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Do Your Online Friends Make You Pay? A Randomized Field Experiment on Peer Influence in Online Social Networks

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Demonstrating compelling causal evidence of the existence and strength of peer-to-peer influence has become the holy grail of modern research in online social networks. In these networks, it has been consistently demonstrated that user characteristics and behavior tend to cluster both in space and in time. There are multiple well-known rival mechanisms that compete to be the explanation for this observed clustering. These range from peer influence to homophily to other unobservable external stimuli. These multiple mechanisms lead to similar observational data, yet have vastly different policy implications. In this paper, we present a novel randomized experiment that tests the existence of causal peer influence in the general population—one that did not involve subject recruitment for experimentation—of a particular large-scale online social network. We utilize a unique social feature to exogenously induce adoption of a paid service among a group of randomly selected users, and in the process develop a clean exogenous randomization of treatment and control groups. A variety of nonparametric, semiparametric, and parametric approaches, ranging from resampling-based inference to ego-level random effects to logistic regression to survival models, yield close to identical, statistically and economically significant estimates of peer influence in the general population of a freemium social network. Our estimates show that peer influence causes more than a 60% increase in odds of buying the service due to the influence coming from an adopting friend. In addition, we find that users with a smaller number of friends experience stronger relative increase in the adoption likelihood due to influence from their peers as compared to the users with a larger number of friends. Our nonparametric resampling procedure-based estimates are helpful in situations of networked data that violate independence assumptions. We establish that peer influence is a powerful force in getting users from free to premium levels, a known challenge in freemium communities.

Keywords: peer effects; randomized experiment; social contagion; nonparametric inference; freemium communities; online social networks
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1. Introduction and Background

The general challenge of demonstrating causal inference from observational data has been immortalized in Manski's (1995) reference to the simultaneous movements of a man and his image in the mirror. He asks, "does the mirror cause the man's movement or reflect them?" (p. 1) and concludes that without understanding optics and human behavior, we cannot really tell. Interestingly, this quote from the pre-Facebook era is extremely relevant to the causality questions that arise in today's digital age. The growth of online social networks and the wide availability of online data has renewed interest in the identification of whether influence is "at play" in the general population of users of such networks. Today, a billion plus global citizens are socially connected by general networks such as Facebook and Twitter, as well as by niche networks such as Last.fm, Spotify, and LinkedIn among others.[1] These online social networks are credited with playing roles that range from inspiring political action to driving viral and word-of-mouth spread of products and services (Aral and Walker 2011, Hill et al. 2006, Iyengar et al. 2011, Manchanda et al. 2008, Mayzlin 2006), and as such, represent a vast reservoir of social and economic influence. Central to tapping into this reservoir is the understanding of causal relationships that drive the spread of products, services, and information over these social networks—the central focus of this paper.

[1] Online social networks such as Facebook, with a billion users, and Twitter, with more than a billion users, are consuming an increasingly significant portion of our time and attention. A recent CMO study estimated that 2013 is the first year in which the amount of time spent on social media exceeded that spent on TV, and that Facebook gets one in eight minutes users spend on the desktop, and one in five on the mobile (see Miners and CMO Staff 2013).

It has been consistently demonstrated in the literature that in online social networks, user characteristics and behavior tend to cluster both in space and in time (Aral and Walker 2011). Interestingly, there are several different underlying mechanisms that can lead to this observed clustering with the most frequently cited ones being *peer influence* and *homophily*. Further, in addition to peer influence and homophily, other unobserved external correlations (Bakshy et al. 2012b), say a targeted marketing promotion to friends or offline interactions, can also generate similar observational data. Under the mechanism of peer influence, an individual causes her online friends to undertake a certain action, which in turn, leads to the observed correlation of the behavior of online friends. On the other hand, under the mechanism of homophily, an individual tends to befriend peers that are similar to her on observed and unobserved characteristics and possibly the environment they face. Under homophily, it is not surprising that behavior of an individual is correlated with the behavior of her friends. They may not influence each other at all, but the observed correlation of their actions comes from their intrinsic similarity. This underlying similarity is what forces them to independently make similar choices, leading to observed correlation between actions of online friends.

The importance of *disentangling* peer influence from homophily mechanisms and other confounders stems from the fact that despite leading to very similar observational data, the policy implications of each of these mechanisms are vastly different. Under peer influence, an effective policy may be to identify the most influential people and induce the desired behavior among them so that it would propagate through the social contagion. If, however, the causal mechanism at work is homophily or some other unobserved external source of correlation, promotion policies based on social contagion will have little effect (Aral 2011). Instead, under homophily, a careful segmentation-based targeting strategy might be preferred. Moreover, the mechanisms of peer influence and homophily are not necessarily mutually exclusive and may complement each other, implying that social contagion processes in real online networks may contain a complex mixture of peer influence and homophily. Underestimating peer influence is arguably even more deleterious. It may result in the decision maker underutilizing strategies that leverage the fact that peer influence has the added bonus of bringing with it the *social multiplier effect*. Manski (1995) provides an intuitive example of the social multiplier at work, describing a potential positive feedback loop of peer influence in the context of academic performance of high school students. Manski (1995) posits that if an increase in an individual student's academic performance causes the increase in the performance of the reference group of her peers, then this reference group may in turn increase the performance of that individual even further, and so on, leading to a positive, self-reinforcing feedback loop with the social multiplier effect. On the other hand, homophily-based mechanisms that arise out of similarity of individual characteristics or contextual information do not typically exhibit this multiplier effect, perhaps explaining the importance among researchers and practitioners about peer-influence-based approaches to diffusing products, ideas, and information. These factors make it critical, both for theory and for practice, to causally identify the presence of peer influence in the context of the general population of large-scale online social networks, the focus of this paper.

Our work is inspired by Aral and Walker (2011), who demonstrate, using an in vivo randomized experiment, that significant social contagion can be created by embedding viral features into product design. It also builds on the later work of Aral and Walker (2012), who identify characteristics of influential and susceptible people. Our work also links to the emerging stream of work showcasing the value of "social design" where products and services are designed with social features that help with initial adoption, sustained engagement, and user retention. Dou et al. (2013) develop an analytical model of how software firms can optimize the strength of networks effects, a potential peer influence mechanism, by adjusting the level of embedded social media features in the product's design. Hildebrand et al. (2013) provide evidence from the field (and from the laboratory) on the efficacy of social features, such as commenting on each others' designs of self-designed unique products, on the product quality. They find that social features such as receiving feedback from other community members on initial self-designs leads to less unique final self-designs, lower satisfaction with self-designed products, lower product usage frequency, and lower monetary product valuations. A related stream of work (Bakshy et al. 2012a, b) showcases the great potential of using randomized field experiments to study peer effects in contexts such as information diffusion and social cues in online social networks. Thus, we find that researchers are using the full array of methodologies available, ranging from analytical modeling to field studies to randomized experiments in the laboratory and the field to develop and sharpen our nascent understanding of the impact of social design. The interested reader is referred to Aral et al. (2013), who provide a nice taxonomy of social media design research along the dimensions of users and society, platforms and intermediaries, and firms and industries. Our study fits squarely in this stream of literature. We deploy a controlled randomized experiment in a large online social network and ask whether peer influence in the general population of users exists in the context of an economic decision involving real dollars. The context of our study is a *freemium* music-listening social network (Anderson 2008) called Last.fm.[2] A singular problem for the long-term viability of freemium communities is to convert free users into premium subscribers, the latter being far more profitable (Oestreicher-Singer and Zalmanson 2013). Our work is closely related to that of Oestreicher-Singer and Zalmanson (2013), who conceptualize a ladder of social engagement that leads users in freemium communities to climb from free to fee (premium). We add to this stream of monetizing freemium social communities by asking whether peer influence can play a causal role in converting users from free to premium levels of service. Note that in the case of Last.fm, based on 2009 numbers, free users yield approximately 12¢ per registered user in the network per month, as opposed to paid subscribers who are almost 24 times more valuable, paying $3 per month.

Interestingly, observational data that we collected from the Last.fm website revealed that (a) premium subscribers are extremely rare, accounting for only a few percent of all users, and (b) these premium subscribers are significantly more likely to be socially connected to other premium subscribers even controlling for the number of friends and other known covariates. However, as explained by Manski (1995), inferring the presence of peer influence from such observational data is not judicious. Specifically, there are several sorts of bias identified in making such an inference, including simultaneity (Godes and Mayzlin 2004), unobserved heterogeneity (Van den Bulte and Lilien 2001), homophily (Aral et al. 2009), and correlated effects (Manski 1995). Although multiple attempts have been made to identify peer effects using instrument variables based on network structure (Bramoullé et al. 2009, Oestreicher-Singer and Sundararajan 2010), natural experiments (Tucker 2008), and matched sample counterfactuals (Aral et al. 2009, Susarla et al. 2012, Oestreicher-Singer and Zalmanson 2013), each method has its limitations (Aral 2011, Manski 1995). Prior research has demonstrated that in real-life networks more than 50% of the perceived behavioral contagion can be explained by homophily (Aral et al. 2009), suggesting that homophily is a major force in social networks and must be carefully accounted for when estimating peer influence.

To overcome the aforementioned limitations of using observational data, we deploy the gold standard of randomized controlled trials to test our hypotheses of the presence (or lack of) peer influence (Aral and Walker 2011; Bakshy et al. 2012a, b). Manski (1995) touches on the possible reasons behind the lack of randomized trials involving general populations of different real-world networks. He reminds the reader that it is harder to draw inferences about the general population from a self-selected sample of recruited subjects. In addition to self-selection bias, Manski (1995) argues that generalizable analysis is limited to the observations that are made without undue *intrusion*, since people's behavior may change when they know they are being observed. At a high level, our experimental design, the details of which are presented in §3, contributes to the emerging literature on discerning peer effects in the online social graph in the following ways: (a) by estimating the average treatment effect on the nontreated (ATEN) (close to 97% of the population in our context), we complement the extant prior work that has focused on estimating the average treatment effect on the treated (ATET), completing the picture required to get to the average treatment effect (ATE); and (b) by computing the net total effect of peer influence, we provide a comprehensive picture of the scope of peer influence, which in and of itself is a complex multimechanism phenomenon. These two ideas go hand in hand. Prior literature is based on what are called blocking designs that, by construction, work at a mechanism level.

We present our findings by using a nonparametric inference procedure to cater to networked data that could violate independence assumptions required for traditional parametric statistical inference. In doing so, we are able to utilize our entire 1.2 million strong social network as our control group. Furthermore, we are able to empirically demonstrate that theoretical independence violations can be handled practically using ego-level random effects that lend themselves to standard statistical inference machinery. Our randomized experiment demonstrates that new adoptions were significantly higher in the treatment group versus the control group. Moreover, our nonparametric resampling test, logistic regression, and numerous robustness checks indicate that, on average, the odds of a user adopting the paid subscription increase by more than 60% due to peer influence when her friend is gifted a subscription, indicating significant causal peer effects in the monetization of social networks. In addition, we find that peer influence is weaker for users having a large number of friends. Finally, we

[2] Last.fm is a classical example of a freemium community featuring a large number of free users and a small number of premium subscribers. As is typical for a freemium community, the premium users bring in a disproportionately large share of company profits (Sweney 2010).

compare, in a predictive sense, the strength of peer influence versus homophily in our setting.

To appreciate the economic significance of a 60% increase in adoption rates due to peer influence, consider that although premium subscriptions are a rare event in the context of Last.fm, in totality these 3% of premium users are very valuable. They contribute to more than 18% of the site's total revenue (based on the 2009 numbers to which we had access). With modern social networks having over one billion users, a 60% increase in this kind of a "rare" event constitutes an absolute increase of *several million people*. Furthermore, from a practical point of view, our entire approach, motivated by the desire to test for peer influence in the average user of a social network, is conservative with respect to economic significance. Our randomization (detailed in §3) is such that the influencers in our study were just "average" users who exert significant peer influence on their friends. They are not especially influential users. A separate question for subsequent studies is to discover how much more influence we would get if we were to use a targeted sample that goes after influential people rather than an unbiased random sample of average users.

Formally stated, the main research objective of this study is the testing of the following hypotheses:

Hypothesis 1. *In an online social network, peer influence exists such that an individual's product adoption causes the adoption by her online friends.*

Hypothesis 2. *Peers with a small number of friends experience stronger relative increase in the adoption likelihood due to influence from their peers as compared to the peers with a large number of friends.*

Although the first hypothesis is the focal point of this paper and its rationale has been articulated at length already, it is worth dwelling a bit on the basis for the second hypothesis. Iyengar et al. (2011) make a compelling case for looking at moderating factors that may shape the nature and extent of social contagion at work. Whereas the focus of many studies, such as Godes and Mayzlin (2009), is on the influencer side of the equation—whether better connected adopters exert more influence than do less connected ones—we position ourselves on the susceptibility to influence side of that equation, since peer influence also depends on the susceptibility of the individual being influenced Aral and Walker (2012). A user who has 1,000 friends on Last.fm may not even notice or care that one of her peers purchased a premium subscription. At the same time, a user who only has two friends may be more selective in befriending others and may pay closer attention to them. Therefore, this user is more likely to notice and follow the actions of just one manipulated friend. Similar distinctions between selective and nonselective tie-forming behaviors in the context of trust have been observed in other online social networks such as Facebook (Bapna et al. 2014).

To address our research objectives, we first need to establish a causal link between person B's decision to subscribe and the influence from B's friend, person A. In this paper, our conceptualization of peer influence is based on Aral (2011). This conceptualization is rooted in utility theory in that the actions of one's peers changes the utility one expects to receive from engaging in a certain behavior and thus the likelihood that one will engage in that behavior. Such a conceptualization is flexible and encompassing with respect to the myriad influence mechanisms that could lead to social contagion. In other words, to demonstrate the presence of peer influence, we do not seek to explain which influence mechanism from person A *causes* person B to subscribe; it could be awareness raising, explicit or tacit persuasion, observational or social learning, imitation, or any other mechanism. The only requirement is to demonstrate that person A causes person B to subscribe. Therefore, in this study, we do not raise the question of disentangling the general peer influence into the exact types of peer influence mechanisms as above. Our design (detailed in §3) is a total effects design, which conceptually is at the other end of the spectrum of mechanism-level designs. We believe that mechanism-level disentanglement is a promising area for follow-up research. That said, in §4.5 we use our rich data to rule out several possible mechanisms.

Our work relates to and builds on the propensity score matching-based approaches of Aral et al. (2009), Susarla et al. (2012), and Oestreicher-Singer and Zalmanson (2013). A key advancement of our work is that although propensity score matching accounts for observable user characteristics in crafting usable control groups, it is widely recognized (Aral et al. 2009, Oestreicher-Singer and Zalmanson 2013) that other unobservable user characteristics (say, the amount of free-time an individual has, income level, sensitivity to commercials, etc.) or contextual effects such as marketing promotions (Van den Bulte and Stremersch 2004) could also influence the propensity to be treated and be linked to homophily.

The remaining sections are structured as follows. Section 2 describes the institutional details of our experimental context and describes our data. Section 3 describes the design of our experiment. Section 4 presents our analysis and the results of the randomized experiment. Section 5 tests the robustness of our design and analysis. Section 6 presents the conclusion drawn from our results and outlines prospective future work.

2. Institutional Details and Data

The music industry today serves as a canonical example of how a long-established, growing, and profitable industry can be disrupted and subsequently reinvented by the social machinery of the Internet. One of the important emerging models of today's content consumption on the Internet is a *freemium* social community (Anderson 2008), as exemplified by sites such as Last.fm, Pandora, Spotify, and many others. Freemium social communities typically operate based on a two-tiered business model that offers free access to the basic set of features and content while charging a fee for more advanced, premium features. For example, free users of the Last.fm[3] website can listen to the online music radio interrupted by commercials, whereas premium subscribers enjoy a continuous, commercial-free music listening experience. Premium users also get a prestigious black "subscriber" icon next to their profile photographs that is visible to everyone on Last.fm as a sign of status, can listen to the online radio on a mobile phone, as well as have access to additional colorful statistical charts about their usage patterns. Oestreicher-Singer and Zalmanson (2013) provide a nice overview of the institutional details of the Last.fm website as a freemium social community.

Freemium communities often employ numerous social computing features (Parameswaran and Whinston 2007). Of particular interest to us is the *friendship social network* feature that allows users to become *online friends* with other users. On Last.fm, for instance, online friends can affect each other's music choices while sharing their own music listening experiences, they can listen to friend's "recommended radio," can review friend's "loved songs," and so on. These interactions and information sharing mechanisms between friends can translate into certain peer influence on each other. For instance, Oestreicher-Singer and Zalmanson (2013) establish that the music listening on Last.fm is socially driven, which means it is based on what your friends are listening to, and that a paid subscription appears as a distinct (ostensibly status) symbol visible to your friends. Also, as discussed earlier, a singular challenge for freemium communities is discerning pathways and strategies for moving users *from-free-to-fee*, that is converting users from the large pool of free users to the elite set of premium paid subscribers (Oestreicher-Singer and Zalmanson 2013, Pauwels and Weiss 2008).

In this paper, we present a randomized field experiment providing the evidence that making an individual user a premium subscriber can cause her online friends to pay for a subscription and become premium subscribers as well. We chose Last.fm as a domain for conducting our experiment not only because it provides a typical example of a freemium community, a new and growing model of delivery of online services, but also because Last.fm makes for a unique experimental platform thanks to a social feature that allows gifting any random user in the Last.fm social network with a premium subscription (paid by us). Although this feature of the Last.fm website has not yet been studied extensively in the social networks literature, it offers a great opportunity to create a "gold standard" randomized trial in an online social network. From an experimental design perspective, anyone in the Last.fm social network has an equal chance of receiving a gift from us. Last.fm users cannot decline the gift or hide their subscription status from others. They cannot transfer the gift to anyone else, or postpone using it, or share it with someone else, or refund it. This makes the unrestricted gifting social feature particularly valuable for online social networks in an experimental context, a fact this research is the first to bring forth.

2.1. Snapshot Data

Our panel data set is based on publicly available information about 3.8 million users that make up the largest connected component[4] of the Last.fm network forming over 23 million friendship pairs. In addition to this information, we tracked self-reported demographic information and website-reported social activity information.

For every snapshot at time t, we have collected the following data for each user:

- $Age_{i,t}$: Self-reported age of user i. Age distribution was truncated to the interval between eight and 79 to eliminate outlier data points that are likely fake.
- $Gender_{i,t}$: Self-reported gender of user i. Dummy variable.
- $FriendCnt_{i,t}$: Total count of number of friends of user i at time t.
- $SubscriberFriendCnt_{i,t}$: Total count of number of friends of user i who are paid subscribers.
- $SongsListened_{i,t}$: Total count of all songs ever listened and reported to Last.fm by user i. If a user listened to the same song twice, the song would be counted twice as well.
- $Playlists_{i,t}$: Total count of playlists ever made by user i on Last.fm.

[3] Van Etten (2011) indicates that Last.fm, with reportedly 30 million subscribers, received 9.8 million hits per month in 2010.

[4] We employed multiple checks to ensure that we indeed collected the largest connected component of the network and not some smaller closed clique of users. Our checks ranged from looking for additional users in forums to crawling the lists of recommended music "neighbors" of each user. The total number of extra users we checked outside of our connected component amounts to the additional 0.5 million unique users. We have not discovered any other large connected component.

Table 1 Summary Statistics of Historical Data for Active Users

Subscriber	No. of obs.	Variable	Mean	Std. dev.	Missing	Median	Min	Max
0	1,214,303	*Age*	23.21	6.18	385,200	22	8	79
		Gender (male = 1)	0.66	0.48	234,278	1	0	1
		FriendCnt	24.18	70.65	0	10	1	11,780
		SubscriberFriendCnt	0.65	2.85	0	0	0	541
		SongsListened	24,913.30	32,365.72	1	15,022	0	1,000,472
		Playlists	0.53	3.32	0	0	0	2,291
		Posts	7.67	141.70	0	0	0	64,108
		Shouts	42.19	271.02	27,717	5	0	131,765
		LovedTracks	128.15	406.44	0	35	0	99,109
		RegDate	17,838.23	636.71	584	17,902	15,642	18,877
		LastfmCountry	0.30	0.46	0	0	0	1
1	37,161	*Age*	30.26	9.25	14,165	28	8	78
		Gender (male = 1)	0.76	0.43	8,449	1	0	1
		FriendCnt	33.73	116.62	0	10	1	9,788
		SubscriberFriendCnt	2.85	10.35	0	1	0	709
		SongsListened	31,996.64	43,938.95	0	18,139	0	1,000,070
		Playlists	1.44	5.38	0	1	0	496
		Posts	27.74	465.16	0	0	0	50,740
		Shouts	85.31	531.56	1,275	5	0	36,508
		LovedTracks	370.05	1,104.95	0	149	0	63,595
		RegDate	17,678.54	628.82	1	17,735	15,642	18,868
		LastfmCountry	0.28	0.45	0	0	0	1

- $Posts_{i,t}$: Total count of forum posts ever made by user i.
- $Shouts_{i,t}$: Total count of shouts (that is, wall posts) ever received by user i.
- $LovedTracks_{i,t}$: Total count of all tracks that were "loved" by user i.
- $RegDate_i$: User i original registration date on the website measured as the number of days since January 1, 1960 (standard date representation of SAS statistical package).
- $LastfmCountry_{i,t}$: Dummy variable. If user i's self-reported country is the United States, Germany or the United Kingdom, then $LastfmCountry = 1$ for this user, otherwise 0. This variable is important because Last.fm subscription rules are slightly different[5] in the official Last.fm countries (the United States, Germany, the United Kingdom) versus the rest of the world.
- $Subscriber_{i,t}$: Dummy variable indicating whether user i is a premium subscriber at time t.

The descriptive summary statistics for approximately 1.2 million active[6] Last.fm users are displayed in Table 1. This table provides a breakdown of statistics for active subscribers and active nonsubscribers for one particular snapshot of data collected around September 8, 2011, before our manipulation was done. From this data, we find that active subscribers are consistently different from active nonsubscribers in a variety of metrics: they are older, tend to have more friends (approximately, a 40% increase as compared to nonsubscribers) and disproportionally more subscriber friends (over 300% increase), more playlists, loved tracks, and registered earlier than nonsubscribers. These empirical observations confirm the observed clustering of subscription behavior indicating that the underlying forces of homophily, external correlations, or peer influence are at work in this data. Our summary data are in line with the 2009 Last.fm data reported by Oestreicher-Singer and Zalmanson (2013), suggesting a stable long-term pattern.

2.2. Dynamic Data

The collection of snapshots over time allows us to look into the dynamics of user characteristics in the social network as well as the dynamics of the social network itself. The following network dynamic variable is the variable of particular interest in this study:

- $Adopter_{i,[t,t+1]}$: Dummy variable indicating whether user i who had not been a paid subscriber before time t adopted subscription and became a paid subscriber in the interval of time $[t, t+1]$. Since the minimum possible unit of a premium subscription is one month and we collected our data with the intervals of two to three weeks, our data collection process did not miss any single subscription event for any user in the network beginning in May 2011 over the period of more than two years. Therefore, $Adopter_{i,[t,t+1]}$ variable is an objective and *guaranteed*

[5] Even though the premium subscription costs the same for every country, the subscription is more valuable for people outside the United States, Germany, and the United Kingdom. Several Last.fm services that are normally free for the United States, Germany, and the United Kingdom users require a premium subscription for the rest of the world because of music licensing contracts.

[6] An active user is a user who listened to at least one song within 30 days prior to the collection of that particular snapshot of data.

Table 2 Summary Statistics of Data for Recent Adopters over Two to Three Weeks

Adopter	No. of obs.	Variable	Mean	Std. dev.	Missing	Median	Min	Max
0	1,211,366	*Age*	23.20	6.18	384,294	22	8	79
		Gender (male = 1)	0.66	0.48	233,726	1	0	1
		FriendCnt	24.16	70.43	0	10	1	11,780
		SubscriberFriendCnt	0.65	2.80	0	0	0	465
		SongsListened	24,912.04	32,363.37	1	15,024	0	1,000,472
		Playlists	0.53	3.32	0	0	0	2,291
		Posts	7.67	141.83	0	0	0	64,108
		Shouts	42.14	271.01	27,602	5	0	131,765
		LovedTracks	127.97	406.32	0	35	0	99,109
		RegDate	17,838.13	636.65	584	17,902	15,642	18,877
		LastfmCountry	0.30	0.46	0	0	0	1
1	1,099	*Age*	26.31	7.13	346	25	11	74
		Gender (male = 1)	0.70	0.46	204	1	0	1
		FriendCnt	42.70	196.79	0	14	1	4,730
		SubscriberFriendCnt	2.76	17.58	0	1	0	541
		SongsListened	31,984.12	38,619.43	0	18,991	0	423,529
		Playlists	1.05	1.98	0	1	0	27
		Posts	13.08	96.25	0	0	0	2,266
		Shouts	93.17	381.14	43	7	0	6,247
		LovedTracks	310.65	542.01	0	133	0	6,143
		RegDate	17,712.48	651.39	0	17,734	15,642	18,877
		LastfmCountry	0.24	0.43	0	0	0	1

indicator of adoption or absence of adoption in time period $[t, t+1]$ for every user among 3.8 million users.

Similar to Table 1, Table 2 displays the summary statistics for the dynamic data of recent adopters versus recent nonadopters. Note that there is a subtle, but important difference between the types of information displayed by Tables 1 and 2:

- Table 1 compares the current subscribers versus current free active users. This is the information about the current state that the network has achieved over the years.
- Table 2 displays the information on the recent adopters. This is the information about the change in the current state: a change in the network over a two to three week period.

The difference between Table 1 and Table 2 can be explained better if we mention that many current subscribers have been premium subscribers for a very long time. Clearly, these "mature subscribers" are not considered as either recent adopters or recent nonadopters and are not counted in Table 2, but they are still subscribers and, therefore, are counted in Table 1.[7]

Despite the differences, we observe that there is a similarity between Table 1 and Table 2 suggesting a remarkable consistency in the data generation process over the years: recent adopters resemble the large mass of existing premium subscribers based on the observed characteristics. More specifically, both tables demonstrate that subscribers and recent adopters tend to have a disproportionately large count of subscriber friends: over 300% more as compared to nonsubscribers and nonadopters while the total number of friends is only 40%–70% larger.

We used this dynamic data to simulate and calibrate our experiment before actually running it. In particular, because new adoption is a rare event in our network, a key experimental challenge for us was to decide on the sample size for the manipulation so as to be able to pick up statistically any peer effect that may be there. Details of our quasi-experiment to determine the optimal sample size are provided in Online Appendix C. (Online appendices are available as supplemental material at http://dx.doi.org/10.1287/mnsc.2014.2081.)

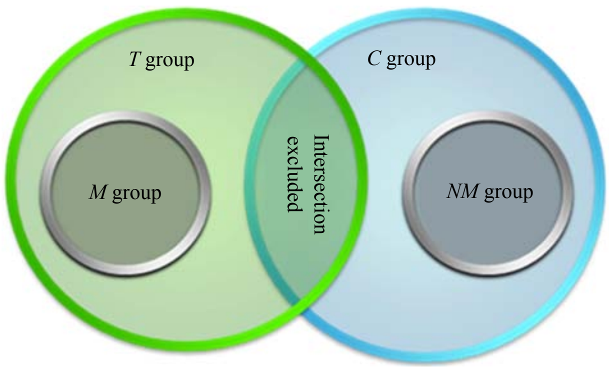
3. Experimental Design

For the experiment, we randomly selected a group of 1,000 Last.fm users, hereafter called the *manipulated group M*, who were chosen to receive premium subscription gifts. These users could not deny the gift or hide its status, ruling out any subject self-selection, impact of individual characteristics, or contextual (observed or unobserved) homophily based decisions that confound the analysis of observational data. We also randomly selected another group of 1,000 random Last.fm users, hereafter called the *nonmanipulated group NM*, who did not receive anything from us and act as a source of a symmetrical control group to establish the causal effect.

We define our *treatment group T* as all immediate friends of M who are not themselves in M or NM

[7] For this reason, the total number of users in Tables 1 and 2 is not the same.

Figure 1 **(Color online) Venn Diagram of Experimental Design**



and who are not friends of someone in *NM*. Symmetrically, we define our *control group* *C* as all immediate friends of *NM* who are not themselves in *M* or *NM* and are not friends of someone in *M*. Figure 1 presents an intuitive Venn diagram for these sets of users. Given the real-world nature of our data, a small number of users will likely turn out to be friends of both *M* and *NM* groups simultaneously. These users cannot be unequivocally put either into the treatment or control group and hence were excluded from the experiment. As a robustness check, we repeated our analysis while keeping the intersection included in both *T* and *C* instead of excluding it. Our results remain strongly significant and almost identical to the "exclusion" case. This is not very surprising, considering how small the intersection is compared to groups *T* and *C*. We analyze this issue in more detail later in this section, as well as demonstrate the rather minimal effect of our strategy of excluding the intersection in §5.3.2 on robustness.

Looking at Figure 1 it is easy to see why our randomization assigns users into treatment and control groups independently of observed and unobserved characteristics of a user. To see this consider group *G* of 2,000 initial users *before* they were randomly split into *M* and *NM*. Also consider a particular person *A* who is in *G* and his online friend *B*.

Before our gifts are assigned, person *A* has absolutely equal chances of becoming a member of *M* or *NM* and the assignment outcome is completely independent of characteristics of person *A* or person *B*. However, if person *A* is assigned to *M*, person *B* becomes a friend of *M*. Alternatively, if person *A* were assigned to *NM*, then person *B* would be a friend of *NM*. Just as person *A* has no way to predict whether he will end up in *M* or *NM*, his friend *B* has no way to predict whether *B* will end up as a friend of *M* or a friend of *NM*.

Therefore, person *B* has absolutely equal chances of becoming a friend of *M* or a friend of *NM* and these chances are independent of any characteristic of person *A* or person *B*.[8] This implies that assignment of person *B* into group *T* versus *C* is independent of observed or unobserved characteristics of person *B*.

Based on the experimental design explained above, our experimental procedure is composed of four stages. In the first stage we randomly assigned users to groups *M* and *NM*, observe their current friend network, and thus calculate groups *T* and *C*. In the second stage, we deployed a pretreatment check and observe the current status of the *M*, *NM*, *T*, and *C* groups immediately before treatment. In the third stage, we deployed our manipulation by giving 1,000 gifts to group *M* using our PayPal script.[9] Finally, we observed the current status of the *M* and *NM* groups immediately after manipulation to make sure our manipulation worked. Given Manski's (1995) concern about subjects' behavior changing when they know they are being observed, we directed users to our Last.fm page, where we took great care in explaining to the users that these were expiring leftover funds from another project that we were simply giving away (see Online Appendix A). We explicitly mentioned that we expected nothing in return and no action was needed from the user. The messaging worked, as can be gleaned from the comments gifted users left on our wall.

Although our experimental design relies on randomization, we need to emphasize that there are certain differences between our experimental design and traditional partial population treatment experiments (Moffit 2001). As mentioned earlier, our goal is to estimate the average treatment effect on the nontreated (ATEN) (Xie et al. 2012). In our case, treatment means having an extra premium adopter friend. Therefore, to be precise, ATEN is the potential effect, on a peer, of exogenously making a previously nonadopting friend an adopter (whereas ATET would be the effect, on a peer, of an adopter friend who chose to adopt).

It is worth noting at this stage that researchers working on network experiments have to be careful in dealing with possible biases that can arise because of the presence of network structure among the peers of manipulated users. For instance, in our scenario the friends of the manipulated group and nonmanipulated group are likely to have at least some intersection as discussed earlier in this section and as illustrated in Figure 1. Also, the users who end up in the intersection are not random since high-degree individuals are more likely to be friends of both groups.

[8] This result is general and holds if person *B* happened to have n different friends in group *G*: persons $A_1, A_2, \ldots, A_n$. It is easy to show that in this case, person *B* is as likely to end up in *T* as in *C* exactly the same way as it was shown in the example on Figure 1.

[9] It only takes a couple of hours to distribute all 1,000 gifts using our script.

The failure to appropriately account for this intersection problem could introduce various kinds of biases threatening either internal or external validity of the experiment. How one deals with this issue depends on the particular methodology and design choices deployed by the researchers.

In this paper, we utilize a nonparametric resampling-based approach that is detailed in §4, and our inference procedure relies on symmetry between the treatment and control groups to achieve internal validity. This requires that in our design, groups M and NM be of equal sizes, and that we deal with the intersection in a symmetric way: either excluding it from both the treatment and control groups or including it in both.

Similar to alternative, say model-based approaches, our approach balances certain trade-offs. For instance, symmetric design allows us to achieve internal validity of our experimental results by construction. Symmetry ensures that everything observable and unobservable that happens to the treatment group, over and above the manipulation, is also mirrored in the control group. It does so without introducing any strong assumptions about the functional form, the shape of degree distribution, and clustering in the network. However, the price that we pay for this is that we systematically exclude the high-degree users in the intersection from our analysis (due to symmetry requirements) and thus we cannot generalize our results to these high-degree users.

In §5.3.2 we provide empirical evidence that, in our particular context, excluding the intersection is not a significant threat to external validity since the estimates do not change significantly depending on whether we include or exclude the intersection from treatment and control groups, and because the size of the intersection itself is small, less than 5% of our sample.

We would also like to be very explicit and state that there are other valid ways of conducting network experiments that may introduce different assumptions and not require the symmetry and/or removing the intersection. For instance, researchers could introduce a particular functional form that explicitly models the shape of how the treatment effect changes with user's degree (Aral and Walker 2014). This would allow to gain the better external validity at the expense of the introduced assumptions of the relevant statistical model. This may be preferable, for example, in cases when the intersection is large and thus, excluding it would constitute a significant generalizability problem.

In addition to that, if a network in question is large enough, stratification by degree analysis (Aral and Walker 2011) would allow to define the control group C without first defining any reference group NM. This approach proceeds to perform the analysis stratified by the degree of the treated and control users provided that users of a given degree are sufficiently represented in both treatment and control groups. Unfortunately, this appears to be a problematic assumption in our setting given that the treatment ultimately impacts a nonnegligible subset of our finite network. In our scenario, very high-degree strata are systematically missing control users and thus, stratification by degree cannot be performed in its purest form for these strata. This would once again require either excluding these strata or introducing extra assumptions.

The availability of these techniques offers numerous ways for designing network experiments and for tailoring the trade-offs for the particular needs of the researchers suitable to their given context. For our analysis, we decided to choose a symmetric design for reasons outlined above as well as for its robustness to assumption violations and simplicity.

3.1. Strengths of the Experiment in Mitigating Threats to Validity

Our design has several intuitive benefits that help us overcome the myriad of challenges (Van den Bulte and Stremersch 2004, Aral 2011) in making causal detection of social contagion from observational data, separating out peer influence from homophily and other external sources of confounds. As mentioned above, one of the ways in which homophily manifests itself in observational data is through self-selection bias, when manipulations are not randomly assigned, which is not the case in our study. Also, since it is not possible to withdraw or refund a gift, we have no subject attrition or mortality bias. This is because users are selected randomly and they cannot escape, decline, or remove themselves from the manipulation. It is also important to mention that each person's network was collected immediately before the manipulation, immediately after the manipulation and with different levels of delay after the manipulation. Importantly, the treatment group T and control group C were determined using the friend network immediately before the manipulation. Clearly, if a person started self-selecting subscriber friends after the manipulation had occurred, it would not have any effect on our experiment. Further, because the subscriptions themselves are not transferable and not refundable, we can rule out any direct treatment diffusion effect, suggesting that any effect that is observed must be through some kind of peer influence other than simple direct transfer of our gift. It is possible, given the real-life social network setting, that our manipulation may "leak" from manipulated group M into control group C through second-degree

Table 3 Groups T and C Have Similar Observed Statistical Properties

Variable	Friend of	Mean	Std. err.	Min	Median	Max	t-value	Pr > |t|
Age	*NM*	22.72	0.261	8	21	79	−1.18	0.2387
	M	22.11	0.260	8	21	77		
Gender (male = 1)	*NM*	0.62	0.013	0	1	1	−1.16	0.2475
	M	0.59	0.013	0	1	1		
FriendCnt	*NM*	100.23	8.829	1	40	7,800	0.52	0.6022
	M	109.35	8.754	1	41	4,700		
SubscriberFriendCnt	*NM*	2.76	0.146	0	1	337	0.24	0.8129
	M	2.83	0.144	0	1	352		
LovedTracks	*NM*	238.76	9.208	0	61	32,387	−1.24	0.2152
	M	216.04	9.252	0	59	23,466		
Playlists	*NM*	0.67	0.037	0	0	204	−0.02	0.9853
	M	0.67	0.035	0	0	465		
RegDate	*NM*	17,747.04	18.607	15,742	17,803	18,844	0.09	0.9257
	M	17,750.51	18.688	15,746	17,807	18,762		
Shouts	*NM*	141.58	14.683	0	27	17,699	0.67	0.5008
	M	161.32	14.730	0	27	25,343		
SongsListened	*NM*	31,970.58	696.408	0	19,480	928,318	0.20	0.8440
	M	32,243.68	695.810	0	19,873	1,000,000		

friendship connections (i.e., there may be a possibility of an indirect treatment diffusion effect). However, this would likely lead to an underestimation of the observed difference, not overestimation. Since the second-degree effect is probably slower and weaker than the first-degree effect caused by the immediate friend, it can be mitigated by postexperimental observations on the shortest distances between the actual adopters in control group C and treatment group T. As we explore in §5, the evidence suggests that this treatment leak is not a concern since it turns out that the observed new adopters in the treatment group are almost universally not connected to the observed new adopters in the control group. Finally, we can rule out any compensatory rivalry, resentful demoralization, or experimenter bias, since neither treatment group T nor control group C know that they are being treated and watched.

It is important to highlight that only manipulated group M received a gift from us and there were no other "disturbances" introduced into the network. Group M was told that the gift was given out of the expiring leftover funds from a prior survey and that a gift receiver was not required to do anything, thus group M itself was not aware of being manipulated. Moreover, group M, the "disturbed" group, was itself not being tracked for the purposes of our experiment, and we were interested in their friends.

Based on the discussion above, we conducted our randomized field experiment by sampling symmetrical groups M and NM containing 1,000 users each and computing their friends T and C, respectively, as summarized in Table 3. Each person in group M subsequently received a one month subscription gift from us, with the 1,000 gifts being distributed over the period of several hours by a PayPal script. The users from group NM did not receive any gift or any other communication from us.[10]

A manipulation check was done immediately after distributing the gifts using a customized Web crawler. This check demonstrated that all 1,000 users in group M received the gift and became premium subscribers immediately. In one month after the manipulation was done, we collected a new snapshot of the social network and compared adoption behavior among all friends of group M versus all friends of group NM as described in the experiment design.

4. Results and Analysis

4.1. Testing for Causal Peer Influence

Given exogenous and independent randomization of our manipulation, the assignment of user i into group T or C is independent of her observed or unobserved characteristics as explained in §3 and confirmed in Table 3. As a result of the experiment, the treatment group has 66 new adopters, whereas the

[10] Although clinical trials frequently use a placebo pill for the control group instead of giving nothing at all, in our study it is not necessary. Clinical trials deal with special circumstances of mind-body connection: it is well known (Ariely 2010) that a placebo pill itself can demonstrate significant improvements in patient health as compared to no treatment at all. Therefore, clinical trials have to demonstrate not that the pill works in general, but that the pill works stronger than the placebo. Therefore, clinical trials typically are a comparison of two alternative treatments both of which work to some degree. In our case, we do not intend to show that our manipulation works stronger than some other alternative manipulation. Instead, we plan to demonstrate that our manipulation works stronger than having no manipulation and simply "going with the flow."

control group has 41 new adopters, indicating a difference of 61% between the groups. However, one has to be cautious in assessing the statistical significance of this difference. Given the network nature of the data, there is a dependency structure within the alter-adoption models that has to be accounted for. Each seed user in M (or NM) has one or more friends in group T (or C, respectively), and thus adoption outcomes for this population of alters (friends of seed users) may be correlated. Therefore, we begin our analysis by presenting a nonparametric resampling test that accounts for the correlation among the adoption outcomes of alters by establishing the histogram of a typical number of adopters to which a random 1,000 users are connected. Later in this section, we discuss a random-effects approach that is also appropriate in this scenario in addition to the resampling test. We utilize the random-effects approach to run a logistic regression as well as a robust t-test method.

4.2. Nonparametric Resampling Test

Assume our manipulation did not cause any effect, so any difference we see is just a chance. Recall, group NM is just an arbitrary random sample of 1,000 users that was sampled from the population of 1.2 million eligible ones. Therefore, we may easily generate many other random samples $NM_1, \ldots, NM_k$, each with 1,000 users, from 1.2 million eligible users and compare the friends of each NM_i against the friends of our true group M. This resampling procedure is detailed in Algorithm 1 in Online Appendix B.[11]

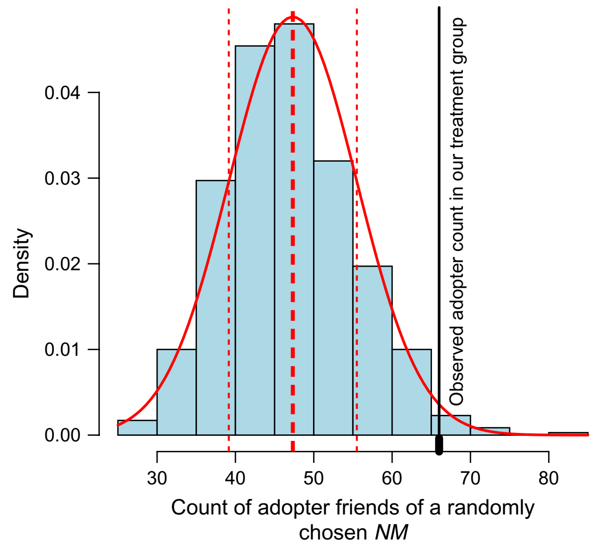
If our manipulation causes no difference in adoptions, we would see that it is rather common to see 66 adopter friends by sampling a random group NM_i of 1,000 users. However, as we see in Figure 2, which is plotted based on Algorithm 2 in Online Appendix B, the friends of M demonstrate a very high level of adoptions that is rarely seen among friends of a random sample of 1,000 users.

It is important to note the following:

- This technique statistically utilizes the entire 1.2 million sample of eligible users to compare against 1,000 manipulated users, while at the same time keeping intact the perfect intrinsic symmetry of our experiment.

[11] It should be pointed out that resampling here is different from bootstrapping. The latter uses sampling with replacement as a key feature to approximate the true distribution; see https://en.wikipedia.org/wiki/Bootstrapping_(statistics) (accessed March 19, 2015). Unlike traditional bootstrapping scenarios where population level inferences are desired from samples of the population, we are fortunate to have access to the full Last.fm network population. Therefore, we do not need to rely on approximation techniques such as bootstrapping, jack-knife etc. to approximate the true distribution, given that we already have the true distribution.

Figure 2 **(Color online) Resampling Test Reveals How Unlikely It Is to See 66 Adopters Just by Chance**



- This technique allows us to include users from the intersection back into sample so that no user is permanently excluded. For example, if a particular friend of M happened to be in the intersection at iteration $i = 1$ and thus was excluded, she will not necessarily be excluded at iteration $i = 2$ once different NM_i is sampled.

After running 700 iterations of this simulation, we plot the distribution of typical counts of adopter friends of each NM_i and contrast it with the observed count of adopter friends of group M that received a gift from us. As is evident from Figure 2, the number of adopter friends of group M really stands out in a histogram of what is typically observed among friends of the random 1,000 users. Only 1.7% of random NMs had 66 adopter friends or more.

We would like to highlight that although it is true that our resampling procedure allows for the inclusion of users in the intersection that previously would have been excluded from analysis, the approach is not intended to solve the generalizability concerns regarding high-degree users: the chance that a user belongs to many "friends of NM" subsets, each from a new sampled set NM, increases with the degree of the user and hence high-degree users will be excluded via intersection much more often than their lower degree counterparts. Although this remains a methodological limitation of our approach, the empirical evidence presented in §5 suggests that excluding the intersection is largely benign for the context of our study.

4.3. Robust t-Test with Ego-Level Random Effects

Consider users j and k from group T who are friends of user i from group M. As is described in the literature (Kuiper and Sklar 2012), a regular two-sample

t-test is equivalent to a t-test of β coefficient of an ordinary linear regression model:

$$\begin{aligned} Adopter_j &= \alpha + \beta \cdot Treatment_j + \eta_j, \\ Adopter_k &= \alpha + \beta \cdot Treatment_k + \eta_k. \end{aligned} \tag{1}$$

A regular t-test approach would assume that η_j and η_k are uncorrelated. However, in our scenario, we know that the adoption outcomes of friends of the same user i can be correlated, so that if user j and user k are both friends of user i then η_j and η_k could contain a common ego-specific component γ_i and thus be correlated:

$$\begin{aligned} Adopter_j &= \alpha + \beta \cdot Treatment_j + \overbrace{\gamma_i + \varepsilon_j}^{\eta_j}, \\ Adopter_k &= \alpha + \beta \cdot Treatment_k + \underbrace{\gamma_i + \varepsilon_k}_{\eta_k}. \end{aligned}$$

However, we should note that since the treatment status of users j and k is automatically derived from manipulation assignment of user i, we have $Treatment_j = Treatment_k = Manipulation_i$, and therefore, we can rewrite this model as

$$\begin{aligned} Adopter_j &= \alpha + \beta \cdot Manipulation_i + \gamma_i + \varepsilon_j, \\ Adopter_k &= \alpha + \beta \cdot Manipulation_i + \gamma_i + \varepsilon_k. \end{aligned} \tag{2}$$

As per the design of our experiment, the randomization of groups M and NM is purely exogenous with the eligible seed users i being independently assigned into groups M and NM in a perfectly random fashion. Therefore, ego-level heterogeneity γ_i is, by design, completely independent of the assigned ego-level manipulation status $Manipulation_i$ suggesting that Equation (2) represents a regular random-effects model[12] rather than a fixed-effects model.

Considering that a regular two-sample t-test is equivalent to a t-test of β coefficient in (1), our model in Equation (2) is essentially nothing more than a two-sample t-test that is robust to correlation among alters of the same ego. Therefore, we refer to this method as a *robust t-test*. We utilize this random-effects approach later in Tables 8 and 9 (see §5.3.2), and we see that it essentially produces the same results as the resampling method, the reshuffling test, and other robustness tests. We also utilize the same idea in the next subsection where we explore heterogeneous treatments effects by number of friends.

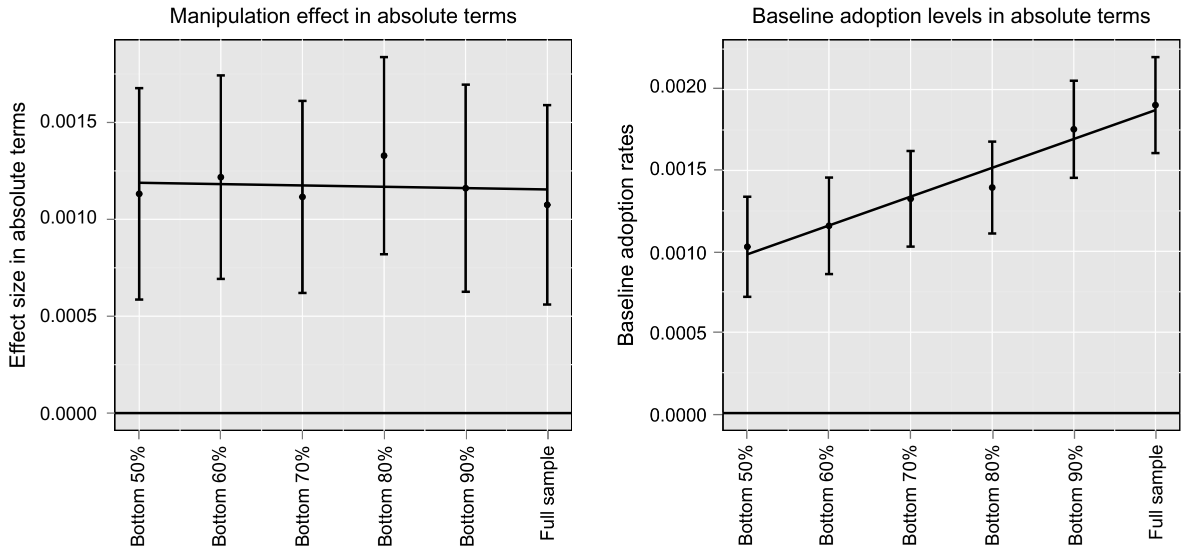
[12] Although $Adopter_j$ is a binary variable that only takes values 0 and 1, it is demonstrated in the literature that a regular ordinary least squares (OLS) estimator $\hat{\beta}$ is an unbiased and consistent estimator of the average treatment effect even in the scenario of a binary dependent variable (Aldrich and Nelson 1984, Angrist and Pischke 2008). This model is known in the literature as the *linear probability model* (Long 1997). Even though applying a regular non-heteroscedasticity-consistent OLS estimator may produce biased standard errors, heteroscedasticity-robust standard errors are a very straightforward way to account for that (Aldrich and Nelson 1984).

4.4. Heterogeneity by the Number of Friends

In addition to testing for causal peer effects, we are also interested in examining whether the number of friends that a user has is associated with more or less susceptibility to influence by their peers, as articulated in research Hypothesis 2. We begin by presenting a nonparametric test (rather than relying on any specific model or functional form to begin with) that showcases heterogeneity in the data as a result of our treatment. We do this based on Algorithm 2 in Online Appendix B. According to this procedure, we first only look at the bottom 50% of users in groups T as measured by the number of friends and compare them to the bottom 50% of users in group C. In doing so, we determine the effect size as applied to the bottom 50% of users by the number of friends. In other words, we measure the effect size for users who have few friends. At the next step, we increase the subsample and look at the bottom 60% of users in group T and compare them to the bottom 60% of users in group C. We repeat this procedure by comparing bottom 70% of users in group T to bottom 70% of users in group C and so on; introducing more and more high-degree people with every iteration of that algorithm, until we compare the full sample of users in group T versus the full sample of users in group C.

As is evident from Figure 3, there is no change observed in the absolute effect size ($p > 0.81$) as more and more of high-degree users are included into the sample. Yet, the right panel reveals that the baseline natural adoption levels are very different for users in different cohorts ($p < 0.03$). Users with few friends have as low as one half the natural rate of adoption of the full sample. Therefore, whereas the effect size is flat in terms of absolute increase in adoption rates, we hypothesize that the effect size measured as percent increase would naturally be significantly stronger for users with low *FriendCnt* as these users tend to have a lower baseline level of adoptions and, therefore, the same absolute increase would constitute a higher percentage change. To estimate the percent effect rather than the change in the absolute value, we deploy a random-effects logistic regression model that accounts for the fact that adoption outcomes of peers of the same seeded user i may be correlated. We report the details of the logistic regression model and its diagnostics in Online Appendix F. As is evident from the model, the *Treatment* variable is statistically significant even after controlling for observed individual user characteristics. Moreover, since *Treatment* is assigned independently of characteristics of user i, this coefficient has causal interpretation: *Treatment* causes the adoption of subscription, thus providing additional evidence for the presence of peer influence. It is important to note that

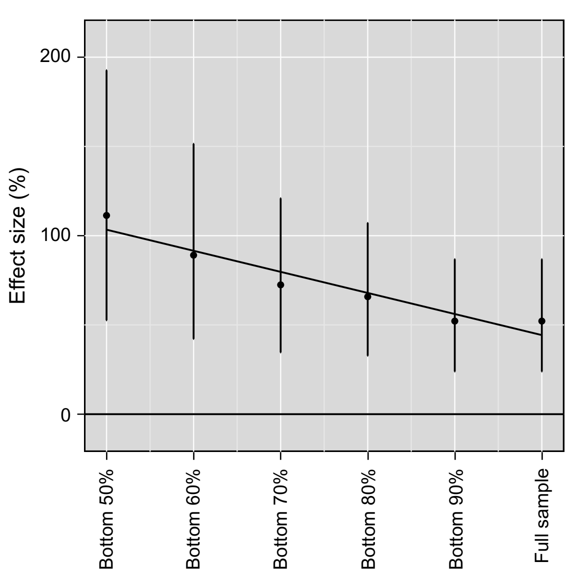
Figure 3 **Baseline Levels and Manipulation Effect in Absolute Terms by** ***FriendCnt***



the estimated coefficient of log(*SubscriberFriendCnt*) is also statistically significant and positively associated with the likelihood of adoption of subscription: the effect that is likely to be observed if peer influence is at work.

We once again demonstrate heterogeneity using Algorithm 2 (this approach splits the original sample into bins by degree and runs the main effects models multiple times) to make the absolute increase from the previous section and the relative increase reported by the logistic model comparable. As we can observe in Figure 4, we obtain a decreasing slope ($p < 0.03$) implying that the larger the *FriendCnt* is, the weaker the percent response to *Treatment*, other things being equal, thus supporting our Hypothesis 2. This reveals the expected decreasing pattern in the relative effect size as the number of friends increases. However, we should note that, as we demonstrated earlier in Figure 3, the heterogeneity of the treatment effect comes from the same absolute increase constituting a different effect size based on differences in the baseline adoption levels for different cohorts of users. For sake of completeness, it should be mentioned that one potential limitation with the above heterogeneity analysis is that it may not, for aforementioned reasons of dealing with the intersection via exclusion, fairly depict the impact for high-degree users. However, given that the evidence in Figure 3 displays a relatively flat trend with degree, this is not a significant concern.

Figure 4 **Heterogeneity Results from Logistic Regression: Effect Size by** ***FriendCnt***



4.5. Potential Mechanisms of Peer Influence

Having established the average treatment effect on nontreated in the general population as well as heterogeneity of the effect by the number of friends in the prior sections, it is a natural question to consider the exact mechanisms at play that can explain the peer influence we observe. As suggested by prior literature (Aral 2011), peer influence may be driven by a combination of different kinds of possible mechanisms such as awareness raising, explicit or tacit persuasion, observational or social learning, or imitation among others. Complicating the issue further, it is possible that some mechanisms act in the opposite direction of other mechanisms. Therefore, the observed social contagion can potentially be the result of a "tug of war" between multiple mechanisms acting in all possible directions.

Table 4 **After-Treatment Website Use Characteristics**

Group	Variable	Mean	Std. err.	Min	Max	t-value	p-value
NM	*DeltaFriendCnt*	1.268	0.5215	−25	441	0.83	0.4073
M	*DeltaFriendCnt*	0.828	0.0996	−9	55		
NM	*DeltaLovedTracks*	4.983	0.8189	0	569	0.78	0.4357
M	*DeltaLovedTracks*	4.245	0.4739	0	249		
NM	*DeltaShouts*	1.017	0.1990	0	115	−0.02	0.9836
M	*DeltaShouts*	1.022	0.1586	0	91		
NM	*DeltaSongsListened*	876.999	43.0050	0	14,397	0.14	0.8899
M	*DeltaSongsListened*	869.078	37.7299	0	13,258		

The virtue of our experimental design is its ability to estimate the ultimate net effect as aggregated across all possible causal paths and mechanisms. For example, if some social contagion goes through offline channels (say, with gifted Last.fm users meeting offline on a music concert and convincing their friends to buy subscription), our experimental estimates would account for this effect as well. Unfortunately, the very virtue of our manipulation that allows us to identify the full net effect across numerous possible channels of peer influence, makes it harder to single out rigorously a particular mechanism of social contagion from the rest of the mechanisms (by means of that same manipulation).[13] Although we cannot naturally confirm the exact mechanism at play in our setting, the design constraint notwithstanding, we can use the richness of our data to rule out certain potential mechanisms. We leave it for future research to isolate the mechanisms at work in a positivist manner.

4.5.1. Increased Usage and Related Exposure. We begin by examining whether gifted users start using the website and its social features more often and more intensely, thus causing their peers to engage with the website more and ultimately leading to the peer's premium subscription. As it turns out, a significant set of peer influence mechanisms that deal with the increased use of the Last.fm website by the originally seeded users can be ruled out with our data.

To rule out the effect of increased usage, we measure the social activity of the originally manipulated users in group M on the Last.fm website by observing a set of website-reported variables. We define the variable *DeltaFriendCnt* in Table 4 as a change in the number of friends that a user made over the one and a half month period[14] after the gifts were given to group M. Other variables in Table 4 are defined similarly.

As demonstrated in Table 4, users in group M did not significantly increase their use of the website during the time of our manipulation as measured by all the social engagement variables.

4.5.2. General Awareness. Another potential peer influence mechanism that we can rule out in our context and with our data is general awareness spreading. The argument in favor of awareness as the mechanism would be constructed as follows: assume that we study peer influence by giving the gifts of some unknown and very obscure product to group M while giving nothing to group NM. Since the product is so unknown and obscure to the general population, friends of NM have almost no chance of adopting it spontaneously. Therefore, friends of M are the only ones who can possibly adopt the product since they are the only ones who can possibly be aware of it.

In our context we can render this mechanism unlikely for the following reasons:

- The premium subscription is widely and openly advertised on the Last.fm website to the general population.
- The actual observed adopters in group T are mature users of the website as demonstrated by the observed values for website tenure (in days) and the *SongsListened* variable in Table 5. It is evident that all of the observed adopters in groups T and C had at minimum four months experience on the website before manipulation (with an average tenure of more than three years) and had listened at minimum to several thousands songs before buying into premium subscription.[15] Also, as is evident from Table 5, new

[13] A mechanism-level experiment that manipulates just one particular channel of peer influence leaving other channels intact would be more useful in this scenario. Such a mechanism-level experiment, however, would have a symmetrical problem attempting to estimate the total net effect of peer influence aggregated across multiple different mechanisms since only one channel is manipulated by such an experiment.

[14] The premium subscription gift was only active for one month after which it expired and the user returned to the "free" state.

[15] One adopter in group C had $SongsListened = 0$ only because he erased all his songs immediately before manipulation. However, he clearly is a mature user since he registered more than three years before our experiment and had 55 friends at the time of the experiment. All adopters in group T had listened to at least 2,000 songs on Last.fm before buying the premium subscription.

Table 5 **Characteristics of Observed Adopters in Groups *T* and *C***

Group	Variable	Mean	Std. dev.	Min	Max	t-value	p-value
C	*Tenure*	1,183.68	634.352	173	3,015	1.7665	0.0807
T	*Tenure*	1,392.45	526.079	145	2,957		
C	*SongsListened*	38,947.49	33,671.05	0	154,748	1.3940	0.1667
T	*SongsListened*	50,638.82	53,083.01	2,128	342,662		

adopters in group T actually tend to be marginally significantly more experienced than new adopters in group C in terms of the tenure on the website. Based on this observation, we deem it unlikely that these mature users are generally unaware of the existence and basic features of the premium subscription, which is the only paid feature on the Last.fm website.

4.5.3. Network Effects. If the hypothesized mechanism of peer influence is a simple network effect—such as, the more friends of the focal user use the product the more valuable the product becomes for the focal user—then we can test this by examining whether dropout rates behave differently from adoption rates in groups T and C. We find that unlike the number of new adopters, the number of subscription dropouts is statistically indistinguishable in groups T and C as demonstrated in Table 6. This observation suggests that peer influence does not significantly affect the decision to drop the subscription among existing premium users, but instead increases the number of new adopters.

We conjecture that the reason for this effect is that since an existing subscriber had personal firsthand experience with the feature, the decision to unsubscribe is more of an independent personal decision. This is in contrast to new adoption when an individual, by definition, has no prior experience with the product and thus has to rely on some external information that (as we show in this paper) includes peer influence. From the utility theory perspective, this result demonstrates that whereas the utility of a potential new adopter changes significantly if one extra friend adopts, the utility of an existing subscriber who is about to drop the premium subscription does not change significantly if one extra friend adopts.

Thus, network effects as a mechanism are not supported by the evidence, given that, in the presence of peer influence due to network effects, an extra adoption would be expected to reduce the dropout rates as well. In the next section we describe a variety of robustness tests for our main results.

Table 6 **Dropout Rates in Groups *T* and *C* Are Indistinguishable**

Group	Dropout	t-value	p-value
C	94	0.47	0.6407
T	100		

5. Robustness Tests

5.1. Reshuffling Test

Assume our manipulation did not cause any effect, so any difference we see is just by chance. This means we can randomly reshuffle users between groups M and NM, repeat our complete analysis with those reshuffled M_i and NM_i, and still see the same magnitude of difference in adoptions fairly often. As we will see, it is actually quite rare to see this magnitude of difference if we were to reshuffle M and NM randomly. For our test, we used Algorithm 3 in Online Appendix B to construct reshuffled M_i and NM_i at iteration i and explore the typical differences between the reshuffled groups. After running 800 iterations of this simulation, we find that only in 3.2% of random reshufflings does group T_i beat group C_i with the difference as large as we see in our data. If we are to believe that the difference we observe is just a chance occurring, it means we need to believe we encountered a 3.2% probability event.

5.2. Survival Model Analysis

In addition to logistic regression model presented in §4, we tried a number of different models including survival models. Survival models offer an alternative

Table 7 **Results of a Survival Model**

Variable	Coef.	Exp. (coef.)	Std. err.	z-value	Pr > z
Treatment	0.5187	1.6799	0.2471	2.099	0.0358
Gender	−0.4670	0.6269	0.2277	−2.051	0.0402
Age	0.0262	1.0265	0.0154	1.699	0.0893
RegDate	−0.0004	0.9996	0.0002	−1.574	0.1156
log(*SubscriberFriendCnt*)	0.5946	1.8123	0.1914	3.107	0.0019
log(*FriendCnt*)	−0.3960	0.6730	0.1696	−2.335	0.0196
LastfmCountry	−0.7390	0.4776	0.2954	−2.502	0.0124
log(*SongsListened*)	0.2806	1.3239	0.1101	2.548	0.0109
log(*Posts*)	−0.0027	0.9973	0.0660	−0.04	0.9679
log(*Playlists*)	0.4646	1.5913	0.1770	2.624	0.0087
log(*Shouts*)	0.0127	1.0127	0.0806	0.157	0.87511
log(*LovedTracks*)	0.2259	1.2535	0.0720	3.138	0.0017

approach that can take advantage of the longitudinal nature of our data. As is displayed in Table 7, the results of running a Cox model with clustered standard errors[16] are very close to the results obtained by logistic regression in Table 3 of Online Appendix F, as expected in our scenario (Annesi et al. 1989).

5.3. Generalizability Issues with Experimentation in Social Networks

Our study makes causal claims due to purely exogenous randomization of the initially seeded users. However, being a network experiment, there are certain potential limitations to the generalizability of our claims due to the complicated network structure of social networks. We discuss these challenges below and provide certain results that demonstrate that these limitations are not likely to be a serious obstacle to the generalization of our results.

5.3.1. Leakage of Treatment into Control Group. Since treatment and control groups are not isolated from each other, but rather a part of the same social network, it is possible for the treatment to "leak" from the treatment group into the control group. To mitigate this potential concern, we can provide both empirical and theoretical evidence suggesting that this is likely not a significant obstacle. In particular, in our study, we find the following:

- Empirically, only one adopter in group C is a friend of an adopter in group T who could have influenced him. So adopters in group C are generally not connected to adopters in group T (with one exception).
- Theoretically, even if the treatment leak does occur from the treatment into the control group, the leak would make groups T and C more similar, making it harder to demonstrate the significant differences between T and C rather than easier.

We do acknowledge, however, that removing all potential SUTVA (stable unit treatment value assumption) violations is a difficult and not yet solved problem in network experiments.

5.3.2. Intersection of Treatment and Control Groups. Per our experimental design, the users who end up in the intersection are excluded from the analysis. These users, however, are not random, but rather extremely high-degree users. Therefore, our results may not generalize to this small set of extremely high-degree users. As it stands, this limitation is not a threat to internal validity of the experiment because group T and group C are statistical counterfactuals of each other due to symmetrical experimental design.

[16] The clustered standard errors approach is appropriate as discussed in §4.2 above because of exogenous randomization of groups M and NM.

Table 8 T vs. C: Robust Estimation with Ego Level Random Effects

Group	Adopter	t-value	p-value
C	0.001903966	2.269976	0.0233
T	0.002972973		

Table 9 $T+$ vs. $C+$: Robust Estimation with Ego Level Random Effects

Group	Adopter	t-value	p-value
$C+$	0.0019609515	2.168841	0.0301
$T+$	0.0029431272		

This limitation only applies to external validity since, because of the exclusion, our results may not generalize to the extremely high-degree people found in the intersection. Tables 8 and 9 compare the results obtained by excluding the intersection and including the intersection. As is evident, the results are very similar in our data. Tables 8 and 9 take into account the possible correlation among adoption decision of peers of the same user in M or NM using ego-level random effects.

In this study, the following circumstances mitigate this intersection bias:

- The intersection constitutes less than 5% of our sample.
- The intersection constitutes very high-degree users that are very rare in the general population and, thus, the loss of generalizability for these users is not critical. Specifically, the median friend count among the users found in the intersection is 440. Contrast this with the general population of the Last.fm network: 99.8% of Last.fm users have less than 440 friends. Therefore, drawing a median user from the intersection is an extremely rare occurrence in the general population.

To the best of our knowledge, the existing literature has not yet solved the problem of a potential bias introduced by either excluding or right censoring the users who end up in the intersection of multiple treatments because these excluded users may be different in a systematic way. We believe this to be a promising area for future research.

6. Conclusions and Future Research

In this paper, we present a novel randomized experiment that allows us to make a causal inference about the presence of economic social contagion and peer effects in the general population of an online social network without any subject recruitment procedures. Specifically, we conduct the experiment in the context of purchasing premium subscriptions of a freemium social network. We deploy a unique website feature that allows us to buy a premium subscription gift for any user in the network, thus creating a perfect "seeding tool." This unique feature induces the proverbial "helicopter drop," an exogenous random assignment of a treatment to a subset of the population, which can be compared against a statistically identical control group. We believe that this research is at the frontier of what information systems can do—an "economic experiment in the wild" with real subjects but without a subject recruitment procedure based on self-selection.

Using a variety of nonparametric, semiparametric, and parametric approaches, ranging from resampling-based inference to ego-level random effects to logistic regression to survival models, we get close to identical, consistent, statistically and economically significant estimates of peer influence in the general population of a freemium social network. In addition, we discover that the relative strength of peer influence decreases with the size of the friendship circle of the influenced user. We also note that the gifted users were selected as a random sample from the general population and not from the subpopulation of very influential people. Thus, we establish that even *average* social network users exert significant peer influence on their friends. Future research aimed at maximizing social contagion will explore how much stronger the influence could have been if a sample of highly influential people (however defined), rather than average users, are manipulated. In addition to that, it is important to point out that we only look at the effect on immediate friends of the gifted users in this paper. Peer influence is subject to *social multiplier effect* such that once influenced, the immediate friends of the gifted users may themselves start influencing their own friends, possibly increasing economic significance of the original first-degree effect dramatically.

Our study advances the vast literature on peer influence in social networks in a variety of important directions. This study reinforces and backs up the evidence from prior studies of self-selected experimental subjects and free products, supporting the idea that online peer influence constitutes a fundamental phenomenon that extends beyond a special set of self-selected experimental participants and their friends.

Our specific contributions include the following:

1. *Novel identification strategy* that establishes peer influence for the general population.

• We avoid any voluntary recruitment procedures by utilizing a unique "gifting" feature and selecting subjects completely at random from the full general population. Therefore, we eliminate the notorious subject self-selection bias where individuals who tend to respond to subject recruitment ads may be systematically different[17] from the target population (Camerer and Lovallo 1999, Harrison et al. 2009).

• Our manipulation is nonintrusive; that is, subjects are completely unaware of being a part of the experiment at all, avoiding any observer bias.

• In our study, subjects cannot withdraw from the experiment and cannot escape our manipulation, avoiding any possible subject mortality bias.

2. *Average treatment effect on the nontreated* is experimentally estimated in our study as opposed to the average treatment effect on the treated from the prior literature. Specifically, our paper computes how much influence a typical nonadopter could achieve if she is promoted, whereas prior experimental literature has experimentally estimated the ATET (Aral and Walker 2012, Bakshy et al. 2012a). In other words, the existing literature has shown how much influence the existing adopters had on their peers, but has not talked about the possible influence of nonadopters (if they were promoted).

The difference of peer influence of existing adopters versus nonadopters is important based on the evidence from Table 2.

• Adopters and nonadopters have very different observed characteristics. Therefore, the results obtained for the peer influence of existing adopters may not generalize to the possible influence that can potentially be exerted by nonadopters.

• Nonadopters vastly outnumber adopters with at least a 30-to-1 ratio. Therefore, in a sense, an average existing adopter is a very special network user, whereas an average nonadopter is a good representation of an average network user.

Therefore, if one were to estimate the potential peer influence that can be achieved in the social network by promoting nonadopters into adopters, the key idea of viral marketing, estimating ATEN is at least as important as estimating ATET.

3. *Outcome is a real purchase decision with real money*. In our study, peer influence is established for economic transactions involving real money, because each observed outcome is a monetary transaction for $3 and subjects must actually pay their own money to buy the subscription. It is well established that customers approach free products in a very different way than even the cheapest $0.01 products, basically acting under two different regimes: social norms versus market norms (Ariely 2010, Shampanier et al. 2007).

In addition to the causal analysis of our data, our experimental data allows us to compare the strength of peer influence and homophily in a predictive sense.

[17] Although the observed characteristics of these individuals can be accounted for by poststratification, it is hard to account for unobserved characteristics that may be systematically biased and thus confound the study.

In Online Appendix D, we discuss how comparing the strength of peer influence and homophily in a social network would naturally request a predictive modeling framework rather than a causal framework due to the difficulty of defining what "manipulating homophily" means. By adopting a predictive modeling framework, we study the change in predictive adoption scores that are assigned by a predictive model to all users as new information is observed. In other words, if focal user A has a network friend B and friend B suddenly adopts a premium subscription by herself, the predictive model will revise A's adoption score (i.e., the predictive model's belief about probability of adoption) upward since B's adoption is a predictive signal. Similarly, if focal user A has a network friend C and friend C was gifted by us, the predictive model will also revise A's adoption score upward since the model learns that C may have peer influence on A as established in this paper. Interestingly, it turns out that the predictive model learns to update the score in both of these cases by a similar amount. In other words, the predictive model does not see friend B's signal as much more predictive than friend C's signal, suggesting that peer influence is the dominant predictive force in the network, at least for short-term adoption decisions. We also separately compare the results of observational quasi-experiment (Online Appendix E) with the randomized experiment on the same data and conclude that quasi-experiments tend to overestimate the strength of peer influence for users with a large number of friends, while underestimating it for users with a small number of friends.

Whereas a predictive framework provides a natural way of quantifying the strength of homophily versus peer influence in a social network, this study suggests looking at peer influence and homophily as forces of nature acting over different time horizons and suggests that a separate study is needed to identify the longitudinal effects of both of these forces.

Our work makes significant contributions to our nascent understanding of the monetization of freemium social communities. Online social networks span way beyond the specific context of Last.fm. They are digitizing long-standing social processes of our everyday lives, and this digitization allows us to access not just demographic characteristics of users, i.e., who they are, but also their network location, i.e., where (in the network space) they lie, and their behaviors, i.e., what they do. Leveraging these three different types of data require (a) determining the mix of their causal impact in changing desirable behaviors, and (b) designing appropriate policy and marketing interventions to inducing such behaviors. Using an experimental design, such as ours, managers can determine whether the network variables such as actions of peers are causal in impacting desirable outcomes.

Our work is not designed to unearth the exact peer influence mechanisms that are at work in the ongoing social contagion process. In our study, we combine all of them under the umbrella of peer influence. In the same breath, our postanalysis suggests that we can rule out the mechanisms of increased usage, general awareness, and network effects as possible mechanisms leading to the observed peer influence. Per our experimental design, we are limited in that we only study the local effect on first-degree friends and do not account for the second-degree effects involving friends of friends of groups M and NM. We expect future research to distinguish between the relative efficacy of tactics like persuading a friend to subscribe versus imitation of a friend, as well as go beyond influence in the first degree.

In this paper, we also do not study whether the influence comes from a few high-influence users or a large number of low-influence users. Our goal for this paper is to demonstrate that significant economic social contagion is at work on average in the general population of a freemium social network such as Last.fm. In addition to that, we limit our attention to the influence on first-degree friends and do not look at second-degree effects or at the importance of strength of ties (Centola 2010). Both of these issues are fertile areas for future research.

Finally, we believe our experimental design can be practically carried out by both researchers and practitioners. Practitioners may build a similar gift artifact into their products and use it as a measure to examine the nature and strength of social contagion in their setting. We expect to see more such random acts of kindness to solve interesting problems facing businesses and society.

Supplemental Material

Supplemental material to this paper is available at http://dx.doi.org/10.1287/mnsc.2014.2081.

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