Firms have Partial Knowledge and they Partially Optimize: Evidence From A Reform

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March 20, 2025

Abstract

Firms may attempt to maximize profits but can fail for various reasons. We examine a case where we find that the only model of the firm consistent with the data is one of misoptimization due to insufficient knowledge about the world. A reform in Israel mandated that prices end with 0 in the cent digit (e.g., 2.90 instead of 2.99). Because consumers exhibit left-digit bias——which causes demand to drop discretely by 5%-9% at round prices——optimal pricing implies setting prices just below round numbers (e.g., 2.99 pre-reform or 2.90 post-reform) and avoiding round prices (e.g., 3.00). In fact, before the reform, supermarkets used just-below prices for 45% of their price points and rarely used round prices. If pre-reform price-setting were driven by the correct model of demand, firms would have updated their pricing immediately in accordance with their beliefs, thereby avoiding round prices. However, pricing after the reform is inconsistent with their pre-reform revealed beliefs, as firms set 20% of prices at clearly dominated levels for almost a year. Whether firms were optimizing a wrong model or making decisions in a model-free manner, their knowledge had to be partial. Partial knowledge driven by incomplete learning can explain how firms persistently deviate from a normative optimum and challenges counterfactual analyses that assume perfect optimization. I propose an approach based on partial optimization to calculate counterfactuals that are more likely to reflect reality.

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I am thankful for Stefano DellaVigna for endless encouragement and support. Ned Augenblick, Zarek Brot-Goldberg, Ben Handel, and Dmitry Taubinsky provided many thoughtful comments. Christy Kang provided excellent research assistance. This research greatly improved thanks to discussions with Nick Barberis, Modibo Camara, Jean-Pierre Dube, Ben Enke, Wes Hartmann, Johannes Hermle, Ella Honka, Ali Hortacsu, Alex Imas, Tesary Lin, Sanjog Misra, Sarah Moshary, Muriel Niederle, and Richard Thaler. All errors are my own. I am in debt to Itai Ater, Roni Rojkin (ICC), Saul Lach, and Merav Yiftach (CBS) for greatly assisting me with data collection and thus making this project viable.

1 Introduction

Understanding the behavior and knowledge of firms is fundamental to economics and marketing. For instance, any counterfactual analysis requires a plausible model of behavior and market structure. Although a model of firm behavior is necessary, such models are typically assumed and rarely can be rigorously tested[1]. In this paper, I take advantage of a reform that enables us to distinguish between different explanations of firm behavior in both the long and short run. I find that the only consistent explanation for firms' behavior is that they do not fully understand demand and consistently operate under partial knowledge.

My findings offer an alternative explanation for a growing body of empirical evidence documenting persistent misoptimization among firms—i.e., companies do not maximize what researchers assume they should[2]. These findings contradict the cornerstone assumption that firms optimize a normative function. This assumption posits that firms evaluate available actions using a normative model and choose optimally (albeit sometimes under constraints). It is a view that spans from neoclassical economics (Muth (1961)) to behavioral industrial organization (Heidhues and Kőszegi, 2018; Spiegler, 2011). In contrast, the empirical literature on misoptimization suggests that firms may persistently deviate from maximizing reasonable normative objectives, even when constraints are taken into account.

We allow for a potential wedge between what the firm actually does and what it should do. To illustrate, we borrow from Kahneman et al. (1997) the distinction between decision utility and experienced utility. In our case, these correspond to a decision objective function Π' and the true, experienced function Π. Each objective comprises a "real" component (e.g., economic profit) that depends on actions, the state of the world, and constraints. The differences between Π' and Π may arise from incorrect beliefs and/or differing constraints. The boundary between objectives and constraints is often ambiguous and open to interpretation.[3] These distinctions are crucial for welfare analysis and can fuel extensive debates leading to opposite conclusions from the same evidence. Nonetheless, regardless of whether the gap between Π' and Π is a mistake or potentially closable, it is Π' that governs firm behavior, and thus Π' describes how firms actually behave rather than how we wish they would.

[1] An influential literature studies market conduct to distinguish between equilibrium models of imperfect competition, assuming that firms optimize or follow a specific rule of thumb based on demand estimation Backus et al. (2021); Berry and Haile (2014); Bresnahan (1982); Bulow and Pfleiderer (1983); Dearing et al. (2024); Duarte et al. (2024); Lau (1982); MacKay et al. (2014); Sumner (1981)

[2] See Bloom and Van Reenen 2007; Cho and Rust 2010; Dalton and Landry 2020; DellaVigna and Gentzkow 2019; Dube et al. 2017; Dubé and Misra 2023; Goldfarb and Xiao 2011, 2019; Hanna et al. 2014; Hortaçsu et al. 2019; List et al. 2023; Rao and Simonov 2019; Shapiro et al. 2021; Strulov-Shlain 2023

[3] For example, holding an incorrect price elasticity or optimizing against the wrong demand shape (e.g., Dubé and Misra (2023) or List et al. (2023)) can be modeled either as a consequence of erroneous beliefs or as stemming from information constraints (if a company must hire a consultant and purchase data to learn the truth). Similarly, neglecting competitors' responses or weather's influence on performance (e.g., Hortaçsu and Puller (2008) or Goldfarb and Xiao (2019)) can be modeled as resulting from wrong beliefs (if learning their importance corrects behavior) or as constraints (if acquiring the necessary knowledge and cognitive abilities is costly).

This paper argues that sustained deviations from the normative benchmark (henceforth, mis-optimization) can persist due to limited learning, leading to *sustained partial knowledge*. It uses data and a reform affecting supermarket pricing to empirically document a case where large firms maintain sustained partial knowledge. The paper then examines as-if explanations for firm behavior—either that firms operate with an incorrect model of the world or that they engage in model-free decision making based on wrong beliefs—and discusses their implications: if firms begin with an incorrect model and do not attempt to refute it, suboptimal behavior will persist (Hanna et al., 2014; Gagnon-Bartsch et al., 2021; Schwartzstein, 2014); if firms are model-free decision makers (Arthur, 1991; Cross, 1973, operationalized with reinforcement learning, Sutton and Barto 2018) and do not explore the action space sufficiently, erroneous beliefs and suboptimal behavior will continue.

The paper then suggests how to incorporate deviations from the normative benchmark to improve research. Claiming that deviations are almost always present, it offers a method to address and test the sensitivity to suboptimal behavior of unknown origin.

Specifically, to test if firms' decision making is consistent with normative optimization, I use data from a reform that limited the set of admissible actions a firm could take. To illustrate my empirical strategy, consider the following thought experiment: There are three options with an objective ranking of $a \succ b \succ c$. In the steady state, the decision maker chooses a, consistent with the ranking. Then, when option a is removed from the choice set, full knowledge would dictate choosing the next best option, b. Instead, imagine that the decision maker initially selects c. Although we cannot rule out that the decision maker knew that $a \succ b$ and that $a \succ c$——knowledge that was initially sufficient——we can conclude that they did not know $b \succ c$. Thus, they operated under partial knowledge in the original steady state. The empirical strategy in this paper tracks this thought experiment.

In 2013, the Israeli government banned pricing in coins that no longer exist—the equivalents of the US penny and nickel—forcing companies to use prices ending in .X0 (Ater and Gerlitz, 2017). For example, 2.96 is banned, but 2.90 or 3.10 are allowed. Before the reform, the modal price-ending in Israeli supermarkets was 99 (e.g., 2.99), accounting for approximately 45% of prices, while 00-ending prices were almost entirely absent. This pre-reform price-ending distribution is consistent with at least partial maximization, specifically with a model of pricing that accounts for left-digit biased demand[4]——a demand structure driven by consumers' distorted price perception, which leads to discontinuous drops in demand at round prices.[5] When this is the demand structure, prices that would have ended with lower digits (e.g., 5.00 or 5.29) in the absence of bias are better set with a 99-ending (4.99 in this case). When 99-endings——previously a profit-maximizing price ending for many, if not all, cost and elasticity configurations——are banned and the highest just-below price-ending becomes 90, pricing predictions still indicate that prices should not end with

[4]That supermarkets realize they face left-digit biased demand, at least to some degree, was also found in supermarkets in the U.S. (Strulov-Shlain, 2023).

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

00.[6] 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 associated with an excess demand loss of 5%-9%. In the notation of the above thought experiment, 99-ending prices represent option a, 90-ending prices option b, and 00-ending prices option c, with $a \succ b \succ c$. Thus, the behavior of companies before the reform was consistent with at least an understanding that $a \succ b$ and $a \succ c$.

At the time of reform implementation, 20% of prices ended with 00. This high share is inconsistent with the demand structure we find where $b \succ c$. In the years that followed, the proportion of 00-endings decreased again. For some chains, a discrete reduction in the share of 00-ending prices occurred about eight months after the reform. The evolution of price-endings has been such that products whose price-ending changed from 99 to 90 remained at 90, while those that switched to 00-ending quickly reverted to 90-ending prices. In other words, we have evidence of movement from a to both b and c, followed by a shift away from c to b.

The long-run behavior of firms, both before and long after the reform, is inconsistent with normative optimization. Beyond the three-choice example, chains' pricing behavior is suboptimal due to an underuse of 99-ending prices (consistent with List et al., 2023; Strulov-Shlain, 2023). The magnitude of left-digit bias should have resulted in more 99-ending and 90-ending prices than were observed. One explanation is that chains used 99-ending prices before the reform because they recognized the value of 99 versus 00; however, they did not use a sufficient number of 99-endings before the reform (or 90-endings after), possibly because they operated under partial knowledge and did not fully understand the demand structure. Furthermore, this partial knowledge is consistent with the previously noted excessive 00-endings after the reform.

How can partial knowledge be sustained in the long run? The data can be explained in several ways, but a common factor is under-exploration. For example, one possibility is that firms were maximizing against an incorrect model of demand that is non-monotonic at 99-ending prices—that is, the demand is generally downward sloping but exhibits spikes at 99-endings. Although this structure is refuted by the current data (as well as by List et al. (2023); Strulov-Shlain (2023)), it was suggested in some cases (e.g., by Stiving (2000) and Anderson and Simester (2003)). Alternatively, firms may be model-free learners who were able to discern the relative benefits of 99-endings versus 00-endings but not those of 90-endings versus 00-endings, only learning about the latter after the reform. Either explanation hinges on under-exploration. Had firms attempted to refute the demand-with-spikes model, they would have; had they experimented with more price endings, they would have learned the full structure of demand.

Why then do firms not learn? One explanation is that this behavior is normatively optimal—learning is costly, and firms acquire only as much information as is justified (à la Rational Inattention) or approximate it by focusing on what they deem most important. Alternatively,

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

they might learn solely what they (possibly erroneously) perceive as important, leaving them arbitrarily wrong (à la Gagnon-Bartsch et al. (2023)). In either case, their long-term behavior deviates from what data and theory would predict, causing normative models to mischaracterize their actions.

Sustained mis-optimization has implications for counterfactual exercises in two ways. First, when recovering unobservables from a model, researchers assume optimization (as seen in every structural paper with a counterfactual section); second, when predicting outcomes under changing conditions, the assumption of optimization is also maintained. Suboptimal response and partial knowledge do not imply a complete absence of optimization but rather a failure to achieve normative optimization. Therefore, rather than discarding the optimization assumption altogether, it is preferable to test the sensitivity of counterfactual conclusions to varying degrees of deviation from the normative benchmark. In the spirit of Afriat (1972), Chetty (2012), and Rabin (2013), I allow for optimization frictions by introducing a "fudge factor" $X \in [0, 1]$, which quantifies how far from the benchmark the decision maker may deviate. The idea is simple: the standard revealed preference argument is that if we observe a firm taking an action $p = p^*$ and we know that, under optimization, $p = \Pi(p; a) = f(a)$ is a function of latent parameters a, then we infer that $a = f^{-1}(p^*)$. If we instead observe a firm taking an action with the goal of maximizing profits but allow for a failure to optimize by a factor of X, then any a satisfying $\Pi(p^*, a) \geq (1 - X) \cdot \Pi(f(a), a)$ is a possible value of the parameter of interest.

Conceptually, this factor is not new. It is closely related to Afriat's famous Critical Cost Efficiency Index (Afriat, 1972) and to subsequent work focusing mainly on individual decision makers (e.g., Choi et al. (2014); de Clippel and Rozen (2021); Halevy et al. (2018); Varian (1990)). These studies propose calculating such a factor to measure how far a decision maker deviates from maximizing utility, or to offer guidelines for choosing the best descriptive model of behavior by minimizing this deviation. I diverge from this literature in two ways. First, I focus on firms maximizing profits rather than consumers maximizing utility. Second, I propose to use this factor even when it cannot be directly estimated from the data. While the existing literature addresses settings where data reject all models and seeks to reconcile that gap, I consider an exercise in which the model is assumed and explore the implications of deviations from optimization. In some simple examples, the implications are highly nonlinear—even a 0.5% loss in profits can lead to changes in consumer surplus of around ±5%.

Across industries—such as tech companies, national retail chains, large airlines, car rental companies, and international brands—the literature documents profit losses of dozens of percent in specific actions, translating into several percent of overall profits (Blake et al., 2015; Cho and Rust, 2010; DellaVigna and Gentzkow, 2019; Dubé and Misra, 2023; Hortacsu et al., 2021; List et al., 2023; Shapiro et al., 2021; Strulov-Shlain, 2023; Zhang and Misra, 2022). These are selected examples; many of the cited papers do not aim to demonstrate misoptimization, yet they find

evidence of it. In general, I believe it is justified to acknowledge the possibility that X is positive, and thus to test the sensitivity of conclusions to various values of X.

1.1 Literature

The main contribution of this paper is to provide empirical evidence of sustained partial knowledge among large, sophisticated firms—even in the presence of a learning process. Prior work on individuals (Schwartzstein (2014); Hanna et al. (2014); Gagnon-Bartsch et al. (2021)) has shown that in multi-attribute decision-making environments, people can make persistent mistakes by ignoring certain optimization-relevant dimensions. Field experiments in India and Uganda (Anderson et al. (2021); Bloom et al. (2013)) demonstrate that management and marketing training increases profits and growth, attributing pre-intervention suboptimal behavior to information gaps. Theoretically, Gagnon-Bartsch et al. (2021) propose a framework arguing that "...misconceptions may persist: we channel our attention to data we deem relevant under our model, whereas it is the data we deem irrelevant that is most likely to contradict our model." According to Gagnon-Bartsch et al., to maintain a misunderstanding of the role of left-digit bias, chains must consider price endings irrelevant to demand. However, firms clearly did not dismiss the importance of price endings, as evidenced by their consistent use of 99-endings before the reform. They recognized that price endings could be relevant, yet did not invest sufficient effort to fully understand their impact.

Second, this paper examines which positive firm model can generate these behaviors. It presents empirical evidence that suggestes firms can be characterized as model-free decision makers with partial knowledge——a formalization that generalizes to many settings. Reinforcement learning is an effective method for solving complex problems and is a central tool in AI (Dayan, 1992; Sutton and Barto, 2018; Watkins and Dayan, 1992). Moreover, there is evidence that when firms explicitly employ these methods, they influence market outcomes (Assad et al., 2020, 2021; Calvano et al., 2020), and these ideas also appear useful in explaining phenomena in financial trading (Barberis and Jin (2021)). This paper provides suggestive evidence that model-free learning occurs at the firm level, leading to long-term model-free decision making. Consequently, the implications of this description of the firm warrant further investigation.

This paper offers a unique empirical opportunity that contributes 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, as exemplified in the studies by Doraszelski et al. (2018) and Huang et al. (2018). In Doraszelski et al. (2018), the ultimate bidding behavior of electricity providers is consistent with optimization, with their learning process framed as acquiring the parameters of the correct model. However, the authors find it challenging to explain firms' pricing during the first 1.5 years of data, presumably because the behavior was inconsistent with any stable beliefs about model parameters. Similarly, Huang et al. (2018) remain within the realm of model-

based optimization while studying retailers setting prices for liquor in Washington, noting that companies initially placed significant weight on recent outcomes——a pattern also consistent with reinforcement learning. In contrast, this paper proposes to adopt the perspective of model-free learning in such exercises.

Finally, the paper presents another example where firms do not optimize—both in the long-run and in the short-run—and offers an explanation for how that can occur. When firms are observed to mis-optimize, the explanations range from the addition of frictions and heuristics (DellaVigna and Gentzkow (2019); Hortacsu et al. (2021); Huang (2021); Strulov-Shlain (2023)), to principal-agent incentive misalignment in organizational economics (Hortacsu et al. (2021); Shapiro et al. (2021)), to management practices (Anderson et al., 2021; Bloom and Van Reenen, 2007, 2010; Bloom et al., 2013), to attention costs (Goldfarb and Xiao (2011, 2019)), or to wrong beliefs (Aguirregabiria and Jeon (2020); Hortaçsu and Puller (2008); Strulov-Shlain (2023); Xie (2018)). This paper further provides evidence that partial knowledge is a common——and in this context, necessary—component, with under-exploration acting as the mechanism that sustains it.

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 suggests two interpretations of the data, through the lenses of model-based and model-free learning. Section 5 introduces the fudge factor, discusses its justification, magnitude, and provides use cases. Section 6 concludes with a discussion of the implications of these findings for 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.[7] Israel abolished the 1-Agora and 5-Agorot coins (the Shekel consists of 100 Agorot) in 1991 and 2008, respectively.[8] 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.

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

[8] https://web.archive.org/web/20201127080757/https://www.boi.org.il/en/Currency/CurrentCurrencySeries/Pages/Default.aspx#Top

2.2 Data

The effects of the reform are manifested through the dynamics of price setting and their effects on demand. Therefore, I 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 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 the prices of a set of predetermined products. A "product" can be thought of as a UPC—of unique producer, weight, and size. ICC collects and posts 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 are a balanced panel, but stores identities are anonymized without chain identifiers, and prices are sampled monthly. More details are provided in Appendix Section A.

The pricing data are 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 be 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.

Finally, a third data set is a mix of daily and weekly scanner data, in which the main variables are the number of units sold and the revenue of each UPC in a store. Like in the ICC data, each store and chain has a unique identifier, though anonymous. Data are provided by StoreNext Ltd, a market research firm that collects transaction level data from store registers and aggregates it. The unit of observation is a product (UPC) in a store in a week.[9] 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

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

the analysis.[10] I merge these data with another sample of StoreNext's *weekly* data for another 13 products from 287 national supermarket stores covering 2013 to 2015.[11]

The scanner data are not ideal for precise price measurements due to the averaging of possibly different prices paid in a time period (see Einav et al., 2010; Strulov-Shlain, 2023). 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 after the 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 one 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 concept of left-digit bias and what it implies for pricing.

Left-digit bias is the tendency of consumers to put excess weight on the leftmost 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 than actual if they share the left digits. In turn, demand will slope downward with discontinuous drops when the left digit changes. The key implication of such a demand structure, i.e. downward sloping with discontinuities, is that prices to the right of the discontinuities should be avoided.

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,

[10] 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 unusual pricing behavior (see Hendel et al., 2017).

[11] 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.

2003; Bizer and Schindler, 2005; Sokolova et al., 2020; Thomas and Morwitz, 2005). Strulov-Shlain (2023) 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 and recently established in non-price settings (e.g., Lacetera et al. (2012); Busse et al. (2013); Li and Qiu (2023)) and recently extended to other demand settings by Hilger (2018) and List et al. (2023), 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 (2023), 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 θ on the price with a fixed focal price ending Δ.[12] 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

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

where $\lfloor p \rfloor$ is the floor of the price. Demand is a function of perceived prices, while 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 (denote 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 (2023), the 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

[12] Δ is in a sense a demand level-shifter, and bears almost no influence on pricing.

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:

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

Proposition 2 in Strulov-Shlain (2023) shows that given an elasticity $\epsilon < -1$, larger left-digit bias θ 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 θ_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 θ_0, solving for it numerically shows that for an extreme elasticity of -7 and $\lfloor P \rfloor = 2$, θ_0 is a mere 0.00075 (θ_0 will be even lower for less elastic demand and higher prices[13]). 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 θ_0 are higher in the post-reform regime. I find θ_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 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 θ in the data. The figure also overlays estimates from Strulov-Shlain (2023) showing that US retailers underestimate the bias, and as such, are getting closer to, but still above, the thresholds.

[13]Proposition 2 in Strulov-Shlain (2023) 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 $P - \lfloor P \rfloor$.

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 Results: Price Patterns and Demand Response

This section shows the effect of the reform on pricing and demand without interpretation, which is deferred to the next section.[14] 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.[15]

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 (from roughly 20% in 2014–2015 in the same sample).

[14] 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 can further reject several descriptions of firms' knowledge and decision processes, to be discussed in the next section, Section 4.

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

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.

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. Do the patterns seem more like planned experimentation or like adaptive 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).[16]

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 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. [17] 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

[16]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.

[17]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.

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.[18] 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 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.[19]

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

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

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 $p^{Modal99}_{is}$. The αs are the price-endings fixed effects, such that κ^{99}_{ist} equals 1 if the price in week t equals $p^{Modal99}_{is}$ in 2013 (0 otherwise), κ^{90}_{ist} and κ^{00}_{ist} equal 1 if the price is 10-Agorot lower (ends with 90) or 1-Agora higher (ends with 00) than $p^{Modal99}_{is}$, 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

[18] 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. (2023) use an RD design using the smooth distribution of prices which creates quasi-random distribution around 00-ending prices, while Dubé and Misra (2023) randomize prices in a field experiment.

[19] Strulov-Shlain (2023) tests for these drops in demand using larger and higher quality data, but lacks the random variation.

product-store on-sale dummy (to capture transitory sales effects[20]), 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 α coefficients capture the unexplained demand that is driven by the price-ending, controlling for the overall price elasticity. The most interesting comparison is between α^{90} and α^{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.[21]

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%--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.[22]

With an average elasticity of about -2.5, average price of 11, and 5%-8% drops correspond 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. Sayag et al. (2024) have replicated the methodology of this paper and with other data from an Israeli retailer estimated θ of 0.11-0.60 depending on sample restrictions used, with 0.22 as their main estimate.

The model and previous literature predicted no 00-ending prices, and the demand analysis suggests that 00-ending pricing was a losing pricing strategy. I quantify its magnitude in the next section.

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

[21]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.

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

4 Interpretation of Results

4.1 Framework

Now we interpret the findings in Section 3 assuming that left-digit bias is stable throughout the period and that firms *try* to optimize. 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 of 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 was priced at 90-ending it was unlikely to be priced at 00-ending later.

First, low shares of 00 and high shares of 99/90 before and long after the reform imply some acknowledgment of left-digit bias. Bunching at just-below prices and avoiding low-ending prices are manifestations of qualitatively correct pricing facing left-digit biased demand. Meaning that 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.

Second, a high initial share of 00-endings and negative demand consequences imply ignorance about the value of 90 versus 00. Meaning, while firms priced as if they were 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 fails 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 price changing 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 consistent with model-free reinforcement learning procedures.

Taken together, these patterns show that we cannot explain the results using maximization of profits with a model of persistent left-digit biased demand. Two consistent explanations are driven by partial knowledge of the structure of demand. One is maximization of a wrong model that is not explored to be refuted; another is model-free partial learning (i.e., not enough exploration) which leads to choosing the believed best action while not updating beliefs about other actions.

4.1.1 Model-based and model-free learning

Model-based decision making is the consensual tool for 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 under constraints. 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. It is rare to consider model uncertainty, hence the goal of learning in a model-based world means solving the uncertainty about the parameters of a known model. For example, when a monopolist sets a price to maximize profits, it needs to know the parameters of the problem, which are the price elasticities and marginal costs of the products it sells (and potentially left-digit bias).[23] 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 result 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 believed 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 learning, in model-free learning, the agent has some beliefs about the value of different actions given the state of the world. The beliefs are updated 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 that a company expects to make $10 in revenue 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 without a model it will not change its expectations about the value of pricing at $1. That is, model-free learning informs the value of actions directly. Indeed, inferring from one action to another is not model-free, it requires some form of a "model", one that allows us to interpolate and extrapolate.

Rejecting a model rather than updating parameters is challenging conceptually and in practice; updating beliefs fully requires sufficient exploration[24] . Therefore, whether firms are model-based with the wrong model or under-exploring model-free decision makers, they can sustainably make sub-optimal decisions.

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

[24] 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 and Dayan, 1992). Distinguishing between model-based and model-free learning processes requires some clever manipulations (Daw et al., 2011; Kurdi et al., 2019).

4.2 Can Wrong Beliefs Explain Pricing?

In this section I attempt to come up with a descriptive model of firm behavior that reconciles all data patterns. If firms strive to maximize some normative Π, what is the smallest deviation descriptive Π' that captures their behavior.

First, 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 optimal. However, there are no beliefs about left-digit bias that create both excess mass of 99-endings before and excess mass of 00-endings after.

In principle, left-digit bias might support 99-endings versus 00, but not 90 versus 00, because of the stronger trade-off. However, as Figure 1 shows, these beliefs about left-digit bias and elasticity must be extreme. Yet, we can estimate beliefs about left-digit bias before the reform, using the same Method of Simulated Moments as in Strulov-Shlain (2023), and see if they are consistent with 00-endings after the reform.

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.[25]

Although the perceived left-digit bias is much smaller than the demand-estimated level, it is still high enough to prevent 00-endings. I estimate that the perceived left-digit bias ranges between 0.02-0.04 while the threshold values of left-digit bias that can support 00-ending prices 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 is 0.22-0.36 (e.g, the impact of a 1c increase from 99-ending to 00 is like a 23-37 cent equivalent price change). The finding of underestimation of bias is common in the US market as well (Strulov-Shlain, 2023), and possibly not surprising by itself. However, estimated beliefs before the reform are inconsistent with pricing behavior after the reform under the null of knowing the shape of demand.[26]

Finally, an *excess of both* 00-ending prices and 90-ending prices is in contrast to any belief of left-digit bias. There is no level of left-digit bias that can justify both at the same time. Excess 00-endings require beliefs about the shape of demand that imply an advantage of crossing the

[25] Appendix section C is copied from Strulov-Shlain (2023).

[26] The finding that after the reform, the 00 ending rarely follows the 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).

leftmost digit (a reverse left-digit bias). In contrast, pre-reform pricing is only consistent with beliefs of positive left-digit bias. Meaning, describing firms' behavior as if they optimize against left-digit biased demand is a poor description of reality.

4.3 Forgone profits

I wish to capture the behavior of firms in a way that will allow us to estimate how far they are from optimization. That is, compare actual pricing decisions to counterfactual optimal pricing decisions had demand shape was known perfectly. Doing so requires two components: how firms price in practice and how should they price. Unlike flagging some prices as a mistake, which only require minimal and reasonable discontinuities in a downward sloping demand, this exercise requires more assumptions and a full model. In order to get more realistic estimates, I use a logit demand structure with two products. I simulate costs and preferences (product utility intercepts, price sensitivity, and left-digit bias parameter) and find, numerically, the optimal prices for each such combination.[27]

First, I provide a purely descriptive model of the firm's pricing rules. The rule is an extension of the "rule of thumb" model from Strulov-Shlain (2023).[28] First, given the marginal cost and elasticity, the firm calculates the optimal price as if there were no left-digit bias (according to Lerner's rule). Second, if it is "close enough" to a round number, then sometimes it rounds it to the nearest 99-ending price. The extension is that I allow with some probability to round to 00, not just 99. Therefore, it can do what left-digit biased optimization models cannot do, and generate both 99- and 00-ending excess masses. The model therefore has parameters of the probability of rounding to 99, to 00, and the range of "naive" prices that are rounded which gives a predictive function $p^r(\cdot)$.

I fit this model to the price distribution month-by-month, and as expected, it fits the data better than pricing facing left-digit biased demand. From that exercise I get monthly values of the pricing rule parameters $\hat{\omega}_t$. I use the same cost distributions and preferences as in the optimal pricing case, but instead set θ to 0 to find the naive "optimal" prices and then use the monthly pricing rule parameters $\hat{\omega}_t$ to weigh predicted profits from keeping the price as it is, changing it to 99/90, or changing it to 00.

[27] I chose intercepts of 1 and 0.5, price sensitivities of -0.5, left-digit bias of 0.22, and drew 1000 costs for product 1 from a Weibull distribution with shape of 4 and scale of 6 (these numbers were calibrated in a trial and error process to generate prices and average elasticity close to the demand data and estimates). Then sampled from the sample again to get 50 costs for product 2. Finally, I sample 1000 sets of parameters and costs and estimate the price for each row twice - once when prices are to the cent pre-reform, and once when they are to the dime, post reform

[28] This exercise is similar to one in Strulov-Shlain (2023), and I use the code from that paper, with the above mentioned modifications, to run it.

From that I calculate the forgone profits as the ratio between profits with the rule-of-thumb prices p^r_{it} and the optimal price p^*_{it}.[29] Hence the forgone profits, in relative terms, are

$$X = 1 - \frac{\sum_i \Pi(p^r(\varepsilon, c_i, \hat{\omega}_t); \varepsilon, \theta, c_i)}{\sum_i \Pi(p^*_i(\varepsilon, c_i, \theta); \varepsilon, \theta, c_i)}$$

The results of this exercise are shown in Figure 5. Before the reform, the forgone profits hover around 2% before the reform, and then increase to 2.75% at the time of implementation, and gradually decline to 1.7% by 2015 – the firms might have ended learning more than they knew before the reform.

These numbers are high, though lower than Strulov-Shlain (2023) (probably due to using the Logit demand structure rather than single-product monopolistic market). Nonetheless, I do not take them at face value, but as a ballpark estimate. The core phenomenon of distorted responses to prices with discontinuities implies a meaningful impact on profits.

In summary, analyzing the pricing patterns following the reform leads to the conclusion that firms did not price optimally against the true model of demand. They did not have a stable belief of left-digit bias, or any coherent belief of left-digit bias. However, if we describe their behavior as following heuristics, of sometimes rounding to 99/90 and sometimes to 00, we can capture their behavior well, and can quantify the share of gross profits they could have made but did not make, over time. We find that even in the best of times profits are somewhat lower than optimal, and the reform response led to a significant increase of forgone profits followed by a learning period of about a year which ended up leading to better performance.

That is, firms seem to constantly be pricing suboptimally due to an incomplete or incoherent model of the world and heuristic pricing, which leads them to greater losses once the environment changes fundamentally, but also jolts them into learning.

5 Fudge Factor

The findings show a gap in optimization, associated with prolonged partial knowledge, and poor initial response to changes. The reason we can quantify these gaps in this paper is thanks to the features of left-digit bias which create sharp predictions. In most cases, however, we cannot quantify deviations directly and have to assume what they are, almost always assuming there are no deviations. If researchers try to analyze companies' behavior under the wrong assumption of full knowledge and optimization, it would lead us to draw wrong conclusions. That is, we

[29] The profit, in any case, is determined by the actual price charged, p, and the parameters of demand and costs:

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

need better positive, rather than normative, models of firm behavior in order to draw correct conclusions.

This paper argues for one reason of deviations from full optimization, but companies may not adhere to the researcher's normative benchmark for a multitude of reasons. So the purpose of the current proposal is a method that will be agnostic to the source.

At the same time, we wish to maintain the hypothesis that companies strive to optimize, even if they fail to do so perfectly or are constrained in ways not observable to the researcher. This assumption is both sensible and useful.

I suggest a method to capture and test of the sensitivity of the results to a general form of deviation from the best guess of the normative benchmark. A *Fudge factor* $X \in [0,1]$ captures the percentage of potential best-guess profits (including frictions) that the firm forgoes. Given X, we ask what unobservables are consistent, under the model, with the observed behavior such that the observed behavior brings at least $1-X$ of the optimum. Formally, assume that the objective function is some $\Pi(p,a)$, where a is a state variable and p is an action the firm can take to maximize Π. Assume that a is the unknown parameter of interest. The standard assumption is that an observed p is a function of a such that $p^* = \mathcal{P}(a) = \arg\max_p \Pi(p,a)$, which then allows finding $a = \mathcal{P}^{-1}(p^*)$. What I propose is to assume that the observed price p is not the maxmizer but a partial-maximizer, and find all a such that

$$\Pi(p,a) \geq (1-X) \cdot \Pi(\mathcal{P}(a),a) \tag{5}$$

That is, assume that p is consistent with partial optimization such that the profits under a and p are at least $1-X$ the profits under the same a and an optimal $\mathcal{P}(a)$.

The fudge factor is similar to the Critical Cost Efficiency Index (CCEI) pioneered by Afriat (1972) and extended and elaborated by others (Choi et al., 2014; de Clippel and Rozen, 2021; Halevy et al., 2018; Varian, 1990). A similar approach was suggested by Chetty (2012) as a way to reconcile micro and macro labor supply elasticities. Afriat's idea was to think about differences in productivity between firms, given a known production function. The CCEI then measures how far, in terms of production efficiency, a given firm is from the frontier. Similarly, he introduced an index for violations of GARP (the General Axiom of Revealed Preference) to measure how "far" are a set of choices from being able to be represented by a utility function. The follow up work then took this individual choices index and expanded it to measures in terms of budget constraints and other money metrics. For example, Halevy et al. (2018) treat the index as a loss metric that allows to do model selection based on minimizing this loss. Specifically, to find the model that best describes participants' behavior, we seek to find the model that minimizes out-of-sample loss, as measured by this index. Another similarity is with the computer science and operation research literature, which often deals with complicated optimization problems. In these problems, which cannot be fully solved, the solution concept is often an approximation. A proposed algorithm is then judged

on the basis of how well it approximates the full solution or by minimizing the maximal regret. In one way or another, firms often face complicated problems and it is almost necessary that they only approximate their solutions rather than fully optimize them. Unlike this literature, I assume that the correct model is known, and ask either what are the implications of a known X on the range of recovered parameters, and how sensitive are conlusions to varying levels of X.

As noted above, different reasons may lead to $X > 0$. For example, a problem that is too complicated to solve routinely will lead to approximations (Camara, 2021; Hortacsu et al., 2021); partial knowledge would make it unlikely to find the optimal action (this paper or Bloom et al. (2013); Dubé and Misra (2023); Hitsch et al. (2017); Zhang and Misra (2022)); lack of experimentation (Berman and Israeli, 2022; Koning et al., 2022); organizational frictions (Rao and Simonov, 2019; Shapiro et al., 2021); psychological biases of managers (Malmendier and Tate, 2005, 2015); low belief in the importance of an attribute will lead to potentially large Xs (Gagnon-Bartsch et al., 2021); and many other reasons exist.

Therefore, the idea of X is to capture a whole host of explanations, and its value will be determined by the specific setting. For example, the more we know about the way a company operates, our model of its behavior will be better, and X will be smaller. Similarly, we might believe that in more competitive environments, or for standard, high feedback, important, frequent decisions X will be smaller.

How big is X? In most cases researchers assume optimization because it is impossible to identify deviations from it. In this paper it is estimated as about 2% of profit, and several other examples should move our priors away from $X = 0$. For example, Blake et al. (2015) find ROI of -63% for eBay Search Engine Marketing expenditures, and Shapiro et al. (2021) study well established CPG manufacturers and find that the median ROI from TV advertising on a given week is -88% and the overall ROI of advertising is -57%. Given typical shares of advertising expenditures, these lead to a loss of a few percents of the overall profit (if these were the only mistakes these companies were making); Dubé and Misra (2023) find that ZipRecruiter's pricing before their pricing experiment led to 35% lower profits compared to the optimal uniform price; Zhang and Misra (2022) find that before randomization coupons sent by a food delivery platform led to 58% lower profits compared to the best uniform promotion depth; DellaVigna and Gentzkow (2019) estimate a profit loss of approximately 10% from uniform pricing across supermarket stores; Hortacsu et al. (2021) find that organizational structure and pricing heuristics lead to 13% revenue loss compared to a unified optimization benchmark; Cho and Rust (2010) demonstrate that rental companies forgo 20%-30% of profits with their fleet management practices. Finally, two closely related papers show the importance of left-digit bias in pricing: Strulov-Shlain (2023) finds that the partial response to left-digit bias causes large US supermarket chains to forgo several percents of gross profits; and List et al. (2023) show that Lyft was completely ignoring the importance of left-digit bias and could have made $160M more in profits and then show that this is the case in a field experiment. Although the lost profits are not the main point of some of these papers,

these are a selected sample. In general, across companies and time, it seems likely that X should be closer to 0 than these papers find. However, together these papers document, for a myriad of reasons, instances where X was much higher than 0 for many sophisticated companies and for sustained periods of time. Therefore, acknowledging the possibility of a positive X in any given analysis seems justified.

For example, assume that a monopolist faces a profit function $D(p)(p-c)-F$ and the parameter of interest is c. Therefore, $D(p)\cdot(p-c)-F \geq (1-X)\cdot(D(\mathcal{P}(c))\cdot(\mathcal{P}(c)-c)-F)$. Although in general this inequality is not analytically solvable, it provides an implicit function that can be solved numerically. Further, if Π is convex the solution will be a segment of costs.[30]

As a toy example, let us assume a constant elasticity of demand without fixed costs and constant marginal costs. An $X = 0.05$ means that the firm acts with at most 5% of unexploited profits. Therefore, if we see a monopolist setting a price of \$1.00 and estimate a price elasticity of -2, then instead of determining that the marginal cost is exactly \$0.50 (as it is under optimization), we get a range of costs $[0.41, 0.58]$. For each cost in this range, a price of \$1 provides at least 95% of the profit as with otherwise optimal prices. Therefore, allowing for a 5% friction in optimization leads up to an 18% deviation in inferred costs, markups of 42%-59% instead of exactly 50%, and consumer surplus that may be 18% lower than if the firm were optimizing or 29% higher. Figure 6 explores the sensitivity of consumer surplus to various elasticity levels and fixed costs and shows how in a constant elasticity, the effects are highly non-linear and steep, and become stronger for higher fixed costs and more elastic demand.

As another example, I demonstrate the relationship between the fudge factor and inferred true left-digit bias and elasticity in the supermarket pricing setting. If we only observe prices, without quantities, we can infer firms' perceived left-digit bias, and ask what levels of actual left-digit bias are consistent with up to X% of unexploited profits. Or, ask the dual equivalent of how big should X be in order to be consistent with some level of true bias. Figure 7 shows for three different perceived bias levels the relationship between X and the possible true bias. For example, if the firm is forgoing 2% of profits, then the real bias may be up to 0.24 if we observe the firm acting as if the bias is 0.03, or the real bias may be up to 0.1 if the firm prices as if there is no bias at all.

The exercise above is asking: If we estimate beliefs, what might be the truth that is consistent with some level of fudge X. We can also ask the complementary question: If we estimate demand elasticity and left-digit bias, what firm beliefs will be consistent with an assumed factor X. Figure 8 shows these two exercises for both left-digit bias and elasticities. Panel (a) addresses the first question: We estimate beliefs from pricing behavior (the white dot) - what true values will lead

[30] For example, if $F = 0$ and $D(p) = A - Bp$, then $\mathcal{P}(c) = \frac{A+Bc}{2B}$ and we can find an analytical, albeit unappealing, solution for the range of costs c that will rationalize an observed p given known demand parameters A and B and a fudge factor X:

$$c \in \left[\frac{2Bp - 2(A-Bp)\sqrt{X} - A(1+X)}{B(1-X)}, \frac{2Bp + 2(A-Bp)\sqrt{X} - A(1+X)}{B(1-X)} \right]$$

these beliefs to be consistent with losses of up to 1%? 2%? etc. Panel (b) addresses the mirror question: If we know the truth (the purple dot), what might we expect a firm to believe if we allow it to be wrong up to 1%? 2%? etc. For example, we see that being wrong about demand elasticity is more impactful on profits than being wrong about left-digit bias. The sensitivity is greater on the elasticity dimension. Similarly, we also see that even substantial under-appreciation of the bias can lead to small losses; however, when left-digit bias is being close to ignored, the impact becomes meaningful.

Another use case is to consider the smallest X that will revert counterfactual conclusions. For example, assume we are interested in changes in markups over time, based on estimating price elasticities and assuming optimization. We then infer an increase markups due to lower elasticities. A more sophisticated version of the toy example above can allow one to determine that if X is greater than some threshold, constant markups cannot be rejected. If this threshold is 1%, 10%, or 30%, it will tell us how sensitive the finding is to the full optimization assumption.

6 Discussion

I find that following a pricing reform, supermarket chains acted in a way inconsistent with full knowledge which led to clear mistakes. The host of evidence on the dynamics of price setting before and after the reform, and our priors and evidence on demand response, suggest that firms were facing left-digit biased demand, yet acted inconsistently and did not take it fully into account. This paper joins other papers in documenting sub-optimal behavior and suggests that sustained partial knowledge leads to this gap in behavior.

How do firms preserve partial knowledge? One potential mechanism is not enough learning and exploration. Exploration may be limited due to constraints and opportunity costs (exploration-exploitation tension), but that should not lead to big mistakes in the long run. Other forms of under-exploration can lead to arbitrarily large mistakes. These can be sustained, as in Hanna et al. (2014); Gagnon-Bartsch et al. (2021); Schwartzstein (2014), if decision makers do not think that some margins matter and never try to learn if they do. In this paper, I document that they have some awareness to the influence of price endings on demand, but they are not trying to figure out the exact form of this influence.

Critiques There are several limitations to this study. First, it is merely a single example. One objection might be that this is not a representative case because price endings do not have a big effect on pricing. In other words, it is an irrelevant example and the counterfactual effects on profits are overblown. This is hard to argue against, since I do not have a validation and have to rely on structural form assumptions to quantify the effects. 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 has large effects on profits according to some papers (List et al., 2023; Strulov-Shlain, 2023), and the retailers themselves noticed that something went awry in early 2014. Consider this quote, by the CEO of Mega, Israel's 2nd largest chain at the time: *"There was a 2%-3% decrease in revenues on Jan-Feb ... No one can explain why that happens ...I'm in business for 25 years, and had never seen such months"*. Finally, they also changed their price ending behavior, and their learning suggest it mattered. Furthermore, the fact that left-digit bias might be viewed as unimportant supports the persistent under-exploration hypothesis. The most extreme example of perceived unimportance is in List et al. (2023), in which a (very sophisticated) company, Lyft, is not aware of the existence of the bias, completely ignores it in its pricing decisions, and loses money for it (following the research, the firm and its competitor, Uber, had changed its pricing to be mostly 9X-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, as mentioned in Section 5. Furthermore, partial learning can help answer the common question raised in those papers of "how is it possible that firms are persistently not optimizing."

Another limitation is that other normative models can be created to rationalize the data. For example, if a firm has beliefs about left-digit bias that are state-dependent, including that positive and negative bias can exist at the same time, then any pricing behavior is possible—if at periods of change firms revert to a null of "anything goes" (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 notion of a "model" loses its bite.

While some readers might find these conclusions hard to accept, others might respond that firms' misoptimizing is obvious, but optimization is a useful assumption.[31] I concur with both claims - I personally believe that firms try to optimize but face a lot of challenges. Therefore, to preserve the assumption of attempted optimization, I suggest adding a "fudge factor" in analyses in order to test the sensitivity of assumptions to deviations from the researcher's favorite normative or best descriptive model.

If firms are partial optimizers, it means that they may act in a persistent way in steady state, without knowing in advance how to respond to new regulations or changes in the market place. A change in the market, such as the one in this paper, might jolt companies out of their steady state and into a learning mode (Gagnon-Bartsch et al., 2021) or may push them further or closer

[31] In an episode of the Freakonomics podcast discussing Shapiro et al. (2021), the economist Steve Levitt says "any economist who tells you that firms are profit-maximizing has never worked with firms. That's a simple model we use when we teach beginning economics because it's easy to solve mathematically."

to optimization if they do not learn more. These changes occur a lot: New establishments open and close, rivals enter and leave, tastes shift, trade agreements change, natural disasters and pandemics happen more than we wish, and costs fluctuate. Treating firms as all-knowing is bound to miss many interesting dynamics and potentially lead to wrong conclusions. Researchers should incorporate a potentially lengthy learning period, and should think about learning ending prematurely and sustained sub-optimization.

In conclusion, there is ample room for future research. Empirically, more examples and tests of partial knowledge should be explored; the effects of market forces on firms' suboptimal behavior and learning is a topic of my own current work. Methodologically, the usefulness of the fudge factor can be studied and challenged. Theoretically, what are the implications of incorporating partial knowledge and optimization into problems of mechanism design, on how regulation might affect markets, or on welfare?

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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

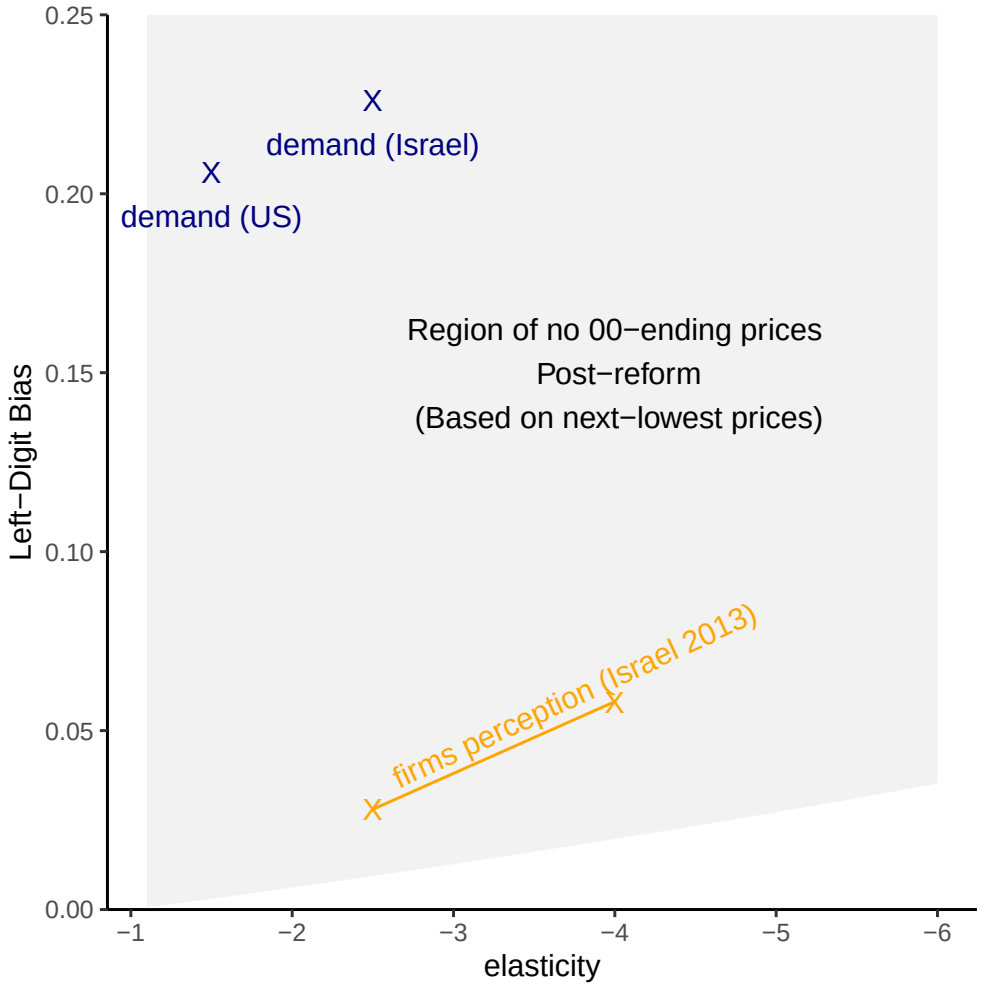
The table shows summary statistics of the three datasets used in the paper, in their raw and cleaned form. ICC, Israeli Consumer Council, collected a panel of prices at the item-store-biweek level. CBS, Israel's Central Bureau of Statistics, collected prices for the consumer price index at the "product 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.

Table 2: Price-ending effects

	Quantity (log-units)			
	(1)	(2)	(3)	(4)
α^{90}	−0.029	0.006	−0.037	−0.018
	(0.024)	(0.033)	(0.024)	(0.023)
α^{00}	−0.082***	−0.077**	−0.095***	−0.104***
	(0.028)	(0.032)	(0.027)	(0.029)
α^{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

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.

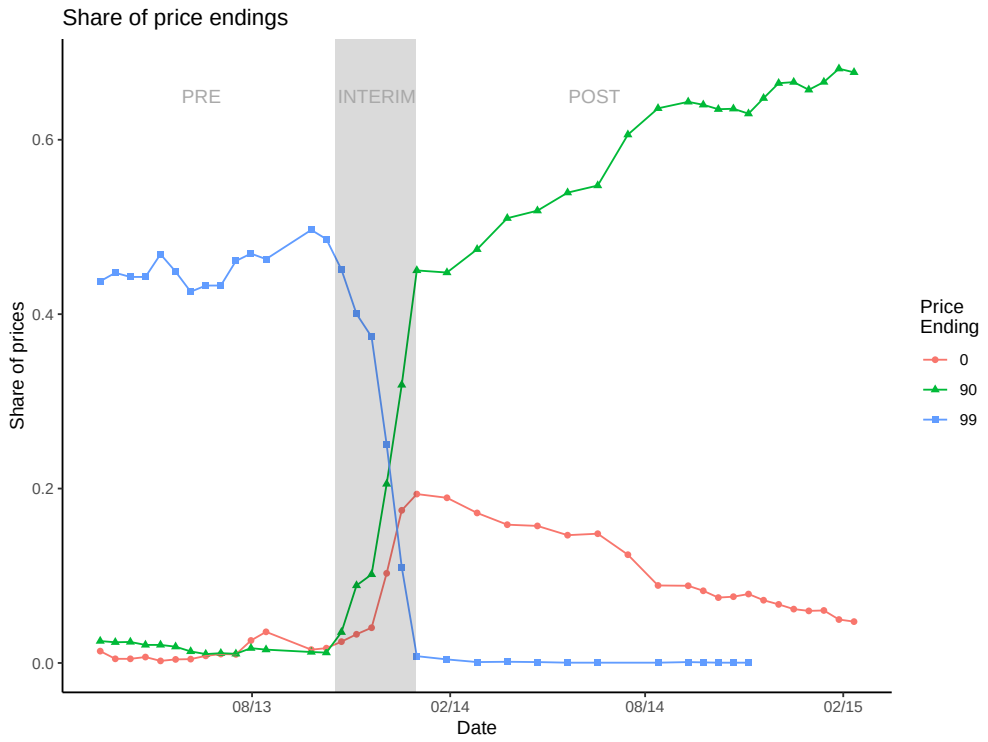
Figure 1: Minimal levels of left-digit bias θ_0 (ϵ) above which 00-ending is never optimal



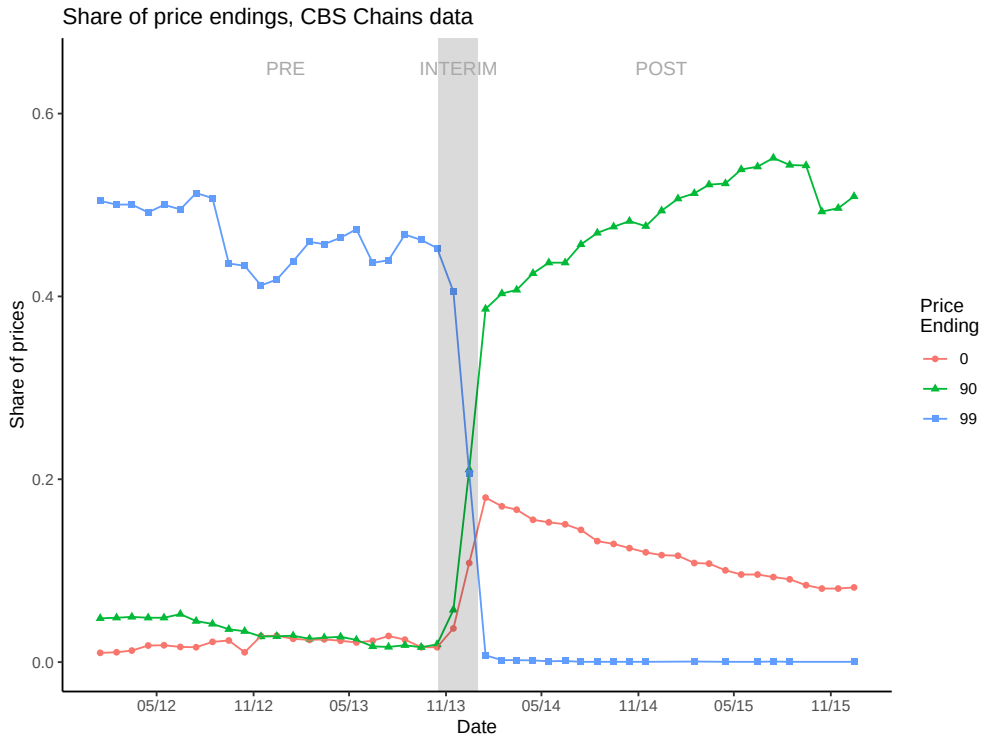
The figure shows the minimal levels of left-digit bias θ above which 00-ending prices are never optimal. The values are a function of the price elasticity and nominal price level. The dark line shows the threshold values in the pre-reform case (when 99-ending and 01-ending prices were allowed), and the 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 (2023).

Figure 2: Shares of price endings over time

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

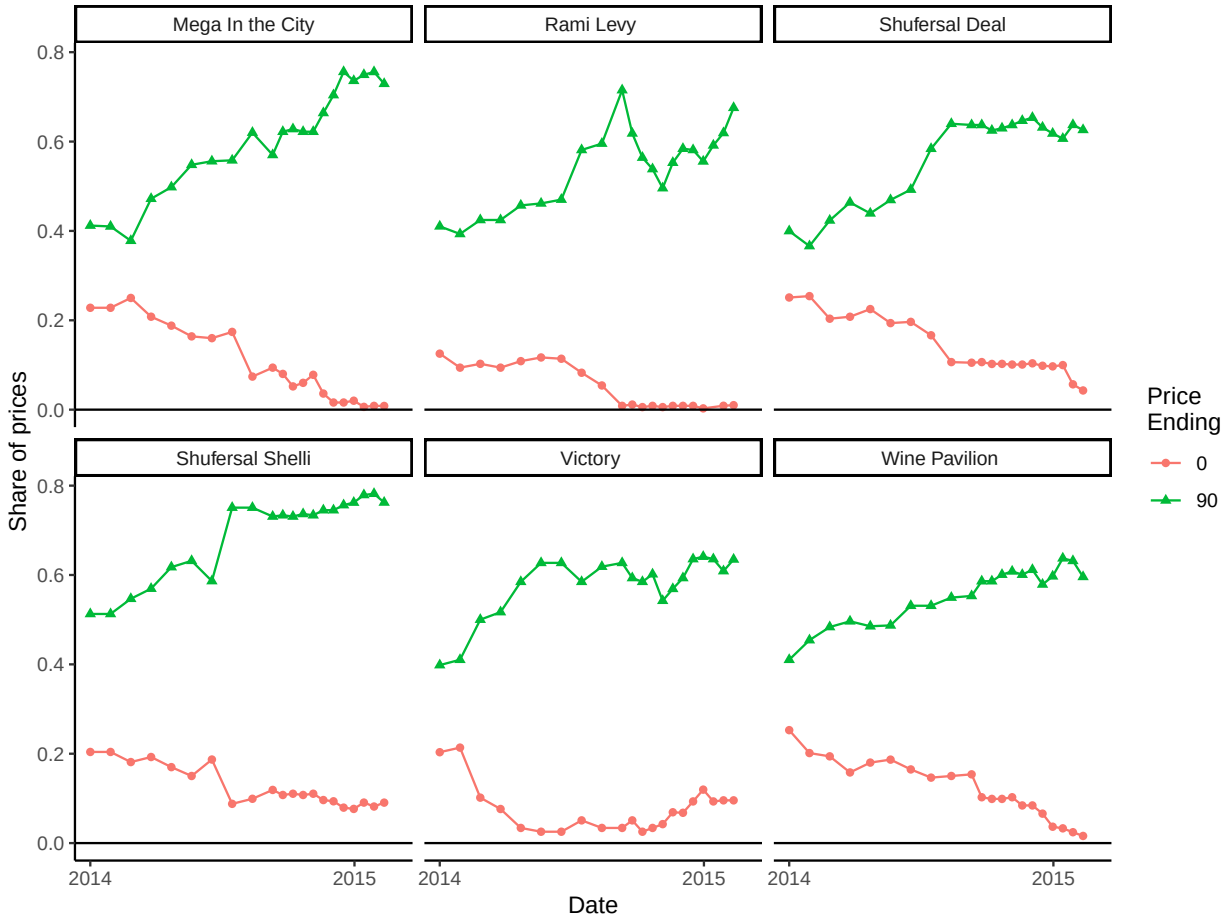


(b) Shares of products that end with 99, 90 and 00, CBS data



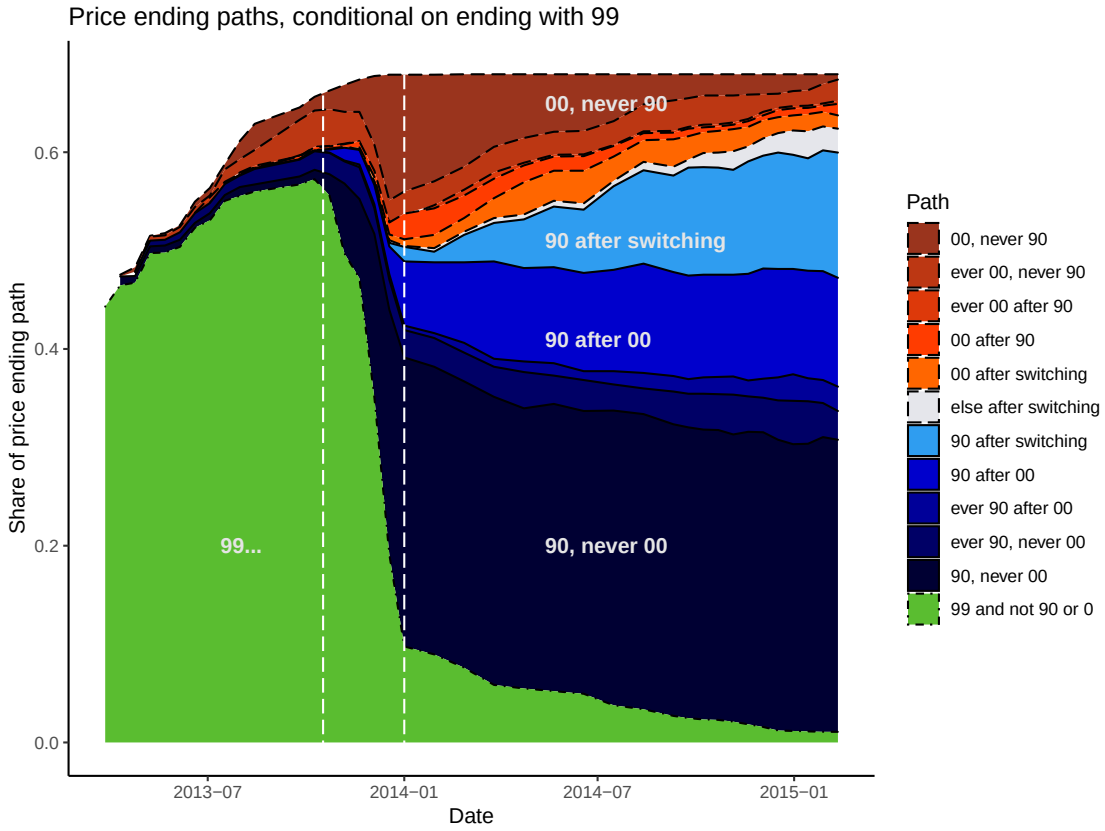
The figures above show that shares of 99-, 90-, and 00-ending prices across products and stores by sampling period. The shaded area represents the time between announcement of the reform on October 17, 2013, and its enactment on January 1, 2014. Squares represent 99-ending prices, triangles 90-ending 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.

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.

Figure 4: Price ending paths of products that 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.

Figure 5: Estimated counterfactual forgone profits month by month

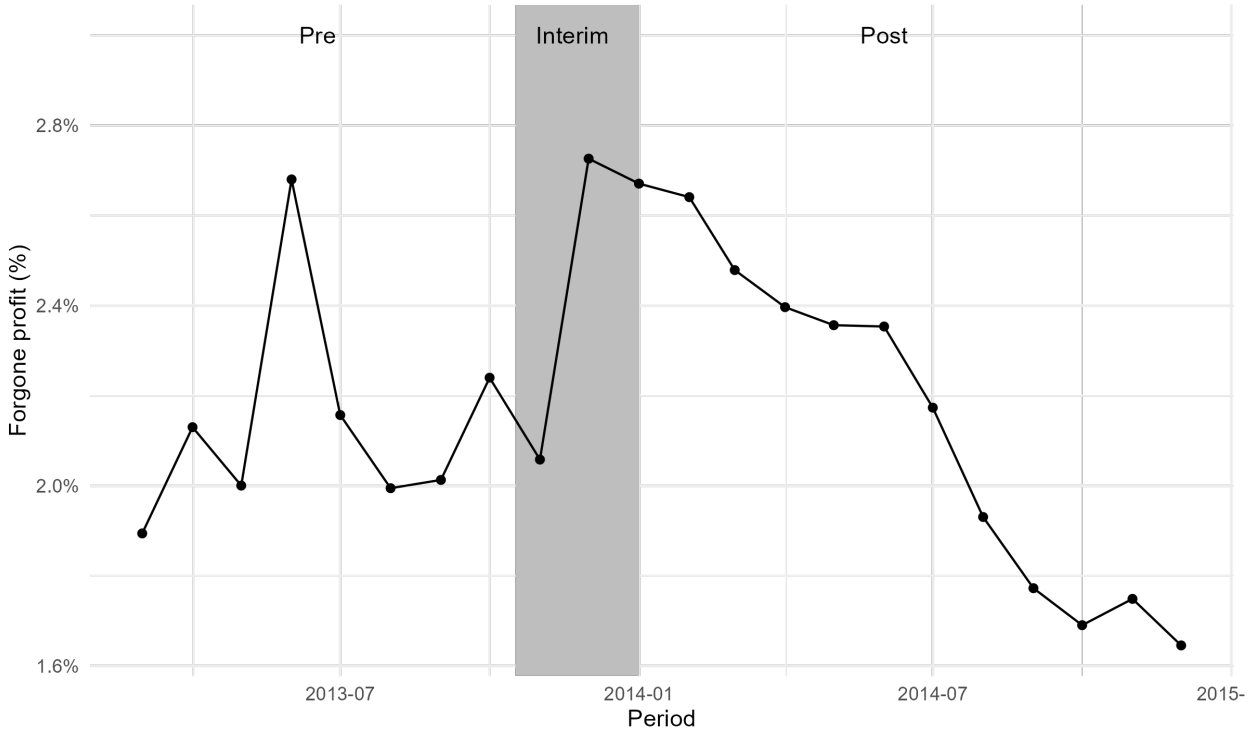


Figure shows counterfactual profits under a monthly estimated predictive rule-of-thumb pricing relative to full optimization.

Figure 6: Illustration of the effect of fudge factor on possible consumer surplus levels

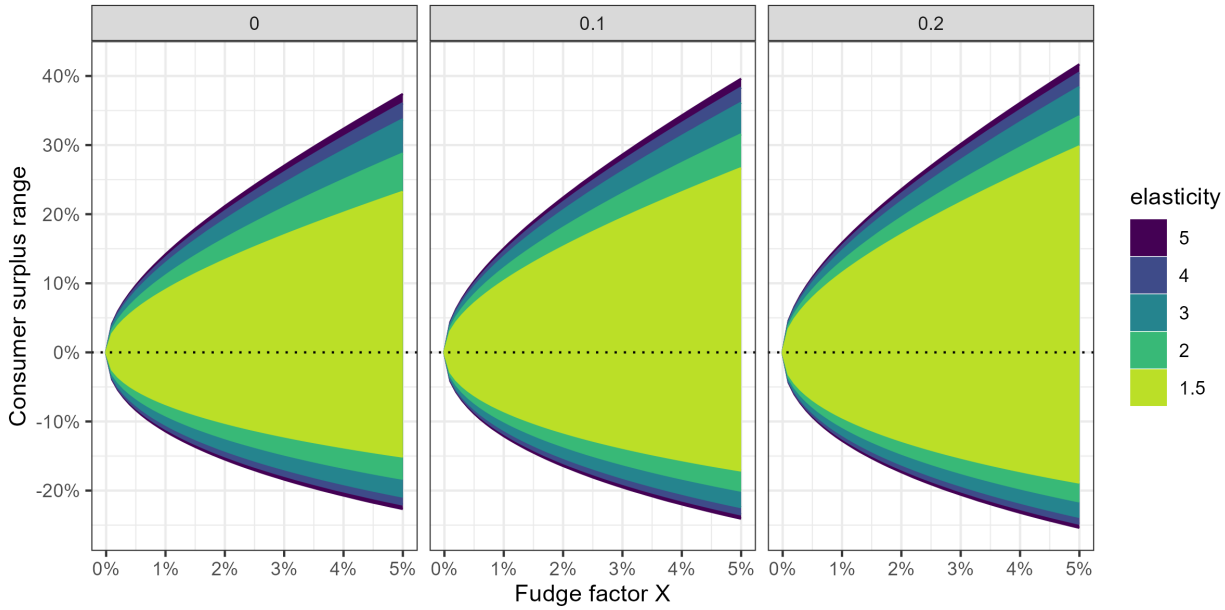


Figure shows the effects of a fudge factor on possible values of consumer surplus relative to full optimization. Each shade is a different price elasticity, and each panel has a different fixed cost. The model solved is $\Pi(p) = p^{-\epsilon} \cdot (p - c) - FC$ and assuming the observed price is \$1.00 and that c satisfies $\Pi(p = 1; c) \geq (1 - X) \max_p \Pi(p; c)$.

Figure 7: Relationship between true left-digit bias and the fudge factor given observed perceived left-digit bias

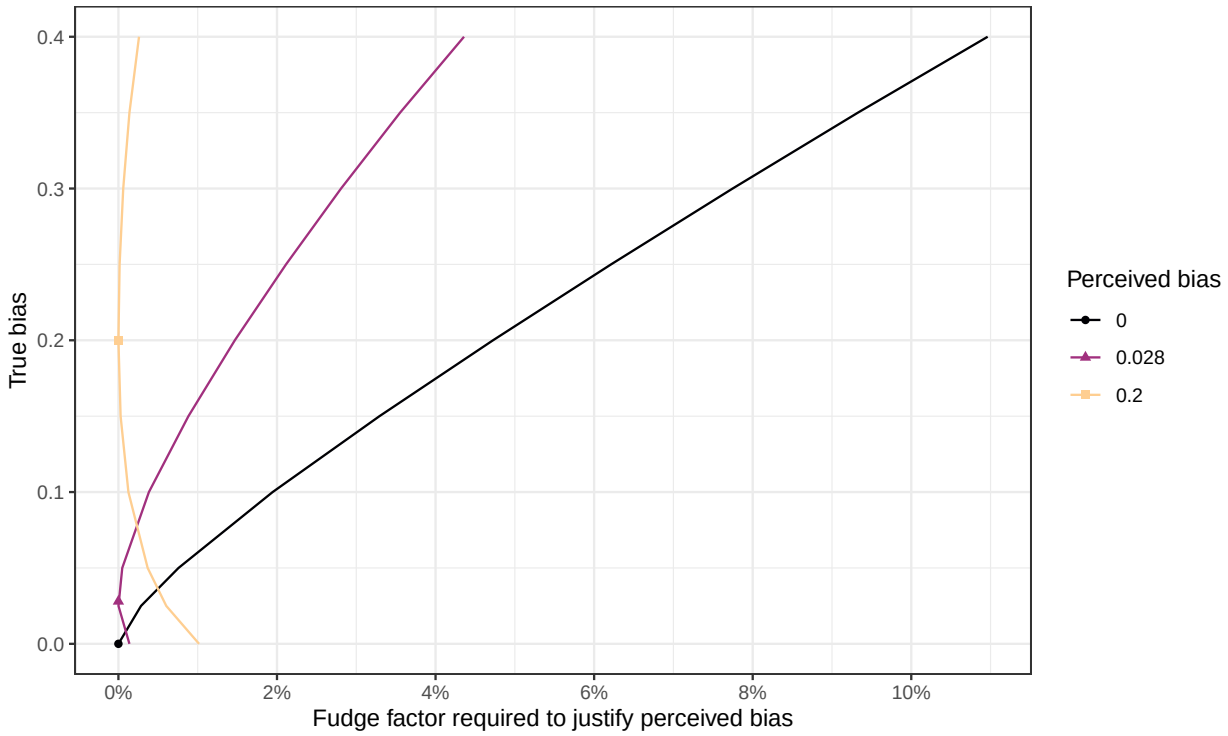
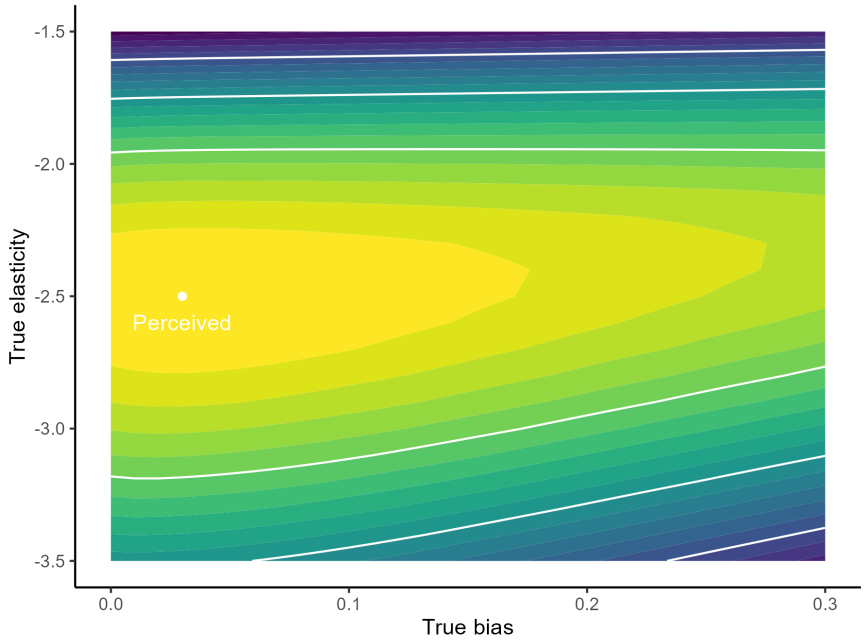


Figure shows the ranges of explained true left-digit bias on the vertical axis if the firm prices as if the bias is 0, 0.028, or 0.2, for every level of Fudge factor (the horizontal axis). This is under the assumption that the true price elasticity is known to the firm and is -2.5.

Figure 8: Relationship between true and perceived parameters and associated forgone profits (fudge factor)

(a) Fudge factor associated with real values given perceived



(b) Fudge factor associated with perceived values given real

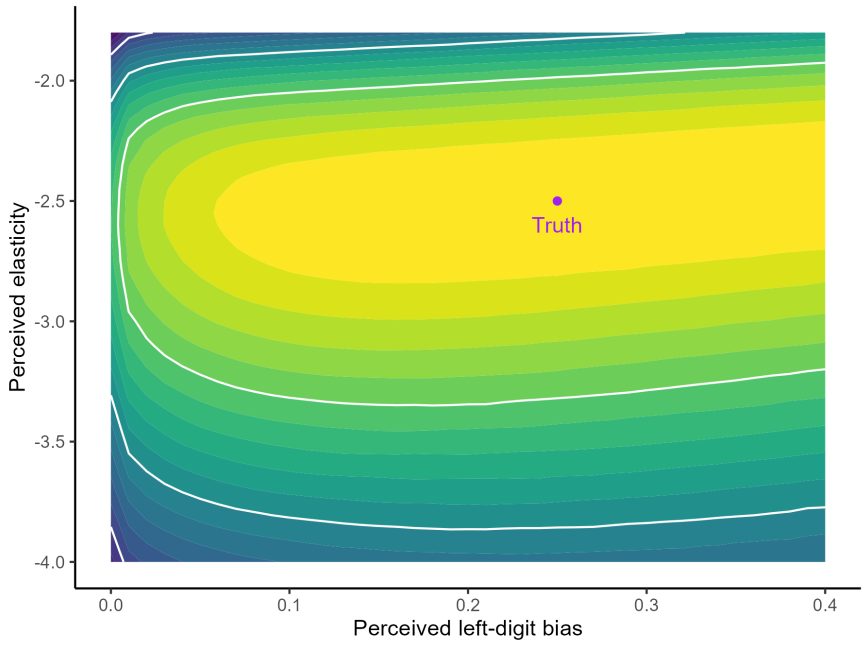


Figure shows for different combinations of real and perceived parameters the associated forgone profits. The top panel fixes the perceived values governing firm behavior (white dot) and shows how big are forgone profits if the true values were different. The bottom panel fixes the truth (purple dot) and shows how big are forgone profits if the firm acts according to perceived values. Every shade is a 1% increment in losses. The white lines designate 5% increments.

Online Appendix, Not For Publication

Firms have Partial Knowledge and they Partially Optimize: Evidence from a Reform

Avner Strulov-Shlain

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

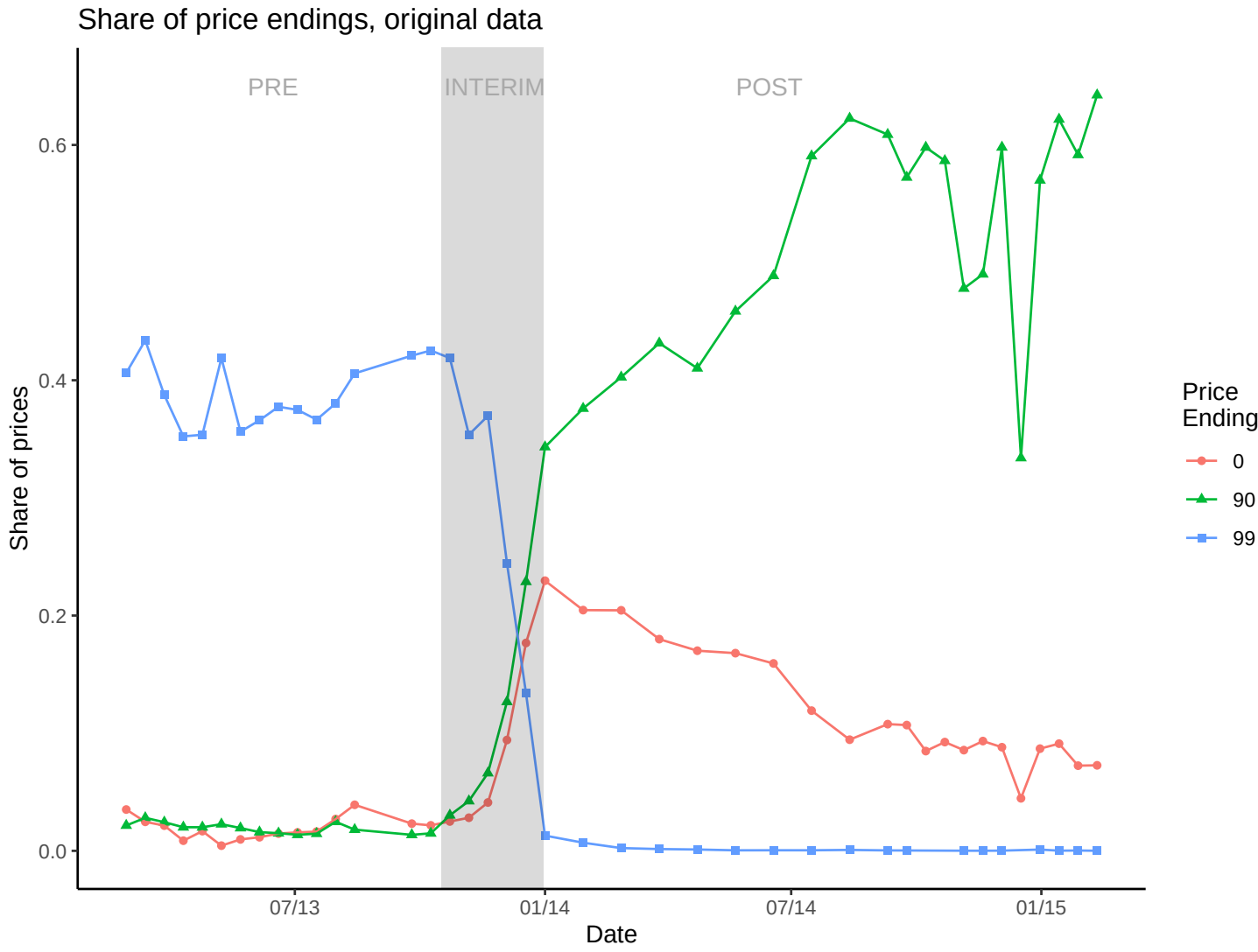
ICC data are collected by surveyors going to a set of predetermined stores with a product list. Each product is equivalent to a UPC. The surveyors record the shelf price of the item, and the total basket price was posted on the ICC website. The micro-data was generously provided to me by the ICC. The 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 1a 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 (2023)), 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[32]. 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)[33]. To be confident that the weekly price represents the true price that consumers paid, I keep observations where the price is indeed

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

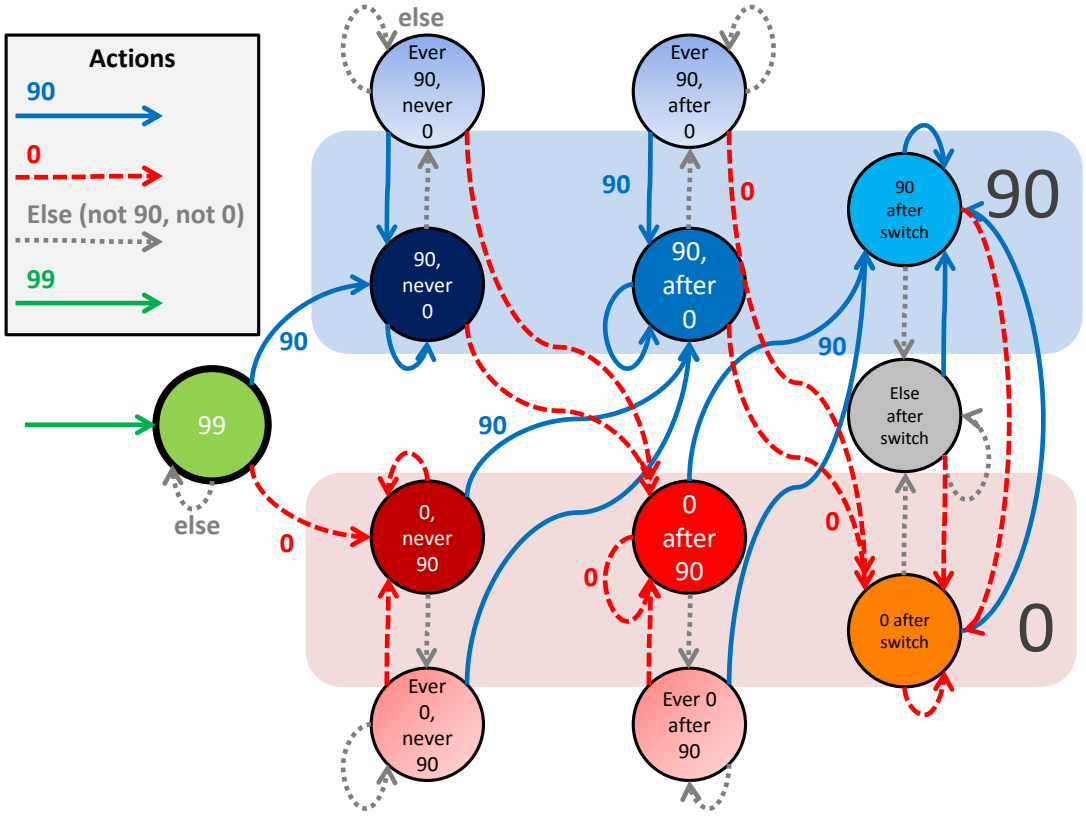
[33] To create a balanced sample of product-stores, I keep product-stores pairs where there are any sales for at least 80% of the total weeks, and for 90% of the weeks of the pre-reform period of 2013. I then calculate for each price sequence of a product in a store the length of same-price spells, deeming 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).

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[34]. 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

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}.

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

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[35]. The correlation is stronger if different 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-2a 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-2c, there were more 00-ending prices for some products, and mostly for items with lower pre-reform shares of 99-endings. Finally, Figure A-2b 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[36]. 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 competing stores within a radius. For example, above and below median within a 5 km radius[37]. I then compare the differences in shares between stores above and below the median of spatial competition. Figure

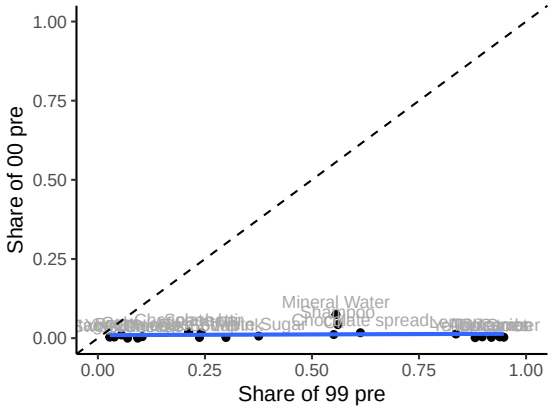
[35]What does it mean to have bias heterogeneity between products? One explanation is that different clientele purchase different products, another is that other characteristics of the products receive more attention than the price (such as observable quality in fresh produce), or that items purchased in bulk or multiple units require price multiplication which puts extra weight on the left-most digits.

[36]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.

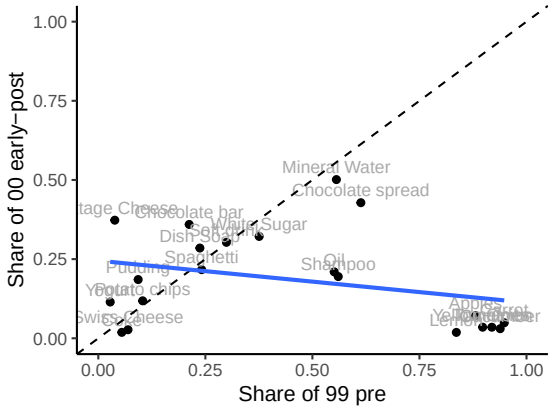
[37]Choice of other radii does not seem to matter. A choice of 5km keeps stores above and below median also within more densely populated areas, and is not a simple separation of urban versus sparse villages. I could not get too narrow due to measurement error in locations, which are based on store name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

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.

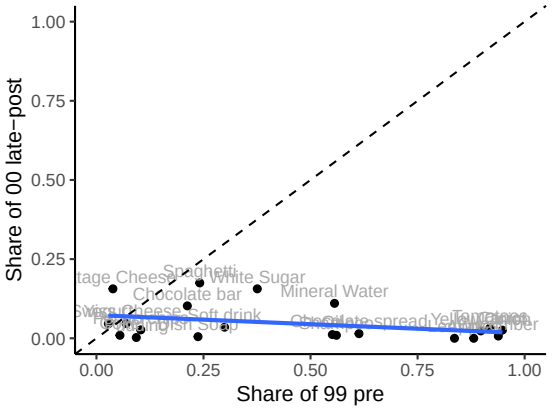
(a) Correlation between share of 99 pre and 00 pre (3-10/2013)



(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)



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-4a 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-4b), where differences in shares of 00 above and below the median[38] 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.

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

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, $\{\hat{S}_p\}$ (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 ϵ and left-digit bias θ: (2) take all relevant 99-ending prices, $\{q_i\}$, and calculate 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 θ and ϵ, 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:

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

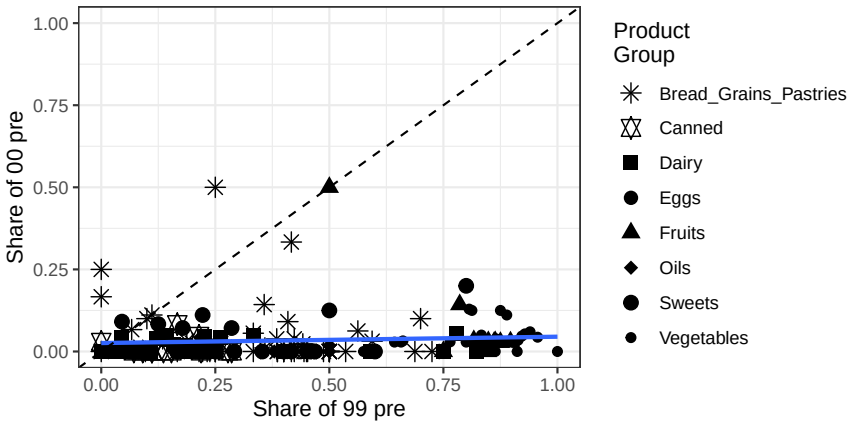
(6) Expand $\mathcal{P}'$ to $\mathcal{P}$ by adding the missing prices between q_i and P_{q_i} and assigning predicted zero shares to them.[39]

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 ϵ and left-digit bias θ. Since I am using the densities of some prices to fit the shape price distribution, I exclude these moments from the minimization problem.

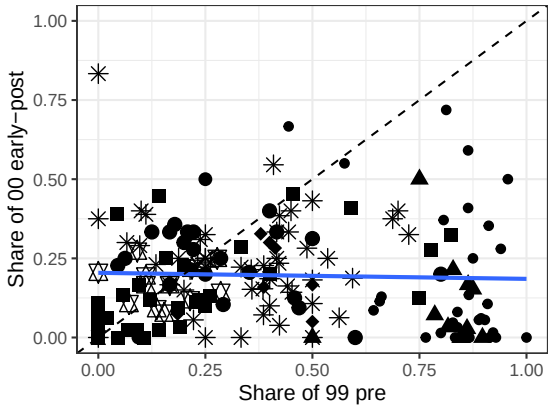
[39] relabel non-9-ending next lowest prices as the lower 9-ending price, without changing the share. For example, 2.51 will be relabeled as 2.49 with a share $\hat{S}_{2.49} = \hat{F}_c\left(\underline{c}_{2.59}\right) - \hat{F}_c\left(\underline{c}_{2.51}\right)$

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.

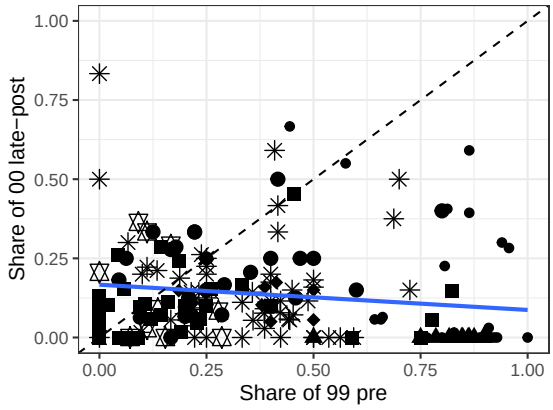
(a) Correlation between share of 99 pre and 00 pre (3-10/2013)



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



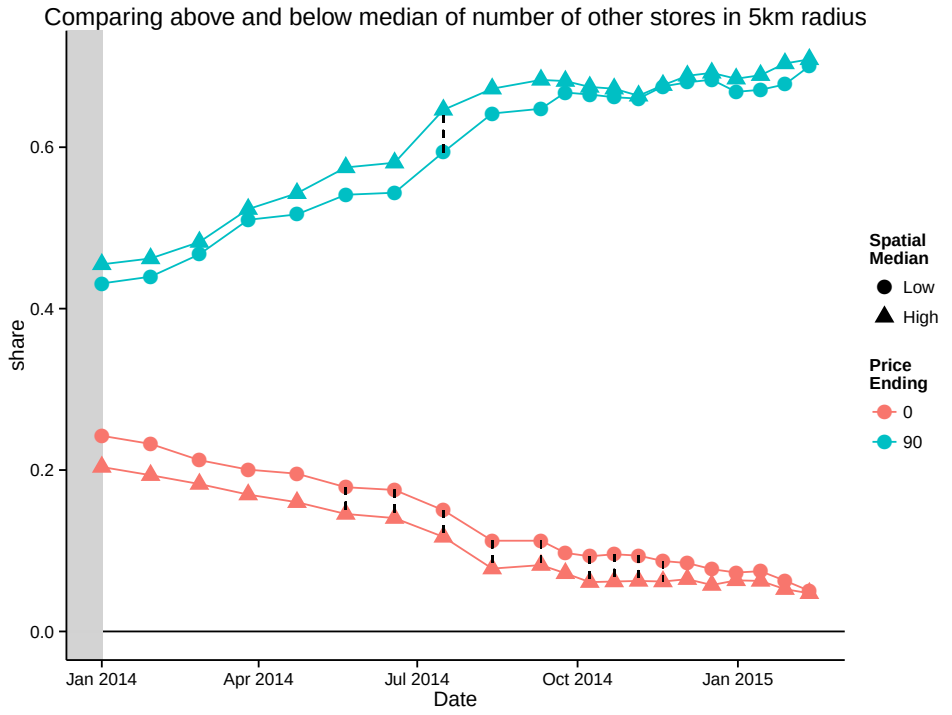
(c) Correlation between share of 99 pre and 00 a year after policy change (1-2/2015)



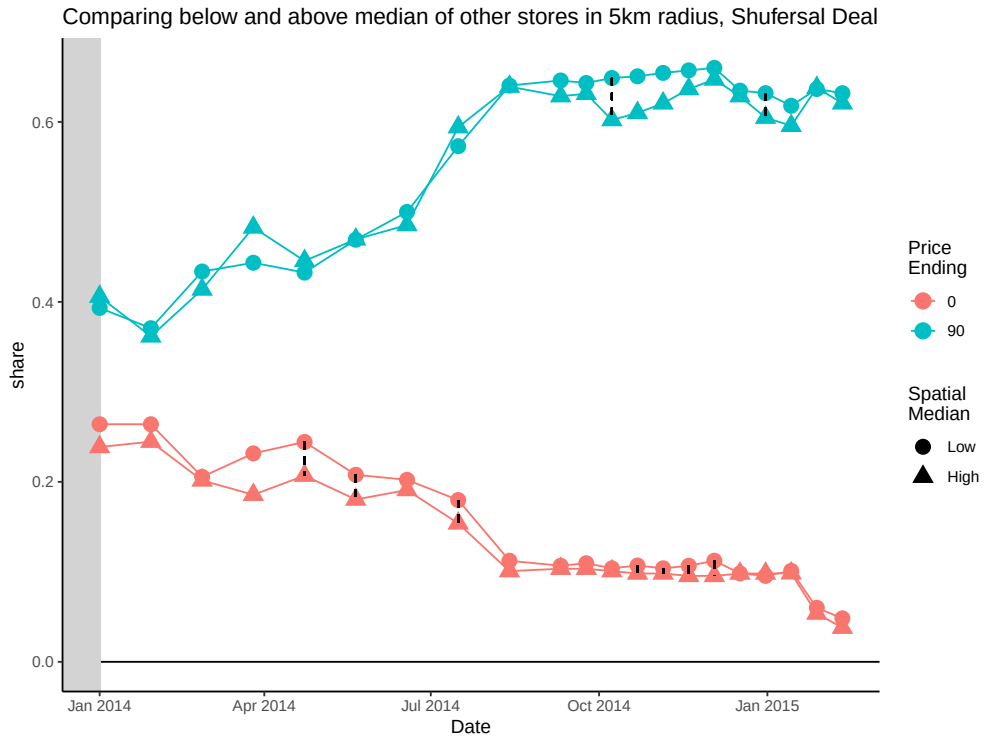
The figures show that shares of 00-ending prices at different periods against 99-ending prices at the pre-period, by product-type in the CBS data. The shares of 00-ending prices per product are calculated before the reform announcement, at the two months following the reform, and at early 2015 a year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

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

(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



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.