Retail Markups and Discount-Store Entry

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Abstract

"Hard discounters" are retail formats that set retail food prices even lower than existing discount formats, such as Walmart and Target. Offering limited assortments and focusing on store-brands, these formats promise to change the competitive landscape of food retailing. In this paper, we study the effect of entry of one hard-discount format on markups earned by existing retail stores, focusing on several important grocery markets across the Eastern U.S. Focusing on establishment-level profitability, we estimate store-level markups using the production-side approach of De Loecker and Warzynski (2012). We find that hard-discounter entry had the expected effect of reducing margins from similar stores, but did not affect markups earned by stores in the same market that are likely to appeal to a different market segment. In general, hard-discounter entry reduced markups for incumbment retailers by 7.3% relative to markups in non-entry markets. These results indicate that the net effect of hard-discounter entry reduces the overall level of store profitability, regardless of higher sales realized by incumbent retailers.

Keywords: Discounters, food retailing, markups, retail pricing, production economics.
JEL Codes: D43, L13, M31

1 Introduction

Hard-discounters, generally defined as retailers that offer limited assortments, high-quality private label brands, and prices that are often $40\% - 60\%$ lower than current "discount" retailers, are an emerging format in the US market (Vroegrijk, Gijsbrechts, and Campo 2013, 2016; Progressive Grocer 2019). In fact, two hard discounters, Aldi and Schwarz Group (parent of Lidl) both accounted for over \$100.0 billion in sales in 2017, and each had nearly *three times* the compound annual growth rate between 2012-2017 as any other store in the global top-10 (Steenkamp 2018). Although Aldi has been in the US market for over 20 years, the hard-discount concept has only recently emerged as a clear competitive threat to existing food retailers. Despite the potentially transformational nature of the hard-discount business model, there is very little empirical research of their impact on existing retailers. In this paper, we provide empirical estimates of the effect of hard-discounter entry on incumbent retailer markups, and store profitability.

Estimating the impact of hard-discounter entry on retail markups is not just a matter of curiosity. In recent decades, there have been a number of entry "waves" from retailing formats that seek to capitalize on the relatively large sales volumes associated with selling food. For example, in the 1990s, Walmart expanded from its base in Bentonville, Arkansas, to occupy nearly every market in the US, and many markets overseas. The impact of Walmart entry has been dramatic, and well-documented (Singh, Hansen, and Blattberg 2006; Basker and Noel 2009; Zhu and Singh 2009; Ailawadi et al. 2010; Courtemanche and Carden 2011; Holmes 2011; Huang et al. 2012; Iacovone, et al. 2015; Arcidiacono, et al. 2016; Atkin, et al. 2018). In the 2000s, existing retailers consolidated in the face of Walmart entry, and club stores, such as Sam's Club and Costco, became the new threat to traditional supermarkets (Courtemanche and Carden 2014; Bauner and Wang 2019). In the 2010s, online shopping emerged, but did not become a force for change in the grocery industry until Amazon acquired Whole Foods in 2017 (Turner, Wang, and Soper 2017), and the Covid-19 pandemic of 2020 accelerated the move online by some 10 years relative to existing trends (Progressive Grocer 2020). Currently, European hard-discounters are entering many key markets in the US, and hope to succeed by providing essential items at prices even lower than Walmart or club stores (Jackson 2020). Understanding the impact of entry on incumbent retailers, therefore, is critical both for retail practice, and for developing fundamental knowledge regarding the forces that shape US retailing.

Researchers generally examine the impact of entry using traditional, demand-side methods, combined with counterfactual simulations. However, estimating the effects of entry using store- and firm-level markup data is arguably more relevant as the average food retailer carries thousands of products (FMI 2021). Moreover, consumers often do not recall individual item prices (Dickson and Sawyer 1990; Loy, Ceynowa and Kuhn 2020), but rather basket-level prices (Bell and Lattin 1998), or firm-level product aggregations (Blonigen and Pierce 2016). Markups are of central concern to retailers because of their implications for profitability, and to policymakers due to their consequences for price-setting conduct and industry competition. For these reasons, we examine the impact of hard-discounters' entry into several important US grocery markets on using a store-wide, markup-based approach.

We are not the first to consider the competitive effects of hard-discounter entry, and the nature of competitor responses. For example, Vroegrijk, Gijsbrechts, and Campo (2013) argue that hard-discounters may have a "complementary" effect on existing retailers, as consumers tend to seek out the lowest-cost source for price-sensitive items, while seeking out traditional retailers for categories in which variety and quality may be more important. In a study most similar to ours, Cleeren et al. (2010) examine the inter- and intra-format effects of entry between hard discounters and supermarkets in Germany. Focusing on firm-level outcomes, they find a significant threshold effect for the impact of hard-discounter entry on supermarket profits. In terms of incumbent responses, Lourenço and Gijsbrechts (2013) suggest incumbent retailers offer top brands at relatively low prices to fend off discounters, while Vroegrijk, Gijsbrechts, and Campo (2016) suggest low-price private labels and Hökelekli, Lamey, and Verboven (2017) recommend price-competition through standard private labels. While these studies provide valuable insight on the reasons why hard-discounters may co-exist with traditional retailers in the same markets, they do not quantify the ultimate impact hard-discounter entry is likely to have on incumbent markups, and focus on empirical evidence from only a limited number of product categories.[1]

[1] A model of equilibrium price differentials, such as Salop and Stiglitz (1977) may provide an alternative

Beyond the specific example of hard-discounter entry, and the evident importance of firm-level profit, we know surprisingly little about retailing markups in general. Conventional wisdom holds that the retailing sector is very competitive (Bersteanu, Ellickson, and Misra 2010; Ellickson 2016), with net margins averaging 2.0% according to industry "stylized facts" (Campbell 2020). However, estimating markups at the store level is a difficult empirical problem.

The typical approach to estimating market power in retailing relies on demand-side methods (Berry, Levinsohn, and Pakes 1995; Nevo 2000; Chintagunta 2002) wherein the researcher estimates a large matrix of own- and cross-price demand elasticities, which condition the retailers' ability to achieve an equilibrium price under some assumed form of an oligopolistic pricing game. However, more recently, others recognize that if the goal is to estimate firm-level markups, then starting from a highly disaggregate set of products is not necessarily the most efficient way to begin, and a very restrictive one at that. De Loecker (2011), De Loecker and Warzynski (2012), Traina (2018), and many others, approach the problem of markup estimation instead from the production side, applying the insight of Hall (1988) that markups can be estimated from a simple condition on the output elasticity of a variable input, and input-expenditure shares of that input. While most applications are in trade (De Loecker 2007; Klette 1999) and macroeconomic markup estimation (De Loecker, Eeckhout and Unger 2020; Traina 2018), this approach is also useful in uncovering markup patterns among food retailers. We provide new evidence on the relative competitiveness of food retailing, and examine the specific case of how market-entry by a discount-retail chain affects markups of incumbent retailers, by estimating store-level markups from a production-side perspective.

Our conceptual approach is well-understood. Extending the growth model of Solow (1957), Hall (1988) shows that "[U]nder competition and constant returns, the observed share of labor is an exact measure of the elasticity of the production function..." so any departure between these two measures, assuming constant returns, is interpreted as a measure

explanation for the coexistence of traditional retailers and hard discounters, but their approach assumes consumers search for homogeneous products. Neither products nor retailers are homogeneous in the food retailing industry.

of imperfect competition (p. 923). De Loecker (2007), De Loecker (2011), and De Loecker and Warzynski (2012) develop the econometric details of how this conceptual model can be used to test for departures from competition in firm-level data, but the underlying logic is the same: With only limited firm-level production data, we can infer market power from changes in observed levels of output, and the employment of a variable input, assuming no adjustment costs, and without imposing constant returns to scale. This approach is particularly well-suited to estimating market power in a retailing context because it makes no assumptions regarding the nature of demand relationships among individual products that are typical of other empirical studies in this literature (Berry, Levinsohn, and Pakes 1995; Nevo 2001). This approach also "scales" well so that it is able to recover markup estimates, even when the firms involved sell thousands of items, across a wide range of potentially-unrelated categories (Gelper, Wilms, and Croux 2016). In this sense, the production-side approach avoids the curse of dimensionality that is common to all other methods of estimating multi-product seller markups.

We develop a variation on the production-side markup estimation approach developed by De Loecker (2011) and De Loecker and Warzynski (2012), and apply our approach to store-level, food-retailer data. While the production approach to markup estimation is typically applied to Census of Manufacturers (CoM) data in the US (Foster, Haltiwanger, and Syverson 2008; Asker, Collard-Wexler, and Del Loecker 2014; De Loecker, Eeckhout, and Unger 2020), the equivalent – Census of Retail Trade (CRT) – are insufficient for our purposes (Foster, Haltiwanger, and Krizan 2006). Because the retailing industry tends to be more concentrated in local areas, the public CRT data are only available on an aggregated basis for reasons of confidentiality, and the establishment-level micro data does not contain the physical input measures we require.[2] Therefore, we use the TDLinx establishment-level data set from

[2]Census-type data are only available at relatively long intervals (5 years for CoM), so data at a higher level of frequency would be preferable. Others in the macroeconomics literature use financial-statement data from Compustat (Traina 2018; De Loecker, Eeckhout, and Unger 2020). Financial statement data are useful for this purpose as they are prepared and gathered according to standard accounting rules, are as accurate as the threat of legal recourse would allow, are collected on at least an annual level, and are vetted by external auditors. However, Compustat data is financial in nature, so the focus is only on dollar values, there is only limited detail on physical input quantities, and data are only available for publicly-traded firms. De Loecker, Eeckhout, and Unger (2020), for example, define the variable input as an aggregate, cost-of-goods-sold measure, which can include many physical inputs, such as labor, wholesale purchases, and energy. Without physical measures of any input, questions regarding how input prices are affected by variations in

Nielsen, Inc. TDLinx is a comprehensive data set that aims to describe the locations and fundamental operating characteristics of all food stores in the U.S. (Cho, et al. 2019). With these data, we are able to conduct our analysis at the store-level, and explain company-wide markups that vary with changes in each store's competitive environment using physical measures of key input quantities, and dollar-sales output values.[3] We address issues that remain with our store-level data below in the description of our identification strategy, but note that our production-side approach remains far less data-intensive than the equivalent demand-side approach for the same objectives.

We find that there are two effects on incumbent retailers due to the entry of a hard-discounter that remain after accounting for the endogeneity of both entry and shocks to labor productivity. First, sales increase for retailers in the proximity of the entering retailer by approximately 2.0% (within 3 or 5 km), which we interpret as a positive traffic-effect due to the limited assortment offered by the entering retailer and/or lower retail prices as a result of more intense competition. In this regard, our findings are consistent with Vroegrijk et al. (2013), who find that "...losses to HDs are not necessarily most severe for incumbents in close proximity" (pg. 609). Consumers are attracted to the hard-discounter as they search for lower prices on either staple items, or items that are unique to the retailer, but then finish their shopping at other, local stores when they cannot find the range of items they are looking for at the hard discounter. Second, markups are lower for incumbent retailers as they reduce prices to compete with the entering retailer, or lose sales on previously high-margin sales that have been taken by the entering hard-discounter (by roughly 7.0%). Regardless of the specific mechanism, store-level markups are lower for stores in proximity to an entering hard-discounter. Aggregating the positive effect on store-sales, and the negative effect on markups, we find that the net effect on incumbent retailers is unambiguously negative. In general, therefore, we find that there is a net negative effect on incumbent performance due to hard-discounter entry, even without allowing for the potential dynamic effects associated with

quality and input-market power are compounded.

[3] Cho, et al. (2019) compare the relative merits of TDLinx with National Establishment Time Series (NETS, from Dunn and Bradstreet) and ReCount from NPD, Inc. Their comparison supports our use of TDLinx as NETS lacks input measures other than labor, and ReCount focuses only on foodservice establishments. One key limitation of TDLinx is that it only covers retailers with greater than $1.0 million in annual sales.

potential non-price competitive effects of rivals (e.g. additional variety, low-price services, enhanced private-label strategy or online delivery options).

We examine the potential response to hard-discounter entry from incumbent retailers through a series of counterfactual simulations. Retailers have been successfully improving productivity through the adoption of labor-saving technologies such as barcodes and barcode scanners (Basker 2012), automated self-checkout systems (Litfin and Wolfram 2006), digital price tags (Inman and Nikolova 2017), or automated warehouses, robotic in-store fulfillment, and autonomous floor cleaning robots (Begley et al. 2019). Each of these advances can be interpreted in our context as labor-productivity enhancing investments that may help incumbent retailers compete with hard discounters on a cost basis. Alternatively, retailers can adopt demand-side strategies by making enhancements to private-label offerings to attract market share from hard-discount retailers, as in Vroegrijk, Gijsbrechts, and Campo (2016) or Hökelekli, Lamey, and Verboven (2017). We examine one example of each of these strategies, and show that a productivity-enhancing investment is able to raise store-level profit by 9.3% in competition with a hard discounter, relative to a do-nothing scenario, while an output-improving investment of the same magnitude may increase profit by 13.5%. Therefore, our hypothetical scenarios suggest that managers would be well advised to consider not competing against an entering hard-discounter, but by making the most of the market opportunities provided by the discounter, and exploiting the part of the market that is not well served by the hard-discount format, in general.

Our empirical model, and our findings, contribute to methodological literature on estimating markups in food retailing, the substantive literature on the profit-effects of retailer entry, and the managerial literature on retailers' response to new-format entry.

In terms of our methodological contribution, we are the first to apply a store-level, production-side approach to estimating market power in the food-retailing sector, and specifically to address the problem of how market entry affects the market power of existing firms. There are a number of reasons why a store-level approach is valuable in estimating the impact of entry. First, and most importantly, retailers sell thousands of items – items that can be either complements or substitutes in demand. While others examine the implications of complementarity among retail products for store-level market power (Thomassen, et al.

2017; Richards, Hamilton, and Yonezawa 2018) their analyses are necessarily restricted to a limited set of products in the store, and do not attempt to study the profitability of the store in general. On the other hand, our production-side approach, by definition, at least implicitly takes into account the store-level profit implications of all manner of product-level interactions.

Second, our approach is sufficiently flexible to account for heterogeneous effects among the retailers in our sample. In fact, the retailers in our sample range from very small, limited-assortment retailers (approximately 3,000 stock-keeping units, or SKUs) to large club stores and supercenters (upwards of 50,000 SKUs). A demand-side approach, which is dependent upon consistently estimating interactions among all possible products, cannot possibly yield comparable estimates across stores that differ in assortment to this extent.

Third, when the unit of observation is the establishment itself, there is a wider range of variables that can serve as instruments for endogenous labor input and entry decisions. Entry-endogeneity is a clear barrier to identification in any study of this type (Cleeren, et al. 2010), so having access to suitable instruments is both important, and necessary.

Fourth, in the entry literature we are first to apply a production-side markup estimation approach to estimate the effect of entry on incumbent retailer profit-performance. Our approach is appropriate for this purpose as retailers are more likely to use store-level measures of profitability to evaluate either the feasibility of entering a new market, or the extent of competitive harm inflicted by an entering rival. In the empirical model, we disaggregate the effects of entry into aggregate, store-level impacts on both sales and markups. Our simulation model shows that the net effect of entry, after accounting for higher sales due to positive traffic-effects in the local market and negative effects on markups due to price competition, reduces the overall level of store profitability.

Finally, our findings provide valuable information to retail managers who seek to stave off the worst impacts of hard-discounter entry. While intuition may suggest that incumbent retailers can survive by doing what hard-discounters do, but better, our findings show that total store sales are actually higher in proximity to hard discounter entrants. Therefore, increasing variety and providing high-quality store brands may be a more appropriate response. This result is reminiscent of Arcidiacono et al. (2016) who show why mimicking

Walmart in the 2000s was likely a bad idea as the retailers who were different from Walmart survived, while direct competitors did not. Our findings in this regard align with Vroegrijk, Gijsbrechts, and Campo (2013), who find that consumers are likely to "trade up and trade down" across retail formats due to hard-discounter entry, taking advantage of low prices in price-sensitive categories from hard discounters, while buying more hedonic items from high-variety traditional retailers. Our ultimate conclusions are the same as theirs, namely that hard discounter entry need not mean the end of retailers with store formats appealing to other consumer segments.

The paper is structured as follows. In the next section, we describe a conceptual model of retail markups that is based on the markup-estimation framework of Hall (1988) and De Loecker and Warzynksi (2012), and motivate our primary hypotheses. In the third section, we describe our data and identification strategy, while we derive an empirical model that is able to recover markups of the form required in our conceptual model in section 4. We present and interpret our empirical findings in section 5, and interpret our results in terms of the implications for retailer performance, and likely response strategies. We conclude and describe the limitations of our research in the final section.

2 Conceptual Framework and Theoretical Expectations

In this section, we frame our theoretical expectations regarding the impact of hard-discounter entry based on the existing literature on retail-format entry, explain our core hypotheses, and provide a theoretically-consistent framework for testing each hypothesis in an empirical production context.

2.1 Hypothesis Development

The entry of a hard-discounter into a local market may impact an incumbent retailer directly by reducing its market share, and indirectly by influencing their pricing and operating strategies.

Hypothesis 1: *Incumbent retailers lower prices in response to hard-discounter entry.* Because hard-discounters' prices are significantly lower than conventional retailers, incumbent retailers may reduce prices in response to hard-discounter entry. By offering lower prices, incumbent retailers can continue attracting customers even after a hard-discounter entry, just as retailers responded to Walmart entry in the 1990s and 2000s (Basker 2005; Hausman and Leibtag 2007; and Basker and Noel 2009). Because hard-discounters offer a Walmart-like price effect, it is possible that hard-discounter entry leads to lower prices at incumbent retailers.

Hypothesis 2: *Store-level sales for incumbent retailers rise upon entry of a hard-discounter.* Hard-discounters carry limited assortments of grocery products, most of which are private labels. Therefore, consumers may visit both hard-discount stores and traditional supermarket stores to fill their shopping baskets. Previous research finds that consumers purchase price-sensitive products at hard-discount stores, and shop at incumbent retailers for categories in which variety and quality may be more important (Vroegrijk, Gijsbrechts, and Campo 2013). If this is the case, geographically-proximate, incumbent retailers may choose to compete in non-price store attributes such as product variety, service quality, and high-quality fruits and vegetables in order to attract customers who shop at nearby hard-discounters for price-based reasons. Further, the size of the entire market may expand as geographies with entering hard discounters draw customers from outside the previous market boundaries (Ellickson, Grieco, and Khvastunov 2020). Therefore, we expect higher customer traffic and sales volume at incumbent retailers that are located near to entering hard discounters.

Hypothesis 3: *Margins at rival retailers fall upon entry of a hard-discounter.* Incumbent retailer sales may increase with hard-discounter entry, but higher sales do not necessarily increase profits or retail markups as a key performance metric for retail managers. For example, the empirical literature on Walmart entry shows that existing retailers' markups fall due to Walmart's generally negative impact on retail prices (Basker 2005; Hausman and Leibtag 2007; Basker and Noel 2009), without a correspondingly larger negative effect on wholesale prices (Huang et al. 2012). Due to the limited, and private label-dominated, assortment of hard-discount stores, we expect the wholesale impacts of hard-discounter entry to be minimal. Whether incumbent markups fall, therefore, depends largely on whether they choose to meet hard-discounter prices, or raise service quality in order to build pricing power. Based on the Walmart experience, therefore, we expect to observe competitive pressures forcing equilibrium prices, and markups, down throughout the market.

2.2 Conceptual Model of Retail Markups

We adopt a store-level, markup-estimation approach to testing the hypotheses developed above (Hall 1988; De Loecker 2007; De Loecker and Warzynski 2012). We believe this approach is appropriate for the problem at hand, as estimating the structure of demand for the thousands of products sold by a typical food retailer is simply intractable (Villas-Boas 2007). Moreover, our approach is particularly appropriate for estimating retail markups as retailers are concerned with store-level and not product-level profits (Bliss 1988). Whereas a focus on elasticities draws attention to product-level profits, it ignores the fact that products are purchased by the shopping-basket, often containing dozens of products, and retailers tend to sell thousands of items at a time. Rather than concern ourselves with how demand interactions affect pricing power, we are more interested in how store-pricing in general manifests in store-level markups.

The production-function approach developed by Hall (1988) relies on the fact that the elasticity of output with respect to a variable input will equal that input's expenditure share of revenue (or, its cost-share) in a competitive equilibrium. Any deviation from this result, assuming constant returns to scale, is interpreted as the evident exercise of market power. This approach is not only powerful but convenient, as it means that we only need to estimate the output-elasticity of a variable input at the establishment level. Both the availability of highly-granular, store-level production-side data for grocery retailers, and advances in the econometric estimation of production functions (Olley and Pakes 1996; Levinsohn and Petrin 2003; Ackerberg, Caves, and Fraser 2015), mean that this approach is appropriate for the type of problem we present here, and both tractable and empirically feasible.

The intuition of our approach is straightforward. Beginning from the production technology described by Hall (1988), the firm-level markup is the difference in the growth of output that cannot be explained by the rise in input use (weighted by each input's share in total expenses), and Hick's neutral productivity growth, which implies that the ratio of inputs to output is constant. We interpret that if a firm is able to derive value from what cannot be explained, then it must be due to the exercise of market power. Thus, market power is in essence a latent construct, or pricing power that is left over after everything else has been accounted for.

Formally, the growth in output is written as:

$$\Delta q_{it} = \mu_{it} \sum_k \alpha_{it}^k \Delta x_{ikt} + \Delta\omega_{it}, \tag{1}$$

where Δq_{it} is firm i's output growth in year t, μ is the markup term, α is input k's cost-share of revenue, Δx_k is the growth in the value of input k in year t, and ω is a productivity shock (all lower-case letters refer to logarithms). As a practical matter, estimating this function directly is not possible, due to the clear correlation between contemporaneous productivity shocks and input employment, but recent advances in the econometric estimation of production functions provide a feasible solution to this problem. Namely, De Loecker (2007, 2011) and De Loecker and Warzynski (2012) describe a tractable solution that requires few restrictions on the production technology, and a general specification that is amenable to estimation with commonly available production data.

Conceptually, the assumptions necessary for this approach are minimal, namely that there is at least one variable production input, the variable input does not have adjustment costs (which would make it quasi-fixed), and that the firm minimizes the cost of production. If these assumptions are valid, then the output elasticity with respect to the variable input, conditional on fixed-input values, can be written as:

$$\phi_{it} = \frac{\partial f(x_{it})}{\partial x_{it}} \frac{x_{it}}{q_{it}} = \frac{w_{it} x_{it}}{\lambda_{it} q_{it}}, \tag{2}$$

for firm i in time period t, where ϕ_{it} is the output elasticity of the variable input, x_{it} is the variable input amount, w_{it} is the price of the variable input, q_{it} is output value, and λ_{it} is the cost-minimizing value of the Lagrange multiplier. In this application, the Lagrange multiplier is interpreted as the marginal cost of production at a given level of output. If the markup is given by the ratio of the output price to marginal cost, or $\mu_{it} = p_{it}/\lambda_{it}$, then we can substitute for λ_{it} in the previous expression to arrive at an expression for the markup in terms of the output elasticity of the variable input, and its expenditure share, or:

$$\frac{\phi_{it}}{s_{it}} = \mu_{it}, \tag{3}$$

where s_{it} is the variable input's expenditure share in firm i's total revenue. This expression implies that the markup is equal to the input's output elasticity relative to its expenditure-share. Importantly, this expression suggests the minimal requirements for estimating the degree of departure from perfect competition: An estimate of the output elasticity for one variable input, and that input's expenditure share to total revenue.

We are rarely interested, however, in simply recovering markups without some explanation for how they vary either over time, across firms, or both. For example, De Loecker (2007) examines how a firm's export status influences markups, while Blonigen and Pierce (2016) use a similar approach to estimate the impact of mergers and acquisitions (M&A), using data from a large number of firms in a variety of industries. In our application, we are interested in how markups for incumbent food retailers are affected by the entry of a "hard discounter," or a rival that is focused on selling a range of consumer products at the lowest-possible prices. With a production-approach to estimating markups, we allow the output-elasticity value in equation (3) to vary with a range of entry-definitions, and test our core hypotheses regarding how we expect markups to respond.

3 Data and Identification Strategy

3.1 Data Description

Our core data set consists of TDLinx establishment-level data from Nielsen, Inc. As noted above, TDLinx is a comprehensive data set that describes the locations and fundamental operating characteristics of food stores in the U.S. We use annual measures of output, labor input, a measure of capital (store size, in square feet), and a measure of technology investment (number of checkouts) for supermarket retailers in the relevant geographic markets over the 2014 - 2019 time period. Because the hard discounter format began a rapid penetration of the US market in 2017, our data covers the time period prior to entry, as well as after entry occurred. Importantly, TDLinx contains the exact name and location of each store in the US, so provides a census of all stores that were possibly affected by Lidl's entry. Our geographic markets include all states that with at least one discounter-location across our sample period, but we define the extent of competitive pressure within each market more precisely, as explained below. TDLinx is also valuable for our purposes because it includes a wide range of attribute measures for each store – attributes that are likely to explain differences in store sales that cannot otherwise be attributed to labor efficiency, location, capital investment, or our other core variables.

Despite the fact that TDLinx contains all of the data we need to accomplish our objectives, that is, a measure of output, and measures of both variable and fixed inputs, over a panel of individual store-locations, TDlinx is not without limitations for our purposes. For instance, TDLinx only measures store-level dollar sales, and not physical quantity. Although a physical measure of quantity is likely ill-defined when the retailer sells products of varying qualities and descriptions over hundreds of categories, it nonetheless means that our primary variable of interest is measured in terms of dollars, and not the more usual measure of physical output.

In the international trade literature, authors commonly use revenue as a proxy for physical output as the latter is rarely available in the data (De Loecker 2007, 2011). The standard procedure is to deflate revenue by a common price index in order to arrive at a measure of physical output that is likely to approximate actual volume sales. De Loecker (2011) argues that this is problematic for estimating production-function parameters if the difference between the price index and actual store prices is correlated with input demand. In our case, we avoid this problem as we rely on the finding by Della Vigna and Gentzkow (2019) that food retailers tend to follow constant-pricing strategies across all of their stores. If this is the case, and the unit of observation is the individual store, then applying a common price index does not create the same econometric issues as in other applications.[4] Therefore, we deflate store-level sales-output using a producer-price index (PPI) from the Bureau of Labor Statistics (US Bureau of Labor Statistics) for food retailers.[5]

Second, TDLinx tends to capture larger retail food stores. As Cho, et al. (2019) explain,

[4] Any departure from the uniform-pricing result fo DellaVigna and Gentzkow (2019) may be correlated with inventory demands (i.e., cost of goods sold, or products purchased to inventory) but not likely the operational inputs we consider here. At the very least, we can see no reason why the difference would be correlated with the demand for labor and capital inputs.

[5] The PPI for NAICS 445 (Food and Beverage Stores) is an output-price index that "...measures the average change over time in the selling prices received by domestic producers for their output. The prices included in the PPI are from the first commercial transaction for many products and some services..." (BLS 2020).

TDLinx tends to use an industry-standard definition of a supermarket, which means more than $2.0 million in annual sales. Although they include smaller stores ($1.0 million - $2.0 million) in their data under a different classification (the "Superette Subchannel"), their reporting may not capture smaller stores as accurately as alternative business-location data sets (e.g., the National Establishment Time Series, NETS, data set). However, for our purposes the error caused by excluding smaller stores is not likely to be significant as our interest lies mainly in estimating the impact of entry on mainline grocery outlets.

Third, TDLinx does not gather exact labor-input values for each store, each year, but rather estimates the number of full-time equivalent (FTE) employees.[6] For this reason, the TDLinx data cannot be interpreted as a panel data set akin to the Census of Retail Trade, but rather a repeated cross-section of observations on the same store over time. In our empirical application, we are careful to not rely on time-series variation in any of our key estimation variables, but rather exploit the cross-sectional differences in entry, and output values, to identify the impact of entry (Ellickson and Grieco 2013). Despite these potential shortcomings, we believe the TDLinx data remains the best alternative for examining store-level markups using the production-side approach of De Loecker and Warzynski (2012).

We summarize the key variables that enter our empirical model in Table 1 below. From this table, it is clear that there is a substantial degree of variation in output and labor input among stores, so the key parameters of interest should be identified. In terms of our entry-estimation objective, the data in this table also show considerable variation in both the distance between each store and an entering hard-discounter, and the likelihood of being proximate to an entering store. Therefore, we are confident that we have the fundamental conditions in place to statistically identify the effect of hard-discounter entry on store performance.

[table 1 in here]

Prior to estimating our structural model of store markups, we first examine the data

[6]FTE employees are calculated as all full-time employees, plus 0.5 of each part-time employee. The technical documents indicate that Nielsen brings together "multiple data sources" including "self-reported retailer input, store visits, questionnaires to store management, bi-annual surveys, as well as public information, government statistics, financial filings and other sources" in estimating the number of FTE employees per store (TDLinx 2022).

for patterns in entry, and store output. We begin by examining the geographic dispersion of store openings. Figures 1 - 3 show that there were only a few stores (10) open in 2017, scattered between Virginia, North Carolina, and South Carolina. By June of 2018, there were 54 stores, with single locations in Delaware, Georgia, and New Jersey, and by June of 2019, there were 67 stores, including stores in Pennsylvania, Maryland, and New York. In each state, we define a competitive market area around each store, and determine which other stores in our data are likely to fall into the competitive area of each entering hard-discounter. From the literature on store-location (Ellickson and Grieco 2003), we define stores within a circle with a radius of 3 miles (5 kilometers) as direct competitors to the enter hard-discounter, and consider these stores as treated. All other stores under the same banners, but outside of the competitive market region are defined as control stores.[7] Given the relatively small size of the hard discounters, we expect that a 3-mile competitive region is likely to be a conservative estimate of their sphere of influence.

Next, we split the data into “entry markets” and “non-entry markets,” where markets are defined by county (FIPS code), and examine the data for any differences in store-level output, before and after entry, using simple summary statistics. In Table 2 below, we see that sales in entry markets are substantially higher than in non-entry markets ($311.3 million per year versus $234.2 million per year). However, this summary statistic does not control for the clear endogeneity of market entry, so it may be the case that the hard discounter is simply attracted to more lucrative markets. Further, this simple approach does not account for the efficiency of the labor input that drives markups in the De Loecker and Warzynski (2012) framework. We calculate a crude measure of labor efficiency, therefore, and report this value in Table 2. Sales dollars per employee is a common managerial measure of labor efficiency in food retailing. According to this measure as well, entry markets appear to be characterized by not only higher-selling stores, but stores that are more efficient. Regardless, the difference in the entry and non-entry values in Table 2 suggest hard-discounter entry has a dramatic effect on store performance. But, conclusive evidence is required for all potential

[7] In our empirical analysis, we define the area of influence around each entering store as a circle of radius 3 kilometers, and 10 kilometers as robustness tests. We also consider entry defined continuously as the distance between a sample store, and the closest entering hard discounter. We estimate the same model in each case, and find that our results do not differ qualitatively. These results are reported in the Results section below.

confounding values, both with respect to entry and labor endogeneity.

[table 2 in here]

3.2 Identification Strategy: Retail Entry

We identify the effect of entry on store-level markups by exploiting the quasi-natural experiment created by the staged entry of our target hard-discounter. That is, the 67 stores in our sample did not enter each market at exactly the same time, so our data describe a sequence of entry-experiments both across time, and across markets.[8] Because retail food markets tend to be relatively small, as most consumers prefer to travel short distances and are loyal to stores that are geographically proximate (Briesch, Chintagunta, and Fox 2009), it is reasonable to assume that the entry of a particular store in each market only exerts a competitive influence on stores in that market. Entry into each market, however, is likely to be endogenous to the expected profitability of the market (Cleeren et al. 2010). We address this clear barrier to identification using a set of instruments through a control-function estimation framework.

First, we include a set of market-level fixed effects that account for unobserved factors that may be correlated with the decision to enter, an approach implemented by Klette (1999) who used fixed effects to capture differences in levels of productivity due to variations in labor quality. Fixed effects are effective instruments as they essentially absorb any local conditions that are unobserved to the econometrician, and yet likely to be important to Lidl's decision to enter at that particular location. Thus, by including fixed effects, we allow cross-sectional differences in productivity to be correlated with output and all factor inputs (Klette 1999).

Second, we include observed attributes of each local market (at the census-tract level) from the American Community Survey (ACS, US Bureau of Census). ACS data are useful in this regard as demographic and socioeconomic measures such as per capita income,

[8] Cleeren, et al. (2010) follow Bresnahan and Reiss (1991) and recognize the inherent multiplicity of equilibria if entry by both supermarkets and hard-discounters is viewed as the outcome of a Nash equilibrium-entry-game. In our case, however, the recent nature of hard-discounter entry means that we can ignore the entry problem faced by traditional supermarkets, and consider only the entry decision of the hard-discounter itself as each retail market is relatively mature. In this sense, we follow Cleeren, et al. (2010) and assume our observed pattern of entry represents a sub-game perfect (SPE) Nash equilibrium of the hard-discounter-only entry game.

average household size, local unemployment, and residential rental rates are likely to be highly correlated with purchasing-power measures considered by retailers in their decision to enter a market, and yet mean independent of the productivity of each individual store. Further, ACS provides exogenous variation at a level of geographic granularity that both helps identify the entry-effect we are interested in, and differentiates local markets over time and space.

Third, and perhaps most importantly, we use commercial real estate data (CRE, CoStar, Inc.) for each entry-location. Our CRE data, defined as commercial sales prices per square foot, represents an ideal instrument for the decision to enter as it reflects market-level demand for the specific location targeted by the entering store. Because we use market-level data for a 3-mile radius around the store, our sales prices are exogenous to the specific case of the retailer, and are likely to capture a general level of demand for the location in question. Retail lots used by grocery stores, moreover, are generally fungible across any retail purpose, so CRE prices are independent of the output of a specific type of retailer.[9]

In our empirical application, we examine the effect of using each of these instruments on the robustness of our empirical results, and determine their validity through first-stage instrumental variable regressions. In a first-stage instrumental-variables regression of this set of instruments on the decision to enter, estimated as a linear probability model, we find an F-statistic of 225.4, so our instrument is not weak according to the threshold (F = 10.0) defined by Stock and Yogo (2005). We report the complete results of these estimates in Table 3 below but, in general, are confident that our identification strategy supports our core hypotheses, and the robustness of our empirical findings.

[table 3 in here]

Given that entry is a discrete variable, there is some debate over the exact form of the control-variable model for the entry variable. While the traditional approach would apply a Heckman-correction type of model with a suitable instrument, this approach is subject

[9] Our method of addressing the endogeneity of entry is similar to that used in the most recent of the vast number of "Walmart entry" papers (Arcidiacono, et al. 2020). Our implicit model of entry maintains that retailers do, in fact, consider market potential in selecting candidates for entry, but conditional on market fixed effects, attributes, and trends, the decision to enter is mean independent of the remaining residual. We go one further, however, and absorb broader market demand for retail real estate by including commercial real estate (CRE) prices among our set of instruments.

to bias in the second stage if the first-stage probit model is mis-specified. On the other hand, Wooldridge (2015) argues that, in models such as this that are linear in the variables with a discrete endogenous explanatory variable (EEV), it is preferred to estimate either a first-stage probit equation, or a linear probability model, and use the generalized residuals as control functions in the second-stage, structural model. When EEVs enter in non-linear ways, as in our second-stage model, a control function method is preferred because "...for general nonlinearities, inserting fitted values for EEVs is generally inconsistent, even under strong assumptions (p. 429)." Therefore, we adopt a control function approach with the generalized residuals from a first-stage, linear-probability model equation for the decision to enter, instrumented with market fixed effects, socioeconomic variables, and commercial real-estate prices.

3.3 Identification Strategy: Labor

As the literature on production-function estimation cited above clearly explains, the amount of labor employed is also endogenous to any unobserved productivity shock at the store level. In our static analog of the ACF (2015) approach, we instrument for the endogenous labor input in the second-stage of the estimation procedure using a variable that is likely to be highly correlated with the amount of store-level employment, but mean independent of store output. According to first principles, firms minimize cost by choosing the level of input employment up to the point where the marginal value product is equal to the market wage. If labor markets are competitive, the market wage should, therefore, be correlated with the level of employment, but not necessarily related to the output level. Therefore, we use market-level wages (defining each labor market at the county FIPS-code level) from the Quarterly Census of Employment and Wages (QCEW, USBLS, 2020b) for workers in retail grocery trade on a quarterly basis, and average over all quarters to arrive at an annual estimate of the average weekly wage. For the labor input, our instrumental-variables regression of labor employment on the average weekly wage and county, year, and store fixed effects produces an F-statistic of 391.5, so again our instrument cannot be described as weak.

In the next section, we describe how these instruments help identify markups, and entry-effects using production-type data at the individual-store level.

4 Empirical Approach

We examine the problem of hard-discounter entry from both a reduced-form and structural perspective. That is, we first examine the data using relatively simple, reduced-form models to see if there are any patterns in the data that are consistent with our expectations regarding the potential competitive effects of hard-discounter entry on both volume and price of incumbent retailers. We then estimate structural production functions that account for the well-understood endogeneity of variable inputs (Olley and Pakes 1996; Levinsohn and Petrin 2003; Ackerberg, Caves, and Fraser 2015) using the approach developed for repeated-cross sectional data by Lowrey, Richards, and Hamilton (2021). We use this approach to test the effect of entry on labor productivity, and markups, more formally. We then use the estimates from this model to conduct a set of counterfactual simulations that compare prices and markups with and without hard-discounter entry. We interpret the results from these simulations as the likely impact of entry on stores in our sample markets.[10]

4.1 Overview of Empirical Approach

In our first set of models, we examine price and volume data for any summary-level evidence of hard-discounter entry. These models are not intended to be conclusive, but merely to examine the data in a way that is relatively free from modeling constraints in order to see if there are any patterns evident in the data. While the findings from these models may be suggestive, there are not definitive tests of our core hypotheses regarding entry. Our structural tests, on the other hand, address the endogeneity of input-choice and entry using recently developed methods of markup estimation from the empirical trade and industrial organization literatures (De Loecker and Warzynski 2012; De Loecker and Scott 2016).

In a food-retail setting, we argue that a production-side approach to estimating the extent of market power is preferred over the traditional, demand-side approach to margin estimation

[10] Note that we define welfare effects here from only the suppliers' perspective, as we do not estimate the structural demand model that would be necessary to estimate consumer-welfare impacts. However, consumer welfare is affected by both variety and prices (Dixit and Stiglitz 1977), so the likely effect on consumers is ambiguous. While consumer welfare would rise due to lower prices, it would fall to the extent that there are any variety-reducing effects attributable to the hard-discounter (Atkin et al. 2018). We leave the estimationn of these consumer-side effects to future research.

(Villas-Boas 2007, for example) for several reasons. First, the accuracy and consistency of markups estimated from a demand-side approach depend on the relevance of the demand-model that is brought to the task. Although it is well understood that random utility models applied to store level data are able to handle multi-product, high-dimension problems that would otherwise overwhelm a traditional demand-system approach (Nevo 2001), they are not particularly well-suited to the scale of demand problem presented by modern food retailers, where retailers typically sell 40,000 - 50,000 products in the same store, and consumers select items from among hundreds of product categories. While others address the dimensionality problem inherent in retail demand analysis using nested models of category-and-store choice (Bell and Lattin 1998; Richards, Hamilton, and Yonezawa 2018), they are still only able to address the problem in an indirect way, reducing the scale of the problem by ignoring most of it.

Second, retail demand estimation is also problematic because consumers' consideration sets are unobserved, and endogenous (Mehta, Rajiv, and Srinivasan 2003; Koulayev 2014; De Los Santos and Koulayev 2017). That is, traditional demand-side analysis assumes consumers only shop from a limited set of products in any category that form the products that they would reasonable consider buying. In actuality, however, the set of products each consumer chooses from is endogenous to their preferences, experience, and the particular shopping environment, and analysts cannot measure (directly) the constituents of each shopper's consideration set. Koulayev (2014) shows that the resulting estimation bias can be substantial.

Third, margin estimates derived from demand-elasticity estimates are conditional on a specific solution concept that describes the game played among oligopolistic retailers. While the literature appears to have settled on Bertrand-Nash rivalry as a reasonable and robust way of thinking about how retailers interact, studies that adopt a "conduct parameter" approach typically show substantial deviations from the maintained form of the pricing game (Richards, Hamilton, and Yonezawa 2018).[11] In contrast, the production-approach to estimating markups makes no prior assumption on the nature of the game played among

[11] Corts (1999) criticizes the conduct parameter approach as being fundamentally unidentified, and lacking in any meaningful theoretical basis in pricing behavior.

competing firms, and provides consistent estimates regardless of how retailers compete (De Loecker 2011b).

Fourth, by definition, market entry presents unique problems for demand-side analyses that are avoided by focusing only on store-level production. Specifically, if we seek to model the structure of rivalry among competing retailers using changes in demand, and the cast of players changes from one period to the next, then it is a difficult task to disentangle the effects of changes in product-level interaction introduced by a new competitor from changes in the game itself. By focusing only on the indirect impact of entry on each store's input-productivity, we obviate the need to take demand-reallocation into account.

Our point is that when we are interested in store-level outcomes, a production approach has many appealing features relative to a more traditional demand-side approach. That said, in our empirical model below, we describe how we need to modify, and thus depart from, the approach developed by De Loecker and Warzynski (2012) to suit our particular needs.

4.2 Empirical Model

Researchers typically use panel data to estimate markups using the production-side approach of De Loecker (2011a,b) and De Loecker and Warzynski 2012. Nothing in the underlying theory requires panel data per se, but identifying the necessary productivity parameters generally requires methods developed specifically for application to panel data. Specifically, the methods developed by Olley and Pakes (1996), Levinsohn and Petrin (2003), and more recently by Ackerberg, Caves, and Fraser (ACF, 2015) all assume that productivity shocks, which are correlated with endogenous input levels, follow Markov processes. In the absence of panel data, researchers must therefore estimate input-productivity parameters using cross-sectional data, as in our application, where the unobserved shocks to productivity occur over stores, and not over time. Therefore, the ACF approach is appropriate for data that only describes variation in output across stores, at the same point in time. Thus, our empirical specification of retail markups adopts the same conceptual framework to identifying production-parameters as ACF (2015), but we depart from their approach, practically, due to the particular application to food retail and our store-level data, which are repeated cross-sections (described in detail in section 3.1).

Our approach requires a consistent estimate of the output-elasticity with respect to one variable input. In our case, the variable input of interest is labor, due both to its importance to food retailers and its central role in empirical models of production more generally. Because most other retailing inputs either have a fixed relationship to output (e.g., materials, or inventory) or are subject to substantial adjustment costs (e.g., capital, in the form of the size of the store, or the technology used in the store) labor is an ideal input for our purposes. It is well-understood, however, that labor employment is endogenous as shocks that affect output are also likely to affect the firm's employment decisions (Olley and Pakes 1996; Levinsohn and Petrin 2003; Wooldridge 2009). Our two-step procedure, therefore, is intended to address the fundamental endogeneity of labor-input, and the endogeneity of Lidl's decision to enter each particular market. We first invert the demand for an input other than the variable input of interest, and use a non-parametric approach to obtain a vector of expected production amounts that hold for any arbitrary parameterization of the production surface. We then use the fitted values from this expression to remove any observed shocks to productivity, and then use a control function approach to account for the endogeneity of labor and the discrete decision to enter. It is in this second step that we necessarily depart from ACF (2015) as our data are not dynamic, as required by their Markov-productivity assumption.

4.2.1 Stage One: Estimating Retail Output

Our first-stage production function is a parametric model of output and two state, or quasi-fixed, input variables. In the Olley and Pakes (1996) and Levinsohn and Petrin (2003) framework, the demand for one state variable serves as a proxy variable for the unobserved shock to productivity. In using this approach, we assume that productivity depends on factors besides the variable input, and that values of these other factors are not determined at the same time as employment decisions for the variable inputs are made. For our purposes, we assume a relatively simple functional form for the production function (Cobb-Douglas), in which output (y_{it}) for store i in period t is a function of the amount of labor (l_{it}) and two quasi-fixed variables (e.g., the size of store i, k_{1it}, and the number of checkouts, k_{2it}), so the production technology is given by:[12]

$$y_{it} = \beta_0 + \beta_l l_{it} + \beta_{k1} k_{1i} + \beta_{k2} k_{2i} + \beta_z \mathbf{z}_{it} + \omega_{it} + \eta_{it}, \tag{4}$$

where each output and input value are expressed in logs, y_{it} is defined as revenue at the store level, normalized by a retail price index, labor l_{it} is measured as the the number of employees (full-time equivalent, FTE) in store i in year t, both k_{1i} and k_{2i} are time-invariant proxies for different forms of capital investment, $\mathbf{z}_{it}$ is a vector of exogenous explanatory variables likely to be associated with inter-store differences in productivity, the variable ω_{it} is a Hicks-neutral productivity measure that cannot be observed by the analyst, and is assumed to be correlated with l_{it}, and η_{it} is an i.i.d. error, which is assumed to be uncorrelated both over stores and time periods.[13]

With this approach, we require a variable that proxies the unobserved shock to productivity, and another that serves as a state variable. We define k_{1i} as the size of the store (in square feet), and k_{2i} as the number of checkouts, which we interpret as a measure of the amount of investment in service technology deployed in the store. Although both measures could plausibly serve in either role, we define k_{1i} as the proxy variable, as store size is more likely to reflect inter-store variation in productivity than any measure of technology usage, and k_{2i} as the state variable. In our data, we also have indicators of whether the store sells gas or liquor. We use these discrete measures on gas and liquor sales as elements of the store-attribute ($\mathbf{z}$) vector in the production-function above in order to capture the effect of observed store-heterogeneity on output, and labor productivity.

Our measures of store-location allow us to test the impact of an entering hard-discounter on store productivity. We capture this effect in two ways. First, we calculate the distance between each store in our sample, and each hard-discounter.[14] We then use this measure to

[12]Using a more complicated functional form would allow for greater flexibility in interacting among inputs, but we opt to achieve econometric flexibility by allowing the parameters of our Cobb-Douglas model to vary across observations. We allow the marginal product of labor to vary over our sample, as in De Loecker and Warzynski (2012), but in a different, more parsimonious, way.

[13]Deflating store-level revenue by a retail-food price index to produce a physical measure of output is likely to induce measurement error in the dependent variable (De Loecker 2011b; De Loecker and Warzynski 2012). However, we assume this error is subsumed in the general econometric error term and, as such, does not affect the consistency of our estimates. Regardless, we deflated store-revenue with a range of different retail-price indices, and our estimates were qualitatively similar under each approach.

[14]TDLinx provides the exact location (latitude and longitude) of each store.

find the closest hard discounter, and define a continuous variable, $COMP$, as a continuous measure of the minimum distance between each store and the closest hard discounter. Second, we use this distance calculation to express competitive pressure as a binary variable. That is, we determine whether there is an outlet of the hard-discounter within 10, 5, or 3 kilometers ($OPEN10$, $OPEN5$, and $OPEN3$).[15] Our hypothesis suggests that an entering hard discounter is likely to reduce incumbent-store markups, which requires that we interact our competitive-pressure variables with the labor input in order to test the effect of entry on the marginal productivity of labor. We also include these competitive effects as intercept-shifting variables as entry is also likely to have a direct effect on store sales, and not just the efficiency of a variable input. Because these measures are likely to be highly collinear, we estimate different versions of the model, with each of these variables entering the model separately. We report our findings as to the differences in outcomes across these models in the Results section below.

Our static version of the ACF (2015) approach retains their core assumptions: (i) the unobservable is a scalar, and (ii) the proxy variable increases monotonically in the productivity shock.[16] Monotonicity is necessary so that we can write the unobserved shock to productivity as an arbitrary function of the state and proxy variables (k_{1i} and k_{2i}) and the vector of store attributes. With these assumptions, therefore, we write the demand for retail floor space as:

$$k_{1i} = h(k_{2i}, \omega_{it}, \mathbf{z}_{it}), \tag{5}$$

where each element of the vector $\mathbf{z}_{it}$ are potentially endogenous. The unobserved productivity shock for i at time t is therefore written in terms of the inverse-demand for k_{1i}, or:

$$\omega_{it} = h^{-1}(k_{1i}, k_{2i}, \mathbf{z}_{it}) = g(k_{1i}, k_{2i}, \mathbf{z}_{it}), \tag{6}$$

where $g(.)$ serves as an index of the productivity of store i.

[15] Our conversations with retail managers suggest that their locational-choice algorithm seeks to locate stores such that there are no direct competitors within a 5 kilometer (3 mile) radius, so our competitive variable is defined to capture a range around this measure. In this regard, our measure is very similar to that used by Ellickson and Grieco (2013) and Arcidiacono, et al. (2020), but defining our variable as binary avoids the measurement error introduced by counting stores, measuring store density, or regional share.

[16] Monotonicity means that $\partial k_{1i}/\partial \omega_{it} > 0$, and De Loecker and Warzynski (2012) explain that this assumption is true under any model of oligopolistic behavior, such as the Bertrand-Nash pricing assumption that is common in the retailing literature (Villas-Boas 2007). Mathematically, we require monotonicity in order to invert the demand curve for the intermediate input.

In order to express the output of store i in terms of only observable and random factors, we then follow the first-stage of the ACF (2015) procedure by substituting this expression for the productivity shock back into the production function to arrive at:

$$y_{it} = \beta_0 + \beta_l l_{it} + \beta_{k1} k_{1i} + \beta_{k2} k_{2i} + g(k_{1i}, k_{2i}, \mathbf{z}_{it}) + \eta_{it}. \tag{7}$$

Our first stage model consists of a non-parametric specification of output as a arbitrary function of proxy, capital and labor inputs: $\psi(l_{it}, k_{1i}, k_{2i}, \mathbf{z}_{it})$. There is some disagreement on the nature of this specification in the literature. While Olley and Pakes (1996) and Levinsohn and Petrin (2003) argue that the marginal product of labor is identified in (7), ACF (2015) maintain instead that the labor parameter is not identified as the amount of labor input is a simple, deterministic function of the other arguments of the production function. We follow ACF (2015) in this regard, as it is not necessary to use any estimates from the first-stage model. Our non-parametric approach consists of a locally-weighted regression model with arguments consisting of non-linear and interaction terms calculated from the values of labor, capital, and proxy variables in the original production model (Levinsohn and Petrin 2003). With this non-parametric model we obtain expected output values, $\hat{\psi}$, for any values of the parameters in, β, and use these estimates to generate a control function, CF_{it}, from the residuals of the locally-weighted regression model.

4.2.2 Stage Two: Estimating Retail Productivity

Our departure from ACF (2015) occurs in the second-stage estimation model. Whereas their underlying assumption maintains that the unobserved productivity shock follows a Markov process, we do not have the sort of panel data that would allow us to exploit a similar assumption. In their case, the Markov assumption means that the productivity shock can be expressed as a stochastic function of lagged-values of productivity and an error term. More generally, despite the number of applications of this model in the literature, we argue that most production data sets are likely not up to this task, so are left in a similar position as us. Instead, we assume that the productivity shocks represent idiosyncratic, store-level differences between stores in the same market. At the store level, differences in productivity between stores, most notably between stores in the same chain, are likely to reflect differences in management style, employee training, or local demand conditions that reflect deviations from average-store productivity. Said differently, the most important shock to productivity is one that causes stores to differ from each other, and not from past versions of the same store. In a mathematical sense, cross-sectional differences in productivity are reflected in the control function estimated in the first-stage model, CF_i, so the process underlying shocks to productivity for store i are given by:

$$\omega_{it} = CF_i(\overline{\omega}_t) + \xi_{it},$$

where $\overline{\omega}$ reflects industry-mean productivity.

In the second-stage model, therefore, we first subtract the CF_{it} value from output, and express the difference as a linear (in logs) function of the proxy variable, capital, labor, store attributes, and a new error term that reflects differences in productivity between store i and the market-average:

$$y_{it} - CF_{it} = y^*_{it} = \beta_0 + \beta_l l_{it} + \beta_{k1} k_{1i} + \beta_{k2} k_{2i} + \beta_z \mathbf{z}_{it} + \xi_{it} + \eta_{it}. \tag{8}$$

In this model, however, labor is expected to be correlated with ξ_{it}, so we estimate the second-stage model using an instrumental variables approach. Further, in our model, recall that there are also terms that reflect discounter-entry in the $\mathbf{z}$ store-attribute vector. Regardless of how these distance measures are calculated, each is endogenous to Lidl's decision to enter the market. Therefore, we include control functions for both the labor input *and* the decision to enter. As we described above, the primary instrument for the labor input consists of market-level wages for grocery store workers in the same county as the store in question. From first principles, we know that wages are expected to be correlated with each store's demand for labor if managers minimize cost, but are mean-independent of store-level output.[17] We also include a set of store, year, and state fixed effects as instruments.

Entry, however, presents a more difficult identification problem. An ideal instrument would represent a measure of market-level demand for space, and hence represent the general attractiveness of a particular location, and yet remain independent of any individual store's

[17] In very-low income areas of the country, the average wage in a particular industry may reflect aggregate purchasing power, but given the general income-inelastic nature of food demand it is not likely to affect store-level sales.

sales. For this purpose, therefore, we obtained ZIP-code level commercial real estate rental rates for retail stores. Commercial real estate prices are exogenous to the performance of any single store, and still reflect the demand for each location for retail purposes. As we report below, this instrument performs well in first-stage regressions on Lidl's decisions to enter each particular market. By adopting this control-function approach, we obtain consistent estimates of the output-elasticity of labor, the rest of the production-function parameters, and the implied markups from the De Loecker and Warzynski (2012) approach.

Others in this literature estimate production functions that are more deeply parameterized than the Cobb-Douglas, for the simple purpose of obtaining labor-elasticity estimates that vary over observations in the data set. Without further modification, a Cobb-Douglas production function implies an average value of the marginal product of labor (labor-elasticity) that is expected to represent all of the stores in the data. However, this assumption is unsatisfactory as there is likely to be considerable heterogeneity remaining in the data, even after accounting for the idiosyncratic productivity shocks above. Further, there are more parsimonious ways of estimating models like this that still allow for parameters to vary over cross-sectional observations in the data, without sacrificing degrees of freedom, or imposing additional structure on the model. Therefore, we account for unobserved heterogeneity in the data, and allow the marginal productivity of labor to vary across stores, by estimating a random-parameter version of the second-stage production function model introduced above. Each of the key production parameters is assumed to be normally distributed, with parameter-heterogeneity also dependent upon the entry variables described above. In this way, both the mean output levels for each store, and their markups, vary with Lidl's entry activity. We use this approach to test our core hypotheses, namely that store-output, and markups, are both likely to be affected by the entry of a hard-discounter.

4.2.3 Stage Three: Counterfactual Simulations

We conduct two counterfactual simulations in order to examine the potential effectiveness of responses by managers of traditional retailers to hard-discounter entry. We consider two types of responses: One on the demand side, store-traffic expansion following Briesch, Chintagunta, and Fox (2009), and the second on the cost-side, akin to the insights by Basker

(2012) on productivity improvements in general retailing.

For the first simulation, we consider a strategy that has been shown to successfully attract customers: a retailer's high-quality, private-label line. While Hökelekli, Lamey, and Verboven (2017) examine private-label strategies designed to defend market share (and profit) from hard-discounter incursion, they could not find a strategy among them (premium, economy, or standard tiers) that was actually successful. Moreover, their findings are based on counterfactual simulations of a demand-based model in which they add and remove private-label lines from a single category, but it is not plausible that adding or removing a product line from a single category will change the number of customers that choose one store over another.

Despite the fact that hard-discounters tend to be known for their private labels, we consider a strategy that is instead designed to play to a hard-discounter's weakness, namely their small assortment, and the proven value of large assortments in attracting customers (Briesch, Chintagunta, and Fox 2009). While our model does not directly include the attractive power of assortments, we simulate the impact of a hypothetical assortment increase by incrementing store traffic using magnitudes that consistent with the existing empirical literature. We do so in steps, from 1% to 10%, and measure the change in store profit that are implied from the structural markup model.

In the second simulation, we consider productivity-enhancing responses such as the technology-adoption examples in Basker (2012). Although the specific example in Basker (2012, electronic scanning technology) is not likely to be repeated in the same way, the size of the productivity gains (mean productivity gain = 4.5%) she describes are again both relevant and likely to describe real-world responses that traditional retailers would consider. Therefore, in this case, we use the existing productivity parameters, and simulate a range of productivity enhancements again from 1% to 10% in increments of 1%. For each gain in productivity, we calculate the implied change in store profit, taking into account the interaction effect between labor productivity and hard-discounter entry in the structural model. We report the results of both simulations in the next section.

5 Results

In this section, we first present results from a reduced-form estimation procedure, and then the structural estimates of our markup-and-entry model.

Our reduced-form estimates control for state, chain, and year fixed effects, as well as the full set of production inputs and store-attribute values. These findings are in Table 4 below. We estimate several versions of the model in order to examine the sensitivity of the core model parameters, that is, the labor elasticity of output, to changing the set of covariates. We estimate a basic model with only fixed effects and production inputs (Model 1), a model that includes store attributes (defined as whether the store sells gas or liquor, Model 2), a model that adds the distance from an entering hard-discounter as an output-shifting variable (Model 3), one that allows entry to affect output and the labor-elasticity (Model 4), and a model that defines entry as a discrete variable that takes a value of 1 if a hard-discounter enters within 5 kilometers (Model 5). From the results in this table, we see that the core output-elasticity estimates are relatively stable across the different specifications, and that the implied returns to scale (the sum of the elasticities) is not statistically different from 1.0.[18] Further, these reduced-form results show that output is greater for stores that sell both gas and liquor, all else constant, so there are clear opportunities for cross-selling services or alternative products in food retailing.

[table 4 in here]

Most important for our objectives, however, Model 3 shows that sales are significantly lower for stores nearer to an entering discounter than otherwise. But, controlling for the dual effects of entry on store output and labor efficiency (Model 4), the results in this table show that the main effect of entry on output is felt through the efficiency of labor, and not gross output per se. In other words, each worker is able to generate less dollar sales than without entry, which we interpret as implying that average prices are lower across the store as management meets the new competitive pressure from the entering hard discounter. In Model 4, however, the direct effect of entry on store sales is not statistically significant, while

[18] We would expect that the returns to scale are not statistically different from 1.0. If it were statistically different from 1.0, this result would imply that the retailers in our sample are not minimizing cost in a long-run equilibrium, which is contrary to the underlying assumptions of the model.

it is in Model 5. We interpret the positive effect of entry on store sales, controlling for the effect on labor productivity, as meaning that the entering hard-discounter drives incremental traffic to the area around the store, but forces prices down in equilibrium. Customers who travel to the hard-discounter for low prices may not be able to find the products they want in every category, however, in particular national brands (Hökelekli, Lamey, and Verboven 2017), so they shop at the nearest full-service supermarket in order to top off their baskets.

While the reduced-form model results are suggestive of deeper patterns in the data, they are likely to be biased for the reasons cited above. After controlling for the endogeneity of entry and of labor inputs, we obtain structural estimates of labor-productivity on a store-level basis, and use these estimates to infer values for the markup over marginal cost. These estimates are in Table 5 below. In this table, we again report estimates from models that consider various definitions of entry, from the distance to an entering hard-discounter (Model 1) to a binary indicator of a discounter within 3 kilometers (Model 2), 5 kilometers (Model 3), and 10 kilometers (Model 4). According to the goodness-of-fit statistics reported in this table, it appears as though Model 4 provides the best fit to the data among the "binary" models, although each fit the data slightly worse than the continuous definition in Model 1. Because of the similarity in fit, and magnitude of the estimated parameters, we interpret the parameters from Model 4, simply due to the fact that we regard the binary definition of entry as the most consistent with how entry likely impacts retailers' decisions in practice. That is, they are not likely to be affected by a discounter outside of what they regard as their market area, but will respond to what they perceive as a direct competitor.

[table 5 in here]

Relative to the reduced-form estimates in Table 4, we see that controlling for both labor and entry endogeneity leads to larger estimates for the output-elasticity with respect to labor. According to the theoretical model of markup determination (Hall 1988), this implies higher markups after controlling for endogeneity. In Model 4, the results also show a negative marginal value of selling liquor, all else constant. Although liquor is notoriously a high-volume, high-margin category, it may be the case that retailers have to employ additional labor to manage the category, so controlling for labor-endogeneity removes any economic benefits to selling liquor. Most importantly, however, the entry effects from Table 4 are confirmed in this case. Namely, we find a similar pattern of entry-effects on incumbent retailers: Store output rises for stores that are near an entering hard-discounter (by an average of 1.9%, using the data in Table 1), but margins fall. Relative to the reduced-form evidence, our structural model shows an attenuated store-output effect, but an accentuated impact on labor-efficiency. As in the previous table, we interpret this result as implying that entering discounters drive traffic to surrounding stores that provide more variety, but increase pricing pressure on nearby retailers, reducing store-level markups.[19]

Our estimates represent non-trivial changes in profitability for stores exposed to hard-discounter competition. In our preferred model (Model 4), the difference in markups implied by these estimates represents a 7.2% reduction in the price-cost margin. While this may seem to be a small impact, retail food net margins, which also take into account fixed costs of operation, tend to be roughly 2% (Campbell 2020). A 7% reduction in the amount of margin available to pay fixed costs, therefore, can be substantial. For example, supposed a 2% margin represents a 50 pound bundle of groceries that sell for $2.00 per pound, with a cost-of-goods-sold of $1.00 per pound, and a fixed cost of $48.00 for the store. Net profit is $2.00 on sales of $100.00. If the price-cost markup falls from $1.00 to $0.93 per pound, then this 2% net margin becomes a loss of 1.5%. Over the longer term, bankruptcy ensues, regardless of the total sales effect implied by our model.

The simulation exercise provides important managerial insights into the type of defensive strategies that are likely to be successful, or not. Our first experiment, which uses a hypothetical change in store-assortments among traditional retailers as a means of attracting additional customers, is successful in generating additional profit throughout the full range of the simulated store-preference parameter (Table 6). However, this effect shows clear diminishing marginal returns as the gains disappear after a 10% increase in store-demand. In the second experiment, increasing the productivity of store labor, we find that the initial gains are maximized through a 3% gain in productivity, but the interaction with disounter-entry dominates after that point. Because markups are diminished by hard-discounter entry, and

[19] We assume that hard-discounter entry has only a modest impact on retailing costs. This assumption is reasonable because even Walmart carrying a larger assortment of national brand products has a minimal impacts at the wholesale market level (Huang et al. 2012).

depend critically on labor productivity in our theoretical framework, higher productivity raises store profit through the direct effect, but reduces it due to the entry interaction. Intuitively, if a retailer becomes too profitable due to labor-related innovations, a hard-discounter will interpret it as a market opportunity, enter, and reverse any gains in profit that were available.

[table 6 in here]

Our findings are important to understanding both the broader, economic impacts of hard-discounter entry, and potential managerial responses. First, although we do not calculate the implied welfare effects on competing firms and consumers that buy from these retailers, it is clear from our results that producer surplus is likely to fall due to lower markups, but consumer surplus will rise for the same reason. To the extent that incumbent retailers may also reduce their SKU count in order to compete with hard discounters, consumers may also experience a reduction in welfare from a loss of variety. In general, however, lower prices tend to be net positive for consumers. Second, retail managers need to be aware of the potential consequences due to the entry of paradigm-changing retailers like hard-discounters, and draft responses that are least likely to be self-destructive. For instance, Arcidiacono et al. (2016) describe the impact on local markets due to Walmart's entry into retail markets throughout the 1990s and 2000s. While the common perception was that "main street" would suffer most, it was instead the direct competitors to Walmart – the supermarkets that competed most directly with Walmart's primary line of business. In fact, retailers that were sufficiently differentiated from Walmart were less affected, if at all. Our findings make a similar case, namely that responding by reducing prices and SKUs in order to match the hard-discounters' business models will likely end in failure, but differentiating by emphasizing variety, quality, and sharply differentiated store brands (Hökelekli, Lamey, and Verboven 2017) is more likely to be successful.

6 Conclusions and Implications

In this paper, we examine the impact of hard-discounter entry on incumbent retailer markups. Because retailers tend to sell thousands of items, we argue that the traditional demand-side approach to entry is intractable for a problem like this, so adopt the production-side markup-estimation approach of De Loecker and Warzynski (2012) for this purpose. Although this approach has been used extensively to estimate the effect of trade status on manufacturing margins (De Loecker 2007) or corporate markups in general (De Loecker, Eeckhout, and Unger 2020), we are the first to use this approach to examine retail-store performance.

We find that, averaged over all incumbent retailers and markets, markups fall by over 7.0% in response to hard-discounter entry, although total store sales rise by nearly 2.0%. There are two mechanisms that explain these effects: First, competitive pressure from entering hard-discounters causes retail prices, and hence markups, to fall and, second, customers shop in nearby stores in order to fill their baskets with items that are not available at the hard-discounter. This finding is remarkably consistent with others who document this same inter-store complementarity effect using entirely different methods and data (Vroegrijk, Gijsbrechts, and Campo 2013). Higher sales, however, are likely irrelevant if the lower markup causes already-low-margin retailers to sell at a loss.

Our empirical model has both macroeconomic and managerial significance. If the spread of hard-discounters leads to lower retail prices for food, then consumers will almost certainly benefit in terms of total welfare. However, if hard-discounter entry leads to the exit of grocery stores in nearby markets, that were perhaps already marginally suited to traditional grocery retailers, then the "food desert" problem may be accentuated in these markets (Allcott, et al. 2019). Finding that store sales rise in response to new entry suggests a way out of the implied competitive trap for existing retailers – rather than respond to entering retailers by restructuring in order to "beat them at their own game," our findings suggest instead that retailers are better off pivoting away from these new formats, offering instead what hard discounters do not. For example, this means offering a greater variety of national brands, and perhaps store brands that offer more favorable price-quality perceptions than those offered by the hard discounters (Cleeren, et al. 2010; Lourenco and Gijsbrechts 2013).

Our research has some limitations. First, the TDLinx data that forms the core of our empirical model only includes estimates of the output for each sample store. However, Nielsen, the provider of TDLinx, has a commercial stake in ensuring the quality and accuracy of these estimates, so are data are likely as good as any available alternative. Second, we estimate a relatively simple production surface for our sample retailers. While others in this literature estimate translog models in order to capture interactions between inputs, and other sources of non-linearity, we take a more parsimonious approach, and introduce flexibility through the use of a random-parameter production specification. To the extent that our approach does not capture the same underlying structure, our estimates will be in error. Third, the retailer that forms the focus of our paper entered only a limited number of markets (10 states) over a 3-year period. A longer time-series of entry events would have likely provided sharper identification conditions for our core model. That said, the effects we describe appear to be precisely estimated, so it may be the case that more data were not needed.

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Table 1. Summary of Retail Markup Estimation Data

Variable	Source	Units	Mean	Std. Dev.	Minimum	Maximum	N
Output	TDLinx	$,000 / wk	235.382	280.727	5.0	2,500.0	85938
Store Size	TDLinx	,000 sq. ft.	20.012	19.699	1.0	126.0	85938
Employees	TDLinx	#	53.443	83.542	1.0	781.0	85938
Checkouts	TDLinx	#	7.185	6.896	1.0	59.0	85938
Gas?	TDLinx	1/0	5.910%	23.582%	0.0%	100.0%	85938
Liquor?	TDLinx	1/0	4.080%	19.782%	0.0%	100.0%	85938
Wages	BLS, QCEW	$ / week	420.099	126.202	0.0	1,610.0	85938
Retail PPI	BLS	Index	161.056	7.753	147.5	171.2	85938
Population	ACS	# 0,000	90.356	73.561	6.2	264.9	85938
Household Size	ACS	#	2.627	0.227	2.0	3.4	85938
Unemployment	ACS	%	5.918%	2.048%	1.1%	17.8%	85938
Income	ACS	$ 0,000	9.135	2.648	4.1	17.7	85938
Real Estate Price	CoStar	$ / sq. ft.	26.738	20.970	6.1	258.8	85938
Distance	TDLinx	Km	187.618	176.495	0.0	915.9	85938
10 KM?	TDLinx	%	3.301%	17.867%	0.0%	100.0%	85938
5 KM?	TDLinx	%	1.531%	12.280%	0.0%	100.0%	85938
3 KM?	TDLinx	%	0.877%	9.326%	0.0%	100.0%	85938

Note: Data sources are Nielsen-TDLinx, CoStar commercial real estate prices, Bureau of Labor Statistics (wages from QCEW, Retail PPI from Producer Price Index), Bureau of Census (demographic variables from American Community Survey). Distance is in km from nearest hard-discounter. The units 1/0 indicate binary variable.

Table 2. Summary Evidence of Entry

Measure	Mean	Std. Dev.
Non-Entry Markets		
Sales	\$234.20	\$280.69
Sales / FTE	\$5.41	\$3.54
Entry Markets		
Sales	\$311.26	\$272.42
Sales / FTE	\$6.29	\$4.27

Note: Entry markets defined as one in which a hard-discounter entered within 5 km. Sales are in millions of dollars. FTE = full time equivalent.

Table 3. IV Estimates for Labor and Entry Variables

	Entry Model		Labor Input Model	
	Estimate	Std. Err.	Estimate	Std. Err.
Constant	0.0688	0.0063	6.0935	0.1434
State 1	-0.0487	0.0047		
State 2	-0.0572	0.0019		
State 3	-0.0571	0.0022		
State 4	-0.0510	0.0020		
State 5	-0.0562	0.0017		
State 6	-0.0258	0.0018		
State 7	-0.0573	0.0018		
State 8	-0.0178	0.0023		
Population	-0.0092	0.0008		
Household Size	0.0132	0.0021		
Unemployment	-0.3761	0.0266		
Income	-0.0022	0.0002		
CRE Rent	0.0003	0.0000		
Ave. Wage			-0.4664	0.0236
F	225.4210		391.4771	
R^2	0.0330		0.0045	

Note: Demographic data from American Community Survey, CRE (commercial real estate) rent from CoStar, and wages are from QCEW (US Bureau of Labor Statistics). A single asterisk indicates significance at a 5% level.

Table 4. Reduced-Form Production Function Estimates

	Model 1		Model 2		Model 3		Model 4		Model 5	
Variables	Mean	Std. Err.	Mean	Std. Err.	Mean	Std. Err.	Mean	Std. Err.	Mean	Std. Err.
Labor	0.6004*	0.0019	0.5995*	0.0019	0.5993*	0.0019	0.6120*	0.0047	0.5997*	0.0019
Capital 1	0.1637*	0.0019	0.1630*	0.0019	0.1633*	0.0019	0.1631*	0.0019	0.1629*	0.0019
Capital 2	0.1941*	0.0029	0.1950*	0.0029	0.1948*	0.0029	0.1948*	0.0029	0.1950*	0.0029
Gas			0.0673*	0.0049	0.0675*	0.0049	0.0669*	0.0049	0.0675*	0.0049
Liquor			0.0192*	0.0061	0.0208*	0.0062	0.0221*	0.0062	0.0203*	0.0061
Entry					-0.0038*	0.0015	0.0037	0.0030	0.1242*	0.0357
Entry*Labor							-0.0022	0.0007	-0.0217*	0.0096
Chain Effects	Yes		Yes		Yes		Yes		Yes	
Year Effects	Yes		Yes		Yes		Yes		Yes	
State Effects	Yes		Yes		Yes		Yes		Yes	
Random Parameters	Yes		Yes		Yes		Yes		Yes	
AIC / N	0.752		0.751		0.751		0.751		0.750	
LLF	-3.154		-3.207		-3.208		-3.207		-3.207	

Note: All covariates are in natural logs. Model 1 is the core Cobb-Douglas production function with random-parameters, and no other variables. Model 2 is Model 1 with store attributes. Model 3 includes entry defined as distance to hard-discounter affecting mean sales. Model 4 is Model 3 with labor and entry interacted, while Model 5 includes entry defined as discrete indicator for new discounter within 5 km. AIC is the Akaike Information Criterion. Note: Data for all models from Nielsen TDLinx, BLS-QCEW, by store. For all models, N = 85,938. A single asterisk indicates significance at a 5% level.

Table 5. Structural Production Function Estimates

	Model 1		Model 2		Model 3		Model 4	
Parameter Means	Mean	Std. Err.	Mean	Std. Err.	Mean	Std. Err.	Mean	Std. Err.
Labor	0.6625*	0.0113	0.6355*	0.0113	0.6385*	0.0112	0.6425*	0.0111
Capital 1	0.1517*	0.0011	0.1537*	0.0011	0.1528*	0.0011	0.1513*	0.0011
Capital 2	0.2313*	0.0016	0.2361*	0.0016	0.2341*	0.0016	0.2309*	0.0016
Gas	0.0376*	0.0029	0.0396*	0.0029	0.0392*	0.0029	0.0384*	0.0029
Liquor	-0.0255	0.0038	-0.0284*	0.0039	-0.0281*	0.0038	-0.0277*	0.0038
Entry	0.0118*	0.0016	0.0200	0.0278	0.0419*	0.0194	0.0463*	0.0120
Entry*Labor	-0.0041*	0.0004	-0.0414*	0.0101	-0.0462*	0.0073	-0.0432*	0.0049
Labor Control	-0.0026*	0.0110	-0.0023	0.0111	-0.0036	0.0110	-0.0052	0.0109
Entry Control	0.0249*	0.0067	0.0132	0.0088	0.0022	0.0012	0.0089	0.0076
Chain Effects	Yes		Yes		Yes		Yes	
Year Effects	Yes		Yes		Yes		Yes	
State Effects	Yes		Yes		Yes		Yes	
Random Parameters	Yes		Yes		Yes		Yes	
AIC / N	-0.393		-0.389		-0.390		-0.393	
Chi-Square	33,859.6		33,542.4		33,649.9		33,848.6	
Markups No Entry	1.652		1.585		1.592		1.602	
Markups Entry	1.595		1.481		1.477		1.494	

Note: All covariates are in natural logs. Model 1 defines competitive pressure as distance from entering hard discounter (kilometers), Model 2 defines competitive pressure as a binary indicator of hard-discounter entry within 3 kilometers, Model 3 is Model 2 with entry expanded to 5 kilometers, and Model 4 is the same model with entry defined as 10 kilometers. All other arguments are the same. AIC is the Akaike Information Criterion. Note: Data for all models from Nielsen TDLinx, CoStar, ACS, and BLS-QCEW. Chi-square statistic compares the log-likelihood of the estimated model relative to a naïve model. A single asterisk indicates significance at a 5% level.

Table 6. Entry Response Simulations and Gross Margin

	Scenario			
	Demand		Productivity	
Change	Mean	Std. Dev.	Mean	Std. Dev.
Baseline	$118.986	$13.609	$118.986	$13.609
+1%	$124.575	$11.851	$126.392	$14.140
+2%	$129.863	$12.072	$129.507	$14.363
+3%	$134.787	$12.270	$130.045	$14.398
+4%	$139.280	$12.441	$129.085	$14.323
+5%	$143.268	$12.581	$127.278	$14.185
+6%	$146.672	$12.687	$125.003	$14.014
+7%	$149.409	$12.754	$122.482	$13.824
+8%	$151.392	$12.779	$119.840	$13.627
+9%	$152.530	$12.758	$117.150	$13.426
+10%	$152.735	$12.688	$114.451	$13.226

Note: Demand scenario represents increase in store demand due to assortment change; Productivity scenario represents increase in labor productivity due to labor-saving technology adoption. Gross margin defined as weekly store revenue above labor expense; in thousands of dollars.