Beyond the Target Customer: Social Effects of CRM Campaigns

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Beyond the Target Customer: Social Effects of CRM Campaigns

Customer Relationship Management (CRM) campaigns have traditionally focused on maximizing the profitability of the targeted customers. We demonstrate that, in business settings that are characterized by network externalities, a CRM campaign that is aimed at changing the behavior of specific customers propagates through the social network, thereby also affecting the behavior of non-targeted customers. Using a randomized field experiment involving nearly 6,000 customers of a mobile telecommunications provider, we find that the social connections of targeted customers increase their consumption and are less likely to churn due to a campaign that was neither targeted at them nor offered them any direct incentives. We estimate a social multiplier of 1.28. That is, the effect of the campaign on first-degree connections of targeted customers is 28% of the effect of the campaign on the targeted customers. By further leveraging the randomized experimental design we show that, consistent with a network externality account, the increase in activity among the non-targeted but connected customers is driven by the increase in communication between the targeted customers and their connections, making the local network of the non-targeted customers more valuable. Our findings suggest that in targeting CRM marketing campaigns, firms should consider not only the profitability of the targeted customer, but also the potential spillover of the campaign to non-targeted but connected customers.

1. Introduction

At the heart of customer relationship management (CRM) is the concept of customer centricity. Customer centricity emphasizes the idea that firms should recognize that customers are different and target only those customers for whom the marketing effort will pay off (Rust and Verhoef 2005; Blattberg, Kim and Neslin 2008; Fader 2012). However, increasingly, customers have a variety of means to connect and interact with one another. The number of business settings in which customers are directly connected to other customers through the firm's product or service is rapidly increasing. Examples include communication settings (such as traditional telecom providers, but also more recent services such as WeChat, WhatsApp, or SnapChat), cloud storage and file sharing services (e.g. Dropbox, Google Drive), "sharing economy" market places (e.g., Uber, Airbnb), payment services (e.g., PayPal, Venmo), or online games. In these settings network effects and network externalities (e.g., Katz and Shapiro 1985) are often present.[1] Consequently, in such business settings, marketing campaigns that are targeted to specific customers aiming at changing the behavior of the targeted customers, may also indirectly affect the behavior of other, non-targeted, customers. While the social interaction literature has shown that social effects exist in a variety of marketing contexts, the CRM literature and practice has largely ignored such social effects in designing and evaluating CRM campaigns.

The objective of this research is to investigate whether targeted CRM campaigns that are aimed at changing the behavior of specific customers also affect the behavior of the targeted customers' connections, *who are not targeted themselves*. Our focus on CRM campaigns excludes referral campaigns (Biyalogorsky, Gerstner and Libai 2001; Schmitt, Skiera and Van den Bulte 2011; Chae et al. 2016), which are directly aimed at creating a social effect by giving

[1] In addition to the aforementioned examples, there are many more traditional business settings that may also exhibit network externality such as retailers, banks and gyms.

incentives to existing customers or prospective customers as well as their connections. Instead, our focus is on typical CRM campaigns that (1) have no explicit social component to them, (2) were individually targeted to specific customers, possibly based on that customers' past behavior, (3) are not transferable, and (4) cannot be shared. Examples include targeted retention campaigns or cross-selling and up-selling tactics, which are generally characterized by offering incentives (e.g., discounts, free consumption units, premium services) to particular customers.

On the one hand, one could expect a positive effect of such a typical CRM campaign on non-targeted customers, because these customers may derive more value from the product or service due to positive network externalities. There are several reasons why we may expect positive network externalities. First, a successful CRM campaign may grow the user base, because new users start using the service. At the same time, a CRM campaign may incentivize existing users to stay with the firm. In both cases, the focal customer experiences a larger and potentially more active (ego) network. (e.g., Aral and Walker 2011; Nitzan and Libai 2011). Second, network externalities are often associated with higher level of product usage. For example, Aral and Walker (2011) show that in a social networking website, network externalities lead to higher levels of adoption of a new feature as well as an increase in usage of that feature among connected friends. Similarly, Trusov, Bodapati and Bucklin (2010) show a social effect on users' activity in a social network site. Manchanda, Packard and Pattabhiramaiah (2015) find that stronger ties in online customer communities lead to higher levels of expenditure within the community. Taken together, these results suggest that CRM campaigns that increase usage among the targeted customers may also increase the usage level of the focal customers' connections.

On the other hand, one could also expect a negative (social) effect of the campaign because the benefit/incentive (e.g., discount or free consumption) is only offered to the targeted customers and is not available to the non-targeted customers.[2] In turn, if the targeted customer talked about the benefits of the campaign with her connections (i.e., if she initiated word of mouth about the campaign), then the non-targeted customers might become dissatisfied with the service due to perceived "peer-induced" unfairness (Nguyen and Simkin 2013; Li and Jain 2015). Such decrease in satisfaction among non-targeted customers could result in a reduction of consumption and even higher churn among them.

The CRM literature and practice has traditionally ignored potential social effects of marketing campaigns, thus implicitly assuming that such effects either do not exist or are too small to be of managerial relevance. An exception can be found in Lemon and Sieders (2006) who call for firms to consider not only the core, or targeted, customers but also what they call the "augmented customers." They postulate that a firm's marketing action not only affects the targeted customers but also the augmented customers. In this research we investigate this issue and explore which types of customer behavior (e.g., usage and churn) of non-targeted customers that are connected to targeted customers can be (socially) affected by a CRM campaign. We estimate the direction and magnitude of these social effects, and discuss the possible mechanisms through which such an effect propagates through the network. Furthermore, we quantify the economic value of the social spillover effect. Our calculations suggest that the dollar value of the campaign spillover may be substantial and should not be ignored by CRM practice.

To investigate the potential social effect of CRM campaigns, we run a field experiment in the context of a telecommunications service provider. We randomize a targeted CRM campaign among current customers such that the focal customers were offered free money to top-up/refill

[2] This is in contrast to campaigns that emphasize a new product, service, or feature that is available to all customers.

their pre-paid telephone plan. We then analyze the activity (cellphone usage and churn) of both the egos (i.e., targeted customers) and their alters (i.e., customers connected to the targeted customers who themselves were not targeted).[3] One important benefit of implementing the experiment in a telecommunications setting is that we can assure that the campaign incentive was made available to the targeted (focal) customers only and *not* to their connections (unlike coupons or referral-type promotions, this top-up credit is non-transferable).

We empirically demonstrate that the effect of the targeted CRM campaign propagates beyond the targeted customers both in terms of usage and churn. In particular, we find that the (non-targeted) connections of egos in the treatment group have significantly higher consumption levels than the (non-targeted) connections of egos in the control group. We show that the campaign caused a 35% increase in usage among the targeted customers. On top of that, the campaign caused a 10% increase in usage among their connections, *who were not targeted themselves*. Furthermore, we find that the campaign reduced churn among the connections of targeted customers. Based on our findings, we estimate that the incremental profit of the "social spillover effect" is, on average, $0.85 per connection across the 12 weeks following the campaign.

One question that naturally arises is, if connections of targeted customers did not receive any direct benefit from the campaign, why do they consume more and why do they churn less than connections of non-targeted customers? We investigate this question by leveraging the randomization of our research design using an instrumental variable (IV) regression approach. We show, consistent with a network externality account, that an increase in communication between the focal (targeted) customers and their connections causes an increase in the consumption of the connections and reduces their churn. Furthermore, the strength of social

[3] In what follows, we will use the terms "focal" and "ego", and "connections" and "alters", interchangeably.

relationship between the ego and her alters moderates the magnitude of the social effect. Particularly, the campaign spillover effect is larger for egos and alters with stronger ties. These findings are all in support of positive network externalities of traditional CRM campaigns.

Our research complements the work on CRM and database marketing (e.g., Reinartz, Krafft and Hoyer 2004; Boulding et al. 2005; Neslin et al. 2006) by quantifying the effects of CRM campaigns beyond the target customer. Our work is also related to the literature on social influence, which has mainly focused on the contagious effect of new product introduction and customer acquisition (e.g., Iyengar, Van den Bulte and Valente 2011; Schmitt, Skiera and Van den Bulte 2011; Haenlein and Libai 2013). In that respect, our research is most closely related to the work of Nitzan and Libai (2011), who demonstrated that churn behavior may be contagious. We differ from their work in three important ways. First, our focus is not on the contagion of churn per se but rather the propagation of a *change* in customer behavior (including both churn and usage) in response to a targeted marketing campaign. Second, we leverage a randomized field experiment to estimate the causal effect of the campaign on the non-targeted connected customers. Finally, using our modeling approach, we quantify the magnitude of the campaign spillover as well as the monetary value of this social effect.

Our findings have clear implications for marketers. In targeting CRM marketing campaigns, firms not only need to consider the profitability generated by the targeted customers, but also the potential spillover of the campaign to non-targeted, but connected, customers. As we discuss in the final section, we believe that this implication is not limited to telecommunications but generalizable to many other contexts where network externalities are present.

This paper continues as follows. In Section 2, we describe and discuss the research setting and the field experiment. In Section 3, we quantify the impact of the targeted promotions on the targeted customers, as well as on their (non-targeted) alters. Then, we examine the mechanism underlying the social spillover effect from the CRM campaign. Section 4 focuses on the managerial implications of this research, quantifying the consumption spillover and estimating the monetary value of the social effect. Section 5 concludes with a discussion of the theoretical and practical implications of our work.

2. Research setting

2.1. Identifying social effects

To investigate the social effect of targeted marketing campaigns one has to look beyond the targeted customers and measure the changes in activity among the customers connected to them. With the appropriate individual-level data and sufficient variation in marketing actions, firms can easily measure the effectiveness of their promotions on the *targeted* customer (e.g., Neslin, Henderson and Quelch 1985; Gupta 1988). However, measuring the effect of the promotion on the non-targeted, but connected, customers (i.e., measuring the social effect) is more complicated, because identification of social influence from observational data is challenging (e.g., Manski 1993; Nair, Manchanda and Bathia 2010; Nitzan and Libai 2011; Shalizi and Thomas 2011). Consider a firm running a marketing campaign and suppose that the targeted customers increase their activity after the campaign. Suppose that the firm also observes an increase in activity among the customers connected to the targeted customers. Can one directly attribute the increase in activity of the connections to a social effect of the marketing campaign? The answer is no, as such observed similarity in behavior between a targeted customer and her non-targeted connections after the campaign could also be explained by *homophily* (i.e., unobserved similarities in customers' preferences such as common preferences for a particular service provider), *correlated unobservables* (i.e., a common shock affecting the behavior of the connected customers such as improved quality of the service/product in a certain area, or other unobserved marketing actions of the firm or its competitors), or *simultaneity/reflection* (i.e., alters' behavior affecting focal's behavior). Hence, using observational data alone, it is difficult to conclude that the observed changes in the non-targeted connected customers' behavior were *caused* by the marketing campaign.

To address the challenges in making causal claims with respect to social effects, we conducted a randomized field experiment in collaboration with a telecommunications provider in which a set of randomly selected focal customers received a marketing promotion that incentivized them to change their own behavior (treatment group), while other focal customers did not receive the promotion (control group). This intervention induces exogenous variation in the behavior of the focal customers, which we leverage to identify the causal effects of interest. In particular, we investigate the social effect of the marketing campaign by comparing the behavior of the customers connected to the focal customers in the treatment group to the behavior of customers connected to the focal customers in the control group. Importantly, none of these connections received the treatment themselves. Hence, we employ a "peer encouragement design," which is characterized by randomly encouraging certain behaviors in a set of nodes — i.e., the egos — in order to analyze the effects of the encouragement (treatment) on the nodes' peers — i.e., the alters (e.g., Aral 2015; Hinz et al. 2011; Bapna and Umyarov 2015). The randomized nature of our research design addresses the potential issues of homophily, correlated unobservables, and simultaneity/reflection.

2.2. Randomized Field experiment

The field experiment was conducted in Australia, where the penetration of cellphones is higher than 130%. During the period of the experiment, there were three main providers in this market; the company we collaborate with was the second largest in terms of market share, with a customer base of, approximately, 10 million people. This is a "calling party pay" market. In other words, the person who initiates the call/text incurs all the costs and the receiver is not charged.

Focal customers ("egos"): Customers selected to participate in this marketing campaign belong to a 28-day non-rollover prepaid plan with unlimited in-network voice, domestic Short Message Services (SMS), and access to major social network platforms (e.g., Facebook and LinkedIn). That is, every time a customer adds (a minimum amount of) credit to her account, she has unlimited in-network activity for a period of 28 days. During that time, the balance/credit can be used to call out-of-network, internationally, and to download or upload data. If a customer reaches zero balance, or if she has not recharged within 28 days, then her balance is set to 0 and her account is suspended. Suspended accounts can receive calls and texts but cannot initiate any type of communication. Once a customer is suspended, she can receive calls/texts for a period of 6 months. At any time during that period, she can become active by adding credit to her account. However, if she does not add credit to her account within 6 months, then the provider cancels the service and the account is terminated.[4] This is in contrast to churn among post-paid customers at the same company who cancel the service by calling the provider (active churn).

The customers selected for this experiment (i.e., focal customers in the treatment and control groups) were customers who were active at the time of the intervention (i.e., they had

[4] We do not observe the firm-initiated cancelation (passive churn) of our focal customers because such suspensions happen after six months of no activity, which extends beyond the length of our data.

credit and could initiate any kind of communication), however these customers were going to be suspended in the following week, unless they added credit to their account. The campaign's goal was to prevent the targeted customers from going into suspension by encouraging them to top up their account. Given that the focus of our study is to study the social effect of marketing promotions, we only considered focal customers who had at least one connection on the same network (further details about the definition of a connection are provided below).

Intervention: We select 1,041 customers with the characteristics described above and randomly split the sample into a treatment (63%) and a control (37%) group. Customers in the treatment group received a text message offering free credit if the customer replies "Yes" to the text message. All customers received the same credit incentive. Once the promotional text is sent, the promotion is valid for 7 days and upon acceptance, the bonus credit expires after 7 days. (See Figure 1 for a visual of the intervention.) It should be noted that the intervention was only based on the targeted customers' calling plan and behavior prior to the campaign, and not in any way based on the behavior of their connections. In that respect, this was a typical CRM campaign aimed at increasing engagement among current customers.

Insert Figure 1 here

Connected customers ("alters"): One of the advantages of using a telecommunications network for our study is that we can perfectly observe the communications between customers, which eliminates the need for constructing (sometimes noisy) proxies for communications between individuals (Nitzan and Libai 2012; Hill, Provost and Volinsky 2006). At the moment of the intervention, we identify all customers who have communicated with each focal customer (either treatment or control) at least twice—either via call or text—in the month *prior* to the experiment.

The edges in our ego-networks are unweighted and undirected.[5] Defining the ego-networks before the campaign ensures exogeneity of the network with respect to the treatment. We only consider the connections who belong to the same telecommunications provider as these are the only customers for whom we can observe their behavior both before and after the treatment. Note that we only track the behavior of the first-degree connections of the focal customers. In theory, the social effect could also reach second-degree or higher order connections. However, looking only at first-degree social effects allows us to simplify the analysis, avoid network sampling issues (Ebbes, Huang and Rangaswamy 2015), and be conservative in our estimated social effect of the marketing campaign.

As with any experiment conducted in a network, we face the challenge of contamination or interference (e.g., Fienberg 2012; Eckles, Karrer and Ugander 2014; Aral 2015) if the control group is exposed to the treatment through their connections. In our experiment contamination could occur if: 1) the alters are treated directly, or 2) the alters are connected to other customers who were treated or who were connected to treated customers. In both these cases, the stable unit treatment value assumption (SUTVA, Rubin 1980) is violated as the egos in the control group are exposed to the treatment indirectly through the alters. This issue would be particularly problematic in the case of first-degree contamination, which happens when the alter is treated, or second-degree contamination, which happens when the alter's connections are treated. To minimize this concern we cross-reference all egos and alters in the sample and find that only 0.60% of alters (i.e., 32 out of 5,308) received a marketing promotion during the time of the study. Moreover, we looked at the alters' first-degree connections and find that 1.60% of alters (85 out of 5,308) had (at least) one connection who had received the marketing promotion. While

[5] We set at least two communications to avoid "random" calls (e.g., a taxi or restaurant) to be part of the ego-network. While the weight and directionality of the edges are not used for the network formation, we leverage this information in Section 4 when we incorporate the strength of ties.

this is a small subset of our entire network, we removed the complete ego networks to which these alters belong to. As a result, our final sample includes 961 ego customers and 4,700 alters, which we use for all subsequent analyses.[6] Importantly, we can assure that this reduced sample is free of first- and second-degree contamination. Admittedly, there is still a possibility of higher degree contamination in the control group if, for example, a third-degree connection of a focal customer in the control group was treated (or, equivalently, a second-degree connection of an alter in the control group). However, given the small incidence of treated alters, the size of the total network (~10 million customers), the scope of the experiment (~1000 focal customers), and the fact that the contamination would need to be transmitted through, at least, 3 nodes, we believe that the likelihood of such higher-degree contamination and its potential biasing effect on estimating the treatment effect is very low.

2.3. Behavioral data

We collect two types of activity data: (a) individual-level activity data of the egos, and (b) individual-level activity data of the alters. We track the behavior of egos and alters for 16 weeks, 4 weeks before and 12 weeks after the intervention.

Ego behavior: For each ego customer we observe weekly activity in the form of texts, calls, and minutes. Each of these metrics is split by inbound and outbound activity (e.g., calls the customer makes and calls the customer receives). We also observe whether an ego customer is suspended in a particular week (i.e., whether or not the customer can initiate calls/text). Regarding churn behavior, customers can churn at any time by calling the provider to close their account (the SIM card will be permanently deactivated). However, this behavior is very rare among the ego

[6] Note that instead of only removing the "contaminated" alter, we use a more conservative approach by removing the entire ego-network containing such node. We also re-ran our analyses on the larger sample without removing the entire ego-networks but only the "contaminated" alters. Our main conclusions did not change.

customers because of the type of (prepaid) plan these customers have. Therefore, we ignore churn behavior among ego customers.[7] Note that churn behavior is relevant for the alters because many of these customers are on post-paid plans. In post-paid plans churn is more prevalent and serves as an important KPI for the provider. Thus, for ego customers we focus on measures of activity, including suspension, minutes, calls, and texts. Table 1 shows summaries of ego behavior (weekly averages) during the four weeks before the intervention.

Insert Table 1 here

From Table 1 it follows that the usage variables have skewed distributions as there are considerable differences between the mean and median of these variables. We address this issue in our difference-in-differences (diff-in-diffs) regression approach by using the log-transformed variables.

Alter behavior: We observe weekly usage, suspension, and churn for each of the alters before and after the experiment. Table 2 summarizes the alter activity during the four weeks prior to the intervention. For suspension behavior, we report, for each ego, the proportion of her connections who were suspended at the moment of the intervention. Similar to the ego behavior variables, we log-transform the alters' usage variables as well.

Insert Table 2 here

Comparing Tables 1 and 2, we see that, on average, alters are more active than egos. This difference is not surprising because the egos selected for the experiment were customers who were at risk of being suspended at the time of the experiment, which suggests that these customers were less active during the weeks prior to the intervention.

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[7] We do not treat 'suspension' as 'churn' because many of the suspended customers re-activate after some weeks of suspension. In our sample 69.5% of the customers were suspended at some point during the 12 weeks following the experiment, of which, 34.7% re-activate. Hence, suspension in this context relates more to usage than to churn.

In addition to usage and suspension, we also observe the size of each ego-network, that is, the number of alters each ego had at the moment of the intervention, as well as the number of connections each of the alters had during the 4 weeks prior to the intervention. On average egos had 4.89 alters and alters had 6.57 connections.[8] Note that these metrics are "static" (i.e., do not change over time) and exogenous to the treatment as they were computed based on behavior prior to the intervention.

2.4. Randomization

Before we can rely on the experiment to estimate the social effect of the campaign we need to verify that the customers who were assigned to the treatment group (i.e., to be targeted by the campaign) are similar in terms of their usage prior to the campaign to those who were assigned to the control group (i.e. not targeted by the campaign). We do not expect any difference between these two groups of customers because of the random assignment between treatment and control. Table 3 shows the sample means of the treatment and control groups as well as statistical tests for the difference in means between the two groups for the different customer activities. We find that, for all types of behaviors, the average activity in the control and treatment groups are *not* statistically different (p-values shown in the right-most column in Table 3).[9] Thus, we conclude that the randomization between the control and experimental groups was well executed.

[8] Due to limitations in the company's database, the ego and alter degree (i.e., number of connections at the moment of the intervention) are not computed in the same way. A connection of an ego is defined as a customer who communicated with the ego at least twice during the four weeks prior to the experiment, while to calculate the number of connections of an alter we count for each week the number of customers who communicated with the alter at least once in that week. We then average the number of connections of each alter across the four weeks prior to the campaign. Hence, a connection of an alter is defined at the weekly level (and we compute it for each of the four weeks prior to the experiment) whereas a connection of an ego is defined at the monthly level (and we compute it once, at the moment of the intervention).

[9] We note that the results in Table 3 are for the log-transformed usage variables. We obtain similar result when we replicate the analyses for the untransformed (before log) variables (see Web Appendix A1).

Insert Table 3 here

3. Results

We now turn to investigate the effect of the marketing intervention on customer behavior. While our main goal is to measure the effect of the treatment on the alters, we first analyze the effect of the marketing campaign on the targeted customers. It is important to establish the effect of the marketing campaign on the targeted customers as it would be unrealistic to expect any social spillover without an effect of the campaign on the focal customers.

3.1. Effect of the marketing campaign on targeted customers (egos)

We evaluate the effect of the marketing campaign on the focal customers by analyzing two managerially relevant behaviors namely suspension and usage (i.e., minutes, calls, and SMS). Recall that the purpose of the campaign was to keep these customers active (prevent suspension) and increase their usage levels. We first present several "model-free" analyses before statistically estimating the treatment effect through "diff-in-diffs" regression models.

3.1.1 Model-free analyses for ego usage and suspension

We start by looking at suspension among ego customers (Table 4). As expected, the campaign was successful at preventing suspension: while 47.5% of the customers in the control group got suspended in the week following the intervention, only 35.4% of the customers in the treatment group did. The difference between the two groups is statistically significant (p<0.01). We also compare the number of customers who are suspended at the end of the observation period (week 12). We observe that treated customers are less likely to be suspended than those in the control group (48.4% vs. 57%, p=0.005) even 12 weeks after the intervention, implying that the lack of suspension caused by the campaign persisted even after the customers used all the free minutes.

Next we investigate ego usage (or consumption), considering only 'outbound' consumption (i.e., calls that the ego initiates) as it better reflects decisions made by the focal customer.[10] Table 4 shows the difference between pre- and post-intervention activity for the treatment and control egos.

Insert Table 4 here

We compute for each ego the difference between her post-treatment weekly consumption and her average weekly consumption in the four weeks before the intervention (both with the original and the log variables). As shown in Table 4, customers in both the treatment and control groups decrease their usage in the 12 weeks following the campaign. This trend is consistent with the targeting selection of the marketing campaign that was aimed at customers who were about to be suspended and with the overall downward trend in usage that the focal firm experienced. More importantly, for all usage variables (minutes, calls and SMS), the decrease in activity is smaller, on average, for customers in the treatment condition than for those in the control condition, thus the treatment effect (third column) is positive, and statistically different from zero, for number of minutes and number of calls, but not for number of SMS. Hence, the campaign has an overall positive effect on the usage of the targeted egos during the 12 weeks following the campaign.

As the treatment offered customers free money for making calls or sending SMSs, the question is whether the observed positive treatment effect on usage across the 12 weeks post intervention is solely driven by the monetary incentive given as the treatment. To investigate this, we analyze the treatment effect at the weekly level for each week following the intervention. If the effect is fully driven by the free money, we should observe a decrease in

[10] While customers could choose not to answer certain calls, the 'inbound' calls/texts are mainly determined by the customer who initiates the call, not by the receiver. We leverage the information obtained from incoming communications when characterizing types of relationships between egos and alters in follow-up analyses below.

suspension and an increase in usage only during the first two or three weeks following the intervention. Recall that the customer had one week to accept the offer and another week to use the free money. Thus, any effect observed three or more weeks after campaign cannot be attributed to the free money offered in the campaign. Figure 2 shows the average differences of weekly consumption (individual differences) for the entire post-treatment period for the treatment (solid line) and control groups (dashed line). It can be seen that that the treatment group exhibits higher usage levels immediately after the intervention (consistent with customers using the free credit) but this effect persists until the end of the observation window (12 weeks after the treatment), indicating that the intervention increased ego consumption above and beyond the economic incentive of the promotion. Figure 2 also shows the weekly suspension rate in the treatment and control groups, confirming that the campaign also had a lasting effect in preventing suspension well beyond the first week. To sum, Figure 2 visually illustrates that the campaign had a positive effect both in the short and in the long run for both usage and suspension. We investigate these effects more formally in the next subsection.

Insert Figure 2 here

3.1.2 Difference-in-Differences regression model results for ego usage

To statistically test for the observed differences in usage between the treatment and control groups, we estimate linear regression models that include the individual difference between pre- and post campaign usage as the dependent variable, and treatment and week dummies as the independent variables. We use the log-transformed variables to account for the skewed distributions of the activity variables (see Table 1).[11] We split the data into two observation windows to investigate the "short-term" (weeks 1 to 6) and "long-term" (weeks 7 to 12) effects

[11] Web Appendix A2 replicates Figure 2 for the log transformed variables that were used in the diff-in-diffs regression models. A full description and motivation of the diff-in-diffs regression approach is given in Web Appendix A3.

of the campaign. More specifically, for each metric of ego usage, we estimate the following "diff-in-diffs" models for the short and long term, respectively:

$$\Delta y_{it}^{ego} = \alpha_0 + \alpha_1 T_i + \sum_{\tau=2}^{6} \alpha_\tau D_{\tau t} + \varepsilon_{it} \quad \text{for t=1,…,6,} \tag{1}$$

$$\Delta y_{it}^{ego} = \beta_0 + \beta_1 T_i + \sum_{\tau=8}^{12} \beta_{\tau-6} D_{\tau t} + \varepsilon_{it} \quad \text{for t=7,…,12,} \tag{2}$$

where Δy_{it}^{ego} is the difference between individual *i*'s usage in period *t* and her (individual) average usage during the four weeks prior to the treatment. T_i is an indicator variable that takes the value 1 if the customer was part of the treatment group, and 0 if she was part of the control group. $D_{\tau t}$ is a dummy variable for week *t* and equals 1 when $\tau = t$ and equals 0 otherwise. The error term ε_{it} has mean 0 and variances $\sigma_{\epsilon,s}^2$ and $\sigma_{\epsilon,l}^2$ for the short-term and the long-term, respectively.[12] We find a positive and statistically significant coefficients of the treatment dummies, which confirms the "model-free" evidence in Table 4 and Figure 2 the treated egos used the telecommunication service more than the non-treated egos both in the short- (Table 5) and in the long-term (Table 6).

Insert Tables 5 and 6 here

The analysis in this subsection demonstrates that treated customers overall consume more than non-treated customers. Furthermore, the difference in consumption caused by the campaign extends beyond the "free money" given to the targeted customers, as the effect lasts for up to 12 weeks post-campaign whereas the credit incentive was available only for 2 weeks. Next we investigate the "social effects" of the marketing intervention. That is, we investigate whether the campaign had an effect on the alters, i.e. on customers who were not targeted themselves but were connected to those who were targeted by the CRM campaign.

[12] We use panel corrected standard errors to account for potential serial correlation in the model error terms in (1) and (2) (e.g., Hoechle 2007).

3.2. The effect of the marketing campaign on non-targeted customers (the alters)

The main goal of our study is to quantify the impact of the targeted marketing intervention on non-targeted connected customers, both in terms of whether they are more likely to stay with the firm (do not churn) as well as whether they change their level of activity (usage). While most CRM marketing campaigns are designed and evaluated considering their effect on the target customers only — thus implicitly assuming that social spillover effects do not exist — we postulate that in contexts where network externalities are present, a targeted marketing campaign will likely propagate through the network, therefore also indirectly affecting (connected) customers who were not originally targeted.

Consistent with the social interaction literature (e.g., Trusov et al. 2010; Aral and Walker 2011; Nitzan and Libai 2011), and given that the campaign positively impacts ego usage and negatively impacts ego suspension, one may expect a positive spillover of the marketing campaign from the egos to their alters. However, unlike contagious effects in the adoption of new products, traditional targeted CRM campaigns (that are not referral-type campaigns) are not likely to generate Word-of-Mouth (WOM) about the campaign itself. Even if the campaign does create WOM, one could expect to find the opposite effect. That is, if the ego discusses the campaign with her alters, then the alters would find out that they did not receive the benefits of the campaign, which could likely lead to a negative effect of the campaign on the non-targeted customers due to perceived unfairness (Nguyen and Simkin 2013, Li and Jain 2015).

We propose that in the presence of network externalities, a successfully targeted marketing campaign is likely to impact the behavior of the non-targeted connected customers in a positive way, but not necessarily through WOM. First, customers derive value from having more connections belonging to their network because, among other reasons, calling/texting customers in-network is cheaper (Nitzan and Libai 2011). Hence, if a campaign is successful at retaining targeted customers, then retention might also be larger among those connected to them. Second, customers derive higher value when other customers that are connected to them use the service more (Trusov et al. 2010). As a consequence, a campaign that increases consumption among targeted customers might also increase consumption among their connections, specially when the connections perceive a more active network (e.g., getting more calls from the targeted customer).

In sum, we would expect that the targeted marketing campaign causes the alters to (a) churn less, and (b) exhibit higher levels of activity. Regarding the latter, we acknowledge that in the context of telecommunications, observed increase in activity among alters could be due to pure reciprocity in calling/texting behavior. That is, an alter could increase her usage just because she is "returning" the calls/texts received from her ego, and not because she derived higher value from the network. To ensure that a potential positive difference in usage is not purely due to reciprocity in calls (i.e., the alter returning the ego's calls), we also create a more conservative metric for alter consumption that ignores the calls that the alter makes to the ego (indicated by 'excl. ego' in Table 7). Using this metric as an outcome variable also helps us to separate the possible confound (Manski 1993) between the variable used to define the network (in our case, calls or texts between egos and alters prior to the intervention) and the outcome variable (alter usage *excluding* the calls made to the ego).

As we did for the egos, we first present "model-free" evidence of the treatment effect on alter usage, suspension, and churn (both at the aggregate and disaggregate level), followed by a "diff-in-diffs" regression models to estimate the treatment effect.[13] Note that in contrast to most

[13] For the remaining of the paper we use 'number of minutes' as a variable representing alter usage. The results of analyzing 'number of calls' and 'SMS' are similar (see Web Appendix A4).

studies investigating social effects, we run a randomized field experiment in which the intervention was exogenously manipulated. In addition, we made sure that each alter is connected to only one ego customer. As such, we can estimate the causal effect of the treatment on alter behavior by simply comparing the activity and churn of the alters whose egos were in the treatment group to the activity and churn of the alters whose egos were in the control group.

3.2.1 Model-free analyses for alter usage, churn, and suspension

Figure 3 shows the average outbound activity of the alters (including and excluding communications with the ego) during the post-treatment weeks, as well as their churn and suspension rates.[14] Alters are grouped by whether or not their ego received the treatment. For example, the solid lines in the figures represent the weekly average (across alters) of the individual differences between the pre- and post-campaign consumption for alters whose egos were treated, whereas the dotted line are the averages corresponding to those alters whose egos were in the control condition.

Insert Figure 3

Figure 3 (top row) suggests that alters whose egos were treated tend to call more minutes than alters whose egos belong to the control group, and this positive difference in consumption is persistent over time. Moreover, this difference seems not to be driven by reciprocity in calls (the alter calling back the ego) as this pattern is robust to excluding those minutes. Regarding suspension and churn, recall that in this context suspension (status observed among some prepay users) implies that the user cannot initiate any type of communication but she can receive calls/SMSs for a period of up to 6 months. At any time during that period a customer can move

[14] The model-free time series plots for the rest of the activities as well as the log-transformed variables are presented in Web Appendix A2. We note that, similar to the time series plots for the egos, the usage trends are negative. That is, during the 12 weeks post-intervention, the average usage declines compared to the four weeks before the intervention. Discussions with the data provider confirmed that the mobile operator was experiencing an overall decline in usage across the customer base during this time period.

from a suspension to an active state by adding credit to her account. Churn, on the other hand, means that the customer has completely terminated the relationship with the firm and the phone has been disconnected. As can be seen from the Figure 3 (bottom row), both suspension and churn are lower for the alters of treated (ego) customers, as compared to alters of the control customers. This difference seems more pronounced during weeks 3—6 for alter suspension while the difference in alter churn appears to be larger during weeks 8—12.

As we did for the egos, we compare the (aggregate) behaviors across conditions by subtracting averaged consumption of each alter in the sample (N = 4,700) during the four weeks before the treatment from her observed weekly consumption after the intervention. We look at behavior both in the short-term (weeks 1—6) and in the long-term (weeks 7—12). Table 7 shows the average differences across the two experimental conditions. With respect to suspension (percentage of alters who are suspended in week 6 or 12) and churn (percentage of alters who cancelled their service by week 6 or 12), consistent with the pattern observed in Figure 3 (bottom row), suspension and churn are lower for the alters that were connected to treated customers. We find that the effect of the treatment on alter suspension and churn is stronger in the later weeks (weeks 7-12) than the earlier weeks (weeks 1-6) following the experiment. This pattern is to be expected given than the treatment has to affect ego's behavior first in order to affect alter behavior later. We find statistically significant long-term effect of the campaign on the alters of treated egos both for suspension and churn. Regarding usage, and similar to the results for the egos, we find a positive treatment effect; average usage is significantly higher for the alters of treated egos than for the alters of control egos, even when excluding the minutes that the alter talks to the ego. Similar to the effect of the campaign on alters' suspension and churn we find stronger effect of the campaign on alters' usage in long term relative to the short term. Note that the alters did not receive any financial incentive from the campaign, hence, the observed increase in activity of the alters cannot be explained by the financial incentive offered in the campaign.

Insert Table 7 here.

In sum, the model-free analyses suggest that there is a potential spillover effect of the CRM campaign on the alters, particularly for churn in the long run, but also for usage. We next formally test for these effects using diff-in-diffs regression models.

3.2.2 Difference-in-Differences regression model results for alter usage and probit results for suspension and churn

We test whether the observed differences from our model-free analyses between the treatment and control groups in alter consumption are statistically significant by estimating a "diff-in-diffs" regression model, similar to the one used for the ego analysis. As before, we use the log-transformed variables. For these activities, we estimate the following regression models:

$$\Delta y_{ijt}^{alter} = \gamma_0 + \gamma_1 T_i + \sum_{\tau=2}^{6} \gamma_\tau D_{\tau t} + \xi_{ijt} \qquad \text{for t=1,…,6,} \qquad (3)$$

$$\Delta y_{ijt}^{alter} = \delta_0 + \delta_1 T_i + \sum_{\tau=8}^{12} \delta_{\tau-6} D_{\tau t} + \xi_{ijt} \qquad \text{for t=7,…,12,} \qquad (4)$$

where Δy_{it}^{alter} is the difference between the post- and pre-intervention of the log of the usage of alter *j* who is connected to ego *i*, and T_i is an indicator variable that takes the value 1 if ego *i* (connected to alter *j*) was part of the treatment group, 0 if part of the control group. The weekly dummies are defined as in Equations (1) and (2), and ξ_{ijt} is an error term with 0 mean and variance $\sigma_{\xi,s}^2$ and $\sigma_{\xi,l}^2$, for the short- and long-term effects, respectively.[15] The estimation results are presented in Tables 8 and 9, columns 1 and 2. These results confirm the results depicted in Figure 3. Alters of treated egos have significantly higher consumption than alters of control egos, both in the short and in the long run. These differences are statistically significant even when the

[15] See Web Appendix A3 for a more detailed explanation of the models used. We use panel corrected standard errors to account for potential serial correlation in the model error terms in (3) and (4) (e.g. Hoechle 2007).

calls to the egos are excluded. The magnitude of the treatment effect on alters is stronger in the long run (Table 9) than in the short run (Table 8), suggesting that some time is needed after the promotion for the effect of the campaign to propagate from egos to alters. For the sake of brevity, we show the regression results only for minutes. We obtain similar results when estimating calls and SMS. These results are given in Web Appendix A4.

Insert Tables 8 and 9 here

Alter suspension and churn: We formally test for the differences in suspension and churn by estimating a binary probit model for each of the behaviors both in the short and in the long term.[16] More specifically, we estimate the following models:

$$Prob(y_{ijt}^{alter}) = P(\pi_0 + \pi_1 T_i + \sum_{\tau=2}^{6} \pi_\tau D_{\tau t} + \nu_{itj} > 0) \quad \text{for t=1,…,6,} \quad (5)$$

$$Prob(y_{ijt}^{alter}) = P(\theta_0 + \theta_1 T_i + \sum_{\tau=8}^{12} \theta_{\tau-6} D_{\tau t} + \nu_{ijt} > 0) \quad \text{for t=7,…,12.} \quad (6)$$

Consistent with the notation in Equations (3) and (4), y_{ijt}^{alter} is a binary variable indicating whether alter j of ego i is suspended/churned in week t and T_i indicates whether ego i was assigned the treatment. The weekly dummies are defined as in Equations (1) through (4) and ν_{ijt} are normally distributed with 0 mean and variance $\sigma_{\nu,s}^2$ and $\sigma_{\nu,l}^2$ for the short and long term, respectively. The results for the probit regressions (last two columns of Tables 8 and 9 confirm that alters of treated egos exhibit statistically significantly lower churn in the long term. This finding is consistent with previous work that has shown that a decrease in usage often precedes customer churn (e.g., Ascarza and Hardie 2013; Neslin et al 2006). Regarding suspension behavior, while there is a negative effect both in the short and in the long run, this effect is not statistically significantly different from zero.

[16] Note that, unlike usage, the models for suspension and churn are not estimated in differences from the pre-campaign period. Therefore, it is possible that not controlling for individual-specific effects in the estimation would lead to inefficient (but still consistent) parameter estimates. We estimate a panel-data model that clusters the data at the alter level to appropriately estimate the standard errors of the estimated regression effects.

Thus, combining the results from Figure 3, and Tables 8 and 9, we have empirically demonstrated that the CRM marketing campaign had a positive impact on non-targeted connected customers of the treatment group as these customers have higher usage and lower churn than the non-targeted connected customers of the control group.

3.3. Investigating the social effect of targeted promotions

We conceptualize the pattern of our findings so far in Figure 4a. We have shown that the marketing intervention affects not only the targeted customers (Section 3.1, and represented by arrow A in Figure 4a), but also those who are connected to them (Section 3.2, and represented by arrow B in Figure 4a). In particular, we find that alters of treated egos have higher consumption and lower long-term churn, as compared to alters whose egos were not treated. The latter finding is particularly interesting because, unlike the egos, alters were not directly targeted by the company, neither did they receive any direct benefit from the campaign. Thus, while we empirically find that treatment has a statistically significant effect on the alters' behavior, we cannot claim that the treatment itself has a *direct* effect on the alters, as depicted by arrow B in Figure 4a. Instead, the effect of the campaign must have propagated to the alters through the behavior of their egos (as depicted in Figure 4b).[17]

Insert Figure 4 here

As discussed in Section 3.2, we postulate that the propagation of the campaign from egos to alters is due to increased consumption of the egos and specifically, increased consumption between the ego and her alters. This type of indirect effect is consistent with the presence of network externalities among the alters who, after the campaign, face a more active local

[17] One might be tempted to run a standard mediation analysis of the effect of treatment on alters' behavior through ego behavior. However, it should be noted that such mediation is unnecessary because a direct effect of treatment on alters is theoretically implausible as alters were never directly exposed to the treatment. Moreover, as we will discuss later, one cannot run such a mediation analysis because the mediator (ego behavior) is endogenous.

telecommunications network from which they derive higher value, hence increase their consumption. In this section we empirically test the causal link between ego usage and alter usage and churn. Moreover, we provide further empirical support for network externality effects by exploring the moderating effect of the strength of the tie between the ego and the alter on the effect of campaign on the alter.

3.3.1. The effect of increased activity of egos on alters' usage and churn

We investigate whether increased communication between the ego and their alters in the weeks immediately following the campaign (i.e., short term) causes an increase in usage and lower churn among alters in subsequent weeks (i.e., long term). That is, we are interested in estimating the dashed-arrow in Figure 4b. A simple regression model that regresses the alter usage on ego usage will likely suffer from an endogeneity bias due to the presence of omitted variables that could affect the usage of both egos and alters. For instance, a drop in network coverage quality in a certain area could lead to both egos and alters (living nearby) decreasing their consumption, and in some cases, even subsequently churn. Similar arguments could be made if one considers the effect of competitors running promotional campaigns or changes in demand around the holiday season. While we can control for some unobserved shocks that are common to all users in the network, such as holiday season effects, it is practically impossible to control for all unobserved common shocks that are particular to every pair of an ego and an alter. As a consequence, simply regressing changes in alter usage (dependent variable) on changes in ego usage (independent variable), even when controlling for other observed factors, can lead to biased estimates of the regression parameters. It should be noted that our analysis thus far of the (causal) effect of the marketing promotion on the alters' usage and churn does not suffer from endogeneity because the treatment variable is exogenous by design and is therefore uncorrelated with any unobservable. The endogeneity problem only emerges when one tries to establish a causal link between ego usage and alter usage or churn.

To address this challenge, we employ an instrumental variable (IV) approach and use the experimental treatment dummy variable as an IV for the (endogenous) ego usage variable. There are two main reasons why the treatment dummy variable is a good candidate for an IV in this analysis. First, treatment is randomized and thus by construction is uncorrelated with any omitted variables in the regression. Second, as the analyses in Section 3.1 shows, the treatment significantly impacted ego usage. We use the control function approach (Petrin and Train 2010; Germann, Ebbes and Grewal 2015) to estimate the model. We choose weeks 1—6 (short-term) to measure egos' behavior and weeks 7—12 (long-term) to measure alters' behavior. Further details about the IV model, variable specification, estimation and robustness checks, are provided in the Web Appendix A5. We would like to highlight that the using the treatment dummy as the instrumental variable helps us split the overall effect of treatment on alter behavior from Section 3.2, into the effect of treatment on the communication between the ego and her alters and the effect of the communication between the ego and her alters on alter behavior. If treatment were the only regressor, we could have arithmetically calculated the effect of the communication between the ego and her alters on alter usage for the linear case, using the results in Sections 3.1 and 3.2. However as we use also weekly dummies as control variables, we have to use an IV regression.

Insert Table 10 here

Table 10 shows the results of the IV regression analyses for the different types of activities (usage and churn) and different specifications of the communication between the ego and the alters. First, as indicated by the last row on Table 10 (1st stage t-stat) we find that, as expected, the instrument has a strong and significant positive effect on the endogenous variables (ego-to-alter usage). More importantly, the results for the three specifications of ego activity are convergent; an increase in short-term ego usage (minutes called, number of calls and number of SMSs) post intervention, leads to an increase in alter usage and reduction in alter churn.

Hence, these results corroborate that the marketing campaign has a spillover effect that propagates to non-targeted users *through the increased usage* of the targeted customers. As discussed earlier, we postulate that the increase in ego usage (due to the marketing campaign) induces a more active local network around the egos, generating a positive network externality for the alters. Next, we provide additional support for this account by investigating the role of tie strength in moderating the treatment effect.

3.3.2. The moderating effect of tie strength

If indeed the indirect effect of the targeted promotion on the non–targeted customers (i.e., alters) propagates through the egos, then the treatment effect should be stronger for dyads of egos and alters that have stronger ties (Manchanda et al. 2015). We investigate this conjecture by quantify the moderating role of the strength of the relationship between egos and alters on the effect of treatment on alters' usage (arrow B in Figure 4a).[18] We operationalize tie strength ($Strength_{ij}$) as the average number of minutes (in logs) that alter *j* called ego *i* during the 4 weeks prior to the intervention. We measure tie strength *prior* to the campaign to ensure independence between the measure of tie strength and treatment. Extending Equation (4), we estimate the following model:

$$\Delta y_{ijt}^{alter} = \varphi_0 + \varphi_1 T_i + \varphi_2 \, Strength_{ij} + \varphi_3 \, T_i \times Strength_{ij} + \sum_{\tau=8}^{12} \varphi_{\tau-4} D_{\tau t} + \varepsilon_{\,ijt}, \quad (10)$$

[18] An alternative approach would be to test the moderating role of tie strength on the effect of ego usage on alter usage (arrow C in Figure 44b). One challenge with such approach is that both independent variables (ego usage and its interaction with tie strength) are endogenous. While in theory this could be estimated with a single instrument, this analysis is likely to be inefficient and less robust (Wooldridge 2007).

for $t = 7, \ldots, 12$. This equation adds two terms to Equation (4), the main effect of $Strength_{ij}$ and the interaction between $Strength_{ij}$ and the treatment variable. As dependent variable we take the (differenced) weekly number of outgoing minutes the alter talked to any connection other than the ego (Δy_{ijt}^{alter}).

Insert Table 11 here

As can be seen in Table 11, we find a significant and positive interaction effect between tie strength and the campaign treatment ($\varphi_3 > 0$), indicating that the social effect presented in Section 3.2 is even larger for ego-alter pairs with a stronger connection prior to the campaign. Because the dependent variable excludes communication from each alter to her ego, reciprocity in calls cannot account for this effect. In a separate analysis, we have also operationalized tie strength as the number of minutes the ego called the alter before the intervention. We find a similar positive interaction effect between treatment and tie strength (detailed results of this analysis are in the Web Appendix A6).

An alternative variable that is expected to moderate the social effect is the number of connections each alter has. All else being equal, one would expect the ego to play a more prominent social role for alters who have fewer connections. Accordingly, we posit a negative sign for the interaction between treatment and number of alter's connections. With reference to Equation (10), we substitute $Strength_{ij}$ by the number of connections alter *j* has, operationalized as the average number of customers of the focal provider that alter *j* communicated with in the 4 weeks prior to the campaign. The results of this regression are given in Table 12 and are consistent with the results reported in Table 11. Specifically, across the two activity types, we find that the lower the role the ego plays in the alter's network (i.e. when the alter has more nodes in her network), the weaker the treatment effect is ($\psi_3 < 0$).

Insert Table 12 here

In sum, using two measures of social importance (tie strength and alter's number of connections), we find that the stronger the social tie between egos and alters, the stronger the propagation of the campaign from the targeted customer to the (non-targeted) connections.

4. Managerial relevance of the social effect

Thus far, we have shown that ego usage (Section 3.1) and alter usage (Section 3.2) increase *because* of the treatment. We have also demonstrated the presence of a statistically significant propagation of the targeted campaign to non-targeted customers (Section 3.3). In this section, we quantify the managerial relevance of the social impact of the CRM campaign. That is, how big is the spillover effect from egos to alters?[19] We calculate two metrics to quantify the social effect of the campaign. The first metric is the magnitude of the *consumption spillover*, which we compute as the percentage increase in usage among the customers who were directly targeted (egos), relative to the percentage increase in usage among their alters. The second metric relates to the monetary value of the social effect, which we compute as the *incremental* value the firm receives from the non-targeted customers (i.e., alters) as a consequence of having targeted their connections (i.e., egos).

4.1. Quantifying the consumption spillover

Using the estimated models for the effect of treatment on ego usage (Tables 5 and 6), and the effect of treatment on alter usage (Tables 8 and 9), we compute the percentage increase in usage for both egos and alters due to the treatment for the 12 weeks following the campaign. Using the average consumption for each customer for the four weeks prior to the campaign as baseline, we

[19] Note that we can only make that comparison for usage and not for churn because in our context egos belong to a pre-paid plan in which churn is hardly ever observed.

convert the parameter estimates to percentage increase by transforming the diff-in-diffs regression specification into usage levels. Following these calculations, we find that the campaign caused a 34.8% increase in number of minutes called by egos in the 12 weeks following the campaign. The corresponding increase in alter usage is 9.7%. That is, the spillover effect of the campaign on alter usage is approximately 28% (0.097/0.348). It is important to recall that while the egos received an economic incentive to increase their usage, the alters did not.

Using the same approach, we also compute the size of the spillover effect for different levels of tie strength. To do so, we use the parameter estimates of the model that incorporates the interaction between treatment and tie strength (Table 11), and calculate the percentage increase in usage for alters whose tie strength is one standard deviation above and below the population mean. For alters who had stronger relationships with their egos, the increase in usage is 14.3% (which translate to a spillover of 41% = 0.143/0.348) whereas those with weaker ties the increase is 5.3%, which corresponds to a spillover of 16%.

4.2. Measuring the financial value of the spillover effect

CRM marketing campaigns are commonly evaluated based on the lift in profitability of the targeted customers relative to the incurred costs of the campaign. In this paper, we have demonstrated that a marketing campaign can also affect the usage and churn, and hence profitability, of the customers connected to the targeted customers, suggesting that there is an additional value obtained from the alters that should be taken into account when firms evaluate the return on investment of their targeted campaigns. Here we quantify that incremental value by comparing post-campaign profit obtained from the alters of treated customers with that of the alters of customers in the control group.

In order to do so, and given that we did not obtain information about the profitability of each individual customer, we need to rely on certain assumptions about average measures of profitability for the customers in our sample. (Details on all the assumptions regarding consumption levels, discount factor, operating margins, and calculations are provided in the Web Appendix A7.) Using these assumptions, we estimate that an alter whose ego was treated generates $0.85 more profit than an alter whose ego was not treated. In other words, above and beyond the effect of the marketing campaign on the targeted customers, this campaign also increases the profits of the non-targeted (but connected) customers by $0.85 per alter. Given that egos have, on average, 5 alters each, the campaign generates an extra $4.25 in profits per targeted customer from social spillover.

We acknowledge that these "back of the envelope" calculations of additional spillover profitability are suggestive as they are based on average levels of revenue and are dependent on several assumptions. Nevertheless, we believe that this analysis highlights that the social effect of CRM campaigns can have a substantial positive financial impact when network externalities are present.

5. General discussion

In this paper we quantify the social effect of CRM marketing campaigns. We show that a CRM campaign that is aimed at changing the behavior of some customers can propagate through the social network of the targeted customers and also affect the behavior of non-targeted, but connected customers. In the context of telecommunications, we find that the social connections of targeted customers were more likely to increase their consumption and less likely to churn due to a campaign that was neither targeted at them nor offered them any incentives to change their behavior. In particular, we estimate a social multiplier of 1.28. That is, the spillover effect of the campaign to non-targeted customers is 28% of the effect of the campaign on the targeted customers. Financially, this propagation translates to an additional profit of $0.85 per non-targeted customer who is connected to a targeted customer.

Using a randomized field experiment, we estimate the causal effect of a CRM campaign on both the targeted and the non-targeted customers. We further leverage the experimental design via an IV regression to estimate the causal effect of the activity of the egos on the activity of their alters. We show that the effect of the campaign propagates from egos to alters through an increase in the activity from the targeted customer to her alters. Furthermore, we observe a stronger social effect for dyads with stronger ties. While we do not observe the content of a conversation between an ego and an alter, it is unlikely that word of mouth is the main driver of the propagation of the CRM campaign. In fact, if the ego were to discuss the campaign with her alters, we would expect a negative effect of the campaign on alters because the targeted campaign is not available to them.

We put forward a network externality explanation, which is consistent with our finding that customers increase the usage of the service and are less likely to churn when their (local) network becomes more active. Network externality research has shown that a larger and more active network often leads to higher value to the network members (e.g., Aral and Walker 2011; Nitzan and Libai 2011). Thus, the decrease in churn and suspension among alters can be easily attributed to such network externality effects. What is less obvious is why, conditioned on not churning, and excluding communication with the ego, alters increase their usage after their ego has been treated. That is, why would an alter call her other connections more because her (treated) ego calls her more? While given the nature of the data we cannot uniquely pinpoint the underlying mechanism of this finding, we postulate that the increased activity in the network (as a result of the increased usage from the ego to the alter) allows the alters to perceive higher value of their network, which subsequently leads to higher levels of usage (Aral and Walker 2011l; Manchanda et al 2015). For example, given the increasing number of alternative methods of communication available to customers (e.g., WhatsApp, WeChat, Skype, Google Hangouts, multiple SIM cards), an increased perceived value of one of the communication networks, can motivate the alter to use that particular network more (often) as the primary mode for communication. We leave the investigation of the specific mechanisms underlying how network externality affects usage and churn for future research.

Our research has clear implications for marketing managers. In business contexts where customers are connected, targeted campaigns might actually have higher return on investment than what is currently believed. Moreover, our findings suggest that firms should leverage social effects in deciding which customers to target. On the one hand, the CRM practice has focused primarily on targeting customers based on the expected lift in profitability of the targeted customer. On the other hand, the social contagion literature has, for the most part, ignored profitability and primarily focused on targeting "hubs" with strong social influence. Our results suggest that firms should consider a combination of these two effects and target customers with the highest lift in *social profitability* due to the campaign. Beyond the profitability of the campaigns, a firm operating close to its capacity limits should take into account the social impact of its targeted actions. For example, in contexts with capacity constraints (e.g., wireless providers in developing countries) or in cases in which utilization capacity directly links to customer satisfaction (i.e., gyms), companies should anticipate increased activity not only from the targeted customers but also from those connected to them.

Our research contributes to the broader CRM literature (e.g., Berger et al. 2002; Rust and Verhoef 2005; Kumar, Lemon and Parasuraman 2006) that has focused on measuring the impact of marketing actions on the (targeted) individual customers. In this research we quantify the effects of marketing actions *beyond the target customer* and show how, in the presence of network externalities, the impact of marketing activities on firm profitability might be higher than otherwise estimated. Our work likewise complements extant work on social influence in new product introduction and customer acquisition (e.g., Iyengar, Van den Bulte and Valente 2011; Schmitt, Skiera and Van den Bulte 2011). Consistent with the findings of Nitzan and Libai (2011), our research confirms that social influence is not limited to new behaviors (e.g., adoption of new products), but is also present in marketing campaigns aimed to change the behavior of existing customers. More broadly, our work complements the research on the spillover effects of marketing actions. Previous research has shown that marketing campaigns can spill over to brands that are “connected” to the focal brand (e.g., Erdem and Sun 2002; Rutz and Bucklin 2011; Chae et al. 2016). In this research, we extended the notion of a marketing action spillover from one customer to another.

We chose to investigate the propagation of social CRM campaigns in the context of a telecommunications firm. There are several reasons for this choice. First, the telecommunications context allows us to directly observe the customer’s network. Second, the telecommunications industry is of major interest to CRM academics and practitioners (Rivera and van der Meulen 2014). And third, the context of telecommunications is characterized with strong network externality effects. Consequently, we expect a weaker spillover effect in applications that are characterized by low network externality, such as, for example, consumer packaged goods applications. That being said, we believe that our findings have implications for industries other than telecommunication, such as file sharing services, peer-to-peer market places, payment services or online games. Elaborating on the generalizability of our results, there are three main conditions needed for a business setting to observe and leverage our results: (1) a reasonable degree of network externalities, (2) the ability to individually target marketing actions, and (3) the observation of the customers' social network. While the first condition is a necessary condition for the effect to occur, the remaining two conditions are needed for the firm to measure the social effect and leverage our findings. We encourage firms across different sectors to better develop their capabilities that allow them to measure social interactions and individually target their marketing actions.

The data we had access to imposes some limitations on our research. First, we investigate a conservative propagation of the campaign only to first-degree connections. Furfure research could investigate whether campaigns propagate beyond the first degree. However, in looking beyond first-degree effects, potential contamination and interference in network experiments becomes more challenging to handle (Aral 2015). Second, the campaign we observe was a successful one in terms of affecting the targeted customers. It is likely that less successful campaigns will have limited propagations. In some cases marketing campaigns may even have a negative effect on the targeted customers (e.g., Ascarza, Iyengar and Schleicher 2016). Do campaigns with negative direct effect generate negative spillover effects? Given the documented network effect of churn (Nitzan and Libai 2011), and the word of mouth effect of negative information (Moldovan and Goldenberg 2004) one may expect negative propagation for such unsuccessful campaigns. We encourage future researchers to investigate these questions as well as measure the degree of the propagation of CRM campaigns in different business settings.

In sum, we provide empirical evidence that CRM campaigns can have a spillover effect beyond the target customer. This finding has implications for the targeting and evaluation of such campaigns. We hope that this research will serve as a stepping-stone in changing the view in the CRM community from thinking not only in terms of customer value, but also in terms of *customer social value*.

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TABLES and FIGURES

	Mean	St. Dev.	25th	50th	75th	N
Minutes inbound	9.6	40.3	0.2	1.9	7.7	961
Minutes outbound	35.1	62.2	4.8	19.5	43.9	961
Calls inbound	3.4	6.0	0.3	1.3	4.0	961
Calls outbound	22.5	28.2	5.3	14.3	28.3	961
SMS inbound	35.9	95.3	1.0	6.3	26.5	961
SMS outbound	71.3	154.1	3.8	16.3	62.8	961

Usage metrics are weekly averages (during the 4 weeks before the intervention), then averaged across customers.

Table 1: Descriptive statistics of ego behavior before the experiment

	Mean	St. Dev.	25th	50th	75th	N
Usage						
Minutes inbound	58.8	102.7	10.9	30.2	68.0	4,700
Minutes outbound	69.3	132.4	10.7	33.0	77.7	4,700
Calls inbound	25.5	33.2	7.0	15.8	31.5	4,700
Calls outbound	46.5	66.2	11.3	27.0	55.8	4,700
SMS inbound	169.7	255.7	28.0	74.5	198.2	4,700
SMS outbound	127.3	222.0	10.8	43.5	141.9	4,700
Suspension						
% alters suspended	12.1	21.1	0.0	0.0	17.9	961

Usage metrics are weekly averages (during the 4 weeks before the intervention), then averaged across customers. Suspension is computed at the moment of the intervention, then averaged across customers.

Table 2: Descriptive statistics of alter behavior before the treatment

	Control		Treatment		Difference		
	Mean	SE	Mean	SE	Diff.	SE	p-value
Ego usage (log)							
Inbound SMS	1.30	0.07	1.32	0.05	-0.03	0.08	0.76
Outbound SMS	2.73	0.08	2.75	0.06	-0.03	0.10	0.79
Inbound MIN	1.02	0.05	1.01	0.04	0.01	0.06	0.91
Outbound MIN	2.54	0.07	2.59	0.05	-0.04	0.08	0.58
Inbound CALLS	2.14	0.09	2.17	0.07	-0.04	0.11	0.73
Outbound CALLS	2.81	0.10	2.91	0.07	-0.10	0.12	0.40
Alter usage (log)[1]							
Inbound SMS	2.43	0.09	2.54	0.07	-0.11	0.12	0.37
Outbound SMS	2.53	0.10	2.59	0.07	-0.06	0.12	0.65
Inbound MIN	1.99	0.08	2.10	0.06	-0.11	0.09	0.25
Outbound MIN	2.39	0.09	2.44	0.07	-0.05	0.11	0.65
Inbound CALLS	3.18	0.11	3.25	0.08	-0.08	0.14	0.59
Outbound CALLS	2.67	0.11	2.75	0.08	-0.07	0.14	0.61
Alter suspension							
% alters suspended	13.16	1.19	11.46	0.82	1.70	1.41	0.23
Other covariates							
Degree (# alters)	5.37	0.46	4.61	0.20	-0.76	0.44	0.08
# connections (of the alters)	5.39	0.18	5.12	0.14	-0.27	0.23	0.24

[1]In order to check the randomization at the randomized unit level, we test the differences in alter usage and degree with a between-effect regression (i.e., averaging alter usage at the ego level across alters and weeks, and regressing the treatment dummy on those averages). We also estimated these differences at the alter level including a random-effect for egos. The random-effect regressions provided similar results.

Table 3: Randomization check in all observed variables in the four weeks before the experiment

	Control	Treatment	Difference	p-value
Suspended status in week 1	47.6%	35.4%	-12.2%	0.00
Suspended status in week 12	57.8%	48.4%	-9.4%	0.00
Difference in minutes	-13.17	-6.62	6.54	0.04
Difference in calls	-8.89	-5.50	3.40	0.02
Difference in SMS	-30.46	-28.79	1.68	0.83
Difference in log(minutes)	-1.23	-0.96	0.27	0.00
Difference in log(calls)	-1.11	-0.89	0.21	0.01
Difference in log(SMS)	-1.34	-1.16	0.18	0.05

Usage behavior includes all outgoing communications initiated by the ego during the 12 weeks following the intervention.

Table 4: Average ego usage and suspension post-intervention

	Outbound usage		
	Minutes	Calls	SMS
Treatment	0.235***	0.188***	0.150***
	(0.045)	(0.039)	(0.047)
Constant	−0.823***	−0.675***	−0.795***
	(0.061)	(0.052)	(0.062)
Week dummies	Yes	Yes	Yes
Observations	5,702	5,702	5,702

Short-term effects of treatment on ego usage. *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Robust standard errors in parenthesis. The number of observations is 6 (weeks) x 961 (egos), excluding egos that cancelled their contract in a particular week.

Table 5: Short-term effect of treatment on ego usage (weeks 1-6 after the treatment)

	Outbound usage		
	Minutes	Calls	SMS
Treatment	0.335***	0.257***	0.230***
	(0.048)	(0.041)	(0.05)
Constant	−1.306***	−1.173***	−1.294***
	(0.064)	(0.056)	(0.067)
Week dummies	Yes	Yes	Yes
Observations	5,625	5,625	5,625

Long-term effects of treatment on ego usage. *** p<0.01, ** p<0.05. Robust standard errors in parenthesis. The number of observations is 6 (weeks) x 961 (egos), excluding egos that cancelled their contract in a particular week.

Table 6: Long-term effect of treatment on ego usage (weeks 7-12 after the treatment)

	Control	Treatment	Difference	p-value
Short Term (weeks 1—6)				
Suspended status in week 6	25.7%	23.5%	-2.2%	0.09
Churned by week 6	1.7%	1.4%	-0.3%	0.48
Difference in minutes	-9.65	-5.94	3.71	0.11
Difference in minutes (excl. ego)	-10.00	-5.78	4.22	0.06
Difference in log minutes	-0.68	-0.60	0.08	0.04
Difference in log minutes (excl. ego)	-0.68	-0.60	0.08	0.04
Long Term (weeks 7—12)				
Suspended status in week 12	30.7%	27.6%	-3.1%	0.02
Churned by week 12	3.7%	2.4%	-1.3%	0.01
Difference in minutes	-20.21	-12.60	7.60	0.02
Difference in minutes (excl. ego)	-19.35	-12.06	7.29	0.02
Difference in log minutes	-1.01	-0.90	0.10	0.03
Difference in log minutes (excl. ego)	-0.99	-0.89	0.11	0.03

Table 7: Average alter usage, suspension and churn post-intervention

	Outbound minutes			
	Total	Total (excl. ego)	Did suspend	Did churn
Treatment	0.0764***	0.0770***	-0.0534	-0.0448
	(0.019)	(0.019)	(0.043)	(0.076)
Constant	−0.598***	−0.600***	−1.624***	−2.605***
	(0.026)	(0.026)	(0.039)	(0.088)
Week dummies	Yes	Yes	Yes	Yes
Observations	27,987	27,987	27,987	27,987

Short-term effects of treatment on alter usage. Linear (diff-in-diffs) regression for usage. Probit regression for suspension and churn *** p<0.01, ** p<0.05. Robust standard errors in parenthesis. The number of observations is 6 (weeks) x 4,700 (alters), excluding alters that are cancelled in a particular week.

Table 8: Short-term effect of treatment on alter usage and churn (weeks 1-6 after the treatment)

	Outbound minutes			
	Total	Total (excl. ego)	Did suspend	Did churn
Treatment	0.0984***	0.100***	−0.0247	−0.236***
	(0.022)	(0.022)	(0.04)	(0.08)
Constant	−0.847***	−0.835***	−1.051***	−2.646***
	(0.03)	(0.03)	(0.032)	(0.096)
Week dummies	Yes	Yes	Yes	Yes
Observations	27,598	27,598	27,598	27,598

Long-term effects of treatment on alter usage. OLS (diff-in-diffs) regression for usage. Probit regression for suspension and churn *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors in parenthesis. The number of observations is 6 (weeks) x 4,700 (alters), excluding alters that are cancelled in a particular week.

Table 9: Long-term effect of treatment on alter usage and churn (weeks 7-12 after the treatment)

	Alter usage (excl. ego) as dependent variable					
	Minutes	Churn	Calls	Churn	SMS	Churn
Ego to Alter (regressor)						
Minutes	3.204***	−7.525***				
	(0.737)	(2.749)				
Calls			1.765***	−7.494***		
			(0.623)	(2.632)		
SMS					0.891*	−5.392***
					(0.52)	(1.878)
Intercept	−0.0315	−4.533***	−0.458***	−3.999***	−0.520**	−5.274***
	(0.171)	(0.654)	(0.105)	(0.448)	(0.242)	(0.888)
Week dummies	Yes	Yes	Yes	Yes	Yes	Yes
Observations	27,598	27,598	27,598	27,598	27,598	27,598
1st stage t-stat	3.957	3.957	5.901	5.901	4.083	4.083

Effect of short-term ego-to-alter usage on long-term alter usage and churn using the control function approach. The regressor ego-to-alter usage is operationalized as the average of (differenced) ego usage during weeks 1 to 6. The dependent variable of alter usage is operationalized as the average of (differenced) alter usage during weeks 7 to 12. Bootstrapping is used to estimate the Robust standard errors (in parentheses). *** p<0.01, ** p<0.05, * p<0.1.

Table 10: Effect of short-term ego-to-alter usage on long-term alter usage and churn (Instrumental variable regressions)

	Outbound Minutes	
	Total	Total (exc. ego)
Treatment	0.0955***	0.0978***
	(0.0226)	(0.0226)
Tie strength	−0.216***	−0.179***
	(0.0202)	(0.0194)
Tie strength * Treatment	0.0523**	0.0591**
	(0.025)	(0.0244)
Constant	−0.844***	−0.833***
	(0.0303)	(0.0302)
Week dummies	Yes	Yes
Observations	27,598	27,598

Long term effects on alter usage. *** p<0.01, ** p<0.05. Robust standard errors in parenthesis. Tie strength is operationalized as the number of minutes the alter called the ego before the intervention.

Table 11: Long-term effect of treatment on alter usage (weeks 7-12 after the treatment) moderated by tie strength

	Outbound Minutes	
	Total	Total (exc. ego)
Treatment	0.0491**	0.0504**
	(0.0225)	(0.0224)
# connections	−0.224***	−0.225***
	(0.0178)	(0.0177)
# connections * Treatment	−0.0844***	−0.0868***
	(0.0232)	(0.0231)
Constant	−0.819***	-0.807***
	(0.03)	(0.0299)
Week dummies	Yes	Yes
Observations	27,598	27,598

Long term effects on alter usage. *** p<0.01, ** p<0.05, * p<0. Robust standard errors in parenthesis.

Table 12: Long-term effect of treatment on alter usage (weeks 7-12 after the treatment) moderated by the number of alter connections

FIGURES

Figure 1: Intervention via SMS

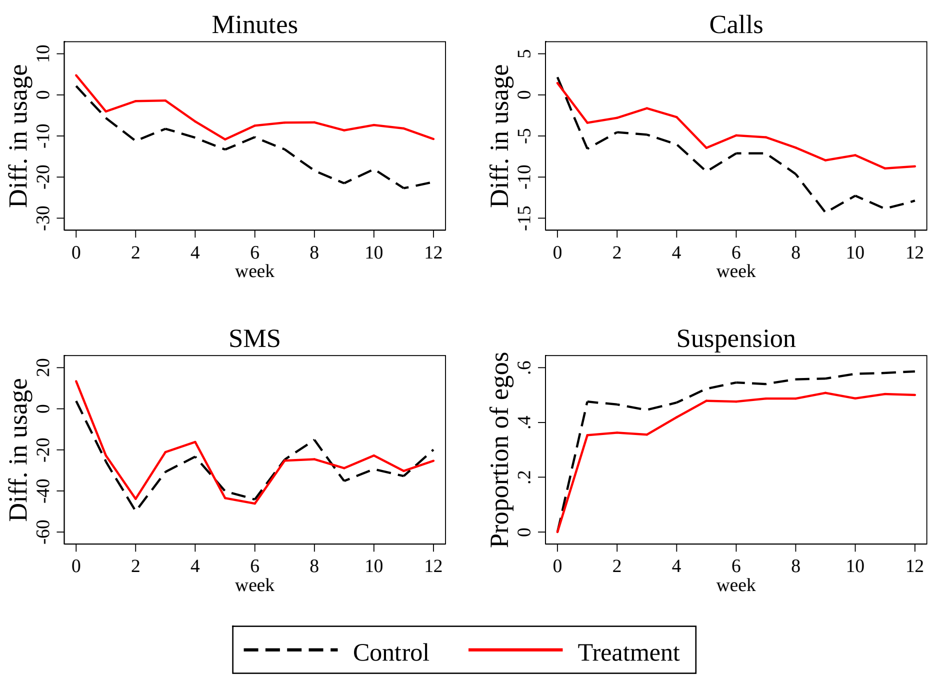

Figure 2: Post-treatment ego *usage* (differences between weekly consumption after intervention and weekly average before the intervention) and *suspension* (average number of customers in suspended status in a given week), by treatment condition

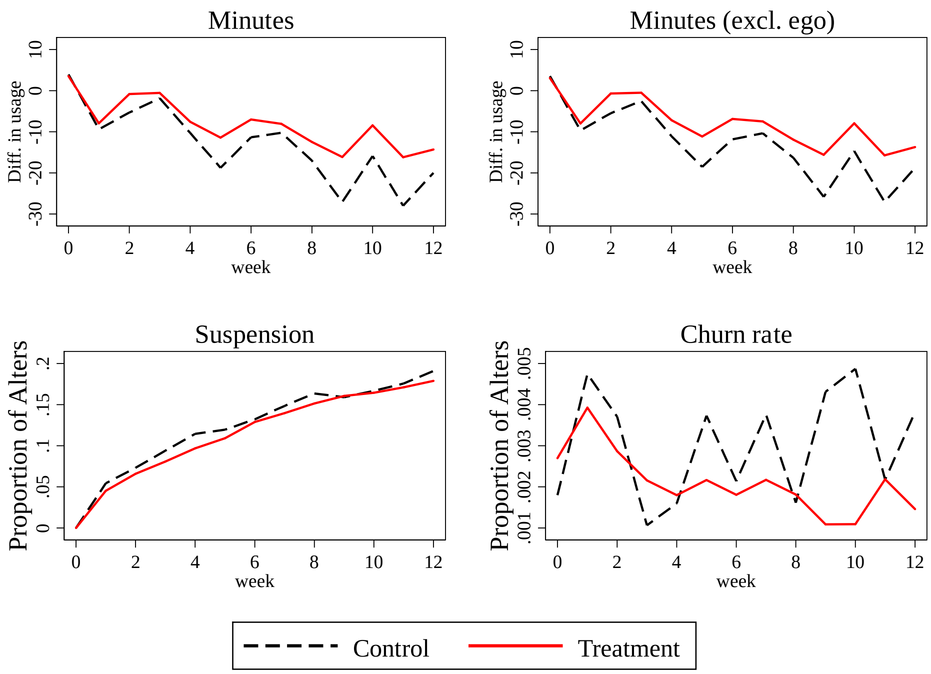

Figure 3: Post-treatment alter *usage* (differences between weekly consumption after intervention and weekly average before the intervention), *suspension* (average number of customers in suspended status in a given week) and *churn* (average number of customers who cancel in a given week), by treatment condition

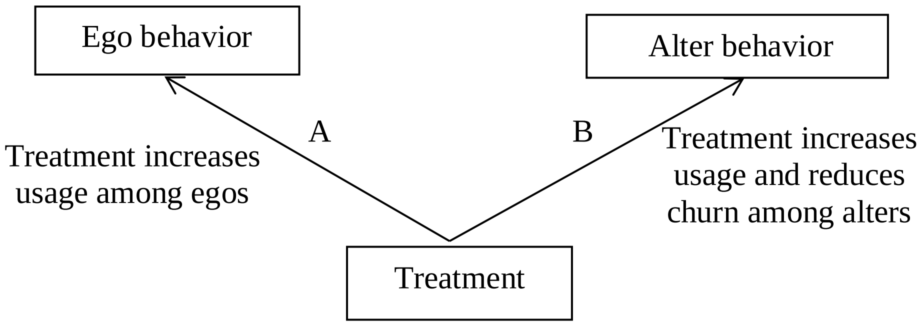

Figure 4a: Direct effect of treatment

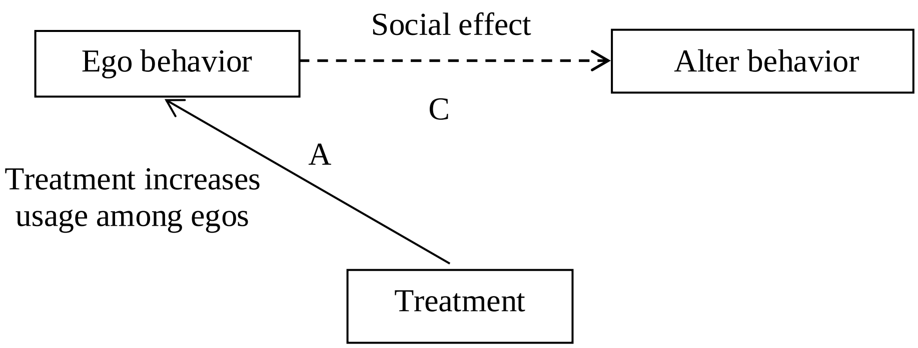

Figure 4b: Treatment propagates through ego behavior

Figure 4: Schematic diagram of the effect of treatment on ego and alter behavior