המכון למחקר כלכלי בישראל
על-שם מוריס פאלק בע"מ (חל"צ)
The Maurice Falk Institute for
Economic Research in Israel Ltd.

Round Prices and Price Rigidity: Evidence from Outlawing Odd Prices

by

Itai Ater Omri Gerlitz

Discussion Paper No. 16.05

November 2016

בנין פרץ נפתלי, קמפוס האוניברסיטה העברית, הר הצופים, ירושלים 9190501
The Hebrew University Campus, MT. Scopus, 91905 Jerusalem, Israel
www.falk.huji.ac.il

Round Prices and Price Rigidity: Evidence from Outlawing Odd Prices[*]

By

Itai Ater and Omri Gerlitz

Tel Aviv Universuty

Abstract

This paper exploits a legal change in Israel that banned the use of non-zero-digit price endings (e.g., 6.99) to study the relationship between digit price endings and price rigidity. We compare the propensity of product prices to change before and after the ban, while distinguishing between products whose prices ended with a zero and products whose prices did not end with a zero digit before the ban. We find that before the ban, zero-digit price endings were more likely to change, typically upward, compared with products with non-zero digit price endings. After the legal change these differences disappeared. Overall, these findings support the Price Point Theory (Blinder 1991).

Keywords: price rigidity, price points, 9-ending prices

JEL Classification: E30, E51

The Maurice Falk Institute for Economic Research in Israel Ltd.

Jerusalem, November 2016 • Discussion Paper No. 16.05

[*] Itai Ater is a Senior Lecturer of Business Economics and Strategy at the Coller School of Management at Tel-Aviv University, Israel, ater@post.tau.ac.il. Omri Gerlitz is affiliated with The Berglas School of Economics, Tel Aviv University, Israel, omri.gerlitz@gmail.com. We are grateful for suggestions made by Daniel Levy, Oren Rigbi, Yehonatan Givati and Dan Zeltser. We thank the Falk Institute for Economic Research for financial support. Remaining errors are our own.

1 Introduction

Macroeconomists have long been interested in the sources and the magnitude of price rigidity, a key element in models of monetary economics. Intuitively, price rigidity hinders the "efficient" response of prices to economic shocks and therefore could have real effects on the economy.[1]

The primary explanation for price rigidity emphasizes that when firms change nominal prices they incur "menu costs", broadly defined to include physical costs of price adjustment and informational frictions (Gorodnichenko and Weber 2016). Due to these costs, firms refrain from adjusting prices as often as they might otherwise prefer (see Barro 1972; Sheshinski and Weiss 1977; Mankiw 1985 for early theoretical works). Empirical studies have attempted to identify the channels through which menu costs affect price stickiness and to quantify these costs. For instance, a study by Levy et al. (1997) exploits variation in item pricing laws across states and products to measure the costs associated with price stickiness. Anderson, Jaimovich and Simester (2015) identify a strong negative relationship between in-store labor costs associated with price changes and the likelihood of price changes. Finally, Gorodnichenko and Weber (2016) use stock price data to show that monetary changes have a stronger impact on the stocks of firms that have stickier prices. Yet, although menu costs are unquestionably the primary determinant of price rigidity, macroeconomists doubt that these costs can explain the phenomenon in its entirety. Rotemberg (2005) contends that "these administrative costs simply cannot be the whole story". Gorodnichenko, Sheremirov and Talavera (2016) write that "macroeconomic models of price rigidities, which emphasize menu costs and search costs, are likely incomplete ... more research is required to understand sources of price rigidities."

Another possible explanation for price rigidity, one that does not hinge on the standard assumptions of the menu cost explanation is the Price Point Theory (Blinder 1991; Blinder et al. 1998). Price Point Theory posits that firms are reluctant to make price changes that involve altering certain digit endings because they are concerned that consumers will respond disproportionately to these changes. In economic terms, a firm believes that there is a significant concave kink in its demand curve at the pricing points and that demand at these pricing points is much more elastic for small price changes, especially price increases. According to Blinder et al. (1998), this theory has its roots in the folklore of marketing and is more related to the concept of salience than to economists' standard model of optimizing

[1] Numerous studies have measured the periodicity and magnitude of price rigidity. See, for instance, Bils and Klenow 2004, Nakamura and Steinsson 2008 and Eichenbaum, Jaimovich and Rebelo 2011, and recent surveys by Klenow and Malin 2010, and Nakamura and Steinsson 2013.

behavior.[2] Consistent with the Price Point Theory, Kashyap (1995) finds that catalog prices indeed tend to be stuck at certain digit endings. Levy et al. (2011) use comprehensive data from a large U.S. supermarket chain and from Internet vendors, and show that prices ending with the digit "9" (9-ending prices) are less likely to change compared with other price endings.[3] Though these studies document evidence from different sources and different markets on the relationship between price endings and price rigidity, a primary concern with the interpretation of their findings is that the use of 9-endings is not random. In other words, retailers choose 9-endings for particular products or in circumstances that are unobserved (to the econometrician), and these factors may influence the duration of time for which these prices last. If this is indeed the case, then attributing price rigidity to price endings is misleading.

This paper aims to address this concern by studying the consequences of an Israeli legal change that banned the usage of the agora, the smallest coin denomination in Israel. The legal change, effective on January 1, 2014, prohibited using prices with non-zero endings (e.g., 5.99 [5 shekels and 99 agoras]; 10.45 [10 shekels and 45 agoras]), and only allowed the use of prices with "round", zero-endings (e.g., 7.60 [7 shekels and 60 agoras]). By investigating price stickiness before and after the legal ban, taking into account the tendencies of different products to end with a non-zero ending in the pre-ban period, we are able to examine the impact of odd-pricing on price rigidity.

The empirical analysis in this paper uses unique bi-monthly panel price data collected in hundreds of grocery stores throughout Israel, before and after the legal change came into effect. To get a sense of the impact of the legal ban on price-endings, Figure 1 shows the distributions of the 2-digit price-endings of the products in our sample in 2013 and in 2014, respectively. In 2013, before the ban on non-zero endings, the most popular 2-digit ending was "99": More than 40% of the prices in 2013 ended with "99", and overall, 60% of the prices had "x9" endings. Round prices (i.e., with zero-endings) constituted about 10% of the prices in 2013. In 2014, after the legal ban came into effect, non-zero endings disappeared, and the most popular 2-digit ending became "90" (accounting for nearly 60% of price-endings), with the endings "00" and "50" lagging far behind.

In our first regression analysis, performed at the product-store level, we classify the products sold in individual stores according to the likelihood that their prices ended in a

[2] The marketing literature (e.g., Anderson and Simester 2003, Thomas and Morwitz 2005, 2009, Schindler 2006) provides evidence that consumers demand is sensitive to the use of 9-endings. Anderson and Simester (2003), for instance, show that the use of 9-endings affects demand, especially for new items.

[3] Other studies that find evidence consisent with the Price Point Theory are: Hackl et al. (2014), Snir et al. (2014), Anderson et al. (2015), McShane et al. (2016), and Gorodnichenko et al. (2016). Basu (2006) offers a theoretical model and Lewis (2015) provides evidence that 9-ending prices help to facilitate collusion.

zero in the year preceding the legal ban. We compare the frequency of price changes for these products both in the year preceding the legal change, and in the year following the change. Our estimates indicate that, in 2013, products whose prices more frequently ended with a zero-digit were more likely to undergo price changes in that year compared to products whose prices less frequently ended with a zero-digit. In 2014, after the legal ban on odd-pricing, we do not find a significant difference in the likelihood of price changes between the same products. Figure 2 provides an illustratative example of these findings. The figure presents a time-series of the prices of the same item sold in the same city in two different stores. As can be seen in the graph, the prices of the product that more frequently ended with a zero-digit before the legal ban (in store 2), changed more often than the prices of the similar product which typically did not end with a zero-digit (in store 1). After the legal change, in 2014, this differences basically disappeared.

While the initial estimates lend support to the Price Point Theory, they are potentially driven by sample selection. That is, they are potentially driven by changes in the frequency at which different products were sampled before and after the legal ban, and by the time of the year in which the products were sampled in each year. To address these concerns, we further disaggregate the data to the product-store-fortnight level and focus on instances in which we identify the prices of a product that was sold in the same store and in the same fortnight in both 2013 and 2014. Using these balanced panel data, we estimate difference-in-differences specifications in which the control group contains products with round prices in a given fortnight in 2013 and a given store, and the treatment group contains the corresponding group of products with odd pricing in 2013. Our estimates in this analysis are qualitatively similar to the one obtained in our first analysis. Before the legal ban, prices that ended in a zero in a given period were 10 percent more likely to change by the subsequent period, compared to prices that ended with non-zero digits. Observing the same products during the corresponding periods in the subsequent year (i.e., after the legal ban), we find no such difference. In a separate analysis based on the balanced panel data, we also distinguish between price increases and price decreases. We find that before the legal change, prices with zero-digit endings were more likely to increase compared to prices with non-zero endings. With regard to price decreases, we find that prices with zero-digit endings were less likely to decrease compared to prices with non-zero endings. After the legal ban, these differences disappeared. To verify the robustness of our results, we experiment with several alternative specifications. We show, for instance, that the results are qualitatively similar if we focus on small or large price changes. Likewise, the results are qualitatively similar if we focus only on 0 and 9 price endings, or when we use alternative approaches to define a price change or to construct the balanced panel data. We also exclude price changes that are potentially temporary discounts and still obtain similar results. Thus, overall our findings provide strong evidence supporting the Price Point Theory as an important determinant of price rigidity.

In addition to contributing to the literature on price rigidity, this study also enhances our understanding of the consequences of the elimination of small coin denominations. To the best of our knowledge, this is the first empirical study that uses micro-level price data to examine the impact of a policy designed to abolish the use of small coin units. Like Israel, other countries, including India, Canada, Australia, Mexico, South Korea, Sweden, Denmark, Hong Kong, and New Zealand, have taken various steps towards the elimination of low-value coins from circulation.[4] The typical rationale for these policies is the potential savings associated with ceasing the production of small-value coins.[5] Yet, to our knowledge, the imapct of such attempts on price-setting behavior and indierctly on the actual savings has not been considered. Our findings indicate that following the legal change in Israel, price-setters did not adjust 99-price endings (e.g., 8.99) to shekel round prices (i.e., 9.00) but rather extensively used 90-digit ending prices (i.e., 8.90) (see Figure 1). This adjustment by price setters suggests that the implied savings should also take into account the cost of 10-agoras coins. In fact, rudimentary back-of-the-envelope calculations suggest that the demand for 10-agora coins in supermarkets increased by 25 percent following the legal change. These findings cast doubt on the expected savings from ceasing the production of the smallest unit coin because the production cost of the second-smallest coin units (e.g., 10 agora coins) is often also at a loss.[6] Ignoring these costs might result in overstating the expected savings from the elimination of the smallest coin denomination.

Finally, another unintended consequence of outlawing the agora is that it restricted the possible set of price changes and eliminated retailers' ability to make small price changes, of less than 10 agoras. Small price changes accounted for a substantial portion of total price changes in 2013: 13.8% of price increases and 9.8% of price decreases were for less than 10 agoras. After the legal change, these small price changes naturally disappeared. The elimination of the capacity to make small price changes is noteworthy, because recent studies (Chen et al. 2008 and Chakraborty et al. 2015) have shown that food retailers make many small price changes, arguably because these price changes fall under consumers' radar.

[4]For instance, starting on February 4, 2013, Canada began phasing the penny out of its coinage system, and in India, coins of 25 paise and below ceased to be legal tender starting on June 30, 2011.

[5]The Canadian Mint estimated that the savings from ceasing the production of the penny are $11 million a year, and according to United States Mints 2013 Annual Report, the production costs of the penny and the nickel resulted in losses of $104.5 million in 2013 alone. In the US the penny is still used in the coinage system. President Barack Obama, in response to a question regarding the future of the penny, responded that "Anytime we're spending money on something people don't actually use, thats an example of things we should probably change". See also (Lombra 2001) and John Oliver on pennies

[6]http://news.coinupdate.com/us-mint-cost-to-make-cent-and-nickel-declines-3113

The remainder of the paper is structured as follows. In Section 2, we discuss the institutional details, describe the data that we use, and present descriptive statistics. The empirical strategy and the regression analysis are presented in Section 3. We conclude in Section 4.

2 Background, Data and Descriptive Statistics

2.1 Institutional background

The Israeli shekel is the main unit of Israeli currency, and one shekel consists of 100 agoras.[7] Due to the low usage of 1 and 5-agora coins, coupled with the high cost of metal, these coins were abolished by the Bank of Israel on April 1, 1994 and January 1, 2008, respectively. Yet, the practice of setting and advertising prices using non-zero 2-digit endings (e.g,. 9.99, 4.75) remained common long after the coins ceased to exist. In practice, retailers were required, in accordance with rules issued by the Bank of Israel, to round the total purchase price to the nearest 10-agora ending: Prices ending in 1-4 agoras were rounded downward, and prices ending in 5-9 agoras were rounded upward. Given that retailers often set prices ending in the digit "9", consumers frequently ended up paying higher prices than the prices they observed.[8] Following consumer complaints that the sole purpose of posting prices with agora units was to misleadingly present a cheaper price than the actual price, the Israeli Consumer Protection and Fair Trade Authority initiated a legal change that would ban sellers from setting and advertising prices based on non-existent coins. On October 16, 2013, the Minister of Economy announced that, starting on January 1, 2014, the use of non-zero price endings (i.e., units of agoras) would be considered a criminal offense under the Consumer Protection Law of 1981.

Prices of grocery items in Israel are typically determined at the store level, and often vary across stores affiliated with the same chain. The cross-sectional price variation within chains is important because it enables us to examine the relationship between price rigidity and price digit-endings of the same product sold by different stores of the same chain in the same week. Using several chains in the analysis is also important, as Nakamura et al. (2011) have shown that pricing policies substantially differ across chains.

[7] On May 17, 2015, 1 agora was worth 0.262509 US cents. In the relevant time period of the study, the available coins were: 10 agoras, $\frac{1}{2}$ shekel, 1 shekel, 2 shekels, 5 shekels and 10 shekels.

[8] In Israel, posted prices are inclusive of taxes.

2.2 Data

The data used for this study, collected between March 2013 and December 2014, contain the posted prices, collected on a bi-monthly basis, of 325 products sold in 587 grocery stores affiliated with 28 chains. These chains include large and medium-size chains as well as several independent stores. The price data were collected by the Israel Consumer Council, the largest consumer organization in Israel. The Council's reviewers recorded prices of products that represent a standard shopping basket purchased by an Israeli consumer. Due to budgetary and managerial issues not all the products and all the stores were kept in the survey throughout the entire time period. In addition to the prices collected by the Consumer Council, we also collected weekly data from the websites of the five retail chains that offer online delivery services in Israel. Table A1 in Appendix A presents the list of chains and the number of stores affiliated with each chain. Table A2 in Appendix A lists the municipalities where these stores are located throughout Israel.

To avoid potential confounding factors associated with the immediate impact of the legal ban on price changes that occurred at the end of 2014, we exclude from the sample prices that were recorded between November 2013 and February 2014. Throughout the analysis, a price change is represented by an indicator variable that assumes the value of one if the price of the same product at the same store in the subsequent time period in which it was recorded has changed, and zero otherwise. We use this data to perform two empirical excercises. One at the product-store level and one at the product-store-fortnight level.

2.2.1 Product - store level

In our initial regression analysis we limit our sample to products whose prices were recorded at least twice in the same store. Following this restriction, our sample contains 208,439 prices of 95 different products sold in 338 stores affiliated with 26 chains. For each year, before and after the legal change, these prices are aggregated to the product/store level, and overall we use 12,199 product/store pairs, and 24,398 observations. For instance, the dependent variable that we construct from this dataset is the fraction of price changes of a 750g ketchup bottle sold in a given store in each year.

2.2.2 Product - store - fortnight level

In our second regression analysis performed at the product/store/fortnight level, we restrict attention to 42 products that were sold in the same store and in the same fortnight in both 2013 and 2014. To construct the balanced panel dataset, we characterize an item on the basis of three characteristics: its physical characteristics (e.g., 1.5-liter Coca Cola bottle), its point of sale (e.g., Shufersal store at 224 Ben-Gurion Street, Tel Aviv) and its time of sale in the year, in terms of fortnight in the Jewish calendar (e.g., the first two weeks of the Hebrew month Adar).[9] We use this characterization to match each item in 2013 with its counterpart item in 2014, and include in our data only matched items. Next, because the outcome variables we are interested in are price changes, we identify the price of each product/store/fortnight also in the subsequent period. We consider two approaches to define a subsequent period. In the first approach (the "broad" approach), a subsequent period is defined as the subsequent period in which the Council's reviewers collected the price of the product in the same store. This approach yields a data set consisting of 59,536 observations. The second ("narrow") approach is based on a more restrictive definition of a subsequent period, focusing only on instances in which the subsequent price of the product in the same store was collected within a one-month period. This approach leaves us with 41,962 observations. Finally, we assign a particular observation (product/store/fortnight) into either the control group or the treatment group according to the last digit of its price in the 2013 fortnight. Consequently, the control group consists of products with prices that ended with a zero-digit in 2013 (in a given store and fortnight), whereas the treatment group consists of products with prices that did not end with a zero digit in 2013 (in a given store and fortnight).

2.3 Descriptive statistics

In this subsection we present descriptive statistics that capture some of the changes following the legal ban. All the descriptive statistics are based on the balanced panel data set collected according to the broad approach, though similar patterns are found in the narrow panel data.

Table 1 displays descriptive statistics for the top 25 products in our sample, including, for each product, the fraction of prices recorded that ended with a zero-digit in 2013. The table shows that there is considerable variation across products in the use of zero-digit price endings. For instance, more than 20% of the observed prices of ketchup and shampoo ended with a zero-digit in 2013, whereas prices of products such as tea bags and white potatoes almost never ended with a zero-digit. The data presented in columns 2-5 demonstrate that there is also considerable variation in prices across stores and over time. For instance, in 2013, the price of a 750-ml bottle of ketchup ended with a zero-digit in 23% of the observations collected from a Shufersal store on Ibn Gvirol Street in Tel Aviv, and not even once at

[9] We use the Hebrew calendar in our main specification because Jewish holidays likely affect food sales. In Israel, the Gregorian calendar determines most of the civil events such as the beginning and the end of the education system. As a robustness test we also use the Gregorian calendar to match fortnights in 2013 and in 2014. The results are qualitatively similar.

the Mega store located on the same street.[10] Price endings also varied considerably across different products within individual stores. For instance, in 2013, the price of a 700-ml bottle of shampoo at a Shufersal store on Yigal Alon Street in Tel Aviv had a zero-ending in 75% of the observations, whereas the price of tea bags in that store never ended with a zero-digit.

Table 2 presents summary statistics for price changes in 2013 and in 2014, while distinguishing between price increases and price decreases and the last digit of (pre-ban) prices. The data provide a preliminary picture of the likelihood of prices with different endings to change between subsequent periods. First, in 2013, among price observations that ended with zero, the likelihood of changing by the subsequent period was (100 - 52.23=) 47.77%, compared to (100 - 61.43=) 38.57% for price observations ending in other digits. When comparing the matched observations from 2014 (i.e., observations corresponding to the same products/stores/periods), we do not observe significant differences in the likelihood of price changes taking place. Specifically, in 2014 the likelihood of changing prices between subsequent periods was 37.94% for products that ended with a zero-digit, and the corresponding likelihood among the complementary sample from 2014 was 39.38%. The table also illustrates the asymmetric relationship between zero-endings, price increases and price decreases. In 2013, among prices with zero-endings, the likelihood of increasing by the subsequent period was 30.18%, whereas the likelihood of decreasing was 17.60%. For prices ending with other digits, the corresponding likelihoods were 21.66% and 16.90%. In the matched samples from 2014, these differences in upward and downward rigidity disappeared.[11] In Section 3, we use regression analysis to further substantiate these preliminary findings.

Figure 3 focuses attention on the magnitude of price changes before and after the legal ban. For expositional purposes, the figure focuses on price changes within the range of -2 and +2 shekels in 2013 and in 2014. The figure shows that the "unrestricted" distribution of price changes in 2013 is smoother and covers a wider range of price changes compared to the 2014 distribution of price changes. In 2014, non-round price changes completely disappeared. The figure also illustrates that retailers tend to make round price (-1, -2, +1, +2 shekels, respectively) and that these round price changes became significantly more common after

[10] Consistent with our findings in the regression analysis, in 2013 the likelihood of a ketchup price change at that Shufersal store was 25% higher than the likelihood of a price change at the Mega store. In 2014, the difference in the likelihoods to change between these two stores fell to 8%.

[11] Similar patterns between price endings and the direction of price changes arise also if we restrict attention only to prices changes. Conditional on a price change, prices of products with zero-digit endings in 2013 increased in 63.1% (30.18/47.80) of price changes, whereas only in 36.9% (17.60/47.80) of instances they decreased. For other digit endings, the corresponding conditional likelihoods of a price increase and a price decrease in 2013 were 56.15% and 43.85%, respectively. In 2014, the conditional likelihoods of a price increase and a price decrease are 47% and 53% respectively for products ending with a zero-digit, and 46.16% and 53.84% for other digit-endings. The same patterns hold also if we restrict attention to price changes that are greater than 10 agoras and one shekel.

the legal ban, accounting for more than 48% of the price changes in that range. Table 3 complements figure 3 by presenting the frequencies of the most common price changes in 2013 and 2014. The table demonstrates that the fraction of small price changes, below 10 agoras, was considerable in 2013: small price reductions accounted for 9.8% of price changes, and small price increases accounted for 13.8% of the price changes. The prevalence of small price changes is consistent with recent studies (Chen et al. 2008; Chakraborty et al. 2015) that have shown that small price changes are common. In the context of price rigidity, it is worth noting that the elimination of small price changes presumably could have led to fewer price changes. Nevertheless, as shown in the table, the total number of price changes did not change between 2013 and 2014. This finding could be explained by the outlawing of non-zero price endings in 2014.

2.4 Representativeness of our sample

Like many other studies on price rigidity, we focus on prices of products sold in grocery stores. In this subsection we explain why we think, for the purpose of our investigation, that the sample of products in our analysis is adequately representative of the population of grocery products. We believe that this is the case for three main reasons: First, the distribution of price endings, and particularly 9-endings, in our sample is remarkably similar to comprable distributions found in other studies. For instance, we find that in our data nearly 60% of prices end with a 9-digit. Levy et al. (2011) and Knotek (2010) report that about 62% of the prices in their data ended in “9”. Like what we find in our data, other studies (Sehity et al. 2005, and Knotek 2011) have reported that 0, 5 and 9 endings are the most common price endings. Furthermore, our estimates regarding the rigidity of prices in the pre-ban period were also documneted in previous studies (Levy et al. 2011; Anderson et al. 2014; Snir et al. 2014). For instance, Anderson et al. 2014 report that 9-ending prices are rigid upward, a finding that we also share. Also, our findings that small price adjustments, less than 10 agoras, are common were documented in previous studies (Chen et al. 2008 and Chakraborty et al. 2015). Importantly, unlike previous studies, our setup enables us to further show that the relationship between price endings and price rigidity disappears in the post-ban period. The large similarities between the patterns in our data and patterns found in other datasets in the pre-ban period is particularly important because it can relieve the concern that retailers represented in our sample were setting different price *endings* because they were aware that the prices in their stores were being recorded by the Consumer Council. We further believe that this concern, if exists, is not a threat for our identification, because our identification strategy exploits within-basket variation, i.e., it focuses exclusively on products that belonged to the basket and whose composition was determined well before the legal change became effective in January 2014. Accordingly, any effect attributable to "belonging to the basket" is kept constant throughout the entire period of data collection.

Another potential concern with the representativeness of our sample is the small number of products that we track, nearly 100 in our initial analysis and 42 products in the panel data analysis, as compared to hundreds of products in other studies (e.g., Chakraborty et al. (2015) track 370 products, and Levy et al. (2011) and Anderson et al. (2014) cover a considerably larger number of products). While we agree that this is a limitation of our study, we also stress that in comparison to other studies, we analyze the price-setting behavior of a considerably larger number of price-setters at any given point in time. Specifically, our sample is based on prices from 26 different owners and 338 stores. Other studies, while tracking a larger number of products, were only able to examine the behavior of a few price-setters at each particular point in time. For instance, in Levy et. al 2011, for the grocery price data there are at most four prices for the same product at any given point in time.

3 Empirical Strategy and Results

Our analysis exploits the ban on non-zero digit endings to evaluate the impact of price-digit endings on the likelihood and the direction of price changes. Exploring the relationship between price endings and price changes only in the pre-ban period is conceptually similar to what previous papers have examined. Yet, focusing only on the pre-ban period makes it difficult to rule out other explanations as to why products with certain price endings might exhibit more or less rigidity than products with different price endings. In this paper, we try to overcome this issue by using information on price changes before and after the ban.

While using data from before and after the legal change is essential, a simple before-and-after comparison of the total number of price changes is probably an inadequate test. First, the sampled products, the stores that carry the products and the time of the year in which the products were sold might have changed between 2013 and 2014. For instance, if the sample of products that we observe changed between 2013 and 2014, then we may erroneously attribute market-level changes in price rigidity to digit-endings rather than to the characteristics of products. Similarly, changes in the number of stores that sells the products in each year may also affect the market-level price rigidity that we observe. Second, market-level factors that changed between 2013 and 2014 might have affected the likelihood of price adjustments in 2014 compared to 2013. Furthermore, the restriction on the set of possible price changes, enabling only price changes with 10 agroas increments, might also have affected price rigidity. Presumably, in the post-ban period we can expect fewer price changes, as small price changes, less than 10 agroas, are no longer possible.

To address these concerns, our empirical analysis distinguishes between products tendencies to end with a zero-ending in the pre-change period. We expect that products with prices that more frequnently ended with a non-zero endings will be affected by the change more than products with prices that more often ended with a zero-digit. Our underlying assumption is that the unobserved circumstances that affect a product price stickiness did not change with the legal ban, or that if the circumstances did change, their effects were similar across products regardless of whether a product specific price was more or less likely to end with a zero digit in the pre-ban period. Our empirical inquiry presented in subsection 3.1 is at the product-store level. In subsection 3.2, we further disaggregate the data and use a balanced panel price data to perform the analysis at the product-store-fortnight level.

3.1 Analysis at the product-store level

We begin the empirical analysis by estimating the relationship between price rigidity and digit-endings at the product-store level. Specifically, we estimate the following specification:

$$\begin{aligned} Share\_price\_change_{pst} = \beta_0 + \beta_1 Share\_zero\_ending\_price_{ps,13} + \beta_2 Post\_change_t + \\ \beta_3 Share\_zero\_ending\_price_{ps,13} * Post\_change_t + \gamma_p + \gamma_s + \varepsilon_{pst} \end{aligned} \qquad (1)$$

An observation is a product p sold in a given store s in year t. The dependent variable, $Share\_price\_change_{pst}$, is the share of price changes of product p in store s in year t, out of the total number of prices recorded for that product-store pair in that year. The main explanatory variable, *Share_zero_ending_price*, is the share of prices corresponding to the product-store pair that ended with a zero-digit in 2013. The *Post_change* variable is a dummy variable that equals one for observations collected in 2014 and zero otherwise. *Share_zero_ending_price* * *Post_change* is the interaction term, which measures the change in 2014 in the relationship between price endings and price changes. Finally, we also add product and store/chain fixed effects, denoted γ_p and γ_s, respectively. The fixed effects capture unobserved differences in product attributes and store characteristics, such as location and nature of local competition. Since the explanatory variable is a simple average, we weight each observation by the number of recorded prices of each product/store pair in each year (the results are qualitatively similar without the weights). We also cluster the standard errors at the store level. The two coefficients of interest are β_1 and β_3 and the results are presented in Table 5.

As can be seen in the table, the coefficients are statistically and economically significant. β_1 equals 0.04 in column 1, and 0.05 in column 2, once we replace the chain fixed effects with store fixed effects. The estimate suggests that a price of a product that always ended with a zero-digit in 2013 was 4% more likely to change than a price of product that never ended with a zero-digit in 2013. Furthermore, we find that the estimate of β_3 is -0.05, which indicates that the entire impact of price endings on price changes disappeared in 2014, after the legal ban, and that we cannot reject the hypothesis that $\beta_1 + \beta_3 = 0$.

The regression results provide evidence in support of the Price Point Theory. Yet, the estimation results in Table 5 are potentially biased for two main reasons. First, the number of observations collected in each period, and the weeks within the year in which the prices were recorded, are not consistent across stores and products. Previous studies have shown that the probability of price changes is highly seasonal (e.g., Nakamura and Steinsson 2008), and therefore our results might be driven by the unbalanced panel that we used. Furthermore, our analysis implicitly assumes a linear relationship between the prevalence of zero-digit endings and price changes. To address these limitations, we now turn to analyzing the balanced panel data and at the product-store-fortnight level. The disaggregated analysis provides us further confidence that we are indeed comparing "apples to apples" and are not capturing changes in pricing decisions that are due to product selection or seasonality.

3.2 Analysis at the product-store-fortnight level

To explore the relationship between price changes (as the dependent variable) and price-digit endings, we use the following standard difference-in-differences OLS specification:

$$Y_{pst} = \beta_0 + \beta_1 Zero\_ending\_price_{ps} + \beta_2 Post\_change_Y + \beta_3 Zero\_ending\_price_{ps} \\ * Post\_change_Y + \gamma_p + \gamma_m + \gamma_s + \varepsilon_{pst} \qquad (2)$$

Where Y is a price change indicator, and an observation is a product p sold in a specific store s during a given fortnight t in 2013 or 2014. The *Zero_ending_price* control variable assumes the value of one for prices that ended with a zero-digit in a given fortnight in 2013 and zero otherwise. Thus, a product sold in a given store could be in the control group in one fortnight and in the treatment group in another fortnight depending on the last digit of its price in each fortnight. The *Post_change* variable is a dummy variable that equals one for observations collected in 2014 and zero for observations collected in 2013. We also add fixed effects for product, fortnight-year and chain/store (γ_p, γ_m, and γ_s, respectively), and include linear and quadratic time trends. These fixed effects and time trends should capture unobserved differences in product attributes, store characteristics and seasonality. We also cluster the standard errors at the store level.

The coefficient on the control variable, β_1, measures the propensity to observe price changes in products in the control group relative to products in the treatment group. A positive and significant value for β_1 would be consistent with findings of previous studies on the rigidity of 9- endings (e.g., Levy et al. 2011). β_2 captures potential market-level changes between 2013 and 2014 in the tendency to adjust prices. The main coefficient of interest is β_3, which measures the likelihood to observe price changes in 2014, i.e., after the legal ban. Thus, $\beta_1 + \beta_3$ captures the difference in the propensity to change prices across the control and treatment groups after the ban. According to the Price Point Theory and the existing literature, we expect β_1 to be positive, indicating price rigidity among nonzero price endings. Furthermore, we expect β_3 to be negative if non-zero price endings are indeed the source of the price rigidity observed in 2013. The identifying assumption is that the trends in the outcome variables (i.e., price changes) for both treatment and control groups before the legal change are similar. The results for the OLS regression are presented in Table 6.

As can be seen in the table, the estimates of the control variable, β_1, are positive and significant. The magnitude of the estimates varies from 0.1 for the sample collected according to the broad approach, and nearly 0.08 for the sample collected according to the narrow approach. The coefficients of the interaction term, β_3, are negative and highly significant. Importantly, in each of the specifications the magnitude of the β_3 coefficient basically offsets the magnitude of the β_1 coefficient. Overall, the results suggest that in 2013, on average, a price with a zero-ending was about 10% more likely to change compared to the price of a similar product with a non-zero ending. Thus, the difference in the propensity to change disappeared in 2014.

3.2.1 Price increases and price decreases

To investigate the heterogeneous effect of digit-endings on price increases and price decreases, we use an ordered probit specification. The specification is similar to equation (2), except that the dependent variable takes the values of '1' for price decrease, '2' for no-change and '3' for price increase. The regression results are presented in panel A of Table 7.

The estimates indicate that prices of products that ended with a zero-digit in the pre-ban period (the control group) were more likely to increase than to remain constant or decrease. To derive the marginal effects for the relevant coefficients, we calculated the marginal derivatives for each category (decrease, no-change and increase) at the mean sample. We report these marginal effects in panel B of Table 7. As can be seen in the table, we find that being in the control group (i.e., being a zero-ending price) is negatively associated with the likelihood of undergoing a price decrease, and positively associated with the likelihood of undergoing a price increase. Furthermore, we find the opposite signs and roughly the same magnitudes for the interaction variable. Thus, these findings suggest that prices with zero-digit endings were more likely to increase and less likely to decrease in 2013 compared to products with other digit-endings. These differences disappeared in 2014.

3.3 Robustness

To test the robustness of the results, we verified that the results are qualitatively unchanged under several specifications. First, following (Nakamura et al. 2013) we repeat the main analysis after excluding price changes that are likely sale prices. The results of this exercise, reported in table 7, are qualititively similar. Second, we experimented with several additional specifications (all results are reported in Appendix B). In particular, we verified that the results hold when we exclude small price changes, defined as price changes in which the pre-change price and post-change price have the same unit digit, or price changes that are smaller than 10 agoras. We also used the Gregorian calendar instead of the Hebrew calendar to generate matched samples of panel data. The results are qualitatively similar. The results are also qualitatively similar when we focus only on 9 and zero-endings in the pre-ban period, i.e., exclude prices ending in 1-8 agora digits. We also obtain qualitatively similar results when we repeat the estimation at the product-store level using the balanced panel data, and when instead of the ordered probit regression, we run an OLS specification with (-1,0,1) as a dependent variable for a price decrease, no change, and a price increase, respectively. Likewise, we obtain qualitatively similar results when we separately estimate an OLS specification using price decreases or price increases as the dependent variable.

3.4 Coin supply

As mentioned in the introduction, the typical justification for policies that eliminate small coins from circulation is the potential savings associated with ceasing the production of these coins. In Israel, the production of the agora coins ceased already in 2008, and hence no such savings were attributed to the legal ban on non-zero endings. Yet, our study offers an opportunity to explore how retailers responded to the legal ban by adjusting their prices. As we show in Figure 1, the legal ban resulted in a shift to prices that ended with 90 agoras. This change is likely to have resulted in a large increase in the demand for 10-agora coins, which were still in circulation. Back-of-the-envelope calculations suggest that there was a 25% increase in the number of 10-agora coins under circulation.[12] These calculations assume that consumers pay "round" shekel payments, and that retailers provide exact change. For instance, a consumer who purchases a product priced at 8.60 shekels is expected to receive

[12]In its 2014 report, the currency department of the Bank of Israel reported an increase of 4% in the demand for 10-agora coins. See http://www.boi.org.il/ (in Hebrew)

4 coins of 10 agoras; similarly, for a product sold at 12.90 shekels, the consumer will get one coin of 10 agoras; and for a product priced at 9.20, the consumer will get 3 coins of 10 agoras and one half-shekel coin. The 25% increase in the demand for 10-agora coins is most likely an upper bound on the actual increase, given that non-cash payments are common. Accordingly, any calculation of the potential savings from eliminating the smallest coin denomination should take into account the potential increase in demand for other small-value coins. One potential implication is that, at least from the point of view of society on metal cost of rounding guidelines, such as those used in Canada or in Israel prior to the legal change,are preferable over completely outlawing 9-endings.

4 Concluding Remarks

Price rigidity is a main element of New Keynesian economic models. In trying to explain the sources of price rigidity, researchers predominantly focused on menu costs - the direct physical costs associated with changing prices - as the primary explanation as to why firms do not change their price as often as they might prefer. Other explanations for price rigidity, such as the Price Point Theory (Blinder 1991), which cannot be easily motivated by standard microeconomic models remained underexplored. According to the Price Point Theory firms may refrain from making price changes that involve altering certain digit endings because they are concerned that consumers will respond disproportionately to these changes. Previous studies have found evidence which is consistent with the Price Point Theory. Nevertheless, due to potential endogenity issues with retailers choice of using different price endings, concerns regarding the correct interpretation of these findings were raised.

This paper tries to address this gap in the literature by examining how outlawing the usage of non-zero-digit price endings affects price rigidity. By comparing retailers pricing decisions before and after an exogenous restriction on the usage of non-zero price endings, we are able to mitigate the concern that unobserved variables might simultaneously affect a products price rigidity and its likelihood of being priced with a zero-ending. In our analysis, we estimate difference-in-difference specifications that compare the impact of the legal change on prices of products that did not end with a zero digit before the ban, and on the prices of products that ended with a zero digit before the ban. Our findings suggest that, before the ban, zero-digit price endings were less rigid compared to non-zero digit endings (and that these prices were more likely to increase than to decrease); these differences disappeared after the legal change. Thus, our findings lend support to the Price Point Theory, which implies that firms are reluctant to change certain prices, fearing the response of consumers.

In addition, this paper offers unique evidence regarding the effects that elimination of low-denomination prices might have on price-setters behavior. In recent years, many countries around the world have been contemplating phasing out low-denomination coins, given rising price levels coupled with increasing production costs. Our paper offers unique evidence on one such policy attempt and suggest that the potentail savings from these policies might be overstated. Future research could further explore the long term impact of this and other similar policies.

References

[1] Anderson, E. T., and D. I. Simester (2003), "Effects of $9 Price Endings on Retail Sales: Evidence from Field Experiments," *Quantitative Marketing and Economics,* 1 (1), 93–110.

[2] Anderson, E., Jaimovich, N., & Simester, D. (2015). "Price stickiness: Empirical evidence of the menu cost channel," *Review of Economics and Statistics,* 97(4), 813-826.

[3] Barro, Robert J. (1972), A Theory of Monopolistic Price Adjustment, *The Review of Economic Studies*, 39 (January), 1726.

[4] Basu, K. (2006), "Consumer Cognition and Pricing in the Nines in Oligopolistic Markets," *Journal of Economics & Management Strategy*, 15 (1), 125-141.

[5] Bils, M., & Klenow, P. J. (2004). "Some Evidence on the Importance of Sticky Prices," *Journal of Political Economy*, 112(5), 947-985.

[6] Blinder, A. S. (1991), Why Are Prices Sticky? Preliminary Results from an Interview Study, *American Economic Review*, 81 (2), 8996.

[7] Blinder, A., E. R. Canetti, D. E. Lebow, and J. B. Rudd (1998), "Asking about Prices: A New Approach to Understanding Price Stickiness". *New York: Russell Sage Foundation.*

[8] Chakraborty, R. P., W. Dobson, J. S. Seaton, and M. Waterson (2015), "Pricing in Inflationary Times: The Penny Drops," *Journal of Monetary Economics*, Economics, 76 (November), 71-86.

[9] Chen, H. A., D. Levy, S. Ray, and M. Bergen (2008), "Asymmetric Price Adjustment in the Small," *Journal of Monetary Economics,* 55 (4), 728-737.

[10] Eichenbaum, Martin, Nir Jaimovich, and Sergio Rebelo (2011), "Reference Prices, Costs, and Nominal Rigidities," *The American Economic Review,* 101 (1), 234-262.

[11] Gorodnichenko, Y., V. Sheremirov, and O. Talavera (2015), Price Setting in Online Markets: Does IT Click?(No. w20819). *ational Bureau of Economic Research.*

[12] Gorodnichenko, Yuriy, and Michael Weber (2016), "Are Sticky Prices Costly? Evidence from the Stock Market," *American Economic Review*, 106 (1), 165-99.

[13] Hackl, Franz, Michael E. Kummer, and Rudolf Winter-Ebmer (2014), "99 Cent: Price Points in E-commerce," *Information Economics and Policy*, 26, 12-27.

[14] Klenow, P.J., Malin, B.A., 2010. Microeconomic Evidence on Price-Setting. Friedman, B.M., Woodford, M. (Eds.), *Handbook of Monetary Economics,* H vol(3A). Elsevier, Amsterdam, pp.231284.

[15] Knotek, Edward S. (2011), "Convenient Prices and Price Rigidity: Cross-sectional Evidence," *Review of Economics and Statistics,* 93 (3), 1076-1086.

[16] Knotek, E. (2010), The Roles of Menu Costs and Nine Endings in Price Rigidity, *Federal Reserve Bank of Kansas Working Paper,* 1018.

[17] Levy, Daniel, Mark Bergen, Shantanu Dutta, and Robert Venable (1997), The Magnitude of Menu Costs: Direct Evidence from Large U. S. Supermarket Chains, *Quarterly Journal of Economics,* 112 (3), 791-825.

[18] Levy, D., D. Lee, H. Chen, R. J. Kauffman, and M. Bergen (2011), Price Points and Price Rigidity, *The Review of Economics and Statistics,* 93 (4), 1417-1431.

[19] Lewis, M. S. (2015), Odd Prices at Retail Gasoline Stations: Focal Point Pricing and Tacit Collusion, *Journal of Economics & Management Strategy,* 24,664685.

[20] Lombra, Raymond E. (2001), "Eliminating the Penny from the US Coinage System: An Economic Analysis," *Eastern Economic Journal,* 27 (4), 433-442.

[21] Kashyap, A. K. (1995), Sticky Prices: New Evidence from Retail Catalogs, *Quarterly Journal of Economics,* 110 (1), 245274.

[22] Mankiw, N. G. (1985). Small Menu Costs and Large Business Cycles: A Macroeconomic Model of Monopoly.Small Menu Costs and Large Business Cycles: A Macroeconomic Model of Monopoly. *Quarterly Journal of Economics,* 100(2), 529-537.

[23] McShane, B. B., C. Chen, E. A. Anderson, and D. I. Simester (2016), Decision Stages and Asymmetries in Regular Retail Price Pass-through, *Marketing Science*, 35(4), 619-639.

[24] Nakamura, E., and J. Steinsson (2008), Five Facts about Prices: A Reevaluation of Menu Cost Models, *The Quarterly Journal of Economics,* 123 (4), 1415-146.

[25] Nakamura, A. O., E. Nakamura, and L. I. Nakamura (2011), Price Dynamics, Retail Chains and Inflation Measurement, *Journal of Econometrics*, 161 (1), 47-55.

[26] Nakamura, E., and Steinsson, J. (2013). Price Rigidity: Microeconomic Evidence and Macroeconomic Implications. *Annual Review of Economics*, 5, 133-63.

[27] Rotemberg, J. J. (2005), Customer Anger at Price Increases, Changes in the Frequency of Price Adjustment and Monetary Policy, *Journal of Monetary Economics*, 52 (4), 829-852.

[28] Schindler, Robert M. and Patrick N. Kirby (1997), Patterns of Rightmost Digits Used in Advertised Prices: Implications for Nine Ending Effects, *The Journal of Consumer Research,* 24(2), p.192-201.

[29] Schindler, Robert M. (2006), The 99 Price Ending as a Signal of a Low-price Appeal, *Journal of Retailing*, 82 (1), 71-77.

[30] El Sehity, T., Hoelzl, E., & Kirchler, E. (2005). Price Developments after a Nominal Shock: Benford's Law and Psychological Pricing after the Euro Introduction. *International Journal of Research in Marketing*, 22(4), 471-480.

[31] Sheshinski, Eytan, and Yoram Weiss (1977), Inflation and Costs of Price Adjustment, *Review of Economic Studies*, 44 (June), 287303.

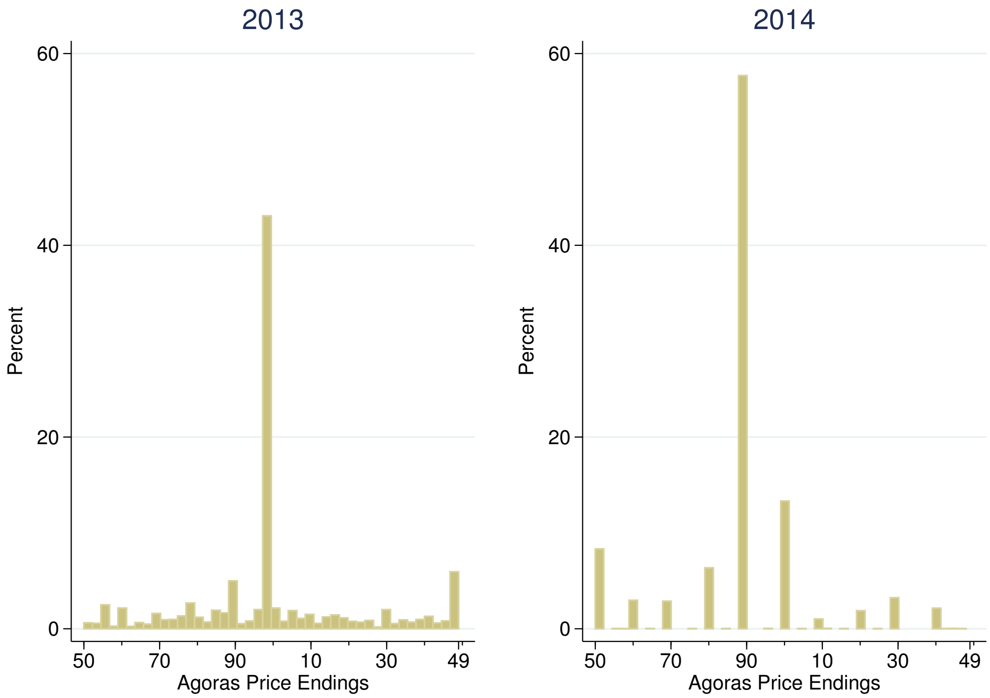
[32] Snir, A., D. Levy, A. Gotler, and H. A. Chen (2014), Not All Price Endings Are Created Equal: Price Points and Asymmetric Price Rigidity, Working paper.

[33] Thomas, Manoj, and Vicki Morwitz (2005), "Penny Wise and Pound Foolish: The Left-Digit Effect in Price Cognition," *Journal of Consumer Research*, 32 (1), 54-64.

[34] Thomas, M., & Morwitz, V. (2009). Heuristics in Numerical Cognition: Implications for Pricing. *Handbook of pricing research in marketing*, 132-149.

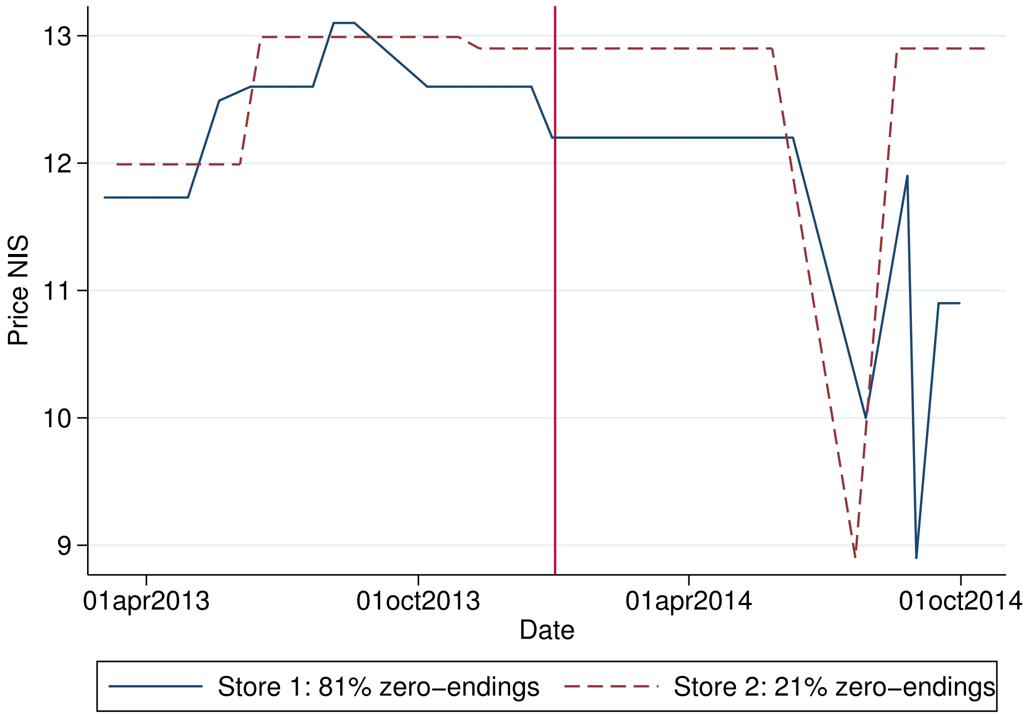
5 Figures

Figure 1



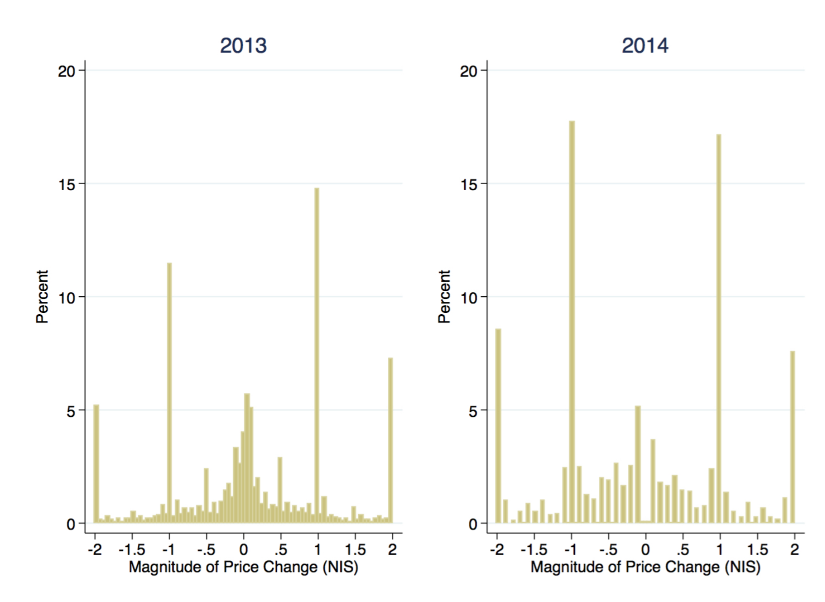
The figure shows the distribution of the 2-digit price endings in 2013 and 2014. The 99 price ending was the most popular price endings in 2013 with roughly 42% of price endings, and about 60% of the prices ended with a 9-ending. In 2014, the most popular price endings is 90 (58%) and the 00 ending accounts for 12% of price endings.

Figure 2



In this graph we present the time series of prices of the same product (750ml Ketchup Osem sauce) sold in two stores in the same city. In store '1', the ketchup price ended with a zero-digit ending in 81% of observations and and its price changed five times before the legal ban. In store '2', the price of the same product ended with a round ending only in 21% of the observations during the pre-ban period. The price of that product changed only twice during that period. After the legal change, the differences in the likelihood to observe a price change nearly dissapeared.

Figure 3



The figure separately displays the distribution of the magnitude of price changes in 2013 and 2014 within the range of -2 and +2 shekels. Price changes within this range account for 77% of all price changes. In 2014, only 10 agoras price increments are observed, and the propensity of round shekel prices (+1, -1 , +2, -2 shekel price changes) significantly increased.

6 Tables

Table 1: Descriptive statistics for the sampled products

Product	N	Mean Price	S.D.	Min. Price	Max. Price	Share of prices ending in 2013: Not with zero agoras units digit	Share of prices ending in 2013: With 9 agoras units digit
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Apple 1 kg	2,426	8.96	1.88	1.5	15.9	0.95	0.95
Bulgarian cheese Tnuva Piraeus 250 grams	1,936	20.73	2.93	10.43	28.16	0.86	0.2
Carrots 1 kg	1,856	3.98	1.38	0.4	8.99	0.98	0.98
Chocolate spread Ha'Shahar Ha'ole 1 kg	1,844	12.24	1.65	6.7	19.9	0.83	0.63
Chocolate bar Strauss-Elite Para 100 grams	2,352	5.83	0.7	2.9	7.99	0.92	0.43
Coca Cola 1.5 liter	2492	6.37	0.42	4.99	8.79	0.95	0.59
Cottage Cheese Tnuva 250 grams	2,548	5.87	0.27	3.95	7.16	0.97	0.12
Cucumbers 1 kg	2,572	4.74	1.88	0.49	13.98	0.98	0.94
Ketchup Osem 750 grams	2,254	11.48	1.4	6.2	19.9	0.77	0.7
Milk substitute Similac 900 grams	2,308	82.63	7.24	59.9	110.99	0.91	0.46
Mineral water Neviot 1.5 liter	1,920	11.2	1.76	5.9	16.99	0.77	0.68
Oil 1 liter	2,514	8.49	1.49	3.9	17.79	0.95	0.58
Onions 1 kg	2,552	3.64	1.36	0.4	8.99	0.98	0.97
Potato snack Strauss-Elite Tapuchips 50 grams	1,736	3.71	0.39	1.5	5.5	0.96	0.41
Pudding Strauss-Elite Milky 170 ml	2,508	2.91	0.24	1.97	10.9	0.94	0.46
Shampoo Hawaii 700 ml	1,490	10.97	2.83	4.9	23.38	0.76	0.46
Sliced cheese Tnuva Emek 200 grams	2,096	15.24	1.2	11.42	29.38	0.94	0.1
Soft drink Yafora-Tavori Spring 1.5 liter	2,532	6.21	0.79	2.89	10.9	0.92	0.57
Spaghetti pasta Osem 500 grams	1,804	5.89	0.9	3.5	9.99	0.85	0.39
Tea Bags Wissotzky Classic 1.5 g	2,052	16	2.81	7.2	28.99	0.98	0.7
Tomatoes 1 kg	2,570	4.79	1.84	0.49	11.9	0.98	0.98
White potato 1 kg	2,208	4.56	1.57	0.5	8.99	0.99	0.99
White sugar 1 kg	2,464	4.42	0.56	1.9	7.38	0.96	0.66
Wine Carmel Mizrahi Selected 750 ml	1,460	28.18	8.15	14.9	50.79	0.96	0.79
Yogurt Tnuva Yoplait 200 grams	2,218	3.89	0.38	2.3	5.69	0.8	0.12
Table total	54,712	11.72	1.84	0.4	110.99	0.91	0.59
Total sample (all 42 products)	59,538	11.43	15.6	0.4	110.99	0.92	0.6

Table 2: Distribution of no change, up and down price changes

	Pre-ban		Post-ban	
	Zero-Digit Ending	Other Digit Endings	Zero-Digit Ending	Other Digit Endings
Price Decrease	17.59%	16.9%	20.13%	21.19%
No Price Change	52.23%	61.43%	62.06%	60.62%
Price Increase	30.18%	21.67%	17.81%	18.19%

The table presents the distribution of price decreases, increases and no-price changes in 2013 and in 2014, distinguishing between product prices with zero-digit endings and other prices. The classification for zero-digit and non-zero digit endings is based on the price of the product in 2013. In 2013, prices that ended with a zero-digit were considerably more likely to change, typically upward. In 2014, the products matched to the treatment and control observations showed no significant difference in the propensity to undergo price changes.

Table 3: Top 10 highest frequencies of price changes

2013		2014	
Below 10 agoras	12.09%	Below 10 agoras	0.00%
1	11.02%	-1	13.37%
-1	8.51%	1	12.94%
2	5.35%	-2	6.45%
-2	3.91%	2	5.71%
3	2.51%	-0.1	3.82%
0.5	1.92%	0.1	2.70%
-3	1.76%	3	2.58%
-0.5	1.59%	-3	2.50%
0.1	1.55%	-0.4	1.97%
0.05	1.31%	-0.2	1.92%
No change	60.70%	No change	60.73%

The table presents the top 15 frequencies of price changes in 2013 and 2014 in the panel data set collected according to the broad approach. No price changes account for slightly more than 60% of the observations in each year. The frequencies of price changes are calculated conditional on observing a price change. "Round" price changes are the majority of price changes in each year and the proportion of round price changes further increased in 2014. Also, the fraction of price changes below 10 agoras (negative and positive) which was substantial in 2013, disappeared in 2014.

Table 4: Regression results for the product-store analysis

	I	II
Zero_ending_price_share	0.04**	0.05***
	(2.92)	(3.98)
Post_change	0.01	0.01
	(0.88)	(1.74)
Zero_ending_price_share*Post_change	-0.05*	-0.05**
	(-2.26)	(-2.65)
Observations	24,398	24,398
R^2	0.28	0.35
Product FE	✓	✓
Chain FE	✓	
Store FE		✓

*** p<0.01, ** p<0.05, * p<0.1. The unit of observation is a product sold in a given store during 2013 or 2014 (before or after the legal change). The dependent variable equals to the share of price changes in each year. The Zero_ending_price_share variable is the fraction of instances that the product ended with a zero-digit before the legal ban, i.e., during 2013. Standard errors are clustered at the store level.

Table 5: Regression results for the balanced panel data analysis

	Broad Panel			**Narrow Panel**		
	I	II	III	I	II	III
Zero_ending_price	0.10***	0.10***	0.10***	0.08***	0.08***	0.08***
	(7.53)	(7.99)	(7.77)	(5.00)	(5.20)	(5.07)
Post_change	0.04	0.01	0.05*	0.15***	0.01	0.03
	(1.81)	(1.03)	(2.08)	(6.97)	(1.54)	(1.39)
Zero_ending_price*Post_change	-0.11***	-0.11***	-0.11***	-0.07***	-0.07***	-0.07***
	(-6.45)	(-6.30)	(-6.45)	(-3.72)	(-3.52)	(-3.74)
Constant	0.68***	0.43***	0.62***	0.86***	0.41***	0.81***
	(6.31)	(34.34)	(5.74)	(6.84)	(29.94)	(6.57)
Observations	59,536	59,536	59,536	41,962	41,962	41,962
R^2	0.10	0.10	0.11	0.09	0.09	0.11
Product FE	✓	✓	✓	✓	✓	✓
Chain FE	✓			✓		
Store FE		✓	✓		✓	✓
Time FE	✓		✓	✓		✓

Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1. The unit of observation is a product sold in a given store in a particular fortnight. Standard errors are clustered the at the store level. The dependent variable equals to one if the price of the product changed in the subsequent period and zero otherwise. The table reports OLS estimation results.

Table 6: Ordered probit results

Panel a

	Broad Panel			Narrow Panel		
	I	II	III	I	II	III
Zero_ending_price	0.16***	0.16***	0.16***	0.11**	0.11**	0.11**
	(5.07)	(5.04)	(4.99)	(2.90)	(2.94)	(3.07)
Post_change	-0.13	-0.14***	-0.14	0.13	-0.13***	0.12
	(-1.50)	(-11.91)	(-1.40)	(0.53)	(-8.84)	(0.47)
Zero_ending_price*Post_change	-0.15***	-0.13***	-0.15***	-0.12*	-0.10*	-0.12*
	(-4.06)	(-3.64)	(-4.05)	(-2.52)	(-2.12)	(-2.50)
Constant cut1	-1.61***	-0.94***	-1.73***	-1.31***	-1.00***	-1.48***
	(-7.55)	(-47.89)	(-7.84)	(-3.78)	(-42.21)	(-4.25)
Constant cut2	0.14	0.79***	0.02	0.49	0.78***	0.32
	(0.64)	(36.00)	(0.09)	(1.45)	(30.90)	(0.94)
Observations	59,536	59,536	59,536	41,962	41,962	41,962
Product FE	✓	✓	✓	✓	✓	✓
Chain FE	✓			✓		
Store FE		✓	✓		✓	✓
Time FE	✓		✓	✓		✓

Panel b - Marginal effects of ordered probit model

	Broad Panel			Narrow Panel		
	Decrease	Static	Increase	Decrease	Static	Increase
$\frac{\partial pr(Y\|X)}{\partial Zero\_ending}$	-0.02***	0	0.02***	-0.01***	0	0.01***
	(0.00)	(0.01)	(0.00)	(0.00)	(0.01)	(0.00)
$\frac{\partial pr(Y\|X)}{\partial Post\_Change}$	0.04***	0	-0.04***	-0.06***	0	0.06***
	(0.01)	(0.01)	(0.01)	(0.02)	(0.03)	(0.02)
$\frac{\partial^2 pr(Y\|X)}{\partial Zero\_ending \partial Post\_Change}$	0.04***	0.01	-0.05***	0.04***	0	-0.03***
	(0.01)	(0.02)	(0.01)	(0.01)	(0.02)	(0.01)

Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1. The unit of observation is a product sold in a given store during 2013 or 2014 (before or after the legal change). The dependent variable equals to a price increase, no price change or a price decrease. Panel A presents the estimates of the ordered probit specification. Panel B presents the marginal effects for the third specification in both the broad and narrow panels. The marginal effects are calculated as Pr(Y=i—X=1) - Pr(Y=i—X=0) at the sample mean of all other varaibles.

Table 7: Diff-in-diff results without prices during periods of sale

	Broad Panel			**Narrow Panel**		
	I	II	III	I	II	III
Zero_ending_price	0.06***	0.07***	0.06***	0.06***	0.06***	0.06***
	(4.48)	(4.82)	(4.64)	(3.82)	(3.98)	(3.95)
Post_change	-0.05**	-0.02*	-0.04*	-0.38***	-0.02*	-0.41***
	(-2.67)	(-2.57)	(-2.16)	(-14.07)	(-2.02)	(-14.57)
Zero_ending_price*Post_change	-0.08***	-0.08***	-0.08***	-0.06**	-0.05**	-0.06**
	(-5.09)	(-5.03)	(-5.07)	(-3.22)	(-3.10)	(-3.20)
Constant	0.39***	0.23***	0.36***	0.54***	0.23***	0.50***
	(4.19)	(16.28)	(3.75)	(5.84)	(13.80)	(4.99)
Observations	30,720	30,720	30,720	21,884	21,884	21,884
R^2	0.12	0.13	0.14	0.12	0.12	0.14
Product FE	✓	✓	✓	✓	✓	✓
Chain FE	✓			✓		
Store FE		✓	✓		✓	✓
Time FE	✓		✓	✓		✓

Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1. The unit of observation is a product sold in a given store in a particular fortnight/year. Standard errors are clustered the at the store level. The dependent variable equals to one if the price of the product changed in the subsequent period and zero otherwise. The table reports OLS estimation results. The regressions with time fixed effects also include a quadratic time trend.

The sample used for estimation includes only prices that are considered non-sale prices. That is, if a price has changed and within a period of 60 days returned to the same price, we consider this to be a sale price and not a price change. Accordingly, all observations in the that period were excluded (the price change; its return and all observations in between). If there was a price change and there were no changes afterwards within a period of 60 days, we also exclude these price changes. Following this restriction, the sample is 50% smaller than the sample used for the main estimation. Nevertheless, the results are qualiaitively similar to the results in Table 5.

7 Appendix A

Table A1: Top 10 highest frequencies of prices below and above 10 shekels

Prices below 10 NIS				Prices above 10 NIS			
2013		2014		2013		2014	
4.99	9.00%	2.90	11.27%	12.99	6.07%	10.90	9.50%
5.99	7.71%	3.90	9.23%	11.99	5.70%	11.90	9.17%
3.99	6.99%	5.90	7.72%	15.55	3.97%	13.90	5.09%
6.99	4.85%	4.90	6.32%	15.99	3.05%	12.00	4.28%
2.99	4.58%	6.90	6.06%	10.99	2.72%	12.90	4.06%
9.99	3.88%	7.90	5.2%	10.00	2.64%	16.90	3.97%
8.99	3.63%	8.90	4.18%	15.42	2.26%	15.50	3.96%
7.99	3.43%	9.90	4.16%	13.99	2.12%	10.00	3.85%
2.89	2.11%	6.00	4.08%	89.99	1.88%	14.90	3.64%
6.04	1.49%	5.80	2.81%	12.49	1.72%	13.00	2.46%

The table presents the top ten frequent prices in 2013 and 2014 separately for prices below and above 10 shekels based on the broad blanced panel data. X.99 price endings were very common in 2013, espeically among prices below 10 shekels. In 2014, X.90 prices became the most frequent prices, and again, more within the lower price range.

Table A2: Table A1

Retail chain	**# Stores Visited**	**Retail chain**	**# Stores Visited**
Shufersal	90	Hatzi Heenam	2
Mega	51	Mister Zol	2
Yeinot Bitan	20	Super Dabach	2
Rami Levi	17	Super Lustrous	2
Victory	8	Yohananoff	2
Osher Ad	7	Co-op Shop	1
Tiv Taam	7	Coast 365	1
Mahsanei Hashuk	5	Machsanai Mazon	1
Zol've'Begadol	4	Shefa Shuk	1
Mahsanei Lahav	3	Single branch	1
Hachi Zool	2	Super Baba	1
		Zol Beshefa	1

Total of 23 chains and 231 stores

Table A3: Table A2

City	# Stores Visited	City	# Stores Visited
Tel Aviv-Yafo	18	Netanya	3
Jerusalem	17	Raanana	3
Beersheba	16	Ramla	3
Ashdod	13	Rosh Haain	3
Haifa	13	Safed	3
Rishon Lezion	12	Shoham	3
Rehovot	8	Tel Mond	3
Beit Shemesh	6	Bat Yam	2
Petah Tikva	6	Hadera	2
Afula	5	Herzliya	2
Kiryat Ata	5	Hod Hasharon	2
Kiryat Shmona	5	Kadima	2
Tiberias	5	Karmiel	2
Ashkelon	4	Kiryat Malachi	2
Kiryat Haim	4	Or Yehuda	2
Modieen	4	Yehud	2
Ramat Gan	4	Galilee[1]	1
Acre	3	Deir al-Assad	1
Bnei Brak	3	Ein HaMifratz	1
Givataim	3	Isaac wells	1
Hatzor	3	Kiryat Gat	1
Holon	3	Kiryat Motzkin	1
Kfar Saba	3	Nahariya	1
Kiryat Bialik	3	Ness Ziona	1
Kiryat Ekron	3	Rosh Pina	1
Lod	3	Tirat Carmel	1
Nesher	3	Usifiya	1
Online stores	5	Zoran	1

Total of 231 stores in 35 municipalities
[1] Communities in the Galilee Region

8 Appendix B

In this appendix we present several estimation results which illsutrate the robustness of the findigs presented in the main text. In particular, we show that the results are qualitatively similar when we exclude prices that are suspected as price during sales period (Table 10). In addition, in other attempts we also exclude small price changes (Table 11), use the Gregorian calender to construct the panel data instead of the Hebrew panel (Table 12), and exclude prices that end with 1-8 digit endings (Table 13). In Tables 14, 15 and 16 we divide the data into price decreases and price increases and still obtain similar results.

Table B1: Diff in Diff without small price changes

	Broad Panel			**Narrow Panel**		
	I	II	III	I	II	III
Zero_ending_price	0.09***	0.09***	0.09***	0.07***	0.07***	0.07***
	(7.00)	(7.28)	(7.19)	(4.61)	(4.61)	(4.59)
Post_change	0.05*	0.03***	0.06*	0.14***	0.03***	0.01
	(2.31)	(3.71)	(2.47)	(6.49)	(3.72)	(0.25)
Zero_ending_price*Post_change	-0.08***	-0.08***	-0.08***	-0.04*	-0.04	-0.04*
	(-4.96)	(-4.66)	(-4.93)	(-2.11)	(-1.75)	(-2.09)
Constant	0.51***	0.43***	0.49***	0.62***	0.42***	0.60***
	(5.70)	(34.93)	(5.14)	(6.12)	(29.85)	(5.49)
Observations	51,212	51,212	51,212	35,626	35,626	35,626
R^2	0.13	0.14	0.15	0.12	0.13	0.14
Product FE	✓	✓	✓	✓	✓	✓
Chain FE	✓			✓		
Store FE		✓	✓		✓	✓
Time FE	✓		✓	✓		✓

Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1. The unit of observation is a product sold in a given store in a particular fortnight. Standard errors are clustered the at the store level. The dependent variable equals to one if the price of the product changed in the subsequent period and zero otherwise. The table reports OLS estimation results. We excluded in this estimation all price changes in our sample that were within the same Shekel unit (e.g., 6.00 to 6.75 and not 6.75 to 7.10) and that the net change was worth less than 10% of the original price (e.g., 8.40 to 8.60 would have been dropped out but a change from 1.00 to 1.20 would not).

Table B2: Diff in Diff based on Gregorian calender

	Broad Panel			**Narrow Panel**		
	I	II	III	I	II	III
Zero_ending_price	0.07***	0.07***	0.07***	0.05***	0.05***	0.05***
	(5.44)	(5.55)	(5.55)	(3.70)	(3.54)	(3.62)
Post_change	0.09	0.02**	0.09	0.14	0.02	0.15
	(0.68)	(2.71)	(0.69)	(0.93)	(1.93)	(0.98)
Zero_ending_price*Post_change	-0.07***	-0.07***	-0.07***	-0.05**	-0.05**	-0.05**
	(-4.69)	(-4.64)	(-4.70)	(-2.99)	(-2.82)	(-2.99)
Constant	0.50***	0.41***	0.45***	0.48***	0.38***	0.43***
	(17.09)	(34.76)	(16.62)	(14.23)	(29.74)	(13.68)
R^2	0.10	0.10	0.11	0.09	0.09	0.11
Observations	61,676	61,676	61,676	44,944	44,944	44,944
Product FE	✓	✓	✓	✓	✓	✓
Chain FE	✓			✓		
Store FE		✓	✓		✓	✓
Time FE	✓		✓	✓		✓

Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1. The unit of observation is a product sold in a given store in a particular fortnight. Standard errors are clustered the at the store level. The dependent variable equals to one if the price of the product changed in the subsequent period and zero otherwise. The table reports OLS estimation results. The matching procdefure to construct the sample was based on fornights in the Gregorian calendar (in 2013 and 2014).

Table B3: Diff in Diff with only 0 or 9 endings in the pre-period

	Broad Panel			**Narrow Panel**		
	I	II	III	I	II	III
Zero_ending_price	0.09***	0.09***	0.09***	0.07***	0.07***	0.07***
	(6.38)	(6.66)	(6.68)	(4.12)	(4.20)	(4.18)
Post_change	0.04	0.01	0.05	0.18***	0.01	0.07**
	(1.65)	(1.47)	(1.95)	(7.87)	(1.48)	(3.10)
Zero_ending_price*Post_change	-0.12***	-0.11***	-0.12***	-0.08***	-0.07***	-0.08***
	(-6.75)	(-6.34)	(-6.71)	(-3.96)	(-3.47)	(-3.94)
Constant	0.63***	0.43***	0.61***	0.76***	0.43***	0.77***
	(6.31)	(36.42)	(5.67)	(6.75)	(31.62)	(6.22)
Observations	39,433	39,433	39,433	27,267	27,267	27,267
R^2	0.09	0.09	0.10	0.08	0.09	0.10
Product FE	✓	✓	✓	✓	✓	✓
Chain FE	✓			✓		
Store FE		✓	✓		✓	✓
Time FE	✓		✓	✓		✓

Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1. The unit of observation is a product sold in a given store in a particular fortnight. Standard errors are clustered the at the store level. The dependent variable equals to one if the price of the product changed in the subsequent period and zero otherwise. The table reports OLS estimation results. The sample includes only prices that ended with either 0 or 9 agora digits in 2013, and their matches in the following year.

Table B4: Diff-in-diff with only price decreases

	Broad Panel			**Narrow Panel**		
	I	II	III	I	II	III
Zero_ending_price	0.04**	0.04***	0.04**	0.04*	0.04**	0.04*
	(2.89)	(3.34)	(3.11)	(2.46)	(2.64)	(2.47)
Post_change	0.01	0.04***	0.02	0.39***	0.03***	0.28***
	(0.77)	(5.39)	(1.11)	(21.97)	(4.55)	(14.00)
Zero_ending_price*Post_change	-0.04*	-0.04*	-0.04*	-0.02	-0.02	-0.02
	(-2.34)	(-2.15)	(-2.32)	(-1.08)	(-0.97)	(-1.05)
Constant	0.27**	0.29***	0.28**	0.33**	0.28***	0.34**
	(2.88)	(20.58)	(2.78)	(3.23)	(17.66)	(2.97)
Observations	37,269	37,269	37,269	27,121	27,121	27,121
R^2	0.08	0.08	0.10	0.08	0.08	0.09
Product FE	✓	✓	✓	✓	✓	✓
Chain FE	✓			✓		
Store FE		✓	✓		✓	✓
Time FE	✓		✓	✓		✓

Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1. The unit of observation is a product sold in a given store in a particular fortnight. Standard errors are clustered the at the store level. The dependent variable equals to one if the price of the product changed in the subsequent period and zero otherwise. The table reports OLS estimation results. The balanced panel used for the estimation is a subset of the entire sample including only prices that either went down or did not change relative to the subsequnet period.

Table B5: Diff-in-diff results with only price increases

	Broad Panel			**Narrow Panel**		
	I	II	III	I	II	III
Zero_ending_price	0.04**	0.04***	0.04**	0.04*	0.04**	0.04*
	(2.92)	(3.36)	(3.13)	(2.50)	(2.66)	(2.50)
Post_change	0.01	0.04***	0.02	0.39***	0.03***	0.28***
	(0.75)	(5.35)	(1.10)	(21.96)	(4.50)	(13.96)
Zero_ending_price*Post_change	-0.04*	-0.04*	-0.04*	-0.02	-0.02	-0.02
	(-2.32)	(-2.13)	(-2.30)	(-1.05)	(-0.94)	(-1.03)
Constant	0.27**	0.29***	0.28**	0.33**	0.28***	0.34**
	(2.88)	(20.58)	(2.77)	(3.22)	(17.67)	(2.94)
Observations	37,275	37,275	37,275	27,127	27,127	27,127
R^2	0.08	0.08	0.10	0.08	0.08	0.09
Product FE	✓	✓	✓	✓	✓	✓
Chain FE	✓			✓		
Store FE		✓	✓		✓	✓
Time FE	✓		✓	✓		✓

Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1. The unit of observation is a product sold in a given store in a particular fortnight. Standard errors are clustered the at the store level. The dependent variable equals to one if the price of the product changed in the subsequent period and zero otherwise. The table reports OLS estimation results. The balanced panel used for the estimation is a subset of the entire sample including only prices that either went up or did not change relative to the subsequnet period.

Table B6: Ordered OLS

	Broad Panel			Narrow Panel		
	I	II	III	I	II	III
Zero_ending_price	0.09***	0.09***	0.09***	0.06**	0.06**	0.06**
	(5.08)	(5.04)	(5.00)	(2.90)	(2.93)	(3.05)
Post_change	0.04*	-0.08***	0.29*	0.22	-0.07***	0.22
	(1.98)	(-11.96)	(2.19)	(1.55)	(-8.92)	(1.41)
Zero_ending_price*Post_change	-0.08***	-0.07***	-0.08***	-0.06*	-0.06*	-0.06*
	(-4.06)	(-3.63)	(-4.05)	(-2.52)	(-2.12)	(-2.50)
Constant	0.35**	0.04***	0.10	0.20***	0.06***	0.15*
	(2.87)	(3.89)	(1.84)	(3.34)	(5.03)	(2.26)
Observations	59,536	59,536	59,536	41,962	41,962	41,962
R^2	0.03	0.02	0.04	0.03	0.02	0.04
Product FE	✓	✓	✓	✓	✓	✓
Chain FE	✓			✓		
Store FE		✓	✓		✓	✓
Time FE	✓		✓	✓		✓

Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1. The unit of observation is a product sold in a given store in a particular fortnight. Standard errors are clustered the at the store level. The dependent variable equals to one if the price of the product changed in the subsequent period and zero otherwise. The table reports OLS estimation results. The dependnent variable is a price change variable similar to the one used in the ordered probit estimation. It receives the values 1,0 and -1 for prices that increased, has not changed and decreased, respectively.

Table B7: Regression results at the product-store level using the balanced panels

	Broad Panel		**Narrow Panel**	
	I	II	I	II
Zero_ending_price_share	0.07***	0.08***	0.05*	0.04*
	(3.87)	(4.07)	(2.18)	(2.08)
Post_change	0.01	0.01	0.02	0.02
	(1.11)	(1.10)	(1.59)	(1.58)
Zero_ending_price_share*Post_change	-0.12***	-0.12***	-0.08***	-0.08***
	(-4.98)	(-4.94)	(-3.41)	(-3.38)
Observations	13,058	13,058	12,242	12,242
R^2	0.24	0.28	0.19	0.23
Product FE	✓	✓	✓	✓
Chain FE	✓		✓	
Store FE		✓		✓

*** p<0.01, ** p<0.05, * p<0.1. The unit of observation is a product sold in a given store during 2013 or 2014 (before or after the legal change). The dependent variable equals to the share of price changes in each year. The Zero_ending_price_share variable is the fraction of instances that the product ended with a zero-digit during 2013. Standard errors are clustered at the store level.