Spring 2019

Essays in health and labor economics

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ESSAYS IN HEALTH AND LABOR ECONOMICS

by

Chelsea Jo Crain

A thesis submitted in partial fulfillment of the
requirements for the Doctor of Philosophy
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ABSTRACT

This dissertation focuses on how changes in public policies have the ability to affect the consumption and nutrition of consumers through changes in product prices and quality. In the first chapter, I examine price pass-through and changes in quality at restaurants in response to an increase in minimum wage. In the second chapter, I examine changes in prices among retailers and restaurants in response to the largest tax on sugar sweetened beverages in the U.S. In the third chapter, I examine changes in nutrition and the labor market effects on the aging population in response to bans on trans fatty acids in food away from home establishments.

In the first chapter, I investigate the responses of the restaurant industry to increases in the minimum wage. I construct a novel panel dataset based on online restaurant menus that allows me to analyze a full suite of potential margins of response including prices, quality, the number and types of items offered, hours of operation, and exit. I find that prices rise 0.3% to 0.8% in response to a 10% increase in the minimum wage. These results are consistent with previous estimates in the literature, as well as what is predicted by the textbook model of competitive factor markets and monopolistically competitive firms. Building on this, I then extend the literature to more broadly understand the price pass-through as well as provide the first estimates of responses on quality. I find heterogeneity in pass through across restaurant characteristics, with higher pass-through among small firms, and lower pass-through for restaurants near the border of a minimum wage policy region. At the menu item level, pass-through is higher for specific types of items, such

as sandwiches, and options with organic or gluten-free ingredients. In contrast, I find no evidence of higher pass-through for popular items. Further, I find significant changes in restaurant quality due to an increase in minimum wage. Specifically, I find that for low quality restaurants, quality decreases after an increase in minimum wage, but that quality increases for high quality restaurants. These quality results are driven more so by changes in service quality than by changes in food quality. Finally, I find no evidence that restaurants systematically change the number or types of items offered, nor hours of business in response to an increase in the minimum wage. Restaurants are, however, significantly more likely to exit due to an increase in minimum wage.

In Chapter 2, we estimate the incidence of a relatively new type of excise tax, a tax on sugar-sweetened beverages (SSBs). We examine the largest such tax to date, which is two cents per ounce, implemented in Boulder, CO on July 1, 2017. As in other communities, Boulder levies this tax on distributors. This paper estimates the extent to which this tax on distributors is passed through to consumers in the form of higher retail prices. To do so, we examine how the retail prices of SSBs changed after the tax in Boulder relative to a control community, using hand-collected data from retailers and internet data of restaurant menus. We find that 50.9 % of the tax was passed through to retail prices 5-7 weeks after the implementation of the tax. Some retailers add the tax only at the register, indicating that estimates solely from posted prices would result in an underestimate of pass-through. Including the taxes that were charged at the register, we find that 78.9 % of the tax was passed through to consumers.

In Chapter 3, I add to the growing literature on the relationship between health status and labor market outcomes by providing estimates of how a ban on the use of trans fatty acids in food away from home establishments, a nutrition based health shock, affected labor market outcomes for the aging population. I estimate that four and more years after implementation of a trans fat ban, the percent of those employed increases by 3.4 percentage points, and that average hours worked per week increases by 1.5 hours. In addition, I find that these increases are driven by a decrease in the percent of people unable to work, not by a decrease in retirement. Further, I find evidence that a decrease in cardiovascular disease incidences is the driving health mechanism behind these labor market effects.

PUBLIC ABSTRACT

This dissertation focuses on how changes in public policies have the ability to affect the consumption and nutrition of consumers through changes in product prices and quality. In the first chapter, I examine price pass-through and changes in quality at restaurants in response to an increase in minimum wage. In the second chapter, I examine changes in prices among retailers and restaurants in response to the largest tax on sugar sweetened beverages in the U.S. In the third chapter, I examine changes in nutrition and the labor market effects on the aging population in response to bans on trans fatty acids in food away from home establishments.

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CHAPTER 1
PRICE AND QUALITY RESPONSES OF THE RESTAURANT INDUSTRY TO INCREASES IN THE MINIMUM WAGE

1.1 Introduction

At \$7.25 an hour, the federal minimum wage in the United States has remained stagnant for almost a decade, and is over 30% lower in real terms than the federal minimum wage in 1970 (Wage and Hour Division, United States Department of Labor, 2016). As a result of this stagnation, a significant number of states, counties, and cities across the country have recently introduced, or are considering introducing, a local minimum wage. In 2012 there were only five city or county minimum wage laws across the country, but by the beginning of 2017 there were over 40 (UC Berkely Center for Labor Research and Education, 2016) . The number of state level minimum wage laws has also increased, with 19 states raising their respective minimum wage at the beginning of 2017 (The Economic Policy Institute, 2017). Despite the prevalence of such policies, there remains no clear consensus in the minimum wage literature about the complete response of firms to an increase in the minimum wage.

Previous studies on the minimum wage provide an incomplete picture of the responses to an increase in the minimum wage due to data limitations. I construct a novel dataset based on restaurant menus that allows me to extend the minimum wage literature. In particular, I examine heterogeneity in the price pass-through of a minimum wage increase across restaurant characteristics and item type, as well as differences in the pass-through for restaurants that approach the border of a minimum wage policy region. Further, I provide the first estimates in the literature of restaurant quality changes as another margin along which firms may react to labor cost shocks. Finally, I am able to address three potential additional margins of response: changes in the number of items offered, changes in hours of operation, and exit from the market.

I examine restaurants in New York, Massachusetts, and New Jersey. These three contiguous states on the East Coast increased their respective minimum wages on January 1, 2017, with the state of New York implementing four different levels of local increase. I examine responses to the minimum wage changes using a panel dataset of restaurant menus from Grubhub.com and Yelp.com. Using these datasets, and depending on the sample used, I estimate the overall price pass-through at the restaurant level to be between 0.3% and 0.8%, due to a 10% increase in the minimum wage.[1] These estimates are consistent with previous findings in the literature (e.g., Aaronson, French and MacDonald, 2008; Basker and Khan, 2013; Allegrotto and Reich, 2018).

Utilizing restaurant-specific characteristics, I find that the price pass-through estimate is heterogeneous across restaurant characteristics, where restaurants with a smaller number of employees showing higher pass-through at 1.1%. Unlike traditional administrative or government datasets, the data used in this study provide granular menu data at the item level. I find that the magnitude of the price pass-through varies significantly at the item category level, with lower pass-through among items such as appetizers and entrées, but higher pass-through among sandwich items. These results suggest that studies which examine prices of only a few menu items may significantly over- or underestimate overall

[1]This translates to an elasticity of between 0.03 and 0.08.

price pass-through. Items on a menu that are denoted as popular, however, have statistically similar price pass-through to all other items. This indicates that there may be high demand for these items but not necessarily lower price elasticity of demand. It also indicates that restaurants are not using popular items as loss leaders. In contrast, items with specific indicators of health, including “organic” or “gluten-free,” have significantly higher pass-through, which indicates that these items may have a lower price elasticity.

One potential negative effect of a minimum wage increase is the inability of restaurants close to the border of a minimum wage policy region to account for these increased input costs in the form of higher prices, without losing business (Allegrotto and Reich, 2018). This paper provides the most comprehensive analysis to date on the existence of border effects, defined as the relationship between the magnitude of price pass-through, and the proximity to the border of a policy region. I find a significant relationship between a restaurant’s proximity to the border and the level of price pass-through for restaurants located close to a bordering area that is facing a smaller minimum wage hike. Specifically, I estimate that the prices for a restaurant that is a 10 minute further drive from the border increased by 0.2 percentage points more than the prices for a restaurant on the border. These restaurants on the border do not increase prices more so than the restaurants on the opposite side of the border. These border effects suggest that restaurants in close proximity to the border of a minimum wage policy region may keep prices low as a competitive response in order to not lose profits.

In addition to changes in price, restaurant quality could change as a result of a minimum wage increase. Quality changes are difficult to quantify using traditional datasets due to a lack of quality measures. However, the data used in this paper provide a means to measure customer-perceived quality, which can be used as a proxy to measure changes in firm quality. Using the overall consumer ratings from the Grubhub and Yelp review platforms, I estimate the impact of a 10% minimum wage increase on the customer rating of restaurants. Although the net response to an increase in the minimum wage was a decrease in quality, I find that the response differs based on initial restaurant quality. Restaurants that were rated at the median or below prior to the minimum wage increase saw a statistically significant decrease of 1% in the consumer ratings due to an increase in the minimum wage. Restaurants that started at ratings above the median saw a positive effect on their consumer ratings (specifically an increase of 0.05%) due to the increase in the minimum wage. These effects are economically significant, representing 40% (10%) of the average rating decrease (increase). These results suggest that firms react to a minimum wage increase differently depending on initial quality.

These quality responses are persistent after controlling for increases in menu prices, suggesting that the effects are not driven by customer dissatisfaction from increases in price. To investigate other potential mechanisms of these quality results, I explore changes in the Grubhub reviews of food quality, order accuracy, and delivery timeliness. I find that the overall quality effects are driven more so by changes in order accuracy and delivery timeliness than by food quality. In addition, I perform sentiment analysis on the text of the Yelp customer reviews, looking for patterns of dissatisfaction or approval aimed specifically at service quality or food quality. Although the small sample size of this supplementary analysis yields imprecise estimates, I find suggestive evidence that the quality responses to increases in the minimum wage are driven more so by changes in service quality than by changes in food quality.

Taking advantage of the unique nature of the dataset, I investigate three other potential margins of response to an increase in the minimum wage. I find no significant changes in the number of menu items offered, or the types of items offered, in response to an increase in the minimum wage. Similarly, I find no evidence that firms decreased open hours of business as a margin of response to the increases in the minimum wage. Using exit from the sample as a proxy for exit from the market, I find a strong response of exit to the increases in minimum wage. Specifically, I find that a firm is 1.3 percentage points more likely to exit due to a 10% increase in the minimum wage, and that these effects are larger for lower quality firms. Overall, this paper provides new estimates to the literature of a more comprehensive set of responses by firms to minimum wage increases by demonstrating that increases in price vary by product type and distance to the border, and that the responses affecting service change the overall quality of customer experiences.

1.2 Background

1.2.1 Responses to the Minimum Wage

Nearly three-fifths of all workers who are paid at or below the minimum wage are employed in the service industry (U.S. Bureau of Labor Statistics, 2016), making restaurants an ideal sector in which to analyze the responses to a minimum wage increase. The majority of minimum wage-related research focuses on the employment effects of changes in the minimum wage (e.g., Katz & Krueger, 1992; Card & Krueger, 1994; Stewart, 2004).

A smaller set of papers have examined the influence of minimum wage increases on summary measures of price as an attempt to provide insight on employment effects. In this paper, I focus on the aspects of minimum wage policies that remain relatively unexplored in the literature: the price and quality responses of firms. In particular, I extend the literature by adding in depth price responses by item type, restaurant type, and restaurant location. In addition to the fact that price responses provide information on the underlying structure of the labor market, price and quality responses of restaurants are important to understand as restaurant food has become an integral part of both the American budget and diet.[2] This paper builds on the current understanding of price responses, and investigates quality as a new margin of response.

Price changes due to a minimum wage increase have implications for the underlying employment structure of the restaurant industry. Card and Kruger (1994) found that, after an increase in the minimum wage, employment was positively impacted to a small degree, while prices remained unaffected. These findings contradicted the textbook model of competitive labor markets, a model which predicts an increase in output prices and a decrease in employment. In response to Card and Kruger (1994), many studies analyzed the existence of monopsony power in the labor market (e.g., Manning, 1995; Rebitzer & Taylor, 1995; Burdett & Mortensen, 1998; Bhaskar & To, 1999), with the monopsony model predicting an increase in employment and a decrease in prices. Neumark and Wascher (2006) conclude in their survey of the literature that the most rigorous and reliable studies have

[2] Americans spend more money eating out at restaurants than they do on groceries (United States Department of Agriculture, 2017) .

found significant price increases but small employment decreases. As Aaronson, French, and MacDonald (2008) conclude, the presence of significant increases in price in response to minimum wage increases is evidence against the prevalence of monopsony power in the labor market. However, the small employment decreases suggest that firms may be adjusting along other margins to the increases in labor costs.

In the literature, the magnitude of the price pass-through to consumers after a 10% increase in the minimum wage varies between approximately 0.3% and 1.5%. Aaronsen, French and MacDonald (2008) utilize the store level data that comprises the "food away from home" component of the Consumer Price Index. This data consists of several bundles of food, usually the equivalent of a meal, at a variety of establishments across the country. By applying variations in state and federal minimum wage changes, the authors estimate a price pass-through of 0.7%. Basker and Khan (2013) estimate a price pass-through of 0.9% for McDonald's Quarter Pounders and Pizza Hut's regular cheese pizzas using state level variation in the minimum wage. Luca and Luca (2018) find pass through rates of 0.1% to 0.9% in response to local minimum wage changes in California, and using delivery order data from a Yelp based, online food delivery service. Though these estimates vary with the types of items and time period analyzed, they indicate that restaurants consistently pass-through the increased labor costs in the form of higher output prices.

In the closest related paper to this, Allegrotto and Reich (2018) use full online menus to analyze prices before and after a local minimum wage increase in San Jose, and find pass-through to be 0.58%. Further, they find heterogeneous effects across restaurant characteristics, and examine the existence of border effects. However, they have only two time periods of price data, and one treatment and one control area. My paper builds on this research to examine multiple minimum wage changes with multiple periods before and after implementation, and a substantially larger sample that allows me to examine heterogeneous responses in more detail and provide the first estimates on quality responses.

The majority of prior research on price pass-through examines only a few menu items at primarily large, chain restaurants, and uses state level variation in the minimum wage increases. This paper utilizes data on primarily non-chain restaurants, with full menu information, and both state and local changes in the minimum wage. Because of the unique format of the minimum wage settings and type of data, I am able to expand the understanding of how local restaurants respond to increases in the minimum wage.

From a policy perspective, it is important to understand if the magnitude of price pass-through is different for firms in close proximity to the border of a policy region. Firms with competitors that face different input costs, in this case differences in labor costs, may take into consideration these differences when setting prices (e.g. Nakamura, 2008; Chicu et al., 2013). Restaurants facing minimum wage increases close to a border where restaurants on the opposite side of the border are not facing an increase in labor costs may keep prices lower in order to compete. These effects are referred to as border effects, and can be measured by the existence of a relationship between the distance a restaurant is to a bordering area with a different minimum wage increase and the magnitude of the price increase. The existence of border effects is crucial with regard to policy evaluation and fully understanding how local businesses are affected by changes in the minimum wage policy.

Quality is another margin along which firms could adjust to the increases in labor costs. As discussed previously, economic theory predicts employment decreases in similar magnitude to price increases after a minimum wage increase. However, most research has found significant price increases but small employment decreases. This suggests that firms may be adjusting to an increase in the minimum wage through other channels, such as changes in quality. Quality could be expected to change after an increase in the minimum wage in either direction. Overall restaurant quality could decrease in response to a minimum wage hike, for instance, due to reduced portion sizes, decreased quality of ingredients, or a firm being short-staffed.[3] On the other hand, an increase in the minimum wage could improve service quality by acting as an efficiency wage (Stiglitz, 1976; Schmitt, 2013). Restaurants could also improve average service quality by decreasing work hours for less productive employees, or increasing the productivity of current workers (Hirsch et al. 2015).

In this paper, I use customer reviews as a proxy for quality. Although online ratings are determined by self-selected reviewers, online ratings have been found to be a reliable predictor of actual firm quality as well as an important determinant of profit for restaurants. In a study comparing Yelp star ratings of hospitals to an industry standard assessment of quality, Bardach et al. (2014) found that customer ratings were significantly related to patient care and health outcomes. Luca (2011) used a regression-discontinuity design to analyze the impact of a change in the customer rating on restaurants' profitability, and found that a one-star increase in the Yelp rating led to a 5-9% increase in revenue. Using

[3]Chakrabarti et al., 2017 found that a $0.10 increase in real minimum wage increased the number of health code violations by 11.45 percent. This indicates a decrease in restaurant quality.

a similar identification strategy, Anderson and Magruder (2012) estimate that a half-star rating causes restaurants to fill all reservation openings 49% more frequently.

1.2.2 Minimum Wage Laws

On January 1, 2017, three contiguous states on the East Coast - Massachusetts, New Jersey, and New York - increased their minimum wage at differing magnitudes, with a variety of levels of increase within the state of New York. Table 1.1 reports the increases in the minimum wage in these areas, which range from 0.72% to 22.22%. These states provide a useful setting for minimum wage analysis as they are in the same geographic region and have similar economic, demographic, and political characteristics. Each area faced the changes in the minimum wage over the same time period. In April of 2016, New

Table 1.1: Minimum Wage Policy Changes

Area	2016	2017	%Δ
NYC & Large	$9.00	$11.00	22.22%
NYC & Small	$9.00	$10.50	16.67 %
NYC MSA	$9.00	$10.00	11.11%
NY Upstate	$9.00	$9.70	7.78%
Massachusetts	$10.00	$11.00	10.00%
New Jersey	$8.38	$8.44	0.72%

Notes: The minimum wage changes from 2016 to 2017 are reported by group. The first two rows show the minimum wage changes for restaurants in NYC. A small restaurant is defined as having 10 employees or less, and a large restaurant is defined by more than 10 employees. For the main analysis I use the average of the two minimum wage changes, 19.45%, for both small and large restaurants in NYC. The NYC MSA group consists of restaurants in the three contiguous counties to NYC: Nassau, Suffolk, and Westchester. NY Upstate encapsulates restaurants in all other areas of the state. NJ and MA minimum wage laws are consistent throughout each state.

York (NY) became the second state, after California, to pass a law that would incrementally raise the minimum wage for all workers to \$15/hour.[4] In this law, which applies to all non-fast food restaurants, the degree of the minimum wage increase is based on the type and location of the establishment (2015-2016 New York Legislative Session, 2016). The first two rows of Table 1.1 report minimum wage changes for restaurants in New York City (NYC). Restaurants in NYC with more than ten employees, denoted as large restaurants, saw a 22.22% increase, from \$9 to \$11 per hour. Small restaurants in NYC saw an increase of 16.67%, from \$9 to \$10.50. The third column of Table 1.1 includes restaurants of all sizes in the three contiguous counties outside of NYC: Nassau, Suffolk, and Westchester. These three counties are referred to throughout this paper as NYC MSA. These restaurants saw an 11.11% increase, from \$9 to \$10. The final NY group, NY Upstate, which includes restaurants of all sizes elsewhere in the state of NY, saw a 7.78% increase, from \$9 to \$9.70.

Two states contiguous to NY saw changes in their own state-wide minimum wage laws on January 1, 2017. Massachusetts (MA) passed a bill in 2014 that increased the minimum wage by \$1 a year from 2015 to 2017. This bill increased the minimum wage on January 1, 2017 by 10.00%, from \$10 to \$11. (State of Massachusetts General Assembly, 2014). In the spring of 2016, New Jersey (NJ) proposed a minimum wage law similar to that of NY that would have raised the minimum wage to \$10.10 per hour on January 1, 2017, and would incrementally raise the minimum wage until the state reached \$15/hour

[4]In 2015, NY passed a minimum wage law only applicable to fast food restaurants, increasing the minimum wage each year for fast food workers (New York State Department of Labor, 2015). Due to the small sample size and unique nature of fast food restaurants in the data, I exclude all fast food restaurants from the analysis.

(State of New Jersey Senate Budget and Appropriations Committee, 2016). The bill passed through the House and the Senate, but in August of 2016, NJ governor Chris Christie vetoed the bill, stating "...[this bill] fails to consider the capacity of businesses, especially small businesses, to absorb the substantially increased labor costs it will impose" (Christie, 2016). Thus, in 2017, NJ increased the state minimum wage by only 0.72%, a yearly adjustment for inflation (State of New Jersey Department of Labor and Workforce Development, 2014). The state of NJ is therefore a strong counterfactual to NY and MA because of the state's similar legislative intent but divergent application due to Christie's veto.

1.3 Data

I use three primary datasets to understand the responses of firms in the restaurant industry to increases in the minimum wage. The first is a panel dataset comprised of restaurant menu and quality information from Yelp.com. The second, and most extensive, is a panel dataset comprised of restaurant menu and quality information from Grubhub.com. The third is a dataset providing detailed business information from ReferenceUSA. I utilize these datasets to determine the magnitude and variability of price pass-through to consumers, how the minimum wage impacted customer-perceived quality, and examine other potential margins of response.

1.3.1 Yelp

Yelp.com is a website in which consumers can find restaurant information including customer reviews, and in many cases, full menus. Yelp was founded in 2004, and currently has an average of 72 million monthly visitors with over 115 million reviews written (Yelp, 2016). I collected restaurant and menu information from restaurants in NY, MA, and NJ over the course of a year. I conducted the first wave of Yelp data collection in April 2016, the second wave in July 2016, and the third wave in October 2016. Two waves of data collection occurred after the minimum wage increases, in January 2017 and April 2017.[5]

I collected information from the Yelp homepage of 69,224 restaurants within NY, NJ, and MA. Of these, 21,688, or approximately one third of these restaurants, provide a full menu on Yelp.[6] In a similar study using online menus, Allegrotto and Reich (2018) also found that, on average, about one third of restaurants posted full online menus. Some Yelp restaurants, primarily chain restaurants, provide a full menu but do not include location specific prices. Out of the restaurants that post full menus, 17,385 of them include location specific prices. To match each restaurant to a minimum wage group, 14,322 restaurants were matched by address to a county using the Census Geocoder. The 47 fast food restaurants were removed from the dataset.[7] To create a balanced panel, the final sample used in the analysis contains the 8,805 restaurants that posted location specific prices in all waves of data collection. Reasons that restaurants fail to be in all waves of data collection include closures, name changes, discontinued use of a Yelp menu, or technical errors.

[5] See the Appendix for further details about the data collection process.

[6] Some restaurants provide an external link to a menu, but since these menus are not formatted uniformly, the menu information cannot be correctly parsed and thus, for the sake of this dataset, these restaurants fall into the same category as those restaurants who do not provide an online menu. These externally formatted menus were hand entered for one round of the scrape, and the average price of these restaurants was not statistically different from the Yelp formatted menus. Appendix Table A1.1 reports comparisons on all characteristics.

[7] As discussed in Section 1.2.2, I do not include fast food restaurants in this study because fast food restaurants in NYC had different minimum wage laws prior to the enactment of the laws that I consider, and were not affected in the same way as were fast food restaurants in the other areas.

Yelp users provide online reviews of restaurants and assign them an overall star rating on a scale of 1 to 5, with 1 representing extremely poor and 5 representing outstanding. The rounded star rating that customers see prominently displayed on each restaurant's homepage is the monthly average of all Yelp reviews rounded to the nearest half star.[8] There is a similar distribution of ratings within each category of price, suggesting that consumers are rating firms based on expectations of quality. There is also significant variation in the Yelp star rating for restaurants over the time period of the dataset, an indication that Yelp users are active in reporting the current quality of the establishments. Over 50 percent of restaurants see a change in star rating between any given observation period, and the average change given an increase (decrease) is 0.62 (0.61) stars.

During the collection of the Yelp menus, it became apparent that restaurants may not consistently post updated menu prices, as Yelp menus are updated at the restaurant owner or manager's discretion and may not accurately reflect current pricing. To examine this potential concern, I began data collection in December 2016, from a second menu source, Grubhub.com. I therefore primarily use the Yelp data as a supplement to corroborate the results of the more accurate Grubhub data, as the Yelp data allow for pre-trend analysis.

1.3.2 Grubhub

Founded in 2004, Grubhub.com is the largest online food ordering company in the U.S., providing its 7.7 million customers in over 1,600 cities access to delivery at over

[8] If a restaurant has less than 10 reviews within a month, then the most recent reviews are added until the sample size reaches 10.

85,000 locations (Grubhub, 2016) . I collected menu information for all Grubhub restaurants in the areas of interest in December 2016, January 2017, February 2017, March 2017 and April 2017.

Restaurants that use the Grubhub delivery service are contractually obligated to match the prices they have set on their in-store menus. Thus, the online menus reflect up to date, accurate item and price information. I collected menus for 12,217 restaurants in the areas of interest. Of these, 10,414 were matched to a county using the Census Geocoder. After removing the 67 fast food restaurants, 8,415 restaurants comprise the final balanced panel. Reasons that restaurants fail to be in all waves of data collection include closures, name changes, discontinued use of the Grubhub service, or technical errors. All restaurants on Grubhub provide a uniform menu and are therefore included in the dataset of parsed menu information.

Grubhub provides customers with a faceted rating system. For customers that have ordered through the delivery service, they are sent a short survey with three questions: (1) Was your delivery on time? (2) Was your order correct? (3) Was the food good?. Food establishments are then given a rating on each of the three dimensions– a number from 0 to 100 that identifies the proportion of customers who responded yes to the given question. I use the average rating, as well as each individual facet of rating as measures of firm quality. The food and accuracy ratings soley reflect the restaurant, where the delivery rating could be a combination of the restaurant or the delivery driver. There is a similar distribution of ratings within each category of price, suggesting that consumers are rating firms based on expectations of quality. There is also significant variation in the customer ratings over the time period of the dataset, an indication that Grubhub users are also active in reporting current quality. Over 40 percent of restaurants see a change in the average customer rating between any given observation period, and the average change given an increase (decrease) is 0.944 (-1.11).

1.3.3 ReferenceUSA

I utilize ReferenceUSA (RUSA), an Infogroup company that provides business data, to define more detailed characteristics of the restaurants. I collect the data for all businesses in NY, MA, and NJ that are categorized under the North American Industry Classification System (NAICS) as a full-service (722511) or limited-service (722211) restaurant. The restaurant level variables obtained from this dataset include sales volume, number of employees, limited service status, and franchise status. I also use the number of years that a restaurant has been in the RUSA dataset as a measure of firm age.[9] Since these data are updated on a yearly basis, the variables are used only as baseline characteristics of the establishments. Using the number of RUSA firms as the total population of restaurants in these areas, I estimate that approximately 18% of restaurants in these areas utilize the Grubhub delivery service, and that approximately 26% of restaurants post a full menu on Yelp.

[9]The maximum age of restaurants is bounded at 34 years when the RUSA database was created, but less than 0.5% of restaurants in the data have an age of 34 years.

1.3.4 Data Construction and Definitions

To determine the minimum wage that the restaurants face, I match each restaurant to a county using the Census Geocoder. In NYC, the minimum wage that a non-fast-food restaurant faces is dependent on the number of employees. The RUSA dataset provides the number of employees at each location, however I am only able to match approximately half of the Grubhub and Yelp restaurants to the RUSA dataset. Therefore, in this paper I use the average minimum wage increase in NYC, 19.45%, for restaurants of all size in NYC.[10] Since there are significantly more restaurants that qualify as small in NYC (less than or equal to 10 employees), using the un-weighted average of the minimum wage increase will only bias my estimates towards zero. Figure 1.1 displays the geographic distribution of the restaurants in the Grubhub dataset.

For the Grubhub and Yelp menu data, I create a balanced panel at the item level, only including restaurants and items that are in all waves of data collection. One concern with using a balanced panel is that firms could respond to changes in the minimum wage by changing the items offered. However, as addressed more extensively in Section 7, I find no clear relationship between changes in the total number of items offered at a restaurant and changes in the minimum wage. Another concern is that firms may change the quality of the items in the balanced sample. These potential quality changes are addressed in Section 5. A third concern is that restaurants may close in response to a minimum wage increase

[10] I match on address, phone number, and restaurant name. The matching problem is consistent over multiple matching methods, including strict string matching, Stata's reclink, and Python's fuzzywuzzy. Relaxing the strictness of the matching programs increases the number of matches slightly but also increases the false match rate. However, the overall pass-through results are consistent across matching types, but with slightly larger magnitudes, when relying only on matched RUSA data and using the specific minimum wage increase by number of employees in NYC.

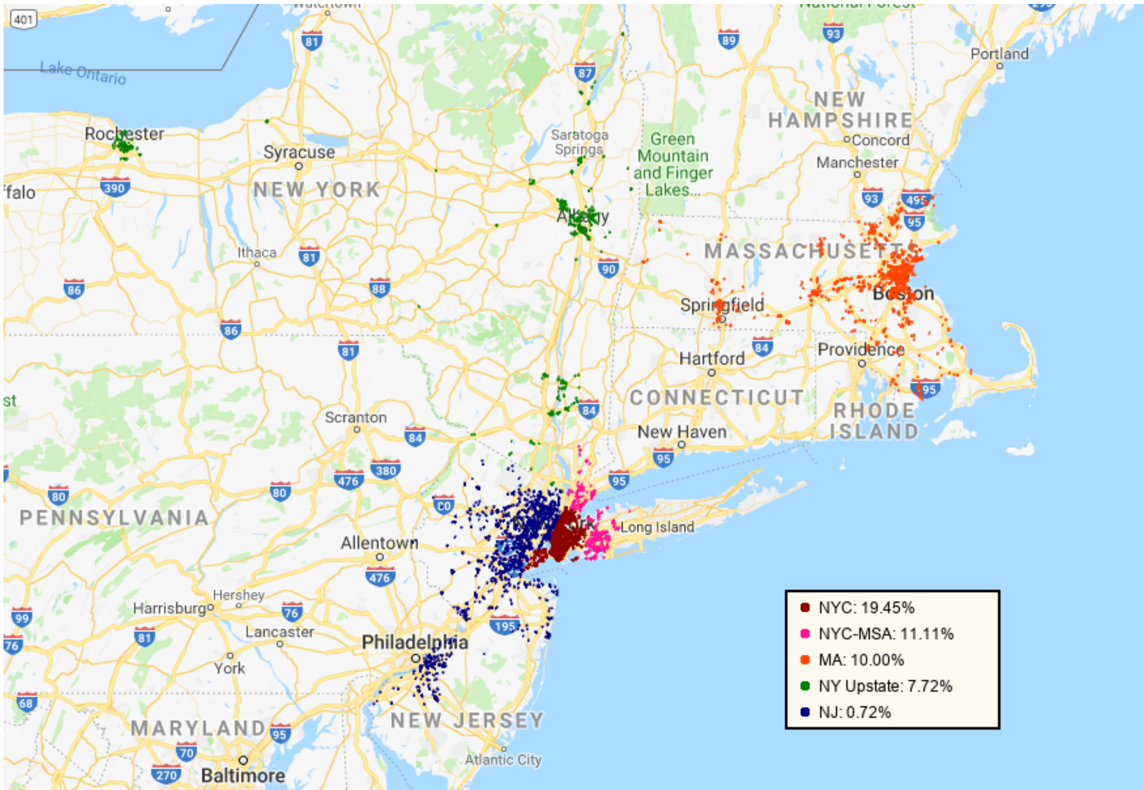

Figure 1.1: Sample of Grubhub Restaurants

Notes: Each data point represents a restaurant in the Grubhubh dataset. Samples are color coded by percent increase in the minimum wage on January 1, 2017. NYC saw the highest increase in the minimum wage at 19.45%. NYC-MSA, which includes Nassau, Suffolk, and Westchester counties, saw the second highest increase at 11.11%. The minimum wage increased in MA by 10.00%, in Upstate NY by 7.72% and in NJ by 0.72%.

(Luca & Luca, 2018). This potential concern is also discussed in Section 7.

Summary statistics of the balanced panels aggregated at the restaurant level for Grubhub and Yelp are reported in Table 1.2 and Table 1.3, respectively. In the Yelp dataset, changes in price from April 2016 to October 2016 are not statistically different in each of the minimum wage groups, providing support that the restaurants in NJ are a strong comparison group to NY and MA. The price changes over the time period of the minimum wage policy changes and the percent of price increases are significantly different between groups in both datasets. As shown in these tables, restaurants update menus less frequently in the Yelp dataset than the Grubhub dataset, but changes in price conditional on an increase or decrease in price are larger in the Yelp dataset. This is consistent with the properties of each web source. Yelp menus are updated at the owner or manager's discretion, and may not always provide updated prices. Grubhub, however, provides updated menu prices as customers order directly from the site. Limited service and franchise restaurants are a small portion (less than 10%) of the restaurants in both data sets. Many previous studies in the literature have focused primarily on limited service and franchise restaurants, thus these data provide information on an under-represented sub population (e.g., Katz & Krueger, 1992; Basker & Khan, 2016).

A concern of using customer reviews as a point of analysis regards the potential of restaurants writing fake reviews – both good reviews of themselves and bad reviews of competitors. Both Grubhub and Yelp use a proprietary algorithm in an attempt to filter out fake reviews, which are then not included in the overall ratings. In addition, both Luca (2011) and Anderson and Magruder (2012) found no evidence that restaurants are able to use fake reviews to manipulate ratings in a discontinuous manner. Although some fake reviews may still exist, it is unlikely that any fake reviews on quality are correlated with changes in the minimum wage.

To analyze the existence of border effects, I restrict the sample to restaurants in NYC and the contiguous NJ counties. I first construct a distance matrix using Google Maps Distance Matrix Application Programming Interface. This service provides travel distance and time for a given origin and destination based on the recommended driving

Table 1.2: Grubhub Restaurant Summary Statistics by Minimum Wage Group

	(1) NYC	(2) NYC MSA	(3) MA	(4) NY Upstate	(5) NJ	(6) F Test
Min Wage Increase	0.194	0.111	0.100	0.078	0.007	0.000
Starting Price (Dec16)	9.356	9.759	9.346	8.640	8.949	0.001
	(0.087)	(0.148)	(0.142)	(0.147)	(0.104)	
Number of Items	107.790	133.704	117.472	105.053	126.917	0.000
	(1.386)	(3.671)	(2.509)	(3.954)	(2.395)	
ΔP *(Dec16-Apr17)*	0.013	0.008	0.009	0.013	0.006	0.000
	(0.001)	(0.001)	(0.001)	(0.002)	(0.001)	
Increase	0.374	0.326	0.294	0.349	0.292	0.000
	(0.007)	(0.020)	(0.015)	(0.025)	(0.013)	
Decrease	0.066	0.060	0.053	0.047	0.073	0.242
	(0.004)	(0.010)	(0.008)	(0.011)	(0.007)	
Price Change $\|$ *Increase*	0.040	0.027	0.035	0.037	0.029	0.000
	(0.001)	(0.001)	(0.002)	(0.003)	(0.001)	
Price Change $\|$ *Decrease*	-0.025	-0.019	-0.030	-0.011	-0.026	0.896
	(0.001)	(0.002)	(0.002)	(0.001)	(0.002)	
Sales (100k)	8.643	3.218	4.976	4.684	2.987	0.000
	(0.654)	(0.171)	(0.262)	(0.336)	(0.083)	
Employees	9.916	5.650	7.996	9.582	5.130	0.000
	(0.290)	(0.294)	(0.416)	(0.714)	(0.142)	
Limited Service	0.042	0.070	0.029	0.067	0.065	0.013
	(0.003)	(0.011)	(0.006)	(0.013)	(0.007)	
Franchise	0.004	0.007	0.011	0.021	0.000	0.006
	(0.001)	(0.004)	(0.004)	(0.008)	(0.000)	
N	4172	565	866	358	1320	

Notes: The means and standard errors of the primary dataset, Grubhub, are reported. Each of the first five columns contains the restaurants that fall into a specific minimum wage group. All data are balanced at the item level across time periods and aggregated at the restaurant level. Starting price is average dollars per item. The fourth row reports mean change in natural log of the price, which is approximately the percentage change. The rows titled “Increase” and “Decrease” report the percentage of restaurants that increased or decreased price between Dec 2016 and Apr 2017. The conditional price changes calculated from Dec 2016 to Apr 2017. Quality rating is the average quality rating for a restaurant in December 2016 and is on a scale of 1-100. Column 6 displays the p-value of the multiple means test using the respective variable and all five groups.

Table 1.3: Yelp Restaurant Summary Statistics by Minimum Wage Group

	(1) NYC	(2) NYC MSA	(3) MA	(4) NY Upstate	(5) NJ	(6) F Test
Min Wage Increase	0.194	0.111	0.100	0.078	0.007	0.000
Starting Price (Apr16)	9.781	10.703	9.891	9.622	9.869	0.044
	(0.108)	(0.268)	(0.136)	(0.375)	(0.183)	
Number of Items	71.679	81.590	68.008	58.653	75.188	0.000
	(1.113)	(2.794)	(1.503)	(2.459)	(1.686)	
ΔP *(Apr16-Oct16)*	0.005	0.002	0.003	0.003	0.005	0.428
	(0.001)	(0.002)	(0.001)	(0.001)	(0.001)	
ΔP *(Oct16-Apr17)*	0.008	0.007	0.005	0.001	0.004	0.091
	(0.001)	(0.004)	(0.001)	(0.001)	(0.002)	
Diff. in Change in Price	0.003	0.005	0.002	-0.002	-0.000	0.405
	(0.001)	(0.005)	(0.002)	(0.002)	(0.002)	
Increase	0.145	0.089	0.115	0.040	0.085	0.000
	(0.005)	(0.012)	(0.008)	(0.009)	(0.007)	
Decrease	0.041	0.025	0.028	0.015	0.032	0.004
	(0.003)	(0.006)	(0.004)	(0.005)	(0.004)	
Price Change $\|\|$ *Increase*	0.083	0.108	0.064	0.048	0.087	0.201
	(0.002)	(0.011)	(0.003)	(0.003)	(0.005)	
Price Change $\|\|$ *Decrease*	-0.100	-0.089	-0.097	-0.048	-0.095	0.950
	(0.003)	(0.006)	(0.002)	(0.003)	(0.003)	
Sales (100k)	8.489	7.597	9.233	5.019	5.631	0.000
	(0.219)	(1.524)	(0.471)	(0.263)	(0.226)	
Employees	10.836	10.227	14.603	10.222	9.267	0.000
	(0.237)	(0.858)	(0.604)	(0.519)	(0.360)	
Limited Service	0.042	0.072	0.019	0.069	0.043	0.000
	(0.003)	(0.011)	(0.003)	(0.011)	(0.005)	
Franchise	0.001	0.006	0.016	0.000	0.010	0.000
	(0.000)	(0.003)	(0.003)	(0.000)	(0.002)	
N	4242	595	1658	519	1793	

Notes: The means and standard errors of the secondary dataset, Yelp, are reported. Each of the first five columns contains the restaurants that fall into a specific minimum wage group. All data are balanced at the item level across time periods and aggregated at the restaurant level. Starting price is average dollars per item. The fourth and fifth rows report mean change in natural log of the price, which is approximately the percentage change. The sixth row reports the difference in the previous two rows, showing the change in trends between pre and post-policy implementation. The rows titled "Increase" and "Decrease" report the percentage of restaurants that increased or decreased price between Oct 2016 and Apr 2017. The conditional price changes are calculated from Oct 2016 to Apr 2017. Quality rating is the average quality rating for a restaurant in April 2016 and is on a scale of 1-5. Column 6 displays the p-value of the multiple means test using the respective variable and all five groups.

route. I calculate a distance matrix for each restaurant in the sample, which is comprised of driving time to all restaurants in the full RUSA sample that are on the opposite side of the border. I record the minimum of these driving times as the distance to the border. This provides a more accurate measure of distance than raw miles. For the border effects analysis, I include all restaurants that are within 12 minutes of the NYC-NJ border (Appendix Figure A1.2), as Iacono et al. (2008) found that 90% of Americans travel 12 minutes at maximum to go to a restaurant. Although the Hudson river separates these two areas, more than 400,000 people commute from NJ to NYC (United States Census Bureau, 2015), suggesting that it is feasible that consumers may be choosing between dining on either side of the border.

In the Grubhub data, there are 371 restaurants within 12 minutes of the border in NJ, and 323 in NYC. The restaurants in NYC that are close to the border are statistically similar to the overall NYC sample on starting price, customer rating, total items, and total change in price. The NJ restaurants that are close to the border also have statistically similar characteristics to the full NJ sample on all of these measures. In the Yelp data, there are 607 restaurants in NYC within 12 minutes of the border, and 395 in NJ. These Yelp restaurants that are close to the border are similar to the full sample of their respective minimum wage group on starting price, total items, and total change in price.

1.4 Methods

My primary analysis estimates the extent to which increases in the minimum wage are passed on to consumers through prices. The equation of price pass-through at the restaurant level regresses the log change in price on the log change in the minimum wage,

$$\Delta \ln p_{jkt} = \sum_{h=l}^{L} \beta_h \Delta \ln(mw_{kt-h}) + \gamma p_{j,t=0} + \zeta T_{jkt} + \eta_k + \lambda_t + \varepsilon_{jkt} \tag{1.1}$$

where p_{jkt} is the average price of the balanced items at restaurant j in the minimum wage group k in observation wave t. Each restaurant is location specific, and p_{jkt} is the mean price in U.S dollars of all menu items at restaurant j in the balanced panel. Since less than 1% of the sample are franchise restaurants, I do not include fixed effects for restaurants in the same franchise. This model is an extension of the standard difference-in-differences model, where NJ is the control group. The coefficient β_h estimates the relationship between the differences-in-differences and the magnitude of the minimum wage increase at time $t - h$. All standard errors are clustered at the minimum wage group level.[11]

I allow for a flexible price response from restaurants by including contemporaneous and lagged changes in the minimum wage.[12] For the specifications using the primary Grubhub data, I include no lead and three lag periods ($l = 0, L = 3$). No lead period can be included since the first period of observation is in December 2016. For the specifications using the Yelp data, I include two lead periods in addition to the one lag period ($l = -2, L = 1$) in order to analyze the existence of any relationship between price increase and minimum wage group before the policy implementation.

[11] Since there are only five clusters, I also implemented the wild cluster bootstrap method as recommended by Cameron, Gelbach and Miller (2008). These bootstrapped standard errors are unchanged but if anything are slightly smaller by 0.005. As a result, I report the more conservative standard errors, which are clustered at the group level, throughout the paper.

[12] Appendix Figure A1.3 displays a cumulative distribution function of the percent of firms who have updated their menu over the time period of data collection. Less than 20% of restaurants update menu prices between any observation periods, providing support for the inclusion of lagged time periods.

Although all groups knew of the policy changes by April 2016, it is unlikely that restaurants responded to the impending wage hikes more than four months in advance. For example, Aaronson et al. (2008) found that restaurants do not respond to changes in the minimum wage more than two months ahead of implementation. Therefore, the estimates of these lead terms are a good indication of the existence of potential policy endogeneity. Although there are three lags included in the Grubhub specification, these time periods encapsulate price changes over the same three month period as the one period lag in the Yelp specification. Figure 1.2 displays the normalized price trends over time for both datasets.

The average price at a restaurant before the change in the minimum wage is the primary means in which to characterize restaurants in the datasets. Thus, $p_{j,t=0}$ is the average price at restaurant j in the first observation period (April 2016 for the Yelp data and December 2016 for the Grubhub data).[13] I account for the variation in how long the data collection took for each wave by including the variable T_{jkt}, an integer representing the number of days between observations for a given restaurant. η_k is a fixed effect for minimum wage group, and ε_t is a fixed effect controlling for the observation period in which I collected the data. Together, these two terms account for any differences in the timing of the data collection between waves as well variation in macroeconomic conditions and restaurant demand.

To test the extent to which other restaurant characteristics are driving the price pass-

[13] Although estimates for γ are significant, including $p_{j,t=0}$ does not significantly change the coefficients of interest.

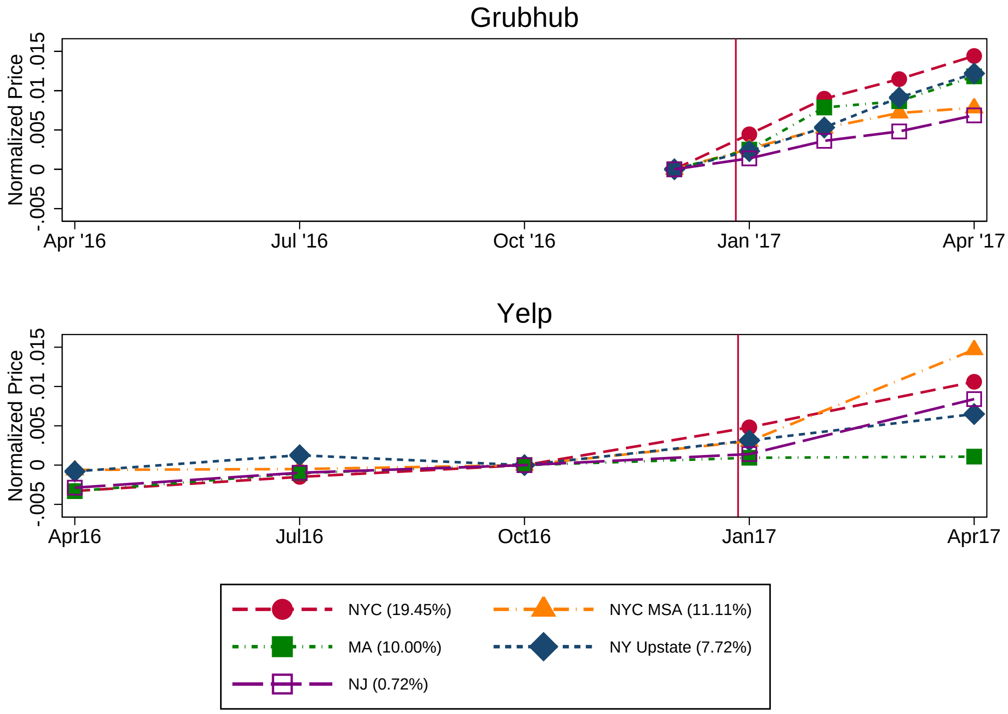

Figure 1.2: Trends in Grubhub and Yelp Menu Prices

<u>Notes:</u> Prices are the average price of all items in the balanced sample. In the top panel, the Grunbhub prices are normalized to zero in December 2016. In the bottom panel, the Yelp prices are normalized to zero in October 2016. Changes in normalized price represent the percent change in price off of the baseline price that is normalized to zero. The percent change in minimum wage in January 2017 is shown in parenthesis in the legend.

through estimates, I include $\boldsymbol{X}_j$, a vector of RUSA variables, in some specifications. This control vector includes sales volume, number of employees, limited service status, franchise status, and firm age.

The key assumption in this identification strategy is that NJ is an appropriate counterfactual for NY and MA, in that the changes in the minimum wage are uncorrelated with unobserved determinants of price. Supporting this assumption, the second panel of Figure 1.2 indicates that price trends were relatively similar for all groups prior to implementation of the minimum wage policies. In addition, as discussed in Section 2, NJ is geographically close and socioeconomically similar to both states that did increase the minimum wage. NJ also has a similar political sentiment amongst elected state congressional representatives, as the state attempted to increase their own minimum wage at the start of 2017.

1.5 Price Responses

1.5.1 Overall Price Pass-Through

I report the results of equation 1.1 in Table 1.4. All estimates are interpreted as the percent change in price due to a 10% increase in the minimum wage over the given time period. The row titled Total Pass Through is a linear summation of the estimated coefficients in all relevant time periods. For the specifications using the Grubhub data (columns 1-3), the coefficients of all time periods are included in the total pass-through estimates as there are no lead terms included in the equation. For the specifications using the Yelp data (columns 4-6), the total pass-through estimates are linear combinations of the October 2016 to January 2017 and the January 2017 to April 2017 estimates. Since the April 2016 to July 2016, and July 2016 to October 2016 estimates are only included in the equation as a means of testing for policy endogeneity, they are not included in the total pass-through.

Column 1 presents estimates using the Grubhub dataset. There was significant pass-through in the contemporaneous and lagged time periods. In total, prices increased by an estimated 0.82% due to a 10% increase in the minimum wage. Controlling for the lagged

Table 1.4: Main Price Pass-Through Results

	Grubhub (Monthly)			Yelp (Quarterly)		
	(1)	(2)	(3)	(4)	(5)	(6)
$Dec16 - Jan17$	0.259	0.260	0.244			
	(0.005)	(0.005)	(0.013)			
$Jan17 - Feb17$	0.228	0.231	0.295			
	(0.013)	(0.012)	(0.029)			
$Feb17 - Mar17$	0.187	0.190	0.189			
	(0.012)	(0.011)	(0.025)			
$Mar17 - Apr17$	0.147	0.149	0.130			
	(0.013)	(0.012)	(0.031)			
$Oct16 - Jan17$				0.163	0.208	0.604
				(0.056)	(0.083)	(0.317)
$Jan17 - Apr17$				0.150	0.180	0.464
				(0.060)	(0.096)	(0.353)
Total Pass Through	0.820	0.830	0.859	0.313	0.388	1.068
	(0.029)	(0.026)	(0.087)	(0.115)	(0.171)	(0.669)
N	8415	8415	3640	8805	5257	2099
NxT	33660	33660	14560	35220	21028	8396
Lagged Change Price		X				
Business Characteristics			X		X	
Changers						X

Notes: The outcome variable for all columns is the log change in price at the restaurant level. All standard errors (in parentheses) are clustered at the minimum wage group level. Each row represents the amount of pass-through occurring in the contemporaneous, lead, and lag time periods of the minimum wage changes. For the specifications using the Grubhub data, (1)-(3), all time periods are included in the total pass-through estimates. For the specifications using the Yelp data, (4)-(6), the total pass-through estimates are linear combinations of the October 2016 to January 2017 and the January 2017 to April 2017 estimates. Additional variables that are included, but not shown, are time and group fixed effects. Business characteristics are the control vector from the RUSA dataset, comprised of sales volume, number of employees, limited service status, and firm age. The sample size is smaller in column 2 than in column 1 (and in column 5 than in column 4) since not all restaurants are matched to the RUSA dataset. The pass-through estimates when using the subsample of restaurants that are matched to RUSA and without including the control vector are slightly smaller than the estimates in column 2. Changers are restaurants that updated the price of at least one menu item throughout the time period of data collection.

change in price does not change the estimates (column 2). Including the vector of control variables from the RUSA dataset (column 3) increases the pass through estimates slightly, but not significantly so, to 0.86%, indicating that sales volume, number of employees, franchise status and age are not substantially driving the estimates.[14]

Next, I examine the price pass-through using the Yelp data. As shown in column 4, there was significant pass-through in the contemporaneous and lag period, for a total pass-through estimate of 0.31%. Adding the vector of control variables provides a marginally higher pass-through rate. To focus analysis on restaurants that are updating menus and not posting outdates prices, column 5 restricts the full Yelp sample to only those restaurants who changed the price of at least one item throughout the course of the dataset. The total pass-through estimate for this subsample is larger at 1.07%. Although imprecisely measured, this estimate is not statistically distinguishable from the pass-through estimates provided by the Grubhub data. This comparison suggests that the full sample Yelp estimates may be a lower bound for the true pass-through, and support the use of the Grubhub data estimates. In addition, in all three of the Yelp specifications, the two lead period pass-through estimates are insignificant and relatively small in magnitude. This provides evidence against the presence of policy endogeneity within these minimum wage groups.

These main pass-through estimates are consistent with findings in the previous literature (e.g. Aaronson, French & MacDonald, 2008; Basker & Khan, 2016; Cooper, Luengo-

[14]The sample size is smaller in column 2 than in column 1 since not all restaurants are matched to the RUSA dataset. The pass-through estimates when using the subsample of restaurants that are matched to RUSA and without including the control vector are slightly smaller than the estimates in column 2.

Prado & Parker, 2017; Allegretto & Reich, 2018). In addition, these pass-through estimates are consistent with what is predicted by the textbook model of competitive factor markets and monopolistically competitive firms. Assuming that firms have a constant returns to scale production function, then an increase in the minimum wage will be proportionally passed on to consumers through output prices based on the minimum wage costs share of total costs. The minimum wage costs share of total costs is estimated to be between 4% and 10% in the restaurant industry (U.S. Department of Commerce, 2002; Aaronson & French, 2007). Taking these estimates as given, the model of monopolistically competitive firms and competitive factor markets then predicts that a 10% increase in the minimum wage would lead to a pass-through of 0.4% to 1.0%.

1.5.2 Heterogeneity in Pass-Through

I next investigate the heterogeneity of the price pass-through.[15] Table 1.5 reports heterogeneity by restaurant characteristics, including sales volume, number of employees, and customer rating. The total pass through results are also shown in Figure A1.4. All estimates are calculated using equation 1.1 and Grubhub data. Total pass-through estimates are linear combinations of all coefficients of interest. Columns 1 and 2 compare restaurants by highest and lowest third of sales volume. Although the low sales volume estimate is larger at 1.1%, it is only marginally different from the pass-through estimate of high sales restaurants, 0.54%. Columns 3 and 4 compare restaurants by the number of employees. The pass-through estimate of 0.85% for small restaurants is higher than the estimate of

[15] Heterogentiy effects are reported using the more precise Grubhub data, but the same general patterns are found using the Yelp data.

0.59% for large restaurants, but not significantly so. These suggestive results are consistent with Allegretto and Reich (2015) who found price pass-through to be larger in magnitude for restaurants with a small number of employees. Columns 5 and 6 compare restaurants by customer rating in December 2016, before any minimum wage changes. The estimated price pass-through for low rated restaurants is not distinguishable from that of high rated restaurants.

Table 1.5: Price Pass Through By Restaurant Characteristics

	(1) Low Sale	(2) High Sale	(3) Low Emp	(4) High Emp	(5) Low Qual	(6) High Qual
$Dec16 - Jan17$	0.275	0.255	0.257	0.243	0.360	0.236
	(0.029)	(0.008)	(0.024)	(0.012)	(0.021)	(0.022)
$Jan17 - Feb17$	0.362	0.103	0.284	0.113	0.171	0.217
	(0.054)	(0.048)	(0.042)	(0.074)	(0.043)	(0.018)
$Feb17 - Mar17$	0.297	0.088	0.207	0.109	0.170	0.249
	(0.061)	(0.049)	(0.037)	(0.057)	(0.037)	(0.017)
$Mar17 - Apr17$	0.148	0.097	0.106	0.124	0.192	0.161
	(0.055)	(0.092)	(0.035)	(0.109)	(0.034)	(0.015)
Total Pass Through	01.082	0.544	0.854	0.588	0.894	0.864
	(0.19)	(0.184)	(0.124)	(0.237)	(0.109)	(0.051)
N	1297	1167	1218	1061	2954	2518
NxT	5188	4668	4872	4244	11816	10072

Notes: The reported estimates compare price pass-through of restaurants in the lowest and highest third based on sales, employees, and customer rating in December 2016 using the Grubhub dataset. The outcome variable for all columns is the log change in price at the restaurant level. Standard errors are clustered at the minimum wage group level. The total pass-through estimates are linear combinations of all coefficients. Additional variables that are included, but not shown, are time and group fixed effects. Low sales restaurants are those firms in the lower third of sales volume at less than 190k. High sales restaurants are those firms in the higher third of sales volume at over 497k. Low employee restaurants are those in the lower third of number of employees with less than 3 employees, where high employee firms have over 9. Low quality firms are those that started with an average quality rating in the lower third of customer ratings with a rating of lower than 78.2. High quality firms are those who started with an average quality rating of over 94.6.

Given the unique nature of the dataset, I can also explore heterogeneity in price pass-through across item type. Table 1.6 reports pass-through results at the item level. I use equation 1.1, except now an observation is an item. The comparisons of total pass-through results are also depicted in Figure 1.3. Column 1 (of both panels) reports pass-through estimates at the item level for all items, with a total pass-through estimate of 0.51%. This estimate is smaller than the pass-through estimates reported when aggregating at the restaurant level. Analyzing pass-through at the item level assigns more weight to restaurants with a large number of items. In the data, total number of items offered on the menu is negatively correlated with the number of items that change price at any given time. In other words, restaurants with a relatively large number of items offered on the menu change prices for a smaller proportion of these items than restaurants with a relatively small number of items. Thus the item level pass-through estimates are expected to be lower than estimates aggregated at the restaurant level.

The subsequent columns of Table 1.6 report pass-through estimates by item categories (panel 1) and item ingredient type (panel 2). Overall, these results show that there are significant differences in the pass-through estimate across some item types. Popular items, however, have statistically similar price pass-through, indicating that restaurants are not using popular items as loss leaders. Sandwich and side items, on the other hand, show much higher pass-through, while appetizers and entrées show lower pass through. This heterogeneity indicates that studies which examine prices of only a few menu items may significantly under- or over estimate price pass-through. It also suggests that firms may understand the differences in price elasticity demand for different categories of items.

Table 1.6: Price Pass Through By Item Type

	(1) All	(2) Popular	(3) Appetizer	(4) Side	(5) Sandwich	(6) Pizza	(7) Soup/Salad	(8) Entre	(9) Dessert
$Dec16 - Jan17$	0.154	0.225	0.138	0.178	0.193	0.224	0.154	0.114	0.165
	(0.002)	(0.003)	(0.009)	(0.007)	(0.005)	(0.010)	(0.004)	(0.002)	(0.004)
$Jan17 - Feb17$	0.153	0.163	0.095	0.120	0.313	0.134	0.129	0.150	0.206
	(0.011)	(0.013)	(0.041)	(0.029)	(0.034)	(0.104)	(0.011)	(0.003)	(0.017)
$Feb17 - Mar17$	0.138	0.104	0.056	0.170	0.196	0.086	0.107	0.100	0.099
	(0.006)	(0.013)	(0.030)	(0.013)	(0.010)	(0.055)	(0.015)	(0.008)	(0.018)
$Mar17 - Apr17$	0.063	0.030	0.002	0.142	0.076	-0.042	0.070	0.064	0.043
	(0.010)	(0.028)	(0.024)	(0.030)	(0.029)	(0.041)	(0.025)	(0.019)	(0.031)
Total Pass Through	0.507	0.522	0.291^{+}	0.61	0.777^{+}	0.402	0.46	0.429^{+}	0.512
	(0.02)	(0.05)	(0.094)	(0.068)	(0.051)	(0.079)	(0.042)	(0.027)	(0.051)
N	1036350	26222	73256	109540	130354	39749	64290	183494	36352
NxT	4145400	104888	293024	438160	521416	158996	257160	733976	145408

	All	Drink	Chicken	Beef	Pork	Fried	Organic	Natural	Gluten Free
$Dec16 - Jan17$	0.154	0.164	0.146	0.118	0.118	0.137	0.157	0.237	0.163
	(0.002)	(0.002)	(0.005)	(0.005)	(0.010)	(0.010)	(0.029)	(0.006)	(0.082)
$Jan17 - Feb17$	0.153	0.185	0.159	0.129	0.122	0.099	1.640	0.479	0.527
	(0.011)	(0.023)	(0.010)	(0.010)	(0.022)	(0.025)	(0.463)	(0.089)	(0.392)
$Feb17 - Mar17$	0.138	0.126	0.142	0.220	0.177	0.169	-0.254	0.049	0.231
	(0.006)	(0.007)	(0.015)	(0.013)	(0.014)	(0.019)	(0.400)	(0.087)	(0.302)
$Mar17 - Apr17$	0.063	0.008	0.088	0.068	0.078	0.070	0.028	0.289	0.368
	(0.010)	(0.011)	(0.023)	(0.025)	(0.042)	(0.030)	(0.047)	(0.052)	(0.341)
Total Pass Through	0.507	0.482	0.534	0.534	0.494	0.475	1.571	1.055^{+}	1.29
	(0.02)	(0.021)	(0.041)	(0.045)	(0.074)	(0.073)	(0.858)	(0.094)	(1.029)
N	1036350	78293	176873.25	56812.25	46078.5	81928.25	3134.25	2249	6302.5
NxT	4145400	313172	707493	227249	184314	327713	12537	8996	25210

+ statistically different than column (1)

Notes: The outcome variable for all columns is the log change in price at the item level using the Grubhub dataset. All standard errors are clustered at the minimum wage group level. All time periods are included in the total pass-through estimates. Additional variables that are included, but not shown, are time and group fixed effects. The item categories in the top panel are mutually exclusive but not exhaustive. The items by ingredient type in the bottom panel are not mutually exclusive.

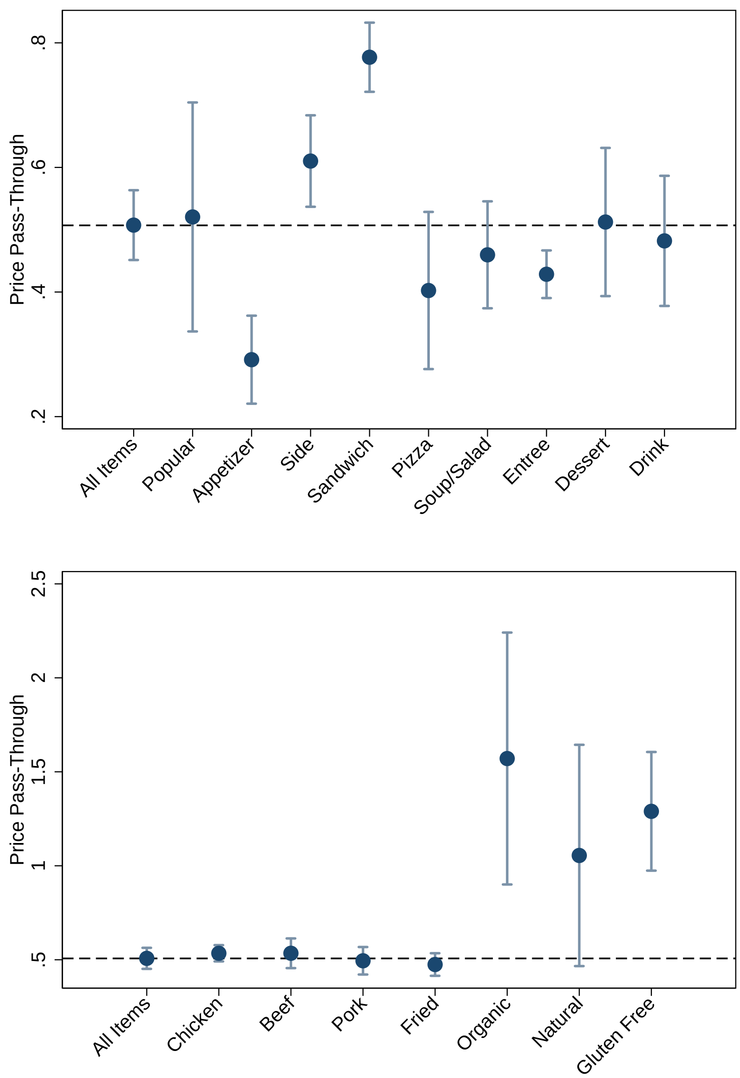

Figure 1.3: Pass-Through By Item Type

Notes: Point estimates and 95% confidence intervals of the total price pass-through using the Grubhub data are depicted by restaurant characteristics. The top panel depicts estimates by the item category listed on the Grubhub menus. The bottom panel depicts estimates by the type of ingredient that is reported in the item name or item description. The horizontal dotted line in each panel represents the pass-through estimate for all items. The y-axis ranges are different between the top and bottom panels to account for the high estimates and large standard errors for the pass-through estimates by ingredient type in the bottom panel.

Although I observe all of these restaurants across the same time period, it is possible that there were changes in local food supply prices that were not constant across the different minimum wage groups. However, I find no difference in pass-through across items with chicken, beef, or pork, suggesting that local changes in the input costs for these are not a driving force of the pass-through estimates. Interestingly, items with the words “organic”, “natural”, or “gluten-free” in the item name or description show much higher levels of pass-through. This suggests that firms recognize a lower price elasticity demand for these healthier items. These are the first estimates of differing magnitudes of pass-through by item type and ingredients in the literature.

1.5.3 Border Effects

To examine the existence of border effects, I restrict the sample to restaurants that are located within twelve minutes of the NYC-NJ border.[16] The specification I use to test the existence of border effects is

$$\begin{aligned}\Delta \ln(p_{j,t_0-t_T}) =& \alpha_0 + \alpha_1 \mathbb{1}(NY=1) \\ &+ \alpha_2 D_j + \alpha_3[D_j * \mathbb{1}(NY=1)] + \varepsilon_j,\end{aligned} \tag{1.2}$$

where $\ln(p_{j,t_0-t_T})$ is the log change in price from the time period before implementation (December 2016 for the Grubhub restaurants and October 2016 for the Yelp restaurants) to the final wave of data collection. $\mathbb{1}(NY=1)$ is an indicator function denoting if restaurant j is located in NYC, and D_j is the driving distance in minutes to the closest restaurant on

[16]The results are persistent further out from the border, but smaller in magnitude.

the opposite side of the border.[17] Due to the Hudson River which separates NYC and NJ, the shortest distance between two restaurants on opposite sides of the border is 8 minutes. In the data there are no significant relationships between distance to the border and sales volume, number of employees or limited service status. I assume that there are no other unobserved variables that are related to both the distance to the border and the change in price.

For restaurants in NYC, the equation becomes

$$\Delta \ln(p_{j,t_0-t_T}) = (\alpha_0 + \alpha_1) + (\alpha_2 + \alpha_3)D_j + \varepsilon_j.$$

The coefficient $\alpha_2 + \alpha_3$ describes the relationship between distance to the border and price increase for restaurants in NYC. For a restaurant in NJ, the equation becomes

$$\Delta \ln(p_{j,t_0-t_T}) = (\alpha_0) + (\alpha_2)D_j + \varepsilon_j.$$

The coefficient α_2 describes the relationship between distance to the border and price increase for restaurants in NJ close to the NYC border. Figure 1.4 displays a binned scatter plot of the relationship between distance to the NYC-NJ border and price increase.

I report the results of equation 1.2 in Table 1.7. Column 1 reports the estimated relationship between price changes from December 2016 to April 2017 and the distance to the border for restaurants on the NYC-NJ border in the Grubhub dataset. The estimates show that a restaurant 10 minutes further from the border increases prices by 0.18 percentage points ($\alpha_2 + \alpha_3$) more than a restaurant on the border. No significant border effects are reported for NJ restaurants (α_2).

[17] I selected the linear specification base on the AIC and BIC values in comparison to the quadratic specification.

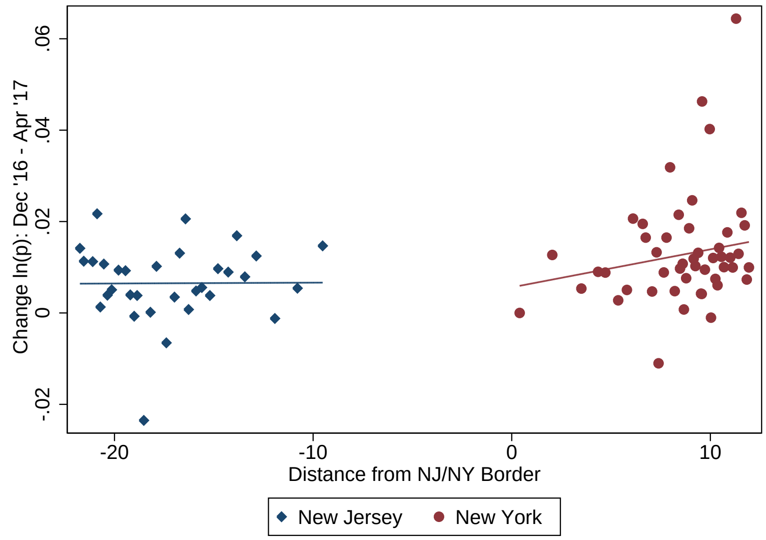

Figure 1.4: Border Effects: Price Pass Through by Distance to NYC/NJ Border

Notes: The figure shows the relationship between the change in price from December 2016 to April 2017 and the distance to the NYC-NJ border using the Grubhub data. Restaurants are binned into 80 quantiles. There is a gap between the NYC and NJ restaurants due to the Hudson River which separates the two states. Distance is measured in driving minutes to the nearest restaurant on the opposite side of the border.

Column 2 reports the results with the Yelp data, estimating a border effect of 0.11 percentage points for restaurants in NY between October 2016 and April 2017. Once again, no border effects are present for NJ. As a falsification test, I estimate this same relationship with the Yelp data but for the change in price prior to policy implementation. Column 3 reports this relationship for a price change from April 2016 to October 2016. This estimate is not significant, suggesting that these border effects are not always present and can be attributed to the minimum wage changes. The average change in price from December 2016 to April 2017 for all NYC Grubhub restaurants was 1.3%, and the average change in price from October 2016 to April 2017 for all Yelp restaurants in NYC was 0.81%. Thus

Table 1.7: Border Effects

Source	Grubhub	Yelp	
	(1)	(2)	(3)
Time Frame	Dec16-Apr17	Oct16-Apr17	Apr16-Oct16
$\mathbb{1}(NY)$ (α_1)	-1.019	0.641	-0.636
	(1.244)	(1.525)	(1.161)
Distance (α_2)	-0.003	-0.088	0.003
	(0.057)	(0.076)	(0.058)
Distance * $\mathbb{1}(NY)$ (α_3)	0.187	0.196	0.066
	(0.090)	(0.112)	(0.085)
Constant (α_0)	0.592	-0.687	0.412
	(1.005)	(1.336)	(1.017)
$\alpha_2 + \alpha_3$	0.1843	0.108	0.0691
	(0.0701)	(0.0826)	(0.0629)
N	694	1002	1002

Notes: The outcome variable is the percentage point change in price due to a 10 minute change in the distance of a restaurant to the border. The first column includes restaurants in the Grubhub dataset within twelve minutes of the NYC - NJ border between December 2016 and April 2017. Column 2 includes restaurants in the Yelp dataset within twelve minutes of the NYC - NJ border between October 2016 and April 2017. Column 3 includes these same restaurants but using the change in price from July 2016 to October 2016 as the outcome.

the estimated border effects are relatively large in magnitude, comprising more than 10% of the total price increase seen during this time period.

1.6 Quality Responses

I now examine changes in quality as another margin of response to increases in the minimum wage. To investigate this relationship, I use the following equation to analyze the effect of a minimum wage increase on the customer rating of restaurant j in the minimum wage group k at observation wave t:

$$\Delta \ln(rating_{jkt}) = \alpha + \sum_{h=l}^{L} \beta_h \Delta \ln(mw_{kt-h}) + \gamma p_{j,t=0} + \eta_k + \lambda_t + \varepsilon_{jkt} \quad (1.3)$$

The variable $rating_{jkt}$ is the customer rating of each establishment. As in equation 1.1, I include contemporaneous and lag terms for changes in the minimum wage to allow for a flexible response. In the primary specification using the Grubhub data, I include no lead terms and three lag terms ($l = 0, L = 3$). In the specifications that use the Yelp data, I also include two lead terms ($l = -2, L = 1$) as a check for any pre-policy change relationships between minimum wage group and changes in quality.

The term $p_{j,t=0}$, the average price at an individual restaurant in the initial period before a minimum wage increase, is included as a control. The average price of items at a restaurant may provide customers with an expectation of what the level of quality should be. These ex-ante ideas of quality level could have an impact on changes in customer-perceived quality. Fixed effects for observation time period and minimum wage group are included in all specifications. I assume that there are no other unobserved characteristics of restaurants that influence both the minimum wage group that a restaurant belongs to and changes in customer-perceived quality.[18]

The results of equation 1.3 are presented in Table 1.8. All estimates are interpreted as the percent change in Grubhub customer rating due to a 10% increase in the minimum wage, and are a linear summation of the contemporaneous and lagged time periods. This term can be interpreted as the "pass-through" of quality due to a 10% increase in the min-

[18] I find no systematic change in the number of reviews that a restaurant receives in relationship to the minimum wage, supporting this claim.

imum wage. Column 1 presents the total change in rating for all restaurants due to a 10%

Table 1.8: Grubhub Quality Changes by Initial Quality Rating

	(1) All	(2) All	(3) $<=$ Median	(4) $>$ Median
Total % Change in Rating: Average	-0.433	-0.435	-0.985	0.055
	(0.001)	(0.001)	(0.003)	(0.011)
Total % Change in Rating: Food	-0.292	-0.288	-0.842	-0.024
	(0.001)	(0.002)	(0.01)	(0.004)
Total % Change in Rating: Accurate	-0.334	-0.338	-0.932	0.255
	(0.002)	(0.002)	(0.017)	(0.008)
Total % Change in Rating: Delivery	-0.654	-0.661	-1.53	0.532
	(0.001)	(0.004)	(0.007)	(0.013)
N	7843	7843	4044	3799
NxT	31372	31372	16176	15196
Change in Price		X		

Notes: The outcome variable for all columns is the total log change in Grubhub quality rating. All standard errors are clustered at the minimum wage group level. The average rating is the average of the food, accuracy, and delivery ratings. Each measure is on a scale from 0-100. Column (2) includes controls for the changes in price. Column (3) restricts the sample to restaurants starting at or below the median quality rating in December 2016. Column (4) restricts the sample to restaurants starting above the median quality rating in December 2016. The median average rating is 90.3, the median food rating is 89, the median accuracy rating is 93, and the median delivery rating is 90. Additional variables that are included, but not shown, are time and group fixed effects.

increase in the minimum wage along four different measures: average quality, food quality, order accuracy, and delivery time. On all accounts, a minimum wage increase is associated with a significant decrease in customer-perceived quality. On average, the effect is estimated at -0.43%. These effects are larger (more negative) for delivery time, and smaller (less negative) for food quality. Controlling for changes in price (column 2) does not significantly change these estimates, suggesting that the changes in quality are not driven by customer dissatisfaction directed at price increases.

To investigate differences in the response to a minimum wage increase by level of initial quality, I partition the sample by starting quality rating.[19] The effects for those restaurants that start at or below the median quality rating is reported in column 3. The average decrease in quality is more severe for these restaurants at -0.99% on average. On the other hand, the average relationship for restaurants that started above the median rating (column 4) is 0.06%. These estimates are economically significant. For example, the average effects for high quality firms represent an increase in quality equal to 3% of the average increase. Further, the average effects for a low quality firm represent a decrease of 40% of the average decrease. In other words, these are relatively small changes in quality, but the minimum wage is driving a relatively large portion of the changes.

Table 1.9 displays the results of the quality responses for the Yelp restaurants. The total percent change in rating is a linear summation of the estimated coefficients from October 2016 to January 2017, and Jan 2017 to Apr 2017. The estimates from April 2016 to July 2016 and July 2016 to October 2016 are not included in the total effect, but provide evidence of the pre-minimum wage relationships. The first column of the table reports estimates using all Yelp restaurants that had a star rating between 2.5 and 4.5, inclusive,

[19]Note that initial quality rating is different than the outcome variable which is the change in rating. This methodology allows for analysis of changes in quality based on initial quality, in contrast to a quantile regression method which would not provide estimates that are based on initial quality.

Table 1.9: Overall Yelp Quality Changes by Initial Star Rating

	(1) All	(2) 2.5	(3) 3.0	(4) 3.5	(5) 4.0	(6) 4.5
$Apr16 - Jul16$	0.035	1.138	0.625	0.068	-1.047	-0.627
	(0.213)	(1.201)	(0.697)	(0.187)	(0.312)	(0.456)
$Jul16 - Oct16$	-0.046	-0.321	-0.305	0.109	0.198	-0.792
	(0.071)	(0.292)	(0.400)	(0.211)	(0.296)	(0.413)
$Oct16 - Jan17$	0.049	0.255	-1.341	-0.268	0.500	0.470
	(0.036)	(1.068)	(0.178)	(0.076)	(0.090)	(0.275)
$Jan17 - Apr17$	-0.416	-1.158	-0.546	-0.728	-0.252	-0.255
	(0.167)	(0.609)	(0.670)	(0.197)	(0.080)	(0.175)
Total % Change Stars	-0.368	-0.903	-1.887	-0.996	0.248	0.215
	(0.138)	(1.667)	(0.813)	(0.161)	(0.123)	(0.164)
N	6392	625	1080	1801	1904	982
NxT	25568	2500	4320	7204	7616	3928

Notes: The outcome variable for all columns is the log change in Yelp star rating. All standard errors are clustered at the minimum wage group level. The total percent change in stars estimates are linear combinations of the October 2016 to January 2017 and the January 2017 to April 2017 estimates. The initial star ratings are the rounded Yelp star ratings in April 2016. Restaurants below a 2.5 rating and above a 4.5 rating are not analyzed as subsamples given that they are close to the lower and upper bounds, respectively, and so only have one direction to move. Additional variables that are included, but not shown, are time and group fixed effects.

in each wave of data.[20] The total change in quality rating due to a 10% minimum wage increase is estimated at -0.4%.

Column 2 reports the estimated change in star rating due to a minimum wage increase for restaurants that had a 2.5 rating in April 2016. The estimated relationship is more negative than the full sample at -0.9%, but imprecisely estimated. Column 3 reports the estimated relationship for restaurants that started at a 3.0 star rating. For these restaurants,

[20]Restaurants below a 2.5 rating and above a 4.5 rating are not analyzed as subsamples given that they are close to the lower and upper bounds of quality ratings, respectively, and so only have one direction to move. Over 90% of the restaurants with star ratings fall within this 2.5 - 4.5 range.

a 10% increase in the minimum wage is associated with a 1.9% decrease in star rating. Column 4 reports estimates for restaurants that began with a 3.5 star rating, the median in the sample. The estimated relationship for these restaurants is -1.0%. As seen in columns 5 and 6, the estimated relationship for restaurants that started at a 4.0 or 4.5 star rating, however, is significantly positive. Restaurants starting at a 4.0 rating saw a 0.3% increase in star rating due to a 10% increase in the minimum wage, and restaurants starting at a 4.5 rating saw a 0.2% increase in star rating.

To further investigate potential mechanisms for these quality results, I also performed sentiment analysis on the text of the Yelp customer reviews, looking for patterns of dissatisfaction or approval aimed specifically at service quality or food quality. I find a similar heterogeneous response to the overall quality effects between lower and higher quality firms for service specific reviews, but not for food specific reviews. Although the small sample size of this supplementary analysis yields imprecise estimates, the evidence suggests that the quality responses to increases in the minimum wage are driven more so by changes in service than by changes in food quality. These results are presented in the appendix.

1.7 Additional Margins of Response

In addition to price and quality, there are three other potential margins of adjustment that can be investigated using this dataset. The first variable is the total number of menu items at each point in time. This variable is of interest since I use a balanced panel in the main results, as well as that restaurants could decrease the total number of items offered to decrease menu costs.

Table 1.10: Change in Number of Items Offered and Hours of Business

	Grubhub		Yelp	
	(1)	(2)	(3)	(4)
	Items	Hours	Items	Hours
$Dec16 - Jan17$	0.014	-1.473		
	(0.044)	(0.582)		
$Jan17 - Feb17$	0.127	0.462		
	(0.122)	(1.558)		
$Feb17 - Mar17$	0.201	0.836		
	(0.116)	(1.558)		
$Mar17 - Apr17$	0.165	0.407		
	(0.311)	(1.558)		
$Apr16 - Jul16$			0.888	-0.086
			(0.370)	(0.021)
$Jul16 - Oct16$			1.083	-0.071
			(0.459)	(0.032)
$Oct16 - Jan17$			1.060	-0.032
			(0.316)	(0.010)
$Jan17 - Apr17$			0.964	-0.039
			(0.398)	(0.021)
Total Pass Through	0.508	0.233	02.024	-0.071
	(0.495)	(03.578)	(0.700)	(0.031)
N	8415	8405	8807	6920.5
NxT	33660	33620	35228	27682

Notes: The dependent variables are the percent change in total items offered (columns (1) and (3)) and the percent change in hours open per week (columns (2) and (4)) due to a 10% increase in the minimum wage. Columns (1) and (2) show these results using the Grubhub data and columns (3) and (4) estimate the relationships using the Yelp data. All standard errors are clustered at the minimum wage group level. Additional variables that are included, but not shown, are time and group fixed effects.

Columns 1 and 3 in Table 1.10 report changes in the total number of items on the menu over time as an outcome variable using equation 1.1 for Grubhub and Yelp restaurants, respectively. Total percent change is a linear combination of the relevant time periods, and can be interpreted as the percent change in items offered on a menu due to a 10% increase in the minimum wage. These estimates are positive, but only marginally significant within the Yelp restaurants. Further, the lead period estimates are similar in magnitude to the post implementation estimates, suggesting that the positive relationship is not driven by changes in the minimum wage. I therefore find no conclusive evidence that restaurants significantly alter the total number of items offered on a menu after an increase in the minimum wage.[21]

The second additional variable is hours of operation, as firms could decrease labor costs by reducing the number of hours the firm is open, and therefore the amount of time paying hourly workers. Columns 2 and 4 report changes in the total number of hours open per week due to a minimum wage increase using equation 1.1. Total percent change is a linear combination of the relevant time periods, and can be interpreted as the percent change in hours open per week due to a 10% increase in the minimum wage. These results are slightly positive using the Grubhub data and slightly negative for the Yelp data but are both imprecisely measured.

A third additional margin of adjustment to the labor cost shock is exit from the mar-

[21]To address the possibility that this net effect could be masking some increases and some decreases across different types of items, I partition the sample by item category and ingredients. I once again find no evidence that restaurants are systematically changing the number of certain types of items offered.

ket. Given the nature of the data and that the data was collected online, I cannot guarantee that a restaurant that exits the sample did indeed exit the market all together. As discussed in Section 3, reasons that a restaurant fails to to be in all waves of data collection include closures, name changes, discontinued use of the online service menu, or technical errors.

Regardless of the reason for a restaurant to drop out of my sample, I first use the Yelp data to address how not including these restaurants that dropped out of the sample affects the pass-through estimates. I estimate the price pass-through from October 2016 to January 2017 using all Yelp restaurants that remained in the panel through January 2017. I then estimate this same price pass-through between October 2016 and January 2017 but using only the restaurants that remained in the final balanced panel through April 2017. I find the two estimates to be statistically indistinguishable, although including the restaurants that eventually dropped out of the panel before April 2017 yields slightly higher pass-through estimates. This suggests that only including restaurants that remain in the balanced sample throughout the time period of the dataset does not bias my results, and if anything would lead the results to be a lower bound of the true pass-through.

I next assume that, with the exception of exit from the market, the reasons that a restaurant fails to remain in the balanced sample are uncorrelated with increases in the minimum wage, and estimate the relationship between the probability of exit from my sample and increases in the minimum wage. Figure 1.5 displays the hazard function by minimum wage group for exit from the sample using the Grubhub data.[22] This figure indicates that the exit rate of restaurants from the sample is related to the increases in the

[22]Appendix Figure A1.5 displays the hazard functions for the Yelp data.

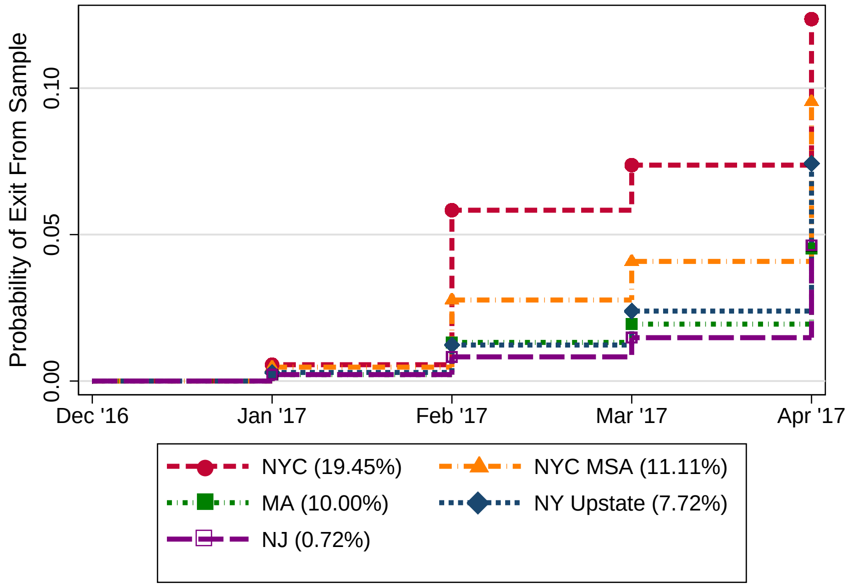

Figure 1.5: Hazard Functions for Exit From the Sample

Notes: The Kaplan Meier hazard functions for exit from the sample are depicted by minimum wage group and using the Grubhub sample. The percent increase in the minimum wage faced by each group in January 2017 is shown in parenthesis in the legend. The χ^2 value of the log-rank test for equality of these survivor functions across groups is 367.81 and the corresponding P value is 0.000.

minimum wage. I further estimate the relationship between exit from the sample and the minimum wage increase using the equation

$$Exit_{jkt} = \beta \Delta \ln(mw_k) + \eta_k + \lambda_t + \varepsilon_{jkt}, \tag{1.4}$$

where $Exit_{jkt}$ is a binary variable denoting whether restaurant j in the minimum wage group k in time t has exited the sample. $\Delta \ln(mw_k)$ is the total change in the minimum wage that a restaurant faced in January 2017. I include time fixed effects as the probability of exit increases with time for all firms.

Table 1.11 reports the results. The baseline specification using the Grubhub data, shown in column 1, indicates that a 10% increase in the minimum wage increases the probability that a firm exits by 1.3 percentage points.[23] Including the vector of RUSA

Table 1.11: Probability of Exit from the Sample: Grubhub

	(1)	(2)	(3)	(4)
$\Delta(ln(mw))$	0.013	0.012	0.023	0.047
	(0.005)	(0.004)	(0.003)	(0.014)
$Quality$			-0.006	-0.005
			(0.001)	(0.001)
$\Delta(ln(mw)) \times Quality$				-0.003
				(0.002)
N	9841	4148	9575	9575
$N \times T$	49205	20740	47875	47875
Business Characteristics		X		

Notes: Coefficients are reported in percentage point changes in the probability of exit from the sample due to a 10% increase in the minimum wage. The quality measure is the starting average customer rating of the establishment in December 2016. Business characteristics are the control vector from the RUSA dataset, comprised of sales volume, number of employees, limited service status, and firm age. Additional variables that are included, but not shown, are time and group fixed effects.

control variables (sales volume, number of employees, franchise status and firm age) does not significantly alter the estimates (column 2). Including the starting measure of average quality, as shown in column 3, indicates that higher rated restaurants are less likely to exit, as would be expected.

Table 1.12 reports the probability of exit from the sample using the Yelp data. Since

[23]The average probability of exit for restaurants in NJ over the course of the Grubhub dataset was 1.5%. Assuming that this is the baseline probability of exit, an increase of 1.3 percentage points translates to an 86.7% increase in the probability of exit due to a 10% increase in the minimum wage.

there are multiple pre-policy implementation observations, I add an interaction term between $\Delta\ln(mw_k)$ and an indicator variable for if the observation occurred after policy implementation, $Post_t$. In this specification, the $\Delta\ln(mw_k)$ term on its own indicates whether

Table 1.12: Probability of Exit from the Sample: Yelp

	(1)	(2)	(3)	(4)
$\Delta(ln(mw))$	0.007	0.014	0.035	0.023
	(0.006)	(0.007)	(0.008)	(0.005)
$\Delta(ln(mw)) \times Post$	0.065	0.078	0.094	0.130
	(0.014)	(0.012)	(0.018)	(0.019)
$Quality$			-0.015	-0.008
			(0.003)	(0.005)
$\Delta(ln(mw)) \times Quality$				-0.000
				(0.003)
$\Delta(ln(mw)) \times Quality \times Post$				-0.014
				(0.003)
N	14275	8424	13379	13379
$N \times T$	71375	42120	66895	66895
Business Characteristics		X		

Notes: Coefficients are reported in percentage point changes in the probability of exit from the sample due to a 10% increase in the minimum wage. The quality measure is the starting star rating of the establishment in October 2016. $Post$ is a binary variable denoting that the time period of data collection was after the minimum wage policy implementations. Business characteristics are the control vector from the RUSA dataset, comprised of sales volume, number of employees, limited service status, and firm age. Additional variables that are included, but not shown, are time and group fixed effects.

or not the probability of exit from the sample is related to the January 2017 minimum wage increases but before implementation. As can be seen in all specifications, the probability of exit in relationship to the increase in the minimum wage is smaller and imprecisely measured compared to the interaction of the minimum wage increase and post implementation. The baseline specification with the Yelp data (column 5) estimates that a 10% increase in the minimum wage increases the probability of exit by 6.5 percentage points.[24] Adding the vector of business control variables does not significantly change these estimates.

Column 7 indicates that lower quality restaurants are more likely to exit. Interacting this quality measure with increases in the minimum wage as well as post implementation provides support that after the increases in the minimum wage (but not before), lower quality restaurants facing an increase in the minimum wage were even more likely to exit the sample. These results are similar to that found in Luca and Luca (2018), and support the conclusion that firms respond differently to a minimum wage depending on initial quality. Although these estimates use exit from the sample as a proxy for exit from the market, they provide suggestive evidence that an increase in the minimum wage causes firms to exit the market at a higher likelihood, and that this response is stronger for lower quality firms.

1.8 Discussion and Conclusion

In this paper, I investigate the responses of restaurants to increases in the minimum wage. I take advantage of a series of simultaneous minimum wage increases and the growing online presence of restaurants to investigate heterogeneity in price pass-through across restaurant characteristics and item type and changes in customer-perceived quality. I find that prices increase between 0.3 to 0.8% in response to a 10% increase in the mini-

[24]The average probability of exit for restaurants in NJ over the course of the Yelp dataset was 15.9%. Assuming that this is the baseline probability of exit, an increase of 6.5 percentage points translates to a 40.8% increase in the probability of exit due to a 10% increase in the minimum wage.

mum wage, results that are consistent with previous literature. Since the data I use in this study are primarily non-chain and full service restaurants, these results are not being driven by large franchises and limited service establishments. The price pass-through estimates differ across restaurant characteristics, with the estimates marginally higher for smaller restaurants.

Unlike traditional administrative or government datasets, the data used in this study provide granular restaurant and menu data at the item level. I find that the magnitude of the price pass-through varies significantly at the item category level, with lower pass-through among items such as appetizers and entrées, but higher pass-through among and sandwich items. These results suggest that studies which examine prices of only a few menu items may significantly over- or underestimate overall price pass-through. Popular items, however, have statistically similar price pass-through to all other items, indicating that restaurants are not using popular items as loss leaders. This also suggest that there is higher demand for these items but not necessarily lower price elasticity of demand. In contrast, items with specific indicators of health, including "organic" or "gluten-free," have significantly higher pass-through. This indicates that firms may recognize the lower price demand elasticity for these items.

Further, I examine the extent to which a restaurant's proximity to a minimum wage policy border affects the level of price pass-through. I find that restaurants close to a minimum wage policy border increase prices by significantly less than restaurants further from the border. These border effect estimates have significant economic implications, suggesting that a local minimum wage increase may impede the ability of restaurants on a minimum wage policy border to fully pass-through prices to consumers.

I provide the first estimates in the literature of restaurant quality changes as another margin in which firms react to labor cost shocks, finding a heterogeneous effect of an increase in the minimum wage on restaurant quality. Restaurants that were rated at the median or below prior to the minimum wage increase saw a significant decrease in the quality rating given to them by consumers after an increase in the minimum wage. Restaurants that started at ratings above the median saw a positive effect on their consumer ratings due to the increase in the minimum wage. The results are consistent over two different measures of customer-perceived quality. These results suggest that lower quality firms may decrease output quality in response to a minimum wage increase. However, a minimum wage may act as an efficiency wage in higher quality restaurants, or higher quality restaurants may be able to substitute toward higher quality workers.

The unique nature of the dataset allow me to investigate three other potential margins of response to an increase in the minimum wage. I find no significant changes in the number of menu items offered or the types of items offered in relationship to an increase in the minimum wage. Similarly, I find no evidence that firms decreased open hours of business as a margin of response to the increases in the minimum wage. Using exit from the sample as a proxy for exit from the market, I find a significant relationship between a minimum wage increase and exit. Specifically, I find that a firm is 1.3 percentage points more likely to exit due to a 10% increase in the minimum wage, and that these effects are larger for lower quality firms. Overall, these data have provided the opportunity to look further into the effects of a minimum wage increase on restaurants and have brought about new areas of exploration for further research.

CHAPTER 2
THE PASS-THROUGH OF THE LARGEST TAX ON SUGAR-SWEETENED BEVERAGES: THE CASE OF BOULDER, COLORADO

2.1 Introduction

[1]The incidence of taxes is a classic topic in public finance. Economic theory indicates that the relative burdens of a tax are determined by the market power of firms and the elasticities of supply and demand (Kotlikoff & Summers, 1987; Fullerton & Metcalf, 2002; Weyl and Fabinger, 2013). For example, in a perfectly competitive market, if demand is completely inelastic or if firms face constant marginal costs, pass-through would be 100 percent and consumers would bear the entire burden of the tax. If the market is imperfectly competitive, taxes can be overshifted (price may rise by more than the tax) if oligopolists find it optimal to reduce output and charge higher prices in response (Anderson, de Palma, & Kreider, 2001; Bonnet & Requillart, 2013). Numerous studies have estimated the pass-through of taxes on products such as cigarettes and gasoline.[2]

We estimate the pass-through of a relatively novel tax on sugar-sweetened beverages (SSBs). Numerous organizations, such as the World Health Organization, Institute of Medicine, American Academy of Pediatrics, and the American Public Health Association, have called for taxes on SSBs because SSBs contribute to obesity and poor health (Rudd Center for Food Policy and Obesity, 2014). In addition to being high-calorie and

[1]This chapter is joint work with John Cawley, David Frisvold, and David Jones.

[2]Empirical estimates of excise taxes on alcohol, clothing, cigarettes, and gasoline often find that 100 percent or more of the taxes are passed through to consumers (e.g., Besley & Rosen, 1999; Poterba, 1996). A smaller body of literature finds partial pass-through, in the range of 45 to 85 percent (e.g., Doyle & Samphantharak, 2008; Harding et al., 2012).

zero-nutrient, SSBs have a high glycemic load (i.e., they significantly raise blood sugar), which, independently of obesity, contributes to insulin resistance and diabetes (Malik & Hu, 2011).

Many countries recently implemented taxes on SSBs, including Australia, Denmark, Finland, France, Ireland, Mexico, and the United Kingdom (Thow et al., 2018). Within the U.S., several cities have adopted taxes on SSBs: first Berkeley, CA, in 2015; followed by Philadelphia, Boulder, and Oakland in 2017; and San Francisco and Seattle in 2018.[3] All of these city-level taxes are imposed on beverage distributors who sell to retailers.

Given the relative newness of the taxes, their effects are not well understood.[4] Comparing changes in prices in Berkeley relative to those in control cities such as San Francisco, both Falbe et al. (2015) and Cawley and Frisvold (2017) estimated that 43-47 percent of the Berkeley tax was passed on to consumers, and the 95 percent confidence intervals rule out full pass-through of the tax. Cawley, Willage, and Frisvold (2017) examine the tax in Philadelphia within the Philadelphia airport, which straddles the city border; thus, some terminals are taxed and others are untaxed. Within the terminals in Philadelphia, the pass-through rate was 93 percent. In response, some stores in the untaxed terminals raised prices by the amount of the tax.

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[3]Many states also impose sales taxes on soft drinks, although they are very small, are primarily a tool to increase revenue, and apply to diet as well as caloric soft drinks (Fletcher, Frisvold, and Tefft, 2010, 2015).

[4]There is also a literature examining the impact of SSB taxes outside of the U.S. Several studies find that more than 100 percent of the SSB tax in Mexico was passed through to consumers, although the studies lack geographic control groups and rely on pre-post comparisons and comparisons to untaxed non-substitute products (Colchero et al., 2015; Grogger, 2017).

We contribute to this early literature on the pass-through of taxes on SSBs. Specifically, this paper is the first to estimate the pass-through of the largest city-level tax on SSBs to date, which is the tax of 2 cents per ounce in Boulder, CO that was implemented on July 1, 2017.[5] Boulder's tax on SSBs is substantial; it represents 22 percent of the pretax price of a 20-ounce bottle, 68 percent of the pretax price of a 2-liter bottle, and 53 percent of the pretax price of a 12-pack of 12-ounce cans.[6] Thus, its impact on retail prices may be different from that of the smaller taxes of 1 cent per ounce in Berkeley and 1.5 cents per ounce in Philadelphia. In addition, pass-through may differ across cities because of differences in the elasticities of supply and demand for SSBs, or the competitiveness of the local retail markets.

Another important strength of the paper is its rich and varied data. We collected data in person from stores in Boulder and two control communities in multiple periods before and after the tax. After the tax, we recorded posted (shelf) prices and purchased a taxed and untaxed beverage. The tax was levied on beverage distributors, in part, because excise taxes are more salient and, thus, more likely to reduce consumption (Chetty, Looney, Kroft, 2009). However, we find that not all retailers included the tax in the posted, or shelf, prices; some instead added it at the register, where it is less salient.

[5]The tax in Boulder passed by ballot initiative in November 2016, with 54 percent of voters in favor of the tax. It is an excise tax on distributors and took effect on July 1, 2017. The tax applies to SSBs with at least 5 grams of caloric sweetener per 12 fluid ounces. It does not apply to diet soda, products in which milk is the primary ingredient, alcoholic mixers, or coffee drinks. The tax is applied to the size of the prepared product; for example, the tax on the syrup used to prepare a 32 ounce fountain drink is 64 cents.

[6]These percentages were calculated using the mean price of SSBs in Boulder in April 2017, according to our hand-collected store data.

We additionally collected price data in person from restaurants in the same communities because restaurants are important points of purchase of SSBs, and the elasticity of supply of SSBs may differ between restaurants and stores, resulting in a different level of pass-through. Finally, we collected weekly data from online menus in these communities. Other strengths of the data include information about the prices of a wide range of taxed products: various sizes (e.g., 20 ounce and 2-liter bottles), various containers (bottles, cans, and fountain drinks), and a wide range of brands and products.

We estimate the pass-through of the SSB tax to consumers using a difference-in-differences design, comparing the changes in prices per ounce over time in Boulder to two comparison areas. We estimate that the tax increased prices immediately after its implementation on July 1, 2017 and that this increase remained relatively constant for the next four months. The posted prices increased by 1.1 cents per ounce on average, a 53.2 percent pass-through rate. However, twenty percent of the stores in Boulder do not include the tax in their posted prices but instead add it at the register. As a result, pass-through is larger when measured by the register prices: 1.6 cents per ounce, or 79 percent of the tax.

2.2 Methods

To estimate the pass-through of the SSB tax to retail prices, we use a difference-in-differences design, comparing the change in prices (in cents per ounce) over time in Boulder to that in the control communities of Boulder County (minus the city of Boulder) and Fort Collins, CO. In our primary specification, based on data from all retail stores and restaurants with two pre-tax periods (April and June) and two periods after the tax was introduced (August and October) that we collected in-person, we estimate:

$$Y_{isct} = \beta_0 + \beta_1(Boulder_c \times April_t) + \beta_2(Boulder_c \times August_t) + \beta_3(Boulder_c \times October_t) + \gamma_c + \delta_t + \theta_s + \psi_i + \epsilon_{isct}\,, \quad (2.1)$$

where Y_{isct} denotes the price per ounce of product i in store s in community c in month t; $Boulder$ is a binary variable equal to one if store s is located in the City of Boulder (and 0 if the store is located in the rest of Boulder County or in Fort Collins); and $April$, $August$, and $October$ are binary variables equal to one if the price is recorded in that month; June is the omitted reference month. When we estimate the equation using the weekly online menu data from OrderUp, we replace the month fixed effects with weekly ones. γ_c represents community fixed effects, with an indicator variable for Boulder County and another indicator variable for Fort Collins. δ_t represents month fixed effects.[7] θ_s represents store fixed effects. ψ_i represents product fixed effects.[8] ϵ is a stochastic error term.

The data include only three geographic clusters (Boulder, the rest of Boulder County, and Fort Collins).[9] Cameron and Miller (2015) show that standard errors that do not account for the number of clusters can overstate precision unless the within-cluster correlation of errors is solely driven by a common shock process, which would be picked up by our store-level fixed effects. We cluster standard errors by store, following Cawley and Frisvold

[7]The results described below are not sensitive to also including day-of-the-week fixed effects and date-of-the-month fixed effects.

[8]We define a product based on the size and the name, so examples of products are a 20 oz. bottle of Pepsi, a 2 liter bottle of 7Up, a 12 pack of 12 oz. cans of Diet Coke, a 8.4 oz. can of Red Bull, and a small fountain drink.

[9]With only two geographic areas and two time periods, clustering can lead to degenerate standard errors (Donald & Lang, 2007; Cameron &Miller, 2015).

(2017).[10] Clustering standard errors at the community level, using the wild cluster bootstrap method as recommended by Cameron, Gelbach and Miller (2008), yields similar, but slightly smaller standard errors on the coefficients of interest. As a result, we report the more conservative standard errors, clustered at the store level.

In the equation listed above, β_2 and β_3 are the coefficients of interest; they represent the difference-in-differences estimates of the impact of the Boulder tax on prices in the post-tax periods of August and October respectively, relative to the pre-tax period of June. Comparing β_3 to β_2 indicates whether the estimate of pass-through changed over time after the tax.

An important assumption underlying this specification is that, in the absence of the tax, the trends in prices in Boulder would be the same as the trends in the control communities of Boulder County and Fort Collins. The geographic proximity of these areas, similarities in demographic characteristics and locations of large, public universities in Boulder and Fort Collins are consistent with this assumption.[11] Boulder County is an appealing control group because it has the advantage of proximity; any unobserved shocks to demand in Boulder around the time of the tax are likely experienced by the rest of the county. However, the disadvantage is that there may be spillover effects of the tax due to cross-border shopping by Boulder residents seeking to avoid the tax. Fort Collins has the

[10]To put our limited number of clusters into context, several previous studies of the pass-through of taxes on SSBs (e.g., Grogger, 2017) had data only for the treated country or state with no geographic control.

[11]The City of Boulder is fully enclosed within Boulder County. When referring to Boulder County as a community in the control group, we are referring to the area of Boulder County that excludes the City of Boulder.

relative advantage of being 45 miles to the north, which makes cross-border shopping from Boulder unlikely.

To investigate the plausibility of our identifying assumption of parallel trends in prices in the treatment and control areas, we assess the trends in prices in these areas over time. In addition, we examine the estimates of β_1, which measure any trend in prices during the two pre-tax periods of April and June that differs between the treatment and control group.

We estimate the above equation for taxed and untaxed products separately. We estimate the impact of the SSB tax on untaxed products because the tax could cause substitution from taxed to untaxed products (e.g., from Coke to Diet Coke) that alters the price of the untaxed products.

For our primary estimates, we pool all products and sizes. However, because the price elasticity, and thus the pass-through, may vary by product size and brand, we also estimate pass-through separately for the most common product sizes and brands.

2.3 Data

We assembled three datasets: 1) hand-collected data of listed prices and purchase prices of beverages from all retail stores; 2) hand-collected data of listed prices of fountain drinks and coffee drinks from all limited-service restaurants; and 3) web-scraped data of prices from a selected sample of restaurant menus. Appendix Figures **??**, **??**, and **??** show the location of each retailer store and restaurant where we gathered prices in Boulder, Boulder County, and Fort Collins, respectively.

2.3.1 Hand-Collected Data of Beverage Prices from Stores

We collected beverage prices at four points in time, twice before the tax (April and June 2017) and twice after the tax (August and October 2017). The four time points enable us to examine trends in prices before the tax and to compare the pass-through of the tax at two points in time after implementation.

We collected data from all grocery stores, pharmacies, and convenience stores in Boulder, Boulder County, and Fort Collins. We identified these stores and their addresses using the ReferenceUSA database, which includes approximately 24 million U.S. businesses and is updated monthly.[12] Data collectors visited and recorded prices from 174 retailers in April, 286 retailers in June, 287 retailers in August, and 288 retailers in October.[13] After the data collection in April, we expanded the set of retailers to include liquor stores.

We collected the prices of soft drinks, energy drinks, sports drinks, iced tea, juice, water, mixers for alcoholic drinks, and fountain drinks. We chose the most common sizes and brands to maintain consistency among the products and reduce the burden on data collectors in the field. We selected a mix of products that are taxed and untaxed. For example, we selected 20 oz. bottles, 2 liter bottles, and 12 packs of 12 oz. cans of Pepsi (taxed), Diet Pepsi (untaxed), Coke (taxed), and Diet Coke (untaxed).

[12]Specifically, we included all retailers with verified listings in Boulder County and Fort Collins, CO that are classified as supermarkets or other grocery stores (NAICS code 445110); convenience stores (NAICS code 445120); pharmacies and drug stores (NAICS code 446110); gasoline stations with convenience stores (NAICS code 447110); warehouse clubs and supercenters (NAICS code 452311), and beer, wine, and liquor stores (NAICS code 445310).

[13]More details on data collection are presented in Appendix Tables A2.1 and A2.2.

Failing to consider the register price could lead to an underestimate of the overall pass-through of the tax to consumers. To test this possibility, we construct the register price, which is equal to the posted price plus the amount of the tax that is itemized on the receipt, before sales tax is included. Specifically, in October (after the tax), in addition to collecting posted prices, data collectors purchased 20 oz. bottles of Pepsi and Diet Pepsi from each retailer and kept the receipt. If the store did not sell these products, the data collectors purchased another taxed SSB and a comparable untaxed product. Based on the receipts, we determine whether the posted price matches the price that retailers charge consumers (excluding sales tax).[14] For most retailers, the posted price is equal to the register price. However, 16 out of 77 Boulder retailers (20.8 percent) did not include the tax in the posted price, and instead, itemized the amount of the tax on the receipt. If a retailer adds the tax at the register for the SSB we purchased, we assume that the retailer does the same for all SSBs in both periods after the tax was implemented.

2.3.2 Hand-Collected Data from Restaurants and Coffee Shops

We collected the price and number of ounces of all sizes of fountain drinks from restaurants, which are taxed if the drink is caloric (not diet). We also collected the prices of a 12 oz. drip coffee, a 12 oz. latte, a 12 oz. mocha latte, and a 12 oz. hot chocolate from coffee shops, which are all untaxed. Although a mocha latte and a hot chocolate are sweetened beverages, the City Council exempted milk-based products from the tax.

We collected data from all limited-service restaurants and coffee shop locations in

[14] One retailer includes sales tax in the posted price. As a result, the receipt price, before the sales tax is included, is less than the posted price in all periods for this retailer.

Boulder County, including the City of Boulder, and Fort Collins.[15] Data collectors visited each of these restaurants to determine whether the restaurant sold fountain drinks or coffee drinks and to record the prices and sizes. We collected this information from restaurants in April, June, August, and October 2017, and from coffee shops in June, August, and October 2017. Data collectors visited 236 restaurants in April, 345 restaurants and coffee shops in June, 342 restaurants and coffee shops in August, and 340 restaurants and coffee shops in October.[16]

2.3.3 OrderUp Data of Restaurant Beverages

As a third source of data, we collected beverage prices from the menus of restaurants that participate in the OrderUp.com delivery platform in the City of Boulder and the Fort Collins area. There are no restaurants in Boulder County, outside of the City of Boulder, that participate in OrderUp. OrderUp is an online restaurant food ordering and delivery company that was founded in 2009 and serves customers in over 60 locations across 22 states.

We were able to collect these data more frequently because we collected these data by web scraping as opposed to in-person recording. We scraped the OrderUp data weekly, beginning every Wednesday, from March 22, 2017 through October 25, 2017. The fre-

[15]Specifically, using the ReferenceUSA database, we included all restaurants with verified listings in Boulder County and Fort Collins, CO that are classified as limited-service restaurants (NAICS code 722513) and snack and non-alcoholic beverage bars (NAICS code 722515), which includes all coffee shops listed under SIC code 581228. Limited-service restaurants are restaurants in which customers order at the counter.

[16]The number of restaurants selling each product in each time period are shown in Appendix Table A2.3.

quency of the data provides us with greater detail on the timing and consistency of price changes after the introduction of the tax and of the trends in prices prior to the tax.

The data collection began with 219 restaurants, of which 158 appeared in all waves of data collection. Reasons for a restaurant not remaining in the sample include termination of use of the OrderUp system, closures, name or address changes (these are the two identifying variables for a restaurant), and technical errors occurring when the website is updated and the scrape incorrectly reads or saves a menu. Of the 158 restaurants consistently in the sample every week, 114 consistently have beverage items throughout the entire period.[17] Of the 114 restaurants, 42 are located within the city of Boulder and 72 are located in the Fort Collins area.[18]

The types of beverages on the OrderUp menus are more varied than the hand-collected retail and restaurant data. The OrderUp beverage items in the final sample range from specific branded items (e.g., Coke, Oogave Rootbeer) to general types of drinks (e.g., apple juice, tea). The full list of items is shown in Appendix Table A2.4. We categorize each beverage item into one of three categories based on the Boulder SSB tax law: taxed, untaxed, or unknown. Most OrderUp beverage items have names that we can categorize as taxed or not under the Boulder SSB law, but some items have generic names such as "Coke products", which we cannot definitively categorize. Of the 877 beverage items in the balanced sample, 688 are identified as taxed or untaxed. Some beverage items con-

[17]We identify products by item name, and size when applicable, thus menu updates that change either variable exclude the item from the balanced sample.

[18]For this sample, the Fort Collins area includes Fort Collins, Evans, Garden City, Greeley, Loveland, and Windsor.

tain information on fluid ounces, but the majority only contain the name of the item. The number of ounces of the product is only known for 67 of the 877 items. As such, for the OrderUp items, we report price per drink instead of price per ounce. We assume that the number of ounces did not change over time for the drinks for which size is not listed. Although this is untestable for all items, there was no change in size after the tax for the 67 drink items of known size, which supports the plausibility of this assumption.

2.4 Results

2.4.1 Evidence Regarding Parallel Trends

The difference-in-differences method assumes that the comparison community is a valid counterfactual for the treated community. To investigate the plausibility of this assumption, we examine whether there existed parallel trends in the outcome (prices per ounce) between the treatment and comparison communities prior to the treatment. We present the trends for taxed and untaxed drinks, for the hand-collected store data (Figure **??**), hand-collected restaurant data (Appendix Figure **??**), and web-scraped restaurant data (Appendix Figure **??**). The trends in prices of all taxed products in Boulder are stable prior to the introduction of the tax in July and are comparable to the trends in prices of taxed products outside of Boulder over this same period (Figure **??**). Graphs of the trends in prices for specific sizes (20 ounce bottle, 2 liter bottle, 12 pack of 12 ounce cans, and fountain drinks) and specific brands (Pepsi products, Coke products, and other brands) sold in stores show similar patterns. The trends in the price per ounce of fountain drinks in restaurants and the price per drink from OrderUp are also stable in Boulder and parallel to the trends for taxed products outside of Boulder prior to the introduction of the tax (Appendix Figures **??** and **??**).

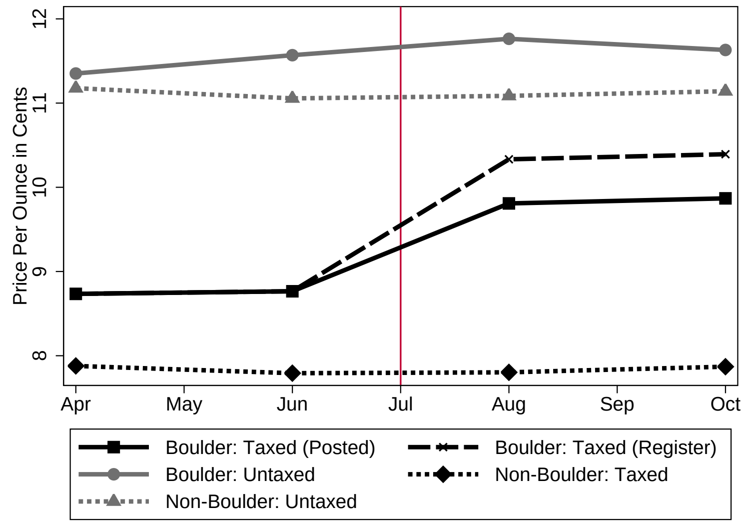

Figure 2.1: Trends in the Price per Ounce of SSBs and Other Beverages at Retailers

Notes: Price per ounce is reported in cents. Taxed and not taxed items are defined according to whether the item is taxed under the law in Boulder. Posted prices are the prices shown on the shelf for each item. Register prices are constructed to account for stores that do not include the SSB tax in the posted price, and is equal to the posted price plus the amount of the tax that is itemized on the receipt. The data are balanced at the store-item level across all four waves of the data collection.

2.4.2 Difference-in-Differences Estimates

Table 2.1 presents the difference-in-differences estimates for taxed and untaxed items, separately for the entire sample (i.e., unbalanced panel) and the balanced panel of products. Results for taxed items are shown for both posted prices and register prices. Column 1 presents results based on posted prices for the entire sample. The posted prices of SSBs increased from June (the last month prior to the tax) to August by 1.018 cents per ounce in Boulder, relative to the control communities.[19] The tax is 2 cents per ounce, so the price increase represents a pass-through of 50.9 percent. In October (3 months after the tax), prices were 1.022 cents per ounce higher than in June. Thus, prices rose from June to August, which is the month following the implementation of the tax, and then remained constant through October. Importantly, the coefficient on the interaction term for $Boulder \times April$ suggests that there was not a differential trend in prices between Boulder and the control communities prior to the tax.

We also selected products that are consumed more commonly in Boulder, such as Hansen's soda (taxed), San Pellegrino (untaxed), and GT's Organic Raw Kombucha (untaxed).[20] For all products, we collected the posted price and whether the product was on sale. If a store did not post prices, data collectors asked an employee for the price of the products. We collected this information for all products in each of the four periods, except that we began collecting the prices of Hansen's, San Pellegrino, and alcohol mixers in June (the second of the two pre-tax periods). The full list of products is shown in Appendix Table A2.1.

Next, we examine pass-through based on register prices (the results discussed in this paragraph are not presented in Table 2.1). Approximately twenty percent of stores itemize the tax at the register; more than half of these (13 out of 16) are convenience stores.

[19]The estimates are similar if we examine each control community separately, instead of combining Boulder County and Fort Collins.

[20]Fermented beverages with less than 11 grams of caloric sweetener per 12 fluid ounces were exempt from the tax. The GT's Kombucha products that were collected meet this criteria.

Table 2.1: Estimates of the Change in Retail Prices in Boulder after the SSB Tax

	Taxed Products Posted Prices Full Sample	Taxed Products Register Prices Full Sample	Untaxed Products Posted Prices Full Sample	Taxed Products Posted Prices Balanced Sample	Taxed Products Register Prices Balanced Sample	Untaxed Products Posted Prices Balanced Sample
Boulder × *Apr*	-0.130	-0.152	-0.385	-0.155	-0.160	-0.339
	(0.109)	(0.100)	(0.184)	(0.083)	(0.082)	(0.129)
Boulder × *Aug*	1.018	1.578	0.127	1.033	1.557	0.164
	(0.129)	(0.139)	(0.121)	(0.210)	(0.206)	(0.226)
Boulder × *Oct*	1.022	1.581	0.179	1.026	1.550	-0.023
	(0.122)	(0.137)	(0.129)	(0.209)	(0.201)	(0.142)
N	4078	4078	2625	1536	1536	919
N x T	11825	11825	7446	6129	6129	3676
Mean	7.907	7.907	11.613	7.985	7.985	11.181
R^2	0.957	0.957	0.929	0.977	0.977	0.953

Notes: Results in this table are calculated using the hand-collected retail data. The dependent variable is the price in cents per ounce. The estimates show the change in the number of cents per ounce of the retail price relative to the prices in June in Boulder County and Fort Collins. Posted prices are the prices shown on the shelf for each item. Register prices are constructed to account for stores that do not include the SSB tax in the posted price, and is equal to the posted price plus the amount of the tax that is itemized on the receipt. Standard errors, in parentheses, are clustered at the store level. Additional variables that are included, but not shown, are community fixed effects, month fixed effects, store fixed effects and product fixed effects. N represents the number of unique store specific items, *N x T* represents the number of unique store specific item observations across all waves. $Mean$ is the pre-tax average price per ounce in cents.

In contrast, only 8 out of 61 stores that only incorporate the tax into the shelf price are convenience stores. Stores that itemize the tax at the register also increased their prices on the shelf. The mean shelf price of taxed beverages in these stores increased by 0.438 cents per ounce (with a standard error of 0.101) from June to August, while the mean price for untaxed items increased by only 0.147 cents per ounce (with a standard error of 0.078). Since these stores also itemized the tax at the register, the mean price paid at the register of taxed beverages increased by 2.438 cents per ounce. In contrast, in stores that only incorporated the tax into the shelf price (and did not itemize the tax), mean prices increased by 0.965 cents per ounce (with a standard error of 0.150) for taxed beverages and 0.348 cents per ounce (with a standard error of 0.138) for untaxed beverages.

Column 2 of Table 2.1 shows the difference-in-differences estimates using the register prices for all stores. Prices in Boulder increased by 1.578 cents per ounce from June to August, for an estimated pass-through rate of 78.9 percent. Again, the estimate for October is very similar to that for August, implying that pass-through remained roughly constant in the months after the tax.

The third column of Table 2.1 reports results for untaxed beverages. The effect of the Boulder tax on the price of untaxed items is small in magnitude and not statistically significant. There is some evidence of a differential trend in the prices of untaxed products from April to June.

In the last three columns of Table 2.1, we find that the estimates are similar when we restrict the sample to the balanced panel of products that are consistently in the sample during all four periods. Thus, changes in products or stores do not drive the estimates for the entire sample.

We next examine whether the extent of pass-through varies by the size of the beverage, whether it is a fountain drink, and by store type. Pass-through could vary by size if demand is more inelastic for individual servings (e.g., 20-ounce bottles) than for larger volumes that are part of larger shopping trips in which people drive. Pass-through could vary by store type if the elasticities of demand and supply differ across store type, because of differences in the stores' marginal costs or because of differences in their clientèle.

Table 2.2 displays difference-in-difference estimates using the entire sample and register prices for beverages by size (20 ounce bottles, 2 liter bottles, and 12 packs of 12 ounce cans), for fountain drinks, and for store types (convenience, grocery, pharmacies, and liquor). There are not major differences in pass-through by the size of the beverage; it is roughly 75 percent for each. Fountain drinks stand out because the tax is over-shifted onto their retail prices; prices on fountain drinks rise by roughly 2.8 cents per ounce or 140 percent of the tax. The pass-through estimates are smaller for pharmacies and for grocery stores than other types at 52 percent and 64 percent, respectively. In contrast, the tax is passed through at 84 percent for liquor stores and at 99 percent for convenience stores.[21]

Table 2.3 reports results using the hand-collected data on fountain drinks and coffee drinks from restaurants. The price of fountain drinks increased by 0.972 cents per ounce in Boulder from June to August, relative to the price in Boulder County and Fort Collins,

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[21] The estimates are similar for chain and independent stores. Pass-through rates do not vary based on the distance of the retailer within Boulder to the nearest competitor in an untaxed area. Pass-through rates are similar for soda, energy drinks, and sweetened teas, but lower for sports drinks at 53 percent in August. Appendix Table A2.5 displays estimates for specific products.

Table 2.2: Heterogeneity in Estimates of the Change in Retail Prices in Boulder after the SSB Tax

	20oz	2L	12Pk	Fountain	Convenience	Grocery	Pharmacy	Liquor
Boulder $\times$ *Apr*	0.182	-0.050	0.018	1.625	-0.099	-0.242	-0.203	
	(0.100)	(0.138)	(0.170)	(0.278)	(0.128)	(0.218)	(0.175)	
Boulder $\times$ *Aug*	1.565	1.450	1.703	2.792	1.989	1.274	1.054	1.679
	(0.157)	(0.154)	(0.169)	(0.430)	(0.201)	(0.234)	(0.350)	(0.242)
Boulder $\times$ *Oct*	1.533	1.459	1.584	2.834	1.933	1.385	1.013	1.787
	(0.150)	(0.159)	(0.166)	(0.440)	(0.212)	(0.215)	(0.304)	(0.246)
N	1357	685	369	365	1643	1071	534	830
N x T	3953	1962	1153	1066	4527	3374	2077	1847
Mean	8.997	3.158	3.86	4.089	8.07	7.667	7.964	7.814
R^2	0.690	0.807	0.843	0.897	0.98	0.928	0.973	0.959

Notes: Results in this table are calculated using the full sample of taxed products from the hand-collected retail data and the prices charged at the register. The dependent variable is the price in cents per ounce. The estimates show the change in the number of cents per ounce of the retail price relative to the prices in June in Boulder County and Fort Collins. Standard errors, in parentheses, are clustered at the store level. Additional variables that are included, but not shown, are community fixed effects, month fixed effects, store fixed effects and product fixed effects. N represents the number of unique store specific items, *N x T* represents the number of unique store specific item observations across all waves. $Mean$ is the pre-tax average price per ounce in cents.

implying a pass-through of 48.6 percent. In contrast to retail prices, the prices of fountain drinks in restaurants continued to rise after August. In October, the relative price per ounce in Boulder was 1.387 cents higher than in June, for a pass-through of 69.4 percent. As also shown in the table, the prices of untaxed products in coffee shops did not change as a result of the tax on SSBs. Again, estimates for the balanced sample of stores are similar to those for the entire (unbalanced) sample.

Table 2.3: Estimates of the Change in Hand Collected Restaurant Prices in Boulder after the SSB Tax

	Fountain Full Sample	Coffee Full Sample	Fountain Balanced Sample	Coffee Balanced Sample
Boulder × *Apr*	-0.187		-0.146	
	(0.316)		(0.342)	
Boulder × *Aug*	0.972	-0.069	1.013	-0.048
	(0.204)	(0.234)	(0.211)	(0.236)
Boulder × *Oct*	1.387	-0.125	1.340	-0.100
	(0.267)	(0.228)	(0.275)	(0.228)
N	689	628	471	419
N x T	2250	1557	1830	1257
Mean	7.963	23.315	7.853	24.048
R^2	0.752	0.904	0.712	0.907

Notes: Results in this table are calculated using the hand-collected restaurant data. The dependent variable is the price in cents per ounce. The estimates for $Boulder \times August$ and $Boulder \times October$ show the change in the number of cents per ounce of the restaurant price relative to the prices in June in Boulder County and Fort Collins. Standard errors, in parentheses, are clustered at the store level. Additional variables that are included, but not shown, are community fixed effects, month fixed effects, restaurant fixed effects and product fixed effects. N represents the number of unique restaurant specific items, *N x T* represents the number of unique restaurant specific item observations across all waves. $Mean$ is the pre-tax average price per ounce in cents.

Table 2.4 displays results using the price data scraped from restaurant menus on OrderUp. An advantage of these data is that they could be collected more often, so we have greater ability to examine any difference in trends between the treatment and control communities prior to the tax, as well as changes in pass-through over time after the tax. A limitation of the OrderUp data is that we generally do not observe the size of the drink in ounces, so we observe price per drink rather than price per ounce, and while we can estimate the change in overall price we cannot estimate percent pass-through.

The interaction of the indicator variable for Boulder with months prior to the tax (March, April, and May) yields no evidence of a differential trend between the treatment and control communities, which is consistent with the identifying assumption of the regression model. For taxed beverages, the tax increased prices by 17.3 cents in August, 21.1 cents in September, and 20.2 cents in October. Prices also rose for untaxed beverages following the SSB tax: by 6.5 cents in August, 8.4 cents in September, and 7.8 cents in October. Beverages of unknown tax status (listed in column 3) experienced changes in price similar to those of untaxed items. Although we cannot estimate the percentage pass-through of the tax, these data serve the important purposes of confirming parallel trends for Boulder and the control communities prior to the tax, and for confirming that the retail prices of taxed drinks rose more in Boulder than in the control communities after the tax.

Table 2.4: Estimates of the Change in OrderUp Restaurant Prices in Boulder after the SSB Tax

	Taxed	Untaxed	Unknown
Boulder × *Mar*	0.013	0.011	0.015
	(0.021)	(0.006)	(0.018)
Boulder × *Apr*	0.010	0.011	-0.007
	(0.009)	(0.006)	(0.012)
Boulder × *May*	0.000	0.007	0.008
	(0.003)	(0.005)	(0.014)
Boulder × *Jul*	0.082	0.027	0.003
	(0.041)	(0.032)	(0.020)
Boulder × *Aug*	0.173	0.065	0.066
	(0.067)	(0.038)	(0.032)
Boulder × *Sept*	0.211	0.084	0.090
	(0.087)	(0.040)	(0.039)
Boulder × *Oct*	0.202	0.078	0.087
	(0.089)	(0.040)	(0.039)
N	343	345	189
N x T	10976	11040	6048
Mean	2.448	2.84	3.447
R^2	0.921	0.753	0.745

Notes: Results in this table are calculated using the balanced sample of the OrderUp restaurant data. The dependent variable is the price in dollars per drink. The estimates show the change in the dollars per drink of the restaurant price relative to the prices in June in Boulder County and Fort Collins. Standard errors, in parentheses, are clustered at the store level. Additional variables that are included, but not shown, are community fixed effects, month fixed effects, restaurant fixed effects and product fixed effects. N represents the number of unique restaurant specific items, *N x T* represents the number of unique restaurant specific item observations across all waves. $Mean$ is the pre-tax average price per drink in dollar.

2.5 Discussion and Conclusion

This paper provides the first evidence of the impact of the tax on SSBs in Boulder, CO, a tax that is noteworthy because it is the largest tax on SSBs passed by any U.S. city. Using hand-collected data from hundreds of retailers and hundreds of restaurants, we estimate that the tax was substantially, but not fully, passed through to consumers in the form of higher prices. Data from transactions at store registers indicate that 79.3 percent of the tax was passed through one month after the tax was instituted, and that the pass-through remained roughly constant for the next several months. The pass-through was similar across sizes of SSBs and was larger for liquor stores and convenience stores than in pharmacies. There is little evidence of any impact of the tax on the store prices of untaxed beverages. Data hand-collected from restaurants indicates that the pass-through of the tax was 69.4 percent on fountain drinks, and the tax had no detectable impact on the prices of untaxed coffee drinks. For restaurants, the increase in prices is slightly more gradual than retailers; this could be due to restaurants in general changing their prices less frequently than retailers.

It is commonly assumed that an excise tax will be incorporated into the shelf price (e.g., Chetty Looney, and Kroft, 2009). However, we find that not all retailers increase the posted price of SSBs in response to the tax. Among retailers in Boulder selling SSBs, 21 percent chose to add the tax at the register and itemize it on the receipt. Ignoring these decisions of retailers would lead to a substantial underestimate of the pass-through rate. The estimated pass-through based on posted prices is 51.2 percent; whereas, pass-through based on register prices is 79.3 percent.

Increasing the price at the register compared to the shelf could have important implications for the impact of the tax on purchases and the regressivity of the tax. The tax is more salient when it is included in the shelf price because it is observed at the point of decision-making; consumers may not notice it being added at the register. Consistent with this, Chetty, Looney, and Kroft (2009) find that alcohol purchases decrease more when the tax is incorporated into the posted price instead of added at the register. Taubinsky and Rees-Jones (2018) find that consumers are less responsive to taxes that are not as salient on low-priced items, such as single-serving SSBs. Goldin and Homonoff (2013) suggest that cigarette taxes imposed at the register could be less regressive than similar taxes incorporated into the posted prices if low-income consumers are more attentive to prices at the register than high-income consumers.

Overall, our estimates suggest that the tax on SSBs in Boulder was substantially, but not fully, passed through to consumers. With the exception of fountain drinks and convenience stores, the 95 percent confidence intervals rule out 100 percent pass-through. The estimates of the pass-through of the tax in Boulder are larger than estimates of the pass-through of the SSB tax in Berkeley (Falbe et al., 2015; Cawley and Frisvold, 2017). They are lower than the estimates of the pass-through of taxes on SSBs in other countries (e.g. Colchero et al., 2015; Grogger, 2017; Berardi et al., 2016; Bergman and Hansen, 2010); although, this may be because those studies lack geographic control groups.

These results have implications beyond Boulder. Many cities have recently enacted taxes on SSBs, and their effects are not well understood. This paper contributes to the growing literature on the impacts of these taxes. These results also have implications for simulations of the effect of SSB taxes on consumption, which have often assumed that taxes will be fully passed through to consumers (e.g., Dharmasena, Davis, & Capps, 2014; Long et al., 2015; Wang et al., 2012). The results of this paper imply that consumers do not always bear the full burden of SSB tax (e.g., pass-through is not necessarily full) and that pass-through rates can vary across different localities.

Strengths of this analysis in Boulder include: (1) multiple periods of prices prior to the implementation of the tax, which allow us to assess whether the trends in prices are similar in the treated and the multiple comparison communities; (2) multiple periods of prices after the implementation of the tax, which allow us to determine how quickly restaurants and retailers respond to the tax; (3) prices from a wide range of products; (4) prices from all retailers and limited-service restaurants in the three communities, which minimizes sampling error; (5) large sample sizes of hundreds of stores and hundreds of restaurants; (6) weekly prices from online restaurant menus; and (7) both posted and receipt prices from retailers.

We acknowledge that the comparison communities may be imperfect controls for Boulder, and we do not observe prices charged by the distributor to retailer. We also lack of information on sales, consumption, or consumer weight. Another limitation of this study is that we have a small number of clusters; we examine three geographic areas and four time periods (in the hand-collected data, with more periods in the web-scraped data). Despite these limitations, this paper presents important information about the incidence of the largest tax on SSBs in the United States.

CHAPTER 3
LABOR MARKET EFFECTS OF A HEALTH SHOCK ON THE AGING POPULATION: EVIDENCE FROM A BAN ON TRANS FAT

3.1 Introduction

Health status is a major determinant of labor market outcomes including labor force participation, retirement, and hours worked. From 1996 to 2016, the labor force participation rate for those ages 55 and older increased from 30.3% to 40.0%, and from 12.1% to 19.3% for those ages 65 and older. Since the risk of many serious health conditions increases with age, this aging workforce increasingly must deal with the onset of serious health conditions while still in the labor force. This paper adds to the growing literature on the relationship between health status and labor market outcomes by providing estimates of how a ban on trans fatty acids, a nutrition based health shock, affected labor market outcomes for the aging population.

There is a large literature linking health status and labor market outcomes, dating back to Grossman (1972), who argued that health is an investment good of which the labor supply function is dependent. In the labor market, workers must choose between leisure and goods, and the onset of a serious health problem steepens the indifference curve when substituting between leisure and goods by the same as multiple additional years of age (Gustman and Steinmeier, 1986; Pelkowski and Berger, 2004). Similarly, poor health increases the value of retirement relative to either part-time or full-time employment (Berkovec and Stern, 1991; Chan and Huff Stevens, 2001; French and Jones, 2011). In addition, self reported poor health has the largest impact on the probability of retiring, more so than eduction, income, or marital status (Diamond and Hausman, 1984; Van der Klaauw and Wolpin, 2008). Although the relationship between health and the labor force is well established, it is important to further understand how specific changes in health can directly impact labor force outcomes, specifically for the aging population due to the increasing participation rate. One such prevalent health condition that has the ability to drastically change health status is cardiovascular disease.

Cardiovascular disease (CVD) is the leading cause of death in the U.S, and in 2010 constituted $273 billion a year in direct medical costs and $172 billion in lost productivity (Heidenreich, Trogdon, Khavjou, Butler, Dracup, Ezekowitz, Finkelstein, Hong, Johnston, Khera et al., 2011). CVD can be particularly influenced by diet and nutrition (Bazzano, He, Ogden, Loria, Vupputuri, Myers and Whelton, 2002; Harris, Mozaffarian, Rimm, Kris-Etherton, Rudel, Appel, Engler, Engler and Sacks, 2009). One such dietary nutrient that has been specifically linked to CVD is trans fatty acid (TFA), or partially hydrogenated oil. TFAs have been shown to reduce HDL (good) cholesterol and increase LDL (bad) cholesterol and inflammation (Mensink and Katan, 1990; Mensink et al., 2003; Brouwer et al., 2010). In a survey of the literature, Mozaffarian et al. (2006) concluded that the most reliable research suggests that a 2% increase in daily energy intake from TFAs is associated with a 23% increased risk of CVD. As an informative example, according to the 2002 Harvard Food Composition Database, the average single order of French fries contained over 5 grams of TFAs, which is the equivalent of over 2% of daily energy intake based on a 2,000 calorie per day diet. Thus, TFAs incur a substantial increased risk of CVD at relatively low levels of consumption.

In response to the establishment of the unfavorable physiologic changes due to consumption of TFAs, cities and counties began to restrict the use of TFAs in food establishments in 2007, and by June 2018, all TFAs were removed from manufactured goods by order of the FDA. The first ban on TFAs was implemented in New York City (NYC) on July 1, 2007. This ban on the use of TFAs applied to all eating establishments, including but not limited to, restaurants, bakeries, caterers, cafeterias, and senior-meal programs (City of New York, 2006). Over this time period in the U.S., consumption of food prepared away from home comprised over 40% of total food expenditures (Economic Research Service, USDA and National Bureau of Economic Research, 2016), and more than one third of daily food energy intake was from food prepared away from home (Moshfegh, Goldman, Ahuja, Rhodes and LaComb, 2009). Brandt et. al (2017) concluded that the TFA ban in NYC decreased hospital admissions for heart attack and strokes by 6.2%. In addition, Restrepo and Rieger (2016) found that cardiovascular disease mortality rates decreased by 4.5% due to the implementation of this TFA ban in NYC. These two studies provide evidence that the NYC ban on TFAs provided a positive health shock to the aging workforce.

In this paper, I examine the impact of the nutrition based health shock from a reduction in TFA consumption on labor market outcomes of the aging population in the decade following the ban on TFAs in NYC. This paper is the first to examine the labor market impact of a TFA ban. Using data from the Current Population Survey (CPS), I estimate the impact of the TFA ban on retirement, employment, ability to work, and hours worked for people ages 50 and over. Using the preferred specification of control group, I find that the percent of those employed at ages 50 and over increased by 3.4 percentage points, and that hours worked per week increased by 1.5 hours four and more years after the implementation of a ban on TFAs. I find that these changes were driven by a decrease in the percent of those who were unable to work, and not by a decrease in the percent of those who retired. To gain a better understanding of the health mechanisms through which these labor market effects occur, I utilize data from the Health and Retirement Study (HRS). I estimate that the incidence of a heart condition decreased by 2.3 percentage points three to five years after the implementation of a TFA ban. These results suggest that the decrease in CVD events is the driving health mechanism behind these labor market effects.

3.2 Background

3.2.1 Trans Fatty Acids

Trans fatty acids (TFAs) are unsaturated fatty acids with one or more double bond(s) in the trans configuration. The most common form of TFAs are created through an industrial process called partial hydrogenation, which converts vegetable oil into a semi solid fat. Prior to the government restrictions on its use, the primary form of consumption of TFAs was through fried foods, bakery products, packaged snacks, margarines, and crackers (Kris-Etherton, Lefevre, Mensink, Petersen, Fleming and Flickinger, 2012). The benefits of using TFAs, from a food industry standpoint, include a long shelf life, stability during frying, they are relatively inexpensive, and the semisolidity which can be used for multiple types of food. Small amounts of TFAs also occur naturally in meats and dairy products from animals with multi-chambered stomachs. These sources produce levels which are significantly smaller than the levels found in foods with artificial TFAs, and have not been found to have detrimental health effects; thus, for the remainder of this paper I will consider only the artificial TFAs found in manufactured foods.

Dietary fat composition is widely demonstrated to be a strong determinant of cardiovascular related diseases. TFAs, specifically, are associated with decreased high-density lipoprotein (HDL) cholesterol, commonly known as “good cholesterol”, and increased low-density lipoprotein (LDL) cholesterol, commonly known as “bad cholesterol” (Mensink and Katan, 1990; Zock and Katan, 1992; Willett, Stampfer, Manson, Colditz, Speizer, Rosner, Hennekens and Sampson, 1993; Mensink, Zock, Kester and Katan, 2003; Brouwer, Wanders and Katan, 2010). TFAs are also associated with higher levels of systemic inflammation as well as endothelial cell dysfunction (Han, Leka, Lichtenstein, Ausman, Schaefer and Meydani, 2002; Mozaffarian, Pischon, Hankinson, Rifai, Joshipura, Willett and Rimm, 2004).

In a survey of the literature on the relationship between TFAs and cardiovascular disease, Mozaffarian et al. (2006) report that TFAs increase the risk of cardiovascular disease more so than any other macronutrient. On average, the literature finds that a 2% increase in energy intake from TFAs is associated with a 23% increase in the risk of cardiovascular disease, a substantial increase in risk at a relatively low level of consumption (Mozaffarian, Katan, Ascherio, Stampfer and Willett, 2006). To put this in perspective, on average, one serving of french fries contains 4.6 to 6.1 grams of TFAs, which represents 2.1 to 2.7 percent of daily energy intake based on a 2000 calorie diet. As another comparison, based on data from the 1999-2002 NHANES, TFAs comprised an average of 2 to 3 percent of total energy intake in the U.S. (Kris-Etherton, Lefevre, Mensink, Petersen, Fleming and Flickinger, 2012). In response to the establishment of the unfavorable physiologic changes due to consumption of TFAs and the large proportion of energy intake consumed from food away from home, cities and counties began to restrict the use of TFAs in food establishments.

3.2.2 Policies on Trans Fatty Acids

Between 2007 and 2010, 15 localized areas in 6 states banned the use of TFAs in food establishments. In 2013, the U.S. Food and Drug Administration declared TFAs "not generally recognized as safe" and were removed from all food products by July 2018 (Food and Drug Administration, 2015).New York City was the first area in the U.S. to enact a ban on TFA. The ban on TFAs was implemented on July 1, 2007 and applied to eating establishments including restaurants, bakeries, caterers, cafeterias, senior meal programs, and others (City of New York, 2006).

The first phase of the restriction, implemented on July 1, 2007, banned the use of TFAs in frying, grilling, and use as a spread. The second phase, implemented on July 1, 2008, banned the use of TFA in deep frying, batter, and yeast dough. In this paper, I define the start of phase one as the implementation of the TFA ban, as this was the earliest date of a reduction in TFA consumption. Estimates from NYC fast food restaurant receipts collected prior to and after implementation suggest that the average TFA content per meal decreased by 2.4 grams, from 2.9 grams to 0.5 grams (Angell, Cobb, Curtis, Konty and Silver, 2012). Based on a 2,000 calorie per day diet, this is a decrease of 1.2% energy intake from TFA. In addition, by 16 months after the implementation of the ban, less than

2% of NYC restaurants were found to use any TFAs (Angell, Silver, Goldstein, Johnson, Deitcher, Frieden and Bassett, 2009).

3.3 Data

3.3.1 CPS

To estimate changes in labor market outcomes for the aging population due to the nutrition based health shock of a decrease in TFA consumption, I utilize data from the Current Population Survey (CPS). The CPS is a monthly survey of U.S. households sponsored by the U.S. Census Bureau and the U.S. Bureau of Labor Statistics. It is the primary source of labor force statistics in the U.S., providing comprehensive household data on labor force participation, employment, unemployment, hours of work, as well as other demographic characteristics. I use the Basic Monthly CPS IPUMS Sample (Flood, King, Ruggles and Warren, 2017), restricting the sample to those ages 50 and above.

3.3.2 HRS

As a second source of data, I use the Health and Retirement Study (HRS) in order to explore the health mechanisms by which a ban on TFAs affect the labor market outcomes of the aging population. The HRS is a longitudinal panel study that surveyed a representative sample of approximately 20,000 people in the U.S. between 1992 and 2016. The survey data obtained consists of extensive economic, health, and family information. Funding for this survey is provided by The National Institute on Aging and the Social Security Administration. Surveys were conducted by the Institute for Social Research Survey Research Center at the University of Michigan. Sample selection was driven by a multistage area probability sample of households based on the Survey Research Center's 84 National Sample frame.

Throughout the duration of the survey, there have been seven cohorts of respondents. For each cohort, the sampling included households in the contiguous U.S. with at least one spouse born between the years of the identified cohort sample.[1] Appendix Figure **??** depicts he HRS longitudinal cohort sample design. The original HRS cohort was surveyed starting in 1992 and was administered every other year. The Asset and Health Dynamics Among the Oldest Old cohort was surveyed in 1993, 1995, and then every other year from 1998 to 2012. Primary household respondents in this cohort were born before 1924. The majority of this cohort was deceased prior to the implementation of the TFA bans, thus is not included in the analysis for this paper.

The Children of the Depression cohort and the War Babies cohort were surveyed every other year from 1998 to 2016. The Children of the Depression cohort primary household respondents were born between 1924 and 1930. The War Babies cohort primary household respondents were born between 1942 and 1947. The Early Baby Boomers cohort was surveyed every other year from 2004 to 2016. These primary household respondents were born between 1948 and 1953. The Mid-Baby Boomers were surveyed every other year between 2010 and 2016, and the primary household respondents were born between 1954 and 1959. The Late Baby Boomers were surveyed starting in 2016, and the primary household respondents were born between 1960 and 1965. The last two cohorts, the Mid

[1] Individuals that were in institutions were excluded from the survey population, however survey participants were followed if they moved from a household into an institution throughout the course of the survey.

and Late Baby Boomers, are excluded from analysis since they were not in the data prior to the TFA bans.

The primary HRS variable of interest is the prevalence of a heart condition. This is a binary variable denoting whether a respondent has been told by a doctor in the last two years (or since the last survey wave) that the respondent has had a heart attack, coronary heart disease, angina, congestive heart failure, or other heart problems.

3.3.3 Control Groups

A key assumption underlying the difference-in-differences model used in this paper is that the trends in the outcomes of interest in NYC, in the absence of the ban on TFAs, would be comparable to trends in the outcomes of interest in the comparison group. To address this, I utilize multiple comparison groups. The first comparison group is comprised of respondents that live in the state of New York (NY) but outside of NYC.[2] Upstate NY has been used as the counterfactual group in the previous studies that have examined effects of the TFA ban (Brandt, Myerson, Perraillon and Polonsky, 2017; Restrepo and Rieger, 2016), as upstate NY is geographically and socioeconomically similar to NYC in multiple dimensions.

As a second comparison group, I use individuals that live in a metropolitan statistical area (MSA) of larger than 5 million people. Pre-TFA ban, baseline characteristics and sample sizes of these two comparison groups and the treatment group in the CPS and HRS datasets are shown in Tables 3.1 and 3.2, respectively.

[2] Three other counties in NY (Nassau, Westchester, and Albany) implemented a ban on TFAs between 2008 and 2009, and thus are excluded from all control groups.

Table 3.1: Baseline Characteristics of Comparison Groups: CPS

	NYC	Upstate NY	Lg MSA
Female	0.581	0.541	0.549
	(0.002)	(0.002)	(0.001)
Age	64.455	64.121	63.726
	(0.041)	(0.035)	(0.016)
Low Income	0.288	0.197	0.173
	(0.002)	(0.001)	(0.001)
High Income	0.065	0.083	0.085
	(0.001)	(0.001)	(0.000)
High School	0.607	0.566	0.505
	(0.002)	(0.002)	(0.001)
Retired	0.424	0.456	0.412
	(0.002)	(0.002)	(0.001)
Working	0.371	0.405	0.446
	(0.002)	(0.002)	(0.001)
Unable	0.093	0.058	0.051
	(0.001)	(0.001)	(0.000)
Hours	40.042	38.951	39.963
	(0.072)	(0.068)	(0.028)
$N \times T$	64442	88732	414087

Notes: Monthly CPS IPUMS data from 2000-2006 were used for all statistics. These are pre TFA ban averages. Upstate NY includes all CPS respondents that live within the state of NY, but outside of NYC and excluding the three other counties that later enacted a TFA ban; Westchester, Nassau, and Albany counties. The Lg MSA comparison group includes all respondents that reside in a metropolitan statistical area of greater than 5 million people and that do not live in a county that enacted a ban on TFA between 2000-2016.

Table 3.2: Baseline Characteristics of Comparison Groups: HRS

	NYC	Upstate NY	Lg MSA	Other
Female	0.635	0.581	0.590	0.586
	(0.010)	(0.010)	(0.003)	(0.002)
Age	69.429	69.135	67.824	67.937
	(0.250)	(0.222)	(0.060)	(0.041)
Low Income	0.659	0.714	0.602	0.630
	(0.010)	(0.009)	(0.003)	(0.002)
High Income	0.094	0.070	0.110	0.084
	(0.006)	(0.005)	(0.002)	(0.001)
High School	0.658	0.636	0.545	0.600
	(0.010)	(0.010)	(0.003)	(0.002)
Heart Condition	0.223	0.254	0.235	0.242
	(0.009)	(0.009)	(0.002)	(0.002)
Rate Health	3.125	2.787	2.803	2.863
	(0.024)	(0.022)	(0.006)	(0.004)
$N \times T$	2168	2371	34119	73891

Notes: Biannual HRS data from 2000-2006 were used for all statistics. These are pre TFA ban averages. Upstate NY includes all CPS respondents that live within the state of NY, but outside of NYC and excluding the three other counties that later enacted a TFA ban; Westchester, Nassau, and Albany counties. The Lg MSA comparison group includes all respondents that reside in a metropolitan statistical area of greater than 1 million people and that do not live in a county that enacted a ban on TFA between 2000-2016. The Other comparison group includes all survey respondents outside of NYC.

3.4 Methods

To determine the impact of the TFA ban in NYC on labor market and health outcomes in the decade following implementation, I compare changes in labor market and health outcomes over time in NYC to a comparison group. Specifically, I estimate:

$$Y_{ict} = \alpha + Years\_Post_{ct}\beta + \gamma Treat_c + X_{it}\zeta + \theta_t + \epsilon_{ict} \tag{3.1}$$

where Y_{ict} denotes the labor market or health outcome of interest of individual i in city c in time t, $Years\_Post_{ct}$ is a vector of binary variables that represent the number of years since implementation of the TFA ban, all of which are zero for individuals outside of NYC, $Treat_c$ is a binary variable equal to one if the individual resides in NYC. X_{it} is an array of time-varying, and individual specific demographic variables. θ_t represents time fixed effects.[3] The vector of coefficients β are the coefficients of interest. β coefficients that represent years prior to implementation provide evidence of potential policy endogeneity. β coefficients that represent years after implementation represent the primary estimates of interest.

Trends in the labor market outcomes of interest for NYC, including retirement, working, unable to work, and hours worked are depicted in comparison to upstate NY in Figure **??**. Retirement, working, and unable to work are binary variables. Hours worked is an integer representing usual hours worked per week. Although the comparison group does not show directly parallel trends prior to the implementation of the TFA ban, the figure plots the raw data and is not adjusted for demographic variables. The coefficients in the β vector that represent time periods prior to the TFA ban will be a stronger indication of the validity of the parallel trends assumption. Similarly, Figure **??** plots the trends in the prevalence of heart conditions from the HRS data, comparing the treatment group with the primary control group.

[3] In specifications with CPS data, month fixed effects and month-year fixed effects are included in addition to year fixed effects.

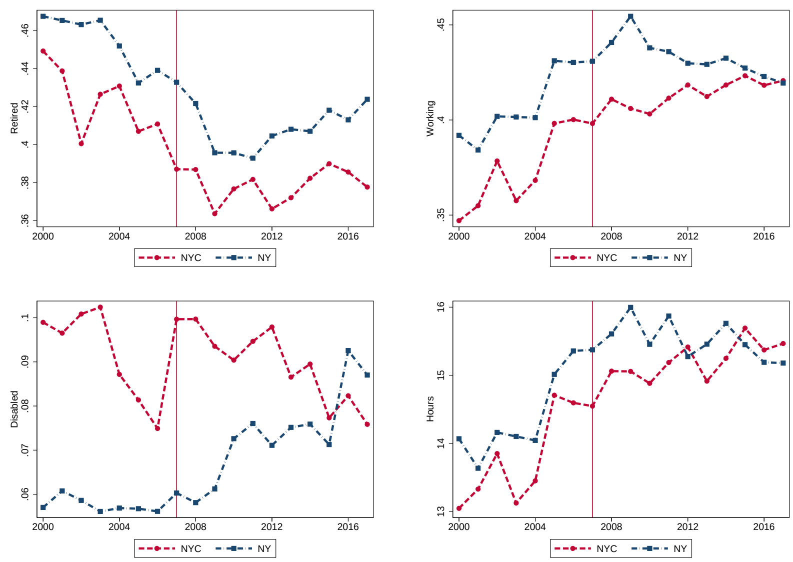


Figure 3.1: CPS Labor Market Trends: NYC vs. Upstate NY

Notes: Labor market trends from the monthly CPS IPUMS data are shown. Data is collapsed to the year level for visual clarity. The vertical line at 2007 represents the year that the TFA ban was implemented. NYC includes all CPS respondents that live in NYC. NY includes all CPS respondents that live within the state of NY, but outside of NYC and excluding the three other counties that later enacted a TFA ban: Westchester, Nassau, and Albany counties. Retired is a binary variable denoting if the respondent is retired. Working is a binary variable denoting if the respondent is currently working at a job. Disabled is a binary variable denoting if the respondent is unable to work at a job. Hours is an integer denoting the usual number of hours worked per week, with a value of zero if the respondent is not working for any reason.

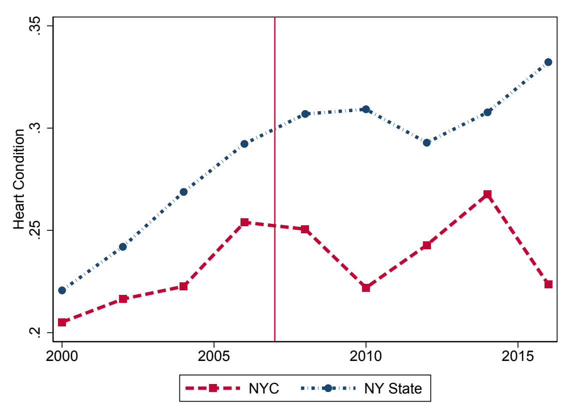


Figure 3.2: HRS Heart Condition Trends: NYC vs. Upstate NY

Notes: Trends in the prevalence of heart conditions are shown in each panel. Heart condition is a binary variable that is equal to one if the respondent has been told by a doctor in the last two years that the respondent has had a heart attack, coronary heart disease, angina, congestive heart failure or other heart problems.

3.5 Results

Table 3.3 shows the main results for the labor market effects of a ban on TFAs in NYC throughout the decade following implementation. The preferred control group, upstate NY, is used in this specification. As the vector of coefficients of interest (β) is rather large, the estimated coefficients along with the corresponding 95% confidence intervals are plotted in Figure **??**. The first panel in Figure **??** suggests that there were no significant changes in the proportion of those ages 50 and above who retired after the implementation of the TFA ban. The estimates for working and hours worked, however, show a significantly positive effect five and more years after implementation. The proportion of people working increased by an average of 3.4 percentage points in comparison to upstate NY, and the average number of hours worked per week increased by 1.5 hours. These results are driven by the decrease in the proportion of those unable to work, which decreased by an average of 1.9 percentage points.

The labor market outcome results are similar when using the secondary control group: those living in another large MSA. Table 3.4 shows the results of this analysis. I find no significant effect on the proportion of those ages 50 and above who are retired. The percent of people working and the average number of hours worked per week increased significantly four and more years after implementation, by a magnitude of 2.6 percentage points and 1.1 hours per week. Dissimilar to the upstate NY control group, however, this comparison group suggests no significant effects on the proportion of people who are unable to work.

Table 3.3: CPS Labor Market Effect Estimates: Upstate NY as Control Group

	Retired	Working	Disabled	Hours
NYC × 6 Years Before	0.007	-0.011	0.018	-0.111
	(0.007)	(0.008)	(0.005)	(0.334)
NYC × 5 Years Before	0.010	-0.001	0.010	0.378
	(0.007)	(0.008)	(0.005)	(0.336)
NYC × 4 Years Before	-0.020	-0.003	0.018	0.108
	(0.007)	(0.008)	(0.005)	(0.335)
NYC × 3 Years Before	-0.010	-0.012	0.025	-0.164
	(0.007)	(0.008)	(0.005)	(0.337)
NYC × 2 Years Before	0.004	-0.001	0.012	0.217
	(0.007)	(0.008)	(0.005)	(0.343)
NYC × 1 Year Before	0.008	-0.008	0.006	0.214
	(0.007)	(0.008)	(0.005)	(0.348)
NYC × 1 Year After	-0.010	0.002	0.013	0.079
	(0.007)	(0.008)	(0.005)	(0.353)
NYC × 2 Years After	-0.001	0.003	0.017	0.212
	(0.007)	(0.008)	(0.005)	(0.351)
NYC × 3 Years After	0.005	-0.015	0.005	-0.082
	(0.007)	(0.008)	(0.005)	(0.347)
NYC × 4 Years After	0.010	0.006	-0.011	0.586
	(0.007)	(0.008)	(0.005)	(0.345)
NYC × 5 Years After	0.009	0.025	-0.011	0.852
	(0.007)	(0.008)	(0.005)	(0.343)
NYC × 6 Years After	-0.009	0.035	-0.006	1.549
	(0.007)	(0.008)	(0.005)	(0.345)
NYC × 7 Years After	-0.027	0.046	-0.020	1.540
	(0.007)	(0.008)	(0.005)	(0.346)
NYC × 8 Years After	-0.008	0.037	-0.013	1.074
	(0.007)	(0.008)	(0.005)	(0.344)
NYC × 9 Years After	0.003	0.032	-0.019	1.248
	(0.007)	(0.008)	(0.005)	(0.345)
NYC × 10 Years After	0.003	0.022	-0.028	0.767
	(0.007)	(0.008)	(0.005)	(0.345)
NYC × 11 Years After	-0.015	0.041	-0.039	1.432
	(0.007)	(0.008)	(0.005)	(0.347)
R^2	0.48	0.33	0.10	0.31
N	376,353	376,353	376,353	376,353

Notes: The regression estimates using equation (1) are reported. Controls that are included but not reported are sex, age, income, education, county, and state. Fixed effects include month, month-year, and year. The x-axes represent years since the implementation of the TFA ban in NYC. All coefficients are in comparison with year 0, which is equivalent to 2007.

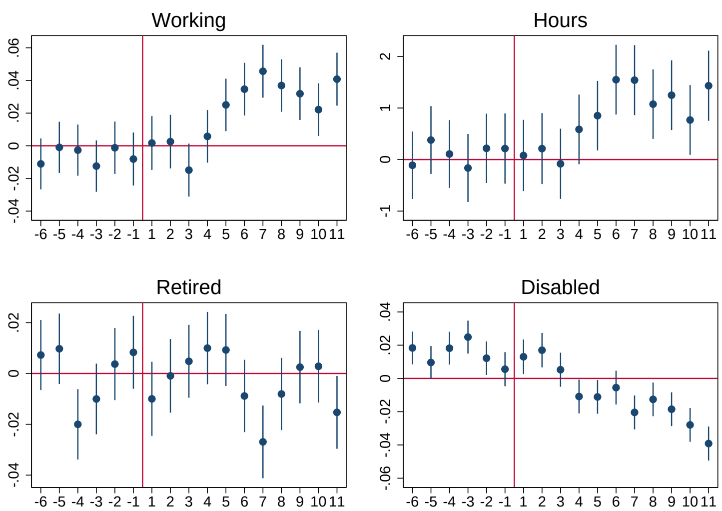


Figure 3.3: CPS Labor Market Effect Estimates

Notes: Each graph plots the coefficient and corresponding 95% confidence intervals for the regression estimates using equation (1). The coefficients are calculated using the preferred control group, upstate NY. Controls include sex, age, income, education, county, and state. Fixed effects include month, month-year, and year. The x-axes represent years since the implementation of the TFA ban in NYC. All coefficients are in comparison with year 0, which is equivalent to 2007.

Table 3.4: CPS Labor Market Effect Estimates: Other Large MSAs as Control Group

	Retired	Working	Disabled	Hours
NYC × 6 Years Before	-0.010	-0.019	0.026	-0.168
	(0.006)	(0.007)	(0.004)	(0.282)
NYC × 5 Years Before	-0.008	-0.021	0.025	-0.253
	(0.006)	(0.007)	(0.004)	(0.285)
NYC × 4 Years Before	-0.045	0.004	0.032	0.261
	(0.006)	(0.007)	(0.004)	(0.285)
NYC × 3 Years Before	-0.023	-0.011	0.031	-0.032
	(0.006)	(0.007)	(0.004)	(0.285)
NYC × 2 Years Before	-0.012	-0.001	0.013	0.351
	(0.006)	(0.007)	(0.004)	(0.298)
NYC × 1 Year Before	-0.017	0.009	0.007	0.591
	(0.006)	(0.007)	(0.004)	(0.301)
NYC × 1 Year After	-0.023	0.004	0.020	-0.132
	(0.006)	(0.007)	(0.004)	(0.304)
NYC × 2 Years After	-0.022	0.014	0.021	0.217
	(0.006)	(0.007)	(0.004)	(0.302)
NYC × 3 Years After	-0.019	0.004	0.013	0.275
	(0.006)	(0.007)	(0.004)	(0.299)
NYC × 4 Years After	-0.014	0.020	0.003	0.973
	(0.006)	(0.007)	(0.004)	(0.295)
NYC × 5 Years After	-0.013	0.023	0.008	1.174
	(0.006)	(0.007)	(0.004)	(0.294)
NYC × 6 Years After	-0.021	0.023	0.011	1.141
	(0.006)	(0.007)	(0.004)	(0.296)
NYC × 7 Years After	-0.032	0.031	-0.000	1.037
	(0.006)	(0.007)	(0.004)	(0.296)
NYC × 8 Years After	-0.028	0.037	0.003	1.211
	(0.006)	(0.007)	(0.004)	(0.293)
NYC × 9 Years After	-0.013	0.029	-0.009	0.972
	(0.006)	(0.007)	(0.004)	(0.292)
NYC × 10 Years After	-0.025	0.020	0.003	0.568
	(0.006)	(0.007)	(0.004)	(0.292)
NYC × 11 Years After	-0.032	0.027	-0.009	0.807
	(0.006)	(0.007)	(0.004)	(0.294)
R^2	0.49	0.34	0.07	0.32
N	1,148,771	1,148,771	1,148,771	1,148,771

Notes: The regression estimates using equation (1) are reported. Controls that are included but not reported are sex, age, income, education, county, and state. Fixed effects include month, month-year, and year. The x-axes represent years since the implementation of the TFA ban in NYC. All coefficients are in comparison with year 0, which is equivalent to 2007.

To investigate the health mechanisms through which these labor market effects are occurring, I estimate the effect of the TFA ban on the incidence of a heart condition. Table 3.5 reports the estimated effects of a TFA ban on the incidence of a heart condition. Since the frequency of the HRS survey is every other year and the sample sizes are relatively small, I bin years together into 5 & 7 years before the ban, 1 & 3 years before the ban, 1 & 3 years after the ban, and 5 & 7 years after the ban. The reference bin is 1 & 3 years before the ban. The baseline specification using upstate NY as a control group, as shown in the first column, estimates that the incidence of a heart condition decreased by 2.3 percentage points 1-3 years after implementation of the TFA ban.

Smaller estimates are found when using respondents who live in other large MSAs and all other respondents in the survey, at -1.0 and -1.3 percentage points. The estimates for 5-7 years after the ban are much smaller. Since the HRS is a repeated longitudinal panel, the last three columns of Table 5 report the same estimates but include individual level fixed effects. In this specification, only respondents who are in the panel at least once before and at least once after implementation contribute to the coefficients of interest. The estimates for 1-3 years after the ban are smaller, between -0.9 and -1.6 percentage points when using individual fixed effects. However, the estimates for 5-7 years after the ban remain larger in magnitude at between -0.3 and -2.2.

3.6 Discussion and Conclusion

The estimates from the CPS show that the ban on TFAs in NYC increased the percent of those working by an average of 3.4 percentage points, and increased average

Table 3.5: HRS Heart Condition Effect Estimates: Comparing Control Groups

	No Ind. FE			Ind. FE		
	NY	MSA	Other	NY	MSA	Other
NYC × 5-7 Years Before Ban	0.016	-0.013	-0.014	0.033	0.005	0.004
	(0.020)	(0.014)	(0.014)	(0.016)	(0.011)	(0.011)
NYC × 1-3 Year After Ban	-0.023	-0.010	-0.013	-0.013	-0.009	-0.016
	(0.016)	(0.010)	(0.010)	(0.016)	(0.011)	(0.011)
NYC × 5-7 Years After Ban	0.008	-0.000	-0.003	-0.003	-0.012	-0.022
	(0.022)	(0.010)	(0.010)	(0.018)	(0.012)	(0.012)
Female	-0.042	-0.063	-0.072	-0.114	-0.082	-0.053
	(0.017)	(0.007)	(0.006)	(0.113)	(0.050)	(0.035)
Age	0.012	0.011	0.011	0.015	0.014	0.014
	(0.002)	(0.000)	(0.000)	(0.001)	(0.000)	(0.000)
R^2	0.10	0.07	0.06	0.06	0.06	0.07
N	7,905	67,320	139,927	7,905	67,320	139,927

Notes: The regression estimates using equation (1) are reported. Controls that are included but not reported are sex, age, income, education, county, and state. Survey year fixed effects are included in all specifications. The last three columns also include individual level fixed effects.

hours worked per week by 1.5 hours. Both percent working and hours worked per week began increasing in comparison to the control group four years after implementation of the ban, and continued increasing until 7 years after the ban, after which the effects remained relatively constant. Estimates suggest that these effects were driven by a decrease in the percent of those unable to work, and not by a decrease in the percent of those retired. Further analysis with the HRS data supports previous findings in the literature that the ban on TFA did decrease the prevalence of heart conditions. Overall, the findings presented in this paper extend the literature by providing the first estimates of how a nutrition based health shock affected labor market outcomes for the aging population.

APPENDIX A
APPENDIX TO CHAPTER 1

The webscrape for the Yelp data collection was originally written to collect data for a different project, funded under the National Institute of Diabetes and Digestive And Kidney Diseases of the National Institutes of Health under Award Number R01DK107686. This paper analyzes the effect of the calorie posting aspect of the Affordable Care Act (Frisvold, Courtemanche, and Price, 2017). The specific aim of that project is to determine whether and why the Affordable Care Act (ACA) menu labeling requirement for restaurants impacts obesity by examining changes in consumer behavior and restaurant menus.

The geographical areas of interest for this calorie posting project were created based on counties in the U.S. which had already enacted a calorie posting law for restaurants prior to the enactment of the ACA. These areas which had already enacted a calorie posting law were defined as the control groups, and the surrounding areas which had not yet enacted a calorie posting law and were affected by the ACA requirement were defined as the treatment groups. The control groups included New York City, NY, Philadelphia County, PA, King County, WA, Albany and Schenectady Counties, NY, Montgomery County, MD, and Vermont. The treatment groups included New York City MSA, Philadelphia MSA, Seattle MSA, Washington, DC MSA, Albany, NY MSA and Connecticut, Maine, Massachusetts, New Hampshire, and Rhode Island. The list of areas for data collection was thus based on obtaining a representative sample from these treatment and control groups.

I began collecting Yelp data in April, 2016, and continued data collection quarterly thereafter. Data collection began on the 15th of the first month of each quarter. The first two waves of the Yelp scrape took approximately two months for each round, but after improving the program, the subsequent scrapes took approximately two weeks for each round. This is why there is substantial variation in the time between observations for the Yelp restaurants.

After three rounds of Yelp data collection, it became apparent that restaurants may not consistently post updated menu prices. To examine this potential concern, I began menu data collection using a second source, Grubhub, in December 2016. Figure A4 depicts the timeline of the data collection for both sources and the minimum wage policies. The webscrape for Yelp and Grubhub work in a similar manner. For both sources, the scrapes iterate through each area of interest, creating a list of the web page links for all restaurants. The same order of areas is used in each wave of data collection. The scrapes then randomize these restaurants and iterate through each location saving the home page and menu page for each restaurant.

After data collection is complete, I use a parsing program to manipulate the restaurant menu data into a usable format. As noted in the paper, for restaurants in the Yelp dataset, only restaurants with a uniform Yelp menu are parsed for analysis. For the round of data collection in April, 2016, the other externally formatted Yelp menus were hand entered to examine restaurant characteristics. These externally formatted menus include PDF menus and other non-Yelp HTML menus. Table A1 reports these results, comparing restaurant characteristics from the Yelp formatted menus and the externally formatted menus in April 2016. These restaurants are statistically similar on average price, percent of limited service restaurants, and percent of franchise restaurants. The external menus have more menu items, higher star ratings, lower sales volume and a smaller number of employees.

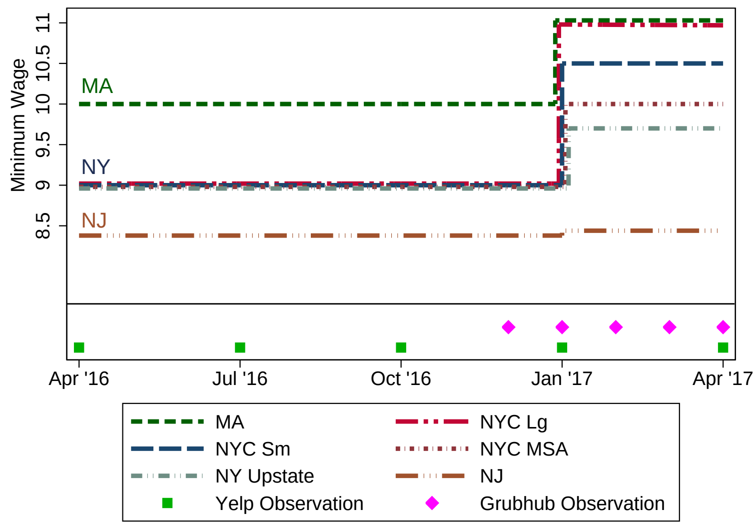


Figure A1.1: Data Collection and Minimum Wage Policy Timeline

Notes: Minimum wage is measured in U.S. dollars. All policy changes went into effect January 1, 2017. The plots of Yelp and Grubhub observations represent the month in which each round of data collection began. Data collection for both sources began on the 15th of each given month. Yelp data was collected in April 2016, July 2016, October 2016, January 2017, and April 2017. Grubhub data was collected in December 2016, January 2017, February 2017, March 2017 and April 2017. Minimum wage groups and policies are defined in detail in Section 2 of the paper.

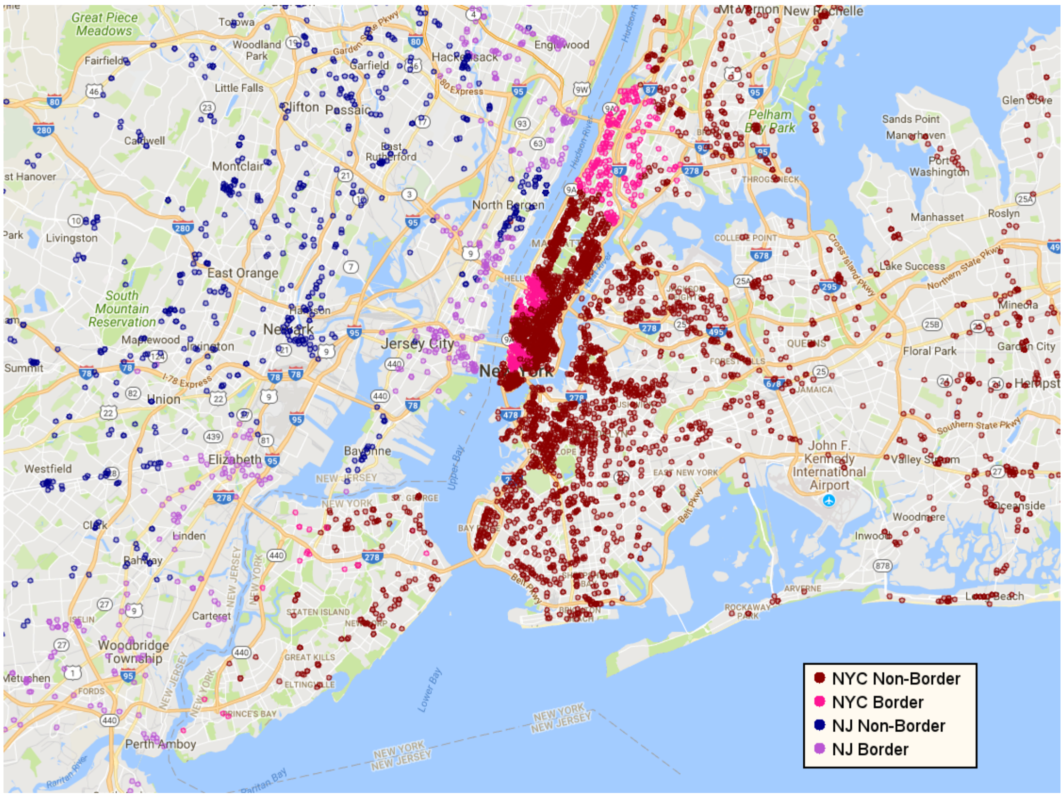
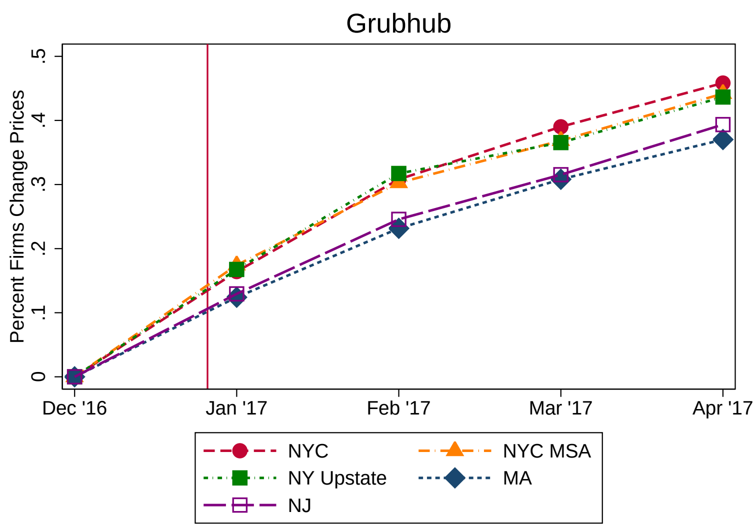


Figure A1.2: Geographic Location of Restuarants Used in Border Effects Analysis

Notes: Each data point represents a restaurant in the Grubhub dataset in NJ and NYC. Samples are color coded by the magnitude of the increase in the minimum wage on January 1, 2017. The highlighted restaurants in each group represent the restaurants that are within twelve minutes from the border and are used in the border effects analysis.



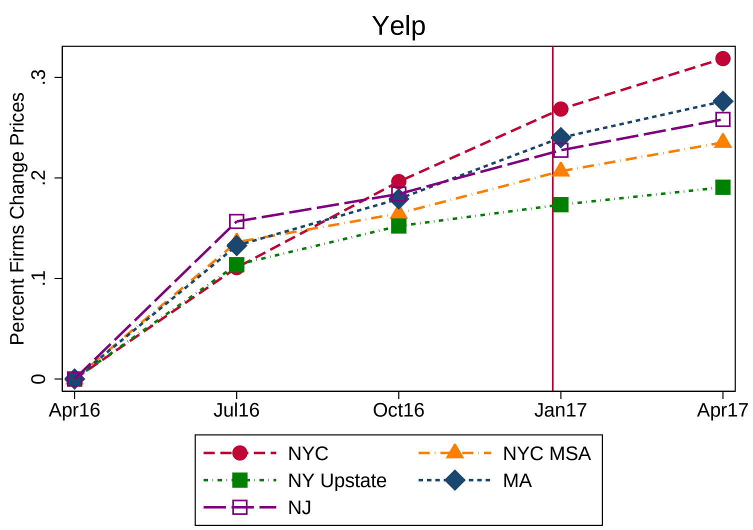


Figure A1.3: CDF of Restuarants Updating Prices

<u>Notes:</u> Each point on the graph represents the percent of restaurants in the sample that had updated prices by the given point in time. Updating prices is defined as changing the price of at least one menu item.

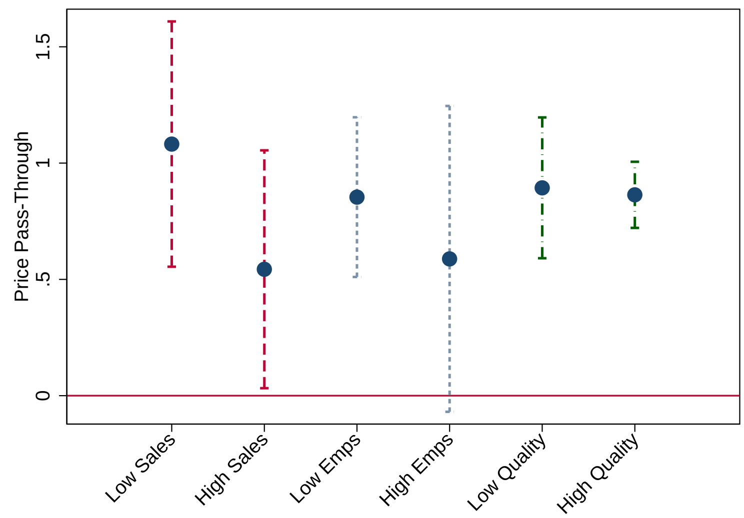


Figure A1.4: Pass-Through By Restaurant Characteristics

Notes: Point estimates and 95% confidence intervals of the total price pass-through using the Grubhub data are depicted by restaurant characteristics. Low sales restaurants are those firms in the lower third of annual sales volume at less than 190k. High sales restaurants are those firms in the higher third of annual sales volume at over 497k. Low employee restaurants are those in the lower third of number of employees with less than 3 employees, where high employee firms have over 9. Low quality firms are those that started with an average quality rating in the lower third of customer ratings with a rating of lower than 78.2. High quality firms are those who started with an average quality rating of over 94.6.

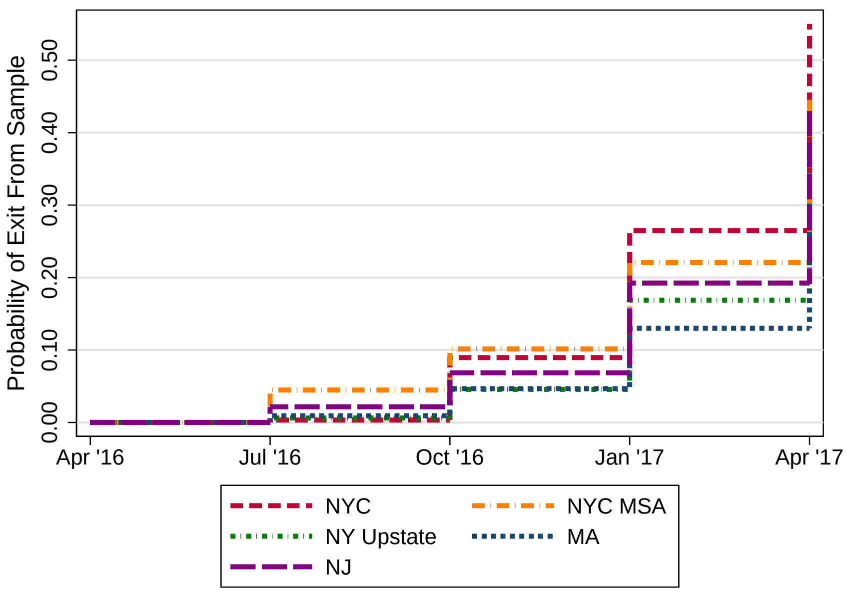


Figure A1.5: Hazard Function for Exit From the Sample

Notes: The Kaplan Meier hazard functions for exit from the sample are depicted by minimum wage group using the Yelp sample. The χ^2 value of the log-rank test for equality of these survivor functions across groups is 367.81 and the corresponding P value is 0.000.

Table A1.1: Yelp Formatted - Externally Formatted Menu Comparison

	(1) Yelp Menus	(2) External Menus	(3) F Test Sig.
Price	10.224	10.234	0.957
	(0.067)	(0.127)	
Number of Items	85.472	103.821	0.000
	(0.545)	(1.911)	
Stars	3.552	3.712	0.000
	(0.004)	(0.011)	
Limited Service	0.055	0.060	0.372
	(0.001)	(0.004)	
Franchise	0.020	0.019	0.835
	(0.001)	(0.002)	
Sales (100k)	905.036	653.031	0.012
	(22.244)	(21.426)	
Employees	12.845	10.524	0.002
	(0.163)	(0.256)	
N	29559	3657	

Notes: The means and standard errors of all baseline characteristics are reported. Price, stars and total items are calculated using the online menu data. Limited service, franchise, sales volume and number of employees are calculated using the RUSA matched restaurants. All restaurants were collected in the April 2016 wave. Column 3 reports the p-values for the means test for each variable.

Table A1.2: Price Pass Through By Restaurant Characteristics

	(1) Low Sale	(2) High Sale	(3) Low Emp	(4) High Emp	(5) Low Stars	(6) High Stars
$Apr16 - Jul16$	0.288	0.182	0.249	0.122	-0.076	-0.003
	(0.145)	(0.271)	(0.147)	(0.260)	(0.142)	(0.182)
$Jul16 - Oct16$	0.249	-0.073	0.286	0.019	-0.131	0.028
	(0.182)	(0.204)	(0.140)	(0.245)	(0.126)	(0.073)
$Oct16 - Jan17$	0.260	0.096	0.364	0.032	0.170	0.033
	(0.126)	(0.121)	(0.119)	(0.155)	(0.064)	(0.064)
$Jan16 - Apr17$	0.468	0.334	0.238	0.386	0.097	0.056
	(0.122)	(0.142)	(0.132)	(0.176)	(0.130)	(0.094)
Total Pass Through	0.728	0.43	0.602^{+}	0.418^{+}	0.267	0.089
	(0.244)	(0.262)	(0.231)	(0.331)	(0.19)	(0.156)
N	1556	1723	2142	1894	2020	2995
NxT	6224	6892	8568	7576	8080	11980

+ statistically different than comparison group

Notes: The reported estimates compare price pass-through of restaurants in the lowest and highest third based on sales, employees, and number of stars in April 2016 using the Yelp dataset. The outcome variable for all columns is the log change in price at the restaurant level. All standard errors are clustered at the minimum wage group level. The total pass-through estimates are linear combinations of the October '16 to January '17 and the January '17 to April '17 estimates. The cutoff values for sales are 204 and 598 thousand. The cutoff values for employees are 4 and 10, and the cutoff values for stars are 3 and 4.

Table A1.3: Changes in Yelp Service Specific Quality

	(1) All	(2) <= Median	(3) > Median
Apr16-Jul16	-0.086	0.341	0.024
	(0.025)	(0.015)	(0.361)
Jul16-Oct16	-0.181	0.272	-0.045
	(0.042)	(0.015)	(0.249)
Octr16-Jan17	-0.138	-0.144	0.007
	(0.046)	(0.010)	(0.861)
Jan17-Apr17	-0.133	-0.246	0.035
	(0.052)	(0.015)	(0.445)
Total Pass-Through	-0.272	-0.387	0.042
	(0.064)	(0.212)	(0.048)
N	5511	2856	2933
N x T	22045	11427	11732

Notes: All estimates are percentage point change in the proportion of positive Yelp service specific reviews. All standard errors are clustered at the minimum wage group level. Total percentage point change is a linear combination of the Oct 2016 to Jan 2017 and Jan 2017 to Apr 2017 estimates. Below the median and above the median restaurants are grouped together due to the small sample sizes.

APPENDIX B
APPENDIX TO CHAPTER 2

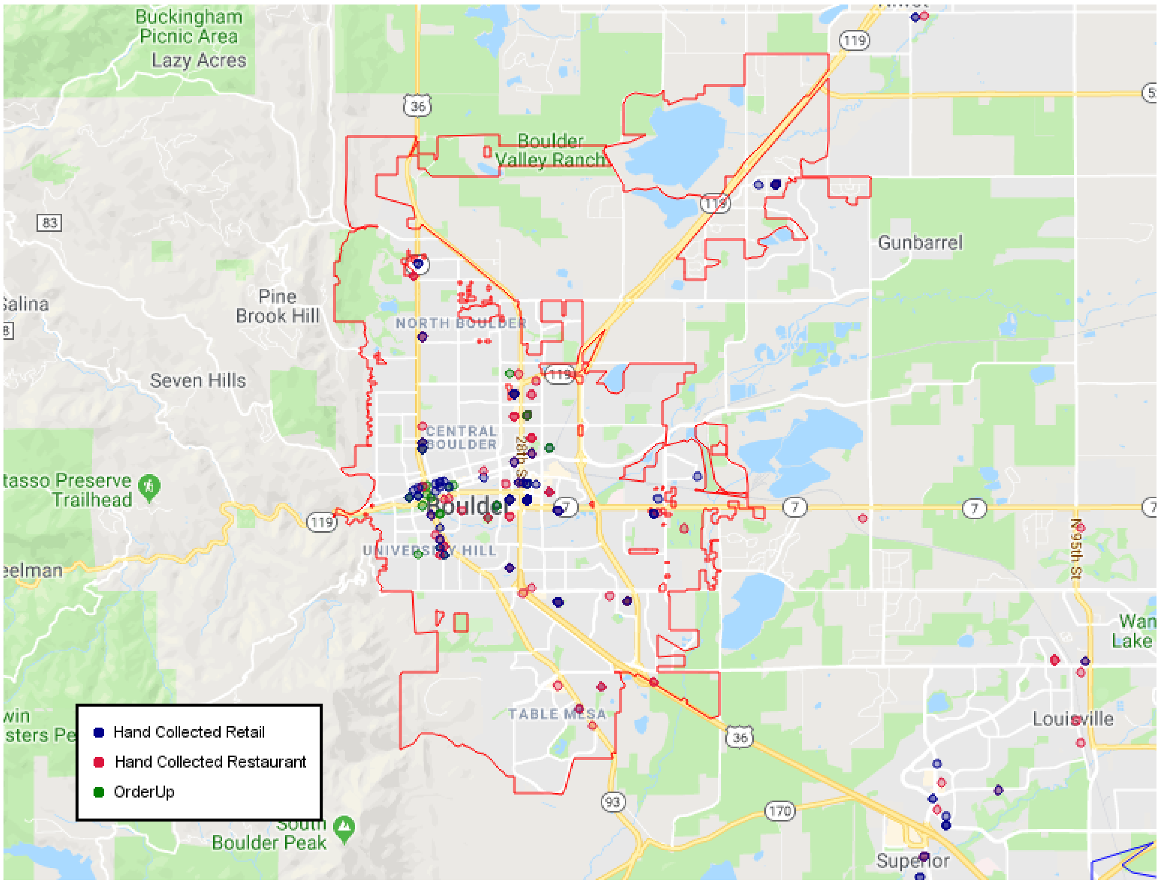


Figure A2.1: Geographic Location of Retail Stores, Hand Collected Restaurants, and OrderUp Restaurants in the City of Boulder

Notes: The red border signifies the city limits of Boulder. All hand collected retail stores and restaurants that had at least one observation throughout the four waves of data collection are included. In the city of Boulder, there are 77 hand collected retail locations and 113 hand collected restaurant locations. All OrderUp restaurants that are included in the balanced panel from March 22 to October 25, 2017 are included in the map. There are 42 OrderUp restaurants within the city limits of Boulder.

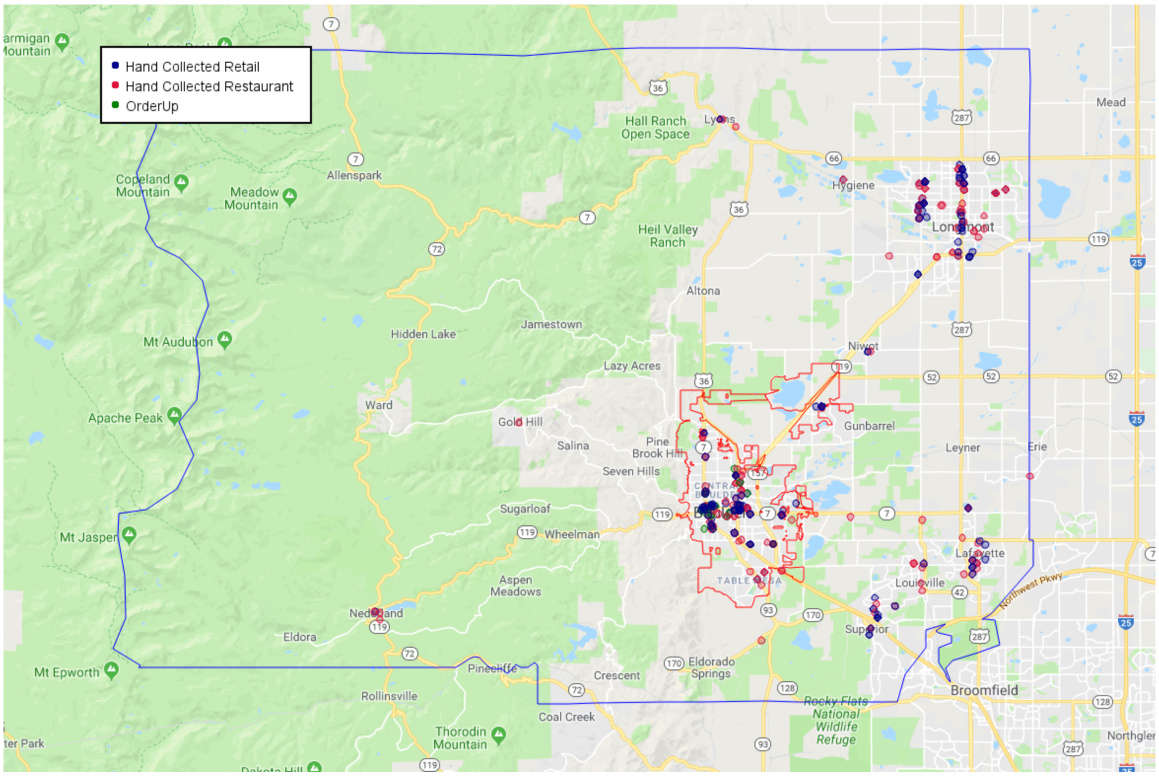


Figure A2.2: Geographic Location of Retail Stores, Hand Collected Restaurants, and OrderUp Restaurants in Boulder County

Notes: The red border signifies the city limits of Boulder. The larger, blue border signifies the county limits of Boulder County. All hand collected retail stores and restaurants that had at least one observation throughout the four waves of data collection are included. In Boulder County but outside of the city of Boulder, there are 102 hand collected retail locations and 132 hand collected restaurant locations. There are no OrderUp restaurants outside the city limits of Boulder but within Boulder County in the balanced panel.

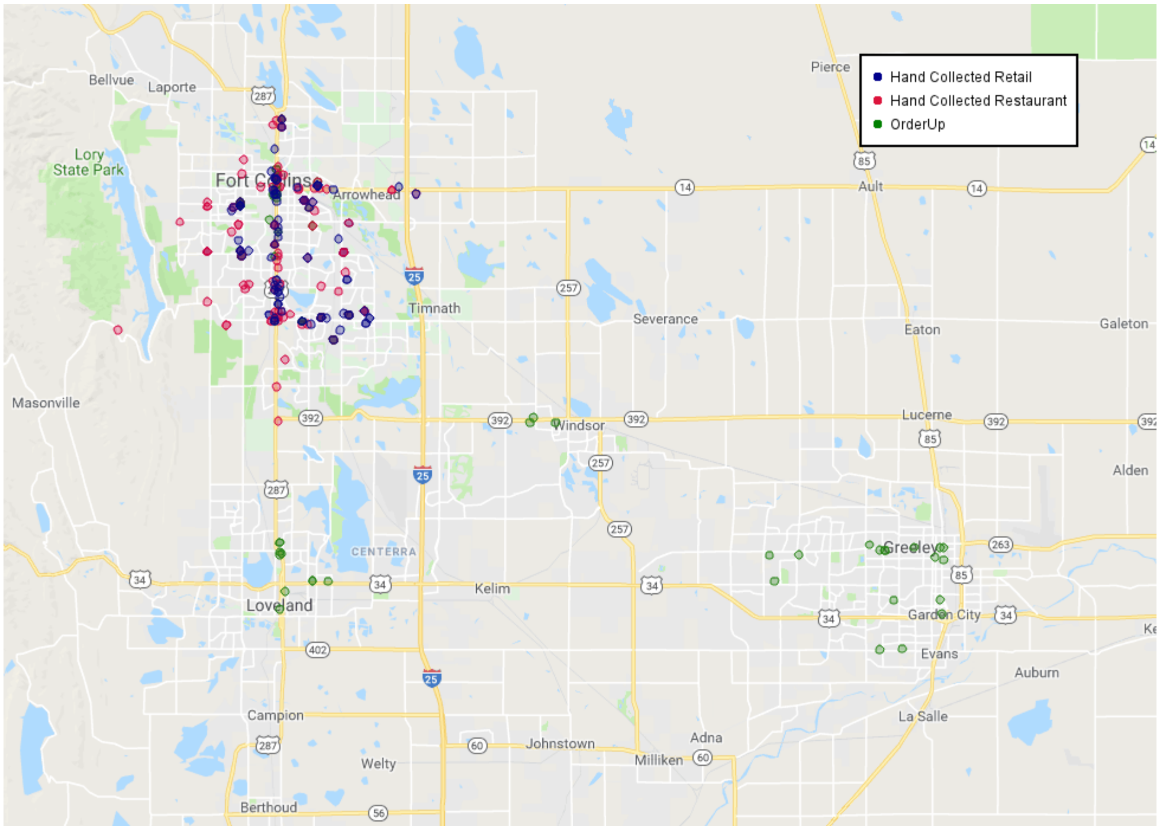


Figure A2.3: Geographic Location of Retail Stores, Hand Collected Restaurants, and OrderUp Restaurants in Fort Collins

Notes: All hand collected retail stores and restaurants that had at least one observation throughout the four waves of data collection are included. Outside of Boulder County in the Fort Collins area, there are 113 hand collected retail locations and 140 hand collected restaurant locations. All OrderUp restaurants that are included in the balanced panel from March 22 to October 25, 2017 are included in the map. There are 72 OrderUp restaurants in the Fort Collins area, which includes Fort Collins, Evans, Garden City, Greeley, Loveland, and Windsor.

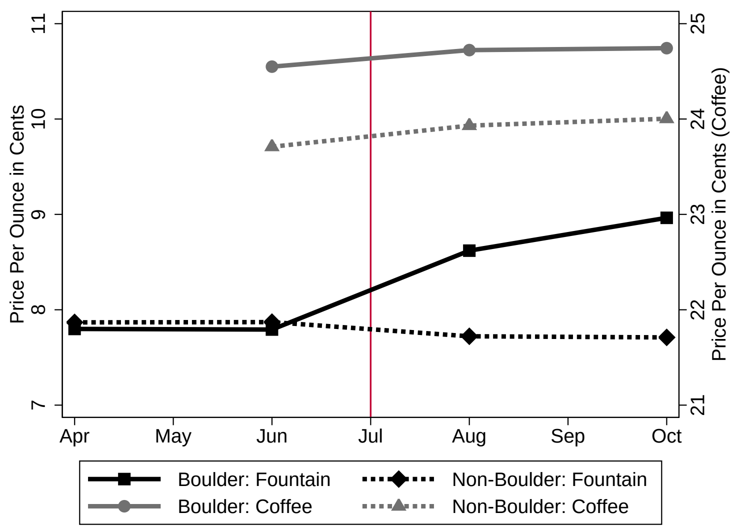


Figure A2.4: Trends in the Price per Ounce of Fountain Drinks and Coffee Drinks at Restaurants

Notes: Price per ounce is reported in cents. Fountain drinks are taxed items under the law in Boulder. Coffee drinks are not taxed under the Boulder law. The data are balance at the store-item level across all four waves of the data collection for fountain drinks, and across June, August, and October for the coffee drinks since those items were not part of the April data collection.

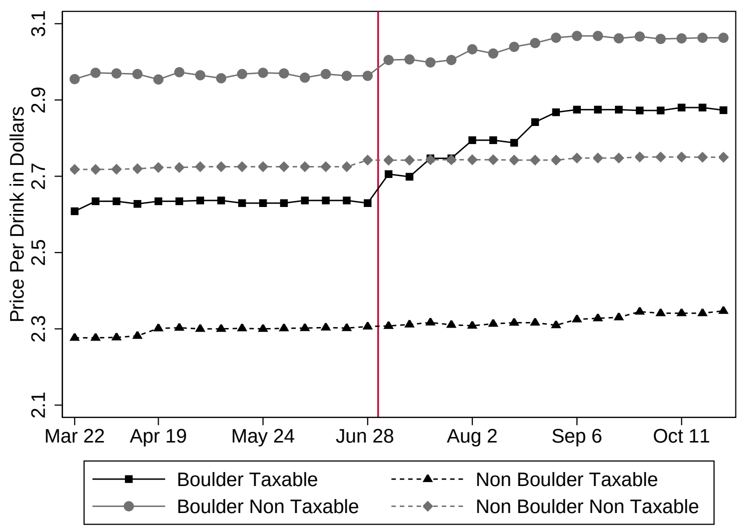


Figure A2.5: OrderUp Trends in the price per drink from March to October

Notes: Price per drink is reported in dollars. Taxed, not taxed and unknown items are defined according to whether the item is taxed under the law in Boulder. A complete list of the taxed status of items is shown in Appendix Table 3. The data are balance at the store-item level across all waves of the data collection.

Table A2.1: Description of Items from Retailers

Category	Item	Size (oz)	Taxed	Number of Stores Apr	Jun	Aug	Oct
Soda	Pepsi	20	Yes	144	190	185	189
	Pepsi	67.6	Yes	110	184	184	184
	Pepsi	12 x 12	Yes	118	151	150	149
	Diet Pepsi	20	No	140	181	179	185
	Diet Pepsi	67.6	No	107	163	167	175
	Diet Pepsi	12 x 12	No	115	142	150	144
	Mountain Dew	20	Yes	139	187	181	189
	Mountain Dew	67.6	Yes	104	167	175	182
	Coke	20	Yes	137	191	179	193
	Coke	67.6	Yes	113	198	203	198
	Coke	12 x 12	Yes	113	157	155	160
	Diet Coke	20	No	136	185	175	189
	Diet Coke	67.6	No	108	181	189	191
	Diet Coke	12 x 12	No	113	153	150	154
	Sprite	20	Yes	136	181	174	190
	Sprite	67.6	Yes	104	178	191	194
	7Up	20	Yes	118	159	153	147
	7Up	67.6	Yes	91	162	169	166
	Hansen's	12	Yes	0	8	12	13
	Hansen's	6 x 12	Yes	0	24	28	26
	San Pellegrino	11.15	Yes	0	26	31	35
	San Pellegrino	6 x 11.15	Yes	0	49	63	56

Notes: These items were collected in April, June, August, and October 2017. The April round of data collection did not include Hansen's Sodas, San Pellegrino, mixers or formula. In the April wave, 3,359 total item prices were collected from 174 retailers. In April, data collectors visited retailers to record prices in Boulder between April 3 and April 21, in Boulder County between April 3 and April 22, and in Fort Collins between April 3 and April 26. On May 16, the Boulder City Council exempted alcoholic mixers from the tax. In the June wave, 5,250 total item prices were collected from 286 retailers. In June, data collectors recorded prices in Boulder between May 30 and June 16, in Boulder County between May 30 and June 16, and in Fort Collins between June 1 and June 15. The tax was implemented on July 1. In the August wave, 5,337 total item prices were collected from 287 retailers. In August 2017, data collectors visited retailers in Boulder between August 4 and 19, in Boulder County between August 4 and 17, and in Fort Collins between August 8 and 21. In the October wave, 5,478 total item prices were collected from 288 retailers. In October 2017, data collectors recorded prices in Boulder between October 11 and 23, in Boulder County between October 9 and 27, and in Fort Collins between October 11 and 29.

Table A2.2: Description of Items from Retailers (Continued)

				Number of Stores			
Category	Item	Size (oz)	Taxed	Apr	Jun	Aug	Oct
Energy Drinks	Red Bull	8.4	Yes	129	199	206	217
	Red Bull	4 x 8.4	Yes	99	116	131	134
	Red Bull Sugar Free	8.4	No	119	193	191	217
	Red Bull Sugar Free	4 x 8.4	No	66	96	99	111
Sports Drinks	Gatorade	20	Yes	79	138	156	157
	Gatorade G2	20	Yes	23	24	8	6
Iced Tea	Arizona	23	Yes	101	160	159	154
	Arizona	128	Yes	50	56	58	57
Juice	Tropicana Orange Juice	12	No	61	70	69	70
Water	Dasani	20	No	101	120	119	124
	Aquifina	20	No	110	132	133	150
Mixers	Jose Cuervo Margarita Mix	33.8	No	0	38	36	47
	Jose Cuervo Margarita Mix	59.2	No	0	66	61	69
	Tres Agaves Margarita Mix	33.8	No	0	37	41	42
	Mr. T Bloody Mary Mix	33.8	No	0	83	86	70
	Mr. T Bloody Mary Mix	59.2	No	0	57	54	58
Other	GT Kombucha	16	No	38	58	62	68
Fountain Drinks	Small	-	Yes	60	91	101	100
	Medium	-	Yes	52	86	95	95
	Large	-	Yes	53	72	83	80
	Extra Large	-	Yes	20	31	25	21

Notes: These items were collected in April, June, August, and October 2017. The April round of data collection did not include Hansen's Sodas, San Pellegrino, mixers or formula. In the April wave, 3,359 total item prices were collected from 174 retailers. In April, data collectors visited retailers to record prices in Boulder between April 3 and April 21, in Boulder County between April 3 and April 22, and in Fort Collins between April 3 and April 26. On May 16, the Boulder City Council exempted alcoholic mixers from the tax. In the June wave, 5,250 total item prices were collected from 286 retailers. In June, data collectors recorded prices in Boulder between May 30 and June 16, in Boulder County between May 30 and June 16, and in Fort Collins between June 1 and June 15. The tax was implemented on July 1. In the August wave, 5,337 total item prices were collected from 287 retailers. In August 2017, data collectors visited retailers in Boulder between August 4 and 19, in Boulder County between August 4 and 17, and in Fort Collins between August 8 and 21. In the October wave, 5,478 total item prices were collected from 288 retailers. In October 2017, data collectors recorded prices in Boulder between October 11 and 23, in Boulder County between October 9 and 27, and in Fort Collins between October 11 and 29.

Table A2.3: Description of Items from Hand Collected Restaurants

Category	Item	Size (oz)	Taxed	Number of Restaurants Apr	Jun	Aug	Oct
Fountain Drinks	Small	-	Yes	235	228	222	226
	Medium	-	Yes	208	201	202	203
	Large	-	Yes	126	125	119	121
	Extra Large	-	Yes	22	21	27	27
Coffee Drinks	Drip Coffee	12	No	-	161	128	129
	Latte	12	No	-	133	129	128
	Mocha Latte	12	No	-	127	126	123
	Hot Chocolate	12	No	-	121	126	126

Notes: These items were collected in April, June, August, and October 2017. The April round of data collection did not include coffee shops. In the April wave, 591 total item prices were collected from 236 retailers. In the June wave, 1,117 total item prices were collected from 321 retailers. In the August wave, 1,079 total item prices were collected from 318 retailers. In the October wave, 1,084 total item prices were collected from 317 retailers. The timing of data collection is the same as that described in the notes of Appendix Table 1.

Table A2.4: Description of Items from OrderUp

Item	taxed	Number of Stores In Each Wave	Item	taxed	Number of Stores In Each Wave
1% Low Fat Milk	No	1	Lassi	No	8
100% Juice	No	6	Latte	No	10
2% Milk	No	2	Lemonade	Yes	18
A&W Root Beer	Yes	1	Mango Juice	Unknown	1
Allegro Coffee	No	1	Matcha	No	3
Americano	No	8	Mello Yello	Yes	1
Amp Energy Drink	Yes	1	Mexican Coke	Yes	1
Apple Juice	Unknown	10	Mexican Fanta	Yes	1
Arabic Coffee	No	2	Mexican Soda	Yes	1
Arizona Flavored Tea	Yes	9	Milk	No	25
Banana Milk	No	2	Minute Maid	Unknown	2
Baristo	Unknown	1	Minute Maid Lemonade	Yes	3
Barq's Root Beer	Yes	7	Monster	Yes	4
Big Yellow Cup	Unknown	4	Mountain Dew	Yes	9
Black Tea	No	2	Mountain Dew Kick Start	Yes	2
Boba Tea	Unknown	9	Mr. Pibb	Yes	2
Blueberry Pomegranate Juice	No	2	Mug Rootbeer	Yes	1
Boylan Soda	Unknown	7	Nantucket Tea	Unknown	1
Cappuccino	No	5	NOS Energy Drink	Yes	1
Chai Tea	No	18	Oogave Ginger Ale	Yes	1
Cherry Coke	Yes	1	Oogave Rootbeer	Yes	1
Cherry Limeade	Yes	1	Oolong Tea	No	2
Cherry Pepsi	Yes	1	Orange Crush	Yes	2
Chocolate Milk	No	17	Orange Juice	Unknown	17
Coconut Water	No	3	Orange Pellegrino	Yes	2
Coffee	No	38	Orange Soda	Yes	1
Coke	Yes	27	Peach Tea	Yes	1
Coke Products	Unknown	2	Pepsi	Yes	11
Coke Zero	No	2	Pepsi Products	Unknown	1
Cold Brew	No	6	Perrier	No	2
Craft Soda	Yes	1	Pibb Extra	Yes	1
Cranberry Juice	No	3	Pink Lemonade	Yes	1
Dasani	No	10	Pomegranate Juice	No	1
Drink	Unknown	51	Pomegranate Pellegrino	Yes	1
Diet Barq's Root Beer	No	1	Powerade	Yes	7
Diet Coke	No	27	Raspberry Tea	Yes	5
Diet Pepsi	No	10	Red Bull	Yes	6
Dr. Pepper	Yes	22	Rockstar	Yes	2
Energy Drink	Yes	2	Rootbeer	Yes	7
Espresso	No	2	San Pellegrino	Yes	4
Fanta	Yes	10	Seltzer Water	No	1
Flavored Latte	No	31	Shirley Temple	Yes	1
Flavored Tea	Yes	6	Sierra Mist	Yes	9
Fountain Drink	Yes	29	Simply Apple Juice	No	2
Fruit Punch	Yes	3	Simply Lemonade	Yes	2
Gatorade	Yes	6	Simply Orange	No	2
Ginger Ale	Yes	5	Smart Water	No	1
Gold Peak Green Tea	Yes	1	Snapple	Yes	2
Gold Peak Sweet Tea	Yes	3	Sobe Life Water	No	2
Grapefruit Juice	Unknown	1	Soda	Unknown	32
Green Tea	No	1	Soy Milk	No	1
GT Kombucha	No	1	Sparkling Ginger Lime Juice	Yes	1
Herbal Tea	No	1	Sparkling Lime Juice	Yes	1
Hi-C	Yes	4	Sparkling Water	No	1
Honest Tea	Unknown	2	Sparkling Orange Drink	Unknown	1
Hot Chocolate	No	9	Sprite	Yes	24
Hot Cider	Yes	1	Sprite Zero	No	1
Hot Tea	No	14	Stewart's Soda	Yes	1
Hubert's Lemonade	Yes	2	Strawberry Lemonade	Yes	4
IBC Cream Soda	Yes	1	Sweet Tea	Yes	14
IBC Rootbeer	Yes	1	Tea	No	9
Iced Coffee	No	2	Thai Tea	No	16
Italian Soda	Yes	1	Tomato Juice	No	2
Izze	Yes	2	Tropicana Lemonade	Yes	1
Jarritos	Yes	5	Unsweetened Tea	No	10
Juice	Unknown	5	Vitamin Water	Yes	4
Kombucha	No	2	Water	No	29
Lacroix	No	1	Yoo-hoo	Yes	1

Notes: These items were collected weekly from menus on OrderUp from March 22, 2017 to October 25, 2017, for a total of 32 weeks of observations. On these online menus, some beverage menu items have a general name (e.g. soda), and the customer must choose a more specific item when they check out (e.g. Coke). The webscrape only saves initial menu item names, thus the taxed status of some items is unknown.

Table A2.5: Heterogeneity in Pass-Through Estimates By Specific Items

	Coke 20oz	Coke 2L	D. Coke 20oz	D. Coke 2L	Pepsi 20oz	Pepsi 2L	D. Pepsi 20oz	D. Pepsi 2L	Mt. Dew 20oz	Mt. Dew 2L
Bould $\times$ *Apr*	0.018	-0.006	0.096	-0.075	0.169	-0.101	0.175	-0.097	0.195	-0.125
	(0.140)	(0.212)	(0.122)	(0.189)	(0.105)	(0.167)	(0.098)	(0.150)	(0.132)	(0.165)
Bould $\times$ *Aug*	1.670	1.424	0.213	0.216	1.634	1.528	0.262	0.215	1.696	1.664
	(0.204)	(0.191)	(0.155)	(0.212)	(0.195)	(0.207)	(0.154)	(0.172)	(0.227)	(0.207)
Bould $\times$ *Oct*	1.462	1.370	0.218	0.169	1.797	1.536	0.213	0.189	1.784	1.572
	(0.209)	(0.199)	(0.150)	(0.192)	(0.206)	(0.205)	(0.143)	(0.188)	(0.211)	(0.213)
N	227	247	222	238	222	224	218	210	222	214
N x T	700	712	685	669	708	662	685	612	696	628
Mean	9.148	3.227	9.172	3.225	9.247	3.156	9.272	3.141	9.249	3.16
R^2	0.875	0.879	0.844	0.854	0.87	0.874	0.874	0.824	0.865	0.891

	Sprite 20oz	Sprite 2L	7 Up 20oz	7 Up 2L	Red Bull 8.4oz	SF Red Bull 8.4oz	Gat 20oz	Az. Tea 23oz	Dasani 20oz	Aquafina 20oz
Bould $\times$ *Apr*	0.028	-0.046	-0.177	-0.093	-1.820	-2.130	-0.559	-0.717	-0.209	-0.058
	(0.131)	(0.265)	(0.253)	(0.184)	(0.676)	(0.733)	(1.261)	(0.407)	(0.217)	(0.206)
Bould $\times$ *Aug*	1.642	1.526	1.312	1.394	1.394	0.083	0.948	1.661	0.032	0.212
	(0.207)	(0.211)	(0.381)	(0.263)	(0.355)	(0.657)	(0.391)	(0.248)	(0.153)	(0.278)
Bould $\times$ *Oct*	1.504	1.457	1.128	1.446	1.243	0.137	0.849	1.829	0.037	-0.030
	(0.207)	(0.215)	(0.315)	(0.244)	(0.401)	(0.444)	(0.348)	(0.319)	(0.167)	(0.123)
N	223	241	211	214	256	250	197	197	165	178
N x T	681	667	577	588	750	720	530	574	464	525
Mean	9.202	3.221	8.859	3.074	28.693	28.63	8.103	4.321	7.758	7.948
R^2	0.875	0.894	0.877	0.824	0.792	0.621	0.803	0.778	0.939	0.899

Notes: Results in this table are calculated using products from the hand-collected retail data and the prices charged at the register. The dependent variable is the price in cents per ounce. Items that are taxed include Coke, Pepsi, Mountain Dew, Sprite, 7 Up, Red Bull, Gatorade, and Arizona Iced Tea. Untaxed items include Diet Coke, Diet Pepsi, Sugar Free (SF) Red Bull, Dasani Water, Aquafina Water. The estimates show the change in the number of cents per ounce of the retail price relative to the prices in June in Boulder County and Fort Collins. Standard errors, in parentheses, are clustered at the store level. Additional variables that are included, but not shown, are community fixed effects, month fixed effects, store fixed effects and product fixed effects. N represents the number of unique store specific items, *N x T* represents the number of unique store specific item observations across all waves. $Mean$ is the pre-tax average price per ounce in cents.

APPENDIX C
APPENDIX TO CHAPTER 3

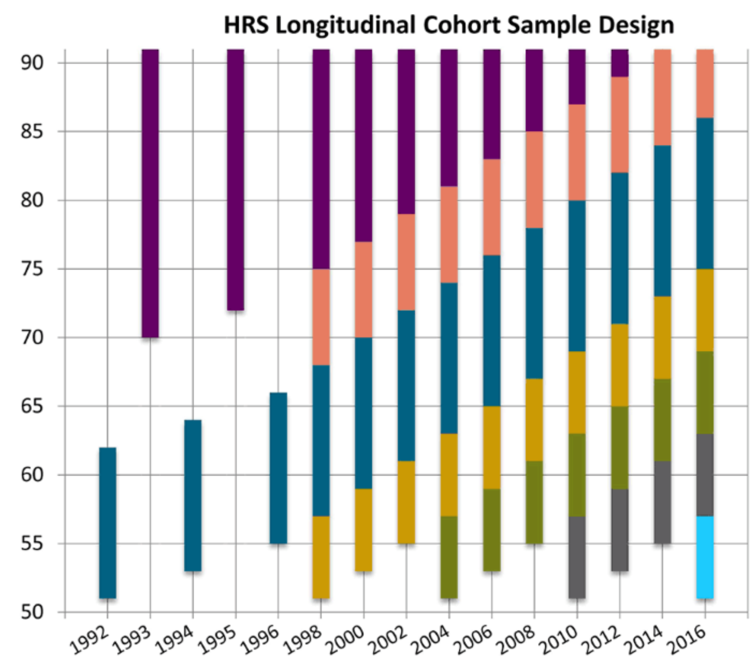


Figure A3.1: HRS Longitudinal Cohort Sample Design

Notes: This figure depicts the longitudinal sample design for the HRS.

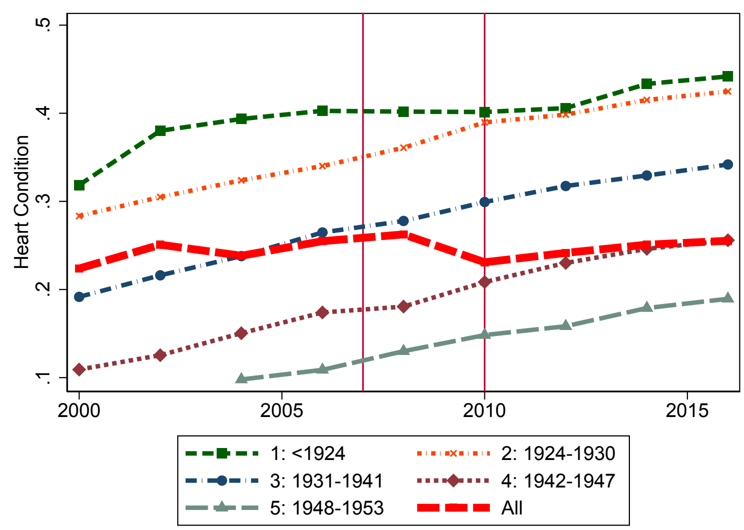


Figure A3.2: HRS Heart Condition Trends By Cohort

Notes: This figure depicts the trends in incidence of heart condition over time and by cohort.

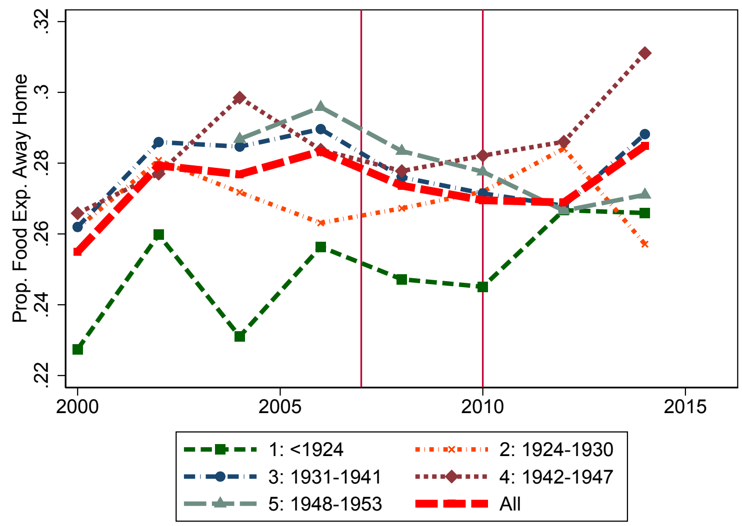


Figure A3.3: HRS Food Expenditure Trends By Cohort

Notes: This figure depicts the trends in proportion of total food expenditures spent on food away from home over time and by cohort.

Table A3.1: HRS Rate Health Effect Estimates: Comparing Control Groups

	No Ind. FE			Ind. FE		
	NY	MSA	Other	NY	MSA	Other
NYC × 5-7 Years Before Ban	-0.128	-0.027	-0.034	-0.156	-0.091	-0.091
	(0.047)	(0.037)	(0.036)	(0.044)	(0.033)	(0.033)
NYC × 1-3 Year After Ban	0.016	0.028	0.011	0.063	0.083	0.071
	(0.030)	(0.024)	(0.023)	(0.046)	(0.034)	(0.034)
NYC × 5-7 Years After Ban	0.017	-0.003	-0.023	0.107	0.105	0.102
	(0.049)	(0.045)	(0.045)	(0.050)	(0.037)	(0.037)
Female	-0.105	-0.039	-0.016	0.057	0.111	0.369
	(0.044)	(0.019)	(0.013)	(0.317)	(0.149)	(0.105)
Age	-0.023	-0.021	-0.019	-0.040	-0.038	-0.038
	(0.004)	(0.001)	(0.001)	(0.003)	(0.001)	(0.001)
R^2	0.07	0.05	0.05	0.04	0.05	0.05
N	7,906	67,330	139,943	7,906	67,330	139,943

Notes: The regression estimates using equation (1) are reported. Controls that are included but not reported are sex, age, income, education, county, and state. Fixed effects include month, month-year, and year.

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