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Abstract

I study how sales tax policy influences prices, sales, and retail employment. I focus on apparel and food markets because of considerable variation in sales tax policy over time and across locations. In the first and second papers, I estimate tax incidence from short-term and long-term sales tax exemptions. I am the first to use restricted access Consumer Price Index micro data on item-level price quotes in this literature. In addition, I find a negative impact of sales taxes on employment in the retail apparel industry; to my knowledge, no other paper has convincingly established this link between sales taxes and inputs. In my third paper, I use Nielsen Scanner Data to explore how the discrepancy between sales tax on restaurant meals and groceries affects the expansion of the pre-prepared meals market.

In Chapter 1, I use frequent state/local policy revisions of apparel tax exemptions in Connecticut, New York, and Vermont and find that consumers bear the burden of a sales tax. In addition, a sales tax hike lowers retail service employment. Unlike the previous literature on this topic, I employ confidential item-level price data, use larger changes in the tax rate, and control for price trends within states. I find that the pass-through on pre-tax apparel prices is small and negative, implying that consumers pay the sales tax, with some exceptions. The almost full shifting onto consumers is surprising given the well-documented fact that the demand for apparel at local stores is quite elastic. This lack of response suggests even more elastic supply, and also that equilibrium output and, hence, inputs should decrease in response to a tax. I use data from the Quarterly Census of Employment and Wages and estimate that county employment in the apparel retail sector increases by 0.33% following a one percentage point drop in sales tax. Finally, using the Consumer Expenditure Survey data, I find that a 5% sales tax rate generates on average a 13 cent deadweight loss for every tax dollar collected.

In Chapter 2, I estimate how short-term apparel tax exemptions, tax holidays, affect retail prices on apparel. Theory shows that a large spike in demand, which tax holidays induce, may result in either a price increase or decrease, depending on the competitive structure of the market. Because the Consumer Price Index micro data reports the exact date of price observation, I obtain precise estimates and find, again, that retailers do not alter prices. Thus, retail apparel sales are competitive, and consumers fully enjoy the benefits of tax holidays.

In Chapter 3, I explore whether the differential taxation of close substitutes influences the equilibrium set of products in supermarkets, i.e. product bunching, by studying sales taxes on food. In a number of US cities, the sales tax on restaurants meals, the service tax, exceeds that on groceries by 5-10 percentage points. This creates a clear incentive for supermarkets to invest in supplying pre-prepared meals, if households prefer these lower-taxed alternatives to highly taxed restaurant meals. Theory suggests that such an investment should lead to substantial deadweight losses. My results show that households do respond to the tax differential. Low-income households increase expenditures on dry groceries by 1.6% in response to a one percentage point increase in the differential between service tax and groceries tax. High-income households increase the expenditures on meat products by 1.9%.

JEL Classifications: H22, H25, H26, H71, J30

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Chapter 1

The Effect of the Sales Tax on Retail Prices and Employment: Evidence from Exemptions on Clothing

1.1 Introduction

Good-specific exemptions are a common part of sales tax policy. Optimal tax theory suggests that the higher the potential distortions from taxing a good, the lower the tax rate levied on it ought to be. Two particular distortions draw the attention of policymakers: higher prices for consumers and lower number of jobs. Few economic papers precisely estimate the good-specific ad valorem tax effect on prices (Carbonnier, 2007; Doyle and Samphantharak, 2008), and none on employment. In this paper, I fill in this gap in the literature for apparel, a good on which consumers spend around 3% of their incomes, by estimating the effect of the apparel sales tax rate on incidence and employment and computing deadweight loss.

Estimating the effect of a good-specific sales tax using general rate changes is challenging because of two intrinsic features. First, the changes are small. The response of market participants to such subtle tax changes is even smaller, making it difficult to obtain precise estimates. This argument gains even more relevance given the lack of sales tax salience for consumers, who observe the tax at the register rather than in listed price (Chetty et al., 2009) and the costs of price adjustments for retailers.[1] Second, changes in the general sales tax rate typically alter the relative prices for a broad set of goods and even real disposable household income at the same time, which may bias estimates of its effects due to general equilibrium effects. Using recent revisions of the sales tax exemptions *solely* on apparel implemented by state and local governments in Connecticut, New York and Vermont, I overcome both of

[1] Lack of salience is unlikely to contaminate my estimation, which is based on large changes in permanent tax rate — tax exemptions. Recall that Chetty et al. (2009) provide three criteria for the salience of a tax: (i) high tax rates, (ii) large elasticity of demand and (iii) high prices for a given item. Indeed, relative to grocery items, apparel purchases readily meet the last two criteria. In case of the tax rate, the comparison is also in favor of apparel: though the tax rates are similar in magnitude, the changes in apparel industry are large and occur at higher frequency.

these issues. A tax exemption makes items priced below a certain threshold tax-free, the most common threshold being \$110. Thus, any exemption revision generates a sizable and, thus, salient tax change equal to a state or local sales tax rate, at least for some items. In my data, the maximum changes in the sales tax rate equal 6.5% in Connecticut, 7% in Vermont and 8.625% in New York. The alterations to exemptions affect only the apparel market, rendering general equilibrium effects negligible. In addition, the exemption changes happen quite frequently.

The fundamental identification problem with estimating the effect of sales tax on prices or employment is spurious correlation between a state's economic conditions and its tax policy changes. Negative shocks to a state economy may simultaneously lower the outcome variable and increase the budget deficit which, in turn, forces legislators to impose higher tax rates. I solve this problem differently for prices and employment.

In the first case, I take advantage of the fact that legislators exempt items priced below a certain threshold but not above. This allows me to use the items priced above it as a control group for price trends in the state.[2] My results are robust to different threshold levels that are instituted in different states, and I argue later on that the non-exempt items offer a useful control group. I also show that pricing around the threshold is not responsive to changes in the threshold value, and I use an IV strategy to deal with the spurious correlation between prices and tax rates.

When estimating the effect of the sales tax on employment, I do not have an in-state control group as I do in the case of sales tax incidence estimation. To argue that spurious correlation between the sales tax and employment does not drive my results, I perform a robustness check where I use employment in retail stores selling

[2] This is in addition to the other control group, which is prices in the other states

goods for entertainment instead of clothing and do not find a negative effect of the sales tax changes.

To this solid foundation for identification of sales tax effects, I add very detailed, item-level, confidential data from the Consumer Price Index on apparel prices. This use of price data yields the first key result of my paper — the estimate of apparel-specific sales tax effect on pre-tax prices, i.e. the *pass-through rate*.[3] Using a triple difference IV empirical strategy, I find that the pass-through rate is tightly estimated to be close to zero for most types of apparel, implying that consumers fully bear the burden of the tax. This estimate is similar to the estimates for other goods (DeCicca et al., 2013; Harding et al., 2012; Kenkel, 2005), but different from previous estimates for the apparel industry (Besley and Rosen, 1998). For some apparel items, the pass-through rate is small but not zero, suggesting more elastic demand. Specifically, retailers pay 21% of the sales tax on non-seasonal goods, whereas they pay 38% on girls apparel and 24% on footwear.

The zero pass-through rate is common for many other goods but surprising for apparel, with its well-documented high elasticity of demand (Einav et al., 2014; Hu and Tang, 2014). It suggests an even more elastic supply and, hence, a substantial decrease in quantity produced.[4] This makes it interesting to explore how the sales tax rate affects the equilibrium usage of inputs. After merging the sales tax exemptions with data from the Quarterly Census of Employment and Wages, I find that a one percentage point increase in the sales tax rate implies a 0.4% decrease in the number of employees hired by local apparel retailers and a 0.6% decrease in expenditures on

[3]Under the assumption of perfect competition, this pass-through rate coincides with tax incidence on producers

[4]Throughout my paper, I assume perfect competition because none of my results contradicts it. (Şen, 2008) shows evidence that apparel industry indeed is pretty competitive

employees, suggesting that wages do not decline much. I do not find any effect on the number of stores, which represents a type of capital. In contrast, the previous literature explores the effects of a general sales tax rate on total locality employment (Thompson and Rohlin, 2013) and does not find a robust link between the two.

This result helps explain the usual practice of state enterprise zones that include lower sales tax rates as a business incentive. It also suggests that the transition between different tax rate regimes may be costly. Indeed, a New York state 4% sales tax exemption on clothing results in more than 2,150 new jobs, which is close to the average county employment in the apparel retail sector. Given that the average unemployment duration is 27 months in the sales industry according to the Census, the constant changes of exemptions in this state may lead to substantial costs.

The large effect of sales tax on the apparel retail employment implies that a sales tax substantially reduces the equilibrium quantity of sales in the market and, hence, social welfare. I use the Consumer Expenditure Survey to arrive at my third key result, about deadweight loss. Data on apparel purchases has two limitations: it does not provide detailed geographical information about the consumer or information on whether a certain transaction occurs online or offline. I employ the estimates from Einav et al. (2014) as a proxy for changes in the Internet purchases. Using Goulder and Williams (2003), I find that a 5% sales tax rate generates a 17¢ loss for every tax dollar collected.

The rest of the paper proceeds as follows. In Section 2.2, I provide a detailed literature review on the empirical research relevant to my question. Section 1.3 gives an overview of tax incidence theory. I also use this section to formulate empirical hypotheses I will later test. In Section 2.4, I thoroughly describe the data. Section 2.5 contains the explanation of my empirical strategy. In Section 2.6, I present my main results, followed by robustness checks of my pass-through estimation in Section 2.7. In Section 1.8, I calculate deadweight loss.

1.2 Evidence on the Effects of Good-Specific Taxation

This section explains how my paper adds to the empirical literature on the effects of good-specific taxation on prices and employment. I start by describing the literature on the effect of taxes on consumer prices, i.e. tax incidence, which has a one-to-one correspondence with the effect of taxes on retail prices, i.e. the pass-through rate. Throughout my discussion, I stick to the terms pass-through rate on retailers vs. tax incidence on consumers because it (i) clearly denotes whether I refer to consumer or producer prices and (ii) is correct regardless of the assumption about competition. Then, I provide an overview of the few research papers that explore the effect of the sales tax on employment, which I augment with a discussion on the apparel expenditure literature.[5]

A few empirical papers study tax incidence on prices. I classify them into three categories. The papers from the first category specialize in precisely estimating the tax incidence parameter for a given market. They usually find that consumers fully pay the tax, which is the case in the gasoline market with a sales tax (Doyle and Samphantharak, 2008), in the clothing market with a sales tax (Poterba, 1996), and the alcohol market with an excise tax (Kenkel, 2005). There are two noticeable ex-

[5] I do not review the papers that study the effect of taxes on sales and purchased quantity of non-apparel markets because this literature is not directly related to my paper. Its recent focus is on tax avoidance issues (Beatty et al., 2009; Merriman, 2010)

ceptions to this rule. First, retailers pay half of the VAT on haircuts in Finland (Kosonen, 2015). Second, Besley and Rosen (1998) shows that the sales tax overshifts on consumers for some grocery items and the type of apparel they consider (underwear). I extend the analysis of Poterba (1996) and Besley and Rosen (1998) by using more granular data and a cleaner identification strategy. Particularly, I am the first in public economics field to use the CPI confidential micro data.[6]

My estimates also add to the recently emerging literature which studies the effect of the sales tax on total expenditures (Agarwal et al., 2013), online expenditures (Einav et al., 2014) and catalog expenditures (Hu and Tang, 2014) in the apparel market. I consider prices at traditional retailers, so the first paper is the most relevant to my research. The authors find that consumers are very responsive to taxes: a 1% change in the sales tax rate leads to a 2-6% drop in total expenditures.[7] My finding of zero pass-through on pre-tax prices suggests that one can easily convert their estimates into the demand elasticity.

Moreover, their result adds to my argument that a lack of salience is not a concern for my estimation. In their seminal paper, Chetty et al. (2009) provide three criteria for the salience of a tax: (i) high tax rates, (ii) large elasticity of demand and (iii) high prices for a given item. Indeed, relative to grocery items, apparel purchases readily meet the last two criteria. For the first criteria, the comparison is also in favor of apparel: though the tax rates are similar in magnitude, the tax rate changes in the apparel industry are large and occur at higher frequency.

The papers on tax incidence in the two other categories, which I define respectively

[6]The most prominent examples of using this data in other fields are Cortes (2008) and Matsa (2011)

[7]The authors use sales tax holidays on apparel as a source of variation in the tax rate. When estimating deadweight loss below, I find my estimate to be similar to the lower bound of their result, which is not surprising given that I employ permanent tax rate changes in my analysis

as *demand* and *supply* papers, go one step further and explore which characteristics of the two sides of the market affect the tax incidence parameter. The demand papers explore the differences between consumers. Harding et al. (2012) presents evidence that consumer geographical location matters: smokers who live closer to the state border face substantially lower pass-through of an excise tax on cigarettes compared to those in the state interior. In addition, DeCicca et al. (2013) argues that search costs explain the varying tax incidence across consumers; the authors find that heavier smokers, who have higher benefits from investing time in looking for lower prices, experience lower tax incidence. I contribute to the demand papers literature by estimating sales tax incidence for various apparel groups. First, I find that the sales tax only partially shifts onto consumers who purchase girls clothing and footwear, the demand for which is arguably more elastic. Second, I am the first to consider whether the sales tax pass-through differs across seasonal and non-seasonal goods. Traditional tax incidence literature generally ignores seasonality of goods despite their surprising price behavior during peak demand (Chevalier et al., 2003).

The supply papers consider how industry characteristics affect tax pass-through rate through the elasticity of supply. Marion and Muehlegger (2011) considers the diesel fuel industry, where the elasticity of supply decreases as the refineries and inventories approach full capacity. They use variation across states in this measure for their research. Carbonnier (2007) compares the elasticity of supply between two industries: cars vs. service. The latter industry is substantially more concentrated and, thus, its supply should be less elastic. Consistent with theory, both papers find that the more inelastic is supply, the higher is the pass-through rate on producers. My paper is closer to these two supply papers. Theoretically, my pass-through result is only consistent with very elastic supply, which I show indirectly. Both academic papers (Şen, 2008) and case studies provide evidence that it is indeed the case.[8] Interestingly, despite the inelastic supply of hybrid vehicles in the US, Sallee (2011) finds full shifting of the tax on consumers.[9] The other empirical finding in the supply literature is that the pass-through rate depends on the stage where the state collects tax (Kopczuk et al., 2013b).

Elastic supply implies that firms easily adjust their output and, hence, inputs. Confirming this mechanism, I find that apparel sales taxes decrease employment in the retail industry. To the best of my knowledge, this is the first paper that establishes a link between labor employed to produce a good and a tax rate on the good. It adds to a thin literature that explores the effect of the general sales tax rate on employment. While Fox (1986) and Harden and Hoyt (2003) do not find evidence for it, Thompson and Rohlin (2013) and Rohlin et al. (2014) show that the effect is present for border counties under certain conditions.

1.3 Theoretical Background

In this section, I briefly describe the theory behind my two main empirical questions: the effect of the sales tax on prices and employment. For the price effect, I show how demand and supply elasticities affect a pass-through rate of the sales tax on producer prices under the assumption of perfect competition. In the section, I also discuss what

[8] "The Gap between Company's Domestic and Global Market Positioning", by Talia Lambar, MBA (April 3,2012)

[9] In this case, a tax credit is a subsidy. The discrepancy of Marion and Muehlegger (2011) and Sallee (2011) together with recent evidence of consumer's differential response to small tax and subsidy (Homonoff, 2013) suggests that one should be cautious of using them interchangeably. This is the main reason I do not describe economic literature on subsidy incidence in this review

changes if I relax this assumption.[10] For the employment effect, I present graphical evidence that firm expenditure across all inputs should increase in the event of a sales tax rate drop.

Under the assumption of perfect competition, the pass-through rate on producers is:

$$\rho = -\frac{\epsilon_D}{\epsilon_S + \epsilon_D}, \tag{1.1}$$

where $\epsilon_D \geq 0$ is the absolute value of demand elasticity and $\epsilon_S \geq 0$ is supply elasticity. The constraints on the elasticities transmit into the constraints on the pass-through rate itself: $\rho \in [-1, 0]$. As I show in Appendix 1.10, under the assumption of imperfect competition, the pass-through rate can take any value. At this moment, I am ready to state that:

Proposition 1. Producer prices can either increase or decrease in response to a rise in the sales tax. Under the assumption of perfect competition, the pass-through equals the tax incidence and belongs to the interval [-1,0].

In my empirical analysis, the pass-through of a sales tax on producer prices always belongs to the interval [-1,0]. Thus, I stick to the assumption of perfect competition. Under this assumption, the concept of a pass-through rate of sales tax on either consumers or producers is synonymous with the tax incidence. Weyl and Fabinger (2013) shows that under imperfect competition, they are synonyms only for consumers. For producers, the magnitude of the pass-through rate is a lower bound on the tax incidence magnitude. This is true for dead-weight loss as well. Thus, I consider my estimates of the pass-through rate and deadweight loss as conservative.

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[10] See Appendix 1.10 for the derivation of the pass-through rate on consumers and producers under the assumption of imperfect competition.

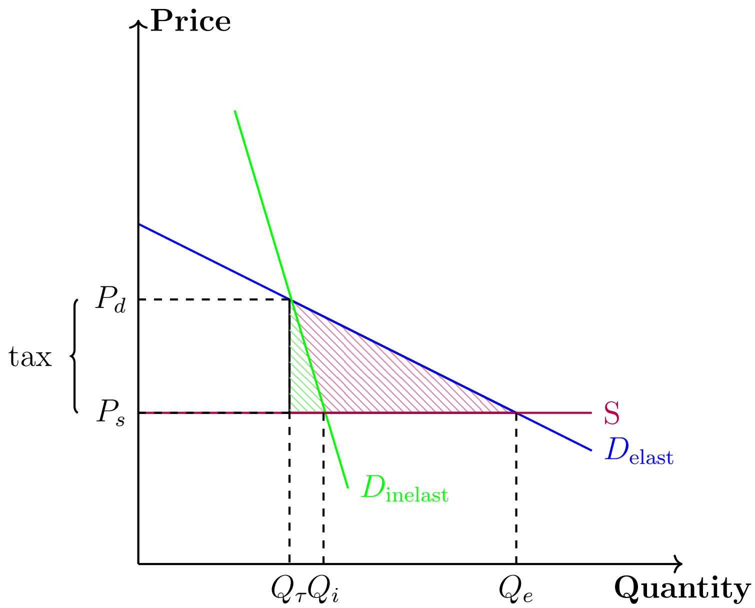

Figure 1.1: Comparison of deadweight losses when pass-through rate is zero
Notes: Deadweight loss generated by a sales tax is substantially larger when demand is elastic (blue line) rather than inelastic (green line). In the former case, the deadweight loss is all the shaded area, whereas in the latter — the green shaded area

In my empirical analysis, I estimate tax incidence for different groups of apparel separately because the elasticities of demand and supply are likely to vary across these groups. So, I establish the comparative statics of the pass-through rate on producer prices with respect to the demand and supply elasticities. Formula (1.1) suggests that:

Proposition 2. The pass-through of sales tax on producer prices increases in the elasticity of demand and decreases in the elasticity of supply.

In addition to changing the prices, the sales tax may also decrease the equilibrium quantity. This forces firms to employ fewer inputs. The magnitude of this decrease depends on the demand and supply elasticities. In Figure 1.1 I show that the output responses increase in the elasticity of demand: $Q_i - Q_\tau < Q_e - Q_\tau$. In Figure 1.2, I

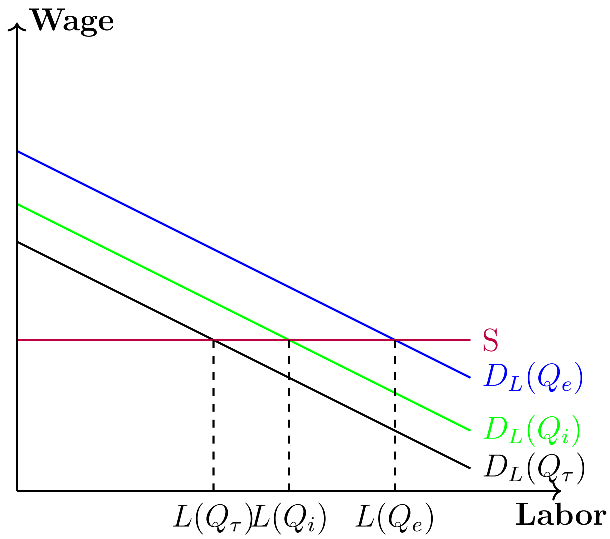

Figure 1.2: Comparison of changes in employment when pass-through rate is zero

Notes: Decrease in derived demand for employment generated by a sales tax is substantially larger when demand is elastic (blue line) rather than inelastic (green line). In the former case, the change equals $L(Q_e) - L(Q_\tau)$, whereas in the latter — $L(Q_i) - L(Q_\tau)$

depict the corresponding input responses in the elasticity of demand: $L(Q_i) - L(Q_\tau) < L(Q_e) - L(Q_\tau)$. The same result is true for the supply elasticity, which allows me to state:

Proposition 3. The imposition of a sales tax may lead to a decrease in the equilibrium output, and, hence, inputs employed by suppliers. The higher the elasticities of demand and supply, the larger the decrease.

Note that the deadweight loss of a sales tax also increases with respect to both elasticities. Thus, a large effect on the inputs used by firms signals substantial losses due to the tax.

1.4 Data

The tax data effectively dictates the geographical and time coverage of my sample. All substantial changes in the legislation start in 2000 and occur in Connecticut, New York and Vermont, my treatment states; the other states in the Northeast Census Region and, in case of pass-through rate estimation, states in the Midwest Census region serve as a broad control group.[11] So, my sample comprises these states from the years 1997 (three years before any tax exemption policy changes) through 2012 (the last available year in the rest of the data sets). For all Northeast states, I construct data on local and state tax exemptions and tax rates.

For my analysis, I merge the tax data set with three external data sets:

1. confidential price information from the rich Consumer Price Index (CPI) micro data set, made available by the Bureau of Labor Statistics (BLS), to estimate the pass-through rate

2. the Quarterly Census of Employment and Wages, to estimate the effect of the sales tax rate on local employment

3. the Consumer Expenditure Survey, to estimate the demand elasticity in order to calculate deadweight loss

Below, I detail the construction and/or collection of these data for the sales tax and then for the first two data sets. The description of the third data set is in Appendix 1.11.

[11]The changes in the general tax rate in Midwest States are small to affect my estimates. I do not refer to them as to treatment states.

1.4.1 Sales Tax Rate

In this subsection, I describe in detail how the sales tax rate varies for different price categories in the treatment states and compare it with the variation in the control states. For the Northeast, I collect the data on state and local sales tax exemptions and the general sales tax rate from previous and current versions of state government websites. I find that the only discrepancy between my data and the CPI data occurs due to alternations in the exemptions. Given that there are no apparel-specific exemptions in the Midwest, I use the tax rates from Consumer Price Index micro data for this region.

A sales tax exemption makes items for a certain price-category tax-free. In the past two decades, four states have changed their sales tax exemption rules on apparel, defined as clothing plus footwear: Connecticut (twice), New York (six times), Rhode Island (once) and Vermont (twice). I, however, ignore the exemption in Rhode Island because it affects only a small share of apparel items that cost more than $250.[12] The other exemptions generate tax rate decreases or increases ranging from 5 to 9 percentage points, and, thus, can substantially influence the behavior of market participants. Note that a given tax exemption affects only a certain, *treated*, group of items in a treatment state, allowing all the other items in the state to serve as a control. In this subsection, I describe in detail how the sales tax rate vary for different price categories in the treatment states and compare it with the variation in the control states.

In Columns (1-2) of Table 1.1, I provide New York cumulative tax rates (that is, in both the state and in particular counties and municipalities) for items priced below

[12] Also, the revision in the Rhode Island exemption happens at the very end of my sample: in October of 2012. Such items generally include night dresses, fancy suits, etc.

and above $110, the most common threshold value in this state, and in Figure 3 I demonstrate the path of New York state and New York City sales taxes and exemptions. The three drops of the rate for cheaper items are due to three introductions (2000, 2006, 2012) and two repeals (2003, 2010) of state tax exemptions. The phase-in of the last introduction occurred in two steps: in April 2011, the threshold rose from $0 to $55 and then doubled in April 2012. When the exemption is in place, the tax rate is not necessarily zero because New York state legislation allows its cities (but not towns or villages) and counties to establish as well as exempt their own sales tax rates. The magnitude of the local and state taxes are around 4% each. In addition, there is a small Metropolitan Commuter Transportation Mobility tax (0.375%) levied by eight counties.[13] 22 of the 62 counties and 6 of the 61 cities in the state of New York have tried exemptions at least once. New York City is a leader in this policy: its exemptions are more generous even than the state's (see Figure 1.3). The items priced above the exemption threshold face a general sales tax rate, which slightly rises throughout my sample. Its behavior and magnitude is very similar to that in Midwest states, which also generally allow their localities to administer sales tax rates.

In Connecticut (Columns 3-5 of Table 1.1) and Vermont (Columns 6-7), legislators have changed the exemption policy twice. Starting from the $75 threshold in 1997, the Connecticut legislators first decreased the threshold to $50 in 2003 and then completely repealed the exemption in 2011. Alternatively, Vermont started exempting apparel items priced above $110 in 2001, followed by an extension for all items in 2007. Connecticut does not allow localities to administer a sales tax — and in Vermont only four municipalities charge a 1% additional to state tax before 2007, when the new exemption rules eliminates any tax on clothing.

[13] I use all state and local changes in the tax rate for my estimation

Table 1.1: Population Weighted Average Cumulative Sales Tax Rates in Northeast and Midwest States for Apparel by Price Categories

	Average Sales Tax Rates in Treatment States							Control States	
	NY		CT			VT		IL	Other MW
	for Different Price Categories							for All Prices	
Year	$\leq$ \$110	$>$ \$110	$\leq$ \$50	(\$50; \$75]	$>$ \$75	$\leq$ \$110	$>$ \$110		
1998	7.91	7.91	0.00	0.00	6.00	5.00	5.00	7.48	5.32
2001	1.74	7.93	0.00	0.00	6.00	0.00	5.00	7.55	5.36
2004	8.40	8.40	0.00	6.35	6.35	0.00	6.06	7.63	5.63
2007	2.34	8.27	0.00	6.35	6.35	0.00	0.00	7.68	5.67
2010	6.60	8.47	0.00	6.35	6.35	0.00	0.00	8.02	5.72
2012	2.44	8.47	6.50	6.50	6.50	0.00	0.00	7.60	5.77
The Northeast control states (MN,MA,NH,NJ,PA,RI) have constant rates									

Notes: The data on the state and local tax rates is from state government websites, whereas the population numbers for states and municipalities are from 2010 Census. I use December tax rates for each year in the table. For treatment states and Illinois, I compute the average of cumulative tax rates weighted by population of municipalities. For the "Other Midwest" states, I use state and average municipal tax rate weighted by state population. "Other Midwest" states are Kansas, Kentucky, Michigan, Minnesota, Missouri, Nebraska, North Dakota, Ohio, West Virginia, Wisconsin

The tax rates in the control states stay almost constant throughout my sample. New Hampshire does not impose any sales tax, whereas Pennsylvania and New Jersey fully exempt clothing from taxation. Maine' cumulative/state tax rate is equal to 5%. In Massachusetts, a 2008 state sales tax increase from 5% to 6.25% affects only a small share of items priced above \$175. The same argument applies to Rhode Island's exemption repealed in October 2012 that makes items priced above \$250 taxable. In Illinois (Column 7), there is a tiny increase in the sales tax rate from 7.5% to 7.6% in the last two decades, whereas the rest of the Midwest states experience an almost a 9% increase from 5.32% to 5.77%.

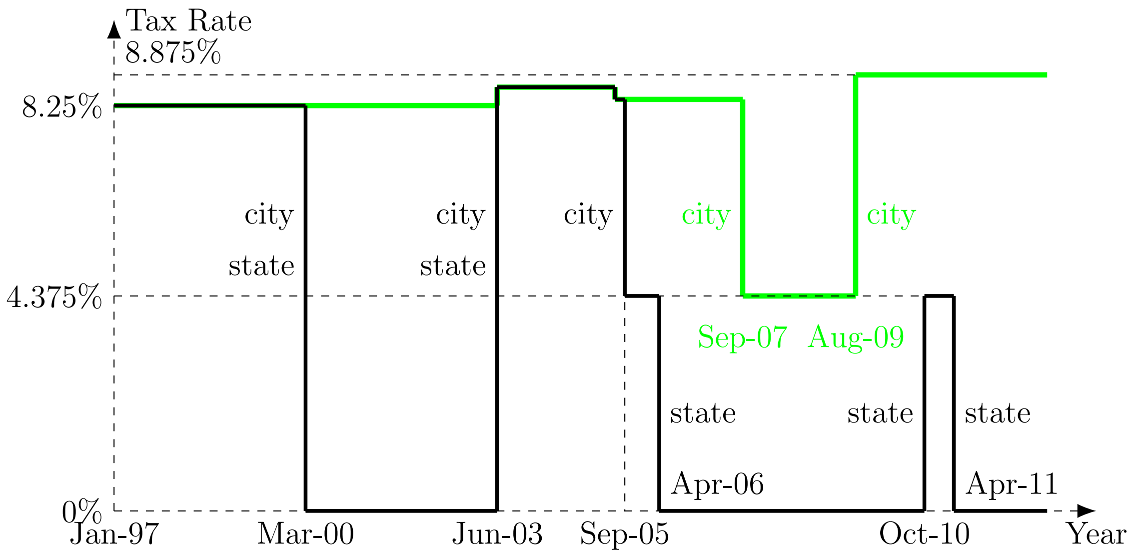

Figure 1.3: Cumulative Sales Tax Rate in New York City

Notes: Green line represents the tax rate for items priced above $110, whereas black — below $100. "city" or "state" denotes city or state tax exemption change.

1.4.2 CPI Micro Data

For estimating the pass-through rate on pre-tax prices, I use the confidential Consumer Price Index (CPI) micro data, to which the Bureau of Labor and Statistics has graciously granted me access. This nationally and regionally representative data is a panel with around 36,000 apparel price observations every year. After randomly selecting a specific item at a specific store for the sample, the surveyors follow it until the item becomes permanently unavailable at the store. They also collect detailed information on item characteristics, such as seasonality, apparel category and local tax rate. Previous research in public economics has not used these data. The most prominent examples of its usage in other fields of economics are papers by Cortes (2008) and Matsa (2011)

Below, I explain how I use the CPI data for my analysis. First, I flesh out the specifics of the CPI data collection process that affects my choice of variables and empirical strategy.[14] Second, I provide general summary statistics for my price regressions. I describe how the BLS categorizes missing observations and provide summary statistics for each category in Appendix 1.12.

BLS Data Collection Process

The CPI microdata is designed to compute price indexes and inflation rates. Ideally, it is a balanced panel, which consists of n quotes, or units of observations. Any quote $\mathbf{q}^i$ is a vector of prices on a good $i \in 1, ..., n$ from month $t = 1$ to month $t = T$: $\mathbf{q}^i = (p_1^i, p_2^i, ..., p_T^i)$. For instance, a good could be a red, large-size, 100% cotton, men's t-shirt displayed at the Gap store with the following address: 543 Madison Avenue, Poughkeepsie, NY.[15] Then, the price level PL_t at any time, t, is a weighted average of all the quote prices collected at t: $\frac{1}{n} \sum w_i * p_t^i$.[16]

In practice, most apparel goods are not on the market for a long time period. At some point, the Gap may close this particular store or decide to permanently cancel this good from the store shelves for a number of reasons: red becoming unfashionable, cotton turning into an expensive commodity, and so forth. Once, the BLS surveyor learns about the cancellation, she substitutes another good for the t-shirt . The new good may be very similar to the old t-shirt, or maybe a different shirt. However, the prices for the new good still enter in the old quote. Such goods, which provide the prices for the same quote, the CPI methodology calls quote versions. For brevity, I refer to a quote version as an item. More formally, an item is a good m_i, a vector

[14] Bureau of Labor and Statistics Chapter 17 provides a detailed description of the whole CPI collection process

[15] This is a hypothetical example, which is not taken from the actual data

[16] The weights depend on the relative share of the good in consumer expenditures. They do not matter for my analysis.

of prices which enters quote i from $t = h \geq 1$ to $t = k \leq T$: $\mathbf{q}_{m_i} = (p^i_h, p^i_{h+1}, ..., p^i_k)$. Figure 1.4 provides an example of a quote evolution over time, with one substitution in May and another in December.

In my data, a unit of observation is an item with one exception. When replacing one quote version for another in a sample, the surveyor first looks for a close substitute for the missing item; otherwise, she randomly selects another item from the same apparel category. In the t-shirt example, a close substitute may be a t-shirt containing 97% cotton. If the data shows that the surveyor manages to replace a canceled item with a similar one, I consider these two items as a single unit of observation or as one item in my data.[17] In Figure 1.4, Version 1 and Version 2 are close substitutes which explains why their prices are equal in March and April, but Version 3 is not a close substitute and has a lower price. My choice of the unit of observation results in an unbalanced panel.[18]

To make good substitutions of versions, the BLS surveyors collect detailed descriptions for every item, including its color, size, material and country of origin. In my analysis, I do not use the majority of these characteristics because it is not possible to construct a consistent data set for them over the years. However, I employ the information on item categories and groups. In the CPI micro data, there are 6 apparel groups (Men, Boys, Women, Girls, Footwear and Babies) and 29 categories or Entry Item Levels (ELI). A category usually consists of three or four types of apparel. For

[17] In most cases, these two goods are essentially the same good which the collector may re-describe due to unrelated reasons. The CPI data has a special variable determining the similarity of two items

[18] Alternatively, I could choose a quote as the unit of observation and employ price-quality adjustments that the BLS uses to compare prices across two consecutive quotes which are not close substitutes. I believe that this may result in biased estimates. Based on hedonic regressions, the quality adjustment methodology varies over time and maybe analyst-dependent. If analysts are inclined to make adjustments in a certain way (for instance, to keep inflation constant), this may affect my estimates in an unpredictable manner

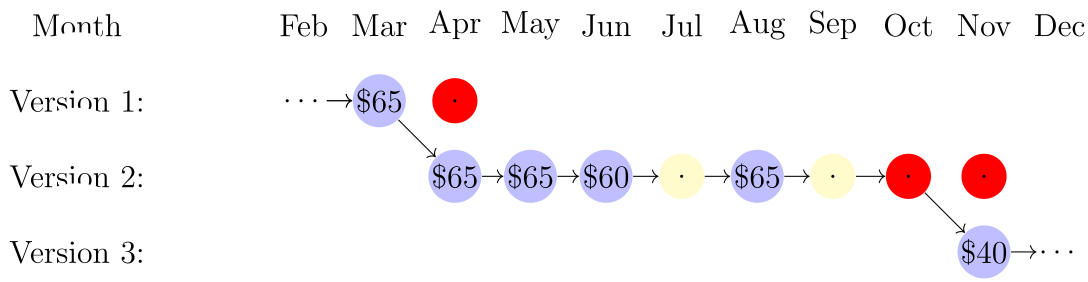

Figure 1.4: CPI Quote Evolution Over Time

Notes: The black arrows show the observations used for computing CPI. A yellow circle represents temporary missing observation (stockout), whereas red - permanent missing observation (cancellation). The number in each circle shows the price of the item. Versions represent different items entering one quote

example, ELI "AA011" includes men sweaters, men t-shirts and men vests.

In addition to item categories and groups, I use several other variables from the CPI data. First, I observe the exact date when a surveyor collects a price observation, which allows me to control for sales tax holidays, a popular cross-state policy that exempts the sales tax on apparel for a small period of time (1-10 days). Second, the data provide information on the months when an item is on season.The procedure for defining a season range for a given item has two steps. First, BLS employees located at headquarters decide whether an item's category, or ELI, is seasonal. If so, they assign a six-month season to all items in that category. When the surveyor initiates an item in the sample, she asks a store representative when the season for the item starts and ends. Finally, the "Sale" determines the cases when a given item is on sale.

Summary Statistics

My sample consists of item-month observations. It spans from 1997 to 2012 with the exception of March 1997 and December 2012, when the data is unavailable for analysis at the BLS cluster due to technical issues. For each unit of observation, time series are either monthly or bimonthly, which depends on the location of price collection. Only in three metropolitan areas (New York City, Chicago and Los Angeles) do the surveyors report prices on the same units every month. This fact suggests that prices in Chicago could be a good control in New York. Keeping this in mind, I add to my sample Illinois and all other states from Midwest States. The inclusion has two other benefits. First, it increases the number of control observations and states in my sample which is useful given that the CPI data does not have prices from several Northeast states. Second, it smooths the average prices across odd and even months in control states, which is important for my instrument construction procedure.

There are several sample adjustments that I make. I consider only observations on prices from traditional retailers because quotes from catalog and online stores have a small representation (2-3%) in the BLS data.[19] I drop from my sample all the items that ever cost more than $1000 because their behavior may differ from the rest of the items.

Table 2.2 shows summary statistics for all the variables involved in my estimation of tax incidence. Columns (1-2) show means and standard deviations for the two treatment states, NY and CT, whereas Columns (3-5) provide the same information for the control geographical areas: other Northeast states, Illinois and other Midwest states.[20] Average prices are more than a quarter higher in NY and CT relative to the

[19]Data on the Internet stores enters the CPI micro data in 2003

[20]The CPI data does not cover price quotes in Vermont, but I use Vermont as a treatment state

Table 1.2: Summary Statistics for Apparel Items, Price Regressions

	NY	CT	Other NE	IL	Other MW	All
Price, $	86.4	88.8	58.7	67.2	56.0	65.1
	(156)	(174)	(94)	(112)	(89)	(113)
Tax Rate	0.048	0.021	0.008	0.075	0.049	0.039
	(0.035)	(0.029)	(0.020)	(0.022)	(0.028)	(0.035)
Instrument for Tax Rate	0.044	0.020	0.008	0.075	0.049	0.039
	(0.035)	(0.029)	(0.020)	(0.022)	(0.028)	(0.035)
Item is on Sale, %	35.2	44.1	41.6	38.1	40.6	39.6
	(47.7)	(49.7)	(49.3)	(48.6)	(49.1)	(48.9)
Sales Tax Holiday, %	1.16	1.96	0.005	0.150	0.030	0.309
	(10.72)	(13.87)	(0.712)	(3.86)	(1.72)	(5.55)
Monthly Quotes	0.828	0.556	0.289	0.756	0.050	0.387
	(0.377)	(0.497)	(0.453)	(0.429)	(0.219)	(0.487)
Nonseasonal Goods	0.307	0.267	0.296	0.309	0.310	0.304
	(0.461)	(0.442)	(0.456)	(0.462)	(0.462)	(0.460)
Fall Seasonal Goods	0.132	0.095	0.118	0.111	0.111	0.117
	(0.339)	(0.294)	(0.323)	(0.314)	(0.315)	(0.321)
Spring Seasonal Goods	0.131	0.099	0.113	0.099	0.109	0.113
	(0.337)	(0.299)	(0.317)	(0.299)	(0.312)	(0.316)
Men's Clothing	0.313	0.320	0.278	0.283	0.290	0.291
	(0.464)	(0.466)	(0.448)	(0.450)	(0.454)	(0.454)
Women's Clothing	0.315	0.321	0.360	0.329	0.325	0.334
	(0.465)	(0.467)	(0.480)	(0.470)	(0.468)	(0.472)
Footwear	0.185	0.149	0.181	0.196	0.200	0.189
	(0.389)	(0.356)	(0.385)	(0.397)	(0.400)	(0.392)
No. of Obs.	94,793	16,411	157,742	76,866	171,242	517,054

Notes: The data comes from Consumer Price Index micro data with one exception. I fix the inaccurate reporting of the sales tax rate after exemption alterations in the CPI data by self-collecting data on exemptions. It covers a time period from January 1997 to December 2012. There are five geographic areas that I compare: New York, Connecticut, Other NorthEast Census Region states, Illinois and Other Midwest Census Region states. Price is exclusive of a sales tax. Sales tax holiday equals to one if on the day of price collection a state holds sales tax holidays on apparel. I explain in "Empirical Strategy" section how I construct an instrument for the sales tax. All the variables, except for price, tax rate and instrument for tax rates, are dummies.

other states. I attribute this to the fact that more stores in these areas sell luxury brands.[21] About 15% of prices are above \$110, the most common threshold value in NY, implying that the in-state control group is large.

I control for other variables that reflect price or tax variation or item characteristics. Sales tax holidays are popular in both treatment states. The distribution of seasonal items and groups is almost the same across the states. 30.4% of all quotes are non-seasonal goods. About 11% each are Fall and Spring Seasonal Goods, where I define fall season to last exactly from August to January and spring season exactly from February to July. The most represented groups of apparel in my sample are adult clothing (women - 33%, men - 29%) and, footwear (19%).

1.4.3 Employment Data

To estimate the effect of the sales tax on employment, I use data from the Quarterly Census of Employment and Wages (QCEW). The data provide total number of employees on monthly basis as well as total payroll, average wages, and total number of establishments on quarterly basis for different industries at the county level. I merge it with county-level self-constructed tax rate data and the U.S. Census population data. In this subsection, I present summary statistics for the variables obtained from QCEW and the Census.

Bureau of Labor and Statistics (BLS) collects the data on wages and employment for all the industries in most of the counties. For a subset of small counties, BLS does not publish the data due to non-disclosure restrictions. Generally, the set of

when working with the other data sets

[21] Indeed, there are three times more observations of items priced from \$500 to \$1,000 in NY and CT relative to IL, which has the highest average price among the control areas.

these counties is fixed for long time periods (5-10 years) and is unlikely to bias my estimates.[22] For my analysis, I obtain the data for two industries: "448-Clothing and clothing accessories stores" and "451-Sporting goods, hobby, book and music stores", thus excluding department and big box stores that sell apparel and entertainment goods. The first industry describes the employment of the apparel retailers and is of my primary interest. The second industry is for robustness checks of my results.

[22] I do not find any relationship between the tax rate and counties entering/exiting the sample

Table 1.3: Summary Statistics for Retail Establishments, Employment Regressions

	CT	ME	MA	NH	NJ	NY	PA	VT	RI
Apparel and Entertainment Retailers: Data at County-Monthly Level									
No. of Employees	2,272	381	2,387	712	2,648	2,187	1,031	1,037	264
(Apparel Retailers)	(2,260)	(596)	(2,140)	(794)	(2,076)	(6,047)	(1,893)	(1,082)	(398)
Missing, %	1.7	18.9	5.6	0.5	3.0	9.4	14.8	10.1	25.4
No. of Employees	1,116	267	1,227	492	1,087	793	463	393	179
(Entertainment Retailers)	(1,076)	(478)	(1,235)	(516)	(929)	(1,436)	(720)	(287)	(221)
Missing, %	15.5	23.8	19.9	14.5	17.7	27.0	26.7	15.1	28.8
Population	436,038	81,542	459,349	127,895	409,429	313,722	188,805	210,829	44,077
	(340,558)	(68,798)	(389,168)	(112,977)	(242,632)	(524,308)	(265,011)	(211,649)	(34,145)
No. of Obs.	1,544	3,088	2,702	1,930	4,053	11,773	12,738	965	2,702
Apparel Retailers: Data at County-Quarterly Level									
Establishments	214	44	229	72	243	209	102	103	32
	(197)	(52)	(168)	(70)	(175)	(486)	(169)	(84)	(32)
Payroll, \$1,000 \$	10,353	1,532	11,447	2,801	13,299	13,785	4,196	4,210	1,045
	(11,576)	(2,537)	(12,114)	(3,164)	(11,984)	(59,705)	(8,570)	(4,593)	(1,489)
Taxed Payroll, \$1,000 \$	4,266	641	4,374	974	6,563	3,249	1,291	1,978	399
	(6,491)	(1,364)	(6,553)	(1,648)	(8,349)	(15,529)	(3,457)	(3,164)	(829)
Weekly Wages, \$	334	297	355	324	358	284	288	332	323
	(76)	(65)	(94)	(98)	(112)	(98)	(67)	(82)	(84)
No. of Obs.	615	998	1,042	777	1,592	4,263	4,348	352	801

Notes: The data comes from Quarterly Census of Employment and Wages except for population which is from the U.S. Census. It covers the period from January 1997 to December 2012. “Payroll” represents total expenditures on labor by retailers in a county, whereas “Taxed Payroll” - total expenditures on labor subject to Unemployment Insurance tax. Missing observations are due to non-disclosure requirements. There is a bigger proportion for them for “Entertainment Retailers” (“451-Sporting goods, hobby, book and music stores”) because it is a smaller industry. The data on the number of employees and population is at monthly level, the letter variable obtained by extrapolation. The rest of the variables are at quarterly level.

Table 1.3 presents summary statistics for the variables involved in my empirical analysis. The data on the number of employees is available at a monthly level. Average employment by county varies substantially across the Northeast states. It is proportional to county population, which determines the percent of missing observations. For instance, two states with the lowest average county population (Maine and Rhode Island) have the highest proportion of missing observations for the employment in apparel stores. Average employment in entertainment stores is about half as large but varies similarly across states. This, perhaps, explains a greater number of missing observations, ranging from 19% to 35%.

The other variables for the apparel industry are at quarterly level. They include the number of stores, overall expenditures on employees (payroll), overall expenditures on employees subject to Unemployment Insurance tax and average weekly wages. The average number of stores in a county varies from 32 to 243. The average payroll ranges from $1 million to $14 million. The taxable amount is generally 2.5 lower. Finally, average weekly wages do not vary as much as the other variables, the smallest being in New York ($284) and the largest in New Jersey ($358). Given that Census does not adjust for the hours spent at work, one should not conclude that the discrepancy in weekly wages signals any discrepancy in per-hour rates across states. Nevertheless, the average for weekly wages in my sample is very close to what a full-time employee on the minimum wage would earn.

1.5 Empirical Strategy

In this section, I explain my empirical strategy for estimating the effects of the sales tax on retail prices and employment. I begin by illustrating the difference-in-difference

(DD) methodology for estimating the effect on employment using temporal and spatial variation; in this case, the changes in the sales tax occur at the county level, so I can include state-time trends. When estimating the pass-through rate of the sales tax on pre-tax prices, I extend my methodology to triple difference using item-level variation within localities because the exemptions change the tax rates only for the items priced below certain thresholds. The latter feature of the exemptions implies that the sales tax rate is dependent on the outcome variable and, hence, endogenous. To deal with this issue, I construct an instrumental variable, making my empirical strategy — IV triple difference. To the best of my knowledge, this is the first paper in the tax incidence literature that uses such strategy to control for the main concern of estimating tax effects: the endogeneity of tax policy with respect to business cycles.[23] Another feature that distinguishes my empirical analysis is the usage of local tax rate changes, which helps me explicitly account for any issues related to localities responding in a certain way to state tax rate changes (Agrawal, 2014).

1.5.1 The effect on Employment and Expenditures

To estimate the effect of sales taxes on employment, I use a standard difference-in-difference methodology. My dependent variable is the logarithm of the number of employees $Log(1 + Emp_{cm})$ in apparel retailers, where c and m denote county and month.[24] I regress it on the sales tax rate τ_{cm}:

$$Log(1 + Emp_{cm}) = \alpha + \beta_1 \times \tau_{cm} + Controls_{cm} + \nu_c + \mu_m + \epsilon_{cm}. \tag{1.2}$$

[23]When estimating the effect on employment, I address this issue by performing a robustness check.

[24]I use log-tranformation because the sales tax rate affects the relative rather than absolute employment in a same way across counties

County fixed effects control for constant characteristics affecting retail employment in the county, such as the number of interstate roads passing through. The month fixed effects control for any common shocks that simultaneously affect the states in the Northeast apparel markets, for instance, Christmas sales. I also use population as a control variable because it helps predict the overall number of retail employees in any county.

The DD methodology provides unbiased estimates under the assumption of no systematic difference in trends between treatment and control groups. To show that this assumption is likely to hold in my case, I explicitly control for trends using state specific fourth-order time polynomial, which is possible because of my focus on county-level data. In addition, I perform a robustness check using the number of employees in the entertainment retail industry (books, hobby, music and games) as a dependent variable while keeping the changes in the sales tax rate for the apparel retail industry. Given that permanent tax exemptions occurs only in the latter industry, I expect the coefficient estimates to equal zero in the robustness exercise.

The empirical specification for estimating the effect of the sales tax on household expenditures is very similar to (2.1) with slight adjustments. First, the unit of observation is a household h, resulting in household rather than county fixed effects ν_h. I observe each household at most four times. Second, the tax rate varies at the state level but not the local level because I only observe the household's state of residence: τ_{cm} becomes τ_{sm}.

1.5.2 Pass-through rate

To estimate the pass-through rate of the sales tax on pre-tax prices, I substantially extend the methodology discussed above, which is possible because the CPI data allows me to observe the items priced above and below the exemption threshold in treated localities and states. Thus, I have an in-state (in-locality) control group, which makes my empirical methodology for estimating the pass-through rate essentially triple difference. The unit of observation is now an item denoted by i.[25] I extend the definition of my time index. My data continues to be at a monthly level. However, the variation in tax rate is at finer level due to sales tax holidays, occurring on certain days of a month in some states. Luckily, the CPI data reports the exact day of price collection. So, I am able to control for sales tax holidays that happen disproportionately more in treatment states. Another control variable is item seasonality, $Seasonality_{im}$. I describe it later. Finally, I include item ν_i and month μ_m fixed effects. Thus, my main specification for estimating the pass-through rate becomes:

$$Log(p_{im}) = \alpha + \beta_1 \times \tau_{im} + \beta_2 \times SalesTaxHolidays_{im} + Seasonality_{im} + \nu_i + \mu_m + \epsilon_{im}. \tag{1.3}$$

I omit location fixed effects because item fixed effects controls for this particular characteristic as well as for other constant characteristics: color, size, composition, etc.[26] There is, however, one potential limitation that the inclusion of item fixed

[25] Reminder: an item is a certain good ("Levi's" gray, 100% cotton, M-size, men t-shirt) located in a given store (1235 5th Avenue, New York City)

[26] An alternative approach is to substitute item FE with quote FE. Remember from "Data Section", that a quote consists of consecutive items, which are not necessarily similar. To account for the differences between two items belonging to the same quote, BLS analysts provide a price adjustment measure, a value that explains all non-inflationary discrepancies in the prices of the two items. The BLS analysts often estimate the adjustment using hedonic regression models and personal judgment. This methodology is not consistent over years and across analysts, which may lead to either imprecise

effects imposes on my estimates. It mainly identifies changes in prices for the items that are in the market both before and after the policy change. Yet, in apparel industry, many items are seasonal. Usually, the stores keep them on the shelves for one or two seasons. If, after a change in tax exemption policy, the store manager prefers to wait until item replacement for a price adjustment, perhaps because of menu costs, my estimates of long-run effects are biased towards zero. In my empirical estimates, I show that this bias is not substantial by considering seasonal and nonseasonal items separately at some point of my analysis.

I use monthly fixed effects to control for any common shocks that simultaneously affect both the Northeast and Midwest apparel markets. An example of such a shock could be very cold weather during a particular season or federal government trade regulation changes. In certain cases, I also include state-month-year fixed effects to account for state-specific shocks in prices. The seasonality variables control for substantial changes in prices throughout a seasonal item's lifetime. My data allows me to see the first and last months the item is supposedly in season.[27] I find that the number of months till the end of the season captures sizable price changes. I use a set of eleven dummies to control for seasonal changes:

$$Seasonality_{im} = \sum_{0}^{10} \psi_{season\ end(m)-m} \mathbb{1}(in\ season(m)) + \psi_{11} \mathbb{1}(nonseasonal\ good), \tag{1.4}$$

and I add another dummy for non-seasonal goods.

The changes in tax exemptions cause the tax rate for items in some price cate-

or biased estimates.

[27]The BLS office in Washington decides the months when a certain item is on/off season. This measure has a flaw in that it is not geographically specific. However, given that the weather conditions are presumably the same in the states I consider, it should not affect my estimates.

gories to change, leaving it unchanged for other items. Thus, these unaffected items serve as a within-locality control group in my estimation, which makes my empirical strategy triple differences. Their presence helps me address the concern that state tax policy may be a response to state or local business cycles. For example, state legislators may decrease the tax rates during recessions when prices also fall. Such policy behavior would bias my pass-through rate estimate downwards without the control of unaffected items. To sum up, my empirical strategy provides unbiased estimates of the pass-through rate conditional on no systematic differences in price trends for items priced above or below the threshold. Additional identification comes from the tax-independent seasonal drop in prices (the drop natural to apparel items due to the approach of the season's end). The average magnitude of the drop for an item in season for six months is around 40%. Thus, when the threshold is in place and does not change, the items priced above the threshold, but not substantially above, experience a tax regime change, whereas the rest of the items serve as the control group.

Instrumental Variables

On one hand, the presence of exemption thresholds allows for item-specific tax rate changes, which helps in addressing the endogeneity of the tax policy. On the other hand, the thresholds create an incentive for stores to decrease pre-tax prices that are slightly above the exemption threshold to generate a substantial drop in the after-tax prices. Suppose, a store in Connecticut sells jeans for $50 in March 2010 and the tax rate depends on the price in the following way:

$$\tau_{i,March2010}(price_{i,March2010}) = \begin{cases} 0, \text{ when } price_{i,March2010} < \$50 \\ 6.35\%, \text{ otherwise.} \end{cases} . \quad (1.5)$$

With a cumulative state and local tax rate τ_{lt} of 6.35%, the after-tax price is \$53.17. If the store drops the price by 1¢ the tax rate becomes 0 and the after-tax price \$49.90.

To overcome this endogeneity concern, I use an instrumental variable approach similar to the literature that estimates the income tax elasticity with respect to the marginal tax rate (Gruber and Saez, 2002). The instrument is the would-be tax applied to the predicted price of an item in the absence of the policy. To obtain this predicted price, I regress price $Log(p_{im})$ on fixed effects for the 24 item categories, 4 regions (New York, Connecticut, other Northeast states, Midwest states), month-year dummies and seasonality dummies,

$$Log(p_{im}) = \alpha + \gamma_{item\ category} + \gamma_{region} + \mu_m + seasonality_{im} + \epsilon_{im}, \quad (1.6)$$

restricting my sample to observations not affected by exemption policy. Thus, I exclude observations after year 2000 in both treatment states: New York and Connecticut.[28]

Plugging coefficient estimates into (1.6), I predict the logarithm of prices for the items in the two states after 2000. I then plug in these predicted logarithms into tax rate functions similar to (1.5) to get tax rates in the two states after 2000 that serve as instrumental variables. The intuition for this instrument is the following: the average prices on different apparel categories should be correlated among the control

[28]Vermont does not enter in the CPI data sample

states, New York, and Connecticut; however, the average prices in the control states are not responsive to changes in New York and Connecticut state policies. In results not presented here, I use this average price directly as an instrument. None of the results presented below change in a substantial way.

The instrument averages are pretty close to those of actual sales tax rates. This is not surprising, given that the instrument should be different from the actual tax rate only around the exemption threshold in the treatment states. In all the results presented below, the coefficient before the instrument in the first stage is strongly significant. Its magnitude does not fall below 0.8 and $F-$static always exceeds 20.

1.6 Results

I present graphical evidence and the main regression results from my empirical analysis. First, I show that there is no change in apparel retail prices in response to the exemptions that alter the tax rate, implying that it is the consumers who bear the full incidence of a sales tax rate. Given that consumer expenditures are quite responsive to the sales tax, as I demonstrate in the end of this section, a lack of response by retailers in the price dimension implies an increase in the quantity supplied by retailers. This, in turn, should raise the amount of inputs employed. In this section, I consider two inputs: labor (the number of employees) and one type of capital (the number of stores). While the sales tax does not affect the capital, the retail employment increases by 0.33% in response to a one percentage point sales tax drop. In absolute terms, this number implies that a 4% New York state sales tax exemption makes apparel retailers in this state hire an additional 2,150 employees.

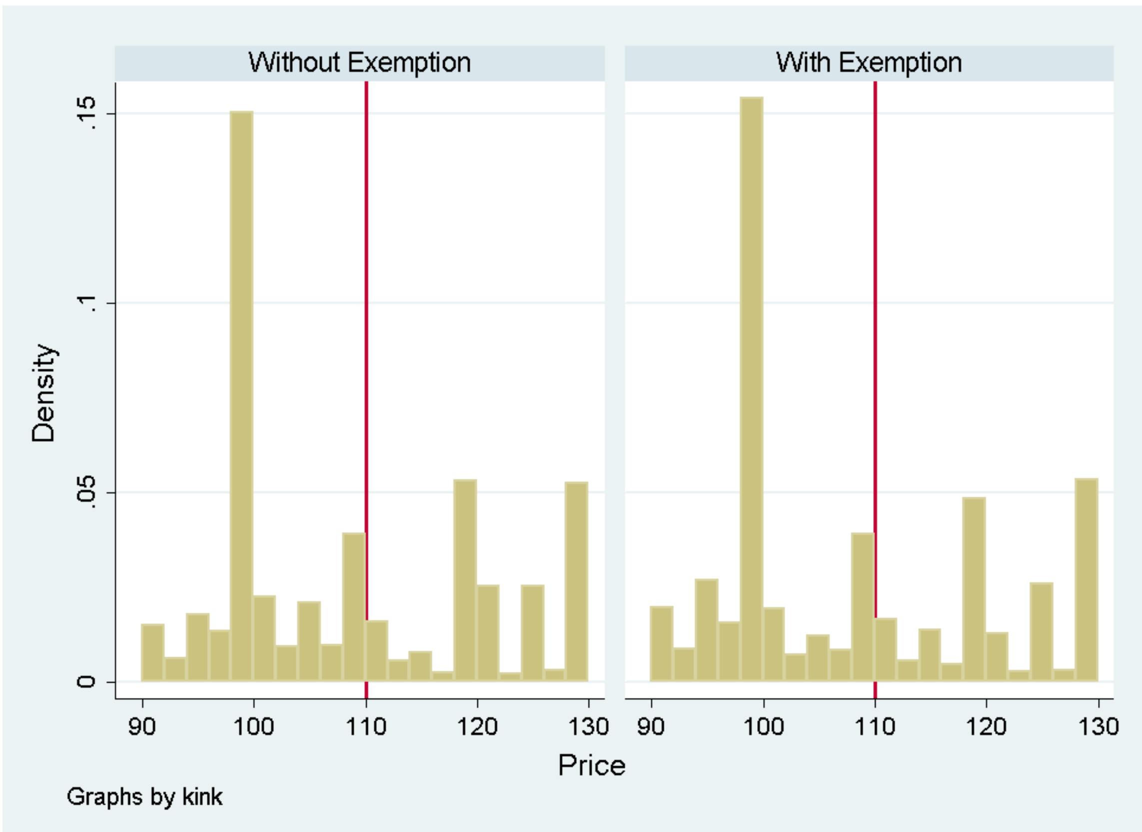

Figure 1.5: Comparison of price distribution around the $110 exemption threshold in New York with (right) and without (left) the tax exemption. Items priced between $90 and $130 are included in the sample. There are 5,250 observations. McCrary (2008) test shows that the drops in the price density around $110 equal 0.74 with exemption and 0.77 without exemption.

1.6.1 Sales Tax and Prices

Tax Incidence Around Exemption Threshold

Before presenting my regression results, I provide graphical evidence that stores do not change pre-tax prices in response to the tax rate, even in the extreme situation of a discontinuous drop in the tax rate when prices drop by a small amount. In Figure 1.5, I plot the distributions of prices for apparel items in New York around the most common exemption threshold value ($110). On the left, I include only the observations from the months without the exemption, and on the right — with the $110 exemption. One would expect a higher density of price distribution just below the threshold value (bunching) when the exemption is in place because a 1¢ drop in retail price from $110 to $109.99 saves a typical New York consumer almost $9.

Surprisingly, the densities behave almost identically around the threshold point. Using the McCrary (2008) test, I find that bunching is slightly higher *without* exemption than with it. There could be several explanations for this behavior, one of which is the lack of tax exemption saliency (Chetty et al., 2009). This explanation, however, is not consistent with a large response of the equilibrium quantity to the sales tax. Another explanation is that a lot of apparel retailers operate nationwide. If consumers do not buy a certain item in New York, the store can ship it to another state to sell. This evidence suggests that it is reasonable to expect consumers to bear the full incidence of sales tax on apparel.

Estimation of Pass-through Rate

Table 3.6 shows the results of my triple difference IV estimation of pass-through rate on apparel pre-tax prices, controlling for sales tax holidays and seasonality. Main changes in the sales tax rate come from the revisions of the tax exemptions in three Northeast states. Given that these revisions affect the prices only for some items in any locality or state, my estimates are robust to the endogeneity of tax policy in response to business cycles, conditional on the assumption that the price trends for items above and below the exemption thresholds ($50-$110) are the same.

In Column (1), I present the estimates for my main specification. I include all items priced below $1,000 and consider both Northeast and Midwest states. The coefficient equals -0.06, which implies that producers pay only 6% of a sales tax. One cannot reject the hypothesis that the consumers bear the full incidence of the tax because the coefficient is not statistically significant. Given that the point estimate of the pass-through rate is quite small, one would like to know whether it has a narrow 95-percent confidence interval that does not include economically important values. Based on the numbers in Column (1), the upper bound on the magnitude of the pass-through rate equals $-0.0632 - 1.96 \times 0.051 = -0.17$. As I show in the end of this section, the deadweight loss value is almost the same whether I use the point estimate or this upper bound value of the pass-through rate.

In Column (2), I show that this result changes sign but still has a wide confidence interval when excluding the Midwest states; in this case, the main control states for New York and Connecticut become Massachusetts, New Jersey and Pennsylvania.[29]

In Columns (4,5), I show that the estimates are similar using either of the two treatment states, where identification comes from state and local exemption alterations in New York or only state exemption alterations in Connecticut. I find that in Connecticut, in Column (5), the effect of sales tax on prices is significant, and its magnitude equals 15%, suggesting that retail stores bear a small share of a sales tax.

[29]There are no observations for Vermont in the Consumer Price Index data

Table 1.4: Panel Data Estimates of the Effect of Sales Tax on Apparel Prices

	Dependent Variable: Log of Pre-tax Price				
	(1)	(2)	(3)	(4)	(5)
VARIABLES	Whole Sample	W./o Midwest States	W./o Sales Tax Holiday Months	Treatment States NY	CT
Sales Tax Rate, %	−0.0632	0.0538	−0.0782	−0.0566	−0.152**
	(0.0515)	(0.0418)	(0.0625)	(0.0478)	(0.0698)
Item and month FE included in all specifications					
No. of Obs.	508,788	267,042	333,334	491,632	418,954
R^2	0.056	0.054	0.046	0.056	0.059
No. of Items	61,331	31,127	51,731	59,471	52,322

*** p<0.01, ** p<0.05, * p<0.1

Notes: Robust standard errors clustered at the state level are in parentheses. Apparel price quotes, tax rate and control variables come from Consumer Price Index micro data; tax exemption data is self-collected from official State government websites. The sample covers January 1997 to December 2012 and all states from the Northeast and Midwest Census Regions. Given the non-linear sales tax schedule in treatment states, I instrument for the sales tax rate . The first-stage F-statistic is substantially above 20 in all the regressions. Each regression includes item and month-year fixed effects and controls for seasonality and sales tax holidays. In all regressions, I exclude items ever priced above $1,000.

However, this estimate is not statistically or economically different from the estimate in Column (1). It may reflect the fact that the average pre-tax price level on apparel is the highest in Connecticut (at $89) relative to all other states in my sample, implying that consumers may invest more in avoiding the sales tax.

I confirm the robustness of zero pass-through rate of a sales tax on producer prices in section 2.7. In addition, I show that the sales tax does not alter the retailers quality, measured by the number of stockouts, and product variety, measured by cancellations. In the next subsection, I consider the effect of sales tax on employment.

1.6.2 Sales Taxes and Employment

Given (i) zero pass-through of sales taxes to retail prices and (ii) evidence from the other papers (Einav et al., 2014; Hu and Tang, 2014) that consumers reduce their pre-tax expenditures at traditional retailers when tax rates increase, stores should respond to changes in the sales tax policy in the quantity dimension. Higher sales tax rates should lead to lower equilibrium quantity, and, in turn, lower usage of inputs.

In this subsection, I estimate how the sales tax affects retailer use of two main inputs: labor and a specific type of capital, measured by the number of establishments. I find that a 1 percentage point increase in the sales tax rate leads to a 0.4% drop in the number of retail employees and a 0.6% decrease in the overall payroll, suggesting lower wages for the remaining employees. However, there is no evidence of any significant reduction in the number of establishments.

Employment

Before presenting my regression analysis, in Figure 1.6 I provide graphical evidence for my main result that employment does indeed decrease in response to hikes in the sales tax. For this, I compare the normalized number of employees hired by apparel retailers in New York and three other Northeastern states: Massachusetts, New Jersey and Pennsylvania. I consider the time period from January 1997 to January 2009.

There are four reasons for choosing these months and states. First, the alterations in the sales tax exemption policy in New York occurs three times: two introductions and one repeal. In each case, the alterations affect a substantial part of the apparel market — items priced below $110 — making these policies symmetric. Third, the changes happen every three years and so are uniformly spread across time. Finally, at least in the first two changes, New York state exemptions affect all counties simultaneously. The last thing to note about the sample for Figure 1.6 is that I include in it only counties that stay in every month for the entire period. The dropped counties usually have the smallest population in their state, and therefore, are unlikely to influence this graph.[30]

In Figure 1.6, I show how normalized number of employees varies in New York versus MA-NJ-PA. For normalization, I subtract the overall number of employees at entertainment retail stores (which sell music, books, games and goods for hobbies) from that at apparel retail stores. This subtraction allows me to eliminate the positive trend component in New York employment. In addition, I divide the resulting series for both geographical areas by their values in March 2000 for the ease of comparison. The normalized employment is lower in New York relative to the other three states

[30]For my regression analysis, I show that my estimates stay significant for both samples with and without these counties.

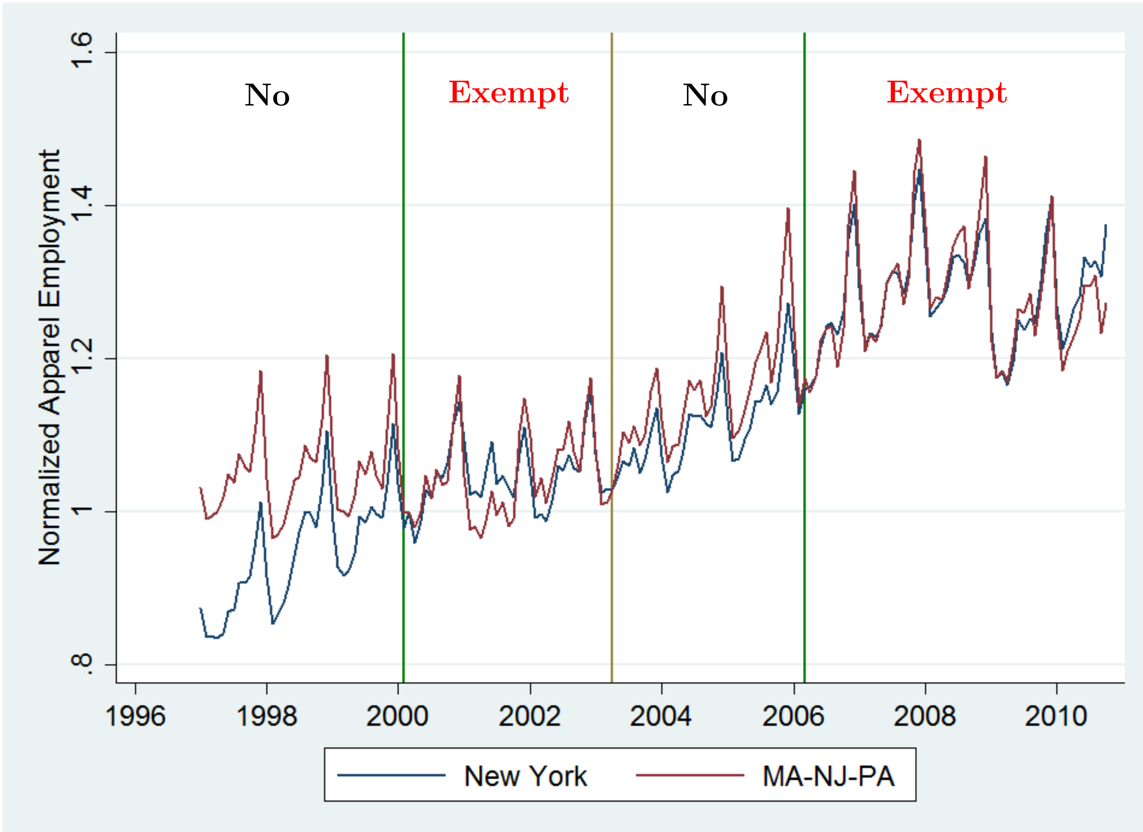

Figure 1.6: Comparison of normalized employment in New York and Massachusetts-New Jersey-Pennsylvania are. Green lines denote exemption introductions, brown – repeals. Normalized employment is the difference between the number of employees in apparel and entertainment stores, divided by its values in March 2000.

when the sales tax is in place: before the first introduction of the tax exemption (left vertical line) and in between the repeal (middle line) and second introduction (right line). When apparel is tax exempt, the employment levels are almost the same in the treatment and control areas.

Estimation of the Effect of Sales Taxes on Employment

Table 1.5 shows the results of my difference-in-difference estimation of the effect of the sales tax rate on employment. Column (1) presents the results for a regression without time trends: a one percentage point increase in the sales tax rate results in a 0.42% decrease in the number of employees. The coefficient is not statistically significant, the standard error being almost equal to the coefficient.

To decrease the standard error, in Columns (2-6) I use state time trends, which are polynomials of the 4^{th} order interacted with state dummies. These trends control for various state economic conditions that may affect apparel employment, such as changes in minimum-wage laws or the unionization of the industry. Indeed, in Columns (2-6) standard errors decrease by roughly a fourth compared to Column (1) after controlling for state trends. In Column (2), I find that the point estimate is almost the same but now strongly significant. I consider its value (-0.40) as my key estimate. It implies that a New York state 4% sales tax exemption on clothing increases the number of employees by $4 * 0.4\% = 3.35\%$, which explains about 10% of time variation of the New York apparel retail employment in my sample. In absolute numbers, tax exemption gives New York state $0.033 * 2,188 * 62 = 2,143$ new jobs, where $2,188$ is the average number of employees in a county and 62 is the number of counties.

For some small counties in my sample, the Census omits the data due to nondisclosure concerns, particularly at the beginning of my sample. The omission may bias my results towards zero if the counties that experience a higher drop in employment are more likely to have missing observations. This is plausible in my case given that lower employment in a county generally results in higher nondisclosure.[31] To show how the nondisclosure affects my results, I perform the regression on two restricted samples. In Column (3), I exclude all the counties that have any missing observations, and the coefficient becomes one and a half times larger: -0.67. This restriction,

[31] I do not find any effect of the sales tax rate on the probability of nondisclosure

however, is on the dependent variable and, thus, may not be effective. In Column (4), I use a restriction on the independent variable, considering only the counties with population higher than 50,000 at any moment in time. The coefficient, as expected, decreases compared to that in Column (3) but stays bigger than in Column (2): -0.49, confirming the concerns of a downward bias.

To make sure that there are no unaccounted factors driving my results, I do two other checks. First, I explicitly control for any changes in retail industry in a given state by using the data on employment in the Entertainment Industry as a control. The coefficient stays strongly significant and equals -0.57. Second, as a placebo test, in Column (6) I regress employment in the entertainment industry on the sales tax rate for clothing items. If there is some general trend in employment that is correlated with the tax rate variable, I expect the coefficient to be similar to Columns (1-5). As we can see, the coefficient in Column (6) is positive, assuaging any concerns. An interesting caveat is that the coefficient is also significant, suggesting that the employees fired in the apparel retail sector may flee into the entertainment retail sector.

In addition to changing the number of employees, apparel retailers can use other instruments of adjusting labor expenditures. Given that apparel retailers hire a number of part-time employees and use a sales-based remuneration system, they can either reduce the number of working hours or the per-hour wage. While the data does not allow me to distinguish between these two channels, I can estimate their cumulative effect using variables for weekly wages and overall payroll. In Table 1.6, I use them as outcome variables. Column (1) shows that the average weekly wage decreases by 0.29% following a one percentage point sales tax increase. Payroll changes consist of the number of employees (estimates in Columns (1-5) in Table 1.5) and the wages

Table 1.5: The Effect of Sales Tax on Retail Employment in Apparel Industry

	Dependent Variable: Log of Number of Employees in Stores					
	(1)	(2)	(3)	(4)	(5)	(6)
			Apparel			Ent.-ment
	No Trend	Trend	Counties w. all observations	Large counties	Control	
Tax Rate	−0.415	−0.395***	−0.666***	−0.489***	−0.569***	0.342***
	(0.264)	(0.083)	(0.071)	(0.076)	(0.144)	(0.089)
Entertainment Employment					0.189**	
					(0.057)	
State Trends	No	Yes	Yes	Yes	Yes	Yes
County and month FE included in all specifications						
No. of Obs.	34,558	34,558	26,788	29,501	29,887	29,887
R^2	0.130	0.154	0.203	0.179	0.185	0.162
No. of Counties	206	206	153	163	201	201

‡ p<0.01, ** p<0.05, * p<0.1

Notes: Robust standard errors clustered at the state level in parentheses. "Ent.-ment" (short for entertainment) stores sell books, computer games, music and items for hobby. Data on employment comes from the Quarterly Census of Employment and Wages, on population – from the Census. "State Trend" is a control variable, which is an interaction of a fourth order polynomial with a state dummy. The number of counties is smaller in Columns (3) and (4) because I consider only the counties that are present in the sample for all the months and counties that have population above 50,000 respectively. In Columns (5-6), I use data on the entertainment employment as a robustness check.

Table 1.6: The Effect of Sales Tax on Employee Remuneration and Establishments in the Apparel Retail Industry

	(1)	(2)	(3)	(4)
Dependent Var.	Wage	Payroll	Taxed Payroll	Establishments
Tax Rate	−0.289***	−0.630**	−0.873*	−0.122
	(0.0714)	(0.247)	(0.395)	(0.285)
County, month FE, and state trends included in all regressions				
No. of Obs.	13,784	13,784	9,889	13,784
R^2	0.499	0.223	0.664	0.282
No. of Counties	206	206	206	206

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes: Robust standard errors clustered at state level in parentheses. All the dependent variables refer to the apparel industry. "Taxed Payroll" are the taxed expenditures on labor borne by the apparel retailers. In all the regressions I include the logarithm of population as well as state trends, which is an interaction of a fourth order polynomial with a state dummy, as controls.

of remaining employees. Columns (2) is in fact quite close to the overall combined effect: -0.63.

Finally, I check whether the sales tax rate also affects the equilibrium amount of capital, measured by the number of establishments. The coefficient on tax rate in Column (4) of Table (6) is of the right sign but is not significantly different from zero. Indeed, it is hard to expect that the sales tax rate, which varies every three years on average, would influence the capital decisions which are usually long-term.

1.6.3 Sales Tax and Total Expenditures on Apparel

I present the empirical results for the effect of sales tax on total household expenditures on apparel. Using the Consumer Expenditure Survey (CE) data for the same geographical and time span as above, I find that consumers are quite responsive to apparel prices, with the elasticity of demand ranging from -4.9 to -1.1. Data limitations, which allow me to use tax rate variation only at the state level, might influence the precision of my estimates. Though one should treat them critically, I prefer to keep them here in order to estimate the elasticity of supply and perform deadweight loss analysis later.

I use the CE interview component. Given that the respondents report their monthly apparel spending for the past three months at all interviews, I only consider the data for the first month prior to the interview. It does not substantially affect my estimation power but allows me to avoid any bias associated with incorrect reporting of purchases that increases with time away from the interview (Bradburn, 2010).[32]

[32]For example, a sales tax increase may lead to a higher saliency of a sales tax rate

I use the difference-in-difference empirical strategy explained in subsection 1.5.1. This strategy has three particular features. First, the regressions are at the household-month level. Second, I use either a dummy variable, which equals 1 when expenditures for a household in a given month are non-zero, or a continuous variable for expenditures. Given that the expenditures can be nonnegative, I apply the inverse hyperbolic sine transformation to the dependent variable. It has the same interpretation as a log-dependent variable but permits zero values.

Finally, consistent with the previous subsection, I define my main explanatory variable to equal the tax rate for the lowest price category exempted from the sales tax. This implies that exemptions with different thresholds ($50, $55, $75, $110) have the same treatment. I show that this assumption does not substantially affect my estimates in two ways. First, I restrict my sample to include only tax exemptions with the threshold equal to $110; second, I create an adjusted tax rate, which equals the average tax rate for the items priced below $110.

Table 1.7 shows the estimates of the effect of sales tax on household apparel expenditures. In Column (1), controlling for state trends, I find that the coefficient of interest equals -2.3: a 1% percentage point increase in sales tax leads to a 2.3% decrease in tax *inclusive* household expenditures on apparel. Given that the expenditures are tax inclusive, the coefficient on the sales tax is an estimate for the sum of the demand elasticity plus one.[33] In Column (1), I show that the coefficient is significantly different from 1, and, hence, I readily reject the null hypothesis that the demand elasticity is zero. In Column (2), I use a dummy for whether a household purchases any apparel in a given month or not. I find that a 1 percentage point in-

[33]This formula is an approximation because it ignores the presence of the apparel items purchased at the Internet/catalog retailers, which do not charge sales tax. In section 1.8, I provide a precise formula

crease in sales tax decreases the probability that a household shops in a given month by 0.3%. This implies that the sales tax affects consumer shopping behavior on the extensive margin too.

In Columns (1-2), my main explanatory variable is the tax rate on exempted items. To see whether ignoring the exemptions thresholds affect my results, I do the following two exercises. First, in Columns (3-4), I consider an alternative measure of the sales tax rate. I compute the average tax rate for the items priced below \$110 $\tau_{\text{adjusted}} = \tau \times \Pr(\text{price} < \text{threshold})$, where the probability is from a normal distribution with mean and standard deviation based on the summary statistics for prices in Table 2.2. Surprisingly, the results stay almost the same as in Columns (1-2). Second, in Columns (5-6), I restrict my sample to exemptions where the threshold equals \$110 . This excludes Connecticut and all observations after April 2011, when New York exempts items priced below \$55. The coefficients in both regressions slightly decrease and stay significant. For the continuous dependent variable, the coefficient on the tax rate now equals -2.0, implying that a 1 percentage point increase in the sales tax rate decreases the tax inclusive expenditures by 2%. The implied demand elasticity from the coefficient equals -3.0, the 95% confidence interval being $[-3.9; -1.1]$. In the computation of elasticities and deadweight loss in section 1.8, I use these both the point estimate and the upper confidence interval bound. The result for the dummy variable in Column (5) is almost identical to that in Column (1).

Table 1.7: The Effect of Sales Tax on Clothing Expenditures

	Dependent Variable: Overall Apparel Expenditures or Its Dummy					
	(1)	(2)	(3)	(4)	(5)	(6)
	Whole Sample				\$110 Thresholds	
	Ex.-tures	Dummy	Ex.-tures	Dummy	Ex.-tures	Dummy
Tax Rate	$-2.26^{\dagger\dagger\dagger}$	-0.35^{**}			$-2.01^{\dagger\dagger}$	-0.33^{**}
	(0.876)	(0.112)			(0.955)	(0.121)
Adj. Tax Rate			$-2.27^{\dagger\dagger\dagger}$	-0.35^{**}		
			(0.897)	(0.119)		
Household, month FE, and state trends included in all regressions						
No. of Obs.	78,824	78,824	78,824	78,824	61,864	61,864
R^2	0.045	0.033	0.045	0.033	0.047	0.034
No. of HH	26,043	26,043	26,043	26,043	20,143	20,143

Notes: Robust standard error clustered at the state level in parentheses. Data on household monthly purchases in between January 1997 and January 2013 comes from the Consumer Expenditure Survey. I consider only the data for the first month prior to the interview. The coefficient before all the tax rate variables in odd Columns (1,3,5), is significantly different from one, with $P-$value less than 0.05 in all the cases. In these columns, I apply an inverse hyperbolic sine transformation (IHST) for the outcome variable, the interpretation being similar to log-transformation specifications. In even Columns (2,4,6), the outcome variable is a dummy equal to 1 if a household buys any apparel in a given month. In Columns (3-4), I use an average tax rate for items priced below \$110 as my main explanatory variable. All the specifications in the table include state, month fixed effects and individual state time trends of 4^{th} order. Alternatively, in Columns (5-6), I restrict the sample to include only exemptions with thresholds equal to or above \$110, dropping all observations after April 2011 and observations for Connecticut households.

1.7 Robustness Checks

I augment my estimation of the pass-through rate started in the previous section. First, I confirm the robustness of my zero pass-through rate result in a variety of ways. Along the way, I show that the tax incidence differ on various subsamples. I find that the tax rate is not fully passed through to consumer prices of non-seasonal goods (tax incidence is 79%), girls apparel (62%) and footwear (76%). Second, I check whether the probability of items missing from store shelves changes in response to a sales tax rate, which can bias my pass-through rate estimates. I divide the missing observations into temporarily missing (stockouts) and permanently missing (cancellations) and present evidence that there is no effect of the sales tax rate on either of them. Simultaneously, the latter statement implies that both store quality (measured by the rate of stockouts) and store variety (measured by the rate of cancellations) stay constant in response to the sales tax rate.

1.7.1 Sensitivity of Zero Pass-through Rate Estimate

Table 1.8 shows that my estimates of pass-through rate are similar across time periods and the direction of tax changes. As a baseline, I use the sample from Column (1) of Table 3.6, which excludes all the items that are ever priced above $1,000. First, I divide the sample into pre-crisis (before 2008) and post-crisis (after 2008) periods. There are two rationales behind this step. First, the behavior of market participants after the Great Recession may be different, in and of itself. For instance, the American Footwear and Apparel Association reports that consumer spending on apparel shrank after the Great Recession and returned to pre-crisis levels only in 2012. Second, both NY and CT changed their tax rate at some point during the recession. NY repealed tax exemption in October 2010 and reintroduced it in April 2011. This coincided with a big change in cotton prices in late 2010 and early 2011.[34] Column (1) shows that the point estimate during the pre-crisis period is almost the same as in the main specification (Column (1) in Table 3.6). After the start of the Great Recession (Column (2)), the coefficient becomes positive but still does not differ from zero. In the last two columns of Table 1.8, I provide estimation results for tax increases

Table 1.8: Sensitivity to Time, Treatment State and Tax Change Direction

	Dependent Variable: Log of Apparel Pre-tax Price			
	(1)	(2)	(3)	(4)
	Pre-Crisis	After-Crisis	Tax↓	Tax↑
Sales Tax Rate, %	−0.070*	0.060	−0.042	0.118
	(0.037)	(0.167)	(0.040)	(0.085)
Item and month FE included in all specifications				
No. of Obs.	379,801	127,408	195,115	92,948
R^2	0.058	0.054	0.058	0.058
No. of Items	46,022	19,208	28,964	14,303

*** p<0.01, ** p<0.05, * p<0.1

Notes: Robust standard errors clustered at the state level are in parentheses. The first-state F-statistic for sales tax rate instrument is substantially above 20 in all the regressions. Each regression includes item and month-year fixed effects and controls for sales tax holidays and seasonality. Column (1) excludes all observations from Connecticut, whereas Column (2) – from New York. Column (3) considers time period before January 2008, whereas Column (4) after December 2008. Column (5) restricts the sample to include time periods when the tax decreases (1999-2001; 2005-2007), whereas Column (6) – when the tax increases (2002-2004). In all regressions, I exclude items ever priced above $1,000.

and decreases separately. From classical economic theory we know that the estimates should be the same. Yet, previous research finds that they differ, for instance in (Doyle and Samphantharak, 2008). For the tax increase, I choose the time period from 2002

[34] "Era of Cheap Apparel May Be Ending for U.S." by Floyd Norris, 17 January 2014, New York Times

to 2004, when both New York and Connecticut repeal sales tax exemptions, whereas for the tax drop, I consider two time periods when NY introduces a tax exemption (1999-2001 and 2005-2007). The coefficient is still insignificant, implying that the sales tax is fully passed through to consumer prices regardless of the direction of change in the tax rate.

Table 1.9: Robustness of Tax Incidence Estimates To Time Controls

Dependent Var.	Log of Apparel Pre-tax Price				Item on Sale	
	(1)	(2)	(3) NY	(4) CT	(5) Men and Women	(6)
Tax Rate	−0.015	−0.070	−0.048	−0.145**	4.21	5.64
	(0.054)	(0.088)	(0.086)	(0.070)	(9.26)	(6.47)
Month FE	X				X	X
Controls for State Trends:						
4th order polynomial	X					
State-Month FE		X	X	X		
Item fixed effect included in all specifications						
No. of Obs.	508,788	508,788	491,632	418,954	287,929	508,788
R^2	0.057	0.057	0.057	0.060	0.045	0.035
No. of Items	61,331	61,331	59,471	52,322	39,083	61,331

*** p<0.01, ** p<0.05, * p<0.1

Notes: Robust standard errors clustered at the state level in parentheses. Column (1) adds a 4th order time polynomial for treatment states. In Columns (2-4), I use a state dummy interacted with month fixed effects. Column (3) excludes observations from Connecticut, whereas Column (4) from New York. In Columns (5-6), I explore how sales tax rate affects the probability of item being on sale. Each regression includes item fixed effects and controls for seasonality and sales tax holidays. The F-statistic for sales tax rate instrument is substantially higher than 20 in all the regressions. In all regressions, I exclude items ever priced above $1,000.

A key advantage of my empirical strategy is that it allows me to control for state specific time shocks. Thus, including explicit controls for the state trends or state-month interactions should not affect my estimates. The left panel of Table 2.6 shows that it is indeed the case. Column (1) shows the results for the whole sample when I include an interaction between state dummies and a fourth-order time polynomial, whereas Column (2) shows them when I include an interaction between state and month dummies. Columns (3) and (4) repeat Column (2) but exclude observations from Connecticut and New York respectively. The coefficients on the tax rate in all these columns stays almost the same compared to the cases when I do not control for trends. This outcome is also true for the pass-through rate in Connecticut alone, which remains significant.

In the right panel of Table 2.6, I present additional evidence for the zero pass-through rate on retail prices. I show that the tax rate does not affect the likelihood of items being on sale, a common way of changing the prices in the apparel industry.[35] My summary statistics show that sales occur in 40% of the price observations which permits the use of linear specification rather than logit or probit regressions. Column (5), which restricts the sample to observations for men's and women's apparel only, and Column (6), the whole sample, show that the estimate of pass-through rate is not statistically different from zero. In the second case, its magnitude implies that a 1 percentage point hike in the sales tax increases the likelihood of a sale by 5%.

In Table 2.7, I explore whether pass-through rate differs across apparel groups. Following the BLS, I consider six groups of apparel: Men, Boys, Women, Girls, Footwear and Babies.[36] The point estimates for Boys, Girls and Footwear (Columns 2,4, and 5) are substantially different from the full sample results. In all these cases,

[35] The price variable in CPI data reflects the price changes associated with sales. I employ this regressions to estimate the extensive margin of price changes in response to the tax rate.

[36] This are mutually exclusive goods. Footwear is a separate category

Table 1.10: Tax Incidence for Different Apparel Groups

	Dependent Variable: Log of Apparel Pre-tax Price					
	(1) Men	(2) Boys	(3) Women	(4) Girls	(5) Shoes	(6) Babies
Sales Tax Rate	−0.037	−0.159	0.013	−0.377***	−0.241**	0.034
	(0.077)	(0.188)	(0.124)	(0.103)	(0.098)	(0.103)
Item and month FE included in all specifications						
No. of Obs.	141,911	25,868	146,016	34,733	90,166	31,904
R^2	0.036	0.060	0.121	0.092	0.029	0.078
No. of Items	11,780	2,453	27,306	5,066	8,482	3,446

*** p<0.01, ** p<0.05, * p<0.1

Notes: Robust standard errors clustered at the state level in parentheses. Columns (1-6) restrict the sample to apparel items of certain type: Men, Boys, Women, Girls, Footwear, Babies. Each regression includes item and month-year fixed effects and controls for seasonality and sales tax holidays. The first-stage F-statistic for sales tax rate instrument is substantially above 20 in all the regressions. In all regressions, I exclude items ever priced above $1,000.

the coefficient is negative and relatively large in magnitude. In the last two cases it is also statistically significant.

These results suggest that consumers of these items have a higher elasticity of demand for apparel sold at local retail stores. This is a plausible argument for the footwear industry where e-commerce is thriving.[37] A good signal of it is the emergence of a big online shoe retailer "Zappos.com" in the early days of the Internet, which does not have an analog in the clothing industry. A potential explanation for more elastic demand for girls' and boys' clothing is that for kids fit matters less, allowing people to search more in thrift stores or get items from friends.[38]

In Table 2.8, I break the sample into seasonal and non-seasonal goods, the latter consisting mainly of cheaper items like underwear and t-shirts. Focusing on the sample to non-seasonal goods allows me to consider the effect of the sales tax on retail prices when both sides of the market have time to adjust to a new tax policy. 70% of price observations in my sample come from seasonal items, which are present in the market only for certain months of a year and often go out of the market soon after a tax exemption change. Instead of adjusting the price immediately, retailers may be willing to wait until item replacement. In this case, my estimates of the pass-through rate would be biased towards zero.

For non-seasonal goods, which have an average lifespan in my sample of two and a half years, this argument is unlikely to hold. Indeed, in Column (1), the pass-through rate for non-seasonal goods is statistically significant and different from the point estimate for the whole sample. In the long run, retailers share some burden of the sales tax and pay 21% of it for such items.

[37] "Why shoes have dominated this generation of e-commerce", 18 Feb 2013, by Michael Carney

[38] Another explanation may be that teenagers (a) spend a bigger portion of their income on clothing and (b) have more free time to search for a good bargain.

Columns (2-5) show that the pass-through rate is not significantly different from zero for seasonal goods. In Column (2), I consider all seasonal items. Columns (3-5) focus on two most popular seasons: fall (season lasts exactly from August to January) and spring (exactly from February to July). In all cases, the pass-through rate, though insignificant, is positive, which is theoretically possible only under the assumption of imperfect competition. The sign of the pass-through rate signals another economically important difference between seasonal and non-seasonal items, through the extent of competition. Non-seasonal goods are usually more generic which promotes competition across retailers supplying these goods. As I showed in Appendix 1.10, under some plausible conditions, this should lead to a higher pass-through rate for retailers. Thus, my conclusion is that long-run incidence lies somewhere in the middle between the estimates in Columns (1) and (2). As I show below, my deadweight loss analysis is not sensitive to changes in the tax rate within this range.

1.7.2 Stockouts and Cancellations

In this subsection, I test whether the tax rate affects the probability of encountering different types of missing observations. There are two reasons for this analysis. First, a change in the overall probability of missing observations may bias my estimates of the pass-through rate. Consider the case when a decrease in the sales tax leads to a disproportionally higher demand for cheaper items. Given this change in the demand, more low-priced items might be sold out. This, in turn, raises average prices, thus biasing the pass-through rate estimates downwards.

Second, the changes in the rates of stockouts, items temporarily missing from the shelves, and of cancellations, items permanently missing from the shelves, is of

Table 1.11: Tax Incidence for Seasonal and Non-seasonal Items

	Dependent Variable: Log of Apparel Pre-tax Price				
	(1) Non-Seasonal	(2) Seasonal	(3) Fall Spring	(4) Spring	(5) Fall
Tax Rate	-0.211***	0.003	0.113	0.145	0.081
	(0.050)	(0.077)	(0.080)	(0.127)	(0.117)
Item and month FE included in all specifications					
No. of Obs.	184,579	324,193	101,954	49,894	52,059
R^2	0.010	0.075	0.091	0.087	0.102
No. of Items	13,962	47,410	15,466	7,651	7,821

‡ p<0.01, ** p<0.05, * p<0.1

Notes: Robust standard errors clustered at the state level are in parentheses. In additionIn Column (1), I restrict the sample to only non-seasonal items, whereas in Column (2) - seasonal. In Columns (3-5), I consider Fall and Spring goods. Fall season starts in August and ends in January, whereas Spring one starts in February and continues until July. I control for sales tax holidays in all regressions. The first-stage F-statistic for sales tax rate instrument is substantially above 20 in all the regressions. In all regressions, I exclude items ever priced above $1,000.

interest in and of on their own because they represent quality and variety dimensions of the stores respectively. For example, the increase in the stockout rate implies low store quality as a consumer is less likely to find the size or the color of an item she likes. Of course, this statement is completely true only if the variety of items, proxied by the cancellation rate, stays the same.

For my estimation, I use the same specification as for prices:

$$Log(Stockouts_{it}) = \alpha + \beta_1 \times \tau_{it} + \beta_2 \times SalesTaxHolidays_{it} + Seasonality_{im_t} + \nu_i + \mu_{m_t} + \epsilon_{it}, \tag{1.7}$$

the only main difference being the dependent variable. It is now a dummy that is nonzero when the observations are missing, out of stock or canceled, depending on the regression. To avoid too many zeros at the beginning of coefficient values, I multiply this dummy by 100. For cancellations, I also use fixed effects at the quote rather than item level; otherwise the number of observations drops substantially.[39] This change, however, does not affect my results. When price is missing, the sales tax rate is also missing in the BLS data. In this case I use the last observed tax rate for this item as the actual tax rate. This substitution does not bias my results because I use an instrument that is constructed the same way as in the price regression.

In Table 2.4, I present the results for non-seasonal goods. Column (1) shows that the sales tax rate does not influence the probability of encountering a missing observation on a unrefined sample. I find some evidence that the quality of retail stores increases (the number of stockouts decreases, in Column (2)) and variety decreases (the number of cancellations increases in Column (3)). After applying the sample refinements discussed in Appendix 1.12 for stockouts and cancellations in Columns

[39] A quote consists of several consecutive items

Table 1.12: The Effect of Sales Tax Rate on Stockouts and Cancellations, Nonseasonal Goods

				With Data Refinements	
Dependent Variable	All Missing	Stockouts	Cancellations	Stockouts	Cancellations
	(1)	(2)	(3)	(4)	(5)
Tax Rate	−0.0502	−0.280***	0.109**	−0.101	−0.00641
	(0.101)	(0.0788)	(0.0524)	(0.0774)	(0.0370)
Item and month FE included in all specifications					
No. of Obs.	215,197	175,821	202,284	126,906	194,277
R^2	0.069	0.020	0.003	0.003	0.002
No. of Items	23,155	18,765		13,437	
No. of Quotes			8,560		8,550

*** p<0.01, ** p<0.05, * p<0.1

Notes: Robust standard errors clustered at state level in parentheses. Dependent variables are dummies that equal 100 if an item is missing, temporarily out of stock (stockout) or permanently out of stock (cancellation). Column (1) includes data for both stockouts and cancellations. Columns (2,4) contain only observations with stockouts, whereas Columns (3,5) – with cancellations. The number of observations in Columns (4-5) is smaller because I apply the Matsa (2011) refinement for dependent variables: in case of stockouts, I drop all the observations three months prior to their cancellations. In addition, I consider only observations in between first and last valid price observation. I only keep cancellations that occur after valid price observations in the sample. Each regression includes item and month-year fixed effects and controls for seasonality and sales tax holidays. The first stage F-statistic for the sales tax rate instrument is substantially above 20 in all the regressions. In all regressions, I exclude items ever priced above $1,000.

(4-5), both coefficients become small and insignificant. The point estimate in Column (4) says that a 5% rise in sales tax results in 0.5% decrease in missing observations. While this effect is big relative to the average rate of missing observations for nonseasonal items (5%), it is small overall. The point estimate in Column (6) is almost zero. Thus, I conclude that for nonseasonal items (a) missing observations do not influence my pass-through rate results and (b) neither quality nor variety of apparel retailers change in response to changes in the sales tax rate.

In Table 2.5, I repeat the regressions in Table 2.4 but now for seasonal items. In Columns (1-3), I consider the effect of the sales tax rate on all missing observations, stockouts and cancellations. In all three regressions, I observe significant positive effects that are high in magnitude on both stockouts (0.5%) and cancellations (0.18%). The first coefficient is counterintuitive; a higher rate of stockouts implies higher demand for the items. It is hard to expect that a sales tax increase boosts the demand for items. Indeed, after the refinement I find no significant effect of the sales tax rate on either stockouts or cancellations in Columns (4) and (5). The point estimate for both effects essentially equals zero in both Columns (4) and (5). As in the case of nonseasonal items, I conclude that my (a) missing observations do not influence my pass-through rate results and (b) neither quality nor variety of apparel retailers change in response to the sales tax.

Table 1.13: The Effect of Sales Tax Rate on Stockouts and Cancellations, Seasonal Goods

	(1)	(2)	(3)	(4)	(5)
				With Data Refinements	
	Missing	Stockouts	Cancel.	Stockouts	Cancel.
Tax Rate	0.660***	0.502***	0.184***	−0.0447	−0.0343
	(0.0686)	(0.0797)	(0.0379)	(0.0780)	(0.0259)
Sales Tax Holiday	1.689**	1.751*	−1.091*	0.896	−0.505
	(0.742)	(0.987)	(0.626)	(1.065)	(0.475)
Item and month FE included in all specifications					
No. of Obs.	828,846	434,807	764,751	191,412	399,059
R^2	0.253	0.108	0.084	0.011	0.004
No. of Items	99,111	78,448		28,952	
No. of Quotes			30,024		28,814

‡ p<0.01, ** p<0.05, * p<0.1

Notes: Robust standard errors clustered at state level are in parentheses. Dependent variables are dummies that equal 100 if an item is missing, temporarily out of stock (stockout) or permanently out of stock (cancellation). Column (1) includes data for both stockouts and cancellations. Columns (2,4) contain only observations with stockouts, whereas Columns (3,5) – with cancellations. The number of observations in Columns (4-5) is smaller because I apply the Matsa (2011) refinement for dependent variables. For both stockouts and cancellations, I drop all out-of-season observations. In addition, for stockouts I consider only observations in between first and last valid price observation. I only keep in the sample cancellations that occur after valid price observations. Each regression includes item and month-year fixed effects and controls for seasonality. The first-stage F-statistic for the sales tax rate instrument is substantially higher than 20 in all the regressions. In all regressions, I exclude items ever priced above $1,000.

1.8 Elasticities and Deadweight Loss

Substantially decreasing the labor expenditures of traditional apparel retailers, sales tax must generate sizable distortions in apparel market. Using the methodology in Goulder and Williams (2003), which explicitly accounts for distortions in the labor market, I find that a sales tax of 5% results in a 17¢ average deadweight loss for every tax revenue dollar collected. In addition, I find that the presence of the catalog and online retailers does not substantially affect my estimates. Along the way, I also compute the demand and supply elasticities to make my results comparable with other papers.

First, I derive the formula that expresses the demand elasticity for apparel sold at traditional retailers $\epsilon_{D,l}$ as a function of Einav et al. (2014)'s parameter, $\beta_o = \frac{d\log(Exp_o)}{d\tau}$, and the two parameters estimated earlier in this paper (the pass-through rate of a sales tax on pre-tax prices, $\rho = \frac{d\log(p_l)}{d\tau}$, and the effect of the sales tax rate on total apparel expenditures, $\beta_{total} = \frac{d\log(Exp)}{d\tau}$).[40] Index l represents the fact that most purchases at brick-and-mortar retailers happen locally, whereas index o represents online.

Note that one can express the effect of the sales tax rate on local expenditures as a function of the demand elasticity, $\epsilon_{D,l}$, and the pass-through rate ,ρ:

$$\frac{d\log(Exp_l)}{d\tau} = \frac{d\log(Exp_l)}{d\log(p_l \times (1+\tau))} \frac{d\log(p_l \times (1+\tau))}{d\tau} = (\epsilon_{D,l} + 1)(\rho + \frac{1}{1+\tau}). \quad (1.8)$$

To recover the elasticity, I first totally differentiate the trivial equality for overall

[40]The pass-through rate formula here is the first approximation of the formula in section 1.3 because $log(1+\tau) \approx \tau$

household expenditures on apparel,

$$Exp = Exp_o + Exp_l, \tag{1.9}$$

by tax rate τ and divide both parts of it by Exp to obtain:

$$\underbrace{\frac{d\log(Exp)}{d\tau}}_{\beta_{total}} = \underbrace{\frac{d\log(Exp_o)}{d\tau}}_{\beta_o} \times \underbrace{\frac{Exp_o}{Exp}}_{\alpha} + \frac{d\log(Exp_l)}{d\tau} \times \underbrace{\frac{Exp_l}{Exp}}_{1-\alpha}, \tag{1.10}$$

where α is the share of online/catalog expenditures out of the total. Then, I plug (1.8) into (1.10) and rearrange the terms to get the following representation of the elasticity of demand for apparel at traditional retailers:

$$\epsilon_{D,l} = \frac{\beta_{total} - \alpha\beta_o}{(1-\alpha)(\rho + \frac{1}{1+\tau})} - 1 \tag{1.11}$$

This formula provides two comparative statics results important for this discussion. First, the demand elasticity increases in absolute terms in α and β_o. Thus, disregarding the online segment of apparel market results in a lower elasticity of demand. Given that the household generally avoids paying sales tax on online purchases in that period, this value serves as a lower bound for the elasticity of demand for the total expenditures on apparel. Second, lower pass-through rate results in a higher demand elasticity.

The derivation of expression (1.11), as well as of the demand elasticity itself, does not depend on the degree of competition. This is not true for the supply elasticity, which I can only recover from the pass-through rate and expression (1.11) based on the assumption of perfect competition:

$$\epsilon_{S,l} = \frac{\beta_{total} - \alpha\beta_o}{(1-\alpha)\rho} - (1 + \frac{1}{(1+\tau)\rho}). \tag{1.12}$$

The expressions (1.11) and (1.12) show that, keeping pass-through rate fixed, the ratio of two elasticities stays constant. Hence, they are inversely related.

Now, I am ready to compute both elasticities and the deadweight loss for the traditional apparel market by using the empirical values for β_o, β_{total}, ρ and α. I use the formula from Goulder and Williams (2003) for computing average deadweight loss:

$$\overline{\text{DWL}} = \frac{\frac{1}{2}\tau_{apparel}\epsilon_{D,l} - \tau_{Labor} * \epsilon_{Labor}(\theta + 1)}{1 - \tau_{Labor}\epsilon_{Labor,Income}}, \tag{1.13}$$

where I set the tax rate on apparel, $\tau_{apparel}$, equal to 5%. I use the values for the parameters associated with labor markets suggested by Goulder and Williams (2003): tax rate for labor supply, $\tau_{Labor} = 40\%$, the compensated labor supply elasticity, $\epsilon_{Labor} = 0.25$, the income elasticity of labor supply, $\epsilon_{Labor,Income} = -0.2$, and I assume that clothing is an average substitute for leisure, $\theta = 0$. An implicit assumption of this model is perfectly competitive markets for labor and apparel. Relaxing this assumption leads to a higher estimate of the loss.

In the first three rows of Table 1.14, I explicitly account for the presence of the online segment of the apparel market. I compute its average over-the-years share based on the Annual Retail Trade Survey performed by Census. I use the estimate of 1.82(0.86) for the effect of the sales tax rate on online sales (β_o), from Column (1) of Table 6 in Einav et al. (2014). In the first row named “Preferred Values”, I use the point estimates of the coefficients from the main specifications: the effect of the sales tax on overall apparel expenditures, β_{total}, comes from Column (3) of Table 1.7, and the pass-through rate on retailers, ρ, from Column (1) in Table 3.6. With these values, I obtain a pretty elastic demand: -3.37. This result is consistent with Agarwal et al. (2013) who also use the Consumer Expenditure Survey and short-term source of variation in sales tax – sales tax holidays– to estimate the elasticity of clothing demand to be in between 4-6 in absolute value.

Given the close-to-zero pass-through rate, the supply curve is close to being absolutely elastic: 62. Note, that it is fully consistent with Figure 1.5 presented in the "Results" section. Perhaps, a plausible explanation for this result is the following. Since my natural experiment affects the sales tax in only three states, the supply function here may represent the ability of firms to divert in or out the flow of items that are already produced from other states to the treatment ones, rather than the ability of firms to produce an additional unit of output.

The entry in the previous to the last column of Table 1.14 shows the annual deadweight loss $DWL =$ \$2.1 billion if there is a 5% sales tax rate on clothing for the whole US. The average deadweight loss in this case equals 0.17: \$1 raised in revenue results in a loss of 17¢. In fact, the two terms in equation (1.13) are almost equal, thus the marginal deadweight loss, e.g the loss of increasing a tax rate from 5% to 6%, is one and a half times larger: 25¢.

I do not present the computations of the standard errors for neither elasticities nor losses because this requires the knowledge of the non-diagonal elements in β_{ce}, β_e and ρ variance-covariance matrices.[41] To obtain the estimates of the elements, I

[41] When I use some reasonable assumptions for this matrix, I find that standard errors are very small, resulting in the strong significance of all the estimates in the table below

would need to have simultaneous access to three separate data sets, two of which have substantial non-disclosure restrictions. Instead, I show how my computations change under alternative values of the parameters, which in certain cases go to and beyond the 95% confidence intervals for the estimates. In the second row, I show how my computations of the elasticities and losses change when I use an upper bound of the 95% confidence interval for the effect on expenditures. All the estimates decrease in their magnitudes. The demand elasticity becomes equal to -1.4, supply one — 34, and average deadweight loss 0.12. This estimate of the deadweight loss is conservative in the sense that to arrive at this number I make all the assumptions in favor of minimizing the deadweight loss.

In the next rows of Table 1.14, I present the estimates of the elasticities and losses under an alternative value for the pass-through rate of the sales tax on retail prices, $\hat{\rho}$, from Column (1) of Table 2.8, which considers only nonseasonal items. The supply elasticity drops by almost four times: 17. However, both deadweight loss and its ratio to revenue are very close to the "Preferred Values" case. This is not surprising given that small changes in the pass-through rate do not alter the demand elasticity estimate.

In the last row of Table 1.14, I check how the elasticities and deadweight loss estimates change if I do not consider online retailers. All the estimates again are very close to the "Preferred values" case. This is due to a relatively small share (less than 6%) of online and catalog retailers in the market during the time period considered.

Table 1.14: Estimates of Demand Elasticity, Supply Elasticity and Deadweight Loss

	(1)	(2)	(3)	(4)	(5)	(6)
	Estimated Parameters		Resulting Values			
Scenarios	β_{total} $\frac{dExp_{total}}{d\tau}$	ρ Pass-through	$\epsilon_{D,l}$	$\epsilon_{S,l}$	DWL,$	$\frac{\text{DWL}}{\text{Rev.}}$
With Online Retail:						
Share $\alpha = 0.052$; $\beta_o = \frac{dExp_o}{d\tau} = 1.82$						
Preferred Values	-2.01	-0.062	-3.37	62.7	2.09 billion	0.17
Upper Bound of β_{total}	-0.237	-0.062	-1.37	34.0	1.48 billion	0.12
Alternative ρ	-2.01	-0.211	-3.81	16.9	2.21 billion	0.18
Without Online Retail: $\alpha = 0$						
Preferred Values	-2.01	-0.062	-3.14	59.3	2.03 billion	0.17

Note: This table shows which demand elasticity, supply elasticity and deadweigt loss correspond to the parameters, pass-through rate ρ and the effect of the sales tax on total apparel expenditures β_{total} estimated in this paper. Preferred values for these parameters come from Column (1) of Table 3.6 and Column (5) of Table 1.7 respectively; the alternative value for the pass-through rate comes from Column (1) of Table 2.8. "Upper Bound" represents upper bound of the 95% confidence interval. I use the data from the Annual Retail Trade Survey to compute the share of online expenditures, α, averaged over the sample years. The effect of the sales tax rate on online expenditures, β_o, comes from Column (1) of Table 6 in Einav et al. (2014).

1.9 Conclusion

In this paper, I estimate the effect of the sales tax on retail employment and prices. Using the CPI micro data and large apparel-specific tax rate changes, I find zero pass-through rate of the sales tax on pre-tax prices, implying that consumers fully bear the burden of the sales tax. Such a pass-through rate is common for a few other goods, but surprising for apparel with its highly elastic demand.

My result implies that apparel retailers have an even more elastic supply than the highly elastic demand. Thus, a sales tax increase should substantially decrease some equilibrium output, and, hence, inputs. I show that the labor expenditures of retailers decrease by 0.6% in response to a 1 percentage point increase in the sales tax, whereas the number of employees decreases by 0.4%. The magnitude of this effect in absolute numbers can be state as follows: a New York state sales tax exemption on clothing of 4% generates more than 2,150 new jobs, which is close to the average county employment in the apparel retail sector.

The substantial drop in the equilibrium output should lead to a large deadweight loss of apparel taxation. Indeed, using the Consumer Expenditure Survey data, I find that a 5% sales tax rate generates a 17¢ average deadweight loss for every tax dollar collected. These results are of particular interest to policymakers, given the lack of consensus among states over how (or even whether) apparel, with annual sales totaling $245 billion, should be taxed. The implications from this paper are twofold. Given the substantial deadweight losses of apparel taxation, the recent trend of tax avoidance through the Internet purchases may be welfare improving. Second, tax exemption alterations, which themselves require numerous legislative hours, also affect the labor market significantly, and are, thus, too costly to happen frequently.

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1.10 Appendix A: The Pass-Through Rate Under Imperfect Competition

Using the framework of Weyl and Fabinger (2013), I derive the pass-through rate of an *ad valorem* tax under the general case of imperfect competition between single-product firms.[42] This framework has the advantage of nesting all commonly used models of complete information with imperfect competition. Originally, the authors employ it to obtain the general formula for the pass-through rate and tax incidence of excise taxes.

Under imperfect competition, when all the firms are identical, a profit-maximizing quantity q (or after-tax price p) is such that the price mark-up satisfies:

$$p(q) - \theta(q)ms(q) = mc(q)(1+\tau), \tag{1.14}$$

where θ is a market structure parameter, which is equal to 0 in case of perfect competition, $\frac{1}{n}$ in case of Cournot with n firms and 1 in case of monopoly.[43] $ms = -p'(q)q$ represents the marginal surplus for an individual firm, whereas mc – the marginal cost of the firm. Note that p is the price paid by consumers; thus, the sales tax rate τ multiplies the marginal cost. Taking logs of both sides of equation 1.14 and differentiating them by $\frac{d}{d\log(1+\tau)}$ results in the following:

$$\frac{\rho_c - \theta'(q)q'(p)\rho ms(q) - \theta ms'(q)q'(p)\rho}{1+\theta\frac{p'q}{p}} = \frac{mc'(q)}{mc}q'(p)p\rho + 1, \tag{1.15}$$

[42]For the analysis of tax incidence under competition between multiproduct firms, see Hamilton (2009)

[43]This is the conduct parameter introduced by Genesove and Mullin (1998) variation on Bresnahan (1989). However, in this case it can also depend on the output as in the differentiated products Nash-in-prices model.

where $\rho_c = \frac{d\log p}{d\log(1+\tau)}$ represents the pass-through rate of a sales tax on consumer prices, or, alternatively, the tax incidence on consumers. Solving for it leads to:

$$\rho_c = \frac{1 - \frac{\theta}{\epsilon_D}}{1 + \frac{\theta}{\epsilon_\theta} + \frac{\theta}{\epsilon_{ms}} + \frac{\epsilon_D - \theta}{\epsilon_S}}, \tag{1.16}$$

where $\epsilon_D = -\frac{d\log q(p)}{d\log p}$ is the absolute value of demand elasticity, $\epsilon_\theta = \frac{d\log q(\theta)}{d\log\theta}$ — the inverse elasticity of the market structure parameter, $\frac{1}{\epsilon_{ms}} = \frac{d\log ms(q)}{d\log q}$ — the inverse elasticity of marginal surplus, and, finally, $\frac{1}{\epsilon_S} = \frac{d\log mc(q)}{d\log q}$ — the inverse elasticity of supply of an individual firm. The formula for sales tax incidence on consumers differs from that for excise tax incidence derived in Weyl and Fabinger (2013) only by the presence of the second term in the numerator.[44]

Using expression (1.16), I can show that the pass-through rate of a sales tax on producer prices, $\rho = \rho_c - 1$, can be of either sign or exactly zero, depending on the values of ϵ_D, ϵ_{ms} and θ. First, under the assumption of perfect competition ($\theta = 0$), the pass-through is non-positive and its magnitude is less than or equal to one in absolute terms:

$$\rho = \frac{1}{1 + \frac{\epsilon_D}{\epsilon_S}} - 1 = -\frac{\epsilon_D}{\epsilon_S + \epsilon_D} \in [-1, 0]. \tag{1.17}$$

Second, the pass-through rate can be equal to zero. Consider the case of monopolistic competition, when consumers have iso-elastic demand, firms have constant marginal costs and the number of products is large (like in Fullerton and Metcalf (2002)). The firm entrance does not affect conduct parameter: thus, $\epsilon_\theta = \infty$. Iso-elastic demand and large number of products imply the equality of the demand elas-

[44]Given how I define ϵ_D, this term is always negative. This observation allows me to extend the result in Anderson et al. (2001) that a more-than-full pass of a sales tax on consumers necessarily implies a more-than-full pass of an excise tax, regardless of the cost curve form and under a broader set of symmetric imperfect competition models.

ticity and the inverse elasticity of marginal surplus: $\epsilon_D = -\epsilon_{ms} = \frac{1}{1-\gamma}$, where γ is a measure of substitutability. In addition, firms have constant marginal costs, which leads to $\epsilon_S = \infty$. Plugging all the values of the parameters in (1.16) yields zero pass-through rate:

$$\rho = \frac{1 - \frac{\theta}{\epsilon_D}}{1 + \frac{\theta}{\epsilon_{ms}}} - 1 = 0. \tag{1.18}$$

Finally, for a generalized CES-logit model with constant elasticity of demand $\alpha > 1$ and constant marginal costs, Anderson et al. (2001) show that the pass-through rate is positive. In this case,

$$-\frac{\epsilon_D}{\epsilon_\theta} - \frac{\epsilon_D}{\epsilon_{ms}} > 1. \tag{1.19}$$

These examples confirm that the pass-through rate can be of any sign and value.

1.11 Appendix B: Expenditure Data

To estimate the effect of the sales tax on consumer apparel expenditures, I use the interview component data from the Consumer Expenditure Survey (CE). I augment the CE data with self-constructed data on sales tax rates at state level, the finest geographical level at which the data is publicly available. I restrict my sample to the Northeast states where all the tax exemptions occur. My data set spans from January 1997 to December 2012. It is a rotating panel of households. I observe apparel expenditures of every households at most four times. In this section, I describe the features of the survey relevant to my research. Then, I explain how I aggregate the tax rate variable to a state level, the finest geographical area in the data. Finally, I provide summary statistics.

The respondents of Consumer Expenditure Survey report their apparel purchases every quarter for a one year period. The surveyor asks them to provide the following information about every item purchased in the three complete calendar months preceding the interview: month of purchase, tax-exclusive price, broad apparel group (men's shirt vs. women footwear) and whether the item is a gift. (Bradburn, 2010) shows that such data collection leads to the underreporting of expenditures: every month away from the interview on average makes consumers forget about 15% of their purchases. Given that sales tax may alter the reporting of expenditures, I consider the data only for the first month preceding the interview in my estimation. Before publishing the data, BLS makes two price adjustments. First, it computes after-tax prices, where the sales tax corresponds to the consumer's place of residence. Second, it imputes and changes some prices in accordance with its internal policy. In the second case, BLS explicitly indicates any changes. In my analysis I use the adjusted prices, though my results do not change after dropping them.

After observing the household purchasing behavior for one year, BLS employees replace it with another randomly selected one. To preserve continuity in the averages, 25% of households in the sample change every quarter, rather than all of them at a certain moment. Theoretically, given my sample restrictions, I should have 4 observations for each household. In practice, I have on average 3 observations due to non-response issues. The main feature and advantage of using the data from the CE interview component is the possibility of observing apparel purchases for a number of households before and after the change in sales tax rates. This makes it outstanding relative to other data sources.[45]

[45]For instance, the Diary component of the Consumer Expenditure Survey reports household expenditures only for two consecutive weeks. This period is too short to provide data about enough households before and after the policy changes

The main limitation of my data is the tabulation of household location to a state level which prevents me from using the local tax rate data. Fortunately for me, only one state (New York) in my sample allows localities to impose sales tax rates on clothing. I assume that the cumulative rate in this state is equal to that in New York City because Big Apple contains half of NYS population and BLS oversamples this area. Another assumption that I rely on is that the sales tax rate is equivalent to the one applied to goods priced below the exemption threshold. In most cases, the threshold value is quite high ($110 in NY and VT) and covers 80%-85% of price distribution, exceptions being $50 and $75 thresholds in CT from the beginning of the sample to 2011 and NY from April 2011 to April 2012. Excluding these cases from my data does not substantially alter my results.

In Table 1.15, I compare summary statistics for apparel expenditures and sales tax rates across five geographical areas. The first three areas correspond to the treatment states: Connecticut, New York and Vermont. The other two areas consist of the control states divided according to their population and geographical size: MN-NH-RI and MA-NJ-PA. Table 1.15 shows that a family in the Northeast on average spends monthly $92 on clothing and $16 on footwear. There are two areas that slightly deviate from this pattern; CT households spend more on both goods ($120), and the three New England Control States spent less on clothing ($66). This difference in behavior is negligible relative to the standard deviation, which is 2-3 times bigger than the mean expenditures. Households roughly shop for clothing once in two months and once in five months for footwear. The sales tax rates vary substantially within the treatment states: the standard deviation for the tax rate is bigger than the mean for all of them. Since the standard deviation for the tax rate within each control state is almost zero, the high value in Column (4) represents substantial across-state

Table 1.15: Summary Statistics For Households, Expenditure Regressions

	Treatment			Control		All
	CT	NY	VT	ME-NH-RI	MA-NJ-PA	States
Clothing Expenditures, $	120	104	105	66.1	101	92.2
	(287)	(300)	(242)	(150)	(267)	(235)
Any Expenditures	0.499	0.396	0.495	0.379	0.453	0.450
Footwear Expenditures, $	21.6	17.0	17.3	14.6	16.3	15.9
	(66.3)	(73.4)	(46.6)	(46.8)	(52.2)	(54.8)
Any Expenditures	0.226	0.156	0.223	0.173	0.187	0.183
Tax Rate, %	0.59	2.83	1.38	2.88	0.00	1.02
	(1.84)	(3.80)	(2.23)	(2.47)	(0.00)	(2.59)
No. of Obs.	5,622	24,170	1,249	2,242	45,541	236,185

Notes: Standard deviations are in parentheses. The data are from the Interview Component of the Consumer Expenditure Survey and covers states from the Northeast Census Region and months from January 1997 to December 2012. The statistics from Column (3) informs us that a household in New York State on average spends $104 per month, the probability of any expenditures on apparel being 40%. Columns (1-3) present information for each treatment state individually (Connecticut, New York and Vermont); Columns (4) and (5) are for the control states divided according to their geographical and population sizes (Maine, New Hampshire and Rhode Island vs. Massachusetts, New Jersey and Pennsylvania). Column (6) presents the information for the whole sample. The tax rate variable is in percentage points and represents the cumulative state and local tax rate that consumers face. Given that geographical location data is available only at state level, the New York State sales tax rate equals the New York City one.

variation (unlike other control states, Maine has non-zero tax).

1.12 Appendix C: Item Disappearance

In addition to item being permanently unavailable (cancellation), there are two other reasons for a price observation to be missing. First, a store may temporary exhaust its supply of the item, which, following Matsa (2011), I call a stockout. In Figure 1.4, there are two stockouts in April and November. Second, an item may be missing from the shelves because it is out of season. In Table 1.16, I present summary statistics for the different types of missing observations. I divide the sample into seasonal and nonseasonal goods because they are very different: average lifespan for the former is 1.5 years, whereas for the latter 3 years. In the raw CPI micro data, there are 59% of missing observation for seasonal goods and 20% for non-seasonal, the former being strikingly large. These numbers are due to the practices of CPI methodology to collect all plausible information about underlying processes of item disappearance.

The practice, generating the biggest portion of missing observations, is the collection of data for seasonal items when they are off-season. The BLS surveyors do it to make sure that the item is seasonally but not permanently unavailable.[46] In the latter case, they substitute the item with another one. The more imminent the new season, the better the representative is aware of whether the item will be present on the shelves. Indeed, dropping all the off-seasonal observations leads to a decrease in the fraction of missing observations down to "only" 26%, which is comparable with non-seasonal items.

The second practice is collecting the data for two different quote versions simultaneously. In Figure 1.4, this happens when "Version 2" replaces "Version 1" in April.

[46] Another reason for collecting this data is, perhaps, for a better assignment of seasonal frames for the current and future items. See the explanation of determining item seasonality in CPI data below

Table 1.16: Summary Statistics for Apparel Items, Missing Observations Regressions

	Nonseasonal Items		Seasonal Items	
	(1)	(2) Refined	(3)	(4) Refined
All Missing Observations, %	20.0	-	59.0	-
	(40.0)	-	(49.2)	-
No of Obs.	216,242	-	833,660	-
Stockouts, %	9.48	5.41	23.4	8.62
	(29.3)	(22.6)	(42.3)	(28.1)
No of Obs.	178,546	128,914	454,210	204,951
Cancellations, %	6.90	3.07	13.0	4.66
	(25.4)	(17.2)	(33.7)	(0.05)
No of Obs.	202,455	194,445	764,976	400,065

Notes: Data for missing observations is from January 1997 to December 2012. There are three main cases for the items in my data to be missing: temporarily missing (stockout), permanently missing (cancellation) and out of season. Columns (1-2) and Columns (3-4) show the summary statistics for nonseasonal and seasonal items respectively. In Columns (2,4), I show summary statistics for the samples after refinements similar to Matsa (2011). See the text for the details on the refinements. The first three rows of Columns (2,4) have no values because they the refinements are done separately for stockouts and cancellations.

Note that two observations for the same quote and month are present in the data, one being a missing observation. If the dependent variable is a cancellation, I drop the April observation for "Version 2". Alternatively, if it is a stockout, I drop the observation for "Version 1". This is an example of a more general pattern of cleaning the data. When the dependent variable is a stockout, I exclude from my sample all the cancellations. This leads to 9.5% of stockouts for nonseasonal goods and 23% for seasonal goods in my sample. Alternatively, when using cancellations as the outcome variable, I drop all the stockouts. This results in 6.9% of cancellations for nonseasonal goods and 13% for seasonal goods. Bigger numbers for seasonal goods reflect the fact that the seasonal frames in CPI data may not coincide with the actual ones.

In practice, the classification of items in stockouts and cancellation is also not ideal. As Bils (2004) emphasizes, misclassification usually occurs right when the item becomes seasonally or permanently unavailable. For instance, suppose an item sells out one month before the season ends. Ideally, if the season indeed ends the following month, it should be tabulated as a stockout. However, the store representative may think of this item as being out-of-season because the season for this item according to representative is over. Moreover, the manager may be uncertain about the item returning to the shelves next season at this moment. If it is not, then the item is actually a cancellation. Even for the non-seasonal goods, an uncertainty of whether the item comes back may lead to the mistabulation of stockouts and cancellations.

Based on the suggestions in Bils (2004) and Matsa (2011), I refine my data in the following way. For the stockout regressions, I drop all the observations in the three months preceding the permanent unavailability of an item. I do not apply this rule to the seasonal items because in the apparel market a lot of these items stay in the market for only one season. So, this refinement leads to a substantial drop in the sample size. In addition, for any item, I consider only the observations between the first and the last valid price observations. I drop those first and the last observations because they are always equal to 1. These refinements lead to a 5.4% of stockouts for nonseasonal items and 8.6% of seasonal items. For the cancellation regressions, I apply a single refinement criterion: a cancellation must appear right after a valid price quote. This results in 3% of cancellations for nonseasonal items and 4.7% for seasonal ones.

Chapter 2

How Do Retailers Respond to Sales Tax Holidays

2.1 Introduction

Sales tax holidays, a rather popular policy tool among legislators, affords consumers a brief respite—typically one to ten days—from the sales tax on certain items. Since 1997, when New York held a sales tax holiday on apparel for the first time in the modern era, 23 states and the District of Columbia have implemented it at least once. The set of the exempted goods and the duration of the sales tax holidays vary significantly across states and time. For instance, Florida exempted hurricane-preparedness items in June 2007 for 12 days; Connecticut exempted sales of home-weathering products for 4 months during the winter of 2005-2006, and South Carolina exempted handguns, rifles, and shotguns from taxes for 3 days in October 2008. Yet, the most popular sales tax holidays are those exempting apparel. These holidays often coincide with back-to-school period and can cover additional goods on occasion, such as basic school supplies, computers and books.

This paper investigates whether the sales tax holidays on apparel truly benefit consumers as intended. Given that the estimated losses in tax revenue from such a tax holiday is in the tens of millions of dollars, this question is of great importance to policy-makers. [1] Previous research shows that sales tax holidays increase consumers' daily expenditures on apparel by at least 25% (Agarwal et al., 2013) and by more than 100% on computers during sales tax holidays (Cole, 2009).[2] However, the short duration of sales tax holidays has made it difficult to study welfare questions because of the lack of available data at the daily level. I overcome this obstacle by using the

[1]The Ohio Legislative Service Commission estimates the cost of a three-day sales tax holiday in 2016 to be $13.5 million, while the Georgia State Department of Revenue calculates a two-day sales tax holiday in 2012 to cost roughly $41 million. (**??**)

[2]In addition, (Cole, 2009) finds that consumers face lower pre-tax prices on computers during the sales tax holidays.

confidential Consumer Price Index micro data provided by the Bureau of Labour and Statistics, which crucially records the date of data collection.

Specifically, I study two aspects of consumer welfare in this paper: pre-tax price and store quality, defined as the number of stockouts in a given store (Matsa, 2011). While the choice of the first outcome variable is straight-forward, I include the second one as well because sufficiently higher expenditures on apparel without change in prices imply that consumers could suffer from an increase in stockouts. A higher stockout rate could be considered a sign of poor store quality because the items customers wish to purchase are absent in the stores. Thus, even if the pre-tax prices do not increase, sales tax holidays may still provide less intended benefit to consumers by increasing non-monetary costs.

Economic theory is ambiguous about how retailers adjust their pre-tax prices in response to tax cuts (Anderson et al., 2001; Weyl and Fabinger, 2013). If the extent of competition between retailers stays the same, they could raise prices and enjoy part of the tax discount. If on the other hand, sales tax holidays triggers more competition, the pre-tax prices may go down, saving consumers more money. This ambiguity necessitates an empirical investigation. Regarding the store quality, there is no good economic theory that links the relationship between short-term sales tax cuts and stockouts. Yet, this is an interesting empirical question from the public policy perspective.

To answer both questions, I merge the Consumer Price Index micro data with self-constructed data on the sales tax holidays on apparel. Using difference-in-differences methodology and after controlling for the sales tax rate and time trends (including seasonality), I find that there is no effect of the sales tax holidays on pre-tax prices, suggesting that consumers fully enjoy the discount induced by the sales tax drop.

This result is robust to alternative specifications or considering various groups of apparel separately. It differs from (Cole, 2009)'s estimate, who finds that prices of certain categories of computers decrease during the sales tax holidays. On the other hand, this estimate is similar to those in Chapter 1, where I explore the effects of permanent changes in the sales tax rate on apparel prices. In addition, I find that there is a 1.7% increase in the number of items unavailable on the shelves during sales tax holidays. Though not robust, this effect points out that sales tax holidays may lead to higher non-monetary costs for shoppers because they are not able to find items on the shelves that they otherwise would.

The rest of the paper proceeds as follows. In Section 2.2, I explain how my paper adds to economic literature. In Sections 2.3 and 2.4, I thoroughly describe the policy changes and the data involved in my analysis respectively. Section 2.5 contains the explanation of my empirical strategy. In Section 2.6, I present my main results, followed by the robustness checks of this result. In Section 2.7, I show how sales tax holidays affect retail quality.

2.2 Literature Review

In this section, I explain how my paper adds to the Public Economics literature. I start by describing the brief literature on sales tax holidays. Then, I proceed with a discussion of the papers that study the relationship between short-term spikes in demand and prices. Finally, I provide an overview of the research on tax and subsidy incidence.

First and foremost, my main contribution is to the research that studies the effects of sales tax holidays in different markets. In his dissertation, (Cole, 2009) finds that there is more than a 100% increase in computer sales during sales tax holidays. He provides two explanations for this result. First, prices drop by around 20% for certain categories of goods during the holidays. Second, he shows that intertemporal substitution may account for 90% of the increase in expenditures. Meanwhile, (Agarwal et al., 2013) find that sales tax holidays increase daily expenditures on apparel by 25%, though they do not find evidence of intertemporal substitution. In fact, consumers seem to spend more on weeks before and after the sales tax holiday, a phenomena that (Agarwal et al., 2013) call the "momentum" effect. In this paper, I find that the effect of apparel sales tax holidays on prices is negligible, eliminating the possibility that a pre-tax price drop explains the spike in apparel purchases.

Second, this paper contributes to the literature that explores the effects of short-term spikes in demand on retail prices. In my case, sales tax holidays initiate the spike. Theoretically, this can result in either an increase or a decrease in pre-tax prices. The decrease is plausible if the spike triggers extensive competition between firms Anderson et al. (2001); Weyl and Fabinger (2013) or if the spike significantly increases the share of consumers with low-valuation for the good (**?**). Empirical papers rarely find evidence for this Doyle and Samphantharak (2008); **?**, except for the already mentioned (**?**). Yet, some of these papers find that prices may decrease in response to demand peaks (**??**), with one additional condition: the set of goods facing the peak should constitute a low share of the consumption bundle in a supermarket. (**?**) shows that, in this case, supermarkets follow the loss-leader strategy in setting prices. They commit to a decrease in prices for certain goods to attract customers to their store. Sales tax holidays generally exempt most apparel items; thus, this condition is unlikely to hold, and I find no decrease in pre-tax prices, which is different from (Cole, 2009)'s previous result.

Finally, I add to the growing research that empirically identify the factors that affect store or product quality. The researchers generally assume that these factors affect quality through competition in one or another way. (Matsa, 2011) argues that Walmart's entrance improves competition and find that the quality of supermarkets significantly increases. Several papers show that the decrease in transaction costs associated with the Internet penetration spurs competition between airlines **?** or book sellers **?**, thus increasing consumer satisfaction with both products. There are also papers that do not find robust link between competition increase and product quality. **?** provides a good survey of literature that explores vertical mergers and competition.

2.3 Policy Overview

This section provides a brief overview of the history of sales tax holidays on apparel for the two regions in my sample: the Northeast and Midwest. I point out which states I include in the treatment and control groups and show that treated states hold sales tax holidays in some years but not others. Thus, for estimating the effect of the sales tax holidays I can use difference-in-differences (DD) empirical strategy based on both the temporal and spatial variation.

Since 1997, when New York held sales tax holidays on apparel for the first time in the modern era, 23 states and District of Columbia implemented similar policies. The set of the exempted goods and the duration of the sales tax holidays vary significantly across states and time, but the most common sales tax holidays are those that exempt apparel solely or together with some other back-to-school items.

For studying the effects of such sales tax holidays on apparel prices and store quality, I consider only the Northeast and Midwest regions because of more frequent sampling (See Section 2.4 for a more detailed discussion on this issue). In these two regions, 7 states held sales tax holidays that exempted apparel: Connecticut, Illinois, Iowa, Massachusetts, Missouri, New York, Vermont. From this list, I exclude three states from the treatment group: Iowa, Massachusetts and Vermont. For Iowa, there are no price observations during sales tax holidays in the CPI data. In Massachusetts and Vermont, sales tax holidays are unlikely to affect the demand for apparel substantially because these two states additionally have a permanent tax exemption on apparel items priced below \$175 and \$110 respectively; thus, the sales tax holiday in these states only affects apparel items priced above this high threshold, arguably a small proportion of the sample.

This leaves me with 4 treatment states: Connecticut, Illinois, Missouri and New York. In Table 2.1, I describe the characteristics of the sales tax holidays in these states. New York implemented the highest number of sales tax holidays on apparel, 13, with an average duration of around 7 days during my sample period (1997-2012). Connecticut is a close second with 12 sales tax holidays and average duration equaling 7. Both states exempt solely apparel. The sales tax holidays are less popular in the other two states, Illinois and Missouri. Illinois tried the policy only once in 2010. Missouri has held sales tax holidays 9 times thus far, but all of them lasted for 3 days or less. Given that the data on prices in my sample is only for weekdays, I do not expect to find a substantial amount of price observations from either state.

Table 2.1: The Characteristics of the Sales Tax Holidays

State	Years	Number of holidays	Average Duration, days	Average Threshold , $	Other Goods
Connecticut	2001-2012	12	7.0	300	No
Illinois	2010	1	10.0	100	School Supplies
New York	1997-2000; 2003-2006	13	7.1	290	No
Missouri	2004-2012	9	3.0	100	Computers, School Supplies, Books

Notes: In this table, I present the characteristics of the sales tax holidays held in the four treated states in my sample. Column (1) provides the years when states implemented the policy. Column (2) shows the overall number of the sales tax holidays in each state. In the majority of the years, New York held them twice, whereas the rest of the states once a year. Column (3) presents the average durations of the sales tax holidays, whereas Column (4) – the average thresholds. The states exempted only goods priced below a certain threshold during the sales tax holidays. Before 2011, Connecticut had a permanent exemption on apparel priced below $75 (prior to 2003) and $50 (after 2003).

2.4 Data

Below, I explain the way I construct the data for this analysis. In addition, I provide the summary statistics of key variables for the three treated states with the highest number of observations during sales tax holidays: Connecticut, Illinois and New York. I compare the summary statistics between two time periods: during the sales tax holidays and for the whole sample period.[3] This is the same data I use for the analysis in Chapter 1 and is described in greater detail there.

Data for my sample comes from two data sources. I obtain data on prices and item characteristics from the Consumer Price Index micro data collected by the Bureau of Labor and Statistics. This data perfectly suits my analysis because it has information about the exact date of price collection, which allows me to precisely construct my main explanatory variable - a dummy for the sales tax holidays. I collect the data on the sales tax holidays myself by merging information from Cole (2009) (before 2005) and from CCH OmniTax Library accessible online through the University of Virginia School Law Library (after 2005).

I restrict my sample to include observations from the Northeast and Midwest Census Regions due to a few reasons related to the interaction between the CPI data collection and the short-term nature of the sales tax holidays. First, among the three Census Regions where sales tax holidays occur, these two have metropolitan areas (New York City and Chicago) where the BLS surveyors collect price data on a monthly basis (as a opposed to once in two months). This substantially increases the share of items in my sample that I observe at least twice. Note that my empirical strategy, which is difference-in-differences, ignores the other items. Second, the longer

[3]Given that the share of observations during the sales tax holidays is always less than 2%, one can easily substitute "for the whole sample" with "excluding sales tax holidays."

duration of the sales tax holidays in New York City relative to all other places and the oversampling of New York City allows me to obtain a substantial amount of observations for my analysis . Finally, the data on the sales tax rate, which I use as one of the controls, is sometimes inconsistent in the CPI data. For both the Northeast and Midwest Census region, I have checked and where necessary recollected the data on this variable (see Chapter 1 of this dissertation for more details).

There are several sample adjustments that I make. I consider only observations on prices from traditional retailers because quotes from catalog and online stores have a small representation (2-3%) in the BLS data.[4] I drop from my sample all the items that ever cost more than $1000 because their behavior may differ from the rest of the items.

Table 2.2 presents summary statistics for the main variables in the three treated states with the highest number of observations during sales tax holidays: Connecticut, Illinois and New York. I compare the averages and standard errors between time period with sales holidays (even columns) and the whole sample time period (odd columns). For New York (Columns (1-2)) and Connecticut (Columns (3-4)), it is clear that the prices are substantially lower during sales tax holidays. This is not surprising given that both states hold sales tax holidays in the end of spring (end of July) or fall seasons (end of January), and seasonal items on average loose 40% of their price throughout the season. In case of Illinois (Columns (5-6)), there is no such dependence. I attribute this to the fact that during the sales tax holiday in Illinois, the BLS surveyors happen to collect most of their data in Chicago, which is indicated by a disproportionally higher ratio of monthly quotes, and prices in Chicago are higher relative to the rest of the state.

[4]Data on the Internet stores enters the CPI micro data in 2003.

Table 2.2: Summary Statistics for Apparel Items, Price Regressions

	NY		CT		IL	
	All	STH	All	STH	All	STH
Price, $	86.4	71.2	88.8	68.8	67.2	84.5
	(156)	(119)	(174)	(128)	(112)	(141)
Tax Rate	0.048	0.00	0.021	0.00	0.075	0.00
	(0.035)	(0.00)	(0.029)	(0.00)	(0.022)	(0.00)
Item is on Sale, %	35.2	31.3	44.1	57.0	38.1	32.1
	(47.7)	(46.4)	(49.7)	(49.6)	(48.6)	(46.9)
Sales Tax Holiday, %	1.16	100	1.96	100	0.150	100
	(10.72)	(0.00)	(13.87)	(0.00)	(3.86)	(0.00)
Monthly Quotes	0.828	0.920	0.556	0.292	0.756	0.929
	(0.377)	(0.272)	(0.497)	(0.455)	(0.429)	(0.259)
Nonseasonal Goods	0.307	0.311	0.267	0.321	0.309	0.232
	(0.461)	(0.463)	(0.442)	(0.468)	(0.462)	(0.424)
Fall Seasonal Goods	0.132	0.123	0.095	0.082	0.111	0.098
	(0.339)	(0.329)	(0.294)	(0.275)	(0.314)	(0.299)
Spring Seasonal Goods	0.131	0.064	0.099	0.089	0.099	0.107
	(0.337)	(0.246)	(0.299)	(0.285)	(0.299)	(0.311)
Men's Clothing	0.313	0.356	0.320	0.344	0.283	0.286
	(0.464)	(0.479)	(0.466)	(0.476)	(0.450)	(0.454)
Women's Clothing	0.315	0.281	0.321	0.269	0.329	0.366
	(0.465)	(0.450)	(0.467)	(0.444)	(0.470)	(0.484)
Footwear	0.185	0.162	0.149	0.230	0.196	0.170
	(0.389)	(0.369)	(0.356)	(0.421)	(0.397)	(0.377)
No. of Obs.	94,793	1,056	16,411	305	76,866	112

Notes: The data comes from the Consumer Price Index micro data with one exception. I fix the inaccurate reporting of the sales tax rate after exemption in the CPI data by self-collecting data on exemptions. I include in the table only data for the three treated states with the highest number of observations during sales tax holidays: New York, Connecticut and Illinois. Odd columns have summary statistics for all the observations, even— only for the observations during sales tax holidays. The data spans January 1997 to December 2012. Price is exclusive of sales tax. Sales tax holiday equals to one if on the day of price collection, a state holds sales tax holidays on apparel. All the variables, except for price and tax rate are dummies.

The other variables in Table 2.2 seem to behave in the same way during the sales tax holidays and for the whole sample period with several exceptions. First, in Connecticut the retailers seem to hold more sales during sales tax holidays (57% vs. 44% for the whole sample period). Second, in New York the share of spring seasonal goods is substantially smaller (6.4% vs. 13.1%). Finally, there is a considerable increase in the share of footwear sold in Connecticut (23.0% vs. 14.9%).

2.5 Empirical Strategy

To estimate the effect of the sales tax holidays on pre-tax prices, I use a standard difference-in-differences methodology. The methodology for estimating the effect on retail quality is the same, the only difference being that I change the dependent variable. In case of price regressions, it is $Log(1+price_{im})$, and in case of quality it is a dummy which equals to 100 if the item is missing (or temporary unavailable). i and m denote clothing item and month respectively.[5] An item in my sample is a piece of clothing or footwear sold at a particular store: for instance, men's grey 'London Fog" golf jacket sold in the Belk store located on Seventh Avenue in Annapolis, Maryland. I regress the logarithm of price on the sales tax holidays dummy STH_{im} which equals one if the state implements this policy at the date of price collection:

$$Log(1 + price_{im}) = \alpha + \beta_1 \times STH_{im} + Controls_{im} + \nu_i + \mu_m + \epsilon_{im}. \tag{2.1}$$

I omit location fixed effects because item fixed effects controls for this particular

[5]I use log-tranformation because the sales tax holidays affect relative rather than absolute prices in the same way across stores

characteristic, as well as for other constant characteristics: color, size, composition, etc.[6] There is, however, one potential limitation that the inclusion of item fixed effects imposes on my estimates. I have to observe prices for a given item at least twice, otherwise my empirical strategy disregards the item from the analysis. Despite substantial seasonality in the apparel data, the majority of items met this condition.

I use monthly fixed effects to control for any common shocks that simultaneously affect both the Northeast and Midwest apparel markets. An example of such a shock could be very hot weather during a particular season or any federal government policy changes which affect apparel prices. In certain cases, I also include state-month-year fixed effects to account for state-specific shocks in prices, making my estimation strategy essentially triple differences. In this case, the within-state control group consists of items for which the BLS surveyors collect price quotes on the same month (but not on the same days) of the sales tax holidays. Of course, the condition for the unbiased estimates is that the prices do not systematically differ across different weeks of the month.

There are two main control variables that I use in my analysis. First, I control for the sales tax rate within localities. This is of particular importance because states and localities that hold sales tax holidays tend to have higher sales tax rates in general. Thus, if there is an interdependence between the sales tax rate and price level, one can easily come up with a story of how this could bias my estimates. I take the data on the sales tax rates from the CPI data. It has an inconsistency - in certain cases

[6] An alternative approach would be to substitute item FE with quote FE. A quote consists of consecutive items, which are similar but not necessarily the same. To account for the differences between two similar items belonging to the same quote, BLS analysts provide a price adjustment measure, a value that explains all non-inflationary discrepancies in the prices of the two items. The BLS analysts often estimate the adjustment using hedonic regression models and personal judgment. This methodology is not consistent over years and across analysts, which may lead to either imprecise or biased estimates.

it does not take into account the presence of the permanent exemptions that apply for clothing and footwear items priced below certain thresholds. These thresholds create some issues for identification, which I solve by using instrumental variables (see Chapter 1 of this dissertation for further discussion on this point).

The second control variable(s) accounts for substantial changes in prices throughout a seasonal item's lifetime. A seasonal apparel item on average loses 40% of its price throughout the season. My data allows me to see the first and last months the item is supposedly in season.[7] I find that the number of months until the end of the season captures sizable price changes. I use a set of eleven dummies to control for seasonal changes:

$$Seasonality_{im} = \sum_{0}^{10} \psi_{season\ end(m)-m} \mathbb{1}(in\ season(m)) + \psi_{11} \mathbb{1}(nonseasonal\ good), \tag{2.2}$$

and I add another dummy for non-seasonal goods.

My DD empirical strategy provides unbiased estimates under the assumption that there are no systematic changes in price behavior throughout a season between treatment and control states. To show that this assumption is likely to hold in my case, I show in the "Results" section that my results are robust to a number of alternative specifications. Particularly, the one where I use state-month-year fixed effects.

[7]The BLS office in Washington decides the months when a certain item is on/off season. This measure has a flaw in that it is not geographically specific. However, given that the weather conditions are presumably the same in the states I consider, it should not affect my estimates.

2.6 Results

In this section, I present the main regression results from my empirical analysis. First, I show that there is no change in apparel retail pre-tax prices in response to the sales tax holidays, implying that the consumers receive all the intended discount in the after-tax price. Second, I find that in my main specification there is 1.7% increase in the probability of the item being unavailable in a store. Yet, this result is not robust. Thus, I conclude that consumers fully benefit from the sales tax holidays – the after-tax prices decrease by the full amount of the sales tax without a decrease in store quality.

2.6.1 Sales Tax Holidays and Prices

Table 3.6 shows the results of my difference-in-differences estimation of the sales tax holidays' effect on apparel pre-tax prices, controlling for sales tax rates and seasonality. I use price data for the Northeast and Midwest Regions from 1997 to 2012. There are four treatment states in my sample: New York, Connecticut, Illinois and Missouri.[8] Given that I use both state and month fixed effects, the main identification assumption for my coefficient of interest is that in the absence of the sales tax holidays there is no systematic difference in the behavior of the pre-tax prices in the treatment and control states during the months when sales tax holidays generally occur.

In Column (1), I present the estimates for my main specification. I include all items priced below $1,000 and consider both Northeast and Midwest states. The

[8]Massachusetts also holds sales tax holidays on all items during this time period. However, Massachusetts has an permanent exemption which makes all items below $175 tax free and, thus, has a vary small share of treated items.

coefficient equals -0.27, implying that firms actually decrease the prices by a quarter of a percent in response to the short-term drop in the sales tax rates. Thus, in addition to the tax discount consumers also enjoy slightly lower pre-tax prices. However, this result is not statistically, and arguably economically, significant, and one cannot reject the hypothesis that the consumers bear the full incidence of the tax.

In Column (2), I show that the coefficient becomes larger when I exclude the Midwest states; in this case, the main control states for New York and Connecticut become Massachusetts, New Jersey and Pennsylvania.[9]. The coefficient implies that retail stores decrease pre-tax prices by 0.9% during sales tax holidays. Yet, the coefficient is not significant.

The results in both columns are not statistically different from those obtained using permanent changes in the sales tax rate as the source of variation instead, confirming the validity of my estimates. For the ease of comparison, I compute the pass-through rate based on the estimates in Columns (1) and (2). It is positive in sign and equals $0.2/0.05 = 4\%$ and $0.9/0.05 = 18\%$ respectively, assuming that sales tax holidays on average decrease the tax rate by 5%.

In Column (3), I show that my results stay the same when excluding Connecticut from the sample. I do so because this state, through most of its history, does not tax apparel items priced below either \$50 or \$75. Thus, the effect of the sales tax holidays in this state may differ from those in the other treatment states. The coefficient switches sign but stays statistically insignificant from zero. Thus, we can conclude that exclusion of Connecticut does not affect my result.

In Columns (4-5), I confirm the robustness of the estimated effect by considering Pre- and Post-Great Recession observations separately. The coefficients may differ

[9]The number of observations for Vermont in the Consumer Price Index data is small

Table 2.3: Panel Data Estimates of the Effect of Sales Tax on Apparel Prices

	Dependent Variable: Log of Pre-tax Price				
	(1)	(2)	(3)	(4)	(5)
VARIABLES	Whole Sample	W./o Midwest States	W./o CT	Pre-Crisis	Post-Crisis
Sales Tax Holiday	−0.27	−0.89	0.67	-0.25	-0.18
	(0.904)	(0.829)	(0.529)	(0.659)	(2.30)
Item and month FE included in all specifications					
No. of Obs.	508,788	267,042	491,632	379,801	127,408
R^2	0.056	0.054	0.056	0.058	0.054
No. of Items	61,331	31,127	59,471	44,022	19,208

*** p<0.01, ** p<0.05, * p<0.1

Notes: Robust standard errors clustered at the state level are in parentheses. Apparel price quotes, tax rate and control variables come from the Consumer Price Index micro data; tax holiday data is self-collected from official State government websites. The sample covers January 1997 to December 2012 and all states from the Northeast and Midwest Census Regions. "Sales Tax Holiday" dummy equals one if a store's state holds a sales tax holiday on the price collection date. The table presents the coefficient on this dummy multiplied by 100. Each regression includes item and month-year fixed effects and controls for sales tax rate and seasonality. In all regressions, I exclude items ever priced above $1,000.

because consumers may become more price savvy after the Great Recession. As the results show, I do not find any evidence that this is the case. In fact, the coefficients in both columns are very close to that in Column (1). The standard error in Column (5) is so high because only three states hold sales tax holidays on non-weekend days during this period, and BLS does not collect price quotes over the weekend. In the next subsection, I perform the same regressions for my other outcome variable: retail store quality.

2.6.2 Sales Tax Holidays and Retail Store Quality

In this subsection, I test how and whether sales tax holidays affect the retail store quality. I define retail store quality in two ways. First, following the original definition suggested by (Matsa, 2011), I consider the probability of encountering items that are temporary unavailable on the shelves, or stockouts. Using this definition, I do not find any effect of the sales tax holidays on quality. Second, I define quality as the probability of encountering items unavailable on the shelves regardless of the reason. I base this definition on the fact that sales tax holidays usually last for a short period of time and, theoretically are unlikely to influence the probability of encountering items permanently or seasonally unavailable on the shelves. Thus, I can attribute any empirical change in item unavailability due to these two reasons to actually represent stockouts. When using this measure, I find that the probability of stockouts increases by 1.7% during sales tax holidays.

Exploring the effect of sales tax holidays on the rate of missing observations has another benefit: I can check whether my price results are biased. Consider the case when sales tax holidays affect the rate of missing observations for cheaper items, perhaps due to an increase in shopping by low-income households during this policy; this would bias the estimates of the effect on prices upwards. Thus, it is worth checking the effect on missing observations even if one is skeptical of using this as a measure of store quality.

For my estimation, I use the same specification as for prices, the main difference being the dependent variable. It is now a dummy that is nonzero when the observations are missing, out-of-stock or canceled, depending on the regression. To avoid too many zeros at the beginning of the coefficient value, I multiply this dummy by 100. For cancellations, I also use fixed effects at the quote rather than item level; otherwise the number of observations drop substantially.[10] This change, however, does not affect my results. Recall that I use two controls: sales tax rate and seasonality dummies. When price is missing, the sales tax rate is also missing in the BLS data. In this case, I use the last observed tax rate for this item as the actual tax rate.

In Table 2.4, I present the results for non-seasonal goods. Column (1) shows that the sales tax holiday does not influence the probability of encountering a missing observation in the unrefined sample. The coefficient is small and not statistically different from zero. Same results hold in Columns (2) and (3) when considering stockouts and cancellations, i.e. permanently unavailable items, separately. I also explore how the estimates change after applying the sample refinements discussed in Appendix 1.12 of Chapter 1 for stockouts and cancellations in Columns (4-5). Again both coefficients are not significantly different from zero. This allows me to conclude that sales tax holidays do not affect the rate of missing observations for nonseasonal items.

In Table 2.5, I repeat the regressions in Table 2.4 but now for seasonal items. In

[10] A quote consists of several consecutive items.

Table 2.4: The Effect of Sales Tax Rate on Stockouts and Cancellations, Nonseasonal Goods

				With Data Refinements	
Dependent Variable	All Missing	Stockouts	Cancellations	Stockouts	Cancellations
	(1)	(2)	(3)	(4)	(5)
Sales Tax Holiday	0.233	−0.271	−0.118	−0.553	−0.0178
	(1.446)	(1.141)	(1.030)	(1.125)	(0.729)
Item and month FE included in all specifications					
No. of Obs.	215,197	175,821	202,284	126,906	194,277
R^2	0.069	0.020	0.003	0.003	0.002
No. of Items	23,155	18,765		13,437	
No. of Quotes			8,560		8,550

*** p<0.01, ** p<0.05, * p<0.1

Notes: Robust standard errors clustered at state level in parentheses. Dependent variables are dummies that equal 100 if an item is missing, temporarily out of stock (stockout) or permanently out of stock (cancellation). Column (1) includes data for both stockouts and cancellations. Columns (2,4) contain only observations with stockouts, whereas Columns (3,5) — with cancellations. The number of observations in Columns (4-5) is smaller because I apply the Matsa (2011) refinement for dependent variables: in case of stockouts, I drop all the observations three months prior to their cancellations. In addition, I consider only observations in between first and last valid price observation. I only keep cancellations that occur after valid price observations in the sample. Each regression includes item and month-year fixed effects and controls for seasonality and sales tax rate. In all regressions, I exclude items ever priced above $1,000.

Table 2.5: The Effect of Sales Tax Rate on Stockouts and Cancellations, Seasonal Goods

	(1)	(2)	(3)	(4)	(5)
				With Data Refinements	
	Missing	Stockouts	Cancel.	Stockouts	Cancel.
Sales Tax Holiday	1.69**	1.75*	−1.09*	0.90	−0.51
	(0.742)	(0.987)	(0.626)	(1.065)	(0.475)
Item and month FE included in all specifications					
No. of Obs.	828,846	434,807	764,751	191,412	399,059
R^2	0.253	0.108	0.084	0.011	0.004
No. of Items	99,111	78,448		28,952	
No. of Quotes			30,024		28,814

‡ p<0.01, ** p<0.05, * p<0.1

Notes: Robust standard errors clustered at state level are in parentheses. Dependent variables are dummies that equal 100 if an item is missing, temporarily out of stock (stockout) or permanently out of stock (cancellation). Column (1) includes data for both stockouts and cancellations. Columns (2,4) contain only observations with stockouts, whereas Columns (3,5) — with cancellations. The number of observations in Columns (4-5) is smaller because I apply the Matsa (2011) refinement for dependent variables. For both stockouts and cancellations, I drop all out-of-season observations. In addition, for stockouts I consider only observations in between first and last valid price observation. I only keep in the sample cancellations that occur after valid price observations. Each regression includes item and month-year fixed effects and controls for seasonality and sales tax rate. In all regressions, I exclude items ever priced above $1,000.

Column (1), I find that the sales tax holidays increase the rate of all the types of missing observations by 1.7%. Though statistically significant, this effect seems to be economically small — roughly 3.4% of the average rate of missing observations, which is 50% for the whole sample. In Columns (2) and (3), I consider stockouts and cancellations separately. In both cases, the coefficient is marginally significant. In case of stockouts, it is almost the same as in Column (1), whereas in case of cancellations, the coefficient flips sign. I attribute this result to the fact that the BLS surveyors sometimes make errors in assigning the reason for an item's unavailability. Indeed, after the refinement I find no significant effect of the sales tax rate on either stockouts or cancellations in Columns (4) and (5). The point estimate for both effects essentially equals zero in both Columns (4) and (5). As in the case of nonseasonal items, I conclude that (a) missing observations do not influence my sales tax holidays results and (b) the quality of apparel retailers stays the same during this policy, increasing consumer benefits even more.

2.7 Robustness Checks

In this section, I confirm the robustness of my zero pass-through rate result in a variety of ways. Along the way, I show that the tax incidence differs for various subsamples. I find that the tax rate does not pass through fully onto consumers for non-seasonal goods (tax incidence is 79%), girls apparel (62%) and footwear (76%).

The classic endogeneity concern of any tax policy is that it may be a response to local economic conditions. Indeed, the decision about whether or not to hold a sales tax holiday usually takes place in July, right after the budget year is over. Generally, when there are budget shortfalls, legislators may prefer to abandon the holidays: "A

Table 2.6: Robustness of Tax Incidence Estimates To Time Controls

Dependent Var.	Log of Apparel Pre-tax Price			Item on Sale	
	(1)	(2)	(3) W/o. CT	(4)	(5) Men and Women
Sales Tax Holiday	−0.27	−0.78	0.17	−1.63*	−0.80
	(0.914)	(0.899)	(0.539)	(0.844)	(0.882)
Month FE	X			X	X
Controls for State Trends:					
4th order polynomial	X				
State-Month FE		X	X		
Item fixed effect included in all specifications					
No. of Obs.	508,788	508,788	491,632	508,788	287,929
R^2	0.057	0.057	0.057	0.035	0.045
No. of Items	61,331	61,331	59,471	61,331	39,083

*** p<0.01, ** p<0.05, * p<0.1

Notes: Robust standard errors clustered at the state level in parentheses. Column (1) adds a 4^{th} order time polynomial for treatment states. In Columns (2-3), I use New York and Connecticut state dummies interacted with month-year fixed effects. Column (3) excludes observations from Connecticut. In Columns (4-5), I explore how sales tax holidays affect the probability of an item being on sale; "Item on Sale" equals to 100 if the item is on sale. "Sales Tax Holiday" dummy equals one if a store's state holds a sales tax holiday on the price collection date. In the table, I present the coefficient on this dummy multiplied by 100. Each regression includes item fixed effects and controls for seasonality and sales tax rates. In all regressions, I exclude items ever priced above $1,000.

number of states have tried sales tax holidays and then canceled them, a trend that has accelerated during the current recession and related state government revenue downturn".[11] Thus, if better state economic conditions lead to both higher prices and likelihood of sales tax holidays, the coefficient of interest will have an upward bias. To address this issue, I check whether my results are robust to including various state specific time controls.

In Column (1) of Table 2.6, I include an interaction between state dummies and a fourth-order time polynomial. In Column (2), I use a more deliberate control for the trend: an interaction between New York and Connecticut state dummies and month-year dummies. Column (3) repeats Column (2) but excludes observations from Connecticut. The coefficients on the sales tax holidays dummy in all these columns remain mostly the same compared to the cases when I do not control for trends, allowing me to conclude that sales tax holidays do not affect pre-tax prices.

In the last two columns of Table 2.6, I explore how sales tax holidays affect the probability of items being on sale. Interestingly, for the whole sample (Column (4)), I find that the coefficient before sales tax holidays is negative and marginally significant, implying that sales tax holidays actually decrease the probability of items being on sale by 1.6%. A potential explanation for this result could be that during sales tax holidays retailers need less triggers to induce consumers to make purchases when they visit a store. In Column (5), I consider only items from two main apparel groups: men and women apparel. The point estimate of the coefficient decreases by a half and becomes insignificant. As such, I conclude that the effect of sales tax holidays on probability of observing a sale is negligible.

[11] "Sales Tax Holidays: Politically Expedient but Poor Tax Policy 2015" by S. Drenkard, J. Henchman, August 17, 2015

Table 2.7: Tax Incidence for Different Apparel Groups

	Dependent Variable: Log of Apparel Pre-tax Price					
	(1)	(2)	(3)	(4)	(5)	(6)
	Men	Boys	Women	Girls	Shoes	Babies
Sales Tax Holiday	−0.729	0.192	−0.362	2.50	1.56	1.52
	(1.92)	(1.14)	(0.551)	(2.21)	(1.26)	(1.25)
Item and month FE included in all specifications						
No. of Obs.	141,911	25,868	146,016	34,733	90,166	31,904
R^2	0.036	0.060	0.121	0.092	0.029	0.078
No. of Items	11,780	2,453	27,306	5,066	8,482	3,446

*** p<0.01, ** p<0.05, * p<0.1

Notes: Robust standard errors clustered at the state level in parentheses. Columns (1-6) restrict the sample to apparel items of certain type: Men, Boys, Women, Girls, Footwear, Babies. "Sales Tax Holiday" dummy equals one if a store's state holds a sales tax holiday on the price collection date. In the table, I present the coefficient on this dummy multiplied by 100. Each regression includes item and month-year fixed effects and controls for seasonality and sales tax rate. In all regressions, I exclude items ever priced above $1,000.

Given that sales differ across various categories of goods, it is worth exploring in more detail the effect on prices in different categories. In Table 2.7, I explore whether the effect of the sales tax holidays on apparel prices differ across apparel groups. Following the BLS classification, I consider six apparel groups: Men, Boys, Women, Girls, Footwear and Babies.[12] The point estimates for Girls, Footwear and Babies (Columns (4-6)) are substantially different from the full sample results. In all these cases, the coefficient is positive and relatively large in magnitude.[13] Yet, none of them is statistically significant, which one can attribute to the high standard errors due to lack of observations in each of these groups. For Men and Women categories, the coefficient is negative and similar in magnitude to that in the main specification.

In Table 2.8, I break the sample into seasonal and non-seasonal goods, the latter consisting mainly of cheaper items like underwear and t-shirts. The main goal of holding sales tax holidays is to decrease expenditures of households during back-to-school shopping. Seasonal apparel items prevail in the consumption bundle of households. Thus, the effect of the sales tax holidays on the prices of these items is of particular interest. Columns (1) and (2) show that the coefficients are almost identical to that in the main specification. In Column (3), I find that for Fall and Spring items (with each season lasting 6 months), the retailers actually increase prices during sales tax holidays. In Columns (4) and (5), I show that it is actually the Spring items that are responsible for this outcome.

[12] These are mutually exclusive goods. Footwear is a separate category

[13] This goes in line with the decrease in sales found in Table 2.6

Table 2.8: Tax Incidence for Seasonal and Non-seasonal Items

	Dependent Variable: Log of Apparel Pre-tax Price				
	(1)	(2)	(3)	(4)	(5)
	Non-Seasonal	Seasonal	Fall Spring	Spring	Fall
Sales Tax Holiday	−0.302	−0.254	0.414	2.17	−0.707
	(0.891)	(1.10)	(1.10)	(2.44)	(1.52)
Item andmonth FE included in all specifications					
No. of Obs.	184,579	324,193	101,954	49,894	52,059
R^2	0.010	0.075	0.091	0.087	0.102
No. of Items	13,962	47,410	15,466	7,651	7,821

‡ p<0.01, ** p<0.05, * p<0.1

Notes: Robust standard errors clustered at the state level are in parentheses. The "Sales Tax Holiday" dummy equals one if a store's state holds a sales tax holiday on the price collection date. In the table, I present the coefficient on this dummy multiplied by 100. In Column (1), I restrict the sample to only non-seasonal items, whereas in Column (2) - seasonal. In Columns (3-5), I consider Fall and Spring goods. Fall season starts in August and ends in January, whereas Spring one starts in February and continues until July. Each regression includes item and month-year fixed effects and controls for seasonality and sales tax rate. In all regressions, I exclude items ever priced above $1,000.

2.8 Conclusion

In this paper, I estimate the effect of the sales tax holidays on retail prices and quality. Using the CPI micro data and self-constructed data on sales tax holidays in the Northeast and Midwest Census Regions, I find no effect of the sales tax holidays on pre-tax prices, implying that consumers fully enjoy the benefits of this policy.

In addition, I do not find robust evidence for the lowering of store quality during sales tax holidays, where store quality is defined as the rate of missing observations. This result implies that stores are able to supply enough items on the shelves to address the increase in consumer expenditures found by (Agrawal, 2014).

Both my estimates of the effect on prices and quality are consistent with the results presented in Chapter 1, where instead of short-term changes I use long-term changes in the sales tax rate for identification. They also imply that sales tax holiday fulfill their main policy goal: allowing households to shop at lower prices.

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Chapter 3

Tax Notches in the Food Market

3.1 Introduction

The average person today spends an astonishing 41.5 minutes a day *thinking* about food, whether it be what to cook for dinner or where to eat out for lunch.[1] This single-minded preoccupation with food consumption, while not new in human history, has certainly evolved over time, from viewing food as a mere means of survival to food as a form of expression/source of joy. This dichotomous nature of food consumption poses an interesting dilemma to policy-makers, who might find it morally repugnant to tax food that continues to be a necessity, but simultaneously wasteful to not tax the luxurious food culture that supports a \$ 783 million restaurant industry.[2]

Indeed, a number of states and localities apply different sales tax rates for restaurant/fast-food meals and groceries. This non-linearity in food taxation comes from two sources. First, the majority of the states exempt groceries from the sales tax altogether (33 states) or tax them at a lower rate (6 states), while levying the full state sales tax rate on restaurant meals.[3] Second, localities in addition tack on a further "luxury" tax on meals in addition to the state sales tax; out of the 50 most populated cities in the US, 15 levy such a tax. As a result, the tax differential, i.e. the percentage point difference between the effective grocery sales tax and the restaurant food service tax, is pretty high, with Seattle, WA and Washington, DC leading the list with 10 percentage points. On the other hand, there are also jurisdictions like Tulsa, OK and Wichita, KS with a 0 percentage point tax differential, i.e. the grocery sales tax rate and the restaurant food service tax rate are the same. The absence of consensus among legislators on how to tax food consumption makes it interesting to investigate

[1] http://www.dailymail.co.uk/femail/article-1189218/Food-thought-Women-spend-TWO-YEARS-lives-thinking-meal.html.

[2] http://www.restaurant.org/News-Research/Research/Facts-at-a-Glance.

[3] Federation of Tax Administrators, January 2015.

the economic consequences of this policy. This paper studies how the tax differential between the restaurant food service tax rate and grocery sales tax rate affects the food consumption of households and whether the effect varies among households with different levels of income.

Using Gillitzer et al. (2015) and Slemrod (2013) framework, I consider the higher tax on restaurant meals as a tax on a particular characteristic of the food: service to prepare and, more importantly, serve the meal to consumers. In fact, this is the actual condition for restaurant meal taxation in most states— if the place serves the food to consumers, it has to charge the food service tax. Such a specific condition for taxation increases the potential for close substitutes that escape taxation to capture a part of the market. For example, Whole Food's "food bars" allow consumers to select certain cooked items that the grocery store prepares on site and box it up to eat at home; since the food was not served to the consumer in the strictest sense (the consumer serves himself), he will not pay the food service tax and only the grocery tax. [4]. Indeed, the substitutes in a grocery store for a fast-food or restaurant meal are very different. For the former it could be a frozen pizza or other ready-to-serve meal; for the latter it could be marinated meat or special French cheese. This makes it an empirically interesting exercise to evaluate which goods households substitute into.

In this paper, I perform the analysis for the overall sample and separately for high- and low-income subsamples. For the whole sample, I find that in response to a 1 percentage point increase in the tax differential between the restaurant food service tax and grocery sales tax, households consume 2% more meat products, leaving their

[4] Actually, this depends on the particular state and its specific definition; some states try to close these loopholes with various rules

overall food expenditures at grocery stores fixed. The results for high-income households are similar to the whole sample, whereas the response of low-income households is substantially different. They increase their overall expenditure on food by 2%, particularly on ready-to-serve dry meals and other dry-meals. These result suggests that the response to the tax differential is heterogenous with respect to household socio-demographic characteristics.

For estimating the effects, I employ a difference-in-differences empirical strategy. For variation in the tax differential, I use the introduction of the sales tax exemption on groceries in 4 states (Arkansas, New Mexico, Utah, Wyoming) between 2004 and 2007. The states bordering these four treated states serve as the control group. To mitigate any concerns related to cross-border shopping, I consider only the households living in counties located at least 50 miles away from the state borders. The main identification assumption for the coefficient of interest is that the grocery exemption policy is exogenous to the household decisions of where to shop. This assumption is plausible, provided that states introduce the sales tax exemption on groceries to save households money and to make the tax system less regressive.

For my analysis, I use the household component of the Nielsen Scanner Data accessible through the University of Virginia subscription. This data provides detailed records of household purchases of groceries throughout the year, including date, price and quantity of every item purchased. This allows me to compute the overall expenditures of households on certain food categories. In addition, the Nielsen Scanner data contains information about the socio-demographic characteristics of each household, which allows me to test for heterogeneous effects between low- and high-income households.

The rest of the paper proceeds as follows. In Section 3.2, I explain to which strands of literature my paper contributes to. Section 3.3 details how I construct the data set for the analysis and provides a brief overview of the policy in treatment states. I lay out my empirical strategy and identifying assumptions in Section 3.4. Section 3.5 presents the results of my analysis.

3.2 Literature Review

This paper contributes to three different strands of public economics literature. First, my paper adds to a newly-developing literature that empirically studies how notches in the tax schedule (or as I call it, tax differentials) divert the production towards goods that get a preferential tax treatment. Second, I contribute to the literature that identifies which policy measures affect food expenditures. Finally, I add to the literature that explores sales tax policy.

A traditional theoretical approach in the optimal commodity taxation literature is finding the optimal tax schedule on goods, under the assumption of a limited number of different tax rates based on the compensated elasticities of demand (Belan and Gauthier, 2004, 2006; Belan et al., 2008). As Gillitzer et al. (2015) emphasize, this approach has little value for policymakers because it requires the knowledge of the elasticities for all the goods (including newly invented ones), which is difficult to estimate. Instead, the authors propose to optimize the commodity tax schedule based on item characteristics, something that is straight-forward and easy to implement. The main rule is that the larger the number of similar characteristics between two goods, the more substitutable are these goods and, hence, the less should be the tax differential between them Slemrod (2013).

While there are a lot of real-life situations where the tax policy violates this rule, there is a lack of empirical evidence showing a substantial bunching of goods with similar characteristics due to notches in the tax schedule. The only exception is the paper by Sallee (2011) who provides evidence that automakers manipulate fuel economy ratings just to qualify for more favorable treatment under the Gas Guzzler Tax. My paper addresses a different market— the market for food. In addition to showing that consumers do respond to notches in the tax schedule, I find evidence of heterogeneous responses across the socio-demographic characteristics of consumers. In this regard, my paper is similar to that by Almunia and Lopez Rodriguez (2014), who find that firms from different industries have heterogeneous responses to effective tax enforcement.

The latter paper belongs to another strand of literature I contribute to. The papers in this strand study the effects of notches in the tax schedule on the same entity. Kleven and Waseem (2013) consider notches in Pakistan income tax and use them to uncover the elasticities of earnings with respect to the tax rate. Best and Kleven (2013) find that notches in the housing markets substantially generate bunching in the timing and the volume of house transactions in the UK. Kopczuk et al. (2013a) obtains similar result for New York and New Jersey markets. Finally, Best et al. (2015) finds substantial increase in the bunching of loan-to-value variable in response to notches in the interest rate that depend non-linearly on the loan-to-value variable.

My paper also contributes to the literature that studies which policy tools affect food consumption patterns. The ultimate goal of this research is to decrease obesity by increasing the consumption of healthy foods. Increasing the incomes of households achieves this goal (Akee et al., 2013). Another policy measure is to inform consumers about the caloric intake of certain foods (Bollinger et al., 2011). Currie et al. (2010)

find that the proximity of a household or school to a fast-food restaurant increases the propensity of gaining weight for the pregnant women in these households or the school's pupils. Anderson and Matsa (2011) do not confirm this result for the rest of the household's members. In this paper, I show that a increase in the tax differential induces high-income households to buy more meat items. On the other hand, the fact that low-income households start spending more money on dry foods in grocery may not be a good outcome, even if they visit less fast food restaurants.

Finally, I add to the understanding of the sales tax policy. Recent papers focus on two main aspects. First, there is substantial amount of competition between localities and states due to the ability of consumers to travel to the neighboring localities for shopping (Agrawal, 2014, 2015). Such competition results in lower tax rates. Other papers explore how tax-free sales on the Internet affect the incentives of consumers to shop. Einav et al. (2014) and Baugh et al. (2014) find that consumers shop more online in response to the sales tax increase. Both sets of papers do not consider the effect of sales tax on food expenditures because it is unlikely to expect that consumers purchase a substantial part of groceries either online or in the neighboring jurisdictions. However, given that food service tax can vary substantially between localities, studying the effect of sales tax on food expenditure is worthwhile; I fill in this gap by estimating the effect of the sales tax on grocery expenditures.

Table 3.1: Changes in Sales Tax on Food Items in Treatments States

	Date of Tax Change	Initial Tax Rate	Final Tax Rate
Arkansas (5)	1-Jul-2007	6%	3%
New Mexico (35)	1-Jan-2005	5%	0%
Utah (49)	1-Jan-2007	4.75%	2.75%
Wyoming (56)	1-Jul-2006	4%	0%

3.3 Data

3.3.1 Policy Overview

In 1989, exactly half of the U.S. states provided an exemption of some kind from the sales tax on groceries. Since then, another 14 states started preferential treatment of groceries.[5] The main argument for the exemption is the decrease in the sales tax regressiveness, as low-income households spend a bigger share of their income on food.[6] Another argument is particularly relevant for the treatment states considered in this paper. Most of the neighboring states have already made groceries tax-free, so exempting food saves travel time for the households who engage in cross-border shopping.[7]

For my estimation, I use the introductions of the sales tax exemptions on groceries between 2004 and 2008 in four treatment states: Arkansas, New Mexico, Utah and Wyoming.[8] Table 3.1 shows the timing of the policy changes in the treatment states and the magnitude of the drop in the sales tax rate. It ranges from 2 percentage points to 5 percentage points, which is likely to be large enough to be salient to

[5]Numbers are from Ring Jr (1999) for 1989 and from Center on Budget and Policy Priorities report

[6]"Should States Tax Food" by Johnson. L. and Lav I.

[7]"Texas and the Economy" by Pakko M., Institute for Economic Advancement

[8]Another state that introduced a sales tax exemption during this time period is South Carolina. I exclude this state from estimation due to the timing of the event (November 1, 2007) which requires the use of the data after the Great Recession. The latter could bias my results

consumers (Chetty et al., 2009).

Before I proceed to explain the Nielsen Scanner Data, I need to clarify an important issue with the sample selection. I exclude years during the Great Recession from my analysis because the Recession affected states differentially; in addition, the way the data are constructed prevents me from observing current household income and controlling for it, which could bias my results if I include those years. Given that two changes in exemption rules occur in 2007, I can only have one year of observations after the treatment. Thus, to be consistent across the sample, for every treatment state, I include observations for two years – the year prior to and the year of the policy implementation. As a control group, I consider all the neighboring states. This gives me 16 states in the sample. On average a state is present in the sample for 2.6 years because some states serve as a control for two or more treatment states. In Table 3.2, I show which observations for which state-years enter in my sample.

3.3.2 Nielsen Scanner Data

All the data for my analysis, except for the sales tax rates, comes from the Consumer section of the Nielsen Scanner Data. This panel data contains detailed information about grocery purchases of a representative sample of U.S. households. After any grocery trip, a household is supposed to scan the receipt, which allows Nielsen to identify purchased items, amount paid and stores visited. Every household stays in the sample for a discrete number of years. If a households drops out of the sample in the middle of the year, the observations for that household are not present in my data. The Nielsen survey year coincides with a calendar year.

To estimate the effect of the sales tax on food consumption, I refine the data

Table 3.2: Households from State-Years in the Sample

	2004	2005	2006	2007
Arizona	X	X	X	X
Arkanzas			**X**	**X**
Colorado	X	X	X	X
Idaho		X	X	
Mississippi			X	X
Missouri			X	X
Montana		X	X	
Nebraska		X	X	
Nevada			X	X
New Mexico	**X**	**X**		
Oklahoma	X	X	X	X
South Dakota		X	X	
Tennessee			X	X
Texas	X	X	X	X
Utah		**X**	**X**	**X**
Wyoming		**X**	**X**	**X**

in several ways based on the way the dataset is constructed. I start by aggregating household food expenditures into eight food categories; I enumerate each of them and provide examples in Table 3.3. Except for the last two, the categories coincide with the food departments suggested by the Nielsen Scanner Data. I categorize ready-to-serve meals separately because I expect them to be the closest substitutes for restaurant meals.

Next, I aggregate the expenditures to a monthly level. This allows me to abstract from highly volatile daily data on food expenditures. In the resulting data, the unit of observation is a household in a given month. Then, I drop all January/December months from my sample because there is noticeable underspending of households in the first and last months of the year when households enter and exit the sample.

While Nielsen reports the location of the store where a given purchase occurred,

Table 3.3: The Description of Food Categories Used in My Analysis

Categories of Food	Examples
Dry Grocery, excluding Ready-To-Serve	Baking Mixes, Bread, Cereals, Cookies
Frozen Foods, excluding Ready-To-Serve	Ice-Cream, Breakfast foods-frozen, frozen vegetables
Meats	Fresh Meat, Ham, Sausages, Bratwurst
Deli	Dressings, Salads and Special Entrees
Dairy	Milk, Eggs, Yogurt, Cheese
Fresh Produce	Apples, Kiwi, Lettuce, Herbs
Dry Ready-To-Serve Meals	Pickled Vegetables and Fruits, Canned Mexican Dinners, Dry Dinners, Dumplings, Canned Tuna
Frozen Ready-To-Serve Meals	Prepared Entrees, Frozen Pizzas

the way Nielsen does this prevents me from estimating the effects of sales tax policy on grocery shopping across states. Trips to a neighboring state in the data are very rare because in the absence of information on the exact store location, the data analysts assume that a household representative went to the store closest to her house of a given brand. Given that cross-border shopping may bias my estimates, I include in my sample only the households living in the counties located at least 50 miles away from the nearest state border.

In addition to the detailed records on purchases, Nielsen surveys a number of socio-demographic and geographic household characteristics. This happens before the new survey year starts and, thus, is not very precise. For most of the variables, if the change in characteristics happen during the middle of the calendar year, it appears in the data only the following calendar year. One exception is income, which is actually lagged by two years because the respondents report their income from the previous tax return before the survey year begins. To clarify, suppose for the survey year 2007, sociodemographic characteristics are collected in December 2006;

Table 3.4: Summary Statistics by Treatment Area. Panel A - Monthly Household Expenditures

	(1)	(2)	(3)	(4)	(5)
Treatment Area	Arkansas		NM-UT-WY		All
	Treat.	Control	Treat.	Control	
All Foods, $	171	172	167	182	176
	(120)	(119)	(16)	(127)	(122)
Dry Groceries, $	48.9	50.0	49.3	52.8	51.0
	(45.9)	(37.4)	(35.2)	(40.6)	(38.6)
Frozen Groceries, $	10.9	11.0	10.8	11.3	11.1
	(10.0)	(10.7)	(9.48)	(11.2)	(10.8)
Meats, $	10.1	9.32	10.1	9.16	9.29
	(8.98)	(8.88)	(10.4)	(8.81)	(8.87)
Dry Ready-To-Serve Meals, $	5.55	5.21	6.06	5.81	5.42
	(5.67)	(5.62)	(6.34)	(5.80)	(5.69)
Frozen Ready-To-Serve Meals, $	12.6	12.6	10.9	13.4	12.6
	(13.0)	(12.1)	(9.19)	(12.9)	(12.4)

Notes: Column (1) presents summary statistics of expenditures on different food categories by households located in Arkansas, Column (2) – by household located in 6 control states (Louisiana, Mississippi, Missouri, Oklahoma, Tennessee, Texas), Column (3) – by households located in the three treatment states (New Mexico, Utah, Wyoming), Column (4) – for the rest of the states in my sample and Column (5) – for the whole sample. Table 3.3 describes food categories.

the household income would the income reported in the 2006 income tax returns, which is in actuality what was earned in the year 2005, resulting in the two year lag.

Table 3.4 and 3.5 provide summary statistics for the main variables involved in my analysis for the whole sample and for four subsamples which describe four geographic areas: Arkansas, Control states for Arkansas, New Mexico/Utah/Wyoming, and Control states for the latter area. As one can see both expenditures and socioeconomic variables are not significantly different across the areas.

Table 3.5: Summary Statistics by Treatment Area. Panel A - Monthly Household Expenditures

	(1)	(2)	(3)	(4)	(5)
Treatment Area	Arkansas		NM-UT-WY		All
	Treat.	Control	Treat.	Control	
Household Income	17.7	19.4	17.4	18.8	19.1
	(5.92)	(6.01)	(6.07)	(6.03)	(6.03)
Household Size	2.33	2.42	2.38	2.24	2.35
	(1.28)	(1.29)	(1.25)	(1.29)	(1.29)
Married Couple	0.62	0.63	0.55	0.56	0.60
	(0.48)	(0.48)	(0.50)	(0.50)	(0.49)
Have Children	0.25	0.25	0.23	0.20	0.23
	(0.43)	(0.43)	(0.42)	(0.40)	(0.42)
Male Head	0.42	0.48	0.35	0.42	0.45
Employed Full Time	(0.49)	(0.50)	(0.48)	(0.49)	(0.50)
	0.312	0.41	0.29	0.39	0.40
College Degree	(0.46)	(0.49)	(0.45)	(0.49)	(0.49)
Female Head	0.40	0.40	0.37	0.36	0.39
Employed Full Time	(0.49)	(0.49)	(0.48)	(0.48)	(0.49)
	0.28	0.36	0.28	0.38	0.37
College Degree	(0.45)	(0.48)	(0.45)	(0.49)	(0.48)
White	0.87	0.77	0.68	0.87	0.81
	(0.33)	(0.42)	(0.47)	(0.34)	(0.39)
Have Internet Connection	0.73	0.72	0.60	0.66	0.70
	(0.45)	(0.45)	(0.49)	(0.47)	(0.46)
WIC Participants	0.01	0.01	0.00	0.00	0.01
	(0.08)	(0.08)	(0.00)	(0.06)	(0.07)

Notes: For the column description, see Table 3.4. Variable "Household Income" is in ranges: 17 refers to \$35,000-\$39,999, 18 – \$40,000-\$44,999 and 19 – \$45,000-\$49,999

3.4 Empirical Strategy

For my analysis, I use differences-in-differences empirical strategy. The unit of observation in my sample is a household hh, for which I observe monthly expenditures $Exp_{hh,month}$ on various categories of groceries purchased at supermarkets or other stores. My dependent variables are the logarithms of these expenditures $Log(1 + Exp_{hh,month})$. My main explanatory variable is a sales tax differential which equals the difference between the sales tax on restaurants $\tau^R_{state,month}$ and the sales tax on groceries $\tau^G_{state,month}$, where index *state* represents the residential state of a household. Note, I drop households that move from one state to another during the sample period. Thus, there is a one-to-one correspondence between households and states. The control variables are socio-economic characteristics of every household, which vary on a yearly *year* basis. Thus, my main specification is

$$\begin{aligned} Log(1 + Exp_{hh,month}) = \alpha + \beta(\tau^R_{state,month} - \tau^G_{state,month}) + Controls_{hh,year} \\ + \xi_h + \mu_{month} + \epsilon_{hh,month}, \quad (3.1) \end{aligned}$$

where a household fixed effect ξ_h controls for permanent household and state characteristics, such as the quality of supermarkets and restaurants in a given area. Month-year fixed effects μ_{month} control for any shocks common across all the households shocks.

The main coefficient of interest, β, should theoretically be positive: the higher the difference between restaurant food service tax and grocery sales tax, the more consumers should spend on groceries. This is, of course, under the plausible assumption that the change in the sales tax does not affect consumer income substantially. The main source of variation that identifies the coefficient of interest comes from changes in the sales tax exemptions on groceries.

I assume that these exemption changes are exogenous to the typical household's decision of how much to spend on groceries because the main goal of the policy is to reduce the regressiveness of the sales tax. However, there are two issues that could have affected the decision of policymakers and also bias my results. First, the extent of cross-border shopping by households living close to the neighboring states could influence the policy. An increase in the share of cross-border shopping may simultaneously induce the exemption on groceries and increase the purchases of groceries in the state supermarkets not related to the tax differential. I deal with this potential source of bias by dropping the households in counties located less than 50 miles away from the border. Second, state legislators might try to minimize the extent of influence of the sales tax exemption on state revenues. If the probability of policy implementation depends on the share of households income spent on grocery items, than I should observe a reverse causation— lower expenditures should result in higher tax differential. Given that my coefficient of interest is positive, this would bias it towards zero. If so, my results should be considered as a upper bound on the actual value of the coefficient.

3.5 Results

In this section, I show how the tax differential between restaurant food service tax and grocery sales tax affects household expenditures on different types of goods. I present here the results for the categories for which I either obtain significant coefficients in any of the regressions (meats, ready-to-serve dry meals, other dry groceries) or which are similar in nature to these categories (frozen meals, other frozen groceries). I find that for the whole sample only meat expenditures rise significantly: a one percentage point increase in the differential leads to 1.4% more spending on meat products, though overall expenditures remain constant. The results for high- and middle-income household are very similar, with the coefficient for meat products increasing to 1.8% and staying significant. However, the response of low-income households to an increase in the differential diverges: they increase overall food expenditures at grocery stores, with ready-to-serve dry meals mainly driving these results.

Table 3.6 presents the results for my main specification and the whole sample. In Column (1), my dependent variable is expenditures on all food items at grocery stores. I find that the coefficient on the tax differential is positive, though not significant. When considering each food category separately, the results look alike, except for the case of meats in Column (4). The coefficient is significant: a one percentage point increase in the differential leads to 1.4% more expenditure on meat. This implies that a tax differential similar to the one in Charlottesville, Virginia ($10.3\% - 2.5\% = 7.8\%$) results in a 11% increase in meat purchases at supermarkets. Given that on average a quarter of household expenditures on food at supermarkets is on meat, one can argue that the effect for meat alone would increase overall food expenditures in grocery stores by 2.7%.

For the next step in my analysis, I separately consider middle- and high-income versus low-income households. I define a middle-/high-income household as one that has income above $39K for two consecutive years prior to entering the sample.[9] In Table 3.7, I show that for high- and middle-income households, the results are similar

[9] This is an artifact of the Nielsen survey methodology that asks respondents for their two year lagged income.

Table 3.6: The Effect of Restaurant Meals and Grocery Differential on Food Consumption. Whole Sample.

	Dependent Var.: Log(1+Expenditures)					
	(1)	(2)	(3)	(4)	(5)	(6)
					Ready-To-Serve	
Categories	All Foods	Dry Groceries	Frozen Groceries	Meats	Dry Meals	Frozen Meals
Tax Differential, %	1.04	0.42	-0.62	1.39***	1.19	0.83
	(0.71)	(0.60)	(0.51)	(0.46)	(0.80)	(0.82)
No. of Obs.	261,640	261,640	261,640	261,640	261,640	261,640
R^2	0.007	0.008	0.005	0.006	0.005	0.004
No. of HHs	11,455	11,455	11,455	11,455	11,455	11,455

‡ p<0.01, ** p<0.05, * p<0.1

Notes: Robust standard errors clustered at the state level are in parentheses. The "Tax Differential" is the difference between the food service tax and groceries sales tax. I present the coefficient on this variable multiplied by 100, which shows in percentage terms how much the consumption of a certain category of food rises in response to a one percentage point increase in the tax differential. The data is at the household-month level. It includes households located in 16 states and covers the time period from February 2004 to November 2007 (for exact states and years see Table 3.2). Each regression includes item and month-year fixed effects and controls for household socio-economic characteristics. In Column (1), the dependent variable is expenditures on all food items, in Column (2) – only on dry groceries, excluding ready-to-serve meals (examples: rice, canned fruits, canned tuna, tea, etc.), in Column (3) – frozen foods, except for ready-to-serve ones (ice cream, frozen vegetables, frozen desserts), Column (4) – all packaged meats and fresh meat, Column (5) – ready-to-serve prepared meals (canned ravioli, canned roast beef, pickled vegetables, etc) and Column (6) – frozen ready-to-serve meals (frozen pizza and frozen entries).

Table 3.7: The Effect of Restaurant Meals and Grocery Differential on Food Consumption. High- and Middle- Income Individuals.

	Dependent Var.: Log(1+Expenditures)					
	(1)	(2)	(3)	(4)	(5)	(6)
					Ready-To-Serve	
Categories	All Foods	Dry Groceries	Frozen Groceries	Meats	Dry Meals	Frozen Meals
Tax Differential, %	-0.23	-1.11	-0.12	1.88***	-0.68	0.92
	(0.82)	(0.68)	(0.53)	(0.27)	(1.66)	(0.83)
No. of Obs.	133,740	133,740	133,740	133,740	133,740	133,740
R^2	0.007	0.009	0.006	0.007	0.006	0.005
No. of HHs	6,171	6,171	6,171	6,171	6,171	6,171

‡ p<0.01, ** p<0.05, * p<0.1

Notes: Robust standard errors clustered at the state level are in parentheses. I define high- and middle- income households as those with yearly income above $39,000. Robust standard errors clustered at the state level are in parentheses. The “Tax Differential” is the difference between the food service tax and grocery sales tax. I present the coefficient on this variable multiplied by 100, which shows in percentage terms how much the consumption of a certain category of food rises in response to a one percentage point increase in the tax differential. The data is at the household-month level. It includes households located in 16 states from February 2004 to November 2007 (for exact states and years see Table 3.2). Each regression includes item and month-year fixed effects and controls for household socio-economic characteristics. In Column (1), the dependent variable is expenditures on all food items, in Column (2) – only on dry groceries, excluding ready-to-serve meals (examples: rice, canned fruits, canned tuna, tea, etc.), in Column (3) – frozen foods except for ready-to-serve ones (ice cream, frozen vegetables, frozen desserts), Column (4) – all packaged meats and fresh meat, Column (5) – ready-to-serve prepared meals (canned ravioli, canned roast beef, pickled vegetables, etc) and Column (6) – frozen ready-to-serve meals (frozen pizza and frozen entries).

to those for the whole sample, with a larger coefficient for meat— 1.9. This result implies that households with above median income consider the meat at grocery stores as a substitute to restaurant meals.

For the subsample of low-income households, the results vary substantially from the main sample. First, Column (1) of Table 3.8 shows that households increase their overall expenditures on groceries by 2.15% in response to a one percentage point increase in the differential. The coefficient indicates that a tax differential in Charlottesville should lead to a 17% increase in food purchases at grocery stores by low-income households. Given that these households are more likely to eat at fast-food restaurants, this maybe an important result from the public health viewpoint if we find that the households start buying more of healthy food categories (fresh vegetables, fresh fruits, meats). Unfortunately, I do not find evidence for it. Instead, the main category that drives this result is dry groceries (Column (2)), and, particularly, ready-to-serve dry meals (Column (5)), such as canned ravioli or pickled vegetables. For the latter, the coefficient is equivalent to an 18% increase in such purchases at Charlottesville grocery stores.

On the other hand, dry foods are probably the closest substitutes for fast-food. Thus, in accordance with the theoretical results of Slemrod (2013), the differential affects the consumption of these food categories disproportionately.[10] Such behavior implies product bunching attributed to taxation, i.e. overproduction of goods that would not be present without the tax schedule. Interestingly, I do not obtain similar results for the frozen food categories in Columns (3) and (6); this could be due to the fact that frozen pre-packed meals are typically more expensive than dry ready-to-serve

[10]In the results not presented here, I use as a dependent variable the share of food expenditures on ready-to-serve dry meals and find that the coefficient is significant. This is another way to show that the differential affects different types of food disproportionately

Table 3.8: The Effect of Restaurant Meals and Grocery Differential on Food Consumption. Low-Income Households

	Dependent Var.: Log(1+Expenditures)					
	(1)	(2)	(3)	(4)	(5)	(6)
					Ready-To-Serve	
Categories	All Foods	Dry Groceries	Frozen Groceries	Meats	Dry Meals	Frozen Meals
Tax Differential, %	2.17**	1.57*	-1.06	1.38	2.30***	0.69
	(0.90)	(0.80)	(1.05)	(0.89)	(0.78)	(1.67)
No. of Obs.	87,160	87,160	87,160	87,160	87,160	87,160
R^2	0.011	0.011	0.007	0.007	0.006	0.005
No. of HHs	3,930	3,930	3,930	3,930	3,930	3,930

‡ p<0.01, ** p<0.05, * p<0.1

Notes: Robust standard errors clustered at the state level are in parentheses. Low-income households are those with yearly income below $39,000. The "Tax Differential" is the difference between the food service tax and grocery sales tax. I present the coefficient on this variable multiplied by 100, which shows in percentage terms how much the consumption of a certain category of food rises in response to a one percentage point increase in the tax differential. The data is at the household-month level. It includes households located in 16 states from February 2004 to November 2007 (for exact states and years see Table 3.2). Each regression includes item and month-year fixed effects and controls for household socio-economic characteristics. In Column (1), the dependent variable is expenditures on all food items, in Column (2) – only on dry groceries, excluding ready-to-serve meals (examples: rice, canned fruits, canned tuna, tea, etc.), in Column (3) – frozen foods except for ready-to-serve ones (ice cream, frozen vegetables, frozen desserts), Column (4) – all packaged meats and fresh meat, Column (5) – ready-to-serve prepared meals (canned ravioli, canned roast beef, pickled vegetables, etc) and Column (6) – frozen ready-to-serve meals (frozen pizza and frozen entries).

meals.

3.5.1 Robustness Checks

The household income variable provided by Neilsen Scanner data is lagged by two years and, hence, imprecise. Given that low-income households are usually young and may experience substantial growth in earnings in a short period of time, I use an alternative measure for income to check the robustness of my results. First, I use the absence of Internet at home as a measure of low-income. Given that my sample covers the years from 2004 to 2007, there is a substantial share of such households in my sample. I find that my results are similar to those using a direct measure of income. Second, I separately consider how the tax differential affects households that can devote time to cook versus those that cannot. The idea here is that the availability of time to cook is negatively correlated with household income. Indeed, I find similar results for the households with cooking time and low-income households. Apart from being another robustness check, this estimation is interesting in and of itself, as it shows that the exemption on groceries benefit households with unemployed members of the family.

In Table 3.9, I repeat regressions in Table 3.8 for households without the Internet connection. In most columns, the magnitude of the coefficients become smaller; however more of them are significant. In case of expenditures on all food items, the coefficient is 1.7, i.e a one percentage point increase in the tax differential between restaurant meals and groceries results in 1.7% more spending overall. The coefficient on dry meals excluding ready-to-serve items is now significant and equals 1.2%. Finally, for the ready-to-serve meals, it is now 1.9%.

Table 3.9: The Effect of Restaurant Meals and Grocery Differential on Food Consumption. Households without Internet.

	Dependent Var.: Log(1+Expenditures)					
	(1)	(2)	(3)	(4)	(5)	(6)
					Ready-To-Serve	
Categories	All Foods	Dry Groceries	Frozen Groceries	Meats	Dry Meals	Frozen Meals
Tax Differential, %	1.67**	1.22***	-1.16**	2.64	1.94**	0.39
	(0.68)	(0.36)	(0.44)	(1.66)	(0.69)	(1.54)
No. of Obs.	68,470	68,470	68,470	68,470	68,470	68,470
R^2	0.009	0.011	0.007	0.009	0.007	0.009
No. of HHs	2,705	2,705	2,705	2,705	2,705	2,705

‡ p<0.01, ** p<0.05, * p<0.1

Notes: Robust standard errors clustered at the state level are in parentheses. The "Tax Differential" is the difference between the food service tax and grocery sales tax. I present the coefficient on this variable multiplied by 100, which shows in percentage terms how much the consumption of a certain category of food rises in response to a one percentage point increase in the tax differential. The data is at the household-month level. It includes households located in 16 states from February 2004 to November 2007 (for exact states and years see Table 3.2). Each regression includes item and month-year fixed effects and controls for household socio-economic characteristics. In Column (1), the dependent variable is expenditures on all food items, in Column (2) – only on dry groceries, excluding ready-to-serve meals (examples: rice, canned fruits, canned tuna, tea, etc.), in Column (3) – frozen foods except for ready-to-serve ones (ice cream, frozen vegetables, frozen desserts), Column (4) – all packaged meats and fresh meat, Column (5) – ready-to-serve prepared meals (canned ravioli, canned roast beef, pickled vegetables, etc) and Column (6) – frozen ready-to-serve meals (frozen pizza and frozen entries).

Now I explore whether a household's ability to spend time on cooking affects their purchasing behavior. My cooking time availability measure is a dummy that equals to one if there is an individual in the family between the ages18 and 80 years who works less than 30 hours per week. The results in Table 3.10 are almost identical to those for low-income families. Meanwhile I find no effect of the differential for households that cannot afford much time to cook, as seen in Table 3.11. The coefficient is not significant even when I consider meats in Column (4). This is an important result as it shows that the exemption on groceries disproportionately benefits households with unemployed members of the family.

Table 3.10: The Effect of Restaurant Meals and Grocery Differential on Food Consumption. Households with Enough Cooking Time.

	Dependent Var.: Log(1+Expenditures)					
	(1)	(2)	(3)	(4)	(5)	(6)
					Ready-To-Serve	
Categories	All Foods	Dry Groceries	Frozen Groceries	Meats	Dry Meals	Frozen Meals
Tax Differential, %	1.96**	1.34*	-0.06	1.95	2.45***	0.89
	(0.89)	(0.68)	(0.89)	(1.25)	(0.67)	(0.88)
No. of Obs.	136,230	136,230	136,230	136,230	136,230	136,230
R^2	0.008	0.009	0.006	0.007	0.006	0.0049
No. of HHs	6,221	6,221	6,221	6,221	6,221	6,221

‡ p<0.01, ** p<0.05, * p<0.1

Notes: Robust standard errors clustered at the state level are in parentheses. I define "Cooking Time" measure as a dummy that equals to one if there is an individual in the family in between 18 and 80 years old who woks less than 30 hours per week. The "Tax Differential" is the difference between the food service tax and grocery sales tax. I present the coefficient on this variable multiplied by 100, which shows in percentage terms how much the consumption of a certain category of food rises in response to a one percentage point increase in the tax differential. The data is at the household-month level. It includes households located in 16 states from February 2004 to November 2007 (for exact states and years see Table 3.2). Each regression includes item and month-year fixed effects and controls for household socio-economic characteristics. In Column (1), the dependent variable is expenditures on all food items, in Column (2) – only on dry groceries, excluding ready-to-serve meals (examples: rice, canned fruits, canned tuna, tea, etc.), in Column (3) – frozen foods except for ready-to-serve ones (ice cream, frozen vegetables, frozen desserts), Column (4) – all packaged meats and fresh meat, Column (5) – ready-to-serve prepared meals (canned ravioli, canned roast beef, pickled vegetables, etc) and Column (6) – frozen ready-to-serve meals (frozen pizza and frozen entries).

Table 3.11: The Effect of Restaurant Meals and Grocery Differential on Food Consumption. Households without Cooking Time.

	Dependent Var.: Log(1+Expenditures)					
	(1)	(2)	(3)	(4)	(5)	(6)
					Ready-To-Serve	
Categories	All Foods	Dry Groceries	Frozen Groceries	Meats	Dry Meals	Frozen Meals
Tax Differential, %	-0.44	-1.09	-1.57	0.43	-0.92	0.63
	(0.67)	(0.73)	(1.07)	(1.12)	(1.35)	(1.16)
No. of Obs.	125,410	125,410	125,410	125,410	125,410	125,410
R^2	0.009	0.009	0.007	0.008	0.006	0.006
No. of HHs	5,234	5,234	5,234	5,234	5,234	5,234

‡ p<0.01, ** p<0.05, * p<0.1

Notes: Robust standard errors clustered at the state level are in parentheses. I define "Cooking Time" measure as a dummy that equals to one if there is an individual in the family in between 18 and 80 years old who woks less than 30 hours per week. The "Tax Differential" is the difference between the food service tax and grocery sales tax. I present the coefficient on this variable multiplied by 100, which shows in percentage terms how much the consumption of a certain category of food rises in response to a one percentage point increase in the tax differential. The data is at the household-month level. It includes households located in 16 states from February 2004 to November 2007 (for exact states and years see Table 3.2). Each regression includes item and month-year fixed effects and controls for household socio-economic characteristics. In Column (1), the dependent variable is expenditures on all food items, in Column (2) – only on dry groceries, excluding ready-to-serve meals (examples: rice, canned fruits, canned tuna, tea, etc.), in Column (3) – frozen foods except for ready-to-serve ones (ice cream, frozen vegetables, frozen desserts), Column (4) – all packaged meats and fresh meat, Column (5) – ready-to-serve prepared meals (canned ravioli, canned roast beef, pickled vegetables, etc) and Column (6) – frozen ready-to-serve meals (frozen pizza and frozen entries).

3.6 Conclusion

In this paper, I test whether the tax differential between the restaurant food service tax and grocery sales tax disproportionately affects the consumption of certain food categories. Following the framework suggested by (Gillitzer et al., 2015; Slemrod, 2013), I consider the differential as a notch in the tax schedule that gives a preferential treatment to food sold at grocery stores as opposed to food served at restaurants. Given that these two types of food are close substitutes, one would expect households to spend more on groceries with a widening of the tax differential.

Using difference-in-differences empirical strategy and the Nielsen Scanner Data, I find that the response of households to the tax differential depends on their socio-demographic characteristics. High-income households only increase their expenditures on meat products. However, low-income individuals purchase more of dry groceries, including read-to-serve dry meals, which seems to be a very close substitute to fast foods. Interestingly, they do not increase the consumption for another close substitute to fast foods – frozen meals, though this could be explained by their higher cost. Though the tax differential decreases fast food consumption, the increase in purchases of ready-to-serve dry meals might not necessarily be a more healthy choice. Thus, policy makers will have to find other means to combat obesity and induce consumers to eat healthily, such as complete tax exemptions for produce only or higher taxes on preservative-laden food.

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