# Firms as Model-Free Decision Makers – Evidence from a Reform

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**Abstract**

Economics typically assumes that firms use a model of the environment to choose optimal actions. I use a reform that restricted the set of admissible prices to test this assumption. Specifically, a reform in Israel limited prices to end with X0 as the cents digits (e.g. 2.90 but not 2.99). When consumers are left-digit biased, demand drops at round numbers, hence optimal pricing prescribes bunching at just-below prices and avoiding round prices. Israeli supermarket chains respond to left-digit bias in the long-run and act as if they know this demand structure, setting just-below prices for 45% of prices. This response is consistent with awareness of the bias; However, it implies underestimation of its magnitude since estimated demand should lead to even higher shares of 99-endings. Further, following the reform, 20% of prices were round (e.g. 3.00). If firms were model-based their response to the reform would have been to update immediately according to their beliefs and avoid round prices; However, firms set clearly dominated prices for almost a year, re-learning something they seemed to know. Further, price changes at the product-store level were that 00-endings changed into 90-endings but 90-endings were absorbing. Together these findings suggest that firms learn in a model-free way, which may lead them to be model-free decision makers. Model-free incomplete learning can explain how firms behave sub-optimally in a persistent way and challenges counterfactual exercises that rely on the assumption of model-based optimization.

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# 1 Introduction

Learning impacts decision-making. If there is full learning, *how* firms learn does not matter. But since learning can be incomplete, how learning occurs is important as it affects long-run decision-making and our understanding of the firm.

A cornerstone assumption of economics and marketing is model-based decision-making and learning. The assumption states that firms use a model to choose the best action, and overwhelmingly further assumes that firms choose optimally (from neoclassical economics (Muth (1961)) to behavioral IO (Heidhues and Kőszegi, 2018; Spiegler, 2011)). A growing body of evidence shows that firms are not always optimizing (Cho and Rust 2010; DellaVigna and Gentzkow 2019; Dube et al. 2017; Goldfarb and Xiao 2011, 2019; Hanna et al. 2014; Hortaçsu et al. 2019; Hortacsu et al. 2021; Huang 2021; List et al. 2021; Rao and Simonov 2019; Shapiro et al. forthcoming; Strulov-Shlain 2019). That firms do not optimize creates some dissonance - how can firms be wrong? The literature provides many answers, that to be wrong firms must have the wrong model, wrong beliefs, or that some frictions arise. In contrast, firms might be model-free decision makers (Arthur, 1991; Cross, 1973). "Model-free" builds on the idea of reinforcement learning (Sutton and Barto, 2018) — taking an action informs the firm about the value of that action but not about the value of other actions. Therefore, incomplete or limited model-free learning leads naturally to sustained sub-optimization since the value of actions not taken is not being learned.

This paper uses data from a reform to test the assumption that firms use a model to make decisions. I find that firms do not use a model, nor even with wrong beliefs, and propose the alternative of model-free decision making to reconcile the patterns in the data.

To test if firms decision making is consistent with model-based maximization I use data from a reform that changed the admissible actions a firm could take. Specifically, I examine the pricing behavior of large supermarket chains in Israel. Model-based and model-free decision making differ in their predictions about how firms should react to the reform: under model-based optimization, new maximizing actions are predicted from the pre-reform beliefs and enacted immediately with no learning period; under model-free, a learning period in which "good" actions are repeated and "bad" actions are shied away from, might be needed if original learning was incomplete.

In 2013, the Israeli government banned pricing in non-existing coins—the equivalents of the U.S. Penny and Nickle—forcing companies to price at .X0-ending prices. For example, 2.96 is banned, but 2.90 or 3.10 is allowed. Before the reform the modal price-ending in Israeli supermarkets was 99 (e.g. 2.99) with ˜45% of prices, and there were almost no 00-ending prices. This price-ending distribution *before* the reform is consistent with model-based maximization, specifically with a model of pricing to left-digit biased demand[1] — a demand structure driven by consumers with distorted price perception leading to discontinuous drops in demand at round prices.[2] When this is the demand structure, prices that absent a bias would have ended with low-endings (e.g., 5.00 or 5.29) are better priced at the lower 99-ending price (4.99 in this case). When 99-ending—previously a profit-maximizing price ending for many costs and elasticities—is banned and the highest just-below price-ending is 90, still, pricing predictions are that prices should not end with 00.[3] Indeed, demand analysis using scanner data shows that changing a product's price from, for example, 4.99 to 5.00 versus from 4.99 to 4.90 was accompanied with excess demand loss of 5%-9%.

However, on the reform enactment date 20% of prices ended with 00. The use of 00-ending prices is inconsistent both with the demand structure and, more importantly, with what firms' pre-reform pricing implied about *their own beliefs* about the demand structure. Over the course of the following years, the share of 00-endings had declined again. For some chains a discrete reduction in the share of 00-ending prices occurs about 8 months after the reform. The way these price-endings had evolved is that products whose price-ending changed from 99 to 90 remain at 90, while products whose price changed into 00-ending

[1]That supermarkets realize that they face left-digit biased demand, at least to some degree, was also found in supermarkets in the U.S. (Strulov-Shlain, 2019).

[2]For example, if 4.99 is perceived as much lower then 5.00, demand discontinuously drops at 5.00.

[3]There is indeed a tradeoff due to the reform: a larger revenue loss from pricing below the round price threshold (e.g., at 4.90 vs. 4.99), and the discontinuity is effectively "smoother." Yet, left-digit bias that is enough to justify many 99-ending prices also leads to prices being either 90-ending or higher, but not at 00.

quickly switch to 90-ending prices.

Chains pricing behavior in the long-run, either before the reform or long after, may be consistent with model-based pricing albeit with wrong beliefs (see also Strulov-Shlain, 2019). The fact that chains used 99-ending before the reform was likely because they understood the value of 99 versus 00; further, firms did not use enough 99-endings before the reform (or 90-endings after) is likely because they only learned it partially and underestimated left-digit bias. However, firms' pricing behavior in the year following the reform suggests that this long-run suboptimal behavior is likely driven by partial model-free learning. First, it is inconsistent with model-based decision making, since even potentially wrong beliefs that are consistent with pricing before the reform should have led to no 00-ending prices. Further, that chains used many 00-ending prices after the reform is because they did not understand that the relative value of 99 to 00 informs them about the relative value of 90 to 00; that is, like in model-free learning, knowing the value of one action does not extrapolate to the value of others. Finally, the paths of price-ending updates, of a product in a store staying at 90 but updating away from 00, is a textbook example of local reinforcement learning path.

This paper is the first to provide empirical evidence for firms and model-free decision makers, an idea that was suggested before theoretically (Cross, 1973). There is ample support for reinforcement learning as the way humans learn in the neuroscience and psychology literature (Daw et al., 2011; Thorndike, 1911). Reinforcement learning had been studied, mostly in the lab, for individuals (Camerer, 2011). Since firms are consisted of people, it seems likely to extend that idea from people to firms. Further, reinforcement learning is an incredibly useful way to solve problems and a central tool of AI (Dayan, 1992; Sutton and Barto, 2018; Watkins and Dayan, 1992); we have evidence for how when these methods are explicitly employed by firms they affect market outcomes (Assad et al., 2020, 2021; Calvano et al., 2020). This paper provides evidence that model-free learning happens at the firm level and leads to long-run model-free decision making. Therefore, the implications of that description of the firms should be further studied.

This paper also relates to the literature on firms' learning (see review by Aguirregabiria and Jeon (2020)).. Unlike in other settings, firms did not have to learn a new demand structure, most notably as in the papers by Doraszelski et al. (2018) and Huang et al. (2018).. In Doraszelski et al. (2018) electricity providers ultimate bidding behavior is consistent with optimization, and their learning is studied as learning about the parameters of the model. Further, there is no sense in which learning type (model-based or model-free) can be identified, because we can not know firms' beliefs before the start. Yet, the authors find it hard to explain firms' pricing in the first year and half of the data. Huang et al. (2018) also remain in the realm of model-based optimization, studying retailers setting prices of liquor in Washington. Yet, they show that firms put a large weight on recent outcomes initially, a finding that is also consistent with reinforcement learning. This paper proposes to use the lens of model-free learning in such exercises.

Finally, the paper provides another example for a case in which firms do not optimize, both in the long-run and in the short-run, and contributes an explanation for how can that happen. Recent literature explores other explanations. When firms are found to be mis-optimizing it is explained by adding frictions (DellaVigna and Gentzkow (2019); Hortacsu et al. (2021); Huang (2021)), principal-agent incentives misalignment (Rao and Simonov (2019); Shapiro et al. (forthcoming)), attention costs (Goldfarb and Xiao (2011, 2019)), or wrong beliefs (Aguirregabiria and Jeon (2020); Hortaçsu and Puller (2008); Strulov-Shlain (2019); Xie (2018)).

Model-free learning implies that even if firms converge to the optimal behavior in the long run, they are sensitive to small changes in the environment. Therefore counterfactual exercises that assume model-based optimization — such as mergers implications, inferring markups, or studying the effects of regulation — should be done with this alternative description of the firm in mind.

The paper proceeds as follows: Section 2 describes the setting, the data, and left-digit biased demand and its effects on pricing. Section 3 then shows how prices evolve and the demand response. Section 4 interprets the data through the lenses of model-based and model-free learning. Section 5 concludes with a discussion of the implication of these findings on future research.

# 2 Setting and Data

## 2.1 Reform details

On October 17, 2013, the Israeli Ministry of Economics announced that starting January 1, 2014, it will start to enforce an existing law banning pricing in non-existing coins.[4] Israel abolished the 1-Agora and 5-Agorot coins (the Shekel is consisted of 100 Agorot) in 1991 and 2008, respectively.[5] Yet prior to 2014, the majority of prices ended in non 10-Agorot ("dime"), most commonly with 9- and 5-endings. Therefore, most prices had to change due to the reform. This policy came about because consumers felt that they were being tricked by non-existing prices, (e.g., a store posts a price of 4.99 but the actual price is 5.00; prices are VAT inclusive) as they pay the total shopping basket price when paying with a card or the price rounded to the nearest dime when paying cash.

## 2.2 Data

The effects of the reform are manifested through the dynamics of price setting and their effects on demand. I therefore use data on prices and purchases in supermarkets. I employ three data sources—two of them are shelf prices before and after the policy changes, and the third one is scanner data containing revenue and quantities. In all datasets, the unit of observation is a product in a store in a period. I first describe the pricing data, and then the scanner data.

In the pricing data, the main variables are the shelf price as displayed in a store, and

[4] except for "continuous" products—gas, water, and electricity

[5] https://web.archive.org/web/20201127080757/https://www.boi.org.il/en/Currency/CurrentCurrencySeries/Pages/Defa

identifiers of the product, the store, and the chain. The main pricing data was collected by a consumer advocacy group, "Israel Consumer Council" (ICC). Its employees went to supermarket stores with a varying list of products every 2–4 weeks between early 2013 and early 2015 and wrote down the prices of a set of predetermined products. A "product" can be thought of as an UPC—of unique producer, weight and size. ICC collect and post the data on their website to inform consumers which of the nearby stores sells the cheapest bundle in a given week. Therefore stores and chains are identified by their name and address. The other pricing data, from the Central Bureau of Statistics (CBS), is similar but a "product" in the data is a product-type—for example, a carton of omega3-enriched brown medium eggs (any brand). Data is a balanced panel but stores identities are anonymized without chain identifiers, and prices are sampled monthly. Further details are in Appendix Section A.

The pricing data is not a balanced panel due to missing observations and changes in the basket of goods sampled. Therefore, I focus on a consistent subset of products and impute missing prices as described in Appendix Section A. For example, for the main analysis I require that the price of a product in a store is sampled at least once in each reform period.

Table 1 shows the differences in the number of products, stores, and prices between the samples. The main ICC sample consists of 21 products in 143 stores, and the main CBS sample consists of 171 product-types in 99 stores. The share of 99-ending prices is 45%–47% in the pre-period.

Table 1: Summary Statistics of Prices Data

| | ICC | | CBS | | StoreNext | |
|---|---|---|---|---|---|---|
| Variable | Original | Main | Original | Main | Original | Main |
| No. of Products | 67 | 21 | 233 | 171 | 19 | 17 |
| No. of Stores | 659 | 143 | 155 | 99 | "580" | "573" |
| No. of Chains | 38 | 20 | – | – | "23" | "23" |
| Sampling Periods | 41 | 40 | 48 | 48 | 156 | 156 |
| First Observation | 02/2013 | 03/2013 | 01/2012 | 01/2012 | 01/2013 | 01/2013 |
| Last Observation | 02/2015 | 02/2015 | 12/2015 | 12/2015 | 12/2015 | 12/2015 |
| | | | | | | |
| Mean Weekly Units Sold | – | – | – | – | 56.69 | 66.09 |
| Mean Price | 12.99 | 6.95 | 10.48 | 10.04 | 12.36 | 11.06 |
| Price SD | 1.59 | 0.89 | 1.88 | 1.3 | 6.2 | 4.56 |
| Pre 99 Share | 0.38 | 0.45 | 0.36 | 0.47 | 0.13 | 0.23 |
| Post 90 Share | 0.52 | 0.59 | 0.39 | 0.48 | 0.29 | 0.39 |
| Post 00 Share | 0.12 | 0.10 | 0.09 | 0.12 | 0.15 | 0.15 |
| | | | | | | |
| Observations | 472,572 | 116,995 | 292,471 | 156,336 | 846,392 | 185,775 |

The table shows summary statistics of the three datasets used in the paper, in their raw and cleaned form. ICC, Israeli Consumer Council, collected a panel of prices at the item-store-biweek level. CBS, Israel's Central Bureau of Statistics, collected prices for the consumer price index at the "product type"-store-month level. StoreNext, is a market research firm, collecting scanner data from retailers including quantities and revenues at the store-week-item level. These data are a merge of two datasets, and thus the exact number of stores is bounded between 295 and 580, but is more likely to be much closer to 295, and the number of chains closer to 12 than to 23.

Finally, a third dataset is a mix of daily and weekly scanner data, in which the main variables are the number of units sold and revenue for each UPC in a store. Like in ICC data, each store and chain has a unique identifier, though anonymous. Data are provided by StoreNext Ltd, a market research firm collecting transaction level data from store registers and aggregating it. The unit of observation is a product (UPC) in a store in a week.[6] From an initial sample of 10 UPCs from a balanced set of 295 supermarket stores over 3 years (2013--2015), 6 products can be used for the analysis.[7] I merge this data with another sample of StoreNext's *weekly* data for another 13 products from 287 national supermarket stores

[6]Because of the mix of daily and weekly readings, I aggregate the data at the weekly level.

[7]One of the UPCs has a single price throughout the sample (canned corn); another one is a product that was not available before the reform (400g Hummus). Two others are cottage cheeses that follow uncommon pricing behavior (see Hendel et al., 2017).

covering 2013 to 2015.[8]

Scanner data is not ideal for precise price measurements because of the averaging of possibly different prices paid in a time period (see Einav et al., 2010; Strulov-Shlain, 2019). StoreNext's data is the only research-available scanner data from Israel but has some excess measurement errors, requiring careful data cleaning (elaborated in Appendix A).

The main scanner dataset, described in column "StoreNext Main" in Table 1, includes 185k observations. Compared to the price data, there are fewer 99-ending prices (23% vs. 43%) and more 00-ending prices post reform (15% vs. 11%). In some analyses, I also use a "long price spells" sample. A same price spell is when the average price of the product in a store is identical over consecutive weeks. In these cases I restrict the sample to be same-price spells of at least 2 weeks, aiming to capture non-promotional product-weeks.

Together, these data allow to explore the price evolution of products in supermarket stores at the aggregate, use the panel structure of the data, and examine the effects of price endings on demand.

## 2.3 Pricing under left-digit bias

Many prices end with 99 before the reform and with 90 after the reform, likely as a response to left-digit biased demand. This section describes the construct of left-digit bias and what it implies for pricing.

Left-digit bias is the tendency of consumers to put excess weight on the left-most digits of a price. Under left-digit bias, the perceived difference between prices is bigger than actual if they have different left-most digits and smaller if they share the left digits. In turn, demand will be downward sloping with discontinuous drops when the left-digit changes. The key implication of such a demand structure—downward sloping with discontinuities—is that prices to the right of the discontinuities should be avoided.

[8]While the set of stores is likely largely overlapping, I cannot match stores or chains between these two sources since store identifiers are unique to each data set.

The literature broadly supports this explanation, finding left-digit bias in supermarket pricing and other settings. Early literature on the reasons why firms use psychological prices reached mixed conclusions about their profitability (Basu, 1997; Gedenk and Sattler, 1999; Ginzberg, 1936; see Stiving and Winer, 1997 for a brief review) and suggested various explanations for their existence (e.g., Basu, 1997, 2006; Bizer and Schindler, 2005; Anderson and Simester, 2003). Some experimental and empirical analyses were conducted to see whether and how 9 and 99 price endings affect demand in practice, and found mixed results as well (Anderson and Simester, 2003; Bizer and Schindler, 2005; Sokolova et al., 2020; Thomas and Morwitz, 2005). Strulov-Shlain (2019) provides large scale evidence from 25 US supermarket chains and thousands of products supporting high levels of left-digit biased demand. Left-digit bias was previously established in non-price settings (most notably by Lacetera et al. (2012); Busse et al. (2013)) and recently extended to other demand settings by Hilger (2018) and List et al. (2021), providing further support for its effects on demand.

To study the implications on pricing requires incorporating left-digit bias into demand and solving for optimal pricing. To solve for optimal pricing, I am using the model from Strulov-Shlain (2019), in which biased consumers meet monopolistic firms. In the model, a price is perceived in a distorted way, as a mix with weight $1-\theta$ on the true price, and $\theta$ on the price with a fixed focal price ending $\Delta$.[9] It causes the perceived price to change discontinuously when the left-most digit changes, and also be less sensitive to price changes that do not involve left-most digit changes. For example, with left-digit bias $\theta = 0.2$, a 1 cent difference between 4.99 and 5.00 is perceived as a 20.8 cents gap, while the difference between 5.00 and 5.01 is only a 0.8 cent difference. Formally, if the true price is $p$, the perceived price is

$$\hat{p} = \hat{p}\left(p; \theta\right) = \left(1-\theta\right)p + \theta\left(\lfloor p \rfloor + \Delta\right) \tag{1}$$

where $\lfloor p \rfloor$ is the floor of the price. Demand is a function of the perceived prices, while

[9] $\Delta$ is in a sense a demand level-shifter, and bears almost no influence on pricing.

consumers pay the true price when purchasing an item. Assume a demand curve with constant elasticity. The demand curve is then

$$D(p;\theta) = A\hat{p}^{\epsilon} = A\left((1-\theta)\,p + \theta\left(\lfloor p \rfloor + \Delta\right)\right)^{\epsilon} \tag{2}$$

which means that gross profits are of the form

$$\Pi(p;\theta) = D(p;\theta)(p-c)$$

where the assumption of a unit cost $c$ is an approximation for supermarket pricing. It can be shown that prices under this demand structure bunch just below round numbers (denominate those just-below prices with $q$), with ranges of missing prices with low price-endings., i.e., excess mass below round prices is mainly drawn from higher prices. As shown by Strulov-Shlain (2019), predicted ranges of missing prices are pinned down by the parameters of the problem.

Given the pricing function, we can focus on a key outcome—the *Next-Lowest Price*—which defines the range of missing prices given the parameters of the problem. The *Next-Lowest Price* $P$ is the lowest price that should be charged that is greater than a just-below price $q$. The just-below price, $q$, ends with 99 before the reform and with 90 after. The Next-Lowest Price satisfies two conditions – it maximizes profits for some cost $c$, and for that cost profits are equal at $P$ and at $q$. It can be shown that it is the solution of the following implicit equation:

$$P + \frac{(1-\theta)P + \theta\left(\lfloor P \rfloor + \Delta\right)}{\epsilon(1-\theta)} - \frac{\left((1-\theta)P + \theta\left(\lfloor P \rfloor + \Delta\right)\right)^{\epsilon} P - \left((1-\theta)q + \theta\left(\lfloor q \rfloor + \Delta\right)\right)^{\epsilon} q}{\left((1-\theta)P + \theta\left(\lfloor P \rfloor + \Delta\right)\right)^{\epsilon} - \left((1-\theta)q + \theta\left(\lfloor q \rfloor + \Delta\right)\right)^{\epsilon}} = 0 \tag{3}$$

Proposition 2 in Strulov-Shlain (2019) shows that given an elasticity $\epsilon < -1$, larger left-digit bias $\theta$ increases $P$. Therefore, since for $\theta = 0$ demand is smooth and $P = \lfloor P \rfloor$, there is some threshold $\theta_0(\epsilon)$ such that for $\theta > \theta_0(\epsilon)$, $P > \lfloor P \rfloor$. Meaning, there is some threshold

$\theta(\epsilon)$ above which there should be no 00-ending prices. Since prices are discrete, I will ask what the corresponding $\theta_0$ is for $P = \lfloor P \rfloor + 0.01$, or, in other words, for the next lowest price 1 cent above the round price. While there is no analytical solution for $\theta_0$, solving for it numerically shows that for an extreme elasticity of -7 and $\lfloor P \rfloor = 2$ , $\theta_0$ is a mere 0.00075 ($\theta_0$ will be even lower for less elastic demand and higher prices[10]). In contrast, typical left-digit bias estimates are between 0.1 and 0.5, and typical elasticities in these settings are about -1 to -4 (Butters et al., 2019; DellaVigna and Gentzkow, 2019; Hausman et al., 1994; Hitsch et al., 2017; Nevo, 2001). This calibration exercise implies that for any reasonable levels of left-digit bias and price elasticity, *if firms optimize*, we should observe no 00-ending pricing when 99-ending is available.

However, the reform made the trade-off stronger: The post-reform just-below price is 90-ending rather than 99-ending, and the next price above 00-ending is 10-ending rather than 01-ending. The former makes just-below prices provide lower revenue per item, and the latter makes an upward correction more constrained. Due to these two forces, the threshold levels of $\theta_0$ are higher in the post-reform regime. I find $\theta_0$ in the post-reform setting by asking for what parameters would $P > \lfloor P \rfloor + 0.1$, given that the just-below prices are now 90-ending. Although these two forces do have a combined strong effect on the minimal bias leading to no 00-ending prices, the levels are still very low. The threshold levels are depicted in Figure 1. Indeed, while before the reform with elasticity of -4 and $P_1 = 2$, the minimal bias is close to zero, after the reform the minimal bias becomes $\theta_0^{Post} = 0.02$, which is still an order of magnitude lower than levels found in such data, but not outside the scope of what firms might consider the bias to be.

Figure 1 shows threshold values of $\theta_0(\epsilon)$, above which there should be no 00-ending prices, before and after the reform. The dark lines are the pre-reform thresholds, showing that 00-ending prices should not be observed for almost any value. The light curves are

[10] Proposition 2 in Strulov-Shlain (2019) shows that less elastic demand increases $P - \lfloor P \rfloor$, and Corollary 1 shows that $P - \lfloor P \rfloor$ is increasing in $\lfloor P \rfloor$. Meaning, less elastic demand and higher prices will lead to an increase in $P - \lfloor P \rfloor$.

for a post-reform setting, showing that the threshold indeed increases substantially, but not enough to justify 00-ending pricing given the estimated levels of $\theta$ in the data. The figure also overlays estimates from Strulov-Shlain (2019) showing that US retailers underestimate the bias, and as such, are getting closer to, but still above, the thresholds.

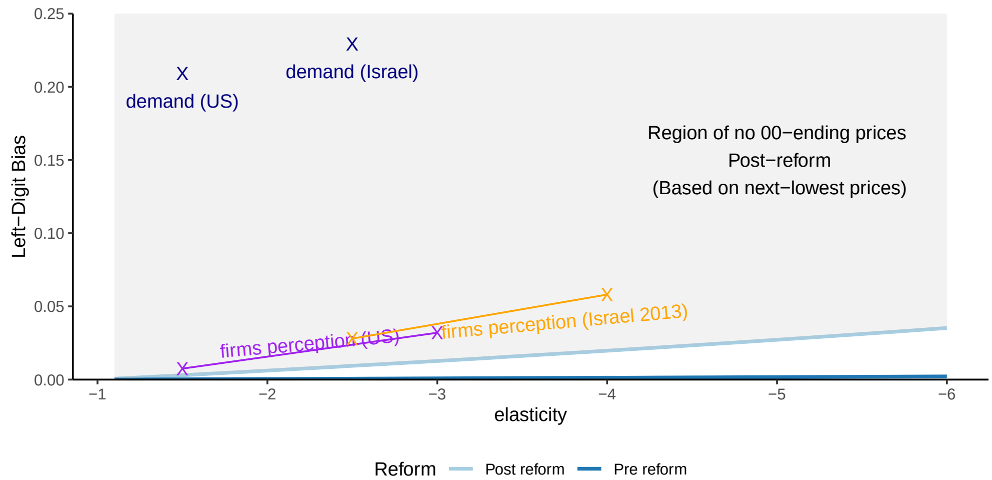



Figure 1: Minimal levels of left-digit bias $\theta_0(\epsilon)$ above which 00-ending is suboptimal

The figure shows the minimal levels of left-digit bias $\theta$ above which 00-ending prices are never optimal. The values are a function of the price elasticity and nominal price level. The dark line shows the threshold values in the pre-reform case (when 99-ending and 01-ending prices were allowed), and the light line shows post-reform values (where 90-ending is the highest price ending and 10-ending is the lowest price ending above 00). The gray area is the parameter set for which there should be no 00-ending prices after the reform. The "X demand (Israel)" is an approximation of the level of left-digit bias found in demand, given the drops in demand estimated from the reform and the price elasticity. "Firms perception (Israel 2013)" shows the range of estimates regarding firms' beliefs about the level of left-digit bias consumers have, that might rationalize their pricing behavior before the reform.
The "X demand (US)" marks the demand-side estimates of left-digit bias, and "Firms perception (US)" the range of beliefs about left-digit bias that U.S. firms hold, as estimated in Strulov-Shlain (2019).

Therefore, the literature shows that there is economically significant left-digit bias, and the model predicts that with these levels of bias there should be bunching at 99- and 90-ending prices, and no 00-ending prices.

# 3 Price Patterns

Given that the model of pricing to left-digit biased consumers predicts no 00-ending prices, we expect that prediction to hold, and can learn from it failing. The actual patterns in the data inform us regarding how firms make decisions (as model-based or model-free learners), and I explain how in the next section, Section 4. This section shows the main patterns without interpretation. First, I describe aggregated and chain-level price-endings shares before, during, and after the reform. Second, I analyze price-endings paths at the product-store level. Finally, I estimate the impacts on demand from increasing a 99-ending price to 00-ending versus lowering it to 90-ending.

## 3.1 Price-endings shares

Figure 2 shows the shares of prices that end with 99, 90, and 00 by period. The shaded area represents the period between the announcement and enactment of the policy change.

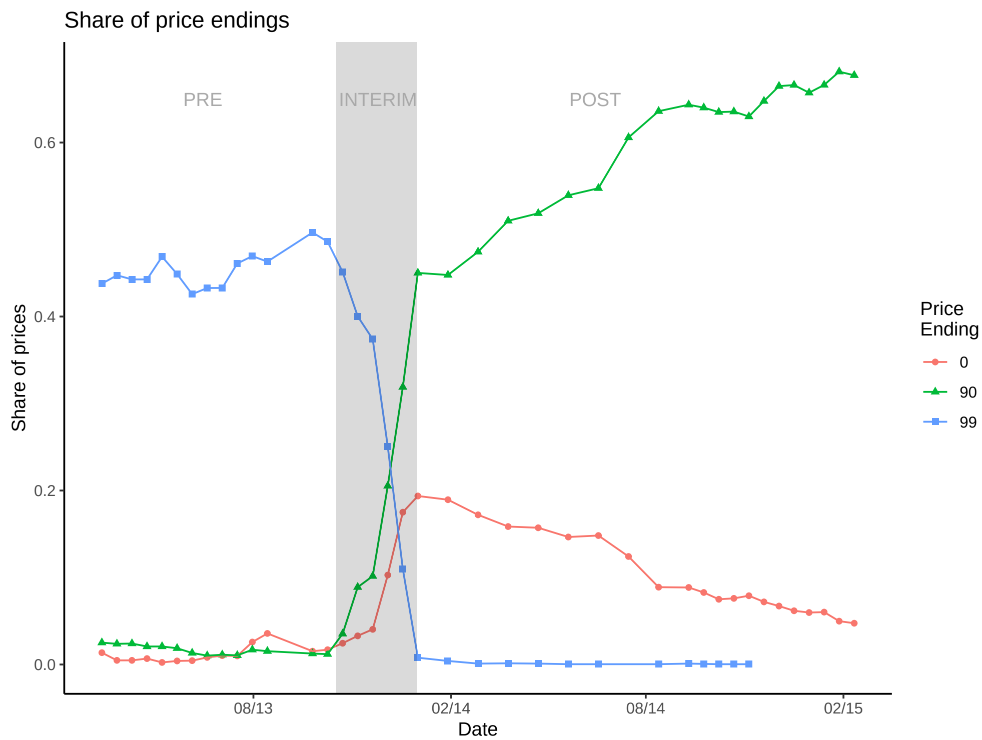
Before the reform, the share of products that end with 99 is high and stable while the shares of 00 and 90 are very low. In both pricing data sets, the share of products that end with 99 before the policy announcement was about 45% and kept stable. In contrast, the shares of 00 and 90 were low, with 1.2% for 00 and 1.7% for 90.[11]

Immediately after the policy is announced but before it is enacted, firms start to change prices. Chains had 2.5 months to update prices, and 79% of products had a price change in December 2013. However, chains did not wait for the policy to bind to update prices and started changing prices away from 99-endings promptly.

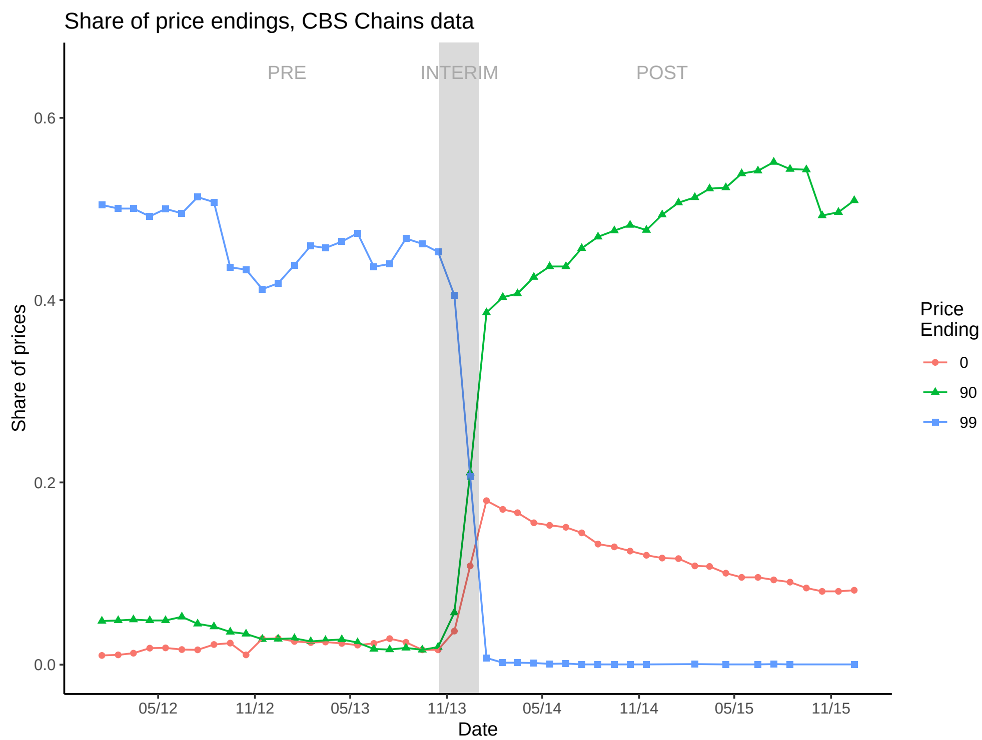
As the policy starts to bind in January 2014, there are about 40% of prices that end with 90 and 20% that end with 00. Yet, a gradual and consistent downward trend in the share of 00-endings starts soon after.

A year after the reform, the share of 00-endings is lower, at about 5% in ICC data and 8% in CBS data; the share of 90-ending prices is potentially higher than the share of 99-ending prices before the reform. A sub-sample of StoreNext's data extends to 2019, showing the share of 00-endings consistently declining to 3% of prices by 2018–2019 (after roughly 20% in 2014–2015).

[11] The second and third most frequent price endings were 49 (6.6%) and 89 (3%).




(a) Shares of products that end with 99, 90 and 00, ICC data




(b) Shares of products that end with 99, 90 and 00, CBS data

Figure 2: Shares of price endings over time

The figures above show that shares of 99-, 90-, and 00-ending prices across products and stores by sampling period. The shaded area represents the time between announcement of the reform on October 17, 2013, and its enactment on January 1, 2014. Squares represent 99-ending prices, triangles 90-ending prices and circles are 00-ending prices. The top panel shows these shares in the ICC data and the bottom panel in the CBS data.

**Between-chains heterogeneity** While the decline in share of 00-endings is gradual on the aggregate, for some chains there is a step-wise update downward. Figure 3 shows the price-endings shares after the policy change for the biggest 6 chains. For most chains, the share of 00-endings behaves in an almost step-wise fashion, where for "Mega in the City" and "Rami Levy" (the second and third most largest chains in the market) the shares go to almost zero. "Shufersal" (the largest chain) with its two subsidiaries "Deal" (discount stores) and "Shelli" (urban stores) goes down to 10% of 00 price endings in mid 2014. "Wine Pavilion" and "Shufersal Deal" seem to also have fewer 00-endings by 2015, but the data ends too soon to see that convincingly.

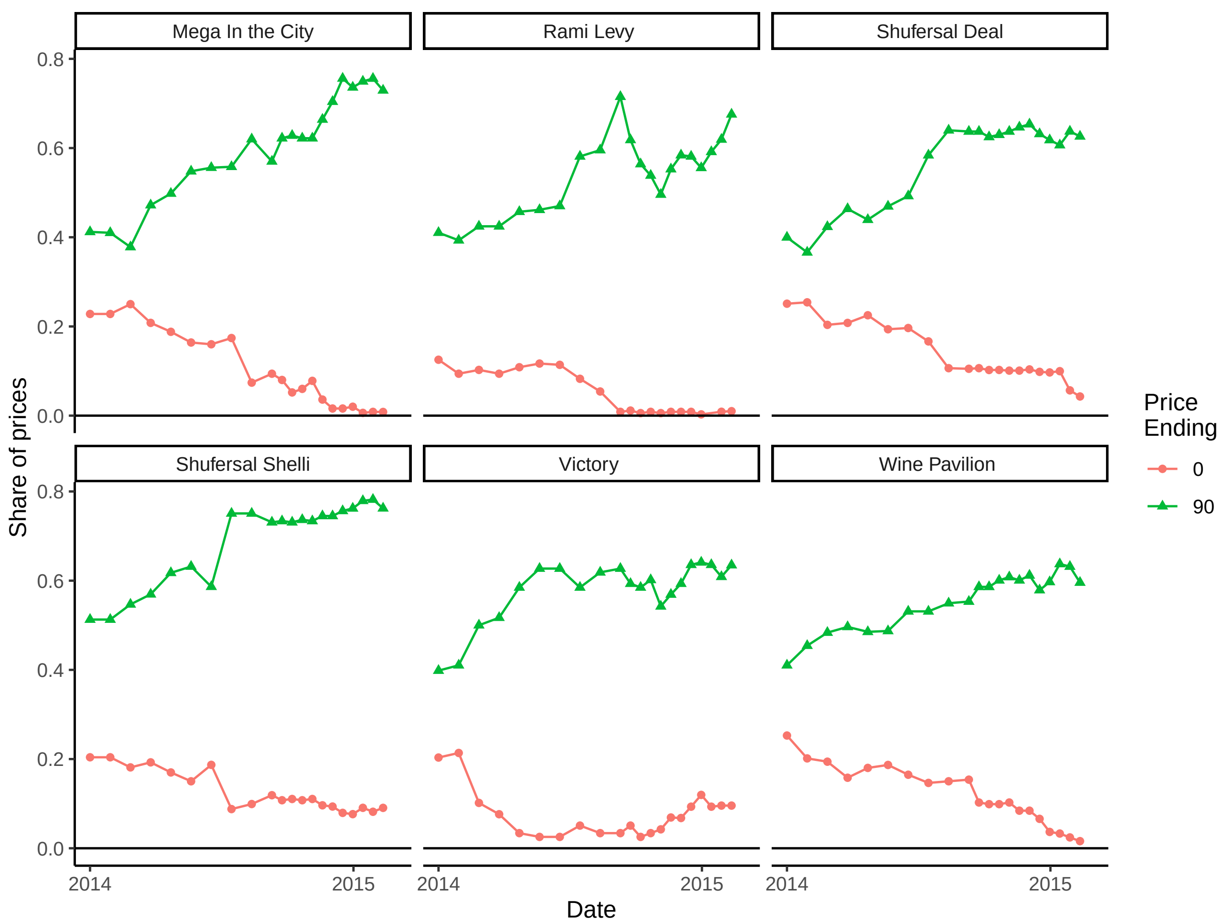



Figure 3: Shares of price endings post-reform by chain

The figure shows that shares of 90- and 00-ending prices across products and stores by sampling period, broken down by chain. Data show shares at the post-reform period (starting January 2014). Results are shown for the 6 biggest chains in the data. Triangles represent 90-ending prices and circles 00-ending prices.

The share of 00-endings prices therefore had been low before the reform, increased until its enactment, and since then gradually declined. In contrast, 99-endings, which were the just-below prices before the reform, were quickly and persistently replaced with 90-endings prices after the reform. But were some products priced at 90-endings and others at 00, or were many products priced at both price endings?

## 3.2 Price-endings paths

We now move from aggregated price patterns to consider the price-ending paths at the product-store level. The price-ending paths can inform us better about the learning processes used by firms. Different leaning processes can generate these aggregate patterns. I elaborate on these tests in Section 4, and the key differentiation between model-based and model-free learning is the contrast between testing multiple actions in model-based learning versus settling on "good" outcomes in model-free learning.

To see those patterns, consider the price path of a product in a store that was initially priced at some 99-ending. To simplify the problem, we consider three potential price-endings: 90, 00, or Other. That is, if a price $p_{ist}$ of product $i$ in store $s$ at period $t$ ends with 90, the current price-ending state is $pe_{ist} = 90$. The states history $h_{ist} = (pe_{is1}, pe_{is2}, ..., pe_{ist})$ is then categorized into price-ending path histories, as described by the automaton (state-machine) in Figure A-2.

The results of the exercise are shown in Figure 4, showing the shares of the different price-ending paths as different shades. The green represents 99, the blue shades 90 (or Other after 90), and the red shades 00 (or Other after 00).[12]

The main patterns are the following. First, products that changed their prices into 90 are unlikely to change, and specifically not into 00. To see that, compare the large and stable dark blue "90, never 00" to the small orange "00 after 90." Second, in contrast, products

[12]The green are the products that ended with 99. Almost all of these changed at one point or another to either 90 or 00. Rarely is there a product that was priced at 99-ending and changed its price to anything else—in the figure, if changed to Other remains green at the post period.

with a 00-ending price are unlikely to stay at 00, and highly likely to change to 90, e.g., compare the shrinking dark red "00, never 90" to the blue "90 after 00." Indeed, products that changed into 00 initially are 15 times more likely to end with 90 than 00 at the last period of data. [13] Third, there is a small set of products with prices ending with both 00 after 90, and 90 after 00, and those tend to eventually end at 90 (e.g., 99 → 90 → 00 → 90). This is evident in the growing light blue "90 after switching" versus small light orange "0 after switching." At the last period, switching products are 8.4 times more likely to end with 90 than with 00.

Therefore, the price-ending analysis shows that product-stores that try a 90-ending are likely to just stay at 90, while 00-ending prices are unstable. There is some "experimentation" of switching between both, but that is relatively rare, and also tends to quickly resolve in 90-ending.

## 3.3 Demand response

In the previous sections we saw that there were many 00-ending prices early after the reform. A natural question to ask is if these prices had any impact on revenues. The model predicts that they are detrimental to demand and should be avoided, and the price updating behavior suggests that this is plausibly the case.

The ideal experiment to learn the effect of price-endings on demand is if price-endings were randomized. In a supermarket setting, it means to randomize prices within a product-store across different periods.[14] A second-best is an experiment in which comparable products' prices are randomized between different stores (the approach taken by Chetty et al. (2009)). The reform allows a quasi-experimental version of these ideal experiments. Consider

[13]Products that changed into 00 initially are 3.5 times more likely to end with Other (not 90, not 00) than 00 at the last period.

[14]If prices could have been randomized at the consumer level, that would also been ideal, but that is hard to implement in a brick-and-mortar store. In online marketplaces List et al. (2021) use an RD design using the smooth distribution of prices which creates quasi-random distribution around 00-ending prices, while Dubé and Misra (2019) randomize prices in a field experiment.

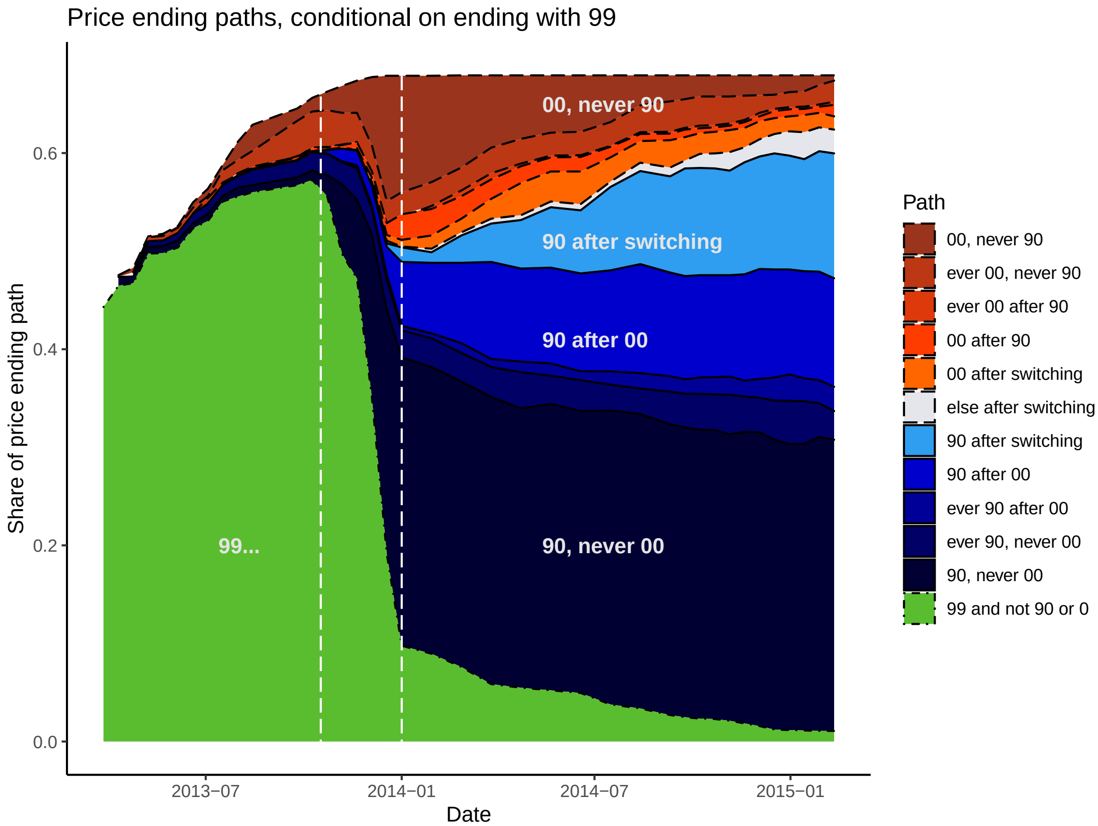



Figure 4: Price ending paths of products who ended with 99 before the policy change

The figure shows the shares of each price-ending path (which are described in Figure A-2). The green area is the 99 or other price endings, the blue shades are the 90-ending prices, and the red shades are the 00-ending prices.

the same product, priced the same way in two stores of the same chain before the reform. For example, it is priced at 5.99 a few times in 2013. For the former design, imagine that the products' price is switching between 5.90 and 6.00 within the same store. For the latter, imagine that after the reform a product is priced at 6.00 in one store and 5.90 in another. The goal of the following analysis is to leverage the reform to estimate these effects.[15]

I use the scanner data from StoreNext for the analysis., and estimate the following specification using instrumental variables for prices:

$$\log Q_{ist} = \alpha^{90}\kappa^{90}_{ist} + \alpha^{00}\kappa^{00}_{ist} + \alpha^{99}\kappa^{99}_{ist} + \beta_{ic(s)} \log P_{ist} + X'\beta + \epsilon_{ist} \tag{4}$$

[15]Strulov-Shlain (2019) tests for these drops in demand using larger and higher quality data, but lacks the random variation.

where $i$ is product, $s$ is store, $t$ is date (week), and $c$ is chain. $Q$ is the number of units sold and $P$ is the average price. For each product-store in 2013 I define the modal 99-ending price $p_{is}^{Modal99}$. The $\alpha$s are the price-endings fixed effects, such that $\kappa_{ist}^{99}$ equals 1 if the price in week $t$ equals $p_{is}^{Modal99}$ in 2013 (0 otherwise), $\kappa_{ist}^{90}$ and $\kappa_{ist}^{00}$ equal 1 if the price is 10-Agorot lower (ends with 90) or 1-Agora higher (ends with 00) than $p_{is}^{Modal99}$, respectively, in 2014 (0 otherwise). $X$ includes various controls—a product-by-store fixed effect (to capture baseline store-product demand), a proxy for a product-store on-sale dummy (to capture transitory sales effects[16]), same-price spell-length decile by product fixed effect (to capture transitory sales effects), a month-of-year by product fixed effect (to capture seasonality), and year-by-product fixed effects.

I instrument for the product-chain elasticity with the leave-out average price of the product in stores of the same chain. The idea behind these instruments (often referred to as Hausman IVs after Hausman (1996)), is that the price within the chain in other stores captures shifts in costs but not in local demand shocks.

The meaning of this exercise is that the $\alpha$ coefficients capture the unexplained demand that is driven by the price-ending, controlling for the overall price elasticity. The most interesting comparison is between $\alpha^{90}$ and $\alpha^{00}$. This comparison shows the difference in demand for "the same" product if it were priced at a 90-ending price versus a 10-Agorot higher 00-ending price in 2014, when price endings were more likely set at random. To a lesser degree, the comparison between a 99-ending price in 2013 and the 1-Agora larger 2014 00-ending price is also informative, but time is confounded with price-endings.[17]

Table 2 shows the results of estimating Equation 4. Overall, the concurrent excess difference in demand between a 90-ending price and a 10-Agorot higher 00-ending price, when the price was the nearest 99-ending before the reform, is estimated to be between 5%--

[16]The dummy variable equals 1 if the price is lower by more than 3% than any price within 3 weeks (before or after).

[17]For example, if many of the prices tend to end with 99 in 2013, the demand slope will be closer to the 99-ending prices due to the year fixed effects. Yet, removing the year fixed-effect will confound the price-ending effects with other year effects.

9%. Meaning, *beyond* the price sensitivity of a 10-Agorot increase, demand is about 5%–9% lower. The difference in demand between a 99-ending price in 2013 and the 1-Agora higher 00-ending price in 2014, which is harder to interpret, is -1%–4%. Column (2) is closest to the first experiment, as it compares the demand at a 90-ending and a 00-ending for a product priced at, say, 5.99 in a store and then priced at 6.00 and 5.90 in the same store. The difference in demand is 8.2% with a p-value of 0.025. Columns (3) and (4) resonate the second approach, comparing demand for the same product priced, for example, at 5.99 in two stores and then priced at 6.00 in one store and at 5.90 in another. The difference in demand is 5.8% (p-value 0.061) in column 3, and 8.6% (p-value 0.0045) in column 4.[18]

With an average elasticity of about -2.5, and average price of 11, and 5%-8% drop corresponds to left-digit bias of $\hat{\theta} \approx 0.22 - 0.36$ (since $\theta \approx \frac{\%\text{drop}}{\text{elasticity}} \cdot \text{price}$). This estimate is depicted as X (Israeli demand) in Figure 1.

The model and previous literature predicted no 00-ending prices, and the demand analysis suggests that 00-ending pricing was a losing pricing strategy.

# 4 Model-based and Model-free Learning

## 4.1 Framework

I now turn to interpret the findings in Section 3 assuming that demand is left-digit biased. The premise of this exercise is that all patterns must be taken into account, while striving for the simplest explanation. The main three patterns are: (1) high shares of 99-endings before the reform and 90-endings after it, low shares of 00-endings before the reform and long after; (2) high shares of 00-endings initially with negative demand impact; and (3) shares of 00-endings decreased consistently (and abruptly in some chains), and once a product-store were priced at 90-ending it was unlikely to be priced at 00-ending later.

[18]Column 4 focuses on prices that were identical for at least two consecutive weeks to reduce price measurement error.

| | Quantity (log-units) | | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| $\alpha^{90}$ | $-0.029$ | 0.006 | $-0.037$ | $-0.018$ |
| | (0.024) | (0.033) | (0.024) | (0.023) |
| $\alpha^{00}$ | $-0.082^{***}$ | $-0.077^{**}$ | $-0.095^{***}$ | $-0.104^{***}$ |
| | (0.028) | (0.032) | (0.027) | (0.029) |
| $\alpha^{99}$ | $-0.058^{***}$ | $-0.091^{***}$ | $-0.062^{***}$ | $-0.066^{***}$ |
| | (0.014) | (0.025) | (0.014) | (0.014) |
| 90 minus 00 | 0.053 | 0.082 | 0.058 | 0.086 |
| [p-value] | [0.083] | [0.025] | [0.061] | [0.0045] |
| 99 minus 00 | 0.024 | -0.014 | 0.034 | 0.038 |
| [p-value] | [0.44] | [0.68] | [0.23] | [0.2] |
| Leave-out price IV | Yes | Yes | Yes | Yes |
| Spell-length restrictions | | | | 2+ |
| 99 prevalence 2013 | 1+ | 5+ | 5+ | 5+ |
| 90 or 00 post prevalence | | 5+ for both | 5+ for either | 5+ for either |
| $N$ | 176,996 | 176,996 | 176,996 | 149,784 |
| $R^2$ | 0.859 | 0.859 | 0.859 | 0.872 |

Table 2: Price-ending effects

The table shows the results from estimating equation 4 under different restrictions and samples. In all columns, a product-store is kept only if there is at least one occurrence of a 99-ending price in 2013. Further, in columns (2)--(4), $\kappa^{99}_{ist} = 1$ only if the price ends with 99 at period $t$ and it ends in 99 for at least 5 weeks in 2013 for product $i$ in store $s$. Sample restrictions in column (4) keep observations only if prices are the same for at least 2 consecutive weeks. In column (2) $\kappa^{90} = 1$ or $\kappa^{00} = 1$ if the price ends in 90 or 00 in 2014, at least 5 times *each* for the same product $i$ in the same store $s$. Meaning, this is estimating the effect on items that switched price endings within the same store over time. In columns (3)--(4) $\kappa^{90} = 1$ or $\kappa^{00} = 1$ if the price ends in 90 or 00 in 2014, at least 5 times for *either* 90 or 00 in the same store. Meaning, this is comparing price-ending effects between "99 to 00" stores and "99 to 90" stores.

First, low shares of 00 and high shares of 99/90 before and long after the reform imply acknowledging of left-digit bias. Bunching at just-below prices and avoiding low-ending prices are manifestations of correct pricing facing left-digit biased demand. Meaning, in the long-run companies realize that just-below prices are better than 00-ending prices, even if they do not realize the extent of it (Strulov-Shlain, 2019).

Second, high initial share of 00 and negative demand consequences imply unawareness regarding the value of 90 versus 00. Meaning, while the firms priced as if they are aware of the relative benefit of 99-ending versus 00-ending, that knowledge did *not* extend to predict the relative value of 90-endings. In other words, although pre-reform pricing behavior is consistent with an as-if model of optimal pricing facing left-digit bias, it failed to predict behavior post-reform.

Third, downward trends of 00, chains' abrupt changes, and price ending paths are consistent with learning. It seems that over time chains realized that 00-endings are worse than 90-endings. The abrupt changes are consistent with "lightbulb" moments of sudden realization, and can rule out a slow adjustment process driven by frictions. Finally, the price-ending paths, in which 90-endings are an absorbing state while 00-ending are not—and further 00-endings are unlikely to follow 90-endings while the converse is not true—are in-line with a model-free reinforcement learning procedures.

Taken together, these patterns can inform us on how firms learn and therefore make decisions. Except for the first pattern, which is consistent with model-based behavior of the firm with wrong beliefs, the second and third patterns are inconsistent with model-based decision making and call for a model-free description.

Model-based decision making is the consensual tool of modeling economic agents. It consists of two components—a function that translates parameters of a problem and actions into outcomes, and a maximization operator choosing the action delivering the best outcome. Importantly, in order to know what the best action is, the decision maker needs to learn about the parameters of the target function—which leads to model-based learning. The goal of learning in a model-based world means resolving uncertainty about the parameters of the problem. For example, when a monopolist sets a price to maximize profits, it needs to know the parameters of the problem, which are price elasticities and marginal costs of the products it sells (and potentially left-digit bias).[19] That is, model-based learning updates the expected value of every possible action.

Compare the above to model-free decision making, which is a private case and outcome of reinforcement learning (Sutton and Barto (2018)). The model-free reinforcement learning system is comprised of an environment of actions and states, and three components: a reward signal – the feedback from taking an action; a value function – the benefits of an action given the state and predicted behavior and evolution of the environment from taking an action (or the optimal action); and a policy – defining which action to take based on beliefs and state. Similarly to model-based, in model-free learning the agent has some beliefs about the value of different actions given the state of the world. The beliefs update based on the reward signals, and a policy will choose the highest-value action. However, in contrast to model-based learning, resolving uncertainty means only learning about the value of a particular action taken. For example, assume a company expects to make \$10 in profits if the price is \$1 and \$12 if the price is \$2. If it sets the price at \$2 and makes \$8, the company will update the value of pricing at \$2, but will not change its expectations about the value of pricing at \$1. That is, model-free learning informs the value of actions in a direct way. Indeed, to infer from one action to another is not model-free, it requires some form of a "model", one that allows to interpolate and extrapolate.

In theory, full learning can be achieved by using either model-free or model-based learning, making behavior indistinguishable in the long-run. For example, Q-learning, a leading model-free reinforcement learning rule, converges to the correct values if every action is tried in every state infinitely often and if new estimates, from trials of actions, are blended with previous ones using a slow enough exponentially weighted average (Dayan, 1992; Watkins

[19] Recent empirical examples are reviewed in Aguirregabiria and Jeon, 2020.

and Dayan, 1992). Even during the learning process, distinguishing the processes requires some clever manipulations (Daw et al., 2011; Kurdi et al., 2019).

This paper explores a different identification strategy. If learning has subsided but "full learning" had not been achieved, the behavior may differ in the following way: If there is no full learning, a change in the action set alone may require learning in the model-free environment but not in the model-based world.[20] To illustrate, consider the pricing example from above and imagine costs doubled, thus requiring higher prices that were not tested before. The model-based monopolist will have a set of beliefs about the demand function shape and therefore will know the best price, while the model-free learners will rely on an uniformed prior and have to start learning again. Since 90-endings were rarely charged, the reform provides such experiment, thus allowing to separate between the methods.

## 4.2 Perceived parameters in model-based pricing

Since the model-based framework is in consensus, I describe here an attempt to reconcile the data patterns with firms' behavior. This exercise is similar in spirit to Doraszelski et al. (2018); Huang et al. (2018). I assume that firms know that demand is left-digit biased, but allow them to be wrong about the level of left-digit bias. If firms think that bias is small (and demand is very elastic), then pricing at 00-ending can be perceived as optimal even though it is accompanied with demand losses. Wrong beliefs will not be able to generate excess shares of 00-endings, but even ignoring the excess share, can they support any 00-endings?

In principle, underestimated left-digit bias might support 99-endings versus 00, but not 90 versus 00, because of the stronger trade-off. As Figure 1 shows, these beliefs about left-digit bias and elasticity need to be extreme. Yet, we can estimate beliefs about left-digit bias before the reform, using the same algorithm as in Strulov-Shlain (2019), and see if they

[20] A change in the action set may require learning, depending on the exact algorithm used. In machine learning, model-free learning algorithms that are "generalised" (e.g., Rummery and Niranjan, 1994), for example, use neural networks to extend predictions of value to actions that hadn't been tested. However, I think of the problem of the monopolist as closer to the discrete case.

allow for 00-endings after the reform—at least in principle.

The algorithm uses the empirical distribution of prices as moments, and predicts the price distribution according to the model. The excess shares in 99-endings and the distribution of lower-ending prices identify the parameters. In Appendix Section C I describe the algorithm in detail.[21]

Even though perceived left-digit bias is much smaller than the demand-estimated level, it is still higher than the thresholds. I estimate that the perceived left-digit bias is ranging between 0.02-0.04 while the threshold values of left-digit bias are less than 0.01. In other words, supermarkets price as if they believe that drops in demand from a 1c increase from 99-ending to 00-ending is equivalent to a 3-5 cent price increase, while a belief of a 2c-equivalent increase is enough to avoid 00-endings. Importantly, the demand side estimated left-digit bias of 0.22-0.36 (e.g, the impact of a 1c increase from 99-ending to 00 is like 23-37 cent equivalent price change). The finding of underestimation of the bias is common in the U.S. market as well (Strulov-Shlain, 2019), and possibly not surprising by itself. However, that even underestimation cannot support 00-endings after the reform is the main case against model-based learning. Namely, estimated beliefs before the reform are inconsistent with pricing behavior after the reform.

That 00-ending rarely follow a 90-ending at the product-store level suggests, though not conclusive by itself, inconsistency with model-based learning as well. If firms are trying to learn the left-digit bias parameter, they need to randomize price endings. The natural way to do so is to change prices into 90- and 00-endings in a methodological way. Yet, that the vast majority of products stay at 90-endings even if they cannot be compared to 00-endings suggests reliance on recent feedback. This evidence is weaker, since there are multiple ways to experiment (e.g., randomize prices between stores and not within product-store).

Finally, an *excess* of 00-ending prices and 90-ending prices at the same time is in contrast to any beliefs of left-digit bias. Excess 00-endings requires beliefs about the shape of demand

[21] Appendix section C is copied from Strulov-Shlain (2019).

that mixes positive and negative left-digit bias. In contrast, pre-reform pricing is only consistent with beliefs of positive left-digit bias. Meaning, pre-reform model-based beliefs are inconsistent not only in magnitude but also in sign.

Taken together, the evidence is inconsistent with model-based learning that had occurred over the years leading to the reform. Therefore, long-run behavior also implies a lack of model-based decision making.

## 4.3 Model-free partial learning

Model-free *partial* learning can explain all the observed patterns. By "model-free learning" I mean a learning rule in which there are prior beliefs about the value of each action; these beliefs are updated for actions taken. By "partial" I mean that full learning is not achieved, perhaps because not all actions are taken. If an action is not taken explicitly (e.g., a price that had not been charged), beliefs regarding the value of that action do not update.

First, the pattern of high shares of 99/90-endings in the long-run are consistent with learning. Specifically, companies learn that these price-endings are associated with higher-than-expected demand over time. Companies are driven towards the correct action, and market forces work to some extent in the long-run.

Second, that there were high shares of 00-endings initially even though they were accompanied with losses is consistent with partial learning. If companies had understood not only the value of 99-endings relative to 00, but also of the value of 90 versus 00, they would probably have not chosen 00-endings after the reform. Once chains try both 90-endings and 00-endings, they realize that 00-endings are associated with worse returns and indeed update.

Third, the step-wise updating at the chain level, and the price-ending paths at the product-store level are fully consistent with product-store level model-free learning. The former implies that there is some point where the firm realizes the relative benefit of one action over another. The price-ending paths, especially that 90-endings are absorbing states, show that not all actions are tried and that good actions are just repeated-a hallmark of model-free reinforcement learning models. As argued in the previous section, this behavior can be consistent with model-based learning as well.

Taken together, the data show that firms need to learn how to price in the post-reform period, and their learning procedures are more aligned with partial model-free learning rather than model-based learning. I now turn to discuss the implications of these findings.

# 5 Discussion

I find that following a pricing reform, supermarket chains had to relearn what they seemed to know and how to price facing left-digit biased demand. The host of evidence on the dynamics of price setting and demand response implies that firms are model-free partial learners. As such, even in the long-run, they are limited in which actions they may take because their beliefs about the returns to rarely-used actions are not calibrated.

**Critiques** There are several limitations to this study. First and foremost, it is merely a single example. One objection might be that this is not a representative case because price-endings do not have big effect on pricing. In other words, it is an unimportant example. Indeed the view that left-digit bias is not important is common, including by retailers themselves. I interviewed several retail executives, in the US and Israel, and they all think it is more of a norm than they make a calculated pricing decision. However, this view is misguided since under-appreciation of left-digit bias bears large effects on profits (List et al., 2021; Strulov-Shlain, 2019). Further, that it might be viewed as unimportant supports the partial learning idea. The most extreme example is in List et al. (2021), in which the (very sophisticated) company is not even aware of the existence of the bias, ignoring it completely in its pricing decisions and losing money for it (since the research the firm had changed its pricing to be all 99-ending). Here, firms are aware to some degree, but only partially.

A related objection is that this example is not a representative case because it is an outlier, and firms often do optimize. However, optimality is often assumed, not tested. A growing body of work shows cases in which companies persistently act in a sub-optimal manner (partial list - Cho and Rust, 2010; DellaVigna and Gentzkow, 2019; Goldfarb and Xiao, 2011; Hanna et al., 2014; Hortaçsu et al., 2019; Hortacsu et al., 2021; Huang, 2021; Shapiro et al., forthcoming; Strulov-Shlain, 2019). Further, partial learning can help answer the common question raised in those papers of "how are firms not optimizing." A recent example, which had inspired this paper, is by Barberis and Jin (2021), showing how model-free reasoning by investors can simultaneously explain many phenomena in individual trading.

Another limitation is that other models can be created to explain the data. For example, if a firm has beliefs about left-digit bias that are state-dependent, then any pricing behavior is possible—if at periods of change firms revert to a null of no bias (similar to Enke and Graeber (2019)), then the observed behavior is consistent with such a model. However, if we expand the model to agree with the data whenever the data do not agree with the model, then the model loses its credibility.

While some people might find these conclusions hard to accept, others might respond that firms' misoptimizing is obvious but model-based is useful.[22] This paper suggests that model-free learning procedures—that have been immensely successful in solving real world problems (a whole field of CS and widely implemented ML routines), and that are represented in our brains (Daw et al., 2011)—can provide an alternative descriptive model which is closer to the truth *and* useful. For example, the insightful analysis of electricity companies learning to bid in a new market (Doraszelski et al., 2018) must disregard the first 16 months of data, because firms' behavior is inconsistent with even qualitative predictions of the model— could model-free learning help explain *all* of the data?

Incorporating partial learning is an exciting avenue for future research, which seems like the logical extension bringing these methods from the Olympus of sophisticated algorithms

[22]In an episode of the Freakonomics podcast discussing Shapiro et al. (forthcoming), the economist Steve Levitt says "any economist who tells you that firms are profit-maximizing has not ever worked with firms. That's a simple model we use when we teach beginning economics because it's easy to solve mathematically."

to the reality of brick-and-mortar stores and real businesses run by humans.

**Implications and avenues for future research** If firms are model-free partial learners, it means they may act in a persistent way in steady state, without better knowledge of the effects of many regulations or changes in the market place. But how often are firms in a true steady state? New establishments open and close, rivals enter and leave, tastes shift, and costs fluctuate. Treating firms as all-knowing is bound to miss many interesting dynamics and potentially lead to wrong conclusions. For example, when analyzing the effects of a change in the market (say a new regulation, or a new competitor entry), researchers should incorporate a learning period, which might be lengthy.

In the long-run firms may behave sub-optimally consistently (e.g., Ely (2011) analyzes how path dependence may lead to suboptimal steady state behavior; Thrun and Schwartz (1993) show when reinforcement learning algorithms might fail). This is especially true due to partial learning, and further investigation of this avenue as a probable explanation can help solve cases in which it is unclear how firms consistently deviate. There is accumulating evidence that consistent deviation from optimality is prevalent. Dubé and Misra (2019) find that a company was consistently charging a price that was about a third of the profit maximizing price; Hortacsu et al. (2021) find that airlines misprice tickets even though they have been doing this for years; Shapiro et al. (forthcoming) find that companies get negative returns on investment from advertising too much on TV. Most closely related to this paper, List et al. (2021) and Strulov-Shlain (2019) find that big, otherwise sophisticated, companies persistently underestimate left-digit bias and lose significant levels of profits.

To conclude, there is ample room for future research. Empirically, more examples and tests of this hypothesis should be made. This paper is only a first step. Methodologically, if firms do not optimize in that specific way, what does that imply on inferred markups from demand estimation, or on valuation inferred from bidding behavior? Theoretically, what are the implications of incorporating model-free partial learning into problems of mechanism design (current behavioral IO literature assumes that firms are fully sophisticated and only consumers are biased (Spiegler, 2011)), on how regulation might affect markets, or on welfare.

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# Appendix

## A Data appendix

Prices, and their dynamics, are the core focus of this paper. This section elaborates on how prices are represented in the data, and the data-cleaning procedures taken to create the final samples assuring that inferences can be drawn from the data.

### A.1 ICC

ICC data are collected by surveyors going to a set of predetermined stores with a product list. Each product is equivalent to a UPC. The surveyors record the shelf price of the item, and the total basket price was posted on the ICC website. The micro-data was generously provided to me by the ICC. The product list may change from week to week, but a core set of products remained constant. Surveyors went to the stores every 2-4 weeks (every 4 weeks in January-September 2014 due to budget cuts, and every 2 weeks otherwise).

On January 2015 Israel passed the "food law" mandating full price transparency starting May 2015 (Ater and Rigbi (2019)). This has made the ICC work redundant, and led to them canceling data collection.

**Price imputation** The ICC data are not a balanced panel, since the product list is changing over time, stores enter and exit the sample, and the sampling rate is inconsistent. Therefore I clean the data to be more balanced. First, I restrict the sample to products that are sampled at all periods. Second, I restrict it to stores that are sampled at least $T$ times within each policy-relevant period. Specifically, I chose 4, 3, and 5 times for the pre-announcement (3/13-10/13), between announcement and enactment (10/13-12/13), and post enactment periods (1/14-2/15) respectively for the Main ICC data; and 15 times at the post period for the Post-only analysis, and at the last period in the data (on February 2015). Third, I balance the panel by completing missing price observations for a product in a store backwards (we also complete forward as a robustness test, but results did not change in a meaningful way). i.e., if a product's prices in 3 consecutive sampling periods are $\{3.9, Missing, 4.2\}$, I complete them to be $\{3.9, 4.2, 4.2\}$. By completing prices backwards I set an upper limit on learning rates.

The data cleaning makes the patterns cleaner and is crucial for exploiting the panel nature of the data for analysis, but does not qualitatively alter the main results. For example, Figure A-1 is equivalent to Figure 2a but is based on the raw data without any sample selection or price imputation.

Figure A-1: Price ending patterns using the full ICC sample


An important trait is that prices are recorded for the first unit purchased. That is, if there is a promotion such as "2nd item for 50% off", this is not taken into account. As such, the prices mostly represent non-sale prices.

## A.2 CBS

The second database is prices collected by Israel's Central Bureau of Statistics (CBS) in order to create the Consumer Price Index. The data are similar in nature to ICC, in the sense that surveyors collect displayed prices of a list of products from stores every month. Since the goal of the CBS is to create a reflection of the representative shopping list, the CBS collects an extensive share of data from non-supermarkets (e.g., markets, specialty shops and convenience stores); each product is sampled from a somewhat different set of stores; and products are a “product-type” rather than an UPC. For example, the product “cottage cheese” may be of different size, manufacturer, and fat content between stores and within stores over time, and collected mainly from supermarkets in big cities. However, the panel is balanced, sampled monthly from 2012 to the end of 2015, and the set of product-types is an order of magnitude larger than in the ICC data (with 171 product-types in the final sample). Summary statistics can also be seen in Table 1. To make the data more comparable to the ICC data I restrict it in the same way and consider supermarkets only.

Most of the pricing pattern analysis is done using the ICC data, since it allows for analyzing the heterogeneity between chains which is of main interest. The basic analysis is conducted for both the ICC and the CBS data, showing very similar results.

## A.3 StoreNext

The StoreNext data is a scanner data recording store-product-period level data on revenue and number of units sold. It also include store and chain anonymous identifiers. Prices in the data are therefore the quantity weighted average of prices paid during the period, and equal revenue over units.

Data have some clear measurement errors in it. To understand the issues, consider the following (true) series of prices in the data, of a chocolate in the same store from four consecutive days in 2015. The price is the division of revenue by units sold in that day. On Sunday the price was 10.9025 (4 units sold), on Monday exactly 10.90 (1 unit sold), and on Tuesday it was 10.902308 (13 units sold). That is, the price on Sunday seems like an average of 3 units sold for 10.90 and 1 unit for 10.91 (and on Tuesday of 10 units for 10.90 and 3 for 10.91). A same-day price difference of 1 Agora is extremely unlikely and 10.91 was not even an admissible price in 2015. Of course, when the price is so close to 10.90 it is easy to guess that 10.90 is the true price, but prices also fluctuate by a few Agorot from day to day, which still seems unlikely to be a true price update and make it harder to determine what the true price(s) might be. To get a sense of the magnitude of such noise in the data, consider that while weekly level data from a national US retailer exhibit 90% of prices that are "to-the-cent" (Strulov-Shlain (2019)), the *daily* Israeli data shows only 55% of those.

I however aggregate the data to the weekly level to merge it with another excerpt of 13 products[23]. To make the data comparable I am keeping records from 2013 to 2015.

Given the price measurement issues, I round prices to the nearest admissible price (at the 1-Agora level in 2013 and 10-Agorot level in 2014-2015)[24]. To be confident that the weekly price represents the true price that consumers paid, I keep observations where the price is indeed admissible absent rounding (34.6% of prices), or if it is also of a same-price spell of at least three weeks (35.8%), keeping a total of 47.26% of the popular product-store observations[25]. The descriptives are shown in column "StoreNext main" of Table 1. In the main sample, 72% of observations are of admissible prices and 84% are of same-price spells of at least 2 weeks

[23]I am thankful for Itai Ater for making the data available.

[24]To create a balanced sample of product-stores, I keep product-stores pairs where there are any sales for at least 80% of the total weeks, and for 90% of the weeks of the pre-reform period of 2013. I then calculate for each price sequence of a product in a store the length of same-price spells, deeming the price to be the same if the differences between two consecutive weeks are at most 2-Agorot (unless the change is between 99- and 00-endings then I deem it different prices).

[25]For the same-price spells I code "the" price as the modal price of the spell.

# B Additional pricing patterns

## B.1 Automaton Figure

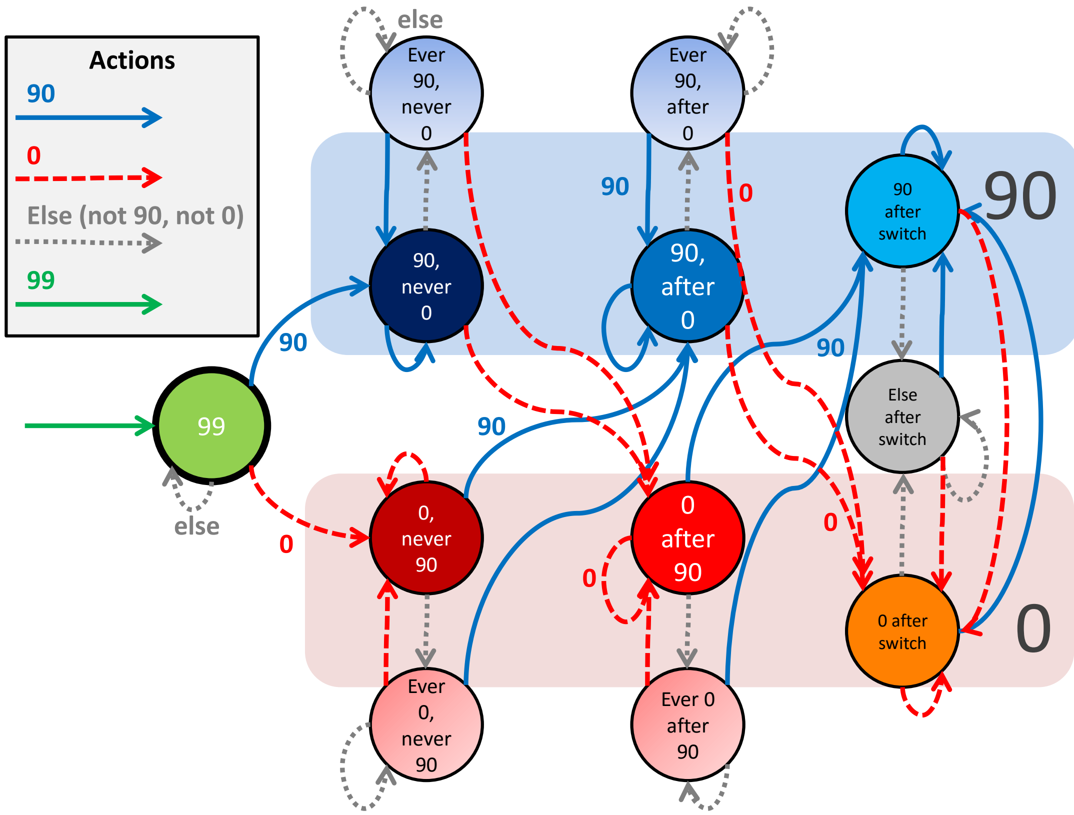



Figure A-2: Automaton tracking price endings path

The figure illustrates a state-machine that assigns each observation into a "price-ending path" as a function of 4 possible price endings: 99, 90, 00, or other. For example, a price sequence of $\{4.99, 4.90, 5.00, 5.90\}$ will have the corresponding states {99, 90 never 0, 0 after 90, 90 after switch}.

## B.2 Product heterogeneity

Figures 2 and 3 show price-endings shares aggregated across the different products, but we can learn more by thinking of heterogeneity. Consider the assumption that products with higher shares of 99-endings are associated with stronger left-digit bias, or at least with stronger perceived left-digit bias by the chain[26]. The correlation is stronger if different

[26] What does it mean to have bias heterogeneity between products? One explanation is that different clientele purchase different products, another is that other characteristics of the products receive more

products have similar cost distribution and elasticities. To the extent that more 99-endings reflect stronger perceived left-digit bias, these should be associated with fewer 00-ending prices. Meaning, due to the true or perceived left-digit bias, if firms understand the model we would expect items that had more 99-endings to have fewer 00-endings post reform. Do these predictions hold in the data?

Figure A-3 gives an idea on how firms responded. The panels show for each product the share of 00-ending prices at different periods against the share of 99-ending for these products in the pre-period. First, Figure A-3a shows that even for products with few 99-endings (presumably due to lower left-digit bias), there was almost no 00-ending pricing taking place before the reform. Second, a year after the reform, as shown in Figure A-3c, there were more 00-ending prices for some products, and mostly for items with lower pre-reform shares of 99-endings. Finally, Figure A-3b shows the patterns immediately after the reform. The figure shows that there were many more 00-ending prices, and here too, mostly for items with lower shares of 99-endings though the negative correlation is driven by one group of products, namely fresh produce[27]. That is, for all products except for fresh produce, there is a strong positive correlation, suggesting that stores rounded 99 to 00 rather than the other way around. In Appendix Figure A-4, we see similar patterns in the CBS data, though less pronounced, where each point is a product-type rather than a specific product.

## B.3 Spatial competition

Another dimension to look at heterogeneity of responses is through spatial differences. Can competitive forces increase the speed of convergence?

I examine whether stores that have more nearby competitors lower the share of 00 faster than those with fewer competitors. For each store in our data, I count the number of

attention than the price (such as observable quality in fresh produce), or that items purchased in bulk or multiple units require price multiplication which puts extra weight on the left-most digits.

[27] One group of products, of fresh produce, was very high on 99-endings pre-reform and translated immediately to 90-endings post-reform. The reason for these items different pricing might rely on that they are purchased by weight (the pricing norm in Israel is to price per kg), but that is only a conjecture.

competing stores within a radius. For example, above and below median within a 5 km radius[28]. I then compare the differences in shares between stores above and below the median of spatial competition. Figure A-5a shows that stores in a denser area have consistently a lower share of 00, at least initially. The dashed lines represent significant differences when controlling also for product-level shares, and clustering standard errors at the store level. The differences are significant mainly at the second half of 2014, implying that maybe differential learning is in place. However, different chains are located differently between more or less dense regions, and the between-chain heterogeneity can be the source of differences. Therefore, I do the same exercise only for the 25 stores of "Shufersal Deal" (Figure A-5b), where differences in shares of 00 above and below the median[29] are less pronounced but still similar qualitatively. Most of the gap is on January 2014, and it is somewhat persistent, suggesting if anything there is a negative effect on the speed of convergence.

# C Perceived left-digit bias algorithm

A step-by-step algorithm explains the mapping between parameters of optimal pricing and moments in the data. Let $S_p$ be the price share of prices set at a price $p$ resulting from one such price aggregation. I use minimum-distance estimation to match the empirical price densities, $\{S_p\}$, to the predicted price densities, $\left\{\hat{S}_p\right\}$ (where $p$ belongs to a grand price vector $\mathcal{P}$). For example, take $\mathcal{P} = \{2.29, 2.39, ..., 3.69\}$. The algorithm works as follows: (1) Fit a logistic polynomial to the empirical price cumulative density function using prices that are not bunching at 99 or missing due to the bias, and call that *price* distribution $\hat{F}_p$. The parameters from that regression give the shape of price distribution. Then, for each pair of elasticity $\epsilon$ and left-digit bias $\theta$: (2) take all relevant 99-ending prices, $\{q_i\}$, and calculate

[28] Choice of other radii does not seem to matter. A choice of 5km keeps stores above and below median also within more densely populated areas, and is not a simple separation of urban versus sparse villages. I could not get too narrow due to measurement error in locations, which are based on store name and city, only sometimes with a precise address, and then extracted from Google Maps.

[29] The allocation to medians is still based on all chains

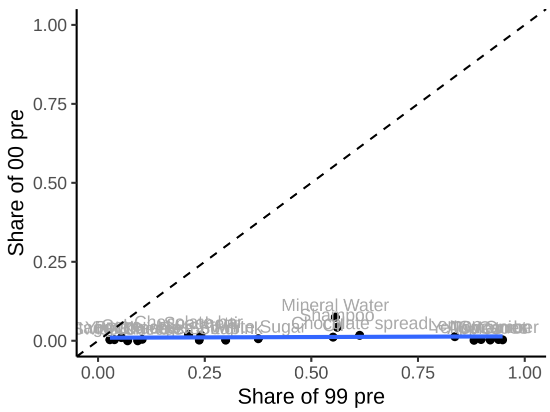
the matching next-lowest prices $\{P_{q_i}\}$ using Equation 3 (for example, say $\{q_i\} = \{1.99, 2.99\}$ and that $\{P_{q_i}\} = \{2.51, 3.53\}$). (3) Expand the vector of prices to include $\{q_i\}$, $\{P_{q_i}\}$, and all 9-ending prices between each pair of $P_{q_i}$ and $q_{i+1}$, giving a vector of prices $\mathcal{P}'$ (e.g., $\mathcal{P}' = \{1.99, 2.51, 2.59, 2.69, ..., 2.99, 3.53, 3.59, 3.69\}$). (4) Given $\theta$ and $\epsilon$, calculate for each $p_i \in \mathcal{P}'$ the cost $\underline{c}_{p_i}$ such that $p_i$ is the profit-maximizing price. By construction, this is given by the first order condition as $\underline{c}_{p_i} = p_i \frac{1+\epsilon}{\epsilon} + \frac{\theta}{1-\theta} \frac{\lfloor p_i \rfloor}{\epsilon}$. (5) For each price, the share of observations at that price is $\hat{S}_{p_i} = \hat{F}_c\left(\underline{c}_{p_{i+1}}\right) - \hat{F}_c\left(\underline{c}_{p_i}\right)$, where $\hat{F}_c$ is the *cost* distribution. I recover $\hat{F}_c$ using the following identity coming from the model:

$$
\begin{aligned}
\hat{F}_c(\underline{c}_p) = Pr(c \le \underline{c}_p) &= Pr\left(p\frac{1+\epsilon}{\epsilon} + \frac{\theta}{1-\theta}\frac{\lfloor p \rfloor}{\epsilon} \le \underline{c}_p\right) \\
&= Pr\left(p \le \underline{c}_p \frac{\epsilon}{1+\epsilon} - \frac{\theta}{1-\theta}\frac{\lfloor p \rfloor}{1+\epsilon}\right) \\
&= \hat{F}_p\left(\underline{c}_p \frac{\epsilon}{1+\epsilon} - \frac{\theta}{1-\theta}\frac{\lfloor p \rfloor}{1+\epsilon}\right)
\end{aligned}
$$

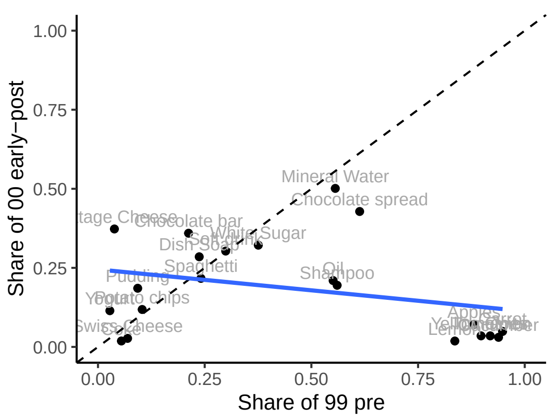
(6) Expand $\mathcal{P}'$ to $\mathcal{P}$ by adding the missing prices between $q_i$ and $P_{q_i}$ and assigning predicted zero shares to them.[30]

To estimate parameters, I minimize the sum of squared differences between predicted moments and actual moments.The above procedure generates the predicted price moments $\hat{S}_{p_i}$ as a function of elasticity $\epsilon$ and left-digit bias $\theta$. Since I am using the densities of some prices to fit the shape price distribution, I exclude these moments from the minimization problem.

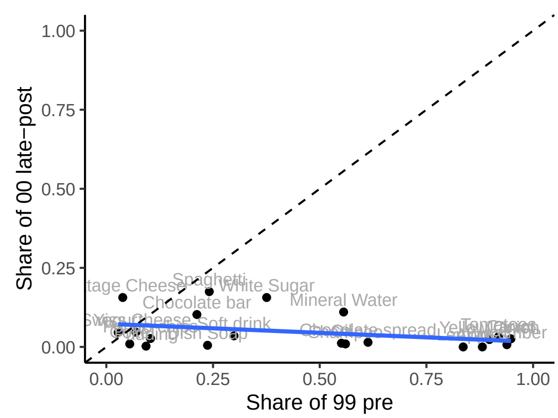
[30]relabel non-9-ending next lowest prices as the lower 9-ending price, without changing the share. For example, 2.51 will be relabeled as 2.49 with a share $\hat{S}_{2.49} = \hat{F}_c\left(\underline{c}_{2.59}\right) - \hat{F}_c\left(\underline{c}_{2.51}\right)$




(a) Correlation between share of 99 pre and 00 pre (3-10/2013)



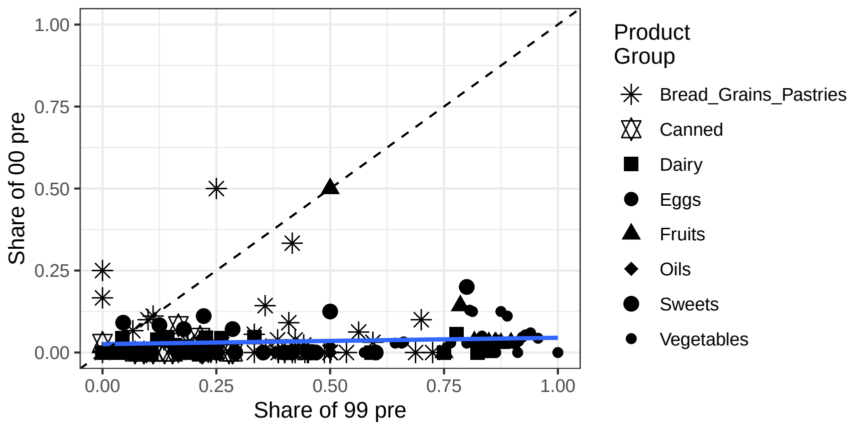

(b) Correlation between share of 99 pre and 00 immediately after policy change (1-2/2014)



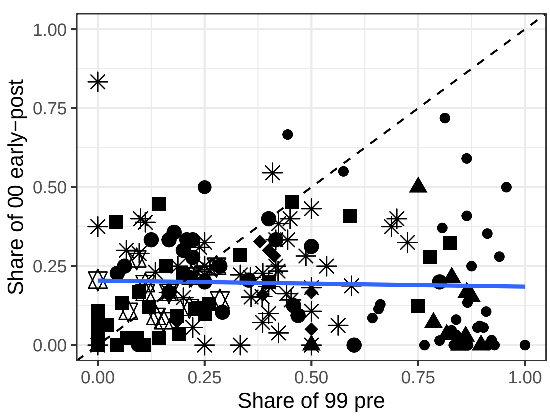

(c) Correlation between share of 99 pre and 00 a year after policy change (1-2/2015)

Figure A-3: Correlation between pre-shares of 99 and post-shares of 90 and 0. Each dot is a product in the ICC data.

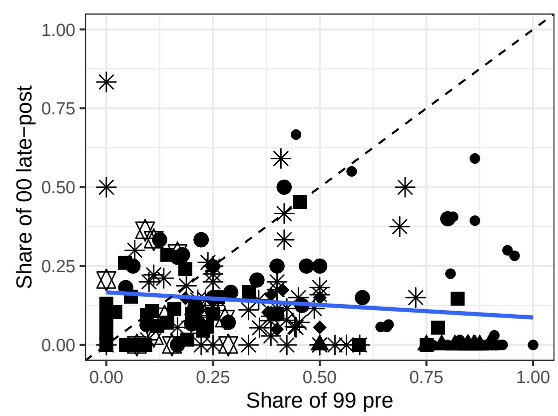
The figures show that shares of 00-ending prices at different periods against 99-ending prices at the pre-period, by product. The shares of 00-ending prices per product are calculated before the reform announcement, at the two months following the reform, and at early 2015 a year after the reform.




(a) Correlation between share of 99 pre and 00 pre (3-10/2013)



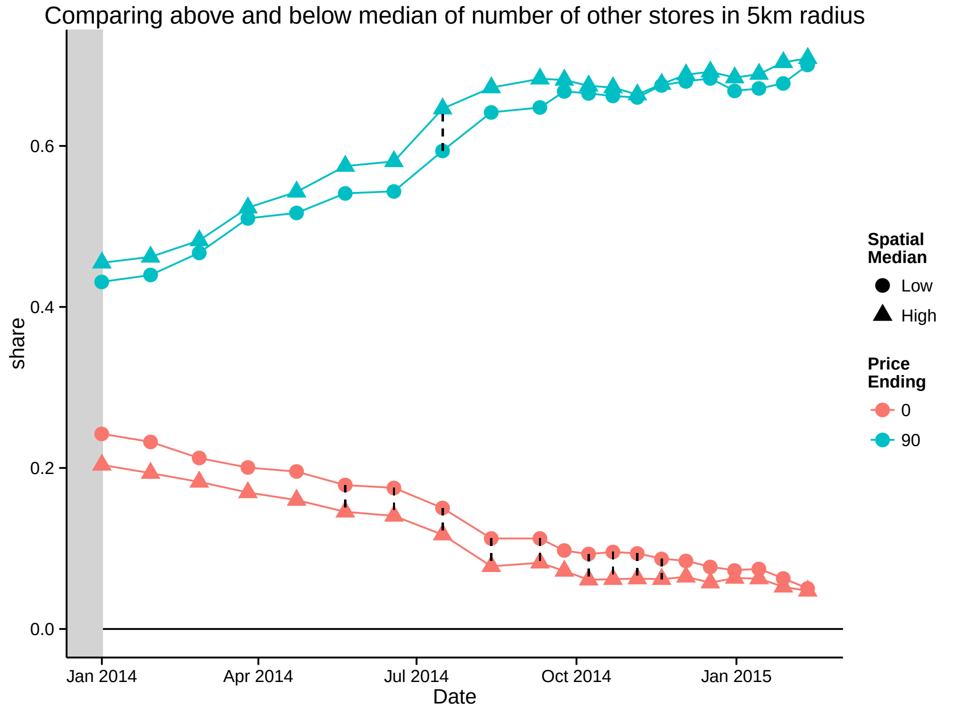

(b) Correlation between share of 99 pre and 00 immediately after policy change (1-2/2014)



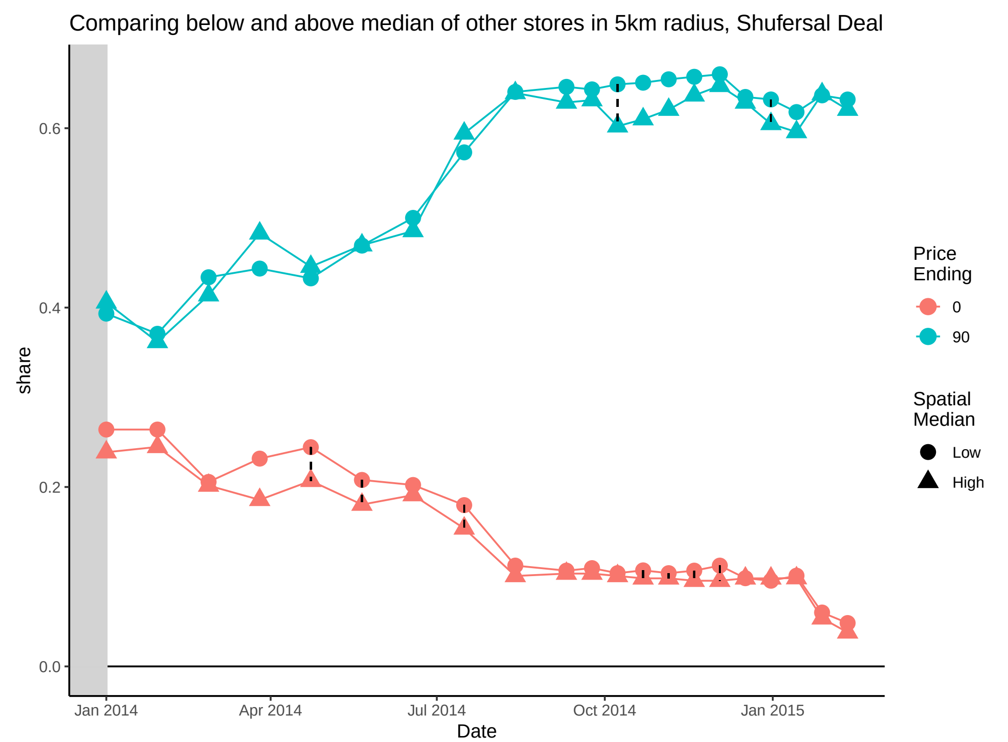

(c) Correlation between share of 99 pre and 00 a year after policy change (1-2/2015)

Figure A-4: Correlation between pre-shares of 99 and post-shares of 90 and 0. Each dot is a product in the CBS data.

The figures show that shares of 00-ending prices at different periods against 99-ending prices at the pre-period, by product-type in the CBS data. The shares of 00-ending prices per product are calculated before the reform announcement, at the two months following the reform, and at early 2015 a year after the reform.




(a) Shares of 00 and 90 for stores above and below median of spatial competition




(b) Shares of 00 and 90 for "Shufersal Deal" stores above and below median of spatial competition

Figure A-5: Spatial competition and shares of price ending

The figure shows the shares of 00- and 90-ending prices at the post-period for stores that are above and below the median of number of local competitors. The top panel show all stores, and the bottom panel only the stores of the Shfersal Deal chain.