Demand Externalities from Co-Location:
Evidence from a Natural Experiment

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Abstract

Demand externalities arise from co-locating different types of stores in a mall or shopping district or within a larger retail store. Empirical researchers typically infer externalities through supply decisions such as entry and rental rates, under the assumption that managers have perfect information about these externalities. In contrast, we directly measure demand externalities from co-location by leveraging detailed household level changes in spending in response to a natural experiment, i.e., changes in grocery spend after a supermarket opens a gas station. We estimate the externality by comparing the “difference” between gas buyers and non-gas buyers in their “difference” in grocery spending before and after the gas station opened. Demand externalities are moderated by pre-existing loyalty to the store: the least loyal households drive the greatest relative increase in spending. Further, we find that the advent of the gas station alters the competitive market position of the supermarket, not only against competing supermarkets, but also against convenience stores. We control for selection on observable and unobservable time-varying differences between gas buyers and non-gas buyers to provide tight bounds on demand externalities from co-location. Even controlling for such selection effects, the demand externality from the gas station on grocery purchase is very significant, ranging from 7.7% to 9.3% increase in spend on groceries. Interestingly, this analysis further suggests that the profit from additional sales of groceries (demand externality) can be as high as 130% to 150% of the profit from gasoline sales.

Key Words: Revenue economies of scope, demand externalities, one stop shopping, selection, retail industry.
1. Introduction

The measurement of demand externalities arising from spatial co-location is a central problem in economics and marketing, of interest to both firm managers and policy makers. To a retail firm, it is a question of the right level of scope. Supermarkets have recently expanded to include many completely unrelated categories such as banking services, pharmacies, DVD rentals, coffee shops and gas stations. With deregulation of the financial services, retail banks have expanded their traditional banking services to include insurance and financial planning in their offerings, while expanding the range of credit and debit services. Given the likely diseconomies of scope due to the increased costs entailed in provision of additional services (Christofferson et al. 2004; Cummins et al. 2010), do the positive demand externalities make spatial co-location worthwhile? Mall developers, for instance, routinely account for inter-store demand externalities in evaluating store mix in malls when setting rents and recruiting retailers. In many cases, anchor stores serve to create foot traffic that provides positive demand externalities for smaller stores and hence they are offered rental discounts (Benjamin et al. 1992; Gould et al. 2005).

Demand externalities also have implications for city planners and policy makers. In recent years, faced with dilapidated downtowns, many local governments and civic organizations have begun to encourage big-box store openings in downtown areas (Philips 2010). Gould et al. (2005) speculate that one potentially important reason for the decline of the central business districts across the United States was their inability to price the externalities of anchor stores, leading to the flight of large anchor stores from downtown areas to malls.

Despite its significance, there is little work on the empirical measurement of demand externalities from spatial co-location. The limited research typically uses indirect approaches to infer the magnitude of demand externalities through the supply side choices of firms such as the decision about whether (and where) to enter (Vitorino 2008; Datta and Sudhir 2011) or rental prices (Gould et al. 2005). The inference is based on the maintained assumption that firms have knowledge of the magnitude of externalities and make optimal decisions given this information.
However, firms may not necessarily have accurate knowledge of the level of externalities. Recently, Stop & Shop, a northeastern supermarket, shut down Dunkin Donut coffee shops at its stores, presumably due to limited demand externalities relative to the opportunity cost of space.\footnote{Bailey (2009) documents the case of a regional bank that decides to close branches located within grocery stores. Fein (2009) discusses the lack of revenue synergies for the drugstore CVS from the acquisition of a pharmacy benefit manager.} In the banking sector, the conventional wisdom is that few revenue synergies have been realized through one stop shopping. Citigroup’s lack of success in becoming a one stop financial supermarket for consumers is widely considered Exhibit A of the challenges associated with exploiting demand externalities in banking (Gaffen 2010). Academic research on the banking and insurance industries has also failed to find evidence of these desirable demand externalities (Berger et al. 1996, 2000; Cummins et al. 2010). In general, there is consensus that managers tend to be overly optimistic about realizing revenue synergies across product categories, often because these synergy goals are predicated on taking significant share from competitors through cross-selling opportunities, which ultimately fail to materialize: a recent McKinsey study finds that 70 percent of mergers in their data failed to achieve expected revenue synergies (Frieswick 2005). Moreover, the Economist comments sarcastically on the beliefs of European bank managers in offering one-stop shopping: “...activities that are chronically unprofitable are carried on [by European banks] in the belief that they help a bank to keep its customers” (Klemperer and Padilla quoting *Economist*, 1993).

Accurate estimates of demand externalities can have significant implications for managers’ decisions. Indirect measurements can only provide net average estimates of the externality, but yield little insight on the structure and source of the externalities. For example in the context of malls, it is clear from the rental subsidies for anchor stores that they serve to create demand for the smaller stores. However, we have little idea as to whether and how much smaller businesses may help an anchor store increase sales. Such measurements can be valuable to mall-developers in encouraging the right kind of anchor stores to enter the market given the mix of smaller businesses

\footnote{Bailey (2009) documents the case of a regional bank that decides to close branches located within grocery stores. Fein (2009) discusses the lack of revenue synergies for the drugstore CVS from the acquisition of a pharmacy benefit manager.}
in the mall. In contrast, using our direct measurement approach, we are able to answer a similar question: does the secondary small gas business helps expand the primary business of groceries, and if yes, by how much? The direct measurement approach can also help describe which segments of customers generate the externality and from which competitors the externalities are derived. Thus, direct measurements can (i) not only help managers and firms obtain better insights for making scope (or retail format) decisions, (ii) but also obtain greater insights on the changes in competitive market structure.

In this article, we design our analysis around a natural experiment, in which a supermarket opens a gas station on its parking lot, to illustrate how one can directly measure demand externalities through co-location. The phenomenon of grocery stores and co-located gas stations has become relatively widespread (Goic et al. 2010), but little is known empirically about the magnitude of externalities from opening a gas station. Using detailed household level grocery expenditures and trip information before and after the introduction of the gas station, we directly measure the demand externality on grocery spending due to the introduction of the gas station.²

Our basic empirical strategy is based on a “difference-in-difference” approach. We compare the within-household change in grocery spend and trips “before and after” the introduction of the gas station (the first difference in time dimension) between households that buy gas at the new gas station relative to those who do not (the second difference in consumer segments). This provides us an average estimate of demand externalities from co-locating the gas station at the grocery store. We then seek a deeper understanding of the mechanism underlying these demand externalities by understanding (1) the types of customers who respond to the externalities and (2) the types of competitors from whom the additional revenues are drawn. Specifically, we study the moderating effect of customer loyalty to the supermarket on the extent of grocery spillovers. This helps us address questions such as: Does the expansion lead to greater grocery sales through new customers or cement the loyalty of existing customers? This is critical information because the costs of

² There has been some recent work on estimating cross-elasticities between gasoline and groceries (for example, Gicheva et al. 2010; Ma et al. 2011).
increasing the loyalty of low-loyalty customers (or equivalently, the costs of acquiring new customers) and the cost of cementing loyalty of existing customers (the costs of retaining customers) are different. Further, to understand the competitive impact of the gas station, we segment consumer spending by the product categories purchased. This analysis sheds light on whether spillovers come at the expense of other supermarkets (intra-format effects) or from convenience stores (inter-format effects).

For our analysis, it is important to control for potential selection effects, which present a concern because households self-select into buying (or not buying) gas. Specifically, we are concerned with time-varying effects, since unobserved effects that do not vary over time are already accounted for through the difference-in-difference approach. How might such a time-varying differences in spending between gas and non-gas buyers actually arise? To take one example, the retailer might have improved some other dimensions of the store (such as product selection) over the same time period that the gas station was opened. In this case, it is possible that these other effects might have increased grocery purchases of households that already buy more often at the store. If such households are also more likely to buy gas, then the estimate of demand externality through the difference-in-difference approach may be biased.

To assess the robustness of our results to such potential selection effects, we perform a variety of robustness checks to deal with observed and unobserved selection. We control for potential selection on observables using propensity-score matching (DiPrete and Gangl 2004, Leuven and Sianesi 2003). This nonparametric approach effectively matches households on observable variables between the “treatment – gas buyer” and the “control – non gas buyer” groups, in order to more accurately estimate the effect of the treatment.

In addition, there could still be “hidden bias” due to selection on unobservables. We address selection on unobservables using two approaches. First, we use techniques from the observational experiment literature to measure the extent of unobserved selection effects that would completely nullify the measured demand externalities by estimating Rosenbaum bounds (Rosenbaum 2002, Leuven and Sianesi 2003). However, this approach does not provide a point estimate of the
externality. Therefore, we adapt an approach developed by Altonji, Elder and Taber (2002, 2005) that can provide a point-estimate of the externality when selection on unobservables can be as high as selection on observables. We offer the estimates that controls for selection on observables (unobservables) as the upper (lower) bound of the size of co-location externality.

Our key findings are as follows: First, we find clear evidence of demand externalities for groceries due to the opening of the gas station. For households that use the new gas station, grocery revenue increases by 7.7-9.3% and number of trips increases by 14-15% during the six week period after the gas station opened, relative to the prior six weeks. This magnitude of externalities in grocery purchase volume is surprisingly large, given that grocery consumption tends to be relatively stable across time, suggesting that there are large business-stealing effects due to co-location. In terms of revenues, grocery spillovers contribute about 18-22% of revenues from the direct sales of gas. But given the substantially larger gross margins on groceries, relative to gas, the gross profits associated with grocery spillovers are 130-150% of gross profits from gas sales. Thus, the grocery externalities make the overall economics of investing in a gas station for a supermarket chain far more favorable.

Second, we identify the source of demand externality from co-location. These revenue economies of scope arise primarily through increased trips to the store, which lends support to a travel cost based explanation for revenue economies of scope (Bell et al. 1998, Gauri et al. 2008, Wernerfelt 1994). Moreover, the households with low and intermediate loyalty show the greatest response to the increased scope of the firm. Finally, through a product-type analysis (convenience store products versus traditional supermarket products), we identify the likely competitor formats from which additional revenue is drawn; we find that revenue economies of scope come at the expense of both traditional grocery competitors as well as convenience stores.

In summary, we believe that this paper contributes in several dimensions. First, we contribute to the literature on the economies of scope by introducing a direct measurement approach to quantify and identify the source of demand externalities. Second, we gauge the effect of customer loyalty on the demand externalities and show that the introduction of the secondary service may
have a large impact on the profits from the primary product, particularly among lighter users of the primary product. Methodologically, we introduce to the marketing literature two approaches for addressing the impact of unobserved selection effects.

The structure of this article is as follows: Section 2 describes the data and Section 3 presents the main results on the extent of demand externalities from increase scope. In section 4, we check for the robustness of our results to observable and unobservable selection effects. We quantify the economic significance of our results in Section 5, and conclude in Section 6.

2. Data

We take advantage of a natural experiment when the supermarket opened a gas station in its parking lot in September, 2005. To perform pre and post analysis, we obtained data from June, 2005 and up to June, 2006. The data are at the level of household spending in each product category on each transaction. Purchase amounts are categorized into 593 categories; this allows us to perform analysis of how spillovers differ across product categories.

Demand externalities are likely moderated by the level of loyalty to the chain. To proxy for store loyalty, we use the retailer’s own internal estimates of household “share of wallet (SOW)” which they shared with us. The retailer estimates SOW by comparing actual household spending at the chain to that household’s potential grocery spending. Potential grocery spending is estimated using a proprietary algorithm and household demographic information from a third party firm. Note that the metric was constructed prior to the time-period of our data; therefore it is not influenced by the changes in spending induced by the gas station.

Descriptive Statistics

We summarize the average grocery and gasoline purchase behavior of households at the focal store in Table 1. To identify the changes in household purchasing behavior due to the opening of the gas station, we focus our analysis on a twelve-week window around the opening of the gas station: six weeks before (pre-period) and six weeks after (post-period).
Overall, households spend an average of $41 per week on groceries, and this stays stable across households between pre- and post-periods. Among the households that do not purchase gas in the six-week post-period, we find a slight decrease in grocery sales with average weekly sales in the post-periods is 6% lower than the pre-periods (p < 0.01). The increase in grocery spending is almost entirely among households that purchase gas with 7% increase in average grocery spending (p < 0.01). We also note that households who purchase gas tend to a priori spend more at the store (with average weekly spending for grocery in the pre-periods being $50.78 for gas-buyers and $35.04 for non-buyers, respectively). Thus, there are clear “intrinsic” differences between gas buyers and non-buyers. Households that purchased gas in the post-period spent on average $13.80 for gasoline per week.

In terms of trips, there is no statistically significant difference in the number of trips between pre- and post-periods, with overall household trips to the store at 0.86 and 0.85 trips per week in the pre- and the post-periods, respectively. But there is a significant difference between the gas-buyers and non-buyers. For households who did not buy gas, there was a decrease in trips between the pre- and post-periods from 0.73 to 0.66 (p < 0.01). Among households that bought gas, average trip per week indeed increases from 1.07 to 1.13 (p < 0.01).

Even though there are no consumption complementarities between the grocery products and gasoline, the increase in grocery spending and trips among households that bought gas provides prima-facie evidence for the demand externalities created by the gas station.

3. Analysis and results

We use a difference-in-difference approach to estimate demand externalities from co-location. The first difference is a temporal “before-after” comparison based on differences in grocery purchase behavior before and after the gas station was opened. The second difference is cross-sectional between households who buy gas versus those who do not after the gas station is opened.
Our analysis consists of three parts. First, we demonstrate the existence of demand externalities from co-location. We achieve this by quantifying how the introduction of the gas station impacts grocery spending and grocery trips. Second we investigate how these externalities vary by household loyalty. This helps us determine whether the gas station expands grocery spending by consolidating demand from existing loyal customers or by increasing spending among the store’s infrequent (i.e. low-loyalty) customers. Essentially, this allows us to answer whether the secondary service serves as a customer acquisition or retention device. We then address the impact of the gas station on the competitive market structure for groceries by studying the source of increase in grocery spend at the product category level, distinguishing between convenience store categories as opposed to traditional super market categories. Having established our main findings, in the next section (section 4), we evaluate the robustness of our primary results to selection effects.

3.1 The Effect of Gas Station Introduction on Grocery Spend and Trips

Impact of Gas Station Co-location on Grocery Spend

Let the grocery spend of household $i$ at the focal store before and after the gas station opening be given by $\text{Gro}_{i}^{pre}$ and $\text{Gro}_{i}^{post}$. Denote the difference in grocery spending of household $i$ before and after the introduction of gas station, as $\Delta \text{Gro}_{i} = \text{Gro}_{i}^{post} - \text{Gro}_{i}^{pre}$. To estimate the difference in spending between households that purchase gas versus those that do not, we estimate the following model

$$
\Delta \text{Gro}_{i} = \text{Gro}_{i}^{post} - \text{Gro}_{i}^{pre} = \alpha_{0} + \beta \cdot 1\{\text{Gas} \_ \text{Buy}\}_{i} + \varepsilon_{i},
$$

(1)
where $1\{\text{Gas\_Buy}\}_i$ is an indicator variable for whether household $i$ purchases gas in the post-period, and $\varepsilon_i$ is the household $i$ specific error. The coefficient $\beta$ captures the difference in difference and is the parameter of interest. The estimates\(^3\) are reported in Table 2, Column 1.

*** Table 2 ***

On average, gas-buyers spend $5.78 more on groceries per week, relative to non-gas buyers after introduction of the gas station. This difference is statistically significant at the 1 percent level, providing evidence of demand externalities on grocery spend from the co-location of the gas station.

Since households differ substantially in their pre-period purchases, and the gas station is likely to have a proportionate impact on household grocery spend, we next estimate the percentage change in spend per household after the introduction of the gas station by estimating the following specification:\(^4\)

$$\Delta\%\text{Groc}_i = \frac{\text{Groc}^{post}_i - \text{Groc}^{pre}_i}{\text{Groc}^{pre}_i} = \alpha_0 + \beta \cdot 1 \text{ Gas\_Buy}_i + \varepsilon_i. \quad (2)$$

Column 2 of Table 2 reports the results. We find that gas buyers spend 13% more on groceries relative to non-gas buyers after the gas station is introduced. This effect is consistent

\(^3\) The metric of grocery spending in this analysis and for the rest of the paper is gross sales – i.e. revenues before accounting for discounts on specific grocery products. For robustness, we conducted this analysis using net sales (i.e. including the effect of grocery product-specific discounts); the results do not change.

\(^4\) Normally, one would use $\log(\text{Groc}^{post}) - \log(\text{Groc}^{pre})$ to estimate percentage differences. However, this is not feasible in our setting because for a number of households, $\text{Groc}^{pre}$ is zero. When using the dependent variable, $\Delta\%\text{Groc}_i = \frac{\text{Groc}^{post}_i - \text{Groc}^{pre}_i}{\text{Groc}^{pre}_i}$, to be consistent with the aggregate percentage change in grocery spend, we weighted each observation by $\text{Groc}^{pre}_i$. In effect, we weigh changes from large spend households more than the changes from small spend households.
with the descriptive statistics reported in Table 1, and reflects significant demand externalities on
grocery spend from co-locating the gas station.

Impact of Gas Station Co-location on Grocery Trips

Co-locating the gas station may cause increase in grocery spend for two reasons. First, trips
that might previously have been made to gas stations, along with incidental spending at gas-
station convenience stores, are now made to the focal grocery store. Second, after the introduction
of the gas station, households may shift their regular grocery trips away from other grocery stores
that do not sell gas. Instead, these trips are made to the focal supermarket, thereby allowing
customers to avoid the additional trip required for gasoline. Either of these hypotheses requires
that the number of trips to the focal grocery store should increase after the introduction of a gas
station.

Let the number of trips of household \( i \) before and after the gas station opening be given by
\( \text{Trip}_{i}^{pre} \) and \( \text{Trip}_{i}^{post} \). Denote the difference in grocery trips of household \( i \) before and after the
introduction of gas station, as \( \Delta \text{Trip}_{i} = \text{Trip}_{i}^{post} - \text{Trip}_{i}^{pre} \). Then to estimate the difference in trips
between households that purchase gas versus those that do not, we estimate the analogous model
for changes in number of trips and percentage change in number of trips below:

\[
\Delta \text{Trip}_{i} = \text{Trip}_{i}^{post} - \text{Trip}_{i}^{pre} = \alpha_{0} + \beta \cdot 1 \{ \text{Gas} \_ \_ \text{Buy} \}_i + \varepsilon_i,
\]

\[
\Delta \% \text{Trip}_{i} = \frac{\text{Trip}_{i}^{post} - \text{Trip}_{i}^{pre}}{\text{Trip}_{i}^{pre}} = \alpha_{0} + \beta \cdot 1 \ \text{Gas} \_ \_ \text{Buy}_i + \varepsilon_i.
\]

*** Table 3 ***

The results are reported in Table 3. The number of trips increases by 0.13 trips per week
among households using the gas station, relative to those who do not; in percentage terms, trips
increase by 15%. These differences are statistically significant at the 1 percent level. From these
results, we conclude that co-location of the groceries and gasoline leads to consolidation of trips.
3.2 The Moderating Effect of Pre-Existing Loyalty on Scope Externalities

How pre-existing loyalty moderates the demand externalities effects of the increased scope is an interesting and unresolved empirical question. The more loyal the customer, the more likely it seems that such a customer might also patronize a new product offering. Hence, one might expect that the most loyal customers are those who would be more likely to respond to the new product offering and perhaps even increase their grocery spending through increased visits. On the other hand, those who are already loyal (i.e. high share of wallet) customers may simply not have further room to increase their grocery spending. In that case, it might be the moderate- or low-loyalty customers who respond most favorably to increased scope and increase their purchases on the firm’s original offering.

In light of these contradictory hypotheses, it seems naive to assume that the average increases of 13% and 15% in grocery spend and trips, respectively, are uniform across all households. Therefore we seek to empirically answer the question: Does the gas station consolidate loyalty among the customers who are already most loyal to the store? Or does the introduction of the gas station lead to increased spending among moderate- or low-loyalty customers?

As a first step to see the relationship between the store loyalty and demand externality effects, we plot a bar chart of average absolute dollar changes (difference) in grocery spending between before and after the opening of gas station ($\Delta \text{Gro}c_\acute{c} = \text{Gro}c_{\acute{c}}^{\text{post}} - \text{Gro}c_{\acute{c}}^{\text{pre}}$) for different levels of household loyalty (Figure 1-(a)).

*** Figure 1 ***

The black bars on the figure show the difference-in-difference (difference of $\Delta \text{Gro}c_\acute{c}$ between gas-buyer and non-gas buyer). Figure 1-(a) clearly shows that (1) gas buyers drive consistently higher levels of incremental grocery spending relative to non-gas buyers (a comparison of means shows that differences at all levels of SOW are significant at <0.01) and (2) the moderate-level
loyalty customers shows the largest increase in grocery spending (households with 41-60% SOW drive a $7.19 increase).

On a relative basis, this difference-in-difference is greatest for low SOW households, given that they spend fewer total dollars in the pre-period. Figure 1-(b) shows the moderating effect of share of wallet on spending in relative terms. Here, we plot a bar chart of average relative changes (\% difference) in grocery spending before and after the opening of gas station (\(\Delta \% Groc_i = (Groc_i^{post} - Groc_i^{pre}) / Groc_i^{pre}\)) by household loyalty level. It confirms that the relative difference-in-difference (difference of \(\Delta \% Groc\) between gas-buyer and non-gas buyer) monotonically decreases in customer loyalty; the relative change is largest at the lowest loyalty level and the smallest at the highest loyalty level. Thus households with previously low loyalty levels are seen to drive the largest relative response in increased grocery spending due to the opening of the gas station.

To obtain estimates of the moderating effect of customer loyalty (on grocery sales and number of trips) we estimate the following two regressions based on percentage changes as before. In these regressions, we explicitly include share of wallet (%SOW) and its interaction with the consumer’s gas purchase (1{\text{Gas\_Buy}} \cdot \%SOW) as covariates (Table 4 reports the results).

\[
\Delta \% Groc_i = \alpha_0 + \beta_1 \cdot 1 \text{ Gas\_Buy}_i + \beta_2 \cdot \%SOW + \beta_3 \cdot 1 \text{ Gas\_Buy}_i \times \%SOW + \epsilon_i, \\
\Delta \% Trip_i = \alpha_0 + \beta_1 \cdot 1 \text{ Gas\_Buy}_i + \beta_2 \cdot \%SOW + \beta_3 \cdot 1 \text{ Gas\_Buy}_i \times \%SOW + \epsilon_i.
\]

*** Table 4 ***

We find evidence of the moderating effect of share of wallet on the incremental effect on percentage difference of grocery spending and trips. In general, customers’ grocery spending is lower during the post-periods (column 1 of Table 4, \(\alpha_0 = -0.19\)). However, we find the percentage difference in grocery spending for gas-buyers across the entire range of loyalty, is positive (\(\beta_1 = 0.26\)), suggesting that an average of 7% (= 0.26 - 0.19) increase in grocery spending after the opening of the gas station. This percentage difference monotonically increases in customer’s share
of wallet across all customers ($\beta_2 = 0.21$), but the effect reduces significantly for gas-buyers ($\beta_3 = -0.20$). That is, while both groups of customers have a change in spending that increases monotonically over the range of SOW, the trend for gas-buyers is almost flat (the slope of the line is $0.01 = 0.21 - 0.20$) relative to non-buyers (the slope is 0.21). Hence, the difference (between gas buyers and non-gas buyers) in difference (between before and after) is monotonically decreasing in the share of wallet. The difference-in-difference in grocery spending is 26% for the lowest share of wallet customers and a 6% for the highest share of wallet customers. These general findings about the moderating effect of share of wallet on spending are well captured graphically in Figure 1-(b). A similar pattern can be observed for the percentage difference in trips (column 3 of Table 4). Trips also increase for gas-buyers, with a 22% increase for the lowest share of wallet customers and an 8% increase for the highest share of wallet customers.\footnote{Allowing for non-linear relationships with respect to loyalty (columns 2 and 4 of Table 4) reduces the significance of the interaction of gas-buying behavior and household loyalty; however, the main effect of having bought gas ($\beta_1$) remains significant and similar to the estimates without non-linear terms.}

Overall, we find that the relative change in grocery spending and trips due to the gas station is largest for consumers with low share of wallet and declines monotonically with share of wallet. In the present context, revenue economies of scope are driven by all levels of loyalty, with the biggest absolute response coming from moderately loyal customers. The largest relative responses are from low-loyalty customers. This suggests that the increased scope of the product offering (i.e., introduction of gas station) serves as an effective device for customer acquisition.

### 3.3 The Competitive Impact of Revenue Economies of Scope

We next investigate the sources of revenue economies of scope: specifically, where might spillover grocery revenues come from? Since customers are unlikely to systematically experience an increase in their household’s demand for groceries, we suspect that the increased grocery revenue must come from capturing customers’ business from competitors. Following this logic, we investigate the impact of increased scope on competition between retailers.
We do so by investigating how opening of the gas station affects the *types of products* each households purchase. For this, we exploit information about the types of products in each transaction. We distinguish between product categories that are most likely purchased at traditional grocery stores (“traditional grocery” products) versus those that could be purchased at both traditional grocery stores as well as gas-station convenience stores (“convenience” products). If incremental spending mostly occurs on traditional grocery products, this serves as evidence of increased intra-format competition vis-à-vis traditional grocery competitors. On the other hand, if incremental spending occurs mainly in convenience product categories, this provides support for increased inter-format competition.

For the purpose of this analysis, we classified 464 of the 593 categories in our data as “traditional” and the remaining 129 categories as “convenience.” For instance, cigarettes are a “convenience” category, while fresh vegetables are a “traditional” category. Because of the systematic difference in dollar amounts between the two categories, we focus only on the percentage change in spending within each of these two broad categories. Table 5 below reports the results.

*** Table 5 ***

Gas purchasers increase spending in both categories relative to non-gas buyers, with a 13% increase for traditional grocery products and a 15% increase for products in the grocery and convenience category (Table 5, Columns 1 and 2). Our product-level analysis yields evidence of intensified competition with respect to both traditional grocery competitors and convenience stores. Combining data from both product types shows a significantly larger effect for convenience

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6 We surveyed four grocery shoppers (who are primary shoppers in their households) to classify categories into two mutually exclusive groups: “convenience” products and “traditional grocery”. The classification across these respondents were very consistent (Cronbach’s α of 0.87). When there were occasional disagreements, we used the classification chosen by the majority of respondents.
products (Table 5, Column 3). Qualitatively, we therefore find evidence of both intra- and inter-
format competition, with the larger changes coming from inter-format competition.

4. Accounting for Time Varying Selection Effects

Thus far, we have quantified demand externalities by measuring how the opening of the gas
station impacts grocery spending among gas-buyers relative to non gas-buyers. Since “buying gas”
is not an exogenous treatment, and households self-select into this treatment condition, the
difference-in-difference approach controls for only time-invariant selection effects between the two
groups, i.e. if $\Delta_{control}$ were the same for both groups in Figure 2-a. But, as discussed in the
introduction, if changes to the store between the pre- and post-period of introducing the gas
station affect gas and non-gas buyers differentially, our estimated treatments will be biased due to
selection. In this section, we assess the robustness of our results to time-varying selection effects.
We first consider selection on observables and then selection on unobservables.

*** Figure 2 ***

4.1 Selection on Observables

We use the technique of propensity matching to control for selection on observables. The basic
idea is to ensure that the comparisons are made between households in the control and treatment
groups, who are as similar as possible on observable characteristics in terms of their propensity to
be in the treatment group (DiPrete and Gangl 2004, Leuven and Sianesi 2003). See Figure 2-b to
conceptually see how a propensity matched control household help eliminate potential differences
due to selection.

The summary statistics (Table 1) show that gas-buyers on average make more trips to the
store: non-buyers average 0.73 trips per week, while gas-buyers average 1.07 trips per week before
the gas station opened. Gas-buyers also buy more groceries per week relative to non-gas buyers –
$50.78 as opposed to $35.04 – even before the gas station opened. To the extent that we have used
percentage differences in spending before and after the gas station as the outcome variable,
selection effects may not be necessarily significant, because we already allow for proportional differences in spend and trips between the control and treatment groups. Nevertheless, it is still possible that the impact of the gas station may be different from the proportional impact. Propensity score matching allows us to test for such differences.

Define the propensity score to be the predicted probability of buying gas, based on parameters estimated from the following probit regression, using household spend and trips in the six-week period before the gas station opened as covariates:

\[
1\{\text{Gas Buy}_i\} = \lambda_0 + \lambda_1 \log(Grocery_{i,\text{pre}}) + \lambda_2 \text{Trips}_{i,\text{pre}} + \epsilon_i
\]  

As before, differenced outcome variables are used for all households, and each gas-buyer household is compared to non-gas-buyer households with similar propensity scores. As expected, the propensity matched estimate using grocery spending alone has a relatively small effect on the percentage change in spending; the percentage change drops from 13.3% to 12.0% (comparing the first and second rows in column 2 of Table 6). However, the percentage change in trips (row 2, column 4) drops by a larger amount from 14.7% to 10.6%. Similarly, when households are compared only on the basis of trips (row 3) alone, the estimate of percentage change in trips is similar to the un-scored estimate (14.7% vs. 15.3%) while the difference in the estimate of total spending is much larger (13.3% vs. 7.6%). Taken together, these results suggest that spend and trip information from the pre-period capture somewhat different information about the behavior of households, and that combining both pieces of information provides better comparability between gas-buyers and non-buyers.

*** Table 6 ***

When spend and trip information are both used to create a propensity score (row 4 of Table 6), we find that the percentage change in trips is almost the same as in the non-propensity-scored estimates (14.7% vs. 14.6%). However, the estimate of percentage change in spending is very different from the non-scored estimates: specifically, our estimate falls from 13.3% to 9.6.
Moreover, combining trip and spend information restores the estimated change in trips to close to the un-scored estimate, providing further evidence that spend increases are moderated by trips. Therefore, incorporating trip information in comparing gas-buyers and non-buyers yields a more accurate measure of the relative change in spending due to the gas station.

Just as the number of trips in the pre-period provided important information about whether a household chooses to buy gas, we should expect that the household’s share of wallet may also have an impact on our results. We therefore estimate a specification that includes the household’s share of wallet (row 7 of Table 6).\textsuperscript{7} The results show that gas-buyers increase their weekly grocery spend and trips by $6.85 and 0.16, respectively, relative to non-buyers. In relative terms, the percentage differences in grocery spend and trips are 9.3% and 14.1%, respectively. Thus even controlling for selection on observables, the demand externalities on grocery spend and trips are substantial.

4.2 Selection on Unobservables

We next address potential selection on unobservables. First, utilizing a non-parametric bounds approach (Rosenbaum bounds) developed in the observational experiment literature, we assess the magnitude of unobserved selection required to nullify the estimated demand externality from co-location (Rosenbaum 2002). Second, we adapt an approach developed by Altonji, Elder and Taber (2002, 2005) in the labor and econometrics literature to obtain a point-estimate that accounts for both observed and unobserved selection effects.

Rosenbaum bounds

Rosenbaum bounds are a method of assessing how large the unobserved selection effects need to be to overturn the qualitative results about the existence of a treatment effect based on

\textsuperscript{7} Since the metric of loyalty (SoW) is calculated using the observed dollar spending by each household, incorporating grocery spend in the pre-period and SoW directly would be incorrect and induce multicollinearity. To avoid this, we calculate an additional covariate, log(potential), which is the logarithm of spending in the pre-period divided by SoW. The new covariate, log(potential), and SoW can now be treated as uncorrelated.
propensity matching. If the Rosenbaum bound for unobserved selection, appears too large to be true in reality, a researcher may conclude that the qualitative results from propensity matching holds.

The natural starting point for setting up Rosenbaum bounds on unobservable selection is the propensity score. Denote the propensity to buy gas as \( \pi_k \) for gas-buyers \((k = 1, \ldots, K)\) and \( \pi_l \) for non-buyers \((l = 1, \ldots, L)\), where \( \pi_i = X_i \alpha + u_i \) for \( i \in \{k, l\} \). In this case \( X_i \) denotes a set of observed covariates for household \( i \) (as in the probit regression in equation (5)), while \( u_i \) denotes unobservable selection effects. In the absence of unobserved selection effects, \( u_i = 0 \) for all households; so we obtain an odds ratio of one, or\(^8\)

\[
\frac{\pi_k}{\pi_l} \frac{1 - \pi_l}{1 - \pi_k} = 1 \text{ for } X_k = X_l
\]  

(6)

However, if unobserved selection occurs, then the odds ratio is greater than 1, or equivalently, the propensity score for gas-buyers to buy gas is higher than the propensity for non-buyers, even when observed covariates are the same \((\frac{\pi_k}{1 - \pi_k} > \frac{\pi_l}{1 - \pi_l})\). Rosenbaum bounds place a constraint on the effects of unobserved selection to change the odds-ratio by some known amount gamma \((\gamma)\), such that

\[
\frac{1}{\gamma} \leq \frac{\pi_k}{\pi_l} \frac{1 - \pi_l}{1 - \pi_k} \leq \gamma \Rightarrow \frac{1}{1 + \gamma} \leq \frac{\pi_k}{\pi_k + u_i} \leq \frac{1}{1 + \gamma} \text{ for } X_k = X_l
\]  

(7)

\(^8\) In the case of randomized experiments, randomization allows us to assume that \( E(u_i \mid X_i) = E(u_j \mid X_j) \) thereby yielding the relationships shown in (6).
This parameterization allows us to test whether our findings (of a positive effect from choosing to buy gas) are robust to unobserved selection effects of size $\Gamma$, where the parameter is defined over $\Gamma \geq 1$.\(^9\)

Inference is based on properties of the Wilcoxon signed rank statistic ($T$), and its associated distribution. The Wilcoxon’s signed rank test is typically used for rejecting the null hypothesis that a response variable is equal between matched observations drawn from two groups. In the present context, if the null hypothesis were true, the differences in behavior for gas-buyers would be equal to the differences for non-buyers. The distribution of possible values for $T$ under the null hypothesis has a known expectation and variance.\(^10\) Moreover, for a sufficiently large number of matched observations, a standardization of the statistic is asymptotically normal. In other words, for $n$ paired observations, as $n \to \infty$, the standardization of the null distribution has the following property:

$$\frac{T - E(T)}{\sqrt{\text{var}(T)}} \xrightarrow{d} N(0,1).$$

This asymptotic result allows us to generate a p-value for $T$ from data under the null hypothesis.

The effect of $\Gamma$ is that the probability of being a gas-buyer is now different from the standard Wilcoxon null distributions, which assume that the probability of being a gas-buyer is 0.5, up to observable covariates. Instead, the ranking of each of the $K$ possible gas buyers (note: $k = 1, \ldots, K$) is now included with probability $\Gamma / (1 + \Gamma)$ (see Equation (7)) for the upper bound distribution. Denote this null distribution $\Omega_{\text{upper}}$. Similarly, the probability $1 / (1 + \Gamma)$ for the lower bound distribution yields another null distribution $\Omega_{\text{lower}}$. Given the value of the statistic $T_{\text{obs}}$, calculated from data, we can now assess the probability that this value of $T_{\text{obs}}$ was drawn from the null

\(^9\) Notice that the first expression in (7) reduces to (6) if $\Gamma = 1$. For $\Gamma > 1$, the odds ratio can now take on some range of values, bounded by functions of $\Gamma$.

\(^10\) Specifically, for $N$ paired observations, under the assumption that the null is true (no difference between matched pairs), the expectation is given by $E(T) = \tfrac{1}{2} \sum_{k=1}^{K} r_k$ and the variance by $\text{var}(T) = \tfrac{1}{4} \sum_{k=1}^{K} (s_k + \gamma)^2$, where $\gamma = 1$ if $r_k > 0$ and zero otherwise, $r_k$ is the ranking of the absolute value of the difference.
distribution of the lower- and upper- null distributions. A p-value can now be generated from
\( \Pr(T_{\text{obs}} \mid \Omega_{\text{upper}}(\Gamma)) \) and \( \Pr(T_{\text{obs}} \mid \Omega_{\text{lower}}(\Gamma)) \). As long as these p-values reject the null hypothesis that
the difference in behavior between gas-buyers and matched non-buyers is zero, we can claim that
our results are robust to unobserved selection effects of a given size \( \Gamma \).

### Table 7a ###

We first discuss the Rosenbaum bounds results for changes in grocery spend.\(^{11}\) Our results are
robust up to \( \Gamma \) of 1.7 (columns 2 and 3 of Table 7a), by which we mean that a one-sided p-value
of the observed data rejects the null hypothesis.\(^{12}\) We can interpret the effect of \( \Gamma \) more intuitively
if we normalize the relative selection effect. Assume that only gas buyers receive an additive
unobserved selection shock (i.e. \( u_i \neq 0 \) in the propensity for gas buyers, \( \pi_i = X_i \alpha + u_i \)), and that
non-buyers receive no such shock (i.e. \( u_i = 0 \)). Re-arranging Equation (7) above, yields the
following bounds for the unobserved effect for gas buyers:

\[
\frac{(1 - \Gamma)X_i \alpha}{\Gamma} \leq u_i \leq (\Gamma - 1)X_i \alpha
\]

The mean propensity for gas-buyers is 0.43 (average value of \( X_i \alpha \) for gas buyers) in the
absence of unobserved selection. Given that our results are robust to unobserved selection effects
even when \( \Gamma = 1.7 \), this implies that unobserved selection will nullify our qualitative conclusion
about the spillover effects on grocery spend, if the mean propensity for gas buyers increase from
0.43 to 0.43\( \times 1.7 = 0.73 \). It is highly unlikely that the probability of buying gas shifts from 43% (based on observables alone) to 73% (including unobservable effects). Hence we conclude that

---

\(^{11}\) We restrict our analysis in this section absolute (dollar) changes in spending. This allows us to avoid complications
that arise from the need to weight observations when comparing percentage changes in spending behavior.

\(^{12}\) Technically, from Table 7a, we should conclude that our results are robust up to \( \Gamma = 1.8 \). However, since further
confirmatory tests (discussed later in this section) do not support this value, we discuss and present our results for
\( \Gamma = 1.7 \). We note that our estimate of \( \Gamma = 1.7 \) is on the same order of magnitude as Rosenbaum bound results
reported by DiPrete and Gangl (2004).
unobserved selection is unlikely to overturn the conclusion about the existence of demand spillovers due to the introduction of gas station, based on the propensity matching results.

Our findings from the Rosenbaum bounds can be translated in two further ways to provide a range of estimates on the change in behavior in absolute terms (Rosenbaum 2002). These extensions necessitate making the additional assumption that the change in spending behavior among gas-buyers (i.e. the treatment effect) is additive and equal for all gas-buyers. While it is obviously not true that gas buyers all have the same absolute change in grocery spending, we consider additivity to be a plausible assumption when evaluating the average size of the effect across gas-buyers. The Hodges-Lehmann point estimates of the average additive effect of the gas station (in columns 4 and 5 of Table 7a) present the size of the effect, given a bias-level \( \Gamma \). At the upper bound of unobserved selection effects (for \( \Gamma = 1.7 \)), the data support a significant (at significance level of 0.1) average additive effect of $0.95 increase in grocery spend. Moreover, the confidence intervals on the treatment effect (columns 6 and 7 of Table 7a) continue to be positive and cover our original estimate up to \( \Gamma = 1.7 \). Since our estimates of the average effect are within the ranges of both these intervals, we are confident in our overall assessment of increased spending among gas-buyers.

*** Table 7b ***

For completeness, the equivalent results for changes in weekly trips to the store are shown in Table 7b. Our findings of an absolute increase in trips among gas-buyers are robust up to \( \Gamma = 1.9 \), lending further credence to our results.

The Altonji, Elder and Taber (AET) Approach

Though the Rosenbaum bounds indicate that our conclusion about demand externalities is robust to a large level of unobservable selection, they did not provide a point estimate for the effect of the gas station on grocery sales. Altonji, Elder and Taber (2002, 2005; henceforth AET) obtain a point estimate of the treatment effect by specifying a restriction on the correlation
between the residuals in the selection and outcome equations, where the outcome is binary. We adapt that approach for a continuous outcome variable.

Similar to AET, we specify the outcome and selection equation as follows:

\[
\Delta \%\text{Groc}_i = \alpha \cdot 1 \cdot \text{Gas}_i \cdot \text{Buy}_i + X'_i \gamma + \varepsilon_i, \\
\text{Gas}_i \cdot \text{Buy}_i^* = X'_i \beta + u_i,
\]

(9)

where \[
\begin{pmatrix}
\varepsilon_i \\
u_i
\end{pmatrix}
\sim N
\left(
\begin{pmatrix}
0 \\
0
\end{pmatrix},
\begin{pmatrix}
\sigma^2 & \rho \sigma \\
\rho \sigma & 1
\end{pmatrix}
\right)
\]

Here, we model the selection using the latent variable, \( \text{Gas}_i \cdot \text{Buy}_i^* \), and the outcome of interest using the same covariates \( (X_i) \), which is comprised of the same three variables used for propensity scoring: \log(\text{potential}), \text{SOW} \text{ and trips} in the pre-period).

Unlike the traditional approach to selection problems, which typically utilize one or more exclusion restrictions, AET do not require exclusion restrictions, but achieve identification through the use of an additional restriction on \( \rho \) as follows.

We begin by projecting the latent variable that represents the choice to buy gas onto the other components of the outcome equation. We can then represent the projection as

\[
\text{Proj} \text{Gas}_i \cdot \text{Buy}_i^* | X'_i \gamma, \varepsilon_i = \phi_0 + \phi_{X_i} X'_i \gamma + \phi_{\varepsilon_i} \varepsilon_i
\]

(10)

Restricting the effect of unobservables to be relatively the same as the effect of observables is then equivalent to specifying that \( \phi_{X_i} = \phi_{\varepsilon_i}. \)\(^{13}\) Under this assumption, and an adaptation of the original AET framework to allow for a continuous outcome variable, we are able to show that the correlation between the residuals in the selection and outcome equations lie in the range

\(^{13}\)Note that the restriction \( \phi_{X_i} = \phi_{\varepsilon_i} \) is more realistic than the standard OLS approach, which would require that \( \text{Cov}(\text{Gas}_i \cdot \text{Buy}_i^*, \varepsilon_i) = 0. \)
\[0 \leq \rho \leq \sigma \left( \frac{\text{Cov} \left[ X' \beta, X' \gamma \right]}{\text{Var} \left[ X' \gamma \right]} \right) \]  

(11)

To aid interpretation, note that if \( \rho = 0 \) in (11) above, selection on unobservables is not a concern, because unobservable influences on the choice to buy gas have no bearing on the percentage difference in groceries purchased. At the other extreme (i.e. the upper bound), \( \rho \) takes on some positive value less than 1, and a positive shock towards being a gas buyer, results in correlated shock in the amounts of groceries purchased.

Having formalized the notation for the correlation term, we briefly discuss why the equality-of-effects assumption (i.e. \( \phi_{X' \gamma} = \phi \)) is a plausible one (the formal arguments and proofs are presented in Altonji, Elder and Taber, 2002). Consider the universe of variables that completely explains the selection decision, and given our context, and let us denote them as follows:

\[Gas \_ \_Buy^* = X' \beta + X' \beta \]  

(12)

Here, the subscript ‘o’ denotes “observed”, while the ‘u’ denotes “unobserved.” If the universe of covariates were partitioned such that \( X' \) denoted a random partition, then the correlation of the index \( X' \beta \), with any outcome variable of interest should be approximately the same as the correlation of the index \( X' \beta \), with that same outcome variable. Since in reality, we do not observe \( X' \beta \), these unobserved effects enter as a single quantity \( u \), the residual term on the selection equation. Under random selection of observed covariates, we expect that the extent to which the index of the observables explains an outcome is about the same as the extent to which the unobservables explain the same outcome (equality-of-effects). Hence, \( \phi_{X' \gamma} = \phi \). This also explains the intuition that underlies the upper bound of the correlation in Equation (11).

In our application, since prior shopping behavior is actually a very good predictor of future shopping behavior, explanatory power of the observed variables is likely to exceed the explanatory
power of the residuals; hence the AET upper bound is likely to be conservative in accounting for unobserved selection effects.

*** Table 8 ***

Table 8 presents the estimates of the AET model in (9), which suggest an average percentage change of 7.7% in response to the opening of a gas station. While less than the propensity-matched estimate of 9.3%, these results do not reject our conclusion from the propensity matching; furthermore, the quantitative magnitude of the effects is reasonably close. Moreover, the relatively small estimate of $\rho = 0.093$ under the AET assumption on unobservable selection, suggests that the magnitude of selection on unobservables is relatively small.\textsuperscript{14} Thus even in the presence of selection, grocery spending among gas-buyers increases by 7.7%-9.3% as the externality from the opening of the gas station. The equivalent model for $\Delta \%\text{Trip}_i$ shows that the relative change in trips is close to estimates without accounting for unobservable selection (compare 15.3% versus 14.1% from propensity-matched analysis in Table 6). Taken together, we conclude that our primary findings on spend and trips are robust to unobservable selection effects.

5. Profitability Implications of Expanding Scope

Should a retailer’s decision to launch a gas station take into account the magnitude of the demand externalities on groceries? This would depend on relative magnitude of profits due to increase grocery sales, relative to the profits from gas. We perform a simple back of the envelope calculation to answer the question.

\textsuperscript{14} The high significance and sign of the log (potential) variable suggest that we may have inadvertently induced spurious correlation in our model because the dependent variable $\Delta \%\text{Groc}_i$ is already a function of sales in the preperiod. To address this concern, column 2 of Table 8 shows estimates from a model that excludes log(potential) as a covariate. Although significance levels change, the point estimate of change in grocery spending for gas buyers is no different (7.7% versus 8.2% are not significantly different), and the point estimate of correlation remains very similar.
From the retailer, we know the amount of gasoline sold is approximately 30,000 gallons per week. At an average retail price of approximately $2.85 per gallon on June 2005 to June 2006 in Connecticut (obtained from Gasbuddy.com), the revenue from gasoline is $85,000 per week. At the upper bound of the externality (9.3%), and with 39% of households buying gas, the unconditional increase in grocery spending is 3.6%. At the average weekly sales level of $527,000 per week, this translates to additional revenues of $19,000 per week. At the lower bound of the externality (7.7%), the unconditional increase in grocery spending is 3.0%, or $15,700 per week. Thus the increased revenue from groceries is about 18-22% of revenues from gas.

Given that the percentage gross margin on groceries and gas are 35% and 5%, respectively (as indicated by managers at the grocery chain), the dollar gross margin is $6,600 per week in increased groceries and $4,300 per on gasoline sales at the upper bound of 9.3%. At the lower bound of 7.7%, the corresponding dollar gross margin from increased grocery sales is $5,500 per week (gasoline margins remain the same). Thus, the increased profit from groceries is approximately 130% to 150% of the profits from gas.

On an annualized basis, the increased spillover profit from groceries ranges from $286,000 to $345,000, while the direct profits from gas is $220,000. Interviews with managers indicated that the fixed costs of opening a gas station (during the period of the data) are approximately $900,000. It is easy to see that the spillover benefits from groceries should have a significant impact on whether a retailer should expand scope by introducing a gas station.

Finally, we note that our analysis potentially understates the true spillover gains from increased scope in this context. We have only measured the extent to which existing customers change their behavior to drive revenue economies of scope. By design, such an analysis excludes two important additional effects: (1) the benefits for attracting new customers whose spending
would be entirely incremental; and (2) the longer-term effects of improved customer retention.  
Both effects could further increase the demand externalities resulting from increased scope.

6. Conclusion

We demonstrate the existence of significant revenue economies of scope based on direct measures of household behavior in a grocery retail context. Our results are also robust to controls for selection on observables and unobservables, and we estimate the overall increase in grocery spend due to gas-station co-location to be between 7.7% and 9.3%.

We find that households with low and intermediate loyalty show the greatest response to increases in scope, suggesting that the gas station helps in customer acquisition and retention of such customers. Our analysis also shows that the spillover dollars accrue primarily through increased trips to the store, which supports a search- or travel-cost based explanation for revenue economies of scope. In terms of competitive impact, the focal firm’s gain from expanding scope comes at the expense of both traditional grocery competitors and convenience stores.

The issue of demand externalities that arise from firm scope has been a major issue of research in economics and marketing. This paper offers a direct demand based household level approach to measure the externality and thus complements traditional supply side approaches (based on supply decisions such as entry and prices), which require the assumption that firms behave optimally. We hope our demand side approach serves as an impetus for closer examination of demand externalities in a variety of contexts and informs broader the decision-making – ranging from decisions about retail scope and mall design to more ambitious goals such as the revival of downtown business districts.

---

15 “Retention” effects could come from two sources. The first is simply a result of lower attrition over time among existing customers. A second source is the possibility of incremental spending from “late-adopters” of the gas station. Specifically, these are households who buy gas for the first time after our six-week window of analysis, but still receive some benefit from increased scope and respond accordingly.
References


Table 1: Summary of Household Purchasing Behavior at Focal Store: Weekly-average household grocery, gas and trip behavior in Pre- and Post-Periods (i.e. six weeks before and after opening of gas station).

<table>
<thead>
<tr>
<th>Household Type</th>
<th>Grocery Sales</th>
<th>Gasoline Sales</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>HH Count</td>
<td>% Of Households</td>
</tr>
<tr>
<td>Gas Buyer</td>
<td>4,215</td>
<td>39%</td>
</tr>
<tr>
<td>Non-Gas Buyer</td>
<td>6,675</td>
<td>61%</td>
</tr>
<tr>
<td>Overall</td>
<td>10,890</td>
<td>100%</td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th>Household Type</th>
<th>Grocery Trips</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Pre-Period</td>
</tr>
<tr>
<td>Gas Buyer</td>
<td>1.07</td>
</tr>
<tr>
<td>Non-Gas Buyer</td>
<td>0.73</td>
</tr>
<tr>
<td>Overall</td>
<td>0.86</td>
</tr>
</tbody>
</table>

*** p < 0.01
Table 2: Change in Grocery Spend by Gas Buyers*

<table>
<thead>
<tr>
<th></th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td>Intercept (%)</td>
<td>-2.20 **</td>
<td>-0.06 ***</td>
</tr>
<tr>
<td></td>
<td>(0.29)</td>
<td>(0.01)</td>
</tr>
<tr>
<td>Bought Gas (β)</td>
<td>5.78 ***</td>
<td>0.13 ***</td>
</tr>
<tr>
<td></td>
<td>(0.49)</td>
<td>(0.01)</td>
</tr>
</tbody>
</table>

| N                  | 10,890                  | 10,890                    |
| F-Value            | 136.52                  | 134.1                     |
| Prob>F             | 0.00                    | 0.00                      |

* p<.10, ** p<0.05, *** p<0.01

* Robust standard errors in parentheses. Regression in second column (Percentage Difference in Grocery Spend) is weighted by household grocery purchases in the pre-period.

Table 3: Change in Grocery Trips by Gas Buyers*

<table>
<thead>
<tr>
<th>Dependent Variable</th>
<th>Diff. Trips (Post - Pre)</th>
<th>Pct. Diff. Trips (Weighted)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Intercept (%)</td>
<td>-0.06 **</td>
<td>-0.09 ***</td>
</tr>
<tr>
<td></td>
<td>(0.00)</td>
<td>(0.01)</td>
</tr>
<tr>
<td>Bought Gas (β)</td>
<td>0.13 ***</td>
<td>0.15 ***</td>
</tr>
<tr>
<td></td>
<td>(0.01)</td>
<td>(0.01)</td>
</tr>
</tbody>
</table>

| N                  | 10,890                  | 10,890                    |
| F-Value            | 209.62                  | 234.28                    |
| Prob>F             | 0.00                    | 0.00                      |

* p<.10, ** p<0.05, *** p<0.01

* Regression in second column (Percentage Difference in Grocery Trips) is weighted by the number of trips made by the household in the pre-period.
Table 4: Percent Difference in Grocery Spending and Trips (between Before and After the opening of gas station) by Household Loyalty*

<table>
<thead>
<tr>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td>Intercept ($\alpha_0$)</td>
<td>-0.19 ***</td>
<td>-0.32 ***</td>
<td>-0.18 ***</td>
<td>-0.26 ***</td>
</tr>
<tr>
<td></td>
<td>(0.02)</td>
<td>(0.03)</td>
<td>(0.01)</td>
<td>(0.02)</td>
</tr>
<tr>
<td>Bought Gas ($\beta_1$)</td>
<td>0.26 ***</td>
<td>0.26 ***</td>
<td>0.22 ***</td>
<td>0.20 ***</td>
</tr>
<tr>
<td></td>
<td>(0.03)</td>
<td>(0.05)</td>
<td>(0.02)</td>
<td>(0.04)</td>
</tr>
<tr>
<td>Pct. SoW ($\beta_2$)</td>
<td>0.21 ***</td>
<td>0.80 ***</td>
<td>0.18 ***</td>
<td>0.61 ***</td>
</tr>
<tr>
<td></td>
<td>(0.02)</td>
<td>(0.12)</td>
<td>(0.02)</td>
<td>(0.09)</td>
</tr>
<tr>
<td>Pct. SoW Squared ($\beta_4$)</td>
<td></td>
<td>-0.50 ***</td>
<td></td>
<td>-0.38 ***</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(0.10)</td>
<td></td>
<td>(0.08)</td>
</tr>
<tr>
<td>Bought Gas × Pct. SoW ($\beta_3$)</td>
<td>-0.20 ***</td>
<td>-0.25</td>
<td>-0.14 ***</td>
<td>-0.11</td>
</tr>
<tr>
<td></td>
<td>(0.04)</td>
<td>(0.19)</td>
<td>(0.03)</td>
<td>(0.14)</td>
</tr>
<tr>
<td>Bought Gas × Pct. SoW Squared ($\beta_5$)</td>
<td>0.06</td>
<td></td>
<td>-0.01</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.15)</td>
<td></td>
<td></td>
<td>(0.12)</td>
</tr>
<tr>
<td>N</td>
<td>10,890</td>
<td>10,890</td>
<td>10,890</td>
<td>10,890</td>
</tr>
<tr>
<td>F-Value</td>
<td>75.84</td>
<td>58.92</td>
<td>111.21</td>
<td>74.67</td>
</tr>
<tr>
<td>Prob&gt;F</td>
<td>0.00</td>
<td>0.00</td>
<td>0.00</td>
<td>0.00</td>
</tr>
</tbody>
</table>

* p<.10, ** p<0.05, *** p<0.01

* Percentage difference in grocery spend regressions are weighted by total spending in the pre-period. Percentage difference in trips regressions are weighted by total household trips in the pre-period.
Table 5: Percentage Difference in Grocery Spending (between Before and After) by Product Category*

<table>
<thead>
<tr>
<th>Dependent Variable:</th>
<th>(1) Traditional</th>
<th>(2) Convenience</th>
<th>(3) All Products</th>
</tr>
</thead>
<tbody>
<tr>
<td>% Diff. Spending</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Product Type</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Intercept</td>
<td>-0.07</td>
<td>-0.05</td>
<td>-0.07</td>
</tr>
<tr>
<td></td>
<td>(0.01)</td>
<td>(0.01)</td>
<td>(0.01)</td>
</tr>
<tr>
<td>Convenience Products</td>
<td>0.01</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Bought Gas</td>
<td>0.13</td>
<td>0.15</td>
<td>0.13</td>
</tr>
<tr>
<td></td>
<td>(0.01)</td>
<td>(0.01)</td>
<td>(0.01)</td>
</tr>
<tr>
<td>Gas × Convenience Products</td>
<td>0.03</td>
<td>*</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.01)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>N (households)</td>
<td>10,890</td>
<td>10,890</td>
<td>10,890</td>
</tr>
<tr>
<td>F-Value</td>
<td>115.49</td>
<td>108.26</td>
<td>52.73</td>
</tr>
<tr>
<td>Prob&gt;F</td>
<td>0.00</td>
<td>0.00</td>
<td>0.00</td>
</tr>
</tbody>
</table>

* p<.10, ** p<0.05, *** p<0.01

* There are 593 total categories, of which 129 are considered “Convenience” product categories. The remaining 464 are considered “Traditional” grocery products. Observations weighted by household grocery spending on the relevant product types in the pre-period. Robust standard errors in parentheses (clustered standard errors for regressions 3).
Table 6: Propensity-Matched Results: Increase in Weekly Grocery Spending & Trips

<table>
<thead>
<tr>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td>(1) Results w/o matching (Tables 2 &amp; 3)</td>
<td>5.778</td>
<td>0.133</td>
<td>0.127</td>
<td>0.147</td>
</tr>
<tr>
<td></td>
<td>(0.495)</td>
<td>(0.012)</td>
<td>(0.009)</td>
<td>(0.010)</td>
</tr>
<tr>
<td>(2) Spend</td>
<td>7.062</td>
<td>0.120</td>
<td>0.135</td>
<td>0.106</td>
</tr>
<tr>
<td></td>
<td>(0.501)</td>
<td>(0.010)</td>
<td>(0.009)</td>
<td>(0.008)</td>
</tr>
<tr>
<td>(3) Trips</td>
<td>6.646</td>
<td>0.076</td>
<td>0.163</td>
<td>0.153</td>
</tr>
<tr>
<td></td>
<td>(0.513)</td>
<td>(0.010)</td>
<td>(0.009)</td>
<td>(0.008)</td>
</tr>
<tr>
<td>(4) Spend, Trips</td>
<td>7.038</td>
<td>0.096</td>
<td>0.160</td>
<td>0.146</td>
</tr>
<tr>
<td></td>
<td>(0.514)</td>
<td>(0.010)</td>
<td>(0.009)</td>
<td>(0.008)</td>
</tr>
<tr>
<td>(5) log(Potential Spend)</td>
<td>5.892</td>
<td>0.093</td>
<td>0.130</td>
<td>0.112</td>
</tr>
<tr>
<td></td>
<td>(0.498)</td>
<td>(0.009)</td>
<td>(0.009)</td>
<td>(0.008)</td>
</tr>
<tr>
<td>(6) log(Potential Spend), Trips</td>
<td>6.789</td>
<td>0.086</td>
<td>0.164</td>
<td>0.156</td>
</tr>
<tr>
<td></td>
<td>(0.513)</td>
<td>(0.010)</td>
<td>(0.009)</td>
<td>(0.008)</td>
</tr>
<tr>
<td>(7) log(Potential Spend), Trips, Pct. SoW</td>
<td>6.850</td>
<td>0.093</td>
<td>0.158</td>
<td>0.141</td>
</tr>
<tr>
<td></td>
<td>(0.515)</td>
<td>(0.010)</td>
<td>(0.009)</td>
<td>(0.008)</td>
</tr>
</tbody>
</table>

* All results are significant with p-value of <0.01. “Spend” indicates grocery spending in the pre-period. “Potential Spend” is grocery spend in the pre-period divided by the percentage share of wallet (“Pct. SoW”, above). Standard errors are in parentheses. Percentage changes are weighted by household grocery spending or trips in the pre-period (as relevant).

First row of results are identical to those reported in Tables 2 and 3 (coefficient for “Bought Gas”), and reflect estimated change in behavior without any propensity matching.

Propensity scores reflect the probability of a household buying gas, and are estimated using a linear probit model. Matching is implemented using Gaussian kernels (bandwidth of 0.05) on variables as shown. Numbers in the table above should be understood as the average treatment effect on the treated.
Table 7a: Robustness to Unobserved Selection Effects: Difference in Weekly Grocery Spending

<table>
<thead>
<tr>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td>1.0</td>
<td>0.00</td>
<td>0</td>
<td>5.843</td>
<td>5.843</td>
<td>5.290</td>
<td>6.399</td>
</tr>
<tr>
<td>1.1</td>
<td>0.00</td>
<td>0</td>
<td>4.944</td>
<td>6.750</td>
<td>4.307</td>
<td>7.315</td>
</tr>
<tr>
<td>1.2</td>
<td>0.00</td>
<td>0</td>
<td>4.132</td>
<td>7.588</td>
<td>3.386</td>
<td>8.160</td>
</tr>
<tr>
<td>1.3</td>
<td>0.00</td>
<td>0</td>
<td>3.392</td>
<td>8.367</td>
<td>2.850</td>
<td>8.951</td>
</tr>
<tr>
<td>1.4</td>
<td>0.00</td>
<td>0</td>
<td>2.714</td>
<td>9.098</td>
<td>2.173</td>
<td>9.691</td>
</tr>
<tr>
<td>1.5</td>
<td>0.00</td>
<td>0</td>
<td>2.088</td>
<td>9.785</td>
<td>1.544</td>
<td>10.389</td>
</tr>
<tr>
<td>1.6</td>
<td>0.00</td>
<td>0</td>
<td>1.504</td>
<td>10.433</td>
<td>0.957</td>
<td>11.045</td>
</tr>
<tr>
<td>1.7</td>
<td>0.00</td>
<td>0</td>
<td>0.956</td>
<td>11.046</td>
<td>0.407</td>
<td>11.668</td>
</tr>
<tr>
<td>1.8</td>
<td>0.09</td>
<td>0</td>
<td>0.442</td>
<td>11.628</td>
<td>-0.111</td>
<td>12.263</td>
</tr>
<tr>
<td>1.9</td>
<td>0.55</td>
<td>0</td>
<td>-0.043</td>
<td>12.184</td>
<td>-0.602</td>
<td>12.827</td>
</tr>
<tr>
<td>2</td>
<td>0.93</td>
<td>0</td>
<td>-0.505</td>
<td>12.714</td>
<td>-1.070</td>
<td>13.368</td>
</tr>
</tbody>
</table>

* Results are based on differences between behavior of gas-buyers and propensity-matched non-buyers. p-values and confidence intervals are one-sided and at the 90% level. H-L indicates Hodges-Lehmann point estimates.

Table 7b: Robustness to Unobserved Selection Effects: Difference in Weekly Grocery Trips

<table>
<thead>
<tr>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td>1.0</td>
<td>0.00</td>
<td>0</td>
<td>0.142</td>
<td>0.142</td>
<td>0.131</td>
<td>0.151</td>
</tr>
<tr>
<td>1.1</td>
<td>0.00</td>
<td>0</td>
<td>0.123</td>
<td>0.157</td>
<td>0.111</td>
<td>0.170</td>
</tr>
<tr>
<td>1.2</td>
<td>0.00</td>
<td>0</td>
<td>0.105</td>
<td>0.176</td>
<td>0.095</td>
<td>0.187</td>
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<tr>
<td>1.3</td>
<td>0.00</td>
<td>0</td>
<td>0.090</td>
<td>0.191</td>
<td>0.078</td>
<td>0.204</td>
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<tr>
<td>1.4</td>
<td>0.00</td>
<td>0</td>
<td>0.075</td>
<td>0.207</td>
<td>0.065</td>
<td>0.220</td>
</tr>
<tr>
<td>1.5</td>
<td>0.00</td>
<td>0</td>
<td>0.063</td>
<td>0.222</td>
<td>0.055</td>
<td>0.231</td>
</tr>
<tr>
<td>1.6</td>
<td>0.00</td>
<td>0</td>
<td>0.054</td>
<td>0.232</td>
<td>0.042</td>
<td>0.243</td>
</tr>
<tr>
<td>1.7</td>
<td>0.00</td>
<td>0</td>
<td>0.042</td>
<td>0.243</td>
<td>0.030</td>
<td>0.257</td>
</tr>
<tr>
<td>1.8</td>
<td>0.00</td>
<td>0</td>
<td>0.031</td>
<td>0.256</td>
<td>0.018</td>
<td>0.268</td>
</tr>
<tr>
<td>1.9</td>
<td>0.00</td>
<td>0</td>
<td>0.020</td>
<td>0.266</td>
<td>0.010</td>
<td>0.280</td>
</tr>
<tr>
<td>2.0</td>
<td>0.06</td>
<td>0</td>
<td>0.012</td>
<td>0.277</td>
<td>-0.001</td>
<td>0.291</td>
</tr>
<tr>
<td>2.1</td>
<td>0.39</td>
<td>0</td>
<td>0.002</td>
<td>0.288</td>
<td>-0.010</td>
<td>0.302</td>
</tr>
<tr>
<td>2.2</td>
<td>0.82</td>
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<td>0.299</td>
<td>-0.017</td>
<td>0.310</td>
</tr>
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<td>2.3</td>
<td>0.98</td>
<td>0</td>
<td>-0.014</td>
<td>0.307</td>
<td>-0.024</td>
<td>0.317</td>
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<tr>
<td>2.4</td>
<td>1.00</td>
<td>0</td>
<td>-0.021</td>
<td>0.314</td>
<td>-0.030</td>
<td>0.326</td>
</tr>
<tr>
<td>2.5</td>
<td>1.00</td>
<td>0</td>
<td>-0.026</td>
<td>0.321</td>
<td>-0.037</td>
<td>0.335</td>
</tr>
</tbody>
</table>

* Results are based on differences between behavior of gas-buyers and propensity-matched non-buyers. p-values and confidence intervals are one-sided and at the 90% level. H-L indicates Hodges-Lehmann point estimates.
**Table 8: Robustness to Unobserved Selection Effects:** Percent Difference in Grocery Spending

<table>
<thead>
<tr>
<th>Covariate</th>
<th>Percent Diff. Groceries</th>
<th>Percent Diff. Groceries</th>
<th>Percent Diff. Groceries</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Outcome Equation</strong></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Gas_Buy</td>
<td>0.077 *</td>
<td>0.082 ***</td>
<td>0.153 ***</td>
</tr>
<tr>
<td></td>
<td>(0.033)</td>
<td>(0.005)</td>
<td>(0.028)</td>
</tr>
<tr>
<td>Intercept</td>
<td>1.580 ***</td>
<td>-0.076 ***</td>
<td>0.876 ***</td>
</tr>
<tr>
<td></td>
<td>(0.171)</td>
<td>(0.011)</td>
<td>(0.029)</td>
</tr>
<tr>
<td>log(Potential)</td>
<td>-0.247 ***</td>
<td>-0.127 ***</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.024)</td>
<td>(0.004)</td>
<td></td>
</tr>
<tr>
<td>Trips</td>
<td>0.0026</td>
<td>-0.0128 ***</td>
<td>-0.0058 ***</td>
</tr>
<tr>
<td></td>
<td>(0.004)</td>
<td>(0.001)</td>
<td>(0.002)</td>
</tr>
<tr>
<td>SoW</td>
<td>-0.0378</td>
<td>0.1983 ***</td>
<td>-0.2314 ***</td>
</tr>
<tr>
<td></td>
<td>(0.072)</td>
<td>(0.013)</td>
<td>(0.011)</td>
</tr>
<tr>
<td>Sigma</td>
<td>0.6162 **</td>
<td>0.6364 ***</td>
<td>0.5445 ***</td>
</tr>
<tr>
<td></td>
<td>(0.230)</td>
<td>(0.003)</td>
<td>(0.005)</td>
</tr>
<tr>
<td><strong>Selection Equation (Gas_Buy)</strong></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Intercept</td>
<td>-1.1739 ***</td>
<td>-0.5467 ***</td>
<td>-0.4475 ***</td>
</tr>
<tr>
<td></td>
<td>(0.313)</td>
<td>(0.026)</td>
<td>(0.072)</td>
</tr>
<tr>
<td>log(Potential)</td>
<td>0.0944 *</td>
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<td>-0.0140</td>
</tr>
<tr>
<td></td>
<td>(0.042)</td>
<td></td>
<td>(0.013)</td>
</tr>
<tr>
<td>Trips</td>
<td>0.0521 ***</td>
<td>0.0580 ***</td>
<td>0.0671 ***</td>
</tr>
<tr>
<td></td>
<td>(0.009)</td>
<td>(0.003)</td>
<td>(0.004)</td>
</tr>
<tr>
<td>SoW</td>
<td>0.2463</td>
<td>0.1570 ***</td>
<td>-0.2442 ***</td>
</tr>
<tr>
<td></td>
<td>(0.134)</td>
<td>(0.037)</td>
<td>(0.033)</td>
</tr>
<tr>
<td>Correlation (restricted)</td>
<td>0.0933</td>
<td>0.0693 ***</td>
<td>0.0550</td>
</tr>
<tr>
<td></td>
<td>(0.077)</td>
<td>(0.005)</td>
<td>(0.036)</td>
</tr>
</tbody>
</table>

* Maximum-Likelihood estimates with restrictions on correlation term. Observations are weighted by grocery spend or trips in the pre-period (as relevant). Standard errors are obtained from 200 bootstrap iterations.
Figure 1a: Change (Difference) in Grocery Spend by Household Loyalty
10,890 households in data.

Figure 1b: Percent Change in Grocery Spend by Household Loyalty
Figure 2: Difference-in-Difference Estimate and Propensity Matching

(a) Difference-in-Difference Estimate: Same changes for Treatment (gas buyer) and Control (non-gas buyer) in the absence of treatment (opening of the gas station).

(b) Propensity Matched Difference-in-Difference Estimate: Different changes for Treatment (gas buyer) and Control (Non gas buyer) even in the absence of treatment (opening of gas station).