES	Log price index						
Log benefits per population	0.0740***	0.0813***	0.0700***	0.0786***	0.0767***	0.0800***	0.0777***
	(0.0216)	(0.0234)	(0.0213)	(0.0240)	(0.0234)	(0.0273)	(0.0245)
Observations	382524	382524	382524	382524	382524	352188	352188
R-squared	0.919	0.915	0.917	0.915	0.899	0.891	0.882
Prob > F	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Number of units	7970	7970	7970	7970	7970	7338	7338
Number of clusters	48	48	48	48	48	48	48
First stage F-stat	20.194	20.194	20.194	20.194	20.194	18.680	18.680

Notes: Robust standard errors are in parentheses, clustered at the state level. *** p<0.01, ** p<0.05, * p<0.1. Control variables as well as store and period fixed effects are included. Details on each of the index construction methods used above are shown in Appendix Section 2.10.4.

Table 2.6: SNAP participation and effect of SNAP-benefit changes on prices and real sales

	(1)	(2)	(3)	(4)
Specification	OLS	IV	OLS	IV
VARIABLES	Log price index		Log real sales	
Log benefits p.p.	0.0165	0.0738***	0.0466	-0.0108
	(0.0107)	(0.0250)	(0.0335)	(0.104)
x Participation rate	0.0774**	0.130***	0.254**	0.265**
	(0.0329)	(0.0371)	(0.115)	(0.128)
Observations	382524	382524	382524	382524
R-squared	0.904	0.899	0.963	0.963
Prob > F	0.000	0.000	0.000	0.000
Number of units	7970	7970	7970	7970
Number of clusters	48	48	48	48
First stage F-stat		10.439		10.439

Notes: Robust standard errors are in parentheses, clustered by state. *** p<0.01, ** p<0.05, * p<0.1. Control variables as well as store and period fixed effects are included. Log benefits p.p. refers to log benefits per population. Participation rate refers to the ratio of SNAP participants to population in each county fixed to the pre-period of 2006.

Table 2.7: Concentration measures and effect of SNAP-benefit changes on prices

VARIABLES	(1)	(2)	(3)	(4)
	Log price index			
Log benefits p.p.	0.0726***	0.0338	0.0662***	0.0264
	(0.0232)	(0.0288)	(0.0246)	(0.0295)
x HHI	0.0399***		0.0360***	
	(0.00730)		(0.00741)	
x Log est. per pop.		-0.00407***		-0.00397***
		(0.000959)		(0.000937)
x Participation rate			0.116***	0.129***
			(0.0370)	(0.0374)
Observations	382524	381984	382524	381984
R-squared	0.900	0.900	0.901	0.901
Prob > F	0.000	0.000	0.000	0.000
Number of units	7970	7958	7970	7958
Number of clusters	48	47	48	47
First stage F-stat	10.098	10.017	6.950	6.956

Notes: Robust standard errors are in parentheses, clustered by state. *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Control variables as well as store and period fixed effects are included. Log benefits p.p. refers to log benefits per population. Participation rate refers to the ratio of SNAP participants to population in each county fixed to the pre-period of 2006. HHI refers to the county-level measure of HHI for the grocery industry fixed to the pre-period of 2006. Log est. per pop. refers to log of the number of grocery establishments per population in each county fixed to the pre-period of 2006.

Table 2.8: SNAP participation and effect of SNAP-benefit changes on prices and real sales by product eligibility

	(1)	(2)	(3)	(4)
SNAP eligibility	Ineligible		Eligible	
VARIABLES	Log price index	Log real sales	Log price index	Log real sales
Baseline				
Log benefits p.p.	0.0898***	-0.126	0.0724***	0.0605
	(0.0286)	(0.107)	(0.0269)	(0.108)
Interaction				
Log benefits p.p.	0.0750**	-0.123	0.0657**	0.0376
	(0.0303)	(0.103)	(0.0277)	(0.104)
x Participation rate	0.234***	-0.0489	0.106**	0.362***
	(0.0418)	(0.179)	(0.0401)	(0.117)
Observations	382428	382428	382140	382140
Number of units	7968	7968	7962	7962
Number of clusters	48	48	48	48

Notes: Robust standard errors are in parentheses, clustered at the state level. *** p<0.01, ** p<0.05, * p<0.1. Control variables as well as store and period fixed effects are included. Log benefits p.p. refers to log benefits per population. Participation rate refers to the ratio of SNAP participants to population in each county fixed to the pre-period of 2006. SNAP-eligible goods account for around 75% of revenue in grocery stores.

Table 2.9: Effect of SNAP-benefit changes on consumption of households

	(1)	(2)	(3)	(4)
Specification	OLS, log-log	IV, log-log	OLS, level-level	IV, level-level
VARIABLES	Consumption			
Both ineligible	0.0444	0.193	0.106*	0.200
	(0.0587)	(0.180)	(0.0571)	(0.148)
Household eligible only	0.0635	0.208	0.170*	0.254
	(0.109)	(0.219)	(0.0908)	(0.172)
Product eligible only	0.0231	0.193	0.0376	0.128
	(0.0376)	(0.167)	(0.0420)	(0.140)
Both eligible	0.252***	0.411**	0.354***	0.442**
	(0.0557)	(0.164)	(0.0866)	(0.170)
Observations	2044662	2044662	2044666	2044666
R-squared	0.561	0.561	0.573	0.573
Prob > F	0.000	0.000	0.000	0.000
Number of units	23911	23911	23911	23911
Number of clusters	49	49	49	49
First stage F-stat		12.258		15.642

Notes: Robust standard errors are in parentheses, clustered by state. *** p<0.01, ** p<0.05, * p<0.1. Each coefficient comes from a regression of (log) consumption on (log) SNAP benefits per recipient, interacted with 4 group indicators. Both ineligible refers to expenditures by SNAP-ineligible households on SNAP-ineligible products, household eligible only refers to expenditures by SNAP-eligible households on SNAP-ineligible products, product eligible only refers to expenditures by SNAP-ineligible households on SNAP-eligible products, and both eligible refers to expenditures by SNAP-eligible households on SNAP-eligible products. Control variables as well as household-month and period fixed effects are also included. Observations are weighted by sampling weights.

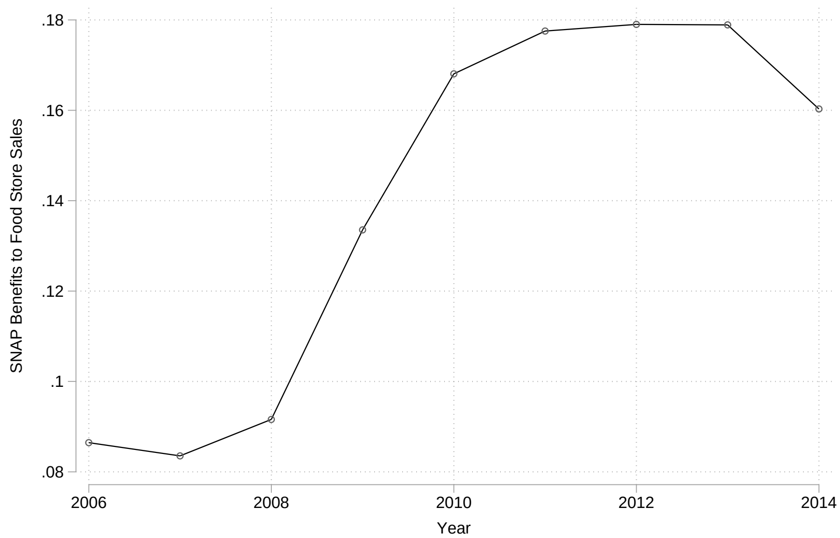
Table 2.10: Incidence of an additional dollar of SNAP benefits for SNAP-eligible goods

Market conduct	MPC elasticity	Demand elasticity	Proportion of SNAP sales	Pass-through elasticity	Shift magnitude	PS	CS	CS (SNAP)	CS (non-SNAP)
0	0.339	0.709	0.168	0.0724	0.0802	0.431	0.0466	0.405	-0.358
0.2	**0.339**	**0.709**	**0.168**	**0.0724**	**0.0802**	**0.440**	**0.0466**	**0.405**	**-0.358**
0.4	0.339	0.709	0.168	0.0724	0.0802	0.449	0.0466	0.405	-0.358
0.6	0.339	0.709	0.168	0.0724	0.0802	0.459	0.0466	0.405	-0.358
0.8	0.339	0.709	0.168	0.0724	0.0802	0.468	0.0466	0.405	-0.358
1	0.339	0.709	0.168	0.0724	0.0802	0.477	0.0466	0.405	-0.358
0	0.522	0.709	0.168	0.0724	0.124	0.431	0.3051	0.664	-0.358
0.2	**0.522**	**0.709**	**0.168**	**0.0724**	**0.124**	**0.492**	**0.3051**	**0.664**	**-0.358**
0.4	0.522	0.709	0.168	0.0724	0.124	0.553	0.3051	0.664	-0.358
0.6	0.522	0.709	0.168	0.0724	0.124	0.614	0.3051	0.664	-0.358
0.8	0.522	0.709	0.168	0.0724	0.124	0.675	0.3051	0.664	-0.358
1	0.522	0.709	0.168	0.0724	0.124	0.736	0.3051	0.664	-0.358

Notes: MPC elasticities are obtained from Section 2.5.2 and from HS. Demand elasticity is estimated using panel variation as described in Section 2.6.3. Proportion of SNAP sales is obtained from USDA data. Pass-through elasticity is obtained from Section 2.5.1 and the shift magnitude is the predicted pass-through elasticity obtained using equation (2.11) assuming a subsidy pass-through rate of 1. Surplus calculations are changes in surplus per marginal dollar of SNAP disbursed. PS refers to producer surplus and CS refers to consumer surplus, CS(SNAP) and CS(non-SNAP) refers to consumer surplus for SNAP consumers and consumer surplus for non-SNAP consumers, respectively.

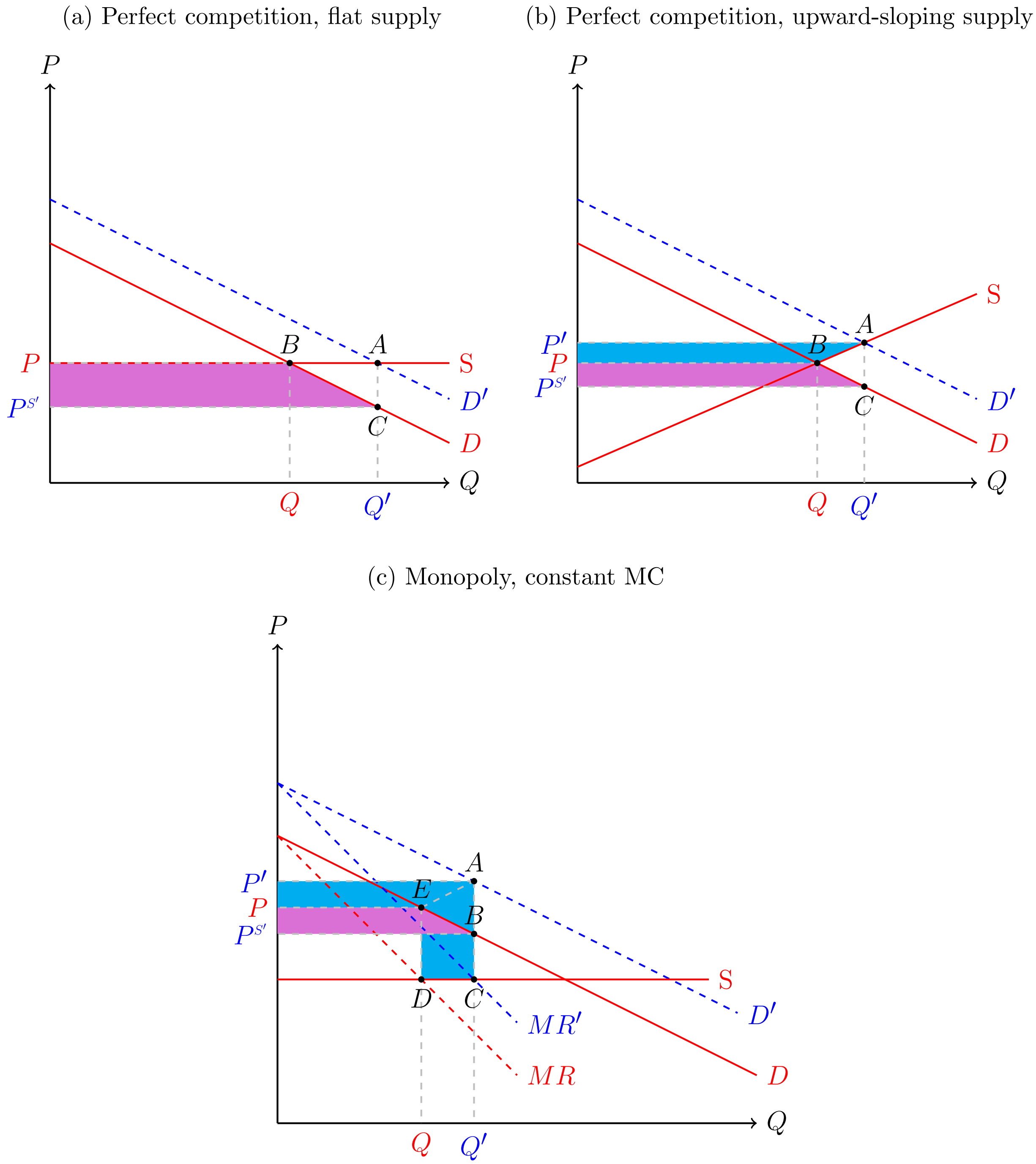
2.9 Figures

Figure 2.1: SNAP benefits as a proportion of total food-at-home sales in food stores, 2006-2014



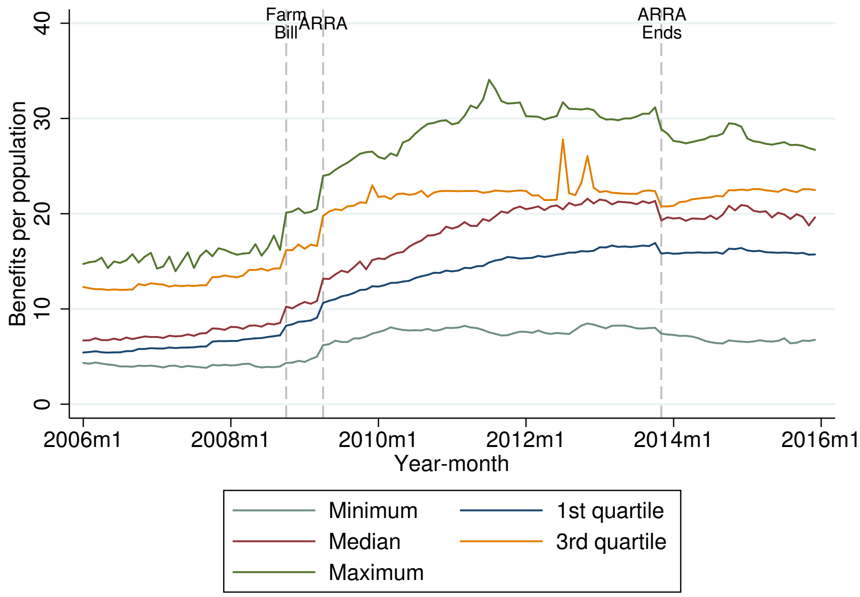
Notes: This figure plots the ratio of SNAP benefits to total food-at-home sales at food stores, as defined by the USDA ERS, from 2006-2014 using data from the USDA ERS and FNS.

Figure 2.2: Incidence under different assumptions



Notes: In this figure, we graphically illustrate how economic incidence changes in a simple partial equilibrium framework under different assumptions about the food market. For simplicity, we begin with an example in which all consumers receive SNAP benefits. In Figure 2.2a, we assume a perfect competitive market with a flat supply curve. A SNAP-benefit increase shifts the demand curve out from D to D', and prices remain constant while quantities consumed increase from Q to Q'. Assuming no income effects or a parallel shift in demand, we evaluate incidence under the original demand curve. All of the surplus generated goes to the consumer as consumer surplus increases by $PBCP^{S'}$. In Figure 2.2b, we again assume perfect competition but let the supply curve be upward-sloping. Prices now increase from P to P' when SNAP benefits increase. The increase in surplus is now divided between the producer and the consumer, with producer surplus increasing by $P'ABP$ and consumer surplus increasing by $PBCP^{S'}$. In Figure 2.2c, we assume the firm is a monopolist with a constant marginal cost. A SNAP-benefit rise increases prices from P to P' because the firm raises its markup and sells at a more inelastic portion of the demand curve. The increase in surplus is again split between the producer and the consumer, with producer surplus increasing by $P'ABCDEP$ and consumer surplus increasing by $PEBP^{S'}$.

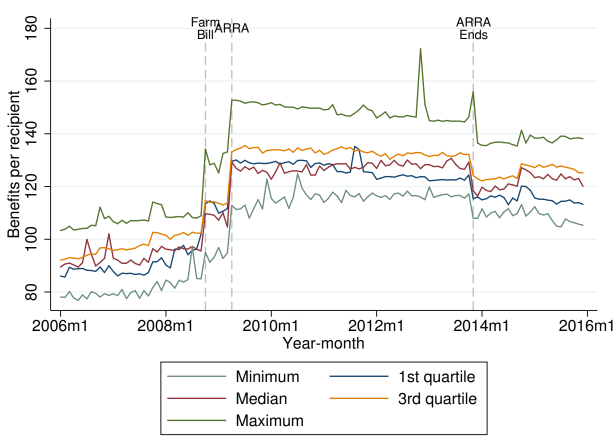
Figure 2.3: SNAP benefits per population by state, 2006-2015



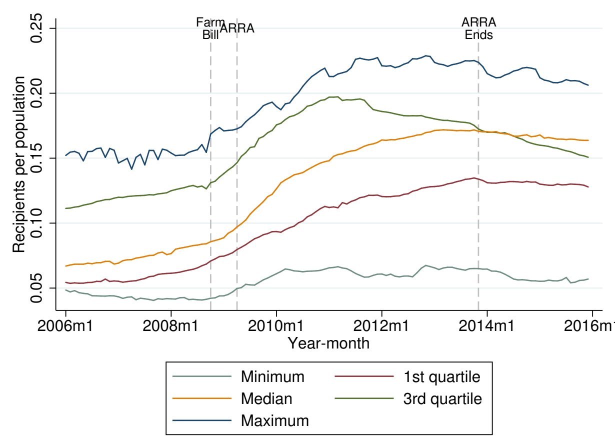
Notes: This figure plots out the SNAP benefits per population for five states from 2006 to 2015, ranking states by their average SNAP benefits per population from 2006 to 2015 and picking the states at the minimum, 1st quartile, median, 3rd quartile, and maximum.

Figure 2.4: SNAP benefits per recipient and recipients per population by state, 2006-2015

(a) Benefits per recipient

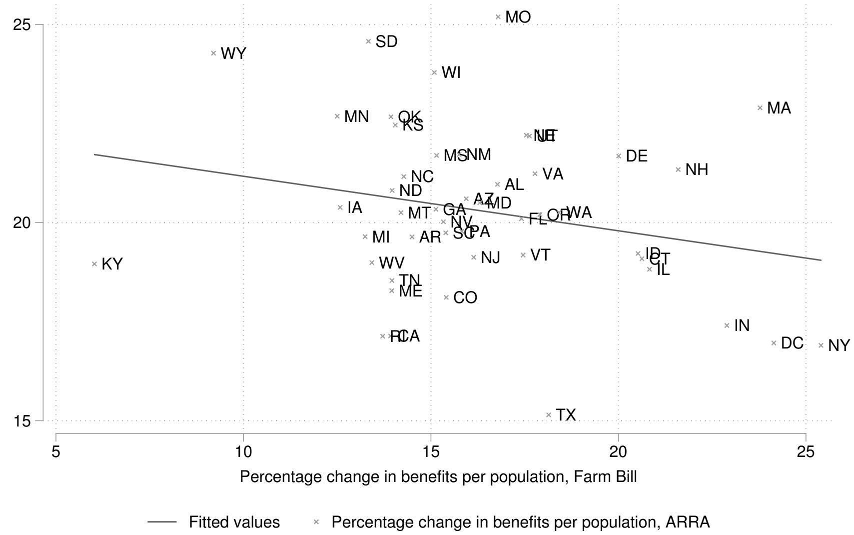


(b) Recipients per population



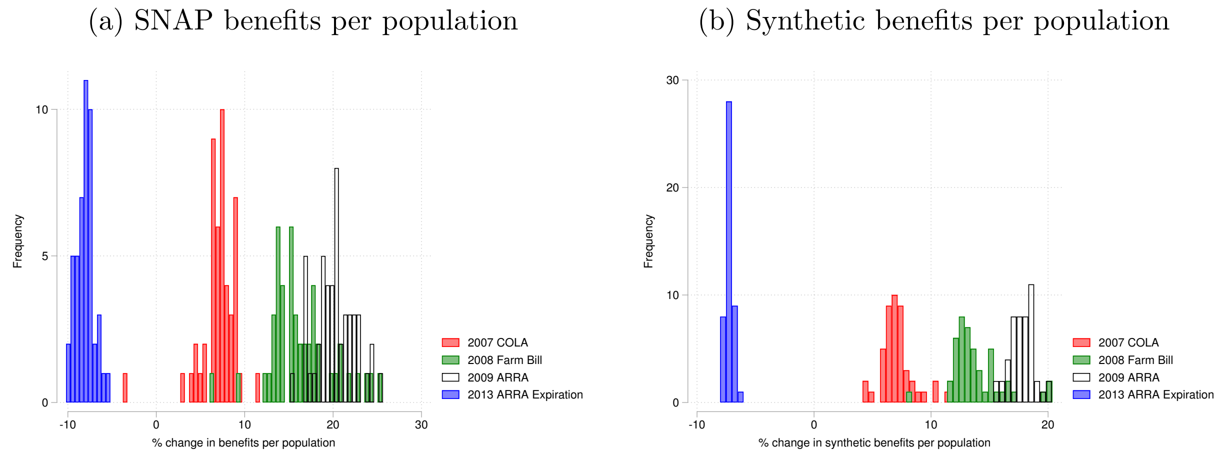
Notes: This figure plots out the SNAP benefits per recipient and recipients per population, each for five states from 2006 to 2015, ranking states by their average SNAP benefits per recipient or recipients per population from 2006 to 2015 and picking the states at the minimum, 1st quartile, median, 3rd quartile, and maximum.

Figure 2.5: Percentage change in SNAP benefits per population due to the 2008 Farm Bill and the 2009 ARRA by state



Notes: This figure plots out the percentage change in SNAP benefits per recipient for all states, excluding Ohio and Louisiana which were both outliers due to disaster relief events. The slope is negative and insignificant with a point estimate of -0.138 (0.098).

Figure 2.6: Variation across states during major changes in SNAP benefits per population and synthetic benefits per population, 2006-2015

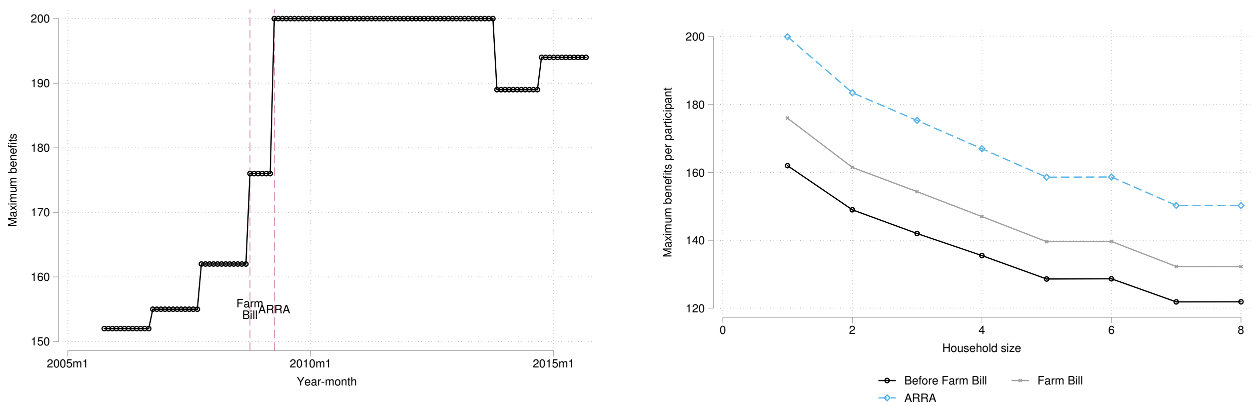


Notes: This figure plots out the changes in SNAP benefits per population and synthetic benefits per population from 2006 to 2015 across four major events in which SNAP benefits changed. These four events are 2007 cost-of-living adjustments, 2008 Farm Bill, 2009 ARRA, and 2013 ARRA expiration. For SNAP benefits per population, the mean (standard deviation) of these 4 events are 0.072 (0.016), 0.16 (0.037), 0.20 (0.022), and -0.080 (0.010) respectively. For synthetic benefits per population, the means (standard deviations) of these four events are 0.073 (0.021), 0.14 (0.028), 0.18 (0.010), and -0.072 (0.0033), respectively. Several outliers due to disaster-relief events are dropped.

Figure 2.7: Changes in maximum benefits

(a) Maximum benefits, single-person household

(b) Maximum benefits per participant by household size

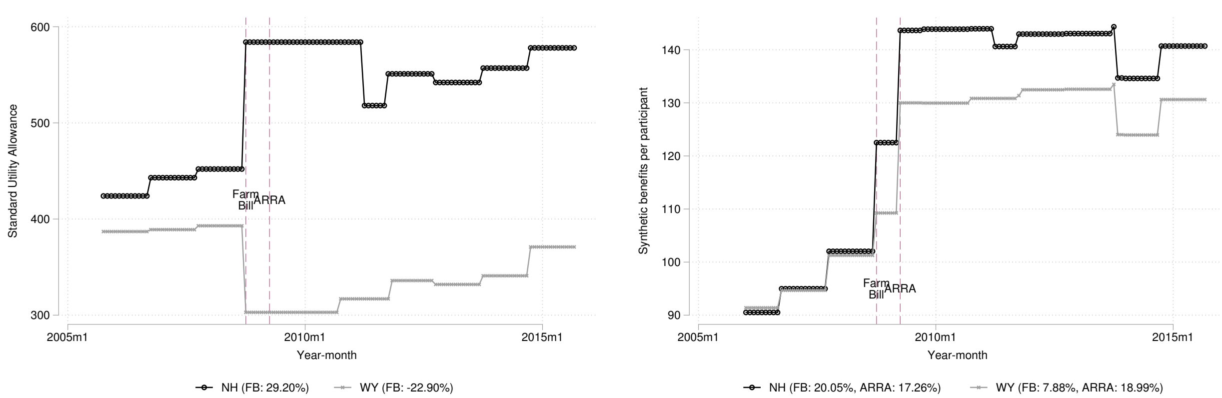


Notes: This figure plots out the maximum benefits for a single-person household over time in Figure 2.7a, and the maximum benefits by household size before the Farm Bill, after the Farm Bill, and after the ARRA in Figure 2.7b.

Figure 2.8: HCSUA and synthetic benefits per recipient, New Hampshire and Wyoming

(a) HCSUA

(b) Synthetic benefits per recipient

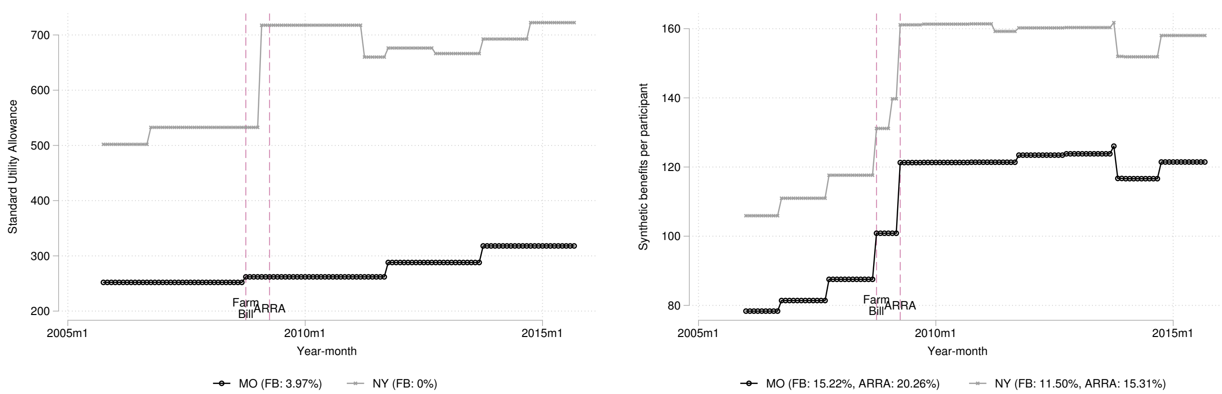


Notes: This figure plots out the heating and cooling standard utility allowance (HCSUA) and synthetic SNAP benefits per recipient from 2006-2015 for two states, New Hampshire and Wyoming, to illustrate how differential changes in SUA lead to differential changes in synthetic benefits per recipient and synthetic benefits per population. Inside the parentheses behind each state, we show the percentage change of the HCSUA during the Farm Bill (FB) for Figure 2.8a, and the percentage change in synthetic benefits per population during the FB and the ARRA for Figure 2.8b.

Figure 2.9: HCSUA and synthetic benefits per recipient, Missouri and New York

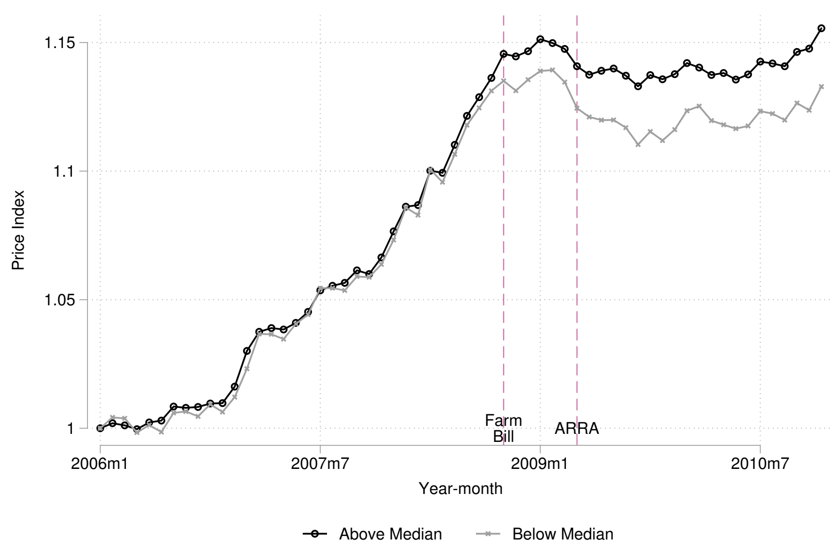
(a) HCSUA

(b) Synthetic benefits per recipient



Notes: This figure plots out the HCSUA (heating and cooling standard utility allowance) and synthetic SNAP benefits per recipient from 2006 to 2015 for two states, Missouri and New York, to illustrate how differences in initial levels of synthetic benefits per recipient lead to different percentage changes in synthetic benefits per recipient and synthetic benefits per population. Inside the parentheses behind each state, we show the percentage change of the HCSUA during the Farm Bill (FB) for Figure 2.9a, and the percentage change in synthetic benefits per population during the FB and the ARRA for Figure 2.9b.

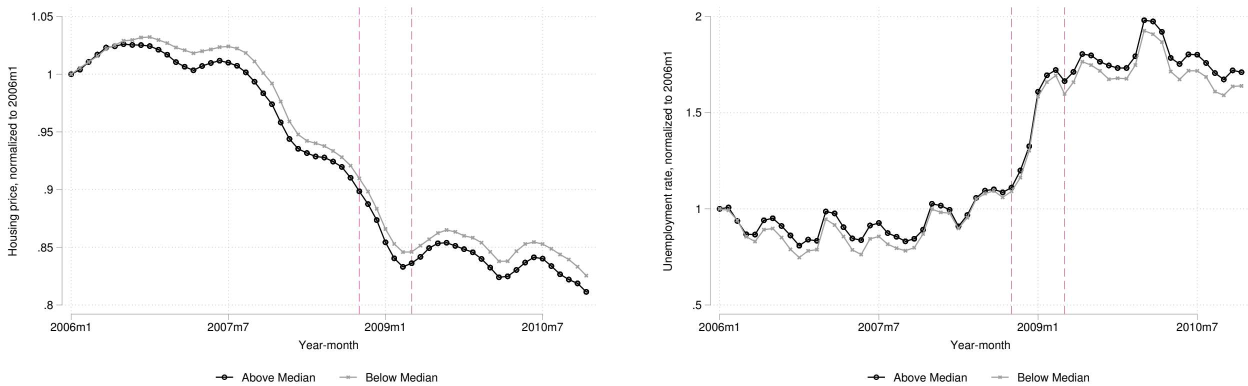
Figure 2.10: Grocery store price indices by quantile of change in log synthetic benefits per population, 2006-2010



Notes: This figure plots the revenue-weighted average grocery store price indices by the quantile of change in log synthetic benefits per population for the first and second quantile, respectively. To obtain these quantiles, the total change in the log synthetic benefits per population during the Farm Bill and the ARRA for each state, residualized by control variables, is used to separate the states into quantiles. The first quantile denotes the 24 states with the largest changes in residualized log benefits per population, and the second quantile denotes the 24 states with the smallest changes.

Figure 2.11: Housing price and unemployment rate by quantile of change in log synthetic benefits per population, 2006-2010

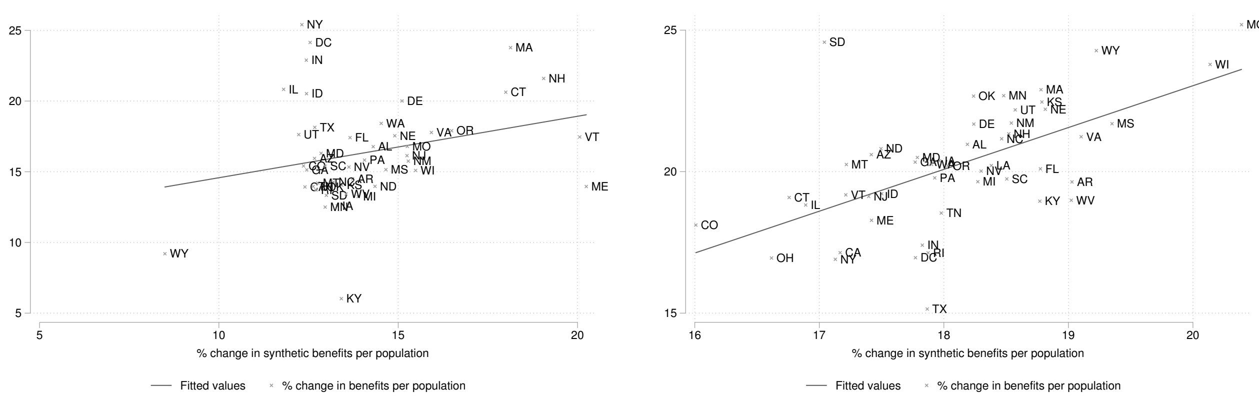
(a) Housing price (b) Unemployment rate



Notes: This figure plots the housing price and unemployment rate, normalized to 2006m1, by the quantile of change in log synthetic benefits per population for the first and second quantile, respectively. To obtain these quantiles, the total change in the log synthetic per population during the Farm Bill and the ARRA for each state, residualized by control variables, is used to separate the states into quantiles. The 1st quantile denotes the 24 states with the largest changes in residualized log benefits per population, and the 2nd quantile denotes the 24 states with the smallest changes.

Figure 2.12: Changes in SNAP benefits per population against changes in synthetic benefits per population, Farm Bill and ARRA

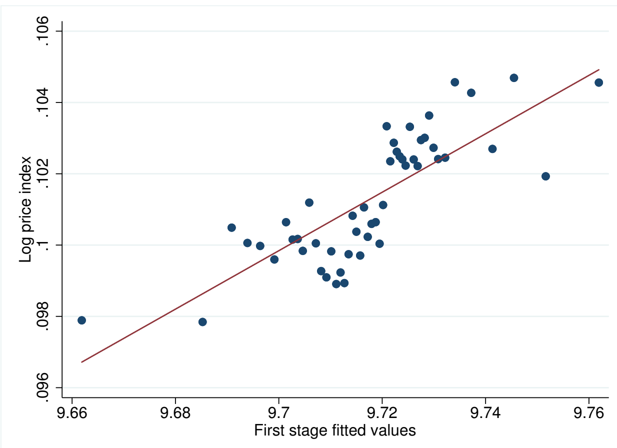
(a) Farm Bill (b) ARRA



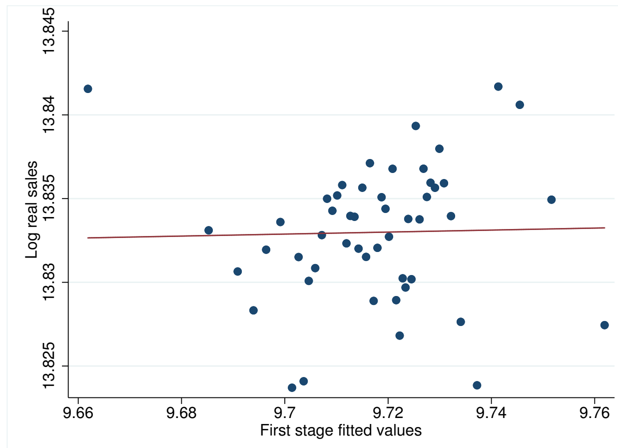
Notes: This figure plots the changes in SNAP benefits per population against changes in synthetic benefits per population during the Farm Bill and the ARRA. The slopes are 0.43 (0.27) and 1.48*** (0.28), respectively. Robust standard errors are in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Ohio and Louisiana are dropped during the Farm Bill due to disaster-relief events.

Figure 2.13: Effects of SNAP-benefit changes on prices and real sales

(a) Log price index on fitted log benefits per population



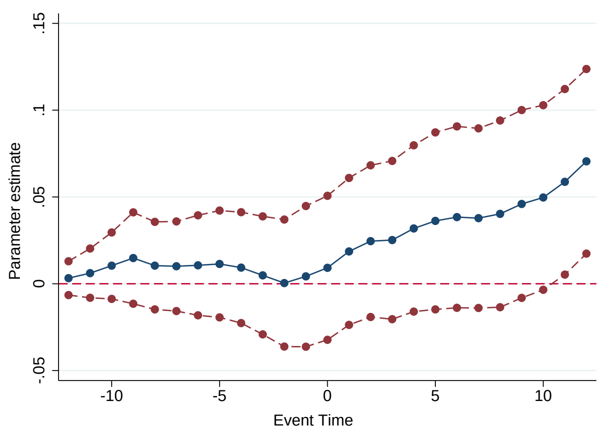
(b) Log real sales on fitted log benefits per population



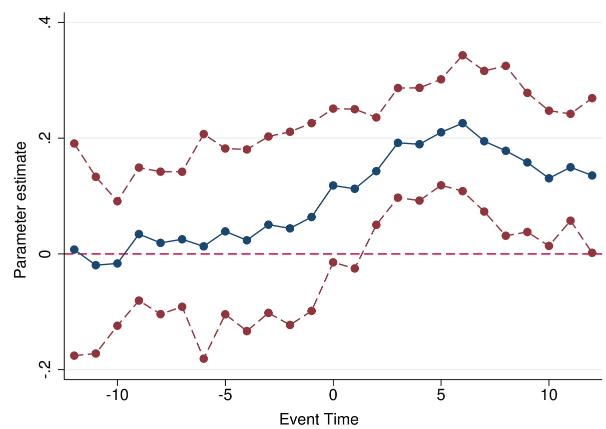
Notes: This figure plots the log price index against the fitted values of log benefits per population as predicted by the IV. Both variables are residualized by regressing on a set of controls, store fixed effects, and period fixed effects. For each store-year-month observation, the first-stage fitted values are calculated and grouped into 50 quantiles. The x-axis displays the mean of the residualized first-stage fitted values in each quantile. The y-axis shows the mean of the residualized log price index in each quantile. The line of best fit is obtained from the regression using all observations in each sample. The slopes are 0.082 (0.023) and 0.00597 (0.109), respectively.

Figure 2.14: Cumulative effect on prices

(a) OLS: SNAP benefits per population

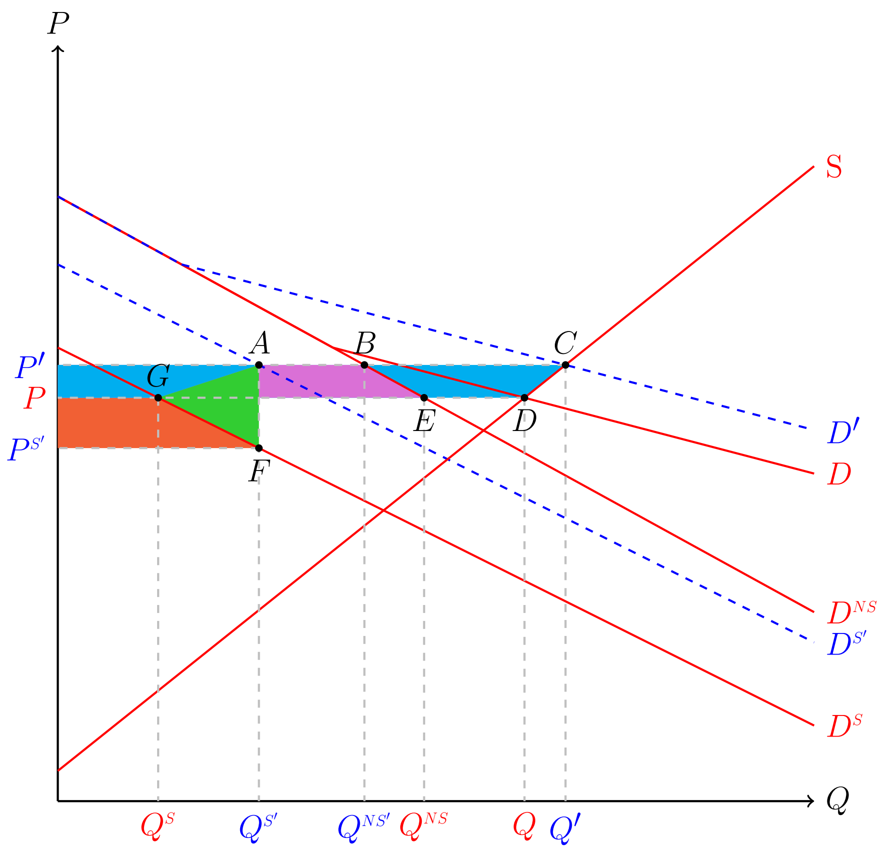


(b) Reduced form: Synthetic benefits per population



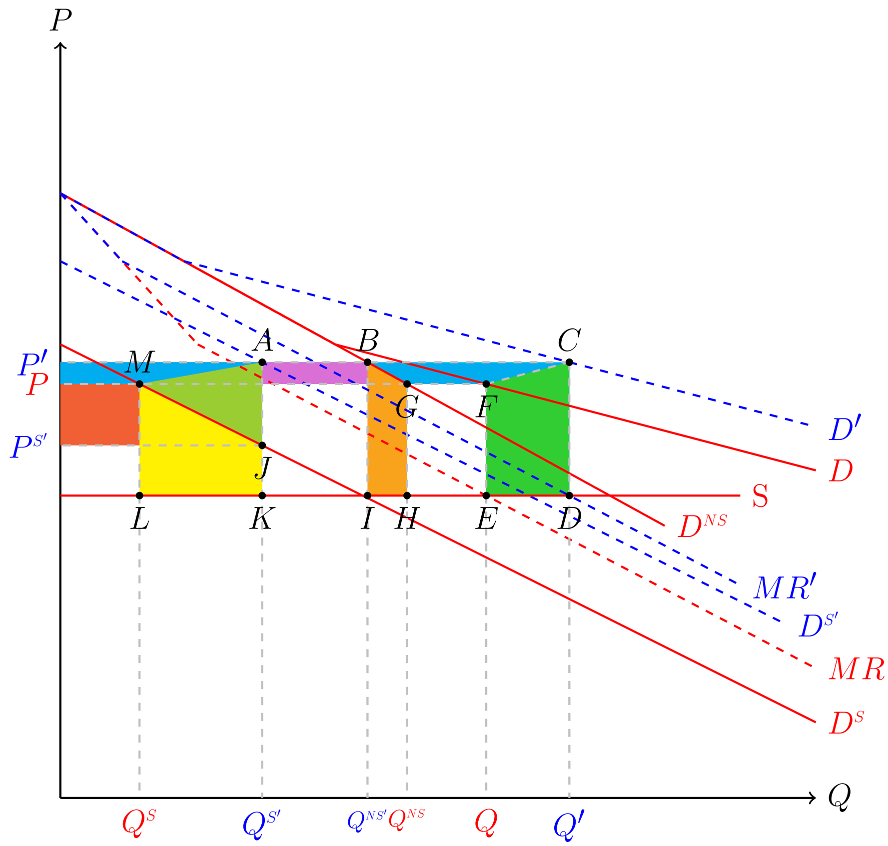
Notes: This figure plots the sum of estimated coefficients for each period, along with the 95% confidence intervals, from regressions using a distributed lag model, where log price index is regressed on log SNAP benefits per population and log synthetic benefits per population respectively. Control variables as well as store and period fixed effects are included.

Figure 2.15: Full incidence of SNAP benefits under perfect competition



Notes: This figure illustrates the incidence of increased SNAP benefits in a partial equilibrium setting under perfect competition. Market-demand curve D for SNAP-eligible products is obtained by the horizontal summation of demand of SNAP recipients D^S and non-SNAP recipients D^{NS}. Equilibrium price P and quantity Q are obtained by the intersection of demand D and supply S. The government then disburses SNAP benefits to recipients, shifting the demand curve of SNAP recipients up to $D^{S'}$. Equilibrium price increases to P' and quantity increases to Q' by the intersection of new market demand D' and supply S. The increase in price and quantity leads to increased producer surplus of $P'CDP$. Assuming no income effects or a parallel shift in demand, we evaluate the welfare of SNAP recipients under their old demand curve D^S. SNAP recipients now act as if they face lower prices of $P^{S'}$ and quantity demanded increases to $Q^{S'}$ due to the disbursement of benefits, and hence their consumer surplus increases by $PGFP^{S'}$. On the other hand, non-SNAP consumers now face a higher price P', and their consumer surplus decreases by $P'BEP$. To consider the deadweight loss of the program, we first observe that the loss in non-SNAP consumer surplus $P'BEP$ directly transfers to a gain in producer surplus. The remaining producer surplus $BCDE$ from SNAP consumers is equal in area to $P'AGP$. The cost of SNAP benefits to the government is $P'AFP^{S'}$, which implies that the deadweight loss of the program is GAF. This deadweight loss is a result of SNAP consumers buying marginal units they value at less than the marginal cost for producers.

Figure 2.16: Full incidence of SNAP benefits under monopoly



Notes: This figure illustrates the incidence of increased SNAP benefits in a partial equilibrium setting under monopoly. The market demand curve D for SNAP-eligible products is obtained by the horizontal summation of demand of SNAP recipients D^S and non-SNAP recipients D^{NS}. Equilibrium price P and quantity Q are now obtained by the intersection of marginal revenue MR that is obtained from demand D and marginal cost given by supply curve S, which we assume is constant, based on evidence by Stroebel and Vavra (2015). The government then disburses SNAP benefits to recipients, shifting the demand curve of SNAP recipients up to $D^{S'}$. Equilibrium price increases to P' and quantity increases to Q' by the intersection of new marginal-revenue curve MR' and supply S. We now evaluate the changes in welfare as a result of SNAP. The increase in price and quantity leads to increased producer surplus of $P'CDEFP$. SNAP recipients now act as if they face lower prices of $P^{S'}$ and quantity demanded increases to $Q^{S'}$ due to the disbursement of benefits, and hence their consumer surplus increases by $PMJP^{S'}$. On the other hand, non-SNAP consumers now face a higher price P', and their consumer surplus decreases by $P'BGP$. To consider the deadweight loss of the program, we first observe that the loss in non-SNAP consumer surplus $P'BGP$ directly transfers to a gain in producer surplus. The remaining producer surplus $BCFG$ from SNAP consumers is equal in area to $P'AMP$. The cost of SNAP benefits to the government is $P'AJP^{S'}$, which implies part of the deadweight loss of the program is MAJ. However, this deadweight loss is offset by the change in producer surplus $FCDE$. To better understand the change in total surplus, note that $FCDE$ is identical in area to $MAKL$ minus $BGHI$. Therefore, the change in total surplus of $FCDE$ minus MAJ is equivalent to $MJKL$ minus $BGHI$. In other words, the change in deadweight loss of the program is given by the decrease in deadweight loss when the monopolist sells more to SNAP consumers and the increase in deadweight loss when the monopolist further restricts output to non-SNAP consumers.

2.10 Appendix

2.10.1 Formalizing the Actual SNAP Formula and Instrumentation Strategy

We first formalize the SNAP formula illustrated mathematically in Section 2.3.2. Comprehensively, let $X_i = \{N_i, S_i, I_i, R_i, U_i, UI_i, EI_i, C_i, D_i, M_i, L_i\}$ represent the set of histories of a potential SNAP recipient i's demographic characteristics the government sees until a given time period. Characteristics important to the government include N_i, the person's household size; S_i, the person's state of residence; I_i, the person's gross income minus earned income deduction; R_i, the person's rent; and U_i, the person's utility expenditure. Let $p = \{b, o, u, \hat{h}, \check{h}\}$ represent the set of SNAP parameter functions of X_i. b represents the maximum-benefits formula. The SNAP formula is given by

$$B_i = max\Big(0.08 \times b(N_i) \times 1[N_i \leq 2],\ b(N_i) - 0.3 \times max\left(0, I_i - o_i - H_i\right)\Big). \tag{2.17}$$

Note that, in the real formula above, households of sizes 1 and 2 are allowed a minimum benefit (8% of the maximum benefit), should their calculated benefit fall short of the minimum benefit threshold. Note also that in the real formula above, expected ability to contribute to food spending is bounded from below at 0. Note that these bounds do not affect our heuristic analysis in the previous subsection as the average marginal relationships between the arguments and the output of the formula remain the same.

Next we define each component I_i, o_i, and H_i, which are given by:

$$I_i = UI_i + 0.8EI_i \tag{2.18}$$

$$o_i = d(N_i) + C_i + D_i + M_i \tag{2.19}$$

$$H_i = \begin{cases} R_i + u(N_i, S_i, U_i) + 0.5o_i - 0.5I_i & L_i = 1 \\ \min(R_i + u(N_i, S_i, U_i) + 0.5o_i - 0.5I_i, \hat{h}) & L_i = 2 \\ \max(R_i + u(N_i, S_i, U_i) + 0.5o_i - 0.5I_i, \check{h}) & L_i = 3 \end{cases} \tag{2.20}$$

Equation (2.18) shows I_i is the sum of unearned income UI_i and earned income EI_i minus 20% of EI_i applied as an earned income deduction.

Equation (2.19) shows deductions for basic needs other than shelter include standard deduction per household (set annually at the federal level) $d(N_i)$, legal child-support-expense deduction C_i, dependent-care-expense deduction D_i, and medical-expense deduction M_i.

Equation (2.20) describes how the excess shelter deduction H_i is determined for different types L_i of households: $L_i = 1$ identifies non-homeless households with elderly or disabled members; $L_i = 2$ identifies non-homeless households without elderly or disabled members; $L_i = 3$ identifies homeless households. The equation shows that, for households of type $L_i = 1$, H_i is the sum of rental cost R_i and utility-standard cost $u(N_i, S_i, U_i)$ in excess of half of net countable income prior to considering shelter costs $0.5I_i - 0.5o_i$.[18] For households of type $L_i = 2$, H_i is bounded by $\hat{h}$, a shelter cap set at the federal level (inflation-adjusted every year). For households of type $L_i = 3$, H_i is bounded from below by $\check{h}$, a homeless-shelter allowance set at the federal level (which, curiously, has not been updated since 2003).

18. The excess shelter deduction seems to be motivated by a desire of the federal government to ameliorate differences in shelter costs across regions not fully reflected in differences in wage levels across regions. The federal government seems to assume one half of a household's net countable income before shelter costs is available to cover the household's cost of shelter.

Summarizing the discussion, we have,

$$B_i = max\Big(0.08 \times b(N_i) \times 1[N_i \leq 2],\ b(N_i) - 0.3 \times NI(X_i; p)\Big), \tag{2.21}$$

where NI stands for calculated net countable income. The actual instrument we use in the paper is given by,

$$Z_{st} = \frac{1}{n_{s,0}} \sum_{i \in s,0} max\Big(0.08 \times b(N_{i0}) \times 1[N_{i0} \leq 2],\ b(N_{i0}) - 0.3 \times NI(X_{i0}; p_t)\Big). \tag{2.22}$$

For most households for whom the minimum-benefit threshold does not bind, the changes over time in log points of the instrument can be expressed as

$$\Delta \ln Z_{st} \approx \frac{Z_{s,t} - Z_{s,t-1}}{Z_{s,t-1}} = \frac{\bar{\Delta}_s\, b(N_{i0}) - 0.3 \times \bar{\Delta}_s\, NI(X_{i0}; p_t)}{\frac{1}{n_{s,0}} \sum_{i \in s,0} [b(N_{i0}) - 0.3 \times NI(X_{i0}; p_{t-1})]}. \tag{2.23}$$

The same intuitions from the previous subsection carry over to our comprehensive formulation of the SNAP formula, although they are more difficult to see here. As in the previous subsection, changes in the numerator no longer depend on realized trends either in rents or in incomes of program participants. The simulated variable is also free from the influence of participants joining the program after 2006. Again, note that we find it necessary to include a housing-price control to net out the influence of these new participants on the actual benefits, which crucially affects the first stage. The simulated benefits depend only on the intensive-margin changes based on the levels of demographic characteristics of participants X_{i0} realized prior to the Great Recession.

2.10.2 Adjustments to the SNAP Formula Made by Individual States

Although the SNAP formula is set at the federal level as described above, a number of adjustments to the formula occur at the state-level during implementation that introduce cross-state idiosyncrasies in final disbursement levels. These include, most importantly, the standard utility allowance program (SUA).

2.10.2.1 Standard Utility Allowance

First, all states participate in the SUA. States set their own utility-deduction standards, which are designed to reflect the average within-state utility costs of households. According to Holleyman, Beggs and Fox (2017), states' methodologies fall into two categories: (1) methodologies that rely on state-specific recent utility data and (2) methodologies that adjust a base number using an inflation measure such as the Consumer Price Index (CPI) of utility costs. Some states use a methodology that combines both approaches. Considerable variation exists within these methodologies. For example, some states use only data for low-income households, whereas others gather data for all households. States incorporate a variety of fuel types, and some assign weights to the different fuel types, whereas others do not. Over time, FNS has found some variation between established SUA values and average household utility expenses in many states. Over 40 states make the usage of these standards mandatory, and the standards are generally larger than utility expenses to encourage usage and simplify application procedures. Furthermore, 15 states participate in a related program called "Heat and Eat," which allows households participating in the Low-Income Home Energy Assistance Program (LIHEAP) to receive the highest-bracket utility deduction without having to provide utility-cost verifications. The "Heat and Eat" program especially benefits households with elderly or disabled members, because LIHEAP gives priority to these households during application.

Because shelter expenses account for a substantial amount of deductions and SUA accounts for over 50% of shelter expenses, SUA has a particularly strong effect on state-level variation in benefits per population. We plot the relationships between changes in synthetic SNAP benefits per population, SUA per population, energy usage per population and prices for the residential sector by different fuel types, and temperature by state during the Farm Bill in Figure 2.18. We focus on this period because SUA only changes in October and not in April when the ARRA led to a rise in maximum benefits. We also focus on heating and cooling SUA (HCSUA) because households predominantly use HCSUA and HCSUA has the largest value relative to other types of SUA, such as telephone expenses. We see that changes in SUA are highly positively correlated with changes in synthetic benefits per population and that these changes are also correlated with drops in temperature and rises in total energy usage from heating using sources such as electricity, LPG, and natural gas, and less so for changes in fuel prices. These figures also show variation in synthetic benefits per population that is not explained by HCSUA, since maximum benefits are also changing during the Farm Bill. Therefore, we show our results are robust to controlling for a vector of energy usage and prices at the state-level in Table 2.2, because these variables could have an impact on retail prices directly. Residual policy variation should be driven by prediction errors by states in setting the SUA and idiosyncratic state-level differences in methodologies used to set the SUA.

2.10.2.2 Other Adjustments to the SNAP Formula Made by Individual States

Second, states have the flexibility to relax the federal eligibility rules through a policy called Broad-Based Categorical Eligibility (BBCE).[19] For instance, in the case of the gross income limit in 2015, 11 states followed the federal 130% limit, whereas 14 states extended the limit to 200%, with other states choosing a limit in between. In the case of the asset limit, 24 states waive it altogether; 11 states waive it for most households, but if a household cares for an elderly or a disabled member and the household's gross income exceeds 200% of the federal poverty line, the states apply an asset limit of $3,250; New Hampshire waives the asset limit for households with children; New York waives it for households with dependents or households with earned income. All states except four allow for at least one vehicle per household to be excluded from countable assets. This implies that heterogeneity exists in

19. BBCE allows states to equate their SNAP eligibility rules with TANF eligibility rules, over which states have some jurisdiction.

the composition of SNAP participants across states.

Third, some state-level policies change the formula entirely, and these include MFIP and SSI-CAP. MFIP applies to recipients of Temporary Assistance for Needy Families (TANF) program, and determines SNAP payment levels in conjunction with TANF payment levels via a state-specific formula.[20] SSI-CAP is a program that allows SSI recipients to apply for SNAP benefits through the SSI application process rather than requiring them to step through a separate application process for SNAP, and participating states are given substantial leeway over the setting of eligibility rules and benefit levels. Among the 18 states that have participated in the program, some states have allowed only single-member households to participate, whereas others have allowed couples to participate; some states have allowed households with earned income to participate, whereas others have not; age restrictions and shelter-expense cutoffs vary across states. No deductions are allowed for participating households, and benefit levels are determined by a state-specific function of state-specific household rent brackets.

Another policy aimed at standardizing deductions at the state level also introduces additional variation across states: the medical-deduction demonstration program. Sixteen states participate in the former, each state stipulating a fixed amount of medical deduction for households with any elderly or disabled member, whose recorded medical expenses fall below a state-specific threshold.

An additional reason SNAP-benefit levels may vary across states is disaster-relief payments. Disaster-relief payments can be made when a presidential disaster declaration is made for a particular area. Our results are robust to cleaning out the disaster-relief payments from the benefit series.

Lastly, not all eligible recipients participate in SNAP, hence varying the participation-rate

20. To highlight some important differences, deductions other than the earned income deduction are not considered, and the earned-income-deduction rate is set higher at around 40 % to 50%. The formula itself is also quite different, although it uses some of the same SNAP parameters. Practically, though, the average payout amounts do not differ greatly from the payout amounts of other states. Our results are robust to excluding Minnesota from the sample.

series across states. Ganong and Liebman (2013) study the variation in the participation rate between 1994 and 2011, and note the participation rate showed a negative correlation with the unemployment rate between 2001 and 2007, but then showed a positive correlation during the Great Recession. They also note that variation in the unemployment rate can explain two thirds of the the participation rate variation, whereas BBCE rules can explain 18%.

2.10.3 Bartik-Style Shift-Share Design

We show that our simulated instrument can be written as a general Bartik-style shift-share instrument. The initial local shock exposure measure is determined by pre-period household characteristics, while the shocks are policy changes, which include the increase in federal max benefits and SUA during the Farm Bill and the increase in federal max benefits during the ARRA.

The canonical outcome equation in a Bartik-style shift-share design estimates the labor supply elasticity β using cross-sectional variation across locations l by investigating the impact of employment growth x_l on wage growth y_l as follows:

$$y_l = \alpha + \beta x_l + \varepsilon_l. \tag{2.24}$$

Employment growth x_l can be written as the inner product of industry shares s_{lk} and local industry growth rates g_{lk}:

$$x_l = \sum_k s_{lk} g_{lk}. \tag{2.25}$$

Location-industry growth rates g_{lk} can be decomposed into a national industry component g_k and a location-industry component η_{lk}:

$$g_{lk} = g_k + \eta_{lk}. \tag{2.26}$$

The Bartik-style IV z_l is then the inner product of the industry-location shares s_{lk} and the national industry component of growth rates g_k:

$$z_l = \sum_k s_{lk} g_k. \tag{2.27}$$

In this paper, the outcome equation is the impact of SNAP benefits per population growth on price or sales growth. We can simplify the estimating equation (2.5) into the cross-sectional equation form across locations l as in equation (2.24) by using the residualized price index or sales y_l and residualized benefits per population x_l. y_l is now price or sales growth, while x_l is benefits per population growth.

Benefits per population growth can be written as the inner product of household-type shares s_{lk} and growth in benefits per household g_{lk} as in equation (2.25). We define a household-type as a household with a given value of initial level of benefits before policy changes, interacted with relevant pre-recession household characteristics that could impact how policy changes affect their benefit growth rate, e.g. whether the shelter cap binds for the household.

Location-household-type growth rates g_{lk} can be decomposed into a national household-type component g_k driven by max benefit changes, an exogenous location-household-type component $\tilde{g}_{lk}$ driven by SUA, and an endogenous location-household-type component η_{lk}:

$$g_{lk} = g_k + \tilde{g}_{lk} + \eta_{lk} \tag{2.28}$$

The Bartik-style IV z_l is then the inner product of the household-type shares s_{lk} and the location-household-type growth rate components driven by maximum benefits and SUA, denoted as g_k and $\tilde{g}_{lk}$ respectively:

$$z_l = \sum_k s_{lk}(g_k + \tilde{g}_{lk}) \tag{2.29}$$

2.10.4 Construction of Price Indices

Beraja, Hurst and Ospina (2015) adopt a two-stage procedure that is very similar to the one used by the BLS in constructing the CPI, and introduce some improvements enabled by scanner data. A viable alternative would have been to use an exact-price index as defined in Diewert (1976) for the CES unit-cost function by applying Sato (1976) and Vartia (1976) weights, which would be theoretically founded. These exact-price indices can also account for new product varieties within the CES framework as demonstrated in Feenstra (1994) and implemented in Broda and Weinstein (2010). However, we did not choose this alternative for two reasons. First, we wish to make the indices more comparable to the CPI for easier comparison. Second, theoretically founded indices that account for product turnover require estimation of parameters that are very computationally intensive given the size of the dataset.

In the first stage, the index is constructed at both the monthly and quarterly level for each product group (125 groups) and store. Stores that do not appear throughout the entire sample period are dropped, retaining around 23,500 stores in the sample. Therefore, the results are not affected by store entry and exit. Among the stores that are in the sample in 2006, 84% remain throughout the entire sample period. Although the scanner data are weekly, they are aggregated up to the monthly or quarterly level to decrease missing values and reduce chain drift, as pointed out by Ivancic, Diewert and Fox (2011). Each base observation is a monthly or quarterly unit value for each store-product, that is, monthly or quarterly revenue divided by the total number of units, which is equivalent to a quantity-weighted average price. Products are defined as UPC codes. Alternatively, weekly prices can be sampled from each store-product-period. Only goods that appear consistently across an entire year are included, such that around 50% and 70% of all sales are used in constructing store-level monthly and quarterly indices, respectively.[21] Quantities are directly observed and used as weights, which is a major advantage relative to the CPI, which collects price quotes at the store level but not quantities, so that they use quantities that are lagged three to four years and are obtained from the BLS CEX. Quantity weights are updated yearly to avoid chain drift, and the weights (denoted as $q_{i,y}$) are lagged one year to ensure price changes are not a result of changing consumption patterns in response to current prices or shocks. The CPI weights are updated every two years, which is less frequent than the scanner index. Hence, the CPI is more subject to substitution bias and the basket is less relevant over time. The price index $P^L_{j,t,y}$ at time t and year y for product group j for each store is shown below in equation (2.30):

$$P^L_{j,t,y} = P^L_{j,t-1,y} \times \frac{\sum_{i \in j} p_{i,t} q_{i,y-1}}{\sum_{i \in j} p_{i,t-1} q_{i,y-1}}. \tag{2.30}$$

Each unique item is defined by its UPC code. Prices and quantities are observed for each store and UPC pair, which is denoted as product i. By construction, changes in the price index only reflect relative changes in prices for a given bundle and are unaffected by price levels. Therefore, product switching among consumers to more expensive bundles does not change the price index for given price levels.

The second stage is similar to the first stage and aggregates the product group-specific price indices for each store using expenditure shares $s_{j,y-1}$ that are lagged one year and fixed within year. To follow the CPI more closely, a Tornqvist price index can also be constructed using the average expenditure shares between two periods as weights. Although we present results using the first method, both methods give almost identical results and are shown in

21. Prices of goods that did not sell within a given week are not recorded in retail scanner data. Therefore, aggregating up to the monthly or quarterly level decreases missing values. Furthermore, products that are not bought by consumers are inherently not an important part of the consumer basket.

equation (2.31):

$$\frac{P_t}{P_{t-1}} = \sum_{j=1}^{N} s_{j,y-1} \left(\frac{P^L_{j,t,y}}{P^L_{j,t-1,y}} \right) \tag{2.31a}$$

$$\frac{P_t}{P_{t-1}} = \prod_{j=1}^{N} \left(\frac{P^L_{j,t,y}}{P^L_{j,t-1,y}} \right)^{\frac{s_{j,t}+s_{j,t-1}}{2}} . \tag{2.31b}$$

We also construct a range of price indices at the quarterly level using alternative methods. The first index weights each product group using the expenditure shares of only products chosen to construct the price index, that is, products that satisfy the consistency criterion illustrated above, as opposed to all products in the data. The second index uses the Tornqvist index mentioned above. The third index constructs the price index in one stage instead of two stages. The fourth index uses a weighted geometric average in the first stage instead of a weighted arithmetic average. The fifth index uses weights that are fixed over time at the base period to ensure that the results are not driven by shifts in the consumption bundle over time as opposed to actual price changes. To construct such an index, only products that appeared consistently over the entire sample period can be used. The sixth index again uses fixed weights but also base observations that are sampled from the last observable posted price for each store-product-quarter.[22] All indices are highly correlated and results are robust to using any of the above methods. We show these indices for New York City in Figure 2.17, which are nearly identical across construction methods.

2.10.5 Auxiliary Data

Data on housing prices are obtained from the Federal Housing Finance Agency (FHFA), which produces housing price indices at the state level from 2006 to 2015. County housing-

22. The posted price is actually recorded as a weekly unit value for Saturday-ending weeks. DellaVigna and Gentzkow (2017) highlight that this feature creates a slight aggregation bias but the bias is relatively small for state-level shocks in their calibrations.

price indices from 2006 to 2015 are also obtained from CoreLogic through the Fama-Miller Center at the University of Chicago Booth School of Business. Results are presented using state-level housing prices from FHFA, whereas results are robust to using county level housing prices. Labor force and demographic variables are obtained from the BLS and the Census. Tobacco tax data are available from the industry-funded annual report *Tax Burden on Tobacco, 2017* and are assembled by *Campaign for Tobacco-Free Kids*. SNAP-recipient characteristics are obtained from SNAP QC data described above. Energy controls, which include annual state-level total energy, electricity, LPG, and natural gas quantities consumed and prices for the residential sector, are obtained from the State Energy Data System (SEDS) maintained by the US Energy Information Administration (EIA). Data on monthly temperature by state are obtained from Berkeley Earth. Data on policy controls refer to state-year measures of amounts paid out by transfer programs available from the BEA. The transfer amounts are logged and these programs include social security, other retirement and disability insurance transfers, Medicare, Medicaid and other vendor payments, military medical insurance, SSI, EITC, other income maintenance programs such as TANF and WIC, state unemployment insurance, other unemployment insurance, Veterans' benefits, education and training assistance, other government transfers, transfers from nonprofit institutions, and transfers from businesses. Measures of grocery industry concentration, which include the number of establishments per population and the HHI, are obtained from the County Business Patterns (CBP) made available by the Census Bureau. The HHI is calculated using the mid-point of available employment size brackets.

2.10.6 Within-chain Price Rigidity

To understand why the pass-through elasticity estimates are heterogeneous across store types as shown in Table 2.14, I first show the proportion of revenue generated by each of five product departments across store types in Table 2.13. Drug stores earn most of their revenue from health and beauty care products, whereas both grocery and merchandise stores earn most of their revenue from food, although grocery stores earn a lot more from food at around 77%. If consumers respond to SNAP-benefit increases mostly by changing demand for products such as food but not other types of products as shown in Section 2.5.2, we would see smaller price responses in drug and merchandise stores, both of which do not derive the majority of their revenue from selling food.

Next, I follow DellaVigna and Gentzkow (2017) to measure the extent of price rigidity for each of the retail chains in the data. First, I pick the top UPC from each of 12 product modules with high revenue: canned soup, cat food, chocolate, coffee, cookies, carbonated soft drinks, yogurt, orange juice, bleach, toilet tissue, paper towel, and tooth cleaners. Next, I calculate the weekly correlation in prices for each store pair as a similarity measure, first demeaning the price at the store-quarter-product level before calculating the correlations over all weeks and products that are not missing both store pairs. For each chain, I define the flexibility measure as the percentage difference between the average correlation for store pairs within the same state and the average correlation for store pairs across different states. The closer the flexibility measure is to zero, the more rigid the chain pricing. Because multiple product modules exist, I take either the mean or median flexibility measure across product modules for each chain. DellaVigna and Gentzkow (2017) perform the same exercise using an alternative similarity measure.

The pass-through elasticity estimated from local variation in SNAP benefits per population should be affected by the extent to which chains are pricing rigidly. Chains that price flexibly across states as well as chains that price rigidly but are located primarily in one state should exhibit local pricing and react to local shocks, whereas chains that price rigidly and locate across many states should exhibit national pricing and will not react to local shocks. I plot the distribution of flexibility measures as well as number of states each chain is in across stores by store type in Figure 2.19. Both drug and merchandise stores belong to a few large chains that price rigidly, whereas a large amount of grocery stores belong to chains that price flexibly or chains that are located only in a few states. Over 50 grocery chains are in the sample whereas both drug and merchandise stores come from around five retail chains each.[23] This implies most grocery stores are engaging in local pricing, whereas drug and merchandise stores are not.

2.10.7 Firm Dynamics and Market Structure

To investigate the impact of SNAP-benefit changes on firm dynamics and market structure, we use two additional datasets. First, we use FNS data on a yearly panel of retail stores that participate in SNAP from 2006 to 2015. For each store, we observe the years in which it is contained in the sample, that is, registered with the FNS to sell to SNAP consumers. We also observe the exact authorization date, the name and address of the store including the county it is located in, and the store type. To focus on the grocery industry, we restrict our sample to stores that are classified as grocery stores, supermarkets, or superstores. We then construct five county-level outcomes of interest from the data: (1) authorization rate, the number of newly authorized stores per month divided by the total number of stores, (2) entry rate, the number of new stores in the sample each year divided by the total number of stores, (3) exit rate, the number of stores that exit the sample the next year divided by the total number of stores the previous year, (4) reallocation rate, the sum of entry and exit rates, and (5) log count per population, the log of the total number of stores per population. Second, we use County Business Patterns (CBP) data from the Census Bureau to obtain two additional annual outcomes at the county-level: (6) log establishments per population, the log of the number of establishments per population, and (7) HHI, the sum of squared market shares calculated using employment size as a measure of firm size.[24] We then estimate equation (2.5) at the county-month level using the seven outcomes, again using data

23. Data from the Economic Census also show the grocery store industry is much less concentrated and less dominated by chains than the drug and merchandise store industries. For example, market share of the 4 largest firms is 30.7%, 54.4%, and 73.2% for grocery stores, drug stores, and merchandise stores respectively in 2007.

24. Since the CBP only provides employment size in 13 size brackets, we use the mid-point of each bracket to construct the HHI.

from 2007 to 2010 to focus on the periods in which SNAP benefits were increased.

As shown in Table 2.15, the estimated effect on entry and exit is positive but almost all of the estimates are statistically insignificant except the reallocation rate, suggesting that SNAP-benefit changes had little impact on entry and exit margins and market structure. A 10% increase in benefits per population increases the reallocation rate by 0.01, compared to a mean of around 0.17 for grocery stores in the SNAP panel.

2.10.8 Derivations

Under perfect competition, we can obtain the pass-through elasticity by differentiating the equilibrium condition with respect to benefits:

$$
\begin{aligned}
Q^D(p,b) = Q^{D,S}(p,b) + Q^{D,NS}(p) &= Q^S(p) \\
\frac{\partial Q^{D,S}(p,b)}{\partial p}\frac{dp}{db} + \frac{\partial Q^{D,S}(p,b)}{\partial b} + \frac{\partial Q^{D,NS}(p)}{\partial p}\frac{dp}{db} &= \frac{\partial Q^S(p)}{\partial p}\frac{dp}{db} \\
\rho \equiv \frac{dp}{db} &= \frac{\frac{\partial Q^{D,S}(p,b)}{\partial b}}{\frac{\partial Q^S(p)}{\partial p} - \frac{\partial Q^D(p,b)}{\partial p}} \\
\varepsilon_\rho \equiv \frac{dp}{db}\frac{b}{p} &= \frac{\frac{\varepsilon_{Q^{D,S},b}}{-\varepsilon_D}\frac{pQ^{D,S}}{pQ}}{1-\frac{\varepsilon_S}{\varepsilon_D}}. \qquad (2.32)
\end{aligned}
$$

Likewise, we can derive the pass-through formula under symmetric imperfect competition. First, we start from the profit-maximization condition and differentiate it with respect to the amount of benefits. In addition, we allow the demand elasticity to depend on the amount of benefits. We obtain an expression for the quantity response to benefits:

$$
\begin{aligned}
P(Q,b)+\theta\frac{\partial P(Q,b)}{\partial Q}Q-c'(Q)&=0\\
MR(Q,b)-MC(Q)&=0\\
\left(\frac{\partial MR}{\partial Q}-\frac{\partial MC}{\partial Q}\right)\frac{dQ}{db}+\frac{\partial MR}{\partial b}&=0\\
\frac{dQ}{db}&=-\frac{\frac{\partial MR}{\partial b}}{\frac{\partial MR}{\partial Q}-\frac{\partial MC}{\partial Q}}\\
\frac{dQ}{db}&=-\frac{\theta\frac{\partial^2 p}{\partial b\partial Q}Q+\frac{\partial p}{\partial b}}{\frac{\partial MR}{\partial Q}-\frac{\partial MC}{\partial Q}}.
\end{aligned}
\tag{2.33}
$$

Next, we use to above expression to obtain the pass-through formula:

$$
\begin{aligned}
\frac{dp}{db}&=\frac{\partial p}{\partial Q}\frac{dQ}{db}+\frac{\partial p}{\partial b}\\
&=\left(1-\frac{\frac{\partial p}{\partial Q}}{\frac{\partial MR}{\partial Q}-\frac{\partial MC}{\partial Q}}\right)\frac{\partial p}{\partial b}+\left(-\frac{\frac{\partial p}{\partial Q}}{\frac{\partial MR}{\partial Q}-\frac{\partial MC}{\partial Q}}\theta\frac{\partial^2 p}{\partial b\partial Q}Q\right)\\
\varepsilon_\rho\equiv\frac{dp}{db}\frac{b}{p}&=\left(1-\frac{\frac{\partial p}{\partial Q}}{\frac{\partial MR}{\partial Q}-\frac{\partial MC}{\partial Q}}\right)\frac{\varepsilon_{Q^{D,S},b}}{-\varepsilon_D}\frac{pQ^{D,S}}{pQ}+\frac{\frac{\partial p}{\partial Q}}{\frac{\partial MR}{\partial Q}-\frac{\partial MC}{\partial Q}}\frac{\theta}{-\varepsilon_D}\varepsilon_{p',b}\\
&=\left(1-\frac{1}{1+\frac{\theta}{\varepsilon_\theta}+\frac{\varepsilon_D-\theta}{\varepsilon_S}+\frac{\theta}{\varepsilon_{ms}}}\right)\frac{\varepsilon_{Q^{D,S},b}}{-\varepsilon_D}\frac{pQ^{D,S}}{pQ}+\left(\frac{1}{1+\frac{\theta}{\varepsilon_\theta}+\frac{\varepsilon_D-\theta}{\varepsilon_S}+\frac{\theta}{\varepsilon_{ms}}}\right)\frac{\theta}{-\varepsilon_D}\varepsilon_{p',b}.
\end{aligned}
\tag{2.34}
$$

Next, we derive the changes in consumer and producer surplus of the program. Using the original inverse market-demand curve $P(Q)$, the change in consumer surplus from an increase in SNAP benefits can be written as follows:

$$
\begin{aligned}
CS(p) &= \int_{P(Q(p))}^{\infty} Q^D(x)dx \\
\frac{dCS}{db} &= \left(-\frac{\partial P}{\partial Q}\frac{dQ}{db} \right) Q \\
&= \left(-\frac{\partial P}{\partial Q}\frac{\rho - \frac{\partial p}{\partial b}}{\frac{\partial P}{\partial Q}} \right) Q \\
&= \left(\frac{\partial P}{\partial b} - \rho \right) Q \\
&= \left(\frac{\varepsilon_{Q,b}}{-\varepsilon_D} - \varepsilon_\rho \right) \frac{pQ}{b} \\
&= \left(\frac{\varepsilon_{Q^{D,S},b}}{-\varepsilon_D} - \varepsilon_\rho \right) \frac{pQ^{D,S}}{b} + \left(-\varepsilon_\rho \right) \frac{pQ^{D,NS}}{b}.
\end{aligned} \tag{2.35}
$$

Note that if we allow the demand curve to shift out from the benefit increase rather than using the original demand curve, $\frac{dCS}{db} = -\rho Q + \int_p^\infty \frac{\partial Q^D(x,b)}{\partial b}dx = -\rho Q + \int_0^Q \frac{\partial P(x,b)}{\partial b}dx$. Hence, the assumption of no income effects and the use of the original demand curve, can be replaced by the assumption of approximating the integral $\int_0^Q \frac{\partial P(x,b)}{\partial b}dx$ by $\frac{\partial P}{\partial b}Q$, that is, assuming $\frac{\partial P(x,b)}{\partial b}$ is constant. In other words, we assume that the demand shift is parallel. Under perfect competition, the change in producer surplus can be written as follows:

$$
\begin{aligned}
PS(p) &= \int_0^p Q^S(x)dx \\
\frac{dPS}{db} &= \rho Q = \varepsilon_\rho \frac{pQ}{b}.
\end{aligned} \tag{2.36}
$$

We can extend our results to the case of symmetric imperfect competition following Weyl and Fabinger (2013). $\frac{dCS}{db}$ remains unchanged whereas the change in producer surplus can be rewritten as follows:

$$
\begin{aligned}
PS &= (p-c)Q \\
\frac{dPS}{db} &= \rho Q + (p-c)\frac{dQ}{db} \\
&= \left(\varepsilon_\rho + \frac{p-c}{p}\frac{dQ}{db}\frac{b}{Q}\right)\frac{pQ}{b} \qquad (2.37)\\
&= \left[\rho + \theta\left(\frac{\partial p}{\partial b} - \rho\right)\right]Q \\
&= \left[\varepsilon_\rho + \theta\left(\frac{\varepsilon_{Q,b}}{-\varepsilon_D} - \varepsilon_\rho\right)\right]\frac{pQ}{b}. \qquad (2.38)
\end{aligned}
$$

To better understand the price and quantity movements seen in Table 2.8, we extend pass-through formulas to a multi-product setting. In the case of perfect competition, we rewrite the equilibrium condition with demand $Q_j^D(p)$ for good j as a function of a vector of prices $p = (p_1, ..., p_n)$ below:

$$
Q_j^D(p,b) = Q_j^{D,S}(p,b) + Q_j^{D,NS}(p,b) = Q_j^S(p)
$$

$$
\sum_{i=1}^n \frac{\partial Q_j^{D,S}(p,b)}{\partial p_i}\frac{dp_i}{db} + \frac{\partial Q_j^{D,S}(p,b)}{\partial b} + \sum_{i=1}^n \frac{\partial Q_j^{D,NS}(p)}{\partial p_i}\frac{dp_i}{db} = \sum_{i=1}^n \frac{\partial Q_j^S(p)}{\partial b}\frac{dp_i}{db}
$$

$$
\begin{aligned}
\rho_j \equiv \frac{dp_j}{db} &= \frac{\frac{\partial Q_j^{D,S}(p,b)}{\partial b} + \sum_{i\neq j}\frac{\partial Q_j^{D,S}(p,b)}{\partial p_i}\frac{dp_i}{db} - \sum_{i\neq j}\frac{\partial Q_j^S(p,b)}{\partial p_i}\frac{dp_i}{db}}{\frac{\partial Q_j^S(p)}{\partial p_j} - \frac{\partial Q_j^D(p,b)}{\partial p_j}} \\
\varepsilon_{\rho_j} \equiv \frac{dp_j}{db}\frac{b}{p_j} &= \frac{\frac{\varepsilon_{Q_j^{D,S},b}}{-\varepsilon_j^D}\frac{p_j Q_j^{D,S}}{p_j Q_j} + \sum_{i\neq j}\frac{\varepsilon_{j,i}^D - \varepsilon_{j,i}^S}{-\varepsilon_j^D}\varepsilon_{\rho_i}}{1 - \frac{\varepsilon_j^S}{\varepsilon_j^D}}. \qquad (2.39)
\end{aligned}
$$

2.10.9 Pass-through Rates and Demand Curvature

Using derivations from Weyl and Fabinger (2013) and the fact that the subsidy pass-through rate is 1 minus the cost pass-through rate, the subsidy pass-through rate under symmetric imperfect competition is given by the following expression:

$$1 - \frac{\frac{\partial p}{\partial q}}{\frac{\partial MR}{\partial q} - \frac{\partial MC}{\partial q}} = 1 - \frac{1}{1 + \frac{\theta}{\varepsilon_\theta} + \frac{\varepsilon_D - \theta}{\varepsilon_S} + \frac{\theta}{\varepsilon_{ms}}}. \tag{2.40}$$

Given we do not find evidence that the market structure changes when benefits change and the fact that Stroebel and Vavra (2015) find evidence that marginal costs are not responsive to demand shocks, we assume θ is constant, that is, $\frac{1}{\varepsilon_\theta}$ is zero, and supply is perfectly elastic, so the pass-through rate can now be rewritten as

$$1 - \frac{\frac{\partial p}{\partial q}}{\frac{\partial MR}{\partial q} - \frac{\partial MC}{\partial q}} = 1 - \frac{1}{1 + \frac{\theta}{\varepsilon_{ms}}}. \tag{2.41}$$

The pass-through rate is now dependent only on the market conduct and the curvature of demand. As mentioned in Section 2.6.3, we obtain demand elasticities by regressing log real sales on log price indices for both SNAP-eligible and -ineligible goods with store and period fixed effects. To obtain an estimate of the curvature of demand, we first show log real sales against log price in a binned scatter plot in Figure 2.20 for both SNAP-eligible and -ineligible goods. Both groups exhibit log-concave demand, and we quantify the degree of log-concavity by regressing log real sales on log prices as well as additional polynomial terms of log prices. Results are similar by adding either a quadratic or cubic term, so we present results using a quadratic term only. More specifically, we estimate the following equation with either $m = 2$ or $m = 3$:

$$lnQ_{it} = \alpha + \sum_{k=1}^{m} \beta_k (lnP_{it})^k + \alpha_i + \alpha_{zt} + \varepsilon_{it}. \tag{2.42}$$

We can then obtain an estimate of the inverse elasticity of marginal surplus, which can be written as:

$$\frac{1}{\varepsilon_{ms}} = -\frac{\sum_{k=1}^{m} \beta_k k(k-1)(lnP_{it})^{k-2} - \sum_{k=1}^{m} \beta_k k(lnP_{it})^{k-1}}{\left(\sum_{k=1}^{m} \beta_k k(lnP_{it})^{k-1}\right)^2}. \tag{2.43}$$

We show the estimation results in Table 2.20. Plugging in the mean of lnP_{it} and assuming a market conduct parameter of 0.2 and 0.5 for eligible and ineligible goods, respectively, we calculate a subsidy pass-through rate of 0.49 and 0.15, respectively. Given the predicted pass-through elasticity, that is, the magnitude of the shift effect under complete pass-through, is 0.08 and 0.124 under our MPC estimate and the estimate from HS, this implies a price response of 0.039 and 0.06, respectively, which is slightly lower than our empirical estimate of 0.0724, implying a relatively small slope effect.

2.10.10 Multi-product Pricing Models

We illustrate how the baseline model in Chen and Rey (2012) can generate the following result: as the valuation of one product increases, the price of the other product increases. We first follow their setup and assume that there are two retailers. The first retailer is a large firm L with a broader product range that monopolizes product A and also sells product B of variety B_L in the competitive market. The second retailer is a small firm S that sells product B of variety B_S in the competitive market. Applied to our setting, product A is a SNAP-ineligible good (non-food) whereas product B is a SNAP-eligible good (food). Almost all retail stores in our data sell both food and non-food and belong to retail chains so they are considered as "large" firms. Small firms may be small discount grocers that specialize in selling food only. Consumers have unit inelastic demand for both products A and B, and value A, B_L, and B_S at u_A, u_L, and u_S, respectively, whereas producers face unit costs of c_A, c_L, and c_S, respectively. S supplies B_S at cost ($p_S = c_S$). For each unit of B_S, consumer surplus v_S equals total surplus w_S of $u_S - c_S$. Suppose $w_S > w_L \equiv u_L - c_L > 0$ to ensure multi-stop shopping. This can be driven either by the fact that $c_S < c_L$, where small firms are hard discounters, or $u_S > u_L$, where small firms are specialists. Likewise, define total surplus for a unit of A as $w_A \equiv u_A - c_A > 0$ and total surplus from buying a unit of both A and B_L as $w_{AL} \equiv w_A + w_L$. Finally, shopping cost s is drawn from a cumulative density function $F(\cdot)$ and density function $f(\cdot)$. Let $r_{AL} \equiv p_A - c_A + p_L - c_L$ be the producer surplus of a unit of A and $v_{AL} \equiv u_A + u_L - p_A - p_L = w_{AL} - r_{AL}$ be the consumer surplus from buying a unit of A and B_L each. One-stop shoppers prefer L to S if $v_{AL} \geq v_S = w_S$ and patronize L as long as $v_{AL} \geq s$. Consumers will multi-stop shop if $\tau \equiv v_S - v_L = w_S - (w_L - r_L) \geq s$, that is, the extra cost of visiting S is exceeded by the extra value it offers. Profits of L are then given by the following equation:

$$\pi_L = r_{AL}(F(v_{AL}) - F(\tau)) + r_A F(\tau) = r_{AL}F(v_{AL}) - r_L F(\tau). \tag{2.44}$$

The mass of one-stop shoppers is $F(v_{AL}) - F(\tau)$, whereas the mass of multi-stop shoppers is $F(\tau)$. Now we consider the optimal pricing strategy of firm L. Assume the inverse hazard rate $h(\cdot) \equiv F(\cdot)/f(\cdot)$ is strictly increasing to ensure quasi-concavity of L's profit function. The first order condition for r_L and r_{AL} gives the following equations:

$$
\begin{aligned}
r_L^* &= -h(\tau^*) < 0 && (2.45)\\
\tau^* &\equiv l^{-1}(w_S - w_L) > 0 \\
r_{AL}^m &= h(v_{AL}^m) && (2.46)\\
v_{AL}^m &\equiv l^{-1}(w_{AL}),
\end{aligned}
$$

where $l(s) \equiv s + h(s)$ and $l' > 0$. As long as $v_{AL}^m \geq w_S$, the optimal strategy of the firm is to charge r_{AL}^m for the bundle and $r_L^* = -h(\tau^*)$ for B_L. Now suppose the valuations u_L and u_S for product B_L and B_S increase due to an exogenous increase in SNAP benefits, and for simplicity, let $w_S - w_L$ remain unchanged. Then r_L^* remains unchanged, whereas r_{AL}^m increases since w_{AL} increases. Therefore, firm L increases the price for A and holds the price for B_L fixed. Chen and Rey (2012) show that their results on loss leading can be extended to heterogeneous valuations for A and imperfect competition among large retailers. The result that an increase in valuation for the more competitive product will lead to a rise in price of the less competitive one can be extended similarly.

2.10.11 Tables

Table 2.11: SNAP participation and effect of SNAP-benefit changes on prices by product department, grocery stores

	(1)	(2)	(3)	(4)	(5)
Product department	Health & Beauty Care	Food	Non-food Grocery	Alcohol	General Merchandise
VARIABLES			Log price index		
Baseline					
Log benefits p.p.	0.0747**	0.0741***	0.163***	0.0910	-0.0116
	(0.0357)	(0.0267)	(0.0451)	(0.0544)	(0.0286)
Interaction					
Log benefits p.p.	0.0660**	0.0667**	0.148***	0.0893	-0.0169
	(0.0326)	(0.0277)	(0.0453)	(0.0537)	(0.0279)
x Participation rate	0.140***	0.117***	0.247***	0.0260	0.0857**
	(0.0481)	(0.0397)	(0.0688)	(0.0728)	(0.0372)
Observations	381612	382140	381900	364044	380988
Number of units	7951	7962	7957	7585	7938
Number of clusters	48	48	48	48	48
Revenue share	0.0512	0.768	0.0949	0.0653	0.0206

Notes: Robust standard errors are in parentheses, clustered at the state level. *** p<0.01, ** p<0.05, * p<0.1. Controls refer to the full set of controls including log housing prices, log unemployment rate, log employment to population ratio, log population, log average wage, poverty rates at both the state and county levels, as well as log SNAP participants at the state level. Store and period fixed effects are also included. Log benefits p.p. refers to log benefits per population. Participation refers to the ratio of SNAP participants to population in each county fixed to the pre-period of 2006.

Table 2.12: SNAP participation and effect of SNAP-benefit changes on real sales by product department, grocery stores

	(1)	(2)	(3)	(4)	(5)
Product department	Health & Beauty Care	Food	Non-food Grocery	Alcohol	General Merchandise
VARIABLES	Log real sales				
Baseline					
Log benefits p.p.	0.00913	0.0613	-0.265*	-0.199	0.0385
	(0.145)	(0.108)	(0.138)	(0.280)	(0.160)
Interaction					
Log benefits p.p.	0.00337	0.0387	-0.247*	-0.199	0.0320
	(0.143)	(0.103)	(0.132)	(0.264)	(0.156)
x Participation rate	0.0918	0.358***	-0.276	0.00306	0.104
	(0.200)	(0.118)	(0.188)	(0.399)	(0.184)
Observations	381612	382140	381900	364044	380988
Number of units	7951	7962	7957	7585	7938
Number of clusters	48	48	48	48	48
Revenue share	0.0512	0.768	0.0949	0.0653	0.0206

Notes: Robust standard errors are in parentheses, clustered at the state level. *** p<0.01, ** p<0.05, * p<0.1. Controls refer to the full set of controls including log housing prices, log unemployment rate, log employment to population ratio, log population, log average wage, poverty rates at both the state and county levels, as well as log SNAP participants at the state level. Store and period fixed effects are also included. Log benefits p.p. refers to log benefits per population. Participation refers to the ratio of SNAP participants to population in each county fixed to the pre-period of 2006.

Table 2.13: Proportion of revenue earned by each product department by store type

Store Type	Product department				
	Health & Beauty Care	Food	Non-Food Grocery	Alcohol	General Merchandise
Drug	0.479	0.216	0.171	0.052	0.083
Food	0.053	0.765	0.094	0.065	0.023
Merchandise	0.219	0.312	0.225	0.008	0.237

Notes: This table lists the proportion of revenue earned by each product department by store type across all stores from 2006 to 2015.

Table 2.14: Effect of SNAP-benefit changes on prices and real sales, drug and merchandise stores

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Store Type	Drug				Merchandise			
Specification	OLS	IV	OLS	IV	OLS	IV	OLS	IV
VARIABLES	Log price index		Log real sales		Log price index		Log real sales	
Log benefits per population	0.00113	0.0250	-0.0155	0.198*	0.00567	0.0311*	0.00950	-0.0646
	(0.00651)	(0.0251)	(0.0363)	(0.113)	(0.00408)	(0.0175)	(0.0236)	(0.0754)
Observations	445467	445467	445467	445467	385527	385527	385527	385527
R-squared	0.912	0.912	0.977	0.976	0.915	0.914	0.991	0.991
Prob > F	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Number of units	9281	9281	9281	9281	8032	8032	8032	8032
Number of clusters	48	48	48	48	49	49	49	49
First stage F-stat		21.167		21.167		14.903		14.903

Notes: Robust standard errors are in parentheses, clustered by state. Control variables as well as store and period fixed effects are included. *** p<0.01, ** p<0.05, * p<0.1.

Table 2.15: Effect of SNAP-benefit changes on grocery store firm dynamics and market structure

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Sample	SNAP stores					All stores	
VARIABLES	Authorization	Entry	Exit	Reallocation	Log count p.p.	Log est. p.p.	HHI
Log benefits p.p.	0.0319	0.0525	0.0481	0.101**	0.0778	-0.0237	0.00716
	(0.0330)	(0.0362)	(0.0430)	(0.0487)	(0.0697)	(0.0817)	(0.00733)
Observations	149178	149178	149178	149178	144342	146562	146562
R-squared	0.497	0.438	0.398	0.526	0.978	0.968	0.980
Prob > F	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Number of units	3108	3108	3108	3108	3023	3070	3070
Number of clusters	49	49	49	49	49	49	49
First stage F-stat	22.822	22.822	22.822	22.822	22.878	22.846	22.846

Notes: Robust standard errors are in parentheses, clustered by state. *** p<0.01, ** p<0.05, * p<0.1. Control variables as well as county and period fixed effects are included. Authorization refers to the authorization rate, that is, the number of newly authorized stores per month divided by the total number of SNAP-authorized grocery stores in each county. Entry and exit refers to the entry and exit rates, that is, the number of entering and exiting stores in a particular year divided by the total number of SNAP-authorized grocery stores in each county. Reallocation is the sum of entry and exit rates. Log count p.p. refers to the log of the total number of SNAP-authorized grocery stores per population in each county. Log est. p.p. refers to the log of the total number of grocery establishments per population in each county. HHI refers to a county-level measure of HHI, using the number of employees as the measure of firm size for the grocery industry.

Table 2.16: Effect of SNAP-benefit changes on consumption of households, weekly

	(1)	(2)	(3)	(4)
Specification	OLS, log-log	IV, log-log	OLS, level-level	IV, level-level
VARIABLES	Consumption			
Both ineligible	0.105**	0.287**	0.0281*	0.0321
	(0.0501)	(0.125)	(0.0166)	(0.0404)
Household eligible only	0.0508	0.239*	0.00297	0.00526
	(0.0778)	(0.139)	(0.0210)	(0.0403)
Product eligible only	0.0174	0.223*	0.0221*	0.0273
	(0.0393)	(0.113)	(0.0121)	(0.0375)
Both eligible	0.158***	0.348***	0.0521**	0.0563
	(0.0491)	(0.118)	(0.0229)	(0.0434)
Both ineligible x fraction issued	0.00846	0.0101	0.00717	0.00410
	(0.0248)	(0.0294)	(0.0117)	(0.0136)
Household eligible only x fraction issued	0.0380	0.0396	0.0436***	0.0404**
	(0.0252)	(0.0300)	(0.0131)	(0.0153)
Product eligible only x fraction issued	0.00561	0.00724	0.00801	0.00485
	(0.0247)	(0.0293)	(0.0116)	(0.0135)
Both eligible x fraction issued	0.0439*	0.0455	0.0796***	0.0765***
	(0.0242)	(0.0291)	(0.0125)	(0.0152)
Observations	6797627	6797627	6798234	6798234
R-squared	0.271	0.271	0.270	0.270
Prob > F	0.000	0.000	0.000	0.000
Number of units	23789	23789	23789	23789
Number of clusters	49	49	49	49
First stage F-stat		6.840		8.643

Notes: Robust standard errors are in parentheses, clustered by state. *** p<0.01, ** p<0.05, * p<0.1. Each coefficient comes from a regression of (log) weekly consumption on (log) monthly SNAP benefits per recipient, interacted with four group indicators and the fraction issued. Both ineligible refers to expenditures by SNAP-ineligible households on SNAP-ineligible products, household eligible only refers to expenditures by SNAP-eligible households on SNAP-ineligible products, product eligible only refers to expenditures by SNAP-ineligible households on SNAP-eligible products, and both eligible refers to expenditures by SNAP-eligible households on SNAP-eligible products. Fraction issued refers to the fraction of days out of a month in which SNAP benefits are issued during that week. Only the first four weeks of each month are kept in the sample. Control variables as well as household-month and year-month-week fixed effects are also included. Observations are weighted by sampling weights.

Table 2.17: Effect of SNAP-benefit changes on shopping behavior of households

Specification	(1) OLS	(2) IV	(3) OLS	(4) IV	(5) OLS	(6) IV	(7) OLS	(8) IV
VARIABLES	Coupon share		Deal share		Store brand share		Log trips	
Both ineligible	0.00394	0.0272**	-0.00605	0.00485	-0.0301**	-0.107**	-0.0384	-0.00719
	(0.00323)	(0.0107)	(0.0122)	(0.0555)	(0.0112)	(0.0469)	(0.0325)	(0.165)
Household eligible only	-0.00732*	0.0161	-0.0687***	-0.0589	-0.0461***	-0.123**	-0.0360	-0.00918
	(0.00374)	(0.00995)	(0.0184)	(0.0557)	(0.0156)	(0.0493)	(0.0514)	(0.179)
Product eligible only	0.00314	0.0269**	0.0210	0.0376	-0.0290**	-0.108**	-0.0449	-0.0157
	(0.00235)	(0.0102)	(0.0133)	(0.0557)	(0.0121)	(0.0461)	(0.0319)	(0.163)
Both eligible	-0.00569**	0.0177	0.00192	0.0195	-0.0542***	-0.132***	-0.0441	-0.0185
	(0.00266)	(0.0106)	(0.0194)	(0.0588)	(0.0143)	(0.0462)	(0.0520)	(0.183)
Observations	2044662	2044662	2044662	2044662	2044662	2044662	2044666	2044666
R-squared	0.624	0.623	0.734	0.734	0.454	0.454	0.770	0.770
Prob > F	0.000	0.000	0.000	0.000	0.000	0.000	0.011	0.000
Number of units	23911	23911	23911	23911	23911	23911	23911	23911
Number of clusters	49	49	49	49	49	49	49	49
First stage F-stat		12.258		12.258		12.258		12.258

Notes: Robust standard errors are in parentheses, clustered by state. *** p<0.01, ** p<0.05, * p<0.1. Each coefficient comes from a regression of the outcome on log SNAP benefits per recipient, interacted with four group indicators. Both ineligible refers to outcomes by SNAP-ineligible households on SNAP-ineligible products, household eligible only refers to outcomes by SNAP-eligible households on SNAP-ineligible products, product eligible only refers to outcomes by SNAP-ineligible households on SNAP-eligible products, and both eligible refers to outcomes by SNAP-eligible households on SNAP-eligible products. Control variables as well as household-month and period fixed effects are also included. Observations are weighted by sampling weights.

Table 2.18: SNAP participation and effect of SNAP-benefit changes on prices, ARRA expiration

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Specification	OLS	IV	OLS	IV	OLS	IV	OLS	IV
VARIABLES	Log price index							
Log benefits p.p.	0.0366***	0.101	-0.00252	-0.0441	0.0250	0.0895	0.00389	-0.0431
	(0.0122)	(0.0901)	(0.0127)	(0.0492)	(0.0159)	(0.113)	(0.0156)	(0.0612)
x Participation rate					0.0775	0.0629	-0.0427	-0.00699
					(0.0537)	(0.147)	(0.0780)	(0.0954)
Observations	85836	85836	321885	321885	85836	85836	321885	321885
R-squared	0.972	0.971	0.915	0.914	0.972	0.971	0.915	0.914
Prob > F	0.000	0.000	0.000	0.004	0.000	0.000	0.000	0.002
Number of units	7153	7153	7153	7153	7153	7153	7153	7153
Number of clusters	48	48	48	48	48	48	48	48
First stage F-stat		5.182		9.670		2.632		4.952
Sample	2013	2013	2012-2015	2012-2015	2013	2013	2012-2015	2012-2015

Notes: Robust standard errors are in parentheses, clustered by state. *** p<0.01, ** p<0.05, * p<0.1. Control variables as well as store and period fixed effects are also included. Log benefits p.p. refers to log benefits per population. Participation refers to the ratio of SNAP participants to population in each county fixed to the pre-period of 2011.

Table 2.19: Effect of SNAP-benefit changes on consumption of households, ARRA expiration

	(1)	(2)	(3)	(4)
Specification	OLS, log-log	IV, log-log	OLS, log-log	IV, log-log
VARIABLES	Consumption			
Both ineligible	-0.0641	-0.452	0.0141	0.217
	(0.121)	(0.489)	(0.0926)	(0.308)
Household eligible only	-0.0302	-0.142	-0.123	0.0915
	(0.250)	(0.535)	(0.154)	(0.335)
Product eligible only	-0.144	-0.525	-0.0681	0.202
	(0.106)	(0.526)	(0.0954)	(0.277)
Both eligible	0.207	0.220	-0.0182	0.304
	(0.269)	(0.701)	(0.182)	(0.318)
Observations	518146	518146	1943066	1943066
R-squared	0.510	0.510	0.467	0.467
Prob > F	0.000	0.000	0.000	0.000
Number of units	24748	24748	24748	24748
Number of clusters	49	49	49	49
First stage F-stat		0.936		6.693
Sample	2013	2013	2012-2015	2012-2015

Notes: Robust standard errors are in parentheses, clustered by state. *** p<0.01, ** p<0.05, * p<0.1. Each coefficient comes from a regression of (log) consumption on (log) SNAP benefits per recipient, interacted with four group indicators. Both ineligible refers to expenditures by SNAP-ineligible households on SNAP-ineligible products, product eligible only refers to expenditures by SNAP-ineligible households on SNAP-eligible products, household eligible only refers to expenditures by SNAP-eligible households on SNAP-ineligible products, and both eligible refers to expenditures by SNAP-eligible households on SNAP-eligible products. Control variables, household-month and period fixed effects are also included. Observations are weighted by sampling weights.

Table 2.20: Estimates of demand elasticity, demand curvature, and pass-through rates for SNAP-eligible goods

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
VARIABLES					Log real sales				
Log price	-0.605***	-0.659***	-0.709***	0.214	-0.118	-0.144	0.442	0.306	0.389
	(0.185)	(0.222)	(0.233)	(0.370)	(0.457)	(0.485)	(0.422)	(0.446)	(0.469)
$(\text{Log price})^2$				-2.042**	-1.389	-1.491	-3.631	-4.830***	-5.858***
				(0.842)	(0.899)	(0.949)	(2.335)	(1.661)	(1.679)
$(\text{Log price})^3$							2.920	6.505***	8.286***
							(3.650)	(2.390)	(2.519)
Observations	838260	837168	827388	838260	837168	827388	838260	837168	827388
R-squared	0.934	0.946	0.954	0.934	0.946	0.954	0.934	0.947	0.955
Prob > F	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Number of units	7004	6995	6914	7004	6995	6914	7004	6995	6914
Number of clusters	48	48	48	48	48	48	48	48	48
Time FE	X			X			X		
DMA x Time FE		X			X			X	
Zip3 x Time FE			X			X			X
Implied rate				0.740	0.522	0.489	0.767	0.480	0.597

Notes: Robust standard errors are in parentheses, clustered by state. *** p<0.01, ** p<0.05, * p<0.1. Control variables as well as store and specified period fixed effects are included. DMA refers to Nielsen designated market areas. Zip3 refers to 3-digit zip codes. Implied rate refers to subsidy pass-through rates implied by the estimated coefficients assuming markups are 0.3, which implies a market conduct of about 0.16. The coefficients are used to calculate the elasticity of marginal surplus at the mean log price.

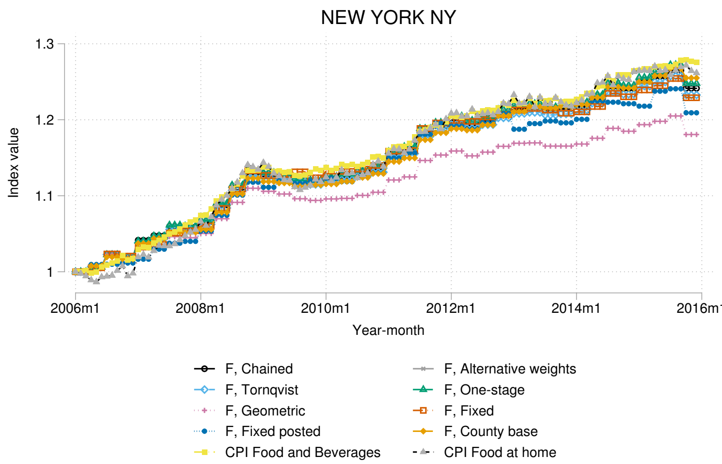
Table 2.21: Incidence of an additional dollar of SNAP benefits for SNAP-ineligible goods

Market conduct	MPC elasticity	Demand elasticity	Proportion of SNAP sales	Pass-through elasticity	Shift magnitude	PS	CS	CS (SNAP)	CS (non-SNAP)
0	0.1182	2.279	0.168	0.0898	0.009	0.178	-0.1608	-0.013	-0.148
0.25	0.1182	2.279	0.168	0.0898	0.009	0.138	-0.1608	-0.013	-0.148
0.5	**0.1182**	**2.279**	**0.168**	**0.0898**	**0.009**	**0.098**	**-0.1608**	**-0.013**	**-0.148**
0.75	0.1182	2.279	0.168	0.0898	0.009	0.057	-0.1608	-0.013	-0.148
1	0.1182	2.279	0.168	0.0898	0.009	0.017	-0.1608	-0.013	-0.148

Notes: MPC elasticities are obtained from Section 2.5.2. Demand elasticity is estimated using panel variation as described in Section 2.6.3. Proportion of SNAP sales is obtained from USDA data. Pass-through elasticity is obtained from Section 2.5.1 and the shift magnitude is the predicted pass-through elasticity obtained using equation (2.11) assuming a subsidy pass-through rate of 1. Surplus calculations are changes in surplus per marginal dollar of SNAP disbursed. PS refers to producer surplus and CS refers to consumer surplus, CS(SNAP) and CS(non-SNAP) refers to consumer surplus for SNAP consumers and consumer surplus for non-SNAP consumers, respectively.

2.10.12 *Figures*

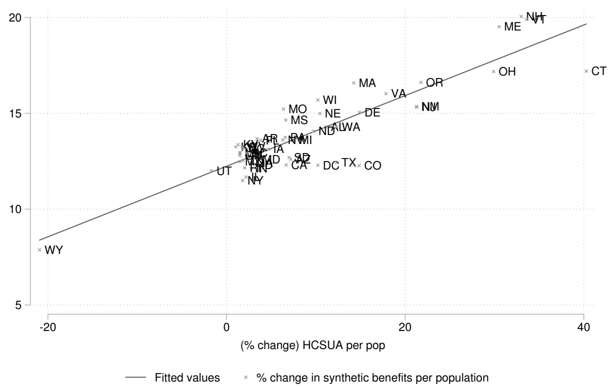
Figure 2.17: Comparison of Nielsen price indices with CPI, grocery stores



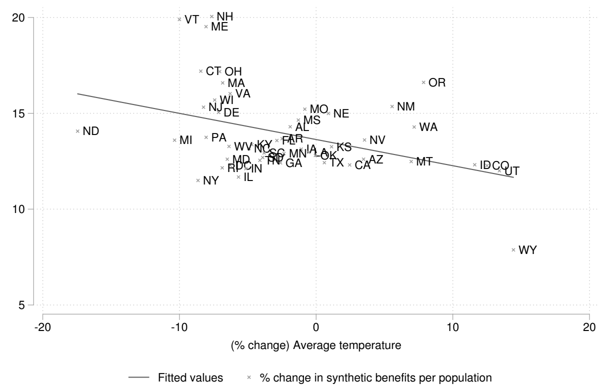
Notes: This figure plots city-level price indices from 2006 to 2015 constructed using Nielsen retail scanner data with alternative methods against those used by the BLS to construct the CPI. F correspond to Nielsen price indices for drug stores, grocery stores, and mass merchandise stores respectively. Nielsen price indices are first constructed at the store level, and aggregated to the city level by taking a sales-weighted average.

Figure 2.18: Changes in synthetic SNAP benefits per population, SUA, energy usage and prices, and temperature by state, Farm Bill

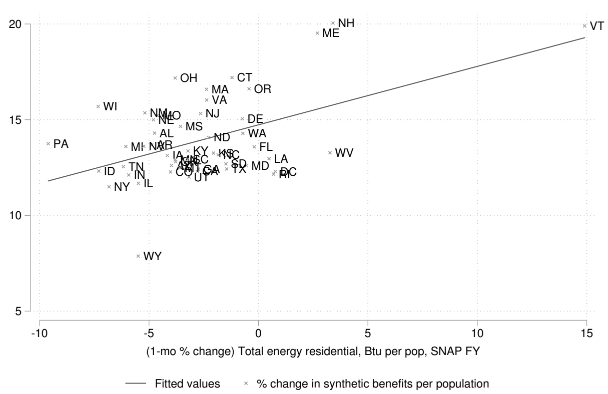
(a) Changes in synthetic benefits per population and HCSUA, Farm Bill



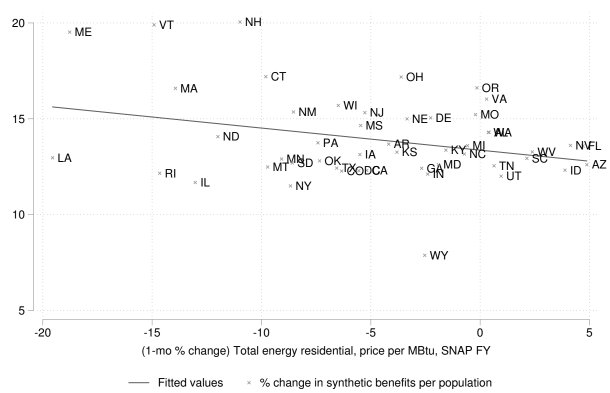
(b) Changes in synthetic benefits per population and temperature, Farm Bill



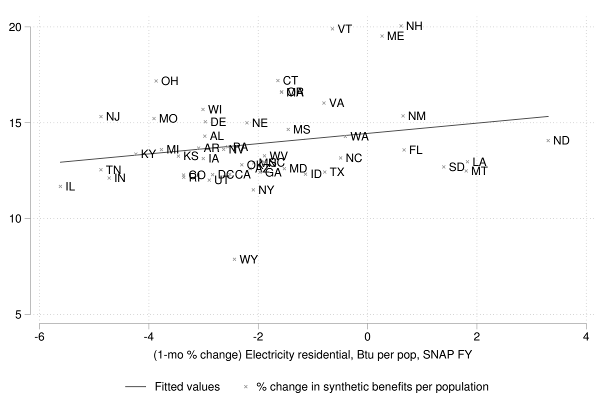
(c) Changes in synthetic benefits per population and total residential energy usage per population, Farm Bill



(d) Changes in synthetic benefits per population and residential energy prices, Farm Bill



(e) Changes in synthetic benefits per population and electricity usage per population, Farm Bill



(f) Changes in synthetic benefits per population and electricity prices, Farm Bill

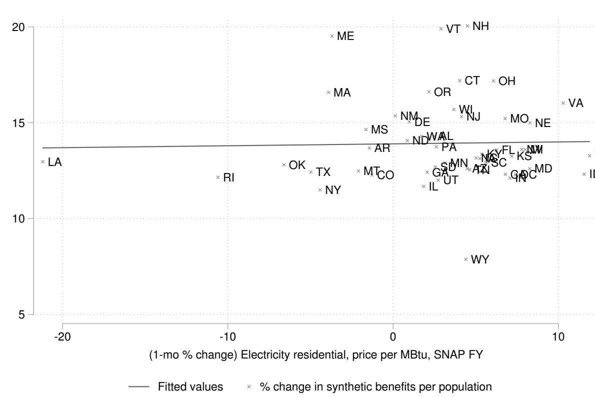
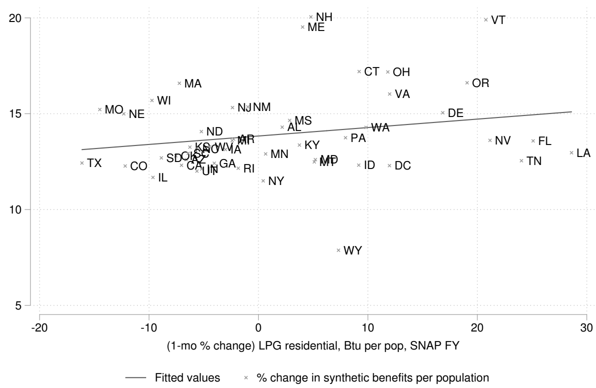
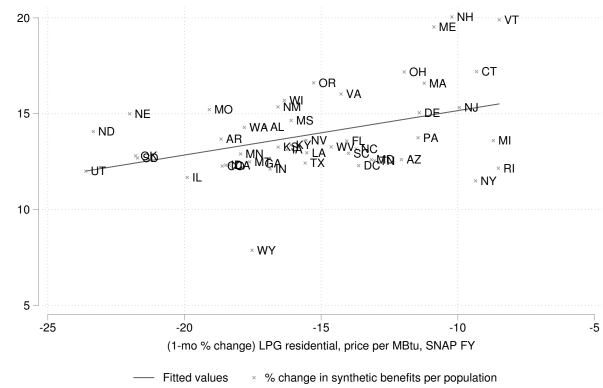


Figure 2.18, continued: Changes in synthetic SNAP benefits per population, SUA, energy usage and prices, and temperature by state, Farm Bill

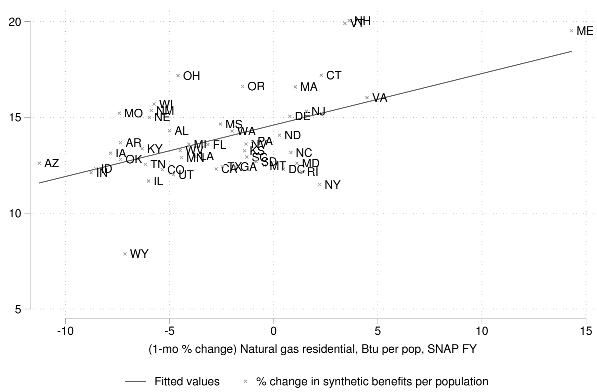
(g) Changes in synthetic benefits per population and LPG usage per population, Farm Bill



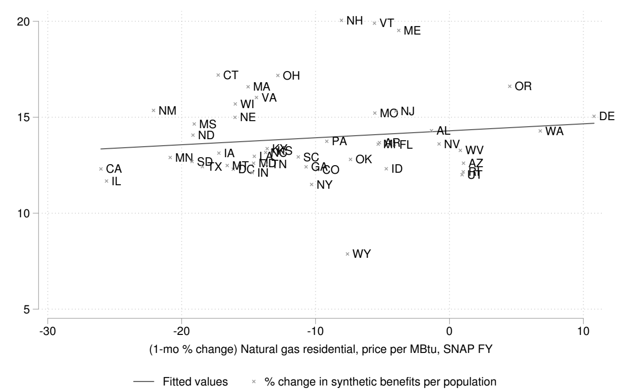
(h) Changes in synthetic benefits per population and LPG prices, Farm Bill



(i) Changes in synthetic benefits per population and natural gas usage per population, Farm Bill



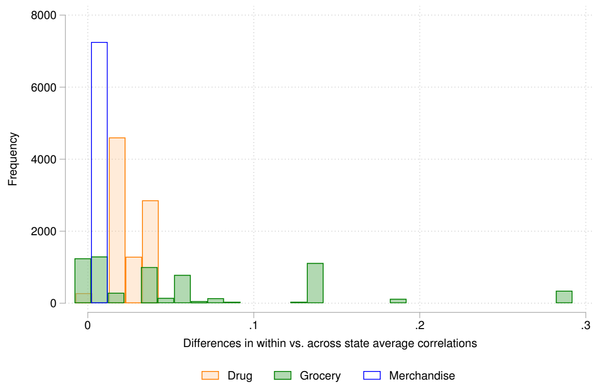
(j) Changes in synthetic benefits per population and natural gas prices, Farm Bill



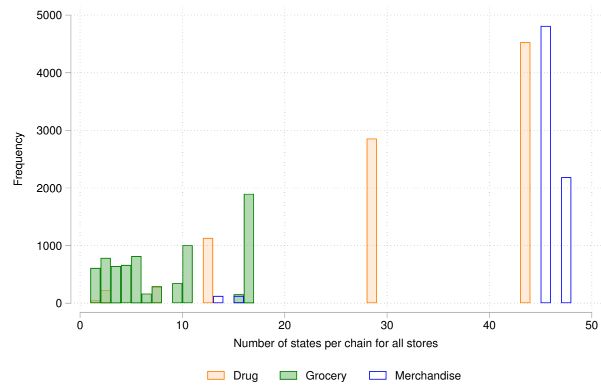
Notes: This figure plots the changes in synthetic SNAP benefits per population, SUA per population, energy usage per population and prices for the residential sector and different fuel types, and temperature by state during the Farm Bill.

Figure 2.19: Distribution of flexibility measures across store types

(a) Flexibility measures



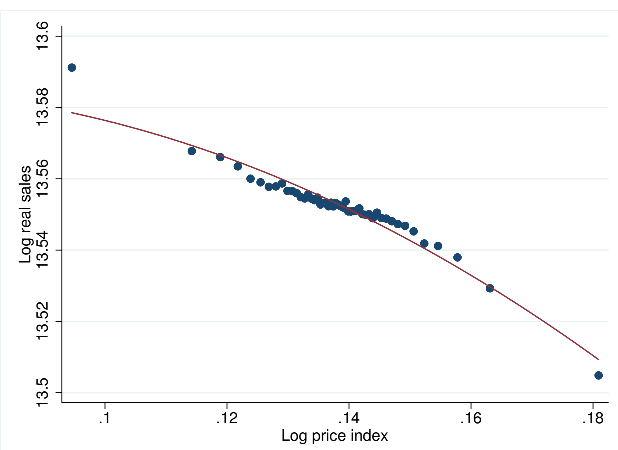
(b) Number of states in each chain for all stores



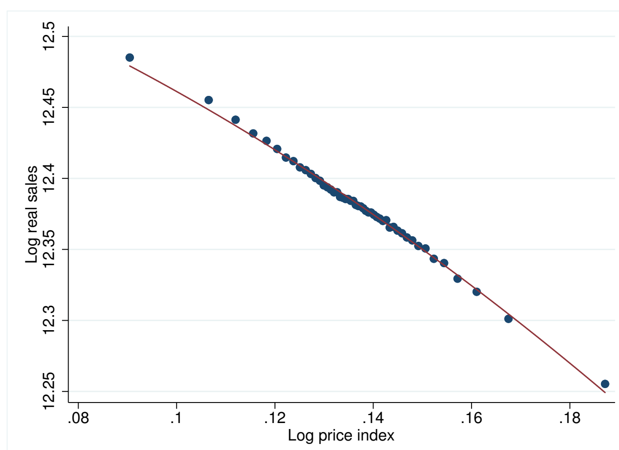
Notes: This figure plots the chosen flexibility measure, differences in within vs. across state average correlations, as illustrated in Appendix Section 2.10.6, and also the number of states in each chain for all stores.

Figure 2.20: Binned scatter plot of log real sales on log prices, SNAP-eligible and ineligible goods

(a) Eligible goods



(b) Ineligible goods



Notes: This figure plots the log real sales against the log price index for SNAP-eligible and ineligible goods. Both variables are residualized by regressing on a set of controls, store fixed effects, and 3-digit zip code x period fixed effects. For each store-year-month observation, log prices are grouped into 50 quantiles. The x-axis displays the mean of the residualized log price in each quantile. The y-axis shows the mean of the residualized log real sales in each quantile. The quadratic curve of best fit is obtained from the regression using all observations.

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