

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

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Abstract

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—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, the frequency of price adjustment for nonfavored endings rises 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: favored price endings, zero-ending prices, nine-ending prices, pass-through

JEL codes: E3, L110

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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 as far as the late nineteenth century.² A subset of these literatures links the two concepts and documents 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, including Kashyap (1995), Blinder et al. (1998), Hoffmann and Kurz-Kim (2006), Knotek (2008, 2011, 2019), Levy et al. (2011), Macé (2012), and Hahn and Marenčák (2018). 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 which they used extensively. Taking advantage of frequent changes in the Value Added Tax (VAT) rate during our sample, which affect prices regardless of their ending, we provide novel evidence that these favored price endings played a causal role in generating price rigidity.

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 simplify or complicate transactions

¹ For examples of this large literature across countries and across time, see, e.g., 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 evidence of similar patterns, see, e.g., Holdershaw et al. (1997), Deutsche Bundesbank (2002), Hahn and Marenčák (2018). Jones (1896), Sherman (1928), and Hower (1943) provide historical evidence on price endings.

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%—reveal a preference for a single, “favored” price ending that comprises more than half of all price observations. Favored price endings tend to be either zero or nine and are used heavily: among stores with a favored price ending, 85.6% of their prices used 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 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. 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 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.

Because fresh fruits and fresh vegetables are exempt from VAT in Israel, we are able to 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. We find 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, and there is no statistically

significant increase in their price change probabilities around the VAT rate change itself. 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 a price-ending-specific shock. 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.

Finally, we examine the pass-through of VAT rate changes to aggregate inflation rates. Focusing on the four episodes in which the VAT rate increased by 100 basis points, we group our micro price observations based on whether they had a favored or a nonfavored ending in the period prior to the VAT rate change and calculate the cumulative inflation among these groups following the shock. In the aggregate, changes in the VAT rate are passed through fully and immediately to the prices that had nonfavored endings. By contrast, aggregate inflation among the observations with favored endings rises sluggishly, converging to the inflation rate among the nonfavored prices after about 8 months. These results suggest that favored endings play a key role in generating price rigidity at both the micro 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 digit 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) finds 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 non-cash methods, payment time is independent of the sum paid. Consumers purchasing many items in a single shopping trip out of a large selection face incentives to economize 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 until the first difference is noticed, which in turn 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. Nine-ending prices are 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). Chen et al. (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 has a special place as the absence of value, allowing consumers to process integers and simple fractions faster and more accurately than other numbers. Beyond transaction convenience, Chen et al. (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, the prices of items that have favored price endings will be relatively rigid, regardless of the actual ending digit. 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. Favored price endings can help channel prices to certain levels, thereby helping to explain the prevalence of certain regular “reference” prices as documented in Eichenbaum et al. (2011).

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 nearly all items regardless of their price ending.

To study the relationship between price endings and price rigidity in the presence of VAT changes, we use micro price data collected by the Central Bureau of Statistics (CBS) in Israel on a monthly basis for compiling the consumer price index (CPI) in the period January 2002 through December 2013.³ 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 dataset contains all observations for ELIs that fall into 124 categories of goods. While a small subset of the overall CPI dataset, 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, ranging in price from NIS 0.05 to NIS 33,980.00, with an average price of NIS 161.48 and a median price of NIS 8.00.⁴ Our data come from 1,995 stores (establishments).⁵

To pursue the suggestion from earlier studies that pricing behavior may differ based on characteristics at the point-of-sale, we 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 shopping baskets in these locations. We classify a store as a

³ In January 2014, Israel voted to outlaw the use of non-zero-ending prices, creating a structural break in the dataset. We make use of limited data prior to 2002 for calculating stores' favored price endings and inflation over the 1-year period leading up to the first VAT change in 2002, as described below.

⁴ NIS stands for the New Israeli Shekel. Over our sample period, the exchange rate averaged NIS 4.09 to US\$1.

⁵ We do not have a flag from the CBS indicating whether a price was a sale (i.e., a temporary price markdown) or not. To create a sale flag, we follow the algorithm in Knotek (2019). This algorithm only requires that prices decline in a given month and then subsequently increase in the next month; it does not require that prices return exactly to their previous, pre-sale level.

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.

A unique feature of our dataset compared with U.S. micro price data is the presence of VAT. Given that VAT is common in many countries, especially in Europe, we view our price data as fairly representative of pricing behavior across a number of advanced economies.⁶ VAT has been charged in Israel on goods and services since July 1976. Fresh fruits and fresh vegetables are exempt from VAT, as are items sold in the city of Eilat, facts that we are able to exploit in our analysis. Unlike in some countries, the VAT rate has changed rather frequently in Israel: 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 relatively smaller establishments selling a limited

⁶ During the sample period 2002-2013, year-over-year inflation in the CPI in Israel (including VAT) averaged 2.3%, with a maximum of 6.9% and a minimum of -2.8%. Over the same period, the comparable figures for the U.S. CPI were 2.3% for average year-over-year inflation, with a maximum of 5.6% and a minimum of -2.1%.

number of goods (convenience-like locations) setting zero-ending prices, while relatively larger establishments where consumers purchase a larger basket of goods (superstore-like locations) have incentives to set nine-ending prices. Using a hand-collected sample of prices from a single U.S. city, Knotek (2011) documents support for this conjecture.

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% of all observations end in the digit zero. For superstore-like locations, the vast majority of prices—66.4%—have a final digit of nine. Thus, our first finding is that we document broad support using CPI micro data for the idea that price endings correlate with characteristics at the point-of-sale, in line with Knotek (2011).

However, a fair number of prices within superstore-like locations did not have nine endings during our sample, 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 said to have a favored ending if more than 50% 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 in convenience-like locations and superstore-like locations. As with the distribution of prices themselves in these location-types, zero is clearly the most common favored ending among convenience-like locations, used by 88.9% of stores. Among superstore-like locations, nine is the most common favored ending for 55.7% of stores, followed by zero with 33.5% of stores.

Table 3 further explores the incidence of favored price endings. Our second key finding is that most stores have a favored ending and use that favored ending extensively. In our sample, 94.0% 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% 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. As such, we focus on this revealed preference in our empirical exercises.

IV Favored Endings and Implications for Price Rigidity

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. However, establishing the direction of causality from favored endings to price rigidity requires exogenous shocks that affect prices regardless of their ending. Otherwise, firms could use favored endings for goods which they anticipate will subject to fewer shocks and hence change less often, while the goods that are expected to be subject to more shocks and change price more frequently could be set to nonfavored endings, and correlation would not signify causation; see Levy et al. (2011).

In general, observations with a favored price ending in the previous period are more rigid than observations that had a nonfavored ending. Table 4 presents summary statistics showing

the relationship between price endings and price rigidity in our dataset.⁷ Observations that had a favored price ending are more rigid 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, would not be classified as having a favored ending). Similar patterns hold whether we look at all observations (column (a)), if we exclude VAT-exempt prices (column (b)), if we exclude sales (column (c)), or if we exclude sales and VAT-exempt prices (column (d)). Given the prevalence of zero-ending and nine-ending prices as the favored endings as documented earlier, the table shows that favored endings are correlated with price rigidity in a similar fashion to zero- and nine-ending prices, as has been documented elsewhere (e.g., Knotek 2008, Levy et al. 2011, Knotek 2019). While we focus only on the final digit of the price in this paper, the same general 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 relatively more rigid than prices ending in the digits 01 through 98.⁸

To further explore the causal role of favored endings in generating price rigidity, we focus on pricing behavior around changes in the VAT rate, which affect nearly all goods regardless of their price ending, other than VAT-exempt goods (i.e., fresh fruits, fresh vegetables, and items sold in Eilat). 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

⁷ 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% of all of its price observations have the same ending over the entire sample 2002–2013. Assuming that a store either has a favored ending or not throughout the sample simplifies the summary statistics. However, implicitly this process involves using future price information to classify whether a price had 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.

⁸ 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.

subtracting the normal frequency of price adjustments for that series outside of months with a change in the VAT rate. Panel (a) shows that price setters do not front-run the changes in the VAT rate: the frequency of adjustment is basically normal in the months leading up to the change in the VAT, spikes in the month of the VAT rate change, and then falls off sharply 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% per month (see Table 4), this suggests that prices are 55% 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%. By contrast, among items that are exempt from VAT, there is very little response to the frequency of adjustment either before or after the change in the VAT rate.⁹

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 period 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 period jumps 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.¹⁰ Within stores that

⁹ Among items exempt from VAT, which are mostly fresh fruits and fresh vegetables, the average frequency of a price change is 60% 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.

¹⁰ In the latter group, all prices would be defined as having a nonfavored ending.

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). 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 a favored ending which could help to rationalize this behavior, 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 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; inter alia, see 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 period 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 period 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 period 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 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 period, $D_{\text{Sale},i,t-1}$, 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.¹¹

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 period reduces the probability of adjustment by 2.5 percentage points. Consistent with state-dependent price adjustment models, firms respond to VAT changes with a higher-than-normal frequency of price changes. 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 period’s price ending. On average, a change in the VAT rate is associated with a 15.1 percentage point increase in the probability of a price change for goods that had a nonfavored ending in the previous period. While there is generally a lower

¹¹ 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. While the estimated coefficients on $D_{\Delta\text{VAT},t}$ and $D_{\text{Favored ending},i,t-1} \times D_{\Delta\text{VAT},t}$ were effectively unchanged based on different fixed effects specifications, 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 when we implemented only product fixed effects; results from the other fixed effects specifications were generally similar to the baseline results reported.

average frequency of a price change associated with having a favored ending in the previous period, a change in the VAT rate is associated with only a 7.9 percentage point increase in the probability of a price change for goods that had a favored ending in the previous period. Thus, the increase in the frequency of price adjustment for goods with nonfavored endings is 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. We obtain similar results if we also exclude the observations that were part of a sale around the VAT rate change (column (b)) or if we include all observations, whether they were subject to VAT or not (column (c)). Given that changes in the VAT rate are exogenous shocks that affect all goods equally regardless of their price ending, and yet the frequency of price adjustments in response to VAT rate changes is far more sensitive for prices with nonfavored endings in the previous period as opposed to prices with favored endings in the previous period, these results suggest that favored price endings are playing a causal role in generating the price rigidity that 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.¹² To this end, we create a propensity score matched

¹² 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 items sold in Eilat are exempt from VAT, the small number of observations from this region produces poor matches and repeatedly samples the same observations. Hence, we prefer to use fresh fruits and fresh vegetables as our control group.

sample of goods subject to VAT (the treated group) and goods not subject to VAT (the control group) and run comparable regressions on the two samples. For each date τ that experienced a 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 $v_{j,\tau}$.^{13,14} 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 and 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 18.6 percentage points for the items subject to

¹³ 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 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, and we also drop observations if the matched good in the control group is used more than 100 times.

¹⁴ 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 vegetables with nonfavored endings to conduct statistical inference. Qualitatively, other results were similar to those reported in Table 6.

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 not subject to VAT in column (b), a change in the VAT rate does not have a statistically significant impact on the frequency of price adjustment.

Columns (c) and (d) run the same regressions on the subset of the two matched samples in which the previous price had a favored ending, while columns (e) and (f) do likewise on the subset 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.¹⁵ Among goods subject to VAT, the combination of a VAT rate change and a nonfavored price ending in the previous period in a store that had a favored ending is associated with a 26.0 percentage point increase in the probability of adjusting its price, while a good with a favored ending saw the probability of adjusting its price increase by just 11.7 percentage points. Thus, the response of the frequency of adjustment following a change in the VAT rate is at least twice as large for nonfavored endings as it is for favored endings. By contrast, 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, and there is no statistically significant increase in their price change probabilities around the VAT rate change itself. 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.

¹⁵ These subanalyses 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. Our specification takes the form:

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

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 size of the price change is larger than it is for nonfavored endings, by 0.6 to 1.1 percentage points.¹⁶ This result is largely 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 at the same time are smaller than normal in absolute terms for prices that had nonfavored endings. The absolute size of price changes are smaller for favored endings than for nonfavored endings at VAT rate changes after we exclude VAT-exempt goods.

Abstracting from the issue of favored versus nonfavored endings, the general findings of smaller and more frequent price changes when the VAT rate changes can be reconciled within multiple theoretical frameworks. In an environment of imperfect information (e.g., Sims 2003),

¹⁶ For the sake of comparison, the mean absolute size of price changes in our sample is 22.0%, and the mean absolute size of nonsale price changes is 18.8%.

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 quite large in percentage terms, adjusting prices simply 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, due to economies of scope in price changes (e.g., Midrigan 2011) or lower customer antagonization costs because consumers are aware of the VAT rate changes (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 Knotek (2019) instead of time-dependent models such as Hahn and Marenčák (2018).

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 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. 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, in June 2002, July 2009, September 2012, and June 2013, and we examine pricing behavior in the wake of these four comparable episodes. Denoting the timing of these four VAT rate changes generically as 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 ($NF|F$). For each observation i we calculate cumulative inflation from time $\tau-1$ through time $\tau+k$, $\pi_{i,\tau+k} = 100 \ln(p_{i,\tau+k} / p_{i,\tau-1})$.

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 period 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.

These cumulative inflation rates allow us to compare pass-through of the VAT shocks based on price endings in the period 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 for the favored endings to the nonfavored endings in stores with a favored ending,

$(1/4) \sum_{j=1}^4 \pi_{j,\tau+k}^F / \pi_{j,\tau+k}^{NF|F}$. In the month of the VAT rate change, the pass-through rate among the favored endings is one-fourth of the pass-through rate among the nonfavored endings based on our cumulative inflation measures. Pass-through among the favored endings only catches up to

the nonfavored endings eight months after the VAT rate changed.¹⁷ 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 are generating considerable price rigidity compared with their nonfavored-ending counterparts.

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 , we calculate the inflation trend as the year-over-year inflation rate for the twelve preceding months:

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

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 period after the shock, $\pi_{j,\tau+k}^g - [(1+k)/12]\bar{\pi}_j$. Figure 2(b) shows the average of these detrended cumulative inflation measures across the four VAT rate changes. On impact, detrended cumulative inflation among the nonfavored endings increases more than 1 percentage point. Because the VAT rate increased by 100 basis points in each episode, this result implies that on net the entire 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 eventually greater than one-for-one. By contrast, pass-through is much smaller initially for prices that had a favored ending in the period before the VAT changed and only builds slowly over time. Given that prices with a favored

¹⁷ We only look at pass-through for 8 months after the VAT rate changed because the VAT rate decreased in January 2010, 6 months after the July 2009 change; the June 2013 change was 9 months after the September 2012 change; and our sample ends in December 2013, 6 months after the June 2013 change, leaving us with only a single +100 basis point VAT rate change with uncontaminated observations extending more 8 months past the VAT rate change. When calculating averages, we omit statistics from the July 2009 and June 2013 changes after 6 months.

ending comprise a majority of all prices in our dataset, the sluggish pass-through of the VAT rate change to all prices in our dataset 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.

VII Conclusion

This paper looks at the relationships 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 which they used extensively: 94.0% of firms had a single, favored price ending that comprised more than half of all price observations, and 85.6% of their prices used the favored ending digit.

Based on frequent changes in the VAT rate during our sample, 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 changes is due to favored endings; changes in the VAT rate are passed through fully and immediately to nonfavored endings.

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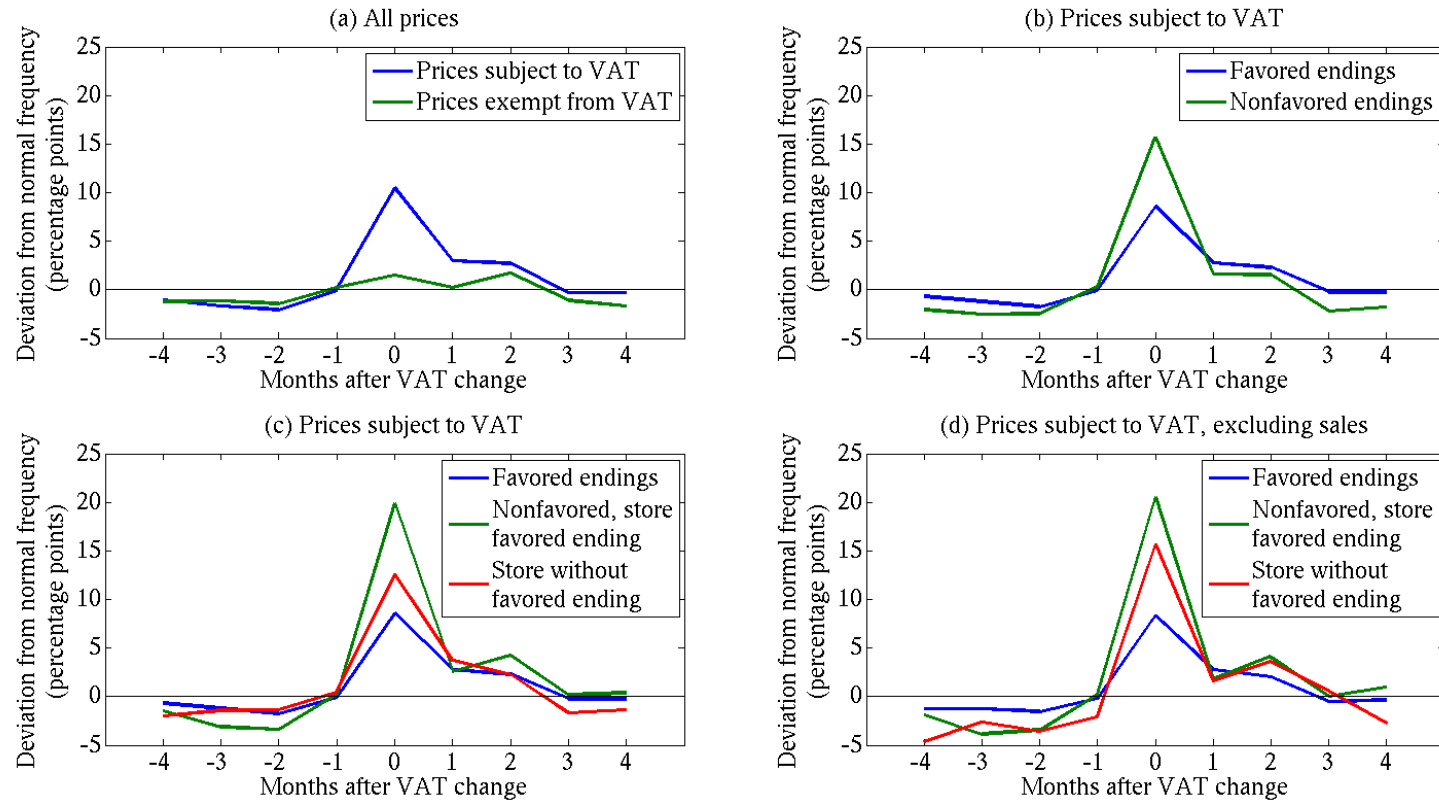
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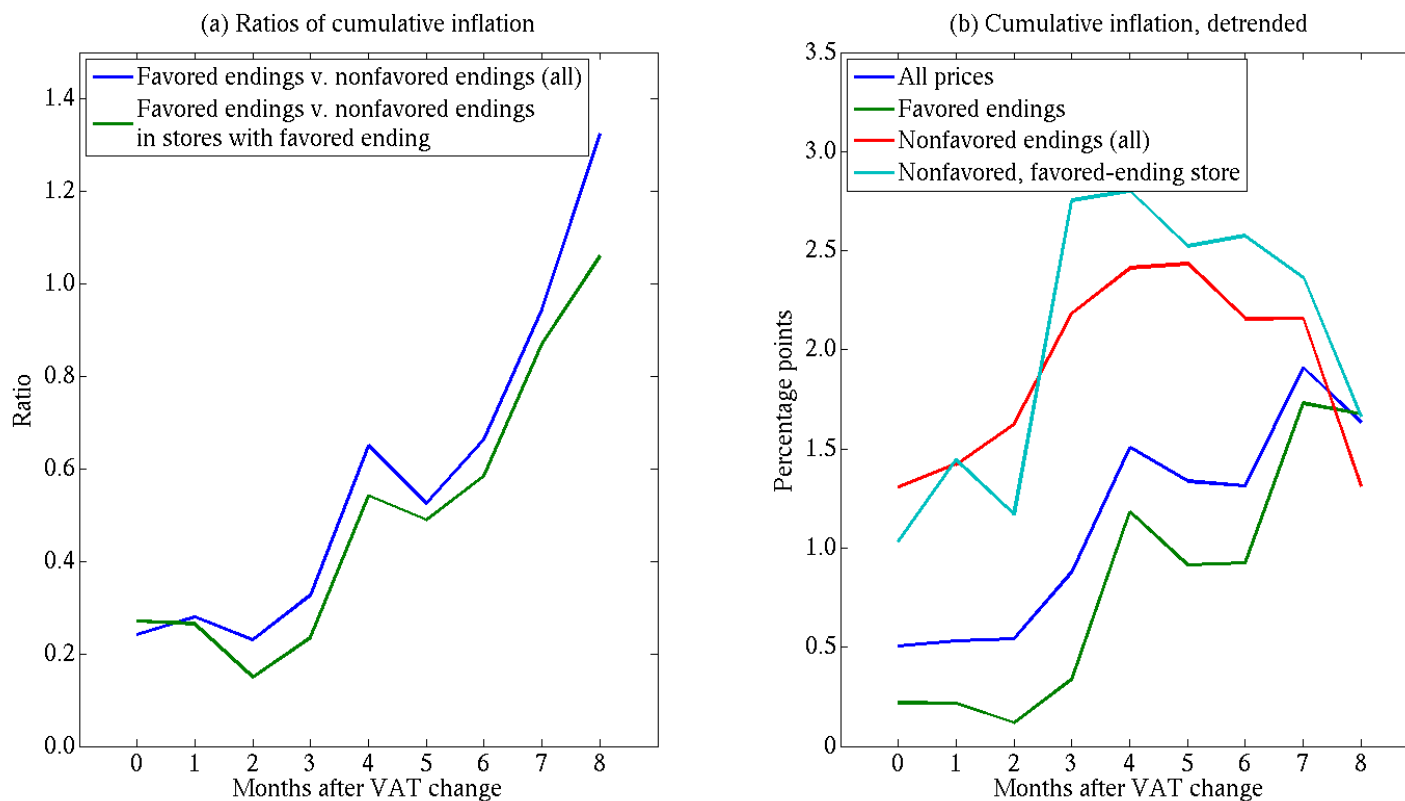
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Figure 1: Frequency of Price Changes Around VAT Changes



Notes: Figures show the frequency of price changes relative to their normal frequency, which is defined as the average frequency during the sample excluding months in which the VAT changed. Favored and nonfavored endings are determined by price endings in the previous period. 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 Changes



Notes: The figures show cumulative inflation from the month before the VAT 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 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. Convenience-like locations comprise convenience stores, small groceries, open market stalls, or specialty stores. Superstore-like locations comprise supermarkets, chain stores, department stores, or drugstores. A store had a favored ending if more than 50% of its prices ended with the same digit.

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% 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.

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. A store had a favored ending if more than 50% 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 (b) excludes VAT-exempt prices. Column (c) excludes observations classified as being part of a sale (temporary price markdown) in either the previous or current period. Column (d) excludes VAT-exempt prices and sales.

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)
$D_{\Delta\text{VAT},t}$	0.151*** (0.022)	0.170*** (0.021)	0.122*** (0.019)
$D_{\text{Favored ending},i,t-1} \times D_{\Delta\text{VAT},t}$	-0.072*** (0.022)	-0.089*** (0.021)	-0.072*** (0.017)
$\ln(p_{i,t-1})$	-0.008 (0.006)	-0.011* (0.006)	0.121*** (0.025)
$D_{\text{Sale},i,t-1}$	0.511*** (0.094)		0.448*** (0.034)
Time trend	-2.47e-04*** (6.60e-05)	-2.27e-04*** (4.83e-05)	-5.91e-04*** (7.91e-05)
Exclude VAT-exempt observations	Yes	Yes	No
Exclude sales	No	Yes	No
Observations	337,332	290,849	554,284
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%, 5%, and 10% levels, respectively.

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
$D_{\text{Favored ending},i,t-1}$	-0.009 (0.011)	0.017 (0.025)				
$D_{\Delta\text{VAT},t}$	0.186*** (0.027)	0.053 (0.033)	0.117*** (0.013)	0.080 (0.048)	0.260*** (0.029)	0.076 (0.046)
$D_{\text{Favored ending},i,t-1} \times D_{\Delta\text{VAT},t}$	-0.065** (0.028)	0.015 (0.042)				
$\ln(p_{i,t-1})$	0.003 (0.013)	0.224*** (0.062)	-0.004 (0.013)	0.211*** (0.055)	-0.066 (0.050)	0.338*** (0.056)
$D_{\text{Sale},i,t-1}$	0.400*** (0.098)	0.575*** (0.033)	0.683*** (0.015)	0.557*** (0.036)	0.600*** (0.025)	0.584*** (0.040)
Time trend	-1.61e-04 (1.37e-04)	-8.88e-04*** (2.86e-04)	-2.23e-04** (1.08e-04)	-1.02e-03*** (2.81e-04)	2.60e-04 (3.39e-04)	1.34e-04 (7.07e-04)
Included price endings from previous period	All	All	Favored	Favored	Nonfavored	Nonfavored
Observations	50,025	50,025	32,935	36,375	6,509	13,391
R ² (within)	0.085	0.100	0.101	0.109	0.157	0.109

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%, 5%, and 10% levels, respectively. The treated group is subject to VAT, while the control group consists of fresh fruits and fresh vegetables that are exempt from VAT. The regressions using nonfavored endings restrict attention to observations from stores with a favored ending.

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

	(a)	(b)	(c)
$D_{\text{Favored ending},i,t-1}$	0.620** (0.291)	1.072*** (0.306)	1.146*** (0.221)
$D_{\Delta\text{VAT},t}$	-1.537*** (0.558)	-1.723*** (0.525)	-1.819*** (0.507)
$D_{\text{Favored ending},i,t-1} \times D_{\Delta\text{VAT},t}$	0.301 (0.526)	-1.827*** (0.682)	-1.780*** (0.563)
$\ln(p_{i,t-1})$	-3.987** (1.885)	-13.590*** (2.556)	-6.324*** (2.280)
$D_{\text{Sale},i,t-1}$	1.307*** (0.261)	1.627*** (0.520)	
Time trend	0.018** (0.009)	0.055*** (0.011)	0.021** (0.009)
Exclude VAT-exempt observations	No	Yes	Yes
Exclude sales	No	No	Yes
Observations	192,442	63,291	38,308
R^2 (within)	0.008	0.052	0.021

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%, 5%, and 10% levels, respectively.