Social Dollars: The Economic Impact of Customer Participation in a Firm-sponsored Online Customer Community

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Abstract

Many firms operate customer communities online. This is motivated by the belief that customers who join the community become more engaged with the firm and/or its products, and as a result, increase their economic activity with the firm. We describe the customer expenditures generated from increased engagement as a result of joining the community as “social dollars.” This paper tests for the existence and magnitude of social dollars via a difference-in-differences estimator using data from a multi-channel entertainment products retailer that launched a customer community online. We find that a significant increase (of at least 18%) in post-launch expenditure from community customers can be attributed to their joining the community. This result is robust across a variety of tests for self-selection and alternative explanations. These social dollars persist over the time period we observe and arise in both online and offline channels. Examination of mechanisms conceptually linked to community benefits reveals that social dollars are moderated by the extent of a customer’s social engagement (posting versus lurking) and by the unique informational benefit of product information sourced from a community of shared interests, which we observe as a shift in the preference heterogeneity of products purchased by community members.

Keywords: Online Customer Communities, Online Customer Behavior, Retailing, Natural Experiments, Panel Data, Difference-in-Differences Estimation.
1 Introduction

Human beings have long communced as consumers. From Apple acolytes to Java junkies, Tupperware partiers to European car clubbers, like-minded consumers have engaged with one another in customer communities – networks of individuals who engage in social interactions regarding their shared enthusiasm for and/or use of specific brandmarks, products or consumption activities (Algesheimer et al., 2005; Bagozzi and Dholakia, 2002; Porter and Donthu, 2008; Rheingold, 1993). The last several years have seen an explosion of marketer interest in these consumer-powered social engines of brand engagement, trust, and loyalty (Porter and Donthu, 2008; Williams and Cothrel, 2000). Consumer adoption of the Internet, social media and mobile technologies has been a central driver of this enthusiasm. Over 70% of Americans (Forrester, 2012) and an estimated one billion people across the planet (Eddy, 2012) are using social media, spending over one third of their waking hours in online environments that allow them to present a public or private profile, establish social ties with like-minded others, and exchange information of mutual interest socially (Boyd and Ellison, 2007). The movement of like-minded consumers into online communities of shared interests represents a major business opportunity for firms, regardless of whether these communities are embedded in independent websites, firm-operated websites, or third-party social media platforms (Forrester, 2012; Wirtz et al., 2013). A recent IBM survey of 1,709 CEOs from over 60 countries reveals that they believe online customer communities are the second most important means by which they will engage customers in the future (after face to face interactions and well ahead of traditional media. Nearly 60% of these executives plan to invest (or invest more) in such online communities over the next three years, leading to an expected total annual marketing expenditure in online customer communities of nearly US $5 billion by 2016 (Paul, 2012; Schniederjans et al., 2012; Forrester, 2012).

Interestingly however, while firms and their top managers express high levels of confidence in the marketing efficacy of these communities, there is surprisingly little hard evidence documenting the economic benefits of online customer communities (whether firm-sponsored or hosted by third parties). In fact, doubts have been expressed in industry regarding the positive return on investment of online customer communities hosted on third-party social media platforms such as Facebook (Vranica and Raice, 2012). Firms have also articulated other concerns such as loss of control
and the potential of consumer backlash when customer communities are hosted by a third-party. This has led many companies to invest in building their own online customer communities. We estimate that, depending on the definition of the attributes of a firm-sponsored online customer community,\(^1\) between 25 and 50 of the top 100 global brands (Interbrand, 2011) host their own such community. In addition, Forrester (2012) reports that 18% of all businesses around the globe are making online customer community investments independent of third-party platforms. In this paper, we focus on quantifying the economic benefit of such communities using actual transaction and activity data. We do this with the help of a novel dataset from a multi-channel (online and offline) retailer that decided to launch an online customer community. Specifically, our focus is on isolating the incremental customer expenditure generated as a result of joining the community. We call this incremental expenditure “social dollars.”

Our data and research approach adds to the literature on online customer communities on multiple dimensions. First, and perhaps most important, we use actual behavioral data to investigate the economic impact to the firm of setting up and operating such a community. Second, the availability of consumer panel data before and after the formation of the community allows us to control for self-selection at a basic level. While we cannot rule out selection on unobservables with perfect certainty, our robustness checks provide compelling support that self-selection cannot explain our results. Third, the long time series of our data allows us to investigate whether the change in purchase behavior that results from joining the community is a short-term effect driven by the novelty of the event (the formation of the community) or a more persistent phenomenon. Fourth, given the multi-channel nature of our data, we are able to test whether the formation of the community affects behavior differentially across channels. Finally, while a primary objective of this paper is to document the existence and magnitude of social dollars, we also use the observed actions and interactions among community members to isolate mechanisms that moderate the economic effect of online customer community participation, allowing us to provide theoretically-grounded empirical evidence for why and/or how social dollars come about.

\(^{1}\)For the present discussion, we looked at three attributes described as identifiers of online or social media-based customer communities (Wirtz et al., 2013) to determine if a firm sponsors such a community. These were: (1) the ability for consumers to create and maintain a personal profile page in the firm’s brand or product-centered website, (2) the ability for consumers to create and maintain friend ties in this setting, and (3) the ability to post and consume user-generated content at the website. The most conservative definition is based on the presence of all three attributes while the least conservative definition is based on the presence of at least one attribute.
Our results suggest that social dollars represent at least 18% of expenditure from customers post their joining the community. In other words, these social dollars represent the incremental expenditure by customers who join the online customer community we observe, over and above their pre-existing purchase behavior with the firm, and relative to a control group. This magnitude is economically significant for the firm as it more than covers the fixed cost of setting up the community as well as the variable cost of operating it. We subject our estimate of the social dollars to multiple robustness checks and demonstrate that it is indeed robust. For example, we find that the social dollar estimate is robust to selection on observables and unobservables. We also find that social dollars persist over time. Another interesting finding is that we do not find evidence of channel cannibalization - we observe positive social dollar effects in both online and offline retail channels. Finally, our analysis of mechanisms behind the economic effect of online customer community participation suggests that indicators of the informational and social benefits attributed to customer communities in prior theoretical and survey-based research (e.g., Algesheimer et al., 2005; Algesheimer et al., 2010; Jang et al., 2008; Dholakia and Vianello, 2009) moderate the social dollar.

The rest of the paper is organized as follows. In § 2, we discuss the conceptual and empirical literature in which our research is grounded. We then describe the research setting and data in § 3. Section § 4 describes our modeling and analysis strategy, followed by multiple robustness checks. Leveraging theory, we then examine two potential moderators of the social dollar in § 5. Finally, we discuss the managerial implications of our findings in § 6 and then conclude in § 7.

2 Conceptual Background

Conceptual definitions of the attributes and consequences of customer communities are rich and varied. The most cited of these is Muniz and O’Guinn’s (2001), who described a brand community as a, “specialized, non-geographically bound community, based on a structured set of social relationships among admirers of a branded good or service” and offered three specific markers of these communities: shared consciousness, rituals and traditions, and a sense of moral responsibility. Researchers subsequently proposed an expanded conception to describe marketplace communities or consumption communities as relationships and behaviors not only of customers with brands, but
among customers themselves, between the customer and firm, and between the customer and the product-in-use (McAlexander et al., 2002). Research examining these relationships in technology-mediated settings has sometimes described them as virtual communities (Balasubramanian and Mahajan, 2001; Dholakia et al., 2004; Kozinets, 2002; Porter and Donthu, 2008). Overall, customer community has come to be the most common term used to describe a group or network of individuals who engage in social interactions regarding their shared enthusiasm for and/or use of specific brandmarks, products, retail environments, or consumption activities whether in online or offline settings (Algesheimer et al., 2010; Bagozzi and Dholakia, 2002; Porter and Donthu, 2008; Rheingold, 1993). Common social practices or interactions enabled by customer communities include the sharing of product experiences, recommendations or advice, planning for product-centered social interactions (e.g. online or offline group events), and in many online cases, the maintenance of a personal profile page intended to convey a user’s personality, interests and/or status in the community (Algesheimer et al., 2010; Brown et al., 2007; Dholakia et al., 2004; Muniz and O’Guinn, 2001; Schau et al., 2009). Empirical investigations of customer communities include those that operate independently of the brand or firm (e.g., Dholakia et al., 2004) and those that are organized and controlled by a commercial firm i.e., “firm-sponsored” (Kannan et al., 2000; Porter and Donthu, 2008).

Firms that operate customer communities are said to have the opportunity to increase customer engagement and loyalty (Fournier and Lee, 2009; Porter and Donthu, 2008; Williams and Cothrel, 2000). The expectation is that this increased engagement and/or loyalty will lead to better economic outcomes for the firm, as exemplified by predictions that firm sponsors of customer communities will be “richly rewarded with peerless customer loyalty and impressive economic returns” (Hagel and Armstrong, 1997, p. 2). Using surveys and self-report data, some academic research has reported an increase in purchase intention among online customer community members (Algesheimer et al., 2005; Porter and Donthu, 2008). Other researchers have shown that enabling consumer interactions in a firm-sponsored online customer community is one of seven factors linked to increased purchase intentions and willingness to pay a price premium with online retailers (Srinivasan et al., 2002).

Two more recent studies have examined the consequences of customer community membership using behavioral data. Zhu et al. (2012) found that firm-sponsored online customer community membership was linked to greater financial risk-taking as observed in lending (Prosper.com) and
bidding (eBay Germany) behaviors. A subsequent lab experiment supported arguments that this behavior was motivated by the perception that other community members will aid them should difficult situations arise in the future. Algesheimer et al. (2010) examined the behavioral consequences of customer community membership at eBay Germany. They found that eBay bidders and sellers became more selective and conservative in their auction behavior as a result of online community participation, leading to null or negative effects of community participation on individual-level bidding volume, product listings, average amount spent by buyers, and revenue earned by sellers. A unique aspect of the customer communities investigated in the above two studies is that they both exist to “make” markets. Thus, most of the important marketing mix elements (such as product and price) in both of these settings are a function of the actions of independent agents rather than of the firm. The setting and findings from these studies, and especially the Algesheimer et al. (2010) study, can be seen as complementary to the setting and findings from this paper. Overall therefore, with the two exceptions mentioned above, there has been little empirical assessment of the direct economic consequences of online customer communities to the firm or brand. In addition, almost no work (Zhu et al., 2012 being a notable exception) has proposed and tested theoretically-grounded mechanisms through which an economic consequence of customer community participation may occur.

3 Research Setting and Data

Our data come from a large North American retailer of entertainment and informational media products (e.g. books, movies, music).\(^2\) The firm is the largest retailer in its market by sales volume in its core product category, and operates in both retail and online channels, with approximately 10% of total revenues occurring online for the firm’s fiscal year 2009.

The firm launched its own online customer community in September 2007. The formation and existence of this community was communicated via mass marketing to consumers and current customers. Marketing communications were comprised of signage in stores, banner advertising on the firm’s website, print advertising in national newspapers, and the firm’s house opt-in email list. Advertising announcing the launch of the online customer community was untargeted—different

\(^2\)Due to the proprietary nature of the data, the firm has requested that its identity not be divulged.
customer segments were not given differential exposure to this announcement. Participation in the community was purely voluntary on an “opt-in” basis, and no financial incentive was given to customers to join the community.

Our empirical setting is consistent with this literature’s conceptual description of a *customer community*. Specifically, we observe a firm-organized and operated online environment that the firm explicitly describes internally and to the public as a customer community. Individuals who join the customer community share textual and graphical information about themselves and their product preferences and/or recommendations with other customers, graphically display a variety of personal and product-related content on a personal profile page, engage in discussions on community chat boards and establish formal friend ties. Customer community participants contribute a variety of user-generated content for the consumption of others who are either within their own network of friend ties (i.e., “private” content) or for the customer community at large (i.e., “public” content). These options include targeted (peer-to-peer) product recommendations, the initiation and management of exclusive (by-invitation) special interest sub-groups (e.g., “Vampire Movie Lovers Club”), the publication of “Top Ten Lists,” and product reviews that can be read by others. While the content of the customer community interactions we observe is most commonly over products-in-use (McAlexander et al., 2002), we also observe conversations regarding the firm brand (the retailer). Thus, our setting is highly consistent with prior descriptions of a firm-sponsored online customer community (Algesheimer et al., 2010; Brown et al., 2007; Dholakia et al., 2004; Kannan et al., 2000; Muniz and O’Guinn, 2001; Schau et al., 2009).

The data used in our analysis were extracted in January 2009. Using an “nth-select” random sampling procedure, the firm generated a random selection of 26,624 community members (from a population of about 266,000 such members) for analysis. The firm provided us with two kinds of data – transactional data and community activity data – for these members. The transactional data represent actual purchases made by these members in the firm’s online and retail (offline) channels. We are able to observe offline purchases for some community members via the use of a firm sponsored loyalty card. Customers could sign up for this card by paying a modest annual fee ($20). All customers in our primary analysis sample (across both treatment and control groups) had signed up for the loyalty card, hence there are no differences on this dimension between the two groups. Across the firm’s entire customer database, approximately 16% of customers had a loyalty
card and they accounted for approximately 40% of the firm’s total sales revenue. Each record in the transactional data includes the date of the customer’s first purchase, his/her first name, his/her geographic location, and details on each purchase event. Each purchase event indicates the channel and date of purchase, the specific product(s) purchased, customer expenditure net of any standing or promotional discounts received for each product within the transaction, and each product’s category classification. While we focus on the loyalty card holders in our primary analysis because it provides the most conservative estimate of the social dollar and supports a multi-channel view of the effect, to enhance generalizability we also report our analysis restricted to customers who are not loyalty card holders for whom we observe the online channel only.

The community activity data we observe includes the date members joined the community and the social behaviors in which they have participated within the community. Specifically, we observe the volume of several different types of user-generated content such as peer-to-peer product recommendations, product reviews written, Top 10 lists published, and the number of products (e.g., book cover graphics) displayed on their personal profile page.

There was a difference of fifteen months between the data extract (January 2009) and the formation of the customer community (September 2007). We therefore also asked the firm to provide fifteen months of data before the launch of the community for the random sample extracted. This allowed us to create a “pre” period for comparison. The firm provided transactional data going back to June of 2006 (i.e., fifteen months before the launch of the community), for the full analysis sample. In addition to the sample drawn from the community members described above, we asked the company to provide transactional data on customers who did not participate in the community to create a control group. The firm drew a random sample from customers (the total population was just under one million customers) who had not become members of the customer community during our observation period and who transacted at least once with the firm (online or offline) in the thirty months from June 2006 to January 2009, inclusive. They were able to provide us data for 6,091 online transactional accounts for our control group.3 Of these accounts, 2,352 were also loyalty card holders, which provides full visibility of their purchase behavior with the firm (online

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3The size of the full treatment group sample (26,624) is larger than the full control group sample (6,091). This is because the firm was principally interested in our analysis of the treatment group’s behavior and wanted us to have the flexibility of being able to create reasonably large sub-samples from this group to investigate specific issues. We show in our robustness checks that this does not impact our results in any meaningful way.
and offline (retail) channels).

In the subsequent discussion, we designate the 15-month period before the launch of the community as T1 (“pre-community,” June 2006 to September 2007, representing five quarters denoted Q1 through Q5) and the period after the launch of the community as T2 (“post-community,” October 2007 to January 2009; quarters denoted as Q6 through Q10). Note that while the exogenous change (the launch of the customer community) occurs at a specific point in time (September 2007), a customer can decide to join the community at any time after the launch. We address this issue in detail and exploit this attribute of the data in our analysis. Taken from the full sample described above, our primary analysis sample includes customers for whom we observe behavior across both sales channels (via the loyalty card) and who transact at least once in T1 and T2. We do this to ensure full visibility of the customer’s expenditure with the firm (across both channels) and to control for differential entry and exit patterns in the treatment and control groups (Blundell et al., 1998). This has the added benefit of making our results as conservative as possible (results without the entry/exit restriction are described in Appendix A). The application of our primary analysis criterion limits this sample to 7,909 (30% of the full treatment sample) and 1,255 (21% of the full control sample) customers in the treatment and control groups respectively.

A comparison of the two groups on demographics and behavioral variables (e.g., total expenditure) is provided in Table 1.\textsuperscript{4} The table shows that there is no significant difference in the total expenditure per customer and the number of orders per customer in the fifteen month period before the community was launched. However, average purchase size or the per order dollar expenditure is marginally higher for the control group (and statistically significant). After the launch of the community, the total mean expenditure and the number of orders increases for both the treatment and control groups while the average purchase changes very little for both groups. In terms of demographics, we see some minor differences, with the treatment group having a slightly higher number of women, a slightly larger household size and slightly lower access to computers.

\textsuperscript{4}Overall, the primary analysis sample consists of heavier spenders. The total expenditure before the launch of the brand community is 30% higher for the analysis sample we draw from the treatment group and 73% higher for the analysis sample we draw from the control group.
4 Estimation Strategy and Results

In this section, we first present a roadmap for our analysis approach (as laid out in Table 3) with a brief description of the role played by each of these analyses. First, we describe our modeling approach to estimate the magnitude of social dollars (if they exist) in § 4.1. As noted earlier, we exploit the exogenous formation of the online customer community and the availability of a control group. However, given that our data come from a natural experiment (and not a field experiment), we need to ensure that our findings are not confounded with other explanations. Chief among these is self-selection (or non-random assignment to treatment and control groups). We run a series of robustness checks to first control for self-selection based on the observed customer level demographic differences in § 4.3.1. We then run another series of robustness checks to control for self-selection on unobservables in § 4.3.2. Having demonstrated the robustness of social dollars to selection concerns, we then explore the validity of other possible explanations in § 4.4.

4.1 Existence and Magnitude of the Social Dollar

Given the structure of our dataset, we use a difference-in-differences (DD) estimator to help us obtain the magnitude of the social dollars. The use of this econometric estimator helps us rule out alternative explanations such as selection (via the observation of transactions of the same customers before and after) and exogenous factors (via the control group). While much of the research leveraging this estimation method entails (quasi) randomization of the treatment, we note that the DD model is also commonly applied in the marketing and economics literatures without (quasi) random assignment to groups (e.g., Autor, 2003; Chevalier and Mayzlin, 2006; Bronnenberg et al., 2010). We aggregate the detailed purchase data to these two periods (T1 and T2) rather than leveraging a more fragmented time series form to mitigate potential serial correlation and grouped error term effects (Bertrand et al., 2004). The specification we estimate is

$$ R_{igt} = \beta_1 I_g + \beta_2 I_t + \beta_3 I_g I_t + \beta_4 X_{ig} + \varepsilon_{igt} \tag{1} $$

where $R_{igt}$ is the outcome of interest (the total dollar expenditure at the individual level for most of our analyses) for consumer $i$ in group $g \in \{Treatment, Control\}$ at time $t \in \{T1, T2\}$. The $X_{ig}$ consists of a vector of county-level demographic variables (e.g, median household income, avg. household size, avg. household educational spending, computer access) and controls for observable
differences across customers in our sample. $^5$ $\beta_1$ captures possible differences between the treatment and control groups, $\beta_2$ controls for expenditure differences that are common across treatment and control groups between T1 and T2, and $\varepsilon_{igt}$ is the error term. The coefficient we describe as the “social dollar” is $\beta_3$, which estimates the causal effect of treatment (community membership) on purchase behavior, controlling for biases in permanent group differences and biases within the treatment group due to individual trends across the time periods.

Differential group entry and/or exit represents significant threats to the assumption of no sample composition changes across groups in DD estimation (Blundell et al., 1998). For example, one could expect that a customer community attracts new customers to the firm. From the firm’s perspective, it would be natural to include these customers in calculating the returns from launching the community (as these new customers also generate additional purchase revenues for the firm). Note that this makes for a less conservative test than the one we adopt. The criterion for our primary analysis sample that each consumer transact at least once in T1 and T2 ensures that we include only existing, “active” customers in both groups. This resolves uncertainty in the causes of any differential entry or exit by fixing group composition over time and avoids the possible confound of attributing higher post-period revenues to the community, which may have been contributed mainly by new customers. In Appendix A, we relax the use of both the loyalty card and entry/exit restriction criteria used in our primary analysis to explore their respective impacts on our findings.

In order to carry out our basic DD analysis, we use the date of the launch of the community as the temporal “break,” even though customers do not all join on that particular date. Specifically, we classify a customer who joins the community at any point in time (till the end of our data series fifteen months later) into the treatment group. The reason we do this is because otherwise, the same customer will enter both the control and treatment groups, invalidating our identification strategy. It is also important to note that this classification scheme works against our finding social dollars. This is because by including customers who do not join right away, we are in effect including “untreated” customers in our treatment group, thus biasing our estimate of social dollars towards zero. In other words, our estimate of social dollars will be conservative. We also exploit this feature of our data (that not everybody in the treatment group joins at the same time) as one of our tests ruling against selection on unobservables.

$^5$These data were collected from the 2006 National Census county-level database.
As shown in Table 4, our results show that the social dollars, as represented by $\beta_3$ in equation 1 above, exist (i.e., are statistically different from zero) and are $127.01 in magnitude over the 15 month observation period (approximately $102 on an annualized basis). Using an annual base expenditure of $676.69 (in T2) for the treatment group, social dollars are estimated to be 19% of all expenditure post the launch of the customer community. To the best of our knowledge, this is the first empirical result documenting that an online customer community can lead to a direct increase in total customer-level expenditure for its firm sponsor.6

Two aspects of this result are noteworthy. First, as noted earlier, the brand name under which this customer community operates is a retail brand selling a variety of individually-branded books, DVDs, CDs, and a selection of ancillary gift items (e.g., bookends, pens, greeting cards). In essence, the retail brand name is an “umbrella” brand associated with a specific assortment of product categories. This can be contrasted with a brand such as Lego that sets up its customer community in the context of Lego’s toy building-brick product only. We expect that the social dollars effect in an assorted product community would be weaker than that of a single product community given the potential diffusion of customer interest over multiple brands and/or product categories. Second, our results are quite different from the two other studies that quantified changes in customer behavior as a consequence of joining a customer community (Algesheimer et al., 2010; Zhu et al., 2012). The most direct comparison can be made with Algesheimer et al. (2010), which found that participation in eBay Germany’s customer community leads to null or small negative effects on economic outcomes. eBay can be contrasted with most e-commerce websites in that consumers play the role of both seller and buyer. As the authors note in their study, customers who join the community become educated about both the site as well as the behavior of other buyers and sellers. This education leads them to be more efficient and effective in their marketing behavior, leading to fewer listings. We propose that the customer community we investigate may be more representative of firm-sponsored online customer communities in general. In such communities, consumers engage with one another in generally positive social interactions pertaining to shared product or consumption interests rather than in competitive transactions to determine best prices.

6The firm’s research motivation supported the extraction of a larger treatment group compared to the control group. As a robustness check, we run our DD analysis using the same sample size (via random sampling) for both groups. The results (see Appendix A) show that the statistical significance and magnitude of the social dollar effect is robust to sample size.
(eBay) or loan interest rates (Prosper.com). Thus, in many ways, our results may be seen as complementary to those found in the above studies with the difference in results likely driven by the distinct nature of the sponsoring firm’s business model (and the resulting nature of its online customer community).

Applying our DD analysis to each of order size and order frequency as dependent variables, we find an increase in both due to membership in the customer community (Table 4). The increase in average purchase size is only marginally significant and relatively small in magnitude (4.3%). Order frequency appears to drive the majority of the social dollar effect— we observe nearly three additional purchase occasions over the 15 month observation period, representing an 18.4% increase in order frequency. This finding is consistent with the online customer community literature which argues that the array of informational content and opportunities for social engagement available in the community should increase the number of visits the consumer is likely to make to the community website, as well as the conversion rate per visit (Brown et al., 2002; Holland and Baker, 2001; McWilliam, 2000), leading to an increase in order frequency.

Given that the firm operates in both online and offline channels, a reasonable hypothesis could be that the social dollars arise from differential expenditure across sales channels. Specifically, the online nature of the customer community we observe may cause social dollars to be generated more from the online channel or, in an extreme case, to originate entirely from channel switching. To assess this, we first check to see if there is a significant difference in the share of customer expenditure between the online and offline channels before and after the community is formed. We carry out a DD analysis for the proportion of total sales for a given customer that comes from the online channel. We find that the online proportion goes up by 13 points (Table 4 column 4), suggesting that the community does shift purchase behavior towards the online channel.

To assess the size of this shift, we replicate our basic DD analysis by channel and find that the total social dollar magnitude - $127.01 - is composed of $87.79 from the online channel and $39.23 from retail (offline), representing a 37.0% and 8.9% increase in T2 purchases in the respective channels. Two things about this decomposition are noteworthy. First, as predicted by research reporting that the ability to exchange information in customer communities enhances loyalty to e-commerce providers (Srinivasan et al., 2002), 70% of the social dollar arises in the firm’s online channel. Second and perhaps more important, community membership increases customer
expenditure in the offline channel as well. To our knowledge, academic research has yet to consider the presence and magnitude of cross-channel effects of online social interactions such as those found in customer communities. This finding thus adds to the literature by documenting an economic measure of positive channel spillover for online social interactions.

4.2 Controlling for observed customer-level differences

We now demonstrate the robustness of our findings. We first seek to replicate our results after accounting for several observables. These include the minor demographic differences across the treatment and control groups described earlier, cross-sectional differences in expenditure prior to the introduction of the community and the possibility of differential temporal trends across groups. Next, as we do not have (quasi) random assignment of consumers to treatment and control, we conduct a series of robustness tests to examine the possibility that unobservable differences between the comparison groups give rise to our results.

4.2.1 Accounting for demographic differences

First, we compare the treatment and control samples on observables—demographic and past transaction behavior. Recall that we had only first names for each customer in our data. Using a standard “genderizer” list, we were able to infer gender for 82% of the sample (all analyses that follow that include gender are restricted to this 82%). As shown in Table 2, group comparison t-tests indicate that the proportion of males was slightly lower (albeit significant statistically) in the treatment than control group. There were also minor differences in the average household size and computer access between the two groups, with the average treatment group household being slightly larger.

We therefore replicate our main analysis with explicit controls for these minor differences across the treatment and control groups. Specifically, we add a vector of demographic controls (including gender, observed tenure with the firm, average household size, median family income, household educational spend and computer access) to the main specification (equation 1). As can be see from Table 4, we replicate our results after controlling for these demographic differences.

7A list of over 100,000 international first names recommending assignment of records to one of three categories (e.g., “Christopher” = male, “Christina” = female, “Chris” = unclassified).

8We report the estimates of the Social Dollar in Table 4 with the demographics entering as main effects. We also
4.2.2 Is the Social Dollar driven by heavy spenders?

While it is true that DD estimation is commonly carried out at the group level— and within group analysis is usually not performed— it is important for us to ensure that the existence and magnitude of the social dollars is not driven by outliers. To check this, we divide our treatment and control groups into expenditure quartiles using their total purchases in T1 as the baseline. We then carry out a separate DD analysis for each quartile. As shown in Table 6, the social dollar is statistically significant for all four quartiles and has the strongest statistical significance level for the bottom three quartiles. In terms of magnitude, the largest coefficient estimates are for the middle two quartiles, approximating the “average” customer in our primary analysis sample. The statistically significant, but relatively weaker effect in the top quartile is driven by the variance in this high purchase volume group. It is also likely that the firm already accounts for a greater share of wallet for these heavy spending customers, thus leading to a smaller estimate for their social dollars. Overall, this analysis strongly suggests that our results are not driven by a change in expenditure for a small minority of customers, and especially not only by customers who already spent heavily with the firm prior to the launch of the community.

4.2.3 Exploring the temporal persistence of the Social Dollar

We next explore whether social dollars may arise because the online customer community is new and different but may then dissipate over time. In other words, we examine the possibility that the social dollar is driven by the novelty of the community. It is possible that customers respond positively to the community as soon as it is launched but then lose interest and revert to their normal (pre-community) purchase behavior. To test this, we estimated the social dollar using our DD analysis on a rolling quarter basis. We first focused on three months after the launch of the community to create a treatment time period and used three months before the launch of the community as the control time period. Thus, all activity in the first quarter after the launch of the community is contrasted with the first quarter before the launch of the community. We then extend the treatment time period to the second quarter, i.e., months four through six after the launch of the community and add the corresponding control time period prior to the launch of the community and ran analyses where we included interactions (treatment X demographics) and find that our results do not materially change. Detailed results are available from the authors on request.
the community. As shown in Table 7, the social dollar persists over time, with the significance of
the difference the weakest in the one quarter window but becoming very strong in the two to five
quarter windows. The quarterly change in social dollars over each prior quarter appears persistent
at $22.63 ($44.25 less $21.62), $20.41, $37.21 and $25.14 from the second through fifth quarter
in T2, respectively.

4.3 Dealing with self-selection

While an experimental design involving random assignment to conditions is ideal, many field ap-
plications of DD estimation observe non-random group assignment (e.g., Autor, 2003; Chevalier
and Mayzlin, 2006; Bronnenberg et al., 2010). Given that customers in our field data are similarly
not randomly assigned to treatment and control groups, it is possible that the two groups could
differ on unobservables that drove treatment group members to join the community. While the DD
model reduces these selection concerns by design,\textsuperscript{9} we pursue additional robustness checks to more
thoroughly assess a self-selection explanation for our results. Given that we observe the behavior of
consumers in T1 (before the community), differences on unobservables become an issue only if the
unobservables have a differential interaction with the treatment (the formation of the community).
For example, it could be that the customers in the treatment group were more engaged with the
firm in T1, as previous research has shown that customer communities have a much larger effect
on more engaged customers (Algesheimer et al., 2005). Our strategy first examines the possibility
of self-selection affecting our results via statistical analyses. We then exploit a feature of our data–
The fact that not all consumers join the community at the same time– to create a new control group
and check whether self-selection could indeed account for our results.

4.3.1 Selection on observables

Though customers in the treatment group did not appear to be markedly different from those in the
control group based on the demographics we observe (Table 2), it is still possible that observable
differences between customers in these two groups may drive differences in their purchases. To
account for this, Table 5 column 3 presents results of a “kernel matching estimator” (Heckman
\textsuperscript{9}Difference-in-differences designs have been used by other researchers in similar contexts to exploit the advantages
they provide via the elimination of individual-level differences across analysis groups/conditions (e.g., the elimination
The matching estimator computes the effect of a treatment on the treated by matching each treated person with an untreated person based on observable characteristics. The results show that the social dollar estimate is significant and 20.99% in magnitude.\footnote{We also replicated this result using a “nearest neighbor” matching estimator (Heckman et al., 1998a; Abadie and Imbens, 2006) which produced an estimate for the social dollar of 21.86%. The results were also substantively invariant to the number of nearest neighbors chosen for the matching process (results from matching based on 1, 5 and 20 nearest neighbors are available from the authors on request). The results were also insensitive to the choice of kernel.}

### 4.3.2 Selection on unobservables

In this section, we carry out two types of analyses to see if selection on unobservables can explain our results. In the first set of analyses, we carry out different (formal) statistical tests to check for this. In the second set of analyses, we exploit a unique feature of our data to set up a control group by using customers from the treatment group before they select into the treatment.

Table 5 column 4 presents results of a selection model based on the Heckman selection framework. We do not find evidence for selection as the selection correction term $\lambda$ is insignificant with a $p$-value $= 0.85$ (for more details on the implications of the significance of $\lambda$, see Heckman, 1979, p. 158 and Puhani, 2000, p. 55). An interesting observation is that $\rho$, the correlation coefficient between the selection equation and outcome equation errors, is negative at -0.0527. This suggests that a positive shock to the probability of selection reduces the treatment effect through a negative shock to the outcome (cf. Shaver, 1998; Lemke and Reed, 2001). In other words, the negative directional result above indicates that if there was selection, accounting for it is likely to increase the social dollar magnitude relative to our current estimate (which is based on there being no selection). The estimate of social dollars based on the Heckman selection framework is 32.82%, again supporting our main result as conservative.

We also employed a semi-parametric estimator (Choi, 1990; Rosenbaum and Rubin, 1983; Heckman et al., 1998a; Heckman et al., 1998b) that takes a weaker stance on the joint distribution of errors in the outcome and selection equations. Here, we use a probit regression model of the treatment indicator on instruments, to generate predicted probabilities of participation in treatment. A linear/quadratic function of these probabilities is used to generate a control function (Choi, 1990). The role of this control function is to proxy for the conditional mean of unobservable covariates, alleviating concerns with selection on the outcome equation parameters. Bootstrapped standard
errors are used to correct for error introduced by the estimated probabilities. From column 45 in Table 5, we see that the semi-parametric estimator yields a higher value of the social dollar magnitude (20.29%) than our main result (18.77%). However, this is more conservative than the estimate from the Heckman selection model, possibly due to the lack of assumption on the joint error term distribution.

To further test for the effect of unobserved selection on our estimates, we report two other analyses as part of Appendix B - the Rosenbaum bounds approach and the Relative Correlation Restriction (RCR) approach. The results from both of these analyses also suggest that selection based on unobservables is unlikely to have driven our results.

We now turn to a different approach to check for selection on unobservables. Recall that while the community formation appears as an exogenous shock to customers of the firm, customers are not required to join the community at the same time. In the five quarters after the community was launched, the proportion of our treatment group customers who joined was 44%, 17%, 14%, 14% and 11% in Q6, Q7, Q8, Q9 and Q10 respectively. In other words, the majority of the customers (56%) in our (right-truncated) sample joined after the first quarter.\footnote{\textsuperscript{11}}

Once the community was available to customers, if there is a differential interaction between the availability of the customer community and the unobservable attribute(s), then a change in transaction behavior should manifest itself even if a (future) member had not yet joined the community. We present a series of analyses in which we show that social dollars are not driven by \textit{whether} some customers join the community but that they occur only once they choose to join the community. In other words, that it is the act of joining and not the mere availability of the treatment that impacts transaction behavior. In fact, the temporal joining data discussed above already suggest that the availability of the customer community was not enough to sort customers on an unobservable attribute. If this had been the case, a large majority of customers would have joined the community immediately (in Q6).

In the first analysis, we group customers who join the customer community within specific quarterly time intervals into cohorts and contrast cohort behavior across time to see if this impacts the size of the social dollar. A challenge here, however, is that the control group definition for these

\footnote{\textsuperscript{11} As noted earlier, the dispersion in the community join date also suggests that our main social dollars estimate ($127.01 or 19% of T2 expenditure) is conservative as many customers did not "benefit" from the community until later in T2.}
cohorts is not obvious. This is because our current control group by definition consists of people who do not join the community during the 15 month period of its operation that we observe. We therefore use a different strategy to test for the possibility that differences in the time period in which customers join the community impacts the social dollar. We first consider all customers who join the community in the first quarter after the formation of the community (Q6) as our treatment group cohort. The control group for this cohort includes all customers in the treatment group who did not join the community until after the first quarter of its operation; that is, they joined the community between Q7 and Q10, inclusive (Table 8 Panel B, column 1). If our prediction (that behavior changes when rather than whether they join) is correct, until these customers join the community, we should be able to treat them as control group customers. This analysis can be seen in the same spirit as the falsification analysis carried out in Goldfarb and Tucker (2011).

We perform the DD analysis for pre (T1) and post (T2) periods of equivalent length limited by the duration of the T2 period for which the treatment cohort were community members. Analysis for subsequent “join cohorts” proceeds similarly (Table 8 Panel B, columns 2-4). As shown in the table, the social dollar effect is positive and significant for all four cohorts. This suggests that even when we restrict our analysis to all customers who possessed the (presumed) unobservable attributes that interacted with the treatment, and divided them into treatment and control groups as above, the social dollar effect is present and significant. Thus, it is not whether they join the community but when they do that matters.

We next focus on a second cohort-based analysis of customers who join the customer community. However, here we compare behavior within cohorts by comparing a cohort’s quarterly expenditure post joining the community with their own quarterly expenditure pre joining the community. Specifically, we compare the average T1 quarterly revenue for customers who ended up joining the community in a specific quarter with their average revenue in each quarter after becoming members. For example, for the cohort of customers who joined in the first quarter after the community launch (Q6, column 1 in Table 8 Panel A) we observe their revenue in each of four T2 quarters after the join quarter, and compare each of these four quarters after joining to the T1 quarterly mean. The analysis proceeds similarly for those who joined in later quarters until those who joined in Q9 (column 4), for whom we only observe one additional quarter in T2 after this time (Q10). As shown in Table 8 Panel A, nine out of the ten quarterly comparisons in the table
are statistically significant. These results further support the argument that there is a significant change in the behavior of these customers based on when (rather than whether) they join the community.

In the above analysis, note that we do not have a control group. One way to reduce the impact of not having a control group would be to shorten the window before and after joining the community in a “regression discontinuity” style analysis. For this analysis, we use the day of joining as the “origin” and contrast mean expenditure before joining with mean expenditure just after joining. The shorter the temporal window on each side of the treatment, the less likely that factors besides the treatment will affect outcomes (Imbens and Lemieux, 2008; Hartmann et al., 2011). We examine behavior in the shortest possible window we observe - a day - as well as two, three, four, five, six and seven days.\footnote{The sample size changes for each window as we need to drop treatment group customers for whom the end of the “after” window exceeds the end date of our data.} As expected (Table 9), the mean expenditure increases (for both pre and post launch) as the duration of the window gets longer. We find that the post-launch mean expenditure is higher than the pre-launch expenditure and that the difference is statistically significant for all of the windows we consider.

Overall, the analyses presented above suggest that it is unlikely that a differential interaction between unobservable attributes and the treatment was a material driver of our findings. While it is impossible for us to rule out the effect of all possible unobservables with certainty, it is unlikely, given our robustness analyses, that they played a role in our estimation of social dollars.

4.4 Assessing the validity of an alternative explanation for the Social Dollar

A factor that could potentially be driving the social dollars estimate is a differential temporal trend in expenditure between the treatment and control groups in T1. For example, it could be the case that while the total expenditure in T1 for the treatment and control groups is not statistically different (as shown in Table 1), there is an increasing trend in the expenditure for the treatment group relative to the control within the T1 period. With the passage of time, this trend could widen the gap between the two groups – a difference that could improperly be ascribed to customers joining the online community. To test for this possibility, we perform across-group trend analysis of total revenue for the treatment and control groups. The statistical analysis we carry out
is a mixed-effects model estimated by restricted maximum likelihood (Verbeke and Molenberghs, 2000; Wallace and Green, 2001). This approach is preferred over traditional repeated measures using GLM methods as it allows for a more accurate depiction of serial correlation and correlated error structure, and can accommodate unbalanced group sizes. This model is represented as

\[ R_{iq} = X_i \beta + Z_q u + (X_i \beta Z_q u) + e_{iq} \]  

(2)

where \( R_{iq} \) is a 5 x 1 vector representing the total revenue of customer \( i \) in quarter \( q \) within the five quarters of T1, predicted by the fixed component of analysis group (\( X_i \beta \)), the random time component (\( Z_q u \)) and their interaction (\( X_i \beta Z_q u \)). To control for expected serial correlation and correlated error structure in the within-customer revenue trend we allow an AR(1) process on the error term (\( e_{iq} \)). The interaction term (\( X_i \beta Z_q u \)) - that would indicate a difference across comparison groups in the linear slope of the purchase trend across the five quarters of T1 is non-significant (t-test = 0.50, \( p = 0.62 \)). Given the quarterly purchase trend approximates an inverted-U shape (see Table 2), we also specified a model adding a quadratic main effect and interaction for time. The quadratic interaction term was also non-significant (t-test = 1.47, \( p = 0.14 \)), failing to support a difference in curvilinear trends.\(^{13}\) As an additional test, in Table 2, we present simple group mean comparisons by quarter within the T1 period, which also supports non-significant differences in each of the five quarters prior to the community launch.

These results allow us to rule out the possibility that any differences that we find between the treatment and control groups after the launch of the community are driven by differential trends in behavior prior to the community launch.

5 Moderating the Social Dollar

In the previous sections, we have documented the existence and magnitude of social dollars in the context we examine and demonstrated that they are robust to self-selection concerns and an alternative explanation. We now turn our attention to assessing mechanisms that should be

\(^{13}\) The results were also identical using a traditional GLM repeated measures model for both the linear (Huynh-Feldt adjusted \( F(3.5, 32355) = 0.81, p = 0.51 \)) and quadratic interaction terms (Huynh-Feldt adjusted \( F(2.0, 18740) = 1.76, p = 0.17 \)). We were also able to replicate this finding at the individual level (as opposed to the group level) using a probit model. Details on these analyses are available on request from the authors.
related to the observed increase in expenditure by customer community participants. Specifically, we leverage prior theoretical or survey-based research on why community participation leads to consumer and firm value in order to identify factors that should be related to the size of the economic outcome we observe.

The literature on customer communities specifically, and online, virtual and brand communities more broadly, proposes a variety of reasons why and how customers and/or firms accrue benefits from these communities. We focus on two of the most commonly examined benefits – social and informational benefits (Mathwick et al., 2008). Social benefits are the intangible benefits said to be derived from community activities engaged in by their members (e.g., content contribution). Informational benefits accrue from the enhanced information community participants are able to obtain regarding the brand(s) or product(s) of common interest to the community. While Mathwick et al. (2008) are primarily concerned with how these benefits enhance value to customers, they and other researchers also point to increased customer loyalty and commitment, leading to incremental economic gains as downstream consequences for the firm (Algesheimer et al., 2005; Balasubramanian and Mahajan, 2001; Porter and Donthu, 2008). Examples of related conceptualizations of these two benefits includes Dholakia et al.’s (2004) articulation of the purposive value obtained in virtual communities as utility gained through (a) contributing brand or product information to the community, and (b) obtaining brand or product information in the community, as well as Balasubramanian and Mahajan’s (2001) description of utility functions that are (a) derived from the satisfaction that others in a customer community will consume their own content contributions, and (b) the participant’s consumption of information posted by other community participants. In the present research, we contribute a first empirical test of the economic consequences of these two constructs, which may also shed light on the relative strength of these benefits on the consequences of customer community participation.

Social Benefit: Poster/Lurker. Schlosser (2005) describes two types of consumer participants in Internet-based transmissions of product information: posters, who actively share their product experiences online, and lurkers, who read others’ postings without communicating themselves. Lurkers are generally believed to represent the majority of people in online customer communities, while a small minority of posters generate the content (McWilliam, 2000). Participants who post content have been said to gain “approval utility” from the benefit of others consuming and approving
their contributions to the community, with an expected consequence of increased expenditure with the firm (Balasubramanian and Mahajan, 2001). Participants who commit more conspicuous public behaviors in a community (i.e., posting) are expected to be more engaged with the focal brand or product whether the community is offline/"real" (Algesheimer et al., 2005) or online/virtual (Kozinets, 1999). Further, a survey-based investigation of online customer communities found that the act of content contribution by community participants was positively related to member brand commitment (Jang et al., 2008). We contribute an empirical test of the economic consequences of this proposition, predicting that being a lurker (poster) will negatively (positively) moderate the social dollar.

**Informational Benefit: Preference Heterogeneity.** Participation in the online customer community we observe should offer consumers unique informational benefits due to the nature of product information available within (versus outside) the community. Most of the product opinions shared by consumers online are offered by anonymous or socially distant sources, giving the consumer little means by which they can assess the personal relevance of the recommendation (Dellarocas, 2003; Ma and Agarwal, 2007). In contrast, both offline and online customer communities are by definition organized around the shared interests of like-minded individuals (Algesheimer et al., 2010; Bagozzi and Dholakia, 2002; Porter and Donthu, 2008) who are more demographically and psychologically similar to each other (Dholakia and Vianello, 2009) Customer communities should thus tend to exhibit heightened homogeneity in individual attitudes; that is, homophily (McPherson et al., 2001; Watts et al., 2002). Consumers expect that because homogeneous sources of product information are more likely to share the consumer’s own attitudes and preferences, they are more diagnostic to the information task and are more likely to influence purchase intentions (Brock, 1965; Eagly et al., 1978; Gershoff et al., 2001).

While we do not observe the extent to which customers share attitudes towards products, we do observe the nature (i.e., category or genre) of some of the products they purchase. To link product attributes to the shared interests or like-mindedness (attitudinal homogeneity) of community participants, we rely on Feick and Higie (1992), who empirically demonstrate that homogeneous sources of product information are particularly persuasive for products with more heterogeneous preference structures (e.g., restaurants) than for products that can be more objectively assessed (e.g., personal computers). In short, “like-minded” people are better sources of information for
products for which subjective tastes or preferences vary. If there is an informational benefit to gaining product information from like-minded participants in a customer community, we should observe a disproportionate benefit (i.e., increased expenditure) for products that are higher in preference heterogeneity. Therefore, we predict that the effect of customer community participation on purchases (the social dollar) will be moderated by the extent to which the products purchased are perceived to hold greater preference heterogeneity.

To sum, we expect that the social dollar will be moderated by the social benefit of community activity engaged in by the participant, which we operationalize as whether the participant is a poster or lurker. We further predict that the unique informational benefit of product information sourced from a community of shared interests, which we operationalize as moderation by the preference heterogeneity of products purchased, should be linked to the magnitude of the social dollar. Lastly, while we are unaware of prior research proposing a compounding or interactive effect of community benefits, we assess an additional interaction effect whereby lurkers who are less active in community activities (social benefit) may recognize a diminished informational benefit in community participation due to their weaker social participation in the community.

5.1 Data and Model

To generate a measure of product preference heterogeneity, we restrict our analysis to the purchase of books (i.e., excluding music, movies and other products) as the sub-category classification data that is needed for this analysis is only available for this category. Books are the firm’s largest product category, representing 77.3% of treatment and 76.9% of control group expenditure in the 15 month pre-period (t-test = 0.69, p > 0.45). Five hundred and sixteen Amazon Mechanical Turkers residing in the United States with a performance rating of 95% or higher were each paid a nominal fee to rate the perceived preference heterogeneity of three randomly selected book sub-categories out of 53 available from the firm. This resulted in approximately 30 ratings per book sub-category. Participants were asked to indicate the extent to which they agreed with five seven-point scale items measuring product-level preference heterogeneity (cf. Feick and Higie, 1992). The five scale items were - (1) “Most people want the same things from [sub-category] books,” (2) “Personal tastes and preferences are not important in how people choose [sub-category] books,” (3) “People look for different things when it comes to [sub-category] books,” (4) “Whether people will enjoy a particular [sub-category] book is very much an individual, personal matter” and (5) “People can generally agree on what makes a [sub-category] book good or bad.”

14 Scale reliability was
above the desired threshold ($\alpha = 0.74$) and the items converged on a single factor solution. The mean ranking of book categories was 4.64 on a seven-point scale, with a standard deviation of 0.47.\footnote{A detailed list of book sub-categories and their ratings is available from the authors on request.} The top three book sub-categories in terms of preference heterogeneity were art, fiction and poetry books. The bottom three categories were math textbooks, general reference and language (instruction) books. Sub-categories closest to the preference heterogeneity index mean were game-related, general psychology and true crime books. These results show face validity as the preferences of customers are likely to be more heterogeneous for a sub-category such as poetry but exhibit much less variance for a sub-category like math textbooks. The analysis that follows incorporates each product sub-category’s preference heterogeneity index as a normalized continuous measure. We also carry out a robustness check using a median split on the index to classify sub-categories into high preference heterogeneity and low preference heterogeneity to report dichotomous terms for both moderators.

Consistent with prior research (Schlosser, 2005), our criterion for lurkers is that the community participant has not posted any content at the community website during the observation period. Based on this definition, the number of lurkers in the book purchaser data used for this analysis is 3,326, representing 72.2% of the sample. We use a dummy variable to capture lurkers versus posters (where lurker = 1 and poster = 0). The moderating effect of the social benefit of community participation is therefore the coefficient of the dummy variable.

Our analysis strategy for understanding the moderation of the social dollar by informational (product preference heterogeneity; $HETE$) and social (poster/lurker; $LURK$) benefits is as follows. We incorporate each of these terms as a moderator of the treatment-time [$I_g \times I_t$] interaction in the basic model (Equation 1), resulting in a three-way term indicating the extent to which the third-level moderator enhances or attenuates the social dollar effect. This has been described as a difference-in-difference-in-differences approach, and can be specified as below:

\[
R_{igt} = \beta_1 I_g + \beta_2 I_t + \beta_3 I_g I_t + \beta_4 (I_g I_t \cdot I_{LURK}) + \beta_5 (I_g I_t \cdot HETE) + \\
\beta_6 (I_g I_t \cdot I_{LURK} \cdot HETE) + \beta_7 X_{ig} + \varepsilon_{igt} \tag{3}
\]
The parameters of interest are the coefficient of the three-way interaction term including the poster/lurker dummy \( I_{LURK} \), the coefficient of the three-way interaction term including the product preference heterogeneity measure \( HETE \) and the coefficient of the four-way interaction including both \( HETE \) and \( I_{LURK} \).

### 5.2 Results

We estimate three models (Table 10). Model 1 replicates our main result for the book category alone, with the coefficient of the \([Treat \times Time]\) term being positive and significant, representing a 25.3% estimate of the social dollar when limited to the books category. Model 2 is the main model for this analysis, containing the two three-way and the one four-way interaction term described above. The \([Treat \times Time \times LURK]\) interaction is significant and negative, the \([Treat \times Time \times HETE]\) interaction is positive and significant and the \([Treat \times Time \times LURK \times HETE]\) interaction is negative and significant. As noted above, in Model 3, as a robustness check, we classify subcategories into high preference heterogeneity and low preference heterogeneity using a median split on the index and re-estimate Model 2. The results from Model 3 replicate the results in Model 2.

Overall, this analysis provides support for the social and informational benefits of customer communities as mechanisms related to the social dollars effect we observe. Notably, we find moderation by informational benefits \( HETE \) is weaker than the effect of social benefits \( LURK \), at least within this constrained sample (book products). This can be most easily compared by the coefficients of the two interaction terms in Model 3 as they are both coded as dummy variables. The four-way interaction indicates that the social benefit is more crucial when product preference heterogeneity is high, suggesting that active contributors of user-generated content to the community benefit more from the “like-minded” product information available within the community. These results are novel in the sense that they provide behavioral evidence for the theoretical predictions in the extant customer community literature. From a more practical point of view, our results show an objective and quantifiable link between customer community engagement (measured via participation or the lack thereof) and economic outcomes.
6 Managerial Implications

Our results suggest that the firm can use the insights on how customer community benefits moderate the social dollars to improve community design and interaction mechanisms. For example, as revealed in our analysis in § 5, the difference in posting versus lurking in the community is linked to a significant positive lift in an individual’s contribution of social dollars. A subsequent regression analysis revealed that there is a positive and significant marginal effect of each unit of any kind of posting on consumer expenditure (restricting our analysis to posters only). We find that that this effect size (at the mean level of posting) accounts for about 3% of the total expenditures post joining the customer community.\textsuperscript{16} Thus, before even accounting for the possible impact of a customer’s posts on other community members (i.e., word-of-mouth influence), the firm has evidence supporting the pursuit of tactics to encourage posting in the community to enhance the return on their community investment.

Such tactics could include designing the community platform so as to highlight sub-categories that have greater preference heterogeneity, in order to better leverage the unique informational benefit of customer communities. An example of such a design feature could be to invite authors who write books in such sub-categories to participate in the community in order to increase the amount of interaction and consequently, the availability of information and potential for information exchange. The firm may also consider pursuing automated mechanisms by which community members would be more likely to see the user-generated content of posters with similar preference structures, enhancing the informational benefit of product recommendations from like-minded community members. Design features that have the highest marginal impact on posting behavior could be explored via experiments or through the common industry practice of A/B testing.

Besides the direct economic benefits to the firm from setting up the online customer community, there are also considerable indirect benefits in terms of the information the community generates for the firm. For example, the data produced as a by-product of the customer community offers a more complete picture of each customer’s preferences and behavior by integrating pre-purchase, purchase

\textsuperscript{16}For the sample of posters (treatment group minus lurkers), we regressed the post-period quarterly expenditure on posting activity (represented as the total count of all user-generated content) in the previous quarter and other controls (including relevant demographics and pre-period expenditure to control for heterogeneity). The marginal effect of posting was positive and significant and at the mean level of posting per quarter (14.3), it represents, on average, 2.89% of all post-period expenditure.
event and post-purchase activities (e.g., community interactions and purchases). This represents an informational boon for customer relationship management and other life-cycle based marketing strategies. The community interactions content can further be mined to identify products that are trending in terms of social relevance, to identify those who discuss them (e.g., opinion leaders or special-interest groups), and to understand how they talk about them (e.g., attribute or lifestyle-related language). This insight can subsequently be used to optimize marketing mix decisions. The firm providing the data for the present analysis also reports that the massive quantity of user-generated content produced by community members strongly improves the firm’s position in organic search results (i.e., the website appears before competitors when its product offering is sought on major search engines). While it is evident that attempting to host online customer communities on third-party websites such as Facebook provides reach to a broader audience (FanPageList, 2013), this strategy does not offer the same level of access and control over customer interaction management and data offered by a firm-sponsored online customer community, nor is the third-party’s social interaction data commonly available to the firm in a manner that can be easily linked to customer-level purchase behavior.

In terms of extending our results to other product categories, the preference heterogeneity finding suggests a possible boundary condition for the social dollar. Firms that sell products that may be evaluated more objectively (e.g., personal computers, kitchen appliances) rather than goods high in preference heterogeneity (e.g., clothing, music, travel) may be less likely to recognize positive economic returns for their investment in a customer community.

Finally, from the firm’s perspective, an important question is whether the launch of a customer community results in increased customer expenditure and profits, especially relative to the investment made in terms of the community’s development and ongoing operations. In order to quantify this, we approached the firm and were able to obtain estimates of community development and operating costs. Based on the estimated social dollars, community costs, and firm-level margin percentages available in public financial statements we estimate that the firm achieves break-even on its investment when 33,000 of its current customers (our conservative restriction case) sign up for the community. Given that the firm acquired over 260,000 members within the first fifteen months after community launch, this was a very profitable investment for the firm, especially as

\[\text{Due to confidentiality reasons, we are unable to reveal these figures.}\]
this number is comprised of a mix of both current and newly-acquired customers.

7 Conclusion

Our paper adds to the small, but growing literature on the economic impact of online customer communities. While there is much theoretical and survey-based research available on the motivations of consumers who participate in such communities, there is a paucity of research that uses behavioral (market) data to quantify the possible economic benefits to firms that set up these communities. Using a novel dataset from a firm that operates such a community, we are able to quantify the incremental expenditure resulting from customer engagement in a community. The availability of customer expenditure both before and after the formation of the customer community allows us to create treatment and control groups, helping to rule out multiple selection issues. We find that social dollars represent at least 18% of revenue once customers join the community. These social dollars arise primarily via more frequent orders with the firm rather than increased shopping basket sizes.

As is important for studies that leverage natural events, we carry out a series of robustness analyses to check that our estimate of social dollars can be attributed to customer membership in the community. We find that our estimate is robust to the novelty effect, to differences in expenditure levels across customers before they join the community, to temporal trends between the treatment and control groups before they join the community, and to both observable and unobservable attributes that characterize each group. Furthermore, the social dollar persists over time and exists in both the online and offline channels.

We then examined theoretically-supported moderators of the social dollar. We find that participants who partake more in the social benefit of communities by posting user-generated content tend to exhibit higher social dollars relative to participants who are less actively engaged in content-sharing in the community (lurkers). Our results also suggest that customers obtain an informational benefit from joining the customer community. In our setting, this manifests itself in positive moderation of the social dollar as products increase in preference heterogeneity. The social benefit and the information benefit also interact, further bolstering the social dollar for active community participants and more heterogeneous products, respectively. Finally, we are able to document the
direct benefit of setting up the community to the firm by reporting the small number of customer participants required to earn a return on this investment. As we note, there are also many indirect benefits that may be reaped by the firm-operator of an online customer community such as the one we observe in the present research.

Our analysis suffers from some limitations, primarily due to our data. First, we only examine consumer behavior in a small range of experiential goods categories. Second, our data extends to only fifteen months after the formation of the community, restricting our ability to investigate even longer-term effects on customers and the firm. Third, and as discussed earlier, since we only observe customer-level purchase events in the firm’s offline (retail) channel for customers with a loyalty card (as the firm otherwise had no customer-level identifier for in-store purchases), our ability to extrapolate the results to the firm’s entire customer base is limited. That said, our supplementary analysis of the social dollar for customers without a loyalty card in the online channel (Appendix A) reveals that the effect for these non-loyalty customers was even stronger than that of our primary analysis sample. Fourth, given that we do not assign customers to treatment and control groups randomly, we cannot rule out the effect of unobservable attributes (i.e., self-selection) with perfect certainty, although multiple analyses suggest that this is highly unlikely to be an issue. Finally, given that we do not observe customers who also shop for the product categories offered by the firm at its competitors, we cannot pinpoint the source of social dollars precisely (i.e., market growth versus store-switching). We hope that future work might address these limitations.
Appendix A: Alternative Sampling Plans and the Social Dollar

Samples with no restriction on loyalty cards and/or entry and exit

In this analysis, we first relax the requirement that analysis sample customers must be loyalty card holders. To do this, we restrict our analysis to online channel shoppers for whom there is no way to track offline (retail) sales via loyalty cards. The online channel represents approximately 10% of customer expenditure with the firm during the observation period. For online channel shoppers who are not loyalty card holders, the estimate of social dollars is $55.37 (SE = 16.35, \( p < 0.001 \)) or 20.81% of post-period expenditure if we require at least one transaction in the pre- and post-periods. If we relax the entry/exit restriction (for the online channel only), the estimate of social dollars is $78.43 (SE = 5.75, \( p < 0.001 \)) or 46.52% of post-period expenditure. Next, we relax the entry/exit restriction for the loyalty card sample that enables observation of both online and offline channel sales. In this case, the estimate of the social dollar is $170.96 (SE = 22.09, \( p < 0.001 \)) or 28.85% of post-period expenditure. The much larger magnitude of the latter two numbers is not surprising as these capture the expenditure of customers who transacted in the post-period but not in the pre-period (these could be new customers or current customers that did not transact in the pre-period). Details on these analyses are available on request from the authors. Overall, these results suggest that our primary analysis sampling strategy (loyalty card holders AND no entry/exit [at least one transaction each in the pre- and post-periods]) provides the most conservative estimate of the social dollar (18.77%).

Equally sized treatment and control samples

Our objective here is to test whether size differences in the treatment and control groups may be impacting our results. As mentioned previously, the firm’s research motivations demanded the extraction of a larger treatment than control group. To control for differences in sample size, we randomly sampled 1,255 customers from the primary analysis treatment group (\( n = 7,909 \)) ten times (with replacement) to precisely match the size of the primary analysis control group (\( n = 1,255 \)). We then ran our basic DD analysis (Equation 1) on each reduced sample. As shown in Table A.1, the statistical significance and magnitude of the social dollar effect is robust to the group sample size difference.
Appendix B: Further Tests for Selection on Unobservables

Rosenbaum Bounds Approach to Control for Selection

To assess the magnitude of selection on unobservables needed to nullify our main treatment effect, we report the Rosenbaum bounds (Rosenbaum, 2002). Rosenbaum bounds convey important information about the level of uncertainty contained in matching estimators by showing how large the influence of a confounding variable must be to undermine the conclusions of a matching analysis (Rosenbaum, 2002; DiPrete and Gangl, 2004). Rosenbaum bounds for unobserved selection are generated by matching of data from the treatment and control group based on a propensity score. Let $P_T$ be the propensity of a treatment user to select into the community, which is specified as a function of observable covariates as:

$$P_T = X_T \beta + u_T$$

(4)

for all $T = (1 \ldots 7909)$ Let the corresponding term for the control user be $P_C$. The $u_T$ term represents the effect of unobservables on the decision to select into the community. If unobservables did not play a role, $u_T = 0$. The odds-ratio of treatment and control users, in the absence of unobserved selection is 1.

$$\frac{P_T (1 - P_C)}{(1 - P_T) P_C} = 1$$

The typical effect of unobserved selection is to increase the odds-ratio to >1 i.e., the propensity score for treatment users to select into the community is higher than for the control users conditional on all the observables. Rosenbaum bounds place a constraint on the effects of unobserved selection to change the odds-ratio by a known amount ($\Gamma$) such that

$$\frac{1}{\Gamma} \leq \frac{P_T (1 - P_C)}{(1 - P_T) P_C} \leq \Gamma$$

$$\Rightarrow \frac{1}{(1 + \Gamma)} \leq \frac{P_T}{(P_T + P_C)} \leq \frac{\Gamma}{(1 + \Gamma)} \text{ for all } X_T = X_C.$$  

(5)

$\Gamma$ may be interpreted roughly as the bias-level introduced in our treatment effect due to unobserved selection or the degree of departure from randomization of the treatment (Rosenbaum, 2005). This sensitivity parameter is the maximum odds-ratio of unobserved treatment probabilities among pairs.
of cases that have been matched on observed characteristics. If $\Gamma = 1$, the odds-ratio of treatment for two units selected from the sample (matched on observables) is the same and the study is free of bias due to unobservables. If $\Gamma = 2$, then two units selected from each group in the sample (with the same $X^\prime$s) could differ in their odds of receiving treatment by as much as a factor of 2. Therefore one unit would be twice as likely to receive the treatment as the other unit owing to unobserved selection. In a sensitivity analysis, $\Gamma$ can be varied to measure the degree of departure from a study that is free of bias (e.g., experimental settings that have perfect randomization). We can use different values of $\Gamma$ to show how inferences might change if hidden bias were present. We test the effect of changing $\Gamma$ (for a range of $\Gamma > 1$) on the treatment effect to assess the extent to which unobserved selection may bias our results. We find that our results are positive and significant up to $\Gamma = 1.6$. This means that a unit selected from our sample must be 1.5 times more likely (in terms of the odds-ratio) to select into treatment based only on unobservable factors in order to nullify our effects. The advantage of Rosenbaum bounds is the ability to use standard inference techniques to assess to what extent unobserved selection may impact the treatment effect. A Wilcoxon signed rank statistic ($T$) is used for rejecting the null that the outcome is identical between matched observations drawn from two separate groups: treatment and control. Here, under the null, the average difference in expenditure (before and after the community launch) for treatment users would be equal to the average difference for control users. So long as we have a sufficiently high number of observations to match treatment and control group users (based on the propensity score), the asymptotic approximation of the test statistic to a $N(0,1)$ distribution allows us to use a p-value for “$T$” under the null. The interested reader is referred to Rosenbaum (2002) for further details on the applicability of the asymptotic approximation in applied settings. $\Gamma$ governs the extent to which the probability of treatment assignment differs from the Wilcoxon null distributions that predict a probability of 0.5 (conditional on observable covariates). The ranking of each of the $N = 1...7909$ treatment group users is now included with probability for the upper bound and lower bound distributions of $\Gamma/(1+\Gamma)$ and $1/(1 + \Gamma)$ respectively - as shown by the above expression in Equation 5. Given the value of the T-statistic calculated from the data, we can calculate the probability that this value of $T$ was drawn from null distributions of the upper and lower bounds respectively (columns sig+ and sig- below). As long as these p-values reject the null hypothesis that the difference in spend between treatment and (matched) control group
users is zero, our results are robust to unobserved selection up to a given level of $\Gamma$. Columns representing $t$-hat+ and $t$-hat- (representing Hodges-Lehman upper/lower bounds) provide point estimates of the average effect of the community on the treatment group users for a level of $\Gamma$ and the corresponding confidence intervals are presented in columns 6 and 7 in Table A.2 below.

From the table, we can see that the social dollars estimate is positive and significant (at the 5% level) for $\Gamma \leq 1.6$ (our cutoff described above). Note that our OLS estimate of the social dollar also falls within the range of the Rosenbaum bounds in this specified range of $\Gamma$. Finally, while the Rosenbaum bounds provide an assessment of the degree of correlation needed between the focal covariate (the treatment indicator in our case) and an unobserved confounder to drop the treatment effect to non-significance, they do not provide precise bounds on the estimates of the treatment effect for different levels of unobserved correlation. So as a next step we adopt a recently proposed approach (Krauth, 2011) that aims to estimate bounds on the treatment effect.

Relative Correlation Restriction Approach to Control for Selection

Krauth (2011) generalizes the approach proposed in Altonji et al. (2005). The focus here is to estimate bounds on the treatment effects in the presence of unobserved selection. The approach has typically been applied in labor economics. Krauth (2011, 2007) essentially proposes an estimator to identify bounds on the treatment effect for varying levels of correlation between an unobservable confounder and the endogenous variable as a ratio of the correlation between the endogenous variable and the control variables employed in the study.

Consider a regression specification:

$$\Delta Y = \theta_0 Z + x\beta + u$$

This is the outcome equation, similar in structure to our DD model specification but with change in expenditure (between T1 and T2) as the dependent variable. Here, $z$ is the treatment indicator and the $x\beta$ represent control variables that are also modeled to determine treatment assignment. Note that the Krauth model assumes a specification that includes control variables (in our case demographics) in the outcome equation.

The OLS formulation imposes a strict assumption that $z \perp u$ or $corr(z, u) = 0$. The Krauth
approach (the relative correlation restriction model) makes a weaker assumption than that of the
OLS model by allowing this correlation to range between 0 and 1 and comparing it with \( \text{corr}(z, x\beta) \).
Thus, consider the expression for \( \lambda_0 \) given by:

$$
\lambda_0 = \frac{\text{corr}(z, u)}{\text{corr}(z, x\beta)} \in \text{some set } \Lambda \text{ of lower/upper bounds on this correlation}
$$

Table A.3 below shows the bounds on the estimate of the social dollar for increasing values of
lambda. We find that our Social Dollar effect could be nullified (due to the presence of unobserv-
ables) for lambda greater than 3.23 (=\( \lambda_0 \)). Krauth notes that high values of \( \lambda_0 \) imply that the
estimate of treatment effect from a differences-in-differences model is more robust to departures
from perfect randomization. Krauth (2011) provides results of the applications of his model to test
the robustness of findings reported in previously published work (pp. 22-28). Based on the values
of \( \lambda_0 \) that Krauth reports, the value of \( \lambda_0 \) for our dataset falls well within the acceptable range and
is much higher than the values of \( \lambda_0 \) (\( \lambda_0 = 0.23 \)) that may imply the possibility of large biases in the
treatment effect introduced by any unobservable confounders. This gives us additional confidence
that our results are not driven by unobservables.

<table>
<thead>
<tr>
<th>Sample</th>
<th>( \beta_3 ) (Social Dollar)</th>
<th>% of T2</th>
<th>Sample</th>
<th>( \beta_3 ) (Social Dollar)</th>
<th>% of T2</th>
</tr>
</thead>
<tbody>
<tr>
<td>1</td>
<td>122.13***</td>
<td>18%</td>
<td>6</td>
<td>155.76***</td>
<td>22%</td>
</tr>
<tr>
<td></td>
<td>(19.34)</td>
<td></td>
<td></td>
<td>(22.15)</td>
<td></td>
</tr>
<tr>
<td>2</td>
<td>114.30***</td>
<td>17%</td>
<td>7</td>
<td>115.29***</td>
<td>18%</td>
</tr>
<tr>
<td></td>
<td>(22.38)</td>
<td></td>
<td></td>
<td>(20.05)</td>
<td></td>
</tr>
<tr>
<td>3</td>
<td>140.57***</td>
<td>21%</td>
<td>8</td>
<td>120.53***</td>
<td>19%</td>
</tr>
<tr>
<td></td>
<td>(20.55)</td>
<td></td>
<td></td>
<td>(20.38)</td>
<td></td>
</tr>
<tr>
<td>4</td>
<td>130.38***</td>
<td>19%</td>
<td>9</td>
<td>114.73***</td>
<td>18%</td>
</tr>
<tr>
<td></td>
<td>(21.70)</td>
<td></td>
<td></td>
<td>(18.78)</td>
<td></td>
</tr>
<tr>
<td>5</td>
<td>163.08***</td>
<td>23%</td>
<td>10</td>
<td>129.54***</td>
<td>21%</td>
</tr>
<tr>
<td></td>
<td>(22.29)</td>
<td></td>
<td></td>
<td>(19.24)</td>
<td></td>
</tr>
</tbody>
</table>

Standard errors appear in parentheses below estimates.  

*** \( p<0.001 \)

Table A.1: Reduced Sample DD
### Table A.2: Rosenbaum bounds

<table>
<thead>
<tr>
<th>Relative Correlation Restriction (Λ)</th>
<th>Bounds on the estimate of Social Dollars</th>
<th>95% CI</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>$\hat{\theta}_L(Λ), \hat{\theta}_H(Λ)$</td>
<td></td>
</tr>
<tr>
<td>{0.00}</td>
<td>146.06</td>
<td>(116.31, 175.81)</td>
</tr>
<tr>
<td>[0.00, 0.10]</td>
<td>[142.28, 146.06]</td>
<td>(111.28, 175.81)</td>
</tr>
<tr>
<td>[0.00, 0.20]</td>
<td>[138.46, 146.06]</td>
<td>(105.28, 178.81)</td>
</tr>
<tr>
<td>[0.00, 0.30]</td>
<td>[134.59, 146.06]</td>
<td>(98.45, 175.81)</td>
</tr>
<tr>
<td>[0.00, 0.40]</td>
<td>[130.69, 146.06]</td>
<td>(90.93, 175.81)</td>
</tr>
<tr>
<td>[0.00, 0.50]</td>
<td>[126.75, 146.06]</td>
<td>(82.87, 175.81)</td>
</tr>
<tr>
<td>[0.00, 1.00]</td>
<td>[106.36, 146.06]</td>
<td>(37.41, 175.81)</td>
</tr>
<tr>
<td>[0.00, 2.00]</td>
<td>[62.04, 146.06]</td>
<td>(-67.46, 175.81)</td>
</tr>
<tr>
<td>[0.00, 3.00]</td>
<td>[12.32, 146.06]</td>
<td>(-185.72, 175.81)</td>
</tr>
<tr>
<td>[0.00, 4.00]</td>
<td>[-43.63, 146.06]</td>
<td>(-317.56, 175.81)</td>
</tr>
<tr>
<td>[0.00, 5.00]</td>
<td>[-106.89, 146.06]</td>
<td>(-464.87, 175.81)</td>
</tr>
</tbody>
</table>

$\lambda_0 = 3.23$

Table A.3: Relative Correlation Restriction Model
References


Eddy, Nathan. 2012. Social media revenue nears $17 billion in


Paul, Sonia. 2012. CEOs are finally warming up to social media. URL http://mashable.com/2012/05/24/ibm-ceo-social-media/.


### Tables

#### Table 1: Purchase Statistics by Group - Both Periods

<table>
<thead>
<tr>
<th></th>
<th>Control</th>
<th>Treatment</th>
<th>t-stat</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>T1: 15 Months Pre</strong></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Total Spend</td>
<td>511.38</td>
<td>489.73</td>
<td>-1.12</td>
</tr>
<tr>
<td>Average Purchase</td>
<td>49.82</td>
<td>46.24</td>
<td>-3.61***</td>
</tr>
<tr>
<td>Purchase Frequency</td>
<td>11.43</td>
<td>11.90</td>
<td>1.12</td>
</tr>
<tr>
<td><strong>T2: 15 Months Post</strong></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Total Spend</td>
<td>571.32</td>
<td>676.69</td>
<td>4.25***</td>
</tr>
<tr>
<td>Average Purchase</td>
<td>48.05</td>
<td>46.48</td>
<td>-1.80*</td>
</tr>
<tr>
<td>Purchase Frequency</td>
<td>12.25</td>
<td>15.60</td>
<td>6.62***</td>
</tr>
<tr>
<td><strong>Observations</strong></td>
<td>1255</td>
<td>7909</td>
<td></td>
</tr>
</tbody>
</table>

*** p<0.001, ** p<0.01, *p<0.05

#### Table 2: Summary Statistics by Group

<table>
<thead>
<tr>
<th>Variable</th>
<th>Control</th>
<th>Treatment</th>
<th>t-stat</th>
</tr>
</thead>
<tbody>
<tr>
<td>% Female&lt;sup&gt;A&lt;/sup&gt;</td>
<td>70</td>
<td>73</td>
<td>2.01*</td>
</tr>
<tr>
<td>Tenure at launch (months)</td>
<td>38.31</td>
<td>37.98</td>
<td>0.33</td>
</tr>
<tr>
<td>Average Household Size&lt;sup&gt;B&lt;/sup&gt;</td>
<td>2.92</td>
<td>2.94</td>
<td>2.94**</td>
</tr>
<tr>
<td>Median Family Income (x1000$)&lt;sup&gt;B&lt;/sup&gt;</td>
<td>70.04</td>
<td>69.29</td>
<td>-1.65</td>
</tr>
<tr>
<td>% with Computer Access&lt;sup&gt;B&lt;/sup&gt;</td>
<td>74.35</td>
<td>74.09</td>
<td>-1.99*</td>
</tr>
<tr>
<td>Educational Spend ($)&lt;sup&gt;B&lt;/sup&gt;</td>
<td>1367.3</td>
<td>1375.1</td>
<td>0.78</td>
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</table>

#### T1 (Pre) Quarter Spend

<table>
<thead>
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<th>Quarter</th>
<th>Control</th>
<th>Treatment</th>
<th>t-stat</th>
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<tr>
<td>Q1</td>
<td>66.68</td>
<td>64.76</td>
<td>-0.41</td>
</tr>
<tr>
<td>Q2</td>
<td>121.42</td>
<td>112.26</td>
<td>-1.61</td>
</tr>
<tr>
<td>Q3</td>
<td>113.02</td>
<td>107.17</td>
<td>-1.19</td>
</tr>
<tr>
<td>Q4</td>
<td>109.23</td>
<td>106.01</td>
<td>-0.65</td>
</tr>
<tr>
<td>Q5</td>
<td>101.37</td>
<td>99.96</td>
<td>-0.30</td>
</tr>
</tbody>
</table>

<sup>A</sup> Gender inferred for 82% of sample using a standard “genderizer” database.

<sup>B</sup> County-level statistics.

Table 2: Summary Statistics by Group
4.1 Baseline OLS expenditure regression
Quantifying our treatment effect of interest

4.2.1 Expenditure regression:
Controlling for observed customer
heterogeneity and exploring
differences in trend of the social
dollar

Appendix A
b) on a sample stratified by expenditure
quartiles
c) testing for temporal persistance

4.2.3 Expenditure regression with:
Controlling for potential biases due
to treatment self-selection

4.3.1 a) Selection on observables – Matching
estimators
b) Selection on unobservables –
Heckman/Semi-parametric selection
models

4.3.2 Expenditure regression using a subset of
the treatment as the “control” group
Regression-discontinuity style analysis

Appendix B
Rosenbaum bounds approach
RCR (relative-correlation restrictions)
approach

4.4 Testing for the validity of an alternate
explanation:
Testing for differential quarterly
expenditure trends between the
treatment and control group
Assessing whether competing
alternative explanations can
explain our results

<table>
<thead>
<tr>
<th>§</th>
<th>Analysis</th>
<th>Objective</th>
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<tr>
<td>4.1</td>
<td>Baseline OLS expenditure regression</td>
<td>Quantifying our treatment effect of interest</td>
</tr>
<tr>
<td>4.2.1</td>
<td>Expenditure regression:</td>
<td>Controlling for observed customer</td>
</tr>
<tr>
<td></td>
<td>a) including demographic controls</td>
<td>heterogeneity and exploring</td>
</tr>
<tr>
<td></td>
<td>b) on a sample stratified by expenditure quartiles</td>
<td>differences in trend of the social dollar</td>
</tr>
<tr>
<td></td>
<td>c) testing for temporal persistance</td>
<td></td>
</tr>
<tr>
<td>Appendix A</td>
<td></td>
<td></td>
</tr>
<tr>
<td>4.2.3</td>
<td>Expenditure regression with:</td>
<td></td>
</tr>
<tr>
<td></td>
<td>a) Selection on observables – Matching estimators</td>
<td></td>
</tr>
<tr>
<td></td>
<td>b) Selection on unobservables – Heckman/Semi-parametric selection models</td>
<td></td>
</tr>
<tr>
<td></td>
<td>Expenditure regression using a subset of the treatment as the “control”</td>
<td></td>
</tr>
<tr>
<td></td>
<td>group Regression-discontinuity style analysis</td>
<td></td>
</tr>
<tr>
<td>Appendix B</td>
<td></td>
<td></td>
</tr>
<tr>
<td>4.4</td>
<td>Testing for the validity of an alternate explanation:</td>
<td></td>
</tr>
<tr>
<td></td>
<td>Testing for differential quarterly expenditure trends between the</td>
<td></td>
</tr>
<tr>
<td></td>
<td>treatment and control group</td>
<td></td>
</tr>
</tbody>
</table>

Table 3: Overview of Analyses

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<thead>
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<th>(4)</th>
<th>(5)</th>
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<td>OLS</td>
<td></td>
<td></td>
<td></td>
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</tr>
<tr>
<td></td>
<td>Two-periods</td>
<td>Monthly</td>
<td>Kernal matching</td>
<td>Heckman selection*</td>
</tr>
<tr>
<td>Social Dollar</td>
<td>$127.01</td>
<td>$8.07</td>
<td>$142.09</td>
<td>$221.98</td>
</tr>
<tr>
<td></td>
<td>(14.43)</td>
<td>(0.92)</td>
<td>(15.56)</td>
<td>(75.36)</td>
</tr>
<tr>
<td></td>
<td>18.77%</td>
<td>18.34%</td>
<td>21.01%</td>
<td>32.82%</td>
</tr>
</tbody>
</table>

* ignoring the non-significance of the $\rho$-parameter
SEs clustered at the user level appear in parantheses below the estimates. Bootstrapped SEs with 100
replications are reported for the semi-parametric regression and matching estimator.

Table 5: Expenditure regression results assessing Selection
<table>
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<tr>
<th></th>
<th>(1a)</th>
<th>(1b)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
<th>(5)</th>
<th>(6)</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Total Spend</td>
<td>Total Spend</td>
<td>Average Purchase</td>
<td>Purchase Frequency</td>
<td>Proportion Online</td>
<td>Online Spend</td>
<td>Retail Spend</td>
</tr>
<tr>
<td></td>
<td>(Online + Retail)</td>
<td>(Online + Retail)</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>$\beta_1$ (Treatment)</td>
<td>-21.65</td>
<td>12.86</td>
<td>-3.57**</td>
<td>0.47</td>
<td>-0.04**</td>
<td>-14.95</td>
<td>-6.69</td>
</tr>
<tr>
<td></td>
<td>(24.16)</td>
<td>(26.28)</td>
<td>(1.09)</td>
<td>(0.49)</td>
<td>(0.01)</td>
<td>(12.46)</td>
<td>(20.66)</td>
</tr>
<tr>
<td>$\beta_2$ (Post period)</td>
<td>59.94**</td>
<td>33.34**</td>
<td>-1.76$^+$</td>
<td>0.83**</td>
<td>-0.07***</td>
<td>26.96**</td>
<td>32.98**</td>
</tr>
<tr>
<td></td>
<td>(12.86)</td>
<td>(12.87)</td>
<td>(1.00)</td>
<td>(0.26)</td>
<td>(0.01)</td>
<td>(8.34)</td>
<td>(9.33)</td>
</tr>
<tr>
<td>$\beta_3$ (Social Dollar)</td>
<td>127.01***</td>
<td>148.56***</td>
<td>2.01$^+$</td>
<td>2.87***</td>
<td>0.130***</td>
<td>87.79***</td>
<td>39.23**</td>
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<tr>
<td></td>
<td>(14.43)</td>
<td>(15.08)</td>
<td>(1.07)</td>
<td>(0.29)</td>
<td>(0.01)</td>
<td>(9.27)</td>
<td>(10.55)</td>
</tr>
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<td>Demographics</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>% Female$^A$</td>
<td>34.21$^+$</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(19.38)</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Tenure (months)</td>
<td>4.80***</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.32)</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Average Household Size$^B$</td>
<td>-31.57</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(52.79)</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Median Family Income (x1000$)$$^B$</td>
<td>1.4e-3**</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(6.3e-4)</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>% with Computer Access$^B$</td>
<td>1317.6**</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(429.6)</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Educational Spend ($)$^B$</td>
<td>-0.16**</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.06)</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Intercept</td>
<td>511.38***</td>
<td>-473.91$^+$</td>
<td>28.41**</td>
<td>11.42***</td>
<td>0.39***</td>
<td>137.78**</td>
<td>373.61***</td>
</tr>
<tr>
<td></td>
<td>(23.20)</td>
<td>(267.18)</td>
<td>(9.21)</td>
<td>(0.47)</td>
<td>(0.01)</td>
<td>(12.18)</td>
<td>(19.77)</td>
</tr>
<tr>
<td>Observations</td>
<td>9164</td>
<td>6723</td>
<td>9164</td>
<td>9164</td>
<td>9164</td>
<td>9164</td>
<td>9164</td>
</tr>
</tbody>
</table>

Standard errors clustered by user appear in parentheses below estimates.

*** p<0.001, ** p<0.01, *p<0.05, $^+$p<0.1

$^A$ Gender inferred for 82% of sample using a standard “genderizer” database.

$^B$ County-level statistics.

Table 4: Treatment Effect on Total Spend, Avg. Basket Size and Purchase Frequency and Channel Breakup
### Table 6: DD by Pre-Period (T1) Total Purchase Volume Quartile

<table>
<thead>
<tr>
<th>Distance from Launch</th>
<th>One Quarter</th>
<th>Two Quarters</th>
<th>Three Quarters</th>
<th>Four Quarters</th>
<th>Five Quarters</th>
</tr>
</thead>
<tbody>
<tr>
<td>( \beta_3 ) (Social Dollar)</td>
<td>21.62* (9.06)</td>
<td>44.25*** (9.40)</td>
<td>64.66*** (11.09)</td>
<td>101.87*** (14.43)</td>
<td>127.01*** (17.24)</td>
</tr>
</tbody>
</table>

| Observations | 6259 | 7842 | 8627 | 9001 | 9164 |

Standard errors appear in parentheses below estimates.

*** p<0.001, ** p<0.01, *p<0.05

### Table 7: Temporal Persistence DD

<table>
<thead>
<tr>
<th>Window</th>
<th>Obs.</th>
<th>$ Pre</th>
<th>$ Post</th>
<th>t-stat</th>
</tr>
</thead>
<tbody>
<tr>
<td>1 day</td>
<td>7865</td>
<td>4.88</td>
<td>17.25</td>
<td>20.31***</td>
</tr>
<tr>
<td>2 days</td>
<td>7859</td>
<td>6.77</td>
<td>20.26</td>
<td>20.81***</td>
</tr>
<tr>
<td>3 days</td>
<td>7850</td>
<td>8.42</td>
<td>22.23</td>
<td>20.65***</td>
</tr>
<tr>
<td>4 days</td>
<td>7846</td>
<td>10.17</td>
<td>23.91</td>
<td>19.91***</td>
</tr>
<tr>
<td>5 days</td>
<td>7844</td>
<td>11.87</td>
<td>25.19</td>
<td>18.44***</td>
</tr>
<tr>
<td>6 days</td>
<td>7841</td>
<td>13.36</td>
<td>26.71</td>
<td>17.45***</td>
</tr>
<tr>
<td>7 days</td>
<td>7839</td>
<td>15.07</td>
<td>27.86</td>
<td>16.64***</td>
</tr>
</tbody>
</table>

*** p<0.001, ** p<0.01, *p<0.05

### Table 9: “Regression-Discontinuity” Style comparisons of Treatment Group Means

---
<table>
<thead>
<tr>
<th></th>
<th>(A) Join Cohort Group Comparisons</th>
<th></th>
<th>(B) Join Cohort DD</th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
<td>(3)</td>
<td>(4)</td>
</tr>
<tr>
<td>Treatment Join</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Quarter Cohort</td>
<td>Q6</td>
<td>Q7</td>
<td>Q8</td>
<td>Q9</td>
</tr>
<tr>
<td>T1 Quarterly Mean</td>
<td>72.31</td>
<td>94.04</td>
<td>92.47</td>
<td>92.39</td>
</tr>
<tr>
<td>Spend</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>t-stat for T1 Qtrly. Mean versus</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>T2 Q7 Spend</td>
<td>13.09***</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td></td>
<td>10.26***</td>
<td>2.46*</td>
<td></td>
</tr>
<tr>
<td>T2 Q8 Spend</td>
<td>11.24***</td>
<td>1.49</td>
<td>2.40*</td>
<td></td>
</tr>
<tr>
<td>T2 Q9 Spend</td>
<td></td>
<td>23.08***</td>
<td>7.42***</td>
<td>5.69***</td>
</tr>
<tr>
<td>T2 Q10 Spend</td>
<td></td>
<td>8.70***</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td></td>
<td>Observations 7909</td>
<td>4406</td>
<td>3069</td>
</tr>
</tbody>
</table>

Standard errors appear in parantheses below estimates.

*** p<0.001, ** p<0.01, *p<0.05

Table 8: “Join Quarter” Cohort Group Comparisons/“Join Quarter” cohort DD
<table>
<thead>
<tr>
<th>DV: Book purchases</th>
<th>Model 1</th>
<th>Model 2</th>
<th>Model 3</th>
</tr>
</thead>
<tbody>
<tr>
<td>n = 5,544 (T=4606, C=938)</td>
<td>( \beta ) SE</td>
<td>( \beta ) SE</td>
<td>( \beta ) SE</td>
</tr>
<tr>
<td>Treat</td>
<td>37.83** (112.34)</td>
<td>38.28** (13.78)</td>
<td>38.29** (13.79)</td>
</tr>
<tr>
<td>Time</td>
<td>141.05*** (13.87)</td>
<td>141.05*** (17.64)</td>
<td>141.05*** (17.65)</td>
</tr>
<tr>
<td>Treat x Time</td>
<td>47.27* (17.76)</td>
<td>95.80*** (26.60)</td>
<td>107.08*** (27.64)</td>
</tr>
<tr>
<td>Treat x Time x LURK</td>
<td>-69.96** (21.10)</td>
<td>-91.37*** (22.75)</td>
<td></td>
</tr>
<tr>
<td>Treat x Time x HETE</td>
<td>65.67** (19.18)</td>
<td>59.75* (24.94)</td>
<td></td>
</tr>
<tr>
<td>Treat x Time x LURK x HETE</td>
<td>-84.46*** (21.68)</td>
<td>-62.11* (28.33)</td>
<td></td>
</tr>
<tr>
<td>Gender (F)</td>
<td>-1.84 (8.45)</td>
<td>-6.03 (8.42)</td>
<td>-6.45 (8.43)</td>
</tr>
<tr>
<td>Avg HHL size</td>
<td>4.19 (17.35)</td>
<td>1.72 (17.25)</td>
<td>1.19 (17.27)</td>
</tr>
<tr>
<td>Med. fam. income</td>
<td>0.00 (0.00)</td>
<td>0.00 (0.00)</td>
<td>0.00 (0.00)</td>
</tr>
<tr>
<td>Comp. Access (%)</td>
<td>557.19** (179.59)</td>
<td>619.98** (178.61)</td>
<td>609.26** (178.69)</td>
</tr>
<tr>
<td>Educ Spend</td>
<td>-0.08** (0.03)</td>
<td>-0.09** (0.03)</td>
<td>-0.09** (0.03)</td>
</tr>
<tr>
<td>Loyalty tenure (months)</td>
<td>0.90*** (0.12)</td>
<td>0.93*** (0.12)</td>
<td>0.93*** (0.12)</td>
</tr>
<tr>
<td>Intercept</td>
<td>-223.24* (112.34)</td>
<td>-252.45* (111.70)</td>
<td>-245.54* (111.77)</td>
</tr>
</tbody>
</table>

*** p < .001, ** p < .01, * p < .05, + p < .10

Table 10: Moderating the Social Dollar