# The Effect of Social Interaction on Economic Transactions: Evidence from Two Retail Format Changes

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**Abstract**

Examining two different changes in retail format, we show that consumers alter their purchase behavior when the retail context changes. In both settings, the change in purchasing behavior is consistent with the effect being driven by an interpersonal interaction between the consumer and the retail clerk. We therefore model the change in format as a "social friction" in which consumers may not purchase what they want most because of an implicit cost of purchasing some items in social settings. Taking this model to data in one of our retail settings, we show that reducing social frictions increased consumer surplus by 5.4% and producer surplus by 3.5%.

*JEL: D12, L81, L86*

*Keywords: social cues; consumer choice; retail; social frictions*

# 1 Introduction

Retailers face a key choice in deciding how customers can purchase their products. Such choices include non-store retailing, self-service, self-selection, limited-service, and full-service (Kotler & Keller 2009). Using two different changes in retail format, we examine how the interpersonal nature of retail format might affect consumer purchase decisions.

In our first setting, we use data from a field experiment conducted by Sweden's government-run alcohol monopoly retailer, Systembolaget, in which stores changed formats from behind-the-counter to self-service. From seven pairs of matched towns, each with a single retail outlet, we show that the stores randomly converted to self-service sell a greater variety of products (as defined by a less-concentrated sales-distribution), with a significant fraction of this change coming from products with difficult-to-pronounce names. Products with difficult-to-pronounce names might experience such a sales increase because consumers may fear being misunderstood or appearing unsophisticated if they mispronounce a name when ordering from a sales clerk; once a store introduces a self-service format and eliminates the need to pronounce a name, consumers may become more comfortable pursuing an otherwise mildly embarrassing or frustrating transaction. The market share of products with difficult-to-pronounce names increases a statistically significant 8.4% in stores that switch to self-service. Further analysis suggests this increase is likely due to an aspect of the interpersonal interaction between the consumer and clerk. Therefore, we argue that the increase in sales of difficult-to-pronounce products is driven by the reduction in interpersonal interaction in the selling process.

In our second setting, we use individual-level panel data from a pizza delivery restaurant that introduced a Web-based ordering system to supplement its phone and counter service. Comparing sales from before and after the advent of online ordering, we document a considerable change in consumers' purchases toward higher calorie and more complex items. The increase in high calorie items might be driven by a desire to avoid negative social judgment of their eating habits, while the increase in the complexity might be driven by a desire to avoid negative social judgment by appearing difficult or unconventional.[1] The average item in an online order is a statistically significant 14% more complex and has a statistically significant 3% more calories. Importantly, we exploit several institutional details to provide supportive evidence for the hypothesis that the less-social nature of online transactions drives these differences.

Combined, these findings suggest that interpersonal exchange affects the types of products purchased by consumers. In light of these descriptive results that suggest changes in the degree of interpersonal interaction lead to changes in purchase behavior, we model this change in context as a "social friction" that imposes a (perhaps heterogeneous) cost on purchasing some products but not others. We structurally estimate this model using the individual-level data from the pizza delivery setting and show that reducing social interaction through online ordering has increased measured consumer surplus by 5.4%. Moreover, we estimate that producer surplus has increased 3.5% due to non-verbal online orders.

The institutional details of both settings help us better isolate the effect of social interactions on market outcomes while allowing us to rule out several alternative explanations for our results.

First, the products and prices remain fixed for each of our settings, reducing concerns that concurrent institutional changes cloud our results. For example, retail formats may differ in ways beyond just the extent of social interaction. The panel nature of both settings — and the field experiment used in the alcohol setting — reduce the concern

[1] It is well-documented that people change their eating habits in social situations. For example, Polivy et al. (1986) show from an experiment that subjects eat less when they believe others will be aware of their consumption and Ariely & Levav (2000) show that the desire to impress a clerk by order low-calorie items changes restaurant ordering behavior. The popular culture also reflects the idea that making complicated orders could be seen as being difficult. For instance, the movie 'When Harry Met Sally' provides a memorable example in which Harry has a negative view of Sally's "high maintenance" ordering habits (http://www.youtube.com/watch?v=cnlm2e3EN78s accessed January 28, 2014). Theories of impression management (Goffman 1959, Banaji & Prentice 1994) suggest that complexity may cause embarrassment or frustration if customers fear appearing difficult or unconventional. For example, in their study "Who is Embarrased by What", Sabini et al. (2000) use a customer returning to a store several times as one of several embarrassing situations they study. Belk (1980) shows that unconventional consumption choices yield an unfavorable impression. Olsson et al. (2009) discuss how special requests can be embarrassing. Even among patients with above average education and knowledge, the fear of being seen as difficult or demanding or taking time from others can prevent them from discussing their care with their doctors (Aldred et al. 2005, Boyd et al. 2004, Frosch et al. 2012).

that these other factors confound our findings.

Second, the straightforward menus and webpage in our settings, as well as the nature of the products themselves, allow us to provide evidence that search and learning are unlikely to drive our results. For example, in the alcohol setting, the increase in sales comes from difficult-to-pronounce products in particular, rather than from the broader set of historically unpopular products. Thus, the identification assumption is that two products at a similar level of sales are similarly familiar and see a similar change in search costs, even if one is more difficult to pronounce than the other. We test this, for example, by conditioning on country of origin. In the pizza setting, the website does not have sophisticated search tools that Brynjolfsson et al. (2011) argue might confound a comparison of different retail formats. Furthermore, results are robust to focusing only on customers likely to have a menu when they order.

Third, similar settings have been considered extensively in the economics and management literatures to study sales distributions (Pozzi 2012, Brynjolfsson et al. 2003), search costs (De los Santos et al. 2012), and economic efficiency (Seim & Waldfogel 2012). Thus, our settings are firmly in the mainstream and complement previous studies by explicitly examining the impact of social frictions on market outcomes.

Fourth, while not from an experiment, the pizza data allow us to control for individual-level tendencies and selection into the online channel because the transaction history includes customers who purchased from the store both before and after online ordering became available, reducing concerns over selection bias. Combined with information on profit margins, the pizza data also permit us to estimate the changes in consumer and producer surplus attributable to online ordering.

Fifth, the pizza data allow us to show that the social friction is unlikely to be driven by consumers' desire to avoid misunderstandings while ordering. Although we cannot reject this explanation in the alcohol setting, in the pizza setting we show that customers who made more complex or error-ridden orders before online ordering was available are not more likely to make subsequent orders online. Moreover, instructions that are trivial to make on both channels but associated with more calories and complexity, such as ordering double toppings, appear more often in online orders. For these reasons, we argue that concerns over mistakes in complicated orders do not primarily explain the markedly different choices consumers make online.

The notion that individuals avoid potentially uncomfortable social interactions has received considerable attention in sociology, psychology, medicine, and political science. The foundation for these ideas dates back (at least) to Goffman's claim that social interactions are performances in which individuals act to project a desired image of themselves (Goffman 1956, 1959); that is, individuals alter their behavior to project a positive image of themselves, in order to help someone else avoid embarrassment, or to avoid being embarrassed themselves.

Much of the existing literature that documents how social interaction affects behavior emphasizes embarrassment. Goffman defines embarrassment as a social phenomenon in which the desired projection of the self is disrupted. While shame may happen in solitude, embarrassment requires the presence of at least one other person. In their review article on the psychology of embarrassment, Keltner & Buswell (1997) discuss how a fear of embarrassment harms individuals as they take self-destructive steps to avoid embarrassment in social situations. For instance, a fear of embarrassment leads patients to delay seeking medical help for chest pain (Meischke et al. 1995), as well as for more sensitive conditions such as urological and breast cancers (Chapple et al. 2004, Lerman et al. 1990, McDevitt & Roberts 2011). Others have shown that embarrassment can affect voting choices (Niemi 1976), alter food consumption (Lee & Goldman 1979, Polivy et al. 1986, Banaji & Prentice 1994, Roth et al. 2001, Allen-O'Donnell et al. 2011), and stifle contraceptive purchases (Dahl et al. 1998). Within this vein, removing even one layer of social interaction by using electronic questionnaires rather than in-person interviews at doctors offices significantly increases patients' willingness to report incidents of domestic abuse (Ahmad et al. 2009).

Our paper contributes to this literature by applying an economic perspective to the evidence that social interaction changes behavior, modeling the change as the result of a higher implicit cost of some actions in social situations. Our paper is therefore part of a growing literature that takes an economic perspective to ideas from sociology and psychology in order to examine the economic impact of emotions and social cues. Recent economics studies have shown that anger following a loss by the local football team leads to increased violence (Card & Dahl 2011), that emotions affect time preferences (Ifcher & Zarghamee 2011), and that guilt impacts family resource allocations and money transfers (Li et al. 2010). Other research has shown that social cues may influence individuals' choices. For instance, Akerlof & Kranton (2000) and Akerlof & Kranton (2008) show that social identity affects how individuals behave; Ariely & Levav (2000) find that social norms change variety-seeking behavior; and Rabin (1993) and Fehr et al. (1993) document that perceptions of fairness influence actions both in theory and in practice. Similarly, DellaVigna et al. (2012) show that "social pressure costs" reduce donors' welfare in door-to-door fundraising and impact charitable giving.

Also closely related to our framework is the model of privacy in Daughety & Reinganum (2010), where they derive a demand for privacy within a model in which agents receive utility from other agents' perceptions of their type; when actions are public, "social pressure" influences individuals' choices. In some sense, our analysis examines the basic assumption of this model: whether social pressure does indeed affect choices.

Related to its implications for privacy, our paper contributes to the Internet economics literature by explicitly examining the effect of social interaction on market outcomes. The perceived anonymity of digital technology (perhaps best captured in a 1993 *New Yorker* cartoon showing a dog sitting at a computer saying, "On the Internet, nobody knows you're a dog") has been credited with an increase in the distribution of pornography (Edelman 2009) and with the recent bestseller status of erotica novels such as *Fifty Shades of Grey* (Rosman 2012). To this point, Griffiths (2001) asserts that internet pornography is popular because "it overcomes the embarrassment of going into shops to buy pornography over the shop counter," a phenomenon Coopersmith (2000) labels a "social transaction cost." While a lengthy social psychology literature has studied how a lack of personal interaction affects online behavior (Gackenbach 2007), labeling it the "online disinhibition effect" (Suler 2004), no work (to our knowledge) has examined its implications for market outcomes.

The purpose of our paper is therefore to formalize and measure the impact of retail context on market outcomes across two common retail settings. We proceed by first detailing the results from a field experiment that moved alcohol purchases from behind the counter to self-service, providing evidence that difficult-to-pronounce products experienced a disproportionately large increase in sales. We then document a change in sales patterns at a pizza delivery restaurant after the introduction of online ordering, providing evidence of a rise in high calorie and complex orders. We then model the change in retail format as removing a "social friction" that prompts customers to make more complicated orders. We structurally estimate this model, and the results suggest a substantial impact of "social frictions" on consumer and producer surplus. We conclude by summarizing our results, discussing their limitations, and speculating about their broader implications.

# 2 Systembolaget's Sales Format Experiment

## 2.1 Data and Setting

In our first setting, we examine a field experiment conducted in the early 1990s by Systembolaget, Sweden's government-run alcohol retail monopoly.[2] For Sweden's 1990 population of 8.5 million, Systembolaget operated approximately 400 stores across the country. Outside of these stores, Swedish law prohibits the sale of wine, distilled spirits, and strong beer (above 3.5% ABV). Systembolaget's directive stipulates that the organization's sole purpose is to minimize alcohol-related problems by selling alcohol in

[2] Much of this description comes from Skog's (2000) assessment of the experiment's impact on alcohol consumption. Skog showed that sales went up by a similar magnitude to what we find. Skog did not examine the distribution of sales nor did he examine whether difficult-to-pronounce products had the largest increase.

a responsible way. As such, it prohibits profit maximization from being an aim of the organization and dictates that no brands or suppliers be given preferential treatment. Instead, their objective function is some (not clearly specified) weighting of goals such as controlling alcoholism, promoting customer and employee satisfaction, and being financially efficient.[3]

Prior to 1989, all transactions at Systembolaget's stores occurred behind the counter, whereby customers approached the counter and ordered from a clerk who then retrieved items from a storeroom. In 1989, Systembolaget began to explore the impact of adopting a self-service format. To identify the likely effects of self-service and reduce the chances of simply cannibalizing sales across stores, Systembolaget chose 14 relatively isolated towns, each with a single Systembolaget store, to participate in a field experiment.[4] According to Skog (2000), Systembolaget used the 1984 to 1989 period to match towns into seven pairs "in such a way as to make the members of each pair as similar as possible in terms of population size, economic bases and sales of alcoholic beverages; the latter both in terms of volume per capita and pattern of variation over time." Systembolaget also chose pairs sufficiently far apart to prevent spillover effects and randomly selected the store converted to self-service within each pair. Table 3 lists the pairs of stores and their characteristics.

Table 1: Summary statistics for Systembolaget stores in the field experiment as of Jan. 1991.

| Pair | Town | Treatment or Control | Date of Change | Town Population | Sales (Units) | Herfindahl | Revenue (Kr. mil.) |
|---|---|---|---|---|---|---|---|
| 1 | Filipstad | Treatment | June 1991 | 13296 | 58413 | 0.0296 | 234.7 |
| 1 | Nybro | Control | None | 20997 | 53542 | 0.0184 | 281.0 |
| 2 | Köping | Treatment | July 1991 | 26345 | 97701 | 0.0215 | 418.0 |
| 2 | Säffle | Control | None | 17960 | 46807 | 0.0207 | 223.2 |
| 3 | Vänersborg | Treatment | Nov. 1991 | 36734 | 99028 | 0.0144 | 449.0 |
| 3 | Lidköping | Control | None | 36097 | 84143 | 0.0163 | 374.4 |
| 4 | Motala | Treatment | May 1992 | 42223 | 92758 | 0.0155 | 441.3 |
| 4 | Falun | Control | None | 54364 | 123305 | 0.0094 | 614.2 |
| 5 | Karlshamn | Treatment | Sept. 1993 | 31407 | 82538 | 0.0145 | 425.8 |
| 5 | Lerum | Control | None | 33548 | 88043 | 0.0167 | 345.5 |
| 6 | Ludvika | Treatment | Sept. 1994 | 29144 | 78178 | 0.0237 | 371.6 |
| 6 | Vetlanda | Control | None | 28170 | 65646 | 0.0192 | 307.0 |
| 7 | Mariestad | Treatment | Jan. 1995 | 24847 | 92972 | 0.0140 | 427.6 |
| 7 | Värnamo | Control | None | 31314 | 88514 | 0.0141 | 424.1 |

[3] See http://www.systembolaget.se/English/Our-mandate/

[4] Because the experiment was restricted to one-store towns, Stockholm and the other major cities in Sweden are not in the data.

Several institutional details make Systembolaget's experimental design an appealing empirical setting for our analysis. First, prices and product offerings did not change in the converted stores relative to the control stores during the experiment — only the format of the stores changed. As a result, endogenous changes in prices and product offerings will not confound any observed changes in sales patterns. Second, Systembolaget is a monopoly seller of alcohol (above 3.5% ABV) within Sweden, and therefore, because there are no competitors, there are no competitive responses to the format change outside of the weak beer and non-alcoholic drink segments. Third, according to the 2007 annual report, prices are based on a fixed (legislated) per-unit markup. Fourth and finally, Sweden prohibits advertising and promotions for alcohol above 2.25% ABV (though foreign magazines sold in Sweden may carry alcohol advertisements).

Systembolaget lists each item for sale at its stores in a menu. Every store provides the same menu (though they may stock different items), with Figure 1 showing a sample page from a 1996 menu. The menu lists product names (sorted by category and price) and prices, and is especially important at stores with behind-the-counter service because customers cannot simply pick up a bottle from the shelf before purchasing it. At behind-the-counter stores, shown in Figure 2, customers approach the counter and order verbally (with the option of pointing to an item on the menu); the staff then retreat to the back of the store to retrieve the items. At self-service stores, shown in Figure 3, customers make their selections from the shelves where items are arranged by category and price, with each item given shelf space roughly in line with its popularity (recall that Systembolaget is brand-neutral by its directive in the sense that there are no slotting allowances or promotions that could change a particular brand's placement); customers then bring their selections to the cash register for purchase. Thus, the key changes in the experiment are that (i) customers may browse the aisles of products on display and (ii) customers need not ask a clerk for a product. For these reasons, if social frictions do impact consumers, then the format change should disproportionately affect difficult-to-pronounce products, rather than the broader set of products with historically lower sales for which browsing shelves may represent a type of learning or search process by consumers.

Our data contain monthly sales and prices for each product at the 14 stores in the experiment from January 1988 to December 1996, with products divided into seven categories: vodka, other spirits, wine, fortified wine, Swedish beer, imported beer, and non-alcoholic drinks.[5] Category-by-category results are shown in the appendix.

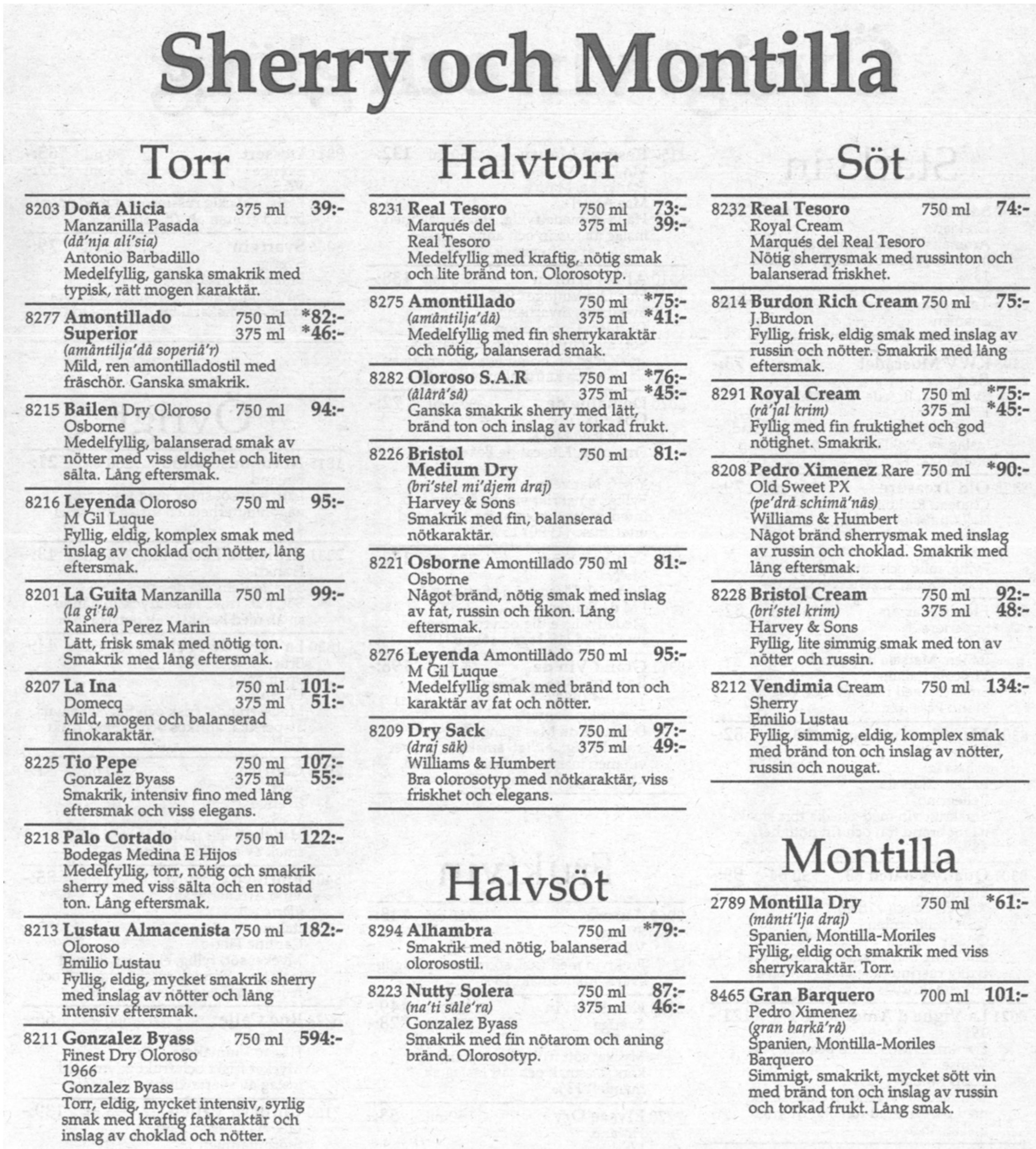
We examine the data at the store-category-month level. We first show how a store's format affects the variety and quantity of products purchased by consumers, with variety measured using a Herfindahl index of the sales concentration for each category in each store; this is the sum of the squared market shares of the products (stock-keeping units) in each store-category-month. Table 2 provides descriptive statistics, and Table 3 compares the treatment and (paired) control stores before and after the treatment stores changed format. The raw averages show that the Herfindahl fell faster in the treatment stores than the control stores and that the share of sales from difficult-to-pronounce products rose in the treatment stores but fell in the control stores.

We next show the differential sales patterns for difficult-to-pronounce products, which we classify using three distinct measures. First, we identify whether the menu provides a pronunciation guide for the product. As shown in Figure 1, several product listings are accompanied by a phonetic spelling of the product's name. We interpret the presence of these guides as indicating that a name is difficult to pronounce and use this as our primary measure. Notably, the inclusion of a pronunciation guide varies across products' countries of origin, with just 4% of Swedish products given guides compared to 78% of French products;[6] we will control for such regional variation in many of the specifications below. Second, we use the number of characters in the product's name. Third, we use the assessments of three native Swedish speakers hired to evaluate the difficulty of pronouncing each product listed in the January 1991 menu.[7]

[5] We also have data on product availability and popularity from January 1984 to December 1987.

[6] In total, France represents 35% of difficult-to-pronounce products and we therefore show below that the results are not driven by a change in sales of French products overall.

[7] Details of this exercise appear in the appendix.



# Sherry och Montilla

## Torr

8203 **Doña Alicia** 375 ml **39:-**
Manzanilla Pasada
*(då'nja ali'sia)*
Antonio Barbadillo
Medelfyllig, ganska smakrik med typisk, rätt mogen karaktär.

8277 **Amontillado Superior** 750 ml ***82:-** 375 ml ***46:-**
*(amåntilja'då soperiå'r)*
Mild, ren amontilladostil med fräschör. Ganska smakrik.

8215 **Bailen** Dry Oloroso 750 ml **94:-**
Osborne
Medelfyllig, balanserad smak av nötter med viss eldighet och liten sälta. Lång eftersmak.

8216 **Leyenda** Oloroso 750 ml **95:-**
M Gil Luque
Fyllig, eldig, komplex smak med inslag av choklad och nötter, lång eftersmak.

8201 **La Guita** Manzanilla 750 ml **99:-**
*(la gi'ta)*
Rainera Perez Marin
Lätt, frisk smak med nötig ton. Smakrik med lång eftersmak.

8207 **La Ina** 750 ml **101:-** 375 ml **51:-**
Domecq
Mild, mogen och balanserad finokaraktär.

8225 **Tio Pepe** 750 ml **107:-** 375 ml **55:-**
Gonzalez Byass
Smakrik, intensiv fino med lång eftersmak och viss elegans.

8218 **Palo Cortado** 750 ml **122:-**
Bodegas Medina E Hijos
Medelfyllig, torr, nötig och smakrik sherry med viss sälta och en rostad ton. Lång eftersmak.

8213 **Lustau Almacenista** 750 ml **182:-**
Oloroso
Emilio Lustau
Fyllig, eldig, mycket smakrik sherry med inslag av nötter och lång intensiv eftersmak.

8211 **Gonzalez Byass** 750 ml **594:-**
Finest Dry Oloroso
1966
Gonzalez Byass
Torr, eldig, mycket intensiv, syrlig smak med kraftig fatkaraktär och inslag av choklad och nötter.

## Halvtorr

8231 **Real Tesoro** 750 ml **73:-** 375 ml **39:-**
Marqués del Real Tesoro
Medelfyllig med kraftig, nötig smak och lite bränd ton. Olorosotyp.

8275 **Amontillado** 750 ml ***75:-** 375 ml ***41:-**
*(amåntilja'då)*
Medelfyllig med fin sherrykaraktär och nötig, balanserad smak.

8282 **Oloroso S.A.R** 750 ml ***76:-** 375 ml ***45:-**
*(ålårå'så)*
Ganska smakrik sherry med lätt, bränd ton och inslag av torkad frukt.

8226 **Bristol Medium Dry** 750 ml **81:-**
*(bri'stel mi'djem draj)*
Harvey & Sons
Smakrik med fin, balanserad nötkaraktär.

8221 **Osborne** Amontillado 750 ml **81:-**
Osborne
Något bränd, nötig smak med inslag av fat, russin och fikon. Lång eftersmak.

8276 **Leyenda** Amontillado 750 ml **95:-**
M Gil Luque
Medelfyllig smak med bränd ton och karaktär av fat och nötter.

8209 **Dry Sack** 750 ml **97:-** 375 ml **49:-**
*(draj säk)*
Williams & Humbert
Bra olorosotyp med nötkaraktär, viss friskhet och elegans.

## Halvsöt

8294 **Alhambra** 750 ml ***79:-**
Smakrik med nötig, balanserad olorosostil.

8223 **Nutty Solera** 750 ml **87:-** 375 ml **46:-**
*(na'ti såle'ra)*
Gonzalez Byass
Smakrik med fin nötarom och aning bränd. Olorosotyp.

## Söt

8232 **Real Tesoro** 750 ml **74:-**
Royal Cream
Marqués del Real Tesoro
Nötig sherrysmak med russinton och balanserad friskhet.

8214 **Burdon Rich Cream** 750 ml **75:-**
J.Burdon
Fyllig, frisk, eldig smak med inslag av russin och nötter. Smakrik med lång eftersmak.

8291 **Royal Cream** 750 ml ***75:-** 375 ml ***45:-**
*(rå'jal krim)*
Fyllig med fin fruktighet och god nötighet. Smakrik.

8208 **Pedro Ximenez** Rare 750 ml ***90:-**
Old Sweet PX
*(pe'drå schimä'näs)*
Williams & Humbert
Något bränd sherrysmak med inslag av russin och choklad. Smakrik med lång eftersmak.

8228 **Bristol Cream** 750 ml **92:-** 375 ml **48:-**
*(bri'stel krim)*
Harvey & Sons
Fyllig, lite simmig smak med ton av nötter och russin.

8212 **Vendimia** Cream 750 ml **134:-**
Sherry
Emilio Lustau
Fyllig, simmig, eldig, komplex smak med bränd ton och inslag av nötter, russin och nougat.

## Montilla

2789 **Montilla Dry** 750 ml ***61:-**
*(månti'lja draj)*
Spanien, Montilla-Moriles
Fyllig, eldig och smakrik med viss sherrykaraktär. Torr.

8465 **Gran Barquero** 700 ml **101:-**
Pedro Ximenez
*(gran barkä'rå)*
Spanien, Montilla-Moriles
Barquero
Simmigt, smakrikt, mycket sött vin med bränd ton och inslag av russin och torkad frukt. Lång smak.

Figure 1: Sample page from Systembolaget's 1996 menu.

Figure 2: Picture of a typical behind-the-counter Systembolaget store.


Figure 3: Picture of a typical self-service Systembolaget store.

### 2.2 Store Format and the Concentration of Sales

To estimate the impact of a store's format on the level and concentration of its sales, we use a straightforward difference-in-difference identification strategy. For store $s$, product category $c$, and month $t$, our estimating equation is:

$$Outcomes_{sct} = \beta TreatmentGroups_{sc} * AfterTreatments_{sct} + \mu_{sc} + \tau_t + \varepsilon_{sct}, \quad (1)$$

where outcomes are either a Herfindahl index or sales volume in this subsection, and the fraction of sales within a store-category-month that are difficult to pronounce in the next subsection. Given this specification, we control for store-category fixed effects in our main specification ($\mu_{sc}$), as well as month fixed effects ($\tau_t$); as such, all differences across stores at the category level and all systematic changes over time are controlled for in the regression. We also show results with store pair-category fixed effects to take advantage of the potential additional power from the pairing in the experimental design. The coefficient $\beta$ will therefore capture how sales in the treatment group of stores change

Table 2: Descriptive statistics for Systembolaget stores.

| | Mean | Std. Dev. | Min. | Max. | N |
|---|---|---|---|---|---|
| *Unit of Obs.: Store-Category-Month* | | | | | |
| Herfindahl | 0.0900 | 0.0778 | 0.0088 | 0.8059 | 10570 |
| Units Sold | 12439 | 15423 | 15 | 159917 | 10570 |
| Liters Sold | 6246 | 7092 | 3 | 63220 | 10570 |
| Swedish Products | 0.3819 | 0.3873 | 0 | 1 | 10570 |
| French Products | 0.0596 | 0.0739 | 0 | 0.4348 | 10570 |
| *Market Share Difficult-to-Pronounce* | | | | | |
| Guide (by Units) | 0.2162 | 0.2348 | 0 | 0.7737 | 10570 |
| Guide (by Volume) | 0.2347 | 0.2420 | 0 | 0.8193 | 10570 |
| Over 30 Characters (by Units) | 0.0099 | 0.0193 | 0 | 0.1255 | 10570 |
| Over 30 Characters (by Volume) | 0.0101 | 0.0194 | 0 | 0.1254 | 10570 |
| Coder Rates Below Top (by Units) | 0.4217 | 0.2872 | 0 | 1 | 10570 |
| Coder Rates Below top (by Volume) | 0.4626 | 0.3124 | 0 | 1 | 10570 |
| *Unit of Obs.: Product* | | | | | |
| Pronunciation Guide | 0.5428 | 0.4983 | 0 | 1 | 1658 |
| Word Length | 17.820 | 8.5537 | 3 | 70 | 1658 |
| Mean Coder Score | 8.3923 | 0.7953 | 5.33 | 9 | 1625 |
| Coder 1 Score | 8.1594 | 0.6612 | 6 | 9 | 1631 |
| Coder 2 Score | 8.7813 | 0.5341 | 4 | 9 | 1628 |
| Coder 3 Score | 7.9300 | 1.8721 | 1 | 9 | 1628 |
| Vodka | 0.0730 | 0.2602 | 0 | 1 | 1658 |
| Other Spirits | 0.2467 | 0.4312 | 0 | 1 | 1658 |
| Wine | 0.4608 | 0.4986 | 0 | 1 | 1658 |
| Fortified Wine | 0.0766 | 0.2660 | 0 | 1 | 1658 |
| Swedish Beer | 0.0844 | 0.2781 | 0 | 1 | 1658 |
| Imported Beer | 0.0308 | 0.1727 | 0 | 1 | 1658 |
| Non-Alcoholic Drinks | 0.0277 | 0.1642 | 0 | 1 | 1658 |
| *Unit of Obs.: Store-Product-Month* | | | | | |
| Units Sold | 129.35 | 485.17 | $-203^a$ | 29836 | 1016428 |
| Behind-the-Counter Format | 0.2219 | 0.4156 | 0 | 1 | 1016428 |
| Price (Krona) | 90.011 | 80.467 | 3 | 2325 | 1016428 |

Only includes products in the 1991 guide (and therefore coded for pronunciation difficulty).
[a] Sales can be negative if returns for a product at a store in a month exceed sales. Negative sales represent less than one tenth of one percent of the observations. These observations will be dropped from most of the analysis because we use a measure of logged sales.

after they convert to self-service compared to the control group of behind-the-counter stores over the same period.

Because our data come from a randomized field experiment, we have fewer concerns about endogeneity and omitted variables that typically arise in difference-in-differences studies — the differences between the treatment and control groups should be random.

Table 3: Summary statistics for Systembolaget treatment and control stores.

| Town | Treatment or Control | Mean Before | Std. Dev. Before | Mean After | Std. Dev. After |
|---|---|---|---|---|---|
| Herfindahl | Treatment | 0.0884 | 0.0712 | 0.0621 | 0.0558 |
| | Control | 0.0816 | 0.0687 | 0.0712 | 0.0668 |
| Units sold | Treatment | 15327 | 18833 | 16443 | 19236 |
| | Control | 14492 | 18263 | 13042 | 16651 |
| Liters sold | Treatment | 7726 | 8440 | 8222 | 9148 |
| | Control | 7314 | 8485 | 6679 | 8382 |
| Revenue in million Krona | Treatment | 62.2 | 58.9 | 69.3 | 60.2 |
| | Control | 57.5 | 55.8 | 56.6 | 55.6 |
| Fraction hard to pronounce | Treatment | 0.2021 | 0.2316 | 0.2157 | 0.2297 |
| | Control | 0.2260 | 0.2412 | 0.2185 | 0.2347 |

First six rows includes all products.

Final two rows only include products in the 1991 guide (and therefore coded for pronunciation difficulty).

Nevertheless, we also verify that the change in sales is coincident with the format change. Because we observe each store multiple times and because the matched treatment-control pairs of stores might have correlated sales in each category, we cluster the standard errors by store-pair-category to reduce the potential for overstating statistical significance (Bertrand et al. 2004).[8]

Table 4 shows the results of regressing the format change on the concentration of sales and on sales in units. The dependent variable is the concentration of sales (measured by the Herfindahl) in the odd numbered columns and sales in units in the even numbered columns. Across a variety of specifications, the results show that the Herfindahl falls substantially after a store changes to self-service: the estimated marginal effect in column (1) is 0.0154 relative to an average of 0.0900. The results also show that sales increase by approximately 20%, a magnitude similar to that found in Skog (2000).

Our main specification focuses on the sample of products appearing in the 1991 guide because it has the pronunciation key and is therefore usable in the next subsection. This specification, described in Equation (1), is shown in columns (1) and (2). One potential concern with this specification is that it does not directly take into account the pairing of stores in the experimental design. This may have two consequences. First, if the pairing was done poorly, it might yield concerns about the proper specification of the functional form of the time series. Second, it might be possible to exploit the matched pairs to

[8] Results are robust to clustering by store. We cluster by store-pair-category because of the potential for correlated sales of similar products across the similar treatment and control stores.

increase power (Imai et al. 2009, Imbens 2011). Fryer (2013) addresses these concerns by using flexible specifications for the functional form of the time series and by aggregating the fixed effects to the pair level. Columns (3) and (4) add quartic polynomial time trends for each of the 14 stores. Columns (5) and (6) include the quartic time trends and use store-pair-category fixed effects rather than store-category fixed effects. The qualitative results do not change. Columns (7) and (8) show robustness of the main specification to using the full sample of products across all guides.

Figure 4 repeats the analysis in Column (1) at a finer level of temporal detail. Rather than one discrete variable identifying when a store changes format, we substitute the *Self-Serve Stores After Change* variable with a sequence of dummy variables for the quarters before and after the format change. We find that, prior to the format change, stores in the treatment group (i.e., those that change format) exhibit no trend towards a decreased sales concentration; the timing of the change in the estimated coefficient is coincident with the timing of the format change. The coefficients for the before-change period are jointly statistically different from the coefficients of the after-change period.

## 2.3 Store Format and Difficult-to-Pronounce Products

To assess how the format change affects the sales of difficult-to-pronounce products, we reestimate Equation (1) using the fraction of products sold in each store-category-month that are difficult to pronounce as the dependent variable, while adding controls for the Herfindahl index and the log of total quantity sold for that store-category-month. We use three different measures for difficult-to-pronounce products: (i) whether the menu provided by Systembolaget includes a phonetic pronunciation guide for the product, (ii) whether the product's name has over 30 characters, and (iii) whether any of the coders rated the product less than a "9" for ease-of-pronunciation.[9]

[9] Qualitative results are robust to various perturbations of the definitions of difficult-to-pronounce product, particularly using the hand-coded pronunciation measure. We show three representative examples here and, as discussed earlier, prefer using the pronunciation guide because the threshold is determined by a third party, independent of our study.

Table 4: Treated stores sell more volume and more variety after the change.

| | Only Products in 1991 Guide | | | | | | All Products | |
|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| | Herfindahl | Log Sales in Units | Herfindahl | Log Sales in Units | Herfindahl | Log Sales in Units | Herfindahl | Log Sales in Units |
| Self-Serve Stores After Change | -0.0154*** | 0.1964*** | -0.0181*** | 0.2214*** | -0.0181*** | 0.2244*** | -0.0158*** | 0.2283*** |
| | (0.0041) | (0.0246) | (0.0045) | (0.0371) | (0.0046) | (0.0366) | (0.0037) | (0.0279) |
| N | 10570 | 10570 | 10570 | 10570 | 10570 | 10570 | 10570 | 10570 |
| Number of FEs | 98 | 98 | 98 | 98 | 49 | 49 | 98 | 98 |
| Polynomial time trend | No | No | Yes | Yes | Yes | Yes | No | No |
| Fixed effect type | Store-category | Store-category | Store-category | Store-category | Store pair-category | Store pair-category | Store-category | Store-category |
| $R^2$ | 0.09 | 0.44 | 0.09 | 0.46 | 0.10 | 0.49 | 0.22 | 0.39 |

Regressions include fixed effects as specified (differenced out) and 107 monthly fixed effects.

Unit of observation is the store-category-month.

Polynomial time trend allows separate quartic polynomial time trends for each of the 14 stores.

Robust standard errors clustered by store-pair-category in parentheses.

* significant at 10%; ** significant at 5%; *** significant at 1%.

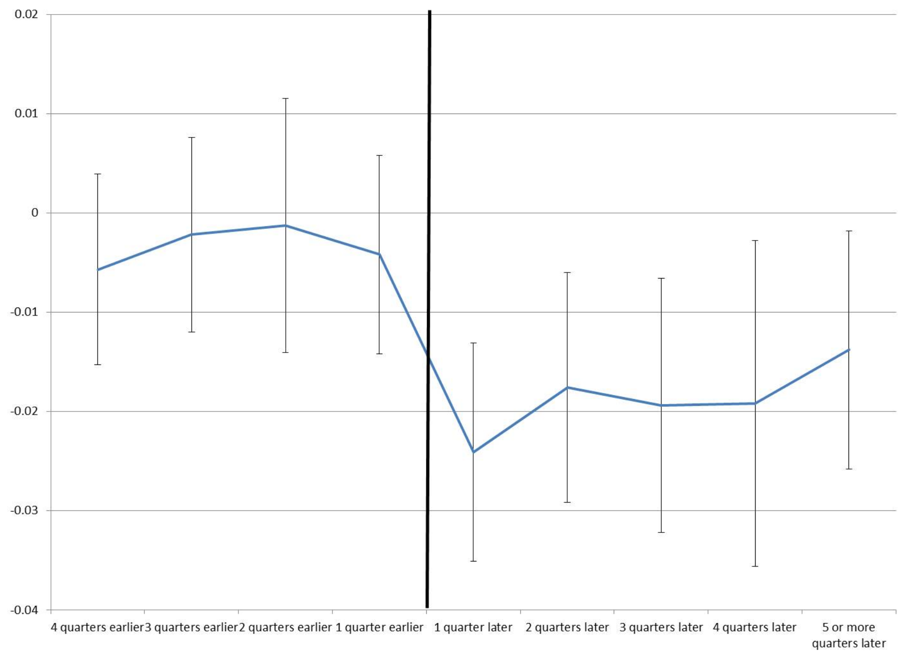



Figure 4: Coefficients of regression of Herfindahl on being in the treatment group over time Specification resembles Column (1) of Table 4.

Table 5 presents the results from nine specifications that regress difficult-to-pronounce product sales on an indicator variable equal to one after a store converts to a self-service format, among other controls. In each specification, there is a positive and statistically significant relationship between the fraction of sales from difficult-to-pronounce products and self-service stores.

As a baseline, Column (1) regresses the fraction of difficult-to-pronounce product sales on the treatment dummy. Column (2) adds controls for the Herfindahl index and an interaction between the Herfindahl and the store format change. The coefficient of 0.0169 is relative to an overall propensity of difficult-to-pronounce products at treatment stores in the pre-treatment period of 20%, suggesting an 8% increase relative to baseline. Column (3) adds controls for the percentage of sales that are of domestic (Swedish) products, as labeled in the menu, and an interaction between fraction domestic products and the format change. Column (4) adds unreported controls for the Herfindahl in second, third, and fourth degree (i.e., a quartic polynomial), as well as their interactions with the store format change. In each case, the results remain robust. To deal with concerns regarding the proper matching of stores in the experiment, Columns (5)–(8) add separate quartic polynomial time trends for each of the 14 stores. Columns (6) and (8) also use pair-category fixed effects rather than store-category fixed effects. Finally, column (9) uses 5292 separate fixed effects (differenced out) for each pair-month. Thus, it allows a nearly-perfectly flexible time trend for each pair. While this soaks up much of the variation in the data (the differenced out fixed effects are not included in the $R^2$), there is still a positive and significant relationship between fraction difficult-to-pronounce and self-serve stores.

Table 5: Difficult-to-pronounce products have a disproportionately large sales increase

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
|---|---|---|---|---|---|---|---|---|---|
| Self-Serve Stores After Change | 0.0220*** | 0.0169** | 0.0293*** | 0.0181*** | 0.0251** | 0.0231** | 0.0443*** | 0.0385*** | 0.0078* |
| | (0.0065) | (0.0066) | (0.0061) | (0.0063) | (0.0095) | (0.0091) | (0.0108) | (0.0101) | (0.0043) |
| Herfindahl | | -0.7423*** | -0.7958*** | -3.9622*** | -0.7046*** | -0.6636*** | -3.2792*** | -3.3469*** | -3.2832** |
| | | (0.0972) | (0.0979) | (0.6965) | (0.0913) | (0.0933) | (0.6771) | (0.6615) | (1.2961) |
| Herfindahl x After Change | | -0.2213** | 0.0504 | 1.9648*** | -0.2815*** | -0.2939*** | 1.1787** | 1.4256** | 1.7708 |
| | | (0.1025) | (0.0967) | (0.5398) | (0.0991) | (0.1005) | (0.5467) | (0.5604) | (1.3468) |
| Fraction Domestic | | | 0.1219*** | 0.1078** | | | 0.1140** | 0.1305** | 0.1361** |
| | | | (0.0451) | (0.0503) | | | (0.0481) | (0.0503) | (0.0563) |
| Fraction Domestic x After Change | | | -0.2775*** | -0.2866*** | | | -0.2992*** | -0.3038*** | -0.1618*** |
| | | | (0.0347) | (0.0367) | | | (0.0351) | (0.0363) | (0.0594) |
| Polynomial time trend | No | No | No | No | Yes | Yes | Yes | Yes | No |
| Herfindahl polynomial | No | No | No | Yes | No | No | Yes | Yes | Yes |
| Fixed effect type | Store-category | Store-category | Store-category | Store-category | Store-category | Store pair-category | Store-category | Store-pair category | Store-pair-category-month |
| N | 10570 | 10570 | 10570 | 10570 | 10570 | 10570 | 10570 | 10570 | 10570 |
| Number of FEs | 98 | 98 | 98 | 98 | 98 | 49 | 98 | 49 | 5292 |
| $R^2$ | 0.07 | 0.35 | 0.42 | 0.46 | 0.37 | 0.35 | 0.48 | 0.46 | 0.12 |

Unit of observation is the store-category-month.
Dependent variable is percent sales that are difficult to pronounce, measured by guidance on the menu.
Percent sales defined by units sold except in column (4).
Regressions include fixed effects as specified (differenced out) and 107 monthly fixed effects.
Polynomial time trend allows separate quartic polynomial time trends for each of the 14 stores.
Herfindahl polynomial is quartic. Regression coefficients not shown to save space.
Uses all products observed in the 1991 data. Robust standard errors clustered by category-store pair in parentheses.
* significant at 10%; ** significant at 5%; *** significant at 1%.

Table 6: Further exploration of results on difficult-to-pronounce products

| Dependent variable | (1) Percent sales Hard-to-pronounce | (2) Percent sales Hard-to-pronounce | (3) Percent sales Hard-to-pronounce | (4) Percent sales Hard-to-pronounce | (5) Percent sales Hard-to-pronounce | (6) Percent sales Hard-to-pronounce | (7) Percent sales Hard-to-pronounce | (8) Log Sales | (9) Log Sales |
|---|---|---|---|---|---|---|---|---|---|
| Definition of Hard-to-Pronounce | Any Coders Below Top | Word Length Over 30 | Pronunciation Guide | Pronunciation Guide | Pronunciation Guide | Pronunciation Guide | Pronunciation Guide | Pronunciation Guide | Pronunciation Guide |
| Sample | All products | All Products | Non-French Products | Products with Short Names | French Products w/ Short Names | Top Quartile Products (1984-87) | Not Top Quartile Products (1984-87) | Only Hard-to-Pronounce | Only Not Hard-to-Pronounce |
| Self-Serve Stores After Change | 0.0208** | 0.0013* | 0.0201*** | 0.0436*** | 0.0065** | -0.0070 | 0.0255*** | 0.3561*** | 0.1768*** |
| | (0.0101) | (0.0008) | (0.0064) | (0.0102) | (0.0032) | (0.0053) | (0.0084) | (0.1214) | (0.0337) |
| Herfindahl | -1.0243*** | -0.0036 | -0.6096*** | 0.0077 | -0.0874** | -0.3574*** | -0.2675*** | -3.7699 | 3.2582*** |
| | (0.1894) | (0.0027) | (0.0851) | (0.1362) | (0.0397) | (0.0759) | (0.0668) | (2.5821) | (0.5218) |
| Herfindahl x After Change | -0.5411*** | 0.0054 | -0.2333*** | -0.7889*** | -0.0040 | 0.1770*** | -0.2255** | 3.7439 | 1.2819*** |
| | (0.1668) | (0.0041) | (0.0869) | (0.1548) | (0.0051) | (0.0602) | (0.0877) | (2.5111) | (0.3940) |
| Avg Dep. Var. pre-treatment | 0.4350 | 0.0101 | 0.2072 | 0.1966 | 0.5238 | 0.1570 | 0.3253 | 4.8355 | 8.2889 |
| N | 10570 | 10570 | 10570 | 10570 | 7549 | 9052 | 10439 | 10570 | 10570 |
| Number of FEs | 98 | 98 | 98 | 98 | 84 | 84 | 98 | 98 | 98 |
| $R^2$ | 0.44 | 0.12 | 0.33 | 0.26 | 0.13 | 0.26 | 0.22 | 0.09 | 0.56 |

Unit of observation is the store-category-month.

Dependent variable is percent sales that are difficult to pronounce. Unless otherwise specified, difficult to pronounce defined by pronunciation key on the menu. Percentage defined by units sold except in column (4).

Regressions include fixed effects by store-category (differenced out) and 107 monthly fixed effects.

Unless otherwise specified, regressions use all products observed in the 1991 data. Robust standard errors clustered by category-store pair in parentheses.

* significant at 10%; ** significant at 5%; *** significant at 1%.

## 2.4 Alternative Explanations Unrelated to Social Interaction

The results presented above could be explained by factors other than social transaction costs. For example, the assignment of stores in the experiment may not have been independent of an increasing sales trend for difficult-to-pronounce products, which would then bias our results. To address this concern, we verify that the sales of difficult-to-pronounce products did not rise in the treatment stores relative to the control stores prior to the format change. In particular, Figure 5 shows the estimated coefficient of a regression of the fraction of sales that are difficult to pronounce on being in the treatment group, quarter by quarter. The results show a sharp increase in the share of difficult-to-pronounce products after the format change. The coefficients for the before-change period are jointly statistically different from the coefficients of the after-change period.

More broadly, our interpretation of the results from Table 5 — that the format change that reduced social interaction had a causal impact on sales of difficult-to-pronounce products — is potentially just one of several competing explanations. Next, we address several of these alternatives, often referring to the specifications shown in Table 6.

To address the concern that the pronunciation guide should make phonetic reading easy — and thus render the presence of such guides a poor proxy for difficult-to-pronounce products — columns (1) and (2) show robustness to alternative definitions of difficult-to-pronounce names. Because these definitions are only weakly correlated with the presence of a pronunciation guide, we do not consider this a mechanical result.

In addition, consumers may be unfamiliar with foreign products, and therefore a lack of familiarity and difficulty in remembering product names, rather than any difficulty with pronouncing their names, causes the sales of difficult-to-pronounce products to increase as consumers become aware of obscure products while browsing the store's shelves. Another way to interpret this concern is to assert that search costs fall disproportionately more for hard-to-pronounce products when the stores move to a self-service

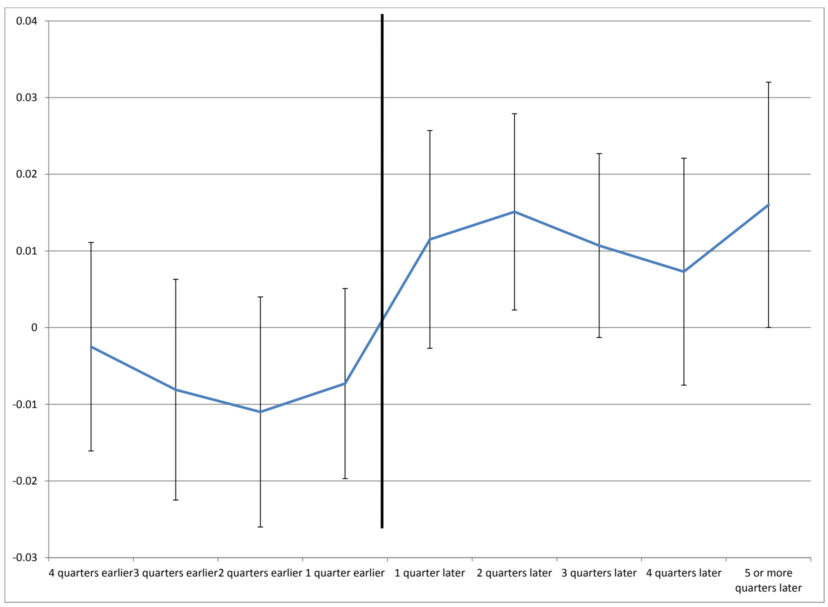



Figure 5: Coefficient of regression of fraction difficult-to-pronounce on being in treatment group over time. Specification resembles Column (1) of Table 5.

format. Our flexible controls for the Herfindahl index and the fraction of sales from domestic products partly address this concern. In addition, column (3) shows that the results are not driven by a particular set of potentially unfamiliar (and disproportionately hard to pronounce) foreign products, French products. The results change little when French products are dropped. Columns (4) and (5) addresses a similar concern related to difficulty in remembering names. While we cannot definitively rule out this possibility in the absence of an explicit memory test, our results are nevertheless robust to considering only products with shorter names, which may be easier to recall from memory (Baddeley et al. 1975). In particular, Column (4) shows robustness to restricting the sample to products with 20 or fewer characters and column (5) shows robustness to restricting the sample to French products with 20 or fewer characters.[10]

Columns (6) and (7) provide a specification check on the intuition that difficulty-of-pronunciation is unlikely to be a barrier to ordering familiar products as consumers may already have learned how to pronounce them. Column (6) shows that, among relatively popular products (as defined in the four years prior to our sample) that the menu labels as difficult-to-pronounce, the percent of sales from difficult to pronounce products is unrelated to the retail format. In contrast, Column (7) shows that for relatively unpopular products, sales are substantially lower in the behind-the-counter format.[11]

We view the above results as substantially alleviating concerns that search costs fall disproportionately for hard-to-pronounce products. Given the various ways to control for familiarity and sales, our identifying assumption is violated if hard-to-pronounce products are less familiar than other products *with similar levels of sales and from similar countries.*

[10] Another useful specification would be to condition on Swedish products only. Unfortunately, there are not enough hard-to-pronounce Swedish products to run this analysis.

[11] We thank a referee for bringing up another interesting question: whether the increase in the sales of hard-to-pronounce products yields an increase in overall sales or merely generates substitution away from other products. Columns (8) and (9) use logged sales as the dependent variable in order to examine this question, but the answer in inconclusive. Because sales of both hard-to-pronounce and non-hard-to-pronounce products rise with the format change, it is not clear whether hard-to-pronounce products take sales from the other products or whether they increase the overall sales.

Another possible explanation is that consumers do not order difficult-to-pronounce products verbally because they do not want to be misunderstood by the sales clerk. While we cannot definitively reject this possibility, we still interpret it as a type of social transaction cost. In other words, it is still the social nature of the interaction that influences behavior, whether out of frustration, impatience, or embarrassment.

It is also possible that treatment stores made hard-to-pronounce products more readily available in anticipation of an increase in sales when the formats changed. We do not think this is likely to negate our interpretation for two reasons. First and most importantly, as we understand it, the treatment and control stores were instructed not to change the selection of available products substantially in order to make the experiment clean. Second and perhaps less compelling, if treatment stores stocked hard-to-pronounce products because they anticipated an increase in sales, the nature of the experiment changes but the interpretation does not. In particular, the experimental unit is the store manager and the underlying assumption is that the manager understands the buying behavior of the customers.

Out-of-stock items may also pose a challenge to identification. For example, out-of-stocks may lead us to underestimate the impact of the format change if managers did not anticipate the higher sales of difficult-to-pronounce products, leading hard-to-pronounce products being disproportionately out-of-stock in the self service format. In contrast, out-of-stocks may also lead us to overestimate the impact of the format change if clerks disproportionately recommend easy-to-pronounce products for reasons unrelated to the social interaction.[12]

Finally, we may overstate the magnitude of the effect if consumers who plan to buy difficult-to-pronounce items choose to go to the self-service stores specifically to avoid ordering from a sales clerk. We believe this is an unlikely explanation because Systembolaget is a monopoly retailer that deliberately selected geographically isolated stores for inclusion in the experiment to prevent this type of behavior.

[12]We thank a referee for pointing out the latter issue.

Overall, we interpret the results presented in this section as evidence that personal interactions have a meaningful impact on the sales of particular types of products: consumers are less likely to buy a product when they want to avoid a difficult pronunciation (or at least the need to point to it on a menu). We argue that this social transaction cost is likely related to the potential for embarrassment, but we cannot rule out the possibility that it is explained by a consumer's desire to avoid misunderstandings and the frustration that comes with them. Furthermore, the store-level data make it difficult to estimate the effect of these social frictions on welfare given consumers' heterogeneous tastes. As such, we turn next to an alternative setting where we document a similar result, and also calculate its impact on welfare.

# 3 Online Ordering at a Pizza Delivery Restaurant

## 3.1 Data and Setting

To continue examining how social interaction affects consumers, this section uses data from a franchised pizza delivery restaurant operating in a mid-size metropolitan area.[13] The franchise is similar to prominent chains such as Domino's and Papa John's, but has a narrower regional presence. The store's menu is standard, offering pizza with traditional toppings, breadsticks, baked subs, wings, and salads. The store also sells beverages, but its distribution agreement prohibits the sharing of any beverage sales data and we therefore exclude beverages from our analysis.

The store's customers can place their orders over the phone, at the counter, or, since January 2009, through the franchise's website, shown in an anonymous format in Figure 6. By our own (admittedly casual) comparison of the store's website to larger national chains', it is less sophisticated and offers only basic functionality; it has no search capabilities, no consumer ratings, no recommendations, no online-specific promotions,

[13]Due to a confidentiality agreement required to access the data, many specific details related to both the franchise and store are omitted.

and no saved order list. The store's rudimentary website is a virtue for identification because it closely resembles the layout of physical menus distributed to customers by the store – including an exhortation to create one's own pizza – suggesting that consumers are unlikely to alter their behavior based on any particular feature of the website.

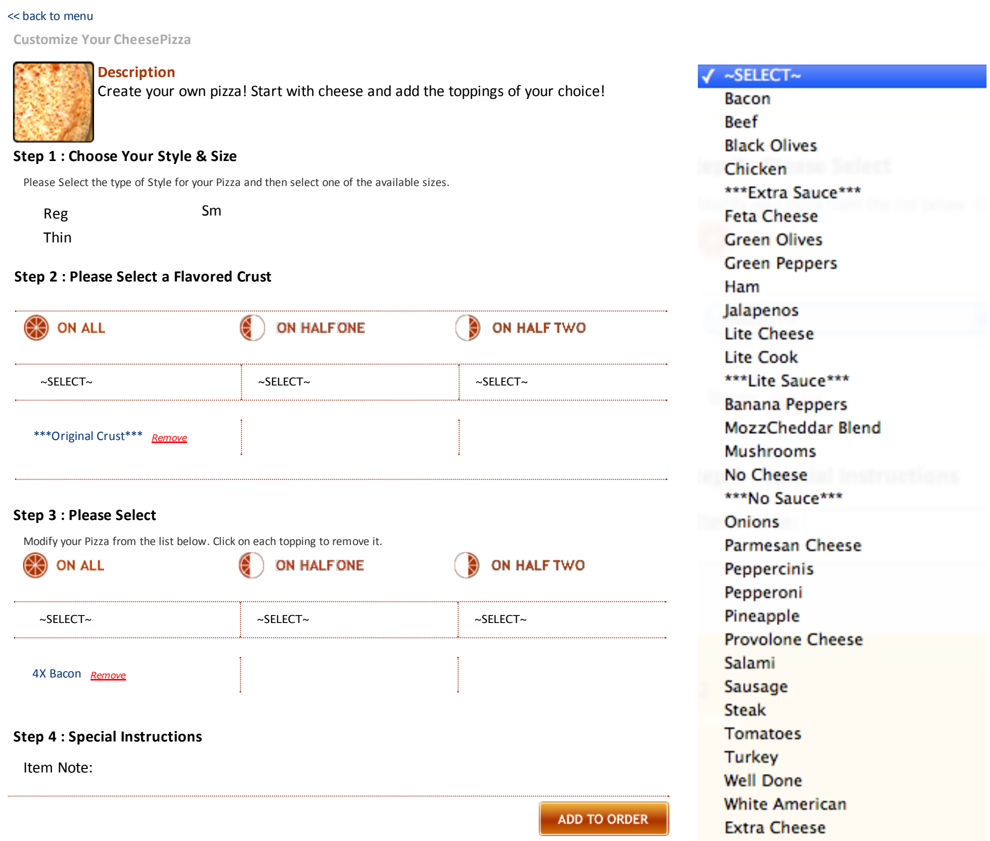



Figure 6: Screenshot of the store's website (stripped of identifying content), and the drop-down menu for toppings.

For phone and counter orders, an employee enters instructions through a touchscreen point-of-sales terminal which are then transmitted to a display in the food preparation area. For website orders, a customer clicks on a link for a particular base item and then configures it through a series of drop-down menus; the order then goes directly to the food preparation display. For all channels, customers may either pick up their orders at the store, or have them delivered for a fee plus an optional gratuity.

The dataset used for our analysis includes all food items from orders made between July 2007 and December 2011.[14] The store anonymized the data before releasing it and assigned a unique identifier to all households through a third-party proprietary system. Because the store's identifier is at the household level, we use the terms household and customer interchangeably. Figure 7 provides a sample order made by a customer containing two base items placed over the phone for delivery.

The measure of complexity in this paper refers to the number of instructions a customer provides for each base item in his order. For example, we define a large pizza as having a complexity equal to 1, a large pepperoni pizza as equal to 2, a large pizza with half pepperoni and half sausage as equal to 3, and so on. Thus, the minimum complexity for any base item is 1, while the maximum in the data is 21. This store, like most pizza franchises, also offers "specialty" pizzas that have preconfigured toppings, such as a "veggie" pizza with seven toppings. We code specialty pizzas to have a complexity equal to 1 unless the customer provides instructions to add or remove toppings. Under this definition, the order in Figure 7 has a maximum base item complexity of 6 and a mean base item complexity of 4.[15] The mean complexity comes from having two base items and a total of 8 instructions, which includes the base of 1 for each item.

The store also provided information for the number of calories in each item. As a benchmark, a large cheese pizza has 2080 calories, whereas a small garden salad with no dressing has 40 calories. In the data, the mean and maximum number of calories for the base items within an order are constructed in an equivalent manner to the measures for complexity. Using the example in Figure 7, the mean base item has 2521 calories and the maximum base item has 2779.

The dataset comprises 160,168 orders made by 56,283 unique customers, with summary statistics reported in Table 7. Of the store's orders, 6.7% have been placed online

[14] To preserve the confidentiality of sensitive competitive information, the store did not release data for orders over $50 (typically large institutional orders) or for promotional orders under $3.49, the price of the least-expensive food item.

[15] Each base item counts as one, each special crust counts as one, and each topping counts as one.

Table 7: Descriptive statistics for pizza data.

| | Full Sample | | | | Web Comparison | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|
| **Variable** | **Mean** | **Std. Dev.** | **Min.** | **Max.** | **Web Mean** | **In-Store Mean** | **t-stat** | **Web Mean** | **Phone Mean** | **t-stat** |
| Web Order | 0.067 | 0.25 | 0 | 1 | 1 | 0 | | 1 | 0 | |
| In-Store Order | 0.084 | 0.278 | 0 | 1 | 0 | 1 | | 0 | 0 | |
| Phone Order | 0.849 | 0.358 | 0 | 1 | 0 | 0 | | 0 | 1 | |
| Order Price | 14.702 | 6.829 | 3.49 | 49.99 | 15.46 | 11.80 | 38.31 | 15.46 | 15.11 | 4.84 |
| Items in Order | 2.036 | 1.156 | 1 | 17 | 1.99 | 1.59 | 26.41 | 1.99 | 2.06 | 6.22 |
| Complexity – Mean Order Item | 2.646 | 1.217 | 1 | 21 | 3.06 | 2.51 | 26.84 | 3.06 | 2.67 | 30.71 |
| Complexity – Max Order Item | 3.273 | 1.399 | 1 | 21 | 3.81 | 2.87 | 40.32 | 3.81 | 3.29 | 36.6 |
| Calories – Mean Order Item | 1694.613 | 607.077 | 110 | 6010.84 | 1798.84 | 1512.11 | 30.52 | 1798.84 | 1711.27 | 14.21 |
| Calories – Max Order Item | 2022.724 | 625.991 | 110 | 6010.84 | 2154.81 | 1699.34 | 45.51 | 2154.81 | 2035.65 | 19.15 |
| N | | 160168 | | | 10693 | 8244 | | 10693 | 96558 | |

Summary statistics from the full dataset of orders, excluding beverages, appear on the left-hand side and from orders made in the post-Web period on the right-hand side. The unit of observation is an individual order. The variable “Web Order” is an indicator variable equal to one if the order was made through the website. The variable “In-Store Order” is an indicator variable equal to one if the order was made at the store. The variable “Phone Order” is an indicator variable equal to one if the order was made over the phone. The variable “Order Price” is the total price of the food items within an order before tax, delivery, and gratuity. The variable “Items in Order” is the total number of base items (pizzas, breadsticks, baked subs, wings, and salads) within an order. The variable “Complexity – Mean Order Item” is the average number of instructions provided per item within an order, with a base complexity of 1. The variable “Complexity – Max Order Item” is the maximum number of instructions provided for the items within an order, with a base complexity of 1.

| | | | | | |
|---|---|---|---|---|---|
| **Date:** | 03/12/2010 | **Taken By:** | David Robison | **Customer:** | |
| **Order Number:** | 50 | **Table:** | | | |
| **Order Type:** | Delivery | | | | |
| **Order Time:** | 05:17 PM | | | | |

| | | |
|---|---|---|
| 1 | Lg Create Your Own Pizza | 9.99 |
| | ***Butter Chz Crust*** | |
| 1 | Lg Create Your Own Pizza | 9.99 |
| | Pepperoni | 1.49 |
| | Sausage | 1.49 |
| | Green Peppers | 1.49 |
| | Mushrooms | 1.49 |
| | ***Butter Chz Crust*** | |
| | **Subtotal** | **25.94** |
| | **Delivery Fee** | **2.00** |

Figure 7: Sample order from the store's sales terminal. Rows with a "1" in the leftmost column contain base items. The rows below a base item represent instructions to alter the base item above them (e.g., add a topping).

and notable differences exist between these and non-Web orders. Comparing orders in the post-Web period, customers using the Web spend \$0.35 more than those ordering over the phone, on average, though they order slightly fewer base items; this disparity stems from online customers ordering more toppings. The price difference is even more severe for those who order in person at the store, as they spend \$3.66 less, mainly because they order 0.4 (roughly 20%) fewer items — for this reason, we, and the store's managers, consider in-store orders to be fundamentally different types of transactions, and our regressions below will compare only phone and Web orders. In addition, in-store orders did not track the household and hence they cannot be used with household fixed effects, our preferred specification. The mean base item is 14.6% more complex and has 5.1% more calories in an online order compared to a phone order, while the maximum base item is 15.8% more complex and has 5.9% more calories. Compared to in-store orders, the differences on these dimensions are even more pronounced.

The average customer has made 2.8 orders since the store's opening, with a range from 1 to 88. Of all customers, 4,582 (8.1% of total) purchased from the store both before and after online ordering became available. Among this group, 700 (1.2% of total) made an order both during the pre-Web time period and through the website after the introduction of online ordering. These customers will be crucial for identifying the causal effects of Web use, as observing orders across both regimes makes it possible to difference out unobserved heterogeneity that might drive selection into the online channel.

The store frequently offers promotions, with the average customer using a coupon in 54.3% of his orders. All promotions are available across all channels, and Web customers are slightly less likely to use a promotion. Because physical coupons come affixed to menus, any customer using a promotion can easily access the full list of the store's products, an institutional detail exploited as a robustness check below.

## 3.2 Online Orders and the Concentration of Sales

The store's online orders exhibit a significantly less concentrated sales distribution even though product selection, prices, and search capabilities remain fixed across channels. To establish the significance of this result, we compare the sales distribution of the store's 69 items (i.e., the five base items, specialty pizzas, and toppings) across the Web and non-Web channels. Throughout, we consider distributions that do and do not distinguish items by size (e.g., whether a large pizza is considered distinct from a medium pizza). We drop any item purchased fewer than 500 times, a conservative restriction given the more dispersed nature of online sales.

As in our analysis of the alcohol setting, we use a Herfindahl index to provide a concise measure of the sales concentration: it is 0.0429 for the pre-Web period, 0.0403 for non-Web sales in the post-Web period, and 0.0308 for Web sales. Using the percentage of total sales generated by the bottom 80% of products as an alternative measure of concentration, the share for pre-Web orders is 32.2%; the share for non-Web orders in the post-Web period is 32.3%; and the share for Web orders is 38.7%. Thus, the share of the bottom 80% of products is 6.4 percentage points greater for Web orders compared to non-Web orders during the same time period, which resembles the 4 percentage point difference documented by Brynjolfsson et al. (2011) for online and catalog clothing sales.

Finally, the top ten products comprise 52.6% of sales pre-Web, 52.1% of non-Web sales in the post-Web period, and 45.4% of online sales.

To establish that the difference in sales concentrations across channels is statistically significant, we consider a regression similar to Equation (1) where the dependent variable is a Herfindahl index for the sales channel in a given month and "Web Orders" is an indicator variable equal to one for online sales. Table 8 presents the results from these regressions, and all specifications show that online sales are significantly less concentrated. For Columns (1) and (2), the sales distribution is approximately 26% less concentrated online, treating different sizes of the same item as distinct; adding a time trend does not affect the main parameters. For Column (3), the sales distribution is approximately 33% less concentrated online, treating different sizes of the same item as equivalent; adding a time trend in Column (4) moves the decline to 36%. Across all specifications, restricting the sample only to months in the post-Web period does not affect the qualitative results.

Table 8: Online orders have a less concentrated sales distribution.

| | Items Distinguished by Size | | Items Not Distinguished by Size | |
|---|---|---|---|---|
| | (1) Herfindahl | (2) Herfindahl | (3) Herfindahl | (4) Herfindahl |
| Web Orders | -0.0107*** | -0.0107*** | -0.0279*** | -0.0292*** |
| | (0.0006) | (0.0006) | (0.0008) | (0.0008) |
| Constant | 0.0414*** | 0.0412*** | 0.0836*** | 0.0801*** |
| | (0.0004) | (0.0009) | (0.0005) | (0.0011) |
| Month Trend | No | Yes | No | Yes |
| N | 92 | 92 | 92 | 92 |
| Number of months | 56 | 56 | 56 | 56 |
| $R^2$ | 0.7608 | 0.7611 | 0.9317 | 0.9458 |

Unit of observation is the channel-month.
Robust standard errors clustered by month in parentheses.
* significant at 10%; ** significant at 5%; *** significant at 1%.

Consistent with the results found for alcohol sales in the previous section, these regressions establish that the store's online orders have a significantly less concentrated sales distribution. While other online markets also exhibit this pattern, the underlying cause of the shift is unlikely to be the same here as in previous studies — the selection of available products remains constant and search capabilities change little. We next consider how social interaction might affect the types of products sold.

### 3.3 Online Orders and Items Affected by Social Interaction

As we did for alcohol sales in Section 2, we now consider whether the impersonal nature of online transactions changes the types of products ordered. Specifically, we expect that consumers who place orders through the store's website are more likely to make choices that might otherwise be inhibited by social frictions. Following an extensive literature in social psychology that has shown individuals alter their behavior when others observe them eating excessively or unconventionally, we examine two order attributes that consumers may wish to keep private: calories and complexity.

First, several studies have linked the presence of others to lower calorie consumption. For example, Polivy et al. (1986) show from an experiment that subjects eat less when they believe others will be aware of their consumption. At the extreme, studies of bulimia also find that binge eating occurs less often in the presence of others (Waters et al. 2001, Herman & Polivy 1996). While these studies considered the negative implications of others' witnessing one's high calorie consumption, including potential embarrassment, other scholars have considered the positive implications of others' witnessing one's low calorie consumption. For example, Ariely & Levav (2000) show that the desire to impress a clerk by ordering low-calorie items changes restaurant ordering behavior.

Second, making a complex order could potentially be seen as being difficult — a situation people like to avoid. Perhaps the best demonstration of this idea in the popular culture is in the movie "When Harry met Sally" when Harry describes Sally as high maintenance for making a complex order at a restaurant and she views that as an insult.[16] Theories of impression management (Goffman 1959, Banaji & Prentice 1994) suggest that complexity may cause embarrassment or frustration if customers fear ap-

[16]See http://www.youtube.com/watch?v=czZo1GdfCRk

pearing difficult or unconventional. For example, in their study "Who is Embarrassed by What," Sabini et al. (2000) use a customer returning to a store several times as one of several embarrassing situations they study. Belk (1980) shows that unconventional consumption choices yield an unfavorable impression. Olsson et al. (2009) discuss how special requests can be embarrassing. These issues are also manifest in situations like medical treatment where the potential cost of not making complex requests is higher. Even among patients with above average education and knowledge, the fear of being seen as difficult or demanding can prevent them from discussing their care with their doctors (Aldred et al. 2005, Boyd et al. 2004, Frosch et al. 2012). In keeping with these ideas, moving orders online and thus removing a layer of social interaction may lead consumers to purchase a different mix of items.

To test this theory, we consider a sequence of regressions that take the form

$$Y_{ij} = \alpha + \beta X_{ij} + \gamma Web_{ij} + \delta_i + \varepsilon_{ij}, \tag{2}$$

with $Y_{ij} \in \{\text{complexity, calories}\}$ for order $j$ by customer $i$; $X_{ij}$ includes order-specific characteristics such as the day of the week, the time of day, a customer's past order count, and a time trend; $Web_{ij}$ is equal to one if the order was made online; and $\delta_i$ is a customer-level fixed effect.

Table 9 presents the results from 16 different linear regressions based on Equation (2) that use various dependent variables. For the regressions in Columns (1)–(12), we also restrict the sample to customers who have made at least 10 orders and have ordered during both the pre-Web and post-Web periods; this restriction rules out household-level selection into the sample based on the availability of Web ordering and therefore more cleanly identifies the causal effect. Because the store does not link walk-in orders to its customer identifier, walk-in orders are dropped under this restriction, meaning that the difference in Web orders is compared to phone orders only. We cluster all standard errors by household.

Table 9: Regression results of order characteristics potentially influenced by social interaction among online orders.

| | All Orders | | | | | | Coupon Orders | | | |
|---|---|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) |
| | Complexity Mean Item | Complexity Max Item | Calories Mean Item | Calories Max Item | Order has a Half Topping | Order has a Double Topping | Complexity Mean Item | Complexity Max Item | Calories Mean Item | Calories Max Item |
| Web Order | 0.386*** | 0.465*** | 51.52** | 71.62*** | 0.107*** | 0.0328*** | 0.415*** | 0.462*** | 117.95*** | 148.25*** |
| | (0.0466) | (0.0515) | (21.24) | (23.296) | (0.0148) | (0.00812) | (0.0679) | (0.0689) | (28.61) | (34.52) |
| N | 48446 | 48446 | 48446 | 48446 | 48446 | 48446 | 25590 | 25590 | 25590 | 25590 |
| Number of FEs | 2030 | 2030 | 2030 | 2030 | 2030 | 2030 | 1993 | 1993 | 1993 | 1993 |
| $R^2$ | 0.378 | 0.383 | 0.334 | 0.353 | 0.306 | 0.231 | 0.395 | 0.402 | 0.333 | 0.368 |

| | One Item Orders | | Small Pizza Orders | | Six+ Item Orders | |
|---|---|---|---|---|---|---|
| | (11) | (12) | (13) | (14) | (15) | (16) |
| | Complexity Mean Item | Calories Mean Item | Complexity Mean Item | Calories Mean Item | Complexity Mean Item | Calories Mean Item |
| Web Order | 0.463*** | 81.81** | 0.514** | 4.10 | -0.008 | -168.18 |
| | (0.0827) | (40.27) | (0.2429) | (24.26) | (0.1345) | (105.58) |
| N | 18437 | 18437 | 7556 | 7556 | 2708 | 2708 |
| Number of FEs | 1880 | 1880 | 4890 | 4890 | 1972 | 1972 |
| $R^2$ | 0.500 | 0.456 | 0.871 | 0.839 | 0.902 | 0.951 |

Standard errors clustered by household in parentheses.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Each column represents an OLS regression based on Equation (2). All regressions include controls for the day of the week and time of day an order was made, a customer's past order count, a monthly time trend, and customer fixed effects. Columns (1) - (12) are restricted to customers who have made (i) at least ten orders, (ii) at least one order during the pre-Web period, and (iii) at least one order during the post-Web period. Columns (7) - (10) are restricted further to those customers who used a coupon for their order. Columns (11) - (12) are restricted to those customers who ordered only one base item. Columns (13) - (14) are restricted to those customers who ordered only one small pizza. Columns (15) - (16) are restricted to those customers who ordered at least six base items.

The first two regressions show that consumers make more complicated orders online. Using the mean complexity of the order's base items as the dependent variable in Column (1), online orders are approximately 14.6% more complex than the sample mean. Similarly, in Column (2) where the maximum complexity of the order's base items is the dependent variable, online orders are 14.2% more complex.

A customer may also wish to avoid making an order with excessive calories in front of others (Allen-O'Donnell et al. 2011). To test this theory, Column (3) uses the mean calories of the order's base items as the dependent variable. Here, the mean base item within an online order has 3.0% more calories compared to the sample mean. Using the maximum calories as the dependent variable in Column (4), online orders have 3.5% higher calories.

Collectively, these regressions suggest that customers' choices are influenced by social interaction. To conclude that these findings stem from a social friction rather than some other unobserved factor, we next show that several alternative theories do not fully explain the differences among online orders.

## 3.4 Alternative Explanations Unrelated to Social Interaction

While the findings discussed above are robust to customer-level fixed effects and conservative sample restrictions, we now present additional evidence to support our claim that the inhibiting effects of social frictions best explain our results.

**Information About Available Items** One potential explanation for some items being ordered more often online is that customers without access to a menu may order different items. That is, without information about the full menu of products, a customer may simply order a pepperoni pizza because he recalls that item more readily, not because social frictions inhibit ordering complicated items verbally. Several pieces of supporting evidence suggest that this is not a primary explanation for our results.

First, this setting is a familiar one for most customers and the store's menu is typical; anyone who has ordered from another pizza delivery restaurant presumably could surmise most of the full menu. Moreover, the estimation sample contains only customers who purchased from the store before online ordering became available, which suggests that they have familiarity with the store's offerings from previous transactions. As such, customers having better information about available items seems unlikely to be a primary cause of the substantial changes we observe for online orders.

Second, consider the results from the regression of complexity in terms of topping size presented in Columns (5) and (6). Here, the dependent variable is equal to one if the order has a customized topping instruction of a half or double portion, respectively. In this case, any customer who knows that a topping is available is also likely to know the topping is available in different amounts. And because Web customers are more likely to alter the size of their toppings, especially for larger portions, it seems unlikely that information about product offerings is responsible for the greater complexity among online orders.

Third, consider Columns (7)–(10) which present results from a sample restricted to customers who used a coupon. Because coupons come affixed to menus for this store, any customer who uses one plausibly has access to the same information about products as those who order online. All results are robust to this more conservative sample restriction.

Fourth, previous studies have shown that consumers with better access to nutritional information may consume fewer calories (Bollinger et al. 2011). Because the store's website has more prominent information about nutrition, the results pertaining to the impact of online ordering on the number of calories per item are conservative along this dimension.

Finally, customers do not exhibit behavior consistent with learning after ordering online. If a lack of information about product offerings leads consumers to order more-prominent items over the phone, then becoming aware of less-prominent items after using the website should result in customers altering their behavior for subsequent phone orders. Based on a comparison of Web and non-Web orders for customers following their first online purchase, no such change occurs: customers continue to purchase more popular items (as well as items with fewer instructions and calories) in their subsequent phone orders, suggesting that the website does not make them more aware of less-prominent items.[17]

**Ease-of-Use and Order Accuracy** Another potential explanation for more complex and higher calorie items ordered online is that complex orders are easier to make on a website; that is, the results may be driven entirely by an easy-to-use online interface. We contend that ease-of-use is unlikely to explain our results for three primary reasons. First, an ease-of-use explanation also would apply to the number of base items within an order, as the mechanics of the website that would facilitate customized topping instructions also would facilitate ordering more base items. Recall from Table 7, however, that the average online order actually contains slightly fewer base items. Second, the store's employees likely have greater facility with the ordering system than any customer could possibly have with the website; they are simply more adept at using the store's sales terminal than a customer is at navigating the website. This is especially true for complex orders that require multiple button clicks online but could be entered quickly on the store's touchscreen sales terminals. Third, recall from Table 9 that customers order double portions of toppings more often online even though it is as trivial for a customer to say, for example, "double bacon" over the phone as it is for him to click through the online drop-down topping menu twice. As evidence for this, it is double and triple orders for high calorie items that are relatively high online. For example, double and triple bacon orders increase more than ten times as much as double and triple orders for vegetable toppings.

Related to the ease-of-use explanation, consumers may avoid making complex orders over the phone to reduce the potential for misunderstandings. While in the alcohol

[17] Summary statistics reported in Appendix Table 2.

setting we could not rule out a fear of miscommunication as an explanation for why the self-service format affected sales of difficult-to-pronounce items, three institutional details in the pizza setting suggest that social frictions, and not concerns over miscommunication, best explain customers' choices.[18]

First, as discussed above, customers order double portions of toppings more often online, an instruction that is unlikely to be misunderstood. Furthermore, as discussed above, the increase is not driven by vegetable toppings: double and triple bacon orders increase more than ten times as much as double and triple orders for vegetable toppings.

Second, for customers' concerns about order accuracy to confound our results, consumers would have to believe that employees make fewer mistakes fulfilling online orders. It may well be the case, for instance, that an employee taking an order over the phone in a loud restaurant might not understand a customer's instructions and mistakenly deliver the wrong items. For this point, we have a (somewhat noisy) measure of mistakes: "voided" items that occur when an order changes during a call, either because the employee makes a mistake or because the customer alters his order after the fact. To determine if such mistakes prompt customers to place future orders online, we compare customers who had voided items in their orders during the pre-Web period to those who did not. Customers with voided items in the pre-Web period are not more likely to eventually use the Web, suggesting that concerns over the accuracy of complicated orders due to previous bad experiences does not explain Web use.

Third, and relatedly, those who made the most complex orders during the pre-Web period are not more likely to switch to ordering online, as shown in the online appendix. These customers are unlikely to be embarrassed about making complicated orders — they have done so before — but they would benefit the most from switching to online ordering if it were easier to make complicated orders through the website or to ensure that the correct items are delivered.

[18] Regression results in this section are presented in the Appendix Table 4.

**Group Size** Another potential confound for our results is that we do not observe the size of the group making the order. Related to the ease-of-use explanation above, a complicated order for a large group may be easier to make online in the sense that each person can individually input his instructions on the website rather than having one person relay several complicated instructions for the entire group over the phone. To this point, first note that online orders have the same number of base items, on average, suggesting that large groups do not disproportionately use the website. Second, consider Columns (11)-(12) of Table 9 that restrict the estimation sample to those customers who ordered only one base item. These orders are presumably more likely to come from a single individual, and so will not be affected by any group dynamics. In this case, all results are robust. Similarly, Columns (13)-(14) restrict the sample to orders for a single small pizza (though without the other sample restrictions because only 62 Web orders were made for a single small pizza among this group) and the results for complexity remain robust though those for calories are not statistically significant. Finally, Columns (15)-(16) consider orders for six or more base items — these orders are more likely to be made by a large group, and hence the social interaction among group members may overwhelm any disinhibition effect from the website. The results are consistent with this hypothesis, as online orders become statistically indistinguishable from phone orders.

**Selection Bias** Consumers who order online may differ systematically from those who do not (Zentner et al. 2012). For instance, those more likely to use the Internet (e.g., teenagers) may also prefer to order complicated items for reasons unrelated to social frictions (e.g., teenagers have different preferences than adults). While we attempt to control for this confound directly by using household-level fixed effects and conservative sample restrictions, we also provide further evidence that selection bias does not undermine our results in the appendix. Notably, customers who eventually order online make similar choices during the pre-Web period as those who never order online.

**Discussion** Given that the results on complexity and calories do not appear to be driven entirely by information, ease-of-use, order accuracy, or selection bias, we argue that the impersonal nature of internet transactions is the most likely explanation for the different sales patterns across the online and offline channels.

# 4 Model and Estimation of Social Frictions

## 4.1 Interpreting the Descriptive Results as Social Frictions

Thus far, we have shown descriptive evidence from two examples of retail format changes. In each case, the new format required less social interaction between the customer and the clerk. Also in each case, sales patterns exhibited a marked change. As such, we have argued that these changes suggest that the social nature of a transaction affects consumers, and we have provided evidence to support this interpretation of our findings.

Therefore, we argue that consumer behavior is changing because the social nature of the interaction inhibits certain purchases. This result is consistent with psychology, sociology, political science, and medical research (mentioned above) that social interaction can inhibit a variety of behaviors.

In other settings where a feature of the market inhibits behavior, economists often call these features "frictions." For example, menu costs are a friction that inhibit price changes; search costs are a friction that inhibit matching between workers and firms; and transportation costs are a friction that inhibit trade.

For this reason, we refer to the change in behavior driven by the social interaction as a "social friction." Next, we add this friction to a structural model of demand. In keeping with Grace (2007) who shows that negative social interactions (particularly embarrassment) can affect customer satisfaction in retail environments, we model the social friction as a disutility from making a potentially embarrassing purchase. Next, we build and estimate a model that allows us to measure the welfare effects that stem from such social transaction costs.

## 4.2 The Welfare Effects of Reducing Social Interaction

In contrast to the alcohol setting, the individual-level data from pizza orders allow us to estimate the welfare consequences of removing a layer of social interaction, both for consumers and the firm.

**Consumer Surplus** Because a number of customers switched to online ordering when given the choice, a straightforward revealed preference argument suggests that their welfare has increased. These potential welfare gains may derive from several sources. For instance, some consumers may simply find ordering over the internet more convenient, while the lack of social interaction may free others to configure their orders in a way that increases utility. On the other hand, some consumers may find ordering online more cumbersome, or even that complicated orders are easier to make in person. In light of such heterogeneity, this section outlines a random coefficients discrete choice model to quantify the gains in consumer surplus attributable to online ordering.[19]

In the model, let consumer $i$ choose among $k$ discrete complexity options and $m$ methods of ordering for each of his orders, $o$.[20] In this case, $k$ indexes the mean number of instructions for the base items within an order, rounded to the nearest integer such that $k \in \{1, \ldots, 6\}$, which captures 99% of orders[21]. Furthermore, let $m \in \{Web, Non\text{-}Web\}$ represent the chosen method of ordering. The utility a customer derives from an order with a mean of $k$ instructions through method $m$ is then

$$U_{ikmo} = \beta_i^p Price_{ikmo} + \beta_i^c Complex_{ikmo} + \beta_i^w Web_{ikmo} + \beta_i^e Friction_{ikmo} + \varepsilon_{ikmo}, \quad (3)$$

where $Price_{ikmo}$ is the price associated with an order of mean complexity $k$; $Complex_{ikmo} \in$

[19]The revealed preference framework for understanding the impact on consumer surplus explicitly takes a somewhat libertarian perspective that assumes consumers know what they want. Thus, the increase in calories is welfare-enhancing because it is the choice consumers make absent a friction. This is the standard tool for welfare analysis in economics, though we acknowledge that the conclusions are limited by the assumption that people know their true preferences.

[20]We model complexity choices because their discrete nature fits well within a discrete choice model, though in principle the same setup can be used to model calorie choices.

[21]We need enough observations at each level of complexity to estimate the indirect utility from making that choice; above 6, the data are too thin to produce reliable estimates.

$\{0, \ldots, 6\}$ is the mean complexity of the order's base items associated with $k$ ($Complex = 0$ is the outside option of no purchase), while $\beta_i^c$ represents the utility consumer $i$ derives from each unit of instruction; $Web_{ikmo}$ is an indicator variable equal to one if the order was made online, while $\beta_i^w$ represents the "cost" of ordering online — this estimated coefficient will be negative to rationalize why the majority of orders do not occur through the website; $Friction_{ikmo}$ is an indicator variable equal to one if the method of ordering $m$ was not online and the mean complexity of the order's base items was $k \in \{4, 5, 6\}$ — $\beta_i^e$ then represents the disutility of making a complex, potentially embarrassing or frustrating order in the presence of others;[22] and $\varepsilon_{ikmo}$ is an unobserved error term that is identically and independently distributed extreme value and independent of $\{Price_{ikmo}, Complex_{ikmo}, Web_{ikmo}, Friction_{ikmo}\}$ and $\beta_i$. Notably, this random coefficients specification allows for more flexible substitution patterns across products than a standard logit. Finally, the outside option of not ordering has a utility normalized to zero. To estimate the model parameters, we fit a mixed logit by using maximum simulated likelihood.[23]

The sample for estimation is restricted to the 2030 customers (i) who have made at least 10 orders, (ii) who ordered in both the pre- and post-Web period, and (iii) who have a mean base item complexity of six or less. The period spans 56 months and the counterfactual price is taken to be the average price across the sample period.[24]

The results from a random coefficients logit appear in Column (3) of Table 10. The coefficients suggest that the mean "cost" of using the website has an implicit price of nearly \$8.90, with considerable heterogeneity around this mean.[25] In addition, customers derive greater average utility from providing more instructions per item, holding price constant — about \$0.85 per instruction, on average. This preference varies considerably throughout the sample, however, as the standard deviation of the coefficient

[22] Approximately 20 percent of orders have a mean item complexity of 4 or higher.

[23] See Train (2003) for further details.

[24] This store charges identical prices across all channels and did not alter prices following its introduction of online ordering, reducing concerns that our counterfactuals are contaminated by endogeneity bias.

[25] We divide each parameter by the mean price coefficient to discuss effects in terms of dollars.

on complexity is more than twice as large as the mean effect. Finally, and most importantly, social interaction has a meaningful and heterogeneous effect on order choices: for orders that may be embarrassing or frustrating due to their complexity, social frictions have an average implicit price of \$2.75, while those customers two standard deviations above the mean have a price equivalent to \$5.92. Characterizing social transaction costs based on excessive calories yields qualitatively similar results.

Table 10: Coefficient estimates of the structural demand model.

| | (1) | (2) | (3) |
|---|---|---|---|
| Mean Price | -0.763*** | -0.778*** | -0.579*** |
| | (0.00245) | (0.00194) | (0.0217) |
| Std. Dev. Price | | | 0.390*** |
| | | | (0.01118) |
| Mean Web | -3.019*** | -3.007*** | -5.154*** |
| | (0.0226) | (0.0226) | (0.276) |
| Std. Dev. Web | | | 3.187*** |
| | | | (0.3286) |
| Mean Complex | 0.377*** | 0.431*** | 0.491*** |
| | (0.00734) | (0.00613) | (0.0701) |
| Std. Dev. Complex | | | 1.083*** |
| | | | (0.03829) |
| Mean Friction | -0.667*** | -0.751*** | -1.595*** |
| | (0.0225) | (0.0187) | (0.164) |
| Std. Dev. Friction | | | 2.592*** |
| | | | (0.1062) |
| Constant | 1.623*** | | |
| | (0.00446) | | |
| Observations | 3702720 | 3702720 | 3702720 |
| *LL* | -384061.69 | -376992.4 | -208119.25 |

Robust standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

| Covariance | Price | Web | Complex | Friction |
|---|---|---|---|---|
| Price | 0.1524 | | | |
| Web | 0.2464 | 10.16 | | |
| Complex | -0.4085 | -1.0954 | 1.1728 | |
| Friction | 0.7318 | 3.1945 | -2.3106 | 6.7167 |

This table presents the estimated coefficients from the discrete choice model in Equation (3). "Friction" is defined as highly complex requests ordered offline. Column (1) contains the results from a logit specification. Column (2) contains the results from a fixed-effects logit. Column (3) contains the results from a mixed logit.

A full covariance matrix also was estimated for the parameters in the random coefficient logit, as shown at the bottom of Table 10. Our measure of social frictions is positively related to price sensitivity and the cost of Web use, though negatively related to the utility of providing more instructions per item.

Importantly, the random coefficients model permits a calculation of a consumer's willingness to pay for certain order attributes. Following Train (1998), Train (2003), and Revelt & Train (1998), the change in consumer surplus for a given $\beta$ is

$$C_{io} = \frac{ln \sum_k \sum_m exp(\beta x_{ikmo}) - ln \sum_k \sum_l exp(\beta x_{iklo})}{\beta^p}, \tag{4}$$

where $l$ indexes a counterfactual choice setting without online ordering. The compensating variation for consumer $i$ and order $o$ is then

$$CV_{io} = \int C_{io}(\beta) f(\beta|\theta) d\beta, \tag{5}$$

where $\theta$ represents the true parameters.

The average compensating variation constitutes the average of $CV_{io}$ taken over all orders by all consumers in the sample. Based on 1000 Monte Carlo simulations and 1% tail truncation, consumer surplus has increased 5.4% (s.e. 0.03%) due to online ordering as consumers avoid social transaction costs while making more-complex orders. These gains resemble those of Brynjolfsson et al. (2003) who estimate that consumer welfare increased by up to 4.2% due to a larger selection of products available at online booksellers.[26] In this sense, freeing consumers to choose their most-preferred item configuration without the need for social interaction increases utility by an amount similar to having access to a greater selection of products over the Internet. We note, however, that our estimates apply only to existing customers due to our sample selection, and hence represents a lower bound for potential welfare gains if new customers purchase

[26] Brynjolfsson et al. (2003) estimate a consumer welfare gain between $731 million to $1.03 billion in 2000 relative to overall book sales of $24.59 billion.

from the store as a result of online ordering.

**Producer Surplus** Because an item's price is non-decreasing in its complexity, the store stands to gain by reducing social frictions through Web ordering. And the store does benefit, in that customers spend roughly \$0.45 more when they order online, based on a regression with the same controls and restrictions as Equation (2). Notably, this increase in spending occurs on the intensive margin, so the store's per-item margin of approximately 66% applies.[27] That is, conditional on an order occurring, the store earns approximately \$0.29 in additional profits by allowing customers to order on the Web to the extent that other costs do not change (e.g., labor costs do not increase because orders have become more complex).

To account for the full effect of online ordering on the store's profits, note that customers using the Web would have made 0.416 orders per month, on average, but their spending increases by \$0.45. In addition, Web users increase their order frequency by 0.072 orders per month and spend, on average, \$15.46 per order. Thus, the store's average monthly gain from each Web customer is \$1.30, or 21.4%. As Web customers now constitute roughly 17% of the store's total sales, the store earns 3.6% more annually than it would in a counterfactual setting without online ordering. Note, however, that an absence of information about competitors restricts us to providing only a short-run approximation of the incremental profits the store earns each year from online orders. Moreover, this calculation — as with our estimates of consumer surplus — derives solely from existing customers, and therefore represents a lower bound for the full impact of online ordering that includes new customers as well.

These gains may seem underwhelming given the received wisdom that online platforms "disrupt" markets; however, online orders typically come from pre-existing customers — the store would reap a majority of these orders through traditional channels anyway, and thus a counterfactual estimate of the incremental benefits from online or-

[27]Our estimate of the store's average markup comes from a private correspondence with the store's owner.

dering must account for any cannibalized sales. In this sense, the findings here resemble the relatively modest counterfactual gains attributable to the Internet's diffusion documented elsewhere (Greenstein & McDevitt 2011).

**Summary** Overall, our calculations suggest that the frictions associated with social interaction have a substantial impact on welfare in this setting. While the specific numbers generated are driven in part by the modeling assumptions and data (such as consumers knowing their preferences and no entry of new consumers), these findings nevertheless provide a useful perspective on how social frictions affect welfare. For consumer surplus, the gain resembles prior estimates of the impact from online stores' larger selection of products, suggesting that the impact of removing social frictions in online transactions has important but previously unexplored implications for sales. For producer surplus, the increase, while modest, nevertheless rationalizes the firm's decision to implement online ordering.

# 5 Conclusions

We have documented that, in two different retail settings, social interaction relates to the types of products purchased by consumers. First, using data from a field experiment in which stores changed formats from behind-the-counter to self-service, we showed that difficult-to-pronounce products experienced a disproportionately large increase in sales. Second, we showed that the addition of an online ordering channel increased the sales of high-calorie and complex items at a pizza delivery restaurant. Together, these results suggest that personal interactions may inhibit certain kinds of economic activity, perhaps because customers wish to avoid the potential for embarrassment. This is consistent with a prior literature in psychology, sociology, and medicine documenting that individuals behave differently in sexually-charged or health-related settings that involve personal interactions.

These descriptive results led us to build a model in which social interaction can serve as a type of economic friction that inhibits certain behavior. We took this model to the pizza data and estimated that the less social (online) channel increased consumer surplus by a proportion similar to that estimated by Brynjolfsson et al. (2003) for the greater selection of products available at online bookstores.

We hasten to note, however, that our empirical settings have certain limitations that limit the scope of our conclusions. First, we analyze just two settings. And though these settings are common, their applicability to other markets, particularly beyond retail, remains speculative. Second, while the lack of competition in our alcohol setting is an advantage in terms of cleanly linking the change in sales format to the change in sales patterns, our welfare analysis in the pizza setting is necessarily limited in that it does not take into account competitors' responses; thus, our estimate of the impact on welfare is necessarily a short-run approximation. Third, in both settings the retail formats with less social interaction do not move to zero social interaction. In the alcohol setting, the item is still purchased from a clerk (though it is unlikely to be pronounced) and in the pizza setting the item is still delivered by a person. Fourth, our welfare calculations explicitly assume that consumers understand their preferences. If consumers do not make choices according to their true preferences (for example by choosing more calories than is optimal) then our estimates may be misleading. Furthermore, the welfare calculations relate only to existing customers. If the change brings in new customers, then our results will underestimate the true impact. Fifth, while we have attempted to show that other possible interpretations for our results are less relevant, we have simply documented that contexts with different levels of social interaction yield different outcomes — we cannot definitively conclude that this change is due to a social friction such as embarrassment. Thus, a more cautious interpretation of our results is that they simply demonstrate the importance of a transaction's context on the transaction itself, while leaving unsettled which particular mechanism affects consumers. In our case, we emphasize the role of social frictions because other explanations, such as consumers' recall of products from memory, are unlikely to explain our results across both empirical settings.

Despite these limitations, documenting similar outcomes across two distinct empirical settings, each with their own strengths and weaknesses, highlights the extent to which social interactions can influence consumers. Our results are consistent with recent economic models of privacy, especially Daughety & Reinganum (2010), that frame privacy as an individual's desire for others to perceive her choices in a positive light. Consistent with Goffman (1959) and others, our results suggest that personal interactions are an important aspect in enhancing this desire. Thus, our results identify why online settings, which are devoid of personal interactions, lead consumers to alter their behavior and establish an important perceived benefit of online commerce not previously mentioned in the economics literature (Scott Morton 2006).

Overall, our results build on the recent work in economics that explicitly models the effect of emotions and social cues on behavior (Card & Dahl 2011, Ifcher & Zarghamee 2011, Li et al. 2010, Akerlof & Kranton 2000, Rabin 1993, Daughety & Reinganum 2010, DellaVigna et al. 2012). Our results suggest that social interactions may inhibit economic activity, leading to reductions in consumer surplus and overall welfare. Speculatively, as a larger share of transactions move online, the prevalence of what was previously inhibited economic activity will continue to increase.

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