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**The Effects of Price Endings on Price Rigidity:
Evidence from VAT Changes**

Edward S. Knotek II, Doron Sayag, and Avichai Snir

We document a causal role for price endings in generating micro and macro price rigidity. Based on micro price data underlying the consumer price index in Israel, we document that most stores have a favored price ending—a final digit, usually a zero or nine, used by a majority of prices in that store—and that these favored price endings are utilized extensively. Using changes to the VAT rate as exogenous cost shocks that affect prices regardless of ending, we find that the frequency of price adjustment for nonfavored endings increases by twice as much as the frequency of adjustment for favored endings in months when the VAT rate changes. In the aggregate, sluggish pass-through of VAT rate changes is due to favored endings; changes in the VAT rate are passed through fully and immediately to nonfavored endings.

Keywords: sticky prices, favored price endings, zero-ending prices, nine-ending prices, pass-through.

JEL codes: E3, L11.

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I Introduction

Prices often do not adjust instantaneously to shocks. Empirical evidence suggests that nominal prices of many consumer goods typically remain rigid for at least several quarters, even when inflation is relatively high.¹ A distinct empirical literature has documented the broad use of certain final ending digits among prices, especially the digits nine and zero, with the prevalence of these digits dating back at least to the late nineteenth century.² Kashyap (1995), Blinder et al. (1998), Dhyne et al. (2006), Hoffmann and Kurz-Kim (2006), Knotek (2008, 2011, 2019), Levy et al. (2011), Macé (2012), Hahn and Marenčák (2018), and Levy et al. (forthcoming) link the two concepts and document a connection between price endings and price rigidity, with prices ending in the digits zero or nine often being significantly less likely to change than other prices. In this paper, we use a subset of the micro price data underlying the calculation of the consumer price index in Israel from January 2002 through December 2013. During our sample period, we document that most firms had a favored price ending that they used extensively. Taking advantage of frequent changes in the value-added tax (VAT) rate during our sample period, which affect prices regardless of their ending, we provide novel evidence that favored price endings play a causal role in generating price rigidity at both the micro and macro levels.

Firms may face a variety of incentives to set price endings—defined for the purposes of our paper as the hundredths digit, or the farthest-most digit to the right of the decimal—to certain digits, in the belief that those digits convey information, and can either simplify or complicate

¹ For examples of this large literature across countries and across time, see Lach and Tsiddon (1992, 1996), Bils and Klenow (2004), Dhyne et al. (2006), Nakamura and Steinsson (2008), and Gagnon (2009).

² For broad evidence on price endings in U.S. data and the prevalence of zeros and nines, see, e.g., Schindler and Kirby (1997), Levy et al. (2011), and Knotek (2011, 2019). For international evidence on price endings that finds similar patterns, see, e.g., Holdershaw, Gendall, and Garland (1997), Deutsche Bundesbank (2002), and Hahn and Marenčák (2018). Jones (1896), Sherman (1928), and Hower (1943) provide historical evidence on price endings.

transactions and mental coding, in addition to their allocative properties. We document a strong correlation between firms' pricing practices and characteristics at the point-of-sale, such that stores selling a smaller number of items to consumers typically set zero-ending prices, while stores in which consumers purchase a large basket of items typically set nine-ending prices. More generally, however, we find that the vast majority of stores—94.0 percent—reveal a preference for a single “favored” price ending that comprises more than half of all price observations from that store. Favored price endings tend to be either zero or nine and are used heavily: Among stores with a favored price ending, 85.6 percent of the prices had the favored ending digit.

Overall, the prices of items set to a store's favored ending tend to be more rigid than prices set to nonfavored endings. But this correlation need not establish causation, because firms may set the prices for some items to favored endings precisely because they anticipate that those items will be subject to fewer shocks, and hence, their prices are less likely to change. We circumvent this potential endogeneity by examining pricing behavior around an exogenous shock that affects nearly all prices regardless of their ending: changes in the VAT rate.³ Most of the items in our data set are subject to VAT and their posted prices include VAT. During our sample, the VAT rate in Israel changed eight times, with four increases and four decreases. In months when the VAT rate changed, we document that items subject to VAT with nonfavored endings experienced an increase in the frequency of price adjustment that was approximately twice as large as the increase in the frequency of adjustment for prices that had favored endings.⁴

³ Other papers that use changes in the VAT rate as a source of exogenous variation—without considering its interactions with price endings—include Gadzinski and Orlandi (2004) for multiple European countries including Belgium, Denmark, Greece, Spain, and Italy; Carbonnier (2007) for France; Hoffmann and Kurz-Kim (2006) for Germany; Gábel and Reiff (2009) for Hungary; Ribon and Sayag (2013) for Israel; and Karadi and Reiff (2019) for Hungary.

⁴ Ribon and Sayag (2013) present further evidence of the effects of changes in the VAT rate in Israel on the durations between price adjustments. The law in Israel requires that posted prices must include VAT unless the

Using the prices of fresh fruits and fresh vegetables, which are exempt from VAT in Israel, we create a propensity score matched sample of items subject to VAT and items exempt from VAT and compare their pricing behaviors around the VAT rate changes. Among goods subject to VAT, the prices of products that had favored endings were less likely to change than the prices of products that had nonfavored endings. We find no evidence that the probabilities of changes in the prices of fresh fruits and fresh vegetables around VAT rate changes were related to their previous price endings, and limited evidence that their price change probabilities were higher than normal at the time the VAT rate changed. These results provide confirmation that the main shock to pricing activity in the months when the VAT rate changed was indeed the VAT rate change itself rather than other, broader macroeconomic factors, or that there was a price-ending-specific shock that would have differentially affected prices with nonfavored endings more than prices with favored endings. Hence, we are left with the finding that prices with nonfavored endings respond in a more flexible fashion than prices with favored endings to the same shock, consistent with favored endings playing a causal role in generating price rigidity.⁵

Finally, we examine the pass-through of VAT rate changes to aggregated inflation rates. Focusing on the four episodes in which the VAT rate increased by +100 basis points, we group

good or service is exempt. In terms of data collection (see Ribon and Sayag 2013), price observations recorded by the Israel Central Bureau of Statistics fall into three categories: (1) items with a posted price that includes VAT (the vast majority of prices); (2) items that do not have a posted price, where the VAT rate is added later to the reported price (mainly services); and (3) items that are exempt from VAT (fresh fruits, fresh vegetables, and goods and services purchased in Eilat). Our observations come entirely from categories (1) and (3).

⁵ Ater and Gerlitz (2017) take advantage of a different shock that occurred in Israel to study the effect of price endings on price rigidity. In January 2014, the Israeli government outlawed the use of non-zero-ending prices. Using a small sample of data collected in supermarkets before and after the law was changed, Ater and Gerlitz (2017) find that once all prices became zero-ending, the prices of goods that were previously nine-ending were no longer more rigid than other prices, suggesting that price rigidity prior to the change in the law was the result of the ending rather than an attribute of the goods. Snir, Levy, and Chen (2017), however, show that soon after the law changed the most common price ending in supermarkets became 90, and that 90-ending prices acquired many of the characteristics that nine-ending prices had prior to the change in the law.

our micro price observations based on whether they had a favored ending or a nonfavored ending in the month prior to the VAT rate change and calculate the cumulative inflation among the prices within these two groups following the shock. In the aggregate, changes in the VAT rate were passed through fully and immediately to the group of prices that had nonfavored endings. By contrast, aggregate inflation rose sluggishly among the observations that had favored endings immediately before the VAT rate change, converging to the inflation rate among the nonfavored prices after about eight months. These results suggest that favored endings play a key role in generating price rigidity at both the micro level and the aggregate level in response to VAT rate—and likely other—shocks.

II Establishment Types, Favored Price Endings, and Price Rigidity

Using U.S. data, Knotek (2011) provides empirical evidence that characteristics at the point-of-sale are strongly correlated with price endings. For establishments with many cash-based transactions, where purchases typically involve a small number of items and are repeated frequently or involve queueing, prices are often simple multiples of commonly carried currency denominations and are more likely to end in the digits zero or five, to align with the currency denomination structure. Knotek (2008) labels these “convenient prices,” and they create incentives for firms to use these prices to simplify and expedite the physical act of making a transaction; see also Levy and Young (2004). By contrast, Knotek (2011) documents that nine-ending prices are more common in establishments selling many items, where consumers typically purchase a large basket of goods and pay by non-cash methods. When paying with noncash methods, payment time is independent of the sum paid, and consumers cannot save time

by paying convenient prices in noncash transactions. However, consumers purchasing many items in a single establishment can save shopping time by economizing on the amount of time and attention spent on each item purchased; see, e.g., Dickson and Sawyer (1990). In such establishments, consumers often compare prices by processing digits from left to right, paying relatively more attention to left-most digits and relatively less attention to right-most digits, which gives retailers an incentive to set nine-ending prices to take advantage of the truncation of prices, as noted by Thomas and Morwitz (2005).

Price endings also convey information to consumers. For example, Schindler and Kibarian (2001) find that zero-ending prices are associated with higher quality, while nine-ending prices are associated with lower quality. At the same time, nine-ending prices are often associated with bargains, discounts, and temporary price markdowns, to such an extent that nine-ending prices can stimulate demand for a product compared with similar non-nine-ending prices; see Schindler and Kibarian (1996, 2001) and Anderson and Simester (2003).

Several theories posit a special role for the digit zero. Chen, Levy, and Snir (2013) note that zero-ending prices may be attractive per se because zero's shape follows the Gestalt principles of symmetry and closure. Schindler and Kirby (1997) argue that round prices are "cognitively accessible" because zero reflects the absence of value, allowing consumers to process integers and simple fractions faster and more accurately than other numbers. Wadhwa and Zhang (2015) argue that round prices are associated with a subjective experience of feeling "right." Beyond transaction convenience, Chen, Levy, and Snir (2013) posit that, in locations where consumers purchase a small number of items, zero-ending prices facilitate consumers' buying decisions by simplifying comparisons between the posted prices and the consumers' reservation prices.

Incentives to set certain price endings for transactions motives, psychological motives, or other motives can create a favored price ending for an establishment, where a majority of prices end in a particular digit. This can be true whether the benefit of the price ending is real or perceived. In turn, theory suggests that the prices of items with favored price endings will be relatively rigid, regardless of the actual ending digit chosen. In a state-dependent pricing model, Knotek (2008) shows that convenient prices can be relatively rigid for an optimizing firm in response to small economic shocks even in the absence of canonical menu costs. Knotek (2019) obtains similar results for nine-ending prices in a state-dependent pricing model in which firms receive a benefit from setting nine-ending prices and pay a menu cost to change their nominal price. Favored price endings can also help explain the prevalence of certain regular “reference” prices, as documented in Eichenbaum, Jaimovich, and Rebelo (2011), because they serve as a focal point to which the price will return following a sale or temporary price markdown, as documented in Knotek (2019).

III Data

To establish the direction of causation from favored price endings to price rigidity, we need to observe shocks that impact all items regardless of their ending. Otherwise, firms could be setting the prices of certain items to favored endings precisely because they anticipate that the prices of these goods are unlikely to change—implying a correlation between favored price endings and price rigidity, but not necessarily causation from the former to the latter. Changes in the VAT rate serve as an exogenous shock with respect to price endings and accomplish this objective because they impact items regardless of their price ending.

To study the relationship between price endings and price rigidity in the presence of VAT rate changes, we use micro price data collected by the Israel Central Bureau of Statistics (CBS) on a monthly basis for compiling the consumer price index (CPI) in the period January 2002 through December 2013.⁶ The observations are on entry-level items (ELIs). Each observation is a vector of information including the price of the good, the type of store in which the price was observed, information about the locality where the observation was collected, and some further attributes regarding the good. Our study relies on a subset of observations for the entire universe of ELIs used to compute the CPI. Our data set contains all observations for ELIs that fall into 124 categories of goods. While a small subset of the overall CPI data set, our observations span a range of goods, a variety of establishment types, and a wide range of prices. Our sample contains a total of 599,661 monthly price observations, with an average price of NIS 161.48 and a median price of NIS 8.00.⁷ Our data set contains data from 1,995 stores (establishments) in Israel.⁸

While aggregate inflation dynamics in Israel during our sample bore similarities to those in the U.S., a key feature of our data set compared with U.S. micro price data is the presence of VAT.⁹ Given that VAT is common in many countries, especially in Europe, we view our study as fairly representative of pricing behavior across a number of advanced economies. VAT has

⁶ In January 2014, the Israeli government outlawed the use of non-zero-ending prices, creating a structural break in the dataset; see Snir, Levy, and Chen (2017) and Ater and Gerlitz (2017). We make use of limited data prior to 2002 for calculating stores' favored price endings and inflation over the one-year period leading up to the first VAT change in 2002, as described below.

⁷ NIS stands for the New Israeli Shekel. Over our sample period, the exchange rate averaged NIS 4.09 to US\$1.

⁸ To study price changes, we require price observations in both the previous month and the current month—we do not impute missing data. We do not have a flag from the CBS indicating whether a price was a sale (i.e., a discount or temporary price markdown) or not. To create a sale flag, we follow the algorithm in Knotek (2019). This algorithm requires only that prices decline in a given month and then subsequently increase in the next month; it does not require prices to return exactly to their previous pre-sale level.

⁹ During the sample period 2002-2013, year-over-year inflation in the CPI in Israel (including VAT) averaged 2.3 percent, with a maximum of 6.9 percent and a minimum of -2.8 percent. Over the same period, the comparable figures for the U.S. CPI were quite similar: 2.3 percent for average year-over-year inflation, with a maximum of 5.6 percent and a minimum of -2.1 percent.

been charged in Israel on goods and services since July 1976. The VAT rate on all goods and services is identical, and posted prices are required by law to include VAT. The only exemptions are for fresh fruits, fresh vegetables, and goods and services sold in the city of Eilat. We are able to use these exemptions as controls in our analysis. Unlike in some countries, the VAT rate in Israel has changed rather frequently: During our sample period, there were four VAT rate decreases and four VAT rate increases. As summarized in Table 1, each change in the VAT rate was either 0.5 percentage point or 1.0 percentage point.

IV Patterns of Price Endings and Evidence on Favored Endings

We provide empirical evidence that most stores have a favored price ending and that this favored price ending is used extensively within the store. These favored price endings typically—but not universally—correlate with store types, giving rise to revealed preferences regarding pricing behavior that we use later to investigate price rigidity.

Based on a priori incentives, Knotek (2011) posits that price endings should be correlated with characteristics at the point-of-sale, with convenience-like locations setting zero-ending prices and superstore-like locations setting nine-ending prices. We therefore use information on the type of store in which each price observation was collected to classify stores on an ex-ante basis into superstore-like locations, convenience-like locations, and other locations. We classify a store as a superstore-like location if it is identified as a supermarket, chain store, department store, or drugstore, given the typical large volume of goods and large associated shopping baskets in these locations. We classify a store as a convenience-like location if it is identified as

a convenience store, a small grocery, an open market stall, or a specialty store, given the typically more limited options and smaller shopping baskets in these locations.

The top panel of Table 2 looks at price endings by establishment types in our Israeli micro data, relying on the ex-ante classifications for convenience-like locations and superstore-like locations. For convenience-like locations, there is a very strong adherence to prices ending with a final digit of zero: 85.0 percent of all observations end in the digit zero. For superstore-like locations, the vast majority of prices—66.4 percent—have a final digit of nine. Thus, our first finding is that we document broad support using CPI micro data for the idea that firms' preferred price endings correlate with characteristics at the point-of-sale, in line with the findings of Knotek (2011) for a single U.S. city.

However, a number of superstore-like locations did not use nine endings as the most common ending during our sample period, suggesting that favored price endings may reflect a choice on the part of price setters rather than a structural aspect of the retail environment. To this end, we allow revealed preference to dictate stores' favored endings. A store is classified as having a favored ending if more than 50 percent of its prices share the same final digit; this threshold guarantees that a store can only have a single favored price ending.

In the bottom panel of Table 2, we examine the incidence of favored price endings based on revealed preference in convenience-like locations and superstore-like locations. Among convenience-like locations, zero is clearly the most common favored ending used by 88.9 percent of stores. Among superstore-like locations, nine is the most common favored ending for 55.7 percent of stores, followed by zero for 33.5 percent of stores.

Table 3 further explores the incidence of favored price endings. Our second finding is that most stores have a favored ending and use that favored ending extensively. In our sample,

94.0 percent of stores had a favored price ending that was used for more than half of their prices. Among all stores with a favored price ending, 85.6 percent of their prices used the favored ending digit. The heavy usage of the favored ending digit was true regardless of the digit chosen. We view the concept of favored endings as more flexible than tying price endings to establishment types—even though the concepts are strongly correlated in most cases—and as showing a store’s revealed preference for a particular price ending. Therefore, we focus on this revealed preference in our empirical exercises.

V Favored Endings and Implications for Price Rigidity

In general, we find that observations that had a favored price ending in the previous month exhibit greater price rigidity than observations that had a nonfavored ending. Table 4 presents summary statistics showing the relationship between price endings and price rigidity in our data set.¹⁰ Observations that had a favored price ending in month $t-1$ are more rigid in month t whether we compare them with all observations that had a nonfavored ending, observations with nonfavored endings from stores with a favored ending, or observations from stores without a favored ending (which, by definition, had a nonfavored ending in the previous month). Similar patterns hold whether we look at all observations in column (a), if we exclude VAT-exempt observations in column (b), if we exclude sales in column (c), or if we exclude sales and VAT-

¹⁰ In the summary statistics in this table and in previous tables, a store is classified as having a favored price ending if more than 50 percent of all of its price observations have the same ending over the entire sample period 2002–2013. Assuming that a store either has a favored ending or not throughout the sample period simplifies the summary statistics. However, this process implicitly involves using future price information to classify whether a price was a favored ending in the early part of the sample. In the figures and the regressions below, in each month t we recursively use an expanding window to determine whether each store had a favored ending using only the available lagged price data from December 2001 through the previous month $t-1$, which provides an econometrically proper measure to include in our regressions.

exempt observations in column (d).¹¹ Given the above finding that zero-ending and nine-ending prices are the most common favored endings, the bottom portion of the table shows that favored endings are correlated with price rigidity in a fashion similar to zero- and nine-ending prices, as in Knotek (2008), Levy et al. (2011), and Knotek (2019). While we focus only on the final digit of the price in this paper, the same concept can be applied to the final two digits of the price as well. The stickiest of prices end in 00, and prices ending in the two digits 99 are also more rigid than prices ending in the digits 01 through 98.¹²

These findings are consistent with the idea that favored price endings contribute to price rigidity by making prices insensitive to small shocks, akin to a menu cost that prevents frequent small price adjustments, especially if the size of the shock is smaller than the distance to the nearest favored ending, as in Knotek (2008, 2019) and Levy et al. (forthcoming). However, establishing the direction of causality from favored endings to price rigidity requires exogenous shocks that affect prices regardless of their ending.

To explore the causal role of favored endings in generating price rigidity, we focus on pricing behavior around changes in the VAT rate. Figure 1 plots the frequency of price changes during the months immediately before and after changes in the VAT rate. We detrend each series by subtracting the “normal” frequency of price adjustments for that series, defined as the average frequency of price changes outside of months with a change in the VAT rate. The figure shows stark patterns. Panel (a) shows that price setters do not front-run the changes in the VAT rate: The frequency of adjustment is effectively normal in the months leading up to the change in

¹¹ Removing VAT-exempt observations results in a significant decrease in the monthly frequencies of price changes because most of the VAT-exempt observations are fresh fruits and fresh vegetables, which have high frequencies of price adjustment (Bils and Klenow 2004).

¹² As prices increase, it is possible that a store’s set of favored endings may change from, e.g., zero-ending prices to double-zero-ending prices. With a relatively short sample, we do not explore this possibility in this paper.

the VAT rate, spikes in the month of the VAT rate change, and then falls off sharply soon thereafter. The peak response among prices subject to VAT is 10.5 percentage points above its normal frequency. Given that the normal frequency of price changes for goods subject to VAT is 19 percent per month (see Table 4), this suggests that prices are 55 percent more likely to change in the month of a change in the VAT rate than normal. However, the figure also makes clear that while prices are relatively more flexible at VAT rate changes, they are not perfectly flexible: Frequencies of adjustment are far from 100 percent. By contrast, among items that are exempt from VAT (fresh fruits, fresh vegetables, and items sold in Eilat), the frequency of adjustment responds only minimally to changes in the VAT rate.¹³

There are clear and marked differences in the responsiveness of prices to changes in the VAT rate depending on whether prices had a favored ending in the previous month or not. This finding holds even when we examine prices with different endings within the same stores. In panel (b), the frequency of adjustment among goods with a favored ending in the previous month increases by 8.6 percentage points in the month of the VAT change, while the frequency of adjustment among goods that had a nonfavored ending jumps by 15.7 percentage points. Panel (c) splits prices that had a nonfavored ending into two groups: those coming from stores that had a favored ending, and those coming from stores without a favored ending.¹⁴ Within stores that had a favored ending, the frequency of adjustment for prices with a nonfavored ending jumps by 19.9 percentage points—more than twice the response of the goods with favored endings in the exact same stores. Excluding sales does not change these findings, as documented in panel (d).

¹³ Among the items exempt from VAT, which are mostly fresh fruits and fresh vegetables, the average frequency of a price change is 60 percent in our monthly data, and the maximal increase in the frequency of price changes is 1.7 percentage points two months after the change in the VAT rate—a trivially small change compared with the normal adjustment frequency.

¹⁴ In the latter group, all prices would be defined as having a nonfavored ending.

A priori, there is no reason to believe that the literal menu costs of changing prices are lower for goods that had a nonfavored ending than for those that had a favored ending. Thus, it is unlikely that the differences in the frequency of adjustment could be rationalized by menu costs, especially given that goods with both price endings are subject to the same exogenous shock. Rather, the evidence is consistent with a causal role for favored price endings in generating price rigidity.

Regression Results for the Probability of Changing Prices

To further quantify our results, we conduct a formal regression analysis. The price rigidity literature has a long history of using logit, probit, or linear probability models to assess the probability of a price changing based on covariates and controls; see, among others, Cecchetti (1986), Bils and Klenow (2004), Klenow and Malin (2011), and Levy et al. (2011). With a large number of fixed effects in our panel data, our regressions take the form of a linear probability model:

$$y_{i,t} = \beta_1 D_{\text{Favored ending},i,t-1} + \beta_2 D_{\Delta\text{VAT},t} + \beta_3 D_{\text{Favored ending},i,t-1} \times D_{\Delta\text{VAT},t} + \Gamma \mathbf{X}_{i,t} + \alpha_i + e_{i,t} \quad (1)$$

where $y_{i,t}=1$ if the price of item i changed in month t and $y_{i,t}=0$ otherwise, and $e_{i,t}$ is a regression residual. Our key explanatory variables are: $D_{\text{Favored ending},i,t-1}$, a dummy variable taking on the value of 1 if the item's price in the previous month had a favored ending as defined previously and 0 otherwise; $D_{\Delta\text{VAT},t}$, a dummy variable taking on the value of 1 if the VAT rate changed in month t and 0 otherwise; and the interaction of these two dummy variables. The vector $\mathbf{X}_{i,t}$ includes other relevant controls. We include the level of the previous price, $\ln(p_{i,t-1})$, to account for the fact that the distance between successive prices with favored endings shrinks in

percentage terms as prices rise, which could lead to more frequent adjustments from one favored ending to another at higher prices. We also include a dummy variable indicating whether the item was on sale in the previous month, $D_{\text{Sale},i,t-1}$, given that sales, by definition, are relatively short-lived price markdowns followed by a price increase. Further controls include a linear time trend and month dummies to capture seasonality in the frequency of price changes. Our baseline specifications include an interacted product×store fixed effect, α_i , to control for different underlying frequencies of price change across products within a given store and across stores for a given product.¹⁵

Table 5 reports the results from the regression. In our baseline specification in column (a), we exclude items that are exempt from VAT. In general, favored endings are associated with more rigid prices: Having a favored ending in the previous month reduces the probability of adjustment by 2.5 percentage points. Consistent with state-dependent price adjustment models, firms respond to VAT rate changes with a higher-than-normal frequency of price changes.¹⁶ Most importantly for this study, the regression captures the phenomenon illustrated in Figure 1: While the frequency of price change increases when the VAT rate changes, the increase is systematically related to the previous month’s price ending. On average, a change in the VAT rate is associated with a 15.2 percentage point increase in the probability of a price change for goods that had a nonfavored ending in the previous month. A change in the VAT rate is

¹⁵ In results not reported, beyond our interacted baseline specification with product×store fixed effects, we also considered regressions that included product fixed effects only, store fixed effects only, and product and store fixed effects separately. The estimated coefficients on $D_{\Delta\text{VAT},t}$ and $D_{\text{Favored ending},i,t-1} \times D_{\Delta\text{VAT},t}$ were quantitatively similar across fixed effects specifications. When conducting the estimation using only product fixed effects, the coefficient on $D_{\text{Favored ending},i,t-1}$ decreased (i.e., became a larger negative number), implying that favored endings played a bigger role in generating price rigidity; results from the other fixed effects specifications were generally similar to the baseline results reported.

¹⁶ For further evidence of state-dependence around changes in the VAT rate in other countries, see, e.g., Aucremanne and Dhyne (2004), Baudry et al. (2004), and Hoffmann and Kurz-Kim (2006).

associated with only an 8.0 percentage point increase in the probability of a price change for goods that had a favored ending in the previous month. We obtain similar results if we also exclude the observations that were part of a sale around the VAT rate change in column (b) or if we include all observations, whether they were subject to VAT or not, in column (c).¹⁷

Although changes in the VAT rate are exogenous shocks that affect all goods equally regardless of their price ending, the increase in the frequency of price adjustment for goods with nonfavored endings is about twice as large as the increase in the frequency of price adjustment for goods with favored endings in the month of the VAT rate change. These results suggest that favored price endings are playing a causal role in generating the price rigidity we observe.

Regression Results Using Matched Samples

To account for the possibility that other shocks may have occurred around the VAT rate changes that could be driving our results, we use fresh fruits and fresh vegetables as a control group and more directly compare their pricing behavior around the VAT rate changes with the pricing behavior of goods subject to VAT.¹⁸ To this end, we create a propensity score matched sample of goods subject to VAT (the treated group) and goods not subject to VAT (the control group) and run identical regressions on the two samples. For each date τ that experienced a

¹⁷ The sticky price literature has not come to a consensus on a single appropriate way to think about sales as they relate to allocative price rigidity; see, e.g., Bils and Klenow (2004), Dhyne et al. (2006), and Nakamura and Steinsson (2008). The fact that many sales are exactly undone—in that the post-sale price simply returns to its pre-sale level, as highlighted in Guimaraes and Sheedy (2011) and Eichenbaum, Jaimovich, and Rebelo (2011)—suggests that it may be appropriate to simply exclude sales from macroeconomic models.

¹⁸ If we define treatment as an item receiving a VAT rate change shock, then fresh fruits and fresh vegetables have a zero probability of receiving the treatment, which violates the overlap assumption required to conduct a formal average treatment effects estimation. While all items sold in Eilat are exempt from VAT, the small number of observations from this little city produces poor matches and repeatedly samples the same observations. Hence, we prefer to use fresh fruits and fresh vegetables as our control group.

change in the VAT rate and for each item $i=1, \dots, I$ that was subject to VAT and hence “treated” by the change in the VAT rate, we collect a vector of lagged data on prices, sale dummies, and favored-ending dummies for item i during the previous four months:

$$v_{i,\tau} = \langle P_{i,\tau-4}, P_{i,\tau-3}, P_{i,\tau-2}, P_{i,\tau-1}, D_{Sale,i,\tau-4}, D_{Sale,i,\tau-3}, D_{Sale,i,\tau-2}, D_{Sale,i,\tau-1}, D_{Favored\ ending,i,\tau-4}, D_{Favored\ ending,i,\tau-3}, D_{Favored\ ending,i,\tau-2}, D_{Favored\ ending,i,\tau-1} \rangle \quad (2)$$

At that same date τ , we use Mahalanobis matching with replacement to find the nearest neighbor j among the J items not subject to VAT based on the corresponding vector $v_{j,\tau}$.^{19,20} We store the observations for item i from date $\tau-4$ through the VAT rate change at date τ in the first sample and store the observations for item j from the same time period $\tau-4$ through τ in the second matched sample, and we repeat this process for all items i at all VAT rate changes. This process creates matched samples whose observations come from the months immediately prior to and including the VAT rate changes.

In Table 6, we present results from rerunning the price change regressions in equation (1) on the matched samples. Looking across all price endings, a VAT rate change is associated with an increase in the probability of adjustment by 17.5 percentage points for the items subject to VAT in column (a) if the previous price did not have a favored ending. By contrast, if the previous price did have a favored ending, a VAT rate change is associated with an increase in the probability of adjustment of only 12.1 percentage points. For goods in the control group that are

¹⁹ Our baseline matching procedure seeks items i and j with similar price levels, sales behavior, and favored ending dummies prior to the date of the VAT change. Along other dimensions, matching fresh fruits’ and fresh vegetables’ characteristics to those of other items will be inherently imprecise; e.g., fresh fruits and fresh vegetables in our control group exhibit more frequent price changes than other goods. The use of product×store fixed effects helps to control for some of these intrinsic differences. Nevertheless, because some of the treated observations are very different from all of the control observations even along the characteristics that we use to match, we drop treated observations if they are outside of the support of the control group.

²⁰ In results not reported, we forced the nearest neighbor j for observation i subject to VAT to come from the same store and then matched on $v_{i,\tau}$ and $v_{j,\tau}$ as above. The requirement that the match occurs for products within the same store greatly shrinks the sample size—to the neighborhood of 1,000 observations—with too few observations on fresh fruits and fresh vegetables with nonfavored endings to conduct statistical inference. Qualitatively, other results were similar to those reported in Table 6.

not subject to VAT in column (b), a change in the VAT rate has a much smaller impact on the frequency of price adjustment (+5.7 percentage points) that is only marginally statistically significant.

Columns (c) and (d) run the same regressions using only the subsets of the two matched samples in which the previous price had a favored ending, while columns (e) and (f) do likewise using only the subsets of the two matched samples in which the previous price had a nonfavored ending, conditional on the store having a favored ending, to enhance comparability with the favored ending subsets.²¹ Among goods subject to VAT, the combination of a VAT rate change and a nonfavored price ending in the previous month in a store that had a favored ending is associated with a 26.6 percentage point increase in the probability of adjusting its price in column (e), while a good with a favored ending saw the probability of adjusting its price increase by just 11.7 percentage points in column (c). Thus, the response of the frequency of adjustment following a change in the VAT rate is more than twice as large for nonfavored endings as it is for favored endings. By contrast, among fresh fruits and fresh vegetables that were not subject to VAT, there is no statistically significant increase in their price change probabilities around VAT rate changes in column (d) and column (f). Furthermore, there is no statistically significant difference in price change probabilities around VAT rate changes for fresh fruits and fresh vegetables that is related to their previous price endings, based on the similarity of the magnitudes of the estimated coefficients on the VAT rate change dummies. These results provide confirmation that the main drivers of pricing activity around the VAT rate changes were indeed coming from the VAT rate changes and not from other, broader macroeconomic factors or from some type of price-ending-specific factors.

²¹ These sub-analyses exclude observations from stores that did not have a favored ending, which accounts for the dropped observations compared with the numbers reported in columns (a) and (b).

Regression Results for the Size of Price Changes

We complement the frequency regressions with regressions for the absolute size of price changes to assess adjustments along the intensive margin. Our specification takes the form:

$$|100\Delta\ln(p_{i,t})| = \beta_1 D_{\text{Favored ending},i,t-1} + \beta_2 D_{\Delta\text{VAT},t} + \beta_3 D_{\text{Favored ending},i,t-1} \times D_{\Delta\text{VAT},t} + \Gamma\mathbf{X}_{i,t} + \alpha_i + e_{i,t} \quad (3)$$

where the dependent variable is the absolute percentage price change (conditional on a change having occurred) and the explanatory variables on the right-hand side of equation (3) are the same as those in the frequency regression, with the intention of again primarily examining pricing behavior around changes in the VAT rate. Table 7 reports the results.

On average, when there is a change in the price of a good that had a favored ending, the absolute size of the price change is 0.6 to 1.1 percentage points larger than it is for nonfavored endings.²² This result is intuitive in a setting with positive trend inflation, which was the case during most of our sample, as these relatively rigid prices need to adjust by more in percentage terms to offset the additional cumulative inflation that occurred since the last price change. However, when the VAT rate changes, the price changes that occur are smaller than normal in absolute terms. Excluding VAT-exempt goods, the absolute size of price changes for favored endings is smaller than that for nonfavored endings at VAT rate changes—a finding that runs opposite their normal pattern.

Abstracting from the issue of favored versus nonfavored endings, the general findings of more frequent and smaller price changes when the VAT rate changes can be reconciled within

²² For the sake of comparison, the mean absolute size of price changes in our sample is 22.0 percent, and the mean absolute size of nonsale price changes is 18.8 percent.

multiple theoretical frameworks. In an environment of imperfect information where paying attention is costly (e.g., Sims 2003), changes in the VAT rate are well-communicated shocks that could elicit a price response with minimal attention allocated to them. Given that the average size of a price change in our sample is large in percentage terms, adjusting prices only by the amount of the change in the VAT rate would lead to smaller-than-normal changes. At the same time, these smaller and more frequent price changes are also consistent with reduced costs of changing prices during these events because of economies of scope in price changes (e.g., Midrigan 2011) or lower customer antagonization costs because consumers are aware of the VAT rate changes and are more likely to accept them (e.g., Rotemberg 2005, 2011, Anderson and Simester 2010). Accounting for the role of favored endings, the spike in the frequency of adjustment at VAT rate changes requires an important role for state-dependent pricing models such as in Knotek (2019).

VI Aggregate Implications of Favored Endings for VAT Pass-Through

For goods subject to exogenous cost shocks in the form of VAT rate changes that affect items regardless of their previous price ending, our regression results document that favored price endings are less likely to change in response to the VAT rate change and, if they do change, they tend to change by a smaller amount in absolute percentage terms relative to nonfavored price endings at VAT rate changes. When put together, these two channels suggest that favored endings are a significant source of price inertia vis-à-vis nonfavored endings. To highlight the aggregate implications of favored endings, we conduct the following exercise to examine the

pass-through of changes in the VAT rate to inflation, which can be viewed as a proxy for the pass-through of other cost shocks in the presence of favored endings.

There were four equally sized changes in the VAT rate of +100 basis points during our sample period, in June 2002, July 2009, September 2012, and June 2013, and we examine pricing behavior in the wake of these four comparable episodes.²³ Assuming the VAT rate changed at time τ , we store all the price observations from month $\tau-1$ and then classify the price endings as favored endings (which we denote F), all nonfavored endings (NF), and nonfavored endings in stores with a favored ending ($NF|F$). We calculate cumulative inflation from time $\tau-1$ through time $\tau+k$ for each observation i , $\pi_{i,\tau+k} = 100\ln(p_{i,\tau+k} / p_{i,\tau-1})$. Assuming that each observation around each VAT change receives an equal weight, for each of the groups of prices $g \in \{F, NF, NF|F\}$ we calculate aggregate cumulative inflation through the k -th month after each of the four VAT rate changes, $j=1,2,3,4$, as

$$\pi_{j,\tau+k}^g = \frac{1}{N_{j,\tau+k}^g} \sum_{i=1}^{N_{j,\tau+k}^g} \pi_{i,\tau+k} \quad (4)$$

where $N_{j,\tau+k}^g$ is the number of observations in the group of prices g .

These cumulative inflation rates allow us to compare pass-through of the VAT shocks based on price endings in the month before the VAT rate changed. In Figure 2(a), we report the average across the four VAT rate changes of the ratios of aggregate cumulative inflation for the favored endings to all nonfavored endings, $(1/4)\sum_{j=1}^4 \pi_{j,\tau+k}^F / \pi_{j,\tau+k}^{NF}$, and the average of the ratios

²³ We focus on the four VAT rate increases because they were identically sized and were pushing prices in the same direction as trend inflation during the period under consideration; the VAT rate decreases were two different sizes, and the competing forces of VAT rate decreases and positive trend inflation make interpreting their results challenging.

for the favored endings to the nonfavored endings in stores with a favored ending,

$$(1/4) \sum_{j=1}^4 \pi_{j,\tau+k}^F / \pi_{j,\tau+k}^{NF|F}.$$

We find that the aggregate pass-through rate among the favored endings is one-fourth of the aggregate pass-through rate among the nonfavored endings based on our cumulative inflation measures in the month of the VAT rate change. Pass-through among the favored endings only catches up to the nonfavored endings eight months after the VAT rate changed.²⁴ The dynamics are similar whether we look at all nonfavored endings or only the nonfavored endings in stores with a favored ending. In the aggregate, favored price endings generate considerable price rigidity compared with their nonfavored-ending counterparts.

While the above exercise measures pass-through in a relative sense, to measure pass-through in an absolute sense we first need to account for trend inflation around the VAT rate changes. Based on all the observations in our sample around each VAT shock j at time τ , we calculate the inflation trend as the year-over-year inflation rate for the 12 preceding months:

$$\bar{\pi}_j = \frac{1}{N_j} \sum_{i=1}^{N_j} 100 \ln(p_{i,\tau-1} / p_{i,\tau-13}) \quad (5)$$

where N_j is the number of available price observations around VAT shock j at time $\tau-1$ and $\tau-13$.

We then subtract the partial-year value of the inflation trend to detrend each of our cumulative inflation measures g for VAT shock j for the k -th month after the shock, $\pi_{j,\tau+k}^g - [(1+k)/12]\bar{\pi}_j$.

Figure 2(b) shows the average across the four VAT rate changes of these detrended cumulative inflation measures. On impact, detrended cumulative inflation among the

²⁴ We only look at pass-through for eight months after the VAT rate changed because the VAT rate decreased in January 2010, six months after the July 2009 change; the June 2013 change was nine months after the September 2012 change; and our sample ends in December 2013, six months after the June 2013 change, leaving us with only a single +100 basis point VAT rate change with uncontaminated observations extending more than eight months past the VAT rate change. When calculating averages, we omit statistics from the July 2009 and June 2013 changes after six months.

nonfavored endings increases more than 1 percentage point. Because the VAT rate increased by 1 percentage point in each episode, this result implies that, on net, the VAT change is entirely and immediately passed through in the aggregate among the nonfavored prices. The response builds over time, such that pass-through among the nonfavored endings is considerably greater than one-for-one. By contrast, pass-through is much smaller initially for prices that had a favored ending in the month before the VAT changed and only builds slowly over time. Given that prices with a favored ending comprise a majority of all prices in our data set, the sluggish pass-through of the VAT rate change to all prices in our data set is coming from favored endings. Thus, we conclude that favored endings play a key role in generating price rigidity at the aggregate level in response to these—and likely other—shocks.

In complementary work, Knotek (2019) develops a model featuring menu costs and favored price endings, in the form of nine-ending price points, and estimates model parameters using micro pricing moments coming from the scanner databases of large retail grocery stores. The aggregate dynamics of this price point and menu cost model depend crucially on the distribution of firms' desired prices. Under sufficient strategic complementarities, Knotek (2019) documents that price points can generate considerable periods of nonneutrality following monetary shocks, providing further evidence that price endings can have aggregate implications.

VII Conclusion

This paper looks at the relationship between price endings and price rigidity. We do so using a subset of the micro price data underlying the calculation of the CPI in Israel from January 2002 through December 2013. During our sample period, we document that most firms had a favored

price ending that they used extensively: 94.0 percent of firms had a single favored price ending that comprised more than half of all price observations, and 85.6 percent of their prices used the favored ending digit.

Based on frequent changes in the VAT rate during our sample period, we provide novel evidence that these favored price endings played a causal role in generating price rigidity. In months when the VAT rate changed, we document that items subject to VAT experienced an increase in the frequency of price adjustment for nonfavored endings that was approximately twice as large as the increase in the frequency of adjustment for favored endings. We find no such increase in the frequency of adjustment for items not subject to VAT, and among those items, we do not find any differential behavior among prices with or without a favored ending.

In the aggregate, we find that sluggish pass-through of VAT rate increases is due to favored endings; increases in the VAT rate are passed through fully and immediately to nonfavored endings. These results suggest that price endings may be playing a larger-than-appreciated role in generating both micro and macro price rigidity. Indeed, even as new technologies such as electronic shelf labels reduce physical menu costs, price rigidities in online markets correlate with price endings, as documented by Gorodnichenko and Talavera (2017). Future work that builds on Knotek (2008, 2019) by incorporating roles for price endings in macroeconomic models can help further explore the role that price endings play for monetary policy and the macro economy more broadly.

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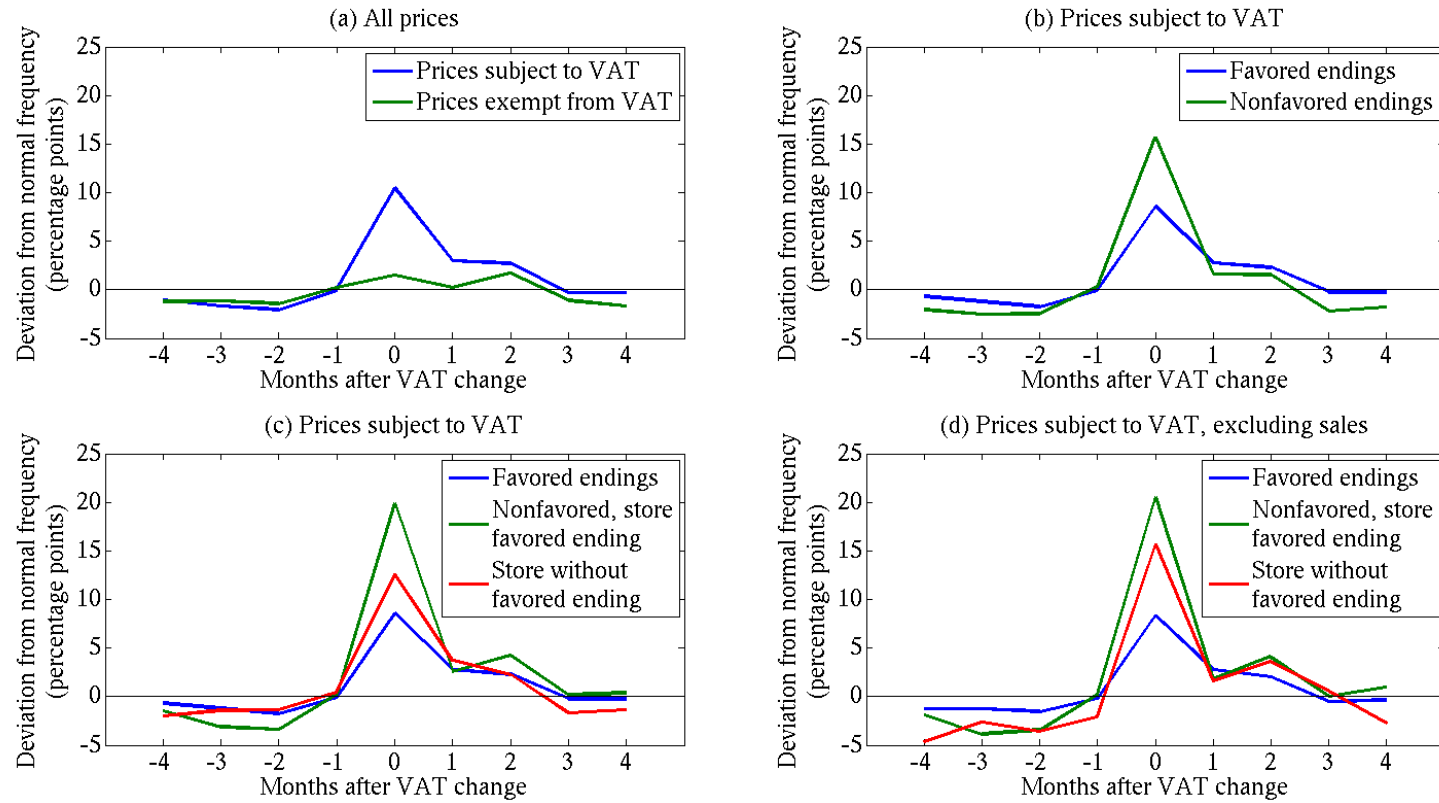
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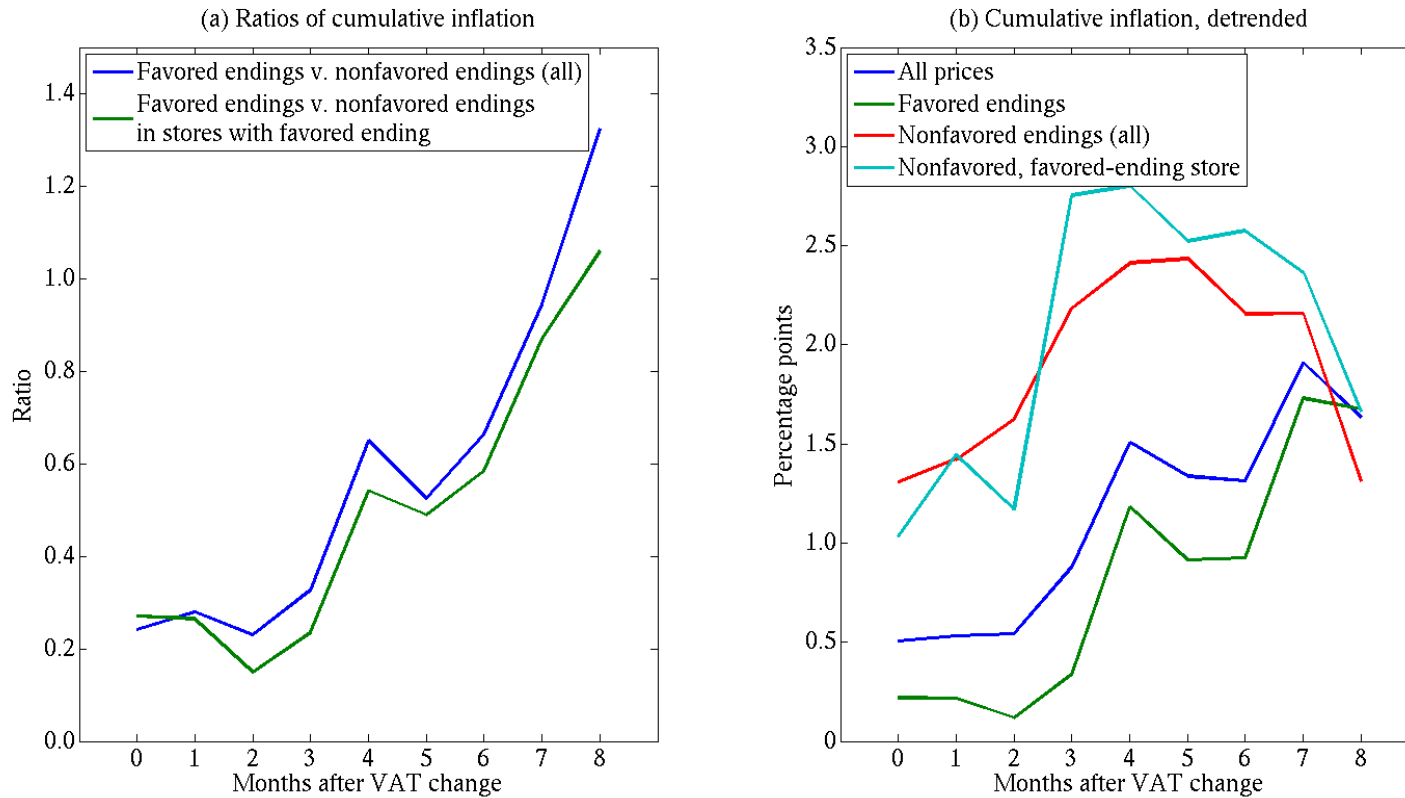
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Figure 1: Frequency of Price Changes Around VAT Changes



Notes: Figures show the frequency of price changes relative to their normal frequency, which is defined as the average frequency during the sample period excluding months in which the VAT changed. Favored and nonfavored endings are determined by price endings in the previous month. By definition, price observations from a store without a favored ending do not have a favored ending.

Figure 2: VAT Pass-Through Following +100 Basis Point VAT Rate Changes



Notes: The figures show cumulative inflation from the month before the VAT rate increased by 100 basis points. Panel (a) shows the average of the ratios of the cumulative inflation rates across the four such VAT increases for prices that had a favored ending in the month before the VAT change divided by all nonfavored endings in the month before the VAT change, and for prices that had a favored ending in the month before the VAT change divided by the prices that had a nonfavored ending in the month before a VAT increase but were from a store with a favored ending. Panel (b) shows the average cumulative inflation rates across the four such VAT increases among all prices, among prices that had a favored ending in the month before the VAT change, among all prices that had a nonfavored ending in the month before the VAT change, and among prices that had a nonfavored ending but were from a store with a favored ending in the month before the VAT change.

Table 1: Changes in VAT Rates in Israel, 2002-2013

Date	New VAT rate	Change (percentage points)
June 15, 2002	18%	+1.0
March 1, 2004	17%	-1.0
September 1, 2005	16.5%	-0.5
July 1, 2006	15.5%	-1.0
July 1, 2009	16.5%	+1.0
January 1, 2010	16%	-0.5
September 1, 2012	17%	+1.0
June 2, 2013	18%	+1.0

Table 2: Price Endings and Store Types

	Convenience-like locations	Superstore-like locations
Percentage of prices ending in:		
Zero	85.0	18.0
Nine	9.5	66.4
Other	5.5	15.6
Percentage of stores whose favored ending is:		
Zero	88.9	33.5
Nine	5.6	55.7
Other	0.3	3.3
No favored ending	5.2	7.5

Notes: All numbers are percentages. A store had a favored ending if more than 50 percent of its prices ended with the same digit. We classify a store as a superstore-like location if it is identified as a supermarket, chain store, department store, or drugstore. We classify a store as a convenience-like location if it is identified as a convenience store, a small grocery, an open market stall, or a specialty store. Authors' calculations using data from the Israel CBS.

Table 3: The Incidence of Favored Price Endings

	Share of total	Percentage of prices with favored endings
Stores with a favored ending	94.0	85.6
Stores whose favored ending is:		
Zero	76.9	92.8
Five	0.9	61.2
Seven	0.1	100.0
Nine	16.3	74.0

Notes: All numbers are percentages. A store had a favored ending if more than 50 percent of its prices ended with the same digit. The percentage of prices with favored endings is conditional on being in a store with a (specific type of) favored ending. Authors' calculations using data from the Israel CBS.

Table 4: Price Endings and Price Rigidity

	(a) All prices	(b) Excluding VAT- exempt prices	(c) Excluding sales	(d) Excluding sales, VAT-exempt prices
Frequency of price change	34.9	19.0	23.5	13.5
$p(t-1)$ had favored ending	33.7	13.7	22.4	10.0
$p(t-1)$ had nonfavored ending	39.6	33.6	27.7	23.9
$p(t-1)$ had nonfavored ending, in store with favored ending	35.7	22.9	24.7	17.1
$p(t-1)$ from a store without a favored ending	46.2	46.0	33.3	33.1
$p(t-1)$ ended in 0	34.3	12.6	22.6	9.4
$p(t-1)$ ended in 9	34.1	20.5	23.2	14.4
$p(t-1)$ ended in digits 1-8	41.1	40.3	29.8	29.3
$p(t-1)$ ended in 00	28.7	10.8	18.6	8.2
$p(t-1)$ ended in 99	34.6	19.5	23.3	13.0
$p(t-1)$ ended in two digits 01-98	39.5	24.7	27.2	17.8

Notes: All numbers are percentages and reflect monthly frequencies of price changes. A store had a favored ending if more than 50 percent of its prices during the sample period 2002–2013 ended with the same digit. By definition, all prices from a store without a favored ending are classified as not having a favored ending. Column (a) reports results using all observations. Column (b) excludes observations that are exempt from VAT. Column (c) excludes observations that are classified as being part of a sale (temporary price markdown) in either the previous or current month. Column (d) excludes observations that are classified as being part of a sale or are exempt from VAT. Authors' calculations using data from the Israel CBS.

Table 5: Regression Results for the Probability of a Price Change

	(a)	(b)	(c)
$D_{\text{Favored ending},i,t-1}$	-0.025*** (0.004)	-0.018*** (0.003)	-0.021*** (0.004)
$D_{\Delta\text{VAT},t}$	0.152*** (0.022)	0.171*** (0.021)	0.123*** (0.019)
$D_{\text{Favored ending},i,t-1} \times D_{\Delta\text{VAT},t}$	-0.072*** (0.022)	-0.090*** (0.021)	-0.072*** (0.017)
$\ln(p_{i,t-1})$	-0.008 (0.006)	-0.010* (0.006)	0.121*** (0.025)
$D_{\text{Sale},i,t-1}$	0.511*** (0.093)	-- --	0.448*** (0.034)
Time trend	-2.47e-04*** (6.59e-05)	-2.26e-04*** (4.83e-05)	-5.91e-04*** (7.91e-05)
Exclude VAT-exempt observations?	Yes	Yes	No
Exclude sales?	No	Yes	No
Observations	337,646	291,133	554,598
R^2 (within)	0.069	0.010	0.078

Notes: The dependent variable $y_{i,t}=1$ if the price of item i changed at time t and $y_{i,t}=0$ otherwise. Month dummies were included in the regressions but are not reported. All specifications include product \times store fixed effects. Robust standard errors clustered at the store and product levels are reported in parentheses. ***, **, and * denote statistical significance at the 1 percent, 5 percent, and 10 percent levels, respectively. Authors' regression results using data from the Israel CBS.

Table 6: Matched Sample Regression Results for the Probability of a Price Change

	(a)	(b)	(c)	(d)	(e)	(f)
	Treated group	Control group	Treated group	Control group	Treated group	Control group
$D_{\text{Favored ending},i,t-1}$	-0.013 (0.011)	0.014 (0.025)				
$D_{\Delta\text{VAT},t}$	0.175*** (0.028)	0.057* (0.031)	0.117*** (0.013)	0.070 (0.059)	0.266*** (0.029)	0.079 (0.046)
$D_{\text{Favored ending},i,t-1} \times D_{\Delta\text{VAT},t}$	-0.054* (0.029)	0.004 (0.046)				
$\ln(p_{i,t-1})$	0.001 (0.013)	0.220*** (0.054)	-0.006 (0.012)	0.213*** (0.044)	-0.074 (0.051)	0.314*** (0.063)
$D_{\text{Sale},i,t-1}$	0.372*** (0.096)	0.573*** (0.028)	0.685*** (0.015)	0.552*** (0.027)	0.594*** (0.025)	0.557*** (0.049)
Time trend	-1.29e-04 (1.37e-04)	-6.65e-04*** (3.37e-04)	-2.25e-04** (1.08e-04)	-6.48e-04*** (3.68e-04)	2.92e-04 (3.21e-04)	1.07e-04 (8.02e-04)
Price endings from previous month included?	All	All	Favored	Favored	Nonfavored	Nonfavored
Observations	51,325	51,325	33,407	36,807	6,778	14,265
R ² (within)	0.081	0.092	0.102	0.095	0.164	0.100

Notes: The dependent variable $y_{i,t}=1$ if the price of item i changed at time t and $y_{i,t}=0$ otherwise. Month dummies were included in the regressions but are not reported. All specifications include product×store fixed effects. Robust standard errors clustered at the store and product levels are reported in parentheses. ***, **, and * denote statistical significance at the 1 percent, 5 percent, and 10 percent levels, respectively. The treated group is subject to VAT, while the control group consists of fresh fruits and fresh vegetables, which are exempt from VAT. The regressions using nonfavored endings restrict attention to observations from stores with a favored ending. Authors' regression results using data from the Israel CBS.

Table 7: Regression Results for the Absolute Size of Price Changes

	(a)	(b)	(c)
$D_{\text{Favored ending},i,t-1}$	1.071*** (0.305)	1.148*** (0.221)	0.620** (0.290)
$D_{\Delta\text{VAT},t}$	-1.718*** (0.524)	-1.819*** (0.506)	-1.535*** (0.558)
$D_{\text{Favored ending},i,t-1} \times D_{\Delta\text{VAT},t}$	-1.832*** (0.680)	-1.780*** (0.562)	0.296 (0.525)
$\ln(p_{i,t-1})$	-13.580*** (2.555)	-6.314*** (2.280)	-3.987** (1.884)
$D_{\text{Sale},i,t-1}$	1.628*** (0.520)		1.307*** (0.261)
Time trend	0.055*** (0.011)	0.021** (0.009)	0.018** (0.009)
Exclude VAT-exempt observations?	Yes	Yes	No
Exclude sales?	No	Yes	No
Observations	63,362	38,358	192,513
R^2 (within)	0.052	0.021	0.008

Notes: The dependent variable is $|100\Delta\ln(p_{i,t})|$. Month dummies were included in the regressions but are not reported. All specifications include product×store fixed effects. Robust standard errors clustered at the store and product levels are reported in parentheses. ***, **, and * denote statistical significance at the 1 percent, 5 percent, and 10 percent levels, respectively. Authors' regression results using data from the Israel CBS.