The Effect of Social Interaction on Economic Transactions: An Embarrassment of Niches?

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Abstract

We show that social interaction reduces the diversity of products purchased by consumers in two retail settings. First, we consider a field experiment conducted by Sweden's monopoly alcohol retailer and find that moving purchases from behind the counter to self-service disproportionately increases the sales of difficult-to-pronounce products. Second, we use individual-level panel data from a pizza delivery restaurant to show that online orders have greater complexity and more calories, which increases both consumer and producer surplus. Combined, these results suggest that social inhibitions can substantially affect market outcomes, likely due to consumers' fear of embarrassment.

JEL: D12, L81, L86
Keywords: social cues; embarrassment; Internet economics; long tail

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1 Introduction

Many economic transactions are verbal and social, such as ordering drinks from a bartender, discussing ailments with a doctor, or seeking advice from a sales clerk. In this paper, we consider whether such social interactions influence consumers and, as a result, inhibit certain types of economic activity. Specifically, we show across two retail settings that consumers purchase a wider variety of goods when transactions require less communication, and that much of this change in sales patterns comes from products that consumers might be embarrassed to order verbally. These findings contribute to the growing literature on the impact of emotions and social cues on economic behavior, and provide a new explanation for why on the Internet, where purchases do not require social interaction, sales distributions are often less concentrated than they are for bricks-and-mortar retailers, a phenomenon commonly referred to as the "long tail."

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 matched pairs of towns, each with a single outlet, we show that the stores randomly converted to self-service sell a greater variety of products, with a significant fraction of this change coming from products with difficult-to-pronounce names. As shown in Section 2, the share of hard-to-pronounce alcohol products increases 6% in stores that switch to self-service.

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. As shown in Section 3, the average item in an online order is 14% more complex and has 3% more calories. Several institutional details suggest that the non-social nature of online transactions drives these differences, which has a substantial effect on both consumer and producer surplus. From a structural demand model, we estimate that reducing social interaction through online ordering has increased consumer surplus by 5.4%, an estimate larger than that of Brynjolfsson et al. (2003) for the benefits of online booksellers' greater selection of products. Moreover, we estimate that producer surplus has increased 3.5% due to non-verbal online orders.

Combined, these findings suggest that interpersonal exchange affects both the type and the diversity of products purchased by consumers. In the case of alcohol sales, consumers may wish to avoid appearing unsophisticated by mispronouncing 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 transaction. In the case of pizza orders, consumers may prefer to avoid social judgment of their food choices — an order with peculiar instructions or excessive calories may provoke negative reactions from others. By moving the transaction online and eliminating a layer of social interaction, a customer once again becomes more willing to order otherwise mildly embarrassing items. In this regard, both settings provide a compelling identification strategy to isolate the effect of social interactions on market outcomes and allow us to rule out a range of 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. Because greater product variety can result mechanically in a less-concentrated sales distribution, markets commonly associated with the long tail where retailers offer a wider selection of products, such as online books or videos, would not provide a suitable setting for our analysis (Brynjolfsson et al. 2003). Similarly, online retailers may differ from bricks-and-mortar stores along dimensions that influence consumers beyond just the extent of social interaction. The panel nature of both settings — and the field experiment used in the alcohol setting — reduce the potential of these other dimensions to 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 do not drive our results. For example, in the alcohol setting, the increase in sales comes from difficult-to-pronounce products in particular, rather than new or less-popular products. In the pizza setting, the website does not have sophisticated search tools that Brynjolfsson et al. (2011) argue will confound a comparison of bricks-and-mortar retailers with online stores that facilitate searching for customers.

Third, similar settings have been considered extensively in the economics and management literatures to study sales distributions (Pozzi forthcoming, 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 the findings of previous studies.

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

Furthermore, these data allow us to consider an important alternative explanation for our results: that consumers may wish to minimize the potential for misinterpretation 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 Web ordering was available are not more likely to make online orders. 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.

The notion that individuals avoid potentially embarrassing social interactions has received considerable attention in sociology, psychology, medicine, and political science. The foundation for these ideas dates back to at least Goffman's claim that social interactions are performances in which individuals act to project a desired image of themselves, and embarrassment occurs when this projection is disrupted (Goffman 1956, 1959). Embarrassment is therefore a social phenomenon.[1]

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 it. 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 the purchase of contraceptives (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).

Within economics, our paper contributes to the growing literature regarding the impact of both emotions and social cues on behavior. While no work has addressed embarrassment directly, recent 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, even if unrelated to embarrassment, may also 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.

[1] This literature emphasizes that embarrassment differs from humiliation and shame. Humiliation relates to a change in an individual's sense of dignity (Lindner 2001), whereas shame relates to a person's core self-image and can be experienced in social isolation. In contrast, embarrassment can only be experienced in the presence of others (Klass 1990).

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 the implications for privacy, our paper informs the online behavior literature by explicitly examining the effect of social interaction on market outcomes. Most notably, the perceived anonymity afforded by digital technologies has been credited with an increase in the distribution of pornography (Edelman 2009) and with the 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."

We explore the idea that social frictions may even affect settings with a comparatively mild potential for embarrassment. As such, our findings provide a new explanation for a commonly discussed Internet phenomenon — that niche products comprise a comparatively large share of total sales online, dubbed the "long tail" in Anderson (2004) — by showing that a reduction in social interaction leads to a less-concentrated sales distribution. The current literature emphasizes the roles of inventory capacity and search technologies (Scott Morton 2006), but does not discuss how the impersonal nature of online transactions could affect sales patterns. 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. As the perception of anonymity represents a distinguishing feature of many online transactions, our paper emphasizes a key aspect of Internet commerce not previously considered by the economics literature.

The purpose of our paper is therefore to formalize and measure the impact of social frictions 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 particularly 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 unusual orders; from this change, we also estimate its impact 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 Sweden's government-run alcohol monopoly, Systembolaget.[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 a responsible way. As such, it prohibits profit maximization from being an aim of the organization and dictates that no brands and suppliers be given preferential treatment.

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 retail model. 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

[2] Much of this description comes from Skog's (2000) assessment of the experiment's impact on alcohol consumption.

experiment.[3] 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 treated store within each pair. Table 1 lists the pairs of stores and their characteristics.

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

Town	Treatment or Control	Date of Change	Town Population	Sales (Units)	Herfindahl	Sales (Liters)	Revenue (Kr. mil.)
Filipstad	Treatment	June 1991	13296	58413	0.0296	28404	234.7
Nybro	Control	None	20997	53542	0.0184	27764	281.0
Koping	Treatment	July 1991	26345	97701	0.0215	50513	418.0
Saffle	Control	None	17960	46807	0.0207	23581	223.2
Vanersborg	Treatment	Nov. 1991	36734	99028	0.0144	51084	449.0
Lidkoping	Control	None	36097	84143	0.0163	43611	374.4
Motala	Treatment	May 1992	42223	92758	0.0155	48069	441.3
Falun	Control	None	54364	123305	0.0094	69196	614.2
Karlshamn	Treatment	Sept. 1993	31407	82538	0.0145	43830	425.8
Lerum	Control	None	33548	88043	0.0167	46687	345.5
Ludvika	Treatment	Sept. 1994	29144	78178	0.0237	41441	371.6
Vetlanda	Control	None	28170	65646	0.0192	33069	307.0
Mariestad	Treatment	Jan. 1995	24847	92972	0.0140	47584	427.6
Varnamo	Control	None	31314	88514	0.0141	45906	424.1

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 treated 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 competitors' responses to the format change are unlikely to be relevant outside of weak beer and non-alcoholic drinks. Third, according to the 2007 annual report, prices are based on a fixed (legislated) per-unit markup. Fourth and finally, Sweden bans advertising and promotions for alcohol above 2.25% ABV (though foreign magazines sold in Sweden may carry alcohol advertisements).

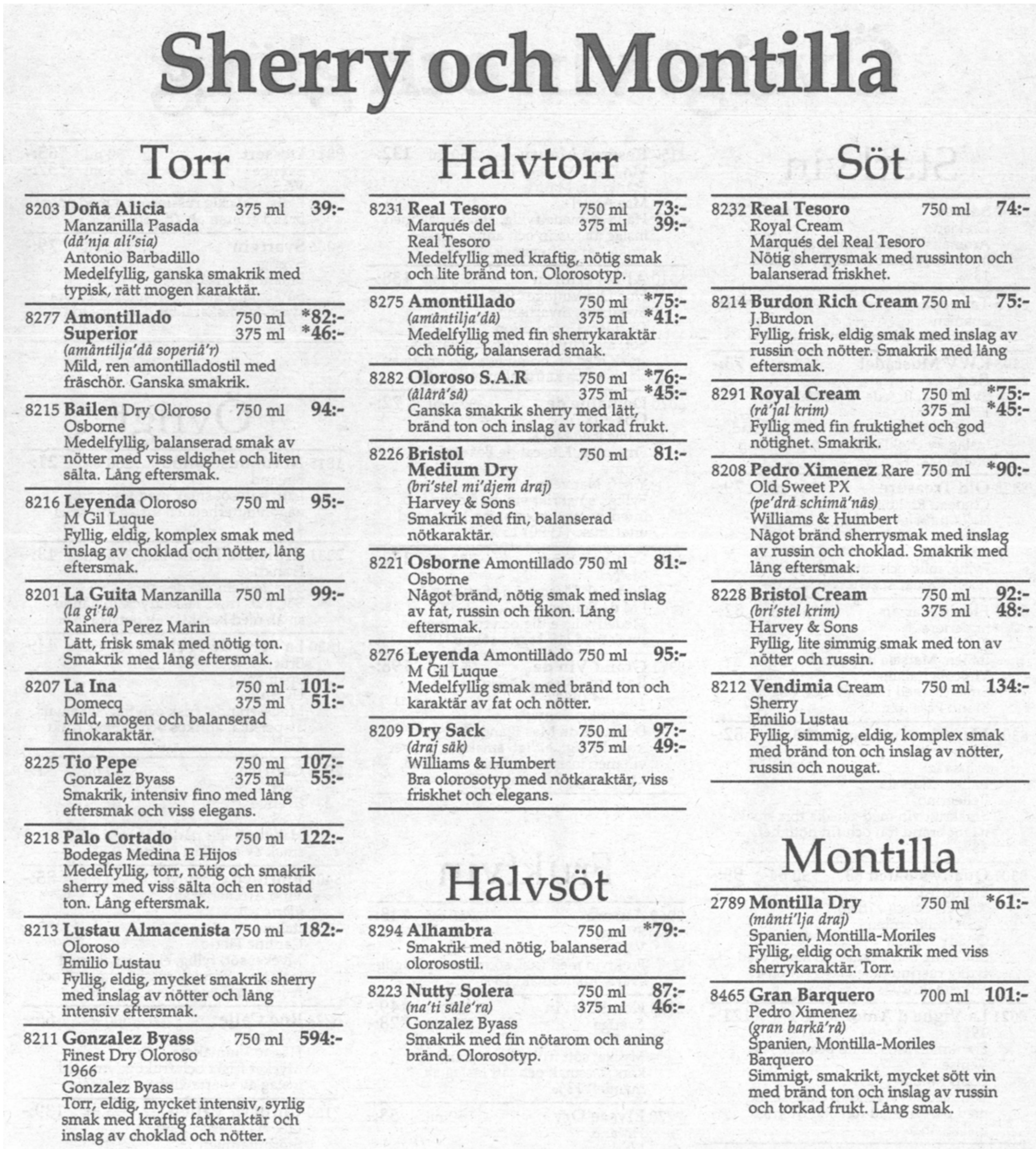
[3] Because the experiment was restricted to one-store towns, Stockholm and the other major cities in Sweden are not in the data.

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 prior to 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 roam the aisles 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). Customers then select items from the shelves before bringing them to the cash register for purchase. Thus, the key changes in the experiment are that (i) customers may browse aisles of products on display and (ii) customers need not ask a clerk for a product. We argue that if social frictions do impact consumers, then the format change should affect only 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 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.[4] Category-by-category results are shown in the appendix.

Our analysis proceeds at two levels of aggregation. First, we examine the data at the store-category-month level to show how a store's format affects the quantity (measured in units) and variety of products purchased by consumers. We then construct a Herfindahl index to measure 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.

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



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 Sherry 750 ml **134:-**
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.

For our second level of aggregation, we examine the data at the product-store-month level to show the differential sales patterns for difficult-to-pronounce products. We use several measures of how difficult a name is to pronounce. 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. 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.[5]

2.2 Store Format and the Concentration of Sales

In order to estimate the effect 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 for each of the four

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

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
Fraction 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.

outcomes listed above is:

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

The analysis thus controls for store-category fixed effects, μ_{sc}, and 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. 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 time period.

Because our data come from a true randomized field experiment, we have fewer concerns regarding 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 check that the change in sales is coincident with the format change. Because we observe each store several times, we cluster the standard errors by store to reduce the potential for overstating statistical significance (Bertrand et al. 2004).

Table 3 shows the results of the regressions described in Equation (1) for both the selected sample of products in the 1991 guide, as well as for the entire sample of products in the data. In most of our analysis, we focus on products that appear in the 1991 guide because they are coded for pronunciation difficulty. Columns (1) and (3) show that the sales concentration, as measured by a Herfindahl, falls substantially after a store changes to a self-service format: the estimated marginal effect is 0.0171 relative to an average of 0.09. Columns (2) and (4) show that sales, measured in units, increase by approximately 20%.

Table 3: 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
Self-Serve Stores After Change	-0.0171***	0.1964***	-0.0168***	0.2283***
	(-0.0037)	(0.0215)	(0.0029)	(0.0230)
N	10570	10570	10570	10570
Number of Groups	98	98	98	98
R^2	0.08	0.44	0.21	0.39

Regressions include store-category fixed effects (differenced out) and 107 monthly fixed effects.
Unit of observation is the store-category-month.
Robust standard errors clustered by store in parentheses.
* significant at 10%; ** significant at 5%; *** significant at 1%.

Figure 4 repeats the analysis in Column (1) but at a finer level. Rather than one discrete variable identifying when a store changes format, we substitute the *Self-Serve*

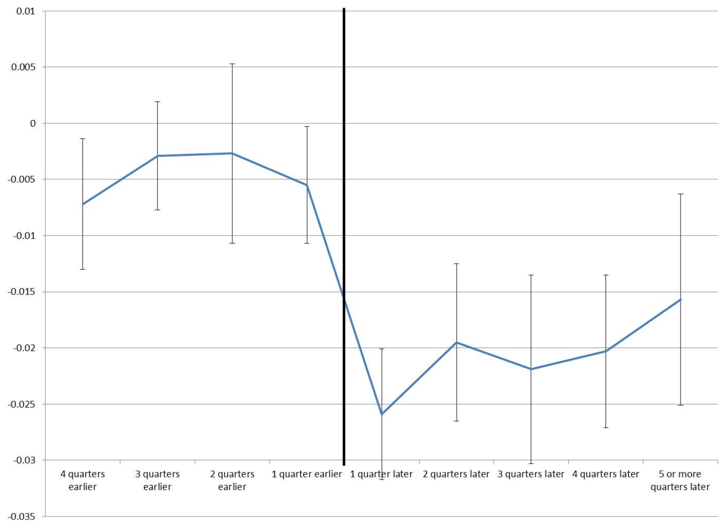


Figure 4: Coefficient on being in the treatment group over time (Herfindahl)
Specification resembles Column (1) of Table 3. Coefficients provided in Appendix Table 4.

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.

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 and the log of total quantity sold. We use four 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, (iii) whether any of the coders rated the product less than a "9" for ease-of-pronunciation, and (iv) the (negative) average coder score.[6]

Table 4 presents the results from ten specifications that regress the fraction of difficult-to-pronounce product sales on an indicator variable equal to one after a store converts to a self-service format, among other controls. 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 and the log of total quantity sold, while Column (3) controls for the percentage of sales that are of domestic (Swedish) products, as labeled in the menu; Column (4) weights the fraction of difficult-to-pronounce product sales by volume rather than units sold. The remaining columns show robustness to the alternative definitions of difficult-to-pronounce. Collectively, all specifications demonstrate that the share of difficult-to-pronounce products rises substantially when stores switch formats from behind-the-counter to self-service. A back-of-the-envelope calculation based on Column (2) suggests that the share of difficult-to-pronounce products increases by 6%.

2.4 Alternative Explanations Unrelated to Social Frictions

The results presented above could derive from sources other than social transaction costs. For example, the assignment of stores in the experiment may not have been independent of a trend toward increased sales of difficult-to-pronounce products, which would then bias our results. To address this concern, we check 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 on pronunciation, quarter by quarter, for the treatment stores in each three-month period leading up to, and following, the format change. The results show a sharp increase in the share of difficult-to-pronounce products after the format change.

[6] We use the negative average coder score so that it measures difficulty of pronunciation rather than ease of pronunciation. Qualitative results are robust to various perturbations of the definitions of difficult-to-pronounce. We show three representative examples here and, as discussed earlier, we prefer the pronunciation guide definition because the threshold is determined by a third party, independent of our study.

Table 4: Hard-to-pronounce products have a disproportionately large sales increase.

		Guidance on Menu			Word Length Over 30		Any Coders Below Top		Mean Coder Score (Negative)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Units	Units	Units	Volume	Units	Volume	Units	Volume	Units	Volume
Self-Serve Stores After Change	0.0220***	0.0131**	0.0111**	0.0159***	0.0010**	0.0008**	0.0238***	0.0348***	0.0056	0.0114**
	(0.0033)	(0.0049)	(0.0046)	(0.0045)	(0.0003)	(0.0004)	(0.0066)	(0.0078)	(0.0035)	(0.0041)
Herfindahl		-0.5693***	-0.8261***	-0.1254***	-0.0062***	-0.0072***	-1.1688***	-0.3634***	-0.7415***	-0.2777***
		(0.0432)	(0.0409)	(0.0224)	(0.0016)	(0.0019)	(0.0669)	(0.0777)	(0.0464)	(0.0462)
Log Sales		-0.0291***	-0.0285***	-0.0259***	0.0024***	0.0026***	-0.1152***	-0.1176***	-0.0625***	-0.0639***
		(0.0066)	(0.0062)	(0.0003)	(0.0065)	(0.0003)	(0.0092)	(0.0113)	(0.0050)	(0.0039)
Fraction Domestic			0.1223***							
			(0.0278)							
N	10570	10570	10570	10570	10570	10570	10570	10570	10570	10570
Number of Groups	98	98	98	98	98	98	98	98	98	98
R^2	0.07	0.38	0.25	0.12	0.12	0.52	0.31	0.43	0.28	0.28

Dependent variable is percent sales that are difficult to pronounce. Columns have different definitions of difficult to pronounce. Regressions include store fixed effects (differenced out) and 107 monthly fixed effects. Uses all products observed in the 1991 data. Robust standard errors clustered by store in parentheses.
* significant at 10%; ** significant at 5%; *** significant at 1%.

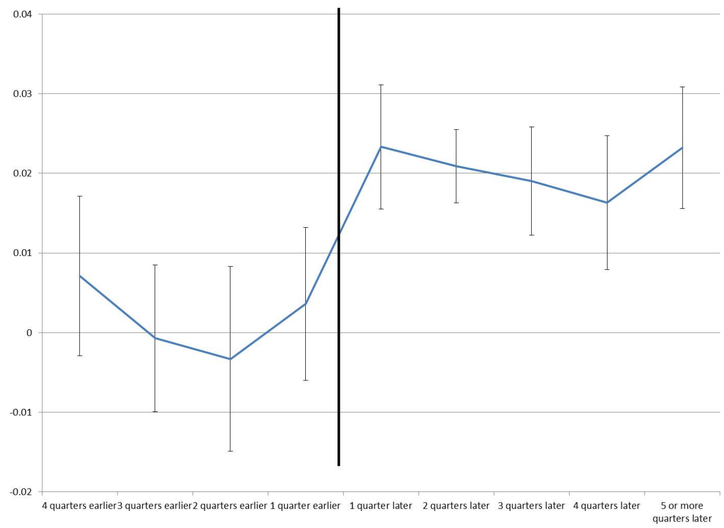


Figure 5: Coefficient on the interaction between difficult-to-pronounce and being in the treatment group over time. Specification resembles Column (1) of Table 4. Coefficients provided in Appendix Table 4.

More broadly, some other factor that is correlated with pronunciation difficulty may drive sales in the self-service format relative to the behind-the-counter format. In the analysis above, we accounted for this concern by (i) demonstrating that the results are not driven by a particular definition of pronunciation difficulty, (ii) controlling for the Herfindahl to demonstrate that the results are unlikely to be driven by difficult-to-pronounce products being less popular, and (iii) controlling for changes in the proportion of domestic products sold (though this may understate the overall effect because domestic products are less likely to be difficult to pronounce).

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. We cannot definitively reject this possibility; however, we still interpret this as a kind of social transaction cost, albeit one unrelated to embarrassment. In other words, it is still the social nature of the interaction that influences 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 complicated pronunciation (or at least the embarrassment of pointing 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 miscommunication. 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.[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 release of any beverage sales data and we 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. The store's website 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

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

identification because it closely resembles the layout of physical menus distributed to customers by the store, suggesting that consumers are unlikely to alter their behavior based on any particular feature of the website.

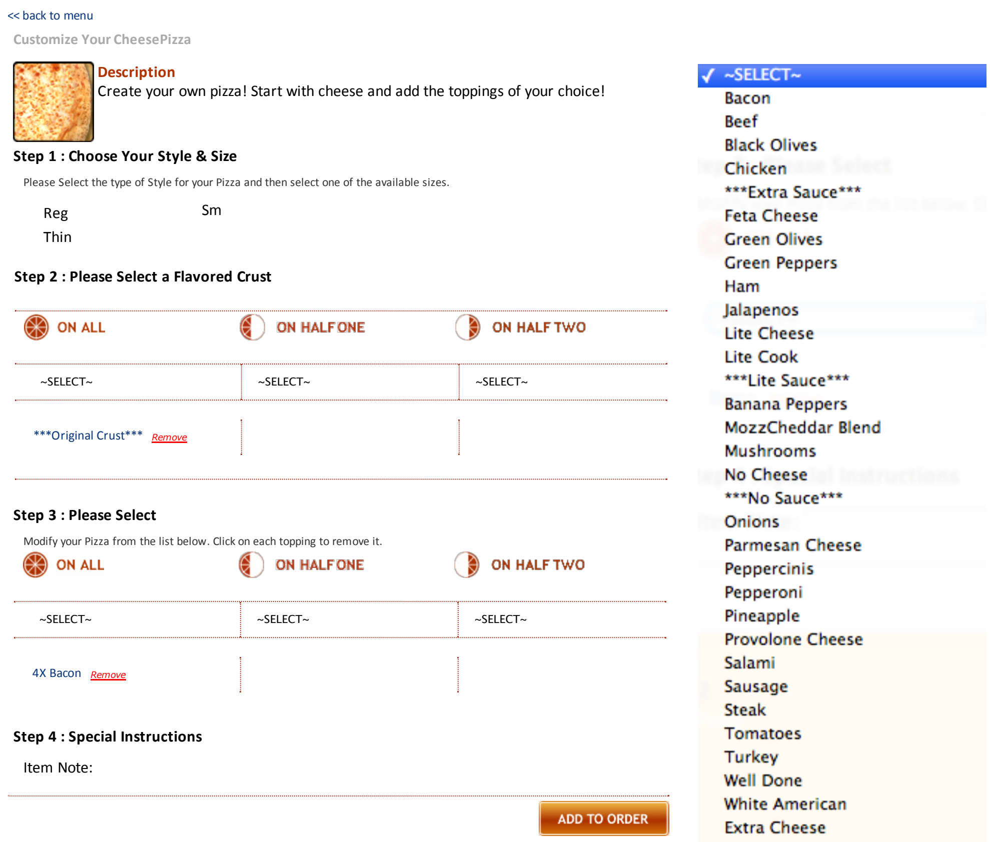


Figure 6: Screenshot of the store's website stripped of identifying content and the drop-down menu for toppings.

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 completed 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.[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 five 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 complexity 1, a large pepperoni pizza as complexity 2, a large pizza with half pepperoni and half sausage as complexity 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 complexity 1 in the data 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 5 and a mean base item complexity of 2.6. The mean complexity comes from having five base items and a total of 13 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 once again, the mean base item has 1196.2 calories, and the maximum base item has 2210.

Finally, we measure popularity based on the number of times an item has been ordered at the store. For instance, a large pizza is the most popular item, having been ordered 95,846 times. For our order-level estimations, we use the proportion of items in

[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.

Date:	11/09/2011	**Taken By:**	Samantha Smith	**Customer:**	
Order Number:	60	**Table:**			CHRIS [redacted]
Order Type:	Delivery				
Order Time:	05:51 PM				

1	Lg Reg Create Your Own Pizza	9.99
	Garlic Herb Crust	
	H1-Sausage	0.75
1	Sm Reg Create Your Own Pizza	7.49
	Sausage	1.19
	Pepperoni	1.19
	Onions	1.19
	Garlic Herb Crust	
1	Cinnamon Bread	3.99
1	Turkey Sub	6.99
	NO Tomatoes	
	NO Lettuce	
1	Sm Antipasto	4.99
	[$8.99 Your Choice Sml3top]	-2.07
	[$8.99 Your Choice Lrg1top]	-1.75
	Subtotal	**33.95**
	Delivery Fee	**2.50**
	Tax	**2.82**
	Tip	**7.00**
	Total	**46.27**

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). Entries contained within brackets represent promotions.

an order among the store's top ten most popular to connect a consumer's choices to the store's sales distribution, which will then allow us to study the effect of social frictions on the long tail.

The dataset comprises 160,168 orders made by 56,283 unique customers, with summary statistics reported in Table 5. Of the store's orders, 6.7% have been placed online and notable differences exist between these and non-Web orders. Customers using the Web spend $0.61 more, on average, though they order slightly fewer base items; this result stems from online customers ordering more toppings. The mean base item is 15.0% more complex and has 6.1% more calories in an online order, while the maximum base item is 16.9% more complex and has 7.2% more calories. In terms of popularity, the average online order contains 9 percentage points fewer top-ten items.

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 both before and after online ordering became available. Among this group, 700 (1.2% of total) made an order

Table 5: Descriptive statistics for pizza data.

Variable	Full Sample Mean	Std. Dev.	Min.	Max.	Web Comparison Web Mean	Non-Web Mean	t-stat
Web Order	0.067	0.25	0	1	0	1	
Order Price	14.702	6.829	3.49	49.99	15.46	14.85	9.04
Items in Order	2.036	1.156	1	17	1.99	2.02	3.27
Complexity – Mean Order Item	2.646	1.217	1	21	3.06	2.66	27.08
Complexity – Max Order Item	3.273	1.399	1	21	3.81	3.26	32.62
Calories – Mean Order Item	1694.613	607.077	110	6010.84	1798.84	1695.60	15.92
Calories – Max Order Item	2022.724	625.991	110	6010.84	2154.81	2009.20	21.74
Popularity – Order Items in Top Ten	0.475	0.325	0	1	0.39	0.48	30.54
N		160168			10693	104804	

Summary statistics from the full dataset of orders, excluding beverages. 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 “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 “Popularity – Order Items in Top Ten” is the proportion of items within an order that are among the store’s top ten most ordered items.

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. 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. Furthermore, the percentage of total sales generated by the bottom 80% of products provides an alternative measure of concentration. For pre-Web orders, the share is 32.2%, for non-Web orders in the post-Web period it is 32.3%, and for Web orders it 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 compares to 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.

Table 6: 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%.

To establish that the difference in sales concentration across channels is statistically significant, consider a regression similar to Equation (1) where the dependent variable is a Herfindahl index for the channel in a given month and "Web Orders" is an indicator variable equal to one for online sales. Table 6 presents the results, and all specifications show that online sales are considerably 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.

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 (Brynjolfsson et al. 2003). As an alternative explanation, we next consider the role of social transactions costs.

3.3 Online Orders and Potentially Embarrassing Items

As we did for alcohol sales in Section 2, we now consider whether the impersonal nature of online transactions can partly explain why online orders have a less-concentrated sales distribution. Specifically, we show that consumers placing orders through the store's website make choices that might cause at least mild embarrassment if they required personal interaction. Following an extensive literature in social psychology that has shown individuals experience embarrassment when others observe them eating excessively or unconventionally, we examine two attributes that consumers may wish to keep private if they make extreme choices: calories and complexity. For example, Polivy et al. (1986) conduct an experiment to measure the effect observers have on an individual's food consumption, finding that subjects eat less when they believe the experimenter will be aware of their food intake. At the extreme, studies of bulimia also find that binge eating occurs less frequently in the presence of others (Waters et al.

2001, Herman & Polivy 1996). Moreover, excessive complexity may cause embarrassment if customers fear appearing eccentric by ordering an unconventional combination of items, which relates to sociological and psychological theories of impression management (Goffman 1959, Banaji & Prentice 1994). To this point, Roth et al. (2001) provide experimental evidence that subjects adhere to norms for "appropriate" eating behavior around others. 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 series 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} represents 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 customer-level fixed effect.

Table 7 presents the results from 11 different linear regressions based on Equation (2) that use various dependent variables; furthermore, we restrict the sample to those 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. We cluster all standard errors by household.

Table 7: Regression results of potential embarrassment and online orders.

	All Orders							Coupon Orders			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	Complexity Mean Item	Complexity Max Item	Calories Mean Item	Calories Max Item	Complexity Half Topping	Complexity Double Topping	Complexity Triple 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***	0.00468	0.415***	0.462***	117.95***	148.25***
	(0.0466)	(0.0515)	(21.24)	(23.296)	(0.0148)	(0.00812)	(0.0037)	(0.0679)	(0.0689)	(28.61)	(34.52)
N	48446	48446	48446	48446	48446	48446	48446	25590	25590	25590	25590
Number of Groups	2030	2030	2030	2030	2030	2030	2030	1993	1993	1993	1993
R^2	0.378	0.383	0.334	0.353	0.306	0.231	0.347	0.395	0.402	0.333	0.368

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) - (7) 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 (8) - (11) are restricted further to those customers who used a coupon for their order.

In the first set of regressions, we find that consumer 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, Column (2) presents the results from a regression using the maximum complexity of the order's base items as the dependent variable; here, online orders are 14.2% more complex under this definition.

A customer may also experience embarrassment if others observe him making an order with excessive calories (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 make choices with less potential for embarrassment when a transaction requires more social interaction. To conclude that these findings stem from a social friction related to embarrassment rather than some other characteristic, we next show that several alternative theories unrelated to embarrassment do not explain these differences among online orders.

3.4 Alternative Explanations Unrelated to Social Frictions

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 consumers' fear of embarrassment explains our results.

Information About Available Items One potential explanation for the long tail of online orders is that customers without access to a menu may be more likely to order prominent items. That is, without information about the full menu of products, a customer may simply order a large pepperoni pizza because he recalls that item more readily, not because ordering complicated items causes embarrassment. And because online customers necessarily have access to the full menu, this may lead to a long-tail sales distribution as they become more aware of less-prominent items. Several pieces of supporting evidence help rule out this explanation.

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. As such, information about available items is unlikely to generate the substantial changes in behavior we observe for online orders.

Second, consider the results from the regression of topping size on online ordering presented in Columns (5)–(7). Here, the dependent variable is equal to one if the order has a customized topping instruction of a half, double, or triple portion, respectively. In this case, any customer who knows 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 (8)–(11) which present results from a sample restricted to those 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. Again, all results are robust to this more conservative sample restriction.

Fourth, consumers with better access to nutritional information may consume fewer calories, as shown by 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.

Ease-of-Use and Order Accuracy Another potential explanation for the long tail of online orders is that complex and calorie-dense 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 does not explain our results for two primary reasons. First, an ease-of-use explanation also would apply to the number of base items within an order — because customers are unlikely to be more embarrassed to order two pizzas as opposed to one, the alleged mechanics of the website that would facilitate customized topping instructions also would facilitate ordering more base items. Recall from Table 5, however, that the average online order actually contains slightly fewer base items. Second, the store's employees have greater facility with the ordering system than any customer could possibly have online; 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.

Related to the ease-of-use explanation, consumers may avoid making complex orders over the phone to reduce the potential for misinterpretation. While in the alcohol setting we could not distinguish between actual embarrassment and a customer's desire to avoid misinterpretation as explanations for why the self-service format affected sales of difficult-to-pronounce items, three institutional details in the pizza setting suggest that embarrassment, and not a fear of miscommunication, best explains customers' choices.[9]

First, recall from Table 7 that customers order double and triple portions of toppings more often online. Although 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, 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 may not understand a customer's instructions and mistakenly de-

[9]Regression results in this section are presented in Appendix Table 8.

liver the wrong items. For this point, we have a (somewhat noisy) measure of mistakes: "voided" items that occur when an order changes during the call, either because the employee makes a mistake or because the customer alters his order. 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.

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 embarrassment (e.g., teenagers have different preferences than adults). While we control for this confound directly by using individual-level fixed effects and conservative sample restrictions to reduce concerns of selection bias, we also provide further evidence that selection bias does not undermine our results in the supplemental appendix. Notably, customers who eventually order online and those who do not make similar choices during the pre-Web period.

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 difference in sales distributions between the online and offline channels. Next, we estimate the welfare effects that stem from such social transaction costs.

3.5 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 one, some consumers may simply find ordering over the Internet more convenient. Moreover, the lack of social interaction may free consumers 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.

In the model, let consumer i choose among k discrete complexity options and m methods of ordering for each of his orders, o. 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. 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

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

where $Price_{ikmo}$ is the price associated with an order of mean complexity k; $Complex_{ikmo} \in \{0, \ldots, 6\}$ is the mean complexity of the order's base items associated with k ($Complex = 0$ is the outside option of no purchase), while β_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 β_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; $Embarrass_{ikmo}$ 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 $k \in \{4, 5, 6\}$ — β_i^e then represents the social cost of making a complex, potentially embarrassing order in the presence of others;[10] and ε_{ikmo} is an unobserved error term that is identically and independently distributed extreme value and independent of $\{Price_{ikmo}, Complex_{ikmo}, Web_{ikmo}, Embarrass_{ikmo}\}$ and β_i. Finally, the outside option of not ordering has a utility normalized to zero. Estimation follows Train (2003).

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.

The results from a random coefficients logit appear in Column (3) of Table 8. The coefficients suggest that the mean "cost" of using the website increases to an implicit price of nearly \$8.90, with considerable heterogeneity around this mean. 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 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 due to their complexity, social interaction has an average implicit price of \$2.75, while those customers two standard deviations above the mean have a price equivalent to \$5.92. Characterizing embarrassment based on excessive calories yields qualitatively similar results.[11]

A full covariance matrix also was estimated for the parameters in the random coefficient logit, as shown at the bottom of Table 8. Our measure of the social costs related

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

[11] We do not report the model estimates using calories as the mechanism for embarrassment due to space constraints; they are available from the authors upon request.

Table 8: 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 Embarrass	-0.667***	-0.751***	-1.595***
	(0.0225)	(0.0187)	(0.164)
Std. Dev. Embarrass			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	Embarrass
Price	0.1524			
Web	0.2464	10.16		
Complex	-0.4085	-1.0954	1.1728	
Embarrass	0.7318	3.1945	-2.3106	6.7167

This table presents the estimated coefficients from the discrete choice model in Equation (3). "Embarrassment" 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.

to potentially embarrassing purchases 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 β 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 θ 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% due to online ordering as consumers avoid embarrassment 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.[12] In this sense, freeing consumers to choose their most preferred item configuration without the potential for embarrassment increases utility by an amount similar to having access to a greater selection of products over the Internet.

Producer Surplus Because an item's price is non-decreasing in its complexity, the store stands to gain by allowing customers to make impersonal Web orders. 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. 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).

[12] 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.

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 the competitive environment restricts us to providing only a short-run approximation of the incremental profits the store earns each year from online orders.

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 ordering 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 related to social interaction have a substantial impact on welfare in this setting. For consumer surplus, the gain resembles prior estimates of the impact from online stores' larger selection of products. For producer surplus, the increase, while modest, nevertheless rationalizes the firm's decision to implement online ordering.

3.6 Social Transaction Costs and the Long Tail Phenomenon

As shown in Table 6, the store's online orders have a significantly less-concentrated sales distribution than its phone and counter orders. Previous theories for why a long tail characterizes online sales — namely, greater product selection and better search capabilities — are unlikely to apply to pizza orders, however, as the menu remains constant and consumers have similar opportunities to search across channels. Instead, the distinguishing feature of online pizza orders is that they require less social interaction. As such, we directly explore the impact of reducing social frictions, and thereby facilitating embarrassing orders, by considering a series of regressions that use the same controls and restrictions as Equation (2); the results appear in Table 9.

In Column (1), we define an "embarrassing" order to have an average complexity of six or greater, which would place the order approximately among the top 2% most complex orders. In this regard, online orders are more than twice as likely to be embarrassing. Similarly, in Column (2) we define an "embarrassing" order to have an average calorie count of 2970 or greater, which would again place the order approximately among the top 2% in terms of average calories; as with complexity, online orders are also twice as likely to be embarrassing in terms of excessive calories. Column (3) then consider an "embarrassing" order to have either excessive complexity or calories, as defined before, and the effect of ordering on the Web remains the same.

Column (4) next links online orders to the long tail. In this specification, those who order online have 7.8 percentage points fewer items among the top ten most popular, as would be expected given the results from Table 6. Columns (5)-(7) then directly show how embarrassing orders impact the sales distribution: for all specifications, those who make extreme orders in terms of complexity or calories purchase fewer items among the store's most popular. As online orders are more likely to be embarrassing, and embarrassing orders are more likely to be less concentrated, the long-tail effect thus prevails among the store's online orders.

4 Conclusions

We have documented that, in two different retail settings, social interactions have a substantial effect on the types of products purchased by consumers. First, using data

Table 9: Regression results of embarrassing orders and the long tail.

	(1) Embarrass–Complex	(2) Embarrass–Calories	(3) Embarrass	(4) Top Ten Items	(5) Top Ten Items	(6) Top Ten items	(7) Top Ten Items
Web Order	0.0374***	0.0205***	0.0556***	-0.0781***			
	(0.00768)	(0.00663)	(0.0101)	(0.0103)			
Embarrass–Complex					-0.0684***		
					(0.00947)		
Embarrass–Calories						-0.0773***	
						(0.0129)	
Embarrass							-0.0802***
							(0.00832)
Observations	48446	48446	48446	48446	48446	48446	48446
R^2	0.190	0.197	0.200	0.408	0.407	0.407	0.408

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. All regressions 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. "Embarrassing" orders are defined to be in the top 5% in terms of complexity or calories. The variable "Top Ten Items" is the proportion of items within an order that are among the store's top ten most ordered items.

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 unusual, high-calorie, and complex items at a pizza delivery restaurant, which 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.

Together, these results suggest that personal interactions may inhibit certain kinds of economic activity, likely because customers wish to avoid the potential for embarrassment. While a prior literature in psychology, sociology, and medicine has documented that individuals behave differently in settings related to health and sexuality depending on whether they involve personal interactions, our results suggest that the phenomenon is broader and applies even to relatively mild causes of embarrassment, such as mispronouncing the name of a product or making a complex pizza order.

We hasten to note, however, that our empirical settings have certain limitations that prevent us from definitively concluding that a fear of embarrassment fully explains our results. 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 customers changes in behavior to the change in sales format, 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, while we have attempted to eliminate other possible interpretations for our results, we have simply documented that requiring less social interaction to complete a purchase has a demonstrable effect on sales patterns; we cannot definitively conclude that this change is due to embarrassment. Instead, we argue that some theories are unlikely to explain our results in the alcohol setting (such as competitors' responses and consumers' selection into the stores because the stores are geographically isolated monopolies), and some theories are unlikely to explain our results in the pizza setting (such as consumers' desire to reduce the misunderstanding of instructions).

Despite these limitations, documenting such effects in two settings with different strengths and weaknesses provides robust evidence that social interactions influence consumers. In so doing, our results provide a new explanation for the prevalence of long-tail sales distributions in online markets: impersonal transactions lead consumers to purchase a different mix of products than they would in settings where social interactions might lead to feelings of embarrassment. Our results are also 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, 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 examines 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 proportion of transactions move online, the prevalence of what was previously embarrassing economic activity will continue to increase.

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