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Avi Goldfarb, Ryan C. McDevitt, Sampsa Samila, Brian S. Silverman

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The Effect of Social Interaction on Economic Transactions: Evidence from Changes in Two Retail Formats

Avi Goldfarb
Rotman School of Management, University of Toronto, Toronto, Ontario M5S 3E6, Canada, agoldfarb@rotman.utoronto.ca

Ryan C. McDevitt
Fuqua School of Business, Duke University, Durham, North Carolina 27008, ryan.mcdevitt@duke.edu

Sampsa Samila
National University of Singapore, Singapore 119245, sampsa.samila@nus.edu.sg

Brian S. Silverman
Rotman School of Management, University of Toronto, Toronto, Ontario M5S 3E6, Canada, silverman@rotman.utoronto.ca

Examining changes in two different retail formats, we show that consumers alter their purchases depending on the retail environment. In both settings, the change in behavior coincides with a reduction in the interpersonal interaction required to complete a transaction. As such, we contend that the format changes reduced a "social friction" that would otherwise inhibit consumers due to an implicit cost associated with ordering certain items in social settings.

Keywords: social cues; consumer choice; retail; social frictions
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1. Introduction

Retailers face a key choice in deciding how customers can purchase their products. Such format choices include nonstore retailing, self-service, self-selection, limited service, and full service (Kotler and Keller 2009), and the type of format chosen may ultimately affect the quality and quantity of items purchased by consumers. Given this motivation, we examine distinct changes in the formats of two different retailers to study how the amount of interpersonal interaction required to make a transaction may affect what consumers purchase. Our results suggest that interpersonal interaction inhibits certain types of consumer behavior, and we consider the most plausible explanation for this to be consumers' desire to avoid negative social judgment.

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 could experience such a sales increase because consumers might fear being misunderstood or appearing unsophisticated if they mispronounce a name in front of 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. Consistent with this notion, 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 required between the consumer and clerk to complete a transaction.

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 show that consumers purchase higher-calorie and more-complex items when ordering online—the average item in an online order has a statistically significant 3% more calories and a statistically significant 14% more instructions compared to an average item in a phone order. Importantly, we exploit several institutional details to support our hypothesis that the less-social nature of online transactions drives these differences: the different prevalence of high-calorie items among online orders compared to those made over the phone might be driven by consumers' desire to avoid negative social judgment of their eating habits, while the difference in complicated orders might be driven by a desire to avoid the negative social judgment associated with being difficult or unconventional.[1]

Combined, these findings suggest that interpersonal exchange affects the types of products purchased by consumers. After considering several explanations, we conclude that the most plausible is a "social friction" that imposes a (perhaps heterogeneous) cost on purchasing some products but not others. 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.

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. 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, our results are robust to focusing only on those customers likely to have a menu—and thus full information about product offerings—when they order.

Third, although 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.

[1] It is well documented that individuals 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 and Levav (2000) show that the desire to impress a clerk by ordering low calorie items changes restaurant ordering behavior. Theories of impression management (Goffman 1959, Banaji and Prentice 1994) suggest that complexity may cause embarrassment or frustration if customers fear appearing 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. The fear of being seen as difficult or demanding or taking time from others can prevent them from discussing their care with their doctors, even among patients with above average education and knowledge (Aldred et al. 2005, Boyd et al. 2004, Frosch et al. 2012).

Fourth, 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.

Fifth, 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 and Waldfogel 2013). Thus, our settings are firmly in the mainstream and complement previous studies by explicitly examining the impact of social frictions on market outcomes.

The notion that individuals avoid potentially uncomfortable social interactions has received considerable attention in sociology, psychology, medicine, and political science (Niemi 1976, Lee and Goldman 1979, Polivy et al. 1986, Dahl et al. 1998, Chapple et al. 2004, Ahmad et al. 2009). 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). Our paper contributes to this literature by applying an economic perspective to the previous work that has shown that social interaction changes behavior.

The purpose of our paper is therefore to formalize and measure the impact of a transaction's 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. We then document a change in sales patterns at a pizza delivery restaurant after the introduction of online ordering. 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 consider a field experiment conducted in the early 1990s by Systembolaget, Sweden's government-run monopoly seller of alcohol, that examined the likely consequences of switching their stores from behind-the-counter stores to self-service. Skog (2000) describes Systembolaget's experimental design and provides an assessment of its impact on overall

Table 1 Summary Statistics for Systembolaget Stores in the Field Experiment as of January 1991

Pair	Town	Group	Date of change	Town population	Sales (units)	Herfindahl	Revenue (kr. mil.)
1	Filipstad	Treatment	June 1991	13,296	58,413	0.0296	234.7
1	Nybro	Control	None	20,997	53,542	0.0184	281.0
2	Köping	Treatment	July 1991	26,345	97,701	0.0215	418.0
2	Säffle	Control	None	17,960	46,807	0.0207	223.2
3	Vänersborg	Treatment	November 1991	36,734	99,028	0.0144	449.0
3	Lidköping	Control	None	36,097	84,143	0.0163	374.4
4	Motala	Treatment	May 1992	42,223	92,758	0.0155	441.3
4	Falun	Control	None	54,364	123,305	0.0094	614.2
5	Karlshamn	Treatment	September 1993	31,407	82,538	0.0145	425.8
5	Lerum	Control	None	33,548	88,043	0.0167	345.5
6	Ludvika	Treatment	September 1994	29,144	78,178	0.0237	371.6
6	Vetlanda	Control	None	28,170	65,646	0.0192	307.0
7	Mariestad	Treatment	January 1995	24,847	92,972	0.0140	427.6
7	Värnamo	Control	None	31,314	88,514	0.0141	424.1
t-statistic of difference between groups				−0.4627	0.6586	0.9807	0.5092
p-value of difference between groups				0.6519	0.5226	0.3461	0.6199

alcohol consumption, which was Systembolaget's main concern with moving forward more broadly with the retail format change. After confirming Skog's finding that sales increased following the format change, we focus on examining how much of this change was driven by a reduction in social interaction between customers and staff.[2]

Systembolaget's stores provide an excellent setting for a study of retail formats. 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 (alcohol by volume)). Systembolaget's directive stipulates that the organization's sole purpose is to minimize alcohol-related problems by selling alcohol in 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, Systembolaget's objective is an unspecified 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 self-service by selectively changing the format of certain stores. To identify the likely effects of switching to self-service and to 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. (Because the experiment was restricted to one store towns, Stockholm and the other major cities in Sweden are not in the data.) According to Skog (2000, p. 96), Systembolaget used the 1984–1989 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 so as to prevent spillover effects and randomly selected the store that was converted to self-service within each pair. Table 1 lists the pairs of stores and their characteristics.

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, meaning that, because it has no competitors, there are no competitive responses to the format change that would confound our analysis. Third, according to the 2007 annual report, prices are based on a fixed (legislated) per-unit markup, reducing concerns that prices varied systematically in ways that might bias our results. Fourth and finally, Sweden prohibits advertising and promotions for alcohol above 2.25% ABV (though foreign magazines sold in Sweden

[2] Skog speculated that there were at least three possible mechanisms by which a format change would lead people to buy more alcohol: impulse purchasing, the "normalization" of alcohol as a product that need not be kept hidden behind the counter, and the freedom to move at one's own pace, "without being pressured by a queue of customers from behind and an impatient clerk up front ... [and without] hav[ing] to pronounce difficult, foreign brand names" (Skog 2000, p. 100).

[3] See "Systembolaget's mandate," http://www.systembolaget.se/English/Our-mandate/ (accessed January 22, 2015).

may carry alcohol advertisements), meaning that unobserved marketing around the format change does not cloud our analysis.

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. If social frictions do impact consumers, then the format change should disproportionately affect difficult-to-pronounce products compared to other similar products.

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 nonalcoholic drinks. We also have data on product availability and popularity from January 1984 to December 1987. Category-by-category results are shown in the

Figure 1 **Sample Page from Systembolaget's 1996 Menu**

Sherry och Montilla

Torr

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

8277 Amontillado 750 ml *82:-
Superior 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
Latt, frisk smak med nötig ton. Smakrik med lång eftersmak.

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

8225 Tio Pepe 750 ml 107:-
Gonzalez Byass 375 ml 55:-
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. Lang 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 750ml 73:-
Marqués del 375ml 39:-
Real Tesoro
Medelfyllig med kraftig, nötig smak och lite bränd ton. Olorosotyp.

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

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

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

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

8276 Leyenda Amontillado 750 ml 95:-
MGilLuque
Medelfyllig smak med bränd ton och karaktär av fat och nötter.

8209 Dry Sack 750 ml 97:-
(draj säk) 375 ml 49:-
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:-
(na'ti såle'ra) 375 ml 46:-
Gonzalez Byass
Smakrik med fin nötarom och aning bränd. Olorosotyp.

Söt

8232 Real Tesoro 750 ml 74:-
RoyalCream
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:-
(ra´jal krim) 375 ml *45:-
Fyllig med fin fruktighet och god nötighet. Smakrik.

8208 Pedro Ximenez Rare 750 ml *90:-
OldSweetPX
(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:-
(bri'stel krim) 375 ml 48:-
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. Läng smak.

Figure 2 Picture of a Typical Behind-the-Counter Systembolaget Store

Source. Systembolaget (http://en.wikipedia.org/wiki/Systembolaget). Copyright 2006 Christian Koehn, used under a Creative Commons Attribution License: http://creativecommons.org/licenses/by-sa/3.0/.

online appendix (available as supplemental material at http://dx.doi.org/10.1287/mnsc.2014.2030).

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, defined as 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;[4] we will control for such regional variation in several 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. Details of this exercise appear in the online appendix.

Figure 3 Picture of a Typical Self-Service Systembolaget Store

Source. Systembolaget (http://en.wikipedia.org/wiki/Systembolaget).

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

$$Outcome_{sct} = \beta\, TreatmentGroup_{sc} * AfterTreatment_{sct} + \mu_{sc} + \tau_t + \varepsilon_{sct}, \tag{1}$$

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 (μ_{sc}), as well as month fixed effects (τ_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 use any additional power from the pairing in the experimental design. The coefficient β will therefore capture how sales in the treatment group of stores change 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. 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

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

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	10,570
Units sold	12,439	15,423	15	159,917	10,570
Liters sold	6,246	7,092	3	63,220	10,570
Swedish products	0.3819	0.3873	0	1	10, 570
French products	0.0596	0.0739	0	0.4348	10,570
Market share difficult-to-pronounce					
Guide (by units)	0.2162	0.2348	0	0.7737	10,570
Guide (by volume)	0.2347	0.2420	0	0.8193	10,570
Over 30 characters (by units)	0.0099	0.0193	0	0.1255	10,570
Over 30 characters (by volume)	0.0101	0.0194	0	0.1254	10,570
Coder rates below top (by units)	0.4217	0.2872	0	1	10,570
Coder rates below top (by volume)	0.4626	0.3124	0	1	10,570
Unit of obs.: Product					
Pronunciation guide	0.5428	0.4983	0	1	1,658
Word length	17.820	8.5537	3	70	1,658
Mean coder score	8.3923	0.7953	5.33	9	1,625
Coder 1 score	8.1594	0.6612	6	9	1,631
Coder 2 score	8.7813	0.5341	4	9	1,628
Coder 3 score	7.9300	1.8721	1	9	1,628
Vodka	0.0730	0.2602	0	1	1,658
Other spirits	0.2467	0.4312	0	1	1,658
Wine	0.4608	0.4986	0	1	1,658
Fortified wine	0.0766	0.2660	0	1	1,658
Swedish beer	0.0844	0.2781	0	1	1,658
Imported beer	0.0308	0.1727	0	1	1,658
Nonalcoholic drinks	0.0277	0.1642	0	1	1,658
Unit of obs.: Store-product-month					
Units sold	129.35	485.17	−203[a]	29,836	1,016,428
Behind-the-counter format	0.2219	0.4156	0	1	1,016,428
Price (krona)	90.011	80.467	3	2,325	1,016,428

Note. 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 1% of the observations. These observations will be dropped from most of the analysis because we use a measure of logged sales.

the potential for overstating statistical significance (Bertrand et al. 2004); our results are robust to clustering at this level.

Table 4 shows the results of regressing the format change on both 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 we have all three measures of pronunciation difficulty for it,

Table 3 Summary Statistics for Systembolaget Treatment and Control Stores

Town	Treatment or control	Mean before	Std. dev. before	*p*-value	Mean after	Std. dev. after	*p*-value
Herfindahl	Treatment	0.0884	0.0712		0.0621	0.0558	
	Control	0.0816	0.0687	0.0005	0.0712	0.0668	<0.0001
Units sold	Treatment	15,327	18,833		16,443	19,236	
	Control	14,492	18,263	0.1040	13,042	16,651	< 0.0001
Liters sold	Treatment	7,726	8,440		8,222	9,148	
	Control	7,314	8,485	0.0408	6,679	8,382	0.0064
Revenue in million krona	Treatment	62.2	58.9		69.3	60.2	
	Control	57.5	55.8	0.0031	56.6	55.6	<0.0001
Fraction hard to pronounce	Treatment	0.2021	0.2316		0.2157	0.2297	
	Control	0.2260	0.2412	0.0003	0.2185	0.2347	0.6620

Notes. The first eight rows include all products. The final two rows include only products in the 1991 guide (and therefore coded for pronunciation difficulty). The *p*-values compare the treatment and control groups. They are artificially low because each store-category-month is treated as a separate observation. In the regression analysis, we cluster the standard errors to address correlated errors within store and across time.

Table 4 Treated Stores Sell More Volume and More Variety After the Change

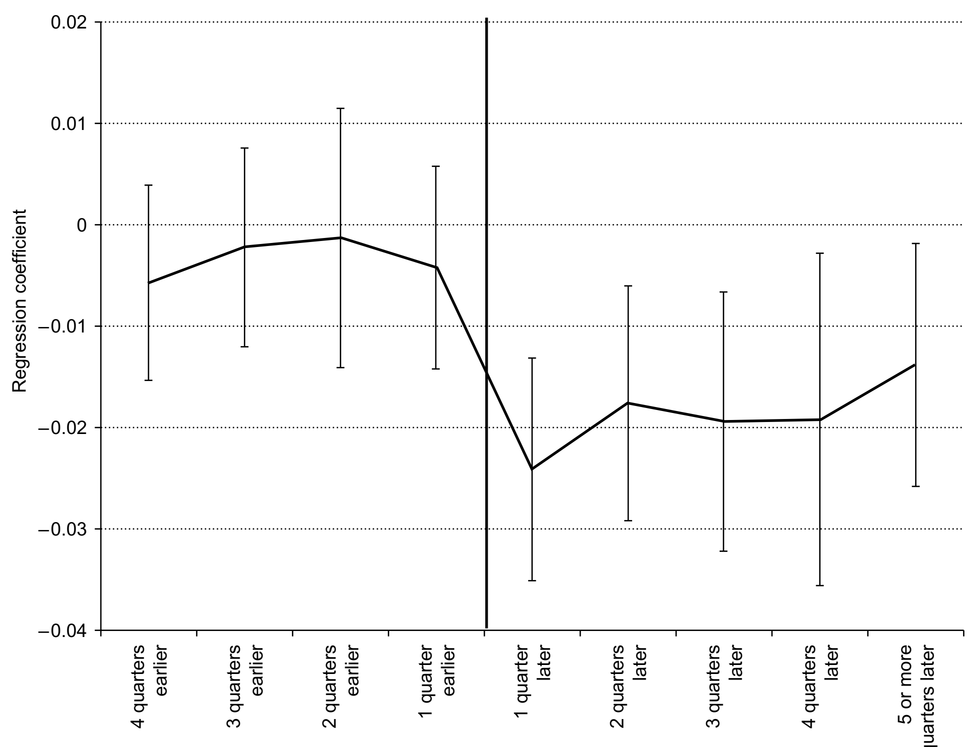
	Only products in 1991 guide						All products	
	(1) Herfindahl	(2) Log sales in units	(3) Herfindahl	(4) Log sales in units	(5) Herfindahl	(6) Log sales in units	(7) Herfindahl	(8) Log sales in units
Self-serve stores after change	−0.0154*** (0.0041)	0.1964*** (0.0246)	−0.0181*** (0.0045)	0.2214*** (0.0371)	−0.0181*** (0.0046)	0.2244*** (0.0366)	−0.0158*** (0.0037)	0.2283*** (0.0279)
N	10,570	10,570	10,570	10,570	10,570	10,570	10,570	10,570
No. of fixed effects	98	98	98	98	49	49	98	98
Avg. for dep. var.	0.09	8.53	0.09	8.53	0.09	8.53	0.08	8.69
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

Note. 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 1%.

making it 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, which may have two consequences. First, if the pairing was done poorly, it might introduce concerns about the proper specification of the functional form of the time series. Second, it might be possible to exploit the matched pairs to increase power (Imai et al. 2009, Imbens 2011). Roland and Fryer (2014) address these concerns by using flexible specifications for the functional form of the time series and by aggregating the fixed effects to the pair level. In this spirit, 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; and columns (7) and (8) show robustness of the main specification to using the full sample of products across all guides. The qualitative results do not change in any specification.

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

Figure 4 Coefficients of Regression of Herfindahl on Being in the Treatment Group Over Time Specification Resembles Column (1) of Table 4



Note. The coefficients for the before change period are jointly statistically different from the coefficients of the after change period.

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 toward a decreased sales concentration; the timing of the change in the estimated coefficient is coincident with the timing of the format change.

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. Qualitative results are robust to various perturbations of these definitions, 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 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. In each specification, we find a positive and statistically significant relationship between the fraction of sales from difficult-to-pronounce products and changing the stores to self-service.

As a baseline, column (1) regresses the fraction of difficult-to-pronounce product sales on the treatment dummy, and column (2) adds controls for the Herfindahl index and an interaction between the Herfindahl and the store format change. Here, the coefficient of 0.0169 is relative to an overall propensity of difficult-to-pronounce products at treatment stores in the pretreatment period of 20%, suggesting an 8% increase relative to baseline. Column (3) adds controls for the percentage of sales coming from 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 5,292 separate fixed effects (differenced out) for each pair-month; that is, it allows a nearly perfectly flexible time trend for each pair. Although this soaks up much of the variation in the data (the differenced out fixed effects are not included in the R^2),

Table 5 **Proportion of Difficult-to-Pronounce Products Increase After Format Change**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Self-serve stores after change	0.0220*** (0.0065)	0.0169** (0.0066)	0.0293*** (0.0061)	0.0181*** (0.0063)	0.0251** (0.0095)	0.0231** (0.0091)	0.0443*** (0.0108)	0.0385*** (0.0101)	0.0078* (0.0043)
Herfindahl		−0.7423*** (0.0972)	−0.7958*** (0.0979)	−3.9622*** (0.6965)	−0.7046*** (0.0913)	−0.6636*** (0.0933)	−3.2792*** (0.6771)	−3.3469*** (0.6615)	−3.2832** (1.2961)
Herfindahl × After change		−0.2213** (0.1025)	0.0504 (0.0967)	1.9648*** (0.5398)	−0.2815*** (0.0991)	−0.2939*** (0.1005)	1.1787** (0.5467)	1.4256** (0.5604)	1.7708 (1.3468)
Fraction domestic			0.1219*** (0.0451)	0.1078** (0.0503)			0.1140** (0.0481)	0.1305** (0.0503)	0.1361** (0.0563)
Fraction domestic × After change			−0.2775*** (0.0347)	−0.2866*** (0.0367)			−0.2992*** (0.0351)	−0.3038*** (0.0363)	−0.1618*** (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	10,570	10,570	10,570	10,570	10,570	10,570	10,570	10,570	10,570
No. of fixed effects	98	98	98	98	98	49	98	49	5,292
R^2	0.07	0.35	0.42	0.46	0.37	0.35	0.48	0.46	0.12

Notes. 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%.

we still find a positive and significant increase in the share of difficult-to-pronounce at self-serve stores.

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 from a regression of the share of difficult-to-pronounce products 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.

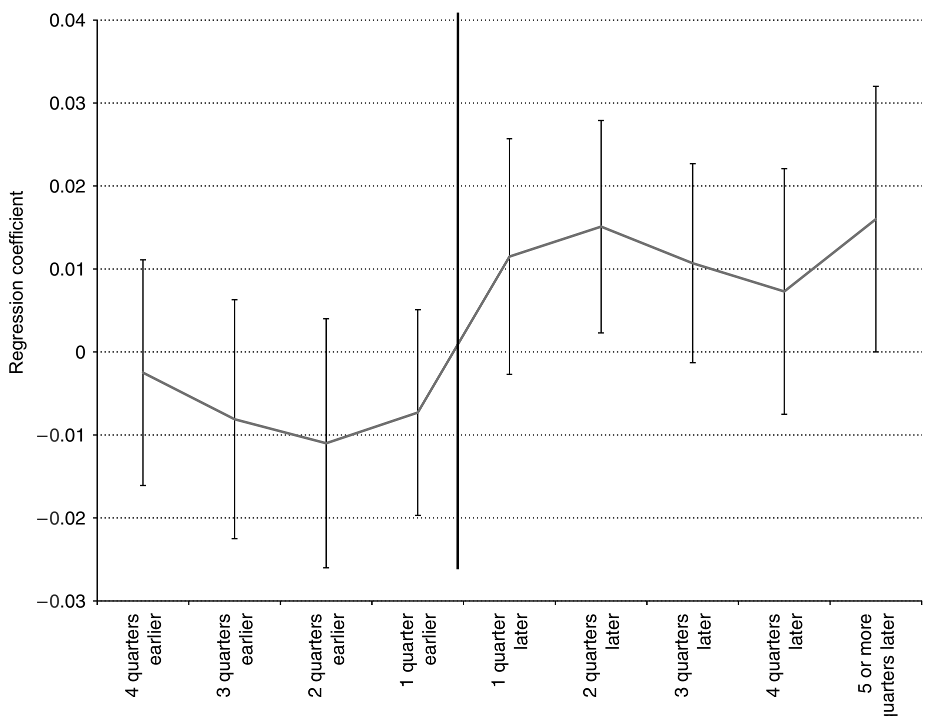
More broadly, our interpretation of the results from Table 5—that changing the format to reduce social interaction had a causal impact on the sales of difficult-to-pronounce products—is potentially just one of several competing explanations. Next, we address several of these alternatives, often referring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

To address the concern that the pronunciation guide should make phonetic reading easier—and thus render the presence of such guides a poor proxy for whether a product is difficult to pronounce—columns (1) and (2) show robustness to alternative classifications of difficult-to-pronounce names. Specifically, in column (1) we define a product's name as difficult to pronounce if any of the coders rated the product less than a "nine" for ease of pronunciation and in column (2) if the product's name has over 30 characters. 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 them, causes the sales of difficult-to-pronounce products to increase as consumers become more aware of obscure products while browsing the store's shelves. Another way to interpret this concern is to assert that search costs fall disproportionately for hard-to-pronounce products when the stores move to a self-service format. Our flexible controls for the Herfindahl index and the fraction of sales from domestic products partly address this concern. Moreover, column (3) shows that the results are not driven by a particular set of potentially unfamiliar (and disproportionately hard-to-pronounce) foreign products, those of French origin. The results change little when French products are dropped.

Columns (4) and (5) address a concern related to the difficulty of remembering names. Although we cannot

Figure 5 Coefficient of Regression of Fraction Difficult-to-Pronounce on Being in Treatment Group Over Time



Notes. Specification resembles column (1) of Table 5. The coefficients for the before change period are jointly statistically different from the coefficients of the after change period.

Table 6 Further Exploration of Results on Difficult-to-Pronounce Products

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Dependent variable:	*Percent sales hard-to-pronounce*	*Percent sales hard-to-pronounce*	*Percent sales hard-to-pronounce*	*Percent sales hard-to-pronounce*	*Percent sales hard-to-pronounce*	*Percent sales hard-to-pronounce*	*Percent sales hard-to-pronounce*	*Log sales*	*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 (1984–1987)	Not top quartile products (1984–1987)	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 × *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. pretreatment	0.4350	0.0101	0.2072	0.1966	0.5238	0.1570	0.3253	4.8355	8.2889
N	10,570	10,570	10,570	10,570	7,549	9,052	10,439	10,570	10,570
No. of fixed effects	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

Notes. 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. The number of observations is smaller in columns (5) and (6) because some store-categories have no sales. For example, the Swedish beer category is always dropped in column (5) and the nonalcoholic category is always dropped in column (6). 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%.

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. Although another useful specification would be to condition on Swedish products only, there are not enough hard-to-pronounce Swedish products to run this analysis.

Columns (6) and (7) provide a specification check on the intuition that pronunciation difficulty is unlikely to act as an impediment to ordering familiar products, because 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) classified on the menu as difficult to pronounce, the percentage of sales from difficult-to-pronounce products is unrelated to the retail format. By contrast, column (7) shows that for relatively unpopular products, sales are substantially lower in the behind-the-counter format.[5]

We view the above results as suggesting that search costs did not fall disproportionately for hard-to-pronounce products. Given the various ways to control for familiarity and sales, our identifying assumption is violated only if hard-to-pronounce products are less familiar than other products *with similar levels of sales and from similar countries.*

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. Although 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 a sales increase following the format change. We do not think this is likely to conflict with 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 so as to not contaminate the

[5] 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.

experiment. 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 would then be the store manager and the underlying assumption is that the manager understands the buying behavior of the customers.

Out-of-stock items could 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, resulting in hard-to-pronounce products being disproportionately out-of-stock in the self-service format. By 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.[6]

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.

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. We turn next to an alternative setting where we document a similar result, suggesting that our results are not idiosyncratic to one particular setting.

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 midsize metropolitan area.[7] 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 them 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, 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.

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 data set used for our analysis includes all food items from orders made between July 2007 and December 2011.[8] 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.

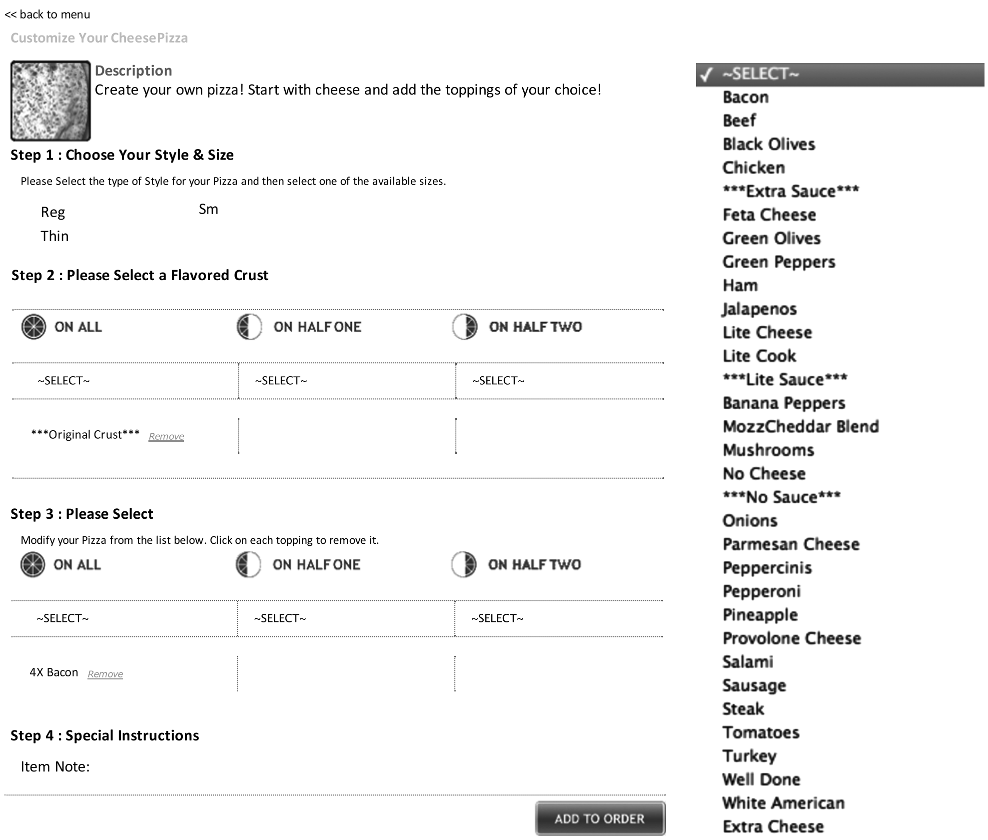
The measure of complexity in this paper refers to the number of instructions a customer provides for each base item in his order. For example, we define a large pizza as having a complexity equal to one, a large pepperoni pizza as equal to two, a large pizza with half pepperoni and half sausage as equal to three, and so on. Thus, the minimum complexity for any base item is 1, and 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

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

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

[8] 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.

Figure 6 **Screenshot of the Store's Website (Stripped of Identifying Content), and the Drop-Down Menu for Toppings**



a complexity equal to one 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—pizza (1), toppings (4), special crust (1)—and a mean base item complexity of 4. The mean complexity comes from having two base items and a total of eight instructions, which includes the base of 1 for each item.

Figure 7 **Sample Order from the Store's Sales Terminal**

Date:	03/12/2010	Taken by:	Customer:
Order number:	50	Table:	
Order type:	Delivery		
Order Time:	05:17 P.M.		
	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	1.49
		Subtotal	25.94
		Delivery fee	2.00

Notes. 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).

The store also provided information for the number of calories in each item. As a benchmark, a large cheese pizza has 2,080 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 2,521 calories and the maximum base item has 2,779.

The data set 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, 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 mean base item is 14.6% more complex and has 5.1% more calories in an online order compared to a phone order, whereas 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. For instance, customers ordering in the store spend $3.66 less than ordering online, mainly because they order 0.4

Table 7 Descriptive Statistics for Pizza Data

	Full sample				Web comparison					
Variable	Mean	Std. dev.	Min	Max	Web mean	In-store mean	*t*-statistic	Web mean	Phone mean	*t*-statistic
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	1,694.613	607.077	110	6,010.84	1,798.84	1,512.11	30.52	1,798.84	1,711.27	14.21
Calories–Max order item	2,022.724	625.991	110	6,010.84	2,154.81	1,699.34	45.51	2,154.81	2,035.65	19.15
N		160,168			10,693	8,244		10,693	96,558	

Notes. Summary statistics from the full data set 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. The variable *Calories–Mean order item* is the average number of calories per item within an order. The variable *Calories–Max order item* is the calories for the item with the maximum number of calories within an order.

(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, the store does not link in-store orders to households, and hence they cannot be included in regressions with household fixed effects, our preferred specification.

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, because 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 channel and non-Web (i.e., phone) channel. 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 four percentage point difference documented by Brynjolfsson et al. (2011) for online and catalog clothing sales. Finally, the top 10 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

Table 8 **Online Orders Have a Less Concentrated Sales Distribution**

	Items distinguished by size		Items not distinguished by size	
	(1)	(2)	(3)	(4)
	Herfindahl	Herfindahl	Herfindahl	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
No. of months	56	56	56	56
R^2	0.7608	0.7611	0.9317	0.9458

Notes. Unit of observation is the channel-month. Robust standard errors clustered by month in parentheses.
***Significant at 1%.

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.

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. Although 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 in this case and search capabilities change little. Instead, we next consider how social interaction might affect the types of products sold, which in turn could explain why the sales concentration falls for online orders.

3.3. Online Orders and Items Affected by Social Interaction

As we did for alcohol sales in §2, we now consider whether making a transaction more impersonal changes the types of products ordered by customers. 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 that 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 shown that eating in the presence of others leads individuals to consume fewer calories. For example, Polivy et al. (1986) show in 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 and Polivy 1996). Although these studies considered the negative implications of others' witnessing one's consumption of excessive calories, including potential embarrassment, other scholars have considered the positive implications of others' witnessing one's judicious food choices. For example, Ariely and Levav (2000) show that the desire to impress a clerk by ordering items with fewer calories changes what individuals order at restaurants.

Second, an individual may be viewed as finicky for making a complex order in the presence of others, a situation most individuals prefer to avoid. Theories of impression management (Goffman 1959, Banaji and Prentice 1994) suggest that complexity may cause embarrassment or frustration if customers fear appearing 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. Further, Belk (1980) shows that unconventional consumption choices yield an unfavorable impression, and 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 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

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

with $Y_{ij} \in$ {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 δ_i is a household 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,

Table 9 Regression Results of Order Characteristics Potentially Influenced by Social Interaction Among Online Orders

	All orders						Coupon orders				One-item orders		Small pizza orders		Six-plus item orders	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
	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	Complexity mean item	Calories mean item	Complexity mean item	Calories mean item	Complexity mean item	Calories mean item
Web order	0.386***	0.465***	51.52**	71.62***	0.107***	0.0328***	0.415***	0.462***	117.95***	148.25***	0.463***	81.81**	0.514**	4.10	−0.008	−168.18
	(0.0466)	(0.0515)	(21.24)	(23.296)	(0.0148)	(0.00812)	(0.0679)	(0.0689)	(28.61)	(34.52)	(0.0827)	(40.27)	(0.2429)	(24.26)	(0.1345)	(105.58)
N	48,446	48,446	48,446	48,446	48,446	48,446	25,590	25,590	25,590	25,590	18,437	18,437	7,556	7,556	2,708	2,708
No. of fixed effects	2,030	2,030	2,030	2,030	2,030	2,030	1,993	1,993	1,993	1,993	1,880	1,880	4,890	4,890	1,972	1,972
R^2	0.378	0.383	0.334	0.353	0.306	0.231	0.395	0.402	0.333	0.368	0.500	0.456	0.871	0.839	0.902	0.951

Notes. Each column represents an ordinary least squares 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 10 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) and (12) are restricted to those customers who ordered only one base item. Columns (13) and (14) are restricted to those customers who ordered only one small pizza. Columns (15) and (16) are restricted to those customers who ordered at least six base items. Standard errors clustered by household in parentheses.

$^{**}p < 0.05$; $^{***}p < 0.01$.

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.

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% more calories.

Collectively, these regressions suggest that customers' choices are influenced by social interaction. To support our conclusion 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

Although the findings discussed above are robust to household 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.

3.4.1. Information About Available Items. One potential explanation for why certain items are ordered more often online is that customers without access to a menu may order different items than those more aware of the available offerings. 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 at least some 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 that 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 on this dimension.

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 makes information about nutrition more prominent, our finding that ordering online leads to an increase in the number of calories per item purchased by consumers is 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. Summary statistics for these results are reported in the online appendix.

3.4.2. Ease-of-Use and Order Accuracy. Another potential explanation for why more complex and higher calorie items are 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 a 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. In particular, it is double and triple orders for high-calorie items that increase the most among online orders, such as double and triple bacon orders rising more than 10 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 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. Regression results in this section are presented in the online appendix.

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 10 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 are recorded 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. 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.

3.4.3. 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) and (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) and (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) and (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 social friction effect from the website. The results are consistent with this hypothesis, as online orders become statistically indistinguishable from phone orders.

3.4.4. Selection Bias. Consumers who order online may differ systematically from those who do not (Zentner et al. 2013). 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). Although we attempt to control for this confound directly by using household fixed effects and conservative sample restrictions, in the online appendix, we also provide further evidence that selection bias does not undermine our results. Notably, customers who eventually order online make similar choices during the pre-Web period as those who never order online.

In addition, if consumers are forward-looking and select the online channel because they anticipate ordering complex or high-calorie items, then our results might be driven by the initial selection into the channel. Still, the interpretation of the results does not change much: the online channel facilitates the purchase of more complex and higher-calorie items.

3.4.5. Fatigue. Fatigued consumers may order online because they find it less tiring than ordering over the phone. In addition, they may purchase higher-calorie foods because fatigue has weakened their self-restraint. In the regressions, we try to correct for this potential confound by controlling for the time of day an order was made, because orders made later in the evening may be more likely to come from fatigued customers. However, to the extent that the onset of fatigue varies across individuals, we cannot completely mitigate this confound. At the same time, we argue that an explanation related to social frictions remains more plausible because (i) we are comparing online and phone orders where the effects of fatigue should be similar and (ii) our results also hold for complexity and unusual items in addition to calories, choices for which fatigue should presumably *reduce* the likelihood of occurrence (see the online appendix for results on unusual items).

3.4.6. 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. Conclusions

We have documented, in two different retail settings, that social interaction influences 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 online orders at a pizza delivery restaurant had more calories and were more complex than orders made over the phone. Together, these results suggest that personal interactions may inhibit certain kinds of economic activity, perhaps because customers wish to avoid the potential for embarrassment.

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, in both settings the retail formats with less social interaction do not move to the extreme of having no social interaction whatsoever. In the alcohol setting, customers still purchase items from a clerk (though it is unlikely to be pronounced) and in the pizza setting customers still receive their orders from a delivery person. Third, although 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 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 are unlikely to be able to explain our results across both empirical settings.

Despite these limitations, documenting similar effects across two distinct empirical settings, each with their own strengths and weaknesses, highlights the extent to which social interactions can influence consumers. Following Goffman (1956, 1959), who emphasizes embarrassment as a likely mechanism through which social interaction influences behavior, we also argue that individuals' desire to avoid embarrassment drives much of our results. Specifically, Goffman defines embarrassment as a social phenomenon in which the desired projection of the self is disrupted; whereas shame may happen in solitude, embarrassment requires the presence of at least one other person. Although our data do not allow us to separately identify this type of embarrassment from other explanations, our results are consistent with prior literature in medicine, political science, psychology, and sociology on the role of embarrassment in changing behavior. In their review article on the psychology of embarrassment, Keltner and Buswell (1997) discuss how a fear of embarrassment harms individuals as they take self-destructive steps to avoid it 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 and Roberts 2014). Others have shown that embarrassment can affect voting choices (Niemi 1976), alter food consumption (Lee and Goldman 1979, Polivy et al. 1986, Banaji and 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 results are also consistent with recent economic models of privacy, especially Daughety and Reinganum (2010), that frame privacy as an individual's desire for others to perceive her choices in a positive light. In keeping 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 often 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). More specifically, 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." Although 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 impact of sales distributions.

Overall, our results build on the recent work in economics that explicitly models the effect of emotions and social cues on behavior (Card and Dahl 2011, Ifcher and Zarghamee 2011, Li et al. 2010, Akerlof and Kranton 2000, Rabin 1993, Daughety and Reinganum 2010, DellaVigna et al. 2012). Our results suggest that social interactions may inhibit economic activity in important ways. Speculatively, as a larger share of transactions are mediated by machines rather than people, the prevalence of what was previously inhibited economic activity will continue to increase.

Supplemental Material

Supplemental material to this paper is available at http://dx.doi.org/10.1287/mnsc.2014.2030.

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